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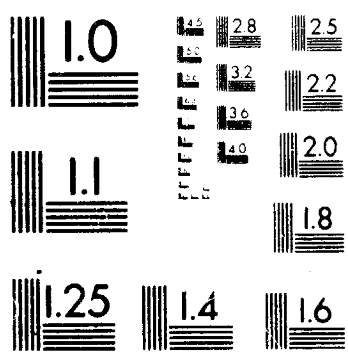

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Report of the

PRESIDENTIAL COMMISSION

on the Space Shuttle Challenger Accident

Volume V

Report to the President

Volume V

By The

PRESIDENTIAL COMMISSION
on the Space Shuttle Challenger Accident

W a shington, D.C.

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with

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PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT WEDNESDAY, FEBRUARY 26, 1986

Dean Acheson Auditorium
Department of State
Washington, D.C.
The Commission met, pursuant to recess, at 9:35 a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ARTHUR WALKER
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA
ROBERT HOTZ
DR. EUGENE COVERT
JOSEPH SUTTER
ROBERT RUMMEL
ALSO PRESENT:
AL KEEL, Commission Executive Director

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PROCEEDINGS

CHAIRMAN ROGERS: I will call the Commission to order, please.

The first witnesses this morning will be Mr. Mulloy and Mr. Hardy. Will they please come forward.

(Witnesses sworn.)

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TESTIMONY OF LARRY MULLOY, MANAGER, SPACE SHUTTLE SOLID ROCKET BOOSTER PROGRAM, MARSHALL SPACE FLIGHT CENTER; AND GEORGE HARDY, DEPUTY DIRECTOR, SCIENCE AND ENGINEERING, MARSHALL SPACE FLIGHT CENTER

CHAIRMAN ROGERS: Welcome, gentlemen. Will you begin by identifying yourselves and giving a little background of the experience that you have had with NASA and your present assignment? I assume that you have some information you want to start with this morning.

Which order do you want to go in?

MR. HARDY: I believe Mr. Mulloy will go first.

CHAIRMAN ROGERS: Thank you. Proceed.

MR. MULLOY: Mr. Chairman and members of the Commission: [Ref. 2/14-2]

I am Larry Mulloy. I am currently the manager of the space shuttle solid rocket booster program at the Marshall Space Flight Center. I joined NASA in 1960. I worked as a loads and dynamics analyst, and then in the Apollo program I worked in the structural subsystem area of the second stage of the Apollo program.

Subsequent to that, I was on a leave of

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absence for a year for some postgraduate work, doing doctoral studies in public administration; and subsequent to that I was assigned as the chief engineer of the external tank project at the inception of the space shuttle program. I held that position until approximately 1979, and then I was the chief engineer for NASA on the inertial upper stage in conjunction with the Air Force until 1982.

In November of 1982, I was assigned as the project manager for the solid rocket booster program. I have a B.S. in engineering from Louisiana State University, a master's in administration from the University of Oklahoma, and, as I previously stated, some postgraduate work in administration at the University of California.

CHAIRMAN ROGERS: Mr. Hardy.

MR. HARDY: Mr. Chairman, members of the Commission: [Ref. 2/26-1]

My name is George Hardy, and I am currently the deputy director of science and engineering at the Marshall Space Flight Center.

I joined Marshall in 1960. I served for a number of years as project engineer on the Saturn 1B booster. I later served as an assistant program manager on that program.

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From about 1967 to 1974, I was in charge of program engineering and integration on the Skylab program. In 1974 I became project manager of the solid rocket booster and served in that position until 1982. Subsequent to that, I served in the position as associate director for engineering in the science and engineering directorate.

I graduated from Georgia Institute of Technology. After approximately six years service in the Navy, I was employed by the Army Rocket and Missile Center.

CHAIRMAN ROGERS: Thank you.

May I ask the still cameras now to take your pictures, and then during the testimony I would ask you to refrain from making shots. It is distracting to the witnesses to have so many shutters clicking each time. And so if you want a period now of taking stills, go ahead, and then I would ask you not to do it during the testimony.

(Pause.)

CHAIRMAN ROGERS: It's not that we object to you taking pictures. We want this to be an open hearing. But with all of the clutter and all of the shutters clicking at one time it's awfully distracting and unfair to the witnesses.

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Okay. Mr. Mulloy, will you proceed?

MR. MULLOY: Yes. Mr. Chairman and members of the Commission, yesterday in the testimony that was given before this Commission, and before that I think in more dramatic statements that have been attributed to Thiokol personnel by the media, a picture has been painted of the events of January 27th that I think at best may be misleading.

Mr. Chairman, with your permission, I would like to state the facts of the events of January 27th, beginning with the 8:45 telecon. I believe there has been a great deal of testimony as to the events leading up to the 8:45 telecon and, with your permission, I would like to begin at that point.

CHAIRMAN ROGERS: Yes, you may. Mr. Mulloy. We want you to feel free to present any evidence that you would like to and as long as you would like to. We will hear anything you want to say.

MR. MULLOY: Thank you, sir.

I previously testified to the flight readiness review process leading up to the launch minus one day review at Kennedy Space Center on January 26th, '86. I have stated how this continuing concern for joint O-ring erosion had been treated in flight readiness review process and all of the events leading up to the decision

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on the 51-L launch.

I would like to now begin with the 8:45 telecon. After being notified of that and arriving at the resident office at the Kennedy Space Center and having the data that had been telefaxed in from the Utah plant—

CHAIRMAN ROGERS: Was that the first time you considered the weather aspects of it?

MR. MULLOY: Oh, no, sir. We—after we stood down from the launch on the 27th at 1:00, I and Mr. Reinartz, Mr. Reinartz polled all elements of the shuttle system for which he is responsible, the SRB, the external tank, and the space shuttle main engines. And I had a discussion on my SRB loop with the SRB people dealing with the question of a 24-hour turnaround to attempt to launch again at 9:38 on the 28th and the effect that the predicted cold temperatures on the night of the 27th might have on that.

The input was received back both to Mr. Reinartz and myself that we were looking at the Launch Commit Criteria relative to temperatures. It was felt there was a need to look at the recovery battery temperatures that are in the forward skirt of the SRB and the fuel service module temperatures that are in the fuel service modules for the thrust vector control

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system in the aft skirt of the solid rocket booster.

The input received back by me was that they did not feel that would be of any concern. They were going to continue to look at it, and if any concern arose they would let me know.

I went to the 2:00 Mission Management Team and reported that there were no constraints to the solid rocket booster for a 24-hour turnaround, that we had taken a look at the recovery battery temperatures and the fuel service module. We did not feel at this time that there would be any Launch Commit Criteria for the low temperature limits that were established for those systems, but that we were continuing to assess that; should anything change in that regard, I would so report that.

CHAIRMAN ROGERS: You referred to the Launch Commit Criteria. What were they as far as you knew in terms of weather conditions? Any?

MR. MULLOY: In terms of weather conditions, yes, sir, I'm aware that there is a Launch Commit Criteria for the system for weather. There are a number of factors in that Launch Commit Criteria. One of them is the ambient temperature, which is established at 31 degrees.

Another is the sea state and winds in the SRB

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recovery area. Another is the cross-winds at the return to landing site runway at Kennedy Space Center. Another is the trans-Atlantic landing site weather, and another is severe weather, which is related to lightning and thunderstorms in the area.

CHAIRMAN ROGERS: And when you say there were no constraints in the 2:00 meeting, does that mean that as far as you could see there were no problems in those areas?

MR. MULLOY: No, sir, I did not evaluate those areas of the Launch Commit Criteria. What I was looking at was the specific Launch Commit Criteria items that are on the solid rocket booster and the effect that the low temperatures would have on that.

I would expect Mr. Aldrich would normally make the judgments on, and his people at Johnson Space Center, would make the judgments on crosswinds and trans-Atlantic weather and the general ambient environment for launch.

CHAIRMAN ROGERS: Just so the Commission has a little better idea, at least I have a better idea, of what you would discuss at the 2 o'clock meeting, would you say, well, we don't know what the weather's going to be like tomorrow, we will have to look at it as we go along, or would you say, we're not sure what the

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seas are going to be and whether we can recover?

What type of discussion was it? I have a little problem following when you say no constraints, and it is the language that I don't quite follow.

MR. MULLOY: Yes, sir. The discussion centered around the conditions that the launch pad would be exposed to during the night, particular attention to freezing of the water system on the pad, freezing of the water in the sound suppression devices that are filled with water at the base of the pad, concern for the formation of ice on the pad, which could cause potential damage to the shuttle vehicle, with primary concern for the orbiter tiles in that case, and I believe for the insulation on the external tank.

Some other discussion, I believe, about the predicted weather for the landing sites, trans-Atlantic landing sites, and the predicted weather for the local weather for KSC.

None of those discussions or the considerations of those discussions posed any constraint to the solid rocket booster.

CHAIRMAN ROGERS: "Constraint" meaning that at that point you thought it would be okay to launch the next day, but you couldn't be sure because those things might change?

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MR. MULLOY: Yes, sir. Based upon the weather conditions that were being looked at at the time and based upon the concerns that were being addressed at that time, I saw—and with the commitment that we were going to continue to look at the weather through the night and then assess that in real time in the morning, which is actually what was done, and the launch was delayed because of the ice on the pad and getting some ice out of the sound suppression blankets.

CHAIRMAN ROGERS: Okay, will you proceed? So at the 2:00 meeting you felt that, although there were problems that might exist the next day, that you felt that those problems probably could be overcome and you could be able to launch?

MR. MULLOY: Yes, sir, I was confident that the only thing that would violate Launch Commit Criteria on the solid rocket booster at that time were a potential violation of the recovery battery low temperature and the fuel service module low temperature.

And as further analysis was done in the afternoon, the initial assessment of that was upheld and we did predict no violation of those Launch Commit Criteria.

CHAIRMAN ROGERS: And at that point the

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O-rings and the seams and so forth were not discussed?

MR. MULLOY: No, sir, they were not.

I subsequently learned that my—and it was testified to yesterday, I believe, by some of the Thiokol people. I subsequently learned that my solid rocket motor element manager, who was at the Huntsville operations support center supporting the launch, did communicate to Mr. Boyd Brinton, who is the project chief engineer for the solid rocket motor for Morton-Thiokol, that the query had come, did we have any constraints for a turnaround.

That had been relayed to Mr. Brinton, who then called, I believe, Mr. Ebeling, it was testified to, at Thiokol in Utah, to begin to look into that. And that led into the events.

CHAIRMAN ROGERS: And what was the man's name you referred to? You said your man?

MR. MULLOY: Mr. Larry Wear. He is my solid rocket motor element manager. I have a solid rocket motor element manager and I have a booster assembly element manager for the other aspects of the solid rocket booster other than the motor.

CHAIRMAN ROGERS: Okay, proceed. I'm sorry to interrupt you.

MR. MULLOY: Okay, sir. When we got the

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charts containing the Thiokol engineering data and the conclusion that Thiokol was drawing from those data arrived at KSC and Marshall—began arriving at about 8:45, when the conference began.

We went ahead and began the conference, but the conclusion and recommendation charts that Mr. Lund subsequently testified to yesterday did not come in until somewhat later. I don't know exactly when they were there, but when we started into the telecon and began discussing the data we did not have those conclusions and recommendations.

We were simply looking at the engineering data and reviewing those engineering data. The concern, of course, that was being expressed was for the low ambient temperatures that were predicted for the night and the effect those low ambient temperatures would have on the propellant mean bulk temperature and on the joint particularly.

If I could have chart SRB-6, please, on the screen, I would like to clarify. When we talk about temperatures—

(Viewgraph.) [Ref. 2/26-2]

—we have a number of them. These specific temperatures don't represent any particular condition on STS 51-L. What we are showing there is the ambient

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temperature at pad B is at ground level, and it is about 50 feet away from the pad. For example, that temperature, as an example, might be 36 degrees at ground level.

Then there is a local ambient temperature—and we will provide hard copies of this, sir, for you later. The local ambient temperature is referred to as in the vicinity of the solid rocket booster, that local ambient temperature will be in a tank condition. It will be below the general ambient because of the effects of the cryogens in the external tank and the heat short that exists through the to the attachments SRB and the wind blowing the cold air around the SRB.

That, for example, might be 30 degrees, while you have a ground ambient of 36 degrees.

CHAIRMAN ROGERS: "Ambient" means outside?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: Outside temperatures?

MR. MULLOY: Yes, sir. The ambient temperature at a point here in this room will be slightly different than the ambient temperature at a point back there by those lights.

But the ambient temperature, it doesn't have to be outside. It could be the ambient temperature of the body of this room or a body of air in this room.

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Then the local ambient is affected by the proximity of heat sinks that are around the solid rocket booster and the wind blowing around that. At that same time, you might have a joint temperature or a temperature right at the field joint that is lower than the local ambient, and that will occur because the local ambient and the ambient temperature has been lower possibly three or four hours earlier, possibly say 18 or 20 degrees. And due to the lag, the local ambient and the ambient may be coming up, but the steel parts are still cold, and so you may have a joint temperature of 27 degrees.

And then when we speak of the propellant mean bulk temperature, that is the average temperature of all the particles of the propellant in the motor taken—it is an average of from the outside, inside. There is a slight gradient through that. That may be 57 degrees, because that is a large heat sink, and if it was 60 or 70 degrees three or four days before, or say 70 degrees, the temperature can get very low and that propellant mean bulk temperature doesn't track that.

And so I just submit that for some clarification. The concern that we were talking about was for the effect of the overnight low on the propellant mean bulk temperature and the effect that it

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would have on the joint and the seals and the performance of those joints and seals.

The Thiokol engineers were stating that they believed the effect of that lower temperature on the O-rings would be to slow the time for the primary O-ring to seal, resulting in greater hot gas past the primary seal and possibly erosion of the secondary seal. The data that they showed included the previous coldest launch, which was STS 51-C, which they stated at least qualitatively had the worst blow-by of any previously observed.

Considerable discussion between Marshall and Thiokol on the significance of those data then ensued. There was at that time—we still didn't have conclusion and recommendation charts. All we were trying to do was understand what the data were telling us.

The major focus of that discussion was the effect low temperatures could have on blow-by of the primary O-ring seal.

Now, if you bring up chart SRB-3, I think at this point it might be helpful to graphically show again the configuration of the joint.

(Viewgraph.) [Ref. 2/26-2 3 of 3]

When we were assessing—and if you could

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scale that down and focus on the left side first, if you would, please, and get the title in.

Okay. We will go from the upper left corner, down the column, and then back up to the right. In the initial condition, the joint is assembled and it has squeeze on the O-rings. What we have been talking to is the O-ring is actually compressed into that joint.

In the specific conditions of STS 51-L, that compression far exceeded the minimum compression required. The compression on the particular joint that has been of interest to us has been 38-thousandths to 40-thousandths, where the minimum requirement is 20-thousandths.

In that initial condition, you have redundant seals, the primary on your left and the secondary on your right.

If you would move down in the left column now.

MR. HOTZ: Mr. Mulloy, may I interrupt you for just a moment. Now, what is the time element there in making that leak check?

MR. MULLOY: That leak check, sir—

MR. HOTZ: What day and what calendar day was it made?

MR. MULLOY: I believe it has been reported

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that it was about 28 days before the attempted launch. It was about the 1st. But when it is made is when the joint is assembled.

MR. HOTZ: Before it goes out to the pad?

MR. MULLOY: Oh, yes, sir. As we assemble each SRM joint, before we put the next SRM segment on we leak check the previous one. In the event that you don't pass the leak check, you have to de-mate and do it over again. So we don't run up the whole stack and then leak check all the joints.

We build it up from the bottom and check each joint as it is assembled.

MR. HOTZ: So it would have been at least 28 days before the launch?

MR. MULLOY: Yes, sir.

DR. COVERT: Mr. Mulloy, may I ask a question, please? You said that there was plenty of squeeze in the O-ring. What temperature would you say that that squeeze referred to?

MR. MULLOY: Okay, sir. In that initial condition, that referred to an ambient condition of 75 degrees. The consideration that was given during the course of the discussion is how much would that squeeze be reduced as the temperature was reduced to 20 degrees?

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That was calculated and it was 3-thousandths of an inch. That occurs for two reason: the diametrical shrinking, as well as the stretching of the O-ring as it is chilled.

DR. COVERT: Your 38- to 40-thousandths then would go 35 to 37?

MR. MULLOY: Yes, sir, that is correct.

DR. COVERT: At 20 degrees.

MR. MULLOY: At 20 degrees, I believe is the temperature that was calculated at.

DR. COVERT: Thank you, sir.

MR. MULLOY: Okay. Then when we do the leak check, the O-rings are then displaced as shown in the second diagram down in the left column. It pushes that primary O-ring back toward the motor pressure side. It pushes the secondary O-ring back against the surface against which it will subsequently seal, if called upon to do so, by any pressure from motor operation impinging on it.

If you will go to the third one in the left column, please. Then, after you take the pressure off—this pressurization is 200 psi initially, to be sure that the O-ring is pushed up against that gap, and then that pressure is reduced. That is with an open source, just turning, opening the valve and letting it flow as

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much as it will, because you will get some blow-by initially in moving that in the pressure check.

Then that is reduced to 50 psi and held, and the spec on that is one psi allowable leakage in 15 minutes. That is with a closed source. It has to hold the pressure between the regulator, the valve on the pressure source, and flow by the seal.

Now, when that pressure is taken off there is some relaxation. Those O-rings don't stay smashed up against that gap, as they were shown when you had the 200 psi.

If you will go to the center column in the top, please.

Now, during motor operation there are two things that can occur. The first is where the primary seal is actuated. Initially in the initial pressurization, as was testified to yesterday, from zero to about 170 milliseconds, which equates to about 200 psi, there is no significant joint rotation.

We have test data, and I believe the engineers stated yesterday, there is a knee in there and it is not linear. It is not linear with pressure. You don't get one-third of the rotation at 200 psi. You get less than one-third, and then it tends to ramp up due to the stiffness of that joint.

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So when the motor is pressurized at about 30 psi, which is 50 to 60 milliseconds, that primary O-ring is translated across from the forward face of the groove to the face that it wants to seal against. We have shown in tests that an O-ring will seal or seat and begin to extrude into the gap at 30 to 50 psi.

Go to the center on that center column, please.

Now, what happens at about 200 psi, again joint rotation is not a significant factor here. You may got 2- to 3-thousandths of joint rotation at a maximum in this kind of a condition. The pressure is impinging there and beginning to extrude the primary seal into the gap.

Go to the bottom of that center column, please.

Now we get the joint rotation. Tests have shown that a good O-ring with a durometer of 90 even—and our spec on the O-ring is 75. This is the sponge and brick analogy that was used by one of the engineers yesterday. I think that is a little dramatic in describing the change in that O-ring stiffness in going from 75 to 90.

But it has been shown that the O-ring will extrude into that gap and seal.

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Now, if you will go to the top on the right, please. Another condition that can happen, the reason we have redundant seals, here at the moment of motor ignition the system is redundant. If for any reason the primary seal does not seat—it is damaged, it has a twist in it, it has a void in it or whatever—the primary seal does not seat, the pressure actuation is now taking place on the secondary seal. That is where the redundancy is, in zero to 170 milliseconds.

Go to the center, please.

Now, that continues, and what happens to the secondary seal is exactly what happens to the primary seal. The primary seal has failed to function, the redundant seal is performing its function at 200 psi.

DR. WALKER: Mr. Mulloy, can I ask a question about that? Would you in your discussion also include your understanding of the waiver and what that meant in regards to how you could consider whether or not the secondary seal was really going to operate?

MR. MULLOY: Yes, sir, I had planned to do that later, but I will just answer that now. My understanding of the waiver is the design goal on the shuttle was to have redundant systems. That design goal is not met in all systems. There are some 829 Crit 1s waivers on the shuttle system. There are 213 Crit 1

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waivers on the SRB.

This particular waiver is one of 18 on the solid rocket motor. Now, my understanding of that waiver, it is required because we have defined a condition under which the secondary seal may not form a seal, and I emphasize "may not."

MR. SUTTER: Could I ask a question, please? Are all of these waivers of equal importance, or of all of these waivers which do you think is the most important?

MR. MULLOY: We are assessing that now, because we're going back and looking at all of the Crit 1s. At the time that the Crit 1s were established, they were all considered to be loss of life and loss of vehicle should that system fail.

I think the question you're asking me is, what is the probability of failure and what is the experience with the system that would say, well, this one is more likely to fail than the next one and the next one, and put a priority order on those. I am not at this point prepared to say that, of the 18 critical systems on the SRM, that an igniter, for instance, is more or less critical than a seal.

But we are asessing that, and the way you have to do that is look at our experience with that.

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DR. WALKER: But you did have a task team working on this particular problem?

MR. MULLOY: Yes, sir.

DR. WALKER: Did you have task teams on any of the other problems?

MR. MULLOY: Yes, sir. Yes, sir, we did. As a matter of fact, on the nozzle we had, as has been related, on STS-8 we found that we were getting some very unusual nozzle erosion. We applied a task team to that to solve the problem, and that is a Crit 1 item. Burn-through of the insulation on the liner of the nozzle is loss of mission and crew.

And yes, we had a task team working on that. And really, on the SRM, those are the two that had very equal importance, really, because the criticality—in answering your question, sir, those two, they would be a real foot race as to which one we would have considered more critical, depending upon the particular time that you looked at your experience with that.

If you'd asked me that question a year and a half ago, I would have definitely said the nozzle.

CHAIRMAN ROGERS: It sees to me, though, that based upon the testimony yesterday, and what I think you're leading up to here, is that the argument is being made that this should have been Criticality 1R. You're

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arguing there was a redundancy in the item and the item itself says there isn't a redundancy, that you have to operate on the basis of Criticality 1; if there is a single failure, it is a loss of life and loss of crew.

MR. MULLOY: Yes, sir, but if you read that total document, which perhaps you have, what it says is under worst case conditions it can be Criticality 1.

CHAIRMAN ROGERS: Wasn't this a pretty bad case, with the weather and all of the alarms that you had, and the recommendations from the engineers at Thiokol? Wasn't this what seemed like a pretty dangerous situation?

MR. MULLOY: It did not seem that way to me then, sir.

Now, if I may continue and answer the question about what the CIL applies to, it says under certain conditions you may have a single point failure. It very carefully says "under certain conditions."

Mr. McDonald testified yesterday—and I cannot assert to the factualness of what he stated, but what he said was, in reality we have never had that worst case condition where we actually flew Crit 1, except on one motor, which was one joint on STS-4. Now, we're looking at that.

I think that certainly is closer to the case

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than we have had Crit 1 on everything except one motor, and the reason it is is because you look at the squeeze that you actually have, given the dimensions that you actually have, and you look at the worst case rotation that can occur under that condition, and that worst case rotation does not result in a secondary seal unseating such that if the primary seal fails the secondary seal will work. Therefore, it is redundant.

DR. RIDE: Does that calculation take into account the out-of-roundness of the segments, the calculation on the squeeze?

MR. HARDY: Yes, it does. If I could make—

DR. RIDE: I would just like to ask whether when you did this calculation for 51-L, which it looks like you did on the 27th, to find out how much squeeze you had on the O-rings, whether you actually did take into account the out-of-roundness on the segments, calculating the squeeze?

MR. HARDY: Yes. Those calculations, Dr. Ride, were made prior to the 27th meeting, but the out-of-roundness is taken into account. The rounding of the three cylinders, if I could describe it that way, you've get inner leg and the outer leg of the clevis, and then the tang itself.

The rounding of those cylinders, which occurs

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at relatively low pressures, is also taken into account.

I would like to mention one thing, if I could read from one place in the critical items list regarding this waiver, I think it clearly describes my interpretation of the waiver, and I don't choose to get into a discussion with somebody else's interpretation. But there is a note that says leakage of the primary O-ring seal is classified as a single failure point due to the possibility of loss of sealing at the secondary O-ring because of joint rotation after motor pressurization. [Ref. 2/26-3]

And I am personally aware of the facts that drove the submission of this waiver, and it was clearly associated with the fact that after motor pressurization, after it's been through the ignition transient, you can have a stackup of tolerances on the metal parts and the O-ring which indeed would not—under which case you would indeed not have a redundant seal.

CHAIRMAN ROGERS: Let's stick with that language, because it seems to me that goes right to the heart of it. Read it again. And as I read it, it means that if the primary seal fails that the mission will fail. Am I wrong?

MR. HARDY: That is not my interpretation.

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CHAIRMAN ROGERS: Well, let's read it. "Loss of mission"—this is actual loss. "Failure effects summary. Actual loss. Loss of mission, vehicle and crew due to metal erosion, burn-through, and probable case burst, resulting in fire and deflagration." Now "Note, leakage of the primary"—and this is the part that I want to refer to.

"Leakage of the primary O-ring seal is classified as a single failure point "—" as a single failure point "—" due to possibility of loss of sealing at the secondary O-ring because of joint rotation after motor pressurization."

Now, that suggests to me that the critical items list says that if the primary O-ring seal fails, that you have got a good probability that the mission will be a catastrophe. Am I wrong about that?

MR. HARDY: You are not wrong, if I might put my clarification into that, if the primary O-ring fails after motor pressurization, after joint rotation.

CHAIRMAN ROGERS: I guess what I'm saying is, isn't that a possibility of exactly what happened in this launch?

MR. HARDY: I don't believe so.

CHAIRMAN ROGERS: Why?

MR. HARDY: Well, I will elaborate on that a

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little bit later here. But in the considerations, at least in the considerations of the subjects at hand, relative to the discussion on the 27th, the discussion on the 27th had to do with the possibility of the cold temperature delaying the complete actuation of the primary seal, thereby extending the duration of blow-by.

Now, when we talk about blow-by of the primary seal, blow-by has to go somewhere, and where it goes to is the secondary seal. If blow-by occurs as soon as the pressure gets to the primary seal, early in the ignition, and that seal doesn't sustain that pressure, it goes immediately to the secondary seal, prior to the time that the joint is rotated.

CHAIRMAN ROGERS: This says "possibility of the loss of the sealing of the secondary O-ring."

MR. MULLOY: After the joint has rotated, sir. The condition that is on the screen now is before joint rotation.

DR. WALKER: But I think a critical and a literal interpretation of that waiver has to be that the primary seal is a single point failure. Now, the wording goes on to explain why this is so, but the wording does not make an exception. It merely explains why the single point failure mode refers to the primary

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seal.

But a strict interpretation of that wording to my mind is that the primary O-ring is a single point failure.

MR. HARDY: I wouldn't deny that. I am relating to what many of us knew about the performance of that joint, its rotation, when we lost—when we could lose, because of the stackup of tolerances, when in the ignition transient prior to full motor pressurization or after full motor pressurization when we could lose that secondary seal.

Our interpretation or my interpretation of the waiver was not to remove the secondary seal from the hardware.

DR. RIDE: It seems to me that really crucial to all of this is the timing function and how quickly you think that the primary will seal, whether it's in that first 160 milliseconds or whether it's not. And if it is not, then you run the risk of getting into the period where joint rotation is more likely.

I guess what I wonder about is the data that you've got to show how the timing function changes at low temperature, because it is certainly going to be a function of the temperature, just because the O-ring is different and it is behaving differently. It is

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deformed in some way.

And it is not clear to me that you've got the data to say that, to discriminate at the level of milliseconds, which is what you are really doing, and to apply engineering judgment based upon really not very much data, and applying that engineering judgment to a Criticality 1 case.

MR. HARDY: Well, I will comment on part of that and maybe Larry would like to comment, too. I would like to clarify and emphasize, if it is not clear, that this seal performance of the primary and secondary seal in the early part of the ignition phase, it is not a step one, step two.

Step one and step two occur simultaneously, meaning that if I get blow-by the primary I am pressurizing the secondary. So it is not a situation where I get blow-by the primary and that extends for some period of time and during that period of time the joint rotates, and then I call on the secondary seal.

I can't have blow-by the primary unless I am trying to pressurize the secondary seal. So it is not a situation where for some number of milliseconds I've got blow-by the primary and then later I call on the secondary seal. I am calling on the secondary seal to function the instant I get blow-by the primary seal.

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DR. RIDE: I think that Mr. Boisjoly described it very well when he said that it is kind of a race. Although you've got blow-by past the primary and the secondary is presumably holding at that point, the race is between the erosion that is occurring on the primary and its attempt to seal.

So it is whether it seals first or erodes first.

MR. HARDY: Which erodes first, Dr. Ride?

DR. RIDE: The primary.

MR. HARDY: Well, if I have initiated—early in the ignition transient phase, if I have initiated a sealing of the secondary seal, subsequent erosion on the primary seal is not of interest.

DR. RIDE: Well, it is if the primary erodes to the point that when joint rotation occurs then you no longer have the secondary.

MR. HARDY: The secondary seal will seal—if the secondary seals at the pressure that it is supposed to seal, and there have been tests run at 30 degrees that indicate that the seal—that a seal will in fact form an extrusion seal.

If that occurs, then subsequent joint rotation, which occurs on every primary seal that ever seals, that will not cause you to lose that seal. And

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it is because of the extrusion of that O-ring into the extrusion gap.

Again, let me re-emphasize, every time the primary seals on a joint early in the ignition phase, that joint, the primary seal, the rotation of the joint is greater on the primary seal than it is on the secondary seal.

So every flight demonstrates the fact that once you initiate an extrusion seal, subsequent rotation of the joint does not affect that seal.

DR. WALKER: But if you have a situation where the rotation begins to occur even after the seals have been made, is this not where temperature is truly critical and where the tests carried out at Morton-Thiokol measuring the response of the seals as a function of temperature should have been very critically considered, because now at lower temperature the seal is going to have a harder time following the movements of the metal parts?

MR. HARDY: I can assure you that that particular characteristic will be thoroughly examined. It is being thoroughly examined. My assessment on the night of the 24th and in fact today is that if I properly extrude that seal into the sealing gap and have the forcing pressure behind it, that the temperatures of interest

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would not have any effect on losing that seal as it goes open.

The key factor which I think many of you have determined, and I agree with you, the key factor is whether or not the initiating pressure on the primary occurs early in the ignition transient. And that was my knowledge at the time, the 24th, and I believe that to be—or the 27th—and I believe that to be the knowledge of everybody participating in that meeting, because there was no discussion to the contrary that I'm aware of.

GENERAL KUTYNA: Mr. Hardy, Ben Powers, one of your engineers, did have an opposite view on that. How did you rationalize his concern?

MR. HARDY: If I may, I'm a little out of sequence here. If I may, when I get into my statement I will address that.

VICE CHAIRMAN ARMSTRONG: I would like to ask a question specifically with regard to the joints on 51-L. Based upon your analysis of the measurements that you have on those joints, would you expect that after full flight pressurization of the motor you would have a secondary seal?

Or, to put it in another way, do you think on these joints you had a Criticality 1 or a Criticality

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1R?

MR. MULLOY: Given the temperature?

VICE CHAIRMAN ARMSTRONG: Given everything you know about it at this point in time.

MR. MULLOY: And the known resiliency. It would be my judgment that, with the resiliency data that was presented on the 27th, that that would be a condition where the secondary seal may not function.

MR. HARDY: After joint rotation.

MR. MULLOY: After joint rotation.

VICE CHAIRMAN ARMSTRONG: So, to just repeat that, in this case we might have a single seal failure, namely the primary seal failure after motor pressurization, that could cause a problem of the kind we are investigating?

MR. MULLOY: Yes, sir. That is the condition that is recognized in the CIL. If I may go to the chart on the bottom, I would like to explain that further, sir.

CHAIRMAN ROGERS: It seems to me that that question and answer become extremely important. Let's be sure that the record is clear on that. I thought Mr. Armstrong's question went right to the heart of the matter, and I'm not sure that I understood the answer.

Is it possible to read that back?

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THE REPORTER: "Specifically with regard to the joints on 51-L, based upon your analysis of the measurements that you have on those joints, would you expect that after full flight pressurization of the motor you would have a secondary seal? Or, to put it another way, do you think on these joints you had a Criticality 1 or a Criticality 1R?"

CHAIRMAN ROGERS: Could we get an answer just to that one? Was that Criticality 1 or Criticality 1R?

MR. MULLOY: Yes, sir. It was our judgment that we were redundant at ignition. As Dr. Ride has pointed out, there was a timing function. We did not conclude on that night that the primary seal would not function and seal. That was inconclusive.

If under the conditions described, if the primary seal did function and seal, then we have joint rotation, then the primary seal subsequently fails, I believe the question was that, under the conditions of temperature, would you anticipate that you had a worst case condition wherein the secondary seal would not be in a position to energize.

I said that may be the case. That may be the case. It has not been shown--what we had was resiliency data that says that the metal under rotation lifts off the seal. It has not been shown that that seal would not function under that condition.

MR. HOTZ: Mr. Mulloy, in the interest of

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clarity, could you just simply state, was it 1 or 1R?

MR. MULLOY: It was 1R, sir.

MR. HOTZ: Thank you.

MR. SUTTER: Could I ask a question, please. I get a little confused. It was my understanding yesterday when the question was asked to transmit data to Kennedy, I asked what was going to be done with the data, was it going to be analyzed so that Kennedy could make a decision? And the answer came back: Well, no decision had been made at that point, and so it was just for information.

But yesterday the impression I got is that the engineers at Thiokol reached a conclusion and, whether it was right or wrong, their conclusion was wait for a warmer temperature. Then the management of Thiokol, as I remember it, reversed that condition to say, go ahead and fly, forget the temperature.

This presentation seems to be another analysis maybe justifying why the decision to fly was all right. Now, if somebody was in charge of this launch--now I see three parties, the Thiokol engineers saying one thing, the Thiokol management saying another thing, and perhaps this discussion saying another thing.

Who does the launch manager listen to?

MR. MULLOY: Well, sir, the launch manager

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listens to Mr. Aldrich, who provides the readiness for launch to the launch director during the launch count. If you go back to the events that you were relating, sir, in the afternoon I was not aware of the engineering conclusions until we were an hour into this telecon, which started at 8:45.

We were looking at data. Your question was was it transmitted to Kennedy so that Kennedy could make the decision? No, sir. The data were actually transmitted to the Marshall Space Flight Center. Part of the Marshall Space Flight Center happened to be at KSC.

Another part—a part of Thiokol happened to be at KSC. Another good part of the Marshall Space Flight Center was still in Huntsville. That telecon that occurred in the evening at 8:45 was a Marshall Space Flight Center solid rocket booster discussion.

MR. SUTTER: Well, somewhere along the line, perhaps after all of the testimony is given, I would like to ask that question again. If the Thiokol people say don't fly, does that stick? Can it be overruled? Who is the final judge that makes a recommendation to whoever has to say launch?

I'm confused and I would like to get it clarified some time.

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MR. HARDY: Mr. Chairman, I think I understand the question and it is certainly a legitimate interest to have. If I might suggest that Mr. Mulloy and myself be permitted to proceed with our statements, I believe that some of the players and where they fit would be a little clearer to you.

CHAIRMAN ROGERS: I think that is correct. Mr. Mulloy, we will come back to some of these questions later on, and particularly Mr. Armstrong's question, because I really hope we can get an answer yes or no.

But in any event, go ahead with your presentation, and we will try to listen carefully.

MR. MULLOY: Okay. I guess where we digressed, I'm to the last diagram here. The condition, the other condition that can exist, where the redundant seal does function, the primary seal has failed and the redundant seal does function, when you start with a redundant system, is depicted on that picture there. The primary seal is compromised, the redundant seal has been pressurized into the gap, and seals. That is another condition that is a "Crit 1" condition.

Should that condition occur—and I will go on now and continue the discussion. As I said, the focus of that discussion at 8:45—and I would reiterate, that was a discussion that was carried on

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under my direction, if you will; I am the SRB project manager and all elements on that discussion were in support of me. As has been stated, Mr. Reinartz, who is the shuttle program manager at Marshall, was also there.

But about halfway through, after we had looked at all of the data, the conclusion and recommendation charts that Mr. Lund had prepared came in and the logic for his recommendation, which did not specifically address don't launch 51-L, what it said was that, within our experience base we should not operate any solid rocket motor at any temperature colder than we have previously operated one, which was 51-C.

CHAIRMAN ROGERS: Didn't you take that to be a negative recommendation?

MR. MULLOY: Yes, sir. That was an engineering conclusion, which I found this conclusion without basis and I challenged its logic. Now, that has been interpreted by some people as applying pressure. I certainly don't consider it to be applying pressure.

Any time that one of my contractors or, for that matter, some of Mr. Hardy's people who come to me with a recommendation and a conclusion that is based on engineering data, I probe the basis for their conclusion to assure that it is sound and that it is logical.

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I found this recommendation not to launch an SRM at a temperature below 53 degrees as to be—it came as somewhat of a surprise to me, and the reason it did, after 51-C, which is when the observation was made about the blow-by, which has been testified to, we dealt with the observations on 51-C very carefully in going into the next launch readiness review.

The conclusions that came out of that was that there may be, there may, be some effect of temperature that is causing this additional blow-by, but the conclusions as presented to by flight readiness review by Morton Thiokol—and I certainly had no dissent at that time—was that 51-E, which was going to be launched in a much warmer temperature, may have exactly the same type of erosion and blow-by, that we might expect that; that the tests and analysis that had been done showing that did show that we were tolerant to that.

And the conclusion was that 51-E was acceptable to launch with full expectation that we might see again exactly what we saw in 51-C. We tested that logic and rationale in great detail. I signed an action item in my solid rocket booster flight readiness review board, which I chaired, to provide additional data relative to that particular recommendation on 51-E. [Ref. 2/26-4]

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Those specific actions were: give me a graphic comparison of the 51-C case joint O-ring erosion dimension with previously observed eroded O-rings; provide description and disposition of all PRs—that is a problem report—on case field joints for both STS-51-C and 51-E; include the metal part problem reports, O-ring, putty, leak tests, all data relative to 51-C and 51-E, since in the initial presentation that was not provided.

Correlate the putty lot that was used on 51-C with the case joints. Did we see any correlation between the lot of putty and erosion in one case joint and not having it in another? Identify any problems with this putty layup.

And number five was, what was the effect of low ambient temperature on the joint?

Number six was, what is joint geometry and O-ring squeeze on both the primary and secondary O-rings for 51-C and 51-E, and to analyze the soot to determine the source.

That action item was answered. The conclusion relative to temperature was that that temperature effect would still give us adequate squeeze and joint operation.

Now, that was the basis that I was coming from,

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which led me to conclude that this was a rather surprising recommendation. There were other factors that were involved in that.

CHAIRMAN ROGERS: Can we have a copy of that, Mr. Mulloy?

MR. MULLOY: Yes.

And this was a rather surprising conclusion, based upon data that didn't seem to hang together, and so I challenged that. And I assure you, sir, that there was no reversal of the tradition of NASA which says prove to me why you can't fly versus prove to me why you can.

As I say, to me that is—it doesn't make any difference. If somebody is giving me a recommendation and a conclusion that is based upon engineering data, I am going to understand the basis for that recommendation and conclusion to assure that it is logical. I think that has been interpreted, when one challenges someone who says, I don't have anything too qualitative, but I'm worried, that that is pressure, and I don't see it that way.

Now, I asked Mr. Kilminster then, who is the vice president of space booster projects, for his recommendation for 51-L. He stated that, based upon the engineering recommendation, he could not recommend

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launch.

Now, I fully understood what Mr. Kilminster was saying at that time. He did not say the engineering data says that you can't launch. He says: I've got an engineering recommendation here and based upon that I certainly cannot recommend launch.

I construed that as making a point that he really had not had an opportunity to think through all of the points that were made during that two-hour discussion, because there were a lot of additional considerations that that data generated during that two-hour discussion, with some 30 engineers involved in that discussion, raising questions, questioning data, questioning the conclusions that are drawn from those data.

And sir, that is not at all unusual in any of our processes. It is not at all unusual.

Now, then I gave—at that point, I gave my assessment of where we had been, including that consideration for 51-E which was in my mind, which I don't believe I stated at that time. But where I was coming from was we had been flying since STS-2 with a known condition in the joints that was duly considered and accepted by Thiokol, it was accepted by me, and it was accepted by all levels of NASA management through

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the flight readiness review process, through special presentations that we had put together and provided up here to the headquarters people. The rationale for accepting that condition had been previously documented.

I would like to now read what I said on that night. You have the notes, because I jotted these things down. This is what was described as a rather logical conclusion by Mr. Russell of Thiokol yesterday.

I stated that, after that beginning, this is a condition that we have had since STS-2, that has been accepted; that blow-by of the O-rings cannot be correlated to the temperature by these data. STS-61 had blow-by at 75 degrees. Soot blow-by of the primary O-rings has occurred on more than one occasion, independent of temperature.

This is in the nature of challenges: Think about this, think about your data.

Primary erosion occurs due to concentrated hot gas passed through the putty. I just wrote that down to say we know why we get erosion. We have done tests, we have done analyses, we understand the limits that the erosion can be, and we understand by tests how much we can withstand.

The colder temperature may result in greater

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primary O-ring erosion and some heat effect on the secondary because of increased hardness of O-ring, resulting in slower seating. Early static tests, however, hydrotests—and I have subsequently found that that is 150 of those, and I will show that on a chart—with a 90 durometer O-ring—now, when we're talking about the hardness of O-rings, this is the brick and sponge.

The 75 durometer O-ring is a little softer than a 90 durometer. The durometer is a measure of the hardness of the O-ring. As the temperature approaches about 30 degrees, the durometer is about 90.

It was stated in the Thiokol data that was presented that we had successfully shown that an O-ring with a durometer of 90 would extrude into the gap and seal. It wasn't too hard to extrude into the gap. Further, I have subsequently found out that the diameter of that O-ring was .275 inches. So it is reduced in diameter also, which gives a pretty good simulation of a cold O-ring. That wasn't stated on the night of the 27th.

The other positive point is that the squeeze at 20 degrees is positive, it is greater than 20 thousandths. We saw a 3 mil reduction in the overall squeeze and we were starting out with a squeeze of some

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36 to—or 38 to 40 mils.

The secondary seal is in a position to seat. It is in a position to seat and seal by the 200 psi and 50 psi pressurization at leak check. The primary may not seal due to reduced resiliency and increased durometer, may not seal, the worst case condition, if the worst thing happens.

However, during the period of flow past the primary the secondary will be seated and seal before significant joint rotation occurs, which is less than 170 milliseconds.

My conclusion was that that condition has been recognized at all levels of NASA management and is applicable to 51-L.

Mr. Reinartz at that point, after I had made that statement, which was in the intent of look at all of the data that we have, look at our flight experience, look at our test data, look at our analysis, don't just look at this isolated body of data that we have on 12 charts in reaching your conclusion, Mr. Reinartz then asked Mr. Hardy for his comments. George will state what he said, but what I understood him to say was that he basically agreed with that summary, but he certainly would not recommend launch against Thiokol's recommendation. And this is

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the gentleman that I rely on for engineering recommendation to launch, and I certainly could not decide to launch in the absence of his recommendation to launch.

Mr. Kilminster then requested an off-net caucus. It has been suggested, implied, or stated that we directed Thiokol to go reconsider these data. That is not true. Thiokol asked for a caucus so that they could consider the discussions that had ensued and the comments that Mr. Hardy and I and others had made.

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That caucus, as has been stated, was going to start. At that point, and Mr. McDonald interjected into the teleconference. At that point, he made the first comment that he had made during this entire teleconference.

Mr. McDonald testified for quite a while yesterday about his thoughts on this, but he did not say any of them until this point. At that point, he stated that he thought what George Hardy said was a very important consideration, and that consideration was, and he asked Mr. Kilminster to be sure and consider the comment made by George Hardy during the course of the discussions, that the concerns expressed were for primary O-ring blow-by and that the secondary O-ring was in a position to seal during the time of blow-by and would do so before significant joint rotation had occurred.

They then went into their caucus, having asked for five minutes—

DR. WALKER: Could I just ask a question?

MR. MULLOY: Yes, sir.

DR. WALKER: That comment by Mr. McDonald, how did you interpret that, as a comment in favor of proceeding to launch?

MR. MULLOY: Yes, sir, I certainly did,

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because—that is, I was sitting—Mr. McDonald was sitting here and it clearly was a supportive comment. I would not say he was recommending launch. What he was saying was that what we were asking them to do was a very important consideration, and as I remember he said, I think that's very important.

And I took it as a supportive comment to the rationale. He certainly didn't say, I think this will support a decision to launch.

DR. WALKER: Could you also, as you proceed with your narrative, tell us about any conversations you had with Mr. McDonald during the 30 or 35-minute caucus?

MR. MULLOY: Yes, sir. If I may, I can't recall the things that have been attributed to me as to whether they occurred during the caucus or whether they occurred subsequent to Thiokol coming off the caucus and making their recommendation.

I don't know that that is germane, but I simply can't recall whether some of these discussions occurred during the 30 minutes we were on mute or subsequent to the decision coming down.

MR. HOTZ: Mr. Mulloy, in that regard, could you perhaps recall just one of those comments? It figures quite prominently in the discussion that you

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were quoted as saying, do you expect us to wait 'til April to launch?

MR. MULLOY: Yes, sir.

MR. HOTZ: Is that an accurate statement or not?

MR. MULLOY: It is certainly a statement that is out of context, and the way I read the quote, sir—and I have seen it many times, too many times—the quote I read was: My God, Thiokol, when do you want me to launch, next April?

Mr. McDonald testified to another quote that says: You guys are generating new Launch Commit Criteria.

Now, both of those I think kind of go together, and that is what I was saying. I don't know whether that occurred during the caucus or subsequent to. I just simply can't remember that.

MR HOTZ: Well, never mind the timing.

MR. MULLOY: Well, yes, sir. I'm going to answer your question now. I think those quotes derive from a single thought that may have been expressed by me using some of those words.

I have not yet encountered anyone other than those at KSC who heard those words, so I don't believe they were transmitted over the net. The total context, I

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think, in which those words may have been used is, there are currently no Launch Commit Criteria for joint temperature. What you are proposing to do is to generate a new Launch Commit Criteria on the eve of launch, after we have successfully flown with the existing Launch Commit Criteria 24 previous times. With this LCC, i.e., do not launch with a temperature greater than 53 degrees, we may not be able to launch until next April. We need to consider this carefully before we jump to any conclusions.

It is all in the context, again, with challenging your interpretation of the data, what does it mean and is it logical, is it truly logical that we really have a system that has to be 53 degrees to fly?

DR. WALKER: I understand how you have clarified that, but I think you have raised an important question, which I hope that you and Mr. Hardy will address, and that is, why was there no Launch Commit Criteria which addressed the O-ring specifically?

We have asked that question of Thiokol and I don't think we have gotten an answer, so I hope perhaps we can get an understanding of that and your thinking and the reason why, after you set up a task team to address this question, you did not also bring your paperwork sort of in line with this concern.

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MR. MULLOY: Yes, sir, I will try, and I think between George and I, possibly we can address that to your satisfaction.

CHAIRMAN ROGERS: I want to also raise a point. You mentioned in your statement that there was somebody from Thiokol that asked for a five-minute recess and you said that the other—I think you used the word—lie or not true. I don't believe anybody testified to the contrary.

I think the testimony yesterday was that they were not certain about who made that suggestion. I wouldn't want the record to state that you said that was a lie, because I don't believe it is in the testimony.

MR. MULLOY: I hope I didn't say that, sir. I hope I said "implied."

CHAIRMAN ROGERS: Maybe I misunderstood it. In any event, I thought the testimony was unclear about that, and I'm not sure it's all that relevant who asked for it. It's pretty clear that you and Mr. Hardy were very unhappy about the recommendation of the engineers.

As we understand it, the recommendation of the engineers was don't launch, and you expressed your displeasure. And somewhere along the line, they decided to have what seems like a five-minute recess, which

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seemed very odd to me the first time I heard it. Why five minutes on a matter of such major importance? Why would anybody say, let's have a five-minute recess? I would have thought they would have had a five-hour recess on a matter of such major importance.

But in any event, I want the record to be clear. I don't believe there is any contradiction of testimony on that point.

MR. MULLOY: I hope that I have not said that I was upset by a recommendation not to launch. What I was challenging were conclusions that were drawn. The recommendation not to launch or to launch at that time wouldn't upset me one way or the other.

CHAIRMAN ROGERS: But that was the whole purpose of the discussion, whether you would launch or not.

MR. MULLOY: Yes, sir, the end result would be that. But the purpose of the discussion was to understand the data and the logic of conclusions being drawn from those data, and that is the way I was working.

GENERAL KUTYNA: Mr. Mulloy, if in fact the criteria were 53 degrees, it would have an impact not only on this launch, but on the shuttle program. Can you describe the impacts that it would have had on the shuttle program as far as launches at Kennedy, at

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Vandenberg?

It is a fairly important decision to say you can't launch below 53 degrees, isn't it?

MR. MULLOY: Yes, sir, I agree with that. I cannot describe the impacts, but, as I say, based upon our previous experience and our actions in flying subsequent vehicles after 51-C, I found that to be a surprising conclusion.

I did not assess what the long-term impact would be. I wasn't in a position to do that at that time.

If I may continue—

MR. SUTTER: I would like to ask a question. Forgetting all of the testimony, what if it came out that there was a deficiency, that in the interest of meeting the launch criteria implied, which is don't launch until safe, if it came out 53 degrees you wouldn't launch, would you?

MR. MULLOY: I'm not sure I understand the question, sir.

MR. SUTTER: Well, this seal has had a history of being in trouble. There was a task force set up. There were memos written, there were test programs planned. The seal, to me, was very, very tender, and it was known to be very, very tender. At least some people

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thought the situation was tender enough that, no matter what the conditions were for this launch, they said, you know, we will wait until the temperature is warmer.

So the launch criteria as written down would be violated, but under circumstances like that it would be. So I don't understand why we hang around the 53-degree thing. I think that seal was tender at 80 degrees maybe, depending upon how it was put together and the tolerances and what-not.

MR. MULLOY: I agree with you, sir.

MR. SUTTER: But then, instead of saying you have to wait until next April to launch, the thing that you do is you go and there were three different levels of improvements that were discussed. The thing to do then was to put those improvements in the program, not infer that these engineers are saying, we're throwing a ringer at you that says don't launch until next April.

I think that is putting those engineers into a little bit of a hot seat. And if they're trying to do their job and say, hey, we ought to do something about this, there ought to have been more attention paid.

MR. MULLOY: Yes, sir. I think we have testified there was a great deal of attention paid to that. Both Thiokol and we have testified there had been a great deal of attention paid to that, from the

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Associate Administrator of Manned Space Flight down to the engineers at Thiokol, over the previous year.

And I agree with your conclusion, sir.

VICE CHAIRMAN ARMSTRONG: Could I ask you to speak a bit more to Dr. Walker's question, which in my view asked why wasn't there a Launch Commit Criteria on seal temperature? Why hadn't something bubbled up through the system that would indicate a more well-defined constraint on launch?

MR. MULLOY: I think that I have to go back over the year where all of the attention had been paid to the seal and the research that we have done and what considerations were being worked in the seal task force at that time.

And I have done some research on that and, starting with, again, starting with the next flight after 51-C and looking at the discussions, the tasks, the program that had been laid out by the Thiokol seal team in conjunction with our people, there just wasn't any great concern expressed about temperature during that time.

VICE CHAIRMAN ARMSTRONG: How about any other factors that might influence the seal constraints that were not currently in the LCCs? Was there anything else?

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MR. MULLOY: Mr. Armstrong, I don't think anyone addressing the seal problem were conscious that they were working a Launch Commit Criteria problem. I think what they were working was trying to improve the margin in that joint and to reduce the incidence of blow-by and erosion.

I doubt that the engineers working that were thinking in terms of Launch Commit Criteria at all. They were trying to look at—they were doing testing, they were doing analysis, they were doing tests on putty, putty layup patterns. They were doing tests on alternatives to putty, they were doing tests on larger size O-rings, putting spacers in the joint to preclude the O-ring, the primary O-ring and the nozzle joints, from having to translate so far.

And I don't believe there was any focus at all on, we have to develop a Launch Commit Criteria for a joint.

DR. WALKER: Mr. Mulloy, yesterday there was a letter which was made public, which was written by Mr. Boisjoly to his superiors, which predicted that unless the seal problem was addressed a catastrophe was possible. And it's my impression that Mr. Boisjoly is the most knowledgeable engineer at Thiokol in regard to the seals.

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Now, was any warning or flavor of that very serious letter transmitted to anyone at NASA, to your knowledge?

MR. MULLOY: No, sir, not that letter. And I guess I wouldn't have expected it to be. That is a correspondence that occurs between an engineer and perhaps his section chief, and I wouldn't expect that type of correspondence to go up the line.

GENERAL KUTYNA: Larry, I have a problem with that. You had a briefing in July that talks about resiliency, you've got a briefing in August at NASA headquarters that talks about resiliency of those seals as a number one concern.

Now, how can you say that wasn't transmitted to NASA?

MR. MULLOY: The memo.

GENERAL KUTYNA: I know the memo. But his concern is what Dr. Walker was asking.

DR. WALKER: Or the flavor of that.

MR. MULLOY: Yes, sir. I have looked back at that briefing. That is one of the things on the title sheet.

GENERAL KUTYNA: It's on the conclusion sheet: "Conclusions: primary concerns, resiliency." [Ref. 2/26-5]

MR. MULLOY: Yes, sir. And what I have looked

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at in that report is for the substance behind that, and I can't find it.

MR. ACHESON: But wouldn't temperature automatically be critical to any elastomer which had a critical function?

MR. MULLOY: Yes, sir.

DR. RIDE: Do you think that there should be a Launch Commit Criteria on the seals or on the joint, as far as temperature goes?

MR. MULLOY: I would prefer to reserve judgment on that until we determine whether there should be or not.

DR. RIDE: I guess that is my point. There wasn't one, but it doesn't appear as though you have data to know whether there should be one or not and if there should be one what it should be.

MR. MULLOY: That is a correct statement.

MR. HOTZ: Mr. Mulloy, could we go back for just a minute to page 4, to your conclusion, which reads: "The risk recognized at all levels of NASA management is applicable to STS-51-L in regard to the O-rings." [Ref. 2/14-2, 5 of 7]

I have a little trouble with that, because in our testimony so far it seems to indicate that the risk on the cold temperatures and the O-rings was not

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transmitted to the highest level of NASA. So how do you explain this conclusion?

MR. MULLOY: Yes, sir. I think the conclusion is the logic that George Hardy will go through in some more detail when he gets to his testimony, but the logic is related to the fact that we have redundancy at ignition, that the concern expressed for the cold temperature based upon the data would appear to be for slower seating of the primary seal, thus more blow-by, and in a worst case condition which would not be quantified, in which case the redundant secondary O-ring seal would seat and seal under worst case conditions.

MR. HOTZ: But was all this information transmitted up to Level II or Level I, to NASA management?

MR. MULLOY: All information—this information that was discussed on the night of January 27th was not transmitted beyond Dr. Lucas at the Marshall Space Flight Center.

MR. HOTZ: Thank you.

DR. WALKER: Mr. Mulloy, let me just clarify your response to one question. In regard to the comments made to Thiokol management by their engineers regarding the concern with the seals, do you think

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Thiokol management discharged its responsibility to NASA in terms of conveying the proper level of concern to you and your colleagues at Marshall?

Should they have raised a larger red flag or did they act properly in giving you the briefings they gave you and alerting you to this problem?

MR. MULLOY: I think we had the attention of Thiokol management relative to putting a dedicated effort on improving the margin in the joint and reducing the incidence of erosion and blow-by. They responded to that by setting up the dedicated team in conjunction with NASA to work those problems.

They did transmit to us all the data through weekly telecons and through periodic meetings that was being generated by that task team in joint meetings, and the total content of the task team concerns is known to NASA.

I simply am saying, to my knowledge, that the effect of temperature never came across as the overwhelming and most important concern on that joint. What came across was the necessity to get a primary O-ring that would not have blow-by such that we wouldn't have erosion of the primary O-ring.

MR. ACHESON: As I understand your testimony, Mr. Mulloy, your argument, if I may call it that, on the

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telecon with the Thiokol people was prompted not by your concern about the launch date, but by a feeling on your part that the data you were looking at to support their recommendation was not what you might call a thorough engineering job.

That is the sense I get from your testimony.

MR. MULLOY: Yes, sir. It didn't hang together. It didn't hang together with all of our other experience and our knowledge, of which Thiokol engineers have tremendous knowledge about the operation of this joint.

MR. ACHESON: Did you have any feeling or apprehension that a delay of the launch date for reasons related to the propulsion system would reflect on you or the Marshall organization?

MR. MULLOY: No, sir. My decision to proceed with the launch as recommended by the Thiokol official responsible for making such recommendations was based solely on the engineering data presented by Thiokol engineering and the Marshall engineering evaluation of those data.

I can assure you, because I am absolutely certain, that no extraneous consideration, such as schedule, came into that decision process.

MR. ACHESON: Thank you.

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CHAIRMAN ROGERS: Do you want to proceed with your presentation?

MR. MULLOY: Okay, sir. At the completion of the caucus, of course, Mr. Kilminster came back on the loop and stated they had assessed all the data and considered the discussions that had ensued for the past couple of hours and the discussions that occurred during their caucus.

CHAIRMAN ROGERS: Was it a couple of hours?

MR. MULLOY: Yes, sir. We started at 8:45 and I believe it was probably 11:00 o'clock before he came back on the loop. It was a long discussion. And I must emphasize that I had no knowledge of what interchange occurred during the caucus at Thiokol, because all sites were on mute. We were on mute at KSC. No communications occurred between myself and Mr. Hardy at Huntsville, nor did any communication occur between KSC and Thiokol during that caucus.

After Mr. Kilminster made that recommendation, Mr. Reinartz then asked if there were any further comments, and to my recollection there were none. There were no further comments made.

I then asked Mr. Kilminster to send me a copy of his flight readiness rationale and recommendation. The conference was then terminated at approximately

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11:15.

I have no knowledge of, as has been testified, of Mr. McDonald being asked to sign that documentation. That would have been unusual, because Mr. Kilminster signs all flight readiness documentation.

Now, after the teleconference was completed, Mr. McDonald informed Mr. Reinartz and me that if the Thiokol engineering concern for the effect of cold was not sufficient cause to recommend not launching, there were two other considerations—launch pad ice and recovery area weather.

I stated that launch pad ice had been considered by the Mission Management Team—

CHAIRMAN ROGERS: Excuse me. Could you identify that discussion, where that took place?

MR. MULLOY: That was after the teleconference was completed, after Mr. Kilminster made his recommendation, after Mr. Reinartz asked, are there any other comments. There were no other comments on the telecon from anyone.

CHAIRMAN ROGERS: And that ended; then there was another discussion?

MR. MULLOY: Yes, sir. Immediately after the teleconference was completed.

CHAIRMAN ROGERS: Who took part in that?

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MR. MULLOY: Mr. McDonald stated to Mr. Reinartz and me that if the Thiokol engineering concern for the effect of cold was not sufficient cause to recommend not launching, there were two other considerations—launch pad ice and recovery area weather.

I stated that launch pad ice had been considered by the Mission Management Team before deciding to proceed and that a further periodic monitoring of that condition was planned. I further stated that I had been made aware of the recovery area weather previously and planned to place a call to Mr. Aldrich and advise him that the weather in the recovery area exceeded the Launch Commit Criteria.

So I stated earlier, when you asked what were the Launch Commit Criteria; one of them was that the recovery area weather has limitations on it. The report we had, that Mr. McDonald confirmed, was that we were outside of those limits.

Now, I must point out that that is not a hard Launch Commit Criteria. That is an advisory call, and the LCC so states that. It does require that we discuss the condition.

So at about 11:30 p.m., Mr. Cecil Houston established a teleconference with Mr. Aldrich and Mr.

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Sestile at KSC. I informed Mr. Aldrich that the weather in the recovery area could preclude immediate recovery of the SRB, since the ships were in a survival mode and they were moving back toward Cape Kennedy at about three knots, and the estimate provided to us by Mr. Sestile was that they would be probably 40 miles from the SRB impact area at the time of launch, at 9:38; and then, continuing at three knots, it was going to be some period of time before they could get back and locate the boosters.

The concern I had for that was not loss of the total booster, but loss of the main parachutes for the booster, which are separated at water impact, and loss of the frustum of the boosters, which has the drogue parachute on it, which comes down separately, because with the 50-knot winds we had out there and with the kind of sea states we had, by the time the recovery ships got back out there, there was little probability of being able to recover those.

I informed Mr. Aldrich of that, and he decided to proceed with the launch after that information. I did not discuss with Mr. Aldrich the conversations that we had just completed with Morton Thiokol.

CHAIRMAN ROGERS: Could you explain why?

MR. MULLOY: Yes, sir. At that time, and I

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still consider today, that was a Level III issue, Level III being an SRB element or an external tank element or space shuttle main engine element or an orbiter. There was no violation of Launch Commit Criteria. There was no waiver required, in my judgment, at that time and still today.

And we work many problems at the orbiter and the SRB and the external tank level that never get communicated to Mr. Aldrich or Mr. Moore. It was clearly a Level III issue that had been resolved.

DR. WALKER: Mr. Mulloy, could I just return for a moment to your conversations with Mr. McDonald. I believe yesterday he stated that he had a discussion with you about the meaning of the temperature on the Launch Commit Criteria of 40 to 90 degrees, whether it applied just to the solid bulk temperature or whether it applied to every element of the shuttle system.

Do you recall that conversation, and could you perhaps tell us your recollection of it?

MR. MULLOY: Yes, sir. There was some discussion that occurred, and I believe this may have occurred during the caucus, it may have occurred after. Mr. McDonald stated that we ought to at least get the joint temperature to 40 degrees. He indicated that at 40 degrees he would feel more comfortable with it,

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because we had a spec that said that we had to be good from 40 to 90 degrees.

I didn't find that logic or that argument to be very logical at all, because based upon the data the engineers were recommending don't launch below 53 degrees.

CHAIRMAN ROGERS: Why is that not logical if he said, why don't you at least require a 40-degree temperature? And you say you didn't think it was logical. It seems very logical to me.

MR. MULLOY: Not on an engineering basis, sir. If one was concerned about the engineering data that said that at a temperature below 53 degrees we have an unsafe condition, there is certainly no logic for accepting that at 40 degrees.

CHAIRMAN ROGERS: Well, it's more logical than 30.

DR. RIDE: What are the solid rocket motors qualified to, what temperature, not the propellant?

MR. MULLOY: Okay. There are two specifications on temperature on the end item specifications to which the solid rocket motors are procured. The first specification states that the motor must be capable of providing a given thrust time trace within limits from zero to 200,000 feet at a temperature

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of 40 to 90 degrees.

The second—

DR. RIDE: And that is basically on the propellant, is that right?

MR. MULLOY: At a propellant mean bulk temperature of 40 to 90 degrees, for clarification. There is another requirement in the end item specification for the solid rocket motor, that states that it must be capable of meeting the natural environments that are specified in JSC document 07700, volume 10, appendix 10.10.

When you go look at that document, it has a number of environmental data in it. The one that is of interest here is the ambient temperature specifications for the launch site, which is then picked up in the Launch Commit Criteria that we discussed earlier.

In that volume 10.10, it states that it must be capable of operating at an ambient temperature of 31 degrees while being exposed to a five-degree sky.

Now, you have to get into heat transfer then to apply that to what the temperatures would be on the SRM.

DR. RIDE: So what you're saying is there was a spec that NASA imposed saying that the SRM should be qualified to launch at 31 degrees. Now, was that taken

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into account in the qual test program for the SRM?

MR. MULLOY: Okay. We came into this discussion not as specific as I can present it to you today, but I have tried to go back and pick up most of the things that we discussed and what we were using in terms of qualification.

May I have the last SRB chart on the screen, please.

(Viewgraph.)

MR. MULLOY: The kinds of things we were discussing during the course of that telecon were what basis did we have for understanding what we would be good for in terms of temperature on the joint. Now, I cannot say that all of these things were discussed, but I know some of them were.

What you will find, you will find a lot of room temperature tests up there at the top of the chart, which is the leak tests which are done on two field joints, the structural test article that was pressure-cycled many times, proof tests that were run to measure the rotation under pressure of the joint, a lightweight cylinder burst test that was done with an intentionally-damaged primary O-ring to assure that the secondary O-ring would seat, and it did.

There were 27 full-scale seal tests with an

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O-ring groove damage tolerances, damage in the grooves and damage tolerance on O-rings. And then there were two cold gas tests.

And these data were presented on the night of the 27th. All of that was at ambient temperature. And then we did discuss what is the development qualification motor experience range, and that is shown on the chart. We had experience everywhere from 40 to 85 degrees.

There then were data presented on two cold gas tests at 30 degrees, where the O-ring was pressurized at the motor pressurization rate at 30 degrees, which would indicate that an O-ring would operate before joint rotation at 30 degrees.

DR. RIDE: Was that actually in a joint?

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MR. MULLOY: No, it is not. It is a full-scale O-ring, full-scale groove, in a scaled test device, where the pressurize rate on that O-ring is zero to 900 psi in 600 milliseconds at a temperature of 30 degrees.

DR. WALKER: You would say, then, that the O-ring was qualified to a temperature of 30 degrees? Would that be an accurate statement?

MR. MULLOY: The data that we were looking at on the 27th were these two tests that indicated that it would perform at 30 degrees under the motor pressurization rate before the joint rotated.

DR. WALKER: What about, let's consider the putty and the O-ring, because that is really the system that responds to the pressure surge. What temperature was the putty/O-ring system qualified to?

MR. MULLOY: The lowest that I'm aware of—and we're still fleshing this out, because this is kind of what we talked about on the 27th, but the lowest that I'm aware of is the 40-degree test on one of the development motors.

DR. WALKER: And of course, during those tests the putty was modified before the test. The putty was not just laid up and then the seal made. The putty was then smoothed out or some attempt was made to remove the

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volcanoes, I think.

MR. MULLOY: Because the horizontal assembly caused that.

Now, there's one other significant point on this chart that we did discuss, that we didn't have the quantities on on the 27th, and I mentioned this earlier. We have 150 case segment proof tests, with a large number of joints with a simulation of a cold O-ring. That is the 90 durometer with a .275, and that was at about 35 degrees.

So those are the certification data that we kind of discussed, all of which we didn't discuss. The two cold gas tests we did, the segment proof tests we did, the development and qualification motor tests we did, as a basis for understanding what we could expect to happen at colder temperatures on the joints.

DR. WALKER: Could we focus on the putty for just a moment? There has not been a great deal of discussion about its response to the cold temperatures, but it is an elastic substance as well. In fact, I am a little surprised that there was not more discussion in your telecon about the effect of the cold temperatures on the putty.

Was that in anyone's minds, and what data did you have to allow you to extrapolate the behavior of the

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putty at these lower temperatures?

MR. MULLOY: Well, I don't recall a discussion of the effect of the cold presented by Thiokol or discussed by anyone—perhaps George does—in the teleconference on the 27th.

DR. WALKER: What about the effects of moisture on the putty? My understanding is that the putty does not respond well to moisture, and I guess it had been raining. Was there any discussion of that?

MR. MULLOY: No, sir.

CHAIRMAN ROGERS: Mr. Mulloy, I want to let you finish, but let me tell you what troubles me very much. I see the charts and I have heard your presentations before, and I recognize your expertise and knowledge in this field.

What is troubling, very seriously troubling, is why, if this is such a convincing matter to you, you are certain of these things, you are sure it's okay, how come then in a matter of such major importance, involving lives of seven astronauts, you apparently were not able to convince any of the engineers at Thiokol who were working on this on a daily basis that you were right?

MR. MULLOY: Sir, I was not aware that they were not convinced. I had no knowledge of what went on

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in the 30-minute caucus at Thiokol, and when asked—when Mr. Reinartz asked, are there any other comments, there were none.

CHAIRMAN ROGERS: Now, would your opinion have changed if you had known all of the engineers or substantially all of the engineers at Thiokol took another position and were opposed to the launch?

MR. MULLOY: Sir, I cannot speculate on that my decision would have been given certain other data.

CHAIRMAN ROGERS: Well, why can't you right now, based on the knowledge that you gleaned yesterday from the testimony, why can't you say whether you would have been influenced one way or the other by the fact that all of those engineers seemed to be opposed to the launch?

MR. MULLOY: Well, I would like to answer your question, sir, except that that is so foreign to the way that NASA does business that I would have to think a long time about an answer to that question.

CHAIRMAN ROGERS: I'm not trying to put you on the spot, but we're checking on the particular process. We're checking on the decisionmaking process. Now, in the process we have the manufacturer or the engineers of the manufacturer who were working with this on a regular basis, testifying they were all against the launch.

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And we have testimony that management took over and made the decision contrary to the advice of the engineers. Now, the question is, if you had known that would it have made a difference? Was the process a total failure, so that you and Mr. Hardy and others didn't know that at all?

MR. MULLOY: No, sir. I presume if Mr. Boisjoly or Mr. Thompson or Mr. Lund said, look, we have a unanimous opinion here that it is unsafe to launch the 51-L vehicle, that would have influenced, obviously, the decision that I would ultimately make.

CHAIRMAN ROGERS: Leave out the word "unanimously". Suppose it's 80 percent of the engineers. Wouldn't that have raised a major red flag to you, and wouldn't you have said to everybody around that this is a very serious matter? We have been concerned about the O-rings for a long time now, and we have a different weather condition and we've never tried it at these temperatures, and the engineers, a lot of them are very worried.

Wouldn't that have made a difference to you?

MR. MULLOY: Yes, sir. And I think what I would have done—and it is speculation. I think what I would have done in that case, I would have asked Mr. Kilminster how, in the light of that recommendation, he

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could recommend launching 51-L, and I would have asked Mr. Hardy how he could concur in that.

MR. ACHESON: May I ask Mr. Mulloy a question going back to the point raised earlier about the logic of the belief one may summarize by saying colder is worse. Now, the Thiokol people had been testing since the summer of 1985, and NASA I believe had a member of its Marshall staff participating in that task force exercise, did they not?

MR. MULLOY: That is correct. We had several.

MR. ACHESON: For the record, who was that?

MR. MULLOY: I need some help with that.

MR. HARDY: Mr. Peoples was our senior representative. There were several other people. I don't know all their names right now, but Mr. Peoples was leading that effort from Marshall.

MR. ACHESON: Seemingly, from what we heard yesterday, this test program led the Thiokol people to the widely-shared view that colder is worse. And the logic of that is, of course, you recognize, because you're dealing with elastomer materials.

Now, did that belief or that logic or, if you want to call it, that general principle, play any part in your thinking or in the discussion you had with your

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own people?

MR. MULLOY: Prior to the 27th, sir?

MR. ACHESON: No, during your deliberations on the 27th.

MR. MULLOY: Yes, sir. The engineering data presented did contain the effects of resiliency and a qualitative assessment of how that would slow down the primary O-ring, and that was considered. And I think, sir, that pretty well completes the points that I wanted to make.

Mr. Hardy I think can address some of these others.

CHAIRMAN ROGERS: Following up on my question before, I want to point out testimony yesterday. The testimony yesterday was to the effect that at one point in the Thiokol discussion Mr. Lund was told to take off your engineering hat and put on your management hat, because

Lund at that point had disapproved the launch decision. So they said, take off your engineering hat and put on your management hat.

And I asked, how do you explain the fact that you seemed to change your mind when you changed your hat? Mr. Lund responded by saying we got our—by blaming NASA's reversal of its launch readiness review policy, we got ourselves in this—and this is a quote

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—"we got ourselves into the thought process that we were trying to find some way to prove to them that it wouldn't work. We were unable to do that. We couldn't prove absolutely that it wouldn't work. That is the kind of boat we got ourselves into that evening."

That is his explanation. He says that NASA put him in that frame of mind and that is why, because he couldn't prove absolutely that it wouldn't work, that he changed his vote. That is his testimony.

Do you accept that testimony?

MR. MULLOY: I heard the testimony, sir. Again, I wasn't privy to the conversation where he was told to change from an engineering hat to a management hat. But, as I stated earlier, I deny that I had any change of philosophy which says prove to me that you can't launch versus prove to me that you can.

I was simply looking at the engineering data that were presented and the conclusion that was drawn from those data, and I found it not to be logical.

MR. SUTTER: Could I ask a question, and it goes back to the last question. When Mr. Acheson was asking you earlier, you made the remark the analysis didn't hold together, and I'm still curious as to which engineering department is the one that calls the shots.

I had the impression yesterday that Thiokol in

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this area had to say yes or no, and Mr. Kilminster was asked for a piece of data saying it's all right to launch. But I believe you are saying that their analysis of the data is wrong and you overruled them. And I'm just curious.

Is this the way this program is managed? I'm just asking a question.

MR. MULLOY: Well, let me explain how I manage the program, sir. I rely on the project manager of my prime contractors to arrive at all flight readiness recommendations. I rely on him to manage the resources that he has available to him within his company.

In this case, it is Mr. Joe Kilminster who has the resources of engineering, manufacturing, quality control, and safety. I do not get involved in resolving conflicts between those departments. I rely on Mr. Kilminster to do that, and I take my recommendations—

MR. SUTTER: But you said you disagreed with the analysis of the engineers' data.

MR. MULLOY: I said, sir, that it did not seem to hold together.

MR. SUTTER: That's a disagreement.

MR. MULLOY: But I rely on Mr. Kilminster to sort all of that out and make a flight readiness recommendation, and he has been doing that for some 24

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flights now.

MR. SUTTER: Even though you knew to some extent that the engineers up there had some pretty grave concerns, you put the burden on Kilminster's back to turn them around?

MR. MULLOY: Not to turn them around. He always has the burden, sir, to make flight readiness recommendations.

I did not detect in the engineering comments—I didn't see this discussion that was going on any different than many other discussions that we have had relative to the validity of data that were being presented as the basis of a conclusion and the logic of the conclusion that was being drawn from those data.

I was not working a problem of I don't want a recommendation not to launch.

MR. SUTTER: But in going back over the history of 1985 and with the concerns over the seal, it seems to me there is a lack of communications and that the real severity of some of the problems has not floated to the surface of the management of NASA properly, and that is what I am trying to get at. And I really am drawing that conclusion.

MR. MULLOY: Sir, I would draw your attention to the August 19th detailed briefing that was given here

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in Washington of last year, where everything that was related to the joint, the experience with the joint, the history of it, the testing that was being done, the analysis that was being done, and the rationale for continuing to fly in the face of that evidence.

MR. SUTTER: I think the rationale to continue to fly isn't very good when you know that work is in place to put—well, to put shims in, to extend the lip to keep the deflection from taking place, to talk about putting O-rings in.

It just seems to me that the whole system was not aware of how serious the concern was, and maybe the flight should have been re-evaluated and maybe some changes should have been made to continue flying. It seemed to me that the philosophy was let's keep her flying, let's work on the changes, but I didn't see any heat to get those changes in the system.

MR. MULLOY: Well, sir, in July of 1985 I ordered billets to allow the incorporation of a capture feature on the steel cases, to eliminate the joint rotation.

DR. COVERT: Mr. Mulloy, about that August 19 briefing, I believe you said a little while earlier, and I may not have written the notes accurately, but I think you said: I knew of the MTI

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seal concerns; I looked at the cover of the briefing charts and felt there was no substance in the briefing.

Did I get that down accurately?

MR. MULLOY: Yes, sir, on temperature.

DR. COVERT: What would it take to have substance?

MR. MULLOY: I think possibly these things that possibly have come out in testimony yesterday could have been discussed.

DR. COVERT: In your experience as an engineer, do you always have complete 100 percent facts available, or do you from time to time have to make decisions based upon incomplete information?

MR. MULLOY: I—most of the time, I have information sufficient to make the decision. You never have all of the data that you would like to have before, sometimes you have to make decisions.

MR. ACHESON: Can I ask just briefly? I still don't understand it. If from 1984 through the Thiokol testing in 1985 through the presentation here in Washington in August, if during all of that time it was recognized that resiliency was important, perhaps critical, to the performance of the seals, why didn't it follow that temperature was a critical part of that resiliency problem?

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I just don't understand that.

MR. MULLOY: Well, sir, I will try again. I'm just saying that that never did come forth in the level of briefings that I took on the subject.

CHAIRMAN ROGERS: But Mr. Mulloy, there's reference to that all through the memoranda. I reread them yesterday.

MR. MULLOY: Sir, I have not seen those memoranda.

CHAIRMAN ROGERS: I will show them to you. There are a lot of discussions about resiliency and the importance to their operation of the O-ring. And, as Mr. Acheson said, it is so apparent that you are going to launch at Vandenberg and Kennedy, there are times when it is cold. And I can't believe that there weren't lots of discussions about the effect of temperature on the O-rings.

In any event, let me come back, because I'm very concerned about the analysis that we are making or that we have to make about that night when you were having the telecon. Now, you make it sound as if this was sort of a routine discussion about data and you always have a free exchange of information about data and so forth.

The thing that is different, it seems to me,

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about this, you had a positive recommendation of no launch. The charts showed that Thiokol recommended against launch. Do you remember any other occasion when the contractor recommended against launch and that you persuaded them that they were wrong and had them change their mind?

MR. MULLOY: No, sir.

CHAIRMAN ROGERS: That is the part that is troublesome. And they said that yesterday, that they thought that you wanted them to change their minds, and that is why they had to rely on management. And they had their contract coming up for renewal, I believe, some time pretty soon this year. So they were under a lot of commercial pressure to give you the answer you wanted.

And they construed what you and Mr. Hardy said to mean that you wanted them to change their minds. They didn't construe it to mean you had a fair discussion about the data. They construed it to mean, just as Mr. Lund said, that's what he thought you wanted.

And I guess what you're saying is that wasn't what you wanted, you just wanted an intellectual discussion about the data.

MR. MULLOY: No, sir. May I address three

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things in your comments, I guess?

Number one, I cannot conceive how Thiokol felt any pressure for the renewal of their contract, because they are our sole source for solid rocket motors at this time, and that contract was going to be renewed. There was no alternative, given the mission model, and so that certainly wasn't a pressure factor for them.

CHAIRMAN ROGERS: Well, can I say that in one of the memos I read yesterday, there was a reference to that fact, and that they were concerned that NASA might be looking for another contractor.

GENERAL KUTYNA: Larry, I think what we're talking about is the dual source, and you've got responses from the contractors due on the 14th of March, I think. And there is some leeway as to how much you're going to buy from Thiokol versus the dual source.

Is that true? A minimum of six from the dual source?

MR. MULLOY: In the solicitation of interest that is on the street, yes. But again, I can't conceive of that being pressure to take a risk of safety of flight if they thought it was unsafe.

The second point, I cannot understand why they would be motivated to take a risk that they thought was

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a safety of flight risk, because they have absolutely no incentive to do that. Our contract incentive is set up in the opposite direction.

Thiokol has a production incentive contract. They don't have an award fee that they get depending upon how I think they respond to my wishes. They have a $10 million penalty if an SRM causes a Crit 1 failure. That $10 million can escalate into much more money than that, given the loss of fee for the mission success fee that is on a block of vehicles, which is another 12.5 percent over the fee otherwise earned.

They have negative incentives to take a risk, and I cannot conceive of how they would allow or think that NASA could pressure them into making an unsafe decision.

CHAIRMAN ROGERS: Could you explain, then, why the management had to take over the decisionmaking process and exclude the engineers?

MR. MULLOY: Well, sir, I was not aware of that conversation that went on, that said, hey, we're going to make a management decision. I am aware that the people who normally take all of the input, study all of the data and then arrive at a recommendation that they make to me is Mr. Kilminster, who is the program manager.

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And not being aware of that, I guess I wasn't surprised that, given the opportunity to think it through, to take advantage of the discussion, the free interchange of discussion in a calm and deliberate manner—I didn't hear a single engineer say it is unsafe to fly 51-L.

What I heard those engineers saying was talking to their data. They were talking to their data and they were talking to their lack of data, and they were giving engineering opinion, which they have stated that they cannot quantify.

And I guess I was not surprised that Thiokol came back after that discussion and came with the here's how I can prove to you we're ready to fly statement.

MR. WALKER: Mr. Mulloy, I know you don't like "what-if" questions, but let's look at the situation a little differently. Let's suppose that the conversations in which the engineers presented their data—and indeed, they have indicated they thought the decision was going to go their way, that is a no launch decision, and so they were really presenting their data, they felt, in a pretty straightforward way—let's suppose that after that presentation Mr. Kilminster had said: Well, the engineers really don't

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have solid data to demonstrate that we have a problem; therefore, my recommendation is to launch.

At that point, would you have questioned Mr. Kilminster on that decision, or would you have been happy with that decision and have terminated the conversation and the telecon at that point?

MR. MULLOY: Yes, sir, I believe I would have questioned him on that. Given that you don't have solid data, how can you make that recommendation, because I would insist on that. I would insist on a rationale and a sound basis for flight readiness, as we always have and which we did in this case.

And I would then turn to Mr. Hardy and ask for his assessment, his engineering assessment of the Thiokol recommendation.

MR. WALKER: So what you're saying is that, no matter what the decision and recommendation was, you would have been the devil's advocate?

MR. MULLOY: Oh, yes, sir.

MR. WALKER: And demanded a more thorough discussion?

MR. MULLOY: Sir, I have tried to establish that when I am looking at a conclusion and a recommendation that is based upon engineering data, it is totally within my character to question those data.

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It even is within my character to look at those data and offer an alternative conclusion, to test how conclusive they are.

DR. RIDE: Their main problem during all of this, the engineers' main problem, was that they felt they didn't have the data. They felt that their temperature data was inconclusive and they were worried about it, but they didn't have the data to quantify what problems the temperature could cause. And that was one of the bases for their recommendation not to launch in the first place, was that they just simply felt that they didn't have the proof that it was safe.

Did you think you had the proof? Did you think that you had the data base to show that it was safe at these temperatures?

MR. MULLOY: No. All I did was recite the data that we had available to us and ask that we consider that decision in the light of all of those data.

CHAIRMAN ROGERS: Could you go to the telefax and see what in the telefax satisfied you? Because the telefax said the evidence was not conclusive.

MR. MULLOY: Yes, sir. The engineering assessment—

CHAIRMAN ROGERS: That is what Dr. Ride is

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pointing out. They say it's not conclusive. They don't say we think it's safe. They say the data is not conclusive. And you say the data is conclusive to you.

MR. MULLOY: The engineering assessment is what I relied upon in the telefax, and I don't know whether I have that here.

Here it is: "Engineering assessment is that colder O-rings will have increased effective durometer, harder." We have no argument with that.

CHAIRMAN ROGERS: Cold will make it harder.

MR. MULLOY: "Harder O-rings will take longer to seat." We had no argument with that. "More gas may pass the primary O-ring before the primary seal seats relative to SRM-15." We had no argument with that.

"Demonstrated ceiling threshold is three times greater than the O38 erosion experienced on 15." That is a fact. "If the primary seal does not seat, the secondary seal will seat."

CHAIRMAN ROGERS: That is pretty positive, isn't it?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: And this critical items list doesn't say that, does it?

MR. MULLOY: No, sir, nor should it. The whole concept of redundancy is if the primary system

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doesn't function the secondary will.

CHAIRMAN ROGERS: But this critical items list says the possibility is loss of sealing of the secondary O-ring. It says the opposite of what this telefax does.

MR. MULLOY: You see, that is a waiver to the requirement to have a redundant system. What this says is the redundant—that if the primary seal does not seat, the secondary seal will. It says you have redundancy and you have not shown that the primary won't function, but if it doesn't the redundant system will still operate.

That is the way I interpret that.

MR. ACHESON: In other words, you say it is consistent with the basis of the waiver?

MR. MULLOY: Sir?

MR. ACHESON: Is that your testimony? As I understand you, you are saying that the conclusion here in Kiiminster's wire is consistent with the basis of the waiver?

MR. MULLOY: No, sir, I am not. I am saying that the waiver acknowledges a condition where under worst case circumstances you may not have redundancy after full motor pressurization and the joint has rotated. What we are saying here is—

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MR. SUTTER: Could I ask a question about this worst case?

MR. MULLOY: Yes. It's tolerance buildup.

MR. SUTTER: I know that. But aren't you always dealing with the worst case? Shouldn't you assume every flight you've got everything going against you?

MR. MULLOY: No, sir, because we measure. We measure the clevis and we measure the tang, and we calculate the gap. And, as Mr. Hardy has stated, we take into consideration the rounding and the further rounding with the initial pressure and work in that gap.

And as Mr. McDonald stated yesterday, to his knowledge there is only one case where we have ever had that worst case condition.

MR. SUTTER: One case is too many.

MR. MULLOY: On STS-4.

MR. SUTTER: Well, it's lucky it got home, I guess.

CHAIRMAN ROGERS: Could I just finish on this telefax, because it says "temperature data not conclusive on predicting primary O-ring blow-by." That suggests to me that there was a possibility of primary O-ring blow-by.

Is that right? Do you accept that?

MR. MULLOY: Yes, sir.

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CHAIRMAN ROGERS: And then they say that "pressure will get to secondary seal before the metal parts rotate."

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: And then they continue to indicate that they are putting some reliance on the secondary seal.

MR. MULLOY: I don't read that that way. That is where we keep, I think, diverging. What they are saying—

CHAIRMAN ROGERS: What does it say to you, then?

MR. MULLOY: Well, it says to me that we have redundancy at ignition. We can expect blow-by at any temperature, at any temperature.

MR. WALKER: Mr. Mulloy, let's try an analogy and see if you think it's an apt one. Let's suppose you are a manager of a baseball team and you have an ace relief pitcher, but let's suppose that that person is injured and has a sore arm and so you put him on the disabled list, and then you can't say that, if my starting pitcher gets into trouble in the fifth inning I can bring in my ace reliever, because he is not available to you by the rules.

It seems to me that that is the situation with

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this waiver, that you had a redundant seal. According to the waiver the redundant seal was not effective. and the waiver didn't excuse in any way you to use the redundant seal.

Now, I understand that you know in the back of your mind that, well, it is still there, even though as far as the paperwork is concerned it is not still there, and I know in the back of my mind how it's going to function. But in fact, by a strict interpretation of that rule, you can't rely on the secondary seal. Now that is the way I understand it.

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MR. MULLOY: That is correct, sir, and what I'm trying to get across, and doing a poor job of it, is that the basic, the basic criteria of 1R was in effect at motor ignition. The data were not conclusive that we wouldn't have 1R at motor ignition. The data were not conclusive that the primary O-ring would not extrude into the gap and seat, but given the worry that was being expressed, if that was the case, the redundant O-ring would, which doesn't require a waiver. That is why you have the redundant system.

DR. WALKER: Well, it is that "but" which gives us a problem.

MR. MULLOY: I understand.

MR. HARDY: I believe Mr. Boisjoly testified yesterday that it was some of the work that he did to define the relationship with pressure and joint rotation that was related to or a backup to the submission of this waiver, and it was that work which shows that in the early part of the ignition transient that the joint rotation has not proceeded to the point that you are in fact, or that you do not in fact have redundancy.

Now, I suspect we could go on for some time on the interpretation of that waiver, and what it means, and how it might be applied in this case, but clearly to me, after motor pressurization, after joint rotation is

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not the time period that we are talking about. What I am talking about in my assessment of the data is the initial part of the ignition transient.

DR. RIDE: I guess what concerns me is that we have got a system that is a Criticality 1, and it is defined as a Criticality 1.

MR. HARDY: Correct.

DR. RIDE: The primary O-ring is what is defined as that Criticality 1 item. Now, all Criticality 1 items are reviewed and signed off all the way up the NASA chain, all the way up to Level I, and have to be signed off and understood at a very high level, and it would concern me, I guess, if I thought that on the day before launch or even the week before launch that engineers were allowed to decide, even based on good engineering data, that well, it was okay to consider that a Criticality 1R because we have added up the tolerances and we have done this sort of analysis, and so we think that we have a redundant seal during these 160 milliseconds. They may be right, but that just hasn't reached the visibility that the original waiver had, and that decision hasn't been signed off at the levels that the original decision was signed off at, and it would concern me to think that Criticality 1s could be handled that way by our system.

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GENERAL KUTYNA: Larry, let me follow through on that, and I am kind of aware of the launch decision process, and you said you made the decision at your level on this thing.

If this were an airplane, an airliner, and I just had a two-hour argument with Boeing on whether the wing was going to fall off or not, I think I would tell the pilot, at least mention it.

Why didn't we escalate a decision of this importance?

MR. MULLOY: I did, sir.

GENERAL KUTYNA: You did?

MR. MULLOY: Yes, sir.

GENERAL KUTYNA: Tell me what levels above you.

MR. MULLOY: As I stated earlier, Mr. Reinartz, who is my manager, was at the meeting, and on the morning, about 5:00 in the operations support room where we all are I informed Dr. Lucas of the content of the discussion.

GENERAL KUTYNA: But Dr. Lucas is not in the launch decision chain.

MR. MULLOY: No, sir. Mr. Reinartz is in the launch decision chain though.

GENERAL KUTYNA: And is he the highest level

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in that chain?

MR. MULLOY: No. Normally it would go from me to Mr. Reinartz to Mr. Aldrich to Mr. Moore.

CHAIRMAN ROGERS: Could we go back to Dr. Ride's question because it seems to me that is really the heart of the matter.

Can you try to answer her question?

MR. HARDY: Well, I believe, if I understood, Dr. Ride was pointing out that since the waiver had been processed to the proper levels, the highest levels, that any consideration on the use of the secondary O-ring should not have been given unless it also was processed to some higher level.

CHAIRMAN ROGERS: Exactly.

MR. HARDY: As far as the levels of review and so forth, I really can't comment on that because that is really not within my purview, but I was making my comments primarily as it related to the engineering assessment of the performance of the seal, and particularly as it was related to the matter of interest, and that is the temperature on the night before.

CHAIRMAN ROGERS: The reason that I think her question and your answer is very helpful is because it was clear that that critical items list was based on the fact that there was no redundancy, that you had to rely

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upon the primary seal. Then you had this discussion back and forth on the eve of the launch, and then the contractor writes temperature data, weather not conclusive on predicting primary O-ring blow-by, and so that says, in effect, because of the weather that the data is not conclusive. You may have primary O-ring blow-by.

MR. HARDY: Yes, sir.

CHAIRMAN ROGERS: And then the Critical Items List says if you have primary O-ring blow-by, it may be a catastrophe. So your contractor himself gives you a signal in this telefax that this may violate the Critical Items List and may be a catastrophe.

MR. HARDY: Well, I think we have a different interpretation of that. I think it is quite obvious we have a different interpretation of that.

Yes, on 51-L one had to be prepared for primary O-ring blow-by. That is also true on every other flight we have had. We have had—

CHAIRMAN ROGERS: But this relates to temperature data not conclusive. This is an analysis of the weather condition.

MR. HARDY: I agree with you, sir, that the temperature data was not conclusive. In fact, we had had blow-by on primary O-rings with joints at 75

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degrees. So it was obviously not conclusive. I agree with that, that it was not conclusive, that the temperature induced O-ring blow-by.

Now, I will discuss later what one can infer in terms of the duration of that blow-by, but it was obviously inconclusive that temperature induced blow-by. We had more blow-by at higher temperatures, relatively higher temperatures, and we had more incident of blow-by at relatively higher temperatures than we had at lower temperatures. There was also no question about the fact that the program had recognized that launch by launch by launch. Every occasion that had occurred in the subsequent flight readiness review—and to my knowledge, this was up to all levels—of the subsequent flight readiness review, the occasion of that blow-by on the previous flight had been evaluated, it had been analyzed. The cause of that blow-by or the phenomenon of that blow-by had been assessed, and it was determined that on each one of those cases, that the understanding of that blow-by and in fact the existence of the secondary O-ring in the early part of the ignition phase to terminate if required, to terminate by sealing the primary O-ring blow-by.

And so, there may well be a misinterpretation or some misapplication, but I don't think there has been

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anything sinister or hidden or, for that matter, even unknown about it.

CHAIRMAN ROGERS: I don't think anybody suggested that.

MR. HOTZ: Mr. Hardy, we heard some testimony yesterday that the character of the damage in the low temperature blow-by and soot was of a much different nature and much more severe than any of your higher temperature experiences.

MR. HARDY: That is correct.

MR. HOTZ: How did you evaluate that factor in reaching your conclusions?

MR. HARDY: I evaluated that as some evidence of the fact that temperature did have an effect on the duration of blow-by. I do not believe that temperature in and of itself induces the blow-by, and I think that is kind of obvious because we have occasions for blow-by at all temperatures, but I do believe that that was indicative of the fact that temperature could have an effect on the duration of the blow-by.

MR. HOTZ: Well, is the duration of the blow-by critical to this whole cycle?

MR. HARDY: The duration of the blow-by is critical only to erosion that might be sustained on the primary O-ring. The duration of blow-by has nothing to

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do with the ability to seal the secondary O-ring. In fact, it is more likely to seal the secondary O-ring in the case that the blow-by is extended.

Again, it is very difficult, I think, difficult for me to convey, but—well, let me request that when I get into my statement I address some of those issues.

CHAIRMAN ROGERS: Mr. Armstrong has one question, and then after that we would like to have a short recess.

VICE CHAIRMAN ARMSTRONG: A question regarding the signature approval or recommendations from the contractor in this case. My question is, in cases of this type is that always done? Do you always have some responsible persons sign off on their recommendations?

MR. MULLOY: Yes, sir, that is the normal process in any flight readiness review, L-1, or any issue related to flight readiness, and the files show that at all levels. We require it at every level.

VICE CHAIRMAN ARMSTRONG: So the fact that Mr. Kilminster signed this thing was typical, and it would be the standard procedure?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: Would it be signed by him and not the man on the scene?

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MR. MULLOY: That is correct, sir. We require the element project manager to certify flight readiness and not anyone else.

CHAIRMAN ROGERS: Let's have a ten-minute recess.

(A brief recess was taken.)

CHAIRMAN ROGERS: The Commission will please come to order.

Mr. Mulloy, Dr. Keel has a couple of questions, and then we will go to you, Mr. Hardy, right away.

DR. KEEL: Mr. Mulloy, I think at the end of the previous session you said that it was normal to ask for a written statement, that all flight readiness reviews required written statements. Is that accurate? Was that your statement?

MR. MULLOY: Yes, sir, that is.

DR. KEEL: But this wasn't a flight readiness review, was it?

MR. MULLOY: It was, in a sense, it was a review like an L-1 or dealing with issues related to the flight. It was not a chaired flight readiness review.

DR. KEEL: Aren't all flight readiness reviews Level I and Level II?

MR. MULLOY: No, sir, we have a Level III

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readiness review which I chair for the solid rocket booster. I also have a flight readiness review that Mr. Wear chairs for the solid rocket motor before it gets to me, a flight readiness review that is chaired by Mr. le Burt, who is the booster assembly manager before it gets to me, and then the prime contractors, USBI, FPC, and Thiokol have a flight readiness review chaired by a management official above the level of the program manager prior to the time of getting to the element managers.

DR. KEEL: But what about the nominal reference to flight readiness review, the one that occurs two weeks prior to launch. Is that a Level I and Level II?

MR. MULLOY: Yes, sir, that's Level I. Then there is one subsequent to that which is the L-2 or L-1 review, which, it also has a signed certification with it.

DR. KEEL: And what level is that?

MR. MULLOY: That is a Level I review that occurs one day prior to the launch.

DR. KEEL: I think there was just a risk of confusion because you referred to this in a statement that it required a written sign-off, since all flight readiness reviews do, and I think nominally the Commissioners think of flight readiness reviews as those

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Level I and II meetings that have been briefed to the Commission, and this certainly wasn't a flight readiness review in that context.

MR. MULLOY: No, sir.

I might say that another thing that does require a sign-off by the program manager is any action that results from anything related to a flight, and this would certainly fall in that category where concern was raised and an action is assigned to go develop data and then review those data.

I view it as part of the flight readiness review process.

CHAIRMAN ROGERS: All right, you may proceed, Mr Hardy, and I apologize for not getting to you sooner.

MR. HARDY: Mr. Chairman, members of the Commission, before I relate to you my participation in and recollections of the teleconference between Morton Thiokol personnel and Marshall Space Flight Center personnel on the evening of January 27, 1986, I would like to describe just briefly to you what my role is and the role of other personnel from Science and Engineering Directorate is with respect to flight readiness assessment for Shuttle flights.

The role of the Science and Engineering Directorate

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at Marshall in this regard is to participate in flight readiness reviews, identify any pertinent issues relative to flight readiness, and review any issues identified by others, including contractors, and to assess the rationale for flight and supporting data on any issue, and then finally, to provide recommendations to the appropriate readiness review board, if such a board is in session or otherwise to the program management.

With reference to the teleconference that started approximately 8:45 Eastern Standard Time on January 27, 1986, I would like to identify the personnel at Huntsville from the Science and Engineering Directorate that were in direct support of me during this teleconference: Dr. Wayne Littles, Associate Director for Engineering; Mr. Jim Smith, the solid rocket booster Chief Engineer; Mr. Robert Schwinghammer, Director of our Materials and Processes Laboratory; Mr. Bill Riehl, an engineer in the Materials and Processes Laboratory, and Mr. Riehl is a nonmetallic materials expert; Mr. John McCarty, Deputy Director of our Structures and Propulsion Laboratory; Mr. Ben Powers, an engineer in the Structures and Propulsion Laboratory specializing in solid propulsion; and Mr. Keith Coates, who is in the Office of the Associate Director for

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Engineering.

At the teleconference on the evening of January 27, 1986, Thiokol engineering personnel in Utah reviewed charts that had been datafaxed to Huntsville and KSC participants just prior to the beginning of the conference. Now, I am not going to repeat a lot of what you have already heard, but I will give you some of my views on the whole matter.

The presentations were professional in nature. There were numerous questions and answers. There was a discussion of various data and points raised by individuals at Thiokol or at Marshall or at Kennedy. I think it was a rather full discussion. There were some 14 charts presented, and as has been mentioned earlier, we spent about two, two and a half hours reviewing those. To my knowledge, anyone who desired to make a point, ask a question or expressed a view was in no way restrained from doing so.

As others have mentioned, I have heard this particular teleconference characterized as heated discussion. I acknowledged that there were penetrating questions that were asked, I think, from both, from all people involved. There were various points of view and interpretation of the data that was exchanged. The discussion was not, in my view, uncharacteristic of

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discussions on many flight readiness issues on many previous occasions. Thiokol Engineering concluded their presentation with the recommendation that the launch time be determined consistent with flight experience to date, and that is the launch with the O-ring temperatures at or greater than 53 degrees Fahrenheit.

Mr. Kilminster at Thiokol stated that with, to the best of my recollection, that with that engineering assessment, he recommended we not launch on Tuesday morning as scheduled.

After some short discussion, Mr. Mulloy at KSC summarized his assessment of the data and his rationale with that data, and I think he has testified to that.

Mr. Reinartz, who was at KSC, asked me for comment, and I stated I was somewhat appalled, and that was referring specifically to some of the data or the interpretation of some of the data that Thiokol had presented with respect to its influence on the joint seal performance relative to the issue under discussion, which specifically was the possibility that the primary seal may take longer to actuate and therefore the blow-by of the primary seal may be longer. I am going to elaborate in that a little further in this statement.

Then I went on to say that I supported the

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assessment of data presented essentially as summarized by Mr. Mulloy, but I would not recommend launch over Thiokol's objections.

Somewhere about this time, Mr. Kilminster at Utah stated that he wanted to go off the loop to caucus for about five minutes. I believe at this point Mr. McDonald, the senior Thiokol representative at KSC for this launch suggested to Mr. Kilminster that he consider a point that I think I had made earlier, that the secondary O-ring is in the proper position to seal if blow-by of the primary O-ring occurred.

I clearly interpreted this as a somewhat positive statement of supporting rationale for launch. Personally, I believe any other interpretation of that is a case of convenience of memory. The status of the caucus by Thiokol lasted some 30, 35 minutes. At Huntsville during this Thiokol caucus, we continued to discuss the data presented. We were off the loop, we were on mute. We were around a table in small groups. It was not an organized type discussion. But I did take that opportunity to discuss my assessment and understanding of the data with several of my key advisors, and none of us had any disagreement or differences in our interpretation of what we believed the data was telling us with regard to the primary issue

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at hand.

When Thiokol came back on line, Mr. Kilminster reviewed rationale that supported proceeding with the launch and so recommended. I have no knowledge of who participated in the caucus at Thiokol. It is not typical to ask for a vote count when a senior responsible official of the contractor presents the company position. What is typical is to assess the rationale for that position, whichever way it may come down. During the caucus I had written my evaluation of the data presented as I developed it in my mind, and as I reviewed it with some of my senior, key advisors during the caucus, and I will review that with you later in this statement.

Mr. Reinartz asked if anyone on the loop had a different position or disagreed, or something to that effect, with the Thiokol recommendation as presented by Mr. Kilminster. There were no dissenting responses.

The telecon was terminated shortly after, and I have no knowledge of any subsequent events or discussions between personnel at KSC or at Thiokol on this matter.

As I stated earlier, during the Thiokol caucus, I discussed the data and the issues with several of my key advisors, and there was no disagreement among

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us. I have also stated earlier that there was no dissenting responses when Mr. Reinartz asked if there were any disagreements following Mr. Kilminster's recommendation to proceed with launch.

I have learned since the 51-L incident that Mr. Ben Powers expressed some concern about the O-ring performance directly to Mr. Smith, the SRB chief engineer, and Mr. McCarty, the Deputy Director of Structures and Propulsion Laboratory. Mr. McCarty is Mr. Powers' supervisor. Mr. McCarty has stated to me that he considered Mr. Powers' comments along with all the other data presented and discussed when he made his input to me.

Now, I believe at this point, if I may, I need to address before I proceed on into my assessment and evaluation of the data, I would like to address two points that have been discussed rather prominently in these hearings. The first one relates to the assertion or inference that pressure was or may have been applied to the contractor during the course of this teleconference. With regard to my participation in the discussion of January 27, 1986, I would like to give you a little insight into my mode of operation in these matters, and I believe those that are familiar with me will recognize these quite readily.

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In any matter related to flight readiness issues, I am dedicated to a thorough and open discussion of the data and the rationale developed from such data. I have tried to the best of my ability to determine the facts in the data. Unfortunately, facts and engineering data are not always interpreted by all engineers the same way. But regardless of the side that I come down on, it is in the pursuit of the facts of the data.

I am likely to probe and sometimes even challenge either a pro position or a con position, or sometimes even both, on an individual issue that is being put forward or a set of rationale or an interpretation of the data that might be associated with that.

And the objective of this is just simply to test the data, test the degree of understanding of the individual that is presenting the data, test his, not only his engineering knowledge, but his engineering assessment of that data. So that is characteristic of the way that I do, and I think anybody that knows me would realize that that is not interpreted as coming on strong or applying pressure.

But I have thought a lot about this business of applying pressure, and the application of pressure is, of course, to no avail unless there is a response to

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that pressure. And if there is to be a response, in my mind, there must be a motivation. And I can't speak for any contractor personnel that was involved, but since the issue was first raised, I have wracked my brain to try to see if there was any conceivable motivation that may have made any contractor representative feel that he was under pressure from anything I said.

And we are not talking about customer relations here. We are talking about flight readiness issues. Mr. McDonald attached it to the thoroughness down to the smallest detail that he was accustomed to in all flight readiness reviews. He also mentioned his conviction that the conservative approach was emphasized.

Thiokol will also remember that on more than one occasion that I have rejected their proposal to fly. One such occasion occurred the latter part of last year when a segment was damaged, the joint clevis, in fact, on that, the clevis joint on that segment was damaged during handling. Thiokol proposed that we fly that segment, and I nonconcurred. And although it is not in my area of responsibility, I am aware that as Mr. Mulloy has attested, that the contractor is highly incentivized by terms of the contract for a mission success with a rather significant penalty clause, and

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further, as I have testified and others have also testified, on the evening of January 27, I clearly stated that I would not recommend launch over the contractor's objection.

And so I have great difficulty in seeing what any motivation for response to "pressure." But I am sure you are going to pursue that even further.

Now, with regard to the second item that has been rather prominent in these hearings, I would like to state that as far as my participation in the discussions at issue, I categorically reject any suggestion that the process was prove to me it isn't ready to fly as opposed to the traditional approach of prove that this craft is ready to fly. I have no responsibility for schedules, I have no responsibility for manifest. I couldn't tell you with any degree of certainty what the next launch cargo was on the next flight. I couldn't tell you with any high degree of certainty even what date it was scheduled to fly.

As I said, I have no responsibility for that, and I have occasion to know that at times, but it is not my primary job.

I would hope that simple logic would suggest that no one in their right mind would knowingly accept increased flight risk for a few hours of schedule. I

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can say certainly that not the dedicated men and women with whom I am associated, many of whom have literally put their blood, sweat and tears into the Shuttle program.

And I will only say one other thing on that subject, and that is that I believe to suggest that flight safety was disregarded or not properly regarded in the discussions on the night of January 27 in my opinion does a great disservice to many dedicated, committed professionals.

Now, if I could, I would like to proceed on with a summary of my assessment of the data presented and the conclusions that I came from this.

CHAIRMAN ROGERS: Could I interrupt because I think you did say something that was disturbing. When you referred to Mr. McDonald, you referred to convenience of memory, which suggested to me that you thought he was making something up for the record or some such.

Was there anything that he said that you thought was not true?

MR. HARDY: No, sir, I did not mean to imply that.

CHAIRMAN ROGERS: Why did you use "convenience of memory?" That is what it sounded like.

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MR. HARDY: Well, I will explain that, and that may not be a good, proper term to use. But there have been many of us that were involved in flight readiness issues for 51-L, even more than the issue of temperature on January 27, and I think there have been many of us that have gone back and asked ourselves questions and second guessed ourselves, that have really probed to bring back to the same level of understanding in the consideration of any of these issues that we had on the other side of 51-L.

And that is not easy. It is not easy just within your own heart and mind to try to separate yourself from what you know has happened. I don't mean by that the cause or failure. I am talking about the tragic incident itself. And I am not suggesting that anyone, and in any testimony before this committee or this Commission has knowingly in any way presented untruth. But I have talked to some of my colleagues, and I have found that they found it very difficult to remember precisely not only everything said and everything done, but even more than that, some of the motivations or some of the thoughts that took place at that time.

CHAIRMAN ROGERS: Can I say, speaking for myself, that I respect your views and I believe what you

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just said. I also have the same feeling about Mr. McDonald, and I would like to have you withdraw those words, "convenience of memory," because I don't think that is what he did.

MR. HARDY: I would be pleased to do that.

CHAIRMAN ROGERS: Okay, thanks.

MR. HARDY: I would like now to give you a summary of my assessment of the data presented and the conclusions that I drew from that data. I have chosen to discuss the assessment of each type of data and then my conclusions that were drawn from that, and that is a little bit different from the arrangement that is in the material that has been provided you. It is the same material but arranged slightly differently.

I interpreted and assessed the data presented by Thiokol in the context of the accepted performance and operational characteristics of the field joint during the motor ignition transient. The essential features of these characteristics are the primary O-ring moves from the position it has assumed during the leak test to its sealing position as the pressure upstream of the primary O-ring increases from ambient pressure to approximately 25 to 50 pounds per square inch.

Secondly, this pressure is imposed on the primary O-ring during the first few milliseconds of the

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ignition transient.

And thirdly, if blow-by or failure of the primary O-ring occurs in this timeframe, that is, in the early part of the ignition transient, the secondary O-ring is in its seating position, and this has been assured by the leak test port.

Now, I might add that I make that statement, Your apparent interpretation of the Critical Items List waiver notwithstanding. No one in the meeting questioned the fact that the secondary seal was capable and in the position to seal during the early part of the ignition transient prior to any significant joint rotation. Thiokol, in fact, presented data to that effect in that meeting, and since Marshall actually originated the waiver, I can also say that that was in fact the interpretation at the time of the submission of that waiver.

Now, Mr. Chairman, I will relate to you my assessment of the data as I understood it, and I assessed it on the 27th. As you appropriately noted at the beginning of the session, the cause or causes of failure have not been determined, and the effects of temperature as well as any other potential cause are being actively and aggressively pursue, and the failure analysis is under way. The issues addressed during the

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teleconference was the potential for the predicted temperatures to adversely affect the performance of the seals during the ignition transient; specifically as it might affect the actuation time of the primary O-ring.

Thiokol engineering presented data on the history of primary O-ring erosion and blow-by at the motor field joint. I concluded that there was no direct correlation with this data and temperature. O-ring blow-by was experienced on 61-A with a joint temperature of 75 degrees, and O-ring blow-by was experienced on 51-C with the joint at 50 degrees. There was no O-ring blow-by or erosion on two ground test motors at somewhat lower temperatures.

MR. RUMMEL: Mr. Hardy, if I may interrupt a second, wouldn't the fact that you had blow-by at the higher temperatures only heighten the concern at the lower temperatures, understanding the elastomeric characteristics of the seal?

MR. HARDY: I think the fact that there was blow-by experienced on the higher temperatures as well as the low temperatures heightened the interest in, number one, understanding the cause of the blow-by, and number two, proceeding with activities associated with eliminating the blow-by.

Now, let me make just a couple of other

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comments. I believe there are a number of factors involved in the phenomenon of blow-by of the primary O-ring. As I have stated earlier, I do not believe that temperature, high or low, induces primary blow-by. I do believe that temperature can have a potential effect on the duration of that blow-by. Blow-by occurs during the period of time when the primary O-ring is being pressure actuated from one side of the seal to the other side of the seal, and being extrusion-sealed into the seal.

And so, I would accept that evidence would indicate, and I think 51-C is supportive evidence of that fact, I would accept that evidence would indicate that the duration, and I think that is very important, the time during the ignition transient when blow-by is occurring, the length of time that it occurs can be affected by temperature, and later on, as you can see, I took that into consideration in my assessment of the data.

MR. RUMMEL: Just another aspect for clarification.

Wouldn't you say, or would you not say that the greater the duration of the blow-by, the greater the risk?

MR. HARDY: If that blow-by occurs in the early part of the ignition phase, which every indication

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of everything we have seen indicates that it has, no, I don't think the duration of blow-by increased the risk. I think the duration of blow-by increases the probability you will have to call on the secondary seal at that time.

MR. RUMMEL: Well, can you help me a little? I would assume that blow-by might be called a first stage to erosion, and that the greater the length of time that the seal is exposed to blow-by, the greater the possibility of erosion, and the longer erosion occurs, if it does, the greater the risk.

Is that incorrect?

MR. HARDY: That is essentially correct.

There are two things that happen to a primary O-ring when you get blow-by, or conceivably three things. One is it terminates itself in a short period of time because the seal continues to pressure actuate, and that is about the end of it. You see some evidence of that blow-by with soot on the back side of the seal, and if the blow-by is sustained for a very long period of time, you might see some evidence of heat effect on the primary O-ring.

However, blow-by, the primary O-ring pressurizes the cavity between the primary O-ring and the secondary O-ring, and as I mentioned earlier, it is

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a mistake to think about these things as step one, step two, step three. If step one is blow-by the primary O-ring and step two is pressurizing the secondary O-ring, then step one and step two are the same thing. They occur simultaneous with each other.

Now, in the event you sustain blow-by the primary O-ring for the full period of time and sealed the secondary O-ring for the full period of time that it took you to fill the cavity between the primary O-ring and the secondary O-ring would limit the amount of erosion you get on the primary O-ring, because when you fill the cavity and equalize the pressure, you stop the flow, and there is no more heat transfer.

And that limiting factor has been calculated to be, I have forgotten the exact number, but it is at least twice, at least twice the amount that it would be allowable as demonstrated by tests before you lost the primary O-ring.

There is a limiting factor on erosion, and that is in every case the amount of flow that you can sustain, and the amount of flow you can sustain is the size of the cavity that you are pressurizing versus the source pressure.

DR. RIDE: Is your factor of two safety, is that considering only whether the O-ring is still able

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to seal, or does that take into account its structural integrity?

MR. HARDY: Both.

DR. RIDE: So you have done an analysis of the structural integrity of an O-ring that has experienced what you consider to be the maximum, the number you are quoting as the maximum erosion?

MR. HARDY: Yes.

DR. RIDE: So you wouldn't expect, for example, high winds or other kinds of forces that the O-ring might see in an overly eroded state to cause it to break?

MR. HARDY: I would not before 51-L, and where I stand right now, I would not, but I would reserve total judgment on that until we complete the failure analysis.

MR. SUTTER: Could I ask a question?

Some of the hypothesis depends upon the secondary seal being in place because of the pressure check, is that right?

MR. HARDY: Yes, sir.

MR. SUTTER: The pressure check is done at assembly.

MR. HARDY: Correct.

MR. SUTTER: The lower units could be pressure

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checked, and then as you put the others on you in effect are changing the load even in the assembly bay.

MR. HARDY: That is correct.

MR. SUTTER: Then the machine is hauled out on a ramp which puts some vibratory loads on it, I assume. Then it is put in position, which puts another load on it. Then it sits there for 28 days with the temperature going up and down, which in effect puts some work on the seal.

What assurance is there that after those 28 days that that secondary seal really is in position and all of them completely around the circumferences?

It seems to me that is a risky assumption.

And another question is has anybody gone out and made a pressure check, say, the day before the launch to make sure those seals are in place?

MR. HARDY: I am not aware of any pressure checks the day before or a few days before the launch anywhere in the program. I am not aware of that.

But if I could address just a couple of points you make, that is, that is true that when the additional segments and then the entire Shuttle is added on to the field joint of interest or added on above the field joint of interest, that that compressive load, the compressive load at that point, in contrast to the tension load,

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that tends to open the clevis when you pressurize the motor, at that point you have got a very high compressive load on it.

Now, it is true that that compressive load also tends to open the gap, if you will, but a very, very small amount compared to that which occurs when you pressurize it.

MR. SUTTER: Well, what about the thing sitting out on the ramp and the temperature dropping, which in effect makes the diameter of the seal get smaller and puts a tension on it so it again gets smaller? Couldn't it pop out of its sealed position then?

MR. HARDY: Well, the temperature, or the shrinkage of the O-ring, if you will, due to temperature effects is calculated and has been calculated and is considered and was considered in this meeting on the 27th.

MR. SUTTER: But does anybody know that the calculations are accurate?

MR. HARDY: I have not personally checked the calculations, but I believe those calculations are fairly straightforward to calculate the shrinkage of the material, but I have not personally checked those calculations.

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MR. RUMMEL: Have you encountered separation of the vulcanized joints in the O-rings, to your knowledge?

MR. HARDY: Excuse me, sir?

MR. RUMMEL: Have you ever encountered any sort of separation during operation of the vulcanized joints in the O-rings?

MR. HARDY: Not to my knowledge. You are talking about the vulcanization of the ends of the O-rings or sections of the O-rings?

MR. RUMMEL: Yes. I understand that five joints are allowed in each O-ring, and I am simply curious as to whether there is any record of premature separation of those joints.

MR. HARDY: I am not aware of any, and I think I would be well aware of them if they occurred anywhere in the field after delivery of the hardware.

CHAIRMAN ROGERS: Not being an engineer, and because I am surrounded by so many capable engineers, I would like to ask a question that is not an engineering question.

At some point, I gather, you have to, you will agree that the colder the weather the greater the risk. Is that accurate?

MR. HARDY: I am not sure that that is an

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accurate statement. I would say—and again, I am going to, with your permission, I am going to look at that question from the other side of 51-L.

CHAIRMAN ROGERS: But I mean at the moment you are inclined to say it doesn't make any difference how cold it gets as far as the risk is concerned on the O-rings?

MR. HARDY: At the moment I would say that the consideration of the effect of temperature on the joint is certainly an active failure analysis, and there are some features of the joint, indeed, where temperature can affect it.

CHAIRMAN ROGERS: But that's a key question, it seems to me, because if you in the back of your mind, in the back of Mr. Mulloy's mind you said it really doesn't make any difference how cold it gets as far as the joint is concerned.

MR. HARDY: No, sir, I don't believe I said that.

CHAIRMAN ROGERS: No. I am asking now do you think that at some point the coldness of the weather makes a difference on the risk?

MR. HARDY: Well, I am sure there must be some point because there is some point at which the structural integrity of the O-rings just wouldn't be

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maintained.

CHAIRMAN ROGERS: At what point would that be?

MR. HARDY: I think that would be somewhere in the minus 40, minus 50 range.

CHAIRMAN ROGERS: So insofar as you are concerned now, it wouldn't make any difference about the risk in connection with the joints if it was above 40 below? In other words, I am trying to see what your thought process was.

I think most people, or a lot of people have felt that the worse the weather, the more the risk insofar as these joints are concerned, and I guess you are saying you don't agree with that.

MR. HARDY: Well, I think probably there might be dozens and dozens of things on the vehicle that one could say—

CHAIRMAN ROGERS: No, I am talking about the O-rings now and the joints.

MR. HARDY: I could not in my mind quantify any increased risk—let me make sure I say that correctly. I could not in my mind determine that there was any increased risk to safety as a result of the temperature that we were discussing on the night of the 27th.

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CHAIRMAN ROGERS: Or any lower temperature?

MR. HARDY: Well, that is extremely hypothetical. We didn't discuss any lower temperature. There is bound to be some lower temperature.

CHAIRMAN ROGERS: Well, it might have gotten colder overnight?

MR. HARDY: No, no lower temperature within the general range of predicted and expected temperature.

CHAIRMAN ROGERS: I think the engineers, though, at Thiokol expressed concern, as you heard yesterday, on the question of the weather, and they felt that the cold weather might have an adverse effect on the joints, and therefore they recommended against launch, as I understood it. And you are saying as far as you were concerned, it really didn't play a role. It wasn't a factor in your consideration.

MR. HARDY: Well, let me say it this way. The key factor, the key area of concern expressed concerning the effect of temperature on the joint, the key area of concern expressed was the potential for increased time, the duration of blow-by on the primary O-ring, and that and its effect on the timing function of the seal of the joint, and addressing that specific key area of concern and the evaluation of the data for that, I came to the

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conclusion in my mind that temperatures at the levels that we were talking about was not dominant in the functioning of that joint, and therefore there was no increased flight risk.

Now, in spite of the fact that I came to that conclusion, and in spite of the fact that I conferred with several of my key advisors who expressed no different opinion, I was fully prepared, and so stated, and frankly—well, I was fully prepared and so stated that I would accept the recommendation of Thiokol, or, the opposite of that, I would not go against the recommendation of Thiokol. I think that these discussions occur frequently between NASA engineers and contractor engineers. We find many areas of agreement and frequently areas of disagreement, and I am relating to safety of flight issues, too.

CHAIRMAN ROGERS: Well, I think we all understand that part of it. I think the more difficult part to understand is the fact that you had a no-launch recommendation, and you have already testified that you can't remember any time when you have had a no-launch recommendation and then had a discussion with the contractors and they changed their minds on something of such key importance on the night before the launch. So it does seem that it is not typical. You said in

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your comments you thought this was sort of a typical situation. I wouldn't think, based upon the testimony, that it is typical. It is quite unusual.

MR. HARDY: I meant to say that the discussion over the data and trying to understand the data, and determine—

CHAIRMAN ROGERS: But it wasn't just a discussion about the data. It was a discussion about the no-launch recommendation. This wasn't just an intellectual exercise. Your statement about being appalled and Mr. Mulloy's about April and so forth all gave the Thiokol people the idea that you strongly disagreed with them on the no-launch recommendation, and that is why they wanted the recess for five minutes, and that is what they did. But it is not just a discussion among engineers about the theoretical matter; it was a very practical question, were you going to launch the next day or not?

And I think that is the problem that I am having with it.

MR. HARDY: Well, maybe the point that is missing is that on recommendations on any matter, regardless of what position that recommendation takes, it is typical and traditional to ask first of all that we don't just do things with people dropping letters in

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to each other; a recommendation is made. The question is asked, or it doesn't have to be asked because they know it is going to be asked, what is your rationale for that. And the next step usually is, well, my rationale is based on this data, and the next question is, well, how do you interpret that data and on what basis do you interpret that data?

DR. WALKER: Mr. Hardy, the data which was discussed by Mr. Thompson yesterday in which they tried to simulate the separation of the metal parts which were holding the O-ring, and Mr. Thompson discussed trying to increase the rate of separation to better simulate the profile which occurred during the launch, was that data presented during the telecon?

MR. HARDY: If you mean the data where tests were run to try to measure the joint separation with pressure—

DR. WALKER: And how the O-ring responded to that.

MR. HARDY: Resiliency data was presented.

DR. WALKER: That's the data I'm talking about.

MR. HARDY: Yes, it was.

DR. WALKER: Was that data at all convincing to you in regard to the sensitivity of the performance

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of the joint with respect to temperature?

MR. HARDY: In the time period and the function of the seals that we were discussing, in the initial phase, transient phase of the pressurize, no.

DR. WALKER: But this data, of course, applied to the rotation period which came later?

MR. HARDY: That is correct.

DR. WALKER: What about that period? Did you think that possibly even if you had a seal during the ignition phase, that the rotation might have broken that seal and the resiliency of the O-ring been insufficient to remake that seal immediately either for the primary or the secondary O-ring?

MR. HARDY: Dr. Walker, if I understand what you are saying—and let me say first of all that the blow-by of the primary O-ring by definition occurs when pressure gets to the primary O-ring. There is no phenomenon that I know of that will allow pressure to hang around somewhere in front of that primary O-ring and not do one of two things, and that is either pressure actuate its seal or blow by.

DR. WALKER: But let's suppose that the O-ring has been sealed, whether blow-by occurred or not, and now rotation occurs.

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MR. HARDY: Yes.

DR. WALKER: And so the metal parts which are surrounding the O-ring begin to move apart. The O-ring must follow those in order to maintain the seal, and it seems to me that the data presented by Mr. Thompson addressed the ability of the O-ring to respond.

And what I'm asking is did that raise a concern that even though the orbiter seal might have been made during the initial phases of the launch, that that seal might have been broken later on because the O-ring resiliency had been reduced to the point by low temperature that it could no longer respond to the rotation transient?

MR. HARDY: I understand what you are saying. There was no discussion that I am aware of, nor has there been any concern expressed regarding the seals, that once a seal, like a primary seal, once it seals, there has been no concern of any magnitude that I am aware of that it would subsequently fail.

DR. WALKER: I guess I am confused now because I thought that that was one of the concerns that Mr. Boisjoly and Mr. Thompson had.

MR. HARDY: It is my understanding that Mr. Boisjoly's and Mr. Thompson's concern, at least on the evening that we discussed this, was that the cold

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temperature could cause the blow-by. Well, first of all, you can get blow-by of the primary O-ring. You can get it any time at any temperature, and that has been recognized, and as I said, this position has come up in flight readiness reviews time and time again. The cold temperature might sustain that blow-by for a longer period of time. I mean, it may be longer, and as someone had mentioned to you, the longer it is, the more erosion, if you get any erosion, that you will see on the primary O-ring, and that if somehow—and I could never figure out in my mind how this happens, but if somehow that while that blow-by is occurring on the primary O-ring, the secondary seal, presumably it doesn't seal anywhere, but the secondary seal is not pressure actuated by that blow-by, then the joint could rotate, and indeed, both joint rotation and resiliency would then have an adverse effect on the secondary O-ring sealing.

But the difficulty I have with the application of that data, there is no question about the fact that temperature affects the resiliency of the material. But the difficulty I had with the application of that data was in the period of the performance of the seal that we were discussing, and the issue we were discussing about sustained blow-by or a longer period blow-by on the

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primary, and in fact it was that that prompted my statement that I was appalled.

DR. WALKER: Of course, emphasize the question of the first few hundred milliseconds when the seal is formed, but the seal, of course, has to maintain itself throughout the period of firing the motor which is some 100 seconds or so.

MR. HARDY: That is correct, and that is correct on any seal at any temperature, and it has been recognized and documented that that secondary seal, at any temperature, may in fact after motor pressurization, after joint rotation, may not be redundant, and a waiver has been processed to that effect.

So in that particular case one has to recognize the fact that that could happen, whether he is talking about cold O-rings or hot O-rings or whatever.

DR. WALKER: But evidently that was not an issue during this teleconference?

MR. HARDY: No, sir. The issue of whether or not the secondary seal is still good after joint rotation, not to my knowledge. We were talking about the initial ignition phase and the potential for an effect on the timing function between the primary and the secondary O-ring.

DR. WALKER: I don't want to get ahead of your presentation, so if this question is too premature for

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you, answer it later, but I hope that you will tell us what you think are the controlling factors which determine whether blow-by will occur or not. Evidently you think temperature is not one of the major factors.

MR. HARDY: Okay.

Well, let me try that now. I think it is, as I mentioned, during the transient period when the O-ring is sliding, and maybe even a certain portion of that O-ring is rolling or—well, you really can't roll it. We are talking 20/1000 to 30/1000 of travel, so you can't roll the O-ring, but tending to roll or twist a little bit, that while that pressurization is going on, then there are some—there is a possibility of gas going by. I think it has to do with the finish on the sealing surfaces themselves, them passing from one finish to the other.

DR. WALKER: That is imperfections in the sealing surfaces?

MR. HARDY: And small, slight imperfections in the O-ring itself. There is quite a difference between the way the O-ring was sealed, as I am sure you are aware, when I am sealing between two surfaces with squeeze or sliding that way as opposed to how it would seal when I extrude it into a gap that is much, much smaller than the O-ring.

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So I think when it is across those two sealing surfaces, that it experiences this blow-by.

DR. WALKER: Could the treatment of the joints be a contributing factor, imperfect joints?

MR. HARDY: The amount of squeeze could be a factor.

DR. WALKER: No, I meant the—let me be a little more careful. The joints in the O-ring, the five joints in the O-ring.

MR. HARDY: Oh, yes. Well, I say oh, yes, yes, that could be a factor.

DR. WALKER: Are those inspected, each joint?

MR. HARDY: To my knowledge, they are.

MR. ACHESON: Mr. Mulloy, have you ever seen an O-ring in which the vulcanized ends had come apart in a motor which was recovered from a flight?

MR. MULLOY: No, sir. I have not, and none have been reported to me.

CHAIRMAN ROGERS: Mr. Hardy, going back to the process itself, the decisionmaking process—and I think that is vitally important because we have to make recommendations to the President about the future, and I think all of us feel that there has been a breakdown of sorts in the process—you said that you would not have

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approved of a launch if Thiokol had recommended against it.

That is correct, isn't it?

MR. HARDY: Yes, sir.

CHAIRMAN ROGERS: Now, suppose that Mr. Kilminster had said to you I am sending the telefax, and this represents management's decision, and there are three or four of us, but all the engineers are still opposed to the launch, how would you have reacted to that?

MR. HARDY: I would not have accepted it.

CHAIRMAN ROGERS: Thank you.

MR. HOTZ: Mr. Hardy, I would like to pursue the Chairman's line of inquiry here, if I may. You have characterized the tone of these discussions as normal engineering discussions, the type of thing that goes on all the time. Yet one of the Thiokol people at Marshall told us that you had spoken as strongly as he had ever heard you speak in that telecon, and I would like to ask you whether you in fact apologized to any of the Thiokol people the next day for speaking so harshly?

MR. HARDY: No, sir, I have no recollection at all of apologizing for anybody speaking harshly. I don't think I spoke harshly. I certainly did not speak in any way harshly toward any individual. I may have

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spoken in a tone that someone my have interpreted as coming on strong. I would suggest if you have continued interest in that that there is a large number of people that you might have access to that could give you a little better insight into my mode of operation and how I conduct myself in meetings of that sort, and then maybe from that you could determine whether I could be properly characterized as coming on strong in that meeting.

MR. HOTZ: We will do that, thank you.

DR. RIDE: Could I just pursue this question of sort of implied pressure or whether Thiokol felt pressure from your comments?

You have said that in engineering discussions you always rightly try and probe the data and make sure that the conclusions follow from the data, and that the data is well presented, and I think in Mr. Mulloy's terms, hangs together.

Normally, contractors are in the position of trying to prove to both of you that they are ready to fly, that their systems are safe to fly, and they are used to having to defend that point of view.

This time they were in the other position. They were so concerned that they in fact recommended not to launch, and I think that you might argue that they were so used to hearing you say are you sure it is safe

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that when you both said are you sure it is not safe, that they were so taken aback that they perceived that as pressure. And I guess I am wondering whether it is possible that you didn't realize that you could have been generating this reaction just because of the reversal of their normal position relative to you.

MR. HARDY: Well, of course, first of all, I don't think you intended to imply this, but we didn't ask them are you sure it's not safe? There was no discussion that I'm aware of where that question was asked like that.

Now, as to whether or not someone else could perceive a line of questioning to try to understand the data and in fact probed the data and in fact challenged certain points on the data, but

I maintain you are not really going to understand it sometimes unless you challenge it, and then I find that—I have found that in most cases engineers, managers or whatever else who have a true conviction in the data that they are presenting to you, will hang tough and not resent someone probing and penetrating that data.

But as to whether or not someone else could interpret that situation as being pressure or being a reverse normal operation or whatever, I couldn't judge that. I would hope not. I would certainly hope not because I

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would like to think that there is a clear understanding of what we are about when we get into discussions like that, and I would like to think that without exception there is a clear understanding that nobody is asking anybody to accept or do anything that has any significant effect on safety of flight, and I have been in this business too long to imagine, I can't imagine who could pressure me in what way to get me to accept some, any significant increase or any increase in safety of flight. I can't imagine who could do that.

DR. RIDE: Well, the indications from the testimony we heard yesterday were that the engineers did, in your terminology, hang tough, that the engineers at Thiokol were still arguing the points that they had been arguing at the beginning.

MR. HARDY: Well, I am not aware of that part of the discussion or the facts that took place.

CHAIRMAN ROGERS: Mr. Armstrong?

VICE CHAIRMAN ARMSTRONG: We heard yesterday what the various points of view and how many people expressed those points at the Utah end. Were you at Marshall?

MR. HARDY: I was at Marshall, yes.

VICE CHAIRMAN ARMSTRONG: Could you characterize for us how many people were involved in the

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meeting at Huntsville? And you have already testified that Mr. Powers expressed some support of the contractor view.

Were there others, and could you say there was a split in opinion among the people at Marshall, please?

MR. HARDY: Well, let me say first, I can't give you the exact number of people, but certainly I think you have the list, but I would guess there were probably 20 people, maybe somewhere in that neighborhood, maybe not quite that many.

Okay, was it 14? I didn't have the count, 14 or 15 people. There was discussion by a number of people, I don't know who or how many right now, during the course of the meeting, asking questions about the data, or asking questions about this test, how many tests were run, was that one data point or was more than that run, how do you interpret that, or do I understand you interpret that this way and things of that nature.

There were a number of people participating during the course of the conversation. I mentioned the fact that I had learned since 51-L that Mr. Powers had made the comments that he had made, not to me, and not during the teleconference when it was in process, but as stated to me by Mr. McCarty, his supervisor, during a side

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meeting. I did not detect any strong dissent.

Well, let me be careful about this because I want to say it right. I did not detect any dissent when Thiokol came back with their final recommendation, and in fact, as I have stated, I didn't talk to everybody in the room. Dr. Lovingood, who was there, is a senior program official. I don't believe he talked to everybody in the room, but had I had—maybe the best way to answer the question is to say that if I had been conscious of—and I am quite confident that this was nonexistent but if I had been conscious of the fact that there were 20 percent or 25 percent or anything like that of opinions that were contrary to the recommendation that was made and the discussion of that data, I would have pursued that with the individuals involved.

VICE CHAIRMAN ARMSTRONG: Going back not to the final decision but at the time of the early recommendation of Thiokol not to launch, were there expressions of support for their position at that point, or can you characterize that in any way?

MR. HARDY: I don't recall that there were any expressions of support or any significant discussion on it.

Well, let me put it this way. If there were

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any expressions of support other than mine wherein I said I will not go against the contractor's objections, and I don't know how anybody else interprets that, but if I was on the other end of the loop I would have interpreted that as support, regardless of what had transpired prior to that time, but I don't believe there are any expressions of support or at least that I heard or was conveyed to me.

MR. ACHESON: A brief question, Mr. Hardy, going to a minor, or maybe not so minor conflict in the testimony. Your testimony appears to be that you used the word "appalled" in connection with the data that had been presented during the teleconference. Mr. McDonald's testimony of yesterday and I think his notes say that your words were "I am appalled at your recommendation."

MR. HARDY: Well, I don't recall that I stated I'm appalled at your recommendation. I do recall that when I made that statement, it was in the timeframe that they made the recommendation, but I also clearly know in my mind and I have testified to that fact, on what basis I made that statement, and again reiterate the fact that I would not recommend launch over the contractor's objection. And I have worked with these individuals for some time, and I think that they knew

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clearly that I would not—that I meant what I said regarding what I would do.

CHAIRMAN ROGERS: We would like to declare a recess in just a moment, but before we do, I would like to ask one or two questions on the subject of pressure.

Has there been any pressure from any source against, on either of you? In other words, has anybody urged you to get this launch off, or has there been any intercession on the part of anybody asking you to be sure that you worked hard to get the launch off or anything of that kind at all?

MR. MULLOY: No, sir.

MR. HARDY: None whatsoever, as far as I am concerned.

CHAIRMAN ROGERS: No outside interference or attempt to put pressure on you at all?

MR. HARDY: No, sir.

MR. MULLOY: No, sir, quite the contrary.

CHAIRMAN ROGERS: Thank you very much.

We will come back at about five minutes after 2:00.

(Whereupon, at 1:10 o'clock p.m., the Commission recessed, to reconvene at 2:00 o'clock p.m., this same day.)

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AFTERNOON SESSION

(2:10 p.m.)

CHAIRMAN ROGERS: Will the Commission come to order, please?

Mr. Hardy, would you proceed, please?

TESTIMONY OF GEORGE HARDY AND LARRY MULLOY—Resumed

MR. HARDY: Yes, sir.

I believe I was down to the point where I had started providing you with my assessment of the data as presented, and first of all, I reviewed in my mind during the course of the discussion and at the caucus and in some discussion with some of my key advisors the question about blow-by, and I mentioned that O-ring blow-by was experienced on 61-A and 51-C, and also not experienced on motors at somewhat lower temperature.

I also mentioned that I think there were a number of things that probably play into the equation for blow-by, but I did not believe that temperature was an inducement factor.

I thought that temperature could potentially affect the actuation time and therefore the time of blow-by. The experience base and data on the primary O-ring erosion of the case joint was also reviewed in this meeting, and the maximum erosion that had ever been experienced on a primary O-ring was at a joint with a

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temperature of approximately 70 degrees. Thiokol had data that showed that subscale hot fire tests had demonstrated the capability of the primary O-ring to sustain erosion of .125 inches and maintain pressure with no leakage, and this was the reference I made to the fact that that is approximately two times the factor that has been calculated as to be the maximum erosion that could occur in the time that is available to equalize the pressure, and that is a factor of approximately three times that experienced on 51-C.

Now, these analyses were made some time ago, and they were reported to us at Marshall in a briefing on August 19, 1986. I was not in that briefing, but I did have that data that was presented. So my conclusions relative to blow-by was that blow-by on the primary O-ring may occur on 51-L because it had occurred on joints that were at ranges all the way from 75 to even below 50 degrees, and it also had not occurred on most occasions at temperatures even over a wider span than that.

But obviously, one had to be prepared in his assessment of the data there that blow-by could occur.

I also assessed and in effect agreed with the Thiokol engineers that the duration of that blow-by on the primary O-ring could be longer than the normal that

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we have seen, and I think that the evidence for that was the amount of blow-by that was seen on 51-C.

With respect to the primary O-ring erosion, well, let me just say one other thing. As I mentioned earlier this morning, in my mind the duration of primary O-ring blow-by was not a key issue. The key issue was, as we stated several times, the existence of the secondary O-ring at that point in the ignition transient to accept that blow-by and in the event that it was not terminated by the primary O-ring actuating and sealing, it would be terminated by pressurizing the secondary O-ring.

DR. RIDE: Can I ask you a quick question about that?

You say that if you get blow-by past the primary O-ring, that because of the pressure check, the secondary will be in a position to seal, and so you won't get blow-by past the secondary O-ring.

Let me just ask you about the black puff of smoke that we saw.

Do you think that a reasonable interpretation of that is that you got blow-by past both O-rings?

MR. HARDY: I think that is one of possibly two or three interpretations.

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DR. RIDE: I guess the reason that I bring that up is that it apparently is conceivable that you could have blow-by past the secondary O-ring, and if you do, then the timing function becomes very important because then you would have the primary O-ring eroding, the erosion wouldn't stop because the secondary had sealed, but the primary could keep eroding possibly to a dangerous point.

MR. HARDY: Well, it was, I think, clearly recognized by everybody involved that the secondary seal has to be prepared to be sealed if you get blow-by the primary O-ring, and it sustains itself. So I don't take issue with what you said there.

But the secondary O-ring, of course, doesn't have to pressure actuate. All of our evidence to date is that the blow-by occurs during the pressure actuation of the primary seal. The primary seal does have to pressure actuate, and the secondary seal doesn't have to pressure actuate, and as the pressure builds up on the secondary seal, it starts—its first function is to extrude into the gap. That is its first function. The primary seal's first function is to pressure actuate and then extrude into the gap.

So the occasion for blow-by on the secondary O-ring, in my opinion, would be extremely nil or maybe

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not even possible.

Now, if there was a defective secondary O-ring or something of that nature, then obviously the primary O-ring blow-by could be substained right on past the secondary O-ring, and that would describe a problem.

CHAIRMAN ROGERS: As I understand, your answer is that the puff of smoke had occurred before the first second had elapsed, might be an indication the secondary O-ring had failed?

MR. HARDY: Yes, sir. What I meant to say is that there are conditions whereby the secondary O-ring could fail to hold that pressure when I get blow-by by the primary O-ring. I do not believe one of those conditions is blow-by the secondary O-ring in the same context that we described that with the primary O-ring, but there are conditions whereby the secondary seal would not seal, and in fact, result in a puff of smoke.

There are others, I might hasten to say, there are other possibilities for that same thing.

CHAIRMAN ROGERS: But that is one of them, though?

MR. HARDY: Yes.

MR. HOTZ: Could you describe those other conditions where the secondary O-ring might not seal?

MR. HARDY: A defective O-ring, a defective

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O-ring sealing surface, and when I say a defective O-ring, I can think of more than one type of defect.

MR. HOTZ: How about displacement?

MR. HARDY: Well, if the pressure for some reason doesn't get to the primary O-ring and when it does, let me say if the pressure is delayed in getting to the primary O-ring, and I mean delayed in terms of a few hundred milliseconds, and then when it does arrive at the primary O-ring, the primary O-ring sustains blow-by, under that condition the secondary seal could be in a position where it would not seal. It would depend upon how long in the ignition transient or the reverse of that, how early in the ignition transient that occurred.

But all of our assessment on blow-by to date had indicated that pressure gets to the primary O-ring very early. It pressure actuates or starts pressure actuating at a relatively low pressure. I think it is 25, 35 psi, and when blow-by occurs, that is when it occurs, and at that position, the secondary O-ring would be in a position to seal.

DR. COVERT: Mr. Hardy, with regard to your comment that there is a pressure delay for several hundred milliseconds, it is my understanding that the putty is a plastic material and therefore it cannot

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withstand pressure loads of any kind.

What kind of a physical phenomenon would give rise to a delay in the pressurization of that volume between the end of the putty and the primary O-ring?

MR. HARDY: Well, I think if for whatever reason the putty kept the pressure off the primary O-ring for several hundred milliseconds, and I might just add at this time that the potential for that to occur was not discussed in the meeting of January 27th. Subsequent to that time, in fact, I would say the first or second day of the failure investigation, I raised that issue, or I asked at that time, we were just starting to formulate failure scenarios, and I asked if there was any potential that the putty might delay the pressure actuation of the primary O-ring. I believe it was Mr. Boisjoly who was with me, and several other people at that time, and he said, or words to the effect that within his knowledge of how that joint pressurizes, he doesn't believe there's any delay at all in pressurizing the primary O-ring by virtue of the putty.

DR. COVERT: If the putty were at 29 degrees, would it possibly cease to be a relatively plastic material and be a brittle material that could for a while carry the pressure load?

MR. HARDY: Dr. Covert, I can't answer that in

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detail. I can tell you that we are intensively investigating the role of that putty at various temperatures in the pressure sequence. There are several things involved there. One is that the structural capability will, if you will, of the putty itself, there is also the fact that there are fibrous materials in the putty which could provide porous type leak paths, and the other thing is that there are dynamics in the joint immediately when, at very low pressures, tending to separate the putty or at least providing a force to try to separate the putty from the rubber.

DR. COVERT: I think let's not go further with this and wait until some of this other technical data becomes available.

Thank you, Mr. Hardy.

MR. HARDY: The second item of discussion was the primary O-ring erosion, and I believe I have pretty well covered that, the fact being that yes, we had to be prepared for primary O-ring erosion on 51-L, as we had to be prepared for that on any flight. There was no evidence of temperature affecting erosion. Erosion phenomenon occurs in one of two ways. One is that the erosion would occur from a concentrated jet, a concentrated jet

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of hot gas that is imposed by virtue of a hole that is formed in the putty when the putty gets pressurized.

Now, the reason for the hole could be many things, but not the least of which would be the variability in the way it is laid up every time, and not the least of which would be the effects of moisture, because we do know that the putty is sticky and less pliable when it is wet. But that issue had been addressed in great detail in a number of previous flight readiness reviews at all levels. Analysis had been done to show that the limiting factor again was the time it takes to equalize the pressure across that jet, the source pressure, which is the motor pressure, and the pressure just downstream of the jet, in front of the primary O-ring. And these were the numbers I mentioned to you earlier that calculated maximum theoretical erosion that you could experience was around 65/1000. That was confirmed in tests, and then there was a test to show that approximately twice of that was tolerable.

The other way to get erosion on the primary seal is the hot gas that blows by when this phenomenon of blow-by occurs.

Now, I think you understand erosion and blow-by don't occur at the same time. They do occur separately. They sometimes occur at the same time even

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though the phenomena that causes them is different. But in this particular case, again, it would be due to sustained blow-by the primary O-ring, at which time the secondary O-ring is in a position to seal.

The other subject that was discussed and material that was presented had to do with O-ring resiliency. The data showed indeed that there was a decreased response rate of the O-rings with decreased temperature, and by that I mean if you compress the O-ring, it takes it longer to come back to its original position with temperature, and I think that is fairly standard data and was of no great surprise.

The data presented, however, coupled with the O-ring erosion and blow-by experience, did not indicate that resiliency was a dominant factor, and I emphasize dominant factor, in the early part of the ignition transient.

Now, certainly resiliency is a factor in that portion of the ignition transient where the joint has started to rotate any significant amount. The data that would plot a curve from those three data points, there was one at 100 degrees Fahrenheit, one at 75 degrees Fahrenheit, and one at 50 degrees Fahrenheit, would show that immediate response of the O-ring from compression set would not occur at temperatures below 70, 75, 80

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degrees. It was the application of that—well, let me go on one step further—which again reinforced my belief that in the portion of the ignition transient that we were interested in, that is, that part where we might sustain longer blow-by or extended blow-by of the primary, resiliency was not a dominant factor, and I think that is substantiated in the experience base that we had.

To make the point again, the primary O-ring actuates and seals early in the ignition transient phase, about 25 or 30 psi, and I know no better way to describe this than to say that when that pressure gets to the primary O-ring, it either seals or it doesn't seal. If it seals, I've got nothing else to be concerned about. If it doesn't seal, what I have got to be concerned about is the secondary O-ring. But at the same time I am getting blow-by on the primary O-ring, I am pressure actuating the secondary O-ring.

Data was presented relative to O-ring hardness and temperature, and while hardness is affected by temperature on the O-rings, in fact, two data points, one that shows a durometer, or a measure of hardness, at 84 at 50 degrees Fahrenheit and 92 at 30 degrees Fahrenheit, but there are two significant data points with respect to "harder O-rings." One was a test that

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had been conducted by Thiokol. The question here is will an O-ring extrude into the gap? Will it seal? Once it gets harder, will it have a higher resistance to sealing in this extrusion gap? And if it does, will that have any effect on the sealing time or any significant effect on the sealing time?

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Tests were run at Thiokol that showed that on more than one test, in fact all the tests—I have forgotten exactly how many it was, but the tests showed that at 30 degrees the seal would seal without leakage, and then beyond that there had been a durometer of 90 O-rings were used earlier in the program. They were used early in the test program, and they were used early in the program to do hydroproof tests on segments.

I concluded that the effects of changes in O-ring resiliency and hardness were not dominant factors in the early part of the ignition transient, since any violation of the primary O-ring is expected to occur early in the ignition transient, while the secondary O-ring remains in a squeezed position, seated by the leak test pressure, and ready to seal.

MR. SUTTER: Do you think that 28 days later that that secondary O-ring is still in the sealed position?

MR. HARDY: Yes, absolutely.

MR. SUTTER: Do you have any data on that?

MR. HARDY: Well, I don't have the data here with me, but there is data on the stiffness of that joint, the characteristics of the joint under various types of loads, everything from on-pad loads to the loads that it experiences when the SSMEs fire up and

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push it over, to the pressure loads.

And without question, the pressure load, of course, is the biggest load that that joint ever sees.

MR. SUTTER: But the thing is sitting there for 28 days, and there are variations in temperature which makes the vehicle work some, and there is nothing to hold it in position, except it has been pushed in position.

But if something tries to pry it out around that great big circumference, it would seem to me there might be a chance that at least parts of it would pop out.

MR. HARDY: Well, of course, it is in a groove, as you know, and it is sitting there under those conditions with about, in this case, 35, 36-thousandths compression.

MR. SUTTER: Except that if it gets colder it might shrink, and that reduces.

MR. HARDY: As it get colder, that 38-thousandths would come down to 35-thousandths.

MR. SUTTER: I have a little bit of trouble. This run-through of data that you're presenting and your analysis of it sounds like everything is copacetic, and yesterday I listened to these other fellows who showed great concerns.

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And it is hard for me to understand two different groups of engineering with the same data, one drawing a very great concern, a critical concern, and the other one being fairly relaxed. And that is why I keep asking the question of these two groups, who is the group that is the group that makes a recommendation to the decisionmakers to make the proper decision?

I'm still very confused on that.

MR. HARDY: Let me just clarify that for you a little bit. What—the responsibility of myself and engineers that work in science and engineering is to review, assess, probe, analyze data that is put forth by the contractor engineering.

MR. SUTTER: And you draw a different conclusion than they draw?

MR. HARDY: That is correct.

MR. SUTTER: So then when they advise their management as to go or no go, it really isn't a very strong input, and when their management says go or no go it may be a decision that doesn't have to be paid attention to?

MR. HARDY: Well, as I was going to say, we have interface with that engineering department and our engineers do. We don't make recommendations to their management. Their engineering doesn't make

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recommendations to our management. Their engineering makes recommendations to the contractor management and then we make recommendations to our project management.

Now, what the project manager is then assessing is the contractor's input to him and his civil service engineering input. That is what he ends up assessing.

MR. SUTTER: And I can tell you that if I were that guy I would have a hell of a time knowing what to do.

MR. HARDY: Well, I don't know of any case—I don't know of many cases—there may have been some, but I don't know of many cases where he has just overridden or ignored the civil service engineering input that he has had.

MR. SUTTER: Then it gets back to the fact that the engineers at Thiokol, who designed the thing, ran the tests, and did some interpreting of it, are a nonentity. They don't count.

MR. HARDY: Well, of course, to us the contractor is the entity. We don't cut the contractor up into departments.

CHAIRMAN ROGERS: Following up on the question, aren't you and Mr. Mulloy still worlds apart from the views of the engineers at Thiokol?

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MR. HARDY: I would say today that, based upon what I heard yesterday, that yes, we have—or I have some significant difference. Mr. Mulloy would have to speak for himself.

CHAIRMAN ROGERS: Okay. Are you about finished?

MR. HARDY: Yes, sir, I think I have finished.

DR. COVERT: Mr. Chairman, may I ask one more question, please?

Mr. Hardy, this assembly was 28 days in a vertical stack and these O-rings were squeezed 14 or 15 percent all these 28 days.

MR. HARDY: That's my understanding.

DR. COVERT: Is the characteristic of this elastomeric material such that it is going to tend to come to equilibrium with the fact that it has been—I don't know, what's the right word—squoze that long? So that could this contribute to a lack of resilience and the ability to slow up the spring-back?

MR. HARDY: I believe that these compressions, one could have a higher, an extremely high compression where that could be more of a problem. But at these compressions, we have had vehicles sitting on the pad for—I can remember, well, we have had them stacked,

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we have had the motors stacked for in excess of a year and have subsequently satisfactorily tested them.

DR. COVERT: Thank you.

MR. RUMMEL: If I could ask a question for clarification, from either Mr. Hardy or Mr. Mulloy or both, with respect to the capture ring development. As I understand it, it was said this morning that that was sent to production, or at least billets were ordered last July.

Now, that is a fairly major change, since it affects the forging and machining, et cetera, et cetera, in the normal course of production. My first question is, when was that decision made to order it to production?

I note in passing that it has already been incorporated in the filament-wound case, and of course the obvious purpose of it is to keep the joint from separating. Was it when you first discovered separation, and when was the capture ring ordered for the steel case?

MR. MULLOY: Yes, sir, the filament-wound case design was done about three years ago. That was subsequent to the time that the measurements had been taken on the structural test article, on the steel case, that did show that, contrary to Thiokol's calculations, that when the pressure load was put on the case the

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joint opened instead of closed.

The original calculation showed that it would close. When we got the filament-wound case designed, it is about 1.6 inch thick composite, which puts a bigger eccentricity into that joint, if you will. So when that is pressurized and it's less flexible, it causes even more bending, and so it was determined that it was an unacceptable rotation on the filament-wound case.

Now, in this activity that began in looking at near-term, mid-term and long-term solutions that some of the Thiokol personnel described yesterday, one of the "long-term" improvements was to incorporate that same latch or capture feature on that inboard leg of the clevis on the steel case.

Even before the August 19th meeting here at headquarters, the discussions had evolved to the point where my element project manager, Mr. Wear, deemed it prudent to go ahead and order the forgings with an additional three inches on the ID such that when we designed and did the stress analysis and got the design complete, we would be in a position to machine that capture feature in for steel cases.

Now, you only need that on the field joints, because there is no problem with the factory joints because they have the insulation over that.

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We're in a position to start getting those segments in, after we go through the design review on those, in the fall of this year.

MR. RUMMEL: It seems to me a substantial difference or a large interval in time, if I understand this correctly, between the availability of the capture joint in the steel case and the availability in the fabricated wound case.

MR. MULLOY: Oh, yes, sir. It was incorporated initially in the design in the wound case, which is just—it is just a ring that goes onto the composite. And it just wasn't decided until July of 1985 that we wanted to protect the option for being able to put that on as a long-term improvement.

And it is about a year and a half lead time from the time you get a forging until you get a part.

MR. RUMMEL: Well, I guess the problem I'm having is it seems apparent to me, as we were hearing the discussion yesterday and today, that considerable concern has existed with respect to maintaining the seal in the O-rings for a very extended period of time. And I am puzzled as to why, if that's so, that is compatible with what may seem to be a delay in the decision to incorporate the capture ring in the steel case.

Do you care to comment on that?

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MR. MULLOY: Yes, sir. There was a great deal of testing, as we have testified, starting in mid-1984, going through 1985, with a dedicated effort at trying to isolate the variables, if you will, that tend to increase erosion and primary O-ring blow-by.

There were a lot of concepts evaluated. The capture feature was one of those. There was—the intense effort was on the near-term solution, because there is such a long lead time on that, and it just was not concluded—and I'm not offering any excuses for that, but it just was not concluded until July, when everything kind of came together that said, what we need to do is put a .292 diameter O-ring in the field joint, put a spacer in the primary O-ring on the nozzle joint, and incorporate a capture feature as soon as possible.

And Thiokol made that recommendation at the August 19th briefing here at headquarters, and we had already moved out in advance of that.

MR. RUMMEL: Could that have represented an effort to use up production on hand without the capture ring, for example, prior to moving into the capture ring situation?

MR. MULLOY: No, sir. As I say, we would mix these, the casting segments. There are two segments with each one. So the only one that we need the capture

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feature on is the tang of the field joint. So we would be mixing in the current inventory with this, with the new case segments with the tang incorporated.

And we would continue throughout the program to use the existing inventory of those segments. We don't have to set those aside. We were able to use them without the capture feature.

CHAIRMAN ROGERS: Thank you very much. We will be able to pursue this later on, I'm sure. Thank you very much, Mr. Mulloy. Thank you, Mr. Hardy.

Now, Mr. Lovingood and Mr. Reinartz.

(Witnesses sworn.)

STATEMENT TO THE
PRESIDENTIAL COMMISSION
ON THE
SPACE SHUTTLE CHALLENGER ACCIDENT

G. B. HARDY
FEBRUARY 25, 1986

[Ref. 2/26-1 1 of 15]

Mr. Chairman - Members of the Commission

I am George Hardy, Deputy Director of the Science and Engineering Directorate at the Marshall Space Flight Center.

Before I relate to you my participation in, and recollections of the teleconference between Morton Thiokol personnel and Marshall Space Flight Center personnel on the evening of January 27, 1986.

- I would like to describe the role of myself and other personnel of the Science and Engineering Directorate with respect to Flight Readiness Assessment for Shuttle flights.

- The primary role of Science and Engineering in this regard is to:

  - Participate in Flight Readiness Reviews
  - Identify any pertinent issues relative to flight readiness
  - Review any issues identified by others (including contractors)
  - Assess the rationale for flight and supporting data for any issue
  - Provide recommendations to the appropriate Readiness Review Board chairman (if such a Board is in session) or otherwise to Marshall Program Management

[Ref. 2/26-1 2 of 15]

With reference to the teleconference that started at approximately 8:45 p.m. EST on January 27, 1986---

The teleconference between Marshall Space Flight personnel and Morton Thiokol personnel at Kennedy, Huntsville and in Utah---you have been given a list of all participants at each location.

I would like to identify the personnel at Huntsville from the Science and Engineering Directorate that were supporting me during the teleconference:

Dr. Wayne Littles (Associate Director for Engineerng)

Mr. Jim Smith (The Solid Rocket Booster Chief Engineer)

Mr. Robert Schwinghamer (Director of our Materials & Processes Laboratory)

Mr. Bill Riehl (an engineer in the Materials & Processes Laboratory - a non-metallics materials expert)

Mr. John McCarty (Dep. Director of our Structures & Propulsion Laboratory)

Mr. Ben Powers (an engineer in the Structures & Propulsion Laboratory - specializing in solid propulsion)

Mr. Keith Coates (in the Office of the Associate Director for Engineering)

[Ref. 2/26-1 3 of 15]

Thiokol engineering personnel in Utah reviewed charts that had been datafaxed to the Huntsville and KSC participants just prior to the beginning of the teleconference.

- The presentations were professional in nature.
- There were numerous questions and answers.
- There was a discussion of various data and points raised by individuals at Thiokol-Utah, at Marshall and at Kennedy.
- A rather full discussion, I would say (Thiokol presented 14 charts and we spent 2 to 2 1/2 hours reviewing and discussing the data and all issues related thereto).
- To my knowledge, anyone who desired to make a point, ask a question, or express a view was in no way restrained from doing so.

[Ref. 2/26-1 4 of 15]

I have heard the teleconference characterized as "heated discussions":

- Penetrating questions were asked
- Various points of view and interpretations of data were exchanged and discussed.
- The discussion was not, in my view, uncharacteristic of discussions of flight readiness issues on previous occasions

[Ref. 2/26-1 5 of 15]

Thiokol Engineering concluded their presentation with a recommendation that the launch time be determined consistent with flight experience to date (launch with the o-ring temperature $\geq$ 53°F).

Mr. Kilminster (Thiokol-Utah) stated that with the engineering assessment he recommended that we not launch Tuesday morning as scheduled.

After a short discussion followed, Mr. Mulloy (at KSC) summarized his assessment of the data and his rationale for flight.

Mr. Reinartz (at KSC) asked me for comments. I stated that I was somewhat appalled (referring to Thiokol's interpretation of some of the data presented with respect to its influence on the joint/seal performance to the issue under discussion). I will discuss this specific data later in this statement. Then I went on to say that I supported the assessment of data presented essentially as summarized by Mr. Mulloy, <u>but I would not recommend launch over Thiokol's objections.</u>

[Ref. 2/26-1 6 of 15]

Somewhere about this time, Mr. Kilminster (Thiokol-Utah) stated that they wanted to go off-the-loop to caucus for about 5 minutes. I believe at this point, Mr. McDonald (The Thiokol Senior Representative at KSC for this launch) suggested to Mr. Kilminster that he consider a point I had made earlier, that the secondary o-ring is in the proper position to seal if blow-by the primary o-ring occurred. To the best of my recollection, that was the only comment made on the loop (i.e., during the teleconference) by Mr. McDonald before or after the Thiokol caucus.

The caucus by Thiokol lasted 30-35 minutes. At Huntsville during the Thiokol caucus we continued to discuss the data presented--we were off the loop--around the table--in small groups. I discussed my assessment and understanding of the data with several of my key advisors. None of them expressed any disagreement or differences.

[Ref. 2-26-1 7 of 15]

When Thiokol came back on line, Mr. Kilminster reviewed rationale that supported proceeding with the launch and so recommended. I had no knowledge of who participated in the caucus at Thiokol. It is not typical to ask for a vote count when a senior/responsible official of the contractor presents the company position. <u>What is typical is to assess the rationale for that position.</u>

During the caucus, I had written my evaluation of the data presented as I developed it in my mind and by the discussion that had ensued. (I will review that with you later in this statement).

Mr. Reinartz asked if anyone (on the loop) had a different position or disagreed with the Thiokol recommendation as presented by Mr. Kilminster. There were no dissenting responses. The telecon was terminated shortly thereafter. I have no knowledge of any subsequent events, or discussions between personnel at KSC or at Thiokol on the matter.

[Ref. 2/26-1 8 of 15]

As I stated earlier, during the Thiokol caucus, I discussed the data and the issues with several of my key advisors and there was no disagreement among us. I also stated earlier, that there were no dissenting responses when Mr. Reinartz ask if there were any disagreements following Mr. Kilminster's recommendation to proceed with the launch.

I have learned since the 51-L incident that Mr. Ben Powers expressed some concern about the o-ring performance directly to Mr. Smith (the SRB Chief Engineer and Mr. McCarty (the Deputy Director of the Structures and Propulsion Laboratory). Mr. McCarty is Mr. Power's supervisor. Mr. McCarty has stated to me that he considered Mr. Power's comments along with all the data presented and discussed when he made his input to me.

[Ref. 2/26-1 9 of 15]

I would like now to give you a summary of my assessment of the data presented and the conclusion I drew from that data.

I interpreted and assessed the data presented by Thiokol in the context of the accepted performance and operational charisteristics of the field joint during the motor ignition transient.

The essential features of these characteristics are:

(1) The primary o-ring moves from the position it has assumed during the leak test to its sealing position as the pressure upstream of the o-ring increases from ambient to approximately 25 to 50 pounds per square inch,

(2) This pressure is imposed on the primary o-ring during the first few milliseconds (100 milliseconds or less) of the ignition transient, and

(3) If a blow-by or failure of the primary o-ring occurs in this timeframe, the secondary o-ring is in its seating position (this is assured by the leak test pressure).

[Ref. 2/26-1 10 of 15]

The issue addressed during the teleconference was the potential for the predicted temperatures to adversely effect the performance of the seals during the ignition transient.

Thiokol engineering presented data on the history of primary o-ring erosion and blow-by at the motor field joints. I concluded that there was no direct correlation with this data and temperature:

- O-ring blow-by was experienced on
    - 51-A with the joint at approximately 75°F and on
    - 51-C with the joint at approximately 50°F
- There was no o-ring blow-by or erosion on two ground test motors at somewhat lower temperatures than 51-C
- The maximum erosion ever experienced on a primary o-ring was with the joint at a temperature of approximately 70°F
- Subscale hotfire tests have demonstrated a capability of the primary o-ring to sustain erosion of 0.125 inches and maintain pressure with no leakage (this is greater than 3 1/2 times the erosion experienced on 51-C)

[Ref. 2/26-1 11 of 15]

<u>Data on o-ring resiliency was presented and discussed</u>

The data showed a decrease response rate of the o-rings with decreased temperatures.

The data presented coupled with o-ring erosion and blow-by history did not indicate that resiliency was a dominant factor in the early part of the ignition transient.

Three data points were presented ---100°F, 75°F and 50°F

- These data would plot a curve that shows that o-rings would not respond immediately to gap opening (due to joint rotation with pressure) if the temperature was below 65 or 70°F.

It was the application and interpretation of these data into our knowledge of the joint sealing characteristics during the ignition transient pressure rise that prompted my statement that I was "somewhat appalled."

[Ref. 2/26-1 12 of 15]

Data was presented and discussed relative to

O-ring hardness vs. temperature

While hardness is effected by the temperature of the o-rings (durometer of 92 at 30°F vs. 84 at 50°F).....

Two significant data points existed to assess the effects of the "harder" o-rings on their sealing capability

(1) Test data was presented by Morton Thiokol that showed successful sealing with no leakage with the o-rings at 30°F

(2) O-rings with a durometer of 90 was successfully used in the early test phases of the program

[Ref. 2/26-1 13 of 15]

My conclusions were:

(1) Blow-by the primary o-ring may occur on 51-L

- It has occurred on flights with joints at approximately 75°F and at approximately 50°F even though it has not occurred on flights and ground test motors with joints over a somewhat wider temperature range

- The duration of blow-by on 51-L may be a few milliseconds longer than 51-C

- The duration of blow-by the primary o-ring is not an issue since it occurs early in the ignition transient while the secondary o-ring is available to seal

[Ref. 2/26-1 14 of 15]

(2) I concluded that the effects of changes in o-ring resiliency and hardness was not a dominant factor in the early part of the ignition transient

- Since any violation of the primary o-ring is expected to occur early in the ignition transient while the secondary o-ring remains in a squeezed position, seated by the leak test pressure, ready to seal...

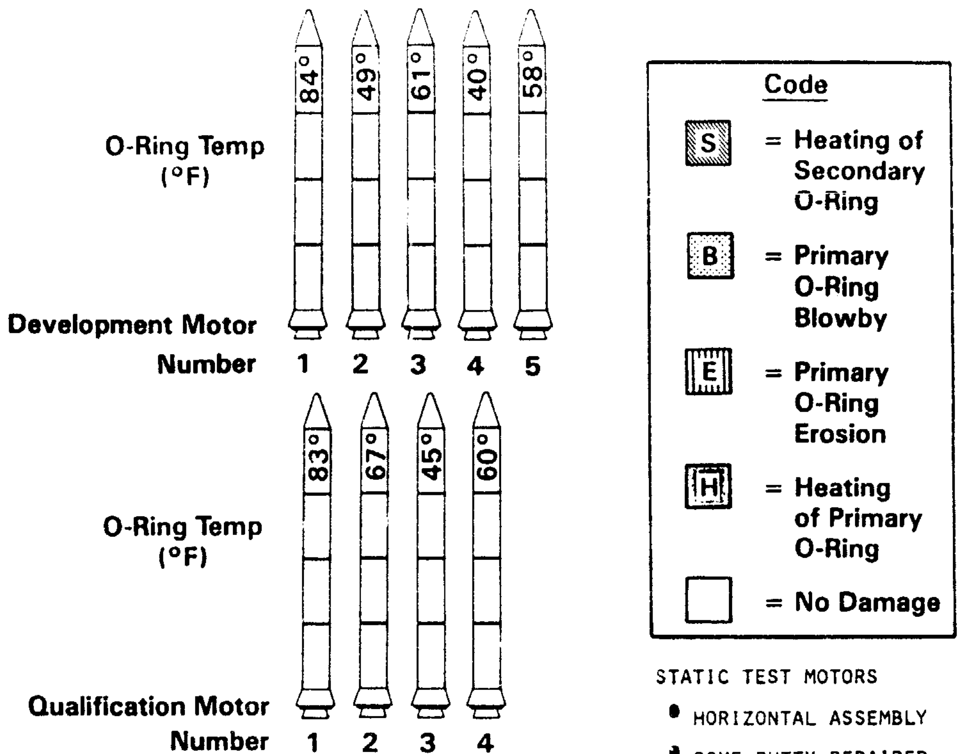
  and further...

- Tests have demonstrated the capability of the o-rings to seal at 30°F

With my assessment of the data and with inputs from my key advisors on the evening of January 27, I supported Morton Thiokol's recommendation to proceed with the launch.

[Ref. 2/26-1 15 of 15]

History of O-Ring Damage in Field Joints



STATIC TEST MOTORS

- HORIZONTAL ASSEMBLY
- SOME PUTTY REPAIRED

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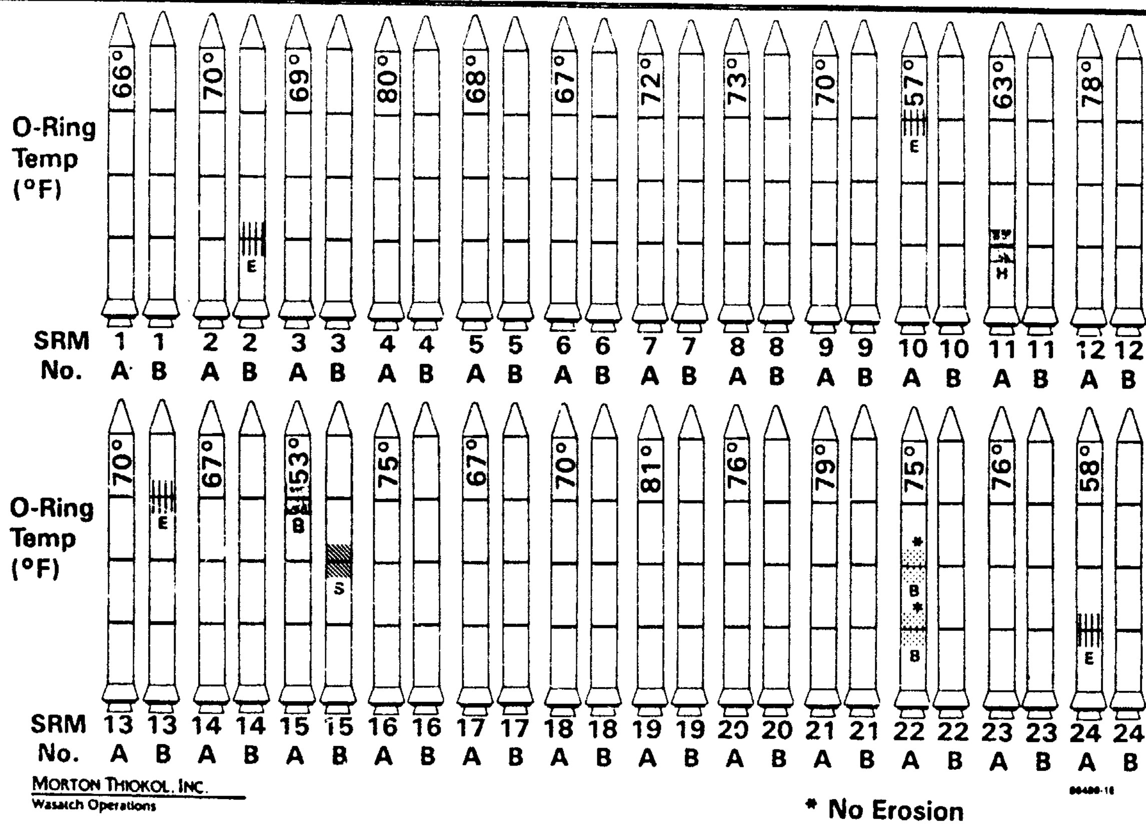
INFORMATION ON THIS PAGE WAS PREPARED TO SUPPORT AN ORAL PRESENTATION AND CANNOT BE CONSIDERED COMPLETE WITHOUT THE ORAL DISCUSSION

[Ref. 2/26-2 1 of 3]

History of O-Ring Damage in Field Joints (Cont)



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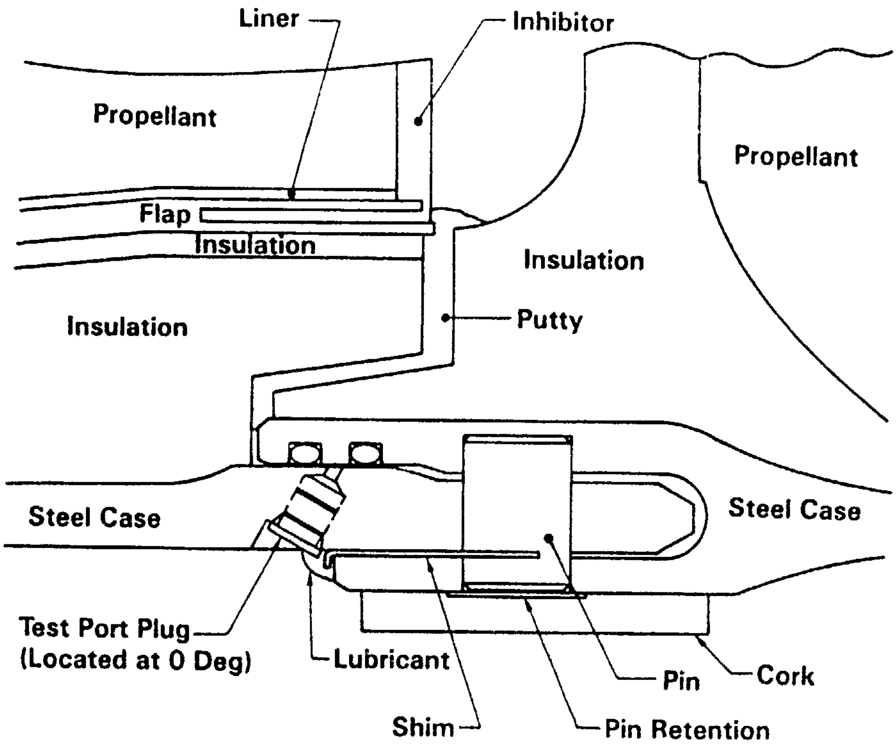
[Ref. 2/26-2 2 of 3]

Field Joint in Rocket Motor



[Ref. 2/26-2 3 of 3]

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SRB CRITICAL ITEMS LI

Sheet 1 of 2

Subsystem: SOLID ROCKET BOOSTER	Criticality Category: 1 Reaction Time: Immediate to Sec.
Item Code: 10-01-01 *Case, P/N (See Retention Rationale) Item Name: (Joint Assys, Factory P/N 1U50147 Field: 1U50747	Page: A-6A Revision:
No. Required: 1 (11 segments, 3 Field joints, 7 plant joints)	Date: December 17, 1982
FMEA Page No. A-4 of MSFC-RPT-724	Analyst: Garber
Critical Phases: Boost	Approved:

Failure Mode & Cause: Leakage at case assembly joints due to redundant O-ring seal failures or primary seal and leak check port O-ring failure.

NOTE: Leakage of the primary O-ring seal is classified as a single failure point due to possibility of loss of sealing at the secondary O-ring because of joint rotation after motor pressurization.

Failure Effect Summary: Actual Loss - Loss of mission, vehicle, and crew due to metal erosion, burnthrough, and probable case burst resulting in fire and deflagration.

RATIONALE FOR RETENTION

Case, P/N 1U50129, 1U50131, 1U50130, 1U50185, 1U50147, 1U50715, 1U50716, 1U50717

A. DESIGN

The SRM case joint design is common in the lightweight and regular weight cases having identical dimensions. The joint concept is basically the same as the single O-ring joint successfully employed on the Titan III solid rocket motor. The SRM joint uses centering clips which are installed in the gap between the tang O.D. and the outside clevis leg to compensate for the loss of concentricity due to gathering and to reduce the total clevis gap which has been provided for ease of assembly. On the shuttle SRM, the secondary O-ring was designed to provide redundancy and to permit a leak check, ensuring proper installation of the O-rings. Full redundancy exists at the moment of initial pressurization. However, test data shows that a phenomenon called joint rotation occurs as the pressure rises, opening up the O-ring extrusion gap and permitting the energized O-ring to protrude into the gap. This condition has been shown by test to be well within that required for safe primary O-ring sealing. This gap may, however, in some cases, increase sufficiently to cause the unenergized secondary O-ring to lose compression, raising question as to its ability to energize and seal if called upon to do so by primary seal failure. Since, under this latter condition only the single O-ring is sealing, a rationale for retention is provided for the simplex mode where only one O-ring is acting.

The surface finish requirement for the O-ring grooves is 63 and the finish of the O-ring contacting portion of the tang, which slices across the O-ring during joint assembly, is 32. The joint design provides an CO for the O-ring installation, which facilitates retention during joint assembly. The tang has a large shallow angle chamfer on the tip to prevent the cutting of the O-ring at assembly. The design drawing specifies application of O-ring lubricant prior to the installation. The factory assembled joints have NBR rubber material vulcanized across the internal joint faying surfaces as a part of the case internal insulation subsystem.

A small MS port leading to the annular cavity between the redundant seals permits a leak check of the seals immediately after joining segments. The MS plug, installed after leak test, has a retaining groove and compression face for its O-ring seal. A means to test the seal of the installed MS plug has not been established.

The O-rings for the case joints are mold formed and ground to close tolerance and the O-rings for the test port are mold formed to net dimensions. Both O-rings are made for high temperature, low compression set fluorocarbon elastomer. The design permits five scarf joints for the case joint seal rings. The O-ring joint strength must equal or exceed 40% of the parent material strength.

B. TESTING

To date, eight static firings and five flights have resulted in 180 (54 field and 126 factory) joints tested with no evidence of leakage. The Titan III program using a similar joint concept has tested a total of 1076 joints successfully.

[Ref. 2/26-3 1 of 2]

SRB CRITICAL ITEMS LIST

Sheet 2 of 2

Subsystem: SOLID ROCKET BOOSTER	Criticality Category 1 Reaction Time Immediate to Sec
Item Code: 10-01-01	Page: A-68
Item Name: *Case, P/N (See Retention Rationale) (Joint Assys, Factory P/N 1U50747 Field: 1U50747	Revision:

RATIONALE FOR RETENTION (CONT'D)

A laboratory test program demonstrated the ability of the O-ring to operate successfully when extruded into gaps well over those encountered in this O-ring application. Uniform gaps of 1/8-inch and over (TWR-13486) successfully withstood pressures of 1600 psi. The Hydroburst Program (TWR-11664) and the Structural Test Program (STA-1) for the standard weight case (TWR-12051) and the Lightweight Case Joint Certification Test (TWR-12829) all have shown that the O-ring can withstand a minimum of four pressurizations before damage to the ring can permit any leakage.

Further demonstration of the capability of joint sealing is found in the hydro-proof testing of new and refurbished case segments. Over 540 joints have been exposed to liquid pressurizations at levels exceeding motor MEOP with no leakage experienced past the primary O-ring. The only occasions where leakage was experienced was during refurbishment of STS-1 where two stiffener segments were severely damaged during cavity collapse at water impact.

A more detailed description of SRM joint testing history is contained in TWR-13520, Revision A.

C. INSPECTION

The tang -A- diameter and clevis -C- diameter are measured and recorded. The depth, width and surface finish of the O-rings grooves are verified. The surface finish of the tang is also verified. Characteristics are inspected on each O-ring to assure conformance to the standards to include:

- Surface conditions
- Mold flashing
- Scarf joint mismatch or separation
- Cross section
- Circumference
- Durometer

Each assembled joint seal is tested per STW7-2747 via pressurizing the annular cavity between seals to 50 ± 5 psi and monitoring for 10 minutes. A pressure decay of 1 psig or greater is not acceptable. Following seal verification by QC, the leak test port plug is installed with QC verifying installation and torquing.

D. FAILURE HISTORY

No failures have been experienced in the static firing of three qualification motors, five development motors and ten flight motors.

[Ref. 2/26-3 2 of 2]

SRB FRR

51-E (STS-22)

ACTION ITEM CLOSURE PAPER

ACTION ITEM NO: SRM-3

ASSIGNED TO: SA42 (J. Thomas)

ACTION DUE DATE: 2/3/85

ACTION ITEM DESCRIPTION: PROVIDE ADDITIONAL DATA SHOWN ON ATTACHED SHEET REGARDING STS-51C CASE JOINT EROSION.

(Continued Pg 2)

BASIS FOR CLOSURE: See the following (attached) FRR charts for each item:

Item	Chart Number(s)
1	RB-21A
2	RB-24, 24A, 24B
3	RB-24, 24A, 24B
4	RB-27, RB-28, RB-29, RB-30, and RB-31
5	RB-30 and RB-31
6	RB-4
7	RB-22

SUBMITTED BY: J. THOMAS

CONCURRENCE: L. WEAR

APPROVAL: L. MULLOY

[Ref. 2/26-4 1 of 14]

SRB FRR
51-E (STS-22)
Action Item Closure Paper
#SRM-3
BA42

Description: (Continued)

1. GRAPHIC COMPARISON OF STS-51C CASE JOINT O-RING EROSION DIMENSION WITH PREVIOUSLY OBSERVED ERODED O-RINGS.

2. PROVIDE DESCRIPTION AND DISPOSITION OF ALL PR'S ON CASE FIELD JOINTS FOR BOTH STS-51C AND 51-E. INCLUDE METAL PARTS PR'S, O-RINGS, PUTTY, LEAK TESTS, ETC.

3. CORRELATE PUTTY LOT WITH CASE JOINTS. IDENTIFY ANY PROBLEMS WITH PUTTY LAYUP.

4. DEVELOP A "MOST PROBABLE" SCENARIO OF HOW AND WHEN:

   - HOT GAS PASSES PRIMARY O-RING
   - PRODUCTS OF COMBUSTION ARE DEPOSITED BEHIND PRIMARY O-RING
   - FLOW PAST O-RING STOPS
   - ETC.

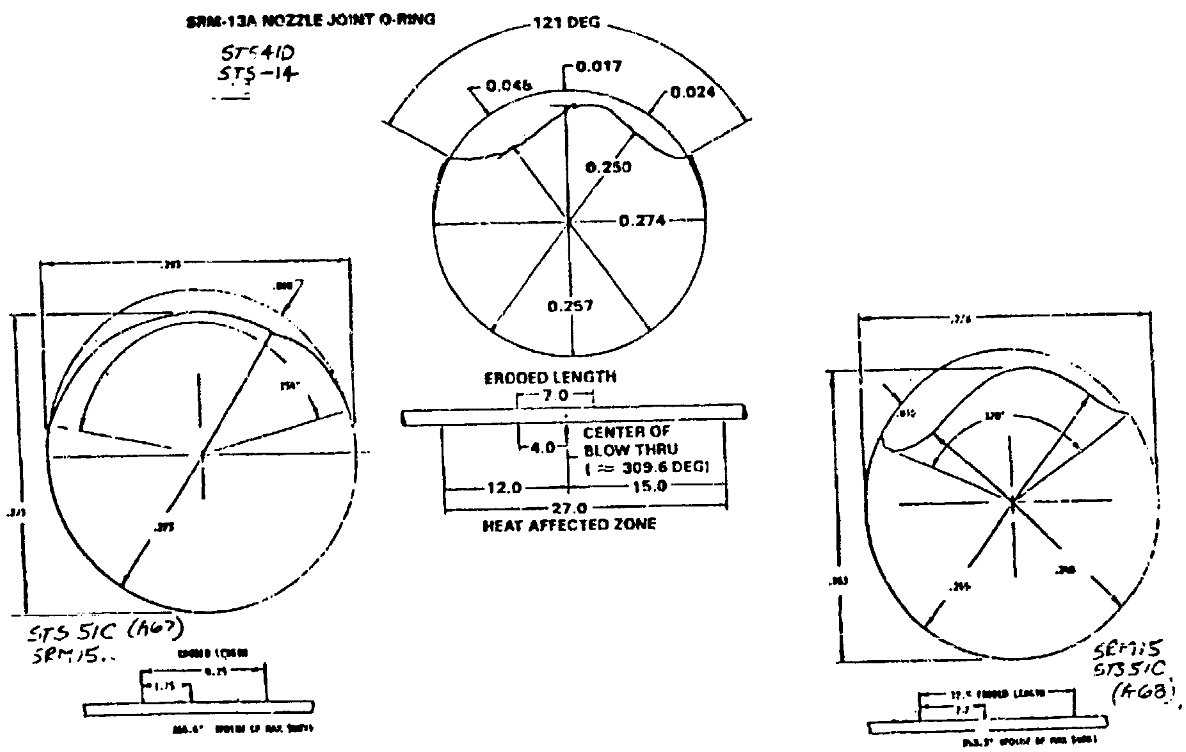
   SUPPORT SCENARIO WITH ANALYSIS/TEST DATA

5. EFFECT OF LOW AMBIENT TEMPERATURE ON JOINT.

6. JOINT GEOMETRY AND O-RING SQUEEZE ON BOTH PRIMARY AND SECONDARY FOR STS-51C.

7. ANALYZE SOOT TO DETERMINE SOURCE.

[Ref. 2/26-4 2 of 14]



[Ref. 2/26-4 3 of 14]

TECHNICAL ISSUES (CONT)

- O COMPARISON OF STS-51C AND STS-51E
  - O NO PROBLEMS ENCOUNTERED WITH PUTTY LOT NUMBERS OR INSTALLATION ON ALL FIELD JOINTS
  - O NO ANOMALOUS CONDITIONS AT ASSEMBLY OF:
    - O O-RINGS
    - O GREASE
    - O LEAK CHECK
      - O ALL STS-51C (SRM-15) AND STS-51E (SRM-16) FIELD JOINTS PASSED LEAK CHECK WITH A DECAY OF 0.2 PSI OR LESS, FIRST ATTEMPT. (1.0 PSI LEAKAGE ALLOWED DURING 10 MINUTE PERIOD AT 50 PSIG)

[Ref. 2-26-4 4 of 14]

TECHNICAL ISSUES (CONTINUED)

O COMPARISON OF STS-51C AND STS-51E

Parameter	STS-51C LH Forward	STS-51C LH Center	STS-51C LH Aft	STS-51C RH Forward	STS-51C RH Center	STS-51C RH Aft
Putty						
Stock No.	5809 5808	5809 5808	5809 5808	5809 5808	5809 5808	5809 5808
Lot No.	0011 0009	0011 0009	0011 0010	0011 0010	0010 0010	0009 0009
DR's*	None	None	None	None	114106	111334, 1190[illegible] 118991
Layup						
Weight, lbs	26.0	24.2	26.5	25.8	24.4	25.9
DR's	None	None	None	None	None	None
Complete Time	10/29, 15:15	10/26, 14:30	10/25, 14:10	10/27, 20:50	10/24, 13:35	10/23, 20:00
Mate						
First Pin Time	10/29, 20:24	10/26, 23:40	10/25, 21:33	10/27, 23:00	10/24, 20:16	10/24, 00:02
Mating Surfaces						
PR's	SR-LF-007-0001 Contamination, sanded	None	AB-BI-015L-000-0003 Scratch above o-ring groove 0°, blend	SB-BI-015R-000[illegible] Gauge in pin-hole, use	SB-BI-015R 0005 Visible pits that could not be felt, use	SB-BI-015R-[illegible] 0004 Surface ins[illegible] not perform[illegible] "just prior[illegible] mate, use

*DR's 114106 - 5808-0010, written 11/20/84, holes in plastic seal, putty retested and resealed
111334 - 5809-0009, written 3/15/84, batches identified as one lot, extra test samples verified putty propertie
119015 - 5809-0009, written 12/14/84, holes in plastic seal, putty retested and resealed
118991 - 5809-0009, written 1/11/85, putty out of 40°F. storage over 30 days, retested

[Ref. 2-26-4 5 of 14]

Parameter	STS-51E LH Forward	STS-51E LH Center	STS-51E LH Aft	STS-51E RH Forward	STS-51E RH Center	STS-51E RH Aft
Putty						
Stock No.	5809 5808	5809 5808	5809 5808	5809 5808	5809 5808	5809 5808
Lot No.	0011 0010	0009 0009	0009 0009	0009 0009	0009 0010	0010 0009
DR's*	114106	111334, 119015 118991	111334, 119015 118991	111334, 119015 118991	111334, 119015 118991, 114106	None
Layup						
Weight, Lbs.	25.2	26.2	26.1	23.5	28.1	23.6
DR's						
Complete time	12/18, 14:30	12/17, 14:30	12/15, 15:00	12/5, 14:20	12/4, 16:30	11/30, 20:00
Mate						
First pin time	12/18, 17:58	12/17, 18:30	12/15, 19:10	12/5, 18:45	12/4, 19:46	11/30, 23:20
Mating Surfaces						
PR's						AB-BI-016R-000-0001 258°-282° repair pits

*DR's 114106 - 5808-0010, written 11/20/84, holes in plastic seal, putty retested and resealed
111334 - 5809-0009, written 3/15/84, three batches identified as one lot, extra test samples verified putty prop
119015 - 5809-0009, written 12/14/84, holes in plastic seal, putty retested and resealed
118991 - 5809-0009, written 1/11/85, putty out of 40°F. storage over 30 days, retested

[Ref. 2/26-4 6 of 14]

STS-51C (SRM-15) STS-20 O-RING EROSION SCENARIO

A 0 NO EROSION UNTIL SONIC FLOW ESTABLISHED IN FIELD JOINT AREA (0 - 50 PSI)

B 0 PRIMARY O-RING SEATING PHASE (50 - 200 PSI)

- 0 LOW TEMPERATURE SHRINKS O-RING AND INCREASES DUROMETER
- 0 GAS JET THROUGH PUTTY IMPINGES ON PRIMARY O-RING
- 0 INITIAL EROSION OF PRIMARY O-RING OCCURS ($<$ 10 MILS)
- 0 GAS FLOW PAST PRIMARY O-RING OCCURS
  - 0 O-RING PRODUCTS FOUND IN SOOT
  - 0 PUTTY PRODUCTS FOUND IN SOOT
- 0 GAS FLOW DEPOSITS SOOT AND LOSES HEAT (ΔT = 5265°F) IN CIRCUMFERENTIAL EXPANSION BETWEEN O-RINGS, ΔT = 570°F BETWEEN O-RINGS
- 0 NO DAMAGE TO SECONDARY O-RING

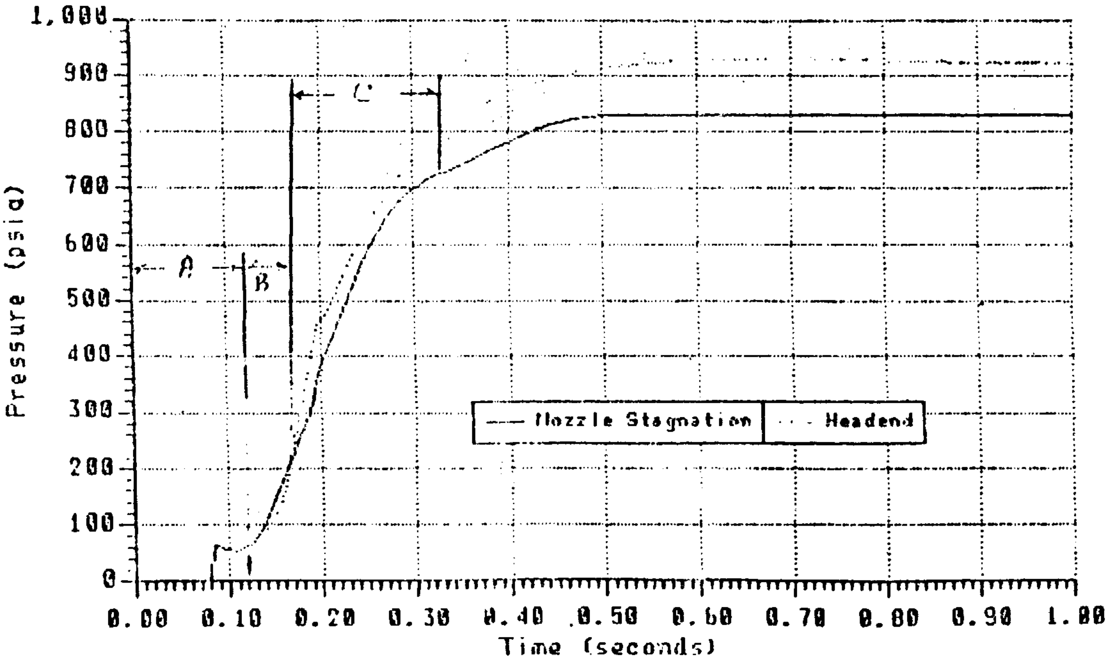
[Ref. 2/26-4 7 of 14]

C 0 PRIMARY O-RING SEALS AND CONTINUES TO ERODE

- 0 PRIMARY O-RING SEATS BY THE TIME PRESSURE REACHES 200 PSI
- 0 FREE VOLUME INCREASES 10X FROM POINT "A" TO POINT "C" DUE TO MOTOR PRESSURIZATION AND JOINT EXPANSION
- 0 MAJORITY OF O-RING EROSION OCCURS DURING THIS PHASE UNTIL PRESSURE EQUILIBRATION OCCURS AT APPROXIMATELY 330MS
- 0 DEPTH OF EROSION IS A FUNCTION OF GAS JET SIZE I.E. SMALLER SIZE HIGHER EROSION
- 0 A67 (L.H.) GAS PATH 2.2 INCHES NEAR O-RING COMPARED TO 1.0 INCH ON A68 (R.H.) I.E. AREA RATIO = 4.8, EROSION DEPTH RATIO = 3.8, ASSUMING FINAL AREA SAME RATIO AS INITIAL

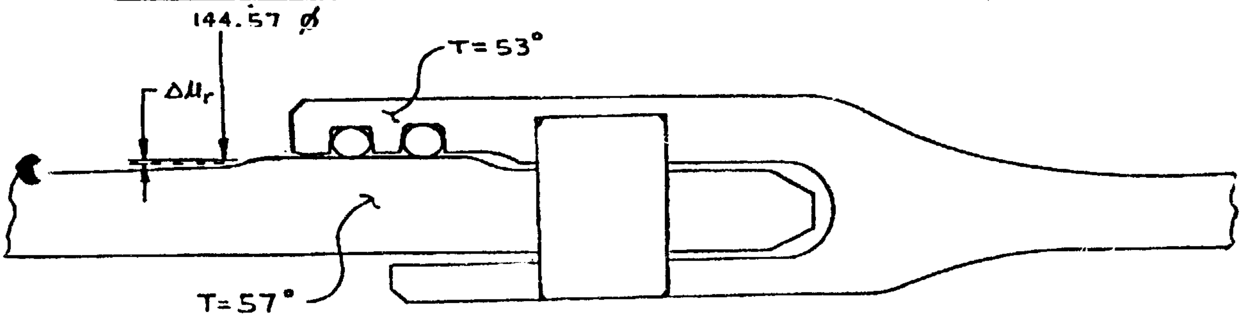
[Ref. 2/26-1 8 of 14]

HPM Predicted Pressure Gradient
70 degree Base Prediction Data



[Ref. 2/26-4 9 of 14]

SRM 16B CENTER FIELD JOINT SQUEEZE ANALYSIS WITH TEMPERATURE DIFFERENTIAL



ΔT ACROSS SEALING SURFACE = 4°

$$\Delta U_r = \frac{\alpha D \Delta T}{2} = \frac{6.3(10^{-6})\,144.57\,(4)}{2} = 0.0018''$$

SQUEEZE WITH TEMPERATURE DIFFERENTIAL

PRIMARY

@ 13% INITIAL STATIC

SQUEEZE = $(.13(0.273) - 0.0018) = 0.0337'' = \underline{12.3}\%$

SECONDARY

@ 4.8% INITIAL AT PRESSURE

SQUEEZE = $(0.048(0.273) - 0.0018) = 0.0113'' = \underline{4.1}\%$ [Ref. 2/26-4 10 of 14]

NOTE: ΔT BASED ON TEMPERATURE CYCLE WITH UNIFORM TEMPERATURE AT 45°F AND AMBIENT TEMPERATURE RAISED TO 64°F

O O-RING SQUEEZE MAINTAINED FOR THE 4°F Δ T CONDITION

TECHNICAL ISSUES (CONT)

- O TEMPERATURE EFFECTS
  - O THERMAL ANALYSIS SHOWS THAT THE SRM FOLLOWS DAILY TEMPERATURES TO A DEPTH OF FIVE INCHES
  - O PUTTY BECOMES STIFFER AND LESS TACKY
  - O O-RING BECOMES HARDER
    - O A VITON O-RING WITH 70 DUROMETER AT 70°F CAN INCREASE TO 85 DUROMETER AT 20°F
    - O TEMPERATURE PRIOR TO FLIGHT DROPPED TO 17°F
    - O TEMPERATURE SQUEEZE REDUCED BY ≈ 1% DUE TO A 4°F DELTA [Ref. 2/26-4 11 of 14]

TECH. ISSUES (CONT.)

FIELD JOINT O-RING SQUEEZE (%)*

		PRIMARY SEAL			SECONDARY SEAL		
MOTOR		FWD	CTR	AFT	FWD	CTR	AFT
SRM 15	A (LT)	16.1 .010"**	15.8	14.7	8.4	7.3	8.1
	B (RT)	11.1	14.0 .038"**	16.1	7.3	8.4	8.4
SRM 16	A (LT)	13.7	15.4	14.7	7.0	8.8	6.2
	B (RT)	12.7	13.0	14.7	6.4	4.8	6.2

* INITIAL SQUEEZE BASED ON NEW OR LATEST REFURBISHED HARDWARE ACTUALS AND MINIMUM O-RING DIAMETER.

[Ref. 2/26-4 12 of 14]

** PRI O-RING DAMAGE DEPTH

TECHNICAL ISSUES (CONTINUED)

0 Analysis of Soot from STS-51C LH Forward Field Joint

	Primary Groove Sample	Land Between O-Rings	Tang Sample	Clevis Sample: Black Putty	Clevis Sample: Green Putty
FTIR Note: No HB Polymer, No Fluorocarbon detected	HD-2, Water	HD-2, Water	HD-2, Water Polyester (Putty)	Polyester, Water	Polyester, Water
EDAX*					
Fluoride (O-ring)	Present	Trace	Present	None	None
Sodium (Grease)	Present	Present	Present	Small Trace	Small Trace
Magnesium (Asbestos, putty)	Present	Present	Minor	Major	Major
Aluminum (Propellant)	Major	Minor	Major	Present	Trace
Silicon (Asbestos, putty)	Present	Present	Minor	Major	Major
Chloride (Propellant AP or Sea Water)	Major	Major	Minor	Present	None
Calcium (Grease)	Major	Major	Major	None	None
Sulfur (Grease)	Minor	Minor	Minor	None	None
Fluoride Ion Test (±20%)	0.8%	1.47%	2.1%	0.64%	

*Relative amounts detected, starting with greatest
Major
Minor
Present
Trace
None

[Ref. 2/26-4 13 of 14]

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FLIGHT READINESS ASSESSMENT FOR STS-51E

- EVALUATION SUMMARY
  - STS-51C PRIMARY O'RING EROSION ON TWO FIELD JOINTS
  - STS-51C SOOT BETWEEN PRIMARY AND SECONDARY O'RINGS ON BOTH FIELD JOINTS PREDICTED AFTER STS-11 OBSERVATION, FIRST TIME OBSERVED
  - EVIDENCE OF HEAT AFFECT ON SECONDARY O'RING OF A68 (RIGHT HAND) CENTER FIELD JOINT BUT NO EROSION - FIRST TIME HEAT AFFECT ON SECONDARY O'RING HAS BEEN OBSERVED
- CONCLUSION
  - STS-51C CONSISTENT WITH EROSION DATA BASE
    - LOW TEMPERATURE ENHANCED PROBABILITY OF BLOW-BY - STS-51C EXPERIENCED WORST CASE TEMPERATURE CHANGE IN FLORIDA HISTORY
  - STS-51E COULD EXHIBIT SAME BEHAVIOR
  - CONDITION IS ACCEPTABLE
- STS-51E FIELD JOINTS ARE ACCEPTABLE FOR FLIGHT

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[Ref. 2/26-4 14 of 14]

General Conclusions

- All O-ring erosion has occurred where gas paths in the vacuum putty are formed
- Gas paths in the vacuum putty can occur during assembly, leak check, or during motor pressurization
- Improved filler materials or layup configurations which still allow a valid leak check of the primary O-rings may reduce frequency of O-ring erosion but will probably not eliminate it or reduce the severity of erosion
- Elimination of vacuum putty in a tighter joint area will eliminate O-ring erosion if circumferential flow is not present - if it is present, some baffle arrangement may be required
- Erosion in the nozzle joint is more severe due to eccentricity; however, the secondary seal in the nozzle will seal and will not erode through
- The primary O-ring in the field joint should not erode through but if it leaks due to erosion or lack of sealing the secondary seal may not seal the motor. ***Data obtained on resiliency of the O-rings indicate that lower temperatures aggravate this problem***
- The igniter Gask-O-Seal design is adequate providing proper quality inspections are made to eliminate overfill conditions

D-15a [Ref. 2/26-5]

MTI ASSESSMENT OF TEMPERATURE CONCERN ON SRM-25 (51L) LAUNCH

- O CALCULATIONS SHOW THAT SRM-25 O-RINGS WILL BE 20° COLDER THAN SRM-15 O-RINGS
- O TEMPERATURE DATA NOT CONCLUSIVE ON PREDICTING PRIMARY O-RING BLOW-BY
- O ENGINEERING ASSESSMENT IS THAT:
  - O COLDER O-RINGS WILL HAVE INCREASED EFFECTIVE DUROMETER ("HARDER")
  - O "HARDER" O-RINGS WILL TAKE LONGER TO "SEAT"
    - O MORE GAS MAY PASS PRIMARY O-RING BEFORE THE PRIMARY SEAL SEATS (RELATIVE TO SRM-15)
      - O DEMONSTRATED SEALING THRESHOLD IS 3 TIMES GREATER THAN 0.038" EROSION EXPERIENCED ON SRM-15
  - O IF THE PRIMARY SEAL DOES NOT SEAT, THE SECONDARY SEAL WILL SEAT
    - O PRESSURE WILL GET TO SECONDARY SEAL BEFORE THE METAL PARTS ROTATE
      - O O-RING PRESSURE LEAK CHECK PLACES SECONDARY SEAL IN OUTBOARD POSITION WHICH MINIMIZES SEALING TIME
- O MTI RECOMMENDS STS-51L LAUNCH PROCEED ON 28 JANUARY 1986
  - O SRM-25 WILL NOT BE SIGNIFICANTLY DIFFERENT FROM SRM-15

Joe C. Kilminster

JOE C KILMINSTER, VICE PRESIDENT
SPACE BOOSTER PROGRAMS

MORTON THIOKOL INC
Wasatch Division

[Ref. 2/26-6]

TESTIMONY OF STANLEY REINARTZ, MANAGER, MARSHALL SHUTTLE PROJECTS OFFICE; AND DR. JUDSON A. LOVINGOOD, DEPUTY MANAGER, MARSHALL SHUTTLE PROJECTS OFFICE

CHAIRMAN ROGERS: Gentlemen, would you briefly identify yourselves and your position and a brief history of your association with NASA?

MR. REINARTZ: All right, sir. I am Stanley Reinartz. I am currently manager of the Marshall shuttle projects office, which incorporates the external tank, the space shuttle main engine, and the solid rocket booster.

And my background: I originally went to the Army Ballistic Missile Agency in 1957, after graduating from the University of Cincinnati in 1956 as an engineer. I joined NASA in 1960 and, after a period of work in the structures and mechanics laboratory, I became deputy of the—after being a project engineer, then became deputy of the Saturn 1, 1B vehicle project office, then moved into being deputy manager of the Skylab project for approximately eight years, director of the test laboratory at Marshall for a period of three years, deputy manager of the Spacelab payload project for several years, and deputy director of the special projects office for a period before I took my current

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position on August 31st, 1985.

CHAIRMAN ROGERS: Mr. Lovingood.

DR. LOVINGOOD: Mr. Chairman and members of the Commission:

I am Judson A. Lovingood, deputy manager of shuttle projects office. I have a bachelor of science degree in electrical engineering from the University of Alabama in 1958.

Upon graduating, I worked at Martin Marietta in Orlando, and then I went to Honeywell, Incorporated, in Minneapolis. I received a master of science degree from the University of Minnesota in applied math.

I joined NASA in 1962. Until 1967, I worked in dynamics and flight control, guidance and celestial mechanics, in various positions. In 1967, NASA sent me to school for one year to complete my doctor of philosophy.

In 1969 I became chief of the dynamics control division of the aero-astrodynamics laboratory. In 1974 I became director of the systems dynamics laboratory. All of the above positions were in the Science and Engineering Directorate of the Marshall Center. The projects supported during this period were primarily Apollo, Skylab, and Space Shuttle.

In 1979, I became deputy manager of the

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shuttle projects office, and in February of 1982 I became manager of the shuttle main engine project. And in October 1983, when the main engine project was divided into separate development and flight project offices, I again became deputy manager of the shuttle.

CHAIRMAN ROGERS: Thank you.

How do you two gentlemen relate to each other in terms of your work assignments?

MR. REINARTZ: I'm the manager of the projects and Jud is my deputy, and he has worked many technical items and with his past experience in the engine area, I have used him for that advice in the engine area.

CHAIRMAN ROGERS: All right. And your immediate superior is whom?

MR. REINARTZ: Dr. William Lucas, the director of the Marshall Space Flight Center.

CHAIRMAN ROGERS: Now, we would like to ask you some questions about when you were first made aware of this problem in connection with 51-L. And you know what the testimony has been, so we don't necessarily want you to repeat it all, but we are particularly interested in how it happened that the decisionmaking people, at particularly the Level I and II, were not made aware of all of these things we have been talking about yesterday and today.

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So will you—Mr. Reinartz, will you relate when you first heard about this problem and what you did from that point on?

MR. REINARTZ: All right, sir. With your permission, I would like to use my statement to pick up those major points, if that is all right.

CHAIRMAN ROGERS: Sure, that's fine.

MR. REINARTZ: My recollections of the decision process on a summary basis for the launch recommendation, Mr. Hardy and Mr. Mulloy have covered a lot of the detail of that and I will show how I interacted with that activity.

In regard to the launch recommendation process, I continue to believe that the activities associated with this process, specifically the January 27th discussion between Thiokol and Marshall, were conducted in a thorough and professional manner and in the NASA tradition of full and open participation of the personnel necessary for appropriate disposition of the specific concern.

And I think one of my roles in that was to see that that was carried out. It was a problem being worked by the shuttle rocket booster, but I felt it was appropriate to see that that was carried out in that manner, and made no indication of my feelings during the

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course of the discussions.

In the case of the 51-L, a concern was raised by Thiokol on the evening of January 27th, the night before the planned launch, concerning possible effects of the predicted temperatures on the solid rocket motor case-to-case field joint O-rings.

After this concern was raised, we set about an orderly and normal process set in motion to assure a full examination of the concern and the engineering data and the rationale associated with this concern. First we had an identification of this concern during the preliminary telecon early in the evening, and during the course of that discussion we had very poor communications.

We were at a number of different locations and were scattered. And so we had a request for Thiokol to supply their written data to us, and then we were to assemble all the necessary personnel at Marshall, Thiokol Wasatch in Utah, and those senior Thiokol project representatives at KSC, to get a good telecon.

The senior 51-L Thiokol representative was Mr. Al McDonald. Mr. Mulloy and I were the senior Marshall project representatives on site. Dr. Lucas and Mr. Kingsbury were in the KSC area for the launch the next day and had been at the 2:00 meeting that

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afternoon that set the plan for the next day.

Then, in an approximately two-hour meeting, Thiokol engineering personnel presented and discussed material relating to their concern. As Mr. Hardy testified, I believe Marshall thoroughly probed the data, and all parties had an opportunity to provide inputs and express their views.

After the initial Thiokol recommendation to not launch until the temperature was at least 53 degrees, Mr. Mulloy, as he indicated, stated a rationale that he believed supported a launch recommendation. I asked Mr. Hardy at that point for his comments. After Mr. Hardy's remarks, to which he has testified earlier, including his clear and unequivocal statement to me, as well as all parties on the line—it was not addressed just to Thiokol—a clear statement that he would not recommend launch over Thiokol's objections, I then asked Mr. Kilminster for his comments at that point.

Then Mr. Kilminster asked for a five-minute caucus. And I might say, Mr. Chairman, that with that comment of Mr. Hardy's, I took that as a very clear and direct thing, not only in discussing with Thiokol but to Mr. Mulloy as the project manager and to myself. And I look to the science and engineering group for engineering recommendations, and have worked personally

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with Mr. Hardy for 25 years, and know that when he says he wouldn't launch that he means exactly that.

CHAIRMAN ROGERS: I don't think anybody has ever questioned that.

MR. REINARTZ: Yes, sir. I just wanted to emphasize that point, sir.

At the end of the two-and-a-half-hour period, including an approximately 35-minute-off-the-loop Thiokol caucus, and after their recommendation, their final recommendation, to launch, I collectively asked all telecon parties if there were any disagreement with Thiokol's rationale and recommendation as stated by Mr. Kilminster.

There were none received from Thiokol at Wasatch, Marshall at Huntsville, nor Mr. McDonald, who was sitting with Mr. Mulloy and myself at KSC. Thiokol was then asked to document their verbal rationale and launch recommendation statement, as is our normal practice.

Based on the process I described and the conclusions reached as a result of that process, including the contractor recommendation and the Marshall engineering support, I concurred with the decision of the Level III project manager, Mr. Mulloy, supporting the launch recommendation and continuing with the launch

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process.

Before going into my post-decision activities, which I am sure are of interest to the committee, I would like to amplify one portion. During the January 27th post-scrub discussions all Marshall support elements and to my knowledge the contractor representatives at KSC were present on the launch center-Marshall voice loop when I made a request for capability to support a 24-hour turn-around capability, recognizing the colder predicted temperatures for the January 28th launch.

The senior Thiokol program representative has a duty station on the loop at KSC, did not provide any input on the loop or to the physically adjacent Marshall SRB representative regarding any items that should be looked into while proceeding with the launch preparation.

During the two-and-a-half-hour telecon between Thiokol and Marshall, extended over that time—it was muted for approximately 30 minutes—I would characterize the presentation and associated discussions as deliberate and intense, and a professional engineering examination of the data, and not highly heated or emotional. And no heated protest was injected into the open discussion by the senior Thiokol representative at KSC during that two-and-a-half-hour

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period.

CHAIRMAN ROGERS: Isn't it true, though, that you were made aware, because you listened to that conference, that Thiokol had recommended no launch, and that they were almost unanimous at that time in recommending no launch?

MR. REINARTZ: No, sir, your statement about nearly unanimous was not a bit of knowledge that I had in that time.

CHAIRMAN ROGERS: How many of the Thiokol people on that telecon voted in favor of launch?

MR. REINARTZ: As far as I know, sir, there wasn't any voting at that time.

CHAIRMAN ROGERS: Well, maybe not a vote, but I mean the expressions that were made by the Thiokol people up until the time of the recess. Information we have is there was nobody on the Thiokol side that was urging a launch.

Do you remember anybody that urged a launch on the part of Thiokol up to that point?

MR. REINARTZ: No, sir. They did not vote or provide—

CHAIRMAN ROGERS: I'm not talking about a vote. I'm talking about did anybody express a view, we think you should launch the shuttle, the Challenger, at

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that point?

MR. REINARTZ: They had provided a recommendation that it not be launched below 53 degrees.

CHAIRMAN ROGERS: Did anybody in the telecon say, we disagree, we think you should go ahead and launch?

MR. REINARTZ: No, sir.

CHAIRMAN ROGERS: So as far as you knew based upon what was said, it seemed to be a unanimous view of the Thiokol people on the telecon that they were recommending no launch?

MR. REINARTZ: At that point in time.

CHAIRMAN ROGERS: That is what I asked.

MR. REINARTZ: At that point in time, yes, sir.

The senior Thiokol representative at KSC did inject one significant comment just after—as I have said, Mr. Kilminster has asked for a caucus; just after he asked for this caucus, at about the two hour juncture in this discussion. This comment by Mr. McDonald was perceived, I believe, by all parties and, I believe, as testified by Thiokol in the hearings yesterday, as a supporting point for a positive launch recommendation, or at least a positive point.

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He did not make a statement, I recommend launch. Mr. McDonald said that Mr. Kilminster should consider a point made by Mr. Hardy earlier, that the secondary O-ring is in the proper position to seal if blow-by of the primary O-ring occurred. And I believe that Mr. Hardy tried to elaborate on the importance of the secondary O-ring.

CHAIRMAN ROGERS: Mr. Reinartz, during the caucus didn't you have a discussion with Mr. McDonald where he voiced his strong opposition to the launch?

MR. REINARTZ: Mr. McDonald during that time of the period that we were on that, he discussed some of the same concerns that were covered during the telecon and had talked about some of those concerns during that time.

CHAIRMAN ROGERS: And weren't those concerns—didn't that—didn't you draw the conclusion from that that he was opposed to the launch?

MR. REINARTZ: At that point I did not draw that conclusion, Mr. Chairman, that he was completely opposed to the launch, having made the statement that you should consider the secondary seal.

CHAIRMAN ROGERS: Didn't he recommend at that time to you that the launch be delayed until late in the afternoon, until the temperatures reached 48 or 50?

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MR. REINARTZ: I'm not sure whether that was done during the caucus, as Mr. Mulloy had talked this morning, whether that was during the caucus or whether that was in the statement that Mr. McDonald made after the telecon was complete, where he said that there were these three things that might be considered for a launch delay.

I'm not sure of that, Mr. Chairman.

CHAIRMAN ROGERS: But you're not really trying to convince the Commission that you didn't know that Mr. McDonald had serious questions about the launch and didn't really want the launch to occur, are you?

MR. REINARTZ: No, sir. I'm trying to relay what came across that evening during the discussion, Mr. Chairman.

CHAIRMAN ROGERS: Okay. Go ahead.

MR. REINARTZ: Marshall elements in Huntsville and those elements at KSC, of course, as has been testified, had no knowledge of the internal Thiokol discussions during the 30 to 35-minute caucus that preceded their launch recommendation. At that point and to my knowledge, Mr. McDonald, who had indicated, as you had stated, some concerns, as far as I know did not take that opportunity to inject any of his thoughts or concerns via private telephone input into the internal

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Thiokol discussions that were going on during that caucus.

As stated earlier, when I asked all the parties collectively if there were any disagreements with the final Thiokol recommendation that I received from Mr. Kilminster, there was no statement or comment from Mr. McDonald, at that time sitting with Mr. Mulloy and myself here at the Cape.

Now, I would like to discuss—

CHAIRMAN ROGERS: And so your testimony is that, after the decision was made, you were satisfied that Mr. McDonald had no question in his mind, that he went along with the decision to launch?

MR. REINARTZ: At the point, Mr. Chairman, when I asked very clearly and very deliberately on the telecon, while all parties were involved, I asked if there were any disagreements to all the parties, including Mr. McDonald, who was sitting right across from me, and there was no comment, no objection or anything raised at that time.

CHAIRMAN ROGERS: I accept that. But later on didn't you know he continued to object and there was a lot of opposition on the part of the Thiokol people?

MR. REINARTZ: I did not know of the lot of opposition of the Thiokol people.

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CHAIRMAN ROGERS: Did you know that he opposed?

MR. REINARTZ: After the launch, as Mr. Mulloy testified—or, excuse me, sir. Not after the launch, but after the telecon, Mr. McDonald said, after we had completed and hung up he said, well, if there was not sufficient reason related to the concerns on O-ring temperature, then how about the ice situation, and how about the recovery sea state.

And Mr. Mulloy testified as to the comments to the ice situation and to the recovery situation, and we participated jointly, which was the next item I was going to discuss—we participated jointly in a telecon with the KSC personnel and Mr. Aldrich about the possible loss of SRB parachutes and frustums resulting from that sea state.

CHAIRMAN ROGERS: But Mr. McDonald testified yesterday under oath that he had a discussion with you and pointed out there were three reasons why he opposed the launch, and one was the O-rings and the other two were the things that you mentioned.

Didn't you know that?

MR. REINARTZ: Yes, sir. I said that he stated that, if—the way that I recall he stated it, he said: If that is not enough of a concern for you, is

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I believe the way he said it, then if I were launch director there were three things, that there would be that and then the other two that he named.

CHAIRMAN ROGERS: Mr. Reinartz, the reason I'm pressing you, as I am sure you know, it's very difficult for the Commission to understand how this serious matter was discussed by so many people just prior to the launch, and there still were serious questions, obviously, in the minds of a lot of people, how those concerns were not conveyed to the people who had to make the ultimate decision whether the shuttle would be launched or not.

That is what is very difficult for us to understand.

MR. REINARTZ: I can appreciate that, Mr. Chairman, the difficulty that you are having, and the people, each one of us, collectively going back to that point in time.

But I think it is important to differentiate between a couple of items. We had the knowledge that evening, Mr. Chairman, of the data that Thiokol presented in their charts and the discussion that was in that telecon. I did not know of any memos that had been circulated, that have been now said they were circulated to some levels within Thiokol. I had none of

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that knowledge.

I had none of the knowledge of any dissension that was going on within the internal discussion at Thiokol. None of that was available.

Mr. McDonald had the opportunity to inject anything into that Thiokol internal if he chose to. As far as I know, there was nothing constraining him. And the only thing that I had, after the results that I had, the recommendation from the Thiokol program manager, I had the Marshall engineering support of that activity, and I had the SRB manager's decision that he wanted to proceed, which I concurred with.

And those were the inputs that I had that evening at that point in time. And then Mr. McDonald said—he did not make any statement that said, hey, I want to go take this to anybody else, or I have a difficulty with this.

He said: If I was launch director, these are the things that I would be considering for tomorrow.

CHAIRMAN ROGERS: I really don't intend to make it just a matter of Mr. McDonald and you. It is much more serious than that, it seems to me. Let's assume that you were satisfied with all of the paperwork, and let's assume when you got the telefax you said: Ah, we've got the piece of paper from Thiokol.

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Knowing what you knew, didn't you have in your own mind some question about the wisdom of going ahead with the launch? Or were you totally satisfied that there was no problem?

MR. REINARTZ: I was satisfied that there was not a flight safety problem, based on the discussion and the advice that I got related to that. That was made up of two parts: the final recommendation from Thiokol program manager and the input from the Marshall engineering people, as detailed by Mr. Hardy.

CHAIRMAN ROGERS: At the time of the telecon and before you got the piece of paper telefaxed, listening to the arguments, didn't you have some concern about the safety of that launch?

MR. REINARTZ: Yes, sir. I have concerns about the safety of every launch.

CHAIRMAN ROGERS: But I mean, this one, a little more so on this one. Did you ever have a situation where the recommendation from the contractor was no launch and then they changed their mind as a result of urging on the part of NASA?

MR. REINARTZ: I would like to answer that question in two parts, if I may. I am not aware—I have not personally participated in that exact situation as you described.

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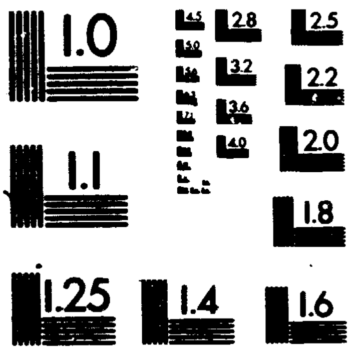

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And secondly, I did not urge, nor do I think my people urged, Thiokol to change their recommendation.

CHAIRMAN ROGERS: I will withdraw that part of the question. Do you ever remember or do you know of any situation where the contractor at any time recommended no launch and subsequently changed their minds?

MR. REINARTZ: I do not recall personally being in that situation.

CHAIRMAN ROGERS: And wouldn't that call to your attention the seriousness of the situation?

MR. REINARTZ: The seriousness of the situation was certainly recognized and examined by us. But I think it is important to understand at the same time that we—that Thiokol had indicated to us during the telecon that they were still working the problem, as I think has been testified to by Thiokol, that there were some parallel activities going on at the same time that was being worked.

And the only—when I asked for comments to Thiokol, I was not asking for a change of decision. I was wanting to be sure that all of their activities that they had been working somewhat in parallel, as I think has come out in the testimony, that that had all been

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completed and they had come up with a final recommendation.

CHAIRMAN ROGERS: I guess the question that still lingers in my mind is, in the Navy we used to have an expression about going by the book, and I gather you were going by the book. But doesn't the process require some judgment?

Don't you have to use common sense? Wouldn't common sense require that you tell the decisionmakers about this serious problem that was different from anything in the past?

MR. REINARTZ: In looking at that one, Mr. Chairman, together with Mr. Mulloy when we looked at were there any launch commits, any Level II, as I perceived during the telecon, I got no disagreement concerning the Thiokol launch between any of the Level III elements, the contractor, with Mr. McDonald there.

I felt that the Thiokol and Marshall people had fully examined that concern, and that it had been satisfactorily dispositioned based upon the evidence and the data that was supplied to that decision process on that evening, from that material, and not extraneous to what else may have been going on within Thiokol that I had no knowledge of.

CHAIRMAN ROGERS: Okay. Thank you. I'm sorry

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for the long interruption.

MR. REINARTZ: Based upon—and as we skipped over, it is only a point to illustrate, Mr. Chairman, that in our discussion about the parachute with KSC and Mr. Aldrich, was to indicate that there was a clear area there where we had a very direct responsibility to inform them of the situation, which Mr. Mulloy did. And after a discussion of that issue, Mr. Aldrich concluded that the launch should proceed in that nature.

Based on the results of the meeting and the conclusions out of the meeting, Mr. Mulloy and I informed the director of Marshall, Dr. Lucas, and the director of science and engineering, Mr. Kingsbury, on the 28th of January about 5:00 of the initial Thiokol concerns and engineering recommendations, the final Thiokol launch recommendation, and the full support of the Marshall engineering for the launch recommendation, that I felt had led to a successful resolution of this concern.

GENERAL KUTYNA: Could I interrupt for a minute? You informed Dr. Lucas. He is not in the reporting chain?

MR. REINARTZ: No, sir.

GENERAL KUTYNA: If I could use an analogy, if you want to report a fire you don't go to the mayor.

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In his position as center director, Dr. Lucas was cut out of the reporting chain, much like a mayor. If it was important enough to report to him, why didn't you go through the fire department and go up your decision chain?

MR. REINARTZ: That, General Kutyna, is a normal course of our operating mode within the center, that I keep Dr. Lucas informed of my activities, be they this type of thing or other.

GENERAL KUTYNA: But you did that at 5 o'clock in the morning. That's kind of early. It would seem that's important.

Why didn't you go up the chain?

MR. REINARTZ: No, sir. That is the time when we go in, basically go into the launch, and so it was not waking him up to tell him that information. It was when we go into the launch in the morning.

And based upon my assessment of the situation as dispositioned that evening, for better or worse, I did not perceive any clear requirement for interaction with Level II, as the concern was worked and dispositioned with full agreement among all responsible parties as to that agreement.

CHAIRMAN ROGERS: Did I understand what you just said, that you told Dr. Lucas that all the

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engineers at Thiokol were in accord?

MR. REINARTZ: No, sir. What I told him was of the initial Thiokol concerns that we had and the initial recommendation and the final Thiokol recommendation and the rationale associated with that recommendation, and the fact that we had the full support of the senior Marshall engineering and, as George has testified, to the extensiveness of the group of people we had involved in that telecon with the various disciplines, that those three elements made up the final recommendation.

MR. HOTZ: Mr. Reinartz, are you telling us that you in fact are the person who made the decision not to escalate this to a Level II item?

MR. REINARTZ: That is correct, sir.

MR. HOTZ: Thank you.

CHAIRMAN ROGERS: Do you think the system should be changed now? Do you think that this Commission should make a recommendation to correct that what appears to be inadequacy?

MR. REINARTZ: I would not give a very quick, off-the-cuff answer to that, Mr. Chairman. I would like the opportunity to study that question some before I would recommend any major changes to the system.

DR. RIDE: Did you discuss with Dr. Lucas at

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all the possibility of reporting it to Arnie Aldrich?

MR. REINARTZ: No, ma'am, I did not.

DR. RIDE: Did anyone recommend to you that you report it to Mr. Aldrich?

MR. REINARTZ: No, Dr. Ride, they did not.

DR. KEEL: Mr. Chairman, can I follow up on that?

Mr. Lovingood, in your personal notes provided to the committee, you indicated that you told Mr. Reinartz to advise Aldrich of the conference. Can you tell us exactly what you told him?

DR. LOVINGOOD: Yes, I think the way Stan just answered that question, I didn't advise Stan after the 8:45 Eastern Time telecon to advise Mr. Aldrich. When we terminated the telecon, Stan and I didn't talk any more. When we had the early evening telecon, that began at, I think it was, 4:45 Huntsville time—

CHAIRMAN ROGERS: Which day?

DR. LOVINGOOD: On the 27th.

Then everybody has mentioned that we had a bad hookup. We had people at home and we didn't have the charts there so that we could clearly follow the data. I probably could hear better than anybody, because I was in my office and I had a good tie-in with Wasatch.

And on the basis of that discussion, I felt

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like we were probably approaching a launch delay, and that is why I suggested then on that loop that we plan to have a later meeting, and we needed to get the charts in there so we could sit down and carefully go through all the data and assess it.

After completing that telecon and then making—and everyone agreeing to meet at certain locations and have the subsequent telecon that evening, I called Stan at the motel and I suggested to him that he go ahead and alert Arnie at that point.

CHAIRMAN ROGERS: "Arnie" being?

DR. LOVINGOOD: Arnie Aldrich. At that point that we were having the meeting, and that we were going to get together within the center and decide what to do, and then prepare Arnie for getting Level I together and then going on up the line.

But what I was looking at then was, I was thinking that there was a good possibility of a launch delay.

CHAIRMAN ROGERS: Of a what?

DR. LOVINGOOD: Of a launch delay.

Now, I never told Stan, if we don't delay the launch then go to Arnie. I never made that recommendation to him. I made the recommendation at the time it appeared to me that we were coming up on a

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delay.

CHAIRMAN ROGERS: Mr. Reinartz, do you remember him advising you that you let Mr. Aldrich know?

MR. REINARTZ: Yes, sir, earlier in the evening. As he indicated, on the telecon at that time I had felt, as he indicated, we had had a very poor communication, we did not have a good understanding of the situation, and I felt it was necessary, Mr. Chairman, to get a full and complete understanding of the situation, and would proceed from there.

CHAIRMAN ROGERS: But I was addressing my question to whether you remember being advised to let Mr. Aldrich know, and if so why didn't you let him know?

MR. REINARTZ: As I indicated, I felt that we needed—that we did not have a full understanding of the situation as I understood it at that time, and felt that was appropriate to do before we involved the Level II into the system.

So I guess, in my brief conclusion, Mr. Chairman, that from the viewpoint as a participant in that January 27th telecon, I believed that the total Thiokol and Marshall teams performed in a responsible manner, based upon the knowledge that I had, in the

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continuation of the dedicated professional approach by which these issues involving the space shuttle are addressed and resolved through an engineering and managerial assessment of the data to determine if the vehicle was ready and safe to fly.

I believe the process was appropriate and that the concern was dispositioned in the proper manner. I did not perceive, nor did I personally exert, any pressure on Thiokol. I stayed basically out of the telecon, as a listener, trying to discern the situation, and did not believe that I was exerting pressure on Thiokol.

Now, I might say, Mr. Chairman, that in light of your question, in hindsight it may have been better to inform Level II of the decisions, since they would—however, since they would only know what we knew from our side of the loop, it is sort of speculative on my part to consider what they would have done with the data that we had reported, considering at that point in time we had a final launch recommendation from the contractor and the concurrence by the Marshall engineering people. That would be the normal checkpoints that would be asked by Level II in the event of some discussion of a decision.

CHAIRMAN ROGERS: I guess you realize that

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this morning we had testimony to the effect that, from Mr. Hardy, that had he known that all of the engineers at Thiokol were still opposed to the launch, that his decision might have been different. In fact, I think he said it would have been different.

So there again, it seems to me at least that there was a failure of the process. And you relied on the telefax from Mr. Kilminster, and Mr. Hardy and others didn't realize that all the engineers at Thiokol were against the launch even then. So that that information never got to you and it never got to Mr. Moore or Mr. Aldrich.

MR. REINARTZ: You are correct, Mr. Chairman, and I think the question that you raised, be it for this issue or many others, will be the question relating to internal processes used within the contractors or within government agencies by which a final decision is arrived at. And I think that is entirely appropriate.

DR. RIDE: Did you appreciate that this was a Criticality 1 system that was being discussed?

MR. REINARTZ: Yes, Dr. Ride, I did.

MR. SUTTER: When you get ready to launch you've got to look at the main power plants, the tank, the whole system. But did NASA perceive that this joint maybe was sub-marginal compared to the rest of the

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system, or did you assume it was business as usual?

MR. REINARTZ: No, sir. I think that how I would like to answer that is, within the total shuttle system there are a number of issues and problems with hardware being worked at any one time, and we had some that were being worked across the board in engines, and this one—

MR. SUTTER: But not as critical as this item?

MR. REINARTZ: Yes, sir, we have items that represent single point failures across the system. As Mr. Mulloy indicated, there is a number in his. There is a substantial number.

MR. SUTTER: And are there changes that are anticipated to bring them up to a better level, like the joint?

MR. REINARTZ: Yes, sir. There are capability improvements that are going on in all of our systems at this point in time.

MR. SUTTER: Capability improvements?

MR. REINARTZ: That would improve—excuse me. Let me say, that would improve their safety margins, are going on at this time.

MR. SUTTER: I would like to see a list of what those items are and prioritize them, and where

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would that joint be on that list?

MR. REINARTZ: I could give you a list. It would be after the fact, any prioritizing I would do, Mr. Sutter.

MR. SUTTER: Well, the reason I ask that question is that when you listen to Thiokol they really gave me the impression that that joint was in serious difficulty, and they mentioned the two meetings and they mentioned the short-term and the middle-term and the long-term changes.

And some of their engineers expressed—well, you read their letters now. Maybe they never showed them to you. But again, is there a communication gap, with one engineering department saying it is okay and another one saying we've got great concerns?

I still think, if there was a total communications system going on, anybody that would be facing that situation would have a tough time saying go. And so I really think there is a serious communication and management problem here.

MR. REINARTZ: During the discussion from the August time frame, Mr. Sutter, the discussions continued for each one of the launches that were associated from that time frame. And the Thiokol personnel—and I do not know at what point that stopped or started—

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presented to the system, to Marshall, to headquarters, and all the rest of the system, the fact that that booster as it existed was completely safe and reliable for launching while these concerns about O-rings were being worked in a parallel fashion.

MR. SUTTER: Well, I may be Monday morning quarterbacking, but the way I read the Thiokol engineers, they knew of these concerns in the joint, and the Priority 1, the 1R and the blow-by, and I really think that they were getting themselves in a position where they suddenly got to a point where they now were outside of the area where they had the experience, and they just sort of said enough was enough.

They built up a real concern themselves, and I'm surprised they didn't build up that same concern in the engineers at NASA.

MR. REINARTZ: Going back in hindsight, Mr. Sutter, I'm surprised that the day before, where we were scheduled to launch and it was in the approximately 40 degrees, Thiokol raised no issue regarding O-ring temperature joints.

MR. SUTTER: It seems there's a communications gap here.

MR. REINARTZ: I just said that I was surprised in hindsight that they had not raised that, in

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light of the subsequent discussions of Monday evening.

DR. WALKER: Mr. Reinartz, I would like to understand your analysis of the technical issues. Mr. Hardy has stated that in his view perhaps the major cause of blow-by were imperfections in the seals and the sealing surfaces, and I think that that may very well be the case. There are certainly strong reasons to think that.

Did you in fact also think that and therefore feel that temperature was really not a significant parameter?

MR. REINARTZ: Dr. Walker, you've had several O-ring experts and other people that have spent their careers in detailed engineering activities. I do not qualify myself as a seal expert, but I listened very carefully to the reasons and the rationale that was generated during the course of that discussion and during Mr. Hardy's comments and discussion, and relied very heavily on the combination of the contractor's recommendation and the engineering recommendation of the people that I have worked with for many years.

DR. WALKER: But did you have an opinion as to why Mr. Mulloy and Mr. Hardy did not agree that temperature was a very important parameter? I mean, that was really the crux of the matter.

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MR. REINARTZ: Yes, sir, I did have an opinion on that, and the fact that, based upon the range at which blow-by had occurred on the various motors and flight experience, and based upon the fact that it appeared from that data that there was not a correlation with temperature and the incipient blow-by, and the whole point of our evening discussion, that we were—a narrow issue that Thiokol raised that evening to discuss was the question of whether or not the primary ring would take longer to seat and therefore increase the risk of blow-by, the longer seating being from the cold temperature.

And during that, that was a possibility that Thiokol and ourselves had acknowledged. The condition of blow-by was a condition that had been acknowledged for the past year in all the flight readiness reviews, that that was a possibility that could occur.

DR. WALKER: Let's suppose that Thiokol was incorrect and that temperature was not really an important parameter. Did you feel that you wanted to understand this situation and therefore develop an alternative theory which would explain the blow-by data?

And presumably, Mr. Hardy had done that in his own mind. He was convinced imperfections and other

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sorts of—imperfections and the nicks and what have you in the surfaces were the real cause. Did you inquire or ask for that analysis so that you would understand why the Thiokol analysis was an incorrect one, or at least incorrect in the opinion of Mr. Mulloy and Mr. Hardy?

MR. REINARTZ: I think, Dr. Walker, that the discussion as I understood was centered around the ability of the primary O-ring to seal and that blow-by might occur in the timing. I think Mr. Hardy, where he talked about the effects, said there could be several reasons why you might be able to lose, I think, in response to a proposed failure scenario that was suggested about the black puff of smoke, saying that there could be other things that might have caused a defectiveness on the ring, that it was not just temperature-originated.

And those other defects, yes, sir, I was aware that there could be possible other defects that might cause that.

DR. WALKER: So were you satisfied, then, that there was enough understanding of the causes of blow-by so that you weren't really concerned and you were able to feel that the temperature problem raised by Thiokol could be laid to rest because you understood what was

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going on well enough to know that that temperature concern was really not a legitimate one?

MR. REINARTZ: The conclusion that the temperature was not a major factor and that we would not see increased launch risk as a result of that, yes, sir, that was my conclusion.

DR. KEEL: Mr. Lovingood, you were the one who was actually contacted by the resident manager of Marshall at Kennedy to set up the early afternoon telecon, is that right?

DR. LOVINGOOD: That's correct.

DR. KEEL: And then you and Mr. Reinartz both participated in that early afternoon teleconference?

DR. LOVINGOOD: Yes. Stan was in the motel.

DR. KEEL: But Mr. Mulloy and Mr. Hardy did not?

DR. LOVINGOOD: Mr. Hardy was at home and had a very bad connection. In fact, a lot of what I did in that telecon was I was trying to relay information from what was being said at Wasatch to Hardy, and then what Hardy said back to Wasatch guys.

DR. KEEL: So Hardy was on it, but had a poor connection?

DR. LOVINGOOD: Yes, he had a very bad

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connection.

DR. KEEL: And Mr. Mulloy was not on it?

DR. LOVINGOOD: That is correct.

DR. KEEL: You just indicated earlier that, based upon that teleconference, you thought there was a good possibility of delay. Is that what Thiokol was recommending then, was delay?

DR. LOVINGOOD: That is the way I heard it, and they were talking about the 51-C experience and the fact that they had experienced the worst case blow-by as far as the arc and the soot and so forth. And also, they talked about the resiliency data that they had.

So it appeared to me—and we didn't have all of the proper people there. That was another aspect of this. It appeared to me that we had better sit down and get the data so that we could understand exactly what they were talking about and assess that data.

And that is why I suggested that we go ahead and have a telecon within the center, so that we could review that.

DR. KEEL: So as early as after that first afternoon conference at 5:45, it appeared that Thiokol was basically saying delay; is that right?

DR. LOVINGOOD: That is the way it came across to me. I don't know how other people perceived it, but

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that's the way it came across to me.

DR. KEEL: Mr. Reinartz, how did you perceive it?

MR. REINARTZ: I did not perceive it that way. I perceived that they were raising some questions and issues which required looking into by all the right parties, but I did not perceive it as a recommendation to delay.

DR. KEEL: Some prospects for delay?

MR. REINARTZ: Yes, sir, that possibility is always there.

DR. KEEL: Did you convey that to Mr. Mulloy and Mr. Hardy before the 8:15 conference?

MR. REINARTZ: Yes, I did. And as a matter of fact, we had a discussion. Mr. Mulloy was just out of communication for about an hour, and then after that I got in contact with him and we both had a short discussion relating to the general nature of the concerns with Dr. Lucas and Mr. Kingsbury at the motel before we both departed for the telecon that we had set up out at the Cape.

DR. KEEL: But based upon that, Mr. Lovingood, that impression, you thought it was a significant enough possibility that Mr. Aldrich should have been contacted?

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DR. KEEL: In addition, did you recommend that Mr. Lucas, who is director of Marshall, of course, and Mr. Kingsbury, who is Mr. Hardy's boss, participate in the 8:15 conference?

DR. LOVINGOOD: Yes, I did.

DR. KEEL: And you recommended that to whom?

DR. LOVINGOOD: I believe I said that over the net. I said that I thought we ought to have an inter-center meeting involving Dr. Lucas and Mr. Kingsbury, and then plan to go on up the line to Level II and Level I.

And then it was after we broke off that first telecon I called Stan at the motel and told him that he ought to go ahead and alert Arnie to that possibility.

DR. KEEL: And Mr. Reinartz, you then visited the motel room of Mr. Lucas with Mr. Kingsbury, and also was Mr. Mulloy with you then?

MR. REINARTZ: Yes, he was, sir. In the first couple of minutes I believe I was there by myself, and then Mr. Mulloy joined us.

DR. KEEL: And did you discuss with them Mr. Lovingood's recommendation that the two of them, Lucas and Kingsbury, participate?

MR. REINARTZ: No, sir. I don't recall

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discussing Mr. Lovingood's recommendations. I discussed with them the nature of the telecon, the nature of the concerns raised by Thiokol, and the plans to gather the proper technical support people at Marshall for examination of the data.

And I believe that was the essence of the discussion.

CHAIRMAN ROGERS: But you didn't recommend that the information be given to Level II or Level I?

MR. REINARTZ: I don't recall that I raised that issue with Dr. Lucas. I told him what the plans were for proceeding. I don't recall, Mr. Chairman, making any statement regarding that.

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CHAIRMAN ROGERS: Thank you very much, Mr. Lovingood and Mr. Reinartz.

DR. LOVINGOOD: Sir, could I make a brief statement concerning the telecon?

CHAIRMAN ROGERS: Sure.

DR. LOVINGOOD: I would like to read this into the record to give my point of view of the 8:30 telecon.

CHAIRMAN ROGERS: Yes, please do, and I didn't realize that you had something you wanted to submit, but go right ahead.

DR. LOVINGOOD: I did not perceive during the discussion of January 27 anyone demanding or even requesting that Morton Thiokol prove to NASA that it was not safe to fly. The presentation by Morton Thiokol was intended to convey data relating to the effect low temperatures might have on the O-rings sealing, and the ensuing discussions were perceived by me to be an attempt to understand precisely what they were saying. This approach is characteristic of all NASA-contractor discussions, regardless of the nature of the issue or the nature of the data being presented.

As Mr. Mulloy has testified, we had previously established a rationale for safe flight in the light of O-ring erosion and blow-by. I was listening intently to

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the discussions to determine if the data presented that night altered that rationale. However, the bottom line had to be the following. If the decision was made to fly, a rationale for safe flight, including the new information we had, would have to be stated and assessed. I believe it was a consensus among NASA personnel involved, and certainly it was my position, that if Morton Thiokol recommended we not launch, we should not launch.

The rationale for safe flight was stated over the network by Mr. Mulloy, as he has testified. After Morton Thiokol made their recommendation not to fly, Mr. Hardy supported Mulloy and stated why, and of course, he has testified to that, why in his opinion the rationale was still valid in light of the MTI data. There was no pressure on MTI to change their recommendation. After Mulloy and Hardy had spoken, Mr. Reinartz, in what I perceived to be a very polite tone of voice, asked Joe Kilminster if he wanted to respond to Mulloy's and Hardy's comments. At this time Kilminster asked for the five-minute caucus.

After the caucus, Kilminster presented the MTI recommendation to launch. I understood the MTI position being that even if the data concerning cold temp effects on the O-rings were correct, the risk of a slow primary

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O-ring was increased, but the risk was not substantially different from STS 51-C. The key to the rationale for safe flight was that if the primary ring experienced blow-by during ignition, the secondary seal would experience the pressure and would seal prior to joint rotation, and that has been elaborated on quite a bit, I think.

MR. SUTTER: There is a waiver saying don't depend on the secondary seal. Within all the NASA ground rules for launch, how could you accept that rationale?

DR. LOVINGOOD: Excuse me, sir. Let me try to explain the way I understand that waiver, and there have been a lot of people doing that.

The waiver says there are two conditions you have to have before you don't have redundancy. One of them is what I call a spatial condition which says that the dimensional tolerances have to be such that you get a bad stackup, you don't have proper squeeze, etc., on the O-ring, so that when you get joint rotation, you will lift the metal surfaces off the O-ring. All right, that's the one condition, and that is a worst case condition involving dimensional tolerances.

The other condition is a temporal condition which says that you have to be past the point of joint

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rotation, and of course, that relates back to what I just said, the joint has to rotate.

So first of all, if you don't have this bad stackup, then you have full redundancy. Now, secondly, if you do have the bad stackup, you have redundancy during the ignition transient up to the 170 millisecond point or 300 millisecond point, whatever it is, but that is the way I understand the CIL.

MR. SUTTER: That is not what I understand a waiver means.

CHAIRMAN ROGERS: Go ahead.

DR. LOVINGOOD: I would like to emphasize that in my opinion NASA was asking itself and MTI whether there was any reason not to expect a safe flight. As far as I was aware at the time, everyone concluded that it was safe to proceed.

Now, I would like to add to that: I would like to respond to the question that you asked Mr. Reinartz concerning would I have concluded at the end of the first, at the caucus, at the beginning of the caucus, would I have concluded that the recommendation by Thiokol engineering was unanimous? I would not have. I have been working for NASA almost 24 years, and I have seen many occasions where there have been differences within our own engineering within

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NASA. I was in engineering. I was in Science and Engineering for 18 years before coming to the project office; many occasions where there have been differences of opinion looking at the same data, different conclusions. I have seen differences between contractors, and in particular, I worked very closely with Rocketdyne as Engine Project Manager, and I have seen many occasions where the Rocketdyne engineers and the NASA engineers looked at the same data and reached different conclusions.

So in fact, since Thiokol was recommending not to launch, I would not have concluded that their engineering was unanimously opposed to launch. On the other hand, when they came back to launch, perhaps incorrectly I concluded that they were all for it, that it was unanimous. I had no reason to believe otherwise. And I never did know that they weren't unanimous until after the launch.

CHAIRMAN ROGERS: Okay, thank you very much.

Unless there are questions, we appreciate it.

DR. KEEL: If we could have Mr. Stevenson, Mr. Davis and Colonel Kolczynski.

(Witnesses sworn.)

CHAIRMAN ROGERS: Gentlemen, we understand that you three have been designated or were designated

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as the Ice Team that worked at Kennedy Space Center, and worked in connection with the launch of the Challenger.

Would you give the Commission some knowledge about yourselves and how you operated, and then tell us in narrative form what happened on the day—well, on the 27th and 28th, if you will. Did you start working on the 28th on this or did you work on the 27th, too?

STATEMENT TO THE
PRESIDENTIAL COMMISSION
ON THE
SPACE SHUTTLE CHALLENGER ACCIDENT

S. R. REINARTZ

FEBRUARY 25, 1986

[Ref. 2/26-7 1 of 5]

February 25, 1986

MR. CHAIRMAN, MEMBERS OF THE COMMISSION

I am Stanley Reinartz, Manager of the Marshall Space Flight Center's Shuttle Projects Office. This office includes the Solid Rocket Booster, External Tank, and the Space Shuttle Engine Projects.

I appreciate this opportunity to present to the Commission data concerning, as I understand it, two points of today's hearing to the Commission. The first is the decision process that was used in arriving at a launch recommendation and the second is the basis for the launch recommendation.

My statement will cover primarily my recollections of the decision process and the summary basis for the launch recommendation emanating from a January 27, 1986, telecon between Morton-Thiokol Wasatch and MSFC elements. I propose that this be followed by Mr. Larry Mulloy, the Level III MSFC SRB Project Manager, who will elaborate on both points, and Mr. George Hardy, Deputy Director of the MSFC Science and Engineering Directorate, who will detail the MSFC engineering considerations and the rationale for agreement with the Morton-Thiokol launch recommendation provided by Mr. Joe Kilminster, Vice President and Manager, Shuttle Projects (Wasatch).

In regard to the launch recommendation process, I continue to believe that all activities associated with this process, specifically the January 27 discussion between Thiokol and MSFC, were conducted in a thorough and professional manner, in the NASA tradition, with full and open participation of personnel necessary for appropriate disposition of this specific concern. In the case of STS 51-L, a concern was raised by Thiokol the evening of January 27, the night before the planned launch, concerning possible effects of the predicted temperatures on the Solid Rocket Motor case to case field joint O-rings.

After this concern was raised, an orderly and normal process was set in motion to assure full examination of the concern and the engineering data and rationale associated with this concern.

First was identification of the concern during a preliminary telecon early in the evening and a request for Thiokol to provide written data related to the concern.

The second step was to assemble all the necessary personnel at MSFC, Thiokol, Wasatch in Utah and those senior MSFC and

[Ref. 2/26-7 2 of 5]

Thiokol project representatives at KSC and establish good telecon communications. The senior 51-L Thiokol representative at KSC was Mr. Al McDonald, Manager of the SRM Project. Mr. Mulloy and I were the senior MSFC project representatives on site.

Then, in approximately a two-hour meeting, Thiokol engineering presented and discussed material related to their concern; MSFC thoroughly probed the data; and, all parties had an opportunity to provide inputs and express their views.

At the end of a 2-1/2 hour period, including a 30-35 minute off-the-loop Thiokol caucus and after Thiokol's recommendation to launch, I collectively asked all telecon parties if there were any disagreements with Thiokol's rationale and recommendation as stated by Mr. Kilminster. There were none received from Thiokol at Wasatch, MSFC at Huntsville, nor Mr. McDonald sitting with Mr. Mulloy and me at KSC.

Thiokol was then asked to document their verbal rationale and launch recommendation statement as is our normal practice.

Based on the process I described and the conclusions reached as a result of that process, I concurred with the decision of the Level III Project Manager (Mr. Mulloy) supporting the launch recommendation and continuing with the launch process.

Before going into my post decision activities, I would like to amplify one portion of the above process.

During the January 27 post-scrub discussion, all MSFC support elements and to my knowledge the contractor representatives at KSC were present on the launch center MSFC voice loop when I made a request for capability to support a 24-hour turnaround, recognizing the colder predicted temperatures for the January 28th launch. The senior Thiokol program representative, who has a duty station on the loop at KSC, did not provide any input on the loop or, to my knowledge, to the physically adjacent MSFC SRB representatives, regarding any items that should be looked into while proceeding with launch preparations.

[Ref. 2/26-7 3 of 5]

During the 2-1/2 hour telecon between Thiokol and MSFC, I would characterize the presentations and associated discussions on the loop as deliverate and intese, and professional engineering examination of the data - not as highly heated or emotional.

No heated protest was injected into the open discussion by the senior Thiokol representative at KSC in the 2-1/2 hour telecon.

However, the senior Thiokol representative at KSC did inject one significant comment just after Mr. Kilminster asked for a caucus at about the 2-hour juncture in the discussion. This comment by the senior Thiokol representative at KSC was perceived, I believe by all parties, as a supporting point for a positive launch recommendation. The senior Thiokol representative said that Mr. Kilminster should consider a point, made by Mr. Hardy earlier, that the secondary O-ring is in the proper position to seal if blow-by the primary O-ring occurred. The importance of this point will be discussed later by Mr. Hardy.

MSFC elements in Huntsville and those MSFC elements at KSC had no knowledge of the internal Thiokol discussions during the 30-35 minute Thiokol caucus that preceded their launch recommendation. To my knowledge, the senior Thiokol representative at KSC did not take this opportunity to inject any of his thoughts or concerns via a private telephone input, into the internal Thiokol discussions at Wasatch.

As stated earlier, when I asked all parties collectively if there were any disagreements with the Thiokol launch recommendation, there was no statement or comment from the senior Thiokol representative sitting with Mr. Mulloy and me at KSC.

Now I would like to turn very briefly to my actions after the conclusions of this telecon...preceded by a short illustration of another STS 51-L launch issue that illustrates the Level III/Level II interaction.

Since we had been informed of a launch recovery area sea state that had forced the recovery ships to move away from their prescribed launch area, Mr. Mulloy, Mr. Houston, and I contacted the KSC operations personnel (Mr. Gene Sestile) and Mr. Aldrich (National Space Transportation System Program Manager) to advise Mr. Aldrich of the recovery area situation and potential SRB impacts. Since the ships would not be in position at launch time, Mr. Mulloy stated there was a good probability of loss of

[Ref. 2/26-74 of 5]

the two SRB Parachute systems, the nose cones and frustums. They would probably disperse before the ships could arrive. After discussion of the issue Mr. Aldrich concluded that the launch should proceed.

I have included this Level III/Level II issue resolution process to illustrate that where there is a Level III activity that can not be dispositioned within Level III authority and responsibility, then Level II is informed and a decision is made by Level II.

I would now like to return to the post-decision timeframe and summarize my actions: In consultation with the SRB Project Manager, I concluded that there was:

- No launch Commit Criteria violation
- No Level II requirement violation that required a waiver.
- No disagreement concerning the Thiokol launch recommendation between any Level III elements.
- That Thiokol and MSFC had fully and openly examined the concern and satisfactorily dispositioned that concern.

Based on the above conclusions, we informed the MSFC Director and the MSFC Director of Science and Engineering early on the 28th of January (about 5:00 a.m.) of the initial Thiokol concerns and engineering recommendations, the final Thiokol launch recommendation, and the full support of MSFC engineering for the launch recommendations that led to a successful resolution of this concern.

In conclusion, I believe that the total Thiokol and MSFC teams performed in a responsible manner in a continuation of the dedicated professional approach by which all issues involving the Space Shuttle are addressed and resolved through an engineering and managerial assessment of the data to determine that a vehicle is ready and safe to fly. I believe the process was appropriate and that the concern was dispositioned in a proper manner. I did not perceive, nor did I personally exert, any pressure on Thiokol.

At this time, we do not know the precise cause or causes of the STS-51L accident, but I can assure the Commission that we are diligently pursuing all aspects of this matter to arrive at a full understanding of the failure, its causes, and the required corrections.

Mr. Mulloy and Mr. Hardy will go into additional detail concerning these issues.

Mr. Chairman: This concludes my statement.

[Ref. 2/26-7 5 of 5]

National Aeronautics and
Space Administration

George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama
35812

NASA

February 20, 1986

Reply to Attn of: SAC-SA-035-86

TO: SMIB/James C. Harrington

FROM: SA01/Judson A. Lovingood

SUBJECT: Personal Documentation

Enclosed is the information you requested concerning the activities relating to the SRM field joint seals on the 51-L launch.

Judson A. Lovingood

[Ref. 2/26-8 1 of 12]

THIS NOTE PREPARED JANUARY 31, 1986

NOTES FROM JANUARY 27, 1986

4:25 p.m. - CST - Huntsville
I received a call from Cecil Houston. Houston had attempted to call Reinartz at his motel in Florida with respect to MTI's (Al McDonald) concern about launching January 28 at 8:36 a.m. because of the overnight temperature prediction and the temperature prediction at launch time. The concern related to the temperature effect on the o-rings at field joints. Cecil wanted to establish a telecon with Reinartz, Mulloy, Hardy, and MTI Wasatch Division to discuss the issue.

I told Cecil to proceed with setting up the telecon and locate Mulloy; I would call Reinartz. I either had my secretary or LIEF Board to get Reinartz on the phone at his motel.

I called Keith Coates in the Science and Engineering Directorate (S&E) to question him about the issue; I was interrupted by Reinartz on the phone waiting to speak to me; I informed Reinartz of the issue and proposed that he get Jim Kingsbury (Director of S&E) to participate in the telecon as he had been closely following the o-ring work of MSFC and MTI.

Kingsbury didn't

I called Coates back and told him to get Jim Smith (SRB Chief Engineer) and come to my office.

I had my secretary call Larry Wear to my office and told Wear to get Center o-ring experts to my office.

4:45 p.m. Telecon
Attendees - my office

Myself	Coates
Smith	Brinton, MTI
Wear	Other MTI
Adams	

Hardy in Athens - Reinartz at motel - McDonald/Houston at KSC. Kilminster and others at MTI - Wasatch.

A data discussion by MTI engineering followed with MTI of the opinion we should delay the launch until noon or afternoon. Their concern was based on previous erosion experience from the January 1985 launch at which o-ring temperature at launch was calculated at 53°F. Specifically, they were concerned about o-ring resiliency at low temperatures. We asked MTI: "What is the criterion for launch in terms of o-ring temp, considering the projected overnight temperature?" Because of difficulty in following the discussion without detailed charts, I proposed to Reinartz another discussion later that night, at which time MTI should crisply define their position using charts and make their recommendation. I said I suggested this be a Center-only meeting, involving Lucas and Kingsbury, and plan to go to Level II if MTI recommended not launching. It was agreed to start another telecon at 7:15 p.m. CST. The 4:30 telecon was concluded at approximately 6:00 p.m. CST.

I called Reinartz at the motel and told him if MTI persisted in their recommendation at the 7:15 meeting, we should not launch. I suggested he advise Aldridge of our meeting to prepare him for having a meeting with Level I to inform them of a possible recommendation to delay.

7:15 p.m. meeting delayed until approximately 7:45 p.m. CST. Charts: MTI Engineering recommended we not launch unless o-ring temp prediction $\geq 53^{\circ}$F.

Mulloy argument based on previous rationale for flight. Hardy agreed with Mulloy and stated he doubted if difference in predicted temp of o-ring at launch and 53°F experience could have any effect beyond the effect of other parameters involved in o-ring sealing.

Reinartz asked Kilminster to respond to Mulloy and Hardy.

Kilminster requested 5-minute caucus.

After 15 to 30 min, Kilminster stated his summary - see notes on chart.

Reinartz asked if anyone disagreed. No disagreement. During night, MTI prepped new recommendation chart (attached).

Attendees - Rm. 411 - Bldg. 4202

- Deputy Director, S&E/George Hardy
- Associate Director of Engineering/Wayne Littles
- SRB Chief Engineer/Jim Smith
- SRM Project Manager/Larry Wear
- Deputy Manager, SRB Project/Frank Adams
- MTI/Boyd Brinton
- Structures and Propulsion, S&E/Ben Powers
- Office of Associate Director of Engineering, S&E/Kieth Coates
- Director, Materials and Processes, S&E/Bob Schwinghamer
- Deputy Director, Structures and Propulsion, S&E/John McCarty
- SRB Project Office/John Miller
- Others

KSC:

- Manager, Shuttle Projects Office/Stan Reinartz
- Manager, SRB Project/Larry Mulloy
- Manager, KSC Resident Office/Cecil Houston
- MTI/Al McDonald

[Ref. 2/26-8 3 of 12]

ADDENDUM - FEBRUARY 15, 1986.

Upon arriving at HOSC at approximately 7:00 a.m. CST on January 28, I informed Jack Lee of the meetings on the evening of January 27. I told him no detail except that Thiokol had at first recommended not launching because of their concern for low temperature effects on the o-rings and, after a Wasatch caucus, recommended we proceed to launch. I also informed him that Thiokol was to provide us, in writing, their recommendation to launch on January 28 at 8:36 a.m.

[Ref. 2/26-8 4 of 12]

3910-81

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

GEORGE C. MARSHALL SPACE FLIGHT CENTER

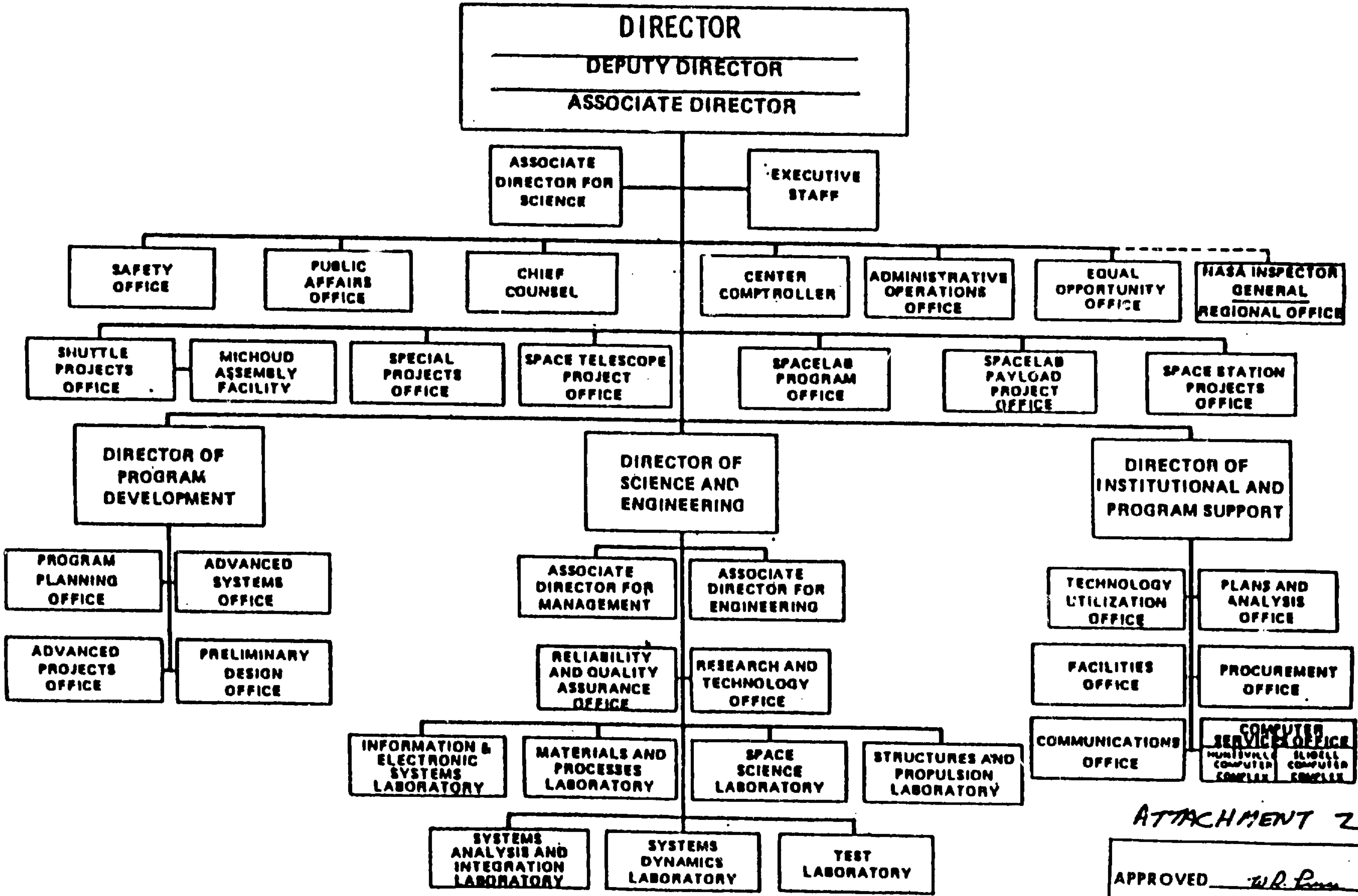

FIG VI-1

ATTACHMENT Z

APPROVED W.R. Lucas

W. R. LUCAS
DIRECTOR, MSFC

DATE 7-16-85

[Ref. 2/26-8 5 of 12]

Notes: ~~1:15~~ 9:30 Delayed telecon 1/31/86

showing my notes from 1/27 meeting GDD

Prior min temp at launch 50°

ORIGINAL PAGE IS OF POOR QUALITY

O-Ring squeeze

SRM 15 Jan 24
27° Avg 3 or 4 days
drops 52° at launch 53°
Heat x fan Joint 10° higher
Blow by primary - two on SRM 15
had erosion on field joint - soot past secondary

worst case

LH Fwd to Fwd Center ~~only secondary~~

RH Cnt to Cnt 2nd heat affected, soot blow by secondary
038" deep on primary
temp effects test ran
50F, 75F, 100F resiliency test - const load at given temp
time O-Ring respond - elasticity

72° storage
Qual program - 36° Ambient - 6 hr exposure
prior to static firing

46° —

Putty effect

Better putty ∴ Also ~~Packed or~~ filled all holes (after) during assembly and leak check

Blow By Tests - during actuation - room temp
No Blow By experienced

[Ref. 2/26-8 6 of 12]

60 noon Midnite 35 7:00 25 Noon 34

Criterion at 53°

MTI Recommends do not launch at predicted launch 23° to 32° F 25°F at 7:00

SRM-15 experience

.038" deep on primary
Secondary heat affected, soot blow by
only time secondary has experienced

Resiliency Tests
Time to respond increased by factor of 5 10 min to 2 sec
from 75°F to 50°F

[Ref. 2/26-8 7 of 12]

CONCLUSIONS:

- TEMPERATURE OF O-RING IS NOT ONLY PARAMETER CONTROLLING BLOW-BY

  SRM 15 WITH BLOW-BY HAD AN O-RING TEMP AT 53°F

  FOUR DEVELOPMENT MOTORS WITH NO BLOW-BY WERE TESTED AT O-RING TEMP OF 47° TO 52°F

  DEVELOPMENT and Qual MOTORS HAD PUTTY PACKING WHICH RESULTED IN BETTER PERFORMANCE
  DIFFERENT PUTTY

- AT ABOUT 50°F BLOW-BY COULD BE EXPERIENCED IN CASE JOINTS

  Lower TEMP ⇒ less Resiliency of O-Ring ⇒ More likely to Blow By

- TEMP FOR SRM 25 ON 1-28-86 LAUNCH WILL BE 29°F 9 AM
  38°F 2 PM

- HAVE NO DATA THAT WOULD INDICATE SRM 25 IS DIFFERENT THAN SRM 15 OTHER THAN TEMP

Attachment 4

RECOMMENDATIONS :

- O-RING TEMP MUST BE $\geq$ 53 °F AT LAUNCH

  DEVELOPMENT MOTORS AT 47° TO 52°F WITH
  PUTTY PACKING HAD NO BLOW-BY
  SRM 15 (THE BEST SIMULATION) WORKED AT 53 °F

- PROJECT AMBIENT CONDITIONS (TEMP & WIND)
  TO DETERMINE LAUNCH TIME

Corrigendum

Temp data not concl - Blow-by by hot and cold

Risk cold O-Ring Delay of prim O-ring to seat - opportunity for gas to get to secondary

Secondary O-ring seating after joint rotation

Rather Now gas by primary and secondary during start

Risks increased but not substantially diff from SRM 15

of 15 with 22 Pri: O-Ring 15 .038; if 2x longer for flight dura would expect 70-80 mil erosion; can be accomodated

P: [illegible] will take Longer time; If pri does not seal, sec will seal. [illegible] launch

Attachment 5

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[Ref. 2/26-8 9 of 12]

MTI ASSESSMENT OF TEMPERATURE CONCERN ON SRM-25 (51L) LAUNCH

0 CALCULATIONS SHOW THAT SRM-25 O-RINGS WILL BE 20° COLDER THAN SRM-15 O-RINGS

0 TEMPERATURE DATA NOT CONCLUSIVE ON PREDICTING PRIMARY O-RING BLOW-BY

0 ENGINEERING ASSESSMENT IS THAT:

- 0 COLDER O-RINGS WILL HAVE INCREASED EFFECTIVE DUROMETER ("HARDER")
- 0 "HARDER" O-RINGS WILL TAKE LONGER TO "SEAT"
  - 0 MORE GAS MAY PASS PRIMARY O-RING BEFORE THE PRIMARY SEAL SEATS (RELATIVE TO SRM-15)
    - 0 DEMONSTRATED SEALING THRESHOLD IS 3 TIMES GREATER THAN 0.038" EROSION EXPERIENCED ON SRM-15
- 0 IF THE PRIMARY SEAL DOES NOT SEAT, THE SECONDARY SEAL WILL SEAT
  - 0 PRESSURE WILL GET TO SECONDARY SEAL BEFORE THE METAL PARTS ROTATE
    - 0 O-RING PRESSURE LEAK CHECK PLACES SECONDARY SEAL IN OUTBOARD POSITION WHICH MINIMIZES SEALING TIME

0 MTI RECOMMENDS STS-51L LAUNCH PROCEED ON 28 JANUARY 1986

- 0 SRM-25 WILL NOT BE SIGNIFICANTLY DIFFERENT FROM SRM-15

Joe C. Kilminster

JOE C. KILMINSTER, VICE PRESIDENT
SPACE BOOSTER PROGRAMS

MORTON THIOKOL INC
Wasatch Division

INFORMATION ON THIS PAGE WAS PREPARED TO SUPPORT AN ORAL PRESENTATION AND CANNOT BE CONSIDERED COMPLETE WITHOUT THE ORAL DISCUSSION

ATTACHMENT 6

[Ref. 2/26-8 10 of 12]

National Aeronautics and
Space Administration

George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama
35812

NASA

SAC-SA-027-86

February 17, 1986

TO: MEMORANDUM FOR RECORD

FROM: SA01/Judson A. Lovingood

SUBJECT: Meetings on January 27, 1986, Regarding Low Temperature Effect on SRM O-Ring

Attached are my notes pertaining to the subject meetings.

Attachment 1: This record was prepared by me on January 31, 1986, on the advice of T. J. Lee that I should write down my recollection of events of January 27, 1986.

Attachment 2: This is a Marshall Space Flight Center Organization Chart which allows identification of the organizational elements of NASA personnel named in Attachment 1. The Shuttle Projects Office, as in the case of the other MSFC Project Offices, receives engineering support from the Science & Engineering Directorate through the Associate Director for Engineering Office. Each MSFC Shuttle Project element (External Tank, Main Engine, and Solid Rocket Booster) has a Chief Engineer, functionally reporting to the Project Manager, and institutionally reporting to the Associate Director of Engineering.

Attachment 3: These are my handwritten notes from the 4:45 p.m. CST, January 27, 1986, telecon.

Attachment 4: This is the conclusion chart presented at the approximately 7:45 p.m. CST telecon. The annotations shown are my notes at the time of the briefing.

[Ref. 2/26-8 11 of 12]

Attachment 5: This chart is the recommendation made by MTI prior to their caucus (the hand-lettering at the top of the page.) The script under Corrigendum are notes I wrote as Joe Kilminster presented the final MTI recommendation after the MTI caucus.

Attachment 6: This chart is the signed recommendation by MTI to proceed with the launch.

Judson A. Lovingood

Judson A. Lovingood

cc:
SA01/Stan Reinartz

[Ref. 2/26-8 12 of 12]

STATEMENT TO THE
PRESIDENTIAL COMMISSION
ON THE
SPACE SHUTTLE CHALLENGER ACCIDENT

S. R. REINARTZ
FEBRUARY 25, 1986

[Ref. 2/26-7 1 of 5]

February 25, 1986

MR. CHAIRMAN, MEMBERS OF THE COMMISSION

I am Stanley Reinartz, Manager of the Marshall Space Flight Center's Shuttle Projects Office. This office includes the Solid Rocket Booster, External Tank, and the Space Shuttle Engine Projects.

I appreciate this opportunity to present to the Commission data concerning, as I understand it, two points of today's hearing to the Commission. The first is the decision process that was used in arriving at a launch recommendation and the second is the basis for the launch recommendation.

My statement will cover primarily my recollections of the decision process and the summary basis for the launch recommendation emanating from a January 27, 1986, telecon between Morton-Thiokol Wasatch and MSFC elements. I propose that this be followed by Mr. Larry Mulloy, the Level III MSFC SRB Project Manager, who will elaborate on both points, and Mr. George Hardy, Deputy Director of the MSFC Science and Engineering Directorate, who will detail the MSFC engineering considerations and the rationale for agreement with the Morton-Thiokol launch recommendation provided by Mr. Joe Kilminster, Vice President and Manager, Shuttle Projects (Wasatch).

In regard to the launch recommendation process, I continue to believe that all activities associated with this process, specifically the January 27 discussion between Thiokol and MSFC, were conducted in a thorough and professional manner, in the NASA tradition, with full and open participation of personnel necessary for appropriate disposition of this specific concern. In the case of STS 51-L, a concern was raised by Thiokol the evening of January 27, the night before the planned launch, concerning possible effects of the predicted temperatures on the Solid Rocket Motor case to case field joint O-rings.

After this concern was raised, an orderly and normal process was set in motion to assure full examination of the concern and the engineering data and rationale associated with this concern.

First was identification of the concern during a preliminary telecon early in the evening and a request for Thiokol to provide written data related to the concern.

The second step was to assemble all the necessary personnel at MSFC, Thiokol, Wasatch in Utah and those senior MSFC and

[Ref. 2/26-7 2 of 5]

Thiokol project representatives at KSC and establish good telecon communications. The senior 51-L Thiokol representative at KSC was Mr. Al McDonald, Manager of the SRM Project. Mr. Mulloy and I were the senior MSFC project representatives on site.

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Then, in approximately a two-hour meeting, Thiokol engineering presented and discussed material related to their concern; MSFC thoroughly probed the data; and, all parties had an opportunity to provide inputs and express their views.

At the end of a 2-1/2 hour period, including a 30-35 minute off-the-loop Thiokol caucus and after Thiokol's recommendation to launch, I collectively asked all telecon parties if there were any disagreements with Thiokol's rationale and recommendation as stated by Mr. Kilminster. There were none received from Thiokol at Wasatch, MSFC at Huntsville, nor Mr. McDonald sitting with Mr. Mulloy and me at KSC.

Thiokol was then asked to document their verbal rationale and launch recommendation statement as is our normal practice.

Based on the process I described and the conclusions reached as a result of that process, I concurred with the decision of the Level III Project Manager (Mr. Mulloy) supporting the launch recommendation and continuing with the launch process.

Before going into my post decision activities, I would like to amplify one portion of the above process.

During the January 27 post-scrub discussion, all MSFC support elements and to my knowledge the contractor representatives at KSC were present on the launch center MSFC voice loop when I made a request for capability to support a 24-hour turnaround, recognizing the colder predicted temperatures for the January 28th launch. The senior Thiokol program representative, who has a duty station on the loop at KSC, did not provide any input on the loop or, to my knowledge, to the physically adjacent MSFC SRB representatives, regarding any items that should be looked into while proceeding with launch preparations. [Ref. 2/26-7 3 of 5]

During the 2-1/2 hour telecon between Thiokol and MSFC, I would characterize the presentations and associated discussions on the loop as deliverate and intese, and professional engineering examination of the data - not as highly heated or emotional.

No heated protest was injected into the open discussion by the senior Thiokol representative at KSC in the 2-1/2 hour telecon.

However, the senior Thiokol representative at KSC did inject one significant comment just after Mr. Kilminster asked for a caucus at about the 2-hour juncture in the discussion. This comment by the senior Thiokol representative at KSC was perceived, I believe by all parties, as a supporting point for a positive launch recommendation. The senior Thiokol representative said that Mr. Kilminster should consider a point, made by Mr. Hardy earlier, that the secondary O-ring is in the proper position to seal if blow-by the primary O-ring occurred. The importance of this point will be discussed later by Mr. Hardy.

MSFC elements in Huntsville and those MSFC elements at KSC had no knowledge of the internal Thiokol discussions during the 30-35 minute Thiokol caucus that preceded their launch recommendation. To my knowledge, the senior Thiokol representative at KSC did not take this opportunity to inject any of his thoughts or concerns via a private telephone input, into the internal Thiokol discussions at Wasatch.

As stated earlier, when I asked all parties collectively if there were any disagreements with the Thiokol launch recommendation, there was no statement or comment from the senior Thiokol representative sitting with Mr. Mulloy and me at KSC.

Now I would like to turn very briefly to my actions after the conclusions of this telecon...preceded by a short illustration of another STS 51-L launch issue that illustrates the Level III/Level II interaction.

Since we had been informed of a launch recovery area sea state that had forced the recovery ships to move away from their prescribed launch area, Mr. Mulloy, Mr. Houston, and I contacted the KSC operations personnel (Mr. Gene Sestile) and Mr. Aldrich (National Space Transportation System Program Manager) to advise Mr. Aldrich of the recovery area situation and potential SRB impacts. Since the ships would not be in position at launch time, Mr. Mulloy stated there was a good probability of loss of [Ref. 2/26-7 4 of 5] the two SRB Parachute systems, the nose cones and frustums. They would probably disperse before the ships could arrive. After discussion of the issue Mr. Aldrich concluded that the launch should proceed.

I have included this Level III/Level II issue resolution process to illustrate that where there is a Level III activity that can not be dispositioned within Level III authority and responsibility, then Level II is informed and a decision is made by Level II.

I would now like to return to the post-decision timeframe and summarize my actions: In consultation with the SRB Project Manager, I concluded that there was:

- o No launch Commit Criteria violation
- o No Level II requirement violation that required a waiver.
- o No disagreement concerning the Thiokol launch recommendation between any Level III elements.
- o That Thiokol and MSFC had fully and openly examined the concern and satisfactorily dispositioned that concern.

Based on the above conclusions, we informed the MSFC Director and the MSFC Director of Science and Engineering early on the 28th of January (about 5:00 a.m.) of the initial Thiokol concerns and engineering recommendations, the final Thiokol launch recommendation, and the full support of MSFC engineering for the launch recommendations that led to a successful resolution of this concern.

In conclusion, I believe that the total Thiokol and MSFC teams performed in a responsible manner in a continuation of the dedicated professional approach by which all issues involving the Space Shuttle are addressed and resolved through an engineering and managerial assessment of the data to determine that a vehicle is ready and safe to fly. I believe the process was appropriate and that the concern was dispositioned in a proper manner. I did not perceive, nor did I personally exert, any pressure on Thiokol.

At this time, we do not know the precise cause or causes of the STS-51L accident, but I can assure the Commission that we are diligently pursuing all aspects of this matter to arrive at a full understanding of the failure, its causes, and the required corrections.

Mr. Mulloy and Mr. Hardy will go into additional detail concerning these issues.

Mr. Chairman: This concludes my statement.

[Ref. 2/26-7 5 of 5]

National Aeronautics and
Space Administration

George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama
35812

NASA

February 20, 1986

Reply to Attn of: SAC-SA-035-86

TO: SMIB/James C. Harrington

FROM: SA01/Judson A. Lovingood

SUBJECT: Personal Documentation

Enclosed is the information you requested concerning the activities relating to the SRM field joint seals on the 51-L launch.

Judson A. Lovingood

[Ref. 2/26-8 1 of 12]

THIS NOTE PREPARED JANUARY 31, 1986

NOTES FROM JANUARY 27, 1986

4:25 p.m. - CST - Huntsville
I received a call from Cecil Houston. Houston had attempted to call Reinartz at his motel in Florida with respect to MTI's (Al McDonald) concern about launching January 28 at 8:36 a.m. because of the overnight temperature prediction and the temperature prediction at launch time. The concern related to the temperature effect on the o-rings at field joints. Cecil wanted to establish a telecon with Reinartz, Mulloy, Hardy, and MTI Wasatch Division to discuss the issue.

I told Cecil to proceed with setting up the telecon and locate Mulloy; I would call Reinartz. I either had my secretary or LIEF Board to get Reinartz on the phone at his motel.

I called Keith Coates in the Science and Engineering Directorate (S&E) to question him about the issue; I was interrupted by Reinartz on the phone waiting to speak to me; I informed Reinartz of the issue and proposed that he get Jim Kingsbury (Director of S&E) to participate in the telecon as he had been closely following the o-ring work of MSFC and MTI. Kingsbury didn't

I called Coates back and told him to get Jim Smith (SRB Chief Engineer) and come to my office.

I had my secretary call Larry Wear to my office and told Wear to get Center o-ring experts to my office.

4:45 p.m. Telecon
Attendees - my office

Myself	Coates
Smith	Brinton, MTI
Wear	Other MTI
Adams	

Hardy in Athens - Reinartz at motel - McDonald/Houston at KSC. Kilminster and others at MTI - Wasatch.

A data discussion by MTI engineering followed with MTI of the opinion we should delay the launch until noon or afternoon. Their concern was based on previous erosion experience from the January 1985 launch at which o-ring temperature at launch was calculated at 53°F. Specifically, they were concerned about o-ring resiliency at low temperatures. We asked MTI: "What is the criterion for launch in terms of o-ring temp, considering the projected overnight temperature?" Because of difficulty in following the discussion without detailed charts, I proposed to Reinartz another discussion later that night, at which time MTI should crisply define their position using charts and make their recommendation. I

[Ref. 2/26-8 2 of 12]

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said I suggested this be a Center-only meeting, involving Lucas and Kingsbury, and plan to go to Level II if MTI recommended not launching. It was agreed to start another telecon at 7:15 p.m. CST. The 4:30 telecon was concluded at approximately 6:00 p.m. CST.

I called Reinartz at the motel and told him if MTI persisted in their recommendation at the 7:15 meeting, we should not launch. I suggested he advise Aldridge of our meeting to prepare him for having a meeting with Level I to inform them of a possible recommendation to delay.

7:15 p.m. meeting delayed until approximately 7:45 p.m. CST. Charts: MTI Engineering recommended we not launch unless o-ring temp prediction $\geq 53^\circ$F.

Mulloy argument based on previous rationale for flight. Hardy agreed with Mulloy and stated he doubted if difference in predicted temp of o-ring at launch and 53°F experience could have any effect beyond the effect of other parameters involved in o-ring sealing.

Reinartz asked Kilminster to respond to Mulloy and Hardy.

Kilminster requested 5-minute caucus.

After 15 to 30 min, Kilminster stated his summary - see notes on chart.

Reinartz asked if anyone disagreed. No disagreement. During night, MTI prepped new recommendation chart (attached).

Attendees - Rm. 411 - Bldg. 4202
- Deputy Director, S&E/George Hardy
- Associate Director of Engineering/Wayne Littles
- SRB Chief Engineer/Jim Smith
- SRM Project Manager/Larry Wear
- Deputy Manager, SRB Project/Frank Adams
- MTI/Boyd Brinton
- Structures and Propulsion, S&E/Ben Powers
- Office of Associate Director of Engineering, S&E/Kieth Coates
- Director, Materials and Processes, S&E/Bob Schwinghamer
- Deputy Director, Structures and Propulsion, S&E/John McCarty
- SRB Project Office/John Miller
- Others

KSC:
- Manager, Shuttle Projects Office/Stan Reinartz
- Manager, SRB Project/Larry Mulloy
- Manager, KSC Resident Office/Cecil Houston
- MTI/Al McDonald

[Ref. 2/26-8 3 of 12]

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ADDENDUM - FEBRUARY 15, 1986

Upon arriving at HOSC at approximately 7:00 a.m. CST on January 28, I informed Jack Lee of the meetings on the evening of January 27. I told him no detail except that Thiokol had at first recommended not launching because of their concern for low temperature effects on the o-rings and, after a Wasatch caucus, recommended we proceed to launch. I also informed him that Thiokol was to provide us, in writing, their recommendation to launch on January 28 at 8:36 a.m.

[Ref. 2/26-8 4 of 12]

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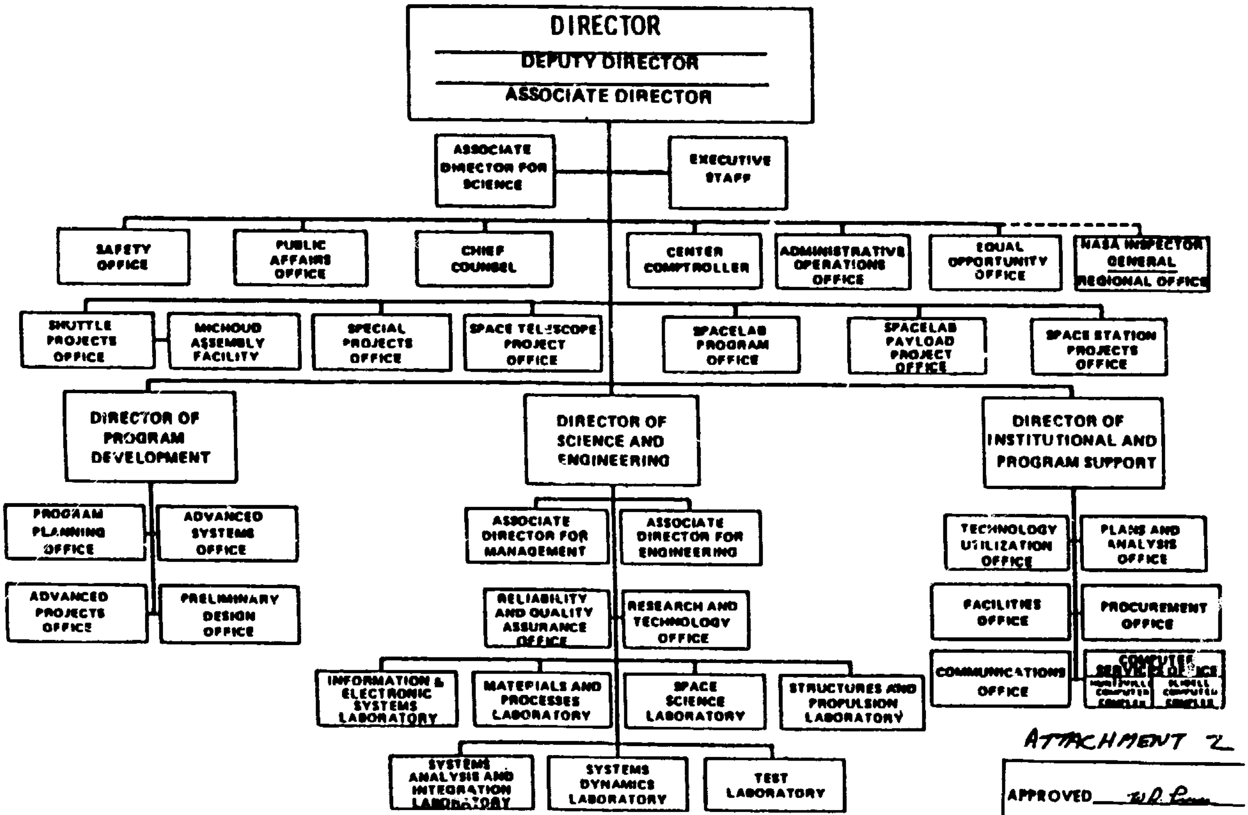
NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
GEORGE C. MARSHALL SPACE FLIGHT CENTER



ATTACHMENT 2

APPROVED W.R. Lucas
W. R. LUCAS
DIRECTOR, MSFC
DATE 7-16-85

[Ref. 2/26-8 5 of 12]

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Notes: ~~4:15~~ 4:30 Delegated tailor 1/31/86

Prior min temp at launch 50°

O-Ring squeeze

Thiokol's notes from 1/27 meeting

SRM 15 Jan 24
27° Avg 3 or 4 days
deriv 52° at launch 53°
Heat x fer Joint 10° higher
Blow by primary - two on SRM 15
had erosion on field joint - soot past secondary
LH Fwd to Fwd Center ~~Only secondary~~

worst case

RH Cnt to Cnt 2nd Seat affected, soot blow by secondary
.038" ero on primary
temp effects test run
50F, 75F, 100F resiliency test - const load at given temp
time O-Ring respond-elasticity

72° storage
Qual program - 36° Ambient - 6 hr exposure
prior to static firing

46° - Better putty? Also
Putty effect ~~packed~~ filled all holes (after) during assembly and leak check

Blow By Tests - during actuation - room temp
No Blow By experienced

Attachment 3
page 1 of 2

[Ref. 2/26-8 6 of 12]

60 am ... Mihalik 2's 7:00 25 Noon 34
Criticism at 53°
MTI Recommends do not launch at predicted launch 23° to 32° F 25°F at 7:00

SRM-15 experience

.038" deep on primary
Secondary heat affected, soot blow by only time secondary has experienced

Resiliency Tests
Time to respond increased by factor of 5 from 75°F to 50°F (10 min to 2 sec)

[Ref. 2/26-8 7 of 12]

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[Ref. 2/26-8 8 of 12]

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Rather than gas by primary and secondary during start
Risks increased but not substantially diff from SRM 15
Risks of 15 with 22 Pri O-Ring 15 .038 ; if 2x larger for flight demo would expect 70-80 mil erosion ; can be accommodated
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  - SRM-25 WILL NOT BE SIGNIFICANTLY DIFFERENT FROM SRM-15

Joe C. Kilminster

JOE C. KILMINSTER, VICE PRESIDENT
SPACE BOOSTER PROGRAMS

MORTON THIOKOL INC.
Wasatch Division

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ATTACHMENT 6

National Aeronautics and
Space Administration

George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama
35812

NASA

Reply to Attn of: SAC-SA-027-86

February 17, 1986

TO: MEMORANDUM FOR RECORD

FROM: SA01/Judson A. Lovingood

SUBJECT: Meetings on January 27, 1986, Regarding Low Temperature Effect on SRM O-Ring

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Attachment 6: This chart is the signed recommendation by MTI to proceed with the launch.

Judson A. Lovingood

Judson A. Lovingood

cc:
SA01/Stan Reinartz

[Ref. 2/26-8 12 of 12]

TESTIMONY OF CHARLES STEVENSON, AND B. K. DAVIS, ICE TEAM MEMBERS, AND LIEUTENANT COLONEL EDWARD F. KOLCZYNSKI, COMMANDER, DETACHMENT 11, 2d WEATHER SQUADRON, PATRICK AIR FORCE BASE

MR. STEVENSON: We did it both days.

Okay, I am Charles Stevenson, and I have been employed by NASA at the Kennedy Space Center since 1965. During the past 20 years I have been a member of the test checkout and launch team for all manned space flights.

CHAIRMAN ROGERS: Would you move the mike over a little bit?

MR. STEVENSON: Starting with the Gemini program and running through Apollo and into the Shuttle program. I am a graduate of North Carolina State University, and I have a degree in engineering mechanics, a B.S. degree, and a B.S. degree also in physics and applied mathematics. I am currently the section chief for the external tank and the solid rocket booster mechanical systems section.

In regards to the questions I am anticipating, I am also responsible for TPS, ice, frost and debris damage on the Shuttle.

MR. DAVIS: I am Billy K. Davis. I have been

1705

with NASA since 1960. I have a degree in mechanical engineering from the University of Alabama, and I started in the manufacturing/engineering area for Marshall Space Flight Center, and then systems engineering, and post that, I have been in the Chief Engineering Office for the External Tank, and in that area, I have been responsible for development of the manufacturing processes, the facilities and the other things that go with the external tank insulation system, and as a part of that, was instrumental in seeing to it that we got a development that could provide us with knowledge as to whether or not we would make ice, and at what time we would have ice, and how we could detect it.

And so, for that reason, for each launch I have been the senior test representative for the external tank and work with Mr. Stevenson in the ice and debris area.

CHAIRMAN ROGERS: Colonel Kolczynski, I think I mislabeled you as a member of the ice team. I understand that you are the Air Force weatherman, is that correct?

COLONEL KOLCZYNSKI: Yes, sir, that is true.

I am Lieutenant Colonel Edward F. Kolczynski. I am Commander of Detachment 11, 2d Weather Squadron at Patrick Air Force Base. In that capacity, I provide

1706

support to the Eastern Test Range, Eastern Space and Missile Center, Air Force Tactical Applications, and then, of course, to NASA in support of all Shuttle operations up to and including launch.

My expertise is I have been in the Air Force 19½ years, and for 18½ of those years I have been a meteorologist. I have a bachelor's degree in mathematics, a master's degree from Texas A&M in meteorology, and I've got some doctoral work at the University of Maryland, also in meteorology. My experience has been as a forecaster at Grand Forks Air Force Base for my early days in the Air Force, then as a staff meteorologist consulting on weather support to developing systems like the F-15 and the A-10 aircraft at Wright-Patterson Air Force Base. I worked at Studies and Analysis in the Pentagon in operations research doing force deployment issues, trade-off analyses and simulations. And of course, at Headquarters, Air Weather Service again at Studies and Analyses. And classified Special Projects. And finally a year at the Kennedy Space Center.

CHAIRMAN ROGERS: Thank you very much.

Mr. Stevenson, would you proceed? Do you have a statement or do you want to proceed to just narrate what happened on those two days?

1707

MR. STEVENSON: Okay, let me—I will do both.

As a TPS ice, frost and debris team we have really seven major activities, and I would just name the seven, and then I will get into the ones we are really interested in.

We initially do a pre-launch pad debris and vehicle familiarization walkdown to familiarize all the people with the possibilities of debris issues and to familiarize them with the latest configuration of the vehicle. That is for the advantage of some of the people who normally do not work the vehicle every day but come to us from off-Center.

We then conduct during the launch a T-3 hour ice/frost walkdown where again we look at the vehicle for ice/frost conditions, TPS anomalies, and any last minute debris that we may find.

Following launch, we immediately go to the pad again and do a postlaunch debris assessment. The purpose of this mainly is to determine if there is any flight hardware on the pad or any damage that may have occurred to the vehicle as a result of some pad debris.

We then do a postflight photo analysis in which we again look at the vehicle, that is, as it is launched through the launch film, to see if there is any

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damage to the vehicle. We also review the SRB for postflight debris assessment, and we review the orbiter once it returns.

In regards to the launch day activities, I guess I should just read a statement.

CHAIRMAN ROGERS: Okay.

MR. STEVENSON: The members of the ice/frost team made three assessments of the icing conditions on the FSS, the RSS, the MLP deck and Pad B apron during the STS-33 prelaunch activities. Due to a drop in temperatures to below freezing during the preceding day and night, the freeze protection plan last used for STS-20 on January 24, 1985 was implemented to protect the various facility systems.

Two actions within the plan were intended to limit the ice debris which could potentially cause damage to the Shuttle vehicle during the launch. The first action involved adding 1450 gallons of antifreeze into the over-pressure water troughs.

CHAIRMAN ROGERS: Which day now are you speaking about, please?

MR. STEVENSON: I am talking about launch day.

CHAIRMAN ROGERS: January 28, launch day?

MR. STEVENSON: Yes.

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CHAIRMAN ROGERS: If you would speak a little bit more into the mike, I have just a bit of trouble hearing you.

MR. STEVENSON: Okay. The water troughs in both SRB holds have a total capacity of 6,580 gallons. The resulting antifreeze to water ratio is calculated to be at approximately 21.3 percent, which protected the water troughs from freezing down to an ambient temperature of 16 degrees.

The second action involved the draining, where practical, of all water systems. Several systems, such as the fiex, the deluge, the emergency shower and eye wash were not drained. These systems were opened slightly and allowed to trickle into the drains. The trickling water was found to cause the drains to overflow, and the high wind gusts spread the water over large areas, and it then froze.

Based upon those conditions, when we came into the firing room on the day of launch and had a call to stations, the ice, frost, TPS and debris team observed the icing conditions which were on the FSS and notified our upper management. A decision was made at that time to send the ice/frost team to the pad for an assessment of the facility icing conditions.

The team arrived at the pad at approximately

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0130 in the morning and remained there for approximately one and one half hours.

CHAIRMAN ROGERS: And who was the team?

MR. STEVENSON: The team at this time consisted of B. K. Davis, myself, and two facility members who were familiar with the water system.

CHAIRMAN ROGERS: Was that unusual to have you go to the pad at that time?

MR. STEVENSON: Yes, it is.

CHAIRMAN ROGERS: Had it ever been done before?

MR. STEVENSON: Once before when we had a similar condition.

CHAIRMAN ROGERS: Okay. Proceed.

MR. STEVENSON: Okay.

Upon arrival at the pad we noted the following conditions, and I will just more or less summarize those.

As far as the pad apron was concerned, ice was concentrated in an area under the RSS and covered approximately 3,000 square feet. The ice ranged in thickness from one fourth of an inch to about three inches. On the MLP deck itself, we had a sheet of ice mostly on the west side between the left hand SRB exhaust holes and the west side, or the east side of the

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FSS.

This ice—the sheet ice was approximately one-eighth inch thick, and the secondary overpressure water troughs, we found that we had ice which was estimated to be approximately one-half inch thick. And the density of that ice was estimated to be approximately 25 pounds per cubic foot.

As for the fixed service structure between the 100 and 220-foot levels, we had a large quantity of icicles which averaged approximately five-eighths inch in diameter and ranged from six inches to one foot long. We had ice on all panels, such as distribution panels, structural members of the facility, valve panels. This ice averaged approximately one-eighth to one-half inch thick.

Upon returning to the LCC, of course, we immediately held a meeting with our upper management, and members of the management system were probably Mr. Aldrich and Horace Lambreth, who is Director of Shuttle Engineering.

CHAIRMAN ROGERS: Mr. Aldrich and who else?

MR. STEVENSON: Horace Lambreth, Director of Shuttle Engineering, and several other key management people who I don't have the names of right off.

The results of that meeting was that we

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immediately started the engineering effort to calculate the trajectories of ice which would be falling off of the FSS. We concluded that the ice which we had seen in the water troughs, the overpressure troughs, would not be acceptable for launch based on our previous experience of debris coming out of the water troughs.

We decided to send a facility technician, group of technicians with us to the pad again when we went during our normal three-hour inspection, and we proceeded to make a second assessment during the T-3 hour hold on the conditions we found there, we left the LCC—

MR. HOTZ: What time did you go out on the pad the second time?

MR. STEVENSON: We arrived at the pad at 0654 in the morning, and we departed at 0844. Again, the team was augmented by several facility personnel to aid us in the removal of ice from the water troughs, which we had determined to be unacceptable for launch.

Our temperatures, our ambient temperatures as we recorded them on our consoles and found them during the time we were there ranged from 26.1 to 30.1 degrees Fahrenheit. The ice was found in the troughs to have thickened and was solid. These are the overpressure water troughs.

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A fish net was employed to break up the ice and remove it. Approximately 95 percent of the ice was removed using this method. The ice and unfrozen antifreeze solution was measured at that time with the infrared pyrometer and found to have a temperature between 8 degrees and 10 degrees Fahrenheit. Those temperatures have since been corrected to read between 14 and 16 degrees. Also most of the icicles that we had seen or reported earlier on the left hand SRB aft skirt were removed during this period.

As far as the FSS goes, the ice had increased, but the overall extent of the ice was generally the same.

Upon returning to the LCC, we immediately held a second meeting with about the same members present. We had at that time completed our trajectory calculations which predicted that the ice falling from the FSS, if it fell at ignition, would probably impact the MLP deck at a distance of approximately 20 feet from the FSS. These calculations did not include the effects of aspiration, which was unknown.

We had decided in the meeting that if we had ice falling away from the vehicle, that aspiration would not draw ice into the water trough holes, and therefore, that the ice on the FSS was not a safety of flight

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issue.

We proceeded back to the pad for a third inspection, and the reason we went back for the third inspection—and this would not normally be an inspection we would conduct—was to remove all of the ice off of the MLP deck on the west side of the MLP deck, which would have a potential for getting—being drawn in by aspiration or by vibration of the deck.

MR. HOTZ: Could you give us the times on that again?

MR. STEVENSON: That time was between 10:30 and approximately 11:00 a.m. We again carried the facility crew with us who aided us in the removal of all of the ice on the west part of the MLP deck away from the left hand SRB. The conditions of the ambient temperatures, as measured in the firing room, too, by our instrumentation for this period of time ranged from 34.8 to 36.2 degrees Fahrenheit. We also found that there was again a little bit of ice in the overpressure water troughs, and again, we fished that ice out, and we returned to the LCC at approximately T-20 minutes.

CHAIRMAN ROGERS: Your function was to make the inspections that you referred to and report back to Mr. Aldrich, is that it?

MR. STEVENSON: I report back normally to the

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launch director.

CHAIRMAN ROGERS: In this case, who was he?

MR. STEVENSON: In this case the launch director was Mr. Gene Thomas, but in our normal report we do it over the communications system, which also includes whatever top management.

CHAIRMAN ROGERS: But in this case who did you report to?

MR. STEVENSON: In this case, each time we held a special meeting off-line to discuss the ice issues and what we should be doing about it.

CHAIRMAN ROGERS: Was that because it was a little worse than previous launches?

MR. STEVENSON: Yes, sir.

CHAIRMAN ROGERS: Did the low temperature readings cause any concern among the ice team members?

MR. STEVENSON: You mean our scanner readings?

CHAIRMAN ROGERS: Yes.

MR. STEVENSON: Well, we took a considerable amount of readings using the IR scanner. We normally take a lot of readings using the IR scanner. We are charged with the responsibility of and use the IR scanner mainly to determine the temperatures on the ET skin because the ET surface is where we are really

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expecting ice, and the icing conditions, and that is really what we look for when we make an ice inspection is the ice that is normally on the tank that will cause damage to the orbiter during flight.

CHAIRMAN ROGERS: But in this case you did something more?

MR. STEVENSON: Well, we took the—in this case we took readings that we normally take. We take more readings than we are required to take, if that is your question.

CHAIRMAN ROGERS: Did you take readings on the SRBs?

MR. STEVENSON: Yes, sir.

CHAIRMAN ROGERS: Do you normally do that?

MR. STEVENSON: Yes, sir.

CHAIRMAN ROGERS: And were there differences in the readings between the left and the right booster?

MR. STEVENSON: Approximately 14 degrees delta.

CHAIRMAN ROGERS: And what were those readings?

MR. STEVENSON: The uncorrected readings were 9 degrees and 23 degrees. We have since scaled those up to 19 degrees and 33 degrees.

CHAIRMAN ROGERS: So the right was 19 degrees,

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you think, and the left was 33 degrees?

MR. STEVENSON: Yes, sir.

CHAIRMAN ROGERS: And was there any discussion about those readings with other members of the launch team?

MR. STEVENSON: With other members of the launch team, no, sir.

CHAIRMAN ROGERS: And what were those discussions?

MR. STEVENSON: We did not discuss those specific temperatures with other members of the launch team.

CHAIRMAN ROGERS: Did you report those temperatures to others?

MR. STEVENSON: No, sir, not those specific temperatures.

CHAIRMAN ROGERS: Why not?

MR. STEVENSON: Because, in our opinion, first—well, two reasons, I guess. Number one, the vehicle was operating within the red lines that we have, the guidelines that we have to go by, and when we go out and make our inspection, we are required to report on anomalies, number one, and having no anomalies, you report on the points of interest.

Since the vehicle is operating within its red

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lines, within my guidelines, within its Launch Commit Criteria, within the OMRSD requirements, that was not a point to report.

CHAIRMAN ROGERS: Mr. Davis, did I see you about to say something?

MR. DAVIS: Well, when you first asked the question earlier, did we discuss it among ourselves, Charlie and I did discuss it at the time and concluded that this 9-degree reading probably might not be exactly right, and felt that the set of readings that you get on the left-hand booster was more representative of what the true conditions were.

As far as the temperature readings taken on the tank itself, they followed fairly close to the predictions that we had, although right at the bottom the temperature readings were somewhat further than normal away from the predicted readings. They were colder, but they were not so much colder that it would cause any particular consternation or anything because our insulation is quite capable of working at liquid hydrogen temperatures, and in fact, you can immerse it in liquid hydrogen and it really doesn't do anything to it other than it just takes a while for it to get cold in the middle.

So as far as the tank was concerned, and all

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of the things that it normally would be expected to do, it was doing it in fact better than usual because we had ice in smaller amounts in places that we normally had a lot of ice, like interfaces between the orbiter and the tank and the interface between the solids and the tank. They had ice in the regular places but it was--and there are certain places where it is quite acceptable to have them. In this case we had much less than usual, and it can be attributed to the fact that there was no atmospheric moisture available to be forming on it.

CHAIRMAN ROGERS: In other words, the icing conditions were not as bad on this flight as on others?

MR. DAVIS: That is correct.

MR. STEVENSON: As far as the vehicle is concerned.

CHAIRMAN ROGERS: As far as the vehicle is concerned.

MR. STEVENSON: Right.

CHAIRMAN ROGERS: What about other aspects of the Shuttle?

MR. DAVIS: Do you mean like the orbiter?

CHAIRMAN ROGERS: Yes, everything else.

MR. DAVIS: Well, the temperatures, of course, were much colder than we have ever seen before. Some of the readings, like the temperature, for example, that I

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took on the side of the orbiter, were colder than I have ever seen, but the orbiter is quite capable of handling those kind of temperatures, too. In fact, if you leave the orbiter pointed in one direction during flight, it will get extremely cold if it is looking at the night sky. And so the actual readings that I got were not anything of any significance, but the fact that they were reading a low almost as cold as if it were on the side of the tank made me wonder if maybe I wasn't getting some effect from the night sky.

And so once we had recorded all of the data, I was prepared afterwards, you know, somebody would probably ask me whether those readings were really true or not, and all I could say at the time of the readings was that this was what the gun said, and it was possible then to go back and do something to find out what it really was, maybe.

CHAIRMAN ROGERS: You have talked about the orbiter and the external tank. You haven't mentioned the boosters.

MR. DAVIS: The booster, I have made a practice of measuring temperatures on the boosters simply because I had gotten into the habit of measuring the whole vehicle since other elements had asked for specific inputs because of changes that they were making

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or modifications or something, and so I said, why not make the record of the whole thing because, who knows, someday somebody might want to know, and somebody might be able to pick a trend out of it. And I've seen differences in temperature between the two boosters quite often.

The sun makes an extreme difference, and like one time, 118 degrees on one side and about 84 on the other, a 34 degree difference. And this didn't cause any problems. It was just something that I noted.

We don't try to report the details because, first of all, if you look at this drawing that we will give you a copy of, you will see that there are no readings on the right hand booster above the lower end of it, simply because I am unable to reach it, and I can't take the normal reading. And so I cannot trust those readings at all.

I know that they give you odd readings when you have a glancing angle with the Omegascope or with the other IR gun that we have used in the past. So for that reason we have always concentrated on what the differences were at the bottom and then take a general view of the side of the SRB.

There was one difference, this time, too. We had ice for about 30 feet up the side of the left hand

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solid. We had a small amount of ice. This is at T-3 hour inspection point, on the left hand wing. By the time that we were back out there at T-20 minutes, the ice on the wing had melted off and was gone completely. The ice on the SRB was still solid and was in the shade.

CHAIRMAN ROGERS: But that was the left?

MR. DAVIS: The left, yes, sir.

VICE CHAIRMAN ARMSTRONG: Glaze ice?

MR. DAVIS: Yes, sir.

VICE CHAIRMAN ARMSTRONG: And what do you attribute the source of that to be?

MR. DAVIS: It came from the FSS, the wind blowing moisture off of the FSS and hitting the side of the SRB.

DR. RIDE: Is that the sort of thing that you would report to the launch director? Was that reported?

MR. DAVIS: We did report that, yes, ma'am.

DR. COVERT: Did you have any idea what that ice weighed?

MR. DAVIS: Do you mean its density?

DR. COVERT: No, how much additional weight was on the vehicle due to the ice?

MR. DAVIS: It was about an eighth of an inch thick from what I could observe. So it couldn't have

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weighed very much, maybe 25 pounds or so.

DR. COVERT: Thank you.

MR. RUMMEL: In your opinion, would any propellant leaks in the ET have showed up on the infrared scanner?

MR. DAVIS: Yes, sir, they would have particularly in that I was looking at a small, let's see, rhombus shaped area that had a little bit more frost on it than the others in this general vicinity that everybody is now seeing the puff of smoke in, and at the time that I was taking a reading of that, which by the way, read minus 20, but I knew that it was a thin place, and we talked among ourselves about what was that representative of and concluded what did it, and there are other places on it, is where a gun is spraying insulation on will either slow down or else it will stop, and you pick a new gun up, and we have it built so that you have a redundancy in the operation. And so that was a slightly thinner place than in the other places, and you would expect it to be cold. And I took a scan in that general area on the tank. If we had had a hydrogen leak, it would have gone off scale.

In fact, if I look at the overboard bleed on the engines, it goes off scale.

MR. RUMMEL: I take it, then, you are fairly

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satisfied that no leak was apparent at that time?

Was this scan coverage quite complete do you think?

MR. DAVIS: Well, in the area of the question as far as the smoke is concerned, it was very, very well covered because of this little anomaly that I was looking at there.

MR. RUMMEL: I didn't intend to limit the question to that area.

Did you see anything anywhere in the tank, or were you able to look at other places?

MR. DAVIS: I make a habit of taking a scan at the interface between the orbiter and the ET on both the liquid hydrogen and the liquid oxygen connection points, and if we had had a liquid hydrogen leak there, I would have seen it. We would have gone off scale then. If we had had an oxygen leak on the other side, I would have seen it, and it would have gone off scale.

VICE CHAIRMAN ARMSTRONG: I understand you take that through optics? Is that measurement taken through optics?

MR. DAVIS: Yes, sir. The IR gun is a thing that in this particular case the one we are using is an Omegascope, and it is a pistol grip type gun with a telescopic sight on it, and it actually looks through

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the center line of the view so that what you see in the scope is what you are actually measuring the temperature of.

VICE CHAIRMAN ARMSTRONG: And what percentage of the total vehicle are you able to scan?

MR. DAVIS: Well, I can answer that two ways. I can probably make a scan of about 40 percent of it, but I would say that we probably don't look at more than 10 percent.

MR. RUMMEL: Are those readings recorded?

MR. DAVIS: Yes, sir, they are.

MR. RUMMEL: Thank you.

MR. ACHESON: Is it a fact that that infrared gun must be exposed to the operating temperature for some length of time before it is accurate?

MR. DAVIS: Yes, sir. In fact, that's the reason for the corrections we made in the temperature readings. We were lucky in that we have a very good time line that we keep of when we leave where and what we do in between times, and we were able to reconstruct which—and I knew which one of the readings were taken first because I have set up a sort of an operating sequence that I always follow to try to keep from forgetting anything. And so we could account for within a minute or so when every reading was made, and then run

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the IR gun through a test chamber which we have set up to run at, say, 24 degrees, and watch it come down, and keep a record of every minute as we went through, and we were able to reconstruct the necessary corrections on it.

And one of the things that we felt we had to do on this before we could make a legitimate correction on it was to have the correction come out and match the water trough temperatures because we knew that the water in there had to melt at 16 degrees and it had to be solid at something below that, and an antifreeze solution is like solder in that it has to cool off. It is not like just plain pure water that would go from a liquid to a solid at the same temperature. It has to be colder.

And so the readings that I got from the solidly-frozen areas were in the 8-degree range, and the ones that had some water that I was looking at with the antifreeze solution in it were at 10 degrees. And so I just went through the timing and let the other gentlemen that ran the calibration tests for us give me the corrections, and I simply added them to it, and it came out that the cold ice part was 14 degrees and the melting part was 16 degrees, and we felt that that kind of pegged it pretty close to what was real.

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VICE CHAIRMAN ARMSTRONG: After you applied all of the corrections to the readings of the Omegascope and so on, what do you conclude the actual temperatures of the SRB skirt and so on were now?

MR. DAVIS: For the skirt—and I would like to talk about that just a little bit, but I will go ahead and give you the numbers first. The skirt corrects to 16 degrees; the solid motor case on—and this is on the right hand, to 19 degrees. Now, the left hand at the 95-foot or the 100-foot level, as they call it over on Pad B, corrected to 33 for the solid motor case and 36 for the aft skirt. And in looking back at pictures of the area—and we, by the way, also take representative pictures of different angles of this—there is a light television camera set that they follow us around while we are out there and try to keep up with what we are doing, and it was shining at the same time I was making the measurements on this area, and I am sure that we were getting some moisture from the leaking area behind us, and I believe I was reading water that was in the process of freezing and seeing some reflections of the lights in it so that we got, I believe, a slightly erroneous reading on the 23 and 25 degrees.

And the correction, then, is as erroneous as it was originally, and so it probably was on the order

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of the temperatures that we got up higher, like 28, 29, maybe 30 degrees at the bottom. And as I was about to say earlier, the skirt on the right hand booster was colder than the solids for two reasons. One is that it has a better night sky view angle, and another is that it is insulated on the inside, and it is a relatively thin skinned material whereas the insulation and other material inside the solid motor itself has got a much lower—I mean a much higher heat transfer capability than the insulation on the inside of the skirt.

VICE CHAIRMAN ARMSTRONG: Was the night sky you were looking at, was it a cloudy sky?

MR. DAVIS: No, sir, it was absolutely clear.

MR. HOTZ: Mr. Davis, how do you account for the difference between the ambient temperatures on the pad which you said were 34 to 36 degrees and these much colder temperatures down in the water trough, and at the field joint of the right solid booster which were 14 to 16 degrees in the water and 19 degrees on the booster?

MR. DAVIS: Okay, let's go back to the 33 and 36-degree reading that I mentioned earlier. What I was reading there was a reflection as well as the fact that I had water that was blowing off the fixed service structure on the side of the solid.

MR. HOTZ: Were these pyrometer readings?

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MR. DAVIS: No, the pyrometer readings gave me 23 and 25, but the corrected readings then said that it would have been reading 33 and 36.

MR. HOTZ: No, but what I mean is you were making all of these measurements with the Omegascope. You weren't using any normal thermometers or things like that.

MR. DAVIS: No, sir. Our ambient temperatures at this time, and we had several readings, and we do not use just the coldest that we get, we use a composite average for our calculations for the tank, were reading in the general vicinity of, say, 26 degrees.

Later, at launch time we were up to, I believe, 36 or 38 degrees, and as far as the temperature in the water troughs, we have attempted to explain it, and I am not sure that we have completely put that one to bed, but it appears that this is a real special case that we had in that we had extremely dry air, and it was extremely cold air, like about, say 25 or 26-degree air, and when it acts as a heat transfer material to warm the tank effectively because you are conduction limited through the insulation of the tank, you end up having a sensible heat change, or, to say it another way, the temperature of the air has to go down in order to furnish the heat through the heat leak into the hydrogen

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tank and into the oxygen tank. So this will give you a big change in temperature, and it will cause that cold air to fall down.

So we feel a part of the problem in trying to explain all of this came from the fact that cold air was running off the bottom of the tank much like cold air will run down the side of a Coke bottle or anything else that you have got cold around.

The other thing is that the wind was blowing just right so that it was blowing more on the right hand booster side, and we may have gotten a little bit of it over on that side, and that helps to account for the lower temperature on the right hand booster. And the other area that would account for part of it is night sky radiation. Night sky radiation tends to take either way. I am sure you have all noticed that quite often you will come out in the morning to find ice on your windshield when it never got below freezing the night before, and that is the phenomenon that you see here. And so for that reason we believe that that explains the water trough temperature, that part of it may have been simply that the air was running straight down, and then fanned out and ran over the top of the water, and it did freeze from the top first, and that the other would be it is looking directly at night sky, and so that would

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help to chill it also, and you also get some air motion over it which would tend to bring it down, too.

CHAIRMAN ROGERS: Mr. Davis, I understand that there is an investigative group that is working on all aspects of the weather in Cape Kennedy, is that correct? Isn't there a report about to be made on this subject?

COLONEL KOLCZYNSKI: I think I may more appropriately answer that. That has to do with the environmental weather conditions at Cape Kennedy and improve the forecasting capability of such. It is called the Meteorological Systems Modernization Program, which has since reported to NASA headquarters. There is a team headed up by Dr. John Theon that is looking at improvements to models, instrumentation, and systems which would improve the forecast capability to give us a better forecasting capability at the Cape.

CHAIRMAN ROGERS: Dr. Ride points out that there is an ice team working group that is working at Cape Kennedy.

Are you familiar with that?

MR. STEVENSON: Not in those terms, no, sir.

DR. RIDE: I guess that I was under the impression that there was a group set up originally by the interim board, and then that has either continued on

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or just completed its work to just basically evaluate and assess the things that you found prelaunch and postlaunch.

MR. STEVENSON: I am not aware of that.

CHAIRMAN ROGERS: Well, we will look into that.

I think we are going to take a recess pretty soon here and ask you gentlemen maybe to come back in the morning.

Before you leave, though, was there any—and we will ask you some more questions in the morning—was there anything that was called to your attention or that you noticed that caused you any concern about whether Challenger should be launched or not? Were you required in your duties to use any discretion in connection with that, or was that something that was passed on to other people and they assessed?

MR. DAVIS: We were specifically asked what we thought about it.

CHAIRMAN ROGERS: And what did you think about it? Did you have any serious concerns or any other concerns about whether that launch should be made?

MR. DAVIS: Well, I had some concerns relative to the ice on the fixed service structure. I didn't know anything about the solid booster problem and all at

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the time, and that was that in my experience, when the sun starts to shine on adhered ice on steel structures and other things of that type, it loosens at the interface between the ice and that substructure that is underneath it. And I will let Charlie say what he said about the—because he got asked the same question, but in my answer to the question what I thought about the subject of launching was this, that considering this fact that the ice would get looser as it got warmer, I felt that if we decided or if they decided that we should launch, that they should do it as soon as possible because this rising sun was shining on the fixed service structure as well as the ice that was on the mobile launch platform surface and would tend to loosen it and thereby make it more dangerous as far as a quantity of it falling off, and that if they were going to fly, they should fly as early as possible and not wait until later.

CHAIRMAN ROGERS: Did you pass judgment at all or give any advice on whether you thought it was desirable to launch at all or not?

MR. DAVIS: No, sir.

CHAIRMAN ROGERS: How about you?

MR. STEVENSON: What we really do, we do a total assessment of the pad and the vehicle based upon

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the conditions that we observe, and based upon the conditions that we observed, particularly for the flight vehicle, there was no reason to say not to launch. Our concern most of that day, and we spent the last ten hours before launch, we spent at least four hours of that on the pad, our major concern on that day was to facility ice, and once the facility ice problem had been resolved to the point that as long as the ice was not falling into the SRB hole, that there would not be a safety of flight issue, then the team as a whole had no problem with the launch.

DR. RIDE: Did you feel that the ice problem had eased to that point that the ice was no longer going to be falling into the SRB?

MR. STEVENSON: In our mind, and based on our experience, the thing that was missing and that gave us problems as a team was the unknown associated with the aspiration. We were smart enough to figure out that the ice that fell off of the facility would fall off within just free-fall would be 15 to 20 feet away from the edge of the FSS. The big unknown, and for this program we hadn't done any studies on aspiration, and so, and based upon previous launches, which we had not had any problem with the facility icing up the side of the structure, and so we had no data base, let's say, to

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make an assessment of the aspirational effects on the falling ice.

And once the management had assessed that problem and had decided that it was not a safety of flight issue for the conditions we had described, then we really didn't have any problem with launching.

The launch film, by the way, does support that they probably did make the right decision in that we do not have any impacts over the vehicle from ice off of the facility. We did have a slight amount of impact to the SRB, left hand SRB aft skirt area, but we had no impacts to the orbiter vehicle.

CHAIRMAN ROGERS: Is there any way to judge whether the ice had any impact on the orbiter based upon the pictures?

MR. STEVENSON: Well, the photographs of the film is high speed film, and if there are any impacts to the vehicle, we were able or have been able to determine that there were none.

CHAIRMAN ROGERS: So far as you can tell, there was no damage?

MR. STEVENSON: No damage to the orbiter vehicle or damage to the SRB.

CHAIRMAN ROGERS: All right.

Could you provide in the morning some sample

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pictures that were taken of your ice?

MR. STEVENSON: Yes, sir.

CHAIRMAN ROGERS: If there are no further questions by Commissioners, let's adjourn until tomorrow morning at 9:30.

(Whereupon, at 4:20 o'clock p.m., the Commission recessed, to reconvene at 9:30 o'clock a.m., Thursday, February 27, 1986.)

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PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—THURSDAY, FEBRUARY 27, 1986

Dean Acheson Auditorium
State Department
Washington, D.C.
The Commission met, pursuant to recess, at 9:35 a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ARTHUR WALKER
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA
ROBERT HOTZ
DR. EUGENE COVERT

JOSEPH SUTTER
ROBERT RUMMEL
ALSO PRESENT:
AL KEEL, Commission Executive Director

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PROCEEDINGS

CHAIRMAN ROGERS: The Commission will come to order, please.
DR. KEEL: Mr. Stevenson, Mr. Davis, and Colonel Kolczynski.

CONTINUED TESTIMONY OF CHARLES STEVENSON, B.K. DAVIS, AND LIEUTENANT COLONEL EDWARD KOLCZYNSKI

CHAIRMAN ROGERS: Good morning.

The Commission would like to receive any information that you have after the session is over. I know you provided some information for us, and we would like to, as we go along, get any information that you have from any of your documents for our records.

Mr. Hotz and General Kutyna have some questions they would like to ask you, and we will probably not keep you too long today.

Bob, do you want to go ahead.

MR. HOTZ: Charlie, you mentioned yesterday that when you were going around the pad with your pyrometer, none of the readings you received on the pyrometer exceeded the red lines and therefore you didn't report them.

Could you explain what those red lines are and what the limits of the red line is.

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MR. STEVENSON: Okay. My reply was intended to mean that we operate by certain requirements, OMRSD requirements, and certain Launch Commit Criteria requirements. And those measurements we had—let's say the vehicle was operating within those limits. As far as the particular measurements of the SRB which you were mentioning, we have no requirements as to the temperature of the SRB in the area that you were questioning, the 19 degrees.

MR. HOTZ: So you have no red lines as far as the SRB's are concerned?

MR. STEVENSON: No, sir.

MR. HOTZ: And what are the red lines concerned with the external tank or the orbiter or what?

MR. STEVENSON: The red lines as far as the ice frost goes refers to the tank itself, to the external tank, and this is mainly in terms of temperatures relating—our duties are mainly in terms of temperatures relating to the external tank. The orbiter temperatures, again, as has been stated, 31 degrees to 99 degrees.

MR. HOTZ: But that is the same as the general launch criteria?

MR. STEVENSON: Yes, sir.

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MR. HOTZ: So you didn't report these temperatures on the SRB because there is no place in the launch criteria that requires any?

MR. STEVENSON: That is correct.

MR. HOTZ: Have you ever had antifreeze freeze on the pad before this launch?

MR. STEVENSON: As far as the facility goes?

MR. HOTZ: Yes.

MR. STEVENSON: STS-20 was a similar case the night before we launched, but it was not the same conditions at launch time.

MR. HOTZ: Was that the one where the facility became encased with ice?

MR. STEVENSON: Yes, sir.

MR. HOTZ: You didn't actually launch after that one?

MR. STEVENSON: No, we did not.

MR. HOTZ: Is this the worst pad ice that you have ever encountered in your excursions out there where an actual launch took place?

MR. STEVENSON: As far as the facility goes, that is correct. As far as the launch vehicle goes, that is not correct. We have had more ice on the vehicle at a launch time than we had this time. For this particular launch, the vehicle was performing well,

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and it was mostly frost.

MR. HOTZ: Now, you mentioned earlier that you, as you move around the pad, you are in constant communication with the launch control room, because they need to know where you are and what you're doing. And so, in addition to your conference reports when you come back, you are also giving real time chatter to them while you are out on the pad, is that correct?

MR. STEVENSON: We are in communication with two different groups. We're in communication with the launch, the NTD or the LTD.

MR. HOTZ: Could you spell that out for us, please.

MR. STEVENSON. Okay. We are in communications with the launch test director on the 101S net, and our responsibility is mainly to tell him where we are, where we're going, and what we're doing. He occasionally asks what conditions we are finding.

We are also in communications with the—

MR. HOTZ: In this particular case, were you reporting findings back to him?

MR. STEVENSON: Not specifically, no, sir.

MR. HOTZ: You waited until you came in for your conference?

MR. STEVENSON: Yes, sir.

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There was a case where we also were reporting back through the management loop on channel 245, and we also normally carry on communications with our people back in firing room 2, who are doing the monitoring for our ice frost. We report various conditions that we say which may be different than what is being observed on the TV system, so that we can have a better understanding of the conditions.

MR. HOTZ: Thanks very much.

I have a couple of questions for Colonel Kolczynski on the weather forecast. Could you give us in brief terms when and where you made you pre-launch forecast when you predicted the hard freeze for the night of January 27th-28th?

COLONEL KOLCZYNSKI: We gave several briefings throughout the time period. We gave some briefings on Saturday which in the afternoon—

MR. HOTZ: Could you use the dates.

COLONEL KOLCZYNSKI: Yes. On the 25th at 1100 local, we had a Mission Management Team briefing which we attended and presented our data, which indicated a strong cold frontal system coming down with Arctic air, strong Arctic air behind it. And we anticipated at launch time having layered clouds, approximately broken at 3,000 feet, layered up through about 24,000 feet.

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Based on that, we came back in the evening on the telephone conference on the 25th at 2100 local. The system still was moving very rapidly toward the Florida peninsula, and it was still our estimate that we would have the clouds in there and potentially some rain showers or even some thunder showers to the West.

Based on that, the mission was scrubbed. We subsequently came back on the 26th at 1400 local. That was an in-person briefing to the Mission Management Team again. At that time, we talked about the cold frontal system passing through, strong winds for the Monday morning launch time period, potential crosswinds.

As an outlook which we give—we always give an outlook for the day after. We indicated that once the cold high had set in in the Florida area, that we did anticipate having some colder temperatures. At that time, we were forecasting the mid-20's to the upper 20's.

As you well know, we scrubbed for strong crosswinds again on the Monday morning. That is the 26th—I'm sorry, the 27th. So we came back. on the telephone this time, the 27th at 1400 local, and we presented again a forecast of clear conditions, basically, good winds, no precipitable kind of weather.

But we did indicate that we would have a

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little colder temperature than we had predicted before. In front of you you can see—at least you could for a while—the actual weather temperature trace, which is in the blue dashed line versus our predicted temperature.

(Viewgraph.) [Ref. 2/27-1]

We started it at about 1200 to 1200 for the temperature trace, and we started our forecast trace at midnight because it was our anticipated prediction that at 12:00 midnight we would go below 32 degrees. We were about three degrees warm approximately until about 4:00 a.m., at which time we caught up with the curve, and then we were a slight bit, as you can see, colder in our prediction than the actual weather was.

Is that sufficient?

MR. HOTZ: Yes.

I have no further questions, Mr. Chairman.

DR. COVERT: Colonel Kolczynski. I would like to ask you a question or two if I may on the information that you had generated on the winds aloft particularly the wind shear conditions and the jet stream location at 35, 40,000 feet at the time of launch, how often that was updated and so forth.

COLONEL KOLCZYNSKI: I did provide a package. Unfortunately, you don't have the launch times of our

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upper air sounding systems. But I do have available for the panel a listing of all of the balloons and rockets that were sent up, and I will provide that.

DR. COVERT: How often do you send up balloons?

COLONEL KOLCZYNSKI: The balloon that was used I believe for the loads calculations was the 3 hour and 30 minute prior to launch balloon.

DR. COVERT: That would be 8:00 a.m., roughly?

COLONEL KOLCZYNSKI: No, sir. The launch time—if the launch time were at 10:30, that would have been somewhere around 0700, I believe. And the actual time of that was—I believe that is 7:05, but I will double check that for you.

Basically, that information is collected and then transmitted to the Johnson winds people, who are Marshall people. I'm sure, they go to Johnson to do the wind loads calculations.

We send up a balloon. It is my contractor Pan Am that does the launching and the data collection and the transfer of that data in automated fashion to those people. Per se we don't do wind shears, and I would defer that question to the Marshall people because they can explain that much better than I can as to how they

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do the loads calculations.

All we do is the measurement of the upper air soundings and pass the information to Marshall.

DR. COVERT: That is all I am interested in at this point. About the time that balloon went through 35 to 40,000 feet, how far was it displaced from the position where the space shuttle would have penetrated that altitude?

COLONEL KOLCZYNSKI: I don't have that information. I can get that for you.

DR. COVERT: Would you guess it was a substantial distance?

COLONEL KOLCZYNSKI: I really wouldn't want to speculate. I just haven't looked at the data that closely. But I can get you that information.

DR. COVERT: Are there other ways of doing altitude soundings that would be more precise?

COLONEL KOLCZYNSKI: We have several techniques that we use at the Cape. One of them is a radar track mylar balloon with little conical shaped protrusions all over it. It is called a JIMSPHERE, and we track that balloon, and it has an accuracy of approximately about—we can collect data in approximately 100 foot intervals.

The balloons rise at about 1,000 feet per

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minute. Then there are the rawinsondes and the windsones, and they are substantially less accurate.

There are other techniques that are being investigated, the upper air radar sounders, doppler acoustic radars, that show some potential. But again, as to the real capability of those kinds of systems, I think those questions again would be better addressed to the Marshall people, because they have folks right there that have been looking at those kinds of systems.

DR. COVERT: I guess I would just like to go back and again emphasize one point. All of these drift techniques, the balloons are launched near the pad?

COLONEL KOLCZYNSKI: No, the balloons are launched on the Cape Canaveral side of the complex. So they are south of the area where the shuttle is. And I'm not very good with distances, but I would estimate perhaps five miles, and that could be in error.

DR. COVERT: What kind of gradients exist in the upper air winds, so that if you have a balloon that is ten miles from where the shuttle is going to penetrate this altitude, how do you correct? Or again, is this a Marshall task and not yours?

COLONEL KOLCZYNSKI: We don't correct the balloons. All we do is—essentially, when it moves, it is a radio frequency kind of thing. We get radio

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signal back and as the balloon drifts that gives some indication of not only the speed, but also the direction in which we at a certain level feel the wind is blowing. It's not a feeling. We measure it to the best of our ability.

DR. COVERT: Okay.

COLONEL KOLCZYNSKI: So if you are asking me how continuous is the atmosphere, I can't answer that question. I would—again, I'm not an upper air dynamicist, and I think you probably could find somebody who could give you a much better answer than I could.

DR. COVERT: Thank you, Colonel.

CHAIRMAN ROGERS: Mr. Davis, you said that the weather conditions on the pad were the worst of any launch, any previous launch, is that correct? Or maybe it was Mr. Stevenson that said that.

MR. DAVIS: We probably both said it.

CHAIRMAN ROGERS: Do you want to discuss that a bit? In other words, the conditions on the launch pad themselves were the worst of any flight, any previous flight, is that accurate?

MR. DAVIS: Okay, I will give it a try. Earlier in the year before January launch, we got ice all over the fixed service structure. However, we were able to decide not to launch—

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CHAIRMAN ROGERS: You scrubbed that launch?

MR. DAVIS: We scrubbed that launch, and it all melted off before the time of the actual launch. And so as far as launch time is concerned, it was relatively warm by the time that we did launch.

This time, however, we were in the 20's, like 24 to 26 degrees, at the time we were out there at the T minus 3 hold, and this is well below anything we had ever experienced before. And it did serve to verify our math model which we use to predict the amount of ice that we may get on the outside of the tank.

Normally, we all talk about the fact that below 32 degrees you're going to have ice. But this isn't strictly true if the ice has to come from the moisture in the air, which is the case here. So as far as the tank itself is concerned, we verified within a reasonable amount of error what the actual temperatures were, and they were well below freezing. And we also verified that the predicted amount of frost was really what was getting on it, which was practically none.

And so as far as the vehicle itself was concerned, the external tank part of it, and also the interface between the tank and the orbiter, we had less ice than we have seen before. But the weather conditions for the other things that we encountered were

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much worse.

CHAIRMAN ROGERS: But the condition, the ice on the launch pad, was worse than any previous flight?

MR. DAVIS: Yes, sir, it was. And we can go through that in some detail. We have some charts that would show that if you like, that show where the ice was and essentially how much it was, and also I think will partly explain my remark yesterday to the effect that the sunlight would make some of it on the eastern side of the stand be more likely to fall off during launch.

CHAIRMAN ROGERS: Well, why don't you go ahead and do that. Do you have pictures that you could show us?

MR. STEVENSON: Yes, we have them, and I guess we can have them projected on the screen up there.

CHAIRMAN ROGERS: Fine, go ahead, why don't you.

(Viewgraph.) [Ref. 2/27-2]

MR. STEVENSON: The first picture is just an orientation type picture showing the MLP deck, the FSS, and the RSS.

CHAIRMAN ROGERS: Could you explain some of those acronyms so we understand them?

MR. STEVENSON: The platform that the shuttle sits on is the mobile launch platform, the MLP.

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CHAIRMAN ROGERS: You refer to that as the launch platform?

MR. STEVENSON: Yes, sir, the mobile launch platform.

The structure off to you right with the swing arms attached which come out to the vehicle is called the fixed service structure, the FSS. And the part that is rotated back on the tracks to what we call the south is the rotational service structure. And we use that for payload changeout.

CHAIRMAN ROGERS: Okay.

MR. STEVENSON: The next chart.

(Viewgraph.) [Ref. 2/27-3]

MR. STEVENSON: Okay, this chart just shows you a different view of the FSS and the MLP and the shuttle vehicle. This chart also shows some of the projections we made as far as the trajectories for the ice that would come off of the facility.

CHAIRMAN ROGERS: Say that again? Now, what are the figures that you show in the chart? They are projections, you say?

MR. STEVENSON: Trajectories of the ice particles as we were predicting that they would fall off of the facility.

CHAIRMAN ROGERS: I see.

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MR. STEVENSON: The highest level which we had ice on the facility—and I will go into those in more detail in the future charts here, but the highest elevation was at the 220 foot elevation. And if you see the—we made predictions for three size particles, a six inch by one inch by one inch particle, which is one of those traces: a 12 inch by one inch by one inch particle, which is the center trace; and the 24 inch by one inch by one inch ice particle, which is the inboard trace.

And we had predicted that they would come out on the MLP deck approximately 16 feet from the FSS toward the shuttle vehicle, based upon a 10 knot wind at 300 degrees azimuth.

DR. RIDE: Again, I think you mentioned this yesterday, but that calculation doesn't include the effects of aspiration?

MR. STEVENSON: That is correct. And there's a note on the chart up in the upper right-hand corner that says "Effects of aspiration not included." That was unknown to us.

The next chart.

(Viewgraph.) [Ref. 2/27-4]

MR. STEVENSON: Okay. This is just a break across the fixed service structure at the 220 foot

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level. At the 220 foot level is the first level at which we encountered ice, and the darkened area you see off there where the arrow pointing toward the north is is where the ice was on that level.

It is the first fire X hose which was let trickled to keep from freezing, and it was drained into an eye bath or an eye wash, and the hose had fallen out of the basin and the water was running on the facility. So that is the first, the highest level at which we had ice, and the darkened area is away from the vehicle to the northwest.

Next chart.

(Viewgraph.) [Ref. 2/27-5]

This chart is the level immediately below the 200 foot level, which we probably had the most ice. The darkened area again shows the part of the facility of the FSS at which we had ice. There is a color.

The next chart.

(Slide.) [Ref. 2/27-6]

Okay, this is a color photograph that was taken at T minus 3.

CHAIRMAN ROGERS: What are we looking at?

MR. STEVENSON: You are looking at the northwest corner of the fixed service structure, the plumbing, the structural moments itself where you see

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the icicles that have formed as a result of the water coming, running down from the level immediately above.

Icicles on this level were approximately two feet long and a maximum diameter of about three-quarters of an inch.

The next chart.

(Viewgraph.) [Ref. 2/27-7]

The previous chart, by the way, was the level of the access arm, orbiter access arm, from which the crew enters the vehicle. This happens to be the 160 foot level, and again the blackened area shows the area where we encountered ice.

This area does show a little bit more ice over on what I'm calling the east side, which is the side toward the vehicle, which would be in the top of the frame.

CHAIRMAN ROGERS: The last picture you say showed the area where the crew entered the vehicle?

MR. STEVENSON: Yes, the same level from which the crew enters the vehicle. The photo was actually taken from the area of the slide wire, which is the emergency egress system.

The next chart.

(Slide.) [Ref. 2/27-8]

MR. STEVENSON: Again, this is a color photo

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taken at T minus 3 hours from the MLP deck. But it does show the 160 foot level. Icicles on this level were approximately one foot in length and approximately five-eighths inch in diameter, and you can see them on the stairways and all of the substructure of the facility.

MR. DAVIS: If I may, in this area the sun would be hitting the eastern section of it, which is right in this area, and all of those small icicles up at the top would be the ones that would be getting the sun first and would begin to loosen and would be ready to fall at the time of liftoff.

CHAIRMAN ROGERS: Mr. Davis, did you perform the same functions on previous launches, particularly the two previous launches?

MR. DAVIS: Yes, sir.

CHAIRMAN ROGERS: Those were both launched from pad A, is that correct?

MR. DAVIS: Yes, sir.

CHAIRMAN ROGERS: And this was at pad B?

MR. DAVIS: That is correct.

CHAIRMAN ROGERS: And this is the first time pad B had been used?

MR. DAVIS: That is correct.

CHAIRMAN ROGERS: Did pad A have weather

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protection of some kind?

MR. DAVIS: No, sir.

CHAIRMAN ROGERS: On pad A wasn't there some weather protection device?

MR. DAVIS: We have one, but it is not in this area, as far as protecting that structure.

CHAIRMAN ROGERS: But there was some weather protection equipment on pad A, wasn't there?

MR. STEVENSON: Yes, there's the same freeze protection plan for both pads, the same type of protection.

CHAIRMAN ROGERS: We were told, I think when we were in Kennedy, that there was weather protection equipment on pad A, but it had not been installed on pad B.

MR. DAVIS: No, I think they were referring to weather protection devices for the orbiter. That was to keep rain and so forth off the orbiter itself. It was not to protect a structure like this. The only thing that is done to protect the structure as such is like draining pipes and preparing it to handle the cold weather situation.

CHAIRMAN ROGERS: Could you explain the equipment that was on the launch pad A that wasn't on launch pad B?

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MR. STEVENSON: The equipment that is on pad A in terms of weather protection is equipment which is used to protect the orbiter from rain and hail, the weather. It is part of the rotational service structure, and for launch, of course, that is rolled back. It is rolled back at approximately T minus 20 hours, say.

CHAIRMAN ROGERS: What kind of equipment is it? Is it a cover of some kind that covers the orbiter?

MR. STEVENSON: Currently it is a canvas type. We call it a sail, and it also involves some hard structure. For this pad and for the future, it will all be hard structure.

CHAIRMAN ROGERS: But it didn't have anything to do with the external tank or the solid rocket boosters?

MR. STEVENSON: No, sir, not in this time frame. Both pads would be identical in this time frame.

CHAIRMAN ROGERS: I'm not sure that I know what you mean by "this time frame." I'm trying to figure out whether there was a difference in the protection against the weather in the last launch compared to this one.

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MR. STEVENSON: In this time frame, the part of the weather protection for the orbiter which is attached to the RSS would not be in place, because the RSS would be in its launch position. And regardless, both pads would have the same configuration at this point in time.

CHAIRMAN ROGERS: Well, why don't we go ahead. We can straighten it out later.

MR. DAVIS: Could I just for a second? I might be able to fix it. What it is, we have batwing type things that are on the rotating service structure, and those things will fold up out of the way. And you have to rotate that service structure back away from the vehicle. That takes all of that stuff with it.

At this time, those things are not on pad B, but they would have been away anyway for this period of time. Like we're fixing to launch, so they would move it back out of the way.

And any of the things that you see here, like the icicles and so forth, would have been just like they are now, regardless of what the weather protection device and so forth had been, had it been identical to pad A.

VICE CHAIRMAN ARMSTRONG: But for the several days before launch there would have been a difference in

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protection of the orbiter?

MR. DAVIS: That is correct.

CHAIRMAN ROGERS: Now, in your mind that had no significance as far as this flight is concerned?

MR. DAVIS: No, sir.

CHAIRMAN ROGERS: Well, if that is the case, why bother having the protection on the orbiter, if it made no difference?

MR. DAVIS: Well, I will try to answer it, though I am not a Kennedy person. But I will give it a shot, because of this—the real intent of that protective device that they're putting over the orbiter itself is to prevent absorption of moisture into the tiles.

Now, they did have some protection. That was to keep the orbiter from getting damaged by the fact of getting wet, and that protection is what we're talking about. That has just been moved away regardless, and between the time that that was rolled back and the time of launch there was no precipitation and nothing happened that would have made a difference one way or the other.

CHAIRMAN ROGERS: Okay. Go ahead.

MR. STEVENSON: Okay. Next chart, please.

(Viewgraph.) [Ref. 2/27-9]

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The next chart is just a plan view, again moving down the FSS to the 140 foot level. Again, the darkened area shows the area at which we encountered ice of some form.

Next chart.

(Slide.) [Ref. 2/27-10]

Again, this is a color photo showing the ice. At this time it's on the vehicle side of the FSS. The vehicle in the background is the left-hand SRB at the ET attach ring. That is the area at which we attach the tank to the SRB and the area at which we attach the orbiter to the external tank.

You see the orbiter in the background. The lower surface, the icicles here are approximately one foot in length and about one-half inch in diameter.

Next chart, please.

(Slide.) [Ref. 2/27-11]

Again, this is a color photo of the same area, just taken one slice higher on the vehicle. You can actually stack the previous photo and this one together, and you will get the idea—if they were stacked in this fashion, you would get the idea of what the total level looks like on the 140 foot level.

The next photo.

(Slide.) [Ref. 2/27-12]

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I included one to show some of the icing conditions on some of the equipment that we were using. This happens to be a communications station which we use to communicate back to the LCC and other areas on the pad.

Icicles here are approximately one foot in length and about a half an inch in diameter. There was ice, as we mentioned yesterday, on various distribution panels and valve box panels, as well as on side walls of the structure.

CHAIRMAN ROGERS: Was there any concern that that ice would affect the operation of those instruments?

MR. STEVENSON: That particular instrument I couldn't use because of the ice, because I couldn't turn the knobs to get to the right channel, and I had to go to another.

CHAIRMAN ROGERS: Say that again so I can understand it? A little louder, please?

MR. STEVENSON: This particular box I tried to use, as a matter of fact, and I had to go to another station, because I could not turn the dials to get to the proper channel.

CHAIRMAN ROGERS: But that had no effect on the safety of the launch as far as you were concerned?

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MR. STEVENSON: No, sir.

CHAIRMAN ROGERS: It was just an alternate source of equipment?

MR. STEVENSON: That is correct. And as a matter of fact, these units are not in operation during a normal launch except when we're on the pad, and they are usually in the inert, let's say, condition.

DR. RIDE: You said that you saw ice on other equipment on the launch pad, the distribution boxes, presumably valves and that sort of thing?

MR. STEVENSON: All of which are in the remote condition.

Next chart, please.

(Viewgraph.) [Ref. 2/27-13]

MR. STEVENSON: Moving down the structure, this happens to be the 120 foot level, and again the darkened area shows the area where we found ice. And in all of these, I should point out that most of the areas that you've seen, the darkened area is really the north and northwest part of the facility. That is where we saw the most heavy concentrations of ice. The east side, which is in the top part of the TV screen, we actually saw the least amount of ice.

The next chart.

(Slide.) [Ref. 2/27-14]

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MR. STEVENSON: This is a color photograph looking at the 120 foot level from the mobile launch platform deck, that shows the icicles that were forming on the roof or on the floor, whichever may be the case, on the grating, as the water had dripped through the grating.

Again, this side is the northeast corner and it is toward the vehicle.

The next chart.

(Slide.) [Ref. 2/27-15]

This photograph was taken from the 100 foot level of the FSS. It shows the orbiter in the background with a part of the external tank, and on the far left the part of the SRB. It shows one of the beams, structural moments of the FSS, with the icicles on it.

These icicles are about three inches long and less than half an inch in diameter.

The next chart.

(Viewgraph.) [Ref. 2/27-16]

This chart is just a plan view of the MLP deck. We are showing in the crosshatched area the 16 feet that we were predicting would be the footprint for ice which we were projecting would fall off of the FSS at ignition.

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And again, this chart does not take into effect the effect of aspiration. And we will go over in the next few charts and talk about the SRB flame holes which you see in the center part of the photo.

The next chart.

(Slide.) [Ref. 2/27-17]

This photo is a photo of the MLB deck looking from the FSS to east, toward the vehicle. The vehicle would be in the top part of the frame. The shiny part of the MLB deck is sheet ice approximately one-eighth of an inch thick.

Down in the bottom of the frame, you see icicles, which were running from a fire hose into the drain. Those icicles are about four feet long, and that is over the edge of the MLP deck.

Next chart.

(Slide.) [Ref. 2/27-18]

You need to turn that one over.

Okay. This is the chart of what are called water troughs within the SRB flame holes to prevent overpressure. They are filled with the 6,500 gallons, roughly, of water and antifreeze. The antifreeze was put in for this launch as part of our freeze protection plan.

The water was protected against freezing down

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to 16 degrees Fahrenheit. And this chart over on the right-hand side, the first four or five water bags, you can see that there is ice on the surface. This picture was taken after we had begun to remove the ice with, let's call it, a fish net. We actually broke the ice up and dipped it out.

The next chart.

(Slide.) [Ref. 2/27-19]

This again is the left-hand SRB flame hole, and it shows the amount of ice we have taken out. It shows a little bit of ice that is left in there. We did come back out after this photo was taken and fish out most of the remaining ice that you see there. There's a few pieces still left in there floating, about the size of your hand.

The next chart.

(Slide.) [Ref. 2/27-20]

This is just a repeat of what we've said, and again you can see a few pieces of ice were left in there. We felt we got a minimum of 95 percent to 98 percent of the ice that was available in the trough. We did fish it out.

And we did make one more attempt at T minus 20 to get out additional ice that you do see floating around in there when this photograph was taken.

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The next chart.

(Slide.) [Ref. 2/27-21]

This is a typical photo of the ice that was taken out of the water troughs and dumped on the MLP deck, where we later swept it off. We were estimating that the water—that the density of the ice here was approximately 25 pounds per cubic foot. It was approximately one half inch thick.

The next chart.

(Slide.) [Ref. 2/27-22]

The two next charts are of the aft skirt on the left-hand SRB. There was a little bit of water—or a little bit of water had gotten on the aft skirt of the SRB as a result of the overflow from the eye wash basins, and that water had turned to ice.

If you could zoom in on the blue ring on the bottom. The little white that you see on the blue ring is the ice we were referring to. We did remove that ice as best we could.

The next chart.

(Slide.) [Ref. 2/27-23]

This is a closeup of the same area. We had decided in the early meetings that ice in the area and ice in the water trough was unacceptable for launch, and that is the reason we went back out a second time and

1767

attempted to remove all of the ice in the area of the flame hole.

CHAIRMAN ROGERS: Would you explain that a bit. You decided earlier what, that that was unacceptable?

MR. STEVENSON: Okay. Ice in the water troughs, which we were showing, or any debris is unacceptable for launch, because we have seen in the past film analysis that some of this debris can be thrown up toward the vehicle.

And so we decided that we would have to remove the ice, particularly in the primary water bags, which could become a source of debris and impact the vehicle.

CHAIRMAN ROGERS: And you did that?

MR. STEVENSON: And we did that. The water bags that you've seen here in these photographs are mainly what we call the secondary water bags. The primary water bags are just below this photo, close in around the nozzles of the SRB's, and that had considerably less ice in it, and also we took more pains to make sure we fished it all out.

The next chart, please.

(Slide.) [Ref. 2/27-24]

We put in about three charts to show the overall condition of the vehicle. We haven't talked

1768

much about the vehicle, but this chart does show the frost conditions on the vehicle, and this is launch time, not T minus 5. These are pad cameras, taken at launch.

And you do see a little bit of frost on the external tank. We see no anomalies with the orbiter or the SRB.

MR. DAVIS: If I could interrupt here for just a second. If you notice, on the right side the frost is a little more evident on the back side where it's in the shade. You see the white on the tank itself, and this is the kind of frost that we were seeing earlier. And you see on the side that is next to the orbiter, it is pretty well melted off.

MR. STEVENSON: Next chart.

(Slide.) [Ref. 2/27-25]

This chart is at the T zero.

The next chart.

(Slide.) [Ref. 2/27-26]

This chart shows the vehicle rise of approximately 100 feet.

DR. RIDE: What time is this?

MR. STEVENSON: I would say approximately two seconds.

DR. COVERT: Mr. Chairman, could I suggest we

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have copies of that picture, please?

MR. STEVENSON: Yes. sir, I will supply you copies.

DR. RIDE: Can you pan that down just a little bit.

MR. STEVENSON: Would you pan that down just a little, please.

MR. HOTZ: Charlie, this may not be your field, but it looks like there's a little puff of smoke off on the right hand side there. Can you describe that for us?

MR. STEVENSON: Yes, sir. That is the puff of smoke that has been released before to the press and to the world. That puff of smoke in that picture is, I believe it is about 100 or so inches tall and about five feet across.

MR. HOTZ: And what is the time frame of that picture?

MR. STEVENSON: I believe it's about two seconds. I would have to go back and check the time.

GENERAL KUTYNA: Would it pay to back up one chart? Was there smoke on that chart previously?

MR. STEVENSON: Yes, would you back up one chart.

(Slide.) [Ref. 2 27-27]

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MR. HOTZ: Is there any smoke visible in that picture, Charlie?

MR. STEVENSON: No, sir, I don't think so. They are not sequential pictures. I took them out of a string of photos, and they are not necessarily in the correct order.

MR. HOTZ: But this appears to be a different angle from any of the pictures that we have seen. I wonder if you could make a sequential series of that available to the Commission.

MR. STEVENSON: Yes, sir. I have 130 photographs in that area.

MR. HOTZ: Thank you very much.

CHAIRMAN ROGERS: Could you go back to the one where the smoke appears for the first time, please.

MR. STEVENSON: I believe that is chart 24.

DR. COVERT: Could the operator focus the one, focus that to the left. Put the one back on you just had.

(Slide.) [Ref. 2 27-28]

MR. STEVENSON: In one of those photographs, now that I've pulled the notes out, the cloud of smoke is 36 inches by 108 inches. And then in the larger photo it's 72 inches by 130 inches.

CHAIRMAN ROGERS: Have any of you made any

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interpretation of these pictures as far as weather is concerned?

MR. STEVENSON: Not as far as the weather is concerned, no, sir.

CHAIRMAN ROGERS: Have you any other conclusions on it?

MR. STEVENSON: Well, our conclusion would be speculation based on—but if you want that, I will speculate.

[Laughter.]

CHAIRMAN ROGERS: Well, go ahead.

[Laughter.]

MR. STEVENSON: Engineers don't like to speculate, but based upon our photo data—and we have analyzed all of the photos—we feel that that is a leak. It may or may not be related to temperature, and we feel it is coming out of—the most likely spot is the joint between the aft booster and the aft segment.

CHAIRMAN ROGERS: And this is the right booster?

MR. STEVENSON: This is the right booster, yes.

MR. DAVIS: And this is the area that I mentioned yesterday, where I was taking a shot of that area above it, which is the white area there, where all

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of that extra frost is up there. I panned up and down that area on the tank and got the temperature readings earlier.

And so at that time we could see absolutely nothing that would indicate any kind of a leak from the tank itself, and this is in that specific area.

CHAIRMAN ROGERS: So your speculation would be the same as Mr. Stevenson's?

MR. DAVIS: Yes, sir.

CHAIRMAN ROGERS: Mr. Armstrong.

VICE CHAIRMAN ARMSTRONG: Would you tell us again how long before the time of this picture your last reading with the IR gun was, or any other measurement?

MR. STEVENSON: That time was 7:14. Give us chart 26.

(Viewgraph.) [Ref. 2/27-29]

MR. DAVIS: That was almost five hours earlier than the picture we were just looking at.

VICE CHAIRMAN ARMSTRONG: About how long?

MR. DAVIS: About five hours. We were right at noon when we launched, and that was roughly at 7:00 o'clock.

VICE CHAIRMAN ARMSTRONG: And have you made any attempt to project what the first—or correct your measurements to give the actual temperature of the

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seal area at the time you made the measurements, and then to project what they may have been at launch time?

MR. DAVIS: Yes, sir, we have. Taking the 19 degrees corrected measurement, which is indicated on this chart in front of you there, and taking into account the change in ambient temperature and a minor consideration, which could be a major one and completely make my guess wrong in that it might be a higher temperature than I'm going to project, it comes out that the temperature should have been approximately 28 to 30 degrees at the time of launch.

VICE CHAIRMAN ARMSTRONG: Thank you.

Is that all of the charts you had?

MR. STEVENSON: Yes, this is all the ones we planned to present, and we will make these available to you.

CHAIRMAN ROGERS: Thank you very much.

Mr. Armstrong.

VICE CHAIRMAN ARMSTRONG: Yes. I would like to return to your testimony of yesterday indicating your past experience with seeing leaks from the ET, not necessarily leaks but perhaps vents or whatever. And is it your conclusion, based upon your past experience, that a leakage out of the external tank, either LOX or hydrogen, would in fact be visible because there would

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be associated condensation in the air and so on, or not?

MR. DAVIS: Yes, sir, that is correct. And in fact, we have corroborating evidence of it in our photographs that showed the purge coming out of the hydrogen to—let's see—the hydrogen-umbilical interface between the tank and the orbiter. You can see the condensation and see the nitrogen purge coming out, and it is considerably warmer than the nitrogen itself would be.

We can also see the oxygen coming out of the oxygen vent system, and as it comes out you can see it, and we have a mixture of purged nitrogen and mixed with that oxygen, and that is dry nitrogen we are using. And so if we had a leak of hydrogen we would certainly see it, because very minutely it would give you a rather large plume.

VICE CHAIRMAN ARMSTRONG: And you would both see it and, if you went over that area with your gun, you would probably see it with that. So you had two sources.

And the last time of your visual inspection of the SRB and the ET was again how long before launch?

MR. DAVIS: T minus 20 minutes, which would be about 30 minutes before launch.

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VICE CHAIRMAN ARMSTRONG: Can you say that your impression is that, based upon your visual observations at T minus 20 minutes, there was no evidence of any leak?

MR. DAVIS: That is correct.

MR. RUMMEL: Was the specific charge of yours to in fact look for hydrogen leaks, or was this done more or less in passing while you were there?

MR. DAVIS: We are not specifically charged to look for hydrogen leaks as such. We are charged to look for any leaks, and any leaks to the substrate, through a crack or anything of that nature in the thermal protection system.

And as part of the interface area down there, we have a possibility of a leak always, although we have good seals. And up to this point we don't have a record of having a leak.

But we always make a specific inspection for that, and we make specific inspections, and I looked personally, as well as all the other members on the team, for any indication of a crack or a leak or anything that goes with it.

But we aren't charged to go look for hydrogen.

MR. RUMMEL: I take it there are other means,

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then, as well? Are there TV cameras that continuously scan with respect to propellant leaks in the ET?

MR. DAVIS: Yes, sir, that is correct. We have them, but we don't have total coverage. We do have pretty good coverage in the area that I'm speaking of.

MR. RUMMEL: And where is that reported? Is that in the control center? Where are the cameras read that continuously report?

MR. DAVIS: That is the launch control center, in what we commonly refer to as the Ice House. We have a multitude of cameras that are directable by Charlie and his people and myself on request, to look at any particular spot. We have trained observers who watch it at all times, so that we are really looking for this kind of thing. This is our basis for existence.

MR. RUMMEL: Thank you.

CHAIRMAN ROGERS: Is there any evidence at all that you have seen that would suggest to you there is any possibility of a leak in that area on this Challenger flight?

MR. DAVIS: Absolutely none.

CHAIRMAN ROGERS: There have been, as you know, stories that somehow a hydrogen leak on the external tank might have been a cause of this accident.

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But so far you've had no evidence to that effect at all?

MR. DAVIS: No, sir. I can't support even a consideration of it.

CHAIRMAN ROGERS: Dr. Ride.

DR. RIDE: I have one question on your speculation about the smoke. I was wondering to what extent you have been able to localize the origin of the smoke on the photographs? Can you pin it down to a one square foot area or five square feet or 12 square feet?

Just how close can you do that photographically now?

MR. DAVIS: Maybe we should both answer that one. I can say that it looks to me, my personal observation of it, that it is inboard of the connection of the lower strut between the SRB and the ET. And I believe that is about something inboard of 45 degrees off the centerline.

MR. STEVENSON: It is approximately 300 degrees.

DR. RIDE: Okay. I guess really my question is, how sure are you of that? What are your error bars on that? Can you say that within a couple of feet or within a couple of inches?

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MR. STEVENSON: We will say it is within a foot of the joint, and it is at 300 degrees.

DR. RIDE: Thank you.

CHAIRMAN ROGERS: That's pretty precise.

VICE CHAIRMAN ARMSTRONG: It is my understanding that you also have as part of your responsibilities the search for debris post-launch.

MR. STEVENSON: Yes, sir.

VICE CHAIRMAN ARMSTRONG: Can you tell us what the results of that search were for this occasion?

MR. STEVENSON: Okay. Normally at post-launch we inspect the pad proper inside, let's say, the perimeter fence, unless we find flight hardware or some reason to go beyond. We have gone as far as the beach and we have gone as far as 15, 20 miles north and south of the beach, depending upon the conditions which would warrant that.

For this time, my normal inspection team was delegated the responsibility to look inside the pad, and inside the pad we found—as of this time, we have found no flight hardware or parts off of the flight vehicle. We have not found anything in the nature of facility debris that would cause damage to the flight vehicle.

GENERAL KUTYNA: May I ask, there were som

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new doors on this launch pad that were supposed to slap down after launch and I was told they were fairly large springs that slap those doors down, and those springs are missing. And the last time I heard we hadn't found them.

Have you found them yet?

MR. STEVENSON: One spring was found on the MLP deck by holddown post number one. The other three springs are still missing. The springs have a plunger mechanism. We have found two of those. The other two are still missing.

The two we found were approximately 1200 feet north of the pad, next to the perimeter fence. One was on the inside of the fence and one was on the outside of the fence. We feel that the other two are out in the lagoon and under water.

GENERAL KUTYNA: So no speculation that these might have bounced off the orbiter or something?

MR. STEVENSON: We have—we are in the process of doing a film analysis, and the film analysis shows that the doors to reach their 60 degree or 57 degree position, the timing is such that the vehicle was what we consider to be far enough away that when those springs could have come out and the plunger, that the vehicle would be in a safe position.

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VICE CHAIRMAN ARMSTRONG: To follow my previous question, had there been ice impacts on the orbiter tiles, for example, and damage to those tiles, would you normally expect that you would be able to find some evidence of that in your post-launch inspection?

MR. STEVENSON: I feel that, number one, if we had any damage to the vehicle during liftoff, the high speed film, launch film that we take, in all likelihood would show us that.

Secondly, if we had lost any flight type hardware—and we have done that in the past—we would have found it, and we have found it in the past.

VICE CHAIRMAN ARMSTRONG: For example, losing tiles, for example, you would expect it?

MR. STEVENSON: The time we lost tiles, we found nearly every single one.

VICE CHAIRMAN ARMSTRONG: Colonel Kolczynski, could you characterize for us the nature of the Cape weather and the difficulties that you encountered as a result of having to meet the launch and recovery criteria, since there are so many weather constraints on both of those areas?

COLONEL KOLCZYNSKI: I perceive that as two questions of a generic sense. To answer the first one, anyone that has taken an aerial photograph of the Cape

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knows full well that we are surrounded by water in that particular area. We have got the Indian River and Banana River to the west, and of course the Atlantic Ocean and several lagoons around, to provide enough moisture source.

Consequently, in the winter time in early mornings we run into problems with fog. In the summer time we encounter convection thunderstorm rain shower activity, which unfortunately oftentimes builds up directly over the Cape and KSC.

As far as the second question, I believe was—

VICE CHAIRMAN ARMSTRONG: What difficulties or how fast, really how fast does weather change, and what difficulties does that provide you in being able to meet the launch and recovery criteria that are associated with those?

COLONEL KOLCZYNSKI: If we could become specific with 51-L, for example, the frontal system, that frontal system seemed to move rather well as it came toward the Gulf and even midway through the Gulf. Once it got toward the Florida peninsula, it suddenly slowed down and, as you very well know, we had better than anticipated cloud conditions.

VICE CHAIRMAN ARMSTRONG: Better being?

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COLONEL KOLCZYNSKI: There were scattered clouds, less clouds than anticipated, right about launch time.

And so I guess what I'm trying to say is it's very difficult to hold continuity, especially 12 hours in advance on a system which is moving even as rapidly as this one was. There are occasions where you're going to miss the timing a little bit, and of course we were off a little bit on the timing this time.

About an hour as I recall, in terms of the relationship of all of the criteria that need to be met. As you well know, we, the Air Force, are responsible up until the launch of a vehicle. So we were responsible for the launch forecast.

The forecast post-launch up to and including the landing of the shuttle, whether that be at Edwards or at Kennedy, is the responsibility of the Spaceflight Meteorology Group down at Johnson. They are the National Weather Service people.

So we coordinate every forecast that goes out to the Mission Management Team, to the launch director, to the flight director, with Johnson. We have a very close relationship with those people because, as you well know, and as depicted in the brochure that I've given you, there are launch criteria which are different

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from the RTLS criteria.

Oftentimes the weather looks extremely good over the Cape for launch in winds, for example, so that we don't have a wind constraint per se for launch, however we could have a crosswind at the shuttle landing facility, which would have to be considered by the management as to whether or not we would follow those guidelines.

So yes, there are, as you point out—and of course, we've got the trans-Atlantic abort weather criteria which have to be met, and the once around abort criteria, both at Edwards and at White Sands Space Harbor, that have to be met.

And so we have to meld all of these together in a coordinated forecast which encompasses all of those requirements, so that we can give the decision managers a better picture of what the weather looks like everywhere.

VICE CHAIRMAN ARMSTRONG: Can you characterize whether that is a difficult task in terms of being sufficiently able to accurately project the weather conditions at launch time in order to meet all of the constraints? Is it difficult, or do you have a high degree of confidence in that ability?

COLONEL KOLCZYNSKI: I believe the people on

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my staff and the people at the Johnson staff and the weather team do as good or better job than anybody can do in predicting the weather. But anyone that has done an analysis of forecasting knows that the farther away you get from an observation in terms of time, the less precise, accurate, your forecast is, and it drops off very rapidly after about six hours.

So when you're making a 12-hour projection, the likelihood of being 100 percent accurate is relatively—or is slimmer than it would be if you were making a three-hour forecast. But with the equipment provided by NASA and the kind of people that we have supporting the mission in terms of the weather people, I think, as I said before, we do as good a job as you can get done.

DR. RIDE: The weather at KSC, of course, is very dynamic. Do you feel that there are maybe times of year at the Cape where it is difficult for you to project even say 30 minutes ahead, as you have to do for an RTLS, or an hour ahead?

COLONEL KOLCZYNSKI: That is especially true of the two situations that I brought up to your attention. As you well know, when you get into a situation of very light winds in the winter time mornings, you get very close dew point depression. Does

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everybody understand what that means?

So that the atmosphere is relatively wet. If the winds get very light, we could get a fog condition, which is a problem for RTLS. And so that is a situation where one has to very cautiously watch the weather and watch the temperatures and winds.

The same thing with thunderstorms. Because of the dynamic nature of the weather at Kennedy, we can actually have storms build up right over Canaveral or Kennedy itself. We are doing a lot of—we are acquiring a lot of equipment to try and get a better handle on that.

We are getting equipment which will tell us about the vertical motion of the air. If we see an area where the air is moving in toward the center, we know there is only one place it can go and that's up, and that is a place that we can look far for potential storm development.

And so we're trying to get a handle, for example, on hour or two or even 30 minutes, for that matter, ahead of the storm developing, to be able to tell the decisionmaker, we know we've got a developing system right here, it may or may not cause us problems.

Does that answer your question?

DR. RIDE: Yes. Roughly what time scale have

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you seen storms, thunderstorms in particular, develop? Can you see them? Do they pop up within an hour of when you weren't expecting them, or is it—do you have an hour's warning or two hours warning?

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COLONEL KOLCZYNSKI: Basically, if they are going to pop up inside an hour or 30 minutes, we have a relatively unstable atmosphere, and we would anticipate that we will have storms in the area, at least rain showers, and probably thunder storms. Otherwise, we have a relatively good handle on the fact that storms will occur within the area, and when I say within the area, within 50 nautical miles, perhaps, of the Cape, at least six hours and sometimes even 24 hours in advance based upon the flow patterns that occur, based upon the atmospheric condition, whether or not we have a frontal system in the area, the stability of the atmosphere, the predicted stability.

CHAIRMAN ROGERS: Mr. Stevenson, Mr. Davis, in the performance of your duties, were you called upon to make a recommendation about launch or no launch as far as weather is concerned, either one of you or both?

MR. STEVENSON: Normally that is based upon the weather predictions. We, when the weather is bad, we will go to management and say we are totally out of it, there is no reason to try to launch.

CHAIRMAN ROGERS: What happened this time on 51-L?

MR. STEVENSON: For this particular time, the weather, the conditions were such that we were

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predicting that we would have frost on the vehicle, and therefore we had no reason to say not to load the tank with cryo.

CHAIRMAN ROGERS: But do you go to someone and tell them what your recommendation is, or your view on it?

MR. STEVENSON: Yes, sir, starting a day to two days before launch, we start to do that.

CHAIRMAN ROGERS: And what about the day of the launch? Did you have the occasion to make a recommendation on that day?

MR. STEVENSON: On the day of launch, whether we launched or not, yes, sir.

CHAIRMAN ROGERS: And to whom did you make the recommendation?

MR. STEVENSON: Well, as we have testified, once we came on station that night, which was roughly around midnight, we immediately began to see that we had a problem with the facility, and that was brought to the management attention starting with my Director of Engineering. It went on up the loop.

CHAIRMAN ROGERS: To whom?

MR. STEVENSON: Up through the management chain, and for the next ten hours prior to launch, we spent approximately four hours on the pad as a result of

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confrontations with management and our concern and their concern for what was going on out there, and we also spent approximately three or four hours in discussion with the management system discussing the results of our findings on the pad.

And so I think they are very responsive.

CHAIRMAN ROGERS: What was the result? Now, you've told me the system. What did you finally say to somebody at that point?

MR. STEVENSON: In the last meeting we presented the subtotal, let's say, of all of our findings. We were able to say that the facility ice, which we were all concerned with, would stay within the 16 feet or so of the FSS. We expressed concern for the effects of aspiration, which was an unknown, and we left it at that. We were not worried about the ice that would fall off the facility if we could be assured that the ice would stay within the 16 feet of the FSS and not be drawn into the left hand SRB flame hole and thereby become debris and be ejected into the vehicle.

The management system again wrestled with that for quite a while and made phone calls and finally decided that it would not be a safety of flight issue to launch.

CHAIRMAN ROGERS: And you made that

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recommendation yourself, that there would not be a problem as far as safety of launch was concerned?

MR. STEVENSON: Once the opinion had developed that the aspiration would not draw the ice into the SRB flamehold, we had no problem with saying that it was okay to launch.

CHAIRMAN ROGERS: Did you make that recommendation, and if so, how did you make it, orally or in writing.

MR. STEVENSON: Orally.

CHAIRMAN ROGERS: To whom?

MR. STEVENSON: To—well, starting with, let's see, Jesse and Arnie.

CHAIRMAN ROGERS: Mr. Moore and Mr. Aldrich?

MR. STEVENSON: Yes.

CHAIRMAN ROGERS: And was the same thing true in your case, Mr. Davis?

MR. DAVIS: Yes, it was. In fact, my suggestion that if we were going to fly, that we should do it as early as possible, was picked up. Mr. Moore specifically stated that he felt that that was true, too, and they proceeded that way.

CHAIRMAN ROGERS: But you were both satisfied there was no safety problem as far as this launch was concerned?

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MR. DAVIS: As far as the ice was concerned, yes, sir.

CHAIRMAN ROGERS: Or any other weather problem, related problems, or did you just concern yourself with the ice conditions?

MR. DAVIS: Well, in this particular case we were only discussing the ice, but I saw no other thing that would have caused me to bring up anything else.

CHAIRMAN ROGERS: In those discussions, did anyone express any doubt about the wisdom of flying this launch in view of the weather conditions?

MR. STEVENSON: Yes, sir.

CHAIRMAN ROGERS. Who did that?

MR. STEVENSON: Rockwell.

DR. RIDE: I think it may be important to point out here that the ice team is basically coming back and reporting facts, reporting what they have seen, and then acting in an advisory capacity, and you are not really in the decisionmaking chain for the launch.

MR. STEVENSON: We make recommendations based on what we found.

DR. RIDE: And you really don't do the analysis of what could happen to the orbiter tiles if the ice hits. You just calculate the trajectories of the ice that you have seen is that right?

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MR. STEVENSON: And again, based upon size, we know what damage would occur, yes.

VICE CHAIRMAN ARMSTRONG: What was your reasoning in recommending the earliest possible launch?

MR. DAVIS: I believe I mentioned it yesterday, but I will go through it again. Sunlight, first of all, is pretty transparent—ice is transparent to sunlight. The substrate underneath it in the case where we are talking about here is a dark gray, and it is rather rough, and so it absorbs sunlight and turns it into heat energy right at the substrate between the ice and the mobile launch platform, for example, and this is the part I was specifically referring to, and also the handrails that had the icicles on it. So the sun makes it turn loose at the bottom, so to speak, and it makes the sheet be loose so it could literally be picked up and sucked into the flame transient and flown up.

So we made every effort to get all of the even partially loosened ice off of the deck and to make sure that there is no loose ice out there.

So the longer you wait after we have done our job, obviously the longer you would have to absorb solar energy and turn some more ice loose.

VICE CHAIRMAN ARMSTRONG: The question was,

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your conclusion was that the earlier you launched, the less loose ice there would be, is that correct?

MR. DAVIS: That is correct.

CHAIRMAN ROGERS: Mr. Stevenson, you just referred to the fact that you heard Rockwell raise some question about the weather.

Would you tell the Commission what you recall about that and who said it and what they said?

MR. STEVENSON: Rockwell expressed concerns, they did not express a strong opposition to launch.

CHAIRMAN ROGERS: Who? Mention names if you are able to.

MR. STEVENSON: I believe it was Bob Glaysher.

CHAIRMAN ROGERS: And what time was that?

MR. STEVENSON: That was after the T-3 hour walkdown.

DR. RIDE: Was that the meeting at about 9:00 in the morning, or was that before you went out for the last time?

MR. STEVENSON: That was about 9:00 in the morning.

MR. DAVIS: It was the same meeting we had the meeting before we went out the last time, and we discussed it at 9:00 o'clock also.

CHAIRMAN ROGERS: Thank you very much. We

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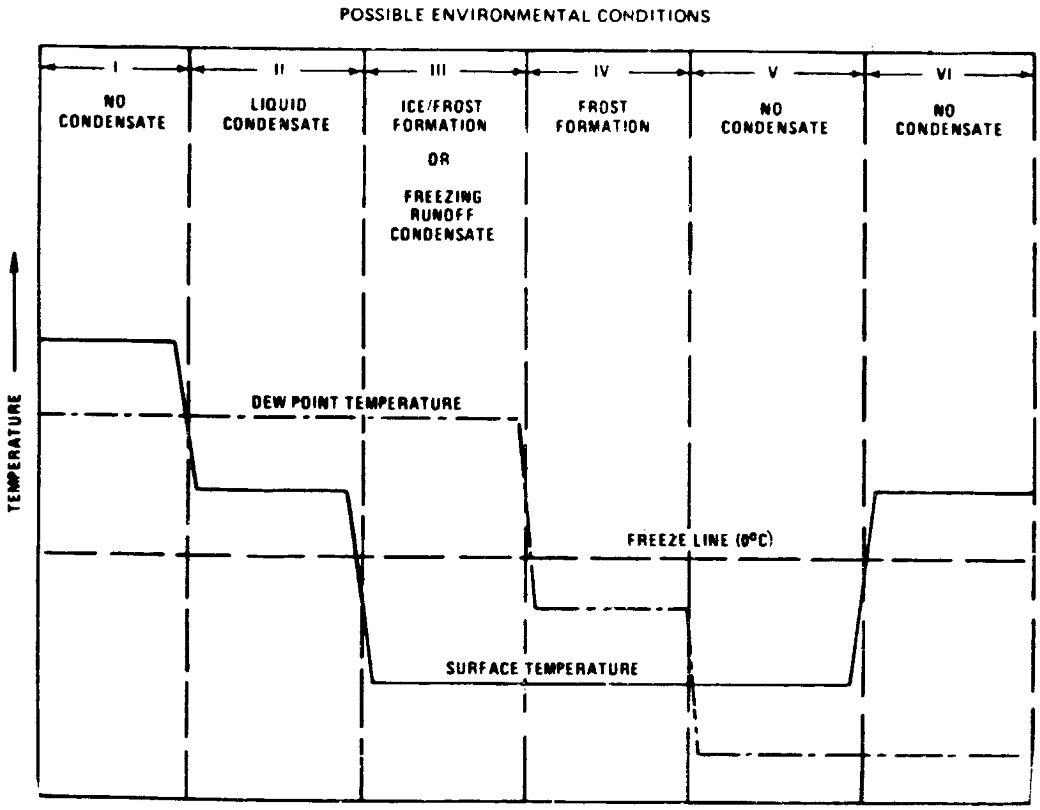
appreciate your testimony

DR. KEEL: Mr. Petrone, Mr. Glaysher, Mr. Cioffoletti and Mr. Martin, please.

(Witnesses sworn.)

CHAIRMAN ROGERS: Gentlemen, will you be seated and give your names and your present employment and what your jobs are?



POSSIBLE ENVIRONMENTAL CONDITIONS
I
NO CONDENSATE
II
LIQUID CONDENSATE
III
ICE/FROST FORMATION
OR
FREEZING RUNOFF CONDENSATE
IV
FROST FORMATION
V
NO CONDENSATE
VI
NO CONDENSATE
TEMPERATURE
DEW POINT TEMPERATURE
FREEZE LINE (0°C)
SURFACE TEMPERATURE

[Ref. 2/27-1]

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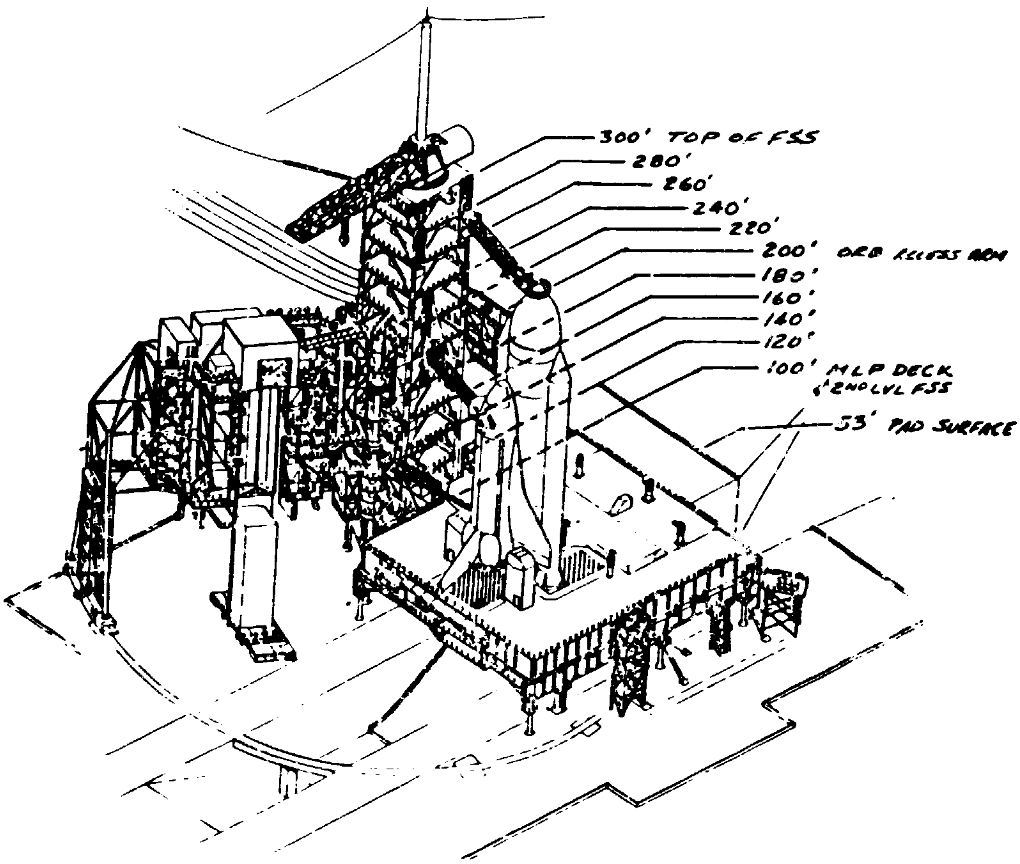
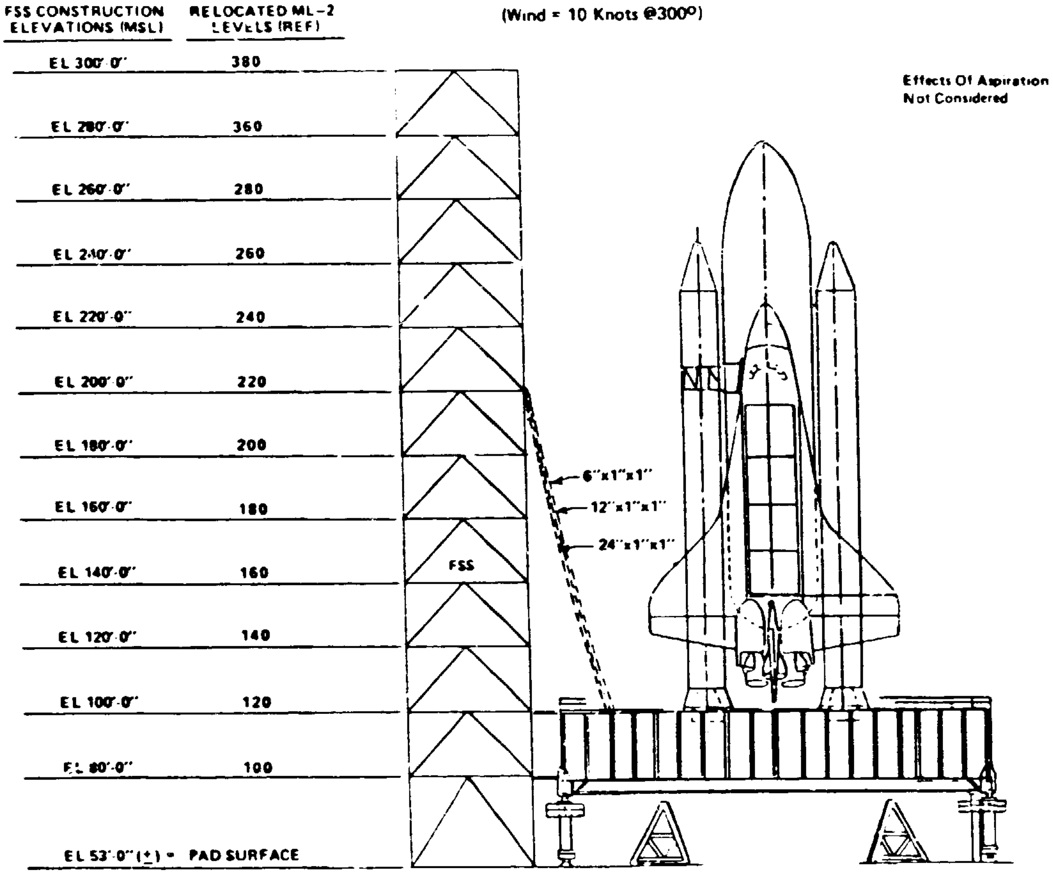
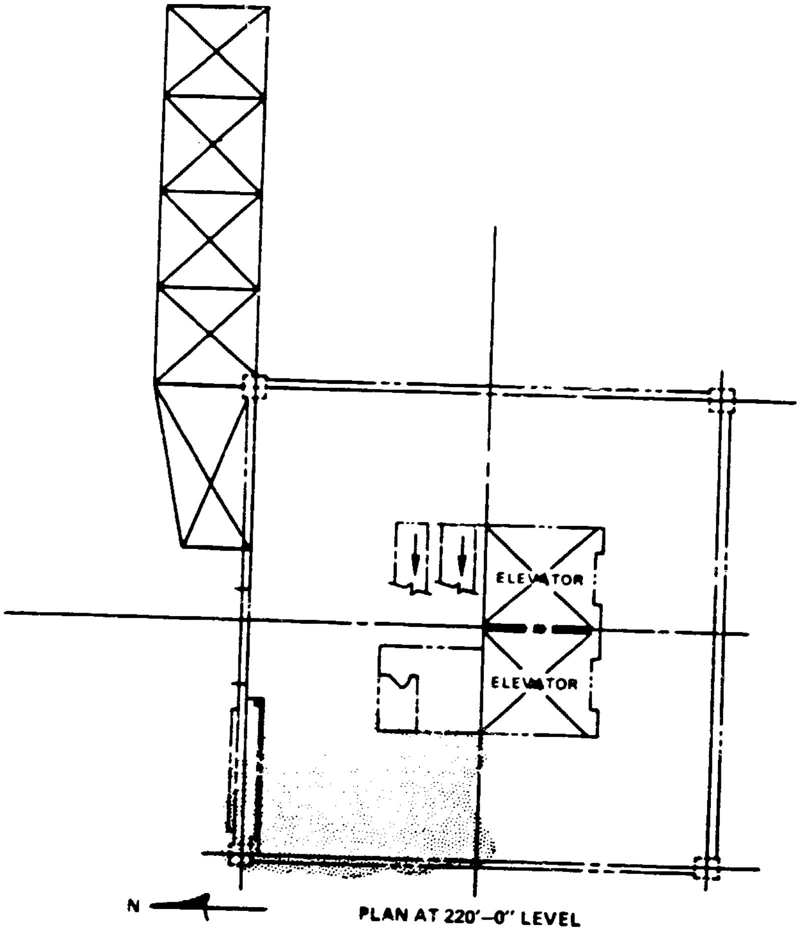


[Ref. 2/27-2]

FIGURE 2 FSS ICE DEBRIS TRAJECTORY
DURING 51–L LAUNCH – SIDE VIEW
(Wind = 10 Knots @300°)



[Ref. 2/27-3]



PLAN AT 220'–0" LEVEL

Figure 7

[Ref. 2 27-4]

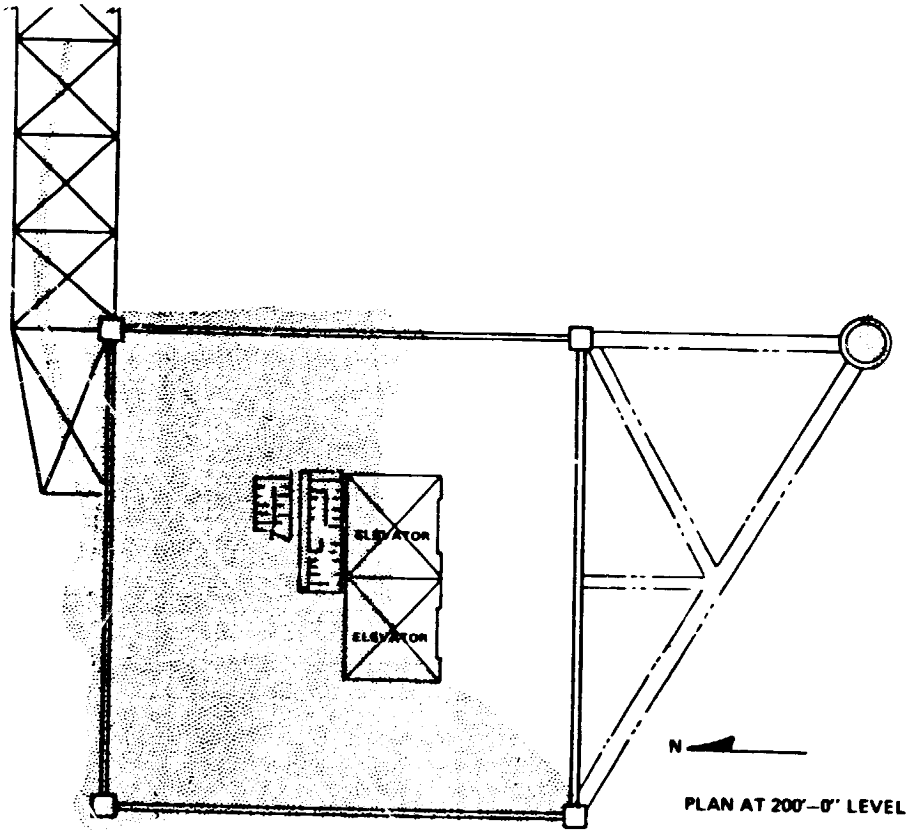


Figure 6

[Ref. 2/27-5]

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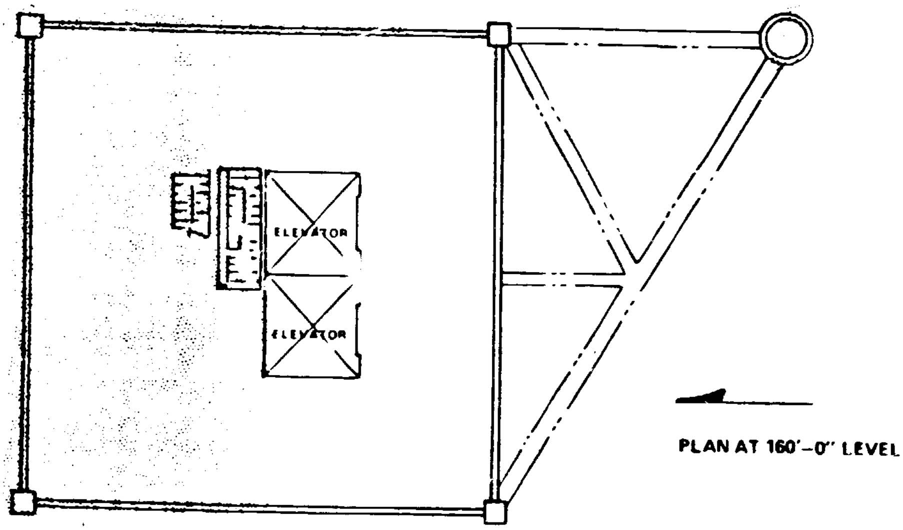

Figure 4

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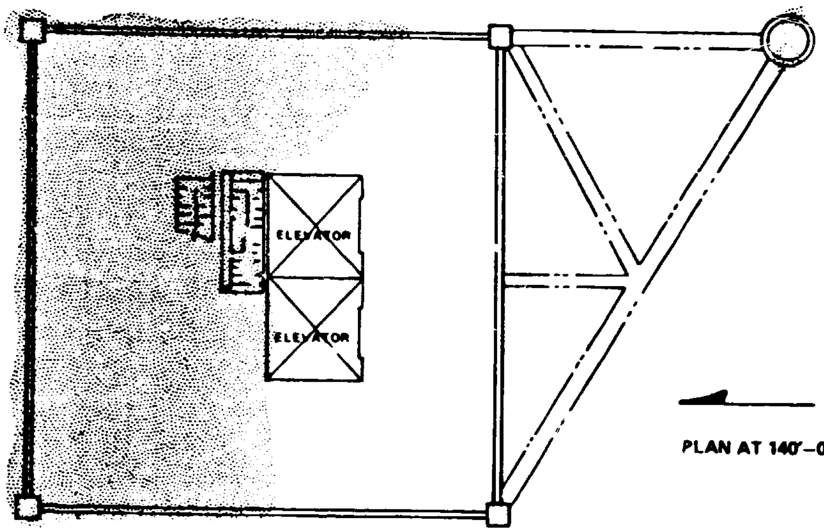
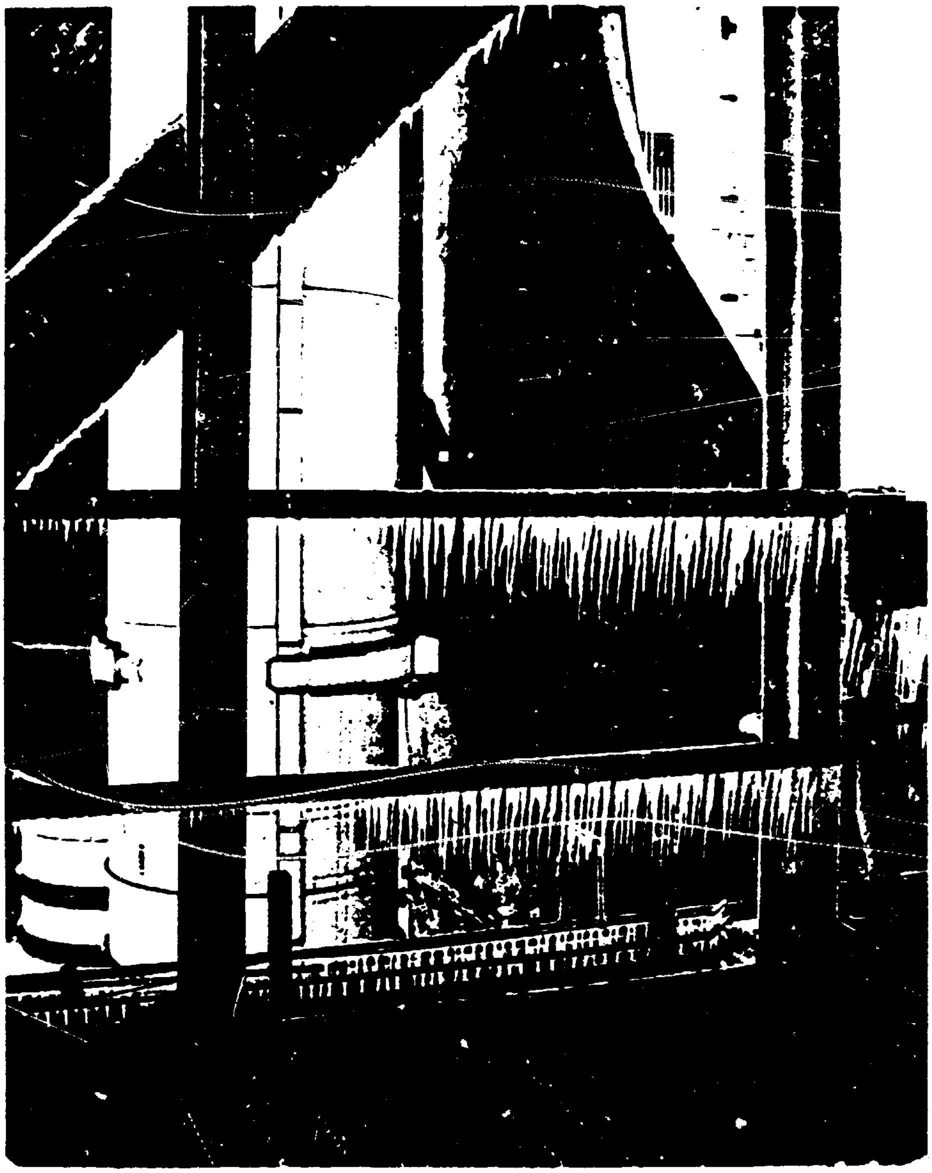


Figure 3

[Ref. 2/27-9]



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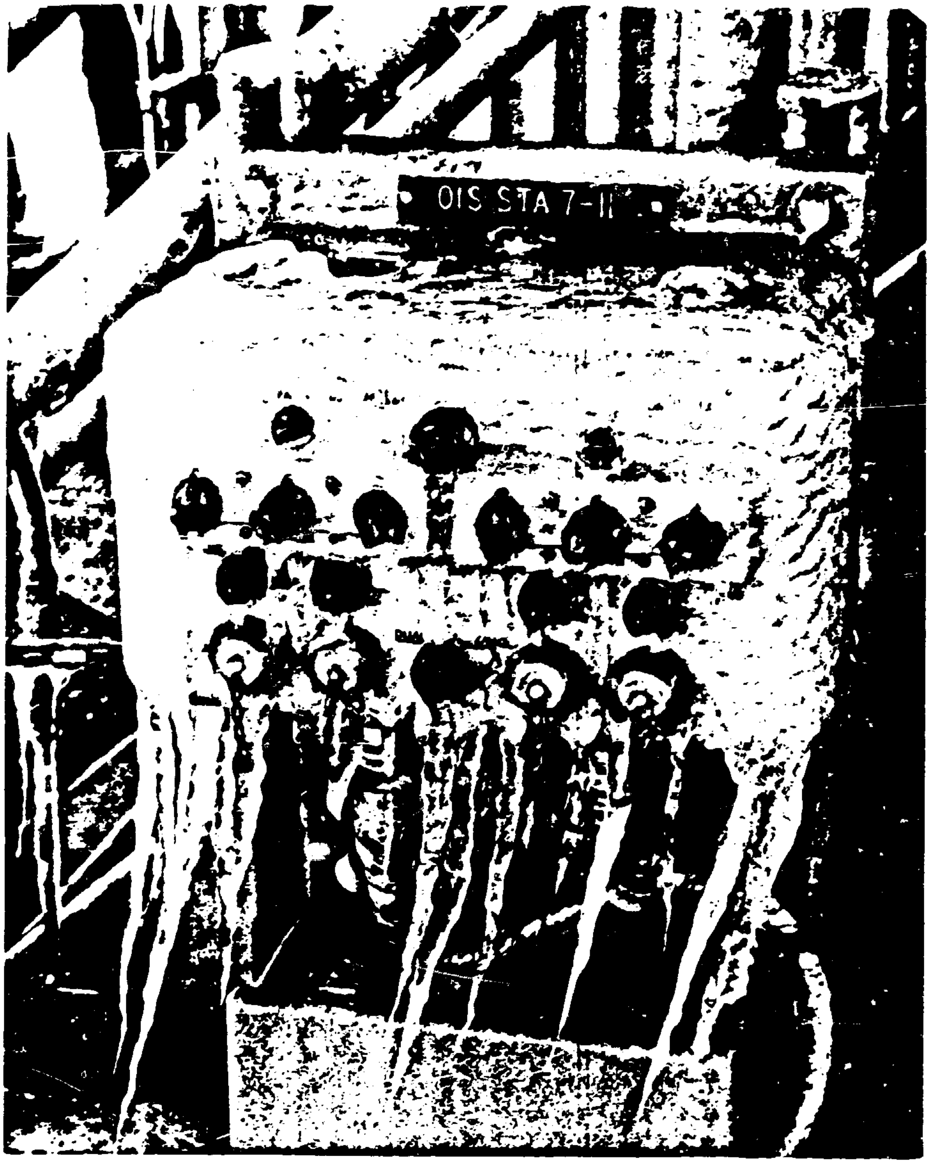

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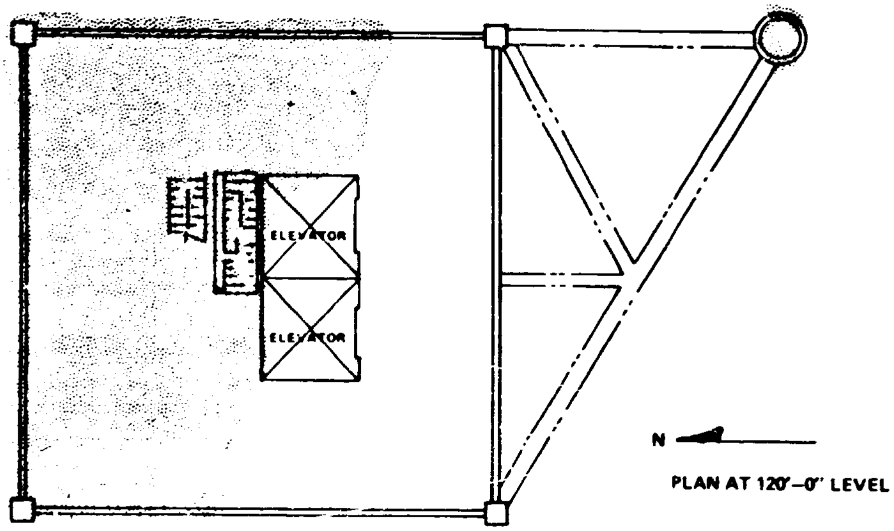
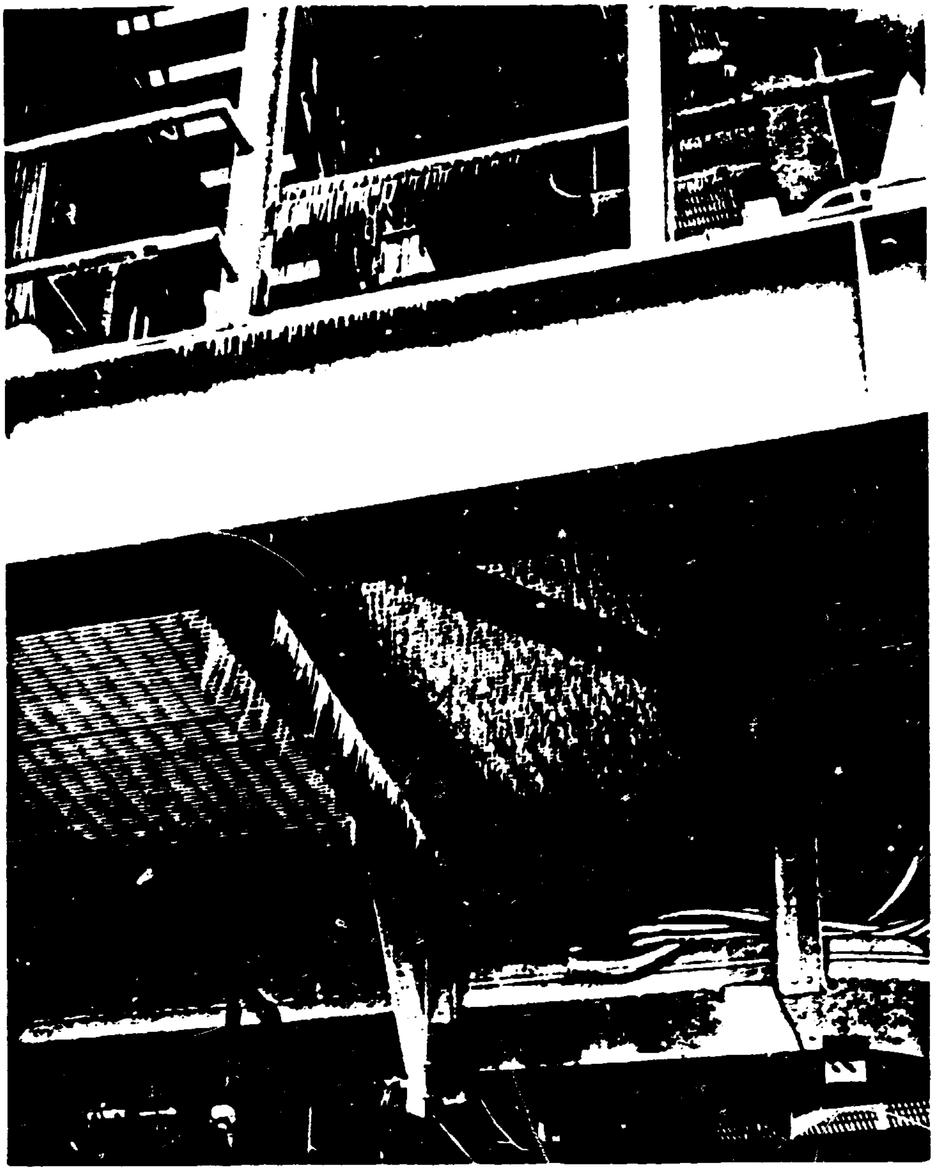


Figure 2

[Ref. 2/27-13]



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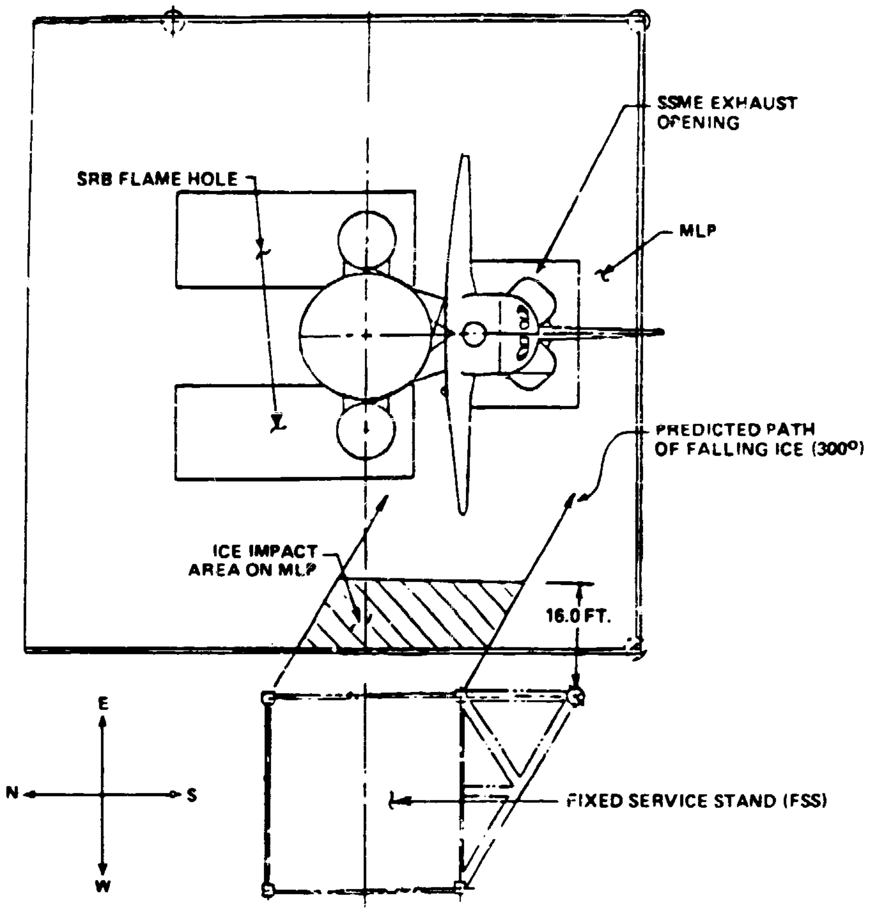

FIGURE 1 FSS ICE TRAJECTORY – DURING 51–L LAUNCH – PLAN VIEW

Effects Of Aspiration Not Considered
Particle Size : 6"x1"x1"
Wind Speed: 10 Kts

[Ref. 2/27-16]

[NOT REPRODUCIBLE]

[Ref. 2/27-17]

[NOT REPRODUCIBLE]

[Ref 2/27-18]

[NOT REPRODUCIBLE]

[Ref. 2/27-19]

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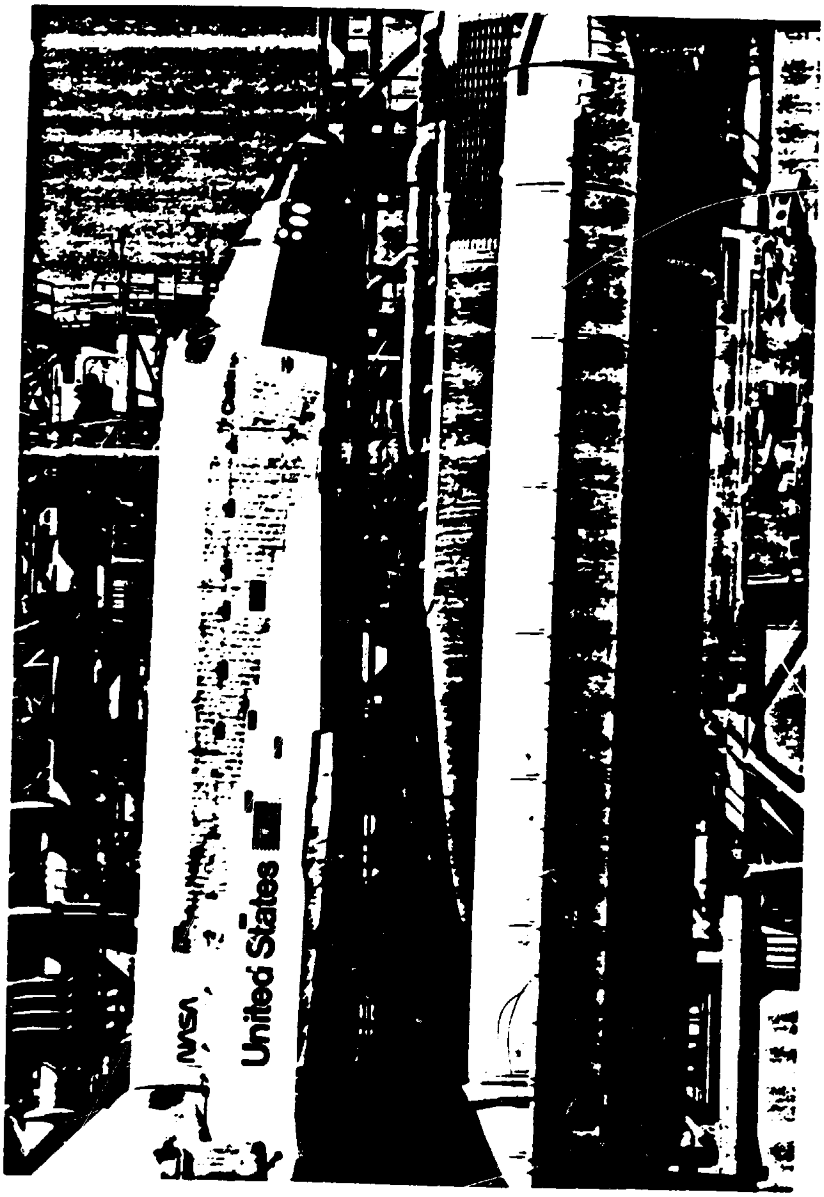
[Ref. 2/27-23]



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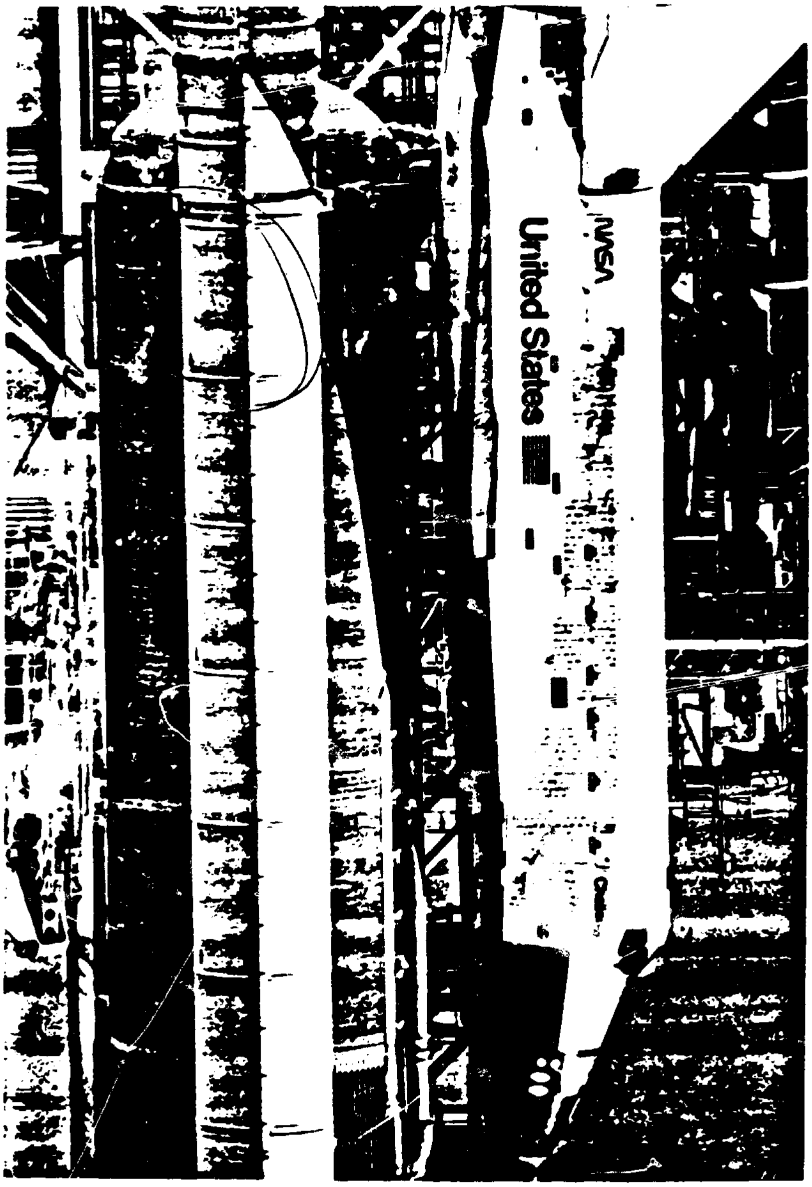

[Ref. 2/27-25]

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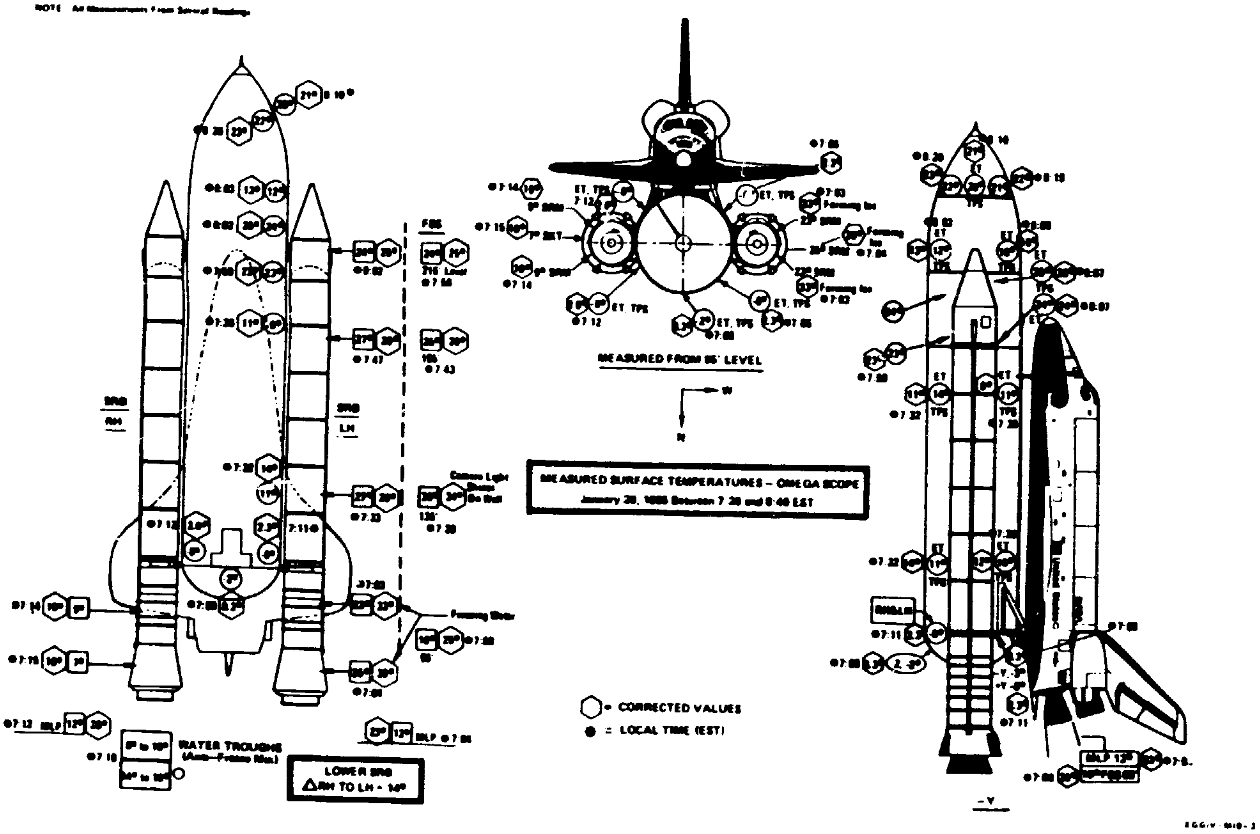

[Ref. 2/27-29]

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TESTIMONY OF ROCCO PETRONE, PRESIDENT, SPACE TRANSPORTATION SYSTEMS DIVISION, NORTH AMERICAN SPACE OPERATIONS, ROCKWELL INTERNATIONAL; BOB GLAYSHER, VICE PRESIDENT AND PROGRAM MANAGER, ORBITER OPERATIONS SUPPORT, ROCKWELL INTERNATIONAL; MARTIN CIOFFOLETTI, VICE PRESIDENT, SPACE TRANSPORTATION SYSTEMS INTEGRATION, ROCKWELL INTERNATIONAL; AL MARTIN, SITE DIRECTOR, LAUNCH SUPPORT OPERATIONS, KENNEDY SPACE CENTER, ROCKWELL INTERNATIONAL

(COMMISSIONER ACHESON RECUSED HIMSELF FROM THE HEARING DURING THE PRESENTATION AND QUESTIONING OF THE ROCKWELL WITNESSES.)

DR. PETRONE: Mr. Chairman, I am Rocco Petrone, President of the Space Transportation Systems Division of Rockwell International, located in Downey, California. I have been employed by Rockwell for five years. I have been in my current position for the last two years, and in the prior three with Rockwell held senior management positions in the Space Shuttle program for which Rockwell is responsible. Prior to joining Rockwell I served with NASA from 1960 to 1975.

My division is responsible for the design,

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testing, certification and manufacture of the Space Shuttle orbiter, and we provide operational support to the delivered orbiters to NASA. The division is also responsible for integration support and for cargo integration into the Space Shuttle System.

CHAIRMAN ROGERS: Thank you.

MR. MARTIN: I am Al Martin. I have been with Rockwell 33 years. My present position is I am the site director for Rockwell located at the Kennedy Space Center, and our organization provides technical advisory support to NASA, also logistic support and configuration management support. I have worked on the Shuttle Program for 13 years. I have been at the Kennedy Space Center four years, and prior to that I was the Chief Program Engineer for orbiter development at our plant in Downey, California. I also worked on the Apollo program for ten years, and one of my assignments there was Launch Director, director of launch operation for Rockwell for the Saturn S-II stage at the Kennedy Space Center.

MR. GLAYSHER: I am Robert M. Glaysher. I am Vice President and Program Manager, Orbiter Operations Support for the Space Transportation System Division, Rockwell International, based in Downey, California. I joined Rockwell 13 years ago. I have worked on the orbiter project for that entire time in various

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management capacities. I have been in my present capacity for approximately two years. My responsibilities as program manager are to provide operations support direction to delivered orbiters and also to provide logistics support activities in support of delivered orbiters.

CHAIRMAN ROGERS: Thank you.

MR. CIOFFOLETTI: I am Martin Cioffoletti. I have been with Rockwell for 20 years, approximately 13 years on the Space Shuttle program, and I am Vice President of System Integration and Cargo Integration, and the responsibilities there are to provide systems engineering support to the Level II Program Office at the Johnson Space Flight Center for both integration and cargo.

CHAIRMAN ROGERS: Mr. Petrone, you and I had a brief discussion on the telephone about the Rockwell participation in the events leading to the launch on January 28. The Commission would like to have you, in any order you care to, explain the participation of you four gentlemen in that process, with particular reference to the weather, who was at the meeting, what was said, because as we understand it, there was concern on your part as to weather. We would like to know exactly what was said and how that concern was

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expressed, and to whom.

DR. PETRONE: I had been at the Cape since Friday, and Monday afternoon after the scrub on that morning, I returned to Downey. I left at the Cape my two program managers you have heard from Bob Glaysher and Marty Cioffoletti, and I told them that I would be in our Mission Support Room in Downey for the launch scheduled for the next morning. Al Martin is our site director and is normally stationed at the Cape, whereas the two gentlemen here traveled with me for this particular launch and supported it.

I first heard about an ice concern about 4:00 a.m. Pacific Standard Time. I had gotten up and went to the support room to support this launch. We have people monitoring consoles, and I checked in, and they told me there was a concern, and when I arrived at about 4:40 a.m. PST, I was informed we were working the problem with our aerodynamicist and debris people, but very importantly, we would have to make an input to Kennedy for a meeting scheduled at 6:00 a.m. our time and 9:00 a.m. Florida time.

We had approximately an hour of work to bring together. The work had been under way when I arrived and was continuing.

At that time I got on the phone with my two

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program managers just to discuss background of where we were, how things stood, and what their concerns were locally. They described what they knew in Florida, and we also in Downey did television input, and we could see some of the ice scenes that were shown here this morning.

We arrived through a series of meetings to a top level discussion at approximately 5:30 a.m. PST, from which we drew the following conclusions: Ice on the mobile launcher itself, it could be debris. We were very concerned with debris of any kind at the time of launch. With this particular ice, one, could it hit the orbiter? There was wind blowing from the west; that appeared not to be so fast, that ice wouldn't hit the orbiter but it would land on the mobile launcher. The second concern was what happens to that ice at the time you light your liquid fuel engines, the SSMEs, and would it throw it around and ricochet and potentially hit the orbiter.

The third aspect is the one that has been discussed here of aspiration, what would happen when the large SRM motors ignite and in effect suck in air, referred to as aspiration, and ice additionally would come down, how much unknown.

The prime thing we were concerned about was

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the unknown base line. We had not launched in conditions of that nature, and we just felt we had an unknown.

As to specifically that trajectory the ice might fly, one can make estimates, but we felt that it was an unknown condition.

I then called my program managers in Florida at 5:45 a.m. and said we could not recommend launching from here, from what we see. We think the tiles would be endangered, and we had a very short conversation. They had a meeting to go to, and I said let's make sure that NASA understands that Rockwell feels it is not safe to launch, and that was the end of my conversation.

And with that, I would like to turn it over to my program managers and my site manager.

MR. GLAYSHER: Let me pick up. I was also alerted to the ice problem about 4:00 a.m., but this is Eastern Standard Time. I received a call from the base explaining the condition that ice was on the fixed service structure. I then made sure that the necessary wheels were in motion to get the proper people in Downey brought in so that they could evaluate the ice. That was also done through Mr. Cioffoletti. I then called about 6:00 a.m. and verified what was happening,

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and was informed there was a meeting going on on the subject.

I got to the base myself at about quarter to 8 Eastern Standard Time and then discussed with our Chief Engineer, Vice President of Engineering our position, and as Dr. Petrone mentioned, with him and developed a position that Rockwell would take at the 9:00 o'clock meeting that was scheduled.

At the 9:00 o'clock meeting, the ice debris team presented their report on the status of the ice. Following that, various people were asked their recommendations and their positions. When I was asked Rockwell's position, I reiterated that there were three major unknowns in evaluation of the ice. As Dr. Petrone indicated, the first event was aspiration effects. The second was ice that would ricochet from the fixed service structure and head toward the vehicle. And the third category of unknown ice was ice that was resting on the mobile launcher platform at engine ignition.

The fourth category of ice, which was ice in the trough, had already been discussed and resolved once the debris team had removed that ice. Those three categories of ice that I mentioned, however, we have no data base on which to base judgments of that. This is the first time it has occurred. It is not a design

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condition for the orbiter.

We therefore felt that since we were in an unknown condition and were unable through any analytical techniques to predict where the ice would go or the degree of damage that would result should that ice strike the orbiter TPS, I then gave the following recommendation to NASA in which I said that Rockwell could not assure the safety of flight, or let me give you a better quote, if you would.

Yes, my exact quote was—and it comes in two parts. The first one was, Rockwell could not 100 percent assure that it is safe to fly which I quickly changed to Rockwell cannot assure that it is safe to fly.

We then had a discussion about what that meant and the data base that we didn't have in effect. They then moved on to Mr. Al Martin and asked for a position or an opinion from him.

So I will ask Al to pick up there.

MR. MARTIN: In the 9:00 o'clock meeting, Bob Glaysher was our spokesman, but I was asked also if I had anything to add, and statements that I made in the meeting were that I made a statement like it has already been said, meaning that Bob Glaysher had stated the Rockwell position. I also added that we do not have the

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data base from which to draw any conclusions for this particular situation with the icicles on the tower, and also, we had no real analytical techniques to predict where the icicles might go at lift-off.

The other thing that I did was review the fact that prior to each launch there is great care taken to make sure that there is no debris out on the launch pad. A day or two before launch a crew goes out and they walk down the entire tower and walk down the mobile launcher surface, and also the concrete apron around the launch pad for the purpose of removing any debris such as nuts, bolts, rocks or anything that might be there.

And I drew the corollary that the icicles in this case could very well become debris, that they might become dislodged from the tower when the SSMEs ignite a few seconds before liftoff, and they could impact on the mobile launcher surface and then become debris when the solid rocket motors lifted off, and we had no way of predicting that.

So I was drawing a corollary between the care that is normally taken for debris and painting a picture, that the icicles appeared to me to be in that same category. And so those were my only comments in that meeting.

MR. CIOFFOLETTI: Similarly, I was called in

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and told about the problem and came into the 6:00 o'clock meeting which you heard about a few minutes ago, and at the conclusion of that meeting I spoke with Mr. Dick Kohrs, the Deputy Program Manager from Johnson Space Flight Center, and he asked if we could get the Downey folks to look at the falling ice and how it might traverse toward the vehicle, and also, did we have any information on aspiration effects.

So I did call back to Downey and got the John Peller folks working on that problem, and they did, as you saw from Charlie Stevenson's sketches, predict that the ice would travel only about halfway to the vehicle, free falling ice carried by the winds. So we felt that ice was not a problem. However, it would land on the mobile launch platform. That we considered a problem. We also investigated the aspiration data base we had, and we had seen the aspiration effect on previous launches where things were pulled in to the SRB hole after ignition, but we had never seen anything out as far as the fixed surface tower. So we felt in fact it was an unknown. We did not have the data base to operate from on aspiration effects.

At the 9:00 o'clock meeting I was asked by Arnie Aldrich, the program manager, to give him the results of our analysis, and I essentially told him

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STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

1805

what I just told you and felt that we did not have a sufficient data base to absolutely assure that nothing would strike the vehicle, and so we could not lend our 100 percent credence, if you will. to the fact that it was safe to fly.

CHAIRMAN ROGERS: When Mr. Petrone made his statement, he didn't use the words 100 percent sure, I suppose. Nothing is 100 percent sure.

MR. CIOFFOLETTI: I didn't use those words either. I just paraphrased that.

CHAIRMAN ROGERS: Why don't you testify what you said, please.

MR. CIOFFOLETTI: I said I could not predict the trajectory that the ice on the mobile launch platform would take at SRB ignition.

CHAIRMAN ROGERS: And?

MR. CIOFFOLETTI: And that was the end of it.

CHAIRMAN ROGERS: But I think NASA's position probably would be that they thought that you were satisfied with the launch.

Did you convey to them in a way that they were able to understand that you were not approving the launch from your standpoint?

MR. CIOFFOLETTI: I felt that by telling them we did not have a sufficient data base and could not

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analyze the trajectory of the ice, I felt he understood that Rockwell was not giving a positive indication that we were for the launch.

CHAIRMAN ROGERS: Mr. Glaysher, did you make it clear that you felt there was a safety aspect and that you were not approving the launch?

MR. GLAYSHER: Yes, we actually discussed our position and I stated more than once during the meeting Rockwell's position that we could not assure that it was safe to fly. It was stated when I first was asked to give our position, and it was also my last statement at that meeting, as the meeting wound up. I also reiterated the statement several times.

And so we felt that we had communicated Rockwell's position and that we felt it was unsafe to fly.

DR. RIDE: Had Rockwell ever taken that position before on previous launches when the launch had occurred?

MR. GLAYSHER: No, this was the first time where we had been in a position where we really had no data base from which to make a judgment, and this was the first time that Rockwell has taken an unsafe to fly position.

GENERAL KUTYNA: Dr. Petrone, you've got a lot

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more experience than I have in this business, but the few launch conferences that I have been in on the question is very simple. Are you "go" or are you "no go" for launch, and "maybe" isn't an answer. I hear all kinds of qualifications and cautions and considerations here. Did someone ask you are you go or no go? Was that not asked?

DR. PETRONE: At this particular meeting, I was not in Florida, and so I cannot answer that. It had been done at earlier meetings. This was a technical evaluation of a series of problems, and we talked about debris hitting the TPS and the tiles, and the long series of reviews that we had done that morning and all led us to a conclusion that they were not safe to fly. And we transmitted that to our program managers along with the technical evaluation quickly of why we had arrived at that.

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So much of it is how the question gets raised because earlier we had aspiration work, ricochet work, a number of things which we did, and then we came up with our recommendation.

CHAIRMAN ROGERS: And your recommendation now you say it was, it was unsafe to fly?

DR. PETRONE: Correct, sir.

CHAIRMAN ROGERS: Thank you very much,

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gentlemen.

Let's take a ten minute recess.

(A brief recess was taken.)

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CHAIRMAN ROGERS: The Commission will come to order.

I would like to ask Dr. Petrone to take the stand, please. Dr. Petrone, just a couple of questions.

Did you or any of your associates as far as you know make any phone calls after the discussions that were referred to just before the recess?

DR. PETRONE: I received a phone call from my program manager back to me. I had no talks with NASA that morning. I was transmitting through my officially designated representatives, my two program managers.

CHAIRMAN ROGERS: But as far as you know, did they have any further conversations with the NASA people after the one that was testified to?

DR. PETRONE: There was one conversation that went on, I guess, while the meeting was going on. The meeting had started, apparently, but had not ended, where my chief engineer received a phone call in Downey from a NASA counterpart in engineering.

CHAIRMAN ROGERS: And what was your man's name?

DR. PETRONE: John Peller.

CHAIRMAN ROGERS: And do you know the substance of that conversation?

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DR. PETRONE: Really not. I could only hear just a little part of it. I did not participate in that particular phone call. I just could hear a little bit of it, say on one side.

CHAIRMAN ROGERS: Well, without getting into the specifics, just generally speaking what was the topic of conversation?

DR. PETRONE: Mr. Peller is here today, and if you care to have that it would be far better for him to talk, sir.

CHAIRMAN ROGERS: Yes. We will have him wait here. Mr. Aldrich asked to have the opportunity to testify next.

Do you have any other questions, Mr. Armstrong?

VICE CHAIRMAN ARMSTRONG: Were you or your program managers informed of the activities that were going on at the Cape during the interruptions to the count and the resumptions to the count?

DR. PETRONE: My two program managers were at the Cape.

VICE CHAIRMAN ARMSTRONG: You were informed, I assume, of the resumption of the count?

DR. PETRONE: Yes. After the meeting I

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received a phone call from Bob Glaysher, who sat here this morning, and then followed by another phone call a few minutes later from Al Martin, in which he told me he had made his input at the meeting, I believe called the 9:00 o'clock meeting, and told me that it looked like NASA was going to go ahead, and we knew that was within a few minutes of the loading of the crew proceeding.

VICE CHAIRMAN ARMSTRONG: Now, clearly when they resumed the count you knew that your recommendation essentially had been either considered and overruled or dispositioned in some way?

DR. PETRONE: That's right, sir.

VICE CHAIRMAN ARMSTRONG: And subsequent to that point in time, were there any other—did you take any other opportunities through yourself or your people to express your opinion again?—

DR. PETRONE: Mr. Armstrong, I felt we had expressed our opinion to the proper level, on the proper occasion of the meeting that had been set up for it. I felt I had done all I could do.

VICE CHAIRMAN ARMSTRONG: Thank you.

DR. RIDE: Did it surprise you that NASA picked up the count?

DR. PETRONE: I was disappointed that they

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did, yes.

CHAIRMAN ROGERS: Have you had any discussions since that time with NASA people about it?

DR. PETRONE: Only one, sir. Mr. Aldrich gave me a call reflecting on what time—had I made a later phone call, I believe was the question he asked me. And I said, no, after the input I made through my program managers at that meeting, I made no further calls. And that was the question he had posed to me.

CHAIRMAN ROGERS: I see. Okay. Thank you very much, Mr. Petrone. I believe that's all. Mr. Aldrich?

(Witness sworn.)

CHAIRMAN ROGERS: I might say that we have a somewhat different schedule of witnesses, but Mr. Aldrich asked if he could testify next, and of course we said fine.

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TESTIMONY OF ARNIE ALDRICH

MR. ALDRICH: Well, Mr. Chairman, if this doesn't please your order, I would be perfectly happy to go in any sequence that suits the Commission.

CHAIRMAN ROGERS: No, that's fine. I just want to be sure that it is understood that we are acceding to your wishes, and I think it is a perfectly proper request and an actual request, and I think it fits in in a consistent manner with what I said at the beginning of these hearings, that at any time anything is said that requires an answer, or at least appears to require an answer right away, we try to provide it. And I think this is a perfect example of that.

I think you should testify now about your recollection of the events that we talked about here just a moment ago.

MR. ALDRICH: Yes, sir.

CHAIRMAN ROGERS: Mr. Aldrich, you've already identified yourself and have testified previously, and we know about you and your background. We know that you're—I guess it's you and Mr. Moore are the two key people in the final decision whether to launch or not to launch the Challenger.

MR. ALDRICH: Yes, sir, we are. I would say that, in a different category—that is, the launch

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director proceeds with all conditions normal by the documentation, and all of the procedures and guidelines and constraints that are built into the launch process, and Mr. Moore and myself deal with unusual situations or concerns of the kind that the Commission has been discussing.

And we frequently have those on many launches. In fact, I would like to refer to some of that in my testimony.

CHAIRMAN ROGERS: Please go ahead.

MR. ALDRICH: I had prepared a discussion that started at the beginning of the period leading up to this launch on the L minus one day. I think the interest here probably is to respond to the previous testimony on the ice team and to the—

CHAIRMAN ROGERS: Yes, we would like that, because we plan a little later in the day maybe to have you and Mr. Moore and others testify overall on some of these matters. But for the moment, and particularly to continue the proper sequence, if you could confine your remarks to the ice problems and the testimony that we just heard from the Rockwell people, I would appreciate it.

MR. ALDRICH: Yes, sir. I will do that. Then let me pick up at the point on the scrub of January

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27th. We scrubbed on January 27th after dealing with the problems with the crew hatch, on closing that hatch and then removing the ground support equipment, and then the requirement to realign the inertial measuring units delayed our launch several hours from the planned launch time.

And during those several hours, the crosswinds, which have been mentioned previously, the high winds following the weather front, arrived at Cape Kennedy and were outside our guidelines for landing at the Kennedy launch facility. In the event of an RTLS abort, we have a requirement of crosswinds to be less than 15 knots for the orbiter to land, and they were in excess of that.

And so we scrubbed and immediately had a Mission Management Team meeting. Perhaps in later testimony I can discuss more of what was discussed in that Mission Management Team meeting, but basically there was thought to be no problem with clearing the hatch condition for the following day, that is to launch on January 28th, and there were no other system or complex issues, launch complex issues.

The weather was forecast to continue to be clearing and the crosswinds to subside. So that all parameters appeared to be good for a launch of the

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following day, on the 28th, except for the additional weather forecast that now behind the crosswinds was coming the clear sky, the clear air, and the very cold weather which has been discussed in some detail.

In fact, as was discussed here earlier, in that Mission Management Team meeting it was quickly determined that the only issue was in fact the cold weather that was coming, and it was predicted to be 23 to 27 degrees Fahrenheit at the coldest part of the night, and then rising into the thirties at launch time.

We then asked—this is a Mission Management Team meeting, and I might refer back to what the Mission Management Team is. The Mission Management Team is an assemblage of government managers, NASA and DOD, who have responsibilities for various aspects of the shuttle system. And we utilize it both during this pre-launch period and we use it after flight and launch.

We have regular Mission Management Team meetings in Houston while these flights progress, right up to the landing. And it is our policy after the L minus one day meeting, which is a broader meeting, to deal with questions closer into the launch or as the launch slides in what is called a Mission Management Team meeting.

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It is quite a large representation and most often not all of the people that are members are actually in attendance. Given what the subject is and the content of what's being discussed, particularly here is a decision to revert from a scrub on the 27th and issues with picking up with the various elements for the launch on the 28th, and the project people from the various centers that were pertinent to that were in this meeting.

But there are other Mission Management Team members who were not there and we would not have expected to be there.

We also have to consider whether at the cross-Atlantic sites—for this mission we had trans-Atlantic abort into Dakar and into Casablanca, and particularly Dakar had been very troublesome for its launch weather acceptability to allow us to launch so that if we had an abort we would come in there.

However, they were also in a go condition for the January 28th time period as predicted by the weather forecast. So the only issue was the temperature, and we asked each of the projects, the Kennedy launch and landing project, the orbiter, the external tank, the solid rocket boosters, and the space shuttle main engines, how they felt about launching with temperatures

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as predicted in this Mission Management Team meeting.

And each of them listed the concerns and constraints they might have with temperatures that I have just described. They each did so list, and the one major concern that came out of those lists—and I would be glad to go through those with you, either now or later—is the fact that the launch facility was going to experience cold temperatures, and in fact that could give them problems with their water systems and perhaps some of the other systems on the launch complex.

As has been pointed out in previous testimony, the STS 51-C launch in January of 1985 was attempted on a very cold night, and in fact we did have launch lines break and ice and freezing, and the conditions were such that the launch complex was not able to proceed. It had enough problems that it precluded consideration to proceed with the launch, and so that was a known problem.

Over the ensuing year, a freeze protection plan had been put in place at the Kennedy Space Center to deal with specifically that kind of an issue, so that we could proceed to launch under those conditions.

Kennedy reported in this Mission Management Team meeting that the freeze protection plan was going

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to be put in process that dealt with draining the fire X water systems that are up on the fixed structure, the fixed support structure that Charlie Stevenson referred to a few minutes ago.

They would be drained back to the first shutoff valve and then allowed to dribble, to drain, the way you might do in your home on a cold night to keep water pipes from freezing.

They also said the eye washes and the showers that are up on the fixed support structure would be allowed to drain into their drains to keep them from freezing and bursting, and it is in fact these drains that were allowed to run during the night that caused the ice that we saw in the pictures.

This ice didn't come from the climatic conditions themselves or from an unknown condition. They came in fact from the procedures that were taken intentionally in order to protect the facility and to enable it to do its job to proceed with the countdown.

They also indicated that they would be required to put the antifreeze in the sound suppression water troughs that you've seen pictures of to keep them from forming ice, because ice under the vehicle when it ignites is an issue.

Other issues were discussed with the KSC and with the orbiter and with the SRB and with the ET.

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There were no issues on the space shuttle main engines. All of them were thought to be well manageable.

And at the end of that meeting—I should say that I am the chairman of the Mission Management Team meeting. However, when Mr. Moore is in attendance, I make the decisions at that meeting and conduct the meeting, but I yield to him for the final decision. And collectively he, following my recommendation, agreed that we should proceed to tank, with the decision to launch on January 28th.

However, we asked all members at the Mission Management Team meeting to continue to consider and review the situation of the temperatures and to bring any additional issues that arose to our attention.

We then left that meeting, and in mind at that time I was clearly concerned about the ice on the facility as a constraint to launch, because I knew that we would be dealing with that as a problem on the following day, after we had gone through the cold night.

The next input that I had with respect to the conditions at the launch pad came at 11:30 that evening at the motel. There was a call from Larry Mulloy and Stan Reinartz and one of several KSC console operators, and the report that I would make is identical to the one

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you heard yesterday.

That is, they discussed the concern with the recovery ships dealing with the high winds. The same winds that had given us the problem for the crosswind at the landing site were causing high seas and the ships were off station.

And Larry discussed how we dealt with that, and I could say some more about that at another time if you would like or now. But my concern was first for the ships and then whether that should be a constraint that would cause us to stop tanking, and I felt we should proceed to tank and we would discuss it at the following day if it was continuing to be an issue.

CHAIRMAN ROGERS: At that time, did they tell you that there had been serious concerns expressed by Thiokol and Thiokol engineers, and that they had had a long teleconference on the subject, and that first Thiokol had recommended against launch and secondly management, in the person of Mr. Kilminster, had changed its mind and Thiokol then had decided to recommend launch?

Did you know any of that sequence at all?

MR. ALDRICH: None of that was discussed, and I did not know until after the 51-L launch that there had been such a meeting.

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CHAIRMAN ROGERS: Thank you.

MR. ALDRICH: However, there was a second concern discussed in that telephone call, and that was really raised by Larry Mulloy, but also by the KSC people who were on the same call with me, and that was that they were beginning to experience the ice that we had expected on the launch pad.

It was not specifically identified at that time as to the various locations, and they knew that that was going to be an issue for them during the night. However, the countdown was proceeding normally at that time.

We had a second call during that morning. We had a call at 3:00 a.m., and that was made to Mr. Richard Kohrs, who is my deputy, who stays in the same motel I do. And that report was to say again that the countdown was proceeding, but that they were one hour behind schedule and they had had problems with the formation of ice and the temperatures of the facility, but the delay was caused in fact by electronics problems with the console.

And they were calling to notify us that everything the next morning would be one hour later than we had all expected.

I arrived at the firing room about 4:30 a.m.

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on January 28th, and in fact the countdown was proceeding normally, satisfactorily at least, with no new problems that the ones I just discussed. In fact, the offshore winds had subsided and at that time they had predicted that the recovery ships would be on station and would be able to provide support.

The ice team had been at the launch pad from 1:30 in the morning until 3:00 in the morning to assess this icing condition. They weren't there doing their normal job, looking at the external tank. They were in fact assessing the facility and the launch vehicle in general, because the conditions were different.

They had come back and made a full report to the engineering teams that they report to, and I will tell you who those teams are here in just a minute. They made that full report, however, and that is not a standard time for them to go out. That was a unique inspection and a unique report and a unique set of considerations.

I talked to the launch director at that time when I arrived, and that was in fact the issue we discussed. And we both agreed that the proper thing to do was to let the engineering teams assess what had been found by the earlier ice inspection and to allow the normal T minus three-hour ice inspection to proceed, the

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one we do every flight, and get some additional data on the conditions as we came into the daylight time period.

The ice issue that we were concerned about is the one that has been discussed, that is damage from this ice to the thermal protection system of the orbiter, such as it will cause it to have a problem later in flight during the re-entry phase if there is significant damage to the thermal protection system. It is not other issues related to the ice.

I don't know of—I think all of us were talking about that as the singular issue about this icing condition. For most launches, we do send this ice team out to assess the condition, primarily the external tank, at T minus three hours. In fact, the ice team was created for that purpose very early in the flight program.

We found conditions—we expected there was icing on the tank, and that clearly could be a threat to the orbiter, depending on how it manifested itself and how thick it was, and some very specific plans and procedures and techniques were put in place so that every flight the ice on the tank is inspected and characterized and reported to the engineering teams, assessed, and decisions made.

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They have a very detailed process for doing that, much more than we went into here today. The icing starts when you put liquid oxygen and liquid hydrogen into the external tank and, depending on the humidity and the temperature and the winds, you can get a wide variety of patterns of ice or no ice on the tank. And having the coldest day doesn't necessarily represent the most concern or issue for the ET.

In fact, this was a particularly good day for the external tank in terms of generating ice on its external surface.

When the ice team finishes their report, they actually can report, as you heard, as they're in process over the loops, and sometimes over television they can make extensive reports while in progress, or they can wait until they come back and report in detail on the loop to the respective engineering teams that are in place.

There is an engineering team in place in Florida in an alternate firing room. It's adjacent to the one that is controlling the launch, and in it are engineering managers for each of the projects: external tank, orbiter, main engines, solid rocket boosters. And they have with them key technical people who are there specifically for analyzing unique problems or possible

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failure modes that come up while we're going through a countdown that weren't anticipated as being part of a normal count sequence.

Particularly the orbiter team and the external tank team are well aware of threats from ice and icing, and they listen in detail, as does the Kennedy engineering team, to these reports and determine whether there are concerns and if so how to assess them.

CHAIRMAN ROGERS: I notice you excluded the boosters in what you just said. You said there was attention paid to the effect on the orbiter and the external tank. Wasn't the same attention paid to the boosters, too?

MR. ALDRICH: I was discussing the way the process worked in the past, and it is probably not different particularly for this countdown. There was not an icing question or concern known to me about icing on the solid rocket boosters. They don't normally form ice.

And in this case, as was reported by Mr. Stevenson, there was some ice found on the skirt of the left SRB, but that was removed by the ice team specifically for the condition I will talk about later. But in a normal launch, it is a concern with the external tank and with the orbiter, and checks for

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things like leaks and propellant line interconnections also. But the ice part of the job deals with those two elements of the launch system.

In support of the orbiter team is an engineering team in Houston, in a facility called the mission evaluation room, and back there there is more extensive engineering available to them. They have many of the loops that are available at Kennedy Space Center, or at least some of them, and they also have data live real time from Florida.

And at certain points during the countdown, they have television. So the key people that are in Florida in this alternate firing room communicate with the orbiter people that are at that location in Houston and in support of the orbiter people in Houston.

I can't remember what Mr. Petrone, what Dr. Petrone or Dr. Glaysher mentioned, but there is in Downey also a mission support team there on engineers at the contractor's facility, that also works with these engineering teams and has data to deal with questions and decisions of an engineering nature.

On the external tank side, there is a support facility in Huntsville called the Huntsville operations support, HOSC, Huntsville operations support center. And there also are key engineers for the external tank,

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solid rocket booster, space shuttle main engines, who have live data from Kennedy and have loops from Kennedy so they can communicate in detail.

The ice team reports are made to these engineering teams, and those reports are assessed and issues or lack of issues are determined based upon those assessments.

I sit in the firing room for launches with a number of other key managers who you've talked to and heard about. I sit in a place that is called the operations support room. It is a little corner of the primary firing room that looks down on the firing room.

In there is Mr. Moore, Mr. Mulloy, Mr. Reinartz, Dr. Lucas. I can give you a complete listing of who is there. I am trying to give you a feel for the kind of people who are there. Also Mr. Richard Colonna, who is the orbiter project manager from JSC, is there. In fact, he sits right beside me. Mr. Richard Kohrs is my deputy; he sits beside me. And a number of other people we could go into as you want to find out more about that.

About 8:30 in the morning, about the time that the ice team was returning from the launch pad from their standard inspection, Mr. Colonna, orbiter project manager, reported to me the assessment of the

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engineering team was that the icing condition on the launch pad they believed was looking very favorable to proceed with the launch, and they were waiting for the final report of the ice team after they returned from their visit.

But they had been analyzing the previous reports and data and felt quite good about it. However, they also reported that Rockwell might have some concern with that recommendation.

As I previously reported, although the normal channel is for the orbiter contractor, Rockwell, to report to Mr. Colonna, it is what we call Level III, and we have been through quite a lot of that in previous discussions. Mr. Colonna reports to me for the orbiter at the Level II.

My concern about this ice had been going on since 2:00 o'clock the previous day, and I wanted personally to find out what the situation was and to get a detailed feel for it. And so I called what I would refer to as a partial Mission Management Team meeting, told Mr. Colonna that I wanted to review the ice team report and the engineering assessments with respect to it.

And we went out of the firing room area into a conference room that is one floor up in the launch

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control center, but very close at hand, and scheduled that meeting for 9:00 o'clock. I believed that that meeting was necessary, not because of the icing conditions that had been assessed that were on the launch vehicle itself, but specifically to deal with the ice that had formed on the fixed service structure and on the mobile launch platform itself.

We convened in that meeting. It was quite a large group of people, and we have not to this date made an effort to identify everyone who was there. There might have been 20 or 30 people. We have identified who the key management people who were there, and I have that listing, and perhaps we will discuss some of them, and I can give you who all of them were.

We started the meeting by having a discussion, a report by the ice team, and Mr. Stevenson, who you have heard from previously this morning, made that report. And he was reporting about specific concerns in three different areas.

One was the launch vehicle, primarily the external tank. Second was the mobile launch platform deck, the deck that the orbiter, that the space shuttle is standing on. And third is the fixed service structure that has the—that goes up one side, the side of the space shuttle, and had much of the ice I was

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concerned about on it.

His report I will summarize as follows. His report said the icing conditions on the flight vehicle, primarily the external tank, were okay and they felt there were no concerns in that area. They stated that there had been sheet ice in most of the sound suppression water troughs under the solid rocket boosters and space shuttle main engines, but that ice had been broken up and removed.

They said that the residual ice that was existing in the water troughs and on the MLP deck was okay in their view. That is, there was not very much of it and it was not in the vicinity of the launch vehicle, and they did not believe that either was a threat to the orbiter TPS.

On the fixed service structure, however, the fixed service structure had varying amounts of ice between the 95 foot level and the 215 foot level, and you've seen some pictures of that here this morning. However, there was no ice existing above the 255 foot level.

I say, it had ice, varying ice between 95 feet and 215 feet. Above 255 feet, there was no ice at all. The fixed service structure, north and west sides had very large amounts of ice and icicles. The east

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side, toward the orbiter, had significantly less ice.

At that point, we went into a detailed characterization—

MR. HOTZ: Excuse me, Mr. Aldrich. Could I interrupt for just a moment. Did Mr. Stevenson at that time report to you anything about the extremely low temperatures they were recording on the solid rocket boosters?

MR. ALDRICH: No, sir, he did not.

MR. HOTZ: Thank you.

MR. ALDRICH: The detailed characteristics of the ice on the fixed service structure were then discussed in quite an amount of detail for all of the team that was assembled. And this was not a telecon at this point; this was the engineering people who were there in Florida, and specifically the director of engineering for Kennedy Space Center was there, Mr. Horace Lamberth; Mr. Richard Colonna, who is the director of the orbiter project, was there.

And there were some other people, and perhaps you would like me to tell you who they were. For the ice team, Mr. Stephenson was there, and in fact his boss is Mr. Lamberth, who was there. From JSC, I was there; Mr. Richard Colonna, the orbiter project manager was there.

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From Marshall, Cecil Houston, who is the resident manager for the Marshall projects, was there; and also Mr. Davis was there. And from Rockwell, Mr. Cioffoletti and Mr. Glaysher and Mr. Martin was there.

During the course of the discussion, Mr. Jim Kingsbury, who is head of engineering at the Marshall Space Flight Center, came in, but he was not there for the entire discussion. And Mr. Richard Smith, who is the director of the Kennedy Space Center, also came in late in the discussion, but did not hear all of it.

And as I will tell you in a minute, I also placed a telephone call personally to Mr. Thomas Moser, who's the director of engineering at the Johnson Space Center, and discussed some of this situation with him, and I will tell you about that.

Following the discussion of the acceptability of the ice threat to the orbiter, based upon the conditions described in detail of the fixed service structure—and some of that you've seen here portrayed well this morning—I asked the NASA managers involved for their position on what they felt about the threat of that to the orbiter.

Mr. Lamberth reported that KSC engineering had calculated the trajectories, as you've heard, of the falling ice from the fixed service structure east side,

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with current ten knot winds at 300 degrees, and predicted that none of this ice would contact the orbiter during its ignition or launch sequence; and that their calculations even showed that if the winds would increase to 15 knots, we still would not have contact with the orbiter.

Mr. Colonna, orbiter project manager, reported that similar calculations had been performed in Houston by the mission evaluation team there. They concurred in this assessment. And further, Mr. Colonna stated that, even if these calculations were significantly in error, that it was their belief that falling ice from the fixed service structure, if it were in fact to make it way to the orbiter, it would only be the most lightweight ice that was in that falling stream, and it would impact the orbiter at a very oblique angle.

Impacts of this type would have very low probability of causing any serious damage to the orbiter, and at most would result in post-flight turn-around repairs.

At this point I placed a phone call to Mr. Moser that I had previously mentioned, director of engineering at the Johnson Space Center, who was in the mission evaluation room, and he confirmed the detailed agreement with Mr. Lamberth and Mr. Colonna's position.

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From these discussions—well, let me say, in addition to the discussion—I don't have it in my notes, but I remember it in more detail. We had some discussion of the falling ice, if it would hit the orbiter after it was on the launch pad; was there in fact an issue from that ice coming back up and hitting the orbiter.

And both Mr. Lamberth and Mr. Colonna reported that their assessment was that the time it took for the ice to fall, to hit the orbiter and to rebound, and the location of the fixed service structure on the MLP would not cause that ice in their view to be a concern to rebound and come up and impact the rear end of the orbiter.

Following these discussions, I asked for a position regarding proceeding with the launch. Mr. Colonna, Mr. Lamberth, and Mr. Moser all recommended that we proceed.

At that time, I also polled Mr. Robert Glaysher, the vice president, orbiter project manager, Rockwell International STS division, and Mr. Marty Cioffoletti, shuttle integration project manager, Rockwell International STS division. Mr. Glaysher stated—and he had been listening to this entire discussion and had not been directly involved with it,

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but had been party to this the whole time.

His statement to me as best I can reconstruct it to report to you at this time was that, while he did not disagree with the analysis that JSC and KSC had reported, that they would not give an unqualified go for launch as ice on the launch complex was a condition which had not previously been experienced, and thus this posed a small additional, but unquantifiable, risk. Mr. Glaysher did not ask or insist that we not launch, however.

No other comments or recommendations were offered by the large group assembled with respect to the concern—with respect to concern for proceeding with the launch.

At the conclusion of the above review, I felt reasonably confident that the launch should proceed. However, I was concerned that during the ensuing time period between when the ice team had been out at the launch pad, starting at 6:30, and the time that the launch would occur, now roughly 11:00 or 11:30 in the morning, the conditions at the launch pad might have changed.

Thus, I asked for an additional ice inspection team visit to the pad to be performed as close to launch as possible, to include a full assessment of any

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significant changes in icing conditions; for the removal of any additional ice from the MLP deck that might have fallen; and for the removal of any new ice from the water troughs.

At this point, I returned to the operations support room in the launch complex, where I reviewed the context of this meeting with Mr. Jesse Moore, the associate administrator for space flight, who was seated at that time with Mr. Philip Culbertson, the NASA general manager. In that summary, I clearly indicated the qualified position taken by Rockwell International, recommended that the launch proceed unless the ice team identified a significant change in launch pad condition on their final visit to the pad.

The report of the ice team following the final launch pad inspection indicated no significant changes, although several pieces of ice were swept from the MLP deck and some additional ice was removed from the water troughs.

Now, you probably want to ask questions. I have some more I would like to say, too, and I would like to say that with respect to TPS damage, if I could, and perhaps I could say that now, or you might want to interrupt me.

CHAIRMAN ROGERS: You're anticipating

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questions?

MR. ALDRICH: Well, that is part of my rationale.

CHAIRMAN ROGERS: I want to do it whichever way you want. I guess that you have sort of answered the question that I had in mind. That is, you acknowledge, then, that Rockwell did take a position as described by them this morning, which essentially said that they couldn't assure that it was safe to fly, or another version was it was an unsafe to fly position? Was that your understanding of their position?

MR. ALDRICH: Let me describe that, and that leads into what I wanted to say in addition about the TPS damage. In any discussion of damage to the thermal protection system on the orbiter, there is the implication of safety to the vehicle in flight during the re-entry phase

There is also strong implication of damage to the vehicle that is of a repair category, and that damage would be required to be repaired after the orbiter returned. On every flight of the STS system, we have had damage to the orbiter TPS system from various situations during flight, and upon return those repairs have had to be made.

In most instances, they have not been

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extensive and they have not interrupted the turn-around. On a few conditions, we had quite significant damage to the TPS and there have been more extensive repairs.

Let me say in fact about TPS damage, early in the program when we first began to fly this system we experienced quite a bit of damage to the bottom of the orbiter from ice from the external tank, and that led to some of these ice team activities that we have talked to you about today and previously.

During the period of the first four to six flights in the program, there was an extensive effort working with the Marshall Space Flight Center to make modifications to the thermal protection system on the external tank, to remove the conditions that created ice by changing the installation, removing certain lines, and finding other ways of mechanization, to give a more complete protection so that ice could not form from the atmosphere during the countdown.

That in fact was a $40 million program and it stretched over a significant period of time. During that period of time, however, we continued to fly the orbiter with those conditions after careful assessment, and continued to experience some damage to the orbiter from tank ice damage to the TPS.

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Decisions I think were carefully made at that time about the risks and the choices to proceed were carefully made.

Also, during the early flights we experienced a lot of damage from the launch platform itself from debris and loose items that had been left there, and procedures had to be instituted at Kennedy Space Center for more complete cleanliness of the mobile launch platform area, to be sure there was no debris there of a kind that could come up during the ignition sequence and damage the orbiter.

Again, while those concerns were going on and assessments were made that that had been completed, we still continued to fly the orbiter. And we have had TPS damage that couldn't probably be directly coupled to incomplete cleaning of the launch pad during that time, but might have been precluded by further action later in the program.

In fact, one of those areas, the four holddown posts for each of the solid rocket boosters are covered or were initially in the program covered with a heavy layer of hard rubber to protect those posts from the exhaust of the solid rocket boosters, so that they would not be damaged and would be readily reuseable for the subsequent launches.

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And we found that during the early flights much of that rubber—it is a white hard RTV—was being thrown up from the flame pits and impacting the orbiter and causing quite extensive damage. We undertook a program of reduction of those conditions, but initially it was a concern that we could not remove all the RTV, and we went through a series of taking off more and more of the RTV and seeing how much damage was still—or how much debris was still created.

During the time that we were flying with RTV still on those holddown posts, we did not delay or defer shuttle launches. We carefully assessed the damage and made determinations that what we were seeing was acceptable to proceed, although we had post-flight TPS damage repair.

GENERAL KUTYNA: Arnie, can I interrupt just a second? You mentioned this rubber that covers the holddown posts, and on launch this stuff bounces up and could possibly hit the orbiter. Why do you rule out the possibility that the springs that we lost on the holddown posts, which are in the same place, didn't bounce up and hit something?

MR. ALDRICH: Well, I will get to that. That is a good question and it follows directly what I was going to say next.

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What I was going to say next was, eventually we did remove all of the rubber, because we continued to see this material coming up on our pictures and continued to have orbiter TPS damage, some of which you know exactly what it comes from and some you aren't sure.

And so we instituted the trap doors that have been talked about earlier, the doors on the top of the holddown posts, so that when the solid rocket clears the door will flap down and the booster rocket plume will impinge on the top of the door instead of on the holddown post.

And the change for the springs and the plungers that we talked about earlier were a recent addition to those trap doors, and they have been lost during this 51-L launch and that has been a concern of our investigation or our analysis of things that could have contributed to the problem, to the STS 51-L launch.

GENERAL KUTYNA: So you haven't ruled them out, is that what you're saying?

MR. ALDRICH: I have personally not ruled them out. Now, I'm not directly involved in the task force currently and not day to day current with what all of our analysis shows, but that is one of the items that is

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in work at KSC in their engineering department.

GENERAL KUTYNA: Thank you.

CHAIRMAN ROGERS: As you go through this history, which is interesting but a little bit beside the point for the moment—

MR. ALDRICH: I'm sorry.

CHAIRMAN ROGERS: That's all right. I don't want to prevent you from giving it to us. But has there ever been a time when a prime contractor or any other contractor voiced objections to the launch when you went ahead and overruled them?

MR. ALDRICH: Let me—

CHAIRMAN ROGERS: Maybe "overruled" isn't a fair word, but where you went ahead notwithstanding the objection of the prime contractor in this case.

MR. ALDRICH: My interpretation of the input that was made to me in the Mission Management Team meeting that I described is that a concern was voiced, and it was not an objection to launch. And I think the people that were in that meeting from Rockwell intended to offer me that concern, but they did not intend to ask me not to launch.

CHAIRMAN ROGERS: Well, we will get to that. That is, when you finish.

It seems to me that that is, at least to me,

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the important point. If the decisionmaking process is such that the prime contractor thinks he objected and says, testified under oath that they took a position that it was unsafe to launch, and you say, that was not our understanding, that shows us serious deficiencies in the process.

MR. ALDRICH: Well, let me explain to you, while I'm talking about these previous TPS damages, what I'm going to lead up to was a discussion of another condition we had on the external tank this past year, here several of the external tanks had material coming off them, their insulation material, and damaging the orbiter during the launch phase.

And that was of great concern to the program, and corrective actions were taken. But we continued to fly with tanks with that condition, with the repair fix. And during those discussions and discussions on the previous conditions I talked about that have led to damage to the orbiter TPS, the Rockwell team has been involved in all of those discussions. They have been extremely and rightly conservative about the risk to the orbiter because of these kinds of debris.

And in my past experience in working these problems directly with Rockwell, they have taken positions very similar to the ones that —the one that

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they reported this morning with respect to whether we should proceed with a known potential for debris on past flights.

None of those have been that close to launch as the one that we're talking about here on STS 51-L, but I feel both the nature of the threat and the risk—that is, is it safety or is it turnaround damage—and the kind of input that Rockwell made in that meeting, that is, we have a concern, we can't be completely sure that 's going to be satisfactory, but it is your decision, is consistent with the way that they reported to me in the past

CHAIRMAN ROGERS: That's fine. Why don't you go ahead, because I think it would be helpful if you could give some examples of cases where Rockwell has been too conservative and you've gone ahead anyway. I think that is very relevant, so why don't you proceed.

MR. ALDRICH: Well, you said too conservative. I think it is appropriate for Rockwell to be conservative.

CHAIRMAN ROGERS: Well, you expressed the view that on previous occasions they had been what appeared to you to be conservative and expressed their views, and then you went ahead anyway. I say those experiences would be very relevant to our discussion here today, I

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think.

MR. ALDRICH: Well, I have been describing some of the experiences, and it would be impossible for me to recall the specific conversations from one, two, and three years ago. The most recent is the one in fact where, on a series of four or five external tanks, the thermal insulation around the inner tank—it is the structure between the oxygen tank and the hydrogen tank—in fact had large divots of insulation coming off and impacting the orbiter.

We found significant amount of damage to one orbiter after a flight and weren't sure where it came from. And on the subsequent flight we had a camera in the equivalent of the wheel well which took a picture of the tank after separation, and we determined that this was in fact the cause of the damage.

At that time, we wanted to be able to proceed with the launch program if it was acceptable and utilize the tanks that were in Florida, that were built with the same configuration. And so we undertook discussions of what would be acceptable in terms of potential field repairs.

And during those discussions, Rockwell was very conservative because, rightly, damage to the orbiter TPS is damage to the orbiter system, and it has

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a very stringent environment to experience during the re-entry phase.

And I can't say more specifically direct correlation between their reports to me on the morning of the 51-L launch and what was said in the activities leading up to those earlier launches, and for those earlier launches Rockwell definitely gave a go for launch at the flight readiness, at the L minus one review, following more lengthy deliberations than perhaps we were able to have during the morning of the 51-L launch.

But it was a very familiar kind of a report and discussion to me.

DR. WALKER: How large were these pieces of material that came off of the external tank?

MR. ALDRICH: The external tank, some of them were quite big. And it is fairly dense material. And maybe I shouldn't say that. We ought to have a specific report to tell you the density. Some of it is not dense.

There are quite large pieces.

DR. WALKER: Well, a foot square or how big?

MR. ALDRICH: Half a foot square or a foot by half a foot, and some of them much smaller and localized to a specific area, but fairly high up on the tank. So

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they had a good shot at the orbiter underbelly, and that is where we had the damage.

CHAIRMAN ROGERS: I just want to express a personal unhappiness with this development, because we have been dealing with NASA now for some time, two or three weeks, and we asked everyone to be forthright, to tell us anything that they knew that might relate to this in any way, whether it was damaging or not.

And I—maybe through some fault of the Commission, we hadn't heard about Rockwell's position until a few days ago. And I'm a little surprised that that wasn't volunteered, because certainly they are the prime contractor and their testimony this morning, whether you accept it exactly as stated or not, still is very significant testimony.

I'm really surprised and disappointed that we didn't know about it earlier.

MR. ALDRICH: Mr. Chairman, I've discussed this subject of the ice team with the Commission on three occasions.

CHAIRMAN ROGERS: But not about Rockwell's position.

MR. ALDRICH: On the second occasion in the Executive Office Building, on the first closed hearing, I, in fact, reviewed in much briefer summary this meeting,

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and I believe you asked if someone from Rockwell International had expressed a concern with the launch, and I reported the situation precisely so. That is, Rockwell had a concern with that issue.

And I believe General Kutyna—

CHAIRMAN ROGERS: I reread it and I didn't get the impression that they had taken as strong a position as they did. And maybe I will reread it again.

MR. ALDRICH: I intended to convey it exactly in the manner I have done here. Of course, we did not put that amount of time on the subject in that meeting.

CHAIRMAN ROGERS: Going back to their testimony, do you accept the testimony they gave as factually correct, or is your position that they waffled more than they suggested this morning?

MR. ALDRICH: I don't recall such an extensive discussion of safety as was reported this morning. However, I will fully admit what I said earlier, and that is any discussion of damage to the TPS has to be considered in the context of safety.

CHAIRMAN ROGERS: Try to state what you thought their position was.

MR. ALDRICH: I tried to do that and I tried carefully to reconstruct that, and I will say it again. And Mr. Glaysher made the statement when I polled him at

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the end of the meeting, and I thought the first statement that I believe he made represented a key input to me in this regard.

I've just described for you the assessment that KSC and JSC engineering reported on their feelings about the situation at the launch pad, and Mr. Glaysher reported that he did not disagree with the KSC and the JSC analysis, which they drew the conclusion to recommend go.

CHAIRMAN ROGERS: Did you think that meant that he agreed with the go position?

MR. ALDRICH: No, sir. I thought that meant that they did not have any additional factual material or hard analysis that could contribute to a better understanding of the situation, and I think that is what they reported this morning.

DR. RIDE: Did you have an assessment from the Rockwell engineering group out at Downey?

MR. ALDRICH: I did not have an assessment from the Rockwell group at Downey. However, Mr. Moser reported that he had been in contact with the Rockwell group at Downey. And in understanding how those organizations operate, I fully believed that the JSC organization would fully reflect and understand any specific issues that were represented in the Downey

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mission support room.

DR. RIDE: So you had the impression that the Downey engineering position was in concert with JSC's engineering position?

MR. ALDRICH: I had the impression that on any specific data that could be presented or discussed or calculations made, that there was no disagreement and that this was a question of judgment and a question of decision on the launch, which I felt was my decision.

And I would like to have the opportunity some time to describe some of the other launch decisions which we are required to make each flight with respect to weather and threats to the orbiter vehicle in flight, because I consider this to be a very similar kind of an issue.

VICE CHAIRMAN ARMSTRONG: The report included some concern about unknowns of aspiration. Would you clarify your own thinking, which I understand aspiration has to do with airs or flows that are induced because of the engine exhaust?

MR. ALDRICH: I may have missed the discussion of aspiration in that meeting. To my knowledge, the discussion on drawing ice into the orbiter and SRB plume at ignition was discussed in the context of drawing ice from the MLP deck into the orbiter, and that led very

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clearly to the situation of how much ice there would be on deck and where it would be located, and the conclusion of the ice assessment teams that the ice had been removed and my reaction that we should go back to check and see if no more ice had fallen or was in a threatening location to the orbiter.

The specific term "aspiration" and that consideration I don't recall being addressed as an issue in that meeting.

VICE CHAIRMAN ARMSTRONG: If I could ask one other question. The amount of ice that we saw on the pictures, which was a result of the freeze protection system implementation, seemed an unusually large amount.

I don't have any other experience that leads me to understand why that happened. Was that freeze protection system carefully analyzed and designed, and did you expect this amount of ice, and didn't the facility in its original design have the ability to handle some of that ice?

MR. ALDRICH: Apparently the facility in the original design did not have protection for this kind of consideration, although it may have been intended, and I think the freeze protection plan that was put into place was expected to be able to handle the conditions with much

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less free water than was found as a result of going through that experience for the first time under real conditions.

VICE CHAIRMAN ARMSTRONG: Perhaps I should make my question more specific. It would seem that to keep the pipes unfrozen reasonably small amounts of water are required, even though I recognize these are large pipes. But if there were large amounts of water required to keep from freezing, you would think there would be some kind of containment and two lines, hoses, or that sort of thing. And was the freeze plan not incorporating those kinds of things or were they not implemented?

MR. ALDRICH: I was not a party to the development of the freeze protection plan, nor understanding really how well it would work. We did ask that one be put in the program. That was accomplished and, as you point out, it does not appear to be the kind of a complete program that you would like to see for these kind of conditions.

CHAIRMAN ROGERS: Mr. Aldrich, I think in fairness to everybody, particularly to you, because I may have overstated what happened in executive session, let me read it.

Dr. Ride said: "Well, I guess the question is

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whether at the end of the meeting Rockwell was saying, we don't want to launch."

And I said: "That is exactly it. If Rockwell comes up in a public session and says, we advised NASA not to launch and they went ahead anyway, then we have got a hell of a problem." And I guess I shouldn't have said that.

But on the other hand, if there is no dispute about the facts and it is conveyed to everybody and everybody, after consideration, everybody agreed to it—and then Dr. Wheelon said, "What position did Rockwell take?"

And your answer was: "Everyone at that meeting—and I just told you who they were, Kennedy facility people at the meeting—everyone in that meeting voted strongly to proceed and said they had no dissent, except for Rockwell. The comment from Rockwell, which was not written specifically to the exact word and either recorded or logged, was that they had some concern about the possibility of ice damage to the orbiter, although it was a minor concern. They felt they had no experience base launching in that exact configuration before and therefore they thought we had some additional risk of orbiter damage from ice that we had on previous meetings or from previous missions."

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CHAIRMAN ROGERS: Did they sign off on it or not?

MR. ALDRICH: We don't have a signoff at that point. It was not maybe 20 minutes, but it was close to that. It was in the last hour of launch. But they still objected. They issued what I would call a concern, a less than 100 percent concurrence in the launch.

And you indicated "less than 100 percent concurrence. They did not say, we do not want to launch, and the rest of the team overruled them. They issued a more conservative concern. They did not say don't launch."

Now, I guess that is a fair statement of—well, I know it is an accurate statement of the testimony that was given, and I think it states your position a little better than I stated it a moment ago.

Is that your recollection of the testimony?

MR. ALDRICH: Yes, sir. And of course, that was my first discussion of that meeting in that context.

CHAIRMAN ROGERS: Very well.

MR. ALDRICH: Mr. Chairman, I don't want to take more time at this time if it's not your pleasure, but I would also like to describe for you at some other

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point the other kinds of decisions I have had to make during the last six launches in terms of marginal conditions and weather threats to the orbiter in flight, and how I believe this kind of a decision couples rather closely to the kind of decisions that I have to make for launching with rain clouds in the area, launching with the potential for low ceilings or crosswinds at the RTLS landing site and overseas.

And in my mind, what I did in this regard was my responsibility, and I executed it in the same manner as I have on other flights for other conditions. They were also a threat and also marginally acceptable or unacceptable, based upon specific assessment of the conditions at the time.

CHAIRMAN ROGERS: I think you will remember that we said at some of our other meetings that we recognize the difficulty of your position and we respect it and respect you. So that our questioning is really not directed to that.

Our questioning is directed to the process. Does everybody know what everybody else is recommending? Does everybody have the facts? And obviously, your interpretation of what Rockwell said this morning is somewhat different than Rockwell's interpretation of what they said.

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MR. ALDRICH: Yes, sir.

CHAIRMAN ROGERS: That is the part that is very troublesome. I mean, nobody—I don't think anybody questions the terrible responsibility you have, you and Jess Moore have, to make the decisions. But we are very seriously concerned about whether everybody has the facts available that they should have.

MR. ALDRICH: Could I say one more point in that regard?

CHAIRMAN ROGERS: Surely.

MR. ALDRICH: One of the other reasons that I asked for an additional ice team inspection is that I had in my own mind that there was some concern expressed in this meeting and I wanted to give the team time to think further about what we were proceeding to do and doing.

And as I have said previously, I have worked with the Rockwell team closely for a number of years and I fully expected that I would receive a call from one of the key officials at Rockwell if they felt that my decision, based upon the kind of input they gave me, was a problem to them.

When the situation has been reversed on previous launches, I felt free to call Downey, California, and talk to a key official there to get him

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to tell me directly his opinion when I thought there was some question in a decision. And I would think that it was more than reasonable that, if someone were still concerned that this was a very bad judgment or a bad action to take, that they would call me. And I left every opportunity for that to happen.

CHAIRMAN ROGERS: I suppose it is unfair to ask you, but suppose that call had been made. Is it conceivable you might have changed your mind?

MR. ALDRICH: If Rockwell had told me that they were no go, I would have reported to you in the same manner that George Hardy reported in discussion. I would not have overruled a no go discussion from the Rockwell team.

CHAIRMAN ROGERS: Thank you.

VICE CHAIRMAN ARMSTRONG: It might be appropriate to get this on the record, Arnie. There has been a lot of discussion about the launch time and its appropriateness, and we heard in testimony this morning that the ice team felt that it would be advantageous to launch as early as possible. And we've heard in previous testimony that some people were supporting the idea of launching later in the day, at higher temperatures.

Could you just review for us what the general

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nature of the launch window was for 51-L and what your considerations were in selecting a time within that launch window?

MR. ALDRICH: The 51-L had a three hour launch window. It was really constrained by the amount of time that we could go through the whole process of putting the crew in the cabin and having them wait through the count and then wait through the hold. And so we established a time of 9:30 to 12:30 in the morning.

I mentioned the problem they had at Kennedy Space Center early in the morning that lost them an hour, and so our window then became 10:30 to 12:30. And during the course of the morning, informally there were some discussions about how rapidly the conditions were warming and that it might be warmer during the day at noon than we predicted, and perhaps launching at the end of the launch window would be the most opportune time because we would have more melting opportunity.

Following this meeting which I discussed on reviewing the ice, I did get the input from Mr. Davis that there was a feeling of the ice team that perhaps melting was not going to be good and you might get more material to fall and be down at the bottom of the FSS and that we should proceed to launch as quickly as possible.

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I wanted to proceed with the final ice inspection, and also because of the delays we incurred we put the IMU's back into a recalibration sequence, which takes an hour. And so the earliest time we could launch, based upon that discussion, was 11:30 a.m. And we would have had the option to go to 12:30 and had more melting if we had chosen to, but we did not.

The ice team did finish in that amount of time. And the only other thing I ought to say about that, it was discussed earlier that Jess Moore was in that discussion, and I don't believe that was brought to his attention, but if so only casually.

I discussed the exact time of launch with the launch director.

MR. HOTZ: Mr. Aldrich, are we correct in assuming that in all of this discussion of temperature and delay until late in the afternoon or whenever, there was no discussion of the considerations of temperature on the seals of the solid rocket booster?

MR. ALDRICH: No, sir. Up until the events following STS 51-L, I was not aware of temperature concern with the solid rocket booster seams or seals.

MR. HOTZ: Thank you.

VICE CHAIRMAN ARMSTRONG: If I could get back to the launch window, was there a requirement to have

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Dakar in daylight, and what set the thing to be 9:30 to 12:30? Was it TDRS considerations?

MR. ALDRICH: I think it was crew duration and crew day. We had lights at Dakar and that was go, but we could not go later in the day because of the schedule we had set the launch crew and the flight crew on. There is a limit of the amount of time that we agree that they will stay in the cockpit in the position they were in, and I think that is what ended at 12:30.

VICE CHAIRMAN ARMSTRONG: I guess what I'm getting at is, couldn't it have been 12:00 to 3:00?

MR. ALDRICH: It could have been if we had planned it much earlier in the day. The launch team, of course, plans the countdown. They are in the countdown. They are in the countdown with a series of holds, but they pick up the count and proceed to tank the launch vehicle based on a given launch time.

And also, several days before launch we put the flight crew on the sleep-wake cycle that supports them to be in the right configuration for launch that day and do a full work day in orbit. We would have been able to go a three hour launch window in the afternoon if we had set that in motion several days prior to that time.

But you couldn't change in the middle of the

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night and say, well, we will just adjust them around by some number of hours, because there are too many constraints and additional problems that that creates.

CHAIRMAN ROGERS: Mr. Aldrich, you said that, in referring to previous flights, that Rockwell had been conservative in your opinion in some of their discussions. In this case there were the worst ice conditions, I guess we have heard testified, on the launch facility. Have they ever said no go before and then you have overruled them or gone ahead anyway?

MR. ALDRICH: No, there has never been a time when Rockwell said no go and we have overruled them and gone anyway. There have been times when they have said no go and we have agreed with them.

CHAIRMAN ROGERS: Say something less than no go? Has there ever been a time when they expressed concern of the type expressed here and you have gone ahead anyway?

MR. ALDRICH: This is a unique kind of discussion with Rockwell, I think. I don't recall an equivalent situation. Some of these other activities we have with respect to launch weather with other parts of the team, the launch team in Houston, and the weather people, we frequently have discussions about the weather conditions and their threat and their potential for

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violating a rule or not a rule, and you get the same sort of a qualification on an acceptability from one area or another about whether they feel the launch is a good choice.

CHAIRMAN ROGERS: Okay. Any other questions?

(No response.)

CHAIRMAN ROGERS: Suppose we take a recess until 2:00 o'clock.

(Whereupon, at 12:50 p.m., the hearing in the above-entitled matter was recessed, to reconvene at 2:00 p.m. the same day.)

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AFTERNOON SESSION

(2:15 p.m.)

CHAIRMAN ROGERS: The Commission will come to order, please.

Mr. Lucas.
(Witness sworn.)

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TESTIMONY OF DR. WILLIAM LUCAS, DIRECTOR, MARSHALL SPACE FLIGHT CENTER, HUNTSVILLE, ALABAMA

CHAIRMAN ROGERS: Mr. Lucas, would you identify yourself, please.

DR. LUCAS: Yes, sir, Mr. Chairman and members of the Commission.

I am the director of the Marshall Space Flight Center, located in Huntsville, Alabama. I have been director there for about eleven and a half years. Prior to that time I was deputy director, and prior to that I was director of an element called program development, devoted to the feasibility studies and the development of new programs. I have had about 34 years of experience in rocketry and space flight.

The Marshall Space Flight Center as an institution reports to the associate administrator, office of space flight. But we do work various projects for each of the other four associate administrators, program associate administrators, and headquarters. We have assigned to the center some six or seven major projects and several others, 35 or 40 smaller projects.

We implement those projects by the delegation of authority to program managers, senior program managers, who are held accountable by the director of

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the center for the exercise of their programs.

One of the programs that we have, of course, is the space shuttle programs that have been under discussion here, specifically the external tank, the space shuttle main engine, and the solid rocket booster.

Is that adequate, sir?

CHAIRMAN ROGERS: Yes. Thank you.

And Mr. Reinartz and Mr. Hardy and Mr. Mulloy all work for you?

DR. LUCAS: Well, in a fashion they do. Mr. Reinartz is the only one of the three who reports directly to me. Mr. Mulloy reports to Mr. Reinartz. Mr. Hardy is the deputy director of the science and engineering directorate and as such reports to Mr. Jim Kingsbury, who is the director of that directorate. That is the directorate that contains the preponderance of our engineering talent.

It serves as our in-house engineering arm that is applied across the various programs assigned to the center.

CHAIRMAN ROGERS: Do you have a prepared statement?

DR. LUCAS: No, sir, I do not.

CHAIRMAN ROGERS: All right. Well, we wanted

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to ask—the Commission wanted to ask a few questions, particularly relating to the testimony that was given yesterday and some today.

Would you please tell the Commission when you first heard about the problem of the O-rings and the seals insofar as it involves launch 51-L? And I don't want you to go way back, but go back to when you first heard. I guess it was on January 27th, was it?

DR. LUCAS: Yes, sir. It was on the early evening of the 27th, I think about 7 p.m., when I was in my motel room along with Mr. Kingsbury. And about that time, Mr. Reinartz and Mr. Mulloy came to my room and told me that they had heard that some members of Thiokol had raised a concern about the performance of the solid rocket boosters in the low temperature that was anticipated for the next day, specifically on the seals, and that they were going out to the Kennedy Space Center to engage in a telecon with the appropriate engineers back at Marshall Space Flight Center in Huntsville and with corresponding people back at the Wasatch division of Thiokol in Utah.

And we discussed it a few moments and I said, fine, keep me informed, let me know what happens.

CHAIRMAN ROGERS: And when was the next time you heard something about that?

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DR. LUCAS: The next time was about 5:00 a.m. on the following morning, when I went to the Kennedy Space Center and went to the launch control center. I immediately saw Mr. Reinartz and Mr. Mulloy and asked them how the matter of the previous evening was dispositioned.

CHAIRMAN ROGERS: You had heard nothing at all in between?

DR. LUCAS: No, sir.

CHAIRMAN ROGERS: So from 8:00 o'clock that evening until 5:00 o'clock in the morning, you had not heard a thing?

DR. LUCAS: It was about 7:00, I believe, sir. But for that period of time, I heard nothing in the interim.

CHAIRMAN ROGERS: Were you familiar with the concerns that had been expressed in the previous year, and in fact previous years, I guess, about that problem?

DR. LUCAS: I was not informed of the temperature problem. I had been aware of a problem with the seals on the solid rocket motor—that is, the case joint seals—since the beginning of the program. I believe it was in STS 2 where we first had some evidence of either minor erosion or blow-by in the seal, and then

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subsequently we have had that repeated several times. I think it is about six times in the case of the case joints, and maybe 15 or 16 times in the case of the nozzle joint.

And that has been considered and dispositioned on each and every succeeding flight readiness review. So I am familiar with that part and have never considered the seals, however, a safety of flight issue.

There was a less than the best situation, but in no case that I am aware of have we ever had any penetration or perforation of the seals.

About April of last year, I think it was, April of 1985, when we had the erosion on the secondary nozzle seal, is when we became very concerned because that followed, it seemed to be—I think it was the only time, in fact, in the program we have had that, and so we did become concerned and began to accelerate ways of improving, increasing the margin that we have on that seal.

CHAIRMAN ROGERS: Mr. Lucas, I'm surprised you hadn't heard about the safety questions that have been raised about the seal until April of the year before.

DR. LUCAS: No, sir, if I said that I misspoke. I said I had never considered it a threat to

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the safety of flight. I was aware that the seals were Criticality 1 in terms of the program, as well as many other things. But I had never considered the seals as a threat to flight safety, because I thought adequate margin was available.

CHAIRMAN ROGERS: Were you familiar with SRB critical items lists that were signed on December 17th, 1982?

DR. LUCAS: Yes, sir.

CHAIRMAN ROGERS: And didn't that indicate to you that there was a serious problem of flight safety?

DR. LUCAS: It indicated to me that if—as I understood the CIL, it indicated to me that if we had a failure of the primary O-ring after the rotation of the joint and the secondary O-ring had not—had opened up and didn't seal, that there would be a problem.

But never in our experience that I'm aware of had that happened.

CHAIRMAN ROGERS: Well, I was just commenting on your testimony you never considered it as a matter of flight safety. It seems to me the critical items list and the waiver goes directly to that. It says "actual loss, loss of mission, vehicle and crew due to metal erosion, burnthrough, and probable case burst, resulting in fire and deflagration."

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I mean, I don't see how you could say that didn't involve flight safety.

DR. LUCAS: Well, if it happened it would involve flight safety. My conception was that it was a reasonable risk to take in Criticality 1.

CHAIRMAN ROGERS: Why did they remove the R from that Criticality 1 list?

DR. LUCAS: As I understand it, the reason they removed Criticality 1 is that they discovered, after doing the structural tests, that there was a rotation of this joint which would lift or remove the pressure on the primary and perhaps secondary seal after about 300 or 400 milliseconds, when at any rate, when we had built up to the ignition pressure or full pressure within the rocket motor itself.

As I understand that CIL as it has been explained to me and as I have read it, I believe that it is still Criticality 1R until ignition occurs, after which it becomes a Criticality 1.

CHAIRMAN ROGERS: Well, when they took off the "R" from that, what was the rationale?

DR. LUCAS: The rationale was, as I understand it, the recognition that after ignition and the buildup of pressure in the solid rocket motor that there was a rotation of the joint that could remove the redundancy,

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and therefore after that time it was Criticality 1.

CHAIRMAN ROGERS: But the redundancy was removed, as I read this, and I think it is accurate, that whoever did this was of the view that the redundancy was not going to be available on this joint.

Now, I don't think it makes all that much difference whether it's the first second or the second second or the tenth second. The fact was that the Criticality 1 was the way it was listed. It was not Criticality 1R. Isn't that so?

DR. LUCAS: I don't think so, sir.

CHAIRMAN ROGERS: Well, let's go back. What was the condition before this change? What was it called, Criticality 1R? And now, what did that mean?

DR. LUCAS: That meant that one had a redundant seal throughout the whole period of flight.

CHAIRMAN ROGERS: Now, when you take off the "R" as they did here, what does that mean to you?

DR. LUCAS: Well, if you only looked at the 1 versus the 1R, it would mean it's Criticality 1. But if you read—

CHAIRMAN ROGERS: Well, let's just stick to that. That is the change, as I understand it, that was made, and I think that it was construed to mean that there was no redundant seal after this change was made.

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One was Criticality 1R and then the change was made and the "R" was removed.

Now, doesn't that suggest that whoever made this decision was of the view that you didn't have redundancy on that seal?

DR. LUCAS: That is not the understanding of the people who were preparing this, I believe, and it is not my understanding from reading. My understanding from reading it is that one still maintained 1R until after ignition.

CHAIRMAN ROGERS: So what happens after ignition? Then there's no redundancy, is there?

DR. LUCAS: No, sir. But what the experience has been, that after the seal has extruded into that gap of about one-eighth of an inch, I believe, there has never been any problem that I'm aware of, never. The erosion that has occurred on the seal has not happened, to my knowledge, except on that portion of the O-ring that is not extruded into that joint.

CHAIRMAN ROGERS: Well, I guess, if I understand your testimony, that the decision that was made to take the redundancy off this seal really was meaningless. There was no change at all, according to your testimony.

DR. LUCAS: No, sir, that is not what I mean

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to imply at all. It is Criticality 1, as I understand it, after ignition, after the ignition transient has occurred.

CHAIRMAN ROGERS: So after ignition there's no redundancy, right?

DR. LUCAS: That's the way I understand it, after the ignition transient and you have built up to chamber pressure.

CHAIRMAN ROGERS: Well, after ignition then there is no redundancy?

DR. LUCAS: Well, if you define the ignition as being after what is built up, the pressure in the chamber—this was discussed by people who know far more about it than I do, and they indicate that during the first 200, 300 milliseconds, one is building up the pressure very gradually, and you get up to a point at which the pressure is sufficient to extrude the O-ring into the seal, and then it seals.

CHAIRMAN ROGERS: Were you here yesterday when Mr. Sutter asked questions and he said that it was clear from his standpoint that this was a very tender joint and it had been so considered by NASA for three years? Do you disagree with that?

DR. LUCAS: Well, I don't know what his meaning of "tender" is. I would agree that—

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CHAIRMAN ROGERS: Well, put it this way: It's dangerous. I mean, you started out saying you didn't consider this a flight safety problem. It seems to me from all the testimony that we have received so far there is a very serious flight safety problem. You don't agree with that?

DR. LUCAS: I think it is a problem, and that when we understood the principal nature of the problem about a year ago in April, that we undertook immediately to correct. But when I said I didn't think it was a problem, had I believed it was a serious risk I would never have agreed to committing to flight. And I, along with all the other people, the senior people in the agency, did commit to flights after STS No. 2, with the problem that it was experienced on that vehicle.

CHAIRMAN ROGERS: Beginning in April 1985, did you then begin to think it was a problem of flight safety?

DR. LUCAS: I did not think it was a problem sufficient to ground the fleet. I thought it was a problem more serious than I had thought earlier.

CHAIRMAN ROGERS: Of course you didn't, apparently, because you continued to fly. But didn't you think it was a serious problem, so that if the weather conditions were different and a lot of people

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sounded alarms, that that would have caused you to have some concern about the safety of the flight?

DR. LUCAS: Well, if many people sounded alarms, that's true. But I was not aware of the alarms that had been sounded, that we have heard testified this week. That was completely new information to me.

CHAIRMAN ROGERS: And if you had heard those alarms you would have been concerned about flight safety, I presume, wouldn't you?

DR. LUCAS: If I had heard the alarms that have been expressed in this room this week before the flight, I certainly would have been concerned, yes, sir. That is right.

CHAIRMAN ROGERS: And you heard Mr. Reinartz say he didn't think he had to notify you, or did he notify you?

DR. LUCAS: He told me, as I testified, when I went into the control room, that an issue had been resolved, that there were some people at Thiokol who had a concern about the weather, that that had been discussed very thoroughly by the Thiokol people and by the Marshall Space Flight Center people, and it had been concluded agreeably that there was no problem, that he had a recommendation by Thiokol to launch and our most knowledgeable people and engineering talent agreed with

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that.

So from my perspective, I didn't have—I didn't see that as an issue.

CHAIRMAN ROGERS: And if you had know that Thiokol engineers almost to a man opposed the flight, would that have changed your view?

DR. LUCAS: I'm certain that it would.

CHAIRMAN ROGERS: So your testimony is the same as Mr. Hardy's: Had he known, he would not have recommended the flight be launched on that day.

DR. LUCAS: I didn't make a recommendation one way or the other. But had I known that, I would have then interposed an objection, yes.

CHAIRMAN ROGERS: I gather you didn't tell Mr. Aldrich or Mr. Moore what Mr. Reinartz had told you?

DR. LUCAS: No, sir. That is not the reporting channel. Mr. Reinartz reports directly to Mr. Aldrich. In a sense, Mr. Reinartz informs me as the institutional manager of the progress that he is making in implementing his program, but that I have never on any occasion reported to Mr. Aldrich.

CHAIRMAN ROGERS: And you had subsequent conversations with Mr. Moore and Mr. Aldrich prior to the flight and you never mentioned what Mr. Reinartz had told you?

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DR. LUCAS: I did not mention what Mr. Reinartz told me, because Mr. Reinartz had indicated to me there was not an issue, that we had a unanimous position between Thiokol and the Marshall Space Flight Center, and there was no issue in his judgment, nor in mine as he explained it to me.

CHAIRMAN ROGERS: But had you known, your attitude would have been totally different?

DR. LUCAS: Had I had the advantage at that time of the testimony that I have heard here this week, I would have had a different attitude, certainly.

CHAIRMAN ROGERS: In view of the fact that you were running tests to improve the joint, didn't the fact that the weather was so bad and Reinartz had told you about the questions that had been raised by Thiokol, at least, didn't that cause you serious concern?

DR. LUCAS: I would have been concerned if Thiokol had come in and said, we don't think you should launch because we've got bad weather.

CHAIRMAN ROGERS: Well, that's what they did, of course, first. That is exactly what they did. You didn't know that?

DR. LUCAS: I knew only that Thiokol had raised a concern.

CHAIRMAN ROGERS: Did you know they came and

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recommended against the launch, is the question.

DR. LUCAS: I knew that I was told on the morning of the launch that the initial position of some members of Thiokol—and I don't know who it was—had recommended that one not launch with the temperature less than 53 degrees Fahrenheit.

CHAIRMAN ROGERS: And that didn't cause you enough concern so you passed that information on to either Mr. Moore or Mr. Aldrich?

DR. LUCAS: No, sir, because I was shown a document signed by Mr. Kilminster that indicated that that would not be significant, that the temperature would not be—that it would be that much lower, as I recall it.

CHAIRMAN ROGERS: Did you see the document telefaxed?

DR. LUCAS: Yes, sir.

CHAIRMAN ROGERS: What in the telefax convinced you not to mention it to anybody?

DR. LUCAS: The whole document says that the assessment is that if the primary seal—well, let me see. It says: "Morton Thiokol recommends proceeding on January the 28th, 1986. SRM 25 will not be significantly different that SRM 15"—which was the one that we had launched approximately one year

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earlier.

CHAIRMAN ROGERS: Did you see, though, that they said: "The temperature data was not conclusive on predicting primary O-ring blow-by."

DR. LUCAS: Yes, sir. I saw that. I had a little trouble with the term "conclusive." I think as it was explained to me, "conclusive" might not have been the word, but "consistent."

I saw, as I recall it, three data points: one at about 100 degrees, another at 50 degrees, and I think 75 degrees. And the way this was explained to me was that the erosion had not been a function of temperature in a consistent way.

CHAIRMAN ROGERS: Do you know how they happened to show this telefax to you?

DR. LUCAS: When I asked what had been the resolution of the concern.

CHAIRMAN ROGERS: So you knew then that the original recommendation was not to launch by Thiokol?

DR. LUCAS: I knew that some members of Thiokol, I don't know who, had expressed reservation against launch.

CHAIRMAN ROGERS: Didn't you ask who?

DR. LUCAS: No, sir, I did not, because I did not know the Thiokol people well enough to make a

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judgment.

I do know Mr. Kilminster. Mr. Kilminster is the person who participates or has participated in all of our flight readiness reviews. He is a man I respect and believe to represent the position of Thiokol. I had no reason to question that.

CHAIRMAN ROGERS: Nobody told you that the engineering group was as a whole, was against the launch?

DR. LUCAS: No, sir. No one told me that. As a matter of fact, I didn't know that until this week. I, in previous testimony before this Commission, I got the impression that a substantial number of the people were opposed to the launch. I heard this week that all of the engineers virtually were.

But that was information that I did not have.

CHAIRMAN ROGERS: And who gave you this telefax?

DR. LUCAS: This particular copy I have, or who showed it to me?

CHAIRMAN ROGERS: Yes.

DR. LUCAS: It was I believe Mulloy. I'm not sure whether it was Mulloy or Reinartz. Both of them were there, and I think it was Mulloy who showed it to me.

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CHAIRMAN ROGERS: Did he indicate how long it took them to talk this over and finally come up with this telefax?

DR. LUCAS: I don't know that he said specifically, and I can't say now whether I gained that subsequently or at that time. I was of the opinion that it took a substantial amount of discussion, which is not surprising.

CHAIRMAN ROGERS: And you know, I guess, based upon the testimony, had it not been for this telefax that flight would have been delayed?

DR. LUCAS: You say do I know that? No, sir, I don't know that.

CHAIRMAN ROGERS: Well, Mr. Hardy said that if he had known what the engineers had recommended unanimously, he would not have recommended the launch.

DR. LUCAS: In that context, then, I think that that is correct. As director of the center, I have to rely upon my experts, and I consider Thiokol to be the expert in solid rocket motors. And I have great respect for Mr. Hardy and the engineers who work with him, and when those two groups agree then I don't have a basis for recognizing an issue.

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CHAIRMAN ROGERS: Well, I guess as we pointed out yesterday, an argument could be made maybe because of this piece of paper that Mr. Reinartz lived up to the book, but there was no application, as far as you can tell, of common sense. I mean, this obviously was a very serious matter, and by insisting that this piece of paper was giving everybody the blessing of Thiokol, the fact was that the top people who made the decision never knew about what happened in this long telecon and didn't know that the decision to launch, recommending a launch was made really by just a couple of people, Mr. Kilminster and Mr. Mason, and maybe one other.

I mean, the fourth gentleman, Mr. Lund, said he was in a position where he couldn't prove that it was not safe, and therefore he put on his management hat and changed his mind.

So this piece of paper which really resulted in the launch was made by just a couple of people, and apparently none of you gentlemen knew about it.

DR. LUCAS: I did not know that it was made by a couple of people. As I said, I recognized Mr. Kilminster as the senior Thiokol individual for the space booster programs.

CHAIRMAN ROGERS: Well, maybe I am a little—maybe I am not being conservative enough.

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It was Mr. Kilminster, Mr. Mason, Mr. Wiggins, and I think the fourth one was Mr. Lund, but Mr. Lund said—well, I will read you what he said because I think it illustrates the whole problem.

Here's his testimony. Mr. Lund said, and this is his testimony, "we got ourselves into the thought process that we were trying to find some way to prove to them," that is, NASA, "that it wouldn't work. We were unable to do that. We couldn't prove absolutely that it wouldn't work. That is the kind of boat we got ourselves into that evening."

That was his testimony.

DR. LUCAS: Yes, sir, I heard that testimony, but that was testimony that I had this week. I didn't know that at the time. I have never had any reason to suspect that the representation of Mr. Kilminster was not a carefully considered company position taking advantage of all the expertise that Thiokol had. I have never found it necessary to poll Thiokol to say now, Mr. Kilminster, did everybody agree with you? I have assumed as a responsible manager he did have sufficient agreement to justify a position.

MR. HOTZ: Dr. Lucas, did you know that Mr. Kilminster earlier had formally recommended against launch and then had reversed his position?

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DR. LUCAS: The testimony I heard was that based upon the engineering evaluation or some engineering evaluation where the engineer says we don't propose to launch if the temperature is less than 53, that Mr. Kilminster said, well, I can't go against my engineering and something to the effect, let us now have a caucus and discuss this.

It is not unusual in our system for one or more engineers to raise a concern and then have those concerns discussed and threshed out.

CHAIRMAN ROGERS: Mr. Lucas, that's not the testimony, though. The testimony was they had a long teleconference, and Thiokol made a formal recommendation against the launch. It wasn't just casual conversation; they had a chart there. The chart said we recommend against launch. I mean, it wasn't what you said at all. It was something quite different. They made a formal representation, no launch, and then they had a long off-the-record or off-the-telephone conference caucus, and it turned out that Mason, Wiggins and Kilminster apparently supported this document, and all the engineers were against it, and Lund said, well, I'm chicken, I have to go along. I can't figure out a way to prove that it's not safe.

DR. LUCAS: Mr. Chairman, I heard that

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testimony this week. I did not have that testimony, though, at the time this happened.

CHAIRMAN ROGERS: No, but you're describing the telecon as though it were just sort of one of those ordinary things, and I don't believe that is accurate.

DR. LUCAS: That was my perspective of it at the time. I would conclude also on the basis of what I have heard this week that it was not an ordinary situation.

CHAIRMAN ROGERS: Well, in any event, at no time did you pass on the information you had, even though it was sketchy, to either Mr. Moore or Mr. Aldrich.

DR. LUCAS: No, sir, I did not. As I indicated, the channel, the project channel is from Mr. Reinartz to Mr. Aldrich, and Mr. Reinartz I did not on the basis of what he and Mr. Mulloy had told me, I didn't consider it an issue. I considered it in line with the kinds of decisions that had already been made relative to the launch.

CHAIRMAN ROGERS: You had occasion, though, to talk to both Mr. Aldrich and Mr. Moore before the launch?

DR. LUCAS: Yes, sir.

CHAIRMAN ROGERS And whether it was in the

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line of authority or not, you had ample opportunity to pass on the information that there has been serious concern about the seal, isn't that right?

DR. LUCAS: Yes, sir, I had the opportunity to talk to them.

CHAIRMAN ROGERS: Okay. I have no further questions.

DR. WALKER: Dr. Lucas, I have a question.

Perhaps you recall that on Tuesday Mr. Boisjoly read a memo into the record which he had sent to his management. I think it went to Mr. Lund through Mr. Boisjoly's supervisor, which indicated that he had a very serious concern with the safety of the seal and indicated that catastrophe could result if this problem was not addressed, and Mr. Lund set up a joint working group, and they proceeded to develop some procedures to try to improve the seals.

The question I have is do you think that the Thiokol management should have taken some additional actions such as warning Marshall, your project, of this very serious concern and suggesting that one should consider stopping launches for a period of time until the problem was solved?

DR. LUCAS: As I recall that memo, and I have not seen it, I have only heard it read I think this week

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in which one member of Thiokol had written a letter saying that we should delay launch, if that information had come, I think it should have come to the Marshall Space Flight Center.

DR. WALKER: No, that was Mr. Thompson's memo. There were two memos, actually. Mr. Thompson wrote a memo actually explicitly saying that launches should be stopped. Mr. Boisjoly's letter merely warned of catastrophe. But indeed, you are right, there were two memos.

DR. LUCAS: In either case, I think that information should have been communicated to the Marshall Space Flight Center directly and immediately, and I think it should have been mentioned in the process of flight readiness reviews, of which there have been three or four since that time.

DR. WALKER: Is there some procedure for a manufacturer to respond in a systematic way to these kinds of concerns that their engineering staff has in terms of relaying information to NASA, or do you think that is just a matter of their initiative which they should take upon themselves to carry out?

DR. LUCAS: I have always considered the contractor people that we deal with to be reliable, professional people and have counted on them to bring

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their concerns to us if they have them. That has been the NASA mode of encouraging people to speak up and to express concerns if they have them. I don't know of any reason at all why they would not, and they have certainly had that opportunity if in no other way, when we have had our flight readiness reviews, and to be dealing with the claim that somebody made that we ought to ground the vehicle and not pass that on, it is very difficult for me to understand.

We have motivated, I believe, our contractors to be conservative, particularly so in the case of Thiokol with an incentive contract that has substantial penalty for flight failure. There is no motivation that I know of for contractors to take risks of the nature that would be implied by those memos.

CHAIRMAN ROGERS: Mr. Lucas, in view of what has transpired here, do you feel now that Thiokol should have gotten in touch with you or somebody else to make it clear how much concern there was on their part about this launch?

DR. LUCAS: Yes, sir, I do indeed. I'm surprised, if there was the degree of concern expressed this week, that it didn't come to my attention. I heard Mr. Reinartz on at least two occasions during the countdown poll all the elements of his program, if they

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were ready to launch. That involved the Marshall elements and the contractor elements, and in no case did I hear any reservation expressed about readiness to launch.

And so, if the contractor as such or any member thereof should have had these reservations, I was available and it could have been expressed.

DR. WALKER: Who was the Thiokol person involved in these two polls that you are speaking of?

DR. LUCAS: I can't recite all of the persons involved. The poll at the Kennedy Space Center went to the Firing Room II, as has been mentioned earlier, and it's my understanding and belief that Mr. McDonald was at the console along with our people in that firing room.

In the HOSC, the Huntsville Operation Support Center, Mr. Boyd Brinton was the senior Thiokol person now there, and I don't know if others were there or not.

DR. WALKER: Could you ask Mr. Reinartz to document that information for the Commission?

DR. LUCAS: That the whereabouts——

DR. WALKER: Those polls and who exactly responded for Thiokol.

DR. LUCAS: I don't believe anybody responded

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for Thiokol. The response was whoever was on the console, which I believe was a government person, but the contractor person was there and he can document for you whether or not that person participated in that.

DR. WALKER: I would very much like to receive that.

MR. ACHESON: Dr. Lucas, were you present at the review conducted in Washington in I think August of 1985?

DR. LUCAS: No, sir, I was not.

MR. ACHESON: Was the result of that review regarding the joint seal reported to you by the people who were there?

DR. LUCAS: Yes, sir, it was so, and action was taken, already being taken, as a matter of fact, at the time of that report.

MR. ACHESON: Was there any suggestion made to you that temperature, low temperature might be related to the performance of the seals?

DR. LUCAS: No, sir, I don't recall that it was.

MR. ACHESON: Thank you.

CHAIRMAN ROGERS: So the weather and the fact that it was cold weather didn't cause any alarm as far as you were concerned in connection with the seals?

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DR. LUCAS: Not on the basis of my knowledge, no, sir.

DR. RIDE: Had you been present at the—I think it was the 51-E FR where there was a presentation on the seal problem that had been discovered after the 51-C launch?

DR. LUCAS: Yes, Dr. Ride, as I mentioned, the 51-E, which I believe occurred in April of 1985, was the occasion first insofar as I knew, the only occasion where there was erosion on the secondary seal of the nozzle. That was the one that got our attention and indicated that the problem might be more serious than originally thought, and therefore we did move out to improve our margin.

DR. RIDE: I don't have a copy of the presentation here, but as I recall, one of the statements in that presentation was that the low temperature may have contributed to the problem with the seals, and it appears that that concern wasn't carried through in the August presentation at Marshall.

Do you have any recollection of that temperature being mentioned at the 51-E FR?

DR. LUCAS: I can't say yes or no. I just simply don't recall.

CHAIRMAN ROGERS: The occasion, I guess, was

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51-C where the weather was the coldest.

DR. LUCAS: I think I misspoke. I said 51-E. 51-C I guess was in preparation for the 51-E FR. I guess that is correct.

CHAIRMAN ROGERS: And 51-C was the coldest weather and the worst blow-by, wasn't it?

DR. LUCAS: No, sir, it was the coldest weather, but I don't think it was the worst blow-by. I think the worst blow-by was at a temperature higher than 60 degrees, and I'm not sure.

GENERAL KUTYNA: Dr. Lucas, I think you are wrong there. The worst blow-by was on 51-C. It was erosion.

DR. LUCAS: I'm sorry.

CHAIRMAN ROGERS: The worst blow-by was on 51-C, and that was in the coldest weather, and it is surprising that there wasn't a correlation between 51-C and 51-L, the Challenger flight, in your mind.

DR. LUCAS: Well, as this indicates here, this Thiokol position was that there wouldn't be a substantial difference between those.

CHAIRMAN ROGERS: But you're relying solely on Thiokol now.

DR. LUCAS: Yes, sir. I have to rely on Thiokol. They are our experts.

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CHAIRMAN ROGERS: So you put the total responsibility on Thiokol?

DR. LUCAS: No, sir, I do not. I indicated earlier that I listened to two sources. One is the Thiokol prime contractor, and the second is our own engineering personnel, some of whom have testified, George Hardy and several others, that are knowledgeable engineers, and I rely upon them also.

CHAIRMAN ROGERS: I don't have any other questions.

Dr. Keel?

DR. KEEL: Dr. Lucas, just one further question on your awareness of temperature concerns. There was a July 2, 1985 briefing by Thiokol to Marshall personnel.

Were you not in that briefing?

DR. LUCAS: I don't recall having been at that briefing.

DR. KEEL: That specifically did talk about O-ring resiliency and temperature effects, and that was the basis, it is my understanding, on which Marshall and Thiokol then briefed headquarters in August 1985.

DR. LUCAS: I don't recall having been in that briefing, and I don't say this as a matter of excuse, but as you know, the Marshall Space Flight Center has

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many projects for which I am responsible, and I don't spend nearly all my time on Space Shuttle. But I am sure if that meeting was there, senior people from the Marshall Space Flight Center were involved.

DR. KEEL: Just two other questions.

You mentioned in your testimony that when you first learned about this concern at 7:00 p.m. that Mr. Reinartz and Mr. Mulloy said they had heard about a concern, but is that an accurate characterization, heard about the concern? That sounds like it was second hand information.

DR. LUCAS: I didn't intend to imply that. It is my understanding Mr. Mulloy had only heard of the concern a few maybe minutes or an hour before. I understood that Mr. Reinartz had had a telephone conversation with someone where that concern had been expressed.

DR. KEEL: Mr. Reinartz actually participated in the 5:45 p.m. teleconference. So he had firsthand knowledge.

Did you know that Mr. Lovingood was concerned enough about the nature of this that he thought there was a good prospect for a delay, and he had actually recommended that Mr. Aldrich be given notice that there might be a need to elevate this?

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DR. LUCAS: I did not know that, no, sir.

DR. KEEL: Did you know he had asked for you and Mr. Kingsbury both to participate in the 8:15 conference?

DR. LUCAS: I did not know that, sir.

CHAIRMAN ROGERS: Mr. Lucas, going back to the previous question asked about the responsibility of Thiokol, you said you didn't rely solely on them. Yesterday both Mr. Hardy and Mr. Mulloy said had they known about the position of the engineers at Thiokol, they would not have recommended a launch, and I understood that that was your position too now.

DR. LUCAS: Had I been involved as a project manager, I would not—I would put it this way. Had I been told that all of the engineers at Thiokol recommended against the launch, then I certainly would have interposed an objection.

CHAIRMAN ROGERS: Which I guess is another way of saying that the responsibility really lies with Thiokol.

DR. LUCAS: Well, I would think that—I don't recall that we have ever, that I have never knowingly overridden a go/no-go decision by a contractor.

CHAIRMAN ROGERS: So the responsibility rests

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with Thiokol?

DR. LUCAS: The responsibility rests with Thiokol, but I'm not trying to shake the responsibility of the Marshall Space Flight Center. Thiokol reports to us. But I do rely upon the contractor, the prime contractor, to recommend launch.

CHAIRMAN ROGERS: But as you know, the Commission is considering the decisionmaking process, and if I understand the testimony of you and Mr. Hardy, and Mr. Mulloy, it was to the effect that had Thiokol recommended against the launch, the flight would not have occurred. Am I correct?

DR. LUCAS: That is correct.

CHAIRMAN ROGERS: Thank you. We have no further questions.

DR. KEEL: Mr. Moore, Mr. Aldrich, Mr. Thomas and Mr. Smith.

(Witnesses sworn.)

CHAIRMAN ROGERS: I think maybe that Mr. Moore and Mr. Aldrich are clearly identified. Mr. Smith and Mr. Thomas, you might say a word for the record about your present employment.

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STATEMENT OF JESSE MOORE, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT, NATIONAL AERONAUTICS AND SPACE ADMINISTRATION HEADQUARTERS; ARNIE ALDRICH, MANAGER, SPACE TRANSPORTATION PROGRAMS, JOHNSON SPACE CENTER; DICK SMITH, DIRECTOR, KENNEDY SPACE CENTER; AND GENE THOMAS, DIRECTOR, LAUNCH AND LANDING OPERATIONS, KENNEDY SPACE CENTER

MR. SMITH: Yes. I am Director of the Kennedy Space Center, and I have been there since September of 1979. The year preceding that I was the Deputy Associate Administrator for Space Flight, which would correspond to Mr. Jesse Moore's deputy at that time. It was actually John Yardley. Previous to that I was the Deputy Center Director at Marshall Space Flight Center and had spent all my career since 1951 at the Center, starting out in design and going up through the ranks of increasing responsibility.

MR. THOMAS: I am Gene Thomas. I am presently the Director of Launch and Landing Operations at Kennedy Space Center and report directly to Bob Sieck, who reports to Mr. Smith, and I am also, in that capacity, I serve as a launch director for the Shuttle launches and have served in that capacity for the last five launches. And I started with NASA in 1962 as a systems

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engineer, and worked on all the manned programs to date.

My last experience before coming to the Launch Director position was as a Shuttle project engineer for all elements of the Shuttle, and I was the launch director for STS-51-L.

CHAIRMAN ROGERS: Thank you.

By way of a question, could I ask, did any of you gentlemen prior to launch know about the objections of Thiokol to the launch?

MR. SMITH: I did not.

MR. THOMAS: No, sir.

MR. ALDRICH: I did not.

MR. MOORE: I did not.

CHAIRMAN ROGERS: So the four, certainly four of the key people who made the decision about the launch were not aware of the history that we have been unfolding here before the Commission.

MR. MOORE: That is correct.

CHAIRMAN ROGERS: Turning to another subject—and I am not sure which one of you gentlemen wants to refer to it—we have had a lot of discussion about the Critical Items List, and particularly the decision that was made in December 17th, or on December 17th, 1982.

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Would one of you or a combination of you attempt to explain what this is and what it's significance was and whatever regulations apply to it in connection with the launch?

MR. MOORE: Let me try to give you a general feel, and then I will ask Mr. Aldrich and maybe Dick if he would like to comment on the Critical Items List.

As has been reported here, Mr. Chairman——

CHAIRMAN ROGERS: Do you have a copy of this, and would you like one?

MR. MOORE: I do not have a copy of that at this point in time.

As has been reported, there are a lot of critical items in the Space Shuttle Program, and I think in the Category 1's there are about almost 1,000, maybe 900, something on that order. When I look at Criticality 1 items is they have to do with flight safety, and they put kind of a special flag on the various issues associated with Criticality 1 items, be they 1 or be they 1R, and you are concerned about flight safety, and they are then issues related to those need to be appropriately dispositioned in the program as they come up from time to time. In other words, they should be raised all the way from the contractor, if they're at the contractor plan, to the responsibility of project

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management, to the responsible Level II management, and then to the responsible Level I management. And so they raise a flag in terms of having to do with flight safety, and there are issues that come up from time to time on a number of the flights, not only in the SRB area, but things that could happen that have to do with flight safety, for example, and those get supposedly dispositioned in detail at the various levels of the program until they reach my particular level.

And so that is what the Criticality 1 thing in general means to me.

And maybe, Arnie, you would like to pick it up from your perspective at Level II.

CHAIRMAN ROGERS: Being a little more specific, I gather that prior to this date this was a Criticality 1R item. Now, on this date a change was made, and so the R was removed.

Would you explain why the change was made and after it was made, what did it mean, what was the significance of the change?

MR. MOORE: I can't explain why the change was made. I read this Criticality 1R, or this Criticality 1 item, as it got unfolded post the 51-L—I have not read all the Criticality 1 particular items, but what it means to me is it means that you are losing an element

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of redundancy, and as has been reported here, that during the ignition transient of the SRB, during the early part of that phase, there is some thought process that you do not have redundancy at that point in time. But after you get that ignition transient, the pressure builds up, you do have redundancy, but that is a critical time. And to me the change from Criticality 1R to 1 means that it should be paid very, very close attention, that you have an area here that you need to watch very, very closely, and it says you do not have redundancy during all the periods of times you would like to have redundancy, and therefore it needs, again, very, very close attention.

That is how I read the change from 1R.

CHAIRMAN ROGERS: Does anybody know why it was made at this time?

MR. MOORE: That was before, I think, this, as you stated, was in December of 1982.

CHAIRMAN ROGERS: Yes, December 17, 1982.

MR. MOORE: Well, I joined the Space Shuttle Program in February of '83 and so I do not have the history nor the background on this.

CHAIRMAN ROGERS: Mr. Aldrich, can you answer that?

MR. ALDRICH: I can comment about the failure

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mode and effects analysis, and Critical Items List.

CHAIRMAN ROGERS: But if you could, though, just on this, do you know why the change was made at this time?

MR. ALDRICH: No, sir, I have not studied this Critical Items List item on the SRB in detail.

CHAIRMAN ROGERS: Once the change was made, what is different about it?

MR. ALDRICH: Well, I can explain my interpretation of 1 and 1R.

CHAIRMAN ROGERS: Good.

MR. ALDRICH: A case where you have an item that is Criticality 1R, it means that you have an item which, if all similar items would fail at the same time, it would be cause for loss of vehicle and crew. However, there is a redundancy involved in that you have two similar items, like the two O-ring seals, and that unless you had a failure mode where they both failed or both did not work at the same time, then you in fact have redundancy.

But if you had two O-rings that had the same common failure, then it is really a Criticality 1 item, and you would expect they would both fail at the same time.

CHAIRMAN ROGERS: I don't understand that.

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Why the change, and what was the result of the change?

MR. ALDRICH: I can't describe the solid rocket booster Criticality 1 and 1R items. I have not reviewed those in detail. I review very many of these items for the orbiter vehicle, and the description I just tried to make is my understanding of those criticalities as they have applied to other systems that I have worked on.

CHAIRMAN ROGERS: Let me try another tack.

After you make this change and you issue this piece of paper and it circulates and everybody has to be more careful, what in fact happens? Is there a different treatment of when you get ready to launch of these items, or is this just paperwork?

MR. ALDRICH: Let me try to explain that.

Each project in the program is required to do a detailed failure effects analysis of their hardware down to the component level to determine the criticality, if that component were to fail, what it would mean to the system that it is in, and then to the vehicle and the flight conditions that it sees, each project being, each contractor is required in the contract to do that analysis.

As a part of that analysis, some items turn

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out to be critical. That is, they create failure modes that have significant consequence to the safety of the vehicle or to the success of the mission in which it will fly, and these are extracted as the total FMEA process proceeds. And they are highlighted and formally documented in a separate document. However, both the failure mode and effects analysis document for each project, and the critical items list for each project is submitted to the NASA project involved.

For instance, the SRB would be submitted to the SRB project organization at the Marshall Space Flight Center. Those are reviewed and critiqued by the NASA engineering counterparts for the people who have made those analyses, and the intent is that there becomes agreement that these things are correctly analyzed, correctly documented, but the contractors know this hardware in much greater depth and familiarity, and so they start the process by providing full documentation. However, the critique is made by both government and contractor to reach complete agreement that what has been documented to the best of everyone's knowledge is accurate technically.

The Criticality 1 items are categorized, and you mentioned 1 and 1R. There is also 2 and 3, and they are less critical as you go up that chain. The

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Criticality 2—and all of these items, all of these criticality items in the Criticality Items List are reviewed by the contractor. In fact, the contractor is required to prepare and submit them by the contract, reviewed by the project at Level III for accuracy and correctness, and then they are submitted to my organization at the Johnson Space Center, the Level II program, again for review, understanding, critique and approval. The Criticality 1 and 1R items deal with safety of the vehicle. They take one more step. They are forwarded to the Level I organization at NASA headquarters, again for critique, review and approval.

CHAIRMAN ROGERS: So if I understand what you are saying, that meant that you and Mr. Moore would be very conscious of this problem because it was a Criticality 1 item, is that right?

MR. ALDRICH: I would be very conscious of it if it had been brought to my attention.

CHAIRMAN ROGERS: Now, what requirement was there to bring it to your attention?

MR. ALDRICH: The requirement on the project is that these items be fully documented and fully approved, and this one—

CHAIRMAN ROGERS: But you know, that doesn't really mean much to me, Mr. Aldrich. I mean, the people

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that have testified here, say they are Level III, they didn't have to do anything except get the pieces of paper and handle it at Level III, and I am asking, did this require something else, that because the criticality nature of this and because it says that the loss of mission and vehicle and crew will result from it, did that put added burden on the people in the process, decisionmaking process to notify you people at the top about it?

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MR. ALDRICH: The people at the top of the program were notified in 1983—and please let me finish. As you mentioned, there are a large number of these Criticality 1 items, and they are not each individually and uniquely reviewed for each launch. Changes to these criticalities in terms of the rationale, the understanding of how the system performs, or any failures that occurred are highlighted and it is the responsibility of the projects, the contractors, to notify their project office at NASA and up the chain of changes to performance of the equipment or to the rationale that applies to the category of these Criticality 1 items.

CHAIRMAN ROGERS: Did that mean, then, that in 51-C you were all notified about what happened on that and the fact that it was the coldest day and the most serious blow-by? Did you know that?

MR. ALDRICH: Well, this Criticality 1 item was not brought up for attention to my level, that there was a change either in the engineering assessment of how the system performed or in the flight experience that would change the rationale that is documented here that caused this to be a Criticality 1 item.

CHAIRMAN ROGERS: Let's go back to my question. Did you know about 51-C and the fact that it

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was the coldest launch and you had had the most trouble with this seal?

MR. ALDRICH: I knew that 51-C was launched under cold conditions. I knew—I do not know—I did not know at the time that 51-C had had the most blow-by or the most erosion, if that in fact is true.

CHAIRMAN ROGERS: None of you did, I gather.

MR. MOORE: No, sir, neither did I. I did not realize that, and I did not have a correlation of temperature with that. I knew that 51-C was a cold launch because I remember we scrubbed the day on the 51-C flight because of excess ice on the external tank, and we were worried about the thermal protection system again that we talked about this morning.

But I did not recall any correlation between temperature and the erosion experience that we had seen on 51-C.

GENERAL KUTYNA: Jess, if I could interrupt, you know there was a briefing brought up in August that had three bullets about concerns, one of which was resiliency, which is temperature-related. How was that dispositioned in headquarters?

MR. MOORE: That particular review, General Kutyna, came about as a result of the memo that Mr. Davids had written to me expressing some concern about

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case-to-case erosion and also nozzle-to-case erosion on the previous flights. It also, the memo, resulted from a visit that Mr. Davids of my office and Mr. Hamby of my office went down to Marshall, and the memo said he was going to set up a briefing to headquarters to go over the situation in some more detail.

That particular activity started in our own thinking after the 51-B flight, and the 51-B flight, as I recall, was on April 29 of 1985, and at that time we experienced for the first time secondary erosion in the nozzle joint. And I think we got—we got a little bit more concerned about the erosion problem at that time.

I assigned an action item to my deputy for technical matters. Then that led to the trip down to Marshall with Mr. Davids and Mr. Hamby. And following that was a meeting that Mr. Weeks in my office and Mr. Bardos had at Thiokol discussing this particular problem. Mr. Bardos then wrote a memo for the record, I guess, after that particular meeting.

And then on August 19 the briefing was set up where the Marshall folks and the Thiokol people were coming in to headquarters. It was on my calendar on the 19th—it was on a Monday—until about three or four days before the actual briefing, and at that point in time I was working temperature sensor failure

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problems that had occurred on the July flight and I was not able to attend that particular briefing.

And so Mike, Mr. Weeks, I should say, attended that briefing, along with—let's see, I've got a list of the other large number of people that attended that briefing. Mr. Weeks, Deputy Director, Deputy Associate Administrator in my office; Mr. Winterhalter, who was Shuttle Propulsion Division Acting Director at that time. Mr. Bill Hamby was the STS program integration Deputy Director. Mr. Paul Wetzel, who was the solid rocket booster programs chief; Mr. Paul Herr, who was the solid rocket motor program manager; and Mr. Harry Quong, who was the reliability, maintainability and quality assurance director of the chief engineer's office.

Those were the group of people at NASA headquarters who attended the meeting. Mr. Mulloy of Marshall Space Flight Center, who was a solid rocket booster program manager, attended; and Mr. Bob Swinghammer of Marshall also attended, who is the materials and processes laboratory director at Marshall. Thiokol had a total of six people there, including Mr. Mason, Mr. Wiggins, Mr. Kilminster, Mr. McDonald, and Mr. Speas.

That briefing was given on August 19

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and then Mr. Weeks came back a day later or two days later and reported back to me that he had heard the briefing and that he thought the data basically supported continuing to fly. He did not think it was an issue that we ought to ground the fleet. We did need to press on to get this thing fixed, as we had already started some activities back in the April time frame.

He cited the Titan experience, something that he thought was relevant to his conclusions, and he also mentioned to me that he had talked to Mr. Hardy, I guess, at Marshall about this whole thing. And so that was how that particular briefing was dispensed, disposed of at headquarters.

And at the same time we had had going on at that time this program leading to the Qual Motor-5 firing in February of this year time frame. That was to do some of the tests on some changes in the particular solid rocket motor that might be implemented in the shuttle flight program.

DR. WALKER: Mr. Moore, since the waiver from 1R to 1 was executed in late December 1982. why did intensive program to restore the redundancy of the seal not begin until April or so of 1985?

MR. MOORE: There was planning going on, to my understanding, Mr. Walker, in terms of doing some tests

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and analysis as early as 1984. In the spring of 1984, I believe, was when Marshall and Thiokol began some initial interactive discussions relative to putting a plan together for understanding and characterizing the O-ring situation.

In fact, there was an action item, as I recall, out of the STS 41-C flight readiness review that was assigned, I believe it was assigned to Marshall to go and look at the characteristics of the sealing process and procedures to ensure consistency in terms of the mating of the joints, and that actually followed 41-C, where I believe we saw some erosion at that point in time.

An action was given out of that flight readiness review, so during this period of time some tests and analysis were under way. And then we saw the 51-B flight in April of 1985, and then that was the first experience we had seen on the secondary erosion, secondary O-ring erosion on the secondary seal, and that is when we accelerated moving out on some changes, potential changes, in this thing leading up to the QM-5.

CHAIRMAN ROGERS: In view of all of that, it didn't occur to any of you to ask the question about the weather and the seal problem prior to launch?

MR. MOORE: Mr. Chairman, let me tell you my

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thoughts on the temperature situation. I had remembered in my own mind the Launch Commit Criteria, and that has been cited here before. But that says that all systems should be able to operate between a 31-degree ambient temperature and a 99-degree ambient temperature, and that was what was fixing temperatures in my particular mind as far as the overall temperature relationship.

CHAIRMAN ROGERS: But at the time this was considered a Level III matter, this seal, and the weather-related questions related to the seal, so that was all considered a Level III matter, so nobody seemed to think it was important enough to take to Level II or Level I; is that right?

MR. MOORE: Well, it did not come up to our level, obviously, during this particular count, and in the past, over the past years of this program, that I have been involved in this program, I had never personally associated any temperature variation with respect to the sealing performance of these O-rings

So it had not been brought up to my attention during this particular process. And, as I have said, I was looking at the Launch Commit Criteria, which went from the 31-degree ambient temperature up to the 99-degree ambient temperature, and that is what I was boxing in in terms of an operating plan.

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And it was my assumption that the hardware was qualified to operate under those kind of conditions.

CHAIRMAN ROGERS: And the same thing was true, Mr. Aldrich, in your case, when you had all of the discussions about the weather and the ice and all of that—the fact that you in the back of your mind had heard about the seal problem and the possibility that the seal was harder when it was colder?

MR. ALDRICH: I had not heard any of that information that you are commenting on, Mr. Chairman.

CHAIRMAN ROGERS: You hadn't heard any of that?

MR. ALDRICH: No, sir.

CHAIRMAN ROGERS: And I gather the others—

MR. SMITH: I had not heard that information.

CHAIRMAN ROGERS: Mr. Armstrong.

VICE CHAIRMAN ARMSTRONG: If I could ask several groups whether or not they thought some additions to the Launch Commit Criteria might be required, because clearly we were in this 51-L case apparently inside the Launch Commit Criteria, and yet we had groups saying that they didn't want to fly.

So would you explain to us how changes in the Launch Commit Criteria are incorporated and why they should or should not have applied to this situation?

MR. ALDRICH: Well, let me tell you my

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understanding of the Launch Commit Criteria. It is a formal document in the program. It is a document we spend a lot of time on to try to make it right so that the launch team can in fact make the proper decisions without having late close-in deliberations on things that might be complicated to understand, and each project in the program—the same as I talked about the FMEA and the CIL—each project in the program is responsible for specifying conditions which they can support or constraints that they must levy on the launch system.

The fact that there is not a constraint in the Launch Commit Criteria, in my view, is not that it was overlooked. In my view, the Launch Commit Criteria correctly reflects the program's understanding of the certification of the solid rocket booster and other elements, which say that they have been certified to operate between 31 degrees Fahrenheit and 99 degrees Fahrenheit ambient temperature at the time of launch.

And that is a complicated thing to say because it implies prior conditions and length of time in different temperatures. But in fact our program spec does levy that on the projects, and the projects are obligated to come forward and identity conditions where they do not meet the conditions that they are requested

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to be designed for.

My understanding in general is that the SRB project has certified to those conditions, and I think we can provide to you a documentation trail that we believe indicates that. And, in any event, in the launch environment I certainly rely on what we have put extra effort and time into in the Launch Commit Criteria to tell us what constraints we have to honor and what things are acceptable.

And by reading the Launch Commit Criteria for weather and finding no constraints on the solid rocket booster I don't imply that something has been left out by accident. I imply that the projects agree with those constraints as stated, and I would have said that the night before launch, and I would say it again today.

VICE CHAIRMAN ARMSTRONG: OK. But how do we correlate that with the Thiokol recommendation that they not launch outside their experience?

MR. ALDRICH: I cannot understand some of the reports in that regard that we have heard based on the kind of certification program that I believe is in place in the program that has gone on for almost 20 years and was very thoroughly done over the certification phase. That track needs to be understood by all of us.

VICE CHAIRMAN ARMSTRONG: Do I understand you

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to imply, then, that someone should have initiated amendment to Launch Criteria so that they would be willing to approach a certain limit that was not currently in the LCC?

MR. ALDRICH: I believe—well, yes. My belief is that the SRB project by all of the prior participation in the project was committed intentionally to the 31- to 99-degree temperature range, and if in fact there was some concern with that, large or small, they would be required to submit additional Launch Commit Criteria to us that says for the given parameter or given system on the SRB that system in fact has a temperature range that is somewhat different, and the launch team must honor that constraint, and we would honor that constraint.

We might elect to make some change to try to correct that situation, but we would honor it.

VICE CHAIRMAN ARMSTRONG: Did anyone submit such amendments?

MR. ALDRICH: No, sir, not to my knowledge, and certainly we don't have it in the record today.

MR. THOMAS: I might point out that for each mission we have an amendment to the LCC that picks up changes, some of them as late as the day before, and history has shown that Marshall is the most conservative, that they cover everything. Their history

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shows that they cover everything with an LCC when it is required.

CHAIRMAN ROGERS: I assumed that. That is what Mr. Mulloy scolded Thiokol for trying to do, was to change the Launch Commit Criteria on the eve of launch; isn't it?

MR. MOORE: Well, I can't speak for the rationale. I just heard the comments yesterday and so forth.

CHAIRMAN ROGERS: But I gather that is what Thiokol was saying. We think now that we should not launch, or at least the engineers did. We don't think we should launch in this temperature. And Mr. Mulloy, according to the testimony, said well, you can't do that. You can't change the Launch Commit Criteria on the eve of launch. This is Friday night; you can't do that.

MR. MOORE: I'm not sure what he meant by that. I think you would have to ask him his intention there. But, as Gene said, there are Launch Commit Criteria that are changed.

CHAIRMAN ROGERS: Well, we heard him yesterday. I think he is saying the same thing Mr. Aldrich is. But, as it turned out, Thiokol, however late in the game, then apparently said we don't think

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the criteria is right. We think it would be dangerous to launch at this temperature. That was their first recommendation.

MR. MOORE: You certainly can raise objections to Launch Commit Criteria. I mean, there is nothing in the program that says if someone has a problem with the Launch Commit Criteria. Until it is T-0 you can raise an objection to it and so forth.

DR. COVERT: Mr. Aldrich, you have just said that you believe the Launch Commit Criteria is 31 degrees to 99 degrees.

MR. ALDRICH: Yes sir.

DR. COVERT: I [illegible] yesterday, I believe, that there was discussion of the certification of the solid rocket motor [illegible] 40 to 90 degrees. There seems to be something here that I don't understand completely. Could [illegible] help me out with that, please?

MR. ALDRICH: Yes, sir. I have here the Launch Commit Criteria document that was in effect—only the pages for weather. It is quite a thick document and there are many other parameters besides weather. In fact, some of the parameters have unique red lines and criteria for each individual measurement.

The general specification for the entire launch vehicle is 31 to 99 degrees Fahrenheit, and that

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is clearly specified. There is a special criteria on the solid rocket booster bulk propellant temperature and I believe that is the other parameter that you were referring to. I haven't looked at that here immediately, but let me see if I can find it on the document.

DR. COVERT: I believe you are correct.

MR. MOORE: I think it is mean bulk temperature of the propellant in the solid.

DR. COVERT: Thank you.

MR. ACHESON: A question for Mr. Moore or Mr. Aldrich. When a contractor has raised a red flag on a Criticality 1 item, as in this case the contractor did initially with regard to the seal, what reporting channel or procedure would you expect to follow within the NASA organization, and would you expect it to be different from that followed in a Criticality 1R item?

MR. ALDRICH: I would think if a recognized concern existed in a Criticality 1 or 1R system that represented a change in thought, rationale or concern from what might have been reviewed thoroughly previously that it would be raised to all levels of the program.

VICE CHAIRMAN ARMSTRONG: Is there a rule to that effect?

MR. ALDRICH: We have been looking for some of

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our language in this regard in our NASA management instructions and our program directives at Level II which guide the program, and I believe there is language of that type. However, it is somewhat general and interpretive and it certainly requires the projects to make that interpretation.

But the implication is clear: that a critical problem should be reported up the chain.

MR. ACHESON: But, as I understand you, there would then be some ambiguity as to what was a new concern.

MR. ALDRICH: Well, it has to be interpreted, clearly.

MR. MOORE: Let me just add, if I can, to that. All of our major contractors participate in our flight readiness reviews and in our Launch-1 Day Reviews. They are all polled individually. A senior member of that contractor team is polled individually to see if they are ready to launch.

At our mission management team meetings that Mr. Aldrich chairs—and I attend as many of them as I can—the contractors are not represented at the mission management team meetings, and we look to the project elements then to raise any particular issues. However, I would like to point out that there is nothing

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in the program that says if a contractor has any problems that he could not pick up the phone and call me or call Mr. Aldrich or get a message to a particular concern.

And at that point in time our response will be we will take that concern and have it discussed, and those issues.

MR. ACHESON: I understand that. But, of course, what I'm wondering is if at Level III, when the Level III manager thinks that he has disposed of it, if it is a Criticality 1 item, should he really see that it is reported all the way up, as opposed to a 1R?

MR. ALDRICH: That is a hard question to answer because it is interpretive and the language that requires critical problems to be reported doesn't refer specifically to the Critical Items List or to Criticality 1 as a terminology. And so I think the intent of reporting problems up in our documentation is proper. It requires interpretation and you have heard some of the interpretations that were made in this case by other people involved.

CHAIRMAN ROGERS: There is judgment, too. The trouble with so much paperwork is you eliminate the good judgment and common sense. I can't imagine why some of these people who knew about the seriousness of the

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problem didn't pick up the phone themselves and call you, not just the contractor.

MR. MOORE: Let me say that in the past, Mr. Chairman and members of the Commission, there has been no shortage of issues discussed, I guess both at the flight readiness reviews, the Launch-1 Day reviews, and even after that in terms of launches. And if you look at the past mission before this, where we scrubbed something like five or six times, I guess, there were all discussions of this type that took place on whether or not our weather ceiling conditions were being violated or whether we had a technical problem that we could work around and so forth.

So the issues, there are opportunities to bring up the issues and so forth, and we attempt to get them properly dispositioned.

VICE CHAIRMAN ARMSTRONG: Could you give us a description of the functional differences between the Levels I, II and III?

MR. MOORE: Well. I will start, Mr. Armstrong, and try to tell you. I think I tried to present a picture maybe it was the first day of the public hearings of this Commission to try to show how the boxes tier down. You know, my office and the Office of Space Flight has a number of additional responsibilities in

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addition to the space shuttle program. We have responsibility for upper stages and expendable launch vehicles and Spacelab. So there is a broad range.

My office has been broken up into elements that look after specific parts of the shuttle from what I would call a policy objective or resource and a top-level program management point of view, trying to set the objectives for the program, try to make sure the program is managed properly, trying to make sure the resources, the proper resources, are applied to the program.

That in turn tiers down into essentially three NASA centers, with the Kennedy Space Center having the responsibility for launch and landing operations, with the Marshall Space Flight Center, as Dr. Lucas reported, having responsibility for the space shuttle main engines.

VICE CHAIRMAN ARMSTRONG: Excuse me for interrupting, but I am not asking for the organizational differences. I am asking for the functional differences between the levels.

MR. MOORE: Well, the functional difference between the levels starts out at my level, which is called Level I, and all the issues that are not properly resolved to everybody's level of satisfaction in the program are brought up to me. Arnie will bring those up

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and I will look to Arnie, Mr. Aldrich here, to make sure that the other project elements and so forth all feel responsible or have all resolved all their issues in their particular program.

And then he in turn will look down to the various program elements, project elements, I should say, at the various NASA centers to make sure they are all ready from a functional standpoint. Each project element then will have their budgeting responsibility, their own management responsibility. And when we get into a launch situation we pyramid ourselves up in terms of the launch decision process.

VICE CHAIRMAN ARMSTRONG: I would interpret that to mean if they could solve it at a lower level it doesn't come up, and I am wondering if that is really the truth.

MR. MOORE: Well, I think that again it is a judgmental kind of a thing, Mr. Armstrong, and issues that people are sensitized to that, for example, reflect flight safety, reflect other major concerns in the program, they should be properly brought up to the appropriate levels.

DR. RIDE: Would you have expected this problem to have been brought up to Level II?

MR. MOORE: Well, I will answer from my

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perspective. I think, looking back on everything and the amount of discussion that went on, even though the people decided that it was judgmental and they thought it had been put to bed, I would have thought it would have been brought to Level II, if you want my honest opinion.

DR. RIDE: And I guess maybe there are two parts to that question. We had two different aspects to it, I guess. One is would you expect any new concern about a Criticality 1 item to be brought up to Level II?

MR. MOORE: Well, I think a new concern on Criticality 1 items I think should be brought up to Level II. And I think at this point in time in the program I believe Level II is our repository for CIL items in the program and handled the Critical Items List. So yes, any changes and deviations and so forth to that I certainly expected to be brought up to Level II.

DR. RIDE: And I guess the other aspect is would you expect to hear that a contractor had originally given a no-go and then subsequently given a go? Would you expect to be informed of that?

MR. MOORE: Well, again that is hard to say. I would hope that I would have been informed of those kind of things, but I am not sure that the people who

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figured they had made a judgment on this thing, Dr. Ride, had satisfactorily resolved this whole thing. I have nothing in the program that says it is required.

CHAIRMAN ROGERS: Why? Wouldn't that be the easiest thing in the world to put into the program? If the contractor recommends no launch, please advise us.

MR. MOORE: That would be very easy to do.

Let me just add one more comment to that. I have never felt that anyone in the program has been reluctant to speak up, and therefore I was operating under the ground rules and principles that people who had issues in the program or in the process of reviews could in fact speak up and raise those issues.

And I think we have all tried to be very open about it, and I think many, many reviews we participate in we have very lengthy discussions on various kinds of issues involving not only the headquarters people but the field center people and the contractor people.

DR. WALKER: Mr. Moore, I would like to return again to the August briefing. At the time of that briefing, Morton Thiokol had in fact received, or management had received the two letters that I spoke of earlier from their expert engineers, one suggesting termination of flights until the seal problem was remedied, and another discussing the possibility of

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severe catastrophe.

I know you were not at that briefing, but could you tell me how strong a concern about safety Morton Thiokol expressed at that briefing?

MR. MOORE. I can only relay to you what Mr. Weeks relayed after that particular assessment. He sat through the entire briefing, and I think he came back and said that we ought to press on with our QM-5 testing and get the test under way, but that there was not enough concern to ground the fleet. For example, we wanted to build margin of safety and eliminate all of the erosion. We were not happy with the erosion.

And, as has been expressed earlier, erosion had been seen as early as Flight 2 on the particular shuttle, and in the case-to-case joints—that was the worst case, I guess—erosion that had been experienced. And so I was not of an opinion, based upon input fed back to me, nor did any of my other people feel, that the fleet should be grounded because of the experiences to date, but we should get on with a program to eliminate the concern that we were expressing with the O-rings.

DR. WALKER: Would it be possible to get copies of the briefing charts that Morton Thiokol presented at that briefing?

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MR. MOORE: Yes, I think we have already provided the Commission a full set of documentation on the August 19 briefing. I think we provided that to the Commission two or three days after the Commission was formed. So you should have a copy of that.

DR. WALKER: All of the briefing charts of Morton Thiokol would be in what you have given us?

MR. MOORE: Well, I'm not familiar with any additional briefing charts that Morton Thiokol may have presented. This was a package briefing, as I later looked at it, that was documented and put into a bound volume, and I think we provided the entire document to this Commission. That is all the data that I am aware of was presented at that August 19 briefing, Mr. Walker.

DR. RIDE: Mr. Weeks was the person who signed the Critical Items List for this particular item and was also the one who attended the August briefing as your representative on the joint and the seal problem?

MR. MOORE: Yes.

DR. RIDE: When he presented the results of that briefing to you and he discussed with you the problems associated with seal erosion and then the problems associated with the joint, did he put it in the context of we have got a problem with a Criticality 1

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item and we are sustaining damage or some erosion to something that is considered a Criticality 1 item?

MR. MOORE: I don't recall it being put in that context, but I had participated, as I have done in the past, Dr. Ride, in a flight readiness review that was held on July 2 where we were dispensing of an item of secondary erosion that we had seen for the first time. And the chart that was presented and discussed in that flight readiness review had flight safety, and so I knew that it was an area that was concerning flight safety.

But I don't think Mr. Weeks expressed it in those exact terms.

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MR. HOTZ: Mr. Chairman, could I have a question for Mr. Thomas, please?

Mr. Thomas, you are familiar with the testimony that this Commission has taken in the last several days on the relationship of temperature to the seals in the solid rocket booster?

MR. THOMAS: Yes, sir, I have been here all week.

MR. HOTZ: Is this the type of information that you feel that you should have as Launch Director to make a launch decision?

MR. THOMAS: If you refer to the fact that the temperature according to the Launch Commit Criteria should have been 53 degrees, as has been testified, rather than 31, yes, I expect that to be in the LCC. That is a controlling document that we use in most cases to make a decision for launch.

There are some other judgments that we make based upon the clock, the hold times that we have remaining, the window for the day, other things like that that are not normally in the LCC, we get that information from the program elements late in the timeframe before launch, but most of the no-go criteria are the go/no-go criteria we expect to see in the LCC.

MR. HOTZ: But you are not really very happy

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about not having had this information before the launch?

MR. THOMAS: No, sir. I can assure you that if we had had that information, we wouldn't have launched if it had been 53 degrees.

MR. ACHESON: A question for Mr. Moore.

Mr. Moore, there have been some implications in the press, and I am sure you have seen them, that there might have been an unusual degree of eagerness to get on with the launch schedule in the case of 51-L that might or might not have changed the balance of caution.

Would you comment on that, please?

MR. MOORE: Yes, sir, I would be happy to comment on that.

As a matter of fact, I would like to review, with the permission of the Chairman, some of my thoughts on the evening or so leading up to the launch, a couple of evenings, which will get into the question of pressure.

As we have talked about before, we held our normal launch minus one day review on the 25th of January, I think about 11:00 o'clock in the morning. We were all go. Everybody polled said they were ready to support the launch. We did have a concern about weather, and that evening we met to discuss weather for

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the next morning, which was on the 26th. It was a launch opportunity, and that happened to be a Sunday morning. As you heard from the Air Force person talking about the weather, we did not get a very positive forecast, and I think our mission management team was unanimous in saying we probably should not try a launch because of the weather problems that were likely to occur on Sunday morning. Also we were told that afternoon that we had an awful lot of dignitaries in to watch the launch. We had people here from the People's Republic of China and several Congressmen, and a large number of other outsiders, as well as we understood that afternoon that the Vice President was possibly going to stop over on Sunday morning to view the launch, and so forth, and that was on Saturday afternoon.

Nevertheless, we decided to scrub the launch as a result of the weather forecast at 9:30 p.m. that night, and as a matter of fact, to my knowledge, no one had any political pressure whatsoever to try to get the launch off, and that was the case through the entire sequence on this flight, and that has been the case on every flight that I have been associated with.

We have got roundly criticized in the press as a result of the flight just prior to this about all of the multitude of delays starting in the December 20

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attempt, I guess. We shut down the week of the holidays to give our team a rest and so forth, and then we had four or five additional scrubs before we finally got it launched, and we also waved off three times at the Kennedy Space Center trying to get it landed at Kennedy.

So we have not been under any political pressure. This program operates on launch-by-launch basis. We try to make sure all launches are safe, all issues are put to bed, and worry about downstream schedules later. You always have to lay out downstream schedules.

I have got schedules going into the early '90s, but you take them one at a time, and that is the philosophy by which the Shuttle team operates, and you worry about how you adjust your downstream schedules after you have safely launched and safely landed the particular mission you are concerned about.

VICE CHAIRMAN ARMSTRONG: I am going to ask this question to Mr. Smith, and I asked it earlier to Mr. Aldrich, and you are really better qualified to answer it as it has to do with the facility design and whether it was designed to handle freezing temperatures, and if not, why it wasn't.

MR. SMITH: Neil, early in the program it was

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recognized that we were not equipped, coming out of Apollo, to handle freezing conditions in the water systems on the pad.

VICE CHAIRMAN ARMSTRONG: But why would that be? I mean, certainly any civil engineer knows that he has to handle the normal environments where he is building his building.

MR. SMITH: Well, the answer to that would be yes, except the history in the past ten or fifteen years, freezing pipes in Florida in that area has been a very rare thing. The decision was made to not implement insulation and so forth on the water systems that would preclude rupture of lines, that the risk, the cost was not justified.

We had the experience then in January '85 flight, and we could not support the launch because of damage to the facility. And since that time we had implemented the plan by which we felt we could minimize the damage, could keep the firing systems and all and the safety systems up in the functional state and support, fully recognizing that doing that we would have to bleed the systems, so we would have ice on the structure.

So I think we went into that evening fully knowing that condition.

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VICE CHAIRMAN ARMSTRONG: Let me follow that with the other question I asked Mr. Aldrich, or a variation of that.

At this point in time do you think that the freeze protection scheme that was implemented is an adequate one?

MR. SMITH: No, I do not, and we were not satisfied with that.

CHAIRMAN ROGERS: Dr. Keel?

DR. KEEL: Mr. Moore, did you know that consistent with the testimony this morning, that Rockwell had apparently said that "Rockwell could not assure it was safe to fly?"

MR. MOORE: Yes, sir, I had some indication of that from a report that Mr. Aldrich had given me. As was mentioned this morning, Mr. Aldrich had an ice team meeting, and with the various project elements around to discuss the impact of ice on the possible damage to the thermal protection system.

Following that ice meeting, which lasted about an hour or so, and I did not attend the particular meeting, Mr. Aldrich came back and said—reported to me that he had given that very careful consideration. He had talked about both the Kennedy people and the Johnson people had taken a look at it and a lot of

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analysis on it, but Rockwell did have a concern. He did not indicate that that was a safety of flight concern, and it was more indicated in the thermal protection system which might have some damage that it might have to be repaired and cause some delays to turnaround, and he said that he had tagged up with everybody and he felt that it was all go, and he recommended that we launch, and I accepted his recommendation.

DR. KEEL: Let me go back to the beginning, though.

I said "Did you know that Rockwell said Rockwell cannot assure it was safe to fly." And you said, "Yes, sir."

MR. MOORE: Well, I knew that Rockwell had made a comment. I did not have Rockwell—knowledge that Rockwell had talked about being safe to fly. I knew Rockwell expressed some concerns as relayed to me by Mr. Aldrich, and he put those in the context of TPS issues, things that would have to maybe possibly affect turnaround time. But I did not have a feeling that they had expressed anything concerning flight safety.

DR. KEEL: Just one final question.

Based upon all of that, did you think Rockwell was saying go or no go?

MR. MOORE: I didn't really think, from a

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Rockwell point of view, I knew how thorough Mr. Aldrich had worked this problem, and I knew he had a lot of people in that particular meeting, and that I looked to Mr. Aldrich for a go on that particular flight. But the thought did not cross my mind that, for example, Rockwell was saying no go. No.

DR. KEEL: Thank you.

MR. SMITH: Might I add something to that, please?

I came in on the latter part of that meeting, Mr. Aldrich conducted on that morning, and I heard the statements made as Mr. Glaysher stated this morning, and as I understood that, we could not give 100 percent assurance that there was no safety concern. It was my opinion at the end of the discussion that the consensus of the group, including the Rockwell people, that—and I put this in my words, now, and it wasn't stated this way—that the probability of damage from the ice on the structure was no greater than we would normally expect from ice on the external tank during a typical day, and I left that meeting fully convinced that everybody had signed up to the launch. I did not hear any recommendation that said Rockwell does not support launch.

DR. KEEL: I guess the question, though, is

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did you hear any recommendation that said Rockwell does support the launch?

MR. SMITH: No, they did not make that recommendation. They did not non-concur, and I have heard people non-concur. I have heard Rockwell non-concur on things.

CHAIRMAN ROGERS: I guess that illustrates one of the things that obviously has to be addressed. I mean, there are a lot of maybes. A lot of people have been voting maybe or I don't vote. It would seem to me that the decisionmaking process would require people to take stands, and you should have a record of it either in a recording or a piece of paper or something to make it clear. I mean, the Rockwell people clearly think that they indicated that they have been concerned to the point that they did not recommend launch, and apparently that was the first time that they had done it. Apparently they had not taken that position before.

And so they I think believed that they were saying we do not recommend launch. But you, on the other hand, were saying that it was okay to launch. And it seems to me that if you are going to have a decisionmaking process with key people involved, that it ought to be clear where they stand. Otherwise a lot of second guessing.

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In any event, let me say that I think the Commission is going to—we are going to have one more witness because he flew up here at our request, Mr. Powers, and we want to complete his testimony today.

But insofar as you gentlemen are concerned, let me say that I think we have finished with this aspect of the investigation in terms of the decisionmaking process. At least we have finished it as far as public testimony is concerned. We will probably want to get some additional information to complete the record. We can do that by deposition or by letters or in other fashions. I don't think it is necessary to continue this discussion on the decisionmaking process.

You will remember that I did say at one point that we thought the decisionmaking process may be flawed I believe I am speaking for the whole Commission when I say that we think it is flawed. And I think probably you gentlemen would agree with us on that, that the process as it worked in this case was clearly flawed because the recommendations that were made were either not fully understood by you or not conveyed to you.

Be that as it may, we appreciate very much the cooperation we have received from all of you, and it has been 100 percent, and you have provided everything that we have asked for. You have been available to testify,

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and we appreciate that. And we want to also express our appreciation for all of the things you have done over the years, and we hope you understand that in order to conduct this kind of an investigation, we have to ask a lot of tough questions, and it is not intended in any way to be a reflection on any of you. We all appreciate what you have done for this country, and we will be in touch with you. We assume that you are working on improvement of the decisionmaking process, and we look forward to further space flights and manned space flights and womanned space flights, and we will l ok forward to working with you.

And we again appreciate very much your cooperation. And thank you.

MR. ALDRICH: Mr. Chairman, could I make one brief correction to my testimony this morning? I understand this is the simplest way to do it.

I indicated that Mr. Kingsbury from the Marshall Space Flight Center was in the ice meeting that I conducted, and in fact, that was an error. It was Mr. Mindeman from KSC who was there part time during that meeting.

CHAIRMAN ROGERS: Mr. Stevenson made one correction, too, about Jesse Moore was not in a meeting that he said he was.

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Do you want to correct the record on that?

MR. MOORE: Yes, I would like to correct the record that Mr. Stevenson indicated this morning that I was in the Ice Team meeting proper. I was not in the Ice Team meeting, and Mr. Arnie Aldrich came back and reported to me the results of that particular Ice Team meeting.

CHAIRMAN ROGERS: We will try to make testimony available for you so that you can make any corrections you want to the testimony.

Mr. Aldrich?

MR. ALDRICH: I have one other correction on Mr. Stevenson's testimony. He indicated that I was in both the review of the Ice Team results about 3:00 in the morning and at the meeting at 9:00 a.m., and I was not in the earlier meeting, but I understood the people who were.

CHAIRMAN ROGERS: Thank you very much, gentlemen.

DR. KEEL: Mr. Powers?

(Witness sworn.)

CHAIRMAN ROGERS: Mr. Powers, would you give your name and your present employment, please?

1944

TESTIMONY OF BEN POWERS, ENGINEER, STRUCTURES AND PROPULSION LABORATORY, MARSHALL SPACE FLIGHT CENTER

MR. POWERS: My name is Ben Powers. I work at Marshall Space Flight Center. I have been working at Marshall in the Propulsion Laboratory for approximately 20 years. I have been recently working in the Solid Rocket Motor Branch for some 14 years. My responsibility is in the design, development and qualification of solid rocket motors.

CHAIRMAN ROGERS: Were you here yesterday when testimony was taken from some of your colleagues from Marshall?

MR. POWERS: Yes, sir.

CHAIRMAN ROGERS: And you took part in the telecon on January 27, 1986 that was referred to yesterday?

MR. POWERS: Yes, sir.

CHAIRMAN ROGERS: And do you want to recount for the Commission your recollection of that telecon, and particularly your participation in it?

MR. POWERS: I would be pleased to, sir.

There was quite a lengthy telecon, much longer than a normal type thing, and we were called in late at night. The charts were being prepared real time, and we [Ref. 2/14-3] were discussing data even before we were getting the charts in, and the engineers at Morton-Thiokol were presenting their case, and I specifically remember Brian Russell going over his charts and Arnie Thompson going over his and also Roger Boisjoly going over his charts.

CHAIRMAN ROGERS: And they recommended against the launch to begin with?

MR. POWERS: Yes, sir.

CHAIRMAN ROGERS: And do you remember what was said by Mr. Mulloy and Mr. Hardy?

MR. POWERS: I remember some of the things that Mr. Hardy said, mostly because I was sitting with him. Mr. Mulloy's remarks, really, he was not there, and I can't remember his remarks as well, but I do recall Mr. Hardy's remarks pretty well.

CHAIRMAN ROGERS: Do you want to describe them, please?

MR. POWERS: He was probing the data, which is typical, trying to ascertain if there was a valid concern with the temperature as it would affect the behavior of the seals at the joints, the field joints, not the nozzle, just the field joints.

CHAIRMAN ROGERS: And there was testimony yesterday that he at one point said he was appalled, and Thiokol people thought he was appalled at

the decision. Mr. Hardy said he was appalled at the data that was presented.

Were you appalled by the data or the decision?

MR. POWERS: Sir, I fully supported the Thiokol engineering position and was in agreement with it.

CHAIRMAN ROGERS: And you made that known to Mr. Hardy?

MR. POWERS: No, sir. I report to Mr. John McCarty, and we were caucusing, and I also reported it to Mr. Jim Smith, which is our chief engineer, and this would be a typical thing that we would do. I would report to my boss and to my associate project management in Engineering. I don't want to confuse this.

CHAIRMAN ROGERS: Did you report, too, that you agreed with the Thiokol engineers?

MR. POWERS: Yes, sir.

CHAIRMAN ROGERS: To whom did you report?

MR. POWERS: I'm sorry.

CHAIRMAN ROGERS: Who did you report that to?

MR. POWERS: Mr. John McCarty. He is my—well, he is not my immediate supervisor. He is my deputy lab director, but he was the senior man in line

at that time, and I reported to him that I thought that the temperature would reduce the margin of safety for the joint performance.

CHAIRMAN ROGERS: And were there others in that telecon that agreed with you, that you know of?

MR. POWERS: I can't identify anyone joining me in that position, sir, I cannot make that statement.

CHAIRMAN ROGERS: And have you talked to them since, any of the people that were in that telecon, to find out how they stood on the issue?

MR. POWERS: Yes, sir, I have.

CHAIRMAN ROGERS: And what did you find out?

MR. POWERS: Some of the engineering people have mentioned that they, too, were concerned, primarily with the temperature effect on the O-ring resilience, the spring-back ability of the O-ring.

CHAIRMAN ROGERS: Was there anybody who agreed with Mr. Hardy or Mr. Mulloy, as far as you remember, on that telecon?

MR. POWERS: There was no dissent with Mr. Hardy, to my knowledge, other than the discussion that I had. I was the only dissenting engineer.

CHAIRMAN ROGERS: But the others remained quiet, I assume?

MR. POWERS: Yes, sir.

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DR. COVERT: Mr. Powers, yesterday we were talking with Mr. Hardy about the effect of longtime squeeze on the resilience of the O-rings, and their performance if they had been compressed for a substantial period of time, and he said that he had seen no evidence that this was an effect, and in fact, some motors had been stacked for a year and still worked properly.

What is your experience with O-rings that have been compressed for a substantial period of time and then are caused to react by a change in the geometry in which they find themselves?

MR. POWERS: Sir, I find that to be not a problem. I think that they are talking about set, and I am talking about resilience, and they are two different things, and we fully accounted for the fact that we did not think that compression set would be the problem. We were zeroing in on two factors which were resilience and durometer.

DR. COVERT: Do you think that there is an effect upon set? This had been stacked for 28 days, remember, and it had been hot, cold, hot, cold.

MR. POWERS: We account fully for that, sir, and I have no concern with compression set for that configuration.

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DR. COVERT: How do you account for it?

MR. POWERS: We find that that particular selection of O-ring material is one of the very best materials that one could select for a compression set performance, and I do want to make sure I differentiate the difference between set and resilience. Resilience was a concern. I don't want to come across wrong. I would like to be understood, and I was concerned about resilience.

DR. COVERT: I understand what you are saying. Set is the permanent deformation or reluctance to change its shape.

MR. POWERS: Yes, sir.

DR. COVERT: And resilience is the sort of a short term spring back, I guess.

MR. POWERS: Yes, sir, which would be affected by temperature.

DR. COVERT: Yes.

MR. POWERS: Yes, sir, and that was the concern.

DR. COVERT: How do you account for set?

MR. POWERS: We have data that we have collected, and on this specific compound of O-ring material for hours versus the effect, and we have that characterized, and that data is available, and we do

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take that into account, and we degrade the performance of the O-ring accordingly for the characteristic data that we do have for set. And we are very confident that we understand compression set, sir.

DR. COVERT: So that the performance of the O-ring is apt to be poorer after 21 days in the stack than it would be after three days in the stack?

MR. POWERS: Yes, sir. If it were one hour versus 1,000 hours versus 10,000 hours, as time increases, set will degrade the performance, yes, sir, that is correct.

DR. COVERT: Thank you very much.

MR. POWERS: Yes, sir, thank you.

DR. WALKER: Mr. Powers, I realize that you agreed with the conclusions of the Thiokol engineers, but do you think they made a convincing case? Mr. Hardy and Mr. Mulloy have said that the data they presented in their view was not convincing, that the data was inconclusive.

Do you think that the Thiokol engineers made a good case for their view that temperature was a serious problem?

MR. POWERS: Now, that is hard for me to quantify, sir.

DR. WALKER: I understand it is an opinion.

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MR. POWERS: I would say that the data that they presented was clear to me because I am very familiar with that data. I would think that vice versa, other people that were not as familiar with that data would be somewhat less appreciative of the effect.

Did that answer your question?

DR. WALKER: I think that is a very good answer.

What you are saying is that someone without your background in the subject might be confused or unpersuaded by the data?

MR. POWERS: Yes, sir. I think the data would be more difficult to follow unless someone were working with that data, and it is a technical issue, and being familiar with it, then I was easily persuaded that the temperature was something that would degrade the margin.

CHAIRMAN ROGERS: Were the other people working with you that you say may have the same point of view?

MR. POWERS: I don't understand.

Which people, sir?

CHAIRMAN ROGERS: Well, I asked you whether there were others at Marshall who agreed with you, who agreed with Thiokol, and you said yes, you thought there

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were some.

MR. POWERS: Do you mean at that telecon, or later on?

CHAIRMAN ROGERS: Later on.

MR. POWERS: Yes, sir, I understand the question, later on, okay.

CHAIRMAN ROGERS: And are there a number of such people? And I don't want you to name them. Just tell us are there a number of such people?

MR. POWERS: There are some that share my opinion, sir, yes, there are. I would not say that I am—I represent the majority report.

CHAIRMAN ROGERS: I understand. But you indicated that you think you have had more experience in this field than some others, and the others might not understand.

Are there others like yourself who have quite a lot of experience who would agree with you now?

MR. POWERS: Yes, sir.

MR. ACHESON: Mr. Powers, were you familiar at that time, January 27, with the work of the Thiokol task force on the O-ring problem back from the summer of '85?

MR. POWERS: Yes. I was not on that task force, I was not assigned to it, but I am familiar with

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it, sir.

MR. ACHESON: Can you tell us how far, and particularly how far up in your organization, that educational product was spread or was passed along?

MR. POWERS: It was certainly passed all the way through our engineering.

MR. ACHESON: Was Mr. Hardy familiar with that work?

MR. POWERS: I would certainly think so, sir. I can't answer that specifically, but we were working vigorously, and I don't see how it could have escaped him.

DR. WALKER: Mr. Powers, the emphasis on temperature which characterized the meeting on the 27th seemed to have been considerably more than the emphasis when the task force was started and during the task force activities.

Do you understand why the Thiokol engineers did not emphasize the role of temperature more before this particular occasion arose?

MR. POWERS: No, sir, I can't explain that to you, sir.

DR. WALKER: Do you think, however, they did have the understanding or at least the information?

MR. POWERS: Without a doubt.

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DR. WALKER: And that information was apparently not emphasized in the briefings they gave to Marshall until this particular occasion of 52-L?

And I am trying to understand how well the information about the temperature concern as opposed to the general concern for the seal was emphasized.

MR. POWERS: I would say the temperature concern was not placed in the front row seat, if I might use that word. The data was there, it was presented. I would say the emphasis was not, and certainly in hindsight, and it is not fair for me to talk at this time. I would say that it should probably be emphasized more, but here again, I want to emphasize that that is speaking today.

VICE CHAIRMAN ARMSTRONG: I would just like you to characterize for us the nature of your concerns in terms of what things might cause what consequences with which you would be displeased in the sealing process.

MR. POWERS: First of all, I would have to say that the extremely cold temperatures that we just heard the testimony about are certainly outside of the norm, and we don't expect to see that very often at KSC, and it is not something that you are working with often, and I was concerned that those temperatures, as I understood

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them, were outside the data base.

So that would be my first concern, that one would prefer to launch within his data base, and as I understood the presentations from the Marshall—or excuse me, from the Thiokol personnel, that the resilience would degrade the O-ring springback force, and that it would also increase the durometer, and of course, there is the stiffening of the grease, the O-ring lubricant that we pack into the joint. And as you know, it is very critical for us to achieve due to the rotation and dynamics of the hardware, to achieve a rapid seal. And we would like to see that achieved at 100 psi, certainly no more than 200 psi. We would like to see a hand seal achieved at 200 psi. And those are talking 100- and 200-millisecond timeframes. Those are fast.

And the degradation of temperature on the performance of the timing was a concern to me, and I don't know whether I've met you head on there or not.

VICE CHAIRMAN ARMSTRONG: That's exactly what I wanted to know. Thank you.

GENERAL KUTYNA: Mr. Powers, just a couple of questions to put this in context.

How long have you been concerned about temperature?

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MR. POWERS: I was alerted to this problem some 6 months ago, and there was work that was ongoing and was coming up to speed.

GENERAL KUTYNA: And the two briefings that we have talked about in the last two days, there was a briefing in July which had a great section on resiliency, which means temperature, and then the briefing in August that went clear up to NASA headquarters had as its first bullet on the concerns chart again resiliency.

MR. POWERS: I might have missed it by a couple of months, sir, but yes, sir, those are the times, that is true.

GENERAL KUTYNA: But how can you say that NASA was not aware at the higher levels of the concern with temperature if those briefings went forward?

MR. POWERS: Well, I'm not saying that NASA wasn't aware of the concern. I don't think this was a thing that was highlighted in terms of center stage. We were more concerned, I would think, about at the time of the very heavy erosion that we were seeing on the nozzle joint, and that was a very serious concern of ours at that time.

DR. WALKER: One more question, Mr. Powers.

Were you aware of any concern about the

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temperature properties of the putty and its loss of resilience at low temperature?

MR. POWERS: Sir, I consider the putty totally plastic.

DR. WALKER: Even at the low temperatures where this launch occurred?

MR. POWERS: Sir, I have no data at very low temperatures or had not the data at that time at a very low temperature that would have concerned me other than I would think that it would slow down the process of pressurization. I would certainly have to agree that the putty would be slowed down with the low temperature.

DR. WALKER: Are you saying that this launch was outside of the temperature range for which there was information on the putty?

MR. POWERS: I don't know of any data that we were working with that was below the like 40 degree range.

CHAIRMAN ROGERS: Well, thank you very much, Mr. Powers. We appreciate your coming on such short notice. I hope we didn't ruin your day.

MR. POWERS: You are more than welcome, sir.

CHAIRMAN ROGERS: And we will recess now, and we have not decided when we will have our next public

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session, but probably it will not be next week. It certainly will not be Monday. Tuesday, and Wednesday, and maybe not next week at all.

So we will make an announcement later on.

Thank you very much.

(Whereupon, at 4:15 o'clock p.m., the Commission recessed subject to the call of the Chair.)

PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—FRIDAY, MARCH 7, 1986

Kennedy Space Center
Cape Canaveral, Florida
The Commission met, pursuant to recess, at 9:40 a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ARTHUR WALKER
DAVID C. ACHESON
DR. RICHARD FEYNMAN
MAJOR GENERAL DONALD KUTYNA
DR. EUGENE COVERT
JOSEPH SUTTER
ROBERT RUMMEL
ALSO PRESENT:
AL KEEL, Commission Executive Director

1960

PROCEEDINGS

STATEMENT OF COLONEL EDWARD O'CONNOR, USAF, DIRECTOR OF OPERATIONS, 6655TH ASTG, ESMC

CHAIRMAN ROGERS: Will you please give your name and present assignment and a little bit about your background and what you're presently doing here at NASA.

COLONEL O'CONNOR: Edward O'Connor. I'm a Colonel in the United States Air Force. I'm assigned to the Eastern Space and Missile Center. Currently I'm acting as director of search and recovery operations for NASA on the 51-L accident.

I've been involved in this activity since the day of the accident. Prior to that, I was director of operations in the 6555th Aerospace Task Group, where we put together the payloads that fly on the shuttle. And I've worked in different shuttle missions.

CHAIRMAN ROGERS: All right, you may proceed, please.

COLONEL O'CONNOR: First chart, please.

(Viewgraph.) [Ref. 3/7-1]

Immediately after the accident, we went about establishing the search blocks so we could go out and recover components. We were able to use our radar

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optical data that's available from the range instrumentation cameras and the range radars. We were also able to use visual sightings obtained by some of the aircraft in the area to identify those areas where the majority of the shuttle components impacted the water.

Since that time we've also used a trajectory calculation from our radars and optical sensors to better improve our location of debris on the ocean bottom. Since that time, we've also had to modify our search areas based on the located components that we have recovered, as well as additional sonar contacts that we have obtained through the Navy assets.

The second chart, please.

(Viewgraph.) [Ref. 3/7-2]

The first box in our search area I would like to point out to you is a ten nautical mile by 25 nautical mile search box, the lower side of the chart. That particular box was initially established in the days immediately following the accident. Since that time, we have added that top section, 8.5 nautical miles on the eastern edge, three nautical miles on the western edge.

That was added because it was felt that the majority of the right-hand SRB components should be

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located in that area based upon trajectory calculations and optical data.

Next chart, please.

(Viewgraph.) [Ref. 3/7-3]

In order to indicate the difficulty of the recovery activity we are engaged in, I would like to go over very quickly the oceanographic conditions that we were experiencing in the area. We are quite close to the Gulf Stream axis in the search box and we are experiencing—

CHAIRMAN ROGERS: Could you move your microphone up just a little bit closer?

COLONEL O'CONNOR: At this time we have a search box that is very close to the axis of the Gulf Stream. Therefore, we are experiencing currents of the level of four to five nautical miles. We are experiencing a little seasonal variation as we progress toward the summer in the position or the speed of the Gulf Stream.

So this is a constant factor we're going to have to deal with during the whole recovery. The depth in the vicinity of the SRB's we feel is between 220 feet and 1200 feet. This has been established by some of the videos we have taken on the ocean bottom and some of the recovery action we have taken to date.

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The remainder of the search area, the area where we expect to find the majority of the orbiter, as well as the orbiter payloads, is in a search depth of approximately 90 to 220 feet.

Next chart, please.

(Viewgraph.) [Ref. 3/7-4]

The at-sea operations are being directed by the United States Navy Superintendent of Salvage. The most experienced salvage team that has been put together in a long time. The assets we have employed at this time in the search and recovery activity are eleven ships, one manned submersible, two remotely operated vehicles, seven sonars, and 41 divers

This is a fairly constant number of ships and assets that we are involving every day, seven days a week in the recovery.

Next chart, please.

(Viewgraph.) [Ref. 3/7-5]

In order to maximize the utility of the use of these assets, we have broken them into three basic tasks, the first being the SRB recovery as being the highest priority recovery. We are using right now the submarine NR-1, which is a small nuclear-powered submersible that the Navy uses for underwater submarine search and rescue. We are using this as a sonar

1964

platform to map the SRB area.

We have also rented the STENA Workhorse, which is a dynamically stabilized oil support ship that has been used in the Gulf during high winds and during high currents. It is very suitable working close to the Gulf Stream for these types of recoveries.

We are also using the Seward Johnson, which is a support ship for the submersible Johnson Sea Link II. This is a small four-man submarine that lets us get outstanding video and photography of pieces on the bottom, and in a few moments I will show you a piece of video from that particular submersible.

The shallow water recovery, we're using basically diving teams. We're diving off the United States Navy vessel Preserver and the United States naval vessel Sunbird.

We're also using the Independence, which is one of the Thiokol SRB recovery vessels, which has a sonar platform.

We are also engaged in a wide area sonar search. We're going to search with sonar, side-scan sonar, the entire search areas that were indicated in that initial chart. That is approximately 350 nautical square miles, a significant piece of ocean to be covered, and it is going to be a laborious process to

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recover that.

Next chart, please.

(Viewgraph.) [Ref. 3/7-6]

Of the 350 square nautical miles that we have to cover, as of today we have covered 190 nautical miles. In this 190 nautical miles that we have searched, we have had 227 sonar contacts that have to be verified.

The ones that we have verified to date give us 17 shuttle components, definite pieces of the 51-L mission. We have 25 non-shuttle components. These are things such as geology on the bottom, rocks, oil drums that have been discarded by ships passing through the area previous to the accident.

We have 185 sonar contacts that have not been characterized as shuttle or non-shuttle at this time. We are working as quickly as possible to characterize those, to complete our search and start the recovery of those items.

CHAIRMAN ROGERS: Colonel, when you say 227 contacts made, what does that mean?

COLONEL O'CONNOR: That means that as our sonar ships were going through the search area they had hard contacts, acoustic returns off of objects on the bottom. The objects, we don't know what it is. We know

1966

it is large enough in all cases, probably something the size of a 55 gallon drum or larger, that we need to go out and look at and characterize visually.

CHAIRMAN ROGERS: But you have identified 17 shuttle components that you feel confident are part of the shuttle system?

COLONEL O'CONNOR: Yes. We have those components. We have videotapes and still photography of those components. We take that verification photography and video from some of our remotely operated vehicles.

CHAIRMAN ROGERS: Have you any way of knowing which of those 25—or which of those 17 might involve the SRB?

COLONEL O'CONNOR: Yes. A few charts down, I will have a chart that identifies the components on the bottom that we will attempt to recover shortly.

CHAIRMAN ROGERS: Fine.

COLONEL O'CONNOR: Next chart, please.

(Viewgraph.) [Ref. 3/7-7]

We have established a recovery priority list of the things that we feel that are important to the analysis of the failure of 51-L. Naturally, the most important item is the right SRB aft components, those components in the vicinity of where the suspected failure occurred.

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We also would like to obtain significant portions of the left SRB aft component so that we can use that, if you would, as a witness plate, so that we can compare the effects of the range destruct action and the impact with the water and also be able to look at other components to see if they experienced similar types of failure mechanisms that we may find on the right-hand components.

We also feel it is important to obtain some external tank to SRB attach struts. Part of the breakup mechanism had to involve failure of some of these structural elements, and we want to recover those so that we can better understand that failure mechanism. And the crew compartment is also a priority recovery.

Next chart, please.

(Viewgraph.) [Ref. 3/7-8]

As of today, we have located small portions of the right-hand SRB. We have the aft skirt assembly, which contains the thrust vector control system, the nozzle, and a few other components from the aft of the SRB.

We also have some portions of the aft segment, some case components. In the case of the left SRB, we have the aft skirt assembly, almost the entire skirt assembly, and a larger number of aft segment

1968

components.

We also have a forward dome igniter from the top of the SRB stack. We have not been able to recover that at this time, nor can we characterize whether it be right or left.

I would like to show you some video now. This particular video that you will see was taken by the Sea Link II on a dive in the area that we have now identified as the right-hand SRB.

Roll the video.

(A videotape was shown.) [Not published]

COLONEL O'CONNOR: There we are looking at part of the aft skirt assembly, and also the rear portion of the aft segment. As you can see in the video, here are some particles drifting by. That is characteristic of the water conditions we have in 1200 feet of water.

We're experiencing about a half a knot of current on the bottom at this time. As we go up through the water column, the current begins to increase up to about four and a half knots. It makes it a difficult recovery activity because we have to work all of our tools down through that varying current.

I also have some photographs which show more clearly the components that we have located.

1969

(Slide.) [Ref. 3/7-9]

These photographs were taken at the same time that the video was taken, also by the Sea Link II. As you can see, we have broken-up aft components. Most of this damage we feel probably occurred at the time of water impact of the components.

We have multitudes of parts on the bottom that must be recovered.

Next picture, please.

(Slide.) [Ref. 3/7-10]

In this picture you can see part of the clevis joint. This is not the joint that is suspect at this time. It is what is referred to as a factory joint, something that was fabricated at the manufacturing plant.

There is no indications of failure on this particular joint, although you can see towards the center right of the photograph some O-rings that are out of the groove and hanging down in the water. Naturally, it is important that we bring all of these O-rings and this clevis material up to the surface.

Next chart, please.

(Viewgraph.) [Ref. 3/7-11]

This sketch is indicative of the right-hand SRM hardware that we have identified to date. At the

1970

left, upper left, is the top of the case, the joint area that we are trying to locate at this time. The shaded portions indicate those portions of SRB case that we have located and feel that we can recover in the near future.

As you can see from this chart, we are not very close yet to that particular field joint. We have been impeded by some severe weather conditions, as many of these fronts have been going through the area, which impeded dive operations, and we hope to have improving weather conditions shortly so we can make—

DR. FEYNMAN: Could you explain a little bit better which end is which and how big this is? This isn't the whole booster, it's just a piece?

COLONEL O'CONNOR: No, this is the aft segment. This is the segment as it is brought from the factory to Kennedy for assembly with the rest of the SRB segments.

DR. FEYNMAN: And it has a skirt around it?

COLONEL O'CONNOR: It has a skirt at the bottom, at the lower right-hand corner, and the nozzle is attached to the circular opening you see also in that lower right-hand corner.

DR. COVERT: Colonel, do you think that that mid-section forward of the critical joint is going to be

1971

in roughly the same neighborhood as the after section?

COLONEL O'CONNOR: We had initially hoped we would find it very quickly after we found these components. So far we have been unsuccessful in locating it. We have used the NR-1 to do wide-area mapping around this particular point.

At this time, we're also trying to improve our radar and ballistic studies, so that we can have a better idea of where to locate these components. We are hoping that it is in the near vicinity and we just haven't found it yet.

DR. COVERT: We wish you good luck in this enterprise.

COLONEL O'CONNOR: Thank you.

Next chart, please.

(Viewgraph.) [Ref. 3/7-12]

In view of the critical nature of the SRB components to the investigation, we have put together a team to go and develop detailed procedures for the recovery, so that we can maximize the benefit of the recovered material.

We're going through a very methodical process of getting the engineering people involved, both from the manufacturers as well as from NASA design centers. We're getting metallurgists involved so that we can do

1972

on-site examination of the components before we attempt any lift, and they are also characterizing those components we're finding as to whether they be critical to the investigation or not. Therefore, we can prioritize the recovery.

Guidelines have been also thoroughly established and briefed to the task force on what recovery priorities we need to have, what other guidelines they feel are important to the investigative process.

Next chart, please.

(Viewgraph.) [Ref. 3/7-13]

I would like to now briefly cover some of the components we have recovered. These components were recovered both from the surface immediately following the accident, as well as some few parts that we have already recovered from the ocean bottom.

In the case of the orbiter, the significant items that we have found are a multitude of pieces of exterior skin with thermal tile. We now have the majority of the main propulsion system, the power heads, expansion nozzles, and turbo pumps.

We have located on the ocean floor but have not recovered major portions of the thrust structure of the aft part of the vehicle.

In the case of the SRB, we have both

1973

frustums, the forward part of the SRB, the left drogue chute, some gyros, a hydraulic reservoir, and some systems from the forward skirt, as well as those components that I have previously shown you on the other charts.

On the external tank, we have 60 percent of the inner tank skin. This is of high significance, showing burn patterns and some of the failure mechanisms.

Next chart, please.

(Viewgraph.) [Ref. 3/7-14]

In order to maximize the benefit associated with the recovered components, we are directing a detailed development implementation of handling documents, procedures, for all of the components that we have in hand.

We are taking these components, running them through metallurgical analysis, chemical analysis, and reconstructing as best we can the failure mechanism and the breakup modes that the vehicle experienced at the time of the accident.

After we have correlated the thermal and blast effects and determined probable breakup patterns from the metallurgists, we will use these particular scenarios and this evidence to go back and attempt to validate the

1974

conclusions that the rest of the NASA task force teams are putting together as far as what they feel from telemetry and optical data was the breakup failure mechanism.

In order to maximize the benefit of this process, we have brought an individual aboard of great experience.

Next chart, please.

(Viewgraph.) [Ref. 3/7-15]

Shortly after the accident occurred, the National Transportation Safety Board was contacted so that they could provide us some of their expertise. They have released to us Mr. Terry Armentrout, who is the director, Bureau of Accident Investigation, of the National Transportation Safety Board.

He is giving us his expert assistance in directing the activity that will reconstruct the flight components that we have recovered and direct the analysis of the failure mechanisms. He has been temporarily assigned to NASA and released from his other duties with the National Transportation Safety Board. He has also been granted permission to bring whatever other assets he feels necessary from National Transportation Safety Board.

He has been of great benefit to us.

1975

Next photograph, please.

(Slide.) [Ref. 3/7-16]

This is a photograph of the orbiter components that are now located in the NASA logistics building. Mr. Armentrout has directed the development of a grid system for placing all of the components in their proper geometric relationship so that we can best understand the breakup patterns, the flame patterns, the other failure mechanisms that might have been incident and collateral to the breakup of the vehicle.

Next chart, please.

(Slide.) [Ref. 3/7-17]

This photograph indicates where we are mocking up the external tank components. As you can see, we're using the same technique, gridding the floors, arranging the components so they can be available for review on both sides, and putting them in the proper geometric relationships to fully understand the breakup mechanisms.

Next chart, please.

(Viewgraph.) [Ref. 3/7-18]

As a result of the review of the components we have to date, we have some preliminary estimates of what we feel the orbiter experienced and how it broke up. With approximately ten percent of the orbiter now

1976

located, the metallurgical and chemical teams feel that we had a catastrophic in-flight breakup due to a combination of blast effects and aerodynamic overloads.

Basically, there is no evidence of anything exploding or igniting within the orbiter itself, from the evidence that we see. The external tank mockup, where we have approximately eight percent, also indicates a complete in-flight catastrophic breakup due to structural overload forces applied to it at angles and at velocities and at levels that are not normally experienced by an external tank.

We have no preliminary identification of the SRB components. We just don't have enough at this time.

I would like to also restate that these are preliminary estimates, based upon a relatively small amount of debris that we have recovered to date.

DR. COVERT: Colonel, are these loads on the external tank primarily internal loads or external loads?

COLONEL O'CONNOR: There appears to be a combination of both.

DR. COVERT: Are there places where you can identify differences?

COLONEL O'CONNOR: Yes, there are, and there

1977

are places on the external tank where we can detect impacts from the right SRB.

DR. COVERT: So these are consistent with the pictures that we have seen, then?

COLONEL O'CONNOR: Yes, they are.

DR. COVERT: Thank you.

CHAIRMAN ROGERS: Do you have any idea of how long the process may take? And I realize it's difficult to predict, but can you give us some idea of how you think it may progress?

COLONEL O'CONNOR: Seeing that we're putting the maximum available assets to work on it, we're hopeful that within another few weeks we will have completed our mapping effort, hopefully by the 1st of April.

And at that time we will be able to characterize all of the sonar contacts we have on the ocean floor. I would like to be able to say that we would have all of the parts of interest up within a week. I think a more likely case would be it would take us at least a month or two additional to locate and recover significant portions of the right-hand SRB.

CHAIRMAN ROGERS: So that you would estimate that—and I realize that this is just your best guess. I gather you would think, then, that within

1978

three months you might have completed your work?

COLONEL O'CONNOR: I would hope within three months that we would have recovered all necessary components to let us complete the analysis, and that the analysis at that time would be fairly far along, and that we would be doing an extensive set of cross-correlation with the other NASA design centers to understand and validate their conclusions.

CHAIRMAN ROGERS: Commission members were shown some of the reconstruction yesterday, and it really is truly remarkable the work that has been done, and I congratulate you and all of the team that have worked on it.

I notice that—what is the name for the SRB rocket, the frustum? I notice that you found both the right and the left.

COLONEL O'CONNOR: Yes.

CHAIRMAN ROGERS: Are you in a position to make any comments about the fact that the right one is quite severely damaged and the left one is not?

COLONEL O'CONNOR: Not at this time. We really need to have some more of the right-hand SRB to more fully understand that mechanism. We're also taking some photography and we're enhancing that photography at this time.

1979

It is quite likely that some of the damage to the right-hand frustum might have been the impact damage on the external tank, but we really can't confirm that at this time.

CHAIRMAN ROGERS: Well, I guess the other possibility would be on the ocean, when it struck.

COLONEL O'CONNOR: Yes, that's also true. It could be on the ocean, and we need to do quite a bit more work in that area before we can define that.

CHAIRMAN ROGERS: Thank you. Thank you very much, Colonel. We appreciate it.

DR. KEEL: Mr. Lang, Mr. Kennedy, and Mr. Barsh.

(Witnesses sworn.)

DATE: 7 MARCH 86

SEARCH

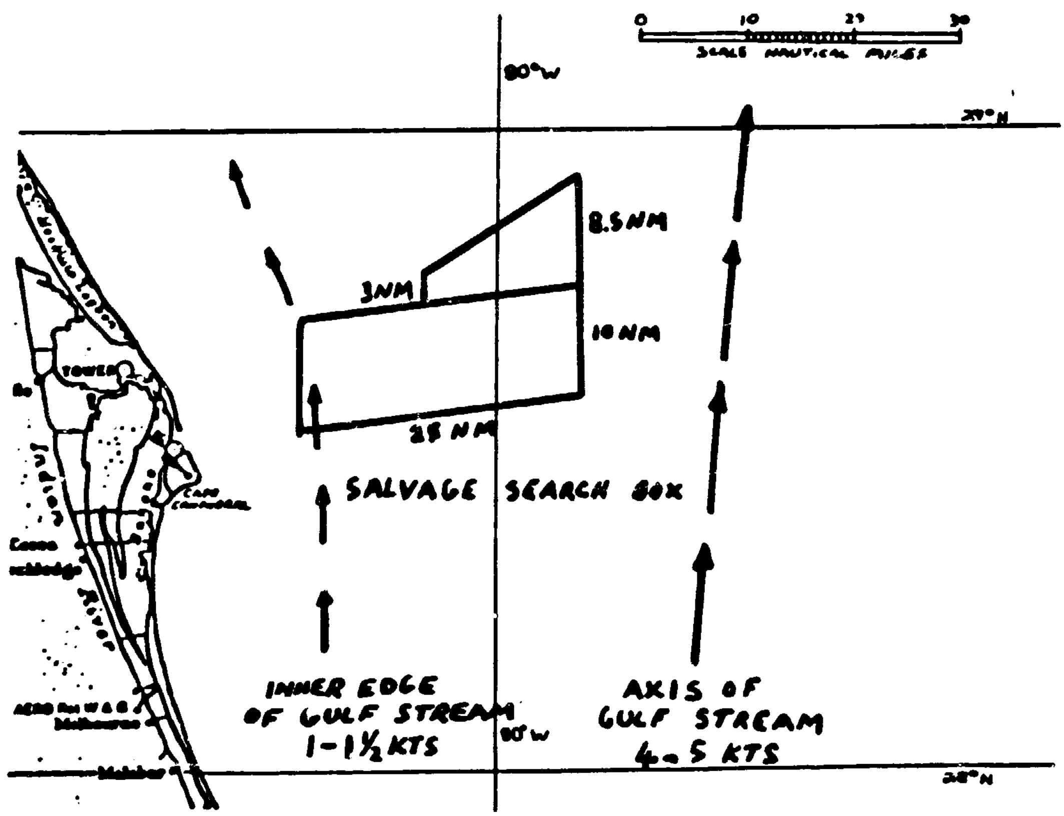
RECOVERY

RECONSTRUCTION

EDWARD O'CONNOR, JR.

COLONEL, U.S. AIR FORCE

[Ref. 3/7-1]



REFERENCE CHART

[Ref. 3/7-2]

OCEANOGRAPHIC CONDITIONS

OCEANOGRAPHIC FACTORS AFFECTING SALVAGE

- PROXIMITY OF THE GULF STREAM AXIS TO SEARCH BOX AND SRB AREA RESULTING IN 4 - 5 KTS CURRENT
- LITTLE SEASONAL VARIATION IN SPEED OR POSITION OF GULF STREAM
- DEPTH IN VICINITY OF THE SRBs (220 - 1200 FT)
- REMAINDER OF THE SEARCH AREA (90 - 220 FT)

[Ref. 3/7-3]

RECOVERY OPERATIONS

0 AT SEA OPERATIONS DIRECTED BY USN SUPERINTENDENT OF SALVAGE

0 ASSETS

11 SHIPS
1 MANNED SUBMERSIBLE
2 REMOTELY OPERATED VEHICLES
7 SONARS
41 DIVERS

[Ref. 3-7-4

ALLOCATION OF ASSETS

- SRB RECOVERY

  1) SUBMARINE NR-1 WITH USS SUNBIRD AS SUPPORT SHIP.
  2) STENA WORKHORSE WITH THE SUBMERSIBLE GEMINI
  3) SEWARD JOHNSON WITH THE SUBMERSIBLE JOHNSON SEA LINK II

- SHALLOW WATER RECOVERY

  1) PRESERVER
  2) USS SUNBIRD WHEN NOT NEEDED BY NR-1
  3) INDEPENDENCE WITH DEEP DRONE
  4) UTILITY CRAFT (LCU)

- SONAR SEARCH

  1) FREEDOM STAR
  2) LIBERTY STAR
  3) PAUL LANGEVIN III
  4) PIERCE

[Ref. 3/7-5]

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0 SONAR SEARCH RESULTS

350 SQUARE NAUTICAL MILES TO COVER
190 SQUARE NAUTICAL MILES SEARCHED

0 CONTACT STATUS

227 CONTACTS MADE
17 SHUTTLE COMPONENTS
25 NON-SHUTTLE COMPONENTS
185 REMAINING TO BE CONFIRMED

[Ref. 3/7-6]

RECOVERY PRIORITY

- O RIGHT SRB AFT COMPONENTS
- O LEFT SRB AFT COMPONENTS
- O ET TO SRB ATTACH STRUTS
- O CREW COMPARTMENT

[Ref. 3/7-7]

SOLID ROCKET MOTOR COMPONENTS LOCATED

O RIGHT SRB

- AFT SKIRT ASSEMBLY
- AFT SEGMENT (LOWER PORTIONS ONLY)

O LEFT SRB

- AFT SKIRT ASSEMBLY
- AFT SEGMENT

O FORWARD DOME AND IGNITER (LEFT OR RIGHT - NOT ESTABLISHED)

[Ref. 3/7-8]

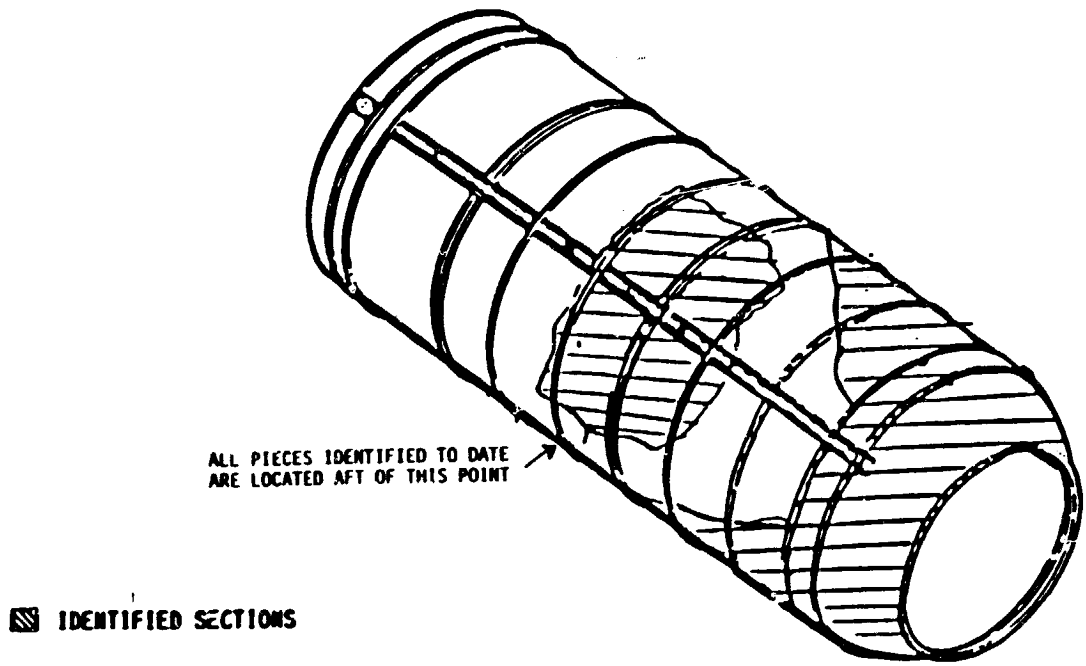
[NOT REPRODUCIBLE]

ORIGINAL PAGE IS OF POOR QUALITY

[Ref. 3/7-9]

[Ref. 3/7-10]

R.H. SRM HARDWARE IDENTIFIED



[Ref. 3/7-11]

SOLID ROCKET MOTOR RECOVERY PLAN

- NASA AND CONTRACTOR TEAM MADE UP OF SPECIALISTS IN DESIGN, HANDLING, AND METALLURGY WILL PROVIDE ONSITE SUPPORT IN THE RECOVERY OPERATION.
- IDENTIFICATION OF CRITICAL HARDWARE WILL BE PROVIDED TO THE RECOVERY TEAM ALONG WITH SUGGESTED SCHEMES FOR PRECLUDING DAMAGE DURING OPERATIONS.
- GUIDELINES HAVE BEEN ESTABLISHED FOR RECOVERY IN CONJUNCTION WITH DESIGN ENGINEERS.

[Ref. 3/7-12]

51L COMPONENTS RECOVERED

ORBITER

- LOWER FWD FUSELAGE PANELS
- ET/ORBITER LO_2 UMBILICAL
- MAJORITY OF MPS SYSTEM
- MAJORITY OF RUDDER SPEED BRAKE
- MAIN LANDING GEAR DOORS
- BODY FLAP
- RIGHT ELEVON
- PORTION OF PAYLOAD BAY DOORS
- PORTION OF RADIATORS
- LOWER SKIN PANELS
- PORTION OF AFT THRUST STRUCTURE
- PORTION OF ALL 3 MAIN ENGINES

SRB

- BOTH FRUSTRUMS
- LEFT DROGUE PARACHUTE
- L.H. RATE GYRO
- HYDRAULIC RESERVOIR
- SYSTEMS TUNNEL FROM FWD SKIRT

ET

- 60% OF INTER TANK SKIN
- PORTION OF LO_2 TANK
- PORTION OF LH_2 TANK
- NOSE CONE SKIN
- PORTION OF LO_2 FEEDLINE
- MAJORITY OF RANGE SAFETY DESTRUCTION SYSTEM

[Ref. 3/7-13]

RECONSTRUCTION AND ANALYSIS EFFORT

- DIRECT THE DEVELOPMENT AND IMPLEMENTATION OF PROCEDURES FOR HANDLING/DOCUMENTING THE RECOVERED 51-L COMPONENTS.
- DETERMINE PROBABLE BREAKUP SCENARIOS FROM EXAMINATION OF PHYSICAL EVIDENCE
- CONSIDERING THE FOLLOWING FACTORS:
  - INFLIGHT THERMAL AND BLAST EFFECT
  - INFLIGHT BREAKUP PATTERN (CRACK AND FRACTURE ANALYSIS)
  - DAMAGE ATTRIBUTED TO WATER IMPACT
- CORRELATE AND TEST SCENARIOS DEVELOPED BY NASA AGAINST WRECKAGE RECONSTRUCTION SCENARIOS

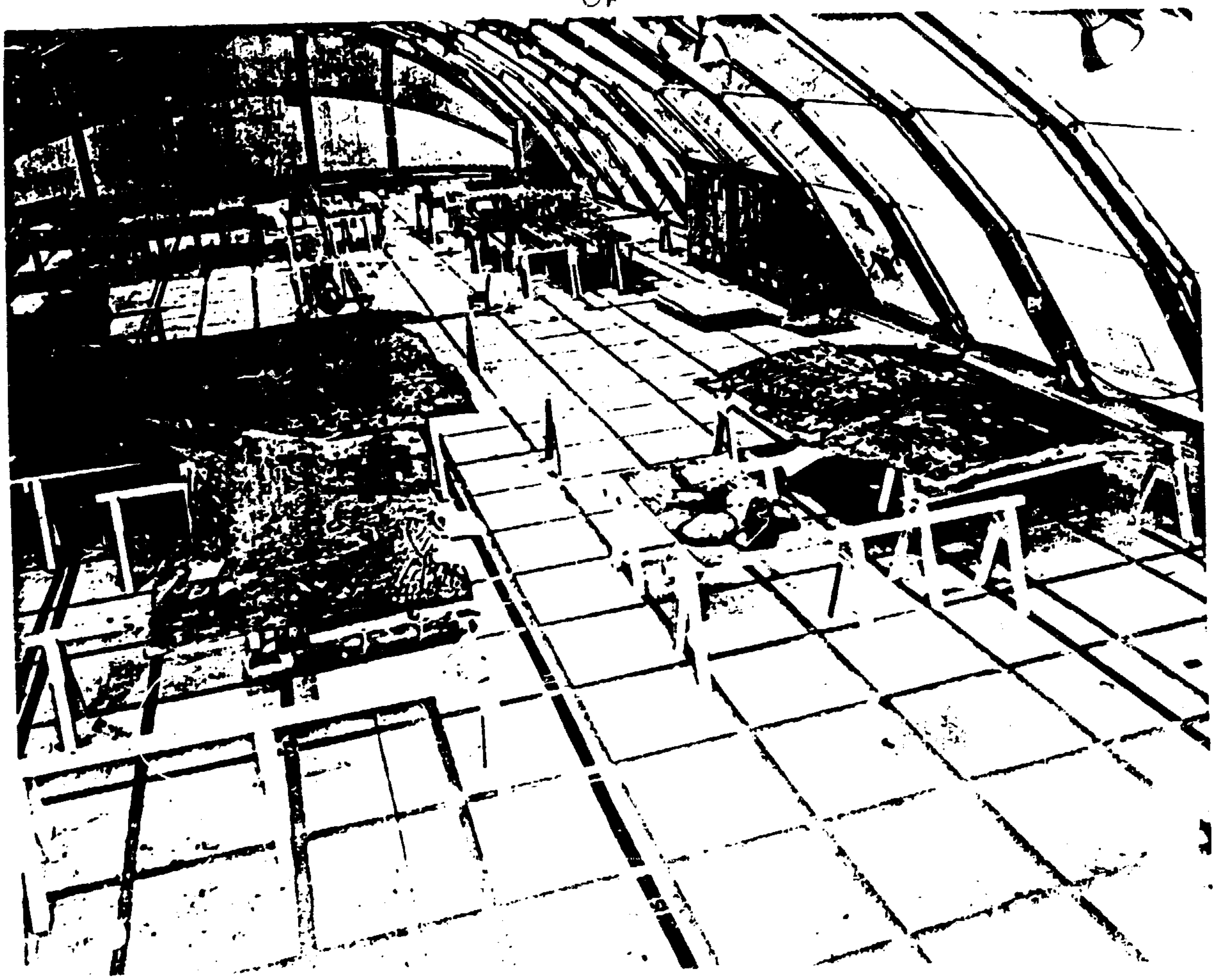
[Ref. 3/7-14]

MR. TERRY ARMENTROUT, DIRECTOR, BUREAU OF ACCIDENT INVESTIGATION, NATIONAL TRANSPORTATION AND SAFETY BOARD (NTSB) HAS BEEN SELECTED TO DIRECT THE ANALYSIS OF RECOVERED COMPONENTS FROM ALL FLIGHT ELEMENTS OF STS FLIGHT 51-L.

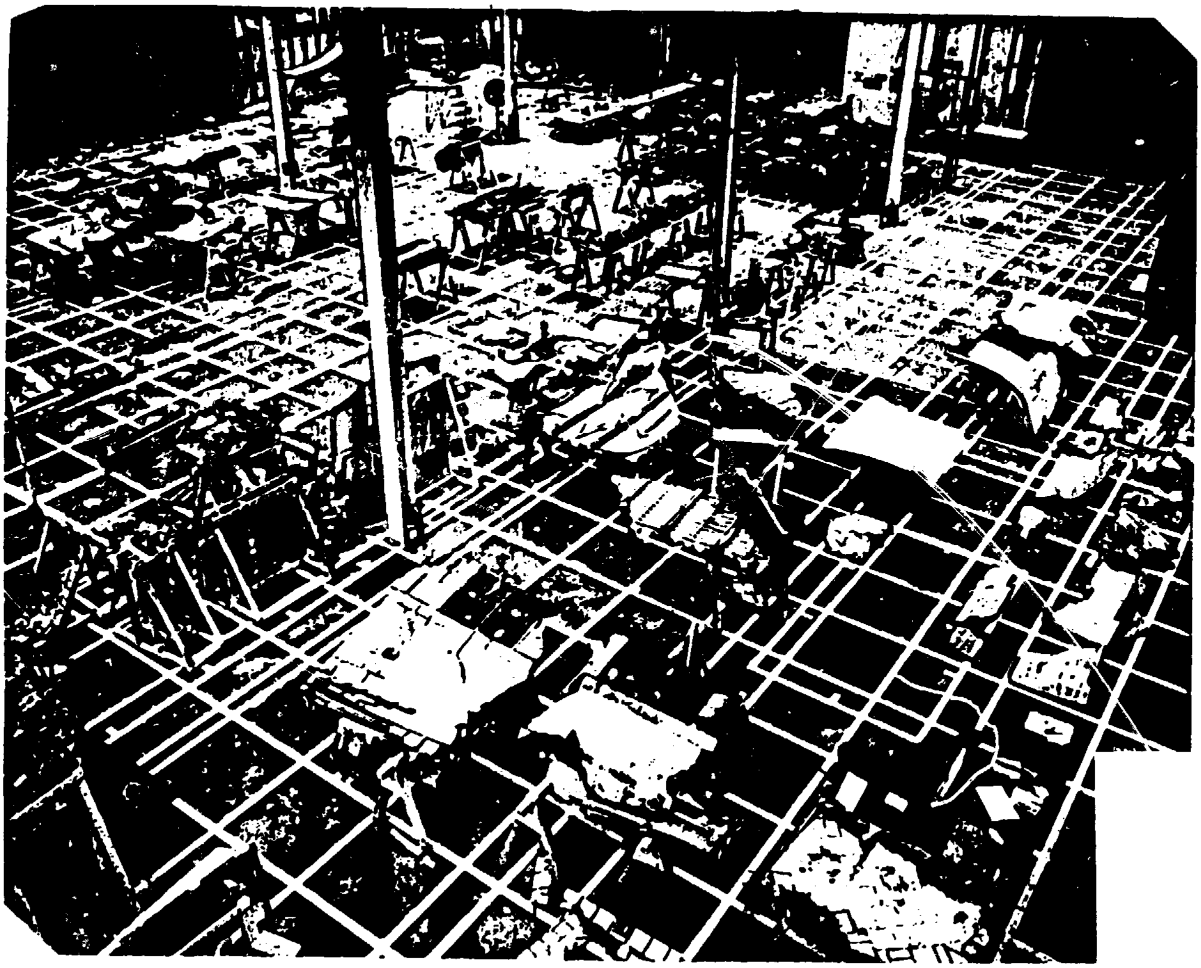
MR. ARMENTROUT HAS BEEN TEMPORARILY ASSIGNED TO NASA TO DIRECT THIS EFFORT. HE HAS AT HIS DISPOSAL THE RESOURCES OF THE NTSB AND NASA. HIS REPORTING WILL BE ONLY TO THE TASK FORCE AND THE COMMISSION.

[Ref. 3/7-15]

OR SHTER



[Ref. 3/7-16]



[Ref. 3/7-17]

PRELIMINARY ESTIMATES

- 0 THE ORBITER MOCK-UP OF AVAILABLE WRECKAGE (APPROXIMATELY 10%) INDICATES A CATASTROPHIC INFLIGHT BREAKUP DUE TO A COMBINATION OF BLAST EFFECT AND AERODYNAMIC OVERLOADS.

- 0 THE EXTERNAL TANK MOCK-UP OF AVAILABLE WRECKAGE (APPROXIMATELY 8%) INDICATES A COMPLETE INFLIGHT CATASTROPHIC BREAKUP DUE TO STRUCTURAL OVERLOAD, BASED UPON PRELIMINARY MACROSCOPIC FRACTURE SURFACE PROPAGATION FAILURE ANALYSIS.

[Ref. 3/7-18]

1980

TESTIMONY OF: ROBERT LANG, SHUTTLE OPERATIONS, MECHANICAL SYSTEMS DIVISION, KENNEDY SPACE CENTER; CARVER KENNEDY, DIRECTOR, VAB OPNS, MORTON-THIOKOL, INC., KSC; AND BILL BARSH, ENGINEERING MANAGER, EXTERNAL TANK/SOLID ROCKET BOOSTER OPERATIONS, LOCKHEED SPACE OPERATIONS COMPANY, KSC

CHAIRMAN ROGERS: Gentlemen, will you identify yourselves and make reference to your present assignment and anything else you care to tell us about yourself before you start the presentation.

MR. LANG: My name is Bob Lang. I'm with NASA here at Kennedy Space Center. I have been here for the past 19 years.

My present position is chief of the mechanical systems division and shuttle engineering. I have been in that position for the last eight months. Prior to that I was in the fluids division of the same directorate.

MR. KENNEDY: My name is Carver Kennedy. I'm employed by Morton-Thiokol. My present position is as director, vehicle assembly building operations, for the shuttle processing contractor. We perform this work under

1981

subcontract to Lockheed Space Operations, who is the SPC.

MR. BARSH: My name is Bill Barsh. I work for Lockheed Space Operations Company at KSC. I am the engineering manager for the ET/SRB operations. I have been involved in SRB stacking since 1977.

MR. LANG: Mr. Chairman, what I would like to do is have Mr. Kennedy present a brief overview of an SRB segment stack operation, and I will follow that with a discussion of this particular joint on the right SRB to point out any anomalous conditions that we noted during the processing, if that is okay.

CHAIRMAN ROGERS: Yes, that is fine. Thank you.

MR. KENNEDY: What I'm going to do is, utilizing charts, is take you through a standard flow from the time the segment arrives here at KSC and is turned over to the shuttle processing contractor until the completion of the making of the joint between the aft segment and the aft center segment, which is the one in question.

(Viewgraph.) [Ref. 3/7-19]

The first chart here, reading from left to right, the segments arrive by rail car. They weigh approximately 250,000 pounds—

1982

CHAIRMAN ROGERS: Can I interrupt to say, is this just a general description or are you talking about 51-L now?

MR. KENNEDY: Both. This is the process that 51-L followed and it's also the general pattern that we follow throughout.

CHAIRMAN ROGERS: Okay. If you can, when you present it, make reference to 51-L so that we know the difference between the general procedure and what happened in that case.

MR. KENNEDY: All right, sir.

The segments arrive by rail car. They are brought into the building, which we call the rotation processing and surge facility, or the RPSF. The rail car cover is removed and, utilizing two cranes in the building, we remove the segment from the rail car, rotate it from a horizontal position to a vertical, and place it on a support stand.

At that point, there are two heavy metal rings, one on each end, which completely enclose the joint, the clevis and the tang ends of the segment, and those heavy rings are used to tie the segment to the rail car during shipment.

After putting the segment vertical in the stand, we remove the bottom ring. In the meantime, we

1983

bring in an aft skirt assembly, which is furnished by USBI, completely assembled and delivered to us. We place it in what we call the buildup stand.

We then pick up this aft segment and mate it to the aft skirt assembly, as shown in section 6 on this chart. While we have the aft segment suspended, we do an inspection of the tang, which is the lower end, before we mate that to the aft skirt assembly.

Following the mating to the aft skirt assembly—

CHAIRMAN ROGERS: Can I ask, who is responsible for that mating? Who certifies that it is properly done?

MR. KENNEDY: That is performed by the Morton Thiokol technicians. It is signed off by Morton Thiokol quality, and in some cases, depending on the operation, there is a NASA quality buy in addition.

This particular operation, this joint right here is not a pressure vessel/joint. This is a structural joint between the aft skirt assembly and the aft segment. It does not see the internal gases from the rocket motor. It is strictly a structural attach between the skirt and the segment itself, the first aft segment.

CHAIRMAN ROGERS: But the ultimate

1984

responsibility for seeing that that is done properly is with Thiokol?

MR. KENNEDY: Yes, that is correct.

CHAIRMAN ROGERS: Thank you.

DR. COVERT: Mr. Kennedy, is this joint usually quite concentric and the mating proceeds without any necessary procedural modifications to ensure a fit?

MR. KENNEDY: Generally. Sometimes there are problems, but generally it proceeds fairly routinely.

DR. COVERT: What fraction of the time are there problems?

MR. KENNEDY: I cannot answer that without reviewing the data, sir. I'm sorry. This particular joint being a mechanical structural joint, we do not have those data.

DR. COVERT: Is it a normal procedure on 51-L, with no exceptions?

MR. KENNEDY: Yes.

DR. COVERT: No exceptions?

MR. KENNEDY: We had no what we call problem reports written against that one.

DR. COVERT: Thank you.

MR. KENNEDY: Following the completion of that structural joint, the components that are attached to the aft segment of the solid rocket motors, which

1985

consist of some electronics components, a structural external tank attach ring, and struts—and at this point there is a nozzle extension cone installed. It is shipped separately from the plant in Utah because of its length and size.

The nozzle itself is attached to the aft segment at the factory. We receive that here already attached. We install an extension cone after it arrives here, which is shipped separately.

Following the completion of this work, the completed aft assembly is picked up from the buildup stand and placed on what we call a transportation pallet. At that point, the top handling ring, which is still in place, having been on there since we came off the rail car, the top handling ring is removed and the clevis portion of that segment is inspected. That is the first inspection it receives.

It is inspected for any shipping damage, pitting, scratches, so forth, particularly in the O-ring areas. This is the clevis area that has the two O-ring grooves.

That completes the processing—

CHAIRMAN ROGERS: Who does that inspecting?

MR. KENNEDY: We also do that. Morton Thiokol does the inspection, and I believe it is signed off also

1986

by a NASA inspector.

CHAIRMAN ROGERS: Were any anomalies found in that process?

MR. KENNEDY: No, sir.

CHAIRMAN ROGERS: As far as 51-L is concerned?

MR. KENNEDY: No, sir.

MR. LANG: Mr. Chairman, let me restate, if you would. What Carver was doing was basically running through a standard type process. I was going to follow that up with a detailed discussion of any anomalies we found on the 51-L process.

CHAIRMAN ROGERS: Okay, fine.

MR. KENNEDY: The answer is still correct. We did not find any anomalies on the O-ring grooves.

May I have the next chart, please.

DR. FEYNMAN: These rings you're talking about, are they very strong? Do they keep the shape of the thing while it is on the car?

MR. KENNEDY: They are a weldment of steel, a one-piece weldment. They interface with the joint configuration of the end of the segment. They would offer some support, because they are strong enough to tie the segment to the rail car, and so they take the normal shipping loads and restrain it.

1987

And of course, they are not flight weight thickness. They are heavy sections.

(Viewgraph.) [Ref. 3/7-20]

This chart shows a similar process, again in the same building, for the center segments and the forward segments that are received. The main difference here is that we attach no hardware, nor do we attach any other parts to the center or forward segments.

It is unloaded in a similar manner. The rail car cover is removed, the segment is removed, it is placed on a stand. The bottom ring is removed, it is moved to a transportation pallet. The top ring is removed.

At both of these times when the shipping ring is removed, there is an inspection made of that particular end of the segment, looking for shipping damage, pitting, rust, et cetera. We do that with the segment suspended from the crane for the bottom part or the tang end of the segments. We do it after we remove the ring on the top end, just prior to moving the segment into the surge building.

At the completion of those inspections, the segments are moved into what we call the surge facility, which is—

DR. FEYNMAN: What do you inspect for? What

1988

do you look for? Scratches, broken pieces?

MR. KENNEDY: We have a criteria which looks for pitting, scratches, contamination, dirt, rust.

DR. FEYNMAN: How much do you allow?

MR. KENNEDY: We have a specification which allows visual blemishes which are not detectable by a five thousandths piece of shim stock or fingernail check. If it will not hang up a five thousandths shim stock—now, I'm being very general, because there are various and sundry inspections for various parts. That is the most critical one. That is the sealing surface on the tang.

MR. WALKER: Is there any grease on the tang at this point in time?

MR. KENNEDY: We remove the grease prior to inspection and re-apply it afterward.

MR. WALKER: It is shipped with grease and then that's cleaned off and inspected?

MR. KENNEDY: Yes.

MR. WALKER: Are there photographs made at this point in time?

MR. KENNEDY: Not at this point.

MR. WALKER: Thank you.

CHAIRMAN ROGERS: Could I ask a question about who does these inspections. Are these long-time

1989

employees of yours, and do the same people do the same work all the time or do you change them around often?

MR. KENNEDY: Generally, the inspectors are assigned to SRB, and they will do the inspections on any of the SRB activities.

They are our employees. They have been employed either by Morton Thiokol or by the predecessor USBI in most cases.

CHAIRMAN ROGERS: The Commission has heard reports that some of these people are overworked because of the pressure of getting these launches off, and that therefore they may not be as capable of doing their inspecting jobs as they might otherwise be.

Have you any records of whether they are overworked or not, or whether they have normal days of activity?

MR. KENNEDY: Well, they have normal days, and they do work some overtime. It depends on the activity. For instance, when we—and we're getting ahead of the presentation, but when we do our stacking operation we do that around the clock. We have propellant exposed. It is a hazardous operation and we continue around the clock until we complete the job.

And both the personnel, the supervisory and inspection and technical personnel, do work around the

1990

clock, and they will work some overtime depending on what the particular stack requires.

CHAIRMAN ROGERS: Do you have any reason to believe that any of these inspectors were not capable and not able to perform their duties efficiently?

MR. KENNEDY: No, sir, I do not.

CHAIRMAN ROGERS: Thank you.

MR. RUMMEL: Can you explain the fingernail check? Is this a process where the inspector is supposed to feel an imperfection?

MR. KENNEDY: The normal technique is to use a piece of a five thousandths brass shim stock. The specification which is placed on us here permits inspection by either method to determine whether there is a detectable imperfection in the surface.

CHAIRMAN ROGERS: Would you provide the Commission with a report of the inspectors who did this work on 51-L and how long they have been employed and whether they were overworked or not, how much time they had been given to do this responsible work?

MR. KENNEDY: Yes, sir, we will do that.

CHAIRMAN ROGERS: Thank you.

MR. RUMMEL: I'm still not clear on the fingernail check. How does that enter into this inspection procedure? Is the surface examined 100

1991

percent with fingernails, or is it visual and when he finds something he sees with his eyes then he scratches with his finger?

I just don't understand.

MR. KENNEDY: It is a visual inspection. Any visual blemish is then inspected with shim stock or fingernail or both, the visual inspection first.

MR. RUMMEL: Fingernail sounds unscientific. That is why I was asking.

MR. ACHESON: Mr. Kennedy, are you going to discuss the frequent loss of roundness phenomenon in your presentation?

MR. LANG: I was going to cover that.

MR. ACHESON: Very well.

DR. FEYNMAN: This surface where, on the tang, where the O-ring is in contact with the tang, the question is how flat, how smooth that is, how deep. Is this the last test that we make? What experience do we have on that?

MR. KENNEDY: I'm sorry, I missed your question.

DR. FEYNMAN: This isn't the last inspection?

MR. KENNEDY: No, this is the receiving inspection as it arrives on rail car, to detect any anomalies that have occurred during shipment or that may

1992

have been shipped from the plant, to detect—this is the first inspection. They are inspected again before stack operations.

VICE CHAIRMAN ARMSTRONG: What is the experience on those inspections? Have you found rejections?

MR. KENNEDY: On reused hardware, which is the majority of the hardware we see now, we do see some what I would call arrested corrosion. That is, it has been immersed in salt water, but it has been arrested, detailed, and used and sent back. It has been inspected at the plant and approved for use and sent back.

The inspection here, if there is any question about any of them, we take dental mold impressions and have it inspected. There is a criteria which comes, which is applied to us here at Kennedy, that we're not allowed to have any imperfections of certain dimensions and certain sizes, depending upon locations.

If there is any question about that, we take dental impressions and have it inspected and measured to see if it meets the criteria or not.

CHAIRMAN ROGERS: You may proceed.

MR. KENNEDY: May I have the next chart, please.

(Viewgraph.) [Ref. 3 7-21]

1993

This chart shows the process used in the vehicle assembly building. This is the large building where we actually assemble the solid rocket boosters. The segment, the aft segment, is brought in first by a transporter on a transportation pallet in the vertical orientation.

It is brought in and placed in place in front of the particular high bay that is to be used in assembling the vehicle. The lifting beam is attached. The segment is picked up, transported up into the high bay, and placed in position on a mobile launch platform.

At that point we install four holddown studs, which is the stud used to retain the entire assembly and to hold it to the launch pad until the time for launch. And we conduct a tensioning operation, which is an operation where we tension the stud and then run a large nut down to retain it, so we will retain the tension on the stud that is a compressive load between the skirt and the mobile launch platform.

This is typical of both sides, this operation. That is called placing the aft—placing the aft booster and tensioning.

May I have the next chart, please.

(Viewgraph.) [Ref. 3 7-22]

1994

The aft center segment is brought in from surge in a vertical orientation, placed in position, lifting beam is installed. That segment is lifted off the pallet, and at this point again the lower end or the tang end of the joint is cleaned. It has been previously greased. It has been in storage with grease.

It is cleaned, it is inspected again for pitting, corrosion, scratch, nicks, damage of any sort, contamination. It is an inspection by a company inspector, followed by a NASA inspector.

DR. FEYNMAN: How irregular is the surface of the tang? How many wiggles and how many scratches? How deep are they?

MR. KENNEDY: I'm sorry, sir?

DR. FEYNMAN: How good is the surface of the tang? How deep are scratches and so on?

MR. KENNEDY: Well, they are controlled by specification, and there is a myriad of those. I would be glad to furnish the exact specification, depending on the location.

For instance, in the pinholes where the retaining pins go through there is a criteria. There is a criteria on the O-ring sealing surface. There is a criteria on the non-sealing surfaces at those points,

1995

and that is a fairly detailed set of specifications.

DR. FEYNMAN: Are all of these criteria checked when you mate these?

MR. KENNEDY: Yes, they are inspected for that series of criteria.

MR. ACHESON: How frequent is it to find imperfections that are serious enough so that that particular segment is recycled or sent back to the factory for further processing?

MR. KENNEDY: I don't believe we have sent one back to the factory. There is a procedure by which blemishes or pits or scratches can be blended out and re-inspected, and that happens occasionally.

That requires what we call a problem report and what we call a material review board action if one of those is developed, which means it has to go back to the development center and the manufacturing plant for approval. And they provide the information on how they want it repaired or done.

MR. RUMMEL: Have you had any such cases in the ring grooves that you can recall?

MR. KENNEDY: Where we had to rework the O-ring grooves?

MR. RUMMEL: Yes.

MR. KENNEDY: I cannot answer precisely, but I

1996

suspect there may have been one during the program at least. I cannot recall precisely.

MR. RUMMEL: I would be quite interested in knowing that if you could find it out.

MR. BARSH: We have had some scratches in the O-ring grooves that we got concurrence with the design agency to go in there and polish them out using a fine-grit sandpaper.

MR. RUMMEL: I assume there are specific tolerances?

MR. BARSH: Yes, there is.

DR. COVERT: Are there detailed instructions on how you remove the results of the fine-grit sanding? Do you blast it and then inspect it with a magnifying glass?

MR. BARSH: After we use the sandpaper, we then go in with a cleaning solvent and take away all of the grit and wipe it clean, and regrease it again.

DR. COVERT: How far on either side along the length do the instructions ask you to clean it?

MR. BARSH: I would say that the groove is very small, and you have got to try and put a piece of sandpaper in there, and you sand it in a circumferential motion. You probably go about a foot on each side.

DR. COVERT: Thank you.

1997

CHAIRMAN ROGERS: Thank you.

MR. KENNEDY: Following the inspection of the tang—and the segment is hanging from a crane in what we call transfer aisle—measurements are taken at six locations around the tang end 30 degrees apart, to determine the diameters in those planes.

In the meantime, the segment which is on the MLP—that is, the aft segment with the clevis end up—is similarly inspected again and similarly measured in the same planes.

These measurements are compared and, assuming that we are within our criteria or our own self-applied criteria for successful mate, the segment is lifted over and placed. The putty is installed on the aft segment end or the clevis end.

The O-rings are emplaced, the segment is mated, and the pins are installed. The leak test is conducted and the closeout or the installation of the retainer band, and the external insulation is applied over that, on that particular joint.

And that is the standard flow. That is the flow that the 51-L right aft segment and the right aft center segment followed.

MR. LANG: Now I would like to back up, if you will, and address each of the general areas that Carver

1998

talked about, and starting with the aft segment operations in the processing facility.

(Viewgraph) [Ref. 3 7-23]

As he stated, we didn't have any anomalies of any kind on that particular segment in that facility, but I did want to mention—

CHAIRMAN ROGERS: Are all of your remarks going to be directed to 51-L?

MR. LANG: Yes, sir. In fact, all will be directed to the two segments on the right side booster, the aft and the aft center.

On the aft center or in the aft booster assembly, part of that operation involves, like Carver said, lifting the handling ring off the segment. That went nominally on this segment. And I do want to point out, originally in the processing of the segment intended for 51-L, the original booster—excuse me. The original segment intended for the left side forward center segment, there was an incident where the procedure was not properly followed.

The clevis end was damaged in the lifting operation. We took steps to correct the procedures to clarify all of the operations.

CHAIRMAN ROGERS: Could you be a little more specific? We have heard about this, but now it would be

1999

helpful if you could be real specific about this particular incident: What happened, who did it, what was the result, and so forth?

MR. LANG: I didn't bring lot of information with me, but basically what happened, in the handling ring as delivered from Thiokol is in two pieces, two general pieces. One is the solid ring that the crane hooks actually attach to. That is bolted to what we call a segmented ring that actually is—it is like an elbow shape, that fits down inside the clevis of the segment.

The procedure is intended to loosen all of the bolts between the solid ring and the segmented ring, and then to slightly lift up on that solid ring to relieve the load so the pins can be removed from the segmented ring. The segmented ring is the part that fits down in the clevis.

For whatever reason—and I can't give you the answer right now, but I can provide you a copy of the report—the bolts between the solid ring and the segmented ring were not loosened, so therefore there was no slop or tolerance in between those two particular rings.

We had a second thing happen to us, naturally. The lifting operation, the guys who were

2000

running the procedure, running the crane, were using a load cell to determine how much lift they were lifting up on, how much load they were lifting up on.

As it turned out, the particular load cell had failed. The operator was trying to lift a certain weight, thinking he was just lifting the ring. The load cell wasn't giving him indication, so he kept lifting. And at that time we had all but 31 of the pins removed, and he kept lifting.

And what happened was, he wound up trying to lift the total weight of the segment on 31 pins, and most of those pins were in the same quadrant. So it was just an unbalanced load, trying to lift too much weight on too few pins.

The damaged area was restricted to the segmented ring. One of the segments actually broke, physically broke, and I think it was two or three—and Bill, correct me if I'm wrong—of the clevis pinholes were slightly elongated, therefore damaged.

That segment was moved aside and another segment put in its place. All of the procedures, by the way, were modified such that all of the segments that flew on 51-L had had their rings removed with the modified, updated procedure. And we will be glad to provide you whatever details you would like.

2001

VICE CHAIRMAN ARMSTRONG: What happened to the ejected segment?

MR. LANG: I think it's still here. We have a team from Thiokol coming down to do a full inspection to determine flightworthiness. I don't know the results of that inspection. Bill or Carver, if you know—

MR. KENNEDY: I don't think we know here. The segment is still here in storage.

CHAIRMAN ROGERS: Is there any possibility that that incident contributed at all to the accident of 51-L?

MR. LANG: No, sir. We took special steps to go back and review that incident, the corrections to the procedure to prevent that kind of an incident, and all of the segments that flew on 51-L had been handled with the modified procedure. And we are very confident that that did not contribute at all.

CHAIRMAN ROGERS: So none of the damage that was involved in that incident could possibly have affected the 51-L launch?

MR. LANG: That is correct.

DR. RIDE: Just to be perfectly clear, the segment of the solid rocket that was damaged was not used on 51-L, so no damaged hardware from this incident flew?

2002

MR. LANG: That is correct.

DR. COVERT: Mr. Lang, are you going to talk about whether or not this had any influence on the stacking of the right-hand booster?

MR. LANG: I can mention that. It had an influence in this manner: The booster that was damaged, like I said earlier, was the left side forward center segment. And I don't understand the propulsion match from left side to right side. It is a performance matching that maybe Carver can address.

But to match the performance characteristics of the left side booster to the right side, the right side aft center segment, which is one of the segments that form the aft joint, was swapped out. And we did at that time bring in another booster, another segment, to be used for the right aft center segment for 51-L.

DR. COVERT: Would this require unstacking anything?

MR. LANG: No, sir. We had not at that time started the stacking operation. We were still just handling the segments off of the rail car and preparing them for transfer into the VAB.

DR. COVERT: Thank you.

MR. RUMMEL: When you speak of matching, are you speaking of burn rate?

2003

MR. LANG: Like I said, I really don't understand the performance matching, but I was told that was the reason for swapping out a right side segment.

DR. FEYNMAN: The variations in the propellant from different batches of making explosives is relatively large, and so what happens is two segments are poured from the same batch so that one will go on the right booster and one will go on the left booster, and they will burn at the same rate.

Then when the next pair of segments is put in they may burn at a different rate, but they will be the same on the left and right, so that both rockets will go always the same.

So if you have some trouble with one of the segments on the left side, it isn't right to leave the right one in because it may burn at a different rate, and it is better to take the other segment which was poured from the same explosive batch as the one that you just spoiled and use that on the other side with the new one that you put on the right side. And that is why it is done that way.

MR. LANG: That was the extent. There were no other anomalies, then, on the handling of the aft segment.

The aft center segment that we wound up moving

2004

into stack for 51-L, in its process, like Carver explained the inspection, we found two things in the detailed inspection.

One, we found a de-bond between the rubber insulation and the metal case on the lower end, the tang end. At that time we picked up a problem report and measured the depth of that de-bond area. The criteria is we are allowed to have de-bonds, by the way, up to five thousandths deep—excuse me, 50 thousandths deep.

We measured that particular de-bond and it was greater that 50 thousandths, and so we followed a standard repair procedure for that type of anomaly. We packed that de-bond area with an epoxy sealant, covered it with putty to protect it. And in parallel with that, we took a sample of that epoxy sealant, set it aside for cure test later on.

And I might note that that type of de-bond anomaly is fairly common. We have seen several on individual segments before. I think probably every stack has had one or two of those type anomalies. We have seen depth of de-bond areas much greater than I think this particular one was, .109 inches deep. And we have seen de-bonds much greater than that.

So we followed the standard repair procedure

2005

and packed that de-bond area with sealant and continued on with the rest of the inspection. The discussion that we just had about the tang inspection, there was in the area of between circumferentially 252 degrees to a little more than 300 degrees, there were pitting. There was pitting noted in that area of the tang at the O-ring sealing surface.

We did apply the criteria of using a five thousandths shim stock to see if we could tell any feel at all. Any feel at all would have driven us to, number one, a problem report condition; number two, mold impression to actually measure that depth.

In this case, there was absolutely no feel with the shim stock, with the five thousandths shim stock, and so we noted it in notes and comments in the processing log as an observation that met the criteria. It was not a problem report condition, and so we again pressed on.

MR. RUMMEL: Was the de-bond and the repair in the area where the smoke was seen to come out or some other location?

MR. BARSH: The de-bond was located between 165 degrees and 168 degrees.

MR. RUMMEL: And how does that relate?

MR. BARSH: Pardon me?

2006

MR. RUMMEL: How does that relate to the area where the initial smoke emission was observed?

MR. LANG: We think the area of concern is in the area of 300 degrees.

MR. RUMMEL: Thank you.

VICE CHAIRMAN ARMSTRONG: Does that mean that there was no burnishing or other finishing of the imperfections in the tang in this case?

MR. LANG: That is correct.

VICE CHAIRMAN ARMSTRONG: Could there have been on prior occasions, prior inspections or prior uses of this segment?

MR. LANG: Of this same segment? We researched that and we couldn't find any evidence of any documentation showing any anomaly in that or any other area on that tang. We went back through the data packs.

VICE CHAIRMAN ARMSTRONG: If there was finishing or burnishing, it would be recorded in the paperwork?

MR. LANG: I can't be sure of that. We think that it should be if they did any kind of what I would consider rework, but I am unsure of this. I probably shouldn't say it, but we think that at Thiokol they are allowed to do certain minor polishing, cleanup, if you

2007

will, without calling it rework. And therefore, if that was the case it would not appear in their rework records.

DR. FEYNMAN: You say you took an impression of this apparent place?

MR. LANG: No, sir, we did not. I didn't mean to imply that.

What we would have done had the shim stock caught or given us an indication of feeling something, we then at that time would have taken a mold impression to actually measure the depth of the anomaly. But we did not feel anything and so we did not take a mold impression.

DR. RIDE: Do you know how common it is to note pitting like this, that is less than your criteria? Has that happened on previous flights, or have you had a chance to go back to the logs?

MR. BARSH: Yes, it happens quite often that it is less than—I mean, it is an acceptable criteria. We had pitting in six locations, six degree locations on that tang.

DR. FEYNMAN: In the handbooks about O-rings and the conditions in which they're supposed to be used, they talk about surfaces having something like, they say, 64 RMS. I don't know what that means. And I would

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like to know how much RMS, or however you describe the character of the surfaces that you have on the tang that the O-ring is in contact with, so I can compare it to normal practice.

But I need to have some way of converting. I don't know what the "64 RMS" means, and I don't know what it means that you scratch with a fingernail or a piece of shim stock and find no five thousandths. I can't convert one number to the other. Can you help me?

MR. BARSH: Well, our inspection criteria doesn't require us to check the surface finish. We have to look at for visual imperfections, visually only. And if we see something like this, in the case of the pitting then we will use the shim stock method to try and determine if that is a new pit or if it is a pit that has been there and reworked.

DR. FEYNMAN: Okay. Now, when you rework it and you have some sandpaper, which, as you said, very fine or whatever it is, and you polish it away, that five thousandths thing that you discovered, then you've created a surface with some kind of an RMS. What is it?

MR. BARSH: I don't know, sir.

DR. COVERT: Mr. Lang, could you describe—

2009

you cut a little piece of shim stock. At the time you use it, is it sort of pointed or rounded, just so I can have a feel?

MR. LANG: I'll let Mr. Barsh describe that. He's witnessed that before.

MR. BARSH: It is a piece of shim stock, probably about a quarter of an inch wide, and it has a rounded tip on the end. It is not a pointy tip, but it is a rounded tip, and—

DR. COVERT: How long is it?

MR. BARSH: Probably four or five inches long; just something to be able to hold in your hands. The inspector can hold it in his hands and also to get down into the O-ring grooves if they see something in that area.

DR. FEYNMAN: What do you do, move it along and see if it gets caught?

MR. BARSH: That's right, see if you snag. If you have a pit, that is like a little crater, and if you snag the shim stock against the crater then you have a problem. But if it runs across there smoothly, you don't have a problem and it is an acceptable condition.

DR. COVERT: Is there a relation of the roundness of the edge to the maximum acceptable pit size?

2010

MR. BARSH: I don't know, sir.

CHAIRMAN ROGERS: Okay. Go ahead.

MR. LANG: Okay. The rest of the handling in the processing facility of that segment went well. We completed—we moved it to what Carver described as the surge facility, which is the storage facility, on November 26th.

On December 5th, we were then moving it into the VAB to begin the stacking operation. We have a safety requirement that says we can't have more than two segments in the VAB at one time unstacked—only one allowed in the transfer out, excuse me. We had one in there, and so we left that segment outside for a day.

What we were doing—and I will get to this in a little bit—we had a problem with the right side aft segment, mounting that on the mobile launch platform. We had a problem with the holddown post, and I will describe that in just a minute.

But what we did, we left that right aft center outside for a day. It got caught in a very heavy rainstorm, and we noticed water coming out from underneath the plastic cover on the lower end of the segment.

At that time we picked up a problem report, removed the cover, cleaned up all of the water from all

2011

of the visible accessible areas, cleaned up the tang so that it was perfectly dry, did the same thing to the top end, cleaned any water off the insulation and the clevis, cleaned it out, and closed the problem report as cleaning up the water.

DR. COVERT: What day was that?

MR. LANG: I think that was December 5th, was the day we moved to the VAB. I think the next day was the 6th, was the day we discovered the water and cleaned it up.

DR. COVERT: Thank you.

MR. LANG: Okay. Now, getting back to putting the aft segment on the MLP, typically what we do, we alternate left to right side each segment, aft, aft, aft center, aft center, and on up the stack. We had a problem in this case.

When we mounted the right side aft segment to the holddown posts—those are the four posts that the booster sits on and actually gets bolted in place—the holddown post number one is the post that is most inboard and on the orbiter side. It is inboards toward the tank, and on the orbiter side, on the right side booster, we had a problem.

We have to tension the bolts so that the bolts are loaded such that they will not relax, even through

2012

all of the orbiter mating operations, the external tank servicing, and even through SSME ignition. You've heard of the twang concept. When the main engines start before booster ignition, the bolts—the load on those bolts, holddown posts, will relax as the vehicle rotates.

And the requirement is to make sure the tension bolts, the stud tensioning bolts, and the holddown posts never slack, so there's always a positive tension in those bolts under all those conditions.

We measure the tension of those bolts with an ultrasonic transducer. The number one holddown post had a transducer that failed, and so we had to go and replace that booster—or that holddown post assembly or that portion of the holddown post assembly, the stud, the washers, the nuts, or the nut, that whole assembly.

Because of that work, we went ahead and stacked the entire left booster. It doesn't have any bearing, but it is just something that is different in the process. We normally rotate. In this case we stacked the whole left booster and then came back to the right side.

DR. RIDE: Had you ever done that before?

MR. BARSH: Yes, we have done that before.

DR. RIDE: Which flight? Do you know

2013

offhand?

MR. BARSH: Not offhand, I don't.

MR. LANG: Okay. We went ahead and put the right side, replaced the number one holddown post hardware, had no other problems with holddown post numbers one, three, or four.

Holddown post number two, however, we couldn't achieve the total desired tension on the bolt. The process is, we put a tensioner device, hydraulically operated tensioner device, on the end of the bolt, onto the stud, and pull it. And I don't know the forces off the top of my head. I'm sure Bill has them here, but pull it such that we get the maximum load in the bolt, and then torque a nut down to hold that bolt, and then relax the tensioner, and the tension between the head of the stud and the nut remains at a high load.

Well, it appeared we couldn't—then, by the way, we measured the transducer to see what that result in tension is. We couldn't, after removing the tensioner from the end of the stud, read the kind of tension we were shooting for.

We did, however, have a load in that stud that was more than adequate for actual stacking the booster. But we felt we were marginal for the rest of the loads on through SSME ignition.

2014

What we decided to do, however, was go ahead and stack that booster all the way up with that bolt tensioned to a good enough load for that stack, and after the stack was over we came back and replaced that bolt. We relaxed the tension on it, replaced the bolt, the stud, the nut, the washer assembly, and re-tensioned it.

MR. RUMMEL: When you replaced the bolt, didn't that place a sizable eccentric load in the stack?

MR. LANG: No, sir. The load in the stack stays the same. The load in the bolt—the stud is there just to squeeze the skirt of the booster to the holddown post.

MR. RUMMEL: In other words, it's sitting on the surface and the bolt simply holds it down?

MR. LANG: Yes, that is correct. And again, we've done this operation, either replaced or at least de-tensioned bolts, I think on four previous occasions.

Okay. Let me then go through the rest of this, the mating. We went through the putty application and I think all of you saw what that putty application looks like on the aft segment. We—this time frame now is December 7th.

We lifted the right aft center segment in the

2015

VAB in the transfer aisle, and there was a picture on the screen of that lifting operation.

(Viewgraph.) [Ref. 3/7-24]

The initial lift, as you can see, is what we call a four-point lift. There are four—just that, four points on the top of the segment that equally carry the load. It is at this point we do our initial what we call rounding measurement, the circumferential checks. And we have a criteria. We have a criteria that says the outer diameter of the tang cannot exceed the inner diameter of the outside leg of the clevis by more than .25 inches.

I think at this point if I could get chart 7 up, I would like to show that before I go into the use of the rounding tool, just in case there is any—and zero in, if you would, please, on the top half of that.

(Viewgraph.) [Ref. 3/7-25&26]

What I've shown here is the tang OD with relationship to the clevis, what I will just call the clevis ID, and you can see it's the outer leg of the clevis. That particular measurement we would call zero. The tang OD and the clevis ID are the same dimension and therefore that would certainly fit our criteria and we would go ahead and mate that.

And by the way, we take this dimension, this

2016

measurement, six places, six locations around the circumference of the segment.

If you would go to the lower one of that page, please. Here I tried to show what we would call plus. Again, if you look at the right side, the tang OD and the clevis ID are coincident, because we measure from the same point every time. But in this case the tang ID is slightly greater than the clevis ID, and that would be a plus and our criteria—let me back up.

I said our criteria is .25. That is our goal. Our requirement criteria really is to find that there is no flat metal on flat metal. And the way I drew this, ironically, it turns out I showed it flat on flat. If you can look at the left side, the bottom horizontal face of the tang, if you brought that straight down, would be flat on top of the upper flat level of the outer clevis leg.

That would violate our criteria. We would not mate that in that condition. As it turns out, that is a condition we had on this particular segment. We want to—now could you go back to number 5, please.

(Viewgraph.) [Ref. 3/7-25&26]

MR. SUTTER: How far out was it?

MR. LANG: We had a delta, if you will, of .512 inches of tang exceeding the clevis ID.

2017

MR. SUTTER: And the limit was .25?

MR. LANG: That is correct.

So we went to an option that we have used many times, what we call the two-point lift. If you can see—how to describe it? The lower right and the upper left legs of the four-point attach, those are hydraulically controlled such that we can put a man in a boatswain's chair, or I guess we use a High-Ranger or a Condor, and relieve the hydraulic pressure at those two points, so that we are now hanging the total load by the other two points.

And what that does for us, experience shows that as the booster is hanging, all of the propellant weight and mass tends to sag the segment, and the experience shows that—you can see the left side of the booster, the line running up and down, is the 90 degree point. That is the cable tray.

When the booster is shipped, that portion is up. The 90 degrees from that seems to be, in our experience base, the elliptical part that the booster seems to come together, form somewhat of an ellipse. And so what we do, we hang at the zero and 90 degree points—I'm sorry, zero and 180. And the other two sides tend to sag in and come in together.

Now, with the two-point lift we took three

2018

sets of measurements, and the best we could do is improve to a .334 positive difference. At that point, if you go to the next chart, please—

(Viewgraph.) [Ref. 3/7-27]

—we installed what we call a rounding tool or what I call a rounding tool. The guys call it a circumferential alignment tool. But the intent of this tool is to put a squeeze on the segment itself and try to deflect, bring the segment back into a dimension that will match up with the clevis.

We've used this type of tool six previous times, and the first time we used it we had a design that was strictly a manual mechanical design. It didn't have—it was not a hydraulic system. It was just a rod with nuts on both ends, and we would twist the nuts and just measure the deflection.

We had a criteria not to exceed a half-inch deflection using the rounding tool, and in this case we, like I said, we started off with the dimension of .334, and with the rounding tool in place we were able to deflect an additional .236 by squeezing in with the rounding tool.

MR. SUTTER: So in effect you changed from .51 down to almost zero?

MR. LANG: Actually, we changed from .51 down

2019

to .098.

MR. SUTTER: So about .4, roughly?

MR. LANG: That is correct, a total deflection of about .414, I think it is.

MR. SUTTER: What could this do to the bonding and the insulation? You had some de-bond. Would this bring on any more de-bonding?

MR. LANG: Our experience has shown that it has not, and the Marshall designers and Thiokol designers agree that that is not going to be, that is not a concern.

MR. SUTTER: What does cause de-bond? Is it transportation loads, or does anybody know?

MR. LANG: I don't know.

MR. KENNEDY: I think you would probably best ask that of the Morton Thiokol plant design folks. It is not an uncommon phenomenon in these segments to see. And these are minor.

These are two or three degrees in arc, 100 thousandths or so, and it is probably related to the method of manufacture of the internal insulation in the case.

But I'm not qualified to answer that question.

MR. SUTTER: I was wondering, though. Something starts it. There must be some kind of a load,

2020

and would that load change if you take it out of round from .5 to .4. I was just curious. You started out with some de-bond, and then you had to move the joint roughly four tenths of an inch, and that has got to put some kind of a load, and could it effect—could it bring on more de-bonding?

It builds a load in there that wasn't there. It has to.

MR. KENNEDY: I don't feel, since I'm not familiar with the design of that rocket motor, that I'm qualified to answer. I could get the answer for you on that particular subject. That requires some propellant mechanics and some bonding expertise that we don't have here at Kennedy.

MR. SUTTER: To change that joint four tenths of an inch and do it all with, say, the hydraulic unit, if you did it all with the hydraulic unit, what would be the load pushing on the side of the case? Do you know that?

MR. LANG: We have a conversion, reading the hydraulic pressure, a calculation that says 1200 psi hydraulics on that, on the piston, would equate to 3,000 pounds force into the segment. Thiokol has stated we could go as high as 5,000 pounds force on the segment with no problems.

2021

MR. RUMMEL: For clarification, on the quarter-inch limit, does that mean that if you have a differential dimension of a quarter inch between segments that you go ahead and mate? Is that the correct interpretation?

MR. KENNEDY: No, that's not exactly correct. A quarter inch is a self-imposed criteria. Based on experience, we know that if we are within a quarter of an inch interference fit, if you will, that the chances are we can successfully mate that segment to a clevis without reaching the, if you will, the flat on flat, which is the actual controlling criteria.

The criteria that are placed on us in all of these cases we keep talking about are placed on us by a design agency, and we're not allowed to have the end of the tang, the flat end metal end of the tang, contact the flat end of the outer clevis leg. And that is the controlling criteria.

The quarter inch is a factor we have developed here with experience, saying that if we have no more than a quarter, an apparent quarter inch interference, we can in fact avoid a flat on flat condition and attain a successful mate.

MR. RUMMEL: One more question on this line. The one unit was 512 thousandths out of round. Is there

2022

a limit as to the maximum out of roundness that would be acceptable, beyond which you would not try to squeeze it back into shape? Is there no limit?

MR. KENNEDY: No, sir.

DR. COVERT: Mr. Kennedy, if you have this thing within a quarter of an inch and then the beveled part of the tang slides into the clevis, there is a metal on metal action at the time that this mating takes place.

MR. KENNEDY: Yes.

DR. COVERT: Is it possible that chips or cracks or scratches result from this, and that ultimately might affect the surface smoothness so that it would exceed the 46 micro-inches?

MR. KENNEDY: The section we're talking about right now, of course, is the outer clevis leg and the tang, which is—

DR. COVERT: That gets covered up by that shim anyway?

MR. KENNEDY: Yes, that has a shim applied there. And keep in mind that both components are greased when they are assembled. They have a coating of grease of both clevis and tang components.

DR. FEYNMAN: How much is that—when I look at the picture of the tang, I see there's a kind of a

2023

beveled edge. That is not a sharp corner of the tang.

MR. KENNEDY: It does have a chamfer on it, yes, sir.

DR. FEYNMAN: How deep is that? That is, how far does it come in from the wall? Do you know?

MR. KENNEDY: The length, as I remember from the drawing, is .34, the length. That is up the length of the tang, and the angle called out on the drawing is 25 to 35 degrees. And so I believe mathematically—

MR. LANG: We calculate it comes out to .196, if you assume a 30 degree angle.

VICE CHAIRMAN ARMSTRONG: Has your experience been that the major axis of the ovality tends to be predictable based upon shipping considerations and so on?

MR. LANG: Yes, it does.

VICE CHAIRMAN ARMSTRONG: Can you identify what the tendency is?

MR. LANG: The tendency—if we could go back to number five, please.

(Viewgraph.) [Ref. 3 7-28]

MR. LANG: The tendency is that, since the cable tray, as you can see on the left side of that photograph there or sketch—the vertical line is the cable tunnel. It is shipped up, and therefore that is

2024

at 90 degrees. And our experience is at zero and 180 is the major axis.

And so while it's on the rail car with the 90 degrees up, it tends to flatten somewhat, if you will, and that has been our experience.

MR. SUTTER: How many times have you had to use a load of as high as 3,000 pounds to get your roundness criteria? Do you have the records on previous segments?

MR. BARSH: We have always had to go up to the maximum 1200 psi on the pressure gauge, each time. The deflection may be different, depending upon that particular case and propellant, but we pump it up to 1200 psi.

MR. SUTTER: And how many times has that happened? How many cases have you handled, and what percent of the time do you go that high?

MR. BARSH: Well, we've used the rounding tool six times, and every time we've gone up to 1200 psi.

MR. LANG: Let me back up. The first I think three or four, we didn't even—we weren't able to measure anything except deflection. It was all twist the nut and squeeze the case. So we actually couldn't measure any force at all.

And the criteria, the only criteria we had at

2025

that time was the maximum allowable deflection of a half an inch.

MR. SUTTER: Well, what percent of the joints have you had to use this technique?

MR. LANG: It's six out of how many? It's a lot.

MR. KENNEDY: I would like to calculate that, because there are six joints per assembly, and I need to go back and see the first assembly that we used it on. We have not used it since the beginning of the program. It has been in use since approximately December 1984. The first use of the rounding tool as such occurred in December 1984.

MR. SUTTER: It's a little bit unusual, then. That is what I am asking: is it unusual or not?

MR. KENNEDY: We don't use it every stack, that is correct.

MR. LANG: Bill has got some experience earlier in the flow, before we had a rounding tool, where we would hang a two-point configuration, trying to let the propellant weight and mass bring it into round. And didn't we have some fairly lengthy two-point hang times?

MR. BARSH: In the earlier part of the program, we had a different lifting beam than what you

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see on the monitor there. It interfaced with the handling ring, which stayed on the segment until after we stacked that segment, and then we removed the handling ring.

We had four lifting eyes on the handling ring, where we could take this two-point lift and switch axis in order to make the segment round in the way we needed to mate it. That was quite frequent, that we had to do that switch from a zero-180 axis over to a 90-270 axis in order to get the segment to egg out.

MR. SUTTER: As you started the program, you had new units, and now the units have been used one or two times. Have you noticed any difference between new and used units on having to use this technique?

MR. BARSH: I really can't answer that.

MR. KENNEDY: We would have to go back and look at the pedigree of the segments and which ones had new and which ones had used parts and which ones we used the rounding system on. It is not uncommon. As Bill said, it is not uncommon to have to shift from a four-point lift to a two-point lift and remain suspended from the crane for some number of hours to allow the mass of the segment to realign and bring it closer into the conformance to the clevis we're trying to put it in. That is, just about every flow we do that.

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MR. ACHESON: What is the longest it has taken, that you can recall, to achieve enough roundness to assemble?

MR. KENNEDY: That is probably before the rounding tool was developed, and it may be that the rounding tool was developed as a result of a situation like that, where it had hung a number of days and would not round up. And I would have to say, that would probably be in the neighborhood of days.

MR. LANG: We will have to find a good answer for you, because I think that data is available.

DR. FEYNMAN: You've explained that you can't [illegible]e the tang OD bigger that about .25 in your experience, to avoid metal contact. On the other side, suppose the tang OD is too small. Is there some criteria about how much too small it is before you get into trouble again on the other side?

MR. LANG: Could we have chart number 8, it is please.

(Viewgraph.) [Ref. 3/7-29]

I have already showed you the zero and the positive. Your question would be the negative side of that same measurement. The top half there shows a case where the tang OD is in fact smaller, like your described, than the outer leg of the clevis ID.

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Now if you would go to the lower, what I showed here is what we would consider a case where the negative number would be such that the inside, if you will, of the tang, it actually comes down in the same line, the same surface of the outside diameter of the inner clevis leg.

DR. FEYNMAN: What minus is that?

MR. LANG: Well, that minus, that depends, of course, on the actual dimensions of the tang and clevis in question. What I did on my own, I took the numbers off the drawing, took the worst case tang, the thickest tang by design allowance, and the narrowest clevis opening.

And I don't remember those precise numbers, but that dimension as shown there with the worst case tang and clevis would be a .320 negative difference.

DR. FEYNMAN: I understand it was minus .393, actually, when you went to mate these.

MR. LANG: That was our largest negative number recorded, that is correct.

DR. FEYNMAN: In other words, the situation that you have drawn, it is even worse.

MR. LANG: Well, it may be. But again, 320 is based on the worst case tang and clevis dimensions. I don't know the exact dimensions of this particular

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joint, although if you assume they are the worst case tolerances then that 393 would have been this case, that is right.

DR. FEYNMAN: In other words, this piece will come down and push directly on the O-ring as it is coming in.

MR. LANG: And it will squeeze the O-ring as it comes in. The chamfer of the tang would in fact ride the O-ring side and push it into the O-ring groove.

DR. RIDE: Is there a chance it could have come down metal on metal on that side?

MR. LANG: We don't think so, although it is hard to see, because you can see it is on the inside as the segment comes down. To actually get metal on metal, again, if you will take the worst case, the 320 worst case of those surfaces coincide, the chamfer on the tang, if we say .2 is the correct dimension, it is certainly close to that.

Add that to the 320. What is that? Five something, and all that says is you're going to ride down at minus 5 something, you will ride down the chamfer, just like we do on the outside. Metal on metal I think would be a larger number, and I don't know what that would be. We could probably calculate it or something close.

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DR. RIDE: Could you tell us where roughly circumferentially the minus .393 occurred?

MR. BARSH: It was at 120 degrees, 120 and 300.

MR. KENNEDY: I don't think we answered all the questions. I think the first question that started this discussion was do we have any criteria placed on us on a negative. We do not.

And this segment, by the way, was not the maximum negative we have assembled.

DR. FEYNMAN: It's possible we would have some rubbing and produce some metal shavings in ther[e] by the rubbing of one piece against another?

MR. LANG: I don't think we can say that it is impossible, but the surfaces are inspected for any kind of metal deformation, raised metal that might cause scraping. And certainly, like Carver stated, the surfaces are greased, and our experience has been, taking them apart—and we had—like Carver said, we know we've had several cases bigger than this, and we have had probably quite a few that had metal on metal, not quite this bad, if we assume 393 was bad.

But we still haven't seen, taking the boosters apart, that kind of evidence or that kind of damage.

MR. RUMMEL: Do you know how much out of

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roundness occurred in this particular joint in 51-L after the shims were applied?

MR. LANG: I'm sorry? Say again, please?

MR. RUMMEL: Do you know how much out of roundness existed after the shims were applied to this joint?

MR. LANG: No, sir, we don't have any way to measure that.

MR. RUMMEL: One of the purposes of the shims is to help to keep it round, is it not?

MR. LANG: It's my understanding it is to help the squeeze on the O-rings, that is right, that is its primary purpose.

MR. ACHESON: When you have this condition, have you ever, after seating the upper segment, have you ever lifted it off again to see what the effect was on metal to metal contact or on the O-rings?

MR. LANG: The only time we have pulled a segment apart is if we had—if we failed a leak check of the O-rings, and I think that has happened on three different occasions. And as far as I know—in fact, I can state that there was no metal damage.

MR. ACHESON: Have you examined—where you had these extreme out of roundness problems, have you examined the segments that have been recovered to

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ascertain metal to metal damage or damage to the O-rings by the tight fit?

MR. KENNEDY: The inspection of the recovered hardware is all done at the Morton Thiokol plant in Utah. We don't do any of that inspection here, and that data would have been recorded and examined there.

I am not aware of any indication of any damage. But their post-flight reports would have indicated that if they found it.

MR. ACHESON: During the stacking process, the inspectors who initially inspect the work, I take it these are Thiokol direct employees?

MR. KENNEDY: Inspect the tang and the clevis, yes.

MR. ACHESON: And in fact inspect each stage of the assembly process?

MR. KENNEDY: That is correct, until the joint is made.

MR. ACHESON: Now, Thiokol being responsible to Lockheed, Mr. Barsh, perhaps you can tell me, does Lockheed then have its inspectors inspect that work, or do they accept the Thiokol inspection?

MR. BARSH: They accept the Thiokol inspectors, and there are certain places in the procedures where a NASA inspector has also got to buy

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that step.

CHAIRMAN ROGERS: Dr. Ride.

DR. RIDE: Have you had a chance to go back and talk to the technicians who were watching the mating process, watching the tang go into the clevis, to find out whether they have—actually were able to witness any anomalies?

MR. LANG: No, we have not.

DR. RIDE: How many people do you have stationed around the circumference during that process?

MR. LANG: I think we have a requirement that says four minimum. Normally there are six or seven. I think in this case, Carver, you said six?

MR. KENNEDY: I believe that there would be at least six technicians alone. There would be an inspector. There would be probably some representative of what we call the LSS, which is an oversight group from Morton Thiokol plant. And a NASA inspector would be there.

DR. RIDE: Do you think that they would have been able to see around that area at either 120 degrees or 300 degrees, to watch the tang as it went in and see whether it affected the O-ring or the inner surface, the sealing surface?

MR. KENNEDY: I would have to say that, the

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emphasis being on not reaching a flat on flat condition on the outer leg, the concentration on watching the outer leg is focused there. And that is where most of the attention is.

However, since the inner leg is higher than the outer leg, there is a step in the procedure that is, as you come down you must center the tang, so that it does not—so that it is not eccentric to the clevis. That is, you look—it is implicit that you look to see that you're not going to hit the tang on the end of the inner clevis leg.

And if you are, in fact there are instructions in the procedure of how to direct the crane to move the segment so that it is centered over the clevis. But insofar as being a step in the procedure to look for flat on flat on the inner leg and the clevis, there is no step in the procedure.

The requirement it places on us is on the outer leg.

DR. RIDE: Is that area around 300 degrees, is that an easy area to see? Is that something that, if they were looking there, they probably would have noticed that?

MR. KENNEDY: You can walk completely around the clevis. There is nothing else there except the

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platform. You can walk around the platform 360 degrees.

DR. RIDE: Do you have plans to go back and talk to those technicians?

MR. LANG: Yes, we do.

DR. FEYNMAN: Technicians have a certain amount of trouble with a particular fact that the procedure is unsatisfactory. If you have decided that six diameters are correct, you still don't know that it will fit.

Let me suppose, for example, that the bottom is an exact circle, so the six diameters of the bottom of the clevis are all equal, that then the tang could be a figure something like this, which has six diameters, if you figure it out, all equal; and yet, it wouldn't fit in because it is more triangular

And they find this from time to time, and have given it a name and tried to fix it. But there is no fix-up up and down between the guys who are doing the work and the people who have written the procedure to discover that they have this difficulty from time to time.

MR. LANG: Well, again, that case, I guess, that you drew could be true some time, some day. But the criteria, again, is no flat on flat, and that is

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carefully inspected all the way around.

MR. KENNEDY: That is a 360 degree requirement, not just at six locations.

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6-28977 UNCLAS

1.0 4.5 2.8 2.5
5.0 3.2 2.2
5.6 3.6
6.3 4.0 2.0
7.1
8.0
1.1 1.8
1.25 1.4 1.6

MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS
STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

VICE CHAIRMAN ARMSTRONG: When you have a difficult fit, do you forward that information back to the plant so that on their post-recovery inspection they can correlate their inspections to those pre-flight difficulties, stacking difficulties?

MR. KENNEDY: The information is forwarded to the plant. I do not know what use is made of that, but the information is forwarded. In fact, as I said, they have a group here called LSS, a representative group here, and they actually prepare that report. They prepare a summary of each stack and furnish that information back to the plant in Utah.

And it covers other things other than this dimension. It covers all of the activities on the SRB's.

CHAIRMAN ROGERS: Mr. Lang, does that complete your presentation?

MR. LANG: I had a couple of more points to go over.

CHAIRMAN ROGERS: Why don't you go ahead.

MR. LANG: We, as discussed, wound up meeting the criteria of no flat on flat, and—well, let me

2037

back up a minute.

While we were in the process of those final dimensions, we proceeded with the O-ring installation. I wanted to mention particularly the O-ring installation and the O-ring inspection criteria because it has become obvious to most people we do not inspect the O-rings per se here at Kennedy Space Center.

At one time in the program, we did. That inspection took place prior to installation up on the platform, up on the level. And subsequently in the greasing process, it was a long-term—not a long-term, but a handling process.

It was decided that the inspections would take place back in the Thiokol plant. The O-rings would be greased there, packaged very carefully, and all we would do here at Kennedy Space Center is inspect the bag for any damage and then remove it from the bag and install it.

CHAIRMAN ROGERS: When was that change made?

MR. LANG: That was—I don't have the date for this. I promised I would do that and I didn't. That change was made for the STS-13 stack, and it has been that way ever since.

CHAIRMAN ROGERS: So you've had a lot of launches since then.

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MR. LANG: Yes, we have.

April 1984, I'm told. In this case, in this particular segment, the first two O-rings that we took out of the box to install we rejected because the inner bag had some problem with it. I'm not sure—we have been trying to find the exact nature of that. We think it was merely the bag was not thermally sealed; it was folded over and stapled.

But at any rate they were rejected. We got two new O-rings out. The bags were very carefully inspected and opened and installed.

We then went through the process of then lowering, after the rounding tool was removed, of course, then lowering the tang into the clevis. That operation was completed, the pins installed, and of course the pin retainer clips, as they're called, were installed, and we performed the joint leak check.

Now, that leak check procedure is an initial pressurization to 200 psi between—in the cavity between the two O-rings, and followed by—and that 200 psi, by the way, is maintained throughout a 15-minute time period, constantly maintained such that if there was a leak of any kind we wouldn't see it, because we maintain the source.

After the 15-minute wait, we vent that

C-4

2039

pressure down, let everything settle for a 15 minute stabilization period, come back up to 50 psi, isolate the source at this time, and do a 50 psi pressure check for ten minutes. And our criteria is we are allowed one psi decay from the 50 in ten minutes, and this particular joint, it didn't indicate any leakage at all.

Now, that leak check procedure, by the way, has had some modifications, too. We started off initially in the program through STS-7 just pressurizing it to 50 and doing a leak check. For STS-8 and 9, the procedure was modified to pressurize to 100 psi for—just momentarily come up to 100, and I think some number of seconds—was it eight seconds?

And we were told that the rationale there was to make sure that if there was a problem with the O-ring the flow, the GN-2 flow would go through that O-ring, through the putty, so that when we turn around and pressurize to 50 to do the leak check putty would not mask an O-ring leak.

STS-11, that procedure was again modified, to increase that initial pressure to 200 and to maintain it for 15 minutes, like we did on this joint. And again, the primary reason was to make sure that the putty did not mask a faulty O-ring.

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That is the history of the O-ring leak check procedure. And I was just going to say, that is all we had planned.

CHAIRMAN ROGERS: Mr. Lang, I gather you've been head of the SRB joint group investigating?

MR. LANG: Yes, I have been, that's right

CHAIRMAN ROGERS: And how large is that group?

MR. LANG: About 12 people, roughly.

CHAIRMAN ROGERS: Can you give us some idea when that—when your investigation will be completed?

MR. LANG: Actually, we are fairly close, I think, to completing our portion of it. We directly looked at this one joint to make sure, to go find out everything we did to it, to uncover anything at all that may indicate that there was something in our process that may have been a problem.

CHAIRMAN ROGERS: Do you plan to have a written report?

MR. LANG: Yes, sir, and I would hope to have that report completed in maybe a couple of weeks.

CHAIRMAN ROGERS: As long as that, two weeks?

MR. LANG: A week.

[Laughter.]

CHAIRMAN ROGERS: Good. We're making

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progress.

[Laughter.]

CHAIRMAN ROGERS: Well, the reason I ask, of course, this Commission only lasts another 90 days, and we're anxious to move ahead as quickly as possible. And we would like as soon as you can to get a copy of your report, because we want to work with you, and we're going to have to rely in large measure on the work that you do.

We will be free, of course, to make suggestions or to make any further investigation. But by and large, we appreciate the speed with which you are operating, and we hope that you will give us the report as soon as you can.

Thank you very much for your presentation. We will take a ten minute recess now, please.

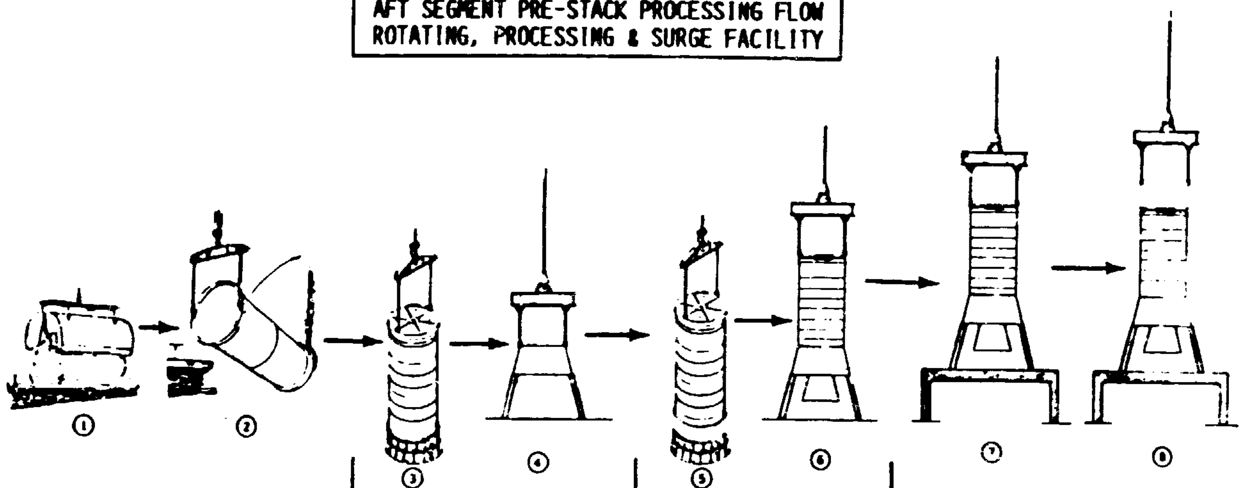
(Recess.)

CHAIRMAN ROGERS: The Commission will come to order, please.

DR. KEEL: Mr. Moser, Mr. Littles, and Mr. Lee.

(Witnesses sworn.)

AFT SEGMENT PRE-STACK PROCESSING FLOW
ROTATING, PROCESSING & SURGE FACILITY



① o RECEIVE RAIL CAR IN RPSF
o REMOVE RAIL CAR COVER
o RETRIEVE SEGMENT DATA PACK
o CONDUCT RECEIVING INSP. PROPELLANT GRAIN

② o ROTATE SEGMENT TO VERTICAL TWO CRANE OPERATION

③ o PLACE SEGMENT IN SUPPORT STAND SHIPPING RINGS ATTACHED, BOTH ENDS

④ o INSTALL AFT SKIRT ASSY IN BUILD-UP STAND
o PREPARE CLEVIS FOR STACK (TOP END)

⑤ o UNBOLT/UNPIN LOWER RING
o LIFT SEGMENT FROM RING
o INSPECT JOINT TANG (LOWER END)

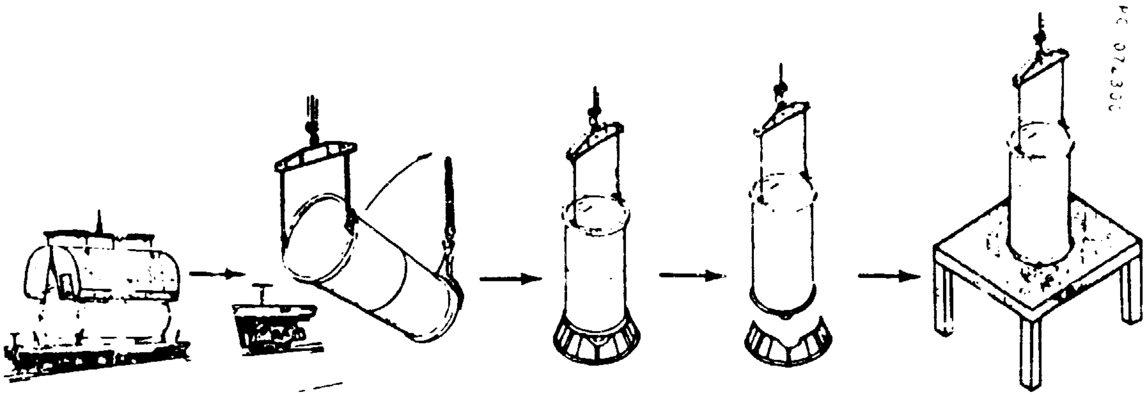
⑥ o MATE SEGMENT TO AFT SKIRT ASSY IN BUILD UP STAND
o INSTALL COMPONENTS
- NOZZLE CONE, ET RING/STIFFNER, IEA AND CABLING

⑦ o LIFT AFT ASSY FROM BUILD UP STAND
o INSTALL SEGMENT ON PALLET

⑧ o UNBOLT/UNPIN UPPER RING
o LIFT RING FROM SEGMENT
o INSPECT JOINT CLEVIS (UPPER END)
o TRANSFER SEGMENT ON PALLET TO SURGE

[Ref. 3/7-19]

CENTER & FORWARD SEGMENT PROCESSING FLOW IN ROTATING, PROCESSING & SURGE FACILITY (RPSF)



- RECEIVE RAIL CAR IN RPSF
- REMOVE RAIL CAR COVER
- RETRIEVE SEGMENT DATA PACK
- CONDUCT RECEIVING INSP. PROPELLANT GRAIN

→

- ROTATE SEGMENT TO VERTICAL TWO CRANE OPERATION
- PLACE SEGMENT IN SUPPORT STAND SHIPPING RINGS ATTACHED, BOTH ENDS

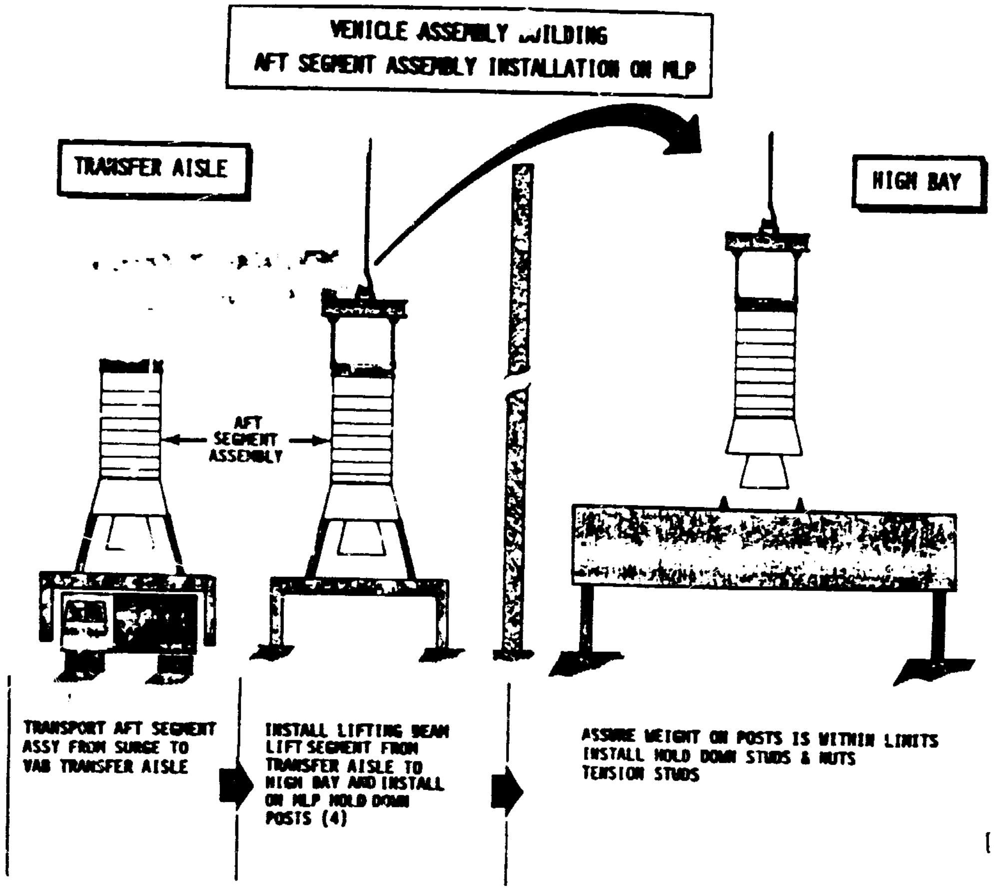
→

- UNBOLT/UNPIN LOWER RING
- LIFT SEGMENT FROM RING
- INSPECT JOINT TANG (LOWER END)

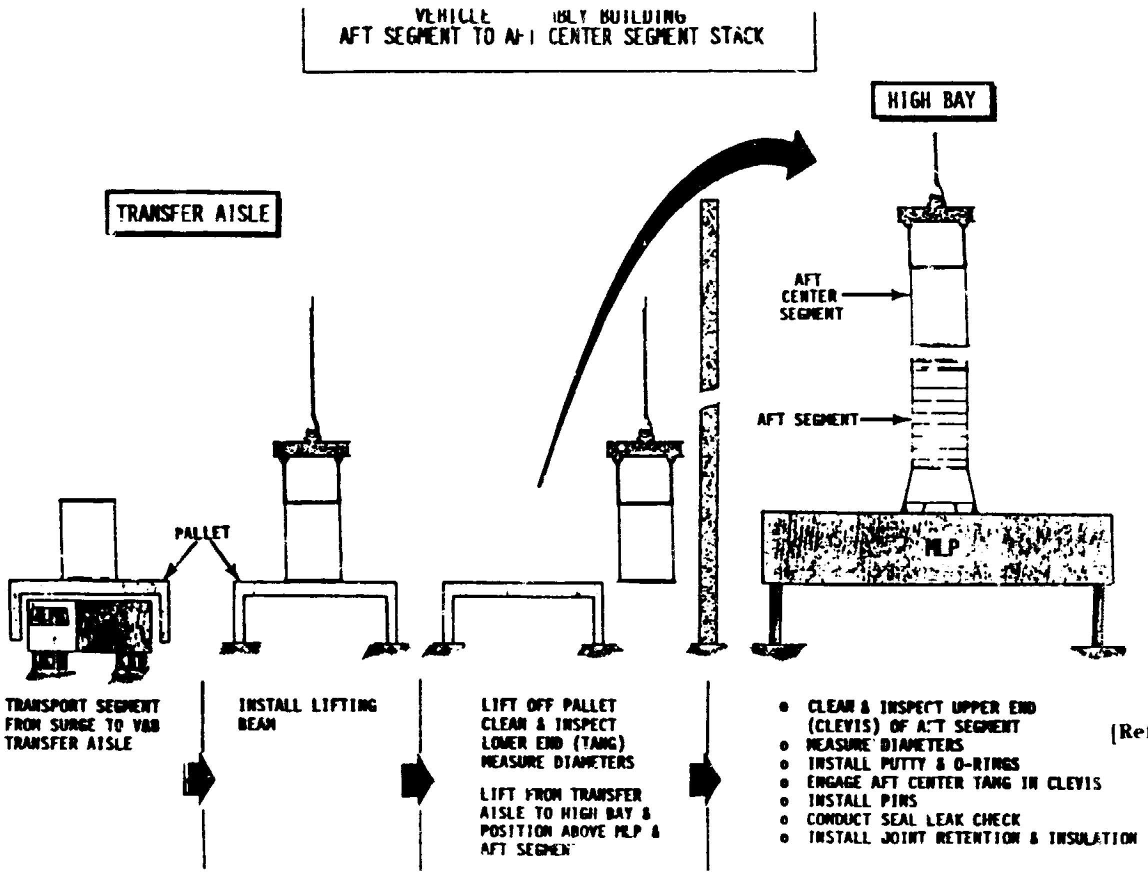
→

- INSTALL SEGMENT ON PALLET
- UNBOLT/UNPIN UPPER RING
- LIFT RING FROM SEGMENT
- INSPECT JOINT CLEVIS (UPPER END)
- TRANSFER SEGMENT ON PALLET TO SURGE

[Ref. 3/7-20]



[Ref. 3/7-21]



[Ref. 3/7-22]

KSC SHUTTLE OPERATIONS

NAME:
ORG.:
DATE:

ACTIVITY	DATE	REMARKS
O AFT SEGMENT OPERATIONS		
o GRAIN INSPECTION COMPLETE	5 NOV. 85	
o OFFLOAD COMPLETE	5 NOV 85	
o MATE TO AFT SKIRT COMPLETE	6 NOV 85	
o BOOSTER BUILDUP COMPLETE	27 NOV 85	
o MOVE TO INSPECTION STAND	27 NOV 85	
o FORWARD HANDLING RING REMOVAL AND INHIBITOR INSPECTION COMPLETE	27 NOV 85	
o CLEVIS INSPECTION COMPLETE	27 NOV 85	
o TRANSFER TO VAB COMPLETE	2 DEC 85	

[Ref. 3/7-23 1 of 3]

KSC SHUTTLE OPERATIONS

NAME:
ORG.:
DATE:

ACTIVITY	DATE	REMARKS
O AFT CENTER SEGMENT OPERATIONS		
o GRAIN INSPECTION COMPLETE	20 NOV 85	PR-SR-RAC-50-0001 CASE DEBOND
o OFFLOAD & TANG INSPECTION COMPLETE	23 NOV 85	NOTES & COMMENTS ON PITTING
o FORWARD HANDLILNG RING REMOVAL & INHIBITOR INSPECTION COMPLETE	23 NOV 85	
o CLEVIS INSPECTION COMPLETE	25 NOV 85	
o MOVE SEGMENT TO SURGE	26 NOV 85	
o MOVE SEGMENT TO VAB	5 DEC 85	

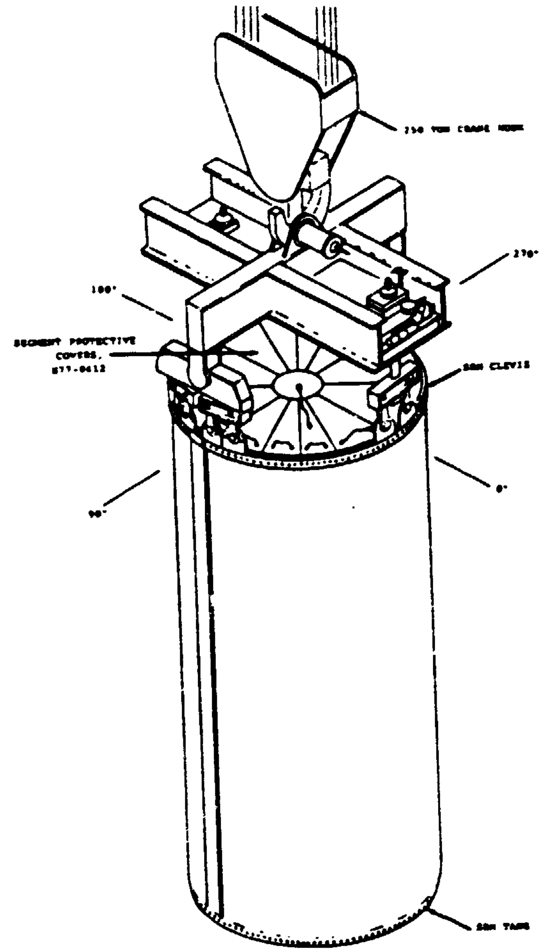
[Ref. 3/7-23 2 of 3]

SUMMARY OF KSC SEGMENT ASSEMBLY OPERATIONS
RH AFT (SRM-25) TO RH AFT CENTER (SRM-26)

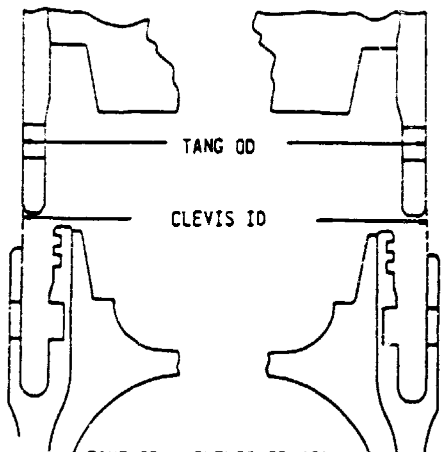
SEGMENT STACK SEQUENCE - 7 DEC 85

	ACTIVITY	TIME	REMARKS
O	POSITION RH AFT CENTER SEGMENT IN TRANSFER AISLE	2300	6 DEC 85
O	LIFTING BEAM CONNECTED	0100	--
O	JOINT PUTTY APPLICATION COMPLETE	0120	
O	FIRST ROUNDNESS MEASUREMENT - 4-POINT LIFT	0145	UNACCEPTABLE
O	FIRST ROUNDNESS MEASUREMENT - 2-POINT LIFT	0300	UNACCEPTABLE
O	SECOND ROUNDNESS MEASUREMENT - 2-POINT LIFT	0354	UNACCEPTABLE
O	THIRD ROUNDNESS MEASUREMENT - 2-POINT LIFT	0415	UNACCEPTABLE
O	COMPLETE RH AFT CENTER TANG INSPECTION	0500	NO PROBLEM REPORT
O	COMPLETE PR REPAIR, CASE TO INSULATION DEBOND - TANG	0500	PR-SR-RAC-050-0001
O	INSTALL JOINT ROUNDING TOOL TO TANG	0545	016° - 196° LOCATION
O	"O" RINGS INSTALLED	0910	
O	CLOSEOUT PHOTOS TAKEN	0915	
O	REMOVE TOOL AND TAKE ROUNDNESS MEASUREMENT	0935	ACCEPTABLE
O	TANG ENGAGED	1015	
O	FIRST PIN INSTALLED	1020	
O	LAST PIN INSTALLED	1030	
O	LEAK CHECK COMPLETED	1220	0.0 PSI DECAY

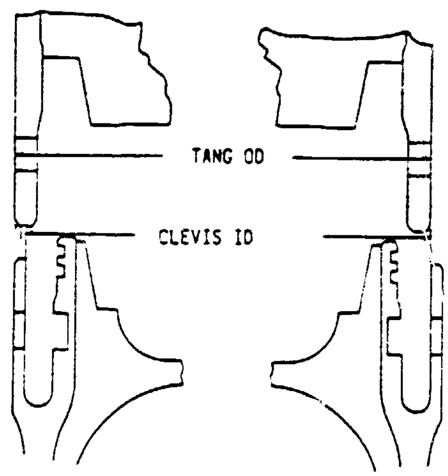
[Ref. 3 7-23 3 of 3]



[Ref. 3/7-24]



TANG OD = CLEVIS ID (0)



[Ref. 3/7-25]

[Ref. 3/7-26]

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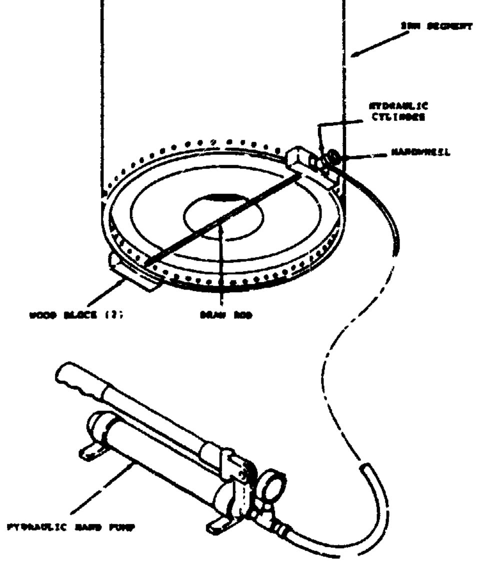
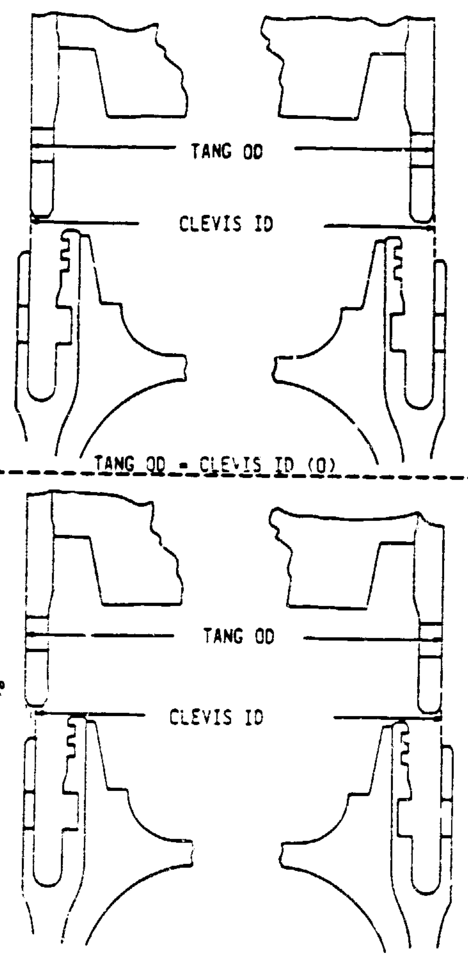


SRB CIRCUMFERENCE ALIGNMENT TOOL,
MODEL NUMBER A77-1[illegible]58

[Ref. 3/7-27]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CLEVIS ID
TANG OD = CLEVIS ID (0)
TANG OD
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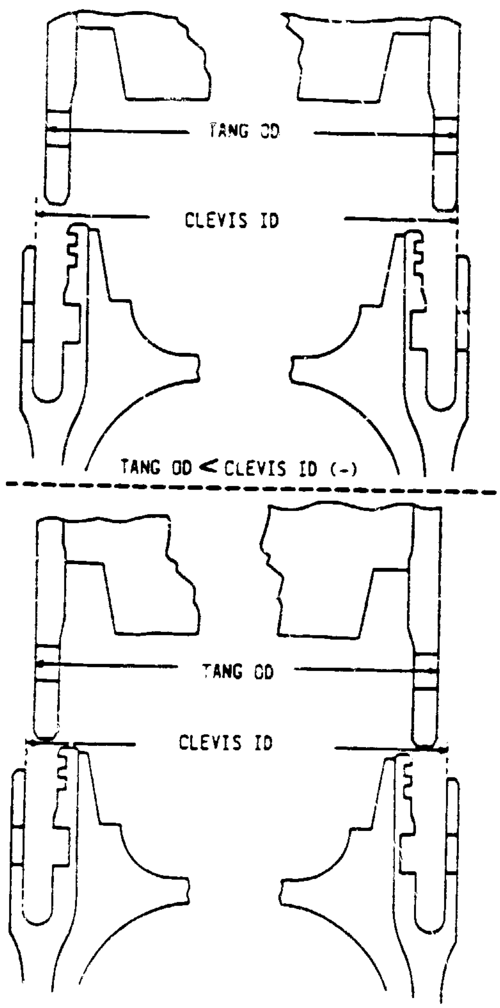
[Ref. 3/7 28]



[Ref. 3/7-29]

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TESTIMONY OF: THOMAS MOSER, DEPUTY ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT, NASA; DR. JERROL WAYNE LITTLES, ASSOCIATE DIRECTOR FOR ENGINEERING, MARSHALL SPACE FLIGHT CENTER; AND JACK LEE, DEPUTY DIRECTOR, MSFC

CHAIRMAN ROGERS: Mr. Moser, will you go ahead and identify each of you and your present assignments.

MR. MOSER: Okay, Mr. Chairman, members of the Commission.

I'm Tom Moser. I'm reporting today in the capacity as the head of the failure scenario team from the Johnson Space Center. I have held that position until just recently at the same time I was director of engineering at JSC.

I began my experience with the space shuttle in 1969, and most of that time I served in capacities related to strength integrity of the orbiter and as deputy project manager of the orbiter itself.

DR. LITTLES: Mr. Chairman and members of the Commission:

My name is Wayne Littles. I am associate director of engineering at Marshall Space Flight Center, and I've been at Marshall for 18 years, and I've held my

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current position for two years. And prior to that, I was deputy to the same position, and prior to that, I served as the chief of the engineering analysis division in S and E.

My education is in mechanical engineering, and I have a bachelor's degree from Georgia Tech, a master's degree from the University of Southern California, and a Ph.D. from the University of Texas.

MR. LEE: Mr. Chairman, members of the Commission:

I'm Jack Lee, deputy director of the Marshall Space Flight Center. I have held that position since 1980. I have been with the Marshall Center since 1958, and since the date of the incident I have directed the analysis of failure evaluation effort for the Marshall elements for those elements for which we're responsible. That includes the external tank, the solid rocket booster, the shuttle main engine, and the inertial upper stage.

MR. MOSER: Mr. Chairman, if it pleases the Commission, the next five speakers will proceed through a failure scenario beginning at the top level with the evidence as we see from the accident, and then into the top level fault trees, and then down into the detailed failure scenarios of each one of the possible causes.

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And so I will begin that.

And to facilitate this, I will present an abbreviated time line this morning. I will provide to the Commission, if you so desire, a detailed four-page time line. But for the sake of presentation and clarity, I have abbreviated that for your presentation today.

CHAIRMAN ROGERS: I was going to ask, could you just as a preliminary matter explain the terms first? What is a failure scenario? What do you mean by failure scenario? I think I know, but it would be helpful.

MR. MOSER: And you will see that in one of our presentations here. Basically, the fault tree that you will see on the presentation—

CHAIRMAN ROGERS: First you used the term "failure scenario." What is that?

MR. MOSER: Failure scenario is a detailed analyses of a fault tree as to why the various elements of the failure events could have taken place. It is using a model of a failure, a theory for a failure, and then the analysis to substantiate or to deny that each element of the failure model is in fact correct.

CHAIRMAN ROGERS: Am I correct that a failure scenario is a hypothesis of something that might have

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happened, and then you consider each aspect of that to see whether it in fact did happen?

MR. MOSER: Yes, sir.

CHAIRMAN ROGERS: And then you draw conclusions from that?

MR. MOSER: Yes, sir, and we verify or deny or disprove each one of the steps of that. And today as we proceed through this, we will show you that some of the steps in the failure scenarios indicate that it is not a viable cause, and I think that will become very clear as we proceed through.

CHAIRMAN ROGERS: When you get to a fault tree, would you explain what that is, too, so the listeners understand what it is?

MR. MOSER: Okay, sir.

If I could have the first chart, please, M-1.

(Viewgraph M-1.) [Ref. 3/7-30]

This is—what I've shown here on M-1 is an outline of the investigation process, beginning with the incident. The description of the incident is captured by the evidence from the flight data, the physical data, and the photographic data. It is factual as best we know it, and I will present that to you today, along with the time line. That is the given for the problem, if you will.

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The next level is what could have caused this accident, and that is the—please leave M-1 up, please, on the screen. If you could go back to M-1, please.

The second block per se is the fault tree development. It is what could have gone wrong and caused the accident. It could have been the orbiter, it could have been the external tank, it could have been any number of things which caused that failure. It could have been the weather conditions.

Those, it is the anomaly and the vehicle for the failures which constitute the fault tree. From those possible fault tree elements, we then establish and gather data to either substantiate or deny any one of the branches of that fault tree, and we will have a detailed fault tree for you for each one of these things by the other presenters.

To support that, in the middle of that investigation process, the incident data bases are derived, both from the evidence which has been measured, which we document as given up in the first block, and also there is data which is derived by test and analysis, looking at records, the type of things which you gentlemen—the gentleman just spoke to you on on processing the hardware and the checkout. This is what

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is then used to verify or deny parts of the failure process and analysis.

All of that then, with the details of the failure analysis which postulates the sequences, the causes, and the special tests and analysis which is conducted, which we will talk to you about in detail, are then fed back and the entire loop is iterated back and forth until we finally decide on those events which are still possible or probable and work those until we can either prove or disprove them, hoping to come out with conclusions as to what the causes were or findings.

And today what we're going to present to you are some findings, and I think some of these elements, we are going to show that there is a very, very low probability that they contributed, in all probability they did not, they are not probable causes.

Others, we're going to report to you on the status of our analysis and investigation.

If I could have the next chart, please.

(Viewgraph M-2.) [Ref. 3/7-31]

This is the beginning of the abbreviated. If you can scan in on the left-hand side of that chart if you will, please.

The time line that I have shown here begins

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before T-zero. It begins with the SSME start command. As I stated earlier, I have abbreviated the time lines and the events just for the sake of clarity, and the start of this event in this time line is for the launch itself.

As we develop possible failures which were caused by any one of the elements that require that the time line begin earlier than this, during the checkout and processing of any one of the elements, that is shown with those particular scenarios.

I will now present to you evidence as we know today to be factual—it is not derived data that I am going to show—that goes with each one of the events of the time line. And so this is a given to any one of the detailed failure scenarios that you will see.

Starting with the SSME startup, that is the main engine startup, that begins at approximately 6.6 seconds before T-zero, which is the ignition signal to the solid rocket booster motor. Subsequent to that, at about one-half a second, .531, we saw—and if I could have chart M-3, please.

(Photograph M-3.) [Ref. 3/7-32]

We saw a puff of smoke at the aft field joint of the solid rocket booster motor. We have deduced from photographic evidence from multiple camera positions that

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the puff of smoke is coming, originating from between the solid rocket booster and the external tank. It is in the quadrant that is to the right of the picture as you see it there and on the far side of the solid rocket booster.

That smoke was evident from .5 seconds up until a little less than four seconds. There are some photographic observations that indicate that that smoke and gases can be seen as late as 12 seconds. That is still under investigation by enhanced photography.

CHAIRMAN ROGERS: At what point in the time line do you think the smoke occurred?

MR. MOSER: It occurred at .531 seconds after ignition signal to the solid rocket booster.

CHAIRMAN ROGERS: About a half a second after liftoff?

MR. MOSER: Yes, sir.

CHAIRMAN ROGERS: And how closely have you identified the source of the smoke?

MR. MOSER: What we have done is we have identified it to be originating in the quadrant of the solid rocket booster, to the right-hand side of the solid SRM as you see it there, and between the SRM and the external tank.

We have not pinpointed from our enhanced

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photography the exact location of the source of the smoke yet, sir.

CHAIRMAN ROGERS: How close are you to doing that?

MR. MOSER: I can't answer that right now. We thought we were about there about ten days ago, and that photography enhancement has not panned out and proven that yet, sir.

One of the cameras that had a better view of that was non-functional at the time of launch. So we are having to use some other techniques, and I don't know that we will be able to, with the data that we have, to pinpoint exactly what the source is.

We have concluded, however, from the photographic data that it is not coming from the extreme, in this photograph, from the extreme right-hand side of the solid rocket booster, which is where the check port, the test port, is located.

DR. RIDE: Can you put limits on the circumferential arc?

MR. MOSER: No, only to say that it is within that quadrant, Sally.

DR. RIDE: Between about 300 degrees and 210, is that the quadrant?

MR. MOSER: From this photograph, we can't get

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it very close. From one of the other photographs that we have during the ascent we can get closer, and I will show you that here in a moment. But that is a different event.

CHAIRMAN ROGERS: It must have been an earlier photograph, though, than this, because the smoke is pretty far up on this photograph.

MR. MOSER: There could have been one, yes, sir. I have chosen the sequence of photographs to best describe it. There is one—we have some photographs that are earlier than this and that are later than this, but this is just indicative of that.

We can provide you with all of the photographs of every frame of this.

CHAIRMAN ROGERS: I think Mr. Stevenson gave us photographs in Washington which were prior to this one, which showed with a little more delineation exactly where the smoke initially came from. Am I correct in that?

MR. MOSER: The photographs that I remember seeing were about from this same angle, the same camera, as a matter of fact, I was just advised. Yes, sir, the same camera.

DR. COVERT: Mr. Moser, where vertically does the smoke first come out? You talked about the angular

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position. How about the longitudinal position?

MR. MOSER: It comes out from, in the proximity of the aft field joint.

DR. COVERT: And that's close also to that ring there that we can see there?

MR. MOSER: Pardon me?

DR. COVERT: We can see a strap going around with some cork insulation or something under it, and that is reasonably close?

MR. MOSER: Yes, sir.

DR. COVERT: And that is also reasonably close to where the external tank attaches to the solid rocket motor.

MR. MOSER: That is correct, sir.

DR. COVERT: Thank you.

DR. FEYNMAN: Isn't it, so is this the black smoke you're talking about at .5 seconds? Isn't it true that in even the first few frames immediately after the ignition that you can see some white smoke earlier?

MR. MOSER: From comparing the color of that smoke to this smoke, it appears to be a lighter color, yes, sir. I think there is some disagreement as to what the color actually is, whether it is less dense or whether it is the reflections.

DR. FEYNMAN: I'm not worried about the color,

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but the time. It seemed to me that it was an extremely early incident in the pictures that could see that.

MR. MOSER: I don't believe it would be before a half a second, though, sir. That is the first visible evidence that we see.

CHAIRMAN ROGERS: Which is the first visible evidence? How would you describe, based on the first photograph that you've seen, the first visible evidence was what? Was it white smoke or black smoke?

MR. MOSER: It was a lighter colored smoke, and I don't want to say it's white. It was not white like the SRB is white, but a greyish colored smoke. That is the way it appears in the photograph.

CHAIRMAN ROGERS: And that preceded what appears to be blacker smoke?

MR. MOSER: Yes, sir, that is correct. It appeared to be—it appeared to be continuous from, certainly from a half a second to about 3.375 seconds it was continuous. After that, it is very difficult to interpret the photographs as to whether or not there is a continuous stream of anything coming out there. In some viewers' eyes and based upon the photographs that we have today, it appears to last as long as 12 seconds. But that is not conclusive at this time.

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What I'm trying to do to set the stage here for some of the other detailed work is to tell you what we know to date with a lot of confidence.

CHAIRMAN ROGERS: Well, just to conclude the thought as far as I'm concerned, would you say that in your observation of the photographs the first thing you noticed was smoke that appeared to be white, and right about the same time some black smoke?

MR. MOSER: After that—first of all, it is a light colored smoke, and then it gets darker, as you see it here, and then it begins to dissipate and leave, especially as the flow around the vehicle begins and the vehicle begins moving. And so it begins light and then it gets darker.

CHAIRMAN ROGERS: But would you conclude it all came from the same area?

MR. MOSER: Yes, sir. It appears to all come from the same area.

CHAIRMAN ROGERS: Okay. Thanks.

MR. WALKER: Mr. Moser, have you verified that this phenomenon was not observed on any previous launch?

MR. MOSER: I'm sorry?

MR. WALKER: Have you verified that this phenomenon was not observed on any previous launch?

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MR. MOSER: We have—I have, to my knowledge, we have not seen that event on any previous launch. There was a report at one time that they thought that an anomaly was seen after an

SRB separation, but there has not been any signs of black smoke or any type of anomalous venting of the SRB's on previous launches.

MR. WALKER: Thank you.

MR. MOSER: Proceeding on in the time line, at about 20 seconds is where the solid rocket booster begins to decrease its thrust in getting ready for maximum dynamic pressure during ascent.

If I could have chart M-5, please.

(Viewgraph M-5.) [Ref. 3/7-33]

What I have shown here is some data we have acquired from telemetry, which is a presentation of the pressure, chamber pressure of the right-hand solid rocket booster motor versus time. Everything appears to be nominal at the right-hand solid rocket booster as at about 20 something seconds, 21 seconds, when it begins to decrease the chamber pressure. As it goes into the maximum dynamic pressure, everything is nominal essentially during that time.

If I could have chart M-6, please.

(Viewgraph M-6.) [Ref. 3/7-34]

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This is again going back to the time line. Everything is proceeding well in the time line. It goes through the end of the thrust bucket, where it experiences minimum chamber pressure in both left and right-hand solid rocket boosters, and then the pressure begins to increase again, as was shown on the previous chart.

Shortly after that, about four seconds, we see our first evidence of the hot gases of the plume coming from the right-hand solid rocket booster. If I could have chart M-7, please.

(Slide M-7.) [Ref. 3/7-35]

In this photograph you can see the bright spot on the right-hand solid rocket booster, and what we have done to try and enhance our understanding of where things are occurring on the vehicle, we have generated computer-aided graphics of the entire launch vehicle and overlaid them on the photograph so they are to the same scale.

This has enabled us to pinpoint as accurately as we can, with the accuracy of the photographs and the fidelity of the photographs, what the source is from this photograph.

And if I could have M-8 up now, please—

(Slide M-8.) [Ref. 3/7-36]

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which is a computer-drawn version of that same view. We have isolated that bright spot to be again in the quadrant, as shown right here, on the right-hand solid rocket booster. And that is the first indication of a visual plume coming out of that booster.

This we have been able to—Dr. Ride asked if we could identify it specifically, and this we have isolated to be at 45 degrees on the circumference, plus or minus ten degrees; and in the longitudinal direction we have isolated it to be at the field joint, within plus or minus one foot.

And the reason we cannot get any closer than that is just because of the granularity of the photographs, and also because of the angle of the vehicle to the camera.

DR. RIDE: Where are you taking zero degrees?

MR. MOSER: Zero degrees is on the bottom of the solid rocket booster, so it would be just about where that arrow is on the Z axis.

DR. RIDE: Say again what the foot measurement was, what you were referring to? You've isolated it within the area of one foot square?

MR. MOSER: In the longitudinal direction, we have isolated the plume which you see in the previous

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photograph to be in the plus or minus one foot of the aft field joint. We cannot tell any more accurately than that from our photographic data; and circumferentially around the solid rocket booster motor, to be 45 degrees from the Z axis or, in this view, the bottom of the solid rocket booster motor.

We then proceeded into maximum dynamic pressure, and if I could have chart M-9 up, please.

(Viewgraph M-9.) [Ref. 3/7-37]

At 59 seconds we experienced maximum dynamic pressure on the vehicle, which was about 720 pounds per square foot of aerodynamic load. In comparison to other flights, this is less than some of our previous experience. It is well within the design nominal value of maximum dynamic pressure expected.

And the reason—I should point out also on the scale of time line, below the time in seconds shows the throttle profile for the main engines. At this time we had just gotten back up to 104 percent of our maximum thrust out of the main engines. We had been down as low as 65 percent, and that is to keep this maximum dynamic pressure to the levels that I just reported.

Within about a second after that, at a little over 60 seconds, we experienced a divergence in the solid rocket booster chamber pressure. And if you would

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display chart M-10, please.

(Viewgraph M-10.) [Ref. 3/7-38]

If you can zoom in just to the lower right-hand part of that. That's good. Thank you very much.

Here you can see a deviation in the chamber pressure, and that is the internal pressure of the solid rocket booster, to that of the left hand. This is—before this time they had been tracking well within our experience base of comparing pressures between these two motors.

So we are beginning to see evidence here that that plume is consistent with a decrease in the measured pressure.

The next thing that we see is evidence of the plume deflecting. And if I could have chart M-11, please.

(Photograph M-11.) [Ref. 3/7-39]

The plume has grown in size. It is now being deflected because it is impinging on the external tank or because it is being deflected by the aerodynamics. It appears to us now to be deflecting because of impingement of the external tank.

CHAIRMAN ROGERS: I couldn't quite hear the last part of your sentence. What did you say at the

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end?

MR. MOSER: It is impinging on the external tank.

Then at 64.6 seconds, there is, we see with the next chart, which would be—no, I'm sorry. Let me just tell you verbally what we do.

We see a change in the possible LH-2 tank leak at that time, because the pressure—the pressure is decreasing. And that is on chart M-13.

(Photograph M-13.) [Ref. 3/7-40]

DR. COVERT: Did you say, Mr. Moser, that that time corresponded with the pressure dropped in the hydrogen part of the external tank?

MR. MOSER: I'm sorry? Repeat, please?

DR. COVERT: You said at the same time the plume changed character you had evidence that suggested that the pressure drop in the hydrogen tank or the pressure in the hydrogen tank was dropping; is my understanding correct?

MR. MOSER: Yes, sir. And I will show that to you. I'm sorry I got out of sequence here. I wanted to show you the visual evidence that we are seeing of change in the plume characteristic, which is thought to be from the leak, which is thought to be a leak from the hydrogen tank.

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On chart—in viewgraph M-13 is—what you see there, that is a plume characteristic. If you will go to M-15, please, and look at the top part of that plume, you'll see a change in character.

(Slide M-15.) [Ref. 3.7-41]

So that it is deflected more down, or appears to have more of a definite node at the top, as opposed to a rounded condition. That is occurring at the same time as M-17, which is the pressure profile. If you would put M-17 up, please.

(Viewgraph M-17.) [Ref. 3.7-42]

This is the pressure profile of the liquid hydrogen tank. This is where we see that the decrease in the rise rate of the ullage pressure, that is the gaseous pressure in the liquid hydrogen tank is changing over what it had been.

It had been cycling up to that time as the demand required, and the characteristics of those cycles had been consistent within our experience base. Here we see a change in that pressure rise rate which is indicating that something anomalous is going on, like a leak.

And so when I coupled this with the change in the characteristic of the plume coming from the right-hand solid rocket booster, that is two strong

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pieces of evidence that we do have a leak at that time in the liquid hydrogen tank. And I will show you some other supporting evidence to substantiate that.

CHAIRMAN ROGERS: At what time was that conclusion that there was a leak in the external tank?

MR. MOSER: That was at 64.64, and that is on chart M-16, at the lower right-hand side, Mr. Chairman.

(Viewgraph M-16.) [Ref. 3.7-43]

That is when we see the change in the plume characteristic, at 64.64. And about three-tenths of a second after that is when we can determine from the measured pressure data that we are seeing a change in the pressure rate.

The data rate of the pressure is about five samples per second.

CHAIRMAN ROGERS: If you can conclude at that time that there was a leak in the external tank, can you also conclude that up to that point there had not been a leak in the external tank?

MR. MOSER: With some degree of rough granularity. There could be a small leak, I think like eight-tenths of an inch in diameter.

DR. LITTLES: About four pounds per second of liquid hydrogen could have been leaking without detection.

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CHAIRMAN ROGERS: During the whole 60 seconds?

DR. LITTLES: Yes, sir.

CHAIRMAN ROGERS: And what would be the size of that leak?

DR. LITTLES: It would be about a .8 inch diameter hole. I will discuss that in some of these scenarios that I will present.

CHAIRMAN ROGERS: Fine. Thank you.

MR. MOSER: I would like to add—or to tell the Commission that we will provide them with color photographs of all of the copies that you have in there today. Logistics kept us from doing that, so that will be provided to you today.

DR. FEYNMAN: I got confused in interpreting your charts and in what you said. You saw the changes in plume shape at 64.7 or so, and you see the pressure decrease in the rate of rise at 66.7. So it must be about two seconds between, is that right?

MR. MOSER: Yes, sir. I'm sorry, I did state that incorrectly, yes, sir. Thank you.

The next chart is M-18, please.

(Viewgraph M-18.) [Ref. 3/7-44]

Then at a little greater than 72 seconds into the flight is where we see motion of the right-hand solid rocket booster to the rest of the launch vehicle,

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and that is shown on chart M-19, where here I display the rotation—

(Viewgraph M-19.) [Ref. 3/7-45]

—of the pitch of the right-hand solid rocket booster to that of the left-hand.

Not shown on this data is the fact that the left-hand solid rocket booster rate gyro is tracking exactly with that of the orbiter, and that is the way all three of the elements or all four elements—the orbiter, the external tank, and the two SRB's—have been tracking up until this point.

It is 72.201 seconds, we see a deviation from the right-hand solid rocket booster. It is our indication that something has failed in the aft attachment of the solid rocket booster to the external tank, and I will show you more of why we have concluded that.

If I could have chart M-20, please.

(Viewgraph M-20.) [Ref. 3/7-46]

This is a computer-drawn picture of the launch vehicle looking down on top with the solid rocket booster released from its lower link. The evidence that we have is that we have lost the load pad at that link. If that results, then the right-hand solid rocket booster then is free to pivot about its forward

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attachment point and one of the remaining aft attachment points.

This is consistent with maintaining a data source from the solid rocket boosters, because the integrity of everything going on in the solid rocket boosters, the data flows through the top aft link. What is hypothesized here, and is supported by the analysis, is that the lower left-hand or the lower link has failed, the solid rocket booster has both rolled about that new hinge line so it has a new pitch and yaw attitude.

That is what we measure from the flight data. When it does that, it impacts the inner tank region, as shown here on this drawing, between the LOX tank and the hydrogen tank there. It impacts it just at the lower portion of the frustum of the right-hand cone of the solid rocket booster.

If I could have the next chart, please.

(Viewgraph M-21.) [Ref. 3/7-47]

In a different view, we see that the SRB has moved up toward the orbiter at the aft end. And the next view, please.

(Viewgraph M-22.) [Ref. 3/7-48]

This is a view which looks at that same configuration from the forward end, and here you can get

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a better feel for how it has rotated about its new hinge line. This impacts the tank, as I said, causing the tank to load up, rupturing the forward LOX tank, the hydrogen tank, and at the same time probably causing the aft bulkhead of the hydrogen tank to rupture.

DR. FEYNMAN: In order to determine the motion of this thing, of the right-hand booster, you have gyros that determine its orientation?

MR. MOSER: Yes, sir.

DR. FEYNMAN: Do you also have inertia measurement to tell whether it moves forward or back?

MR. MOSER: No, sir, just the rate gyro, sir.

DR. FEYNMAN: You don't have any inertia measurement?

MR. MOSER: No, sir.

DR. FEYNMAN: So there's no way to determine the absolute position except to guess that the upper support hadn't slipped yet, is that right? That is how you did that?

MR. MOSER: That is correct, sir, yes.

And then that is part of the continuing photo analysis, too, is to verify that it is in fact still attached there. We did not see any other motion, and I don't know that it is a sufficient solution to look at the rate of change of both pitch and yaw, given that the

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fixed geometry, okay, of rotating about those points, all of that data supports itself.

And then looking at the times at which the SRB rotating would have bottomed out and induced high loads in the tanks, is when we see changes in the pressure and also see physical evidence, visual evidence I might add, from the tanks, where they are beginning to lose liquid hydrogen and liquid oxygen.

And so we have about three pieces of data which supports that.

MR. RUMMEL: The aft rupture in the ET is after the explosion, due to explosive force? On what do you postulate the cause to be?

MR. MOSER: I'm sorry, Mr. Rummel. Could you repeat that, please?

MR. RUMMEL: I think you mentioned that after the LOX tank and the hydrogen tank and the inter-tank area had been damaged, that was followed by a separation in the aft end of the hydrogen tank. Did I understand that correctly?

MR. MOSER: Yes, sir. Let me verify that. We first see that, the spillage of the aft dome of the liquid hydrogen tank, at 73.137 seconds. We see—that is visually, and I think I'm going to show you a picture of that in just a moment.

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MR. RUMMEL: Well, my question—perhaps you're coming to it—is the cause of the aft rupture. It appears that the SRB didn't hit the tank in that area. Was this due to overstressing from the rupture forward?

MR. MOSER: Yes, sir. The aft attachment is connected, the remaining aft attachment about which it is rotating, is connected right at the seam of the aft bulkhead to the cylindrical portion of the tank. And as soon as it rotates over and interferes with that region, then it loads it up in an out-of-plane load for the tank, and so it should rip the tank right in that region.

Plus, the solid rocket booster is rotating about 40 degrees per second at that time, and so it fits with the analysis that we have done that says that it should have in fact tore the tank in that region.

MR. RUMMEL: So you're postulating the failing of that part of the attach fitting that is attached to the ET at that point in time?

MR. MOSER: That is correct, sir.

MR. RUMMEL: Thank you.

CHAIRMAN ROGERS: Mr. Moser, yesterday we looked at the debris and the right frustum is badly damaged. The left one looks as though it's not damaged at all. The right one seems totally consistent with

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this photograph.

Have you seen that debris? In other words, the right frustum has damage which would be almost totally consistent with that photograph.

MR. MOSER: It was reported to me. I have not physically seen it myself, but it was reported to me what it appeared, and it does appear to be consistent with our failure model here, yes, sir.

DR. RIDE: Do you think that the contact between the SRB and the upper portion of the tank, the LO_2 tank, is what caused the LO_2 tank to rupture?

MR. MOSER: Yes.

If we could proceed on to the next chart, please, which would be M-23.

(Viewgraph M-23.) [Ref. 3/7-49]

Here, I have already described the time of events of when we see the ullage pressure drop. Go to M-24, please.

(Viewgraph M-24.) [Ref. 3/7-50]

It is—and I'm going to repeat myself somewhat here. At 72.564 seconds is the point you see here on the ullage pressure of the liquid hydrogen tank. It can now not keep up with the demands, with two flow control valves open.

Up until that time, the pressure had been

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decreasing, but it had been maintaining its pressure. Now it can no longer do that. And Dr. Ride, that is when we think that the forward—we have lost the integrity of the forward end of the hydrogen tank.

DR. FEYNMAN: Sir, I hate to interrupt you. While we have the chart up, I notice that the decrease coming down earlier—and we're talking about something earlier and so I'm interrupting—from 64 seconds, the decrease is slower than the previous decreases.

Can we interpret that in some way?

MR. MOSER: I don't think I can give you an adequate discussion of that. We had seen, I think if we go all the way back in the pressure profiles, that that was not uncharacteristic. But I will verify that for you.

DR. FEYNMAN: Thank you. I'm sorry to interrupt you.

MR. MOSER: No, that's quite all right.

If you would go to chart M-25.

(Viewgraph M-25.) [Ref. 3/7-51]

Here we see the actuator motion from the right-hand SRB, and that is very simply the flight control system trying to respond to what the rate gyro from the SRB is telling it to do. And so that tells us our flight control system is still behaving properly at

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that time.

It is—

VICE CHAIRMAN ARMSTRONG: Is the rate gyro package a two-gyro package or three?

MR. MOSER: It is a three-gyro package—excuse me. Two axes, two gyros.

On chart M-26—

(Viewgraph M-26.) [Ref. 3/7-52]

—now that both the LOX and the hydrogen tanks have lost their pressure, we see the inlet pressure to the SSME's dropping, and that will be discussed more by Mr. Hopson on what that event means to the main engines.

(Viewgraph M-27.) [Ref. 3/7-53]

M-27 is the same type of data for the LOX pump.

If you would go to M-28, please, back to the time line.

(Viewgraph M-28.) [Ref. 3/7-54]

This is where I pick up, at 73.137 seconds, the vapors near the inner tank region, and that is shown on chart M-29.

(Slide M-29.) [Ref. 3/7-55]

It's going to be very difficult for you to see the vapors in the inner tank region. If you can scan up

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just a little bit on that, you can barely detect it. This photograph in the display here is not adequate, but that is our first indication that we can see vapors coming from the forward.

Also, at this same time there is spillage from the aft region beginning to initiate. Then in M-30—

(Slide M-30.) [Ref. 3/7-56]

—we can see the increase in the vapors coming from forward, and here you can see it along the side of the external tank, just a few tenths of a second later. And then M-31, please.

(Viewgraph M-31.) [Ref. 3/7-57]

Again, with the overlay from our computer-aided drawings, that is about the characteristic of the vapor coming from up forward.

M-32, please.

(Slide M-32.) [Ref. 3/7-58]

We now see a bright flash between the orbiter and the external tank, and what that is is an apparent—and we can't prove this conclusively, but it appears to be now that that is a reaction or a burning of the liquid hydrogen and liquid oxygen. They have now mixed sufficiently in that region as they flow back to flash. Up until that point, it had just been vapors.

And then the next chart, please, M-33.

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(Slide M-33.) [Ref. 3/7-59]

And that same type of thing propagates forward, and continuing to burn.

The next chart, please.

(Viewgraph M-34.) [Ref. 3/7-60]

That is just a highlight of where this rupture initiated with the LOX tank.

M-35, please.

(Viewgraph M-35.) [Ref. 3/7-61]

What I have done with the time line, as best I can, is to sort out anything—we try to have it as factual and true to feed into the other failure model analyses. We have categorized or—

GENERAL KUTYNA: Mr. Moser, before you start on that I wonder if I could pursue one point on the winds and air turbulence. We saw down at Marshall that the flame appeared on the side of the solid rocket at 58 seconds, and we had our max aerodynamic pressure at 60 seconds, so that was after the flame.

However, I think it is important, as you told us before, that it was quite a bumpy ride prior to that time. There were air currents or wind changes that caused the flight controls to react considerably more than we had seen in the past. While it was not out of limits, there was more activity than you had seen.

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Could you characterize that for us?

MR. MOSER: Yes, I will do that. I do not have a detailed discussion for you today, but as the plume emanated from the solid rocket booster we have gone back through simulations and analysis to try and correlate the vehicle response.

GENERAL KUTYNA: No, prior to the plume, as you're going through.

MR. MOSER: Prior to the plume?

GENERAL KUTYNA: Yes. Could you characterize that? You said the nozzles were working harder than they had before.

MR. MOSER: That region, we have reconstructed all of the loads and the dynamic response of the vehicle from liftoff up until that point. The max dynamic pressure region we have not completed yet. I will give you my view of where it is at this time.

Everything appears to be okay, but we have not recreated all of the trajectory parameters.

GENERAL KUTYNA: Jack, do you know what I'm driving at?

MR. LEE: Yes, General, I believe I do. I believe we reported at that time that, even though—well, you characterized it as a rough ride, with some wind shears. They were within our flight data base.

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Now, what Mr. Moser is referring to is the Max Q region. We do not have that completely reconstructed yet to be able to relate all the vehicle activities to that.

I would like to point out, General, that even though you mention Max Q at 60 seconds, Max Q really is kind of a region, say from 58 or so. It is not specifically a point. So we do relate the first evidence of hot gas emanating from the solid rocket motor as being in the region of Max Q for our analytical purposes.

GENERAL KUTYNA: But the point I was trying to make, and see if you agree with me, there was quite some load, although not out of limits, but there was some load on that vehicle prior to Max Q, from 40 to 50 seconds, that might have given it some stresses. Not out of limits again, but it could have given it some stresses that could have caused something.

MR. LEE: Yes, sir. At about 40 seconds we did see some wind shears that gave us about a two degree rate, which is not out of our data base again, but it is not exactly nominal.

VICE CHAIRMAN ARMSTRONG: Would you like to comment on the TVC limit cycle in that time?

MR. LEE: As it relates to that, the 40

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seconds?

VICE CHAIRMAN ARMSTRONG: Yes, when the limit cycle reached.

MR. LEE: Can I refer that to Dr. Littles? Do you know that?

DR. LITTLES: I don't believe that the limit cycling at that point in time was really anomalous. I have heard it referred to as we were working them hard and that kind of thing, but I think that is a qualitative assessment.

I think the data we were getting was within the experience base. It is true that at Max Q we were seeing the loads that those may have had an impact, for instance, on the joint if we had something anomalous in the joint already, is what we believe.

The loads don't look to be near design limits at any point in time during there. We do still have some work to do, but the work we have done to date doesn't indicate that the loads are anomalous. We do, however, have more work to do looking at the 51-L loads, specifically as they apply to the joint for 51-L and as they might relate to some potential anomaly in the joint. So we do have some work to do there.

MR. MOSER: Thank you.

I think it is important for this, too, it

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really appears now that everything is within the design limits, but what we're trying to do is reconstruct from the actual data and the best estimated winds and trajectories and everything else what the load is. And even if it is 30 percent of the design load, we want to know exactly what it is, to see if it is in fact contributing to already weakened joint.

And so it will be—I think that that analysis will be completed, perhaps this week—I'm sorry, next week is I think the schedule we are on for that.

DR. FEYNMAN: We ought to say what "Q" is. It is the resistance, the force of resistance for moving through the air, or the combination of the density of the air and the speed squared, I think, that you move through?

MR. MOSER: Yes, sir.

DR. FEYNMAN: As it goes faster, it is increasing, but as you go higher the density is decreasing. So it reached a maximum and then falls off as we go into the vacuum of space further up.

MR. MOSER: Yes, sir, right.

DR. FEYNMAN: It's just worth explaining.

CHAIRMAN ROGERS: Does that complete your presentation?

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MR. MOSER: No, sir. On chart M-35, what I have done is I have categorized the anomalies as we know them, and that is our category A, and those are the events that I have just gone through associated with the time line.

There are other possible anomalies, conditions, that we also know of that can be contributors to this accident. For instance, the temperature or the water in the joints, the dimensions at stacking, and on and on.

Now, all of those things then are picked up by the specific element which that can effect its failure and whether it could have caused it, and so that is our category for possibles. And we will have another list of anomalous conditions, those which we have derived, and so we hope to put those in the appropriate basket.

On chart M-36—

(Viewgraph M-36.) [Ref. 3/7-62]

—this chart will serve to introduce the very top level of the failure analysis, starting with the explosion and then the external tank breakup, which we know, and then we want to get into unknowns of the causes.

The color code that you see here is green, a green indication along the analysis path, indicates that

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it is improbable.

Today we will talk to you about—if you will move that, your camera, over to the right, please—we will talk about the orbiter, the main engine, and the cargo, the IUS, as being improbable to have caused the accident.

To the left, the things which are still possible are the external tank; and probable is the solid rocket booster.

And I would now introduce Dr. Littles, who will talk about the failure analysis associated with the external tank.

CHAIRMAN ROGERS: Mr. Moser, before you do that, may I have the pictures that we are referring to? I think we referred to them as 35. Can we show that first?

And the reason I want to do this is because there is so much discussion about a white flame first, or white smoke before the black smoke, and there's been all kinds of articles written about it, and so forth. And the photographs really don't show it.

Can you see this?

(Slide.) [Ref. 3/7-62]

MR. MOSER: Yes, sir. That in contrast—the lighting changes here, too, I think, with the

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reflections and the intensity of the engines, and so that is one possible explanation for that

CHAIRMAN ROGERS: But we're all talking about the same smoke?

MR. MOSER: Yes, we're all talking about exactly the same thing.

CHAIRMAN ROGERS: That is what I wanted to get straight for the record, because there has been a lot of discussion about the white smoke and black smoke, as if there were two different accidents or incidents. This suggests there's just one

DR. LITTLES: I think it is just one instance, but I think if you look at different cameras and with different shadings you can get different impressions of the color. There is a black and white camera that we have some photographs of, and if you look at that—or when I look at it, and I'm not a photographic expert, but when I look at it the first smoke that I see does look more white, and then later it gets dark.

So I think you get different impressions from different films.

CHAIRMAN ROGERS: Does everyone agree it's all the same smoke?

DR. LITTLES: I believe everyone agrees it is all the same smoke, yes.

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CHAIRMAN ROGERS: Thank you very much.

DR. LITTLES: Could I have L-1, please.

(Viewgraph L-1.) [Ref. 3/7-63]

This chart continues where Tom left off, and in his chart he had the external tank and the SRB in a circle, and so I am picking up at that point and carrying to the right a little more detail at this point the items that we have been investigating and some that we are continuing to investigate.

There are some things which we have categorized as improbable and shaded green, both in the external tank and the solid rocket booster. Would you come a little closer to the right top so I can see the blocks a little more clearly. Thank you very much. That's good.

Okay, coming down from the top, the damage at liftoff and going over to the right, the pad debris. Those we have shaded yellow at this point and I will discuss those later within other scenarios. I won't go any further with that this point.

Now, premature detonation of the lin. shaped charge has been ruled as being improbable. That is based upon physical data, the hardware that has been recovered. We have recovered portions of the linear shaped charge and they weren't detonated, and so we know

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that wasn't the cause of the explosion of the external tank.

A structural flaw, I will discuss also later that line, so I will leave that one for now.

Now, structural overload, we have touched on that in the last few minutes. We have looked at the loads. We have a complete reconstruction now of the liftoff loads, and we see nothing anomalous there. The loads are well below design.

We have done a lot of work already on the Max Q and the other phases of flight and, as Mr. Moser said, we haven't completed that, but the data that we already have indicates that that is not a probable cause. We are going to continue to do that reconstruction, and we should finish that within the next I think week or ten days and rule that one out.

DR. COVERT: Dr. Littles, when you say you have ruled it out or potentially ruled it out, do you mean as a single cause in and of itself, but initially there could be other events that took place so that that combined with these loads could in fact be a potential cause? Do I understand you correctly?

DR. LITTLES: Yes, sir, that's absolutely correct. As I mentioned a minute ago, we are still particularly interested in these specific loads for

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51-L, for instance as applied to the 51-L joint. And so there is continuing work in that area and will be even after we look at the final reconstruction of the loads. I will discuss that as I go through some of these other scenarios.

But we are doing some work in that area and we will continue to do that for awhile.

DR. COVERT: Thank you.

DR. LITTLES: We have lost our screen. I will continue from the chart, then.

All right, then. To the right of structural overload, the block is the thermal protection system on the external tank. We think that to be an improbable cause for initiating the failure. We have done analysis assuming that we have lost TPS in various places on the tank and through the flight regime that we had progressed before we had the problem and the failure beginning at 59 seconds.

We didn't see any areas that would be overheated to the point of having a failure. We also have photographic evidence and don't see anything there, and so we don't believe a loss of any part of the thermal protection system on the tank initiated the failure.

Coming down then to the solid rocket booster,

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the same comments that I made relative to the external tank apply to the structural overload and to the liftoff and flight loads. But again, as Dr. Covert points out, we are still interested in those loads relative to other potential failure modes.

Relative to the item on the bottom, the premature linear shaped charge detonation, we know of course that the range safety system operated properly when we gave—when the range gave the boosters destruct at roughly 109 seconds. And so we've ruled that out, or we think that is an improbable cause of failure.

Now, case membrane anomaly, to the bottom right, that is covered in one of the scenarios that I will address, and so I will cover that one later. And also, the SRM pressure integrity and the joint seal, those are the primary items in the scenarios that I will present, and so I'll cover those in the next few charts.

Going then to L-2—

(Viewgraph L-2.) [Ref. 3/7-64]

What I have on this chart is all of the scenarios that we are pursuing for the hot gas leak. I will step through these one at a time. The line that I will be pursuing is highlighted in red, so you can

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follow it through.

What I will do is describe for you the failure mechanism which is hypothesized for each one of these lines. I will mention to you the work that we are doing, the analysis and the tests that we have going on to assess that hypothesis, and then I will give you a summary of our findings to date. And if we think we can say it is not probable, I will indicate why.

Okay. The first one, then, as you see in this chart is the solid rocket motor inhibitor flaw. Could I have chart L-3, please.

(Viewgraph L-3.) [Ref. 3/7-65]

Okay. This is a schematic representation of the joint. I think you have seen this before. The failure which is hypothesized in this particular scenario is that we could have possibly had a flaw in the inhibitor.

Come in a little closer, please, just at the top. Move it up to the right, please.

Okay, you see the indication there of the molded inhibitor. If we had a flaw in that inhibitor—and it would have to be fairly large, something like an inch in diameter. If you had a flaw there and you started burning, getting hot gas through that flaw and igniting the propellant and had the propellant started

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burning back toward the case membrane, then you could create a situation where in the time frame near 60 seconds you could get a burn-through of the membrane.

In this particular scenario, as you see, it is not dealing with the joint itself on this leg. It is dealing with the potential of a case membrane burn-through someplace other than the joint.

We have done a number of things in looking at this. We have reviewed the build records and process papers for 51-L, the right aft segment inhibitor. We have reviewed our experience base with these inhibitors, and we have done some analysis to determine whether by analysis that failure mechanism is possible.

One thing that we know is that the molded inhibitor is laid up, it is eight plies of rubber which are vulcanized together. Now, having eight plies of rubber vulcanized together, it is not likely that you would have a flaw in eight pieces. So that is against having this particular scenario be true.

We have had no previous problem with this inhibitor. The problems in this area are not in our data base. The analysis of that, as I mentioned, indicates that in order to get the burn-through or the burn-back of the propellant and the burn to the insulation and burn through that insulation and do that

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in the time frame of 59 seconds would require about two and a half times as much heat transfer as you would expect to get at a maximum there.

So that doesn't make it look likely.

There are a couple of things that are positive. It could—as Tom mentioned, we saw a pressure change at 59 seconds in the motor. In order to have a failure somewhere inside the motor with anomalous propellant burning, you have to get it at a place where it won't allow the pressure to change in that motor. Normally, if you had a crack in the propellant which came from the middle down, you get a higher burn rate. You would see a higher pressure.

In this particular area, since you're burning back and beneath the propellant, you wouldn't see an increase in pressure. It would be very slight. It would be less than the bit change on pressure, and so that would support it.

But all of these things taken together—oh, another thing, too, of course, is that this would not be consistent with the smoke puff that we see initially. This would happen some time later in flight. And so, taking all of these things together, we don't think this is a probable scenario, and we have listed it as not probable.

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Could I have L-4, please.

(Viewgraph L-4.) [Ref. 3/7-66]

On this particular one, I'm going to discuss two routes of a scenario together because they are very similar. This one hypothesizes what was mentioned a minute ago, the small hydrogen leak which you could have and not detect it with instrumentation, and that, as we mentioned a minute ago, is four pounds per second.

If you had a leak of four pounds per second and it leaked and the hydrogen burned and impinged either on the area of the joint, the aft field joint, or on an area of the membrane and heated it up enough, you might generate a failure, and that is what this hypothesis is.

Okay. The failure mechanism again is you have to have either an undetected flaw in the structure of the tank or you would have to have debris or some foreign object strike the tank at or near liftoff and cause the hole to create the leak. And then, as I said, the hydrogen burns and impinges on some location of the solid rocket motor and overheats either the joint or the membrane to cause the failure.

I have mentioned that we have established the size of that hole that is within the limitations of the instrumentation. We have reviewed all of the tank build

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records and we are reviewing any potential for pad debris.

We have done analysis to determine what kind of heating we would get if we had burning hydrogen impinging either on the joint or the membrane. And we are also conducting tests to characterize hydrogen burning against the foam thermal protection system on the tank.

DR. FEYNMAN: Is this to explain also the black smoke?

DR. LITTLES: It could explain the black smoke, yes, because if you had the hydrogen ignite and burn on the TPS, you can get black smoke from smoldering TPS. So it could explain that if you had the hydrogen at ignition.

What you have to hypothesize is that actually, of course, the ice team went out, as has been reported, previously to the mission, about 20 minutes prior to launch and they didn't see anything that would indicate a leak. And I believe if there had been a leak there at that point in time, they would have seen some indication of it.

And so the leak somehow has to occur between that point in time and a half a second after ignition to be synonymous with the black smoke. So you're talking

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about debris or either some overload during the ignition transient. And as I will tell you in a minute, we've already talked about the load thing and we don't think that the load is it.

So it really boils down to a debris or something else causing the hole in the tank.

DR. FEYNMAN: Burning hydrogen making the smoke would not be visible as a flame or a light in the photographs?

DR. LITTLES: Well, it depends upon where it is. You know, there is a considerable area that we can't see between the external tank and the SRB, and so it could be burning in an area where we don't have photographic coverage, possibly.

The smoke that we see initially, it is indicated to be somewhere between zero degrees around toward the tank. It could be burning in an area where those cameras even can't see it, and it could be emanating from the back and coming around. It is hard to say.

CHAIRMAN ROGERS: On the hypothesis, though, don't you have to conclude that debris hit the external tank and caused the leak, and that then caused the—what, it would cause some damage to the right booster, and all of this within a half a second?

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DR. LITTLES: No, sir, not all of that within a half a second. If you have the hydrogen burning from the tank—

CHAIRMAN ROGERS: On that hypothesis, though, what would cause that? What do you think would cause the leak in the external tank at that point?

DR. LITTLES: Well, it would have to be debris. You would have to have debris or something from the outside, we think. It could of course be a flaw in the structure which is overloaded and leaks.

CHAIRMAN ROGERS: And you've looked at that and excluded it?

DR. LITTLES: We haven't completely excluded it, but it is becoming remote. We are going back through all X-rays and all build records. We have had at least three different people review all of the X-rays and so far we've found nothing in that area that is anomalous.

We haven't quite completed that yet, but it is—

CHAIRMAN ROGERS: Continuing with the hypothesis, though, about the debris, suppose there is debris there. When would that have hit the external tank and caused the leak?

DR. LITTLES: Well, in order to be consistent

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with the puff of smoke initially, it would have had to have been at that time or slightly before, some time before that time, to be consistent with the puff of smoke.

CHAIRMAN ROGERS: Okay. Is there any evidence at all of any debris that might have caused that?

DR. LITTLES: We haven't found anything yet. That is still being looked into, but we have no indications of anything anomalous there. That is still something—

CHAIRMAN ROGERS: But it is at least a possibility?

DR. LITTLES: It is a possibility, yes, sir. But it is not something that we have seen on photographs or the movies that we can point to. I wouldn't say personally that it is a strong possibility.

VICE CHAIRMAN ARMSTRONG: Dr. Littles, if there were a hydrogen leak that you had at T-zero or approximately and it was ignited immediately, how long do you think it would take for it to heat up some area, to make black smoke?

DR. LITTLES: We have done that analysis and I'm going to discuss that now. We started—this analysis is an iterative process. We started assuming that we had hydrogen burning with complete combustion

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somewhere on the tank and flowing down by the tank within the boundary layer and heating up the joint.

That type of heating on the joint did not give us enough heating of either the joint or the membrane to cause a problem. We then went to the assumption that we had a jet of hydrogen coming out of the tank and impinging directly on, over the distances there, impinging directly on the joint or the membrane.

DR. FEYNMAN: By "the membrane," do you mean the metal surface of the rocket?

DR. LITTLES: Yes, sir. It is a little thinner than the joint. If you move down just a few inches, it is a thinner cross-section.

DR. FEYNMAN: About how thick is it?

DR. LITTLES: I have that on a chart here. I never can remember the numbers. It is .479 inches, the membrane itself

If you make that assumption, if you assume that the hydrogen is leaking from the tank and impinging directly on the surface, and if you assume that you have complete combustion—that is, you have all of that hydrogen mixing well with air—you get a stoichiometric mixture and it burns.

You can get temperatures under those conditions that would create some problems, for

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instance, on the joint. You could have a temperature of approximately 900 degrees. If that happened for roughly 60 seconds, a 900 degree temperature on the joint would open the gap enough to unseat the O ring, and you could get leakage coming out of there.

During that same period of time, with the same assumptions, you might heat the membrane up to 1300 degrees. Now, those numbers, however, let me quickly add, are unreasonable from the standpoint that we assumed complete mixing to get that. We did that as a test case, to see if we could just put it to bed and forget it. These numbers say that we have to do more work, so what we're doing now is we're going back and doing some jet mixing, get a more reasonable combustion in there, and see what more reasonable numbers will do.

Now, we are also going to do a test. In fact, we have already started some testing where we take a hydrogen tank with a hole in its side, ignite the hydrogen as it flows out, impinge it on a surface with a thermocouple, and get some physical evidence. And so we are doing both of those things to try to put this one to bed, and that is the direction we are going at this point in time.

Okay, could I go to L-5, please.

(Viewgraph L-5.) [Ref. 3/7-67]

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The next six scenarios—I'm sorry. Five out of the next six scenarios all deal with the joint itself. Before I move into those, we have been looking very carefully at both the left and right-hand boosters for 51-L. We of course have three joints on each vehicle, and we have looked very carefully at similarities and differences between those joints, because of course we had one anomaly on one joint, we had none with the other five as far as we know.

And if you look at the temperatures on all those joints, you find that there really is not a lot of difference in the type of ranges that we're talking about.

Could you come a little closer to the center so that we can see the temperatures, please. What you see there is, on the right-hand booster the aft field joint, the temperatures around the booster circumferentially range from 28 degrees to 52, and that is about the same temperature that you had at the top.

Now, I'm quoting nominal numbers and these are—we have been through the reconstruction that was discussed earlier with the IR sensor and those have been played into the analysis, and these are nominal numbers that I'm quoting in both cases just for comparison purposes.

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On the left-hand boosters, you see roughly the same kind of temperatures, and so the temperatures were roughly the same for all of the joints.

In terms of the loads, the on-pad liftoff loads, where we saw the puff of smoke, the maximum load is not at the aft field joint. The maximum load is at the center joint. The maximum load, however, at Max Q is at the aft field joint, but at the liftoff case, we first saw the puff of smoke, the maximum load would be on the second joint.

If you will move to the right, please. There's a note on the right relative to joint rotation which we will discuss in some following scenarios, and the subject of a lot of discussion.

If you look at the joint rotation that you get on the various joints, the lowest joint rotation that you get is on the aft field joint, because the rotation is a strong function of pressure. As a matter of fact, the pressure is the major driver in that, and you have a lower pressure in the aft field joint than you do at the forward two.

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The pressure at the aft field joint is about 760 psi. By the time you get to the forward field joint it's about 900 psi. So the rotation is lower at the aft field joint.

Would you scan to the bottom and pick up the two notes. There are two things that we have discovered that are unique relative to this aft field joint on the right. One of them is, in a scenario that has been discussed somewhat this morning, we did have to use the rounding fixture which was described earlier to mate those segments.

And the note here that I have says this was the maximum reshaping to date. There is some new data which was mentioned to you this morning, which we just found out yesterday. When we made this note what we were referring to was the data that we had looked at at that point in time.

The first thing we did when we started looking at this data base—and we are continuing to do that—we picked all of the segments which had been subjected to this rounding tool, thinking those were probably the population of the worst data and this is the worst one out of that group where the rounding tool has been used.

And there are some others that we will have to look at, but it was anomalous in that data base, in that

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if you took the six where the rounding tool had been used, this one would have been at something like a three sigma extreme relative to the amount of rounding that had to be done on it. So it stood out from that standpoint.

We also have another thing that I will discuss, in that on this particular mating, the aft field joint, we have a photograph which indicates a suspect O-ring in the aft field joint, and I will discuss that later.

And so there are some similarities between the joints, the things like rotation going in the wrong direction, temperature is about the same.

Oh, one thing that I omitted to mention is that if you look at squeeze on the O-ring—and those numbers are on the chart. The squeeze on the O-rings on the right, on the joints on the right booster, are roughly the same, and they are a little lower on the left-hand booster, and so there is nothing there that stands out.

DR. FEYNMAN: Excuse me. About the squeeze, this is a single number, but as you go around the circumference I presume the squeeze must vary. The two pieces don't fit absolutely perfectly. There are stresses and strains.

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So the question is what was the maximum squeeze or the minimum squeeze? How does it vary as you go around? Can we say anything?

DR. LITTLES: Well, you are right. You're absolutely right. What we are quoting here is the minimum squeeze, and you get that minimum squeeze by assuming the maximum eccentricity of the joints and considering the actual dimensions, the measured dimensions of the tang and the clevis for this joint.

Now, you can have cases, I believe, where you can have probably metal to metal on one side, with almost complete squeeze of the O-ring, and then the other side it depends upon what those dimensions are. And so you're right, you're absolutely right, you could have a large variation around the circumference.

DR. FEYNMAN: And that can have an effect, because the thing is cold and it is squeezed very hard, and then due to stresses or with the pressure it changes the dimensions and leaves a little gap because it doesn't have the resilience on account of the lower temperature.

That is an interesting possibility, so that it is important to think about the variation of squeeze around the circumference.

DR. LITTLES: Yes, sir, that is correct. As a

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matter of fact, relative to resiliency, the maximum squeeze I'm sure is worst case. That is absolutely true.

So you have to consider both. But on the other hand, if you look at these numbers you can see that the maximum—the minimum is about the same, so the maximum is probably about the same as well. But you are correct.

Okay, could we move on now to L-6.

(Viewgraph L-6.) [Ref. 3 7-68]

Okay. L-6 is the scenario that deals with the topic that was discussed this morning and which was one of the notes on the prior chart, and that is the potential damage to the primary and/or the secondary O-ring at assembly.

Due to the out of round condition, the mating process has been well discussed this morning and so I won't go into that at all. And I've already mentioned that we've made the comparison with some other data and we find this one to be a little anomalous.

We are doing some additional things to evaluate this condition. We are going to do some tests to simulate the conditions, which I will discuss in a moment, that we had on this particular joint. We have a partial segment that we are going to simulate the right

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conditions and mate and de-mate with O-rings in there, and see if we can induce some damage.

We are also going to simulate defects in some O-rings to see if we can get past the leak check, because if you damage an O-ring during assembly—and this one did pass the leak test—

you have to have it in a position some way where you can pass the leak test and still get performance.

We are also requesting, and I understand that it is going to be done, that we de-stack STS 61-G. STS 61-G is one of those which has had the rounding fixture used in mating that joint. It is not as bad as this one was, but it is, I believe, the next worse that we have seen in use of the rounding tool.

And so we're going to request that one be carefully de-stacked, using very careful procedures, to see if we can see any damage there.

MR. SUTTER: Can I ask a question or maybe make a suggestion? I am still interested in the fact that you put a load on that one joint, and you make the note that that is one thing you're going to study, but that load is put on there and the pressure check is made in the assembly hall and then the unit is taken out and put onto the launching pad and it sits there for 28 days.

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And I know the metal isn't up to yield, but you've got the insulation and you've got the charge, and maybe it wants to go back to where it was, and what happens in those 28 days? And shouldn't this analysis be expanded to maybe look at the effects of what happens while it's sitting there in the weather for 28 days?

DR. LITTLES: Yes, sir. We have that exact thing going on. We are going to take—start with mating, with stacking the segments, and carrying it all the way through the flow and doing that total analysis. We have that in process.

CHAIRMAN ROGERS: Somewhere along the line I guess we should take a recess for lunch. Is this a good place to stop, or would you rather continue?

DR. LITTLES: It's your pleasure.

CHAIRMAN ROGERS: How much time do you have?

DR. LITTLES: It might be good to stop now. It might take another 10 or 15 minutes to finish this one.

CHAIRMAN ROGERS: Well, maybe we should have a recess.

And are you going to talk about the possibility of ice in the joints?

DR. LITTLES: Yes, sir. That is one of the scenarios I will discuss, yes, sir.

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MR. MOSER: Mr. Rogers, I think I didn't give you a crisp answer between the difference in a fault tree and a failure scenario. Let me see if I can do that just a little bit better.

As you continue, these fault trees are the things which could have gone wrong, and the failure scenario then is the process to determine if they did or didn't, what did or didn't, or why that did or didn't go wrong.

CHAIRMAN ROGERS: But then you gradually exclude different aspects of it?

MR. MOSER: Yes.

CHAIRMAN ROGERS: And I gather from reading this that you have, without being totally conclusive, you have generally eliminated the orbiter, the space shuttle main engine, and the cargo inertial upper stage?

MR. MOSER: We will present those facts to you and then present those to the task force, but that is about where we are, yes, sir.

CHAIRMAN ROGERS: Okay. Thank you.

We will come back at 2:00 o'clock.

(Whereupon, at 1:05 p.m., the Commission was recessed, to reconvene at 2:00 p.m. the same day.)

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AFTERNOON SESSION

(2:10 p.m.)

CHAIRMAN ROGERS: The Commission will come to order.

All right, Mr. Littles, would you proceed, please.

DR. LITTLES: We will proceed with chart L-7.

(Viewgraph L-7.) [Ref. 3/7-69]

We were beginning to discuss the out of roundness situation relative to a potential O-ring damage, and again the mating situation was discussed this morning so I won't go into that.

The chart on the screen depicts a number of things, but among those things the dimensions that we actually had on the 51-L right field joint, the first column of numbers. Could you zoom in on the bottom left, please, so we can read the numbers.

And these were discussed to some extent this morning. The column on the left indicates the initial measurements prior to the beginning of the rounding, and the column on the right the final measurements.

I talked to Bob a few minutes ago and I want to make sure that we are all together. I want to make a correction. He indicated that the dimension was .094 inches, I believe, at the end of rounding, and it was

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.094, but that was the dimension before the rounding tool was removed.

The dimension I have on the chart here, .216, is the dimension when the mating was actually made, and so they are both real numbers. It is just at different time frames.

As was discussed this morning, the critical dimension here is the maximum negative dimension, which is in the 120 to 300 degree area of .393 inches. If you will go now to chart L-8.

(Viewgraph L-8.) [Ref. 3/7-70]

We have tried to do, Dr. Feynman, something along the lines that you mentioned this morning, to get a feel for what these dimensions mean to us. This is a graphic illustration and it is, of course, not to scale, and what we have done is assume that the clevis is completely round and then take the dimensions that were recorded and move the tang to the outboard leg of the clevis and look at the resulting negative dimension.

And the negative dimension, again, is a potential interference between the surface where the O-rings are and the outboard leg of the tang. And what you see in doing that is that there is a maximum interference potentially of .209 inches.

Now, that is the worst case, of course,

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because what that assumes is that you have metal on metal on the outside, and there is a tolerance between the inboard leg of the tang and the adjacent clevis dimension of .184.

So if you subtract those two, you have the other side of that tolerance, which is .025. So depending upon how the tang and the clevis are centered, you can have an interference between .025 and .209. So that is of some concern to us.

There unfortunately is no requirement specified on that negative dimension. There is the guideline of the .25 on the positive dimension, but there really should be a tolerance on that side. But that does lead to the potential of having an O-ring damaged as you are mating it, and of course we are concerned about that and that is an active scenario.

I have mentioned that we are going to de-mate STS 61-G and look at that, and that we are also doing some work at Marshall with some clevis and tang segments, looking at interference mating and looking at what kind of O-ring damage we might get that might affe the seal and those kind of things.

So we are continuing with that.

DR. FEYNMAN: There's a small trivial point that is confusing me. I don't know how accurate these

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drawings are.

DR. LITTLES: They're not accurate at all.

DR. FEYNMAN: Because on the inside where the O-rings are the piece of metal in this drawing has a 0 degree corner, which does cutting much better than if it had a cut on it. And in the previous drawing you had a little cut off of there, and I just don't know.

Does anybody know whether that is a 90 degree piece of metal that comes in?

DR. LITTLES: You're talking about the top of the outboard leg of the clevis?

DR. FEYNMAN: Yes, because on the previous drawing it was indicated that it wasn't so sharp, the one that was on some other speaker earlier on. And I'm just curious.

DR. LITTLES: There is no chamfer there.

DR. FEYNMAN: That is why I mentioned that, because I was told that, by the people who work on it, they've seen this 90 degree sharp piece rubbing up against the curved part as it comes up. You see, as it goes down there is a ramp, so to speak, and they look at it and it worries them. But they haven't communicated it very well up into the system.

DR. LITTLES: Okay. Can we go to chart L-9, please.

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(Viewgraph L-9.) [Ref. 3/7-71]

We are now going into a leg of the scenario which has four branches, the beginning point for each one of those being a blow-by or leakage of the primary O-ring, and then we will address the four branches individually.

The first one is the secondary O-ring defect. That defect could come from a number of sources: some damage to the O-ring in manufacture, not caught in inspection, or something else. We have been looking at closeout photos from this stacking. We have been reviewing O-ring records. We have been doing special inspections on O-rings.

I will tell you what that has produced, and we are going to do tests to evaluate the performance of a simulated O-ring defect and how it might pass a leak test.

Could we go to chart 10, please.

(Slide L-10.) [Ref. 3/7-72]

There is a closeout photo which I think you've probably seen before for this aft field joint, which has an indication of a potential O-ring anomaly.

Could you go on to chart L-10, please, which is a closeup view of this.

(Slide L-11.) [Ref. 3/7-73]

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We have been analyzing this photograph in-house and we have had some experts from outside doing some work—

CHAIRMAN ROGERS: Before you do that, could you just explain, identify the photograph, when was it taken and how was it taken and so forth?

DR. LITTLES: It was taken prior to mating, after the O-ring was installed on the aft field joint for 51-L.

CHAIRMAN ROGERS: Do we know the date?

DR. LITTLES: No, sir. I don't know the date. I have heard it. but I don't have it. I can get that date for you. I don't have it in my mind.

CHAIRMAN ROGERS: But it was some time before the launch, though?

DR. LITTLES: Yes, sir. It was during the stacking.

CHAIRMAN ROGERS: Okay. Go ahead.

DR. LITTLES: This is a closeup view of the same area, and the feature of interest is the apparent increase in the distance between the edge of the O-ring and the top of the O-ring groove. If you look at that, it looks as though there may be an anomaly in the O-ring and some change in the dimension at that point.

The work is still proceeding and it hasn't

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been completed, but there have been two estimates of that made and they have both come out in the neighborhood of 15 mils. But we are still working on that.

CHAIRMAN ROGERS: What does that mean?

DR. LITTLES: 15 thousandths of an inch.

CHAIRMAN ROGERS: Is there a deficiency in it? Is it smaller?

DR. LITTLES: Yes, sir, a smaller dimension than it should be. The nominal dimension of the O-ring is .280 inches, and this would be—indicates a 15 thousandths reduction in that.

CHAIRMAN ROGERS: Because the last time we looked at this we were told, I think, that you were uncertain about whether it showed an anomaly or whether this was putty that was put on, that would make it meaningless, or grease, I guess it was.

DR. LITTLES: Grease has been one of the primary concerns in trying to interpret this data, because of shadows it produces and because of the apparent changes in thickness due to the grease

CHAIRMAN ROGERS: Well, have you decided now it was not grease that made the photograph look like this?

DR. LITTLES: I don't think we can say it is

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not grease that makes the photograph look the way it does relative to the streaks. That may be. The thing we are concentrating on is the gap distance between the edge of the groove and the O-ring.

CHAIRMAN ROGERS: In other words, you think there is an anomaly now. At that time you weren't sure; you didn't think there was.

DR. LITTLES: The experts at this point think there is, yes, sir. And it looks anomalous to me, but there is still work going on there and we're not ready to conclude yet. This is an interim report.

GENERAL KUTYNA: Have you looked at the other closeout photos of that ring?

DR. LITTLES: The other closeout photos, as I understand it, have been looked at and they don't see anything.

GENERAL KUTYNA: You might take a look at them. We looked at them a lot the other day and there were a lot of areas of darkness and separation, so that it is tough to tell.

DR. LITTLES: We will look at those again.

VICE CHAIRMAN ARMSTRONG: Have you definitely identified the location of this spot in the clock?

DR. LITTLES: Yes. This is located—the quadrant of interest relative to the puff and the plume

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is between zero and 270, zero being the Z axis and coming back toward the tank to 270. This indication is not in that quadrant. It is in the quadrant right above it.

It is still in a quadrant that would be hidden and be between the SRB and the tank. So it is in a location where you could have initiated a puff or a leak at that point and have it propagate around.

VICE CHAIRMAN ARMSTRONG: Between zero and 90?

DR. LITTLES: Between 270 and 180.

MR. ACHESON: For the record, does any such slight variation in the gap appear anywhere else on the circumference from the photograph?

DR. LITTLES: Well, we haven't seen it in the photographs we have looked at. General Kutyna indicates that he may have seen something that we should look further at, but we haven't seen it in the things we have looked at.

MR. ACHESON: What I'm getting at is, can you compare this with the remainder of the circumference to see whether—for example, if three or four more such seeming anomalies appeared in the photographs, you would wonder whether this is illusory or significant or what.

DR. LITTLES: Yes, sir, and we have not seen

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that. But we will go again and look at those photographs again.

And Jack has pointed out, in this joint we have looked all around it and we haven't seen it.

MR. LEE: In this particular photograph, we have gone as far as we can see on each side of both O-rings and we see no other such anomaly.

CHAIRMAN ROGERS: I'm not quite clear. If this is an anomaly, would it be related to the puff of smoke or is it another area?

DR. LITTLES: It could be related to the puff of smoke. As we've discussed, the exact origin of that puff of smoke is not pinned down. We believe it is somewhere between the plus Z axis—or the minus Z axis, excuse me, and around toward the tank, around the SRB.

And we can't see—we have the two camera locations: D-63, if this is the SRB, pointing in this direction; E-60 pointing in this direction. And if the tank is over here, we can't really see back in this area.

So it could possibly be that, the puff of smoke coming out somewhere in this area and coming around with time. It is possible.

CHAIRMAN ROGERS: When you say it is possible,

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but is it realistic to think it is? I mean, is there another area?

DR. LITTLES: Yes, sir, I think it is realistic to think that it could be. We're only talking now about being 90 degrees away. I will discuss later some results that we've gotten from some small subscale motor tests, where we see with simulated anomalies and O-rings and sealing surfaces, we see leaks starting at one place, stopping at that place, and then starting another place.

And so it is conceivable that you could have had a puff of smoke in a quadrant or in a location that would be 90 degrees around from where you finally had the burn-through. The small motor tests say that that is possible.

Also along the lines of potential O-ring defects, we have been inspecting O-rings and doing some special X-ray on O-rings. X-ray of O-rings is not something that is normally done in inspection. We found two things.

One is that in reviewing the records of the inspection for the O-rings which were on all of the joints on 51-L, on both right-hand and left-hand SRB, we found that there were seven out of the twelve O-rings which were not subjected to the same level of inspection

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as prior O-rings had been.

The situation that occurred was that there was an engineering change order made to transfer some inspection notes from a drawing to an inspection document. It was properly approved, but in the process of making the change to the document there were two steps that were inadvertently left off and not caught.

Now, whether those are significant or not we are not making a judgment at this time. But the two things that were omitted was that: one, there was a requirement for only having five joints in an O-ring, and that was omitted and that inspection was not made at Morton Thiokol by the Air Force.

And the other is—and this one is more interesting to me relative to that photograph—there is a requirement that you can only have a ten mil or ten thousandths of an inch offset where the scarf joint is made, and that inspection was omitted.

Now, that applies to seven of the twelve O-rings. The secondary O-ring in this aft field joint was one of those. Like I say, we don't know what to make of that at this time, but it is something we have learned.

GENERAL KUTYNA: We have been looking at that for about a week now and are closing out that particular

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item. I think there were three inspections that were dropped, as a matter of fact.

DR. LITTLES: I'm sorry?

GENERAL KUTYNA: There were three inspections that were dropped when that engineering drawing was changed by Morton Thiokol. And I guess our teams have been looking at that for about a week now.

The one thing we found last night, that those O-rings were inspected by someone at some time, either Thiokol or a sub at least once, and what was missing was the backup inspection by the government.

DR. LITTLES: It wasn't done by Thiokol, but it was done by a vendor.

MR. WALKER: Do you have a record of where the O-ring joints are relative to this particular position?

DR. LITTLES: No, sir

MR. WALKER: No record is made of where the joints are?

DR. LITTLES: No, sir. Relative to the inspection we've been doing and x-raying O-rings, this is something that is ongoing. Just recently, it started within the last few days, I guess four or five days ago.

We have found one O-ring that has four inclusions in it. The depth is about 25 thousandths and the other

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dimensions range from 35 thousandths to 60 thousandths. We don't know yet what the conclusion is. They're still doing chemical analysis on it.

Inclusions of this size probably wouldn't be any problem, but since we've found one we're going to continue to look and see if there might be something worse. So we are continuing inspection and test and photo analysis in this scenario.

Could we go now to chart 12, please.

(Viewgraph L-12.) [Ref. 3 7-71]

Okay. This is the scenario that you mentioned earlier. Mr. Rogers, relative to ice in the joint, we have been looking at that from two standpoints, the first being whether ice freezing in a joint—could you bring L-3 back up, please?

The first being whether ice could freeze in the joint between the tang and the clevis—

(Viewgraph L-3.) [Ref. 3 7-65]

—and exert enough force on it to cause an opening and an unseating of the O-ring; and the second aspect of that being whether you could have water between the tang and the clevis on the inboard side beneath the O-rings and freeze the water and have the water move up and unseat the secondary O-ring.

We have looked at both of those things and

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we've run some tests relative to the freezing and the potential gap opening to unseat the O-ring. The movements are very, very small. That doesn't appear to be a problem.

Relative to unseating of the O-ring by freezing of the ice, if you have a situation where you have ice freezing in the bottom of the clevis and drive a column of air above it, you cannot unseat the O-ring that way because you can only get about two psi on the O-ring.

However, if you had a situation where you had a column of grease above the water—and there is liberal grease applied to these tangs and clevises—so that you might have a situation where you had water and had grease above it up too near the secondary O-ring, then if you freeze the water and compress or remove the column of grease hydraulically, you might unseat the O-ring.

So that that one is a possibility, so we are continuing to look at this a bit. We're going to run some tests simulating that kind of condition and see what it does to potentially unseating the secondary O-ring or causing a leakage by the secondary O-ring. So this one is continuing.

CHAIRMAN ROGERS: Would a test be very

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conclusive in this instance?

DR. LITTLES: It would not be 100 percent conclusive, no, sir. We can draw conclusions for the test configuration that we set up, but we can't say for this particular hypothesis that you could get that. It is a possibility, is what we can say.

CHAIRMAN ROGERS: Are you familiar with the other occasion when there was ice found in the joint?

DR. LITTLES: Yes, sir.

CHAIRMAN ROGERS: What happened on that occasion?

DR. LITTLES: On that occasion, that was STS-9, and we were not aware of that until after the instant, when we started doing investigations. We didn't know that we had water in the joint, but there was water in that joint.

There was a significant amount of rain and they had occasion to de-stack and when they did water ran out of the pins when they pulled them out. And of course, we had seven inches of rain while 51-L was on the pad.

CHAIRMAN ROGERS: Which was about three times the normal rain, I understand.

DR. LITTLES: Yes, sir. And so there was ample opportunity for the water to get into the tang and clevis area. And then of course, with the cold weather

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and the freezing, the possibility certainly exists, if you could have the configuration that we have hypothesized, that you might unseat that secondary O-ring.

And with the amount of grease that is in that tang and clevis area, you don't have to stretch your imagination very far to get to the point where you could have water under grease.

CHAIRMAN ROGERS: So probably you won't be able to exclude that as a possibility?

DR. LITTLES: It will be difficult to exclude as a possibility, unless the tests we run, by putting cold grease up against the O-ring, maybe slightly unseating it, and then hitting it with the ignition transient pressure, if that will seal, that would tend to exclude it.

If that doesn't seal, then I don't think you will be able to prove either by test or analysis that you can't generate this condition. So it would remain a possibility.

CHAIRMAN ROGERS: That is likely to be an uncertain area, then, whether the rain and the weather might have caused ice in the seal, and thereby caused the accident.

DR. LITTLES: Yes, sir, unless we can prove by

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that test that that would not result in a situation that you would fail to seal.

Could we go now to chart 13, please.

(Viewgraph L-13.) [Ref. 3/7-75]

This scenario deals with a leak in the leak check port. One of the things that tends to put this to bed is, if everyone agrees, is that the initial leak did not come from the leak check port. There are a lot of people who think it did not. There have been a few people who thought it did.

We have had our experts off for the last few days working on that. I was told before I came that they are ready to report to us, and we will be taking that report as soon as we get back and try to draw that conclusion ourselves.

But in our mind at this point in time, it is still a viable scenario. We have done some analysis and tests on this, the hypothesis being that you get a leak through the leak check port at an early time, like a half a second, to generate the puff of smoke, and then you continue leaking through that leak check port until you damage the secondary O-ring or you erode the port enough to blow the port out and then start growing the leak there.

The analysis that we have done indicates that,

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with flow rates that are reasonable—and by reasonable what I mean is we have done some analysis and some tests to establish a range of flow rates that you might get if you had an O-ring, for instance, or had a leak check port, somebody left the O-ring off and installed it, you could get leakage.

We've established what that band of leakage is. And then assume that you have that leakage from zero up through 60 seconds. If you do that, what you can do is you can erode or you can damage the secondary O-ring.

You can heat it to the point where it would not maintain a seal any more. You can get it well above 1,000 degrees, and if you did that then you could blow by the secondary O-ring at about 60 seconds, and that could initiate the leak.

CHAIRMAN ROGERS: Why would the leak check port fail, because you hadn't put the plug back in?

DR. LITTLES: Well, if you didn't put the plug back in, certainly that would be a direct leak. What we are hypothesizing here is—to get the small leakages that we are talking about, what we are hypothesizing is that the leak check port, the plug was indeed installed, but there was some defect in it and it spilled out a small flow.

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GENERAL KUTYNA: I don't know anyone who thinks smoke came out of a leak port. Who do you know who thought smoke came out of a leak check port?

DR. LITTLES: Well, I for one, and I'm not an expert and that is why I asked my experts to go off and put together a story and come convince me, because when I look at the black and white photographs I, with an untrained eye admittedly, can see what I think is white smoke emanating from near that leak check port.

Now, they may be able to come convince me that's not true. They may well be able to do that.

GENERAL KUTYNA: Well, does "near" count or does it have to be directly from the port for your scenario?

DR. LITTLES: I think it has to come from the port for this scenario, yes, sir.

DR. FEYNMAN: Might I suggest that on a thing like this the idea of an expert and an untrained eye as compared to a trained eye is a myth. There is no training for this particular kind of observation about leak check ports on a particular booster.

DR. LITTLES: Yes, sir, I tend to agree with that. By "expert" I was referring to people that we have who routinely, every flight, look at all the photographs and become very competent in looking at

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these kind of deviants.

DR. FEYNMAN: When you get curious, you can do it yourself and make up your own mind.

DR. LITTLES: That is why I wanted someone to convince me. But we are still working on that one as well.

Could we go now to chart 14, please.

(Viewgraph L-14.) [Ref. 3/7-76]

Okay, this leg of the scenario deals with the situation which has been discussed a great deal, that being that we had a blow-by of the primary O-ring and that we had, with the cold temperatures, we had a situation on the O-ring where we might have had a delay in the actuation time and didn't get a seating.

We're doing a great deal of work on this one, and there is a great deal of work to do. We're doing our work primarily in three areas. We're doing some additional, or have done some additional, tests on basic resiliency of the O-ring, and those data tend to confirm the data that we had before.

It shows that, sure enough, there is an increase in the free-standing response time.

We have also done some additional tests with the O-ring at cold temperatures in a static situation, and by a static situation I mean we don't have joint

2125

rotation there, but with the O-ring in a proper groove and with the proper squeeze, as a matter of fact with a variation on the squeeze, and then hitting that O-ring with the ignition transient pressure to see if it would seal.

And we have quite a number of tests now where we can seal down to minus ten degrees. At minus 20 degrees, we start getting some leakage. And these data really are no more 100 percent conclusive than the resiliency data.

It is just another data point, but it does indicate that the O-ring is not so hard, not so brick-like that it won't seal. It will seal if you apply proper pressure to it and the joint is not rotated at that point in time to the point where you have a blow-by.

A key part of the work we're doing, of course, since neither one of these things are conclusive, what we have to do is combine all of the effects, the effects of resiliency, the effect of the pressure being imposed on the O-ring, with the deformation that results from that pressure to move it against the seat, as well as the effect of the gap opening due to the joint rotation.

In order to do that, we have designed and

2126

built what we are calling a dynamic test fixture, which attempts to incorporate all of those things. We have the design such that when we apply pressure that there is a nylon sleeve which opens and gives you the gap rotation simulation.

And we are also hitting it with the right pressure transient, and we are doing it as a function of temperature, at various temperatures. So that is an important test for us. The fixture is available now. We are beginning to do some checkout tests with it.

One of the things that concerns me about it is that the design of this fixture is a development effort in itself. It is difficult to get all of those features in one test rig.

CHAIRMAN ROGERS: If it isn't a perfect match, if the test really does not simulate what actually happened, you could come to a very tragic conclusion.

DR. LITTLES: Yes, sir, and that is why we're being very, very careful with that test rig. The first thing we are doing with it, or we're going to do with it, is to make sure by calibrating the gap opening that we are really tracking that.

And in addition to that, there is some precursor work that has to be done before we are absolutely confident of the gap opening that we should

2127

have in there. The data that have been used and quoted are maximum numbers. Those data were derived or they came from test data actually.

We have done a number of tests, hydroproof proof tests on segments where we have inserted, we have measured through the leak check port, because that was the only location available without damage to the hardware. We have measured through the leak check port and established a gap opening, and it is those data with that single data point on a segment that have established the test data to show what that gap opening is.

We're doing some additional work on that. We have a test fixture that is now in test. We have just started tests on it. It is a full-scale segment, of course. We have eight holes uniformly spaced around, so that we can get eight measurements.

We are beginning to get data on that, and the initial data I have seen indicates that the gap opening is not as much as the maximum number we have been using. And another thing that is very key, and this is very, very key in my mind, those cases are empty. They don't have propellant in them.

Now, you're certainly going to get an effect on the rate of gap opening if you have propellant in

2128

there or not, because the propellant is going to add stiffness. And we're talking about pressure applications. We're talking about—over a small time frame here, we're talking about milliseconds. The time frame of interest, as you are well aware, is between zero milliseconds and 300 milliseconds.

So we are doing some analysis on that. We have two models going, one at Morton Thiokol and one at Huntsville, and we should be beginning to get some results on that pretty quickly.

Could we go now to chart 15, please.

(Viewgraph L-15.) [Ref. 3 7-77]

Okay. This scenario deals with the load exceedance. We have discussed loads earlier. We have been, of course, looking at the liftoff and flight loads and Max Q loads in particular. And as we discussed this morning—

MR. ACHESON: Could I ask one question, before we leave the other chart? In the tests you're running, do you find any evidence of erratic resealing of apertures, gas leak apertures?

DR. LITTLES: Resealing? I'm not sure I follow your question.

MR. ACHESON: Are you using regular booster fuel for your tests?

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DR. LITTLES: No. The static tests we are running are done with cold gas.

GENERAL KUTYNA: But you have used some five-inch motors, right, with real fuel in them, to see what happens when you cut a gap?

DR. LITTLES: Yes.

GENERAL KUTYNA: Could you give us a rundown on the one that opened and closed itself? Are you familiar with that one, the number of seconds that it did or did not leak?

DR. LITTLES: Yes. As a matter of fact, I'm going to discuss that.

GENERAL KUTYNA: Okay, whenever you get to it.

DR. LITTLES: Now, back to the load situation again. As we discussed this morning, we have reconstructed the liftoff loads and relative to comparing those loads to the design case, they're well below that. And as I indicated, for the Max Q case the data we have indicates that we probably won't have any problem there compared to design.

So we have indicated on our chart that this is not a problem. But let me emphasize that this is relative to the comparison of the loads to the design case. The loads are still a very important part of

2130

scenarios that deal with potential damage or to O-rings or other anomalies in the joint.

They are particularly important relative to the concept of the leak, seal leak, which has been discussed very often. It could be that we had some anomaly in the O-ring, had some damage, and then the activity that we see around 40 seconds with the winds, while they are within the experience base, or the conditions that we see at Max Q, while they are within the experience base, might impose enough load on that joint to cause some problems with it.

So loads from that standpoint are still very much a part of our other scenarios. We don't believe, though, that basic loads relative to design are out of bed, and that is why we're putting that part of it in, but the loads stay open for the other things.

CHAIRMAN ROGERS: Going back for a moment to the subject we were talking about a moment ago on the tests, is there any outside group working with you on those tests, other than Thiokol and the people at Marshall?

DR. LITTLES: No, sir, we don't have anyone else doing those tests right now. It may well be that we should, and we will look into that.

CHAIRMAN ROGERS: It is certainly a suggestion

2131

you should consider if you're going to rely on those tests.

DR. LITTLES: We will certainly do that.

Could we go on now to chart 16, please.

(Viewgraph L-16.) [Ref. 3 7-78]

This scenario deals with a slightly different phenomenon. As you are aware, the joint has putty upstream of the O-rings. The understanding of this joint relative to the putty operation has always been that the putty does not hold the pressure off of the O-rings.

As a matter of fact, after the incident when we were developing scenarios I discussed this with the joint experts and I was assured that the putty will not hold the pressure off the O-ring. But we did make a scenario out of it and we started doing some tests of the putty, and our initial data—and these are not conclusive yet, and I will tell you in a minute why we don't consider them finally conclusive, but the data does indicate that the putty in the joint configuration could hold the pressure off of the O-ring for a long enough period of time for the joint to have rotated.

And that condition, in combination with the resiliency, could cause a problem. The data that we have ranges between temperatures of zero degrees and up

2132

to 70 and, as you would expect, there is an influence of temperature on the pressure-holding capability of the putty.

You see it holding pressure for longer periods of time, of course, with colder temperatures. But the important thing is that, even at 70 degrees and even with putty which has been conditioned at 100 percent relative humidity, the data indicate that you can hold the pressure off for long periods of time compared to .5 seconds.

One of the data points went up to about 38 seconds. So what we have decided is that we have to do a more sophisticated test. This initial test had the joint configuration properly configured relative to the putty dimensions, but it did not have the capability to account for any dynamic effects of that joint.

Of course, as you apply pressure you get some slight movement, which would tend to move the NDR insulation slightly away from the putty. And so we are building another test fixture where we have the capability to induce that dynamic effect, and we should have that one in tests shortly.

But this one remains an open scenario at this point in time.

DR. FEYNMAN: That means the pressure is more

2133

than 200 psi, so that it could hold a pressure more than 200 psi?

DR. LITTLES: Yes, sir, it held pressure up to the full motor pressure.

DR. FEYNMAN: Does that mean the test that was made with the 200 psi beginning to load up the rings is not going to work sometimes, because the putty will keep the leak from appearing even if there was a leak?

DR. LITTLES: Well, that was the first thing I thought about, too, when I saw those data. But that test is conducted, that 200 psi test is conducted, for a 15 minute time frame. So you have a long period of time for that pressure to work its way through the putty and blow through.

And we do have data that says it does that. But it is very much a time function, and it is a temperature function, and I'm sure it is a function, a very strong function, of humidity, the conditioning of the putty.

And it is probably also a strong function of the layup. It is laid up, I believe it is, in seven strips, so there is some variation in the layup as well. There are a number of variables in that putty.

DR. FEYNMAN: How far around was it? It was a small model, wasn't it, about ten inches or something?

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DR. LITTLES: Yes, sir.

DR. FEYNMAN: And you have 37 feet of circumference in the other case. So if the statistics of any kind—of course, the 37 feet one is much weaker.

DR. LITTLES: There would be a higher probability of having some weak point, yes, sir.

DR. COVERT: Dr. Littles, you had said earlier that the effect of loads and flight loads and so forth might be important here; is that not correct?

DR. LITTLES: Yes, sir.

DR. COVERT: Might I suggest, for the benefit of us absent-minded folks, that you might add another box onto this flow diagram indicating that you might put that input in here?

DR. LITTLES: Yes, sir. As a matter of fact, Jack and I were talking about that during lunch, the fact that we have taken these loads out of the design load case. We have to have it visible someplace, and so I agree with that completely. We need to have it reflected at the proper places in these areas, and we will do that.

DR. COVERT: Thank you.

DR. LITTLES: Okay. That finishes the basic elements of these scenarios. I now want to address the

2135

two things—could you come to the right of the chart, please. Move the chart to the left.

To address two elements of the scenario which we have been, along with others, trying to explain, the first being that we see a puff of smoke near zero at .5 seconds and we see the plume come out and the leak start at 59 seconds.

There are two ways that could happen, of course. One is that you have a continuous leak after you get the puff and the joint somehow holds together until 59 seconds; and the other being that you get the leak at that point, something makes the leak stop. Maybe loads, even though they're not outside of design loads, but the loads are increasing and maybe at Max Q it opens the joint up.

And so the first thing I will discuss is the work that we are doing relative to substantiating or refuting the fact that you could have a leak through that joint that would let the joint remain intact between zero seconds and 59 seconds.

We have been doing some work on that in two different areas. We've been doing analysis on it. We've also been doing some subscale motor tests, which General Kutyna referred to a moment ago, and I will discuss those.

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I am going to skip charts 17 and 18. 17 and 18 are photos of the initial puff and the plume that emanates at 59 seconds. You've seen those in the time line already, and so I will just go on then to chart 19.

(Viewgraph L-19.) [Ref. 3/7-79 1 of 6]

We are doing the analysis in an iterative process. Of course, it involves a flow analysis, a thermal analysis, coupled with a structural analysis. We started simply—we are getting more and more complicated—to try to make the model fit what could have happened.

We have a two dimensional model which we have been using for some time, which incorporates the flow, the potential melting of the metal, the recession of the O-ring material, NDR rubber, and also incorporates 1D spreading, one dimensional spreading. And what I mean by that is that you have flow which would initially be constrained by the gap in the O-ring, if that is what the problem is, or a flaw in the sealing surface, or maybe by a hole in the putty, since the putty has to erode as well.

And as that flow goes downstream in that joint, it wi l spread circumferentially, and as it does that it decreases the amount of heat it puts into the

2137

metal. We started off with a 1D flow spreading model and a 2D thermal model. We have now gone to a 3D model, which is depicted at the bottom.

We have also done some structural work to determine how much of the joint you would have to heat up in order to have it fail with various conditions. The preliminary data that we have back doesn't allow us to say that we could sustain a leak that long, but we are still doing some work on that. I still think it may be possible.

We, with the 3D model now, are showing that we would get a burn-through in 35 seconds if we started at time zero. But I hasten to add that there are some things we have not included yet. We have to look at the spreading profile some more. We have to put some better 3D conduction effects in the model. And the third thing is, that is very important, is as you get flow through that joint, the products of combustion are going to allow you to deposit aluminum oxide as the hot gas flows through.

And what that does, of course, is two things. It will give you, where you deposit it, it will give you a resistance to heating at that location, because it gives you an insulating effect; and the other thing it will do is it will tend to block the flow and further

2138

reduce the flow rate, to reduce the heat transfer.

We don't have that included in the models at this point in time. As a matter of fact, frankly, it is going to be difficult to do. We are working on it, but that is going to be a difficult thing to do.

But in conjunction with this, if you will go to—no, you don't have a chart. Excuse me.

In conjunction with this, we have been doing some subscale motor tests. We are doing that two ways. We have some motors at Thiokol which can burn—they are called five-inch motors. They can burn for three seconds.

And we have some larger subscale motors which can go for 24 seconds. And what we are doing is inducing various types of defects, either in the sealing surface underneath the O-ring or in the O-ring itself. And we have gotten some very interesting results out of that.

We have had a test with a 24-second motor where we had damage in an O-ring. We had the O-ring cut an eighth of an inch, and that particular one we had smoke for 24 seconds, but we didn't have any damage. We have had two cases, one where we had—we cut a—put a scratch in the sealing surface of 20 mils without putty in the joint, and that one ran for 24 seconds, but

2139

there was significant metal damage in that one.

We had another one where we had the same type of scratch in the sealing surface, about 20 thousandths. In this case we had putty in the joint. The putty held the pressure off apparently for about ten seconds, because for ten seconds we didn't see any leakage underneath that scratch, and then for about five seconds you did see smoke come out, and then the flow stopped again and for the remaining part of the test, between about 15 seconds and 24, there was no leakage.

And so that is qualitative data that indicates to us that you could have a situation with a certain type of flaw in there and get a flow either intermittent or maybe even continuous.

And another thing, too, is the five inch CP motors are indicating, as you would expect, that you can also have aluminum oxide deposited at locations where you have leakage, and it builds up and it will make the flow switch around to another area.

And this is a thing relative to whether the initial leak might have been in some quadrant other than where we finally saw it. You could have that initial leak somewhere else, have it stop up, but had accumulated enough damage in the other quadrant for that to break loose and cause the final failure to be in

2140

another location.

Could you go now to chart 16, please.

(Viewgraph L-16.) [Ref. 3/7-78]

We will dispense with chart 21 as well. That is another photograph you have seen

This last item relates to one of the things that had been discussed earlier, and that is relative to the fact that we saw vehicle rates after we had the plume coming out—we had vehicle rates and TVC gimbal angles that were larger than would have been predicted with the conditions that we knew we had.

So we have been doing a lot of work to match the vehicle conditions we had with what we know about the plume characteristics. We have estimated forces and moments necessary to match the observed response. We have evaluated the plume characteristics using the film data to determine the vent forces and the aerodynamic influences.

And we established from that that you can get about 130,000 pound force, 130,000 pounds of momentum in the correct quadrant, and that doing that you can recreate the vehicle responses, both relative to rates and TVC gimbal angles.

That is a very brief, cursory description of a great deal of very good work. As a matter of fact, our

2141

people say, and I believe that JSC people agree, that this is the best match we have ever had. So they have done a good job of matching that, and we are confident now that what we see with this plume did indeed cause those vehicle rates. They agree very well now.

Could I go now to chart 22, please.

(Viewgraph L-22.) [Ref. 3/7-79 6 of 6]

This is by way of summary. I've discussed all of the scenarios and, as we have indicated as we've gone through it, we think that the inhibitor flaw is improbable, that the load exceedance relative to the design load situation is improbable, and we still have work, both analyses and tests, associated with those other legs of the scenarios.

That is my final chart.

CHAIRMAN ROGERS: Thank you very much.

MR. ACHESON: I would like to ask, in relation to your earlier testimony, I thought I understood you as saying that the smoke at ignition could be seen in the general area of the test port, and I thought I had previously understood from other testimony that the test port was about 90 degrees of arc away from where the smoke had been seen.

Could you clarify that, please?

DR. LITTLES: Well, I don't believe it is 90

2142

degrees away. The leak check port, if this is the tank and this is the SRB, is on the bottom right here. The plume as we see it later emanates from this location. And really, the leak could be anywhere in this area, because we can't see it.

Now, the question is whether with the photographic coverage we have it is coming from this location right here or somewhere around there. And again, there are a lot of people looking at those photographs who conclude that it is indeed not from the leak check port, and we may well be convinced of that after we get the report when we get back home. But I am anxious to see it because, as I said, I'm still one of those who believe I can see it from that area.

But I may be able to be convinced to the contrary.

VICE CHAIRMAN ARMSTRONG: The simulation work that you referred to at the end, the match, is that completed now?

DR. LITTLES: Yes, sir, that is completed.

VICE CHAIRMAN ARMSTRONG: Will there be a report generated for that?

DR. LITTLES: Yes, sir, we will do that.

VICE CHAIRMAN ARMSTRONG: Thank you.

CHAIRMAN ROGERS: Do you think the tests that

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you are running would ever—and maybe this isn't a fair question—that the tests you are running would ever re-establish confidence in that joint and those O-rings without change, so that subsequent flights could continue with the same equipment that was on 51-L?

DR. LITTLES: Yes, sir, that is a tough question. I believe that the tests we have, the test fixtures that we have, if they operate the way we plan for them to, will prove whether or not the joint rotation is a contributor or potential contributor to that leak.

CHAIRMAN ROGERS: Well, maybe I shouldn't press it. But anyway, you're trying—the simulations that you're testing, you're doing in trying to simulate conditions, you think will be fairly conclusive. But it seems to an outsider that it's going to be very difficult to simulate these tests with all of the conditions that existed on 51-L in a way that would help you with any assurance in the future.

DR. LITTLES: I agree with that. I very carefully added in that statement, if they work the way we hope they will. But as I pointed out earlier, this dynamic test fixture or any test fixture like that—and we have looked at other versions of that—is a very difficult fixture to put together and get it to do

2144

what you want it to do.

I personally have some question as to whether it is going to work. It may, but it may be another design than the one we have now. It is not an easy set of conditions to simulate in a subscale test rig.

MR. SUTTER: These tests are aimed at trying to understand the cause of the accident, aren't they? Isn't that their primary purpose?

DR. LITTLES: Yes, sir. That is their sole purpose.

CHAIRMAN ROGERS: But as my previous question suggests, the people who are running the tests are really in the position of running tests which, if they were successful, would prove they were right after all. In other words, the Thiokol people, the engineers, thought that, in view of the weather conditions, that flight should not be launched, and of course your people at Marshall felt the same way, and you are the very people that are conducting these tests.

That is why I suggested that if might be wise to think of some other outside independent source to work with you on the tests.

DR. LITTLES: We will certainly be responsive to that, and of course the tests that—this dynamic test fixture is one that is being used at

2145

Morton-Thiokol, and of course we have people there. And we would welcome anyone participating in that or any other ideas and concepts that anyone might have relative to a test fixture to do this. We would welcome that.

CHAIRMAN ROGERS: Thank you.

DR. KEEL: I've got one question with respect to this last chart, if you will put that back up on, your summary chart.

DR. LITTLES: Chart 22, please.

(Viewgraph L-22.) [Ref. 3/7-79 2 of 2]

DR. KEEL: Let me just ask the question while they're looking for the chart. When you started off with your fault tree analysis, you had three categories of probability: probable, improbable, and possible. And you labeled a fault in the external tank leading to a fault in the SRB as just possible.

Now, this final summary chart, though, has the leak in the external tank as being probable. So how did you get from possible to probable?

DR. LITTLES: That is as it affects the SRB. The initial chart there on the tank had the overall tank yellow, with some items red and some items green. But again, this relates to the impact of that on the SRB. There may be, as I think about it here, a little bit of inconsistency in that.

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MR. LEE: If you look at the overall fault tree, the external tank was colored yellow because we had not completed all of the evaluation assessments, and I think in particular the review of records relative to structural flaws.

That makes the external tank as the failure mode, if you will, to be still suspect. If you notice, in the same fault tree there is an arrow from the external tank into the SRB, which still is a viable potential contributor to the SRB failure. So that is the reason we painted it red or colored it red here and yellow in the overall chart.

DR. KEEL: It still looks to me to be an inconsistency.

CHAIRMAN ROGERS: Pursuing that just a bit further, every time I've gone back to that, each time that representatives from Marshall have testified they've pointed to the external tank as the number one suspect. And I notice Mr. Lucas said that in his press conference the other day.

And yet, ostensibly it seems as if the joint seems to be the number one suspect. And I don't quite understand it.

MR. LEE: I didn't mean to imply that. We don't think the external tank is the number one

2147

suspect. It is a potential as a contributor only. We know that the SRB failed, and until we complete all of the analysis associated with things such as a potential hydrogen leak at liftoff, then we can't close that out.

We know the SRB is the failure.

DR. KEEL: But is the only reason you have this labeled red is because you chose just to use red and green on this chart? If you had used yellow, would it have been yellow?

MR. LEE: I could have used yellow there, you're right. But as it relates to the SRB in general, it is still red.

CHAIRMAN ROGERS: Okay.

MR. LEE: This completes the summary of the failure scenarios and findings. We are now prepared to go into the summary of the other elements if you like.

CHAIRMAN ROGERS: Very good. Thank you.

(Witnesses sworn.)

H-077

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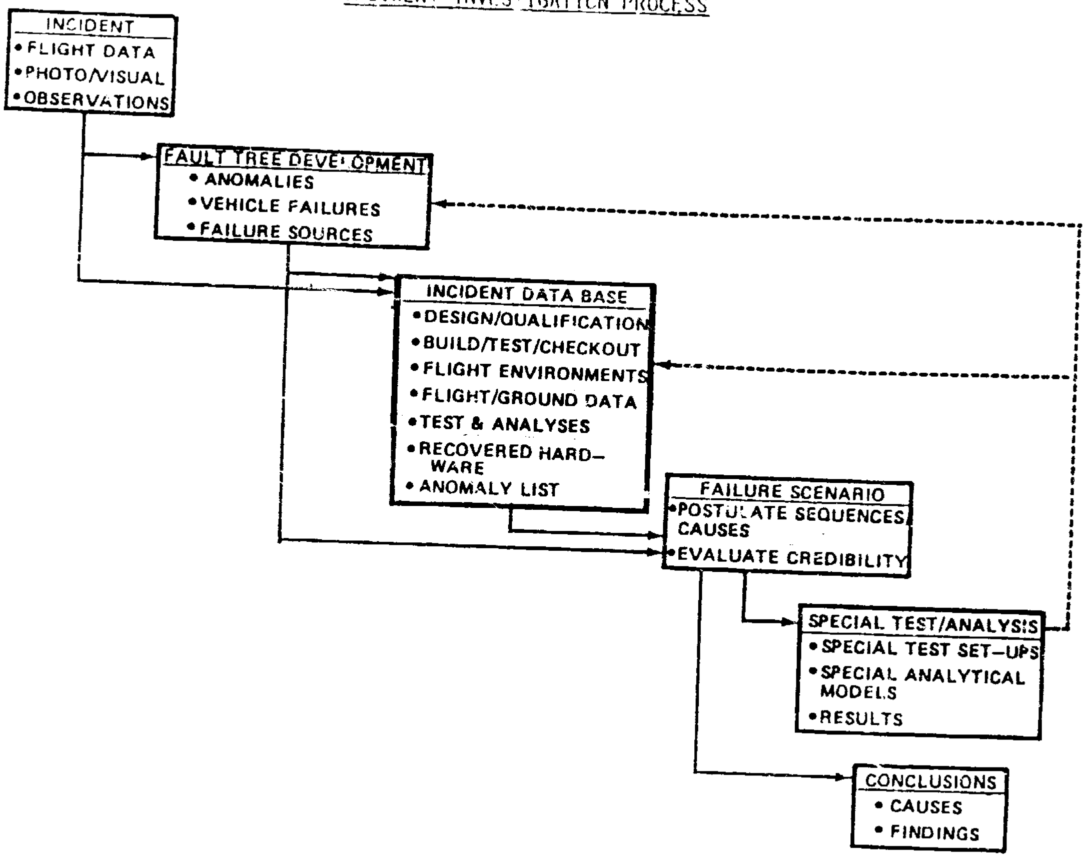

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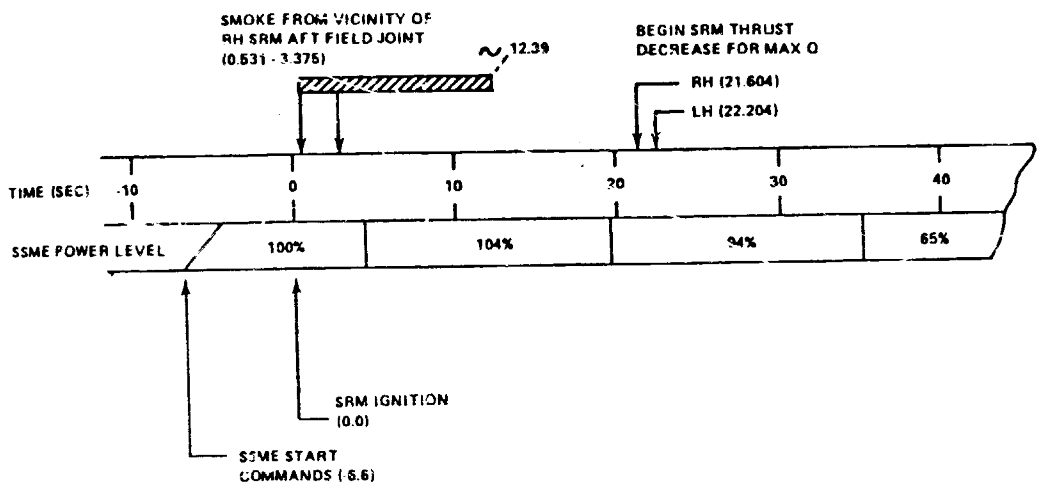

[Ref. 3 7-31]

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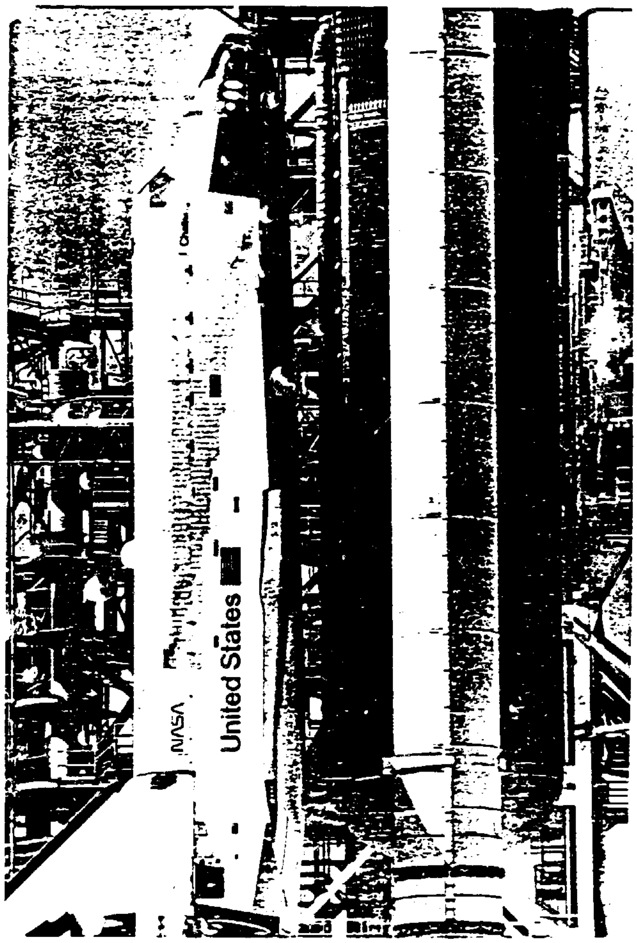

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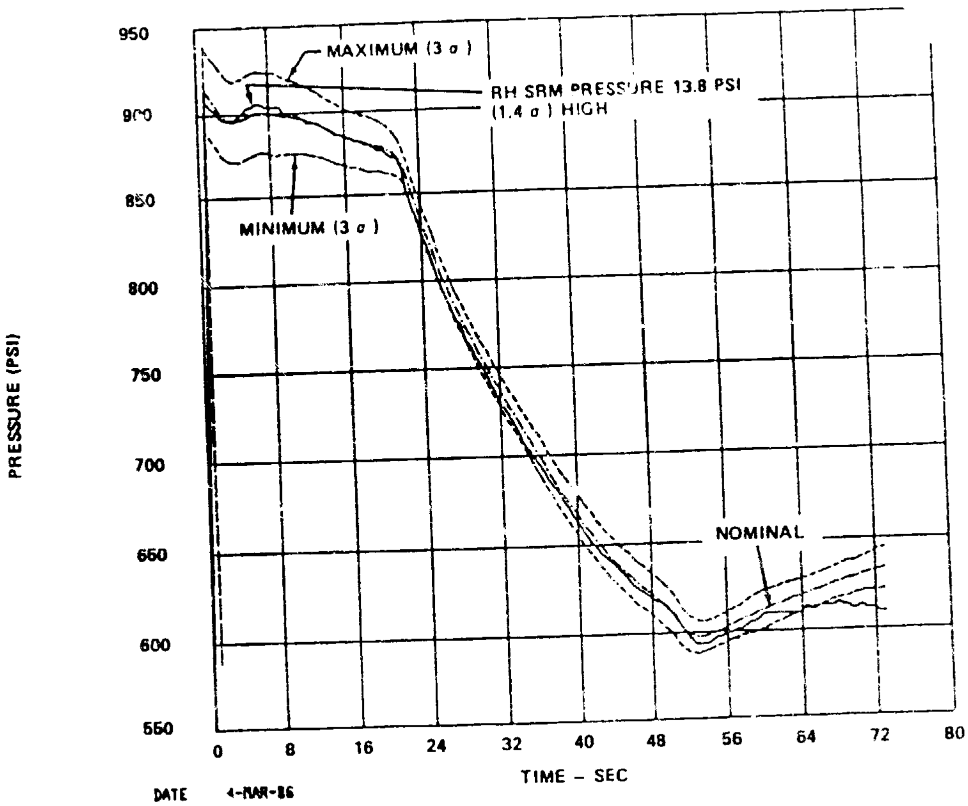

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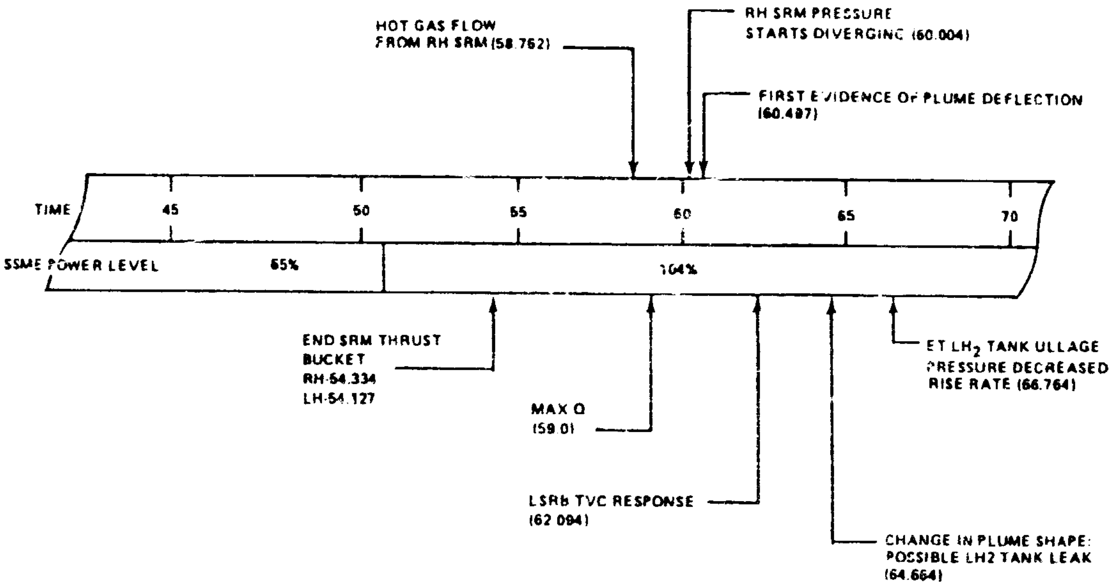

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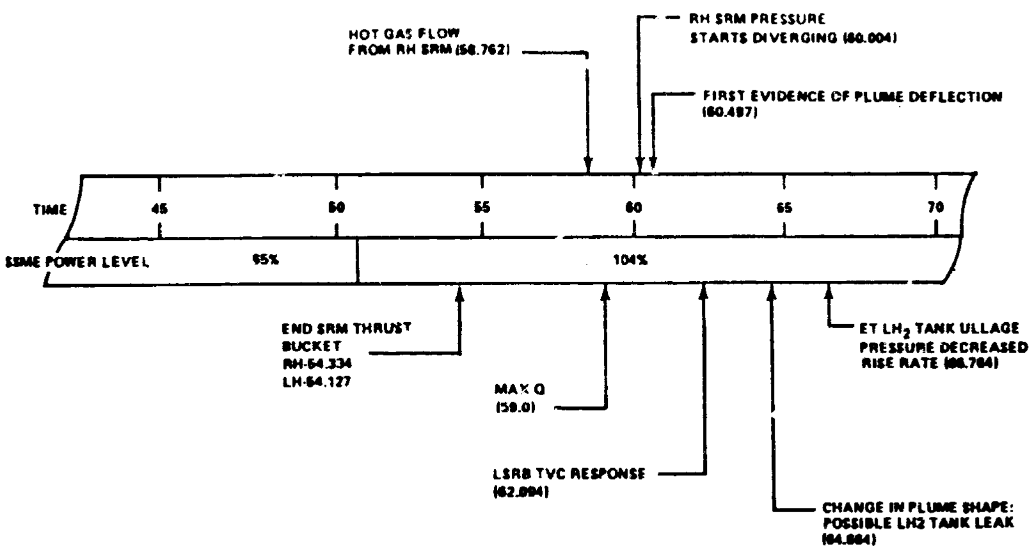

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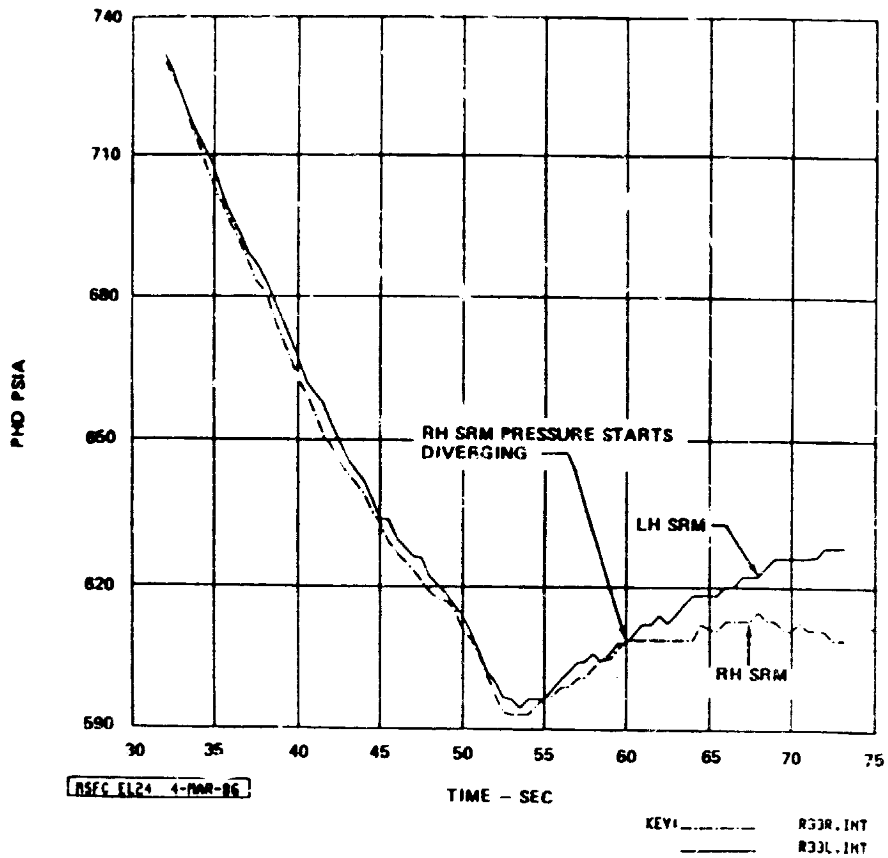

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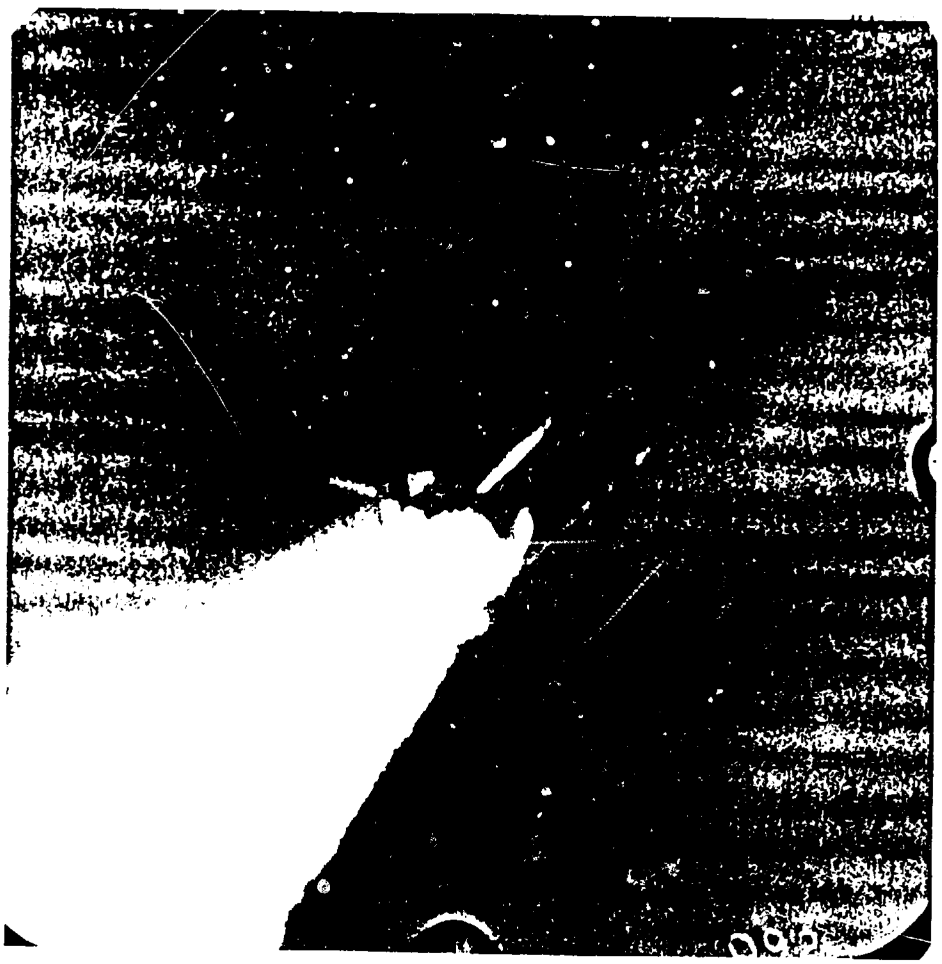
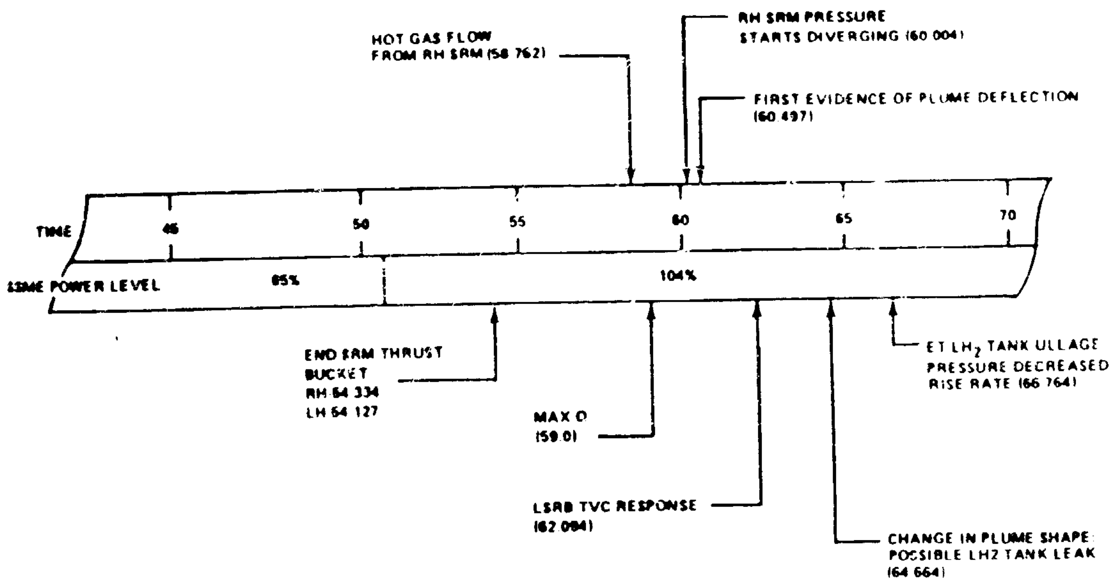


[Ref. 3-7-39]

STS 51L TIME LINE



[Ref. 3 7-40 1 of 2]

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[f. 3 7-40 2 of 2]

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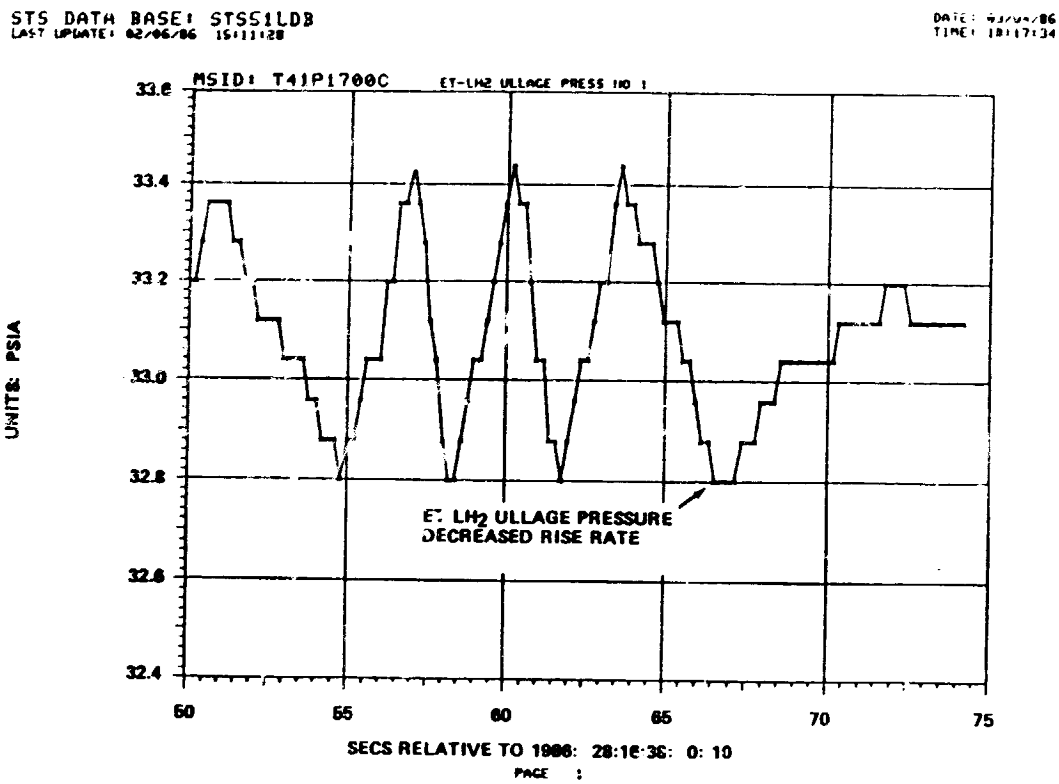

17

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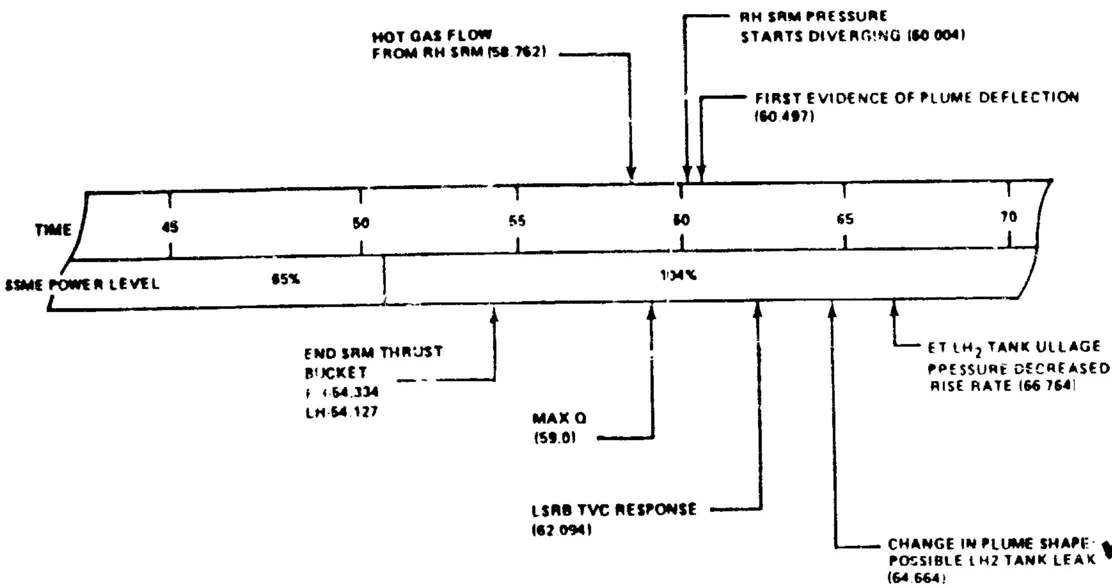

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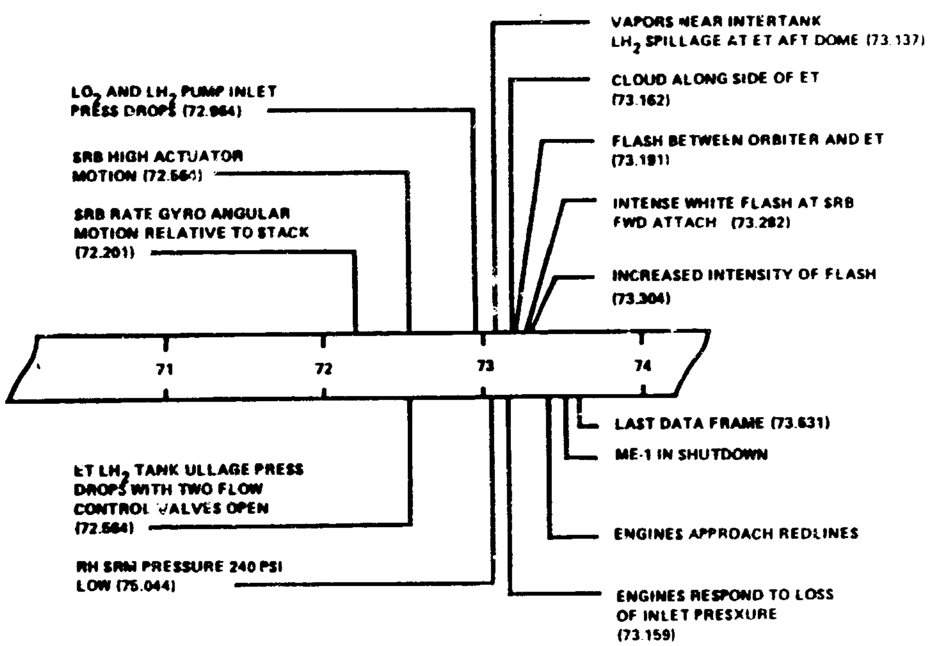

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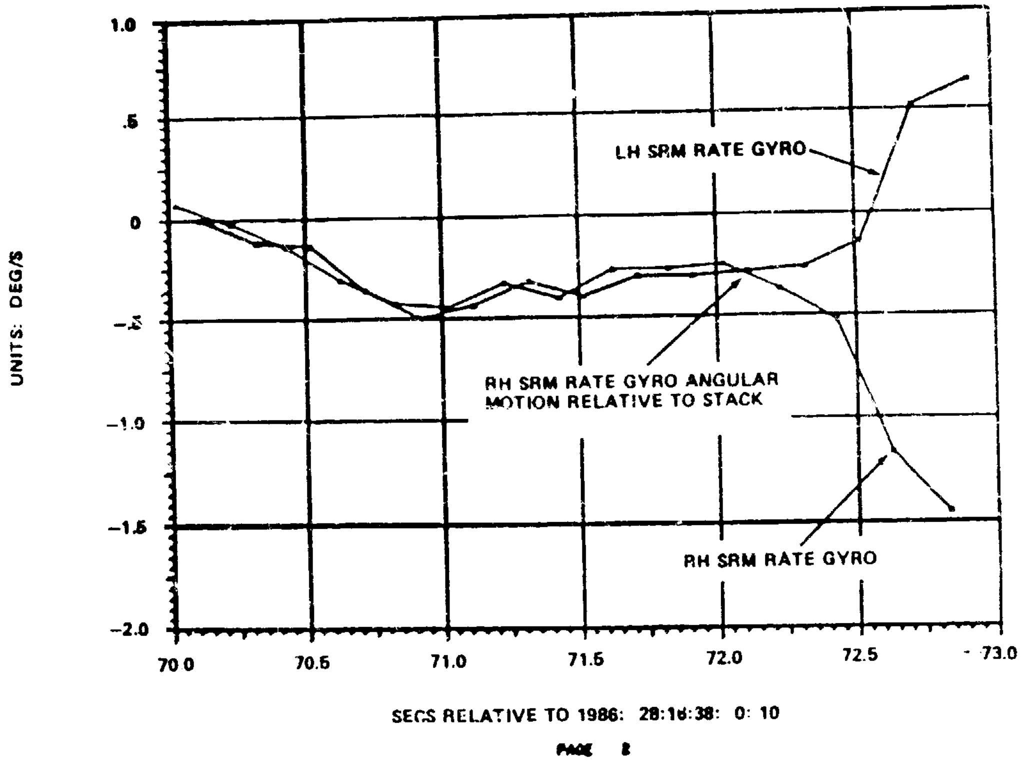

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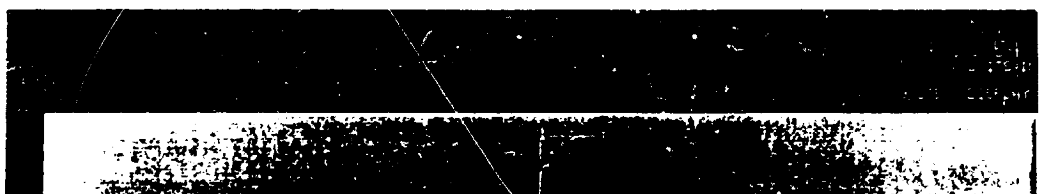

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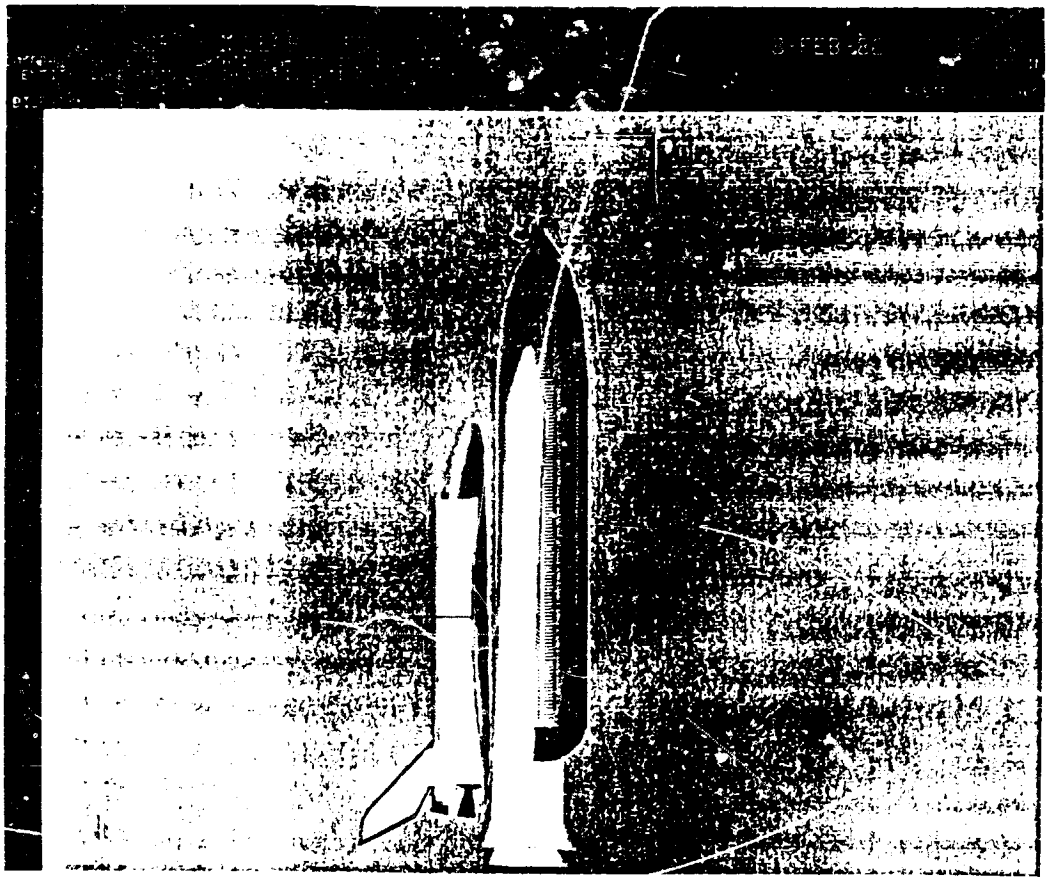

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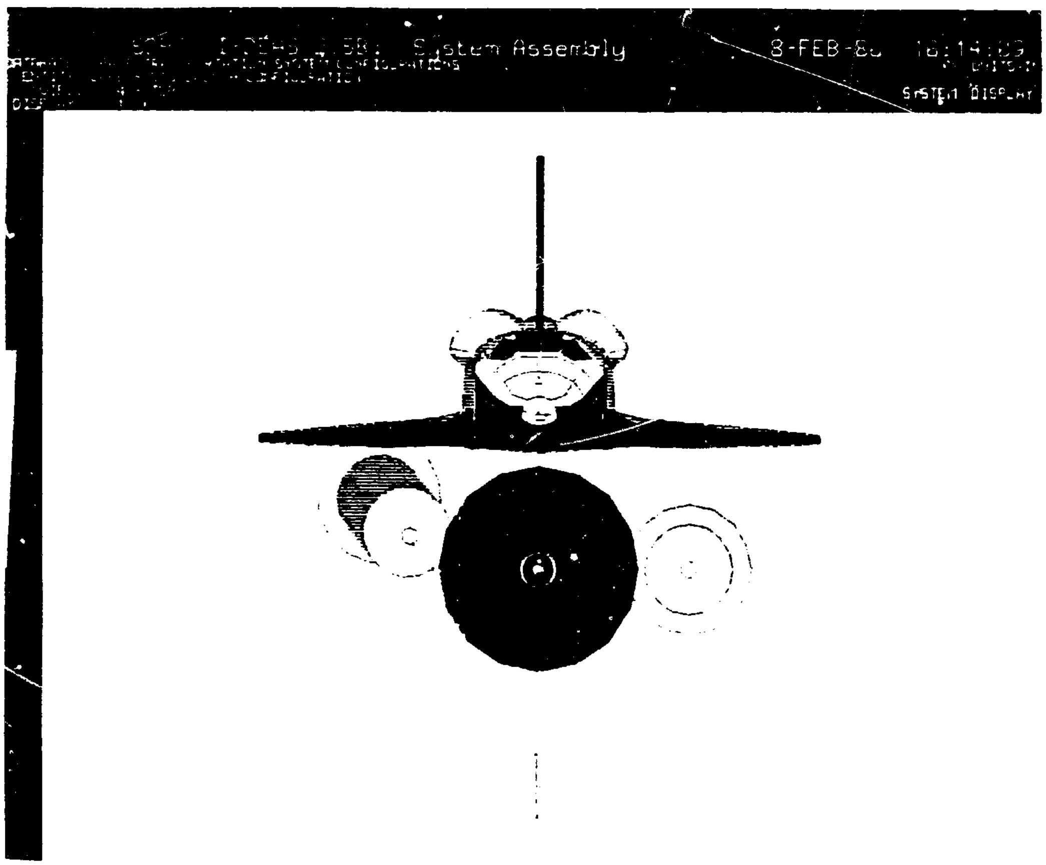

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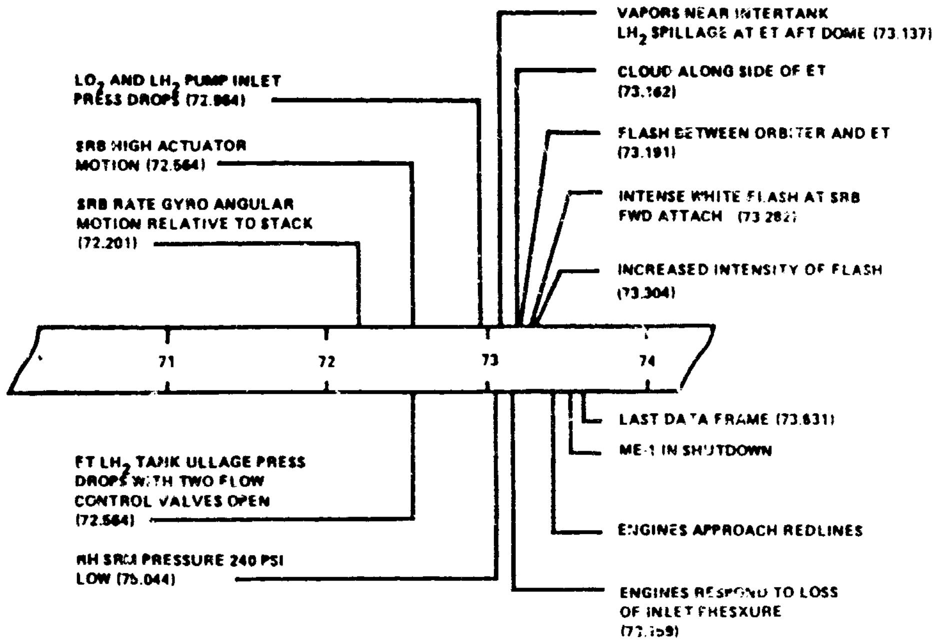
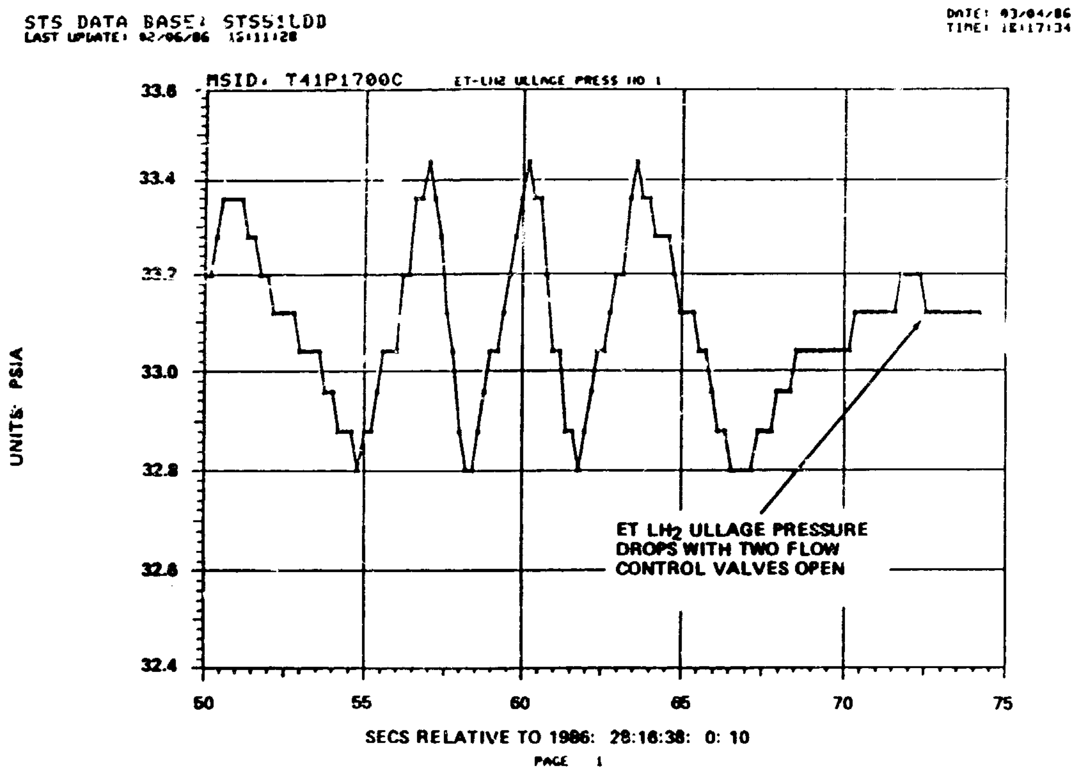
STS 51L TIME LINE



[Ref. 3 7-49]

H-083



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H-050

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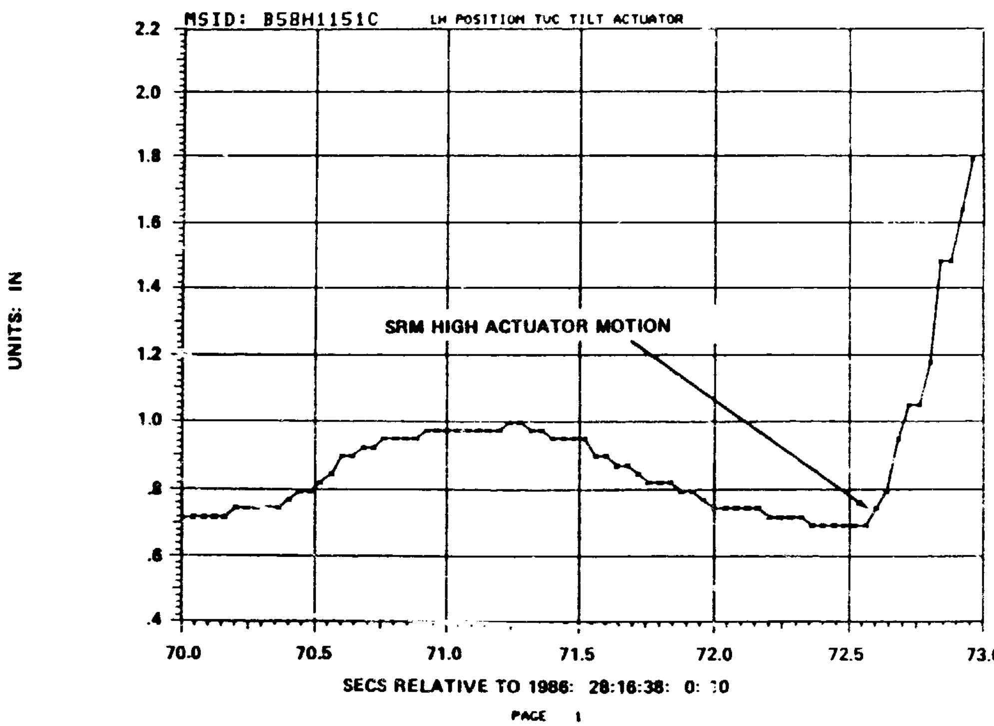
LAST UPDATE: 02/06/86 15:11:28

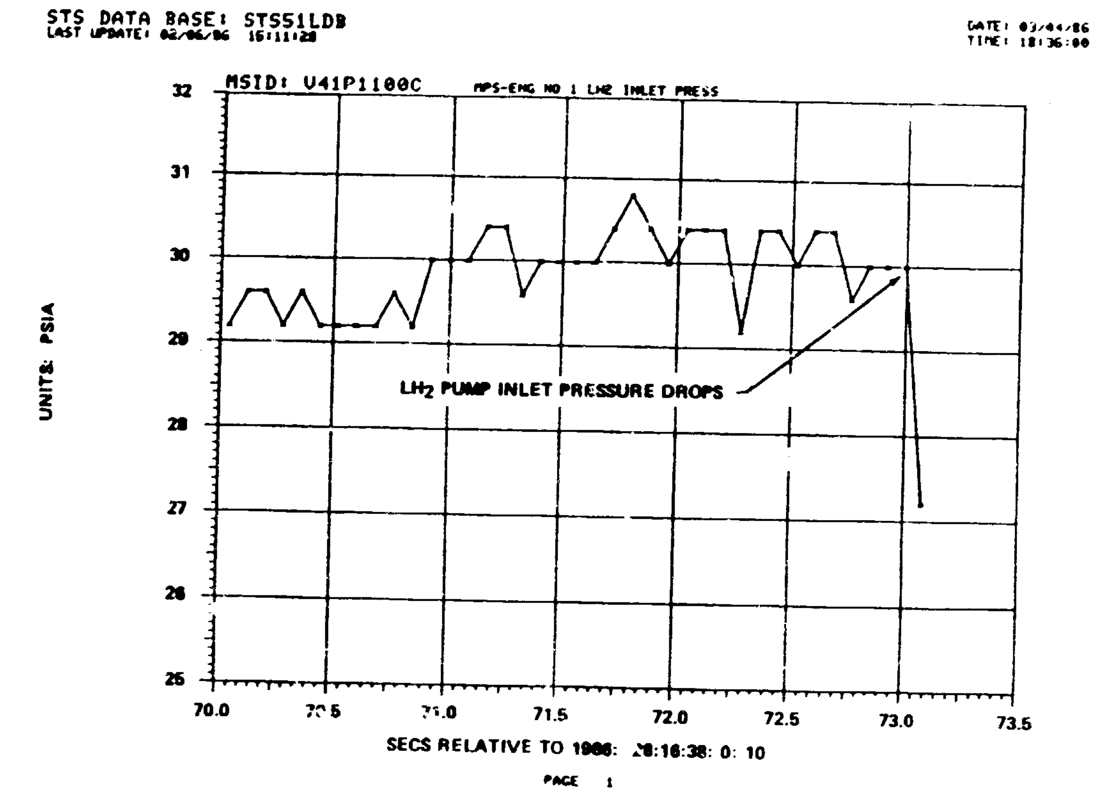
DATE: 03/04/86
TIME: 18:27:42



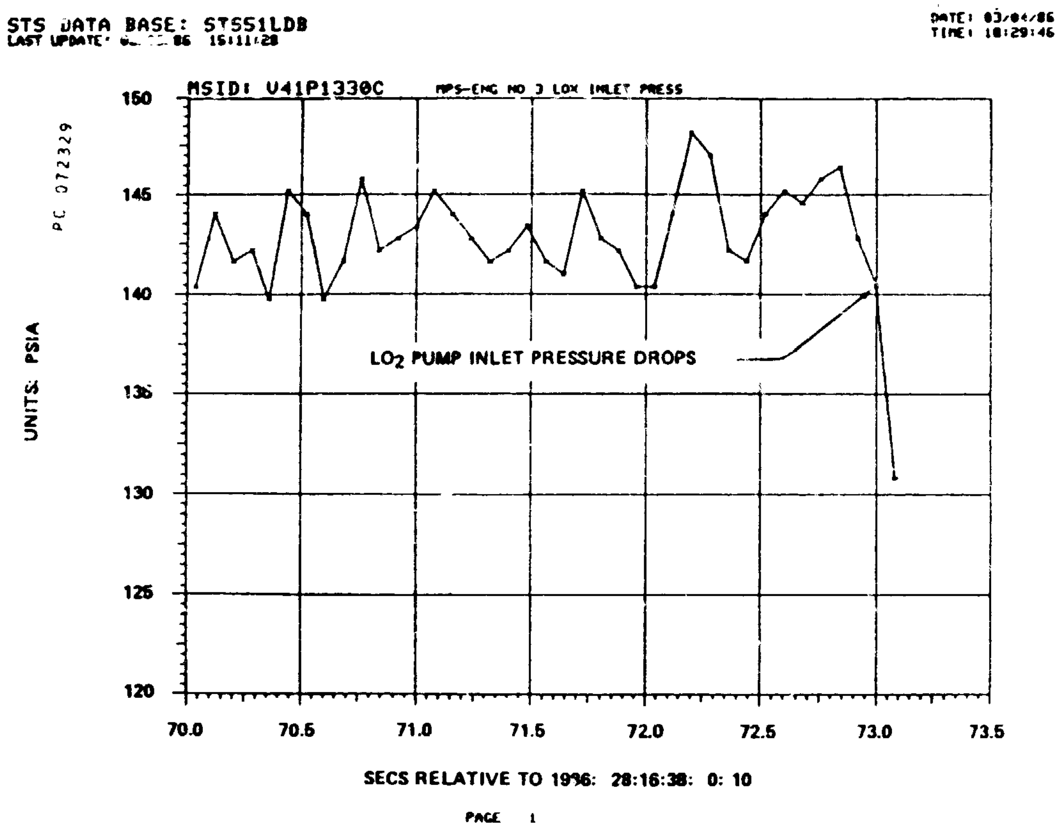
[Ref. 3 7-51]

20

H-061

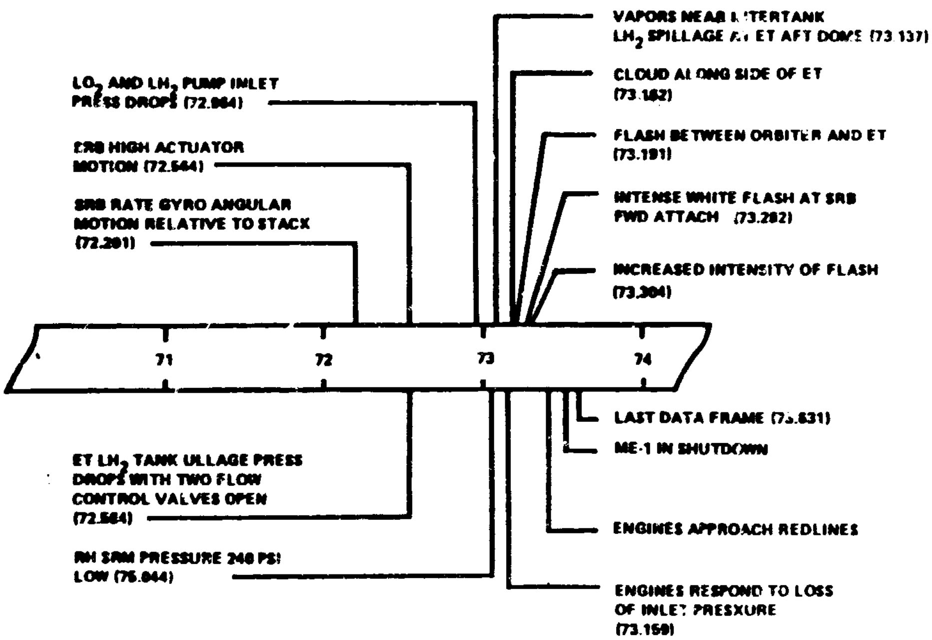


21E [Ref. 3/7-52]



[Ref. 3/7-53]

STS 51L-TIME LINE



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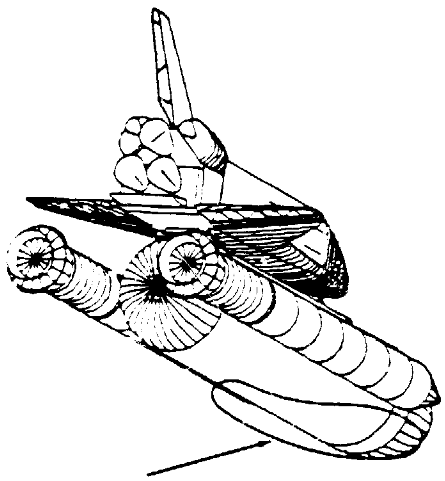
[Ref. 3/7-55]

[NOT REPRODUCIBLE]

[Ref. 3/7-56]



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[Ref. 3 7-58]

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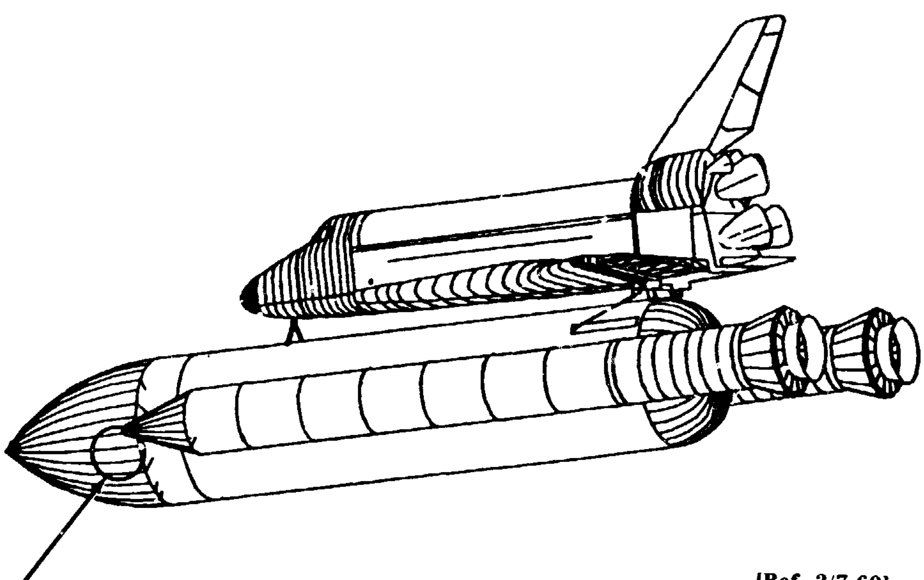
[Ref. 3/7-59]



[Ref. 3/7-60]

PRELIMINARY STS 51-L
ANOMALY LIST

PRELIMINARY

CATEGORY A

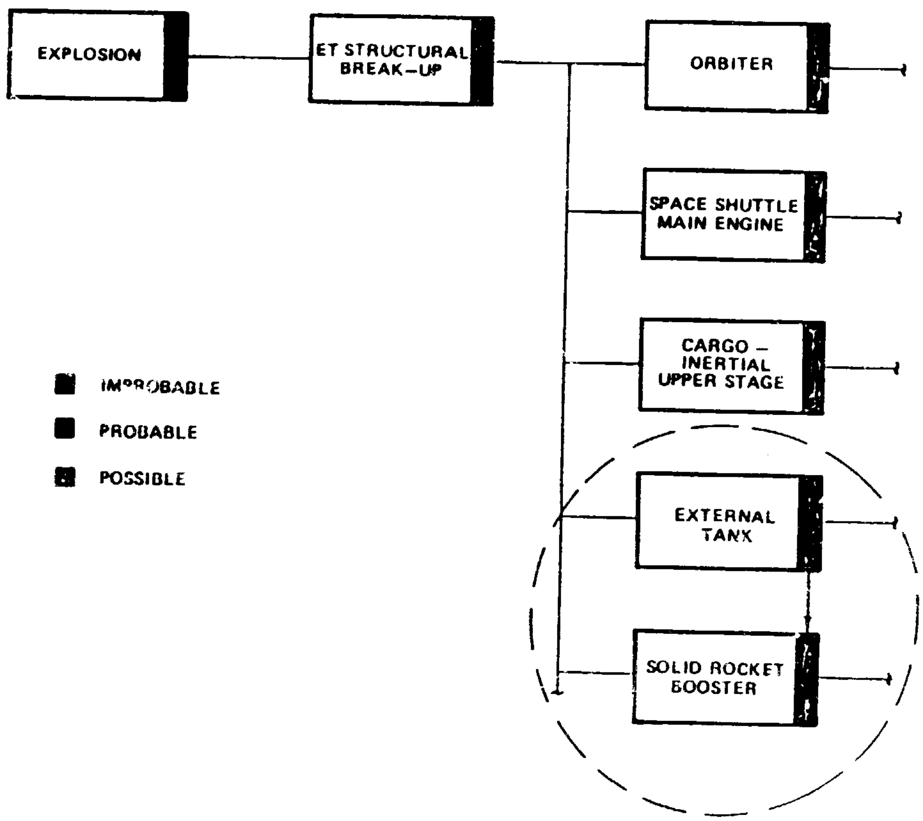
- 0 BLACK SMOKE AT 0.533 SEC.
- ^ HOT GAS LEAK AT RH SRM AT CR NEAR AFT FIELD JOINT AT APPROXIMATELY 58 SECONDS
- 0 RH SRM Pc DIVERGENCE AT 60 SECONDS
- 0 DECREASED H_2 TANK PRESSURE RISE RATE AT 66.8 SECONDS
- 0 YAW RATE DIVERGENCE AT APPROXIMATELY 72 SECONDS
- 0 H_2 TANK PRESSURE DECREASE WITH TWO FLOW CONTROL VALVES OPEN AT APPROXIMATELY 72 SECONDS
- 0 SRB MOTOR HIGH-RATE ACTUATOR COMMAND AT APPROXIMATELY 72.6 SECONDS
- 0 SHARP DECREASE IN ENGINE PUMP INLET PRESSURES AT APPROXIMATELY 73 SECONDS
- 0 RH SRM PRESSURE 19 PSI BELOW LH SRM (4.9 SIGMA LOW) AT APPROXIMATELY 73 SECONDS
- 0 INITIATION OF EXTERNAL TANK STRUCTURAL FAILURE AT APPROXIMATELY 73 SECONDS

CATEGORY B

- 0 POTENTIAL FOR WATER IN THE JOINT
- 0 LOW TEMPS AT LAUNCH
- 0 DIMENSIONS AT STACKING COULD RESULT IN METAL-TO-METAL CONTACT/O-RING DAMAGE
- 0 SUSPECT DEFECTIVE SECONDARY O-RING AT RH SRM AFT FIELD JOINT
- 0 SPECIAL INSPECTION OF IN STOCK O-RINGS REVEALED DISCREPANCIES
- 0 CHANGE IN O-RING INSPECTION CRITERIA OMITTED GOVERNMENT MANDATORY INSPECTION POINTS
- 0 FLAW IN EXTERNAL TANK WELD

[Ref. 3/7-61]

51-L FAULT TREE



[Ref. 3 7-62 1 of 2]

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[Ref. 3/7-62 2 of 2]

STS 51-L

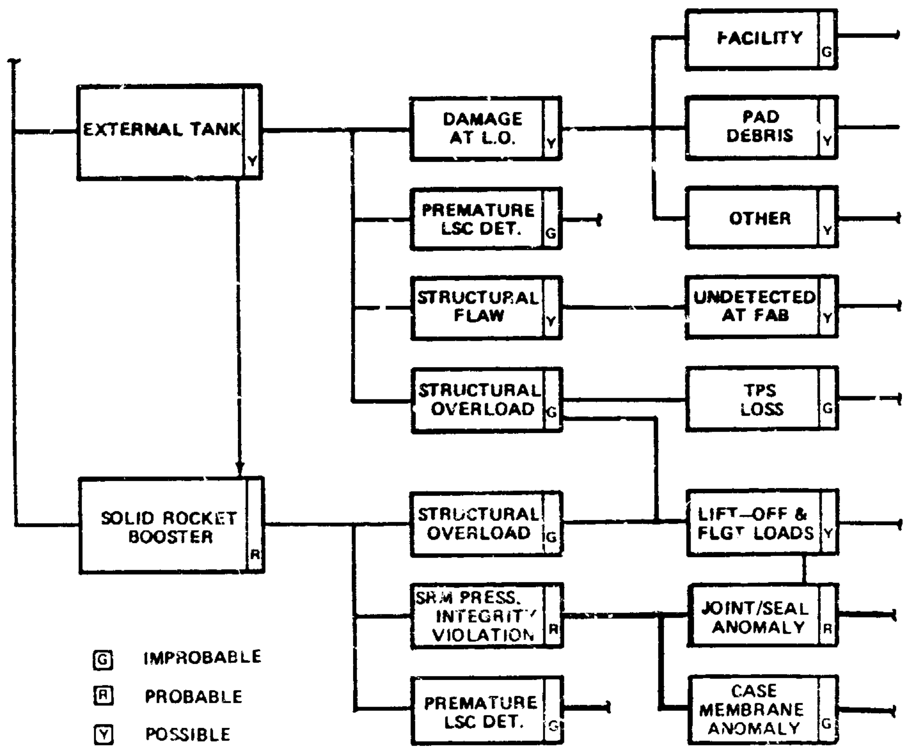
SRM HOT GAS LEAK

FAILURE SCENARIOS

W. LITTLES

[Ref. 3/7-63 1 of 2]

51-L FAULT TREE



[Ref. 3 7-63 2 of 2]

SCENARIO NO. 1

FAILURE MECHANISM:

- 0 DEFECT IN THE AFT SEGMENT MOLDED INHIBITOR (HOLE)
- 0 ALLOWS IGNITION OF THE PROPELLANT (HOT GAS THRU THE HOLE)
- 0 SUB-SURFACE PROPELLANT BURNING DEGRADES CASE INSULATION AT THE MEMBRANE WALL RESULTING IN A CASE BURN THRU AT (OR NEAR) 60 SECONDS

ASSESSMENT METHOD:

- 0 REVIEW BUILD AND PROCESS RECORDS OF 51-L RH AFT SEGMENT INHIBITOR
- 0 REVIEW EXPERIENCE BASE WITH THESE INHIBITORS
- 0 ASSESS FAILURE MECHANISMS/CHARACTERISTICS WITH OBSERVED 51-L DATA
- 0 DETERMINE BY ANALYSIS THE HEAT EFFECT ON INSULATION AND CASE MEMBRANE

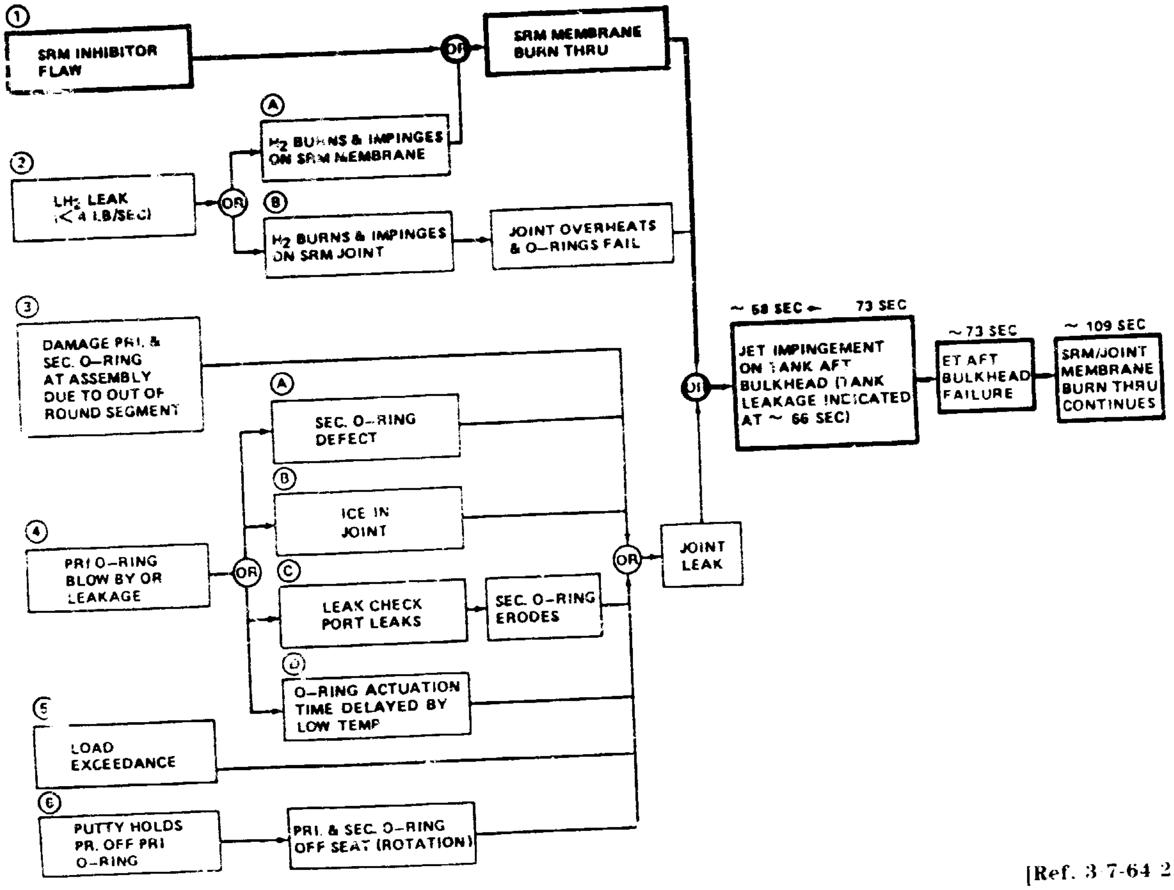
FINDINGS TO DATE:

- 0 THE MOLDED INHIBITOR IS LAYED-UP AS 8 PLIES OF RUBBER AND VULCANIZED ALONG WITH CASE INSULATION
  - LOW PROBABILITY OF A FLAW THRU 8 PLIES OF RUBBER
- 0 NO PREVIOUS EXPERIENCE WITH PROBLEMS AT THIS INHIBITOR
- 0 ANALYSIS INDICATES THAT 2.5 TIMES NOMINAL HEAT TRANSFER WOULD BE REQUIRED TO DEGRADE THE INSULATION AND HEAT THE CASE TO BURN THRU IN APPROXIMATELY 60 SECONDS
- 0 THIS FAILURE MECHANISM WOULD NOT EXPLAIN THE "SMOKE" AT LIFT-OFF
- 0 THIS FAILURE MECHANISM IS NOT PROBABLE

[Ref. 3/7-64 1 of 2]

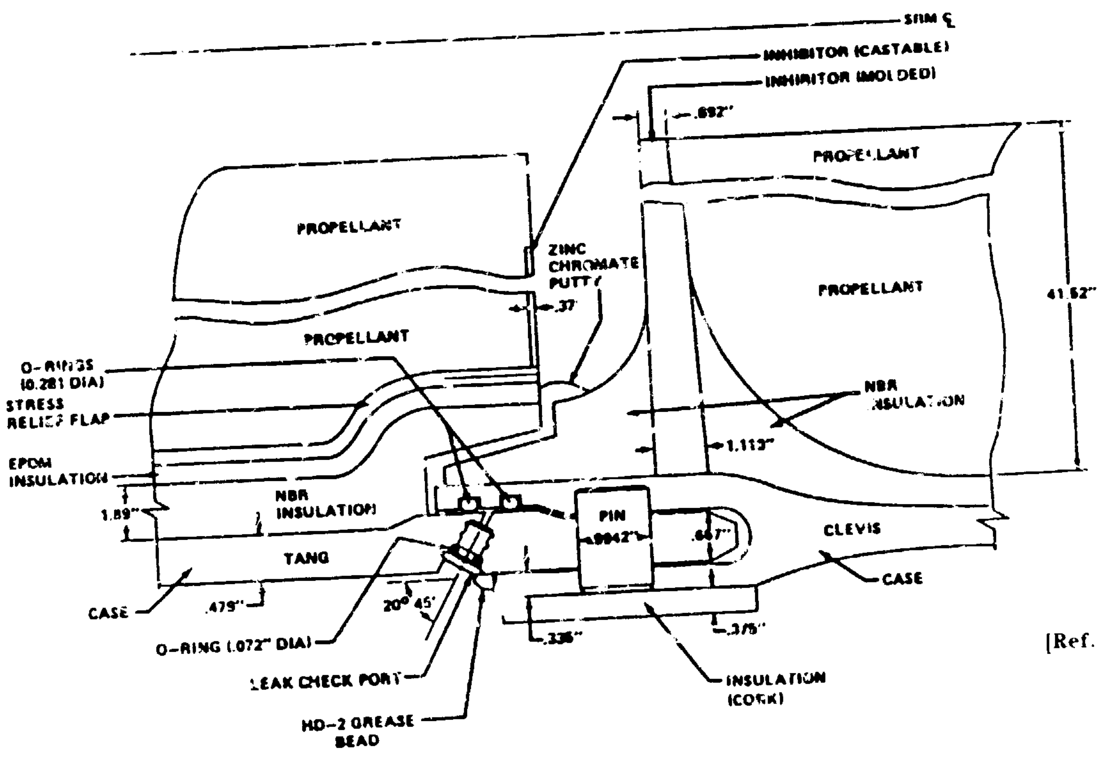
H–060

SRM HOT GAS LEAK FAILURE SCENARIOS



[Ref. 3/7-64 2 of 2]

AFT SEGMENT/AFT SEGMENT FIELD JOINT CONFIGURATION



[Ref. 3/7-65]

FAILURE SCENARIOS 2A AND 2B

FAILURE MECHANISM:

- 0 AN UNDETECTED FLAW OR FOREIGN OBJECT IMPACT RESULTS IN A H_2 LEAK AT LIFT-OFF
- 0 H_2 BURNS AND IMPINGES ON THE RH SRM
  - o DEGRADES SEAL PERFORMANCE (HEAT EFFECT ON JOINT ROTATION AND SEAL MATERIAL) TO THE POINT OF FAILURE IN APPROXIMATELY 58 SECONDS
  - o OVERHEATS CASE MEMBRANE TO A BURN THRU IN APPROXIMATELY 58 SECONDS

ASSESSMENT METHOD:

- 0 DETERMINE BY ANALYSIS THE SIZE OF HOLE IN THE LH_2 TANK THAT IS NOT DETECTABLE IN TANK PRESSURE INSTRUMENTATION
- 0 REVIEW TANK BUILD RECORDS AND PAD DEBRIS POTENTIAL
- 0 REVIEW 51-L TANK LIFT-OFF LOADS
- 0 DETERMINE BY ANALYSIS THE HEATING ON THE SRM JOINT
- 0 CONDUCT TESTS TO CHARACTERIZE TANK TPS BURNING AND SIMULATE BURNING H_2 IMPINGEMENT ON SRM

[Ref. 3/7-66 1 of 2]

FAILURE SCENARIO 2A AND 2B (CONT'D)

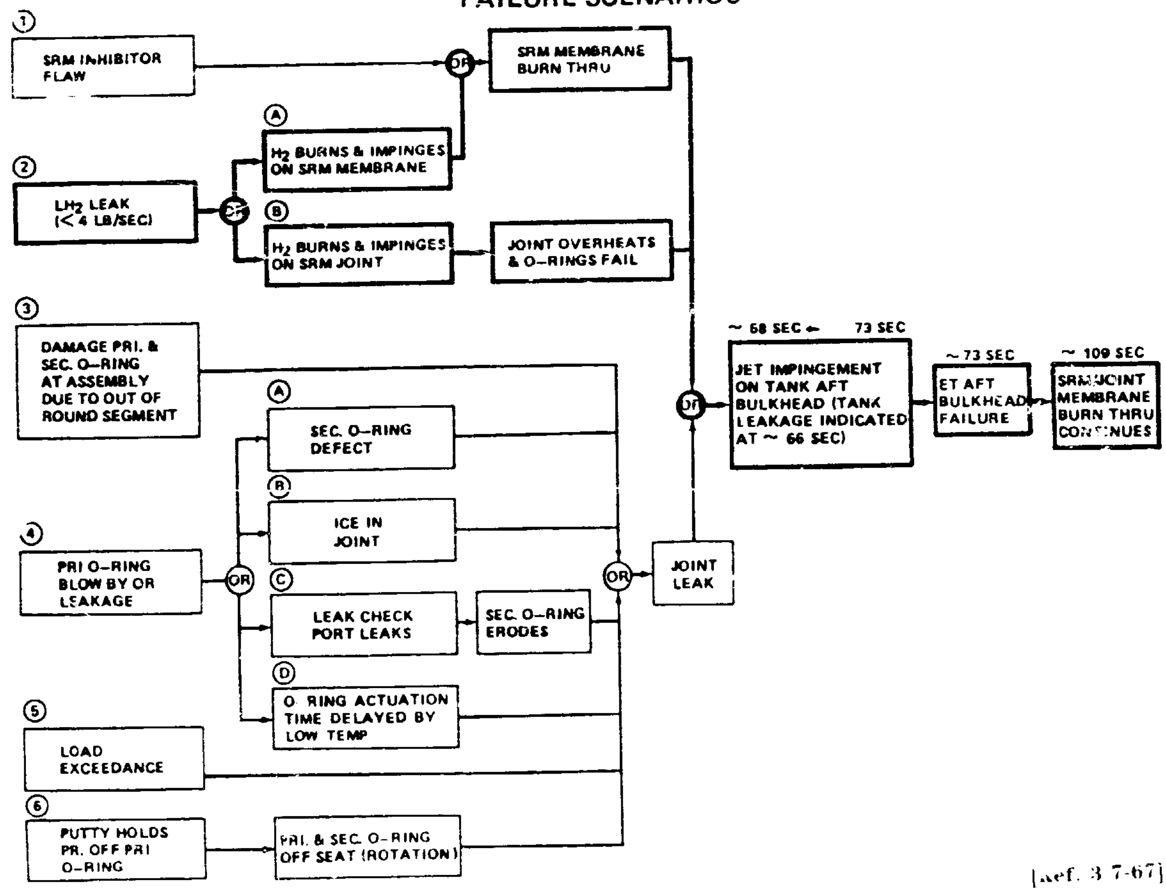
FINDINGS TO DATE:

- 0 ANALYSIS SHOWS 4 LBM/SEC LEAK FROM H_2 TANK IS NOT DETECTABLE IN PRESSURE INSTRUMENTATION (EQUIVALENT TO 0.81" DIA. HOLE)
- 0 PRELIMINARY ANALYSIS OF HEATING ON SRM
  - o ASSUMES COMPLETE COMBUSTION OF TOTAL FLOW
  - o ADDITIONAL JOINT ROTATION AND O-RING UNSEATING CAN OCCUR DUE TO TEMPERATURES OF APPROXIMATELY 900^{0}F
  - o MEMBRANE HEATING TO APPROXIMATELY 1300^{0}F
- 0 BURNING TPS MAY EXPLAIN "SMOKE" AT LIFT-OFF
- 0 LIFT-OFF LOADS ANALYSIS FOR EXTERNAL TANK SHOW NO ANOMOLOUS CONDITIONS FOR 51-L
- 0 THIS FAILURE MECHANISM NOT SUPPORTED BY REVIEW OF BUILD RECORDS TO DATE
- 0 THIS FAILURE MECHANISM REMAINS ACTIVE PENDING COMPLETION OF:
  - o HEATING ANALYSES WITH REFINED MIXING/BURNING ASSUMPTIONS AND EFFECT ON STRUCTURE/PROPELLANT
  - o TESTS TO SIMULATE BURNING H_2 IGNITION SOURCES
  - o ASSESSMENT/ANALYSIS OF DEBRIS POTENTIAL

[Ref. 3/7-66 2 of 2]

H-050

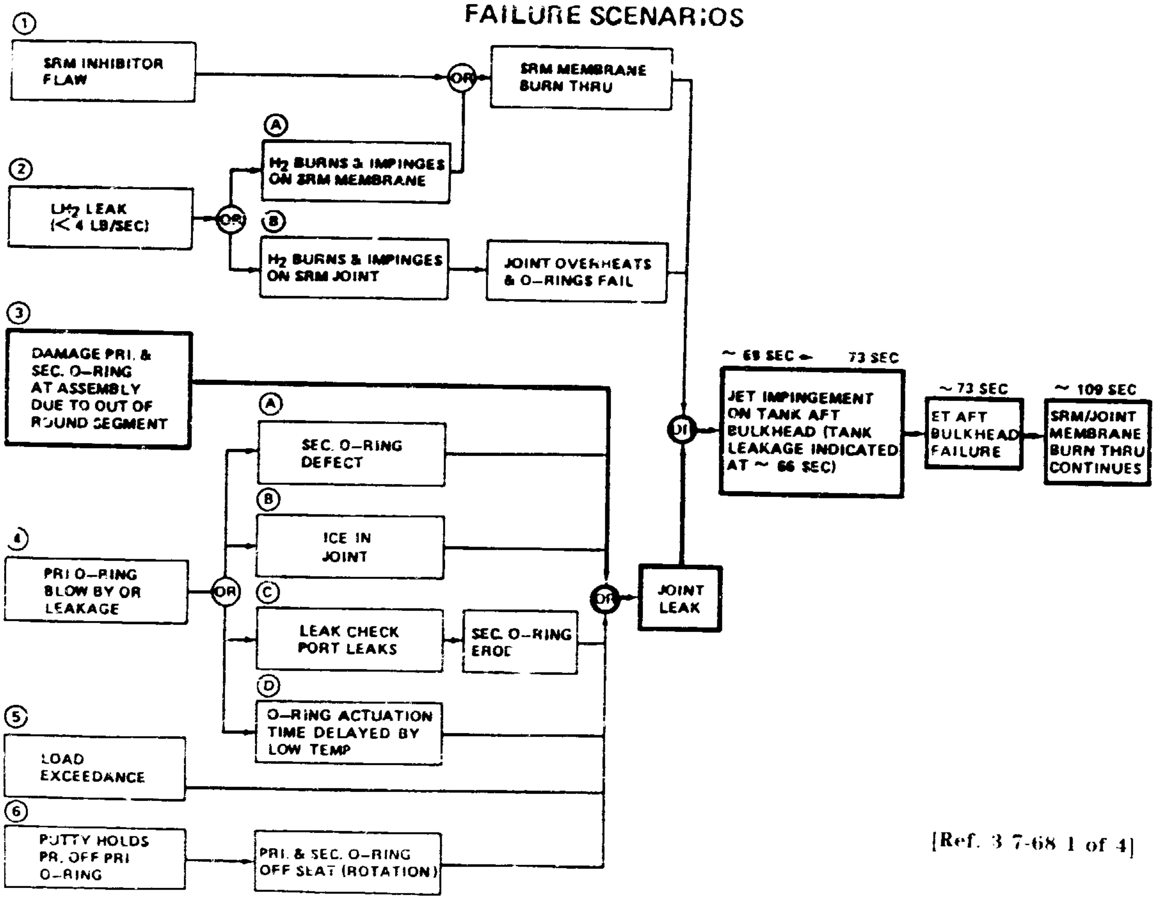
SRM HOT GAS LEAK FAILURE SCENARIOS



[Ref. 3 7-67]

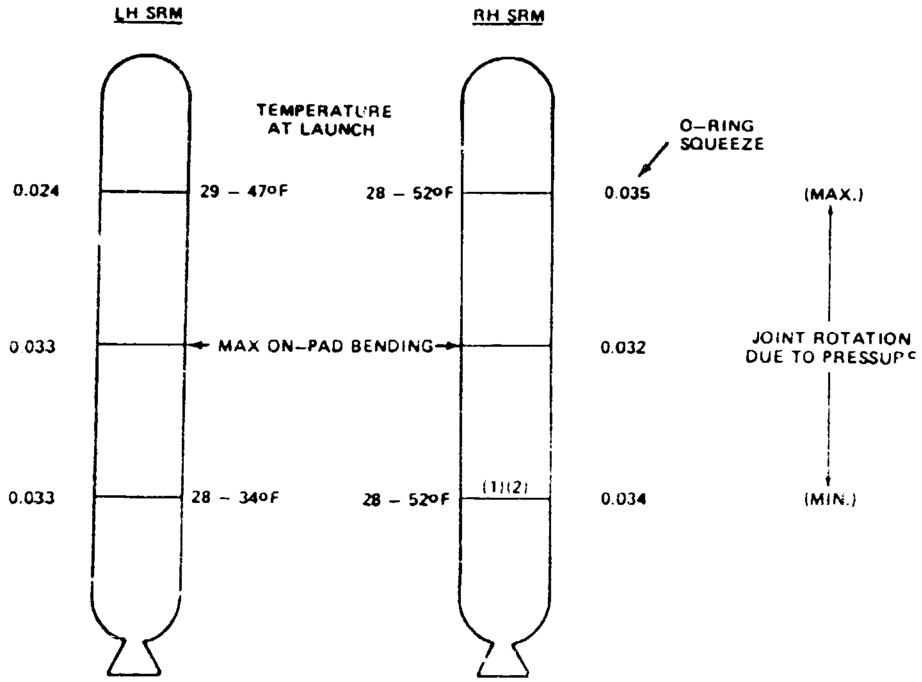
H-05

SRM HOT GAS LEAK FAILURE SCENARIOS



[Ref. 3 7-68 1 of 4]

51-L FIELD JOINTS



NOTES:

(1) REQUIRED ROUNDING FIXTURE TO SEGMENTS/MAXIMUM RESHAPING TO DATE
(2) SUSPECT SEC. O-RING IN CLOSEOUT PHOTOS

[Ref. 3 7-68 2 of 4]

FAILURE SCENARIO 3

FAILURE MECHANISM:

- DAMAGE TO THE O-RINGS INDUCED AT JOINT MATING DUE TO OUT-OF-ROUND CONDITIONS

ASSESSMENT METHODS:

- REVIEW STACKING CONDITIONS FOR 51-L AND OTHER FLIGHTS
- INTERVIEW STACKING CREW
- PERFORM A DIMENSIONAL TOLERANCE STACK-UP ANALYSIS
- CONDUCT TESTS TO SIMULATE 51-L RH SRM AFT FIELD JOINT MATING CONDITIONS TO EVALUATE POTENTIAL FOR O-RING DAMAGE
- CONDUCT TESTS TO SIMULATE LEAK TESTS AND SEAL PERFORMANCE UNDER 51-L RH AFT FIELD JOINT MATING CONDITIONS
- DESTACK STS 61-G TO ASSESS EFFECT OF OUT-OF-ROUND ASSEMBLY

[Ref. 3 7-68 3 of 4]

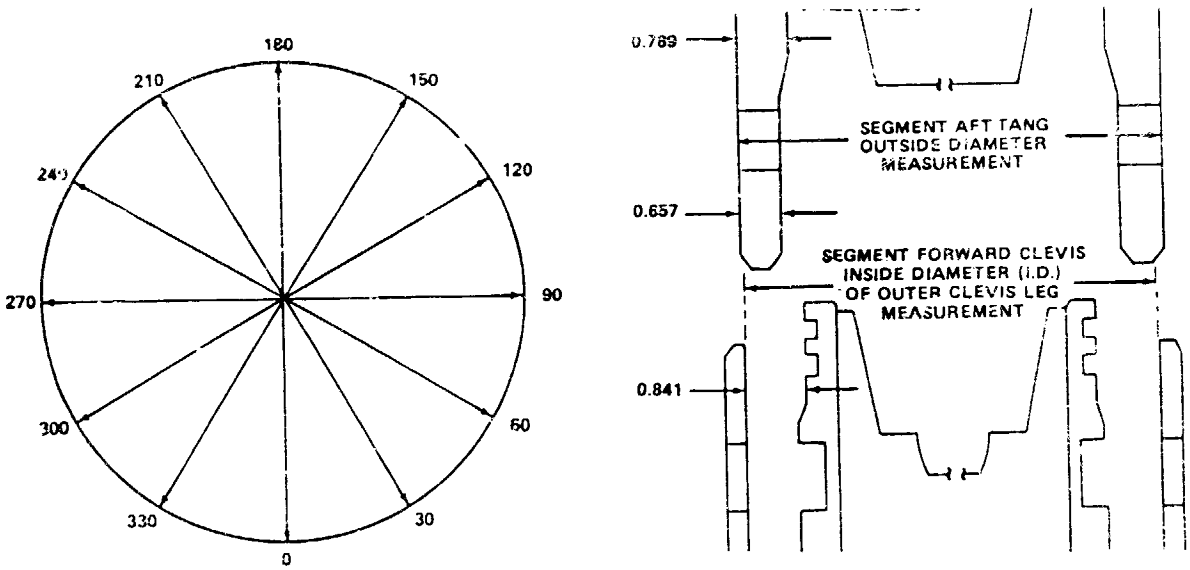
FAILURE SCENARIO 3 (CONTINUED)

FINDINGS TO DATE:

- 0 DIFFICULTY EXPERIENCED IN MAKING 51-L AFT FIELD JOINT (4 TRYS)
  - MECHANICAL ROUNDING FIXTURE REQUIRED
  - WORSE CASE OCCURRENCE OF DATA REVIEWED TO DATE RELATIVE TO POTENTIAL FOR O RING/TANG INTERFERENCE
- 0 DIMENSIONAL TOLERANCE STACK-UP ANALYSIS COMPLETED
  - METAL TO METAL INTERFERENCE (TANG ID WTIH CLEVIS INNER LEG OD - SEALING SURFACES) WAS 0.025" TO 0.209" BASED ON LAST MEASUREMENTS PRIOR TO MATE
- 0 THIS FAILURE MECHANISM REMAINS ACTIVE PENDING COMPLETION OF PAPER REVIEWS, CREW INTERVIEWS, AND TESTS

[Ref. 3/7-68 4 of 4]

SCENARIO 3 (CONTINUED)



51–L AFT FIELD JOINT

LOCATION	FIRST MEASUREMENTS PRIOR TO MATE (DIFFERENCES)	LAST MEASUREMENTS PRIOR TO MATE (DIFFERENCES)
0 – 180 DEG.	+0.512 INCHES	+0.216 INCHES
30 – 210 DEG.	+0.158 INCHES	+0.158 INCHES
60 – 240 DEG.	–0.334 INCHES	–0.118 INCHES
90 – 270 DEG.	–0.728 INCHES	–0.334 INCHES
120 – 300 DEG.	–0.669 INCHES	–0.393 INCHES
150 – 330 DEG.	+0.059 INCHES	–0.059 INCHES

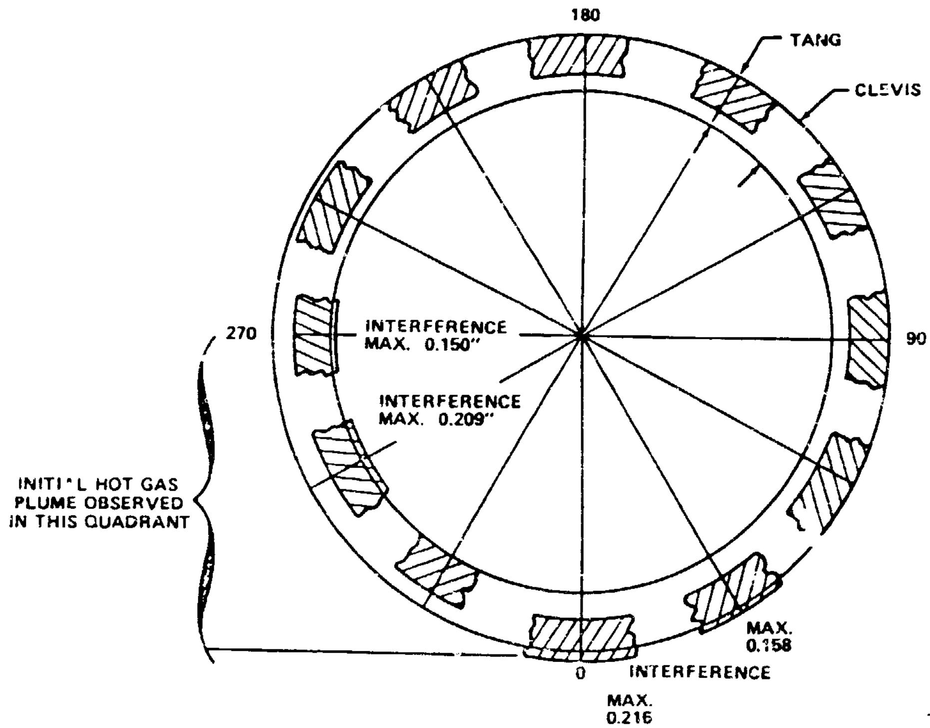
NOTE:

+ DIMENSIONS TEND TOWARD CONTACT OF TANG OD WITH OUTER CLEVIS LEG
– DIMENSION TEND TOWARD CONTACT OF TANK ID WITH INNER CLEVIS LEG (O–RINGS)

[Ref. 3 7-69]

H-078

51-L RH SRM
AFT FIELD JOINT MATING



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[Ref. 3 7-70]

FAILURE SCENARIO 4A

FAILURE MECHANISM

PRIMARY O-RING BLOW-BY/LEAKAGE AND LEAKAGE PAST SECONDARY O-RING DUE TO O-RING DEFECT

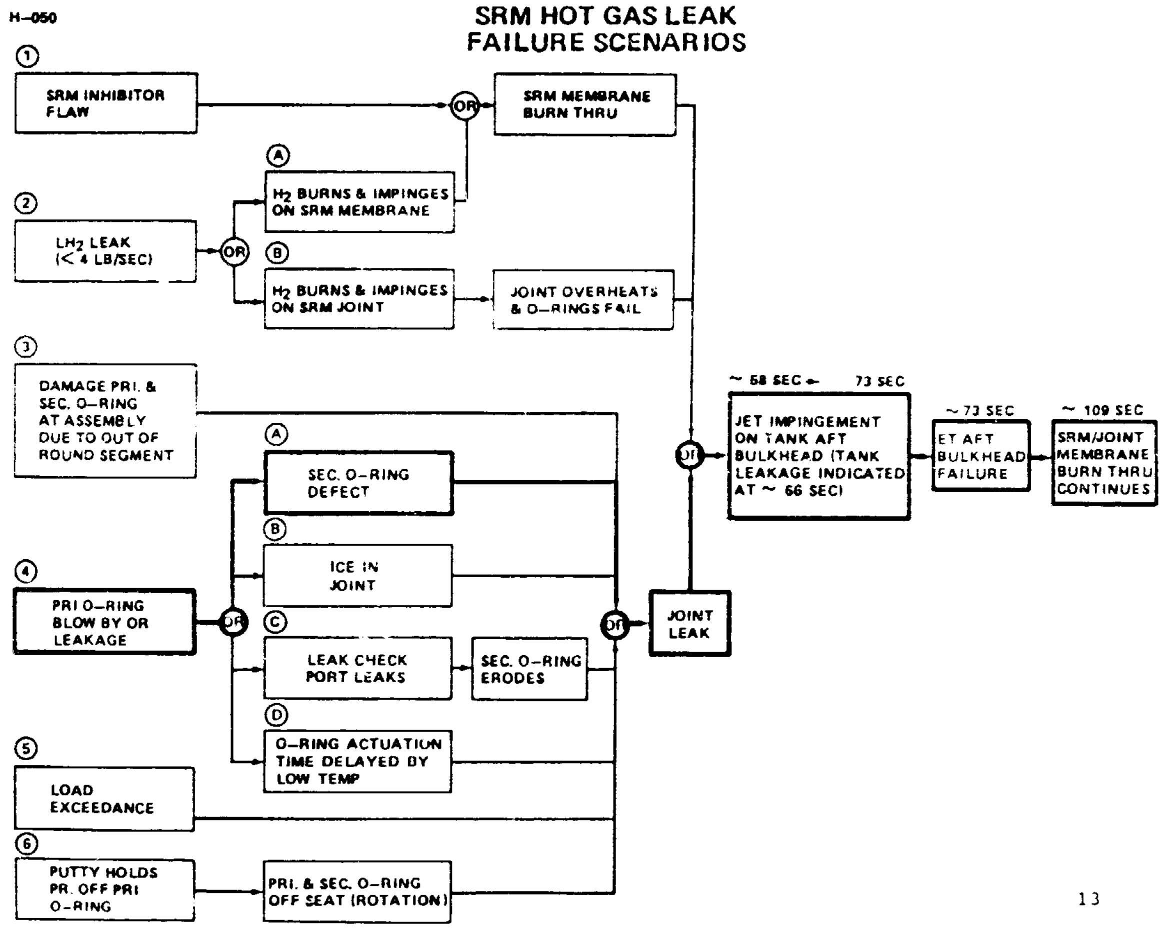
ASSESSMENT METHOD

- O ANALYSIS OF CLOSEOUT PHOTOS
- O REVIEW OF RECORDS
- O SPECIAL INSPECTION OF O-RINGS
- O TESTS TO EVALUATE PERFORMANCE OF SIMULATED O-RING DEFECT AND TO EVALUATE LEAK TEST OF O-RING WITH SIMULATED DEFECT

FINDINGS TO DATE

- O CLOSEOUT PHOTO FOR STS 51-L AFT FIELD JOINT SHOWS SUSPECT SECONDARY O-RING
  - o PHOTO ANALYSIS IN PROGRESS
  - o OTHER CLOSEOUT PHOTOS IN REVIEW

[Ref. 3 7-71 1 of 2]



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[Ref. 3 7-71 2 of 2]

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[Ref. 3 7-72]

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[Ref. 3 7-73]

FAILURE SCENARIO 4B

FAILURE MECHANISM

PRIMARY O RING BLOW-BY AND ICE IN JOINT EFFECTING SECONDARY O RING OR JOINT CLEVIS DIMENSION

ASSESSMENT METHOD

- O TESTS TO EVALUATE JOINT DIMENSION CHANGES WITH WATER FREEZING IN JOINT
- O TESTS TO EVALUATE SEALING CHARACTERISTICS WITH COLD GREASE DOWNSTREAM OF SECONDARY O RING
- O ANALYSIS TO EVALUATE POTENTIAL FOR WATER FREEZING IN JOINT TO UNSEAT SECONDARY O RING

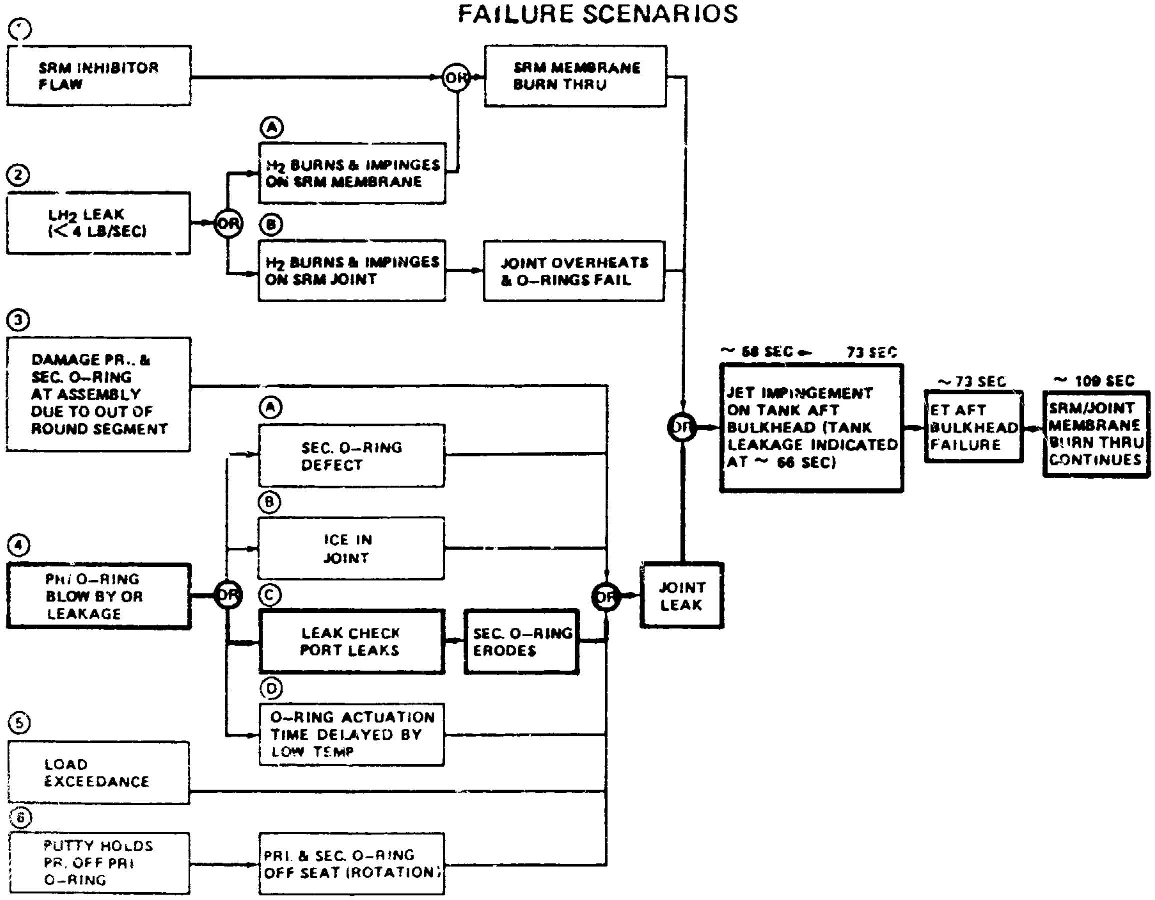
FINDINGS TO DATE

- O STS-51L EXPOSED TO APPROXIMATELY 7 INCHES OF RAIN DURING PAD STAY
- O POST INCIDENT INVESTIGATION REVEALED THAT WATER WAS PRESENT IN JOINTS OF STS-9 WHEN DESTACKED TO REPLACE AFT SEGMENT NOZZLE
- O TESTS SHOW EFFECTS OF ICE ON CLEVIS DIMENSION CHANGE ARE NEGLIGIBLE
- O ANALYSIS SHOWS THAT:
  - FREEZING OF WATER BENEATH COLUMN OF AIR BETWEEN TOP OF PIN AND SECONDARY O RING WILL NOT RESULT IN UNSEATING O RING (MAX. DELTA P APPROXIMATELY 2 PSIG)
  - WATER FREEZING BENEATH A COLUMN OF GREASE REQUIRED TO EFFECT SECONDARY O RING
- O SCENARIO OPEN PENDING FURTHER TEST RESULTS

[Ref. 3/7-74 1 of 2]

H–050

SRM HOT GAS LEAK FAILURE SCENARIOS



[Ref. 3 7-74 2 of 2]

FAILURE SCENARIO 4C

FAILURE MECHANISM

PRIMARY O RING BLOW-BY/LEAKAGE AND LEAK THRU THE LEAK CHECK PORT

ASSESSMENT METHOD

- O ANALYSIS TO CHARACTERIZE HOT GAS LEAK THRU LEAK CHECK PORT AND RESULTING EROSION/DAMAGE
- O ANALYSIS/TESTS TO CHARACTERIZE LEAKAGE RATES
- O TESTS TO ASSESS EROSION/DAMAGE WITH LEAKAGE THRU LEAK CHECK PORT
- O PHOTO ANALYSIS TO DETERMINE IF INITIAL LEAK (SMOKE PUFF) COULD BE FROM LEAK CHECK PORT

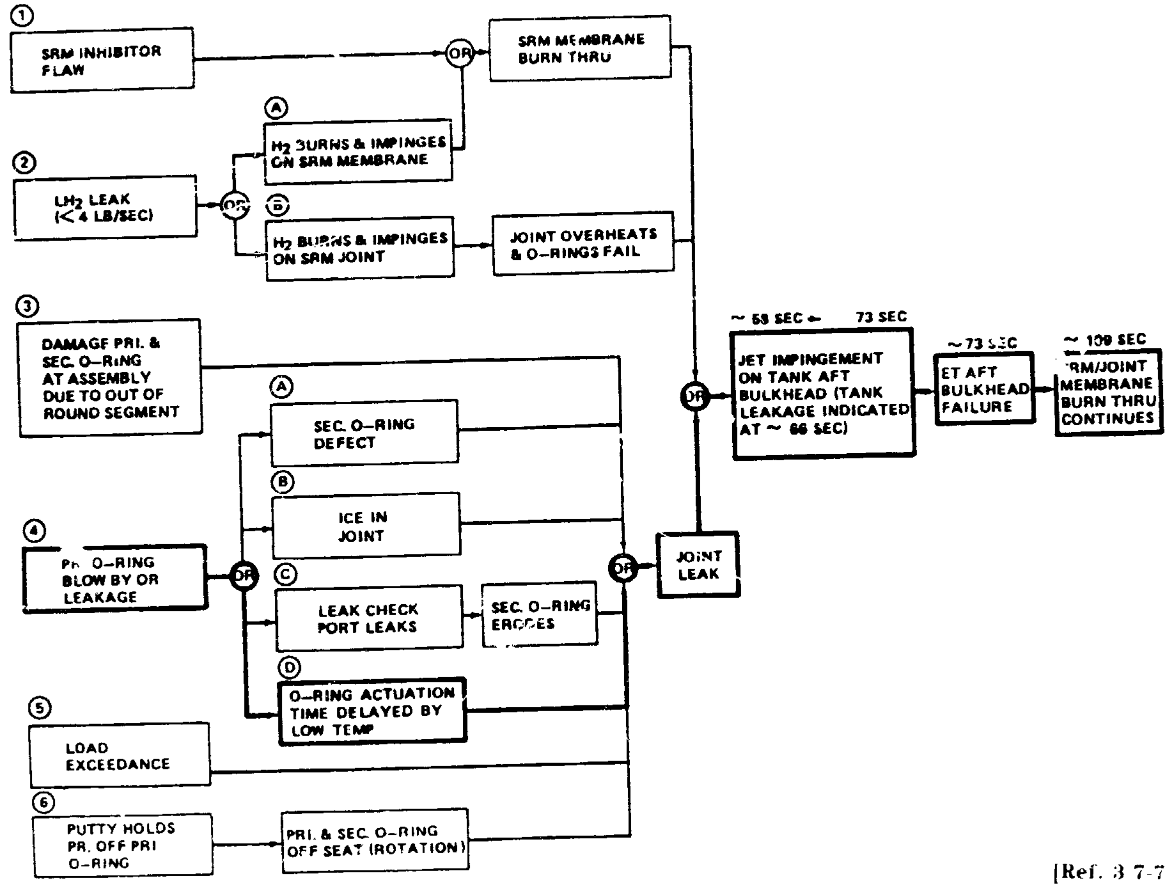
FINDINGS TO DATE

- O ANALYSIS INDICATES THAT SECONDARY O RING COULD BE OVERHEATED AND FAIL AT OR BEFORE 59 SEC.
- O PHOTO ANALYSIS TO DATE DOES NOT SUPPORT THE SMOKE PUFF FROM THE LEAK CHECK POINT - ANALYSES CONTINUING.

[Ref. 3 7-75 1 of 2]

H-050

SRM HOT GAS LEAK FAILURE SCENARIOS



[Ref. 3 7-75 2 of 2]

FAILURE SCENARIO 4D

FAILURE MECHANISM

PRIMARY O-RING BLOW-BY/LEAKAGE AND O-RING ACTIVATION DELAYED DUE TO LOW TEMPERATURE

ASSESSMENT METHOD

- O LABORATORY TESTS TO FURTHER QUANTIFY RESILIENCE OF O-RING VERSUS TEMPERATURE
- O ANALYSES AND TESTS TO ESTABLISH AMOUNT OF JOINT ROTATION FOR STS 51-L CONDITIONS INCLUDING EFFECTS OF PROPELLANT
- O DYNAMIC TESTS TO DETERMINE SEALING CAPABILITY WITH COMBINED EFFECTS OF O-RING PRESSURE ACTUATION, JOINT ROTATION AND RESILIENCY

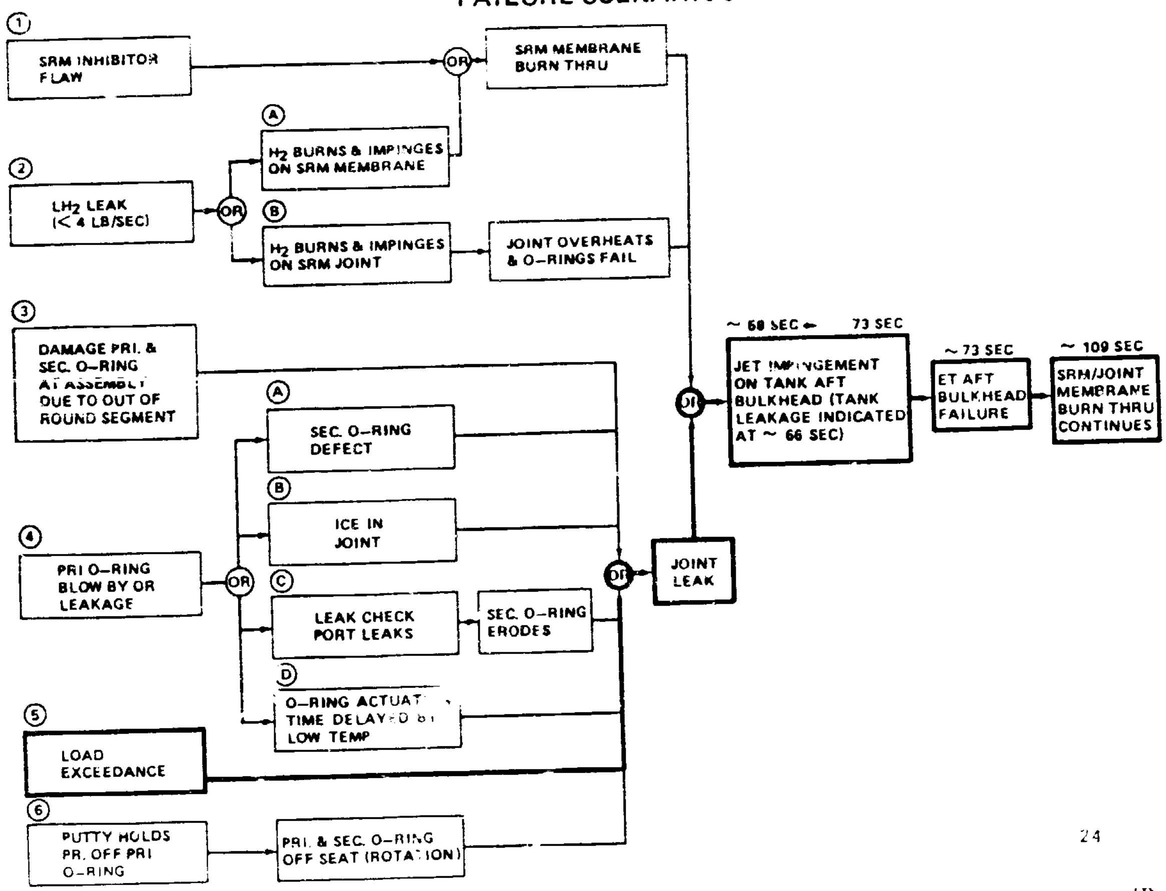
FINDINGS TO DATE

- O RESILIENCY TESTS SUBSTANTIATE INCREASED O-RING RESPONSE TIMES AS O-RING TEMPERATURE IS REDUCED
- O TESTS SHOW THAT COLD O-RINGS ($-10^{o}F$) WILL SEAL WHEN SUBJECTED TO MOTOR IGNITION TRANSIENT PRESSURES UNDER STATIC CONDITIONS
- O ANALYSES AND TESTS TO SIMULATE RIGHT HAND SRM AFT FIELD JOINT DYNAMIC PERFORMANCE UNDER STS 51-L CONDITIONS ARE REQUIRED

[Ref. 3/7-76 1 of 2]

H-050

SRM HOT GAS LEAK FAILURE SCENARIOS



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[Ref. 3/7/76 2 of 2]

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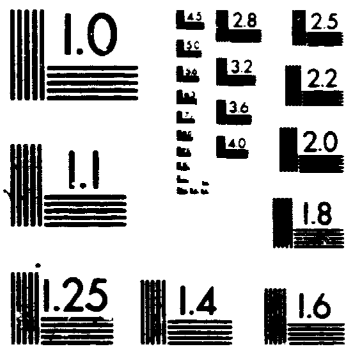


MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS
STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

FAILURE SCENARIO 5

FAILURE MECHANISM

INDUCED LIFTOFF OR FLIGHT LOADS DAMAGED JOINT OR PRODUCED EXCESSIVE JOINT ROTATION RESULTING IN FAILURE

ASSESSMENT METHOD

- O DETERMINE LOADS EXPERIENCED BY STS 51-L FOR ALL FLIGHT EVENTS
- O COMPARE STS 51-L LOADS WITH DESIGN CONDITIONS
- O CONDUCT SPECIAL JOINT DYNAMIC ANALYSIS TO EVALUATE INDUCED LOADS

FINDINGS TO DATE

- O STS 51-L INDUCED LIFTOFF LOADS ARE LESS THAN DESIGN LOADS BASED ON:
  - o CAMERA OBSERVATIONS OF TWANG MOVEMENT
  - o HOLDDOWN POST STRAIN GAUGE DATA
  - o RECONSTRUCTED LOADS BASED ON THRUST BUILDUP, WINDS, THRUST MISMATCHES AND VEHICLE DYNAMIC MODEL

[Ref. 3/7-77 1 of 3]

C-5

FAILURE SCENA...O 5 (CONT'D)

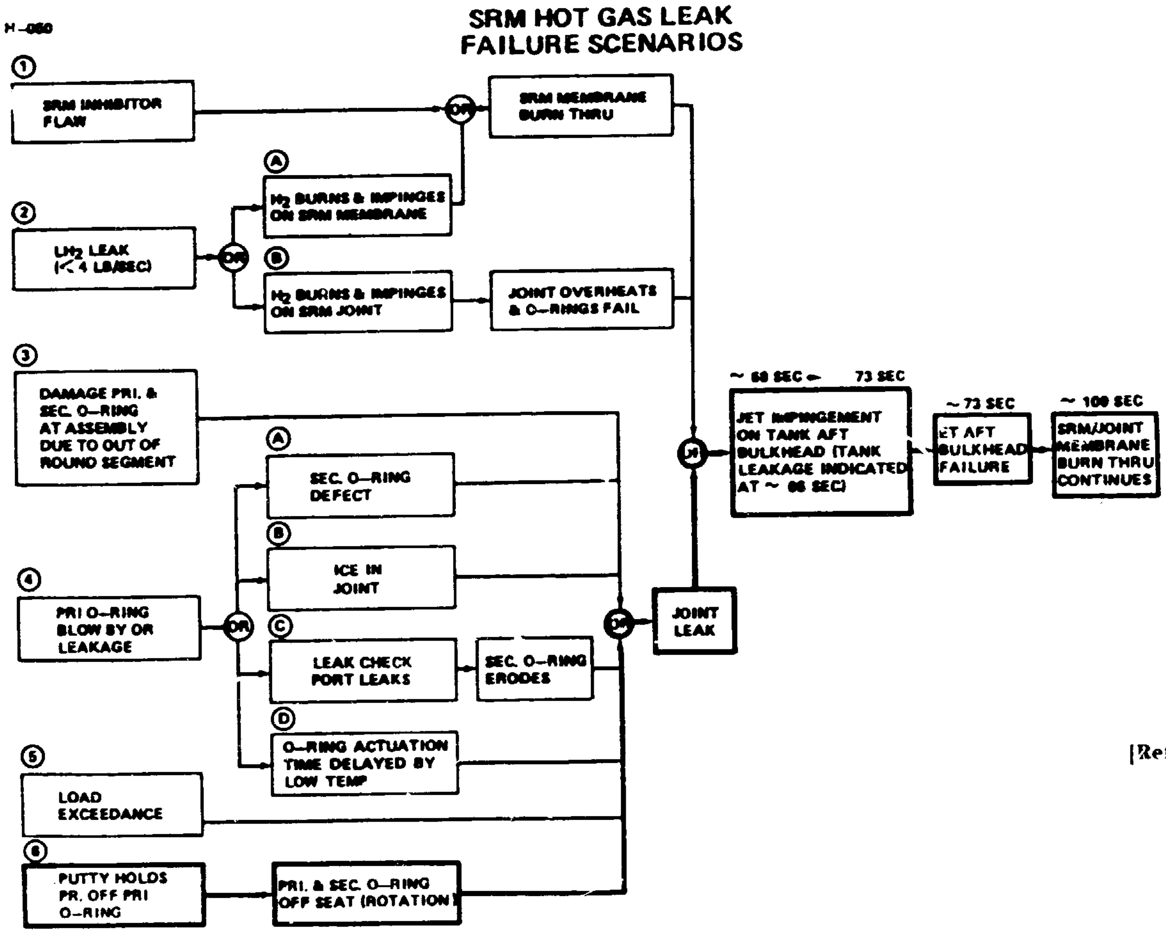
0 LIFT-OFF AND MAX "Q" LOADS WERE WITHIN DESIGN LOADS

- 0 AN OVERLOAD ON STRUCTURE DID NOT OCCUR

0 ANALYSIS CONTINUING TO DETERMINE EFFECT OF ACTUAL MAX "Q" LOADS ON GAP OPENING WITH POTENTIAL OF DEGRADED SEAL

[Ref. 3/7-77 2 of 3]

SRM HOT GAS LEAK
FAILURE SCENARIOS



[Ref. 3/7-77 3 of 3]

FAILURE SCENARIO 6

FAILURE MECHANISM

0 PUTTY HOLDS PRESSURE OFF AND PRIMARY AND SECONDARY O-RINGS ARE OFF SEAT (JOINT GAP OPENING COUPLED WITH RESILIENCY) WHEN PRESSURE APPLIED

ASSESSMENT METHOD

0 ANALYSES AND TESTS TO ESTABLISH AMOUNT OF JOINT ROTATION FOR STS 51-L CONDITIONS

0 TESTS TO EVALUATE PUTTY PRESSURE HOLDING CAPABILITY WITH VARYING GAP DIMENSIONS, ENVIRONMENTAL EXPOSURES PRIOR TO ASSEMBLY AND TEST TEMPERATURES

0 TESTS TO EVALUATE JOINT SEALING PERFORMANCE WITH SPACE BETWEEN O-RING AND SEALING SURFACE

FINDINGS TO DATE

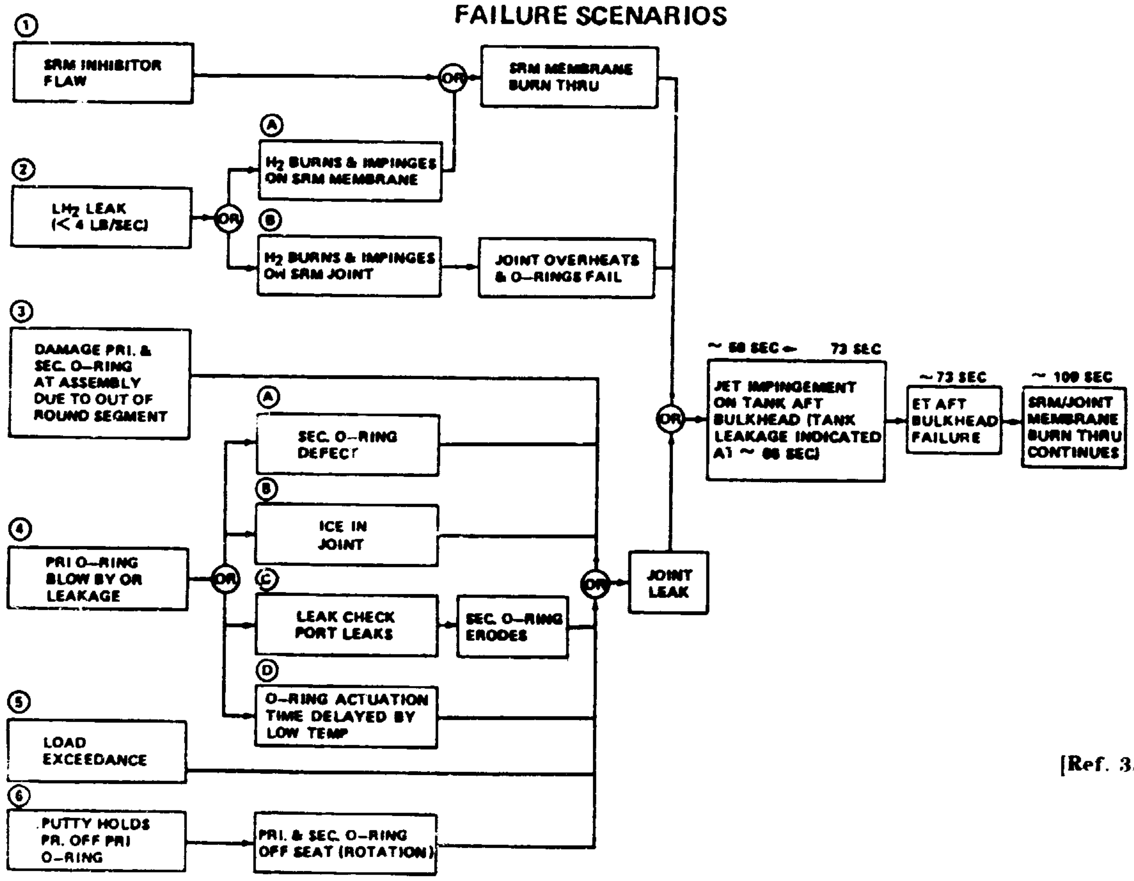
0 INITIAL TESTS INDICATE THAT PUTTY PRESSURE HOLDING CAPABILITY MAY BE SUFFICIENT TO SUPPORT THIS SCENARIO

0 ADDITIONAL TESTS, INCLUDING DYNAMIC EFFECTS OF THE JOINT, ARE REQUIRED

[Ref. 3/7-78 1 of 2]

H-060

SRM HOT GAS LEAK FAILURE SCENARIOS



[Ref. 3/7-78 2 of 2]

JOINT LEAK OBSERVED NEAR 59 SEC.

(EVENT COMMON TO SCENARIOS 3, 4, 5 AND 6)

FAILURE MECHANISM

0 POSTULATED MECHANISMS FOR SCENARIOS 3, 4, 5, OR 6 LEAD TO VISIBLE PLUME EMANATING FROM JOINT AT APPROXIMATELY 59 SEC. TWO POSSIBILITIES EXIST:

- o LEAK CONTINUOUS FROM NEAR IGNITION (OBSERVED SMOKE PUFF)
- o INITIAL LEAK CAUSED DAMAGE TO JOINT AND LOADS IMPOSED ON JOINT NEAR 59 SEC. RESULTED IN FAILURE

ASSESSMENT METHOD

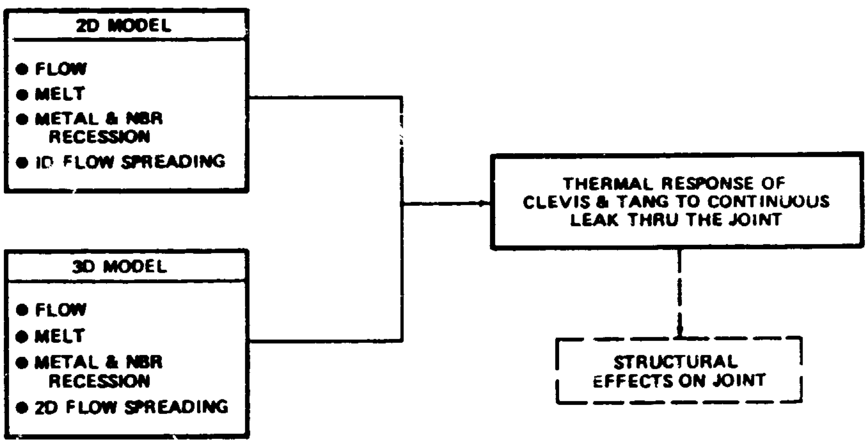
0 FLOW, THERMAL AND STRUCTURAL ANALYSES TO EVALUATE JOINT CAPABILITY WITH CONTINUOUS LEAK PATH

0 SUBSCALE MOTOR TESTS TO EVALUATE JOINT CAPABILITY WITH INDUCED LEAK PATHS

0 STRUCTURAL/DYNAMIC ANALYSIS OF JOINT WITH RECONSTRUCTED LIFTOFF AND FLIGHT LOADS

[Ref. 3/7-79 1 of 6]

THERMAL ANALYSIS OF FLOW THRU CLEVIS



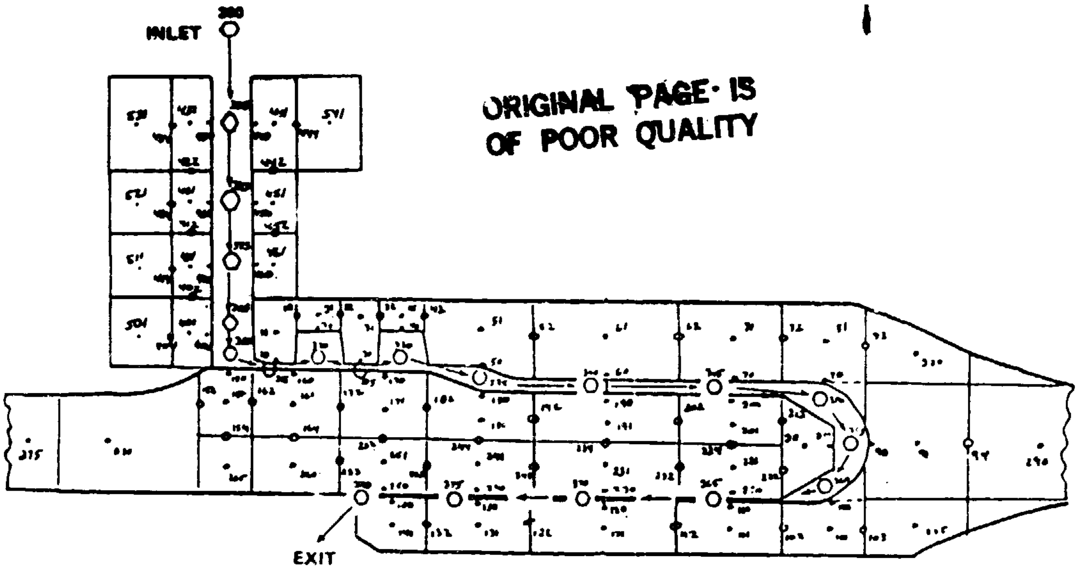
- PRELIMINARY ANALYSIS INDICATES AN EXTENDED LEAK IS NOT FEASIBLE
  - 2D MODEL SHOWS TANG BURN THRU IN 24 SECONDS
  - 3D MODEL SHOWS TANG BURN THRU IN 35 SECONDS

- ANALYSIS CONTINUING WITH
  - REVISED FLOW AND SPREADING PROFILES
  - 3D CONDUCTION EFFECTS
  - PARTICULATE DEPOSITION EFFECT (Al_2O_3)
  - MATERIAL STRENGTH AND FAILURE MECHANISM

[Ref. 2/7-79 2 of 6]

SRB CLEVIS 2D THERMAL MODEL

NODE LOCATION



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[Ref. 3/7-79 3 of 6]

JOINT LEAK OBSERVED NEAR 59 SEC. (CONT'D)

- O INITIAL SUBSCALE MOTOR TESTS INDICATE THAT INDUCED SEALING SURFACE FLAWS OR O-RING DEFECTS CAN RESULT IN:
  - O CONTINUOUS SMOKE FOR 24 SECONDS WITHOUT METAL DAMAGE
  - O INTERMITTENT FLOWS WITH NO METAL DAMAGE
  - O INTERMITTENT FLOWS WITH METAL DAMAGE
- O ADDITIONAL ANALYSES AND TESTS ARE REQUIRED

[Ref. 3/7-79 4 of 6]

PLUME EMANATING FROM SRM AFTER 59 SEC.
(COMMON EVENT FOR ALL SCENARIOS)

OBSERVATION

- 0 VEHICLE RATES AND TVC GIMBAL ANGLES WERE LARGER THAN PREDICTED

ASSESSMENT METHOD

- 0 ESTIMATE FORCE (AND LOCATION) AND MOMENT NECESSARY TO MATCH OBSERVED RESPONSE
- 0 EVALUATE PLUME CHARACTERISTICS USING FILM DATA TO DETERMINE VENT FORCES AND AERODYNAMIC INFLUENCES
- 0 RECREATE VEHICLE RESPONSE

FINDING TO DATE

- 0 RECONSTRUCTED RESPONSE AGREES WITH OBSERVED RESPONSE

[Ref. 3/7-79 5 of 6]

SUMMARY

- 0 NINE FAILURE SCENARIOS ARE POSTULATED FOR THE SRM HOT GAS LEAK
- 0 THE SRM INHIBITOR FLAW AND LOAD EXCEEDANCE SCENARIOS HAVE BEEN DETERMINED TO BE NOT PROBABLE
- 0 TESTS AND ANALYSES ARE CONTINUING FOR OTHER SCENARIOS

[Ref. 3/7-79 6 of 6]

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TESTIMONY OF: GARY COULTAS, ASSISTANT MANAGER, ORBITER PROJECTS, JOHNSON SPACE CENTER; AND GEORGE HOPSON, DIRECTOR, SYSTEMS ANALYSIS AND INTEGRATION LABORATORY, MARSHALL SPACE FLIGHT CENTER

MR. LEE: Mr. Chairman, would like now to summarize the three elements which we believe are non-contributors to this incident. If you remember the overall fault tree which Mr. Keel referred to, there were—each element was recognized as a potential contributor initially, and that would be the orbiter, the shuttle main engine, the IUS or inertial upper stage, the external tank, and the solid rocket booster.

We believe that, from the data that we have available to us—and we believe we have all of the available data—that three of those elements are non-contributors. And at this time I would for us to present to you in summary fashion the rationale for why we think that is the case.

And I would like now to introduce Mr. Gary Coultas from the Johnson Space Center to summarize for you that rationale for the orbiter.

CHAIRMAN ROGERS: Very well.

MR. COULTAS: I'm Gary Coultas. I'm the

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assistant manager for the orbiter project at the Johnson Space Center. I have held this position for about two and a half years. I have been with NASA for 22 years, and I have been involved with the orbiter project for a good deal of that time.

Let me start out with chart C-2.

(Viewgraph C-2.) [Ref. 3/7-80]

This identifies the basic configuration of the vehicle. The orbiter vehicle started out its career as static test article 099. We completed a test program, a structural test program, in 1979 to validate our stress analysis.

We knew going into that test program that we would ultimately make this into a flight vehicle. So as we went into that test program we took great care not to overstress the basic airframe. We did predict as we ran that test program what we would expect from the loading that we induced into the vehicle; what kind of responses we should see. We monitored those responses and we did not exceed those during the tests.

After that test program, we tore the vehicle down, installed systems and wiring and components, and it was delivered as a full operational vehicle in July of 1982. It flew nine flight missions from the period of April 1983 to October 1985.

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During that time period, we had a couple of problems that did not cause us any in-flight problems. These were turn-around issues post-mission. We had two problems with the OMS pods. The OMS pods are the propulsion modules on the aft of the vehicle. We had an ice impact problem which caused some loss of the thermal protection system in that area and also a bonding problem on 41-G. We had to remove those pods from the vehicle for repairs and other pods were substituted.

Also, after the 41-G mission, we lost a tile during that mission and subsequent analysis and review indicated that we had a problem with incompatibility between the waterproofing material that we were using and the tile bonding material. We subsequently replaced around 4,000 tiles on the bottom of the vehicle.

For STS 51-L, we did go back and review all of our documentation and vehicle processing records associated with the Challenger vehicle, which included the design configuration, the modification work that had been done, partial modifications that were in various stages of completion, waivers, deviations, et cetera.

We also had—the Challenger vehicle was one of the two vehicles that we were provisioning to carry Centaur cargo and payloads later this year, and those mods, they were not totally complete. We had been

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installing those over an incremental period of time. We made no mod specifically for Centaur during the flow for 51-L and those mods and the partial configuration were certified.

The bottom line in terms of the configuration and the hardware of the Challenger vehicle itself is that we found no anomalous conditions that would give us any concern relative to the pre-launch processing.

Chart C-3.

(Viewgraph C-3.) [Ref. 3 7-81]

What I would like to do now is go through the various functional areas of the vehicle and just give you some highlights of our findings and observations in these areas. And we have got a few acronyms in here, and I will try to cover those, and if I fail to give you the right information or there is still some misunderstanding, please don't hesitate to call out as we go.

These charts are set up—these next two charts are set up in a propulsion and power area. Starting with the orbiter maneuvering system, these are the maneuvering engines which are in the pods on the aft of the vehicle, that take us into orbit and take us out of orbit.

We evaluated all the measurements associated

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with those engines and that propulsion system. We found nothing anomalous with the data. One comment is that these OMS pods were originally built and used for the OV-102 Columbia vehicle, and this was their first use since STS-9 in October 1983.

They had been extensively reworked, with external TPS being put on, thermal protection system, and also the propellant tanks had been reworked since their prior use. And that is just a note. We found nothing anomalous in the data that would indicate any contributory cause there.

The reaction control system consists of numerous engines on the front and the aft of the vehicle that provide us with attitude control maneuvers in space and during re-entry. We reviewed all the measurements associated with those engines and the propulsion systems, and we also reviewed the flight photos.

All of that data again was nominal. These engines do show at around the 73 second time frame an onset of high pressures. They are basically monitoring or sensing the exterior pressure. Just prior to loss of data, some of those pressure indications were up to around 200 psi, indicating a pretty high overpressure on the vehicle.

We did have a flight anomaly. We lost a

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temperature sensor on one of the engines. It failed at liftoff. There was no mission effect from this.

Another interest item is that we have heaters on the thruster systems to provide thermal conditioning in orbit. We normally launch with the heaters ON on our vernier engine systems, but they are normally OFF on the primary thrusters.

Because of the cold weather, we did turn those thruster heaters on and we launched this way during ascent. We have done this also on the 51-C mission in January of 1985.

The reason that we turn these on is that we can, if the engine valves get cool you get shrinkage in the valve seats and you could get propellant leakage. And knowing this was a concern, we did turn these heaters on pre-launch and we flew that way.

The next subsystem is the auxiliary power unit, which consists of basically—is a hydrazine system that runs through a turbine to provide energy for the hydraulic pumps. Again, all of the measurements in this system are nominal. We did have—APU No. 3 did exhibit some differences, again within our experience base. APU 3 was new for this particular flight.

The hydraulic system provides hydraulic system for gimbaling the main engines, for the control valves

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on the main engines, and for the aero surfaces on the vehicle. We reviewed the fluid dynamic measurements, flows, pressures, temperatures, in that system, and again all were nominal.

We have a fuel cell system and a cryogenic storage system which supplies—basically generates electricpower for the vehicle. All measurements in that system were nominal, again nothing anomalous.

Chart C-4, continuing with the propulsion and power system.

(Viewgraph C-4.) [Ref. 3 7-82]

The pyrotechnic system that we have on board. We reviewed all of the measurements of the NASA standard devices, which are the electrical initiation devices for the pyros. We reviewed all of that data.

The orbiter to ET forward attach bolt was a new design that had a strain gauge in it, and we went back and reviewed all of this acceptance test data to make sure that there was nothing anomalous in that, since it was the first time we had flown that particular design.

Again, we reviewed the recovered debris and the flight photos. Our findings there were that there were no unintentional firing commands. All the hardware that we did find on recovery looked okay. We found no fired pyros, either electrically or thermally induced,

2155

in that area.

We also looked at the battery systems, primarily the SRB and ET, the SRB recovery batteries. And again, everything there was nominal from our perspective.

Chart C-5, the avionics areas.

(Viewgraph C-5.) [Ref. 3 7-83]

In the GN and C area, we had sensors on the SRB, SRB rate gyros. We had sensors on the orbiter, flight control sensors. These sense the characteristics of the vehicle. That data is assessed, messaged essentially through the on-board flight software. Commands then are sent to the SRB actuators and the SSME actuators, the effectors we call them, to steer the vehicle.

And that is the flight control loop, basically, that we fly the vehicle with. We reviewed all of the measurements associated with that system, both internal software measurements that are sent down to the ground in addition to the actual measured data from the sensors and the effectors that were positioned, and so forth.

We also did off-line simulations to confirm that the data was all consistent, and we again reviewed the flight photos. The system response for the flight

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control system for the whole stack, response is as designed.

And we have heard several discussions about transient behavior in certain periods of time, 40 seconds, 52 seconds, and near the end. In all of these areas, the flight control system was responding to the external stimuli, either wind induced or plume induced. And we see nothing anomalous in the behavior of how it did respond.

In the 62 to 65 second time period, we do have a wind-induced transient in that period of time and as you've heard also earlier, we have a contribution from an external force associated with the plume. We did see just before the vehicle breakup, acceleration spikes, positive and negative, and also, as you've heard earlier, the deviation between the right SRB and the rest of the stack in terms of what the rate gyros were saying.

Several interest items. That has been brought out before, but let me just amplify on that a little bit. We did have a slightly higher, two degree higher, max roll error. Now, as we're going uphill we are basically flying, and as we get a wind-induced—as we get wind shears, the vehicle will roll with the wind so it doesn't try to fight it.

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And in the past the most we have seen with that kind of a maneuver is about nine degrees. This time we went up to eleven. Again, it is consistent with our flight control design, but it is higher than we had seen before.

GENERAL KUTYNA: What time was that?

MR. COULTAS: Around the 40 second time frame.

Also, we have what we classify here as a hot SRB, which in our guidance philosophy what we are looking at is velocity as a function of time. And we have in our on-board guidance the ability to detect whether we're running a cold performance or a hot performance, so that we can limit the loads on the vehicle during the Max Q regime.

And so, because of variabilities in the SRB ballistic and performance characteristics, we actually monitor that in real time in the flight. And we have certain points in the trajectory that we will throttle down the main engines in anticipation of not exceeding our design criteria in the Max Q regime that would occur later on.

We would have predicted at this first throttledown point to go down to about 103 percent from 104. We actually, because we were fast or, so to say,

2158

hot in this time frame, we throttled down at the first throttle point to 94 percent and eventually down to 65 percent in the throttle bucket, and then back up again. We have seen this behavior many times before and it was unexpected, but not anomalous.

The data processing system, the DPS, consists of the—

DR. FEYNMAN: Do you mean you have an accelerometer so you can determine—

MR. COULTAS: We measure velocities with the IMU's as a function of time. We get inertial velocity and compare that with, at a given time—

DR. FEYNMAN: It's not rotational velocity; it's absolute velocity?

MR. COULTAS: Absolute velocity.

DR. FEYNMAN: So acceleration you measure?

MR. COULTAS: Yes, we're actually monitoring acceleration integrated over time, basically, to get the velocity. And we have velocity time points during the ascent trajectory, and if at a time point, if we are high in velocity, we throttle down. If we are low on velocity, we don't throttle down, basically to fly a velocity profile.

DR. FEYNMAN: Thank you.

MR. SUTTER: This could produce a different

2159

load on the mounts between the main tank and the solid rocket boosters?

MR. COULTAS: It is intended to control those loads, and so we actually take into account in real time what the performance is of the vehicle stack, so that later on that we limit the max dynamic pressure, and that is a measure of load, basically, on the vehicle, as we get further up into flight.

MR. SUTTER: But I was thinking of the case where the solid rocket motors don't change thrust much and the main engines you say are dropping thrust quite a bit. So that produces some kind of a bending load or a shear load on the mount between the solid rocket booster and the main tank, wouldn't it?

MR. COULTAS: Well, the main engines are designed to throttle and the whole stack is designed to follow a thrust time or velocity time profile, basically. And yes, it does change from, you might say, a nominal.

But again, the flight base or our experience base or our design base takes that into account, and so we have an envelope that we can fly within.

MR. SUTTER: I wasn't thinking of the orbiter as much as I was thinking of what happens to that structure between the main tank and the solid rocket

2160

booster. And we keep looking at that joint and wondering why it did perform the way it did. This could be another item that added to changing its load.

MR. COULTAS: Well, as we are looking at, as Marshall and ourselves are jointly looking at the total load profile through the mission, and that in a reconstruction of the real loads, this is automatically taken into effect, the way we flew it on the day we flew it, basically.

And a loads calculation or a loads profile all the way up is being developed.

Again, the bottom line is that those loads as we see them today are well within our design base, except, and as Marshall has indicated, they need to understand, even with the low loads with an anomalous joint, is there some combined effect that it could be a problem or an issue.

The data processing systems is our on-board software system, consisting of our general purpose computers, and they're basically interfaced with the rest of the vehicle through what we call MDM's. We have again the software, the operating system software which keeps our four machines in synchronization, and then the displays, the crew, and the keyboard functions that the crew can put in.

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Again, in that system all the measurements were nominal. We had no errors on those data buses and so forth. Everything was nominal.

We did put in a program, a special precaution to the recovery crews that if we recovered any computer that we would keep it immersed in salt water until we could get at it. There are some non-volatile memory in those computers that we could perhaps preserve, and so that standing statement is out to the recovery crews.

The display and control system, electrical power distribution system, are all nominal. The communications and tracking system, again everything was nominal there. We did have a couple of data dropouts during the flight, again not anomalous. We have seen these many times before.

The dropout at four seconds is, we believe, due to an antenna. We have to select and de-select antennas as we are flying, depending upon where the ground station is and also to avoid the attenuation through the plumes.

And the one second dropout we also believe was due to plume attenuation.

Instrumentation system, again everything is nominal there. We also got a special request out to the recovery team: Any tape recorder which is recovered, we

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have special procedures for recovering the tape and drying the tape in a controlled manner so that we can recover any data off of that.

Of special interest there is a data system that we have that is not telemetered, which has got a lot of environmental data in the cargo bay and external to the vehicle. We have not gotten those back.

(Viewgraph C-6.) [Ref. 3 7-84]

Chart C-6 is our structures area, consisting of the primary structure, purge vent and drain system. We have to vent this vehicle as it goes, as we go into space. We have to repressurize the structure, the cargo bay, and the wings, et cetera, as we re-enter. And we have special doors that open up at certain times in the flight that do this.

We also have included in this system all of our thermal subsystem, our tiles, and so forth. Looking at all of the measurements we have in these areas, the hardware mods that we made specifically for this mission, and again the flight loads, we see nothing anomalous within any of that data.

(Viewgraph C-7.) [Ref. 3 7-85]

Chart C-7, our mechanical subsystem area, consisting of doors, payload bay doors, the vent doors which I mentioned, which are up and down the sides of

2163

the vehicle, the main landing gear and nose landing gear doors, the retention system—these are the latches which hold the cargo in the cargo bay, also a latch-down for the remote manipulator arm.

We also have in that system, mechanical system, at the umbilicals, the large umbilicals, feeding the propellant into the vehicle. And we also have the hatches on the vehicle, the side hatch and overhead emergency egress hatch.

All of the data that we can see there and the flight photos indicate everything is nominal there. One point of interest is that all of the umbilicals between the orbiter and the external tank and between the external tank and the right and left SRB's were all mated until the loss of signal of all of the data. So there was no premature separation of any of the umbilical functions.

DR. FEYNMAN: I thought there was some fussing about with the hatch door.

MR. COULTAS: We had on the launch attempt on the previous day, we did have a problem with the hatch. We have readouts that tell us whether all of the hatch latches are all down, and we had a readout problem with one of those. And we also had a problem of getting a GSE, a ground support handle, off of the hatch on the

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prior day.

We have looked at that and we see nothing in that information that would cause us any concern. Actually, we're going to fix the problem that we have had with the GSE handle, but on the day that we did launch it was not an issue.

CHAIRMAN ROGERS: Thank you, Mr. Couitas. We appreciate it.

Mr. Hopson.

MR. HOPSON: Mr. Chairman, Commission members:

I'm George Hopson. For the past four years, I've been assigned as director of Systems Analysis and Integration Laboratory at Marshall.

CHAIRMAN ROGERS: Mr. Hopson, would you move your microphone over a little bit.

MR. HOPSON: Prior to that time, I was assigned as director of the Systems Dynamics Laboratory. My present assignment is chairman of the SSME investigation with regard to flight 51-L.

Could I have the second chart, please.

(Viewgraph H-2.) [Ref. 3/7-86]

Here are some of the characteristics of the shuttle main engine, and I will just say it is capable of about a half a million pounds of thrust and it is

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throttleable down to 65 percent.

The next chart, please.

(Viewgraph H-3.) [Ref. 3/7-87]

Our first task on our team was to verify, based upon our ground test experience and using the telemetry data that we received from Challenger, that we would be able to discern an engine failure. And we did verify that capability.

The data that we get, the telemetry data from Challenger, is 25 samples per second, and we did verify that we could pick up, based upon our past experience, an engine problem with that sample rate.

We reviewed all of the data during all phases of the engine operation for any sign of malfunction or degradation in performance, and I will report on these findings subsequently.

We also re-reviewed the records of the pre-flight condition of the engines and found no omissions or discrepancies.

Lastly, we inspected the recovered engine hardware to compare the condition of the engines with what the telemetry data had indicated, and I will report on that also.

(Viewgraph H-4.) [Ref. 3/7-88]

Addressing pre-flight, there are several

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measurements that we can use that we do monitor pre-flight, that have to do with temperature. One thing that we are looking for in the engine compartment is any evidence of a propellant leak, where abnormally low temperatures would be indicated by these measurements that are in the engine compartment. We found no evidence of any propellant leak in the compartment.

We also look at component temperatures to make sure that they are within the limits that those components are designed and have been demonstrated to operate at. All components within the engine compartment were within acceptable temperature limits.

The next chart, please.

(Viewgraph H-5.) [Ref. 3/7-89]

I would like to look now at the start transient. This curve shows the chamber pressure buildup for all three of the Challenger engines during the start. The outer bounds that you see there are the chamber pressures which are acceptable as a function of time during start, and you can see that all three Challenger engines came up well within the acceptable bounds.

So there was nothing abnormal whatsoever about the start.

The next chart, please.

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(Viewgraph H-6.) [Ref. 3/7-90]

I have taken a little bit different tack on this one. This is also a start transient chart. One of our most important measurements as far as determining the health of the engine is the discharge temperature from the high pressure fuel pump.

These three curves that you see on this chart are those type measurements. But rather than being the three Challenger engines, I have picked engine number one from—they are all Challenger engines, but only one of them is from flight 51-L. The other two are from the two preceding flights.

So the message from this curve is—well, there's two messages. One, that performance has been very consistent during those flights, one flight to another; and the other is it is a very satisfactory start as far as those temperatures are concerned.

The next chart, please.

(Viewgraph H-7.) [Ref. 3/7-91]

Now we want to go to—we have looked at the transient and we want to look at the main stage performance, that is after we get up to power. And as it was stated earlier, we start and we go up to 100 percent, and we stayed at 104 percent for a while and then dropped down to, what was it, 95 or 96 percent, and

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then down to 65, and then back out of the start bucket up to 104 percent.

Now, for the first 73 seconds we have got something like—we've got something like 115 measurements of each engine, and we get a measurement every 40 milliseconds, and so we get 25 samples per second from that number of measurements, plus we have some orbiter measurements that we also look at, and we looked at in great detail every measurement during not only this main stage operation, but the transient and naturally during the incident.

Everything was nominal up until about 73 seconds, where this curve goes to—

MR. SUTTER: Is this engine versus thrust time programmed? Is that automatic?

MR. HOPSON: Yes, that is a closed loop control system.

MR. SUTTER: How many flights other than this one did you drop down to the 65 percent? Is that usual?

MR. HOPSON: Every one of them. On these particular engines, two of the engines, this was the fifth flight. We had four previous flights, or was it six? I think we had four previous flights on two of the engines and three previous flights on one of them.

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And in all cases we dropped down to 65 percent during the Max Q regime.

The next chart, please.

(Viewgraph H-8.) [Ref. 3/7-92]

Now, this shows, this is an expanded chart and the whole chart only covers one second, and this again is of our high pressure fuel turbopump discharge temperature. Our red line limit, as you can see, is [illegible]60 degrees roentgen. That red line, if we exceed that red line the controller automatically shuts that engine down.

And you can see that up until—up until 73 seconds—well, in fact past 73 seconds, the data looks very normal and that is what it is supposed to look like, what it looks like here.

We did notice at a little bit before 73 seconds that we had a decrease in oxygen and hydrogen inlet pressure to the engines, and then about two-tenths of a second later than that, then we started seeing things happening. We saw the flash between the orbiter and the external tank, and we saw the turbine discharge temperature start to go up.

Now, this is blank here, that was during the explosion of the external tank and the data was blacked out during that period of time. We didn't get anything during that period of time, but then we did

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pick the data up again a few milliseconds later. And as you can see, it first approaches and then exceeds the red line.

Now, we've got indication that the way the shutdown goes, the computer makes a cycle every 20 milliseconds, and so when that computer gets an indication that this red line on an engine has been violated, that is what we call one strike. That is one indication.

The computer has got to have three consecutive indications of violation of a red line and then it gives the orbiter a signal—or the controller shuts that engine down. So on engine number one, we verified that we got three strikes and that the engine had been—the shutdown had been initiated.

On the other two engines, we got indications that at least one strike had occurred on both of the other two engines. And so we were in the process of shutdown on all three of those engines, and that is the way it should have been.

When you see something like this, when you see those temperatures going up to the red line and exceeding it, what that is indicative of is a LOX-rich shutdown. Whenever the LOX supply gets too high, that is when the temperatures go up and that is when we burn

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the internal parts, the hot gas parts of the engine.

And we have seen this in test stand accidents before, and so we would expect, having seen a curve like this, that the engine would pretty well be gutted in the hot gas regions of the engine.

And the next chart, please.

(Viewgraph H-9.) [Ref. 3/7-93]

When we looked at the recovered hardware, that is in fact what did happen.

I would say first that all of the engines were recovered in close proximity to each other, and in fact looked like that they were still attached to the thrust structure and the bulkhead at water impact. The bearings, the gimbal bearings, had failed in an overload mode and so the engines and the thrust structure did appear to be together.

In other words, it didn't blow up and scatter the parts over a wide distance.

All of the fractures were looked at, and the fractures all had the appearance of ductile overload. That is, overload due to like impact from another piece of structure or impact, more likely, impact with water; and that there was no engine explosions involved.

The engines did have burn-through damage in the hot gas regions due to these expected internal

2172

over temperatures. All three high pressure fuel pumps were burned through.

One thing of particular interest to us, there was no—the housing was burned through, but it was not ruptured or burned through in the vicinity of the turbine blades. And so that told us that the turbine blades—that there was no turbine blade came loose and damaged anything, but they got burned off.

And so I guess in summary, we have looked at the data in great detail. The engine operated perfectly normally, well after, in fact 14½ seconds after, flame was observed coming from the side of the SRB and several other events along those same lines. Engine performance was perfectly nominal. In fact, the engines were even running after the external tank had exploded.

And so our findings are that the engines were in no way associated with the failure of flight 51-L.

CHAIRMAN ROGERS: Thank you very much, Mr. Hopson.

Mr. Lee.

MR. LEE: Now, Mr. Chairman, I would like to now discuss the inertial upper stage. I was not the chairman of this particular working group. The results seem reasonably straightforward. So with your permission, I would like to present those results.

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The approach to this investigation was similar to the ones that you have heard before. In fact, each of these elements were almost identical, and in the case of the inertial upper stage the team was made up of NASA personnel, Air Force Space Division personnel, the Boeing Company, the prime contractor for the inertial upper stage, and the Aerospace Corporation.

We reviewed every technical discipline, as has been indicated in the previous reviews.

(Slide.) [Ref. 3/7-94]

What you see now on the screen is the inertial upper stage with the tracking and relay data satellite attached forward. To give you an example, it is made up of really three major elements: the satellite, the black or darker portion forward; the white portion with "USA" on it is the two-stage inertial upper stage; and then the third element and very significant element is the airborne support equipment, and that is the equipment which hold the IUS or inertial upper stage in the orbiter bay. It's very critical to the entire operation for launching and separation at orbit.

Now, to give you an idea of the dimensions, the stage is approximately 17 feet in length, about 9½ feet in diameter. The payload, that total IUS or the stage weight is approximately 35,000 pounds, and the

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satellite is about 5,000 pounds.

Could I have the next viewgraph, please.

(Viewgraph J-2.) [Ref. 3/7-95]

MR. LEE: This is a continuation of the fault tree you saw initially, where the three major areas that we believe that could contribute to a cargo or inertial upper stage failure would be: one, a premature ignition of the propulsion system; two, an explosion or fire due to other reasons in the bay; and three, element separation.

And I would like to address each of those if I could. In the case of premature ignition, there is three different types of premature ignition we believe we can get.

First let me say that the inertial upper stage is relatively quiescent relative to—during the ascent phase of flight, meaning we have little or no electronics or avionics on, we have little or no instrumentation coming back to ground during that phase of flight. And so what we are having to construct this with with our rationale for acceptance is based on other data, plus other instrumentation data, flight data, plus recovered hardware.

We have been very fortunate in recovering a sufficient amount of the stage to be able to prove our

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rationale or support our rationale.

On the premature ignition of the stage, we found three different areas that could postulate three different types of ignition: one would be from an electrostatic discharge; an inadvertent ignition command that would ignite the motor; and an auto-ignition. And if I might, I would like to talk about each of those briefly to give you our rationale for why we think that we didn't have this case.

I will take the premature ignition first. The orbiter payload bay does in fact have a number of temperature sensors in the bay, and we believe that if we had ignited the stage that we would have had a tremendously high temperature rise rate, which would have been detected by those temperature sensors. As we progressed through the flight, those temperatures remained stable throughout the entire phase of flight until we lost signal.

The second is, if we had ignited that stage we would have no doubt had a tremendous shock within the bay. The orbiter instrumentation indicates no such shock. Plus, the payload, the tracking and data relay satellite, does in fact have an instrumentation which would allow you to be able to detect motion on that stage, which is being transmitted back during the ascent

2176

phase, and we saw nothing on that telemetry.

The main thing I guess would be the inspection of the recovered hardware, which included the alarming devices, igniters, and we found quite a bit of unignited propellant. So that the recovered hardware plus that amount of instrumentation leads us to believe that we did not have a premature ignition during that phase.

The second is the explosion or fire, and we have postulated the only possibility for being able to get a fire other than a premature ignition would be the reaction control system failure, a battery failure, an explosion of a battery, say, some other cause for the solid rocket motor propellant to become—to burn, and that may be from a shock, electrically induced fire, or some—or radio radiation that caused some activity, some activation which we didn't anticipate.

In each of those cases, we found that on the recovered hardware, that the place where the areas where this sort of thing would happen there was no fire indication. The same temperature sensors that I referred to before in the payload bay gave no indication, obviously, of overheating, and so we believe there was no fire from any other source in the bay.

The third category or third area for suspect would be the separation of the elements, and the

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separation of the elements we are talking about here is the entire payload separating from the orbiter some way inadvertently, the two stages separating from themselves and the payload separating from the stage.

Based on the same data, telemeter data we got back from the orbiter that I mentioned earlier, we saw no indication of any abnormal movement of the stage in the bay. We have electrical power going from the orbiter into, throughout the inertial upper stage to the payload, and we believe if there had been any separation of any of that we would have lost continuity electrically.

In addition, in the recovered hardware we see no evidence of any abnormal separation between those stages. Based on—and this is a brief summary, but based upon those findings—and we were quite lucky to get as much of the hardware back which proves this point—we believe that the inertial upper stage and the attached TDRS or the tracking and data relay satellite did not contribute to this incident.

CHAIRMAN ROGERS: Thank you very much, Mr. Lee.

Well, let me express our appreciation for these reports, which have been very helpful. I would like to ask, do you plan to have a written report in

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addition to what you presented here today?

MR. LEE: Yes, sir. We will have a separate independent report. We will probably, in the case of the inertial upper stage and the shuttle main engine, we are probably less than a week away, and I would have to defer to Mr. Coultas on the orbiter.

MR. COULTAS: The same on the orbiter. We plan to have a report out by the end of next week, really.

[Laughter.]

CHAIRMAN ROGERS: You don't think you could get that down to five days?

[Laughter.]

CHAIRMAN ROGERS: Well, thank you very much. We appreciate it.

That concludes the meeting for today. Thank you.

(Whereupon, at 4:00 p.m., the Commission was adjourned.)

STS Orbiter and GFE Projects Office
Johnson Space Center Houston Texas

STS 51-L ORBITER TEAM	Presenter Gary Coultas
CONFIGURATION	Date 3/5/86

- **HISTORY**
  - BASIC AIRFRAME WAS ORIGINALLY A STATIC TEST ARTICLE (STA-099)
    - TEST PROGRAM COMPLETE IN OCTOBER 1979 TO VALIDATE STRESS ANALYSIS
    - TEST CONSTRAINTS IMPOSED TO PRESERVE FLIGHT INTEGRITY
      - LOADS LIMITED TO 1.2 x DESIGN
      - STRAINS PREDICTED PRE-TEST AND WERE NOT EXCEEDED
    - DETAIL INSPECTION AND TEAR-DOWN COMPLETED POST-TEST
  - DELIVERED AS A FULL-UP OPERATIONAL VEHICLE IN JULY 1982
  - NINE FLIGHT MISSIONS
    - APRIL 1983 - OCTOBER 1985
  - SIGNIFICANT PROBLEMS
    - OMS POD DAMAGE — STS 41-B, STS 41-G
    - TILE BOND REPAIR — STS 41-G
- **STS 51-L**
  - RECORDS AND DOCUMENTATION SUPPORTING THE VEHICLE PROCESSING AND MISSION WERE RE-REVIEWED
  - INCLUDES: DESIGN CONFIGURATION, MOD DEFERRALS, PARTIAL MODS, WAIVERS/DEVIATIONS, PROBLEM REPORTS, CERTIFICATION, COMMITT-TO-FLIGHT ANALYSIS
  - CENTAUR: PARTIAL CENTAUR MODS WERE INSTALLED PRIOR TO STS 61-A
    NO ADDITIONAL MODS WERE ADDED FOR STS 51-L
    THE AS-FLOWN CONFIGURATION WAS CERTIFIED
  - NO ANOMALOUS FINDINGS

[Ref. 3/7-80]

STS Orbiter and GFE Projects Office

Johnson Space Center Houston Texas

STS 51-L ORBITER TEAM

PROPULSION & POWER

Presenter Gary Coultas

Date 3/5/86

SUBSYSTEM	EVALUATION	FINDINGS	REMARKS
OMS	ALL MEASUREMENTS	ALL NOMINAL	OV-102 OMS PODS - FIRST USE SINCE STS-9 AND MAJOR REWORK
RCS	ALL MEASUREMENTS EXCEPT VALVE COMMANDS. FLIGHT PHOTOS.	NOMINAL PC SENSORS SHOW EXPLOSION ONSET VERNIER THRUSTER RSD TEMP SENSOR FAILED AT LIFT-OFF (NO MISSION EFFECT)	PRIMARY THRUSTER HEATERS WERE ON DURING ASCENT DUE TO COLD WEATHER; NORMALLY OFF
APU	ALL MEASUREMENTS STS 61-A DATA	ALL NOMINAL APU-3 EXHIBITED SEVERAL DIFFERENCES FROM OTHER APU'S (NO MISSION EFFECT)	APU-3 WAS NEW
HYD	ALL FLUID DYNAMIC MEAS; SELECTED TEMP MEAS STS 61-A DATA	ALL NOMINAL	
FUEL CELL/CRYO	ALL MEASUREMENTS	NOMINAL	

[Ref. 3/7-81]

STS Orbiter and GFE Projects Office

Johnson Space Center Houston Texas

STS 51-L ORBITER TEAM

PROPULSION & POWER

Presenter Gary Coultas

Date 3/5/86

SUBSYSTEM	EVALUATION	FINDINGS	REMARKS
PYRO	ALL MEASUREMENTS RADIOGRAPHS OF NASA STANDARD DEVICES ORB/ET FORWARD BOLT ACCEPTANCE DATA RECOVERED DEBRIS FLIGHT PHOTOS	NO UNINTENTIONAL FIRING COMMANDS ALL HARDWARE OK	
BATTERIES	ALL MEASURMENTS FLIGHT PHOTOS	ALL NOMINAL	

[Ref. 3/7-82]

STS Orbiter and GFE Projects Office

Johnson Space Center Houston Texas

STS 51-L ORBITER TEAM

AVIONICS

Presenter Gary Coultas

Date 3/5/86

SUBSYSTEM	EVALUATION	FINDINGS	REMARKS
GN&C SRB + ORB SENSORS SRB + SSME EFFECTORS SOFTWARE	ALL MEASUREMENTS OFF-LINE SIMULATIONS FLIGHT PHOTOS	SYSTEM RESPONSE AS DESIGNED PITCH TRANSIENT IN 62-65 SEC PERIOD YAW ACCELERATION SPIKE AT 72 SEC RSRB RATES OPPOSITE TO LSRB & ORB AT 72 SEC	INTEREST ITEMS (NO MISSION EFFECT) 2° HIGHER MAX ROLL ATTITUDE ERROR 94% THROTTLE (HOT SRB)
DPS GPC/MDM SOFTWARE CRT/KEYBOARD	ALL MEASUREMENTS	ALL NOMINAL	SPECIAL HANDLING OF ANY RECOVERED GPC
D&C	ALL MEASUREMENTS	ALL NOMINAL	
ELEC PWR	ALL MEASUREMENTS	ALL NOMINAL	
COMM & TRACK	SELECTED MEASUREMENTS (ACTIVE EQUIPMENT)	NOMINAL DATA DROP-OUTS (NO MISSION EFFECT) 4 SEC @ 9 SEC MET 1 SEC @ 60 SEC MET	
INSTRUMENT	ALL MEASUREMENTS	ALL NOMINAL	SPECIAL HANDLING OF ANY RECOVERED RECORDER

[Ref. 3/7-83]

STS Orbiter and GFE Projects Office

Johnson Space Center Houston Texas

STS 51-L ORBITER TEAM

STRUCTURE

Presenter	Gary Coultas
Date	3/5/86

SUBSYSTEM	EVALUATION	FINDINGS	REMARKS
STRUCT PRIMARY PV & D THERMAL	ALL MEASUREMENTS HARDWARE MODS FOR 51-L FLIGHT LOADS ANALYSIS STS 61-A, 51-F TEMP PROFILES	ALL NOMINAL LOADS WITHIN EXPECTED ENVELOPES	

[Ref. 3/7-84]

STS Orbiter and GFE Projects Office

Johnson Space Center Houston Texas

STS 51-L ORBITER TEAM	Presenter Gary Coultas
MECHANICAL	Date 3/5/86

SUBSYSTEM	EVALUATION	FINDINGS	REMARKS
DOORS PLB VENT MLG, NLG RETENTION P/L RMS LO2/LH2 UMB HATCHES SIDE OVERHEAD	ALL MEASUREMENTS FLIGHT PHOTOS	ALL NOMINAL ALL UMBILICALS MATED UNTIL LOS	

[Ref. 3 7-85]

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SPACE SHUTTLE MAIN ENGINE

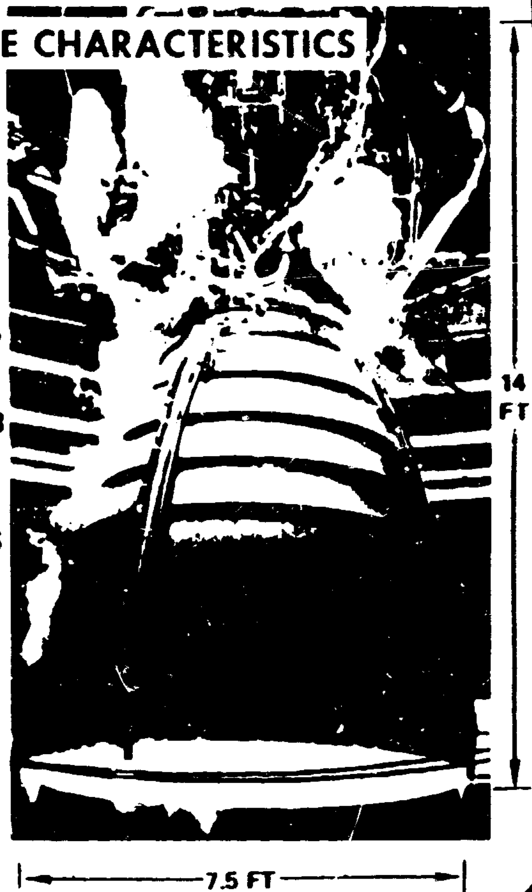
GEORGE HOPSON

MARCH 7, 1986

[Ref. 3 7-86 1 of 2]

SPACE SHUTTLE MAIN ENGINE CHARACTERISTICS

■ ENGINE OPERATION	MPL	RPL	FPL
■ VACUUM THRUST, POUNDS	305K	470K	512K
■ CHAMBER PRESSURE, PSIA	1950	3006	3270
■ AREA RATIO			77.5
■ VACUUM SPECIFIC IMPULSE (NOMINAL)			455.3
■ MIXTURE RATIO			6.0
■ LIFE	7.5 HOURS, 55 STARTS		
■ SPECIFICATION DRY WEIGHT, POUNDS			6958



SC308-551G

[Ref. 3/7-86 2 of 2]

SPACE SHUTTLE MAIN ENGINE

- O TEAM INVESTIGATION APPROACH
  - o REVIEWED ALL PRIOR ENGINE FAILURES TO VERIFY ENGINE FLIGHT DATA PROVIDED EVIDENCE OF OBSERVED FAILURE
  - o REVIEWED ALL STS 51-L DATA
    - o PRELAUNCH
    - o START
    - o MAINSTAGE
  - o REVIEWED EACH DATA FRAME (EVERY 40 MS)
  - o REVIEWED CHALLENGER ENGINES' HISTORY INCLUDING HARDWARE CHANGES AND ANOMALIES
    - o MANUFACTURING
      - o RE-REVIEW OF MR'S
    - o ACCEPTANCE TESTS
    - o PRIOR FLIGHTS
  - o INSPECTED RECOVERED HARDWARE

[Ref. 3/7-87]

SPACE SHUTTLE MAIN ENGINE

PRELAUNCH ENGINE COMPARTMENT TEMPERATURES

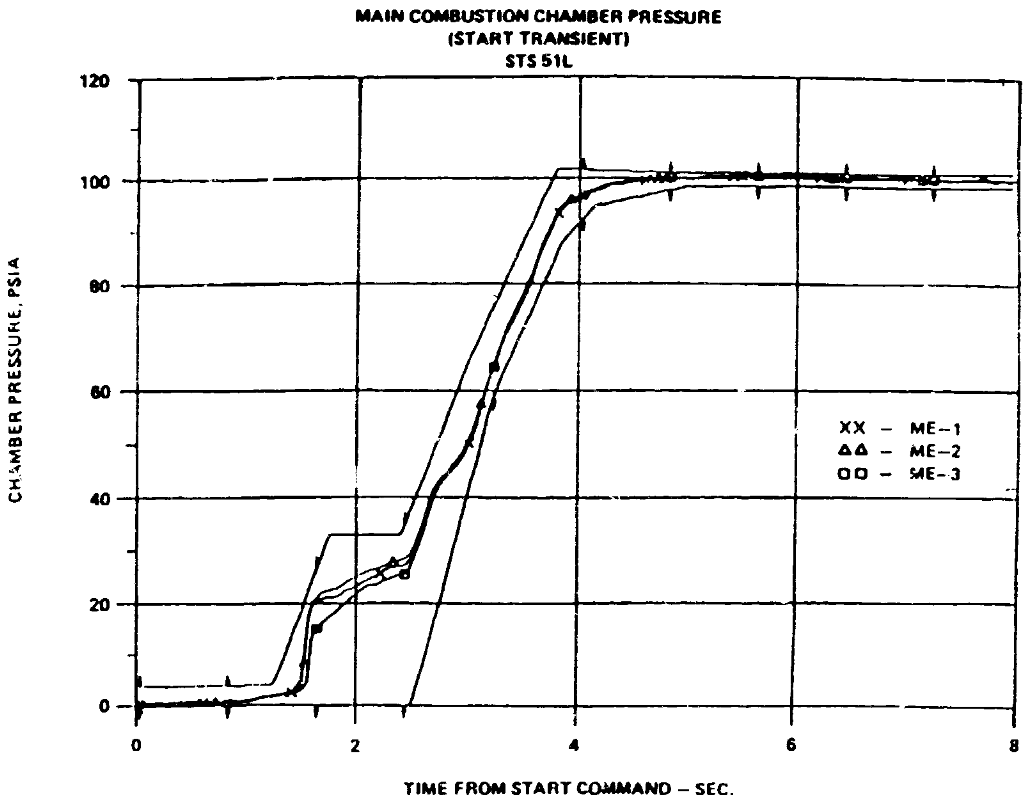
0 MEASURED COMPONENT TEMPERATURES WELL WITHIN ACCEPTABLE LIMITS.

0 NO INDICATION OF PROPELLANT LEAKAGE.

[Ref. 3/7-88]

H-083

SPACE SHUTTLE MAIN ENGINE

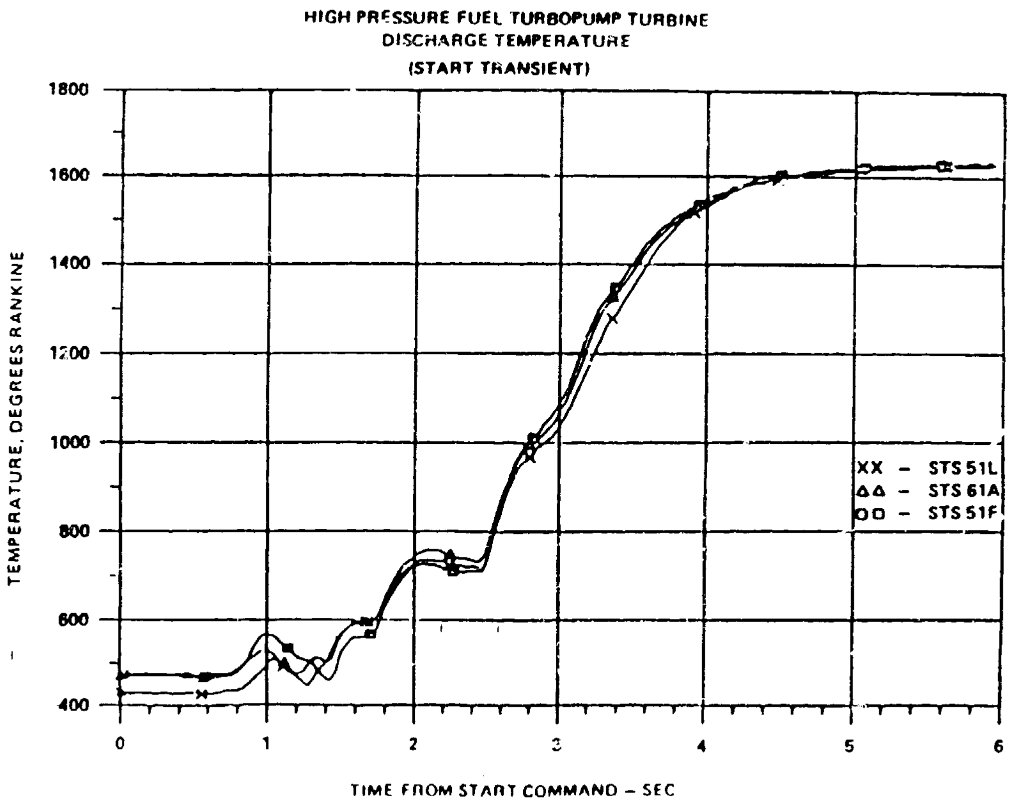


4

[Ref. 3 7-89]

H-082

SPACE SHUTTLE MAIN ENGINE



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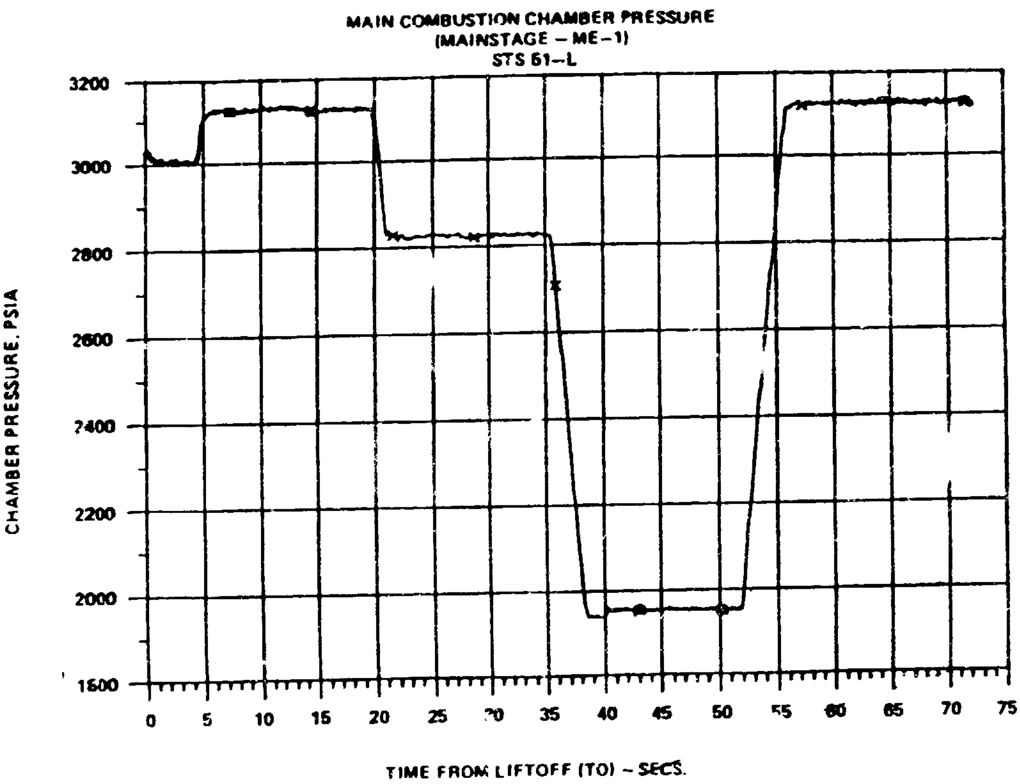
[Ref. 3 7-90]

H—081

SPACE SHUTTLE MAIN ENGINE

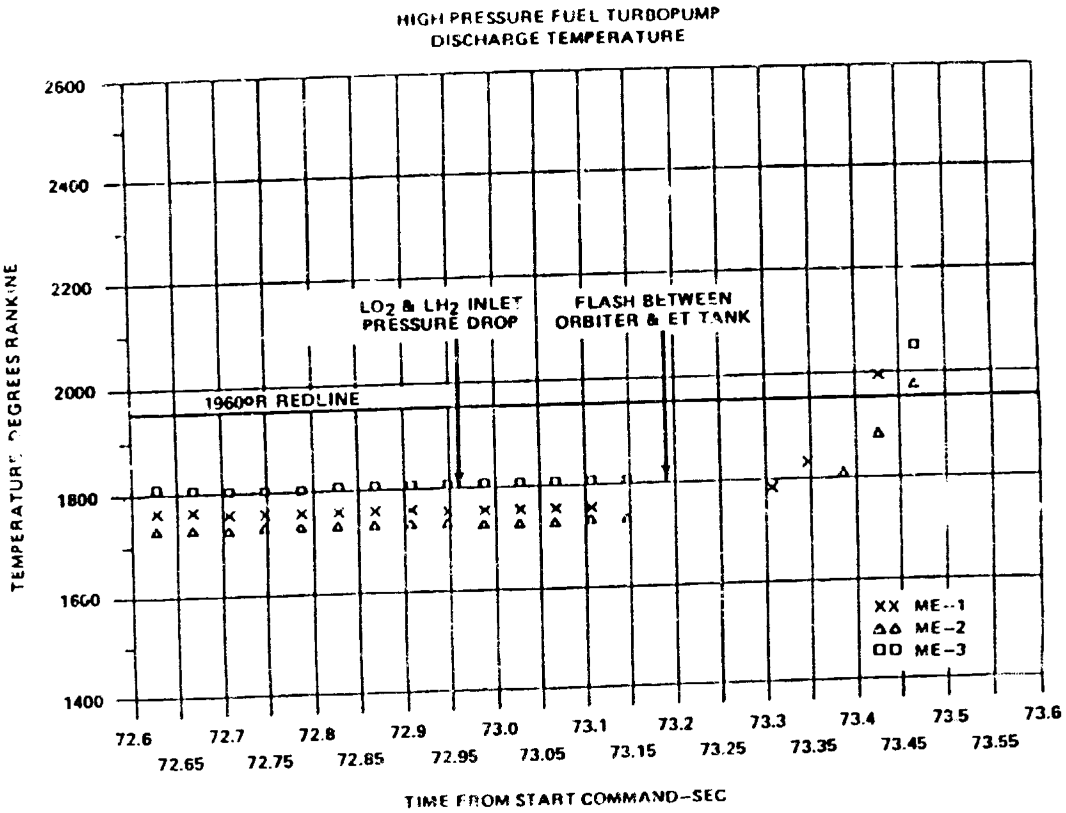


6

[Ref. 3/7/91]

SPACE SHUTTLE MAIN ENGINE

H-090



7

[Ref. 3 7-92]

SPACE SHUTTLE MAIN ENGINE

INSPECTION OF RECOVERED HARDWARE

- ALL ENGINES RECOVERED IN CLOSE PROXIMITY TO EACH OTHER AND GIMBAL BEARINGS FAILED IN AN OVERLOAD MODE
  - ENGINES WERE ATTACHED TO THRUST STRUCTURE AND BULKHEAD AT WATER IMPACT
- ALL FRACTURES WERE DUE TO DUCTILE OVERLOADS
  - ENGINES DID NOT EXPLODE
  - DAMAGE WAS DUE TO WATER IMPACT AND POSSIBLY IMPACT WITH OTHER STRUCTURE
- NO TURBINE BLADE SEPARATION CAUSING HOUSING DAMAGE
- ALL ENGINES HAD BURN-THROUGH DAMAGE IN HOT GAS REGIONS DUE TO INTERNAL OVERTEMPERATURE
  - TYPICAL OF LOX RICH CONDITIONS DUE TO FUEL AND LOX DEPLETION

[Ref. 3 7-93]

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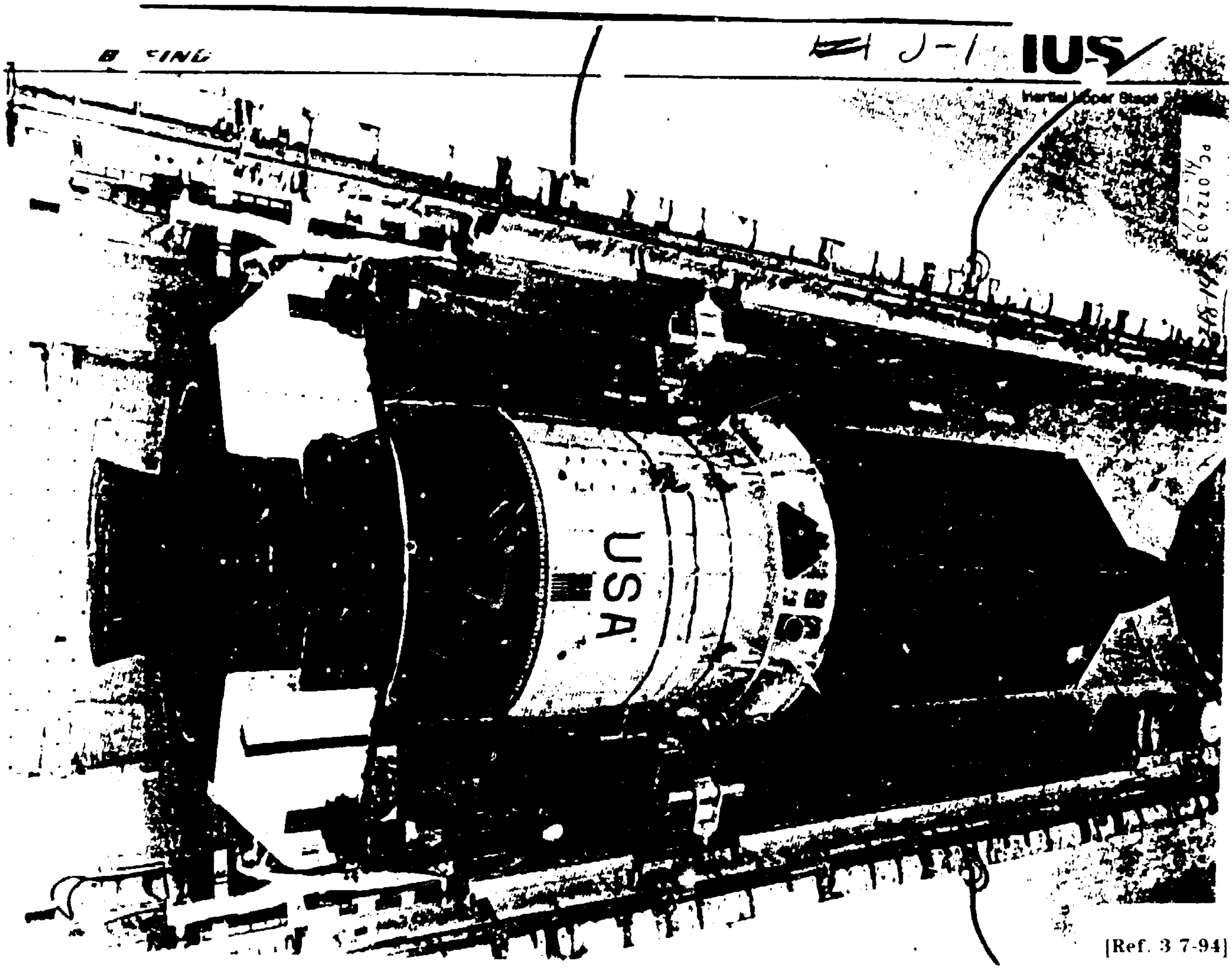
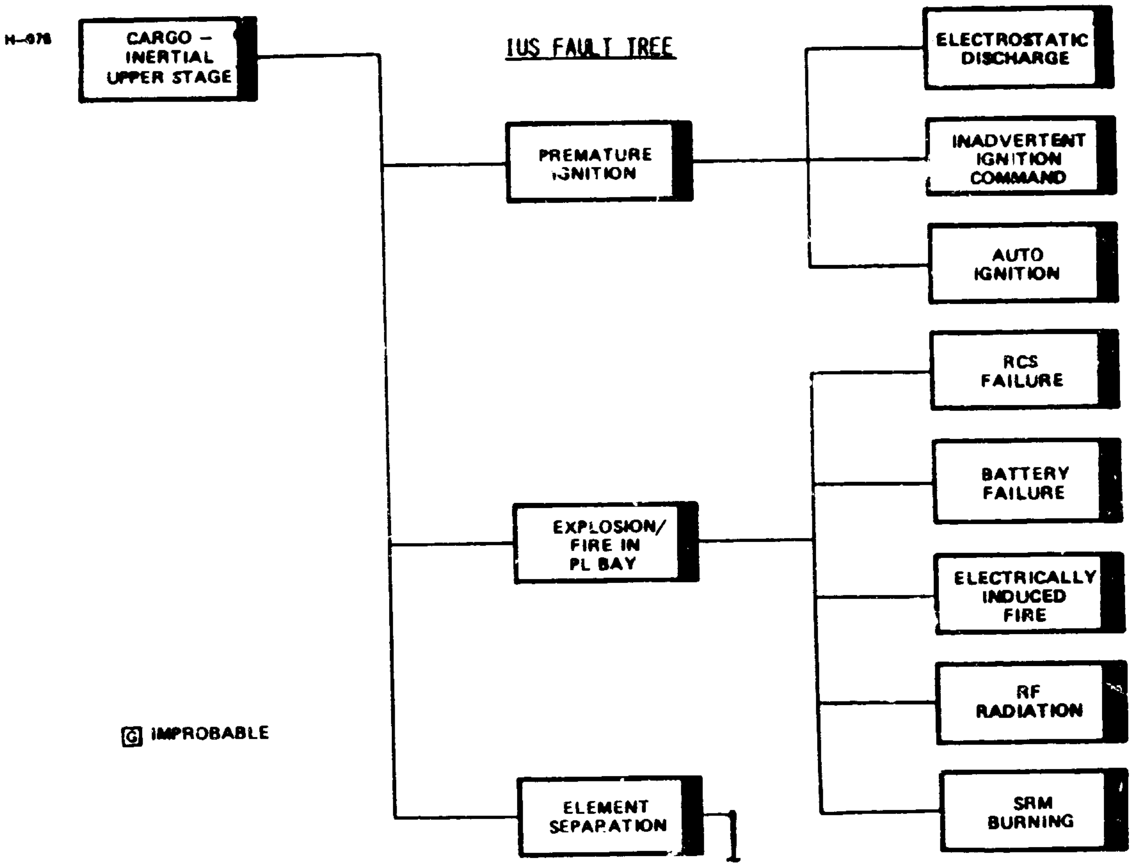


[Ref. 3 7-94]


H-078
CARGO – INERTIAL UPPER STAGE
IUS FAULT TREE
PREMATURE IGNITION
ELECTROSTATIC DISCHARGE
INADVERTENT IGNITION COMMAND
AUTO IGNITION
EXPLOSION/ FIRE IN PL BAY
RCS FAILURE
BATTERY FAILURE
ELECTRICALLY INDUCED FIRE
RF RADIATION
SRM BURNING
ELEMENT SEPARATION
G IMPROBABLE

[Ref. 3/7-95]

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PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—FRIDAY, MARCH 21, 1986

Dean Acheson Auditorium
Department of State
Washington, D.C.

The Commission met, pursuant to notice, at 9:40 a.m.

PRESENT:

WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ALBERT WHEELON
ROBERT RUMMEL
DR. ARTHUR WALKER
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA
ROBERT HOTZ
DR. EUGENE COVERT

ALSO PRESENT:

AL KEEL, Commission Executive Director

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PROCEEDINGS

CHAIRMAN ROGERS: The Commission will come to order, please.

Since our last public hearing at the Kennedy Space Center the Commission, working in part through subcommittees, has made considerable progress in its investigation of the Challenger incident. The Commission has worked very closely with Admiral Richard Truly and his assistant, J.R. Thompson, as well as with the NASA investigative panels, and has been given excellent help and cooperation in all respects.

This session today is for the purpose of updating the information that the Commission has received and to inform the public as accurately as possible of the progress that has been made to date.

Now we will call on Mr. J.R. Thompson to be our first witness.

(Witness sworn.)

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TESTIMONY OF JAMES R. THOMPSON, JR., VICE CHAIRMAN, STS-51L DATA AND DESIGN ANALYSIS TASK FORCE, CAPE CANAVERAL, FLORIDA

CHAIRMAN ROGERS: Mr. Thompson, will you identify yourself and say a few words about your present assignment.

MR. THOMPSON: My name is J.R. Thompson. I'm with Princeton University and I'm on temporary assignment to NASA headquarters, reporting directly to Admiral Truly.

Mr. Chairman, this morning we would like to give you three task force team updates from the report we gave to the Commission, to the full Commission two weeks ago. The specific topics we would like to cover would be: An update on our salvage status. I think we have made a lot of progress in that area and Colonel Ed O'Connor will give you that briefing.

I think we have now almost completed our photo and time line status, and we would like to update that for you, and Dan Germany and his team will be providing the specific information on that.

We would like to focus most of today's discussion on the accident analysis team results and an update from the discussion we had several weeks ago.

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Dr. Wayne Littles will lead that discussion and be supported by five additional engineers from the Marshall Space Flight Center.

In summary, I would like to report I think we have made a lot of progress. I think we've got a lot of work still in front of us, but I think it is coming together quite well. And I think over the next one to two weeks we will be seeing a lot of results of the test program that we have reviewed with your Commission.

CHAIRMAN ROGERS: Thank you very much, Mr. Thompson.

I would call Colonel O'Connor.

(Witness sworn.)

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TESTIMONY OF COLONEL EDWARD O'CONNOR, CHAIRMAN, SEARCH, RECOVERY AND RECONSTRUCTION, CAPE CANAVERAL, FLORIDA

CHAIRMAN ROGERS: Colonel O'Connor, would you identify yourself and give the Commission some information about your present assignment.

COLONEL O'CONNOR: I'm Edward O'Connor, Colonel in the United States Air Force, assigned to the Eastern Space Missile Center, Patrick Air Force Base, Florida. I'm currently assigned to the task force as the leader of the search and recovery team that is in the process of recovering the debris of the shuttle. [Ref. 3/21-1]

CHAIRMAN ROGERS: You may proceed.

COLONEL O'CONNOR: I would like to give you at this time a quick update on the conditions we are facing at sea. The oceanographic features today are predominantly influenced by the weather and the springtime seasonal changes.

We expect that the problems we are having currently with recovery operations will diminish within the next few weeks, permitting us to bring up more components, particularly the right SRB. We are finding most of the right SRB components, as well as left SRB components, fairly close to the axis of the Gulf Stream, still in the depths of 220 to 1200 feet of water.

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The next chart, please.

(Viewgraph.) [Ref. 3/21-2]

The current recovery operations include nine ships, one manned submersible, four sonars, and 27 divers. In light of the expected improvement of weather conditions and the completion of some of the analytic work in identifying probable locations of components, we are proceeding to bring another submersible into the recovery operation.

One of our most successful vessels has been the Sea Link II, which is a manned submersible supplied by the Harbor Branch Foundation. At this time we will bring in the Sea Link I, which is their other vessel, similarly equipped, which would speed up many of our recovery operations.

We have released the NR-1, which was a nuclear submarine which was characterizing many of our deep water contacts. If conditions require, we could bring this asset back in to complete the search of certain areas within the next ten days.

The next chart, please.

(Viewgraph.) [Ref. 3/21-3]

This chart indicates some of the changes we have had in our search area. You will recall we had an initial search area approximately 25 nautical miles by

2187

10 nautical miles as a box. We then added a small truncated portion going out to the north.

Since that time, using the radar data, optical data, and some information supplied by the FAA area radars, we have included a few additional small boxes for location of specific components. We would expect that this would probably be the completion of our expansion of the search area for crucial components.

Next chart, please.

(Viewgraph.) [Ref. 3/21-4]

As of yesterday, our search area encompassed approximately 420 square nautical miles. We have done a sonar search of 400 square nautical miles, leaving just a small portion to be completed. We would expect that to be completed within the next week to ten days.

To date, we have made 571 significant sonar contacts. Each of these contacts has to be further identified through video or manned submersible operations. We have investigated 112 contacts. Shuttle components were found at 29 locations. We still need to check 459 different locations.

Next chart.

(Viewgraph.) [Ref. 3/21-5]

The recovery to date has resulted in approximately 20 percent of the orbiter being recovered

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and made available for analysis. We have significant portions of the main propulsion system and, as you are all aware, we are recovering portions of the crew module at this time.

In the area of the inertial upper stage, we now have 65 percent of that particular part of the payload complement. We have less than one percent of the TDRS. The external tank right now is about ten percent complete, and the solid rocket motor—booster recovery has only brought about ten percent of the components to shore.

We have seen nothing of the Spartan Halley payload.

Next chart.

(Viewgraph.) [Ref. 3/21-6]

This chart lists the components that we have recovered from the 51-L accident. We have added a few new components on this list, predominantly in the crew module area. We have come up with some additional skin panels and some additional portions of the base heat shield.

In the area of the SRB's, we have the frustums, the drogue parachute, the rate and gyro system tunnels. At the end of this briefing, I will be covering the specific components of the right SRB that

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we have recovered at this time.

In the area of the external tank, we have found few additional portions of the external tank. That has not been a high priority search item at this area—at this time. But in the future we will be bringing more of that in. We have located some additional components on the ocean floor and we'll recover them at a later time.

Next chart.

(Viewgraph.) [Ref. 3/21-7]

I would like to discuss with you now for a few moments the right solid booster recovery status. We have formed a recovery team specifically to support the recovery of the right SRB components and their further analysis. This team is comprised of NASA engineers from both Kennedy Space Center and Marshall, a contractor team from the contractors involved, Thiokol and some of their other support contractors.

We have the design team in place to support all of our activities as quickly as possible when we recover the components. The identification of the critical hardware is being provided by this team, and they also suggest schemes for recovery and what things are important to them from an analytic standpoint.

The National Transportation Safety Board

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members of the team are also investigating the components recovered and are establishing investigative protocols to ensure the maximum evidence is recovered from any component.

Next chart, please.

(Viewgraph.) [Ref. 3/21-7]

In order to maximize the effectivity of the search and recovery activities, we have used many data sources. Many of the range radars were used. Optical data was used, and a lot of sonar mapping.

And a quick summary: We have completed the majority of the radar data analysis and data reduction. The optical data has also been reduced in a metric sense to help support the recovery operations. Sonar mapping is approximately 95 percent complete and we are now starting an SRB breakup analysis to better understand the breakup mechanism that occurred on the vehicle after command destruct, to better help locate the components on the ocean floor.

CHAIRMAN ROGERS: Colonel, are you in a position to make any estimates of how long it will take to complete the work you are doing?

COLONEL O'CONNOR: To recover the total right-hand SRB, assuming that we have some good weather and that our radar predictions are accurate, two to

2191

three weeks would let us get up many of the significant components that you would be interested in.

CHAIRMAN ROGERS: Thank you.

COLONEL O'CONNOR: I have one chart up there that I would like you to take a quick look at. This chart shows some of the radar and optical data that we have reduced, and it indicates what portions of the right SRB we have found to date. [Ref. 3/21-8]

As you can see, we have the frustum identified, parts of the skirt associated with the frustum, some portions of the center case elements. And at the rear of the vehicle, we have found a debris field which encompasses the majority of the aft segment and skirt.

It does not include the area of interest in that joint at this time. We are going to continue to look at that. There is one part there, slightly shaded in in the joint area, I will be discussing in a moment.

CHAIRMAN ROGERS: I wasn't clear. In the aft section, the shaded part, have you recovered all of that?

COLONEL O'CONNOR: No. We've located that on the bottom.

CHAIRMAN ROGERS: You have located it, I see.

COLONEL O'CONNOR: We will start recovery activity in that area early next week.

The next chart, please.

2192

(Viewgraph.) [Ref. 3/21-9]

This chart indicates some of the design features we use for identifying the components. We use many of the index devices that NASA uses in the alignment and the stackup and manufacture of the vehicle, hole patterns and dimensions, paint markings, part and serial numbers if they are available. In many cases on the SRB, these part and serial numbers are not available on the components that we have.

Seeing they were manufactured as large segments, they would only occur in one or two places on the case. If the case breaks into many pieces, that gives us a difficult time associating a particular piece with a particular SRB.

Many of the internal features are also assisting us, such as propellant profiles, and some of the inhibitor characteristics, such as depth and shape and the manufacturing records for scratches and blemishes on the case, are also being used to help characterize and help locate these components.

Next chart, please.

(Viewgraph.) [Ref. 3/21-10]

The right solid rocket booster components that we have located are quickly listed here. This coincides also with the chart on the wall and indicates those that

2193

are recovered: the two case cylinders, the ET attach segment, which we are not exactly sure which SRB is associated with at this time; it has not been recovered, and we will discuss that a little more—and part of the frustum have been located and also recovered.

(Viewgraph.) [Ref. 3/21-11]

There is a chart now—and I believe you will have to turn two charts to get to it, and it is a more detailed breakup or breakdown of the central part of this chart, indicating the type of radar tracking that led us to look into that area and the concentration of right SRB components that we are finding.

Now that we have characterized this debris field, we are better able to marshal our assets to speed the recovery of these elements. This is an important step in that we now know the major areas that have to be investigated, and we can put all of our submersibles in that area, as well as our recovery ships.

Next chart, please.

(Viewgraph.) [Ref. 3/21-12]

This chart, the right SRB aft components—the next chart, please. This chart indicates the SRB aft—right SRB aft components as fragments, as laid out on a plane form. As you can see, the aft segment broke into many pieces. They are scattered in a fairly large area of the floor, and it is

2194

going to take a fairly lengthy period of time to recover all of these individual pieces.

We will be using the STENA Workhorse in that recovery. I have not indicated on this chart, the upper part of this chart, any location of anything associated with the clevis or the joint that is suspected of being the failure point.

If there are no questions associated with any of that material, I would like to proceed with the discussion of contact or target 292. This has recently received a lot of press attention because it has been identified as an external tank attach segment.

(Viewgraph.) [Ref. 3/21-13]

The first chart is a sketch of this component as it was on the ocean floor. This component has now been recovered.

Okay, would you roll the video now, please.

(A videotape was shown.) [Not published.]

COLONEL O'CONNOR: I have a short video segment here showing the STENA Workhorse. This is the vessel used to recover this component. The component is being taken from the ship at this time. This was yesterday afternoon at Cape Canaveral.

That is part of the clevis joint. As you can see, it is badly damaged.

2195

On the screen is O-ring grooves.

DR. COVERT: Colonel O'Connor, that damage, has a metallurgist had a chance to decide whether it was impact damage or damage that existed prior to impact with the water?

COLONEL O'CONNOR: In the preliminary—and I need to stress, preliminary—review of that joint, there does not appear to be any erosion or melting associated with it. That is very preliminary. So it would be expected that that joint was probably damaged at the time of command destruct, as the case experienced some torsion, or at water impact.

DR. COVERT: Thank you very much.

MR. HOTZ: Colonel O'Connor, could you give us any additional information you might have on the external tank stub attachment points, the condition of them and how much of them is left?

COLONEL O'CONNOR: Yes, I can. I have two photos here I think that might help go through that process. Let me bring those up, and then I will address your question.

The first photo, please.

(Slide.) [Ref. 3/21-14]

This is a still photograph of that same component. This is showing the inhibitor ... of the

2196

things that we use to characterize whether it be a left or right SRB.

The next photo, please.

(Slide.) [Ref. 3/21-15]

On this photo you have a better view of the clevis joint, if you look at where the white tag is attached. That is part of the ET attach strut assembly. There is some deformation in that area. There is some insulation just above it that has been eroded and removed.

This is helping us to characterize where on the aft segment this would be located.

DR. COVERT: Do you want to make a guess as to where it is, please?

COLONEL O'CONNOR: I'm not prepared to guess at that right now, sir. We have got a lot of contacts out in the area. We think it may be in that vicinity, but this is a fairly small fragment of the case and the dynamics aren't very well understood now to say it would fall in this vicinity or be taken with a larger structure a further distance.

DR. COVERT: I meant, was this at 300 degrees or 180 degrees?

COLONEL O'CONNOR: Where it is on the case?

DR. COVERT: Yes, sir.

2197

COLONEL O'CONNOR: I have a sketch on that, sir. Next chart, please.

(Viewgraph.) [Ref. 3/21-16]

This chart lists the status of this particular component, seeing it is of such high interest. The portion was recovered on the 17th of March. It was brought into port by STENA Workhorse yesterday. It was removed from the STENA Workhorse yesterday about noon.

We took it over to our ordnance area on Cape Canaveral. Our engineers were evaluating it yesterday afternoon and late into the night. The preliminary evaluation indicates there are no part numbers or other positive identifying features.

It is a confirmed aft segment component. It is a confirmed ET attach portion. The external surface is darkened and blistered. The ET attach stud hole spacing and deformation is consistent with this case segment being from a 90 to 180 degree quadrant on a segment.

It is most probably a right SRB component. Because of the lack of any identification number or anything that can definitely tie it to the right SRB, we have to qualify it being right at this time. We are continuing the evaluation at this time. We brought other engineers in.

2198

We have been reviewing the other stacked SRB's at the Cape, looking for other identifying features.

Next chart, please.

(Viewgraph.) [Ref. 3 21-17]

CHAIRMAN ROGERS: Why would you say "most probably" if you don't know which it is?

COLONEL O'CONNOR: Looking at the propellant depth, the inhibitor shape, we have about 20, possibly 25, inferential characteristics would say it is the right SRB. We have nothing that would point it toward being the left SRB.

But we don't have that crisp nice part number stamped on the side that would let us really track it down and say for sure.

CHAIRMAN ROGERS: Thank you.

(Viewgraph.) [Ref. 3 21-17]

COLONEL O'CONNOR: This particular view looking down on the orbiter stack indicates a right SRB with the label pointing to where the black smoke was initially found at the time of launch. Looking at the different characteristics of this particular segment piece, we would suspect it would be, as indicated on that chart, approximately 180 degrees away from the black smoke.

That completes my briefing.

2199

CHAIRMAN ROGERS: Thank you very much.

Any questions?

(No response.)

CHAIRMAN ROGERS: Thank you, Colonel.

Mr. Germany.

(Witnesses sworn.)

OCEANOGRAPHIC CONDITIONS

- O IMPACT OF SPRING WEATHER SYSTEMS IS EXPECTED TO DIMINISH DURING THE NEXT FEW WEEKS. SALVAGE AREA SHOULD BE INFLUENCED BY WINDS FROM THE SOUTHWEST WHICH WILL IMPROVE UNDERWATER VISIBILITY.

- O OCEANOGRAPHIC FACTORS AFFECTING SALVAGE
  - PROXIMITY OF THE GULF STREAM AXIS TO THE SOLID ROCKET BOOSTER (SRB) AREA RESULTING IN 4 - 5 KNOTS CURRENT
  - DEPTH IN VICINITY OF THE SRBs: 220 - 1200 FEET

DATE: 21 MARCH 86

SEARCH

RECOVERY

RECONSTRUCTION

EDWARD O'CONNOR, JR.
COLONEL, U.S. AIR FORCE

[Ref. 3 21-1]

RECOVERY OPERATIONS

0 CURRENT ASSETS

- 9 SHIPS
- 1 MANNED SUBMERSIBLE
- 2 REMOTELY OPERATED VEHICLES
- 4 SONARS
- 27 DIVERS

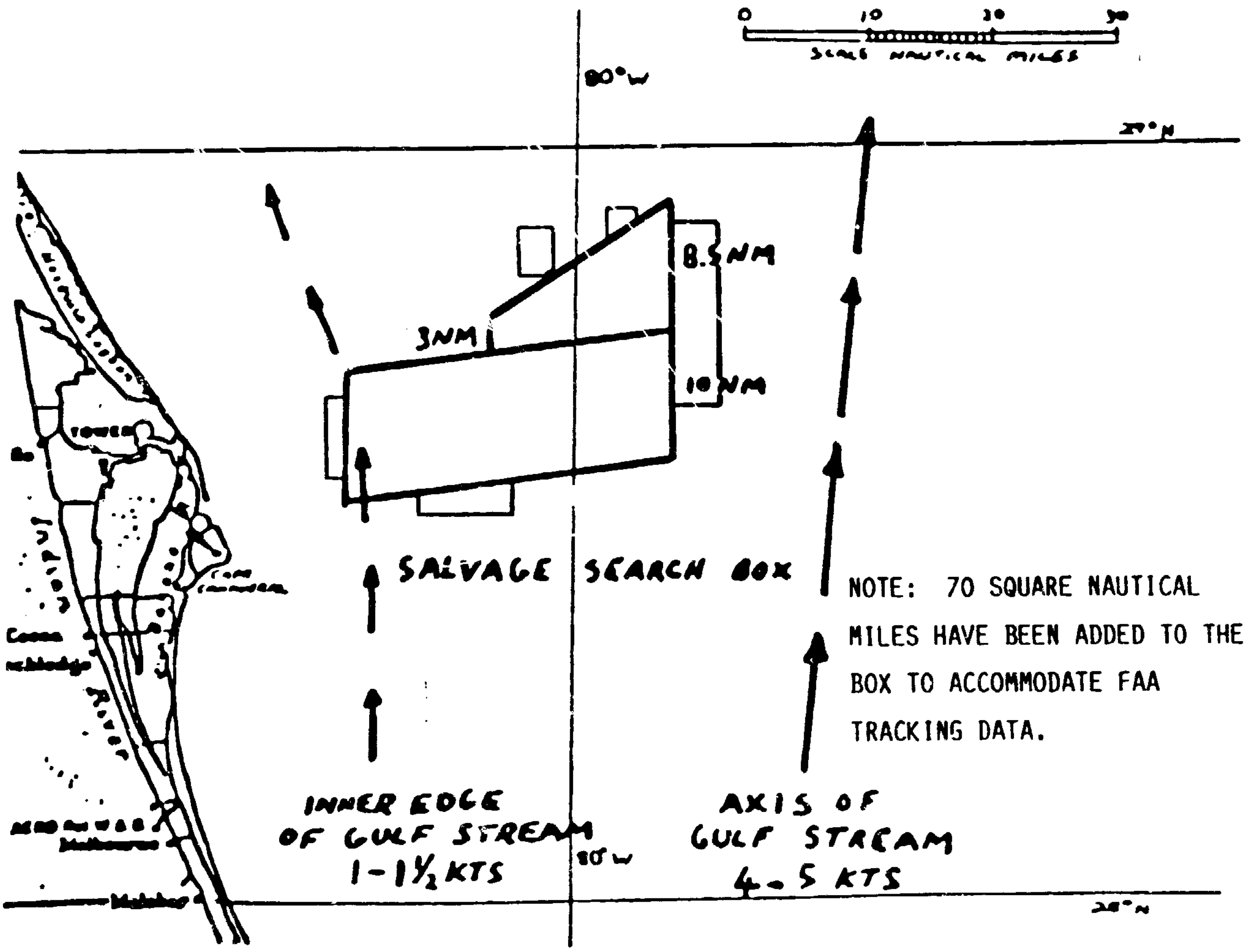
0 NEW ASSETS EXPECTED

- OCEANOGRAPHIC SHIP EDWIN LINK WITH MANNED SUBMERSIBLE - JOHNSON SEA LINK I

0 RELEASED ASSETS

- NR-1/USS SUNBIRD

[Ref. 3/21-2]


0
10
20
30
80°W
8.5NM
3NM
10NM
Indian
River
SALVAGE SEARCH BOX
NOTE: 70 SQUARE NAUTICAL MILES HAVE BEEN ADDED TO THE BOX TO ACCOMMODATE FAA TRACKING DATA.
INNER EDGE
OF GULF STREAM
1-1½ KTS
AXIS OF
GULF STREAM
4-5 KTS

[Ref. 3/21-3]

SEARCH UPDATE

	20 MARCH
0 SONAR SEARCH RESULTS	
SQUARE NAUTICAL MILES TO COVER	420
SQUARE NAUTICAL MILES SEARCHED	400
0 CONTACT STATUS	
CONTACTS MADE	571
CONTACTS INVESTIGATED	112
SHUTTLE COMPONENTS	29
REMAINING TO BE CONFIRMED	459

[Ref.3/21-4]

51-L RECOVERY UPDATE

0 ORBITER: 20%

- MAIN PROPULSION SYSTEM: 75%
- CREW MODULE: 75%

0 INERTIAL UPPER STAGE: 65%

0 TDRSS: < 1%

0 EXTERNAL TANK: 10%

0 SOLID ROCKET BOOSTERS: 10%

0 SPARTAN HALLEY: 0%

[Ref. 3/21-5]

51L COMPONENTS RECOVERED

ORBITER

- LOWER FWD & AFT FUSELAGE PANELS
- ET/ORBITER LO_2 UMBILICAL
- MAJORITY OF MPS SYSTEM
- MAJORITY OF RUDDER SPEED BRAKE
- MAIN LANDING GEAR DOORS
- BODY FLAP
- RIGHT ELEVON
- PORTION OF PAYLOAD BAY DOORS
- PORTION OF RADIATORS
- LOWER SKIN PANELS
- PORTION OF AFT THRUST STRUCTURE
- PORTION OF ALL 3 MAIN ENGINES
- PORTION OF BASE HEAT SHIELD
- RIGHT AFT SIDE WALL
- PORTION OF CREW MODULE

SRB

- FRUSTUMS
- DROGUE PARACHUTE
- RATE GYRO
- SYSTEMS TUNNEL FROM FWD SKIRT
- ET ATTACH SEGMENT
- 2 AFT CENTER SEGMENT FORWARD CYLINDER FRAGMENTS

ET

- 40-50% OF INTER TANK SKIN
- <1% OF LO_2 TANK
- 10-12% OF LH_2 TANK
- NOSE CONE SKIN
- PORTION OF LO_2 FEEDLINE
- MAJORITY OF RANGE SAFETY DESTRUCTION SYSTEM

[Ref.3/21-6]

RIGHT SOLID ROCKET BOOSTER RECOVERY

STATUS

RIGHT SOLID ROCKET BOOSTER RECOVERY TEAM

- NASA AND CONTRACTOR TEAM MADE UP OF SPECIALISTS IN DESIGN, HANDLING, AND METALLURGY IS PROVIDING ONSITE SUPPORT IN THE RECOVERY OPERATION.
- IDENTIFICATION OF CRITICAL HARDWARE IS BEING PROVIDED TO THE RECOVERY TEAM ALONG WITH SUGGESTED SCHEMES FOR PRECLUDING DAMAGE DURING OPERATIONS.
- NATIONAL TRANSPORTATION SAFETY BOARD AND DESIGN ENGINEERS ARE INVESTIGATING RECOVERED COMPONENTS AND SAMPLING SELECTED AREAS FOR FURTHER STUDY.

[Ref. 3/21-7]

LOCATION PREDICTION STATUS

- 0 RADAR DATA REDUCTION IS COMPLETE
- 0 OPTICAL DATA REDUCTION IS COMPLETE
- 0 SONAR MAPPING IS 95 PERCENT COMPLETE
- 0 SRB BREAKUP ANALYSIS IN WORK

[Ref. 3/21-8]

METHOD OF IDENTIFICATION

- 0 DESIGN FEATURES
  - INDEX DEVICES
  - HOLE PATTERNS AND DIMENSIONS
  - PAINT MARKINGS
- 0 PART AND SERIAL NUMBERS
- 0 INTERNAL FEATURES
  - PROPELLANT PROFILE
  - INHIBITOR CHARACTERISTICS
- 0 MANUFACTURING RECORDS

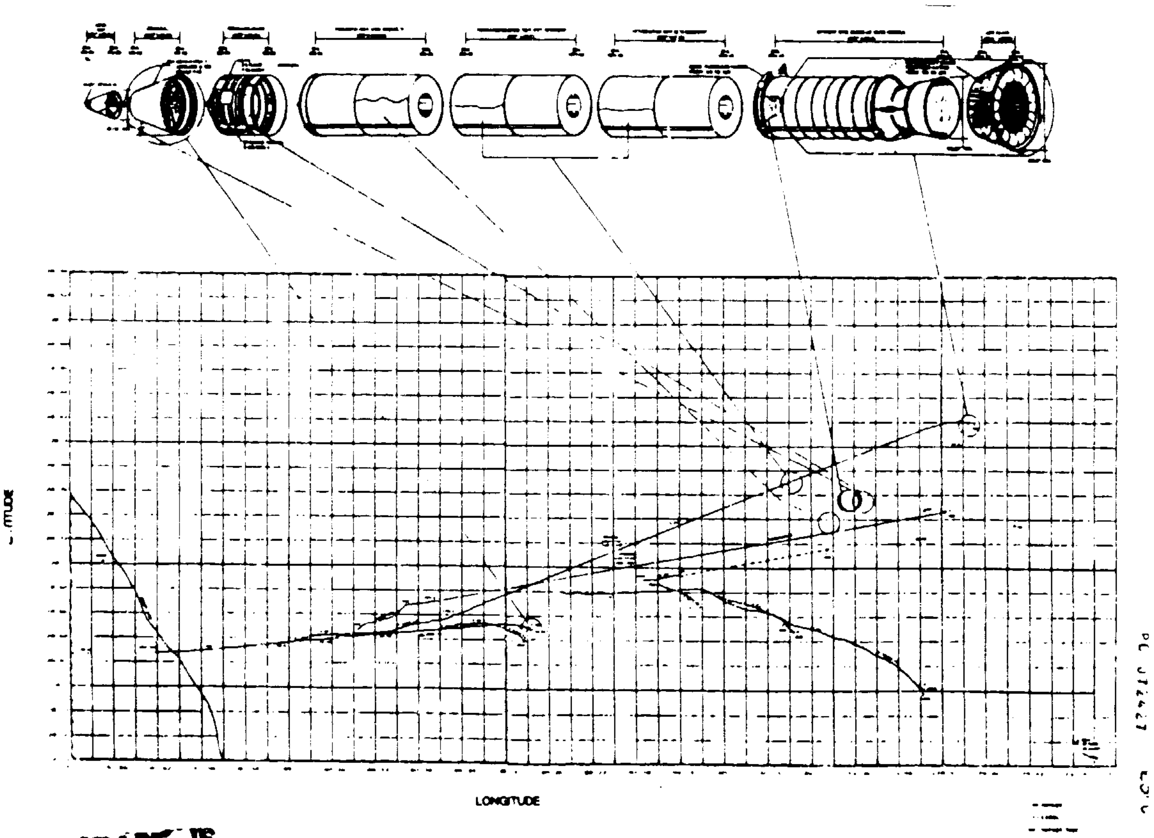
[Ref. 3/21-9]

RIGHT SOLID ROCKET BOOSTER COMPONENTS

		LOCATED	RECOVERED
0	PORTION OF NOSE CAP	X	
0	PORTION OF FORWARD SKIRT	X	
0	AFT SKIRT ASSEMBLY	X	
0	LOWER PORTION OF AFT SEGMENT	X	
0	2 CASE CYLINDERS	X	X
0	ET ATTACH	X	X
0	PART OF FRUSTUM	X	X

[Ref. 3/21-10]

SRB RECOVERY ASSESSMENT



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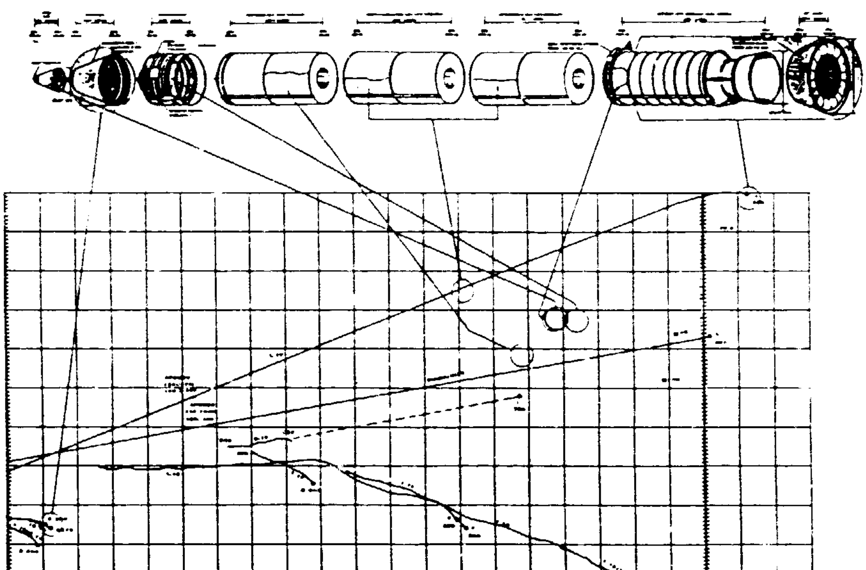
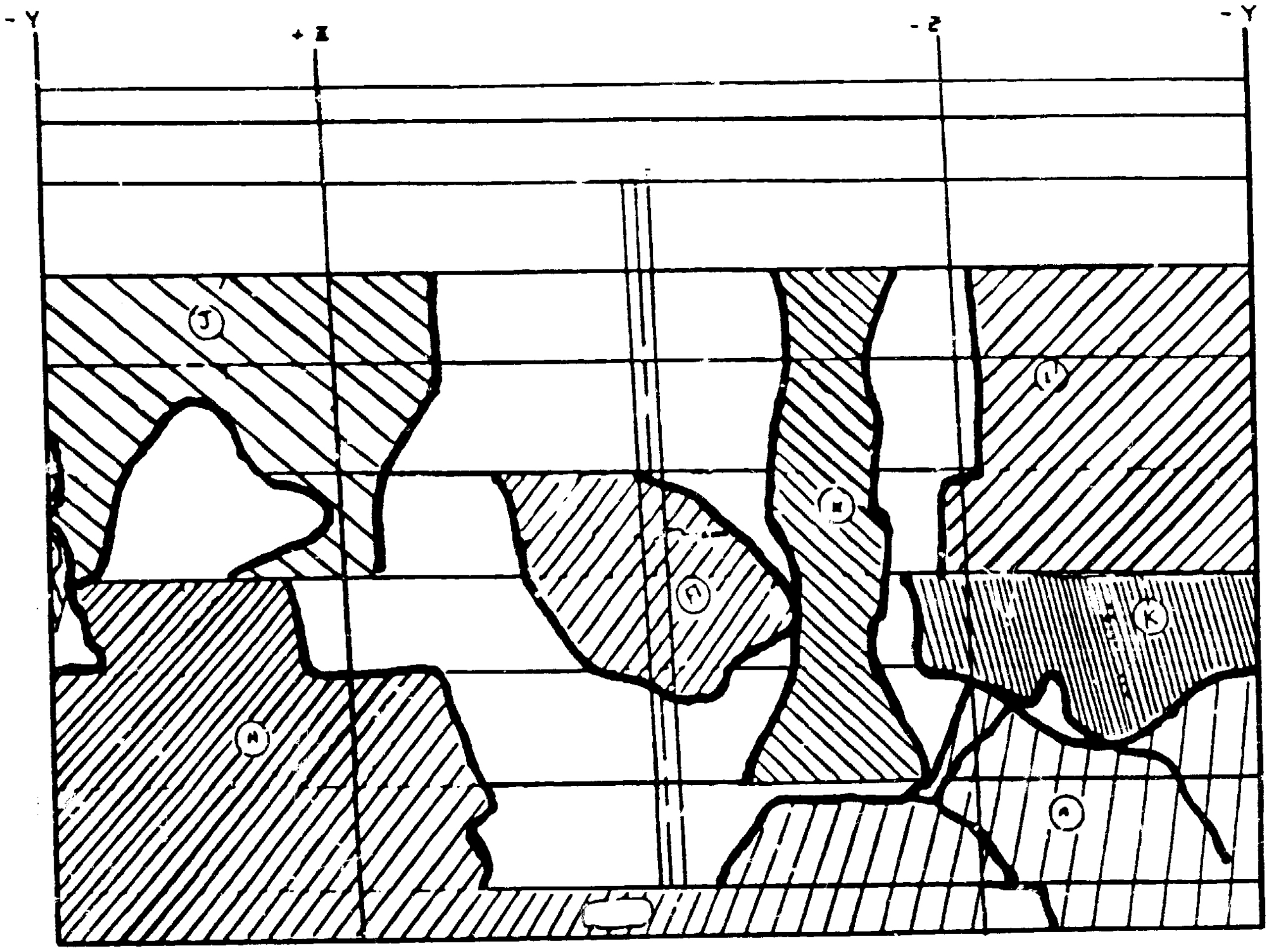
SRB RECOVERY ASSESSMENT



[Ref. 3/21-11]

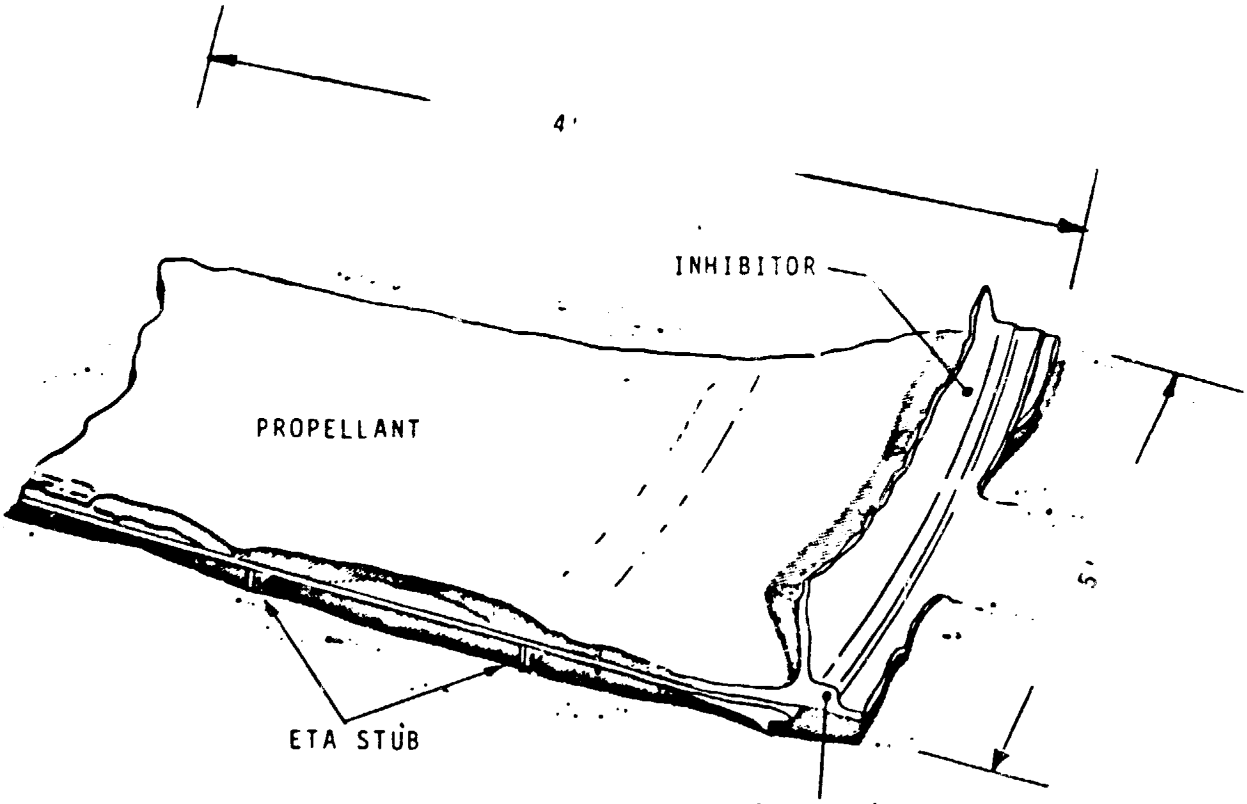
RIGHT SRB AFT COMPONENTS



[Ref. 3/21-12]

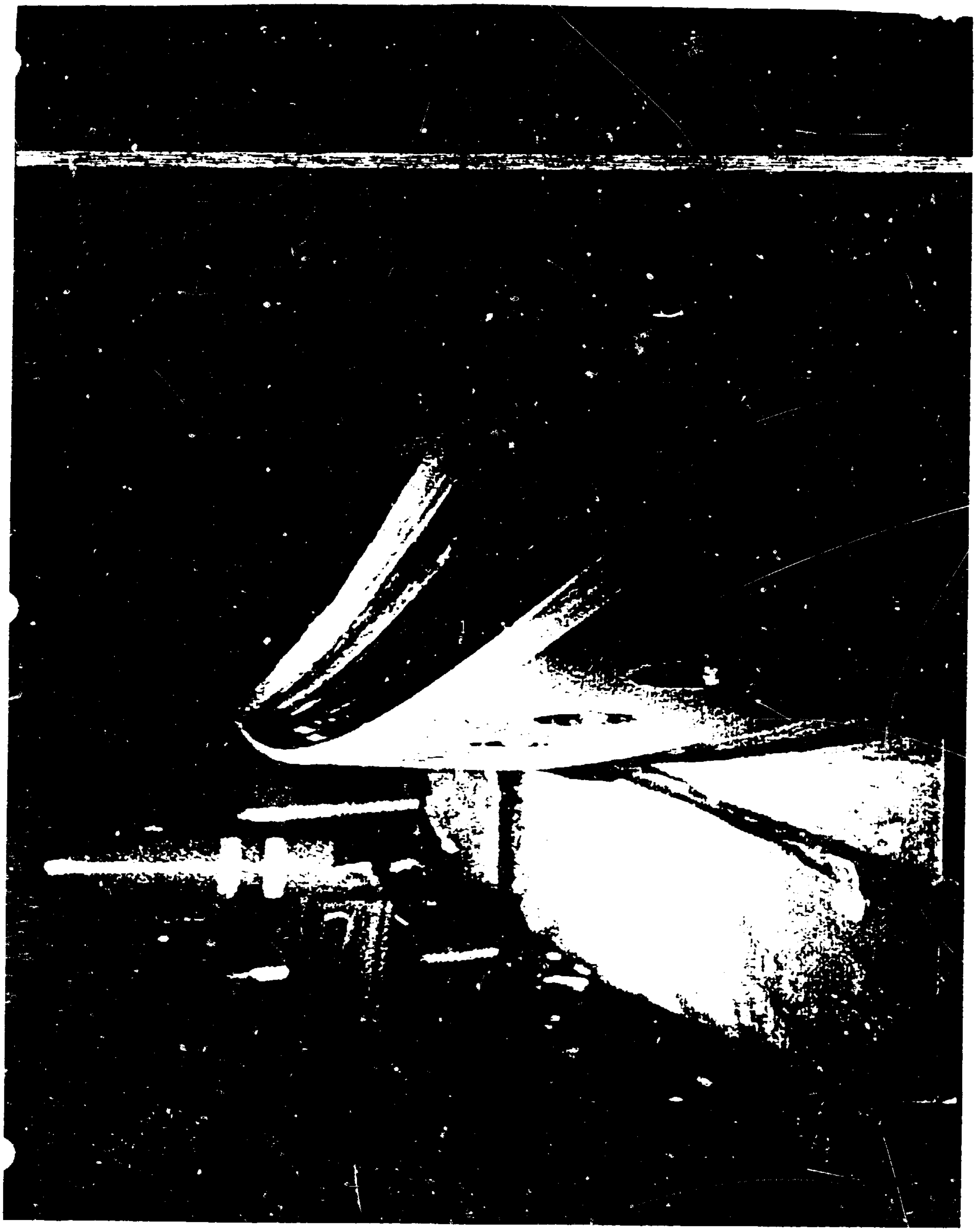
EXTERNAL TANK ATTACH SEGMENT EVALUATION

EXTERNAL TANK ATTACH SEGMENT EVALUATION
(TARGET 292)



[Ref. 3/21-13]

[Ref. 3/21-14]



[Ref. 3/21-15]

EXTERNAL TANK ATTACH SEGMENT EVALUATION
(TARGET 292)

0 PORTION RECOVERED ON 17 MARCH

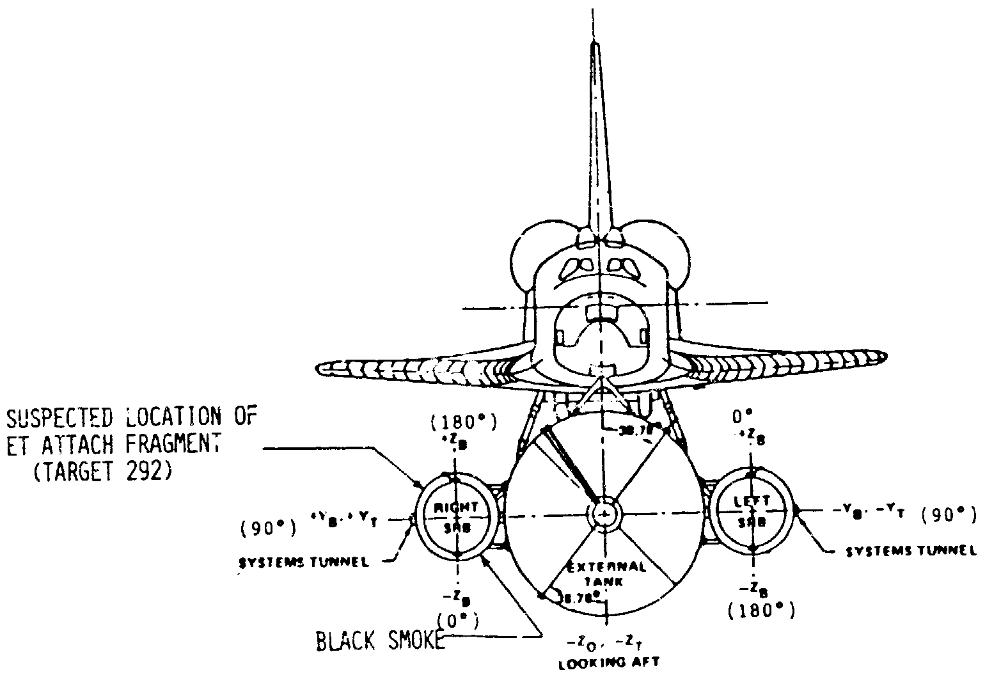
0 ANALYSIS COMMENCED 20 MARCH

0 PRELIMINARY EVALUATION
- NO PART NUMBERS OR OTHER POSITIVE IDENTIFYING FEATURES
- CONFIRMED AFT SEGMENT
- CONFIRMED ET ATTACH PORTION
- EXTERNAL SURFACE DARKENED
- ET ATTACH STUD HOLE SPACING AND DEFORMATION CONSISTENT WITH 90° TO 180° QUADRANT
- MOST PROBABLY RIGHT SRB COMPONENT

0 EVALUATION CONTINUING

[Ref. 3/21-16]

SUSPECTED LOCATION OF RECOVERED ET ATTACH FRAGMENT



[Ref. 3/21-17]

DATE: 21 MARCH 86

SEARCH

RECOVERY

RECONSTRUCTION

EDWARD O'CONNOR, JR.
COLONEL, U.S. AIR FORCE

[Ref. 3 21-1 1 of 2]

OCEANOGRAPHIC CONDITIONS

0 IMPACT OF SPRING WEATHER SYSTEMS IS EXPECTED TO DIMINISH DURING THE NEXT FEW WEEKS. SALVAGE AREA SHOULD BE INFLUENCED BY WINDS FROM THE SOUTHWEST WHICH WILL IMPROVE UNDERWATER VISIBILITY.

0 OCEANOGRAPHIC FACTORS AFFECTING SALVAGE

- PROXIMITY OF THE GULF STREAM AXIS TO THE SOLID ROCKET BOOSTER (SRB) AREA RESULTING IN 4 - 5 KNOTS CURRENT
- DEPTH IN VICINITY OF THE SRBs: 220 - 1200 FEET

[Ref. 3 21-1 2 of 2]

RECOVERY OPERATIONS

0 CURRENT ASSETS

- 9 SHIPS
- 1 MANNED SUBMERSIBLE
- 2 REMOTELY OPERATED VEHICLES
- 4 SONARS
- 27 DIVERS

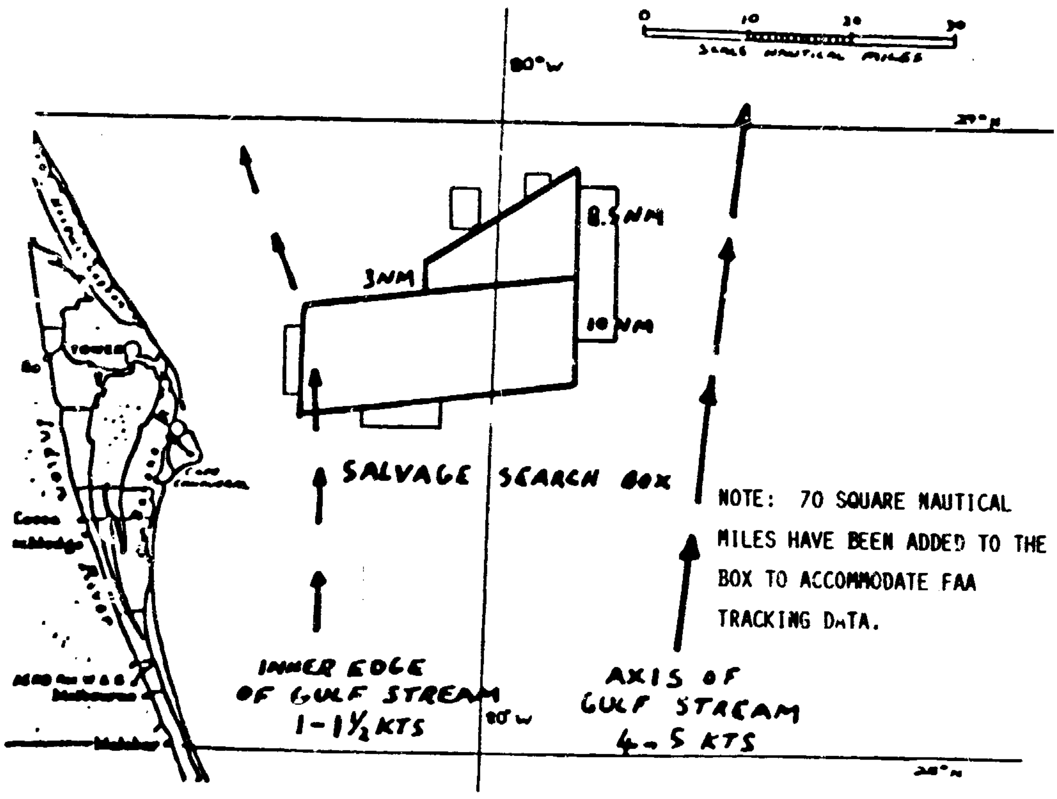
0 NEW ASSETS EXPECTED

- OCEANOGRAPHIC SHIP EDWIN LINK WITH MANNED SUBMERSIBLE - JOHNSON SEA LINK I

0 RELEASED ASSETS

- NR-1/USS SUNBIRD

[Ref. 3 21-2]



[Ref. 3/21-3]

SEARCH UPDATE

	20 MARCH
0 SONAR SEARCH RESULTS	
SQUARE NAUTICAL MILES TO COVER	420
SQUARE NAUTICAL MILES SEARCHED	400
0 CONTACT STATUS	
CONTACTS MADE	571
CONTACTS INVESTIGATED	112
SHUTTLE COMPONENTS	29
REMAINING TO BE CONFIRMED	459

[Ref. 3/21-4]

51-L RECOVERY UPDATE

0 ORBITER: 20%

- MAIN PROPULSION SYSTEM: 75%
- CREW MODULE: 75%

0 INERTIAL UPPER STAGE: 65%

0 TDRSS: < 1%

0 EXTERNAL TANK: 10%

0 SOLID ROCKET BOOSTERS: 10%

0 SPARTAN HALLEY: 0%

[Ref. 3/21-5]

51L COMPONENTS RECOVERED

ORBITER

- o LOWER FWD & AFT FUSELAGE PANELS
- o ET/ORBITER LO_2 UMBILICAL
- o MAJORITY OF MPS SYSTEM
- o MAJORITY OF RUDDER SPEED BRAKE
- o MAIN LANDING GEAR DOORS
- o BODY FLAP
- o RIGHT ELEVON
- o PORTION OF PAYLOAD BAY DOORS
- o PORTION OF RADIATORS
- o LOWER SKIN PANELS
- o PORTION OF AFT THRUST STRUCTURE
- o PORTION OF ALL 3 MAIN ENGINES
- o PORTION OF BASE HEAT SHIELD
- o RIGHT AFT SIDE WALL
- o PORTION OF CREW MODULE

SRB

- o FRUSTUMS
- o DROGUE PARACHUTE
- o RATE GYRO
- o SYSTEMS TUNNEL FROM FWD SKIRT
- o ET ATTACH SEGMENT
- o 2 AFT CENTER SEGMENT FORWARD CYLINDER FRAGMENTS

ET

- o 40-50% OF INTER TANK SKIN
- o <1% OF LO_2 TANK
- o 10-12% OF LH_2 TANK
- o NOSE CONE SKIN
- o PORTION OF LO_2 FEEDLINE
- o MAJORITY OF RANGE SAFETY DESTRUCTION SYSTEM

[Ref. 3 21-6]

RIGHT SOLID ROCKET BOOSTER RECOVERY

STATUS

[Ref. 3 21-7 1 of 2]

RIGHT SOLID ROCKET BOOSTER RECOVERY TEAM

- O NASA AND CONTRACTOR TEAM MADE UP OF SPECIALISTS IN DESIGN, HANDLING, AND METALLURGY IS PROVIDING ONSITE SUPPORT IN THE RECOVERY OPERATION.

- O IDENTIFICATION OF CRITICAL HARDWARE IS BEING PROVIDED TO THE RECOVERY TEAM ALONG WITH SUGGESTED SCHEMES FOR PRECLUDING DAMAGE DURING OPERATIONS.

- O NATIONAL TRANSPORTATION SAFETY BOARD AND DESIGN ENGINEERS ARE INVESTIGATING RECOVERED COMPONENTS AND SAMPLING SELECTED AREAS FOR FURTHER STUDY.

[Ref. 3/21-7 2 of 2]

LOCATION PREDICTION STATUS

- O RADAR DATA REDUCTION IS COMPLETE
- O OPTICAL DATA REDUCTION IS COMPLETE
- O SONAR MAPPING IS 95 PERCENT COMPLETE
- O SRB BREAKUP ANALYSIS IN WORK

[Ref. 3/21-8]

METHOD OF IDENTIFICATION

- O DESIGN FEATURES
  - INDEX DEVICES
  - HOLE PATTERNS AND DIMENSIONS
  - PAINT MARKINGS
- O PART AND SERIAL NUMBERS
- O INTERNAL FEATURES
  - PROPELLANT PROFILE
  - INHIBITOR CHARACTERISTICS
- O MANUFACTURING RECORDS

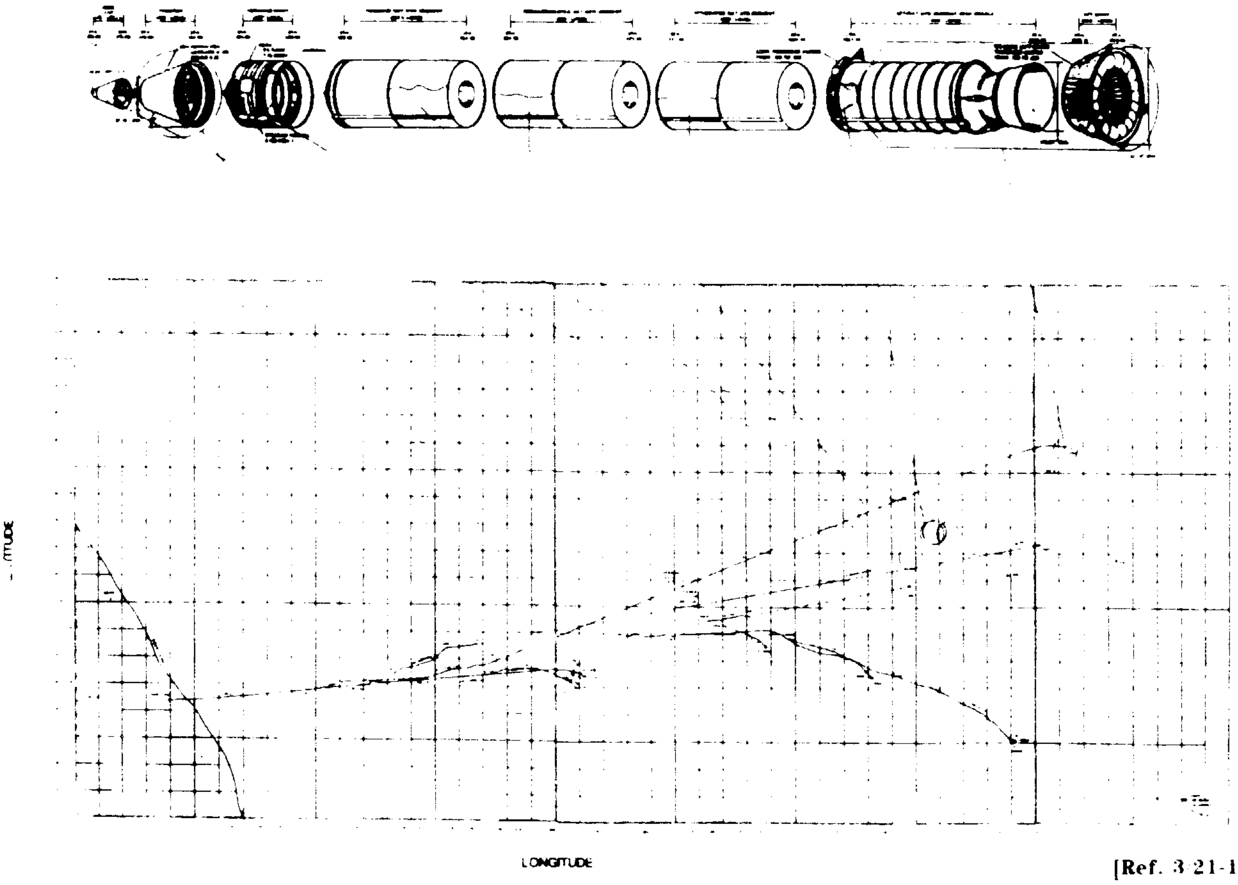
[Ref. 3/21-9]

RIGHT SOLID ROCKET BOOSTER COMPONENTS

	LOCATED	RECOVERED
O PORTION OF NOSE CAP	X	
O PORTION OF FORWARD SKIRT	X	
O AFT SKIRT ASSEMBLY	X	
O LOWER PORTION OF AFT SEGMENT	X	
O 2 CASE CYLINDERS	X	X
O ET ATTACH	X	X
O PART OF FRUSTUM	X	X

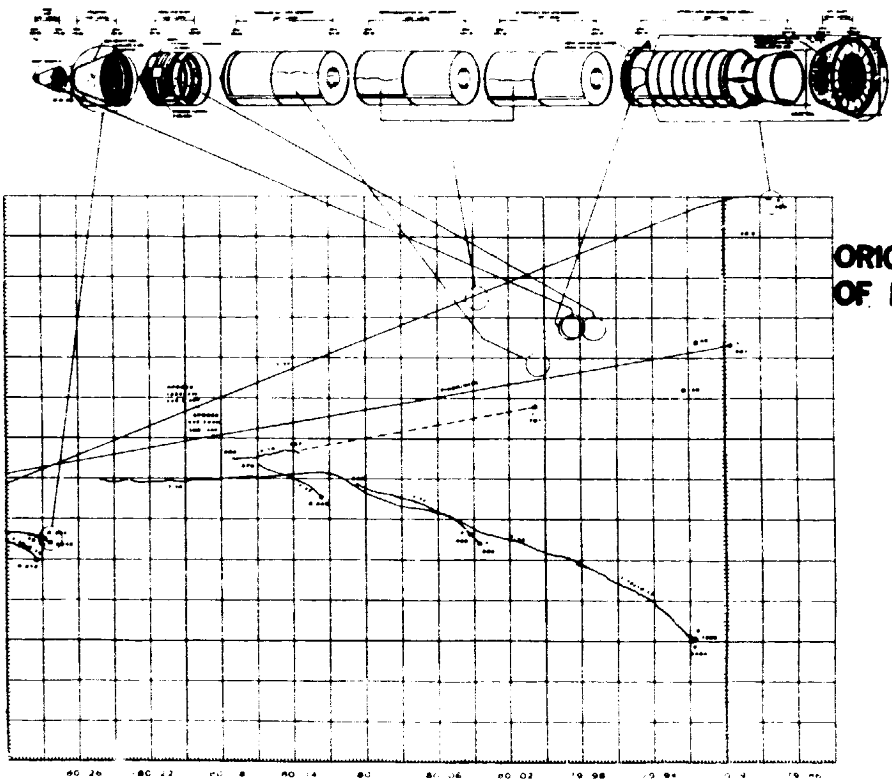
[Ref. 3/21-10]

SRB RECOVERY ASSESSMENT



[Ref. 3-21-11 1 of 2]

SRB RECOVERY ASSESSMENT



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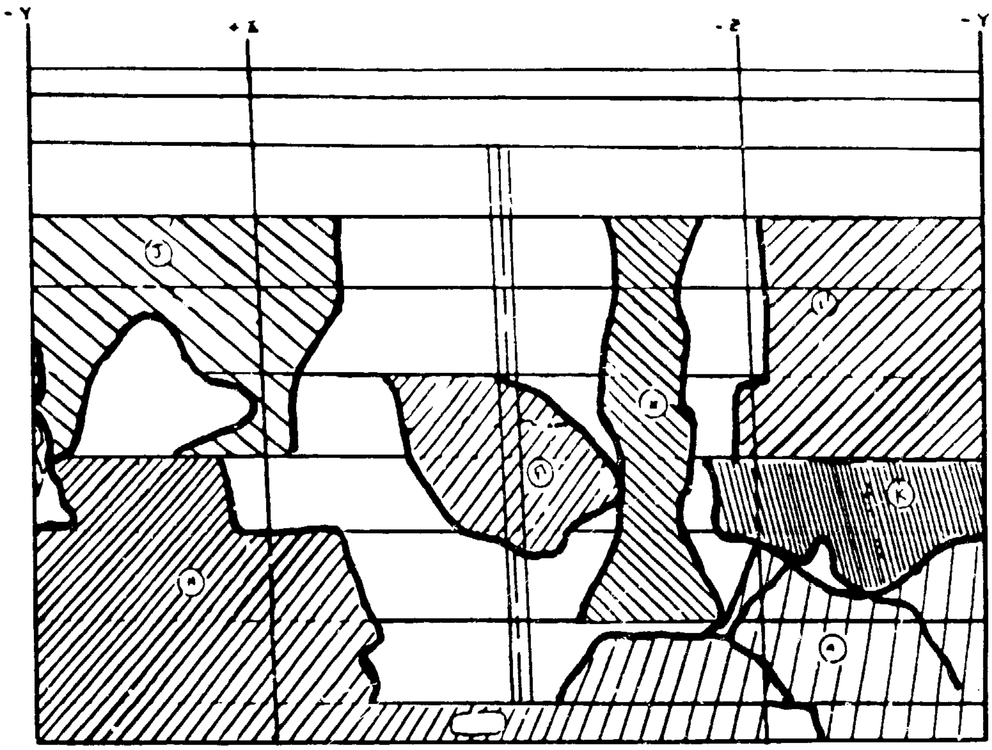
[Ref. 3-21-11 2 of 2]

RIGHT SRB AFT COMPONENTS



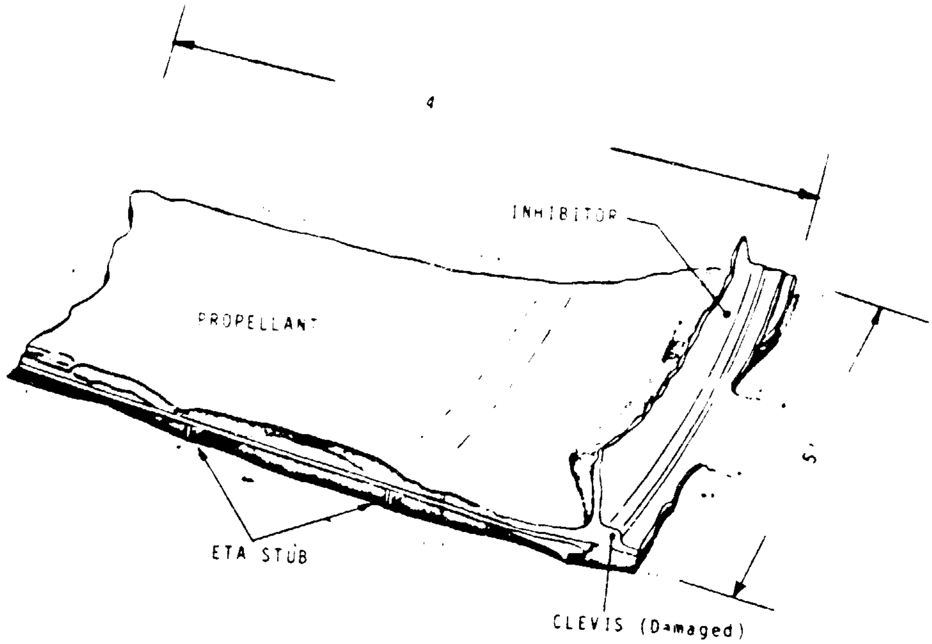
[Ref. 3-21-12]

EXTERNAL TANK ATTACH SEGMENT EVALUATION

[Ref. 3-21-13 1 of 2]

EXTERNAL TANK ATTACH SEGMENT EVALUATION

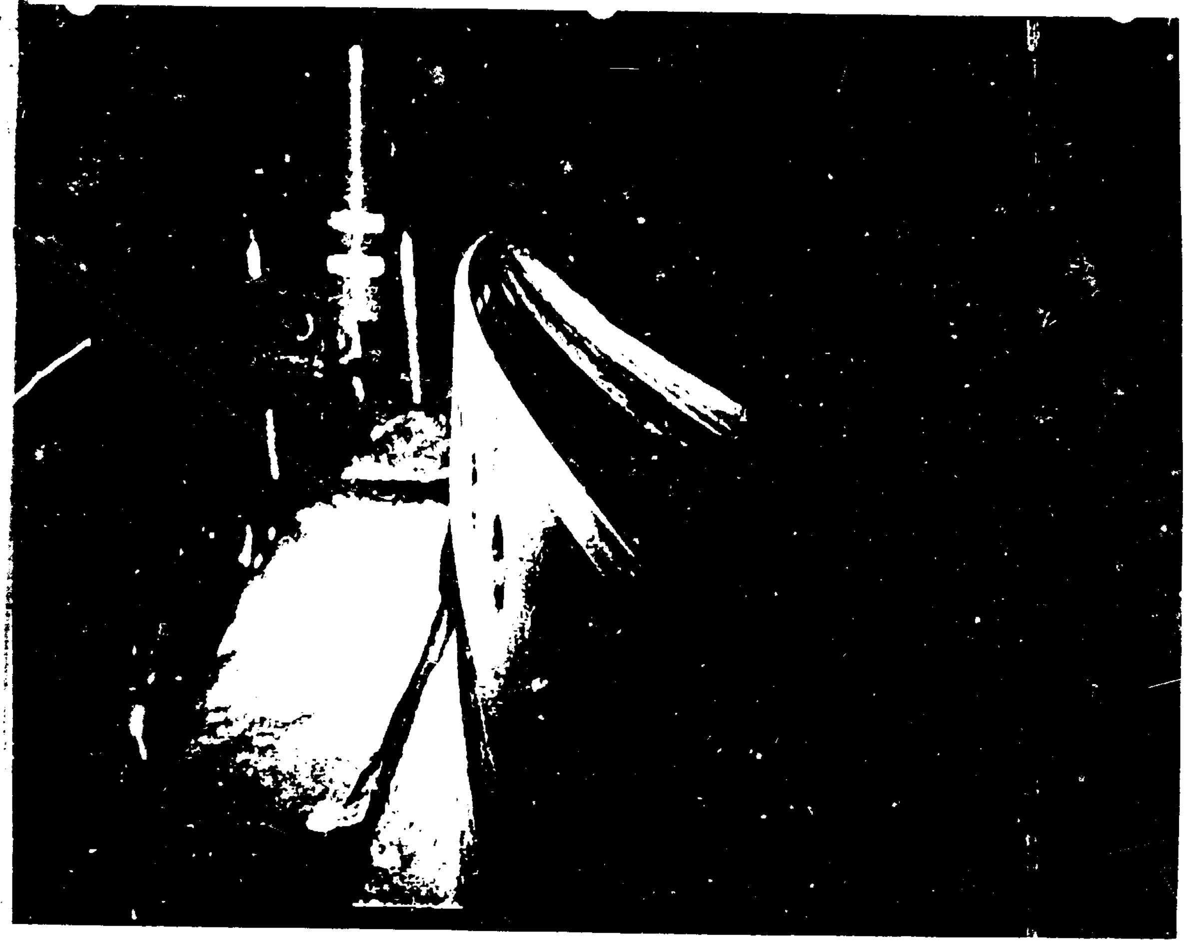
(TARGET 292)



[Ref. 3 21-13 2 of 2]

[NOT REPRODUCIBLE]

[Ref. 3 21-14]



[Ref. 3/21-15]

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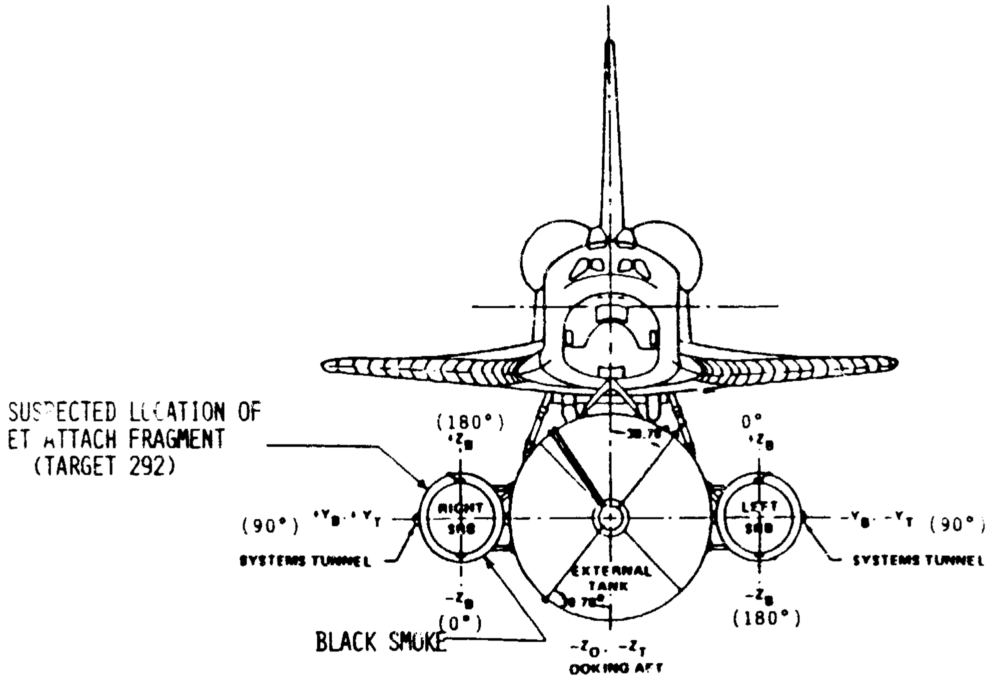
EXTERNAL TANK ATTACH SEGMENT EVALUATION
(TARGET 292)

- O PORTION RECOVERED ON 17 MARCH
- O ANALYSIS COMMENCED 20 MARCH
- O PRELIMINARY EVALUATION
  - NO PART NUMBERS OR OTHER POSITIVE IDENTIFYING FEATURES
  - CONFIRMED AFT SEGMENT
  - CONFIRMED ET ATTACH PORTION
  - EXTERNAL SURFACE DARKENED
  - ET ATTACH STUD HOLE SPACING AND DEFORMATION CONSISTENT WITH 90° TO 180° QUADRANT
  - MOST PROBABLY RIGHT SRB COMPONENT
- O EVALUATION CONTINUING

[Ref. 3/21-16]

SUSPECTED LOCATION OF RECOVERED ET ATTACH FRAGMENT



[Ref. 3 21-17]

TESTIMONY OF: DAN GERMANY, CHAIRMAN, PHOTO AND TELEVISION SUPPORT TEAM, CAPE CANAVERAL, AND DEPUTY MANAGER, SPACE STATION PROJECT OFFICE, JOHNSON FLIGHT SPACE CENTER, HOUSTON, TEXAS; CHARLES STEVENSON, GEORGE McDONOUGH, AND GEORGE ERICKSON

CHAIRMAN ROGERS: Mr. Germany, do you want to proceed?

MR. GERMANY: Yes, sir, Mr. Chairman.

CHAIRMAN ROGERS: I guess you haven't appeared before us. You might identify yourself and identify your colleagues.

MR. GERMANY: I would be glad to do that. My name is Dan Germany. My parent organization is the Johnson Space Center in Houston, Texas. My current assignment is chairman of the Photographic and TV Support Team, which is a part of the task force that is assisting the Commission in this investigation.

I have with me today three additional representatives of the team: Charlie Stevenson on my right, George McDonough on my left, and George Erickson on his left.

These are primary focal points in each of the center. We have a total integrated effort among the

2201

field centers in order to try to focus and understand all of the photographic and TV products that we have accumulated from this particular incident.

CHAIRMAN ROGERS: Thank you.

We know Mr. Stevenson very well. He testified before. Now he is here as an expert. Last time he said he speculated. Now we want to ask him what his opinion is as an expert witness. [Laughter.]

MR. GERMANY: Could I have the first chart, please.

(Viewgraph No. 2.) [Ref. 3 21-18]

I think it is important to try to give you an overview before we get into the details, Mr. Chairman, of some of this photographic activity, to help the Commission fully understand the way we are organized and we are proceeding with the efforts amongst the various teams that we have as part of the task force.

The TV team, as well as the salvage support team that you just heard from, are in fact support groups. The four analysis teams are the four blocks that show right below Admiral Truly's block, as well as Mr. Thompson's block there. Those are the teams that are responsible for taking the results of the activities that they have themselves, as well as the activities

2202

from our two support teams, and integrating them together to come up with the engineering conclusions that will be drawn as a result of the final report for this entire incident.

The activities that we're going to have for you today are centered around looking at photographic products and describing to you what we were able to see from those photographic products. The accident analysis team, with Mr. Jones and his people, are the ones that are responsible for taking that information, combining it with the engineering data base, as well as the other overall telemetry data from the flight, and coming up with the engineering conclusions that we will be forthwith as a result of the final report.

Next chart, please.

(Viewgraph No. 3.) [Ref. 3 21 19]

I have three charts that are coming up, Mr. Chairman, that will give you, help give you an overview of the photography in general. I'm not going to brief the details of the charts. We have included them for reference sake as much as anything, so as the Commission goes through its deliberations and you hear us talking about various cameras this might help you to pinpoint.

The first chart right there we are looking at is one that gives an aerial view of the pad itself.

2203

There are six positions around the radius of that pad where there are cameras that are located. The fifth position, which is the 270 degree point, you are looking across the gantry structure, so we don't really use cameras there.

But they are in those other positions, and from a total point of view we have somewhere over 100 cameras, which includes photographic cameras as well as video cameras, not only here but on the mobile launch platform, the tower itself, and the surrounding areas as you go external to the pad and the tracking cameras.

Could I have the next chart, please.

(Viewgraph No. 4.) [Ref. 3 21 20]

This is a chart that depicts the mobile launch platform itself. As you can see, you have a multitude of cameras that are located on what we call the MLP.

Next chart, please.

(Viewgraph No. 5.) [Ref. 3/21-21]

This is a chart that shows the tower itself and the locations of the cameras on there.

Next chart, please.

(Viewgraph No. 6.) [Ref. 3/21-22]

Now what we're going to do is move into the part of the presentation that deals with the integrated

2204

time line that has been put together. The photographic team uses the integrated time line as a road map, so to speak, to step through all of the events that occurred and try to understand the photography and the video that goes with that.

I'm starting off with this dynamic coordinate system chart for you because I know that it is easy to get confused in the many coordinate systems that we have for this particular vehicle. And so at the front of your integrated time line we have put this chart for clarification as much as anything else, to help you try to remember which axis is which.

And I will try to walk through this to help you try to visualize it. If you were sitting in the cockpit of the orbiter and can visualize yourself sitting there, the plus axis, plus X axis, is in this direction like so. The minus X axis is behind you.

If you think about the torso of your body, the plus Z axis is down through your feet and the minus Z axis is like up through your head. And if you can visualize your arms being out like this, this would be like the Y axis of the vehicle.

So everything that you will see on the time line is related back to this particular coordinate system, to try to help you keep that straight.

2205

The next chart, please.

(Viewgraph No. 7.) [Ref. 3/21-23]

Since the last time that you've seen this, we have changed the graphics of the time line just a little bit, and let me describe the graphics for you so you will understand it as we go through it. We tried to separate out the camera events from the other instrumentation.

On the left-hand side of your chart there, you will see the camera events. You have to read this chart from the bottom of it to the top. The bottom, you start with T-zero, the ignition command. In the subsequent charts that follow it, we read from the bottom to the top.

On the right-hand side of the chart, you will see the other telemetry events that go with the trajectory itself. Now, what we will try to do today for you is, first we will walk through this time line like this, and I will use this model to help depict part of it.

And then what we are going to do is show you a video film that we have put together that is a combination of about 14 different cameras, which includes photographic cameras as well as TV cameras all the way through this time line.

2206

And we also have it set aside—it goes back and corresponds to the events that we have on this time line. But because of the way the presentation facilities are here, we could only do one screen at a time.

So we are going to step through this way first, and then we will do the video second, and we will do the video in a stop-start manner, whichever way you want to go to help you understand the things that we are trying to depict for you.

On this particular chart, if you start at the bottom there, at the T-zero command –and let me reference myself here. This first chart shows the areas of activity that occur at about the first 3.375 seconds, and it all hinges from an imagery anomaly point of view around the business that we talk about, the puffs of smoke or the black smoke.

Up to this point, there have been different numbers associated with different events, as we have talked to you. This particular time line today is a current status of those events, trying to tie everything back and be consistent from this point on.

The confirmed smoke occurs at .678 seconds, as you see on the left-hand side of the chart. We've got two entries there. One says "confirmed smoke above the

2207

field splice," and it moves initially in the plus X direction, and then "confirmed black smoke."

All that's trying to say is that there's been a lot of discussion about the colors of the smoke. We feel, after a thorough analysis of the film that we have available to us, that that smoke is really shades of gray. And it starts off, it is kind of like a light shade and it becomes darker as it goes, before we lose sight of it after a few seconds.

So those particular events there, I have just listed them twice to try to help you understand the fact that it is not a given constant shade.

Then we have what we have been describing as multiple puffs of smoke that occur in a time frame of about .854 seconds up through 2.259 seconds. And in the video film today we will show that to you so you can graphically see what we are talking about.

CHAIRMAN ROGERS: Now, that is new information from the presentation made two weeks ago?

MR. GERMANY: Yes, sir, I believe it is, in terms of the video that you will be seeing.

GENERAL KUTYNA: Can I ask, what was the frequency of those puffs of smoke? How many puffs per second?

MR. GERMANY: Well, we are off trying to

2208

analyze that right now, General. And it appears to have a beat frequency to it. We are not prepared at this point to say exactly what it is. We do have some enhancement work going on that will help us figure that out better.

GENERAL KUTYNA: Can you give me a ballpark? Two or three puffs per second, 20 puffs per second?

MR. GERMANY: Well, the numbers that I'm hearing at this particular point are approximately about three hertz.

GENERAL KUTYNA: Three puffs per second?

MR. GERMANY: Yes. But we're in the process of understanding that and it will be finalized a little bit later on. I want to emphasize that point for you.

The last positive evidence of smoke above the right aft SRB, ET aft ring occurs at 2.733 seconds. The last positive visual indication is 3.375 seconds. And in there you will see a little parentheses that says "E-217." That is a camera that perhaps is indicating potential smoke even further than the 3.375.

I believe, based on the analysis we have done since the last time we have talked to you and what we will be doing over the next week, probably we will be able to delete that particular part. I'm not ready to take it off the chart now, but based on the stuff that

2209

we have done this week we are having difficulty validating for sure that smoke is occurring all the way up to that point.

We do feel very positive about the 2.733. and then the 3.375. So we will be updating this as we go.

DR. COVERT: Mr. Germany, on this 2.733, is the inference to draw that smoke is continually coming up until—in that interval, or is the inference that after the puffs the smoke hangs around at that interval with no material addition?

MR. GERMANY: Well, Dr. Covert, as you will see on the film, the smoke appears to not replenish itself after a point. As it is replenishing itself, it tends to be above that ring. As the vehicle moves on through that, it tends to go down.

So we're trying to figure out exactly, does that mean it stops or not, and we are just not prepared to say for sure yet.

DR. COVERT: Well, it must stop sometime.

MR. GERMANY: It must stop sometime, yes, and we are off to try to understand that.

DR. COVERT: You will report later to us what you have decided?

MR. GERMANY: Yes, sir, we sure will.

DR. WHEELON: Mr. Germany, having reviewed the

2210

similar photography of prior shuttle launches, can you state that no such smoke occurred on prior launches?

MR. GERMANY: We have been off trying to do that, and we have reviewed all of the film that we have from, I believe there is about six different launches for which people have talked about having erosion of the O-rings. On those flights, we have not been able to find any smoke that is duplicative of the smoke that we have seen on 51-L.

There are some other films we are looking at, too, but at this point we have not been able to find any visual evidence of smoke that is duplicative of what we have seen here on 51-L.

DR. WHEELON: Thank you very much.

DR. COVERT: When you say that it is duplicative, does that mean there may be other smoke of other kinds about, or are you just trying to be careful and be precise?

MR. GERMANY: I'm trying to be as precise as we can with the sometimes imprecise analysis that we do.

MR. RUMMEL: Can you account for the puffs versus continuing smoke? Does it relate to, oh, say the natural frequency of the structure or vibratory phenomena or some other situation? Do you know?

2211

MR. GERMANY: Well, the importance of seeing puffs could be related back to what is happening with the joint, and that is why we're putting it on this time line now and trying to understand that. It could be that the aerodynamics of the situation at that point could be accounting for the puffs, and I'm just not ready to say that we know which it is right now.

But we are indicating it and we are doing enhancement work, and as that becomes clear then we will report what we find.

MR. RUMMEL: Thank you.

MR. GERMANY: Could I have the next chart please.

DR. WHEELON: But just to be clear, you have said three hertz. Do you mean three puffs per second?

MR. GERMANY: Yes, sir.

DR. WHEELON: Thank you.

(Viewgraph No. 8.) [Ref. 3/21-24]

MR. GERMANY: Can you zoom in on that just a little bit, that chart, please. Bring it up a little bit closer. There you go.

Now, I've got four charts here to help try to characterize where from a visual imagery point of view we feel the location of the smoke is. There are two

2212

cameras that we use to do this, E-60, which is the one on this particular chart here.

The key to this chart is as much what you don't see as what you do see. The shaded or cross-hatched area is the area that the camera itself cannot see. The white part or the light part of the chart is what the camera can see.

Can I have the next chart, please.

(Viewgraph No. 9.) [Ref. 3/21-25]

Now, this is a little bit closer view, so you can get a better feeling for what I'm trying to describe for you. We can see smoke coming, but we can't see the origin of the smoke from these cameras. So that tells us that on that segment right there around 270 all the way out to 315, which is like 45 degrees, moving from the 270 point around—and for a reference point, the zero segment in this photo is the bottom part of the SRM, which is like—let me get this right here.

Like at this point right here, and it goes around. So we can't see the origin of the smoke from this view, and so that tells you something. And we will lead you through this and then I will draw the conclusion for you.

Could I have the next chart, please.

(Viewgraph No. 10.) [Ref. 3/21-26]

2213

Okay, this is a view from E-63, looking from the other side, and it is the same setup with respect to what the camera can see and what it cannot see.

Could I have the next chart, please, which will bring it up a little bit closer for you.

(Viewgraph No. 11.) [Ref. 3/21-27]

Now, you can see what the camera can't see there, and the significance is, with these two cameras, plus we have a camera that is from the back side, that is not on this chart, looking in this direction. It can't see the origin of the smoke. Plus, on the back side, I believe it is 217, you don't see the smoke coming from behind the SRM.

And so that isolates the smoke in our opinion from the 270 point out to around the 315 point, 315 degrees. And we feel it is somewhere perhaps around 300 degrees or some point like that. And based on other cameras we have seen, we feel it is within plus or minus about a foot of the joint in question, and that is the most we are able to see from an imagery point of view.

Next chart, please.

(Viewgraph No. 12.) [Ref. 3/21-28]

DR. WHEELON: Mr. Germany, before we leave those shaded diagrams of the shuttle indicating the coverage on the cameras that operated, I have an

2214

impression that a number of cameras in fact failed that morning, possibly because of the cold weather, and that had they been working properly, that we would have had complete coverage of the event.

Can you comment on the loss of coverage that morning?

MR. GERMANY: Yes, I can say a little bit for you. You are absolutely right, we did have cameras that failed to operate properly that particular morning. There were two cameras that would have provided excellent viewage of the area in question.

It is suspected at this time probably the cold weather was the culprit with respect to those cameras. I believe we lost somewhere like 11 cameras, and normally we maybe lose two to three on a mission. And we do believe the cold weather was responsible for that.

DR. WHEELON: Mr. Germany, I brought this up only because the photographic team is operating under a limitation of lost data, I think that what you have put forward is quite remarkable in view of that shortage.

MR. GERMANY: In fact, as a matter of fact, you will find in our report when we finish this up, Mr. Chairman, there will be some lessons learned and some

2215

recommendations we will have with respect to being able to provide be er coverage for these types of activities.

CHAIRMAN ROGERS: Mr. Germany, had you anticipated cold weather might affect the camera coverage?

MR. GERMANY: Charlie, do you want to answer that? I'm not that familiar with it.

MR. STEVENSON: We think that the cold weather probably contributed to the film breakage that we had on the two critical cameras We do purge the cameras, but we do not use a heating purge, and probably in the future we will start using some type of heating purge. And there may have also been a humidity problem, and we are looking into the possibility of correcting the humidity problem.

CHAIRMAN ROGERS: Mr. Stevenson, my question, though, was had you anticipated that weather might affect the cameras adversely?

MR. STEVENSON: No, we did not.

CHAIRMAN ROGERS: Thank you.

MR. GERMANY: Mr. Chairman, we have chart 12 on the screen now, and it is the next one after chart 7 that shows something happening from a visual imagery point of view. We have listed on here three flashes

2216

downstream of the orbiter right-hand wing. These were put on the chart because some members of the Commission and others had seen some white flashes downstream of the orbiter before.

And we are off trying to understand that, and in fact I talked with General Kutyna about it this morning. We believe that there are probably more than these three that we have listed here. However, based on the information we have right now, we can't see anything peculiar to this particular flight as a result of those flashes.

In fact, in the video films you will see in a few moments we will try to graphically depict what is being talked about. The words that are used on here was words that were picked up from this particular camera, 202.

Actually, I believe you will see that these flashes are probably as part of the plume within the SSME, and we have seen this type of phenomenon, I guess you would call it, before. So probably the next time we update this chart we will be removing these from there, because we have not been able to ascertain at this point that these are peculiar to 51-L.

Next chart, please.

(Viewgraph No. 13.) [Ref. 3/21-29]

2217

Now, chart 13 shows a time where the time line starts to get a little bit more filled up with activities that are going on. The first part we talked about was the smoke. Now, beginning at 58.788 seconds is the first time we have evidence of the flame appearing on the right-hand SRB.

As you can see from this chart, we're talking about a period of time here of about six seconds of several things happening. The first evidence of flame, and we have the flickering dynamic plume on the right-hand SRB.

And the reason you see that "TBD" or "to be determined" for the time there is because that is an item that you can't really pick up visually with your eyes as you review the film. However, we have been doing some enhancement work. We have some of that back preliminarily. That, it appears there might be a flicker, so we're off to understand that.

Now, the significance of that is as to how it relates back to the analysis of the joint and what's happening to the joint during the Max Q region. In fact, everything you will see us put on these time lines from the imagery point of view is focused to help the accident team do their engineering analysis. So things that we know that might be peculiar to the joint we put

2218

on that to help them proceed with their work, and that is the reason we are showing this.

At 59.262 seconds—

DR. WHEELON: Excuse me, Mr. Germany. Is it possible yet to tell how many flickers per second you're seeing during this flickering period?

MR. GERMANY: No, sir, Mr. Wheelon. I don't want to give you a number on that, because really I don't have a number. But once we get that, then we will be providing it.

DR. WHEELON: Do you think you will be able to measure that eventually?

MR. GERMANY: Well I hope so. But don't pin me down too closely, because we are still trying to analyze that right now, and as soon as we get something we will provide it to you.

DR. WHEELON: Thank you.

MR. GERMANY: At 59.262 we have a continuous, well-defined plume on the right-hand SRB in the plus Z, minus Y coordinates. Then at 59.763 is where you see the visual evidence of the flames from the right-hand SRB in the plus Z direction near the ET attach ring. So this is evidence that it is occurring in about the same place that we were talking about before, the same general location we were talking about before with

2219

respect to the smoke.

And then at 60.238 we have first evidence of the plume deflection, and that deflection of the plume is intermittent. And then at .248, 60.248, the first evidence of the anomalous SRB plume actually attaching itself to the 2058 ring of the ET, which is this ring in this area right here.

Then at 60.988 is the first evidence of the plume deflection being continuous. At first it was intermittent and then it became continuous.

MR. ACHESON: What is meant by the term "deflection" in this context?

MR. GERMANY: It tends to be like moving backwards like this with respect to the vehicle, and we're going to talk about that some more when we get into the film.

Then at 64.660 you see the abrupt change in the anomalous plume shape. It is the first indication of hydrogen, liquid hydrogen, leaking around this or in the vicinity of this ET 2058 ring.

And then at 64.705 is when you see the bright, sustained glow. And all of this will become more evident to you as we go through the film in a few moments.

So that is the series of events that occurs

2220

from the 58.788 up to 64.705. And the significant thing that is really happening here now is that the liquid hydrogen is starting to leak.

The next chart, please.

(Viewgraph No. 14.) [Ref. 3/21-30]

There are quite a few events that occur on this chart. But as you will notice, they all occur primarily in a two to three second point of time. And as you go through there, you can primarily see the 72 second and 73 second time period.

While it may take us a while to describe as we go through this, in fact it is happening very, very fast. On the right-hand side of the chart with the telemetry information, as you scan through there you can see that there is quite a bit of movement and things going on with the vehicle, and we're starting to see the results of that, and that occurs like in the 72 second time frame.

And then we're starting to see the visual evidence of some of that in the 73 second time frame, where we pick up at 73.124 the evidence of a circumferential white pattern on the left side of the ET aft dome.

The LH-2 tank failure, this is the point we feel that the LH-2 tank failed, near this 2058 ring

2221

frame. The next thing is you will see a hint of vapor at the inter-tank area of the ET. That says the "inter-tank stag"; it truly should say the "inter-tank area" of the ET.

After that, a sudden cloud appears along the side of the ET between the inter-tank and the aft dome. That is at 73.162. Then there's a flash that occurs at 73.191 from the region between the orbiter and the LH-2 tank.

And another flash near the SRB forward attach point occurs at 73.213. Then the first indication of intense white flash is at 73.282, and then what we call the greatly increased intensity of the white flash at 73.327, which is essentially at the point you have the major structural breakup of the vehicle.

So on the one hand, it appears that what is happening is that the right-hand SRB, the attach fitting, either the fitting itself—something has broken loose in this area. The liquid hydrogen is leaking at this point, and what's going on is this part of the vehicle is not tied in as tight as it was, so it's free to move a little bit, and we're going to show you some of that in the film.

And as that LH-2 tank lower half fails and the hydrogen is dumped out of there, you get a larger thrust

2222

increase here. At the same time, this thing is moving. You essentially end up with a structural failure of the external tank, which accounts for the total structural breakup.

So the rest of the chart, the rest of the items on the chart, just kind of pick up the remainder of the time line there.

Now, what we would like to do if we can is roll this video, and it is about a 14-1/2 minute film, Mr. Chairman. And it will help you graphically see some of the things we have been describing here.

CHAIRMAN ROGERS: Fine.

MR. STEVENSON: It'll take a second to get this stuff synched up.

(A videotape was shown.) [Not published.]

MR. STEVENSON: The first sequence, we're just going to show you a typical launch sequence.

MR. GERMANY: I might say that part of the film footage we have here on the end of this was supplied by an individual outside the agency that happened to be taking pictures from the New Smyrna Beach area. It turned out to be helpful and so we have incorporated it.

(Pause.)

MR. GERMANY: Because these things are happening so fast, we will go back and pick it through a frame at a time to help you better understand it.

2223

(Pause.)

This is the New Smyrna coverage right here.

(Pause.)

Okay. Now, it's going to continue to play, and we will go back and go through and we will isolate the time line events for you.

Okay, Charlie, why don't you describe this.

MR. STEVENSON: What we have highlighted here is the actual smoke as we see from the camera that is south of the pad and actually looking north, and we will come back and show you another camera view. This would be E-60, and here's camera E-63 that looks from the northwest toward the vehicle.

MR. GERMANY: This is the one where you can see the puffs of smoke more clearly because you've got the white SRB in the background behind you there.

MR. STEVENSON: Now we will show you both of them together.

(Pause.)

And this is just a closeup, another view.

(Pause.)

We're now moving into the three flashes that we described earlier, and we will actually show you four. You will have to watch closely in the center of the circle.

2224

(Pause.)

This is just the next view. There will be three on this view.

(Pause.)

Okay, we're now going to the plume development.

(Pause.)

This is a second view of that, and this camera will stop at two stops. The purpose is to view the plume, the SRB plumes and normal plumes.

(Pause.)

Okay. This is the first evidence of the plume deflection. Again, it's intermittent.

MR. GERMANY: That's at 60.238 seconds on the time line.

(Pause.)

MR. STEVENSON: And now it is constant.

(Pause.)

We're now going into the development of the leak.

(Pause.)

In this first indication of the LH-2 leak.

MR. GERMANY: And that was at 64.660 seconds.

MR. STEVENSON: In this frame you see the glow has moved around to where it's on both the plus and

2225

minus Z sides of the ET.

MR. GERMANY: That's at 64.705. And this is a point just a few milliseconds later that the ET LH-2 pressure deviations begin to occur. And Wayne Littles will be coming a little bit later on today and is going to pick up and talk about that for you.

MR. STEVENSON: Okay. Here we have the divergence of the rates which Dan just referred to.

MR. GERMANY: This is a computer-aided design tool that we're using to help understand what happened to the motions of the SRB at this particular time point. It is itself not doing that rocking. We're just doing that to give you an indication of the motion that takes place, so don't get confused with the information there.

MR. STEVENSON: We're moving now into the LH-2 tank failure.

(Pause.)

There you have it, and here are the first hints of vapor from the ET inter-tank area. And we will stop that for you. That's it.

MR. GERMANY: You see, this is extremely hard to see. So we have picked a frame. You learn to train your eyeballs to look for some of these things, and so we picked some to kind of accentuate it for you, because

2226

it's hard to see it right at first.

MR. STEVENSON: Okay. This one is the cloud of smoke that moves along the ET, down from the inter-tank, and the bright flame that comes between the orbiter. That's it.

Now, this will be the flash between the orbiter and the external tank.

(Pause.)

Now we're moving up to the forward attach between the ET and SRB's.

DR. RIDE: Are those single frames on the camera? Are you stepping ahead single frames?

MR. STEVENSON: Some are single frames, yes.

Okay, here is the white flash at the forward attach point, and the increased intensity.

(Pause.)

Now, this would be a combined view of the steps we just went through, the LH-2 tank failure, and it moves up to vapor from the inter-tank; then the sudden white cloud along the aft of the LH-2 tank; and then the sudden flash between the orbiter and the ET on the LH-2 tank; the flash near the upper ET-SRB attach.

And now we move into the SRB sequence, where we are showing the chute deploy. That's it.

And here we have the right SRB destruct.

2227

(Pause.)

Followed by the left SRB destruct.

(Pause.)

And this is just a summary, again. That's it.

MR. GERMANY: Mr. Chairman, that is the end of our prepared presentation.

CHAIRMAN ROGERS: Thank you. That is very good. Any questions?

Let's take a ten minute recess. Thank you very much, Mr. Germany. That was a very good presentation.

(Recess.)

STS 51-L DATA & DESIGN ANALYSIS TASK FORCE
PHOTOGRAPHIC & VIDEO SUPPORT TEAM

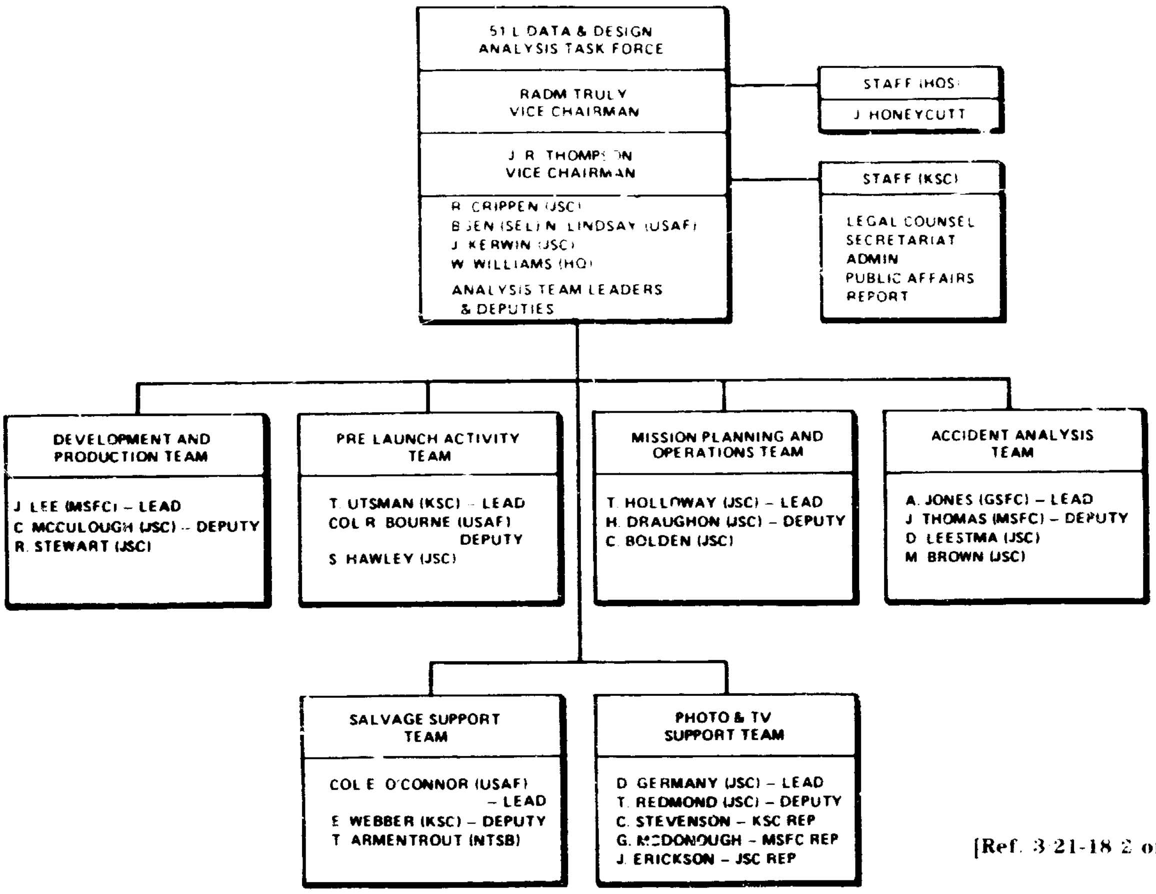
PRESENTATION TO

PRESIDENTIAL COMMISSION ON
THE SPACE SHUTTLE CHALLENGER ACCIDENT

MARCH 21, 1986

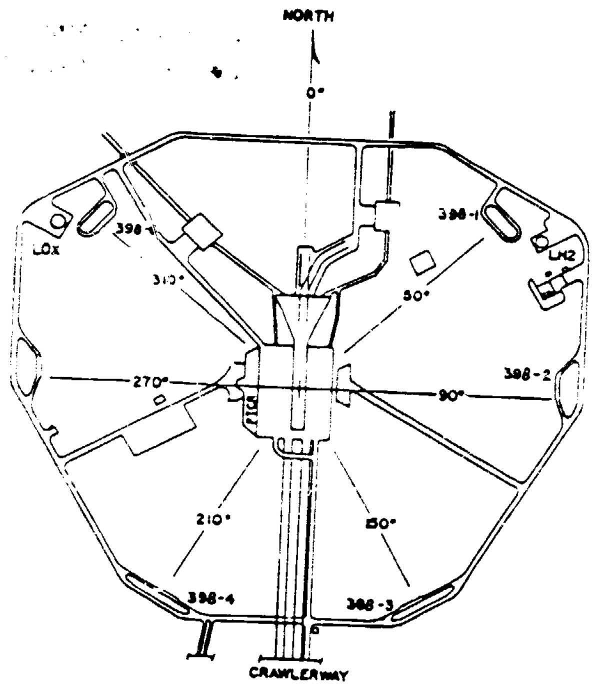
D. M. GERMANY

[Ref. 3/21-18 1 of 2]



[Ref. 3/21-18 2 of 2]

COMPLEX 39-B PERIMETER SITE LOCATIONS



COMPLEX 39B

[Ref. 3-21-19]

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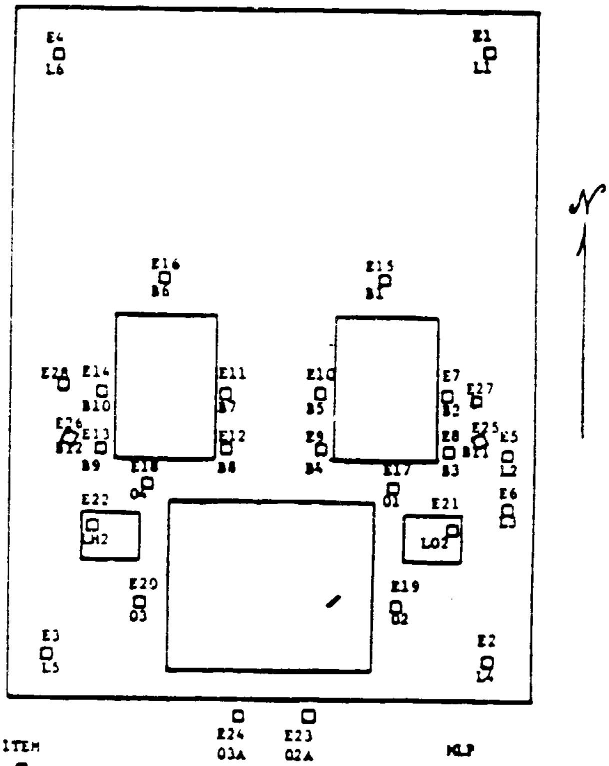

ITEM

POSITION

MLP

CAMERA LOCATIONS

[Ref. 3-21-20]

FIXED SERVICE STRUCTURE, CAMERA LOCATIONS

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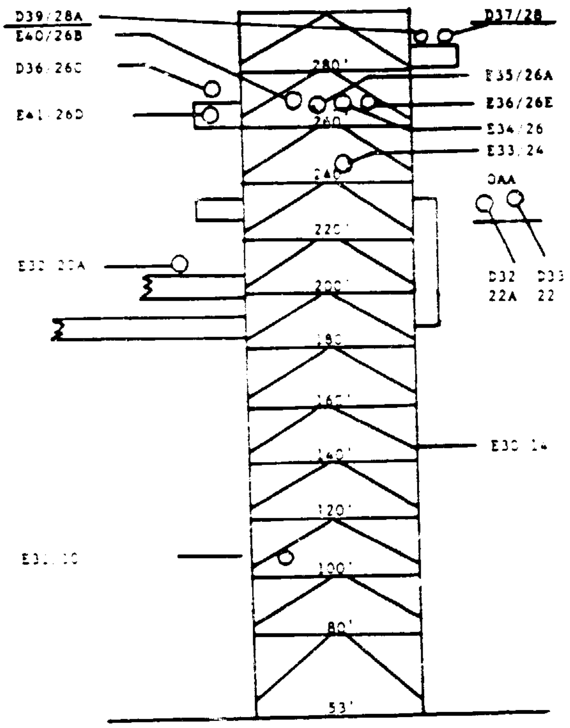
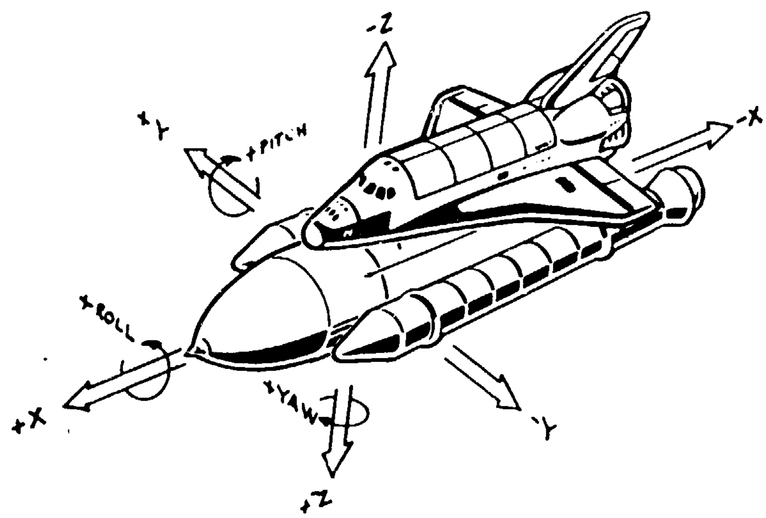


(Item Position)

FSS Camera Positions
Side 1

[Ref. 3 21-21]

DYNAMIC BODY COORDINATE SYSTEM CONVENTION	Presenter D. GERMANY
	Date 3/21/86



[Ref. 3 21-22]

NASA
Johnson Space Center

STS 51-L Incident Investigation
Integrated Events Timeline

CAMERA	PHOTOGRAPHIC EVENT	MET SEC
E60	LAST POSITIVE VISUAL INDICATION OF SMOKE DISPERSING BELOW ET AFT DOME. (E217 INDICATES POTENTIAL SMOKE TO 12.390 ANALYSIS IN PROGRESS.)	3.375
CZR-1	LAST POSITIVE EVIDENCE OF SMOKE ABOVE RIGHT AFT SRB/ET ATTACH RING.	2.733
E63	MULTIPLE PUFFS OF SMOKE	0.854 THRU 2.259
E60	CONFIRMED BLACK SMOKE ABOVE FIELD SPLICE INITIALLY MOVES IN +X DIRECTION.	0.678
E60	CONFIRMED SMOKE ABOVE FIELD SPLICE INITIALLY MOVES IN +X DIRECTION.	0.678
E9	•FIRST CONTINUOUS VERTICAL MOTION	0.230
E8	•HOLDDOWN POST 2 PIC FIRING-SMOKE PUFF FROM DOME OF FRANGIBLE NUT ON HOLDDOWN POST	0.008

MET SEC	INSTRUMENTATION EVENT	DATA SOURCE
22.204	•START L SRB MAX Q BUCKET	B47P1302C
21.604	•START R SRB MAX Q BUCKET	B47P2302C
21.124	•ROLL MNVR COMPLETED	V90R5301C
19.885	•SSME 94% COMMAND	E41M3076D
7.724	•ROLL MNVR INITIATED	V90R5301C
0.000	•SRB IGNITION COMMAND 028:16:38:00.010 G.M.T.	V99W8801C V90W8380C

Timeline scale (MET sec): 22, 21, 20, 19, 7, 3, 2, 1, 0

• INDICATES EXPECTED EVENT

[Ref. 3 21-23]

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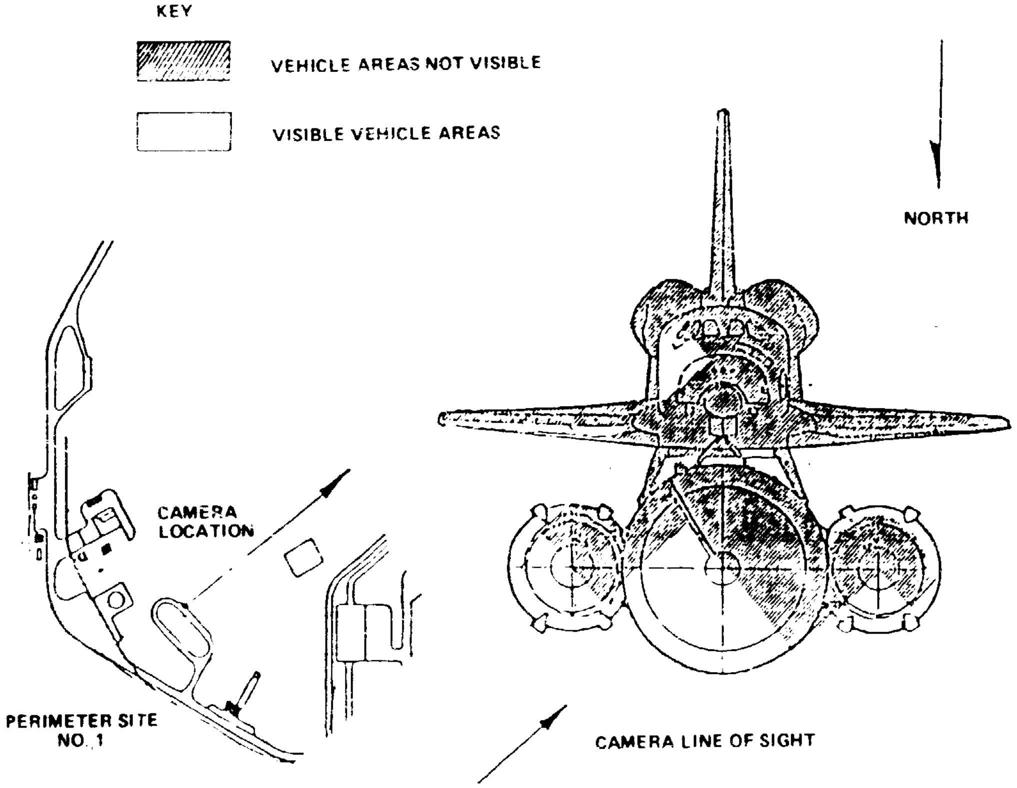
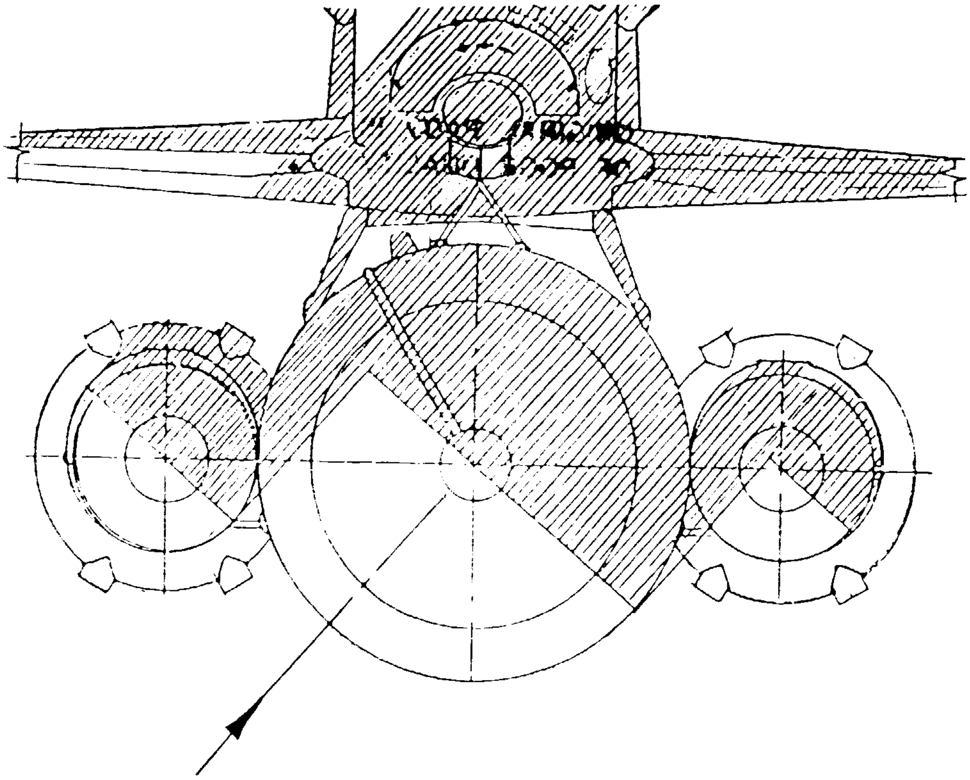


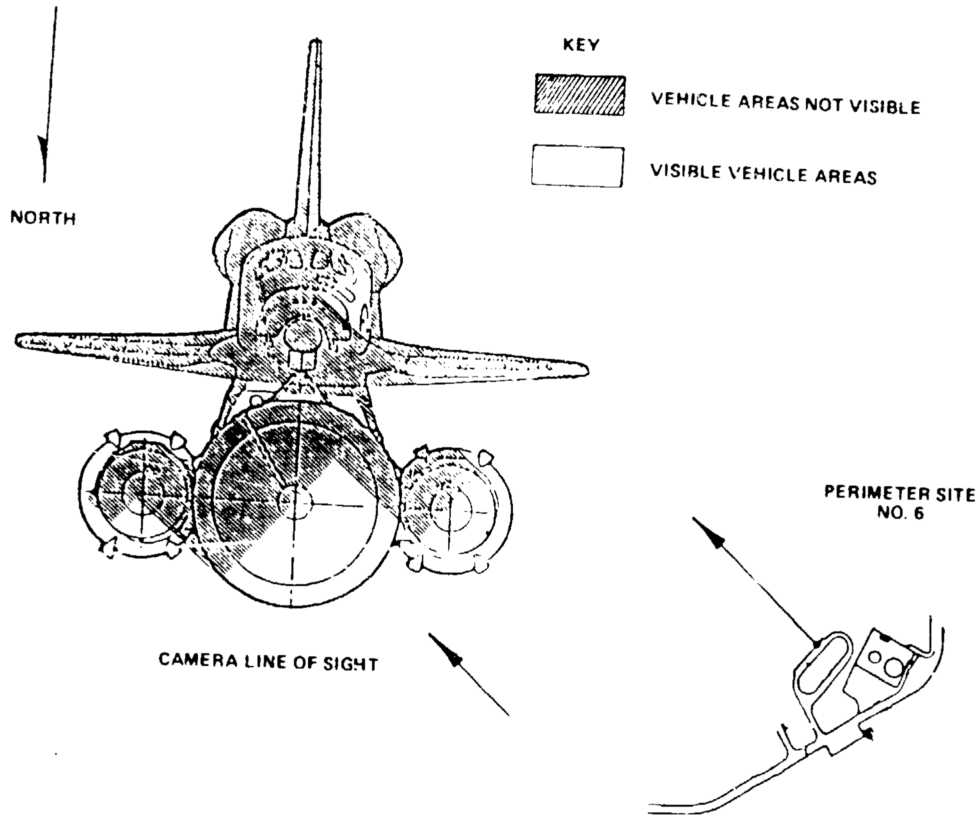
[Ref. 3 21-24]

VEHICLE PROFILE OF AREAS VISIBLE IN FILM ITEM E-60



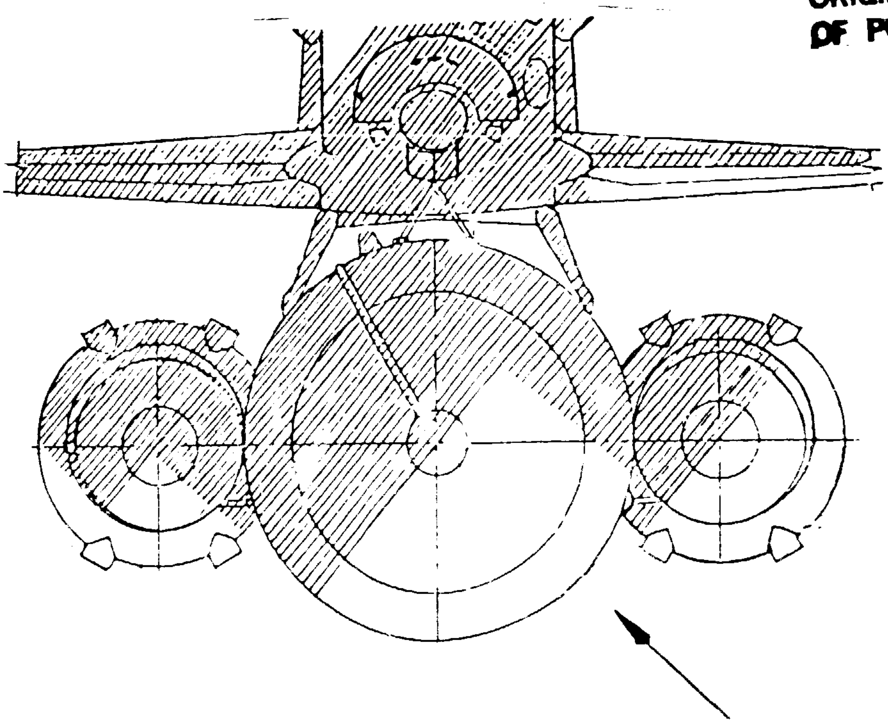
[Ref. 3 21-25]

DETAIL OF VEHICLE AREAS VISIBLE IN FILM ITEM E-60 (ALSO OTV 160)



[Ref. 3 21-26]

VEHICLE PROFILE OF AREAS VISIBLE IN FILM ITEM E- 63



DETAIL OF VEHICLE AREAS VISIBLE IN FILM ITEM E- 63

[Ref. 3/21-27]

NASA
Johnson Space Center

STS 51-L Incident Investigation
Integrated Events Timeline

CAMERA	PHOTOGRAPHIC EVENT	MET SEC		MET SEC	INSTRUMENTATION EVENT	DATA SOURCE
			54	54.324	*END R SRB MAX Q BUCKET	B47P2302C
				54.124	*END L SRB MAX Q BUCKET	B47P1302C
			51	51.886	*SSME 104% COMMAND	E41M3076D
E202	3RD FLASH DOWNSTREAM OF ORBITER RIGHT HAND WING	48.418	48			
E202	2ND FLASH DOWNSTREAM OF ORBITER RIGHT HAND WING	48.118				
E202	1ST FLASH DOWNSTREAM OF ORBITER RIGHT HAND WING	45.217	45			
			36	36.990	INTERVAL OF 36.990 TO 62.990 ROLL AND YAW ATTITUDE ERROR RESPONSE TO WIND	V95H3522C V95H3523C
			35	35.406	*SSME 65% COMMAND TVC-COMMAND RESPONSE	E41M3076D

*INDICATES EXPECTED EVENT

[Ref. 3/21-28]

NASA

Johnson Space Center

STS 51-L Incident Investigation
Integrated Events Timeline

CAMERA	PHOTOGRAPHIC EVENT	MET SEC
E204	BRIGHT SUSTAINED GLOW ON +Z AND -Z SIDES OF ET	64.705
E204	ABRUPT CHANGE IN ANOMALOUS PLUME SHAPE, FIRST INDICATION OF LH2 LEAK NEAR ET 2058 RING FRAME	64.660
E207	FIRST EVIDENCE OF PLUME DEFLECTION CONTINUOUS	60.986
E203	FIRST EVIDENCE OF ANOMALOUS SRB PLUME ATTACHING TO THE ET 2058 RING FRAME	60.248
E207	FIRST EVIDENCE OF PLUME DEFLECTION - INTERMITTANT	60.238
E204	VISUAL EVIDENCE OF FLAME FROM R SRB IN +Z DIRECTION, NEAR ET ATTACH RING (SEEN FROM SOUTH SIDE).	59.763
E207	CONTINUOUS WELL-DEFINED PLUME ON R SRB IN +Z, -Y COORDINATES	59.262
E207	FLICKERING DYNAMIC PLUME ON R SRB	TBD
E207	FIRST EVIDENCE OF FLAME ON R SRB	58.[illegible]88

MET SEC	INSTRUMENTATION EVENT	DATA SOURCE
66.764	START ET LH_2 ULLAGE PRESSURE DEVIATIONS	T41P1700C
65.524	LHOB ELEVON ACTUATOR DELTA P CHANGE	V58P0866C
65.404	END VEHICLE PITCH RATE CHANGE	V90R2525C L
64.937	START ME LARGE PITCH VARIATIONS	V58H1100A
64.604	START VEHICLE PITCH RATE CHANGE	V90R2525C L
63.924	RHOB ELEVON ACTUATOR DELTA P CHANGE	V58P0966C
62.484	RHOB ELEV ACT HINGE MOMENT SPIKE	V58P0966C
62.09[illegible]	START L SRB TVC ROCK ACT SPIKE	B58H1150C
60.004	SRB CHAMBER PRESS DIVERGENCE RH VS LH	B47P2302C R B47P1302C L
59.000	*RECONSTRUCTED MAX Q 720 PSF	JSC DATA

[Ref. 3/21-29]

ORIGINAL PAGE IS
OF POOR QUALITY

NASA
Johnson Space Center

STS 51-L Incident Investigation Integrated Events Timeline

CAMERA	PHOTOGRAPHIC EVENT	MET SEC
E230	L SRB RSS DESTRUCT	110.264
E202	R SRB RSS DESTRUCT	110.250
E207	R SRB NOSE CAP SEP/CHUTE DEPLOYMENT	76.437
E204	BRIGHT FLASH IN VICINITY OF ORBITER NOSE	74.587
E204	GREATLY INCREASED INTENSITY OF WHITE FLASH	73.327
E204	FIRST INDICATION INTENSE WHITE FLASH AT SRB FWD ATTACH POINT	73.282
E204	FLASH NEAR SRB FWD ATTACH (INTERSTAGE) AND BRIGHTENING OF FLASH BETWEEN ORBITER AND ET	73.213
E204	FLASH FROM REGION BETWEEN ORBITER AND LH2 TANK	73.191
E207	SUDDEN CLOUD ALONG SIDE OF ET BETWEEN INTERTANK AND AFT DOME	73.162
E207	FIRST HINT OF VAPOR AT INTERTANK STAGE	73.137
E204	EVIDENCE OF CIRCUMFERENTIAL WHITE PATTERN ON LEFT SIDE OF ET AFT DOME (LH2 TANK FAILURE NEAR 2058 RING FRAME)	73.124

MET SEC	INSTRUMENTATION EVENT	DATA SOURCE
74.130	LAST RF SIGNAL FROM ORBITER	GROUND VIDEO & RECEIVER AGC
73.631	END OF LAST DATA FRAME WITH SYNC AND FRAME COUNT	RAW DATA DUMP
73.618	APU 1 GAS GENERATOR CHAMBER PRESS	V46P0120A
	LAST VALIDATED TELEMETRY MEASUREMENTS	
73.615	L3L RCS JET CHAMBER PRESS	V42P2545A
73.543	ME 1 IN SHUTDOWN HPFT DISCH TEMP IN REDLINE EXCEEDED	E41J1512B
73.399	ME'S APPROACHING HPFT DISCH TEMP REDLINE	E41T2010D
73.377	START RCS JET CHAMBER PRESS FLUCTUATIONS	V42P1552A
73.159	ME'S RESPONDING TO LOSS OF FUEL AND OXID INLET PRESS	E41P3016D
73.055	VEH MAX LATERAL -Y ACCEL, 0.254 G	V98A1581C
73.044	R SRB CHAMBER PRESS 24 PSI (APPROX. 6 SIGMA) LOWER THAN L SRB	B47P2302C R B47P1302C L
73.010	ORBITER & TDRS RATE COMPARE AT LAST TDRS DATA	
72.964	START OF SHARP MPS LO2 INLET PRESSURE DROP	V41P1130C
72.564	SRB MAJOR HI RATE ACTUATOR MOTION	B58H1151C
72.564	START OF LH2 TANK PRESS DECREASE WITH TWO FLOW CONTROL VALVES OPEN	T41P1700C
72.515	VEH MAX LATERAL +Y ACCEL 0.227 G	V98A1581C
72.497	ME PITCH/ROLL GIMBAL RATE (5°/SEC)	V58H1100A
72.476	SRB MAJOR HI RATE ACTUATOR CMD	V79H2111A
72.281	START DIVERGENT +PITCH RATES R vs L SRB	V90R2527C R V90R2525C L
72.201	START DIVERGENT +YAW RATES R vs L SRB	V90R2528C R V90R2526C L

[Ref. 3 21-30]

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CHAIRMAN ROGERS. The Commission will come to order.
Dr. Littles
Dr. Littles, if you would introduce your panel, please.
(Witnesses sworn.)

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TESTIMONY OF WAYNE LITTLES, ASSOCIATE DIRECTOR FOR ENGINEERING, MARSHALL SPACE FLIGHT CENTER, HAROLD N. SCOFIELD, CHIEF, CONTROL SYSTEMS DIVISION, MARSHALL SPACE FLIGHT CENTER, AND ROBERT S. RYAN, CHIEF, STRUCTURAL DYNAMICS DIVISION SYSTEMS DYNAMICS LABORATORY, MARSHALL SPACE FLIGHT CENTER

DR. LITTLES: Yes, sir. My name is Wayne Littles, and I am associate director for engineering at Marshall, and on my left I have Bob Ryan, who will be discussing the loads and joint dynamics, and Harold Scofield will be discussing the flight reconstruction and trajectory.

I would like to begin with a brief overview of the fault tree and scenarios, and update where we have changed since we last presented them to you two weeks ago, and point out the areas we will be providing information on today to allow closure of some of these areas. If you would go to Chart W-1, please.

(Viewgraph W-1.) [Ref. 3/21-31]

DR. LITTLES: Okay, looking down in the righthand corner you will note that we have changed the external tank. When we made our last presentation, we had that as a yellow, which was a possible contributor. We are going to be recommending today with the

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information that we present to you that we change that to improbable.

CHAIRMAN ROGERS: Doctor, could I interrupt just a moment to say that we are going to try if it is possible to conclude this session by 1:00 or 1:30. If it turns out that it runs longer than that, then we will take a recess and come back after the recess, but if it is possible, we would like to end at 1:00 or 1:30, and I thought for the convenience of everyone here I would like to announce that.

Okay, proceed, Doctor.

DR. LITTLES: Okay, go to Chart W-2, please.

(Viewgraph W-2.) [Ref. 3/21-32]

DR. LITTLES: This is the fault tree which is the expansion of the external tank in SRM. When we presented two weeks ago we had a number of areas associated with the external tank in yellow. We were still looking at the possibility of liftoff damage and debris possibly causing impact to the tank. We will present information to you that concludes that that is highly unlikely. We are also looking at a structural flaw as a possible contributor, and we will present data on that.

Could we go then to Chart W-3, please?

(Viewgraph W-3.) [Ref. 3/21-33]

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DR. LITTLES: This is the chart that provides the detailed scenarios associated with the hot gas leak, and when we provide the information to close the tank, we will also be providing the information and recommending that we close Item 2 on the scenario tree, which is the hydrogen leak impinging on the SRM and causing that failure.

We will also be providing information and recommending that we close the Item 4C, which is the leak check port, and we will be providing details of the load item, Item 5. Bob Ryan will be discussing that in detail. We had that in green when we talked to you before. We are going to provide more detailed data today. Also, since we spoke to you last, we have added two items to the scenario tree.

Since we have closed out loads and determined that the loads are within the design limits, as we discussed before, they could still be a possible contributor to a potentially damaged joint. We have reflected that by adding the item on the bottom right, liftoff flight loads effect on the degraded seal, so that is an added item. We have also added Item 7 to make this chart more complete. We have considered the potential of a case rupture.

We have not provided that information yet to

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the task force. We will be doing that next week. But based on what we know, we consider that to be improbable, and we are carrying that as green contingent upon presenting that information to the task force and getting them to approve that next week.

So, when we close those items out, what is remaining then are the items associated with the joint itself. Those items deal with potential damage to the joint during mating which is Item 3, that is, damage to the O-rings and/or the tang/clevis. The items associated with secondary O-ring defect in the joint or a delayed actuation of the O-ring, and Item 6, which is putty holding the pressure off the joint combined with low temperature.

So, we are left with items that—those items which either individually or potentially in combination, defects, will and/or possibly a low temperature, and possibly a combination of those things being the thing that initiated the failure in those joints, and we will go through at the end, the testing that we have under way to try to define which one of those items or which combinations might have caused the failure. Could we go to Chart 5, please?

(Viewgraph W-5.) [Ref. 3 21-34]

DR. LITTLES: I will now be getting to the

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information which we have accumulated in summary fashion, and recommend to you based on these data that we close the tank. The investigation approach—we reviewed all of the flight data for 51L for the tank, and the only anomaly that we saw in the tank itself, and this was mentioned by Dan Germany when we went over the time line, was that there was a change in the ullage pressure rise rate at 66 seconds, and that got progressively worse with time. Other than that, all of the flight data was perfectly nominal.

We have reviewed all of the Challenger external tank history, all of the build paper. We have reviewed all of the x-rays. That has been done by four different people on all the x-rays, and we have found no irregularity in all of that paper review which would indicate that anything on the tank was a contributor to the anomaly.

We have evaluated all of the potential anomalies, the hypothetical anomalies that are on the anomaly tree, and I will now address each one of those items in summary fashion and provide the rationale to you as to why we think they should all be closed out. Could we go now to Chart W-2, please?

(Viewgraph W2.) [Ref. 3 21-32]

DR. LITTLES: Okay, this is just recalling

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that free again to point out the things that we want to cover again. It is going to be the liftoff damage and debris. The potential of a structural flaw, premature year shape larger detonation or structural overload. Could we go now to Chart 6?

(Viewgraph W-6.) [Ref. 3/21-35]

DR. LITTLES: Okay, this item on the fault tree deals with the potential failure of the external tank due to a structural flaw. We have reviewed, as I have said, all of the build paper x-rays, and we found no inappropriate dispositions.

Now, we did find in that review one x-ray which had an indicated defect. It was about four-tenths of an inch long. It was a weld that was located on the minus Z axis on the 2058 ring frame. Now, that is not in the area where we had the anomaly. It is on the minus Z axis of the vehicle. We have analyzed that flaw, and the analysis indicates that it should not have contributed to the failure. As a matter of fact, the critical flaw in that area would be something like 5.2 inches, and you would have to have a flaw of something like .65 inches before it would break through and have a leak.

The growth rate of a flaw in that area with the stresses that are imposed on that weld is very, very

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low. We would expect it to grow something less than 1/10,000ths of an inch per cycle. We have also uncovered the piece of hareware that contained that flaw, and there is no evidence looking at that piece of material that the flaw was involved in the failure, so we conclude that is improbable that a structural flaw initiated the failure of the tank.

CHAIRMAN ROGERS: Dr. Littles, in questioning some of the Martin Marietta people who had the responsibility for this, apparently they found out that an employee failed to discover this imperfection that you have described, and they apparently have interviewed him, and so forth. Is this a problem that you find recurring that there are mistakes on the part of the contractors that maybe are getting more regular? In other words, there is more sloppy work being done by your contractors in some of these things?

DR. LITTLES: Well, I don't know that I have all the information to answer that question completely. I am not aware of any progressive degradation in quality. It does happen from time to time. There are human mistakes that do come about, but I am not aware of any degradation in quality.

CHAIRMAN ROGERS: Well, I think that is one of the things the Commission will want to focus on in its

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report whether we need to beef up the supervision of the contractors in some of these things. I think it is fortunate in this case that you found the portion that we are talking about, and obviously it didn't contribute to the accident.

DR. LITTLES: Yes, that was fortunate, but as I indicated, it was not in the right orientation at all. It was on the minus Z axis, which puts it around pretty far from where we see the plume. It was fortunate that we found it.

CHAIRMAN ROGERS: In other words, the imperfection did show up on the x-ray. It just wasn't reported by the employee?

DR. LITTLES: Well, it did show up on the x-ray. Now, my understanding is that something like a half a dozen people had re-reviewed that x-ray, and I was told that only about half of them actually saw it. So it wasn't something that jumped out at you. It wasn't too obvious. So it wasn't something that he failed to report, but something he just failed to detect, just a human error.

Now, if I could have Chart 7, please.

(Viewgraph W-7.) [Ref. 3 21-36]

DR LITTLES: Okay, this item deals with the structural overload. As I indicated, Bob Ryan will

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discuss the loads in more detail, but what we have found is that the liftoff and flight loads on the tank were well within the design limits. There was nothing greater than 80 percent of the maximum, and of course there was the safety factor on top of this. The ullage pressure was maintained well within limits. There was no cursion on pressure. The trajectory was nominal up to the point of the incident. There was no excessive heating that could have caused an overload of the structure, and we have also done analysis that indicates that if we had lost some TPS in the area of interest during this time frame, that the heating would have been low enough that we still wouldn't have degraded the structure to the point of having the failure. So, we conclud-ed that it is improbable that a structural overload from flight loads or excessive ullage pressure or excessive heating initiated the failure.

Chart 8, please.

(Viewgraph W-8.) [Ref. 3 21-37]

DR. LITTLES: Okay. This is an item I discussed when we last were together, but just for completeness I have included it again, and it deals with the premature detonation of the linear shape charge. The bottom line, of course, is, we did find these linear shape charges with the recovered hardware, and they were

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intact, so they were not involved in the incident.

Chart W-14, please

(Slide W-14.) [Rev. 3 21-38]

DR. LITTLES: Okay, this item deals with damage to the tank from debris which could have caused a leak. The ice team when they went out, and they went out more than once that night, but when they went out at T minus 20 minutes, they saw no evidence of a leak.

Could we see Chart W-14 please, Okay. This is a photograph that was taken during the night that contains the area of interest, and what I wanted to point out to you was that they have a very good view of that area. The right hand SRB is on the left of the picture there, and the area of interest is, of course, around toward the tank, so they had a very good view of that. If you could give me Chart 14A, please.

(Slide W-14A.) [Ref. 3 21-39]

DR. LITTLES: Fourteen-A is just a little closer view of that area, so you see, they had a very good view in that area, and if there had been a leak there, I think they would have seen it. There was no evidence from the cameras on the pad of any debris. If you could give me now Chart 10, please.

(Viewgraph W-10.) [Ref. 3 21-40]

DR. LITTLES: You have seen these camera locations described by Dan Germany earlier. What this

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sketch is is a composite of a number of the cameras. He mentioned E-63. You see that one down at the bottom right. And E-60, to the left, and several cameras that are shooting across from 90 to 270. You see those cameras give you a pretty good view of this area.

After liftoff at about one and a half seconds there is also a camera which is E-31, which is in the top right, which gives you a view of another portion of that area, and so you see there is really only a small portion that you can't see between liftoff and the second and a half, and in reviewing those films we see no evidence of any debris in that area.

One of the possible contributors to debris, one thing that we were concerned about earlier was that there had been a modification made to the covers for the hold-down posts and there are springs in there, and those springs, several of them were missing. There has been a good bit of work done on that, and it has been concluded that those could not have been a contributor to debris. An analysis done by KSC indicates that the springs could not physically get out of that cover prior to greater than eight-tenths of a second. Hence, since we saw the puff of smoke at .68 seconds, that is not consistent, and so we conclude that that was not a debris source.

You will also note in looking at the sketch on

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the screen, Chart W-10, that the area of concern, the righthand SRB, is well away from the fixed support structure on the stand, which is where the ice was. That was of some concern that night. And so if a piece of ice were going to cause debris, it would have to come off of that stand, and it would be a very rough trajectory to get it around to the point of interest, and it is improbable that that could have happened, but there has been some analysis and tests done associated with ice, and what those analyses and tests indicate is that for a reasonable size piece of ice, that you would not get penetration of the tank

There has been some recent testing done with ice particles as large as a quarter of an inch in diameter and four inches long fired out of a gun at a velocity of 300 feet per second, and they did not damage the tank. Of course, it goes through the thermal protection that is on the tank, but it actually shattered when it hit the metal underneath, and there are a number of other tests like that, but a particle of ice falling off the fixed support structure couldn't attain anything like 300-foot-per-second velocity, and so we conclude that ice is not a probable source of debris relative to penetrating the tank and causing a leak.

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There have been some other tests done which are of some interest relative to both the ice team seeing the leak and a jet impinging on the solid rocket motor. There have been tests performed at Denver, and I am going to show a videotape in a moment showing these tests. These tests indicate that very small quantities of hydrogen escaping from the tank would be visible. As a matter of fact, tests have been done down to something like 3/1,000ths of a pound per second, and you can still see the hydrogen coming into the atmosphere, so if the team, if it had been leaking when the team had gone out, they probably would have seen it.

There have also been tests done at Marshall where we have impinged a burning jet of hydrogen, again, fairly small quantities of flow onto the materials which are present on the solid rocket motor in the area of interest. There is a significant quantity if you go back to Chart 14-A, please.

(Slide 14-A.) [Ref. 3 21-39]

DR. LITTLES: There is a significant quantity of instafoam which is on the ET/SRB attach rings. It is about four inches deep. There is also some cork on the ring that closes out the pins on the joint, and of course there is paint in the area, and if you impinge small quantities of burning hydrogen on those materials

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you see significant quantities of smoke and flame, and what I am doing is trying to relate this to the fairly small quantity of smoke that we saw with the initial puff at .68 seconds. Could we see that videotape now, please?

(A videotape was shown.) [Not published.]

DR. LITTLES: There is sound with this. Are we going to get it? Okay, it is narrated, but we are not going to get the sound. Okay, the test conditions are on the screen. The first test, what you see is on the left. You see there is a small hole. This is something like .003 pounds per second. There are three series of tests on here at varying flow rates, but .003 for the equivalent conditions we had at KSC was the lowest case that was run. That wasn't the limit of visibility. It was just the lowest one they ran. Again, here is another view. You can still see it coming out.

CHAIRMAN ROGERS: So your conclusion is, the ice team would have seen that leak if there had been one?

DR. LITTLES: Yes, sir, I am confident they would. They had a very good view, as we showed in the earlier photographs. They could see the area very well, and you can see that you see that leak very well.

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VICE CHAIRMAN ARMSTRONG: Is that water condensation you see?

DR. LITTLES: Yes, it is cold hydrogen with the moisture content of the air making it visible.

(Pause.)

DR. LITTLES: Okay, the next series of tests are those that I mentioned, and this is .01 pounds per second of hydrogen being ignited, and you see the test fixture there. It will be coming from your left to the right. What's on the right are simulated portions of the material on the solid rocket motor. And the burning jet will be impinging on the material on the right, and you can see the smoke and flame characteristics.

There are four of these tests. The narration tells what they were, and I didn't bring that with me, because I thought we would be hearing the sound, but the first one here was, as I recall, just the jet impinging on paint, and you don't see a great deal that comes from that. You can see some smoke, but not a large quantity.

The next one that we will see will be one of the two that is of primary interest because it will be the burning jet, I believe, impinging on the instafoam, and the instafoam again is on the ET attach ring, in

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that area, and you can see large quantities of smoke and flame, and this had a simulated quantity of instafoam, and it burned for quite a long period of time, and again, it is a very low flow of hydrogen.

This again was just some smaller quantities of material, paint primarily. I believe this one also had the cork and the hypolon paint. And the last one you will see will be as composite panel which has the instafoam, the cork, and other materials that are in that same area.

(Pause.)

DR. LITTLES: You can see that there are fairly copious quantities of smoke and flame with that simulated material, and this, of course, is qualitative to some extent. But it would seem to indicate that you would see potentially more smoke than we saw from that initial puff. Okay, could we go to Chart W-10, please?

(Viewgraph W-10.) [Ref. 3/21-40]

DR. WALKER: Could you just repeat what the size of the orifices were in the first set of tests?

DR. LITTLES: That orifice was .06 square inches, I believe, and the flow rate was .01 pounds per second. Are you referring to the burning test?

DR. WALKER: No. I was referring to the first set of tests.

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DR. LITTLES: The first set of tests. I don't have those written down in front of me, Dr Walker, but I will get those for you. I remember the flow rate was .003, but I don't remember what the diameter was. I will get that for you.

Chart W-10, please—I am sorry, W-11. I am out of sequence.

(Viewgraph W-11.) [Ref. 3/21-41]

DR. LITTLES: So even though we don't feel that it was likely that we had a leak from the tank, we have also done some analysis to try to determine what the effect of the leak would be on the joint. What we have done is, we have used the maximum amount of leakage which would not be detected by the instrumentation. That turns out to be something like four pounds per second of hydrogen, and we have mixed that stochiametrically, and really it would be almost impossible to get a stochiametric mixture. So that is a conservative assumption. We have mixed that stochiametrically and burned it and impinged it directly on the membrane and the joint.

Now, what happens when you do that, of course, is that it doesn't stay in one place for the whole period from zero up to the time of the incident, because the velocity of the vehicle, the aerodynamics around the

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vehicle are going to be causing that plume to sweep back away from the point. When we did that analysis, we came up with a maximum temperature on the membrane of about 650 degrees, and on the O-ring of 360 degrees Fahrenheit.

Now, we have done tests that indicated that after you get the O-ring sealed and have pressure behind it, that you can maintain that. It will maintain a seal up to a temperature of 1,000 degrees.

GENERAL KUTYNA: Dr. Littles, when you did those tests, what kind of air flow did you assume on the vehicle? Did you just assume a Mach 2 air flow, or did you actually look at all of the eddies and currents that were going around those tanks at the time?

DR. LITTLES: We used the vehicle aerodynamics. This question has been raised. As a matter of fact, I was going to mention later that this is one area where Thiokol has requested some additional information, and, as a matter of fact, we have Thiokol and the people from Rockwell who were responsible for the flow fields, and our own people, meeting today to review this analysis, and particularly to review the flow fields that we are using.

Now, there were some assumptions made in the flow field that we used. We used the free stream flow

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field. In the area where we have this leak, of course, between the tank and the SRB, at some point in flight after you start getting a lot of shocks in that area, the assumption we made would not be completely valid. So that there is an assumption there, but there are also assumptions associated, as I mentioned, with the fact that we have assumed that it is stochiametric, which is a worst case. We haven't accounted with the large leakage that we have used here for any cooling effect. There would be a cooling core in the center of this jet. So there are some questions about the assumptions we made there, and those are being worked today, and I think we will be able to resolve that, as I said, because there are a lot of conservative things in here even with that simplified assumption that we have made.

There is another point, too, that I think I should make. Well, two points. One is that, you see, we have a reasonably large margin between the temperature we are predicting and what we think or what the test data says the O-rings could withstand.

And the other thing, I guess, is that we don't feel, based upon the other data, and I think there is quite an accumulation of data now, that the tank was involved. What we are doing here is assuming the worst

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case leak to try to see what that would do to the motor, but we have no basis for believing that such a leak existed. So, we are trying to resolve that point.

DR. WHEELON: Dr. Littles, I find your conclusion both reassuring and just a little surprising. Let me read the conclusion. Tests indicate that the O-ring will maintain the seal at 1,000 degrees Fahrenheit. The reason I find that a little surprising is that in prior sessions we have heard a good deal of description of confusion and differences about what the low temperature end validity of that seal would be, whether it would function at 30 degrees or only at 54, and yet you seem quite sure that at the upper end of 1,000 degrees you know that the seal will perform satisfactorily at the high end.

Can you describe the tests that give you that confidence and explain to us why we don't have similar confidence at low end?

DR. LITTLES: Yes, these tests are tests that have been conducted since the incident. Of course, the spec on the O-ring is 500 degrees.

DR. WHEELON: We have heard a lot about the specs. Let's talk about the test.

DR. LITTLES: It was specifically oriented to this type of analysis and the possibility of having

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something impinging on that joint, so there were special tests that were conducted, and what they did was, they had a test rig which had a fairly small O-ring in position, seated it with pressure at the right pressure, 900 psi, and then they just heated that up and monitored it with a thermocouple, the temperature in the O-ring area, and heated it up to the point where they got the O-ring to break loose structurally. And there were, I believe, three or four of those tests, and the temperature at which you had the structural failure was something above 1,000 degrees, and so that is the basis of that statement.

CHAIRMAN ROGERS: The last time you were here, the Commission raised the question of whether there should be outside independent people involved in these tests, and I will now ask General Kutyna to make a comment about that. I understand that we have, the Commission has worked out an arrangement to provide for that.

GENERAL KUTYNA: Mr. Chairman, those people are in place now. We took them out to Thiokol on Wednesday. They are there through today, and they will be down at Marshall next week, and so they will be involved in this particular test and this analysis to make sure that it checks out.

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CHAIRMAN ROGERS: How many people are there?

GENERAL KUTYNA: There are six people, sir: representatives from the Rocket Propulsion Lab, from the National Transportation Safety Board, from the Air Force's Space Division, and from industry support to the MX Missile Office.

DR WHEELON: Mr. Chairman, may I continue my question with Dr. Littles just a little further? If it is so easy to determine the high temperature behavior of the seal, why isn't it equally simple to determine the low temperature end? Can you explain that for me and perhaps other people on the Commission?

DR. LITTLES: I don't believe it is complicated to determine the low temperature performance of the seal, and we are doing that in more detail as a part of this investigation. The thing that is a little more complicated relative to the failure scenario we are pursuing is not the performance of the seal itself at low temperature, but the performance of the seal in combination with the dynamics associated with the joint. That makes the testing of that total effect a little more complicated, or at least the building of a test rig to do that a little more complicated.

As I will report to you in the test section, we do have our dynamic test rig functioning properly

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now. We are getting good data out of it. I believe within the next few days, week to ten days, we will have the information required to resolve that particular failure scenario.

DR. WHEELON: Okay, given that this test is easy to perform either at high temperature or low temperature—I accept your judgment there—is it true that we had done these simple tests prior to the launch of 51L, either at high temperature or low temperature, or are these new tests that are being done for the first time?

DR. LITTLES: These particular tests I am referring to are tests and test fixtures which have been designed and fabricated and are being used specifically to evaluate the scenarios or hypotheses for failure. They are all associated with the failure investigation.

DR. WHEELON: So they were not done prior to the launch?

DR. LITTLES: These particular test fixtures were not used prior to launch, no, sir.

DR. WHEELON: Were any tests of this nature done prior to launch?

DR. LITTLES: To my knowledge there were no subscale tests which incorporated dynamics. There were some subscale tests looking at the O-ring capability of

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the seal under the ignition pressure transient. Those tests had been conducted down to something 30 degrees or a little below, I believe—I know it was 30 degrees; it might have been a little lower—as a part of an evaluation of the joint that had gone on, but there had been, to my knowledge, no subscale tests that included joint dynamics.

DR. WHEELON: Had there been any full-scale tests?

DR. LITTLES: Yes, of course, because the motor firings themselves have joint dynamics, yes, and those have been conducted down to a temperature of 40 degrees on one of the—it was either the development motor or qualification motor, I can't remember which, but that motor was at 40 degrees on the joint, calculated 40 degrees. The environment was 36. And there was an analysis done which indicated that the joint was 40.

MR. RUMMEL: However, those full-scale tests were not really representative of the flight article as I understand it, because additional putty was inserted at the field joints prior to those tests. Is that correct?

DR. LITTLES: There were some modifications made to the putty. The concern was, as I understand it, that they wanted to make sure that they didn't have gaps in the putty, a d there were in some localized areas. It has been reported to us by Thiokol that there were some areas in there where they had to go in and do some adjustment to the putty. It wasn't over all joints. And it wasn't over the total area, we are told, on any joint but there were some areas where they made adjustments to it. That is correct.

MR. RUMMEL: Those same kind of additions of putty, just to be clear, have not as a matter of practice or really at no time was that same thing done to flight articles. Is that true?

DR. LITTLES: No, sir, not to my knowledge

MR. RUMMEL: Thank you.

DR. LITTLES: Could I have Chart W-17, please?

(Viewgraph W-17.) [Ref. 3 21-12]

DR. LITTLES: So, we conclude, based on upon what we have just discussed, that it is improbable that damage to the tank initiated a leak or, based on the analysis, that a leak within the detectable limits of the tank instrumentation would have served to initiate the SRM failure.

Chart W-3, please.

(Viewgraph W-3.) [Ref. 3 21-33]

DR. LITTLES: Having concluded, then, that

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there was no leak in the tank, we did get a failure of the tank, of course. We have indicated that at 67 seconds we saw a decrease in the ullage rise rate, and what I would like to address now is what caused that and also, while we are on this chart, with what we have just presented here, we recommend to close Block 2 on this chart.

Could we go now to Chart W-18, please?

(Viewgraph W-18.) [Ref. 3 21-13]

DR. LITTLES: As you have seen from the film and time line earlier this morning, there was a hot gas leak from the motor which was evident from that film at about the 58 to 59 second time frame. We have used the data, the chamber pressure data that we have from flight, and we have established a leakage from the size hole that the change in chamber pressure would indicate.

We have looked at the heating that would result from that type of plume coming out, and impinged that type heating rate on the external tank, and we have concluded that within the time frame between 58 to 59 seconds, when we first see the hot gas leak coming out of the motor, and the time at 66 seconds, when we see the change in ullage pressure in the tank, that there is more than sufficient heat during that period of time to

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burn a hole through the tank. As a matter of fact, with the heating rates that we could get, which could range up to 880 BTU per hour foot per square second, you could burn a hole in the tank in a couple of seconds, and so we conclude then that it is probable that the leak from the SRM initiated the tank failure rather than vice versa.

This concludes the section on the tank, and we recommend, based upon what we have here, that the tank be colored green on the color code indicating that it is improbable that the tank initiated the failure. As I mentioned, this information has been presented to the task force. Martin Marietta agrees with this, and Marshall also agrees with the recommendation that we close it. There is some, as we mentioned a minute ago, some residual work that we are doing with Thiokol. They are reviewing some of our analysis. We are going to provide some additional data to them, so there is some residual work going on there. But on balance we concluded that the tank is not involved in the failure at this point in time.

Could we see W-20, please?

(Viewgraph W-20.) [Ref. 3 21-44]

DR. LITTLES: Relative to the leak check port, Chart W-20 is a subtier failure tree that was used to evaluate the leak jet port, as was discussed this

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morning with the time line in the film. Dan Germany showed you where the cameras were relative to the liftoff. I am not going to go through all of the details that we went through in this investigation because the bottom line is that in reviewing all of the film, looking at all of the cameras we have, there is nothing visible from that location.

Could we go to W-21, please?

(Viewgraph W-21.) [Ref. 3 21-45]

DR. LITTLES: W-21 is the picture I had up earlier, which shows the locations of the camera, and what you can see from that is that looking at cameras E-63 and E-60, you have a very clear view of that leak check port, and in reviewing all of the film, we have never been able to see anything that comes directly from that port.

DR. COVERT: Dr. Littles, it might be helpful if you would indicate if that leak check port is on the top or the bottom of the righthand booster. The picture you have does not indicate where the leak check port is.

DR. LITTLES: If you go to Chart W-23, please. I believe that shows it.

(Viewgraph W-23.) [Ref. 3 21-47]

DR. LITTLES: I am sorry, it was W-22.

(Viewgraph W-22.) [Ref. 3 21-46]

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DR. LITTLES: On the left part of that figure there you see the arrow pointing to the location of the leak check port on the righthand booster, and what this is is an artist's concept of the view that you would have from camera E-63, and where you might see the puff of smoke emanating if it came out of the leak check port, and in looking at all of the film, we don't ever see an area, a clear area on the motor itself between the leak check point and the tank. There is always smoke over there, up against the tank. So we conclude that it doesn't come from the leak check port, but it comes from somewhere around out of view, coming around the corner there.

Could we see Chart W-23, please?

(Viewgraph W-23.) [Ref. 3 21-47]

DR. LITTLES: This is just the conclusion relative to that that the smoke is plainly visible at .678 seconds. The source itself is not visible, and, as Dan indicated this morning, all of the film indicated that it probably originates between 270 and 310 degrees, and we conclude from this that it is improbable that the smoke origin was from the leak check port, and we recommend closing out that failure scenario.

Okay, that concludes the leak check port. We would like now to move into the section on trajectory

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and flight dynamics reconstruction, and Harold Scofield will cover that for us.

DR. SCOFIELD: Mr. Chairman, Dr. Ride, gentlemen, I joined ABMA in 1956, and have been with Marshall Space Flight Center since its inception, and since that time my specialty has become the analysis of flight dynamics. I am currently the chief of the Control Systems Division,

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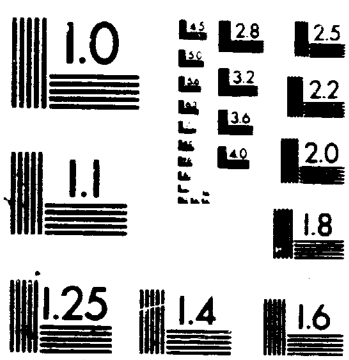

MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS
STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

which applies the analysis of flight dynamics to the synthesis of auto pilots and po ting control systems.

If I could have Chart S-2, please.

(Viewgraph S-2.) [Ref. 3 21-48]

DR. SCOFIELD: This pitch is about reconstructions. Now, reconstruction is a computer simulation, and in that computer simulation we attempt to include all of the environmental effects that we can in order to recreate what happened, in this case on 51L, of course.

So, our task will be to construct math models of the environment, and we hope to get out of these reconstructions three things, truth checks against failure scenarios. That is, if we can detect the effect of some attribute of a failure scenario, and if we can simulate it and can understand it, then that leads to increased confidence that that failure scenario happened

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or that it didn't.

Secondly, the data that is produced from these reconstructions is needed for loads analysis to determine the dynamic loads on the vehicle, again to be reconstructed for the 51L flight, and Bob Ryan t your right will cover that part of the pitch later.

And lastly, of course, the reconstructions enable us to tell whether the vehicle systems operated properly, particularly the guidance and control system in this case. Now, the chart that is on the screen graphically shows you how these analyses fit together.

On the left, the propulsion reconstruction feeds into the trajectory. First, I will talk about propulsion reconstruction. Secondly, I will talk about the winds aloft and the atmospheric reconstruction. That is in the upper left corner of the chart. That also feeds into trajectory. And then on the right part of the chart is a block called plumes and aero. And what that is is a modeling of the failure plumes, the hot gas leak from the SRM using photo data, and the calculation of revised aerodynamics due to that plume. Those aerodynamics have an effect particularly on the control system. They do have some effect on the trajectory also. You will find that those effects are

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measured.

Fourth. we generate a coordinated time line, which we need for the trajectory reconstruction, and with all that data we are able to compare our mathematical simulation on the computer with observed data and feed that into a control system simulation which generates the dynamics or a more detailed explanation and understanding of the dynamics from which we can estimate the loads.

Chart S-4, please.

(Viewgraph S-4.) [Ref. 3/21-49]

DR. SCOFIELD: The main propulsion system performance was reconstructed. That is the Space Shuttle main engine system that I referred to. We determined the propellant loads at main engine start command and the thrust and flow rates during flight. We reconstructed the SRM performance, and that included the internal pressures, the flow rates, and the thrust during flight now. On the screen you have a plot of SRM right chamber pressure, internal pressure, versus time. The heavy line is the measured data from 51L for that right SRM. The dot-dash line that it almost covers is the mean of our flight experience in the first 24 flights, adjusted to 51L conditions. We do that by running tests on each batch

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of propellant. Each batch of propellant is a little different, you see, and we make small motors as well as the big ones that we use from each batch propellant, and fire them to ascertain the differences in performance.

The temperature also has an appreciable effect on this parameter, and that is not the parameter of the temperature of the atmosphere, but, rather, the temperature of the propellant inside the rocket. It varies nowhere near as much as the outside temperature, but nevertheless it is important. Those two main parameters were used to shift what those first 24 flights would have done had they been flown on the day that 51L was launched, and had they been constructed from the same batch of propellant.

DR. WALKER: Are those test firings done at Marshall or at Thiokol?

DR. SCOFIELD: At Thiokol, sir. Now, we have two attributes of this curve that need to be discussed. The first on the left is marked right SRM performance higher than predicted. That is not a large deviation. That is about 1.4 standard deviations above the mean, and you would expect variations on the order of one standard deviation in examining this data. Now, furthermore, there are some mechanisms which are not in this reconstruction which tend to make us believe that

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that pressure would be a little high, for that motor, anyway.

Nevertheless, it is not important in our opinion. The attribute on the lower right, marked SRM leak begins, of course, is very anomalous. You see that it crosses, "it" being the heavy line, crosses the dotted line, which are marked "expected limits", and we would estimate the probability of that behavior at being less than three parts in 1,000 if we only had to worry about random things, and so we conclude that that is not random. We would have concluded that anyway. Even if we had not had a failure on this mission, we would have been very much concerned about this phenomenon. Of course, we know from photo coverage that the SRM leak does begin at about that time that we see the divergence of the internal pressure.

Now, we use—our propulsion people use this divergence from the expected to construct the size of the leak that must have been evident in this right SRM. Now, of course, that leak had a zero area at the start, but it was estimated that it grows to about 45 square inches at 72 seconds. With that data, we can construct a thrust profile from that leak, and we can use it in subsequent reconstructions.

Chart S-5, please

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(Viewgraph S-5.) [Ref. 3/21-50]

DR. SCOFIELD: All right. At this point, with no other reconstructions accomplished, we were able to go to work on liftoff dynamics. We simulated liftoff dynamics with a three-dimensional translation and rotational program that included the flexibility of the shuttle and the initial conditions for this liftoff—that is, the conditions with which the vehicle left the pad—were chosen to match the flight records telemetered from the flight and the first one or two seconds, and we obtained a match for that telemetered data using the values that are listed there which were within the nominal range of expected variations.

That is, the space shuttle main engine thrust rise was within the expected variations in that parameter, and likewise the SRM ignition timing differences and the ground winds which were measured that day, of course. This gives us confidence that we understand this part of flight, and it enables us to conclude that the control system, for example, worked properly during the liftoff phase of flight.

We also compared the SRM gimballing activity for 51L with STS-6. STS-6 was also the Challenger, and it carried the TDRS satellite also, so it had essentially the same payload, and those plots are very

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similar up until the point when the vehicle clears the launch supports, the haunches, as we call them, the supports the SRBs sit upon.

After that, they diverge some, and we attribute that to ground winds. That is a minor thing. We also evaluate the liftoff films to make sure no collision occurred with the ground structure, and we were able to determine, of course, that there was no collision with the ground structure. Now Chart S-7, please. [Ref. 3/21-51]

(Viewgraph S-7.) [Ref. 3/21-52]

DR. SCOFIELD: Next, we needed to reconstruct the environmental model for 51L.

DR. WHEELON: Let me make a point, before we leave that matter for the last time. During this initial part of the liftoff sequence the vehicle has vibrational modes. It twangs like a reed. At what frequency does it twang, in its resonant mode, as it lifts off?

DR. SCOFIELD: The first main mode is essentially three hertz.

DR. WHEELON: Three cycles per second.

DR. SCOFIELD: That's right, and it was contained in the simulation, and indeed that has to match in order to obtain the match that I claimed we do. We were able to see that twang from the photo coverage. You can actually see the vehicle bend or rather you can see the

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vehicle move, and you must include the bending of the vehicle in your simulation to account for how it moves, and so that was included.

DR. WHEELON: So the vehicle in its principal mode is twanging three times a second. Do you make any correlation between that and the fact that the smoke was apparently puffing three times per second?

DR. SCOFIELD: Well, that has been discussed earlier today. I can only say that I have seen that data, and it is only a very rough correlation.

DR. WHEELON: It is better that no correlation.

DR. SCOFIELD: No, sir, it is not no correlation. That is too many negatives. The puffs of smoke do appear to be very, very approximately at a three-puff-per-second rate. However, we cannot account for that. We are still working on it. We feel that it could be a coincidence, but of course we are not going to ignore that as evidence, and we are going to pursue that until we are sure.

DR. WHEELON: It could indeed be a coincidence, but it is worth noting in passing.

DR. SCOFIELD: It certainly is.

VICE CHAIRMAN ARMSTRONG: It is at least not inconsistent.

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DR. SCOFIELD: It is not inconsistent with the mechanism that opens that gap according to the vibration of the vehicle, and I think Bob Ryan will talk more about that later, as a matter of fact.

DR. WHEELON: Thank you.

DR. SCOFIELD: Surely. Now, next we reconstructed the atmospheric environment, and that is mainly winds aloft and pressure and temperatures aloft for this type of analysis. We need that data in order to predict where the vehicle goes and what sorts of air pressure are exerted on it. The ground winds were also reconstructed, and the ground temperatures, and the main fault histories also, and that was mainly to supply it to other teams. Our team was in charge of producing these reconstructions for our own use and for others' use.

Now, the plume of the SRM leak was modeled from photo coverage, and you see the chart which contains some line drawings on the right as seen by Camera E-207 at 60.6 seconds. Now, you can actually see the plume from that camera at that time of flight. The aerodynamicists estimated the size of that plume, and they estimated how much choke flow they get in the channel between the SRB and the tank, and they estimated the differences that would be evident in the plume

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downstream of that leak.

And they produced what they called aerodynamic increments or really new aerodynamic data that would account for the forces and moments on the vehicle due to this phenomenon. Now, on the left part of that same chart, if you could go to the left side, please—thank you—we have an aft view. Unfortunately, we don't have a camera that shows this. It would be really great if we did. We have modeled the plume from the SRM leak along a 45-degree line to the vertical. Some people think that is known fairly well, say, within ten degrees. Others don't.

They think that we cannot reduce that photo data that accurately, and so we are doing a sensitivity analysis at this time, assuming a 30-degree error in the location of that plume, and we are going to see if we can tell any difference in the flight dynamic reconstruction that would aid in pinning down where the SRB leaked.

Now, that is the kind of thing that we are after. And of course the leak was a fan-shaped phenomenon. It wasn't a round hole. And so we expect that this could have a small effect. If it does, we won't be able to help pin down where that leak came from. But, on the other hand, if it does make enough

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difference, we shall.

The maximum aero force due to the aerodynamics, the revised aerodynamics of the plume, are about 130,000 pounds, and I think by anybody's rule book that is a lot, and you will see in a few moments it does move the vehicle. We provided all of this data from the atmospheric reconstructions and from the propulsion reconstructions to JSC, and to all others, contractors included, that needed the data.

Chart S-9, please.

(Viewgraph S-9.) [Ref. 3/21-53]

DR. SCOFIELD: Now, next we determined a coordinated time line, and a trajectory using the winds aloft, the propulsion, and the SRM leak with aerodynamic increments. We have already obtained excellent time line agreement with the JSC time line activities, and we have obtained excellent agreement with both Marshall Center photo activity and the JSC/KSC photo activities. And we are talking about ten milliseconds here. We know these times very well. We may have some difference of opinion on the interpretation of the films, but we know when the phenomena occurred, and we know what is on the film.

The reconstructed trajectory was successfully compared with a JSC product called the best estimated

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trajectory. Now, the best estimated trajectory is constructed from radar data, and it is a very accurate thing. Those radars are quite precise, and the other data is used there, too, to determine true record of where the vehicle went, altitude versus time, for example.

Now, our simulation is not constructed from that data. Our simulation is constructed strictly from the environmental models. And so we compare these two. Now, before you here we have a chart of what we call the dynamic pressure versus time. The dynamic pressure is air pressure.

It is the same force you feel on your face when you ride a bike, and it is a very important parameter in predicting air loads, and so we tried to very carefully reconstruct this parameter. The solid line is the simulation and the dot-dash—no, I am backwards. The solid line is the best estimated trajectory from the radars, and the dot-dash is reconstructed, and you can see that they are virtually identical.

This transient at the top is due to winds aloft, and that is not abnormal. You see excursions in this parameter when the vehicle encounters winds. Chart S-10, please.

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(Viewgraph S-10) [Ref. 3/21-51]

DR. SCOFIELD: Next, we attempted a reconstruction of the flight dynamics between 50 and 72 seconds. We were interested particularly in these times of flight for obvious reasons. That is where the SRM leaked, and we have quite interesting dynamics in that region of flight. The match was a successful match, and the data acts as a check on the SRM leak model. The data was used or is being used in loads analysis.

Now, I might remark that Rockwell also is doing the same sort of activity, and we provide a check against each other. Rockwell is a systems contractor for JSC as well as building the orbiter, and they are doing these same studies, and so they have made a reconstruction similar to this one, and we have compared notes, oh, every two or three days here.

CHAIRMAN ROGERS: What do you hope to conclude by these reconstructions?

DR. SCOFIELD: Well, we hope to provide data that shows that the loads were within limit loads after you do the loads analysis. We hope to conclude that the SRM leak was in the position that I showed, and we hope to conclude that the guidance control system of 51L operated properly. We have not documented all of those things, Mr. Chairman. Some of them we are quite sure

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of, but we haven't studied the test.

CHAIRMAN ROGERS: When do you think you will complete that?

DR. SCOFIELD: The first week of next month.

CHAIRMAN ROGERS: Okay. Thank you.

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DR. SCOFIELD: All of the parameters of vehicle motion were within predicted envelopes, every one of them. Now, the predicted envelopes were generated early in the program. I don't mean a predicted envelope for 51-L here. I am speaking of the dispersions analysis that one goes through to provide data to thermal analysts and to provide data to loads analysts and that sort of thing, and those are large variations. You take very conservative assumptions about what you know regarding the environment in the vehicle, and those variations are, oh, 30 percent above what we saw in this flight.

Now, all of the parameters that were telemetered and reconstructed from 51-L were within flight experience up to 65 seconds. At 65 seconds we did begin to pick up some effects of the failure phenomena that drove us outside of our tube of accumulated flight experience. The peak gimbal angles, SRM gimbal angles, were within flight experience, but the total travel of those SRM gimbals was outside our flight experience.

Now, what I mean by that is if you take each increment of angle, each telemetered number, take the absolute value and add them all up, it gives you a measure of how far that rocket had to gimbal that angle

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during the flight, and that parameter for this flight was 132 degrees. The largest previous was 125.

Let's have Chart S-11, and we will begin to show why that was.

(Viewgraph S-11) [Ref. 3/21-55]

DR. SCOFIELD: This is a plot of pitch attitude error versus time. Pitch is the up and down motion in the plane of flight, and the attitude error is the angle between where the vehicle is and where the guidance system wants it to point, the command angle. And once again you have 51-L measured data, dot dash, and the simulation in the solid lines.

Now, note the region between 40 seconds and 60 seconds. We have quite a transient response there. All of those variations are due to winds aloft, and we can reconstruct as we did from 50 seconds-on a simulation of this. We think that is very close to that. That is a very good reconstruction. We have not only this angle within 1 degree or so, but the slopes of the angle matches up very well with the telemetered data.

At 66 seconds or so we have a response to a planned roll maneuver that comes out of the guidance system. That is normal, but the long feature in that transient response leading to the lower right corner is

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the response to the SRM leak. If the SRM had not been leaking, as hypothesized in all of these reconstructions, that curve would have turned back up and gone towards zero before 70 seconds.

So we know very well—it's a dramatic difference. We know very well that the SRM was leaking at that time. We know fairly well where it was. We have a fairly good measure of what the plume was. We have a pretty good measure of what the flow was. It all adds up. That is the bottom line.

Okay, Chart S-12, please.

(Viewgraph S-12) [Ref. 3/21-56]

DR. SCOFIELD: This plot is actuator extension on the left SRB versus time. Actuator extension is in inches, but actuator extension in inches is approximately the same as gimbal angle in degrees. The point I want to make from this chart is the response to wind gusts between, oh, 35 seconds and 60 seconds. There is a lot of activity there, and even though this parameter was within our flight experience, once again, the dynamic activity was not within our flight experience. This has been referred to by one of the guys on our team as a busy wind. It is a lot of motion.

Now, if we are incorrect about how much that joint opens up due to the loads, then this data would

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become very important. We don't think that this type behavior would open a gap in a healthy SRM seal. It is certainly not designed that way.

MR. ACHESON: How close to the limits of your flight experience were the winds that were experienced in this frame of time?

DR. SCOFIELD: They were very close, sir, up around 60-something seconds. That peak up at 62 seconds is barely within our flight tube. The one at 41 seconds is well within our flight tube. We usually get more activity down in mid-flight, 40 to 60 seconds, than we do at 60 to 70 seconds. That is a healthy maneuver for that time of flight.

Now, I might remark that if we had a degraded structure at that time, these data could even be more important. We don't know the answer to that question. We don't really have a good way of determining the effect of this type of behavior on a degraded SRM structure.

GENERAL KUTYNA: But, Mr. Scofield, what you are saying is we saw the flame appear at about 58 seconds, and if I can use an analogy, this chart shows the Shuttle going on a fairly bumpy road prior to that time with loads on that SRB. If you had a weakened joint that had sealed itself on lift-off, this could be

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the factor that opened that joint up again, is that right?

DR. SCOFIELD: We are very much interested in that theory. We are working on it.

CHAIRMAN ROGERS: Can you give us a little more certain answer on that?

DR. SCOFIELD: Not at this time, no, sir. What we are waiting on is the next presentation, and the next presentation will show you some pretty sporty analysis that needs to be done to try to evaluate the degraded structures, or even a normal structure, for that matter.

If I could have Chart S-13, please.

(Viewgraph S-13) [Ref. 3/21-57]

DR. SCOFIELD: This is a summary of findings. The propulsion performance was within the predicted limits until 69 seconds. At that time the right SRM broke outside our limit band. The SRM hole opened at approximately 60 seconds and grew to 45 square inches by 72 seconds. The measured winds aloft, propulsion reconstruction and aero increments based upon observed SRM plume give us a trajectory reconstruction that matched the best estimated trajectory, and a dynamic simulation that matched the flight data very well. We conclude that the control system worked properly until 72 seconds. It did

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not after 72 seconds. When we got into structural breakup at 72 seconds, it was a bit much to ask of that system. It wasn't designed for that, of course. The parameters were within flight limits from the preflight simulation and were within the flight experience through 65 seconds. However, the SRM gimbal angle duty cycle, as we call it, was large due to winds aloft.

Now, that concludes my prepared presentation.

VICE CHAIRMAN ARMSTRONG: In determining the flight derivatives for the region of interest here with the plume, the CM-alpha/CM-beta, did you do those, determine those new values analytically, or did you use tunnel models?

DR. SCOFIELD: We did those analytically. Rockwell also did, and we compared notes, and we have a wind tunnel program planned. However, that probably will be several weeks off yet. Those things, as you undoubtedly know, take a little time to do. We have to make a model which includes—we have to make a wind tunnel model which includes some sort of representation of that plume, of course.

VICE CHAIRMAN ARMSTRONG: But with the analytical ones you can do a bit more sensitivity analysis with the geometry, is that correct?

DR. SCOFIELD: Of course, and that is going on today.

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DR. LITTLES: Mr. Chairman the next section will be presented by Byb Ryan, and there are two subjects. One is the data that we have used to conclude that the loads were well within the design limit, and the second section is the work that we are doing relative to the joint dynamics and applying the specific 51-L loads to that joint.

MR. RYAN: Mr. Chairman, Commission members, I have been in aerospace engineering for 30 years. I started out with ABMA in January 1956, and went with NASA at the organization of the Marshall Space Flight Center, and been Chief of the Structural Dynamics Division since 1974.

What I would like to do today is give you a status report because I don't have a full report of all of the structural analysis, it is not complete, of the 51-L mission, including the aft field joint in particular, since it is of primary interest. And so I will split my presentation into those two parts. The first part I want to discuss is the overall load.

So if I could have Chart R-1.

(Viewgraph R-1) [Ref. 3/21-58]

MR. RYAN: The objectives of this analysis that we are doing is to determine the loads experienced by 51-L for all flight events, compare those loads to

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the expected design loads and determine if excessive or unusual loads occurred. And then, thirdly, we want to extract from those loads forcing functions for special dynamic analysis. In particular, we want to run response analyses of the SRM, solid rocket motor field joints to see how the gap would open for the different events.

Could I have Chart R-2, please?

(Viewgraph R-2) [Ref. 3/21-59]

MR. RYAN: Those events that we will be reconstructing the flight loads for are on the pad, the liftoff, the in-flight—the in-flight covers all of the dynamic regions that have been discussed by Dr. Scofield as the major events of the gust, the roll maneuver and so forth. To do this, a dynamic model of the 51-L vehicle, its payload, the MLP pad for the lift-off phase, etc., has been generated, and the vehicle then is flown in a simulation through all of these different flight events and different environments that have been created that Dr. Scofield talked about. Also, we made use of telemetry data and certain measured data that were available on the pad for our work.

This work is a joint effort that is carried out by Marshall Space Flight Center, Johnson Space Flight Center and Rockwell International at Downey.

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The next chart, which is R-3—

(Viewgraph R-3) [Ref. 3/21-60]

MR. RYAN:—I have chosen to illustrate the dynamic situation that really occurs during the liftoff phase of the maneuver. This is the most dynamic situation that we have. It is where you saw the puff of smoke. It is where you see all of the structure wringing out. A lot of things are going on at that time, and I would like to take just a minute to step through those basic events for you so that you might get a feel for what takes place.

I have chosen only two parameters to plot out of many parameters that we could plot versus time. The time base I am using here is when we give the commands to start the flow in the Shuttle main engines, all the way through to approximately four seconds after lift-off. Now, the two parameters I have chosen to plot for you are at the aft field joint of the right solid booster, since that one is of concern to everybody. As I say, we could plot all kinds of parameters at any other location on it.

Now, if you look at the left hand side during the first two seconds, there is really nothing going on in terms of loads and so forth. The vehicle is sitting on the pad. The weight of the vehicle you see there

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indicated by the axial load, that trace, and that is a compressive load in the aft field joint which is like the weight of it sitting on it.

The other parameter I have chosen to plot is the bending moment in the Y direction, that is, in the pitch direction, which would be the bending moment that would bend the vehicle this direction if you looked at the model there.

Now, at approximately 2 seconds after we start all the process, the SSMEs start coming up in thrust, and as they come up in thrust, you can see a slight decrease in the axial load. In fact, the thrust of the SSMEs are lifting the vehicle up, particularly the orbiter, reducing some of the weight that is in the SRM joint, and the vehicle is bending over, and it is bending over away from the orbiter. Of course, the orbiter and everything is bending with it, and you can see that in the films. In fact, that deflection is quite dramatic, and if you look down at about 5-1/4 seconds, you see the peak of that bending moment. At that time the SRB, the solid rocket booster motor, has bent approximately 24, 25 inches, and the tank is bent like 32 inches.

Now, we give the signal to light the solid rocket booster motor and to release the hold-down bolts

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that hold the vehicle on the pad such that we have a minimum amount of energy stored in the vehicle, and that occurs there, as you can see, a little past six seconds on the chart is that minimum moment part. You see literally the vehicle is being bent over, is being pushed, which rolls the SRBs, and we are storing a tremendous amount of energy in that structure. You are winding it up like a rubber band, so to speak. And then when the SRB thrust comes up and we get a thrust-to-weight of greater than one, the vehicle lifts off the pad, and it releases from the pad, and all of that energy is released into a dynamic motion.

Now, you can see that occurring from about 6.25, 6.35 seconds on in the MY, and you see that frequency that everybody has been talking about wringing out. That is actually bending moment wring-out in the structure at the aft field joint.

Now, later I will talk about how that corresponds to an effective gap opening, but the structure is wringing out in some very complex structurally dynamic modes at this time, this being mainly a 2.7 to 3 hertz mode, but there are two or three modes in this region that the vehicle is wringing out. So those are the basic events that are going on.

DR. COVERT: Mr. Ryan, could I ask a

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question?

If we had plotted the axial load curve earlier in time, you would have it for, not for the whole assembly but for each solid rocket booster itself, would it be possible to see the dynamic effect on the joint and the pins as the load transfers from being down to being up?

MR. RYAN: Yes, sir, that occurs, as you see that, around 6--

DR. COVERT: No, I am talking not about the whole assembly coming off of the booster, but, rather, as I pressurize the rocket case.

MR. RYAN: Yes, sir, it would do the same thing. It goes in tension just due to pressure.

DR. COVERT: Are you going to talk about that today?

MR. RYAN: Yes, sir, I will talk about that later in the second part.

DR. COVERT: Okay.

MR. RYAN: So those are the dynamic events that go on that affect the joint and affect the loads.

Now, to evaluate that we did three things. We had the film coverage that you have seen some of many times, I am sure, and we observed those films, and compared it with several flights. In particular, we

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compared 51-L with STS-6, which was a comparable flight, and the deflections on the SRB on 51-L was 24 inches, and on STS-6 it was 28 inches. These are within the envelopes of what we have been seeing and indicates that everything was okay, that it was a nominal flight at liftoff. The frequency of that oscillation that occurs is about a quarter of a hertz for both vehicles, in fact, for all of our vehicles, which indicates that the pad effects and so forth were the same. That's the important conclusion there. [Ref. 3/21-61]

We have two other modes that go on there. When the Shuttle main engines come up, due to the fact that the nozzle is not full and the thrust wallowing around in the nozzle, those loads appear to be nominal. One is a 25 hertz load and the other is a 30 hertz load. Dr. Scofield talked about the clearance, and that was nominal.

Now, R-5, please.

(Viewgraph R-5) [Ref. 3/21-62]

MR. RYAN: Now, we also evaluated the loads during the twang motion we had. On each hold-down post of the SRBs, we had strain gauges, and we have those on the flights to tell us how much loads go into those SRB hold-down bolts or the posts. Those are designed by that twang motion that I showed you earlier when that MY

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moment peaks. We evaluated those, and you can see on the chart that I have here, if you look down at the base of that chart where the arrow points to the base of the solid rocket booster motor, you can see that the design bending moment was 347 million inch pounds. We actually experienced by measured data on this flight 291 million inch pounds, which is about typical of what we see. If I transfer that same measured data up to the aft field joint of the right solid rocket booster motor, the design limit load there would have been 248 x 10^6 million inch pounds. We experienced 208 by transferring that measured data to that point. And so, in addition, we could observe some of the posts. We couldn't observe them all. But we could also observe from the film, we could observe from the strain gauge standpoint each one of those four hold-down posts on each SRB released nearly simultaneously, which tells us that there was not undue energy being transferred and undue motion being transferred into the vehicle at lift-off.

VICE CHAIRMAN ARMSTRONG: How did you determine that near-simultaneous? [Ref. 3/21-63]

MR. RYAN: By the timing, sir when the strain gauges show that the load has been released on that hold-down bolt and they were within a couple of milliseconds of each other, which says that there was very little abnormal energy or

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unsymmetrical energy or unexpected energy in that part of the liftoff.

Could I go to Chart R-7, please?

(Viewgraph R-7) [Ref. 3/21-64]

MR. RYAN: The third way that we have evaluated the loads by the different parties that I mentioned earlier is a reconstruction of the liftoff loads used in the dynamic model. I want to use this chart, R-7, to show you where I am going to talk about some loads later. If you notice the Ps that are at the back end of the graph, you have the struts. We called the loads for each strut a P-1, P-2, P-3 and so forth, and if you will notice there that P-1 through P-7 are the ET to orbiter struts and are indicative of the loads that the orbiter relative to the ET are experiencing during the liftoff. And then if you notice the P-8 through P-13 are the loads, the struts that attach the two solid rocket booster motors to the external tank and are indicative of the loads experienced there.

Now, on Chart R-8—

GENERAL KUTYNA: Before you go off that chart, is P-11 about where we think we saw the flame?

MR. RYAN: It is very close, yes, sir.

(Viewgraph R-8) [Ref. 3/21-65]

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MR. RYAN: All right. On Chart R-8, on the first seven flights of the Shuttle, development flights, we had measured data on some of the struts at the strain gauges, and we measured that data, and I am comparing the mean of that data and the standard deviation of that data with the predicted reconstructed loads for 51-L, and then on the far right column I have the design loads, the limit loads.

Now, these do not have the safety factors in them. All loads are multiplied by a factor to put margin in them above this. So that is not in this limit load. And so you have got the safety factor above that. So you can see that the 51-L reconstructed loads of the struts are well within the design loads, and are comparable to the loads that we have experienced in the first seven flights.

GENERAL KUTYNA: Mr. Ryan, you know what I am going to ask you, but if you look at P-11, that strut I just asked about, if you look at STS-1 through 7, we have got a number of about 70, and on 51-L it is 141.

Would you explain why that is not a concern?

MR. RYAN: Yes, sir, I would be glad to.

On STS-1 through STS-5 and on STS-7, we flew a heavy external tank. That tank had a marginal condition in the bulkhead region, and because of that we put the

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margin back into it by preloading the struts. In other words, we put the struts in compression so that when you loaded the tank with its cryogenic propellant and it shrunk, then it would make up that difference and take that load out.

GENERAL KUTYNA: As you look at that load, however, this is in the area of that joint where possibly it failed.

Would there have been any structural deformations as a result of that load that might have been a factor?

MR. RYAN: It should not have, sir, with a 306 versus a 141 SR design and then the 1.4 times that. So that would be a big margin, really.

GENERAL KUTYNA: Thank you.

DR. RIDE: If you compared the 141 with others that we have seen on P-11 using the lighter weight tank on Flights STS-8 and subsequent, is that in the general ball park of experience?

MR. RYAN: Yes, it is. We have not had measured data, though, Dr. Ride, on these other flights, and so we have to do it just strictly by analysis.

Okay, let me go to Chart R-9, please.

(Viewgraph R-9) [Ref. 3 21-66]

MR. RYAN: Taking the same loads and the same

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reconstruction of the liftoff motion to the SRB or the solid rocket motor field joints, I have put on that chart the design loads in terms of an equivalent load, just to keep it fairly simple. All of the joints are designed to the same load. The forward joint determines that design load, and that is the —17.2 x 10^6 pounds.

Now, as you come down the stack, because of bending and because of pressure drops and hoop strains and so forth, that load drops for the aft field joint in 51-L—these are the reconstructed values—the forward field joint would have been —15 x 10^6. If you drop down to the aft field joint, the one with the leak, you see a—4.1 x 10^6. So you could conclude, and some people have concluded, without damage to that joint, then, another joint should have leaked first because it should have had a higher load and a slightly higher opening. There is not a large difference, but there is a difference between the loads in those joints, and the forward joint does have the highest load.

All joints are designed alike. They are equal and interchangeable.

Now, Chart R-10, please.

(Viewgraph R-10) [Ref. 3 21-67]

MR. RYAN: Dr. Scofield talked about the reconstruction of the trajectory and the loads during

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the in-flight regions, the roll maneuver through the maximum dynamic pressure region. That reconstruction has not been completed, and we do not have all of the loads for that reconstruction. However, to get a feel for what those loads were and what we were dealing with there relative to what was seen, we took a simulation that was made at a couple of times, the L minus three and a half hour wind and the L minus zero wind, plus estimations of the loads that occurred due to the gimbal angle excursions that he showed you, and predicted what—and we think they are very conservative loads—predicted what we will get out this coming week out of the total loads reconstruction. And you can see compared to the design loads that in the struts the loads are a good bit lower than the design limit loads.

I have put one load indicator there for the tank. You can see the loads going into the tank there where the struts go into the tank are substantially lower than the design loads. And you can see that even in the SRM right field joint, that that load is substantially lower than the expected design load.

So on Chart R-11 then we conclude that the 51-L systems loads, the overall loads of the vehicle were lower than the design limit loads and were essentially within our flight experience. However, the [Ref. 3 21-68]

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effects of those loads on a degraded solid rocket booster aft field joint is in the process of being assessed.

And the next presentation that I am going into, if there are no questions on this, I will talk about where we are at in that analysis.

DR. COVERT: I have a question or two, Mr. Ryan. As I understand what you have been talking about, you are dealing with static strength of the structure and static loads of the structure, is that correct?

MR. RYAN: I am giving you the equivalent static design loads, yes, sir, derived from dynamic and static loads.

DR. COVERT: Is metal fatigue a critical factor in any of the structural design of this solid rocket booster case?

MR. RYAN: Well, I don't have the total answer on that, Dr. Covert. We will be glad to give you a review of that when you come to the task team. It is designed for 20 reuses. It is basically a pressure vessel, and that is the essential design consideration.

DR. COVERT: If a crack of suitable length existed, is it possible that a case failure could have existed while the case was, to use General Kutyna's words, driving over that bumpy road?

DR. LITTLES: We have added, Dr. Covert, one

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item that I mentioned earlier on the fault tree associated with case rupture, and we intend to present the data on that next week to the task force. But relative to your specific question, I believe the program is set up such that if we had a flaw which would have been driven to a leak-through type situation with the loads we had on this flight, since they are within design limits, then we should have screened that out in the proof test that we run on the case. But we are accumulating all of those data, and we will specifically address your question relative to the fatigue in closing out that action item.

DR. COVERT: I just want to mention in passing that, if I recall, in October of 1983, a crack of sufficient size did slip through the inspection, and it was fortunately caught by the proof testing, so that I guess my point is that it is a little premature, I think, to paint this thing too green.

DR. LITTLES: Well, we are certainly accumulating that data, and we will pursue that.

DR. COVERT: Thank you.

MR. RYAN: Are there any other questions on the first part?

DR. WHEELON: Perhaps just to clear my own mind, if I may, to sum up what I think I heard you say,

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the forces and the bending moments, or the torsion on the SRB was about normal. There was nothing unusual in that save the temperature, which is outside your expertise, but there is more work to be done to translate these forces and moments into the tang-clevis area, is that right?

MR. RYAN: Yes, sir, that is what I am talking about next.

DR. WHEELON: Thank you.

CHAIRMAN ROGERS: You may proceed.

MR. RYAN: Could I go to Chart R-13, please?

(Viewgraph R-13) [Ref. 3/21-69]

MR. RYAN: The objective of this analysis that we are conducting both at Marshall and at Thiokol is to reconstruct the 51-L SRM field joint response, in other words, the gap openings that some of the questions that are being raised here, for all of the events using the mated data, the natural environments, the induced environments, and the reconstructed 51-L loads.

If you will look at Chart R-14—

(Viewgraph R-14) [Ref. 3/21-70]

MR. RYAN:—we feel that we have to start with the mating of the SRM segments. Where you actually start with the first segment, mate the second one to it, take all the dimensions and so forth there, and the

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loads, etc., that they would experience and move through the stacking, the transportation, the on-pad, and then the other conditions that I have talked in the previous section, which was the lift off dynamics, the roll maneuver, and the maximum dynamic pressure or the in-flight regime which really covers all of the areas that Dr. Scofield talked about from approximately 35 seconds on.

Chart 15 then—

(Viewgraph R-15) [Ref. 3/21-71]

MR. RYAN:—is the approach that we are taking to do that analysis. We are developing finite element static and dynamic models of the joint, the field joint, and the structure around it, carrying enough of the structure out through the segments to adequately describe what happens there. We are conducting tests at Thiokol now on joint rotation, and we are tuning these models such that they match or duplicate the tests that are being run at Thiokol on joint rotation.

We are adding to these models, then, the SRM segments, the propellant effects, and the structure, like the ET attach ring and so forth. Then we are characterizing the initial joint condition. In other words, every joint that is mated is slightly different

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because of the ovality and so forth between the tang and clevis, and we are in the process of characterizing those conditions. And then they will be used in conjunction with this model and with this dynamic analysis to say then what was the joint opening on each of the joints, in particular, the aft field joint of the right solid rocket booster.

Then we will determine then the static and dynamic response of the gap at the primary and secondary seal.

Now, Chart R-16 is a chart that kind of summarizes for you some of the different types of models that we have.

(Viewgraph R-16) [Ref. 3/21-72]

MR. RYAN: If you look at the top right hand corner you see the finite element model of the clevis and tang. You see just under it the deformed model. In other words, if I load that with internal pressure or if I load it with a line load or a punch load, you see that that clevis deflects in particular, and it deflects away from the tang, creating a gap between the seals and the joint.

Now, down below that is the aft attach ring. That is our model of the aft attach ring that the struts are attached to, and it is attached to the overall SRM

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segment. Now, the picture on the left hand side of the chart shows all of that put together with a one-diameter length of our model of the segment fore and aft of that aft field joint.

Okay, now, taking that model and taking all of the forces that were recreated or reconstructed in the liftoff loads analysis that I talked in the first session and Dr. Scofield talked in the liftoff sequence, took all of those and got out of that extracted out of that the forcing functions, including the internal pressure rise of the SRM, and all of the loads that would go into that section of the solid, the inertial loads, the point loads going from the struts, etc., we reconstructed that and drove that model. And on Chart 17—

(Viewgraph R-17) [Ref. 3/21-73]

MR. RYAN:—I show you a maximum and minimum response during the short period of time when the SRM pressure is coming up, and all that dynamic load that I showed you earlier is going on.

Now, the main effect that you see here is the effect of the pressure bulging and stretching the SRM, and you see that gap opening that is given in mils there. You see that gap opening, and you see it occurring very fast due to that pressure. As that

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pressure builds up in the solid, the thrust comes up, it opens, and it opens very fast, and you can see there that it is over only about 600 milliseconds. Then you see in addition, and it is not as clean as it looked on that MY a while ago, but you see in addition the three-hertz oscillation in that gap opening, although it is a very small opening, plus or minus a mil and a half or so, you do see that opening taking place, opening and closing of that gap. It is not really opening and closing. It is opening. It is just a delta opening and closing relative to the open position.

GENERAL KUTYNA: Now, Mr. Ryan, this is a pretty important chart. What it says is that gap opens as much as .025 to .030 as soon as you light off that SRB.

MR. RYAN: As soon as I reach maximum pressure, yes.

GENERAL KUTYNA: And what you are telling me is if we had a metal-to-metal contact in the area of that seal, we could open that gap as much as .025 to .030, and the seal would have to follow that opening very quickly, within a half a second, to remain in contact with the other piece of metal.

MR. RYAN: Yes, sir.

GENERAL KUTYNA: And the second thing you are

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telling me is that 3-hertz opening or vibration of that gap, possibly, or at least does not dispute the data that says the smoke was puffing at about three cycles per second.

MR. RYAN: That's right, it doesn't dispute it. It doesn't fit it necessarily, but doesn't dispute it. That's a very small opening and closing, but it does happen.

DR. COVERT: Mr. Ryan, one other minor point here. My memory is not very good, so please refresh it. This 25-mil change corresponds to what change in the percent of squeezing on the O-ring between the static condition and in the as-deflected?

DR. LITTLES: I believe about a .040 is a squeeze of somewhere around 12 percent, I believe. I think that is correct. So we are reducing it by whatever fraction that is.

DR. COVERT: And the 25 would take it down around 7 percent?

DR. LITTLES: Yes.

DR. COVERT: Thank you.

DR. WHEELON: Could I just editorialize on this data for a moment and perhaps say the obvious a second time?

We have this rapid expansion of .025 probably

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in this clevis tang area, and we are counting on the seal to follow it, which is to say, expand and take up that difference, and it is true that cold seals expand more slowly or have more difficulty in doing that adapting than warm seals do.

MR. RYAN: There are two things that cause a seal to seal, though. One of them is the fact that the aerodynamic pressure around that gap is more on the inside than it is on the outside, which drives the seal out also. So you are not depending just on resiliency to get the seal out into the gap to seal. So you have to put the two effects together, sir.

The next chart, let me go to R-19.

(Viewgraph R-19) [Ref. 3/21-74]

MR. RYAN: R-19 is just a static analysis where we can put a little more detail in than we can in a dynamic analysis, but we can't show the dynamics, of the same pressure rise buildup rate, and it shows essentially the same thing that I showed previously, but without the dynamics.

Now, we are continuing this analysis. We have not run it for the Max Q region, the roll maneuver and these kind of things that you saw, and that is in process. When we get the reconstruction of the loads, we will be doing the same thing. We are also

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continually trying to refine this model and to try to predict more accurately just how these gaps and seals behave.

DR. COVERT: Mr. Ryan, I think this is part of the answer to the question that I asked you earlier about the rise time in the longitude. This looks—I realize the motions are small, but it looks like a fairly rapid change in position.

Is there an impact load as this thing seats at the other side of that motion?

Now, remember, these pins are—you can sort of put them in with your thumbs, so there is a certain amount of slack in the system, and I am sure you have been on a Pullman in the middle of the night when they have stopped and started suddenly.

MR. RYAN: Yes, sir, this is a nonlinear problem, obviously, and you are talking about a very difficult analysis. The results I presented to you today was a linear analysis that didn't take into account those contacts. That is very complex and very time consuming for us to step through iteratively to do that. We are looking at that. In the particular data I showed you there they did not contact. But there are conditions where they will contact, and that does give you a bouncing and a twanging motion, and as I say, we

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are looking, and we know we can do some of that type of analysis. But it is very complex.

DR. COVERT: Is there any evidence when you examine the holes in either the clevis or the tang where these big pins go through that there is permanent set of the order of maybe .003 to .005, that sort of thing? Do the holes stop being round and become sort of elongated?

MR. RYAN: I can't answer that, sir. Maybe Dr. Littles can. I haven't been involved in that side of it.

DR. LITTLES: I can't answer that specifically. I am not aware of that, but I would have to check on that.

DR. COVERT: Would you get that for me?

DR. LITTLES: Yes, I would.

DR. COVERT: Thank you.

MR. RYAN: That concludes my discussion, Mr. Chairman.

DR. LITTLES: Mr. Chairman, we need to change speakers at this point.

CHAIRMAN ROGERS: I assume that all the tests you are conducting are for the purpose of assisting the Commission and NASA, too, in deciding the cause of the accident. Are you also going to use that material, or are you thinking about it in connection with redesigning

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the seal?

MR. RYAN: Yes, sir.

DR. LITTLES: We will certainly use anything that come out of those tests or the analysis which indicates any problem with that joint, in the redesign, certainly.

CHAIRMAN ROGERS: But it is primarily to determine the cause of the accident?

DR. LITTLES: At this point in time, yes, sir, these tests and analyses are to determine the cause of the accident.

CHAIRMAN ROGERS: Aren't tests being run to think about redesigning the joint?

DR. LITTLES: We are certainly looking at things that would be necessary to change in that joint to resolve the kind of things that may have gone wrong, and we are looking at that, yes, sir.

MR. RYAN: There are analyses being conducted now, parametrically, to determine some of that at Marshall and Thiokol, too.

CHAIRMAN ROGERS: Do you have separate teams doing that?

DR. LITTLES: Yes, sir. It is different people. It is not the people who are involved in the investigation, although they are aware of what we are

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doing and we are keeping them up to speed, but it is a different group of people.

CHAIRMAN ROGERS: Thank you.

DR. WHEELON: Mr. Chairman, I was going to hold this question until later, but I sense we are about ready to lose the right people from the witness stand, so perhaps I could flash back.

Could I invite your attention, Dr. Littles, to W-10? This was the top view of the Shuttle and the orbiter, and it shows the camera obscuration I think in a very skillful way it localizes the rotation around the SRB, the position of the probable leak that caused the black smoke at take-off, and incidentally precludes the possibility that it was the check port doing the leaking.

(Viewgraph W-10) [Ref. 3/21-40]

DR. WHEELON: Now, this addresses two of the three dimensions, namely, where around the azimuth is the leak likely to take place by camera elimination. Have you thought about the vertical, the other dimension, where along the length of the SRB was the smoke coming from? How consistent is that with the up and down location of the field joint? And incidentally, if there were a leak in the clevis-tang combination, it seems to me the smoke would be shooting

2304

up relative to the stack. And that is in fact what it does. So you are trying to put together the other dimension of the three dimensions of this leak, potential leak location puzzle?

DR. LITTLES: Yes, sir, we have tried to do that, and as you know, the smoke, when you first see it in the film, is not at the location of the leak check port. It is some distance up. And it is apparently coming from the hidden location, and what we have tried to do, we have done an analysis looking at a jet that would emanate from an opening of the dimensions we have in the tang clevis, and using as parameters the pressures that you have in there, and all of those variables, to try to establish at what point in time we might have had to have that leak coming from the joint area to see the smoke at the vertical locations at a given time. And the bottom line of all of that analysis is there are so many variables in it that, depending upon what assumptions you make within the bounds of those that are reasonable, that you can get that time to be anywhere within, where it might first come out, within 50 milliseconds of when you first see it or as far back as 400 or 450 milliseconds. And so that shows you something about what kind of—actually, you can predict the vertical location as well, because those would be

2305

substantially tied together.

There are just so many variables involved in that problem that you just can't tie it down very well. But we have tried to do that.

DR. WHEELON: But simplifying all of those variables in that very complicated problem, simplifying it just a bit, isn't it likely that if the leak were occurring in the shaded area, that is to say, the obscured area, and if the flow was going vertically, because that after all is the way the opening in the tang-clevis combination is, that you would first see on any one of these cameras the puff of smoke above the opening? It would have shot out of sight vertically and then come into view only as it expanded and expanded out?

Would you agree that that is still consistent with the idea the smoke was coming from the O-ring?

DR. LITTLES: Very much so, yes, sir. We feel it does come from the joint, yes, sir.

DR. WHEELON: What was the significance of your remarks about the timing, the delay?

DR. LITTLES: What we were trying to establish was when in the timeframe of the ignition sequence that the leak occurred. It would be of interest to know whether it is close to the maximum pressure time or

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whether it is back down during the early phases of the ignition transient, and that relates to one of the scenarios we are working, which is Scenario 6, which deals with putty holding the pressure off the O-rings, the joint rotating, and then a lack of resiliency and the gas blowing through the putty and by the O-rings. So that is the area of interest there in Scenario 6.

DR. WHEELON: But before, again, you get away, isn't it clear that these optical observations of the black smoke near liftoff and the flashes later on come from about the same angular azimuthal area on the tank and the same vertical or linear dimension, and doesn't the web of evidence seem to be closing in on the location of whatever has happened, or do you want to reserve on that?

DR. LITTLES: I think the location of what is happening, we are narrowing it down very well.

DR. WHEELON: That's my sense, you've really got it.

CHAIRMAN ROGERS: I think there is no objection, we will take a recess for lunch and resume after lunch.

Why don't we say 1:45, and Dr. Littles will not get away.

(Whereupon, at 1:00 o'clock p.m., the

2307

Commission recessed, to reconvene at 1:45 o'clock p.m., this same day.)

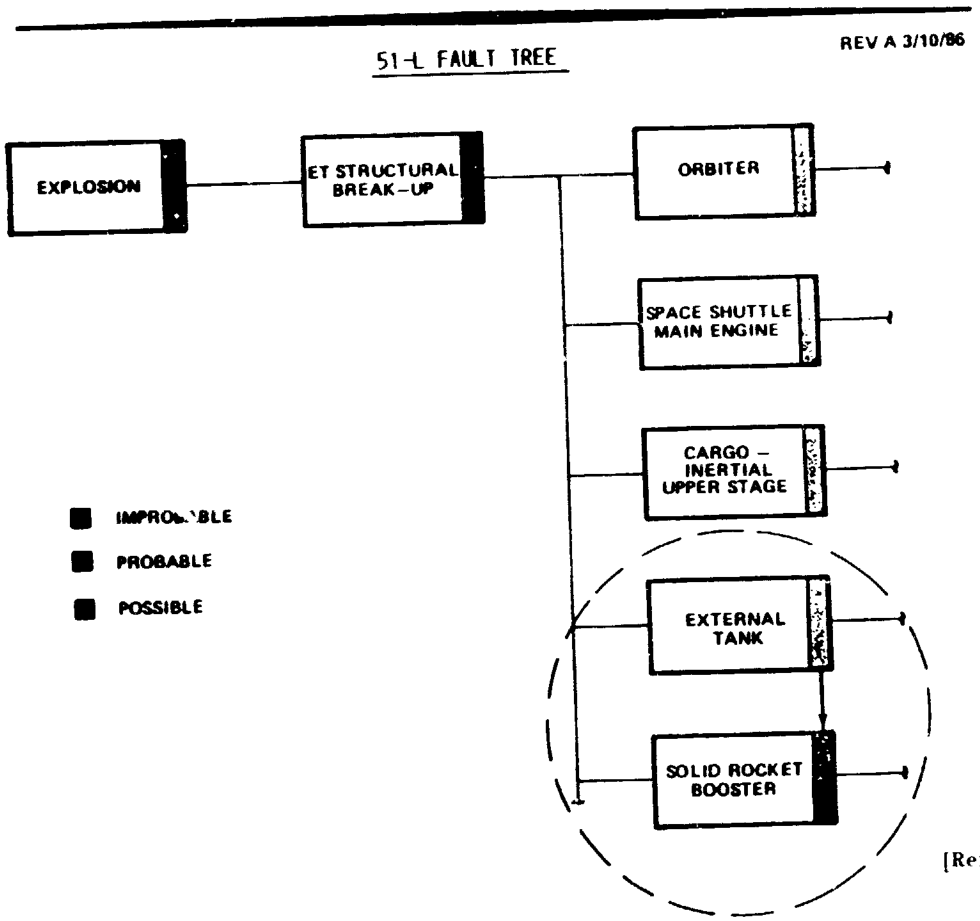
FAULT TREE AND FAILURE SCENARIO UPDATE

W. LITTLES

MARCH 21, 1986

W-0

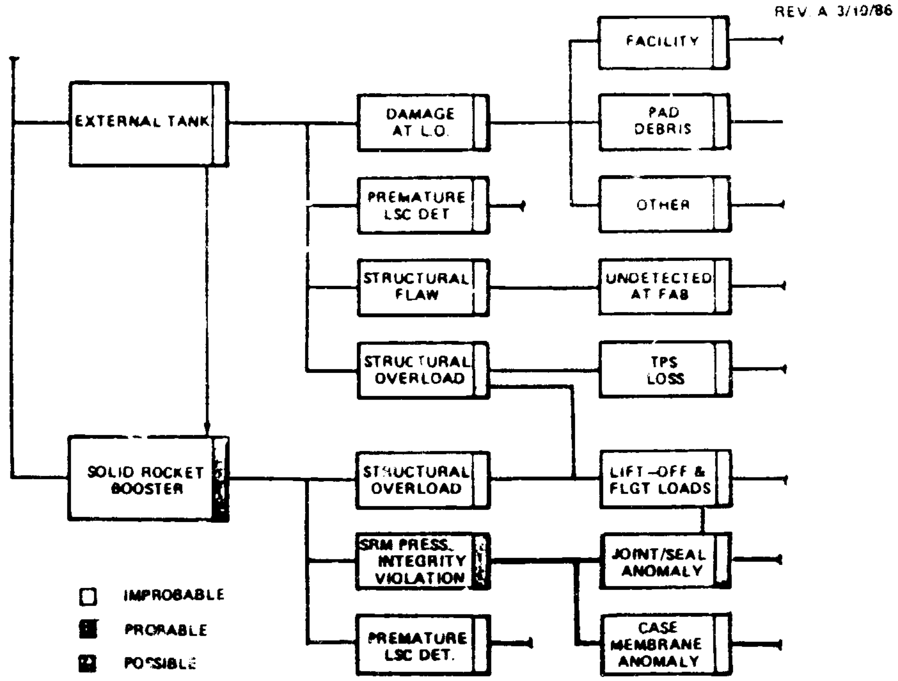
[Ref. 3 21-31 1 of 2]



[Ref. 3 21-31 2 of 2]

H-074

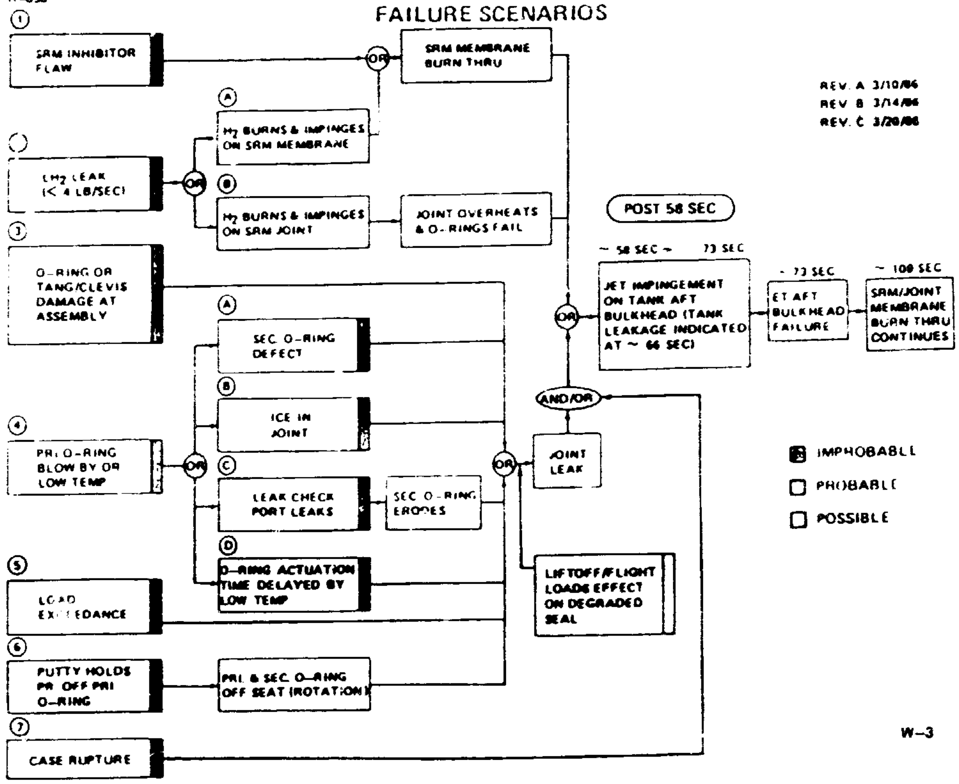
51-L FAULT TREE



W-2 [Ref. 3 21-32]

H-050

SRM HOT GAS LEAK
FAILURE SCENARIOS



W-3 [Ref. 3 21-33]

EXTERNAL TANK

W. LITTLES
MARCH 21, 1986

W-4

[Ref. 3 21-34 1 of 2]

EXTERNAL TANK

INVESTIGATION APPROACH

0 REVIEWED ALL STS 51-L DATA

0 REVIEWED CHALLENGER EXTERNAL TANK HISTORY

- ALL BUILD PAPER

- ALL X-RAYS

0 EVALUATED POTENTIAL ANOMOLIES ON FAULT TREE

[Ref. 3 21-34 2 of 2]

W-5

FAULT TREE EVALUATION

STRUCTURAL FLAW

FAILURE MECHANISM

- O FAILURE OF EXTERNAL TANK DUE TO STRUCTURAL FLAW OF CRITICAL SIZE

ASSESSMENT

- O BUILD PAPER AND X-RAYS RE-REVIEWED--NO INAPPROPRIATE DISPOSITIONS FOUND
- O X-RAY RE-REVIEW DID IDENTIFY A 0.4" INTERNAL FLAW IN A WELD LOCATED ON THE -Z AXIS IN A 2058 RING FRAME WELD
    - o ANALYSIS SHOWS FLAW DOES NOT DEGRADE STRUCTURE BELOW REQUIRED SAFETY FACTORS
    - o RECOVERED HARDWARE INCLUDES THIS FLAW, NO EVIDENCE OF FLAW PROPAGATION
- O NO INDICATION OF LEAKAGE DURING PRE-PRESS AND ASCENT PHASES UNTIL AFTER 66 SECONDS

FINDING

- O IMPROBABLE THAT STRUCTURAL FLAW INITIATED THE FAILURE OF THE EXTERNAL TANK

W-6 [Ref. 3/21-35]

FAULT TREE EVALUATION

STRUCTURAL OVERLOAD

FAILURE MECHANISM

0 STRUCTURAL OVERLOAD INITIATES FAILURE

ASSESSMENT

0 LIFTOFF AND FLIGHT LOADS WERE WITHIN DESIGN LIMITS (80 PERCENT MAXIMUM)

0 ULLAGE PRESSURE WAS MAINTAINED WITHIN LIMITS

0 STS 51-L FLIGHT DATA INDICATES NOMINAL TRAJECTORY PRIOR TO OBSERVED ANOMALY

- NO EXCESSIVE HEATING TO DEGRADE STRUCTURAL CAPABILITY

0 ANALYSIS INDICATES THAT THERMAL PROTECTION SYSTEM LOSS (TPS) WOULD NOT HAVE RESULTED IN OVERHEATING

FINDING

0 IMPROBABLE THAT STRUCTURAL OVERLOAD FROM FLIGHT LOADS, EXCESSIVE ULLAGE PRESSURE OR EXCESSIVE HEATING INITIATED THE FAILURE

W-7 [Ref. 3 21-36]

FAULT TREE EVALUATION

PREMATURE LINEAR SHAPED CHARGE (LSC) DETONATION

FAILURE MECHANISM

- O PREMATURE DETONATION OF LSC INITIATES TANK FAILURE

ASSESSMENT

- O NOT CONSISTENT WITH INITIAL "SMOKE" AT 0.68 SEC. OR OTHER EVENTS FROM FILM AND FLIGHT DATA
- O PORTIONS OF LO_2 AND LH_2 TANK LSC WERE RECOVERED INTACT

FINDING

- O PREMATURE DETONATION OF LSC NOT INVOLVED IN FAILURE

[Ref. 3/21-37 1 of 2]

FAULT TREE EVALUATION

DAMAGE TO EXTERNAL TANK AT LIFTOFF

FAILURE MECHANISM

- O DAMAGE TO TANK CAUSES H_2 LEAK

ASSESSMENT

- O ICE TEAM SAW NO EVIDENCE OF LEAK AT FINAL VISIT TO PAD PRIOR TO LAUNCH (T-20 MINUTES)
- O POTENTIAL FOR DEBRIS DAMAGE
  - NO EVIDENCE OF DEBRIS DAMAGE FROM FILM
  - SPRINGS MISSING FROM HOLDDOWN POST COVERS
    - O KSC ANALYSIS INDICATES THAT SPRINGS COULD NOT BE RELEASED PRIOR TO 0.8 SECOND DUE TO PHYSICAL CONSTRAINTS
    - O NOT CONSISTENT WITH "SMOKE" AT 0.68 SECOND

[Ref. 3/21-37 2 of 2]

[Rev. 3-21-38]

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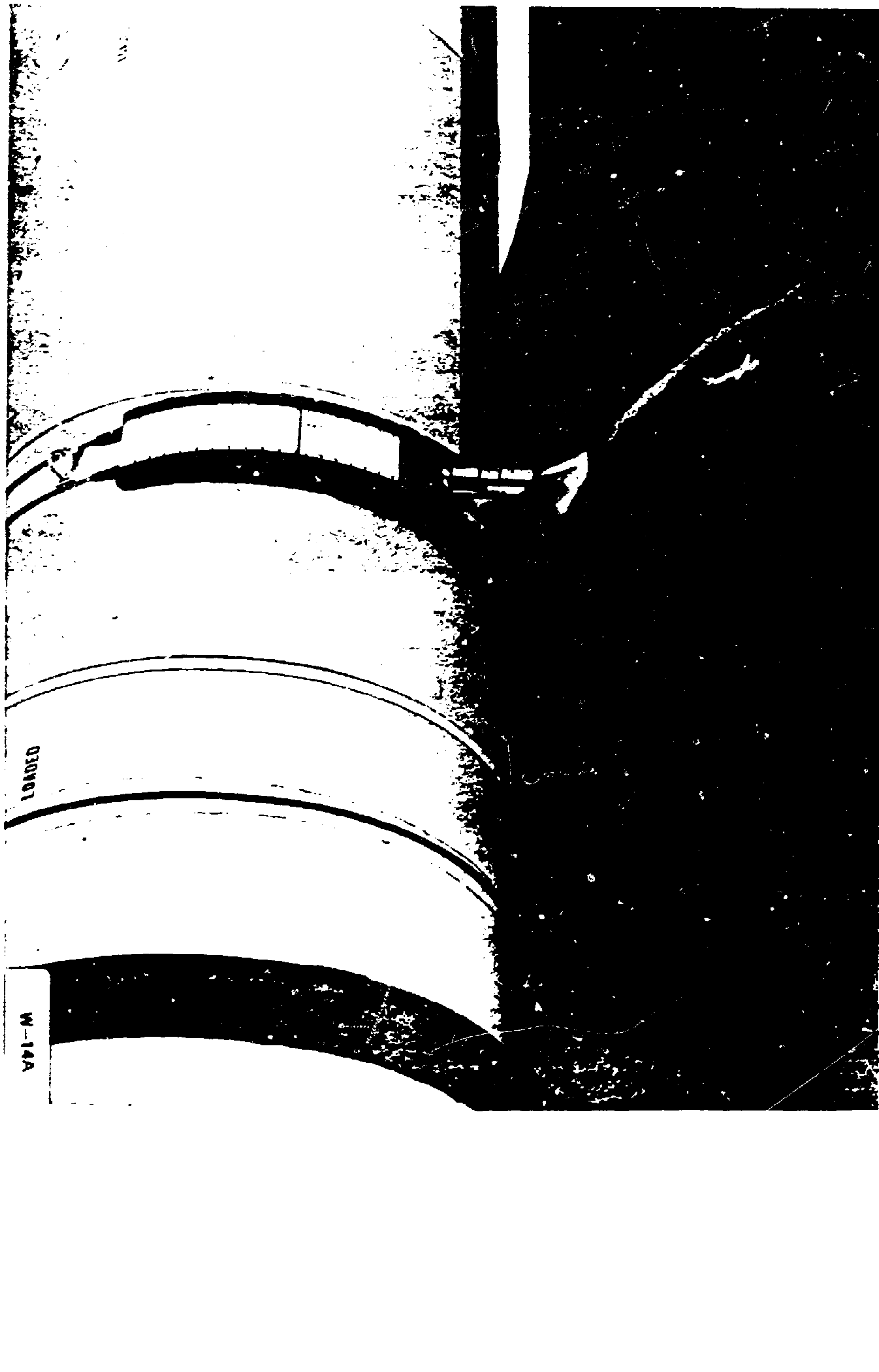
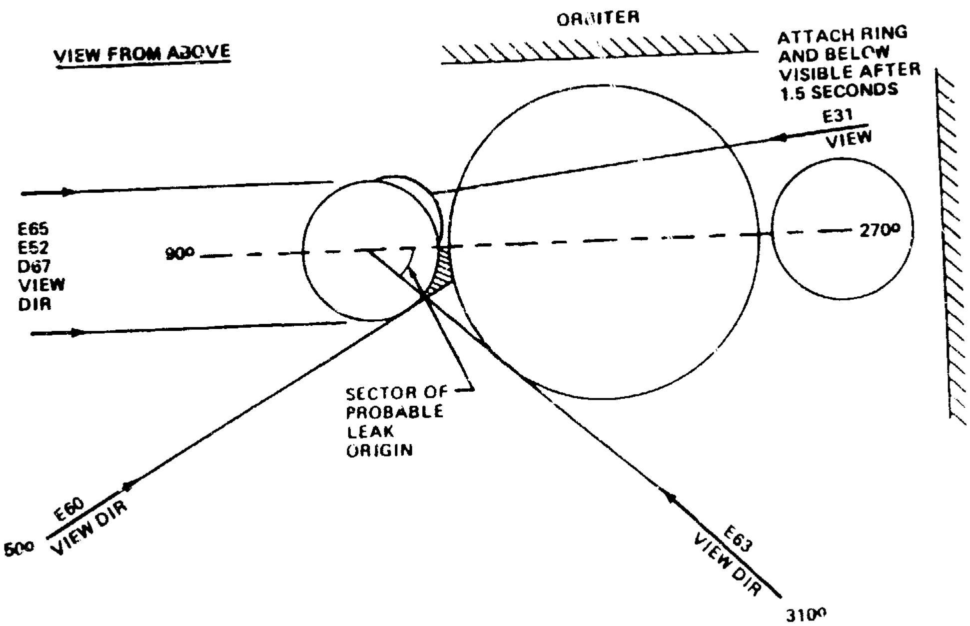


[Ref. 3/21-39]

H–212

CAMERA LOCATION



[Ref. 3/21-40]

W–10

DAMAGE TO EXTERNAL TANK AT LIFTOFF (CONT'D)

- AREA OF CONCERN IS OPPOSITE FSS WHERE ICE WAS PRESENT
- ANALYSIS INDICATES THAT ENERGY ASSOCIATED WITH ICE IMPINGEMENT ON TANK NOT SUFFICIENT TO CAUSE STRUCTURAL DAMAGE
- TESTS INDICATE THAT LEAK OF 0.0037 LBM/SEC. WOULD BE DETECTABLE
- TEST IMPINGING BURNING H_2 JET ON INSTAFOAM INDICATE SIGNIFICANT QUANTITIES OF FLAME AND SMOKE

o EVALUATION OF BURNING H_2 JET IMPINGING ON SRM MEMBRANE OR JOINT

- ENVIRONMENT GENERATED ASSUMING STOICHEMETRIC MIXTURE OF H_2 AND AIR
- MAXIMUM SRM TEMPERATURES WITH JET IMPINGEMENT
  - o 650^0F MEMBRANE
  - o 360^0F O-RING
- TESTS INDICATE THAT O-RING WILL MAINTAIN SEAL AT 1000^0F

W-11

[Ref. 3 21-41]

DAMAGE TO EXTERNAL TANK AT LIFTOFF (CONT'D)

FINDING

0 NO EVIDENCE THAT DAMAGE TO EXTERNAL TANK INITIATED A LEAK OR THAT A LEAK WITHIN THE DETECTABLE LIMITS OF TANK INSTRUMENTATION WOULD INITIATE THE OBSERVED SRM FAILURE

W-17 [Ref. 3 21-42]

OVERHEATING EXTERNAL TANK FROM SRM HOT GAS LEAK

FAILURE MECHANISM

0 HOT GAS LEAK FROM SRM IMPINGES ON EXTERNAL TANK AND INITIATES TANK FAILURE

ASSESSMENT

0 SRM HOT GAS LEAK EVIDENT FROM FLIGHT FILM AT 58.7 SECONDS

0 ANALYSIS HAS ESTABLISHED THAT RESULTING HEATING RATES WOULD BURN THROUGH TANK BETWEEN TIME OF VISIBLE LEAK AND INDICATION OF TANK ULLAGE PRESSURE DECREASED RISE RATE (66.7 SECONDS)

FINDING

0 PROBABLE THAT SRM HOT GAS LEAK INITIATED TANK FAILURE

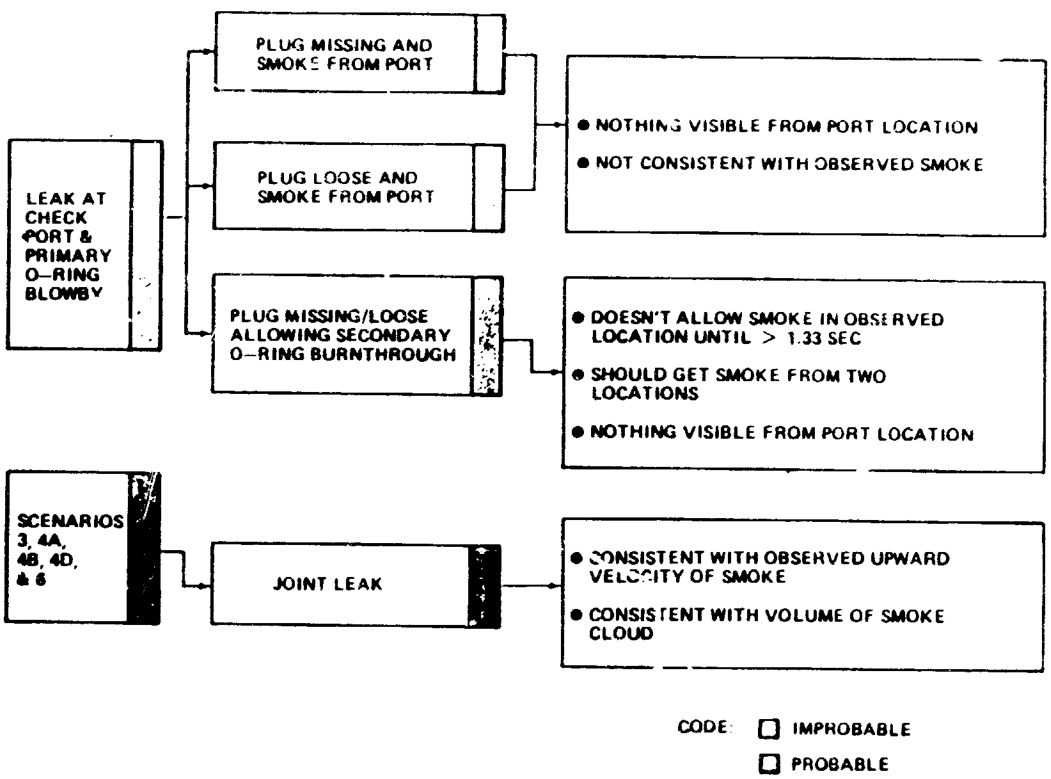
W-18 [Ref. 3/21-43]

LEAK CHECK PORT
(SMOKE AT LIFTOFF)

MARCH 21, 1986

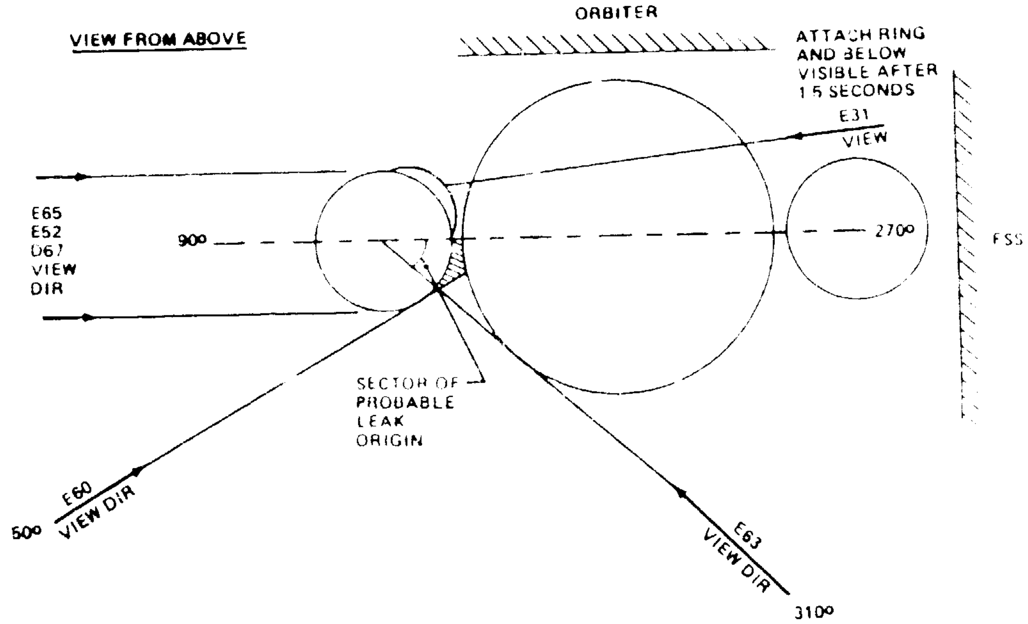
W. LITTLES

H-127



H-212

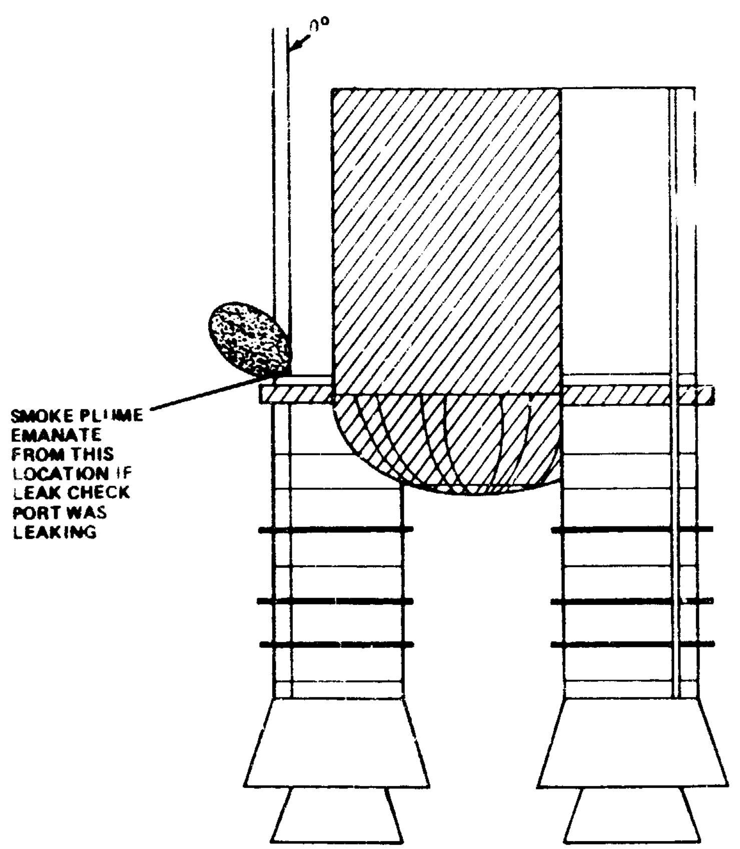
CAMERA LOCATION



W-21 [Ref. 3 21-45]

H-202

CAMERA E-63



[Ref. 3 21-46]

W-22

PROBABLE LOCATION OF SMOKE

- 0 SMOKE PLAINLY VISIBLE AT MET = .678 SEC.
- 0 SMOKE SOURCE NOT VISIBLE
- 0 FILM EVALUATION INDICATES THAT SMOKE PROBABLY ORIGINATES BETWEEN 270° AND 310°
- 0 PROBABLE THAT SMOKE ORIGINATED AT SRM AFT FIELD JOINT

W-23 [Ref. 3/21-47]

TRAJECTORY AND FLIGHT DYNAMICS RECONSTRUCTION

MARCH 21, 1986

HAROLD SCOFIELD

S–0

[Ref. 3/21-48 1 of 3]

PROPULSION SYSTEM PERFORMANCE

0 MAIN PROPULSION SYSTEM PERFORMANCE WAS RECONSTRUCTED

- o PROPELLANT LOADS AT MAIN ENGINE START COMMAND
- o THRUSTS AND FLOW RATE DURING FLIGHT

0 SRM PERFORMANCE WAS RECONSTRUCTED

- o INTERNAL PRESSURES, FLOW RATES, AND THRUST DURING FLIGHT

0 SRM LEAK WAS MODELED

- o HOLE SIZE CHOSEN TO MATCH PRESSURE DROP IN RIGHT SRM
- o THRUST OF LEAK ESTIMATED

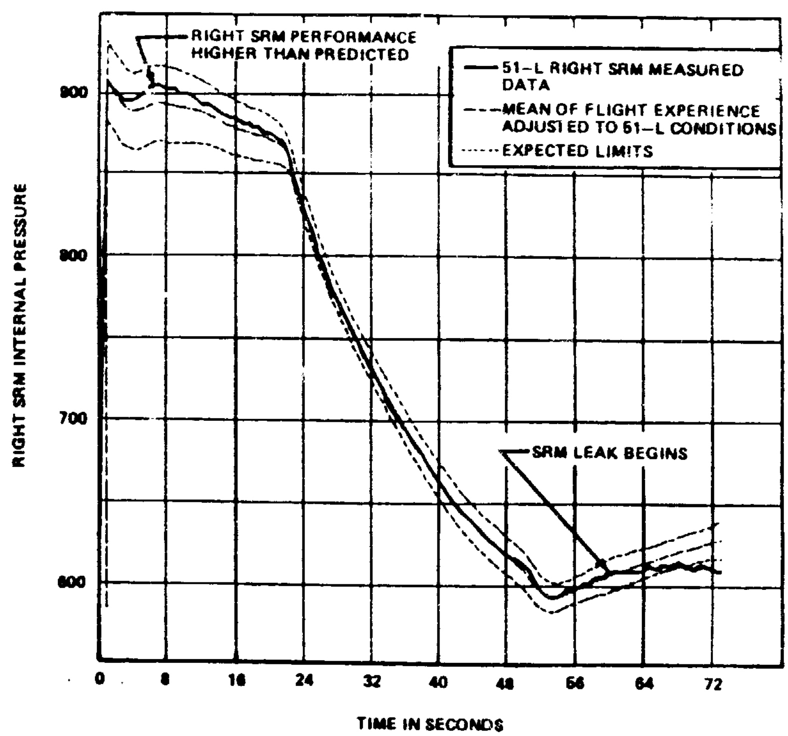
0 ALL DATA SUPPLIED TO JSC AND OTHER MSFC TEAMS

S-3

[Ref. 3/21-49 1 of 2]

H-110

RIGHT SRM INTERNAL
PRESSURE



S-4

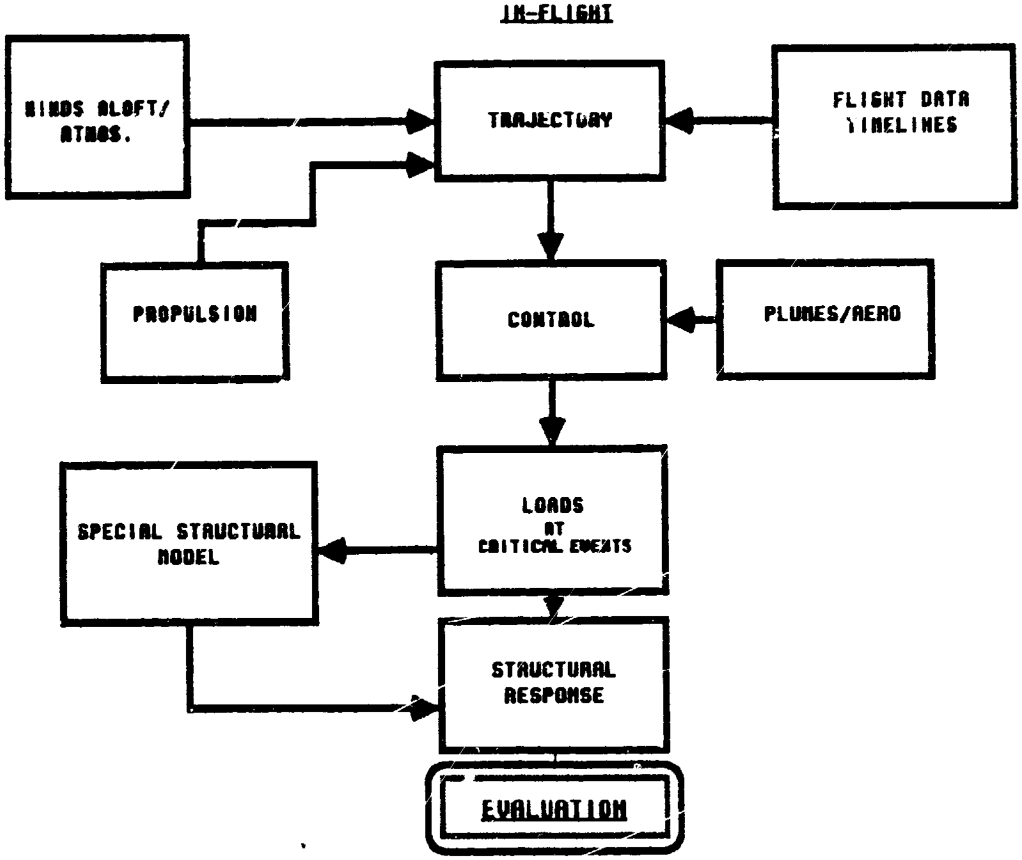
[Ref. 3/21-49 2 of 2]

TRAJECTORY AND FLIGHT DYNAMICS RECONSTRUCTION

- 0 PROPULSION SYSTEMS PERFORMANC
- 0 LIFTOFF DYNAMICS
- 0 ATMOSPHERIC ENVIRONMENT AND AERODYNAMIC
- 0 TRAJECTORY AND FLIGHT PROFILE
- 0 FL. HT DYNAMICS
- 0 SUMMARY

S-1 [Ref. 3 21-48 2 of 3]

FLIGHT RECONSTRUCTION
IN-FLIGHT



[Ref. 3 21-48 3 of 3]

LIFTOFF DYNAMICS

- O LIFTOFF DYNAMICS WERE SIMULATED
  - o THREE DIMENSIONAL TRANSLATION AND ROTATION WITH FLEXIBLE BODY INCORPORATED
  - o INITIAL CONDITIONS FROM LIFTOFF CHOSEN TO MATCH FLIGHT RECORDS
- O MATCH WAS OBTAINED USING VALUES WITHIN NOMINAL RANGE
  - o EXPECTED VARIATIONS IN SSME THRUST RISE
  - o EXPECTED VARIATIONS IN SRM IGNITION TIMING
  - o GROUND WINDS
- O SRM GIMBALLING ACTIVITY FOR 51-L WAS COMPARED WITH STS-6
  - o STS-6 USED CHALLENGER CARRYING SAME PAYLOAD
  - o PLOTS ARE SIMILAR
- O LIFTOFF FILMS WERE EVALUATED
  - o NO COLLISION WITH GROUND STRUCTURE OCCURRED

S–5

[Ref. 3 21-50]

ATMOSPHERIC ENVIRONMENT AND AERODYNAMICS

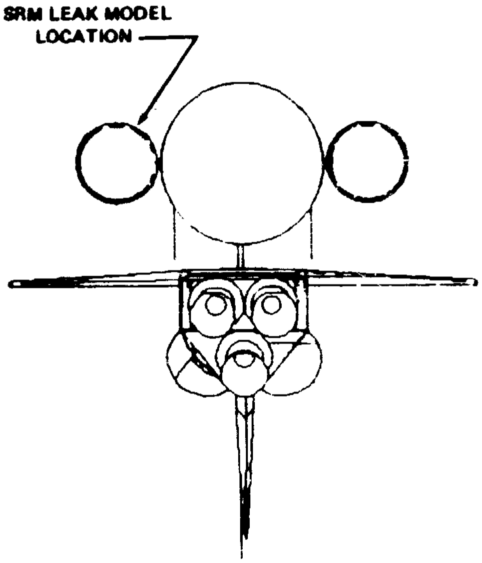
- O ATMOSPHERIC ENVIRONMENT WAS RECONSTRUCTED
  - o WINDS, TEMPERATURES, AND PRESSURE ALOFT
  - o GROUND WINDS
  - o GROUND TEMPERATURES AND RAINFALL HISTORIES
- O PLUME OF SRM LEAK WAS MODELED FROM PHOTO COVERAGE
  - o LEAK LOCATION IS SHOWN IN ACCOMPANYING VISUAL MATERIAL
- O AERODYNAMIC INCREMENTS DUE TO PLUME WERE ESTIMATED
  - o MAXIMUM AERO FORCE ABOUT 130,000 POUNDS AS OPPOSED TO ONLY ABOUT 30,000 POUNDS DUE TO THRUST OF LEAK
- O DATA PROVIDED TO JSC AND MSFC TEAMS

[Ref. 3 21-51]

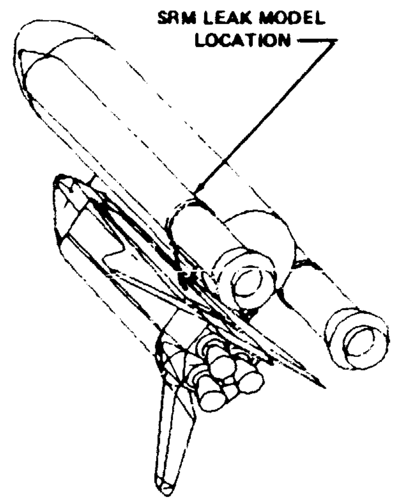
S–6

H–216

SRM LEAK MODEL



AFT VIEW



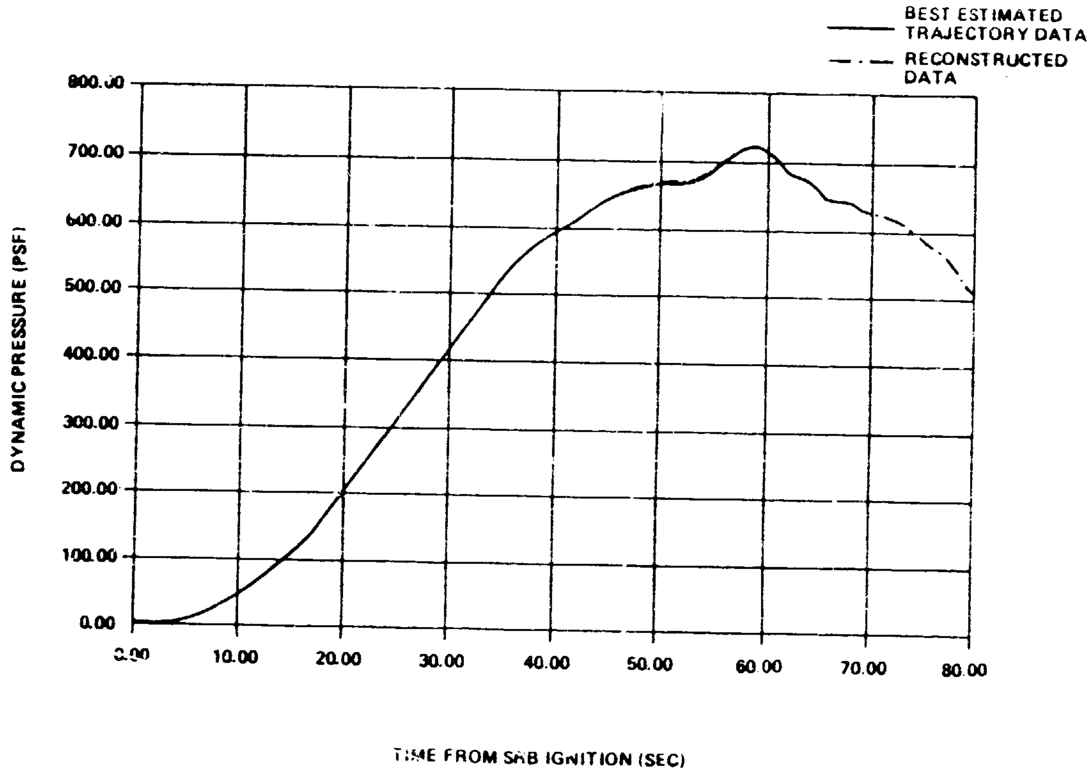
VIEW BY CAMERA E 207
AT 60.6 SECONDS

S–7

[Ref. 3/21-52]

H-210

51-L COMPARISON OF RECONSTRUCTED AND BEST ESTIMATED TRAJECTORY DYNAMIC PRESSURE PROFILE



S-9

[Ref. 3/21-53 1 of 2]

TRAJECTORY AND FLIGHT PROFILE RECONSTRUCTION

- O TIMELINE AND TRAJECTORY WERE DETERMINED USING RECONSTRUCTED
  - o WINDS ALOFT
  - o PROPULSION
  - o SRM LEAK WITH AERODYNAMIC INCREMENTS
- O EXCELLENT TIMELINE AGREEMENT HAS BEEN OBTAINED WITH:
  - o MSFC PHOTO ACTIVITY
  - o JSC PHOTO AND TIMELINE ACTIVITIES
- O RECONSTRUCTED TRAJECTORY WAS SUCCESSFULLY COMPARED WITH JSC BEST ESTIMATED TRAJECTORY
  - o BEST ESTIMATED TRAJECTORY IS BASED ON RADAR AND OTHER INSTRUMENTATION
  - o RECONSTRUCTED TRAJECTORY COMES FROM SIMULATION OF THE VEHICLE IN ITS ENVIRONMENT

[Ref. 3/21-53 2 of 2]

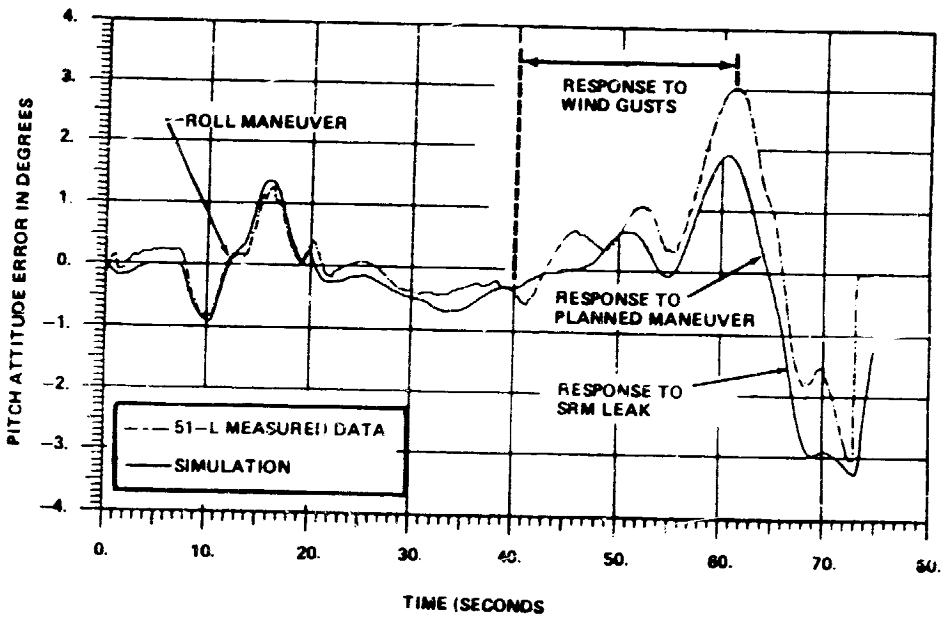
FLIGHT DYNAMICS RECONSTRUCTION

- O FLIGHT DYNAMICS WERE RECONSTRUCTED
  - o EMPHASIS BETWEEN 50 AND 72 SECONDS
  - o MATCH WAS SUCCESSFUL
  - o DATA ACTS AS A CHECK ON SRM LEAK MODEL
- O DATA WAS NEEDED IN LOADS ANALYSES
  - o ALL PARAMETERS ARE WITHIN PREDICTED ENVELOPES
  - o ALL PARAMETERS ARE WITHIN FLIGHT EXPERIENCE UP TO 65 SECONDS
- O PEAK SRM GIMBAL ANGLES WERE WITHIN FLIGHT EXPERIENCE BUT TOTAL ANGULAR TRAVEL WAS NOT
  - o TOTAL GIMBAL TRAVEL (DUTY CYCLE) WAS 132 DEGREES
  - o FOUR PREVIOUS FLIGHTS HAD DUTY CYCLES BETWEEN 120 AND 125 DEGREES

S–10 [Ref. 3/21-54]

H–120

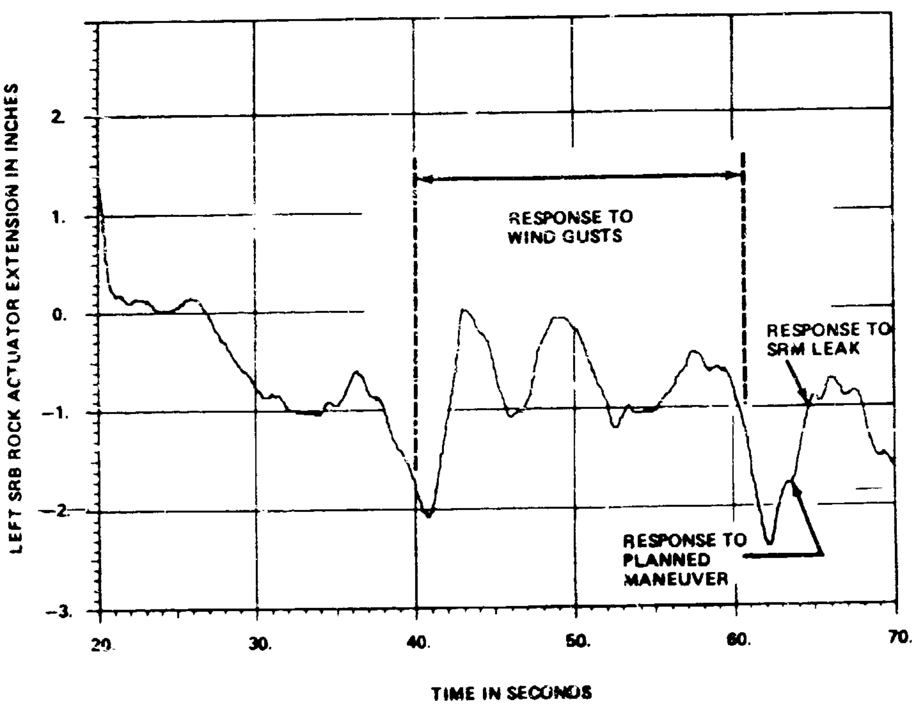
FLIGHT DYNAMICS RECONSTRUCTION



S–11 [Ref. 3/21-55]

H-121

51-L LEFT SRB ACTUATOR EXTENSION
MEASURED DATA



S-12

[Ref. 3/21-56]

SUMMARY OF FINDINGS

- O PROPULSION PERFORMANCE WAS WITHIN PREDICTED LIMITS UNTIL 69 SECONDS
  - o RIGHT SRM PRESSURE BEGAN ANOMALY AT 60 SECONDS
  - o MINIMUM PREVIOUSLY OBSERVED PRESSURE WAS EXCEEDED AT 69 SECONDS
  - o SRM HOLE OPENED AT 60 SECONDS AND GREW TO 45 SQUARE INCHES
- O MEASURED WINDS ALOFT, PROPULSION RECONSTRUCTION AND AERO INCREMENTS BASED ON OBSERVED SRM LEAK PLUME ENABLE:
  - o TRAJECTORY RECONSTRUCTION MATCHING BEST ESTIMATED TRAJECTORY
  - o DYNAMICS SIMULATION MATCHING FLIGHT DATA
- O CONTROL SYSTEM WORKED PROPERLY UNTIL 72 SECONDS
  - o PARAMETERS WERE WITHIN LIMITS FROM PREFLIGHT SIMULATION
  - o PARAMETERS WERE WITHIN FLIGHT EXPERIENCE BEFORE 65 SECONDS
  - o SRM GIMBAL ANGLE DUTY CYCLE WAS LARGE DUE TO WINDS ALOFT

S-13

[Ref. 3/2 57]

LOADS ANALYSES

MARCH 21, 1986

R. RYAN

B-0

[Ref. 3 21-58 1 of 2]

OBJECTIVES

- O DETERMINE LOADS EXPERIENCED BY 51-L FOR ALL FLIGHT EVENTS
- O COMPARE TO EXPECTED AND DESIGN LOADS AND DETERMINE IF EXCESSIVE OR UNUSUAL LOADS EXISTED
- O DETERMINE FORCING FUNCTIONS FOR SPECIAL DYNAMIC ANALYSIS

R-1

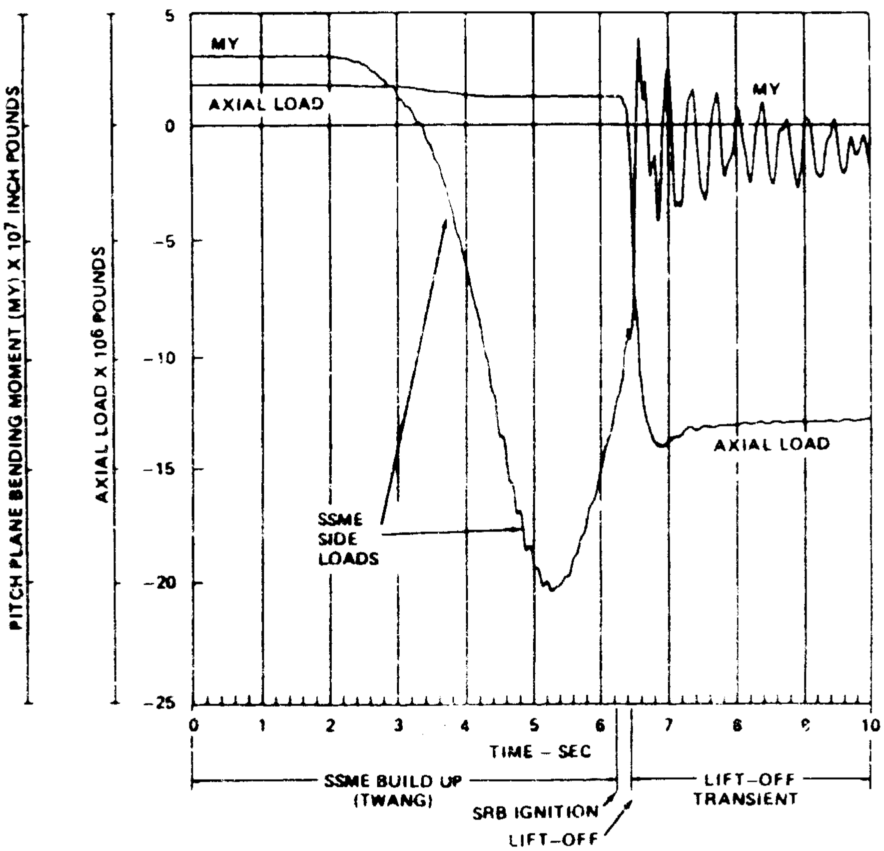
[Ref. 3/21-58 2 of 2]

FLIGHT EVENTS

- O ON PAD
- O LIFTOFF
  - O PRE-IGNITION
  - O SSME BUILD-UP (TWANG)
  - O LIFTOFF TRANSIENT (SRB IGNITION THRU 10 SECONDS)
- O IN-FLIGHT
  - O ROLL MANEUVER
  - O MAX "Q"
  - O 51-L EVENT (POST 58 SECONDS)

R-2 [Ref. 3/21-59]

LIFTOFF SEQUENCING: AXIAL LOAD AND PITCH PLANE BENDING MOMENT



R-3

[Ref. 3-21-60]

LIFTOFF FILM EVALUATION

- 0 51-L WAS COMPARED WITH 41-D
- 0 SRB TIP DEFLECTION 24" (51-L); 28" (41-D)
- 0 FREQUENCY WAS APPROXIMATELY 0.25 HZ FOR BOTH VEHICLES
  - o 25 HZ MODE AT APPROXIMATELY 600 PSIA - THRUST WALLOWS AROUND IN NOZZLE
  - o 30 HZ MODE AT APPROXIMATELY 1200 PSIA - NOZZLE FILLS MACH CONE (VISIBLE)
- 0 LIFTOFF CLEARANCE NEAR NOMINAL

R-4

[Ref. 3/21-61]

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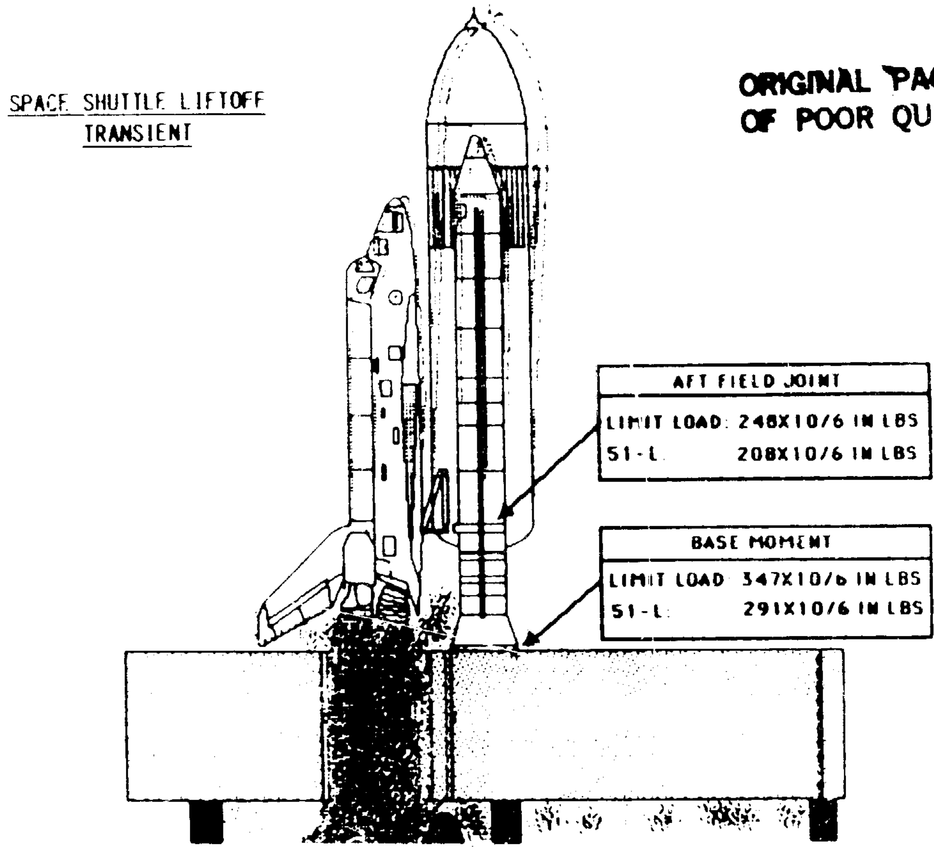


R-5 [Ref. 3 21-62]

51-L MEASURED DATA

HOLDDOWN POST

0 POST DESIGNED BY SSME BUILDUP

0 POST HAD STRAIN GAUGES ON EACH

0 DATA VALID UNTIL SRB RELEASE

0 BASE MOMENT: DESIGN VALUE 347,000,000 IN-LBS. 51-L RIGHT SRB 291,000,000 IN-LBS.

0 NO INDIVIDUAL POST LOAD EXCEEDED LIMIT LOAD

0 MOMENT AT SRB AFT FIELD JOINT (1491)

- o DESIGN 240,000,000 IN-LBS.
- o 51-L 208,000,000 IN-LBS. (RI RECONSTRUCTION)

0 SRB RELEASE AT EACH POST WAS ESSENTIALLY SIMULTANEOUS

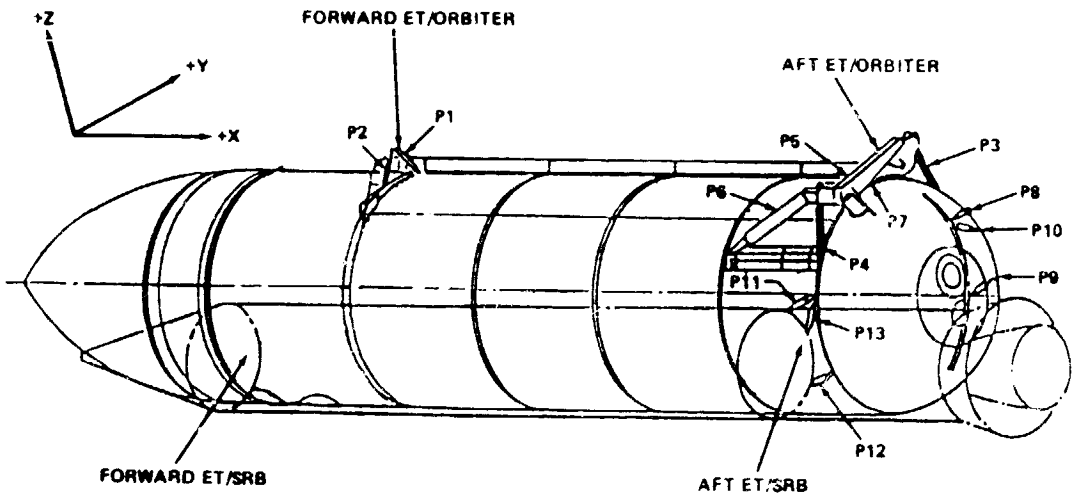
GIMBAL ANGLES

0 GIMBAL ANGLES SMALL, INDICATING NEAR NORMAL LIFTOFF DYNAMICS

R-6 [Ref. 3 21-63]

H–208

SHUTTLE STRUT IDENTIFICATION



P–7

[Ref. 3/21-64]

K-203

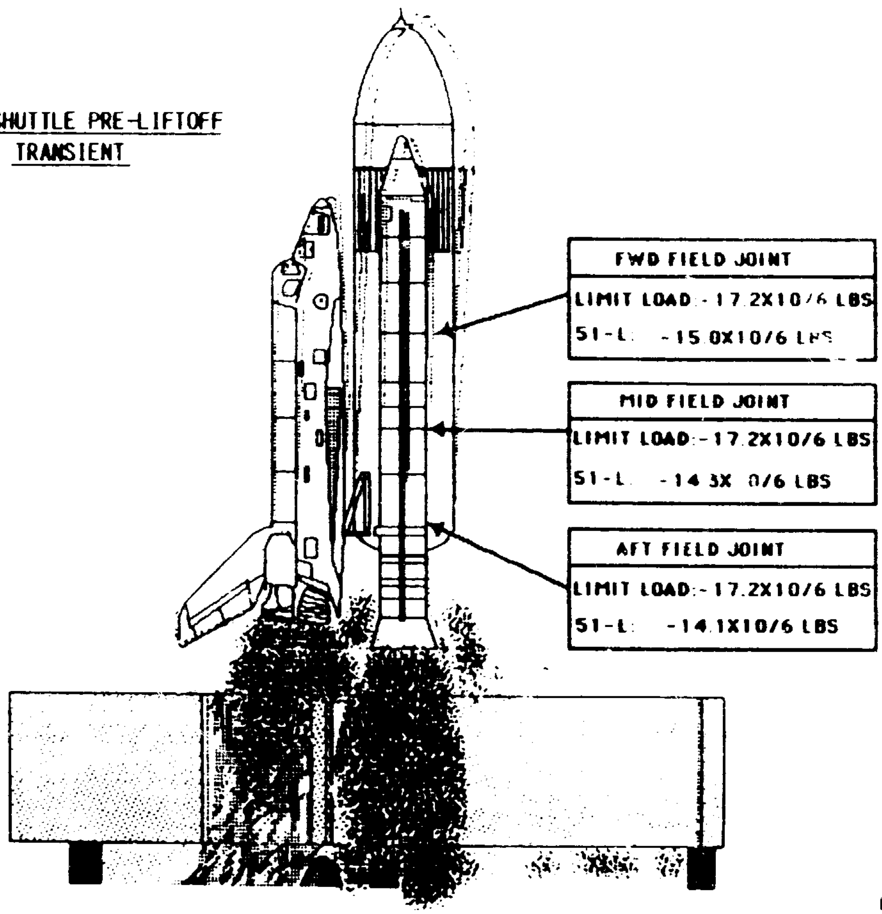
LIFTOFF MEASURED STRUT LOADS VERSUS 51-L CALCULATED

STRUCTURAL ELEMENT		STS-1 → 7 MEAN (KIPS)	STS-1 → 7 SIGMA (KIPS)	51-L (KIPS)	LIMIT LOAD (KIPS)
P1	ET/ORB	-51	2.6	-48	-131
P2	ET/ORB	-46	3.1	-60	-131
P3	ET/ORB	163	7.8	92	484
P4	ET/ORB	163	7.4	111	481
P5	ET/ORB	-541	11.5	-471	-834
P6	ET/ORB	-530	13.0	-532	-834
P7	ET/ORB	8.2	2.0	13	177
P8	ET/SRB	-77	13	-139	-306
P9	ET/SRB	138	10	138	393
P10	ET/SRB	-116	20	-108	-306
P11	ET/SRB	-61	27	-141	-306
P12	ET/SRB	125	10	140	393
P13	ET/SRB	-118	27	-94	-306

R-3

[Ref. 3/21-65]

SPACE SHUTTLE PRE-LIFTOFF
TRANSIENT



R-9 [Ref. 3 21-66]

H-204

MAX "Q" RESULTS

STRUCTURAL ELEM.		LIMIT LOAD VALUE	51-L (ESTIMATED)
P3	AFT ET/ORB	484, -334	210
P4		481, -481	225
P5		168, -834	-415
P6		168, -834	-416
P7		177, -173	20, -20
P8	SRB/ET	393, -306	113
P9			190
P [illegible]			-160
P11			92
P12			95
P13			± 60
ET BARREL 3-40		10,000,000 LOAD IND.	6,000,000

SRB FIELD JOINT 1491

EQUIV. LOAD	-17,200,000 LBS	-10,800,000 LBS

R-10

[Ref. 3/21-67]

FINDINGS

- 0 STS 51-L LOADS WERE LOWER THAN DESIGN LIMIT LOADS

- 0 EFFECTS OF STS 51-L LOADS ON DEGRADED RIGHT SOLID ROCKET BOOSTER AFT FIELD JOINT BEING ASSESSED

R–11

[Ref. 3/21-68]

51-L SRM FIELD JOINT ANALYSIS

MARCH 21, 1986

R. RYAN

R–12

[Ref. 3 21-69 1 of 2]

OBJECTIVE

RECONSTRUCT 51-L SRM FIELD JOINTS RESPONSE (GAP OPENING) FOR ALL EVENTS USING MATING DATA, NATURAL ENVIRONMENTS, INDUCED ENVIRONMENTS, AND RECONSTRUCTED 51-L LOADS.

R-13

[Ref. 3/21-69 2 of 2]

SCOPE

CONSIDER ALL 51-L ASSEMBLY/FLIGHT EVENTS

- O MATING (SRM SEGMENTS)
- O STACKING
- O TRANSPORTATION
- O ON PAD
- O LIFTOFF
  - O SSME THRUST BUILDUP (TWANG)
  - O SRM IGNITION TRANSIENT
  - O LIFTOFF TRANSIENT (AFTER RELEASE)
- O ROLL MANEUVER
- O MAX "Q"
  - O 40 SECONDS
  - O PRE 58 SECONDS
  - O POST 58 SECONDS

R-14

[Ref. 3/21-70]

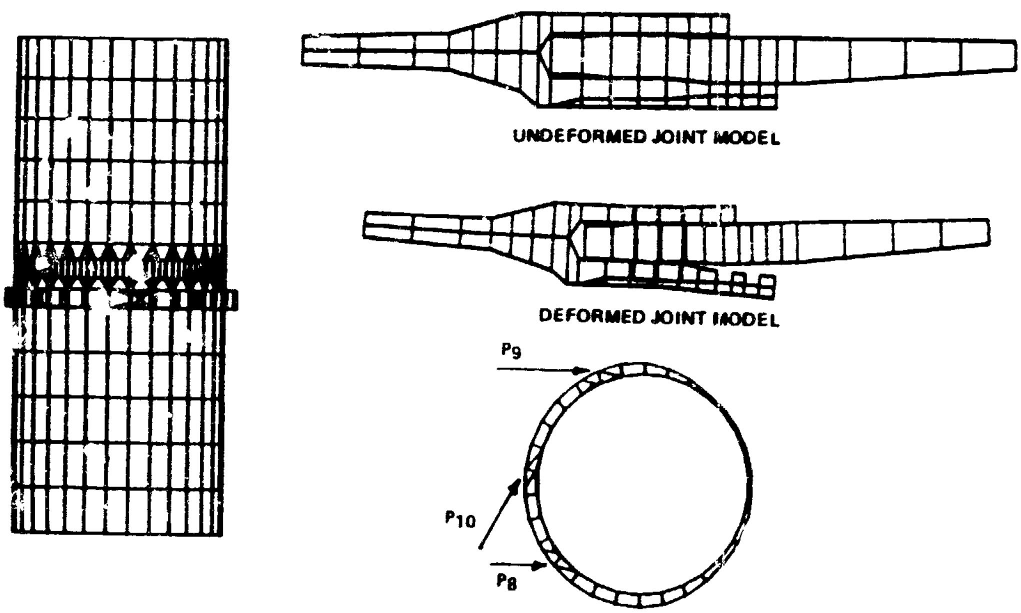
APPROACH

- 0 DEVELOP FINITE ELEMENT STATIC AND DYNAMIC MODELS
- 0 TUNE MODELS TO JOINT ROTATION TEST
- 0 ADD SRM SEGMENTS, PROPELLANT, SRB STRUCTURE TO MODEL
- 0 CHARACTERIZE INITIAL JOINT CONDITION
- 0 CHARACTERIZE NATURAL AND INDUCED ENVIRONMENTS (WINDS, TEMPERATURE, LOADS, ETC.)
- 0 DETERMINE STATIC AND DYNAMIC RESPONSE OF GAP AT PRIMARY AND SECONDARY SEALS.

[Ref. 3 21-71]

H–200

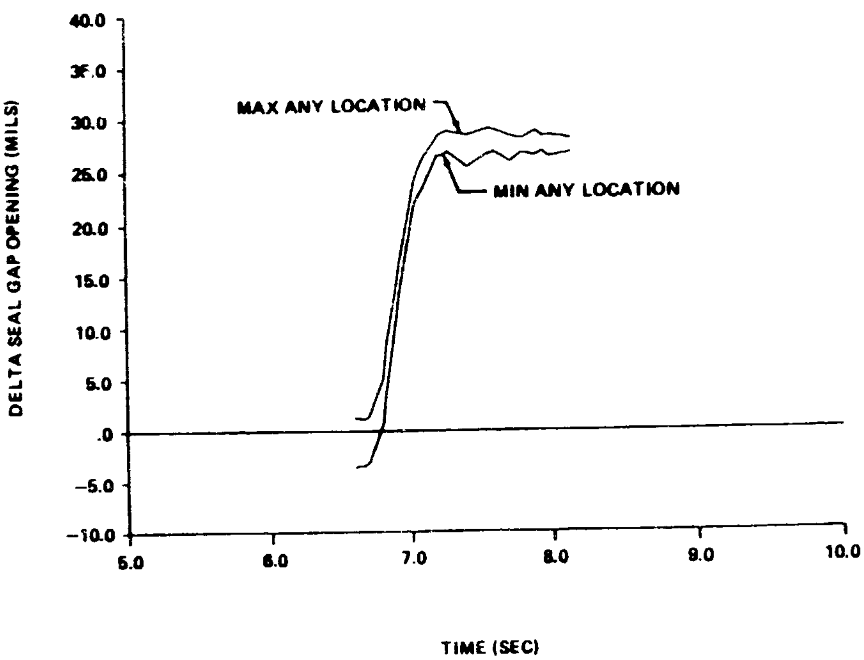
SRB 3D SOLID ELEMENT/1 DIA, LENGTH MODEL



[Ref. 3/21-7]

H-206

MAX-MIN PRIMARY SEAL GAP OPENING LIFTOFF



[Ref. 3/21-73]

STATIC GAP RESULTS

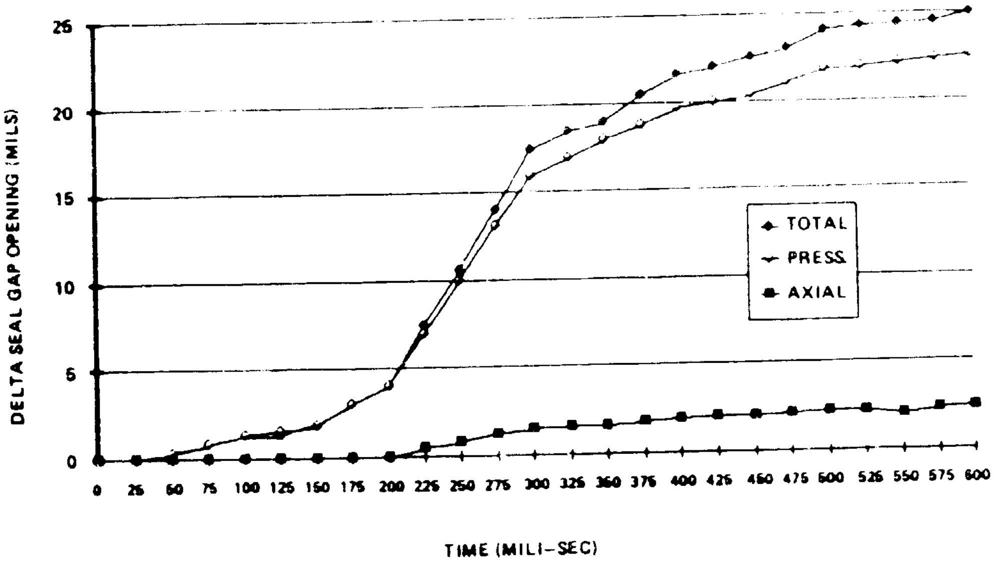
FORCES AND MOMENTS USED

PRESSURE

AXIAL FORCE

H-206

2D AXISYM ELEM. STATIC
(JOINT ROTATION TEST)



R-15

[Ref. 3 21-74 2 of 2]

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AFTERNOON SESSION

CHAIRMAN ROGERS: Will the Commission come to order, please?

DR. LITTLES: Mr. Chairman, we will now go into the final three topics. The first one will be a short review of an O-ring analysis model. This model has been in existence for some long period of time. We are using it in this investigation, and Garry Lyles will tell the Commission what the pedigree of that model is and how it has been verified, and then Rick Bachtel will go into the work that has been going on relative to the joint thermal flow analysis. Primary emphasis of this analysis is trying to establish whether we could have had a continuous link from the time we first saw the puff of smoke until 58 or 59 seconds.

(Witnesses sworn.)

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TESTIMONY OF GARRY M. LYLES, PROPULSION ANALYSIS BRANCH, STRUCTURES AND PROPULSION LABORATORY, MARSHALL SPACE FLIGHT CENTER, AND FREDERICK D. BACHTEL, THERMAL ENGINEERING BRANCH, STRUCTURES AND PROPULSION LABORATORY, MARSHALL SPACE FLIGHT CENTER

MR. LYLES: My name is Garry Lyles. I have been an employee of Marshall Space Flight Center for ten years in the area of propulsion systems analysis, propulsion analysis, dealing in the area of internal flow dynamics, and as Dr. Littles stated, I will present the analytical modeling that has been done to predict O-ring erosion and the model's application as a tool to evaluate possible 51L O-ring failure mechanisms.

Could I have Chart L-1, please?

(Viewgraph L-1.) [Ref. 3/21-75]

MR. LYLES: As an introduction to the O-ring erosion scenario and the two types of O-ring erosion that we have modeled and can cause erosion of the O-rings, what I would like to do is talk from the picture, and I would like to zoom in, if I could, on the area of the putty and the O-rings just a little bit. The hot gas enters the cavity between the putty and the primary O-ring through a path in the putty, this path that we have been calling a blow hole.

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The blow holes range in size. They are nominally one-inch width and flow between the insulation to the primary O-ring, and this causes impingement of a hot gas jet on the O-ring, and this impingement erosion continues until the cavity in front of the primary O-ring equals motor pressure, and this occurs during the pressurization transient of the SRB or the solid rocket motor. [Ref. 3/21-76]

That is the impingement erosion, and that scenario assumes that the primary O-ring seals. The other type of erosion that we have modeled is the erosion which could occur if the primary O-ring does not seal, and in fact we get leakage by the primary O-ring. In this case, we predict what we call blow-by erosion of the primary O-ring. The hot gas then continues on into the cavity between the primary and secondary O-rings, pressurizing that cavity until the two cavities equalize, and we also predict impinging erosion on the secondary O-ring.

Could I have Chart L-4, please?

(Viewgraph L-4.) [Ref. 3/21-77]

MR. LYLES: Chart L-4 is a description of the analysis and the modeling that we have done in this analysis. The first part of the analysis is a cavity pressurization model which we have modeled as a simple

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lumped-parameter cavity pressurization model in which we solved the conservation of mass and energy equations by numerical integration.

The model includes the solid rocket motor pressurization transient which is a function of time, of course. It models the change in volume of the seal cavities as the seal rotates or the tang and interleg of the clevis move apart. It also models the heat transfer from the gas to the metal parts and predicts a cavity pressure and temperature which predicts how long a gas jet can flow into the cavity, and how long it can impinge on the primary O-ring.

We also have an impinging heat transfer model or an erosion model of the O-ring based on impinging heat transfer coefficients in which, again, the hot cast jet pressurizes the O-ring cavity, impinges on the primary O-ring, and we erode the O-ring by stagnation point heat transfer.

CHAIRMAN ROGERS: If I may ask you a question, is this sort of a general study of the O-rings, or is this trying to simulate what happened in the accident, or what?

MR. LYLES. This analysis originated about a year ago, and it originated because we were getting some erosion of the O-rings, and we wanted to study that and

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determine if we could the cause and try to—

CHAIRMAN ROGERS: In other words, this has been a study that has been going on for a year?

MR. LYLES: Yes, sir, and it started about a year ago, and it was presented in, I think, July of last summer.

CHAIRMAN ROGERS: And why was it started?

MR. LYLES: It was started because flight data on disassembly indicated that we were getting some erosion on the primary O-ring.

CHAIRMAN ROGERS: Which flight caused you the most concern or gave rise to the study?

MR. LYLES: Well, it was—I think it was just the general fact that we were getting erosion. I am not sure it was a specific flight. We did—

CHAIRMAN ROGERS: Well, what prompted it? There must have been something that prompted it.

MR. LYLES: Well, I think at the time the model was first created, we were getting erosion, we started getting erosion in the field joint, and we had seen some erosion in the nozzle joint, and those data points indicated to us that an analysis needed to be performed to determine the cause of this.

CHAIRMAN ROGERS: How large a group worked on this?

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MR. LYLES: This was originated by Dr. Salita at Morton Thiokol, and he developed the model. We then, after the model was presented to us, we brought the model in-house and audited the model, and have exercised the model, and have made some modifications ourselves to it.

CHAIRMAN ROGERS: Who worked on that project?

MR. LYLES: Well, Dr. Salita at Morton Thiokol worked on it. Out of my group I have worked on it. And a couple of engineers in my group have run the model.

CHAIRMAN ROGERS: Did it have a name: Did you have a name for the group that worked on it?

MR. LYLES: Yes, sir. The engineers in my group were Sam Lowry and we've got another young engineer, John Hutt, who worked on the model, and we are the ones.

CHAIRMAN ROGERS: Were any of you involved in the telecon the night before the 51L launch?

MR. LYLES: No, sir.

CHAIRMAN ROGERS: Were any of you asked to give information about the work you had been doing for that group on the telecon?

MR. LYLES: No, sir.

CHAIRMAN ROGERS: Do you have a report of the work that you have been doing on that joint, anything in writing that your group has produced?

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MR. LYLES: Dr. Salita has published two reports on the work that has been done.

CHAIRMAN ROGERS: When were those reports made?

MR. LYLES: I think Part 1 of the report was issued in July of last summer. The second report, I believe it was the following month.

CHAIRMAN ROGERS: Did they point out in those reports—are you familiar with the reports?

MR. LYLES: Yes, sir.

CHAIRMAN ROGERS: Did they point out that there was concern about the O-rings and what had happened previously?

MR. LYLES: The reports were mostly a technical writeup on the modeling work itself. Part 2 showed a parametric analysis, and compared that analysis to the flight data. It also gave some analysis looking at what the limit cases could be on O-ring erosion, and it indicated that you could predict the type of erosion that we saw in flight.

CHAIRMAN ROGERS: Were those reports given wide circulation, or were they closely held?

MR. LYLES: I am sorry, I can't answer that. I got a copy. And there are several copies around my office. I assume it got wide circulation.

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CHAIRMAN ROGERS: Was it fairly well known as far as you knew at Marshall that these studies were being conducted?

MR. LYLES: Yes, sir, I think that analysis had been presented a couple of times.

CHAIRMAN ROGERS: And I assume the same thing was true, obviously, in Thiokol too?

MR. LYLES: Yes, sir.

CHAIRMAN ROGERS: Okay, go ahead. Thank you.

MR. LYLES: Could I have Chart L-5, please?

(Viewgraph L-5.) [Ref. 3/21-78]

MR. LYLES: The other type of erosion that we have modeled we call blow-by erosion, and in this model we treat the jet as a hot jet created by an assumed initial blow-by area beneath the primary O-ring, which allows gas to impinge on the secondary O-ring. We erode the primary O-ring by simple pipe flow heat transfer relations. We then spread the jet and erode the secondary O-ring by impingement heat transfer as we have shown in the primary O-ring.

The model has several parameters that affect the magnitude of erosion that you would predict. Those parameters are the width of the jet or the area of the blow-hole that you get, which influences the rate at which you pressurize the cavity and which, of course,

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drives the time at which you are eroding the O-ring.

DR. WALKER: I have a question. How did you determine the size blow-holes to use?

MR. LYLES: We based our estimates of blow-hole size on the disassembly of the spent motor cases from flight, as when they take the joints apart, if there is evidence of a blow-hole through the putty, they measure the blow-hole and over that range of measured data we assumed that we get nominal blow-holes of that size.

Now, we did in our parametric study look at a wide range of blow-hole sizes, and we did show that when you calculate, when we do a worst case analysis, that we do bound the flight data, that is, we predict erosion greater than the available data we have from the flight motors.

DR. WALKER: I would just like to pursue the question of the analysis of the blow-holes observed for a moment. Who carries out that analysis, and is that analysis made for every single case which is disassembled?

MR. LYLES: Well, it is more of an inspection than an analysis, and yes, sir, I believe the putty is inspected every time the joints are disassembled. We have data on all of the flight joints except, I think, one that was lost at sea.

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DR. WALKER: Could the Commission get copies of that, of those records?

MR. LYLES: Yes, sir.

CHAIRMAN ROGERS: Mr. Lyles, were you consulted at all prior to 51L by anybody about O-rings and the study you were doing?

MR. LYLES: Yes, sir, we made a presentation on the modeling work and our calculations on erosion before 51L.

CHAIRMAN ROGERS: When was that?

MR. LYLES: It was last summer, in the August time frame.

CHAIRMAN ROGERS: But not after that. I mean, the summer of 1985?

MR. LYLES: No, sir.

CHAIRMAN ROGERS: Was there any discussion with you after the accident about the work you were doing?

MR. LYLES: Well, when it became apparent that it was a possible joint failure, we immediately turned and started exercising the erosion model again, and looking at the parametrics that we have run, we haven't come up with any different conclusions than we had at the time, that is that the parametric analysis that we have run bounded the data from an impinging erosion

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standpoint.

There is one thing that I should say, and that is, these are two completely—not completely different problems, but they are different problems, the impingement erosion and blow-by erosion. The model does not predict when you would leak past the primary O-ring. We have to assume in the model that you could leak past the primary O-ring, and then we can calculate an erosion rate to the O-ring, but the model is not sophisticated enough to tell us when the O-ring would leak. That is a major assumption in the model, and we have to assume the size of the hole, and so on and so forth.

CHAIRMAN ROGERS: I am not quite clear. Did you finish your work on the model before the accident?

MR. LYLES: No, sir. The work on the model is ongoing, and we have been trying to improve it all along.

CHAIRMAN ROGERS: Did you reach any conclusion before the accident from your model work?

MR. LYLES: The conclusion that was reached by running the analysis was in the range of the blow-holes that we had seen, and even for blow-holes smaller than that and for worst case analysis, that we would not show enough erosion to burn completely through the O-ring,

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and it would leak by impinging erosion.

We did show that if in fact the primary O-ring leaks, that erosion would, of course, continue until you filled the secondary cavity, and you could get large amounts of erosion of the primary seal, and then all you have got left is the secondary seal. On one flight, we did have erosion of the secondary seal, as you probably know, on the nozzle joint, and we matched that data very well, and again, by worst case analysis we showed that you would not get enough erosion on the secondary nozzle joint to burn through, and the worst case analysis was in fact a worst on worst analysis.

It is assumed that all of the gas that passed by the primary seal did impinge on the secondary seal, and in fact it doesn't. It has to go around a 90-degree bend, and it spreads in three dimensions, and we had to take that into account to match the flight data that we had.

CHAIRMAN ROGERS: Were you familiar with the criticality 1 decision that was made in, I guess, December of 1982, which said that the secondary O-ring could not be counted on if there was a failure of the primary seal?

MR. LYLES: No, sir, I was not.

CHAIRMAN ROGERS: You weren't familiar with

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that?

MR. LYLES: No.

CHAIRMAN ROGERS: And your work came to the opposite conclusion, I assume you felt that maybe the secondary O-ring would hold?

MR. LYLES: For the nozzle joint, and we did come to the conclusion that the secondary O-ring would seal on the nozzle joint because it does not go through the same rotation and problems that the secondary seal has on the field joint.

CHAIRMAN ROGERS: You weren't talking about the field joint then?

MR. LYLES: No, sir.

CHAIRMAN ROGERS: Go ahead.

MR. LYLES: Okay, the model was then validated. The erosion model was validated by subscale hot fire data. If I could have Chart L-8, please.

(Viewgraph L-8.) [Ref. 3 ... 79]

MR. LYLES: Chart L-8 shows the calibration of the model or the predicted model results versus the results from the subscale test data, and in this test the blow-hole was simulated by a rectangular orifice upstream of a Viton O-ring. The field joint was simulated in these tests, and it, the data, showed that analytically we could match the erosion, and we did vary

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the width of the upstream orifice to simulate the varying widths of the putty blow-hole, and we varied the cavity volume to calibrate the pressurization model that we have in the analysis, and we are showing a plus or minus 12 percent variation on the measured data, and the picture is there. It just shows—it just represents the type of erosion that we are seeing on the subscale test versus the one case of the flight erosion, and it shows the same type of erosion that we saw in flight.

MR. COVERT: Mr. Lyles, talking about validation of the model, if I understand the model, there is an undefined heat transfer coefficient that allows for the cooling of this gas stream, and there is also an undefined heat transfer coefficient at the stagnation point. Did you do other tests to validate the model before you did the comparison with the subscale data? Or does this data represent the best fit of the results from your model based on the selection of those two coefficients?

MR. LYLES: This does represent the best fit of that subscale data based on varying the heat transfer coefficients to the metal, and I think they had to vary the discharge orifice, discharge coefficient some.

DR. COVERT: Thank you.

DR. WALKER: Mr. Lyles, were you asked or did

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someone else as a result of your analysis try to determine whether this analysis suggested there was a serious safety problem?

MR. LYLES: I think this analysis showed that we did have a problem in the joint with erosion. I don't think this model showed that we had—that we were getting real close to a flight failure. As I said, the worst on worst case that—by analysis that we put on this model did bound the flight data, and we predicted much larger erosion than we had seen. And when we used that worst on worst case, we still did not show that we would burn through an O-ring unless the O-ring did not seat and we got blow-by erosion.

And in the case where the O-rings don't seat, if both O-rings don't seat the model becomes moot, really.

DR. WALKER: Did you consider the possibility that the secondary O-ring would become unseated as a result of the rotation?

MR. LYLES: I was really—at the time we were doing this analysis, really not up to speed on the dynamics of the joint rotation on the field joint.

DR. LITTLES: I think we are getting a little beyond Garry Lyles' involvement in this. He was just an analyst who was doing this analytical work but he was

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not involved in the safety issue or the criticality issue or those kinds of things. So it is a little bit beyond what he does, I think. He is not really involved in those things.

VICE CHAIRMAN ARMSTRONG: I would like to ask with respect to this chart, because I think it may have confused some. We talk about prediction versus measured, and I understand that measured has to do with the after the fact measurement of erosion on recovered seals from previous flights. Is that correct?

MR. LYLES: That is correct.

VICE CHAIRMAN ARMSTRONG: But the prediction has to do with what your model would say the erosion would be given certain conditions appropriate to those flights. Is that correct or not?

MR. LYLES: Well, we can't specifically analyze a flight. We don't have any idea. We can't predict what the blow-hole would look like on a specific flight. All we can really do is take the data that is available to us and try to calibrate the model with that data we have, and it is really not good enough to make a specific prediction on a flight, because we don't know yet how to handle the putty blow through.

VICE CHAIRMAN ARMSTRONG: Thank you. Let me ask it a different way then. It is a comparison of the

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actual erosion as measured post-flight with the prediction that your model would hypothesize, given the conditions as close as you might be able to guess them appropriate to that particular seal. Is that correct?

MR. LYLES: That is right.

VICE CHAIRMAN ARMSTRONG: And it does nothing—the prediction has nothing to do with predicting into the future?

MR. LYLES: No, sir.

VICE CHAIRMAN ARMSTRONG: And one should not assume from this prediction that you could predict what the erosion on 51L might have been or what the prediction on the next flight would be.

MR. LYLES: No. We did—all we did was a parametric analysis, and with the seeming randomness of the blow-hole and the putty, there is no way that we could predict a future flight erosion.

VICE CHAIRMAN ARMSTRONG: Thank you.

MR. LYLES: If I could have chart L-10, please.

(Viewgraph L-10.) [Ref. 3/21-80]

MR. LYLES: This chart just says that we do have a reasonable analytical tool to predict erosion within the bounds of the prediction. We are still improving the model, and we are using it to evaluate the

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O-ring failure mechanisms due to erosion for 51-L failure. Included in that we are improving the analysis relative to O-ring heating as we blow by the O-ring. We will try to correlate that with the mass and mass flow that has been analyzed for the black puff of smoke.

If there are no more questions, that concludes my presentation.

CHAIRMAN ROGERS: Thank you very much.

MR. BACHTEL: My name is Rick Bachtel, and I have been with the Marshall Space Flight Center for the past 21 years, and I have been working in the area of heat transfer thermodynamics and thermal analysis, and since the 51L incident I have been assigned to the SRM failure analysis team in the area of SRM thermal analysis.

What I am going to talk to you today about is some of the work that we have been doing about concerning the flow through the clevis that would be initiated at the leak. We are dealing with a subset of the scenario that says that the leak occurs at liftoff as a result—or the puff of smoke indicates that there is a leak at liftoff, that the smoke is either obscured or disappears, however the leak continues until 58 seconds, when it becomes obvious in the form of the plume that we see.

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The purpose of the analysis we did was to go off and see if that kind of scenario is feasible and to understand the events that happened between the puff of smoke and 58 seconds, and of course the alternate to the scenario that I am going to discuss is that the leak stops and then starts again in 58 seconds.

The objective of the analysis was twofold. One of them was to determine the thermal response of the joint during that continuous leak, and the other one then was to go back and look at what kind of parameters either flow or dimensional or whatever would be required to sustain a 58-second or 60-second leak consistent with the observations.

(Viewgraph B-1.) [Ref. 3 21-81]

MR. BACHTEL: When we are assessing the thermal analysis there are three events we were looking at. One of them is when the exit temperature or the exit gas temperature, the gas coming, emanating from the joint, when that temperature exceeds 3,000 degrees, it was felt that at 3,000 degrees, that it would be clearly luminous, that it would appear as the plume which we saw.

Another one is, when the outer surface comes to a temperature of 2,150 Fahrenheit, again, that was a visibility factor. It was felt that at that temperature

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it should be white hot and may represent what we saw. It is also indicative of a possible failure of the case, of course, and then the third event which we are looking for is any kind of failure of the clevis or the joint that would allow a blowing hole, which is pretty much obvious what we saw at that time.

Some of the parameters which we were assessing to estimate whether or not we could have a 60-second leak was the leak flow rate. We tried to determine what leak flow rate or how small a leak flow rate we would have to have before we could get out to 60 seconds without a catastrophe, what kind of putty blow-hole size, the blow-holes that Mr. Lyles was talking about, how small they would have to be to be consistent with a 60-second leak, and then what kind of clearance between the tang and the clevis that would limit the flow that would be consistent with a 60-second leak.

Now, if you would put up Chart B-3, please.

(Viewgraph B-3.) [Ref. 3 21-82]

MR. BACHTEL: We had two different models we were using. One of them was a two-dimensional model, which is pretty much like what you see on the table here as far as this cutaway. It is one circumferential slice of the clevis and tang. It is between the pins. There is no pin in that particular model, and that model is

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basically a parametric tool. It has fairly good detail thermally of what is going on in here. It is not as complicated as a three-dimension model such that we could turn around the model as rapidly and run some parametrics. It is pretty much appropriate for doing analysis up until when it is we get a failure of the joint.

Now, once we get a failure of the joint, we have to go to a three-dimensional model if we want to assess how the failure progresses in the other dimension. Both of the models, of course, include the heat transfer and thermodynamics that go on inside the joint, the heat transfer outside the joint due to the aerodynamics of flight, which tends to keep the joint cool, the melting of the steel, the recession of the steel, the opening of the gaps, and thus the increase of the flow rate, the ablation and the increase in size of the putty blow-hole with time, the recession of the

O-rings which then allows more flow. And when we do this analysis we do it in two different ways.

One of them is, we run a fairly simplified flow analysis which allows us to do parametrics, and then another method is, we depend upon a separate flow analysis which is a rigorous approach to the flow, a three-dimensional approach which takes into account the

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spreading as you come through the clevis, the interaction of the pins, et cetera.

We take the output from that analysis and feed it into the thermal analysis we are doing, and then finally we are trying to couple those two together, and that is in work currently, and we should be able to report on that in another couple of weeks. If you would go to Chart B-4.

(Viewgraph B-4.) [Ref. 3/21-83]

MR. BACHTEL: This is a detail of the two-dimensional model. The grid work you see on the chart represents the elements that the clevis and the tang are broken into. Within those elements, there are subelements or nodes which allow those elements to become smaller as the metal shrinks, rather, I am sorry, as the metal melts. As the metal melts, then the gap between the clevis and the tang becomes greater and the flow rate becomes greater, and you fairly rapidly cascade the flow rate and the melting process until you go catastrophic, and of course the purpose of the analysis was to find out how small these various gaps either in the NBR, either in the insulation and in the putty coming into the O-ring area here, how small the O-ring clevis clearance had to be or the other clearances to get a 60-second leak.

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Now, if we go to Chart B-5.

(Viewgraph B-5.) [Ref. 3/21-84]

MR. BACHTEL: These are the results of some of our analysis. The box at the top is where we ran some constant flow parametrics to find out what kind of flow rate it would take to get the 60-second leak, and I have got the chart up now. As you can see, it is a fairly small flow rate. It is on the order of .004 to .005 pounds per second. Now, this would be continuous flow rate during that 60 seconds to keep from having either a case failure or an exit gas greater than 3,000 or what have you at 60 seconds.

Now, that .004 pounds per second relates to about 700 cubic inches per second if it was a volumetric flow expanded to the atmospheric pressure. The next thing we did then was look to see how small the blow-holes would have to be to keep the flow rate that low and thus last 60 seconds. The 0-2, which is the smallest one I have on the chart, of course, shows a failure at about 49 seconds. The 0-1 gets us out to 70 seconds. As Mr. Lyles reported, the blow-hole sizes were normally on the order of half an inch, so the minimum or the maximum blow-hole size that I can tolerate to be able to have the joint last for 60 seconds with a continuous leak is almost an order of

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magnitude below what we normally see.

The third box shows the kind of analysis we did for the tang to clevis clearance. Of course, if this closes up enough, it limits the flow, which then allows us to get up to 60 seconds again. The kind of clearance we had to squeeze this down to in order to keep the joint alive for 60 seconds was on the order of .002 inches.

Mr. Ryan showed you earlier this morning where we normally open up to .025 inches, which is almost ten times greater than that. So in almost all cases our analysis shows that if the puff of smoke indicates a leak that was to continue, that we should have destroyed the joint within about 10 to 20 seconds.

So, if we go to the next chart, B-6

(Viewgraph B-6.) [Ref. 3/21-85]

MR. BACHTEL: I will carry this just a little bit further. We have also done some three-dimensional analysis. One of the things that the three-dimensional analysis does is, it picks up conduction circumferentially around which tends to spread the heat out and makes the joint last a little bit longer, and there is a demonstration of that here. When we ran the two-dimensional model on a specific case, we took the joint out at about 24 seconds. The same case on a

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three-dimensional model got us out to 35 seconds, so that gives us a little bit of time.

The box at the bottom shows our best estimate of the flow case. Now, this is a rigorous flow case which is the best estimate of what the blow-hole size would have been, what the O-ring erosion would have been based on some of Garry's work, how the flow would have spread once it comes through the clevis. As the flow goes through the hole in the O-ring, it tends to spread out, which then dissipates, which could possibly let the joint last longer, and again, we are showing on the order of 16 to 20 seconds in this case when the joint should have failed.

So, if you go to Chart B-7.

(Viewgraph B-7.) [Ref. 3/21-86]

MR. BACHTEL: In summary, then, our analysis which we have done to date tends to indicate that the scenario that the leak continued from the puff of smoke all the way out to 50 seconds is probably not the proper scenario. There is still some work that has to be done in this area. For example, one of the things that may limit the flow and which also could give us the other scenario, and that is a stop flow, would be the deposition of aluminum oxide in these gaps, so we are going to start looking at that or we have started

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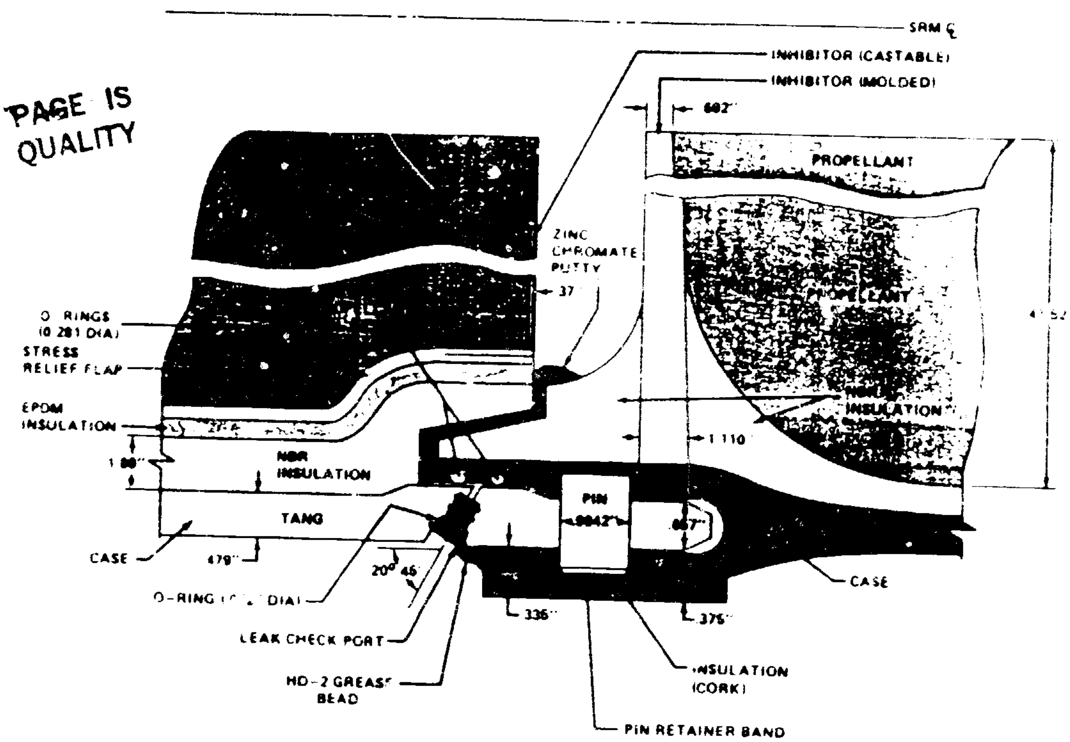
looking at that. We are not prepared to report on it yet. That kind of analysis, as I said, would support both this analysis or this scenario of a continuous leak, an intermittent leak. It may start and stop on the way up, or even a stop at, say, six seconds, and then reissue at 58 seconds. The analysis does show, however, that if we were to reestablish the leak at about 50 to 60 seconds within ten seconds we would expect to go catastrophic, and that concludes what I had to say.

CHAIRMAN ROGERS: Thank you. And that test on the aluminum oxide will be completed in a couple of weeks?

MR. BACHTEL: We are starting to do some analysis. It is a very difficult analysis to do because it involves empirical data. It involves a lot of statistics. It is not an exact analysis. It will only show a probability, and it is going to be based primarily on some test data which we are now acquiring, and we expect to have within a couple of weeks some type of a report on that analysis. Yes, sir.

CHAIRMAN ROGERS: Thank you very much.

AFT SEGMENT/AFT CENTER SEGMENT FIELD JOINT CONFIGURATION



[Ref. 3 21-75]

O-RING EROSION ANALYSIS

MARCH 21, 1986

G. M. LYLES

[Ref. 3 21-76 1 of 2]

EROSION SCENARIO

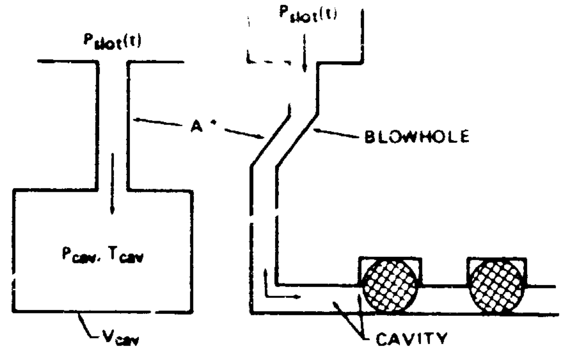
- O GAS IMPINGEMENT EROSION
  - GAS JET PENETRATES VACUUM PUTTY AND IMPINGES ON PRIMARY SEAL
  - SEAL EROSION CONTINUES UNTIL PRESSURE IN CAVITY BETWEEN SEAL AND PUTTY EQUALS MOTOR CHAMBER STATIC PRESSURE
- O BLOW-BY EROSION
  - GAS JET PENETRATES VACUUM PUTTY AND BLOWS BY PRIMARY SEAL
  - PRIMARY SEAL ERODES DUE TO CONVECTIVE HEATING AND SECONDARY SEAL ERODES DUE TO GAS IMPINGEMENT UNTIL PRESSURE OF THE CAVITY BETWEEN PRIMARY AND SECONDARY SEALS EQUALS THE PRIMARY SEAL CAVITY PRESSURE

[Ref. 3-21-76 2 of 2]

ANALYTICAL MODELING OF PRESSURIZATION AND EROSION OF O-RINGS

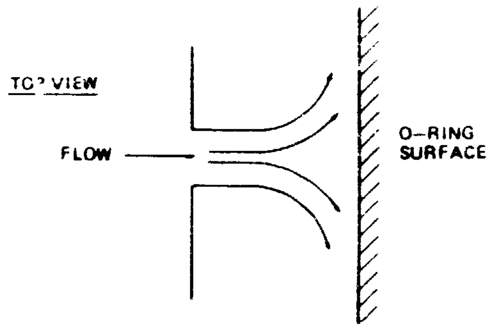
CAVITY PRESSURIZATION MODEL

- LUMPED PARAMETER CAVITY PRESSURIZATION MODEL
- CONSERVATION OF MASS AND ENERGY EQUATIONS SOLVED BY NUMERICAL INTEGRATION



O-RING IMPINGEMENT MODEL

- HOT GAS JET PRESSURIZES O-RING CAVITY THROUGH A PUTTY BLOWHOLE
- JET SPREADS AND IMPINGES ON O-RING CAUSING EROSION BY STAGNATION POINT HEAT TRANSFER

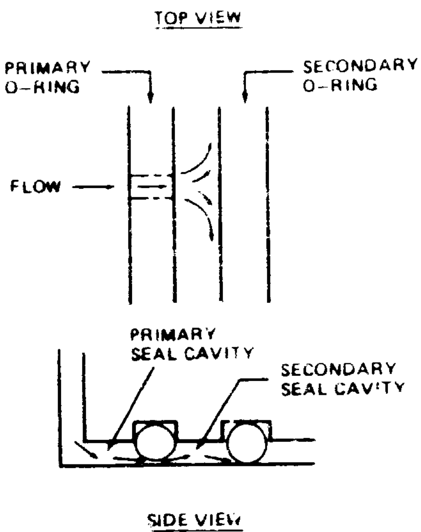


[Ref. 3 21-77]

ANALYTICAL MODELING OF PRESSURIZATION AND EROSION OF O-RINGS (CONT'D)

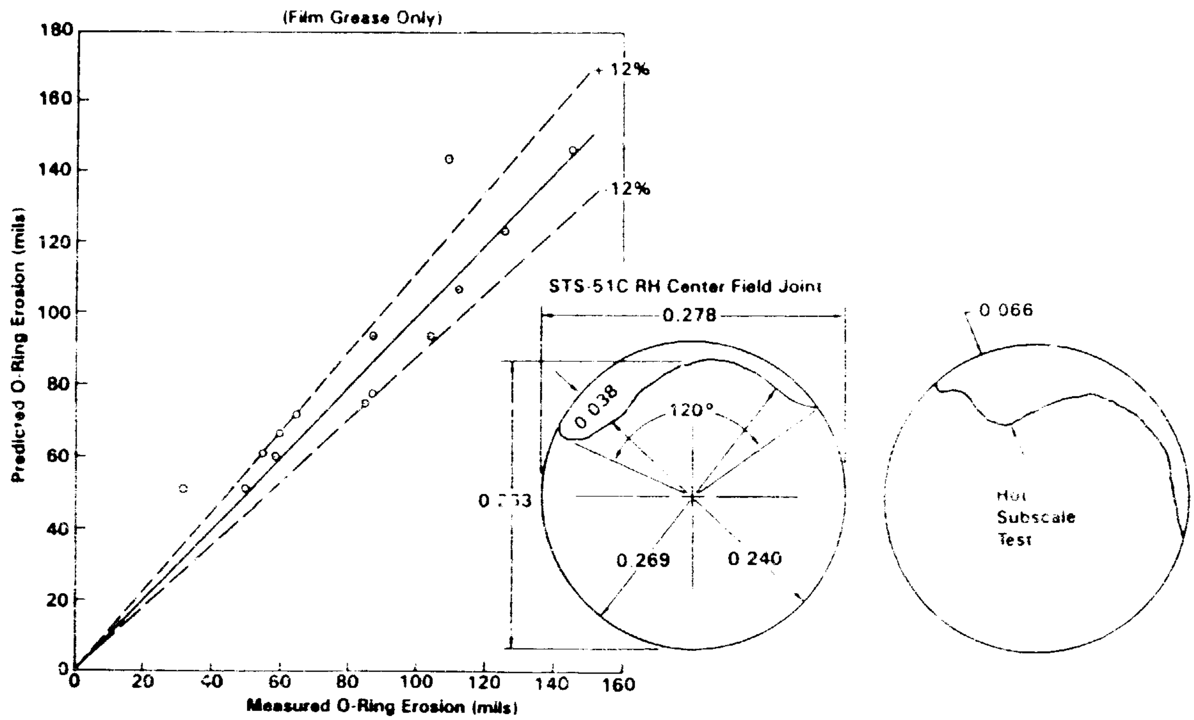
O-RING BLOWBY MODEL

- HOT GAS JET PRESSURIZES SECONDARY O-RING CAVITY THROUGH PRIMARY SEAL BLOWBY
- "PIPE-FLOW" HEAT TRANSFER CAUSES EROSION OF PRIMARY SEAL
- JET SPREADS AND ERODES SECONDARY O-RING



[Ref. 3 21-78]

Simulation of Hot Subscale Data



[Ref. 3 21-79]

APPLICATION TO STS 51-L

- O EVALUATE O-RING FAILURE MECHANISMS DUE TO EROSION

- O EVALUATE O-RING BLOW-BY DURING START TRANSIENT TO CORRELATE BLACK SMOKE WITH O-RING EROSION

[Ref. 3/21-80]

THERMAL ANALYSIS OF SRM FIELD JOINT LEAK

MARCH 21, 1986

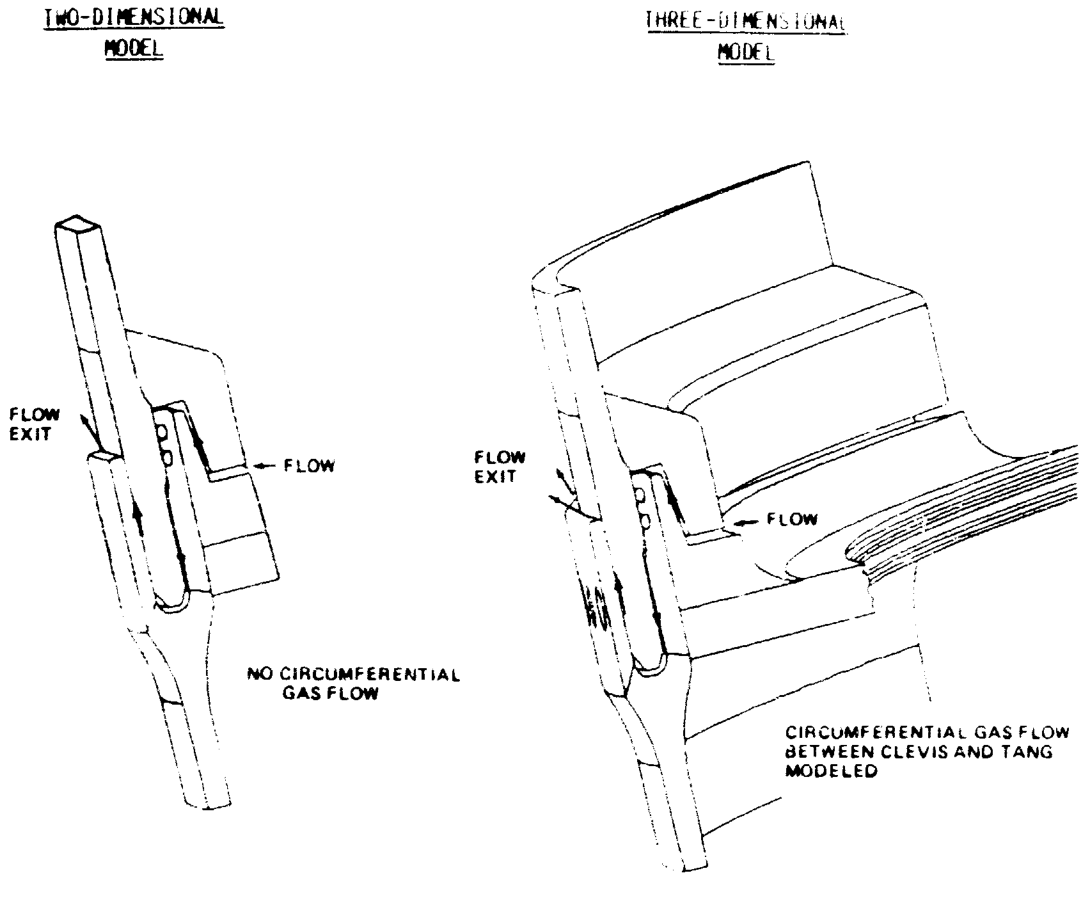
F. D. BACHTEL

[Ref. 3/21-81 1 of 2]

THERMAL ANALYSIS OF SRM FIELD JOINT LEAK

- 0 DETERMINE THERMAL RESPONSE OF THE FIELD JOINT TO A CONTINUOUS LEAK
  - o EXIT GAS TEMPERATURE (3000^{0}F)
  - o EXTERIOR METAL TEMPERATURE (2150^{0}F)
  - o TANG BURN THROUGH OR CASE FAILURE
- 0 ASSESS FLOW AND THERMAL PARAMETERS REQUIRED TO SUSTAIN A JOINT LEAK FOR AT LEAST 60 SECONDS CONSISTENT WITH OBSERVATIONS
  - o LEAK FLOW RATE
  - o PUTTY BLOWHOLE SIZE
  - o TANG TO CLEVIS CLEARANCE

[Ref 3 21-81 2 of 2]



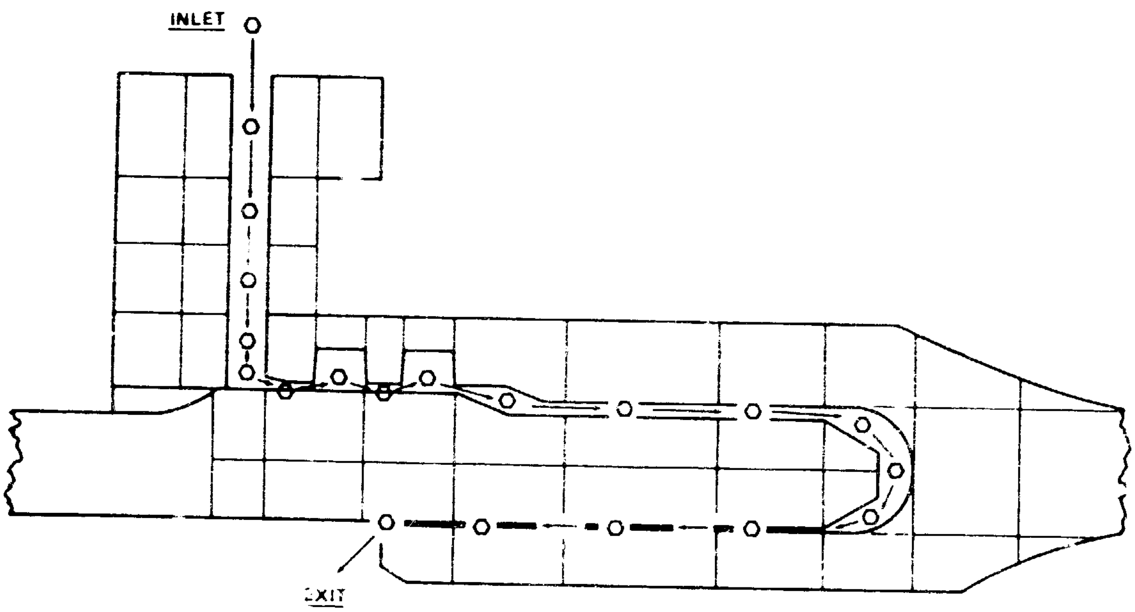
[Ref. 3 21-82 1 of 2]

METHOD

- O TWO-DIMENSIONAL DETAILED THERMAL MODEL
  - o CIRCUMFERENTIAL SLICE BETWEEN THE PINS
  - o ONE-DIMENSIONAL FLOW WITH SPREADING
- O THREE-DIMENSIONAL THERMAL MODEL
  - o 90 DEGREE CIRCUMFERENTIAL SEGMENT WITH PINS
  - o TWO-DIMENSIONAL FLOW
- O BOTH MODELS INCLUDE:
  - o INTERNAL FLOW ENVIRONMENT
  - o EXTERNAL AERODYNAMIC ENVIRONMENT
  - o STEEL MELTING AND RECESSION
  - o NBR/PUTTY ABLATION AND RECESSION
  - o SIMPLIFIED INTERNAL FLOW CALCULATION
  - o FULLY COUPLED THERMAL-FLOW ANALYSIS (IN WORK)

(Ref. 3 21-82 2 of 2)

TWO-DIMENSIONAL THERMAL MODEL



[Ref. 3-21-83]

SUMMARY OF 2-DIMENSIONAL THERMAL ANALYSIS

	TIME GAS @ 3000°F	TIME CASE @ 2150°F	TIME TANG MELTED	TIME CASE FAILURE
CONSTANT FLOW PARAMETRICS				
M = .004	> 70	> 70	> 70	60
M = .006	45	51	70	32
M = .010	16	33	65	15
M = .020	9	15	33	10
BLOWHOLE SIZE STUDY				
HOLE = .02	40	47	52	49
HOLE = .04	17	25	28	26
HOLE = .06	12	19	22	20
HOLE = .10	8	15	18	16
TANG TO CLEVIS CLEARANCE				
GAP = .002	24	34	34	32
GAP = .004	12	19	22	20
GAP = .006	9	17	19	17

M	=	MASS FLOW IN LB/SEC PER INCH OF CIRCUMFERENCE
HOLE	=	INSULATION/PUTTY BLOWHOLE DIAMETER IN INCHES
GAP	=	CLEVIS TO TANG CLEARANCE IN INCHES

[Ref. 3/21-84]

SUMMARY OF 3-DIMENSIONAL THERMAL ANALYSIS

- 3D MODEL INDICATES THAT 3D EFFECTS (i.e., CONDUCTION) ARE IMPORTANT FOR LOW FLOW RATES RELATING TO TIME OF BURN THRU

  EXAMPLE: TIME OF TANG BURN THRU FOR CONSTANT .04 LBM/SEC

2D MODEL	–	24 SECONDS
3D MODEL (2D VERSION)	–	22 SECONDS
3D MODEL (3D VERSION)	–	35 SECONDS

- 3-DIMENSIONAL ANALYSIS OF PREDICTED FLOW PROFILE GENERALLY CONSISTENT WITH CORRESPONDING 2-DIMENSIONAL ANALYSIS

FLOW CASE 3	TIME GAS @ 3000°F	TIME CASE @ 2150°F	TIME TANG MELTED	TIME CASE BURN-THR
2-DIMENSIONAL	18	26	38	16
3-DIMENSIONAL	15	23	43	20

[Ref. 3-21-85]

SUMMARY

- O CURRENT LEVEL OF ANALYSIS DOES NOT PREDICT A CONTINUOUS 60 SECOND FIELD-JOINT LEAK CONSISTENT WITH OBSERVATIONS

  ADDITIONAL MODEL SOPHISTICATIONS REQUIRED TO ADDRESS POTENTIAL FLOW AND HEAT TRANSFER LIMITING MECHANISMS

  - o ALUMINUM OXIDE PARTICULATE DEPOSITION
  - o MELTED STEEL RE-DEPOSITION
  - o INSULATION AND PUTTY DEBRIS BLOCKING
  - o COUPLED THERMAL/FLOW STRUCTURAL MECHANISMS

[Ref. 3 21-86]

DR. LITTLES. I will now conclude, Mr. Chairman, with a summary of the testing which we have under way to try to narrow down some of the remaining

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scenarios. If I could go to Chart W-25, please.

(Viewgraph W-25.) [Ref. 3 21-87]

TESTING TO EVALUATE
SCENARIOS

W. LITTLES
MARCH 21, 1986

[Ref. 3 21-87 1 of 2]

DR. LITTLES: The testing that we have underway relates to these remaining items on this scenario. Items 3, 4A, B, and D, and Item 6. In addition to that, some of the subscale motor testing that we are doing relates to the analysis that Mr Bachtel just discussed relative to the continuous flow or stop-leak flow analysis I will summarize some of those test results for you There is other work that is still going on there And then I will get into a schedule chart and discuss the specific tests related to these scenarios and when we will finish those or when we hope to.

Could I have Chart W-27, please?

(Viewgraph W-27) (Ref. 3/21-88)

DR. LITTLES. This chart depicts one of the two subscale motors that we are using, and this particular one has a full-scale cross-section of the tang and clevis. It is roughly 11 inches in diameter, but you can see if you look at the joint area that it is identical to the design of the case standard size joint design.

It has capability of—or we have the capability of inducing defects with this motor of the O-ring or defects on the ceiling surfaces, scratches,

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and we also have the capability to induce defects in the putty, and we can run the motor over a range of temperatures to evaluate the temperature effect on any kind of defect or parameter that we put in there, and one of the key things we are going to be doing with this motor and the other one is looking at putty performance at various temperatures. The motor simulates the pressure in the actual SRM. We can get up to 1,000 psi.

It operates from 10 to 75 seconds, depending upon how we configure the propellant in there. If we could go to Chart W-28, please

(Viewgraph W-28.) (Ref. 3/21-89)

DR. LITTLES: I just selected a few of the tests here, an initial test to show you what kind of variation we are getting, and these tests are where we have had simulated defects in the motor to see what kind of results we get relative to the continuous flow or stop-leak type flow evaluation.

The first one there is one where we had 6-inch cutouts in the O-ring, very large defects there. We also put a half-inch wide fault in the putty, and we had a .040 inch extrusion gap and burned for 20 seconds. We had a complete burnthrough on that one of the tang near the primary O-ring at 6 seconds, and so it failed very quickly. The second test was

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identical to the first, except here we had only eighth-inch flaws in the O-rings, had an eighth-inch section cut out, and in parentheses there you see that we think we had about one-sixteenth of an inch after we had it actually put together and squeezed, but in this case it burned for 20 seconds without visible leakage, and when they disassembled it, they found soot between the O-rings, and the gap in the primary O-ring which filled with a combustion residue, so the difference between those tests was the magnitude of the defect, but there was a considerable difference in the result.

VICE CHAIRMAN ARMSTRONG: How do you account for that? Would you guess the O-ring grew back together?

DR. LITTLES: Well, if somebody had told me before we had the test that we could have gotten this result, I guess I frankly wouldn't have believed it. What they saw was that in the gap of the O-ring, they said it was filled with a combustion residue. To me it seems unlikely that you could get that with a hot gas flow through there. But indeed it did occur It seems an unlikely result, but we found some other similar results. I will mention another one in a second that is not on this chart On Test 3, again, it was the same as 1 except both O-rings were ground down from their normal diameter

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of .280 down to .200 for about 3/16 of an inch, and in this one, the burnthrough, it burned through the tang in about 12 seconds.

Now, since then, as a matter of fact, a couple of days ago, and I don't have the detailed results yet, but we ran a test similar to three, and the only difference was that we put some instrumentation in this one to get more thermal data. Other than that, as I understand it, the conditions were identical, but this one ran for the full 20 seconds.

It had aluminum oxide deposited in the area of the initial—of the cutback O-ring, of the defect, and it stopped the flow there and moved it around, so there is a considerable amount of variation that you can get with these motors, and I think they are not very predictable relative to what a defect will do relative to progressing the damage, which makes the analysis that Mr. Bachtel talked to you about extremely difficult, if not impossible.

VICE CHAIRMAN ARMSTRONG: Let me ask you to look at this again to make sure I understand it properly. I am comparing test 2 and test 3 And these were essentially identical tests. In test 2 you just completely removed a section of the O-ring that was 1/8 inch across.

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DR. LITTLES: That is correct.

VICE CHAIRMAN ARMSTRONG: And in No. 3, you had a complete O-ring but you just made it a little narrower, for 3/16ths of an inch?

DR. LITTLES: That is right.

VICE CHAIRMAN ARMSTRONG: So one would think on the surface that test 2 would be much more severe than test 3.

DR. LITTLES: I would think so.

VICE CHAIRMAN ARMSTRONG: And yet in test 2, the more severe test, you didn't get any damage. In test 3 you had a complete collapse.

DR. LITTLES: That is correct.

VICE CHAIRMAN ARMSTRONG: So one would draw the conclusion that it is difficult to be able to predict the characteristics of these burnthroughs past an O-ring.

DR. LITTLES: That is correct. That is the conclusion I draw.

CHAIRMAN ROGERS: I guess one of the things that occurs to me in these tests, and maybe you can explain it, weren't tests like these run before the accident? I mean, knowing that the O-rings were such a—or seemed to be such a serious problem, weren't tests of this kind run before the accident?

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DR. LITTLES: Well, these are failure tests that are being conducted here to try to duplicate or to try to understand the condition that we might have had if we had a puff of smoke and then a continuous leak.

CHAIRMAN ROGERS: Well, I guess my question really is broader than that. Were somewhat similar tests run to determine what would happen to O-rings under certain conditions?

DR. LITTLES: No, sir, no tests were run that I am aware of like this. We always knew, I think everyone would agree, that one cannot tolerate the kind of conditions we are setting up here with an O-ring. I don't think there is anyone who would have predicted that you could sustain that kind of defect and have a successful burn of the——

CHAIRMAN ROGERS: That is really not quite my question. My question is, in the beginning, say, of 1985, when concern was expressed about the O-rings, weren't tests run then either by Thiokol or by Marshall to decide some of these concerns, to answer some of the questions that were raised at the time?

DR. LITTLES: The analysis and the supporting tests that Mr. Lyles discussed were ongoing at that time. As a matter of fact, they had been started a number of months earlier. Those analyses and the

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supporting tests that confirmed that analysis were used to evaluate the degree of erosion that one might expect in a worst-worst case on the primary, and so those kinds of tests and analysis were conducted relative to the phenomenon that we had observed which was impingement erosion of O-rings.

CHAIRMAN ROGERS: Do you or does Thiokol to your knowledge have any history that has been prepared about O-rings and their failure? The reason I ask that is because in 1982 it was known that the O-rings were a problem, and the criticality was changed from criticality 1R to criticality 1.

I would have thought there would have been a lot of tests to determine reliability of the O-rings during that three-year period, and I would have thought that there would be a history of the O-rings and how much you could rely on them, and so forth. To your knowledge, are there any such reports or any history of the O-rings that has been prepared?

DR. LITTLES: Well, there was a history of the O-rings relative to the erosion that had been seen in flight. Those results were available on every occasion when we had erosion in flight, whether it was on a field joint or on a nozzle joint. It was looked at, that specific case was looked at. Those results were

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discussed, and the flight readiness reviews for the subsequent flights and were reported all the way up through channels, and that history was maintained on a continuing basis.

So, yes, sir, that history was available.

CHAIRMAN ROGERS: Well, the Commission would want to get all of that information before we make our report. As I remember some of the original testimony, though, by some of the people who made the decision to launch was that they were not familiar with the problem with the O-rings at all.

DR. LITTLES: Well, I don't recall that testimony, but I do know based on personal experience that those anomalies that have been experienced in flight have been reviewed in flight readiness reviews.

CHAIRMAN ROGERS: So it is your opinion that that concern was generally known both at Marshall and Thiokol among people at the top of the agency?

DR. LITTLES: Yes, sir. I can't imagine that it was not known because it has been, I know, covered in flight readiness reviews. We always discuss any anomaly that we have. We evaluate it relative to its potential consequences on the subsequent flight, and we discussed it in flight readiness reviews. And so I am not familiar with the testimony you refer to, but I personally

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couldn't imagine anyone not being aware of a flight anomaly.

CHAIRMAN ROGERS: Well, of course, the astronauts say they were not aware of it. You have seen that testimony, I guess.

DR. LITTLES: Yes, sir. I have seen that. I am not personally familiar with their participation in flight readiness reviews. If someone had asked me, I would have said I thought that they were probably represented, but I don't know that, but I do know for a fact that they have been discussed in the level 2 flight readiness reviews where I would have expected at least a crew representative to have been present. So I know they have been discussed.

CHAIRMAN ROGERS: So I assume then when you heard statements by the astronauts that they did not know about the problem in the flights where they were involved, that surprised you.

DR. LITTLES: Well, like I say, I am not familiar with their direct participation in flight readiness reviews, and so I don't know what their normal communication channels are with those who do participate.

CHAIRMAN ROGERS: Well, let me ask the question more directly. Were you surprised or are you

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surprised now when you learn that a commander of one of the space shuttles was not told after the fact what had happened in connection with the O-rings on his particular flight? Did that surprise you, or does it surprise you today?

DR. LITTLES: It would surprise me. As a matter of fact, I couldn't believe that that incident had not been covered in the subsequent flight readiness review. Now, again, and I would be surprised if anyone who had been involved in that flight didn't somehow find out about it, but again, I don't know where the communication channels are. I know it was covered in the flight readiness review, but what that commander's opportunity for getting that information might be, I am not familiar with.

CHAIRMAN ROGERS: Okay. We will come to that later on in the investigation. Thank you.

MR. LYLES: Mr. Chairman, can I make a statement for the record? I would like to clarify something that I said before.

CHAIRMAN ROGERS: Surely.

MR. LYLES: I told you that I was not aware of the criticality 1 issue. That is a true statement. However, I think all of us knew that if the secondary O-ring leaked, that it was a bad situation.

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CHAIRMAN ROGERS: Excuse me?

MR. LYLES: If the secondary O-ring leaked, that was a bad situation.

CHAIRMAN ROGERS: The criticality 1 that I am talking about said that if the primary O-ring fails, you can't rely on the secondary O-ring.

MR. LYLES: I was not aware of that criticality. I was aware that the effort that was going on, that Thiokol was going through a redesign effort on the field joint, and we were looking at different O-rings and things like that, but as far as the specifics of all that, we were working the erosion problem.

CHAIRMAN ROGERS: I see. Thank you.

DR. LITTLES: Could we have Chart W-30, please?

(Viewgraph W-30.) [Ref. 3/21-90]

DR. LITTLES: This chart depicts the second subscale motor that we are using. This is a five-inch motor. It is a little smaller than the other one. It can burn between three and 24 seconds. We can get up to 800 psi pressure in this one, and run for 24 seconds. Again, we can vary the temperature over a wide range, and again, we can simulate degraded O-rings and sealing surfaces and putty. Would you focus in on the bottom

2345

right, please? You will see that this one does not simulate the actual joint, so it is not quite as good in that respect as the other motor, but you can get a lot of good, qualitative data with this motor. Could we see Chart W-31, please?

(Viewgraph W-31.) [Ref. 3/21-91]

DR. LITTLES: I will summarize again just a few of the tests that have been run with this motor, and you will see the same kind of trends as with the other motor, a lot of variability. Test 3 had an eighth-inch section of O-ring missing, no putty in this one. It had a 0.03 gap and burned for 3 seconds. And it had smoke at ignition with flame at 1.2 seconds, and the O-ring was mostly consumed, and heavy metal damage on that one.

The next test, test 4, was the same as 3, except there was a 0.004 gap rather than a 0.03, and it burned for 24 seconds. This one we had continuous smoke but no fire, no O-ring damage at the cut, O-ring erosion and heat effect away from the cut, but no metal damage. So the primary variable there was just the gap, and there was a considerable difference in performance.

Could I see W-32, please?

(Viewgraph W-32.) [Ref. 3/21-92]

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DR. LITTLES: Test 6, which is the first one on this sheet, we had a half-inch vent in the putty at this time with a scratch on the sealing surface. The O-ring wasn't damaged, but just a scratch on the sealing surface. It burned for 24 seconds with a 0.019 gap at 70 degrees. It had a small plume immediately on this one, at the defect, and it shut off and then reappeared 90 degrees away from that original defect, and there was considerable damage on this one. It had molten metal and very heavy damage.

Test 7, again, was the same as 6 except here we had the putty intact. There was no vent or defect in the putty, and we cooled this one down to 30 degrees, and it had two scratches on the surface. Now, this one, nothing happened at all for ten seconds, and then we got some flow for five seconds, and then it stopped. The significant item out of this test, at least to me, is that the putty appeared to hold that pressure off of the O-ring for ten seconds, and that is very significant, of course, because you need to get the pressure on the O-ring during the ignition transient, particularly with the joint rotation situation that we talked about.

DR. COVERT: Dr. Littles, I think that these are very interesting results, and I wonder if you in your test matrix intend to repeat these several times,

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in other words, take the configuration exactly the same, for example, as Test 7, and then just refire it, and then build a new fixture, and refire it.

DR. LITTLES: Yes, sir, we are going to do that. You see so much variability with the same general type test. I think you have to run several of the same type to see what really can occur, and we are going to do that. That will probably be a little later, though, because one of the key things we are going to be using these motors for in the next few days is to support failure evaluations for the remaining scenarios. We are going to concentrate on that, and then we will do some more tests relative to the leak stop leak type thing.

DR. COVERT: I would like to pursue this just a little bit further. I wasn't particularly interested in, although I think it is important, the leak stop leak, but in all of these tests the level of variability is such that it suggests to me that if you are going to base any serious conclusions on it, you might want to have some repeat points.

DR. LITTLES: I agree. I certainly do. And even more specifically, I guess, even more importantly for those there we are verifying scenarios, we will have to run a number of tests on those in those areas where we find results that are important to us and might

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support conclusions one way or the other. I agree with you. We have to run a number of tests. That will be very important.

DR. WALKER: Can we just return to a point that I think you made? At 30 degrees evidently the putty becomes quite stiff and can withstand considerable pressure. Is that the point you were making?

DR. LITTLES: Well, that is a point I was making. I was going to continue with that, though. We are running some additional tests at higher temperatures in these motors. I had hoped to have a higher temperature test or two by this time, but I haven't gotten it yet. But one of the things that we have found from the putty tests we are doing, we are doing tests with putty over a range of humidities and temperatures and defects and all these kinds of things.

And we haven't completed that by a substantial amount, but one of the things we are learning out of that is that the putty is temperature-sensitive. It will hold pressure at lower temperatures for longer periods of time, but even at ambient conditions, 70 degrees, the putty still will hold pressure off for long periods of time relative to the half-second of the ignition transient. So, yes, it is temperature sensitive, but even at ambient temperature it will hold

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pressure for long periods of time relative to the time of interest that we are looking at.

Now, those tests, there is still some work that has to be done in that, because those initial tests were conducted in a fixture which did not incorporate the dynamics of the joint. In other words, the joint was held fixed, and actually you get some motion of the insulation around the putty, and we have a test fixture which has just started into test, and we will be getting a lot of results out of that, we hope, in the next week to 10 days which incorporate that feature.

DR. WALKER: Now, it was my understanding from discussions at Morton Thiokol that the way in which the joint was to operate was for the putty to transmit the pressure pulse to the O-ring in order to seat it, and these results would suggest then that that theory of the operation of the joint may have been faulty.

DR. LITTLES: That is absolutely correct. That is correct. And that is the hypothesis of scenario 6, that that concept of the joint operation which people had believed was correct is not in fact correct.

GENERAL KUTYNA: Dr. Littles, this one interests me. You are interested in the putty, but I am interested in the leak-stop-leak. You had a leak and

2350

then it sealed itself, and this is one of the few that it has done that on. How did it seal itself? What was the mechanism? What was the structure of the material in there?

DR. LITTLES: I am not exactly sure how that sealed. I asked the other day somebody to go check on that. I am not sure whether it sealed because of some internal mechanism or whether the putty itself sealed. And we have run some tests, some lab tests in configurations that don't simulate this joint, and we are going to have to do some more, where we have seen the putty have a blow-hole breakthrough, and then seal itself. So, it could be that the putty sealed itself here, or it could be something internally.

GENERAL KUTYNA: This is, of course, very similar to the actual shuttle launch, in that we had a leak and possibly it sealed itself. I had heard at Marshall that in some of these cases the material that sealed the leak was kind of a glassy substance rather than something very tough, and if it were glassy and brittle, it might be something that could break off later in flight—break open later in flight after you had loads imposed on it.

DR. LITTLES: That is correct.

GENERAL KUTYNA: Do you have any of that experience to report?

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DR. LITTLES: No, we don't have anything that simulates that.

GENERAL KUTYNA: Are you aware of that glassy substance, though?

DR. LITTLES: Yes. I would not be at all surprised that that could happen either with the loads that we were putting on it at that point in flight or, you know, we were, of course, beginning to increase in motor pressure. At about that time the pressure was going back up, which imposes a little additional opening on that joint, and so there were things going on which, with a damaged seal, and possibly having some deposition like you referred to could have caused it to open up.

GENERAL KUTYNA: Thank you.

DR. LITTLES: Chart W-33, please.

(Viewgraph W-33.) [Ref. 3/21-93]

DR. LITTLES: This is just a summary of observations, and I think we have touched on most of them as I have gone through. There is more work, as I said, to be done in this area, but my conclusion, I guess, at this point in time, it is obvious, is that it is a highly variable phenomenon which I don't believe is amenable, frankly, to a very detailed analysis. There are just too many variables to try to do it, and I think it is possible based upon what we have seen that you

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could have had a leak that stopped and started some time later. I think at this point in time it is highly unlikely based upon the analysis that Rick Bachtel reported that you had a continuous leak at one location for that long. I think that is highly unlikely.

Chart W-34, please.

(Viewgraph W-34.) [Ref. 3/21-94]

DR. LITTLES: Okay, we have a number of tests that are going on relative to the other elements of the joint scenario. The first item on the chart there is testing that we are doing relative to the assembly, potential assembly damage that we have discussed before. We have a test fixture at Marshall which is a partial joint tang/clevis. It is a segment about two and a half feet wide, and we have it in a fixture, and we are using that to simulate various off-tolerance situations in the mating to look at potential damage of the tang and clevis or the O-ring, and depending upon the results of that, and we may go further and do some full-scale short attack tests on that.

Item 2 deals with scenario 4-A.

MR. ACHESON: May I ask a question about 1? I see that 61G is to be destacked in April.

DR. LITTLES: Oh, yes. I failed to point that out.

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MR. ACHESON: Is there any reason to believe that 61-G experienced particular assembly problems, out of roundness or difficulty in stacking?

DR. LITTLES: 61G, as you might recall from our last discussion, 51L had a negative dimension which puts the tang back over the O-ring section of the clevis of .393, I believe it was. 61G is not a direct analog of that. It had a negative dimension, I believe, of .274, but there are some reports that there was some difficulty in making that mate, and we think it would be very informative to take it apart and see what the condition is, but it is not a direct analog of 51L.

MR. ACHESON: Thank you.

DR. LITTLES: So then, back to Item 2, the seal test relative to a defective seal, and this relates primarily to the closeout photo that we have discussed. We are still doing some work on that to try to determine whether what we see there is indeed a defect. We have a test fixture that we put together to do that with, and to take some photographs and see if we can simulate that, depending upon the results of that, we might go to our dynamic test fixture which I will discuss in a minute, or maybe even to some hotfired tests to close that out.

Item 3 is testing related to ice in the

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joint. We have a visual fixture that has been built to look at what might happen when you get ice up near the second O-ring or ice and grease above it in a column to potentially unseat that O-ring. We are doing tests on that. Depending upon what comes out of that, we will probably do some tests in some of the other fixtures possibly in the joint motor, the motor that has the joint simulated very well, test 104, and probably even in the dynamic test fixture.

Could we see Chart W-35, please?

(Viewgraph W-35.) [Ref. 3 21-95]

DR. LITTLES: The first item on this chart is the relative to scenario 4D. This is the scenario which deals with the situation that was discussed on the Monday night before the flight, where you have cold situation, you have O-rings which have degraded resiliency, and you have the joint rotation. We have this test fixture in tests now. We have gotten the first series of tests back. We ran the first series of tests with the minimum squeeze that we had on the 51L conditions, and we ran it with a gap opening of 0.020. In other words, we had it set at the minimum squeeze, and then what we do with this test fixture is that we open that gap at a rate which simulates the gap opening on the actual motor tang/clevis during flight,

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and of course it is simultaneously—you have the pressure that you see during the ignition transient imposed on it. Those first series of tests, there were seven run at a range of temperatures. There were two run at 75 degrees, one at 40, two at 25 degrees, one at ten degrees, and one at minus ten, and in all cases the seal performed properly except at minus ten degrees, and in that case it did not, but down through ten degrees it did perform properly.

Now, we are now going into another series of tests with that fixture where we are going to impose the maximum squeeze condition. In other words, in looking at the way this joint is assembled and the tolerances, it looks as though you can have a situation where you have, rather than—you can have a situation where you have almost metal to metal contact, and to us that seems to be a worse situation relative to the response of the joint than the minimum squeeze, and so we are going into that test series now. We will set that up with the maximum squeeze and then run through the full joint rotation and we expect to have those tests probably some time next week, so that is a very interesting set of tests to us.

DR. KEEL: Dr. Littles, could I just ask a question there for clarification? You say you are

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simulating the gap opening due to rotation, but you aren't simulating, as I understand it, the gap opening due to this going back from out of round to round, if you will, based upon this load analysis you have done.

DR. LITTLES: That is correct. It doesn't do that, because it only moves in one direction, and doesn't have that in it, but again, I think the worst condition is going to be where you have the maximum squeeze, and in that case it is going to be going in one direction anyway. The effect of the rounding situation where you have the maximum squeeze is much less than it is where you have the minimum squeeze. If you have the minimum squeeze, what you actually get during the rounding process is that when that takes place in the neighborhood of 50 to 150 psi, you actually get more squeeze put on that O-ring, and it continues to open up, but you are right. That feature is not there.

The next item is Item 5, which are the putty tests. I have mentioned these already. We have that second fixture ready now. It is in tests. We are conducting a range of parameters there, temperature and relative humidity. And again, we expect to complete that by the end of the month and have the data evaluated by the end of the first week in April.

Item 6 is also—

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DR. WALKER: One question on Item 5. Are there putty tests both at Marshall and at Thiokol? Or just one place or the other?

DR. LITTLES: Well, the primary tests are being conducted at Thiokol. That is where we have this more high fidelity feature. We had conducted some tests at Marshall on very simplified lab rigs. We have done some work there, but the predominant work is being done at Thiokol. Item 6 is another very important series of tests for us. We have a test fixture for this which we can use to simulate the dynamic situation that I discussed under Item 4D.

But it can also be used to simulate the situation that you get where the putty holds the pressure off of the O-ring. The gap or the joint opens, the gap opens, and then you get a pressure through the putty, and you hit the O-rings while you already have a gap underneath them, and that is the situation that we will be simulating with this test fixture, and that test fixture is now available, and we should be testing it next week, so in the next week to ten days we hope to get a significant amount of data relative to these prime scenarios, and that concludes my presentation, Mr. Chairman.

VICE CHAIRMAN ARMSTRONG: Mr. Chairman, I

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think these reports of test results are a substantial contribution to the Commission's investigation. And I would like to ask Dr. Littles a couple of questions by way of summary. I think the time line we have seen is a substantial improvement in accuracy and understandabilty, and my understanding of the information you have presented today is that there have in the past been a lot of reports, both in the popular press and the trade press and so on of potential external tank leaks, and being contributory to the event, and I understand that your analysis to date indicates that in fact although you have still some people that are reviewing the conclusions that you have drawn, that at this point in time you have no evidence to indicate that leakage to the external tank was contributory first or even existent. Would that be a fair conclusion?

DR. LITTLES: That is a good summary of that, yes.

VICE CHAIRMAN ARMSTRONG: Secondly, there have been a variety of speculations regarding potential loads, both unusual loads either at liftoff or at the combination of Max Q and wind sheer events, and a detailed look at the load profiles up to this time indicates that those are in fact in the normal range.

However, if there were earlier alternate failures, it is possible that loads could have contributed to the subsequent breakup, that normal loads would have contributed to the subsequent break-up?

DR. LITTLES: Yes. I think that is a possibility.

VICE CHAIRMAN ARMSTRONG. And thirdly, you find no evidence to indicate that the leak check port had any applicability to the failure?

DR. LITTLES: No, we don't think it did. I think the evidence indicates that it did not emanate at that point.

VICE CHAIRMAN ARMSTRONG: And now I am going out on a limb a little bit with the last presentations. We had earlier, some had earlier thought that the prediction of O-ring erosion was perhaps impossible or an unpredictable kind of event, and I think Mr. Lyles' presentation indicates that in fact there is a certain predictability to O-ring erosion, given the proper initial conditions and characteristics of the flow. But at the same time the tests that you have done in your latter presentations indicate that in fact if there are abnormalities present of one sort or another, then the way in which a joint might fail are quite variable and even unpredictable.

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DR. LITTLES: I think that is true. I think that the work that was done, the analysis that was done to develop the O-ring erosion model and the test data did a good job of bounding that problem, but when you get the situation, the situations that we are simulating in these tests where you have defects that allow blow-by and don't give the joint or the O-ring an opportunity to seal, I think it is a highly variable situation in that case, and very difficult to predict.

VICE CHAIRMAN ARMSTRONG: Thank you. That is all I have, Mr. Chairman.

CHAIRMAN ROGERS: Just one other. I assume that the conclusion you reached last time is still valid, and that is the joint still seems to be the area of most suspicion, the one you are most concerned about. Is that correct?

DR. LITTLES: That is correct. I believe we have eliminated all of the other possibilities except these items we have just discussed here relative to the tests, and those all deal with the joint.

CHAIRMAN ROGERS: Thank you very much. I think there has been real progress since the last session. Thank you. There may be some other questions. If not, thank you very much for a very good presentation.

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(Whereupon, at 3:25 p.m., the hearing was adjourned.)

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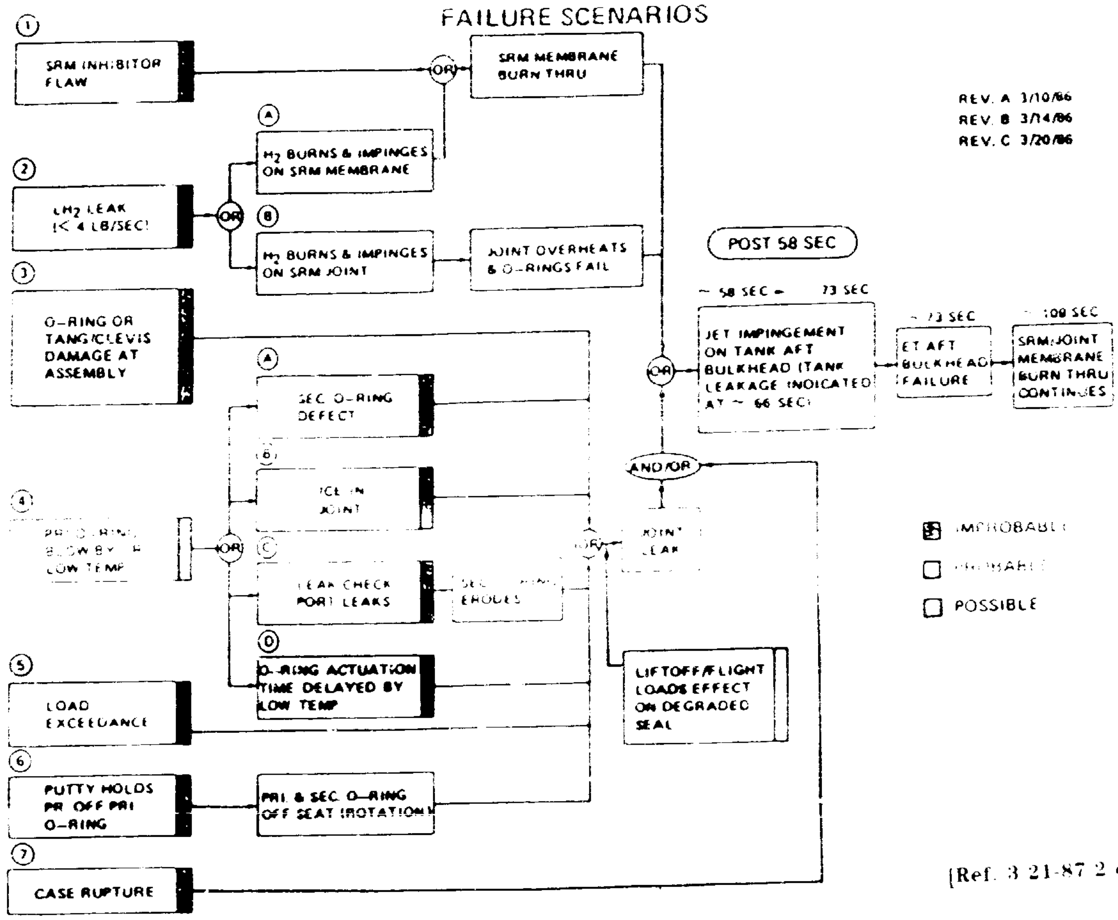
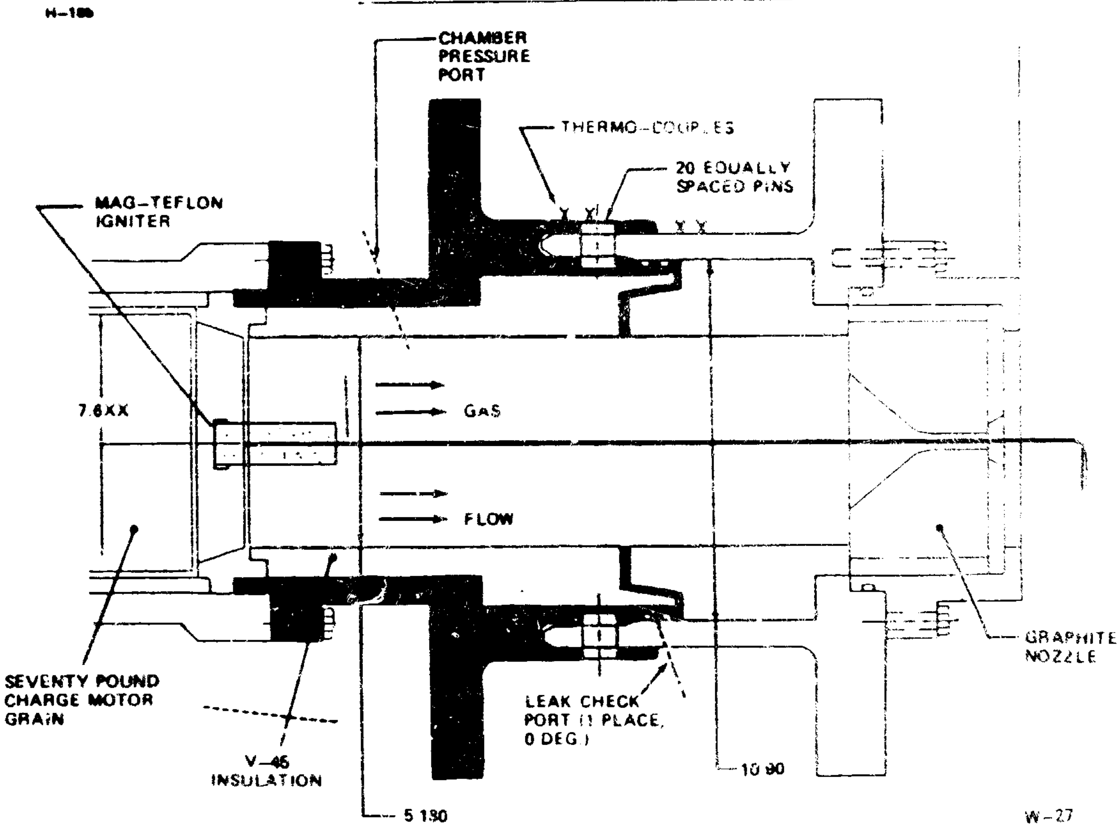
SRM HOT GAS LEAK
FAILURE SCENARIOS



[Ref. 3 21-87 2 of 2]

FULL SCALE JOINT CROSS SECTION MOTOR



[Ref. 3/21-88]

STATUS

0 3 TESTS TO DATE:

TEST 1 - 6" CUTOUTS BOTH O-RINGS, 1/2" WIDE PUTTY FAULT ALIGNED WITH O-RING FAULTS. .040" EXTRUSION GAP, 20 SECOND BURN, 70°F.

- BURNTHROUGH OF TANG NEAR PRIMARY O-RING AT 6 SEC.

TEST 2 - SAME AS TEST 1 EXCEPT 1/8" CUTOUTS BOTH O-RINGS (ABOUT 1/16" AFTER ASSEMBLY).

- BURNED 20 SEC. WITHOUT VISIBLE LEAKAGE/SOOT BETWEEN O-RINGS/GAP IN PRIMARY O-RING FILLED WITH COMBUSTION RESIDUE.

TEST 3 - SAME AS TEST 1 EXCEPT BOTH O-RINGS GROUND DOWN FROM .280 TO .200 DIAMETER FOR ABOUT 3/16" LENGTH.

- BURNTHROUGH OF TANG NEAR FAULT IN PRIMARY O-RING AT 12 SEC.

W-28

[Ref. 3/21-89]

5 INCH SUBSCALE MOTOR

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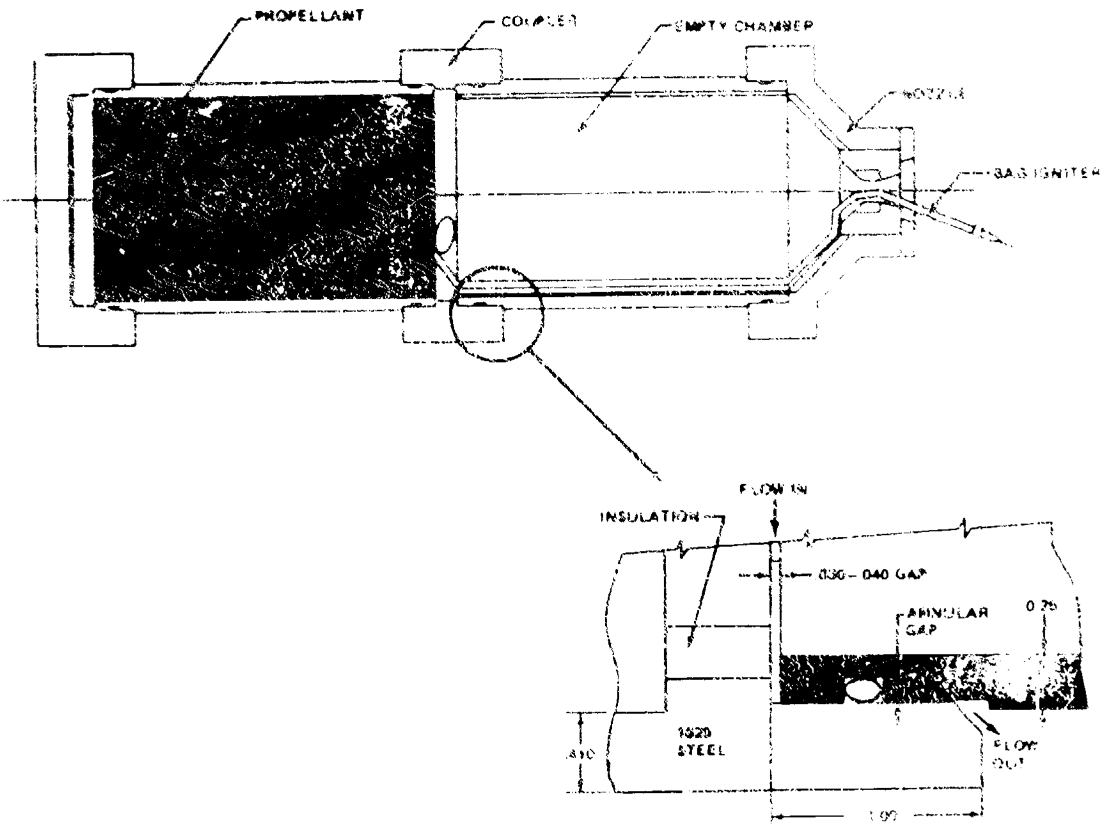

(Ref. 3-21-90)

SUMMARY OF SELECTED TESTS

TEST 3 - 1/8" SECTION OF O-RING MISSING, NO PUTTY, 0.030" GAP, 3 SEC. BURN, 70^{0}F

- SMOKE AT IGNITION WITH FLAME AT 1.2 SEC., O-RING MOSTLY CONSUMED, METAL DAMAGE

TEST 4 - SAME AS 3 EXCEPT, 0.004" GAP, 24 SEC. BURN

- CONTINUOUS SMOKE/NO FIRE, NO O-RING DAMAGE AT CUT, O-RING EROSION/HEAT EFFECT AWAY FROM CUT, NO METAL DAMAGE

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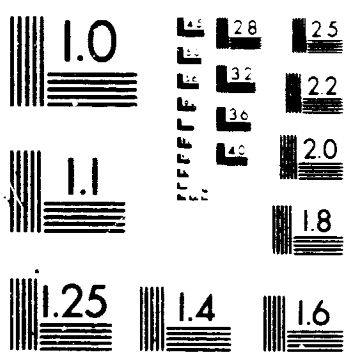

MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS
STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

SUMMARY OF SELECTED TESTS (CONT'D)

TEST 6 - PUTTY WITH 1/2" VENT, V-SHAPED SCRATCH ON SEALING SURFACE 0.020" X 0.020", 24 SECOND BURN, 0.019" GAP, 70^{0}F

- SMALL PLUME IMMEDIATELY AT DEFECT/SHUTOFF AND THEN REAPPEARED 90^{0} FROM DEFECT, MOLTEN METAL FROM JOINT, SIGNIFICANT METAL DAMAGE

TEST 7 - SAME AS 6 EXCEPT, NO VENT IN PUTTY, 30^{0}F, 2 SCRATCHES

- SMOKE APPEARED AFTER 10 SECONDS, FLOWS FOR 5-6 SECONDS THEN CEASES

[Rev. 3 21-92]

OBSERVATIONS

- 0 VISUAL FLAMES SIGNIFY HARDWARE DAMAGE
- 0 METAL-TO-METAL GAP SIZE IS A SIGNIFICANT VARIABLE
- 0 PUTTY CAN DELAY PRESSURE TO O-RING FOR SECONDS
- 0 MOTOR CAN PRODUCE SMOKE FOR MANY SECONDS WITHOUT A BURNTHROUGH
- 0 FLOW CAN BE STOPPED AT AN INITIAL DEFECT AND REAPPEAR ELSEWHERE

[Ref. 3 21-93]

H-211

51-L FAILURE ANALYSIS - TEST ACTIVITY SUMMARY

TESTS	FEBRUARY / MARCH / APRIL (weeks 1 2 3 4)
① JOINT DAMAGE AT ASSEMBLY (SCENARIO 3)	DES/FAB/C.O. — TESTING — DATA EVAL POTENTIAL FOR SEAL/JOINT DAMAGE AT ASSEMBLY PARAMETERS – 51-L STACKING DIMENSIONS – PERFORMANCE OF DAMAGED SEALS (LEAK CHECK, COLD GAS, HOT GAS) 61-G DESTACKING: PREP — LEFT SRB
② SEAL TESTS (DEFECTIVE SEAL) (SCENARIO 4A)	DES/FAB/C.O. — TESTING — DATA EVAL DEFECTIVE SEAL PERFORMANCE PARAMETERS – CLOSE-OUT PHOTO "DEFECT" CHARACTERISTICS – LEAK CHECK CONDITIONS – TEMPERATURE – JOINT DYNAMICS – MOTOR IGNITION PRESSURE TRANSIENT
③ SEAL TESTS (ICE IN JOINT) (SCENARIO 4B)	DES/FAB/C.O. — TESTING — DATA EVAL SEAL PERFORMANCE WITH ICE IN THE JOINT PARAMETERS – SEAL DISTURBANCE CHARACTERISTICS – JOINT DYNAMICS – MOTOR IGNITION PRESSURE TRANSIENT

[Ref. 3/21-94]

H-211a

51-L FAILURE ANALYSIS - TEST ACTIVITY SUMMARY (CONT'D)

TESTS	FEBRUARY / MARCH / APRIL (weeks 1 2 3 4)
④ DYNAMIC TESTS (SCENARIO 4D)	DES/FAB/C.O. — TESTING — DATA EVAL SEAL PERFORMANCE WITH JOINT ROTATION PARAMETERS – VARYING INITIAL GAP (ASSEMBLY) – TEMPERATURE – PRESSURIZE PER MOTOR IGNITION TRANSIENT
⑤ PUTTY TESTS (SCENARIO 6)	DES/FAB/C.O. — TESTING — DATA EVAL. PUTTY PERFORMANCE IN THE SEALING PROCESS PARAMETERS – LAY-UP – RELATIVE HUMIDITY – TEMPERATURE – MANUFACTURING TOLERANCES (GAP)
⑥ SEAL TESTS – PRESSURE DELAY (SCENARIO 6)	DES/FAB/C.O. — TESTING — DATA EVAL. SEALING CAPABILITY DUE TO PRESSURE DELAY/JOINT ROTATION PARAMETERS – JOINT ROTATION – TEMPERATURE – INITIAL GAP (ASSEMBLY)

[Ref. 3/21-95]

PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—THURSDAY, APRIL 3, 1986

Dean Acheson Auditorium
Department of State
23rd & C Streets, N.W.
Washington, D.C.
The Commission convened at 9:40 a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE, Commissioner
DR. ALBERT WHEELON, Commissioner
MR. ROBERT RUMMEL, Commissioner
DR. EUGENE COVERT, Commissioner
MR. DAVID C. ACHESON, Commissioner
MAJOR GENERAL DONALD KUTYNA, Commissioner
MR. ROBERT HOTZ, Commissioner
DR. RICHARD FEYNMAN, Commissioner
ALSO PRESENT:
DR. ALTON G. KEEL

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PROCEEDINGS

CHAIRMAN ROGERS: The Commission will come to order, please. Today the Commission will hear presentations by representatives of NASA's Johnson Space Center. We are interested in operational aspects of the Space Shuttle System with particular emphasis on the methods by which technical and safety concerns are considered.

It might be well to refer again now to the mandate given to this Commission by the President. It is: one, to review the circumstances surrounding the accident, to establish the probable cause or causes of the accident; and two, to develop recommendations for corrective or other action based upon the Commission's findings and determinations.

The matters we will discuss today may not directly relate to the cause or causes of the Challenger accident: however, they do relate to the second part of the Commission's mandate, to make recommendations on how to make future flights safer.

It is in that connection that the Commission is giving careful attention to concerns expressed by astronauts because the Space Shuttle Program will only succeed in the future if the competent and highly qualified men and women who fly the Shuttle have confidence in the system.

One other point before we get started. The agenda for today includes a presentation by the Office of Flight Crew Operations. Dr. Ride, who works in that office, feels that it would be more appropriate for her not to take part in the questioning of witnesses from that office, and of course the Commission agrees.

Now we will proceed, Dr. Keel, with the witnesses.

DR. KEEL: Mr. Abbey, Mr. Young, Mr. Weitz, Mr. Crippen, and Mr. Hartsfield, please

(Witnesses sworn.)

TESTIMONY OF GEORGE ABBEY, DIRECTOR OF FLIGHT CREW OPERATIONS, JOHN YOUNG, CHIEF, ASTRONAUT OFFICE, P. J. WEITZ, DEPUTY CHIEF, ASTRONAUT OFFICE, ROBERT CRIPPEN, ASTRONAUT, HENRY HARTSFIELD, ASTRONAUT

CHAIRMAN ROGERS: Gentlemen, we would appreciate it if you would identify yourselves and give a little background about each of you with particular reference to the astronaut program and any flights that you have taken part in.

MR. ABBEY: I'm George Abbey, the Director of

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Flight Crew Operations. I'm a graduate of the U.S. Naval Academy. I served in the Air Force as a pilot for 13 years and received a master's degree in electrical engineering and subsequently was assigned to various management and technical positions since that time. I was assigned to NASA in 1964; assistant to the Manager of the Apollo Spacecraft Program Office, Assistant to the Director of the Johnson Space Center, Dr. Gilruth and Dr. Kraft, and then Director of Flight Operations and more recently Director of Flight Crew Operations.

CHAIRMAN ROGERS: Thank you, Mr. Abbey.

Mr. Young, would you want to move the microphone over a little bit in front of you? Thank you.

MR. YOUNG: I'm John Young, Chief of the Astronaut Office. I've been working in the Navy from 1952. I went to Georgia Tech, bachelor of aeronautical engineering. I've been working for NASA since 1962 and been working on the Space Shuttle since December of 1972, and been Chief of the Astronaut Office since about 1975.

CHAIRMAN ROGERS: Which flights did you take part in?

MR. YOUNG: Gemini III, Gemini X, Apollo X,

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Apollo XVI and STS-1 and 9.

MR. WEITZ: I'm Paul Weitz. I was a Naval aviator when selected as an astronaut in 1966. I have been in Houston since then. I'm Deputy Chief of the Astronaut Office. I flew on the first Skylab flight in 1973 and on STS-6, the first flight of Challenger in 1983.

CHAIRMAN ROGERS: You're Deputy to Mr. Abbey?

MR. WEITZ: No, sir, to Mr. Young.

CHAIRMAN ROGERS: To Mr. Young I understand. So it's Mr. Abbey is head of the office and is not an astronaut, yourself and Mr. Young, and then you are Mr. Young's Deputy, is that right?

MR. WEITZ: Yes, sir.

CHAIRMAN ROGERS: Mr. Crippen.

MR. CRIPPEN: My name is Bob Crippen. I'm a Captain in the United States Navy. I received a bachelor of aerospace engineering from the University of Texas. I've been assigned at the Johnson Space Center since 1969 as an astronaut.

I served in support roles on the Skylab program, the Apollo-Soyuz program, worked on the development of the Space Shuttle, and served as pilot on STS-1, and have been commander of three subsequent Shuttle flights and missions STS-7, 41-C and 41-G, and

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have spent a period of time serving as Deputy to Mr. Abbey as the Flight Crew Operations Director. I'm currently assigned as the commander of the first flight from Vandenberg.

CHAIRMAN ROGERS: You flew with Dr. Ride I assume?

MR. CRIPPEN: I flew with Dr. Ride on two flights, STS-7 and mission 41-G.

CHAIRMAN ROGERS: Thank you. Mr. Hartsfield.

MR. HARTSFIELD: I'm Hank Hartsfield. I have a bachelor in physics from Auburn, a masters of engineering science from the University of Tennessee.

My background is in the Air Force, a fighter pilot. I joined NASA in 1970 as an astronaut. I worked in support of Apollo XVI and all the Skylab flights, in CAPCOM's support crew. I was a pilot on STS-4 and a commander of flights 41-D and 61-A.

I also participated in the developmental phase of the Shuttle.

CHAIRMAN ROGERS: Thank you very much.

I understand, Mr. Abbey, now you have a statement that you will make and begin with?

MR. ABBEY: I thought I might clear up a little confusion relative to the organizations. I was going to tell you a little bit about—

CHAIRMAN ROGERS: Why don't you move the mike over if you're going to do the talking for a while.

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MR. ABBEY: If I could have that first chart, please.

(Viewgraph [Ref. 4-3-1])

MR. ABBEY: I've tried to show on this chart where we fit as far as the organization is concerned. We are the Flight Crew Operations Directorate, the one outlined in the dark lines, and I am the Director of that organization.

We have two major activities within the Directorate, the Astronaut Office and then we also have the Aircraft Operations Division.

The Astronaut Office has 91 astronauts currently assigned along with supporting personnel. In the Aircraft Operations Division we operate 35 aircraft, and these vary from the Shuttle carrier aircraft that ferries the orbiter after we land [illegible] landing on the West Coast, to Kennedy and around the country.

We also have T-38s and we also have the three Shuttle training aircraft that are pretty key in training the crews to be able to land the orbiter.

We also operate a zero GKC-135 and a super guppy which hauls large cargo that go into the Shuttle around the country.

We have astronauts involved, I think, in a

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variety of activities, and we will try to cover that a little later, but I thought I might just touch on the fact that we have astronauts participating in nearly every phase of the program.

We have astronauts assigned at KSC doing the test and checkout work in support of the vehicle at KSC. We have other astronauts that are assigned to the Shuttle Avionics Integration Laboratory, where they are doing the engineering and software verification tests.

In Houston we have astronauts doing design and development work, doing engineering simulations. We also have astronauts assigned to the flight control team when we fly the flights and as we prepare to fly the flights as CAPCOMs and in other roles supporting the mission.

So we are involved, and the Directorate is involved, I think, throughout all phases of the program.

Could I get the next chart, please?

(Viewgraph.) [Ref. 4-3-2

MR. ABBEY: This shows the Directorate, and again we have the Astronaut Office which is headed by Captain Young. His Deputy is Paul Weitz, on John's right.

Then we have the Aircraft Operations Division. That is headed by Joe Algranti, who is

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probably one of the more senior aviators in NASA, and we have an astronaut serving as his Deputy on a rotating basis, Don Williams. We have rotated a number of astronauts in that position.

I think it has been very good for the organization because that Aircraft Operations Division is primarily involved in supporting the astronauts, and I think it has given a better understanding to both the Astronaut Office and to the Aircraft Operations Division of the objectives and problems at both organizations.

So that is something that we started sometime ago, and I think it allows us to use some of the experience and talent that we have within the Astronaut Office.

We also have a Vehicle Integration Test Office, and that is primarily providing engineering support to the astronauts at KSC as we support, test and check out operations there. They are also involved in preparing to do the work at Vandenberg in support of Captain Crippen and his crew.

We also have a Payload Specialist Liaison Office. That is headed by Don Bourque, and this group is responsible for bringing in the payload specialists that fly with us, training those individuals, and integrating them into our activities and integrating

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them into the flight crews. That office has been a part of our organization probably only about six months.

As far as how we operate and how we work, John is going to talk a little bit about what astronauts do other than when they fly and maybe give you a better understanding of what we do and where we have people assigned.

Paul Weitz is going to talk about our participation in all the various activities across the program; the boards, the panels, many groups that we get involved in.

Then Bob Crippen is going to talk about our involvement in the Flight Readiness Reviews and how we participate in the L-minus Reviews and then how we participate in the launch decision. They will cover that in some detail.

As far as how issues and problems get identified, we have people, as I say, involved across the program. They bring these issues and we status them. John and Paul status them in pilots' meetings at least once a week where we have—the astronauts have opportunities to raise issues.

Of course, during the course of a week daily they will come forward with problems. Usually if they can be resolved, John or P.J. will resolve those

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problems. If I need to get into it, they will bring them forward to me. I will attempt to resolve them and, if necessary, if I can't I will go forward to the individual I work for, who is head of Space Operations, or I will go to the Program Manager, or I will go to the Center Director, or in certain instances I will go to the Associate Administrator for Space Flight.

We have, of course I think, a lot of inputs that come in. We are successful, I think, on getting a number of those inputs accepted. Sometimes they get fully accepted. Sometimes they get partially accepted, and of course I think sometimes they don't get accepted. Usually those are due to programmatic considerations.

Usually if they are not accepted, they are usually due to some programmatic considerations where they weigh the inputs that we give them and for other reasons decide that they would do otherwise, so we will talk a little more about the specifics I think a little later.

CHAIRMAN ROGERS: Mr. Abbey, I'm having a little trouble with seeing the chart. Mr. Young is Director of Flight Crew Operations?

MR. ABBEY: No. I am up in the top of the chart, and then John is in the head of the Astronaut Office.

CHAIRMAN ROGERS: So they've got their own chart. John is head of the

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Astronaut Office?

MR. ABBEY: Yes, sir.

CHAIRMAN ROGERS: Who do you report to?

MR. ABBEY: I report to Cliff Charlesworth, who is the Director of Space Operations. We are one of three elements that make up this Space Operations Directorate. There are two other organizations that are part of that organization, and we are only one part of it. We report through Mr. Charlesworth to the Center Director.

CHAIRMAN ROGERS: Are all of the astronauts in the Astronaut Office under Mr. Young?

MR. ABBEY: Yes, they all are assigned to that office. We do have individuals, as I say, working in other jobs as collateral duties, so they still have to keep up with all their astronaut duties and all their training but we use them in other positions.

As I say, within Aircraft Operations we have the Deputy of that division is Don Williams, one of the astronauts, and he has flown once and is scheduled to fly again as a commander.

CHAIRMAN ROGERS: So Mr. Crippen and Mr. Hartsfield are both involved in the Astronaut Office and they have other assignments, too?

MR. ABBEY: Yes, they are both actively

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involved. Mr. Crippen is my Deputy, and then I also have other individuals that are involved as technical assistants, and we rotate astronauts through that position. So astronauts are involved, I think, in every phase of the Directorate's operation.

I think that has been very beneficial to us because they have a direct involvement in the management, and we can make use of their experience. I think that has been very good for us.

CHAIRMAN ROGERS: Okay, I think I understand now. Mr. Crippen, then, is your Deputy?

MR. ABBEY: Yes.

CHAIRMAN ROGERS: Mr. Weitz is Mr. Young's Deputy?

MR. ABBEY: Yes.

CHAIRMAN ROGERS: Mr. Hartsfield works in the Astronaut Office?

MR. ABBEY: Yes, sir, he works for Mr. Weitz and Mr. Young.

CHAIRMAN ROGERS: And he has other assignments as well?

MR. ABBEY: Yes, he does.

CHAIRMAN ROGERS: I think I'm clear now.

(Laughter.)

MR. ABBEY: John is going to touch on that, talk about the

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different assignments and where they get involved and how Henry, as well as the other astronauts, get assigned to other duties.

CHAIRMAN ROGERS: Fine.

MR. ABBEY: So I will turn it over to John.

CHAIRMAN ROGERS: Mr. Young.

MR. YOUNG: Give me the first chart, please.

(Viewgraph.) [Ref. 1 3 3]

CHAIRMAN ROGERS: I think maybe one of the reasons I'm having a little trouble is my name tag blocks out—there.

MR. YOUNG: You can see this is a pretty straightforward requirement for astronauts, but we are providing those flight crews for NASA space vehicles and when we do that they get an awful lot of time in flight planning meetings and simulators and test and checkout and training. The commander will get about 1,000 hours probably, the pilot 500-plus, a mission specialist 500 to 700 depending upon what kind of assignments they have and training.

When the commander is assigned to a mission, no matter what it says in any book anywhere, the commander is responsible for getting that mission organized and getting it going and getting his crew trained and getting everybody ready to fly. When he is in that

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machine he is responsible for that part of the thing too, from the time it lifts off to the time it lands. All the things that he can do he will be responsible for.

When you talk about participating in design and development, what you're really talking about is hours, and hours and days of meetings and reviews and engineering simulations. You talk about operating techniques and procedures, you're talking about more days and more meetings, desktop reviews, engineering simulations, reviews of malfunction procedures, and so on and so on.

The test and checkout, more time on your back and simulators and places like that, a lot of places around the country. So the first four bullets really specify the kind of people that we really need in the Astronaut Office. We need pilots, engineers and scientists, the best people that we can get who are interested in taking care of those first four bullets, and I mean they have to be interested.

The qualities that we look for besides being pilots, engineers and scientists are desire, dedication, determination, drive and the ability to work with others. It is particularly important in flight crew teamwork because it is critical to the success of every

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mission. You take a flight crew with five people in it. Those five people may know the vehicle thoroughly, and when they learn to work together as a team they can do things that people couldn't even imagine, that they wouldn't even imagine.

But most of the work that is done in the Astronaut Office, strangely enough, is desk work. Eighty to ninety percent of the time it is behind a desk somewhere, and that is just the way it is. It is studying and figuring out how they're going to do the right job, and that is the kind of people we want and that we're looking for. If they're interested in that kind of work, we can use you.

The next chart, please.

(Viewgraph (Ref. 4-3-4)

MR. YOUNG: This chart was made before we had the 51-L accident, and at that time you can see that we were primarily involved with flight crews assigned to missions. I'm not going to go into the second bullet because we have a lot of mission development work going on. We're always looking at new things to do, but I would like to talk a little bit about the third bullet, mission support.

You just heard we have people that are in the mission control center. They work days and nights in

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integrated simulations. They work 24 hours a day around the clock when we have missions going on at Kennedy, and we have a full team of people down there, the cape crusaders who support, test and check out the vehicle and payloads 24 hours a day for as long as it takes to get them checked out.

The software in the Space Shuttle is a very interesting thing. There are about 110 pages you can call up on a cathode ray tube that you have to know the information that is on there so you can react to it.

There is also a thing called program notes, 84, at last count of program notes plus another 20 or so depending on what software drop you have, that crews have to know how to operate in real time so that they don't do something, talking to the software, that might mess up their system operations.

You have people supporting Johnson Space Center Shuttle Avionics Integration Laboratory. When this chart was made, they were working two shifts a day, five days a week, but they work around the clock when necessary. Our flight data file is a very interesting compilation of procedures and techniques established over the years for normal missions that weigh 85 to 95 pounds. That is a lot of books. For a space lab mission or something like a space lab mission, it could weigh as much as 108 pounds.

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Ascent and entry is a separate bullet that people work on, and the reason that is so is because there are about 175 separate crew procedures involved in doing assents and entries, and people have to know those and practice them a lot. We're right now working on aim simulation in the vertical motion simulator to do some things to learn more about landing and rollout, and I will talk some more about that later.

The next chart, please.

(Viewgraph (Ref. 4-3-5)

MR. YOUNG: This is what we are doing right now. We have cut way back on flight crews. We are still working with the last four assigned.

I won't talk very much about this chart except to show you that the next to last line, the 51-L accident investigation and recovery—and that number is not right. It changes daily. I think yesterday it was 63 people, and when more people get back from spring break there will be more people on that chart.

Ninety-five percent of this work that the Astronaut Office is doing is desk work, and the part that is not, I'm sure, is not very pleasant because it mainly involves sifting through crash-type wreckage.

Let me show you the next chart, please.

Viewgraph Ref. 4 3 6

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MR. YOUNG: This is what we did last year. It is called the 1985 Astronaut Activity. Last year was an incredibly good year for the space program. We flew nine Space Shuttle missions. which was four more than we flew in 1984. and we almost flew ten but we had two hold kills and one remanifest.

People say. well. that's bad luck. Well. I tell you when you have a hold kill in this vehicle the best place to have it is before liftoff. There is no doubt about it. We had to do a remanifest. and if you're not ready to fly that is exactly what you ought to do. so I think those were good luck.

But in terms of training with respect to crew requirements and almost everybody else's requirement. that was equivalent to about 11 flights. Since we have people all over the agency working in almost every area—

DR. FEYNMAN: Excuse me. sir. What's a hold kill and what's a remanifest?

MR. YOUNG: That's a good question. A hold kill is when you're down there at the launch pad and you get right up to engine start. and the engine doesn't start or it starts and then shuts down because it has a problem.

A remanifest is when you roll back and decide to go with another payload. so you put a new payload in. Im sorry.

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DR. FEYNMAN: Thank you.

MR. YOUNG: There is. in terms of requirements there is an awful lot of people who make this program work. as you all know by now. in engineering. in training. in mission operations and vehicle turnaround and payload test and checkout and so forth and so forth.

With my people all over the area. I can tell you that we were working about as hard as this system can work from where I could see last year. We really did some amazing things. Because we had such good flight crew training we were able to do some of these exercises. some of which require enormous amounts of crew coordination that you would never even try.

The extravehicular activity bullets down there. for example. The first one was a try to restart a communications satellite. the LEASAT that had gone bad. and the next extravehicular was a teamwork repair of that same satellite. and then the third was a space construction demonstration. all of which required all five people on each mission to work together.

It was a very good year also from the standpoint of flying. We flew 58 seats with 54 astronauts. and we got 14 new crew people experienced in space flight. so by the end of 1985 we had a really good effort going. Right now we have 57 of 91 astronauts who

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have Space Shuttle flight experience. and so right at this moment we have a lot of professional flight crews with a lot of experience.

They are ready to deliver and do all the things that we need to do in space with people. but I expect until we recover from this accident it will be a while before they get to try out those skills again.

CHAIRMAN ROGERS: Excuse me. May I ask. did at the end of 1985 you feel or your office feel that you had had too much to do in 1985?

MR. YOUNG: I think unless—it was hard for me to see how we could do a lot more with our people unless we do something different. and there are ongoing plans to improve that situation.

but it really—we really had some people that were just working long hours and they were working long periods of time

I would like to say that we could do more missions than that, but from an operational standpoint it would be tough unless we do something different, which people are proposing to do and you will hear some about that.

CHAIRMAN ROGERS: In other words, you thought the activity in 1985 was about all you could handle, but that the pressure in 1985 was not too great, is that correct?

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MR. YOUNG: I thought 1985 was a really outstanding year for the space program.

CHAIRMAN ROGERS: But that if you had had to do more that year, it might have been too much?

MR. YOUNG: I think we would have been pushing it, yes, sir.

CHAIRMAN ROGERS: Thank you.

MR. RUMMEL: What do you mean by new crew people? Are these people who have not flown before?

MR. YOUNG: Yes, sir. They are people who got space flight experience in 1985.

MR. RUMMEL: Would that include people like payload specialists?

MR. YOUNG: No, sir, these are just people that work in the office on a regular basis.

MR. RUMMEL: In other words, the 54 would be a total number, 54 astronauts?

MR. YOUNG: In the office, yes, sir.

MR. RUMMEL: In the office.

MR. YOUNG: Yes, sir.

MR. RUMMEL: I see. All right. Thank you.

MR. HOTZ: John, in view of your statement that 1985 was about as hard as you could push the system with nine flights, how do you view the 15 launch schedule for this year as far as the load on your system?

MR. YOUNG: It is really hard for me to assess it from where I sit, but I think that it would have been

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pretty tough.

MR. HOTZ: Thanks.

CHAIRMAN ROGERS: Okay, go ahead, John.

MR. YOUNG: Well, that's about the size of what I had to say in my prepared statement.

MR. ABBEY: Paul Weitz was going to go next.

CHAIRMAN ROGERS: We want to ask you some questions later on, but we'll come back to you, Mr. Young. Fine.

MR. WEITZ: If I could have my first chart, please?

(Viewgraph.) [Ref. 1/3-7]

MR. WEITZ: Mr. Chairman, what I'd like to do is, without going into detail on this again is to kind of round out a little bit what John touched on, was to point out to you, to try to give you some feel, you and the other members of the Commission, for the involvement of the Astronaut Office in the day-to-day activities at the various levels of the program as they occur.

The Program Requirements Control Board, to take the first bullet for example, is a board that is what we call level two, which is chaired by Mr. Aldrich, to whom you've already spoken and will later today. The purpose of that board is to approve most changes to the Shuttle transportation system both as they pertain to the fleet generically or any changes that may be made to a specific unique vehicle for any particular purpose

Another very important level is the Configuration Control Board, which is at level three, which is obviously the next level down below Mr. Aldrich. They have signif cant but limited authority to authorize changes of the same type that the PRCB would do. Their primary purpose in life is, as their name implies, is to maintain a record of the configuration of the vehicles so we really know what we're flying, and that is a lot more significant than it perhaps sounds.

Another, if we go down to the fourth bullet, the Orbiter Avionics Software Control Board is essentially the same type of activity and control of the software. Remember that, as you have probably been briefed, that this vehicle flies totally by wire.

Everything in it is controlled by the computers, and the software control and design is very, very important in every aspect of managing this vehicle, both on orbit and during the active assent and entry flight phases. So this is essentially the software equivalent of the PRCB, as we have referred to the Program Requirements Control Board.

Another significant one is the next to the

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last bullet, the Mission Integration Control Board, which primarily its primary purpose is to assure that the Shuttle transportation system will in fact support the missions that are assigned to it.

They are the folks, for example, who decide that—and these folks work for Mr. Aldrich, and they will decide to remanifest, for example, if the situation warrants, to use John's previous example.

Could I have the next page, please?

(Viewgraph [Ref. 1 3 8.]

MR. WEITZ: We have many other functions, as you can see here For example, the Flight Operations Review is a periodic assessment of our progress toward the planned flight rate. This looks at it from an overall programmatic point of view to see that we are doing the things we can in the right manner to get to where it is we're trying to go as far as our manifest and our flight schedule.

CHAIRMAN ROGERS: Which one is that?

MR. WEITZ: That's the first one, sir, on the second page, the Flight Operations Review.

CHAIRMAN ROGERS: Just to make it a little more easy at least for me to understand, in the event of a problem with the joint that caused the trouble in 51-L, when information developed about that which one of

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these groups would that information normally go to?

Would it be—in other words if you have trouble on a flight with blow-by and it was clear from that flight that there was erosion and blow-by and it be ne a problem, how would that information be conveyed to these panels, and who would convey it?

MR. WEITZ: I'm not, I think, well versed or qualified enough to answer that question. I would prefer, if you don't mind, that you address that to Mr. Aldrich when he is on later because that is with . his organization.

CHAIRMAN ROGERS: In other words, you wouldn't know how that would get—I'm trying to find out how that information gets to the astronaut community, and I gather in this case it didn't.

MR. WEITZ: Yes, sir.

CHAIRMAN ROGERS: And the information about the joint that failed and we think probably caused the accident was not known to any of you gentlemen, as I understand it.

MR. WEITZ: That is true, which means, therefore, that if it surfaced at one of these activities as represented on these pages is either we were not made aware of it while we were there or we did not realize the significance of the item.

CHAIRMAN ROGERS: Well, I gather from so far in our investigation that it wasn't presented to you from an

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organizational standpoint. None of these groups knew about it, or at least you didn't know about it. That is correct, isn't it? None of you knew about the problems that you had been having with this joint?

MR. WEITZ: Yes, sir, that is correct.

CHAIRMAN ROGERS: I guess one of the things we have been trying to find out is how that happened. How did it happen that the astronauts who are so vitally concerned with safety aspects didn't know about this problem?

MR. WEITZ: That is part of what we're trying to reconstruct, also.

CHAIRMAN ROGERS: I guess my question is where would you normally have expected that information to go organizationally? Which one of these groups would normally, or should have rather than normally, should have received that information?

MR. ABBEY: I think, if I might answer that, if it was a flight anomaly it usually would come up at the Flight Readiness Review, which Mr. Crippen is going to talk about in a minute.

CHAIRMAN ROGERS: Okay, thank you.

MR. WEITZ: Well, but as a design change I would expect it to show up, if it is recognized as a problem, and something potentially requiring a design

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change, it would probably show up at the first bullet on the first page.

CHAIRMAN ROGERS: Well, let's be specific because obviously I'm having a little problem with so many suborganizations. I understand it is necessary, and you do a wonderful job, but for us to decide where it should have—where that information should have gone. In this case beginning in August of 1985 concern had been expressed to a lot of people about this joint and about this seal and about the O-ring and about putty and so forth, and at that point there were tests being made to possibly redesign that joint.

Now, what I have trouble understanding is why none of you gentlemen knew about that. That was a redesign question at that time. They were considering the redesign of it at the beginning of 1985, and they were considering the redesign of that joint even at the time the accident happened, and yet none of the astronauts were given that information.

I'm asking the question to see if we can find out what happened in the system that caused that failure, that you didn't know anything about it. Well, we will get to Mr. Crippen later. Go ahead.

MR. WEITZ: We do not have an answer to that, Mr. Rogers.

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CHAIRMAN ROGERS: I wasn't pressing you. I'm just trying to find out why it might happen, because one of the things we're going to have to consider in our report is what can we do to recommend corrective action so that it doesn't happen again so that everyone, particularly the astronauts, are aware of these problems when they arise.

Now, we realize that you're not going to be aware of every single problem, but certainly critical ones like this that had gotten to the point where a redesign is under consideration, it is difficult for us to understand why you didn't know about it.

I can see why Mr. Young and others were upset about it, Mr. Hartsfield. I know, I noticed the same thing.

MR. WEITZ: We feel the system is in place but it broke down in some way, and I think part of the message that comes across here, perhaps, is we do not have enough people in the Astronaut Office to be intimately involved in all of these details along the way. I think from John's presentation, and you can see from some of these activities, these do not represent all of the activities of these various boards and panels that go on at the Johnson Space Center, either, and that we must in

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fact have a system that does identify and correct these deficiencies.

MR. HOTZ: Mr Weitz, do you think the joint problem is the kind of problem that should have surfaced in Flight Readiness Reviews or configuration control boards or wherever, that it ought to have come to your attention?

MR. WEITZ: I don't see how we could say otherwise, Mr. Hotz. I have to say I believe that, since it turned out to be apparently a fatal flaw.

MR. HOTZ: Well, we're interested in your opinions. That is why we're asking these questions.

MR. WEITZ: Yes, sir. The answer is yes.

CHAIRMAN ROGERS: Thank you. Go ahead.

MR. WEITZ: As we move down this page, this mainly is a representation of how we get out of the design and engineering type of activities and into the mission and flight specific planning activities.

All of these groups are related one to the other. They tie together in various ways, and we either directly or indirectly have tried to maintain awareness and cognizance of what is discussed and what will be discussed and what has been decided at these various boards. As I say, we can't always go but we are within the Space Operations Directorate.

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One of the other divisions was the Mission Operations Division. Those are where the flight directors are resident, and the flight and controllers, and they are our principal representatives at many of these boards and panels.

VICE CHAIRMAN ARMSTRONG: I'm sorry. Could you say that again, Paul.

MR. WEITZ: Yes. The folks in MOD, the Missions Operations Directorate.

VICE CHAIRMAN ARMSTRONG: Are representatives of whom?

MR. WEITZ: We often depend upon them to represent us, either directly or indirectly, at these different boards and panels when we can't make it. We lean heavily upon those folks, the flight directors and the flight controllers, to keep us up to speed and apprised of what is going on because those are the operational system experts.

If you have no further questions, Mr. Rogers, Mr. Chairman, I think I'll leave this viewgraph and turn it over to Bob Crippen, who will discuss a little bit about our involvement in the mission specific activities.

MR. CRIPPEN: Yes, as George Abbey, John Young and Paul Weitz have indicated, the Astronaut Office is intimately involved with all phases of the program.

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Obviously one that you've brought out shows a breakdown somewhere, but it is not because we don't think we're involved.

What my remarks are going to be directed toward, what it takes in the final week or so prior to launch, the major milestones that we go through in deciding that we are ready to fly. There are many things that lead up to a mission, many meetings, many milestones, and I'm only going to address those that occur in the final weeks.

If we could go to chart C-2, please?

(Viewgraph C-2 [illegible])

MR. CRIPPEN: The first one is the Flight Readiness Review, which was alluded to earlier. The Flight Readiness Review occurs approximately one to two weeks prior to flight. It is a Level I, meaning the Associate Administrator for Space Flight is the gentleman that is the main leader of that particular meeting.

One of the prime things that we do in a Flight Readiness Review is review the anomalies that have occurred on previous missions and decide how we have resolved those or why we think it is acceptable to go fly with those.

Specifically in mentioning the joint problem,

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I was the participant representing the Flight Crew Operations. As I indicated on the second bullet there, we normally are represented by the Director of Flight Crew Operations, George Abbey or myself; the Deputy, as his representative; and a senior member of the Flight Crew or Astronaut Office. Normally it is the one that we refer to as our Deputy for Integrated Ops.

On Mission 51-E, which was the mission that we flew right after 51-C where we did have a blow-by problem, that was presented, and it was presented that we had had a blow-by and that there was sooting. In truth, from my perception it wasn't considered that much of a big deal, and it wasn't like we had a major catastrophe awaiting in front of us. I guess the emphasis was not such that one would think that it was the major problem that it was.

CHAIRMAN ROGERS: Who presented that anomaly?

MR. CRIPPEN: That was presented by the Marshall Space Flight Center. In going through their stuff on the solid rocket boosters. It was presented as an anomaly and, if my memory serves me right, we had had a putty change on the joint just prior to that, and it was alluded that perhaps the putty modification may have had something to do with that but that it really wasn't that big of a deal.

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That was the only specific flight that I can remember, although I think in some other Flight Readiness Reviews that perhaps was discussed.

CHAIRMAN ROGERS: Mr. Crippen, looking at a document that related to that Flight Readiness Review, I notice that the resolution said acceptable risk because of limited exposure and redundancy on the seal.

Mr. CRIPPEN: Yes.

CHAIRMAN ROGERS: Did you at that time realize that the Criticality One list had indicated that there was no redundancy?

MR. CRIPPEN: I did not, and I was not aware of the waiver that had changed the joint from a 1-R to a 1, and I was not aware of the rotation problem. If I had been aware of that in association with the sitting I would have taken the problem much more seriously.

CHAIRMAN ROGERS: Thank you.

MR. CRIPPEN: So at the Flight Readiness Review we do go through all of the anomalies from the previous flights, and if it is—usually what we are addressing is the flight just prior, and since we are also rotating vehicles now, orbiters, we also go back to the last flight of the orbiter that we are just getting ready to fly.

In addition, we get essentially a go from all of the levels of the program that we are ready to go in

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the mission and sometimes it's normal that we do receive action items out of these meetings that are to be closed prior to the actual flight.

Could we go to chart C-3?

(Viewgraph C-3.) [Ref. 4/3-10.]

MR. CRIPPEN: The next major milestone is addressed at the Launch Minus One Day Review, which is just that. One day prior to the flight we will hold another Level I meeting, and it is primarily a final tagup looking at the actions as have been closed from the Flight Readiness Review.

It looks at specifics like what is the weather prediction for tomorrow and any other anomalies that might have occurred in that period of a week. In general, it is more or less of a final tagup, and again every element of the program gives a final go.

We are represented again at that meeting normally by Mr. Abbey and Captain Young. Now, that is the minimum representation. We quite often have other people that will attend or be on a telecon hookup. For example, the Deputy of Flight Crew Operations normally monitors that from Houston.

Could we go to chart C-4, please?

(Viewgraph C-4.) [Ref. 4/3-11.]

MR. CRIPPEN: When we get into the final

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launch count there are numerous people involved, obviously. The most obvious, of course, is the flight crew. The flight crew, like everybody else in the program, can only work on the basis of the knowledge that they have. They normally are kept fairly up to date regarding all details with regard to problems that are directly mission related or vehicle related.

In truth in the final count the commander and the pilot are the only two people in the Astronaut Office that actually end up giving a final go. In the final count, the launch director will ask if the commander is go and if the pilot is go, and in truth I have, nor do I think any of the other gentlemen who have flown, have ever thought that if we said we were no go I'm sure we wouldn't have lifted off.

We, of course, would have to have had a reason for doing that. For example, Dick Scobee on 51-L when they had the hatch problem wanted to make sure that the hatch was go and didn't want to take that responsibility on the crew and wanted somebody to come in and check it, and requested and received that kind of an inspection.

The Director of Flight Crew Operations, George Abbey, is located at the launch site, the place right off the launch control center called the Operations Support Room, and he is intimately aware of what is

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going on in the final count.

We have a weather pilot up flying one of our Shuttle training airplanes, and that is normally John Young doing the final check of the weather in case we might be involved in a return to launch site abort.

In addition to that we also have a weather coordinator, who is an astronaut located at the landing field itself talking to both all of the Cape weather people and to Houston and to whoever is flying the weather airplane, normally John.

The Deputy Director of Flight Crew Operations will normally be back at the Johnson Space Center in Houston, available in the flight control area if there are any questions that come up that he should be making an input to.

In addition, we have a senior astronaut assigned in a support room. That is referred to as spacecraft analysis with the acronym of SPAN just off this flight control room, and that person is intimately aware of what is going on in the final count and can make inputs.

Of course, we also have the CAPCOM, who is assigned to the flight control room who is an astronaut that does do all of the communications with the vehicle.

So we definitely are intimately aware of what

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is going on in the final count and have numerous means of making inputs to that.

MR. RUMMEL: I note on the first chart 2 and chart 3 that the Astronaut Office is represented by a Director of Flight Crew Operation and so on. Is that a mandatory participation?

MR. CRIPPEN: It is a mandatory participation that he is either there or his representative.

MR. RUMMEL: Well, now I note the absence of the mention of the specific flight crews that are involved in the mission being considered. Do I read that correctly?

MR CRIPPEN: You read that correctly, and that is by intent. It is mainly that the crew is so involved in the training and the daily preparation of getting ready to go fly that it would be impossible to pull them directly into these meetings. It is always, they are always given a summary, quite often given the detailed pitches of everything that has gone on in the particular meetings involved.

It is also true that we have a health stabilization program that starts at a week prior to flight where we don't have them normally getting in large groups just to make sure they don't come down with a cold or something.

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MR. RUMMEL: But of course there is TV and telecon and that sort of thing that might take the place of that. But what I'm getting at you've already touched upon, is in exercising the final judgment, which I now understand the commander has from what you said, as to whether to go or no go. Since he did not participate directly in the Flight Readiness Review or the Launch Minus One Review, he is relying on passed-on information, I take it, to the extent that he may receive it rather than direct participation?

I wonder if that doesn't raise the question of the degree of knowledge that he may have in his possession as to any potential problems or questionable areas that he may be confronted with on the flight?

MR. CRIPPEN: Sir, I can understand your concern; however, you must realize that putting the mission together to go fly is a very complicated thing that involves lots of people, and there is no way that the flight crew themselves can personally participate in everything that it takes to get ready to go fly.

They are totally reliant on being supplied information, and we have set up chains of command and funnels for doing that. I think that there has not been anything that has occurred in one of these reviews that was considered in any way significant that the flight

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crews haven't heard.

MR. RUMMEL: I think I understand the inherent complexity and the difficulty of the commander being apprised of each and every item that might be brought into consideration with respect to the flight.

What I was addressing, though, were the potential hazards, those conditions, O-rings, whatever, that have existed or that may exist, but which review should shed light and make clear, so that he at least is aware of those things with which he may be confronted with as contrasted to actual problems that have developed.

I would have thought that that category would not be limitless, that it might be contained and it might be an important item for him to consider in deciding whether or not to go or no go.

MR. CRIPPEN: In general the system is set up such that those items are brought to his attention, and they are numerous that we go through and talk about and get the flight crew's inputs as to what we should be doing about them, in the case of the O-rings again.

That isn't just true of the Astronaut Office. I might admit there are several layers of NASA management that were also apparently not aware of the O-ring problem.

DR. RUMMEL: But it is indirect information?

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MR. CRIPPEN: It is indirect information, yes, sir.

DR. RUMMEL: Thank you.

MR. ABBEY: If I might, there is a system set up where we do have the systems people at KSC that do the test and checkout work. We have the flight control team go over each of the systems in a telecon, realizing that the crew is in quarantine. That is done at L-1, and we go through each of the systems and any problems are discussed over that telecon directly with the KSC test and checkout personnel as well as the flight control team in Houston.

MR. RUMMEL: Well, are those problems that have manifested themselves in the normal course of checkout or are these potential problems that maybe existed on prior flights or which analysis indicates could occur?

MR. YOUNG: They really fall into both categories. They are any concerns that any of the systems people are knowledgeable of in Houston and the flight control team or any of the system problems that they are knowledgeable of at KSC.

In addition to that we do have, as we said, the astronauts involved in the test and checkout operation, and they do the switch list and they support all of the activities before the crew comes out to the

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vehicle. They are very knowledgeable of any problems that the team is aware of, and they brief the crew on any anomalies or any problems during that last day and right on until when the crew goes out to the vehicle.

MR. HOTZ: Bob, could you explain a little bit more about this authority of the mission commander on launch, the go or no go call? Is this a formal documented authority, or is this just developed over usage and tradition?

MR. CRIPPEN: It is a part of the formal OMI or the Operational Maintenance Instruction that the Cape uses to count down the launch, and so it is a formal NASA document and he is one of the—he and the pilot are the two people that have a formal go.

MR. HOTZ: Thank you.

MR. RUMMEL: How long prior to launch is that go no go decision given by the commander?

MR. CRIPPEN: Somebody can probably help me out. It is about the nine minute point, the 0-9 minus 9 minute.

MR. RUMMEL: Prior to the au omated liftoff procedure?

MR. CRIPPEN: Yes, sir.

DR. COVERT: Captain Crippen, you've been an operational pilot and a pilot in development squadron and a test pilot as well as a pilot and commander of the

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Space Shuttle.

Would you sort of help me to understand some of the similarities and differences between these different kinds of operations, particularly in terms of the complexity and the decision points and the pilot's input into the activities, or is this such a broad thing that you would be here the rest of the day talking about it?

MR. CRIPPEN: I'm afraid we would be here the rest of the day talking about it.

DR. COVERT: Could you summarize it briefly, then?

MR. CRIPPEN: Flying a spacecraft, of course, is a lot more complex than taking off in an airplane. Even if you take a test flight, for example, that will have a test team behind it and usually a test conductor or a test engineer on the gro id and the pilot. You are not working with as many people. You are working with—even though it can be complex, it is a simple piece of machinery. I don't think there's a vehicle anywhere in the world that is probably as complex as the total Space Transportation System when you put it together.

Consequently, the biggest difference is you have to, as I was saying for Mr. Rummel, you have to be reliant more on people supporting you, and you have to

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have confidence in the system. You have to have trust in the system, otherwise you never would go fly it.

We are normally cognizant of things that we consider significant problems, and we are intimately involved with working those. Again, if—just like a pilot would have the opportunity when he went out and ran the engines up, if something didn't look right to him or didn't feel right to him you could say no go, and the commander has an opportunity to do that, however, he is only as good as the data base that he's working with.

DR. COVERT: It is essentially the complexity of the data base here that puts the increased challenge and the increased reliance on the whole team to make this a successful decision?

MR. CRIPPEN: In truth, with most of the modern day aircr t we are flying, as they become more and more complex the pilot in the cockpit is faced with a similar situation. He really is.

DR. COVERT: Fine. Thank you very much.

VICE CHAIRMAN ARMSTRONG: How much total fl ght crew personnel are assigned to each flight in addition to those that are actually going to go in?

MR. CRIPPEN: I think I might defer to Paul on that one.

MR. WEITZ: We don't have the support teams

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anymore, Neil, so directly assigned to follow a flight is just the flight crew itself.

MR. CRIPPEN: But we do have the CAPCOMs that are directly associated coming up on a mission; however, they rotate from flight to flight, but we do not have in a role like we used to have of a backup crew plus an astronaut support crew. We do not have that anymore.

MR. ABBEY: We do have at least four or five astronauts on a full-time basis supporting the final count and supporting the crew down at the Cape.

VICE CHAIRMAN ARMSTRONG: Other than those individuals assigned to the specific flight, who would be most knowledgeable about the specific flight consideration systems, payloads and so on for a given launch?

MR. CRIPPEN: That would be the CAPCOMs.

VICE CHAIRMAN ARMSTRONG: Is he assigned that position a substantial period ahead of time so that he develops that body of knowledge along with the crew?

MR. CRIPPEN: Normally what we have been doing is supplying CAPCOMs in for a period of at least six months to a year, and we rotate them but we use the same CAPCOMs over on each flight. The more often you are flying flights the less time they have in between them to get familiar with the payloads that are associated on

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a particular mission.

VICE CHAIRMAN ARMSTRONG: It appears as though you are dependent, to some extent, on a conduit of information from in terms of the review processes that Paul was talking about of information from whoever, Mr. Abbey or his representatives, that are in the review process getting to the commander.

So that is a conduit, but a conduit also acts as a filter and cuts down the amount of information that comes through to the most important points. I think the question that concerns the Commissioners is how do you assure that the filter doesn't filter out too much information or provides the proper amount?

MR. CRIPPEN: We do depend upon the filter, but we are flying crews of normally five people from the Astronaut Office. My personal experience has been probably the most knowledgeable people on the center with the payloads were in the crew, and I'm not sure that isn't of the vehicle if you want to take it from an overall standpoint. It is mainly because, as John indicated, still our job is not sitting in simulators. It is sitting down and going over paper, and we are normally assigning crews like mission specialists to a flight a year ahead of time. They work in great detail with that

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payload and with the payload support officers that we have there in our flight control room.

Those people know that payload as well as anybody, and so I don't think that they are really dependent from a payload standpoint on that much of a filter, if you will, to get data.

From the orbiter side or the vehicle side, I think that our flight crews probably understand it as good as any person that possibly could when you have to look at the overall system. They rely very heavy, as we always have, on our systems division people who monitor each system specifically, but everyone will—is not bashful about picking up a phone and saying hey, come talk to me about this, and sit down and go over any details of any problems.

We normally have the anomaly list from a flight to go through as soon as that flight is over, and you start looking at it yourself as well as having observed what's going on in the flight.

So I don't want to leave the picture that the flight crew is off here training and they don't know anything about what is going on down in the bowels of the ship because that is not correct. Going to these formal reviews, we have to depend on other people to handle it for us.

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MR. YOUNG: In the countdown demonstration tests, the people down at the Cape go through a complete listing of what's happened with that particular vehicle, what has gone wrong with it, and then when they get down there for the flight the Cape people go through another listing of what has happened with that vehicle Each vehicle is very different.

We also get a complete listing of what has happened on the vehicle from the systems people in the Missions Operation Directorate that says what has been changed out, what are the new problems, what are the old problems, what are the unresolved problems.

This is about a five-page memo of specific things that have happened to that vehicle that people are interested in, and since they're going to be watching the machinery while it flies they are the best. They know that, and they also participate in that L-1 briefing so they know what happened to the vehicle before it launches.

It is about as good a job as you can do on this kind of thing. You really want to tell astronauts what's going on about problems that they can do something about. When you go to a Flight Readiness Review and you hear what people have said, there may be some very interesting things in there, but if the

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astronaut really can't do something about it or be aware of it or take some kind of action and it's not a serious problem that anybody has brought up, the Flight Readiness Review lasts all day long, and those people are terribly busy, you probably wouldn't tell them about it. I don't recall anything coming up in the Flight Readiness Review on the solid rocket motor seals.

MR. RUMMEL: Why wouldn't what he could do something about include discussion, demands, requirements, whatever, for design improvements in cases where such appears to be the case?

MR. YOUNG: I think that's exactly what would happen, and I think if anybody in the gang had known about this business and understood it we might have said something, but really it should have been taken care of by the process long before it ever got to a Flight Readiness Review, I believe.

MR. RUMMEL: Absolutely, but in cases where it does not that would be a legitimate concern, would it not, or might not it be on the part of the astronauts?

MR. YOUNG: Yes, sir, and they would talk about it.

MR. ABBEY: Neil, I think we don't just rely upon John or I or Crip going to the Flight Readiness

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Review. We also have a very formal meeting with the flight directors and the CAPCOMs, and the flight directors have been at the Flight Readiness Review, so you're getting really two inputs coming into the flight crew.

We schedule that meeting at L-, probably L-4 after the Flight Readiness Review, so they're getting an input not only from John and I but they're also getting an input from the flight directors and as well as the CAPCOMs, so they're getting kind of a redundant input there.

VICE CHAIRMAN ARMSTRONG: George, I understand that, and I think I understand that we have a system of very complex information flow and a system that you've devised with checks and balances to make sure that information flow properly gets to the right people.

Nevertheless, we have to face the fact that somehow it hasn't. Can you be at all specific about whether you think changes are appropriate? If so, do you have any idea as to what you could recommend?

MR. ABBEY: Yes, I think we have some thoughts along that line, and we are going to cover them a little later.

VICE CHAIRMAN ARMSTRONG: Okay, thank you.

CHAIRMAN ROGERS: To be a little more specific there was some Mission Management Team meeting

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on the 27th of January, the day before the launch. Was there an astronaut at that meeting or a representative, and who was it?

MR. ABBEY: We were tied into that meeting by telecon, sir.

CHAIRMAN ROGERS: According to the testimony, as I recall the testimony Mr. Aldrich said at that time that there were weather concerns expressed.

MR. ABBEY: We were in on the meeting on the 26th. We were not involved even on the telecon on the 27th.

CHAIRMAN ROGERS: Well, to go back to it, the meeting was held at 2:00 p.m. after the launch was scrubbed that day, and it was 2:00 p.m. on January 27th.

At that time Mr. Aldrich said that there was a concern about the weather the next day, and he advised everyone at that meeting that if they had any problems with weather or any concerns about the weather to let him know.

As the testimony disclosed, he was not advised about the O-ring, the joint problem and the weather as it related to that joint, and my question was, was any astronaut present at that meeting?

MR. WEITZ: Yes, sir, I think I was, along with, as Bob said before, the Spacecraft Analysis Room representative.

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CHAIRMAN ROGERS: Then at the 9:00 meeting the morning of the launch, were you there, too, Mr. Weitz?

MR. WEITZ: Yes, sir.

CHAIRMAN ROGERS: Do you remember that Mr. Aldrich advised the people at those meetings if there were any concerns about the weather that he should know about it and that he should be told about them?

MR WEITZ: Yes, sir.

CHAIRMAN ROGERS: And he wasn't, apparently?

MR. WEITZ: Well, the meeting on the—

CHAIRMAN ROGERS: I'm speaking about the weather as it relates to the joint, the O-rings.

MR. WEITZ: We were not aware, no, sir. We were not aware of any concern at all with the O-rings, let along the effect of weather on the O-rings.

CHAIRMAN ROGERS: So he was not, nor were you, advised of all of the problems that existed in the minds of the people in Thiokol and the people at Marshall about the weather? Neither you nor Mr. Aldrich were advised about that?

MR. WEITZ: Not that I remember at those meetings, no, sir.

CHAIRMAN ROGERS: Thank you.

MR. CRIPPEN: Okay, if I could go on to chart C-5, please?

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(Viewgraph C-5.) [Ref. 1/3-12]

MR. CRIPPEN: As you brought up, the Mission Management Team meetings are a mechanism that the Level II program manager uses to make decisions throughout a mission, and he can use that Mission Management Team meeting prior to the mission, as was done in this particular case.

Normally as I address them here, they are daily meetings that occur during a mission that give the program office an opportunity to make any major decisions that need to be made. We are well represented there, as I indicated, again as a minimum by Mr. Abbey, Captain Young, and again our SPAN representative will also be there.

Chart C-6, please?

(Viewgraph C-6.) [Ref. 1/3-13]

MR. CRIPPEN: For entry essentially it is an inverted process from what we do for launch. Of course, the flight crew is there, and any time that they elect that they might want to wave off the entry they certainly have the authority to be able to do that.

Again, we have a Shuttle training airplane checking the weather at the landing site and in general if the landing site is intended to be the Kennedy Spacecraft Center we also have one as a backup at

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Edwards in case that should become the prime site. We also have our weather coordinator, an astronaut on the ground at the landing site itself passing that information back and forth.

MR. RUMMEL: Now, would these things you mentioned apply to alternate sites, for example, overseas sites?

MR. CRIPPEN: The only overseas site that we normally staff would be our trans-Atlantic abort site, and that is not normally done with an astronaut but it is done by a member of the Flight Crew Operations Directorate who is very familiar with the capabilities of the Shuttle. They do check the weather at that particular site, yes, sir, but that is the only one that we check.

DR. COVERT: Do they check that by standing there or by getting in an airplane and flying around and seeing how it looks from the air?

MR. CRIPPEN: Normally getting in an airplane and flying and checking it out, yes, sir.

We also, back in Houston, will be staffed with normally the Deputy Director of Flight Crew Operations, again in a standby mode in case he is needed, our SPAN representative and again the CAPCOMs, of which there is a prime and a backup, that are all aware of that.

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Those actually conclude my formal remarks. If there are no other questions, I will pass it on to Hank Hartsfield.

DR. KEEL: Mr. Chairman, could we just ask one question here to follow up on Neil's to make sure that we do cover this?

Are your recommendations, Mr. Abbey, downstream going to apply also to improvements that you may see in the astronaut participation in the Flight Readiness Review or the Mission Management Team meetings, or is that just going to be in terms of resolving issues?

MR. ABBEY: No, they're not going to address that aspect of it.

DR. KEEL: Well, can we ask now, then, are there any recommendations you have now, any of you, with respect to improving astronaut participation in Flight Readiness Reviews or the launch decision process?

MR. CRIPPEN: Mr. Keel, from my standpoint I don't know of anything that I would recommend that we would change in that particular area.

I think that the basic system is good, but again it is only as good as the data base that you have feeding it, and I don't know how we could change that at this particular time as far as the actual participation that we have from the Astronaut Office.

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DR. WHEELON: I have a question for those of you that were there the day before that fateful day. Did you detect an unusual urgency to proceed with the launching? Was it out of the pattern of prior launches that you had experienced?

MR. YOUNG: I think there's an urgency to proceed with every launch once you get a vehicle loaded and on the launch pad. I don't see anything wrong with that, but it is there. I think in the future the higher the launch rate the more that urgency exists, and I'm not sure that that's something we have a whole lot of control over, but I think we ought to watch it very carefully.

DR. WHEELON: I accept that answer, of course, but the question, granting that each launch has an urgency, was there an unusual urgency surrounding this launch?

MR. WEITZ: I did not perceive any different sense of urgency with this one as with any other. I agree with John. I think that that sense to get it off, there is a general feeling once you start into the count that a lot of work has gone to get you out there on launch morning, and we would like to within reason do those things necessary to get the launch off.

But I think that that has, you know—if

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you're asking for perceived differences, I did not perceive any.

MR. ACHESON: I have a question for Captain Crippen. I would be interested in your reflections on a suggestion that some of us have heard made informally, that perhaps new problems that don't have preplanned responses ought not to be decided or disposed of after, let us say, the L-1 or at some stage fairly near the launch, but should simply be the occasion for a scrub and then a more deliberate consideration.

Would you regard that as an unmanageable approach to the problem, or would you think it had merit, or would you have some modifications on that idea of your own?

MR. CRIPPEN: There comes a point in time in any count where if you have an unusual problem develop that the answer is to go ahead and scrub. I think that if L-1, if you're talking about L-1 days, would be unreasonable myself.

There are numerous things that can happen, and quite often they are not what you anticipated but we have a crew of people, both at the Kennedy Space Center, the Marshall Space Flight Center, and the Johnson Space Center, as well as all the supporting contractors that are very knowledgeable on the vehicle.

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In general, if they have time to work a problem and run it through the checks and balances that we have within our program level management and everybody is satisfied with it, then I think we ought to be able to lift off.

I personally think that the way we handle that today is reasonable.

MR. ACHESON: Thank you.

MR. HARTSFIELD: Let me make a comment on that. I think I can give you an example on flight 41-D, because this is something that came up, I think, in the thing you're talking about, something of a general nature that doesn't really apply specifically to something uncovered during the launch.

We had had a problem with some of the jet driver boxes, and we began to look at our electronics, and in the process of looking at all our electronic boxes we found a possible single point failure in the master events controller box that would cause a timing problem such that if it occurred we would not get rid of the solid rockets.

The possibility of this single point failure occurring was very, very low. There was a way to temporarily fix the problem by patching the software to open up the period in which it would give the PC less

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than 50 and let the thing operate.

Well, this problem only surfaced the day before we were supposed to launch, and George and John brought the problem to me as the commander and the crew, and we discussed all the aspects of this. We could, as it was presented to me, we could accept the risk and go fly. We had flown 11 flights already with this problem and didn't know it; or we could delay a day and fix the software and verify the patch. We had the patch ready to go but it had not been verified in our avionics lab.

Our input as the crew and the commander was let's wait a day and do it right and fix the software. They took that back to the program, and as far as I know—well, we did delay. That is what happened and so that is, in my mind, the way the system works and it is supposed to work.

But again, the information has to surface. In the case of the solid rocket motor, somewhere along the line the information didn't get to us.

CHAIRMAN ROGERS: I suggest we take a ten-minute recess.

(A brief recess was taken.)

MR. JOHNSON: Let's continue, please. Mr. Weitz, do you want to make a correction, please?

MR. WEITZ: Yes, sir, there were many meetings

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involved with the efforts to launch 51-L, and as best I can recollect I went to more than one meeting on the 26th and 27th but apparently the particular one under question, the 2:00 p.m. Mission Management Team meeting on that date I was not present at.

CHAIRMAN ROGERS: That is fine. It was only important because, in my mind, because Mr. Aldrich had pointed out to everybody at that meeting if there were any weather concerns, not just about the launch pad but any weather concerns, that he would like to be notified himself the next day.

Mr. Hartsfield, would you proceed?

MR. HARTSFIELD: Yes, sir. If I could have the first chart, please?

(Viewgraph.) [Ref. 1/3-11]

MR. HARTSFIELD: What I would like to talk about here is how I think we ought to address some of the issues. You know, we know that there are certain issues that some of us have been concerned about, and we've had a lot of discussions, as you might imagine, at our office as to how we should handle these.

What I would like to present to you now are my views on the subject, and I might say that it is my opinion that the general thrust of what I am going to tell you is shared by almost everyone in the office as

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to how do we get from here to flying again.

CHAIRMAN ROGERS: Could I say, too, before you get started again, these are the very things that the Commission is most interested in.

As I said in my opening statement, one of the things that we were asked to do was to make recommendations to the President on safety factors that should be considered and make recommendations along the lines of how can we make flight safer in the future.

So what you are about to do and what we will be asking others on the panel about are the safety factors that should be addressed by the Commission, and so we appreciate this presentation.

MR HARTSFIELD: Well, this is going to be very general in nature, and we're going to talk about some issues when I get through here, but what I am proposing is how, I guess, that I would like to see things proceed. As I said, I think that it is shared in the office.

If I could go to the next chart?

(Viewgraph.) [Ref. 4-3-15]

MR HARTSFIELD: All of us know that we have just suffered a great tragedy, and I think it has given me a lot of time to reflect, and I think it is time for introspection in all areas of our business, starting

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right at the Astronaut Office and going on up through all of the facets of this program, to see what we should do.

Now, most of the things that I'm going to mention here are already being done, and we have astronauts, as you saw a while ago, participating in all of these things. In my mind when we go through all these things and when you read into these recommendations, safety is the watchword. That is the thing that is, of course, foremost in all of our minds because, really, if you are safe that equates to a successful mission, and after all that is what we're trying to do.

If I could have the next chart?

(Viewgraph.) [Ref. 4-3-16]

MR. HARTSFIELD: The first thing that I think we ought to do and we are doing is revalidating the design. Now, that is getting rather basic but I think we need to go back and look at what we set out to build, what our design requirements are and whether they were consistent or not.

For example, it doesn't make much sense to build a wing that is good for four G's and a tail to fall off at two G's, so we need to make sure that we've got a good basic design, that we did lay out our requirements

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properly, and then look at what we built. Does it meet the design? How did we test that?

For example, if we had a spec that said a certain device was supposed to operate between two temperature limits, did we test it? If we didn't test it to those limits, how did we certify the design? Did we do it through analysis? Is it acceptable? If there are waivers, do we now still say that those were good waivers and then the end result, then, is there are constraints that have to get into our operational design? We want to review those and make sure that—

MR. HOTZ: Excuse me just a minute. Could you clarify as to whether these are conceptual things that you are recommending or these are processes that are actually going on now?

MR. HARTSFIELD: Well, this one is going on. I'm giving you my personal opinion that I think is shared in the office of how we get well, and we are going to look at all facets of the program which I think Admiral Truly has started us doing.

MR. HOTZ: So what you're telling us here is basically an ongoing process and not something that you plan to do at some future date?

MR. HARTSFIELD: No, it is already started.

MR. HOTZ: Thank you.

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(Viewgraph.) [Ref. 4-3-17]

MR. HARTSFIELD: We want to review the high criticality items. This is already in review. In addition, we are soliciting concerns from all elements of our organization. We have had each organization, for example, at JSC, the engineers in each specialty present up the line their concerns. What systems do I have concerns about? Those are being categorized and consolidated.

We are also reviewing this famous critical items list. We are going through those and seeing whether that list is properly validated. We're looking at each item individually. We may add to that list. We may not. When we get all through with this we're going to try to identify and prioritize our concerns and fixes.

One category, of course, is what have we got to fix before we fly? We know of one item already we want to fix before we fly again, and that's the seals. We've got to do that but, are there others? As an example, maybe the 17-inch disconnect flapper valve?

But all of these things have to be prioritized, and we have to use some good engineering judgment on which ones we need to fix. Some of them we may decide that the risk is acceptable as is; that that shouldn't be a concern for us.

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GENERAL KUTYNA: You got off the design and safety chart quicker than I thought you would. Let me ask a question in that respect.

In the design stage, the Shuttle had several crew escape and survivability features that were contemplated but for one reason or another weren't put on the vehicle that we have today. In view of our experience, what crew survivability and escape provisions would you like to see on today's Shuttle?

MR. HARTSFIELD: Well, I personally would like to see some sort of a low altitude escape system. This all fits together in a package; you know, some ability to bail out of the vehicle. We have abort modes that we call contingency aborts, in which we lose two engines, and the end result of that is ditching.

I personally don't think the vehicle would survive a ditching. When you talk about smacking the water at 200 knots with an airplane that is basically an airliner type design, I'm convinced it's going to break up. If you've got a 60,000 pound payload behind you, it's probably going to come in the cockpit with you.

So I personally would like to see something along that line. Whether we can develop such a thing reasonably or not I'm not sure, but there are options I would like to see us look at and review this once again.

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GENERAL KUTYNA: John, you've had some thoughts on this. Do you agree?

MR. YOUNG: I have been at this for a very long time. Back in the early 1970s, and this wasn't an idle situation, we went all over the country and we talked to people about solid rocket motors, and we talked to people about engines, and we talked to people about great numbers of things. They told us there was no way to do all these things and make them 100 percent reliable.

So at that time we did try to—we had ejection seats in for the vertical flight test phase. We got them put in for that flight phase, and then they came back out. Since then on numerous occasions we have talked to people about doing things like putting in bailout systems such as the tractor rocket system, or just plain bailout, and they have always seemed to be more than people could put up with.

But I really believe that manned space flight, manned space vehicles, if we don't do it for this one, for surely the next vehicle that we develop there should be an escape system.

GENERAL KUTYNA: But are there things you would like to see in this one?

MR. YOUNG: I would but it is not going to be

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a cheap type quick fix, I don't think, to do it to give you any reasonable chance for escape

GENERAL KUTYNA: Thank you.

CHAIRMAN ROGERS: This is one of the things that the Commission might want to—well, but maybe this isn't the right question to ask you but certainly it is one of the things that I think the Commission should consider. If we have views on it, we should make recommendations.

One of the things we would like to see from you are your recommendations about whether money should be appropriated for that purpose or not, and how soon could it be done.

Captain Young, do you want to address that?

MR. YOUNG: Sir, I think it would be touch and go to put any escape system in there before you fly. Again, depending upon how long it takes to get back up, which I really don't have a good feel for but it would be a tough proposition.

I guess if you put the right people on it with the right money and the right effort, you ought to be able to do it pretty darn quickly, but I'm not sure that we have that kind of capability at NASA.

CHAIRMAN ROGERS: What about in building a fourth orbiter? Should that be one of the things we

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consider?

MR. YOUNG: Yes, sir. I believe that would be a good idea for other reasons probably, too, yes, sir.

CHAIRMAN ROGERS: Do any other of you have views on that? We will be asked as a Commission.

MR. YOUNG: I'll tell you, there's a wide disparity of ideas on that in the Astronaut Office.

MR. CRIPPEN: Mr. Chairman, I guess, as John knows, he and I have worked this particular problem long and hard on the Shuttle, and it's more than money. It is a tough problem to solve technically with the vehicle that we have.

If you were going back from square zero and you went to some kind of concept like the F-111 cabin escape system, you might—and build it from the ground up, you might be able to do something like that. You might be able to come up with some kind of a system that would satisfy the requirement that Hank brought up of giving you capability to bail out in one of these contingency abort kind of things.

But again, I've said this before publicly and I'll say it again. I don't think I know of an escape system that would have saved the crew from the particular incident that we just went through. I don't think it is possible to build such a system.

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MR. HOTZ: Are there other incidents in which a type of escape system could have saved the crew?

MR. CRIPPEN: The one that Hank alluded to is a very real possibility. We have a large portion of the assent phase where if you lose multiple engines, multiple of your main engines, the only option is a ditch which puts it in the water, and it is questionable whether that is a survivable situation.

It would be possible or potentially possible that you might have time to get all the crew bailed out. That would be a tough problem itself considering you would probably have them spread out across the ocean coming out at pretty high velocities, but it is probably more survivable than ditching the vehicle

But that is about the main situation I think we would be talking about

MR HOTZ: How about your return to launch site abort mode where I know there are some concerns about whether this is really a feasible mode or not, and wouldn't some kind of an escape system like that function in the time that you might have when you were trying to make a return to, an abort and return to the launch site?

MR. CRIPPEN: That type of system would work in any kind of a landing situation if you ran into a problem where you weren't, from an energy standpoint,

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couldn't make the field or something of that nature.

But with regard to the RTLS, whether return to launch site abort mode, I guess contrary to what you just voiced I think that everything that we have seen says yes, that is a doable kind of a mode.

DR. FEYNMAN: I would suggest another possibility that has never happened that I hope would never happen. It is possible that the crew, because of some gas in the cockpit or something or heart attacks or whatnot, is unable to make the reentry completely, and so would it be possible to make a backup computer-driven reentry that could back up the crew if the crew is unable to operate and therefore save both the mission and as many of the crew as are still alive?

MR. CRIPPEN: Today, in the way the vehicle is designed and the computer software put together, we do not have any kind of capability to have it all done automatically or executed from ground.

That was one of the considerations in designing the Shuttle, because there was an element of the program that thought we ought to fly it completely unmanned the first time. In fact, I think Captain Young and myself were probably two of the opponents for doing that because we thought that putting the man aboard probably gave it more of a chance for success.

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I personally don't think the scenario you just raised is a reasonable one where you could have the whole crew totally incapacitated and such that they were going to end up surviving. But technically, yes, that is a feasible kind of thing to do but it is not in the current system.

DR. FEYNMAN: I think in fact it is fairly simple to do, although there are some things that are not built in that make it impossible right now that are very easy to fix up, such as the automatic lowering of the landing gear which can't be done now.

GENERAL KUTYNA: Can it be done by the ground?

MR. CRIPPEN: No, it cannot.

DR. FEYNMAN: That seems odd because it is such a simple thing to put in, and although the contingency is unlikely it is only thought of as a backup idea. I wondered what you thought of that.

MR. CRIPPEN: Any time you put in a system like that that can be automatically activated or automatically done by some other, you have built in another failure mode in it, and putting the landing gear down at the wrong time on this vehicle you can cost the vehicle. Consequently, it was a very conscious decision of ours.

Like I think it was pointed out earlier, most

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of the things are done by the computer. The gear lowering is strictly a manual electrical operation that bypasses the computer totally, and it was made as simple and as straightforward as we could to preclude any additional failure modes.

MR. HOTZ: John, I believe you raised the question in one of your previous discussions about the possibility of putting a thrust terminating device into the solid rocket boosters.

Would that have had any effect if you had such a device, that is short of total destruct and I'm not talking about marine safety packages, but would that have had any effect on the 51-L incident?

MR. YOUNG: No, sir, I don't think it would have been able to do anything for them. The manned orbiting laboratory program had thrust termination based on chamber pressures in the two solid rocket motors being different, and they would thrust terminate and get off, and they had a Gemini escape module. I'm not sure that thrust termination would have helped this situation.

MR. HOTZ: What was your point in recommending it?

MR. YOUNG: I recommended thrust termination back in the early days in order to avoid the range

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safety system problems that you have when you separate—when you activate the range safety package either on purpose or inadvertently.

I think the range safety package, if we have to carry one, should be one that doesn't tear up the whole piece of machinery, including the crew. I think with humans on a vehicle that the range safety package, if you have to have one, should be man-rated. That is just my opinion.

MR. HOTZ: Is this one, the one you carry now, man-rated? It is pretty destructive, isn't it?

MR. YOUNG: The solid rocket motors ride down the side and will blow up the external tank, and people are looking at that to see if we really should fly one at all on the system, I guess, through the range safety, through the range safety panels.

But I will tell you, we fought this long and hard to even have a range safety package on the vehicle in the early 1970s, and we were never successful to get it removed. In fact, we had sort of an unwritten agreement that when we took the ejection seats out the range safety package would come off, and it just never did.

MR. HOTZ: How do you feel about it now? Do you still feel that it should come off?

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MR. YOUNG: I was sure if the vehicle is reliable enough to go where you want it to go, I think the range safety package should come off. On the other hand, they've just got some data that says it is not as reliable as it should be.

MR. HOTZ: Thank you.

CHAIRMAN ROGERS: To come back to building another orbiter—because this is one of the things that Congress is obviously going to consider, and when we make our report it is one of the things we are going to be questioned about by Congress—is it feasible to provide for an abort system in the fourth orbiter, if you build a new one?

MR. WEITZ: You mean an escape system? A modular escape system?

CHAIRMAN ROGERS: Yes.

MR. WEITZ: I think probably not.

MR. YOUNG: I think you can put a low altitude tractor rocket system in there without too much trouble.

CHAIRMAN ROGERS: You could put a what in there?

MR. YOUNG: A tractor or a rocket escape system in the new orbiter without a lot of difficulty.

VICE CHAIRMAN ARMSTRONG: You're talking about a Yankee sort of a system?

MR. YOUNG: Yes, a Yankee system that takes

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out the top of the cockpit and lets people jump out that way.

CHAIRMAN ROGERS: Would that be a unanimous view of you gentlemen, or is there division on that?

MR. WEITZ: No, sir.

CHAIRMAN ROGERS: One of the reasons we're having these hearings is let's talk about the differences. In a free system that is what we do. Let's talk about them.

MR. WEITZ: Well, I think what we're down to is basically we have three alternatives, and these have been looked at at various times over the last several years and will be looked at again. In fact, we're starting to look at them again now.

You have an escape module. You have some sort of probably rocket-assisted personnel extraction where you use, as Neil said, the Yankee system, or you have some sort of bailout system. With the orbiter as it presently exists, it is really—you cannot modify the existing orbiter to accommodate an escape module, and so therefore we have considered these other two methods.

John likes the rocket extraction system because it does cover a wider flight regime and allows you to get out perhaps with the vehicle only under partial control as opposed to complete control; however,

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any system that—when you add more parts like rockets it gets more complex.

Another system is a bailout system which we have looked at in which you have to modify the side hatch or the top hatch, depending upon what you wanted to go out, so that it could be pyrotechnically blown off the vehicle to give you the route out, and you then hook up to your parachute system and bail out the side. What that says is you obviously have to be subsonic in stable flight.

So there are tradeoffs and it is not simple and straightforward in my mind, anyway, as to which way you really want to go of those two. That is why we have asked and have reinstituted a tradeoff study to be done on those two.

MR. CRIPPEN: I guess I'm very much for first building a fourth orbiter. I think that is the right thing to do. I think it is also important that we keep the configuration of the vehicles essentially the same. It is a real problem, and we've seen this previously.

Any time we get multiple configurations between vehicles we have got a training systems problem that is phenomenal, and somehow it doesn't make much sense to me that we would end up with one vehicle that

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had an escape system on it and three that didn't.

CHAIRMAN ROGERS: You all agree you couldn't modify the three existing orbiters to provide that?

MR. CRIPPEN: The only kind of a system that I'm aware of that I think is even somehow feasible would be maybe some kind of a bailout system that could be used subsonic, and we've looked at that. At this point in time we haven't seen fit that it was worth the complexity to go through it.

I mean, we can reevaluate that again, and I'm sure, as Paul says, we will do that.

CHAIRMAN ROGERS: I understand Dr. Ride's subcommittee is considering this matter and is going to have some further discussions next week about it, so her panel can discuss it further but it is going to be one of the things we're going to be questioned about by Congress.

We have already had questions about what we recommend, and we are in the position of saying we haven't come to any conclusions about it, but that is why this discussion is helpful.

Thank you.

VICE CHAIRMAN ARMSTRONG: It was mentioned a moment ago that you did not feel the orbiter had a good chance of a successful ditching, at least some felt that way. It is my understanding that some tests had been

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conducted in the past, but they may not be directly applicable to the orbiter and other tests were never conducted.

Would it seem reasonable from your collective points of view to beef up the understanding of whether or not an orbiter is ditchable because, as I understand it, you don't really know yet?

MR. YOUNG: $\frac{1}{2} MV^2$. There's a lot of that.

MR. CRIPPEN: I think that would be a very reasonable thing to do, because it is the sort of an unknown that we have in the program right now.

MR. WEITZ: I don't agree with that.

MR. YOUNG: I don't think it's an unknown. There's just no evidence that hitting the water that fast with the kind of ultimate crash loads that the orbiter has in systems associated with the orbiter—we've got a 20-G cockpit, but the stuff in the payload bay is a lot less than that, and the stuff in the nose is a lot less than that.

MR. WEITZ: When I said that I didn't agree with Crip, what I meant was my feeling is so strong that the orbiter will not survive a ditching, and that includes land, water or any unprepared surface, that such a study would frankly be a waste of money.

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I think if we put the crew in a position where they're going to be asked to do a contingency abort, then they need some means to get out of the vehicle before it contacts earth, the surface of the earth.

VICE CHAIRMAN ARMSTRONG: Do you mean the first test would be a real one?

MR. WEITZ: Yes, sir.

DR. COVERT: Could I ask a question of one of you, please?

What is your experience in terms of the maximum dynamic pressure you would like to bail out into considering the high adrenaline flow and all of those other contingencies of that kind?

MR. WEITZ: The mechanics of separating from the orbiter in a bailout, which means no assistance at all, requires you to be—if you go out the overhead hatch you would probably hit either the OMS pods or the vertical tail. If you go out the side hatch at air speeds in excess of much over about 220, 220, 240 knots, you would hit the wing.

DR. COVERT: The window is pretty narrow, then, in terms of the available—

MR. WEITZ: Yes, sir, because you're now trying to bring the orbiter down to a regime where you are at relatively high angles of attack.

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VICE CHAIRMAN ARMSTRONG: Was that partly due to the fact that there is no spoiler at the hatch?

MR. WEITZ: Yes.

VICE CHAIRMAN ARMSTRONG: Could that envelope, do you think, perhaps be improved somewhat if there were?

MR. WEI.Z: Intuitively I feel that it could, Neil, but we just don't know.

DR. COVERT: Still, there's a basic $^{1}{}_{2}{}^{2}$ that a person is reluctant to enter into at some point.

MR. WEITZ: Yes, sir.

DR. COVERT: So would you envision then that they would bail out wearing a helmet or some other sort of—

MR. WEITZ: Well, they would wear the basic flight—yes, we would like to minimize the number, the amount of additional equipment that is necessary in order to do the bailout, and so I would envision doing it with the flight equipment that we presently have for launch.

DR. COVERT: But bareheaded and barefaced?

MR. WEITZ: No, sir, we launch with helmets on.

DR. COVERT: Okay, fine. Thank you.

DR. WHEELON: This question goes in a different direction, if I may.

Earlier John Young spoke about the problems of mounting the flights in 1985 and the potential

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difficulty of going even higher than that in the future. This question builds on that and goes in a different direction.

Prior to January we had four orbiters. We now have three. Our former plan was to begin operating this summer both from Vandenberg and Kennedy. That presumed that we would have four orbiters, four Shuttles. We now have three.

Can each of you give a view on whether it is feasible to operate now from both launch pads, both Vandenberg and Kennedy, with only three orbiters and what strains that would put on the astronaut crews?

MR. CRIPPEN: I rea y think that that is primarily a programmatic question more than it is an Astronaut Office question, the last question you had. I don't think from our standpoint it is going to put any additional strains on us. Now, as to whether you can shuffle the vehicles around enough to do that, that is a different question and somebody else would have to address that.

I think that you can certainly operate still from both sides, especially if we're landing at Edwards. You have just got which way your Shuttle carrier craft goes when it leaves to take it back to the launch site.

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But with three you can't fly as much as you can with four. I think that's the basic answer.

DR. FEYNMAN: I think it is an astronaut-related question because it has to do with a kind of launch pressure. Whenever you have a decision whether to fly or not, there are other considerations: how important it is to fly in order to keep up the number of flights, and if you have too large a number of flights you are going to have a big pressure on the astronauts.

Would you then consider that in order to reduce this pressure it would be worth considering the possibility of having unmanned missions for those satellites and those operations which don't require men and thus take some of the load off the Shuttle?

MR. CRIPPEN: I guess going back to your original question regarding pressures, pressures are a normal part of the system and I think they are a normal part of most systems.

I think that the American public would want us to try to keep what we think was a reasonable launch schedule, that was still a safe schedule, and I personally believe that, contrary to the evidence of 51-L, that we thought we were doing that.

I don't think that our management nor our office really thought that we were trying to press into

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an unsafe situation, and I don't think that anybody would have made a conscious decision to go fly if they even thought that there was a possibility, if they personally thought that there was a possibility of that seal coming loose.

I guess I'm a big proponent of manned space flight, naturally. I think we have done a super job in the past of showing that we can deliver satellites to orbit with the Shuttle. That doesn't mean that there is no place for expendable boosters. I think that the proper answer is really what we have now, and that is a marriage between the two and that program management needs to decide what the balance should be.

I personally am biased to go over toward having four orbiters and a reasonable launch rate with those four orbiters and supplementing it as required with expendable boosters if you need it.

DR. FEYNMAN: That is not what I meant. I agree. I think there will be enough manned things to take care of the Shuttle but we should be sending up things on—the question is whether we should be sending up things on a manned Shuttle when we could have done it without them because of the new pressures, that is the new total number of things that we want to do.

MR. CRIPPEN: Again, I guess I think using man

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to deliver satellites to orbit is a viable capability that we can and have used man for and should in the future, personally.

MR. WEITZ: I think the key to the whole thing is manifesting or scheduling, whatever you want to call it, your resources in a manner that you feel confident you can support.

It is my personal opinion that in the past that we have basically, for different reasons, wound up requiring what appears to be 110 to 120 percent of your resources. That serious deficiency in the program, I feel, is a serious lack of spare parts but what this requires is frequent cannibalization of other orbiters at KSC in order to properly outfit the one that is going up.

All this does is really add work to the flow at the Cape. It increases the exposure of both orbiters, the one you're taking the equipment off and the one you're putting it on, to intrusion by people. Every time you get people inside and around the orbiter you stand a chance of inadvertent damage of whatever type, whether you leave a tool behind or whether you, without knowing it, step on a wire bundle or a tube or something along those lines.

I think, and it is pointed out in Admiral

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Truly's memo, that what we need to do is look at it from the bottom up. We really ought to decide if we have a four, three, whatever the number is, airplane, air force, we've got to decide what it is we can do and those types of things that it makes sense to utilize this resource for.

If it turns out that man can do many wonderful things in orbit, and I think we have demonstrated that, but we've really got to look at what it is we're using a very valuable national resource for.

MR. HOTZ. Could we move along to some of the other flight safety concerns on the current orbiter fleet, such as the microwave landing system and your crosswind problems and your brake problems?

Could we just poll the delegation and get a cross-section of your views on those problems?

MR. WEITZ: Henry, we haven't heard from you in a while.

CHAIRMAN ROGERS: Mr. Hartsfield, would you rather go ahead with your presentation, then we can come back to some of these questions?

MR. HARTSFIELD: Well, we've sort of talked about what is here. I might go through this rather quickly, and I would like to because there are a couple of points I would like to make.

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CHAIRMAN ROGERS: Okay, why don't you go ahead and continue.

MR. WEITZ: I think we're going to touch on Mr. Hotz's subject later.

CHAIRMAN ROGERS: It might be well to take them one at a time, too, as we go along.

MR. HARTSFIELD: This is just the approach one takes.

The next thing we've already talked about here, alluded to, is taking a look at our maintenance, our turnaround. We're doing that; we're looking at the requirements—

(Viewgraph.) [Ref. 1 3 18]

MR. HARTSFIELD:—and checking what the requirements say about the high criticality items that we discussed previously, and then we would see if we could trace these requirements to the turnaround procedures themselves, the test and checkout.

I personally think that there is already some mechanism in place, but I think we ought to assure that in all of our checkout procedures that when we are checking some Criticality One item that that is especially flagged in the procedure itself so that the workers involved know that, hey, this thing needs close scrutiny.

CHAIRMAN ROGERS: You're reviewing that whole

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question of Criticality One and what waivers mean and so forth, I assume?

MR. HARTSFIELD: Yes, sir. The system is doing that, and we have, for example, people from our office sitting with groups, for example, going through our maintenance instructions.

CHAIRMAN ROGERS: Yes, I meant the system. You are involved in that? The astronauts are involved in that review?

MR. HARTSFIELD: Yes, sir.

CHAIRMAN ROGERS: Good.

MR. HARTSFIELD: I think we need to take a look at our waiver mechanism as well, because many times checks are called out and then for one reason or another we want to waive that for this particular flight. It was due to be inspected, but let's put it off and just make sure that we have our process going right on the waivers and that the right people are being approached.

(Viewgraph.) [Ref. 1 3 19]

MR. HARTSFIELD: Now, that is the maintenance side of the house. In the next chart I get into what I think we ought to be doing to the OPS side, and it is the same sort of thing, and we're in the process right now, for example, of looking at all our flight rules.

We have a set of rules that you may have been

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told about that are general in nature of how we operate the orbiter and what we would do in light of certain failures or contingencies, and we try to think all of this out ahead of time so that if the case presents itself during a flight, that the flight team has a course of action that they are going to follow.

We don't always, I might point out don't always follow this course of action but the flight rules are a departure point. At least all the thinking has been done in advance under more carefully controlled situations and not in the excitement of the moment.

These flight rules, of course, are going to be measured against, again, what we think the critical items are in the critical systems.

I have the training hat on in the office now, so I am in the process of re-evaluating our training that aspect of the thing to see what I think about the crew workloads and the preparation for flight. There is some evidence, for example, that the demands on the crew, the training process, gets very heavy. Well, it is not evidence. It is there.

About two-thirds of the crews that have flown so far have made statements regarding that they felt there was a time compression in the training in the few weeks before flight, and I think that is something that

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we are re-evaluating and saying we are looking at what we can do to offload this work period so that the crew doesn't go flying tired.

There is going to be a little tailup at the end, naturally, because that is when you get your final flight software and you're trying to hone your skills to the highest level, and so you're going to intensify your efforts closer to flight. But some of us have some concerns that maybe this load is getting a little bit on the high side and we ought to look at turning that back down.

CHAIRMAN ROGERS: What is the age limit of an astronaut, or is there one?

MR. HARTSFIELD: Ask John.

(Laughter.)

MR. YOUNG: No, there is no age limit. As long as you're physically qualified you can still fly.

CHAIRMAN ROGERS: Which astronaut was the oldest when he flew?

MR. YOUNG: Dr. Karl Henize.

CHAIRMAN ROGERS: Do most astronauts—and you may not be able to answer this, but do most astronauts think of their career as astronauts, or would some of them like to move into management?

MR. HARTSFIELD: I think from my standpoint

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flying spacecraft is a very interesting job, and I think a lot of people would.

CHAIRMAN ROGERS: I notice there aren't many astronauts in management, and I've been impressed with one astronaut that has recently gone into management, and that is Admiral Truly.

I wonder if more opportunities should be given to astronauts to move into management? (Laughter.)

MR. ABBEY: I know I get a lot of complaints about using them the way we do now, but I think I would certainly agree with that. I think we have had astronauts, and I was going to touch on that a little later in a number of management positions, and I think they could provide and could contribute a lot within the program office.

Frank Borman, for example, after the Apollo accident in 1967, took over the redesign effort and ran that until it came about before we went to fly again, and I think we could well do that again with an astronaut; put him on accomplishing all of this effort within the program office.

CHAIRMAN ROGERS: Does the management make that possible? Do they encourage that development from the astronauts to management?

MR. ABBEY: Within the last month I think we

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have felt or I have felt that we are getting a lot of encouragement to do that.

CHAIRMAN ROGERS: Thank you.

DR. WHEELON: A followup question to the previous comment. You indicated that at the present rate you are pretty busy, perhaps too busy I think you implied.

Can you identify the number of flights that you think it is safe and prudent to get off with three orbiters per year?

MR. HARTSFIELD: Well, I can't do that and I think it is only by going through this process. I have a chart that addresses the flight rate here later. The flight rate has to be determined, as P.J. said I think, from the bottom up, and I think that is the way Admiral Truly is going to approach it.

We need to determine how often we can fly the orbiter from the standpoint of resources, and then we will look at how we support it. I know that we can from the crew's standpoint.

CHAIRMAN ROGERS: How is the decision made as to which astronaut should fly on a particular launch?

MR. HARTSFIELD: A lot of us wish we knew that.

(Laughter.)

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MR. YOUNG: It is primarily a rotation basis, and depending upon what missions are up and what people are coming due for training. It is really nothing magic about it. It is sort of straightforward. Once a person flies they are put back in line and they are supposed to get to fly again, and there may be some reason why they don't but it is very rare.

CHAIRMAN ROGERS: So it is just an ordinary rotation system?

MR. YOUNG: Pretty much.

MR. ABBEY: There might be some special mission requirement on a particular flight due to an EVA or due to a rendezvous or some special aspect of the flight.

MR. YOUNG: Department of Defense missions are all military.

CHAIRMAN ROGERS: But who makes the decision? You, Mr. Abbey or Captain Young, or a committee or what?

MR. ABBEY: I think as far as determining who flies on any particular mission, I rely very heavily on the input I get from Paul Weitz and John Young, and really that is kind of a mutual thing that we come up with, depending upon what we have to do on that flight and, as John says, what the rotation is and who's available.

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I think John has—

MR. YOUNG: Sometimes people come in and ask if they can fly a mission, and we try to honor that if we can do it. Nobody believes that, but it's true.

(Laughter.)

MR. ABBEY: Probably John has the biggest input as far as that goes.

MR. RUMMEL: I have a question relating to workload that lies in a somewhat different area, but first I have to ask this.

As I understand it from what has been said and inferred today, the astronauts are indeed given the opportunity to inject the lessons of experience and their opinions in new development programs, either redevelopment of the Shuttle or new things coming onstream. Is that correct?

MR. WEITZ: Yes, sir.

MR. RUMMEL: Okay, well, my question then has to do with workload and available manpower concerning new programs that might affect the efficiency or the conduct of the Shuttle program.

One that comes to mind would be the space station, for example, which is a major project. That is coming onstream, and are you able to cope with the kind of workloads that that has imposed or will impose

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concerning manpower and the injection of the experience which at least I think is most important that it be done?

MR. ABBEY: Prior to the accident we had approximately one individual in the office working on space station, and then we would draw in other individuals as they became available.

So with the workload that we had prior to the flight, we were not able to put a lot of people into the space station definition.

MR. RUMMEL: Well, does that suggest that this is a budgetary problem of some kind or not?

MR. ABBEY: I think it was just a manpower problem on getting experienced people that could be available to contribute to space station. Most of those people were tied up getting ready for flights or going back into preparations for our mission, so we just didn't have I think sufficient crewmen to make available at that time.

MR. RUMMEL: Do you contemplate that that sort of—I hate to use the word but—deficiency will continue?

MR. ABBEY: No. I would hope that we would be able to free up some experienced crewmen and get them involved in that effort.

MR. RUMMEL: May I ask in this general regard,

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too, and take the space station again as an example, in coming to the policy decision to move in that direction, which is probably superb, if various alternatives were also considered, that is the general direction in which the future space effort would go? Are you folks involved in those processes, too?

I suppose if you are at least some degree of manpower loading would be required. Can you comment on that?

MR. YOUNG: Kathy Sullivan is on the National Space Commission, and she has been taking our inputs into that thing probably a lot of the time.

MR. RUMMEL: Do you do your own manpower planning in terms of the numbers of people?

MR. YOUNG: Yes, sir.

MR. RUMMEL: You do. Thank you.

MR. YOUNG: As best we can. It is a very dynamic situation most of the time.

MR. RUMMEL: Thank you.

MR. HARTSFIELD: One area I think that we can improve on, and I've got it under the bullet of mission planning milestones, is we need a little bit more, I think, stability in our manifest. We need to freeze the basic manifest very early so that the products that have to be generated to support a mission can be delivered on

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time

Now, by products I mean the trajectory data that has to be built, for example, for a flight and the mass properties of the vehicle that go into the software load that is used to fly that flight. There's a long lead time on that to build that, and if we take something off the flight or put something on it after that process has been started, then that is just like a square wave in the system and they almost have to go back to square one and start over again.

We have had cases where the actual flight software was delivered within a week of the flight, and in several occasions two weeks of the flight, which some of us feel is just too doggone close to the flight to see the final software. We are looking at that now, and I think that we need to be a little tougher in defining the flights.

One other aspect of that is late add-ons to a flight. Now, in some respects it is very easy to say, well, gee whiz, I've got a place to put this little thing and I want to put it on the flight, and it is no big deal. We've got the weight margin. We've got the stowage.

But on the other hand, late additions to a flight, even though they seem innocuous enough, they

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still have to go through the safety reviews that we conduct to make sure that there are no materials problems, and it is safe to fly the item.

But in my mind it detracts from what we ought to be concentrating on close to flight. When we're getting within a couple of months of flight we ought to be thinking about the flight we're going to execute and not worry about, well, we've put this on, now we've got to change the procedures, or we've got to put a place in the time line to do it.

What I'm getting at is we'd like to see more stability in the last couple of months toward the flight, and I think we can do a better job there. We certainly are evaluating that now.

(Viewgraph) [Ref. 1/3-20]

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VICE CHAIRMAN ARMSTRONG: I have a question on the section training crew and training. What is the status of the load on your simulators, mission simulators and so on, let's say over the past year before the accident?

MR. HARTSFIELD: You mean how many hours are we getting on them?

VICE CHAIRMAN ARMSTRONG: No, in terms of is it fully loaded or overloaded?

MR. HARTSFIELD: Yes, all of the above.

VICE CHAIRMAN ARMSTRONG: It is heavily loaded. You're simulator limited?

MR. HARTSFIELD: We are pretty much so, and in fact that is one when we get through with this kind of overview we were going to address a number of subjects and that is one of them that I would like to talk about.

MR. HOTZ: Could you elaborate just a little bit on the software deliveries on time? Are you experiencing problems with that now?

MR. HARTSFIELD: We have had those problems, yes, sir. For example, on my last flight, 61-A, we got the flight software about two and a half weeks, as I recall, before we flew the final software.

Now we had had previous loads but because of launch date changes and trajectory there wasn't too much trajectory, when you change the launch dates, you change the time period with the general weather

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patterns. There's a lot of things that go into building the flight software. We had to redo it. In fact, we had three software versions built for that flight. Normally we only build two.

We had to build a third one, and that forced me, because I wanted to see the software, to get involved in the actual checkout of the training load in the simulator, which meant taking the crew and going over late at night when they normally do these things and going through it.

We have, starting with the next load delivery, we have started, because of this, and I'm putting on my training hat here, we've instituted a process to allow the crew to look at the software earlier. One of the things we worry about is shelf life on the software, is what we call it.

In other words, we would like to fly this final software and use it enough so that if there is some little bug that has crept into the software or some bad I-load, that we catch it before we fly. We feel that the more eyes that are looking at the software, the better off we are.

The next chart gets to the thing that I think really is the key in the accident, and that is communications. Apparently we had some sort of

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breakdown that the word about the seals certainly didn't get to the right places. It didn't get to JSC in any sort of a fashion that I'm aware of even to our Level II people, and that is something we have got to correct. We have got to get the communications.

I think basically, as Bob has said, we've got a good system but it's only as good as the data that gets into it.

CHAIRMAN ROGERS: Well, we have some reservation about the last comment on whether the system is good. I think the system is probably good as described here this morning, but the system according to the people at Marshall, they complied with the system. They say we had no obligation under the system to do anything that we didn't do. It was a Level III question. As they say, we worked it and therefore we didn't have any responsibility under the system to do anything else.

If that is the system, that's wrong. That is not a good system.

MR. HARTSFIELD: I couldn't agree with you more. Personally, I think that is wrong.

CHAIRMAN ROGERS: So to that extent if that is the system, it's not a good system.

MR. HARTSFIELD: Let me clarify. When I said

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the system I meant the system as we're working it at our Ops side is good, but this part of the channel apparently has broken, or it certainly was broken for this problem. I think we have to correct that, and we are going to correct it. I don't think we can afford to let something like that continue.

CHAIRMAN ROGERS: It seems here, speaking again for myself but I think most people on the Commission agree with me, that if that is the system that there is no obligation when a critical matter is involved and the contractor or the engineers and the contractor have serious questions about it. There is no—under the system there is no responsibility to convey that information to Level II or Level I. That is a flaw of the system.

It may be that that is not the system. Maybe the system is that they should have conveyed it. If that is the case, I think that is a failure of those involved to comply with the system. Either one is wrong, and it seems to me we have to be sure that we correct both.

MR. HARTSFIELD: Well, that's true. The system obviously depends upon people. I mean, the people are the weak link.

CHAIRMAN ROGERS: But going back, they say no. They say—the people in Marshall say no, we

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complied with the system. We did exactly what the system told us to do. We considered this, and we had a Telefax, and we didn't have any obligation to tell Level II or Level I.

If they are correct at that, that is a flaw of the system it seems to us. We must see that on matters of critical importance of that type, the system would require that that information be provided to Level II and Level I, and all of the astronauts should be aware of it.

Now, maybe that isn't the system. I think that probably Mr. Aldrich and others will feel that that was not the system. The system required them to tell Level II and Level I about this problem.

Mr. Aldrich did say in that meeting that I referred to at 2:00 on the day before the launch, if there is any considerations about weather that we should know about let me know. So he apparently relied upon that instruction that anything to do with weather concerns, including the joint or anything else, should be conveyed to him, and it wasn't conveyed to him.

Anyway, I think the Commission has received a lot of information about the lack of communication, and I think we are in a position to make some recommendations that will be constructive.

MR. HARTSFIELD: Well, good, because the

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[illegible]

CHAIRMAN ROGERS: Well, I'm glad because there again that is one of the things we are thinking about recommending is some kind of independent safety panel. We would be very happy to get your views on that because we think it is very important and we're not quite sure how that should be set up, but I think all of us believe that there should be an independent safety review or panel of some type, so we look forward to that.

MR. HARTSFIELD: All right. You mentioned flight rate, and I think my next chart addresses that.

(Viewgraph.) [Ref. 1 3-21]

MR. HARTSFIELD: I think we've already discussed the bottom-up approach, so there is no need of belaboring it, but the flight rate has to be established on what our capabilities are. With this

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review we're doing on our test and checkout procedures, we're going to come up with what it takes to turn the orbiter around, and then we play that against what resources we have.

How many people do we have working at KSC, and what do we have at the Cape? Do we have to share those people and move them back? I don't know how that is set up, but it boils down to resources and what we have to do.

Certainly initially, as Admiral Truly has said, we are going to establish a flight rate based upon those kind of inputs, and then as we start flying we may be able to increase the flight rate.

and I certainly hope we would but that has got to be based upon, again, what our capability is and I say new reliability history.

What I really mean is as we get more information on the systems, maybe for example we're checking the system after every flight and we never see anything wrong with it, and maybe through agreement we can say maybe we'll check this on every other flight, that's just an example or we might find something we're checking every fifth flight is showing some problems, and we ought to increase the check rate.

So this is a give and take, but we only can expand the flight rate or increase it based upon our experience.

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Well, now, we'll get to the chart you're all looking for, the summary.

(Viewgraph.) [Ref. 1/3-22]

MR. HARTSFIELD: I think we're on the right track. I really do. I think we are doing the right things. The only thing I'm concerned about is that we don't stop and we carry them right on to fruition and we look at what we need to do before we fly.

Despite what has happened, I still feel that we've got some of the best people in the world working at NASA. I know that they're some of the finest people I've ever worked with. They're dedicated people, and they take a lot of pride in their work. They were just as much hurt by this accident as we were, and they want to get us on the road again, and I think they are. I've got faith in them.

There is one thing that I would like to leave here, and that is in that last "but" down there. We've got to be very careful. I'm concerned that the cure may be worse than the illness. I don't think we ought to run off doing some half-hearted—or let me put it this way—hasty fixes, thinking we've got to go do this and let's throw it on the bird because that's going to fix this problem. Sometimes we find that when you do those kind of things what we just put on there is more of a

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problem than the system we had.

So we need to fix those things that we have to fix that we've identified through this review process, like the seals. It may be that, as I said earlier, that some things we might want to live with by very careful quality control while we research the fix, and we need to fix it but let's don't be very hasty about it. We need to very thoroughly and rationally evaluate all of these things.

So that, to me, is a concern and I don't know whether that is shared by other folks. I think it is.

(Viewgraph.) [Ref. 1/3-23]

MR. HARTSFIELD: My final thoughts are that I think we have got to accept—this has been discussed before, too—that the STS is certainly not an operational system in the traditional sense, and it can't be. Certainly for those of us that fly it, it will never be routine and there will always be risks associated with flying in space. We will fix all these problems and go fly again, but it is still going to be a risky business, and I think everybody should remember that.

The bottom line is, though, that this daggone vehicle is probably the most magnificent and fantastic machine I've ever seen, and it is something that we all ought

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to be proud of. It has capabilities that are totally unmatched anywhere in the world. There's nobody that has a machine like this, and if we use it properly I think it can do a great deal for this country.

I think that is the way. We've got to exploit the things that it does well and carry on.

DR. FEYNMAN: I would like to make some comments about that. I agree with you, that it is the most fantastic machine and so forth, but one has the possibility of forgetting something in the process, and that is what you kept talking about, accepting risks.

It is, therefore, important to understand that although it is the most wonderful and fantastic machine as you mentioned it, but I'm just trying to emphasize it for you, and that it is a risky machine and has flaws. It has difficulties. It is not complete. It is not perfect. It is not operational exactly.

I'm trying to figure out where the difficulty is in this system that made it go wrong, and I think you may be addressing it later when you're talking about the safety panel, but let me suggest something and see if we agree with what your questions are.

The problem is communication, and the communication will be fixed if you have the safety panel, if there is a member of the astronauts on the

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safety panel, because then you will be fully aware of all the things that are unsafe.

So the communication problem in the safety panel, as long as it has astronaut representation on it, will automatically fix each other with regard to an understanding of what the real risks are.

Because the idea that you accept risks, the consideration of this thing is always during flight. It is a flight review, and so you decide what risks to accept. I read all of these reviews, and they agonize whether they can go even though they had some blow-by in the seal or they had a cracked blade in the pump of one of the engines, whether they can go the next time or this time, and they decide yes. Then it flies and nothing happens.

Then it is suggested, therefore, that that risk is no longer so high. For the next flight we can lower our standards a little bit because we got away with it last time. If you watch the criteria of how much blow-by you're going to accept or how many cracks or how long the thing goes between cracks, you will find that the time is always decreasing and an argument is always given that the last time it worked.

It is a kind of Russian roulette. You got away with it, and it was a risk. You got away with it.

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but it shouldn't be done over and over again like that. When I look at the reviews, I find the perpetual movement heading for trouble.

So I would like to know if by a safety review board you mean this, that there should be after each—there should be during a flight review a permanent place, the safety board or whatever you want to call it, which rides herd on that difficulty and tries to get rid of it as quickly as possible and actively for the next flight.

I think that is what is missing in the system that you say is so good. I think we haven't got a direct action, positive activity, someone whose responsibility is to work as hard as possible to keep everybody awake to the last thing that had to be accepted.

Would you agree with something like that? Is that what you meant by your safety board?

MR. HARTSFIELD: Yes. I think you've said that very well. That would certainly be my concept of the way the system would work. The safety panel, he has no axe to grind. His only interest is the safety of the vehicle and mission success, and they go hand-in-hand.

CHAIRMAN ROGERS: Well, thank you very much. I must say that is an excellent presentation.

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We would like, if we may, to adjourn for lunch and then come back. I don't think we can finish before lunch, and so I would like, if it is okay with you, to take an hour's break and then come back at quarter after one.

(Whereupon, at 12:20 p.m. the proceedings in the above-entitled matter recessed, to reconvene at 1:20 p.m. this same day.)

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AFTERNOON SESSION

(1:20 p.m.)

CHAIRMAN ROGERS. On the record. Mr. Abbey.

MR. ABBEY: We were kind of at the point where we wanted to address some specific concerns so, if we may, John was going to bring up at least two and then we would go from there.

CHAIRMAN ROGERS: Fine.

MR. YOUNG: We have been talking about some concerns we have had in the Astronaut Office for many years, and one of them is the prudence of landing the vehicle at Edwards or Northrup strip complexes. The reason is because of the things that you learn about both the vehicle and the system when you start using it. I will try to explain that. I'm a Florida boy myself. I always thought that the program should land the orbiter at Kennedy.

Over the past five or six years what has happened is we have come to some very different conclusions based on learning about the environment that exists in that world and learning about the limitations of the orbiter that we have in that environment.

One significant difference in Florida is the difficulty of accurately forecasting the occurrence of thunderstorms, fogs or crosswinds for an end-of-mission

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landing. You have to do that about an hour and a half prior to landing the vehicle, and that is a very difficult and complex job because of the dynamic environment that Kennedy area presents.

The orbiter requires much better weather than you might imagine to be able to make reasonable approaches and landings. We're looking for ceilings with a microwave landing system in excess of 8,000 feet so that the crewmen can make the proper corrections in case those things are not working just properly.

Then we're looking at crosswinds not very high, because right now with the vehicle we have, we have a system that is single string to its nosewheel steering. There are numerous failures that can cause you to be no string to its nosewheel steering.

We have a brake system on the orbiter that has—that is very heavily loaded and is sort of energy limited. It is very difficult to use precisely right now. In fact, we're finding out we don't really have a good technique for applying the brakes, and on one landing we were told that we put the brakes on too long at too high an air speed and kept them on too long. Then the next landing, that was perfect as far as we were concerned. We were told we put the brakes on too short and kept them on, and put them on too hard.

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Well, that is a very strange thing. We don't believe that astronauts or pilots should be able to break the brakes, and that is sort of what has been happening to us. When you land at Kennedy, with the tires that we have because of the runway, the vehicle is heavily loaded during rotation. As it pitches over the elevons come up and the main tires are heavily loaded. If either one of those tires has leaked down on you which you don't know right now because we don't know once we get the vehicle up in the stack whether the tires have leaked or not

If either one of those main landing gear tires have leaked down on you, the information is that the next tire will fail. When that happens, if you don't have nosewheel steering, with the simulations that we have run and any amount of crosswind at Kennedy in excess of ten knots, the data shows that you have trouble keeping the vehicle on the runway. These are from simulations, not from anything that is new.

So we think we've learned, at least as far as we've gone so far, how the vehicle operates. We are pushing to make our nosewheel steering more than single string so that many failures can't take the nosewheel system down.

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We found out also that handling qualities of the nosewheel steering are very sensitive to what kind of tire model we use. We're running the simulation right now at Ames, as I told you, and we found that this vehicle that we have now with the new tire model which is based on Langley test data does not handle nearly as well as the system that we used last time with the old tire model.

So now we're having to tune the general purpose computer handling qualities to a new tire model, and I'm not sure that when we land on the real runway for the real first time that we won't be looking at a totally different tire model.

At Kennedy the runway surface is very rough in a high crosswind, which it is difficult to predict what kind of crosswind you're going to have when you start out. You may want to limit your end-of-mission crosswind to ten knots, but when you arrive there an hour and a half later it could be something different than that.

In a high crosswind it tends to scrape the cords off the tires, and that is very hard on tires. The runway is surrounded by a moat, and depending upon how much rain you've had the water could be pretty close to the runway. It doesn't meet Air Force runway

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standards. If you have certain failures, it's going to be very difficult to make the runway.

One failure that we talked about the other day that might hurt you on the end of mission is an early geardown situation. That requires two failures to get it but if that happens to you while you're up around 180 it is equivalent to having halfspeed brakes all the way down, and unless you have visual contact with the runway and when the 180 degree position is 23,000 or 24,000 feet—excuse me, 28,000 feet, it might be difficult to get there. We think that would be very bad.

On the lake bed complex like Edwards, with 20 miles—that is 20 miles long and 7 miles wide, you don't have that problem; or at White Sands, where we have two intersecting runways that are the equivalent of being 29,000 feet long and 900 feet wide.

We just think it would be more prudent and safer for the program to take this vehicle and land it at the runway complexes for end of mission. What I mean, I think it would avoid some of the risks associated and make sure that we get the vehicle back every time.

Now, we know we spend a lot of program resources on building up the Kennedy system and making

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it possible for us to land there and that we do have to land there and the times that we have return-to-landing-site abort because there's just nowhe e else. but if we ever have a landing accident—and that was really the first accident that I thought we would ever have going into Kennedy. We spent a lot of money on that place and we did a lot to it, but if we ever run off the runway at Kennedy the repair bill is going to be probably enough to build five or six more runways there at Kennedy.

So we think the lake bed complex, the dry lake bed complexes would be much better for the overall good of the space program. That is kind of what we recommend.

MR. RUMMEL: Has consideration been given to any sort of landing barriers at the end of the runway; large, very strong nylon nets or whatever to take care of or help take care of overruns?

MR. YOUNG: Well, there again, sir, you would be looking at some vehicle damage. We are looking at barriers and barricades but what kind you want to have and how big you make it and how do you avoid getting any damage to the vehicle is certainly a consideration.

People are looking at barriers. I think that they would be more applicable to trans-Atlantic abort sites where your runway lengths are limited and you are

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heavyweight.

MR. RUMMEL: Yes, that is why I asked, because as you indicated there may be no alternative except to land at Kennedy.

MR. YOUNG: Well, that is certainly something that people are looking at. The difficulty with the barricade or barrier is to design one that this geometrically shaped machine will go through and not hurt everything so bad and slow everything down at the same time. It is really a tough engineering problem.

MR. RUMMEL: Yes, it is.

CHAIRMAN ROGERS: What about the weather considerations? What is the effect of rain on the orbiter? If you landed at Kennedy and a sudden squall came up that was not predicted, what would the effect of rain be on the orbiter?

MR. YOUNG: Well, Mr. Rogers, I'm sure glad you asked.

(Laughter)

CHAIRMAN ROGERS: I didn't realize you had this, John.

MR. YOUNG: Here's a visual aid. That is the effect of 10 to 15 seconds of light to moderate rain on the real orbiter tile, and I think the repair bill for doing that to the whole orbiter, if it didn't hurt the

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lift-to-drag ratio very bad, would be—I mean the turnaround time would be unbelievable. That is why if you can't predict when a thunderstorm is going to arrive at the Cape, it is better to avoid that problem.

CHAIRMAN ROGERS: What about the safety factors though, aside from the damage to the orbiter? Is it safe to land the orbiter at Kennedy if it's raining?

MR. YOUNG: Well, you have a wet runway but the runway is highly grooved, so it might be safe. I would be more worried about the other part of the environment. If you ever came through a rain like that—I mean, you're not going to save yourself any turnaround time because you're talking about many days of serial time to repair the damage. That would be what you would be concerned about. I would be concerned about getting that damage all over the vehicle and picking up maybe 100 or 125 or 130 drag counts and then having a wind situation where it wouldn't be able to make the runway.

CHAIRMAN ROGERS: That is what I was asking about.

MR. YOUNG: The engineering people say it would only be half that is the most drag counts they could ever get, but I don't know if they've ever seen

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any damage like that I mean, I don't know if they were really—

CHAIRMAN ROGERS: How good are the weather predictions in Florida in terms of quick squalls like that that might cause that kind of damage?

MR. YOUNG: One day we took Mr. Walt Williams out to give him a flight in the STA, and there was one little thunderstorm sitting 13 miles off the end of the runway. Thirty minutes later there was a squall line across both ends of the runway. Before we went out we checked with the weather and they said there wasn't going to be any.

That is not unusual, and it wouldn't hurt a regular airplane. You wouldn't care at all. You would just go right ahead and fly but you worry about that with a real orbiter because of the problems associated with those kind of events.

CHAIRMAN ROGERS: What choice do you have once the decision is made to land at Kennedy and you have an hour and a half to go? I mean, is there anything you can do during that hour and a half period if the weather changes?

MR. YOUNG: Once you have been given the go for deorbit if you lose communications or if you don't have communications right up to the time you deorbit, which is a little over an hour, maybe an hour and a

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minute prior to deorbit—we waved off Crip one time three minutes prior to deorbit on STS-7 or 41-C, and they reported to us the weather was going to be clear at the time of landing.

At the time of landing, there was 11,500 foot rain showers over the end of the runway, so it's a difficult problem, and I think we were about three minutes away from having Crip land in some pretty interesting rain showers.

CHAIRMAN ROGERS: Once deorbit occurs is there any option left?

MR. YOUNG: No, sir. It is not like an airplane where any time you go somewhere in weather you always have an alternate. You are committed to land on one end of the runway or the other end of the runway. You can swap runways maybe from about Mach 6 which is 12 minutes prior to deorbit is when they would like to do it—I'm sorry, 12 minutes prior to landing, but that is about the extent of your capability in terms of going to an alternate.

MR. HOTZ: John, do you have any crossrange alternates?

MR. YOUNG: No, sir. We have talked about that, but the problem you get into there is that's even more harmful to the program. Suppose you did have a

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crossrange alternate and you ended up in Orlando, for example. It's perfectly safe, but then you're looking at a long time to get your machine back to Kennedy. What do you do, close the Beeline and tow it?

(Laughter.)

MR. YOUNG: That would really be a tremendous problem to do that. You would probably have to chop up some overpasses and stuff.

MR. HOTZ: But it would be a safety alternative?

MR. YOUNG: It could be, but I'm not sure. Would it be worth that risk? I mean, would it be worth it to the program to slow it down that much? I don't think so. I mean, I think you would be better off flying it into Edwards and bringing it back in four days or so, and that way you wouldn't have to worry about that safety alternate.

CHAIRMAN ROGERS: What conclusions do you draw from what you've said, that you should not land at Kennedy except in an emergency?

MR. YOUNG: I think for a return-to-landing-site abort it's the place to land because it's the only one there is, but the rest of the time I think it would be in the best interest of the Space Shuttle program to land at one of these lake bed complexes

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because of all of the reasons that I've given there, and I left out some others.

For example, there are nine—there are 14 different procedures that crews have to use during breaking and rolling out, and there are seven other safety-critical procedures they must use during breaking and rollout. These are different from what you use.

I mean, we have to train the pilots to do that using simulators, and the only one that is really a valid simulation of that kind of thing is the Ames Research Center simulator. It's a good simulator, but you just can't go one time to a simulator and do all the rest of your training somewhere else and say that you know how to do it because you revert to your old habits.

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CHAIRMAN ROGERS: Do any of you other gentlemen have any other views on this subject?

Mr. Hartsfield?

MR. HARTSFIELD: I tend to agree with John. I for one began very much as you did, thinking that as soon as we get things squared away we ought to land at the Cape. I have since changed my mind, based primarily on the fact that we have got a lot of problems with the brakes and the nosewheel steering. The problem that I have with the Cape, if those were fixed, if we had a good, redundant nosewheel steering system and brakes that we could depend on, that were more natural to operate—and incidentally, let me point out that we are pushing the state of the art on these brakes, and so it is not an easy solution, but then, the weather factor is certainly something to consider. I know that of all my years of flying at Edwards, that the weather is very predictable there, and it is generally good. When it is good, it is going to be good, and you've got a complex of runways to choose from, and we can make a last minute change of runways, for example, if the wind shifts on us, and put the bird more into the wind to avoid the cross wind problems.

The thing that bothers me about the Cape is the weather. Even the first flight we took in there,

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STS-11, I believe it was, if you look at that, it is a little frightening. You see patches of ground fog all over the place, and they're pulling streamers off the wingtips as he makes his final flare to land, and that wasn't predicted. So we were just that close to him coming back to a socked in airport and I don't think we want to risk that kind of thing.

MR. CRIPPEN: I don't think you would get any pilot in the astronaut office to disagree with the basic premise that you are much safer landing at Edwards. There are some things you could do, as was indicated, to make Kennedy better, but you're never going to overcome the weather unpredictability.

CHAIRMAN ROGERS: So you all agree with Admiral Truly's plan that certainly for the next few flights you plan to land at Edwards?

MR. CRIPPEN: Yes, sir.

MR. ABBEY: I think the question John raises on the stability of the weather is a fairly key point because you want to go into an environment where the weather pattern is going to be stable and predictable for a long period of time, and at Edwards and in New Mexico you have both those situations.

We have tried to get into Kennedy a number of times, and we ha e proven that we couldn't predict the

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weather, and it has changed on us very rapidly. Crippen was waved off twice, and in December we had three wave-offs.

CHAIRMAN ROGERS: Mr. Weitz, do you agree?

MR. WEITZ: Yes, sir.

CHAIRMAN ROGERS: Do you have anything to add to what has been said?

MR. YOUNG: No, sir.

DR. WHEELON: Mr. Chairman, may I make a comment on this? If we were to accept this very reasonable consensus and implement it so that we land only at Edwards except in the case of emergency, then our attention turns naturally to the way that we get from Edwards back to Florida for relaunch. That way is a single 747 that has been specially modified to carry the orbiter. That 747 becomes a single point of failure in the Shuttle program, and I submit that we ought to think hard about the possibility of buying a second one.

Do you have any comment on that, gentlemen?

MR. ABBEY: We have been, I think, talking about doing that, and I am very hopeful that at is going to be one thing that we get into the budget.

CHAIRMAN ROGERS: If there are no other questions, do you want to proceed, Captain Young?

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MR. YOUNG: Another issue that many people have raised is why don't we do auto land. nd they are talking about auto land both to improve our ability to handle low ceilings and visib. ty, and as you see what happens when you talk about doing auto land at the low ceilings and visi. ity, you are probably going to be in the rain. If you are not going to be in the rain, you are going be in very low ceilings which are about zero-zero practically, and I suggest that you probabl. don't want to land the orbiter in those kind of conditions, for many, many reasons.

We propose, it has been proposed to do automatic approaches to low altitude and low altitude operations in clouds or in a ceiling or reduced visibility does introduce more risk into the human controlled approach, and it is not just the machine itself; it is the interaction of the human beings with that machine. Our current mission rule has 8,000 foot altitude, and that gives us five miles of visibility to the precision approach indicators where if a fellow is doing a heavyweight landing into Dakar, he has got about 28 seconds to preflare altitude, which is where you have to be set up right, and less than 18 seconds to 4000 feet, and with the automatic speed brake system that we have, it is mandatory for a Dakar landing with a short runway

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to be set up in a stable dive by 4000 feet to do a successful preflare and landing, and it is particularly true when that vehicle is heavyweight and the runway is short and narrow like it is at Dakar, or if it is like it is with a rough surface as Kennedy, and it has a cross wind or tail wind. We need ground sensors operating full time to do auto lands, and we feel that with only three orbiters left right now, that lower weather minimums is about the opposite of what the Space Shuttle program ought to want to do for a long time.

We also sort of have a requirement because people up in space tend to get rusty, and people who haven't flown the orbiter tend to get rusty, to have them fly the control stick so that they get some stick time just before landing, and normally that is about three minutes of attitude control stick time all the way around the heading alignment circle to landing. And therefore, we believe that automatic approaches would not be the way to fly the orbiter successfully when the pilot has got to do the landing. Crew monitoring for auto-land the way the Navy does automatic landings, they require two independent auto land monitoring systems before they can do auto-lands. At the present, with the Shuttle auto-land system, we have no independent monitoring system, and no one really knows how to

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implement one independent monitoring system for successful low altitude crew takeovers. Even if successful crew takeover could be done, which it really can't, crew interaction with auto-land in the Shuttle, crews really can't successfully interact with the auto land system at low altitude because we have these great big elevons on the back, and we have a little bitty attitude control stick, and it can move those big elevons an awful long way awful fast. And when that happens, exciting things happen.

We actually train our pilots not to use the attitude control stick very much. They set up a trajectory, and the closer they get to the ground, if they are set up right, the less they move that control stick. And the orbiter exhibits what we call reverse altitude responses, and I will tell you what that means. And when it does that, a natural reflex takeover pilot action would be the exact wrong thing to do. For example, if you've got a sudden nosedown pitchover when you are doing auto land, the reflex action to take over would be to pull back on the stick, to pull the nose up, and it would be—what it would do, it would raise those elevons, drop lift on the wing, and drive the wheels right into the ground at a high sink rate. And then the only way that people do operate

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on an auto-land touchdown machine safely is with a go-around capability provided by throttles. That is how you get yourself out of all kinds of jams with airliners, and needless to say, the hundreds of approaches to touchdown that airliners must make to get FAA certified to auto-land, we will never be able to do in an orbiter.

And the auto land system is dependent on many sensors, and it is always one failure away from not working at all because it isn't even in the backup flight system. An inavertent backup flight system engagement would be catastrophic if you had to do an auto land.

For all those reasons and for many others, we don't view auto lands as a practical solution to any problem in the Space Shuttle program, and I guess we have stated that to the program folks. And I guess the overall basis for that is that it is just not—it is not something that is going to do the program any good.

And I wish it was because I am not against auto lands, but I am sure against not being able to do it right.

CHAIRMAN ROGERS: Thank you, Captain Young.

I assume that part of your job over the years

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has been to learn about concerns of astronauts and to express them to the system, and to express your own concerns.

One of the questions I think the Commission has and wants to address to you, do you feel that those concerns have been properly and appropriately handled by the system?

MR. YOUNG: Sometimes yes and sometimes no. You know, the NASA way of doing things is an interactive, argumentative way of doing things; everybody presents a pretty solid image to the public and everything, but when you get in a meeting with engineers, you are in a knock down, drag out discussion about how you want to do things, and that is the way we operate, and I don't see anything wrong with that, but I hate to lose. But we sure don't win them all.

CHAIRMAN ROGERS: Well, I think we all understand that. I think the thing that would concern us is if you felt that the concerns that you expressed and the concerns you expressed on behalf of the astronaut community were not considered by the system.

MR. YOUNG: Sometimes they are and sometimes they are not, and I think that is pretty understandable.

CHAIRMAN ROGERS: Well, I guess maybe I didn't

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ask the right question.

I don't mean do they always agree with you, but did you have a feeling that consideration was given to the questions that you raised about safety?

MR. YOUNG: Yes, sir. I don't know how much, though. I mean, I am not able to judge what weight they would put on those kind of things.

CHAIRMAN ROGERS: Well, without going back over the past history of problems that were presented to the system and how they were handled, do you now think that there are matters in addition to the ones that you have talked about today that are important safety considerations that are not being given the proper attention?

MR. YOUNG: I think as a result of this review that Henry talked about, I think they will be, yes, sir.

CHAIRMAN ROGERS: You are satisfied, then, that the concerns that you have expressed in the past and the ones that you may have in your mind today are being properly considered under Admiral Truly and in view of the study that is now being undertaken?

MR. YOUNG: Yes, sir, and I think that is going to be an ongoing process for a long, long time. And I am going to keep an eye on it, yes, sir.

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CHAIRMAN ROGERS: Do you have any recommendations to make in the structural organization as it exists? Now, as I understand it, the astronauts' concerns are normally expressed to you, although sometimes they are handled directly by particular astronauts, most of them are expressed to you and Mr. Weitz, and then as I understand it, you would pass those on to Mr. Abbey and Mr. Crippen in that office, and they would consider them and do whatever was appropriate, including pass them on?

MR. YOUNG: That's how the system works, and we work a lot of off line things in other areas, other arenas, flight techniques meetings and things like that; where we can work them at low levels, we do.

CHAIRMAN ROGERS: Do you have any recommendation for changes in the structure?

MR. YOUNG: I have one that we will talk about later, yes, sir.

CHAIRMAN ROGERS: Do you want to wait until later, or do you want to do it now?

MR. YOUNG: No, sir. I would rather wait until Mr. Abbey sets me up for it.

(Laughter.)

CHAIRMAN ROGERS: Any way you want is fine with us.

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(Laughter.)

MR. WEITZ: I would like to address a couple of concerns we have, and as a demonstration of the fact that John and I did not rehearse and compare notes, he has kind of spiked my guns a little bit on the first one. We do have some concerns over the considerations that have been given to the possibility of executing a TAL, a transatlantic abort landing, which by our approach to the rules under which we decide whether the conditions are acceptable for a TAL are somewhat modified and flavored by consideration of the probability of occurrence. By that I mean that—and of course, we have gotten new data recently which I think only serves to underscore our concerns. One is that we have in the past launched flights that required a waiver over the maximum allowable TAL weight at Dakar, and I am referring primarily to Dakar, because the bulk of our missions do go out at 28-1/2 degree inclination launch, and therefore Dakar is the prime TAL site.

As John alluded to, we have serious reservations about the nosewheel steering system and the capability of the brakes to stop this vehicle, and we also accept lower weather minimum at Dakar in light of or in the regime of visibility requirements. The ceiling requirements are the same, the visibility

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requirements there are five miles rather than eight, or six rather than eight, but it is on the order of something less. What we do not want to do is run an airplane, an orbiter, off the end. We only have one suitable runway at Dakar since the wind is primarily from the northeast quadrant there, and the prevailing wind runway. Dakar often has visibility problems caused by the winds that blow across the northwestern African deserts and pick up dust and sand and keep it in the area, and I lose track of our numbers, but I know that we did delay for several days on one mission because of poor visibility at Dakar.

So basically we do have a concern with the approach to how we systematically approach the requirements for the conditions at Dakar when it is a primary TAL site.

Another concern that we have is night landings. We have demonstrated more than once, twice now, that we do in fact have the capability to safely land the orbiter at night. However, a night landing is not quite the same as a day landing, principally because it is at night, which I will get into a little bit, but in those cases where the conduct of the mission absolutely requires it, then we will accept the responsibility to go and either launch at night or land

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at night or do both, if required. It is just that sometimes there have been instances in the past when the hard, firm, absolute requirements of the mission did not in fact mandate the conditions which resulted in a night launch or a night landing. It was more to maximize, either to increase by some percentage the scientific return on a mission or to have a wider launch window to take advantage of on launch morning.

The reasons we don't like night landings are, as I said, because it is at night. In fact, your landing the orbiter is like landing any other airplane in which you primarily use your outside scene as the cues on which to get down once you get in close to the ground. Once you get past the preflare point and get within that last couple of hundred feet of the ground, at night those cues just aren't there. You must rely on the guidance and navigation system which is resident within the orbiter, and you basically use electronic displays in order to get you down. You don't perceive—if you have a cross wind, for example, it is much more difficult to perceive drift across the runway at night than it is in the morning.

We have a principal example of that by the fact that a very highly experienced pilot of ours landed at night on a wet runway at Ellington, experienced

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hydroplaning which he did not recognize the fact that he was drifting off the runway and wound up leaving the runway and nearly destroying the airplane. Fortunately he was not injured, but had that occurred in the daytime, he would have recognized the drift, the condition, much earlier, and been able to do something about it.

So it basically is not suppression but the lack of outside environment visual cues which you experience in the night landings that we would like to avoid then to the maximum extent possible.

Another consideration along those lines is that just as the pilot is deprived of his ability to discern what is going on around him, so are the folks involved in weather observing and forecasting. In fact, we had one mission that would have been a night TAL at Dakar, and even after the fact—was that Dakar or Morone? Anyway, I think it was Dakar where there was some dispute as to whether the ceiling was on the order of 3,000 or 4,000 feet or whether it was up around 8,000 or 9,000 feet. And so the weathermen are making the observations, and therefore, that is going to influence the validity of their forecast and also obviously influence the launch director's decision whether to go or not.

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Another thing we cannot overlook is that training to do a night landing requires a significant amount of crew time. Our nearest training site using the Shuttle Training Airplane is at the White Sands Space Harbor in El Paso which, if you go out there to fly at night, you basically have shot nearly a whole day and a half. By the time you get out you observe crew rest requirements from a pilot freshness standpoint, and it is very hard on the crew when they are trying to get out to come up to the level of proficiency that we feel is required for a planned night landing.

We do train every crew to be capable of performing a night landing, but if we know that folks are in fact going to be required to perform one, then it does require—we do expose those folks, require that they do get more training.

CHAIRMAN ROGERS: Thank you, Mr. Weitz.

VICE CHAIRMAN ARMSTRONG: I understand that Casablanca is a potential alternate TAL, and is there significant performance penalty in terms of the inclination to successfully utilize that?

MR. WEITZ: Well, it's on order. We would have to go up to about 31, 31½ degrees, and what number are we using now, 600 pounds a degree or a 1000 pounds a degree? So you are talking on the order of

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2,000 to 3,000 pounds performance penalty. But in those cases where we have that performance, we would like to consider shaping to go do it, which requires more resources. Another reason we have locked in on a standard 28 1/2 degree inclination is there are a lot of things in trajectory planning and analysis we don't have to do over and over when you start picking difference launch azimuths.

CHAIRMAN ROGERS: How many night landings have there been?

MR. WEITZ: Two.

CHAIRMAN ROGERS: Who was the commander?

MR. WEITZ: Admiral Truly made the first one, and he complained bitterly. He stayed up all night and went to bed during the day, but no one else did.

(Laughter.)

MR. WEITZ: And the second one was Hoot Gibson landed at Edwards about an hour before sunrise, I think.

CHAIRMAN ROGERS: Thank you.

Any other questions?

GENERAL KUTYNA: John, let me ask you, if you go into Dakar on a TAL with some of the heavyweight payloads that we have planned, and the brakes you have now, can you stop?

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MR. YOUNG: It depends upon what brake energy margin and what kind of a head wind or tail wind you've got. I think the latest Ames Sim showed that. It was really touch and go. You have to put the brakes on with 5,000 feet to go in a no-wind situation when you are doing 165 knots, and I think that would be really, you know, you are putting those brakes on so darned fast that you use the energy up just like nothing flat, and if they don't hold up, if they don't give you the full brake amount, which is 55 million foot pounds, if they fail at 40 million foot pounds, like 44 million foot pounds per brake, as we have had happen, or maybe at 34 million foot pounds, like happened on Hoot Gibson's flight, which we still don't understand the reason for that failure, I know you would be in real trouble on stopping.

GENERAL KUTYNA: And what's at the end of the runway?

MR. YOUNG: Well, there's a block house and a cliff.

(Laughter.)

MR. HOTZ: John, what kind of an engineering effort is ongoing on the brake problem, and is it a matter of technology or money to fix it?

MR. YOUNG: It is a matter, I believe, of

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money, yes.

MR. HOTZ: Thank you.

MR. YOUNG: But I can't tell you where that stands right now. That is one of the issues that we are carrying on right now with the program office.

MR. HOTZ: But is there any kind of an engineering program under way?

MR. YOUNG: They are looking at carbon brakes. I saw the spec on them the other day, and some of those were rated at up to 100 million foot pounds per brake, an I've seen others with 70 million foot pounds per brake. And there are four brakes, two on each tire.

MR. CRIPPEN: John, if I could interrupt probably Mr. Aldrich can give you more data on this, but there is an ongoing program for improving the brakes, and there has been for some time.

MR. YOUNG: But it takes a long time to build a set of new brakes.

MR. HOTZ: But is it a question of technology? Do you have to go into another order of brake technology?

MR. YOUNG: These are carbon brakes.

MR. HOTZ: Are they the same kind they use on the Concord?

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MR. CRIPPEN: They do have carbon brakes on the Concord and several other aircraft. It is not so much developing new technology, but we have discovered that the making of aircraft brakes is more of an art than it is a science, and especially on airplanes, you have the opportunity to take them out and do ground runs and make adjustments for them, and we really don't have that kind of a facility available to us. Consequently, we are also developing a new lab at Wright Pat to help us do some of those kinds of tests so that we can improve the brakes.

MR. HOTZ: Thank you.

MR. HARTSFIELD: I would like to talk about a couple of items that we are concerned about that relate—they are training items, really, but they are things starting to concern us, and what PJ and John have been talking about lead right into it. One is handling qualities. The orbiter is a really nice flying machine up and away, but when you get it down close to the runway where it becomes important to have good handling qualities, it has some very strange characteristics. Neil will understand this. I know but the pilot is located at or slightly aft of the apparent center of rotation, which is kind of a bad place for the pilot to be. It is just the nature of the airplane because of

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the large percentage of the wing area that is in the elevons themselves, and the end result is what John talks about, when the pilot makes an input, he doesn't get any physical feedback that he has done something, and so you have to learn to fly the orbiter pretty much open loop. You make small inputs and then wait and see what happens, and that is not a natural pilot instinct. So we spend a lot of time training the pilots in the training airplane.

Further aggravating this problem of sensing what is happening is the eye heighth. You are about 33 feet, your eyeballs are, above the ground when you touch down, and that is in the same ballpark as, say, like a 747 or something, and of course they have perception problems, too, and I have talked to those guys on how they land those things, and they pretty much set up a steady rate of descent and just fly them on.

Our problem is aggravated by the fact that we are decelerating rather rapidly and losing over 5 knots a second. We are just a glider, and so we have to set it up and get it on the ground. As John says, we learn not to make inputs close to the ground because anything you do is wrong.

For example, the sink rate, if you sense the sink rate is a little bit too high, the instinct is

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to put in a little backstick. As soon as you do that, you move the elevons. One degree of elevon at 200 knots is worth 6,000 pounds of lift, so you move them 3 or 4 degrees, and you've really dumped a lot of lift, and the thing really drops in on you.

So it takes a lot of time to learn this technique.

Now, the STA, our training airplane, the modified Gulfstream II, is the essential link in this training, and that is where we really learn to fly the orbiter and learn to land it, and we can't do this in a simulator. We need the real flight environment, and you need this training airplane to learn how to land.

Now, all the pilots who have flown the orbiter come back and sing the praise of the training airplane. I know it has been my case and everybody that has ever flown it, that you come back after six or seven days in space and the first time you do this, I guess almost every time you are a little apprehensive that, you know, gee, I haven't flown in a long while, I've been up there for six or seven days and my reflexes maybe aren't what they should be. You take over the machine, and you feel it out, but as soon as you start around the heading alignment circle, all of a sudden it clicks and you say I've been here before, and the reason you have is that training airplane.

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Well, what I'm leading up to is we are beginning to get in a lifetime problem with those airplanes, and that is starting to worry us. Let me give you an example. A pilot that starts into the flow, by the time he becomes a commander and lands the orbiter for the first time, he will have made in the vicinity of 900 approaches with that training airplane, and we find this, that this is about right. In fact, some pilots will tell you they would like to have a little more, but that is about what we are getting.

For 12 flights a year, to support that sort of program. I am just giving you a rough number, we need about 1400 hours of training time a year in the training airplanes. At optimum, we can get about 750 hours on each airplane, and you figure, we find that we get roughly a 70 percent effectiveness factor. In other words, a lot of the flying time on an airplane is not dedicated totally to training. You are moving the airplane back and forth to Ellington or to the factory for inspections, you know, there is overhead. So three airplanes can give you, if you could get 750 a year, which we had last year, you could get 575 hours, so roughly three airplanes will support 12 flights a year, which is what we were approaching when we had the accident.

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Two of those airplanes now have over 5,000 hours on them, and the predicted fatigue life of some of the components, like basic wing structure and things like that, were under that number, 4,800. So we have reached a point where we are having to be very careful on our inspection programs and we are reaching the point where we can expect to start having failures in major structure or cracks. The airplane is failsafe, let me point that out. You're not going to have a catastrophic failure, but when you do detect the crack or whatever indication of a failure you see, of a fatigue life problem, you've got to fix it, and if it is a major structure, you are going to be down for a while.

So one of the concerns we have had in the office is we see ourselves reaching a point where we may not be able to support the crew training, and we have in the mill right now a push to get a fourth STA to carry us through. Our third STA doesn't have that much time on it, but we need another airplane. Otherwise, I am afraid that we won't be able to support the flight rate.

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VICE CHAIRMAN ARMSTRONG: Is that fatigue life at that level because you are highly loading the airplane during practice approaches?

MR. HARTSFIELD: Yes, sir.

VICE CHAIRMAN ARMSTRONG: So it wouldn't be comparable with the normal G-II?

MR. HARTSFIELD: No. Every approach we cycle the gear. Well, you know, we've probably got more gear cycles in those airplanes than any G-II that's flying.

MR. ABBEY: It's about a factor of four.

MR. HARTSFIELD: The other training item that is causing us a little concern now that I want to mention and I told you earlier I would is the SMS—our shuttle mission simulator.

What we have is two training simulators. One of them is a fixed based—in other words, it doesn't move. A full cockpit mock-up; the other one is a moving base and I think you've been to Houston and looked at that and those have an aft station in that part of the cockpit that doesn't move attached to it.

Those two bases are essential to our training, and it's a very complex machine. And it has a heck of a lot of fidelity that all of the crews brag about.

At the time of the accident we were getting a hundred and 48 hours a week on the shuttle mission

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simulators; that is total for the two bases, out of a possible, of course, 24 hours a day, seven days a week out of a possible 336 hours. We were planning to ramp up to about 160 hours a week. Our best estimate is that the two bases at best could only support about a 180 hours a week.

There is an awful lot of time that can't go directly into training for reconfigurations for new training loads and just the maintenance on the simulator itself.

And so we're talking about optimally there may be somewhere between 50 and 55 percent utilization rate on those bases. That is what has been predicted for them.

Well, 180 hours a week will support somewhere around 12 or 14 flights a year, based on the templates we have. In other words, at the time that we had the accident, we were already facing up to – we were getting them into a crunch on crew training. And eventually that leads into worries.

Now we complicate that a little bit, and that's obsolescence. It's a bad word, but it's true. We've got 1970 technology in both mission control and in the shuttle mission simulators and we have budget-line items to start upgrading these facilities. As an example, the

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computers that are being used in the mission simulator are no longer being made. And the vendor says he won't make parts for them in three more years.

So we know we are going to have to remachine the simulators. And we have already got that going. But in addition to this, we have problems with the simulators, known deficiencies. We have a pretty lousy visual system; we know that and we're trying to get something on line to improve it.

We have a large number of the models in the simulator that are not what they ought to be. As an example, the main engine model—in other words, the interaction between the crew and the simulation of the shuttle main engines is so bad that in some cases we get negative training, and it is one of our high priority items to fix.

And we have now finally turned on some efforts to get some of these problems fixed. The word we had was that with the flights rate as it was, and the limitation of the simulator and the growing list of problems, of which there is something close to 600 of them, and a lot of these are little nits, but they've documented over 600 discrepancies against the simulator and it got us worried about maintaining our training.

And what we really needed, I think, is another

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at least one more SMS base in order to support a flight rate of a dozen or more and do it comfortably.

Some other things that we needed and we are stepping out on this now is a guidance and navigation trainer. Right now if we want to teach a pilot, a commander flight techniques or teach him how to do the nav, we have to tie up the whole base to do that, and we shouldn't do that. We should have a part-task trainer. And we've been trying to get one of those for a long time. It looks like right now that we may very well be able to do that. We have got the thing roiling.

And the only reason I bring this up is to show you that we have concerns and we still do; but there is still a lot of work to do to make the training side of the house with our airplanes and our mission simulators match up to the flight schedule.

MR. WEITZ: If I might add, Henry, the inadequacies in the SMS affect the level of the training of the flight control teams also in that they get significant amounts of training while they're running integrated simulations with the flight crew in the simulator.

MR. ABBEY: I think those covered at least all of the major specific concerns we were going to talk about. I don't know whether there were any other

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questions or not that you might want to ask us.

MR. ACHESON: I have a question on internal agency safety, particularly the way the safety function is organized in the agency.

I would be interested in the reactions of any of you gentlemen to the question whether the internal safety function is set up in a way that you think makes the optimum contribution to flight safety. And if not, why not, and in what way you would like to see it changed.

MR. ABBEY: Well, that was really the next topic that we were going to touch on, and we can get into that now.

MR. ACHESON: Very good.

MR. ABBEY: Both John and I were very much involved in the activities after the Apollo fire in 1967.

CHAIRMAN ROGERS: Would you move your microphone a little closer.

MR. ABBEY: Both John and I were very much involved in the activities following the Apollo fire in 1967 and at that point there was considerable strengthening of the reliability and quality assurance and the safety organization within NASA. We established a safety office within the office of space flight. We also had an independent reliability and quality

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assurance and safety organization at each of the centers reporting to the directors.

And Dr. Gilruth and George Lowe, who was the Apollo spacecraft program manager, were very insistent upon having that be an independent function that didn't get into the programmatic considerations.

We had, at that point in time, a lot of flight anomalies from Apollo 7 to Apollo 11. We had a period probably of about six weeks in between flights where we had to resolve all the flight anomalies.

George Lowe, I think more than any individual I've ever met, put a lot of emphasis on identifying all of those anomalies, right up to the top level up through NASA headquarters, and they were reported and acted upon and satisfactorily resolved.

Even with that six-week time period, in between seven and eleven, I think that only happened because of a lot of painstaking attention to detail and I think dedication to get the job done well by all levels within the Apollo program.

I think that same attitude exists within the shuttle program. I think we were successful on the flights before 51-L, to a large degree, by that same kind of attitude.

But I think as we tend to attempt to fly more

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frequently, I think we probably need to look at a better check and balance of the system and John and I have talked quite a bit and we talked to other people about looking at establishing a more independent safety organization and activity within NASA and within the center.

We have, I think, a good system within our aviation program. We have an aviation safety officer that worries about our aircraft operations. John has an aviation safety officer within the astronaut office. They always aren't the most popular individuals because, I think, they don't always tell us what we would like to hear.

For example, we have got eight of our airplanes grounded now. But I think that is the kind of attitude and the kind of feeling you have to have in that kind of a program, and I think it is important to have.

And John, I think, could probably better express than I some thoughts in that regard.

MR. YOUNG: This is really the key to the reason that I'm glad to be here today because I have this feeling that the very biggest problem that must be solved before the space shuttle flies again is that one of communications. And that is communications with

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respect to the early identification and proper appreciation of program-wide safety issues.

That NASA internal working paper on space shuttle program flight safety, the one that I wrote that was released—that, for the record, I didn't really release—covered several concerns that are safety issues and each of those concerns is being dealt with right now and change control boards and other reviews.

And prior to the accident, many of these safety issues were being worked and they were in the system. But they were not being worked and I was told mainly because we didn't have the money to deal with them.

Now that is a worrisome condition to me, and it needs to be corrected and it ought to be worrisome to everybody else. But by itself, it is kind of a communications problem because I didn't know some of those were in the system.

The space shuttle, after 24 missions, is exactly what we should expect from the first of its kind. It has certain risk associated with its normal operation, if you can call what it does normal. And it is a vehicle that we're all working very hard to make operational. And I think once you get it on orbit and start doing what people are supposed to do in space, it

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really is what you can call an operational system.

And I wonder, though, sometimes why if the space shuttle is inherently risky; why we should accept additional avoidable risks in order to meet launch schedules and we do that sometime, or to reduce operating costs, and that has been proposed. Or to fly unsafe payloads. And I think sometimes that happens.

The problem is that we've got right now and I think everybody in the astronaut office appreciates it; we just can't afford to have another accident. We cannot.

But I maintain if we are very, very careful, we can still have an outstanding space shuttle program just like we did in 1985, and that doesn't mean that the program has to accept avoidable risks.

And one of the problems we have is to get a communications link and properly define those risks.

Furthermore, we need a fool-proof way to surface to the top and correct safety issues early so that we can prevent another accident.

There is a great bunch of engineering people at NASA, and I guarantee beyond any reasonable doubt that all the working troops right this minute know exactly what all the space shuttle issues are right this minute.

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But what we have to worry about is five years from now, when "Joe Engineer" comes in to his boss and he says, "Hey, how about this data here that shows the [illegible] keeps breaking and it's going to blow the side off the orbiter."

And somebody says—and his boss says, "That hasn't failed in 60 flights; get out of my office."

And so here's something that's bad that could happen; a single point that this guy has discovered by desk work or by qualification or by test and it doesn't get through the system because this boss has got a million things on his mind. He's worried about something else; he doesn't have the money to do anything about it and so forth and so forth, and it shouldn't happen.

And the way I think you can prevent that one way--and I sure hope it isn't the only way--would be to get safety-wide and agency-wide flight safety organization, similar to those of many airplane programs.

If such a program were developed, the guidelines for that program might be the safety people in the organization would be independent of the cost and schedule concerns of their branches or divisions or directorates or centers.

And don't misunderstand me: The branch, the

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division, the directors, center bosses would still be responsible for safety and their responsibility should be documented clearly for everybody to see. But the grass roots and pervasive safety people report to their organization heads and still pass the word on safety issues that are going on right there up the line through the independent safety organization.

Unless we take very positive steps to open safety communications and to identify and fix early-on safety problems, we are asking for another shuttle accident.

These flight safety people would be continuously involved in design, manufacture, qualification test and turn-around test and checkout and inspection. Flight safety people could have continuous involvement in launch mission entry and landing operations. What they would do would be to identify and report—in real time, if necessary—space shuttle program flight safety problems to their bosses—their boss in that division, and also up their independent chain of command in their safety organization.

And the sole purpose of this group of people would be like any other flight safety group to prevent program accidents.

This is offered only as a constructive

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suggestion. If there is some other way to keep the lines of communication open and guarantee that they stay open for as long as we run this program—if there is a better and more foolproof way to do it, then we ought to do that.

And, needless to say, I go along with Crip: Without responsible people—the right kind of people, who are independent and safety conscious and oriented—such organization will be useless to NASA and wouldn't do anybody any good.

But it is requested that some reasonable system be added to checks and balances and added to the launch rate and cost reduction process because it is really important to being successful in this program. And I think we can be successful.

We proved it up to a point, and we want to continue to prove it; we just have to be very careful and we have to keep those lines of communications open. NASA doesn't do business without everybody knowing what everybody else is doing. You can't keep a secret anywhere in NASA; we never have been able to do it.

And so let's not start now. But let's make sure we don't do that anymore.

CHAIRMAN ROGERS: Captain Young, you know the Commission has been considering this suggestion for some

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time now, and I think most of us agree with you. It would be helpful to us if each of you would give it a little thought to the desirability of that kind of safety—independent safety panel, and with specific suggestions about how it should be set up and how many people should be involved and so forth, because if that does turn out to be a major recommendation of this Commission, I think it will carry a lot of weight. And this is the time to do it between now and the time we make our report.

And so any suggestions that you have along those lines, we would appreciate them.

MR. WEITZ: I don't have any specific suggestion, Mr. Chairman, because the more we look into all of the aspects of what it takes to make flying the shuttle safe, it's a very complex operation. It starts, you have to control design—design changes, maintenance, turn-around inspection—the whole aspect of it.

Basically what we feel intuitive or what I feel intuitively is that what we need is some system very similar to what is used in aviation and throughout the Armed Services, and I'm confident in the airline world in which you need a functional safety organization in place, which must have the support and the very obvious

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and visible support of command at every level within the organization.

That starts with the director of NASA right on down, in which those folks must be heeded and certainly their concerns paid attention to, regardless of, as John said, an impact on schedule and cost.

MR. ACHESON: May I ask a question, going to Captain Young's recent statement?

I assume that independent safety functions of the kind you described would have to work really at the project level; otherwise, they wouldn't be likely to see the work in enough detail, and yet they couldn't—I don't suppose you would want to have their promotions and their salary reviews dependent on the project people with whom they work.

Would you comment on that?

MR. YOUNG: I view this as just the way you would do it in, I hate to say, a military program, but safety people in military programs are still working for the commanding officer of that outfit and they get promoted the same way everybody else does.

I'm not sure how much independence that safety person would have to have. My safety officer is an astronaut and he's going to fly spacecraft. I don't see why a safety person in a division who is a knowledgeable

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person at the same working levels couldn't have knowledge of the safety issues that is going on in his division and be reasonably successful. And one of these days he could be the division chief.

I don't see anything wrong with that. And just because he reports these things off to the side, wouldn't necessarily keep him from going right up the same chain. And he could end up being the administrator of NASA. And I see nothing that would be necessary to stop that

I think one of the things those kinds of people could do for the program properly oriented, they could educate everybody else in the division of the importance of safety; there may be people sitting down there with this problem that is ongoing that they're working that they don't really know whether it is a safety problem or not and once attuned to this kind of thing, they would be more likely to report it.

I think it is just as important to have those kind of people throughout the agency to keep the lines of communications open. I don't think you would get points taken off for keeping open your communication lines in NASA because that is the way we work. And we just want to make sure that those lines of communication never get closed again for whatever reason.

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If there is a better way to do it, I would sure think that would be a good thing to do—a better, faster, a quicker way to do it; we ought to do that. I wish I knew what it was.

CHAIRMAN ROGERS: Just before you leave, Captain Young, I just want to, for the record, indicate as I understand none of the memos that you've talked about were released to the press by you. It happened some other way, right?

MR. YOUNG: I believe that is the case, yes, sir. I don't give out internal working papers because—shoot, you ought to see those other people's internal working papers.

(Laughter.)

MR. YOUNG: Because that is just what they are: They are structured to open up some ideas and avenues of thought to pursue to see if you're right or you're wrong. And you get your facts straight and then you work with it.

CHAIRMAN ROGERS: So those memos you wrote were part of your job?

MR. YOUNG: Yes, sir.

CHAIRMAN ROGERS: And that's the type of thing you're supposed to do is to point out problems and hope that they are corrected; is that right?

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MR. YOUNG: I think they are part of my job, yes, sir. I feel responsible for a lot of things in this area. I feel very responsible to these people that fly this machinery, and I sure want that to be successful. And if there is something that is more risky than it needs to be, I think it is really important to get that up the chain and let people at least look at it.

There may be good reasons for not doing everything; of course. I never think of that, but other people might. But I sure think it is important to get the word around.

CHAIRMAN ROGERS: And I gather you're very satisfied with the way Admiral Truly is proceeding with improving the program and taking into account some of the concerns you've expressed in the past?

MR. YOUNG: Yes, sir. I think that the word that he put out in his memorandum is really outstanding and everybody in the astronaut office believes that is a safe way to go, considering the problem that we just encountered. And it will help us get back on the track and the program will be much stronger and much better, if we do those things.

CHAIRMAN ROGERS: Well, thank you, gentlemen, very much. We appreciate this and I think it's been

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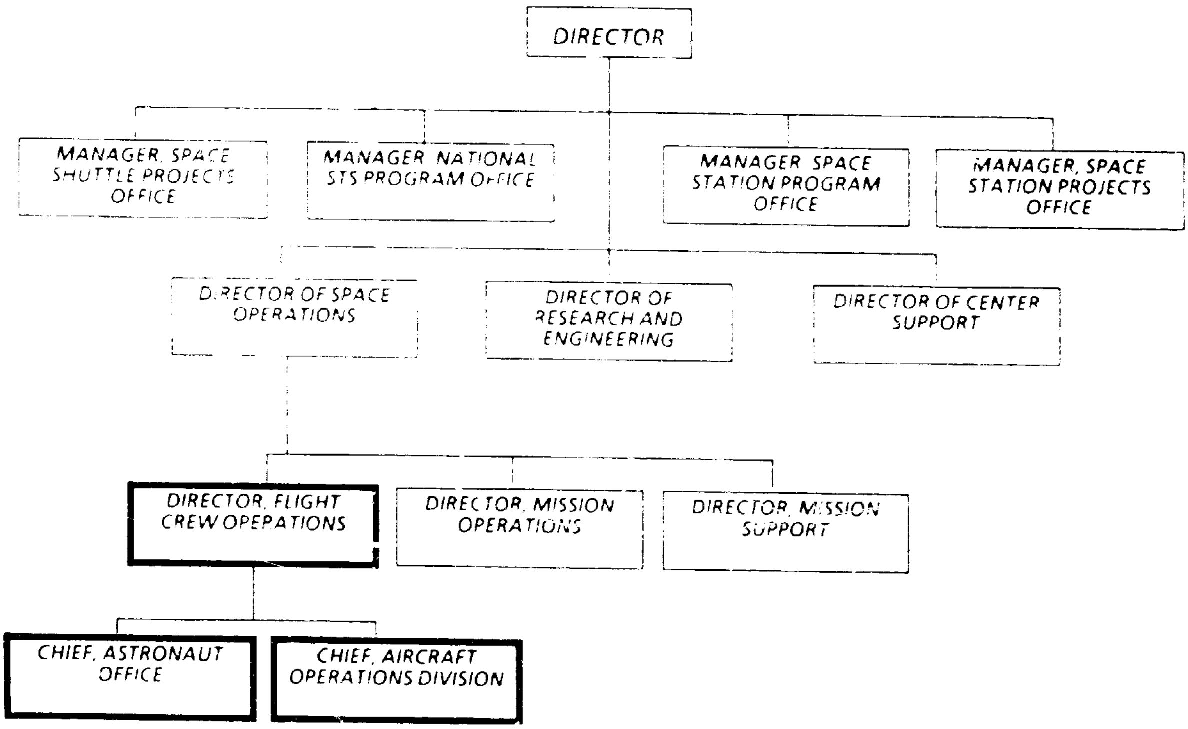
very constructive and helpful. Thank you.

DR. KEEL: Admiral Truly, Mr. Aldrich, and Mr. Charlesworth, please.

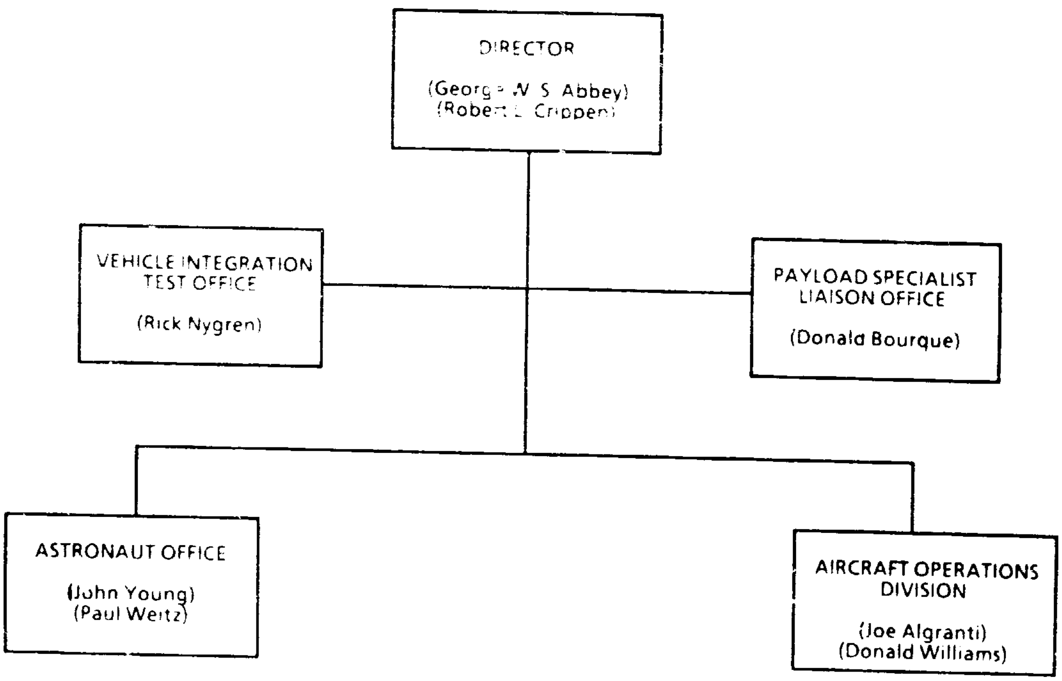
(Witnesses sworn.)

JOHNSON SPACE CENTER



[Ref. 4-3-1]

FLIGHT CREW OPERATIONS DIRECTORATE



[Ref. 4-3-2, 1 of 3]

ASTRONAUT MANAGEMENT POSITIONS

• DEPUTY DIRECTOR, FLIGHT CREW OPERATIONS	- R. L. CRIPPEN - D. C. BRANDENSTEIN
• TECHNICAL ASSISTANTS TO DIRECTOR	- J. P. ALLEN - R. H. TRULY - M. J. SMITH - D. M. WALKER - L. J. SHRIVER - C. F. BOLDEN - S. A. HAWLEY - J. H. CASPER - M. J. MCCULLEY - J. D. WETHERBEE
• DEPUTY CHIEF, AIRCRAFT OPERATIONS DIVISION, FLIGHT CREW OPERATIONS	- R. F. OVERMYER - M. J. SMITH - R. N. RICHARDS - R. L. GIBSON, 2P - J. A. MCBRIDE - D. E. WILLIAMS
• DEPUTY FOR OPERATIONS INTEGRATION, SHUTTLE PROGRAM OFFICE	- D. E. WILLIAMS - F. R. SCOBEE - R. J. GRABE
• APPROACH AND LANDING TEST PROGRAM MANAGER	- D. K. SLAYTON
• ORBITAL FLIGHT TEST MANAGER	- D. K. SLAYTON
• DEPUTY CHIEF, COLUMBIA MANUFACTURING & KSC TEST ACTIVITIES (PRIOR TO STS-1)	- R. F. OVERMYER
• CHIEF, APOLLO COMMAND MODULE REDESIGN (POST AS-204 ACCIDENT)	- F. BORMAN
• SPACE STATION PROGRAM MANAGER	- F. BORMAN

[Ref. 4-3-2 2 of 3]

MANAGER FOR DEVELOPMENT OF LUNAR MODULE ASCENT ENGINE	-C. M. DUKE, JR.
DEPUTY MANAGER, APOLLO SPACECRAFT PROGRAM	-J. A. McDIVITT
MANAGER, APOLLO SPACECRAFT PROGRAM	-J. A. McDIVITT
DEPUTY ASSOCIATE ADMINISTRATOR FOR OFFICE OF ADVANCED RESEARCH AND TECHNOLOGY	-N. A. ARMSTRONG
DIRECTOR OF DRYDEN RESEARCH CENTER	-D. R. SCOTT
ASSISTANT TO PROGRAM MANAGER, APOLLO SOYUZ TEST PROGRAM	-D. R. SCOTT
ASSOCIATE ADMINISTRATOR FOR LEGISLATIVE ADFFAIRS	-J. P. ALLEN
NASA REPRESENTATIVE IN AUSTRALIA	-J. P. KERWIN
DIRECTOR OF SPACE & LIFE SCIENCES	-J. P. KERWIN
OFFICE OF SPACE FLIGHT	-J. H. ENGLE
DIRECTOR OF SCIENCE AND APPLICATIONS	-O. GARRIOTT
EARTH RESOURCES PROGRAM	-P. J. WEITZ
CHIEF SCIENTIST SPACE STATION	-O. GARRIOTT
SPACE STATION	-D. LIND
CHIEF, ENERGY PROGRAMS, NASA HEADQUARTERS	-H. SCHMITT
EARTH RESOURCES PROGRAM	-W. POGUE
ASSISTANT TO PROGRAM MANAGER, APOLLO SOYUZ TEST PROGRAM	-E. CERNAN

[Ref. 4-3-2 3 of 3]

WHAT DO ASTRONAUTS DO?

- PROVIDE FLIGHTCREWS FOR NASA SPACE VEHICLES AND STATIONS
- PARTICIPATE IN THE DESIGN, DEVELOPMENT AND OPERATIONS OF SPACE VEHICLES AND SPACE STATIONS
- PROVIDE CREW INPUTS INTO THE DESIGN AND DEVELOPMENT OF SPACE VEHICLES AND STATIONS OPERATING TECHNIQUES AND PROCEDURES
- PERFORM CREW EVALUATIONS SUPPORTING DESIGN, TEST AND CHECKOUT OF SPACE MACHINES
- PERFORM OTHER DUTIES AS ASSIGNED

[Ref. 4-3-3]

ORIGINAL PAGE IS
OF POOR QUALITY

CURRENT ASTRONAUT ACTIVITY	*SLOTS*
• FLIGHTCREWS ASSIGNED TO 11 MISSIONS (53 CREW SEATS)	51
• MISSION DEVELOPMENT: DEPLOYS-UPPER STAGES/PAYLOAD DEPLOYMENT/RETRIEVALS; EVA SERVICING, MAINTENANCE, CONSTRUCTION; LIFE & MATERIALS SCIENCE, OBSERVATORIES, SMALL PAYLOADS, MISSION INTEGRATION	18
• MISSION SUPPORT: CAPCOMS; KENNEDY SPACE CENTER, SHUTTLE AVIONICS INTEGRATION LAB, CREW EQUIPMENT, SOFTWARE/HARDWARE, FLIGHT DATA FILE, TRAINING, ASCENT/ENTRY, AMES SIMULATION	38
• FIREFIGHTER TASKS: DISPLAYS/CONTROLS, LANDING & ROLLOUT, LANDING SITE AIDS & NIGHT LANDING, IMPROVED IN-ORBIT AUTO PILOT, SHUTTLE ONBOARD COMPUTER UPGRADE, SHUTTLE TRAINING AIRCRAFT	6
• OPERATIONS/ADMINISTRATION BRANCH LEVEL ASSIGNMENT: DEPUTY FOR FCOD, AOD, NATIONAL SPACE COMMISSION, DOD COORDINATION, SPACE STATION, OPERATIONAL INTEGRATION, APPEARANCES & SAFETY	25
• BOTTOM LINE: MANY ASTRONAUTS DO MORE THAN ONE JOB	138

JANUARY 22, 1986

[Ref. 4-3-4]

CURRENT ASTRONAUT ACTIVITY	SLOTS
• FLIGHTCREWS ASSIGNED TO 4 MISSIONS (18 CREW SEATS)	18
• MISSION DEVELOPMENT: DEPLOYS-UPPER STAGES/PAYLOAD DEPLOYMENT/RETRIEVALS, EVA SERVICING, MAINTENANCE, CONSTRUCTION; LIFE & MATERIALS SCIENCE, OBSERVATORIES, SMALL PAYLOADS, MISSION INTEGRATION	18
• MISSION SUPPORT: CAPCOMS, KENNEDY SPACE CENTER, SHUTTLE AVIONICS INTEGRATION LAB, CREW EQUIPMENT, SOFTWARE/HARDWARE, FLIGHT DATA FILE, TRAINING, ASCENT/ENTRY, AMES SIMULATION	38
• FIREFIGHTER TASKS: DISPLAYS/CONTROLS, LANDING & ROLLOUT, LANDING SITE AIDS & NIGHT LANDING, IMPROVED IN-ORBIT AUTO PILOT, SHUTTLE ONBOARD COMPUTER UPGRADE, SHUTTLE TRAINING AIRCRAFT	6
• OPERATIONS/ADMINISTRATION BRANCH LEVEL ASSIGNMENT: DEPUTY FOR FCOD, AOD, NATIONAL SPACE COMMISSION, DOD COORDINATION, SPACE STATION, OPERATIONAL INTEGRATION, APPEARANCES & SAFETY	25
• 51-L ACCIDENT INVESTIGATION AND RECOVERY	60
• <u>BOTTOM LINE</u>: MANY ASTRONAUTS STILL DO M RE THAN ONE JOB	165

APRIL 3, 1986

[Ref. 4.3.5]

- NINE SPACE SHUTTLE MISSIONS FLOWN (80 PERCENT MORE THAN 1984)
  - TWO "HOLD KILLS" AND ONE REMANIFEST -- ABOUT EQUAL TO ELEVEN FLIGHTS WITH RESPECT TO CREW REQUIREMENTS
  - MISSION TYPES: FOUR DEPLOYS, THREE SPACELABS, and TWO DOD
  - OPERATIONS:
    - ELEVEN COMMUNICATION SATELLITES DEPLOYED
    - FOUR RENDEZVOUS - ONE UNPLANNED TO LEASAT
    - THREE EVA'S - LEASAT ACTIVATION ATTEMPT; LEASAT REPAIR; SPACE CONSTRUCTION DEMONSTRATION
    - ONE ABORT-TO-ORBIT
    - NOT COUNTING DOD, IN SEVEN MISSIONS HAULED OVER 130 TONS UP AND DEPLOYED OVER 51 TONS IN ORBIT
  - TWELVE FLIGHTCREWS IN TRAINING FOR FLIGHTS THROUGH OCTOBER 1986
    - 58 SEATS AND 54 ASTRONAUTS, 14 NEW CREW PEOPLE
  - 57 OF 91 ACTIVE ASTRONAUTS HAVE SPACE SHUTTLE FLIGHT EXPERIENCE
- BOTTOM LINE - THE NATION HAS PROFESSIONAL FLIGHTCREWS READY <u>NOW</u> FOR SPACE OPERATIONS - TO DELIVER, RETRIEVE, REPAIR, REFURBISH, REFUEL, CONSTRUCT, OR WHATEVER.

APRIL 3, 1986

[Ref. 4.2.6]

REVIEWS, PANELS, BOARDS, WORKING GROUPS, AND TEAM PARTICIPATION

- PROGRAM REQUIREMENTS CONTROL BOARD - SHUTTLE LEVEL II
- DESIGN CERTIFICATION REVIEWS
- CONFIGURATION CONTROL BOARD - ORBITER LEVEL III
- ORBITER AVIONICS SOFTWARE CONTROL BOARD
- AVIONICS SYSTEM REVIEW
- CARGO INTEGRATION REVIEW
- MISSION INTEGRATION CONTROL BOARD
- SYSTEM INTEGRATION REVIEWS

[Ref. 4 3-7]

- FLIGHT OPERATIONS REVIEW
- PAYLOAD OPERATIONS WORKING GROUP
- GOVERNMENT FURNISHED EQUIPMENT (GFE) CONFIGURATION CONTROL BOARD
- PROGRAM OPERATIONS MANAGEMENT TEAM
- FLIGHT TECHNIQUES MEETINGS
- FLIGHT OPERATIONS INTEGRATION GROUP
- MISSION OPERATIONS DIRECTORATE DAILY TELECONFERENCES
- CREW PROCEDURES CONTROL BOARD
- SIMULATION PLANNING IMPLEMENTATION REVIEWS

[Ref. 4-3-8 1 of 2]

- SHUTTLE MISSION SIMULATOR CERTIFICATION WORKING GROUP
- COMPREHENSIVE UPGRADE PLANNING GROUP (FACILITIES)

[Ref. 4-3-8 2 of 2]

FLIGHT READINESS REVIEW

- o LEVEL I PROGRAM REVIEW OF MISSION STATUS AND ITS PREPARATION FOR LAUNCH.

- o ASTRONAUT OFFICE REPRESENTED BY DIRECTOR OF FLIGHT CREW OPERATION DIRECTORATE AND A SENIOR ASTRONAUT.

[Ref. 4-3-9]

LAUNCH MINUS ONE DAY REVIEW

- FINAL LEVEL I FORMAL TAG-UP ON THE MISSION'S STATUS FOR LAUNCH. THIS MEETING CLOSES ANY ACTIONS OPEN FROM THE FLIGHT READINESS REVIEW.

- ASTRONAUT OFFICE REPRESENTED BY DIRECTOR OF FLIGHT CREW OPERATIONS DIRECTORATE AND CHIEF OF THE ASTRONAUT OFFICE

[Ref. 4.3-10]

FINAL LAUNCH COUNT

o ASTROAUT OFFICE REPRESENTATION

- FLIGHT CREW
- DIRECTOR FLIGHT CREW OPERATIONS DIRECTORATE IN OPERATIONS SUPPORT ROOM AT KENNEDY SPACE CENTER.
- CHIEF OF ASTRONAUT OFFICE IN AN AIRCRAFT CHECKING RETURN TO LAUNCH SITE WEATHER.
- DEPUTY DIRECTOR OF FLIGHT CREW OPERATIONS DIRECTORATE AT FLIGHT CONTROL ROOM (FCR) AT JOHNSON SPACE CENTER (JSC).
- SENIOR ASTRONAUT IN SPACECRAFT ANALYSIS (SPAN) ROOM AT JSC.
- CAPCOM, PRIMARY AND BACKUP, IN FLIGHT CONTROL ROOM AT JSC.

[Ref. 4-3-11]

MISSION MANAGEMENT TEAM MEETINGS

- o DAILY MISSION STATUS MEETING FOR PROGRAM MANAGEMENT.
- o ASTRONAUT OFFICE REPRESENTATION
  - DIRECTOR OF FLIGHT CREW OPERATION DIRECTORATE
  - CHIEF OF THE ASTRONAUT OFFICE
  - SENIOR ASTRONAUT, SPAN REPRESENTATIVE

[Ref. 4-3-12]

ENTRY PREPARATION

o ASTRONAUT OFFICE REPRESENTATION

- FLIGHT CREW
- CHIEF OF ASTRONAUT OFFICE, IN AN AIRCRAFT CHECKING LANDING WEATHER.
- DIRECTOR OF FLIGHT CREW OPERATIONS DIRECTORATE AT LANDING SITE.
- DEPUTY DIRECTOR OF FLIGHT CREW OPERATIONS DIRECTORATE AT THE FLIGHT CONTROL ROOM, JOHNSON SPACE CNETER.
- SENIOR ASTRONAUT IN SPAN AT JSC.
- CAPCOM, PRIMARY AND BACKUP, IN FLIGHT CONTROL ROOM AT JSC.

[Ref. 4-3-13]

UNRESOLVED ISSUES

OR

WHAT MUST BE DONE TO ACHIEVE A VIABLE STS PROGRAM

H. Hartsfield
April 2, 1986

[Ref. 4-3-14]

- THE VIEWS EXPRESSED HERE ARE MY OWN
- THE GENERAL THRUST OF MY REMARKS ARE SHARED BY ALMOST ALL IN THE ASTRONAUT OFFICE

[Ref. 4-3-15]

REVALIDATE THE DESIGN

- DESIGN REQUIREMENTS
  - ADEQUATE DEFINITION
  - CONSISTENCY
- DOES AS-BUILT STS MEET REQUIREMENTS?
  - WAS DESIGN TESTED AGAINST REQUIREMENTS?
  - IF NOT, HOW WAS DESIGN CERTIFIED?
  - WAIVERS?
- WHAT ARE OPERATIONAL CONSTRAINTS?

[Ref. 4-3-16]

REVIEW HIGH CRITICALITY ITEMS

- SOLICIT CONCERNS FROM ALL ELEMENTS
  - CATEGORIZE
  - CONSOLIDATE
- REVIEW CRITICAL ITEMS LIST
  - REVALIDATE
  - ADD NEW ITEMS, IF ANY FOUND
- IDENTIFY AND PRIORITIZE CONCERNS/FIXES
  - FIX BEFORE FLY
    - SRB SEALS
    - OTHER
  - FIX AS SOON AS PRACTICAL
    - HIGH LEVEL OF QUALITY CONTROL UNTIL FIX
  - ACCEPT RISK

[Ref. 4-3-17]

REVIEW MAINTENANCE

- REVIEW TURN AROUND REQUIREMENTS
  - ADEQUATE PREFLIGHT CHECKOUT OF ALL SYSTEMS
  - CHECK REQUIREMENTS ON ALL HIGH CRITICALITY ITEMS
- REVIEW TEST AND CHECKOUT PROCEDURES
  - TRACEABLE TO REQUIREMENTS
- ESTABLISH METHOD OF VERIFYING HIGH CRITICALITY ITEMS
  - SPECIAL NOTATION IN PROCEDURES?
  - NASA QUALITY CONTROL?
- REVIEW WAIVER MECHANISM

[Ref. 4-3-18]

REVIEW OPERATIONS

- FLIGHT RULES
  - REVALIDATE RATIONALE
  - MEASURE AGAINST CRITICAL ITEMS, SYSTEMS
  - RE-EVALUATE LAUNCH/ABORT RULES, LAUNCH COMMIT CRITERIA
- TRAINING
  - EVALUATE CREW WORKLOAD
  - REVIEW TRAINING FLOWS
  - REVIEW FACILITIES
- MISSION PLANNING MILESTONES
  - FREEZE BASIC MISSION EARLY
    - TIMELY DELIVERY OF MISSION PLANNING PRODUCTS
  - FREEZE MISSION CONTENT AT L-5 MONTHS
    - CONCENTRATE ON FLIGHT CLOSE TO LAUNCH
  - DELIVER SOFTWARE ON TIME
    - TRAINING CREW AND MCC
    - SHELF LIFE

[Ref. 4-3-19]

REVIEW OPERATIONS (continued)

- COMMUNICATIONS
  - INSURE ADEQUATE CHANNELS FOR PROBLEMS TO SURFACE
  - CHANNEL TO CREW

- SAFETY
  - ESTABLISH INDEPENDENT SAFETY PANEL

[Ref. 4 3-20]

ESTABLISH FLIGHT RATE

- FLIGHT RATE MUST BE ESTABLISHED ON CAPABILITIES
  - TEST AND CHECKOUT REQUIREMENTS
  - RESOURCES

- FLIGHT RATE INCREASE BASED ON
  - PROFICIENCY IN TEST AND CHECKOUT
  - MORE RESOURCES
  - NEW RELIABILITY HISTORY

[Ref. 4 3-21]

SUMMARY

- THE RIGHT THINGS ARE BEING DONE
  - WE MUST CARRY THEM TO FRUITION
- NASA HAS SOME OF THE BEST PEOPLE IN THE AEROSPACE INDUSTRY
  - DEDICATION
  - PRIDE
- THESE SAME GOOD PEOPLE WILL MEET THE CHALLENGE TO PUT US IN THE AIR AGAIN

BUT

WE MUST NOT LET THE CURE BE WORSE THAN THE ILLNESS

- DO NOT MAKE HASTY FIXES
- FIX THOSE THINGS WE MUST
- THOROUGHLY, RATIONALLY EVALUATE ALL CHANGES

[Ref. 4-3-22]

FINAL THOUGHTS

WE MUST ACCEPT:

- THE STS IS NOT OPERATIONAL IN THE TRADITIONAL SENSE
- TO THOSE WHO FLY IT, IT WILL NEVER BE ROUTINE
- THE RISKS WILL ALWAYS BE HIGH

BUT WE SHOULD REMEMBER:

- THE SPACE SHUTTLE IS THE MOST FANTASTIC, MAGNIFICENT FLYING MACHINE IN THE WORLD
- THE CAPABILITIES OF THE SPACE SHUTTLE ARE UNMATCHED AND MUST BE EXPLOITED

[Ref. 4-3-23]

TESTIMONY OF REAR ADMIRAL RICHARD TRULY, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT; ARNOLD ALDRICH, MANAGER OF SPACE TRANSPORTATION SYSTEMS; AND C.E. CHARLESWORTH, DIRECTOR OF SPACE OPERATIONS

CHAIRMAN ROGERS: Admiral Truly, will you identify yourself, and for the record explain what your present duties are. We've had Mr. Aldrich, and so you might all do that briefly.

ADMIRAL TRULY: Yes, sir. I'm Richard Truly, and I'm associate administrator for space flight at NASA Headquarters.

My background is that I am a Georgia Tech graduate with a bachelors degree in aeronautical engineering; I was commissioned in the Navy from Georgia Tech and am still in the Navy; I was selected first in the astronaut program for the Manned Orbiting Laboratory in 1965; when that program was canceled in 1969, I went to NASA.

My experience at NASA was that I was on the support crew for the three Skylab missions; also for the

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Apollo Soyuz mission.

I flew with Joe Henry Engle on the Enterprise on the approach and landing test in 1977. I was a back-up crewman on STS-I, backing up Bob Crippen. I flew again with Joe Engle on the STS-II, commanded STS-VIII.

Immediately after that flight, I was asked by the Chief of Naval Operations, Admiral Watkins, if I would consider returning to the Navy to be the first commander of the Naval Space Command about two and a half years ago, which I did.

After the accident—a few weeks after the accident, I was again asked if I would consider coming back again under these unfortunate circumstances to this job, and here I am.

CHAIRMAN ROGERS: Mr. Charlesworth?

MR. CHARLESWORTH: My name is Cliff Charlesworth. I joined NASA in 1962. I started out in the flight control world, the ground flight control world, working as a flight controller during the Mercury, the end of Mercury and the Gemini program.

During the second half of the Gemini program, I was appointed a flight director and I did that job up through Apollo 12. From that point on I moved into program office work for a number of years, both in the world of

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applications and in the world of—some of the early shuttle work on the payload part of the business.

I spent a tour as the deputy director of the center, and now I am currently director of space operations.

CHAIRMAN ROGERS: Mr. Aldrich, I don't see any sense in going through your background. You can say anything you want to, but we have had the advantage of having you here before. And so I think we are quite familiar with the work you do.

How did you want to proceed, Admiral? Did you want to start or Mr. Aldrich?

ADMIRAL TRULY: It is your call, sir. This has been an interesting day for me. I might—with your permission, I might make a couple of remarks and then proceed anyway you would care to go. I found it interesting today to listen to this morning's testimony because I feel like, in a sense, that I can bridge the gap from the points of view that you have heard in the testimony of the guys in the crew office and yet also see the same problems with the responsibility of having to make the decisions required, along with particularly with Arnie, to solve those that we must and those that we can within the constraints that we have.

As you have seen this morning, the astronaut

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office is filled with a number of individuals with individual views and that has not changed from the way I remember it.

I think you have also seen that they are extremely supportive of the program that they are in and the people that are solving that problem and following the accident. They obviously and necessarily and quite appropriately are very concerned that we identify and fix those problems that have turned up.

Flight safety must always be balanced, in any real program between the real programmatic problems of cost and schedule and performance. Nothing is different in this situation than it is in airplane safety or airplane programs; nor was it different in Mercury or Gemini or Apollo or Skylab or Apollo Soyuz and now the shuttle.

I would, however, point out that the strategy that I issued of—I forget when; a week or so ago—which is a strategy to try to lead us back into operation of the space shuttle was not based on the inputs of just the astronauts, although I did discuss it with many of them. But I also discussed it in great detail with a lot of other people.

And it is a strategy; it is not, as we would say in the Navy, rudder orders. It does leave the

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response—the primary responsibility and accountability for us getting back to flying to the proper place in the organization, which is led by Arnie at our Level II for most of the actions that I outlined.

You heard this morning suggestions that we undoubtedly do not have the funds available to correct each and every one; and I think that is appropriate. As a matter of fact, there are some that I agree with more than others, but we do intend to take on each and every one of those suggestions and those concerns. And I think they are extremely important.

Interestingly enough—and I jot it down as George and John and the other crewmen talked this morning, each of the specific suggestions that they made—and I did not hear a single one that was new to me—these are concerns that in the shuttle program up to now have been argued, and I think that they must be now re-looked at on the basis of our experience, and that is what I intend to make sure that we do. And I know that I have their support.

But it is not only their support that we have. Everybody in NASA could not be more attuned to starting from today and getting back to safe, sustainable flight.

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As usual, I'm afraid I may have run on too long, but those were the only comments that I had on the basis of this morning, sir.

CHAIRMAN ROGERS: I think all of the Commission appreciated very much the strategy you have worked out. We appreciated the fact that you consulted with us ahead of time and we had a chance to make comments although I must say that we accepted readily the suggestions you made and we think that what you're doing is exactly right.

It is particularly important, I think, for the Commission because it would have been a mistake if people had come to the conclusion for 120 days because that is our mandate that we were going to report it after 120 days and NASA was going to stop everything for 120 days and do nothing but to look at the cause of the accident.

And I think the fact that you have taken the lead with the support of NASA generally to make corrections already, you are moving ahead as quickly as you can to correct some of these things that are quite apparent as a result of this investigation.

And so we compliment you for that. And we also appreciate the fact that our staff has been working very diligently and has had full cooperation from you and your people and the Subcommittees of this Commission that

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have been working with you and your people, I think, have coordinated very well, and we appreciate that, too.

Mr. Aldrich, did you want to make some comments about the testimony this morning and what Admiral Truly has said?

MR. ALDRICH: I found the testimony this morning to be interesting, but also very familiar. And this space shuttle system is, as was pointed out, the most complicated, technically sophisticated machine and vehicle in the world today. And when it was built, it required some very significant technology application and developments and breakthroughs—specifically the space shuttle main engines and the characteristics they run with.

The fly-by-wire fault-tolerant software and avionics system that controls the vehicle, the thermal protection system that goes over a standard aircraft skin and stringer system to allow it to fly the unique environments of the shuttle and the very configuration that it is in for flying the ascent flight and the entry flight regime and the demanding characteristics of those regimes make it an extremely technologically impressive technological development.

And sitting here this morning were two men who probably did what I think is one of the most technically

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demanding and courageous things in the history of man's space flight in that they piloted the space shuttle system for the first time.

And you can't but be impressed with these men. And the discussions here this morning were about their concerns. And in the jobs I've had before the shuttle program and in the shuttle program, I've spent many hours working technical concerns and issues and understanding them and attempting them—either to work them or to bring them forward.

And it was a very familiar discussion. And as Admiral Truly pointed out, the issues discussed this morning, are ones that I am familiar with and that we have dealt with previously and we certainly had stirred up a large amount of activity to be certain that they all are reheard and re-thought and re-assessed again.

And I have started on an activity to do that, prior to the time that Admiral Truly put his plan in place for the strategy for the next few months and year.

In mid-February I asked each of the projects and division level organizations that support the program to come forward with those critical areas that I knew had been of concern to them in the past or that might be potential for concern and change in the future because, again, I agree with the earlier discussion that

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a very high percentage of areas where we have potential for critical risk or where judgments have been made that ought to be reassessed are well known to the members of the shuttle team across the program.

And I felt it was important to bring this information forward and to start on it both to get an early lead on understanding it for corrective actions that we would want to make in parallel with the fixes that might be required related to the 51-L tragedy and also to understand upfront the budget structure that we may be dealing with in terms of some of the things we will now say we feel we want to do or we must do.

So I initiated that activity; it is very consistent with several of the items in the directive that Admiral Truly passed down from myself and my office also to integrate and to coordinate and bring forward and that is well underway. I have already had two meetings of detailed technical review on systems and operational issues of that kind. I have five more scheduled within the next 10 days. And we are proceeding as well as we can but in an orderly way to understand all aspects of each issue and treat it uniquely and understand how we want to deal with it, as we move forward.

DR. COVERT: Mr. Aldrich, would you clarify

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for me the role of—I think it is called the Aerospace Safety Board in terms of how they interact with the safety needs of your program.

MR. ALDRICH: The Aerospace Safety Advisory Panel is a panel that was created early in the man-space program before the shuttle, but I can't recall exactly at what point. It is a panel that has been formed, I believe, by the NASA administrator and reports to the NASA administrator, and it is a standing panel of external experts to the agency, primarily in the field of aerospace.

And they have each year they prepare an annual agenda of areas of interest or perhaps critical concern that they choose to review, and that takes them across the breadth of the program in the same manner the Presidential Commission has been operating in the last few weeks and months, and they key on both the known issues they're aware of in the program that they want to access and provide independent recommendations on. And as they do this work, they often uncover other things that are going on and will also highlight them. And their prime emphasis in the program is seeking safety concerns and safety issues and providing high-level recommendations on the thrust NASA ought to take in that regard.

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They are presented in an annual report, but concerns that they develop as they go are also relayed to us, and we will certainly give them immediate consideration.

DR COVERT: Would you feel, in terms of the discussions we have had earlier today, that that might be a suitable steering committee for an independent safety review organization within the agency or do you think that there is a need for separation between two such organizations?

MR ALDRICH: Well, I'm not probably broad enough in my knowledge to recommend to you what I think an optimum safety organization structure should be for the program. I firmly agree with the recommendations you've heard here today that the system and structure in place in the shuttle program today needs to be strengthened significantly and how it would be done and exactly the scope and the involvement I haven't collected my thoughts on.

I think the nature of the gentleman in the Aerospace Safety Advisory Panel and the opportunity they have to participate while it is broad in its coverage, it is not the day-to-day kind of augmentation I think is important in terms of the kind of business we have discussed a little bit earlier.

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DR. COVERT: Thank you.

VICE CHAIRMAN ARMSTRONG: Perhaps by way of background, could you clarify for the Commission how the program office and the operations directorate divide their respective responsibilities and what the difference is between those and how you coordinate your activities?

MR. ALDRICH: Well, I can talk a lot about that. I want to be sure that I talk about it in a way that relates directly to your intent.

VICE CHAIRMAN ARMSTRONG: With regard to what responsibilities fall into the program office and what falls under the directorate of operations generally and then see how you coordinate those activities and respond to concerns from either side?

MR. CHARLESWORTH: Arnie, why don't you let me try to start on that.

MR. ALDRICH: All right.

MR. CHARLESWORTH: In my job as Director of Space Operations, I am responsible to the center director, as Mr. Abbey pointed out this morning. I have three major elements. You heard from one of them this morning. I have another major element, which the best way to describe is the ground control function.

These people do the ground flight

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controllers' job; they do the—maintain all of our facilities, like the control center, the simulator that Mr. Hartsfield talked about. They do the training of the astronauts themselves. It is a very large, complex organization under Mr. Gene Kranz.

Then I have a third group, which is principally development that develop new facilities or upgrade facilities. They do things like the software development and that sort of thing.

So it is the three major-line institutional organizations which I oversee. We report—I report to the center director. I also, in effect, report to Mr. Aldrich because I take his programmatic direction and go implement it. For example, if a manifest is baselined, we're going to fly flight X and we're going to put these payloads on it, that comes out of the program office.

But my organization, from our point of view, then go implement that. We go do the flight design, we go do whatever is required in our area of responsibility to fly the mission.

So, in effect, I work for the program office. But I maintain the institutional organization to effect that work.

Now as far as coordination, I think one of the

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questions that I saw and Mr. Abbey touched on it—how do I handle issues that are brought to me? And they come to me in a variety of ways.

In my weekly staff meeting, for example, which I hold with Mr. Abbey and Mr. Kranz and Mr. Berry—my three directorates—issues are brought to me; they may be brought to me on an ad hoc basis, they may be brought to me outside of Mr. Abbey.

For example, recently I had a meeting with the Centaur Commanders over some issues that they wanted to talk about. I address these in a number of ways. Normally when they come to me, they have already been integrated between the Gene Kranz ground control systems people and the flight crew people.

They work together and come to me with an integrated suggestion. If I agree with it, which usually I do, I will take it to Mr. Aldrich. Depending on the urgency, I will take it right away or, if necessary, I will schedule a meeting to discuss it with him.

If necessary, I certainly feel free, if I don't feel like we're satisfied from the program office, I will go to the center director, if that's required. I can't think of an occasion we've had to do that other than informational. But that is the process we follow.

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Do you want to add to that, Arnie?

DR. FEYNMAN: You said you haven't collected your thoughts yet on what kind of a safety organization would be a good one. But have you collected your thoughts yet on what you think is the cause—I wouldn't call it of the accident but the lack of communication which we have seen and which everybody is worried about from one level to another?

Do you understand why? And, therefore, that helps of course in the first step in trying to figure out what to do.

MR. ALDRICH: Well, there were two specific breakdowns at least, in my impression, about that situation. One is the situation that occurred the night before the launch and leading up to the launch where there was a significant review that has been characterized in a number of ways before the Commission and the Commission's Subpanels and the fact that that was not passed forward.

And I can only conclude what has been reported, and that is that the people responsible for that work in the solid rocket booster project at Marshall believed that the concern was not of a significance that would be required to be brought

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forward because clearly the program requirements specify that critical problems should be brought forward to Level II and not only to Level II but through myself to Level I.

The second breakdown in communications, however, and one that I personally am concerned about is the situation of the variety of reviews that were conducted last summer between the NASA Headquarters Organization and the Marshall Organization on the same technical area and the fact that that was not brought through my office in either direction—that is, it was not worked through by the NASA Headquarters Organization nor when the Marshall Organization brought these concerns to be reported were we involved.

And I believe that is a critical breakdown in process and I think it is also against the documented reporting channels that the program is supposed to operate to.

Now, it in fact did occur in that manner. In fact, there is a third area of concern to me in the way the program has operated. There is yet one other way that could have come to me, given a different program structure. I'm sure you've had it reported to you as it has been reported to me that in August I think or at least at some time late in the summer or early fall the

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Marshall SRB project went forward to procure some additional solid rocket motor casings to be machined to new configurations for testing of the joints.

Now it turns out that the budget for that kind of work does not come through my Level II office. It is worked directly between the Marshal Center and NASA Headquarters and there again had I been responsible for the budget for that sort of work, it would have to come through me, and it would have been clear that something was going on here that I ought to know about.

And so there are three areas of breakdown, and I haven't exactly answered your question But I have explained it in the way that I best know it and—well, I can say a fourth thing.

There was some discussion earlier about the amount of material that was or was not reported on O-ring erosion in the FRRs and I researched the FRR back reports and also the flight anomaly reports that were forwarded to my center—to my office—by the SRB project and as was indicated, there is a treatment of the solid rocket motor O-ring erosion, I believe, for the STS 41-C FRR, which quantifies it and indicates some limited amount of concern.

The next time that is mentioned, I believe is in the STS 51-E, FRR in January 1985 or early in

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February, and that indicates, again, a reference to it but refers back to the 41-C as the only technical data.

And then from there forward the comment on O-ring erosion only is that there was another instance and it is not of concern.

Clearly the amount of reporting in the FRR is of concern to me, but in parallel with that, each of the flight anomalies in the STS program are required to be logged and reviewed by each of the projects and then submitted through the Level II system for formal close-out.

And in looking back and reviewing the anomaly close-outs that were submitted to Level II from the SRB project, you find that O-ring erosion was not considered to be an anomaly and, therefore, it was not logged and, therefore, there are not anomaly reports that progress from one flight to the other.

Yet, that is another way that that information could have flagged the system, and the system is set up to use that technique for flagging.

But if the erosion is classified as not an anomaly, it then is in some other category and the system did not force it in that direction. None of those are very focused answers, but they are all factors.

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DR. FEYNMAN: Thank you very much.

CHAIRMAN ROGERS: There is one other consideration that should be mentioned in the flight readiness review on 51-D, where it refers to evidence of hot gas passing the primary O-ring.

The resolution says acceptable risk because of limited exposure and redundancy. And yet, under the critical items list, it was decided there was no redundancy. And it seems to me that the Marshall people seem to proceed on the basis that they had a redundancy when the system had determined there was no redundancy.

Am I correct on that?

MR. ALDRICH: Your understanding of that information and mine come from the same post-event review and that is the way I understand it, yes.

CHAIRMAN ROGERS: And one other, I guess, while we're on the subject. And that is the uncertainty about the position that Rockwell took as to the weather conditions.

MR. ALDRICH: Yes, sir.

CHAIRMAN ROGERS: And that seems to me to be you understood it and I would say based on the testimony, understandably so, that there was expressed a minor concern but not a no-go vote, whereas they in their testimony indicated that they were recommending against

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the launch or at least they thought they were.

That certainly is a flaw in the system that can be corrected easily and that is if you are going to count on the contractor's opinion, they should vote either go or no-go and it should be clear how they voted so there is no ambiguity about it later on.

MR. ALDRICH: It is clear to me, sir, after the fact, that that was not crisp enough dealing with that discussion. I can assure you that aspect of my forward work and the projects define procedures which will take that into full account and correct it.

CHAIRMAN ROGERS: Thank you.

VICE CHAIRMAN ARMSTRONG: I would like to have you comment, if you will, on a few of the comments that we have heard in earlier testimony today. We heard today and we've heard from other sources on other occasions of the matter of cannibalization in spares and the potential problem that that might create in the future, had the accident not occurred.

Would you just comment on that consideration as you saw it from the program office point of view?

MR. ALDRICH: Yes, I can comment on that fairly assertatively because prior to my current assignment as the NSTS program manager this past summer for the prior three years I was manager of the orbiter project at JSC and

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dealt directly with issues with the orbiter and, in particular, that one.

And the situation on the spare parts for the orbiter project or program is that there had been fund contentions in the program for a number of years, at least starting in the mid-seventies and running through into the early to mid-eighties. And with that fund contention over a number of years toward the end of the 1970s and early in the eighties, intentional decisions were made to defer the heavy build-up of spare parts procurements in the program so that the funds could be devoted to other more pressing activities in the program.

And I can't sit here and recall what they were; but it was a regular occurrence for several annual budget cycles. And once the flight rate really began to rise and it was really clear that spare parts were going to be a problem, significant attention was placed on that problem by all levels of NASA and efforts had been made to catch up. But it has been a catch-up problem and it has put us in a problem of several years that we're right in the middle of now when our parts availability is well behind the flight need and through cannibalization the flight rate has been and can be maintained.

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But that was strictly a funding priorities issue that was created quite some time ago and that we are still dealing with.

CHAIRMAN ROGERS: Is the cannibalization problem a concern about the future of flight safety, as far as the three orbiters are concerned now?

MR. ALDRICH: Well, there certainly was a good point made this morning about the more you work on a vehicle and climb around in it, the more likely you are to do something that you inadvertently create and don't find.

On the other hand, some parts are readily interchangeable between vehicles. And so it is probably a mixed kind of an issue with respect to safety. And the plans are in place in the program to buy the parts we need. It is just strung out over a number of years now, probably through the late 1980s before the total parts inventory for the whole orbiter system would be in place. That is not an insignificant cost. The cost of a full inventory of spare parts for a four orbiter fleet is roughly the cost of another orbiter. So it is a significant workload in manpower and funding issue to deal with.

And that is a thing that I have been describing that has been difficult for the program to

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maintain current with some of the other progress.

VICE CHAIRMAN ARMSTRONG: We have been told by some that in fact the 1986 launch schedule would have been impossible because of spare parts shortages and I wonder if you share that conviction or whether you believe that cannibalization and other techniques might have been able to keep you going.

MR. ALDRICH: I think that unless we ran into one or two unique areas where the system had really trapped itself with work around strategies that the parts program would have been consistent with a flight rate, given a robust willingness for cannibalization.

CHAIRMAN ROGERS: Any other questions?

MR. ACHESON: Yes. I have a question relating to the flight readiness procedure in confirmation of remarks you made earlier.

I was struck by the fact that the certification of flight readiness of 51-L by the contractors including Thiokol and the forward certification by Level III to Level II did not make any mention of the previous program of testing the heightened concerns of both the Marshall staff and the Thiokol staff of the gas seal and the joint.

My question is: had that been appreciated by

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Level III to be a serious problem? Is that the sort of thing you would have expected to find in those certification documents?

MR. ALDRICH: Well. I'm not sure I've had the opportunity to review the whole certification history of the solid rocket booster, particularly the field joints through the Marshall Center and through them to their contractor.

I would have expected such a review as it may have or may well do to show that they were fully certified. And that was the basis that we were approving 51-L launch and any other launch on with that understanding across the board for the flight systems.

MR. ACHESON: But I guess my specific question is: had Level III appreciated the seriousness of the problem, would you not have had a notation to that effect to have appeared in the flight readiness document reporting which would have served as the flag to you that the system called for? And that is what I'm reaching for.

MR. ALDRICH: I really would have expected that if the Level III organization had realized that they had a critical problem there that they would have immediately called in and have led to special meetings and special activities and they would have been taken right out of the sequence for STS-51-L and would

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have been a major program activity.

And that appears what might have been underway last summer. But if it was, it did not follow through in any manner that caused the whole program to get involved.

VICE CHAIRMAN ARMSTRONG: Again, following the earlier testimony today, it was the view of the earlier witnesses that the brake programs were in work and they may or may not be technology limited but resource limited and I wanted to add the view of the program office on the brakes.

MR. ALDRICH: I'm really glad you asked me about brakes because that is a subject that has had a lot of attention earlier and deserves it and has also had a lot of effort and I would like to try to give you a feel for the effort that has gone along in parallel with the frustration of a problem we have had a very difficult time solving.

That problem is probably most frustrated by a comment made earlier that we only get one landing per flight and there is no way to have a real flight break test program, so whatever fix you make, you have to fly on the next shuttle mission and then you have to land it and see how well you did.

And sometimes you don't get any answer and

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sometimes you get a negative answer. But you have to repeat the process. We started having brake problems initially in the program. During OFT there were minor problems with some of the components in the brake. OFT was the first four flights. Columbia flew again on the fifth flight.

On the first four flights there were only minor wear and chipping problems on some of the internal parts; but on STS V we had a major failure of the brakes and a heavy landing. And from that point forward, we have had some degree of problem on the brakes on almost every landing.

And the problems have been of two kinds. They are the kind that indicate the system is energy-limited and when you put high energy into the brakes and stop, you do damage to the stators in the system and can run the risk of a wheel lock-up. And I would like to come back to that.

We have had that happen on two occasions; we have had the stator damage and heavy landing loads on STS-V and on STS-IX and we also had a fairly high energy landing, as Captain Young pointed out on STS-61-C and did some stator damage.

And so you can really say we have had three landings with stator damage, and those are of concern

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for damage to the vehicle and for wheel lock-up. On almost all the other flights we've had the kind of damage that is post-flight rework—cracking of carbon pads, chipping, clips bending, all of which appear to have relation to a vibration or an oscillatory condition in the brakes when you apply them or when you use them, which have made everyone very nervous about the brakes, but which have not directly led to a critical problem on the flights where those have occurred. You just have to repair the brakes.

Since the early time in the program, the late OFT time period, we have had a series of repairs and corrections that have been attempted to find the cause of the brake problems and find solutions, and we have been frustrated by this limited amount of time to test them and no really excellent ground facility for a real test.

We also have called a panel of independent experts from the braking industry for brakes for large aircraft on two occasions: In January of 1984 and again in June of 1985 we assembled this panel and had them look at all of our flight data, the fixes we've tried, where we were in the program then and asked for recommendations.

And we have implemented or are in the process of

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implementing all of the recommendations made in the two reviews. However, as witnessed by the last landing in January, we are still frustrated and have not solved the problem.

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The last time we landed at Kennedy Space Center was April 12 of last year, a d that was, in fact—let's see, I think I gave you the wrong flight for the second stator damage. It was 51-D, and it was last April. It sn't STS-9, but anyway, that one landed in April of 1985 at Kennedy runway, and we had a cross wind and therefore a high braking requirement, and we had stator damage, and wheel lock-up before stop, and it caused the tire to blow, and that gave us great concern. We have not landed at Kennedy Space Center since that time and we did not plan to until we implemented a nosewheel steering system that was failsafe, and we worked on that all last year. From that event, the 51-D landing, we worked to put a failsafe nosewheel steering system in to take the steering load off the brakes so that they could be applied for only braking load. And the plan was to land the 61-C flight again back at the Kennedy Space Center.

As Captain Young pointed out concerns for landing at Kennedy relate directly to these brakes. They also relate weather, and on 61-C we tried three mornings in a row to come into Kennedy, assuming our nosewheel steering and our brake system would perform n a satisfactory manner, and we were not able to solve the problem of variable weather just before daybreak, and

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waved off and landed at Edwards. Of course, we had damage again on that flight, and we now I am sure are in a position regardless of a firm decision for the long term, are in a position where we will not want to land at Kennedy again until we see some more significant improvement in the braking system.

We have instituted last summer, in parallel with this, a complete redesign of the system which is long term and expensive, but we have decided the various fixes we have tried to the beryllium-carbon brake system that we have been using throughout the program to date, may never give an adequate solution that will satisfy all of the program rec irements. And so we implemented a detailed design phase for a carbon, all-carbon brake, and that is now ready for implementation. It will take about two years to implement this system, but the design reviews have been held, and it is ready to go, and I am certain the program will be implementing it within the next several months.

VICE CHAIRMAN ARMSTRONG: I am glad you answered the question that way because that leads also into the question of the landing at Edwards that we heard about this morning, and I would like both program office and the operations directorate to comment, if they would. We heard a persuasive argument on behalf of

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the crews, and I would like to hear what the operations directorate, how they have supported or rejected that, and how the program office has supported or rejected that proposal.

MR. CHARLESWORTH: Let me say that follow 51-D, the instance that Mr. Aldrich described where we wound up blowing the tire, one of the first things I did was go talk to then program manager, Mr. Lunney, and say we don't want to try that again until we understand that, which he completely agreed with, and we launched into this nosewheel steering development. We thought with the development of failsafe nosewheel steering and close control of the environment landing conditions, and that means very conservative, that once we got that nosewheel steering system to failsafe, that it was probably reasonable to go back and try the Cape again. And that was the basis for 61-C, and I was satisfied with that

As Mr. Aldrich described, the weather bug got us on that flight. We do play that very conservatively. Sometimes we are not completely popular, but we have held our ground on playing those games conservatively relative to landing at the Cape.

Given the problem that has come up now with the brakes, I think that whole question still needs some

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more work before I would be satisfied that yes, we should go back and try to land at the Cape.

MR. ALDRICH: Two points on that. One, I certainly would not propose to attempt to land at Kennedy again until we think we have a brake solution that is satisfactory and we have tried it enough times to have some level of confidence in it beyond one or two flights.

Over the last six months, it seems to me, in being involved directly in the program and comparing it to what I've been with in the program prior to that time, which was also directly involved with the launches at Kennedy, we have had an extremely significant run of variable weather at Cape Kennedy, and a number of the launch days that I have had to deal with since last July. We have had weather that came right down the middle of the ground rules, which we write very clear, very good ground rules. They are as definable as you can define on paper, and we have the best weather forecasting and assessment people and equipment that you can have, and we have our crewmen in airplanes up in the weather assessing it, and even yet we have had multiple days of very marginal calls. That is, yes, it just barely meets the ground rules, but it may not stay that way, or the other way around, it doesn't but we're sure it's going

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to clear within a short period of time.

And we have had repeated events like that at a much higher degree than I can really feel we had during the earlier several years in the program. And I don't know why that is, but it has become very frustrating. And it occurred to us three times in the landing phase for 61-C.

On the other hand, the runway at Kennedy Space Center was built, that was one of the first things built in the Space Shuttle program, was built with the intent of landing there. I think it was built to all of the known specifications that were put on the table at the time. That is, there were no constraints on it. It is as wide as a football field. It is 15,000 feet long. We have to train there because we have to land there for RTLS, and so one day in the future when our brakes really do work, there periodically are days like the one outside here today where you know you can count on the weather, you know it is good, and I think there should be consideration to sometime later in the program that we use the Kennedy runway in some manner. I would think it would be wrong to draw a finite conclusion at this point other than that we have problems today, and a long period of working brakes before I would want to readdress them.

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MR. CHARLESWORTH: And Mr. Armstrong, I would like to say I agree with that because, you know, we even have runs of days in Houston where it is CAVU, as they call it, and I don't think it is unreasonable to expect that you can pick up runs of weather like that. And given we can figure out a situation with the brakes and be satisfied with that, to go back into Kennedy, but I think you've got to meet all these conditions.

VICE CHAIRMAN ARMSTRONG: Was it possible that just the characterization of a so-called operational vehicle in some ways influenced the desire or pressures or inclination or whatever to land at KSC?

MR. CHARLESWORTH: I think it started out as a program goal, and I think we were trying to do it, and I think in my judgment we ran into problems. And as we saw the problems, we began to back away.

And until we are satisfied we've got a solution to those problems, I don't think we ought to try it again. Once we do, and if we can make the weather cooperate for a reasonable period of time, I don't see anything wrong with it.

MR. ALDRICH: There is one other factor. It is not a major factor, but in the way our resources are deployed in the program today, it takes about six extra days and about $1 million extra for landing at Edwards versus landing at Kennedy, and those are certainly parameters that are easily dealt with, but they are

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factors.

CHAIRMAN ROGERS: But you have to offset that with the fact that with weather conditions being so chancy in Kennedy, every time you have to scrub or change your plans because of the weather, that is costly, too.

MR. ALDRICH: Yes, sir, it is.

MR. HOTZ: Mr. Aldrich, with the advantage of hindsight, this Commission has been accumulating a lot of evidence over the past two months on trends in the Shuttle program which are basically a greatly increased flight schedule in 1986, plus several exotic payloads such as Centaur, balanced against a series of trends, including an increasing number of launch scrubs and increasing shortage of parts and cannibalization, and crew training load that was only able to keep up with the schedule because of the launch scrubs, and a very demanding mission software program that was difficult to maintain, and I just wonder if in any of the meetings that you sat in during the last year or so, that anybody in NASA management was plotting these trends and putting them all together and wondering about where they were leading the Shuttle program.

MR. ALDRICH: Well, I can say yes to all of that except maybe assessing where it was leading the Shuttle program. I think it was leading the Shuttle

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program where we were trying to go.

I have in my organization in Houston a fairly large organization that is responsible for implementing the manifest that is requested by the Agency, and in doing that, we do the detailed flight planning for laying out all of the parameters you mentioned and assessing what is realistic and what is doable and what is achievable and where it is not, and all of the constraints you mentioned are well known, and each is worked with the organizations involved, and within the understood boundaries and parameters we were attempting to meet what the program could achieve in terms of the requirements it was asked to achieve.

MR. HOTZ: And you felt that in a realistic analysis this was achievable?

MR. ALDRICH: Do you mean the manifest for 1986?

MR. HOTZ: Yes.

MR. ALDRICH: I believe it was a good goal, and by setting that goal we would achieve as much of it as was real as we move forward. I think we discussed in other testimony that we set an ambitious manifest and plan, and we work each day to meet it and we deal with each day's problems as they come. We did not meet the goal we set for 1985, even though Captain Young said that it was a fine performance; it did not meet the plan that was laid out. We did not achieve the number of flights that were on the books. And that quite likely could have happened in 1986 also.

Some of the most important things that we believe were on the agenda were the flights with the Centaur early in the summer, and the flight from Vandenberg later in the summer. Those were both very high priority activities in the program and still are not for those same schedules. And they were receiving extensive attention and review, and had great concern about them.

There are a number of safety issues with the Centaur that were a concern before the 51-L that were in work in the program, and those are still in work today, and others are on our books as a result of our re-review.

MR. HOTZ: But nobody expressed any concern that the system faced an overload in 1986.

MR. ALDRICH: Each of the areas you mentioned expressed concerns, and those concerns were worked in detail. Specifically, the ones with crew training and with the software deliveries were worked directly with Mr. Charlesworth, and we attempt to understand what the program would like to do and understand what is believed by the implementing organizations can be done, and we

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put on paper what we believe is an achievable plan.

Granted, the longer you project it downstream, perhaps the more likely it is you will not be able to achieve it.

MR. ACHESON: So I take it you do not agree with the comment that was made at some point earlier today that the impact of spare parts shortages would really have begun to slow the program very seriously by the middle of 1986?

MR. ALDRICH: It was impacting the program seriously. I am not sure that it would have slowed the program. For instance, our plan for 1985 was to land each of the flights at Kennedy Space Center, which saves five or six days per flight, and the manifest was built originally assuming that we would have each of those five or six day periods. After the blown tire accident I discussed, we diverted and delivered all of our orbiters to Edwards, and turned around and brought them to the Cape, and we found in fact even in doing that, that did not pose significant impact on our manifest, even though we would have predicted before the fact that it would have.

So, I believe the plan for 1986 was workable. It was packed as complete as it could be packed for an optimistic shot at the things the program

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and the Agency would like to have done, and we would have clearly changed it as we crossed the line where something could not be continued in the manner it was structured.

DR. WHEELON: Mr. Chairman, I would like to invite the comments of Admiral Truly and his colleagues, on the following proposal.

If we take ourselves back to early January, the country had four orbiters, we had a base in Florida and were preparing a base in Vandenberg from which the Shuttle could be launched. It was due to go into operation in the summer of 1986. As we sit here today, we have three Shuttles and we are still proposing to bring into operation that base at Vandenberg. If we do so, the operations and maintenance costs for that base will be about $350 million to $400 million a year we will pay this price whether we launch or not, just in order to maintain the option to do so.

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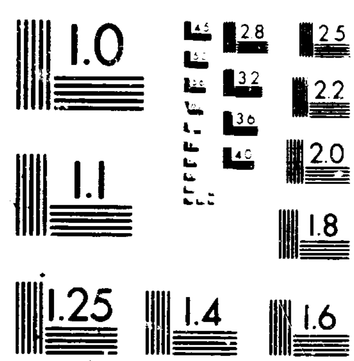

MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS
STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

Now, with only three Shuttles left, why doesn't it make sense to concentrate those three at Kennedy and forgo the beginning of operations at Vandenberg for a number f years until the fourth orbiter is available?

I make that proposal because it is quite clear that there is no compelling reason to open Vandenberg

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All of the launches going out of there can be accommodated by the expendable launch vehicles or can be moved to Florida.

And in that connection I invite your comments on the following. One is the apparent problem in the design of the flame bucket at Vandenberg which will require correction anyway. The second is what risk you attach to the use of the new filament wound cases on the solid rocket boosters rather than the steel cases which are used from Florida?

That is a lot, but that is the essence of the proposal. In other words, why not delay Vandenberg, concentrate our Shuttle assets in Kennedy and save the O&M costs which are very significant in these days of tight budgets?

ADMIRAL TRULY: Let me make a couple of brief comments, and then I will turn it back over to Arnie.

There has been a tremendous expenditure of national funds to get the Vandenberg launch site into operation, and we have been working, and I know in the last few weeks since I have been back on board, have worked extremely hard with the Department of Defense to work these very issues that you bring up. And they are being considered.

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I personally don't see an impediment to us getting Vandenberg into operation because we only now have three orbiters for the immediate time. Your point on expenditure of O&M costs is certainly a valid one, and as I said, we are working that with DOD.

As to the filament wound case issue, I view that as—well, let me back up. As to the launch facility issue, that also is being worked very diligently, primarily by the U.S. Air Force, and the use of that facility on the schedule that we can support it with a flight vehicle is dependent on those efforts being successful.

Again, with the filament wound case, to me that is clearly a development and qualification issue, and assuming that the design passes its development and qualification testing, I am personally comfortable with a flight, including the first flight off Vandenberg, using the filament wound case.

And so the bottom line from a strategy point of view is that as long as our discussions with the Department of Defense indicate that that is in the national interest to continue to use the Vandenberg launch site beginning or shortly after we come back into flight status, we will continue to support that.

MR. ALDRICH: I would second almost all of

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what Admiral Truly said.

The filament wound case is a separate issue, and it needs to be understood, and the qualification needs to be understood, and when we understand it and feel we're confident with it, I think we should use it.

The flame bucket issue, the entrapped hydrogen in the main engine flame bucket at Vandenberg, I think we have several ways to solve that problem and can solve it consistent with the

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schedule we are on that would lead to a first flight out of Vandenberg following the resumption of the Shuttle flights.

In terms of whether we should do it, I personally believe in a two-coast launching system and a four orbiter fleet, and I believe some of the plans that have been expressed for the nation at the end of this decade and on into the next decade really require a four orbiter fleet.

DR. WHEELON: The suggestion was quite in line with that, but we don't have four orbiters now, we have three, and that will be our situation for several years. The question was simply to delay the beginning of operations at Vandenberg until that fourth is available. That was the proposal.

MR. ALDRICH: Well, and the amount of delay, of course, would be another question. There is some

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advantage to the program uniquely in the Shuttle vehicle to continue to get a launch at Vandenberg as early as we can. The launch environment, the ascent environment going out of Vandenberg is different from Kennedy Space Center, and we specifically have instrumented the orbiters for their first flights out of Vandenberg to take measurements which will characterize that flight regime and give us an engineering indication of the performance of the vehicle on the west coast as a launching system.

So there are benefits not only bringing the launching system on line, but to completely round out the understanding of the Shuttle design requirements and its ability to meet them in continuing to fly.

DR. WHEELON: I missed what are the benefits of bringing the facility on line?

MR. ALDRICH: Well, I may not have said any benefits for bringing it on line except that I believe it is an advantage to continue through with the process of checking and verifying the facility and bringing it to the point of flight readiness to complete that work which as been in work and delayed for some lengthy period of time for a variety of reasons over the years.

DR. WHEELON: There is an old role that every accountant knows, it isn't how much money

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you have paid out so far in the project, there is only how much is left to be paid. It is a hard theorem to accept, but it really is true. I submit that the $3 billion that we have invested thus far in Vandenberg is irrelevant to what we do from here on.

Do you agree with that?

MR. ALDRICH: I shouldn't think that would be a major priority in deciding what we should do forward.

DR. WHEELON: I thought I heard Admiral Truly make that argument. Maybe I was wrong.

ADMIRAL TRULY: No. The argument or the statement I meant to make was to point out that the nation has invested a great deal of its treasure in that facility and that we are responding to the Department of Defense in their wish to get the facility on line. The missions out of there are national security missions, and we are responding to that desire and are working it extremely diligently with them to look at the available options.

DR. WHEELON: Thank you very much.

CHAIRMAN ROGERS: Mr. Aldrich, since you were here last there have been certain comments made, public comments made that media criticism of NASA, some of it was very harsh and unfair, prior to launching 51-L, may have influenced you in your decision making, and I assume

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that you and Mr. Jesse Moore really are the two key people in making that decision to launch 51-L.

Did the media criticism or comments influence you to vote go on that launch?

MR. ALDRICH: No, they did not. Media criticism would not have that effect of any of that variety on my actions officially within NASA.

CHAIRMAN ROGERS: And that would also be true, I am sure, of Mr. Moore?

MR ALDRICH: Well, I can't speak for him, but I would be amazed if he would answer in any other manner.

CHAIRMAN ROGERS: Thank you.

MR. ACHESON: I have a question relating to Orbiter 103. On any reasonable assumptions of funding and availability of spare parts and progress and repairs, what would be the earliest you think that Orbiter 103 would be, let's say, repaired of its cannibalization and could be ready to the point of being considered an available orbiter for the fleet?

MR. ALDRICH: 103 has three major classes of modification going into it. None of those have to be made to cause it to be an orbiter available for the fleet. In fact, if we were able to launch, we could launch any of our three orbiters within a few months,

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depending on their current configuration today.

However, 103 has been delegated to receive the Centaur modification configuration, and that will be done over the spring and summer in Florida. We are also looking at an additional safety feature for the orbiters that will fly the Centaurs, and that is to take the Centaur hydrogen vent which currently comes out beside the vertical tail on the fuselage and move it to the tip of the vertical tail so that it will exhaust the hydrogen away from the rest of the vehicle. That is an extensive mod, and it is a new mod. It is starting on the drawing boards in this timeframe, but it will take ten months for that implementation to be able to begin, and then a down period on the orbiter to install it.

Both of those to 103 are only required to enable it to fly the Centaur. The third modification is the completion of a series of modifications to the orbiter wings and underbelly to strengthen the 103 and 104 configuration orbiters for the west coast environment, the west coast flight environment. That has been known as a requirement for approximately nine months to a year, but the parts had to be made, the kits had to be made, and then the work periods had to be scheduled, and they were going in incrementally in Florida because the Florida flights could proceed

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without them.

103 is scheduled to receive the remainder of those mods between now and the end of the summer. If, however, we had a requirement and a capability to fly in that timeframe, any of that work could be deferred, and 103 is flightworthy, as is 104 or 102.

VICE CHAIRMAN ARMSTRONG: Regarding the Centaur, I would like to ask Admiral Truly if it would be his intention to have the, in view of your recent memo directive and so on, to have the Centaur maintain the same standards of safety and flightworthiness as other payload packages in the past.

ADMIRAL TRULY: Yes. I wrote that portion of the memorandum very carefully after working with both Arnie and a number of other people, and what it says in that section, it has to do with some guidance for the first flight of the orbiter when we return and the first year of operations, and in the case of the first flight, I specifically laid on some guidance like a day launch and a day landing at Edwards, etc., in order to allow the Level 2 to do its normal job of looking at many options. The guidance for the remainder of the first year was much more general in nature but laid out some guidelines so that Level 2 could steer its course.

And what it says is that we intend in general

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to fly the first year of operation within our flight experience. That is, if we have flown the engines at, say, 104 percent, we will remain within 104 percent unless whatever the new thing that we do, including classes of payloads, which includes Centaur, goes through the same sort of conservative scrutiny for flight safety as we are putting the rest of the system to.

So in a word, the answer is yes, if we fly Centaur or any other new class of payload, we will apply that rigor to it.

VICE CHAIRMAN ARMSTRONG: Just one last quick note to that in the subject of Criticality 1 kinds of things. I think we heard this morning that it was the feeling by some at least that Criticality 1 items should deserve some special treatment as they proceeded through processing and flight readiness and inspection and so on. Other views apparently in the past have been that if you treat Category 1 specially, then the Category 2 kinds of things might get unduly slighted and you want to be cautious about that.

Do you have a feeling that there is a practical way to treat Criticality 1 items with some special care? Can that be done?

ADMIRAL TRULY: Well, I should let Arnie I

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think expound a little bit on where he is because we haven't had a lot of opportunity in the last week or so to discuss it, and I know he has done the most detailed work and is closest to the CIL review, but in the guidance it clearly states that we will do two things in the time we have in there, the two most important things that I think that we should do.

First is to relook at each Criticality 1 and 1R item from the ground up, and that work was already in process and had been started by Level 2 prior to, as many other things had been, prior to my memorandum. As to Category 2, we intend to have a review to make sure that none are miscategorized and have somehow slipped through the crack and in fact should be categorized at a more critical level.

Beyond that level of detail, I might ask Arnie to comment.

MR. ALDRICH: Well, a lot of the things in the program that are Criticality 1 items have been defined and accepted, I think as you have been briefed, in a general category. That is, they are things that are structure or they are things that are vessels, and therefore they are not other than inspections for use or assessments of engineering use. They are basically static kinds of items, and they don't undergo a lot of

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unique testing from one flight to another.

The situation with the SRB joint is quite different. That is a very dynamic and active set of components, and when it is called Criticality 1, it implies a whole lot more regular work and rework and activity, and one of the proposals that has been implemented in this review or rereview of our activity is that we tie all of the operations, maintenance instructions at KSC, all of the formal procedures for working and handling the equipment on Criticality 1 items directly back to the design centers for formal review and configuration control.

In the past in the program the operations and maintenance requirements documents have come from the design centers, have been maintained by the design centers and controlled by them. But the Kennedy Center has implemented those requirements through another set of documentation called the Operations and Maintenance instructions, and those have not been reviewed back through the rest of the program for formal concurrence and control.

And as part of this rereview, we are going to institute that kind of control on all OMI's that relate to Criticality 1 and Criticality 1R hardware. Specifically, the reviews I am talking about are the

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FMEA CIL review which was started several weeks prior to the time Admiral Truly's memo asked us to start it. That is being headed by Bill McCarty out of the Level 2 safety office but involves all elements of the program across the projects and the contractors and up through myself, and then to the Level 1 organization.

There is also a complete rereview of all of the OMI's at KSC, and that is under the lead of Bob Sieck at Kennedy, and it is in that review where we will tie the CIL work and the OMI work into a new process that gives this additional assessment and control.

And then the OMRSD's are all being re-reviewed also by the various projects, and that is being led by Mr. Bill Fisher at my level office at JSC.

And so that is a very specific and finite answer to your question. Perhaps you had a broader implication as well. That is the most specific thing that comes to mind, and I think it is a very positive step in that regard.

GENERAL KUTYNA: Mr. Charlesworth, in your role as operations, this morning we talked about safety, and crew survivability quite a bit. As we have flown the Shuttle, the crews have varied in number anywhere from two in the early development days to eight recently. In his last chart, Hank Hartsfield said that

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the STS is not operational in the traditional sense and the risks will always be high. In light of that and what we have learned recently, have you given any consideration to limiting the crew to the minimum essential size to do the job; and what would that size be for the various missions?

MR. CHARLESWORTH: The answer to your question is no, I haven't considered it a great deal to this point, but I think as we go back through reflections on where we are and what has happened, that will be a question we ask ourselves. I can't give you an answer as to what the minimum size is. We started out, clearly we have flown with as few as two, but that is a tremendous workload. It is very difficult to do. Three, four, we have debated on occasion because of space station, but I am sure we will address that as part of our reflection on where we go from here.

CHAIRMAN ROGERS: If there are no other questions, I would like to close with an easy question to Admiral Truly.

First, I want to compliment you on the excellent work you have been doing, and we appreciate it very much, and ask you, do you agree with the testimony this morning that astronauts are a very important management resource?

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(Laughter.)

ADMIRAL TRULY: Astronauts come in various varieties, and some of them I think would make outstanding managers in the Shuttle program, and in various parts of NASA, and some frankly would rather remain in the cockpit. As a matter of fact, most of them would rather remain in the cockpit, but I think there's a lot of men and women in the astronaut office that would serve very well in a number of areas, particularly in this next year or so as we get the system back on its feet. And we fully intend to provide that opportunity to selected people in selected positions.

CHAIRMAN ROGERS: Good. Good idea.

Thank you very much.

(Whereupon, at 3:45 o'clock p.m., the Commission recessed, to reconvene at the call of the Chair.)

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MEETING OF THE PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—FRIDAY, MAY 2, 1986

MIC Conference Room 10214
Department of Transportation
400 7th Street, S.W.
Washington, D.C.
10:05 a.m.

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ATTENDANCE:
WILLIAM P. ROGERS, Chairman
NEIL A. ARMSTRONG, Vice Chairman
SALLY RIDE
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA
ROBERT HOTZ
EUGENE COVERT
RICHARD FEYNMAN
ARTHUR WALKER
JOSEPH SUTTER
ALBERT WHEELON
ROBERT RUMMEL
ALTON KEEL, Executive Director
RANDY KEHRLI, Staff Investigator

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MORNING SESSION

PROCEEDINGS

CHAIRMAN ROGERS: I call the Commission to order.

Mr. Mulloy, Mr. Wear, we have been considering the history of the joint that failed, and in connection with that we have been considering the history of it going back several years. The purpose for the meeting this morning is to discuss some of the documents that relate to the history of it and give all of you who were involved in its development and the experience that you have had with it the opportunity to refer to some of the documents that have been provided to us.

This will be on the record. I assume—has Mr. Wear been sworn?

MR. WEAR: I have not.

CHAIRMAN ROGERS: Well, let me finish. Then we will do that. We decided to have this in executive session because we want it to be informal, and secondly, we want to deal with several documents. In an exploratory kind of discussion of this kind, it isn't really suitable for public session, but we will make the testimony public at some time later on, because we may use some of it in our final report, and we thought that we would have one of our staff members refer to the documents, Mr. Kehrli, so that you can identify the

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documents, so that we are all talking about the same documents, and then he will ask some questions about those.

It is our intention to end the hearings of the Commission today. We do not plan to have any further hearings, so this will be the last hearing. If we might swear Mr. Wear.

(Witnesses sworn.)

CHAIRMAN ROGERS:

Randy, do you want to start, please?

MR. KEHRLI: Thank you, Mr. Chairman. Gentlemen, in front of you you have a book that contains the documents that we would like you to refer to today. I am going to refer to them specifically and read portions of them into the record, and you might want to follow along while I do that, before the Commissioners begin asking you questions.

That booklet is divided into two sections. The first one is a major tab, O-Ring History, and the attachments run, the tab attachments run from Number 1 through Number 35. The second major tab, the second division is Launch Constraint, in the back, and those attachments run from 1 through 4.

I would like to direct your attention first of all to the Launch Constraint Attachments 1 through 4.

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During the course of the investigation, either as a result of interviews or specific requests by the Commission to NASA, we received some of these documents, as the Chairman indicated to you.

The first document I would like to refer to is Launch Constraint Attachment Number 1, which is a document dated September 15, 1980, to distribution from Mr. Lindstrom, and the subject of this memorandum is signing launch constraints on open problems submitted to Marshall PAS. This is in the back of the book, in back of the Launch Constraint section, Attachment Number 1. [Ref 5 2 1]

The memorandum reads, "The following guidelines have been established to aid in making constraint decisions on open problems and are limited to recurrence control determination only. In accordance with practices established on past programs, remedial actions, for example, removal and replacement of defective hardware, et cetera; for correcting discrepancies on the vehicle to be launched are considered launch constraints and are tracked by the KSC system."

This is an excerpt from the document. I haven't read it in its entirety. It is Paragraph A. Subparagraph 1 reads, "All open problems coded Criticality 1, 1R, 2, or 2R will be considered launch

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constraints until resolved, which is recurrence control established and its implementation effectively determined, or sufficient rationale; in other words, different configuration, et cetera, exists to conclude that this problem will not occur on the flight vehicle during prelaunch, launch, or flight."

Now, I would like to direct your attention to Attachment Number 2 of the Launch Constraint section, and this attachment is a Marshall problem assessment system. This document was in fact obtained from Marshall. It is dated February 26th, 1986, and it is my understanding, and of course you are free to correct me on any of this, that this is a chronological history of entries from the Marshall problem assessment system.

The relevant items on this document that we would like to have you address include the fact that the document indicates that a launch constraint was assigned to STS-51F, 51I, 51J, 61A, 61B, and 61C. The date, I believe that is R-e-c—which apparently means recorded or received—over on the right is July 10, 1985. That matches with the first entry on the document, which is dated July 11th, 1985, and it indicates that post—the entry on the 11th indicates that post-flight inspection, SRM-16A revealed a gas path through the vacuum putty at 54 degrees, and it goes on to describe essentially the

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erosion problem in the nozzle joint, I believe, in 51—STS-51B, which was inspected on June 25th at Thiokol, in 1985.

There is an indication that the constraint closure recorded was December 18, 1985, in the document under the date recorded, and without belaboring the further entries on the document, it indicates that the problem was "not considered a constraint to 51F; 100 psi leak check is performed, which confirms seating of the secondary O-ring. The nozzle O-rings have been shown to survive erosion, gaps of 125 mils in hot subscale test," and it continues to discuss the math model, as you can read.

Additionally, there are individual entries later on in that document which indicate that the constraint was lifted for subsequent flights that were listed previously, 51I, 51J, 61A, 61B, and 61C.

Finally, there was a handwritten notation on the last page of the document that says: "constraint can be lifted by project manager," and this notation was on the document when we received it. Additionally, the last entry on this problem assessment report dated 1/23/86 indicates that there was a resolution: "In the SRM, field and nozzle joints have experienced erosion of the primary O-rings during several missions and static

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tests as determined by post-flight inspection," and it continues to go on through that entry and give the rationale for closure of the problem.

The last paragraph reads: "This problem is considered closed based on MTI report TWR-14359, Revision A, improvements in Space Shuttle SRM motor seals dated 8/30/85, and MTI letter E150BGR86144, rationale for closure of the O-ring erosion problems."

The last important information on this document is the—at the bottom of the first page there is an indication of assignee and approval, and the names there are R. McIntosh, D. Newman, L. Wear, J. Fletcher, and there is an indication of the status of the document: "PAC review is complete." The status is still marked open, Code M, I believe.

Finally, or additionally, the next attachment, Attachment Number 3, is a similar problem assessment which was initiated on February 17th, 1984, and summarized in this document. This is a problem assessment begun after the erosion, the O-ring erosion problem on Flight 41B, and it again tracks that problem up until the time the problem was closed, which is the same date as the nozzle, the previous nozzle problem. 1/23/86. [Ref. 5/2-3]

Again, the same names are on the bottom. The difference, the key difference on this document is under

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"launch constraints" it says "none," and there is no indication on the document that a launch constraint was attached to this particular problem assessment report on the field joints.

Again, I won't go through the various entries. You gentlemen are probably familiar with them, and the Commission has seen them. The closure entry is the same on this document as it was on the nozzle, the nozzle document described previously.

CHAIRMAN ROGERS: May I ask, how did this first document you referred to come to our attention?

MR. KEHRLI: We first discovered through an interview conducted by one of the investigators at Thiokol that there was a monthly problem report that Thiokol filed internally which tracked the various problems that arose during and after a particular flight, and tracked the problem until it was worked and/or closed.

We discovered in one of those entries a reference to a specific Marshall problem assessment report, and it indicated that there had been a tracking number, a problem number assigned to the 41B erosion problem, and also to 51B, and additionally DM7, I also believe, had a separate report. Then upon talking to George Hardy and, I believe, Mr. Mulloy,

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Marshall searched their records and found these documents which you have in front of you, the Marshall assessment report. So these are in fact Marshall documents.

CHAIRMAN ROGERS: And that was after our public hearings, so that was how we found this document?

MR. KEHRLI: Yes, this was after the public hearings, and we found these in the course of interviews.

CHAIRMAN ROGERS: Just to be sure the record is complete, this J. Fletcher is a separate J. Fletcher from the Fletcher that has been nominated and affirmed, I guess, as administrator.

MR. KEHRLI: Yes.

CHAIRMAN ROGERS: Well, I think what the Commission would like to hear from you, Mr. Wear and Mr. Mulloy, is an explanation of this, first, why we didn't know about it, and secondly, what gave rise to the launch constraint, how they were handled, who made the decision to waive on all of these flights. I guess these were the flights that preceded 51L.

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: And why it was closed out just before 51L.

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TESTIMONY OF LAWRENCE B. MULLOY, MANAGER, SOLID ROCKET BOOSTER PROJECT OFFICE, AND LAWRENCE O. WEAR, MANAGER, SOLID ROCKET MOTOR PROJECT, MARSHALL SPACE FLIGHT CENTER

MR. MULLOY: Okay, sir. I will start to address that, and then Mr. Wear, I think, can elaborate.

The problem assessment system was put in place to provide visibility throughout the shuttle system for the types of problems that do occur, not just in flight, but also in qualification tests, and in failure of hardware that is back for refurbishment at a vendor or whatever. And it is a closed loop tracking system that lists the anomaly.

Now, in accordance with the memo there from Bob Lindstrom in September of 1980, the procedure was established that in all flight readiness reviews, all open problems would be flagged at the flight readiness review, and it is our quality organization that does that, and that requires them when the problem is not completely closed out, it required dealing with it in the flight readiness review process, as I testified in both the previous private and public hearings, that these anomalies were covered in the flight readiness review.

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There was a cross-check by the quality people to assure that the problem had been discussed and resolved, a rationale for making the next flight, in view of the fact that the problem had not been completely eliminated in making the flight with the acknowledged chance that there would be a recurrence of the type of thing that had been observed in the initial problem report, so on each flight up to, from the time that the constraint was shown, it required a signature by the project office, which is Mr. Wear's office for the solid rocket motor, that that had been addressed and closed in the flight readiness review, and it's required, his initial, essentially on the problem assessment, and where you see on the problem assessment there, I believe there is a JWT initial in there for each one of those. That JWT is Jim Thomas, who works in Mr. Wear's office, and is director of his engineering branch in the project office.

Now, the entry that is shown in there that the problem was closed prior to 51L is in error. What happened there was, one of your documents here which we did not discuss is the letter from Mr. McDonald to Mr. Wear which proposed that this problem be dropped from the problem assessment system and no longer be tracked for the reasons stated in Mr. McDonald's letter.

That letter was in the review cycle. The

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letter, I believe, was dated 10 December 1985. It came into the center, it was in the review cycle. After Mr. Wear brought this letter to my attention, my reaction was, we are not going to drop this from the problem assessment system because the problem is not resolved and it has to be dealt with on a flight-by-flight basis. [Ref. 5-2-1]

Since that was going through the review cycle, the people who run this problem assessment system erroneously entered a closure for the problem on the basis of this submittal from Thiokol. Having done that then for the 51L review, this did not come up in the flight readiness review as an open launch constraint, so you won't find a project signature because the PAS system showed the problem was closed, and that was an error.

CHAIRMAN ROGERS: Who made the error? Do you know?

MR. MULLOY: The people who do the problem assessment system.

MR. WEAR: Mr. Fletcher, and he reports within our quality organization at the flight readiness review, at the incremental flight readiness reviews, as I think have been described to you before. There is one from Thiokol to me, and there is one from my group to Larry, and then Larry, of course, does one with the Shuttle

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project office, and so forth, on up the line. At my review and at Larry's review, there is a heads up given to the quality representative at that board for what problems the system has open, and they cross-check to make sure that we address that problem in the readiness review.

On this particular occasion, there was no heads up given because their PAS system considered that action closed. That is unfortunate.

CHAIRMAN ROGERS: Let's go back just a bit, because I think it is helpful to me if you—you use words that I understand a little bit. What caused the constraint to be put on in the first place?

MR. MULLOY: The constraint was put on after we saw the secondary O-ring erosion on the nozzle, I believe.

CHAIRMAN ROGERS: Who decided that?

MR. MULLOY: I decided that, that that would be addressed, until that problem was resolved, it would be considered a launch constraint, and addressed at flight readiness reviews to assure that we were staying within our test experience base.

CHAIRMAN ROGERS: And was that—what is meant by the problem description?

MR. MULLOY: Which document, sir?

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MR. KEHRLI: That is Number 2.

MR. MULLOY: Yes, reference similar reports. Yes, sir.

DR. RIDE: Why didn't you put a launch constraint on the field joint at the same time?

MR. MULLOY: I think at that point, and I will react to that question in real time, because I haven't really thought about it, but I think the logic was that we had been observing the field joint, the field and nozzle joint primary O-ring erosion. This erosion of a secondary O-ring was a new and significant event, very new and significant event that we certainly did not understand. Everything up to that point had been that the primary O-ring, even though it had experienced some erosion, does seal. What we had evidence of was that here was a case where the primary O-ring was violated and the secondary O-ring was eroded, and that was considered to be a more serious observation than previously observed.

DR. RIDE: Correct me if I am wrong, but weren't you basing most of your decisions on the field joint on analysis of what was the maximum, what you believed to be the maximum possible erosion, and you had that analysis for the field joint and for the nozzle joint. When you saw the complete erosion of the primary

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O-ring on the nozzle joint, that showed you that your analysis on the nozzle joint wasn't any good, I would think. That would indicate to you that your analysis on the field joint wasn't very good, either, or at least should be suspect.

MR. MULLOY: The conclusion, rightly or wrongly, for the cause of the secondary O-ring erosion on the nozzle joint, it was concluded from test data we had that 100 psi pressurization leak check, that the putty could mask a primary O-ring that was not sealing. The conclusion was—and that one was done at 100 psi. The conclusion was that in order to get that type of erosion that we saw on the primary O-ring, that that O-ring never sealed, and therefore the conclusion was that it never was capable of sealing.

The leak check on subsequent nozzles, all subsequent nozzles was run at 200 psi, which the test data indicated would always blow through the putty, and in always blowing through the putty we were guaranteed that we had a primary O-ring seal that was capable of sealing, and then we further did, and we already had that on the field joints at that time.

DR. RIDE: The 200 psi check on the field joints were started about a year earlier. Is that right?

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MR. MULLOY: I don't recall specifically where. I don't recall the dates.

MR. WEAR: I believe it was—I think there was a separation of about three flows, I believe.

MR. MULLOY: So what concerned us about the nozzle joint was that we had, as you have stated, Dr. Ride, we had to go back and then look at the analysis for a violated primary O-ring, so the rationale for the proceeding with flight, having looked at the occurrence on the nozzle joint, another analysis was run which included violation of the primary O-ring to determine what is the maximum erosion that could occur on the secondary O-ring.

That analysis matched very well with the observations that we had from the flight that that occurred on, and that was corroborated by test, and that was the rationale for proceeding with the next flight, and whether or not it was treated as launch constraint in the past, it was treated as an issue to be discussed in each one of the flight readiness reviews on the field joints as well as the nozzle joints.

DR. WALKER: Were you at all concerned about violations of the integrity of the putty, which was really the first line of defense in this joint? In fact, apparently you were deliberately trying to violate the putty by going to 200 pounds per square inch.

MR. MULLOY: Yes, sir. We were concerned with

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violation of the putty, and as has been laid out during this investigation, there was a program under way to find something as an alternative to the putty. We were not deliberately trying to violate the putty. What we wanted to be sure was that we got a leak check on the primary O-ring, and the only way you could be sure you got a leak check on the primary O-ring was to put a pressure between the two rings that would, if the primary did leak, that it would blow through the putty.

DR. WALKER: Well, all right. Then if you found that—well, if you were going to violate the putty, there was no procedure you had which could determine that. You could violate the putty in your test, and yet you would never know that, and yet that was an integral part of your protection, and no actions were instituted to address that problem, as I understand it.

MR. MULLOY: Oh, yes, sir, that was the whole test program that was laid out, to find an alternative to the putty.

DR. KEEL: Mr. Mulloy, didn't it occur to you that if the putty could be masking a leak check, then the putty could sure be masking pressurization actuation of the primary O-ring, and hence your whole analysis about how that primary O-ring sealed and the time it

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takes to seal could be just as suspect?

MR. MULLOY: No, sir, it did not. The pressurization is an entirely different direction.

DR. KEEL: Of course, the putty is still a barrier in both instances.

MR. MULLOY: Yes, sir. And the putty has a substantially different pressure carrying capability, depending upon which side you pressurize it from.

DR. KEEL: That was your assumption?

MR. MULLOY: That is a fact.

DR. KEEL: Do you still think the putty doesn't have anything to do with the pressurization of the primary O-ring?

MR. WEAR: You are speaking of today?

DR. KEEL: Yes.

MR. WEAR: Well, today there has been quite an extensive testing conducted.

DR. KEEL: What is your answer?

MR. WEAR: Well, I think that the testing that has been done has demonstrated that the time delay factor to the putty, as I understand, and I haven't been that close to the investigation per se—

DR. KEEL: So regardless of which side you pressurize, it has an effect of delay.

MR. MULLOY: The putty is highly variable, and

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we understand that its ability to sustain or not sustain pressure is unpredictable.

DR. WALKER: But you were not aware of that earlier. Is that correct?

MR. MULLOY: Yes, sir, we were aware of it, because sometimes we saw paths through the putty, evidence of paths through the putty. Sometimes we saw evidence of paths through the putty and O-ring erosion. Sometimes we saw no paths through the putty and no evidence of hot gas past the putty, and so we knew that it was performing in that variable manner.

DR. WALKER: The evidence of paths through the putty that you speak of, was that evidence directly looking at the putty after demating, or was that by implication because you saw O-ring erosion?

MR. MULLOY: Both. It was looking at the putty and at the joint, looking for evidence of soot between the putty and the primary O-ring, and the distribution of that soot, and the evidence of the path, hot gas path through the putty.

DR. WALKER: So you knew that in most cases pressurization was occurring by leaks through the putty?

MR. MULLOY: I don't believe it is in most cases. No, sir, because we had a very limited number of observations of the 170 some odd joints. There is a

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very limited number of observations where you have pressurization by breakdown of the putty. The rest of them, there is no evidence of paths through the putty.

DR. WALKER: But there was a memo from the Titan program suggesting that pressurization, at least in the case of Titan, was occurring primarily through blow holes in the putty. Did you receive that memo?

MR. MULLOY: Yes, sir. You are speaking of, I think it was on your list for about a March, 1984, memo that I got from George Morefield. Yes. [Ref. 5/2-5]

DR. WALKER: And I think that was passed on to you from the chief engineer's office at headquarters. Is that correct?

MR. MULLOY: No, sir, that was written directly to me from Mr. Morefield at my request, observing this problem, asking CSD and Hercules to look at this problem and give me their observations about what they thought was happening and what could be done to rectify it, and so that is what we were working on.

DR. WALKER: So did you accept their view that pressurization in the shuttle was occurring primarily by blow holes, or did you think that the situation was different in your booster than the Titan?

MR. MULLOY: Yes, sir, I thought it was different based upon the evidence that we had that we

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had a lot of joints that didn't have any blow holes in them, and evidence that—no evidence of any kind of a hot gas past the putty. And that is an established fact, I believe.

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CHAIRMAN ROGERS: Assuming that you were advised as you were by Thiokol that they opposed the launch on the 27th because of weather, would you have reacted differently?

MR. MULLOY: No, sir. Frankly, I was not aware that this erroneous entry had entered in the PAS because it did not come up.

CHAIRMAN ROGERS: To you what does a constraint mean, then?

MR. MULLOY: A launch constraint means that we have to address the observations, see if we have seen anything on the previous flight that changes our previous rationale, and address that at the Flight Readiness Review.

CHAIRMAN ROGERS: When you say "address it," I always get confused by the word. Do you mean think about it? Is that what you mean?

MR. MULLOY: No, sir. I mean present the data as to whether or not what we have seen in our most recent observation, which may not be the last flight, it may be the flight before that, is within our experience base and whether or not the previous analysis and tests that previously concluded that was an acceptable situation is still valid, based upon later observations.

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CHAIRMAN ROGERS: Okay. So in this case, because you didn't know that the constraint had been closed due to an error, if the constraints were still on, if no error would have been made, you would still have reacted the same way?

MR. MULLOY: Reacted to what, sir?

CHAIRMAN ROGERS: Reacted as you did on the telecon.

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: And so each time one of these flights took off that you knew there was a constraint on, you had to make a decision to waive it, what went through your mind?

MR. MULLOY: Okay. What went through my mind is, we looked at the most recent observation of recovered hardware. We compared what we were seeing to our previous successful experience with the joint, realizing that we were having some O-ring erosion, looking if we were seeing anything that changed the previous rationale. That is what led to opening the problem report, when we saw that we violated the primary O-ring. That was something that was different and therefore required additional analysis and test and until that was done.

CHAIRMAN ROGERS: But what did you do about

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it, though? It seems to me in that case, when you say that you addressed it, no change was made in it. All you did on these waivers was to waive it. You just apparently—there is no indication. What did you do? There is nothing in this chart that suggests that you corrected the joint.

Each time there was further experience, further erosion, and we don't see any examples of correction or effective action taken or anything of that kind.

MR. MULLOY: I guess I would have to take issue with that, sir. On page 2——

CHAIRMAN ROGERS: That is why we want you here.

MR. MULLOY: On page 2 of 3 of document No. 2, if you look at--and I will just pick one—and I think there's a rationale here. It says this problem is not considered a constraint to 51-F; a 100 psi leak check is performed which confirms seating of the secondary O-ring. Nozzle O-rings have been shown to survive erosion depths of 125 mils in hot subscale test. The math model predicts maximum erosion depth for the secondary O-ring to be 75 mils in the worst case condition. [Ref. 5-2-2

The erosion on the secondary O-ring was 32

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mils, well below the demonstrated sealing capability.

What was done was analysis and test to substantiate that rationale for accepting that condition for that flight. It is not just a matter of nothing was done.

DR. WALKER: You are relying on the secondary O-ring.

MR. MULLOY: No, sir. What we were saying was if we got a repeat, we didn't think we would get a repeat of the condition because we had gone to 200 psi and it was concluded that, we knew the putty could mask 100 psi leak check and we could have had a bad O-ring primary to begin with on that one.

We were sure on the 51-F flight because of the 200 psi leak check that we had a good primary O-ring was a substantial part of the rationale. The second then was if the primary O-ring was violated, the maximum erosion that could occur on the secondary O-ring was only 75 mils, which tests had shown could sustain 125 mils.

Now, rightly or wrongly, that was the rationale.

DR. WALKER: So you then were relying on the secondary O-ring in that case.

MR. MULLOY: We were relying on the

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redundancy, yes. We showed that we had redundancy that, should the primary O-ring fail, the secondary would function.

DR. WALKER: Well, I guess we are going to address the case of the criticality later, and so I think I will save those questions.

MR. RUMMEL: Mr. Mulloy, I wonder how detailed your investigations were when you applied the experience of one flight to a subsequent flight.

For example, did you consider the differences in dimensional tolerances and dimensions of the various joints—out of roundness, they varied in diameter, they varied in numbers of ways—and so when you evaluated a specific flight, did you look into where the previous flight or flights stood in this regard and then analyzed what would be applicable on a specific flight under consideration?

MR. MULLOY: Out of roundness? No, sir. We gave no specific consideration to a variation in gap that could occur due to the out-of-round condition.

What we did do was look at the dimensional tolerances for the specific flight, the tang dimension and the clevis dimension, to assure that we had the minimum O-ring squeeze that was specified and assure that that was within our experience base.

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We did not go in and look at what would be the effect of out-of-roundness and possibly a higher squeeze on O-rings in a local area of the joint.

That is a revelation, I think, that has come from the investigation of the 51-L failure.

DR. FEYNMAN: Mr. Mulloy, when you use a math model, do you have any idea of how accurate it is?

MR. MULLOY: We did not just use the math model. What we did was build a math model that was correlated to test. There was a test fixture that was built to empirically determine the maximum erosion that could occur while filling the annulus between the putty and the primary O-ring and the annulus between the primary and the secondary O-ring.

Then Thiokol's, Dr. Salita's math model was shown to correlate very well with that, and I guess I can't put a percentage accuracy on that. But the fact that the math model correlated pretty well with the test results gave us some confidence in that and the fact that the test demonstrated that there was a significant margin that was tolerable in terms of the amount of erosion, given the dimensional tolerance.

DR. FEYNMAN: I think that the math model determined how the constants were determined and a line was put through the previous data on a somewhat similar

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material. And the line that was put through deviated. It doesn't always give the same answer.

You took an average rather than the maximum, so that there were factors of 2 above and factors of 2 below on the original data. If you would have known that, you could have appreciated that wha this thing predicted could easily be a factor of 2 below the right answer, because in fact it didn't even fit with the data on which it was constructed.

You weren't aware of that?

MR. MULLOY: No, sir. I was not aware of that.

DR. KEEL: Well, Mr. Mulloy, what was your previous conservative estimate of maximum erosion on the primary O-ring for a nozzle joint? Do you remember that? You briefed it at several Flight Readiness Reviews.

(Pause.)

DR. KEEL: Going back to the record, it is 90/1000, based upon what you've characterized as a conservative estimate, by which you presumably meant worst case. Is that correct?

MR. MULLOY: That is correct.

DR. KEEL: And what erosion was there on 51-B that caused the launch constraints?

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MR. MULLOY: On the primary O-ring erosion it was 171/1000.

DR. KEEL: So 171 compared to the previous worst case analysis prediction of 90.

MR. MULLOY: Yes, sir. And what was different—that again is why we were concerned about this and established the understanding of this as a launch constraint.

What we observed was we were seeing a different type of erosion on this primary O-ring. And that different type—what we had been seeing previously was——

DR. KEEL: By different type, you mean worse erosion than predicted?

MR. MULLOY: No, sir. I mean a different type. The erosion that we had been seeing was due to the hot gas direct jet impingement on the surface of the primary ring as it seated.

What we saw here, it was evident that the primary ring never sealed at all, and we saw erosion all the way around that O-ring, and that is where the 171/1000 came from, and that was not in the model that predicted a maximum of 90/1000. The maximum of 90/1000 is the maximum erosion that can occur if the primary O-ring seals.

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But in this case, the primary O-ring did not seal; therefore, you had another volume to fill, and the flow was longer and it was blow-by and you got more erosion.

DR. KEEL: But I think that the bottom line of all that is that perhaps your analysis didn't consider all the physical phenomena, including how that primary O-ring seals and how long it takes to seal, and if it doesn't seal, what erosion should take place then.

MR. MULLOY: It did consider how long it takes to seal and how long it takes to fill that volume. What it didn't consider was that the primary O-ring did not seal and you had hot gas impingement for the additional period of time that it takes to fill the volume during the primary and secondary. It did not consider that.

DR. KEEL: And as a consequence, it was off by a factor of almost 2.

MR. MULLOY: No, sir. It was correct for the mode that it was analyzed for. It was not correct for an O-ring that was not sealing. It is absolutely correct and has been demonstrated to be correct for a primary O-ring that seals.

DR. KEEL: It's hard, I think, certainly for me and I think the rest of the Commission, to understand how you can say it's absolutely correct. I don't know

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of any models you've done since then or tests you've done since then that can correlate any more accurately than Dr. Feynman has indicated with your erosion models, an analytic empirical model. And it has to have uncertainty about it.

MR. MULLOY: Absolutely. That is a very unfortunate adjective. I withdraw that. Nothing is absolute.

DR. KEEL: I would have thought when you had this experience that you wouldn't have immediately gone back to your analytic methods as the basis of your confidence for lifting these launch constraints because certainly this case of erosion was certainly worse than what you would hypothesize from the previous worst case.

MR. MULLOY: Yes, sir. And that is why we ran additional tests and expanded the analytical model to account for this mode where the primary O-ring did not seal.

DR. KEEL: And you expanded the analytical model and what was the worst case erosion that that model then predicted?

MR. MULLOY: I believe I said 75 mils on the secondary O-ring if you had a primary O-ring that was violated and never sealed. And that is what the problem assessment system report shows on page 2 of 3.

DR. RIDE: Did you consider that acceptable?

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MR. MULLOY: Yes.

DR. RIDE: 75 mil erosion of the secondary O-ring.

MR. MULLOY: Yes.

DR. COVERT: Mr. Mulloy, at the time this joint was conceived of, did you envision that the O-rings would be eroded to this extent?

MR. MULLOY: I'm sorry, Dr. Covert?

DR. COVERT: At the time that the joint was designed, was it designed with the intent in mind that the O-rings would be eroded to this extent?

MR. MULLOY: No.

DR. COVERT: So in some way, then, the acceptance of this erosion as a fact of life represented a departure from margins of safety that you originally had in mind at the time you were designing it?

MR. MULLOY: Yes, sir. It was treated as an anomaly.

CHAIRMAN ROGERS: Going back just for a moment to the criticality I statement, one of the things that has troubled me from the start and still troubles me is, if I understand English at all, this says that the leakage of the primary O-ring seal is classified as a single failure point due to the possibility of loss of the sealing at the secondary O-ring, because of joint

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rotation after motor pressurization. [Ref. 5/2-6]

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: That to me says that if the primary O-ring fails, then there is apt to be a loss of mission, vehicle and crew. That is what it says.

Then you have experiences of one kind or another over a period of time with a seal which certainly causes a lot of discussion and a lot of concern.

At one point in the same document it says, based on the amount of charring and erosion paths through the primary O-ring, and what is understood about the erosion phenomenon, it is believed that the primary O-ring SMR-61A never sealed or never seated.

Now, why at that point, wouldn't you all have said we were lucky not to have a loss of mission and crew at that point because criticality I says if that primary O-ring fails, we will lose everything? Why wasn't that a cause for concern on the part of the whole NASA organization?

MR. MULLOY: It was cause for concern, sir.

CHAIRMAN ROGERS: Who did you tell about this?

MR. MULLOY: Everyone, sir.

CHAIRMAN ROGERS: They all knew about it?

MR. MULLOY: Yes, sir.

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CHAIRMAN ROGERS: And they all knew about it at the time of 51-L?

MR. MULLOY: Yes, sir. You will find in the Flight Readiness Review record that went all the way to the L minus one review.

CHAIRMAN ROGERS: That's why I'm saying we want to talk to you this morning. That suggests that was pretty well glossed over; that they didn't really realize it. But in any event——

DR. SUTTER: Well, could I ask a quick question? I'm confused about this thing being signed off and that was a mistake, apparently. But is it still on the books as being signed off? When was that mistake discovered?

MR. WEAR: It is in the books today, just like you see it here.

DR. SUTTER: So then a lot of——

MR. WEAR: We haven't gone back and fixed the books.

DR. SUTTER: A lot of people must read these constraints, and a lot of people could read it as saying, hey, that's signed off; don't worry about it any more. Who reads this? It's in the books.

MR. WEAR: Let me explain how this occurs. There is what is called a Problem Review Board Meeting.

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that is held within each project. There's one for each of the projects at Marshall as a result of Bob Lindstrom's letter that was read earlier that set this out, where they go over these items.

The reason you see Jim Thomas there, he's the alternate Chairman of that Board for me. That is discussed at that time and that is where these are recognized as being within our data base or perhaps not within our data base. And it is covered in the Flight Readiness Review and that is the process.

DR. SUTTER: So at the Flight Readiness Review, the people read this and understand it.

MR. WEAR: He or I in doing this activity know it's in the Flight Readiness Review or it's not, as the case may be. Plus the other participants there know that. And therefore it is listed as a result of being presented and discussed in the Flight Readiness Review.

And it would be within our data base or not.

CHAIRMAN ROGERS: What do you mean by data base, because Thiokol keeps saying that experience was not within your data base on Flight 51-L, and you all say that it was within your data base.

Which is right?

MR. WEAR: The Thiokol that I addressed, sir, says it is within their data base.

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DR. KEEL: I think the Chairman's point, though, is with respect to the night of the telecon, Thiokol was arguing just what you are arguing now with respect to erosion. They were arguing that we want to stay within our data base and we want to go with an O-ring temperature not any lower than 53 degrees, just like you're arguing now that it's okay to fly because we are within our data base, implying you wouldn't fly outside of your data base.

CHAIRMAN ROGERS: What do you mean by data base?

MR. WEAR: The data base to me, sir, is the previous test and flight experience that we have, as supported.

CHAIRMAN ROGERS: You've never had experience in this cold weather so when you're talking about your data base on Flight 51-L you didn't have any. It seems to me it is used as sort of a slick way of just getting over the problem, saying it's within our data base. But you hadn't had any experience of that kind before and so you can't say it was within your data base.

The engineers at Thiokol were saying it is not. We have never had that experience. We warn you: Don't do it. We don't know what's going to happen.

And we keep hearing from NASA, it was within

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our data base.

MR. WEAR: Well, going to that particular evening, which I think what you are referring to is the January 27th evening, on that particular evening in my process, in my mind process, we had faced a cold launch the year before, in which we had had some erosion.

CHAIRMAN ROGERS: The worst experience you had.

MR. WEAR: Which they had addressed and they, the Thiokol that you're referring to, they had said that that condition, that cold condition on that particular occasion the year before had—I forget the exact words, but in effect had aggravated the situation, but that it was acceptable and would perform. That was the conclusion.

CHAIRMAN ROGERS: But you agree, though, it was not within your data base, don't you?

MR. WEAR: That experience of the prior year was within my data base; yes, sir.

CHAIRMAN ROGERS: Could I say on that one, though, you had your worst result on that one.

MR. WEAR: Yes.

CHAIRMAN ROGERS: So you can say well, we almost had an accident, but we didn't quite. Therefore, it is within our data base. And then you get to another day when it is colder and you still argue it is within

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your data base.

Thiokol said it is not in your data base because you had never tried it in this cold weather.

Now, what I'm asking is how do you explain that controversy? How do you explain that conflict?

MR. WEAR: If I may continue, their engineering organization and my engineering organization had agreed the prior year that that experience was—could be acceptable on the next launch; that if the same condition occurred, that that was the findings that were made at that time, if that same condition existed it could be accepted.

And that is the same engineering organization that was talking on January 27th, so their conclusions and report to us on the previous year was that at those conditions and what we observed on that particular launch, that if that condition—meaning that type of—that entire condition, that weather condition, whatever—occurred again, that that was acceptable.

MR. HOTZ: But you didn't h ve those conditions.

CHAIRMAN ROGERS: Let me finish. That was within your data base, you argue. But what I'm saying, you've got a new condition now which was not within your data base.

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Now, how do you keep relying on it was within your data base? The Thiokol people said it's not. The engineers said it's not. We've never tried it at this cold a temperature.

How can you keep saying that it was within your data base? It wasn't. It exceeded your data base. You never had that kind of experience before.

MR. WEAR: Well, on that particular evening I heard what the Thiokol engineering people stated. I also know that they then counciled and they came back and they stated that the conditions could be accepted. And so I have to conclude some engineering people must have changed their minds.

CHAIRMAN ROGERS: I'm not taking issue with that. I'm taking issue with the slickness of the words "within our data base," as if that excuses everything.

What I'm pointing out is I don't think you ever had a data base of this kind in Flight 51-L.

MR. WEAR: That's true.

CHAIRMAN ROGERS: Well, that's really all I was pointing out.

DR. FEYNMAN: Can I ask something, too? I would like to understand this idea of within the data base. For instance, 51-C had many seals on which there was no erosion at all. Which seals should we take?

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Why is it—suppose we had an accident? You see, we have an accident on five of the six seals. And maybe it wasn't five, but anyway, most of them—there's no erosion, statistically.

Accidentally, it could happen that there was no erosion on any of the seals, only a little bit, and then the next flight a lot of erosion. In other words, I don't understand how the logic works, that because something just made it, that the next time it wouldn't be a bigger variation.

Could you explain to me, therefore, why when you have a successful flight which is successful in the sense that the entire flight takes place, but which is unsuccessful in the sense that you get effects that you didn't expect, that you consider that the next time it isn't going to be accidentally a little bigger?

MR. MULLOY: Yes, sir. That is what we addressed with trying to determine when we would see a 10/1000 erosion or 20/1000 erosion or a 30/1000 erosion. That is why the test setup was made, to determine how much erosion can you physically get.

And that is where the 90 mil calculation came from, because that erosion is limited by the amount of time that hot gas can impinge on the primary O-ring, on the assumption that the primary O-ring seals.

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So the logic was that there was a large margin against what were actually observing versus what the theoretically possible erosion was, and then there was a large margin between the theoretically possible erosion and that which you could sustain as demonstrated by tests.

Now, that was the logic.

DR. FEYNMAN: How many tests were made that would show that you really could sustain 90 mils of erosion?

MR. MULLOY: I can't recall. There were several hot gas tests made, maybe ten, with 125/1000 erosion on the primary O-ring seal.

CHAIRMAN ROGERS: How did the results of those tests compare with the results of the tests that have been done lately?

MR. MULLOY: They are correlatable in terms of the erosion. There are no differences. I don't believe. I haven't seen all of the test data.

DR. FEYNMAN: This isn't quite right, sir. You said there were tests with 125 mils erosion.

MR. MULLOY: Yes, sir.

DR. FEYNMAN: That isn't quite the way I remember it. I think the tests were made by cutting the ring away.

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MR. MULLOY: There were two tests run. One of them was a hot gas impingement that sustained up to 125 and still sealed.

DR. FEYNMAN: How did you make the test with 125?

MR. MULLOY: By allowing the hot gas to impinge for a longer period of time and having it impinge longer. And then there were cold gas tests made where the O-ring was—simulated erosion was put in there, and the maximum demonstrated there I think was 95/1000 as I recall.

But that was the logic. And I guess anyone can question that logic.

DR. SUTTER: These were all the five-inch?

MR. MULLOY: Yes, sir; with a full-scale gland and O-ring, but a short length. Well, a 10-inch diameter, I guess.

MR. HOTZ: Mr. Mulloy, I would like to try to understand this in somewhat simpler terms than you people are used to using.

Is it correct to state that when you originally designed this joint and looked at it, that you did not anticipate erosion of any of the O-rings during flight?

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MR. MULLOY: That is my understanding. I entered this program in November of 1982 and I wasn't there on the original design of the joint, but when I took over the program there was no O-ring erosion anticipated.

MR. HOTZ: So that when you did run into signs of O-ring erosion, this was a bad sign.

MR. MULLOY: Yes, sir.

MR. HOTZ: You didn't like it?

MR. MULLOY: No, sir.

MR. HOTZ: So then you decided to introduce a standard based on the measurement or the possibility of the limits of O-ring erosion. And as those limits, as the experience went up, your criteria for safe flight went up too.

In other words, when you experienced more than maximum anticipated O-ring erosion, you waived the flight and said well, it's possible to tolerate that. We still have a margin left.

MR. MULLOY: Are you speaking of the case where we did not have a primary seal?

MR. HOTZ: Yes.

MR. MULLOY: Yes, sir. That is correct. There was another piece of logic that went into that though; that that flight, the next flight, we were

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positive we did have a good primary seal because we increased the leak check pressure to 200 psi.

MR. HOTZ: But getting back to that, you mentioned that the ability of the putty to sustain pressure was pretty unpredictable.

MR. MULLOY: Yes, sir.

MR. HOTZ: And wasn't that a cause for concern that you had an unpredictable element in your equation?

MR. MULLOY: Yes, sir. And what was done there was to look for some alternative to the putty, such as MBR rubber strips or carbon fiber or carbon wool or steel wool or something that would allow pressure to go directly to the primary seal.

MR. HOTZ: But to continue flying in the meantime?

MR. MULLOY: Yes, sir.

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MR. HOTZ: Then you finally, you're talking about these margins of safety, and I wonder if you could express in either percentages or actual measurement terms—you have used the term "wide margin". I wonder if you could give us a quantitative measurement as to what you consider a wide margin?

MR. MULLOY: Yes, sir. Well, as I said we had demonstrated that we could stand 125 thousandths of erosion and still seat. The maximum erosion that we had seen in the case joint was on SDS-2, which was 53 thousandths, so that is a factor of two and a half.

MR. HOTZ: But it is still based on a very narrow physical measurement between 125 thousandths.

DR. KEEL: Could I clarify something here, Bob? I think, Larry, if you go back and look at your Flight Readiness Reviews, that you were relying on smaller margins than that.

You were arguing in the Flight Readiness Reviews where you briefed the problems of primary O-ring erosion that for the worst case for the field joint also that it would be 90 thousandths.

MR. MULLOY: That is correct.

DR. KEEL: At that point you were pointing out that's okay, because you can seal at 95, not at 125 but at 95. It wasn't until later on during the process that

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you determined you could seal at 125.

MR. MULLOY: That is when we got the hot gas test data.

DR. KEEL: So that's a five percent margin, roughly, five and a half.

MR. MULLOY: On the 90 to 95 on a max predictable, yes.

MR. HOTZ: Just one more question, sir. With all of this experience base, wasn't there any time in this history of the flight that you or anybody else connected with the solid rocket booster said we are getting a lot of anomalies here. We are getting things that are outside our original predictions, and shouldn't we take a look at it and stop flying until we've fixed it or have a better feel for what is actually happening in the joint?

MR. MULLOY: Only the first part of that, that we are seeing something here that needs to be corrected. We continually emphasized to the contractor that we need to put more emphasis on resolving this problem. We did not recommend that we stop flying for the logic that was presented in the Flight Readiness Reviews and which I reiterated here today.

MR. HOTZ: Thank you.

CHAIRMAN ROGERS: Was consideration

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given, as far as you know, to stop flying and fix the joint? Was there any discussions about that? Maybe we should stop flying for a while?

MR. MULLOY: Not to me, no sir. I have become aware of that in the course of some of the testimony since the 51-L accident that there are some memos that are internal memos to that effect.

MR. SUTTER: I still am confused with the answers I got to the question about the statements as the problem was closed. The question that's going on right now is how deep was the concern over the joint.

The statement here that says this problem is closed is against the whole NASA philosophy of really documenting, controlling and having checks and balances. When I read this statement that it was a mistake but it's left on the book, that gives me the impression that NASA did not believe this was a very big problem. [Ref. 5/2-3]

DR. RIDE: How serious do you consider a launch constraint?

MR. SUTTER: Do you have any comments on that?

MR. MULLOY: The comment I have, it's very unfortunate that that was erroneously entered. I had no intention of closing that problem, because I considered this to be a very serious problem.

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MR. SUTTER: But did you know this?

MR. MULLOY: No, sir.

VICE CHAIRMAN ARMSTRONG: Am I right that you didn't close it because you didn't want to change anything after the accident? Isn't that right?

MR. SUTTER: But when was this signed off? This was signed off on 12/18/85.

MR. WEAR: This entry was made by this PAS system organization based upon the entry that they got from the contractor where he recommended its closure and they did it.

MR. SUTTER: I'm really confused, because the contractor was the guy that said don't launch, yet he wrote you a piece of paper saying close this item out? If you're going to depend on the paperwork as to controlling the operation—and I've heard this now for three months, all of these documents that, my God, we really controlled and we followed this to the point where we can't get into trouble, yet here's a piece of paper that says there's no problem.

How the—nobody reads this, I guess.

MR. WEAR: No, I think it is read.

MR. SUTTER: Do we have a copy of the letter?

MR. WEAR: Yes, sir, it's in the book here.

DR. KEEL: Mr. Chairman, we're going to ask the contractors exactly that question.

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CHAIRMAN ROGERS: But I think we have to follow up on Joe's point. You mean just because somebody wrote a letter, the whole thing was closed out and you didn't know? Nobody knew anything about it? After all of this history of it, one letter would close it out?

MR MULLOY: That was a failure of the human being within the system.

CHAIRMAN ROGERS: It was a little more than that. It's a failure of the whole system if one letter and one human being can close out a constraint that has been concerning you for many years.

DR. KEEL: Can I ask for one clarification before you go away from this, Mr. Wear? You're listed here as the project representative under the approval line?

MR. WEAR: That's right.

DR. KEEL: Does that mean you did approve this, or you didn't approve it?

MR. WEAR: No. I did not approve this. It was not brought forward for us to consider.

DR. KEEL: So even though your name is on here as approval, you didn't approve it?

MR. WEAR: That's right. I think if you will notice, there is not an entry here that says so. On the others, I think you will notice over here that there is

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an entry of initial, either JWT or LOW on the others as having lifted that for that particular flight. I think most of them them put an L after it or something that refers to it's lifted. In their nomenclature there's an L entered for that purpose.

DR. KEEL: So as far as you're concerned you were still operating as if this was—remained a launch constraint?

MR. MULLOY: Yes, sir.

MR. WEAR: Yes, sir.

DR. KEEL: But may I follow up? That is correct, but as a matter of practicality even though this was a launch constraint it was being waived for each launch?

MR. MULLOY: That is correct, on the basis of the presentation at the Flight Readiness Review.

DR. KEEL: So you in effect waived this for 51-L?

MR. MULLOY: That is correct.

DR. KEEL: But it doesn't show up on your summary?

MR. MULLOY: No because the man assumed when the closure came in from Thiokol that this was going to close the problem, and that requires project concurrence, Mr. Wear's concurrence, which Mr. Wear and

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I had discussed it and that was not going to happen.

DR. KEEL: As far as you were concerned, though, you still considered it a constraint in spite of this document?

MR. MULLOY: Yes, sir.

DR. KEEL: So the night of the telecon, for example, when you were arguing about whether the primary O-ring would seal, you still considered the fact that there was a launch constraint on the primary O-ring for 51-L?

MR. MULLOY: Yes, sir.

DR. KEEL: Hence, in effect, the fact that in a strict sense there was no launch commit criteria with respect to temperature, there certainly was a launch constraint with respect to the primary O-ring sealing?

MR. MULLOY: Launch constraint relative to an understanding of our previous history to go into the next flight.

DR. KEEL: Well, it's primary O-ring, isn't that what the launch constraint is on?

MR. MULLOY: Yes, it is related to the primary O-ring on the field joint.

DR. KEEL: And the reason you could have so much erosion is because it didn't seal on 51-B?

MR. MULLOY: On the nozzle joint, that is correct.

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DR. KEEL: That's right, and they were arguing on the field joint. They had concern about it sealing or having time to seal.

MR. MULLOY: Yes, that was the presentation. The concern for the time that it would take for the primary O-ring to seal.

DR. KEEL: So in that sense, all that argument was in the context of there being a launch constraint.

MR. MULLOY: I didn't think of it in those terms at that time.

MR. ACHESON: But I take it it would have made no difference whether it was formally closed out or not because, as I see the way the system had worked, a flag raised on 51-L—I would have thought the determination of the problem was contained by the fact that the leak check had been changed to 200 psi from 100, and you could go on as before with that change in the procedure.

Is that not the way it would have been resolved?

MR. MULLOY: Yes, it was contained by the fact that with the 200 psi leak check we were sure that we had a good primary O-ring.

MR. ACHESON: So it really didn't make any difference whether it was formally closed or not as to what would have happened.

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MR. MULLOY: I would like to go back to the point Mr. Wear made, because I think it is important in addressing your concern, Mr. Sutter, and it's a very important point that Mr. Wear made, is that you do not see the accepted closure on that last entry. That is an entry that the guy who makes entries into the PAS system made, and you do not see project signature concurring in that.

So in essence, it is still open. It is open until Larry Wear concurs that it's closed.

MR. SUTTER: Do the people who read these look for all the signatures?

MR. MULLOY: No, sir. I agree, it is unfortunate that that error was made by the gentleman who make the entries into the PAS system on the basis of a submittal from the contractor that this was no longer a problem of significance to carry in the problem assessment system.

MR. KEHRLI: Mr. Chairman, there are some documents, four documents, that we haven't looked at that address these specific questions that have been asked for the last three or four minutes that I would like to address the witness' attention to and the Commission's on the chronology of this closure.

The first one is the launch constraint

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document number 4, which is a letter dated December 10, 1985 to Mr. Wear from Mr. McDonald of Thiokol, which references the particular problem that we've been discussing and suggests that or requests that the subject critical problem be closed. [Ref. 5/2-4]

Additionally, the next document that follows that page is the letter from Thiokol to Mr. Jack Fletcher of Rockwell, and it indicates that there is further information that's being provided with regard to the closure of critical problems. [Ref. 5/2-7]

Back on O-ring history document number 34, there is a letter from Mr. Wear to Morton Thiokol; subject, SRM Problem Review Board. The date of that letter is December 24, 1985.

It reads: "During a recent review of the SRM Problem Review Board open problem list, I found that we had 20 open problems, 11 opened during the past six months, 13 opened over six months; one three years old, two two years old, and one closed during the past six months. As you can see, our closure record is very poor. You are requested to initiate the required effort to assure more timely closures and the MTI personnel shall directly coordinate with the S&E personnel the contents of the closure reports." The letter is signed by Mr. Wear. [Ref. 5/2-8]

The final document is attachment number 30 to

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the O-ring history. This is, again, part of the Marshall package that was received when the launch constraint document that we've been talking about, the problem assessment documents were received. (Ref. 3/2 ...)

MR. WEAR: What's the number, please?

MR. KEHRLI: Number 30 in the O-ring history. It gives a chronology. As you see, it notes on the entry of 51-B there is an asterisk next to the Marshall tracking number A09288 which indicates that "this problem contains secondary O-ring erosion of the nozzle joint and constrains launch anomalies."

Then down at the bottom, on STS-61-C, by the date, 1/12/86, there is an indication of erosion or an O-ring anomaly problem on the field and nozzle-to-case which was not reported and not given a problem tracking number in January of 1986.

CHAIRMAN ROGERS: What do you conclude by that?

MR. KEHRLI: Well, one, I am wondering if the closure of the item is the reason that the January 12, 1986 erosion or O-ring anomaly was not reported or given a tracking number.

MR. WEAR: I'm afraid I didn't follow your question there.

MR. KEHRLI: Well, on document number 30, the field and nozzle joint erosion problem, on STS-61-C on the

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primary ring, unlike the other problems—except for STS-2—it has an indication that it was not reported.

Is that because the problem had been closed as a result of those previous letters we just referred to, the Thiokol request to close it and your letter indicating that you want open items closed?

MR. WEAR: I'm afraid I can't tell you why this one was not reported, looking at this right here, now.

As far as my letter is concerned, the thrust of my letter was we had problems. They are within this tracking system. It's more than the normal deviation, the deviation records. What the thrust of my letter is is that they are not making adequate progress to satisfy me in resolving problems. We were continuing to have the problem but we weren't making sufficient progress to suit me in resolving them, that they were hanging on for too long and we weren't closing them out.

MR. MULLOY: Let me take a try at answering this question as to why it was not reported. I see this was revised 3/19/86. This would be speculation, but I didn't get the information on the inspection of 61-C until after the Flight Readiness Review for 51-L. That came in—I believe we got those joints demated just two or three days before the L-1, and I reported this in the L-1 review.

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Given that timing and given the events of 51-L, I just—and perhaps you could address this question to Thiokol, but I suspect they haven't written up this four mils of erosion that occurred on 61-C and put it into the system since the 51-L accident.

There is no reason why it wouldn't be reported because there was four mils of erosion.

DR. KEEL: Can I ask one followup question with respect to these memos and the documents that Randy has referred to here? Based on our staff interviews with Mr. Thomas from Marshall, he indicated that he had told Thiokol on the telecon to in fact close out all issues that were over six months old, and Mr. Russell at Thiokol confirmed that.

Was that your understanding, also, that engineering at Marshall was saying close out the problems over six months old?

MR. WEAR: I'm afraid I don't know the context of that discussion. He and I had had conversations.

DR. KEEL: Wel , it's the same context here, I assume

MR. WEAR: Well, when you say close them out, I say I'm not sure what the context was of the statement he made. My intent--and I trust it is the same with Jim's because he and I usually communicate well--my

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intent was let's get off our duff and work and reach solutions for these problems, not continue to drag them out for six months, five months, et cetera. That is the thrust of my direction to Thiokol

DR. KEEL: Did ultimately Thiokol recommend, in fact, closing out by taking the joint, SRB joint problem off the problem report list?

MR. WEAR: That's the nature of McDonald's letter back here where he does recommend taking them off, and as I understand the thr ist of the letter the way I read it now and the way I read it then was that we are discussing these problems in the Flight Readiness Review. Let's not also put them someplace else, so that in effect he has to report them two places.

As I read his letter, he's saying we are doing the trac ing job in the Flight Readiness cycle. Let's take them out of this other tracking system. He's sa ng let's do it once. I think that's the thrust of his letter, the way I read it.

DR. KEEL: What was Marshall's response to that?

MR. WEAR: My response to that was no

MR. MULLOY: Mine, too.

MR. WEAR: We will keep it in the system because it a formal check and balance. That is what its

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intention was, to be sure if something didn't slip through a crack someplace. That's the way I've always looked at it, was to create a double check by the quality organization to see to it that something wasn't overlooked by the project. Perhaps the project might not even be aware that it could be overlooked, that it had to be faced.

DR. FEYNMAN: Is this answer of yours to McDonald in some document?

MR. WEAR: Pardon?

DR. FEYNMAN: How did you answer Mr. McDonald, in a document?

MR. WEAR: I never have.

DR. FEYNMAN: The answer you've just given us as to how you ans ered Mr. McDonald, in what document is that where you say you won't close it out?

MR. WEAR: I say I never have answered his memorandum; therefore, the system stands, the Marshall system stands.

DR. FEYNMAN: Except for the mistake.

MR. WEAR: Yes, sir. It makes mistakes, but it stands. Frankly, it wouldn't be within my power to accept his recommendation and take this out anyway, because the establishment of the system is well above me, and so it is not my prerogative.

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DR. FEYNMAN: Nobody answered this letter of Mr. McDonald's?

MR. WEAR: No.

DR. RIDE: How many of the problems in your tracking system carry launch constraints? How many launch constraints have you been waiving flight to flight?

MR. WEAR: I haven't researched it specifically, but I think you would find every one of them having to do with SRM or related to the joint putty, the joint and or the putty type of affairs that we've discussed here. I don't recall anything else that would be there as a launch constraint.

DR. RIDE: Say on flight 61-C, how many launch constraints did you waive?

MR. WEAR: Offhand I can't tell you.

DR. RIDE: I mean, it must be, and I mean I hope the answer is one. If the answer is more than one, then there is more that we don't know.

MR. WEAR: Well, you've got the record here, and I would just stand on that. I mean, you've got the records from the past system.

CHAIRMAN ROGERS: Well, let's talk about it while you're here.

DR. RIDE: What we've got, I think, is the record of the launch constraints as a result of the

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erosion problems on the joint.

MR. KEHRLI: That's correct.

DR. RIDE: I guess my question is are there other problems in the SRM or SRB system that also carry launch constraints that you have been waiving at your FRRs.

Maybe I'm wrong, but I consider a launch constraint to be a big deal that you have to address in a significant, substantial way before every launch, and I would think that if there is more than one you would know that there is more than one.

MR. WEAR: Well, you asked me how many, and that infers that I can pull off the top of my head exactly how many there are, and I can't right here, but we are required –

CHAIRMAN ROGERS: Would you be willing to say it's very few?

MR. WEAR: Yes, sir. Absolutely.

CHAIRMAN ROGERS: Would you be willing to say they all relate to the SRB? All the waivers would relate to the solid rocket booster?

MR. WEAR: Do you mean that are going through the whole FRR process, is that what you're saying?

CHAIRMAN ROGERS: Yes.

MR. WEAR: No, because I'm only exposed to the

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SRM, per se, so I couldn't answer that.

DR. RIDE: Was there more than this one?

MR. WEAR: In each Flight Readiness Review we addressed problems that occurred on a past flight and/or significant waivers or deviations that occur during the manufacturing process, the manufacturing acceptance process of the hardware. Those are categorized as being within previous data experience or not.

DR. RIDE: I'm trying to understand how you deal with the launch constraint. How important do you think a launch constraint is and how unusual it is in your system?

MR. WEAR: I think a launch constraint is a significant event in our system, and it is one that has to be addressed within the Flight Readiness cycle because I don't have the authority to not do that.

CHAIRMAN ROGERS: The question is how many have you waived?

MR. WEAR: Well, of course each of these joint and putty entries that you've seen, those are there.

CHAIRMAN ROGERS: Can you think of any others?

MR. WEAR: Yes, sir. I believe I've got one. I think if you check the record I believe it would be categorized as one. On a nozzle, where we had de-bond on a nozzle, I believe it is somewhere in the system and

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we had to process it.

CHAIRMAN ROGERS: Is that the only other one you can think of?

MR. WEAR: Well, I know in the history of the program we have addressed several.

CHAIRMAN ROGERS: We're not talking about addressing. We're talking about waivers.

MR. WEAR: There has been a lot of discussion in the Flight Readiness Review process regarding the nozzle itself. I think some of you are aware that we struggled with a severe problem on the nozzle erosion.

CHAIRMAN ROGERS: I'm talking about how many have you waived?

MR. WEAR: Specifically I can't answer that.

CHAIRMAN ROGERS: So probably most of them were in connection with this joint.

MR. WEAR: I think the preponderance of them have to do with this joint and/or the nozzle-to-case joint, and I think it is the preponderance of everything that has been waived is there.

The reason I'm struggling here, I think if I went back and went through the whole record there would be some related to the nozzle because we did have severe nozzle erosion problems about two and a half years ago. We struggled with that problem for a while, and I suspect I would find some there.

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VICE CHAIRMAN ARMSTRONG: Is it an easier question if you ask how many waivers were there on 51-L, or is that still a hard question?

MR. WEAR: I just don't recall it, and I don't recall that there were any on 51-L that we haven't discussed.

DR. FEYNMAN: In your letter of the 24th you said there were 20 open problems. These open problems are not all launch constraints?

MR. WEAR: That's right, they are not all launch constraints because the PAS system is concerned with problems other than Crit 1, Crit 1R problems. It is an attempt to keep the record clean for lesser problems as well.

DR. RIDE: That's exactly the point, because you've got the system that records open problems, and you have to have some way of distinguishing unimportant problems from important problems from very important problems, and it seems to me the one that says launch constraint next to it must be the very most important problem.

MR. WEAR: That's right, and that is why it has to be cleared by this PAS system before we can proceed.

DR. RIDE: What I'm trying to understand is

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how many problems are in the launch constraint category in your system?

MR. WEAR: I can't give you a precise answer.

CHAIRMAN ROGERS: A followup question which really does surprise me is that I would think that if you have waivers and there weren't many, you would remember them all.

MR. WEAR: When you asked me how many, I would have to go back over history, and historically the only other problem of consequence that I recall we have wrestled with on the SRM has either been related to nozzle erosion and/or these joints.

DR. WALKER: Have you ever refused to waive a launch constraint because you thought the problem was so serious?

MR. WEAR: No.

DR. WALKER: What would happen if you didn't waive a launch constraint? Let's say you or Mr. Mulloy had looked at this O-ring situation and said, well, I don't think we can waive this because it is too serious. What would happen then?

MR. WEAR: Well, I think immediately there have been some times when we have looked at a problem and either I or someone else associated with my review have not been satisfied with the data that was presented.

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On those specific occasions there was some additional work that was done, some additional data was provided, whatever, and then either I or that person was reconciled. But if I had said I refuse to accept this, I refuse to go forward, I refuse to accept this problem and fly with it, that would have gone to Larry and to Larry's board, and it would have had to have been reviewed with them.

DR. WALKER: Let's suppose it now goes to you, Larry, and you're not going to waive it.

MR. MULLOY: The effect of not waiving it the first time, it is presented as a launch delay, and we have had several of them. One of those was after STS-8 nozzle erosion. There were some people who felt that we could proceed with the flight of the next flight, which was STS-9, with the nozzles that we had on there. I did not accept that.

The consequence was we rolled STS-9 back and demated the aft segment and put in an aft segment with a nozzle that had, after understanding, spending some time to understand what the cause of that excess erosion was on STS-8, we changed out a segment.

So the effect of not waiving a constraint when it has first been presented has been launch delay. Up to this point, we have taken the time necessary to do

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the analysis and tests to provide sufficient rationale to proceed with the next flight in the face of the open problem.

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MR. HOTZ: Then you have refused to do it? On STS-8, you did it?

MR. MULLOY: On STS-9. On STS-9, after the problem was open on the nozzle, until we understood what the probable cause was, and as I say, the effect was a rollback from the pad and about a two-month delay in the launch of that vehicle.

DR. WALKER: So the nozzle was changed out?

MR. MULLOY: Yes, and that is what we would do in any case when there is insufficient rationale to proceed on the schedule that is proposed.

MR. HOTZ: Well, I am confused, Mr. Wear. You said you have never refused to waive a launch constraint, and I understand now that there was one instance when you did. Which is correct?

MR. MULLOY: I said both, sir?

MR. HOTZ: No, Mr. Wear said you never refused.

MR. WEAR: My point is, either the problem was resolved before it went forward, or else we wouldn't have launched, but it would be resolved before we would go forward. That is a case where we hit a problem, and I don't know exactly.

MR. HOTZ: But you did not waive it to go on with STS-9?

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MR. MULLOY: As it was configured when it was on the pad. That is correct. And in fact I think that was a two-month launch delay, and that is what we would do in any instance when there has been insufficient time from the time that the problem was observed to do the test and analysis to understand whether you can proceed with the next launch, and as I say, the effect to date has been launch delays, but then ultimately that sufficient data is available to proceed, and then the waiver is before you can fly it. The waiver in the case for STS-9 was that the cause of the excessive erosion on STS-8 had been determined, and that condition did not exist on the nozzle in STS-9, and the rationale as to why the condition did not exist after we changed out the nozzle, and the material change.

CHAIRMAN ROGERS: Do you have ultimate responsibility for waiving the launch constraints?

MR. MULLOY: Yes, sir, I have ultimate responsibility for the launch readiness of the solid rocket boosters.

CHAIRMAN ROGERS: So there was a launch constraint, and you waived it.

MR. MULLOY: Yes, sir, all flights subsequent to.

CHAIRMAN ROGERS: All those you waived on your

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own? Who did you consult with in connection with those waivers?

MR. MULLOY: Well, in terms of consulting on the waiver, the waiver is approved by my element managers, and the system that comes to me, the quality people who run the problem assessment system, when they come to my board, will say, you have an open launch constraint that has not been properly closed out. The rationale for closing out that launch constraint is presented by Mr. Wear to me in my flight readiness review, which is that rationale that exists throughout this document that says why it is okay, given this observation to proceed with the flight.

CHAIRMAN ROGERS: Well, I have trouble following this. Could you do it a little slower? There is a launch constraint put on by somebody, some decision.

MR. MULLOY: By me in this case.

CHAIRMAN ROGERS: By you. Okay. Now, who has the authority to waive it?

MR. MULLOY: I do.

CHAIRMAN ROGERS: Okay, you put it on and you take it off.

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: Who do you consult with when

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you take it off?

MR. MULLOY: Mr. Wear, in this case, who brings me the rationale as to why we can proceed for the next flight in light of the observation that we have seen.

CHAIRMAN ROGERS: So in all of these flights, Mr. Wear, you were involved in waiving it, too?

MR. WEAR: Sure.

CHAIRMAN ROGERS: Now, did you know anything about the telecon before Flight 51L? Were you involved in that?

MR. WEAR: The one on January 27th?

CHAIRMAN ROGERS: Yes.

MR. WEAR: I was there on January 27th. Yes, sir.

CHAIRMAN ROGERS: And did you point out to everybody there was a launch constraint, and therefore it was a little different than the usual launch? When the Thiokol people said we ought to, because of the weather, we ought to delay this, did that cause you any concern?

MR. WEAR: Did the existence of a launch constraint——

CHAIRMAN ROGERS: The combination of launch constraint and a warning by the manufacturer not to do

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it, did that cause you any concern?

MR. WEAR: Let me make sure I answer that in the right context. The joint problem and its understanding had been a matter of concern for some time, and there was no one on that conversation that night to my knowledge—well, there was no one in that conversation that night that was not well aware of the problem and its seriousness.

CHAIRMAN ROGERS: Put aside the word "problem." Was everybody aware of the fact that there was a launch constraint?

MR. WEAR: There was a launch constraint to address the erosion. The problems on prior flights, yes.

CHAIRMAN ROGERS: Now, following along Dr. Ride's comment, we assume or at least I assume that that means there was something a little different or special, more serious, the fact that you had a launch constraint, this was a more serious problem than just an ordinary problem. She pointed out there must be gradations of problems, and this, when you have a launch constraint and it is in a Criticality 1 item, that is pretty serious.

Now, when the manufacturer then said we recommend don't launch to begin with did that cause you

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any concern particularly in view of the fact that there was a launch constraint on it?

MR. WEAR: Yes. When the Thiokol engineering people expressed their concern, yes. That caused me some concern, yes.

CHAIRMAN ROGERS: And they suggested a slight delay until the weather was better.

MR. WEAR: That is what the Thiokol engineering people stated that night.

CHAIRMAN ROGERS: And you were willing to go ahead even in the face of that recommendation, even though the weather was not good, and even though you had no, what you have said, data, no data base that would say it was safe to do it under those conditions, you still were ready to go ahead and launch?

MR. WEAR: Yes. Now, let me explain one thing, though. In my dealings with Thiokol, I deal principally with Mr. Kilminster. There are other occasions, and beyond this one there are numerous occasions where the Thiokol engineering people and/or my engineering people have discussions and so forth. He occasionally has to deal with his people and understand the problem, and they give me recommendations. I mean, they are not always one for one. He is not just a mimic from them, and therefore I have to depend upon him to

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present what Thiokol concludes.

DR. WALKER: But you also have to depend upon your own judgment, don't you?

MR. WEAR: Yes, I do.

DR. WALKER: It is not a situation where you are dealing with something you procured and you don't understand it and you have to accept it.

MR. WEAR: Ultimately you have to go with your own judgment.

DR. WALKER: So you concurred in that judgment?

MR. WEAR: Yes, sir.

DR. RIDE: You said that the thing that carries a launch constraint has to be presented at your FRR, and then you have the opportunity to waive it. You then take information from your FRR and make presentations to the Marshall center FRR and also to the Level 2 FRR, and I assume also to Level 1 FRR.

MR. MULLOY: Yes.

DR. RIDE: Did you ever mention in any of those cases that there was a launch constraint on the SRB nozzle joint that you had waived?

MR. MULLOY: No. What we did was, I am sure that we did address this recurring concern. We didn't address it in highlighted launch constraint, waive

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launch constraint.

CHAIRMAN ROGERS: Did you ever say it in those words, though?

MR. MULLOY: No, sir.

CHAIRMAN ROGERS: If you read the documents, it seems to me everything was almost covered up, ever so slightly noted, and it seemed to be such a serious problem, and the papers reflect that a lot of you thought it was serious, and yet it doesn't seem that serious when you read the documentation. In other words, others that we have questioned said they didn't realize that it was serious, and they apparently didn't realize it was a launch constraint.

Well, anyway, that is not really in the form of a question. I don't have any others. Are there any other questions?

DR. KEEL: I just have one other, if I can, Mr. Mulloy, just so we might understand it. There are two bases on which you waive the constraint at least immediately after 41B. One was the analysis, which we have already questioned. The other was, as you have mentioned, going to higher leak check pressure at 200 psi. But you must have been aware at the same time that there were people at Marshall and your engineers in particular who were concerned about the effects of the

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leak check pressure procedures on problems with the O-ring and on erosion.

Is that a correct statement? You were aware of that?

MR. MULLOY: No, sir, I was not then and I am not now. That 200 psi leak check was on the recommendation of our engineer.

DR. KEEL: Well, let me just read this memo then from Mr. Miller to Mr. Hardy and Mr. Coates that, Mr. Wear, you have got a copy of and, Mr. Mulloy, you have got a copy of. It is Tab 16. And it is "burned O-rings on STS-11." This was back in February, 1984, and the first sentence, "The recent experience of two burnt O-rings in STS-11 coupled with the missing putty finding at disassembly raises concerns with respect to STS-13." [Ref. 5/2 10]

If you drop down further in the paragraph the last sentence in that paragraph, "The O-ring leak check procedure and its potential effect on the ZCP," that is putty, "installation and possible displacement is also an urgent concern which requires an expedition of previously identified full-scale tests," so that to me, and clarify it if I am misinterpreting it, says we are concerned about the leak check procedure and the possibility of during a leak check displacing putty blow holes.

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MR. MULLOY: Yes, sir, and filling in behind that their concern, of course, was that the 200 psi leak check is designed to ensure again that you do seat the primary O-ring to verify that it is capable of seating, although you are seating it in the wrong direction because you are pressurizing between the two, and what they are acknowledging is, what you are probably doing in the time that the primary O-ring is traversing, you are probably blowing by there and creating blow holes in the putty, but then following up on that the question was, do you recommend reducing the leak check pressure?

DR. KEEL: Can we just go step by step, though, because that is exactly what they are saying here, and in fact that was the advantage that you were mentioning previously of going to 200 psi.

MR. MULLOY: That is correct.

DR. KEEL: That the putty wouldn't mask, and a blow hole would be created, so they are saying we have got concerns about the leak check procedure creating blow holes which requires urgent concern, which requires expedition of previously identified full-scale tests, and what you are saying is, you are reassured by the fact that you were creating the blow holes, and hence not masking the leak.

MR. MULLOY: That is correct, and what I am

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saying is, these engineers, and I presume you are going to have Mr. Miller here, will say that he would not recommend reducing the leak check pressure from 200 psi.

DR. KEEL: Well, we will ask him that question, but the point is that certainly they were concerned about the leak check procedures putting blow holes through.

MR. MULLOY: Yes, sir.

DR. KEEL: And that in fact was what reassured you from the standpoint of making sure the primary O-ring was seating, that you wouldn't mask—that the putty wouldn't mask, to assure it wouldn't mask you would go to a higher pressure, and there you were sure that you would get a blow hole through the putty, and hence not mask the leak check.

MR. MULLOY: That is correct, to assure that you have a primary leak check.

DR. KEEL: The penalty was this concern, to the degree that it is concern.

MR. MULLOY: That is correct.

DR. KEEL: They are also concerned about the cavity volume size, and obviously that is a critical quantity to predict erosion. You have to know that presumably with some accuracy to be able to predict

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absolutely, as you said previously, on an analytic basis.

MR. MULLOY: I withdrew that absolute.

DR. KEEL: I'll withdraw that. The other thing we found out, Mr. Mulloy, and I am not sure you are aware of it, and I believe your Marshall quality assurance people weren't aware of it, is that after you went to the higher pressure, if you look back at problems with the joints, the nozzle joints, and the problem being blow by or erosion or heat effect, that for the eight flights where you had a 50 psi leak check you had one problem, for the eight flights where you had a 100 psi leak check you had five problems, for the eight flights where you had a 200 psi check you had seven problems.

MR. WEAR: Yes, sir.

DR. KEEL: That seems to correlate between the problems that these were—that Mr. Miller at least was raising with respect to the leak check procedures, doesn't it?

MR. MULLOY: No, sir. I don't think you can take that single variable and make any conclusions from it. You have to also look at the putty changes, and the changes in the layup of the putty, and probably changes in the roundness of the segments as we begin to reuse

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them.

DR. KEEL: Is it enough to cause you concern?

MR. MULLOY: Oh, yes, yes, and that is what was being addressed.

MR. WEAR: The specific question you raise there about does this lead you to think, to have a second thought about some things you have done, yes, that was all addressed by our engineering people and by Thiokol's engineering people, about what is the correlation between leak check pressure, the change in putty manufacture, the changes we have made in putty layup patterns. Those were all questions that were raised, and you have got one item here addressing one particular aspect of it, but that question was raised, and attempts were made between these two engineering organizations to try to correlate and trace out what is the significant factor, and they were unable to do so.

DR. KEEL: I guess the reason for going through all of that is that you are using these two bases, the higher leak check pressure and your ability to analytically predict erosion, as the basis of waiving these launch constraints, and it is fairly clear if you go back through the history of all of this that they introduce problems, too, and the tradeoffs weren't quite that clear that that was absolutely reassuring that this

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was a basis upon which you could waive these launch constraints.

DR. WALKER: Do you agree that the primary cause of the erosion is the blow holes in the putty?

MR. MULLOY: I believe it is. Yes.

DR. WALKER: And so your leak check procedure created blow holes in the putty.

MR. MULLOY: That is one cause of blow holes in the putty.

DR. WALKER: But, in other words, your leak check procedure could indeed cause what was your primary problem. Didn't that concern you?

MR. MULLOY: Yes, sir.

DR. COVERT: Mr. Mulloy, what happens if you have, make a leak check and you discover that the primary O-ring leaks? Do you now stop and destack it?

MR. MULLOY: Yes, sir.

DR. COVERT: And then put a new O-ring in?

MR. MULLOY: Yes, sir.

MR. WEAR: You clean it all up back to zero and reputty.

GENERAL KUTYNA: Larry, let me follow Art's question. Blow holes in the putty cause erosion. The Titan joint doesn't have a lot of putty. Does the Titan joint show erosion?

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MR. MULLOY: Yes, it does, a higher rate than is happening on the SRM.

GENERAL KUTYNA: It is not necessarily blow holes that are causing erosion, but could I pursue just a quick line of questioning? This joint, is it the only one of this type in the industry?

MR. MULLOY: No.

GENERAL KUTYNA: Which one is the next closest to it?

MR. MULLOY: The Titan.

GENERAL KUTYNA: As a matter of fact, this was fairly highly derived from the Titan.

MR. MULLOY: Yes, that was the basis

GENERAL KUTYNA: What are the primary areas where it is the same as the Titan?

MR. MULLOY: It is the same in that it is a tang and clevis pin joint that uses a bore seal O-ring sealing.

GENERAL KUTYNA: It doesn't have the putty?

MR. MULLOY: It has a small amount of putty, but not filling a large gap like we have on the SRM.

GENERAL KUTYNA Now, isn't it usual in the industry and between agencies that if we have a problem we come running to each other and advise if we have similar pieces of equipment? When we had the inertial

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upper stage we worked together very closely on that. When the PAMs failed, we were the program office on PAM and got together on that, and even when you had the burn-through on the nozzle on STS-8 you alerted us about that.

I had the program office for Space Shuttle for two years, 1982 to 1984, and of course was in charge of all Titans at that time, and I never once heard about problems with the O-rings. When I called the program office this morning on the Titans, they had received no prior indications that there was a large problem with the O-rings. Why would that be? Why wouldn't you have notified them?

MR. MULLOY: Well, the approach taken was, I did contact someone from CSD, and I think there is a memo in the stack of documents here that is in response to that back in March of 1984, and there were some interchanges at lower levels between engineers who know each other in the field, but nothing, no official letter that went to Space Division.

GENERAL KUTYNA: But Larry, here you are, and you have grounded this thing almost. You have a flight constraint that you can't fly unless you clear it, last year, 1985. I in headquarters was not aware of it. The program manager out at Space Division was not aware of

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it. It just wasn't made that big a thing, and yet you have got a flight constraint. Why is that?

MR. MULLOY: It would be because as far as I know I didn't contact you or the program manager directly to share that information or to request information on the Titan experience. There were at other levels some interchanges in that regard, and I think Larry Wear can talk to them.

MR. WEAR: Yes, there were. Specifically, Miller put his signature on this particular letter, has quite particular and specific contacts within the ETC echelon regarding the Titan. There was considerable exchange of data of what Titan has done and what Titan's experience is and why they think it is. You might want to ask him about that.

GENERAL KUTYNA: Who in the Air Force?

MR. WEAR: I know of no specific contacts with the Air Force per se, but we did hold a session at Thiokol. We spent a whole day with Joe Banna from Aerospace, and a group of six or seven people, which were principally from Aerospace and some other organizations.

GENERAL KUTYNA: When was this?

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MR. WEAR: About a year and a half ago at Thiokol, where we discussed, we laid out for them, we spent a whole day laying out with them all of the problems we have had with our joint, the design details of it, the idiosyncrasies of it, and we asked them to exchange back to us comparable data from the Titan program, and their thoughts of what we were doing, and to be honest with you, I never received a response from Mr. Banna.

GENERAL KUTYNA: Now, during this time, a year and a half ago, was there not a competition for a follow-on launch vehicle to complement the Shuttle? Were you not aware of that?

MR. WEAR: There were some discussions, yes. There was an exercise for another——

GENERAL KUTYNA: Larry, did Marshall put an entry in the competition?

MR. WEAR: Yes, there was a Marshall design submitted to that.

GENERAL KUTYNA: And is that sort of unusual because Marshall was competing against two industry guys?

MR. WEAR: That is not normal for Marshall's line of business.

GENERAL KUTYNA: And what was the construction

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of that competitor? What was the makeup of that competing rocket?

MR. WEAR: It would have been United Technologies.

GENERAL KUTYNA: But what were the segments of that rocket made of?

MR. WEAR: They were derived from our SRM.

GENERAL KUTYNA: There were three solid rocket boosters?

MR. WEAR: That is right.

GENERAL KUTYNA: That is all.

MR. ACHESON: I have one question. I have never understood why it was that cold was not originally thought to be a variant which could be important or might conceivably become a launch constraint. Reading these documents, we see an explicit flag in the flight readiness review presentation from Thiokol, February of 1985, on 51E pointing out that low temperature enhances the probability of erosion.

You have the presentation at headquarters in August of 1985 that made it plain that resilience was an important factor in the function of the O-ring. If you accept that concept, it is very hard for me to see why a lot of difference in temperature wouldn't be understood

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to make substantial difference in the functioning of the O-ring, and when you come to the coldest day you have ever had for a launch by a margin of 20 degrees, why it was not thought important, and yet you have the description of the telecon in which reference was constantly made to the higher temperatures at which erosion had been observed, as if this somehow established that temperature was not a discriminator.

I have never understood that. You are dealing with an elastomeric material which, certainly, you don't have to be an engineer to know is affected by temperature, I have difficulty understanding, and perhaps you could explain, why that has been a missing factor in these equations, seemingly, all through the history of this project.

MR. MULLOY: Are you speaking that it seems to be missing in the Marshall documentation? Where concerns have been expressed in other areas, concerns are not expressed in the cold temperature areas?

MR. ACHESON: This is a concern Marshall apparently had for that problem.

MR. MULLOY: I can give you my thought process, and again, that can be questioned. Looking at the total context of Thiokol's conclusions on 51E, where it was concluded that cold temperature does affect or

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enhance the possibility of erosion. If you continue, though, to read that total Thiokol presentation, the conclusion is that what they saw on 51C really fits within previous experience, and so it turns right around and kind of refutes that there is much different, even though the temperature was a little colder.

They further conclude that that type of erosion can be expected on subsequent flights no matter what the temperature is, I believe, if I am remembering what they said in that presentation to me.

DR. KEEL: Can I read you the conclusion, so we all know what it was? [Ref. 5 2-11]

MR. MULLOY: 51E.

DR. KEEL: It is the 51E flight readiness review, after 51C, of course, which was the previous coldest launch, and I won't read all of this, because it is an extensive review they gave to you.

MR. MULLOY: It is.

DR. KEEL: Based upon an urgent request from you to review all of the O-ring history that you sent out a few days after the 51C launch? [Ref. 5 2-12]

MR. MULLOY: All my messages are urgent, by the way.

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DR. KEEL: I will go through your conclusion, and I recognize there is a lot more to the analysis and the scenarios in trying to explain what happens, but the conclusion is that STS-51C is consistent with the erosion data base. That means that the erosion there presumably in your vernacular hasn't exceeded the previous experience or is greater than worst case, but then in a subbullet they say low temperature and hence probability of blow-by. STS-51C experienced worst case temperature in Florida history, and so that is the conclusion they draw there.

CHAIRMAN ROGERS: Now, getting back to Mr. Acheson's question in light of that statement and others on weather—why wasn't more consideration given to the weather, particularly on Flight 51L, why wasn't—why didn't that become extremely serious in light of all the other things that the documents reflect?

MR. MULLOY: Yes, sir. I had started to explain my thought process in that, and I go back to the extensive look at that we did with 51C after experiencing the worst, the coldest temperature in Florida history, that the erosion was not outside of what we had experienced at 80 degrees, and that also the conclusion that that type of erosion which we all understood could be anticipated in the future because we

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know we can get paths through the putty and hot gas impingement through those paths will cause O-ring erosion, and the conclusion that 51E could be flown under those circumstances.

The next thing in the August presentation, if you look at that total presentation, I think there is maybe one short reference again about the same thing that was covered in the 51E FRR, and then you get to the night of the discussion of January 27th, where the engineers were essentially citing the same data relative to the effect of resiliency, and that their concern was increased blow-by of the primary O-ring seal, which I took to be what engineers always do, realize what risk you are taking because you could have increased blow-by, thus higher erosion.

The ultimate conclusion of that was that the data did not specifically conclude that you would have higher erosion on the primary O-ring seal, and the second part of the conclusion was that during that pressurization sequence the joint was redundant. Now, that was the thought process which, as I say, has been and will continue to be questioned.

DR. KEEL: Can I just ask again, Larry, for a clarification? You talk about erosion, of course, but the footnote of the conclusion was a reference to

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blow-by. This was the worst case of blow-by.

MR. MULLOY: That is correct.

DR. KEEL: It was the largest arc. There was black soot, black grease, and what it said was, it enhances the probability of blow-by.

MR. MULLOY: Yes, and it further states the condition is acceptable, I believe.

DR. KEEL: That is correct. You are exactly right.

MR. MULLOY: And that was my basis.

DR. KEEL: They do say that the condition was acceptable throughout all of the flight readiness reviews, that the risk was acceptable.

CHAIRMAN ROGERS: But they don't say that the worst condition would be acceptable. In other words, they don't say if the weather was even worse it would be accepted, and when the weather got worse they said, we recommend against launch, and so I have trouble with your logic.

MR. MULLOY: I understand, sir.

DR. KEEL: Could I ask just a couple more questions on this, Mr. Chairman, since we are on this subject?

What we have determined from our individual staff investigations and staff interviews, Mr. Mulloy, is

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that apparently after this extensive briefing that Marshall gave to you—excuse me, that Morton Thiokol gave to you, and in fact at your request to a large extent, my understanding is that the center board—and I assume that's Dr. Lucas—wasn't given that briefing.

MR. MULLOY: No, they were given a summary briefing.

DR. KEEL: My understanding is, in fact, that the erosion problem wasn't mentioned.

MR. MULLOY: Yes, sir, on 51E it was mentioned. At the Level—it was discussed at the Level 1, 2 FRR at the Marshall center board, the Shuttle projects office.

DR. KEEL: Can you tell us what was said at the center board?

MR. MULLOY: No, sir. It is the type of chart, I think, that Mr. Rogers was referring to, which is a summary chart.

DR. KEEL: Well, I know it was mentioned at Level 1. Maybe you could provide that, because we have been unable to get that center board briefing. We have Level 1, but our information was in fact from people there that it was not mentioned, so if you could clarify for the record, that is the purpose.

MR. KEHRLI: We do have the center board

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briefing, and in that briefing I have not seen the chart. If you could provide it, that would be fine.

DR. KEEL: That is even more important. We have a briefing that doesn't show it, but then the next question, and then a final one on this is, you are right, at the Level 1 it was mentioned, but in fact you had this extensive briefing, which was made up of six or eight charts, I guess, from Morton Thiokol going into the whole history of ring erosion and mentioning that conclusion I just mentioned.

Then at the Level 1 briefing it was condensed down to one entry under problem summary. That basically said evidence of hot gas passed primary O-rings on two case joints, and then concerned mission safety resolution acceptable risk because of limited exposure and redundancy. Can you explain why?

MR. MULLOY. Does it reference another flight readiness review?

DR. KEEL: It references STS-41C, flight readiness review.

MR. MULLOY: That was a more extensive review then that documents that rationale again, which is the rationale for the limiting of the erosion, and the Thiokol conclusion, of course, that what they saw on 51-C was consistent with the previous experience, and it

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could be anticipated on subsequent flights. So you would have to go to the 41G FRR.

DR. KEEL: I have got the———

MR. MULLOY: I'm sorry.

DR. KEEL: It is two charts that were mentioned there on 41C, and they didn't go through all of the O-ring history, of course, because that was several missions before that Thiokol had presented to you, so it made a reference to that, and presumably you didn't have the charts there, and secondly there were two charts, and they didn't reflect everything that had happened since 41C flight readiness review, and so I guess it is still difficult to understand from my viewpoint why a more extensive reference wasn't made to this problem.

MR. MULLOY: Well, I can give you—there is a reason. There is a very practical reason for that, and that is that the Level 1 FRRs are generally limited to four hours and not two days. So there is a screening and a summary of the information as it goes forward. You have the total FRR packages. You will see the FRR I take is this—as you will see the FRR that goes to the shuttle is this thick, and you will see the one that goes to the center is this thick, and you will see the one that goes to Level 2 and 1, a very abbreviated form

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of that, and then what happens is, folks who take those briefings ask and assign actions usually say, I would like to hear more about that and have the total briefing, in which case we accommodate that outside the flight readiness review activity, but that is the practical reason.

DR. KEEL: I understand that when you condense from six to eight pages to one bullet it is a little difficult to understand, especially when you actually trigger the assessment based upon an urgent request message sent back to Mr. Wear, in fact, who then presumably relayed that to Morton Thiokol. The other aspect, I guess, that is puzzling is that you—even that one bullet, the resolution is based on one acceptable risk, but also redundancy.

Can you explain how redundancy is a factor for resolving this concern when Criticality 1 says it is not redundant?

CHAIRMAN ROGERS: Just to take that question a little more directly, doesn't that fly directly in the face of the Criticality 1?

MR. MULLOY: No, sir. I don't think so, and I know that we have had—you all have had difficulty understanding that.

CHAIRMAN ROGERS: I sure do.

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MR. MULLOY: I don't know that we will have any more success with it, but it is again—I can see your interpretation of the words in the CIL. I would acknowledge that from—that those words, the wording could have been clearer. The intent was to show what is the physical phenomenon. The physical phenomenon is that after motor pressurization under "worst case conditions, the secondary O-ring may not be in a position to seal if called upon to do so by failure of the primary O-ring.

CHAIRMAN ROGERS. And the conclusion was, it was not redundant.

MR. MULLOY: After motor pressurization. That is the key. After motor pressurization. Those words are in there, and the sentence structure is probably very poor, but the fact is that on all flights that we have flown to date, with the exception of one case joint on STS-4, given the dimensional tolerances that exist, that with after motor pressurization, the secondary O-ring still has positive squeeze on it, and is indeed a redundant seal.

CHAIRMAN ROGERS: So you relied on redundancy then ever since?

MR. MULLOY: No, sir. We relied first on the test and analysis that said that the primary O-ring

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erosion would not cause failure of the primary O-ring to seal. That is the first thing. The second thing then is, one has redundancy.

CHAIRMAN ROGERS: So you relied on redundancy since then?

MR. MULLOY: Yes, sir.

MR. KEHRLI: Mr. Chairman, there is one document that addresses that specific question that I would like the witnesses to examine, and that document is from Thiokol, from Brian Russell to J.W. Thomas. It is Attachment 32 to the O-ring history, and I am wondering if you would care to expand on your answer in light of that document, which specifically addresses the timing function of the secondary seal and also raises resiliency as an issue. [Ref. 5 2-13]

CHAIRMAN ROGERS: Try to keep your voice up. What is the question?

MR. KEHRLI: The question is, I am wondering if the witnesses would respond to the same question with regard to the timing function of the O-ring secondary seal in light of the letter from Brian Russell to Jim Thomas at Marshall.

CHAIRMAN ROGERS: Do you conclude that this letter is contrary to what Mr. Mulloy just said?

MR KEHRLI: Well, that is what I am

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wondering, whether it is or not, in light of Mr. Mulloy's interpretation.

MR. MULLOY: I have seen this memo. I first saw the front page. I didn't think I had. But if you would go to the second question, if the primary O-ring does not seal, will the secondary seal seat in sufficient time to prevent joint leakage? Answer, on the next page. MTI has no reason to suspect that the primary seal would ever fail after pressure equilibrium is reached, i.e., after the ignition transient. If the primary O-ring were to fail from zero to 170 milliseconds, there is a very high probability the secondary O-ring would hold pressure.

Since the case is not expanded appreciably at this point, if the primary seal were to fail from 170 to 330 milliseconds, the probability of secondary seal holding is reduced. From 330 to 600 milliseconds, the chance of the secondary seal holding is small. This is a direct result of the O-ring's slow response compared to the metal case segments as the joint rotates.

That is the same thing, I believe, as was in the August 19th briefing here at NASA headquarters, and so if your question is, does that change the conclusion relative to the secondary seal being able to hold——

MR. KEHRLI: Let me be specific with the

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question. The question is, and you have repeated it several times, and other witnesses have as well, that this redundancy is affected by the worst case tolerance. The secondary O-ring is still there so long as you don't have bad hardware.

MR. MULLOY: Yes.

MR. KEHRLI: There is no mention of that in the answer to this question.

MR. MULLOY: That is correct.

MR. KEHRLI: What is your understanding? Does this answer incorporate that worst case tolerance?

MR. MULLOY: Yes, I think so. I think it does. The worst case would be that you start out with minimum O-ring squeeze of 7.5 percent, and now what you are doing is reducing the squeeze, and at 170 to 330 milliseconds that squeeze is reduced just somewhat, and then the joint rotates to 32/1,000ths. If you started at 20/1,000ths you don't have any squeeze on it, and that minimum squeeze, though, would only occur in hardware that is outside of the base, as we have flown, is my understanding from Thiokol.

DR. WALKER: What do you understand by the term "high probability?"

MR. MULLOY: I think my understanding would be that the high probability is related to a case where

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either the O-ring is not tracking the metal due to reduced resiliency or a case where you started with a minimum 20/1,000ths squeeze, and due to tolerances you have—well, you started with a minimum 20/1,000ths with tolerances. You rotate 32/1,000ths. You don't have any squeeze at all.

DR. WALKER: I don't think you understood my question. If, for example, I were to say to the President that I think the Shuttle has a high probability of working, do you think he should have the program continue in that state? I mean, is that a precise enough and sufficiently safe enough situation that he should continue to fly? What does high probability mean? Does that mean 75 percent, 80 percent, 82 percent?

MR. MULLOY: I don't know. I can't quantify that.

DR. WALKER: But surely you are basing your decision to proceed on this assertion that the secondary seal has high probability of working.

MR. MULLOY: Yes, sir, and the reason was that the secondary seal would be energized in the zero to 170 millisecond or 330 millisecond time frame.

DR. WALKER: Does that mean 90 percent of the time or 70 percent of the time?

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MR. MULLOY: I don't know, sir.

DR. WALKER: But you are basing your decisions on that.

CHAIRMAN ROGERS: Dr. Walker's question is a very good one in that it said, if we report to the President that we think he should continue the program because it has a high probability of being safe, would that be satisfactory, or there is a high probability that it is not unsafe, and maybe that is not your question.

DR. WALKER: To me, a high probability is not good enough for the operations of something like this.

CHAIRMAN ROGERS: Particularly when you have a constraint on it and you really haven't solved the problem, and you are still speculating on whether it is going to work under certain conditions or not.

Well, unless there are any other questions——

MR. RUMMEL: I have one, just to return for a moment, if I may, to an earlier point.

If I understood you correctly, you indicated that out of roundness was not considered when evaluating the squeeze or the relative fit of the segments.

MR. MULLOY: That is my understanding, sir.

MR. RUMMEL: But you, of course, understood and knew that out of roundness to varying degrees

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existed from Day One due to shipping and so forth, as has been explained a number of times, that these items were out of round, and some required squeeze and some not.

MR. MULLOY: Yes, sir.

MR. RUMMEL: And I guess indeed that is one of the reasons for the destacking operating at the moment, is to see whether any out of roundness might have adversely affected the seals.

MR. MULLOY: Yes, sir.

MR. RUMMEL: My question is, knowing that, why wasn't it considered, why wasn't out of roundness measured and taken into account, or why wasn't it taken into account in your calculations when you were estimating, or your judgmental considerations with respect to the adequacy of the O-rings.

MR. MULLOY: It was an obvious oversight, sir.

MR. RUMMEL: It is a pretty obvious oversight, isn't it?

MR. MULLOY: Yes sir, it is.

CHAIRMAN ROGERS: Okay, if there are no further questions, we will break for lunch.

(Whereupon, at 12:15 p.m., the hearing was recessed, to reconvene at 1:10 p.m. of the same day.)

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National Aeronautics and Space Administration

George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama
35812

NASA

SEP 1 5 1980

Reply to Attn of: SA01

TO: Distribution

FROM: SA01/Mr. Lindstrom

SUBJECT: Assigning Launch Constraints on Open Problems Submitted to MSFC PAS

The Shuttle Projects Office has established a requirement for identification of launch constraints for problems being reported to the MSFC Problem Assessment System (PAS) by element contractors. Each element contractor (Rocketdyne, MMC, USBI, and Thiokol) has been directed to support this requirement by providing launch constraint information on each new problem submitted to the MSFC PAS. The launch constraint information provided by the contractor is based upon their preliminary technical evaluation and will require final concurrence by the responsible element project manager.

a. The following guidelines have been established to aid in making constraint decisions on open problems and are limited to recurrence control determination only. In accordance with practices established on past programs, remedial actions (e.g. removal and replacement of defective hardware, etc.) for correcting discrepancies on the vehicle to be launched are considered launch constraints and are tracked by the KSC system.

(1) All open problems coded criticality 1, 1R, 2, or 2R will be considered launch constraints until resolved (recurrence control established and its implementation effectivity determined) or sufficient rationale, i.e., different configuration, etc., exists to conclude that this problem will not occur on the flight vehicle during prelaunch, launch, or flight.

(2) Problems coded criticality 3 will not be considered launch constraints unless (a) the potential exists of leading to a criticality 1 or 2 failure mode; or (b) the failed component has multiple use on the element and more than one occurrence could lead to a criticality 2 condition; or (c) the failure could result in multiple loss of flight instrumentation channels. If a criticality 3 is determined to be a launch constraint, it will be treated the same as a.(1) above.

[Ref. 5-2-1 1 of 4]

b. To assure that each reported problem is reviewed for correct criticality and constraint assignment by the appropriate MSFC personnel, the following procedure will be followed:

(1) The responsible S&E design actionee and the element project office actionee will review each problem upon receipt to assure that the criticality and constraint assignments meet with their approval. Exceptions to criticality or constraint assignment will be coordinated with the Problem Assessment Center (PAC) actionee within two working days from receipt of the problem report.

(2) The Problem Assessment Center will prepare a weekly constraints list by element. This list will be submitted to the Shuttle R&QA Support Office, EG03, for input to the Shuttle Projects Manager, SA01. Copies of the constraint list will be furnished concurrently to each Shuttle Element Project Manager.

(3) Launch constraints will be reviewed at each Problem Review Board (PRB) meeting.

The Problem Assessment Center will be responsible for coordinating all launch constraint activity and assuring that information is properly documented in the Problem Assessment System (PAS) data base and transmitted to MSFC management.

Robert E. Lindstrom
Manager
Shuttle Projects Office

Distribution:
See page 3

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MSFC PROBLEM ASSESSMENT SYSTEM
FEBRUARY 26, 1986

PAGE 1 OF 3
MSFC RECORD NUMBER
A09288
CONTRACTOR REPORT NUMBER
DR4-5/259

PROBLEM TITLE: O-RING EROSION IN THE CASE TO NOZZLE JOINT

ELEMENT SRM	CONTRACTOR THIOKOL	SYSTEM STRUCTURE	CRIT 1R	SYS LEVEL YES	MISC CODE ABCDEFGHIJKLMNO 2 X A49
HARDWARE IDENTIFICATION NONCONFORMING ITEM NEXT HIGHER ASSY ARTICLE		NOMENCLATURE PRIMARY O RING AFT SEGMENT SRB ASSY	PART NUMBER 1U52557-00 1U50454-01 10101-0001	SERIAL/LOT SRM-16A SRM-16A BI01AL	MANUFACTURER HYDRAPAK THIOKOL THIOKOL
TEST / OP FLT	PREV COND PT-FLT INS	LOCATION KSC-X	SYMPTOM 121	FAIL/UNSAT UC	FAIL MODE 123
CAUSE W20	FMEA NUMBER 10 01-01				
LAUNCH CONSTRAINTS 51G - 51F, 51I, 51J, 61A, 61B, 61C.	CONCUR JMT	TIME CYCLES N/A	WUC A. MCDONALD	DATE OCCURED 04/29/85	DATE REC 07/10/85
VEHICLE EFFECTIVITY A1019 & 5005			R/C A20	EST RES DATE	CONT CLOSURE REC. 12/10/85

PROBLEM DESCRIPTION:

REF: SIMILAR REPORTS - A07934, A08299, A08014, A08615, A08687, A08939, A09740
PRIMARY O RING EROSION WAS EXPERIENCED ON THE AFT SEGMENT OF BOTH SRM 16A & B. SRM-16A HAD THREE DEEP EROSION POCKETS. THE GREATEST BEING 0.171 IN., WITH SOOT BETWEEN THE PRIMARY & SECONDARY O-RING & NONE EROSION ON THE SECONDARY O-RING. SRM-16B HAD MINOR EROSION, THE GREATEST POCKET BEING .005 IN., WITH NO SOOT BETWEEN THE PRIMARY & SECONDARY O-RINGS.

INVESTIGATION / RESOLUTION:

7/11/85 - POST FLT INSPECTION OF SRM 16A REVEALED A GAS PATH THROUGH THE VACUUM PUTTY AT 54 DEGS. THE PRIMARY O-RING HAD 12 IN. OF CIRCUMFERENTIAL (HOOP) LENGTH WHICH EXPERIENCED HEAT DEGRADATION AT THIS LOCATION. MAJOR EROSION OCCURRED ADJACENT TO THE HOLE IN THE PUTTY FOR 1.50 IN. IN THE CIRCUMFERENTIAL (HOOP) DIRECTION. WITHIN THE 1.50 IN. OF MAJOR EROSION THERE WERE THREE DEEP EROSION POCKETS. THE GREATEST AMOUNT OF EROSION WAS 0.171 IN. & THE GREATEST AMOUNT OF CROSS SECTIONAL PERIMETER ERODED WAS 132 DEGS. BETWEEN THE PRIMARY & SECONDARY O-RING THERE WAS HEAVY DEPOSIT OF SOOT FOR 90 DEGS TO EACH SIDE OF THE GAS PATH WITH A LIGHTER DEPOSIT FOR THE REMAINING 180 DEGS.

AT 54 DEGS THE SECONDARY O-RING EXPERIENCED 0.032 IN. OF EROSION & THE CROSS SECTIONAL PERIMETER WAS ERODED FOR 145 DEGS. THE CIRCUMFERENTIAL EROSION WAS 3.1 IN. WITH NO ADDITIONAL HEAT AFFECTED LENGTH. THERE WAS NO SOOT PAST THE SECONDARY O-RING.

POST FLT INSPECTION OF SRM 16B REVEALED A GAS PATH THROUGH THE VACUUM PUTTY AT 14.6 DEGS. AT THIS LOCATION THE PRIMARY O-RING WAS ERODED 0.005 IN. DEEP WITH 124 DEGS OF CROSS SECTIONAL PERIMETER ERODED. MAJOR EROSION EXTENDED FOR 1.4 IN. THE CIRCUMFERENTIAL DIRECTION WITHIN A HEAT AFFECTED LENGTH OF 4.7 IN. THE SECONDARY O-RING WAS UNAFFECTED.

BASED ON THE AMOUNT OF CHARRING, THE EROSION PATHS ON THE PRIMARY O-RING & WHAT IS UNDERSTOOD ABOUT THE EROSION PHENOMENON, IT IS BELIEVED THAT THE PRIMARY O-RING OF SRM 16A NEVER SEATED.

ASSIGNEE	R. MCINTOSH DCRIOR	D. NEWMAN R & Q.A.	L. WEAR PROJECT	J. FLETCHER PAC	PAC REVIEW COMPLETE JP
APPROVAL				MSFC CLOSURE DATE	STATUS OPEN-R

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MSFC PROBLEM ASSESSMENT SYSTEM
(C O N T I N U A T I O N)

PAGE 2 OF 3
MSFC RECORD NUMBER
A07934
CONTRACTOR REPORT NUMBER
DR4-5749

7/15/85 - THIS PROBLEM IS NOT CONSIDERED A CONSTRAINT TO 51F. A 100 PSIG LEAK CHECK IS PERFORMED
WHICH CONFIRMS SEATING OF THE SECONDARY O-RING. NOZZLE O-RINGS HAVE BEEN SHOWN TO SURVIVE EROSION
DEPTHS OF .133 AND WILL NOT SUPERCALE [illegible]. THE MATH MODEL PREDICTS THE MAXIMUM EROSION DEPTH FOR THE
SECONDARY O-RING TO BE 32 MILS IN THE WORST CASE CONDITION. THE EROSION ON THE SECONDARY O-RING WAS
32 MILS, WELL BELOW THE DEMONSTRATED SEALING CAPABILITY. ANALYTICAL RESULTS INDICATE THAT A WORST
CASE EROSION CONDITION WOULD NOT ERODE THE SECONDARY O-RING BEYOND ITS SEALING CAPABILITY. SEAL
CAVITY LEAK CHECK AT 100 PSIG CAN BE MASKED BY THE VACUUM PUTTY. HOWEVER, AT 200 PSIG THE VACUUM
PUTTY CANNOT MASK THE LEAK CHECK. STS-51F (SRM 19) HAS THE 200 PSIG LEAK CHECK AND THEREFORE SHOULD
EXHIBIT BEHAVIOR MORE LIKE STS-51D (SRM-17) AND STS-51B (SRM 18) WHICH EXHIBITED PRIMARY O-RING
EROSION WHICH WAS WITHIN EXPERIENCE BASE WITH NO SECONDARY O-RING EROSION. ALL JOINTS WITH
VACUUM PUTTY WILL BE LEAK CHECKED TO 200 PSIG IN THE FUTURE.

REF. TWR 15088 "STS-51F SOLID ROCKET MOTOR (SRM 19) FLT READINESS REVIEW," DATED 7-1-85. SRM SPECIAL
TOPIC: NOZZLE O-RING ANOMALY OBSERVED ON STS-51B (SR-1AA) - ---JWT 7-19-85 ---

7/23/85 UPDATED FROM 7-11-85 PRB - RATIONALE FOR ACCEPTANCE WAS DISCUSSED AS INDICATED BY THE
7-2-85 UPDATE. NO OTHER SIGNIFICANT INFO WAS PROVIDED.

8/9/85 - STS-51F (SRM-19) FIELD JOINT O-RING INSPECTION REVEALED SEVERAL GAS PATHS THROUGH THE
PUTTY BUT NO O-RING EROSION WAS FOUND ON ANY OF THE CASE JOINTS. THE NOZZLE TO CASE O-RING JOINT
REVEALED A GAS PATH AT 21.6 DEGR WITH THE PRIMARY O-RING EXPERIENCING EROSION OF APPROX. 0.010 IN.
THIS IS WITHIN THE FLT EXPERIENCE DATA BASE & IS NOT A FLT CONSTRAINT TO STS-51I (SRM-20) -
--- JAMES W. THOMAS 8-11-85---

8/19/85 UPDATED FROM 8-8-85 PRB - NOTHING NEW TO REPORT AT THIS MEETING.

9/20/85 UPDATED FROM 21 DAY REPORT - RESULTS OF THE TEAM INVESTIGATION ARE THAT THE PRIMARY O-RING
FAILED TO SEAT AT 51 DEGS WHEN THE GAS JET IMPINGED UPON IT. A PORTION OF THE GAS JET THEN PASSED
THE PRIMARY O-RING & CONTINUED UNTIL IT IMPINGED ON THE SECONDARY O-RING. THE SECONDARY O-RING
SEATED & SEALED THE JOINT. THE PRIMARY O-RING CONTINUED TO ERODE DUE TO A COMBINATION OF HOT GAS
JET IMPINGEMENT & CONVECTION HEATING UNTIL THE IGNITION TRANSIENT WAS COMPLETED & THE MOTOR REACHED
OPERATING PRESSURE APPROX. 600 MILLISECONDS AFTER IGNITION.

9/20/85 - ON STS-51I NO O-RING EROSION WAS EXPERIENCED THAT EXCEEDED THE CURRENT EXPERIENCE BASE.
COAL PRIMARY O-RING EROSION WAS NOTED ON THE LH NOZZLE TO CASE JOINT. NO SOOT WAS FOUND BEHIND THE
PRIMARY O-RING. THIS PROBLEM IS NOT A CONSTRAINT TO STS-51J - --J.W.---

10/16/85 - ON STS-51J NO O-RING EROSION WAS EXPERIENCED THAT EXCEEDED THE CURRENT EXPERIENCE BASE.
THIS PROBLEM IS NOT A CONSTRAINT TO STS-61A - - JAMES W. THOMAS 10-17-85 ---

11/6/85 - ON STS-61A NO O-RING EROSION WAS EXPERIENCED THAT EXCEEDED THE CURRENT EXPERIENCE BASE.
THIS PROBLEM IS NOT A CONSTRAINT TO STS-61B - --- JWT 11-14-85---

12/10/85 - ON STS-61B NO O-RING EROSION WAS EXPERIENCED ON THE FIELD JOINTS. HOWEVER, MINOR O-RING
EROSION WAS EXPERIENCED ON BOTH THE LH AND RH NOZZLE TO CASE JOINTS. SOOT BLOW-BY WAS NOTED ON THE
LH NOZZLE JOINT. THE EROSION EXPERIENCED DURING THE STS-61B LAUNCH WAS WELL WITHIN THE CURRENT
EXPERIENCE BASE. THIS PROBLEM IS NOT A CONSTRAINT TO STS-61C - --JWT 12-12-85---

1/23/86 RESOLUTION - SRM FIELD AND NOZZLE JOINTS HAVE EXPERIENCED EROSION OF THE PRIMARY
O-RINGS DURING SEVERAL MISSIONS AND STATIC TEST AS DETERMINED BY POST FLIGHT INSPECTION.

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MSFC PROBLEM ASSESSMENT SYSTEM
(C O N T I N U A T I O N)

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MSFC RECORD NUMBER
A07934
CONTRACTOR REPORT NUMBER
DR4-5747

THE PRIMARY O-RING IN A NUMBER OF INSTANCES WAS SUFFICIENTLY DAMAGED AND/OR DISPLACED TO ALLOW
BLOWBY, THEREFORE EXPOSING THE SECONDARY O-RING TO HOT COMBUSTION GASES. SOME EROSION WAS
EXPERIENCED ON THE SECONDARY O-RING AT THE NOZZLE JOINT BUT NOT TO THE POINT WHERE THE O-RING
SEALING CAPABILITY WAS JEOPARDIZED.

THE CAUSE OF THE O-RING EROSION HAS BEEN ATTRIBUTED TO HOT GAS PATHS THROUGH THE VACUUM PUTTY
ALLOWING THE HOT GASES TO IMPINGE UPON THE O-RING. IMPROPER SEATING OF THE PRIMARY O-RING ALLOWED
BLOWBY AND MINOR HOT GAS IMPINGEMENT ON THE SECONDARY O-RING. THE MAGNITUDE OF THE O-RING EROSION
IS A FUNCTION OF ORIFICE SIZE AND FREE VOLUME (AREA BETWEEN THE PRIMARY O-RING AND THE PUTTY).

PRIMARY O-RING EROSION IS EXPECTED TO CONTINUE SINCE NO CORRECTIVE ACTION HAS BEEN ESTABLISHED THAT
WILL PREVENT HOT GASES FROM REACHING THE PRIMARY O-RING CAVITY. STEPS HAVE BEEN TAKEN TO ASSURE
THAT THE SECONDARY O-RING WILL BE SEATED AND ANALYTICAL ANALYSIS HAVE INDICATED THAT UNDER A WORST
CASE SITUATION, EROSION OF THE SECONDARY O-RING WILL NOT BE SEVERE ENOUGH TO ALLOW A LEAK PATH PAST
THE SECONDARY O-RING. STW7-2747 WAS CHANGED EFFECTING AN INCREASE IN THE LEAK CHECK PRESSURE FROM
100 PSIG TO 200 PSIG. WITH THE 100 PSIG LEAK TEST THE SECONDARY O-RING WOULD BE SEATED BUT ANY
GAS PATHS THROUGH THE PUTTY WOULD BE UNDETECTABLE. BY INCREASING THE LEAK TEST PRESSURE TO 200
PSIG, ANY LEAK PATHS THROUGH THE PUTTY WOULD BE DETECTABLE. THE 200 PSIG LEAK TEST WAS IMPLEMENTED
ON SRM-19 AND SUBSEQUENT MOTORS.

THE STACKING PROCEDURES HAVE BEEN CLARIFIED TO ASSURE MOTOR-TO-MOTOR CONSISTENCY IN JOINT MATING.
THE OPERATIONAL MAINTENANCE DOCUMENT CHANGES WERE EFFECTIVE ON SRM L024 AND SUBS..

ANALYTICAL STUDIES BASED ON BOTH IMPINGEMENT EROSION AND BLOWBY EROSION SHOW THAT THIS PHENOMENON
HAS AN ACCEPTABLE CEILING SINCE IMPLEMENTING THE ABOVE CHANGES. RECENT EXPERIENCE HAS BEEN WITHIN
THE PROGRAM DATA BASE.

THIS SEAL IMPROVEMENT PROGRAM PLAN WILL CONTINUE UNTIL THE PROBLEM HAS BEEN ISOLATED AND DAMAGE
ELIMINATED TO THE SRM SEALS. STATUS WILL CONTINUE TO BE PROVIDED IN THE FLIGHT READINESS REVIEWS
AND IN FORMAL TECHNICAL REVIEWS AT MTI AND MSFC. AT THE CONCLUSION OF THE PROGRAM, A COMPREHENSIVE
REPORT WILL BE WRITTEN TO CONSOLIDATE THE RESULTS, CONCLUSIONS, AND RECOMMENDATIONS.

THIS PROBLEM IS CONSIDERED CLOSED BASED ON MTI REPORT TWR-14359, REV. A, "IMPROVEMENTS OF SPACE
SHUTTLE SRM MOTOR SEALS" DATED 8-30-85 AND MTI LETTER E150/BGR-86-114 "RATIONALE FOR CLOSURE OF THE
O-RING EROSION PROBLEM", A07934, DR4-5/301 DATED 1-2-86.

[Ref. 5-2-2 3 of 3]

MSFC PROBLEM ASSESSMENT SYS
March 7, 1986

PAGE 1 OF
MSFC RECORD
A07934
CONTRACTOR RE
DR4-5/30

PROBLEM TITLE: SEGMENT JOINT PRIMARY O-RING CHARRED

ELEMENT	CONTRACTOR	SYSTEM	CRIT	SYS LEVEL	MISC CODE	ABCDEFGH
SRM	THIOKOL	STRUCTURE	1R	YES		G K

HARDWARE IDENTIFICATION	NOMENCLATURE	PART NUMBER	SERIAL/LOT	MANUFACTURE
NONCONFORMING ITEM	PRIMARY O-RING	1U50228-24	SRM-MO10A	THIOKOL
NEXT HIGHER ASSY	SRB ASSY	10101-0001	SRM-MO10A	THIOKOL
TEST ARTICLE	SRB ASSY	10101-0001	SIO10L	THIOKOL

TEST / OP	PREV COND	LOCATION	SYMPTOM	FAIL/UNSAT	FAIL MODE	CAUSE	FMEA #
FLT	PT-FLT ING	KSC-K	113	UC	123	W20	10-01-0

LAUNCH CONSTRAINTS	CONCUR	TIME CYCLES	WUC	DATE OCCURED	DATE REC
NONE	JOR	N/A	8 LANEY	02/03/84	02/17/84

VEHICLE EFFECTIVITY	R/C	EST RES DATE	CONT CLOSU
SI019 & SUBS	N20		12/10/85

PROBLEM DESCRIPTION:

REF: SIMILAR REPORTS: 08014 & A08399, A00619, A00687, A00939, A09260, A09288
BLIGHT CHAR CONDITION ON PRIMARY O-RING SEAL IN FWD FIELD JOINT ON SRM A57 OF STS-11 FLT, MISSION 41B

INVESTIGATION / RESOLUTION:

REMEDIAL ACTION - NONE REQ'D; PROB OCCURRED DURING FLT. THE PRIMARY O-RING SEAL IN THE FWD FIELD JOINT EXHIBITED A CHARRED AREA APPROX 1 IN. LONG, 0.030-0.080 IN. DEEP, & 0.100 IN. WIDE. THIS WAS DISCOVERED DURING POST-FLT SEGMENT DISASSY AT KSC.

3/8/84 - POSSIBLY EXIST FOR SOME O-RING EROSION ON FUTURE FLTS. ANALY INDICATES MAX. EROSION POSSIBLE IS 0.090 IN. ACCORDING TO THE FLT READINESS REVIEW FINDINGS FOR STS-13. LABORATORY TEST SHOWS SEALING INTEGRITY AT 3000 PSI USING AN O-RING WITH A SIMULATED EROSION DEPTH OF 0.095 IN., THEREFORE, THIS IS NOT A CONSTRAINT TO FUTURE LAUNCHES. ADDRESSED PRB ON 3-8-84.

3/27/84 UPDATED FROM 5 DAY REPORT - THE FAILURE DEFINED ABOVE WAS A SECONDARY PROB. THE VACUUM PUTTY LAY-UP IS CONSIDERED THE PRIME PROB. THE EFFECT ON THE O-RING SEAL BEING CHARRED HAS BEEN REVISED TO APPROX 3 IN. IN LENGTH, 0.040 IN. DEEP & AFFECTED 817 DEGS OF THE O-RING CROSS-SECTION PERIMETER. THE O-RINGS IN THE SRM SEGMENT ASSY JOINTS ARE DESIGNED AS PRESS. SEALS & ARE NOT INTENDED TO BE EXPOSED TO HOT GASES. STW4-2847 VACUUM SEAL PUTTY IS LAID UP IN THE JOINT AREA DURING MOTOR SEGMENT MATING TO PROVIDE A HEAT/HOT GAS BARRIER TO PROTECT THE O-RINGS. THE PRIME PROB IS THE LAY UP OF THE VACUUM PUTTY ALLOWING A HOT GAS PATH TO THE O-RING SEAL. THE O-RING IS USED IN THE FWD FIELD JOINT WHICH JOINED THE FWD MOTOR SEGMENT (P/N 1U52816-04, S/N 0000001) & FWD CENTER MOTOR SEGMENT (P/N 1U52817-03, S/N 0000001).

4/17/84 UPDATED FROM 4-12-84 PRB & MARCH MONTHLY SUMMARY REPORT - TEST FIXTURE & TEST PLANS HAVE BEEN PREPARED & TEST STARTED TO SIMULATE THE EFFECTS OF THE HOT GAS ON THE O-RING. THESE TESTS ARE EXPECTED TO CONFIRM THAT HOT GAS WILL NOT ERODE THE O-RING TO A POINT WHERE SEAL INTEGRITY WOULD FAIL. THE PUTTY LAY-UP IS STILL TO BE STUDIED.

ASSIGNEE	R. MCINTOSH	O. NEWMAN	L. WEAR	J. FLETCHER	PAO REVIEW COM
	DESIGN	S & Q.A.	P. PCT	PAC	UP
APPROVAL				MSFC CLOSURE DATE	STATUS OPEN-R

[Ref. 5 2-3 1 of 6]

MSFC PROBLEM ASSESSMENT SYSTEM
(continuation)

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MSFC RECORD NUMB
A07934
CONTRACTOR REPO
DR4-5/30

5/1/84 UPDATED FROM 31 DAY REPORT - THE FORWARD FIELD JOINT DESIGN IS BEING EVALUATED. IT HAS BEEN DETERMINED THAT THE FREE VOLUME, THAT PORTION OF THE JOINT FWD OF THE PRIMARY O-RING NOT FILLED WITH EITHER VACUUM PUTTY OR GREASE, IS WHERE THE PROB LIES. IT IS BELIEVED THAT THE O-RING CHAR IS CAUSED BY A SHORT TIME DURATION IMPINGEMENT OF A HIGH ENERGY JET WHICH IS INDUCED DURING IGNITION PRESSURIZATION BY A COMBINATION OF VOIDS IN THE PROTECTIVE VACUUM PUTTY & THE FILLING OF AVAILABLE FREE VOLUMES CREATED BY THE TOL. OF MATING PARTS & THE O-RING SLOT. THE TEST BEING CONDUCTED SHOULD VERIFY THAT THIS CONDITION WILL NOT CAUSE A SERIOUS PROB.

5/23/84 UPDATED FROM 5-10-84 PRB - INVESTIGATIONS ARE CONTINUING. O-RING DAMAGE IS NOT TO THE POINT WHERE SEAL INTEGRITY WOULD FAIL (LESS THAN 0.090 IN. POSSIBLE). O-RING CHAR HAS ALSO BEEN FOUND ON STS-13.

6/30/84 UPDATED FROM MAY MONTHLY SUMMARY REPORT - TWR-14336 "PERFORMANCE CHARACTERISTICS OF THE SRM O-RING ASSY TEST PLAN" HAS BEEN RELEASED & THE 5 IN. CP HOT TESTING INITIATED. DATA FROM THE TESTS IS BEING ANALYZED & WILL BE IN NEXT MONTH'S REPORT.

6/20/84 UPDATED FROM 6-14-84 PRB & JUNE MONTHLY SUMMARY REPORT - PRELIMINARY FINDINGS FROM THE O-RING TEST CONFIRM THE HOT JET IMPINGEMENT SCENARIO & THE MAJOR CONCLUSIONS DRAWN ARE:

1. O-RING EROSION IS A FUNCTION OF ORIFICE SIZE & FREE VOLUME.
2. GREASE ON THE O-RINGS MODERATELY REDUCES EROSION
3. NO DAMAGE TO THE O-RING RESULTS WHEN THE ORIFICE IS ABSENT, SIMULATING THE ABSENCE OF VACUUM PUTTY.

A METHOD OF SOLUTION HAS NOT BEEN DETERMINED. THIOKOL WILL HAVE RECOMMENDED CHANGES IN JULY 1984.

A07934, A08014 & A08299 DOCUMENT DIFFERENT OCCURRENCES OF THE SAME GENERAL PROB. ALL ANALYSES DONE & CONCLUSIONS MADE WILL APPLY TO ALL 3 OCCURRENCE & WILL BE REPORTED UNDER A07934 HENCEFORTH.

7/11/84 ADDRESSED AT PRB ON 7-12-84 - TWO SPECS WERE RELEASED CHANGING THE LAYUP OF THE VACUUM PUTTY & GREASE APPLICATION. THESE CHANGES WERE TESTED SATISFACTORILY, HOWEVER ADDITIONAL TEST ARE REQ'D.

9/24/84 UPDATED FROM AUG. MONTHLY SUMMARY REPORT & 9-13-84 PRB - VACUUM SEAL PUTTY LAYUP SPECS STW7-3193 & STW7-3SRS WITH NEW LAYUP PROCEDURES HAVE BEEN RELEASED. STW7-3194 IS STILL IN THE SIGN OFF PROCESS. INVESTIGATIONS ARE CONT'D. PLANS ARE TO PERFORM FULL SCALE NOZZLE & JOINT TESTS. ALSO LOOKING AT NEW PUTTY. THIS PROBLEM OCCURRED AGAIN ON FLT 41D, SEE PROBLEM REPORT A00619

10/2/84 UPDATED FROM SEPT. MONTHLY SUMMARY REPORT - VACUUM SEAL PUTTY LAYUP SPEC STW7-3253 HAS BEEN REVISED TO REDUCE THE AMOUNT OF GREASE & STW7-3194 IS STILL IN SIGN OFF.

11/7/84 UPDATED FROM OCT. MONTHLY SUMMARY REPORT - THE JOINT FILLER MTRL TEST PLAN IS RELEASED & FULL SCALE JOINT ASSY TESTING WAS INITIATED 10-10-84 AT MORTON THIOKOL'S H-7 CLEARFIELD FACILITY.

11/14/84 UPDATED FROM PRB MTG 11-8-84 - THE TEST PLAN SHOULD BE REVISED & TEST STARTED WITHIN APPROX. 2 WKS.

1/2/85 UPDATED FROM PRB MTG 12-13-84 - A NEW PROGRAM WAS REQUESTED BY MSFC FOR EVALUATION & TESTING. THIS IS NOW IN WORK & THE ESTIMATED COMPLETION DATE IS 1-4-85. ANY DESIGN CHANGES ARE PENDING THE

[Ref. 5 2-3 2 of 6]

TEST RESULTS. A COPY OF THE 40-LB & 5 IN. CP MOTOR TEST SCHEDULE IS INCLUDED IN THIS PAGE. THE O-RING CHAR PROBLEM IS A REVIEW ITEM FOR EACH FLT READINESS REVIEW (FRR).

1/22/85 UPDATED FROM PRB MTG ON 1-10-85 - ALL SCALE MODEL TESTING WILL BE COMPLETED IN JAN. FULL SCALE TESTS ARE TO FOLLOW.

2/15/85 UPDATED FROM 2-7-85 PRB & JAN. MONTHLY SUMMARY REPORT - TESTING OF VACUUM SEAL PUTTY LAYUP CONFIG. USING 5 IN. CP MOTOR IS CONTINUING. TORCH TESTS HAVE BEEN DONE ON ASBESTOS FILLED PUTTY MFR'D BY RANDOLPH & INMONT & A NONASBESTOS PUTTY MFR'D BY INMONT. BOTH ASBESTOS PUTTIES PERFORMED COMPARABLY. THE HEAT TRANSFER RATE OF THE NONASBESTOS PUTTY WAS MORE THAN TWICE THAT OF THE ASBESTOS FILLED PUT. FORTY LB. CHAR TEST MOTORS ARE SCHEDULED TO FURTHER TEST THE PUTTY TYPES.

EROSION WAS AGAIN EXPERIENCED ON FLT 51C. THE O-RING BURNS WERE AS BAD OR WORSE THAN PREVIOUSLY EXPERIENCED. SEE PROBLEM REPORT A09939/CONT. NO. 4-5/41. DESIGN CHANGES ARE PENDING TEST RESULTS. CHANGES BEING CONSIDERED ARE: MODIFYING THE O-RINGS, DOUBLE BARRIER VENTING & ADDING GREASE AROUND O-RINGS TO FILL THE VOID LEFT BY THE PUTTY.

3/18/85 UPDATED FROM 3-7-85 PRB - TESTING & EVALUATION PROGRAM IS CONTINUING. DESIGN CHANGES ARE PENDING TEST RESULTS.

3/29/85 UPDATED FROM FEB. MONTHLY SUMMARY REPORT - DISASSY OF THE SRM 15A & 15B NOZZLE TO AFT CASE JOINTS REVEALED GAS PATHS THRU THE PUTTY AT 109 DEG & 64 DEG RESPECTIVELY. BOTH PRIMARY O-RINGS HAD HEAT AFFECTED AREAS (NO EROSION) AT THE GAS PATH LOCATIONS. SOOT WAS FOUND BETWEEN THE PRIMARY & SECONDARY O-RINGS IN EACH JOINT BUT THE SECONDARY O-RINGS WERE UNTOUCHED. THE SOOT WAS MOST LIKELY DEPOSITED DURING IGNITION PRESSURIZATION WHEN GAS PASSED THE PRIMARY O-RINGS BEFORE THEY SEATED.

A DR4-5 PROBLEM REPORT WAS NOT WRITTEN FOR THIS OCCURRENCE FOR THE FOLLOWING REASONS:

1) THE PRIMARY O-RINGS SEATED & THE PRESSURE INTEGRITY OF THE JOINTS WAS MAINTAINED
2) THE O-RINGS WERE NOT ERODED
3) GAS PATHS & O-RING EROSION HAVE OCCURRED ON PREVIOUS FLTS & ARE EXPECTED TO OCCUR RANDOMLY ON FUTURE FLTS.

IT HAS BEEN REPORTED HERE TO DOCUMENT THE OCCURRENCE.

4/17/85 UPDATED FROM PRB MEETING 4-11-85 - O-RING INVESTIGATION & PUTTY TESTING IS CONTINUING. MSFC'S FINDING THAT THE PUTTY DISSOLVED IN GREASE COULD NOT BE DUPLICATED BY THIOKOL.

4/24/85 UPDATED FROM APRIL MONTHLY SUMMARY REPORT - AT NASA REQUEST, A SOLUTION FOR O-RING EROSION WILL NOT INVOLVE A RADICAL DESIGN CHANGE. THEREFORE, THE POSSIBLE SOLUTIONS UNDER CURRENT INVESTIGATION ARE LIMITED TO:

1. NEW O-RING MTRL AND/OR DIAMETER
2. NEW VACUUM PUTTY AND/OR LAYUP PROCEDURE.

6/6/85 UPDATED FROM MAY MONTHLY SUMMARY REPORT - DISASSY OF SRM 17A & 17B REVEALED GAS PATHS & EROSION IN BOTH NOZZLE TO CASE JOINTS. THE GAS PATH IN THE NOZZLE TO AFT CASE JOINT OF SRM 17A WAS CENTERED AT 14.4 DEGS & THE O-RING HAD A .0?? IN. OF EROSION. SRM 17B'S NOZZLE JOINT HAD A GAS PATH AT 116 DEGS & THE O-RING HAD A .068 IN. OF EROSION. THERE WAS SOOT DEPOSITED IN THE PRIMARY

O-RING GROOVES OF EACH JOINT, HOWEVER, NO SOOT PASSED THE PRIMARY O-RING.

6/28/85 UPDATED FROM 6-13-85 PRB - PLAN TO SHAVE O-RING PAST THE .095 IN. TO DETERMINE WHEN A LEAK WILL OCCUR. HOT FIRE JOINT TESTS ARE PLANNED FOR AUG., 1985.

7/16/85 UPDATED FROM JUNE MONTHLY SUMMARY REPORT DATED 6-17-85 - ONE POSSIBLE EXPLANATION FOR THE GREATER EROSION DEPTHS OBSERVED ON SRB 17B (68 MILS) & MNV DM-7 (90 MILS) RELATES TO AN ASPECT OF NOZZLE JOINT ASSEMBLY: AN ECCENTRIC NOZZLE JOINT CAN OCCUR MORE EASILY THAN AN ECCENTRIC FIELD JOINT DUE TO A) ITS HORIZONTAL VS VERTICAL POSITION DURING ASSEMBLY, B) SMALL JOINT CLEARANCES, AND C) THE ABSENCE OF SHIMS. CONSEQUENTLY, IT IS POSTULATED THAT A REDUCED JOINT GAP (& THEREFORE GAS JET ORIFICE) OCCURRED AT THE CIRCUMFERENTIAL LOCATION OF THE BLOWHOLE, THEREBY CAUSING ENHANCED EROSION. THE VOLUME FILLING MODEL PREDICTS THAT AN ECCENTRICITY OF ONLY 7 MILS WILL ROUGHLY DOUBLE THE 40 MILS EROSION PREDICTED FOR CONCENTRIC NOZZLE JOINTS. THUS AN O-RING EROSION EXCEEDING 80 MILS CAN BE CAUSED BY JOINT ECCENTRICITY IF THE REDUCED GAP ALIGNS WITH THE BLOWHOLE. INVESTIGATION OF THIS SCENARIO IS CONTINUING.

7/23/85 UPDATED FROM 7-11-85 PRB - THIS PROBLEM WAS SCHEDULED FOR STATUSING, BUT NOT DISCUSSED SINCE NO SIGNIFICANT PROGRESS HAS BEEN MADE SINCE THE LAST UPDATE.

8/19/85 UPDATED FROM 8-8-85 PRB & JULY 1985 MONTHLY SUMMARY REPORT - THE ANALYTICAL MODEL OF O-RING EROSION HAS BEEN USED TO PREDICT A WORST CASE EROSION OF 75 MILS ON THE SECONDARY O-RING SHOULD THE PRIMARY O-RING FAIL TO SEAL. PRESSURE TESTS OF O-RINGS WITH MTRL REMOVED TO SIMULATE EROSION & SUBSCALE MOTOR FIRINGS HAVE DEMONSTRATED THAT THIS IS WELL BELOW THE SEAL CAPABILITY OF ERODED O-RINGS. EVALUATION OF THE RESULTS OF STS-51F ARE CONTINUING.

9/3/85 UPDATED FROM JULY MONTHLY SUMMARY REPORT DATED 8-15-85 - DR4-5/48 (A09260) REPORTED .098 INCH OF EROSION OBSERVED IN THE NOZZLE TO CASE JOINT ON DEVELOPMENT MOTOR NO. 7.

DR4-5/49 (A09280) REPORTED .171 IN. OF EROSION ON PRIMARY O-RING & .032 IN. OF EROSION ON THE SECONDARY O-RING IN THE NOZZLE TO CASE JOINT OF SRM 16A.

THE INVESTIGATIONS ARE CONTINUING & WILL BE REPORTED HERE AS THEY ARE THE SAME GENERIC PROBLEM.

INSPECTION OF SRM 19A FIELD JOINTS REVEALED NO EROSION HOWEVER, THE NOZZLE TO CASE JOINT HAD PRIMARY O-RING EROSION AT 19?.8 DEGS WITH A MAX. DEPTH OF .013 IN. SRM 19B HAD NO FIELD JOINT EROSION. THE NOZZLE TO CASE JOINT EXPERIENCED GAS PATHS AT 63, 93.6, 270 & 342 DEGS. THE PRIMARY O-RING WAS ERODED ONLY AT 270 & 342 DEGS WITH DEPTHS OF .005 & .032 IN., RESPECTIVELY. A PROBLEM REPORT WAS NOT WRITTEN FOR THESE OCCURRENCES SINCE THEY WERE WITHIN HISTORICAL LEVELS. THEY ARE REPORTED HERE FOR INFORMATION.

10/2/85 UPDATED FROM AUGUST MONTHLY SUMMARY REPORT DATED 9-16-85 - SRM 19B (STS-51F) EXPERIENCED A GAS PATH AT 81.6 DEGS. AT THIS LOCATION THE O-RING WAS ERODED APPROX. 0.001 IN., WELL WITHIN THE EXPERIENCE BASE. DM-8 WILL BE DELAYED TO ALLOW FOR DEVELOPMENT OF O-RING SOLUTION.

11/4/85 UPDATED FROM OCTOBER MONTHLY SUMMARY REPORT & 10-17-85 PRB - ANALYSIS OF DATA FROM STS-51J & DM-6 TEST ARE IN WORK.

11/19/85 UPDATED FROM 11-7-85 PRB MEETING. STS-61A EXPERIENCED NO FIELD JOINT EROSION. HOWEVER, SOOT BLOWBY WAS FOUND ON 3 OF THE 6 PRIMARY O-RINGS. STS-61A RH NOZZLE EXPERIENCED EROSION OF THE PRIMARY O-RING. THE EROSION WAS WELL WITHIN THE CURRENT DATA BASE.

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BASED ON THE JOINT PUTTY & O-RING PROBLEM, A PROGRAM WAS INITIATED TO ESTABLISH ACCEPTABLE MATERIALS & CONFIGURATION THAT COULD BE TESTED ON DM-8 & ULTIMATELY INCORPORATED IN FLIGHT MOTORS

12/17/85 - UPDATE FROM NOVEMBER MONTHLY SUMMARY REPORT
THE O-RING TASK FORCE HAS STARTED ASSEMBLY AND COLD FLOW TESTING OF NOZZLE PUTTY AND JOINT FILLERS TESTING IS CONTINUING

SRM 21 - NO DAMAGE
SRM 22 - RIGHT HAND (B) NOZZLE

THERE WAS EROSION AT 97.2 DEGREE. IT WAS .075 INCH DEEP AND .. DEGREE OF CROSS-SECTIONAL PARIMETER AFFECTED WITH 13 INCHES OF HEAT DAMAGE LENGTH AND .. INCHES OF HEAT AFFECTED LENGTH. THIS IS NOT THE WORST CASE TO DATE. THERE WAS NO OTHER REPORTED O-RING DAMAGE

12/20/85 UPDATED FROM 12-12-85 PRB MEETING - THIOKOL IS CONTINUING STUDIES RELATIVE TO THE O-RING EROSION AND PUTTY LAYUP ON AN ON-GOING BASIS. A TOTAL SOLUTION TO THIS PROBLEM IS LONG RANGE. HOWEVER, AN ACCEPTABLE DATA BASE HAS BEEN ESTABLISHED AND SUFFICIENT ENHANCEMENT HAS BEEN MADE TO PROVIDE THE NECESSARY CONTROLS TO PREVENT RECURRENCE OF THE PROBLEM OUTSIDE THE ESTABLISHED DATA BASE. CLOSURE PAPER HAS BEEN PREPARED AND IS BEING EVALUATED

1/20/86 UPDATED FROM DECEMBER MONTHLY SUMMARY REPORT. THE TASK FORCE HAS MADE ONE HOT GAS TEST. THE PRELIMINARY RESULTS INDICATE THAT THE TEST CHAMBER NEEDS TO BE REDESIGNED.

SPACE SHUTTLE SRM FLIGHT SET 23 WAS LAUNCHED NOVEMBER 27. INSPECTION OF THE FIELD JOINTS REVEALED NO DAMAGE, WHILE BOTH OF THE NOZZLE-TO-CASE JOINTS HAD EROSION

	23A	23B
EROSION LENGTH (INCHES)	3.0	7.0
HEAT AFFECTED LENGTH (INCHES)	22.3	25.0
EROSION DEPTH (INCHES)	.017	.037
DEGREE LOCATION	346 DEGREES	0 DEG

1/23/86 RESOLUTION - SRM FIELD AND NOZZLE JOINTS HAVE EXPERIENCED EROSION OF THE PRIMARY O-RINGS ON SEVERAL MISSIONS AND STATIC TEST AS DETERMINED BY POST FLIGHT INSPECTION. THE PRIMARY O-RING IN A NUMBER OF INSTANCES WAS SUFFICIENTLY DAMAGED AND/OR DISPLACED TO ALLOW BLOWBY, THEREFORE EXPOSING THE SECONDARY O-RING TO HOT COMBUSTION GASES. SOME EROSION WAS EXPERIENCED ON THE SECONDARY O-RING AT THE NOZZLE JOINT BUT NOT TO THE POINT WHERE THE O-RING SEALING CAPABILITY WAS JEOPARDIZED.
THE CAUSE OF THE O-RING EROSION HAS BEEN ATTRIBUTED TO HOT GAS PATHS THROUGH THE VACUUM PUTTY ALLOWING THE HOT GASES TO IMPINGE UPON THE O-RING. IMPROPER SEATING OF THE PRIMARY O-RING ALLOWED BLOWBY AND MINOR HOT GAS IMPINGEMENT ON THE SECONDARY O-RING. THE MAGNITUDE OF THE O-RING EROSION IS A FUNCTION OF ORIFICE SIZE AND FREE VOLUME (AREA BETWEEN THE PRIMARY O-RING AND THE PUTTY).

PRIMARY O-RING EROSION IS EXPECTED TO CONTINUE SINCE NO CORRECTIVE ACTION HAS BEEN ESTABLISHED THAT WILL PREVENT HOT GASES FROM REACHING THE PRIMARY O-RING CAVITY. STEPS HAVE BEEN TAKEN TO ASSURE THAT THE SECONDARY O-RING WILL BE SEATED AND ANALYTICAL ANALYSIS HAVE INDICATED THAT UNDER A WORST CASE SITUATION, EROSION OF THE SECONDARY O-RING WILL NOT BE SEVERE ENOUGH TO ALLOW A LEAK PATH PAST THE SECONDARY O-RING. STW7-2747 WAS CHANGED EFFECTING AN INCREASE IN THE LEAK CHECK PRESSURE FROM

[Ref. 5 2-3 5 of 6]

100 PSIG TO 200 PSIG. WITH THE 100 PSIG LEAK TEST THE SECONDARY O-RING WOULD BE SEATED BUT ANY GAS PATHS THROUGH THE PUTTY WOULD BE UNDETECTABLE. BY INCREASING THE LEAK TEST PRESSURE TO 200 PSIG, ANY LEAK PATHS THROUGH THE PUTTY WOULD BE DETECTABLE. THE 200 PSIG LEAK TEST WAS IMPLEMENTED ON SRM-15 AND SUBSEQUENT MOTORS.

THE STACKING PROCEDURES HAVE BEEN CLARIFIED TO ASSURE MOTOR-TO-MOTOR CONSISTENCY IN JOINT MATING. THE OPERATIONAL MAINTENANCE DOCUMENT CHANGES WERE EFFECTIVE ON SRM 1024 AND 9038.

ANALYTICAL STUDIES BASED ON BOTH IMPINGEMENT EROSION AND BLOWBY EROSION SHOW THAT THIS PHENOMENON HAS AN ACCEPTABLE CEILING SINCE IMPLEMENTING THE ABOVE CHANGES. RECENT EXPERIENCE HAS BEEN WITHIN THE PROGRAM DATA BASE.

THIS SEAL IMPROVEMENT PROGRAM PLAN WILL CONTINUE UNTIL THE PROBLEM HAS BEEN ISOLATED AND DAMAGE ELIMINATED TO THE SRM SEALS. STATUS WILL CONTINUE TO BE PROVIDED IN THE FLIGHT READINESS REVIEWS AND IN FORMAL TECHNICAL REVIEWS AT MTI AND MSFC. AT THE CONCLUSION OF THE PROGRAM, A COMPREHENSIVE REPORT WILL BE WRITTEN TO CONSOLIDATE THE RESULTS, CONCLUSIONS, AND RECOMMENDATIONS.

THIS PROBLEM IS CONSIDERED CLOSED BASED ON MTI REPORT TWR-14359, REV. A, "IMPROVEMENTS OF SPACE SHUTTLE SRM MOTOR SEALS" DATED 8-30-85 AND MTI LETTER E150/SGR-86-114 "RATIONALE FOR CLOSURE OF THE O-RING EROSION PROBLEM", A07934, DR4-5/30, DATED 1-2-86.

[Ref. 5 2-3 6 of 6]

MORTON THIOKOL INC.
Wasatch Division

10 December 1985
E100-86-26

Mr. L. O. Wear, SA42
National Aeronautics and Space Administration
George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama 358'2

Dear Mr. Wear:

Subject: Closure of Critical Problems Number DR4-5/30, DR4-5/31, DR4-5/35, DR4-5/38, DR4-5/39, DR4-5/41, DR4-5/48, and DR4-5/49 (MSFC Tracking Numbers A07934, A08014, A08299, A08615, A08687, A08939, A09260, and A09288) "Nozzle/Segment Joint Primary O-ring Charred"

The subject Critical Problems are ongoing problems which will not be fully resolved for some time. There is a formal reporting system being used to evaluate the problem resolution progress and to continue to report them as part of the MSFC Problem Assessment System is not necessary.

Therefore, we request the subject critical problems be closed and removed from the next PRB agenda/list

Enclosed is a copy of memorandum E150/BGR-86-95, TWR-15349, and TWR-14359

Very truly yours,

[illegible] McDonald, Director
SRM Project

cc: J. Kilminster
J. Elwell
D. Thompson
I. Adams, MTI/MSFC
J. Thomas, Jr., SA42
E. Skrobiszewski, SA49
J. Fletcher, 041-SRO Rockwell International (MSFC)
R. McIntosh, EE11
B. Holland, EC02
S. Rodgers
B. Russell
B. Ebeling
H. Cit[illegible]

[Ref. 5 2-4]

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UNITED TECHNOLOGIES
UNITED SPACE BOOSTERS

INTERNAL CORRESPONDENCE

To: Larry Mulloy

Fm: George Morefield

SJ: Zinc Chromate Putty in SRM Joints

Dt: March 9, 1984

No: GSM-042-84

Following is an elaboration of my impromptu remarks in yesterday's FRR concerning burned primary pressure vessel "O"-rings.

I alluded to the Titan III SRM history which is quite similar to the current STS SRM experience. Post-fire inspection of Titan SRM static test motors showed that pressurization of the single "O"-rings in the pressure vessel routinely occurred via a single break-down path across the joint putty. There was also evidence that some "O"-rings never see pressure in the Titan motor. The segment-to-segment case insulation design results in a compression butt joint which apparently is often sufficient to withstand P_c.

It should be pointed out that single point pressurization of a Titan "O"-ring annulus is a less severe event than on an STS SRM because, being a smaller diameter motor, the Titan "O"-ring plenum has less volume and comes to pressure equilibrium faster (less time to melt the "O"-ring).

The use of "lucky putty" has always been surrounded by controversy. Its use has become a given, although no one really claims it to be part of either the insulation system or the sealing system. In fact there is evidence that it's use can cause problems other than forcing single-point pressurization. On the few occasions when Titan motors were destacked it was found that the high hydraulic forces associated with joint mating actually caused case insulation to peel away from the case. This is of course aggravated by the pressure of the hydraulic medium, putty, which flows into the separation as well as the "O"-ring plenum.

Your review showed that there was sufficient margin of "O"-ring remaining to do the job. I'm sure you have considered that if it does burn through, the secondary "O"-ring will then be similarly pressurized through a single port. So, some concern remains.

[Ref. 5 2-5]

SRB CRITICAL ITEMS LIST

Subsystem: SOLID ROCKET BOOSTER | Criticality Category: 1 | Reaction Time: [illegible]

Item Code: 10-01-01 | Page: A-64

Item Name: *Case, P/N (See Retention Rationale); Joint Assy, Factory P/N 1U50147 Field: 1U50747

No. Required: [illegible] segments, 3 field joints, 7 offset joints

FMEA Page No. A-4 of TWR-[illegible]-726

Critical Phase: Boost

Date: December 17, 1982

Analyst: Gerber

Approved: [signature]

Failure Mode & Causes: Leakage at case assembly joints due to redundant O-ring seal failures or primary seal and leak check port O-ring failure.

NOTE: Leakage of the primary O-ring seal is classified as a single failure point due to possibility of loss of sealing at the secondary O-ring because of joint rotation after motor pressurization.

Failure Effect Summary: Actual Loss - Loss of mission, vehicle, and crew due to metal erosion, burnthrough, and probable case burst resulting in fire and deflagration.

RATIONALE FOR RETENTION

Case P/N 1U50129, 1U50131, 1U50130, 1U50135, 1U50147, 1U50715, 1U50716, 1U50717

A. DESIGN

The SRM case joint design is common in the lightweight and regular weight cases having identical dimensions. The joint concept is basically the same as the single O-ring joint successfully employed on the Titan III solid rocket motor. The SRM joint uses centering slips which are installed in the gap between the tang I.D. and the outside clevis leg to compensate for the loss of concentricity due to gathering and to reduce the total clevis gap which has been provided for ease of assembly. On the Titan's SRM, the secondary O-ring was designed to provide redundancy and to permit a leak check, ensuring proper installation of the O-rings. Full redundancy exists at the moment of initial pressurization. However, test data shows that a phenomenon called joint rotation occurs as the pressure rises, opening up the O-ring extrusion gap and permitting the energized O-ring to protrude into the gap. This condition has been shown by test to be well within that required for safe primary O-ring sealing. This gap may, however, in some cases, increase sufficiently to cause the unenergized secondary O-ring to lose compression, raising question as to its ability to energize and seal if called upon to do so by primary seal failure. Since, under this latter condition only the single O-ring is sealing, a rationale for retention is provided for the simplex mode where only one O-ring is acting.

The surface finish requirement for the O-ring grooves is 63 and the finish of the O-ring contacting portion of the tang, which slides across the O-ring during joint assembly, is 32. The joint design provides an [illegible] for the O-ring installation, which facilitates retention during joint assembly. The tang has a large shallow angle chamfer on the tip to prevent the cutting of the O-ring at assembly. The design drawing specifies application of O-ring lubricant prior to the installation. The factory assembled joints have NBR rubber material vulcanized across the internal joint faying surfaces as a part of the case internal insulation subsystem.

A small MS port leading to the annular cavity between the redundant seals permits a leak check of the seals immediately after joining segments. The MS plug, installed after leak test, has a retaining groove and compression face for its O-ring seal. A means to test the seal of the installed MS plug has not been established.

The O-rings for the case joints are mold formed and ground to close tolerance and the O-rings for the test port are mold formed to net dimensions. Both O-rings are made for high temperature, low compression set fluorocarbon elastomer. The design permits five scarf joints for the case joint seal rings. The O-ring joint strength must equal or exceed 40% of the parent material strength.

B. TESTING

To date, eight static firings and five flights have resulted in 150 (54 field and 126 factory) joints tested with no evidence of leakage. The Titan III program using a similar joint concept has tested a total of 1076 joints successfully.

[Ref. 5-2-6 1 of 2]

SRB CRITICAL ITEMS LIST

Subsystem: SOLID ROCKET BOOSTER | Criticality Category: [illegible] | Reaction Time: [illegible]

Item Code: 10-01-01 | Page: A-65

Item Name: *Case, P/N (See Retention Rationale); Joint Assy, Factory P/N 1U50147 Field: 1U50717

RATIONALE FOR RETENTION (CONT'D)

A laboratory test program demonstrated the ability of the O-ring to operate successfully when extruded into gaps well over those encountered in this O-ring application. Uniform gaps of 1/8-inch and over [illegible] successfully withstood pressures of 1600 psi. The Hydroburst Program (TWR-11664) and the Structural Test Program (STA-1) for the standard weight case (TWR-12051) and the Lightweight Case Joint Certification Test [illegible] have shown that the O-rings can withstand a minimum of four pressurizations before damage to the [illegible] permits any leakage.

Further demonstration of the capability of joint sealing is found in the hydro-proof testing of new and refurbished case [illegible]. Over 540 joints have been exposed to liquid pressurizations at levels exceeding motor MEOP [illegible] no leakage experienced past the primary O-ring. The only occasions where leakage was experienced was during refurbishment of STS-1 where two stiffener segments were severely damaged during cavity collapse at water impact.

A more detailed description of SRM joint testing history is contained in TWR-13520, Revision A.

C. INSPECTION

The tang -A- diameter and clevis -C- diameter are measured and recorded. The depth, width and surface finish of the O-rings grooves are verified. The surface finish of the tang is also verified. Characteristics are inspected on each O-ring to assure conformance to the standards to include:

- Surface conditions
- Mold flashing
- Scarf joint mismatch or separation
- Cross section
- Circumference
- Durometer

Each assembled joint seal is tested per STW7-2747 via pressurizing the annular cavity between seals to 50 ± 5 psi and monitoring for 10 minutes. A pressure decay of 1 psig or greater is not acceptable. Following seal verification by QC, the leak test port plug is installed with QC verifying installation and torquing.

D. FAILURE HISTORY

No failures have been experienced in the static firing of three qualification motors, five development motors and ten flight motors.

[Ref. 5-2-6 2 of 2]

MORTON THIOKOL INC

Wasatch Division

3 January 1986
E060/DT-FY86-445

Mr. Jack Fletcher, 041-SRC
Rockwell International
Marshall Space Flight Center, AL 35812

Dear Mr. Fletcher:

Subject: Closure of Critical Problems Number DR4-5/30, DR4-5/31, DR4-5/35, DR4-5/38, DR4-5/39, DR4-5/41, DR4-5/48, and DR4-5/49, "Nozzle Segment Joint Primary O-Ring Charred."

Reference: Letter E150-86-25 A. J. McDonald to L. O. Wear, Same Subject, dated 30 December 1985.

Enclosed is the additional information requested by you to close the subject problems. Included is memorandum E150/RGR-86-114 addressing action previously taken. Also attached are specifications STW7-2739, "Putty, Vacuum Seal, Field Joint Assy, Application of" and STW7-2747, "Leak Testing, SRM Case Seals, Space Shuttle Project Solid Rocket Motor" and the current OMI TWA-1130 Rev K.

This information along with that provided in the reference letter should permit closure of these problems.

Very truly yours,

D. Thompson

D. Thompson, Manager
Systems Engineering

DT/jh

cc: L. O. Wear, SA42, w/o enc.
J. Kilminster, w/o enc.
A. McDonald, w/o enc.
J. Elwell, w/o enc.
J. Thomas, Jr., SA42
E. Skrobiszewski
I. Adams, MTI/MSFC
B. Russell
H. Gittins

[Ref. 5.2-7 1 of 2]

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Page 2
GSM-040-84

I recommend that you set up a panel to study the use of putty and consider some alternatives:

1) Is putty needed at all?

2) If the tradition can't be broken, can the putty be applied with multiple (6 or 8) pressurization paths built in?

I think that the primary seal should be allowed to work in its classical design mode. Both the Titan and STS SRM's have been designed for this not to happen. Titan has flown over a thousand pressure joints with no failure. My opinion is that the potential for failure of the joint is higher for the STS SRM, especially when occasionally the secondary seal may not be totally effective.

G. S. Morefield

G. S. Morefield
Chief Engineer

rp

[Ref. 5.2-7 2 of 2]

DEC 24 1985

SA42-481-85
Thomas/3-3234

Morton Thiokol, Inc., Wasatch Division
Attn: Mr. Joe Kilminster
Vice President, Space Booster Programs
P.O. Box 524
Brigham City, UT 84302

Subject: SRM Problem Review Board

During a recent review of the SRM Problem Review Board open problem list I found that we have 20 open problems, 11 opened during the past 6 months, 13 open over 6 months, 1 three years old, 2 two years old, and 1 closed during the past six months. As you can see our closure record is very poor. You are requested to initiate the required effort to assure more timely closures and the MTI personnel shall cordinate directly with the S&E personnel the contents of the closure reports.

L. O. Wear
Manager, SRM Office

cc:
SA41/Messrs. Mulloy/Adams
SA42/Messrs. Thomas/Miller/Brock/Christian/Denton
SA49/Mr. Skrobiszewski
EE11/Messrs. Smith/Jones/McIntosh
EG03/Mr. Newman
AP45/Mr. Woods
MTI-WAS/Mr. McDonald
RI-HSV/Mr. Fletcher
MTI-HSV/Mr. Adams

[Ref. 5 2-8]

REVISED 3-19-86

SRM SEAL EROSION PROBLEMS

FLT	STS	SRM#	JOINT	DATE OCCURRED	SEAL AFFECTED	MSFC TRACKING NUMBER	LAUNCH CONSTRAINT
-	2	2B	FIELD-AFT	11-12-81	PRIMARY	NOT REPORTED	NO
-	-	QM-4	NOZZLE-CASE	03-21-83	PRIMARY	A06658	NO
41-B	11	10A	FIELD-FWD	02-03-84	PRIMARY	A07934	NO
		10B	NOZZLE-CASE	02-03-84	PRIMARY	A08014	NO
41-C	13	11A	NOZZLE-CASE	04-06-84	PRIMARY	A08299	NO
41-D	14	13A	NOZZLE-CASE	08-30-84	PRIMARY	A08687	NO
		13B	FIELD-FWD TO FWD CTR	08-30-84	PRIMARY	A08615	NO
51-C	20	15A	NOZZLE-CASE	01-24-85	PRIMARY	A07934	NO
		15B	NOZZLE-CASE	01-24-85	PRIMARY	A07934	NO
		15A	FIELD-CTR	01-24-85	PRIMARY	A08939	NO
		15B	FIELD-FWD	01-24-85	PRIMARY	A08939	NO
51-D	23	17A	NOZZLE-CASE	04-12-85	PRIMARY	A07934	NO
		17B	NOZZLE-CASE	04-12-85	PRIMARY	A07934	NO
51-B	24	16A	NOZZLE-CASE	04-29-85	PRIMARY/SECONDARY	A09288*	YES
		16B	NOZZLE-CASE	04-29-85	PRIMARY	A09288	NO
-	-	DM-7	NOZZLE-CASE	05-09-85	PRIMARY	A09260	NO
51-G	25	18A	NOZZLE-CASE	06-17-85	PRIMARY	A07934	NO
		18B	NOZZLE-CASE	06-17-85	PRIMARY	A07934	NO
51-F	26	19B	NOZZLE-CASE	07-29-85	PRIMARY	A09288	NO
51-I	27	20A	NOZZLE-CASE	08-27-85	PRIMARY	A09288	NO
61-A	30	22B	NOZZLE-CASE	10-30-85	PRIMARY	A07934	NO
61-B	31	23A	NOZZLE-CASE	11-26-85	PRIMARY	A07934	NO
		23B	NOZZLE-CASE	11-26-85	PRIMARY	A07934	NO
61-C	32	24A	FIELD-AFT	01-12-86	PRIMARY	NOT REPORTED	NO
		24B	NOZZLE-CASE	01-12-86	PRIMARY	NOT REPORTED	NO

*THIS PROBLEM CONTAINED SECONDARY O-RING EROSION OF THE NOZZLE JOINT AND CONSTRAINS LAUNCH FOR ALL O-RING ANOMALIES.

[Ref. 5 2-9]

NASA Routing Slip

Mail Code		Name	Action
TO:	EE01	Mr. Hardy	Approval
			Call me
			Concurrence
THRU:	EE11	Mr. Coates	File
			Information
			Investigate and Advise
			Note and Forward
			Note and Return
			Per Request
			Per Phone Conversation
			Recommendation
			See me
			Signature
			Circulate and Destroy

SUBJECT: Burned O-Rings on STS-11

The recent experience of two burned O-rings (nozzle/case boss and forward/forward center joint) on STS-11 coupled with the "missing putty" finding at disassembly raise concern with STS-13.

Specifically concern is raised about the type II Randolph zinc chromate putty (ZCP) sensitivity to humidity and temperature. The thermal design of the SRM joints depends on thermal protection of the O-ring by the ZCP. ZCP failure to provide a thermal barrier can lead to burning both O-rings and subsequent catastrophic failure. Adhesion service-life and sensitivity to temperature and humidity of the type II ZCP must be reassessed and verified in the light of recent experience. The O-ring leak check procedure and its potential effect on the ZCP installation and possible displacement is also an urgent

[Ref. 5 2-10 1 of 2]

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		Recommendation	
		See me	
		Signature	
		Circulate and Destroy	

concern which requires expedition of previously identified fullscale tests. Effect of cavity volume size (cavity between the ZCP and primary O-ring) on O-ring damage severity must also be assessed.

Your support in this urgent matter is requested.

John Q. Miller
Chief, Solid Motor Branch

cc:
EH01/Mr. Schwinghamer
EH43/Mr. Hill
SA41/Mr. Mulloy
SA42/Mr. Wear
EP01/Mr. McCool
EP21/Mr. McCarty
EP25/Messrs. Powers/Ray

Name	Tel. No. (or Code) & Ext.
John Q. Miller	453-3702
Code (or other designation) EP25	Date 2/28/84

NASA FORM 26 JUN 75 PREVIOUS EDITIONS MAY BE USED

[Ref. 5 2-10 2 of 2]

TWR-14740 REV. D

STS-51E SOLID ROCKET MOTOR (SRM-16)
FLIGHT READINESS REVIEW

SRM SPECIAL TOPIC NO. 3

O-RING EROSION OBSERVED ON STS-51C (SRM-15)

12 FEBRUARY 1985

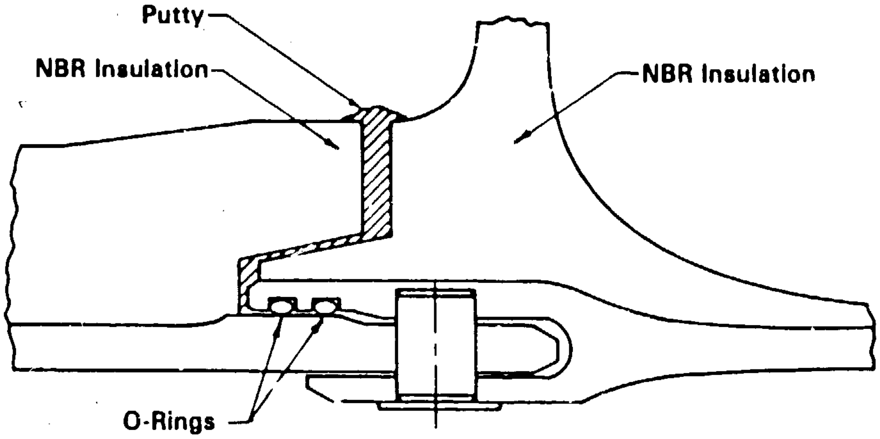
MORTON THIOKOL, INC.

WASATCH DIVISION
P.O. Box 524, Brigham City, Utah 84302 (801) 863-3511

Morton Thiokol, Inc.
Wasatch Division

[Ref. 5 2-11 1 of 18]

SRM-HPM Field Joint



MORTON THIOKOL INC
Wasatch Division

3-1

INFORMATION ON THIS PAGE WAS PREPARED TO SUPPORT AN ORAL PRESENTATION

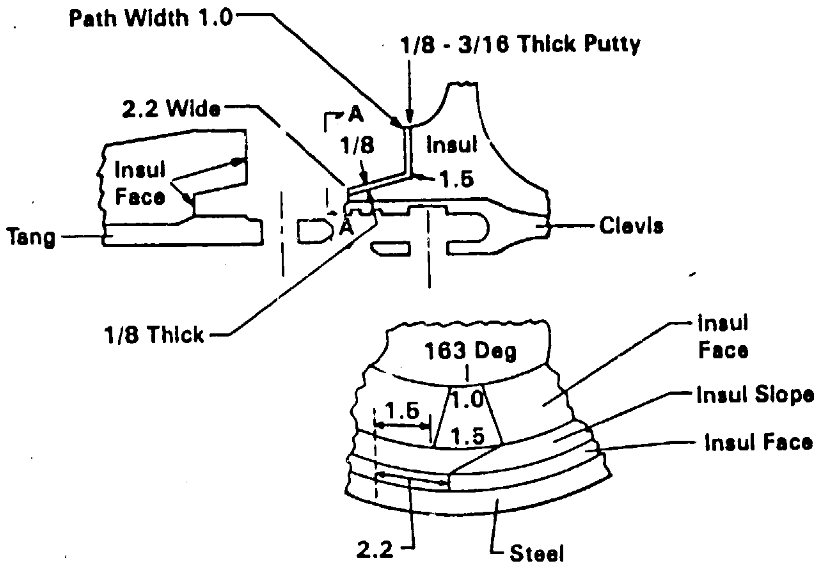
[Ref. 5 2-11 2 of 18]

STS-51C (SRM-15) POSTFLIGHT HARDWARE DAMAGE ASSESSMENT

- o A67 (LEFT FORWARD FIELD JOINT)
  - o GAS PATH THRU PUTTY AND PAST PRIMARY O-RING AT 163°
  - o HARD THIN CHAR ON TANG INSULATION AT 163°
  - o CHAR ON CLEVIS PUTTY STILL SOFT (T = 1/8 - 3/16 INCH) AT 163°
  - o SOOTED ARC ON TANG SEAL SURFACE AND INSUL. FACE 124° TO 188°
  - o BLACK GREASE (SOOTED) BETWEEN O-RINGS 150° TO 230°
  - o PRIMARY O-RING SOOTED AT INTERFACE WITH GROOVE ON UPSTREAM SIDE
  - o SOOT IN THE PRIMARY GROOVE ON THE DOWNSTREAM SIDE
  - o SOOT TOTALLY COVERED THE GREASE ON THE LAND BETWEEN THE O-RINGS
  - o GREASE WAS SOOTED (BLACKENED) BUT STILL WAS GREASY - NOT HARD
  - o BOTTOM AND AFT FACE OF SECONDARY GROOVE HAD CLEAN GREASE
  - o CLEVIS SURFACE AFT OF SECONDARY O-RING HAD CLEAN GREASE
  - o NO SECONDARY O-RING BLOW-BY WAS OBSERVED

[Ref. 5 2-11 3 of 18]

STS-51C (STS-20) (SRM-15) Performance Preliminary Postflight Hardware Damage Assessment



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Wasatch Division

A67 Forward Field Joint Gas Path (STS-51C)

INFORMATION ON THIS PAGE WAS PREPARED TO SUPPORT AN ORAL PRESENTATION

[Ref. 5/2-11 4 of 18]

STS-51C (SRM-15) POSTFLIGHT HARDWARE DAMAGE ASSESSMENT (CONT)

- A68 (RIGHT CENTER FIELD JOINT)
  - GAS PATH THRU PUTTY AND PAST PRIMARY O-RING AT 354°
  - CLEVIS PUTTY (T = 1/8 IN.) STILL SOFT WITH LIGHT CHAR AND SOOT AT 354°
  - SOOTED ARC ON TANG SEAL SURFACE AND INSULATION FACE 320° - 0° - 10°
  - BLACK GREASE (SOOTED) BETWEEN O-RINGS 282° - 0° - 32°
  - PRIMARY O'RING SOOTED AT INTERFACE WITH GROOVE ON UPSTREAM SIDE
  - O'RING EROSION WAS VERY OBVIOUS AND WAS SEEN ADJACENT TO THE FORWARD WALL OF THE PRIMARY GROOVE
  - SOOT TOTALLY COVERED THE GREASE ON THE LAND BETWEEN THE O'RINGS
  - GREASE WAS SOOTED (BACKENED) BUT STILL WAS GREASY - NOT HARD
  - SECONDARY O'RING OBSERVATIONS
    - NO EROSION ON SECONDARY O'RING
    - HEAT AFFECTED ZONE 18 IN. CIRC - APPROXIMATELY 60° CROSS SECTION ARC
    - HEAT AFFECTED ZONE BASIS
      - DIFFERENCE IN SURFACE APPEARANCE W/O LOSS OF MATERIAL
      - SECONDARY O'RING ON A67 HAD NO DIFFERENCE IN SURFACE APPEARANCE
    - BOTTOM AND AFT FACE OF SECONDARY GROOVE HAD CLEAN GREASE
    - CLEVIS SURFACE AFT OF SECONDARY O'RING HAD CLEAN GREASE
    - NO SECONDARY O'RING BLOW-BY WAS OBSERVED

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Morton Thiokol, Inc.
Wasatch Division

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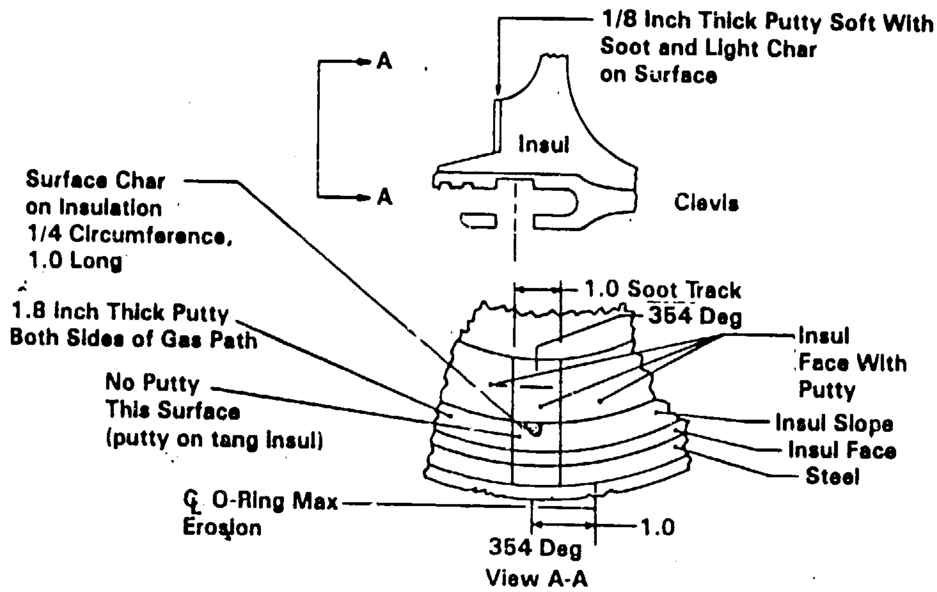
[Ref. 5/2-11 5 of 18]

STS-51C (STS-20) (SRM-15) Performance
Preliminary Postflight Hardware Damage Assessment



A68 Right Center Field Joint Gas Path (STS-51C)

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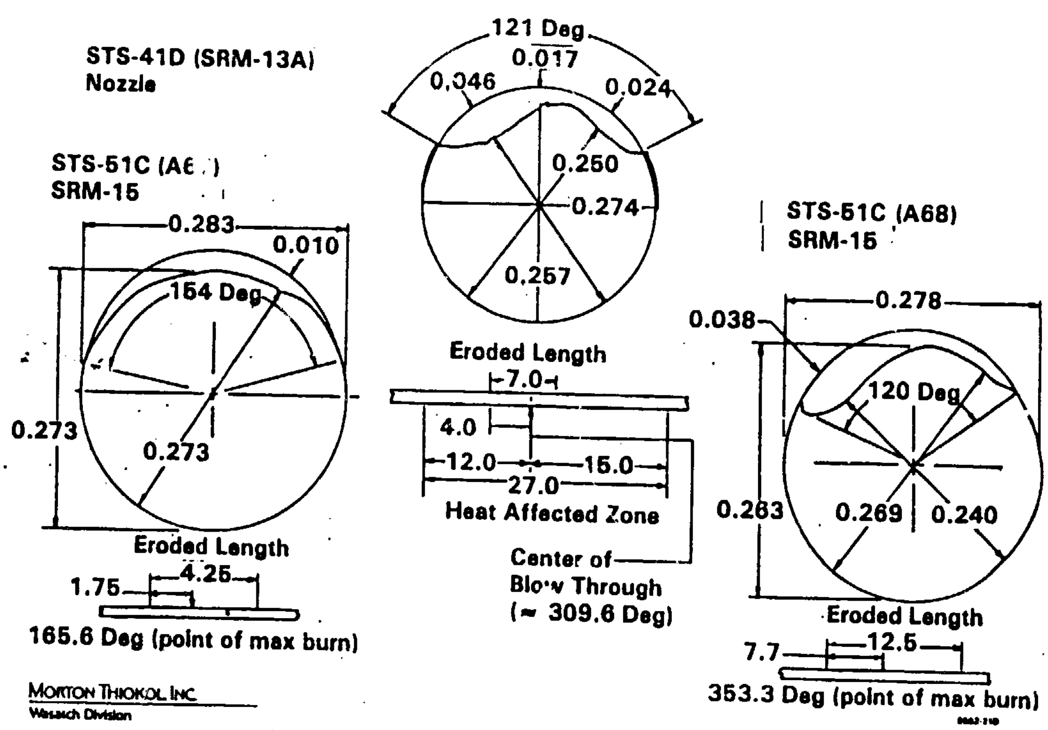
[Ref. 5/2-11 6 of 18]

O-Ring Erosion Patterns



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[Ref. 5/2-11 7 of 18]

o Analysis of Soot from STS-51C LH Forward Field Joint

	Primary Groove Sample	Land Between O-Rings	Tang Sample	Clevis Sample: Black Putty	Clevis Sample: Green Putty
FTIR Note: No HB Polymer, No Fluorocarbon detected	HD-2, Water	HD-2, Water	HD-2, Water Polyester (Putty)	Polyester, Water	Polyester, Water
EDAX*					
Fluoride (O-ring)	Present	Trace	Present	None	None
Sodium (Grease)	Present	Present	Present	Small Trace	Small Trace
Magnesium (Asbestos, putty)	Present	Present	Minor	Major	Major
Aluminum (Propellant)	Major	Minor	Major	Present	Trace
Silicon (Asbestos, putty)	Present	Present	Minor	Major	Major
Chloride (Propellant AP or Sea Water)	Major	Major	Minor	Present	None
Calcium (Grease)	Major	Major	Major	None	None
Sulfur (Grease)	Minor	Minor	Minor	None	None
Fluoride Ion Test (±20%)	0.8%	1.47%	2.1%	0.64%	-

*Relative amounts detected, starting with greatest
Major
Minor
Present
Trace
None

o HISTORY OF SRM O-RING DAMAGE

	Cross-Sectional View: Erosion Depth (in.)	Cross-Sectional View: Perimeter Affected (deg)	Cross-Sectional View: Nominal Diameter (in.)	Top View: Length of Maximum Erosion (in.)	Top View: Total Heat Affected Length (in.)
STS-20A Fwd Field**	0.010	154	0.280	4.25	5.25
STS-20B Cen Field (Prim)***	0.038	120	0.280	12.5	58.75
STS-20B Cen Field (Sec)***	None	45	0.280	None	29.5
STS-14B Fwd Field	0.028	110	0.280	3.0	None
STS-13A Aft Field*	None	None	0.280	None	None
STS-11A Fwd Field	0.040	217	0.280	3.0	14.5
STS-2B Aft Field	0.053	116	0.280	--	--
STS-14A Nozzle **	0.046	121	0.275	4.0	7.0
STS-13B Nozzle	0.034	136	0.275	1.8	5.6
STS-11B Nozzle	0.039	103.5	0.275	0.75	45
QM-4 Nozzle	0.051	[illegible]	0.275	5	11
STS-6A Nozzle*	None	None	0.275	None	None
STS-6B Nozzle*	None	None	0.275	None	None

* Hot gas path detected in putty. Indication of heat on O-ring, but no damage.
** Soot behind primary o-ring
*** Soot behind primary o-ring, heat affected secondary o-ring

- NO PROBLEMS ENCOUNTERED WITH PUTTY LOT NUMBERS OR INSTALLATION ON ALL FIELD JOINTS
- NO ANOMALOUS CONDITIONS AT ASSEMBLY OF:
  - O'RINGS
  - GREASE
  - LEAK CHECK
    - ALL STS-51C (SRM-15) AND STS-51E (SRM-16) FIELD JOINTS PASSED LEAK CHECK WITH A DECAY OF 0.2 PSI OR LESS, FIRST ATTEMPT. (1.0 PSI LEAKAGE ALLOWED DURING 10 MINUTE PERIOD AT 50 PSIG)

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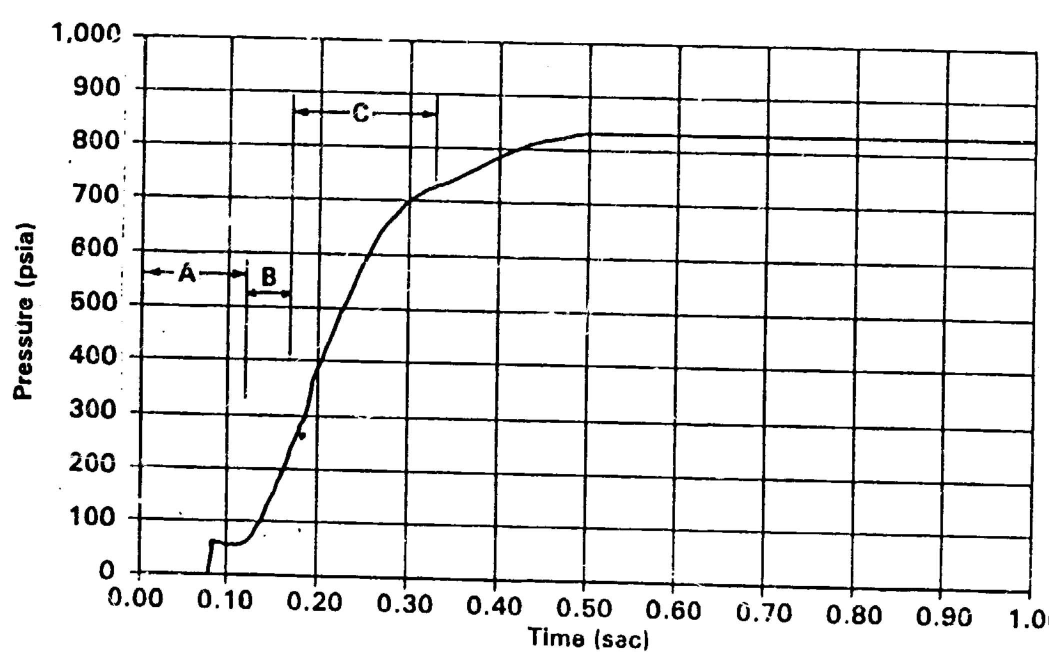
- O'RING ACTUATION SCENARIO IS THE SAME FOR FIELD JOINTS AND NOZZLE JOINTS
- FREE VOLUME IS CREATED BY DESIGN, VACUUM PUTTY LAYUP, O'RING GROOVE AND O'RING SQUEEZE AND CLEARANCES BETWEEN MATING PARTS
- FLOW PATH CAN BE CREATED BY JOINT ASSEMBLY, O'RING LEAK CHECK AND/OR PUTTY BLOW-BY DURING MOTOR IGNITION AND INITIAL PRESSURIZATION
- FLOW PATH LEADS TO THE FREE VOLUME FILLING PROCESS WHICH INDUCES LOCALIZED SHORT TIME DURATION HOT GAS JET IMPINGEMENT ON THE O'RING WITH THE ASSOCIATED HIGH STAGNATION POINT HEATING, PROCESS CONTINUES ONLY UNTIL THE FREE VOLUME IS FILLED AND PRESSURE EQUALIZATION IS ACHIEVED
- TEST DATA INDICATES THAT VITON RUBBER (UNFILLED FLUOROCARBON) EXHIBITS VERY HIGH EROSION RATES (> 200 MILS/SEC) DEPENDING UPON THE HEAT TRANSFER COEFFICIENT

STS-51C (SRM-15) STS-20 O'RING EROSION SCENARIO

- A o NO EROSION UNTIL SONIC FLOW ESTABLISHED IN FIELD JOINT AREA (0 - 50 PSI)
- B o PRIMARY O'RING SEATING PHASE (50 - 200 PSI)
  - o LOW TEMPERATURE SHRINKS O'RING AND INCREASES DUROMETER
  - o GAS JET THROUGH PUTTY IMPINGES ON PRIMARY O'RING
  - o INITIAL EROSION OF PRIMARY O'RING OCCURS (< 10 MILS)
  - o GAS FLOW PAST PRIMARY O'RING OCCURS
    - o O'RING PRODUCTS FOUND IN SOOT
    - o PUTTY PRODUCTS FOUND IN SOOT
  - o GAS FLOW DEPOSITS SOOT AND LOSES HEAT (ΔT = 5265°F) IN CIRCUMFERENTIAL EXPANSION BETWEEN O'RINGS, ΔT = 570°F BETWEEN O'RINGS
  - o NO DAMAGE TO SECONDARY O'RING
- o BLOW-BY PRIMARY O'RINGS OBSERVED MANY TIMES DURING LOW PRESSURE LEAK CHECKS CONDUCTED BETWEEN 50-200 PSI

[Ref. 5/2-11 12 of 18]

HPM Predicted Pressure During Ignition



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[Ref. 5/2-11 13 of 18]

STS-51C (SRM-15) STS-20 O-RING EROSION SCENARIO (CONT)

C 0 PRIMARY O-RING SEALS AND CONTINUES TO ERODE

- 0 PRIMARY O-RING SEATS BY THE TIME PRESSURE REACHES 200 PSI
- 0 FREE VOLUME INCREASES 10X FROM POINT "A" TO POINT "C" DUE TO MOTOR PRESSURIZATION AND JOINT EXPANSION
- 0 MAJORITY OF O-RING EROSION OCCURS DURING THIS PHASE UNTIL PRESSURE EQUILIBRATION OCCURS AT APPROXIMATELY 330MS
- 0 DEPTH OF EROSION IS A FUNCTION OF GAS JET SIZE I.E. SMALLER SIZE HIGHER EROSION
- 0 A67 (L.H.) GAS PATH 2.2 INCHES NEAR O-RING COMPARED TO 1.0 INCH ON A68 (R.H.) I.E. AREA RATIO = 4.8, EROSION DEPTH RATIO = 3.8, ASSUMING FINAL AREA SAME RATIO AS INITIAL

[Ref. 5 2-11 14 of 18]

VOLUMETRIC AND THERMAL ANALYSIS AND TEST RESULTS

	MAXIMUM EXPECTED EROSION	MAXIMUM EROSION EXPERIENCED
FIELD JOINT		
PRIMARY	0.092	0.053
SECONDARY	0.060	NONE*
NOZZLE JOINT		
PRIMARY	0.070	0.051
SECONDARY	0.004	NONE

*HEAT EFFECTS WITH NO DEPTH SEEN ON STS-20B

- 0 TESTS SHOW THAT O-RING MAINTAINS SEAL UNDER 3000 PSI WHEN 0.095 INCH MATERIAL IS REMOVED, A 3:1 SAFETY FACTOR FOR PRESSURE EXTRUSION

- 0 SOOT SEEN BEHIND STS-14A NOZZLE PRIMARY SEAL WAS A RESULT OF MOMENTARY GAS PASSAGE BEFORE THE O-RING SEATED

[Ref. 5 2-11 15 of 18]

WHY DO MOST O-RINGS NOT ERODE AND WHY DON'T WE SEE SOOT BEHIND PRIMARY O-RINGS MORE OFTEN?

- O NO O-RING EROSION OCCURS IF GAS JET IS NOT PRESENT
- O IF NO GAS JETS ARE PRESENT, O-RING IS ENERGIZED BY PUTTY COMPRESSING SMALL VOLUME OF AIR TRAPPED BETWEEN PUTTY AND O-RING
  - O IF BLOW BY OCCURS DURING SEATING, NO SOOT IS PRESENT IN TRAPPED AIR
- O SOOT COMES FROM EROSION OF PUTTY AND/OR O-RING
- NO EVIDENCE THAT PRIMARY O-RING IS LEAKING AFTER INITIAL SEATING PHASE

[Ref. 5 2-11 16 of 18]

RATIONALE FOR ACCEPTANCE

- o O-RING EROSION ON STS-51C WAS WITHIN EXPERIENCE DATA BASE
- o MOMENTARY GAS PASSAGE BY THE PRIMARY SEAL WAS SEEN ON THE STS-14A NOZZLE JOINT
- o SECONDARY SEAL HEAT EFFECTS WERE WELL BELOW ANALYTICAL WORST CASE PREDICTIONS
- o GAS JET PENETRATES THE PRIMARY SEAL PRIOR TO ACTUATION AND SEALING
- o TESTS SHOW THAT O-RINGS WILL SEAL AT 3000 PSI WITH 0.095 INCH OF MISSING MATERIAL (WHICH IS GREATER THAN THE WORST CASE PREDICTION AND ALMOST TWICE THE THE EROSION SEEN ON ANY SRM MOTORS).
- o PRIMARY O-RING EROSION OBSERVED TO DATE IS ACCEPTABLE AND WILL ALWAYS BE MORE THAN EROSION ON SECONDARY O-RING IF IT OCCURS.
  - o PRIMARY O-RING LEAK CHECK PUSHES O-RING IN WRONG DIRECTION -- SECONDARY O-RING IS SEATED BY LEAK CHECK
  - o GAS VOLUME IN FRONT OF PRIMARY O-RING IS 50% GREATER THAN FREE VOLUME BETWEEN O-RINGS
  - o GAS WILL COOL AS IT PASSES PRIMARY O-RING AND DIFFUSES CIRCUMFERENTIALLY
- o SECONDARY O-RING IS A REDUNDANT SEAL USING ACTUAL HARDWARE DIMENSIONS

[Ref. 5 2-11 17 of 18]

FLIGHT READINESS ASSESSMENT FOR STS-51E

- o EVALUATION SUMMARY
  - o STS-51C PRIMARY O'RING EROSION ON TWO FIELD JOINTS
  - o STS-51C SOOT BETWEEN PRIMARY AND SECONDARY O'RINGS ON BOTH FIELD JOINTS - FIRST TIME OBSERVED ON FIELD JOINT
  - o EVIDENCE OF HEAT AFFECT ON SECONDARY O'RING OF A68 (RIGHT HAND) CENTER FIELD JOINT BUT NO EROSION - FIRST TIME HEAT AFFECT ON SECONDARY O'RING HAS BEEN OBSERVED
- o CONCLUSION
  - o STS-51C CONSISTENT WITH EROSION DATA BASE
    - o LOW TEMPERATURE ENHANCED PROBABILITY - STS-51C EXPERIENCED WORST CASE TEMPERATURE CHANGE IN FLORIDA HISTORY
    - o EROSION IN TWO JOINTS OBSERVED BEFORE - STS-11 AND 14
  - o STS-51E COULD EXHIBIT SAME BEHAVIOR
  - o CONDITION IS ACCEPTABLE
- o STS-51E FIELD JOINTS ARE ACCEPTABLE FOR FLIGHT

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MORTON THIOKOL, INC.
Wasatch Division

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[Ref. 5 2-11 18 of 18]

History of O-ring (23)
[T. p. 2665-66]

RECEIVED
31 JAN 1985
Joe C. Kilminster

MESSAGE DISPL

TO LARRY WEAR   TO SANDY COLEMAN

From: Larry Mulloy
Postmark: Jan 31,85 7:39 AM
Status: Certified Urgent
Subject: 51C O-RING EROSION RE: 51E FRR

Message:

FRR DISCUSSION SHOULD RECAP ALL INCIDENTS OF O-RING EROSION, WHETHER NOZZLE OR CASE JOINT AND ALL INCIDENTS WHERE THERE IS EVIDENCE OF FLOW PAST THE PRIMARY O-RING. ALSO, THE RATIONALE USED FOR ACCEPTING THE CONDITION ON THE NOZZLE O-RING. ALSO, THE MOST PROBABLE SCENARIO AND LIMITING MECHANISM FOR FLOW PAST THE PRIMARY ON THE 51C CASE JOINTS. IF MTI DOES NOT HAVE ALL THIS FOR TODAY I WOULD LIKE TO SEE THE LOGIC ON A CHART WITH BLANKS TBD.

----------------X----------------

[Ref. 5 2-12]

MORTON THIOKOL INC

Division

9 August 1985
E150/BGR-86-17

Mr. James W. Thomas, Jr., SA42
George C. Marshall Space Flight Center
National Aeronautics and Space Administration
Marshall Space Flight Center, AL 35812

Dear Mr. Thomas:

Subject: Actions Pertaining to SRM Field Joint Secondary Seal

Per your request, this letter contains the answers to the two questions you asked at the July Problem Review Board telecon.

1. Question: If the field joint secondary seal lifts off the metal mating surfaces during motor pressurization, how soon will it return to a position where contact is re-established?

   Answer: Bench test data indicate that the o-ring resiliency (its capability to follow the metal) is a function of temperature and rate of case expansion. MTI measured the force of the o-ring against Instron plattens, which simulated the nominal squeeze on the o-ring and approximated the case expansion distance and rate

   At 100°F, the o-ring maintained contact. At 75°F, the o-ring lost contact for 2.4 seconds. At 50°F, the o-ring did not re-establish contact in ten minutes at which time the test was terminated.

   The conclusion is that secondary sealing capability in the SRM field joint cannot be guaranteed.

2. Question: If the primary o-ring does not seal, will the secondary seal seat in sufficient time to prevent joint leakage?

[Ref. 5 2-13 1 of 2]

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Mr. James W. Thomas, Jr., SA42
9 August 1985
E15C/BGR-86-17
Page 2

Answer: MTI has no reason to suspect that the primary seal would ever fail after pressure equilibrium is reached, i.e., after the ignition transient. If the primary o-ring were to fail from 0 to 170 milliseconds, there is a very high probability that the secondary o-ring would hold pressure since the case has not expanded appreciably at this point. If the primary seal were to fail from 170 to 330 milliseconds, the probability of the secondary seal holding is reduced. From 330 to 600 milliseconds the chance of the secondary seal holding is small. This is a direct result of the o-ring's slow response compared to the metal case segments as the joint rotates.

Please call me or Mr. Roger Boisjoly if you have additional questions concerning this issue.

Very truly yours,

Brian G. Russell

Brian G. Russell, Manager
SRM Ignition System

BGR/co

cc: L. Wear, SA42
E. Skrobiszewski, SA49
J. Adams, MTI/MSFC

bcc: J. Kilminster
A. McDonald
R. Ebeling
J. Elwell
B. Brinton
A. MacBeth
R. Boisjoly
A. Thompson
S. Stein

[Ref. 5-2-13 2 of 2]

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AFTERNOON SESSION

(1:10 P.M.)

CHAIRMAN ROGERS: Will the Commission come to order, please?

Gentlemen, good afternoon. I guess all of you have been sworn in, and those of you who have not been sworn in, maybe you had better be so.

(Witnesses sworn.)

CHAIRMAN ROGERS: The Commission is coming to the end of its tenure. In fact, we think this is probably the last hearing that we will have, and in the course of preparing our report, we have—our staff has looked at a lot of documents, and we wanted before we closed off the hearings to give you gentlemen a chance to make any comments you want to about the documents, and I guess some of the documents have already been shown to you, and so if that is all right we will just get started and ask questions.

Do you want to say anything?

MR. KEHRLI: I would just like to—there is a book that is in front of you, the red book, that some of you don't have, with additional copies of documents. What we will be referring to is Documents Number 2 and Number 3 in the very back of the book. They are tabbed according to number.

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And what I would like to start off with are those two specific documents which are Marshall problem assessment system tracking reports, and one was started after STS-41B in 1984, and that is Tab Number 3, and Tab Number 2 was the problem report that was started after the 51B nozzle O-ring erosion problem, which was discovered at Thiokol on June 25th, 1985, and you will see from that particular problem that a launch constraint was applied as a result of the 51B O-ring nozzle erosion problem. [Ref. 5 2-14] [Ref. 5 2-15]

What I would like to ask you gentlemen is if you have ever seen those two Marshall tracking reports before, or are you familiar with them?

TESTIMONY OF BRIAN RUSSELL, PROGRAM MANAGER, DEPARTMENT OF SOLID ROCKET MOTOR IGNITER AND FINAL ASSEMBLY; ROBERT EBELING, DEPARTMENT MANAGER, SOLID ROCKET MOTOR IGNITER AND FINAL ASSEMBLY; ALLAN J. McDONALD, DIRECTOR, SOLID ROCKET MOTOR PROJECT; J.C. KILMINSTER, VICE PRESIDENT, SPACE BOOSTER PROGRAMS; AND ROGER M. BOISJOLY, SEAL TASK FORCE, MORTON THIOKOL, INC.

MR. RUSSELL: I am Brian Russell. I'm familiar with them. I can't recall if I have seen exactly these, although I would receive in the mail in my office a

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monthly problem report which would be similar to that, and whether it was this one exactly I can't say without checking my notes back at the plant, but we would get these every month, and they would contain these problems as well as others that were being tracked.

MR. KEHRLI: Now, some of these entries, isn't it correct to say that the entries themselves, the information came from Thiokol that was eventually placed on these documents?

MR. RUSSELL: That is correct.

MR. KEHRLI: How would that work? Could you tell the Commission how that system worked, please?

MR. RUSSELL: We have our reliability engineering department, who is responsible to complete the monthly problem report, and in addition to that we have our monthly problem review board telephone conference with NASA and the contractors, of which we are a part, and the monthly problem review or the monthly problem report that reliability prepares they get the information from engineering or from the program office as necessary to complete their status of what has happened during that month, whether the problem originated that month or what has been done to close the problem out, and that is submitted every month, and I for one do review that before it is submitted to the

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Marshall Space Flight Center, and so much of the information that I would read in these reports would be the same information that we had given in that monthly problem report or over the telephone on the teleconference.

CHAIRMAN ROGERS: Mr. Russell, when you say close the problem out, what do you mean by that? How do you close it out normally?

MR. RUSSELL: Normally, whether it takes engineering analysis or tests or some corrective action, a closeout to the problem would occur after an adequate corrective action had been taken to satisfy those on the problem review board that the problem had indeed been closed out. That is the way that that happens. For example, we had found a loose bolt on the recovery one time, and we had to take corrective action in our procedures and in the engineering to make sure that that wouldn't happen again, and then to verify that corrective action, and at that point that problem would be ready to be closed out. It generally involves a report or at least a mention by the review board stating what had been done to adequately close it out, and then it is agreed upon by the parties involved.

CHAIRMAN ROGERS: Without reference to a particular flight, if you had a problem with erosion and

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blow-by, how would you expect it would be closed out? How would you normally close that out?

MR. RUSSELL: The normal closeout procedure would be after there had been changes to prevent the problem or to control the problem or to prevent it from recurring. That would be the normal system.

MR. KEHRLI: Now, as I mentioned earlier, one of these problems, Tab 2, the problem on the nozzle erosion, the O-ring erosion after 51B, did contain a launch constraint. Is that your understanding?

MR. RUSSELL: Well, my understanding was, until we adequately handled any of the O-ring erosion problems in the subsequent FRR, that I don't know if it would be so formal as a launch constraint, but I know that when we discovered any O-ring erosion or soot blow-by or whatever might have been beyond our experience base, there was always some analysis or some justification in the flight readiness review to continue to fly, and I guess I didn't realize that that one was any different than the others.

MR. KEHRLI: What do you understand a launch constraint to mean?

MR. RUSSELL: My understanding of a launch constraint is that the launch cannot proceed without adequately—without everyone's agreement that the

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problem is under control.

CHAIRMAN ROGERS: Under control meaning what? You just said a moment ago that you would expect some corrective action to be taken.

MR. RUSSELL: That is correct, and in this particular case on this 51B nozzle O-ring erosion problem there had been some corrective action taken, and that was included in the presentation made as a special addendum to the next flight readiness review, and at the time we did agree to continue to launch, which apparently had lifted the launch constraint, would be my understanding.

DR. WALKER: What was the corrective action which was taken in that case?

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MR. RUSSELL: One of the corrective actions was an increase in the leak check pressure because one of the ways we thought that could happen, our analysis said that the blow-by across the O-ring which added to the erosion of that particular O-ring, started right at ignition, indicating the O-ring didn't seal right from the very beginning.

And one of the ways that we thought that could have happened was that the putty could have masked our leak check, and we thought it was of utmost importance to have a verified primary O-ring and so we increased leak check pressure to 200 psi to make sure that we would blow through the putty, realizing that blow holes are not desirable either, but yet it is more important to know that you have a good O-ring and have some putty blow through than otherwise.

DR. COVERT: Mr. Russell, where did 200 psi come from? Is that twice 100?

MR. RUSSELL: No. We did some bench testing of some putty and some gaps that showed we tried at 50, 100 and 150 psi and since we have a ten-minute stabilization period, the question was can the putty hold these certain pressures for ten minutes? And in no case did 150 psi, in no case could the putty hold 150 psi for ten minutes. And we put a factor on top of that

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of another 50 psi, and that was the basis for that.

DR. COVERT: Thank you.

DR. WALKER: The analysis that some of our staff has done suggests that after you increase the test pressure to 200 pounds, the incidence of blow-by and erosion actually increased.

MR. RUSSELL: We realized that.

DR. WALKER: Did you realize that only after the accident?

MR. McDONALD: Could I try to answer some of that because I was very involved in this, and in fact this is the first time I realized that was a launch constraint.

CHAIRMAN ROGERS: Could you speak a little louder, please?

MR. McDONALD: This is the first time I realized that this was flagged as a launch constraint and the others weren't, because the problem of the O-ring erosion was predominant all the way from STS-2. The uniqueness of the 16A nozzle which was 51 Baker, that was the first time we had violated a primary O-ring. And that erosion that caused that violation is different than we had seen previously and was different for a significant reason, in that it had blow-by at ignition which caused the erosion of the primary

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O-ring.

It just wasn't jet impingement on the sealed O-ring which was what we had seen before. And because of that jet impingement on the unsealed O-ring, it made it unique. And we had to understand for the next flight, before we could launch it, as to what happened to that flight that we don't think will happen to the next one.

The point that Brian made was that in looking at the records for the assembly and leak check of that flight, it was the last one which we had one with 100 psi stabilization pressure by the leak check. That was the last one.

And we knew that we had to have blow-by at ignition to consume as much of the O-ring as we consumed on that particular flight. Therefore, we felt that we missed the leak check on that flight because the putty may well have masked it, or the fact that the groove in the nozzle is actually wider than it is in the field joint, and it moves farther, and therefore there is a higher probability that you may lose the leak check there because when you run the leak check it is on the wrong face.

It has got farther to travel, and either contamination or whatever, when you finally pressurize it to the other side, maybe it won't seal when it

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pressurizes the right side where it may have sealed and passed the leak check.

So the conclusion was that may have been unique to that assembly and that all subsequent flights have had the 200 psi leak check to eliminate the potential for the putty masking it. And that, I think, was the basis for proceeding on with the next flight.

Now, I notice this is dated February 1986. I've seen a lot of these and I don't recall that that was a flight constraint earlier.

CHAIRMAN ROGERS: I think it was, wasn't it?

MR. KEHRLI: It's my understanding that that is a February 26th printout, but from those dates on the left-hand column there, the dates of the entry, you will see that the launch constraint was applied in July of 1985.

CHAIRMAN ROGERS: Let's ask some specific questions.

Did you all know that there was a launch constraint placed on the flights in July 1985?

MR. McDONALD: Brian is right as far as every erosion we had. We had to address it prior to the next flight, and I always felt that it could always become a launch constraint if it was not adequately understood and explained why this problem could not get worse on

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the next flight.

CHAIRMAN ROGERS: But really my question is: Did you gentlemen realize that it was a launch constraint?

MR. RUSSELL: I would like to answer for myself. I didn't realize that there was a formal launch constraint on this one, any different than some of the other erosion and blow-by that we had seen in the past.

MR. EBELING: I agree.

CHAIRMAN ROGERS: Isn't there a letter recommending that the launch constraint be taken off from you?

MR. KEHRLI: No. There's a letter recommending the problem be closed, the entire problem.

CHAIRMAN ROGERS: What's the difference? [Ref. 5 2-16 through 19]

MR. KEHRLI: Well, let's ask about that letter. Mr. Russell, you wrote a letter, did you not, or a memorandum indicating that the problem should be closed.

Could you explain to the Commission what you meant by that?

MR. RUSSELL: Yes. In our December telephone call on the Problem Review Board and I can't remember the date it was around the 9th or so there was a request to close the problems out and particularly the ones that had been open for a long time, of which this

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was one, and a long time meaning six months or more.

There was a request from the Director of Engineering, as I recall it, that we close these problems out.

CHAIRMAN ROGERS: What did you think that meant? You've got a lot of problems; close them out. I don't understand that language.

DR. WALKER: That was the Director of Engineering at Marshall?

MR. RUSSELL: Yes, at Marshall Space Flight Center. Now, he wasn't in that telephone call. My understanding is what they told us and my recollection was that Mr. Kingsbury would like to see these problems closed out.

Now, the normal method of closing them out is to implement the corrective action, verify the corrective action, and then the problem is closed, it comes off the board and is no longer under active review.

CHAIRMAN ROGERS: That's what you just said. Now, you get a request to close it out. What did you think then?

MR. RUSSELL: Well, it was expanded. As we talked in that telephone call, we were looking for ways to close out any and all of the problems and we talked

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about each one separately.

DR. WALKER: Is there any other way besides fixing them?

MR. RUSSELL: Well, the way that we discussed is what I'm leading to; that we talked about the fact that this telephone call and monthly Problem Review Board as well as the Problem Report, really in our eyes and in those apparently of the people on the phone call at the Marshall Space Flight Center, was not adding any more visibility to the problem.

The fact was that we had a task force created with full-time people on it. We had constant reviews going on. We had an active program plan that we were working and were doing some test analyses and so forth to come to a solution of the problem.

And so we talked about that and pretty much jointly agreed that we would not be losing any visibility by closing out, and agreed that that would be rationale to close it.

CHAIRMAN ROGERS: I have trouble. Losing any visibility to close it. What does that mean?

MR. RUSSELL: What I mean to say is that from my understanding of why there would be a Problem Review Board is to make sure that things don't inadvertently become lost in the system or ignored for any reason;

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that you have a problem and you adequately attack it, fix it, and implement it and verify it.

And that is my understanding of the purpose of such a board. On this problem, however, I personally felt that there was no chance of possibly ignoring the problem; that the visibility—

what I mean by that is that management at NASA and management at Thiokol were adequately aware of the problem and also understood what was being done about it to fix it.

And that is what I mean by visibility.

CHAIRMAN ROGERS: What was being done to fix it?

MR. RUSSELL: Well, we had a task force created of full-time people at Thiokol, of which I was a member of that task team, and we had done some engineering tests. We were trying to develop concepts. We had developed some concepts to block the flow of hot gas against the O-ring to the point where the O-ring would no longer be damaged in a new configuration.

And we had run some cold gas tests and some hot gas motor firing tests and were working toward a solution of the problem and we had some meetings scheduled with the Marshall Space Flight Center. We had weekly telephone calls where we statused our progress and there was a team at Marshall also of engineering

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people who were monitoring the things that we were doing to fix the problem with the goal of implementing a fix in our qualification motor No. 5, which was scheduled at that time in January, this time frame being about the December time frame of last year.

CHAIRMAN ROGERS: Can I interrupt? So you're trying to figure out how to fix it, right? And you're doing some things to try to help you figure out how to fix it.

Now, why at that point would you close it out?

MR. BOISJOLY: I think if I may——

CHAIRMAN ROGERS: Just let me finish. Why would you write a letter saying let's close it out now?

MR. RUSSELL: Because I was asked to do it.

CHAIRMAN ROGERS: I see. Well, that explains it.

MR. RUMMEL: That explains it, but it really doesn't make any sense. On the one hand you close out items that you've been reviewing flight by flight, that have obviously critical implications, on the basis that after you close it out, you're going to continue to try to fix it.

So I think what you're really saying is, you're closing it out because you don't want to be

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bothered. Somebody doesn't want to be bothered with flight-by-flight reviews, but you're going to continue to work on it after it's closed out.

Is that right?

MR. RUSSELL: I would like to respond to that. Now, this was not a flight-by-flight review. This was a month-by-month review, regardless of what was going on in the flight cycle.

MR. RUMMEL: Excuse me. But each flight up till then for some time had been reviewed. You reviewed prior experience and this had been on a list which really mandated that each flight be reviewed with respect to the O-ring problem. So I assume you did in fact review each flight.

MR. RUSSELL: Yes, but we were not requesting that it be taken off of that review. That was a separate set of reviews, and in fact it was one of the rationale items for removing it from the Problem Review Board. That was just one activity in the system that we had made a move to take it off of that review system, the Problem Review Board.

It had not been removed from the Flight Readiness Reviews.

DR. FEYNMAN: It says here that there was a formal reporting system used to evaluate the problem resolution part of it, and that is why you wanted to close it out of the problem assessment system.

Maybe Mr. Boisjoly could explain that.

MR. BOISJOLY: Basically, closeout may be a poor term because nobody was going to close it out. Instead of tracking the same problem through two or three different channels, we were going to track the problem now in the highlight of the task team, plus the Flight Readiness Reviews, so there was no need to track it in several different locations. It was the same problem.

So we were tracking it as it came up in Flight Readiness Reviews and we were statusing and tracking it within the Seal Team. So it was a simple matter of bookkeeping to take it out of one area because we were already doing it in another area.

DR. SUTTER: Well, was this task force, did it have the priority and ability to really charge out and move on this problem? Was it receiving top management attention?

MR BOISJOLY: As top as we could get; yes. We had statuses every week. And we were reporting the weekly activity.

DR. SUTTER: Well, there were memos written saying they couldn't get parts and you couldn't get

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tests.

MR. BOISJOLY: That's right, but it was just to take it off of one area instead of tracking the same thing in two manners with two different groups of people, two sets of manpower, to put it and consolidate it.

DR. SUTTER: Well, taking it off of that list, maybe that would remove it as a pressure point in upper management to move out and make sure this task team was given the ability to do their job.

MR. BOISJOLY: No. If anything, the task team turned the gain up on it a little bit more, as indicated by my memos.

DR. SUTTER: It is just hard to know why, taking it off of any list, when it was one of the more critical items. Why not leave it there and have everybody get bugged by it?

CHAIRMAN ROGERS: Well, Mr. Russell said they did it because they asked him to. It's as simple as that, I guess.

MR. EBELING: It was being tracked in three separate locations, three written and teleconed descriptions every month or weekly in many cases, in the case of the O-Ring Task Force as it was called, and it was a poor expenditure of people's time tracking it in

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three different places simultaneously.

CHAIRMAN ROGERS: But I say, though, so that you understand, the confusion on the part of some of us is we have a constraint which was placed on July 1985 and Marshall and Mr. McDonald is listed as being one of those who knew about it.

And Mr. Mulloy says—and he testified this morning—he has the right to waive the constraint every time, so on all of these flights he waived it.

Now, during that time, as I understand it, you were doing some tests and so forth. And so each time nothing happened. I mean Mr. Russell said you were working on the problem, but as far as the joint is concerned, things continued on just the way they had been going and you had been having a lot of trouble.

This was a very troubled joint, obviously. And getting back to the group that is working on the problem, trying to fix it, I don't know, my word is repair it or fix it, do something about it, apparently that wasn't going very well either because there's a memo here dated October 1, 1985 on Tab 33 from Mr. Ebeling which starts out—in connection with this it says: "Help. The Seal Task Force is constantly being delayed by every possible means. People are quoting policy and systems and not working on them. Marshall is correct in

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saying that we do not know how to run a development program." [[Ref. 5 2-20]

And later on, after a lot of discussion about O-rings and putty he says: "The allegiance of the O-ring Investigation Task Force is very limited to a group of engineers numbering eight to ten. Our assembled people in Manufacturing and Quality have the desire, but are encumbered with other significant work. Others in Manufacturing, Quality, Procurement, who are not involved directly, but whose help we need, are generating plenty of resistance. We are creating more instructional paper than engineering data. We wish we could get action by verbal request, but such is not the case. This is a red flag."

Now, how does that happen? And then there are constraints on the launches. The constraints were waived and there seems to be total lack of coordination between Marshall and Thiokol.

MR. KILMINSTER: Mr. Chairman, if I could, I would like to respond to that.

In response to the concern that was expressed—and I had discussions with the team leader, the task force team leader, Mr. Don Kettner, and Mr. Russell and Mr. Ebeling. We held a meeting in my office and that was done in the October time period

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where we called the people who were in a support role to the task team, as well as the task force members themselves.

In that discussion, some of the task force members were looking to circumvent some of our established systems. In some cases that was acceptable; in other cases it was not. For example, some of the work that they had recommended to be done was involved with full scale hardware, putting some of these joints together with various putty layup configuration; for instance, taking them apart and finding out what we could from that inspection process.

DR. SUTTER: Was that one of these things that was outside of the normal work, or was that accepted as a good idea or a bad idea?

MR. KILMINSTER: A good idea, but outside the normal work, if you will.

DR. SUTTER: Why not do it?

MR. KILMINSTER: Well, we were doing it. But the question was, can we circumvent the system, the paper system that requires, for instance, the handling constraints on those flight hardware items? And I said no, we can't do that. We have to maintain our handling system, for instance, so that we don't stand the possibility of injuring or damaging a piece of flight

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hardware.

I asked at that time if adding some more people, for instance, a safety engineer—that was one of the things we discussed in there. The consensus was no, we really didn't need a safety engineer. We had the manufacturing engineer in attendance who was in support of that role, and I persuaded him that, typical of the way we normally worked, that he should be calling on the resources from his own organization, that is, in Manufacturing, in order to get this work done and get it done in a timely fashion.

And I also suggested that if they ran across a problem in doing that, they should bubble that up in their management chain to get help in getting the resources to get that done.

Now, after that session, it was my impression that there was improvement based on some of the concerns that had been expressed, and we did get quite a bit of work done.

For your evaluation, I would like to talk a little bit about the sequence of events for this task force.

CHAIRMAN ROGERS: Can I interrupt? Did you know at that time it was a launch constraint, a formal launch constraint?

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MR. KILMINSTER: Not an overall launch constraint as such. Similar to the words that have been said before, each Flight Readiness Review had to address any anomalies or concerns that were identified at previous launches and in that sense, each of those anomalies or concerns were established in my mind as launch constraints unless they were properly reviewed and agreed upon by all parties.

CHAIRMAN ROGERS: You didn't know there was a difference between the launch constraint and just considering it an anomaly? You thought they were the same thing?

MR. KILMINSTER: No, sir. I did not think they were the same thing.

CHAIRMAN ROGERS: My question is: Did you know that this launch constraint was placed on the flights in July 1985?

MR. KILMINSTER: Until we resolved the O-ring problem on that nozzle joint, yes. We had to resolve that in a fashion for the subsequent flight before we would be okay to fly again.

CHAIRMAN ROGERS: So you did know there was a constraint on that?

MR. KILMINSTER: On a one flight per one flight basis; yes, sir.

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CHAIRMAN ROGERS: What else would a constraint mean?

MR. KILMINSTER: Well, I get the feeling that there's a perception here that a launch constraint means all launches, whereas we were addressing each launch through the Flight Readiness Review process as we went.

CHAIRMAN ROGERS: No, I don't think—the testimony that we've had is that a launch constraint is put on because it is a very serious problem and the constraint means don't fly unless it's fixed or taken care of, but somebody has the authority to waive it for a particular flight.

And in this case, Mr. Mulloy was authorized to waive it, which he did, for a number of flights before 51-L. Just prior to 51-L, the papers showed, the launch constraint was closed out, which I guess means no longer existed. And that was done on January 23, 1986.

Now, did you know that sequence of events?

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MR. KILMINSTER: Again, my understanding of closing out, as the term has been used here, was to close it out on the problem actions list, but not as an overall standard requirement.

We had to address these at subsequent Flight Readiness Reviews to insure that we were all satisfied with the proceeding to launch.

CHAIRMAN ROGERS: Did you understand the waiver process, that once a constraint was placed on this kind of a problem, that a flight could not occur unless there was a formal waiver?

MR. KILMINSTER: Not in the sense of a formal waiver, no, sir.

CHAIRMAN ROGERS: Did any of you? Didn't you get the documents saying that?

MR. McDONALD: I don't recall seeing any documents for a formal waiver.

MR. KEHRLI: Now, on the letter to Larry Wear, where Thiokol recommends to Mr. McDonald—where Thiokol recommends closing out the problem, that letter specifically references these AO numbers. If you look at Attachment No. 4 under launch constraint, the subject of the letter written December 10th, 1985, is closure.

CHAIRMAN ROGERS: Which one is this?

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MR. KEHRLI: Number four of the launch constraint attachment, the letter from Mr. McDonald to Larry Wear of Marshall, and this is the request to close the problem. Under the subject, it specifically references the Marshall tracking numbers AO-7934, and those other numbers as well.

When you look at those numbers, those are the documents that you have in front of you, that report specifically lists the launch constraint on the nozzle joint and on the field joint number—the field joint problem, it says: "launch constraint, none".

So, isn't it accurate to say, based upon this letter, you must have seen those documents if you are referencing them, is that correct?

MR. RUSSELL: Answering for myself, yes, and I will have to also say that my understanding was that we were closing it off at the problem review board, and I did not know that there was a launch constraint that had to be waived every time, and I did not realize that this kind of an action would lead to removing any such constraint that I did not know about.

MR. McDONALD: I wrote the letter, and I would like everyone to read the letter to see what it says. It says, the subject critical problems are ongoing problems which will not be resolved for some

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time. So, right away, I don't think that tells anybody that we are going to forget about this and take it off of anything.

It also says that, we request that subject critical problems be closed and removed from the next PRB agenda list, and the reason being that we spend more time each month going through reading all of that same thing that we have been reading every month for two years, because somebody colors a square in down at Marshall, please keep track of this each month.

We said, we've got a full-time task force working this problem. We have weekly meetings. We need more people solving the problem and less people keeping tracking and statusing it, and there's no sense us continuing doing that when we've got a very heavy activity doing that, and we got a letter a couple of weeks later from Larry Wear.

In fact, I was called before that letter, saying we were going to get it, that Marshall was very upset with us because we've got problems that have been on there for several years, and we haven't gotten them off of this list because it keeps getting thicker and thicker.

And, if you ever want to get something where nobody will read it, you get it so thick that they

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finally pay no attention to it and that is exactly the thing the Problem Review Board was doing. It was getting so thick it had problems on there we knew we weren't going to solve for some time. And, they were the same problems we're having today.

And so, we said, okay, if you want to get them off the list, then just take them off and we will handle them through this other mechanism that we're addressing with everybody that really knows about this problem. And, I was unaware that a waiver was ever written for every flight after this problem.

CHAIRMAN ROGERS: So, your idea was really to cut down the paperwork?

MR. McDONALD: Yes. We're spending a lot of time in going through this matter where we had nothing to add, nor did anybody else.

DR. FEYNMAN: Mr. McDonald, this letter is a request. Did you ever get an answer that permitted you to do this?

MR. McDONALD: No, I didn't get any answer that permitted me to do that, nor did I know that anything was done about it. I just said, we've got a lot of things to do. This is one thing we don't need to do.

MR. ACHESON: Did you take the lack of answer

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to mean that it had been closed out, that your proposal had been accepted?

MR. McDONALD: I don't know, because I believe we cancelled the subsequent Problem Review Board and I think the one after that or something, and so I don't know whatever happened after that letter was written. I wasn't involved in the next meeting.

MR. RUSSELL: I would like to add, we wouldn't have expected to get a letter back to this. It was a Marshall tracking system and, like Mr. McDonald said, we would have heard about the action in the next Problem Review Board meeting, which would have then been scheduled in early February, which was after the accident and did not take place.

MR. FEYNMAN: You mean, they come that far apart? This was December 1985.

MR. RUSSELL: But, we wrote the request—oh, they come every month.

MR. FEYNMAN: Well, then what happened?

MR. RUSSELL: Well, my recollection of being there is that——

MR. FEYNMAN: Oh, okay. January 1986 is right after December 1985. I made a mistake. Excuse me.

MR. SUTTER: The task force was trying to improve the joint?

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MR. RUSSELL: Yes.

MR. SUTTER: That in effect does not tie into what you do for the next launch? It seems to me that this conveys an attitude then that, okay, we've got troubles with this joint but it is okay to keep flying with it even though we want to fix it, that you're willing to keep flying with the joint even though you know you've got problems? And, I'm just wondering, what was the attitude.

There wasn't really undue concern about continuing flying.

MR. RUSSELL: The attitude was that it was an undesirable condition but still acceptable for flight, based upon the history that we had seen up until that point.

MR. SUTTER: The history of the erosion and the blow-by and the changing the leak checks and all of that did not really build into your people that should have been responsible, that this was a critical item?

MR. RUSSELL: I don't think that's true. I think we considered it to be a critical problem, which is the reason that we had the task force created to fix it. But, critical to stopping the program, it obviously wasn't.

MR. SUTTER: Well, you were going to run, say

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another 20 or 30 flights without doing anything?

MR. BOISJOLY: No. Those who were intimately familiar with it—that is why we were writing these memos, to try to get a little bit of flame turned up underneath it, to be able to get something done, because we were concerned, we were extremely concerned.

I think that is what those memos reflect. They reflect frustration and concern both, to continue to fly in a condition that is marginal or a condition after the 16-nozzle erosion, if that ever occurred in a field joint, we would have a terrible problem on our hands and that is what we were trying to do.

We were attempting in that team to short-circuit company procedures. We were attempting to get things done that are not normally done, so that we could try things that we could tweak the system and get as many pieces of information in as short a period of time as possible, so that we could effect a change and get it implemented as quick as possible, and that was a big source of frustration because we weren't getting that support.

CHAIRMAN ROGERS: Mr. Kilminster, did you realize at the time you changed your vote on Launch 51-L, about this frustration that Mr. Boisjoly is speaking about?

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MR. KILMINSTER: Yes, sir.

CHAIRMAN ROGERS: And you were not familiar with the fact that there was a constraint that had been placed on it in July?

MR. KILMINSTER: That would have required a waiver, no, sir.

CHAIRMAN ROGERS: Would that have made any difference to you, if you had known that?

MR. KILMINSTER: I don't believe so. We would have reported the information, the engineering evaluation that we had that evening, I think in the same fashion that we did.

CHAIRMAN ROGERS: What would have required you to vote differently, as far as the weather is concerned? If the temperature had been a lot colder, would you still have changed your vote?

MR. KILMINSTER: I've asked myself that same question. It is a difficult question to answer, of course. But I think that if I understood that the temperature was going to be in the low 20's, that I may indeed have changed my mind, but I have to say——

CHAIRMAN ROGERS: You would have changed your mind twice, because you changed your mind once.

MR. KILMINSTER: Well, if we would have known in the first place—

CHAIRMAN ROGERS: You wouldn't have changed

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your mind, I guess, since the answer?

MR. KILMINSTER: If we would have known in the first place it was in the low 20's.

CHAIRMAN ROGERS: But didn't the fact that you had these concerns, the papers show, you were terribly worried about it? And, it also shows you didn't really have any knowledge about what would happen if the weather became colder at that time, didn't you? At the time you made the decision, wasn't that a terribly difficult decision for you to make?

MR. KILMINSTER: Again, the information that we had available to us on the sub-scale work that had been done recently before that, the fact that there was no direct correlation with the full-scale data and the limiting analytical work that had been done, we used that as the basis—I used that as the basis for my recommendation.

CHAIRMAN ROGERS: Is it fair to say that when Mr. Boisjoly and others said that launching under those conditions on that date, January 28th, that you were exceeding your data base? Would you agree with that?

MR. KILMINSTER: Specifically, for the SRM? That's true, yes, sir.

CHAIRMAN ROGERS: So, you were willing to

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exceed your data base to get this launch going?

MR. KILMINSTER: Yes, sir, as we have been from the first launch on. Every launch, of course, extends the data base to a certain extent.

DR. WALKER: I thought the data base should be established by tests and you shouldn't exceed the date base in your operations until you understand from tests or analysis or whatever that it is safe to proceed.

GENERAL KUTYNA: Bob Rummel, you're in the airlines. Do you exceed the data base every time you fly passengers?

MR. RUMMEL: No, sir.

DR. WALKER: What's the purpose of testing, then?

MR. KILMINSTER: Your first comment had to do with testing, and then you eventually said analysis.

DR. WALKER: Test or analysis.

MR. KILMINSTER: And we had indeed run analyses early in the program to evaluate the effects of a cold soaked motor at 40 degrees and a warm soaked motor at 90 degrees. That was analysis. The system had agreed that we would not test or attempt to cold soak motors and conduct those tests, so analytical qualification or certification, if you will, was acceptable.

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DR. WALKER: So, you had some analytical data then, that extended your knowledge to 25 degrees or 20 or 30 degrees?

MR. KILMINSTER: No analytical data. We had some test data at 30 degrees. We also, in my mind, we have other solid rocket motors that utilize the same material in an O-ring at 30 degrees and had been qualified.

MR. SUTTER: But, wasn't your main thrust on the testing, to make sure that the engine had enough thrust and that most of the concerns over the temperature was the temperature of the propellant, to make sure the thrust was right? Is there much of a data base analyzing the joint at 28 degrees, 40 degrees or 50 degrees?

There is not a hell of a big data base.

MR. KILMINSTER: That is correct. There is not.

DR. WALKER: How could you proceed, then, if you didn't have either the analytical or experimental data base with the joint?

MR. EBELING: I have something I can add.

DR. WALKER: Well, let's just respond to the question.

MR. EBELING: We did that 40 degree test data.

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DR. WALKER: But we're talking about 30 degrees, right. We're talking about even colder than that.

MR. EBELING: We had a statement of fact. We had 40-degree test data on the same elastomers.

DR. WALKER: Let's talk about 30 degrees. What did you have there?

MR. EBELING: We had on the 30-degree regime, we had information on one of the components called the safe and armed device, and that's the only thing I'm aware of.

DR. WALKER: So, you didn't have anything at 30 degrees?

MR. KILMINSTER: We also had information on the steel at 20 degrees from a fracture mechanics standpoint.

DR. WALKER: But you didn't have anything on the O-rings?

MR. KILMINSTER: On the seals, we relied upon the material capability, which was stated.

DR. WALKER: What material capability are you talking about?

MR. KILMINSTER: We're talking about the O-ring material capability that's identified in the mil spec.

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DR. WALKER: The mil spec says specifically that the O-ring has to be qualified for each use, doesn't it?

MR. KILMINSTER: It does say that.

DR. WALKER: So, you didn't have anything?

MR. KILMINSTER: In that sense, that's correct.

DR. WALKER: So, you were not within either your analytical or your experimental data base in proceeding to launch?

MR. KILMINSTER: On this specific program, no. As I mentioned, we have other programs where it has been tested.

DR. WALKER: Do you think that your colleagues in NASA understood that?

MR. KILMINSTER: Yes.

DR. WALKER: Do you think they understood that too?

MR. KILMINSTER: I believe they did.

DR. WALKER: So, everybody knew that they were proceeding outside of your knowledge?

MR. KILMINSTER: Specific testing at 30 degrees?

DR. WALKER: Testing or analysis.

MR. KILMINSTER: I believe so.

DR. WALKER: Then, why did you do it?

MR. KILMINSTER: On the basis of the best

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information we had available at the time, we did have this testing data, as we mentioned, at 30 degrees on a subscale that did function without any leakage indicated.

MR. SUTTER: Do you think that knowing what everybody knows now, how much credence should really be put into the subscale testing device that is fired with—that is ten inches in diameter and does not take into account all of the manufacturing tolerances, the usage effects, the fact that for instance even this metal yields a little bit at the loads imposed on it, even in the pressure tests, due to the firing?

Could you draw the conclusion today with what is known that the kinds of subscale testing that had been used to qualify these large solid rocket boosters is way inadequate to prove safety?

MR. KILMINSTER: Specifically in regards to this seal, I absolutely agree with that based upon what we've learned since the accident.

MR. SUTTER: I would like to put that in the record, because that is a finding that I'd like to arrive at. I think one of the things that was missed is, subscale testing is inadequate and I know it is expensive to run full-scale tests with a gadget 12 feet in diameter and 100 feet long, but to continue without doing it, no matter what it costs, I think is a mistake

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and I know we're not drawing conclusions here but I feel so strong about that one, I would like to get it at least in the written record so it is addressed before we're done with this thing.

GENERAL KUTYNA: Joe, you're going to have it in the record twice, now that you've said it, and the second time by our independent tests. We've looked at the tests and essentially recommended the same thing.

CHAIRMAN ROGERS: Who in Thiokol is working on the new design?

MR. BOISJOLY: I am.

MR. RUSSELL: I am.

MR. EBELING: I am.

CHAIRMAN ROGERS: Do you all agree with the comment Mr. Sutter just made?

MR. BOISJOLY: Absolutely, that blow-by rig was a static rig and it was never intended to show sealability of a joint.

DR. WALKER: Is this the same thing?

MR. BOISJOLY: He's referring to the blow-by rig, the rig data that was used on the charts that night, was strictly a blow-by rig. It is not a dynamic rig. It never was intended to be a dynamic rig.

DR. WALKER: This was the 30 degree?

MR. BOISJOLY: Yes, it is, and it was never

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intended to show sealability. It was to show blow-by.

DR. WALKER: Well, why was it raised, for the test, for this purpose? And I wasn't asking you that question specifically.

MR. BOISJOLY: It was put into the presentation that night because of a series of disjointed efforts under an umbrella by different people, trying to put together a presentation without a chance before presentation to go in and polish.

If I had my druthers, I would have taken that thing out of there after the fact because it adds absolutely nothing to the presentation that night.

DR. WALKER: That was our impression.

CHAIRMAN ROGERS: Mr. McDonald, are you working on the new design?

MR. McDONALD: Part-time, when I can. I have had some ideas that I've turned in on how to fix it.

CHAIRMAN ROGERS: Has your assignment changed since the accident?

MR. McDONALD: Yes.

CHAIRMAN ROGERS: What are you doing now?

MR. McDONALD: I used to be the Director of the SRM project. I have a title now called Director of Special Projects. The people that all worked for me work for somebody else. I am involved in reviewing some of

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the failure data that is provided to me and coming up with some ideas on how to fix it, and define a test vehicle that will give us meaningful information which is a full-scale hot-fire test.

CHAIRMAN ROGERS: Were you given any reason for the change in assignment?

MR. McDONALD: Well, that is my second change in assignment since the hearing started. My first change after the 25th—well, the 14th of February testimony in Florida, I was pulled out of my position and given the assignment of scheduling.

CHAIRMAN ROGERS: Was any reason given?

MR. McDONALD: No reason other than that is what I was going to do and my people again were put aside and assigned under somebody else. And I wasn't to be involved.

CHAIRMAN ROGERS: Who notified you to that effect?

MR. McDONALD: Mr. Cal Wiggins. He was general manager of the Space Division at that time.

CHAIRMAN ROGERS: Who?

MR. McDONALD: Mr. Cal Wiggins.

CHAIRMAN ROGERS: Were your people given a different assignment too?

MR. McDONALD: They were put under another

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individual, in fact, one that used to work for me, and for a while I was under him too, during that time.

CHAIRMAN ROGERS: What was your second assignment?

MR. McDONALD: Well, when Mr. Dorsey came in and took over as the new general manager of the space division, and Mr. Wiggins was made his assistant, he gave me the assignment as Director of Special Projects reporting directly to him rather than to Mr. Wiggins.

CHAIRMAN ROGERS: What do you do in that capacity?

MR. McDONALD: In that capacity, he told me I can work on reviewing some of the information that has been provided on the failure analysis generated at Thiokol and some at Marshall, and to help Mr. Pelham in coming up with a test article for the recovery program, full-scale type test article for the seals, and to feel free to make what recommendations that I might want to relative to improvements in the program.

CHAIRMAN ROGERS: Mr. Kilminster, did you concur in the decision to change Mr. McDonald's assignment?

MR. KILMINSTER. My assignment was also changed, Mr. Chairman. I've been located at Marshall Space Flight Center, working on the investigation team.

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That was done while I was down at Huntsville, working there.

CHAIRMAN ROGERS: Do you have any reason, Mr. McDonald, to think that you were given another assignment because of the testimony you gave before the Commission?

MR. McDONALD: Yes, I do. I feel that I was set aside so that I would not have contact with the people from NASA again because they felt that I either couldn't work with them or it would be a situation that wouldn't be good for either party, and so I was taken out of the failure analysis work that I was doing at Huntsville prior to that assignment.

CHAIRMAN ROGERS: So, you were in effect punished for being right?

MR. McDONALD: I feel I was.

CHAIRMAN ROGERS: Mr. Boisjoly, your assignment hasn't changed at all?

MR. BOISJOLY: In one respect yes, in one respect no. I have been designated as seal coordinator. I have been preparing a lot of information for input. But I too have been put on the sideline in that loop with relationship to the customer.

CHAIRMAN ROGERS: Do you feel that may be in retaliation for your testimony?

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MR. BOISJOLY: I think that is a possibility, a distinct possibility.

CHAIRMAN ROGERS: How about any other engineers who testified?

MR. BOISJOLY: That I have personal knowledge?

CHAIRMAN ROGERS: Yes, any change in their assignment?

MR. BOISJOLY: No, not that I have personal knowledge of, no.

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CHAIRMAN ROGERS: How about Arnie Thompson? Is he in the same assignment?

MR. BOISJOLY: Arnie is in the same assignment.

CHAIRMAN ROGERS: Does anybody from Thiokol want to comment on what Mr. McDonald or Mr. Boisjoly said?

MR. EBELING: I'm Bob Ebeling. Prior to this effort I was involved with the night of January 27th. Brian Russell worked for me at that time, and he basically does now. But the Seal Task Force, to give you an instance, was under my auspices as a manager at that time prior to the incident, and since then it has been—that Seal Task Force has been dissolved and has been reorganized under a different manager. My helper here, Brian Russell, now has been assigned to this new manager.

It is just one of the anomalies that has gone on since then, and I think it has got something to do with testifying—it isn't all bad, because there are several of us that already have got a lot of work, and there are some people that don't have as much work, therefore, because the program has been brought to its knees.

These are very qualified managers and qualified people, so therefore why not take advantage of them and redelegate some of this workload to these other

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people? At first when it happened I was very upset, and I challenged the new general manager, Ed Dorsey, with this.

He explained to me that, hey, Bob, you've got more than you can handle anyway. This other fellow, he doesn't have nothing now that we've brought his program to its knees. Why not give this portion of it for him to manage? At the time it seemed logical.

MR. KILMINSTER: Mr. Chairman, I would also like to comment that since the new general manager has come on board, Mr. Dorsey, there has been a basic organizational concept change, structure change, in that we have engineering now who in the past had been in a support organization, supporting all the programs in the plant, including SRM.

Now we have identified specific individuals from that core organization, and they now report directly into the Space Shuttle SRM program. So in that concept or context, there has been some shifting of responsibility in the engineering staff.

CHAIRMAN ROGERS: Well, having said all of that, you know what I'm driving at. I mean, if it appears that you're punishing the two people or at least two of the people who are right about the decision and objected to the launch which ultimately resulted in

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criticism of Thiokol and then they're demoted or they feel that they are being retaliated against, that is a very serious matter.

It would seem to me, just speaking for myself, they should be promoted, not demoted or pushed aside. Do you want to comment on that?

MR. KILMINSTER: There was certainly no demotion involved that I know of.

CHAIRMAN ROGERS: Well, you heard what Mr. McDonald said.

MR. McDONALD: I was not demoted. They just took my people away and gave me a more menial job as far as I was concerned.

CHAIRMAN ROGERS: All right, I'll withdraw the use of the word "demoted". It sounds as if you were demoted.

MR. McDONALD: I felt like it.

MR. KEHRLI: I have a question for Mr. Russell and Mr. Boisjoly on a document, tab 32 in the book, which is the August 9, 1985, letter to Mr. Jim Thomas at Marshall from you, Mr. Russell.

MR. RUSSELL: Yes.

MR. KEHRLI: Could you explain to the Commission what the purpose of that letter was and what the genesis of the letter was, what you were trying to

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do there?

MR. RUSSELL: In the Problem Review Board telecon we had a specific or we received a specific question or two specific questions from Jim Thomas. He wanted a specific answer to those, and he asked us to submit that in writing.

So I talked with Mr. Boisjoly, who does not attend that Problem Review Board meeting, and I wanted his expertise in drafting the letter, which I got and submitted it, and it was in response to a direct request to send in a written answer.

I don't know what the motivation was at the head of that.

MR. KEHRLI: The letter refers to resiliency tests. What resiliency tests were those, sir?

MR. RUSSELL: Those were the ones that were performed in the spring of 1985 that were done with a fixed rate as opposed to a variable rate test. They were done at 100, 75 and 50 degrees to understand the O-ring's response in a nonpressurized condition, because there was some concern with the joint rotation; that even though the O-ring dimensionally should still have squeezed or maybe a timing function there or a time for that O-ring to recover where there is no squeeze.

That was the purpose of these tests, and that

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was pretty much—well, it was driven by the inspection results that we saw on the 51-C flight and in January, and I guess that we inspected in late January or early February.

MR. BOISJOLY: If you look at the answer to the second question, you'll find that is the form of the chart that was used the night before the launch, and that was the chart. There was the chart that was prepared for the August 19th presentation that Al gave to headquarters.

MR. KEHRLI: In conversation with our staff investigators, Mr. Boisjoly, you indicated that this was a new theory or there is a difference between the old theory on the timing function and the new theory on the timing function, or that this letter somehow was a different way of looking at the timing function.

Could you explain that, please?

MR. BOISJOLY: Yes. What had happened is that we had made a presentation as a result of SRM-15, soot past the O-ring, which was the coldest flight at that time, the next Flight FRR we presented that information.

There was a curve in that presentation, that we broke it up into zones A, B and C. When I found out Al was going down to give the presentation at Washington

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in August, I was trying to rework that information into a more concise form and to try to give probabilities to those zones. I'm the one that selected zero to 170 milliseconds as the basis of looking at that curve and said that at those pressure levels we would have a high probability of sealing.

Then I selected the 170 to 330 and from 330 to full ignition and assigned probabilities to those. That's when Al went down, and that was to point out the concern that if we were always in the initial phases we had a very good p obability of success. If we moved into the next region, you lessened that probability. If you moved the event into a third region, you increased the probability towards a failure, and that was the whole impetus of the whole presentation.

DR. WALKER: What did you mean by high probability?

MR. BOISJOLY: Well, for instance, if you have a secondary seal lifted off the sealing surface in the later timing functions and you had crippled the primary O-ring, you would not have had the capability to pressure actuate the secondary O-ring when you take resiliency into consideration.

Prior to this effect we had always termed it in terms of geometry. We always talked of the geometry

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of the gap opening in relationship to an O-ring that always had the capability of following in the seal surfaces and being pressure actuated.

But when you take that same group of data which was in this time frame and apply the resiliency data to it, as we knew it at that time, on a limited number of tests, that said that if the O-ring did not have the ability to follow the loading surfaces as they were pulling away as the gap was opening, the longer the period of time that you were in that regime the less chance you had to seal.

So I tried to define it in a three zone region, to say a high probability, a moderate probability, and a low probability.

DR. WALKER: I understand that By high probability did you mean 95 percent or 75 percent?

MR. BOISJOLY: In the beginning on the basis of the limited test we had, I felt there was 100 percent if it happened within the first 150, 170 milliseconds because there would be an impingement problem and then the margin would truly be a margin of impingement and not blow-by.

DR. WALKER: But you didn't use the word likely. You used high probability.

MR. BOISJOLY: Yes.

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DR. WALKER: Which implies that there is some chance.

MR. BOISJOLY: Well, there always is.

CHAIRMAN ROGERS: Mr. McDonald, as I remember your testimony in public session, it was to the effect that you refused to sign the telefax or refused to go along, and normally when you were at Kennedy you were the one that would have signed. Is that correct?

MR. McDONALD: I said I hadn't had an experience and wasn't aware of one where anybody was ever asked to sign anything, but I felt that was my responsibility if it ever came up there because I am the senior official for Thiokol.

CHAIRMAN ROGERS: Mr. Kilminster said, as I recall, he normally would be the one to sign that and that seemed to kind of conflict with your testimony. Is that what you testified to, Mr. Kilminster?

MR. KILMINSTER: I think what I said, Mr. Chairman, was that that whole business was unusual in the sense that we were talking about something being signed the night before the launch. However under other circumstances the piece of paper that is signed to identify flight readiness, as far as Morton Thiokol is concerned, that is a document that I normally signed.

CHAIRMAN ROGERS: Even when Mr. McDonald is

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there?

MR. KILMINSTER: Yes.

CHAIRMAN ROGERS: Is that correct, Mr. McDonald?

MR. McDONALD: That is true for every formal Flight Readiness that we have prior to the launch. The reason I was there and Joe wasn't there is we alternate doing that. In case something comes up after the last formal review, and the last formal review is the L-1 Review, it is signed by anybody and if anything comes up after that then that's why I'm there and the other contractors have people like me there who can resolve those issues and are responsible for going ahead with the launch.

As I said in my testimony, Jesse Moore makes an oral poll subsequent to that L-1 Review because in many cases there are items brought up at the L-1 that are finally resolved, and then makes a poll to ask each of the contractors if they are ready to fly because now everything is supposed to be in.

I'm the one that has to answer yes, Thiokol is ready to fly. If something happens after that time, it is my impression the reason I'm there is that I'm the guy that's going to have to get up and say yes, we are ready to fly.

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I have never been in a condition where we ever had to sign anything, and I don't think there was ever any condition where we had to sign anything after L-1. I expected, because of this telecon, that I would have to do that because I felt that I had the same responsibility at the launch site as he had when he was there or I shouldn't even take the time in going.

CHAIRMAN ROGERS: You told them that you would not sign?

MR. McDONALD: I told Mr. Mulloy I wouldn't sign and that it would have to come from the plant. Now, he didn't ask me to. I just told him I wouldn't do that because I felt that I was going to have to do that. That was my responsibility. That's why I was there.

CHAIRMAN ROGERS: Did you know that, Mr. Kilminster?

MR. KILMINSTER: No, I did not.

CHAIRMAN ROGERS: You didn't know that he had said he wouldn't sign?

MR. KILMINSTER: No, sir.

CHAIRMAN ROGERS: Why did you think that they required your signature in this case? You said you had never done it before.

MR. KILMINSTER: Because we had not had a

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later than L-1 identified problem.

CHAIRMAN ROGERS: Why would it have to be in writing?

MR. KILMINSTER: I guess the only thing I can say is I was not surprised when Mr. Mulloy asked for that, but again the whole thing was unusual.

CHAIRMAN ROGERS: I'm interested in why you weren't surprised if you had never done it and you had a man there that normally would orally approve this. Why weren't you surprised when they asked you to sign it?

MR. KILMINSTER: I guess I can't comment other than what I've said.

CHAIRMAN ROGERS: Didn't it occur to you that they might be wanting to put the monkey on your back in writing?

MR. KILMINSTER: Well, the monkey is always on my back under normal circumstances.

CHAIRMAN ROGERS: Not normally. It would be on the man who said it.

MR. KILMINSTER: Well, again, I go back to the formal paper that is signed for the flight readiness review sequence, and it normally is my name that is on there unless I am gone at the time that signature was to be made, in which case Mr McDonald or someone else signs it.

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CHAIRMAN ROGERS: In any case, the fact that they never asked you to do anything like that before and that Mr. McDonald was there when normally he would be authorized to say okay, that didn't strike you as particularly alarming at all?

MR. KILMINSTER: Well, the rationale came from the plant. The information that was presented came from the plant, and that is where I was, is at the plant.

MR. HOTZ: I wonder if we could poll the Thiokol delegation as to whether they were or were not aware that a launch constraint was being waived on a formal waiver for each of the flights after 51-B.

MR. EBELING: I was not aware.

MR. KILMINSTER: I was not aware.

MR. RUSSELL: I was not aware.

MR. McDONALD: I was not aware.

MR. BOISJOLY: I was not aware.

MR. HOTZ: Thank you.

DR. WALKER: Let me just follow up on that if I might. Although you weren't aware that there was a formal waiving of the launch constraints, you were aware that anomalies from previous flights had to be explained satisfactorily before the next flight could occur.

As I understood Mr. Mulloy this morning, that was based on the analysis of the amount of damage that

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was acceptable and the amount of erosion that was acceptable versus the amount of erosion which had been seen.

There were some models used to predict the amount of erosion which was to be expected. As Dr. Feynman has pointed out, these models really were not precise models. There is a considerable amount of variation, because all of the parameters were beyond your ability to measure or know, so on what basis did you feel the erosion could be predicted sufficiently well to allow the flight to be continued? What models, what data, what analyses?

MR. McDONALD: I would like to try to respond to that. Dr. Salita developed those models at my request, I guess, starting in the spring of 1985 and published two different parts of that.

At that time, prior to the problem we had on 51-Baker, 16-A we had not seen any erosion other than direct impingement on the seal, and that is what his model was based on.

When we recovered that hardware and saw that we had violated the primary seal, we immediately were very concerned because his model, being a jet impingement model, is a function of time, and it has to have a forcing function which is the delta pressure between

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the chamber and the cavity.

That's a function of ignition time, and so it does have a limit within the model that you can't get any further erosion because there is no other time in the pressure trace, the pressure trace of the motor, that you get that kind of a condition.

Based upon that, we thought we had a limiting model. However, there are a lot of unknowns as far as the fidelity of that model is concerned, but his predictions, models in five-inch tests that we ran and fairly good engineering terms to define how accurate it was and within about a 12 percent spread, he predicts all that data very accurately.

Now, the amount of erosion that we had, we then went and tested to see where we in the real world lose the O-ring relative to what we think would be the maximum with our model, and we did that two ways.

The first way we took an O-ring and actually sliced pieces out of it just like we figured it would be eroded away until we could no longer hold it as a seal. In doing that we found out that we could lose nearly two-thirds of that O-ring before we lost seal. That was cold, a cold gas test.

GENERAL KUTYNA: Was that a dynamic test?

MR. McDONALD: No, that was a static test.

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GENERAL KUTYNA: No rotation of the joints?

MR. McDONALD: Right, no rotation of the joints.

GENERAL KUTYNA: Did you feel that was valid in that case?

MR. McDONALD: We didn't feel it was totally valid, and so we wanted to run some hot scale tests because you eroded differently as well during hot scale, and so we then took some hot scale tests of our motors and focused the jets so there were more and more higher heat transfer rates to keep eating away the O-ring until it failed.

We found that in the evolution of that test series that we could erode up to 125 thousandths, and we had a lot of tests there and below that we never saw a seal ever fail. We went up to 150 thousandths of the material removed, and it still sealed.

However, we had two tests, one at 145 and one at 160, that did fail. Now, both of those failed the primary seal and eroded something like less than 10 thousandths off of the secondary seal.

Now, these joints did not rotate but they did show the sensitivity and the capability of the O-ring to jet impingement erosion. If you took our worst measured erosion on the O-ring relative to what it took

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to really fail it, it was nearly a factor of three to one.

Recognizing the fidelity of the math model is not real good, we did not feel it was that bad at three to one, and as long as we could retain the secondary seal during a good portion of the erosion time period, we felt good. Our concern was if we ever got it past the time that we could have a good secondary seal. That was our concern.

The meeting that occurred on August 19 came about as a result of this problem with the nozzle eroding through, and that is what drove that meeting. Headquarters wanted to hear about that. We lost the primary seal and eroded some secondary.

We all sat down together and got together with the engineering people and put together that presentation and collectively said, you know, we ought to address the whole seal issue, not just that failure, because we all felt that if that ever happened in the field joint we were in bad trouble because the nozzle has a much better secondary seal than the field joint does.

We decided we would put it together as a total pressure seal presentation and highlight the field joint even though there was a nozzle joint that caused the problem that

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apparently drove the restriction to launch I was unaware that we retained.

MR. SUTTER: At this meeting on August 19th at headquarters, that was called because of Thiokol's concern that the joint was really in trouble?

MR. McDONALD: No, it was called—we had had another meeting scheduled at Washington headquarters at that time that had a problem with the mixer fire earlier in the year, and there was a review of that

I believe Mike Weeks either called Joe or I or one of us and said well, you're here, you ought to come and address a couple of other issues that have happened recently that we are very interested in.

One of them is we had broken the structural test article on the filament wound case I believe in July down at Marshall, and they wanted to hear about that.

The other one was they were made aware that we had violated the primary seal in the nozzle and wanted to hear about that and what our rationale was to continue.

So then I called Mr. Mulloy and told him that we had been requested for this presentation, and he said we're going to have to review that with them before we can go to Washington, which is the normal sequence of things, and so we were prepared to have to go down to

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review it with the Marshall people.

We finally held that on a telecon and faxed down all the charts that we planned to take to Washington and reviewed it with all the Marshall people before we went to Washington. He came up and met us here in Washington.

MR. SUTTER: What was the tone of the meeting in Washington? That it was a serious problem or just business as usual? Apparently it wasn't the primary subject on the agenda.

MR. McDONALD: It was not the primary subject on the agenda.

MR. SUTTER: There was another meeting in Washington, too, wasn't there, on this problem? Weren't there two meetings there in the fall of 1985?

MR. McDONALD: Well, I'm not aware of the other meeting.

MR. SUTTER: Well, what I'm curious about was NASA headquarters fully informed about or what was their attitude based upon what they heard from your or anybody else as to how serious this joint problem was?

MR. McDONALD: Well, I think they felt it was serious. They felt that we did a good job of focusing the history of that problem; where we were at, what we understood about it, what we felt we needed to do to fix

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it, because we not only addressed the history, I think, and we focused it quite well.

MR. SUTTER: Would it have put them in the frame of mind that when another launch came up they would ask how is this joint for this flight? Is it okay? Or would they be that concerned?

MR. McDONALD: I don't think so, unless the previous flight showed an erosion problem or something different than they had heard. They, I think, felt that we needed to establish this task force in support of that which we had had in place about that time to put forth a focused effort on trying to resolve the problem because they felt it was the same problem. It didn't matter whether it was in the nozzle or the field joint.

MR SUTTER: Then apparently your presentation to them is a lot less or a lot more cooled down than some of the internal paperwork as to the severity of this problem.

MR. McDONALD: Well, I'm frankly surprised that this piece of paper here generates as much interest as that presentation, because that gets lost in the stack of those that's probably that thick (indicating).

CHAIRMAN ROGERS: Well, I'm not sure when you say more interest, we're just trying to complete the record. I don't think you should judge our relative

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interest. I mean, we're pretty interested in all aspects of this, and we wanted to find out more about this piece of paper because we had never been alerted to it.

No one told us before. We asked NASA and everyone else to be sure to let us know everything there was that might relate to this, and we discovered this ourselves. So we're just asking about it.

I'm going to first let Gene go ahead, and then I have a question.

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DR. COVERT: I had several questions on the sequence. I guess, Mr. Kilminster, I will ask you first.

Were you involved in the original design of the SRM and the joints in any way?

MR. KILMINSTER: Yes, sir.

DR. COVERT: Was it at that time the goal of the design to protect the integrity of the O-rings against thermal distress of any kind?

MR. KILMINSTER: Yes, sir, that's always been the goal.

DR. COVERT: In view of that, do you feel that as the problem has progressed and you now live with a situation that you hadn't anticipated at all, does this give you a feeling of discomfort that this is not the way O-rings ought to be treated?

MR. KILMINSTER: Yes. I believe that especially since this recent testing has been done that we have learned quite a bit from the subscale testing that has been done of higher fidelity than we were doing before, and I think we have learned a lot more about that joint. I think we have learned a lot more about the effect of squeeze on the O-ring. And there has been a move, if you will, from the time that this joint was initially designed, to getting rid of the putty, if you

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will, getting rid of the thermal barrier, the protection.

I believe that perhaps in some motors and in some circumstances, maybe even in the field joints of our motor, that might be appropriate, where you can convince yourselves you're not going to have circumferential flow or a replenishment of the heat, if you will. And that is something we will have to take a very quick look at and deep look at.

DR. COVERT: Have you ever been associated with the use of O-rings in a rocket before where there was continued and repeated thermal distress to the O-rings?

MR. KILMINSTER: Well, usually, Dr. Covert, you don't get the opportunity to look at fired hardware to the extent that we have here.

DR. COVERT: How about Mr. Boisjoly, were you involved in the original design of this joint?

MR. BOISJOLY: No, I was not. I came to work at Thiokol in 1980.

DR. COVERT: Mr. Ebeling?

MR. EBELING: Yes. I was.

DR. COVERT: Does your feeling about the use of O-rings coincide with Mr. Kilminster's?

MR. EBELING: No, it doesn't.

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DR. COVERT: Do you want to tell me the difference?

MR. EBELING: Well, I am a hydraulics engineer by profession, and O-rings and seals in hydraulics are very sacred, but for the most part, a hydraulics or pneumatics engineer controls the structure, the structural design, the structural deformation to make sure that this neat little part that is so critical is given everything it needs to operate. In solid rocket motors I have been there now pushing 25 years. They had a different attitude on O-rings when I came there, and it is not just Thiokol, it is universal.

DR. COVERT: By universal, you mean the solid rocket industry?

MR. EBELING: The entire solid rocket industry. It gets around from one, the competitors' information eventually gets to me by one track or another, and mine to theirs, but my experience on O-rings was and is to this date an O-ring is not a mechanism and never should be a mechanism that sees the heat of the magnitude of our motors, and I think before I do retire, I'm going to make sure that we continue to fly with round seals which I am against round seals anyway. I think seals with memories, not pressure-activated, but energized through mechanical means, and in all cases,

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keep the heat of our rocket motors away from those seals. Whatever it is, you do not need chamber pressure to energize a seal.

DR. COVERT: In this regard, then, did you have an increasing concern as you saw the tendency first to accept thermal distress and then to say, well, we can model this reasonably and we can accept a little bit of erosion, and then etc., etc.? Did this cause you a feeling of—if not distress, then betrayal in terms of your feeling about O-rings?

MR. EBELING: I'm sure sorry you asked that question.

DR. COVERT: I'm sorry I had to.

MR. EBELING: To answer your question, yes. In fact, I have been an advocate, I used to sit in on the O-ring task force and was involved in the seals since Brian Russell worked directly for me, and I had a certain allegiance to this type of thing anyway, that I felt that we shouldn't ship any more rocket motors until we get it fixed.

DR. COVERT: Did you voice this?

MR. EBELING: Unfortunately, not to the right people.

DR. COVERT: How do you feel about inspectability of this kind of a sealing device?

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MR. EBELING: Like the ones that—I think the pressure testing that we do, the leak pressure testing is the only practical method that we can do for this type of a seal. There is a possibility you might invent an N-ray system to rotate the motor and make sure everything is all right by N-ray, because N-ray is opposite of X-ray. It washes out metal, and elastomers and plastics show up real keen.

DR. COVERT: N-ray is neutrons?

MR. EBELING: Neutrons. Such a system might be devised, but it would be very expensive, but the pressure testing and the inspection of the joint the way we are doing it is probably the only practical way of doing it.

MR. SUTTER: Do you think that pressure testing it 28 days before the rollout and the thing sitting out there with the temperature working on it——

MR. EBELING: No problem. You're from Boeing. I am sure your people would say the same thing, no problem.

MR. SUTTER: Well, maybe some people from Boeing would say it. I still have reservations. When you take into account——

MR. EBELING: Then quit flying. You had better quit flying.

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MR. SUTTER: Wait a minute, now. We've got four systems flying everything in our bucket, and a single failure doesn't do anything except give you a yellow light. There's a difference.

MR. EBELING: It makes you tense though.

CHAIRMAN ROGERS: You said you didn't voice your views to the right people.

Did you voice your views to anybody?

MR. RUSSELL: I heard them.

MR. BOISJOLY: I heard them.

CHAIRMAN ROGERS: Did you put it in writing at all, any of your views?

MR. EBELING: No, not exactly, not in the words I just told you.

MR. BOISJOLY: I think I can express the fact that he must have expressed it at least two or three times a week during the times of November and December in our seal meetings.

CHAIRMAN ROGERS: I want to make a comment to Mr. Kilminster, I guess, but to the company as a whole. I am very upset about the testimony Mr. McDonald gave. It's a very serious matter. In this kind of an accident where people come before a Commission and tell the truth and then they are treated as he believes he has been treated, which obviously is in some way punishment or

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retaliation for his testimony, it is extremely serious, and the whole idea of the program is to have an openness and to have an honest exchange of views.

And in this case, Mr. McDonald and Mr. Boisjoly and others, Mr. Thompson and others, were right. If their warnings had been heeded that day and the flight had been delayed, there's no telling what would have happened. We might never have had the accident. And to have something happen to him that seems to be in the nature of punishment is shocking, and I just hope that you convey that to management.

I don't know how the others feel, but that is how I feel. I would think you would want him in all of your discussions, and Mr. Boisjoly and he shouldn't be treated that way. He should be treated the other way, that he was right and you were wrong, and others who changed their decisions were wrong, and they were right, and to have something that seems to me to be in the nature of punishment is very, very distressing, and I just wanted you to know that.

There may be other questions.

GENERAL KUTYNA: I had one question. The briefing on the 19th in Washington, Mike Weeks was there.

Who were the other folks who were there, do

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you recall?

MR. McDONALD: I had a list at one time.

GENERAL KUTYNA: Could you send me that for the record?

MR. McDONALD: Yes.

GENERAL KUTYNA: Secondly, there has been some question that people understood that there was a temperature problem. I remember your conclusions chart, your file chart, and the very first bullet of that chart had the word "resiliency" in it.

Do you feel when you talked about resiliency at that meeting people got the connection between resiliency and temperature, that resiliency was a function of temperature, or was that lost?

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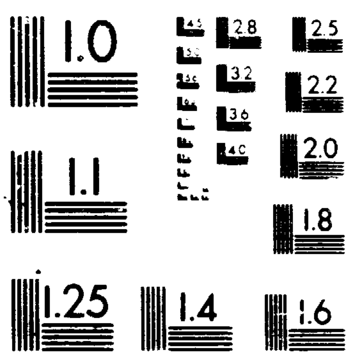

MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS
STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

MR. McDONALD: It may have gotten lost because we hadn't run a very long range of temperatures when we got that data.

GENERAL KUTYNA: So it is possible that people at headquarters from that briefing did not understand temperature was a concern?

MR. McDONALD: I guess it is possible they could have.

GENERAL KUTYNA: Is it probable?

MR. McDONALD: I don't know if it is probable, because we put it as the first bullet of why

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we thought that was our highest concern, and if that hadn't have happened, we wouldn't have had that concern.

DR. KEEL: Could I follow up on that, just one quick question?

In fact, in your 51-E flight readiness review after 51-C, which, Mr. Boisjoly, you have testified was the worst case of blow-by, and in fact, you drew a correlation to temperature in that worst case, the conclusion of that that was briefed to Marshall at the project level, as we discussed this morning in testimony, in fact says that explicitly. It says STS-51C, consistent with erosion base, and then it says sub-bullet under that conclusion, low temperature enhanced probability of blow-by. STS 51-C experienced worst case temperature change in Florida history.

Now, that is an unequivocal reference to temperature and the fact that it makes things worse.

Can you explain, and perhaps you, Mr. Kilminster, since you had testified again today that you sign off on these flight readiness reviews, how it was that that information never got up to headquarters, or did it get to headquarters?

MR. KILMINSTER: I believe that that information was a direct quote from a flight readiness

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review statement that we made for acceptability to fly. From that point on, that's up to Marshall Space Flight Center, what they take forward.

DR. KEEL: Did you sit in on the Center board briefings of that flight readiness review?

MR. KILMINSTER: Either Al or myself, and I don't recall specifically on that one.

DR. KEEL: Al, did you sit in?

MR. McDONALD: Joe sat in as a member of the Center board. I stayed there because I gave the briefing you just read from.

DR. KEEL: So you stayed at the Center board briefing? So you were there at the Center board briefing?

MR. McDONALD: Yes.

DR. KEEL: Who briefed that?

MR. McDONALD: Mr. Mulloy.

DR. KEEL: And Mr. Boisjoly, were you there?

MR. BOISJOLY: Yes, I was.

DR. KEEL: Now, was it any of your recollections, can you recall what was briefed, and was this temperature concern or even the extensive brief you have given now on O-ring erosion problems, was that briefed at the center board?

MR. McDONALD: No, it wasn't. It was kind of

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a one-liner that we had some erosion, but it was within our experience base or something to that effect.

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CHAIRMAN ROGERS: I want to ask one more question.

When you said before that you answered my question that there was reference to how your customer would feel about you, what was the basis for that?

MR. McDONALD: Well, prior to the testimony I gave in Florida, as I indicated, I was spending full time at Huntsville, and subsequent to that time I was not allowed to go back to Huntsville. I wanted, too, to review some of the——

CHAIRMAN ROGERS: Was any reason given?

MR. McDONALD: The only reason given was they didn't think it would be in the best interests of either party that I do that.

CHAIRMAN ROGERS: Who told you that?

MR. McDONALD: I talked with Mr. Dorsey for a while on that, and he felt that I would be better off with working with problems in the plant.

CHAIRMAN ROGERS: Did he indicate anyone from NASA had said that to him?

MR. McDONALD: No, he did not. I do know that even after the recovery team started and I had submitted some ideas on how to fix the problem, before

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we went to our first formal meeting at Marshall, that one of the fixes that we proposed was one that I had developed. I wasn't asked to even go and support that.

CHAIRMAN ROGERS: Any reason given?

MR. McDONALD: No. In fact, I didn't even get a copy of the presentation. I had to go borrow somebody else's, and I also did not get copies of some of the material that was being generated in failure analysis, and I had found out in the hallway that that had been done, and so I went and found some of the data. But I got the distinct impression that it was not being sent to me on purpose because some people knew that I did not agree with all of the conclusions that were being drawn on some of the data.

MR. HOTZ: Mr McDonald, did subsequent to the accident, in your testimony before this Commission, did you receive any personal comment from people at NASA about your testimony?

MR. McDONALD: I think, I'm not sure of the exact meeting, was when part of the Commission came out, I believe it was Mr. Sutter's team, reviewed the development, qualification, certification of the SRMs. It was a two-day meeting, and the second day I was there, Mr. Mulloy came into my office and slammed the door, and as far as I was concerned, was very

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intimidating to me. He was obviously very disturbed and wanted to know what my motivation was—and I won't use his exact words—for doing what I was doing, and I asked him what's his problem? Do you mean what my testimony was? And he said no. As I understand it, you're giving information to the Commission without going through your own management, without going through NASA, and what's your motivation for doing that?

And I told him to calm down, that I didn't think I had to get a note from my mother or anyone to give anybody information, and I felt it was appropriate to give them information. And I asked him what specifically, and it was the information that I had generated on the fourth of February while I was at Huntsville when I got frustrated with regard to the failure analysis on what I thought would be some real, potential causes of the failure, that I had given that to Dr. Covert when he was there, I think before my testimony, at the end of February, and had not gone through I guess the proper channels or something in doing that, and for some reason he was very upset about that and was very intimidating. But I ignored him.

On the other hand, he said that, you know, he never was against me, and he had a high regard for my capability, and he wanted to let me know that. But I

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could see no reason for him doing what he did.

MR. HOTZ: Did you get the feeling that there might be some feeling on the part of the Huntsville people that they wanted to control this flow of information to the Commission?

MR. McDONALD: I got the feeling that that was happening from things that I was reading, the data that I was looking at, and the conclusions that were drawn, and how they were drawn, instead of focusing it, to me was making it fuzzy.

GENERAL KUTYNA: Mr. Kilminster, you are then—

CHAIRMAN ROGERS: Don, before we leave that, Mr. Mulloy, do you have any comment to make on Mr. McDonald's statement?

MR. MULLOY: Yes, sir. If you would like for me to give you my version of that discussion.

CHAIRMAN ROGERS: Why don't you?

MR. MULLOY: Yes, sir, I would be glad to.

As often happens, I guess, when we have a meeting, we don't all remember it being the same. It must have been a different meeting. I came to Mr. McDonald's door and asked him if he had a moment, and he said he did, and I then closed the door. I didn't realize I slammed it. I was not upset. I think I

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started by shaking Al's hand and said, before I say anything, I want you to know that I don't have any personal feelings one way or the other about what has occurred subsequent to the launch. However, I have a curiosity, and the curiosity I have is why you have taken the approach that you have in circumventing your own management and the customer in voicing concerns about the launch of 51-L that were never voiced to his management or to me. His response to that was that he was very upset about the way the investigation was going at Marshall when he was down there, but when he got back to HOSC, after the incident, when he came back to Huntsville, he found NASA organizing a great number of teams and laying out a broad spectrum of areas to look at, which included the external tank and the SSME and the SRB and forming teams to look at in a broad way across the total system to try and determine what caused the accident.

Al I think mentioned to me that he had obtained the information about the temperature readings on the right hand SRB from the IR gun at 9 degrees, and he was trying to introduce that into the team to which he was assigned, which was Mr. Swinghammer's SRM team, and he was very frustrated, that the team seemed to be more interested in getting a structure to look across

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the total spectrum at the possible failure causes as opposed to picking up the obvious thing, which was the low temperature on the right hand booster.

And that is the way I recall the discussion.

GENERAL KUTYNA: May I ask who directed you to look across the whole spectrum at the accident structure? Where did you get that guidance?

MR. MULLOY: I did not do that. I was not participating.

GENERAL KUTYNA: Well, let me tell you where I got—where the guidance came. Our panel gave you the guidance to look across the whole spectrum of the accident structure and look at every facet of it rather than home in on something that you might think was the conclusion.

MR. MULLOY: I think that was the proper approach.

CHAIRMAN ROGERS: But the discussion that Mr. McDonald referred to was initiated by you?

MR. MULLOY: Yes, sir, it was.

CHAIRMAN ROGERS: And what was the purpose of it again? And don't go through the whole thing, but what was the purpose? Why did you do it?

MR. MULLOY: I have known Al for some time and had worked with him for some time, and why I did it was

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based upon his testimony that he had objected to the launch and continued to protest the launch after the discussion on the night of the 27th when that was not a fact.

CHAIRMAN ROGERS: In other words, you were challenging his veracity at this point?

MR. MULLOY: I wondered why he said that when he did not pass that on to his management, nor did he pass it on to me, nor did at any time during the launch process when he was on the console during that morning did he make any comments or continue to object to the launch. As a matter of fact, as he testified, he left his console and left the loop during the launch process.

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CHAIRMAN ROGERS: So you had no particular motive to start this discussion other than to satisfy your own curiosity?

MR. MULLOY: Yes, sir. As to why he felt that there was a concern now that he did not consider it worthy of passing on to his management or to the customer.

DR. KEEL: May I ask one question, Mr. Chairman? We did give directions with respect to a broad investigation, but we never gave a direction with respect to anyone who had information that they thought should be passed to the Commission, that they could pass it directly to the Commission. There was never any direction that that should go through management, or through NASA, or through anyone.

DR. WALKER: I had a question for Mr. Kilminster. In the original Thiokol proposal, the seal was different from the present seal. It was a face seal and a joint seal.

MR. KILMINSTER: That is correct.

DR. WALKER: I wonder if you could or if you are aware of the evolution of that design and why the change was made to two bore seals, and what the rationale for that change was.

MR. KILMINSTER: As I recall, Dr. Walker,

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there were probably three things that entered into that decision. Number One was that if we maintained that configuration, as you will recall, at the end of the clevis leg, where there would be an O-ring groove, that would become a very delicate item to have on the end of the segment, and subject to easy damage. That was one consideration.

DR. WALKER: Do you mean the thickness of the walls was insufficient, you felt?

MR. KILMINSTER: No, not the thickness of the walls, the O-ring groove. There was a notch, as I recall, in that clevis where the O-ring sat and then fitted against a face that was on the tang side. That was one side.

DR. WALKER: I haven't understood the objection yet. Are you saying that the walls of the O-ring groove were too thin, or what was the problem there?

MR. KILMINSTER: Instead of having a total thickness of the clevis end, you would have a groove there where the side walls of the groove, if you will, are thin and therefore subject to damage.

MR. EBELING: We set the cases periodically on-end, and said if you would have done that you would have ruined that $100,000 plus component because you would have deformed that fragile edge, and the distance on

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that O-ring groove is what he is replying to.

DR. WALKER: But of course a face seal is a much more reliable seal. In fact, it is one that satisfies your criteria, as a matter of fact.

MR. KILMINSTER: Let me address the other two items. The analysis showed that we were going to get elongation and therefore unseating of that face seal on the order of 20 to 30/1,000ths during pressurization. That is in the wrong direction. It wouldn't maintain the seal, the squeeze, if you will, during pressurization. It would tend to move away.

And then a third consideration, and I believe those are the three main ones, was that at that time we were trying to extend the total length of those segments so that we would not have to have a weld in those segments, in that high strength D6 material, and we were exceeding what the fabricator had previously done. To put that other notch on there, to form the face for the O-ring to match against would have required more beef in the billet at that point, and there was some question whether he would be able to give us the length of segment that we desired, so those were the three main things, I believe.

DR. WALKER: Were you concerned in making that change that you were going away from the standard way in

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which O-rings were to be used? You were going away from a seal which was a positive seal, which was at least in principle independent dynamics? And I am not sure that I agree with your second point, but you were going toward a configuration which was not recommended by the seal industry?

MR. KILMINSTER: To the contrary, going the way that we did was in keeping with the existing Titan configuration with the bore seal, with all of the history that they had behind that.

DR. WALKER: I am not talking about the solid rocket motors. I am talking about the seal industry. I am talking about recommendations from Parker and other people who had developed O-rings.

MR. KILMINSTER: Subsequently we did have discussions with Parker, and initially they had the same comments that you just mentioned. Subsequently, however, they came back to us and said, with the data base that we had at that time, with the work that had been done, they could understand our utilizing the seal in that configuration.

DR. WALKER: Are those comments documented?

MR. KILMINSTER: Yes, sir.

DR. WALKER: Could you see that we get copies of those?

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MR. KILMINSTER: Yes, sir.

DR. FEYNMAN: Mr. McDonald, Marshall has been making some tests, and so have you been making some tests of the seal and so on in various kinds of jigs and small-scale and that sort of stuff. Do you have something to tell us in which you presumably disagree in some way with the conclusions or the attitudes that we have been getting, because we got most of our information directly from them and not directly from you, and I wanted to correct that, if possible, if you had something to suggest about the way we interpret the results, and have you seen the kind of results that they have had?

MR. McDONALD: Yes, I have seen, I believe, most of them. I don't know if I have seen them all. I don't know what their final recommendations—I guess my comments would be based on the last presentation I am aware of that was made on the 10th of April. I think it was made by Marshall to the task force, and the conclusions from that presentation were that if you look at the conclusions, they don't even mention temperature in there at all, but even where they do mention temperature as having an effect, that it by itself couldn't explain the problem. It had to be in conjunction with other things.

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DR. FEYNMAN: Do you mean like the seal fitting into the groove?

MR. McDONALD: That is correct, and it is obviously, I think, biased towards potential assembly problems, either through the assembly itself or contamination of things that one can't specifically prove other than that there is no indication in any of the records or any of our prior history that this was outside of that, and if you look at their own chart on the dynamic O-ring test, you can go across that chart and see at 25 degrees where they never had a single success. It was 100 percent failure in both the primary and secondary O-rings

If you go across the same chart and look at 55 degrees and up, there was not a single failure. There was 100 percent successes, and it was like 17 tests out of 17 at the higher temperatures and ten out of ten at the lower temperatures.

GENERAL KUTYNA: But, Al, if I can interrupt, the chart was presented as a compilation of your data and their data, and what is the ordinate on that chart? It's temperature, right?

MR. McDONALD: Yes.

GENERAL KUTYNA: And the bottom line of that chart is, boy, when it gets cold, you start failing. Is

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that not true?

MR. McDONALD: Well, that is absolutely true.

GENERAL KUTYNA: Then how can you say they have not considered temperature?

MR. McDONALD: Well, they considered it, but looking at that chart——

GENERAL KUTYNA: Where were the reds on that chart?

MR. McDONALD: That is exactly the point I am making. And how you can look at that chart and then not conclude—I mean, you are making my point.

GENERAL KUTYNA: I think we are saying the same thing.

MR. McDONALD: I look at that chart and I don't know how anybody can not conclude that that wasn't the major driver, if not the whole thing.

VICE CHAIRMAN ARMSTRONG: Well, Mr. McDonald, it is this Commission that is going to make the conclusions.

CHAIRMAN ROGERS: That is right. I wonder, the discussion that you had with Mr. Mulloy, the one you were talking about a moment ago, when was that?

MR. McDONALD: I can't recall the exact date. It was when Mr. Sutter's team was out to our plant.

MR. DUPREE: March the 17th timeframe.

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CHAIRMAN ROGERS: At that time Mr. Mulloy was advised he was not to have anything to do with the investigation, and not to take part in the investigation. And you testified that he advised you about how you should convey information to the Commission?

MR. McDONALD: Well, he asked me why I was doing it, what I was doing, and why I was taking the liberty of giving information directly to the Commission without going through my own management and through NASA first. That is exactly what he asked me. And it wasn't in a nice voice.

CHAIRMAN ROGERS: Do you feel now that you have had the opportunity to present as much information that you want to the Commission? We want to be sure that anybody that has any comments or information, that they have direct access to us. Do you feel that you have had?

GENERAL KUTYNA: Mr. Chairman, let me interject.

CHAIRMAN ROGERS: Well, let him answer first.

MR. EBELING: Does that apply to all of us?

CHAIRMAN ROGERS: To Mr. McDonald.

MR. McDONALD: Well, in violation of Mr.

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Mulloy's concern again I gave some information directly to the Commission without going through the proper channels. I sent a copy to Dr. Keel and also to General Kutyna on a memo that I drafted to my boss about a week ago, and I informed Mr. Garrison last night about that on my conclusions from the failure analysis from the data that I have had access to, recognizing that I haven't seen everything as to what I concluded I think happened and what may have happened, and what seems very conclusively has happened, and what I feel is necessary to make sure that we understand those things that are well substantiated and those that may be speculative.

CHAIRMAN ROGERS: Well, will you feel free to give the Commission any information you want to give us, and if you consider what Mr. Mulloy told you as a direction, forget it, because we are running this investigation, and you have access to us any time you want, and that applies to all of you.

GENERAL KUTYNA: Just for the record, sir, I learned of this possibility that some things had not gotten through early last week, and Mr. McDonald and I have had a conversation where I wanted to assure that his concerns, both his and Mr. Boisjoly's and whoever else—is that not true?

MR. McDONALD: It is true.

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MR. RUMMEL: Mr. Chairman, just in the interest of completing the record and accuracy, Mr. Mulloy did sit in on some of our meetings up at Thiokol and did participate in the discussion quite freely. I thought personally that it was generally more helpful than not. Whether he should have or not, I don't think we chose to impose any restrictions.

CHAIRMAN ROGERS: Well, to make it clear, and I hope it is clear, and I think it has been, the Commission's decision to disqualify those who might have been involved in the decision-making to launch 51-L was that they should not be responsible for matters involving the investigation. That was not to preclude them from giving information or preclude us from asking for information from them, but any instructions from Mr. Mulloy about how the investigation was going to be conducted or how information was going to be conveyed was directly opposite what we told them. They all knew that. And I was very surprised to hear Mr. McDonald say that. Now, Mr. Mulloy, I guess, says he didn't say that.

MR. MULLOY: Mr. Chairman, may I make a clarifying comment?

CHAIRMAN ROGERS: Sure.

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MR. MULLOY: From my vantage point, what I was talking about was not any information related to the investigation. The discussion I was having as to why Mr. McDonald didn't express any of th concerns that he expressed initially in the closed door hearing on a Friday at KSC in the initial discussion where he stated that he had continued to protest against the launch after the decision was made, and my question was why, if he had those concerns, did he not express those concerns to his management on the night of the 27th or on the morning of the 28th?

Now, what I heard him say was, he interpreted something different than that.

CHAIRMAN ROGERS: It wasn't an interpretation. He said you questioned him about why he was giving information directly to the Commission and not going through NASA and his own management. Did you say that?

MR. MULLOY: Yes, sir. What I said was related to the events of the 27th. My question was, if he had all those concerns, why he did not relay those concerns to his management on the night of the 27th and up to launch time on the 28th.

CHAIRMAN ROGERS: So there was a misunderstanding. You were talking about why he didn't

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do something on the 27th. And he construed that to mean something else.

MR. MULLOY: And waiting until the Commission hearings on that Friday to express those concerns when they had not been expressed to his management in time to do anything about the launch of 51-L.

MR. ACHESON: I don't want to stop this episode, but I do have two questions for Mr. Kilminster.

CHAIRMAN ROGERS: Go ahead.

MR. ACHESON: One goes to the design of the joint. A lot of the material we have received, one reads that the designers, presumably both the corporate designers and the NASA supervisors, believed that the joint was designed to compress and seal in the gas tight under combustion pressure. And it turned out very quickly in the joint history that it did the opposite. It opened up.

Now, to a layman it is kind of hard to understand why you wouldn't at that point say this thing works the opposite of what we thought, so there is a lot about it we don't understand, so let's go back to the drawing board and design a new joint that works the way we want it to. If you design an aircraft and you discover the windows are almost but not quite pressure tight, you don't tell the passengers to wear oxygen over

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20,000 feet. You send it back to the manufacturer and you say, make it right.

I just don't understand why the program then developed to go into a lot of little fixes to see if you could compensate for a fundamental error, and maybe you can explain that to us.

MR. KILMINSTER: Again, I think we have to refer back to the experience base that we used to design that configuration in the joint, and that was the Titan joint. The Titan joint has the same gap tolerances, the same diametral tolerances that we have in our joint. It has the same bore seal. It has the same material in that seal, the O-ring seal. However, I believe their O-ring is slightly smaller than ours in cross-sectional diameter, .275 versus .280.

When we first found out, and I think it was due to our hydrotesting, hydroburst test program, and eventually the structural test, Article 1 testing we discussed before, when we found out there that there was not a closing of the gap on pressurization, there was an opening of the gap, then we started to look, and I understand at that time we found out that the Titan gap also opened, and the number that I recall is 28 1,000ths, where ours is in the 40 1,000ths, 42 1,000ths or thereabouts.

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So, again, the basis that that system had been functioning, functioned acceptably with basically the same material, the same O-ring, the same configuration, same diametral tolerances, we thought it was reasonable to proceed with that basic design.

DR. WALKER: You are telling me this accident was the fault of the Air Force and the Titan program?

MR. KILMINSTER: No, sir, I am just answering the question.

DR. WALKER: I just do not understand that remark. You have got a system which you designed, and it behaves exactly the opposite, as Mr. Acheson has said, and it is clearly a serious problem. I don't understand why you are pointing at somebody else who might have the same problem. What does that have to do with your problem?

MR. KILMINSTER: They did not have a problem that was known at that time. They had a long history of success, and it was functioning just fine.

MR. SUTTER: How many successes did they have when they only shoot it into the air once and they don't pick it up and refurbish it? It seems to me that is a hell of a skimpy data base.

MR. KILMINSTER: Well, the analytical work that supported that continued to show, and we have

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recently been surprised about some information that you are aware of about diametral growth, but the information, the analytical information continued to show.

MR. SUTTER: Well, don't you know about the information down at Marshall? You inferred that I know something you don't know about diametral growth. That data was presented down at Marshall when we were down there. From the test firings and from the actual firings you get a deformation that stays built in. You are aware of that now, aren't you?

MR. KILMINSTER: That is just what I was speaking to. That is recent information.

MR. SUTTER: Well, that gets back to the comment that I made before. I think part of this accident was caused by not a sufficient data base to design solid rocket booster joints, even today, and before anything flies there ought to be five or ten of these full-scale things shot off, and after you get about ten of them that work right, then you can say you should fly again.

I will go back to the remark—I asked a question of your people in one of the open sessions and I didn't get a satisfactory answer, and it has been bugging me ever since.

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My question was, looking back on things, do you feel that that testing that had been done prior—well, with all of these problems going on, was that testing adequate to really give you a sufficient knowledge to fly? And the answer I got is, there is not enough money in the Treasury of the United States to do the kind of testing I want.

I really sort of took issue with that comment, and I would like to just comment back to you now. I am going to see that before anything flies again, there is going to be a lot of full-scale testing or something suitable to back it up, not these little dinky ten-inch tests where a whole bunch of scale effects are missing.

And, you know, I don't see why we all don't say we didn't know what this joint was doing. It worked backwards to the design. There was a mistake made due to lack of knowledge, and let's get the knowledge and get on with it. There is a lot of pussyfooting going on about what we did or didn't know.

My view of all of the testing that is done, you don't know whether it is temperature, you don't know whether it's tolerances, you don't know whether it's the screwing ups when you have to take

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these things and put hydraulic jacks on them and squeeze them into position. I still question the fact that something sitting out there for 28 days, and the temperatures going up and down, and it was sealed once—whether that seal is still holding. The knowledge base, the data base that is referred to on joints and solid rocket boosters 12 feet in diameter is about 100th of what you need to say the article is safe, and somebody ought to recognize that before they say they are going to fly this thing again.

CHAIRMAN ROGERS: Fine. Unless we have a lot of urgent questions—did you want to go ahead?

DR. WALKER: I just had one more. Is the Titan a manned system?

MR. KILMINSTER: No, sir.

DR. WALKER: Do you think that there ought to be a higher standard applied to systems where there are people present? I think that can be answered yes or no.

MR. KILMINSTER: The answer is yes. Obviously. I think I have to ask the question, though, not knowing the Titan safety factors, whether they were established for a manned system. I believe early on there was some talk about using it.

GENERAL KUTYNA: Whether these—have that same factor or not I don't know. However, you know,

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when I have a $600 million payload on board, whether I have got a man on board or not, I put a fairly high safety factor in there.

CHAIRMAN ROGERS: Well, thank you very much, gentlemen. I think that will be all.

(Pause.)

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CHAIRMAN ROGERS: Let's come back to order.

Gentlemen, the Commission is in the process of concluding its hearings, and in that connection we are reviewing documents particularly as they relate to the joint and the O-ring history. I guess you probably—the gentlemen have been given the documents.

So we just want to refer to them and ask you a few questions about those documents to be sure you have a chance to respond and make any comments you want about them.

(Witnesses sworn.)

ORIGINAL PAGE IS OF POOR QUALITY

MSFC PROBLEM ASSESSMENT SYSTEM
FEBRUARY 26, 1986

PAGE 1 OF 3
MSFC RECORD NUMBER
A09200
CONTRACTOR REPORT NUMBER
DR4-5749

PROBLEM TITLE: O-RING EROSION IN THE CASE TO NOZZLE JOINT

ELEMENT	CONTRACTOR	SYSTEM	CRIT	SYS LEVEL	MIBC CODE	ABCDE [illegible]
SRM	THIOKOL	STRUCTURE	1R	YES		2 A 2 A&Y

HARDWARE IDENTIFICATION	NOMENCLATURE	PART NUMBER	SERIAL/LOT	MANUFACTURER
NONCONFORMING ITEM	PRIMARY O-RING	1U52837-02	SRM-[illegible]A	[illegible]
NEXT HIGHER ASSY	AFT SEGMENT	1U50454-01	SRM-[illegible]A	THIOKOL
ARTICLE	SRB ASSY	10101-0001	BI0-6L	THIOKOL

TEST / OP	PREV COND	LOCATION	SYMPTOM	FAIL/UNSAT	FAIL MODE	CAUSE	FMEA NUMBER
FLT	PT-FLT INS	KSC-L	123	UC	123	W20	10-01-01

LAUNCH CONSTRAINTS	CONCUR	TIME CYCLES	WUC	DATE OCCURRED	DATE RPT
STS - 51F 51I 51J 61A 61B 61C	JWT	N/A	A. MCDONALD	04/29/85	07/10/85

VEHICLE EFFECTIVITY	R/C	EST RES DATE	CONT CLOSURE REC.
SI019 & SUBS	M20		12/18/85

PROBLEM DESCRIPTION:

REF: SIMILAR REPORTS - A07934 A[illegible] A08[illegible] A08[illegible] A08[illegible] A[illegible] A09[illegible]
PRIMARY O-RING EROSION WAS EXPERIENCED ON THE AFT SEGMENT OF BOTH SRM 16A & B. SRM-16A HAD THREE [illegible] EROSION POCKETS, THE GREATEST BEING 0.171 IN., WITH SOOT BETWEEN THE PRIMARY & SECONDARY O-RINGS & SOME EROSION ON THE SECONDARY O-RING. SRM-16B HAD MINOR EROSION, THE GREATEST POCKET BEING .005 IN., WITH NO SOOT BETWEEN THE PRIMARY & SECONDARY O-RINGS.

INVESTIGATION / RESOLUTION:

7/11/85 - POST FLT INSPECTION OF SRM 16A REVEALED A GAS PATH THROUGH THE VACUUM PUTTY AT 54 DEGS. THE PRIMARY O-RING WAS 12 IN. OF CIRCUMFERENTIAL (HOOP) LENGTH WHICH EXPERIENCED HEAT DEGRADATION AT THIS LOCATION. MAJOR EROSION OCCURRED ADJACENT TO THE HOLE IN THE PUTTY FOR 1.59 IN. IN THE CIRCUMFERENTIAL (HOOP) DIRECTION. WITHIN THE 1.59 IN. OF MAJOR EROSION THERE WERE THREE SEPARATE EROSION POCKETS. THE GREATEST AMOUNT OF EROSION WAS 0.171 IN. & THE GREATEST AMOUNT OF CROSS SECTIONAL PERIMETER ERODED WAS 132 DEGS. BETWEEN THE PRIMARY & SECONDARY O-RING THERE WAS HEAVY DEPOSITS OF SOOT FOR 90 DEGS TO EACH SIDE OF THE GAS PATH WITH A LIGHTER DEPOSIT FOR THE REMAINING 180 DEGS.

AT 54 DEGS THE SECONDARY O-RING EXPERIENCED 0.032 IN. OF EROSION & THE CROSS SECTIONAL PERIMETER WAS ERODED FOR 145 DEGS. THE CIRCUMFERENTIAL EROSION WAS 3.1 IN. WITH NO ADDITIONAL HEAT AFFECTED LENGTH. THERE WAS NO SOOT PAST THE SECONDARY O-RING.

POST FLT INSPECTION OF SRM 16B REVEALED A GAS PATH THROUGH THE VACUUM PUTTY AT 14.6 DEGS. AT THIS LOCATION THE PRIMARY O-RING WAS ERODED 0.005 IN. DEEP WITH 1[illegible] DEGS OF CROSS SECTIONAL PERIMETER ERODED. MAJOR EROSION EXTENDED FOR 1.4 IN. THE CIRCUMFERENTIAL DIRECTION WITHIN A HEAT AFFECTED LENGTH OF 4.7 IN. THE SECONDARY O-RING WAS UNAFFECTED.

BASED ON THE AMOUNT OF CHARRING, THE EROSION PATHS ON THE PRIMARY O-RING & WHAT IS UNDERSTOOD ABOUT THE EROSION PHENOMENON; IT IS BELIEVED THAT THE PRIMARY O-RING OF SRM 16A NEVER SEATED.

ASSIGNEE	R. MCINTOSH DESIGN	D. NEWMAN R & Q.A.	L. WEAR PROJECT	J. FLETCHER PAE	PAC REVIEW COMPLETE UP
APPROVAL				MSFC CLOSURE DATE	STATUS OPEN-R

[Ref. 5.2-14 1 of 3]

MSFC PROBLEM ASSESSMENT SYSTEM
(C O N T I N U A T I O N)

PAGE 2 OF 3
MSFC RECORD NUMBER
A09200
CONTRACTOR REPORT NUMBER
DR4-5749

7/15/85 - THIS PROBLEM IS NOT CONSIDERED A CONSTRAINT TO 51F. A 100 PSIG LEAK CHECK IS PERFORMED WHICH CONFIRMS SEATING OF THE SECONDARY O-RING. NOZZLE O-RINGS HAVE BEEN SHOWN TO SURVIVE EROSION DEPTHS OF 125 MILS IN HOT SUBSCALE TESTS. THE MATH MODEL PREDICTS THE MAXIMUM EROSION DEPTH FOR THE SECONDARY O-RING TO BE 75 MILS IN THE WORST CASE CONDITION. THE EROSION ON THE SECONDARY O-RING WAS 32 MILS, WELL BELOW THE DEMONSTRATED SEALING CAPABILITY. ANALYTICAL RESULTS INDICATE THAT A WORST CASE EROSION CONDITION WOULD NOT ERODE THE SECONDARY O-RING BEYOND ITS SEALING CAPABILITY. SEAL PUTTY LEAK CHECK AT 100 PSIG CAN BE MASKED BY THE VACUUM PUTTY. HOWEVER, AT 200 PSIG THE VACUUM PUTTY CANNOT MASK THE LEAK CHECK. STS-51F (SRM 19) HAS THE 200 PSIG LEAK CHECK AND THEREFORE SHOULD EXHIBIT BEHAVIOR MORE LIKE STS-51D (SRM-1[illegible]) AND STS-51B (SRM 18) WHICH EXHIBITED PRIMARY O-RING EROSION WHICH WAS WITHIN EXPERIENCE BASE WITH NO SECONDARY O-RING EROSION. ALL JOINTS WITH VACUUM PUTTY WILL BE LEAK CHECKED TO 200 PSIG IN THE FUTURE.

REF. TWR 15088 "STS-51F SOLID ROCKET MOTOR (SRM 19) FLT READINESS REVIEW," DATED 7-1-85. SRM SPECIAL TOPIC, NOZZLE O-RING ANOMALY OBSERVED ON STS-51B (SR-16A) - ___JWT 7-19-85 ___

7/23/85 UPDATED FROM 7-11-85 PPB - RATIONALE FOR ACCEPTANCE WAS DISCUSSED AS INDICATED BY THE 7-2-85 UPDATE. NO OTHER SIGNIFICANT INFO WAS PROVIDED.

8/9/85 - STS-51F (SRM-19) FIELD JOINT O-RING INSPECTION REVEALED SEVERAL GAS PATHS THROUGH THE PUTTY BUT NO O-RING EROSION WAS FOUND ON ANY OF THE CASE JOINTS. THE NOZZLE TO CASE O-RING JOINT REVEALED A GAS PATH AT 21.6 DEGS WITH THE PRIMARY O-RING EXPERIENCING EROSION OF APPROX. 0.010 IN. THIS IS WITHIN THE FLT EXPERIENCE DATA BASE & IS NOT A FLT CONSTRAINT TO STS-51I (SRM-20) - ___JAMES W. THOMAS 8-11-85___

8/19/85 UPDATED FROM 8-8-85 PPB - NOTHING NEW TO REPORT AT THIS MEETING.

9/20/85 UPDATED FROM 21 DAY REPORT - RESULTS OF THE TEAM INVESTIGATION ARE THAT THE PRIMARY O-RING FAILED TO SEAT AT 54 DEGS WHEN THE GAS JET IMPINGED UPON [illegible] POSITION OF THE GAS JET THEN ERODED THE PRIMARY O-RING & CONTINUED UNTIL IT IMPINGED ON THE SECONDARY O-RING. THE SECONDARY O-RING SEATED & SEALED THE JOINT. THE PRIMARY O-RING CONTINUED TO ERODE DUE TO A COMBINATION OF HOT GAS JET IMPINGEMENT & CONVECTION HEATING UNTIL THE IGNITION TRANSIENT WAS COMPLETED & THE MOTOR REACHED OPERATING PRESSURE APPROX. 600 MILLISECONDS AFTER IGNITION.

9/30/85 - ON STS-51I NO O-RING EROSION WAS EXPERIENCED THAT EXCEEDED THE CURRENT EXPERIENCE BASE. SOME PRIMARY O-RING EROSION WAS NOTED ON THE LH NOZZLE TO CASE JOINT. NO SOOT WAS FOUND BEHIND THE PRIMARY O-RING. THIS PROBLEM IS NOT A CONSTRAINT TO STS-51J - ___J.W.T.___

10/14/85 - ON STS-51J NO O-RING EROSION WAS EXPERIENCED THAT EXCEEDED THE CURRENT EXPERIENCE BASE. THIS PROBLEM IS NOT A CONSTRAINT TO STS-61A - ___JAMES W. THOMAS 10-17-85___

11/6/85 - ON STS-61A NO O-RING EROSION WAS EXPERIENCED THAT EXCEEDED THE CURRENT EXPERIENCE BASE. THIS PROBLEM IS NOT A CONSTRAINT TO STS-61B - ___JWT 11-14-85___

12/10/85 - ON STS-61B NO O-RING EROSION WAS EXPERIENCED ON THE FIELD JOINTS, HOWEVER, MINOR O-RING EROSION WAS EXPERIENCED ON BOTH THE LH AND RH NOZZLE TO CASE JOINTS. SOOT BLOW-BY WAS NOTED ON THE LH NOZZLE JOINT. THE EROSION EXPERIENCED DURING THE STS-61B LAUNCH WAS WELL WITHIN THE CURRENT EXPERIENCE BASE. THIS PROBLEM IS NOT A CONSTRAINT TO STS-61C - ___JWT 12-12-85___

1/23/86 RESOLUTION - SRM FIELD AND NOZZLE JOINTS HAVE EXPERIENCED EROSION OF THE PRIMARY O-RINGS DURING SEVERAL MISSIONS AND STATIC TEST AS DETERMINED BY POST FLIGHT INSPECTION.

[Ref. 5.2-14 2 of 3]

ORIGINAL PAGE IS OF POOR QUALITY

MSFC PROBLEM ASSESSMENT SYSTEM
(C O N T I N U A T I O N)

MSFC RECORD NUMBER
A07934
CONTRACTOR REPORT NUMBER
DR4-5/30

THE PRIMARY O-RING IN A NUMBER OF INSTANCES WAS SUFFICIENTLY DAMAGED AND/OR DISPLACED TO ALLOW BLOWBY, THEREFORE EXPOSING THE SECONDARY O-RING TO HOT COMBUSTION GASES. SOME EROSION WAS EXPERIENCED ON THE SECONDARY O-RING AT THE NOZZLE JOINT BUT NOT TO THE POINT WHERE THE O-RING SEALING CAPABILITY WAS JEOPARDIZED.

THE CAUSE OF THE O-RING EROSION HAS BEEN ATTRIBUTED TO HOT GAS PATHS THROUGH THE VACUUM PUTTY ALLOWING THE HOT GASES TO IMPINGE UPON THE O-RING. IMPROPER SEATING OF THE PRIMARY O-RING ALLOWS BLOWBY AND MINOR HOT GAS IMPINGEMENT ON THE SECONDARY O-RING. THE MAGNITUDE OF THE O-RING EROSION IS A FUNCTION OF ORIFICE SIZE AND FREE VOLUME (AREA BETWEEN THE PRIMARY O-RING AND THE PUTTY).

PRIMARY O-RING EROSION IS EXPECTED TO CONTINUE SINCE NO CORRECTIVE ACTION HAS BEEN ESTABLISHED THAT WILL PREVENT HOT GASES FROM REACHING THE PRIMARY O-RING CAVITY. STEPS HAVE BEEN TAKEN TO ASSURE THAT THE SECONDARY O-RING WILL BE SEATED AND ANALYTICAL ANALYSIS HAVE INDICATED THAT UNDER A WORST CASE SITUATION, EROSION OF THE SECONDARY O-RING WILL NOT BE SEVERE ENOUGH TO ALLOW A LEAK PATH PAST THE SECONDARY O-RING. STW7-2747 WAS CHANGED EFFECTING AN INCREASE IN THE [illegible] PRESSURE [illegible] PSIG. WITH THE 100 PSIG LEAK TEST THE SECONDARY O-RING WOULD BE [illegible] PATHS THROUGH THE PUTTY WOULD BE UNDETECTABLE. BY INCREASING THE LEAK TEST PRESSURE TO 200 PSIG, ANY LEAK PATHS THROUGH THE PUTTY WOULD BE DETECTABLE. THE 200 PSIG LEAK TEST WAS IMPLEMENTED ON SRM-19 AND SUBSEQUENT MOTORS.

THE STACKING PROCEDURES HAVE BEEN CLARIFIED TO ASSURE MOTOR-TO-MOTOR CONSISTENCY IN JOINT MATING. THE OPERATIONAL MAINTENANCE DOCUMENT CHANGES WERE EFFECTIVE ON SRM [illegible] AND 8 BS.

ANALYTICAL STUDIES BASED ON BOTH IMPINGEMENT EROSION AND BLOWBY EROSION SHOW THAT THIS PHENOMENON HAS AN ACCEPTABLE CEILING SINCE IMPLEMENTING THE ABOVE CHANGES. RECENT EXPERIENCE HAS BEEN WITHIN THE PROGRAM DATA BASE.

THIS SEAL IMPROVEMENT PROGRAM PLAN WILL CONTINUE UNTIL THE CAUSE HAS BEEN ISOLATED AND DAMAGE ELIMINATED TO THE SRM SEALS. STATUS WILL CONTINUE TO BE PROVIDED IN THE FLIGHT READINESS REVIEWS AND IN NORMAL TECHNICAL REVIEWS AT ATI AND MSFC. AT THE CONCLUSION OF THE PROGRAM, A COMPREHENSIVE REPORT WILL BE WRITTEN TO CONSOLIDATE THE RESULTS, CONCLUSIONS, AND RECOMMENDATIONS.

THIS PROBLEM IS CONSIDERED CLOSED BASED ON ATI REPORT TWR-14359, REV. A, "HISTORY OF SPACE SHUTTLE SRM MOTOR SEALS" DATED 0-10-85 AND ATI LETTER E150/BOR-86-114 "RATIONALE FOR CLOSURE OF THE O-RING EROSION PROBLEM", A07934, DR4-5/30I DATED 1-2-86.

[Ref. 5.2.14 3 of 3]

MSFC PROBLEM ASSESSMENT SYS
March 7 1986

PAGE 1 OF
MSFC RECORD
A07934
CONTRACTOR REF
DR4-5/30

PROBLEM TITLE: SEGMENT JOINT PRIMARY O-RING CHARRED

ELEMENT	CONTRACTOR	SYSTEM	CRIT	SYS LEVEL	MISC CODE	ABCDEFG
SRM	THIOKOL	STRUCTURE	1R	YES		O X

HARDWARE IDENTIFICATION	NOMENCLATURE	PART NUMBER	SERIAL/LOT	MANUFACTURER
NONCONFORMING ITEM	PRIMARY O-RING	1U50228-24	SRM-H010A	THIOKOL
NEXT HIGHER ASSY	SRB ASSY	10101-0001	SRM-H010A	THIOKOL
TEST ARTICLE	SRB ASSY	10101-0001	SI010L	THIOKOL

TEST / OP	PREV. NO	LOCATION	SYMPTOM	FAIL/UNSAT	FAIL MODE	CAUSE	FMEA N
FLT	PT-F. [illegible] ING	KSC-K	123	UC	123	W20	10-01-C

LAUNCH CONSTRAINTS	CONCUR	TIME CYCLES	WUC	DATE OCCURED	DATE REC
NONE	JOR	N/A	R LANEY	02/03/84	02/17/84

VEHICLE EFFECTIVITY	R/C	EST RES DATE	CONT CLOSURE
SI019 & SUBS	M20		12/18/85

PROBLEM DESCRIPTION:

REF: SIMILAR REPORTS: A09014 & A08299, A08615, A08687, A08939, A09260, A09288
SLIGHT CHAR CONDITION ON PRIMARY O-RING SEAL IN FWD FIELD JOINT ON SRM A57 OF STS-11 FLT, MISSION 41B

INVESTIGATION / RESOLUTION:

REMEDIAL ACTION - NONE REQ'D; PROB OCCURRED DURING FLT. THE PRIMARY O-RING SEAL IN THE FWD FIELD JOINT EXHIBITED A CHARRED AREA APPROX 1 IN. LONG, 0.030-0.050 IN. DEEP, & 0.100 IN. WIDE. THIS WAS DISCOVERED DURING POST-FLT SEGMENT DISASSY AT KSC.

3/8/84 - POSSIBLY EXIST FOR SOME O-RING EROSION ON FUTURE FLTS. ANALY INDICATES MAX. EROSION POSSIBLE IS 0.090 IN. ACCORDING TO THE FLT READINESS REVIEW FINDINGS FOR STS-13. LABORATORY TEST SHOWS SEALING INTEGRITY AT 3000 PSI USING AN O-RING WITH A SIMULATED EROSION DEPTH OF 0.095 IN. THEREFORE, THIS IS NOT A CONSTRAINT TO FUTURE LAUNCHES. ADDRESSED PRB ON 3-8-84.

3/27/84 UPDATED FROM 5 DAY REPORT - THE FAILURE DEFINED ABOVE WAS A SECONDARY PROB. THE VACUUM PUTTY LAY-UP IS CONSIDERED THE PRIME PROB. THE EFFECT ON THE O-RING SEAL BEING CHARRED HAS BEEN REVISED TO APPROX 3 IN. IN LENGTH, 0.040 IN. DEEP & AFFECTED 217 DEGS OF THE O-RING CROSS-SECTION PERIMETER. THE O-RINGS IN THE SRM SEGMENT ASSY JOINTS ARE DESIGNED AS PRESS. SEALS & ARE NOT INTENDED TO BE EXPOSED TO HOT GASES. STW4-2847 VACUUM SEAL PUTTY IS LAID UP IN THE JOINT AREA DURING MOTOR SEGMENT MATING TO PROVIDE A HEAT/HOT GAS BARRIER TO PROTECT THE O-RINGS. THE PRIME PROB IS THE LAY UP OF THE VACUUM PUTTY ALLOWING A HOT GAS PATH TO THE O-RING SEAL. THE O-RING IS USED IN THE FWD FIELD JOINT WHICH JOINED THE FWD MOTOR SEGMENT (P/N 1U52216-04, S/N 0000001) & FWD CENTER MOTOR SEGMENT (P/N 1U52217-02, S/N 0000001).

4/17/84 UPDATED FROM 4-12-84 PRB & MARCH MONTHLY SUMMARY REPORT - TEST FIXTURE & TEST PLANS HAVE BEEN PREPARED & TEST STARTED TO SIMULATE THE EFFECTS OF THE HOT GAS ON THE O-RING. THESE TESTS ARE EXPECTED TO CONFIRM THAT HOT GAS WILL NOT ERODE THE O-RING TO A POINT WHERE SEAL INTEGRITY WOULD FAIL. THE PUTTY LAY-UP IS STILL TO BE STUDIED.

[Ref. 5.2.15 1 of 6]

ASSIGNEE	R. MCINTOSH	D. NEWMAN	L. WEAR	J. FLETCHER	PAC REVIEW COM
APPROVAL	DESIGN	R & Q.A.	PROJECT	PAC	JF
				MSFC CLOSURE DATE	STATUS OPEN-M

5/1/84 UPDATED FROM 21 DAY REPORT - THE FORWARD FIELD JOINT DESIGN IS BEING EVALUATED. IT HAS BEEN DETERMINED THAT THE FREE VOLUME, THAT PORTION OF THE JOINT TWO OF THE PRIMARY O-RING NOT FILLED WITH EITHER VACUUM PUTTY OR GREASE, IS WHERE THE PROB LIES. IT IS BELIEVED THAT THE O-RING CHAR IS CAUSED BY A SHORT TIME DURATION IMPINGEMENT OF A HIGH ENERGY JET WHICH IS INDUCED DURING IGNITION PRESSURIZATION BY A COMBINATION OF VOIDS IN THE PROTECTIVE VACUUM PUTTY & THE FILLING OF AVAILABLE FREE VOLUMES CREATED BY THE TOL OF MATING PARTS & THE O-RING SLOT. THE TEST BEING CONDUCTED SHOULD VERIFY THAT THIS CONDITION WILL NOT CAUSE A SERIOUS PROB.

5/23/84 UPDATED FROM 5-10-84 PRB - INVESTIGATIONS ARE CONTINUING. O-RING DAMAGE IS NOT TO THE POINT WHERE SEAL INTEGRITY WOULD FAIL (LESS THAN 0.070 IN. POSSIBLE). O-RING CHAR HAS ALSO BEEN FOUND ON STS-13.

5/30/84 UPDATED FROM MAY MONTHLY SUMMARY REPORT - TWR-14336 "PERFORMANCE CHARACTERISTICS OF THE SRM O-RING ASSY TEST PLAN" HAS BEEN RELEASED - THE 5 IN. CP HOT TESTING INITIATED. DATA FROM THE TESTS IS BEING ANALYZED & WILL BE IN NEXT MONTH'S REPORT.

6/20/84 UPDATED FROM 6-14-84 PRB & JUNE MONTHLY SUMMARY REPORT - PRELIMINARY FINDINGS FROM THE O-RING TEST CONFIRM THE HOT JET IMPINGEMENT SCENARIO & THE MAJOR CONCLUSIONS DRAWN ARE:

1 O-RING EROSION IS A FUNCTION OF ORIFICE SIZE & FREE VOLUME
2 GREASE ON THE O-RINGS MODERATELY REDUCES EROSION
3 NO DAMAGE TO THE O-RING RESULTS WHEN THE ORIFICE IS ABSENT, SIMULATING THE ABSENCE OF VACUUM PUTTY

A METHOD OF SOLUTION HAS NOT BEEN DETERMINED. THIOKOL WILL HAVE RECOMMENDED CHANGES IN JULY 1984.

A07934, A08014 & A08239 DOCUMENT DIFFERENT OCCURRENCES OF THE SAME GENERAL PROB. ALL ANALYSES DONE & CONCLUSIONS MADE WILL APPLY TO ALL 3 OCCURRENCE & WILL BE REPORTED UNDER A07934 HENCEFORTH.

7/19/84 ADDRESSED AT PRB ON 7-12-84 - TWO SPECS WERE RELEASED CHANGING THE LAYUP OF THE VACUUM PUTTY & GREASE APPLICATION. THESE CHANGES WERE TESTED SATISFACTORILY, HOWEVER ADDITIONAL TEST ARE REQ'D.

9/24/84 UPDATED FROM AUG. MONTHLY SUMMARY REPORT & 9-13-84 PRB - VACUUM SEAL PUTTY LAYUP SPECS STW7-3193 & STW7-3228 WITH NEW LAYUP PROCEDURES HAVE BEEN RELEASED. STW7-3194 IS STILL IN THE SIGN OFF PROCESS. INVESTIGATIONS ARE CONT'D. PLANS ARE TO PERFORM FULL SCALE NOZZLE & JOINT TESTS. ALSO LOOKING AT NEW PUTTY. THIS PROBLEM OCCURRED AGAIN ON FLT 41D. SEE PROBLEM REPORT A08618.

10/2/84 UPDATED FROM SEPT. MONTHLY SUMMARY REPORT - VACUUM SEAL PUTTY LAYUP SPEC STW7-3228 HAS BEEN REVISED TO REDUCE THE AMOUNT OF GREASE & STW7-3194 IS STILL IN SIGN OFF.

11/8/84 UPDATED FROM OCT. MONTHLY SUMMARY REPORT - THE JOINT FILLER MTRL TEST PLAN IS RELEASED & FULL SCALE JOINT ASSY TESTING WAS INITIATED 10-10-84 AT MORTON THIOKOL'S M-7 CLEARFIELD FACILITY.

11/14/84 UPDATED FROM PRB MTG 11-8-84 - THE TEST PLAN SHOULD BE REVISED & TEST STARTED WITHIN APPROX. 2 WKS.

1/8/85 UPDATED FROM PRB MTG 12-13-84 - A NEW PROGRAM WAS REQUESTED BY MSFC FOR EVALUATION & TESTING. THIS IS NOW IN WORK & THE ESTIMATED COMPLETION DATE IS 1-4-85. ANY DESIGN CHANGES ARE PENDING THE

[Ref. 5/2-15 2 of 6]

TEST RESULTS. A COPY OF THE 40-LB & 5 IN. CP MOTOR TEST SCHEDULE IS INCLUDED IN THIS PKGE. THE O-RING CHAR PROBLEM IS A REVIEW ITEM FOR EACH FLT READINESS REVIEW (FRR).

1/22/85 UPDATED FROM PRB MTG ON 1-10-85 - ALL SCALE MODEL TESTING WILL BE COMPLETED IN JAN. FULL SCALE TESTS ARE TO FOLLOW.

2/15/85 UPDATED FROM 2-7-84 PRB & JAN. MONTHLY SUMMARY REPORT - TESTING OF VACUUM SEAL PUTTY LAYUP CONFIG. USING 5 IN. CP MOTOR IS CONTINUING. TORCH TESTS HAVE BEEN DONE ON ASBESTOS FILLED PUTTY MFR'D BY RANDOLPH & INMONT & A NONASBESTOS PUTTY MFR'D BY INMONT. BOTH ASBESTOS PUTTIES PERFORMED COMPARABLY. THE HEAT TRANSFER RATE OF THE NONASBESTOS PUTTY WAS MORE THAN TWICE THAT OF THE ASBESTOS FILLED PUTTY. FORTY LB. CHAR TEST MOTORS ARE SCHEDULED TO FURTHER TEST THE PUTTY TYPES.

EROSION WAS AGAIN EXPERIENCED ON FLT 51C. THE O-RING BURNS WERE AS BAD OR WORSE THAN PREVIOUSLY EXPERIENCED, SEE PROBLEM REPORT A08939/CONT NO. 4-5/41. DESIGN CHANGES ARE PENDING TEST RESULTS. CHANGES BEING CONSIDERED ARE: MODIFYING THE O-RINGS, DOUBLE BARRIER VENTING & ADD'L GREASE AROUND O-RINGS TO FILL THE VOID LEFT BY THE PUTTY.

3/18/85 UPDATED FROM 3-7-84 PRB - TESTING & EVALUATION PROGRAM IS CONTINUING. DESIGN CHANGES ARE PENDING TEST RESULTS.

3/29/85 UPDATED FROM FEB. MONTHLY SUMMARY REPORT - DISASSY OF THE SRM 15A & 15B NOZZLE TO AFT CASE JOINTS REVEALED GAS PATHS THRU THE PUTTY AT 109 DEG & 64 DEG, RESPECTIVELY. BOTH PRIMARY O-RINGS HAD HEAT AFFECTED AREAS (NO EROSION) AT THE GAS PATH LOCATIONS. SOOT WAS FOUND BETWEEN THE PRIMARY & SECONDARY O-RINGS IN EACH JOINT BUT THE SECONDARY O-RINGS WERE UNTOUCHED. THE SOOT WAS MOST LIKELY DEPOSITED DURING IGNITION PRESSURIZATION WHEN GAS PASSED THE PRIMARY O-RINGS BEFORE THEY SEATED.

A DR4-5 PROBLEM REPORT WAS NOT WRITTEN FOR THIS OCCURRENCE FOR THE FOLLOWING REASONS:

1) THE PRIMARY O-RINGS SEATED & THE PRESSURE INTEGRITY OF THE JOINTS WAS MAINTAINED.
2) THE O-RINGS WERE NOT ERODED.
3) GAS PATHS & O-RING EROSION HAVE OCCURRED ON PREVIOUS FLTS & ARE EXPECTED TO OCCUR RANDOMLY ON FUTURE FLTS.

IT HAS BEEN REPORTED HERE TO DOCUMENT THE OCCURRENCE.

4/17/85 UPDATED FROM PRB MEETING 4-11-85 - O-RING INVESTIGATION & PUTTY TESTING IS CONTINUING. MSFC'S FINDING THAT THE PUTTY DISSOLVED IN GREASE COULD NOT BE DUPLICATED BY THIOKOL.

4/24/85 UPDATED FROM APRIL MONTHLY SUMMARY REPORT - AT NASA REQUEST, A SOLUTION FOR O-RING EROSION WILL NOT INVOLVE A RADICAL DESIGN CHANGE. THEREFORE, THE POSSIBLE SOLUTIONS UNDER CURRENT INVESTIGATION ARE LIMITED TO:

1. NEW O-RING MTRL AND/OR DIAMETER.
2. NEW VACUUM PUTTY AND/OR LAYUP PROCEDURE.

6/6/85 UPDATED FROM MAY MONTHLY SUMMARY REPORT - DISASSY OF SRM 17A & 17B REVEALED GAS PATHS & EROSION IN BOTH NOZZLE TO CASE JOINTS. THE GAS PATH IN THE NOZZLE TO AFT CASE JOINT OF SRM 17A WAS CENTERED AT 14.4 DEGS & THE O-RING HAD A .010 IN. OF EROSION. SRM 17B'S NOZZLE JOINT HAD A GAS PATH AT 116 DEGS & THE O-RING HAD A .068 IN. OF EROSION. THERE WAS SOOT DEPOSITED IN THE PRIMARY

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O-RING GROOVES OF EACH JOINT, HOWEVER, NO SOOT PASSED THE PRIMARY O-RING

6/25/85 UPDATED FROM 6-13-85 PRB - PLAN TO SHAVE O-RING PAST THE .090 IN TO DETERMINE WHEN A LEAK WILL OCCUR. HOT FIRE JOINT TESTS ARE PLANNED FOR AUG ... SEP.

7/16/85 UPDATED FROM JUNE MONTHLY SUMMARY REPORT DATED 6-17-85 - ONE POSSIBLE EXPLANATION FOR THE GREATER EROSION DEPTHS OBSERVED ON SRB 17B (56 MILS) & NOZ DM-7 (58 MILS) RELATES TO AN ASPECT OF NOZZLE JOINT ASSEMBLY. A ECCENTRIC NOZZLE JOINT CAN OCCUR MORE EASILY THAN AN ECCENTRIC FIELD JOINT DUE TO A) ITS HORIZONTAL VS VERTICAL POSITION DURING ASSEMBLY, B) SMALL JOINT CLEARANCES, AND C) THE ABSENCE OF SHIMS. CONSEQUENTLY, IT IS POSTULATED THAT A REDUCED JOINT GAP (& THERE-FORE GAS JET ORIFICE) OCCURRED AT THE CIRCUMFERENTIAL LOCATION OF THE BLOWHOLE, THEREBY CAUSING ENHANCED EROSION. THE VOLUME FILLING MODEL PREDICTS THAT AN ECCENTRICITY OF ONLY 7 MILS WILL ROUGHLY DOUBLE THE 40 MILS EROSION PREDICTED FOR CONCENTRIC NOZZLE JOINTS. THUS AN O-RING EROSION EXCEEDING 80 MILS CAN BE CAUSED BY JOINT ECCENTRICITY IF THE REDUCED GAP ALIGNS WITH THE BLOWHOLE. INVESTIGATION OF THIS SCENARIO IS CONTINUING.

7/23/85 UPDATED FROM 7-11-85 PRB - THIS PROBLEM WAS SCHEDULED FOR STATUSING, BUT NOT DISCUSSED SINCE NO SIGNIFICANT PROGRESS HAS BEEN MADE SINCE THE LAST UPDATE

8/19/85 UPDATED FROM 7-8-85 PRB & JULY 1985 MONTHLY SUMMARY REPORT - THE ANALYTICAL MODEL OF O-RING EROSION HAS BEEN USED TO PREDICT A WORST CASE EROSION OF 75 MILS ON THE SECONDARY O-RING SHOULD THE PRIMARY O-RING FAIL TO SEAL. PRESSURE TESTS OF O-RINGS WITH MTRL REMOVED TO SIMULATE EROSION & SUBSCALE MOTOR FIRINGS HAVE DEMONSTRATED THAT THIS IS WELL BELOW THE SEAL CAPABILITY OF ERODED O-RINGS. EVALUATION OF THE RESULTS OF STS-51F ARE CONTINUING

9/3/85 UPDATED FROM JULY MONTHLY SUMMARY REPORT DATED 8-15-85 - DR4-5/48 (A09260) REPORTED .068 INCH OF EROSION OBSERVED IN THE NOZZLE TO CASE JOINT ON DEVELOPMENT MOTOR NO. 7

DR4-5/49 (A09208) REPORTED .171 IN. OF EROSION ON PRIMARY O-RING & .032 IN. OF EROSION ON THE SECONDARY O-RING IN THE NOZZLE TO CASE JOINT OF SRM 16A.

THE INVESTIGATIONS ARE CONTINUING & WILL BE REPORTED HERE AS THEY ARE THE SAME GENERIC PROBLEM.

INSPECTION OF SRM 18A FIELD JOINTS REVEALED NO EROSION HOWEVER, THE NOZZLE TO CASE JOINT HAD PRIMARY O-RING EROSION AT 191.8 DEGS WITH A MAX. DEPTH OF .013 IN. SRM 18B HAD NO FIELD JOINT EROSION. THE NOZZLE TO CASE JOINT EXPERIENCED GAS PATHS AT 63, 93.6, 270 & 342 DEGS. THE PRIMARY O-RING WAS ERODED ONLY AT 270 & 342 DEGS WITH DEPTHS OF .009 & .023 IN., RESPECTIVELY. A PROBLEM REPORT WAS NOT WRITTEN FOR THESE OCCURRENCES SINCE THEY WERE WITHIN HISTORICAL LEVELS. THEY ARE REPORTED HERE FOR INFORMATION.

10/2/85 UPDATED FROM AUGUST MONTHLY SUMMARY REPORT DATED 9-16-85 - SRM 19B (STS-51F) EXPERIENCED A GAS PATH AT 21.6 DEGS. AT THIS LOCATION THE O-RING WAS ERODED APPROX. 0.001 IN., WELL WITHIN THE THE EXPERIENCE BASE. QM-5 WILL BE DELAYED TO ALLOW FOR DEVELOPMENT OF O-RING SOLUTION.

11/4/85 UPDATED FROM OCTOBER MONTHLY SUMMARY REPORT & 10-17-85 PRB - ANALYSIS OF DATA FROM STS-51J & QM-5 TEST ARE IN WORK.

11/19/85 UPDATED FROM 11-7-85 PRB MEETING. STS-61A EXPERIENCED NO FIELD JOINT EROSION, HOWEVER, SOOT BLOW-BY WAS FOUND ON 3 OF THE 6 PRIMARY O-RINGS. STS-61A R.H. NOZZLE EXPERIENCED EROSION OF THE PRIMARY O-RING. THE EROSION WAS WELL WITHIN THE CURRENT DATA BASE.

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BASED ON THE JOINT PUTTY & O-RING PROBLEM, A PROGRAM WAS INITIATED TO ESTABLISH ACCEPTABLE MATERIALS & CONFIGURATION THAT COULD BE TESTED ON QM-5 & ULTIMATELY INCORPORATED IN FLIGHT MOTORS

12/17/85 - UPDATE FROM NOVEMBER MONTHLY SUMMARY REPORT
THE O-RING TASK FORCE HAS STARTED ASSEMBLY AND COLD FLOW TESTING OF NOZZLE PUTTY AND JOINT FILLERS. TESTING IS CONTINUING.

SRM 21 - NO DAMAGE
SRM 22 - RIGHT HAND (B) NOZZLE

THERE WAS EROSION AT 97.2 DEGREE. IT WAS .075 INCH DEEP AND HAD 162 DEGREE OF CROSS SECTIONAL PARIMETER AFFECTED WITH 12 INCHES OF HEAT DAMAGE LENGTH AND 23 INCHES OF HEAT AFFECTED LENGTH. THIS IS NOT THE WORST CASE TO DATE. THERE WAS NO OTHER REPORTED O-RING DAMAGE.

12/20/85 UPDATED FROM 12-12-85 PRB MEETING - THIOKOL IS CONTINUING STUDIES RELATIVE TO THE O-RING EROSION AND PUTTY LAYUP ON AN ON-GOING BASIS. A TOTAL SOLUTION TO THIS PROBLEM IS LONG RANGE; HOWEVER, AN ACCEPTABLE DATA BASE HAS BEEN ESTABLISHED AND SUFFICIENT ENHANCEMENT HAS BEEN MADE TO PROVIDE THE NECESSARY CONTROLS TO PREVENT RECURRENCE OF THE PROBLEM OUTSIDE THE ESTABLISHED DATA BASE. CLOSURE PAPER HAS BEEN PREPARED AND IS BEING EVALUATED.

1/20/86 UPDATED FROM DECEMBER MONTHLY SUMMARY REPORT. THE TASK FORCE HAS MADE ONE HOT GAS TEST. THE PRELIMINARY RESULTS INDICATE THAT THE TEST CHAMBER NEEDS TO BE REDESIGNED.

SPACE SHUTTLE SRM FLIGHT SET 23 WAS LAUNCHED NOVEMBER 27. INSPECTION OF THE FIELD JOINTS REVEALED NO DAMAGE, WHILE BOTH OF THE NOZZLE-TO-CASE JOINTS HAD EROSION:

	23A	23B
EROSION LENGTH (INCHES)	3.0	7.0
HEAT AFFECTED LENGTH (INCHES)	22.0	28.0
EROSION DEPTH (INCHES)	.017	.037
DEGREE LOCATION	344 DEGREES	9 DEG

23/86 RESOLUTION - SRM FIELD AND NOZZLE JOINTS HAVE EXPERIENCED EROSION OF THE PRIMARY O-RINGS DURING SEVERAL MISSIONS AND STATIC TEST AS DETERMINED BY POST FLIGHT INSPECTION.
THE PRIMARY O-RING IN A NUMBER OF INSTANCES WAS SUFFICIENTLY DAMAGED AND/OR DISPLACED TO ALLOW BLOWBY. THEREFORE EXPOSING THE SECONDARY O-RING TO HOT COMBUSTION GASES. SOME EROSION WAS EXPERIENCED ON THE SECONDARY O-RING AT THE NOZZLE JOINT BUT NOT TO THE POINT WHERE THE O-RING SEALING CAPABILITY WAS JEOPARDIZED.
THE CAUSE OF THE O-RING EROSION HAS BEEN ATTRIBUTED TO HOT GAS PATHS THROUGH THE VACUUM PUTTY ALLOWING THE HOT GASES TO IMPINGE UPON THE O-RING. IMPROPER SEATING OF THE PRIMARY O-RING ALLOWED BLOWBY AND MINOR HOT GAS IMPINGEMENT ON THE SECONDARY O-RING. THE MAGNITUDE OF THE O-RING EROSION IS A FUNCTION OF ORIFICE SIZE AND FREE VOLUME (AREA BETWEEN THE PRIMARY O-RING AND THE PUTTY).

PRIMARY O-RING EROSION IS EXPECTED TO CONTINUE SINCE NO CORRECTIVE ACTION HAS BEEN ESTABLISHED THAT WILL PREVENT HOT GASES FROM REACHING THE PRIMARY O-RING CAVITY. STEPS HAVE BEEN TAKEN TO ASSURE THAT THE SECONDARY O-RING WILL BE SEATED AND ANALYTICAL ANALYSIS HAVE INDICATED THAT UNDER A WORST CASE SITUATION, EROSION OF THE SECONDARY O-RING WILL NOT BE SEVERE ENOUGH TO ALLOW A LEAK PATH PAST THE SECONDARY O-RING. STW7-2747 WAS CHANGED EFFECTING AN INCREASE IN THE LEAK CHECK PRESSURE FROM

[Ref. 5-2-15 5 of 6]

100 PSIG TO 200 PSIG. WITH THE 100 PSIG LEAK TEST THE SECONDARY O-RING WOULD BE SEATED BUT ANY GAS PATHS THROUGH THE PUTTY WOULD BE UNDETECTABLE. BY INCREASING THE LEAK TEST PRESSURE TO 200 PSIG, ANY LEAK PATHS THROUGH THE PUTTY WOULD BE DETECTABLE. THE 200 PSIG LEAK TEST WAS IMPLEMENTED ON SRM-19 AND SUBSEQUENT MOTORS.

THE STACKING PROCEDURES HAVE BEEN CLARIFIED TO ASSURE MOTOR-TO-MOTOR CONSISTENCY IN JOINT MATING. THE OPERATIONAL MAINTENANCE DOCUMENT CHANGES WERE EFFECTIVE ON SRM L024 AND SUBS.

ANALYTICAL STUDIES BASED ON BOTH IMPINGEMENT EROSION AND BLOWBY EROSION SHOW THAT THIS PHENOMENON HAS AN ACCEPTABLE CEILING SINCE IMPLEMENTING THE ABOVE CHANGES. RECENT EXPERIENCE HAS BEEN WITHIN THE PROGRAM DATA BASE.

THIS SEAL IMPROVEMENT PROGRAM PLAN WILL CONTINUE UNTIL THE PROBLEM HAS BEEN ISOLATED AND DAMAGE ELIMINATED TO THE SRM SEALS. STATUS WILL CONTINUE TO BE PROVIDED IN THE FLIGHT READINESS REVIEWS AND IN FORMAL TECHNICAL REVIEWS AT MTI AND MSFC. AT THE CONCLUSION OF THE PROGRAM, A COMPREHENSIVE REPORT WILL BE WRITTEN TO CONSOLIDATE THE RESULTS, CONCLUSIONS, AND RECOMMENDATIONS.

THIS PROBLEM IS CONSIDERED CLOSED BASED ON MTI REPORT TWR-14359, REV. A, "IMPROVEMENTS OF SPACE SHUTTLE SRM MOTOR SEALS" DATED 8-30-85 AND MTI LETTER E150/SOR-85-114 "RATIONALE FOR CLOSURE OF THE O-RING EROSION PROBLEM", A07934, DR4-5/30, DATED 1-2-86.

[Ref. 5/2-15 6 of 6]

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MORTON THIOKOL INC.
Wasatch Division

Interoffice Memo

6 December 1985
E150/BSR-86-95

TO: A. J. McDonald, Director
Solid Rocket Motor Project

CC: D. Thompson, R. Ebeling, S. Rodgers, V. Backman

FROM: Manager, SRM Special Projects

SUBJECT: Closure of the SRM O-Ring Erosion Critical Problems, MSFC Record Number A07934, PR Number DR4-5/30 and Associated Reports DR4-5/31, DR4-5/35, DR4-5/38, DR4-5/39, DR4-5/41, DR4-5/48 and DR4-5/49.

This memo enumerates the reasons for closing the subject open critical problems. The following activities are in work or have been completed:

1. A full-time task force has been created and is pursuing a solution to the o-ring erosion problem.

2. A program plan, TWR-14359, has been released defining the steps to solve the problems.

3. Twenty-three (23) full scale nozzle joint mating tests have been completed and at least seven (7) more are planned.

4. Six (6) field joint mating tests have been completed.

5. A field joint deflection test is planned.

6. Nine (9) o-ring resiliency tests have been completed and sixty (60) more are planned.

7. At least five (5) more 5" CP orifice tests are planned.

8. Forty (40) nozzle joint cold flow tests have been completed.

9. Twenty-nine (29) subscale braid porosity tests have been performed and many more are planned with new braid samples.

10. Eighteen (18) full scale braid flow tests have been completed and at least two (2) more are planned.

TC 2018 (REV 7-84)

[Ref. 5 2-16 1 of 2]

A. J. McDonald, Director
6 December 1985
E150/BGR-86-95
Page 2

11. 3-5" motor material erosion tests have been completed and four (4) more are planned.

12. 3-40 pound charge motor nozzle joint tests (five second burn time) have been completed and at least seven (7) more are planned.

13. 8-24" motor nozzle joint tests (30 second burn time) are planned.

14. 4-40 pound charge motor field joint tests (33 second burn time) are planned.

15. Four full scale field joint short stack hot firing tests are planned.

16. Continued analysis is underway to support the test effort. Thermal, gas dynamics, mechanical and dimensional analyses are included.

17. Nozzles are being removed at KSC after each flight for immediate joint and o-ring examination. Reports on the nozzle joint as well as the field joint conditions are provided in a timely fashion for consideration in subsequent flight readiness reviews.

The MTI task force is in constant contact with MSFC personnel with scheduled teleconferences weekly. With the well defined program underway and proper emphasis on the problem, recommend the problems be closed. Status will continue to be provided in the flight readiness reviews and in formal technical reviews at MTI and MSFC. At the conclusion of the program, a comprehensive report will be written to consolidate the results, conclusions, and recommendations.

Please provide this information to NASA to support the closure of this problem.

Brian Russell
Brian G. Russell

BGR/co

[Ref. 5/2-16 2 of 2]

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MORTON THIOKOL INC.
Wasatch Division

10 December 1985
E100-86-26

Mr. L. O. Wear, SA42
National Aeronautics and Space Administration
George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama 35812

Dear Mr. Wear:

Subject: Closure of Critical Problems Number DR4-5/30, DR4-5/31, DR4-5/35, DR4-5/38, DR4-5/39, DR4-5/41, DR4-5/48, and DR4-5/49 (MSFC Tracking Numbers A07934, A08014, A08299, A08615, A08687, A08939, A09260, and A09288) "Nozzle/Segment Joint Primary O-ring Charred"

The subject Critical Problems are ongoing problems which will not be fully resolved for some time. There is a formal reporting system being used to evaluate the problem resolution progress and to continue to report them as part of the MSFC Problem Assessment System is not necessary.

Therefore, we request the subject critical problems be closed and removed from the next PRP agenda/list

Enclosed is a copy of memorandum E150/BGR-86-95, TWR-15349, and TWR-14359

Very truly yours,

A. J. McDonald, Director
SRM Project

cc: J. Kilminster
J. Elwell
D. Thompson
I. Adams, NTI/MSFC
J. Thomas, Jr., SA42
E. Skrobiszewski, SA49
J. Fletcher, 041-SRO Rockwell International (MSFC)
R. McIntosh, EE11
B. [illegible]land, EG03
S. Rodgers
B. Russell
B. Ebeling
R. Gittins

P.O. Box 524 Brigham City UT 84302 (801) 863-3511

[Ref. 5/2-17]

MORTON THIOKOL INC
Wasatch Operations

Interoffice Memo

2 January 1986
E150/BGR-86-114

TO: D. E. Thompson, Manager
Systems Engineering

CC: A. McDonald, R. Ebeling, S. Rodgers

FROM: Manager, SRM Special Projects

SUBJECT: Rationale for Closure of the O-Ring Erosion Problem, A07934, DR4-5/30

It is recommended that the referenced problem be closed for the following reasons.

1. The increased stabilization pressure (200 psi) prior to the 50 psi leak test minimizes the chance of blowby erosion that was experienced on the STS-51B nozzle joint. This change was effective on STS-51D, STS-51G and subsequent.

2. Stacking procedures have been clarified to assure motor to motor consistency in joint mating. OMD changes were effective SRM 024 and subsequent.

3. Analytical studies based on both impingement erosion and blowby erosion show that this phenomenon has an acceptable ceiling.

4. Recent experience has been within the program data base.

5. The recurring erosion is addressed in subsequent Flight Readiness Reviews to assure visibility of the problem is maintained.

Brian Russell
Brian G. Russell

BGR/co

[Ref. 5/2-18]

MORTON THIOKOL INC.
Wasatch Operations

10 January 1986
E150/RVE-86-118

Mr. L. O. Wear, SA42
George C. Marshall Space Flight Center
National Aeronautics and Space Administration
Marshall Space Flight Center, AL 35812

Dear Mr. Wear,

Subject: Request for Change Order on SRM Seal Anomalies Efforts

Reference: A) MSFC Memorandum SA42-180-85

B) MTI Reply E150/RVE-85-221

MTI requests a contract Change Order to fund the on-going SRM efforts based on the following MSFC requests.

MSFC letter reference A) requested that MTI re-examine all seals and leak test procedures for the entire SRM. In addition it stated that MTI establish a program plan to validate the seals and test procedures. MTI developed a program plan and submitted it in our reply reference B). The plan objectives include all of the SRM joint seals and correcting their related flight anomalies.

MTI has created a Revision A to that (TWR-14359) program plan, which has been in effect since August, 1985. In conjunction with this overall seal effort, MTI has supported MSFC with visits and weekly telecon status reports. In light of MSFC requests and encouragement to fix the seal problem, MTI applies for a contract Change Order to cover these expenditures

Mr. L. O. Wear, SA42
10 January 1986
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Page 2

Currently the planned program is 33% complete and is expected to be finished prior to this summer. At this point in time, MTI Engineering will provide recommendations for the new joint arrangement.

Current expenditures through 1985 have been $2.0 million. The expected funding requirements to final completion is estimated to be $6.0 million. An ECP will be submitted to MSFC near the end of January quantifying the dollars.

Very truly yours,

Brian G. Russell

Brian G. Russell, Manager
SRM Special Projects

RVE: BGR/co

cc: J. Smith, E11
T. Adams, MTI/MSFC
J. Peoples, SA42

bcc: J. Kilminster
A. McDonald
R. Ebeling
T. O'Grady
D. Ketner
H. Hazelton

[Ref. 5 2-19 2 of 2]

MORTON THIOKOL INC
Wasatch Division

Interoffice Memo

1 October 1985
E150/RVE-86-47

TO: A. J. McDonald, Director
Solid Rocket Motor Project

FROM: Manager, SRM Ignition System, Final Assembly, Special Projects and Ground Test

CC: B. McDougall, B. Russell, J. McCluskey, D. Cooper, J. Kilminster, B. Brinton, T. O'Grady, B. MacBeth, J. Sutton, J. Elwell, I. Adams, F. Call, J. Lamere, P. Ross, D. Fullmer, E. Bailey, D. Smith, L. Bailey, B. Kuchek, Q. Eskelsen, P. Petty, J. McCall

SUBJECT: Weekly Activity Report
1 October 1985

EXECUTIVE SUMMARY

HELP! The seal task force is constantly being delayed by every possible means. People are quoting policy and systems without work-around. MSFC is correct in stating that we do not know how to run a development program.

GROUND TEST

1. The two (2) GTM center segments were received at T-24 last week. Optical measurements are being taken. Significant work has to be done to clean up the joints. It should be noted that when necessary SICBM takes priority.

2. The DM-6 test report less composite section was released last week.

ELECTRICAL

As a result of the latest engineering analysis of the V-1 case it appears that high stress risers to the case are created by the phenolic DFI housings and fairings. As it presently stands, these will probably have to be modified or removed and if removed will have to be replaced. This could have an impact on the launch schedule.

[Ref. 5 2-20 1 of 2]

FINAL ASSEMBLY

One SRM 25 and two SRM 26 segments along with two SRM 24 exit cones were completed during this period. Only three segments are presently in work. Availability of igniter components, nozzles and systems tunnel tooling are the present constraining factors in the final assembly area.

IGNITION SYSTEM

1. Engineering is currently rewriting igniter gask-o-seal coating requirements to allow minor flaws and scratches. Bare metal areas will be coated with a thin film of HD-2 grease. Approval is expected within the week.

2. Safe and Arm Device component deliveries is beginning to cause concern. There are five S&A's at KSC on the shelf. Procurement, Program Office representatives visited Consolidated Controls to discuss accelerating scheduled deliveries. CCC has promised 10 A&M's and 30 B-B's no later than 31 October 1985.

O-RINGS AND PUTTY

1. The short stack finally went together after repeated attempts, but one of the o-rings was cut. Efforts to separate the joint were stopped because some do not think they will work. Engineering is designing tools to separate the pieces. The prints should be released tomorrow.

2. The inert segments are at T-24 and are undergoing inspection.

3. The hot flow test rig is in design, which is proving to be difficult. Engineering is planning release of these prints Wednesday or Thursday.

4. Various potential filler materials are on order such as carbon, graphite, quartz, and silica fiber braids; and different putties. They will all be tried in hot flow tests and full scale assembly tests.

5. The allegiance to the o-ring investigation task force is very limited to a group of engineers numbering 8-10. Our assigned people in manufacturing and quality have the desire, but are encumbered with other significant work. Others in manufacturing, quality, procurement who are not involved directly, but whose help we need, are generating plenty of resistance. We are creating more instructional paper than engineering data. We wish we could get action by verbal request but such is not the case. This is a red flag.

R. V. Ebeling

[Ref. 5 2-20 2 of 2]

TESTIMONY OF GEORGE B. HARDY, DEPUTY DIRECTOR, SCIENCE AND ENGINEERING, FORMERLY MANAGER, SOLID ROCKET BOOSTER OFFICE; JAMES E. KINGSBURY, DIRECTOR, SCIENCE AND ENGINEERING; ROBERT G. EUDY, FORMERLY CHIEF ENGINEER, SOLID ROCKET MOTOR, OFFICE OF ASSOCIATE DIRECTOR FOR ENGINEERING; JOHN O. MILLER, TECHNICAL ASSISTANT TO SOLID ROCKET MOTOR MANAGER; AND WILLIAM L. RAY, SOLID MOTOR BRANCH, PROPULSION DIVISION, ENGINEERING DIRECTORATE

MR. KEHRLI: Gentlemen, my name is Randy Kehrli. I'm one of the staff investigators assisting

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the Commission. You all, I believe, have been interviewed by Commission panels or else by Commission staff investigators.

I would like to specifically refer you to a series of memoranda that were written in 1978 and 1979. They are included in this red book that is in front of you as tabs 4, 5 and 6 of the O-ring history. I believe you have all been asked about them in previous interviews. They're often referred to as the Miller to Eudy memos. It's my understanding they were actually written by Mr. Ray. [Ref. 5 2-21]

Just to summarize them—we won't go through them, but the first memo is dated January 9, 1978. It is tab number 4. That is a memo from Mr. Miller to Mr. Eudy at Marshall. It's my understanding it was written by Mr Ray.

That memo raises some concerns in the second page, paragraph (b), about minimum O-ring compression. Joint rotation is mentioned as well as various shimming or thickness of shims used in an attempt to increase the squeeze on the O-ring field joint design.

Additionally, following that memo in the same tab is the document entitled "SRM Clevis Joint Leakage Study," dated October 21, 1977, written by Mr. Leon Ray, which concerns—which identifies the best

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option for a long-term fix to the problems raised in these memos as option number four, to redesign tang and reduce tolerance on the clevis. [Ref. 5 2-22]

The next memo, which is tab number 5, dated January 19, 1979, again is a memo from Mr. Miller to Mr. Eudy, signed by Mr. Miller with a copy to Mr. Hardy, I believe again written by Mr. Ray, which states that the Thiokol position regarding design adequacy of the clevis joint is completely unacceptable for a couple of reasons, among them the large surface gap created by tang and clevis relative to surface movement and then secondly the secondary O-ring seal becomes completely disengaged as a result of this relative moment. [Ref. 5 2-23]

The last memo, tab number six dated February 6, 1979, is a memo directly from Mr. Ray to distribution describing a visit to Precision Rubber Products Corporation and Parker Seal Company, wherein the O-ring design and the function of the O-ring is discussed. [Ref. 5 2-24]

I believe what the Commission is interested in—and they will have some specific questions but if you could comment on the genesis of these memos, why they were written and what the thinking was at this time at Marshall with regard to the O-ring design.

CHAIRMAN ROGERS: And, I guess, how they were

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resolved after they were discussed.

Now, Mr. Ray wrote these memos?

MR. RAY: That is correct.

CHAIRMAN ROGERS: Could you start, and would you want to answer the question about why they were written and what was done about them?

MR. RAY: The reason they were written was as a result of test data that we had, and I have to go back to, I guess, a little bit further back in time than these memos. When the joint was first designed, the analysis produced by Thiokol says the joint would close, the extrusion gap would actually close.

We had quite a debate about that until we did a test on the first couple of segments that we received from the manufacturer, which in fact showed that the joint did open. Later on we did some tests with the structural test article, and this is mentioned in the memo as STA-1.

At that time we really nailed it down. We got some very accurate numbers on joint rotation, and we knew for a fact that during these tests that, just what the memo says, the joint rotated. The primary O-ring was extruded up into the joint. The secondary O-ring did in fact detach from the seat.

Now, the proof of this is that we, in addition

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to measuring the relative movement between the two parts, the two mating parts, we also screwed a pressure transducer into the port so that we were able to judge the performance of the secondary seal as well.

The test data showed that under a normal situation you get negative pressure, the cavity between the two O-rings, when the joint rotates. The reason for this is that while the O-ring is being extruded up into the joint and it should close the volume up and cause the pressure to rise, that is what you would normally think. But the fact that the joint opens up so much, it increases the volume. The ratio then goes the other way, and it becomes unbalanced. The pressure in the joint was negative.

Now, you would expect this to happen while the case is pressurized. What, in fact, happened during these tests is it did go negative but then when we got up on the pressure curve about 700 or 800 psi, the transducer showed—went back to ambient pressure like .12 or .24 psi, whatever the calibration was at that time, which was proof that the joint vented the atmosphere. That tells you the secondary seal is completely off the seat.

We did one other test in the static fire vehicle, which I can't recall which one, which didn't

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show this quite as clear but it was a very suspicious nature when the joint went negative. It's a little bit different. You've got a dynamic situation here where we change pressures during the static firing. The pressure goes up very quickly, flattens out for about 40 seconds, goes down into a saddle and then back up again, so you have fluctuation in the pressure in the joint.

But when we got up to the 700 or 800 psi region, we had a flat spot there very quickly or we were having a transient in the pressure, so it swept on by us. It was a suspicious nature. We had sort of a gentleman's running battle argument, if you want to call it, our people and Thiokol, over the years about this situation.

Our technical folks maintained the situation mentioned in the letter here did in fact happen. The Thiokol folks were saying a much lower number. It probably was not happening. We were successful after a time to convince them. I can't recall the days—I've been in Florida for almost three months now, and I haven't had a chance—I have not had access to my files, and so I can't recall the dates, but we were successful in getting the FMEA and the CIL, which are basically the Failure Mode Effects Analysis documents that state the reliability of the joint.

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We were, in fact, successful in getting this change to say that there were certain conditions that the secondary seal was ineffective.

DR. WALKER: When did that occur, sir?

MR. RAY: Sir?

DR. WALKER: When did that occur?

MR. RAY: I can't recall. I don't have—my records are one place and I'm in another. I haven't been back for three months, and I just haven't had time to look.

DR. COVERT: I think the record should show that Mr. Ray has been down in Florida looking at bits and pieces and hunting for them in the bottom of the ocean for about 90 days now.

DR. KEEL: Are you referring to the change in criticality from 1R to 1?

MR. RAY: Yes, sir.

DR. KEEL: That was December 17th, 1982.

GENERAL KUTYNA: Mr. Ray, those tests showed that you could have a shortfall in the ability to seal regardless of temperature?

MR. RAY: Yes, sir.

GENERAL KUTYNA: Because of rotation of the joint?

MR. RAY: Yes, sir. Rotation has nothing to

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do with temperature.

In regard to the letter, the rather long memo there about the seal, the quality of the O-ring, that was all taken care of. We made a trip to Parker, and we went through all the defects that are mentioned in that memo and were successful in coming up with a good product. As a matter of fact, somewhere in the records there's a letter written that states that fact that we were real proud of the folks that bid that work, and they got us a good quality O-ring at no additional cost.

The letter also refers to shimming of the joint. This letter specifically refers to shimming, in this case, of the STA-1, the static test article. There was a proposal to put, I believe, 15 to 20 thousandths shims in the joint.

If you look at the joint clearances, you will see that potential gap there that could exist could range from 30 to 65 thousandths, so that tells you in the worst case you need to put a rather thick joint in there or shim in the joint like 50 thousandths, which is a reasonable thickness, you could probably get into the joint.

We recommended that we shim up the maximum. As a matter of fact, we came up with three different shim sizes that would be required to put into the

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inventory to shim up, to come up with a range that is recommended by everybody is 15 to 25 percent compression.

Now, this only refers to the initial compression. We have no data that says that shimming will in any way diminish the change in the gap because of rotation. We have no data one way or the other but it is a fact that it is highly desirable to have high initial compression. You need that because of defects in the sealing, potential defects in the sealing surfaces and the O-ring from contamination and so forth.

That was our point in that letter. We would like to have shimmed up the maximum under all circumstances and particularly here for the STA-1. We were unsuccessful in getting our way on this, and I can't tell you who made the decision. I don't know, but I think I can enlighten you on some of the discussions taking place.

The contractor's recommendation was not to do it, to go with a one-size standard shim 35 thousandths thick. They decided the reason is logistics. You have to stock a number of different sizes. It would be a logistics problem, and so I can't remember who made the decision at Marshall. I just have a blank on that.

DR. KEEL: Could I just ask a followup question? Your position according to the memo, Mr. Ray,

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back at least in January 1979, as you have already indicated based upon these static article test as well as the hydroburst test back in 1977 was again, to read from the memo, "that the Thiokol position regarding design adequacy of the clevis joint to be completely unacceptable."

You mentioned the two reasons that have already been referred to. One, of course, is excessive tang clevis relative movement. Now, has your view changed at all? Is it your position, then—it was your position then that it needs to be redesigned, right?

MR. RAY: Yes.

DR. KEEL: Has it changed today?

MR. RAY: No.

DR. KEEL: It of course wasn't redesigned. This memo went to Mr. Eudy?

MR. RAY: Yes.

DR. KEEL: Mr. Miller, you signed it out?

MR. MILLER: I signed it out.

DR. KEEL: Did you concur with that?

MR. MILLER: Yes, I did.

DR. KEEL: Is that still your position today?

MR. MILLER: Yes.

DR. KEEL: And Mr. Eudy, you got the memo?

MR. EUDY: Yes, sir.

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DR. KEEL: What action did you take?

MR. EUDY: Well, of course, I recognized that the problem has been an important problem and an area of concern, and we had a number of technical interchange meetings and a number of telecons with our contractors and of course with these people, and we work with them on a daily basis, and we try to listen to all the data. Of course, we had management reviews, and we tried to consider the test data that has come out of the test firings in the structural tests, and we tried to consider all of the data that was available to us.

Our view was to proceed with the shimming and compressions as we proceed with—this was with everyone being informed of the test data and the results of the tests.

I might comment on the structural test article, I think there was—so far as shimming on the structural test article, I think we did end up shimming on the low side, from what Leon and some other people recommended, and I think at that time part of our position was to go to the low side, to in fact insure on the low side that we were covering those low side concerns.

So that was part of the thinking.

DR. KEEL: The low side with respect to initial

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compression?

MR. EUDY: Right.

DR. KEEL: But that did nothing with respect to the relative motion?

MR. EUDY: That's right.

MR. RUMMEL: At the point when that decision was made, had the cases been committed to production?

MR. EUDY: Oh, yes.

MR. RUMMEL: They had been, so there was a production line going at that time and, I assume, several cases on hand?

MR. EUDY: Yes, the production line was running, and there was considerable data base that we had with tests results from those motor cases.

MR. RUMMEL: Do you know, had you made a decision at that time or had NASA made a decision at that time to redesign the joint and gone to some different configuration, what would the impact have been on the flight schedule? Can you say approximately?

MR. EUDY: Well, I guess—and maybe Mr. Hardy would like to comment, but I guess we would have been down probably two years. We would have been back to the billet stage, all the way through the pipeline.

MR. RUMMEL: I assume that was one of the factors you took into account and decided to continue

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with the unsatisfactory joint, is that correct? I realize the word unsatisfactory——

MR. EUDY: I didn't consider it unsatisfactory. This was flying well in Titan, and we had compressions in the same range we're dealing with in the current design.

MR. SUTTER: How do you know it was flying well in Titan?

MR. EUDY: Well, all of the ground programs and the things that all of the test data we had showed that—well, and all of the programs we had with our own joints looked very good, and so we had absolutely no test failure history of any kind.

MR. SUTTER: Well, there wasn't any history. That's the point I was just making at the last meeting, that there is no data base. I don't consider Titan a data base, since every time they shoot one off it comes back in parts, and here you're talking about designing something that you're supposed to bring home and use 19 times over.

MR. HOTZ: Well, it is a different joint anyway.

MR. SUTTER: But referring back to Titan, I think it's something that I don't think there's any basis for.

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MR. ACHESON: Could I ask Mr. Ray in the tab 6 document the visit to Parker, it's reported here by you the Parker experts thought that the O-ring was being asked to perform beyond its intended design.

Now, what was meant by that, that the O-ring ought not to carry primary combustion pressure or that it had too large tolerances, or just what?

MR. RAY: Let me give you a little background of what actually took place, or I will answer directly if you like, either one.

Mr. Eudy and myself prepared a presentation to Parker Seal and also Precision Seal Company. Mr. Eudy and myself went to precision, and I gave the presentation. What it says is that we were generating a 41 thousandths extrusion gap at that time, and this is the number we knew at that time. We've got the hardware filled. We're going to be flying this thing shortly. What do we do? What do you recommend?

We went further on to Parker to Berea, Kentucky and made the same presentation up there, the identical presentation, and we got the same reaction, that that's not the way you use an O-ring. You don't use an O-ring with a large extrusion gap. You would prefer to have it zero if you could get it, but that's not possible, but we will let you know later on.

So they wrote a letter to us, and I don't know whether you have the letter or not. The letter says, in essence, the best I can remember it says we realize you're in a bad situation. You're not supposed to use it like that but do all the testing you can to satisfy yourself that it will work, and that's what the letter says.

In saying you're not using the O-ring in a way it's not intended, meaning we had a very large extrusion gap, i.e. 41 thousandths, if you look at the Parker curve and the Parker manual, it's very deceiving. There are no curves on static data extrusion gap. This is a dynamic curve in the cycle.

There are numerous pressure cycles. This one is generated by general motors. If you look at that curve, we're not even on the sheet of paper, okay, with that extrusion gap that we have, much less on the curve. But again, that is a dynamic situation and so we really have no written standard as to what our extrusion gap should be except as recommended by the Parker experts which says hopefully zero, as little as possible but sure not 41 thousandths. That is the data, the reaction that they gave us.

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MR. KEHRLI: Mr. Ray, we do not have that letter. If we could get a copy of that, we would appreciate it.

MR. ACHESON: So you have a joint that opens up, much to your surprise, and you thought it ought to close under pressure, and you have an O-ring which the manufacturer thinks is being asked to do a job that it isn't meant to do. And what else entered into the equation that made you satisfied with that design, Mr. Eudy?

MR. EUDY: Well, I was with Mr. Ray on one of the visits, as he mentioned. My perception of what the people were telling us and the conversation we had with people was what we were doing was outside their data base. They were used to, if you would, O-rings on pistons and a cylinder and they understood where we were relative to having flight hardware cut. testing flight hardware.

And I think the bottom line that I received when we left there is that you're going to have to go back and use your data base. You are in a region that we can't give you direct consultation on. You will have to look at your data and how your joints are performing, and that is why at least I, as an individual, put a lot of stock in the test data, looking at the assembly

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problems and how much compression and so forth was needed on the joint, to come to a decision as to where we should be.

DR. FEYNMAN: In other words, you didn't discover that it wasn't true; that it did have a big gap, much bigger than the usual application. And it also was true that you had an opening in the gap. All your tests only confirmed that the conditions were just the conditions.

And what test was it that showed you that that thing would work?

MR. EUDY: Well, from my point of view, we had a number of pressure tests, static tests there in Huntsville. We also had the whole development and qualification series in Utah and every one of those motors were torn down and the O-rings were inspected, and out of that whole development series of seven motors, we didn't have any O-ring problem.

DR. COVERT: Were they fired horizontally?

MR. EUDY: Yes. There was no erosion on any of them. And so again, we tried to use the data base that was available to us with the hardware that was available to us.

DR. SUTTER: Were any of those used segments?

MR. EUDY: I believe in the latter

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firings and George, you have to help me they were used, we did reuse segment in the Qual program.

DR. COVERT: That is my memory as well.

DR. WALKER: There's no discussion in these memos about the role of the putty. To me, the putty is a thermal barrier.

Did that enter into your decision at all, the question of whether or not the putty would indeed serve as a proper thermal barrier? Because temperature isn't mentioned in any of these memos, but when you have 5,000 degrees, that is not the sort of thing that an O-ring is usually designed to do.

MR. RAY: Are you directing the question to me, sir?

DR. WALKER: Whoever feels they have information to answer it.

MR. RAY: I think since the inception of the design of the joint and the use of putty, we have never considered the putty as a seal, although it is called a sealant material, but we don't consider it as a seal. We consider it a thermal barrier.

Now, there's a lot of question as to whether putty is really necessary, and that's another question, whether you need anything or not, because you do have a rather long tortuous path down to the O-ring. so that

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you don't really have any irradiation effect from the combustion temperatures down into the O-ring.

You don't really have a direct look at it. It can't see the O-ring directly. It has to go around quite a few turns.

DR. WALKER: But, by convection, you could get hot gas down there?

MR. RAY: Yes, you certainly can.

CHAIRMAN ROGERS: Do you want to speak a little louder? The stenographer is having trouble hearing. If you would raise your voice.

DR. WALKER: I said you do have the possibility of convection of gas down to the O-ring and that happens if you have holes in the putty.

MR. RAY: That happens because we have holes in the putty and we know why we have holes in the putty; there's no doubt about it. The joint is never round; it's never concentric. It is a rather large flexible piece of hardware.

To aggravate the situation, we have a bunch of rubber in there that has to interface together, and it sags because of the propellant weight, no matter what attitude it's in. It sags worse, of course, if it is in the horizontal as opposed to vertical.

If it's in the vertical, it sags down and

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still changes shape, so you can put the putty on there as carefully as you want to. When you put it together, you're going to have some places that are going to have rather large gaps, some places are going to be rubber to rubber. You're going to wipe it off.

That's one source for generating a hole through the putty.

We've known for some time also that when you put the joint together, that we entrap air down in the end of the little tongue and groove. That's the best way to describe it, I guess. The air bubble escapes and it blows a channel through the putty and I just tore down or destacked one at the Cape and they saw that. It's no news to us.

We actually walked inside of a motor case after it was put together, and you can see those volcanoes come through there. That's another source.

MR. HOTZ: Is this the recent destacking?

MR. RAY: Yes. I don't think there's ever a case when you put a case together without having some injury to the putty. So the putty may be a contributor. We really don't know whether, if you have one hole through the putty as opposed to eight or ten, whether that has any effect on it or not. Some people believe it does; that one jet trying to fill a single

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cavity is more detrimental to the O-ring than having several jets trying to fill the same body.

MR. ACHESON: A question for Mr. Eudy. I guess the implication is sort of in everyone's mind that the threat of program delays and the threat of cost overruns had something to do with your decision, or at least with Marshall's decision to fly with a basically unsatisfactory joint.

My question is: If you know or could guess in an educated way, what threshold of cost or delay would have been acceptable from the point of view of saying okay, with that much cost increment and that much delay, we can tolerate going back and redesigning the joint?

MR. EUDY: I guess I can't say. Let me just say that where I was in the organization at the time, I was, if you would, on the engineering side and so I had the pleasure of being pure in that regard of saying what I thought the right technical judgments were, if you would, apart from cost and schedule.

I can honestly say the judgments I made at that time were based upon my engineering background, the engineering drawings I was looking at, and the engineering data in front of me and the test results in front of me.

Obviously, the issue is there, but at least I

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never sent a recommendation forward to continue with a program that I thought was faulty design.

MR. ACHESON: You're saying from an engineering point of view, you thought it was a satisfactory joint to fly?

MR. EUDY: You bet.

DR. KEEL: Can we qualify the record here, just to make things clear? That at that point you say the engineering data you had—that point, of course, was January 1978 and this is January 1979 for the other—and how many tests did you have in January 1978 other than the two that had failed? The STA article, the static article that had a problem and the hydroburst test where you couldn't get it to seal, or any other test available at that time?

MR. EUDY: I think that is a point of perspective as to whether those failed or not.

DR. KEEL: Well, were any other tests available at that time?

MR. EUDY: Well, I'm a little bit like Leon Ray's condition in that I've been off the program since early 1981, and so you're asking me for memory that I don't have.

DR. KEEL: Does anyone know?

MR. EUDY: There were static tests in Utah at

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that point. I don't know how many, and there were also lots of joint structural tests and other tests that we had been running during that whole period to come up to that point.

So there were——

MR. HARDY: What was the time reference?

DR. KEEL: January 1978.

MR. HARDY: There had been ground firings at that time, one or possibly two. I'm not sure.

DR. KEEL: Well, maybe you had one or two others where you didn't have a problem, but you certainly had two tests where you did have problems.

Now, was that enough engineering basis to say it was sound and you didn't need either a long-term or short-term fix?

MR. EUDY: As I was going to say, on the two tests you referred to having problems, they had problems, I think, within some ranges that you would expect to have problems. You're running one to burst, and structural tests, you're running some limits there, too.

DR. KEEL: But you aren't saying that the fact that you ran the pressure up is why you had rotation, are you?

MR. EUDY: I'm not sure I understand.

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DR. KEEL: Well, are you saying you wouldn't have had a rotation problem if you hadn't exceeded normal operating pressure?

MR. EUDY: Oh, no.

MR. HARDY: But I think one could say that you wouldn't have had rotation, just to failure of the O-ring if you had not exceeded the operating pressure by 50 percent.

DR. KEEL: What about to unseat the secondary O-ring?

MR. HARDY: I think to unseat the secondary O-ring was the information that was learned during the static test.

DR. KEEL: So that wouldn't have had to run above pressure to do that?

MR. HARDY: That is correct. I think that was the "new" information that we learned from that test article.

MR. RAY: I think what you're referring to—excuse me.

MR. HARDY: Well, I just wanted to add that I believe the two instances you referred to that represent test failures is, as Mr. Eudy said, that in both cases those were limit tests where the O-ring was taken far beyond the planned operating pressure.

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DR. KEEL: But I guess the point is, and if I could make the point again, is that the concerns that were raised had nothing to do with the fact that you were raising the pressure above operating pressure; namely, the one of the tang and clevis motion.

MR. HARDY: That is correct.

DR. KEEL: If we could continue here. So, Mr. Eudy, you ultimately, on whatever basis you testified today, you decided that it didn't need to be redesigned.

Now, were you reporting to Mr. Kingsbury at that point?

MR. EUDY: No. During that point I was reporting to, I believe, Bob Marshall during most of this period, William R. Marshall, who was Associate Director for Engineering there, who in turn reports to Mr. Kingsbury.

DR. KEEL: Was he reporting to Mr. Kingsbury?

MR. EUDY: Yes.

DR. KEEL: Did Mr. Kingsbury get passed the information?

MR. EUDY: I feel reasonably certain that that's so.

DR. KEEL: Well, did you do it?

MR. EUDY: I can't say with certainty if I

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personally did. Again, that is a question—somebody else would have to go back to 1978 and 1979.

DR. KEEL: Well, did you get the information, Mr. Kingsbury?

MR. KINGSBURY: No. I cannot comment in this time frame about these detailed concerns of that joint. I just was not involved.

DR. KEEL: Well, they are fairly critical concerns, though. Don't you think if you had known about it, you would remember? Is that a reasonable statement?

MR. KINGSBURY: Well, I would expect I might remember, yes. I have no recollection of these discussions at all.

My discussions became, I got involved in this thing late, as Leon and John know, when we were well into the flight program.

DR. KEEL: But, Mr. Hardy, you got a copy of the memo. Do you recall the memo, in fact both the 1979 memo you got a copy of specifically, which was the one that raised these concerns? You were then the Solid Rocket Booster Program Manager?

MR. HARDY: Yes, that is correct. And I've been asked that question before and I cannot with certainty remember the time and the memorandum itself. I do have some recollection of the issues that were

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being discussed at that time.

In my office those were handled principally by Colonel Rice who was my Deputy. However, I was involved, and I specifically remember the issue having to do with shimming. And I specifically remember the discussions that Mr. Ray referred to here between Thiokol and Marshall technical people on trying to understand precisely what was the joint rotation, and whether or not in fact the measured joint rotation and the static structural test represented the proper quantification of that.

Other things I can remember beyond that is in fact that the joint was shimmed; that we did apply shims. I believe there were 35/1000 nominal shims were put in the joint. Other things I remember was that eventually after this time, there was a formal design review and there was a certificate of qualification executed against this piece of hardware, as there was against each piece of hardware.

And to the best of my knowledge, the responsible design engineering people at the Marshall Space Flight Center and the responsible design engineering people at Thiokol signed that certificate of qualification.

It was presented to me prior to the first

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flight, STS-1, as being an agreed-to executed certificate of qualification.

DR. KEEL: Let's move on then to in fact the certification and verification process.

The next documents or another set of documents that the Commission has refers to the Space Shuttle Verification and Certification Propulsion Committee which was chaired by General Morgan, and we have documents from July 1980 which have been made available to you.

In there, they specifically express concern about verification of redundancy, and they say with respect to this joint, of course, they say the committee was concerned that the redundancy feature was not verified by purposely failing one element in such a way to simulate all important failure modes.

And they go on to say that the committee understands from a telecon that the primary purpose of the secondary O-ring is to test the primary O-ring and that redundancy is not a requirement. [Ref. 5 2-25]

Do you know the basis of that statement?

MR. HARDY: I remember the review that you referred to there that General Morgan chaired. I do remember some discussion about testing the capability of the joint to seal with one O-ring; in effect, the

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capability to seal if one O-ring fails.

To the best of my recollection, that recommendation was implemented with a test that was a hydroburst or a static hydrotest with one O-ring missing. That is my recollection. I could be wrong on that, but I think that's true.

The discussion there or the reference there to a telcon—and I don't know who that was with—that that implies that there was no intent for the joint to be redundant is totally foreign to me.

I don't know where they would have gotten that information because that was the design requirement for the joint.

DR. KEEL: Wouldn't you have seen those documents, though, since you were then the Solid Rocket Booster Program Manager?

MR. HARDY: Which document are you referring to?

DR. KEEL: The committee reports, Space Shuttle Verification Shuttle Propulsion Committee

MR. HARDY: Yes.

DR. KEEL: You would have seen them and presumably read them?

MR. HARDY: Yes. And as I said, I think the one recommendation or discussion there having to do with

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verifying redundancy—to the best of my recollection, the further discussion of that and disposition of that recommendation was to run a test, a hydroproof test, with one O-ring. And I believe that was done. I can find out for sure. That is the best of my recollection.

But at the time, or as you remind me here, I don't know any discussion—and I may have read that—but I don't know any discussion that had to do with any claim on the part of anybody that it wasn't a requirement for the joint to be redundant.

DR. KEEL: I just have—well, it does say it here, of course, and you have that document, but if you have further information maybe you could provide.

Just one further question, since you brought up the verification point, the recommendation, aside from the fact that they had been told redundancy was a requirement, they then made a recommendation that the upcoming lightweight case test should be expanded in scope to also be a thorough field joint verification test and not just for redundancy, but a full field joint verification for STS-1.

Now, were in fact those cases or those tests expanded prior to STS-1 and was that complete verification consistent with this committee

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recommendation done prior to STS-1?

MR. HARDY: I cannot tell you. I just don't remember. The only thing I can tell is that the closeout of every one of those recommendations was reviewed with the committee. That committee, by the way, was chaired by Walt Williams, and there were several panels and General Morgan headed one of those panels.

The closeout of all of the recommendations was reviewed with and, to the best of my knowledge, resolved to the satisfaction of the panel or the committee and also reviewed at Level II and I believe at Level I within NASA.

But I could possibly research and find out exactly what was done with regard to the specific recommendation that you referred to.

CHAIRMAN ROGERS: That would be helpful.

Going to a somewhat different subject on the redesign process that you're working on now, and I guess Mr. Kingsbury heads that up. Are Mr. Ray and Mr. Miller included in that?

MR. KINGSBURY: Mr. Ray has been at the Cape. Mr. Miller is in the Program Office and has been working with us. We have all of the people that have been in this program in the past involved, plus many many others.

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CHAIRMAN ROGERS: How about Mr. Ebeling at Thiokol, is he on it?

MR. KINGSBURY: Mr. Rogers, I can't tell you what the Thiokol staffing is. I have a staff. We are running a parallel effort, and I am not familiar with the details of their staffing.

CHAIRMAN ROGERS: I have no other questions.

GENERAL KUTYNA: Mr. Kingsbury, just one. The Thiokol gentleman, as he left the room at the last session, expressed some surprise that we were letting orders to start cutting metal on the 7th of May on the redesign joint, well before the Commission has come out with its recommendation.

Would you like to clarify that and say what we're doing exactly?

MR. KINGSBURY: The way the SRM program is built, it does have hardware as a function of time, and last July an order was placed for 72 segments, case segments. At that time the program had incorporated a capture feature on the filament wound case which uses only a ring, as you will recall, which attaches to the filament wound case when the joints come together, and the program at the time they ordered the 72 joints asked the forger in Wisconsin to forge these total case segments such that there would be sufficient metal that

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a capture latch could be incorporated at the tang end of the joint if that decision was made.

Now, those segments began arriving at Rohr. I have been up here all week, so I don't know what day it is, but if this is the first of May, they began arriving today. Rohr has asked us, us being Thiokol and Marshall, to give them guidance on what we would like done so that they can begin planning their tooling, and they say they would like that information by the 10th of May.

We have been working parallel but independently now for about three or four weeks. We have met twice, and we meet again next week and attempt to come to a conclusion on any differences we want in the metal itself. It will be, to a degree, a kind of an omnibus thing that leaves us several options that we may then pursue which will require further refinement at a later point in time, but so as to keep the program going. It is our intention to give them information.

DR. WALKER: Who is we?

MR. KINGSBURY: We is Thiokol with Marshall concurrence, but it is a Thiokol contract and Thiokol will issue that direction.

DR. WHEELON: A series of questions, if I may.

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How many of these new forgings are now available?

MR. KINGSBURY: There will ultimately be 72. They are coming in pieces.

DR. WHEELON: But how many are there now?

MR. KINGSBURY: Today, none, unless if today is the first of May, there are two, or the second of May, there are two at Rohr and there will be four more by the end of this month, as I understand it.

DR. WHEELON: And do these new forgings allow for a redesign or a limited redesign or no redesign?

MR. KINGSBURY: A redesign, as we see it at this point in time, we can make a total redesign we need within the forging billet that we have.

DR. WHEELON: When could those first forgings be available, and when could they be loaded, and when would they be ready for flight?

MR. KINGSBURY: I'm going to now give you some very, very early data that we are pursuing. Rohr has committed to deliver two finished forgings end of July, four the end of August, and six each month thereafter.

DR. WHEELON: So when does that get us ready?

MR. KINGSBURY: That gets us to where we can have test motors available, we believe, in the December-January timeframe, and flight hardware could be

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available mid-next year.

DR. WHEELON: If the new design can be accommodated within these new forgings.

MR. KINGSBURY: Yes.

DR. WHEELON: Do you know yet enough about the redesign that you want to make to be confident, sort of confident or not confident about the ability of these new forgings to accommodate the new design?

MR. KINGSBURY: Now, you are going to talk about engineering judgment, because the analysis—

DR. WHEELON: That's what you get paid for.

MR. KINGSBURY: I'm highly confident that we can make a completely reliable, easily assembleable, environmentally independent joint within the forging we are getting.

DR. WHEELON: Super.

VICE CHAIRMAN ARMSTRONG: I have a question.

The Commission has not drawn conclusions to this point as to what recommendations it might make with respect to redesign. It is certainly within our charter to do so, should we elect to do so, but since we haven't made conclusions at this point, it might be helpful, to the extent you are able, to tell the Commission what sort of guidelines and ground rules you are including in the redesign effort.

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MR. KINGSBURY: I would be glad to, Mr. Armstrong, and I understand the Commission has not reported, and I further would comment that with respect to my friend General Kutyna, I must know the answer to the TITAN problem before I can be thoroughly comfortable also, but understanding that those are still outstanding, we are pursuing the basis that there were several things about the joint which were not forgiving. Rotation is a bad thing. We think we can resolve that issue. Putty, so far as I have been able to determine, nobody has found anything good to say about putty, and we expect to be putty-free. We will be environmentally independent either by conditioning or by protection, and by that I mean we will put a weather seal on and preclude water, and if thermal environmental conditioning is required, we will provide that.

Now, I say that not because I don't think temperature may have contributed to this program or to this problem, but we believe now we have a concept where we are where we were in or thought we were in 1976, and I can only tell you that from what I have heard, and that we now have a concept where the joint actually closes when it rotates rather than opens.

And so, resiliency does not become a factor. But we will qualify for a spectrum of temperature that

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far exceeds the requirement. And those are the things that we are putting into it.

Now, anything else that comes out we will have to get back into it, and we will have to go back and look at those factors, but those four elements we recognize need correction.

DR. COVERT: Insofar as it still sounds like it is going to be a reusable joint, do you intend to take a stack and fire it and hydrotest it and clean it and reload it and fire it some number of times so that you get out in front of your usage and encounter the things like case growth and so forth early?

MR. KINGSBURY: Dr. Covert, we've got to understand this case growth thing, and I can tell you now that we do not put sufficient stress in the joint of the membrane of the segment to exceed even the lowest proportional limits you can imagine. And so it is not a simple, we are overstressing it to pressure.

DR. COVERT: I did not mean to imply that it was simple. I meant to suggest, do you test it?

MR. KINGSBURY: Yes. We must understand that very thoroughly. There's absolutely no question about that. We are dealing with tolerances that we are talking about here and you are talking about with these gentlemen of .010 or .020 on a 12 foot diameter piece of

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metal, and although .010 or .020 may seem like sloppy tolerance on a piston for your automobile, it is awfully tight on a 12 foot diameter cylinder. And so we have got to understand that problem and we may end up having a logistics track that says you match segments before you cast them so you know when you get them to the Cape they will fit. We will use ground support equipment to assist the joining, so we know we put two round pieces together and don't worry about somebody has something that doesn't fit, and he hangs it sideways or whatever to try to make it round.

Our intention and our goal is to have a joint that goes together pretty much like anything that you put together at home when you buy it, and it doesn't take any labor-intensive operations.

DR. COVERT: What I am suggesting is it might be prudent to establish a lead-the-fleet type program where you have a test article that you continue to use over and over again and get a lot of usage out of them early on and be ahead of the problems you encounter.

MR. KINGSBURY: That may be, but one of the things that still has potential consequence to us is there is a possibility that we do not have a complete material transition, and if we do, you can solve that with heat treating. And I think once we understand those

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kinds of details, then we should be able to resolve the issue that is in front of us. We simply do not understand that case growth problem, and we cannot describe it mechanically. It does not fall into a mechanical description.

And so it is an unknown that has to be dealt with, and that could cost us some segments, there's no question about that. It is our intention to understand it and see if we can resolve it, and if it means early termination of the life of some segments because of the condition that they are in, that is what it will mean, they will get thrown away.

I guess I should mention that it is our intention to incorporate the new joint at the field joint, and we are looking at the factory joint, but at this point in time we do not plan to do anything more than perhaps increase the insulation thickness, because that is simply just a piece of membrane.

DR COVERT: But let me go back to my question again.

There is a process in many propulsion systems, and many, in fact, aircraft systems, and it might even be prudent in the long run in space systems to select an article, run it through a cyclic loading series that represents the usage, do it repeatedly so that you have

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exposure on this one to all kinds of things that might not show up in analysis because it is the real world, and I'm suggesting you might want to think about this as a useful thing to preclude other problems we might not understand at this point.

MR. KINGSBURY: Certainly I understand what you're saying. You and I have done it on other programs. There is an element of use I cannot introduce which is of concern to me, and that is water impact. And I have got to look at that.

DR. COVERT: We have got to think about there may be a way of having to simulate that.

All right, thank you.

MR. SUTTER: What about the testing to qualify this joint? Some preknowledge of that might be useful, too.

MR. KINGSBURY: We have a very extensive test program. We start at the component level using cold gas just as an indicator because we could do it quickly. We will stay at the component. Our next series will be probably at the component level using hot gas. We will then go to a hybrid system where we can represent the pressure, the ignition pressure cycle that is up at the same rate and stay there so that we can induce whatever mechanical--

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MR. SUTTER: Will it include the twang effect?

MR. KINGSBURY: We will do twang tests, certainly, on the structural test article, but we're looking at how can we with hot gas cause the inflection that occurs in the hardware without having to run two-minute static test program that we can run on a cycle of about one every three or four months, and that is an awful lot of testing. And we have this hybrid system that we are putting in place that looks very attractive at this time. One of the problems that has not been thoroughly resolved or has not been resolved at all deals with how much full scale testing must be done, and it is a very simple question. I can say analytically, I can show you, I have dealt with everything that has come out when it is all over, and I clearly by analysis am better, and I will run a number of these, yet to be determined.

If you are a statistician you can say to me that's not enough, you have got to at least equal the experience with the last joint before I say you are good enough to fly, and so that discussion has not occurred, and it simply hasn't permitted me to resolve that issue at this point in time.

There will be some number, obviously, of

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static firings, hot firings.

MR. SUTTER: It would seem to me that that initiative ought to be pushed to the forefront because that is obviously going to cost a lot of money, and that is why it wasn't done before. And how much money needs to be spent ought to be put out there so people can start worrying about it.

MR. KINGSBURY: Mr. Sutter, that subject is being discussed. I discussed it yesterday, as a matter of fact, while I was here, and it is going on today.

There will be other discussions to size the program so we can know what the money sizes are.

DR. COVERT: Might I suggest also you consider firing vertical as opposed to horizontal, and I realize that is an expensive proposition as well.

MR. KINGSBURY: Let me just offer for your benefit some of the problems firing vertical. I have no way to hold it down. The aft skirt will not hold it, and so I have got to build a structure.

Setting that aside, there is not a test stand available in the country that we have been able to find—and we have been searching for three weeks. We tried vertical either way, fire up or down. We cannot find a facility. And the best time we have had suggested to us, and I have some questions as to the

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validity of the offer, is 15 months to have a test stand available.

MR. SUTTER: How about firing one down and one up at the same time?

(Laughter.)

MR. KINGSBURY: Mr. Sutter, we planned that, but we want to gimbal the nozzles, and we ran into trouble with our gimbal nozzle pattern in getting the balances in.

MR. HARDY: You are not suggesting to take the thrust out on the forward dome of each of those two, are you?

(Laughter.)

MR. SUTTER: I think, though, that asking some of your smart testing guys what kind of testing can we do, they may have been inhibited before due to lack of funds. My only comment is I really don't believe that this ten-inch small-scale test is hardly worth a damn.

MR. HARDY: You are absolutely correct. That is not the way to qualify the new joint. I think that gives you some early on information, but that is not all the answers.

MR. KINGSBURY: The only intention of the small scale testing is, for example, if you remove the putty from the joint, I'm not sure the current joint

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configuration, in fact, I feel reasonably confident the current joint configuration is not adequate. It is too wide. Furthermore, it is not enough of a tortuous path. I need to take more energy out that I will get into turns. So that has to be changed.

And I think we can confirm that path on a small motor.

MR. SUTTER: That kind of testing is very valuable.

MR. KINGSBURY: That's what we use it for.

DR. FEYNMAN: Mr. Kingsbury, if I were a statistician and I was trying to determine if you could fly, what probability would you—should I try to establish that the thing will not fail in the future, that is, in determining the number of tests. I have to know that figure so when you say you qualify it, what kind of probability are you qualifying it for?

MR. KINGSBURY: I can't answer that question at this point in time. I obviously am qualifying for the maximum that I can get, and I have got to have some goal. I am not aware, there is a program goal that says you must demonstrate this reliability with this probability. That is worked out on individual elements, and that has not been done on this particular exercise.

GENERAL KUTYNA: But headquarters is claiming

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or was claiming a probability of failure for the solid prior to this time as a result of that nuclear investigation from Galileo. What was the probability on that?

MR. KINGSBURY: On an order of 10^{-5}. Now, that is at any second of time in the flight trajectory, and that was, I believe, misunderstood by a number of people. If you say what is it over the full spectrum, you have got to divide it by 120. But at any point in time it was on an order of 10^{-5}.

DR. FEYNMAN: 10^{-5} per second, the probability was 10^{-5} per second?

MR. KINGSBURY: No, at any second, in any one second slice, the probability of failure in any one second slice was 10^{-5}.

DR. FEYNMAN: That's what I said, it was 10^{-5} per second. In two second slices it would be 2 times 10^{-5}, is that right?

MR. KINGSBURY: Yes, you could put it that way.

DR. WHEELON: So the chance of a solid rocket motor failing is about 1 in 10^{-3}?

MR. KINGSBURY: Those were the numbers that we had, that is correct.

DR. RIDE: I just wanted to comment, it's my understanding that NASA does have a program philosophy

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on which the acceptable probability of a solid rocket failure is zero.

MR. KINGSBURY: Well, certainly, Dr. Ride, that is the philosophy on everything that we have, but there is nothing in this world that has a probability of either 100 or zero. It is somewhere in between those two numbers.

DR. RIDE: I appreciate that.

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MR. ACHESON: Did I hear you to say that the new design will have as one of its goals that the O-rings will be protected in some way from either binding by misfit in the stacking process, or injury in the stacking process?

MR. KINGSBURY: Yes, you did.

MR. ACHESON: And if so, how would that be achieved?

MR. KINGSBURY: Well, let me have—I have a three-hour briefing that does this. Let me try to tell you quickly.

CHAIRMAN ROGERS: Shorten it, if you would.

[Laughter.]

MR. KINGSBURY: I intend to put a ring on both sides of the joint, a fairly massive ring which will make that joint round at the top and bottom. Now, to assure when they come together, I don't have an offset on the center lines, I have another ring on the bottom that extends out further than the inside—where the outside leg of the clevis, and so I force it wherever it's coming down. I force it to go in the middle.

MR. RUMMEL: How about spring back when the ring is removed?

MR. KINGSBURY: Thank you, sir. At that point in time I will custom shim around the diameter of the joint, and only then will I take the ring off.

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MR. RUMMEL: So, whatever stress may be there the spring back will just be there, and the shims are tight enough so the segments won't move. Is this what you are saying?

MR KINGSBURY: No, there will be some motion, probably of an order to 0.002 to 0.003 maximum, we could have at ignition, and we will calculate based on measurements being made today and that have been made over the past several weeks and will continue to be made on cases that have been loaded, what is the spectrum of out of round that we have so that we know what kind of local strain we have to put in, and then we will do a statistical analysis and put a three sigma worst on that, and that is the number we will use to assume residuals to be sure that we do not have a problem of residuals when we are locked up.

MR. RUMMEL: Are they shipped in those rings?

MR. KINGSBURY: No. The rings will be applied at the Cape.

MR. RUMMEL: So, they could come in out of round?

MR. KINGSBURY: I think it is axiomatic, they will come in out of round. They are shipped flat, lying down, because they cannot—we cannot clear the railways standing up. We will then put them on a

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transportation dolly and attempt to begin the rounding process at that dolly, and then the final rounding will be done with the rings.

MR. RUMMEL: In other words, you will try to push them back into round before the rings—

MR. KINGSBURY: I will stand them up and put them in a round fixture on that dolly that matches the tang which is down, force that to begin the rounding process, but I won't depend on that. I will conclude with an actual fixture that does it.

DR. FEYNMAN: Mr. Kingsbury, I'm very confused. You've been using these probability numbers and I've suddenly discovered they're not what I've thought. The flight has the engines running at least 500 seconds.

MR. KINGSBURY: Five-twenty.

DR. FEYNMAN: And if the probability of failure is one times ten to the 5th per second, that's 500 to the 5th which is about one in two hundred.

MR. KINGSBURY: I would have to go back and look at the engine numbers, Dr. Feynman.

DR. FEYNMAN: It is very easy to multiply 500 times ten to the minus five.

MR. KINGSBURY: I don't know that they were built on the same basis as the SRB numbers were built.

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I'm sorry, I just can't answer that question.

Well, Dr. Feynman, you are saying the SRB is ten to the minus five and you divide it by 120. If the SSME is ten to the minus five, you must divide it by 520, and so it is a very high probability.

DR. FEYNMAN: Let's take the SRB. Would that be one in a thousand, say, the probability of failure from one of the two SRB's is about one in 500 flights?

MR. KINGSBURY: That is what the numbers would say.

DR. FEYNMAN: But you were having a considerable discussion with Mr. Ullian where he was estimating, was somewhere between one in a hundred and one in a thousand. There's a tremendous amount of discussion, I suppose, and I discover you both have the same answer.

MR. KINGSBURY: Eventually we did have that, Mr. Feynman. Originally we did not have—originally Mr. Ullian's number was one in fifty-seven and that was based on every solid rocket motor failure that had occurred since 1958, and I contend there has been learning in the industry in the last 25 years and that some of the things that we did in 1958 to 1960, we now know how to do properly and therefore that data base was invalid.

Mr. Ullian's data base contained some other

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deficiencies, in my judgment, in that he called failures—he called—he identified things as failures where a mission was completed successfully, and I have difficulty calling that a failure, and so we had a lot of discussions.

You're exactly right, ultimately we came to essentially the same number. I never changed, I should say.

MR. SUTTER: I would like to change the subject and maybe this is an improper subject, but since this is the last meeting in our design reviews that Jack Lee supported us on, one of the inputs that came out of there and one of the things NASA is doing is a review of the critical items list and a review of the hazard analysis, it was the intent to make sure that there aren't things in there that shouldn't be changed, or procedures changed or something, prior to the next flight.

And since the joint now, the joint design is well along, how is the rest of the—look at the rest of the system and what may be coming out there and what time may be coming out there as to what else needs to be done.

Do you gentlemen have any knowledge of that?

MR. KINGSBURY: Yes, I think I can speak to that. The system is looking at everything except the solid rocket motor that we are looking at redesigning.

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We will look at that and any changes that we make, we will write a new FMEA, develop a new CIL, or if we don't make a change we will review very thoroughly the FMEA and the CIL.

MR. SUTTER: And this includes things like the main engine?

MR. KINGSBURY: The system is doing—I didn't make my point. There is a group that I have dedicated to redesigning some elements of the SRM. The rest of the system, the shuttle system at the Marshall Space Flight Center, is looking at the external tank, the SSME, the SRB systems that are not included in my activity, and I'm looking at the SRM.

MR. SUTTER: But you don't know how they're coming along on that and what the timing is to reach conclusions?

MR. KINGSBURY: I have not seen the schedule, no, sir, I have not.

DR. WALKER: I have a couple of questions.

MR. SUTTER: Well, I would just like to make a comment. It would be good to know how they're coming on that. Maybe before the report is finalized, it ought to be in the report that this analysis was requested and NASA was doing it anyhow.

MR. KINGSBURY: Mr. Sutter, I'm sure Jack Lee

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can provide that. It's just that I haven't seen it. The program is working that problem and I'm sure has a schedule developed.

MR. SUTTER: Thank you.

DR. WALKER. First, I have a specific question. You're going to remove the putty, which I think is a wise decision. What technique are you going to use to protect the seals? I assume you're still talking about the rubber or Viton seals.

What technique are you going to use to protect those from the interior heat?

MR. KINGSBURY: The very early thermal analyses we've done, and these are far from complete, show that the standing column of gas in the joint at the instant before ignition, then infused by the hot gas at ignition, temperatures at the joint achieve approximately 160 degrees Fahrenheit.

Now, if that was all I had to deal with it would be easy, but the dynamics show also that if you get a fairly small pressure differential around the periphery, you now set up circumferential flow, and so we must build some dams in there which cut off that circumferential flow.

That detail has not been worked out, but I understand it and we know when we must do it. And the

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analytical work and some of the empirical work is starting this week.

DR. WALKER: I thought the compression of the gas generated temperatures of about 1,000 degrees or so.

MR. KINGSBURY: Well, I can't tell you the details. I got the numbers from my folks last weekend, a very quick number, that said, with the joint as we have it now designed, and the path that we now have designed, I can expect somewhere between 150 and 160, maybe as high as 200, but well within the capability of any of the elastomers that we are considering.

DR. WALKER: I have a bit of unease about the speed or the pace at which the redesign is proceeding, and it seems as though there are some external factors such as these new cases which seem to preclude certain design options, for example, eliminating elastic seals altogether.

MR. KINGSBURY: No, sir, it does not. We can still modify the clevis end of the joint and put metal seals, pressure actuated metal seals. I really don't want to do that because I depend upon pressure, but that's a possibility.

We are not limited in what we can do at all.

DR. WALKER: Have you examined the possibility of metal seals in depth?

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MR. KINGSBURY: We are examining the possibility of metal seals. We haven't chosen the seals. We have a program that will run out at the end of June that we are running, and when that runs out, I think in June or July at Thiokol, that they are running that, and both are fairly broad programs and complementary, from which we would draw the conclusions of what the seals should be.

DR. FEYNMAN: I just have calculated the temperature from the compression. Supposing you get to 750 psi and you compress air at room temperature under that, under room temperature which is 50 times the pressure at which it starts, and then the entropy doesn't change and the entropy is a log of the ratio, and then you soon find you get to about 1,000 degrees.

MR. KINGSBURY: Yes, sir, instantaneously, but you are now stagnant. You will not maintain that pressure for the full time. You now have a stagnant column which is going to give up that energy.

DR. FEYNMAN: What happens to the pressure?

MR. KINGSBURY: It stays there.

DR. FEYNMAN: Where does the energy go, to the heating of metal?

MR. KINGSBURY: Yes, and the rubber and anything else that is there.

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DR. FEYNMAN: And the rubber?

MR. KINGSBURY: Yes.

MR. KEHRLI: Mr. Kingsbury, you have been discussing the redesign of the case to case, or the field joint. Did I understand you to say that there will also be a redesign of the case to nozzle joint seal?

MR. KINGSBURY: I don't believe I said that, but there probably will be.

CHAIRMAN ROGERS: I just wanted to say, unless there are other questions, I would like to say first, thank you very much and I guess the most important thing that the Commission, I guess can accomplish would be to have safety in the future, in the foreseeable future.

And we realize it is a difficult job you have, and in a sense you are going to be under the same general conditions that you were under when you launched 51-L, that you had schedule pressures of sorts. And there's been a lot of comment on whether that might have affected a lot of the decisions or not, but certainly it is something that is real in life, and you are going to have those now and in the future too because people—if they're thinking about starting to fly flights—if the flights begin in a year or so and it drags on for two years, you will be under constant pressure to do something right away and that would be repeating the

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mistake—a mistake of the past, I guess.

And second, if you have financial problems, that you can't do some of these tests because you think you can't afford it, that also may be one of the things that contributed to this accident.

And I guess what I'm saying first, if we can help in any respect in our recommendation, we would like to do it. I mean, as far as I'm concerned the most important thing of all is to get back to safe flight. And if you don't, even if you get back quickly and something happens, it is a tragedy of great magnitude because it could almost end manned space flight.

So, you might want to think about what we could recommend that would be helpful.

MR. KINGSBURY: Well, thank you, Mr. Rogers. Let me say that from the bottom of my heart, we are not going to fly again until we are ready, and I have found no one in the system who has argued one bit with me.

We have discussed when we can fly. There have been some of those pressures, that, can't we do it in a year. The answer is no. We cannot do it in a year. We cannot do it until we are safe, reliable, repeatable every time. And I have had no arguments of other than, gee, I wish you could do better, type of thing.

I think we all understand that we are simply

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not going back to a flight program until we are confident we have this thing fully controlled, and I'm not going back to a flight program and I believe I can stand behind this, until I can say there may be another failure in the shuttle in its history but it will not be a solid rocket motor.

I'm not stopping at the joint. We are looking at all of it.

CHAIRMAN ROGERS: Well, very good. I appreciate those comments very much.

DR. WALKER: Mr. Chairman, could I just have one more? Dr. Kingsbury, I'm really still a little concerned that you may be under——

CHAIRMAN ROGERS: Speak a little louder.

DR. WALKER: I'm really still concerned that you may under pressure, schedule pressures that, for example if you decided you needed to build several designs, at least in scale and test them, that there might be pressures which would preclude your doing that? And I'm just very concerned that the pressure to get the system operating again is going to force an early decision on the design of the new joint, and I'm just wondering how you anticipate that that will be resisted.

MR. KINGSBURY: Well, I don't know what else I

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can say. Let me say first that I am Mr. Kingsbury. You spend a modest amount of time in our fair city and I invite you to come and sit with me at any time and let us show you where we are. I just have no intentions of doing anything hastily and I have found nothing in the system above me that is encouraging it, and it is not one of these things I will quit. I'm not going to quit.

CHAIRMAN ROGERS: Well, we wish you luck. It's a tough job and we all appreciate it. Did he call you Doctor?

MR. KINGSBURY: Yes.

CHAIRMAN ROGERS: Chuck Yeager calls me Doctor. I'm not a doctor either.

[Laughter.]

CHAIRMAN ROGERS: Thanks a lot.

[Pause.]

CHAIRMAN ROGERS: We have some Commission members absent. We're almost at the end of the day here and we would like to move along, if we may.

If all of you gentlemen haven't been sworn in, would you rise, please, and take the oath.

[Witnesses sworn.]

George C. Marshall Space Flight Center
Marshall Space Flight Center Alabama
35812

History of O-ring (4)

COPY

Reply to Attn of: EP25 (78-1)

January 9, 1978

TO: EE51/Mr. Eudy

FROM: EP25/Mr. Miller

SUBJECT: Restatement of Position on SRM Clevis Joint O-Ring Acceptance Criteria and Clevis Joint Shim Requirements

In view of recent events relating to proposals suggesting the relaxation of standards for clevis joint O-ring acceptance and the use of a standard shim thickness for clevis joints which allows O-ring compression to fall below minimum industry accepted values, this office feels obligated to restate its opposition to both proposals. The following paragraphs address each of the related subjects in terms of events leading to such recommendations, risks involved by lowering standards, and recommendations ~~to~~ resolve risks.

a. Relaxation of O-Ring Acceptance Standards - During the latter part of November 1977, this office was requested by memorandum EE51 (77-291) to review Thiokol documents STW7-2875, Standard Acceptance Criteria for Preformed Packing (O-Rings) and 171-136, Standard Repair Instructions for O-Rings (see enclosure 1). Our response, which was documented in memorandum EP25 (77-108) dated November 30, 1977 (see enclosure 2), recommended rejection of both documents because of excessive deviations from MIL-STD-413 requirements, "visual inspection for rubber O-rings", and for lack of clarification on several subjects. Our memorandum also outlined recommended allowable flaw sizes per MIL-STD-413 and allowables for other types of defects which were not contained in MIL-STD-413. On December 22, 1977, we were provided with and asked to comment on a draft copy of memorandum EE51 (77-321) to program management (see enclosure 3) which contained EE51 comments and recommendations to Thiokol documents STW7-2875 and 171-136 which were not in agreement with our previous assessment. Because of these differences and to further amplify our position concerning O-ring defect allowables, the following recommendations and justifications are restated:

(1) Inclusions - Remove all visible inclusions regardless of size or type of included material. The included material can be detached during O-ring installation and use, creating debris and probable leakage. Repair is required if the resulting void exceeds 0.025 inch diameter by 0.005 inch deep. Deeper voids create a greater risk for leakage with low compression (example: a void .015 inch deep reduces compression effect by 5.5 percent).

[Ref. 5/2-21 1 of 4]

(2) Mold Deposit Effects, Pits, and Voids - Each defect must be treated according to defect shape. Defects having sharp edges should be treated as a notch sensitive cut and repaired if the defect exceeds 0.025 inch diameter by 0.002 inch deep. Defects having smooth shapes should be repaired if either the diameter or depth exceeds 0.025 inch and 0.005 inch respectively.

(3) Cuts - Radial cuts other than superficial cuts (cuts which cannot be felt with the thumbnail) are not allowed and must be repaired or dispositioned by splicing or rejection. The orientation of radial cuts is such that stretching of the O-ring can cause further tearing. Cuts parallel to the O-ring longitudinal axis must not exceed 0.002 inch deep by 0.060 inch long.

(4) Repair Limitations - The limitations on maximum defect size acceptable for repair should be based on results of Thiokol's test program per TWR-11507. Deviations should be approved by EH01.

b. Below Minimum O-Ring Compression - Prior to the static firing of DM-1 in June 1977, shims were installed in the clevis joints to stop seal leakage caused by tang distortion. Shims of various thicknesses (0.010 to 0.031 in.) were placed around two of the joints according to gap width available (with some exceptions). No leaks were apparent during the test; however, the cavity pressure measurement on clevis joint number 5 (see enclosure 4) showed peculiar behavior (negative pressure to +8.3 psig). Calculations performed by MSFC and agreed to by Thiokol show that distortion of the clevis joint tang for any joint can be sufficient to cause O-ring/tang separation. Data from DM-1 shows that this condition can be created by joint movement (lowering of support chocks) and data from the hydroburst test shows the tang and clevis do not remain concentric during pressure cycling. All situations which could create tang distortion are not known, nor is the magnitude of movement known. Regardless of these unknowns, Thiokol then proposed to use a standard 0.020 inch thick shim for all SRM clevis joints including the STA-1 vehicle (see enclosure 5). Subsequent to arrival of the STA-1 vehicle at MSFC, Structures and Propulsion Laboratory was asked to assess the adequacy of the 0.020 inch shims which had been installed by Thiokol. The response, documented by memorandum EP01 (77-252) (see enclosure 6) recommended shim sizes ranging from 0.034 inch to 0.046 inch thick in order to maintain the industry recommended minimum compression value of 15 percent. It was, and still is, our desire to test with 15 percent minimum compression since this value is the industry wide minimum and was originally the minimum design value used by Thiokol prior to the tang distortion problem.

After issuance of the Structures and Propulsion Laboratory recommendations, an EE51 decision was made to use a 0.015 inch thick shim in the field joint of STA-1 which results in a minimum compression value of approximately 5.5 percent. This value assumes no compression set. We strongly object to this proposal because it creates unacceptable risks which can and should be avoided.

[Ref. 5/2-21 2 of 4]

Calculations conducted by this office show that in some instances, O-ring compression on flight vehicles has the potential of being negative by approximately 1.5 percent; these calculations included the effect of O-ring compression set. Thiokol test report dated August 15, 1977, per TWR-11507 "O-ring repair verification test plan" (see enclosure 7) shows that the parent O-ring material and splice joints exhibited maximum compression sets of 5.3 and 7.0 percent, respectively. Also, when considering that the SRM process demonstration segment O-ring suffered a compression set value of approximately 11.0 percent, one must treat these values as realistic and include their effects when calculating O-ring compression. It is recognized that O-rings will perform properly at lower values than the 15 to 25 percent range recommended; however, the higher values are used as a design point in order to account for losses such as O-ring compression set and defects in the hardware sealing surfaces and O-rings. Our recommendations to redesign on-coming hardware and custom shim each joint (with a range) on existing hardware as presented to you in October 1977, is still valid (see enclosure 8). The following recommendations and justifications are considered mandatory to provide adequate clevis joint sealing on all SRMs.

(1) Reshim STA-1 to obtain a minimum compression value of 15 percent in order to verify the design for flight.

(2) Redesign clevis joints on all on-coming hardware at the earliest possible effectivity to preclude unacceptable, high risk, O-ring compression values. This will eventually negate the use of shims, thereby reducing assembly time and eliminating shimming errors.

(3) Continue to use shims with existing and mixed hardware. Shims should be of sufficient thickness to provide a minimum O-ring compression of 15 percent. This value is used and recommended by Parker, Precision, CSD (Titan), Aerojet, and MSFC Science and Engineering Laboratories. We know of no instance where lower values are recommended.

(4) Direct the prime contractor and booster assembly contractor to reinstate the design requirements of 15 to 25 percent compression for clevis joint O-rings. We see no valid reason for not designing to accepted standards.

In summary, we believe that the facts presented in the preceding paragraphs should receive your most urgent attention. Proper shim sizing and high quality O-rings are mandatory to prevent hot gas leaks and resulting catastrophic failure. We will be pleased to provide assistance in any way possible.

Questions concerning the contents of this memorandum should be referred to Mr. W. L. Ray, 3-0459.

John Q. Miller

John Q. Miller
Chief, Solid Motor Branch

8 Enclosures

cc: w/o enc.
EP01/Mr. McCool
EP41/Mr. Hopson
EP21/Mr. Lombardo

[Ref. 5/2-21 3 of 4]

[Ref. 5/2-21 4 of 4]

EP25	MARSHALL SPACE FLIGHT CENTER SRM CLEVIS JOINT LEAKAGE STUDY	LEON RAY OCTOBER 21, 1977

DESIGN OPTIONS

OPTIONS	REMARKS
1. NO CHANGE	o UNACCEPTABLE -TANG CAN MOVE OUTBOARD AND CAUSE EXCESSIVE JOINT CLEARANCE RESULTING IN SEAL LEAKAGE. o ECCENTRIC TANG/CLEVIS INTERFACE CAN CAUSE O-RING EXTRUSION WHEN CASE IS PRESSURIZED.
2. SHIMS BETWEEN TANG AND CLEVIS (OUTSIDE)	o ACCEPTABLE SHORT-TERM FIX IF PROPER SHIM SIZE IS USED. o PROBABILITY OF ERROR IN CALCULATING PROPER SHIM SIZE. o REQUIRES INCREASED ASSEMBLY TIME FOR SHIM INSTALLATION AND JOINT CENTERING.
3. OVERSIZED O-RINGS	o UNACCEPTABLE SOLUTION - HIGH PROBABILITY OF O-RING DAMAGE OR CLEVIS DISTORTION DURING ASSEMBLY. o DEPARTS FROM RECOMMENDED DESIGN PRACTICES.
4. REDESIGN TANG AND REDUCE TOLERANCE ON CLEVIS	o BEST OPTION FOR LONG-TERM FIX - ELIMINATES USE OF SHIMS WHEN ALL REDESIGNED HARDWARE IS USED. o PREVENTS THE TYPE OF ERROR WHICH COULD RESULT IN CALCULATING JOINT CLEARANCE FOR SHIM INSTALLATION.
5. COMBINATION OF REDESIGN (AS IN OPTION 4) AND USE OF SHIMS	o ACCEPTABLE APPROACH. SHIMS WILL BE REQUIRED IN SOME CASES WHEN REDESIGNED HARDWARE AND PRESENT HARDWARE IS JOINTED. o SHIMS WILL BE DISCONTINUED WHEN PRESENT HARDWARE IS PHASED OUT.

[Ref. 5/2-22 1 of 2]

MARSHALL SPACE FLIGHT CENTER	NAME: LEON RAY
SRM CLEVIS JOINT LEAKAGE STUDY	DATE: OCTOBER 25, 1977

STA-1 PROPOSED SHIM MODIFICATION

- o CALCULATE JOINT CLEARANCES FROM AS BUILT DIMENSIONS
- o REMOVE JOINT SEALING MATERIAL (RTV-700). REMOVE ONLY ENOUGH MATERIAL TO GAIN ACCESS TO SHIM AREA.
- o INSTALL NEW SHIMS TO REDUCE TANG/CLEVIS OUTSIDE GAP TO APPROXIMATELY .005 IN.
- o INSTALL JOINT DEFLECTION INSTRUMENTATION
- o INSTALL CLEVIS JOINT SEAL CAVITY PRESSURE MEASUREMENTS
- o RE-SEAL JOINT AREA.

[Ref. 5/2/22 2 of 2]

History of O-ring (5)...

George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama
35812

Reply to Attn of: EP25 (79-13)

January 19, 1979

TO: EE51/Mr. Eud

FROM: EP25/Mr. Miller

SUBJECT: Evaluation of SRM Clevis Joint Behavior

As requested by your memorandum, EE51 (79-10), Thiokol documents TWR-12019 and letter 7000/ED-78-424 have been revaluated. We find the Thiokol position regarding design adequacy of the clevis joint to be completely unacceptable for the following reasons:

a. The large sealing surface gap created by excessive tang/clevis relative movement causes the primary O-ring seal to extrude into the gap, forcing the seal to function in a way which violates industry and Government O-ring application practices.

b. Excessive tang/clevis movement as explained above also allows the secondary O-ring seal to become completely disengaged from its sealing surface on the tang.

c. Contract End Item Specification, CPW1-2500D, page I-28, paragraph 3.2.1.2 requires that the integrity of all high pressure case seals be verifiable; the clevis joint secondary O-ring seal has been verified by tests to be unsatisfactory.

Questions or comments concerning this memorandum should be referred to, Mr. William L. Ray, 3-0459.

John Q. Miller

John Q. Miller
Chief, Solid Motor Branch

cc:
SA41/Messrs. Hardy/Rice
EE51/Mr. Uptagrafft
EH02/Mr. Key
EP01/Mr. McCool
EP42/Mr. Bianca
EP21/Mr. Lombardo
EP25/Mr. Powers
EP25/Mr. Ray

[Ref. 5-2-23]

National Aeronautics and
Space Administration

George C. Marshall Space Flight Center
Marshall Space Flight Center, Alabama
35812

NASA

Reply to Attn of: EP25 (79-23)

February 6, 1979

TO: Distribution

FROM: EP25/Mr. Ray

SUBJECT: Visit to Precision Rubber Products Corporation and Parker Seal Company

The purpose of this memorandum is to document the results of a visit to Precision Rubber Products Corporation, Lebanon, TN, by Mr. Eudy, EE51 and Mr. Ray, EP25, on February 1, 1979 and also to inform you of the visit made to Parker Seal Company, Lexington, KY on February 2, 1979 by Mr. Ray. The purpose of the visits was to present the O-ring seal manufacturers with data concerning the large O-ring extrusion gaps being experienced on the Space Shuttle Solid Rocket Motor clevis joints and to seek opinions regarding potential risks involved.

The visit on February 1, 1979, to Precision Rubber Products Corporation by Mr. Eudy and Mr. Ray was very well received. Company officials, Mr. Howard Gillette, Vice President for Technical Direction, Mr. John Hoover, Vice President for Engineering, and Mr. Gene Hale, Design Engineer attended the meeting and were presented with the SRM clevis joint seal test data by Mr. Eudy and Mr. Ray. After considerable discussion, company representatives declined to make immediate recommendations because of the need for more time to study the data. They did; however, voice concern for the design, stating that the SRM O-ring extrusion gap was larger than that covered by their experience. They also stated that more tests should be performed with the present design. Mr. Hoover promised to contact MSFC for further discussions within a few days. Mr. Gillette provided Mr. Eudy and Mr. Ray with the names of two consultants who may be able to help. We are indebted to the Precision Rubber Products Corporation for the time and effort being expended by their people in support of this problem, especially since they have no connection with the project.

The visit to the Parker Seal Company on February 2, 1979, by Mr. Ray, EP25, was also well received; Parker Seal Company supplies the O-rings used in the SRM clevis joint design. Parker representatives, Mr. Bill Collins, Vice President for Sales, Mr. W. B. Green, Manager for Technical Services, Mr. J. W. Kosty, Chief Development Engineer for R&D, Mr. D. P. Thalman, Territory Manager and Mr. Dutch Haddock, Technical Services, met with Mr. Ray, EP25, and were provided with the identical

[Ref. 5/2-24 1 of 2]

SRM clevis joint data as was presented to the Precision Rubber Products Company on February 1, 1979. Reaction to the data by Parker officials was essentially the same as that by Precision; the SRM O-ring extrusion gap is larger than they have previously experienced. They also expressed surprise that the seal had performed so well in the present application. Parker experts would make no official statements concerning reliability and potential risk factors associated with the present design; however, their first thought was that the O-ring was being asked to perform beyond its intended design and that a different type of seal should be considered. The need for additional testing of the present design was also discussed and it was agreed that tests which more closely simulate actual conditions should be done. Parker officials will study the data in more detail with other Company experts and contact MSFC for further discussions in approximately one week. Parker Seal has shown a serious interest in assisting MSFC with this problem and their efforts are very much appreciated.

William L. Ray

William L. Ray
Solid Motor Branch, EP25

Distribution:
SA41/Messrs. Hardy/Rice
EE51/Mr. Eudy
EP01/Mr. McCool

[Ref. 5/2-24 2 of 2]

SPACE SHUTTLE

VERIFICATION/CERTIFICATION

PROPULSION COMMITTEE

COGNIZANT ENGINEERS 5th MEETING

L/GEN THOMAS W. MORGAN
CHAIRMAN

10 JULY 1980

[Ref. 5 2-25 1 of 14]

SOLID ROCKET MOTOR
RECOMMENDATIONS

GRAIN INTEGRITY

1. MARSHALL SPACE FLIGHT CENTER CONTINUE THEIR RESPONSIVE ACTIONS TO RESOLVE THE FORWARD GRAIN INTEGRITY PROBLEM IN AREAS OF:
   - o FULL-SCALE, NON-DESTRUCTIVE TESTING AT TEMPERATURE EXTREMES
   - o INDEPENDENT STRENGTH ANALYSIS OF GRAIN STRUCTURE
   - o MATERIAL PROPERTIES

2. DEVELOP AN INSPECTION PROCEDURE FOR ASSEMBLED MOTORS AND IMPLEMENT THE PROCEDURE PRIOR TO STS-1.

3. REFINE REQUIREMENTS, PROCEDURES, ANALYSIS, AND EQUIPMENT USED IN STORAGE AND HANDLING OF MOTOR SEGMENTS AND VALIDATE BY MEASUREMENT.

TEMPERATURE EXTREMES

4. PERFORM ADDITIONAL VERIFICATION AT TEMPERATURE EXTREMES.

FIELD JOINT

8. CONDUCT FULL-SCALE TESTS, LABORATORY TESTS AND PROCEDURE REVIEW TO VERIFY FIELD JOINT INTEGRITY

[Ref. 5 2-25 2 of 14]

JOINT INTEGRITY

Description

The SRM propellant segments are attached to form a complete pressure vessel by means of a pinned tang and clevis joint. Reliable hot gas sealing of the joint is critical for safe SRM operation.

Information was provided in a briefing at Thiokol, a number of Thiokol reports, a MSFC discussion, and a conference call with Thiokol and MSFC. Verification concerns include: (1) adequacy of safety factor to prevent "O" ring leakage or extrusion failure at SRM ignition, (2) redundancy, (3) adequacy of correct joint assembly (at KSC-VAFB) verification.

Approach

a. Verification for Seal Safety Factor. A single motor case "burst" test (TWR-11 664) resulted in "O" ring extrusion failure at a safety factor of 1 50 over MEOP. This failure mode can produce a wide spread of results due to manufacturing variations, including joint and "O" ring variations combined with worst case joint structural deflection under load. Therefore, the committee was initially concerned that the minimum safety factor was inadequate. Thiokol/Wasatch reports TWR-11 564 and TWR-12051 heightened the committee's concern. The committee also considers the finite element analysis of TWR-12019 of little value since it required significant "adjustment" to approximate Structural Test Article (STA-1) test results, and the imposed boundary conditions are questionable. We have since learned that joint design changes, "O" ring changes, and increased shimming have been accomplished and also that the deflection data (multivit gauge) in the reports are erroneous.

NASA and Thiokol were very responsive to our questions. Apparently a lot of information is available beyond that shown in the reports which shows that design/procedure changes have been made and correct deflection data are now available. This data supports a maximum gap at MEOP of 0.081 inches (0.048 inches from STA-1 measurements + 0.033 inches "manufacturing" tolerance) which results in a minimum safety factor of 1.56 using lab test extrusion data on nominal, undamaged "O" rings. Note that "O" ring hardness variations up to 20% are common. This up-to-date data has considerably alleviated the committee's concerns; however, other than one figure in TWR-11992 an up-to-date verfication data package does not appear to exist and sensitivity data on "O" ring damage is lacking.

[Ref 5 2 25 3 of 14]

b. Verification of Redundancy. The sea... sign was originally presented as redundant (two "O" rings plus the overlapped internal case insulation). The committee was concerned that the redundancy feature was not verified by purposely failing one element in such a way (for example, an "O" ring damaged so as to extrude prematurely) to simulate all important failure modes. The committee understands from a telecon that the primary purpose of the second "O" ring is to test the primary "O" ring, and that redundancy is not a requirement. (We recognize some unquantified degree of redundancy exists as shown on the cyclic pressurization tests.)

c. Verification of Proper Assembly. The committee believes minute attention to detail is required here since the primary "O" ring and its sealing geometry and surfaces constitute a critical single point failure mode for the Shuttle. The 225 psi "set" and 50 psi leak test is excellent, but not perfect, since the primary "O" ring is pressurized in the wrong direction, and the effect of insulation putty is not certain. One assembly and verification procedure was examined and found capable of being improved upon.

Recommendations

1. The upcoming lightweight case test should be expanded in scope to also be a thorough field joint verification test for STS-1. This should include verification of manufacturing plus pressurization gaps and safety factors and should consider "O" ring damage testing for safety factor sensitivity quantification.

2. Perform high pressurization rate (followed by hold) lab tests of specimens simulating the putty/insulation joint to determine "O" ring pressurization times. Apply this data in 3 below.

3. An up-to-date rigorous and complete verification package covering safety factor on sealing at ignition and MEOP should be assembled and critically reviewed by NASA specialists by 15 June.

4. If item 3 above is found to be incomplete or lacking, additional testing should be performed to complete verification for STS-1. The inert propellant segments at KSC (which have internal insulation) should be considered as a suitable vehicle.

5. Review and improve field joint assembly procedures.

[Ref. 5/2-25 4 of 14]

PROPULSION COMMITTEE MEMBERS

NAME	ORGANIZATION
LT GEN THOMAS W. MORGAN, USAF RET	PANEL CHAIRMAN (COGNIZANT ENGINEER)
MR HARRY L. GOGAN, ASSISTANT DIRECTOR	DIRECTORATE OF AEROSPACE STUDIES KIRTLAND AFB, NM
MR DON A. HART, DEPUTY DIRECTOR	AIR FORCE ROCKET PROPULSION LAB EDWARDS AFB, CA
DR EUGENE COVERT, PROFESSOR	MIT, CAMBRIDGE, MA
DR BERNARD ROSS FELIX, VICE PRESIDENT	CHEMICAL SYSTEMS DIVISION SUNNYVALE, CA
MR ERNST STAMPFL, MANAGER PROPULSION SEC SPACE TRANSPORTATION DIRECTORATE	THE AEROSPACE CORPORATION LOS ANGELES, CA
MR SHERWIN LEWIS, DIRECTOR AEROSPACE PROPELLANT OFFICE	THE AEROSPACE CORPORATION LOS ANGELES, CA
COL EARL B. ESSING, USAF, RET	AIR FORCE ROCKET PROPULSION LAB (FORMER COMMANDER)

[Ref. 5/2-25 5 of 14]

SOLID ROCKET MOTOR OBSERVATIONS

1. THE CERTIFICATION SYSTEM IS SATISFACTORY.

2. CERTIFICATION PLAN FOR IMPLEMENTATION IS ACCEPTABLE, EXCEPT FOR:

   A. FORWARD SEGMENT GRAIN INTEGRITY

   B. NO TEST AT TEMPERATURE EXTREMES (40° TO 90°)

   C. SHELF LIFE OF THE IGNITERS

3. THERE ARE NO "SHOW STOPPERS" FOR STS-1 AT THIS TIME IF 2A AND 2C ARE SATISFIED

[Ref. 5 2-25 6 of 14]

INTRODUCTION

PAGE 1
SEPTEMBER 15, 1980

THE MATERIAL PRESENTED HEREIN DEFINES THE SPACE SHUTTLE PROGRAM RESPONSE TO RECOMMENDATIONS GIVEN BY THE VERIFICATION/CERTIFICATION SPECIAL STAFF TO THE CHIEF ENGINEER DURING THE PERIOD APRIL 10 - AUGUST 15, 1980. THIS MATERIAL CONSTITUTES A RECORD OF THE JOINT REVIEWS CHAIRED BY THE ADMINISTRATOR OR DEPUTY ADMINISTRATOR WITH PRESENTATIONS BY PROGRAM REPRESENTATIVES AND PARTICIPATION BY THE SPECIAL STAFF COGNIZANT ENGINEERS. ALL REVIEWS WERE PRECEDED BY INDIVIDUAL COORDINATION MEETINGS BETWEEN THE SPECIAL STAFF COGNIZANT ENGINEERS AND PROGRAM COUNTERPARTS. THE LOCATIONS AND DATES OF THE JOINT REVIEWS WERE AS FOLLOW:

AMES RESEARCH CENTER	MAY 28-29, 1980	RESPONSE TO INITIAL RECOMMENDATIONS AS GIVEN AT KSC ON APRIL 10-11, 1980
NASA HEADQUARTERS	JULY 1, 1980	FURTHER REVIEW OF SELECTED ITEMS FROM MEETING AT AMES
JOHNSON SPACE CENTER	JULY 10-11, 1980	RESPONSE TO SECOND SET OF RECOMMENDATIONS AS GIVEN AT AMES ON MAY 28-29, 1980
JOHNSON SPACE CENTER	AUGUST 15, 1980	RESPONSE TO THIRD SET OF RECOMMENDATIONS AS GIVEN AT JSC ON JULY 10-11, 1980

MATERIAL RELATIVE TO EACH NUMBERED RECOMMENDATION WITHIN THE FOURTEEN ASSESSMENT AREAS IS PRESENTED, GENERALLY, IN THE FORMAT OF "RECOMMENDATION", "BACKGROUND", AND "PROGRAM RESPONSE". THE ADDITIONAL HEADING, "DECISION", FOR CERTAIN OF THE RECOMMENDATIONS DENOTES FURTHER DISPOSITION AND ASSIGNMENT OF RELATED ACTIONS BY THE ADMINISTRATOR. THE DATE INDICATED ON THE MATERIAL FOR EACH RECOMMENDATION IDENTIFIES THE JOINT MEETING AT WHICH THE PROGRAM RESPONSE WAS REVIEWED. SIGNATURES FOLLOWING EACH ITEM INDICATE CONCURRENCE BY THE PROGRAM COUNTERPART AND THE COGNIZANT ENGINEER AS A RECORD OF REVIEW AND MUTUAL UNDERSTANDING OF THE RECOMMENDATION, RATIONALE, AND PROGRAM RESPONSE.

Robert F. Thompson
ROBERT F. THOMPSON
MANAGER, SPACE SHUTTLE PROGRAM

John F. Yardley
JOHN F. YARDLEY
ASSOCIATE ADMINISTRATOR FOR SPACE TRANSPORTATION SYSTEMS

[Ref. 5 2-25 7 of 14]

ORIGINAL PAGE IS OF POOR QUALITY

NO VERIFICATION AT TEMPERATURE EXTREMES

The SRM verification program does not include any full-scale firings nor any instrumented storage tests at environmental extremes. These include short-term horizontal storage conditions at Utah or in shipment, long-term storage at 32° - 100°F, horizontally or vertical storage in assembled condition (40°-90°F) or firing at 40°-90°F. Verification by analysis is based on the conservative design approach employed (similar to Titan III-C). The Committee is concerned that this approach is risky considering:

1. The forward propellant segment grain design (basically a formed cylinder with the usual questions in the transition zone) may not be conservative (i.e., have large safety factors).

2. The observed ignition overshoot, the center segment Ballistic Augmentation Rate Factor and the thrust oscillations may or may not be analytically extrapolated to temperature extremes with confidence in the resulting calculations.

3. Two additional, though less closely related factors are the slag anamoloy and the intent to add 0.03% Fe_2O_3 to increase the burning rate for HTR motors.

Recommendations:

1. Reassess the analysis in all the areas to determine safety margins.

2. Consider temperature conditioning the forthcoming segments (forward and center) to extremes both in horizontal and in flight attitude.

3. Prepare an inspection approach for stacked motors to observe any blanching/cracks/separations as environments naturally occur over the months until the motors are used.

4. Write inspection plans so that indications and discrepancies can be measured and recorded during transport from Utah to receipt at KSC to stack.

5. Assure that adequate flight instrumentation exists to precisely track ballistics as the environments naturally occur.

6. Consider a QM-4 margin limit type test.

The information on which these recommendations are made was obtained in briefings at Thiokol/Wasatch Division and at Kennedy Space Center. Mr E. Dorsey, Thiokol and Mr George Hardy, NASA/MSFC, were present at the Thiokol briefing. Mr H. delaRosa, NASA KSC gave the Kennedy Center briefing and Mr J. Greenwood, NASA/KSC, was present.

[Ref. 5 2-25 8 of 14]

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MAY 29, 1980

PROPULSION

REVISED
SEPT 3, 1980

RECOMMENDATION - PROP-21

IMPROVE CONTROL OF MOTOR SEGMENT FIELD JOINT ASSEMBLY PROCEDURES (CATEGORY II)

BACKGROUND

CONCERN IS THAT DAMAGE COULD OCCUR TO THE "O" RING(S) THAT ARE USED IN THE JOINT FOR PRESSURE SEALING. DAMAGE TO THE "O" RINGS COULD ALLOW A HOT GAS LEAK WHICH COULD GROW IN MAGNITUDE AND COULD IMPINGE ON THE ET DURING FLIGHT

PROGRAM RESPONSE - ACCEPT

ACTION: PROJECT AND MOTOR CONTRACTOR WILL AGAIN REVIEW OMI PROCEDURES FOR ASSEMBLY TO ASSURE THAT COMMITTEE'S CONCERNS FOR "O" RING DAMAGE ARE CONSIDERED AND THAT PRECAUTIONS USED ON STS-1 ARE CARRIED THROUGH TO SUBSEQUENT STACKING OPERATIONS

W. H. Branning
PROGRAM COUNTERPART

(*) Thomas W. Morgan 26 Sept 1980
COGNIZANT ENGINEER DATE

(*) NOTE: THIS RECOMMENDATION WAS SUBSEQUENTLY INCLUDED IN PROP-38

[Ref. 5 2-25 9 of 14]

RECOMMENDATION- PROP-22

PERFORM CASE BURST TEST WITH ONE "O" RING REMOVED (CATEGORY II)

BACKGROUND

DURING THE "BURST TEST" FOR FINAL VERIFICATION OF THE MOTOR CASE SAFETY FACTOR, ONE OF THE TWO "O" RINGS FAILED BY EXTRUSION AND LEAKED. THE ANALYSIS USED FOR ADDITIONAL VERIFICATION DID NOT INCLUDE FURTHER GAP OPENINGS CAUSED BY JOINT DEFLECTION AT PRESSURIZATION OR ANY DEFLECTIONS CAUSED BY BENDING LOADS. THE PANEL CONSIDERS THE ABOVE TO BE INADEQUATE TO PROVIDE OPERATIONAL PROGRAM RELIABILITY, AND MARGINAL TO PROVIDE ADEQUATE SAFETY FACTOR CONFIDENCE ON STS-1

PROGRAM RESPONSE - BASELINE

- PROGRAM FEELS INTENT OF RECOMMENDATION HAS BEEN MET
- SINGLE "O" RING AND DAMAGED "O" RING TESTS HAVE BEEN RUN ON THE ORIGINAL HYDROBURST TEST AT THIOKOL. CHAMBER PRESSURE DID LEAK BY THE PRIMARY "O" RING SUCH THAT ONLY THE SECONDARY "O" RING WAS PROVIDING THE SEAL. SIMILAR PROOF OF THE EFFECTIVENESS OF THE SECONDARY "O" RING SEAL WAS DEMONSTRATED ON THE STATIC TEST ARTICLE (STA) AT MSFC

[Ref. 5/2-25 10 of 14]

PROGRAM RESPONSE - BASELINE - (CONTINUED)

- HOWEVER, SINCE THE LIGHTWEIGHT CASE PROGRAM MIGHT HAVE SOME EFFECT ON JOINT ROTATION AND "O" RING SEALING, MSFC AND TC WILL CONSIDER USING A SINGLE "O" RING IN PERFORMING CASE JOINT ROTATIONAL TESTS FOR THE LIGHTWEIGHT CASE PROGRAM

Hubert J Brasseaux
PROGRAM COUNTERPART

(*) Thomas W. F. Morgan
COGNIZANT ENGINEER

26 Sept 1980
DATE

(*) NOTE: THIS RECOMMENDATION WAS SUBSEQUENTLY INCLUDED IN PROP-38

[Ref. 5/2-25 11 of 14]

PROPULSION

RECOMMENDATION PROP-38 (CATEGORY 1) (REFERENCE PROP-21/22)

A. THE UPCOMING LIGHTWEIGHT CASE TEST SHOULD BE EXPANDED IN SCOPE TO ALSO BE A THOROUGH FIELD JOINT VERIFICATION TEST FOR STS-1. THIS SHOULD INCLUDE VERIFICATION OF MANUFACTURING PLUS PRESSURIZATION GAPS AND SAFETY FACTORS AND SHOULD CONSIDER "O" RING DAMAGE TESTING FOR SAFETY FACTOR SENSITIVITY QUANTIFICATION. (PROP-22)

B. PERFORM HIGH PRESSURIZATION RATE (FOLLOWED BY HOLD) LAB TESTS OF SPECIMENS SIMULATING THE PUTTY/INSULATION JOINT TO DETERMINE "O" RING PRESSURIZATION TIMES. APPLY THIS DATA IN "C" BELOW

C. AN UP-TO-DATE RIGOROUS AND COMPLETE VERIFICATION PACKAGE COVERING SAFETY FACTOR ON SEALING AT IGNITION AND MEOP SHOULD BE ASSEMBLED AND CRITICALLY REVIEWED BY NASA SPECIALISTS BY JUNE 15, 1980

D. IF THE JOINT VERIFICATION PACKAGE IS FOUND TO BE INCOMPLETE OR LACKING, ADDITIONAL TESTING SHOULD BE PERFORMED TO COMPLETE VERIFICATION FOR STS-1. THE INERT PROPELLANT SEGMENTS AT KSC (WHICH HAVE INTERNAL INSULATION) SHOULD BE CONSIDERED AS A SUITABLE VEHICLE

E. REVIEW AND IMPROVE FIELD JOINT ASSEMBLY PROCEDURES. (PROP-21)

[Ref. 5/2-25 12 of 14]

PROPULSION

BACKGROUND

- THE SRM PROPELLANT SEGMENTS ARE ATTACHED TO FORM A COMPLETE PRESSURE VESSEL BY MEANS OF A PINNED TANG AND CLEVIS JOINT. RELIABLE HOT-GAS SEALING OF THE JOINT IS CRITICAL FOR SAFE SRM OPERATIONS
- VERIFICATION CONCERNS INCLUDE: (1) ADEQUACY OF SAFETY FACTOR TO PREVENT "O" RING LEAKAGE OR EXTRUSION FAILURE AT SRM IGNITION, (2) REDUNDANCY, (3) ADEQUACY OF CORRECT JOINT ASSEMBLY (AT KSC/VAFB) VERIFICATION

PROGRAM RESPONSE

A. BASELINE

THE LIGHTWEIGHT CASE PROGRAM INCLUDES A DAMAGED "O" RING TEST AND VERIFICATION OF GAPS DURING PRESSURIZATION TO MEOP AND 1.4 MEOP. THESE TESTS HAVE BEEN SUCCESSFULLY COMPLETED

B. BASELINE (INTENT)

THE FIELD JOINT HAS BEEN RIGOROUSLY EVALUATED AND DEMONSTRATED IN LAB TEST (NO LEAKAGE) AT 2000 PSI); CDR; DCR; STRUCTURAL TEST ARTICLE (NO LEAKAGE UNTIL FOURTH PRESSURE CYCLE; ACCUMULATED 2 HRS. 44 MIN. AT PRESSURES GREATER THAN 900 PSI AND 7 HRS. 27 MIN. AT 600 PSI); GREATER THAN 200 JOINTS PROOF TESTED AT ROHR (NO LEAKS); BASELINE CASE BURST TEST (NO LEAKAGE THRU 4 MISSION LIFE PRESSURE CYCLES); SEVEN STATIC FIRINGS (NO LEAKS); AND LIGHT WEIGHT TEST PROGRAM (IN PROGRESS)

[Ref. 5/2-25 13 of 14]

PROPULSION

PROGRAM RESPONSE (CONTINUED)

C. BASELINE

NASA SPECIALISTS HAVE REVIEWED THE FIELD JOINT DESIGN, UPDATED WITH LARGER "O" RINGS AND THICKER SHIMS AND FOUND THE SAFETY FACTORS TO BE ADEQUATE FOR THE CURRENT DESIGN

REANALYSIS OF JOINT WITH LARGER "O" RINGS AND THICKER SHIMS IS BEING ACCOMPLISHED AS PART OF THE LIGHTWEIGHT CASE PROGRAM. COMPLETION DATE IS OCTOBER 1980

D. BASELINE

THE JOINT HAS BEEN SUFFICIENTLY VERIFIED WITH THE TESTING ACCOMPLISHED TO DATE (JOINT LAB TESTS, STRUCTURAL TEST ARTICLE, AND SEVEN STATIC FIRINGS AND THE TWO CASE CONFIGURATION BURST TESTS) AND CURRENTLY SCHEDULED FOR LIGHTWEIGHT CASE PROGRAM

E. BASELINE

PROCEDURES WERE REVIEWED TO ASSURE THAT CONTINUING ATTENTION IS APPLIED TO FIELD JOINT ASSEMBLY TO ELIMINATE "O" RING DAMAGE ON ASSEMBLY. THIOKOL CREWS FROM THE WASATCH DIVISION (TDY) WILL BE USED FOR THE MOTOR ASSEMBLY/STACKING OPERATIONS

Hubert J Brasseaux
PROGRAM COUNTERPART

Thomas W Morgan
COGNIZANT ENGINEER

26 Sept 1980
DATE

(14 of 14)

[Ref. 5/2-25 14 of 14]

TESTIMONY OF L. MICHAEL WEEKS, ASSOCIATE ADMINISTRATOR, (TECHNICAL) FOR SPACE FLIGHT; AND

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GLENN R. LUNNEY, FORMERLY MANAGER, NATIONAL SPACE TRANSPORTATION SYSTEMS PROGRAM OFFICE, ACCOMPANIED BY JOHN R. STOCKER, ASSISTANT GENERAL COUNSEL, CORPORATE OFFICES, ROCKWELL INTERNATIONAL CORPORATION.

CHAIRMAN ROGERS: Would you want to identify yourself?

MR. STOCKER: My name is John Stocker. I am assistant general counsel for Rockwell Corporation. I am appearing here with Mr. Lunney.

CHAIRMAN ROGERS: You don't intend to testify?

MR. STOCKER: I do not, sir.

CHAIRMAN ROGERS: We would like to refer to a couple of documents first, and then maybe ask some questions.

MR. KEHRLI: Mr. Lunney, as you know my name is Randy Kehrli and I haven't met you, Mr. Weeks. I'm one of the Commission staff investigators assisting the Commission, and I would like to refer you to the red book in front of you.

I guess you have two there, turn to Tab Number 13, in that book. If you would turn to that, and I would like to start with you, Mr. Lunney, I have interviewed

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you before out in Seal Beach, is that correct? [Ref. 5 2-26 through 29]

MR. LUNNEY: Yes, sir.

MR. KEHRLI: And, during that interview we discussed, did we not, the change in criticality of the SRM seal, joint seal, from criticality 1-R to 1, do you recall that?

MR. LUNNEY: Yes.

MR. KEHRLI: And also the subsequent waiver that was granted in connection with that criticality?

MR. LUNNEY: Yes.

MR. KEHRLI: And would you tell the Commission at that time, what was your role with NASA?

MR. LUNNEY: At that time I was the—what was called the level two program manager for the space transportation system.

MR. KEHRLI: And your successor in that job is a man named Mr. Arnie Aldridge, is that correct?

MR. LUNNEY: That is correct.

MR. KEHRLI: And that was at Johnson Space Flight Center?

MR. LUNNEY: Yes.

MR. KEHRLI: Would you tell the Commission what your role was with regard to the criticality change from 1-R to 1, please?

MR. LUNNEY: Because of the nature of it, I

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had to approve it or disapprove it. I had to disposition it. And I also, because of the nature of it, I had to send it on up to headquarters for their approval. I approved it and sent it on for their approval.

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MR. KEHRLI: What do you recall specifically about that change? Why was that change made, sir?

MR. LUNNEY: I don't recall exactly what tests were going on, but it was my understanding that some tests were going on at Marshall and some things had been learned about the rotation of the joint in the SRB case-to-case sealing part of the rocket.

As a result of those tests, it was concluded that it was possible under certain circumstances of extreme dimensional tolerance not to have the secondary O-ring seal. In that case we would be left with just one seal, that is the primary O-ring acting as the seal for the SRB case-to-case joint. In that case, then, we would have been dealing with not two seals, as we originally thought, which is why the R was on the nomenclature, but rather one seal and therefore when it was discovered that under these circumstances that such a condition could exist, the criticality was changed from 1R redundant to 1 all by itself.

CHAIRMAN ROGERS: Was the purpose of that to let everyone in the system know the conclusion so they should act accordingly?

MR. LUNNEY: Probably several purposes; that and also——

CHAIRMAN ROGERS: Would that be one purpose?

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MR. LUNNEY: Yes, I think that would be one purpose.

CHAIRMAN ROGERS: The other purposes would be——

MR. LUNNEY: The other purposes would be to keep a good record of those over the life of the program, because we were dealing even at that time with the program that was almost 15 years old, and we wanted to have a good record of what all the critical items were in the program so that as the program evolved new people could be brought up to speed as to what the critical items were and be aware of the design features that were critical.

CHAIRMAN ROGERS: Now suppose that later on a contractor came to the conclusion or Marshall came to the conclusion that after all there was some redundancy. What would you then do?

MR. LUNNEY: Well, the approval of the waiver was in March of '83. At the time I was involved in that, I was operating on the assumption that there really would be redundancy most of the time except when the secondary O-ring had a set of dimensional tolerances add up, and in that extreme case there would not be a secondary seal.

So I was dealing with what I thought was a case were there were two seals unless the dimensional

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tolerances were such that there might only be one seal in certain cases.

CHAIRMAN ROGERS: Now, to me, if you will excuse the expression, that sounds almost contradictory, what you just said. What you first said was you came to the conclusion that you could only rely on the primary seal and therefore you removed the R.

MR. LUNNEY: Yes, sir.

CHAIRMAN ROGERS: And now you're saying, if I understand it, that experience showed that there was redundancy after all.

MR. LUNNEY: No, I don't know of any experience showing that. What I'm saying is that the removal of the R is an indicator that under all circumstances we did not have redundancy. There were a certain number of cases under which we would not have redundancy of the secondary O-ring.

Recognizing that, even though there were a lot of cases where we expected we would have redundancy, we changed the criticality designation.

CHAIRMAN ROGERS: It was saying to everybody else you can't necessarily rely on the primary seal, and if the primary seal fails, as you've said here, there may be loss of vehicle, mission and crew.

MR. LUNNEY: I would only adjust that to say

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you cannot rely on the secondary O-ring but we would expect the primary O-ring to always be there.

CHAIRMAN ROGERS: You would expect after that that if experience showed that there was from time to time a failure of the primary O-ring, then you would say we had better now do something right away because we can't rely on the secondary O-ring?

MR. LUNNEY: And that could happen.

CHAIRMAN ROGERS: The purpose of the change was to alert everybody to this possibility?

MR. LUNNEY: Yes, sir.

MR. KEHRLI: Mr. Lunney, on what basis or what data or what persons were you relying on to determine that the secondary should be there most of the time?

MR. LUNNEY: I have trouble recalling all that transpired in that month, but I recall conversations with Larry Mulloy.

MR. KEHRLI: Tell us about those conversations, please.

MR. LUNNEY: The conversations were that the cases under which the secondary O-ring would not be active were cases where the dimensions of both the seal and the grooves in the cases that the O-ring was being put in would be stacked up in a worst case way such that the secondary O-ring would not be active.

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In those cases we would not have the secondary O-ring active, and therefore we were down to just the primary O-ring, and I don't recall in what forum those conversations were held, but that is what I recall about the waiver at the time.

DR. KEEL: Can I just ask one clarification? If you read the criticality, which of course you signed off on the waiver and I've just re-read it, it doesn't mention tolerances.

MR. LUNNEY: I know.

DR. KEEL: So there's nothing in the criticality that would indicate that that is the basis on which the redundancy was waived?

MR. LUNNEY: I agree, there's nothing in the writeup on dimensional tolerances.

DR. KEEL: This was done, as noted on your waiver, was done outside the Program Review Board?

MR. LUNNEY: Yes, I didn't recall that and still don't, for that matter. Outside the board means that we did not have a meeting where it was discussed at the meeting. Sometimes that is done when the responses to the change are such that there is not any disagreement and there isn't any known issue that wants to be debated on the subject.

In that case, in that kind of case a change

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would be processed outside the board. I would not say that that means that it's outside the review process, which it was well within, but rather it was not dealt with formally at a board with people sitting in the room looking at viewgraphs at the same time.

DR. KEEL: Did this waiver mean that, in fact, that the original design required there be redundancy in this field joint and that you were now waiving that requirement?

MR. LUNNEY: Yes, sir.

DR. KEEL: So you waived that requirement outside the control board and based upon a telephone conversation?

MR. LUNNEY: To the best of my knowledge, it was on the telephone. I don't know. I might have seen Larry personally sometime that month. I don't know. I had no evidence of any quarrel or debate about that.

CHAIRMAN ROGERS: What is the meaning of this whole process, though, if you go through this and you warn everybody and then nothing is done and there's no change made, and then you just keep waiving it? What have you accomplished?

MR. LUNNEY: What we were trying to accomplish was to let people know about the criticality of it and maintain a record of that criticality of all of the

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design items in the program.

CHAIRMAN ROGERS: Which would merely mean that if Larry Mulloy called you and you made a notation of it, you would feel better just because you had a record of it?

MR. LUNNEY: No, I didn't mean for that purpose. I meant to maintain a set of records as to what the criticality of all of these design issues were so that the program would have a ready reference to that many years to come.

VICE CHAIRMAN ARMSTRONG: Just to follow that, at the time you were running the program did you visualize the Criticality 1 items should have any different kind of treatment from 1Rs or 2s or anything else in terms of handling or inspection or any other considerations, or was it just a paperwork kind of exercise?

MR. LUNNEY: Well, I don't know that I felt that it would result in any different handling, but I did not think of it as a paperwork exercise, in that I felt it was important to keep a good record on the program of what the critical items were in the design.

In some cases, those critical items led to special requirements for testing at the Cape before launch. As a matter of fact, the critical items list

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early in the program was used as part of the basis to derive the test requirements and in the test procedures that were conducted at the Cape.

What I mean by that in some cases the rationale for accepting a certain design condition could be that it was testable at the Cape a short time before launch and then it would therefore be tested each time at the Cape before launch.

So it did serve an active purpose in the program and especially on the front end of the program. It had an influence on the kind of test requirements that existed and ultimately the kind of test procedures that were conducted.

CHAIRMAN ROGERS: But you can't really say that in this case, though.

MR. LUNNEY: I cannot say that.

CHAIRMAN ROGERS: In this case, as Mr. Armstrong has mentioned, it is really just a paper process. I mean, you sign it and you say you don't have redundancy that may result in a loss of mission and crew, and then with no change in anything Mr. Mulloy just keeps calling and waiving and nothing happens, and so from that standpoint it is just then a paper transaction.

MR. LUNNEY: Well, it can look that way, yes,

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sir.

GENERAL KUTYNA: There was no priority given in the decision process for Criticality 1? For example, in an airplane there are certain things a sergeant can sign off and certain things the chief of maintenance can sign off, but other things you only fly if the wing commander signs it off, and this was signed off not at Level III? Go/no go on this Criticality 1 and on the O-rings?

Would not the fact that it was a Criticality 1 bump it up to Level II or III?

MR. LUNNEY: General Kutyna, do you mean at the time of this waiver or do you mean later on?

GENERAL KUTYNA: Later on.

MR. LUNNEY: Later on there were several occasions of erosion of the primary O-ring. They were surfaced in Flight Readiness Reviews that I recall as appropriate, and I don't mean to say as I recall. I believe they were always surfaced in the next Readiness Review after the erosion of the O-ring occurred. I am not sure of this, but I believe also that they were treated sometime in the anomaly log from each flight, where we would have an anomaly, and that was also treated.

Then when we went to the Flight Readiness

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Review the subject would be treated again in terms of we had erosion, there were tests conducted to determine that the erosion of the primary seal was a duration limited phenomenon, that it would only occur for a matter of milliseconds, and then it would not propagate any more than that.

They ran tests, as I recall, to be sure you could tolerate more erosion than that and still have a good seal. So each time that there was an event on a flight on erosion of the O-ring, that event triggered a subsequent discussion at the Flight Readiness Review. I would have to say that those discussions were never of the class that you might call a red flag but rather as a class that once we had one.

We saw it occasionally on a couple of the flights while I was there, and it was treated as something that occasionally happened. When we got flow past the O-ring or to the O-ring, that's an erosion but that it was limited in duration and would only go so far, and we had some margin on how far it would go.

DR. KEEL: Mr. Weeks, do you recall the basis on which you waived this for Level I, is that correct?

MR. WEEKS: Yes, sir.

DR. KEEL: Who was the associate administrator then for space flight? That was in 1983.

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MR. WEEKS: Let's see. Officially John Yardley left and General Abrahamson came aboard, and the date I signed it was the 23rd of March, I believe, or the 28th of March. I believe that must have been General Abe.

DR. KEEL: Was it normal for you to sign off on a waiver of a Criticality 1 item as opposed to the associate administrator?

MR. WEEKS: We had no fundamental ground rule about that. During the years that I was deputy to John Yardley we would usually talk about these.

DR. KEEL: Did you talk about this with General Abrahamson?

MR. WEEKS: I can't recall that I did.

DR. KEEL: If you signed off on the 28th, what was the basis that you agreed to waive the requirement?

MR. WEEKS: This whole program of all of the things that were critical on the Shuttle were reviewed, and I think we put in here for your purposes into the report not only this waiver but also the review that Dr. Lovelace and Dr. Frosch [phonetic] put into the program about in the August time frame of 1980 before we flew STS-1.

Of course, I attended every one of the meetings of General Tom Morgan and his propulsion group

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that reviewed all of the propulsion items, and I think it's all here for you to see. We felt at that time—all of the people in the program I think felt that this solid rocket motor in particular more than the SRB was probably one of the least worrisome things we had in the program.

I presume you rather carefully read all of General Morgan's—and General Morgan has retired now but he was head of the propulsion group that did the entire program review. [Ref. 5/2-25]

GENERAL KUTYNA: What year was that?

MR. WEEKS: That was Dr. Lovelace and Dr. Frosch started that about—well, I guess the first meeting was----

GENERAL KUTYNA: When was that meeting when Morgan wrote his comments?

MR. WEEKS: The first review of it that we got was in the May timeframe, but it started a fair bit earlier than that.

GENERAL KUTYNA: Of what year?

MR. WEEKS: In 1980 this was done by the administrator and his deputy because they felt that we really needed to, and properly so, to have a complete verification certification before the program started in 1972 and it was now, what, eight years.

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They thought we should re-review all of the certification on the program.

DR. KEEL: I hate to interrupt you, but can we get back to the original question here for a moment?

CHAIRMAN ROGERS: We have that, so we can look at it.

MR. WEEKS: Well, I presume you did read that.

CHAIRMAN ROGERS: Why do you presume that?

MR. WEEKS: Well, we sent it to you on the tenth of February, sir.

DR. KEEL: Yes, we have it, and amongst the other things it says that General Morgan was told that there was no requirement for redundancy and I assumed that you didn't agree with that at this point, did you?

MR. WEEKS: Well, I don't believe in any of those reviews there was any discussion of abandoning the redundant O-ring seals.

DR. KEEL: Have you read his report of July 1980 where he said that he asked about redundancy verification and he was told that the secondary O-ring was only basically—its primary function was to leak check and there was no requirement for redundancy? Have you read that, in July 1980?

MR. WEEKS: I can't. I can't—is it one of your tabs?

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DR. KEEL: It is probably in the same book you've got there.

MR. WEEKS: I assume I read it, but I can't authoritatively recall the document.

DR. KEEL: But if we can just move along and assume that we have all read it and get back to the question.

What was the basis on which you waived the requirement of redundancy?

MR. WEEKS: The fundamental thing is that there was the review by that impartial group, and then if you read the document itself, I think the fact that it says that the laboratory test program demonstrated the ability of the O-ring to operate successfully when extruded into gaps well over those encountered in this application.

Uniform gaps of an eighth of an inch and over successfully withstood 1,600 psi, which is 1.6 times the operating pressure.

The hydroburst test program out at Wasatch for the standard weight case all have shown that the O-ring can withstand a minimum of four pressurization cycles.

DR. KEEL: I guess the point is, then, that you waived it because you felt the primary O-ring would work? Is that what you're saying, because it would have

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extruded in the gap?

GENERAL KUTYNA: While you're reading from the charts, I would like to make the point that there are no tests at the extreme temperatures is what the next chart says. These are only tests between 40 and 90 degrees, so when you say the tests——

DR. KEEL: But I think that that is irrelevant, and all he's saying is that that says the primary O-ring will seal, and you have to assume that; otherwise, you wouldn't waive it. Is that the basis, then?

MR. WEEKS: But your words were that I signed it because I was depending on the primary.

DR. KEEL: Isn't that what that says, that the primary would work?

MR. WEEKS: No, the secondary seal is still there and provides some capability, and that is the subject of an extreme amount of discussion.

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DR. KEEL: Where does it say that from what you're reading?

MR. WEEKS: It doesn't say that, but everybody knows you have a dual O-ring.

DR. KEEL: Well, that's what you're basing your waiver on. You're just reading from the document. It talks about that the fact that, yes, the primary would work. I certainly wouldn't think you would waive it if the primary wouldn't work because then you would have no seal; no primary, no secondary.

MR. WEEKS: Well, Dr. Keel, the secondary O-ring is still there and does provide, maybe not dual redundancy, but one point X of redundancy.

DR. KEEL: But, Mr. Weeks, I'm not arguing whether it's there or not. I'm just trying to understand why you waived and if you waived it because you thought the primary would work. That is consistent with waiving it.

But if you're saying that you waived it for some other reason, what was the reason?

MR. WEEKS: I waived it because I thought the system would work.

DR. KEEL: Does that mean you thought it was redundant or not?

MR. WEEKS: That I thought it was to a degree

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redundant.

CHAIRMAN ROGERS: Sort of redundant? That's fair enough.

Are there any other questions?

MR. KEHRLI: I have one last question of Mr. Lunney.

During the interviews of some of the people at Marshall, the Commission has received information that in the 1982-1983 timeframe, there was a conscious decision between Level II at Johnson, your office, and Marshall, that some of the flight anomalies or problems that were discovered after flight would no longer be reported from Marshall to Level II to the same degree that they had been in the past.

Do you recall that?

MR. LUNNEY: No, I don't.

MR. KEHRLI: To your knowledge, did that ever happen? Was there ever a decision not to report problems from Marshall to Level II at Johnson?

MR. LUNNEY: I just don't recall anything like that.

CHAIRMAN ROGERS: Mr. Armstrong.

VICE CHAIRMAN ARMSTRONG: I wanted to ask Mr. Weeks as a proper basis for the next question, would you tell me what your responsibility is?

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MR. WEEKS: Today or at the time of this?

VICE CHAIRMAN ARMSTRONG: Let's say in the past year.

MR. WEEKS: In the past year I have been the Deputy Associate Administrator of the Office of Space Flight, Technical.

VICE CHAIRMAN ARMSTRONG: And under you would fall certain experts such as Mr. Winterhalter for the solid rocket booster?

MR. WEEKS: Well, I think, Mr. Armstrong——

VICE CHAIRMAN ARMSTRONG: And what would their responsibilities be?

MR. WEEKS: Well, I performed essentially in the entire time that Mr. Moore was the AA, there was no principal Deputy, and so I basically performed in both roles during the tenure of Mr. Moore.

VICE CHAIRMAN ARMSTRONG: And during the period of 1985 when the questions of O-ring sealing were of major interest, you and your staff were closely involved with those discussions and with presentations to Headquarters in August of 1985 and so on; am I correct?

MR. WEEKS: We made them happen.

VICE CHAIRMAN ARMSTRONG: And you were in fact operating in the Level I/Level II arena? The Level I

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arena particularly.

MR. WEEKS: Well, I recall at the Level I arena particularly, I believe.

VICE CHAIRMAN ARMSTRONG: As you know, we've had a lot of discussions about communication difficulties on the night before the 51-L launch, of information properly getting to Level II and Level I. And should it have, or should it have happened differently?

Is it not a fact that you and your people were very well aware of these particular difficulties and interests and you did—you were at Level I and although you may not have been involved directly in this communication chain, it wasn't a fact that you were not aware of these kinds of problems?

MR. WEEKS: Certainly I was. I happened not to be there. There were congressional hearings, and I was in Washington at the time of the launch and that night, but absolutely, I attended every FRR of the 25 flights and every L-1 of the 25 flights.

VICE CHAIRMAN ARMSTRONG: If I understand it right, you shared these concerns about the proper sealing of the joint under all conditions and so on. And that is what it was that in fact caused you to require some of these presentations to your offices and

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so on; am I right?

MR. WEEKS: Yes. The thing that triggered us most in the Office of Space Flight was the fact that the 29th of April launch of 1985 was the first occurrence in the nozzle of the secondary O-ring ever having any erosion and that caused me to review it in Wasatch with others, and then for a more complete review on August 19th in Washington.

VICE CHAIRMAN ARMSTRONG: Well, it has occurred to me and others that your group was essentially a redundant path of information in this time period and that even though you weren't involved in the particular meetings that we've discussed in earlier public hearings, that you were in fact aware of the problems and it might have well been expected that you might have passed these concerns on, knowing it was a winter launch and so on, to Mr. Moore or Mr. Aldrich or others.

I wonder, am I misinterpreting that that might have been possible?

MR. WEEKS: Well, we certainly in a number of cases brought to—we have a monthly meeting. It is called the Associate Administrator's Montly Meeting. And during that time we bring forward the directors, like Mr. Winterhalter, and the other directors on the orbiter, et cetera, to the Associate Administrator and

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myself and review quite across the board, all technical, programmatic and cost issues.

And that was done and is done and the O-ring was actively brought into those monthly meetings during the—well, during the entire last couple of years, where as you well know, there were many of those erosion cases. There was nothing hidden in the FRRs. There was nothing hidden in the Associate Administrator Monthly Reviews that didn't bring forward that there was a significant O-ring erosion problem.

And then when the first case of the secondary O-ring being eroded to the tune of about 32/1000 of an inch, that caused a number of reviews, which is in that document of February 10th and culminated, I guess you would say, in the August 19th review.

And, of course, Mr. Moore couldn't attend that. He had some other requirement. But we then carried it to him and reviewed it with him, and I remember quite vividly saying I'm still not satisfied and said I've got to talk to somebody, and decided that the most prudent individual that I had confidence in was Mr. George Hardy, and I did so.

But Mr. Hardy allayed my concerns. Maybe I was not tough enough, but I began to believe that what we were doing with the filament wound case changes was

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pretty prudent action.

VICE CHAIRMAN ARMSTRONG: Both Mr. Moore and Mr. Aldrich have testified to us that they didn't get information of this concern, at least on the night before the launch. But I gather from your testimony that they somehow should have been very well aware of this general type of concern as a result of you and your staff's activities and your responsibilities.

MR. WEEKS: Well, I can't speak as authoritatively for Mr. Aldrich. He's in Houston. But certainly Mr. Moore attended every FRR, I believe, and every L-1 in his tenure as the AA, and he certainly knew that we were having those erosion problems starting about two years earlier than that time.

DR. KEEL: If I could follow up, Mr. Armstrong's questions, I think Mr. Aldrich's testimony was to the effect that the level of concern wasn't in fact reflected in the Flight Readiness Reviews, and I guess I would like to follow up on that and find out if you had some way of knowing the level of concern other than the Flight Readiness Reviews before your action you took in June of 1985.

For example, on 51-C, which is the previous coldest launch, Mulloy in fact asked through a certified urgent message ultimately for Morton-Thiokol to recap

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all incidents of O-ring erosion and evidence of low past primary O-ring.

As a consequence of that, that was done by Morton-Thiokol, an extensive briefing was put together including the entire history of O-ring erosion, field joints, nozzle joints back in February 1984 in the 51-E Flight Readiness Review and talked, of course, also about the 51-C experience.

And the conclusion in that presentation was that low temperature enhanced probability of blow-by. STS-51-C experienced worst case temperature change in Florida history.

Now, that turned out not to be presented in the Flight Readiness Review at Level I. Did you know about that and that conclusion?

MR. WEEKS: I don't think I knew about that specific thing you're reading from.

DR. KEEL: It was the preboard Flight Readiness Review to Marshall by Thiokol.

MR. WEEKS: No. Normally, I do not and have not in the 25 flights attended or listened on the network to Level III reviews.

DR. KEEL: So you were only principally in on the Level I Flight Readiness Reviews?

MR. WEEKS: Yes, sir.

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DR. KEEL: So if this information wasn't passed up, you didn't know about it?

MR. WEEKS: No. I don't think that's a proper thing to say, sir.

DR. KEEL: Let me ask that same question again. What did you know about this briefing and this conclusion for 51-E?

MR. WEEKS: I don't recall how bad 51-E was. I don't recall it quite that way. The thing that I recall and triggered upon was the one that was the April 29th of 1985.

DR. KEEL: I was going to get to that next. I think that is what piqued your interest.

MR. WEEKS: But there were others.

DR. KEEL: What I'm trying to find out is if—Mr. Armstrong has asked, did you know more than what was being presented in these Flight Readiness Reviews back in January of 1984?

MR. WEEKS: Absolutely. Yes, sir. And that can be clearly shown if you look through our February 10th submission of what the director level—this is Mr. Winterhalter—brought to the AA.

DR. KEEL: I guess Mr. Armstrong's question then still stands. If you knew more, then why wasn't that communicated to Jesse Moore and to Arnie Aldrich who testified that they didn't know the level of

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concern?

CHAIRMAN ROGERS: In fact, Mr. Aldrich said quite the opposite. He said he didn't remember at all. He was quite unaware that this problem had continued so long. And he also was quite unaware that there had been references to weather affecting the joint.

Your testimony is, it seems to contradict that.

DR. RIDE: He was also surprised that the problem wasn't tracked and worked to resolution through Level II; that it never appeared before his Level II board.

CHAIRMAN ROGERS: I presume you've read his testimony

MR. WEEKS: I have read his testimony.

CHAIRMAN ROGERS: Isn't his testimony quite contrary to what you're saying?

MR. WEEKS: Well, I, Dr. Ride, did not attend Level II FRRs either. So I don't know what he may have heard from Level III to Level II. I just don't know.

CHAIRMAN ROGERS: Well, let's be a little more direct. You've read his testimony and he said he was not aware of the serious nature of this concern. And he was head of Level II and he was in the decision making process. He said he was quite unaware of it, if you go

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back and read his testimony.

Now, in answer to Mr. Armstrong's questions, I got the impression you didn't think there was anything wrong with the system at all. Everybody knew what everybody else was doing, and I gather you don't think the system of communications should be changed.

Is that right?

MR. WEEKS: Well, I think that most any system can be improved, but certainly there was no—I don't know what—I got more information through David Winterhalter and his people that is another loop that gives us information in Level I that does not necessarily get into the FRR Review.

DR. RIDE: Didn't it surprise you that there wasn't any activity on this problem coming out of Level II?

MR. WEEKS: Well, there was activity, Dr. Ride, going on at Level III in changing QM-5 and in changing the filament wound case qualification motor in terms of the O-ring and the separation of the O-ring, et cetera.

DR. RIDE: But aren't major flight problems in the Shuttle Program normally tracked and resolved through Level II? Isn't that where you would expect that problem to be resolved? And wouldn't you expect it

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to come to the attention of Mr. Aldrich and expect him to be taking action on it?

MR. WEEKS: Well, that would be improvement, I believe. Yes.

CHAIRMAN ROGERS: Did you read Mr. Aldrich's testimony where he listed flaws in the system as he saw them and the ones that had to be corrected, and do you agree with what he testified to on that score?

MR. WEEKS: I can't recall all of his statements.

CHAIRMAN ROGERS: Well, we'll see that you get the testimony.

MR. KEHRLI: Were you aware that there was an active launch constraint that had to be waived, flight by flight after 51-B returned with the bad nozzle joint O-ring erosion problem?

MR. WEEKS: I know that at Level I we dispositioned the case of 51-B at the next launch and dispositioned the fact that the nozzle had erosion of both primary and secondary. I knew that, and we did disposition it.

MR. KEHRLI: Did you know there was a launch constraint?

MR. WEEKS: I did not know that there was a launch constraint.

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MR. ACHESON: Mr. Weeks, the contractor certification documents for the pre-FRR certification documents indicate a routine checkoff and ready to fly from Thiokol for 51-L and a routine checkoff and ready to fly from Level III to Level II. And that undoubtedly is why Level II was unaware of any problems with the joints.

The parallel track of work that was dealing with the concerns on the joint seemed to have been known to Marshall and presumably got to Level I through the August headquarters review.

But, of course, Level II wasn't part of that either, and so you appear to have an FRR procedure that made no mention of these problems, presumably on the assumption that everybody knew about them because they had parallel paths of work going forward and Level II was not a party to any of those parallel paths of work. And so it was a system designed to freeze out Level II from any knowledge of the problem.

At least that is the way it strikes some of us. Doesn't that seem a little bizarre?

MR. WEEKS: The Flight Readiness Reviews in no way hid the fact that there was erosion on the O-rings time after time, starting in February of 1984, which was Flight 10, and many following that. And our basic

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system at the Flight Readiness Reviews was to review all anomalies of the previous flight and we did so before proceeding with—

DR. KEEL: Can I just follow that up? I think what's troubling us is not that there was erosion—everyone knew that—but the severity of it. And, for example, going back to the 51-E Flight Readiness Review and the briefing, the Flight Readiness Review that was done at the Project level, the Level III level, was an extensive briefing on the whole O-ring history. When it got up to Level I, and plus they drew the conclusion that temperature made it worse.

When it got up to Level I it basically was, in fact, winnowed down to one bullet entry that said evidence of hot gas past primary O-rings on two case joints. Concern: mission safety. Resolution: acceptable risk because of limited exposure and redundancy.

So that extensive review and history plus the concerns over the temperature effects got diluted down to that statement.

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MR. WEEKS: I am sure that is true.

CHAIRMAN ROGERS: Can I ask a question? Maybe you have already answered, but I want to be sure that I heard it right. Were you aware of the constraints put on Flights 51—what are these numbers?

DR. RIDE: It is 51F and subsequent.

CHAIRMAN ROGERS: All of those flights, that there were constraints on those?

MR. WEEKS: No, I was not.

CHAIRMAN ROGERS: Why wouldn't you be advised of that? If you were in the loop and you had been sort of dealing with this information, and you think the system is working pretty well, why wouldn't you know about launch constraints?

MR. WEEKS: Because I felt our system of FRRs, of bringing forward any anomalies of the previous launches was a pretty good system of us knowing of whether we have a problem or not.

CHAIRMAN ROGERS: But in this case you not only brought them forth and decided a problem, you put constraints on it. And you said you can't fly unless there is a waiver, but you didn't know that.

MR. WEEKS: I did not know that.

MR. HOTZ: Well, wouldn't a launch constraint be brought up in the FRR?

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MR. WEEKS: Yes.

MR. HOTZ: Particularly if it required a waiver?

MR. WEEKS: Yes, if it is a launch constraint, it is a Level 1 item.

MR. HOTZ: But apparently it wasn't brought up at these meetings.

MR. WEEKS: Now, it is possible, but it is the way it is looked at, and every anomaly has to be reviewed.

MR. HOTZ: But every anomaly is not a launch constraint, is it?

MR. WEEKS: Well, I think in the case of this O-ring they are blood brothers.

MR. HOTZ: But I am talking about other anomalies. Because you have an anomaly, it doesn't mean that everything has a launch constraint, right? It has to be a big anomaly to have a launch constraint.

MR. WEEKS: Yes.

CHAIRMAN ROGERS: We had testimony today that it is a very serious matter to have a launch constraint, and there were very few of them, and it was just these that were waived for about six or seven flights, and then they were ended just before Flight 51L.

Well, thank you. I think that is all. Thank

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you very much. And we would like to just have a quick session with the Commission before we end.

(Whereupon, at 5:20 p.m., the Commission was adjourned.)

SRB CRITICAL ITEMS LIST

Sheet 1 of 2

Subsystem: SOLID ROCKET BOOSTER	Criticality Category: 1 Reaction Time: Immediate to Sec.
Item Code: 10-01-01 Item Name: Case, P/N (See Retention Rationale) (Joint Assys. Factory P/N 1U50147 Field: 1U50747)	Page: A-6A Revision:
No. Required: 1 (11 segments, 3 Field Joints, 7 plant joints)	Date: December 17, 1982
FMEA Page No. A-4 of MSFC-RPT-724	Analyst: Garber
Critical Phase: Boost	Approved:

Failure Mode & Cause: Leakage at case assembly joints due to redundant O-ring seal failures or primary seal and leak check port O-ring failure.

NOTE: Leakage of the primary O-ring seal is classified as a single failure point due to possibility of loss of sealing at the secondary O-ring because of joint rotation after motor pressurization.

Failure Effect Summary: Actual Loss - Loss of mission, vehicle, and crew due to metal erosion, burnthrough, and probable case burst resulting in fire and deflagration.

RATIONALE FOR RETENTION

Case, P/N 1U50129, 1U50131, 1U50130, 1U50185, 1U50147, 1U50715, 1U50716, 1U50717

A. DESIGN

The SRM case joint design is common in the lightweight and regular weight cases having identical dimensions. The joint concept is basically the same as the single O-ring joint successfully employed on the Titan III solid rocket motor. The SRM joint uses centering clips which are installed in the gap between the tang O.D. and the outside clevis leg to compensate for the loss of concentricity due to gathering and to reduce the total clevis gap which has been provided for ease of assembly. On the shuttle SRM, the secondary O-ring was designed to provide redundancy and to permit a leak check, ensuring proper installation of the O-rings. Full redundancy exists at the moment of initial pressurization. However, test data shows that a phenomenon called joint rotation occurs as the pressure rises, opening up the O-ring extrusion gap and permitting the energized O-ring to protrude into the gap. This condition has been shown by test to be well within that required for safe primary O-ring sealing. This gap may, however, in some cases, increase sufficiently to cause the unenergized secondary O-ring to lose compression, raising question as to its ability to energize and seal if called upon to do so by primary seal failure. Since, under this latter condition only the single O-ring is sealing, a rationale for retention is provided for the simplex mode where only one O-ring is acting.

The surface finish requirement for the O-ring grooves is 63 and the finish of the O-ring contacting portion of the tang, which slices across the O-ring during joint assembly, is 32. The joint design provides an OD for the O-ring installation, which facilitates retention during joint assembly. The tang has a large shallow angle chamfer on the tip to prevent the cutting of the O-ring at assembly. The design drawing specifies application of O-ring lubricant prior to the installation. The factory assembled joints have NBR rubber material vulcanized across the internal joint faying surfaces as a part of the case internal insulation subsystem.

A small MS port leading to the annular cavity between the redundant seals permits a leak check of the seals immediately after joining segments. The MS plug, installed after leak test, has a retaining groove and compression face for its O-ring seal. A means to test the seal of the installed MS plug has not been established.

The O-rings for the case joints are mold formed and ground to close tolerance and the O-rings for the test port are mold formed to net dimensions. Both O-rings are made for high temperature, low compression set fluorocarbon elastomer. The design permits five scarf joints for the case joint seal rings. The O-ring joint strength must equal or exceed 40% of the parent material strength.

B. TESTING

To date, eight static firings and five flights have resulted in 180 (54 field and 126 factory) joints tested with no evidence of leakage. The Titan III program using a similar joint concept has tested a total of 1076 joints successfully.

[Ref. 5/2-26 1 of 2]

SRB CRITICAL ITEMS LIST — Sheet 2 of 2

Subsystem: SOLID ROCKET BOOSTER	Criticality Category 1 Reaction Time Immediate to Sec
Item Code: 10-01-01	Page: A-68
Item Name: *Case, P/N (See Retention Rationale) (Joint Assys, Factory P/N 1U50747 Field: 1U50717	Revision:

RATIONALE FOR RETENTION (CONT'D)

A laboratory test program demonstrated the ability of the O-ring to operate successfully when extruded into gaps well over those encountered in this O-ring application. Uniform gaps of 1/8-inch and over (TWR-13486) successfully withstood pressures of 1600 psi. The Hydroburst Program (TWR-11664) and the Structural Test Program (STA-1) for the standard weight case (TWR-12051) and the Lightweight Case Joint Certification Test (TWR-12829) all have shown that the O-ring can withstand a minimum of four pressurizations before damage to the ring can permit any leakage.

Further demonstration of the capability of joint sealing is found in the hydro-proof testing of new and refurbished case segments. Over 540 joints have been exposed to liquid pressurizations at levels exceeding motor MEOP with no leakage experienced past the primary O-ring. The only occasions where leakage was experienced was during refurbishment of STS-1 where two stiffener segments were severely damaged during cavity collapse at water impact.

A more detailed description of SRM joint testing history is contained in TWR-13520, Revision A.

C. INSPECTION

The tang -A- diameter and clevis -C- diameter are measured and recorded. The depth, width and surface finish of the O-rings grooves are verified. The surface finish of the tang is also verified. Characteristics are inspected on each O-ring to assure conformance to the standards to include:

- o Surface conditions
- o Mold flashing
- o Scarf joint mismatch or separation
- o Cross section
- o Circumference
- o Durometer

Each assembled joint seal is tested per STW7-2747 via pressurizing the annular cavity between seals to 50 ± 5 psi and monitoring for 10 minutes. A pressure decay of 1 psig or greater is not acceptable. Following seal verification by QC, the leak test port plug is installed with QC verifying installation and torquing.

D. FAILURE HISTORY

No failures have been experienced in the static firing of three qualification motors, five development motors and ten flight motors.

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[Ref. 5/2-26 2 of 2]

DOC NO. TWR-13520 VOL REV A
TITLE

RETENTION RATIONALE, SRM SIMPLEX SEAL

Howard H. McIntosh
Prepared by
Howard McIntosh

Rel - J Tibbitts

1 December 1982

Thiokol / WASATCH DIVISION
A DIVISION OF THIOKOL CORPORATION
P.O Box 524, Brigham City, Utah 84302 801/863-3511

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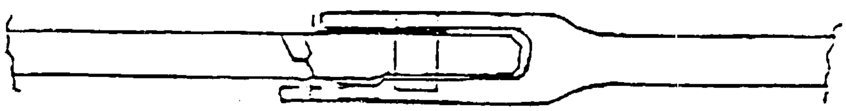
RETENTION RATIONALE, SRM SIMPLEX SEAL

The SRM steel case joint design is common to the regular weight case and the light-weight case. All dimensions are the same and the assemblies are identical, including the shimming after assembly. The joint is basically a modification of the single O-ring Titan tang and clevis joint with the same tolerances, but allowing more clevis gap for horizontal assembly. A second O-ring provides a leak check test to be made, between O-rings, to verify the O-ring seal after assembly. Shims are used between the clevis outer leg and the tang to obtain the maximum amount of O-ring squeeze for sealing.



This report contains a summary of numerous tests and uses of the SRM joint to provide the rationale for the retention of the simplex seal. Total experience to date includes:

a. Similar joints
b. Leak checks at joint assembly
c. Hydrotests
d. Hydrobursts
e. Static motor firings
f. Motor flights
g. Laboratory bench tests

Experience has shown positive functioning of the primary O-ring in all instances of use in the SRM tang and clevis joint. Testing has indicated positive sealing under adverse conditions beyond the required single pressurization for motor operation. It is concluded that considering the SRM joint as a single O-ring seal, sufficient rational exists to retain this design with assurance of performance. A data base is also being established in support of the secondary O-ring positive sealing.

A. Similar Joint Use

1. The Titan case joint contains a single O-ring bore seal. The tang-clevis-pin joint design of the SRM uses the same design

tolerances as this well proven joint. Titan joints have a record of over 1100 joint use data points during the testing and throughout its flights with no loss of pressure. The SRM joint differences include: Increased clevis gap for horizontal assembly, second O-ring added to verify single O-ring presence and uniform shimming to center tang and close the clevis gap.

2. Initial assemblies of SRM joints contained no shims and all routine hydrotests are conducted without shims.

3. Leak Checks (Over-all total of 930)

1. All new segments have leak checks before hydrotest on all tangs and clevises. No leaks experienced.

   a. Increment I 28 domes + 2x122 segments = 272 tests

   b. Increment II 12 domes + 2x73 segments = 158 tests

   Total 430 tests

   Note: Vertical assembly
   no leaks experienced

2. All plant joints have leak checks on case fabrication assembly and refurbishment proof test (in casting segment configuration). Vertical.

   a. Fabrication of 8 static tests - 56
   14 flight cases - 98
   4 GTM + STA-1 - 31
   TOTAL 185

   Two leakers experienced and debris found as cause

   b. Refurbishment hydroproof of
   7 static tests - 49
   26 GTMs - 14
   6 Flight Cases - 42
   TOTAL 105 plant joint

   No leakers experienced. (One stiffener to stiffener joint leak upon depressurization).

3. Field joints are tested horizontally at Thiokol for static firings only. All other leak checks are conducted with vertical assembly.

   a. Static tests 8 x 3 = 24 checks
   Repeats= 8 minimum

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Four leakers experienced with 2 on DM-[illegible] and one each on D[illegible] and DM-5. No leaks after reassembly.

b. CTMs were assembled at MSFC and KSC

$4 \times 2 \times 3 = 24$ checks

c. Flight checks at KSC $5 \times 3 \times 2 = 30$ checks

extra $1 = 1$

Total 31

One leaker experienced and debris identified.

d. Refurbishment hydrotest $17 \times 6 = 102$ checks

e. Extra checks on hydroburst, joint verification and STA-1 efforts produced approximately 20 checks with no leakers on first cycle joints.

C. Hydrotests at Rohr, Thiokol and MSFC have been conducted at pressures above 1037 psig with no leakers.

1. At Rohr $150 + 85 = 235$ tests on segments
2. At Thiokol $17 \times 4 = 68$ tests

   Extra $= 3$

   71 Tests

   No leakers experienced upon or at pressure. (One stiffener to stiffener joint leaked upon depressurization).
3. MSFC tests on STA-1 pressurization with new O-rings produced no leaks even after 4 pressure cycles (experienced O-ring "pinching" during depressurization and nibbling after cycling).

D. Hydrobursts -

1. Regular weight case joints were cycled with proof pressurizations and experienced leakage past the "nibbled" primary O-ring after eight cycles. After twenty cycles, the O-rings were replaced and maximum design pressurization was achieved (1.4 safety factor) however leakage past the rotated joint O-rings occurred at high pressures (1,420 psig or 1.58 x MEOP).
2. Lightweight case joints passed all tests which included four cycles to MEOP (one with a planned defective primary O-ring) and an 1.4 MEOP pressurization. The joints were then sealed on the inside with vulcanized rubber to enable the burst to take place at 1550 psig.

E. Static Motor Firings

No joint leaks have been experienced during eight horizontal static motor firings. Adverse conditions of joint movement from a "sagged" position to flight position upon pressurization were experienced without joint leakage. A pressure reading, taken on each test, between the O-rings of the center field joint, showed variations in pressure traceable to joint movement (from vacuum to pressure above ambient).

F. Five Shuttle flights have flown involving ten SRM cases with no evidence of a pressure leak past the primary O-ring of the joints.

G. Laboratory Bench Tests.

1. High pressure extrusion tests have shown pressure retention of a standard .280 in. dia. O-ring in a gap of 0.125 in. at 1600 psi.
2. Low pressure check with the sealing surface defects testing device show remarkable sealing power of the single O-ring with large, deep and rough surface defects using minimum squeeze.

REDUNDANT SEAL DATA BASE - In order to establish a redundant seal data base, additional data are being obtained on all refurbishment hydrotests by checking the actual joint movement due to pressurization with a direct reading dial indicator through the pressure port. Initial information generated in a lighweight cylinder to cylinder proof test shows a total movement of only .030 in. at 1004 psig in the centerjoint (dial indicator in tang against land between O-ring grooves on the clevis inner leg). This test conducted in the normal vertical mode, indicates that the tang to clevis movement will not unseat the secondary O-ring at operating pressures. This one point data base will be up to ten points after DM-5 and STS-5 cases have been refurbished.

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[illegible]

[illegible]. Dorsey

J. Kilminster

C. Spaak

R. B. Jolly

J. Leavitt

R. Berte

B. Brinton

H. McIntosh (3)

R. Laney

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DOC
NO. ____ VOL ____

PAGE

[Ref. 5/2-27 6 of 6]

MSFC-RPT-725

CHANGE SHEET

FOR

CRITICAL ITEMS LIST

SOLID ROCKET BOOSTER AND

RANGE SAFETY COMMAND DESTRUCT SYSTEM

CHANGE NO. 23

DECEMBER 1982

[Ref. 6/2-28 1 of 14]

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GEORGE C. MARSHALL SPACE FLIGHT CENTER

MARSHALL SPACE FLIGHT CENTER, ALABAMA 35812

CHANGE NOTICE 23

repared By: G. Hoskins
G. Hoskins USBI 12/21/82
Date

pproved By: WLT Robert E Smith
for J. McMillan, Custodian
Critical Items List
for the SRB and RSCD System
12/23/82
Date

proved BY: L Dan Woodruff
for W. Chubb
Chief, Systems Engineering Division
12/23/82
Date

erials' roval: Ted L Knowling
T. L. Knowling, Jr.
12/23/82
Date

ss oval: Paul W Frederick
Paul W. Frederick
12/23/82
Date

[Ref. 5/2-28 2 of 14]

CHANGE NOTICE

1. Remove the following pages and replace with the indicated attached pages:

REMOVE	REPLACE WITH
A-35 and A-36	A-35 and A-36

2. Add the following pages:

A-6A
A-6B

NOTE: A black bar and CN 23 in the margin indicates the data that was changed. CN 23 Add in the margin designates the addition of a new sheet.

Sign and date this page in the space provided below to show that the changes have been incorporated and file immediately behind "Change Notice Sheet."

Signature of Person Incorporating Changes — Date

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Page A-35

ITEM CODE: 10-01-01

FAILURE MODE: "Leakage at case assembly joints" was reclassified from Category 1R to single failure point, Category 1. See CIL pages A-6A and A-6B for SFP analysis.

[Ref. 5-2-28 4 of 14]

2. ECN	MSFC ENGINEERING CHANGE REQUEST (See Instructions on reverse)	3. DATE:	4. PAGE
77701		12-21-82	1 of 1

	6. THRU:	7. FROM:
/Mr. Hardy	EE11/Mr. Horton	EL54/W. Trewhitt

OF CHANGE: SRB and PSCD System CIL Change Notice No. 23 JAN 03 1983

DED PRIORITY: Emergency ☐ Urgent ☒ Routine

10. NEED DATE:

RAM(S)/PROJECT(S) AFFECTED:

13. END ITEM(S) AFFECTED BY NOMENCLATURE:

MMENDED EFFECTIVITY: thru A18

14. BASELINE DOCUMENTATION AFFECTED (Spec., ICD, etc.): MSFC-RPT-725

TED CHANGES (ECR, ECP, CR, etc.) BY NUMBER:

FICATION FOR CHANGE (Include effect if not incorporated) (If necessary, continue on MSFC - Form 2327-1, continuation sheet):

hed Change Notice No. 23 updates the SRB CIL to include current data. This change ssifies failure mode, "Leakage at case assembly joints," from category 1R to single re point, category 1.

TS ON:

Hardware ☐ Facility ☐ Schedule (See Enclosure ______ for impact) ☐ Other (Specify)

Software ☒ Requirements Documentation ☐ Cost (Estimated cost included in Enclosure ______)

OF CHANGE (Include reference to enclosures) (If necessary, continue on MSFC - Form 2327-1, continuation sheet):

ttached Change Notice No. 23.

URE OF ORIGINATOR:	DATE:	TELEPHONE NUMBER:	OFFICE SYMBOL:
[signature] Trewhitt	12-21-82	3-2663	EL54

CONCURRENCE

SIGNATURE & ORGANIZATION	DATE	SIGNATURE & ORGANIZATION	DATE

TECHNICAL APPROVAL

SIGNATURE & ORGANIZATION	DATE	SIGNATURE & ORGANIZATION	DATE

** TOTAL PAGE.10

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DATE

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