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INTRODUCTION

This document has been prepared as a presentation of the proceedings of the Outer Planet Probe Technology Workshop held at the NASA Ames Research Center, May 21-23, 1974. The Workshop was sponsored by Mr. D. Herman of the Advanced Programs and Technology Office, NASA Headquarters; and Mr. B. Padrick of the Advanced Space Projects Office, NASA Ames Research Center. The General Chairman was Mr. A. Seiff of NASA and Mr. N. Vojvodich of NASA Ames was the Technical Chairman.

The purposes of the Workshop were:

- Review and summarize the state-of-the-art concerning mission definitions, probe requirements, systems, subsystems, and mission-peculiar hardware.
- Explore mission and equipment trade-offs associated with a Saturn/Uranus baseline configuration and the influence of Titan and Jupiter options on both mission performance and cost.
- Identify critically required future R&D activities

To accomplish these purposes, the Workshop was organized into ten sessions, or panels, covering the broad spectrum of science and engineering subjects concerned with the planning and implementation of in-situ measurements at the outer planets using atmospheric entry probes. Presentations of subject material were made by the participants as indicated in the program (see next section herein). Following the session presentations, each panel convened a "splinter" meeting during which the topics, problems, etc. were discussed in more detail. The eleventh session was a summary roundtable discussion on the concluding afternoon of the Workshop during which each panel chairman reviewed the key points covered during their respective sessions and splinter meetings.

These proceedings have been prepared by DYNATREND INCORPORATED; Burlington, Massachusetts under NASA Ames Research Center Contract No. NAS2-7541.

SESSION I - KEYNOTE ADDRESS, TUESDAY, MAY 21, 1974:

Introduction by Mr. A. Seiff of NASA Ames Research Center, the General Chairman of this Workshop.

MR. SEIFF: Dr. Hans Mark is not going to be with us this morning. He was required to be in a meeting at Boulder, Colorado but is very ably represented by Si Syvertson.

I would just like to say a word or two to introduce Si even though I think most of you know him. But for those of you who don't, he speaks with some authority in the business of entry technology for the reason that maybe ten or fifteen years ago he was one of the group of people who were working on the early lifting reentry bodies at Ames which were called M-1, M-2 and so on. He has also been in the advance mission business because for a period of time he was the Chief of the Mission Analysis Division, stationed at Ames and reporting to NASA Headquarters. So Si, would you please say a few words to the group here?

MR. SYVERTSON: I'm glad Al can remember when I used to do useful things for a living. It's kind of surprising, and gratifying, to see the size of the turnout to this Workshop. We don't often get this many people in this kind of an area anymore. We are very happy to see everybody here.

As Al indicated, Ames has been interested in entry technology for a long time, going back, I guess, more than twenty years when Harvey Allen first got us started in the business. In more recent years we have been more interested in applying what we've learned rather than in the basic research areas. As everybody here is aware, we are embarking on the Pioneer-Venus program that will send multiple probes into Venus in a few years.

Later today, or tomorrow, you will hear some of the preliminary results from Pioneer 10 with regard to defining the atmosphere on Jupiter. My understanding is that the preliminary results indicate that the entry problem there is not quite so severe as we once thought it was. I understand you are going to be looking at probes for other missions to the outer planets.

I've looked over the schedule and it looks like a very interesting meeting. I hope you enjoy it and I hope you find it informative.

On behalf of Dr. Hans Mark and the rest of the Center, I want to welcome you here to Ames. Thank you.

MR. SEIFF: This is probably the first meeting of a technical nature that I've ever attended that has a Keynote Address. It is going to be made by a man who is parked illegally, I was just informed a few minutes ago. This address is to be given by Dan Herman who has been with the Headquarters NASA Office of Space Sciences for many years. During that whole period, I have felt that he has been a real sparkplug in keeping the Agency moving towards the definition of its future programs. He has been president of practically all, if not all, of the Pioneer-Venus Science Steering Group meetings and playing an active role in the implementation of that project as well. So, Dan is going to talk to us a little bit about what he thinks the prospects are for Outer Planet Probe Missions.

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KEYNOTE ADDRESS
MR. DANIEL HERMAN - NASA HEADQUARTERS

MR. HERMAN: I am not really going to give a keynote address in the formal sense of the word. Rather, what I thought I would do is to tell you what the current status within NASA is for an outer planets probe program.

I will begin with this first picture (Figure 1-1) of the so-called official NASA mission model as of last October. These are the missions Dr. Fletcher presented to the Congress in his testimony in October and have been carried on the books as the official NASA plan. Currently, this plan is in the process of being changed because our thinking with respect to the outer planet probe missions has changed. I will indicate the changes from this so-called official NASA mission model of last October to our current thinking.

Originally, the outer planet probe missions in our plan were those stipulated by the Outer Planet Science Advisory Group, headed by Jim Van Allen. The so-called "three to make two" concept where in three opportunities dedicated Pioneer probe missions are launched to Saturn and Uranus, with the last one to either Saturn, Uranus or Titan as a function of the success or failure of the two predecessors. This strategy of the "October plan" is shown on the second schedule (Figure 1-2).

In 1979, we would send a dedicated Uranus probe mission to fly by Jupiter and be deflected to Uranus. The arrival at Uranus would be 1984. Then, in the 1980 opportunity, we would send a probe to Saturn directly and that probe would reach Saturn in 1984. Then in 1981, we would launch a probe mission, the Saturn-Uranus swing-by opportunity, which would reach Saturn in 1985 after both earlier probes had encountered Saturn and Uranus. If both earlier

Editor's Note: Mr. Herman's remarks accurately reflect the programmatic and fiscal situation at the time of the workshop. Subsequent changes in available resources and other programmatic considerations may alter the mission schedule described in his remarks.

PLANETARY EXPLORATION PROGRAM (PL)

October 1973

Payload Code	Payload	CY	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	Total
	Approved Programs																					
PL-1	Mariner Venus/Mercury		1																			1
PL-2	Pioneer Jupiter Flyby		1																			0
PL-3	Helios			1		1																2
PL-4	Viking 75				2																	2
PL-5	Mariner Jup/Sat 77						2															2
	Inner Planets																					
PL-6	Viking Orbiter/Lander 79								1													1
PL-7	Surface Sample Return													2								2
PL-8	Satellite Sample Return																			1	1	2
PL-9	Pioneer Venus							2														2
PL-10	Inner Pl. Follow-On									1	2		1			1						5
PL-11	Venus Radar Mapper												2									2
PL-12	Venus Buoyant Station														2							2
PL-13	Mercury Orbiter																2					2
PL-14	Venus Large Lander																		2			2
	Outer Planets																					
PL-15	Mariner Jup/Uranus Flyby								2													2
PL-16	Pioneer Jup/Uranus Flyby (Uranus Probe)								1													1
PL-17	Pioneer Saturn Probe									1												1
PL-18	Pioneer Sat/Uranus Flyby (U Probe)										1											1
PL-19	Mariner Jupiter Orbiter										2											2
PL-20	Pioneer Jupiter Probe													2								2
PL-21	Mariner Saturn Orbiter														2							2
PL-22	Mariner Uranus/Nep Flyby															2						2
PL-23	Jupiter Sat. Orb/Lander																			1	1	2
	Comets & Asteroids																					
PL-24	Dual Comet Flyby					1																1
PL-25	Encke Slow Flyby								1													1
PL-26	Encke Rendezvous										2											2
PL-27	Halley Flyby														1							1
PL-28	Asteroid Rendezvous															2						2
	Total		1	1	2	2	2	2	5	2	7	0	3	4	5	5	2	0	2	2	2	49

Note: ○ Approved and On-going
△ Launched

Figure 1-1

PROBE MISSION STRATEGY

5/20/74

	CY 79	80	81	82	83	84	85	86	87	88	89
OCTOBER PLAN											
79 PIO JUP/URANUS F.B. (U PROBE)	L 11/79		J 2/81			U 7/84					
80 PIO SATURN DIRECT		L 12/80				S 1/84					
81 PIO SAT/URANUS F.B. (U PROBE)			L 12/81				S 1/85				U 11/88
CURRENT PLAN											
79 MAR JUP/URANUS (U PROBE)	L 10/79		J 7/81					U 11/86			
80 PIO JUP ORB W/PROBE		L 11/80			J 4/83						
80 PIO SATURN PROBE		L 12/80				S 1/84					
82 PIO SATURN/TITAN PROBE				L 12/82				T 2/86			

Figure 1-2

probes were successful, this probe would then go into Titan. If either the Saturn or Uranus probe was a failure, then this probe would repeat either the Saturn or the Uranus mission.

The scenario had a couple of weaknesses in it, the major one of which was exposed at the Titan workshop held here at Ames about a year or so ago. The strong advice of that workshop, which we have accepted, was we should not try to achieve commonality between a Titan probe and an outer planet high-atmosphere probe; the reasons being that the science to be performed at Titan would be different and, also, that the quarantine restraints to be imposed on a Titan probe would differ from the outer planets probe.

In this old plan (Figure 1-2) you don't see a Jupiter entry because until the Pioneer 10 encounter our entry analysis of the Jupiter probe mission, indicated that facilities would not be available until about 1980 to test an entry probe to the Jupiter entry heating conditions. Hence, we deferred a Jupiter entry probe until the mid-1980's. That thinking has changed and that is going to be a major issue of this workshop.

Let me go to this next schedule (Figure 1-3), and show you our current thinking. For the October mission model we were given a fiscal constraint by the Administrator to formulate all of the new programs we hoped to implement for the next five years. The original mission model was in consonance with that fiscal constraint. However, late last year several things happened, one of which was a forecast overrun in the Viking program.

Since our overall budget does not increase, funds for planning for new missions is from the same funding that has to accommodate overruns. We, therefore, had to alter our thinking and decide which missions we wanted to do as scheduled and which missions would have to be deferred. Since the outer planet probe missions could be done almost in any year - the opportunities to the outer planets occur in about a twelve-or fifteen-month period - these were more easily deferrable than some of our other missions.

PLANETARY EXPLORATION PROGRAMS

April 1974

PAYLOAD CY	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91
APPROVED PROGRAMS																			
MARINER VENUS/MERCURY	1																		
PIONEER JUPITER FLYBY	1																		
HELIOS		1		1															
VIKING '75			2																
MARINER JUP/SAT '77					2														
INNER PLANETS																			
VIKING ORBITER/LANDER							1												
VIKING ORBITER/LANDER (W/ROVER)									1										
SURFACE SAMPLE RETURN												2							
SATELLITE SAMPLE RETURN																		1	1
PIONEER VENUS						2													
INNER PLANET FOLLOW-ON								1	2		1			1					
VENUS RADAR MAPPER											2								
VENUS BUOYANT STATION													2						
MERCURY ORBITER															2				
VENUS LARGE LANDER																	2		
OUTER PLANETS																			
MARINER JUP/URANUS FLYBY (U PROBE)							2												
PIONEER JUPITER ORBITER (ESRO)								1											
PIONEER SATURN PROBE								2											
PIONEER SATURN/TITAN PROBE										2									
MARINER JUPITER ORBITER									2										
PIONEER JUPITER PROBE												2							
MARINER SATURN ORBITER													2						
MARINER URANUS/NEPTUNE FLYBY														2					
JUPITER SATELLITE ORB/LANDER																		1	1
COMETS AND ASTEROIDS																			
ENCKE SLOW FLYBY							1												
ENCKE RENDEZVOUS									2										
HALLEY FLYBY													1						
ASTEROID RENDEZVOUS														2					

Figure 1-3

Consequently, when we formulated that mission model, the dedicated Pioneer outer planet probe missions were deferred. As I indicated before, our thinking changed about commonality between an outer planet entry probe and the Titan entry probe and, also with Pioneer 10 encounter and Arv Kliore's data about the possibility that the probe design for Saturn and Uranus would also have Jupiter capability. Since ephemeris uncertainty of Jupiter has been decreased which allows a shallow entry angle, and if the atmosphere is more toward the so-called "warm expanded" or "nominal" atmosphere, it may be possible to enter Jupiter with the same entry technology that we will use for Saturn and Uranus.

So, for several reasons, our thinking has changed. We have given up the dedicated Pioneer-Uranus entry probe. Instead, our current thinking is to incorporate a Uranus probe in a Mariner Jupiter-Uranus mission which we want to launch in 1979. As far as a Jupiter entry probe is concerned, we are discussing a cooperative program with ESRO at the present time, using Pioneer H to do an orbiter mission in the 1980 opportunity and we are going to discuss the possibility and the advisability of incorporating a Jupiter entry probe in that mission.

Our dedicated Pioneer-Saturn probes are still intact. That thinking has not changed but now you see Pioneer-Saturn-Titan probes. These would be a different kind of a probe. They would be dedicated Titan entry missions. The Pioneer-Jupiter probes is still kept on the books at the old date in case we cannot incorporate the probe into the Pioneer Jupiter orbiter mission with ESRO.

These are some concepts and some of the things that we are considering. The only way the concept of a probe on the MJU flyby is feasible is to first aim the spacecraft so that it would impact Uranus and then release the probe. The probe then need not have an attitude control system or delta-V propulsion, and after the probe is released, the spacecraft is deflected to achieve the flyby. This mode permits use of a simple, "dumb," probe that can be developed within reasonable cost and weight constraints. However, the spacecraft deflection mode requires a new NASA policy position on the quarantine requirements for outer planet entry probes. This is being considered by the Space Science Board. This issue must be addressed since this is the only practical mode to incorporate a probe on a Mariner spacecraft to Uranus.

Figure 1-4 presents a concept of a dedicated Pioneer probe mission into Saturn. Again, the concept for probe release would be the same. The spacecraft, of course, serves as a communications relay for the probe during the entry of the probe into the atmosphere. One of the things that is being studied is the feasibility of designing one probe system which can be completely common, including science for both Saturn and Uranus.

A cooperative Jupiter mission with ESRO that I mentioned, and the possibility of a probe in that is shown here on Figure 1-5. The probe would be released before orbit capture and the spacecraft would serve as a relay for the probe during entry. Then the spacecraft would be captured and would achieve a highly elliptical orbit about the planet. The first formal meeting with ESRO on this mission is here at Ames on June 17 and 18.

Now, let me tell you one announcement that I think will be of interest to some people here. The Mariner Jupiter-Uranus Science Group that has been meeting is coming up with a strong position that an atmospheric entry probe will materially enhance the value of that mission. On the basis of a meeting last week, we at NASA decided that we would go out with an RFP to industry for a Phase B Study in fiscal year 1976 for an entry probe that can be used for Uranus, Saturn and, if possible, Jupiter. The RFP will be entitled, "Outer Planet Probes." The RFP will also state that the first mission for this outer planet probe family will be the MJU mission in 1979. Preceding the release of that RFP, Dr. Rasool is going to form a small science group to evaluate

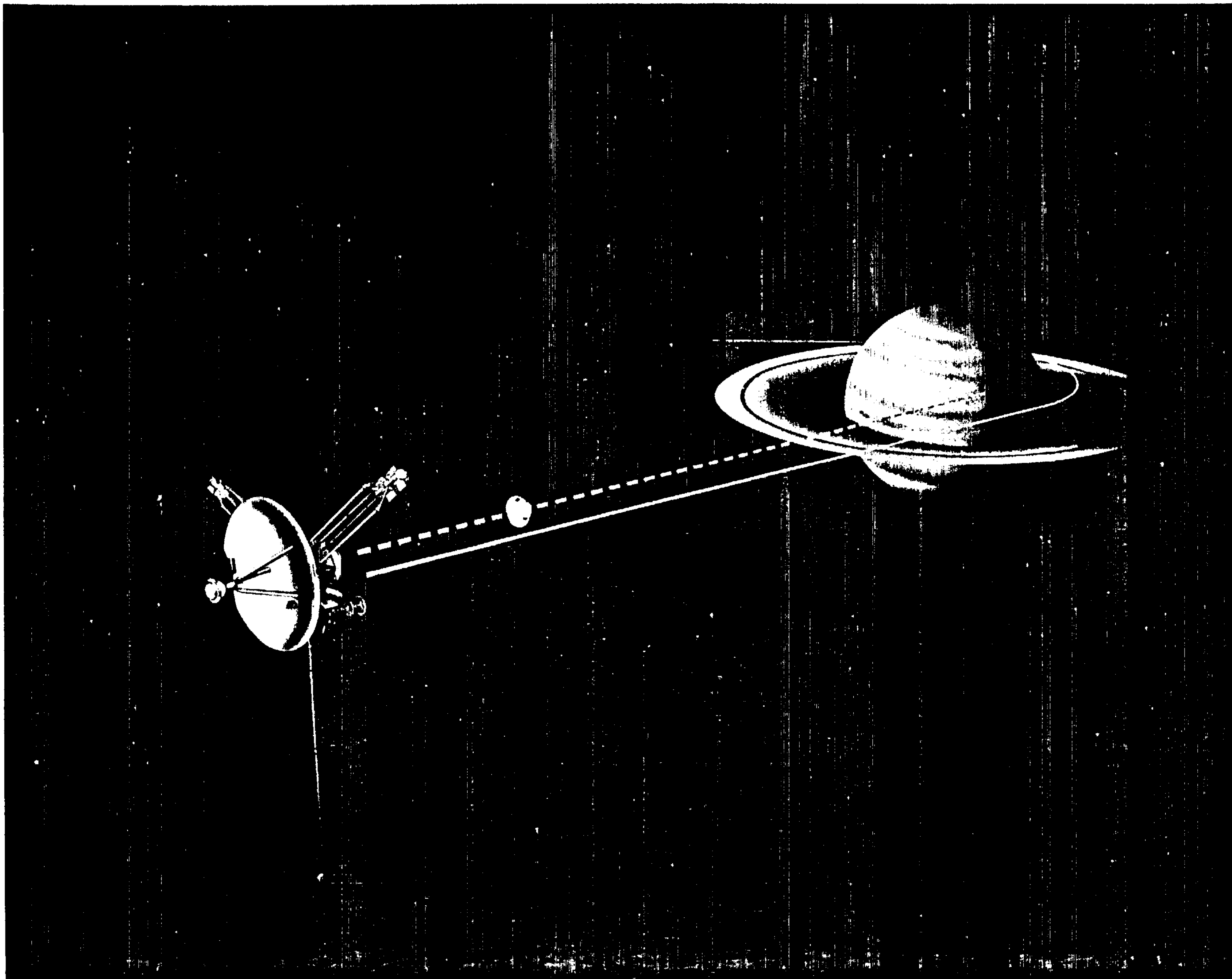


Figure 1-4. Pioneer Saturn Probe Mission

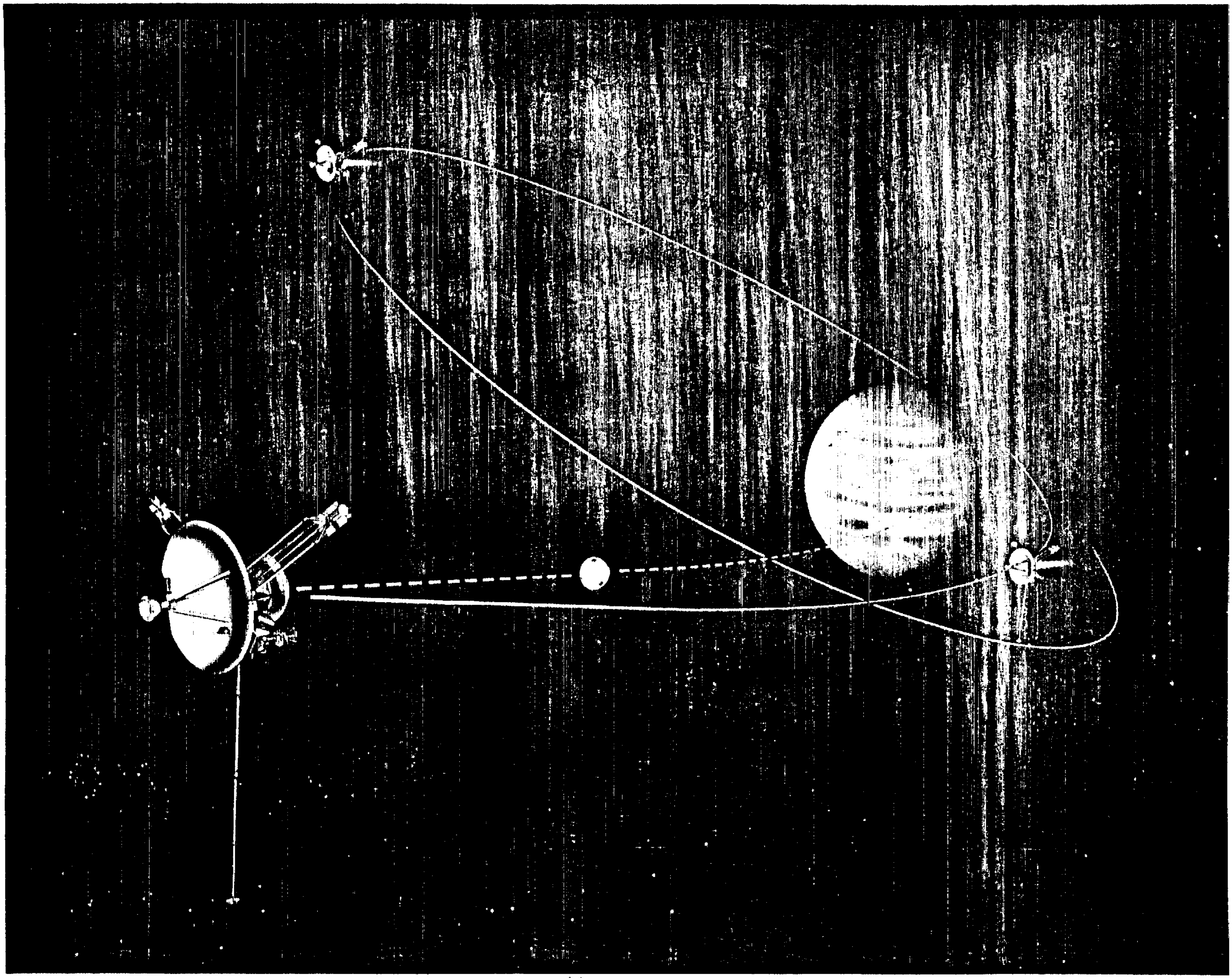


Figure 1-5. Pioneer Jupiter Orbiter with Probe

the payload that should be incorporated in the probe and this will serve as a guideline for the Phase B contractors.

Our current thinking is that this RFP, which would be competitive, would be released about July of next year and the procurement procedure would be similar to Pioneer-Venus. It would be open competition with two contractors selected to conduct a competitive Phase B and only the winners of the Phase B allowed to compete for the execution phase.

SESSION II. SCIENCE RATIONALE AND OBJECTIVES

Dr. Ichtiaque Rasool, Chairman

MR. SEIFF: I think everybody knows Ichtiaque Rasool who is the Deputy Director in the Planetary Programs Office in OSS. Prior to that he was working at Goddard and at the Goddard Institute for Space Studies. He has been of great service to the planetary programs at some professional sacrifice to himself because he has had to give up some of his scientific work in order to help advance the programmatic aspects of these projects. Dr. Rasool has kindly agreed to serve as chairman of this session.

DR. ICHTIAQUE RASOOL: Thank you

Now we come to the most important part of the session. As you know, the planetary program is having great success at the moment; technology wise and science wise, we have done very well.

Last week I was asked by my boss, John Naugle, "Why?" Why are we having such great success? It is very interesting that when we have a failure, we have an inquiry; and when we are having success, we still have an inquiry. But it is an interesting question, why our program, compared to many other programs in other countries, has had great success in the ten years NASA has been in the planetary business.

I have reflected on that quite a bit in the last few days and I think very firmly that the main reason has been the strong base of supporting planetary technology and advance planning. We go through a great deal of research and technology development and we do very careful planning. We go through a great amount of technical development and technical studies. A very important thing is that we have conducted science and technology studies together. I think this mix is extremely important. We design our missions to answer specific questions. This, in the next ten or fifteen years, is going to be very important because now we are entering the second generation of planetary exploration. The first generation was to go and find out what is there and now we know a little bit of what is there on the terrestrial planets, and through very powerful telescopes we have been looking at the outer planets.

Once we know what is there, then the question is why is it there and what does it mean in terms of the history of the solar system? So our major objective is that we would like to understand the processes which took place in the early history of the solar system, what is the history of the Earth and what may we estimate to be the future of the Earth. Those are the specific questions and it is to those questions that our spacecraft design and mission design should be geared to answer. That is the interaction of science and technology. That is what we have been doing and in my opinion that is why our program has been scientifically very productive.

It's very appropriate then that our first session be a definition of science. We have six or seven speakers who will start with a general discussion of what we know about the outer planets. In this last ten or fifteen years we have concentrated on the inner planets and we have used flybys and orbiters. The next decade will be the outer-planet era, hopefully, and there the emphasis will be on flybys, orbiters and probes. As you know, the structure of the outer planets is very different from the inner planets and, therefore, it is very important that we begin this historic meeting - which I think is a very good way to kickoff the 1980's at which time probe technology will be the word of the day - by trying to find out what is there, why are we going there, what do we expect to learn, and what measurements do we need to make.

The first paper is a general review of what we know about the outer planets by Toby Owen. I have asked him to include Titan in his paper because he has become very interested in Titan in the past few months.

NEW IR OBSERVATIONS OF TITAN AND POTENTIAL OF IN-SITU ATMOSPHERIC ANALYSIS OF THE OUTER PLANETS

Dr. T. Owen

State University of New York

DR. TOBY OWEN: The main message I have to offer today is that we really need outer-planet probes. What I will describe is not so much what we know about the outer planets but a number of very confusing problems which we are uncovering at a remarkable rate, thanks to the successes that Ichtiaque has already recounted. It is all very well to have all this success with probes, and so on, but we are lagging a little in terms of understanding the significance of the results.

A. JUPITER

In particular, let me begin with a brief discussion of Jupiter. There are going to be other people this morning talking about the Pioneer 10 atmosphere results. These are extremely interesting and, at the present time, very difficult to reconcile with the other information that we have built up over a period of years on the structure and composition of the atmosphere.

Let me try, first, to review the previous work very briefly. Figure 2-1 is a reproduction of a plot made quite some time ago to show the abundances of various gases in the atmosphere of Jupiter as functions of the temperature or its equivalent, the depth in the atmosphere (Owen, 1969). In those days we thought that we could explain things pretty well by simply assuming solar abundances. In fact, that seemed to fit the infrared spectroscopic data very nicely: an adiabatic lapse rate terminating somewhere near a temperature of 225° at some kind of cloud layer in the Jovian atmosphere. At somewhat lower temperatures, i.e. higher up, another cloud layer existed in the region where ammonia condensed. So, the picture at that point was that when we look at Jupiter in the near infrared, we are looking through this ammonia haze layer

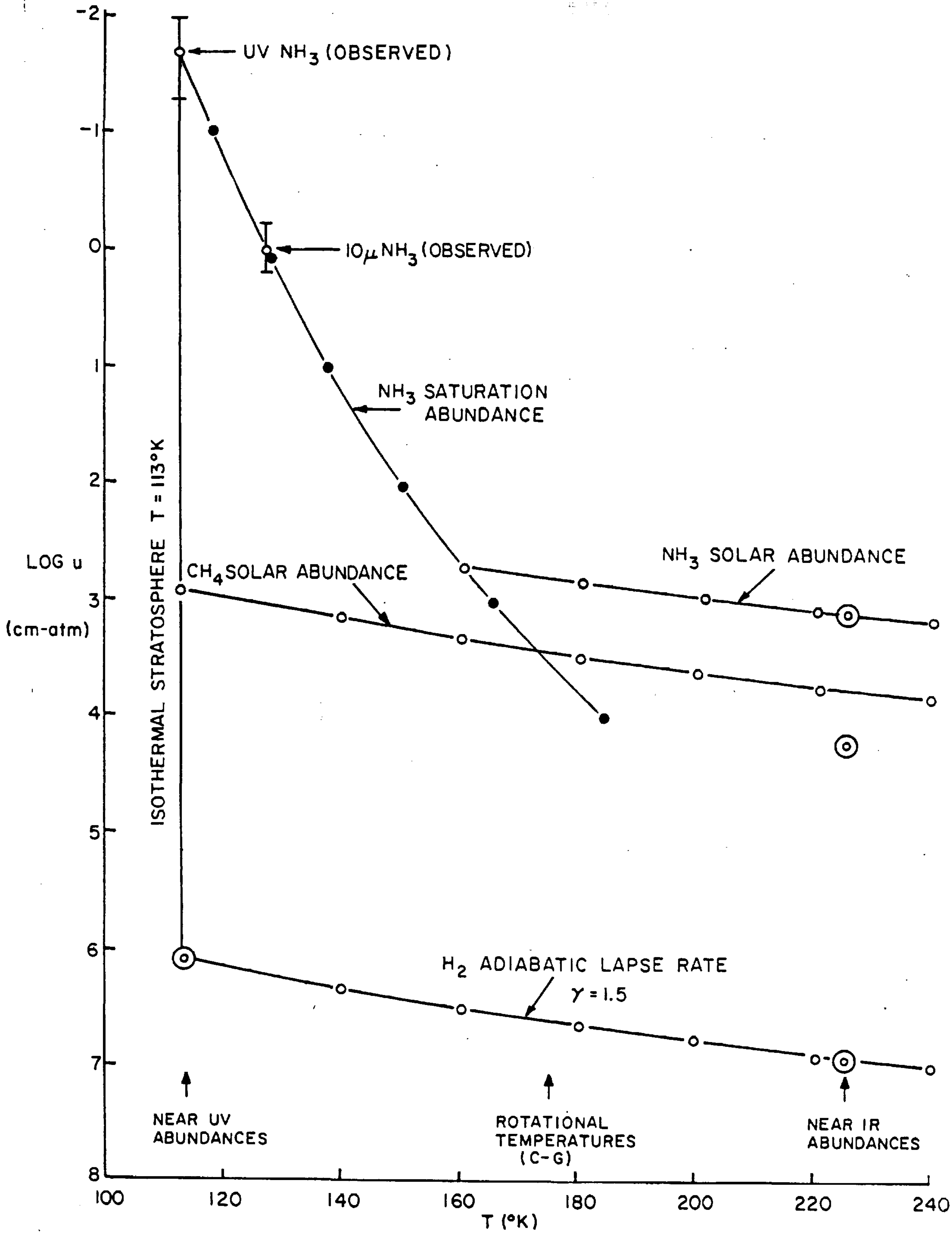


FIGURE 2-1 - Jovian Gas Abundances

down to a thick lower cloud whose upper boundary is at about 225°. In other words, we have two cloud layers and the kinds of temperatures that were deduced, either from analyzing methane molecular bands or using the ten-micron mean temperature or the ultraviolet temperature determined by the saturation vapor pressure of ammonia, all seemed to fit together very nicely with this picture (c.f. Figure 2-1).

One can combine these results very schematically into a kind of standard atmosphere plot for Jupiter, showing pressure versus temperature, again, assuming an adiabatic lapse rate, and adding the ten-centimeter radio emission which corresponds to a temperature of 300° Kelvin while at twenty one centimeters the thermal emission seems to be something on the order of 400° Kelvin. These points correspond to high pressure levels in the Jovian atmosphere (Figure 2-2 Owen, 1974).

All of these data seemed to fit together very nicely until Pioneer 10 went past Jupiter. What we then learned from the occultation was that the atmosphere was much hotter at higher levels than any of the previous data we had accumulated would have indicated (Kliore et al. 1974). So that, whereas, at a pressure of one atmosphere, we had deduced temperatures on the order of 150° or 180° Kelvin, the Pioneer 10 data seemed to indicate temperatures close to 400° Kelvin.

Now, how do you reconcile these two sets of data? As far as I know, there is no reconciliation, yet that really fits everything together; that can explain how the spectroscopic data and the Pioneer 10 data can be brought into agreement with each other.

The additional point I wanted to make this morning is that it isn't just the spectroscopy that one has to worry about.

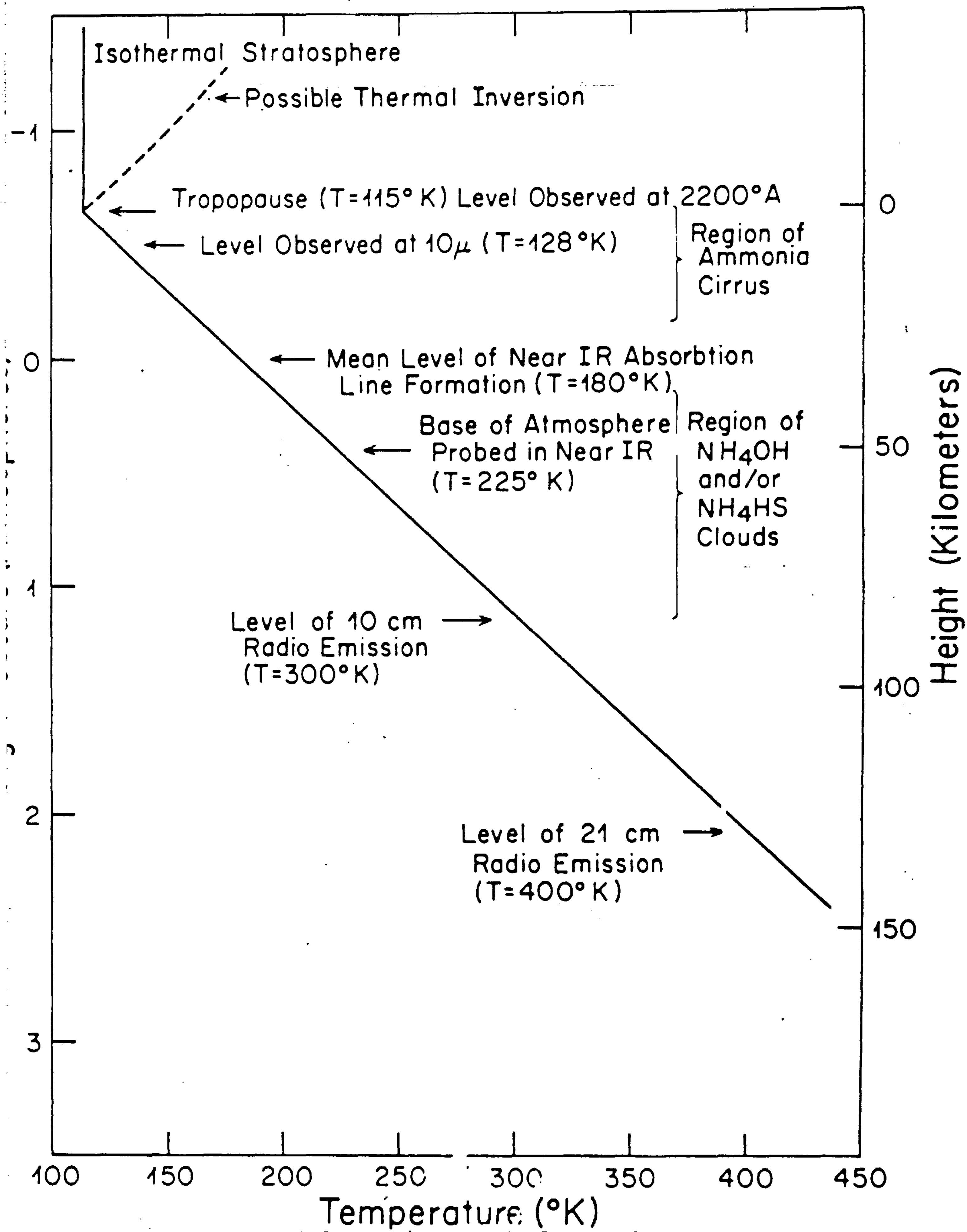
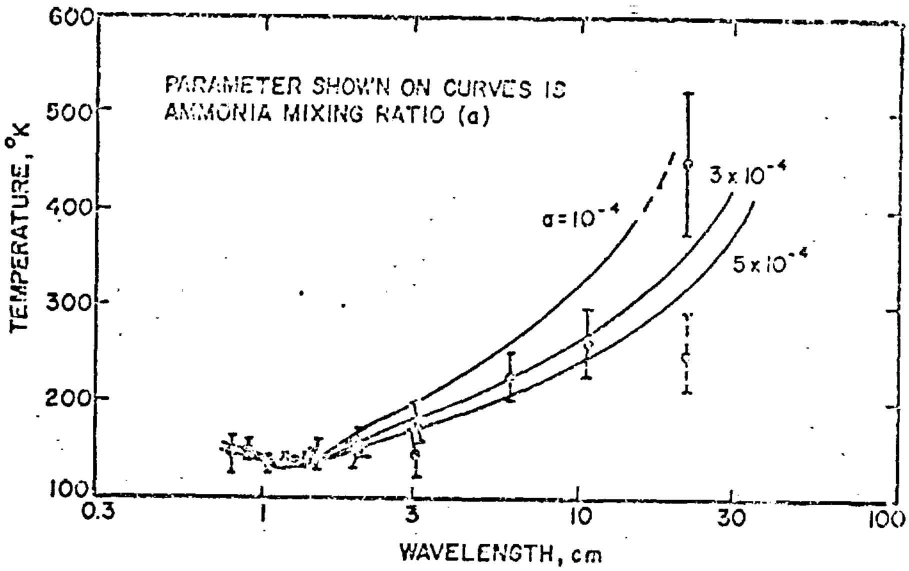


FIGURE 2-2 - Jovian Standard Atmosphere Plot

If that were the only problem, perhaps one could postulate some incredible confusion caused by scattering in cloud layers, although that is rather difficult to work out in any quantitative way that is convincing. There is an additional data set that must be dealt with, viz., the radio results. A plot for some model atmospheres developed by Gulkis and Poynter (1972) is given in Figure 2-3. Here temperature is plotted against wavelength and the parameter "a" is the ammonia-hydrogen mixing ratio. A solar value for this ratio would be between the upper two lines ($a \sim 1.5 \times 10^{-4}$) and that value seems to fit the data pretty well. Gulkis and Poynter concluded that Jupiter exhibits solar abundances, which was the same result one derived from the infrared spectroscopy. With a rather simple model atmosphere, using the hydrostatic equation, assuming the gases were mixed, one could fit the observational data in the radio range. The Pioneer 10 occultation data were obtained at a wavelength of about 12 cm where the ground-based radio measurements appear to correspond to a temperature of about 400°K at a pressure of about 10 atmospheres. It may be that the reason for the disagreement again lies in the model atmospheres that are used to interpret the ground-based data, but now scattering by clouds cannot be the culprit. Clearly much more work is needed in order to achieve an understanding of the relation between pressure and temperature in the Jovian atmosphere.

The other exciting thing that has happened recently in observations of Jupiter has been the discovery of trace constituents, namely ethane and acetylene and, most recently, phosphine in the ten-micron region of the spectrum (Ridgway, 1974 a, b). The reason this is exciting is that these constituents would not be predicted on the basis of simple thermodynamic equilibrium in the planet's atmosphere. They must be caused by some kind of photochemical effects in the upper



The radio spectrum of Jupiter, corrected for non-thermal emission. The lines shown correspond to fluxes predicted by model atmospheres with various ammonia-hydrogen mixing ratios. Note the rise in temperature with increasing wavelength (Gulkis and Poynter, 1972).

Figure 2-3 - Jovian Radio Spectrum

atmosphere and such effects have been suggested for many years as being responsible for the production of the chromophores, the material that colors the clouds on Jupiter. Ethane and acetylene have frequently been suggested as precursors for these more complicated organic polymers if, indeed, organic polymers are the responsible coloring agents. One has to be a little cautious here because there are other alternatives. There are polysulfides that could cause some of the coloration and I would like to remind you of a suggestion made by Rupert Wildt (1939) many years ago that solutions of metallic sodium in ammonia at the (pre-Pioneer 10) temperatures expected in the upper atmosphere of Jupiter, might be brown, red, or blue depending on the concentration of the solution, the temperature, and the amount of other trace metals. The reason for returning to this suggestion is that lately it has been discovered that there is a sodium cloud in the vicinity of Jupiter, apparently associated with the Satellite Io (Brown, 1974). This cloud provides a source for the sodium, thus removing an objection that has been voiced in the past to Wildt's suggestion. There are other difficulties but, again, the point I want to make is we are just beginning to uncover some of the clues to these chromophores which promise to be some of the most interesting chemical substances in the Jovian atmosphere. This is an example of a basic problem that will probably require the use of direct probes for its resolution, and that is not going to be a very easy thing to do either.

B. SATURN

A low resolution spectrum of Saturn was recently obtained by Gillett and Stein (1974) in the spectral region 7-13μm. Once again there are intriguing indications of non-equilibrium products in the planet's atmosphere. Phosphine is indicated, and the big hump at 13μm may well be due to ethane. There is no high-resolution spectroscopy in this region yet but the general shape of the spectrum is certainly similar to the spectrum of Jupiter where, in fact, some of these identifications have been made. We should get some much better observations of Saturn from the ground in the next couple of years. At least the identifications of these substances should become fixed. To determine how they relate to the chemistry in the atmosphere will probably again require the use of probes.

Now, the other piece of news about Saturn that I have is that Therese Encrenaz, Jerry Woodman and I have found, again, the elusive ammonia absorption around 6450 angstroms which was, I think, discovered for the first time by Larry Giver and Hyron Spinrad (1966). It definitely seems to be present but the amount of ammonia we find is very much less than the amount present on Jupiter, even though the hydrogen and methane abundances in the atmospheres of the two planets are roughly identical. We interpret this as an indication that, whereas on Jupiter one can see beneath the ammonia cirrus clouds down to the region where the ammonia and the other gases are mixed, on Saturn that does not happen and, so, the ammonia abundance is fixed by the local saturation vapor pressure. This, in turn, will depend on the local temperature so that fluctuations in cloud density and cloud height could easily lead to the variations in the ammonia abundance which have been reported.

C. TITAN

For the last three years, Titan has seemed to be some kind of perverse machine that's been put into orbit around Saturn by a superior race as a kind of intelligence test for earthlings, to see if they can unravel what's going on out there. So far, I have to report that we haven't done very well. The basic problem that has aroused so much interest is that the temperature of Titan at 13μm is much higher than one would have expected for a small satellite with a rather thin atmosphere at that distance from the sun. On the other hand, at somewhat longer wavelengths one finds the low temperatures that one would have anticipated. How does one reconcile these two sets of measurements? There have been two basically differing interpretations of this. One is based on a hydrogen greenhouse effect which suggests that light is getting down to the surface of the satellite, warming it up and then the resulting infrared radiation is being blocked at the longer wavelengths by large amounts of hydrogen in the satellite's atmosphere.

This view seeks support from the detection of hydrogen by Larry Trafton (1972a) in the 8200 angstroms region of the spectrum of Titan. The kind of greenhouse that results depends on various assumptions for the atmospheric composition. Jim Pollack, who's also here at Ames, has developed a series of models and concluded that the best of these corresponds to a surface temperature of 155° Kelvin (Pollack, 1973). Carl Sagan has gone to the extreme of suggesting temperatures in excess of 200° Kelvin and has stressed the possible biological importance of Titan (Sagan, 1973).

Sagan's extreme greenhouse models, I think, are ruled out on the **basis** both of thermal measurements at five microns, and microwave measurements which correspond to radiation from the surface of Titan and indicate temperatures below 175° Kelvin (Briggs 1974).

Unfortunately, the true surface temperature is still unknown because the microwave measurements have a very large uncertainty. An alternative explanation for the high temperatures on Titan involves the presence of a dust layer, a kind of thin, high cloud in the atmosphere which is absorbing a lot of radiation in the ultraviolet, warming the upper atmosphere and leading to reradiation by the gases at that level. Once again, we expect that methane emission is present at 7 - 8μm and ethane is in emission at 13μm. With this model, proposed by Bob Danielson and his colleagues at Princeton, one can have rather low surface temperatures (Danielson et al, 1973).

Roger Knacke, Dick Joyce and I made some measurements at KPNO this last winter to try to distinguish between these two basic alternatives. Last year we tried and failed to detect the flux from Titan at five microns (Joyce et al, 1972). We chose five microns because we know that in the atmospheres of Saturn and Jupiter this is a "window" region in the spectrum. In other words, the principal atmospheric gases do not absorb at this wavelength and one has the chance of looking very deep into the atmosphere, possibly to the surface of Titan itself. We did not detect any radiation on this earlier measurement but last winter we did (Figure 2-4, Knacke et al, 1974). If one assumes that the radiation is reflected sunlight then the curve sloping down from the left represents the flux expected from a perfect reflector at Titan's distance from the Sun. So the fact that the Titan flux is far below this curve indicates a very low reflectivity at five microns, about 7 percent. In other words, Titan is very black there. Alternatively, if what we are really seeing is thermal radiation, and not reflected sunlight, then we can look at the family of black body curves sloping up toward the right and we

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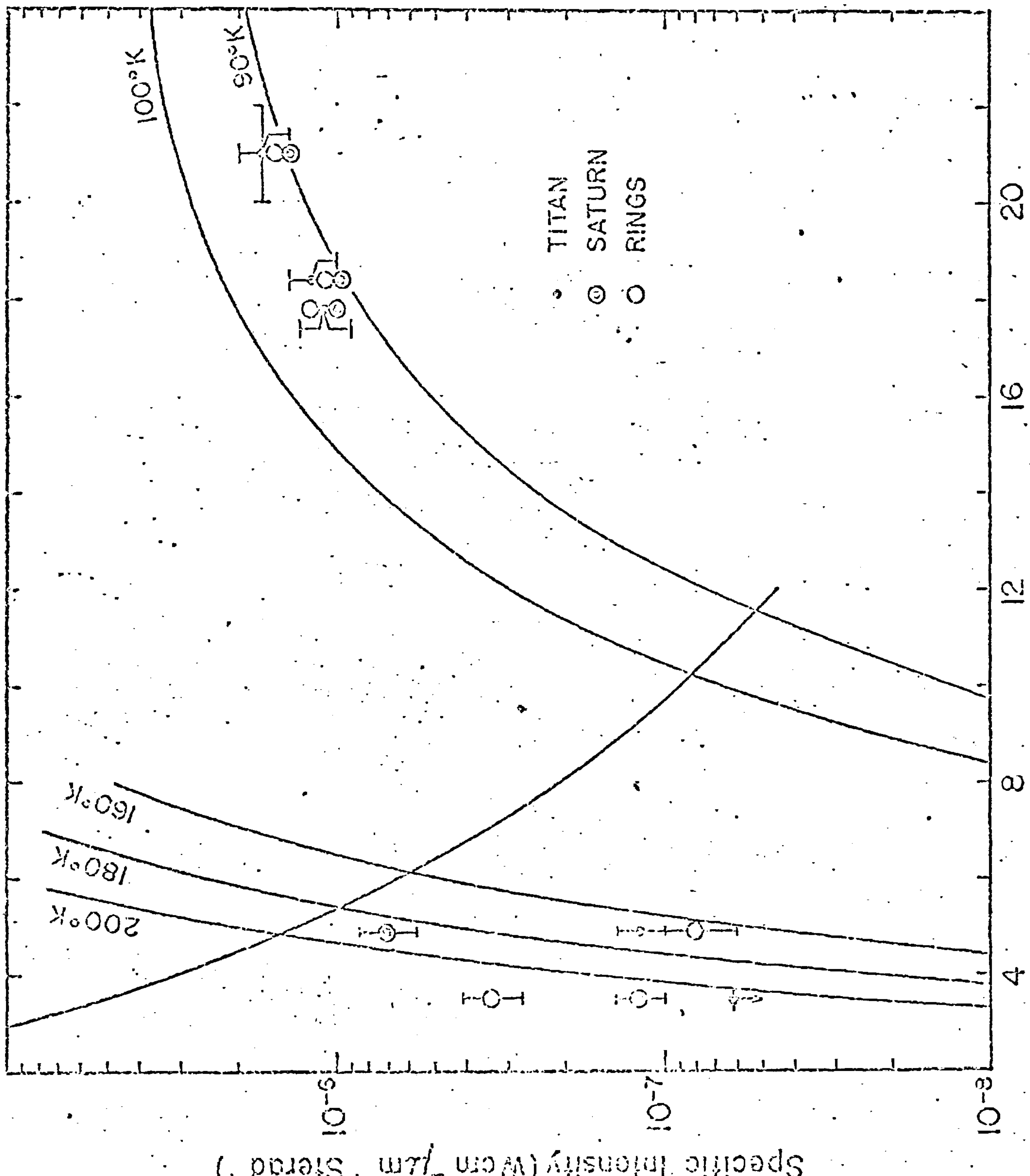


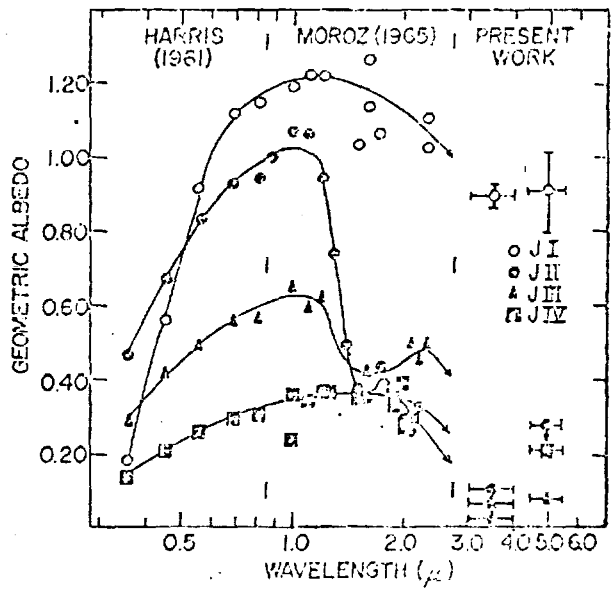
FIGURE 2-4 - Radiation Observations of Saturn
and Titan, 1974

can conclude that the temperature must be less than about 170° Kelvin.

This has some interesting implications for what the satellite's surface may be covered with, if we are seeing the surface; or what the clouds are made of, if what we are seeing is clouds. To explore this further, we can compare Titan with the satellites of Jupiter. Reflectivities of the Galilean satellites are shown in Figure 2-5 (Gillett et al, 1970). It is apparent that the geometric albedo (reflectivity at zero phase) at five microns is rather different for the different bodies. In fact, most of them are poor reflectors and J-III, in particular, has a very low albedo. It approaches the value of Titan. On the other hand, J-I, Io, which is intriguing in so many ways, has a very high reflectivity in this region. In fact, it's very close to a perfect reflector, despite the fact that it is an exceedingly poor reflector in the ultraviolet. Both Io and Titan are extremely red objects. Their surfaces must be covered with something very different from the surfaces of these other satellites or, indeed from any other satellites in the solar system. But, at five microns, their "colors" are not at all similar. That suggests that the red material on the two satellites may be of two different types.

We also have observations of Saturn's rings at five microns and they are even darker than Titan or the Jupiter satellites (Figure 2-4). That we would expect, because we know that there is ice present in the rings of Saturn and ice is a very poor reflector at five microns.

We can examine laboratory spectra of many substances to see how they behave at five microns. A catalogue of such spectra has recently been published by Kieffer and Smythe (1974), and it is easy to rule out some substances as major contributors to the reflectivity of Titan. For example, methane has a very high reflectivity so a thick methane cloud on Titan or a methane frost on its surface won't work. Similarly, covering the surface entirely with H_2S or NH_3 in a frozen state won't satisfy the data.



Geometric albedos (reflectivities) of the Galilean satellites (JI-JIV) as a function of wavelength. Smooth curves are qualitative, but note that all are plotted on the same scale. The high reflectivity of Io (JI) beyond 0.7 μ and its red color (cp. Titan in Figure 1) are especially remarkable. Data for 0.35-0.85 μ are from Harris (1961), for 0.85-2.5 μ from Moroz (1967) and for 3-5.4 μ (labelled 'present work') from Gillett *et al.* (1970).

FIGURE 2-5 - Reflectivities of the Galilean Satellites

On the other hand, NH_4SH is a possible candidate. As mentioned above. Water ice is too dark at 5μm for Titan; something else is needed to brighten it up or perhaps it only covers part of the surface. Most silicates, of course, are rather dark at five microns, too. The possibilities are limitless. You can't do diagnostic compositional analysis on the basis of data like this. It's just interesting that you can exclude a few things.

Now we get into more exotic problems, like what is the red material in the atmosphere - or on the surface? This problem relates to the remarks about the chemistry on Jupiter. We are very interested in the organic chemistry that is taking place in atmospheres like these because of its obvious relation to ideas about what happened on the early Earth prior to the development of life.

Khare and Sagan (1973) have produced a reddish-brown polymer by ultraviolet irradiation of a mixture of hydrogen sulfide, methane and ammonia - all gases we expect to be present in the lower atmosphere of Titan. This doesn't seem to be a very good candidate for the coloring agent on Titan, if it is the only substance present, since it is quite transparent at five microns. On the other hand, a mixture of this material and water ice might reproduce the observations quite well. Tom Scattergood, Peter Lesser and I have also produced colored polymers by using proton irradiation of this same mixture of gases (Scattergood et al, 1974). We found one substance with a rather strong absorption in the five-micron region, which was not present when H_2S was not used in the mixture. So, even starting with the same constituents you can produce different materials if you use slightly different excitation sources. Once again, this is not the ideal way to figure out what the stuff is that's coloring these objects. One can only eliminate some alternatives. This is a prime example of the kind of thing one would love to be able to investigate with a suitably-equipped probe.

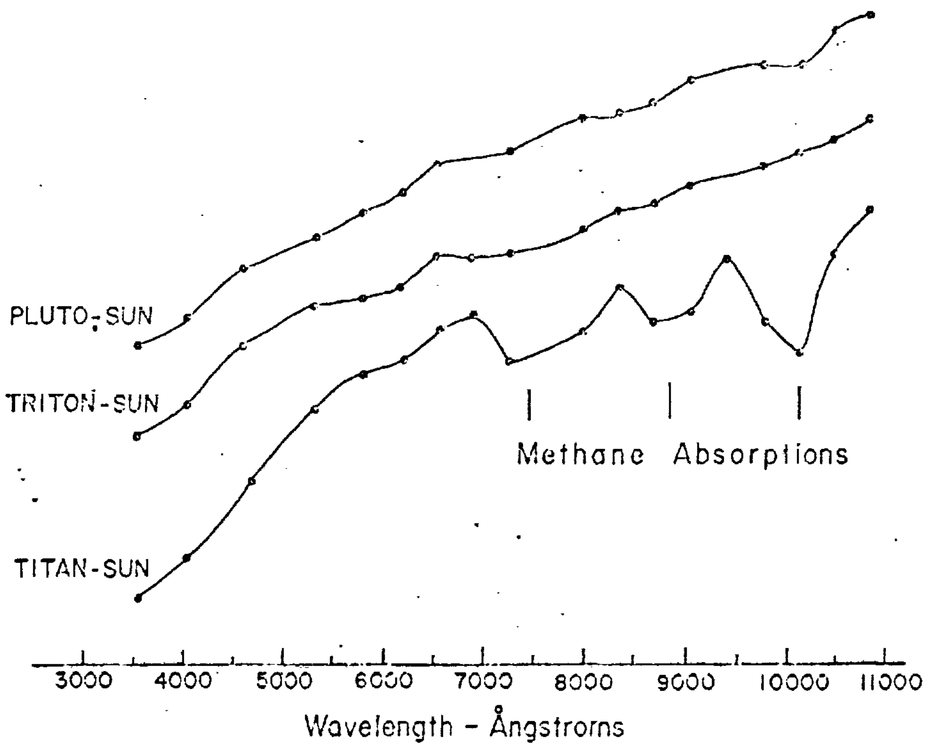
Now, a word about atmospheric models. A family of hydrogen greenhouse models for Titan has been developed by Pollack (1973). In his plots of wavelength against brightness temperature, a decrease in the brightness temperature near 16.7μm is predicted on the basis of the absorptivity of hydrogen. We have measured a point in the wing of this absorption (c.f. Figure 2-4) and we do not see any indication of this dip. Low and Rieke (1974) have obtained essentially an identical result. The absence of any indication of hydrogen absorption argues against a thick hydrogen greenhouse, if the atmosphere is completely clear (no clouds).

Carl Sagan has stressed that a lot of hydrogen could be hidden underneath a thick layer of clouds but, as we have seen, these clouds, if present, must be very thick and very dark at five microns.

The alternative model suggested by Danielson et al (1973) predicts that the flux should be rising toward wavelengths greater than 20μm because they are summing the contributions from the high-altitude dust layer and the surface. Now, in fact, we see a slight decline and a rather flat spectrum in this region, in mild disagreement with this particular model. Slight changes in the two emissivities and the temperatures might reconcile the predictions with the observations. We are not really in a position to make a definite statement in this case. This is the same conclusion reached by Low and Rieke (1974) who suggest that, perhaps, the answer is that Titan simply has a methane atmosphere with little, if any, hydrogen and a small methane-induced greenhouse effect. Titan may thus be a much less fantastic place than it seemed just last year.

D. PLUTO AND TRITON

Figure 2-6 shows some data in the 3,000-to-11,000-angstrom region; very low resolution spectroscopy obtained with the 200-inch telescope and a multichannel spectrometer just to see if there's any indication of methane absorption, i.e., atmospheres



Low-resolution spectrophotometry of Titan, Triton and Pluto (200-in. telescope plus Oke multichannel spectrophotometer). Note the absence of methane on Pluto and Triton and the redder color (steeper slope at short wavelengths) of Titan. (The vertical scale is displaced for each object; these are only *relative* observations.)

FIGURE 2-6 - Low Resolution Spectrophotometry of Titan, Triton and Pluto

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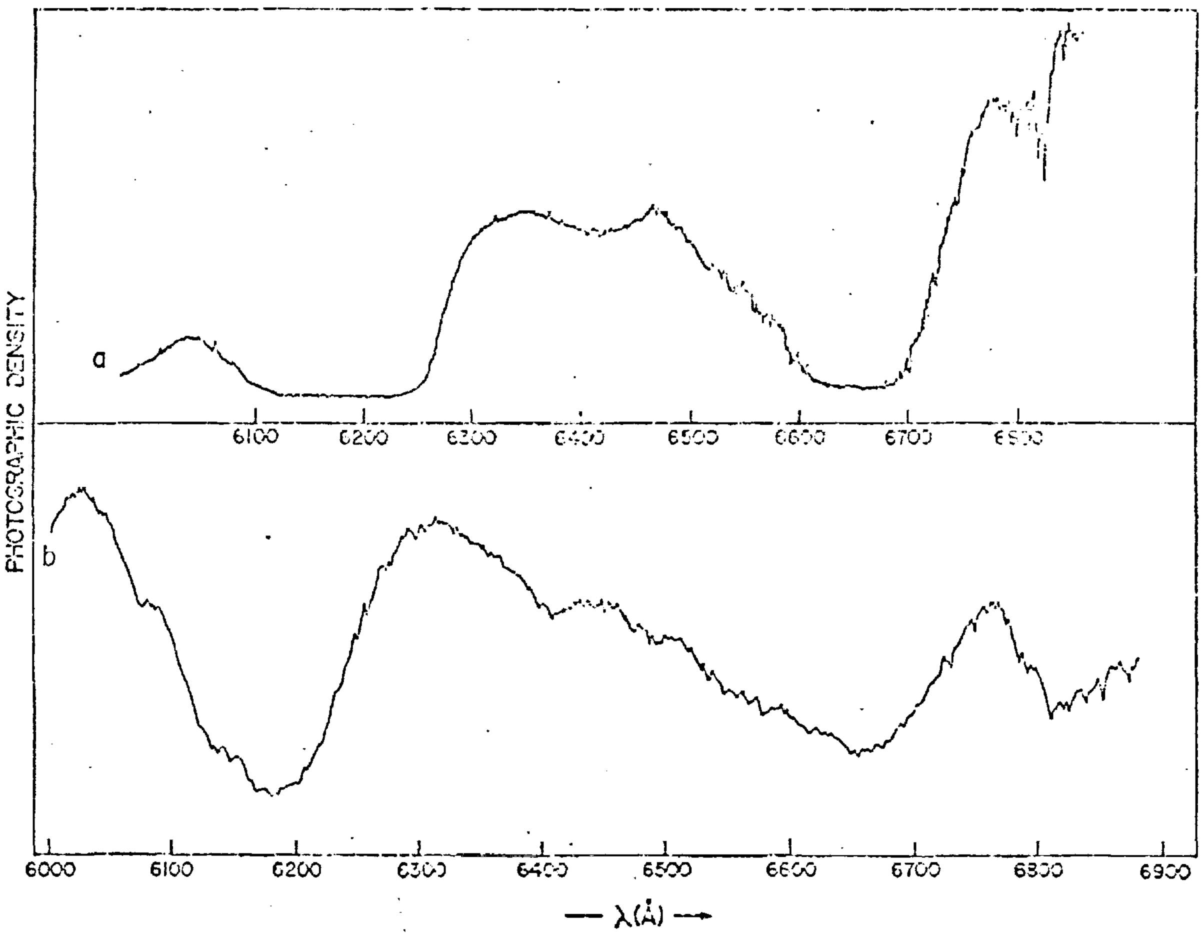
on Triton and Pluto. Titan is shown for comparison. We don't see any absorptions at this kind of resolution with the data available thus far. These two objects would have to have some kind of greenhouse effect in order to get the temperatures up high enough to maintain methane atmospheres, and it appears that unlike Titan, they do not exhibit this phenomenon.

E. URANUS AND NEPTUNE

Even though Uranus and Neptune are very far from the Sun, radio observations at longer and longer wavelengths indicate higher and higher temperatures just as in the case of Jupiter and Saturn and so we should not, a priori, exclude the possibility that these planets have some interesting chemistry going on in their lower atmospheres in spite of their remoteness. This increases their attractiveness as targets for atmospheric probes.

The atmospheres of these two planets are very different from those of Jupiter and Saturn, and this difference has been emphasized by some new results that we obtained just last summer. What we found is that if we take the spectrum of Uranus after dividing out the solar spectrum and try to match the atmospheric absorptions with laboratory spectra of different amounts of methange, we can't do it with the pathlengths that are available to us (Figure 2-7). The maximum attained in the laboratory by Dr. D. A. Ramsay of the Canadian NRC is a five kilometer path at a pressure of two atmospheres, so the total amount of methane is ten kilometer amagats. There must be more methane than that in the optical path into and out of the atmosphere of Uranus (Owen et al, 1974). This was quite a surprise to us because we had looked at some weak bands in the spectra of these planets at longer wavelengths some time ago and thought that we had about the right amount of methane (Owen, 1967). These new results indicate that the methane-hydrogen mixing ratio on these two planets is very much higher than it is on Jupiter and Saturn; not just by a factor of ten as we had thought before.



-(*a*) Density tracing of the 10.1 km amagat spectrum of methane, without the wavelength comparison lines, taken with the 33-m NRCC White cell. The rotational structure longward of 6800 has been tentatively identified as $5\nu_3$ by Owen (1966). (*b*) Intensity spectrum of Uranus divided by the lunar spectrum in the same spectral region as in fig. 2*a*. Note the slight difference in scales, and that the ordinate labeling of "photographic density" applies only to fig. 2*a*.

FIGURE 2-7

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We have made a preliminary attempt to try to compare the two planets at even shorter wavelengths (Encrenaz et al, 1974). This study indicates that there is even more methane on Neptune than on Uranus. The increase seems to be on the order of twenty-five percent or so. The mere fact that one is seeing methane bands down to these very short wavelengths (the shortest is found at 4410 angstroms) is an indication that really immense amounts of this gas must be present.

Model atmospheres for Uranus and Neptune have been suggested by Lewis and Prinn (1973) and revised by Weidenschilling and Lewis (1973). What we are saying implies that the level of methane condensation has to be lowered quite a bit and that condensation is going to occur even lower in the atmosphere than was indicated before. It looks to us as if one is seeing beneath the condensation level in these short wavelength spectra just as one is on Jupiter in the case of ammonia and that the methane abundances are very large indeed. How can this be reconciled with the Rayleigh scattering that should occur in such deep atmospheres? This is one of many questions yet to be resolved.

F. CONCLUSIONS

Let me close by just summarizing the abundance situation as we see it at the moment (Figure 2-8). I am not including here the very exciting new work on ethane and acetylene and so on, these are just the major atmospheric constituents. What we find, in compiling these various numbers and then trying to deduce the hydrogen-to-carbon ratio, is that in the case of Jupiter and Saturn we seem to have roughly solar abundances as far as hydrogen and methane are concerned at least; whereas, for Uranus and Neptune these ratios are way, way down. We simply don't know the exact numbers because we don't have long enough pathlengths to determine them. We don't have any of the methane bands quantitatively analyzed so that we cannot calculate these numbers either.

Figure 2-8

ABUNDANCES IN THE OUTER SOLAR SYSTEM

OBJECT	H_2 (km atm)	NH_3 (m atm)	CH_4 (m atm)	H/C
JUPITER	75 ± 15	12 ± 5	50 ± 15	3000 ± 300
SATURN	75 ± 20	2 ± 1	60 ± 12	2500 ± 400
URANUS	450 ± 100	< 2.5	$>10 \times 10^3$	< 100
NEPTUNE	450 ± 100	---	$>10 \times 10^3$	< 100
PLUTO	---	< 10	< 2?	---
TITAN	5 ± 2.5	< 2.5	200 to 1600	6 to 50
TRITON	---	---	< 2?	---
			SUN	2700 ± 300

Model dependent upper limits are given for the other objects. The hydrogen and methane abundances for Titan deduced by Trafton (1973 a,b) lead to a very low ratio for H/C. There now seems to be the possibility that the ratio is even lower, if the hydrogen observations can't be confirmed.

MR. RASOOL: Those are in kilometers and the others are in meters?

MR. OWEN: That is right; the hydrogen values are in kilometers, the others are in meters.

Incidentally, the ammonia on Jupiter also seems to have the solar ratio and this is what convinces us that we are looking beneath the level where the abundance is set by the saturation vapor pressure, whereas, on Saturn this is obviously not the case.

You may now feel in the midst of total confusion because I have tried to cover a lot of material in a very short time. But some of this confusion is real; there is a large amount of basic information we simply don't have, other sets of data seem to be in conflict with one another, and there are glimmerings of very intriguing problems we are only beginning to solve. That is the point from which we want to go forward and produce the atmospheric probes which are the main subject of this conference so we can finally obtain some really reliable results.

MR. RASOOL: Thanks, Toby, for a very scholarly lecture in which you included some of the very recent results which shows immediately how the science is moving on a daily basis. A year ago when we had a Titan workshop here we thought everything was under control. We had some estimates of the hydrogen pressures going up to 700 millibars. Today we see entirely different things. That will give you an idea how fast this science is moving; the amount of data we get in all the spectral bands is restricted because the ground-based telescope is the only tool we have at the present time, the only means of deducing the abundances except for Jupiter, of course, where we have some new results.

Now, this presentation assumed that all of you know that outer planets are giant bodies with high gravity and made mainly of hydrogen and helium. The helium was absent in the last table because we cannot observe helium from groundbased telescopes. So I am just adding the helium part; we don't know how much there is on the outer planets. That is one very important question we have to answer. The problem you are going to have in the next ten years is to be able to design probes to survive the uncertainty, and also design payloads for the probes which clarify the uncertainties.

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UPPER ATMOSPHERES AND DIAGNOSTIC MEASUREMENTS

Dr. Donald Hunten
Kitt Peak National Observatory N75 20361

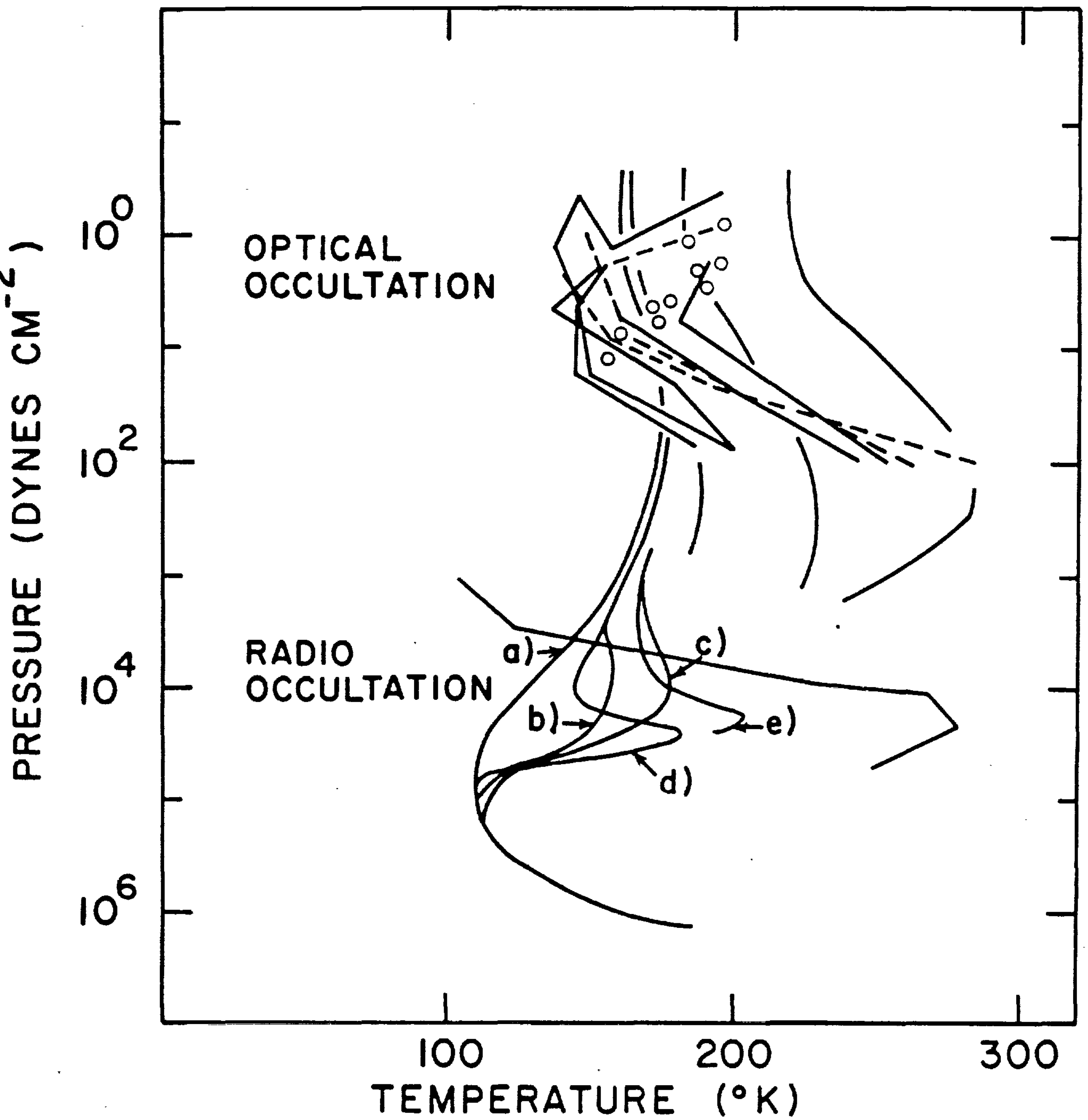
DR. DONALD HUNTEN: As well as the somewhat sophisticated questions mentioned by Dr. Owen, we should also ask elementary ones like: What really are the temperatures in the atmosphere of these planets and satellites? Also, the question of the basic composition which, we are sure for the planets, is dominantly hydrogen and helium with the helium about ten percent by number or twenty percent by mass with the hydrogen; but, we don't even know that for sure, and we would like more assurance than we have at the present.

So even a mission which did nothing but measure a good, credible, and non-controversial temperature profile and measured the ratio of hydrogen to helium would be very valuable scientifically. Of course, most of us would hate to stop at that point, but we must keep reminding ourselves that the most basic questions of all are still in great doubt.

Figure 2-9 was kindly supplied by my colleague Dr. Lloyd Wallace; it is from a paper by Wallace, M. Prather, and M. J. S. Belton, in press in the Astrophysical Journal. Curves (a) - (e) were calculated on the basis of radiative thermal equilibrium, the inputs being solar and planetary radiation. (Note that pressures run from one (1) bar to one (1) microbar, so that this region is the stratosphere and mesosphere.) Owen's and Lewis' talks refer to the region below this figure.

Curve (e) is the hottest that could be obtained with purely radiative heat inputs, and it falls far short of the curve from Pioneer 10, the one without a label. The more recent data, presented this morning by Kliore, carry these temperatures even higher at deeper levels.

The upper part of the figure shows several computed curves, and also several sets of data from the occultation of the star Beta Scorpii, observed and reduced by different people. Although there is an appreciable spread, the agreement is reasonable, and so is the agreement with the calculated temperatures, especially the preferred curve (a). These temperatures are warm, 160-180°K, though nowhere near as warm as the ones from Pioneer.



OPTICAL
OCCULTATION
RADIO
OCCULTATION
a)
b)
c)
d)
e)
10^0
10^2
10^4
10^6
PRESSURE (DYNES CM^{-2})
100
200
300
TEMPERATURE (°K)

Figure 2-9

One would be tempted to say that the optical data are good and that there is some unknown factor perturbing the radio data. But the two methods are based on very similar physical principles, and it is hard to see why one, and not the other, should be rejected. For now we have to conclude that there is something fundamental that we just do not understand. It is not just a matter of the disagreement shown in Figure 2-9. As Owen already discussed, there are several ways of deducing the temperature in the 1-bar region: thermal emission (also measured by Pioneer 10), spectroscopic line strenghts, the presence of clouds. They all agree and the temperature they agree on is 100-130°K, just what is computed. Thus, we have a conflict between data from different sources, not just between observation and a calculated model.

So, simply a probe carrying a thermometer and nothing else would resolve a very fundamental question about the basic nature of the Jovian atmosphere. Of course, if we have this problem that we can't understand Jupiter, there is no basis for suggesting that we understand any other atmospheres in the outer solar system either.

Many of you have been involved in studying candidate missions based on the set of experiments (Figure 2-10) which is sort of a minimum or basic payload, which has been in use for the last few years. It is based on the thinking and experience that we have had so far with the Pioneer Venus probe mission, but it is cut down considerably.

From Owen's description of the atmosphere and the scientific questions, you can see that the measurements on the right are all useful and important.

Properly speaking, the main clouds visible from Earth are in the lower atmosphere and therefore, not really the province of this talk. On the other hand, there is lots of reason to believe that there are clouds, or at least haze, far up into the stratosphere; and this is basically because the atmospheres of Jupiter, Saturn, and Titan are all dark in the ultraviolet. A gaseous atmosphere has no business being dark in the ultraviolet because it scatters; it should be a blue sky, to put it as shortly as possible. It should exhibit rayleigh

FIGURE 2-10

BASIC PAYLOAD

THERMOMETER	TEMPERATURE
BAROMETER	PRESSURE
ACCELEROMETER	DENSITY
	TURBULENCE
MASS SPECTROMETER	COMPOSITION
NEPHELOMETER	CLOUDINESS

FIGURE 2-11

OPTIONS

COMPOSITION BY GAS CHROMATOGRAPH
CLOUD PARTICLE SIZE SPECTROMETER
SOLAR RADIATION FLUX
THERMAL RADIATION FLUX

scattering, to say it in a more scientific manner, and have a higher and higher reflectivity at shorter wavelengths until something starts to absorb. That something has to be methane which doesn't absorb above 1500 or 1400 angstroms.

So, something else is absorbing strongly at wavelengths as long as 3,000 or 3,500 angstroms at very high altitudes in all these atmospheres. The accepted explanation is a fine, absorbing aerosol, or dust, as proposed by Axel (Astrophys. J. 173, 451, 1972). This material is probably related to some of Owen's later figures; presumably there are photochemical products, photochemical smogs if you like, produced by the action of solar radiation mostly on methane and then a slow fallout of the particles to lower levels. It could be regarded as asphalt, or tar, or gasoline. I think those colorful names for this colorful substance give you the general idea.

Returning to Figure 2-10 we show, as we have for Pioneer-Venus for many years, a mass spectrometer as the basic instrument for measuring composition. That should be excellent for getting the hydrogen-to-helium ratio; it should be reasonably good for getting methane and ammonia. But a mass spectrometer isn't really very well suited to measuring other, more subtle things, and in particular photochemical products, chromophores, and so on. One really has to question whether anything is very suitable, considering the extremely small abundance that we have to be dealing with.

However, one should at least consider options like those shown in Figure 2-11 which are, again, based on Pioneer-Venus experience. The mass spectrometer is probably essential in order to get major gases and unexpected constituents. But the gas chromatograph has a lot to be said for it, particularly for chemically active and rather minor constituents. We have a promising gas chromatograph on Pioneer-Venus at the moment and there is no reason why it shouldn't work in the outer solar system as well. It should be considered a prime candidate to supplement the mass spectrometer.

Instead of or in addition to a nephelometer, there is the possibility of a cloud-particle-size spectrometer, a shadowgraph device that measures the shadows of particles as they go through a laser beam. Again, this is a Pioneer-Venus experiment. We would like to know the flux of solar radiation, namely the difference between the up-going and down-going radiation in the visible and neighboring wavelengths and, similarly, for thermal radiation. Now, one wouldn't have considered those last two measurements too important until recently but, again, I must stress that we are absolutely baffled by the problem of the thermal structure of the Jovian atmosphere. We thought we understood it; we could fit all the spectroscopic and thermal data we had, beautifully really, by computed thermal structures. And then along comes this radio measurement from Pioneer 10 which disagrees by orders of magnitude. When I say orders of magnitude I'm thinking of the fact that thermal radiation goes as the fourth power of the temperature. A factor of 3 in temperature means a factor of 81 in thermal radiation.

Before I close, I would like to say a few words about the rest of the upper atmosphere, namely the thermosphere and ionosphere. There again, we have the example of Pioneer Venus, although there is a major difference because at Venus we will have a low-periapse orbiter. I would hope that an attempt would be made to take pre-entry measurements of at least neutral and positive-ion composition. Even a few measurements can be of great value, because we are looking for large effects. Different ionospheric models often disagree completely on which positive ions are present. The whole nature of the upper atmosphere is determined by diffusive separation of light and heavy constituents. The homopause, the level at which this effect begins, can be determined by comparing measurements of two or more gases made before and after entry. In fact, we already have an estimate of the homopause level for Jupiter, based on the Lyman-α measurements on Pioneer 10 by Judge and Carlson. The density seems to be between what we find on Earth and what we think exists on Mars. We can, therefore, make models of Jupiter's upper atmosphere with much more confidence than we could before.

But what about Titan? The question of what measurements to make there was considered briefly by the Titan Atmosphere Workshop last year. It is obvious that one is dealing with a very different atmosphere, one that is much richer in heavier molecules and poorer in the lighter ones, hydrogen and helium. Although we don't expect helium and the amount of hydrogen is in doubt, we probably still have to fly the mass spectrometer. The gas chromatograph, however, very clearly becomes the primary composition experiment for Titan.

The real question, still, about Titan is whether it has enough atmosphere so that we can really hope to probe it with the technology that we're talking about. There were somewhat wild ideas around a year ago that the surface pressure on Titan might be as great as a thousand atmospheres, if you really call it a surface, and pressures of half to one atmosphere were very respectable indeed. They are still respectable, but the strength of the evidence, as we see it, for such high pressures is much less than it was. When we were really pinned down at the Titan workshop to set an absolute minimum surface pressure, the value we could give with confidence was embarrassingly small, about 20 mb. The engineering information available at the time suggested that an entry probe might not yet be on the parachute at that level. If so, the mission is not attractive. Both scientists and engineers must work on this problem: what is the lower bound to the surface pressure, and what minimum pressure is needed for a viable mission. We have a few years yet, and progress is rapid already; hopefully, both sets of answers will be available by the time they are needed.

MR. LOU FRIEDMAN: I was interested in the remark about haze in the upper atmosphere. Are there any analogies with the MVM findings on Venus and similar photochemical haze?

DR. HUNTEN: Well, I dare say it is an analogue in a sense; we have such a haze in our own stratosphere too, and it's chemically very similar to the haze and maybe even the main cloud deck on Venus. So, I think we have to get more and more used to the fact of life than atmospheres are typically quite dirty; especially atmospheres that aren't frequently cleansed by rainstorms. Maybe the Earth's atmosphere is the major anomaly, because rain is so prevalent here and washes things out of the atmosphere. But, in terms of the details of what the haze is made of, I don't think it is safe to draw a close analogy; just the general principle that it's a photochemical haze.

N75 20362

COMPOSITIONAL MEASUREMENTS BY OUTER PLANET ENTRY PROBE

Dr. John S. Lewis
Massachusetts Institute of Technology

DR. JOHN S. LEWIS: I think you have already seen illustrated in Dr. Hunten's talk one of the basic principles of atmospheric physics, which is the tendency for one's attention to sediment down to ever higher levels of density. I think you noticed that he several times found himself dangling down into the lower atmosphere where he felt he had no business being. This is understandable, because we just agreed on the guidelines about half an hour ago, long after he had prepared his talk.

I would like to start ab initio with the formation of the solar system and make it for you in two or three minutes according to my recipe at least and to derive from that very brief discussion a number of things which one ought to do or must do using planetary entry probes as the platform for investigation.

First of all, I think it is almost universally accepted that all of the planets in the solar system owe their parentage rather directly to a solar composition cloud of gas and dust which occupied the entire volume of the present solar system some 4.6 billion years ago. This cloud of gas and dust is called the solar nebula. We believe that we see today in the solar system several bodies which approach rather closely to the composition of this primordial material out of which all of the planets originated.

One of these, of course, is the Sun itself, which seems to be the product of gravitational collapse in such a gas and dust cloud without fractionation between components. Another appears to be Jupiter, which is quite close in its bulk composition to the composition of the Sun. Saturn deviates somewhat in the direction of being composed of intrinsically denser material than Jupiter, yet nonetheless, very close to that of the Sun. Uranus and Neptune, interestingly enough, continue in this sequence, being hydrogen-rich or volatile-rich material, yet progressively farther from the composition of the Sun in the direction of having a higher abundance of heavier elements, these being the so-called ice-forming and rock-forming elements.

Thus, what we see as the density trend of the outer planets is a compositional variation with distance from the Sun, caused ultimately by processes in the solar nebula. Those processes in the solar nebula which directly concern us are, first, the chemical processes (namely the sequential condensation of gases going to ever lower temperatures and ever greater distances from the Sun), and second, the physical accumulation processes by which a planet is assembled out of the gas and dust mixture.

We see in the outer planets a progressive enhancement of the abundance of the condensate component of the planet relative to the gas component of the planet. When we get to Uranus and Neptune we find that these components certainly are comparable in mass; indeed the component of condensed material may be dominant over the component of solar-type gaseous material.

Therefore, one of the things that we most urgently need to know, in investigating the atmospheres of the outer planets, is the chemical composition of the atmosphere down to the greatest depths manageable, for purposes of comparison with the elemental abundances in the Sun. Dr. Owen has already told us a bit about what has been done with spectroscopic studies of the atmospheres above their cloud layers. As you have already heard, those materials which are observable on Jupiter and Saturn: hydrogen, methane and ammonia - have abundances which are compatible with the planets being close to solar composition. But we must recall here that we are sampling one part in 10^{10} or so of the mass of the planet and this is a remarkably small sample on which to base far-reaching conclusions. Furthermore, we are looking at the coldest portion of the atmosphere of the planet, which means that most atmospheric constituents are condensed out and not visible to us.

Finally, we are looking at a portion of the atmosphere in which the majority of the gases present at levels greater than one part per billion are spectroscopically inert gases; hydrogen, which is a very weak absorber, marginally falls into that category, visible on the outer planets only because of its enormous abundance, and then, of course, helium, neon, argon, and the other rare gases. These are not detectable by remote observations with the possible exception of some very specific experiments which may be made in the immediate vicinity of Jupiter by remote sensing.

One point that is extremely important in understanding the fractionation process which distinguishes the outer planets from one another, is the way in which the abundances of the major elements vary from planet to planet. Classically, models for the outer planets have been generated by varying the hydrogen-to-helium ratio in these planets. I think that there is very little ground for believing that such fractionation occurs, but unfortunately, there are no data which we can bring to bear on this issue. It is extremely urgent to determine whether there is variation in the hydrogen-to-helium ratio in these atmospheres. This requires either upper-atmosphere measurements plus a firm knowledge of the location of the turbopause, or a direct measurement in the lower atmosphere. In some ways, since the latter measurement is not much harder and more reliable, that seems like the thing to do.

We would like to know the abundance of the major condensible components of the atmospheres, the components containing the major elements which make up solar material after hydrogen and helium; these are: oxygen, carbon, nitrogen and neon. Then, a factor of ten less abundant than these are iron, silicon, magnesium and the other rock-forming elements. We will not get deep enough into the atmospheres of the outer planets, in the next few centuries, to be able to assess the abundances of the rock-forming elements directly, but it is entirely possible that by penetrating to pressures of a few tens of bars, one can measure directly the abundances of methane, ammonia, water vapor, neon, and so on.

We also would like to have isotopic evidence on these gases. We would like, particularly, to know the isotopic composition of hydrogen - the H:D ratio - which has been reconstructed for the early solar system in two ways: first, by the study of hydrogen compounds in meteorites and, second, by spectroscopic studies of the atmosphere of Jupiter. We would also like to know the helium isotopic composition, and that of carbon, nitrogen, oxygen, and neon.

The precisions to which these isotopic analyses must be known vary greatly from element to element because very different processes are involved. If one measured the H:D ratio in the atmosphere of Jupiter or one of the other planets to a precision of plus or minus ten percent, that would be an extremely valuable experiment. On the other hand, getting the carbon 13 to carbon 12 ratio to a precision of plus or minus ten percent would be almost not worth doing unless, of course, you discovered some phenomenal, enormous isotopic effect which no one had anticipated.

Also, the analytical problems that must be faced in looking at the outer planets are made somewhat more interesting and somwhat more demanding by the fact that there are photochemical products present; materials such as ethane, ethylene, acetylene, methylamine, and other simple carbon-nitrogen compounds. These, however, are largely produced very high in the atmosphere and are high enough so that they may be chemically destroyed, reprocessed, and made back into methane and ammonia.

Thus, the experiments designed for looking at these interesting organic materials will be conducted above the cloud tops, a regime in which the entry probe would normally be traveling quite fast. These are intrinsically difficult measurements.

Other extremely important considerations for the outer planets concern their overall thermal structure. It's been known for some time that Jupiter is a net emitter of energy; that it produces approximately three times as much energy as it receives from the Sun: it has an internal heat source. This has been confirmed in somewhat less detail but still fairly convincingly for Saturn and Neptune. Uranus remains something of an enigma in that the data to date serve to prove neither that Uranus has an internal heat source nor that it does not, and one can only imagine that the middle apple in the row out there should not be different from the others in this respect. Nontheless, the question remains unanswered: Does Uranus have an internal heat source? If it does, then all of our notions regarding the circulation structure of the atmosphere are strongly conditioned by that conclusion. It means that the atmosphere's motions are driven from below by the release of internal heat rather than driven from above by absorption of sunlight. This means, then, that the motions of the atmosphere will essentially penetrate all the way down into the deep interior of the planet. Since the outer planets are essentially gaseous in composition, this means that we are talking about the processes throughout the entire body of the planet being mirrored by our understanding of thermal balance in the upper part of the troposphere. That is a very important kind of thing to understand.

Skimming the cream off all that, there are, I think, a few reasons why a Uranus entry probe looks perhaps slightly more interesting than even a Saturn or a Jupiter one right now. Some of these reasons are quite obvious and are familiar to most of you. One of these reasons is that for the past few years we have been told repeatedly that one cannot confidently plan on surviving entry into the atmosphere of Jupiter with a probe which is not essentially all heatshield. Therefore, we have thought in terms of flying a payload which had a larger weight fraction of instruments in it, relative to heatshield, and putting it into a planet that was somewhat easier to enter. Many of our conclusions are conditioned upon, or predicated upon, the assumption of a very difficult atmospheric entry on Jupiter. This issue, unfortunately, changes every six months. There is a sort of a flip-flop in opinions: it gets harder, then it gets easier. I am predicting that by October it will get harder again.

There is also a telemetry problem, in that if a probe enters to great depths into an atmosphere which contains a large quantity of ammonia, it will have trouble transmitting through the ammonia gas. Studies of space probes common to Saturn, Uranus and Jupiter have to date largely been sized, and had their transmitters designed, on the assumption that the same package would be landed on each of the three planets. This means that entry into Jupiter, because it is so demanding on the communications performance of the spacecraft, would tend to cause design decisions which would hinder the applicability of that same entry probe to deeper investigation of the atmospheres of Saturn and Uranus.

In particular, it leads to the conclusion that, because of communication problems on Jupiter, a pressure vessel need not be included to protect any outer planet entry probe against pressures greater than ten or twenty bars.

Finally, we have the problem of doing analyses of the atmosphere. The questions of composition of the atmosphere are very important; they involve the resolution of questions such as the fractionation of materials between the outer planets; the cosmogonic problems of the composition of the condensed components versus distance from the Sun; the abundance of the isotopes of the light elements in the early solar system; the photochemical products, and so forth and so on; all of which are essentially questions involving analysis of the atmosphere. There is something to be gained, I think, from entering the atmosphere of

Uranus rather than that of Jupiter, because we have fairly good a priori evidence that there has been an enrichment of the minor constituents, namely, those which are not hydrogen and helium. Thus, the analysis for these constituents should intrinsically be easier. It is very promising to try to take advantage of that fact and, perhaps, be able to analyze and get the isotopic composition of some trace constituents which, in the atmosphere of Jupiter, would be extremely hard to detect.

We also must include on our entry probe the experiments shown on Dr. Hunten's graph, essentially a pressure gauge, temperature gauge, accelerometer, and nephelometer. I would add visible and infrared, upward and downward-looking sensors as being extremely important additions to the payload, and this suggestion is by no means unique to me or to Dr. Hunter. Then comes the central issue of the composition experiment. I think it is entirely clear that a mass spectrometer has to be the heart of such an entry probe analytical package. We would like to use whatever this analytical package is to analyze the atmosphere at several different discrete altitudes to see how the composition varies with depth. We need, basically, compositional data on the atmosphere in terms of the major chemical species present. If we want to get the isotopic species, we run into ever and ever and ever more demanding technical problems.

Let me just say a few words on the why getting the chemical abundances is relatively easy, the abundances of the chemical constituents of the atmosphere. On the outer planets, one has essentially a fractional distillation system built into the atmosphere. One may begin analyses at high altitudes (and low temperatures), and look at the mass spectrum of hydrogen, helium, methane and neon. Methane and neon do not interfere with each other in the mass spectrometer, in that they do not have any fragments which appear at the same mass number. The analyses can then be repeated lower in the atmosphere where the temperatures are high enough so that ammonia gas may be present. One can then measure the mass spectrum of the mixture of methane plus ammonia; since the fragmentation pattern for the local variety of methane is already known, you can subtract that out to get the isotopic composition of ammonia. Looking only at the sum of the two would defeat the purpose of getting the isotopic composition because the fragmentation patterns of the two overlap each other extensively. Next, at even higher temperatures, water vapor may be present, and one can do the same thing again on water to get the oxygen 18, 17 and 16 relative abundances.

Difficulties lie in the fact that for the two major elements, hydrogen and helium, the rarer isotopes are extremely rare. Also, although the isotopes such as nitrogen 14 and nitrogen 15 have abundances that are not enormously different from each other; nonetheless, the total abundance of ammonia is low. Thus, it becomes a difficult analytical problem.

Let us illustrate this briefly, by discussing how to get the hydrogen and helium isotopic composition. One cannot simply analyze the bulk atmospheric mixture containing fifteen percent or so of helium in a mass spectrometer and look at the peaks at mass four and three for the $3H_e:4H_e$ ratio, and two and one for the D:H ratio for the simple reason that what you actually see in the mass spectrometer is a very complex mixture in which the H_2^+ and the HD^+ ions produce very large signals, but the HD^+ signal occurs at the very same mass number as helium three and at the same mass number as the H_3^+ ion, which is formed in the ion source of the mass spectrometer in a hydrogen-rich atmosphere. Thus, there is mutual interference of helium and hydrogen. The H_3^+ ion interference is, under some operating circumstances, very important. This problem can be avoided through dropping helium out of the mass spectrum altogether, by operating at an ionizing voltage which is below the appearance potential of H_e^+ ions, thereby seeing the mass spectrometer hydrogen alone. This is the minimum complexity of handling required to determine such a simple thing as the isotopic composition of hydrogen and helium, the two most abundant constituents of the atmosphere.

If the isotopic composition of minor constituents, such as carbon, nitrogen, neon, are required, usually the situation is quite a bit more difficult. This is especially true if one wants to get the abundances of photochemical products which, in only a very few cases, could have abundances in excess of one part per million. This would require, if pursued to its logical extreme, a GCMS package on the entry probe. However, the complexity of such a package and experience over the last few years with a GCMS package on Viking, leads us to ask if there is not anything simpler that might be done. I frankly do not know what else can be done except by backing off from the original analytical goals. Thinking several years into the future, I would rather remain ambitious for the time being and hope that an instrument package could be worked up to solve these problems.

In the near future, I think there are a few important considerations facing us. One is that, in the case of the outer planets perhaps more than elsewhere in the solar system, the role of Earth-based observations of the planets remains extremely important. There are, as Dr. Owen has shown us, many new results, some of a rather unexpected nature, that have been forthcoming in the last few years. These results shall continue to accrue as new observational techniques are applied to the outer planets. I think that final design of the atmospheric entry probes cannot be done right now on the basis of present observations because there are things such as the degree of enrichment of methane in the atmosphere of Uranus which we will be learning that will strongly condition our choice of analytical instruments. This strongly conditions whether we can use a simple mass-spec type experiment or whether we have to go to some method of separating out methane, such as with gas chromatograph, and then analyzing that separately.

There is an important question of the degree of commonality that is practical between Uranus, Saturn, and Jupiter entry probes; whether they really should all use the same heatshield, the same communications system, and the same analytical package. If, as it now appears, the heavy elements are so strongly enriched in Uranus, its composition approaches that of Titan. Although Uranus certainly would not require anything like a Titan entry spacecraft, it still raises the difficult issue of the degree to which commonality for entry probes to these three planets can be maintained without sacrificing important quantities of scientific return.

I have suggested that chemical analysis of the atmosphere will be fairly easy for constituents with abundances more than a few parts per million, and that the isotopic analysis will in general be hard but subject to cleverness. I particularly wish to raise and keep before everyone the idea that the issue of the nature of the analytical experiment is far from settled; that a plain, pure-and-simple gas chromatograph may be helpful by itself, whether or not connected to a mass spectrometer. There might be some very promising compromises that can be worked out in that area. I think, especially in light of quite a number of recent developments, that Uranus still seems a safe and likely target for the first outer-planet entry-probe mission. It certainly has a great number of exciting aspects to it. But still, it is important to keep in mind that we are looking not only at the phenomena which were common to the origin of all the outer planets, but also the processes which distinguish between them. Therefore, entry into any one of the outer planets is not, by itself, sufficient. This forces us once again back to the difficult orbital issue of the degree of commonality that can be designed into probes which can be sent to three or more of the outer planets.

DR. RASOOL: Thank you, John. Any questions?

MR. DAN HERMAN: No questions, but I do have a comment. Your points on the desirability or lack of desirability of commonality are very well taken.

One of the things that we will probably do when we release this Phase B Study is we would ask the contractors, with the help of the scientific community to optimize the probe to Uranus since that is the entry mission that will occur first, and then to see if it makes sense to both the scientific vane as well as the technical vane, to retain that commonality for Jupiter and Saturn; and it may not. I mean, this is something that I think does need intensive study. But both points are very well taken.

PIONEER 10 JUPITER ATMOSPHERIC DEFINITION RESULTS - A SUMMARY

Dr. John Wolfe
NASA Ames Research Center

DR. WOLFE: I will talk about some of the Pioneer 10 results and also about what I think are some of the ramifications of those results with regard to technology and with regard to questions that I think this group ought to address during the next few days.

I will make some introductory remarks, Arv Kliore, who is the PI for the occultation experiment on Pioneer 10 will present some of his data and then I will make some concluding remarks.

Prior to the encounter of Jupiter by Pioneer 10, I was assured by many people, including our public relations office, that Pioneer 10 would answer all the questions with regard to Jupiter. In fact, if you read our project approval document you would swear that another mission is not needed. I assured these people that I felt that Pioneer 10 would more than likely raise many more questions than it answered and I am happy to report that is indeed, the case.

So, I would like to proceed to one of the things that Dan Herman mentioned this morning with regard to a cooperative Jupiter orbiter program with ESRO using the Pioneer H spacecraft, plead for you to consider the rationale during this workshop, the possibility and the justification and the possible need for a very simple probe associated with that mission.

I have listed on Figure 2-12 the rationale for the Jupiter orbiter mission with a probe using the Pioneer-class spacecraft. The fundamental reasoning is that one can do both a probe and an orbiter mission with this spacecraft, because for a Jupiter mission one is not weight restricted. The rationale for the probe is based on the improved ephemeris resulting from the Pioneer 10 flyby, which now permits planning for entry at a shallow angle and, therefore, reducing the peak heating loads; secondly, we may have an improved atmospheric model.

PIONEER JUPITER
ORBITER/PROBE MISSION

- PIONEER CLASS JUPITER MISSIONS NOT WEIGHT RESTRICTIVE
- RATIONALE BASED ON PIONEER 10 RESULTS
  - IMPROVED EPHEMERIS
  - IMPROVED ATMOSPHERIC MODEL
- OBJECTIVES
  - DIRECT ATMOSPHERIC OBSERVATIONS
  - MAGNETOSPHERIC SURVEY
  - MAGNETOTAIL OBSERVATIONS

Figure 2-12

The objective for the probe is direct, in-situ, atmospheric observations. I think that some of the more interesting regions in the higher atmosphere are going to be very difficult to observe and that shows up on a later figure. The objective for the orbiter is a magnetospheric survey in which we are primarily interested in magnetotail observations. Now to Figure 2-13.

We are talking about trip times to Jupiter on the order of two and a half years with a total injected weight of 790 kilograms; for the orbiter we are talking about a spacecraft weight of 260 kilograms and a payload weight of about 30 kilograms. We want to achieve an orbit of about 6 x 200 R_J and I will show that on another figure.

This is how the orbit period turns out; 129 days, and a ten-orbit design lifetime. The Jupiter orbiter people have always considered this to be a minimum on Jupiter orbiter missions. The probe this mission could carry - and we are going to get a lot more details on this throughout the rest of the workshop - is on the order of 132 kilograms. Payload weight, and this may be optimistic, is 15 kilograms. (It may be more like ten.) So, one has to consider for an early Jupiter probe mission what can be done with ten to fifteen kilograms; and, in particular, what can be done to get first order data knowing that more sophisticated probe missions would be flown in the future. We have been considering communications from the probe via the orbiter. In the case of Pioneer-Venus, we are communicating from the probe directly to Earth. Because of Jupiter's distance we must relay through the bus spacecraft using data rates in the order of twenty bits per second with the objective of making observations down to twenty bars. There are some other problems associated with thermal control for the case of Jupiter. At twenty bars we expect temperatures comparable to those on the surface of Venus.

PIONEER JUPITER
ORBITER/PROBE MISSION

- TRIP TIME $\sim$ 2.3 YEARS
- TOTAL INJECTED WEIGHT, 790 Kg
- ORBITER
  - BASIC SPACECRAFT WEIGHT, 260 Kg
  - PAYLOAD WEIGHT, 30 Kg
  - 6 X 200 R_J ORBIT
  - 129 DAY ORBIT PERIOD
  - 10 ORBITS DESIGN LIFE
- PROBE
  - TOTAL WEIGHT, 132 Kg
  - PAYLOAD WEIGHT, 15 Kg
  - COMMUNICATION VIA ORBITER AT $\sim$ 20 BPS
  - OBSERVATIONS DOWN TO 20 BARS

Figure 2-13

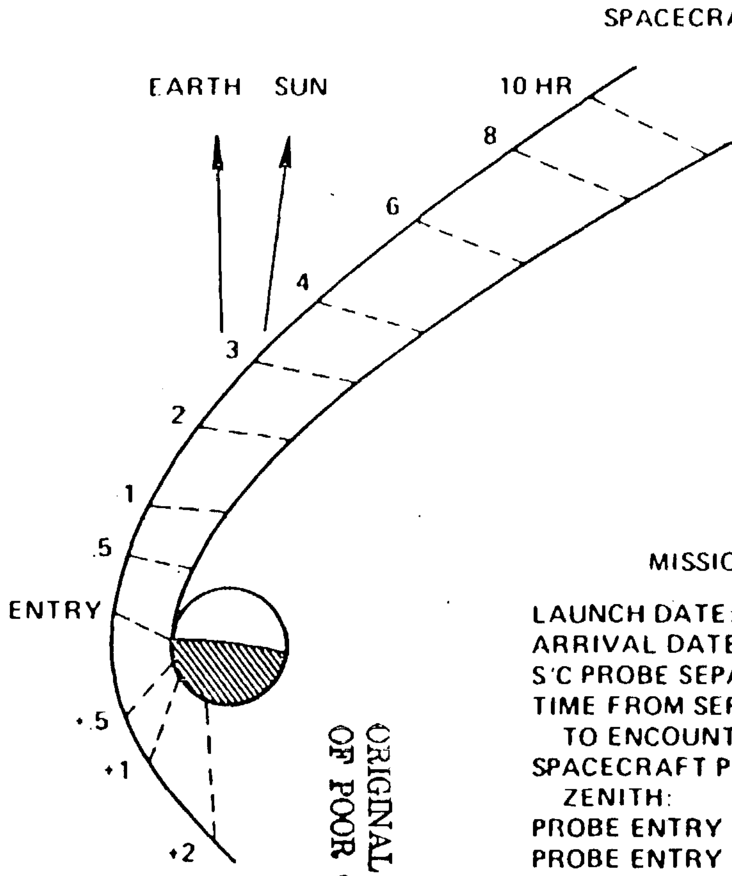
Figure 2-14 shows the probe entering and the bus spacecraft coming around and communicating with the probe. Then, as shown in the figure, after the probe mission is over, the spacecraft is heading out along the dawn meridian. This is particularly useful to the particles and fields magnetospheric survey of the magnetotail of Jupiter with the orbiter. If one was to dedicate a flyby mission to Jupiter in order to investigate the far-down tail of Jupiter where, perhaps, a lot of the magnetospheric physics are really going on, then you are passing so far away from Jupiter that you are not doing a good job with Jupiter itself.

The orbiter, on the other hand, puts the line of apsides (Figure 2-15) along the dawn meridian. The 200 R_J apoapsis allows us to get beyond the shock front and to investigate both the shock and the magnetopause. We would raise the periapsis up to something in the order of four to six R_J simply to keep the radiation levels down so that we can last for ten orbits. The orbits then swing around toward the tail and, essentially, we are back in the tail after ten orbits. This takes on the order of three years or so.

Figure 2-16 is a picture of the Pioneer spacecraft as it presently exists with three additions: a toroidal tank to carry the fuel for making maneuvers, the deboost, the probe, right behind it, and the communications antenna for the link with the probe. The main part of the spacecraft is unchanged from the present Pioneer 10-11 configuration.

Now we come to the problems. Figure 2-17 is a plot of Arv Kliore's data on the occultation experiment as reported in Science. This is Guido Munch's point which I put around one atmosphere; (perhaps it should be a little bit higher, but because of my particles and fields and nuclear physics background, I like to draw nice straight lines between two points that I know). In addition to that I put the region on the figure where one sees the peak

PIONEER JUPITER ORBITER/PROBE
APPROACH TRAJECTORY



MISSION PARAMETERS

LAUNCH DATE:	8 NOV 79
ARRIVAL DATE:	11 MAR 82
S/C PROBE SEPARATION:	500 R_J
TIME FROM SEPARATION TO ENCOUNTER:	53.2 DAYS
SPACECRAFT PHASED TO ZENITH:	E + 15.6 min
PROBE ENTRY ANGLE:	-7.5
PROBE ENTRY VELOCITY:	59.7 Km/sec
PROBE ANGLE OF ATTACK:	29

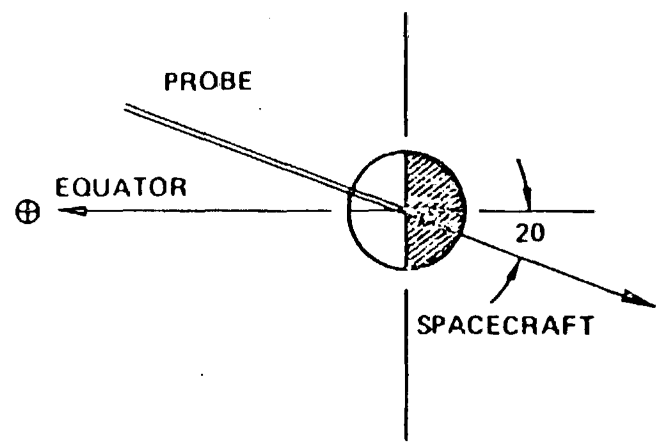


Figure 2-14

APOAPSIS PASS

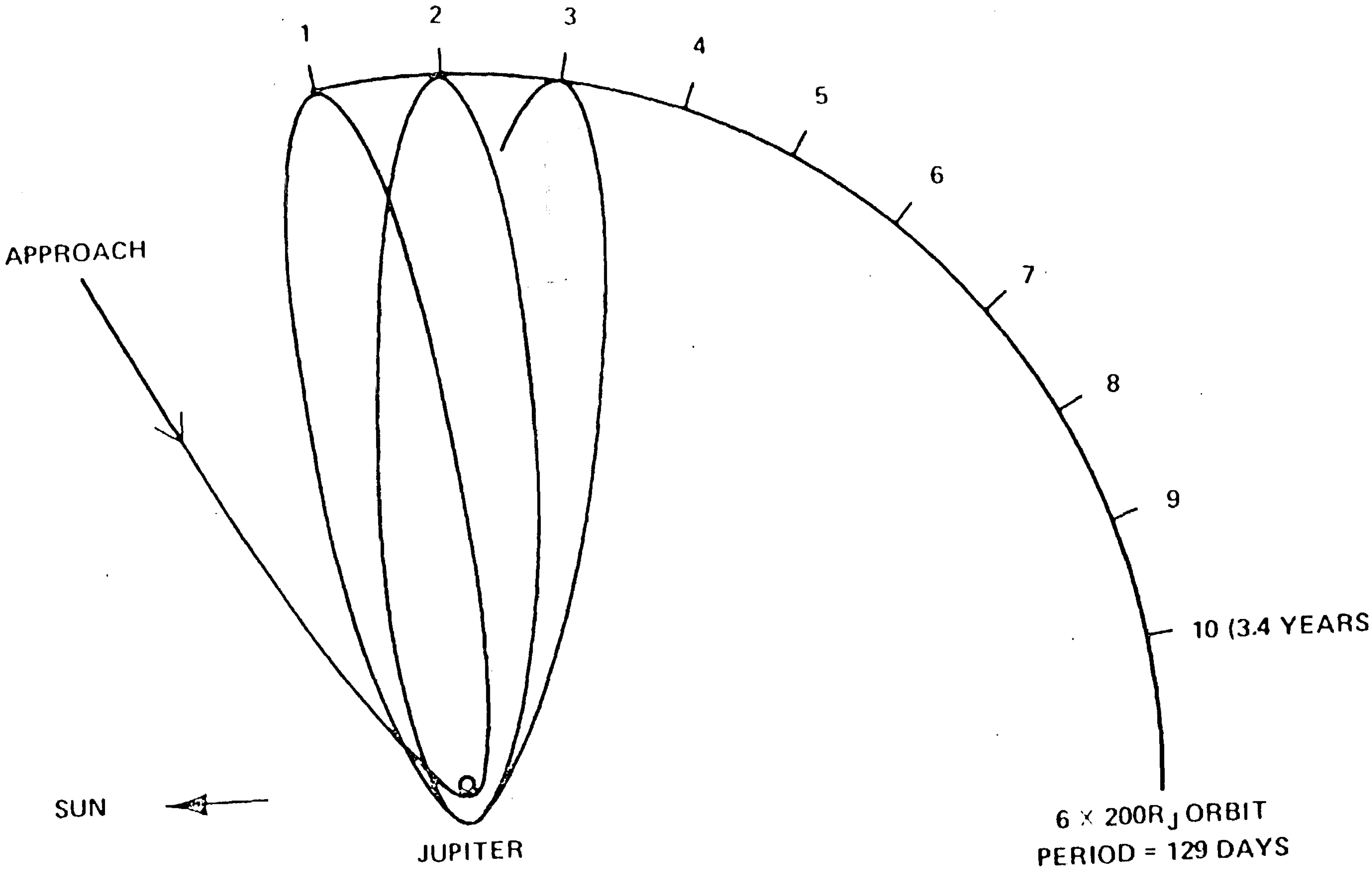


Figure 2-15

MODEL OF PIONEER OUTER PLANETS ORBITER/PROBE

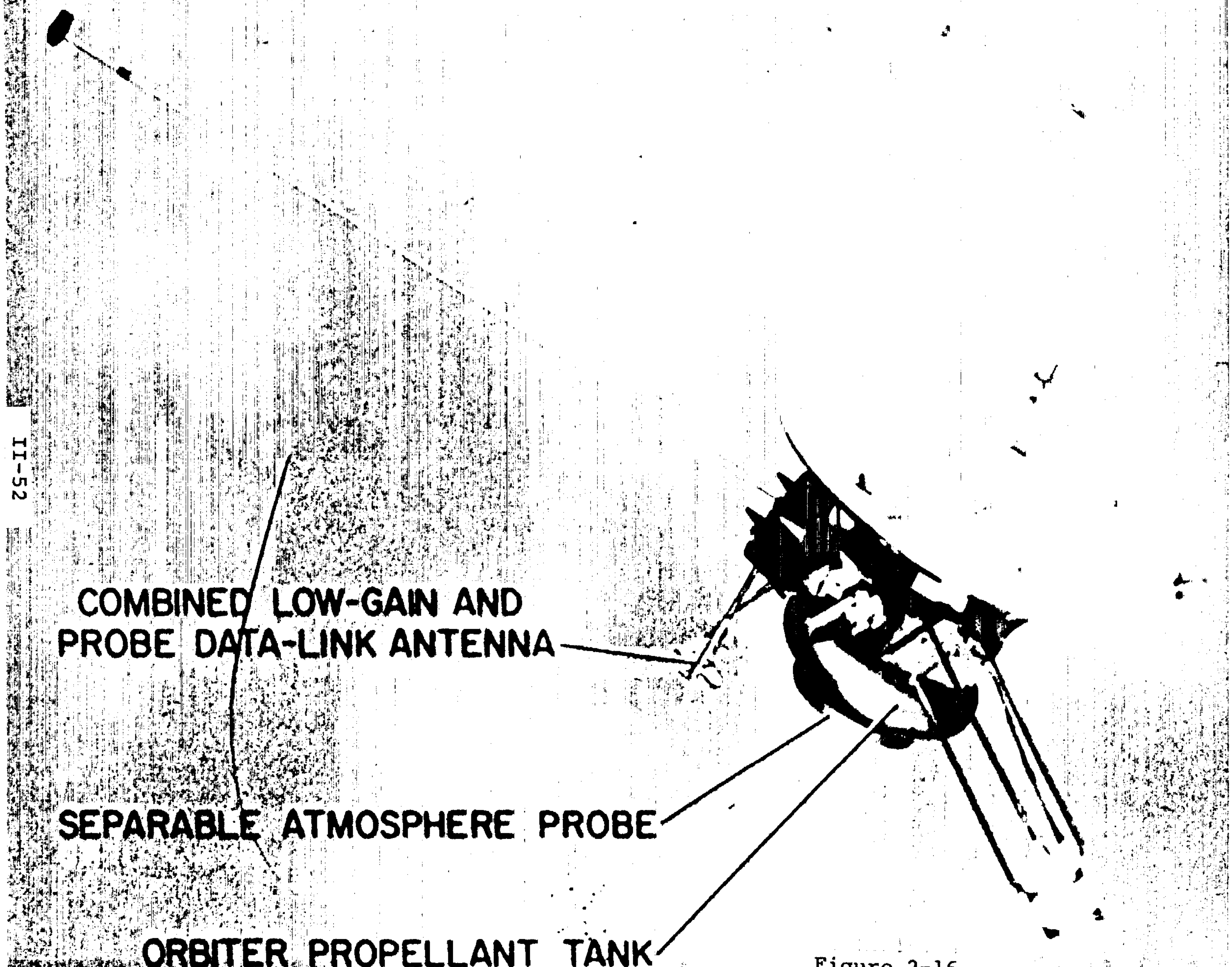


Figure 2-16

JUPITER ATMOSPHERE MODELS

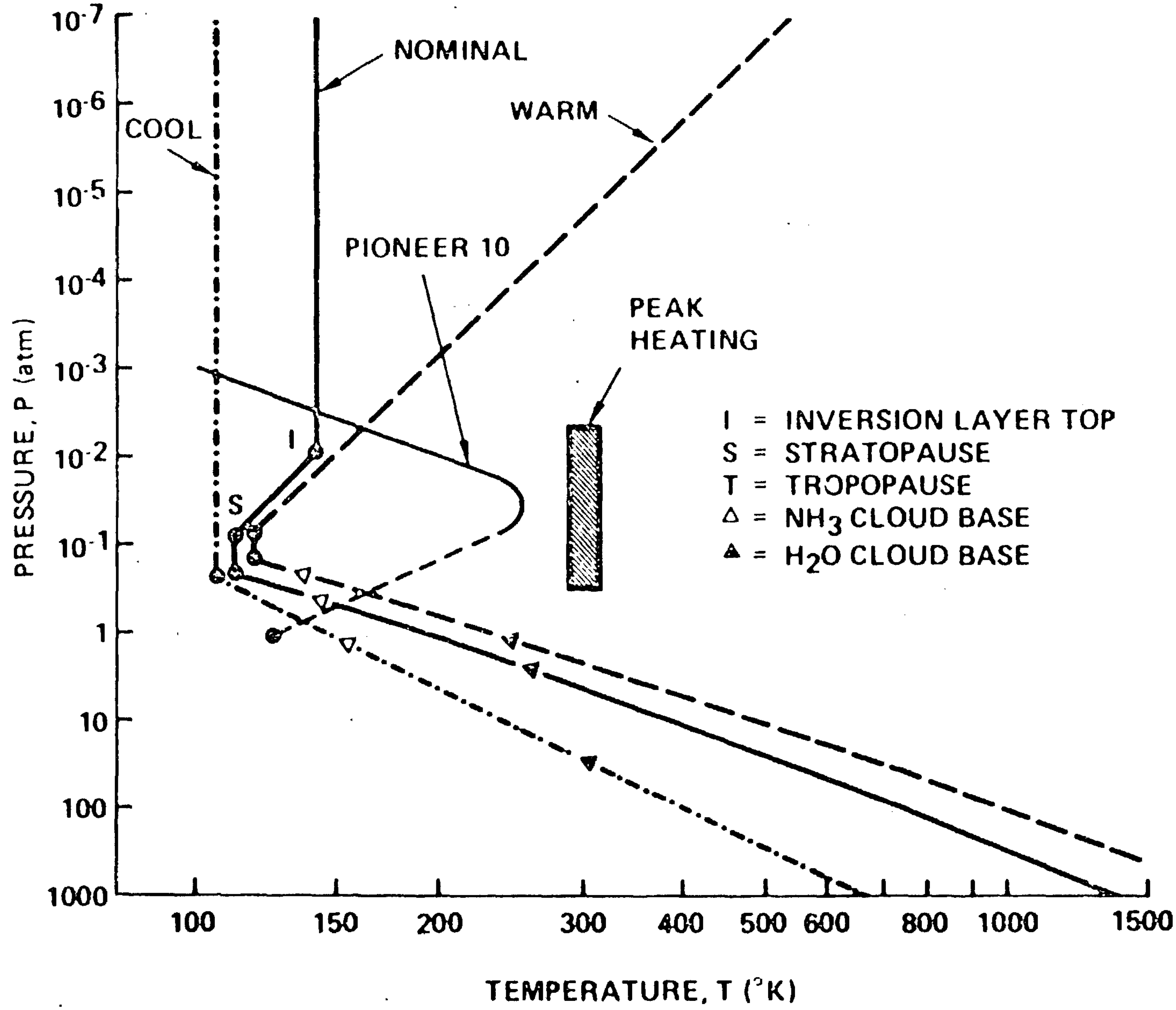


Figure 2-17

heating with regard to an entry probe. So what is happening in the lower atmosphere really doesn't affect the heatshield very much. I have also put on this figure the cool, the nominal and the warm NASA model atmospheres for Jupiter.

I would like you to keep in mind the cool, nominal and warm model atmospheres and, also, roughly the region where the peak heating occurs. With that, I will ask Arv Kliore to discuss some of his results.

DR. ARVYDAS KLIORE: As you know, these occultation measurements contribute to the design of the probe entry structure and heatshield; depending on the warm or cold temperatures at the upper levels of the lower atmosphere. You also know that these measurements are controversial at the moment, because the results don't agree with anybody else's work, and that is not a very good position to be in.

I would like to rapidly go through a discussion of how our results are obtained, and indicate the sort of confidence, or lack thereof, we have in all aspects of the results.

Figure 2-18 shows where the occultation measurements were made. The entry measurement was made in the northern hemisphere on 27° north latitude, between a zone and a belt; just on the sun side of the evening terminator. The exit measurement was made in the north polar area about 59° in latitude, on the dawn terminator.

Figure 2-19 shows the received power level of the signal as the radio beam was entering the atmosphere. There are two things I would like to point out: one is the presence of two signal drop-outs in the region where one expects the ionosphere. This indicates that the probe was far enough behind the planet, in this case about 220,000 kilometers, and that the

PIONEER 10 OCCULTATIONS

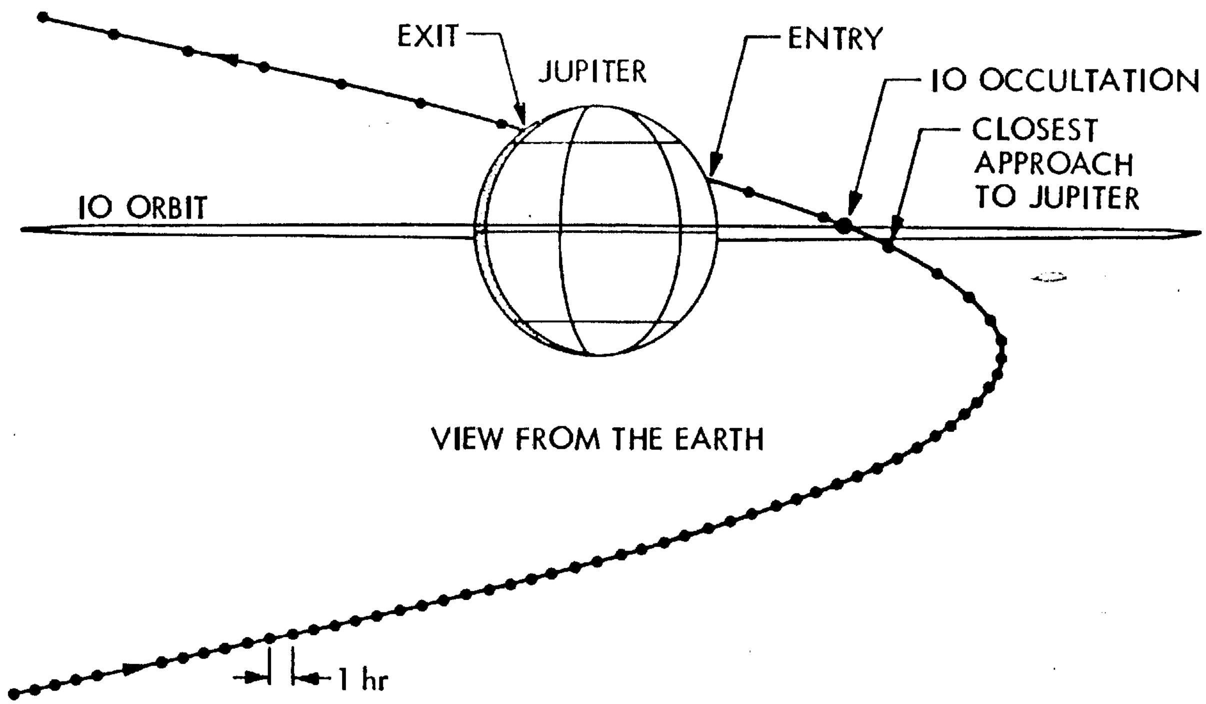


Figure 2-18

PIONEER 10 JUPITER ENTRY

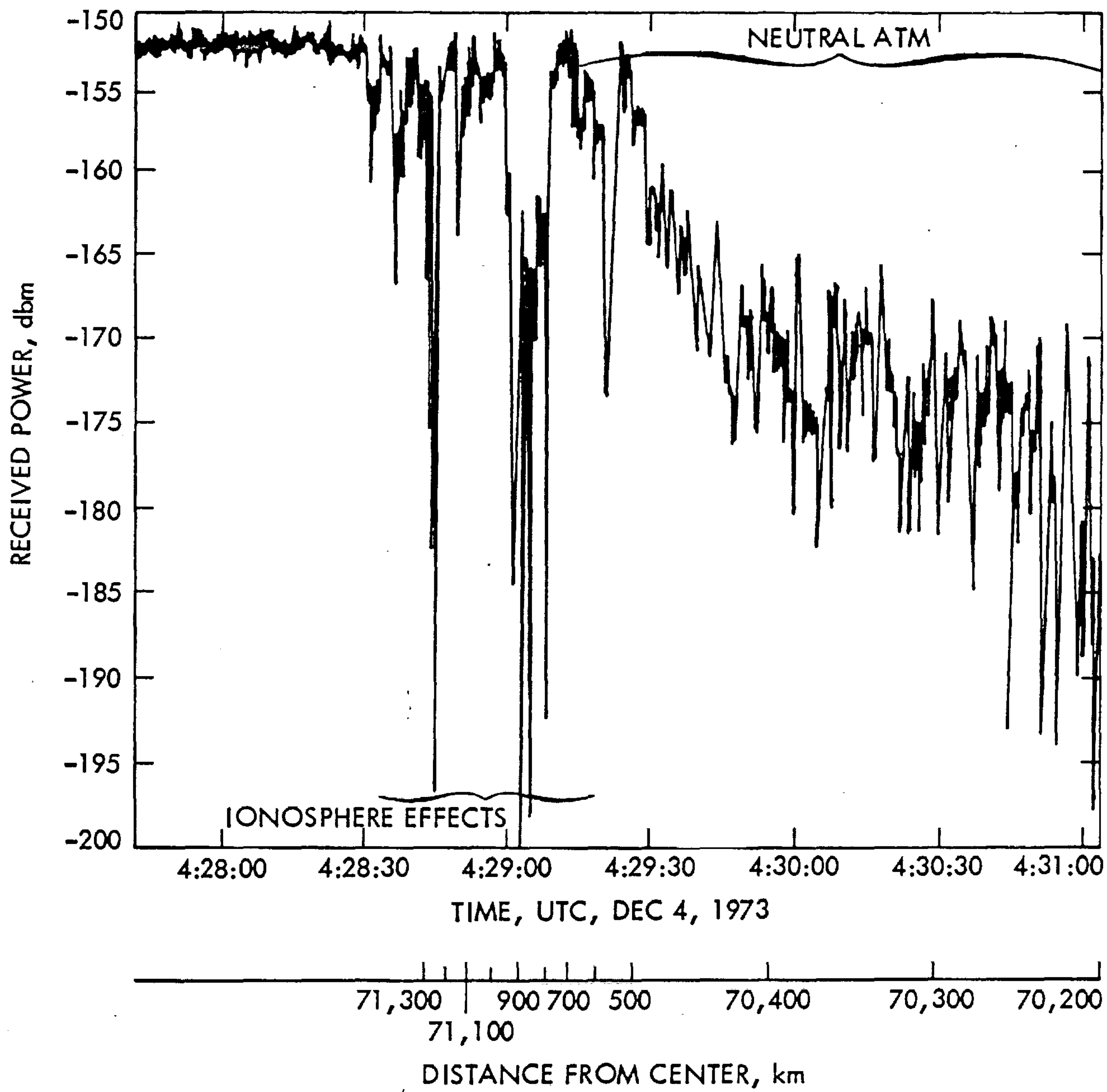


Figure 2-19

ionospheric layers had gradients sharp enough to cause caustics and to induce multi-path propagation.

The other point I want to mention is the long track of the signal in the neutral atmosphere which, as we shall see, corresponds to getting down to pressure levels of two and a half to three atmospheres for nominal-type compositions. This also, I think, indicates that there is less ammonia in the lower atmosphere than we expected because, before the experiment was performed, we thought that with the nominal amounts of ammonia in the atmosphere the signal would be totally absorbed by the time we get to about one half atmosphere. This did not happen; therefore, we think there is less ammonia.

The basic result which we obtained without any assumptions, is the refractivity in the atmosphere, from the phase changes in the signal. We don't use the amplitude because we know it is perturbed by either turbulence or absorption by gases. We know that the phase is affected only by refraction in the atmosphere and should not be affected by the presence of any aerosols, scatterers, or absorbers.

Figure 2-20 is a plot of the refractivity in N units, which is simply the index of refraction minus one x 10^6 as a function of distance from the center.

I would like to point out that this curve is not smoothed. It was obtained by connecting adjacent points obtained at intervals of about a tenth of a second in this case. This corresponds variously to a resolution from about two kilometers to less than a couple of hundred meters in the lower atmosphere.

I would also point out that at the S-Band wavelengths, at a distance of about 220,000 kilometers, the Fresnell zone size which is the effective width of the radio beam as it's passing

PIONEER 10 JUPITER ENTRY

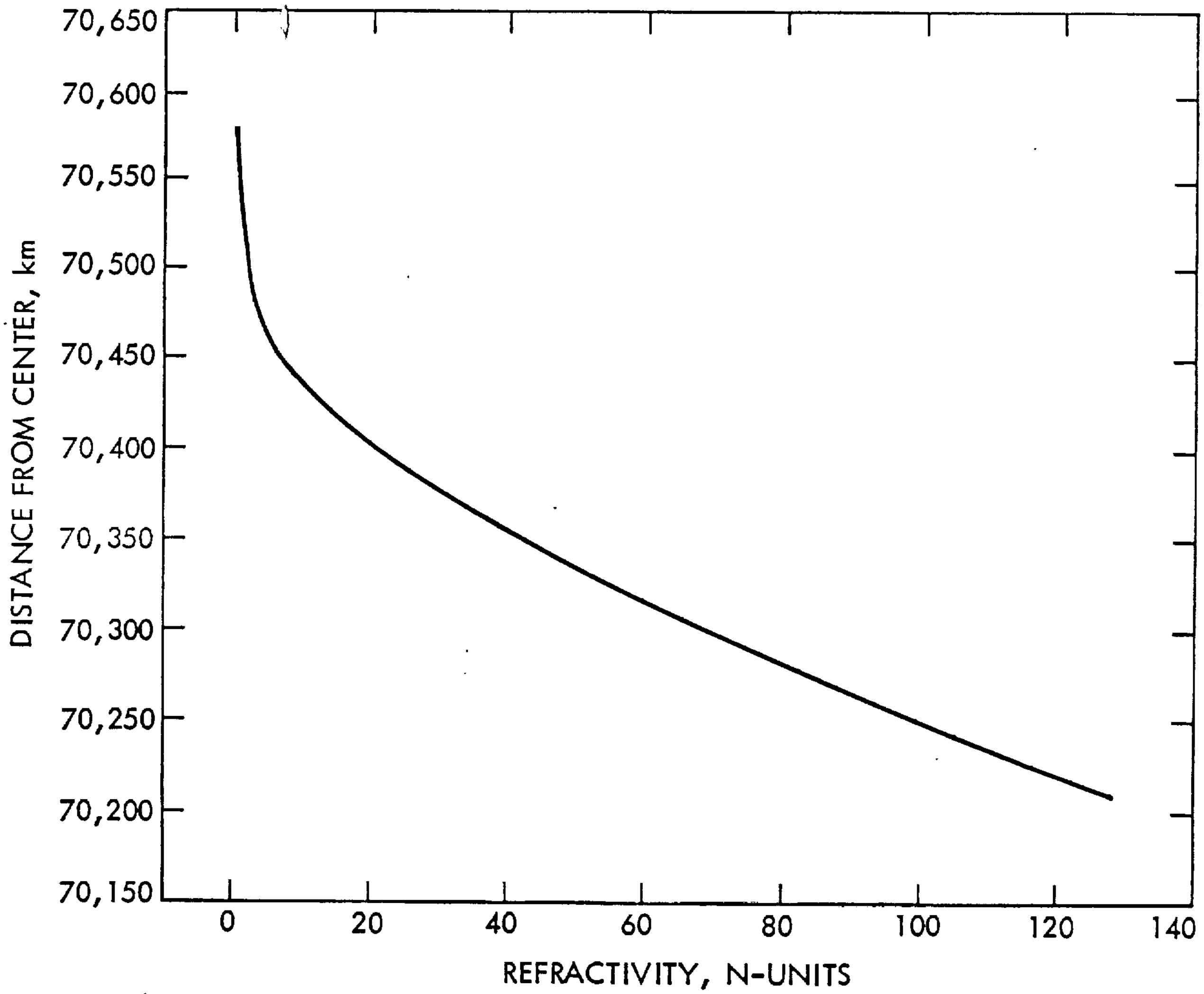


Figure 2-20

through the atmosphere, is about five to six kilometers, so there is an averaging effect in the atmosphere of about five or six kilometers.

DR. DONALD HUNTEN: Arv, can you persuade your computer to re-plot those curves on a semilog scale; it'd be an awful lot more valuable to the rest of us.

DR. KLIORE: Semilog in what direction?

DR. HUNTEN: Log of refractivity versus height.

DR. KLIORE: Well, I can supply you or anybody else with the numerical data in which case you can plot it any way you want. From that point on we must make an assumption of the composition because the refractivity of one gas is different from another, and of course, their molecular weights are different. In order to get properties like temperature and pressure we must first find the density by assuming the composition and then integrate the refractivity, or the density obtained from the refractivity, downward, using the hydrostatic equation to obtain the pressure; then use the perfect gas law to obtain the temperature.

Figure 2-21 shows a temperature profile for a composition of 85% Hydrogen and 15% Helium by number. Also shown are three initial temperatures which we must assume in order to start the integration of the hydrostatic equation. Although I don't show it on this curve, the varying composition between hydrogen and helium does not really make a lot of difference.

Figure 2-22 shows the temperature profile for the early morning or nighttime measurement, at a solar-zenith angle of 94°. The curve has a general characteristic very similar to the daytime one, except that there is no bump in the upper region. I

PIONEER 10 JUPITER ENTRY

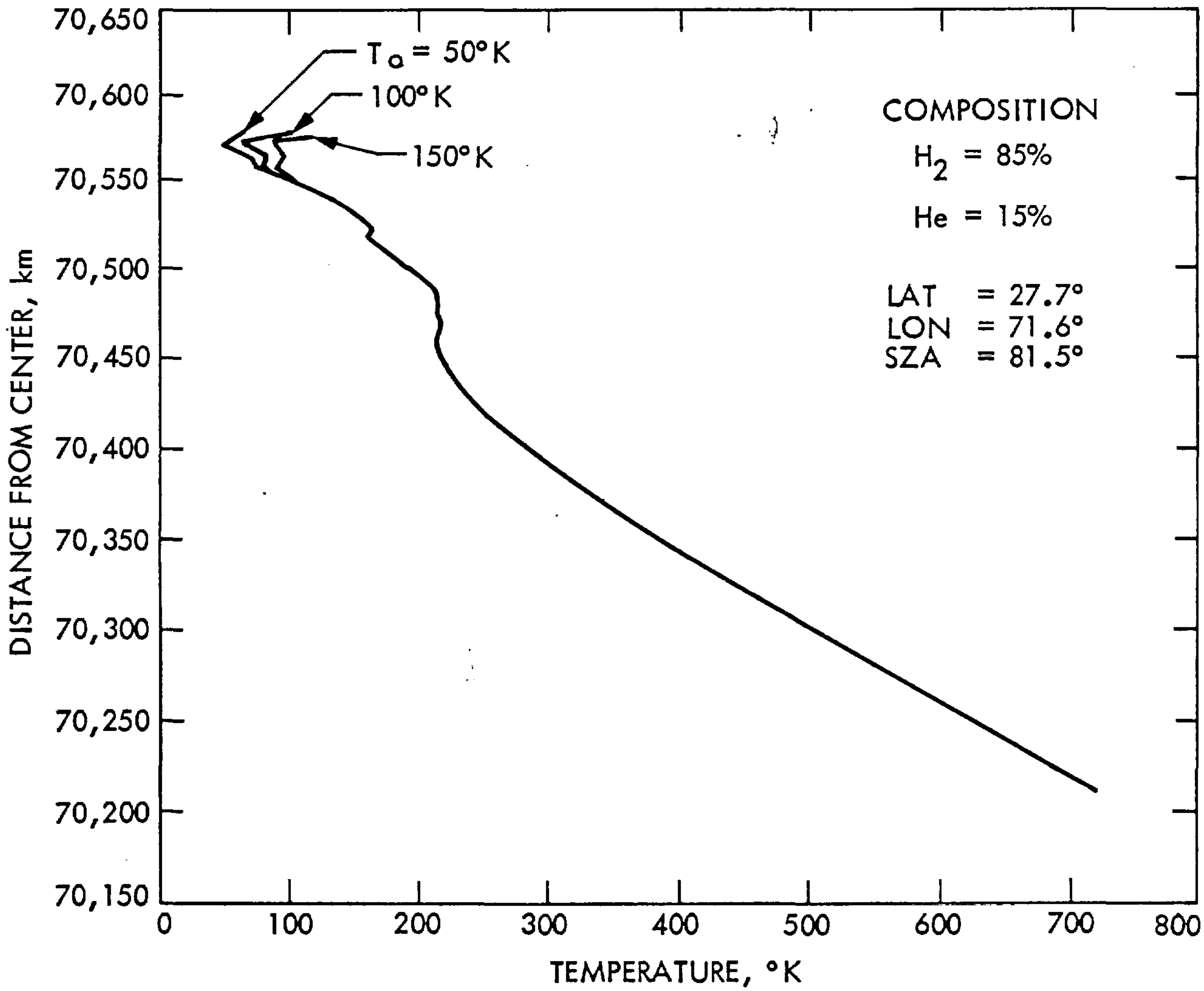


Figure 2-21

PIONEER 10 JUPITER EXIT

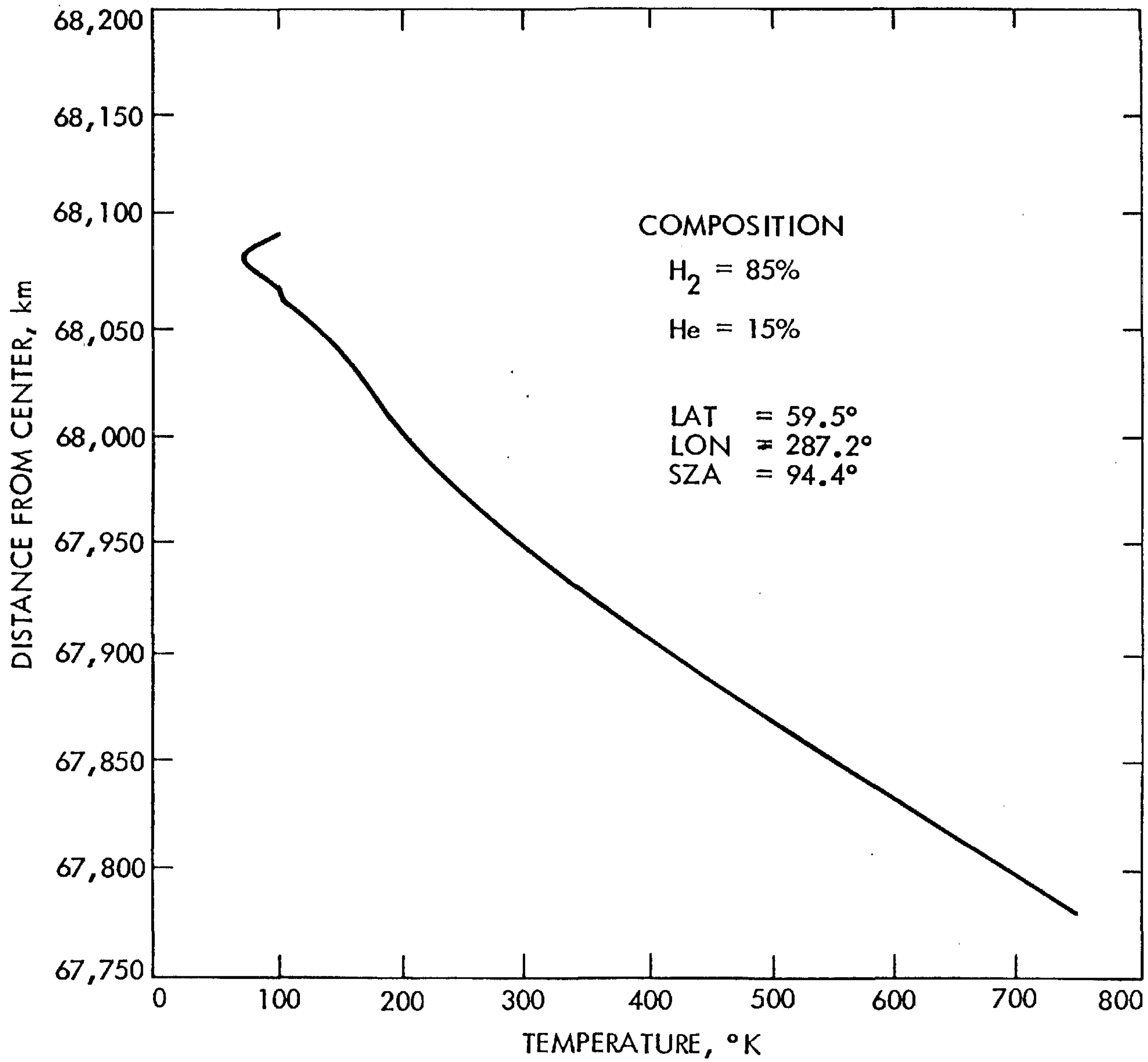


Figure 2-22

interpret the absence of a bump on this curve as an effect of lack of solar illumination. In Figure 2-23 we show these curves plotted on a common scale. There are differences in the lower atmosphere which are caused by the different acceleration of gravity with height at the higher latitude than lower latitude. Because, in the case of Jupiter its rapid rotation is very important in determining the attraction of gravity.

On the left-hand of the figure there is a little box which represents the summary of Earth-based and in this case Pioneer 10 radiometer measurements indicating temperatures of 130° to 150° at about one-half atmosphere of pressure. The cross-hatched area shows the possible extent of a dust or cloud or aerosol layer stretching from about one millibar to fifty millibars. I think there is something there because in the daytime it absorbs solar radiation, causing an increase in temperature of up to about fifty degrees and in the nighttime it does not. There might be some way to interpret the infrared spectroscopy results as being perturbed by multiple scattering and other effects in the cloud layer. That does not, however, take care of the radio observations.

I would like to come back to the composition question. In order to reconcile the temperatures derived from our results with those derived from the spectroscopy, one would have to decrease the refractivity of the mixtures. Our refractivity that we measure should represent more gas than it does. The problem with that is that, assuming pure Hydrogen and Helium, we are using the least refractive gases with the least molecular weight we could possibly have in the atmosphere. The refractivities of Hydrogen and Helium are very low compared to gases like ammonia, methane, carbon dioxide, water, etc. Therefore, whatever one adds to the composition in order to investigate the behavior is not going to make things better; it is going to make them worse.

PIONEER 10 OCCULTATION

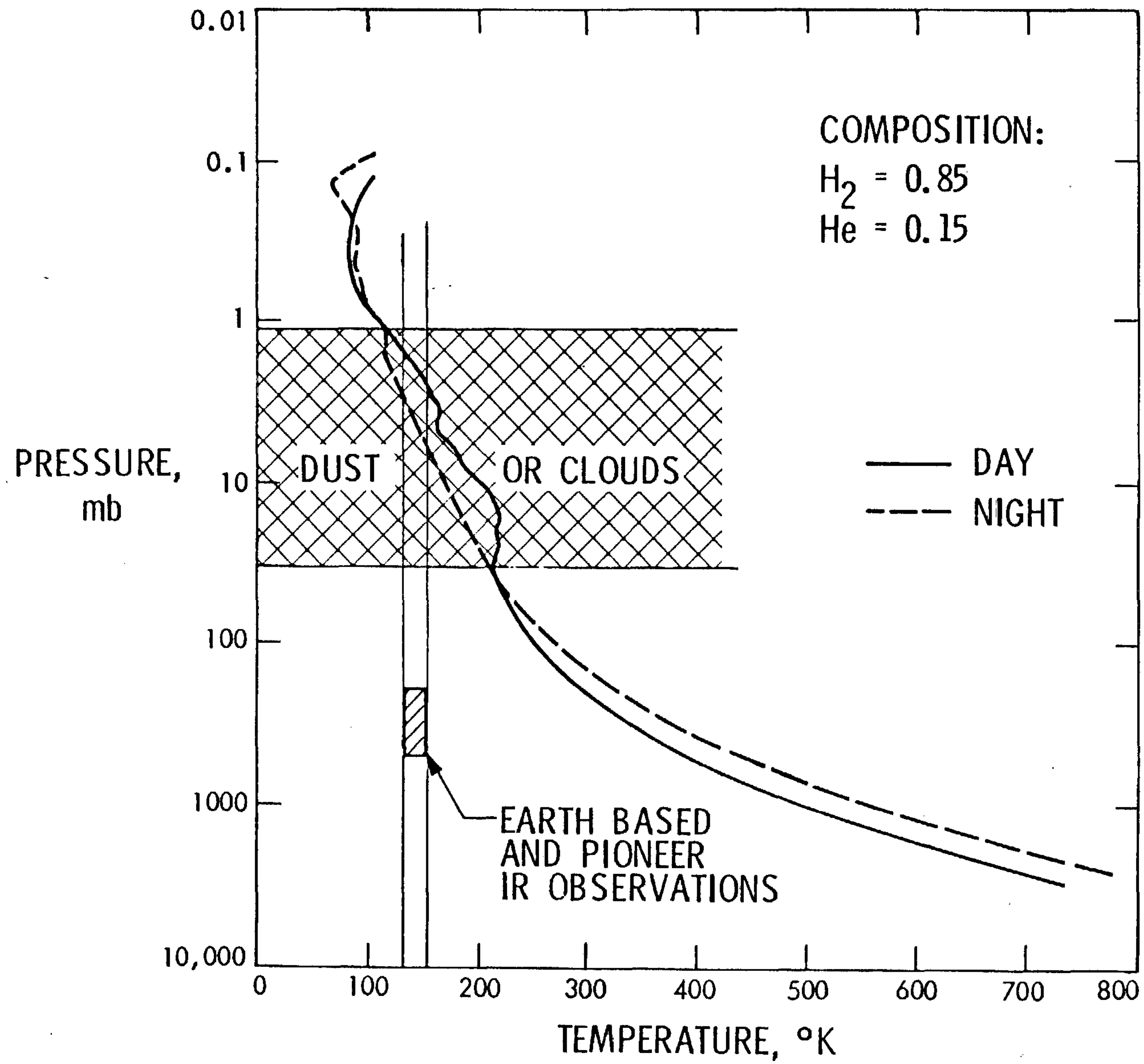


Figure 2-23

One thing we did is to try to adjust the specific refractivities of the gas mixtures; keep the molecular weight the same as Hydrogen and Helium in these amounts, but simply to decrease the specific refractivity of the gas. When we did that, we had to keep decreasing it by a factor of about twenty or so in order to get a temperature of 150°K at 100 to 200 millibars.

So, at the moment there is no way to explain the discrepancy, by adjusting the composition. One of our current jokes is that we have discovered a new element, zeron, which has zero refractivity, behaves as a perfect gas, and has a molecular weight of two.

There have been other possible explanations advanced. One is the presence of ionized particles in the lower atmosphere, mixed with the neutral atmosphere, produced by bombardment by BEV protons, or continuous electrical discharges in a thunderstorm. The problem with that is that even to counteract the presence of about ten n-units of neutral refractivity it would take about a million electrons per cubic centimeter. How these could be produced and kept in equilibrium with a neutral atmosphere is something I would not like to explain, because I don't have an explanation. So, the composition is not the answer. I don't believe it is the ionization hypothesis either. It probably has to do with the fact that the atmosphere of Jupiter is much more complicated than we or the spectroscopists have thought and that the common explanation to both of our results has to take into account more sophisticated models and more sophisticated analysis of data.

Let me just discuss, in support of that hypothesis, the electron density in the ionosphere of Jupiter, which was derived by Dr. Fjeldbo at JPL. The profile shows many peaks. This, to me at least, indicates that there are many species of ions that are creating those sharp layers of electrons and, hence, that there are probably things going on which we don't quite know about. Of course, we can't tell what these ion species are; we are waiting for the probe or a skimmer orbiter to tell us that. Anyway, it is not simple, it's not just hydrogen ionizing at one height.

DR. HUNTEN: It seems a lot like the sporadic E on the Earth, except that it is spread out.

DR. KLIORE: Yes. Well, the entire ionosphere of the Earth would fit in the first 1000 km of the profile.

Okay, let me finish. I would like to suggest, for one thing, that a study of the refractivity at S-Band wavelengths of gases like hydrogen and helium be independently performed at some institution which has the capability for doing so. This would tend to increase our confidence in our results, because now we are using refractivities derived from those measured at optical wavelengths and corrected for radio wavelengths. Other than that, I think we should continue to work together and try to resolve this problem because there is a discrepancy now with which neither we nor the spectroscopists can live, before it's resolved.

DR. HUNTEN: I would like to make a remark while you are transferring. This suggestion that ammonia is even rarer than you expected is an interesting one, too, because that in itself implies that the temperature is relatively low to freeze out the ammonia.

DR. KLIORE: Well, that is one interpretation.

DR. WOLFE: I would like to make some concluding remarks. For example, I think all of us should consider, not only at this workshop but also with regard to mission analysis and NASA future planning, what bearing will Pioneer 11 have on some of the future probe missions. I think I can answer that in a couple of statements here, but we must also consider what Mariner-Jupiter-Saturn in '77 can do for us and, certainly, what can we do with regard to not only groundbased but near-Earth space remote sensing with regard to Jupiter.

I think, from a technology point of view, there are two principal problems with regard to the probe itself. One is the entry problem from the heating point of view where the atmospheric model, of course, is very important. The second one is the trapped particle radiation levels that the probe is going to have to withstand in entering. I think, with regard to the latter, we'll probably be able to get a much better handle on this with Pioneer 11. Right now the radiation belt models from Pioneer 10 are very suspect inside three R_J jovicentric radial distance. We are going in to about 1.6 R_J with Pioneer 11. We are also going around the planet clockwise so we can get a good handle on the higher moments of the magnetic field; and get a good longitudinal survey with regard to the trapped radiation.

We are going to be closer to the planet. I think this may have some bearing on what S-Band occultation will have to say with regard to the ionosphere but I don't think we are going to be able to resolve the IR occultation problems with regard to the upper atmosphere.

And then, finally, I think that the heatshield people should consider the possible effects of a dust layer on entry; what does it do to the heatshield, particularly when it has unknown composition? I think the SX band will give a handle on the ionization with regard to lower levels, although I agree with Dr. Kliore; I don't see how you can get that kind of electron densities down there. So, I don't think that is going to help alleviate the situation either.

I put all these arguments together and it seems to me that if we can support a very, very simple probe on the Pioneer H mission with ESRO which does nothing more than enter and make temperature-pressure measurements it will be exceedingly important with regard to future missions. Thank you.

DR. RASOOL: Thanks, John. Dan Herman

MR. HERMAN: I have one question. It may be an unfair one, but does Guido have any model which tends to reconcile your data and his, any theories?

DR. KLIORE: He hasn't announced any model like that yet, but I do know by having private discussions with him that he cannot interpret his results satisfactorily without invoking some dust or scatterers. However, I don't think it is going to increase his temperature estimates by a factor of two.

DR. RASOOL: The trouble with Guido's results is that I've seen them interpreted by others, but not by him, as yet.

Impact of Science Objectives and Requirements on Probe Mission and System Design

MR. KENNETH W. LEDBETTER: You have heard from previous speakers the basic objectives and rationale for outer-planets probe missions. I would like to build on these basics by discussing some of the problem areas in probe science technology that require a solution before the probe systems can actually be designed.

There are three areas I would like to briefly discuss. First, the effects of the model atmospheres on the probe design; secondly, the effects of implementing the requirements to locate and measure the clouds; and, third, trade-offs between descent sampling and measurement criteria as they affect the probe system design.

Composition is one of the basic objectives and although the probe will measure the actual composition, engineers must have a model with which to design subsystems. The model atmospheres that have been used by both NASA and industry for various studies that have been done are those in the NASA SP series of monographs assembled under the cognizance of Goddard Space Flight Center. The authors for the atmospheric sections were primarily Neil Divine and Frank Palluconi of JPL.

Figure 2-24 lists some of the variant properties of the monograph model atmospheres for Saturn and Uranus. The document numbers are given in the footnotes on the figure. The corresponding number for the Jupiter monograph is NASA SP-8069. Some of the major differences are apparent. Since helium cannot be identified directly from the spectrum, the models are necessarily quite variable in Helium content. It varies extensively at both planets, ranging at Uranus from about 4 percent in the warm to 60 percent in the cool. Adding to this, the variability of methane from a negligible amount at Saturn to 9 percent in the Uranus cool, the resulting molecular weight is between 2.1 and

MAJOR DIFFERENCES IN MODEL ATMOSPHERES

		SATURN[1]			URANUS[2]		
		WARM	NOMINAL	COOL	WARM	NOMINAL	COOL
PERCENT COMPOSITION BY NUMBER	H_2	94.7	88.6	73.0	95.3	85.9	30.6
	He	5.3	11.2	26.3	3.7	11.0	60.0
	CH_4	Trace	0.1	0.2	1.0	3.0	9.0
TEMPERATURE AT 10 BARS ($^{\circ}K$)		424	310	191	300	185	114
PRESSURE AT TOP OF MODELED CLOUDS (BARS)		0.3	0.7	3.0	0.1	0.5	1.0
ALTITUDE DIFFERENTIAL 100 mb to 10 bars	(km)	446	257	132	363	177	75

1. NASA SP 8091

2. NASA SP 8103

Figure 2-24

MARTIN MARIETTA

4.6. Trying to design a probe to this range of atmospheres is extremely difficult and unrealistically restrictive.

The second-most important item on Figure 2-24 is the temperature differential between models at ten bars. It extends from about 114° (Kelvin) in the Uranus cool to over 400° at Saturn; and the Jupiter monograph models show a maximum of about 470°. If you recall Arv Kliore's graph shown earlier, his Pioneer 10 data, extrapolated down to ten bars at the bottom of his graph, would give a temperature on the order of 900° to 1000°. Therefore, there could be as much as an order of magnitude of difference in the final temperature to which a truly common probe must be designed. This, of course, is very significant to both thermal control and to the life of various components of an entry probe.

Figure 2-25 shows the effect of these variations upon entry probe design for Saturn and Uranus with the same set of model atmospheres. Note that the entry ballistic coefficient and the descent ballistic coefficient were essentially constant for all six models. The values are typical for non-parachute probe descents. The slight difference in the descent value is due to the different amounts ablated from the entry heatshield. The peak decelerations vary from a little over a hundred to about six hundred with the entry angles shown. Note that there is a five-degree difference in the entry angle. This allows the design peak G's. specifically about 585, to be about the same for each planet. This flexibility in entry angle permits the designer to account for some of the differences between planets. A Saturn entry at 35° would have greater than 650 peak G's.

Instrument deployment parameters are also shown in Figure 2-25. This particular design was for a non-parachute probe where the instruments were deployed slightly above a hundred millibars in pressure. At three G's descending plus twenty seconds the temperature gauge is deployed, the mass spectrometer opening pyros

ENTRY AND DESCENT SCIENCE MISSION PARAMETER VARIATION

	SATURN			URANUS		
	WARM	NOMINAL	COOL	WARM	NOMINAL	COOL
ENTRY ANGLE (deg.)	→	-30	←	→	-35	←
ENTRY BALLISTIC COEF. (kg/m^2)	→	142.6	←	→	142.6	←
DESCENT BALLISTIC COEF.(kg/m^2)	→	161.3	←	→	160.0	←
PEAK DECELERATION (g)	232	385	585	148	240	570
INSTRUMENT DEPLOYMENT:						
TIME FROM ENTRY TO 3g+20s	96	71	54	121	78	49
MACH NUMBER	.71	.58	.49	.86	.76	.60
ALTITUDE ABOVE 1 BAR (km)	156	86	46	152	84	37
PRESSURE (mb)	63	85	114	39	46	59
TIME TO 10 BARS (min.)	63	43	27	74	47	29

Figure 2-25

are fired, and the nephelometer cover is removed. Again, there are variations in the time from entry, the mach number at deployment, and the altitude above one bar.

The bottom line on Figure 2-25 lists the time to reach ten bars which is also very important for a probe design. It varies from about 27 minutes to 74 minutes; a very large factor when considering thermal control and especially when considering the communications link. The data must be relayed to the spacecraft before it passes out of range of the probe. Also, descent time is important for sizing some of the subsystems, particularly, the power subsystem. In fact, since some components must be designed to the minimum time (e.g. memory dump data rate) while related components are designed to the maximum time (e.g. total battery power) resulting conflicts yield an inefficient design.

It is interesting to note from both Figure 2-24 and 2-25 that the differences between models for a given planet are greater than the differences between planets for a given model, pointing out our overall ignorance as to the real atmosphere.

Of course, we all know we need better models. What can be done to obtain them? Pioneer 10, has changed the essence of these models for Jupiter. In fact, it might be better to discard the old models and start over again. In addition, when progressing from Jupiter to Saturn and Uranus the majority of models that have appeared in the literature have utilized extrapolations from Jupiter. Therefore, when the Pioneer 10 data are fully applied to Jupiter, the results should be extrapolated to Saturn and Uranus.

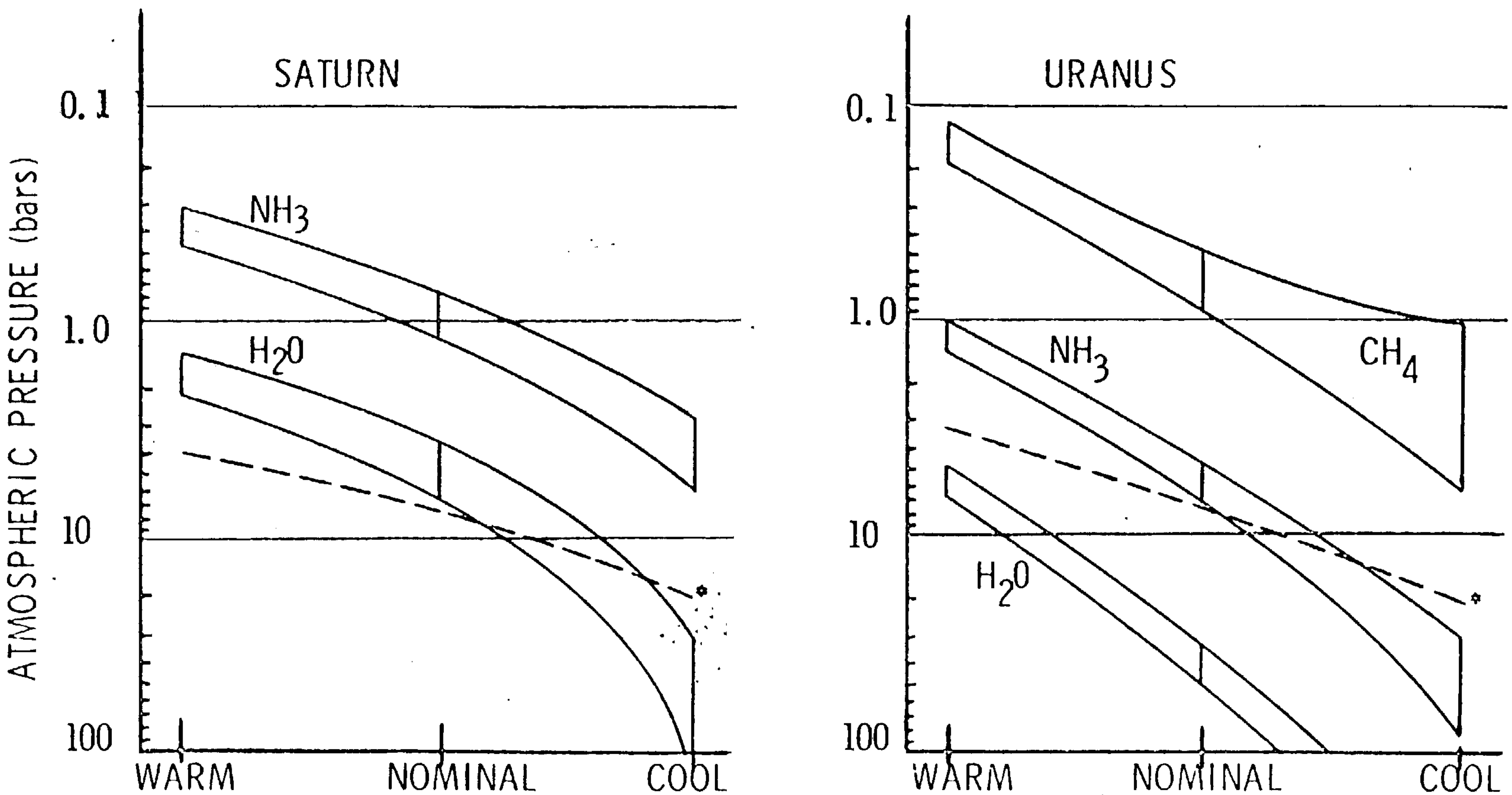
Secondly, statistical means can be used to reduce some of the uncertainty. Starting with a given nominal model and the various 3-Sigma possibilities for each of the individual parameters that comprise the model atmosphere, Gaussian-type distributions can be constructed around that nominal and the extremes decreased. This has been done for Jupiter by W. S. Cook at Martin Marietta. He has a paper appearing in the July, 1974 issue of the Journal of Spacecraft and Rockets which uses the nominal atmosphere from the Jupiter monograph and performs Monte Carlo probabilistic statistics to establish warm and cool limiting models. The results show that Cook's limiting models are less extreme than those in the monograph. This is largely because the monograph models were established with the intent of being worst-case models, therefore, the effects of all worst-case parameters were added together. This means that if a probability distribution were superimposed upon the monograph models, the actual probability of the cool or warm model existing would be near zero since the probability of all parameters being the maximum worst-case value in the same direction at the same time is near zero.

The second topic of discussion is the impact of the basic objective to locate and measure clouds. Figure 2-26 shows the pressure location of the clouds as given in the NASA monograph model atmospheres. The three models are represented by vertical lines as indicated by the abscissa, where for each modeled cloud, the cloud top and the cloud base are shown. The solid lines are smooth fits through the three points, representing the cloud top and the cloud base. The reason for this method of presentation is to emphasize the point that there is only one cloud and that its location is very uncertain, even in these models which the Pioneer 10 data may replace. For example, the water cloud base at Saturn is located between two bars of pressure in the warm and well beyond a hundred bars in the cool.

The dashed line on Figure 2-26 represents the end of a 38-minute mission with a ballistic coefficient of 160 kg/m^2. Note that the probe will just penetrate the cloud base of the second cloud in the nominal atmosphere at about 7 bars. Since the clouds tend to appear higher in the warm models and lower in the cool, the

C-2

CLOUD LOCATIONS AND CONSTANT MISSION TIME



* 38 Minute Mission at B = 160 kg/m^2

Figure 2-26

probe penetrates well past the cloud base in the warm but does not reach the cloud tops in the cool. To penetrate the entire cloud in the cool model is prohibitive.

Therefore, this implies a philosophy of designing to a constant time rather than a constant pressure. This eliminates the problem mentioned earlier of designing to different times for communications, thermal control, and power subsystems. It is also more compatible with the atmospheres themselves since the probe penetrates deeper into the atmosphere in a cool model as do the clouds. The time to reach a given pressure, is a function of ballistic coefficient. The end-of-mission line on Figure 2-26 would basically just move up and down for different ballistic coefficients at different times. (Although for large changes in B, the line would tilt.)

Another important consideration is the difficulty in measuring the high clouds. In the Uranus warm model, the methane cloud is up near a tenth of a bar. The probe has a high velocity at this altitude and low density, and as the atmospheric density increases, it slows down. Figure 2-27 shows that with the indicated ballistic coefficient, the probe spends about seventy-four seconds inside that Uranus cloud. A mass spectrometer with a 1 to 40 amu scan might be lucky to get one measurement inside. For a temperature gauge, to make one measurement per kilometer, the sampling interval would be on the order of about five seconds. Figure 2-27 also shows similar information for the other Uranus modeled clouds.

Thus, a re-evaluation needs to be made of the requirements for measuring the high clouds in any of the outer-planet atmospheres to determine if it is realistic to impose stringent requirements upon the instruments to sample those clouds when the basic objective is to look at the total atmosphere.

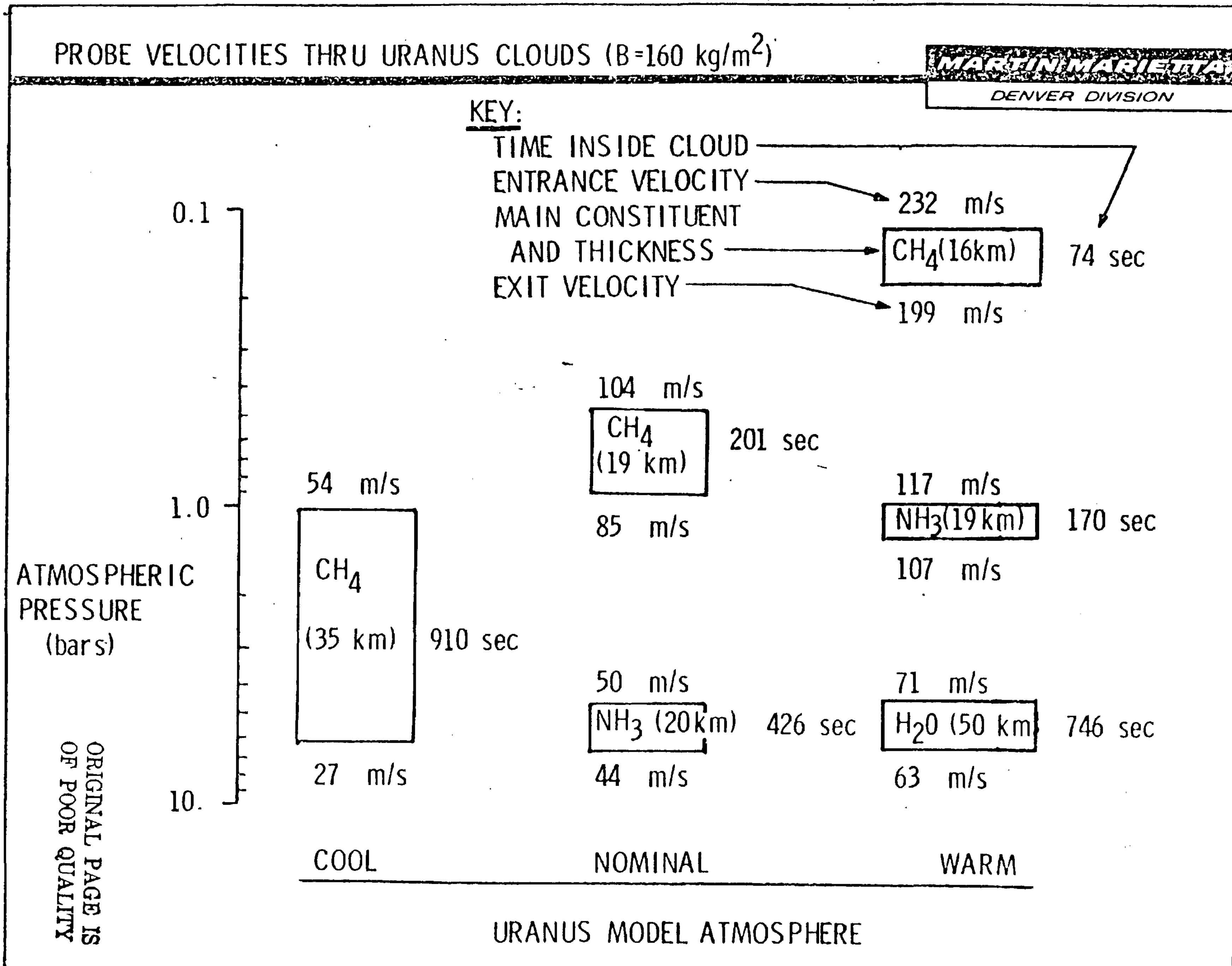

PROBE VELOCITIES THRU URANUS CLOUDS (B=160 kg/m²)
MARTIN MARIETTA
DENVER DIVISION
KEY:
TIME INSIDE CLOUD
ENTRANCE VELOCITY
MAIN CONSTITUENT
AND THICKNESS
EXIT VELOCITY
232 m/s
CH4(16km)
74 sec
199 m/s
0.1
1.0
10.
ATMOSPHERIC
PRESSURE
(bars)
54 m/s
CH4
(35 km)
910 sec
27 m/s
104 m/s
CH4
(19 km)
201 sec
85 m/s
50 m/s
NH3 (20km)
426 sec
44 m/s
117 m/s
NH3(19km)
170 sec
107 m/s
71 m/s
H20 (50 km)
746 sec
63 m/s
COOL
NOMINAL
WARM
URANUS MODEL ATMOSPHERE

Figure 2-27

Figure 2-28 shows the overall trade-offs and related parameters involved in descent sampling. The descent profile, indicated in the left box, is essentially the ballistic coefficient or the rate with which the probe falls into the atmosphere. The sampling criteria or performance in the bottom right-hand box has two meanings: it is criteria before the mission and it is measurement performance after a simulated mission and, hopefully, the performance is equal to or greater than the criteria. The top box is the instrument sampling time or more correctly, the interval between measurements during a descent. It is constrained primarily by the data rate, since there is a maximum amount of data rate available from the power system onboard the probe. If the criteria is fixed and states that the probe must make a given number of measurements in a given altitude differential, the probe can descend fast and have a short sampling time or descend slower and have a longer time. These factors all interplay.

One point to be made from this is brought out by Figure 2-29 and it is that good criteria are needed with which to design. The design criteria directly reflects upon the ballistic coefficient, data rate, and power subsystem. This figure shows three that Martin Marietta has used during contract performance. The first line is one that was used with contract NAS2-7488 with Ames Research Center in 1973 entitled, "Study of Adaptability of Existing Hardware Designs to a Pioneer Saturn/Uranus Probe." The second line is a set of criteria that was obtained from a panel of science consultants that Martin regularly convenes. The third is a set of criteria that was used for Contract JPL 953311 entitled, "Outer Planet Entry Probe System Study" performed for the Jet Propulsion Laboratory in 1972.

For the temperature and pressure gauges, the requirement from set 1 is five kilometers per measurement, that is, one measurement every five kilometers. From the 3rd set, the pressure requirement is one measurement every half a kilometer. There is an order of magnitude of difference between these two requirements. It

FACTORS INFLUENCING DESCENT SAMPLING

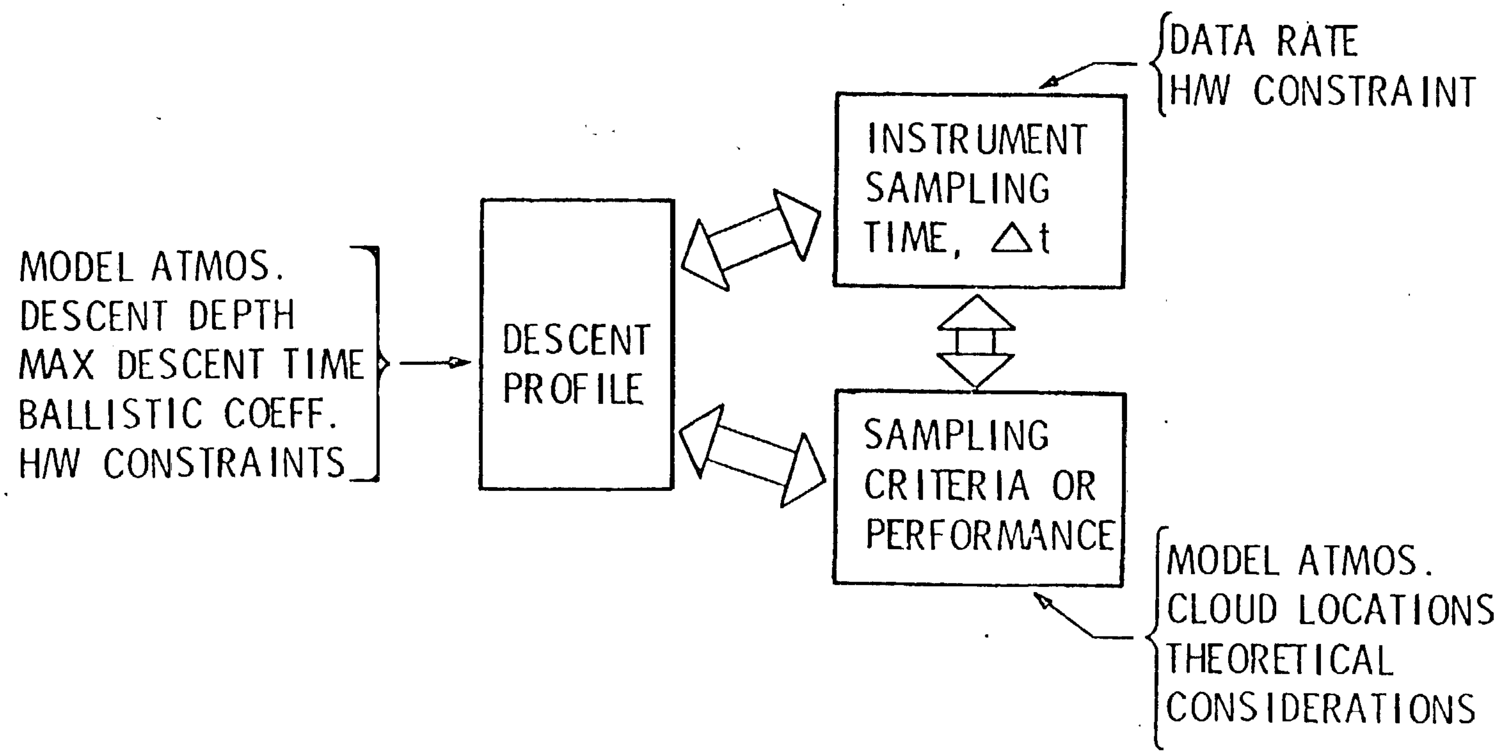


Figure 2-28

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VARIOUS SETS OF PROBE MEASUREMENT CRITERIA

	TEMPERATURE PRESSURE	NEUTRAL MASS SPECTROMETER	NEPHELOMETER
1.	5 km/meas.	6 meas. to 10 bars	1 km/meas.
2.	5-10 meas/scale ht.	4 meas. (2 below 1 bar)	10 meas/scale ht.
3.	P: 2 meas/km T: 1 meas/^{O}K (Below Cloudtops)	2 meas/scale ht. (Below Cloudtops)	1 meas/km (Below Cloudtops)

Figure 2-29

MARTIN MARIETTA

is about a factor of six for the mass spectrometer and, surprisingly, for the nephelometer the requirements are almost identical, when translating a typical scale height.

An improved set of criteria desperately needs to be developed. Perhaps it would be money well spent to employ those principal investigators that will actually receive the data, to determine, perhaps statistically, how close together in the atmosphere the points really have to be measured in order to make a realistic interpretation of the data returned.

The next two figures show additional details of the descent parametrics. Figure 2-30 graphically shows that the measurement performance for a fixed ballistic coefficient and instrument sampling time increases with depth into the atmosphere. This increase is more pronounced with either smaller ballistic coefficients or lower instrument sampling times.

The effects of ballistic coefficient and sampling time variations on performance at a given point in the atmosphere are better shown in Figure 2-31. It displays measurements per kilometer at cloud tops in each of the Saturn model atmospheres versus ballistic coefficient. This is the range of ballistic coefficients for a non-parachute probe. The parachute regime is off the graph to the left and these curves become very much steeper. The third parameter is the instrument sampling time or, again, the interval between samples. Note that with a given ballistic coefficient, changes in sampling time make a significant effect on performance. The solid lines are for the nominal atmospheres; the dashed and dotted lines represent the extremes. The lines indicating four second sampling times illustrate the effect of the three NASA monograph model atmospheres on performance.

The last Figure (2-32) then summarizes the items I feel are important to emphasize. For the model atmospheres: whenever possible extrapolate the Pioneer 10 data to Saturn and Uranus to

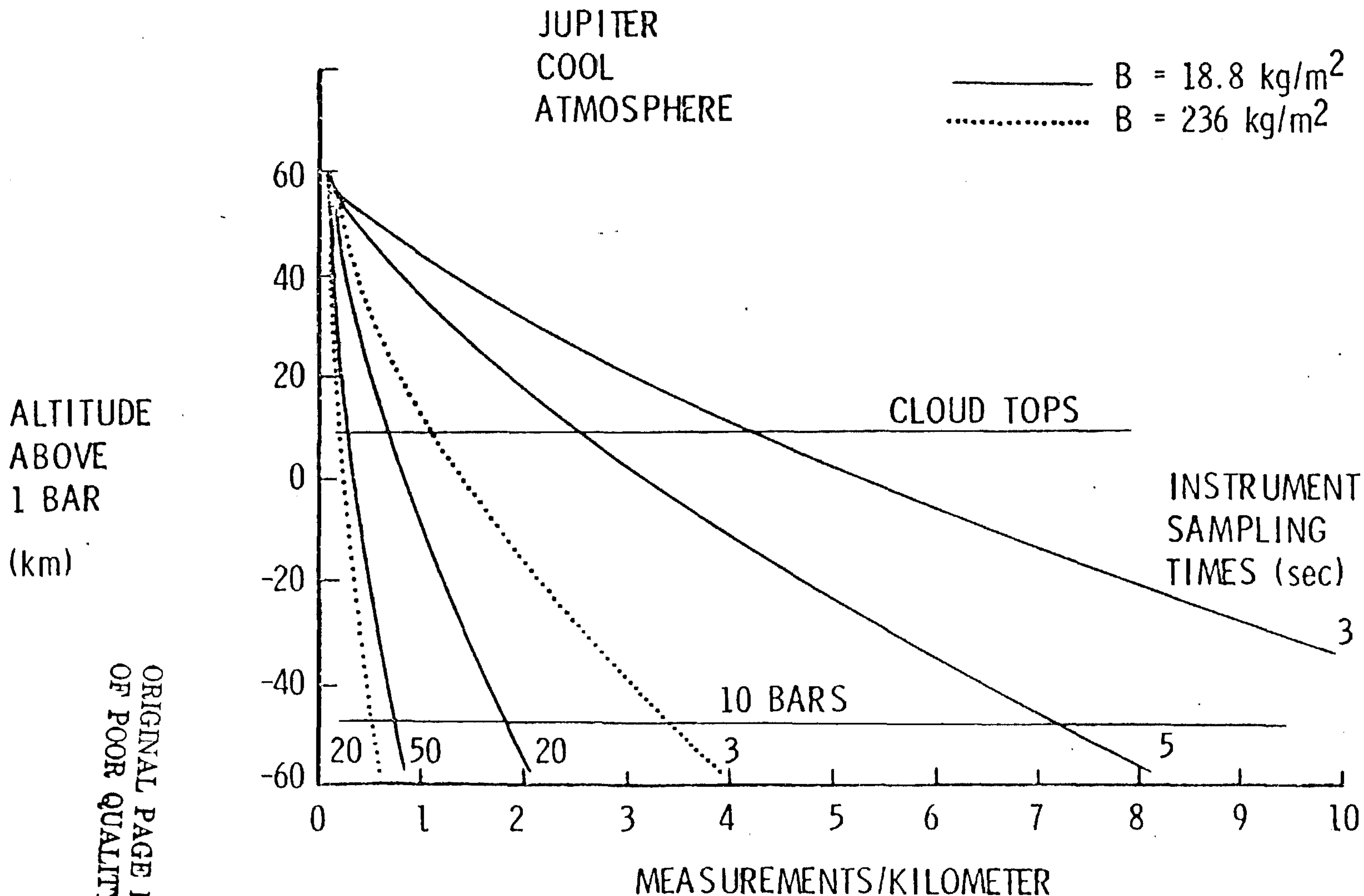

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Figure 2-30

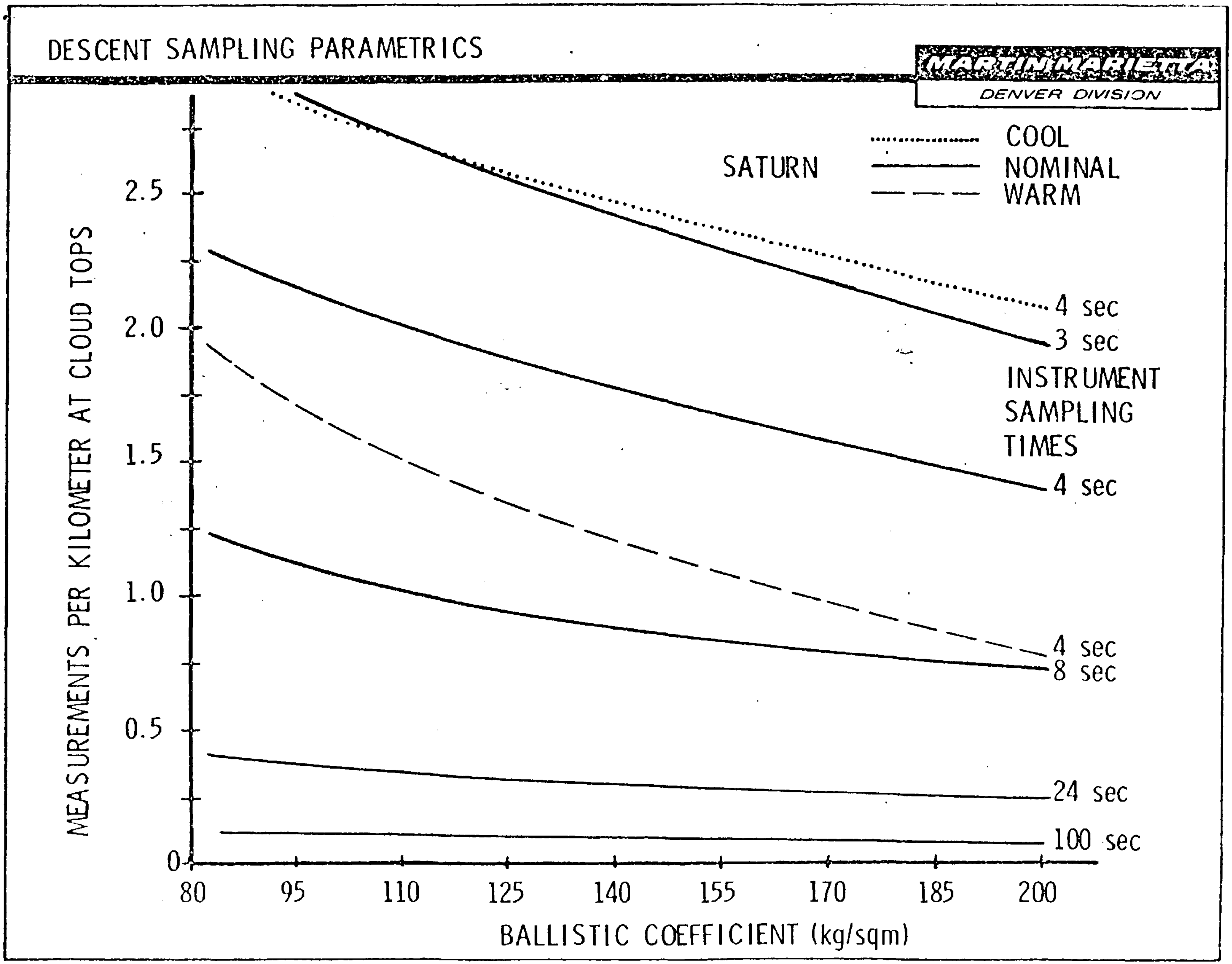
DESCENT SAMPLING PARAMETRICS
MARTIN MARIETTA
DENVER DIVISION
SATURN
COOL
NOMINAL
WARM
MEASUREMENTS PER KILOMETER AT CLOUD TOPS
2.5
2.0
1.5
1.0
0.5
0
4 sec
3 sec
INSTRUMENT
SAMPLING
TIMES
4 sec
4 sec
8 sec
24 sec
100 sec
80
95
110
125
140
155
170
185
200
BALLISTIC COEFFICIENT (kg/sqm)

Figure 2-31

SUMMARY AND CONCLUSIONS

MODEL ATMOSPHERES (IMPROVE TO REDUCE PROBE DESIGN MARGINS)

- o EXTRAPOLATE PIONEER 10 DATA TO SATURN/URANUS
- o USE STATISTICAL ANALYSIS TO REDUCE MODEL UNCERTAINTY

CLOUD LOCATION/MEASUREMENT

- o INSTRUMENTS MUST SEARCH DURING ENTIRE DESCENT
- o MEASUREMENT OF HIGH CLOUDS COSTLY IN DESIGN

DESCENT MEASUREMENT PERFORMANCE CRITERIA

- o NEED STUDY TO ACCURATELY DETERMINE MEASUREMENT REQUIREMENTS

DESCENT DESIGN PHILOSOPHY

- o DESIGN FOR MAXIMUM TIME FOR ALL MODELS
- o BASE REQUIREMENTS ON NOMINAL MODEL WITH EXTREMES AS 3σ LIMITS

Figure 2-32

MARTIN MARIETTA

see what effect this would have on the atmospheres that are currently being used. Secondly, use statistical analysis to reduce some of the model uncertainties to arrive at the best nominal atmosphere possible. Then use statistical analysis and physical relationships in a manner such that the various parameters do not contradict each other when warm and cool atmospheres are derived.

Concerning cloud location measurements, the instruments must search during the entire descent because, for a given cloud, its location is uncertain even in the models currently being used. Also, the measurement of high clouds is costly in design. For descent measurement performance, a set of criteria need to be accurately determined. This, of course, is related to model atmosphere improvement and requires at least a good nominal model atmosphere before this can be satisfactorily done.

Lastly, in descent design philosophy, we recommend designing for a maximum time in the nominal atmosphere, which may be the time to ten bars, but that the overall probe design shouldn't be penalized by going to identical pressures in all models. The requirements should be based on the nominal model and then consider extreme model atmospheres as 3-Sigma limits.

DR. RASOOL: I think Ken made a very important point that we need, much more than ever, communications between the scientists and the people who are designing the mission and, even more so, with the third person involved in between, the model maker. It is not necessarily the scientists who make the models. Usually, there is a time lag of a year and that's very bad because, these days, as you saw, the measurements are being made at a very fast rate. Toby Owen showed some slides which are very interesting, but by the time they get reflected in the model, it's a year or two years. So, we need interaction between the scientists making measurements, the model maker, and the design maker.

MR. HERMAN: Just one comment. At the MJU meeting, Al Cameron stated that it was vital that we reduce the various uncertainties of these models. He felt that these models are unnecessarily unconstrained, which present unrealistic and very complex requirements for the probe design. The models are unnecessarily and unrealistically restrictive and the variables can be reduced.

DR. RASOOL: Ken made another important point; that we have three models of Jupiter and now we have entirely different measurements; and that we should reflect this into Uranus and Saturn.

URANUS SCIENCE PLANNING

Jesse Moore
Jet Propulsion Laboratory

MR. JESSE MOORE: As John Lewis said earlier, Uranus is somewhat of a unique planet in our solar system. I will talk about science planning as related to a mission to Uranus (Figure 2-33).

Specifically, I will talk about the possibility of a 1979 Mariner Jupiter-Uranus mission with the possibility of launching the first outer planet atmospheric entry probe. What I will cover initially, to give you background information, are mission recommendations that have been developed by recent science advisory groups concerned with the type of missions that make sense scientifically, to plan for the outer planets. Then, I will focus on what I call the MJU Science Advisory Committee and talk specifically about the charter, some of the objectives that this group has and some of the outputs that are now emerging. I also will give you a brief summary of where we think we are going from here.

Figure 2-34 presents some of the past advisory groups, and studies that have considered plans for the outer planets over the past couple of years. These certainly are not all; they don't address all the specific things like the Titan Workshop or the Saturn Rings Workshop that have been held. One of the earliest planning groups which existed over a fairly long period of time, was the OPSAG. OPSAG looked at defining a broad program of outer-planet exploration. A Mariner mission to Uranus in 1979 was recommended by OPSAG. Its output was published in the Space Science Reviews in 1973 and it existed for approximately fifteen months. Also, shortly after the OPSAG was initiated, the Space Science Board conducted a Summer Study and in the report of the Space Science Board there was considerable interest expressed in going to Uranus. The Summer Study publication came out in June, 1971.

URANUS SCIENCE PLANNING

- PRESENTATION TOPICS
  - PAST SCIENCE PLANNING GROUPS
    - RECOMMENDATIONS
  - MJU SCIENCE ADVISORY COMMITTEE
    - CHARTER
    - URANUS SCIENCE RATIONALE AND OBJECTIVES
    - PRELIMINARY RECOMMENDATIONS
    - FUTURE ACTIVITIES

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Figure 2-33

JWM
5/20/74

OUTER PLANETS SCIENCE ADVISORY GROUPS

- OUTER PLANETS SCIENCE ADVISORY GROUP (OPSAG)
  - 4/71 - 6/72
  - PUBLICATION: "INVESTIGATION OF THE OUTER SOLAR SYSTEM", SPACE SCIENCE REVIEWS 14, 1973
- SSB SUMMER STUDY, NATIONAL ACADEMY OF SCIENCE
  - 6/71
  - PUBLICATION: "OUTER PLANETS EXPLORATION, 1972-1985," SSB, 1971
- OUTER PLANETS SCIENCE WORKING GROUP (OPSWG)
  - 9/72 - 6/73
- MARINER JUPITER URANUS SCIENCE ADVISORY COMMITTEE (MJUSAC)
  - 12/73 - PRESENT

JWM
5/20/74

Figure 2-34

Following the OPSAG was an Outer Planet Science Working Group (OPSWG) which looked at the work that had gone on previously and recommended various modifications to the programs of exploration.

In December of last year, the Mariner Jupiter-Uranus Science Advisory Committee (MJUSAC) was initiated. Let me now spend a few minutes giving you some of the strategies that came out of these groups and, also, identify the members who participated. My intent here is to illustrate the point of commonality of membership as well as commonality of identifying the Mariner Jupiter Uranus mission as an important mission.

Figure 2-35 presents the membership of the OPSAG group, divided into various disciplines. As you can see, it represented a fairly broad spectrum of the scientific community.

Figure 2-36 shows the recommendations that came from the OPSAG. With regard to the 1979 Mariner Jupiter-Uranus mission two launches were recommended as a logical program to follow the 1977 MJS mission which is currently approved and on-going.

You will also note there was a Pioneer-Uranus entry probe mission planned in 1980, via Saturn. Dan Herman, earlier this morning, mentioned how NASA's plans have changed. Now, the Uranus entry probe is being considered as an integral part of the 1979 MJU flyby. You wall be hearing more during the course of the workshop concerning the mission design and spacecraft design associated with this particular mission.

Figure 2-37 contains the membership list for the OPSWG. I think you can recognize the commonality of membership with the OPSAG. The recommendations from this group came out in two strategies. Strategy A (Figure 2-38) recognized the 1979 Mariner Jupiter-Uranus mission. It also added the Pioneer Jupiter-Uranus mission in 1980 with the Uranus probe.

OPSAG MEMBERSHIP

ATMOSPHERES AND IONOSPHERES

G. MÜNCH, CHAIRMAN
D. HUNTEN
A. KLIORE
J. LEWIS
M. MC ELROY
N. SPENCER
P. STONE

PARTICLES AND FIELDS

J. VAN ALLEN, CHAIRMAN
W. AXFORD
S. GULKIS
C. KENNEL
M. MONTGOMERY
E. PARKER
C. SONNETT
R. STONE
J. TRAINOR

PLANETOLOGY

G. WETHERILL, CHAIRMAN
A. CAMERON
W. HUBBARD
B. MURRAY
S. PEALE

JPL LIAISON

D. REA
J. LONG

ARC LIAISON

B. PADRICK

Figure 2-35

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OPSAG RECOMMENDED EXPLORATION STRATEGY

ON-GOING PROGRAMS

- 1972 AND 1973 PIONEER 10, 11 JUPITER FLYBYS
- 1977 MARINER JUPITER/SATURN (2 MISSIONS)

RECOMMENDATIONS FOR FUTURE MISSION PLANNING

- 1976 PIONEER JUPITER/OUT-OF-ECLIPTIC (1)
- 1979 MARINER JUPITER/URANUS FLYBYS (2)
- 1979 PIONEER ENTRY PROBE TO SATURN
  1980 PIONEER ENTRY PROBE TO URANUS
  VIA SATURN FLYBY (3)
- 1981/1982 MARINER JUPITER ORBITER (2)

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Figure 2-36

OPSWG MEMBERSHIP

DR. J. VAN ALLEN, CHAIRMAN

DR. I. AXFORD
DR. M. BELTON
DR. W. BRUNK
DR. A. CAMERON
DR. W. HUBBARD
DR. J. LEWIS
MR. J. LONG
MR. H. MATTHEWS

DR. G. MUNCH
DR. E. PARKER
DR. I. RASOOL
DR. D. REA
DR. C. SONETT
DR. E. STONE
DR. J. WARWICK
DR. J. WOLFE

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Figure 2-37

OPSWG RECOMMENDED EXPLORATION STRATEGY

STRATEGY A

- 1976 PIONEER H EX-ECLIPTIC (ONE MISSION)
- 1977 MARINER JUPITER/SATURN (2)
- 1979 MARINER JUPITER/URANUS (2)
- 1980 PIONEER JUPITER/URANUS (1)
  - URANUS PROBE
- 1981 PIONEER SATURN DIRECT (1)
  - SATURN PROBE
- 1981/82 MARINER JUPITER ORBITER (2)
- 1982 PIONEER SATURN (TITAN) OR URANUS (1)
  - PROBE AT TITAN OR URANUS

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Figure 2-38

Strategy B (Figure 2-39) was very similar. It however, recommended the 1979 MJU mission with the addition of a Uranus probe on the flybys. It also recommended two launches following the MJS 1977 program.

The remainder of my discussion will be specifically about the MJU mission and the MJUSAC activities. The MJUSAC (Figure 2-40) was asked to develop detail science objectives, rationale and requirements; to quantitatively evaluate payload options and various instrumentation requirements; and to determine the science instruments currently available to meet these requirements.

The final outputs were to develop an advisory committee position on this mission, indicating the scientific value of the addition of the Uranus probe, and to recommend any SR&T developments for the science instrumentation.

Figure 2-41 presents the membership of MJUSAC. It is chaired by Dr. Van Allen with Dr. Al Cameron as Vice-Chairman. Spacecraft and probe inputs for the scientists to consider are being developed while the science objectives, rationale, and payload are evolving. I would like to point out that the spacecraft inputs to this particular group are coming from Ron Toms of JPL and the probe inputs for consideration are being supplied by Ben Padrick and Howard Matthews of Ames. I would like, also, to recognize that Dr. Lewis is a member of MJUSAC.

For the engineers here who may not be familiar with Uranus, I will describe several properties of Uranus (Figure 2-42). It has a very long orbital period, as most of the outer planets do, making the billiard ball or the gravity-assist technique occur in fairly rare opportunities: 1979 is a rare opportunity. The energies and planet alignments are favorable to get a good swing-by of Jupiter to go to Uranus in a reasonable flight time. The

OPSWG STRATEGY - CONTINUED

STRATEGY B

- 1976 PIONEER H EX-ECLIPTIC (ONE MISSION)
- 1977 MARINER JUPITER/SATURN (2)
- 1979 MARINER JUPITER/URANUS (2)
  - URANUS PROBE
- 1980 PIONEER SATURN DIRECT (2)
  - SATURN AND/OR TITAN PROBE
- 1981/82 MARINER JUPITER ORBITER (2)

Figure 2-39

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Figure 2-40

MJUSAC CHARTER (OBJECTIVES)

- FOCUS ACTIVITY ON URANUS SCIENCE
- CONSIDER MARINER-CLASS MISSION ONLY
- DEVELOP:
  - (A) DETAILED SCIENCE RATIONALE
  - (B) QUANTITATIVE SCIENCE OBJECTIVES AND REQUIREMENTS
- QUANTITATIVELY EVALUATE:
  - (A) PAYLOAD OPTIONS
  - (B) INSTRUMENTATION REQUIREMENTS
  - (C) INSTRUMENTATION CURRENTLY AVAILABLE OR UNDER DEVELOPMENT
- DEVELOP SAC POSITION ON 1979 MJU MISSION
  - SCIENTIFIC VALUE OF URANUS PROBE
  - SRT DEVELOPMENTS FOR SCIENCE

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MJUSAC MEMBERSHIP

- DR. J. VAN ALLEN, CHAIRMAN
  U. OF IOWA
- DR. A. CAMERON, VICE-CHAIRMAN
  HARVARD COLLEGE OBSERVATORY

- DR. M. BELTON
  KITT PEAK OBSERVATORY
- DR. W. HUBBARD
  U. OF ARIZONA
- DR. T. JOHNSON
  JPL
- DR. C. KENNEL
  UCLA
- DR. J. LEWIS
  MIT
- MR. J. MOORE, RECORDING SECRETARY
  JPL
- MR. B. PADRICK
  ARC
- DR. D. REA
  JPL
- DR. G. SISCOE
  MIT
- DR. P. STONE
  GODDARD INST. OF SPACE STUDIES
- DR. R. VOGT
  CALTECH

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Figure 2-41

PHYSICAL PROPERTIES OF URANUS

• ORBITAL PERIOD, YR	84.0
• MEAN SOLAR DISTANCE, AU	19.2
• INCLINATION OF EQUATOR, DEG	98.0
• ROTATIONAL PERIOD, HR	10.8
• RADIUS, KM	27,000
• MASS (EARTH = 1.0)	15
• SATELLITES	MIRANDA, ARIEL, UMBRIEL TITANIA, OBERON
∘ RANGE OF RADII, KM	140-1200
∘ RANGE OF SEMIMAJOR AXES, R_U	4.8 - 21.6

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Figure 2-42

distance of Uranus from the Sun is 19.2 AU. Uranus is about twice as far out in the solar system as Saturn, the second planetary target of the 1977 MJS mission. The Pioneer 11 mission also is currently targetted to Saturn as well. One of the unique characteristics of Uranus is its inclination. The equator of Uranus is inclined by 98° which means that as you approach Uranus and its near equatorial satellites, the system appears as a bull's-eye with Uranus at the center. The period of rotation is about ten hours. It is a fairly large planet with a mass about fifteen times that of the Earth. There are five satellites, all within a very compact range. They range from about 4.8 Ru (radius of Uranus) out to about 21.6 Ru. Miranda is closest to Uranus. The satellite radii range from about 140 to 1200 kilometers.

I will now discuss the science rationale (Figure 2-43) and I will summarize very briefly the work that the MJUSAC has accomplished to date. The case for a Mariner Jupiter-Uranus mission can be based primarily on the uniqueness of Uranus; the axial orientation of Uranus; the cosmogonical considerations relating to its origin within the solar system; the unique atmospheric circulation which is likely to result from its axial orientation; and, if it has a dipole field, the characteristics as would be measured by approaching the planet from a head-on position looking at a "pole-on" magnetosphere. Further, the dipole axis would be pointed closest to the Sun at about the time the MJU spacecraft gets to Uranus in 1986.

As John Lewis pointed out, Uranus has a low atmospheric turbulence level, which leads to the conclusion that it apparently lacks an internal heat source, although there is certainly some question on that. One of the other key points of rationale for this mission is that previous groups have stated that the pair of outer planets, Jupiter and Saturn, and the pair Uranus and Neptune form very contrasting bodies. We now have missions that are

URANUS SCIENCE RATIONALE

- CASE FOR MJU BASED ON UNIQUENESS OF URANUS
  - AXIAL ORIENTATION
    - ORIGIN?
    - UNIQUE ATMOSPHERIC CIRCULATION?
    - "POLE-ON" MAGNETOSPHERE? DIPOLE AXIS POINTED NEAREST TO SUN IN 1986?
  - LOW ATMOSPHERIC TURBULENCE (APPARENTLY LACKS HEAT SOURCE)
  - JUPITER/SATURN AND URANUS/NEPTUNE FORM OUTER PLANET PAIRS
  - COMPACT, REGULAR SATELLITE SYSTEM WHOSE COMPOSITION MAY BE DIFFERENT FROM JUPITER AND SATURN SATELLITES

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Figure 2-43

planned or enroute to explore Jupiter and Saturn and a mission to Uranus would certainly give us some data on the other pair of outer planets to compare with the Jupiter and Saturn pair.

Finally, and certainly not of least importance, is the satellite system of Uranus. The satellites are compact. They form a very regular system, and there is considerable speculation that their composition is quite different from the satellites around Jupiter and Saturn.

Figure 2-44 is a generalization of the science objectives that are being formulated in the MJUSAC. From these kinds of objectives, the MJUSAC is formulating the measurement requirements and the payload to meet these particular requirements. The first objective is pointed toward the physical properties of Uranus. Secondly, as John Lewis pointed out, atmospheric characteristics are extremely important with composition probably being the most important. Because of the "pole-on" effect, Uranus may have an exciting magnetosphere and you would like to get very good measurements of its character; you would like to measure the solar wind interaction; and, also, make measurements within the ionosphere. For the satellites, their masses, radii, topography, and rotational period are extremely important determinations. Because of the distance of Uranus, understanding the satellite properties is difficult to do from Earth-based observations. Finally, you would like to measure the interstellar/interplanetary media. This mission will go out to about 20 AU, possibly beyond, and certainly data in that region would add to the base of knowledge we expect to acquire over the next several years from Pioneer 10 and 11 and MJS77.

Figure 2-45 presents the measurement categories that the MJUSAC is developing. On the flyby science we are talking about conducting imaging experiments; experiments both in IR and UV spectral

URANUS SCIENCE OBJECTIVES

- URANUS PHYSICAL PROPERTIES
  - MASS, RADIUS, ROTATIONAL PERIOD, GRAVITATIONAL FIELD
- ATMOSPHERIC CHARACTERISTICS
  - COMPOSITION (MOST IMPORTANT), TEMPERATURE, DYNAMICS, STRUCTURE
- PARTICLES AND FIELDS ENVIRONMENT
  - MAGNETOSPHERE, SOLAR WIND INTERACTION, IONOSPHERE
- SATELLITES
  - MASSES, RADII, TOPOGRAPHY, ROTATIONAL PERIOD
- INTERPLANETARY/INTERSTELLAR MEDIA

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Figure 2-44

URANUS SCIENCE MEASUREMENT CATEGORIES - PRELIMINARY

1979 MJU (W PROBE)

FLYBY SCIENCE

- IMAGING
- IR, UV
- MAGNETIC FIELD
- PLASMA
- ENERGETIC CHARGED PARTICLES
- R. F. RADIO SCIENCE

PROBE SCIENCE

- TEMPERATURE
- PRESSURE
- ACCELERATION
- ATMOSPHERIC COMPOSITION
- CLOUD STRUCTURE
- SOLAR RADIATION

Figure 2-45

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ranges; experiments associated with the magnetic field; plasma experiments; charged-particle experiments; and S- and X-Band occultation measurements as the spacecraft encounters Uranus.

In the probe arena I think you have heard earlier about the particular measurements listed here. I think it is very important as Dan Herman pointed out that NASA is planning to formulate a specific science group to address the Uranus atmospheric question in-depth and, subsequently, define in more detail the probe payload. The data generated by this group will be used to plan a Phase B probe activity beginning in July 1975.

To develop a scientifically viable MJU mission, it is mandatory that flyby and probe science measurements be complimentary in nature. Data from the probe and flyby spacecraft science instrumentation should be designed to contribute uniquely to the total integrated science return.

My final figure (Figure 2-46) describes the current activities and future plans of MJUSAC. We are in the process of getting more specific inputs on the payload options and the instrument requirements, and in the process of developing final MJUSAC recommendations. I will comment that the MJUSAC has strongly endorsed the 1979 Mariner Jupiter-Uranus mission with an atmospheric entry probe of Uranus. I think the outputs of this technology workshop will certainly serve as valuable input to the planning and further development of the 1979 mission possibility. As far as our future plans are concerned, the scientists under the direction of Dr. Cameron, are planning a publication in the fall of this year in Icarus on detail science rationale, objectives and requirements; the MJUSAC is preparing a final report which will appear in draft form in early August. This report will integrate both the science work as well as the mission, spacecraft and probe design work.

MJUSAC CURRENT ACTIVITIES AND FUTURE PLANS

- CURRENT ACTIVITIES
  - DEVELOPING PAYLOAD OPTIONS AND INSTRUMENTATION REQUIREMENTS
  - DEVELOPING FINAL COMMITTEE RECOMMENDATIONS

- FUTURE PLANS
  - PREPARE ARTICLE FOR PUBLICATION IN *ICARUS*
    - "URANUS SCIENCE RATIONALE, OBJECTIVES AND REQUIREMENTS"
  - PREPARE FINAL REPORT WHICH INCLUDES SCIENCE, MISSION DESIGN, SPACECRAFT AND PROBE DESIGNS

Figure 2-46

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Again, I wish to say that I think the probe workshop will provide some very valuable inputs to the MJUSAC and we are looking forward to seeing the outputs.

SCIENCE PAYLOAD

H. Myers
McDonnell Douglas Astronautics

H. MYERS

An outer planet entry probe has two very basic science objectives. One is the determination of atmospheric structure andthe other the determination of atmospheric composition.

ATMOSPHERIC STRUCTURE

With regard to structure, the general approach is that of measuring density with an accelerometer and pressure and temperature with pressure and temperature gages. This is an idea that was first advocated by Al Seiff here at Ames Research Center. It has been tested out very successfully in the Planetary Atmosphere Experiment Test (PAET) Program. It is being implemented on Viking to Mars, and the Russians used a similar procedure in their exploration of Venus.

Accelerometer - The objective of the accelerometer experiment is the measurement of the aerodynamically induced acceleration of the entry probe by the planetary atmosphere. The aerodynamic acceleration is directly proportional to the ambient atmospheric density. The density, ρ, is determined from the component of acceleration along the flight path, a_s:

$$\rho = \frac{-2}{V^2} \frac{M}{C_D A} a_s,$$

where M, V and $C_D A$ are the mass, velocity and aerodynamic drag area of the probe.

In the upper atmosphere, density data is available only from the accelerometer measurements. In the lower atmosphere the accelerometer measurements are enhanced by direct measurements of atmospheric temperatures and pressures. The independent data on temperature, pressure and density are combined statistically with probe trajectory data to yield the best estimate of atmospheric structure profiles.

Accelerometer data are acquired from the beginning of the sensible atmosphere to the end of the mission within the troposphere. The minimum interpretible value from the accelerometer is 4×10^{-4} G_E, which for an entry probe occurs at 656 km above the 1 atm level for the nominal atmospheric models of the Outer Planets. The probe traverses the upper atmosphere at relative velocities up to 30 km/sec; therefore, a high sampling rate is required to trace out the density profile. The analog output of each accelerometer transducer is sampled at the rate of 5 samples/sec. After peak deceleration, when the probe has slowed to subsonic velocities, the accelerometer sampling rate is reduced to 0.02 samples/sec.

The accelerometer unit is a self-contained package that consists of three orthogonally mounted accelerometers and their supporting electronics. It is a modified version of one used on the PAET vehicle. Each transducer is a single-axis, pendulous proofmass transducer which uses a capacitive bridge pickoff to detect the acceleration forces acting on the proofmass. The electromagnetic force required to maintain the proofmass in its null position is a direct measure of the aerodynamic forces exerted on the probe by the atmosphere. This type of accelerometer can measure deceleration in the desired range (.0004 to 800 G_E).

The characteristics of the accelerometer package are listed in Figure 2-47. The accelerometers are aligned orthogonally and assembled in a rigid structure. The package is mounted so that the longitudinal accelerometer lies along the center line of the probe with the proofmass as close as possible to the probe's center of gravity.

The accelerometers are energized on command of the data handling subsystem (DHS) programmer about 40 minutes before the anticipated occurrence Of -0.01 G_E acceleration. The analog output of the accelerometers are sampled by the DHS processor at 5 samples/sec until the probe experiences -2 G_E acceleration after peak deceleration. From the -2 G_E level to the end of the mission, the data is sampled at 0.02 samples/sec. In order to attain a high level of precision in the upper atmosphere density measurements, the longitudinal accelerometer is provided with three range scales; 0 to -0.1 G_E, 0 to -10 G_E, and 0 to -800 G_E. Range switching is activated by the accelerometer electronics. Two bilevel outputs are included to indicate when a range change has occurred.

The outputs of the accelerometer are 0 to 5 VDC analog signals, which are digitized by the DHS processor. The longitudinal signal is quantized into 10 bit words, the lateral signals into 7 bit words. The upper atmosphere data are stored and transmitted (interleaved with real-time science and engineering data) after radio frequency blackout.

FIGURE 2-47

ACCELEROMETER CHARACTERISTICS

RANGE: LONGITUDINAL 0 TO $-0.1g_E$, 0 TO $-10g_E$, 0 TO $-800g_E$
LATERAL: +10 TO $-10g_E$
ACCURACY: 0.01% OF READING
SIZE: 5 x 4.5 x 4.5 CM (SENSORS PLUS ELECTRONICS)
VOLUME: 101 CM^3, (6.2 IN^3) (SENSORS PLUS ELECTRONICS)
WEIGHT: 0.3 KG, (0.66 LB) (SENSORS PLUS ELECTRONICS)
POWER: PEAK: 8.2W FOR 20 SEC; AVERAGE: 2W
DATA OUTPUT: 0–5 VDC DIGITIZED BY DATA HANDLING SUBSYSTEM

DATA RATE:		WORD SIZE (BITS/WORD)	SAMPLE RATE (WORDS/SEC)	DATA RATE (BITS/SEC)
HIGH RATE	LONGITUDINAL	10	5	50
	LATERAL	7	5	35
LOW RATE	LONGITUDINAL	10	0.02	0.2
	LATERAL	7	0.02	0.14

FIGURE 2-48

PRESSURE GAGE CHARACTERISTICS

RANGE: 0 TO 20 ATM IN FOUR RANGE SCALES WITH FULL-SCALE VALUES OF 0.1,5,10 AND 20 ATM, RESPECTIVELY
ACCURACY: ±0.2% OF FULL SCALE
SIZE: 3.8 CM DIA x 16 CM (SENSOR + ELECTRONICS)
VOLUME: 181 CM^3, (11.1 IN^3)
WEIGHT: 0.2 KG, (0.44 LB)
POWER 1.2W AVERAGE
DATA OUTPUT: 0–5 VDC DIGITIZED BY DHS

DATA RATE: WORD SIZE (BITS/WORD)	SAMPLE RATE (WORDS/SEC)	DATA RATE (BITS/SEC)
10	0.02	0.2

INSTRUMENT HISTORY: HARPOON

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Pressure Gage - The objective of the pressure gage measurements is to obtain atmospheric pressure profiles for the troposphere of the Outer Planets. The pressure measurements are made of the stagnation region of the probe. The thermal limits of the sensor restricts the pressure measurements to the lower atmosphere where the probe velocity is subsonic. At the beginning of the measurement regime, the ambient and dynamic pressures are approximately equal in the total pressure measurement of the sensor:

$$P_T = P_\infty + \frac{1}{2} \rho_\infty V^2_\infty$$

where P_∞ is atmospheric pressure, ρ_∞ is ambient density, and V_∞ is velocity. Therefore, accelerometer data are needed to determine probe velocities and ambient densities. These parameters are required in order to derive ambient pressure from the gage data. As the probe approaches its terminal velocity, the dynamic pressure correction to the measurement becomes very small and is neglected. The properties of the pressure gage are given in Figure 2-48.

A capacitive type of sensor is employed because it monitors a wide range of pressure in a single instrument. The pressure gage is a single unit that contains four pressure transducers and a common electronics package. The transducers are in the form of pressure sensing capsules; each capsule is sensitive to a different pressure range. The full-scale values of each capsule are 0.1, 5, 10, and 20 atm, respectively. Automatic range switching occurs from one capsule to another as the pressure profile is traversed.

The circuitry for the pressure gage is given in Figure 2-49. The change in capacitance generated by a change in pressure within the sensing capsule is converted to a high level DC voltage by the signal conditioning electronics. The voltage reference circuit regulates the oscillator and other circuits.

A controlled oscillator, consisting of a control amplifier, a feedback network and an oscillator, excites the capacitive sensing capsule with a closely controlled alternating current voltage. The detector develops a low level DC signal proportional to the excitation and the capacitance of the sensing capsule. A low level signal from the detector then goes to an amplifier that develops the high level DC voltage output signal.

CIRCUITRY FOR THE PRESSURE GAGE

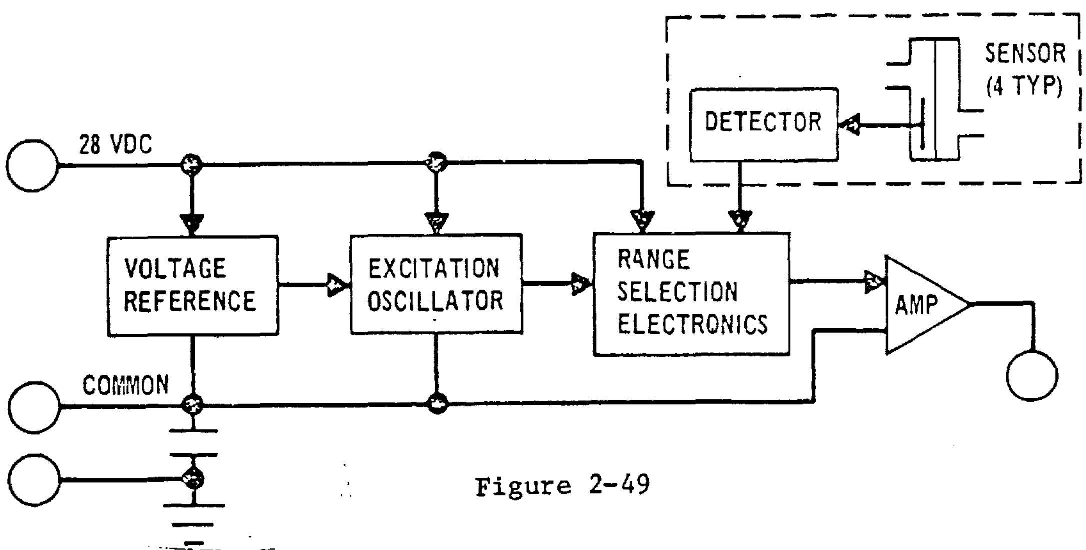


Figure 2-49

The inlet port of the pressure gage is colocated within the mass spectrometer inlet probe assembly in the sampling probe of mass spectrometer system. Pressure measurements are initiated at -2 G_E (after peak deceleration) with deployment of the mass spectrometer sampling probe in order to avoid high Mach number shock wave effects. The output of the pressure gage is an analog signal in the 0 to 5 VDC range. The output signal is sampled once every 50 seconds and is digitized into 10 bit words by the data handling subsystem.

Temperature Gage - The objective of the temperature measurement is the determination of atmospheric temperature profiles in the tropospheres of Saturn and Uranus. The atmospheric temperature measurements are made by deploying the temperature gage directly into the probe flow field. The measurement regime is therefore limited to the lower atmosphere, where local flow field conditions do not exceed the thermal limits of the gage.

The sensing element of the temperature gage is a platinum resistance wire. To provide snsor redundancy, the temperature gage contains two platinum elements in a single housing. The two elements are connected in parallel to one resistance bridge. The circuitry is designed so that, when both platinum elements are operational a 0 to 2.5 VDC output range is obtained. Should one element open, the output voltage range immediately goes to 0 to 5 VDC and the voltage output for a given temperature jumps to twice the previous value. In order to determine the appropriate scale factor, the DHS programmer sends a command to the temperature gage immediately after sensor deployment which introduces a calibrated birdge resistance in parallel with sensing elements. The change in output signal identifies the scale factor to be used in data reduction. Experimental data from similar total temperature sensors have produced a maximum response time of 0.5 seconds. The response is dependent on Mach number and pressure. The lag time decreases as atmospheric pressure increases.

The temperature gage consists of two components, the deployable sensor unit and the electronics package, and is typical of platinum wire sensors used in many space probes except for deployment technique. The physical properties of the gage are given in Figure 2-50. Before deployment, the sensor unit is positioned behind the forward heat shield in the vicinity of the probe maximum diameter. The gage deployment is accomplished by means of a preloaded spring, which is released on command of the DHS when the probe attains the -2 G_E level (after peak deceleration). Upon deployment, the sensor unit is located in a region of high local dynamic pressure within the flow field. The sensor is extended approximately two centimeters beyond the probe boundary layer.

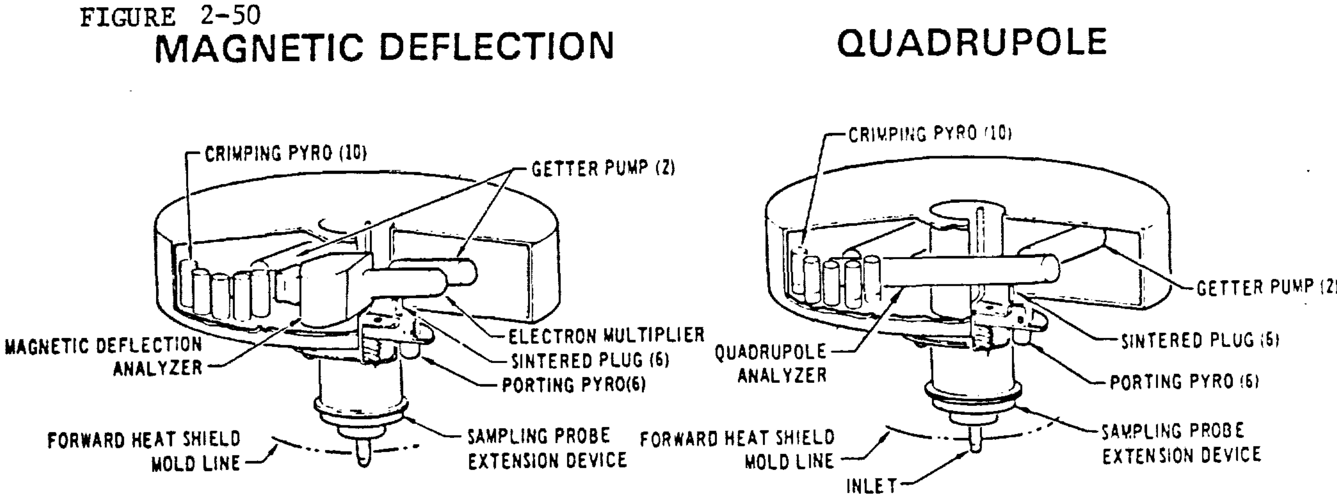
The output of the temperature gage is an analog signal in the 0 to 2.5 VDC range (or in the 0 to 5 VDC range on the failure of one sensor element) which is sampled once every 50 seconds. The analog signal is digitized into 10 bit words by the DHS processor prior to transmission.

ATMOSPHERIC COMPOSITION

With regard to composition, the most important instrument, in terms of probe-design impact, is the mass spectrometer. Additional correlatable data are provided by an ion spectrometer, radiometer and nephelometer.

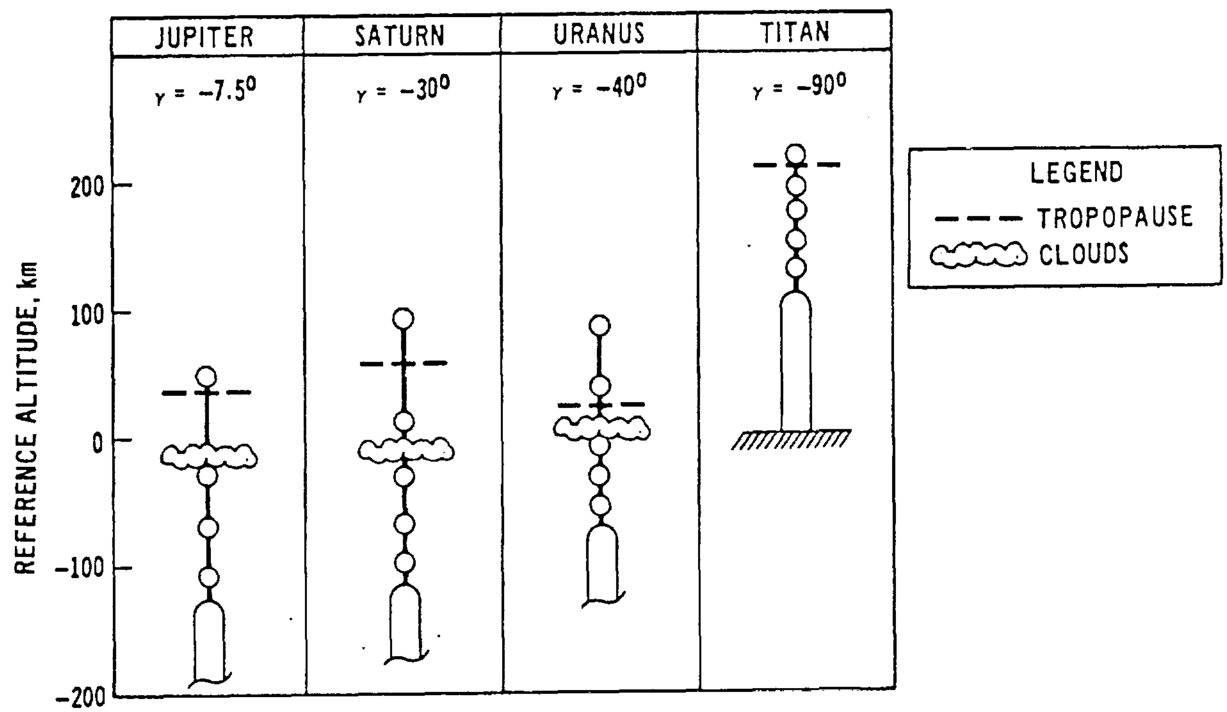
Neutral Mass Spectrometer - The neutral atmosphere mass spectrometer and sampling system (Figure 2-50) is a self-contained unit that acquires Outer Planet atmospheric samples and determines their chemical composition. The integrated instrument package consists of three elements, the sampling system, mass spectrometer and the data control system. The function and properties of these elements are described in the subsections that follow and are summarized in Figure 2-53.

FIGURE 2-50



The neutral mass spectrometer analyzes six discrete atmospheric samples during the mission. The six atmospheric samples are taken at six-minute time intervals. The location of the sampling levels within the various atmospheric models is shown in Figure 2-51.

FIGURE 2-54

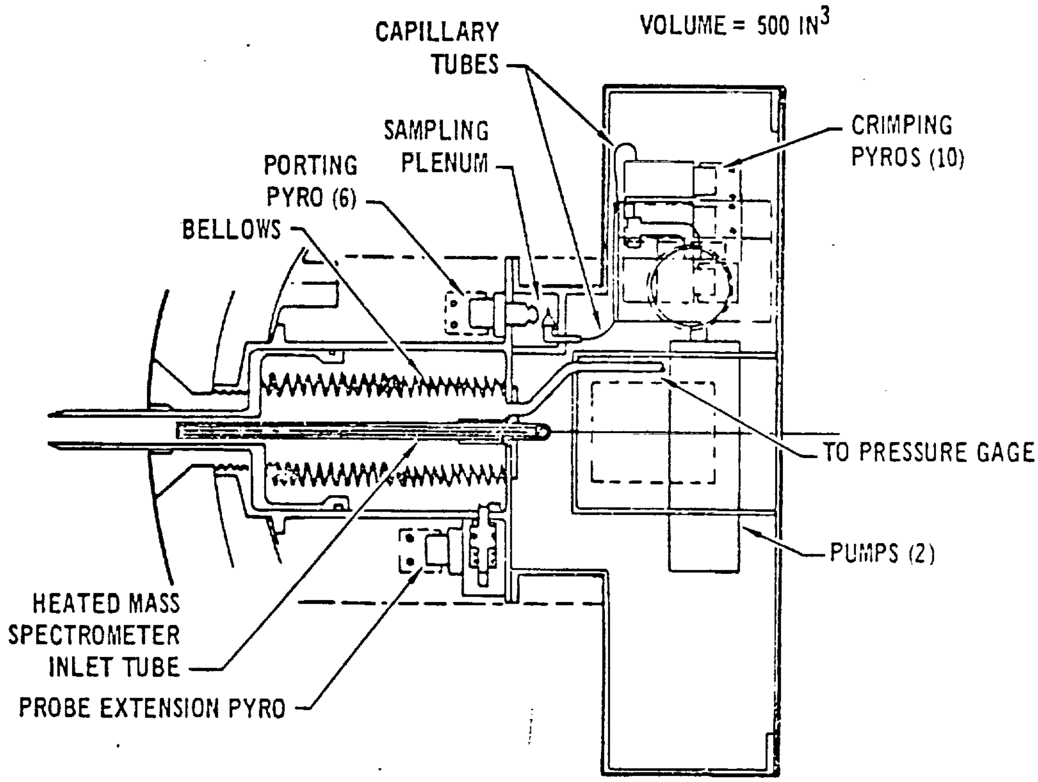


During the analysis of each atmospheric sample the mass spectrometer makes nine five-second scans of the 0 to 40 amu mass range. The first scan is digitized and transmitted directly to the probe data handling subsystem (DHS). This scan provides a detailed representation of the mass spectra of the sample. Eight additional scans are taken and averaged to remove the effect of random noise on the signal of trace atmospheric constituents. The averaged data is then transmitted to the DHS.

Sampling System - The atmospheric sampling system obtains samples of the lower atmosphere and delivers them to the mass spectrometer for analysis. The principal components of the sampling system are the atmospheric sampling probe, sampling tubes, a molecular effusive source, and pumps, Figure 2-52.

FIGURE 2-52

DETAIL OF SAMPLING PROBE ASSEMBLY IN EXTENDED POSITION



Atmospheric gas samples are obtained through a 2 cm diameter tube which is concentrically housed within a deployable tube of 3 cm diameter. Deployment is initiated through a pyro pin-puller device which releases a preloaded metal bellows. The thrust from the bellows causes the 3 cm diameter tube to push a plug out of the forward heat shield and extend 5 cm beyond the mold line into the flow field. In addition, the bellows prevents sample contamination from pyro-gases and is a plenum for the atmospheric pressure sensor.

The atmospheric samples are transmitted from the plenum to the mass spectrometer via sampling tubes. Because of the wide range of atmospheric pressures, 10^{-2} to 15 atm, over which samples are obtained, a separate sampling tube is utilized for each sample. In order to maintain near-vacuum conditions within the mass spectrometer, the sampling tubes must have an extremely small conductance. This small conductance is obtained by the combination of a porous ceramic plug and capillary tubing. Since the flow in the porous plugs and capillaries is viscuous flow, the conductance in the sampling tubes is a function of the mean pressure difference. As the probe descends through the atmosphere, each sample is obtained at a different pressure level. Therefore, the diameter and length of each sampling tube is individually sized for the specific pressure density range over which it obtains samples.

Mass Spectrometers - The mass spectroscopic analysis of a gas sample involves ionizing the gas molecules with an electron beam. The ions that are formed are sorted by the electromagnetic fields of the mass spectrometer. The constituents of the sample are identified by the mass-to-charge ratio of ions.

Atmospheric analysis from spacecraft have been conducted with both quadrupole and magnetic deflection mass spectrometers. Both types of mass spectrometer can be accommodated into the integrated instrument package as shown in Figure 2-50 and the table below.(Figure 2-53)

FIGURE 2-53

NEUTRAL ATMOSPHERE MASS SPECTROMETER AND SAMPLING SYSTEM PROPERTIES

QUADRULOPE MASS SPECTROMETER	WEIGHT		VOLUME		MAGNETIC DEFLECTION MASS SPECTROMETER	WEIGHT		VOLUME	
	KG	LB	CM^3	IN^3		KG	LB	CM^3	IN^3
MASS ANALYZER	2.3	5.0	1432	90.4	MASS ANALYZER	2.3	5.1	2433	148.4
SAMPLING SYSTEM	1.8	3.9	1188	72.5	SAMPLING SYSTEM	1.8	3.9	1188	72.5
DATA CONTROL SYSTEM	1.3	2.9	1033	63.0	DATA CONTROL SYSTEM	1.3	2.9	1033	63.0
STRUCTURE AND TUBING	1.0	2.1	3543	216.1	STRUCTURE AND TUBING	1.0	2.1	3543	216.1
TOTALS	6.4	13.9	7246	442.0	TOTALS	6.4	14.0	8197	500.0

Data Control System - The mass spectrometer data control system consists of two components, the sampling programmer and the data processor.

The mass spectrometer sampling programmer performs power conditioning and controls the sequencing of the atmospheric sampling events. The programmer is energized by an enabling signal from the data handling subsystem five seconds after the deployment of the atmospheric sampling probe. The enabling signal also activates the programmer clock, which times the sequence of sampling events.

The mass spectrometer data processor samples the analog output of the mass spectrometer, formats and stores the data, and transmits the processed data to the probe data handling subsystem at a clock rate provided by that subsystem.

During the mass spectroscopic analysis of a given atmospheric sample, the mass spectrometer makes nine 5-second scans of the 0 to 40 amu mass range. The data is processed into two forms. On the first scan the analog voltage of the mass spectrometer is sampled at 10 samples per amu. These data are encoded as nine-bit binary word by the analog-to-digital converter. The data are stored in a 634 9-bit word unit of the mass spectrometer memory and are transmitted to the probe data handling subsystem on a first-in first-out basis at the rate of 16 bits per second.

The second data sample consists of the eight additional 5-second mass spectrometer scans, sampled at 5 samples per amu. These data are accumulated in a 24 bit/word random access memory for data averaging to remove the effect of random noise on the signal of trace atmospheric constituents. Each 24-bit word location in the random access memory has 12 bits allocated for data summation and 8 bits for address. The averaging process is accomplished in binary code by summing the eight sets of data at each memory location and then discarding the last three bits of the summation. The processed data is transferred to the 634-word memory unit for transmission to the data handling subsystem.

Radiometer - The objective of the radiometry measurement is the vertical distribution within the atmosphere of absorbed solar energy. Measurements are obtained in the visible and infrared region of the spectrum. Both the downward flux of sunlight and the upward flux of planetary emission are determined.

The radiometer obtains narrow band data in three channels in the visible and near infrared and broad band data in two infrared channels. The channel assignments and corresponding spectroscopic features are as follows:

0.5μm	H_2 pressure-induced dipole
1.0	CH_4 absorption
1.1	CH_4 absorption
14-25	H_2 rotational temperature
30-55	H_2 translational temperature

The detectors for the radiometer are solid state detectors in the visible and thermopiles in the infrared.

The radiometer measurements are made in the probe free stream in the vicinity of the probe beltline. The detectors are deployed from the probe by a solar panel deployment mechanism that is spring-released. The detectors are deployed when the probe reaches the lower atmosphere; i.e., at the $-2G_E$ level. The detector housing is alternately oriented in an upward and downward looking position.

FIGURE 2-54

RADIOMETER CHARACTERISTICS

Range:	0.5 to 55 μm in 5 channels		
Size:	656 cm^3 (40 $in.^3$)		
Weight:	3 kg (6.6 lb)		
Power:	3 watt		
Date Rate:	Word Size (bits/word)	Sample Rate (words/sec)	Data Rate (bits/sec)
	9	0.33	3

Nephelometer - The objective of the nephelometer experiment is the detection of cloud layers in the lower atmospheres of Saturn and Uranus. The light-scattering properties of atmospheric condensates are exploited in detecting the clouds. The condensates scatter the incident light originating from the nephelometer light source. A portion of the incident light is scattered back into the nephelometer collection lens.

A forward scattering nephelometer consists of a light source, lenses and optical detectors. Characteristics are defined in Figure 2-55. The light source is a light emitting diode, which illuminates a portion of the atmosphere within the field of view of the detectors. Three photodiode detectors are used, one to measure the backscattering by the atmospheric condensates, the other two to monitor the background atmospheric emission. These components together with the power supply and the data processing electronics are packaged into a single unit. The nephelometer is located in the aft hemisphere of the probe near the maximum diameter and looks out perpendicular to the spin axis of the probe. The nephelometer is recessed within the probe to prevent the accumulation of atmospheric condensation or dust particles on an exterior window. A viewing port is opened in the heat shield at $-2G_E$ just prior to the initiation of nephelometer measurements.

The data output from the nephelometer consists of four channels of photodetector data at 10 bits/word and three channels of instrument status data at 6 bits/word. The analog output of the nephelometer is sampled once every 30 sec. A data processor within the instrument digitizes these data and transfers them to the DHS at 2 bits/sec using a clock furnished by the data handling subsystem.

FIGURE 2-55

NEPHELOMETER CHARACTERISTICS

SIZE: 10 x 5.7 x 7.5 CM, SENSOR PLUS ELECTRONICS			
VOLUME: 427 CM^3 (26 IN^3)			
WEIGHT: 0.5 KG (1.1 LB)			
POWER: 1.2 W PEAK, 1W AVERAGE			
DATA OUTPUT:	1 NEPHELOMETER OUTPUT	1 x 10 BITS	
	3 BACKGROUND LEVEL	3 x 10 BITS	
	3 INSTRUMENT STATUS	3 x 6 BITS	
DATA DIGITIZED INTO A SINGLE STREAM BY THE INSTRUMENT'S DATA PROCESSOR			
DATA RATE:	SAMPLE SIZE (BIT/SAMPLE)	SAMPLE RATE (SAMPLE/SEC)	DATA RATE (BITS/SEC)
	58	0.033	2
INSTRUMENT HISTORY: ARC CONCEPT FOR PIONEER VENUS PROBE			

Ion Mass Spectrometer - The ion mass spectrometer makes ion identity and relative abundance measurements in the outer regions of the atmospheres. The instrument operates between 10^{-14} and 10^{-7} atm. On the low pressure side of the ionosphere measurements are limited by the instrument sensitivity. On the high pressure side, the instrument fails due to RF breakdown within the analyzer section. These pressure limits correspond approximately to 1100 and 500 km, respectively, in the nominal atmospheric model of Saturn.

The method of operation of an ion mass spectrometer is very similar to that of a neutral gas mass spectrometer. The primary difference between the two types of instruments is a consequence of the kind of atmospheric sample that is to be analyzed. For analyzing the ionic components of the atmosphere, there is no need for an electron gun to ionize the sample prior to mass analysis as required in the neutral mass spectrometer. The atmospheric ions are drawn into the ion mass spectrometer by the action of an electrical grid behind the inlet orifice. The ions are directed into the analyzer section by an accelerating grid. Within the analyzer section the ions are mass sorted by the action of a quadrupole field. The mass resolved ions then impinge on an ion collector. The ion current is amplified by an electron multiplier and converted to voltage by an electrometer. The characteristics of the ion mass spectrometer are given in Figure 2-56.

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The positive ions anticipated in the upper atmospheres of Saturn and Uranus are those that result from the solar photoionization of hydrogen and helium: H^+, H_2^+ and He^+. Additional ion species are formed from the reaction of the primary ions with the neutral species present, resulting in H_2^+ and HHe^+. The mass range represented by these ions is 1 to 5 amu. The ion mass spectrometer scans this mass range in 0.6 seconds. The output of spectrometer is sampled at 1.66 samples/sec and quantized into five 5-bit words by the spectrometer data processor. The ion spectrometer data is transmitted at 66.6 bits/sec to the probe data handling subsystem, where it is stored until radio transmission begins, in the vicinity of the tropopause.

Figure 2-56

ION MASS SPECTROMETER CHARACTERISTICS

RANGE: 1 – 5 AMU
OPERATING RANGE: 10^{-14} TO 10^{-7} ATM
SIZE: ANALYZER: 3.8 DIA x 12.7 CM
ELECTRONICS: 12.7 x 12.7 x 7.6 CM
VOLUME: ANALYZER: 145 CM^3 (8.8 IN^3)
ELECTRONICS: 1230 CM^3 (75 IN^3)
WEIGHT: ANALYZER: 0.9 KG, (2 LB)
ELECTRONICS: 0.9 KG, (2 LB)
POWER: 3W
DATA OUTPUT: DATA DIGITIZED INTO FIVE 8-BIT WORDS BY THE INSTRUMENT'S DATA PROCESSOR.

DATA RATE	SAMPLE SIZE (BITS/SAMPLE)	SAMPLE RATE (SAMPLES/SEC)	DATA RATE (BITS/SEC)
	40	1.666	66.6

INSTRUMENT HISTORY: ATMOSPHERIC EXPLORERS, SOUNDING ROCKETS.

SUMMARY

The representative science payload of an outer planet atmospheric entry probe has been described. The instrumental details are based on experiments that have been successfully flown in the atmospheres of Earth and Venus. The incorporation of these instruments into an outer planet probe requires a strong interaction between instrument designer and probe designer. The installation of the instruments into a 250 lb entry probe is depicted in Figure 2-57.

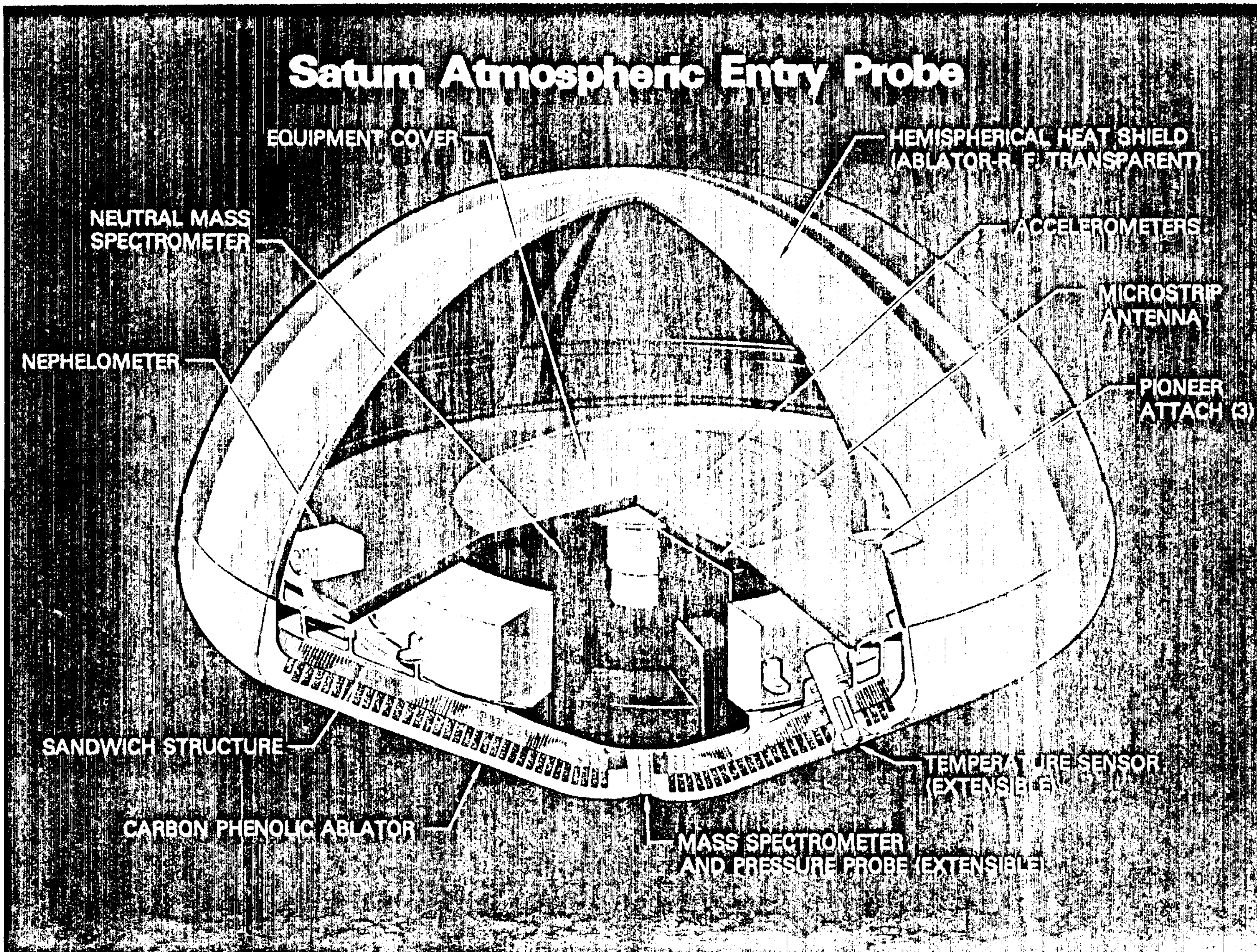

Figure 2-57

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SESSION III

MISSION AND SPACECRAFT DESIGN CONSTRAINTS - 21 May 1974

Chairman: Byron L. Swenson
System Studies Division
NASA Ames Research Center

MR. SWENSON: The title of this afternoon's session is Mission and Spacecraft Design Constraints. In the next two hours, we will be discussion the constraints imposed upon the spacecraft and the probe by the mission and some of the constraints that the spacecraft imposes upon the mission.

I would like to spend about the next ten or fifteen minutes on an overview of the missions under consideration to try to provide a backdrop for the more detailed presentations to follow.

OUTER PLANET MISSION ANALYSIS OVERVIEW

MR. SWENSON: I think we have seen enough of programmatic strategy but before we go into a description of the missions, there are a few things we have to understand, particularly with regard to flying probe missions off the Pioneer spacecraft.

Pioneer is an earth-line stabilized spacecraft, and this presents some unique problems. The schematic for the deflection is shown on the right-hand side of Figure 3-1. At some distance as we approach the planet, we separate the probe, which is spinning in the earth-line direction. After probe separation we deflect the bus in order to miss the planet. The whole idea behind the deflection maneuver is to deflect the bus in such a way as to place it appropriately behind the probe so that the communication angle, the bus aspect angle, is in the aft hemisphere of the spacecraft and, at the same time, the probe aspect angle, after the probe enters and is descending vertically in the atmosphere, is very small.

However, with the Pioneer, we have the constraints that no orientations off the earth line will be permitted, but we will allow perpendicular and/or earth line maneuver capability. We are assuming a very simple probe without any attitude control systems and, therefore, orientations off the earth line are not permitted.

Now with those constraints in mind, the Uranus mission appears as shown in Figure 3-2.

This is a Uranus probe mission flown on a 1980 JU trajectory which is really no longer in consideration programmatically, but it is representative of the type of planetary approach. We approach from nearly right onto the North pole. With the Pioneer spacecraft, we try to swing by on the retrograde side. A Mariner 79 JU would flyby on the posigrade side.

DEFLECTION CONSTRAINTS

- PIONEER BUS
  ORIENTATIONS OFF EARTH-LINE NOT PERMITTED

  PERPENDICULAR AND/OR ALONG EARTH-LINE MANEUVER CAPABILITY

- PROBE
  ORIENTATIONS OFF EARTH-LINE NOT PERMITTED

DEFLECTION SCHEMATIC

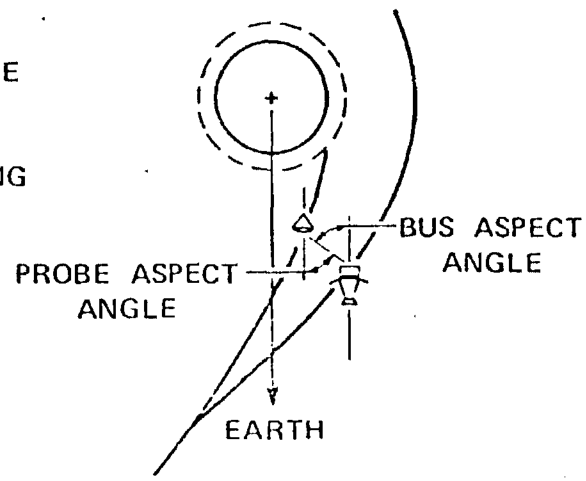


Figure 3-1

URANUS PROBE
1980 J/U

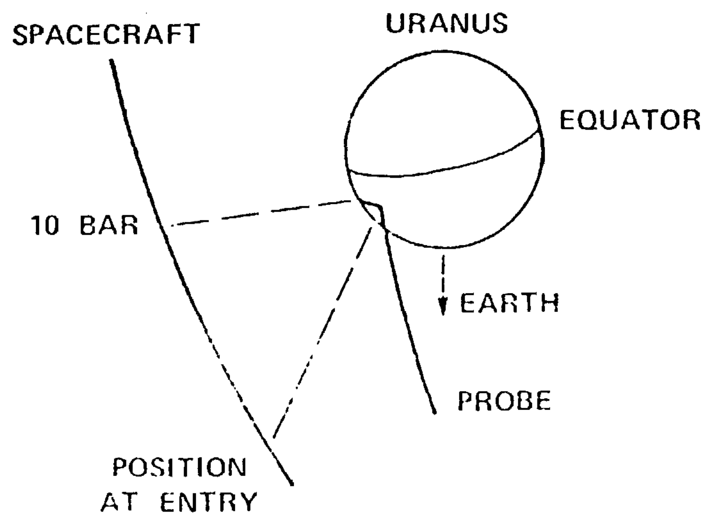


Figure 3-2

SWENSON

The reason the Pioneer flyby is on the retrograde side is to provide a nearly zero angle of attack at entry. The probe hits the atmosphere and descends and is turned by the rotation of the planet during this time. During this descent period, we try to maintain appropriate communication angles.

The spacecraft is pointing toward the Earth, and as shown, the spacecraft is nearly overhead of the probe through the entire descent.

The Saturn mission, shown on Figure 3-3, is for a 1981 dedicated mission. Some thirty days prior to encounter with the planet, we separate the probe. We deflect the spacecraft with a ΔV of about 75 meters per second; when the probe enters, the spacecraft is at the location shown on the figure at the time of entry. (Please excuse the artistic license on the figure, the spacecraft isn't quite that far around at the end of the probe mission.)

Again, the communication angles are fairly common, and the spacecraft is directly overhead of the probe during the entire descent.

The Titan mission is a little bit different. Over on the left-hand side of the Figure 3-4 you see Saturn and Titan's orbit at about 20 Saturn radii. The type of intercept that is attractive is an incoming intercept.

Some thirty days prior to encounter with Titan, we separate the probe. After spacecraft deflection, the probe and the spacecraft travel nearly parallel trajectories.

Over on the right hand side of the figure you see a blowup of the area of Titan.

At entry, we position the spacecraft so that we are about a hundred thousand kilometers away. And, then, you can see that at four hours after the entry we are occulted by Titan and we get a RF occultation experiment at the same time.

SATURN PROBE
1981

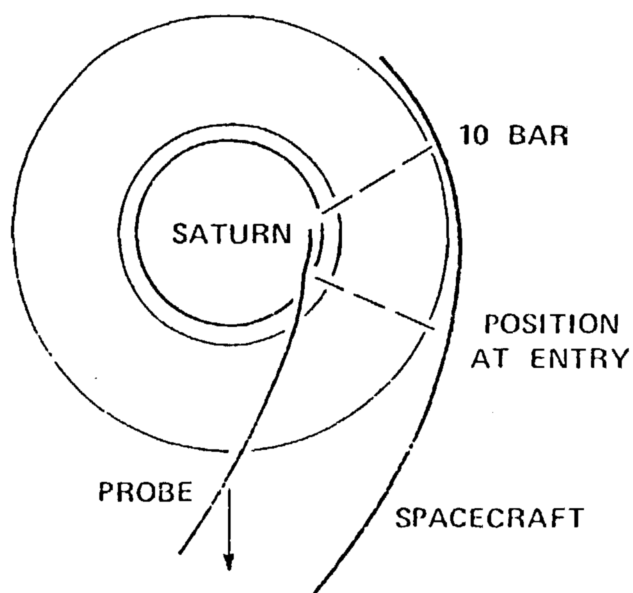


Figure 3-3

SWENSON

TITAN PROBE
1982

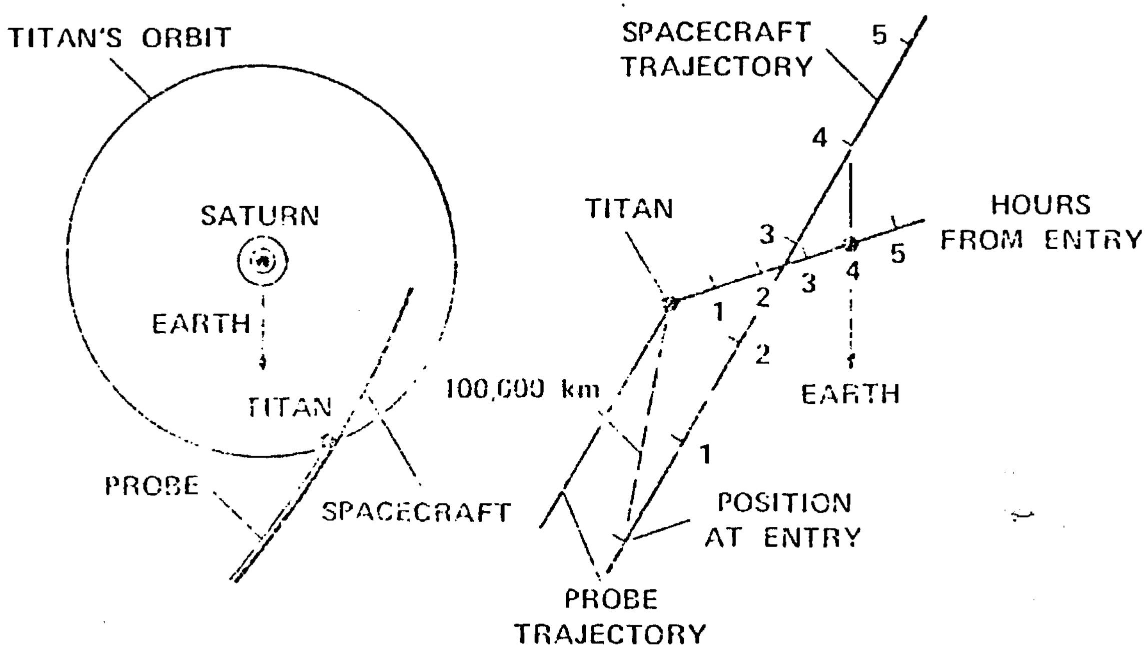


Figure 3-4

The Jupiter probe is shown on Figure 3-5. We have heard a lot about the ephemeris improvement due to Pioneer 10. What it means to us is that the one sigma ephemeris error is now approximately 468 kilometers. What this means, translated into a three sigma entry angle error is that we can now expect to enter very shallow with very small errors. In fact, for entry angles around seven or seven and a half degrees, the three sigma entry angle error is less than half a degree. That means we can be assured within three sigma that we will enter no steeper than eight and no shallower than about seven. This means that the heating is greatly reduced, the accelerations are likewise greatly reduced, and if the atmosphere is as friendly as we now think, we will be able to get in with a lot of less heating.

The conclusions to all the mission analysis work can be put into three main categories as shown on Figure 3-6. We have plenty of launch capability for the Saturn and the Jupiter-Uranus trajectories that we have looked at for Pioneer. We have 480 kilograms with a ten-day launch window off of a Titan/Centaur/ TE 364.

In the Jupiter case, we have capability up to about eleven hundred kilograms.

In all cases, the probe separation occurs about 30 days out, with the exception of Jupiter where we separate about 50 days out. And in all cases, the ΔV to deflect the spacecraft is less than eighty meters per second.

The entry angle of attack for the probe is always low, less than twenty degrees.

In the communications area, the range is always less than a hundred thousand kilometers. The probe aspect angles can be made to be very low and the spacecraft aspect angles are common to all

PIONEER JUPITER

1980

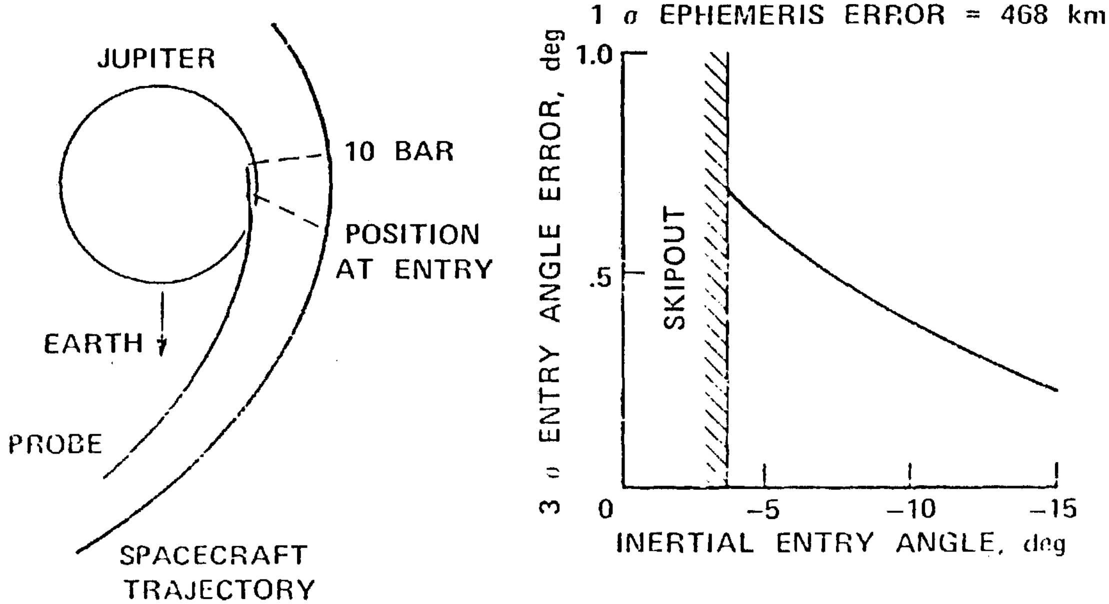


Figure 3-5

SWENSON

CONCLUSIONS

- LAUNCH CAPABILITY
  480 kg 10-DAY WINDOW
  TITAN III E / CENTAUR / TE 364-4
- PROBE DELIVERY
  SEPARATION 30 DAYS OUT
  $\Delta V \sim 80$ m/sec
  ENTRY ANGLE OF ATTACK LOW
- RELAY COMMUNICATIONS
  RANGE ~ 100,000 km
  PROBE ASPECT ANGLE LOW
  SPACECRAFT ASPECT ANGLES COMMON

Figure 3-6

of the missions we have considered. That is, they all lie in the aft hemisphere of the spacecraft.

With that as a backdrop of a description of the missions, the next speaker, Lou Friedman, of JPL, will discuss taking these missions and determining what the guidance and navigation requirements are for the probe mission.

OUTER PLANET PROBE NAVIGATION

Louis Friedman

Jet Propulsion Laboratory

MR. FRIEDMAN: We have been conducting a series of navigation studies in conjunction with the outer planet Pioneer missions that Byron Swenson has just discussed.* These missions are described in Figure 3-7. What I am going to describe is a brief summary of these results and some of the major conclusions from the studies. I will also discuss the more recent work that has been performed in conjunction with the Mariner-Jupiter-Uranus mission and make some overall conclusions as far as navigating probes to the outer planets.

The point of our studies has been to determine navigation requirements for these potential atmospheric probe missions and in particular, to look at proposed measurement systems in order to target probes into the outer planets and Titan. The study work is described in Figure 3-8 and 3-9.

To estimate maneuver sizes and strategy for such missions, we have been interacting with the mission designers with items such as separation times, strategy for making measurements, and finally of course the navigation implementation.

Figure 3-10 shows some of the basic assumptions. The Titan III E/Centaur/TE 364 is the planned launch vehicle for all the missions this implies about an eighty meter per second to correct injection dispersions (that is a mean plus three sigma number). This dictates pretty much the entire cruise requirement for delta-V since the subsequent navigation maneuvers are quite small.

Radio accuracies are more or less traditional as to what has been assumed. In our navigation studies, we have deweighted the range data so as to account for the effect of process noise and we have also investigated both conventional Doppler and ranging and differenced Doppler and ranging.

*This report describes work by Jordan Ellis, Frank Jordan, Charles Paul, Kent Russell and Gary Sherman, in addition to myself at JPL.

FIGURE 3-7

ADVANCED PIONEER

OUTER PLANET PROBE MISSION

	S '79	SU '80	JU '80	TITAN
LAUNCH	11-23-79	11-25-80	12-9-80	1-6-84
ARRIVALS	4-16-83	1-5-84 11-9-87	3-25-82 4-2-86	1-11-87
V_∞ (KM/SEC)	9.1	10.5(S), 13.8(U)	15.1(J), 15.8(U)	11.6
R_P (RADII)	2.6	2.8(S), 4.0(U)	14.7(J), 3.0(U)	13.8 R_S
<u>PROBE</u>				
B (RADII)	3.5	1.5	1.3	0.0 R_T
γ_E (DEG)	-30	-40	-40	-90
R_{SEP} (RADII)	300	700/1000/1300	600/800/1200	
T_{SEP} (DAYS FROM PERI.)	506	14/20/26	12/16/24	27
ΔV (M/SEC) - EARTHLINE	104	45/30/18	56/43/28	37
- NORMAL	0	66/46/36	60/36/30	3

FIGURE 3-8

OUTER PLANET PIONEER NAVIGATION STUDIES

- o DETERMINES NAVIGATION REQUIREMENTS
  - o MEASUREMENTS
    - o RADIO TRACKING
    - o ON-BOARD OPTICAL
  - o MANEUVER SIZES AND STRATEGY
- o CONTRIBUTES TO MISSION DESIGN
- o DESCRIBES NAVIGATION IMPLEMENTATION
  - o SINGLE AND MULTI-MISSIONS
- o DEFINES TARGETTING ACCURACIES

FIGURE 3-9

MAJOR TASKS IN STUDY

- o REDUCTION OF V-SLIT SENSOR DATA TO NAVIGATIONAL INFO.
  - o NO ASSESSMENT OF SENSOR
  - o NO ANALYSIS OF INSTRUMENT ACCURACY
- o STATISTICS OF THE PIONEER MANEUVER EXECUTION
  - o PRECESSION MANEUVER MODEL (HISTORICAL)
  - o RESTRICED DIRECTION MANEUVER MODEL (NEW)
- o ORBIT DETERMINATION PARAMETRIC STUDIES
  - o RADIO (INCL. EPHEMERIS)
  - o OPTICAL
  - o SEPARATION DISTANCES AND COORDINATES
- O COMBINED MANEUVER EXECUTION AND ORBIT DETERMINATION NAVIGATION RESULTS

FIGURE 3-10

ASSUMPTIONS

- TITAN III E/CENTAUR/TE 364-4 INJECTION REQUIRES ∿80 M/SEC ALLOWANCE FOR 1ST MIDCOURSE

- RADIO ACCURACIES
  DOPPLER: 100 MM/SEC (CONV), 2.8 MM/SEC (DIFF.)
  10 KM (CONV), 8.4 M (DIFF.)
  (ALLOWS EFFECT OF PROCESS NOISE)

- TRACKING
  1 PT/MIN DOPPLER, 1 PT/6 HR RANGE, OVERLAP
  E - 120 DAYS TO E
  STATION LOCATIONS CONSIDERED (TIGHT: 1 x 2 x 15 M
  LOOSE: 3 x 5 x 15 M)

- EPHEMERIS
  JUPITER: 400KM
  SATURN: 1000KM
  URANUS: 10000KM

I won't go through the other details depicted on the figure, but note the ephemeris accuracies we assumed in the basic study. These are one sigma ephemeris accuracies that we have assumed for the post-MJS time period. The Uranus ephemeris error, 10,000 kilometers, is quite a bit out of line with the other planets. There is reason for that, but that is a subject being separately studied, and will be discussed more later.

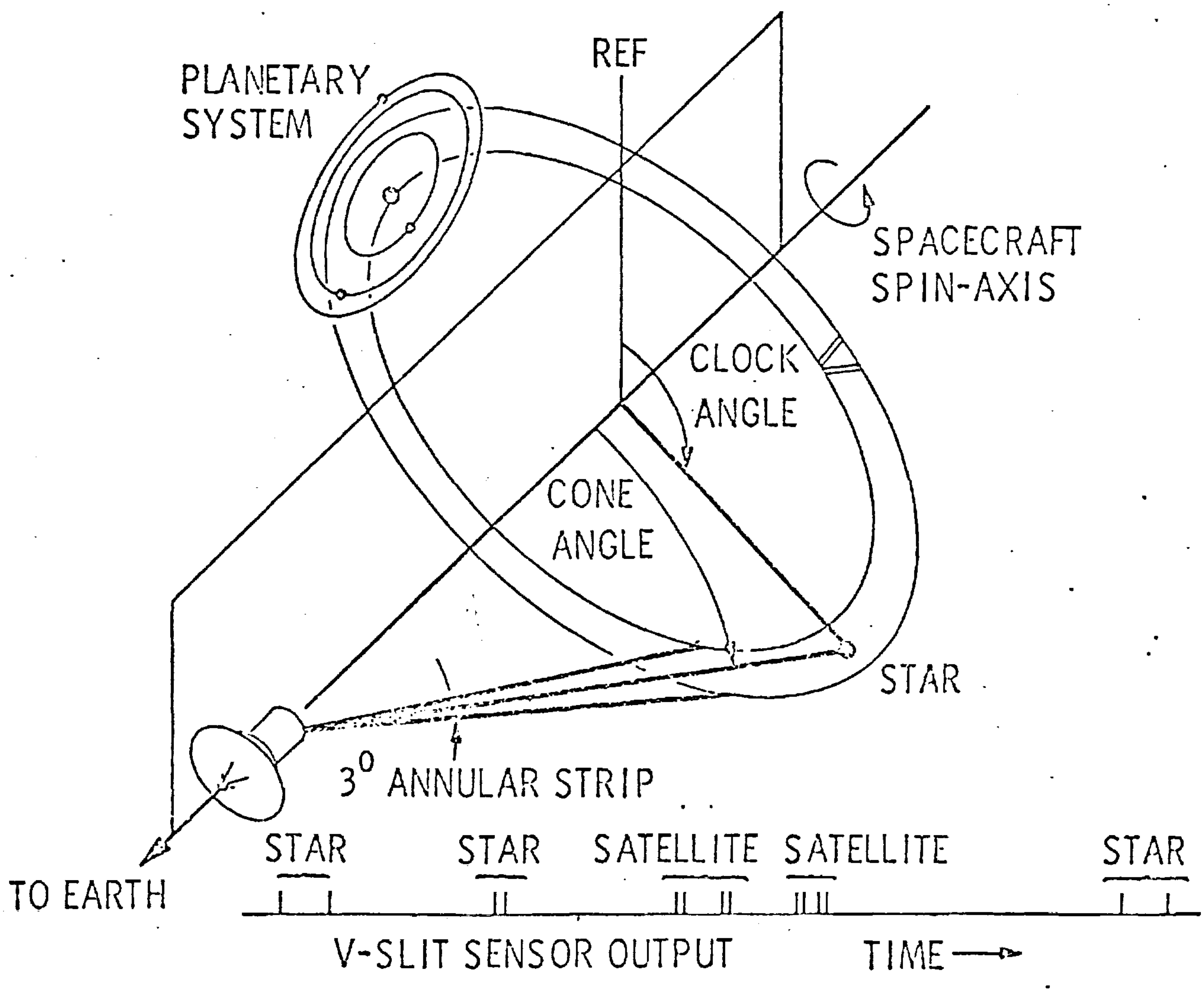
We also, in addition to the radio tracking assumptions, have analyzed the V-slit optical navigation sensor which was proposed by TRW as part of the same series of mission studies. In principle, it is to work on the Pioneer spacecraft by taking advantage of the spin to sweep out a region of the sky, and thereby get a cone and clock angle measurement of the satellite and of a star. By being able to determine the angle between them, it then is possible to obtain a satellite-star angle measurement. Its operation is shown in Figure 3-11.

We have worked through various geometries for the various missions and analyzed the star background. It appears adequate. A sample star background is shown in Figures 3-12 and 3-13 for the S/U mission at Saturn and Uranus respectively. The accuracies assumed by TRW in proposing this particular sensor were fifteen arc/seconds in cone and twenty-five arc/seconds in clock (one-sigma).

This is the only concept we have investigated in our studies although it is applicable to other concepts if you parameterize those other concepts in terms of cone and clock angle errors. Thus, our results generalize to any kind of optical system.

The V-slit sensor can only work when the object is bright enough but also when it is less than the slit diameter. The proposal is to acquire it at a certain magnitude and then, as you get closer to the spacecraft, when it gets larger than twenty arc/seconds, you no longer use the measurement. Figure 3-14 shows these cut-offs for various satellites of the outer planets, and lists

FIGURE 3-11



V-Slit Sensor Geometry

ASSUMED ACCURACY, 1σ

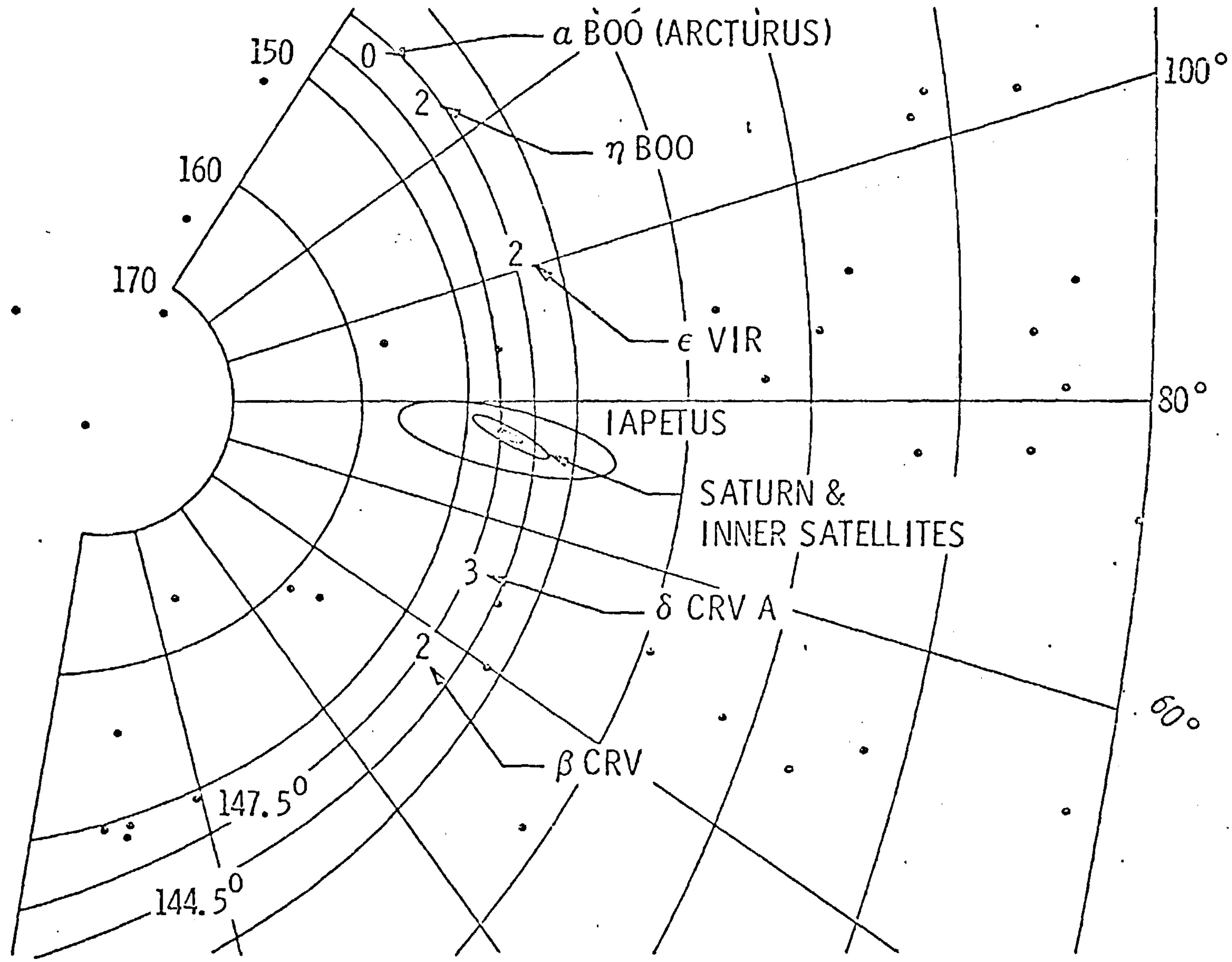
CONE 15″ (75 RAD)

CLOCK 25″ (125 RAD)

LDF - 5

5-21

FIGURE 3-12



Star Visibility at Saturn

S/U 80

LDF - 7

5-21

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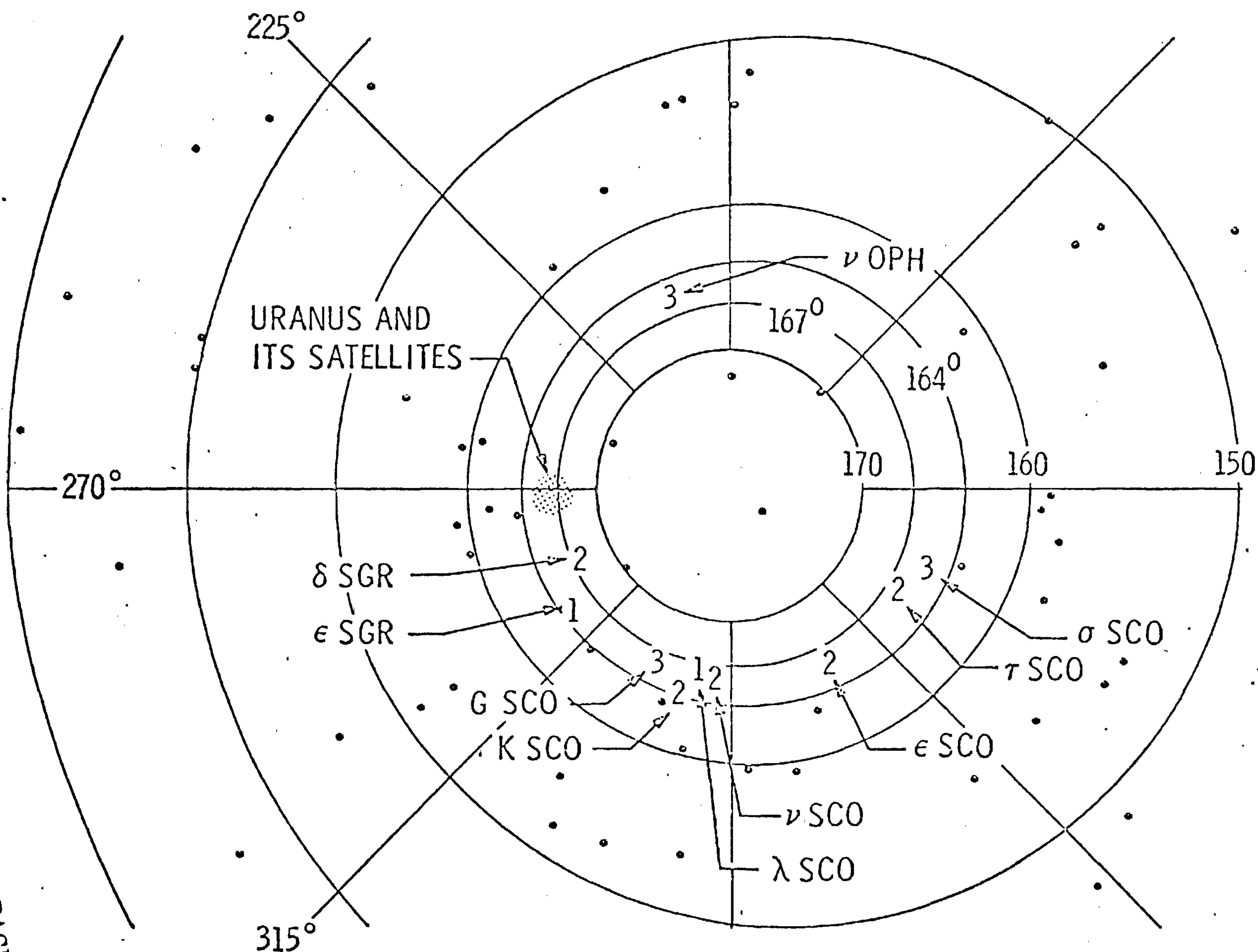
FIGURE 3-13



Star Visibility at Uranus

S/U 80

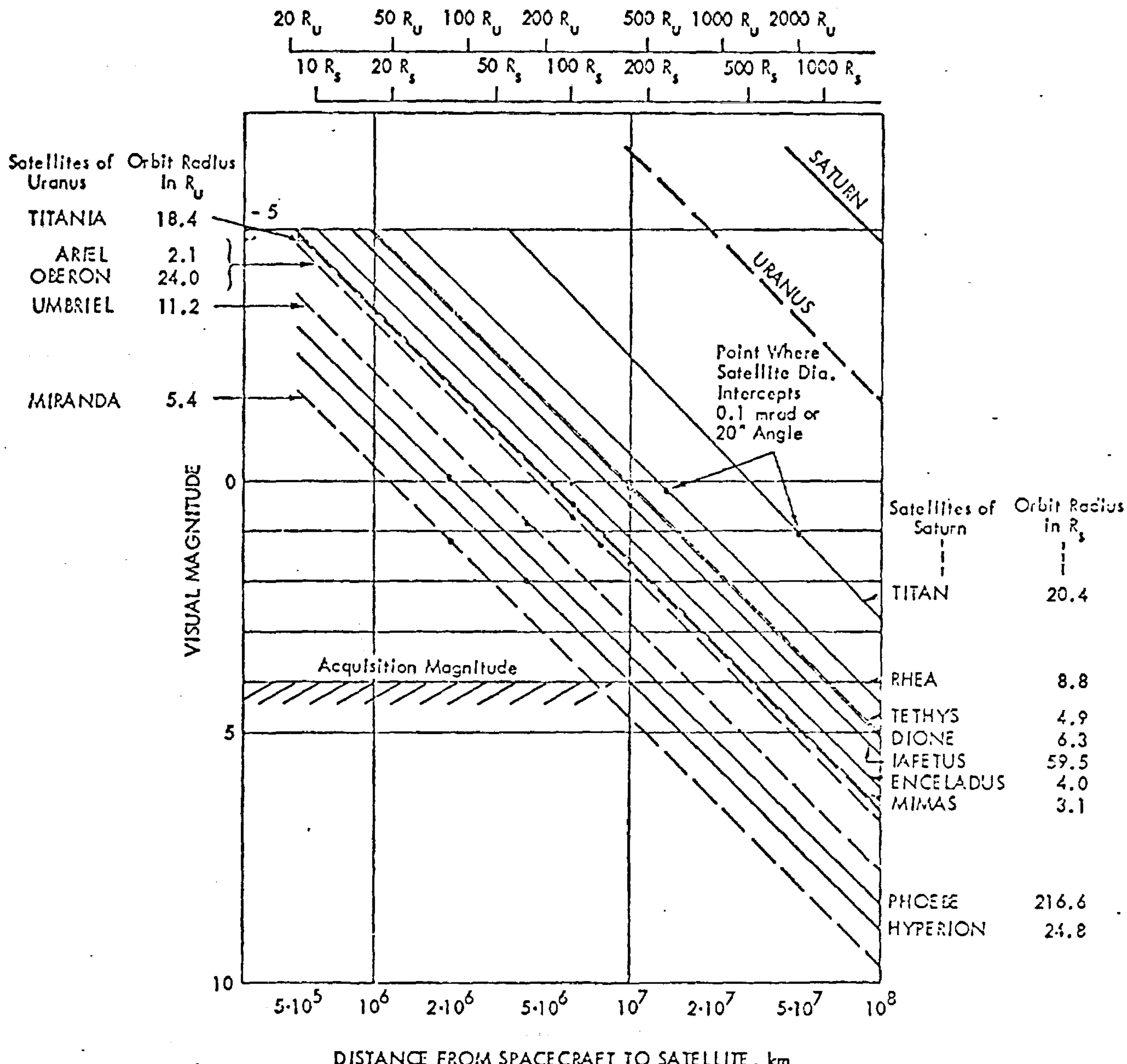


FIGURE 3-14

Visual Magnitudes of Satellites of Saturn and Uranus

LDF - 6

how the magnitude and diameters vary with range of the spacecraft, hence when you can use those satellites as observables. This becomes very important as you can see here for Titan. Quite far away from Titan we are prevented from obtaining useful measurements, and so that either the time of getting measurements must be extended or some other scheme for measurements must be found.

As a brief description of some of the results, areas listed on Figure 3-15 will be covered.

For Jupiter, which is only an intermediate target, we looked at radio only navigation first and found out that the accuracy was sufficient so that the size of the post-Jupiter maneuver could be kept to reasonable levels so that the mission could be carried out; that is, go on to Uranus. We assumed two levels of tracking accuracy - shown in Figure 3-16. The solid line represents what we call loose stations (cf Figure 3-10). The dotted line represents what we call tight station accuracies.

We studied different lengths of tracking arcs and let them go to near encounter. Presumably, tracking is cut off around four days before encounter when a final maneuver is made. Even at four days, we obtained very reasonable post-Jupiter Delta-V requirements. Either the eight meters per second or the thirteen meter per second are acceptable. That is no problem and hence at Jupiter, radio-only navigation suffices.

In Figure 3-17 we show what happens when you try radio-only tracking at Uranus. Here we have to live with the ephemeris error. Shown are three components of position error and because of the geometry, you transfer errors in one component to an error in the other component. Basically, the ephemeris error is near seven thousand kilometers and can not be much improved. However, optical navigation at Uranus offers significant improvement to these resutls. As an example, Figure 3-18 shows navigation accuracy

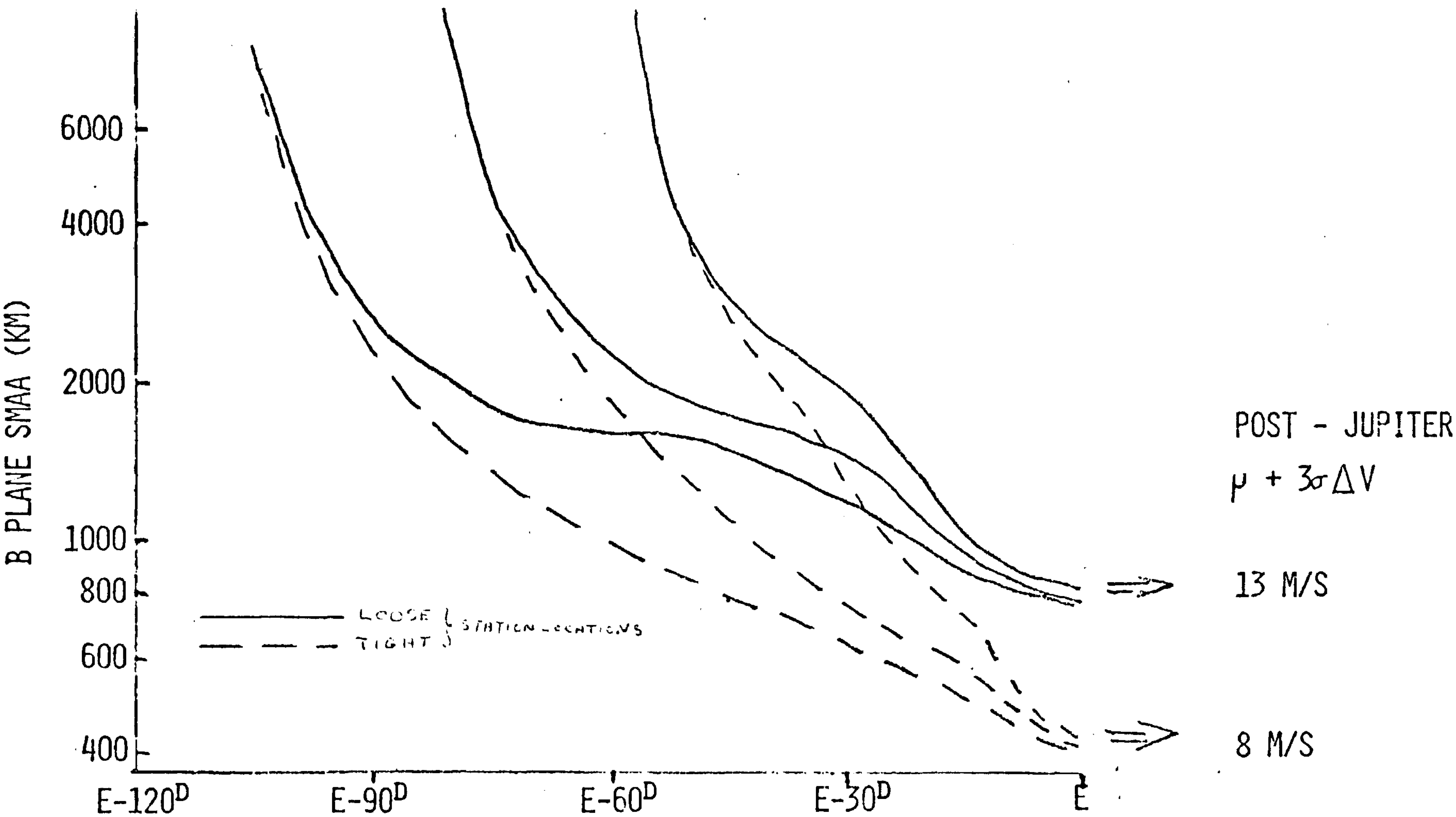
FIGURE 3-15

SAMPLE RESUTLS

- JUPITER RADIO-ONLY RESULTS
- URANUS BUS POSITION ERROR COORDINATES (RADIO ALONE)
- OPTICAL TRACKING AT URANUS
- ENTRY ANGLE ERROR AT URANUS
- SATURN/URANUS VELOCITY REQUIREMENT
- TITAN ORBIT DETERMINATION RESULTS
- TITAN GUIDANCE POLICY

FIGURE 3-16

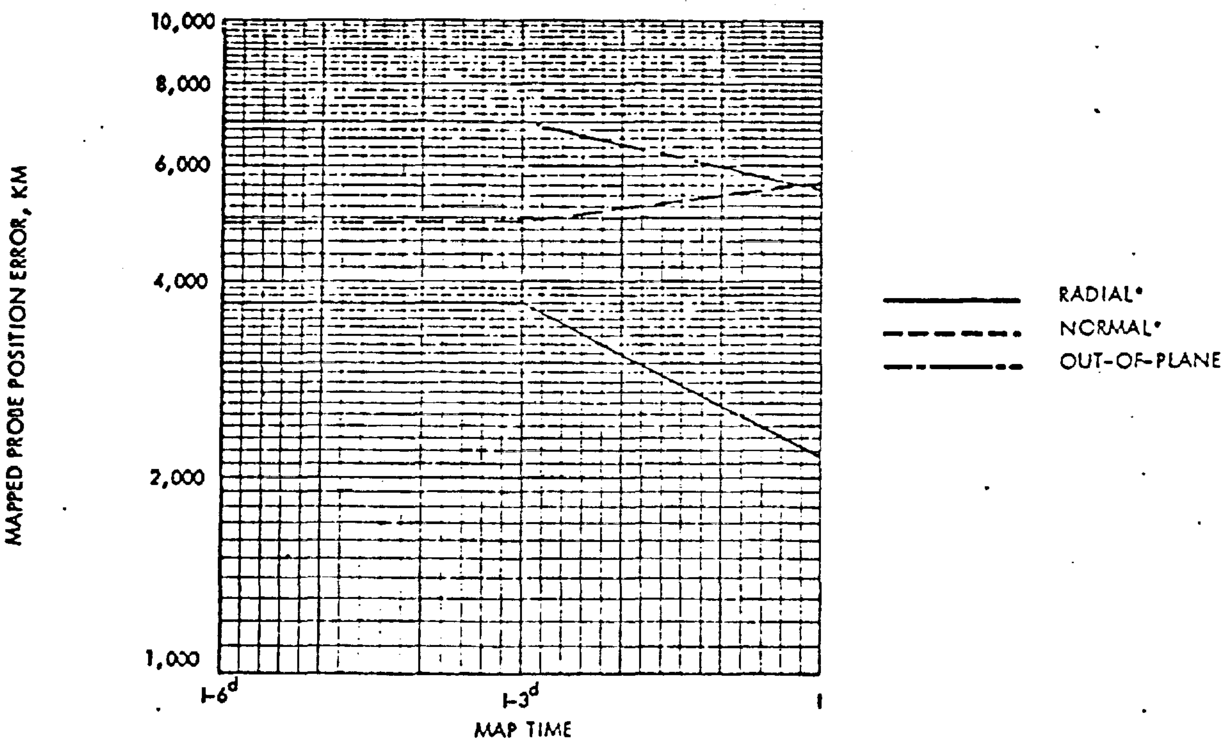
JUPITER PHASE RADIO ONLY



LDF - 10

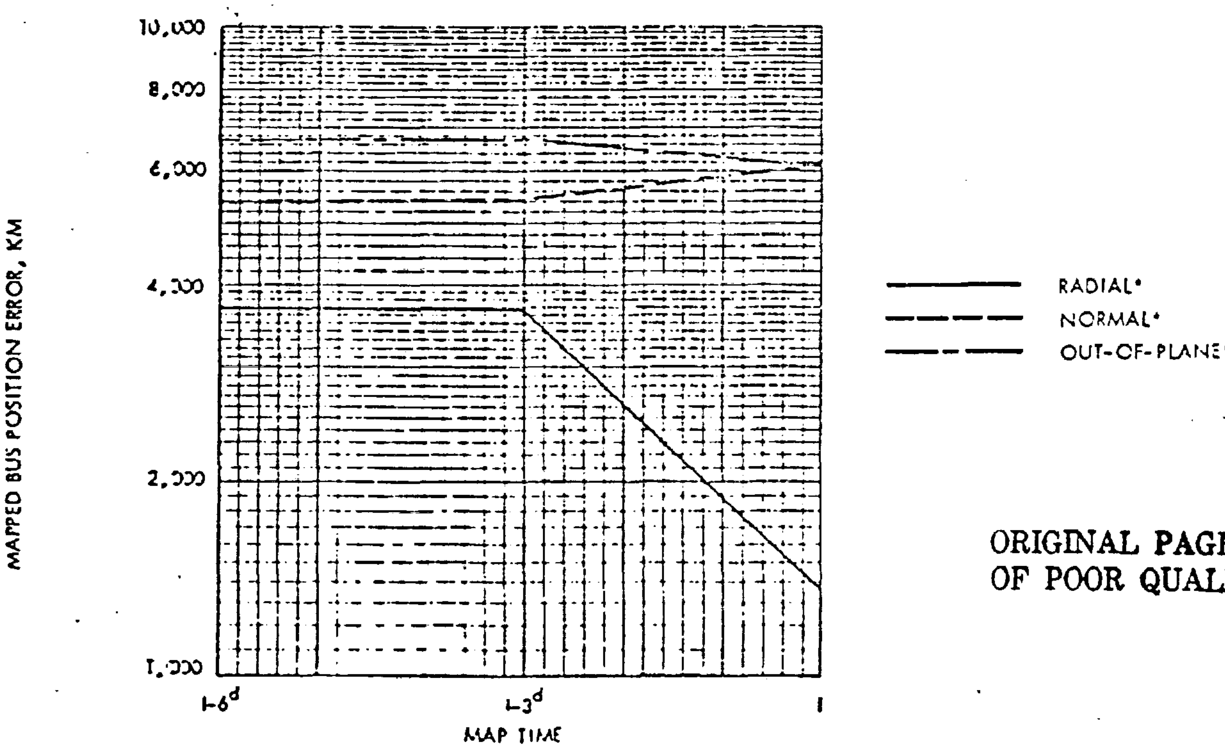
5-21

URANUS BUS - RADIO ALONE



*ALL RESULTS THE SAME FOR "TIGHT" OR "LOOSE" STATION LOCATION A PRIORI

Mapped Probe Position Error vs Map Time



*ALL RESULTS THE SAME FOR "TIGHT" OR "LOOSE" STATION LOCATION A PRIORI

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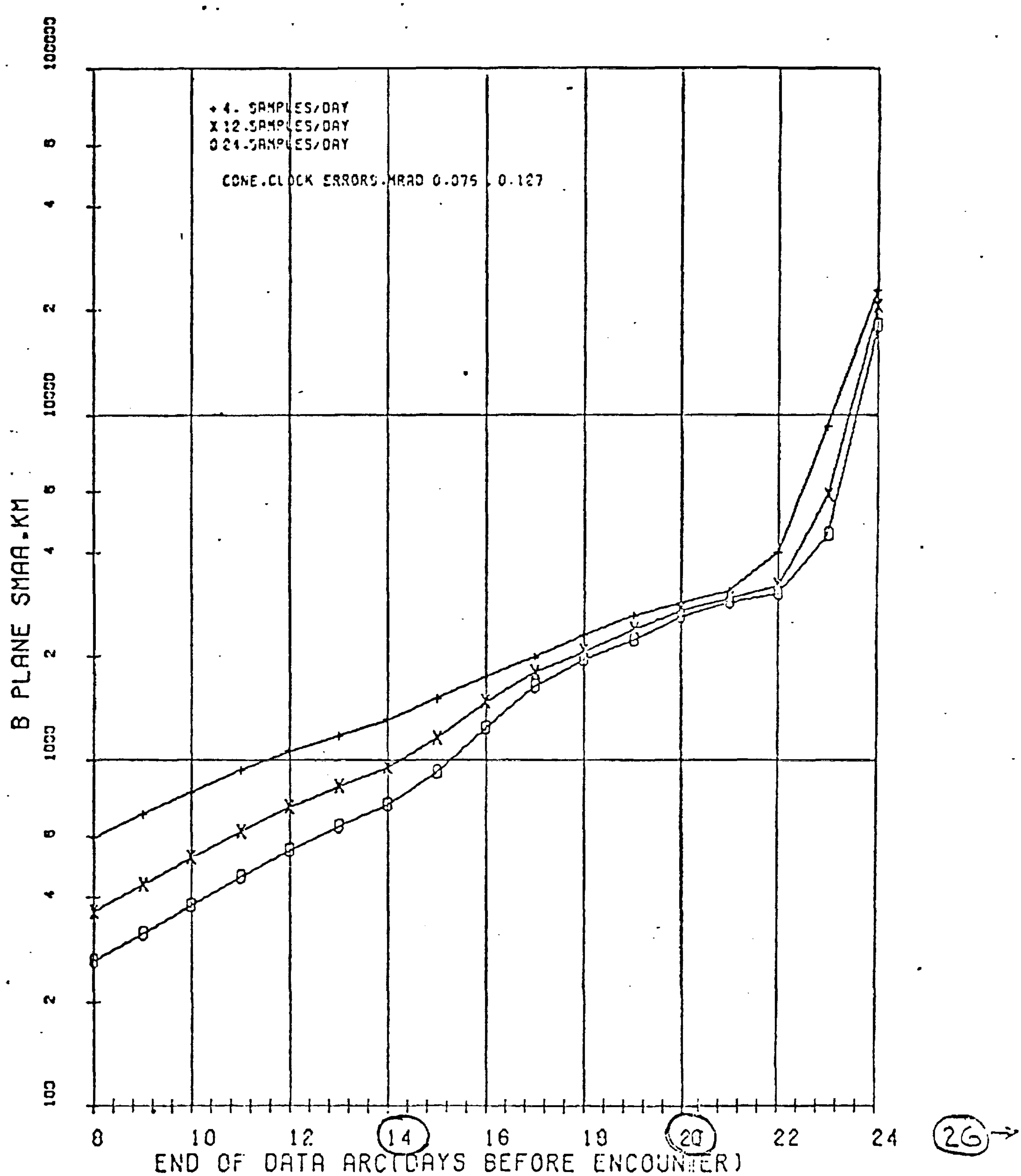
FIGURE 3-17

LDF - 11
5-21



V-Slit Sensor Accuracy Satellite Titania

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LDF - 12
5-21

FIGURE 3-18

obtained using the satellite Titania. More results are in the report that we have given to Ames. We have run many more simulations and these can be checked in more detail.

The point here is that this is navigation accuracy using the V-slit sensor to image the satellite Titania with respect to the star background. Shown is the one-sigma semi-major axis in the B plane versus the end of the data arc in days before encounter. The longer you track the better you can do, but you can't track beyond the time of separation of the probe.

In one concept it was proposed to separate the probe at 27 days, but this is seen as insufficient to bring the errors down from the almost 10,000 kilometer level. If we wait a little longer, we can then bring the errors to below a few thousand kilometers.

Certainly, errors of about a thousand kilometers or somewhat larger are acceptable and so it seems indicated that separation should be made somewhere around twenty days at least. Figure 3-19 relates to the required accuracy in the B plane to the entry angle error. A thousand kilometers at a forty degree entry angle leaves a 2.7 degree entry angle error, which is quite acceptable. And even two thousand would be out around five degrees.

So roughly, as long as we can keep errors within this region, that is track up to about twenty days (using satellite Titania) optical navigation used with this V-slit sensor at assumed levels of accuracy was quite satisfactory.

Looking at the Saturn-Uranus mission, we also sized the Delta-V requirements according to the strategy of Figure 3-20. We looked at the case of radio-only navigation at Saturn just like we did at Jupiter and found that the post-Saturn maneuver would have to be 140 meters per second in the case of radio-only navigation, far

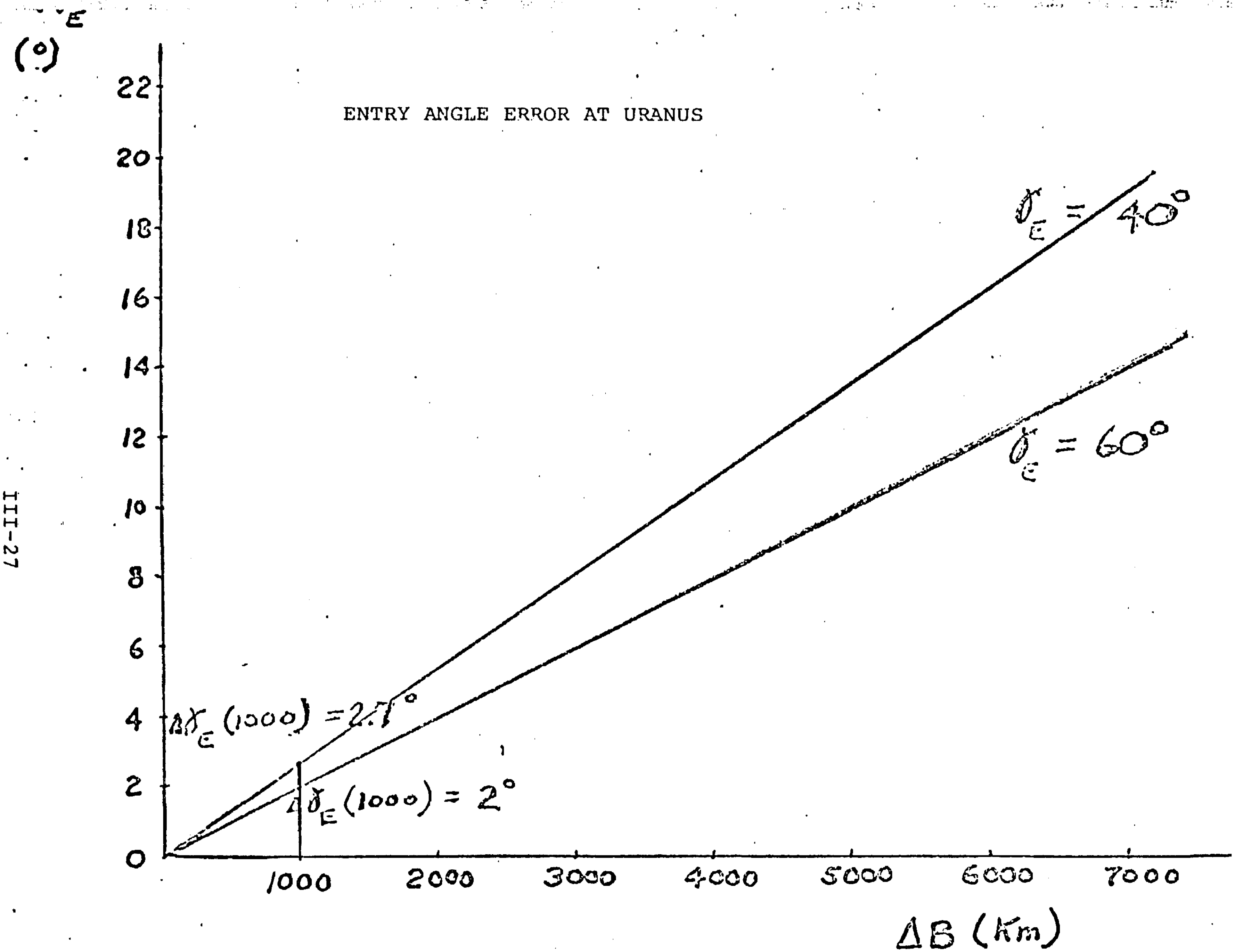
ENTRY ANGLE ERROR AT URANUS
γ_E (°)
22
20
18
16
14
12
10
8
6
4
2
0
$\gamma_E = 40°$
$\gamma_E = 60°$
$\Delta\gamma_E(1000) = 2.7°$
$\Delta\gamma_E(1000) = 2°$
1000
2000
3000
4000
5000
6000
7000
ΔB (Km)

FIGURE 3-19

FIGURE 3-20

Saturn/Uranus Mission

Description	Time
Earth launch	I
1st velocity correction	I + 5 days
2nd velocity correction	S - 200 days
Initiation of radio and optical tracking	S - 150 days
Termination of radio and optical tracking	S - 5 days
3rd velocity correction	S- 5 days
Saturn encounter	S
4th velocity correction	S + 50 days
5th velocity correction	U - 200 days
Initiation of radio tracking	U - 150 days
Initiation of optical tracking	U - 25 days
Pre-separation velocity correction	SEP* - 1 day
Termination of radio and optical tracking	SEP - 1 day
Bus separation maneuver	SEP
Probe entry	U
Bus periapsis	U + 1 hr

*SEP: separation

too large to be acceptable, given amount of fuel that is planned to be carried on the Pioneer mission. However, using the optical V-slit sensor and imaging the satellites at Saturn, that number can be reduced to about 22 meters per second. That is quite satisfactory. The Delta-V values are summarized in Figure 3-21. We assumed this optical navigation would be required on the way past Saturn on to Uranus.

Now to consider the Titan probe mission we recently conducted a study and on Figure 3-22 depict again the navigation accuracy in the B plane, one sigma, semi-major axis versus the end of the tracking arc. We now remember the time of the separation is somewhere around 27 days, so we stopped all the simulation right at that point and see what kind of accuracies we can get.

We examined four cases. One is a 15 and 25 arc seconds which is consistent with the V-slit sensor type of numbers that I mentioned earlier. We considered first improving those numbers (hypothetically) by a factor of 2, and then used values now being quoted for the Mariner TV or vidicon type of system that would be used in the outer planets, which is 2 and 3.3 arc seconds.

Finally, we considered radio alone navigating, starting tracking at E minus 150 days.

The radio-alone navigation is out just where we expected it, at about 8,000 kilometers. Titan's ephemeris is not significantly improved. It has a fairly large ephemeris error, since it hasn't been well observed.

Examining the 15-25 arc/seconds sytem, we find that it can yield about 700 kilometers of B plane error going into Titan. If we can improve by a factor of 2, we can get the errors to less than 500 kilometers. It is about this level of accuracy, 500 to 600 kilometers, that is required in order to target to Titan; that is to achieve a reasonable entry angle dispersion. These results are related to entry angle errors on Figure 3-23. The radio-

Saturn/Uranus Mission Midcourse
Velocity Requirements

Event	Velocity Correction Number	Mean Velocity $\mu + 3\sigma$ (m/sec)	Along Earth-line Component $\mu + 3\sigma$ (m/sec)	Normal to Earth line $\mu + 3\sigma$ (m/sec)
2	1	80.1	29.2	79.6
3	2	14.0	12.0	10.7
4	3	7.0	6.4	4.5
6[a]	4	139.3, 23.2	38.6, 11.4	138.9, 22.4
7	5	18.7, 2.9	3.9, .9	18.7, 2.8
8[b]	6	110.4	44.6	65.8

[a] The first value of each pair pertains to radio-only navigation at Saturn while the second value pertains to the optical V-slit sensor.
[b] Maximum deflection maneuver considered at 700 Uranus radii.

FIGURE 3-21

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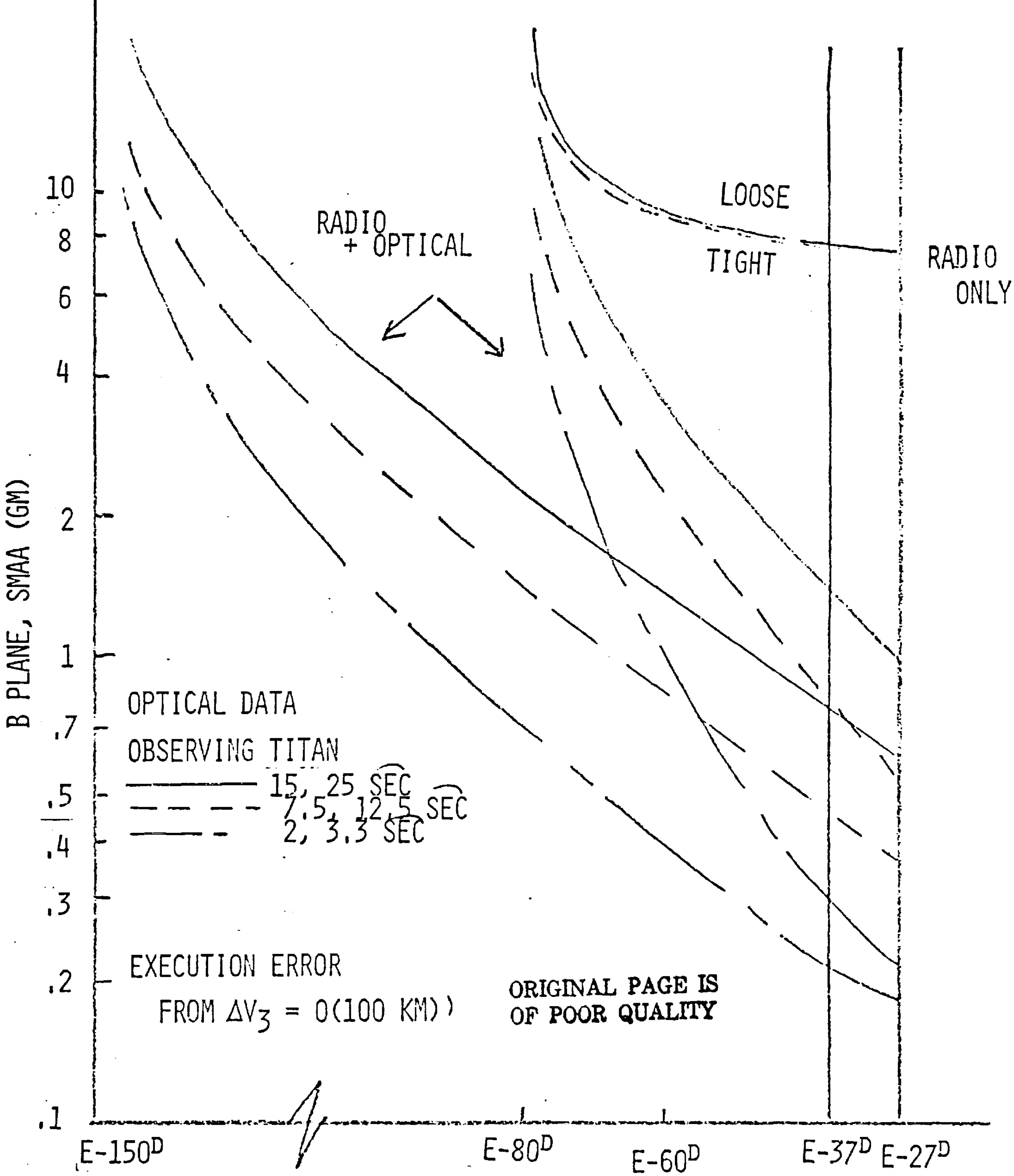
5-21

FIGURE 3-22

TITAN PROBE O.D. ERROR ON APPROACH



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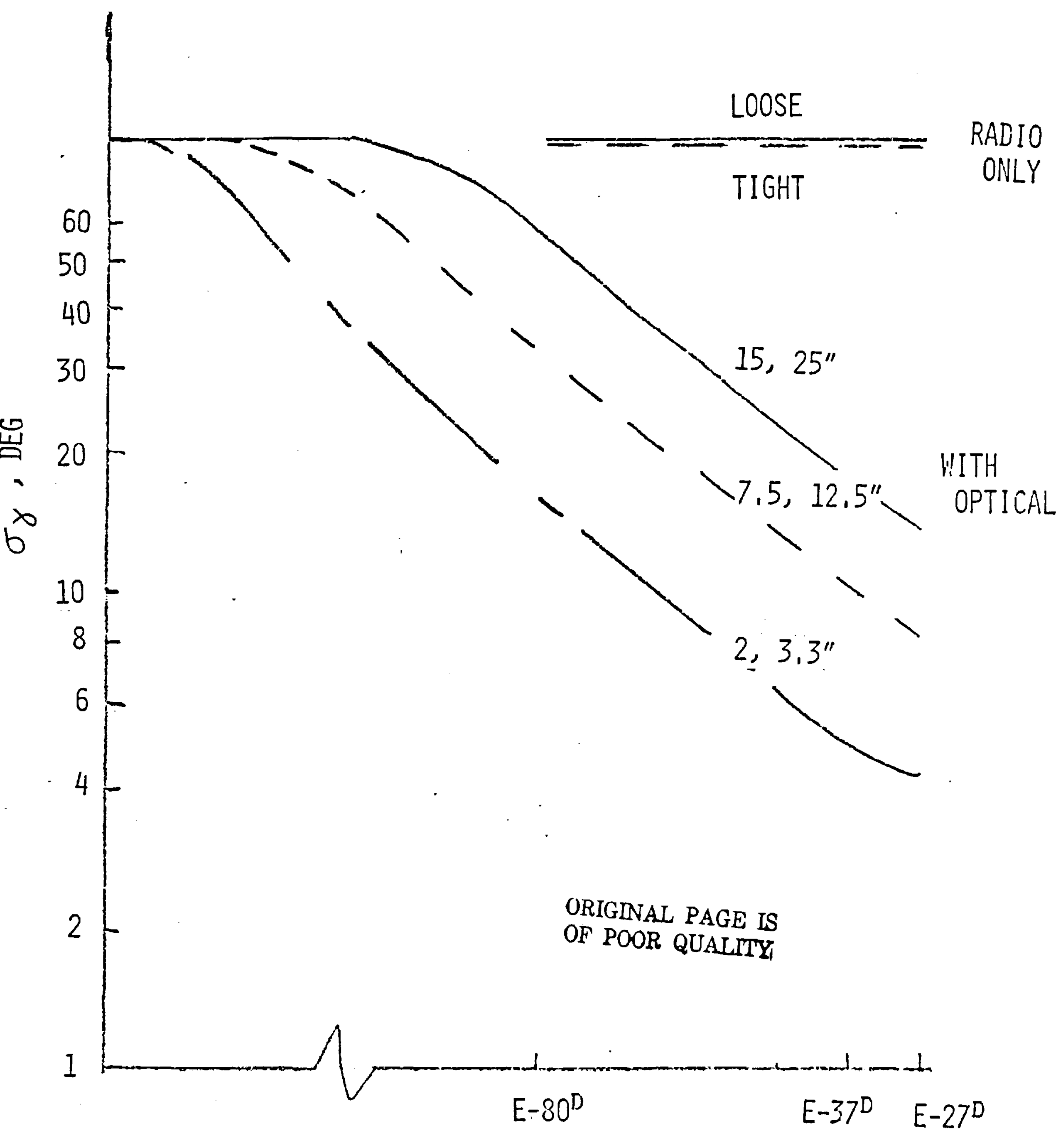
LDF - 16

FIGURE 3-23

TITAN PROBE ENTRY ANGLE ERRORS



alone, the errors would be out around 90 degrees. This is the one sigma entry angle error. Obviously it is unacceptable: you might miss the planet.

The optical navigation errors are also shown. The 15-25 arc/ seconds system gives about 15 degrees of entry angle error. That is a one sigma error, so the three sigma error would be around 45 degrees and that is pretty risky.

If we can improve the accuracy, there is a tremendous payoff as shown on the figure. One thing to be noted is that the gain from improving accuracy is far more significant than the gain from tracking longer.

There are two limitations to the V-slit sensor concept. One was the fact that it couldn't track once the object became big enough to fill the slit; and the other was that it wasn't quite as accurate as we hoped. It looks from these results like the payoff is in improving accuracy, not in making it track longer.

In Figure 3-24 the Delta-V requirements for the Titan probe mission are summarized. Our basic conclusions from the study of the outer planet Pioneer missions, that is, the direct Saturn mission, the Saturn-Uranus mission, the Jupiter-Uranus mission, and the Titan probe mission, are kind of summarized on Figure 3-25. We did find a great advantage in using differenced data, i.e. quasi-very-long-baseline-interferometry. If we delay separation a little bit, we have very acceptable errors in navigating to Saturn on the Saturn probe mission.

On Saturn-Uranus 80, the radio-alone navigation with tight station locations and with the QVLBI data and some other assumptions, might barely be sufficient at Saturn. But there was significant improvement by incorporating optical navigation there. And it was absolutely necessary at Uranus due to the pathologically poor Uranus ephemeris.

FIGURE 3-24

TITAN PROBE STUDY
GUIDANCE POLICY

MANEUVER	TIME	WHAT IT CORRECTS		ΔV	$\Delta V + 3\sigma\Delta V$
ΔV_1	$L + 5^D$	$O(3 \times 10^6)$ KM	INJECTION ERRORS	32 M/S	77 M/S
ΔV_2	$E-200^D$	$O(4 \times 10^4)$ KM	EXECUTION ERRORS IN ΔV_1	4 M/S	9 M/S
ΔV_3	$E-30^D$	O(1000) KM	EXECUTION ERRORS IN ΔV_2	3 M/S	10 M/S
		O(1500) KM	O.D. UNCERTAINTY AT $E-200^D$		
		O(5000) KM	TITAN EPH. $E-200^D$		

FIGURE 3-25

CONCLUSIONS

- SATURN '79
  - QVLBI DATA PROVIDES DRAMATIC IMPROVEMENTS
  - LATER SEPARATION REDUCES ERRORS
- S/U '80
  - RADIO ALONE MAY BE SUFFICIENT AT SATURN - HOWEVER THIS DEPENDS ON "TIGHT STATION LOCATIONS
  - RADIO/OPTICAL NAVIGATION OFFERS SIGNIFICANT ADVANTAGE AT SATURN AND
  - IS ABSOLUTELY NECESSARY AT URANUS (DUE TO PATHOLOGICALLY POOR URANUS EPHEMERIS)
- J/U '80
  - RADIO ALONE AT JUPITER IS SUFFICIENT
  - URANUS CONCLUSION IS SAME AS ABOVE

FIGURE 3-25

CONCLUSION (CONT'D)

- TITAN '84
  - RADIO ALONE DOES NOT GUARANTEE ENTRY
  - V-SLIT SENSOR ADVERTISED CAPABILITY IS MARGINAL
    - ACCURACY
    - EXTENDED OBJECT
  - BIGGEST PAYOFF IS IMPROVING ACCURACY OF OPTICAL NAVIGATION SENSOR
  - TITAN OCCULTATION BY BUS MAY BE MISSED: OPTICAL NAV ERROR RANGES FROM 50-115 SEC (700-1600 KM)

Jupiter-Uranus '80 mission yielded basically the same kinds of conclusions except that radio-alone is certainly adequate at Jupiter.

On the Titan '84 mission, the radio-alone navigation does not guarantee entry. The V-slit sensor advertised capability - realizing this is only a concept and so it might be better than presently advertised, or it might be worse - is marginal. The problem is accuracy and viewing an extended object. The major benefit is in improving accuracy.

Finally, we did look at the question of Titan occultation, which was discussed earlier. With the basic sensor levels here that we are talking about, there is a chance you would miss a Titan occultation. The optical navigation error range is from 50 to 115 seconds, that is about 700 to 1600 kilometers. Titan itself is 2400 kilometers in radius. The chances for occultation actually depend on the geometry as to how you pass by that occultation region whether or not this is sufficient accuracy.

Moving now to the Mariner-Jupiter-Uranus mission study that has been underway, we have been looking at navigation requirements at Uranus in somewhat more depth and somewhat more connected to the Mariner questions.

The situation is a little different than with the Pioneer study because we are not only concerned about the delivery of the entry probe, but we are concerned about imaging the satellites of Uranus on the way in (Figure 3-26). It turns out, not too surprisingly, that we can do a better job than we could in the Pioneer-Jupiter-Uranus study of delivering the entry probes simply because the Mariner vidicon yields far better accuracy. We also looked a little more into the question of the Uranus ephemeris and will modify our conclusions about that. Imaging of the satellites for scientific purposes yields an additional requirement on the navigation system which turns out to be the tighter one rather than delivery accuracy for the probes.

FIGURE 3-26

NAVIGATION REQUIREMENTS AT URANUS

1. DELIVERY OF THE ENTRY PROBE

2. CONTROL OF THE SATELLITE IMAGING
   FIELD OF VIEW

JFJ
5/16/74

In Figure 3-27 the relation of required accuracy on approach (in the B plane) to entry angle error is shown. Again, 40 degrees is nominal plus or minus a probable requirement of ten degrees. This is three sigma accuracy, so one sigma accuracy requirement is about 2,000 kilometers.

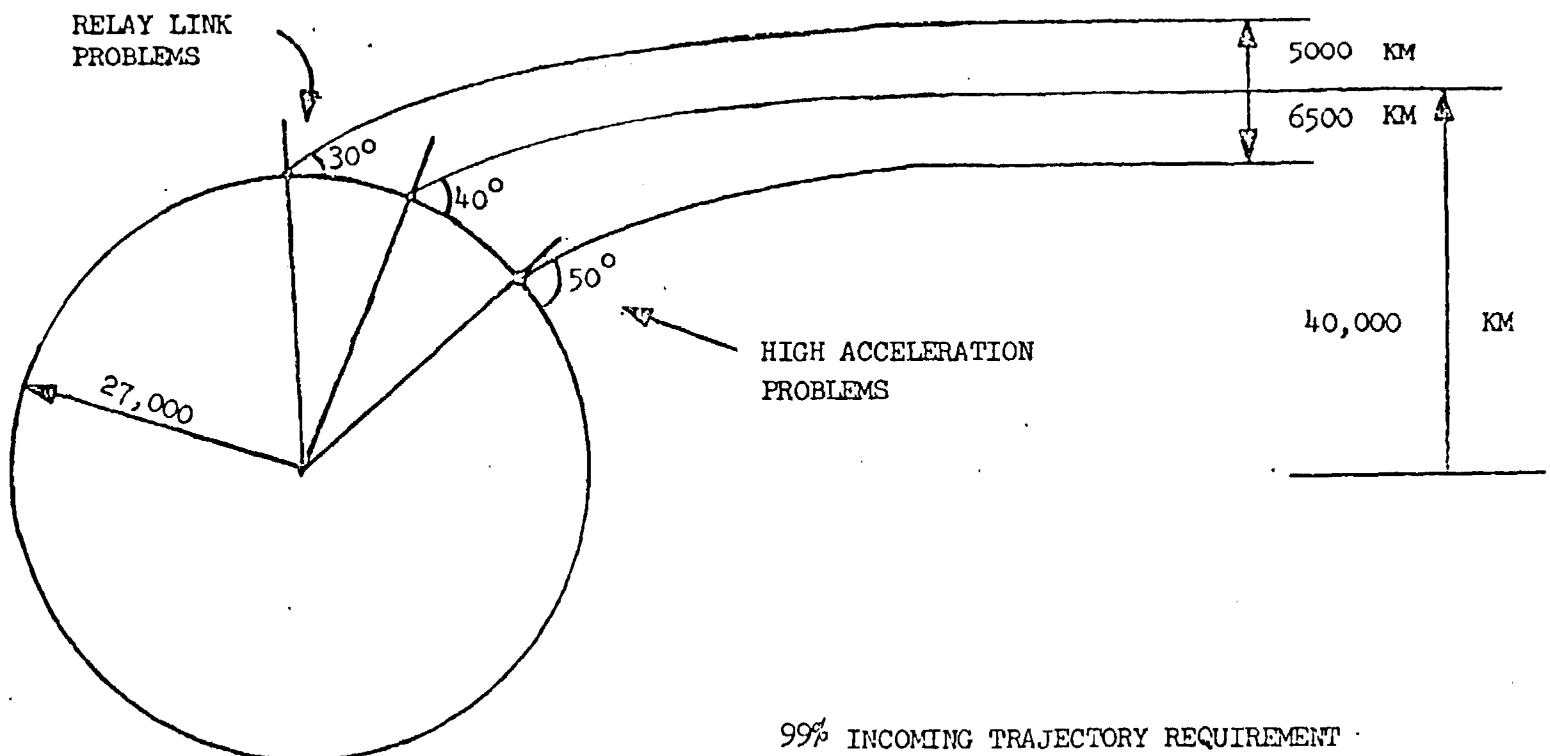
The second requirement, for navigation follows from noting that a trajectory knowledge error can result in a missed satellite image (cf Figure 3-28). This turned out to be an important requirement.

The optical navigation that we studied used the 1,500 mm focal length TV camera. The characteristics are shown in Figures 3-29 & 3-30 for the two types of requirements mentioned above. We investigated two types of imaging systems, one based on the Mariner-Jupiter-Saturn vidicon and one based on a proposed CCD, Charge Coupled Detector; and they have slightly different properties by a factor of two in terms of pixel size.

The conclusions of the study are shown in Figure 3-31. Optical navigation is not required for the entry probe <u>if</u> you improve the Uranus ephemeris. Now we pointed out when we did the Pioneer-Jupiter-Uranus study that we were basically stuck with this 8,000 to 10,000 kilometer level of ephemeris uncertainty. Some recent investigation has suggested that this is true, but that probably with a modest expenditure - modest in terms of project ephemeris development - the Uranus ephemeris, over a number of years could be improved. This would involve collecting all the old observations and incorporating the new observations over this next five-year period. This could bring Uranus ephemeris to the level of about 2,000 kilometers. Recall that 2,000 kilometers is about the level we needed.

FIGURE 3- 27

PROBE DELIVERY



99% INCOMING TRAJECTORY REQUIREMENT
IS 6000 KM

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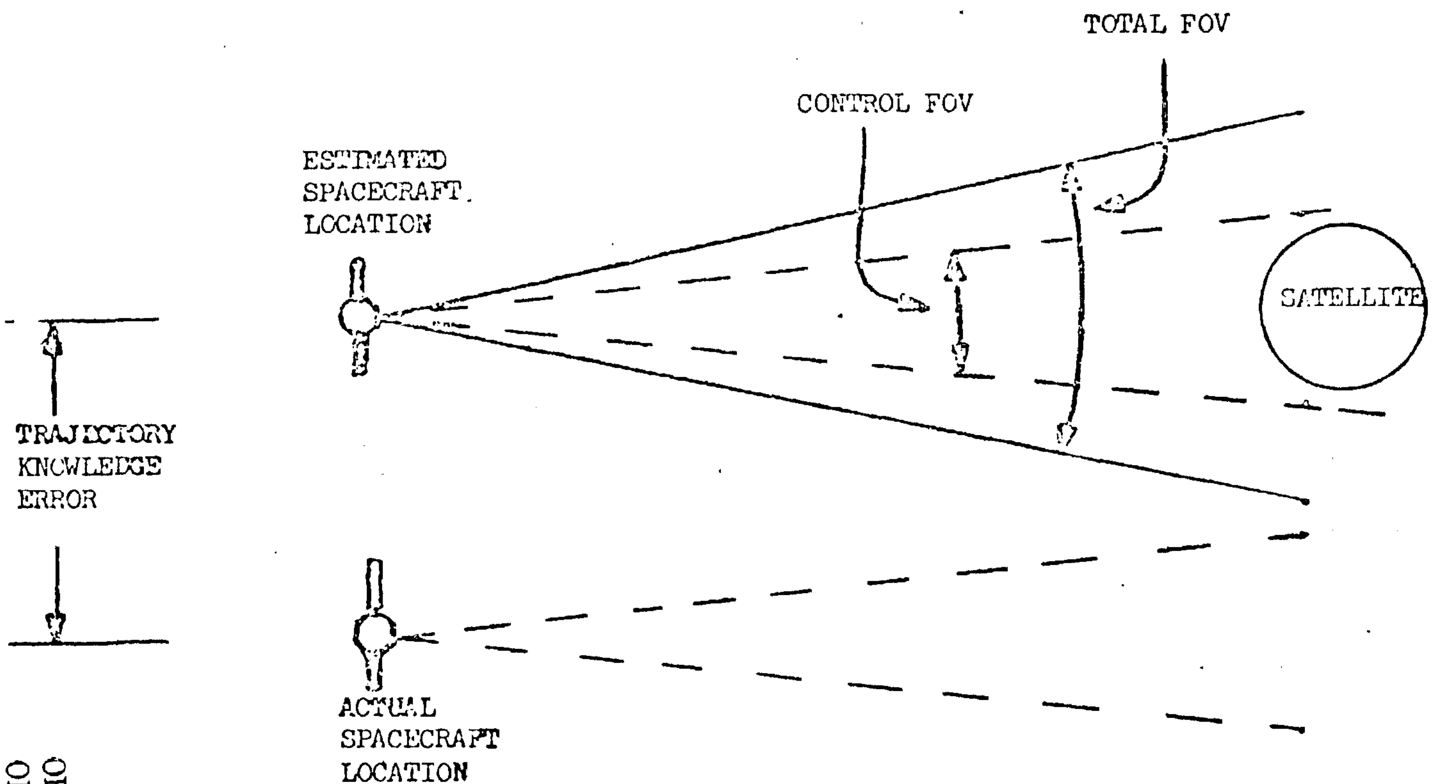
FIGURE 3-28

A TRAJECTORY KNOWLEDGE ERROR CAN RESULT IN A MISSED SATELLITE IMAGE



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CHARACTERISTICS OF OPTICAL NAVIGATION AND ITS CAPABILITY FOR ENTRY PROBE DELIVERY

1. USE 1500 mm CAMERA

2. OPERATE BETWEEN E-130 DAYS AND E-25 DAYS
(< 1000 PICTURES)

3. OBSERVE THE URANUS SATELLITES AGAINST 5th to 10th MAGNITUDE STAR BACKGROUND

4. MJS VIDICON CAPABILITIES: 300 km $\sim \Delta\alpha = 1/2^{\circ}$

5. CCD CAPABILITIES: 600 km $\sim \Delta\alpha = 1^{\circ}$

6. POSSIBILITY OF REDUCING THE SHIELD/STRUCTURE WEIGHT FROM THAT OF THE CURRENT DESIGN

FIGURE 3-29

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CHARACTERISTICS OF OPTICAL NAVIGATION AND ITS CAPABILITIES FOR CONTROL OF SATELLITE IMAGING FIELD OF VIEW

1. USE 1500 MM CAMERA
2. OPERATE BETWEEN E-15d and E-5d (< 300 PICTURES)
3. OBSERVE SATELLITES AGAINST STAR BACKGROUND
4. MJS VIDICON CAPABILITIES: SPACECRAFT TRAJECTORY AND SATELLITE EPHEMERIDES ACCURATE TO 150 KM
5. CCD VIDICON CAPABILITIES: SPACECRAFT TRAJECTORY AND SATELLITE EPHEMERIDES ACCURATE TO 300 KM

FIGURE 3-30

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URANUS NAVIGATION SUMMARY

1. OPTICAL NAVIGATION NOT REQUIRED FOR ENTRY PROBE

2. IMPROVED URANUS EPHEMERIS IS REQUIRED; COST 250K

3. OPTICAL NAVIGATION MAY ALLOW REDUCED PROBE WEIGHT

4. SATELLITE IMAGING REQUIREMENTS CANNOT BE MET WITH RADIO-ONLY NAVIGATION

5. OPTICAL NAVIGATION ALLOWS SATELLITE IMAGING REQUIREMENTS TO BE MET

FIGURE 3-31

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Thus, improving the Uranus ephemeris, if it can be done, would allow use of radio-alone navigation, albeit somewhat marginally, to target the entry probe. There is considerable payoff from use of optical navigation, in reducing the entry angle errors.

However, the satellite imaging requirements cannot be met with radio-alone navigation. Several different schemes were investigated and it was found that either too many pictures or too much data rate was required or it took too long to get back all the pictures with radio-only navigation errors (even in the case of the improved Uranus ephemeris). Hence, optical navigation was incorporated to allow the satellite imaging requirements to be met. The requirements could be met with either a vidicon or CCD imaging system.

In summary, we have done a number of outer planet probe studies and found some particular cases where optical navigation is important and some cases where radio-alone navigation will suffice. We have estimated maneuver sizes that are acceptable to the mission designs.

MR. DAN HERMAN: How long does it take to get an orbit determination update after a V-slit sensor observation of one of those satellites? What is the time, approximately?

MR. FRIEDMAN: You mean the time involved in the real mission?

MR. HERMAN: Yes, including observation and including the time it takes to get an alternate determination.

MR. FRIEDMAN: Basically, of course, you are going to be limited by the round-trip light time. Above and beyond that, this problem hasn't really been factored into the simulation. I have heard estimates through other studies that we have been doing, estimating about a couple of hours once you get the data back to Earth. But, of course, you have to live with the round-trip light time.

MR. HERMAN: The question I was alluding to was have you done any work yet on developing the ground software to accommodate the optical data as well as the radio data?

MR. FRIEDMAN: Yes. For the Mariner system, we tested experimental use of this data; on Mariner 1971 and on Mariner 1969. It is being further developed and used on the Viking mission and it will be completely operational on the MJS mission. By that time we will have operational navigation software to include optical navigation measurements.

MR. HANS MEISSINGER: With regard to making sure that you are aiming the camera at the fast-moving satellite during the short encounter, you can use the camera system and the feedback system and try to correct it as you go; namely, the field of view is large enough to encompass the satellite in a very small area and you can keep it centered that way by autonomous feedback without ...

MR. FRIEDMAN: In actual operation, that might be done but it requires early commitment to do it. I don't think it is an easy job. If that was a requirement, and I am not sure it is, I think that could be put on the thing.

MR. SEIFF: What is a representative number for the uncertainty in the position of one of the satellites relative to the planet?

MR. FRIEDMAN: I think it is about 5,000 to 6,000 km, at present. However, the Galilean satellites are quite a bit less than that.

MR. SEIFF: So that is right at the limit of what you want to allow in terms of entry flight path angle. I notice you were reporting 6,000 km and the desired uncertainty in the B plane for Uranus and the uncertainty in the position of the satellite is comparable to that.

MR. FRIEDMAN: Well, it is even worse than that because for 5,000 or 6,000 km for Titan, that is one sigma, and the uncertainty in the entry angle that you want is three sigma.

That has been factored in. That was basically why radio-alone navigation at Titan did not suffice to meet the entry angle requirements. It wasn't even marginal; it just missed. Is that fair, Kent?

MR. KENT RUSSELL: Yes

DR. W. DIXON: The point should be made, though, that if you use a satellite as your navigation target, then the process of navigating also refines your knowledge of the ephemeris of that satellite, in addition to figuring out what the safest entry angle is.

MR. FRIEDMAN: Oh yes, that is correct. That has been completely factored in, too, in the optical navigation. But we just didn't quote the ephemeris improvements.

DR. DIXON: So If you aim a probe at Titan and you use Titan as the target for navigating, then you also refine where it is as well as where the spacecraft is. It is possible to hit it even if you didn't know where it was to begin with.

MR. FRIEDMAN: That's right, yes, but only with the optical navigation. But that has been factored into the optical navigation results. The results are quoted in terms of spacecraft state relative to Titan, implicit in that is the fact that Titan's ephemeris, relative to earth is improved. It just isn't quoted in those terms.

THE PIONEER SPACECRAFT AS A PROBE CARRIER

Dr. William Dixon
TRW Systems Group

DR. WILLIAM DIXON: What I am going to talk about is the use of the Pioneer spacecraft for probe missions to the outer planets. For this purpose, the Pioneer 10 and 11 spacecraft is taken as the baseline.

The first chart (Figure3-32 is a summary chart and was intended to perhaps be somewhat introductory for this talk and the next one. I have talked with Jim Hyde at JPL about it. What I want to do here is pick out the areas of accommodation that a spacecraft has to have, the characteristics it has to have for this type of mission and then select those in which there is a significant contrast in the characteristics of the Pioneer and Mariner approaches.

The principal areas we thought have to do with the weight availability for carrying the probe, certain aspects of the probe-to-bus link communications and on-board navigation, which has been touched on by Lou Friedman just now. And I'll come back to that later.

I think there is one other difference in philosophy which is worth pointing out here. I am talking about the adaptation of a spacecraft design, a spacecraft which has already been designed, built, and flown and, to some extent, completed its flight objectives. Jim is going to be talking about how you would do these missions with a Mariner. I think he will take as a baseline the Mariner-Jupiter-Saturn and apply it to Mariner-Jupiter-Uranus. Those are spacecraft which have not been built and for which the design is not yet committed.

So when I say "What do you have to do to a spacecraft to accommodate a probe," we have to go back and change something that has already been built and he still has the option of incorporating certain things into the design as it proceeds. And this makes a little difference in philosophy.

MARINER AND PIONEER AS PROBE BUSES, AREAS OF CONTRAST

	PIONEER	MARINER
WEIGHT MARGIN AFTER ACCOMMODATION OF PROBE*	(SU) ~ 100 LB (S) ~ 200 LB (J) ~ 1100 LB	(JU) WEIGHT INCREASE HANDLED BY TRIP TIME PENALTY OF ~1 YEAR (SU, S) CANNOT BE DONE (J) ~ 200 LB (DEPENDS ON LAUNCH OPPORTUNITY)
PROBE-BUS LINK COMMUNICATIONS		
HIGH-GAIN (PENCIL BEAM)	DESPUN ANTENNA	FIXED ANTENNA
MEDIUM-GAIN	AXISYMMETRIC FIXED ANTENNA	
ON-BOARD NAVIGATION SENSING (BEYOND GROUND-BASED RADIO)	SPINNING SENSOR HAS GREATER SKY AREA IN SWATH	FIXED SENSOR HAS GREATER SENSITIVITY (DIMMER TARGETS); CAN SEE SATELLITES FROM FARTHER OUT

FIGURE 3-32

* ATLAS/CENTAUR D-1T/TE 364-4 LAUNCH VEHICLE

On the weight margins - we'll justify these somewhat later - we have looked at the Pioneer for Saturn-Uranus missions. (The underline under the U means Uranus is where the probe goes.) There is roughly a hundred pound margin over what the launch vehicle can carry. We are talking about the same launch vehicle in all cases, the Titan/Centaur/TE 364 launch vehicle.

For a direct mission to Saturn, the spacecraft can be lighter, providing a 200 pound margin.

For a Pioneer to take an atmospheric entry probe to Jupiter, you get an eleven-hundred-pound margin. This is consistent with John Wolfe's discussion this morning that there is enough margin that you can consider an orbiter mission at the same time as a probe mission, in conjunction with it.

The Mariner people first looked at a Jupiter-Uranus mission without a probe. When they put the probe on, there is a certain weight increase and that increase can be accommodated on the launch vehicle, but it comes about by increasing the trip time about one year for every hundred kilograms; and 100 kilograms is roughly what the weight increase is.

I think on the Mariner, using the same launch vehicle, if Saturn is the first stop, I say it cannot be done here, either Saturn-Uranus or Saturn direct mission. Maybe I should qualify that. Most of what we have looked at for Saturn are launches in the late '70's or the early '80's, and that turns out to be about the worst possible time to go to Saturn. If you looked at a different part of the Saturnian year, you might get an improvement and maybe it can be done.

My estimate of the Mariner margin for a Jupiter-only probe is 200 pounds. That would also depend on the launch opportunity somewhat.

In the area of the communications link, we have primarily a different characteristic because Pioneer is a spinning spacecraft and the Mariner is 3-axis stabilized. As Byron Swenson's pictures showed, communication from the entering probe is to the spacecraft's aft hemisphere. With the rotating Pioneer, the easiest thing to use is an axisymmetric fixed antenna. But you are wasting a lot of your beam. It runs around the whole range of spacecraft centered longitudes or clock angles and so it does not have a very high gain. If you want a higher gain, like a pencil beam, you have to despin the antenna on the Pioneer. But with the Mariner, you can use a more direct or fixed antenna. So there is a potential, say, for equal amounts of mechanical complexity using fixed antennas of about a six or seven dB improvement on the Mariner.

Lou Friedman, talking about navigation, has pointed out that certain of the planetary probe objectives can be handled with radio navigation alone. So this comparison of optical navigation applies in other cases, particularly for probe missions to Uranus and for probe missions to the satellite Titan.

Mariner proposes to use a TV camera or vidicon-like sensor. Being 3-axis stabilized, it has a potential for using a longer exposure and having greater sensitivity. Therefore, it can see dimmer targets, it can see certain satellites from farther out.

For the Pioneer, the sensing we have proposed is the V-slit sensor. It has trouble seeing stars much dimmer than fourth magnitude and, therefore, you have to come closer to see them. Your navigation time might be restricted. One compensating point is that a spinning sensor has a greater sky area in the swath. You can use fixed stars from the entire roll - three degrees by a complete revolution - your guidepost for navigating. However, if you are going down to dimmer targets, there are probably more stars per square degree that you can see, anyway. So I think these are areas of greatest contrast.

With Figure 3-33 we will talk about just the Pioneer. Figure 3-33 is a model very similar to the one that John

MODEL OF PIONEER 10 AND 11

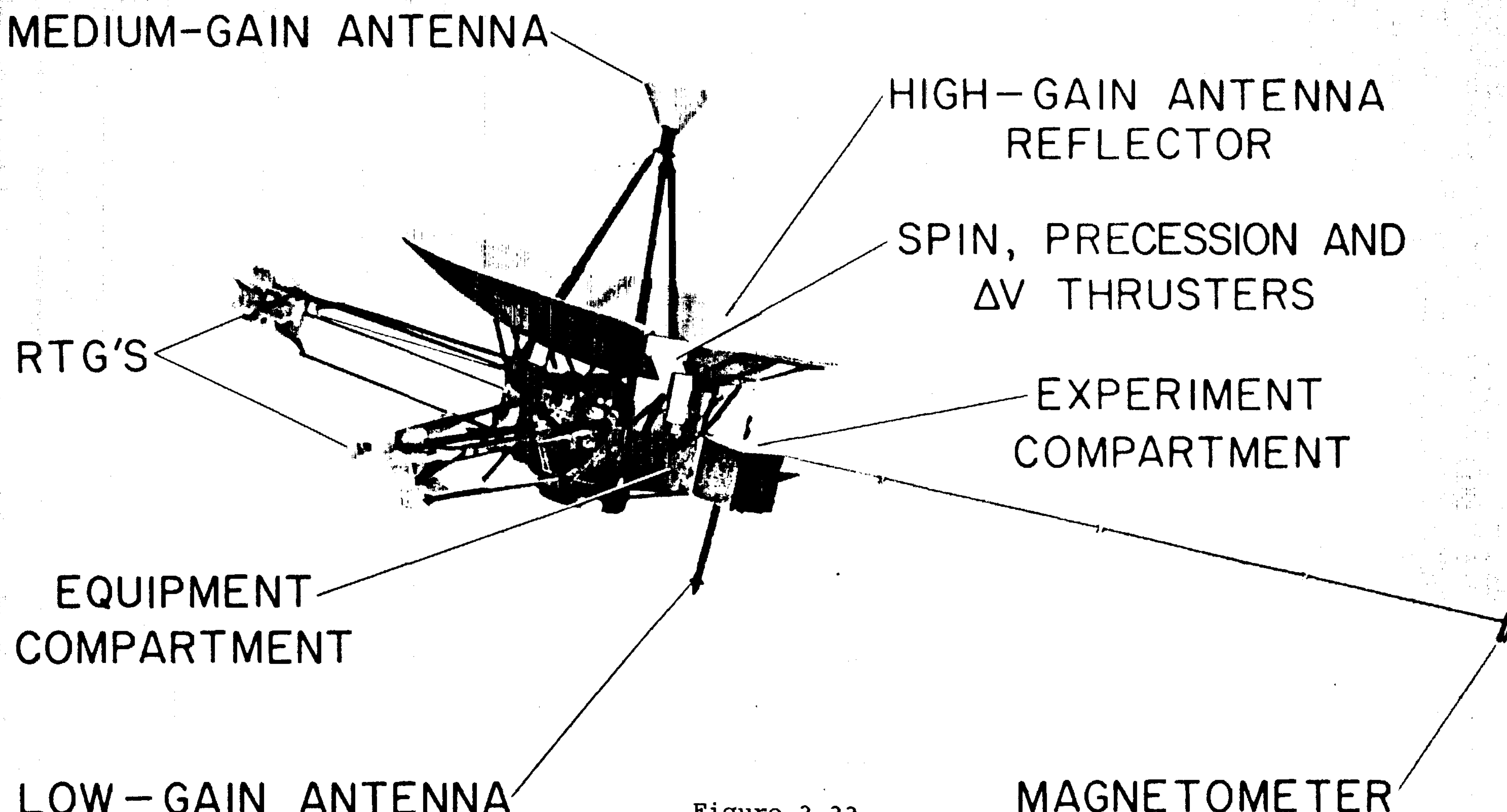

Figure 3-33

Wolfe showed of the Pioneer F&G spacecraft. I am not going to go through it in any detail; I just want you to see what it is like because when we put a probe on we will see how it differs. It is spin stabilized. It has a large dish with an antenna beam along the spin axis. For that reason, we do keep the spin axis pointed toward the Earth, or close to it, during the cruise phase when we are far from the Earth. If you point it significantly far from the Earth, then you do lose downlink communications.

The plane at the bottom is the interface between the spacecraft and the launch vehicle.

Figure 3-34 shows how that region of the spacecraft is used to accommodate a probe. This is looking at the Pioneer from the bottom end. Above the probe adapter which expands out to a 37-inch diameter, is that same interface. The probe adapter matches a standard 37-inch diameter third stage adapter. And the probe, which you will see plenty of other designs of, has a 35-inch diameter which fits within the probe adapter.

This particular version comes from a study of a Saturn-Uranus probe mission, and I might add that it incorporates a number of things that are required because you are going to Saturn and Uranus. In other words, there are differences for the Pioneer if you are going to send it out to Uranus whether you take a probe or not. There are also differences for the Pioneer if you put a probe on it, whether you go out to Uranus or not. So I am going to try to distinguish between those two classes.

Because of the Uranus mission, we do have a star mapper, a navigation device; we have a multi-hundred watt RTG and we have X-band capability.

We have also replaced what was an omni-antenna in the back of the spacecraft by a combination antenna. There is a loop-vee antenna which gives the sort of pattern Byron Swenson indicated was necessary.

PIONEER SATURN URANUS FLIGHT SPACECRAFT

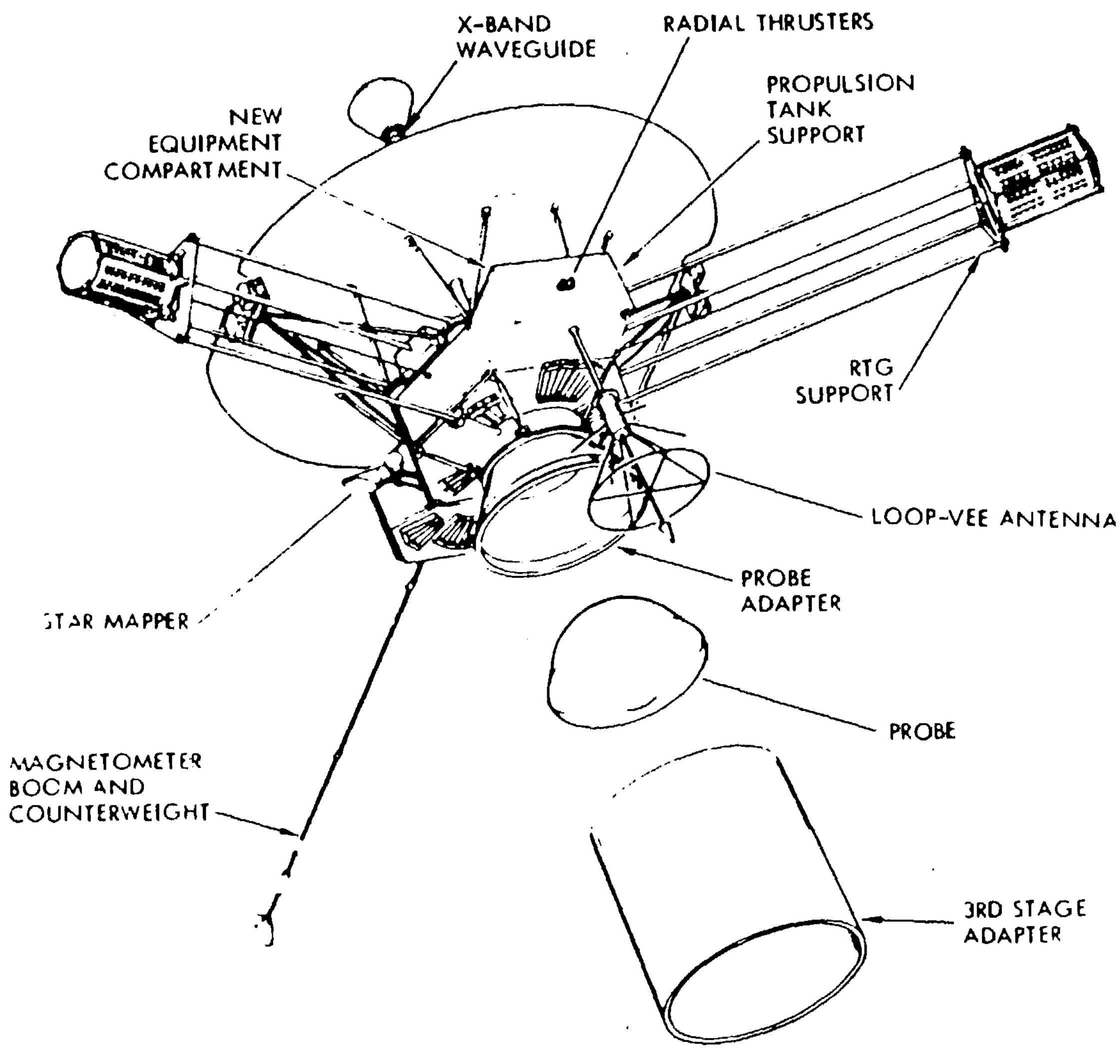

FIGURE 3-34

It is not the full rear hemisphere, but it is a hollow cone-shaped pattern for receiving signals from the probe as it descends. And then we have put the S-band omni-antenna on the end of that.

For this mission and this type of antenna, 400 megaHertz was the link frequency between the probe and the bus. Bus-to-Earth communication is at S-band, around 2300 MHz.

Figure 3-35 shows some details, and probably more than you can see. We have now turned the spacecraft on its side. On the right is the third stage of the launch vehicle. In the center is the probe with the business end toward the right - that is the heatshield end. And it is based on the McDonnell probe concept of which you saw a model this morning. The Pioneer equipment compartment is to the left and the dish would be out of the picture to the left. The newly added conical section is seen to the left of the probe. The probe itself is held at three points by bolts which can take all of the launch loads and can be separated by ordnance to release the probe to the right.

There is a modification in the adapter so that you only need one separation. You separate the launch vehicle from the spacecraft at this point "B". Then, when the probe goes, there is no other separation that has to be made.

Figure 3-36 shows the weight of Pioneer missions. In Column 1 we have the weight of Pioneer G (or Pioneer 11) as launched. Of course, it didn't carry a probe so it has 442 pounds of spacecraft not counting propellant or instruments; 67 pounds of instruments; 59 pounds of usable propellants, for a total weight of 568 pounds. The adapter was about 30, and there was not a lot of margin. That is about what the Atlas Centaur TE 364 could send to Jupiter.

When you put a probe on, you have to go to the Titan launch vehicle if you are going to Jupiter or beyond. So these other three cases show it with a Titan.

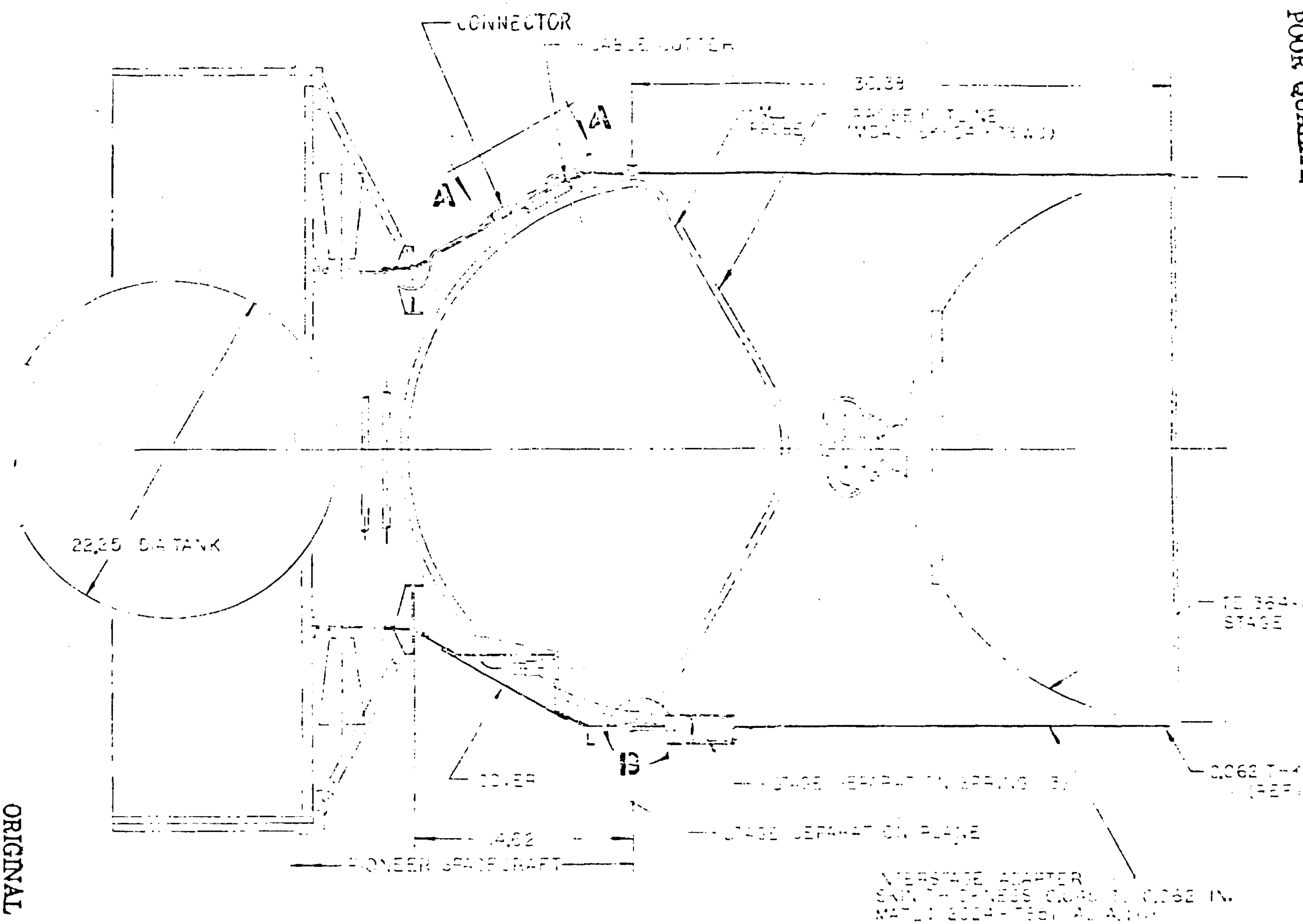

FIGURE 3-35
BUS PROBE INTERFACE LAYOUT

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SPACECRAFT WEIGHT

	PIONEER SPACECRAFT/MISSION							
	G (11)		SATURN PROBE		SATURN/ URANUS PROBE		JUPITER PROBE	
	KG	LB	KG	LB	KG	LB	KG	LB
SPACECRAFT (EXCLUDING USABLE PROPELLANT, INSTRUMENTS, PROBE)	200.7	442.5	240.3	529.8	280.3	617.9	245	540
INSTRUMENTS	30.4	67.0	27.9	61.5	27.9	61.5	32	70
USABLE PROPELLANT	26.9	59.2	34.8	76.7	50.4	111.2	41	90
PROBE	-	-	113.4	250	113.4	250	154	340
SPACECRAFT WEIGHT AT LAUNCH	258.0	568.7	416.4	918.0	472.0	1040.6	472	1040
ADAPTER	13.6	30.0	27.0	59.5	27.0	59.5	27	60
GROSS WEIGHT	271.6	598.7	443.4	977.5	499.0	1100.1	499	1100
INJECTION ENERGY REQUIRED* (APPROXIMATE)	93		140		140		100	
LAUNCH VEHICLE**	A		T		T		T	
LAUNCH CAPABILITY	277	610	549	1210	549	1210	998	2200
WEIGHT MARGIN	5	12	106	232	50	110	499	1100

*KM^2/SEC^2

**A = ATLAS CENTAUR/TE-364-4; T = TITAN/CENTAUR/TE-364-4

FIGURE 3-36

What we did in a study a year ago was a spacecraft that could take a probe to either Saturn or Uranus or both, according to the old plan. That probe was deemed to weigh 250 pounds, although we understand there is a significant margin within that. The spacecraft's dry weight increased about 170 pounds for a number of reasons, and the propellant weight also went up quite a bit to handle all of the maneuvers we are talking about. The bus experiment payload for that mission was a selected payload which was 61 pounds, so the whole thing came out 1040 pounds, or eleven hundred with an adapter. And with the adapter, with a nominal C_3 of 140, the approximate launch capability is around twelve hundred pounds. So that was 100 pounds of margin. (Column 3).

If you make it only a Saturn probe (Column 2) - as we will see in a moment - there are a number of provisions required for Uranus that don't have to be put on; and it would be considerably lighter.

Looking at the Jupiter probe (Column 4), the first indications are that the probe itself, needing a significantly heavier heatshield, would weigh about 340 pounds compared to 250. But the spacecraft, again, would reflect more the Saturn than the Uranus requirements; they would not be so heavy, science just nominally selected, propulsion just a little more than Saturn because you have a somewhat larger deflection at Jupiter. And this is where the eleven hundred pound margin comes. To Jupiter, 100 km^2/sec^2 is typical launch energy.

I might add that these are approximate. They depend a lot on just what launch year and what launch window and other definitions you need are.

Figure 3-37 summarizes the requirements and the impact on the bus to carry a probe. Suppose we start with Pioneer F&G, which is basically a Jupiter mission. We add a probe - I am still talking about a Jupiter mission, and we will look later at what it takes

INFLUENCE OF PROBE ON BUS DESIGN

ITEM	REQUIREMENT	IMPACT
PROBE WEIGHT	SUPPORT STRUCTURE: INTERFACE AREA	ROUTINE
	MASS PROPERTIES CONTROL: DEPLOYMENT COUNTERWEIGHT	MODERATE
THERMAL CONTROL (PROBE)	PROVIDE -20°F TO +32°F ENVIRONMENT	ROUTINE
THERMAL CONTROL (BUS)	ANALYZE EFFECT OF PROBE POSITION	MINIMAL
PROBE-RELATED MECHANISMS AT SEPARATION	NEW MECHANISMS AND ORDNANCE; EXTEND ORDNANCE-FIRING CIRCUITRY AND COMMANDS	MODEST
ELECTRICAL POWER	PROBE THERMAL (≤4 W STEADY) CHECKOUT AND BATTERY CHARGE (DUTY CYCLE BUS INSTRUMENTS) SEPARATION ORDNANCE (TRANSIENT: CAPACITOR DISCHARGE)	NO IMPACT ON RTG COMPLEMENT
PROBE TELEMETRY (PROBE-BUS RELAY)	LINK ANTENNA ON BUS	MODEST
	RECEIVER-BIT SYNCHRONIZER } ON BUS PROBE DATA BUFFER }	ROUTINE
	DATA STORAGE CAPACITY INCREASE (BACKUP MODE)	MODEST
	DATA HANDLING (88 BITS/SEC)	NONE
	RF HARDLINE FOR CHECKOUT BEFORE SEPARATION	ROUTINE

FIGURE 3-37

to extend that out to farther planets. You have the weight of the probe, and we have already demonstrated that that is within the capability of the launch vehicle. As far as the bus is concerned, you need a support structure, an interface area which I have described, and those are routine structural modifications. Mass properties control: on a spin-stabilized spacecraft we have to exert specific control over the principal axis to keep it coincident with the antenna axis, so there are some moderate things we do there, including a counterweight to accommodate the probe weight beneath the spacecraft.

Thermal control of the probe: this was the general requirement, primarily catering to the battery aboard the probe. Although it was permissible to deviate from that early in the mission, that was felt to be a routine thermal control requirement on the spacecraft and not requiring much power. We also have to worry about the thermal control of the bus. Putting the probe in this region of the spacecraft does block the radiation path through the louvres a little bit. We feel that the physical impact is minimal but the analysis is something that has to be done.

There are mechanisms that have to be added so that at separation we can do things like cut cable, fire squibs on the probe, and fire these ordnance activated bolts that actually separate the probe. We feel that is a modest requirement. We have circuitry on the Pioneer now that fires ordnance. The chief difference is that is normally done soon after launch. For this mission, it would be done close to the end of the mission and so it would take additional analysis and tests to verify that the circuitry meets the lifetime requirements.

Electrical power is interesting; really no impact on the RTG complement. The reason is that the probe thermal requirements are very small, less than four watts steady power. The check out and battery charge are things that you can do by duty cycling. This would be done only at isolated times during the mission. The battery would probably be charged just once before probe separation; and for those purposes, you could turn off the instruments on the bus without really harming their mission and use that power. So the presence of the probe does not really aggrevate your RTG requirements at all.

The probe telemetry, using the probe-to-bus relay has a number of requirements. Besides the link antenna on the bus, we need a receiver, bit synchronizer, a probe data buffer. (The probe data comes from one clock and the data is handled on the spacecraft from another clock. And, because, of course, they are opposite ends of the link, they are not synchronized, so you need a small buffer.) Data storage capacity increase: We regard a primary mode as relaying probe data to Earth in real time. The backup mode is to store it on the spacecraft for later transmission. This is in case, for example, of a ground station being down at that instant; you wouldn't want to lose all of the probe data. In our studies the probe would transmit data at an information rate of 44 bits per second, but it is coded two-to-one so it is actually sending 88 symbols per second. The spacecraft would not decode it, so it would have to continue to handle 88 bits per second in its downlink transmission. But that is not a problem. You will see that in a moment on another chart.

Also, for check out of the probe while it is still attached to the bus, there is an RF hardline which would use the same channels on both the probe and the spacecraft, except it would bypass the antennas.

One other requirement which I didn't list here and has been mentioned is the requirement for propulsion capability. We feel the Pioneer is sort of naturally suited for three things: it provides the probe with trajectory control, orientation control, and spin rate control. And these are things it does using the propulsion system essentially as it stands, except, as I have noted, you would have to have greater propellant capability to handle the bus deflection maneuver after separation.

I think the trajectory control is exemplified by Pioneer 10 through its trajectory control or propulsive control achieving an occultation by Io, one of these little satellites far away whose position is not known too well and it is not too big. But I think the fact that this occultation was attained shows that the Pioneer spacecraft, with its propulsion system and radio navigation alone can hit targets the size of a Galilean satellite. That is really the point involved here.

Secondly, the orientation control; Byron Swenson observed it was a constraint that the spacecraft remain Earth pointing at all times. Actually, I don't think that is quite a concrete constraint. It is an operational constraint. The spacecraft has the capability of being directed to point away from the Earth and do something and come back to the Earth, even if that interim attitude takes away your downlink communication. In fact, Pioneer 10 was pointed away from Earth line after the Jupiter encounter. The encounter was last December and this maneuver was around February. It was pointed away and it was out of communication with Earth for a couple of weeks. So it is strictly an operational constraint and not a physical limitation.

On the other hand, I think the mission analyses that have been presented show that releasing the probe in an Earth line attitude is a natural way to control its attitude and still achieve very small angles of attack upon entry. That is, generally speaking, the trajectories that come around each planet in a counter-clockwise manner, approaching with a relatively low angle of attack are those in which the entry trajectory is approximately parallel to the Earth line so that this constraint is not a harmful one.

Figure 3-38 shows what carrying the probe requires of the bus, and I was doing that generally thinking in terms of a Jupiter mission, because that is what the Pioneer F&G does.

Figure 3-38 shows what happens if you make the target planet Saturn or Uranus. Mission duration increases, as shown.

FIGURE 3-38

INFLUENCE OF TARGET PLANET SELECTION ON BUS DESIGN

ITEM	TARGET PLANET		
	JUPITER	SATURN	URANUS
MISSION DURATION (YEAR)	1.4	3.4	6.9
COMMUNICATIONS (BITS/SEC)			
PIONEER 10/11, 8 W S-BAND	1024 (OK)	256 (OK)	32 (INADEQUATE)
8 W X-BAND	≥2048	≥2048	512 OR 1024 (OK)
NAVIGATION	RADIO (OK)	RADIO (OK)	RADIO (DOUBTFUL)
POWER (WATTS)			
PIONEER 10/11 4 SNAP-19	144-150 (OK)	125-134 (OK)	88-102 (INADEQUATE)
2 SNAP-19 (HPG) OR 2 MHW			190 (OK)
SUMMARY	PIONEER F/G IS OK	PIONEER F/G IS OK	• POSSIBLE SELECTIVE REDUNDANCY AUGMENTATION • ADD X-BAND COMMUNICATIONS • ON-BOARD OPTICAL NAVIGATION SENSOR IS DESIRABLE • INCREASE RTG POWER SOURCE CAPACITY

In communications, the Pioneer system's eight watts at S-Band, gave us 1024 bits/second from Jupiter, which is okay for this mission. We would project 256 bits/second at Saturn, and that is still satisfactory for the probe mission. Thirty-two bits/second is all you would get at Uranus, so that is inadequate.

The point here is that to go to Uranus, you have to improve the communications; that is, to conduct a probe mission at Uranus. We propose incorporating X-Band, which would get you plenty in terms of bit rate.

Navigation, I think this has already been discussed. Radio is doubtful, and we would propose an on-board optical navigation sensor at Uranus; and also for Titan, which I haven't listed explicitly here.

In terms of power, if we take the Pioneer 10-11 experience, we would measure at Jupiter arrival about 144 to 150 watts. At Saturn, somewhat less. But the spacecraft budget is only about 105 watts with everything turned on, so this is okay and gives you margin to add things for the probe, which only needs a few watts.

Projecting it out this long (to Uranus), the power is not expected to be adequate for a probe mission so we would also talk about increasing RTG power source capacity.

In conclusion, I have separated the requirements on the bus in what you would do to carry a probe; and also looked at what you would do to move the target planet beyond Jupiter. The Pioneer 10 and 11 design, adapter to carry the probe, is adequate for Jupiter and Saturn. For a Uranus probe mission, additional spacecraft modifications are necessary, as shown.

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THE MARRINER SPACECRAFT AS A PROBE CARRIER

James Hyde

Jet Propulsion Laboratory

Early in 1973, the Outer Planets Science Advisory Committee expressed interest in both a Mariner flyby mission to Uranus, and a Pioneer Saturn/Uranus Probe mission. JPL was also conducting a study to determine the feasibility of carrying the Ames/Pioneer Probe on a Mariner spacecraft of the Mariner Jupiter Saturn '77 design to Uranus. Further study of the combined flyby/probe mission by both JPL and Ames resulted in the establishment of the MJU-Science Advisory Committee (SAC) by NASA in December 1973.

This new effort was directed at developing the science objectives and rationale and mission design options in sufficient detail in order to estimate the Project costs and prepare the pre-project plans. Today I plan to briefly cover the work done in the past several months in developing the Mariner Jupiter Uranus 1979 mission with a probe.

The rare alignment of the outer planets in the last half of the 1970's affords a variety of multi-planet launch opportunities. In particular there are three Jupiter/Uranus launch opportunities allowing deep space penetration and unique approach and encounter geometry with Uranus. Of the three opportunities, the 1979 Jupiter/Uranus (JU79) opportunity is the most attractive from the standpoint of both launch energy and flight time. Additionally the JU79 Jupiter flyby is the most reasonable, since the JU78 flybys passes less than 2 Jupiter radii from the planetary surface and the JU80 flybys provide only distant Jupiter encounters with closest approaches of from 30 to 40 Jupiter radii.

The MJU 1979 mission is a very exciting mission. (Figure 3-39). It has a number of very unique characteristics that make it particularly different from any previous planetary mission we have undertaken.

The JU79 launch opportunity provides an approach unique to Uranus in this century. The rotational pole of Uranus and the satellites are tilted 98° with respect t the ecliptic plane and the spacecraft approach vector to Uranus from the Earth is almost collinear with the approach from the Sun. When viewed from the approaching MJU79 spacecraft, the satellite orbit tracks appear to describe an archery target, or giant bull's eye, with the satellites traveling in concentric circles about Uranus. This kind of spacecraft approach permits a very long observational period of almost all of the northern hemisphere of Uranus. Since Uranus also has an orbital period of 84 years, the alignment of spacecraft approach with the planet pole and Sun and Earth vectors will not occur for another 42 years.

Approaching Uranus, with the Earth and the Sun behind the spacecraft, we will target fairly close to Uranus, between Uranus itself and Miranda. Actual geometries will be discussed in more detail later.

Note that Uranus' satellite system is quite regular, beginning with Miranda at about 5 R_U out to Oberon at twenty R_U or so.

URANUS SYSTEM AS SEEN ON APPROACH

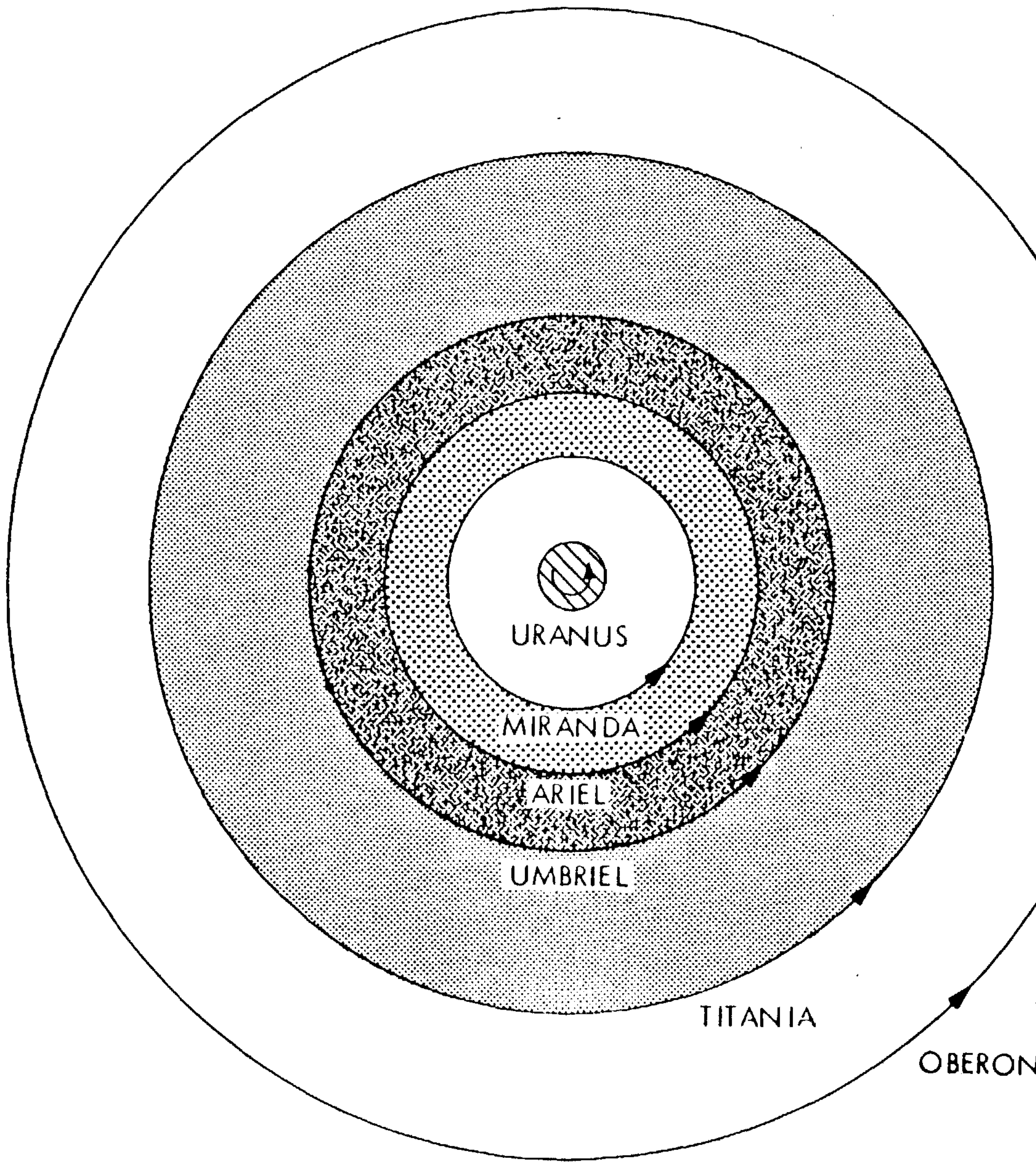

FIGURE 3-39

The first mission consideration I want to discuss relates to Launch Vehicle performance. We are assuming, as baseline, the Titan Centaur with the TE 364-4 adaptation that MJS'77 is using. This adaptation is called the MJS Propulsion Module.

A typical MJU79 trajectory would be launched in late October/early November of 1979 arriving at Jupiter about 1.7 years after launch. After the gravitational field of Jupiter has bent and added energy to the heliocentric trajectory, the spacecraft will proceed to Uranus traveling to a distance of about 20 AU in a little over 5 years, arriving at Uranus late in the Fall of 1986.

Applying the launch vehicle capability to the MJU79 launch energy requirements and requiring a minimum of 21 launch days results in the payload performance curve shown in Figure 3-40.

Flight time to Uranus is a function of flyby altitude at Jupiter and spacecraft mass, which is plotted on this chart. The predominate factor is Jupiter flyby altitude. At this point in the study, we are considering two baseline spacecraft cases, an MJU flyby without probe at 725 kilograms, and MJU_p with a probe, in the 825 range. A more detailed weight statement will be given shortly.

The slope of the performance curve is about one year of added flight time per added 100 kilograms of spacecraft mass. We can operate almost anywhere in the 6-7 year regime with certain exceptions. The Uranus encounter in 1986,

PAYLOAD PERFORMANCE FOR 21 day LAUNCH PERIOD

- LAUNCH VEHICLE: TITAN IIIE/CENTAUR D-IT/P.M.
- FIRST LAUNCH DATE: ~27 OCTOBER 1979

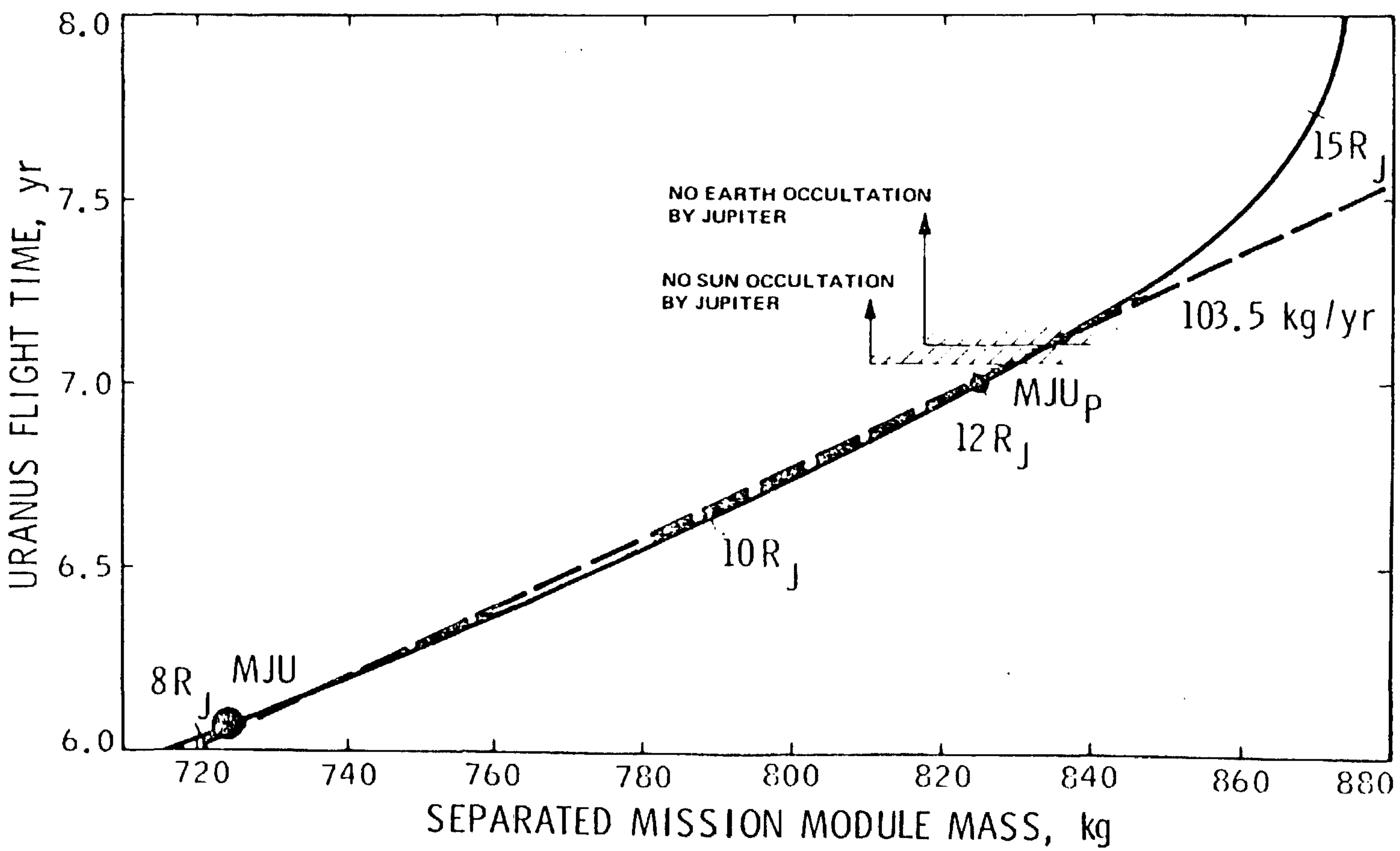

FIGURE 3-40

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is constrained to occur away from the 6.2 to 6.4 trip time due to pointing restrictions of the ground based antennas. The 64m DSN antennas would be looking right into the sun at that time of year, so there will be a constraint on arrival time to preclude encounter in this region.

One other constraint; for very high Jupiter flyby altitudes, the flight times get very long, very quickly. Note also, that for spacecraft masses above 825 kg or so, neither Earth or Sun occultations are achievable at Jupiter.

Figure 3-41 summarizes our current understanding of the Probe design requirements. We have assumed the McDonnell-Douglas conceptual design and configuration. First, it is a requirement at this time that the Probe be both Pioneer and Mariner compatible. The Probe must also be compatible with both Saturn and Uranus entries. The Uranus mission and environmental design conditions that drive its design characteristics are: the cold, dense atmosphere, the entry velocity (26 kilometers per second), the entry angle (40 ± 10°), and the descent time. This is the reference case for the Probe and for determining the Probe interface implications on Mariner.

I have summarized these implications on Figure 3-42. These are the areas we believe to be necessary to consider to integrate the Probe into the design. Probe support, which includes structural adapters, thermal control allocation, spacecraft receiver and relay link antenna, and spin mechanization are all lumped within a ten kilogram weight allowance. The four watt temperature

AMES URANUS PROBE DESIGN REQUIREMENTS

- PIONEER AND MARINER COMPATIBLE
- PROBE COMPATIBLE WITH BOTH SATURN AND URANUS ENTRY
- URANUS DESIGN CONDITIONS
  - WARM AND COOL ATMOSPHERES
  - 26 k/s ENTRY VELOCITY
  - -30° to -50° ENTRY ANGLE
  - 26 to 76 min ATMOSPHERIC DESCENT TIME TO 10 BAR
  - 800 g MAXIMUM DECELERATION
  - 72 to 108 x 10^3 km RELAY COMMUNICATION DISTANCE

FIGURE 3-41

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PROBE REQUIREMENTS ON SPACECRAFT

- 91 kg PROBE MASS, 10 kg PROBE SUPPORT
- TEMPERATURE CONTROL DURING CRUISE - 4 watts
- POWER REQUIRED FOR PERIODIC HEALTH CHECKS, CHARGE/DISCHARGE BATTERIES, PRE-SEPARATION CHECKOUT, ARM ORDINANCE
- PROBE SPIN-UP (3-8 rpm), SEVER UMBILICAL
- ADDED FUEL FOR BUS DEFLECTION MANEUVER (20 kg)
- DELIVERY ACCURACY AT $40^{0} \pm 10^{0}$ ENTRY ANGLE
- 400 mHz RELAY LINK REQUIRES 3 ft DIAMETER BODY FIXED ANTENNA (9 db) PLUS RECEIVER
- 88 sps DATA RATE, APPROXIMATELY 4.3×10^{5} bits TOTAL, TRANSMIT REAL TIME AND STORE ON-BOARD

5-21-74

FIGURE 3-42

control requirement is the same as for Pioneer. The Probe also requires periodic health status checks, charge/discharge of its batteries, pre-separation checkout and arming of the ordnance. Mariner would handle these requirements in the same way Pioneer does and fit them into the spacecraft duty cycle as appropriate.

Mariner must also provide the capability to spin-up the Probe, in the 3 to 8 RPM range, and then sever an umbilical. At the moment, Mariner does not have the capability to do this, but we do not see this as any major problem. Its a relatively straight forward design problem.

To perform the bus deflection maneuver, since this is a "dumb" probe, we would have to add additional hydrazine to our hot gas attitude propulsion system. On the order of 20 kilograms of hydrazine is required for a maneuver of 80 meters per second. The actual magnitude of the maneuver is a direct function of Probe release from the spacecraft relative to encounter. We are considering a nominal separation and maneuver of order 15-25 days. One added point: Mariner has no constraints on this maneuver relative to Earth or Sunline pointing.

The delivery accuracy requirement, I think Lou Friedman convinced you, is an easy requirement for Mariner to achieve with either improved ephemeris or optical guidance. In fact, Mariner can deliver to any desired target within the entry corridor, at ±1° accuracy, and an initial zero angle of attack.

We have mechanized, at this point, the 400 megahertz relay link as for Pioneer. This, however, is not a firm requirement. We could accommodate significantly higher frequencies if that is desired, which might have some implications in easing the job on the Probe. Further, we can accommodate receiving antennas with much higher gain, thus improving overall relay link performance.

The Probe data rate, 88 s/s, and 4.5×10^5 total bits, for either real time transmission or on-board storage are really inconsequential requirements compared to the Mariner capability. The downlink data rates and on-board mass storage requirements are driven so heavily by the imaging system requirements that the Probe numbers look like engineering data.

Figure 3-43 presents the MJU 79 spacecraft configuration. The MJU 79 spacecraft is based entirely on the Mariner Jupiter Saturn 1977 spacecraft design with minor modifications necessitated by Uranus science data requirements, by the longer mission lifetime, and by its Probe-carrying capability. The spacecraft is three-axis stabilized, obtaining attitude information from celestial and inertial sensors and maintaining/attaining the required attitude by the hydrazine-fueled hot-gas jets. Additionally, reaction wheels provide attitude stability for precise instrument pointing. The hot-gas jets, part of the attitude/propulsion subsystem, also provide velocity increments for maneuvers such as spacecraft deflection after Probe separation. The programmable guidance electronics deliver the Probe and also control articulation of the scan platform, in two degrees of freedom, to an accuracy of 2.2 mrad in each axis. All the remote sensing science is on the scan platform. This jpl

SPACECRAFT AND PROBE

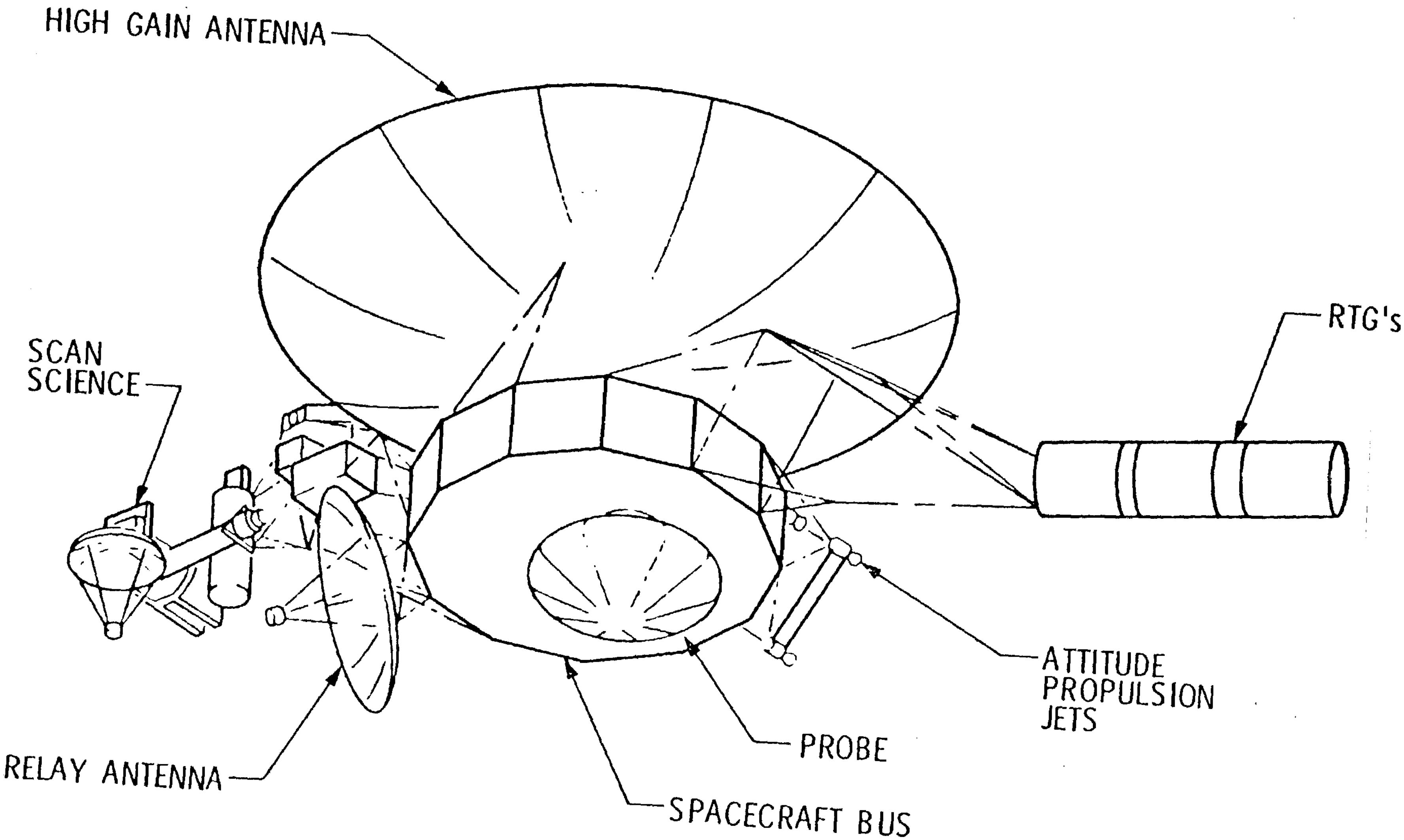

FIGURE 3-43

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includes a pair of new cameras that we are considering for this mission. The principle change from the MJS cameras is the use of CCD sensors. Because of its superior IR response over selenium vidicons, an important science consideration at Uranus, the MJU SAC has recommended its incorporation.

The main electronics housing contains the major spacecraft electronics such as: the power distribution system, the attitude control electronics, radio, computer/command, etc. Power is obtained from three radioisotope thermoelectric generators (RTG) and is also stored in a battery. On-board command and data handling electronics supply an extensive capability for both on-board stored and ground-transmitted commands as well as programmable selection and formatting of engineering, science and probe-relay data. Data can also be stored in a 9×10^6 bit solid-state (MNOS) buffer for later transmission to Earth. Two-way communications are provided by an S- and X-band radio transmitter/receiver system. Downlink transmissions of data streams containing science data are normally sent on X-band. Additionally, a 400 MHz probe-to-spacecraft relay link handles Probe data during entry. Non-imaging science data can be Golay coded resulting in a bit error rate of less than 1×10^{-5}. MJU79 receives and transmits over the 12 foot diameter high gain antenna. The antenna feeds are located on the Sun side of the spacecraft and both the S and X feeds are boresighted together. The lo-gain antenna is also on the Sun side. This would be the side away from you as viewed from the audience. The Probe is carried on the anti-Sun side of the spacecraft which is also closest to the launch vehicle. We would have to make slight changes in the MJS'77 adapter to accommodate the Probe but we do not see this as a significant impact.

For the relay antenna, we are currently carrying a body-fixed 3' diameter dish.

Figure 3-44 is the same as the one Dan Herman showed this morning. Note again the pole orientation of Uranus, (and the bull's eye effect). Both the Sun and the Earth are approximately co-linear with the pole. The spacecraft is targeted between Uranus and Miranda at approximately 3.5 R_U. This targeting affords the best over-all compromise for maximum time overhead for the Probe, high resolution remote sensing of Uranus, and a reasonably close flyby of Miranda to achieve fairly high resolution satellite imagery. At the end I will show a typical near encounter sequence to indicate the options on near encounter timing that can be considered.

Figure 3-44 is shown again in a slightly different view on Figure 3-45. This is a view of Uranus which is essentially normal to the ecliptic plane and also shows Miranda's orbit, and the trajectories of the flyby Bus and the Probe. The Probe was separated at about 17 days. Entry commences at about E-2 hours and is complete at about E-40 min. Probe zenith occurs at entry plus about 40 minutes.

You will note that closest approach occurs after all of the data gathering activity from the Probe is complete. A significant amount of time is therefore available to conduct near encounter high resolution Uranus science or to concentrate on Miranda or the other satellites.

MJU$_P$ ENCOUNTER GEOMETRY

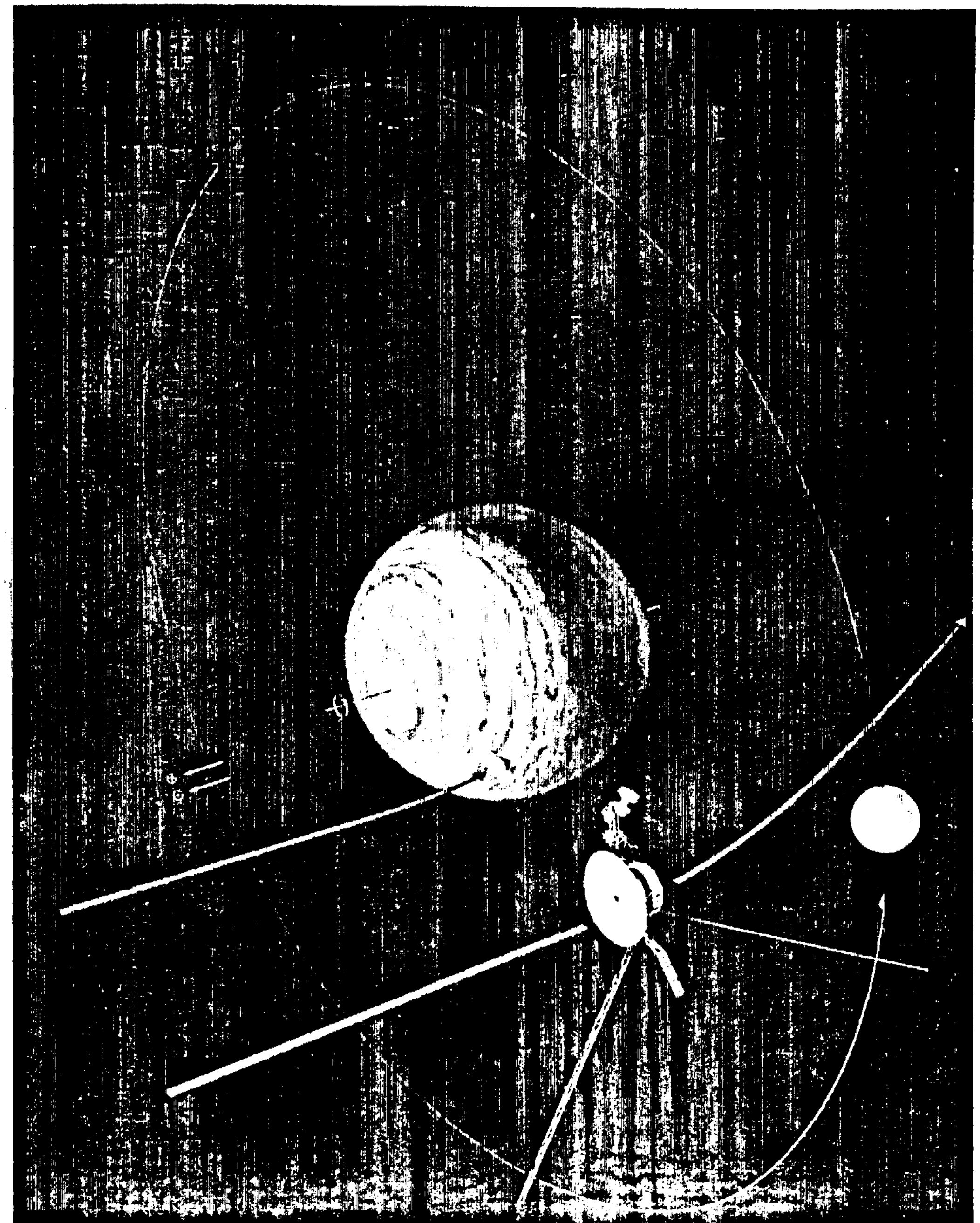

Figure 3-44

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MJU$_P$ TRAJECTORY PLANE (3.5 R$_U$)

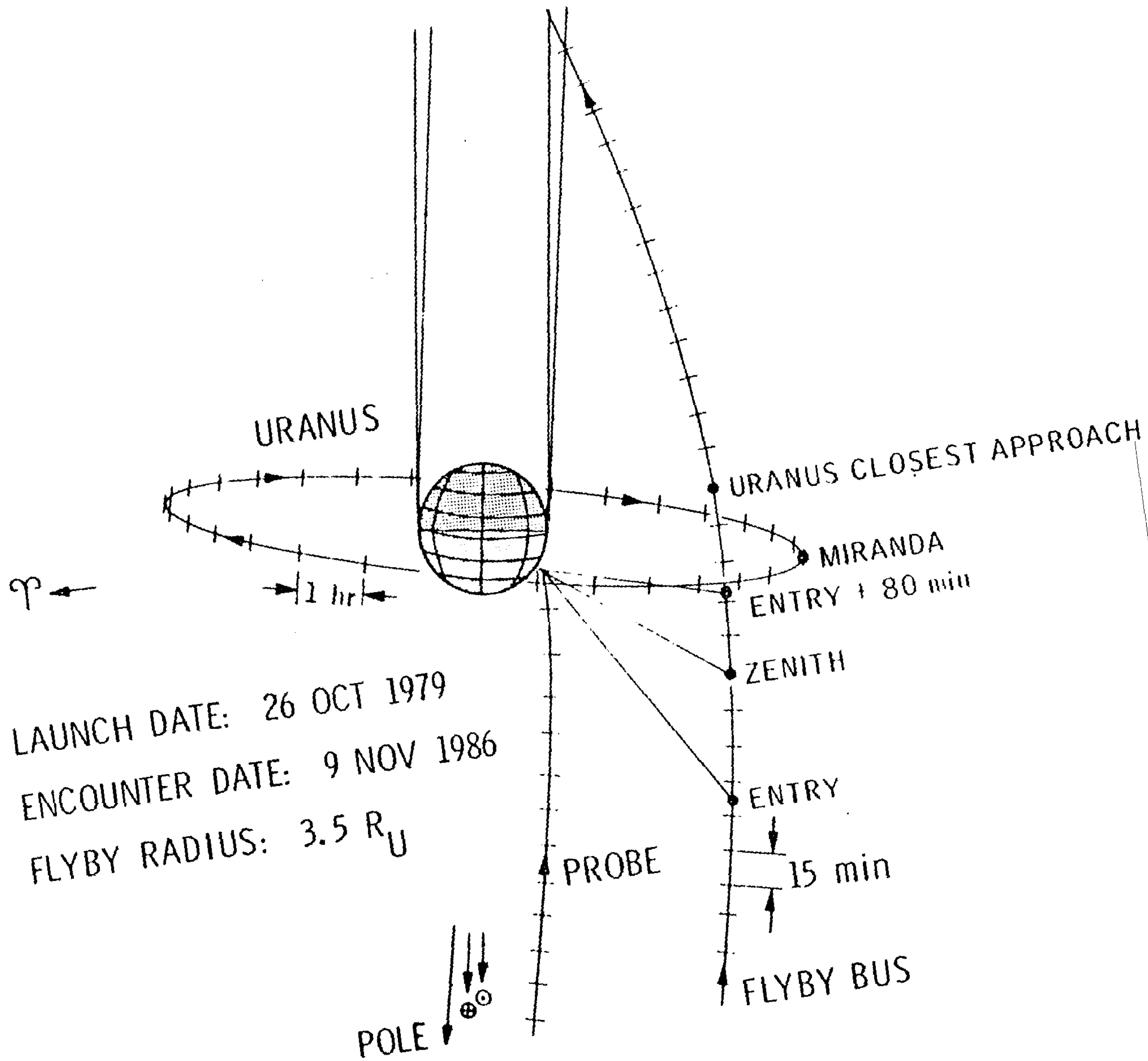

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FIGURE 3-45

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The occultation region is shown, and occurs approximately four to five hours after Uranus closest approach.

There are a number of tradeoffs available. For example, if you target very close to Uranus to achieve lots of trajectory bending so as to provide a very good post-Uranus pass for added satellite surveillance, you tend to shorten the available communication time with the Probe and hence to compromise the Probe data return. A far out pass near Miranda at say 5 R_U tends to cause occultation to occur very, very late relative to Uranus, and that is not very desirable from a science standpoint; so the best compromise at this point looks to be an aiming point on the order of 3 to 3.5 R_U.

I might also point out that, that this is also consistent with the targeting requirements to proceed on to Neptune.

As shown on Figure 3-45, I also have a summary of our latest estimates on the 3-46
gas budget. I was pleased to hear Lou Friedman's earlier discussion on the post-Jupiter correction allocation which now looks more like 15 m/s instead of 50. We are currently carrying a budget of 75 kilograms.

Figure 3-47 is an overall spacecraft mass summary. It was current as of last Friday when we received the new Reference Science Payload requirements from the MJU Science Advisory Committee. The science allocation is now 53 kg.

SPACECRAFT GAS BUDGET

Post Injection Expendables

	m/s	kg
MANEUVERS (4 PER LEG)	55	20.0
POST JUPITER CORRECTION	50	18.2
BUS DEFLECTION	80	29.1
ATTITUDE CONTROL (equiv.)	18	6.6
HOLDUP	3	1.1
TOTAL	206	75.0

FIGURE 3-46

5-21-74

MJU_P MASS SUMMARY

	kg
MJU DRY SPACECRAFT	565.5
PROBE*	91.0
PROBE SUPPORT	10.0
ATTITUDE CONTROL AND TRAJECTORY CORRECTION FUEL (206 m/s)	75.0
REFERENCE FLYBY SCIENCE PAYLOAD	63.0
	804.5

* NO ALLOCATION FOR PLANETARY QUARANTINE

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FIGURE 3-47

The spacecraft mass is also coming down because of probable changes in mechanization of the data system. Other allocations are: Probe at 91 kg, Probe support at ten, and 75 kg for the Delta V budget.

These mass numbers do not incorporate any margin or allocation for planetary quarantine effects on the Probe design; which, as I understand it from Ames, is on the order of an additional 10 kilograms.

Figure 3- 48 is a summary of what Lou Friedman presented earlier. This relates to what we can do with optical navigation. Radio only does not meet the delivery requirements without improved ephemeris.

With improved ephemeris we can achieve 6, 000 kilometers accuracy. With the MJU 1500 mm camera, photographs of Uranus with Earth-based resolution can be taken 1-1/3 years before actual encounter. From an optical navigation standpoint the MJS vidicon and 1500 mm telescope, without stars but with Ariel provides a delivery accuracy of 5, 000 km. The new candidate CCD with the same telescope, with stars, provides 600 kilometers. The vidicon would provide about 300 km. The baseline, however, is the CCD, therefore, we think we will be able to deliver the Probe on this mission to about 600 km accuracy. This delivery corresponds to a one degree entry dispersion.

Figure 3- 49 is presented to give you some understanding of some of the competing characteristics for the Near Encounter sequence.

NAVIGATION CAPABILITY FOR PROBE DELIVERY

99% Requirements

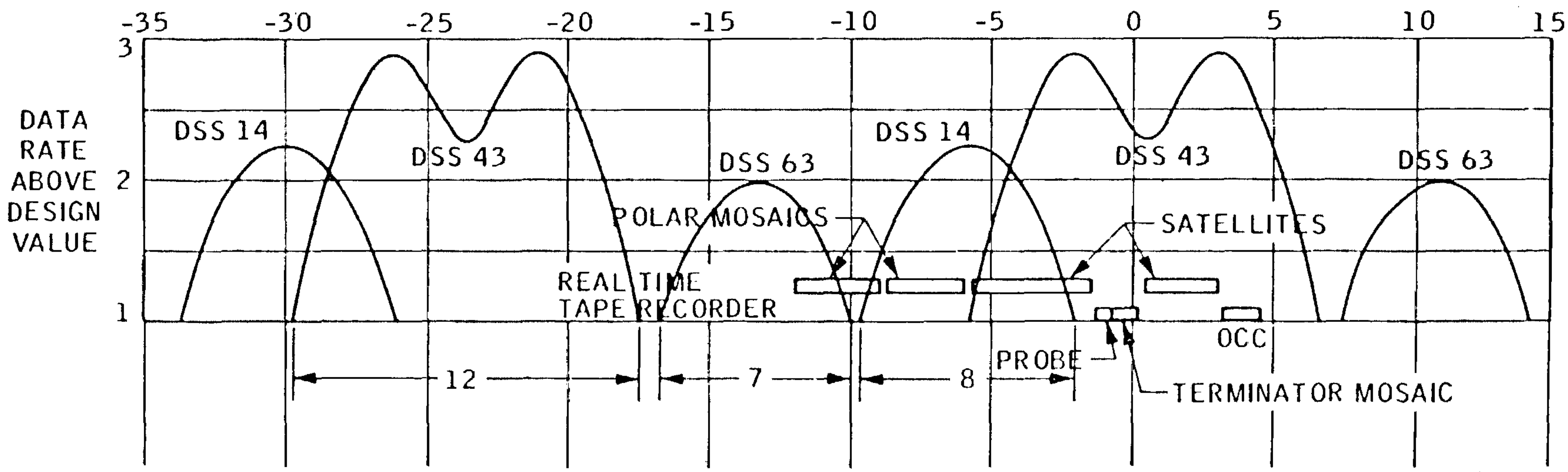
VIDICON/1500 mm OPTICS	ARIEL/W STARS	300 km
CCD/1500 mm OPTICS	ARIEL/W STARS	600 km
VIDICON/1500 mm OPTICS	ARIEL/WO STARS	5000 km
RADIO ONLY (IMPROVED EPHEMERIS)		6000 km
RADIO ONLY		18,000 km

ALL SYSTEMS DELIVER PROBE TO 6000 km ACCURACY. OPTICAL NAVIGATION PROVIDES 300 km OR $\pm$ 1^{o} ENTRY

FIGURE 3-48

5-21-74

REFERENCE URANUS ENCOUNTER SEQUENCE



X-BAND DESIGN VALUE 12.5 kbps

FIGURE 3-49

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Because of Uranus' declination, DSS-43 will be the prime station for the encounter. Uranus is down about 22 degrees in 1986, so we obtain the best coverage from DSS-43, with roughly 12 hour passes.

There is some overlap with DSS-14 but none with DSS-62.

We have hypothesized a typical science sequence. Full planet imaging mosaics are taken from about 12 hours down to nine hours and then repeated. Then the spacecraft performs a satellite imaging sequence from about E-6 hours down to about E-2 hours. Next we devote a dedicated period of time to receipt of Probe data, storing it on-board, and also transmitting it in real time. After completion of the Probe data sequence, the spacecraft begins a high resolution planet mosiac where we image just one-half of the planet's disk, but we get the high resolution data at the terminator. This is where we obtain scale heighth resolution. Next, we return to another satellite imaging sequence post-closest approach and finally the spacecraft enters occultation.

Incidentally, one of these sequences is set up to do in real time and the other on the tape recorder.

Now there is some flexibility in where you pick the closest approach and the Probe data taking sequence. We can select it as shown or with overlapping station coverage. You might want to time the encounter in such a way as to have the longest period of time for the DSS-43 pass to obtain the maximum amount of imaging data return. As you can see, if we time the closest approach for maximum imaging return we can obtain factors of two and a half or so above the 12.5 kb/s communication rate.

I am including two other charts for the Proceedings which I will not address. (Figures 3-50 and 3-51).

MJU$_P$79 COMMUNICATION PARAMETERS

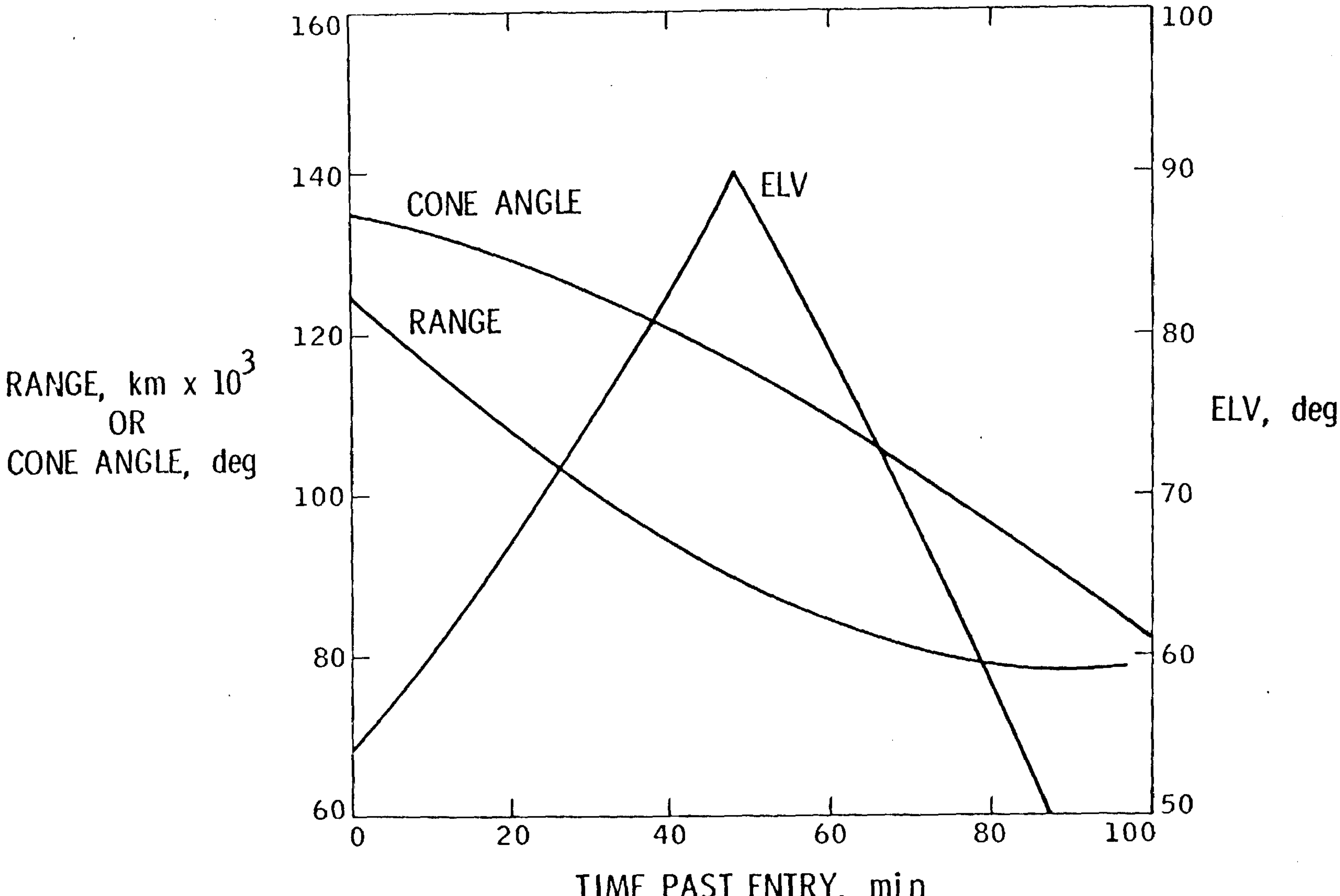

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FIGURE 3-50

URANUS ENCOUNTER SEQUENCE

E - 12.0 hr E - 9.0 hr	POLAR MOSAIC
E - 9.0 hr E - 6.0 hr	2nd POLAR MOSAIC
E - 6.0 hr E - 2.5 hr	SATELLITE IMAGING SEQUENCES
E - 2.5 hr E - 45 min	PROBE ENTRY DATA
E - 45 min E - 0 min	TERMINATOR MOSAIC

5-21-74

FIGURE 3-51

SELECTION OF A COMMON COMMUNICATION LINK GEOMETRY FOR SATURN, URANUS AND TITAN

DR. Thomas Hendricks
Martin Marietta Corporation

N75 20371

DR. HENDRICKS: First of all, I would like to change my title from that shown in the program because I had to reduce it in scope considerably. I am going to primarily be talking about the selection of a common communication link geometry at both Saturn, Uranus, and Titan. A few comments relating to Jupiter will also be made.

To set the stage, I will use Figure 3-52 and talk about what missions are available to the outer planets in the 1970's and 1980's.

Direct missions to both Jupiter and Saturn occur approximately every year with the corresponding launch energies and flight times shown in Figure 3-52. It takes somewhere between a year and a half to two years to get to Jupiter, with launch energies (C_3) in the range of 80 to 115 Km^2/sec^2.

The launch energy required to get to Saturn is increased over that required to get to Jupiter, requiring somewhere between 120 and 140 Km^2/sec^2. So that if you are considering the Pioneer and Mariner class spacecraft, the Saturn direct missions are really viable only for the Pioneer.

The Jupiter-Saturn opportunities occur approximately every three years, and of course we have the MJS flying in 1977. The launch energies, flyby radii, and trip time are somewhat flexible for the Saturn Uranus swingby missions. You can trade reduced launch energy for increased trip time. Increased launch energy corresponds to reduced flyby radii.

One point I want to make here is that the 1979 Jupiter Uranus opportunity is probably the last chance for a derivative Mariner to fly to Uranus. The next chance to go to Uranus via a swingby opportunity would be the S/U missions which start in 1980, but they have launch energies considerably in excess of the kinds of energies you get if you swing by Jupiter first. So this really is a unique opportunity to get a Mariner spacecraft to Uranus by using the gravity field of Jupiter.

OUTER PLANET MISSION SUMMARY

DIRECT MISSIONS

	JUPITER DIRECT	SATURN DIRECT
LAUNCH OPPORTUNITIES	EVERY 13 MONTHS	EVERY 12.4 MONTHS
LAUNCH ENERGY (C_3, km^2/sec^2)	$80 < C_3 < 115$	$120 < C_3 < 140$
FLIGHT TIME (YEARS)	1.5 - 3	3 - 4

SWINGBY OPPORTUNITIES

MISSION	LAUNCH DATE	C_3 (km^2/sec^2)	RCA SWINGBY PLANET (RADII)	ARRIVAL DATE SWINGBY PLANET	TOTAL FLIGHT TIME (YEARS)
JUPITER-SATURN	8/1/76	105	1.0 - 1.2	2/1/78	3.2
	9/1/77	107	2.6 - 15.0	4/16/79	3.4
	10/1/78	110	10.4- 25.0	6/1/80	3.8
JUPITER-URANUS	10/1/78	100	1.2 - 3.9	4/10/80	6.3
	11/1/79	105	4.6 -10.0	6/8/81	6.5
	12/1/80	110	11.0-28.0	8/8/82	6.9
SATURN-URANUS	11/30/80	135	1.2 - 5.5	1/20/84	6.9
	12/15/81	136	4.6 - 6.8	4/30/85	7.3
	12/30/82	140	7.2 -11.6	5/30/86	7.9
MARS-JUPITER	1/11/82	66	(1000 km)	4/20/82	3.5

FIGURE 3-52

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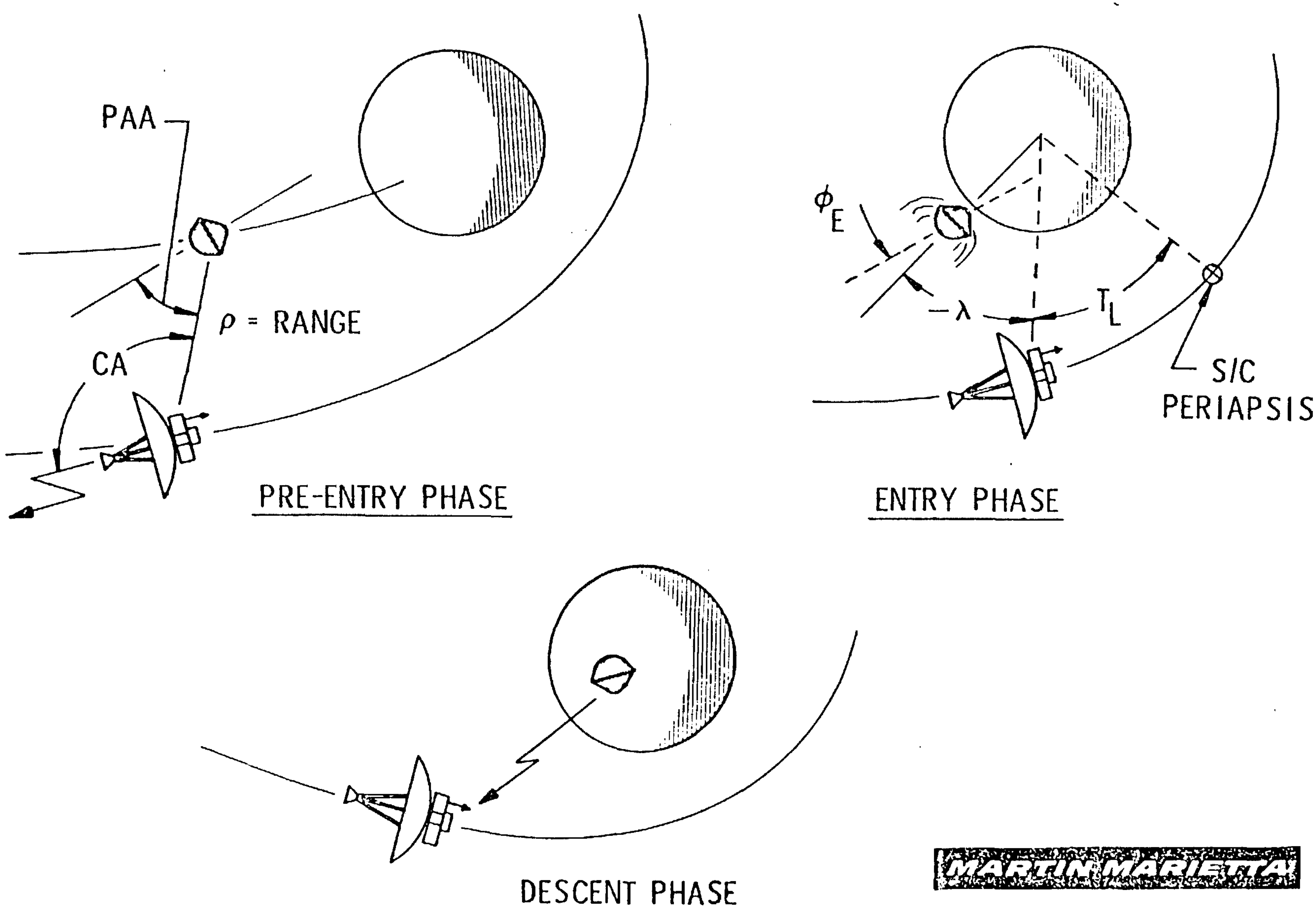
The next mission illustrated is the Mars-Jupiter swingby. You haven't read too much about it because the opportunity occurs infrequently. In 1982 there is a trajectory which takes us by Mars on the way to Jupiter. And we can actually get from Earth, by Mars to Jupiter, with a C_3 of 66 km^2/sec^2. This lower launch energy is reflected in an increased payload capability of approximately 450 kg for the Titan III E/Burner II combination. However, the price you have to pay for this increased payload capability is increased trip time; instead of a year-and-a-half trip time we are talking about a 3.5 years for the Mars-Jupiter opportunity. And this is the penalty that one has to pay; however, if you look at this as a viable option, and I think it is, there are many things you can do with this increased payload. For example, a combined probe and orbiter mission, or an Io rendezvous combined with a probe mission would be feasible mission options.

Figure 3-53 defines some of the relevant mission analysis and communication parameters used in the design of a common relay link. Cone angle defined as the angle from the Earth line to the spacecraft probe line; PAA is a probe aspect angle; and P is range.

A useful mission analysis parameter is T_L which is called lead time. This is the time from probe entry to spacecraft periapsis. Lead time was varied in our link analysis; the specific strategy is illustrated in Figure3-53 and will be described next.

The nominal probe mission was targeted so that the spacecraft was directly overhead half way through the descent phase of the mission. This gave the relative inclinations of the probe and the spacecraft trajectories. Then fixing inclination, lead time was varied for the Saturn and Uranus missions. Shown on Figure 3-54 is the cone angle at entry and end of mission (EOM), probe aspect angle and range as a function of lead time. With this information it is an easy task to select the appropriate lead times at

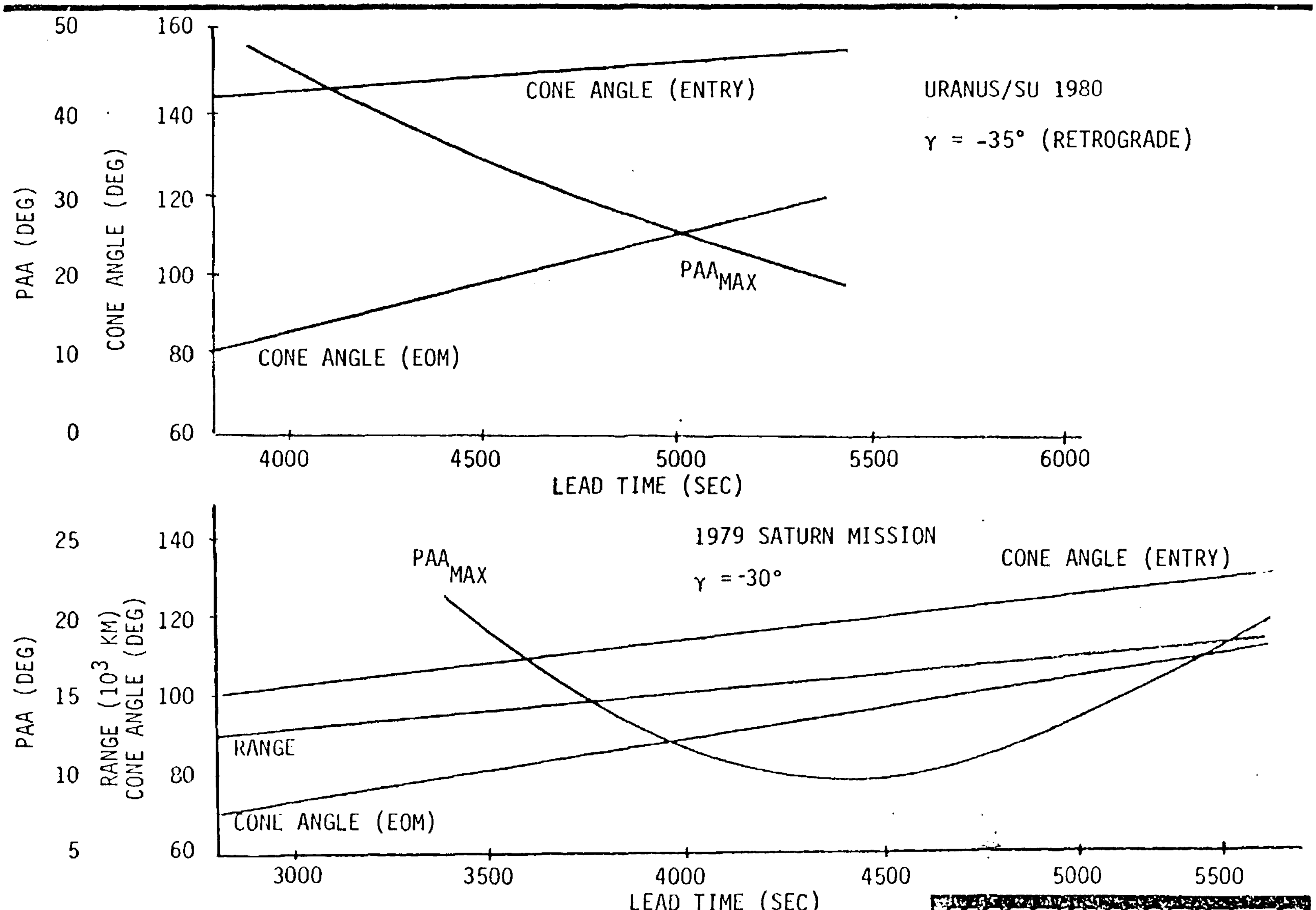
RELAY LINK PARAMETERS



MARTIN MARIETTA

FIGURE 3-53

COMPARISON OF SATURN AND URANUS LINK GEOMETRY



MARTIN MARIETTA

FIGURE 3-54

Saturn and Uranus to yield a common set of cone angles, reasonable ranges (in the order of 100,000 km) and acceptable probe aspect angles. For our baseline designs, the respective lead times at Saturn and Uranus were 5200 sec and 5300 sec. The major constraint in selecting the baseline mission was the cone angle at end of mission. To insure a practical communication link requires a cone angle greater than 90° which in turn sets the lower limit on lead time.

As Byron pointed out, we did pick the retrograde approach at Uranus in order to minimize the angle of attack. This worked out very well. We had the entry flight path angle for our nominal mission of minus 35 degrees, and on Figure 3-55 we'll show you some dispersions associated with the Uranus mission. For the Saturn direct mission, the entry flight path angle was minus 30 degrees.

Figure 3-55 shows in perspective, the probe and spacecraft trajectories and Saturn and Uranus in addition to the probe release sequence. Displayed on each planet are contours of constant flight path angle, the ground traces of the probe and the spacecraft trajectories, the terminator, and the 3σ entry footprints. Of particular significance is the 30 degree by 10 degree entry footprint at Uranus which is primarily the result of the large ephemeris error.

Navigational uncertainties when combined with the execution errors associated with the deflection event produce dispersions in the link related parameters. There are uncertainties in range, the bus and probe aspect angles. All of these have been incorporated in the link analysis.

We are primarily concerning ourselves with the Pioneer type bus with the spacecraft flying in an Earth-pointing attitude. At the deflection event, the spacecraft deploys the probe and then fires the axial and radial thrusters in the Earth, and perpendicular to Earth line, direction in order to establish the

SATURN/URANUS MISSION SUMMARY

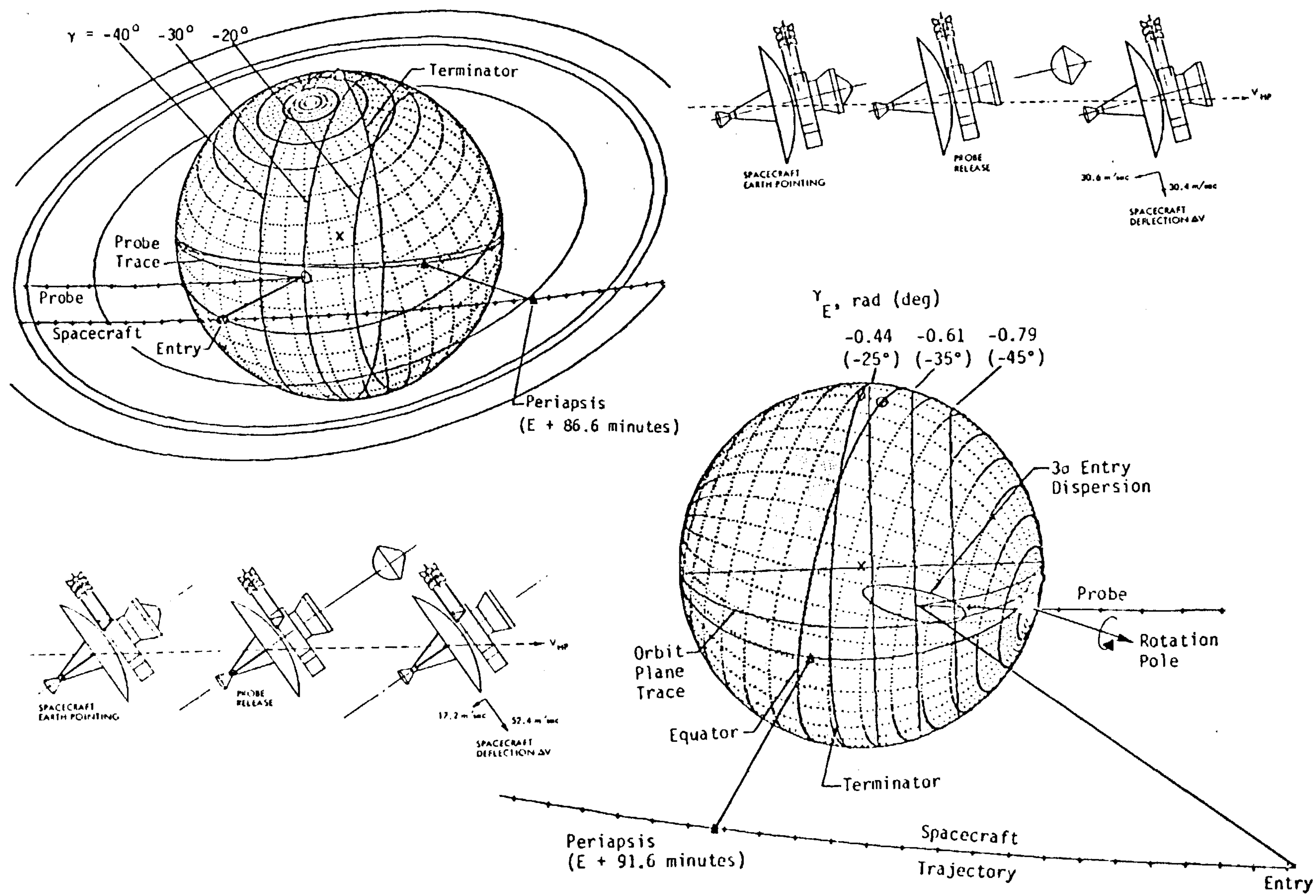


FIGURE 3-55

communication geometry. The magnitude of the spacecraft Delta-V at the deflection event is summarized in Figure 3-55.

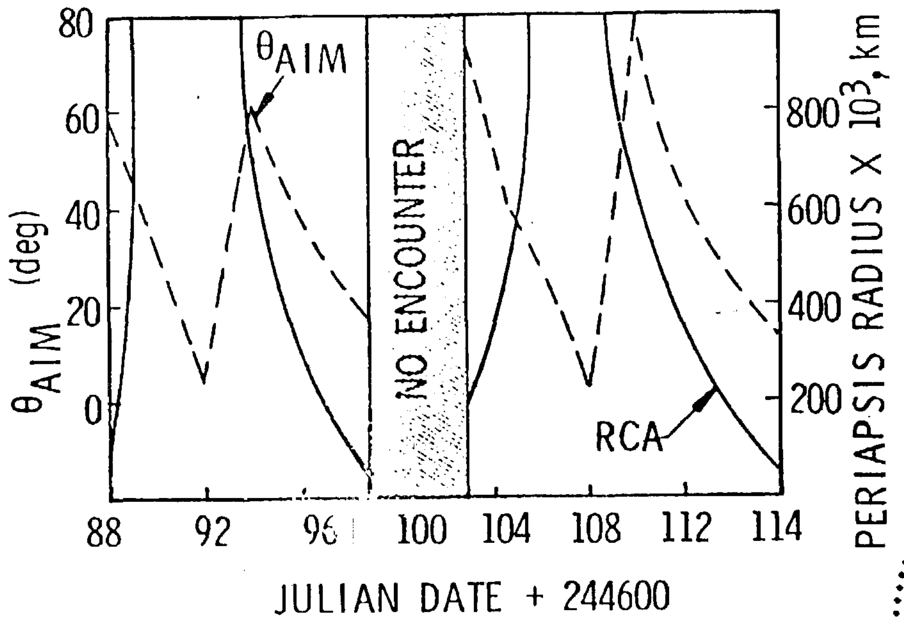
Figure 3-56 shows some interesting mission analysis link parametrics that were performed relative to Titan. This is a rather busy Figure. Let me try to explain what we have here.

The illustration to the right shows Saturn and its natural satellites along with the spacecraft trajectory. The orbits of the spacecraft and satellites are shown at one hour intervals. The position of Titan at spacecraft periapsis corresponds to where the title is printed. The Earth and sun shadows are projected onto the spacecraft orbit plane. From this plot the occultation times are easily calculated. The spacecraft trajectory shown corresponds to what we call a pre-periapsis encounter. That is, the spacecraft encounters Titan before it encounters Saturn. Typical link parameters associated with this mission are shown in the table labelled Mission Summary. The range, cone angle, probe aspect angle and other link parameters are similar to what was obtained at Saturn and Uranus.

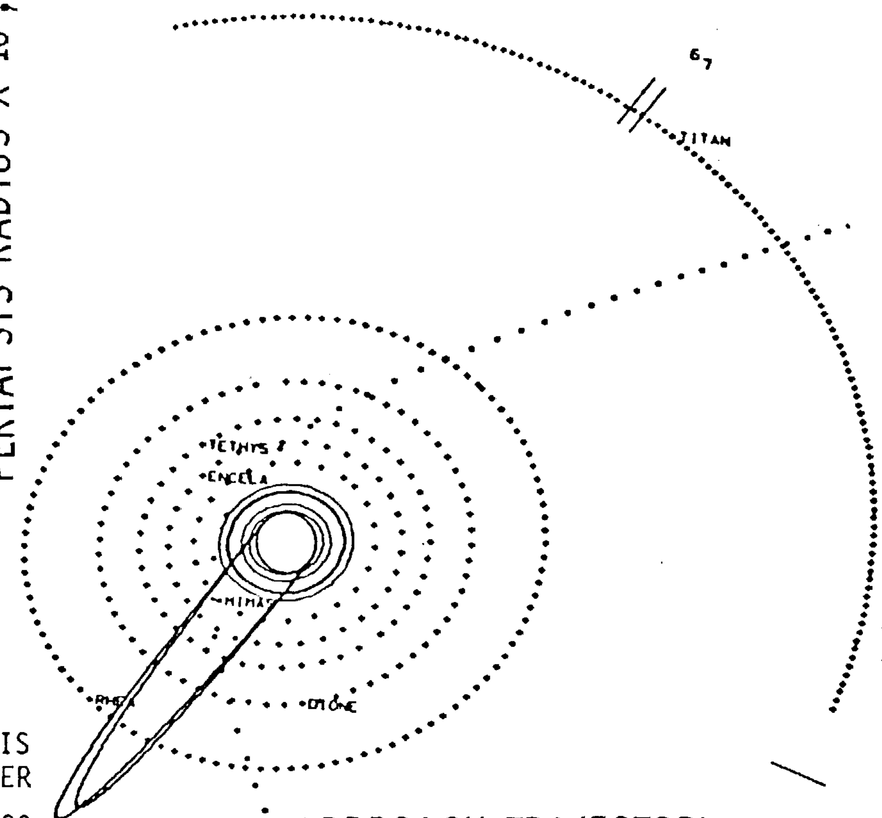
In summary, I would like to point out that it was possible to obtain a common link geometry at both Saturn, Uranus and Titan. If instead of the Pioneer baseline we had a Mariner baseline, the problem from the mission analysis point of view would have been somewhat easier.

In summary I refer to Figure 3-57. Analysis has shown that we have an ephemeris problem at Uranus. In view of this, I think it is justified that we continue Earth-based observations of Uranus in order to reduce the ephemeris error. I might also point out at this time that there is going to be an activity at Arecibo in 1975 where they are going to be taking radar observations of the Galilean satellites and also of Titan. It was estimated by Professor Pettengil of MIT that there is a good chance of reducing the Galilean satellite ephemeris errors to somewhere in the vicinity of maybe ten or fifteen kilometers, which is fairly sig-

TITAN MISSION SUMMARY



TARGETING



APPROACH TRAJECTORY

	PRE-PERIAPSIS ENCOUNTER	PERIAPSIS ENCOUNTER
RANGE (KM)	93,200	100,000
CONE ANGLE (DEG)	143.2	147.7
PROBE ASPECT ANGLE (DEG)	47.9	51.6
ANGLE OF ATTACK (DEG)	13.9	19.3
TITAN RCA (KM)	50,000	70,000
SATURN RCA (KM)	385,000	1,290,000

MISSION SUMMARY

MARTIN MARIETTA

FIGURE 3-56

SUMMARY AND CONCLUSIONS

- o CONTINUED INVESTIGATION OF MISSION OPTIONS (e.g., MARS-JUPITER).
- o FURTHER PROCESSING OF EARTH BASED MEASUREMENTS OF PLANET AND SATELLITE EPHEMERIS - URANUS EPHEMERIS REDUCTION.
- o DIRECT LINK TO ARECIBO GOOD THROUGH 1981.
- o GOOD PROSPECTS FOR REDUCED GALILEAN SATELLITE EPHEMERIS UNCERTAINTIES (ARECIBO TRACKING) 10 (10 km).
- o FURTHER INVESTIGATION OF COMBINED PROBE/ORBITER MISSION SEEMS WARRANTED, ALSO PROBE MISSIONS TO THE SATELLITES.
- o MAXIMIZE UTILIZATION OF EXISTING HARDWARE.

FIGURE 3-57

MARTIN MARIETTA

nificant, since we are talking now about errors of 200 and 300 km. He is talking about maybe order of magnitude reductions in the ephemeris errors of both Jupiter and Saturn also.

I think we should continue to look at various mission options, combining probe and orbiter missions, and looking at probe missions also to the Galilean satellites. Io is a particularly interesting object.

Another option that hasn't been looked into very extensively is the possibility of a direct link with the probe to Arecibo. And a direct Jupiter link to Arecibo is good through 1981. After this time, the geocentric declinations at Jupiter get so negative that you cannot see it with Arecibo. But it is certainly an interesting mission option. It unfortunately cuts off in 1981.

In order to reduce program costs, and this is an important consideration, future studies should be directed toward the use of existing hardware whenever possible. Viking, Pioneer Venus, the Pioneer 10 and 11 programs all offer hardware which has potential in reducing the cost of an outer planet probes program.

N75 20372

COMMUNICATIONS CONSTRAINTS ON A JUPITER PROBE MISSION

Carl Hinrichs
McDonnell Douglas Astronautics Company

MR. HINRICHS: My question was fairly simple compared to some of the questions we have heard today. That question was, "Can we take the Saturn-Uranus design that we performed for Ames previously, communications data handling system design, and fly it on a Jupiter mission?" So that is what we intend to address for a few minutes.

Our point of departure here (Figure 3-58) is Byron Swenson's trajectory to Jupiter. In relay communications terms, this is the arrival date, which means the angle from the roll axis of the spacecraft to the Earth and the excess velocity which describes the trajectories.

Very briefly, without going through them, this is what the trajectory looks like. As he has pointed out, we will deorbit something like 50 days out with about 66 meters per second Delta V. The probe will descend, as we pointed out before, the spacecraft pushed out into a flyby. We have the possibility of a correction maneuver about 26 days out which I will discuss a little bit later on, and go into the planet. So this is a general introduction to the problem we are going to try to attach.

The first thing that we start out with is, of course, the geometry. Tom Hendricks had a slightly different definition of some of the geometric characteristics. So. returning a little bit earlier to the geometry that Byron Swenson was talking about, the spacecraft aspect angles here (Figure 3-59) are the angle from the spacecraft roll axis to the probe, and this is the negative roll axis, if you will, that portion of the roll axis away from the Earth. Of course, the probe aspect angle is the same.

We investigated approximately twenty-one different trajectories, i.e., relative trajectories of the probe and the spacecraft, on our 6600 computer. We varied the spacecraft periapsis from 1.7 RJ to 2.2 RJ, but since the higher RJ data fell off of the interesting side of the chart, for clarity I didn't show it. The other parameter

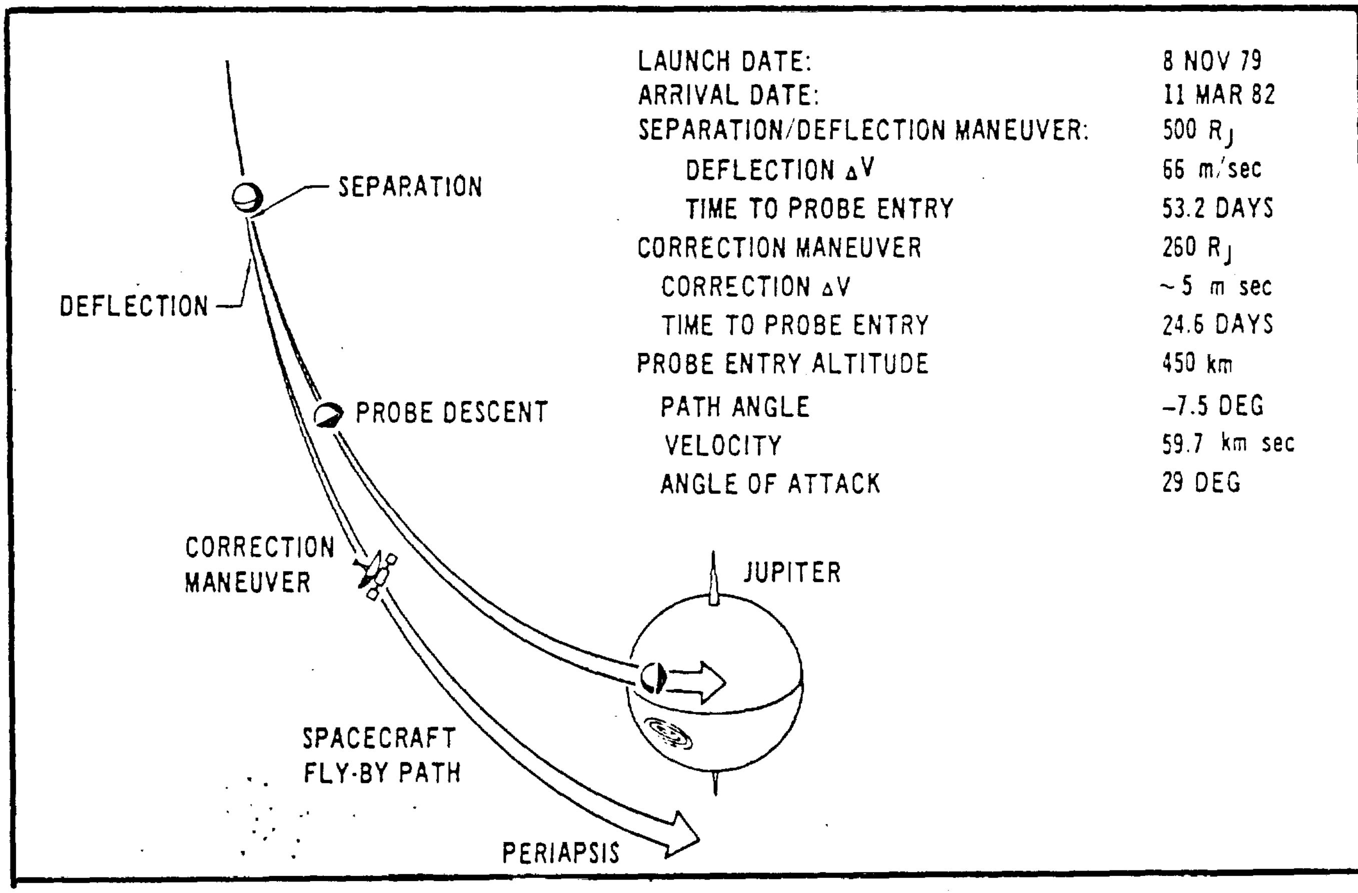


FIGURE 3-58. Jupiter Mission Parameters

in the spacecraft trajectory, besides periapsis, is spacecraft phasing. Now what we mean by spacecraft phasing here is the time from probe entry to the spacecraft at probe zenith. We ran actually .2, .26, .3, .4, and .5 hours phasing.

For the application of the Jovian entry to the Saturn-Uranus design we would like to see the probe view angles below 33 degrees, and the spacecraft angles between 40 and 90 degrees. This is because the spacecraft, as we recall from the Saturn-Uranus design, was Pioneer with a squinted pattern. Finally, we have the communications range we sometimes like to draw maximum ranges like 100,000 kilometers or so, but that fell off the top of this chart. This presents, then, the geometric parameters that we have run through.

Now this geometry is only a portion of the problem, however. Associated with this is the accuracy that we believe that we can meet.

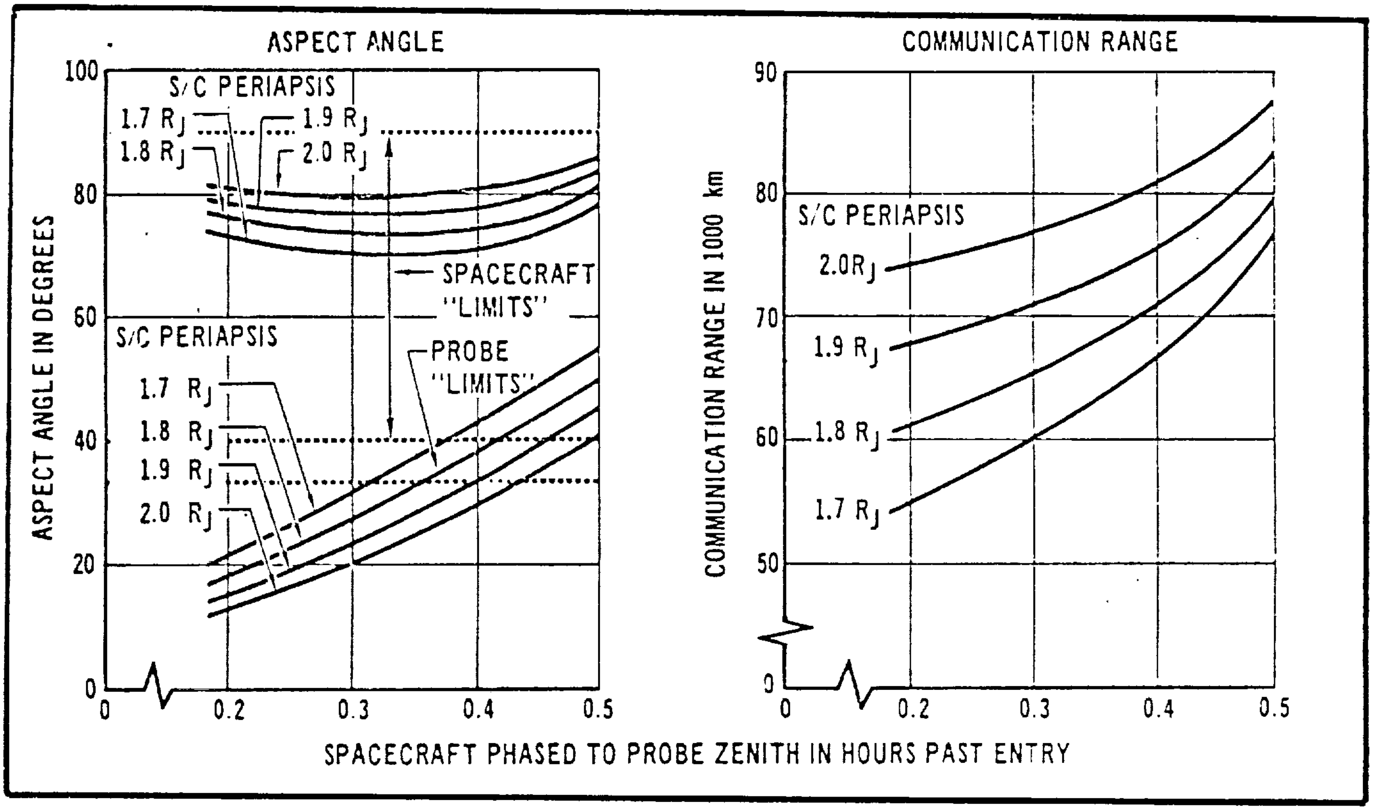


FIGURE 3-59. Parametric Study in Phasing Relationships

For one of these trajectories, (cf. Figure 3-60) the 1.8 RJ periapsis, 0.4 of an hour phasing time trajectory, (they are all very similar). I have illustrated the nominal view angles and ranges together with two sets of three sigma tolerances. The set represented by the solid line are those if we made a single maneuver, i.e., the deorbit maneuver. The set represented by the dashed line is those if we made a second maneuver approximately 26 days prior to entry to correct for the errors in the deorbit Delta V. This second maneuver would be of the order of five meters per second. Recall that the initial Delta V maneuver was of the order of 66 meters per second. We see very quickly, from this type of chart, that as far as the probe is concerned, if we did not make such a maneuver, the adverse tolerance line for a great amount of the trajectory, both in early phases and late phases, would be exceeding the beam width of the design probe antenna.

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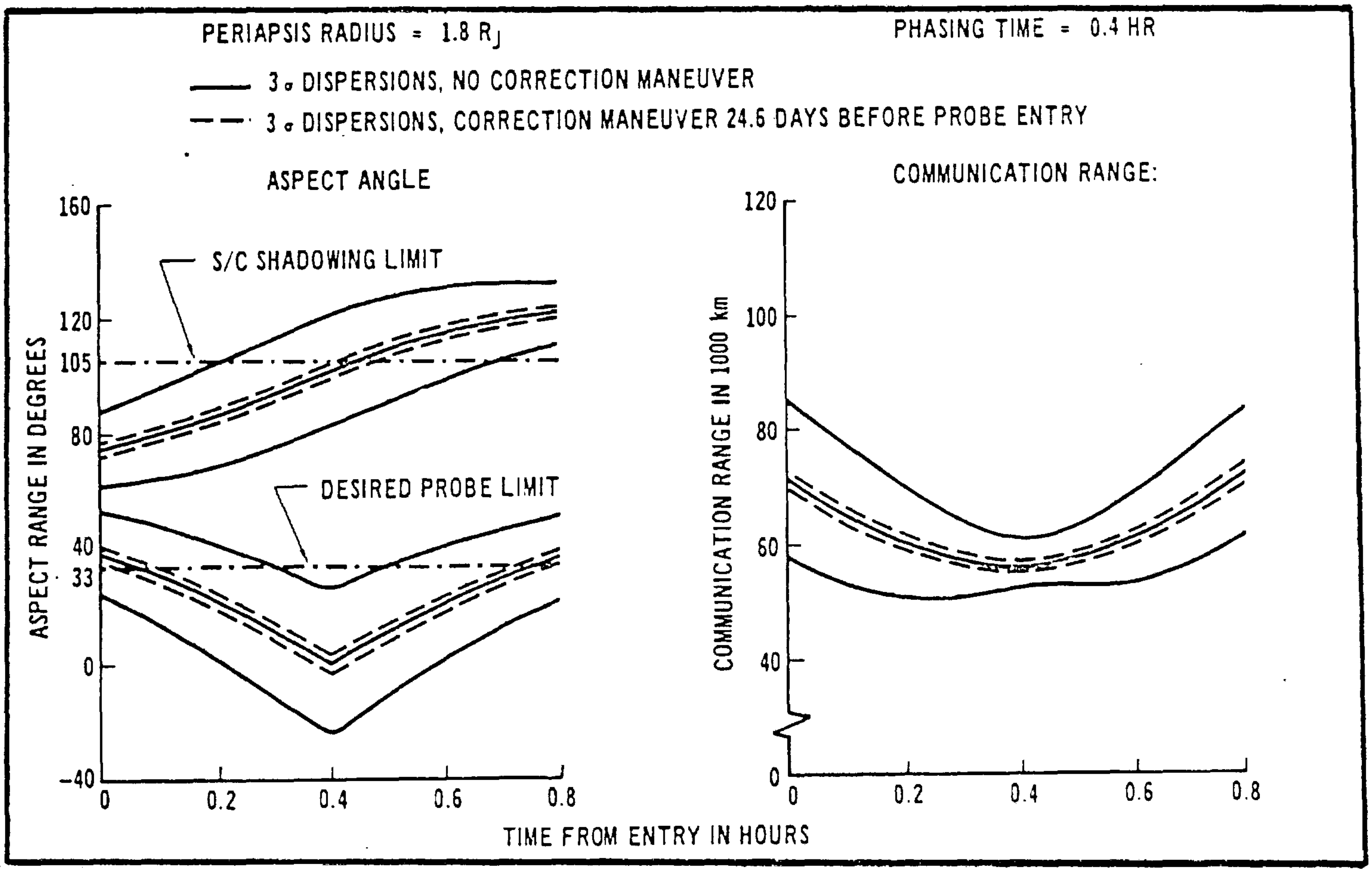

FIGURE 3-60. Trajectory Dispersions

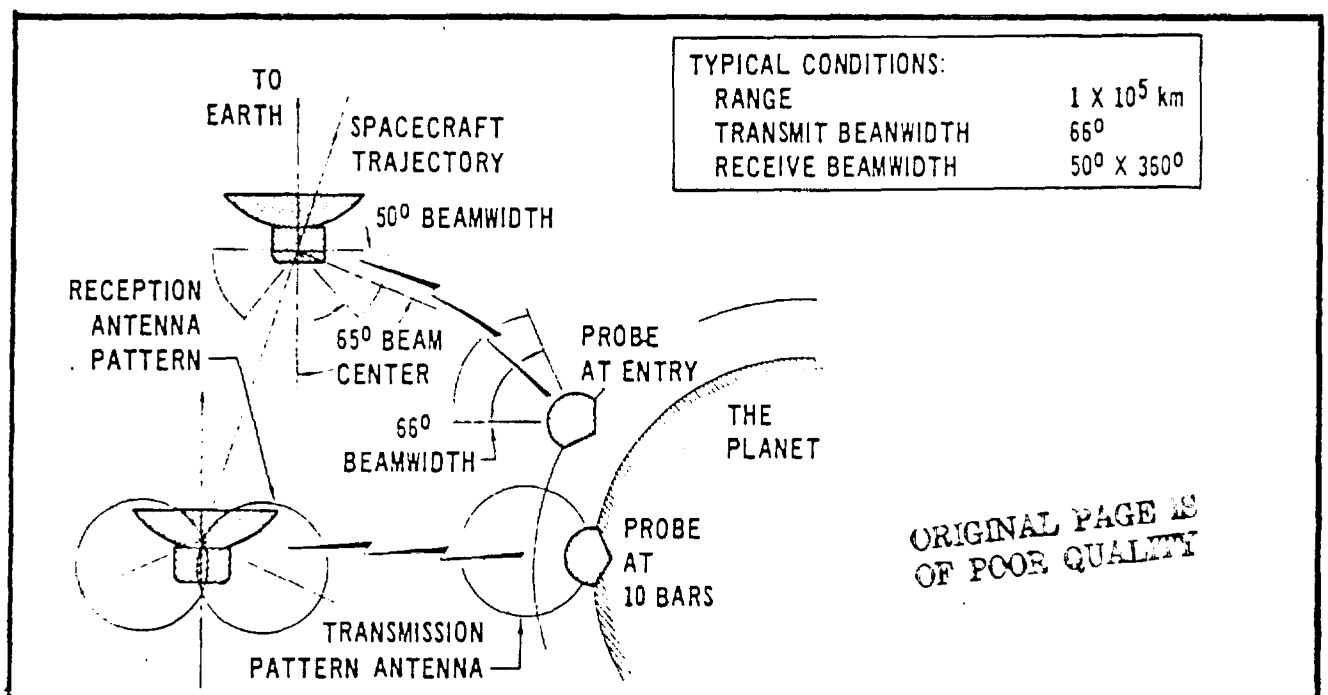

FIGURE 3-61. Communications Relay Geometry

Similarly, we see that we have a shadowing limit imposed on us by the spacecraft. You will recall from the previous chart, we wanted to try to keep the spacecraft aspect angles between 40 and 90 degrees in order to stay inside the beam width. However, if the aspect angle goes beyond approximately 105 degrees, the spacecraft antenna that is receiving the probe data, will be blocked by the large spacecraft dish, which is pointing at the Earth.

In our previous study, we have taken this as being 105 degrees. We will see in some succeeding charts that this begins to impose quite a constraint on us for the nominal mission, at least at time of entry, which this data is showing here. For the nominal mission, we could go out around approximately 0.4 of an hour phasing time and not be shadowed. However, if we wound up with an adverse tolerance with no Delta V correction, this could drop down to slightly below 0.2 of an hour.

And so, phasing will be a significant factor here. The previous small set of charts were strictly the trajectory geometry. On top of this, we have to impose the electrical geometry as shown in Figure 3-61. By this I mean the effects of antenna patterns. (I apologize for the artist here; he insists on flying a spacecraft in a straight line rather than a hyperbola.)

The typical probe pattern in the previous study, as I believe I have mentioned before, was a 66-degree beam width antenna whose maximum is on the roll axis of the probe. And on the spacecraft we have a loop vee antenna that Bill Dixon referred to earlier. This has approximately a 50° beamwidth. The center of the beamwidth is 65° off the roll axis. You will recall now, as I said before, at about 105° - the cartoon, of course, isn't to scale - we will start seeing some abrupt shadowing. I might also point out that the link that we will be talking about here is the Saturn-Uranus link which is specifically one which starts out with a 44-bit data stream. This is transmitted over a 40-watt, 400-Megahertz antenna. This is the basic link that we are talking about, and we really haven't perturbed it yet.

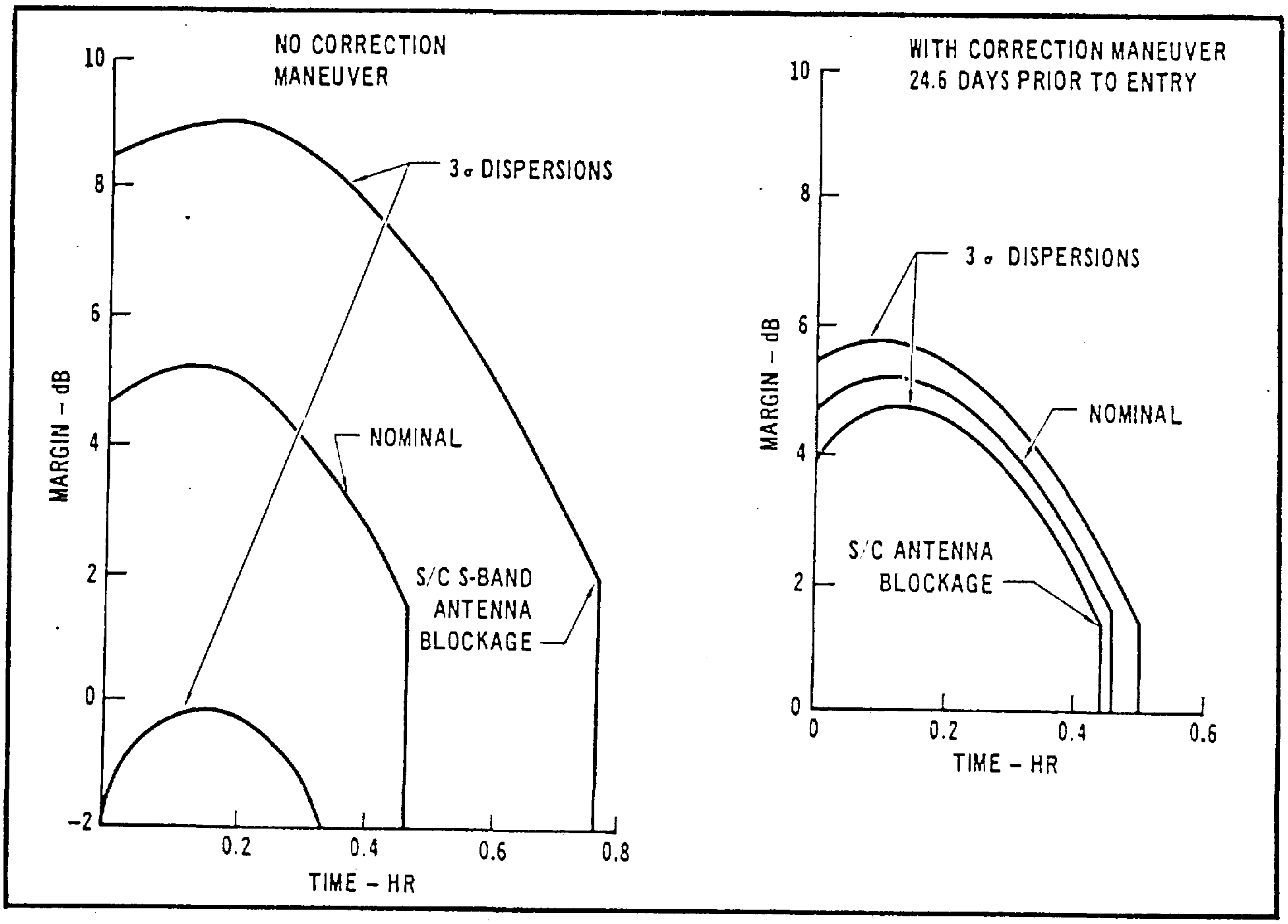

FIGURE 3-62. Effects of Trajectory Dispersions on Link Margin

With the electrical geometry coupled together with the trajectory geometry, we can establish a margin history. (Figure 3-62). The martin of the communications link is a function of the entry time and, if we have no Delta V correction, that is no second maneuver correction, those large antenna look-angle variances reflect in an extremely broad spread in the margin. By margin we mean, in this case, the true margin. At zero db margin we have a fifty percent chance of the link operating. At some value not indicated right now, typically about five db is the adverse tolernace limit. Above that point we will say that we have a one hundred percent probability of communications.

As we move to the chart on the right side for the same trajectory, we can see that if we make a second Delta V correction to take out that error, (the five meter per second maneuver) these tolerances come way down; within about three quarters of a db. So, we can

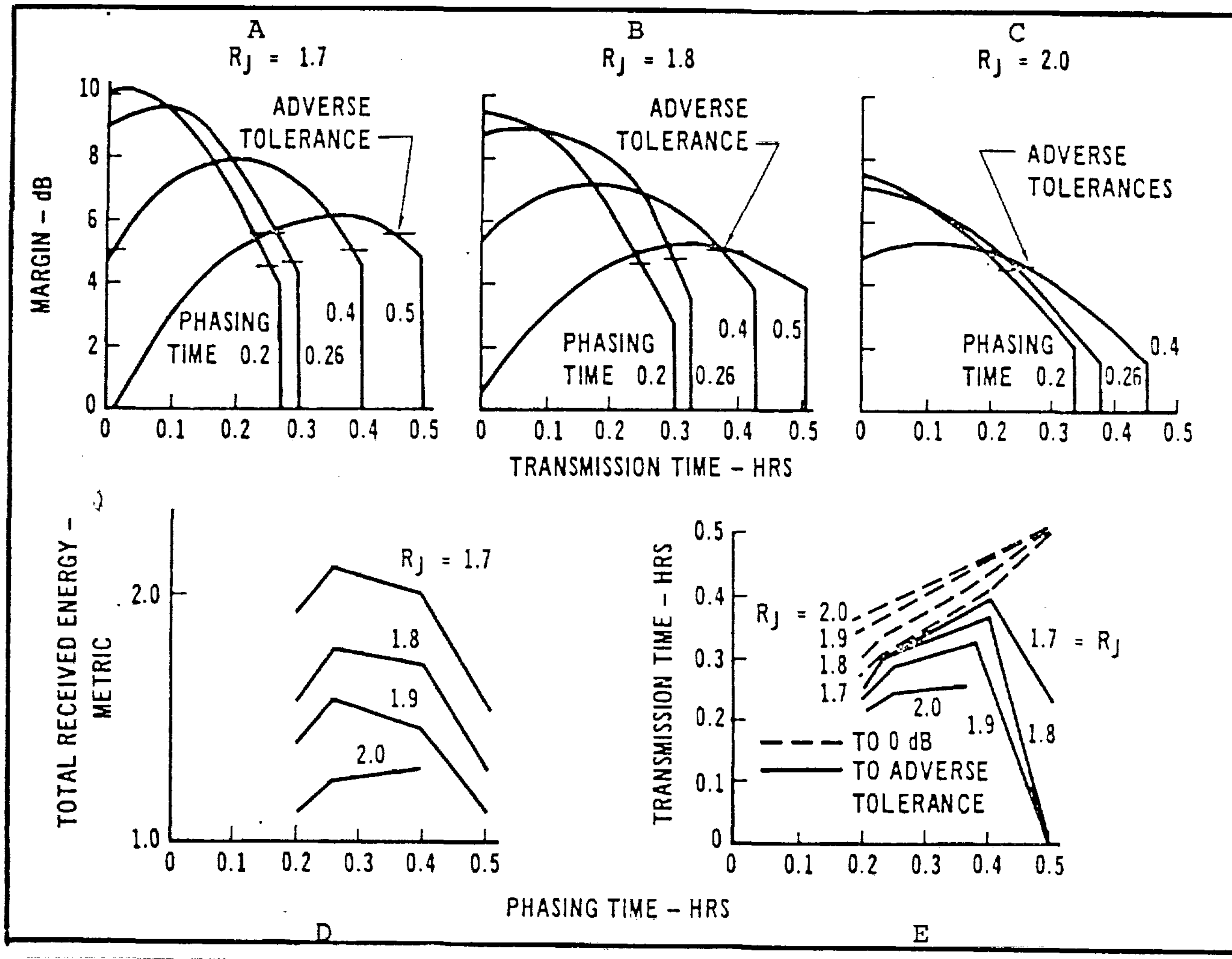

FIGURE 3-63. Link Margin and Communications Time Parametrics

have a greater assurance of the quality of the link simply be reducing those angles. This leads us very quickly to the conclusion for the Jupiter mission that a second burn to reduce the Delta V would be a very advantageous thing from the communications viewpoint.

Given that we have decided to go along with a second burn to eliminate the Delta V errors, we can generate a large, confusing family of margin histories (Figure 3-63). Again, this is the amount of signal strength we have (over and above what the link table would tell us we require) for a number of different trajectories. In this case, we run another computer program for the electrical geometry and the link table, utilizing the trajectory geometry as inputs. On each of these margin charts, I have tick marks to indicate the adverse tolerances. They are slightly different for each trajectory because of the difference in the synchrotron noise (being closer or farther from the planet; and depending on how we integrate to get the amount of noise.)

They also are somewhat different in that we have assumed in the adverse tolerances a five-degree uncertainty in the pointing angle of the probe at time of entry to account for "wobble."

Taking a typical mission, again our friendly 1.8 RJ, four tenths of an hour phasing time, we can see that the margin starts almost at the adverse tolerance point, increases as time goes on, (to about two tenths of an hour,) then begins to decrease until about .35 hours where we drop below the one hundred percent probability of communications. Then at some point the margin abruptly drops to zero where we have hit the shadowing limit of the big dish.

As I said before, these are pretty confusing charts to look at. If you do stare at them for a week or two, you begin to make some sense out of them. One of the ways of making sense out of them is to try to pick a trajectory, let's say that maximizes the total amount of energy at the spacecraft receiver. This is simply the integral of the margin history and we can take this as a metric then to find the "goodness" of a particular trajectory, in relay communications terms.

So, I have plotted this "goodness" for these different trajectories here on Figure 3-63D. The larger the better. We can see that as the spacecraft periapsis moves in the apparent "goodness" is better. In other words, we have about fifty percent more energy for the 1.7 R_J .26 phasing mission than we have for, say, about the 1.9. This "goodness" criteria, however, does not take into account the amount of time that we have to transmit. If we look at just the time that we have to transmit we get somewhat of a different picture. (Figure 3-63E). Again, each point here indicated by a break in the curve represents a complete trajectory; that is a complete run through the communications and a complete run through the exoatmospheric trajectories. So, we can see as we plot, for example, the total transmission time to the adverse tolerance limit, that as the periapsis moves in we get more and more transmission time; things get better and better. This is, fairly obvious because we are moving in closer and we are getting more margin. Things are beginning to look better.

However, if we plot the total amount of time to zero db or in most cases, blockage - I don't believe I have an example up here where zero db does not occur at blockage - we see somewhat of a different trend. In the one case as we drop to periapsis we increase transmission time. For zero db, as we decrease periapsis we decrease the time. In this case, of course, as we are coming closer in we have less and less time to view. So, in the one case the adverse tolerance line moves up to a point where it is, let us say, caught by the zero db transmission time and then it is swept down. The obvious best point, then, is where these two parametrics cross. In this particular case, for this case of geometries, this is at 1.7 R_J and results in a maximum transmission time of about four tenths of an hour if we have a phasing of, also, about four tenths of an hour.

We currently have ignored our scientific friends in that we have only been talking about maximizing the margin and the communications time. We really haven't talked about science. Science, in our terms, is the data handling system. So, I'd like to just very briefly go through the data handling system and show why this communications time was so critical.

The upper diagram of Figure 3-64 is a block diagram of the data handling system of the Saturn-Uranus design. The first thing that happens in the design is that early in the game we would like to catch the earliest possible deceleration (which, by definition, is .0004 G's and is the least resolvable deceleration time,) so that we can monitor the deceleration all the way from that least possible deceleration through the absolute maximum down to the point where we deploy instruments.

So what we will do is early in the game (prior to that .0004G point) we will turn on the data handling system, we will start

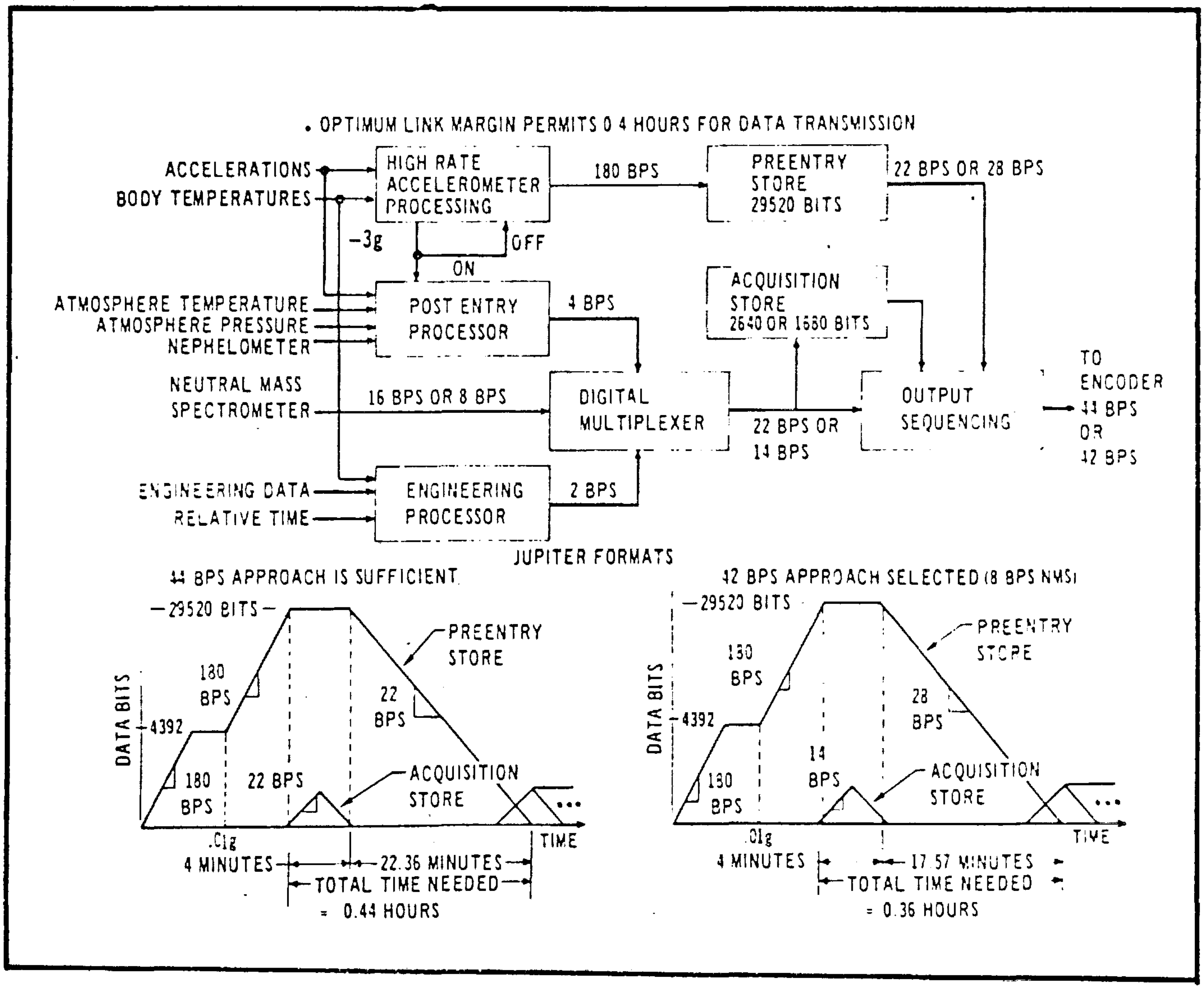

FIGURE 3-64. Data Handling Approach

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monitoring these decelerations and we will store them in a line. We will start filling up that line at 180 bits per second; and when that line is full, the first bit that went into the line falls off and we pump a new bit in. We hold the system at that condition until we see some very definable, highly reliable G level; in our case, arbitrarily, .01 G's. When we hit this level, we have trapped .0004G (that least resolvable G), and a very reliable G. At this point then, the high rate processor, having found the crossover point, ceases filling the first line and fills up another large line to the point where we are now ready to deploy instruments.

This is, typically, like three or six G's (it seems to vary from day to day and from planet to planet). I just ask the trajectory people what the number is currently and use it. At this point the high rate processor turns off. It has sensed the G levels and has decided that we have been through peak deceleration. Then we start our normal processing. This is the normal post-entry data from the nephelometer that we have heard to much about, the temperatures and pressures, the neutral mass spectrometer and other dull stuff that we think is required to help support the mission and define the quality of the data. This is all multiplexed together and sent out as real-time data.

While we are starting to send this data out, we will fill up a small store, the acquisition store on the figure. We fill up this small store and then immediately dump it. We call this an Acquisition Store because it serves as a time buffer for the spacecraft receiver and bit synchronizer to sweep to the appropriate center frequency, taking Doppler and Doppler rate and so forth into account; lock and acquire. Once this has happened, we can begin dumping the big store, (Pre-entry Store). We can dump this out interleaved with the real data out to the transmitter. Once this is dumped, then we can start utilizing the Acquisition Store, which now simply becomes a Redundancy Store.

This is exactly the same technique we used in the Saturn-Uranus design with the exception that we had a much longer time in which to perform this function and we could actually dump these stores redundantly. In the case of Jupiter we don't quite have this time and we can't do it redundantly; but if we dump them once, we can minimize that time. So, if we minimize this time, from the time that we start transmitting live data until we have got all of the deceleration data out, we can do it in .44 hours. (Lower left curve on the figure.) That is too bad because we only had four tenths of an hour to work with so we have lost .04 hours.

Another option, would be to leave the initial portion of the sequence the same up until the point that we begin dumping, but rather than dumping in a one-to-one sequence, 22 bits to 22 bits, if we could dump in a two-to-one sequence, that is 28 bits to 14 bits, we could dump the store quicker. We can actually dump, then in about seventeen and a half minutes compared to about twenty-two and a third minutes. This means, then, that we can acquire all of the data including all of the pre-entry data, and have a .36 hour mission. The trajectory phasing gives us a .4 hour mission and we can do the mission.

What did we pay for this? Obviously, if I have reduced the real time data rate from 22 bits to 14 bits per second, I had to pay something. We have arbitrarily, for purposes of this presentation, decided to pay it in the neutral mass spectrometer rate. In the Saturn-Uranus design, as Howard Myers told you this morning, we had a 16-bit per second data rate. That was nine sweeps out of the NMS: one sweep which was transmitted as raw data; the other eight sweeps were averaged and then sent out as a single stream. So we could delete one or the other of those two streams, for example, retaining the same sampling times, and cut the rate in half.

In conclusion, the question was a relatively simple one: can we use the Saturn-Uranus telemetry design for Jupiter entry? The answer is: not exactly. We have to make some qualifications in the data handling. The qualification is a single dump rather than a dual dump, and a reduction in the neutral mass spectrometer rate, and providing that we can make a second burn, a delta V correction.

SESSION IV - PROBE DESIGN AND SYSTEM INTEGRATION

T. N. Canning, Chairman
NASA Ames Research Center

MR. CANNING: Gentlemen, I am not going to make any introductory remarks and just simply start with the first speaker, Dick Ellis, of DYNATREND, who will summarize the content of the draft report which was provided to you: The Ten Bar Probe Technical Summary.

TEN BAR PROBE TECHNICAL SUMMARY

T. R. Ellis
DYNATREND INCORPORATED

MR. ELLIS: I am going to start with the conclusions of the study. That way, if Tom pulls out the hook and removes me from the podium, at least the major points will have been covered.

In preparation of this report, we read and reviewed a stack of material done by most of the people in the room over the past five years or so, a stack about six feet tall, when piled up, and tried to, in 25 words or less, summarize this material, to provide a management-level technical review and summary.

The major conclusions that we reached, after digesting all of this material, are shown on Figure 4-1. This set of conclusions was reached prior to the Pioneer 10 mission and there are some modifications that must be made to them, as a result of the Pioneer 10 data.

The most significant conclusion was that a common probe design looks quite possible for the five bodies we were considering; that is, Jupiter, Saturn, Neptune, Uranus, and Titan, except possibly for Jupiter since the design for Jupiter is quite a bit heavier. The heat shield fraction is so large that it didn't really make good sense to try to combine Jupiter with the other planets in a common probe mission.

A similar kind of thing, at the other end of the spectrum, could be said for Titan; that is that Titan doesn't quite require the heat shield fraction that is required for Saturn, Uranus and Neptune, and you are paying a penalty in trying to go to Titan with a common probe. But it looked to us that in that case, it was probably worth it, rather than going to a completely new design.

CONCLUSIONS

- OUTER PLANET ENTRY PROBE MISSIONS FEASIBLE BEGINNING IN 1979 – 1980
- COMMON PROBE DESIGN POSSIBLE EXCEPT FOR JUPITER
- BASIC TECHNOLOGY EXISTS EXCEPT JUPITER HEAT SHIELD
- PROBE WEIGHT CLASS – 113 kg (250 lb)
- PIONEER CLASS BUS PRODUCES LIGHTER SPACECRAFT
- MARINER CLASS BUS PROVIDES BETTER BUS SCIENCE AND PROBE BUS COMMUNICATIONS
- STAGING DURING ENTRY UNNECESSARY

DYNATREND INCORPORATED

Figure 4-1.

The Probe weight for the common probe was in the 250 pound class. We did look at the two bus concepts, and I classify them here as Pioneer and Mariner. I am really talking about a spinning bus versus a 3-axis stabilized bus, of which the Pioneer and Mariner are the prime samples.

The Pioneer bus produced a lighter overall spacecraft, able to be launched using smaller launch vehicles. The Mariner class provided slightly better probe communications and a more stable platform for the bus science.

Another significant conclusion, contrary to much of the work that had been done prior to this review, was that staging during entry appeared unnecessary except possibly, again at Jupiter.

A common science payload (Figure 4-2) appeared consistently throughout most of the study work. It included the five instruments that have become quite familiar to everyone, pressure sensor, temperature sensor, accelerometer, neutral mass spectrometer and nephelometer. The science objectives are shown and each instrument is related to the particular science objective that it would primarily accomplish by the deltas on the chart. The cases where an instrument is a secondary instrument for a particular science objective are noted by the X's on the chart.

A couple of other instruments were examined very briefly. One of them was the solar radiometer. It appeared from most of the work that had been done, that the sun angle during probe descent was quite poor in practically every case. And, therefore, while it was a very desirable instrument, perhaps as a replacement for the nephelometer, it was not included.

Figure 4-3 reviews, basically, the sampling rate and shows how the various instruments are sampled during entry and descent.

SCIENTIFIC MEASUREMENT OBJECTIVES

SCIENTIFIC OBJECTIVE	PRESS.	TEMP.	ACC.	NMS	NEPH.
ATMOSPHERIC DENSITY	X	X	△	X	
ATMOSPHERIC TEMPERATURE	X	△	X	X	
ATMOSPHERIC PRESSURE	△	X	X	X	
ATMOSPHERIC CONSTITUENTS	X	X		△	X
CLOUD LOCATION/STRUCTURE	X	X	X	X	△
CLOUD COMPOSITION	X	X	X	△	X
ATMOSPHERIC TURBULENCE	X	X	X		X

△ DIRECT MEASUREMENT
X RELATED MEASUREMENT

Figure 4-2

DATA RATE REQUIREMENTS

DATA TYPE	SAMPLE INTERVAL (SEC)		WORD LENGTH (BITS)	SAMPLE LENGTH (WORDS)	DATA RATE (BITS/SEC)	
	ENTRY	DESCENT			ENTRY	DESCENT
PRESSURE	–	50	10	1	–	0.2
TEMPERATURE	–	50	10	1	–	0.2
ACCELERATION						
LONGITUDINAL	0.2	50	10	1	50	0.2
LATERIAL (EACH AXIS)	0.2	50	7	1	35	0.14
NEUTRAL MASS SPECTROMETER	–	405	9	634	–	14
NEPHELOMETER	–	30	10	4	–	2
			6	3		
ENGINEERING AND CALIBRATION	0.83	VARIOUS	6	1	30	2
HOUSEKEEPING	–	–	–	–	30	3.12
TOTAL ENTRY DATA RATE					180	
ENTRY DATA PLAYBACK						22
TOTAL DESCENT DATA RATE						44

Figure 4-3

The entry data being stored, (the data sampled during entry) is then played back during descent at 22 bits per second. The main body of data being taken during descent also yields 22 bits per second giving it a net 44 bit per second data rate. The sample design we have in our report is basically the McDonnell-Douglas conceptual design as it most nearly approximated the characteristics necessary for this mission.

In reviewing the communications geometry, Figure 4- 4, the communications range at entry and end of mission shown here are the maximum conditions of any of the various missions from all the reports, with the exception of a few where there were special requirements. There are a few missions flown at extremely high spacecraft flyby periapsis, that exceeded these ranges, but most of the missions were within the constraints shown here; also true of the maximum range of probe look angle excursion of 60 degrees and the maximum bus look angle excursion of 45 degrees.

These conditions set the tone for the communications system and the major trades, Figure 4- 5, which showed up in the various studies that were done. To a large degree, I think these trades have been covered by previous speakers.

The bus relay link antenna for the 3-axis stabilized bus, is a dish, in the typical design the dish had a 40-degree half angle pencil beam with about 12 db gain.

In the spinning spacecraft, you have a choice between trying to duplicate that pattern with a despun antenna, which is just about impossible to integrate into the spinning spacecraft design, or using an axisymmetric antenna, as shown in the baseline design. It has a gain of about one and a half db and a 50-degree half angle. This makes the spinning spacecraft appear to have like a 10 1/2 db deficiency in comparison to the 3-axis

COMMUNICATIONS GEOMETRY	
MAX. COMMUNICATIONS RANGE AT ENTRY	120,000 km
MAX. COMMUNICATIONS RANGE AT EOM	105,000 km
MAX. PROBE LOOK ANGLE	60 deg
MAX. BUS LOOK ANGLE EXCURSION	45 deg
MAX/MIN RANGE RATE	25/−20 km/sec
MAX/MIN RANGE ACCELERATION	8/−1 m/sec^2
DATA RATE	44 bits/sec

Figure 4-4

MAJOR COMMUNICATION TRADES

- BUS ANTENNA
- FREQUENCY
- MODULATION TECHNIQUE
- CODING SCHEME

Figure 4-5

stabilized spacecraft, but about three and a half db is recovered because of the difference in the planet noise received. If a dish antenna is looking right at the planet, the entire planet disc is within the beam width of the antenna and a much higher planet noise contribution is received, whereas the axisymmetric pattern looks all the way around the spacecraft; only a small bit of that antenna pattern intercepts the planet disc and the planet noise contribution in the receiver is much less. So that the net difference is about 7 db between the two.

Many of the studies were done at 400 megahertz, and others were done at 860; a few were done at 1,000; and here and there there were some S-band systems. But the principal case could be made for the 860 megahertz frequency and the 400 megahertz frequency. The principal difference here was related, again, to the spacecraft configuration and the spacecraft antenna size. There is a set of communication design link charts in the report that compare the spinning spacecraft with a 400 megahertz communications system with the 3-axis stabilized spacecraft at 860 megahertz, and basically demonstrate that either of these systems can do the job within the design constraints that I showed two slides ago.

Also, in the modulation technique area, both PSK PM and FSK systems were used and, again, both can do the job. There are some advantages and disadvantages to each, mostly relating to the fading conditions that are assumed for the atmosphere. And these are probably not too significant if you consider only the upper atmosphere of these planets, becoming most significant if you try to enter into Jupiter's atmosphere.

In terms of staging, there appeared to be quite a difference when we started looking at the different staging designs and one of the things that emerged very quickly was that some studies were using a staging altitude that was basically trying to reach some low G-level descending; that is, to exit from entry above the tropopause. Others were trying to reach some G-level at a particular velocity; typically, something like Mach .7 above 100 milibars pressure. And when you start looking at what these different ground rules mean on the different planets with the different model atmospheres that have previously been discussed, the design conditions for exit from entry become quite different. For example, all of these shown on Figure 4- 6 are 100 milibar altitudes in kilometers; that is, reference altitude in the model atmospheres. The pressures, if you started talking about coming out above the tropopause, are quite a bit higher.

In trying to compare the results of these studies using different ground rules, we ran into a lot of apple-and-orange problems. As shown in Figure 4- 7 , we did conclude that, with the exception of Jupiter, staging was probably not required. Staging does provide a better science mission in that you can use one ballistic coefficient to arrive at some pressure altitude prior to exposing most of the main science instruments, and then change the ballistic coefficient for descent and optimize the time you spend in the atmosphere, optimize the data sampling rate for the various instruments, and optimize your communications geometry and communications time perhaps a little better. But that is quite a penalty to pay to gain these small improvements.

Unstaged entry turns out to be lighter, in most cases, and we are basing these numbers on our 250-pound probe, by about 15 or 16 kilograms in weight, and removes all of the complexity associated with the parachute design, heat shield jettisoning, and all of the associated mechanisms.

Staged entry accommodates the conflicting ballistic coefficient requirements better. It improves the ability to expose sampling inlets after entry, and while these are advantages, they certainly don't outweigh the advantages of unstaged entry.

DESIGN CONDITIONS FOR EXIT FROM ENTRY

	TROPOPAUSE COOL DENSE ATMOS.		100 mbar COOL DENS ATMOS.
	ALT (km)	PRESSURE (mb)	ALT (km)
JUPITER	19.4	259	31.1
SATURN	35.3	204	48.2
URANUS	35.8	330	50.4
NEPTUNE	15.2	660	30.6

Figure 4-6

MAJOR STAGING TRADES

UNSTAGED ENTRY	STAGED ENTRY
• STAGING COMPLICATES DESIGN PARACHUTE DEPLOYMENT AND HEAT SHIELD JETTISON QUESTION-ABLE RELIABILITY	• BETTER ACCOMMODATES CONFLICTING BALLISTIC COEFFICIENT REQUIREMENTS
• LIGHT WEIGHT ~ 16 kg	• EXPOSES SAMPLING INLETS AFTER ENTRY
• AEROSHELL PROTECTS EQUIPMENT DURING DESCENT	• UNCOVERS COMMUNICATIONS ANTENNA
	• SLOWER DESCENT RATE FOR MORE SCIENCE DATA

Figure 4-7

Now, in terms of heat shield, Figure 4- 8 summarizes very briefly the entry conditions we found at the various planets, and the ranges of these planets. I won't dwell on this because it is all in the report.

Figure 4- 9 shows the principal reason for excluding Jupiter prior to the preliminary information from the Pioneer 10 encounter. Without the ability to go to very shallow entry angles and with the atmospheric model that had been projected prior to Pioneer 10, the Jupiter heat shield mass ratio is just completely out of tune with the heat shield mass ratios for the rest of the missions.

Also, the ability to simulate those heating conditions is quite limited. The heating conditions associated with Jupiter entry as shown on the convective heating and radiative heating plot of Figure 4-10 and the simulation capability shown reveal the very limited simulation capability that exists and this also led us to the feeling that Jupiter should be postponed.

I think I will move ahead to the last, Figure 4-11. (The only thing that I am skipping is the spacecraft interplay, and that was covered very thoroughly just a few minutes ago.)

The impact of the Pioneer 10 data on our conclusions has to a degree been covered already. The potential change in atmospheric model should reduce the entry heating rates. The improved ephemeris should allow a much shallower entry and further reduce the heating rates. And the fact that the radiation environment is now better known should improve the ability to design both the probe and the bus for a Jupiter mission.

MR. CANNING: Are there any questions that would be other than lead to revisions to the Ten Bar Probe Summary?

OUTER PLANET ENTRY CONDITIONS

	JUPITER	SATURN	TITAN	URANUS	NEPTUNE
ENTRY VELOCITY (km/sec)	59 TO 61	36 TO 38	5 TO 12	22 TO 25	25 TO 28
ENTRY ANGLE (deg)	−15 TO −30	−20 TO −30	−60	−35 TO −60	−20 TO −30
3σ ENTRY ANGLE DISPERSION (deg)	1.4	9 TO 1	15	15 TO 7	—
MAX. ENTRY INERTIAL LOADS (G)	1500	585	36	850	300
MAX. PEAK DYNAMIC PRESSURE (MN/m^2)	1.00	0.73	0.17	0.86	0.5
MAX. PEAK HEATING RATE (MW/m^2)	352	120	11	170	68 est.
MAX. INTEGRATED HEATING ($MW\text{-}sec/m^2$)	965	613	216	390	375

Figure 4-8

COMPARISON OF HEAT SHIELD MASS FRACTIONS FOR ENTRY INTO THE OUTER PLANETS

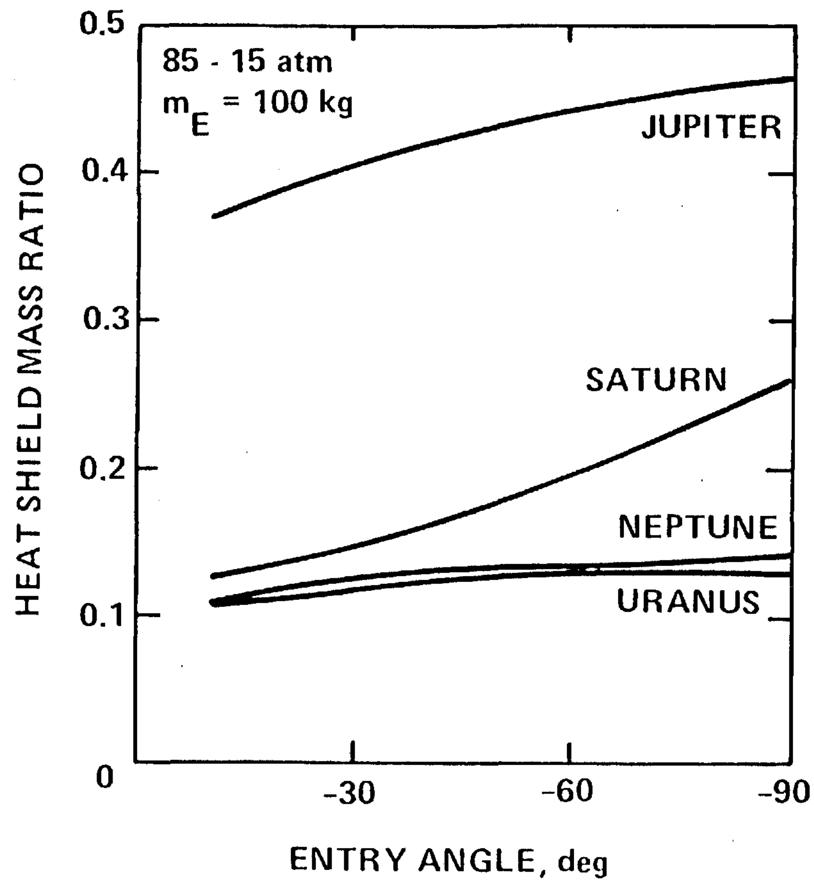

Figure 4-9

EXISTING HEATING SIMULATION CAPABILITIES
AND JUPITER ENTRY REQUIREMENTS

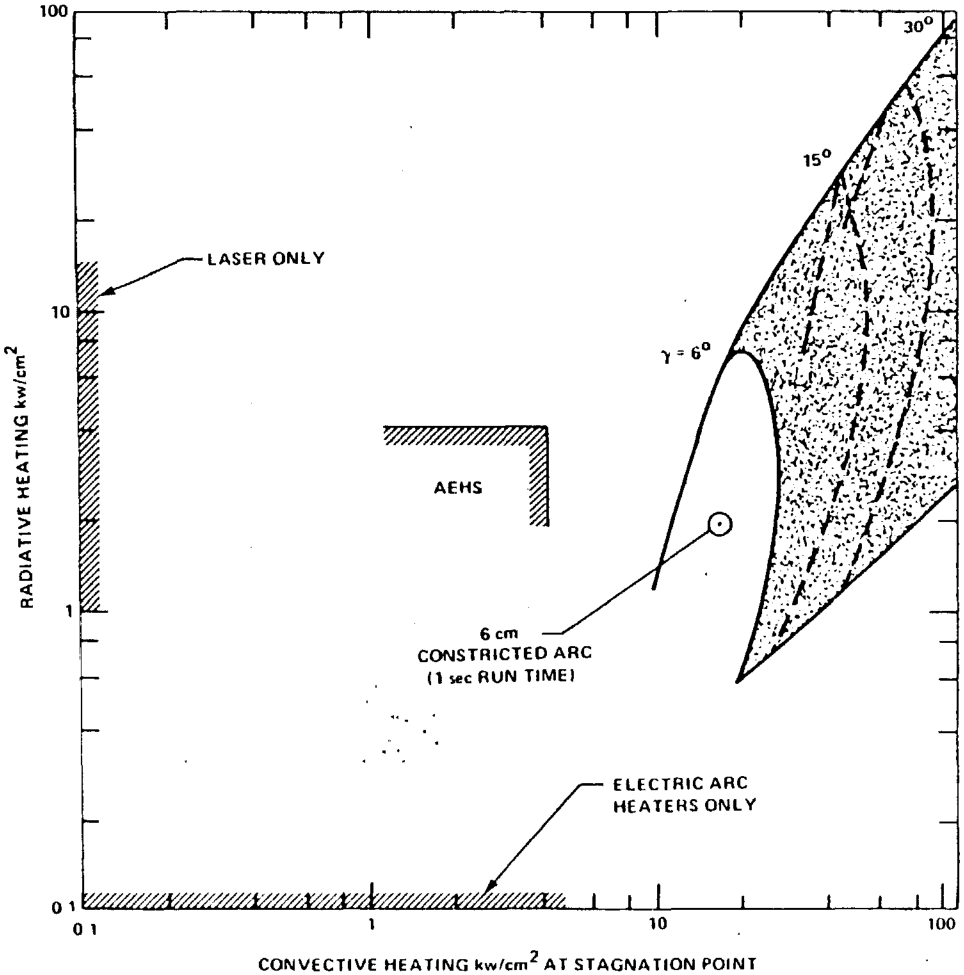

Figure 4-10

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PIONEER 10 IMPACT

- ATMOSPHERIC MODEL
- EPHEMERIS
- RADIATION ENVIRONMENT

Figure 4-11

MR. HERMAN: Not a question, but a comment. What I have seen on the charts indicates why, up to the Pioneer 10 encounter we did not plan a Jupiter entry program until 1985; primarily, because test facilities did not exist in the United States to simulate the entry conditions. And one key issue of this workshop, and subsequent studies, would be another assessment: is a Jupiter entry probe at a shallow entry angle conceivable, from a commonality standpoint, with that of a Saturn and Uranus probe?

MR. CANNING: Yes, I think that you would find that the commonality would be less expensive than indicated by the earlier study.

MR. HERMAN: But is it real? I am still skeptical.

MR. CANNING: It is likely that a Jupiter probe would still be "non-common."

VIKING LANDER DESIGN AND SYSTEMS INTEGRATION

John Goodlette
Martin Marietta Corporation

MR. GOODLETTE: Good Afternoon. I want to address something generally on the subject of integration today, but one which I believe that you, in your deliberations, will eventually face. That is the subject of malfunction protection. There is a dilemma that is there for us all: to return the maximum amount of scientific data that we can, while choosing allocations of our resources to guarantee to the best of our ability to be able to return what we set out for.

Viking is pretty complicated. Many of you are participants on Viking or have been at some point in its development. I will try to address today the question of redundancy. I will describe the principles that we have used for Viking; give you a few examples of some of the implementation; what is not protected and why; and draw the conclusions relative to the effects of this on your mission planning and even on your system test programs.

In your deliberations, as I have noticed today, you very properly were paying attention to those things relative to the science objectives and then the mission design. But when you decide the system that will, in fact, get you there (and you have a very difficult problem I believe, in choosing a common threat to the system that is a multiple planet investigation), you will face the question of how much redundancy should be planned, and how it should be mechanized in order to maximize the chance of getting the data back.

In other words, you want to give yourself a way out in the presence of failure, particularly when you are flying a mission. The things you work with are the same things that we have had to work with: our resources limit, the weight, the power, the money and the data capacity. We chose to follow a principle which goes back to the basic objective of Viking: to land on the planet and acquire data from the surface. Therefore, the first principle in our redundancy was to guarantee the ability to land so that we could provide the data return from the post-landed scientific experiments and, while entering, to acquire atmospheric entry data. We also chose to require most of the decisions, if possible, to be made by the man on the ground, and to have the spacecraft be as simple as possible. This same principle led to the protection of the downlink, which is, of course, the real method by which we get the data back.

Today, I am going to show you a few examples of some subsystems and how we chose to mechanize them. We also used other constraints which you have discussed. They are very real and very important. We tried to limit ourselves to what was available in current technology or, if it wasn't there, to apply our resources to developing it before we mechanize it into a major space system.

Could I have the first slide, please? This is a pretty standard looking fully-redundant RCS reaction control. (Figure 4-12).

On Viking, we do the deorbit impulsive maneuver for the lander system and the attitude control down to the point of deploying the parachute with a single hot gas system. It uses hydrazine, is mechanized with 16 eight-pound thrust engines (which you see at the bottom of the chart there), and it is fully redundant with series valves at each engine. It can tolerate single failures at any point. I will note in passing that we did not try to protect against such things as leakage or rupture of the propulsion plumbing.

The valves are mainly associated with the loading of the gases and the propellants and the necessary unloading in the event we have to recycle after terminal sterilization at the Cape.

RCS DEORBIT PROPULSION SUBSYSTEM

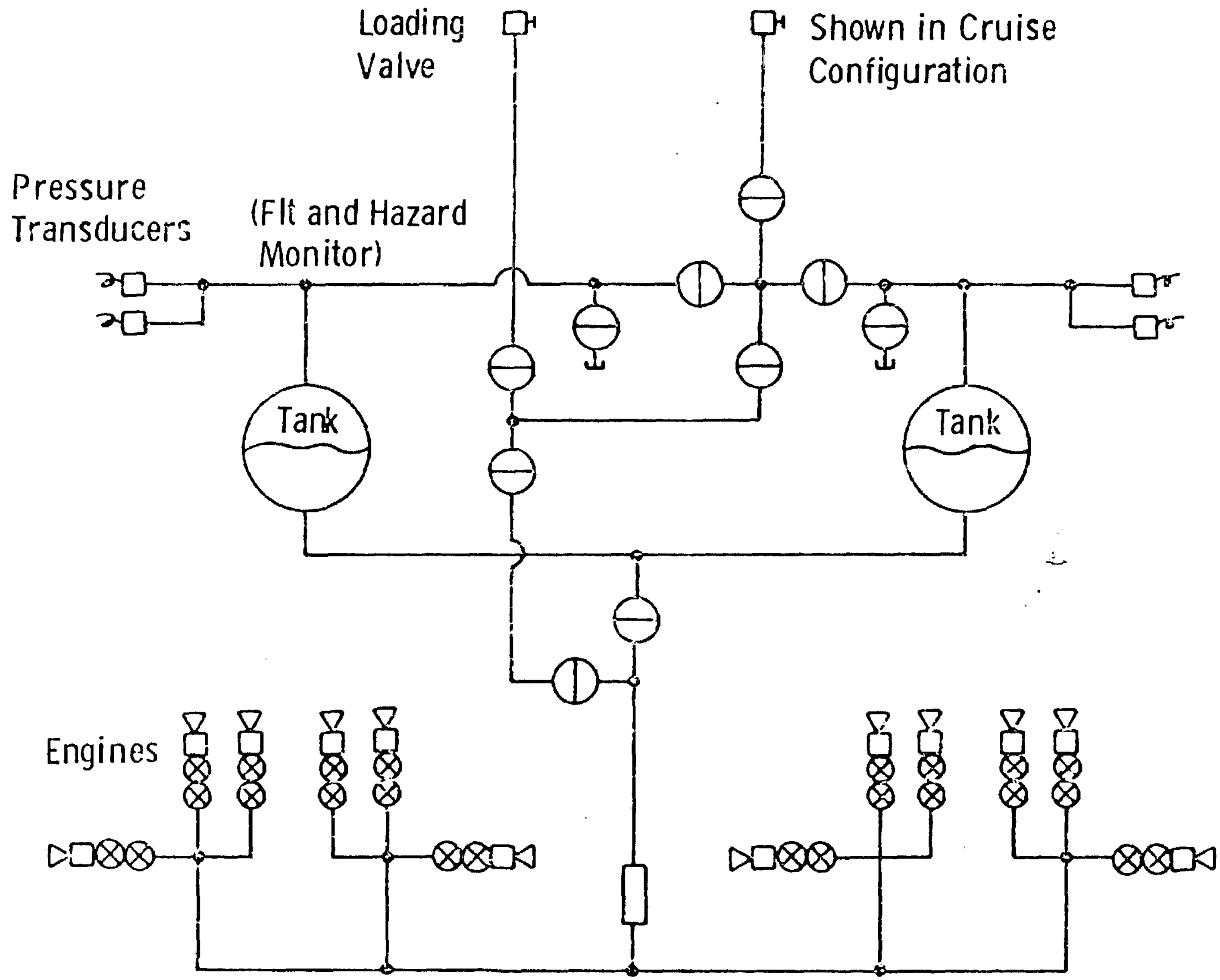


FIGURE 4-12

Figure 4-13 schematically presents the Thermal Control System. We made an attempt to keep the thermal system as passive as possible, but it does have some active elements. There are two active thermal switches mounted immediately under the two RTG's which serve as the only power source the lander has after it separates from its orbiter bus. We do use the orbiter power, of course, with its 680 watt solar array, in the cruise mode and the pre-separation checkout. But after transfer to lander internal power, RTG's are all we have. We use the waste heat from the RTG through the thermal contractors.

You will notice that it is mechanized with redundant bellows to protect and guarantee no single failure will lose us the contact.

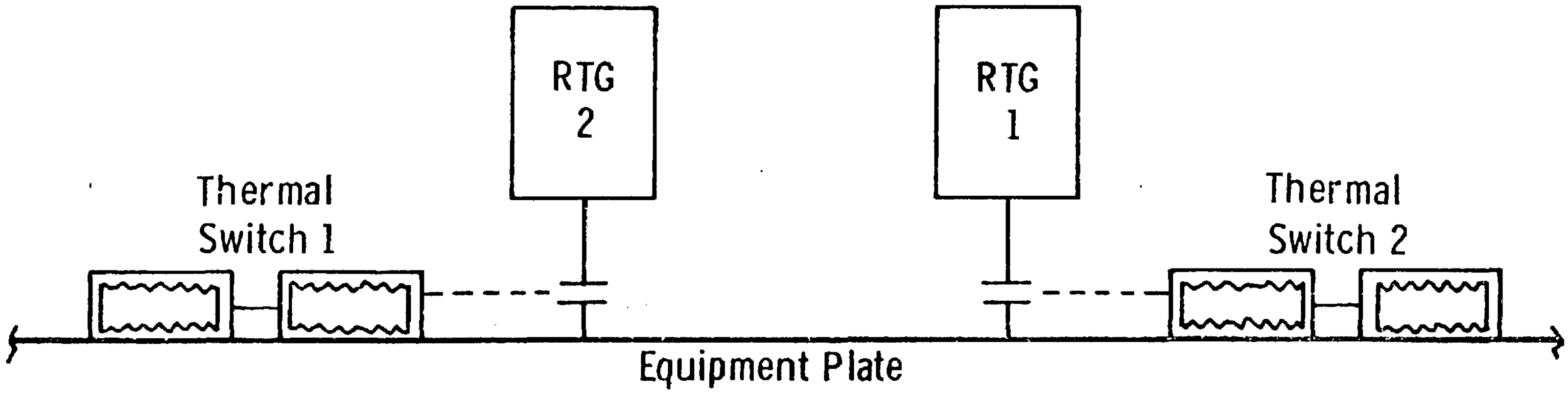
I might say that the chart seems to imply that we can tolerate the loss of one thermal switch. That isn't really true, unless we were very lucky with respect to some of the atmospheric environments in the summer on Mars. We need both of those switches.

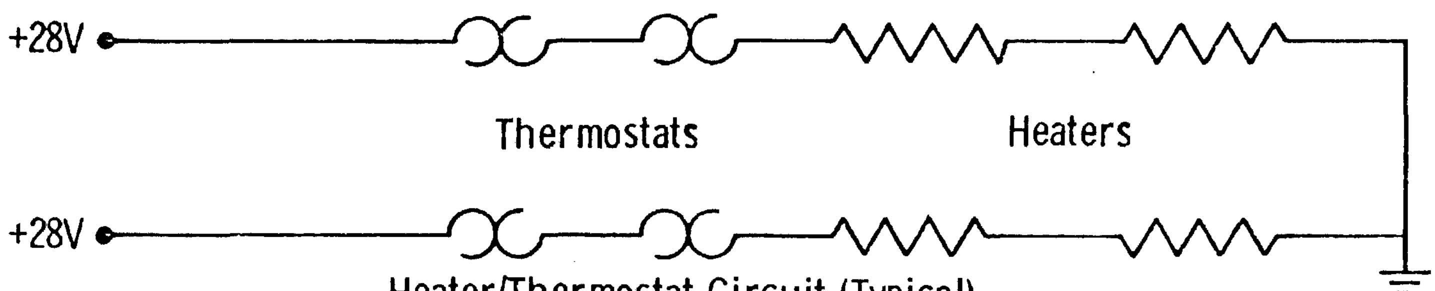
The bottom of the chart describes a pretty standard way of mechanizing thermostats and heaters through series parallel thermostatis switches. We do not try to protect against shorts, generally, in the system, but we do protect against failure open and failure closed in the thermostats. Raw bus power is used for line and tank heaters in the propulsion system, which is on the cold side of the spacecraft on its transit outward from earth to Mars. The lander is opposite side from the sun with respect to the orbiter and, therefore, gets relatively cold.

The deorbit system is mounted on the aeroshell and the terminal engine system is mounted on the lander. Both of them are dry beyond the isolation valve and, therefore, it is necessary to use heat to protect some of the feed lines into the deorbit system, some of the pyro valving, and to keep the propellant itself above the freezing point of hydrazine, which is about 35 degrees Fahrenheit.

C-4

THERMAL CONTROL SYSTEM - ACTIVE ELEMENTS





Heater/Thermostat Circuit (Typical)

Usage: All Propulsion Tanks (4 Circuits)
Deorbit Feed Lines (1 Circuit)
Terminal Engine Pyro Valve
Deorbit Feed Line Back-Up Circuit (New)
(BPA Bypass)

No Exceptions to Policy
In Heater/Thermostat
Circuits

FIGURE 4-13

As shown on Figure 4-14, pyrotechnics is straightforward. We use two independent energy sources off the bus through two pyrotechnic control assemblies, the LPCA's as noted in the chart. The mechanization is fairly standard in that they are enabled, then they are commanded, and then disabled, all by the computer functions through the guidance computer.

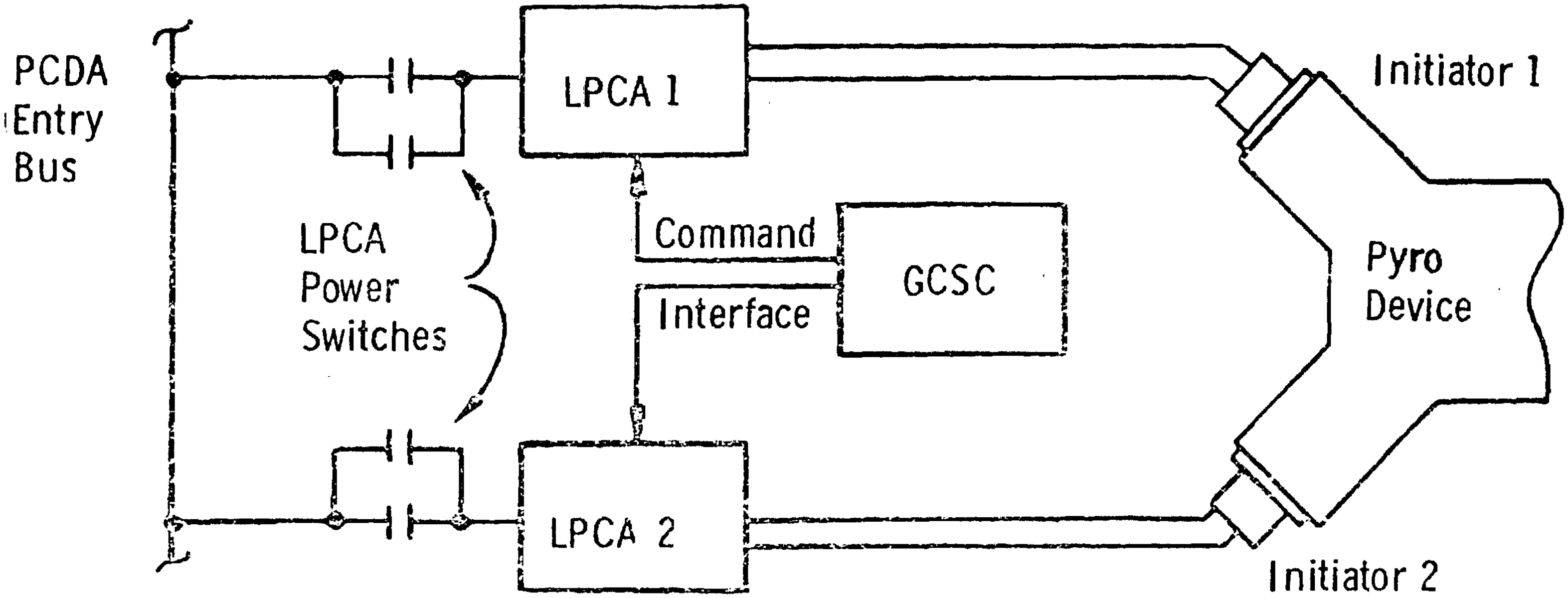
We use a single bridge wire squib arrangement with two initiators per end item, but we do not protect against mechanical single point failures down stream of the initiator. That is to say, there is usually only one set of nuts, one set of pin pullers, and so forth.

The power subsystem on Figure 4-15 is, of course, extremely important to the overall mission success. It is used both during entry and after landing.

To the left of this line is the Viking orbiter, which is based upon the Mariner technology, built by JPL and its suppliers, and we very carefully tried not to require more of the orbiter than is implicit in that Mariner technology. On the other hand, you will find, if you examine the orbiter, that their mechanization principles for redundancy are, to the best degree we are both able, identical. The orbiter supplies the power during cruise. There is a system aboard the lander called the bioshield power assembly which provides dual regulation and dual battery charging that is commandable by uplink from the ground. And that machine stays with the bioshield base, which is attached to the orbiter, and does not enter and land. And, therefore, it is the only thing in the lander system that does not have to be terminally sterilized.

The next assembly, the power subsystem outlined within this line is our power control and distribution assembly.

As you see, we use two SNAP 19 derivative RTG's in series. There is a single point failure in the cabling in between, you might



15 Pyro Device Functions are Single Point Failures

FIGURE 4-14

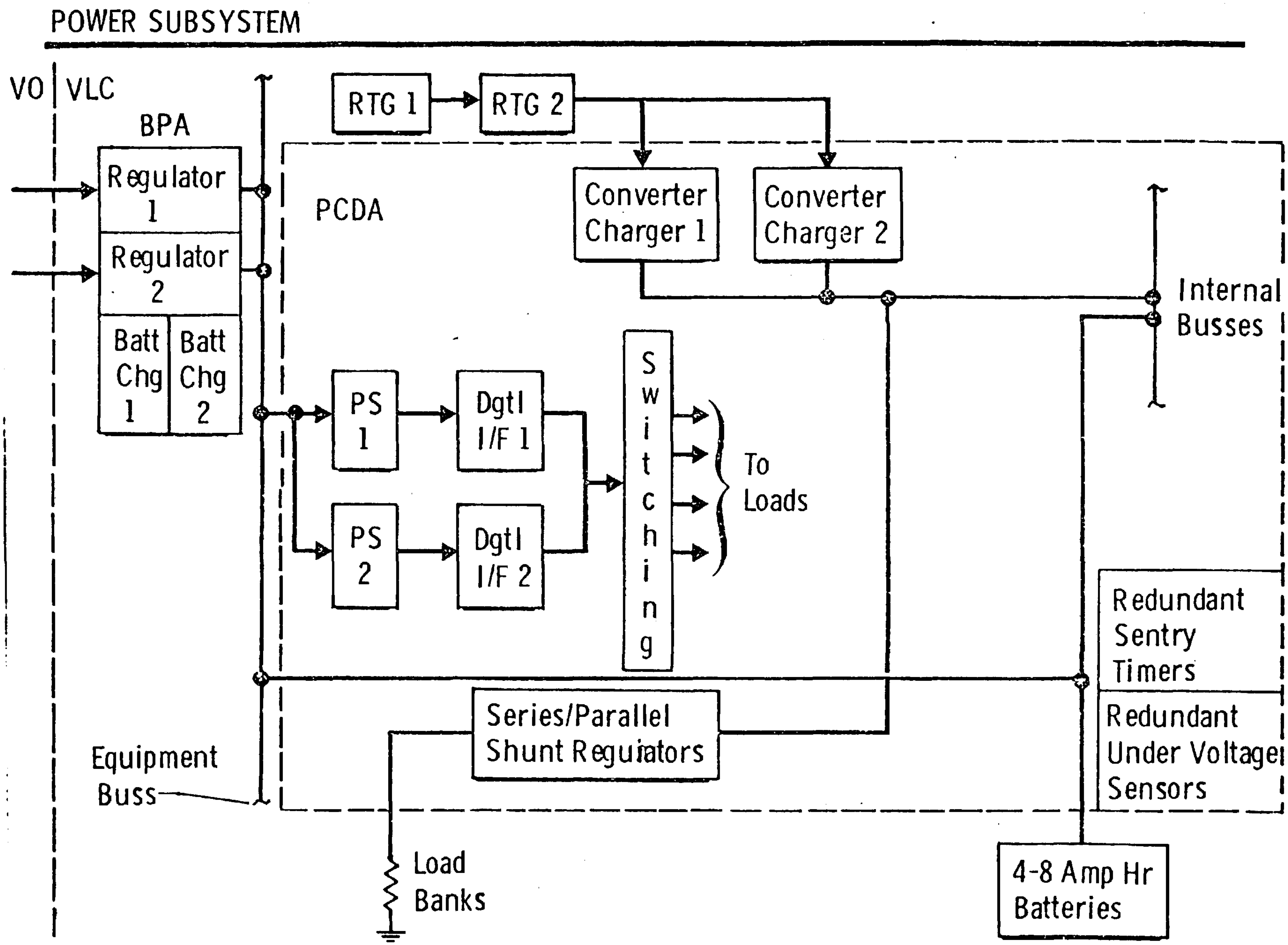

POWER SUBSYSTEM
VO
VLC
BPA
Regulator 1
Regulator 2
Batt Chg 1
Batt Chg 2
RTG 1
RTG 2
PCDA
Converter Charger 1
Converter Charger 2
Internal Busses
PS 1
PS 2
Dgtl I/F 1
Dgtl I/F 2
Switching
To Loads
Redundant Sentry Timers
Redundant Under Voltage Sensors
Series/Parallel Shunt Regulators
Equipment Buss
Load Banks
4-8 Amp Hr Batteries

FIGURE 4-15

notice. But, generally, we then go to dual converter chargers and we have series parallel shunt regulators and we are able to dissipate additional load over and above that immediately needed through lander body-mounted load banks.

We have four eight ampere hour nickel cadmium betteries. Three are required to land and two are required to survive post-land. Sterilizable batteries were a technology problem that was quite important in the beginning.

Our measured capacity after stand times of 25 months, which is somewhat more than the expected lifetime of the mission, has been just above ten ampere hours. Nickel cadmium batteries are sterilizable and one almost gets the impression that one way to make good batteries is to make them tolerate heat sterilization.

You will also notice that there is a dual path for all switching functions. There are two sets of power supplies and two sets of digital interfaces with the guidance computer, which also serves as the sequencer in the mission, both during entry and after landing.

There are two on-board decision points shown over here on the right side. There is a redundant sentry timer, and an under voltage sensor. Their function is required since the lander is out of sight of Earth after landing approximately half of the time, and one really doesn't have real time control. Their function is to place the lander in a safe condition, open the command receivers, and wait for Earth to intervene by command.

Figure 4-16 presents the guidance and control. We have to soft land, of course, on a windy planet, and that leads us to a 3-axis stabilization system. We have to transfer the reference from a celestial reference picked up from the orbiter, navigate inertially

GUIDANCE AND CONTROL SUBSYSTEM

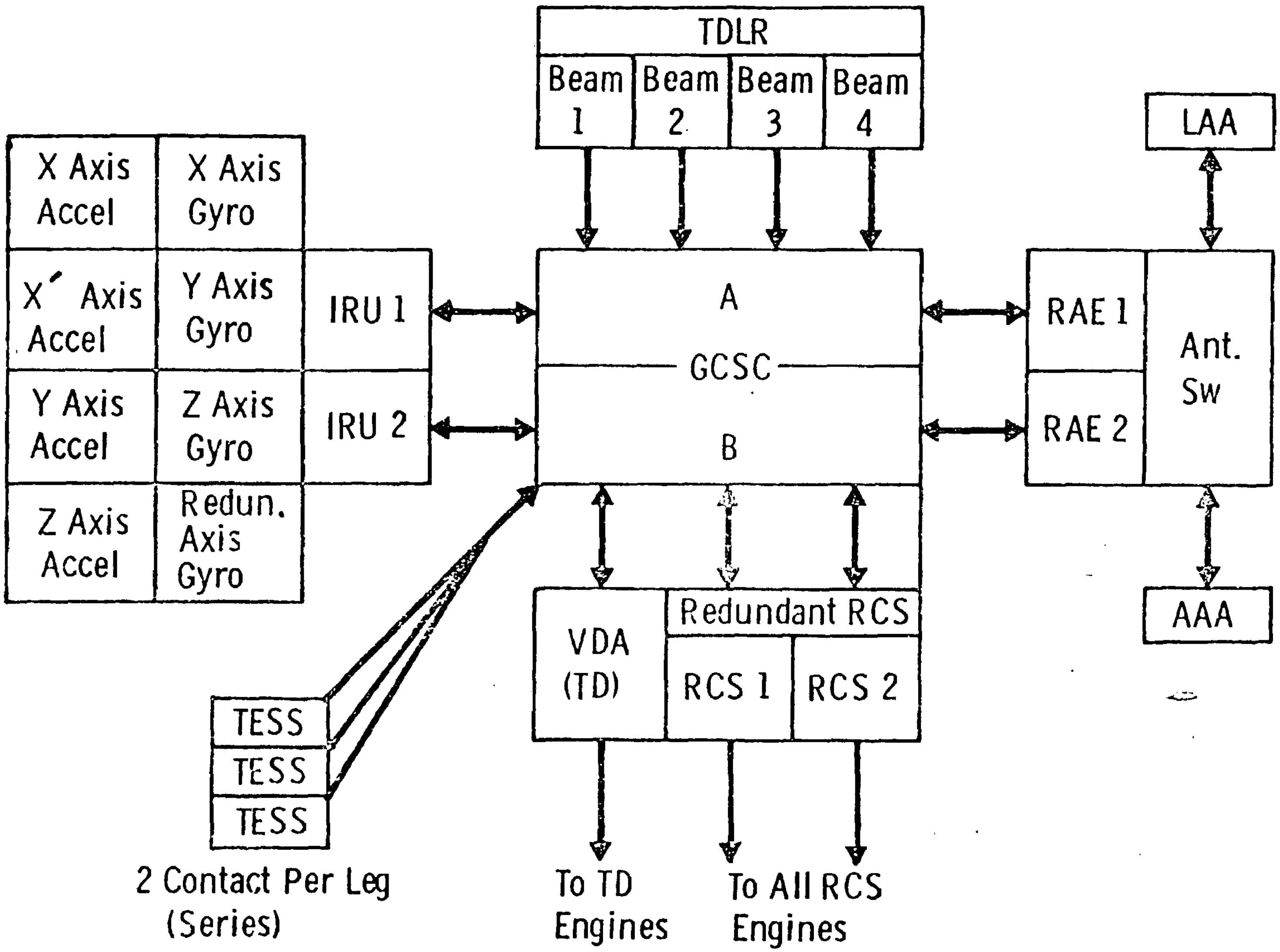


FIGURE 4-16

downward in the inverse of the ballistic missile problem, and then we have to transfer the reference locally to the ground, removing the lateral and the longitudinal velocities in order to land. The equipment required to do this is gryoscopes, at least one accelerometer, a computer, a Doppler velocity measuring radar, and a ranging radar, and the necessary functions to control the engines, which we call valve drive amplifier functions. Finally, there must be a way to shut things down, and we have terminal engine shut down switches. These guidance elements are all redundant.

An on-board decision is made to select between two sets of electronics for the radar altimeter during entry. There are two antennas, one looking through the aeroshell, and another used after aeroshell is separated. There is a switching function between these antennas.

The Doppler radar, called the TDLR, is a four-beam system such that any three beams will solve the equations of motion. There are four independent power supplies, and they are on all the time.

There are four sets of gyros shown in this column, an orthogonal set, X, Y and Z, and one skewed such that one can choose in pre-separation checkout which three to mechanize, and the equations of motion and the software are designed to tolerate the use of any of the three of four on the entry. To land, you really only need one accelerometer longitudinally. However, for entry science reasons, we have also lateral accelerometers; and, to provide the redundancy, we have doubled up on that longitudinal accelerometer. The one to use is chosen in pre-separation checkout. So there really are two IRU's. It is beautiful little package, incidentally. It weighs about 30 pounds with its eight inertial instruments and its shock isolator.

Finally, the terminal engine shut down switches have two series contacts per leg: as we fly into the ground, any closure of both switches on one leg will shut the engine down. And if you bounce and hit another leg, you get another chance - as a matter of fact, you get three chances at it.

The deorbit system valve drive amplifiers are redundant through the electronics, but the terminal engine system and its valve drive amplifiers are single string. We reached the weight limit and were unable to provide redundancy here. There is a mechanization for six engines that is well known, but we could not pay the penalty of that weight.

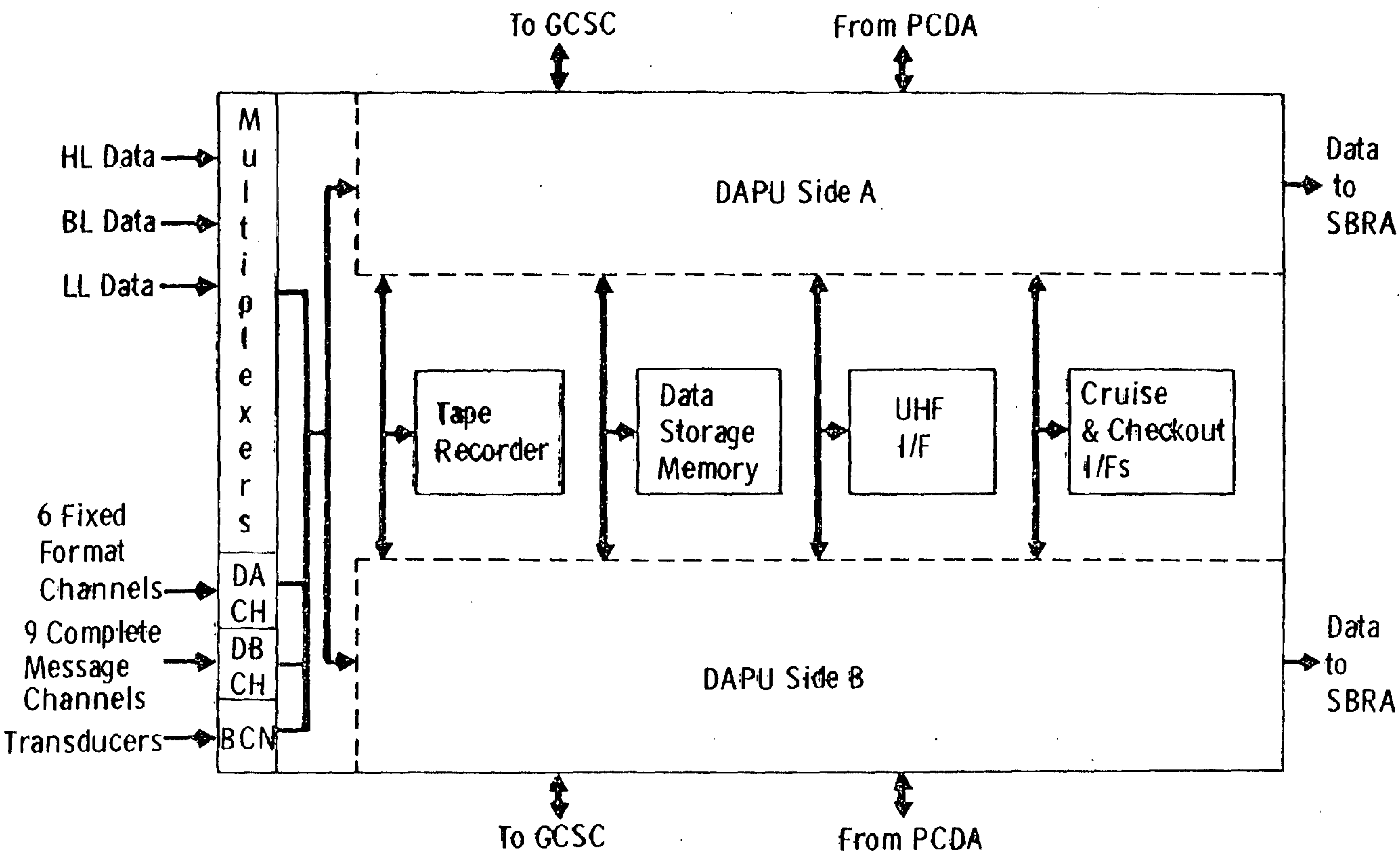
Finally, the guidance computer is block redundant. It has two 18,000K memories, two processors, two power supplies. One of the systems is selectable before separation to enter with: but if both are good, you then have the chance to use them after landing, and the sentry timer in the power subsystem is a device by which, in the event of failure, the lander is shut down to wait for a transfer to the other side by ground command.

Figure 4-17 presents the Telemetry Subsystem which is pretty straight forward. The basic collection device is the data acquisition and processor. The data is analog, digital, high level, low level, and bi-level data; all are converted by DAPU to six fixed format digital channels. The scientific instruments and engineering transducers are the basic source of the data.

The storage systems are functionally redundant. There is a fast access data storage memory of about 200 K capacity, and a slower access 40 million bit tape recorder: it has four tracks and is able to read and write in either direction. The data processer accepts the data, formats the data, and modulates the carriers for the output to the radio systems. These include the UHF system, which is the relay with the orbiter, and the S-band system which is a direct link to the Earth.

On Figure 4-18 is the communications subsystem, the radio subsystem. There is a functional redundancy as I described earlier.

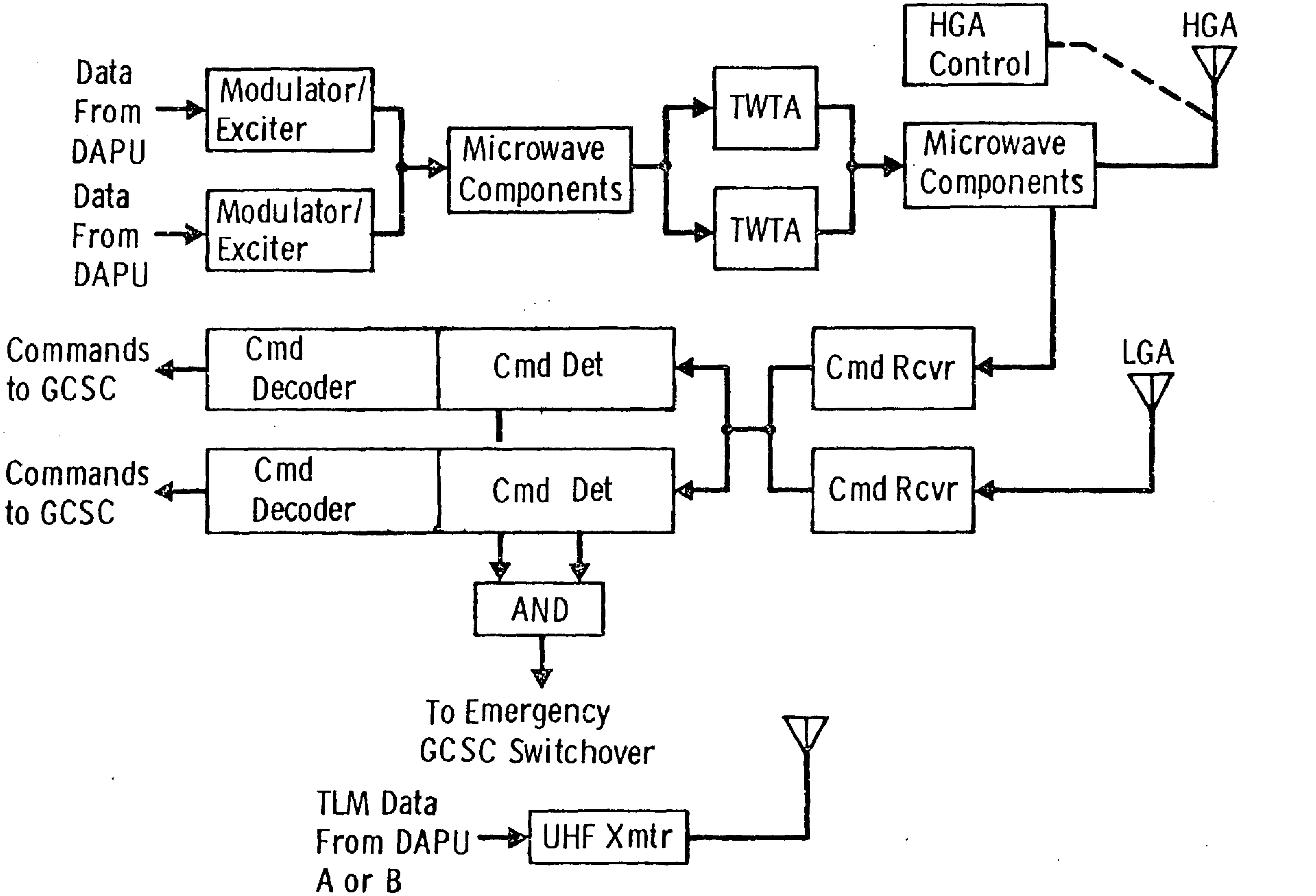
VIKING TELEMETRY SUBSYSTEM



No Exceptions to Policy

FIGURE 4-17

VIKING COMMUNICATIONS SUBSYSTEM



No Exceptions to Policy

FIGURE 4-18

The system has several commandable data rates. The lander can relay through the orbiter with a single string UHF system at a maximum rate of 16,000 kilobits per second after landing; and normally that is the one we will choose. The orbiter, of course, buffers that by a factor of four to get back down to, say, four thousand or by a factor of eight to get to 2K. Lower rates, however, are used during entry. We normally transmit at 2,000 bits per second, but we double that toward the end as we interleave one set of new data with old data delayed about a minute in order to avoid the blackout problem on entry.

The communications system does have the ability to do some on-board switching between the exciters, the command control unit, the microwave components, the two 20-watt TWTA's and the antennas.

There are two ways to get to the dual command receivers: through the low gain antenna or the high gain antenna.

I would like to summarize by saying that the choice of malfunction protection is pretty far-reaching. When you define the spacecraft hardware and its interfaces very carefully and relate it to the science mission, I think you will find that all of your operational alternatives of support software and your system test program will be very heavily influenced by how much redundancy you choose to use. To give you one final number, what I have shown you totals about 170 pounds of hardware in the Viking system for redundancy reasons only. Approximately ten percent is devoted to redundancy.

Thank you.

MR. CANNING: Are there any questions? I had one myself. Would you put up the slide on the guidance and control? The issue is, here, you say, that you have four of these radars, I guess they are, and any three of them can work. Suppose one of them starts working badly, then how do they decide amongst themselves which one is working right?

MR. GOODLETTE: In the pre-separation checkout, you can inhibit the beam you observe to be bad. If one fails during use, a "data good" software flag drops and the software ignores that beam. What you get is a mixed solution.

MR. CANNING: This would be a place where redundancy might in fact introduce, that is, if any one of them goes wrong, a failure mode.

MR. GOODLETTE: Exactly

MR. CANNING: Rather than eliminating failure modes.

MR. GOODLETTE: I think the time you spend on the front end choosing redundancy is very, very important because you can certainly drive yourself into a corner if you have more redundancy that you can use or you can test; it can cause you failures, unless you carefully choose and test the mechanization.

MR. CANNING: My own experience with failures, and I have had a couple, has been that mostly the systems that failed were highly redundant and, in some cases, the very existence of redundancy caused the trouble.

MR. GOODLETTE: That can happen.

UNIDENTIFIED SPEAKER: I didn't quite understand that. Did you way there is a majority voting system in here that would check it after you separate the lander, or does this have to be done by command?

MR. GOODLETTE: No. you can disable one of the beams, but if they are working at pre-separation checkout, there will be four beams operating. The reason for that is that as you swing on the parachute, for example, you can wipe one or more of the beams off the limb of the planet and, therefore, the solution of the equations of motion can lose input. To solve all of the quations all the time, you only need three.

UNIDENTIFIED SPEAKER: While it is doing that determination, does the computer have the capability to switch off a beam and switch another one in?

MR. GOODLETTE: Not that. What we really do is we inertially navigate down all the time. If you do not get a data signal good from at least three beams, then you continue the inertial navigation. What you really have is about two second update time so that you are updating the inertial velocity reference with a two-second time constant. And if you miss it for upwards of twenty or thirty seconds, that will really do nothing more than delay the time that you update that system. You eventually have to get only a few good seconds to land.

MR. SEIFF: Is the TDLR system involved in the pre-separation checkout?

MR. GOODLETTE: Yes, there will be measurements.

MR. SEIFF: In other words, you check it out just a few hours prior to committing?

MR. GOODLETTE: Yes. Pre-separation checkout starts about 30 hours ahead of entry, and we are able to disable a failure by command.

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PIONEER VENUS PROBE DESIGN

L. J. Nolte
HUGHES AIRCRAFT COMPANY

Strictly speaking, I don't belong here because I am going to talk about a set of probes designed to explore an inner rather than an outer planet, and designed to survive to 100 bars rather than 10 bars. Nevertheless, they represent a detailed look at what it takes to fly the complement of instruments that we have been talking about here today, and they will probably be the first such set that flys. We thought you might be interested in hearing where Pioneer-Venus stands at the moment.

Before starting, I would like to note that all the view graphs in this presentation are marked with the Hughes logo. This is somewhat misleading because the probes in this mission are really a joint venture between Hughes and the General Electric Company; Dave Stephenson, the General Electric Program Manager, is with us today.

Figure 4-19 shows the probes, one large and three small, mounted on a bus that transports them from here to Venus. The whole system, as you can see, weighs 1760 pounds, of which a little over 600 pounds is invested in the large probe and about 160 pounds in each of the three small probes. The heart of the problem is going to be the integration of 33 separate instruments into those packages. This may be one of the highest number densities of instruments that has ever been flown. The large probe will carry 77 pounds of instruments, 12 in number. This includes the basic payload that was described this morning, the optional payload, plus a wind-drift radar and a spin-scan photometer. Each of the small probes contain pressure and temperature sensors, an accelerometer, a nephelometer, and a net flux radiometer.

Figure 4-20 addresses the question of where we are going. Simply stated, the basic requirements in probe targeting are

PIONEER VENUS MULTIPROBE SPACECRAFT

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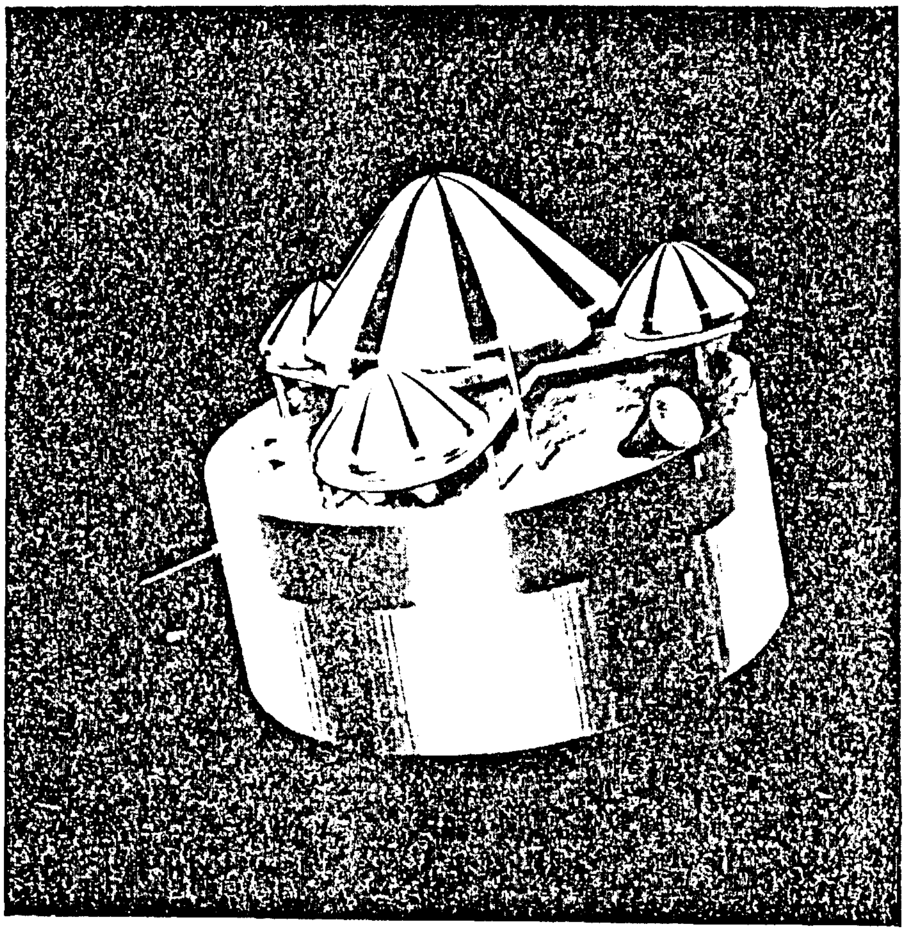

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IV-38

- **SIZE**

WIDTH :	8 FT , 4 IN.
HEIGHT :	11 FT

- **WEIGHT**

PROBE BUS :	677 LB
LARGE PROBE :	605 LB
SMALL PROBES :	160 LB EACH
TOTAL :	1760 LB

- **POWER :** 225 W
- **DATA :** 11 TO 2816 BPS
- **SCIENTIFIC PAYLOAD**

PROBE BUS :	40 LB, 6 INSTRUMENTS
LARGE PROBE :	77 LB, 12 INSTRUMENTS
SMALL PROBE :	5 LB, 5 INSTRUMENTS
TOTAL :	132 LB, 33 INSTRUMENTS

FIGURE 4-19

LARGE & SMALL PROBE TARGETING

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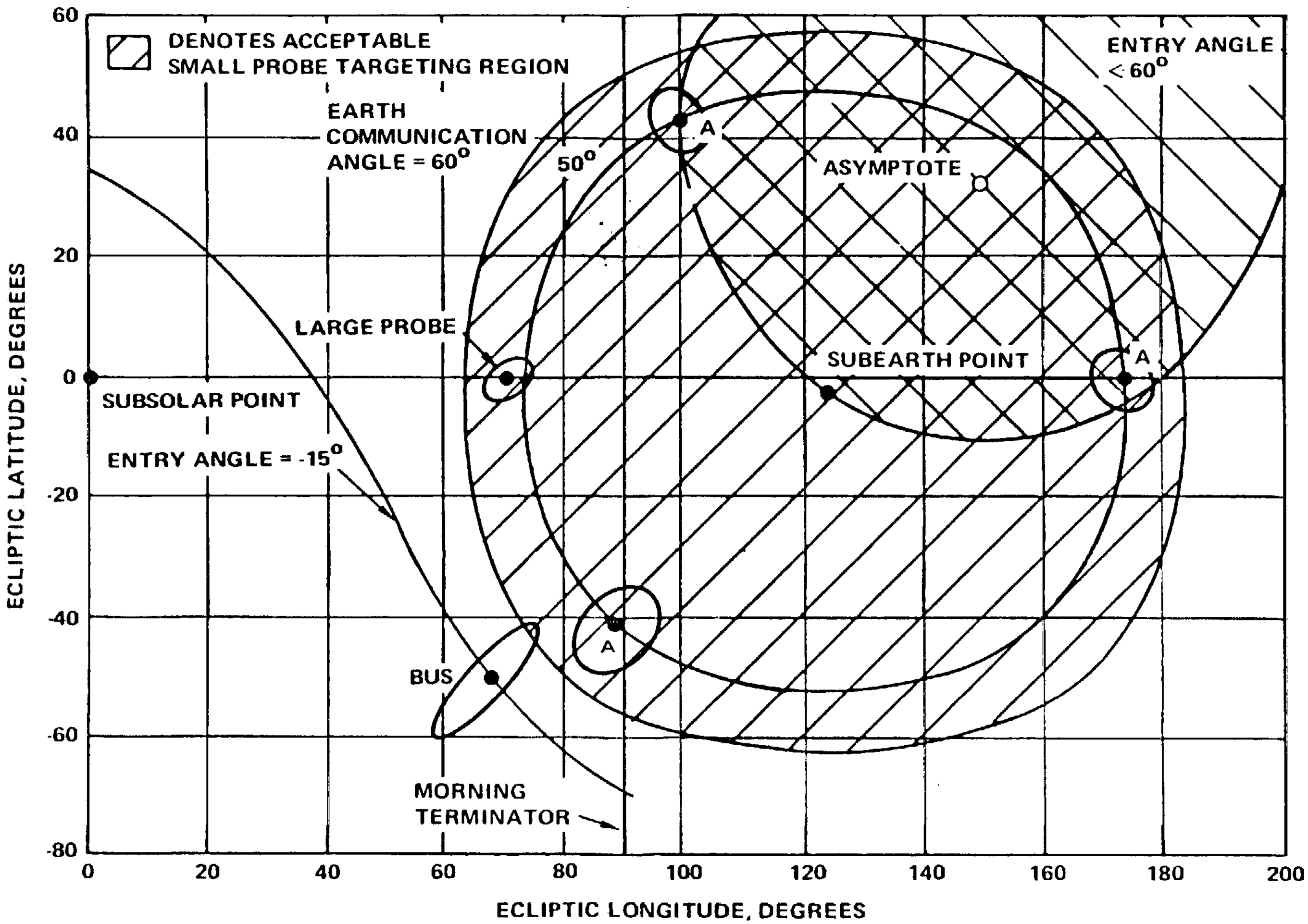

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FIGURE 4-20

these: the large probe wants to look at the clouds; it wants to know what their composition and characteristics are. It wants to make a detailed analysis of the composition of the atmosphere all the way to the surface. It wants to look at the interaction of light and re-radiation at all altitudes. Consequently, it wants to be placed on the daylight side of the terminator, which in this plot is at 90 degrees longitude.

The small probes targeting requirements might be summarized by saying that they want to be as far apart as possible; that is, they want to be widely spread in longitude and in latitude. The objective is to construct a three-dimensional picture, instantaneous, if you will, of the large-scale motions of the atmosphere.

The other lines in this busy figure have to do with non-science constraints. For instance, the specified entry angle design limits of 15 degrees and 60 degrees (down from horizontal) are shown. The cross-hatched circle represents permissible communication angles, and angle between local vertical and the earth line, and we would rather not go below about 60 degrees. Thus, the permissible targeting area for the probes lies in this circle as vignetted by the 60-degree entry angle. (We have chosen to increase the design capability of the small probes so that they are capable of entering at 90 degrees entry angle, and the vignetting is not as severe as represented here.) A possible set of small probe impact locations is indicated by points "A" in the figure.

How do we get there? Figure 4-21 considers that problem. The large probe is carried in the middle of the spacecraft; it is held in place by three explosive bolts and is spring-separated. The three small probes are carried in circular clamp mechanisms, shown in their open position here, and they are targeted on the planet simply by aiming the bus at the center of the targeting area and releasing the latch mechanisms.

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PROBE SEPARATION

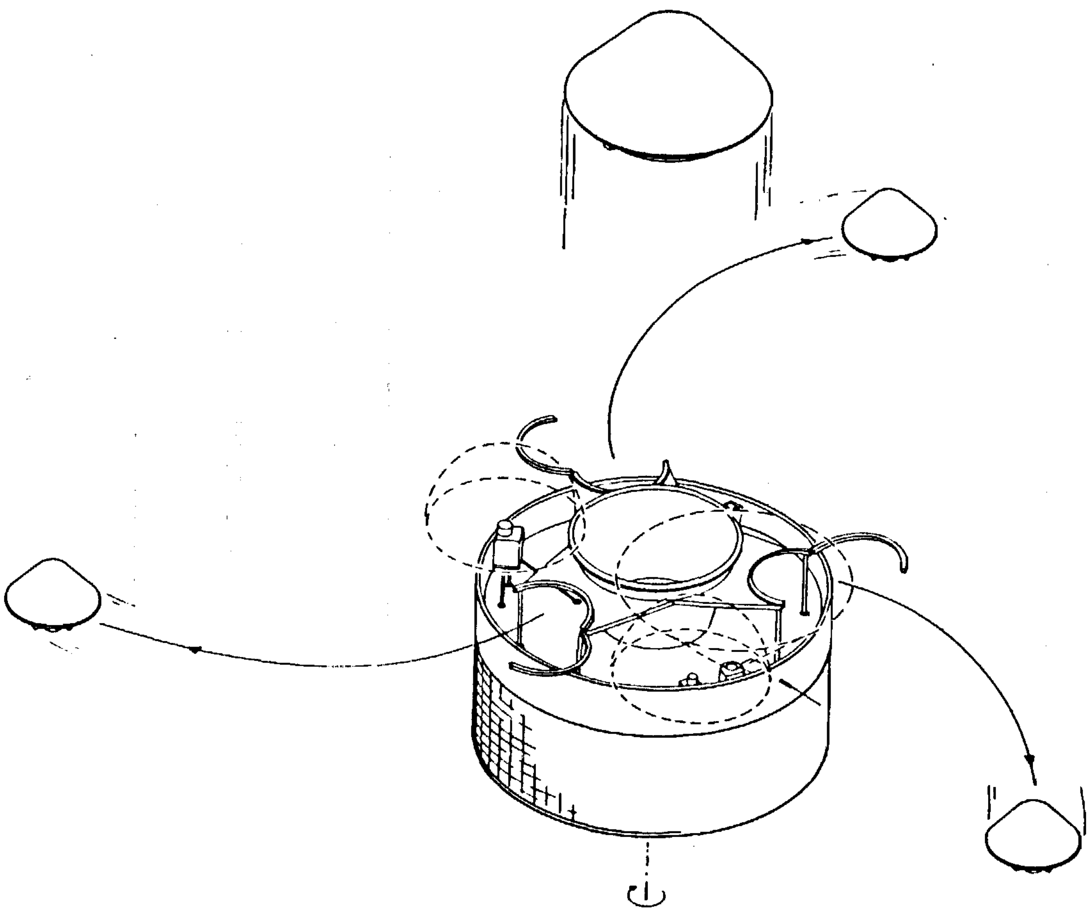

FIGURE 4-21

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The sequence is illustrated in Figure 4-22. About 24 days before encounter, the bus is oriented so that the large probe will enter at zero angle of attack and the large probe is released. About one day later, the bus is retargeted for the small probes, and three days after that it is spun up to about 40 RPM (it had been spinning at 15 RPM in the interplanetary cruise period). About 20 days away from the planet the latches shown in the previous figure are released and the small probes move laterally away from the bus. Two days later, the bus, which is actually a fifth probe, is retargeted so that it will impact the atmosphere at a shallow entry angle, allowing it to explore the upper reaches of the atmosphere before burnup.

Figure 4-23 shows the sequence of events as the large probe descends through the atmosphere. The entry configuration appears in detail 1. At about 68 and 1/2 kilometers above the surface of the planet, the mortar which deploys the pilot chute is fired. The pilot chute removes a cover from the back side of the entry vehicle which, in turn, pulls the main parachute out of its housing. The pilot and main parachutes are both fairly conventional designs: conical ribbon, disc-gap-band configurations, respectively.

The main parachute is attached to a pressure vessel carried inside the entry vehicle. Once it is stabilized, the restraining bolts that tie the pressure vessel to the aeroshell are fired and the aeroshell is jettisoned.

The system configuration remains as shown in detail 5 from 67 kilometers down through most of the clouds to about 44 kilometers above the surface. Here the main parachute is jettisoned and the system falls to the surface in the configuration of detail 7.

Figure 4-24 is a graphical presentation of the large probe descent sequence. The descent requires an hour from the point of initial chute deployment to the surface of the planet, 25 percent of which is spent in the last ten kilometers. The altitude

MULTIPROBE MISSION RELEASE AND ENTRY

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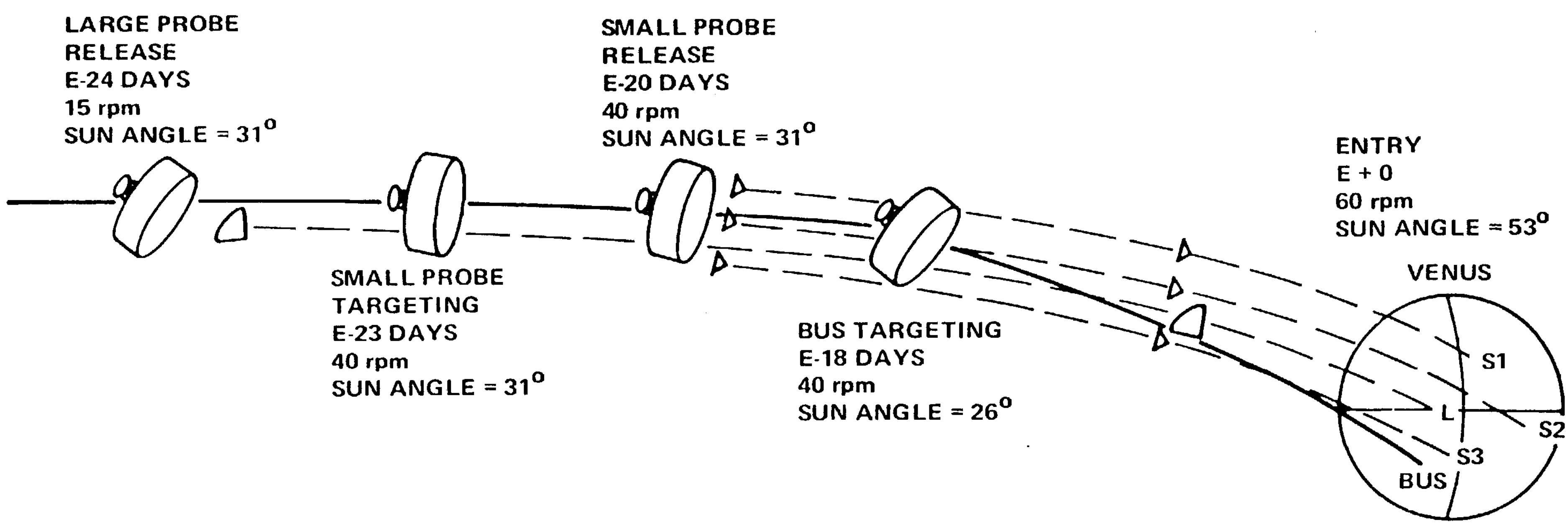
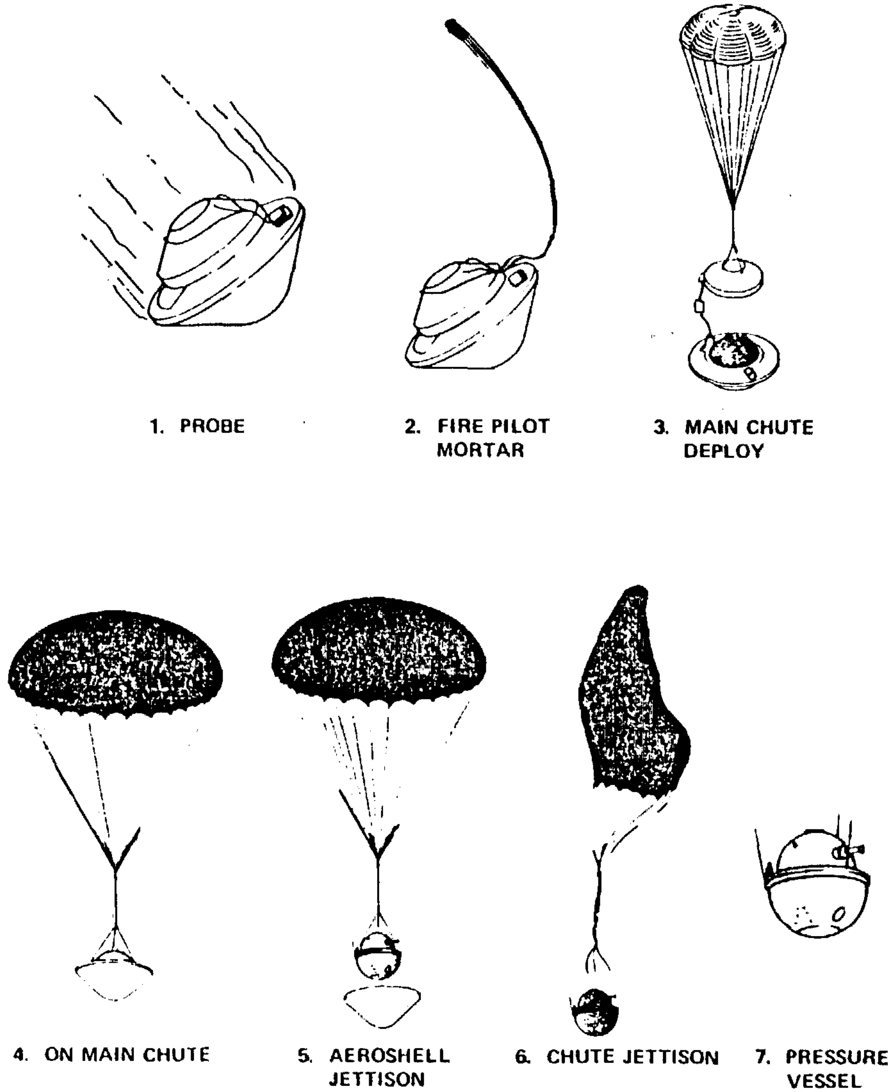


FIGURE 4-22



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PARACHUTE DEPLOYMENT SEQUENCE-LARGE PROBE

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FIGURE 4-23

NOMINAL LARGE PROBE DESCENT PROFILE

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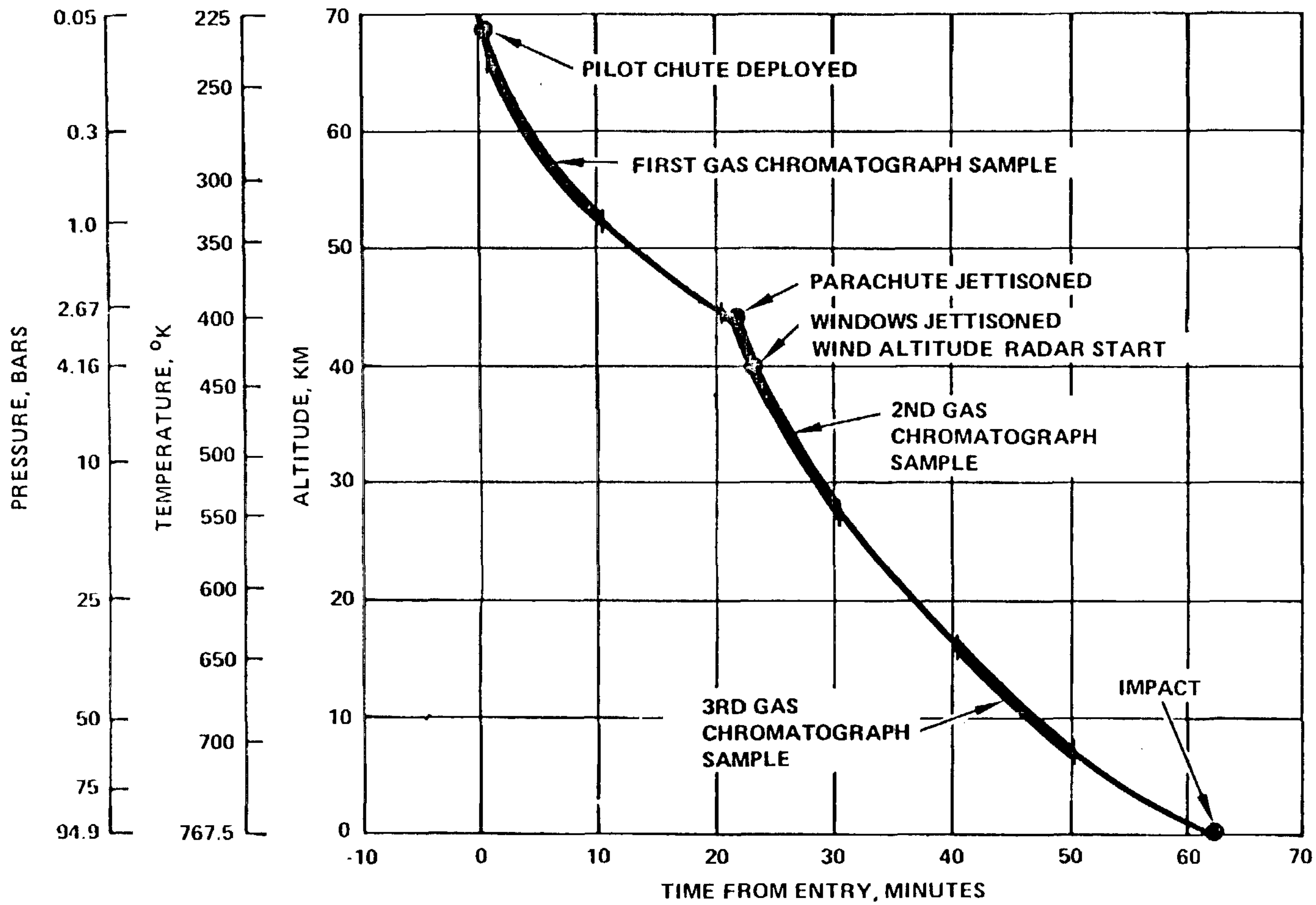

FIGURE 4-24

at which the parachute is jettisoned is a result of a complex trade involving just about every housekeeping subsystem in the probe: data, communications, power, and thermal. It provides the minimum weight mechanization which satisfies the instrument data rate requirements.

Figure 4-25 illustrates similar trajectories for the small probes. Time is taken relative to large probe entry, so that the figure may be compared with the preceding one. The variation in time at which the small probes pass through any given altitude is seen to be of the order of ten minutes. Note that data rate is changed from 64 to 16 bps at 30 KM altitude. This is consistent with instrument requirements because of the large percentage of time spent at the lower altitudes. This could not be done on the large probe because of the staging at 44 KM.

Figure 4-26 begins to show the hardware involved. It is a blowup of a large probe, which comprises a 57-inch diameter, 45-degree half-angle conical entry vehicle and a spherical pressure vessel. The aeroshell is an aluminum monocoque structure protected by a carbon phenolic heat shield. Carbon phenolic was chosen because it is the best characterized material which gives the minimum amount of uncertainty in final shape and base area. The aeroshell, heat shield, aft cover and the parachutes will be built by General Electric Company.

The pressure vessel contains all of the scientific instruments and it is shown exploded in Figure 4-27.

The pressure vessel mounts all of the instruments and housekeeping equipment on two heatsink shelves, of which only the top one is visible. They are mounted together and supported from the spherical pressure shell on a flange located just below the lower shelf. Both are thermally isolated from the pressure shell.

The shell itself is steel, and 28.8 inches in diameter. It is exposed to the atmosphere and consequently is always nearly

NOMINAL SMALL PROBE DESCENT PROFILE

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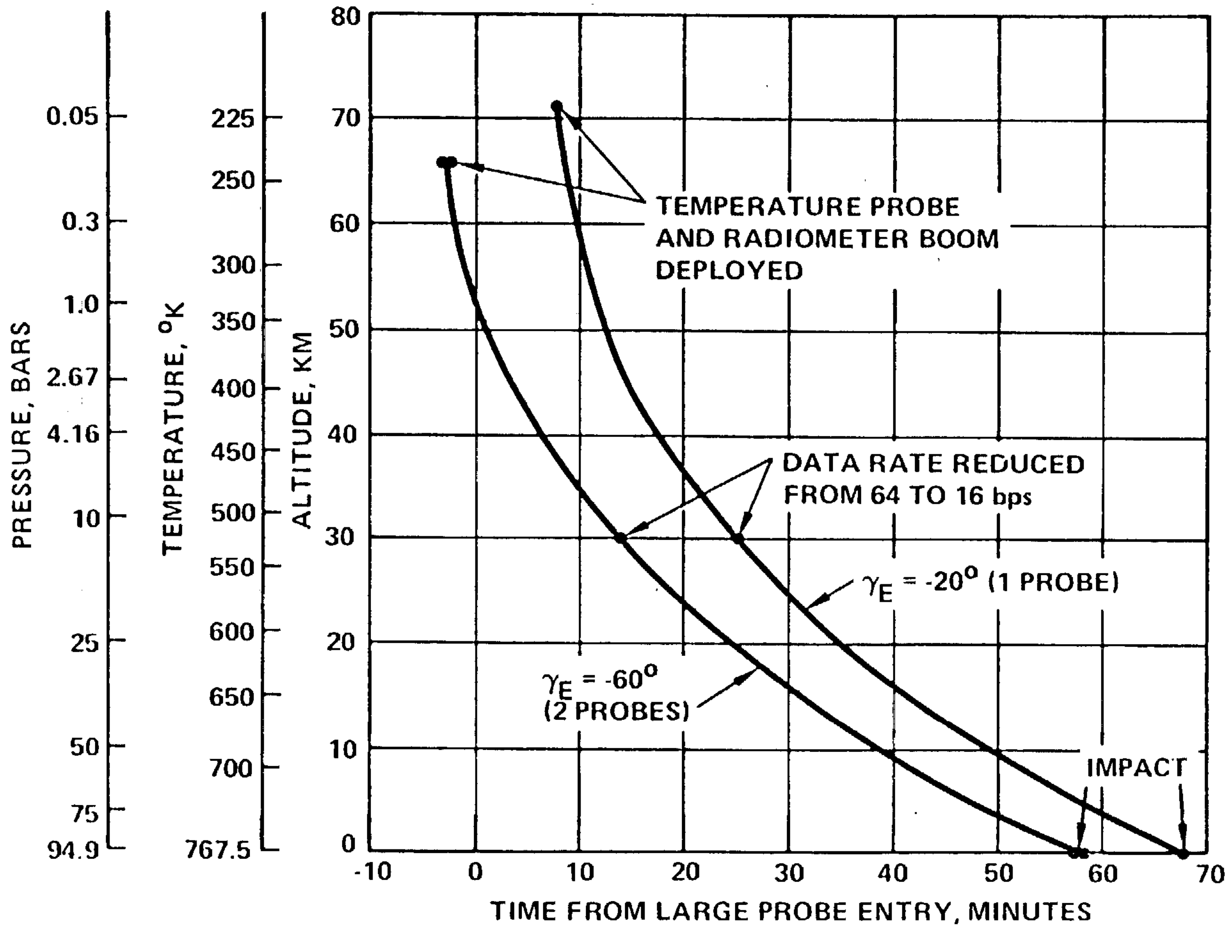
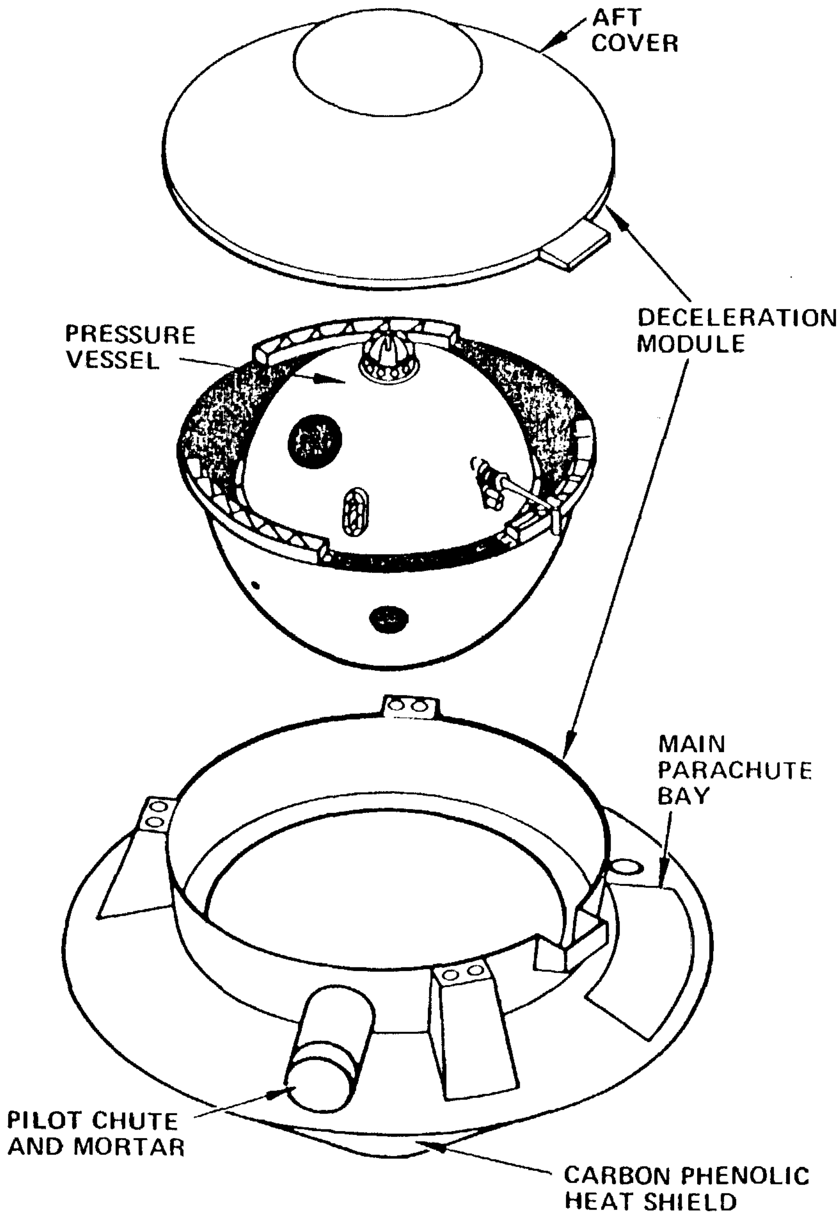


FIGURE 4-25



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LARGE PROBE EXPLODED VIEW

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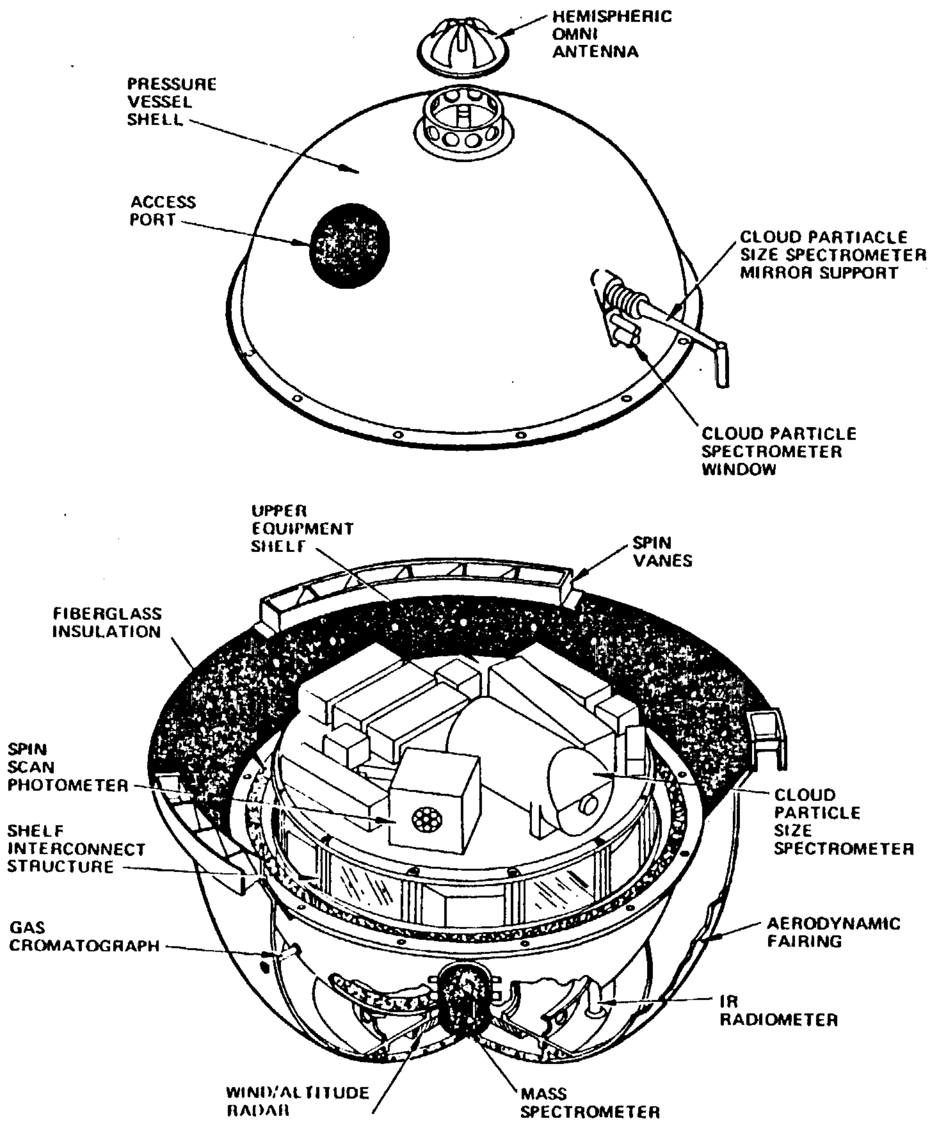
FIGURE 4-26



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PRESSURE VESSEL EXPLODED VIEW

36254 5

FIGURE 4-27

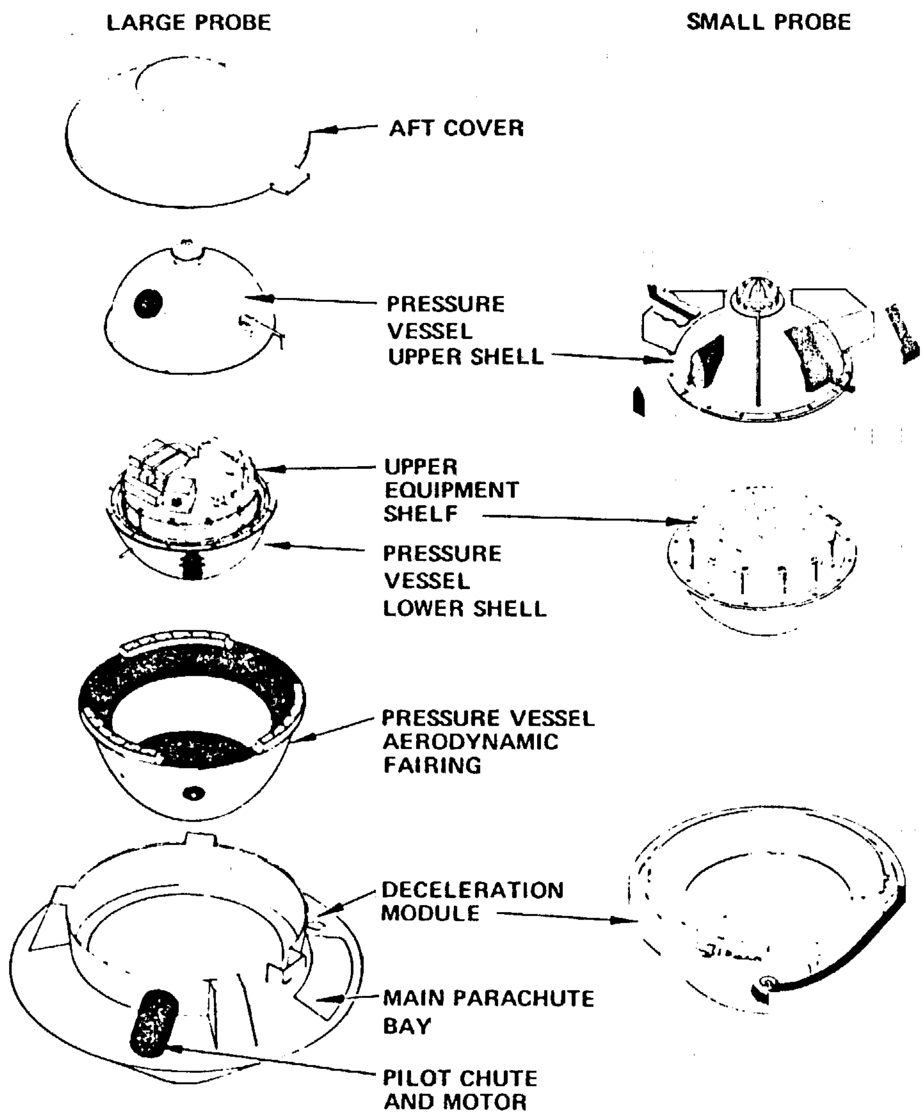
at atmospheric temperature. The equipment is protected by a fiberglass insulation system. One of the objectives of the Pioneer Venus Program is that cost be minimized, and this is one way in which the low-cost philosophy has entered into the design. This is, in our opinion, a more inexpensive way to handle the problem of thermal control than with an external insulation system because it minimizes developmental and system test complexities.

Around the outside of the probe is an aerodynamic fairing. The aerodynamic fairing was necessitated by parts of instruments that must be mounted externally, notably a wind/altitude radar which has a large planar array antenna which wants to be at the stagnation point. For reasons of aerodynamic stability, the antenna is covered by the fairing which contains a radome at its forward end.

Stabilization is further enhanced by separating the flow with a ring just aft of the pressure vessel equator. The ring contains slots in it and the slots contain fins to rotate the probe as it descends.

Figure 4-28 is somewhat redundant with the previous one, but was included because it shows an exploded view of a small probe. The small probe is 28 inches in base diameter and has exactly the same forbody configuration and heat shield as the large probe. The structural and thermal design and materials of the pressure vessel are identical with those of the large probe, and indeed the principal difference between the two is that the small probe aeroshell is retained to the surface.

Figure 4-29 (2 pages), summarizes details of probe subsystems. Note that high degree of commonality between the two vehicles, a feature of the low-cost design approach.



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EXPLODED VIEWS OF LARGE & SMALL PROBES

NOT TO SCALE – LARGE PROBE ACTUALLY TWICE SIZE OF SMALL PROBE

36254-4

FIGURE 4-28

PROBE CHARACTERISTICS AND PERFORMANCE SUMMARY

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SUBSYSTEM	LARGE PROBE	SMALL PROBE
GENERAL	OVERALL HEIGHT: 37.9 IN. DIAMETER: 57 IN. TOTAL WEIGHT: 605.4 LB PRESSURE VESSEL WEIGHT: 401.8 LB SCIENCE: 77.2 LB RELIABILITY: 0.9231	OVERALL HEIGHT: 22 IN. DIAMETER: 28 IN. TOTAL WEIGHT: 159.1 LB PRESSURE VESSEL WEIGHT: 91.5 LB SCIENCE: 5.3 LB RELIABILITY: 0.9438
DECELERATION MODEL	45° BLUNT CONE AEROSHELL W/C_DA: 32.4 LB/FT^2 CARBON PHENOLIC HEAT SHIELD ALUMINUM MONOCOQUE SUB-STRUCTURE 15 FT DISK GAP BAND MAIN PARACHUTE 2.75 FT CONICAL RIBBON PILOT CHUTE	45° BLUNT CONE AEROSHELL W/C_DA: 34.4 LB/FT^2 CARBON PHENOLIC HEAT SHIELD STAINLESS STEEL SUBSTRUCTURE
PRESSURE VESSEL	INSIDE DIAMETER: 28.8 IN. MARAGING STEEL SPHERE PRESSURE VESSEL STABILIZATION-PERFORATED RING WITH SPIN VANES INTERNAL FIBERGLASS INSULATION	INSIDE DIAMETER: 17 IN. MARAGING STEEL SPHERE INTERNAL FIBERGLASS INSULATION

46271-1

FIGURE 4-29

PROBE CHARACTERISTICS AND PERFORMANCE SUMMARY (CONT)

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SUBSYSTEM	LARGE PROBE	SMALL PROBE
RADIO	2295 MHz TRANSMIT 40 W RF POWER TWO-WAY DOPPLER TRACKING	2295 MHz TRANSMIT 10 W RF POWER ONE-WAY DOPPLER TRACKING
ANTENNA	0 DBI HEMISPHERICAL OMNI 180° BEAMWIDTH	→ →
DATA HANDLING	CONVOLUTIONAL ENCODING 256 BPS DATA RATE PCM/PSK/PM MODULATION 2048 BIT SEMICONDUCTOR MEMORY FOUR DATA FORMATS	→ 64/16 BPS DATA RATES → → →
COMMAND	64 COMMANDS FROM PROBE BUS NO GROUND COMMANDS AFTER SEPARATION COMMAND EXECUTIONS: SEQUENCER: 128 MASS SPECTROMETER: 16 22 SEC/24 DAYS CLOCK ACCURACY	22 COMMANDS FROM PROBE BUS → → → →
POWER	26.5 ± 1 VDC BUS 605 W-HR AG-ZN BATTERY 307 W PEAK POWER	→ 176 W-HR AG-ZN BATTERY 66 W PEAK POWER

46271-2

FIGURE 4-29 (Cont'd)

Figure 4-30 attempts to rebridge the gap between the Pioneer Venus probes and the outer planet probes. The latter have been for the most part conceptually designed to survive to the order of 10 bars pressure. We thought that it might be interesting to work our problem backwards, if you will, to see what it costs (in weight) to survive to the surface of the planet, i.e., to about 100 bars, rather than to 10 or 20 bars pressure. This figure illustrates the results for a small probe. It indicates a weight increase of the order 25 pounds to survive to the surface compared to the weight if the probes were designed for, say, ten or twenty bars. This is about 5 times the weight of the instrument payload. Another way of interpreting the figure is to note that there is essentially no pressure-induced weight penalty for survival to 10 bars.

I would like to make one final point. Although I didn't stress the low cost aspects of the Pioneer Venus Program, they are extremely important for program survival. If the outer planet missions are going to be low-cost missions, or moderate cost missions, and the indications would be that they have to be, then this concept must be factored into your planning now. It is not too early.

MR. CANNING: Any questions?

MR. HERMAN: You are treating the bus as a Kamakazi vehicle. How long do you expect it to survive?

MR. NOLTE: Thatis a good question. It may survive to the order of 120 kilometers.

MR. HERMAN: It is certainly not aerodynamically designed.

MR. NOLTE: No, it is not aerodynamically designed.

UNIDENTIFIED SPEAKER: I think the time involved is of the order of ten or twelve minutes.

MR. CANNING: I think we will count ourselves very lucky if we get data below about 135 kilometers that is not dirtied up with ablation products from the thermal control system or blackout.

WEIGHT REDUCTION FOR LIMITED OPERATIONAL ALTITUDE -SMALL PROBE

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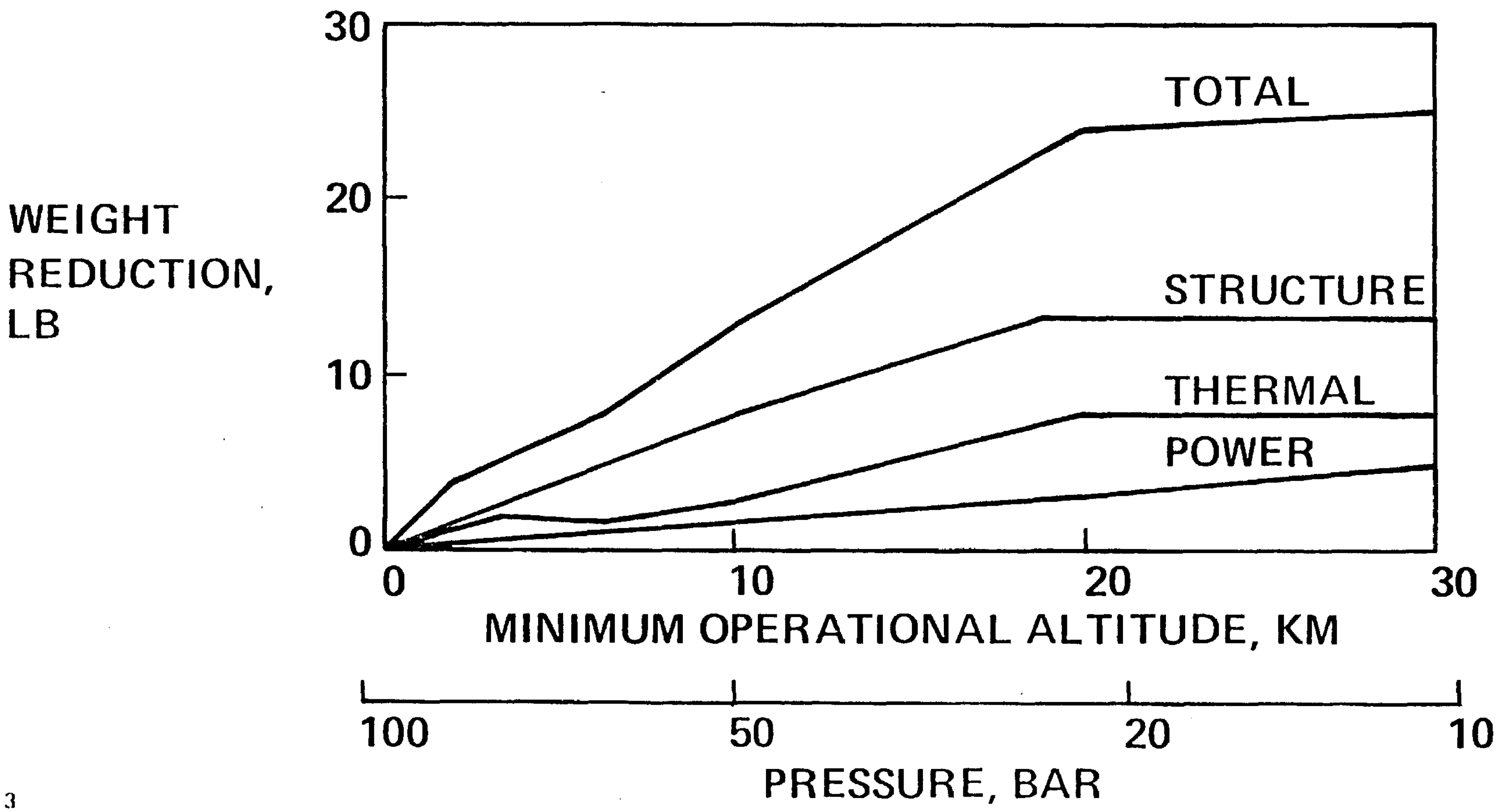

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FIGURE 4-30

(INAUDIBLE QUESTION)

MR. NOLTE: The question is how sulphuric acid-proof is the parachute. That really depends on the abundance of the acid. Although the parachute is not acid-proof, the sulphuric acid content of Venus atmosphere is probably less than that of Earth in some locales. This is a design problem which is shared by every exposed component.

UNIDENTIFIED SPEAKER: Are any of the probes sterilized?

MR. NOLTE: None of them are sterilized.

MR. SEIFF: Is it atmospheric attenuation that forces the communication bit rate down from 64 to 16?

MR. NOLTE: Yes

MR. SEIFF: Is it pure absorption of what?

MR. NOLTE: Yes, it is absorption.

MR. CANNING: Sixteen bits per second is also adequate.

MR. NOLTE: Adequate in terms of bits of data per kilometer because you are going so slow, obviously.

MR. SEIFF: You can live with it?

MR. NOLTE: Yes.

MR. CANNING: I would now like to introduce Mr. Kane Casani; Mr. Casani will speak on the subject of "Probe Interface Design Considerations." Mr. Casani is the Section Manager of the Spacecraft System Design and Integration Section of the Jet Propulsion Laboratory. He has participated in the design of many of the Mariner Spacecraft and over the past ten years has been actively involved in every capsule or probe design activity conducted at the Jet Propulsion Laboratory.

PROBE INTERFACE DESIGN CONSIDERATION

E. KANE CASANI
Jet Propulsion Laboratory

The subject of my talk is "Probe Interface Design Considerations," a rather nebulous subject. Before I get into the subject, I would like to discuss some of the soul searching that I went through in coming up with this presentation. I think maybe I handled it the right way. Of course, when one first thinks about the interfaces between a probe and a spacecraft, the immediate thing that comes to mind is the technical considerations that are involved. I have done considerable work in both probe design and interfacing of probes to spacecraft; my original approach to this presentation dealt with the technical aspect of the interface. After some initial work on the subject, I realized that my approach was altogether wrong. At that point, I sat back and reflected on some of the designs with which I have been involved over the past ten years. My thoughts went back to the early Mariner design, which some of you in the room may remember, at that time we were designing probes of the Discoverer shape for entry into an 80 milibar Mars atmosphere; I thought of many subsequent designs and up through the current designs we have done where we have looked most recently at the interfacing of this Ames probe to a Mariner Spacecraft. In the process of this historical thinking, I isolated what I think are three aspects of that interface design which are worth talking about today.

- Management
- Mission
- Technical

Those three aspects are: first, the management interface; secondly, the mission design interface which I feel, on this particular mission, the outer planet missions, will be more difficult than anything we have ever dealt with previously; and finally, some of the technical considerations which we have heard about today. I will talk in general about those as we move on.

Let me now address the management considerations.

- o Center Responsibility
- o Science Inputs

Two of the most significant considerations are, first of all, the center responsibility. We have designed missions where we have had both the responsibility for the project, the probe and the spacecraft assumed to be at one center; we have also designed missions where the responsibility for the project and the responsibility for the probe is at one center while the responsibility for the spacecraft is at another center. The distribution of these responsibilities is going to be a major influence in the way we go about designing the interface and handling the technical considerations. It is important that before we progress too far into the technical design decisions, that we are sure we understand the management relationship between the participating centers.

The other point, of course, which will be important is how we organize to get the science inputs into the design.

I think that the current MJU Science Advisory Committee which is chaired by Dr. Van Allen has been very influential in our technical thinking. And when we move into a project, it is going to be of paramount importance that we continue this type of activiy and that we maintain a good working relationship between the scientific community and the actual technical implementation of the project.

I reflected a little bit on Dr. Rasool's comment earlier today when he attributed the high success rate of the planetary exploration to the fact that we do have such a closeknit interaction between the science and the engineering aspects of a project.

I will now move on to the next subject, I would like to touch on some of the considerations of the <u>mission design</u>.

- o Organization
- o Flyby vs Probe
- o Relay Link Design

We have seen today some specific technical presentations which have shown some point designs for specific missions. I don't think that we have come anywhere near scratching the surface of the complexity of this mission design. I think that first we have to address ourselves properly to make sure that we do come up with a mission design team in a management sense, which is properly represented by both the people who are designing the spacecraft as well as the people who are designing the probe, and as well, a good way to get the science input into the design.

Two further aspects of importance are the flyby versus the probe trade-off and the relay link design.

If we look at the flyby versus the probe question, there has always been, and I am sure there is going to be even more, a difficult decision making process in determining whether the priority should be put into the probe mission or whether the priority should be put into the flyby mission. There is definitely going to be a conflict of interest in what those two mission designs are going to require. And from time to time we have attempted to say, "Well, why don't we just forget about the flyby mission because we are doing other flyby missions and minimize the flyby requirements and optimize the probe mission." Now that may be the easier way out but I don't think it will yield, necessarily, the overall optimum design or the most return for the investment. The most return for the investment is going to be a design which is optimized and adequately considers inputs on both of those two, what I look at as conflicting flyby geometry.

The relay link design is another interesting consideration. At first blush we would tend to think that the relay link design is merely a communications design problem where we are looking at optimizing the parameters involved in the link design, which are the antenna geometry on the spacecraft, the antenna geometry on the capsule, the caracteristics of the range, range rate, range accelerations, and the look angles between the spacecraft and the bus. But that is really an oversimplification of what is actually involved. I think a few of the papers today touched on bits and pieces of that. In particular, I draw your attention to the presentation that was made by Mr. Hyde where he showed flight time as a function of flyby altitude at the planet versus injected weight. Well, that ties immediately into some considerations that were shown previously where we were trying to optimize the relay link geometry for a certain flyby altitude. It now becomes apparent that the relay link flyby altitude is really tied into the flight time as well as to the injected mass and when we consider two-planet flyby mission, then the flyby altitude at the first planet is going to determine what we can do at the second planet. So what was originally just a simple consideration of the link design has some overriding considerations in not only the launch vehicle capability and the flight time but also the subsequent planet mission performance capability.

I think that this interaction is going to be much more than what we have seen on any previous mission. The Viking mission has a rather interactive mission, spacecraft, capsule aspect, but I don't think it is anywhere near as complicated as what we are looking at here.

Moving on to some considerations relative to the technical design, which by no means is the simplist, but I feel possibly one which we have done enough work that we at least understand what are the real problems.

- Relay
- Data Handling
- Power
- Thermal Control
- Guidance and Control

The relay is going to be one of the overriding considerations in this spacecraft probe interface.

One of the things that we have been discussing in this Mariner-Jupiter mission with Ames is how the responsibility of that design should be divided among the participating centers. At first glance, it would seem that possibly the simplest thing to do would be to have one center provide all of the equipment that is on the probe and the other center all of the equipment that is on the spacecraft.

Well, if you pursue that line of discussion a little further, it turns out that the interaction between the receiver and the transmitter is such that both of those pieces of equipment should be designed and supplied by one center, and that the interaction between the antenna and the spacecraft is such that the antenna should be an integral design of the spacecraft. You then come out with a distribution of hardware which is not what your initial intuition might make you feel is the right thing to do. But in overall sense, it may be the better way to implement that design. I am not suggestiong that this is the proper solution, but only that the solution is tied tightly to the management arrangement of which I spoke earlier.

Data handling: This topic has been touched on by several of the previous speakers. We have looked at this problem in a general sense and feel that the ability on board the spacecraft to handle the data that the probe generates is going to be rather straight forward compared to the kinds of data handling that we are used to doing on the current Mariner class spacecraft.

Power: This interface is one that is rather interesting because on one hand we look at minimizing the overall cost of the project and say, "Well, the way to do that is to use as much of the equipment that is on board the spacecraft to service the probe." That is, for example, to have the capability to do the battery charging on the spacecraft as opposed to on the capsule. While such arrangement could be made, it isn't necessarily obvious that it is the best arrangement in an overall sense because we have turned around and made a more complicated interface between the spacecraft and the probe. And we have also designed a probe which can't be, by itself, tested in terms of its capability to charge its own batteries until it meets up with a spacecraft, which puts us in an untenable position that there could be a fundamental design problem that doesn't get disclosed until later in the program; whereas if the battery charger were part of the probe system, then the interface between those two elements would be checked out earlier in the design. I cite that as a subtle example of the kinds of technical problems that we can get into if we don't understand these things that I talked about previously.

Thermal Control: This is going to be another interesting design interface because the probe is going to have to be considered a major part of the spacecraft in the overall thermal design of the spacecraft. It won't be a simple appendage that is not going to interact with the spacecraft design. And I really don't have a good feel for the exact way in which that problem is going to be handled. We have had several discussions on this. And other than saying we see it as an area that is going to require significant attention early in the design, I don't feel that we have given this one as much attention as it deserves.

Guidance and Control: We have looked at this interface and it appears to be rather straightforward, particularly in our ability to satisfy the probe requirements on the delivery accuracy, zero entry angle of attack and spinning the probe on the spacecraft. We have looked at specific designs where, as far as the probe is concerned, the interface to Mariner is identical to Pioneer.

In summary, I would like to say that in having thought through these considerations, that they are much farther reaching than the simple technical interface but that I believe that a continual cooperative effort between the science and engineering aspects of the design, in addition to the proper management attention early, is going to make this a certainly doable interface design.

PROBE DESIGN

W. Cowan
McDonnell-Douglas Astronautics Company

MR. W. COWAN: We have been wrestling now for some months with ARC on the problem of outer planet probe designs. And we have come to some feelings and convictions, as we have gone through this process about these outer planet probes. One of these is that the technology today will support these early missions (c.f. Figure 4-31).

We also feel that there is a high degree of commonality across these missions. This doesn't necessarily mean the commonality of absolute identicality, but a commonality which really leads to the cost-reduction we have been seeking; one which allows you to take the technology that you have and apply it. This kind of commonality keeps the cost down because you minimize the money spent on new developments.

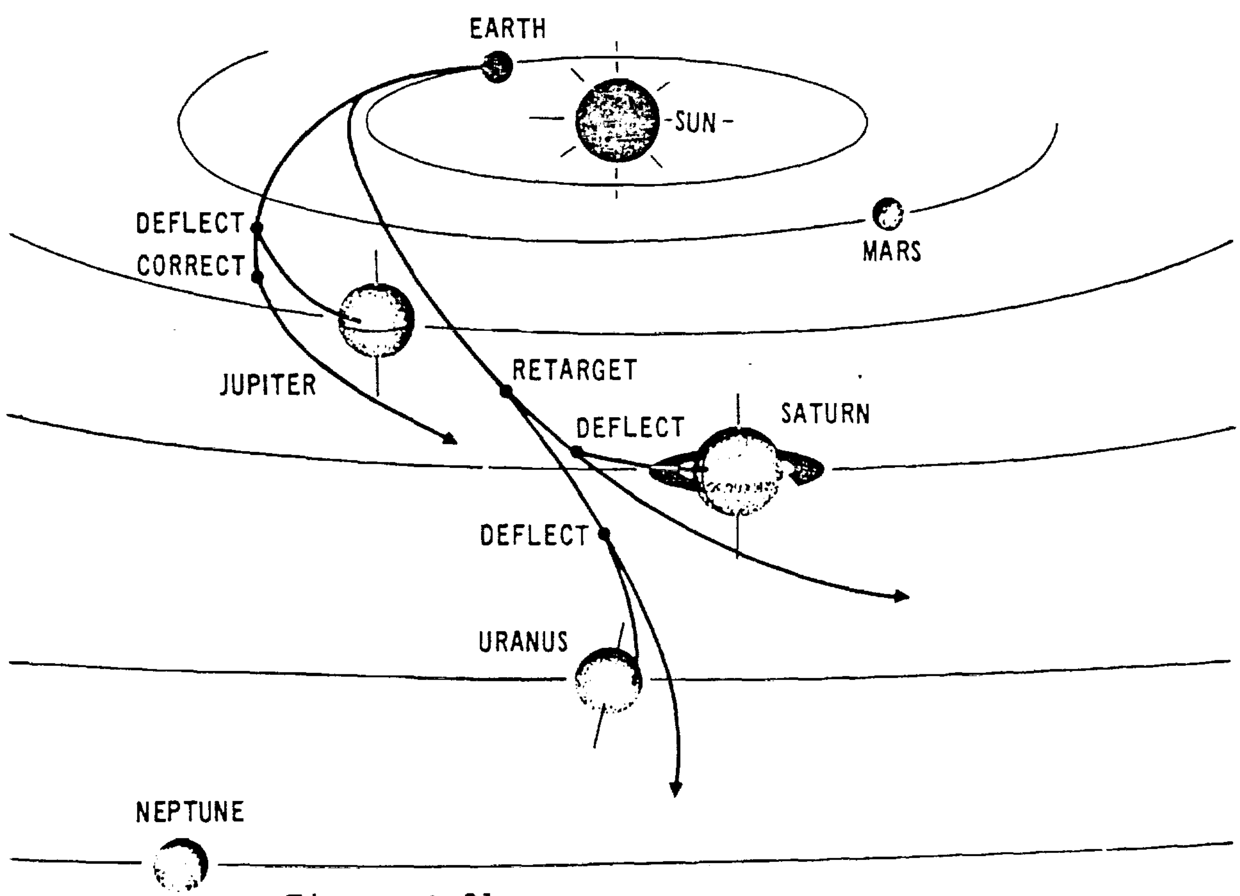


Figure 4-31. Mission Characteristics

And, more recently, because of the confidence that has come from the Pioneer 10 data, we are getting a conviction that an early mission to Jupiter is feasible with what we know today and the materials that we have available.

I would like to take just a few minutes to identify the highlights of this design that has been studied for almost two years. It is a probe design that started out being studied for Saturn-Uranus application; the probe is 35 inches in diameter; it varies in weight from 200 pounds to 350 pounds, depending on the size of the heat shield that is on it, the planet to which it is going and whether it does or does not have planetary quarantine. Basically, it is the same probe used across the several missions that we have looked at for Saturn, Uranus, and/or Jupiter.

Figure 4-32 presents the features of the design. The aft end of the probe has a hemispherical yeat shield after body and proceeding forward we have the equipment cover with its microstrip flat plate antenna, the 66 degree antenna that was described earlier and will be discussed some more tomorrow. The principal feature of the probe design is that everything is packaged far forward. So the CG is far forward, and the probe is then inherently stable, and does not require a parachute or any other separating parts and pieces. This feature supports the goal of achieving the maximum reliability, minimizing complexity, and cost.

The probe was designed as a ten bar probe, however, this vehicle is capable of reaching the 30 bar level or below for Jupiter. I would like to show you one other central feature of this design which Howard Myers talked about this morning and that is the mass spectrometer, which is a central element in the whole probe. The mass spectrometer was designed for a 500 cubic inch volume analyzer section, either quadropole or magnetic deflection and it has an extendable inlet mechanism. The data handling portions of the mass spectrometer are located within it.

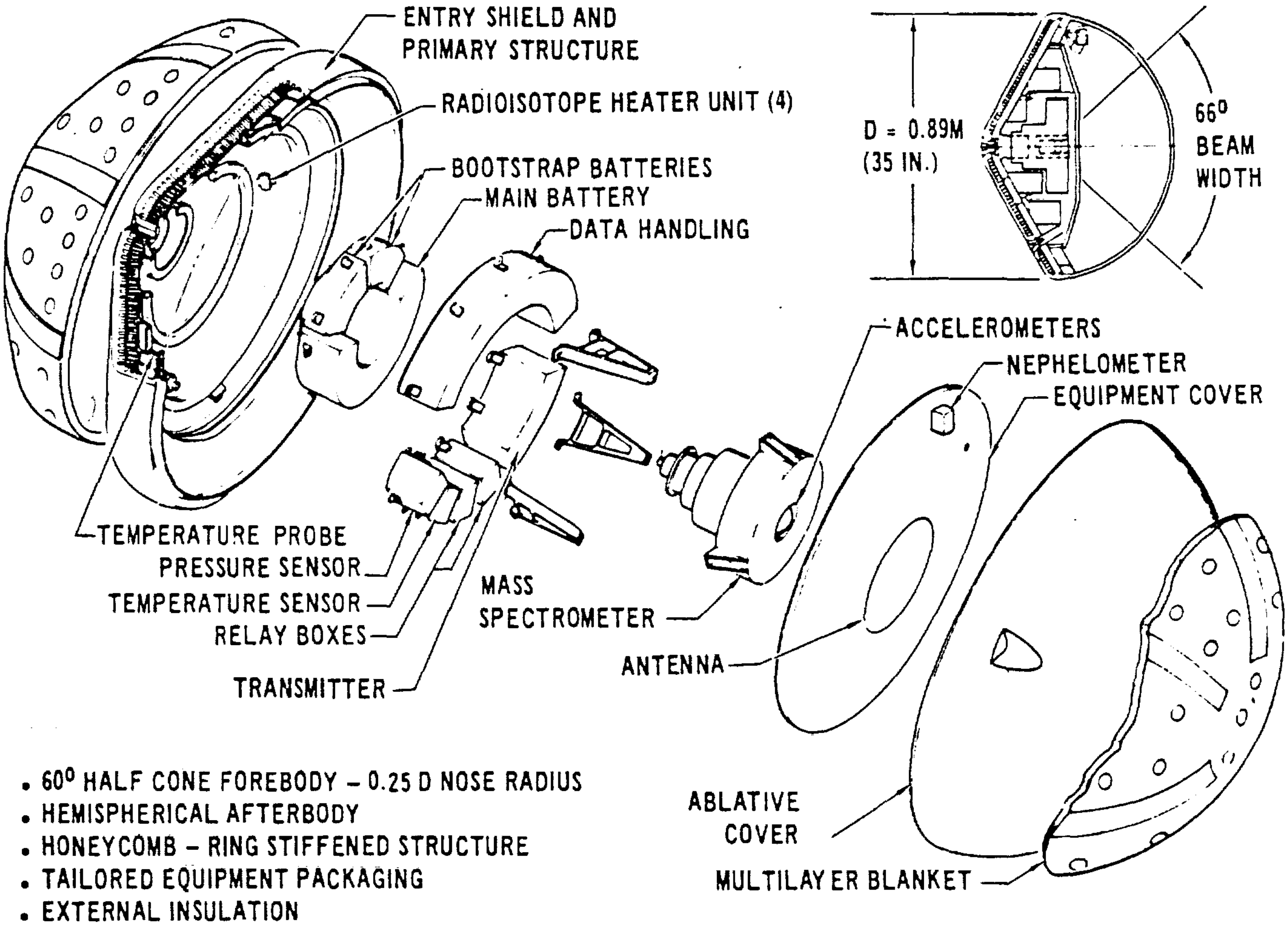


Figure 4-32. Probe Configuration

The probe has an aluminum ring frame structure, a fiberglass honeycomb, a carbon phenolic heat shield, and were all designed around the mass spectrometer as a central structural element. You will notice the accelerometer is mounted inside the mass spectrometer instrument package; placing it on the CG. The batteries are toroidal, trapezoidal batteries. These data handling segments are shown. Throughout the entire flight profile, the CG remains forward.

You will see some pictures in Bill Kessler's presentation tomorrow of the vehicle flying in the ballistics range here at

Ames, and he will have some other data on that and can answer other questions.

The usefulness of this probe, the value benefit of this probe, is related to its ability to do missions at planets; can it be carried by spacecraft that exist or are about to exist? The probe has been designed to be compatible in general either with Pioneer or with Mariner. As was pointed out earlier today, the delivery mode is one in which the spacecraft points the probe at the aim point and then deflects itself and continues with the mission. It also is a relay communication system. The spacecraft maintains Earth lock throughout the entire active portion of the mission and relays the data back. (cf. Figure 4-33)

We have options of swingby and retargeting, and the three principal planets we have looked at are shown on Figure 4-31. We have also taken a cursory look at several of the satellites, and have a small amount of data on Titan.

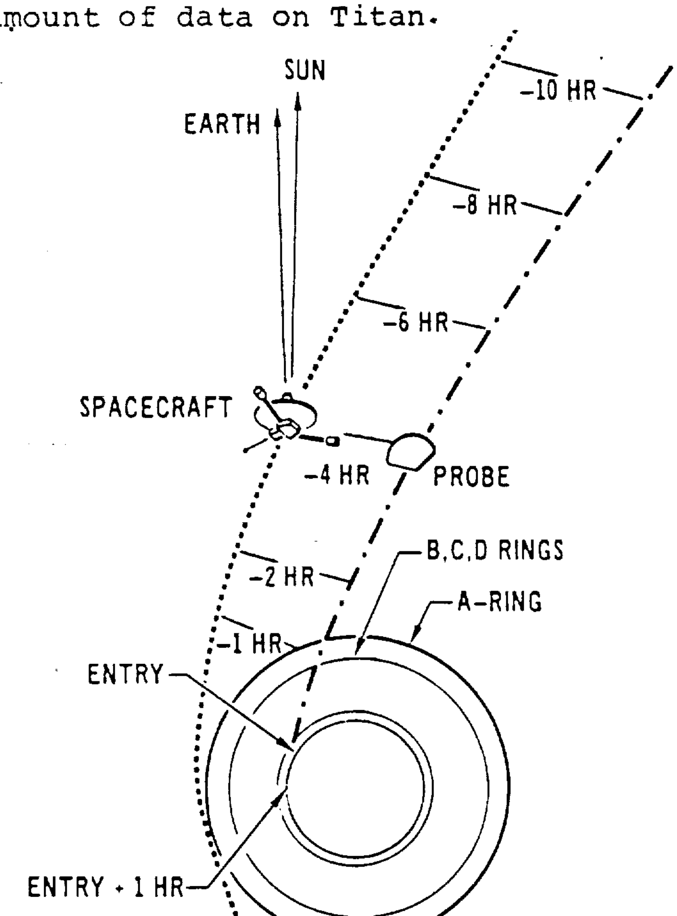


Figure 4-33. Planetary Arrival

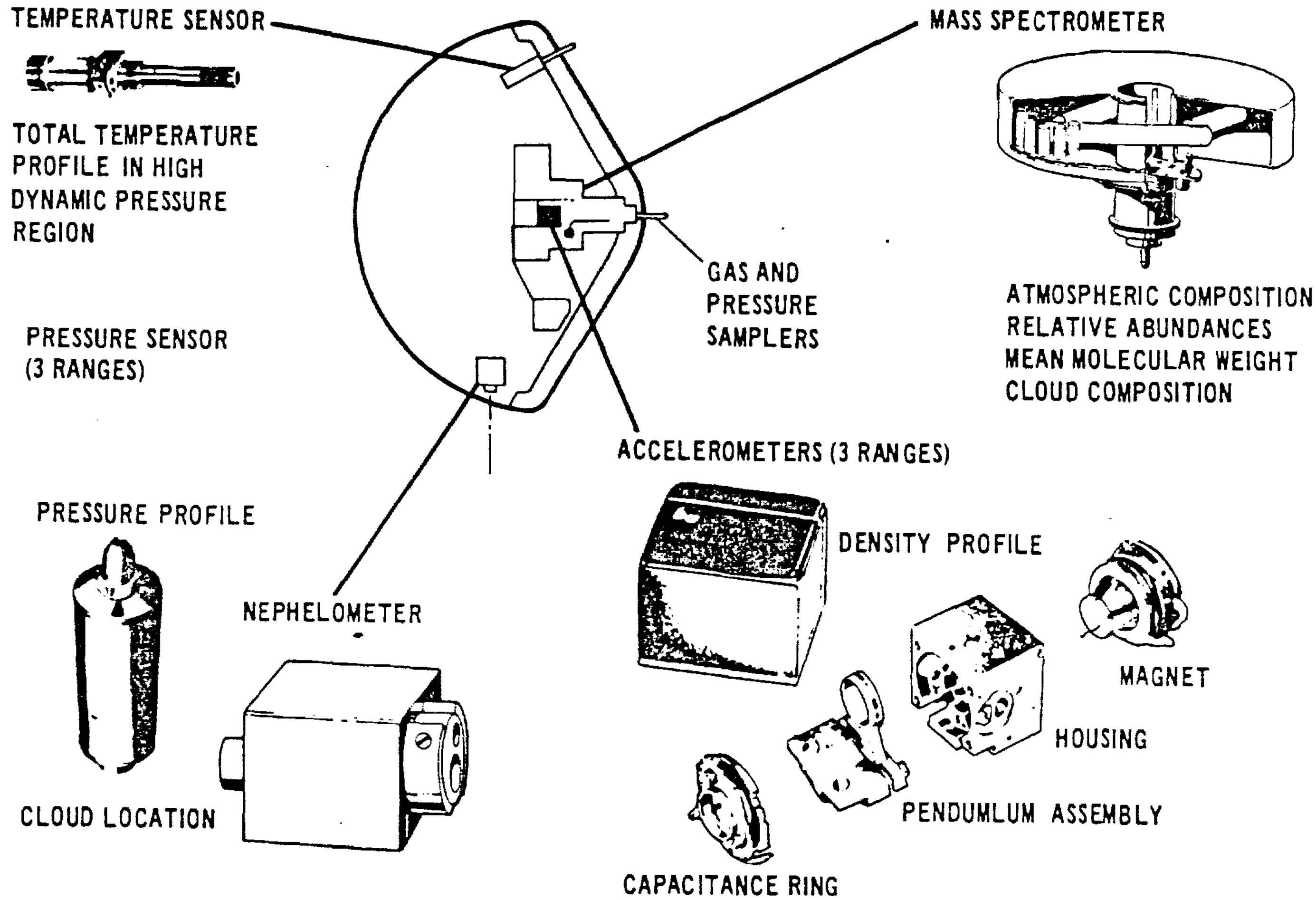
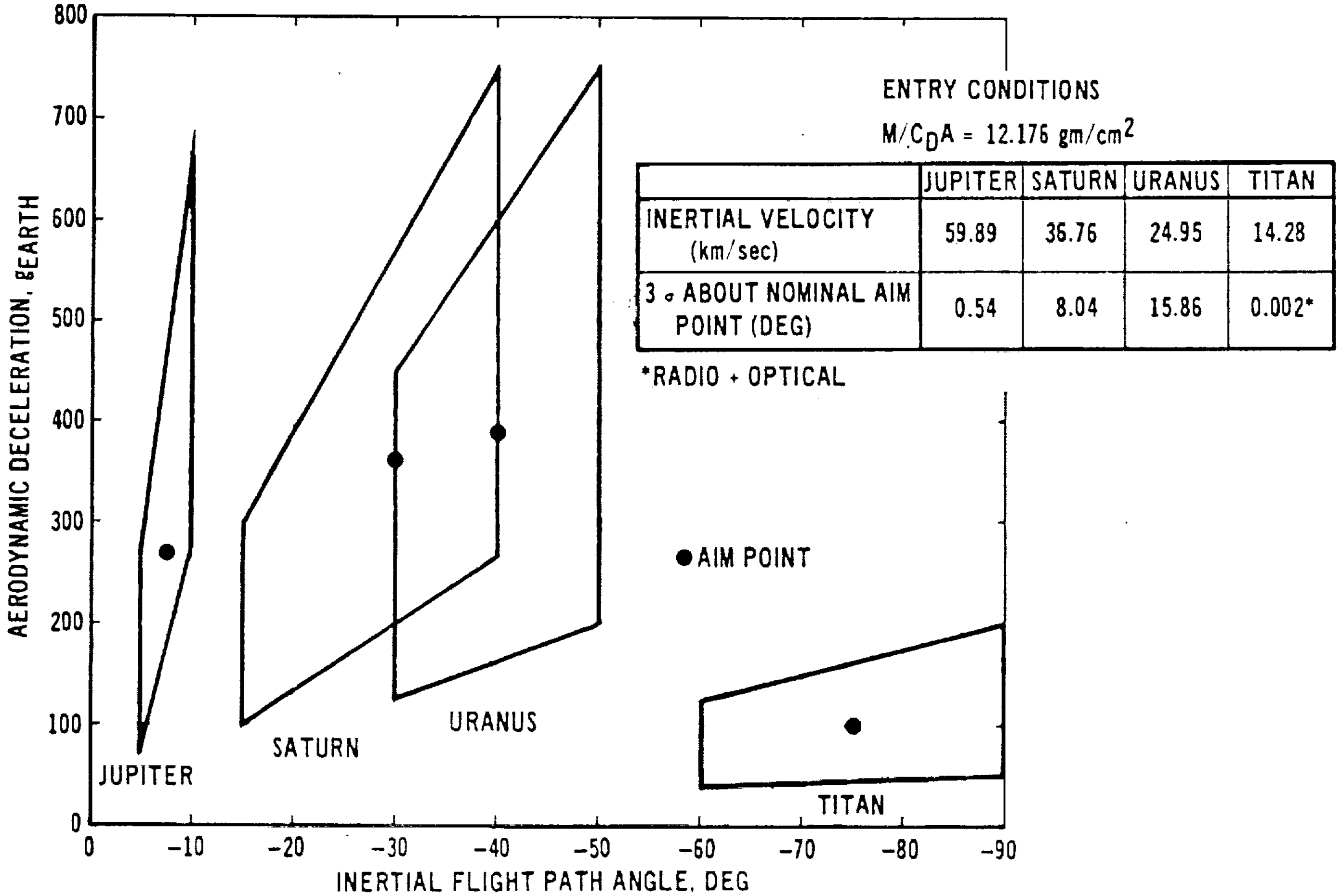


Figure 4-34. Science Payload

The instruments we have identified are shown on Figure 4-34. It is perhaps slightly more than a minimum package. A minimum package might be just the first three instruments, accelerometer, temperature, and pressure measurements; but as a basic package, if you add the mass spectrometer, the nephelometer and perhaps some other candidates, such as the IR radiometer or the gas chromatograph. There is some capability to put some other instruments on board, depending on the weight constraints that you would have. Shown on the figure is the basic package that was looked at. These instruments, either exist or are expected to exist, ready to go, without a lot of new development, by the time an outer planets probe is launched.

The active life for the probe is very short. It is passive throughout most of the mission. Carried on the spacecraft for most of the mission total time, it is released from three to seven weeks before planetary encounter depending to which planet you are going. It coasts along on its own; has a multilayer insulation blanket around it as shown on the exploded view. Shortly before it enters the sensible, high altitude atmosphere, it is activated, it then has an active period during the entry that could extend up to about an hour. As you saw from the phasing curves this morning, in an attempt to maximize the certainty of communications you try to maximize the relationship of the flyover geometrys, the choice of frequencies; and in terms of the constraints of the electronics. As Carl said this morning we are working with a 40-watt solid state transmitter. We did this deliberately because that represents a threshold in knowledge.

Now what else affects design? Certainly, the kind of environment that a probe is going to find itself in is a principal driving force. I have reflected this on Figure 4-35 in deceleration terms. I have reflected it principally for the three planets. These general comments also relate to the heating environment as well. The kind of variation you see on the figure is reflected in the heat shield thickness. Tomorrow there is going to be further discussion on the specific sizing of the heat shields, although I will show you a weight statement in just a few minutes. But notice that as the angles get steeper, as the atmospheres go from warm to more dense, and the boundaries shown represent the extremes of the NASA SP defined atmospheres, the extremes of the potential design conditions go up. The probe was designed originally for 800 G's, with a thousand G ultimate, for the Saturn-Uranus application. It was designed at a time when it was thought that the Uranus, and this was for a Pioneer case at that time, entry angle uncertainty might be as much as 15 degrees. Therefore, if you were to aim at a box in this area, you would



	JUPITER	SATURN	URANUS	TITAN
INERTIAL VELOCITY (km/sec)	59.89	36.76	24.95	14.28
3σ ABOUT NOMINAL AIM POINT (DEG)	0.54	8.04	15.86	0.002*

Figure 4-35. Entry Deceleration Envelopes

be just within the bands. There is always some dilemma here in terms of selection of design criteria so that you don't make them so overly conservative that you drive your design off scale and run your costs up, in a situation which implies a non-feasibility to do the task that can really be done.

So what we are seeing here is that as you are able to resolve your uncertainties in either atmosphere and/or the angle to which you can aim, then you can resolve uncertainties and your design margins can go up. This particular probe, is designed to the 800G level, and you see on the figure, from a G standpoint for flat entry angles near grazing at Jupiter, the G load problem essentially goes away.

Figure 4-36. Mass Properties

SUBSYSTEM	SATURN/URANUS WEIGHT (LB)	JUPITER WEIGHT (LB)
STRUCTURE	28.9	28.8
HEAT SHIELDS	81.6	182.0
HEATERS & INSULATION	15.3	15.3
COMMUNICATIONS & DATA HANDLING	21.1	21.1
ELECTRICAL POWER	20.0	20.4
PYROTECHNICS	8.2	8.2
SCIENCE PAYLOAD	24.2	24.2
INSTRUMENTATION	1.6	1.6
PLANETARY QUARANTINE	16.3	16.3
WEIGHT MARGIN (10%)	21.7	32.0
PROBE WEIGHT	238.9	349.9
LESS: BIOSHIELD	-11.6	-11.6
INTERFACE WIRING	-2.3	- 2.3
EXTERNAL INSULATION	-5.9	-5.9
AT ENTRY	219.1	330.1
LESS: ABLATED MATERIAL	-19.0	-123.3
END OF MISSION	200.1	206.8
C.G. & INERTIAS AT ENTRY		
X AXIS C.G. (IN.)	8.62	7.96
I_X (ROLL) (SLUG - FT^2)	5.61	9.82
I_Y (PITCH) (SLUG - FT^2)	3.63	5.97
I_Z (YAW) (SLUG - FT^2)	3.52	5.86

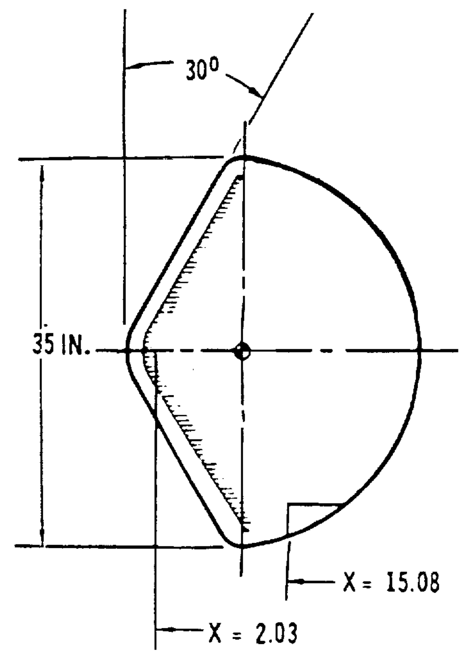
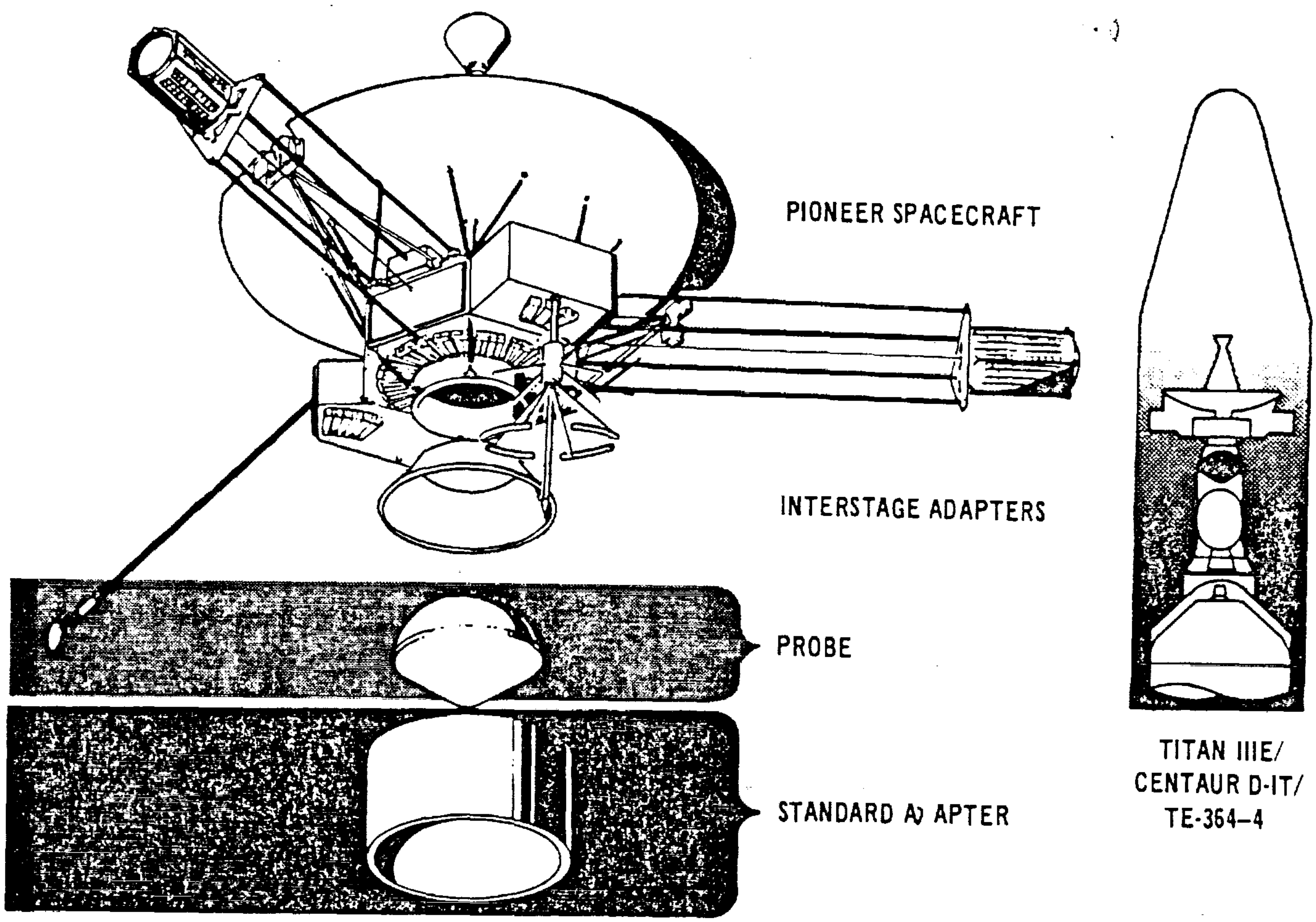


Figure 4-36 presents the weight story for Saturn-Uranus broken down by subsystem, leading to a total weight of around 250 pounds. And for a Jupiter prole at seven and a half degree entry, around 350 pounds. Both of these are with planetary quarantine.

The essential difference between these two is in the heat shield weight. As Sam will show you tomorrow, the carbon phenolic heat shield thickness varies from approximately two inches for the Saturn-Uranus case to three inches for the Jupiter case.

As far as the probe is concerned for the Jupiter mission, there is no other change except a slight rounding of the aluminum structure to provide for the extra carbon phenolic material as it rounds the corner. The probe itself is at the same external diameter. There are perhaps one or two small scale changes on the instruments. Fundamentally, the design is one that is common and almost has identicality in most aspects and, therefore, costs and development and all can be minimized for this set of instruments.

Figure 4-37. Launch Vehicle and Spacecraft Interface



We have shown on Figure 4-37 for illustrative purposes the probe on a Pioneer spacecraft. I would like to reiterate that these early missions, although we see them going on Titan IIIE Centaur, it is anticipated, as time goes on, the shuttle will become available and that there may be applicability of these probes on these and similar spacecraft for those kinds of missions. But for the present, we are planning for the Titan launch vehicle and either the Mariner or the Pioneer spacecraft. Because the probe is essentially an autonomous, passive device, except for minimal transfer of electrical power during the coast phase and minimal attachment and heat interface support, it should then be compatible with either of the two spacecraft.

MR. CANNING: There was a question on spinning, and the answer was that the system is spinning at five RPM.

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PROBE DESIGN AND SYSTEM INTEGRATION

P. Carroll
Martin-Marietta Corporation

MR. CARROLL: I shall discuss a recent contract that Martin-Marietta has had to study the adaptability of existing hardware systems to a Pioneer Saturn/Uranus probe.

A previous speaker has charged the people who are designing for advanced probes to the outer planets, to start thinking about reduced cost. And part of the objective of this study under contract to Ames was to look at just that. What can we do in the way of using existing hardware to reduce program cost?

Figure 4-38 depicts past and current activities of Martin-Marietta and is representative of the type of activities that the whole industry under NASA and JPL sponsorship has been conducting through the last eight years or so.

The early efforts in 1967 and 1968 did bring up the point that it is very difficult to design an engineering system without established and consistent criteria from the scientists. And in those early days, scientists' opinions were varied. It was difficult to design an engineering system because of the large variation in criteria for design.

One of the first attempts, the Venus multiprobe study which was done for JPL, was a rather extensive trade study to assess the value of each of the science instruments and to determine the cost to implement them. As you can see, various approaches were taken. There were at that time both small and large probes. There were balloon systems as well as very high altitude probes designed to obtain data above the clouds.

CHRONOLOGY OF PROBE PROGRAM DEVELOPMENT AT MARTIN MARIETTA

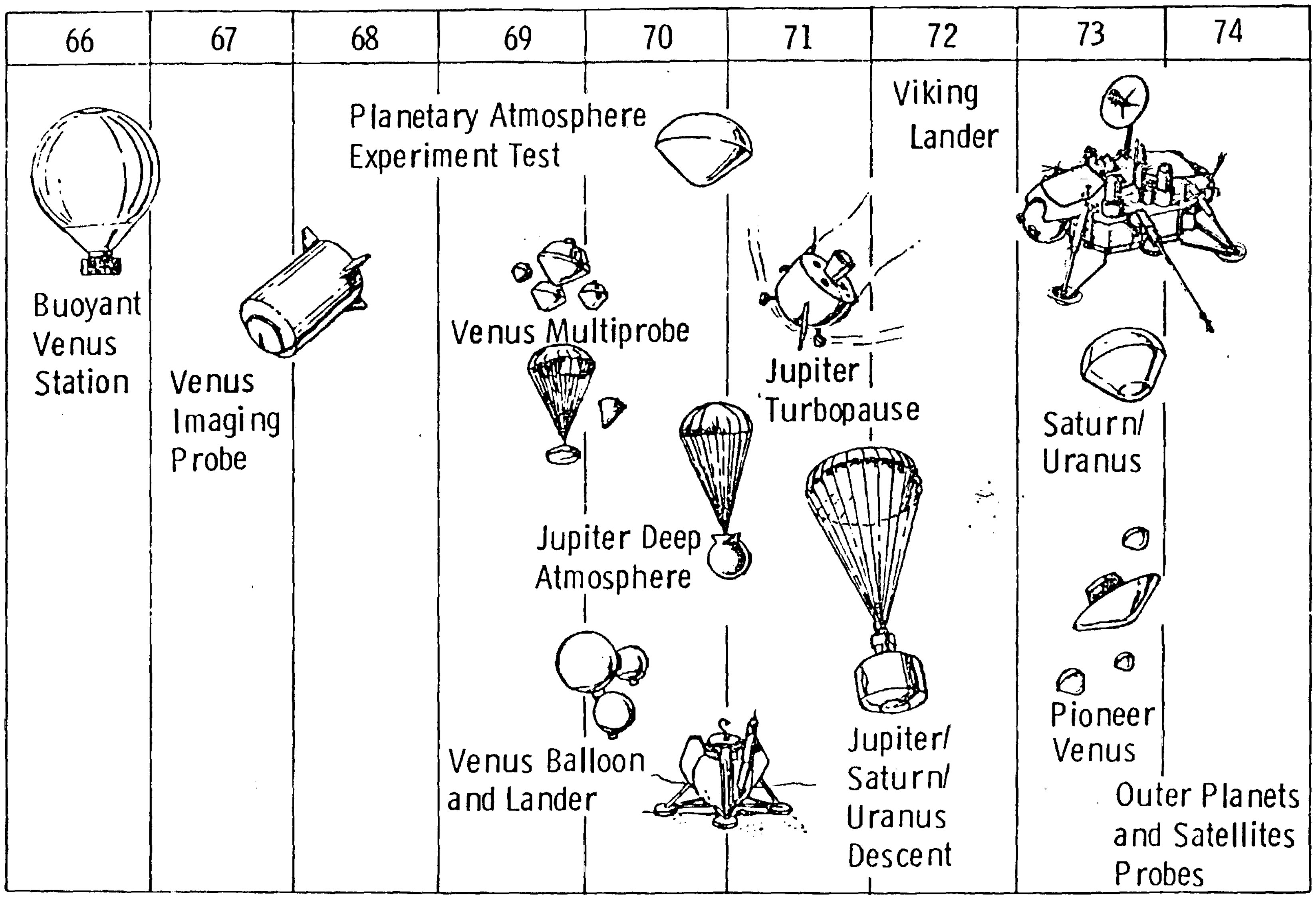


FIGURE 4-38

Those efforts led into the Jupiter deep atmosphere probe studies for JPL. These probe designs went down to 1000 atmospheres pressure; and at that time it was becoming obvious that the cost of descending to 1000 atmospheres pressure within the temperature environment was so great that the scientists then were willing to back off to what they then felt were adequate science criteria, somewhere around ten to thirty bars.

During that time, because of the difficulty and risk of heat shield development, Goddard came up with the concept of a Jupiter turbopause probe. It was a backup position in case it would be difficult or impossible within the budgets to develop heat shields for entry into Jupiter. There was a possibility that one could determine some of the basic science by just skimming into the upper atmosphere. That probe was not required to survive entry, however, the uncertainties in determining survival down to the turbopause where the composition could be measured were quite large. So that idea has been dropped from further consideration.

In addition, JPL looked at other approaches and finally these efforts did lead into Jupiter-Saturn-Uranus concepts of commonality. These efforts then led to the most recent Ames contracts to evaluate and design Saturn-Uranus probe systems.

Of course, Langley was active in much of this early work and the current Viking program, provides us with a comparison of the very sophisticated vehicle, with very sophisticated science, and high cost against our more cost constrained probe design. I think the trends we have talked about are leading to less costly systems with reasonable and adequate science.

Figure 4-39 depicts a configuration that resulted from our studies; although in detail the configuration is a little different from those of some of the other studies, in principle it is similar. We did look at all of the subsystems and assess the

SATURN/URANUS PROBE CONFIGURATION

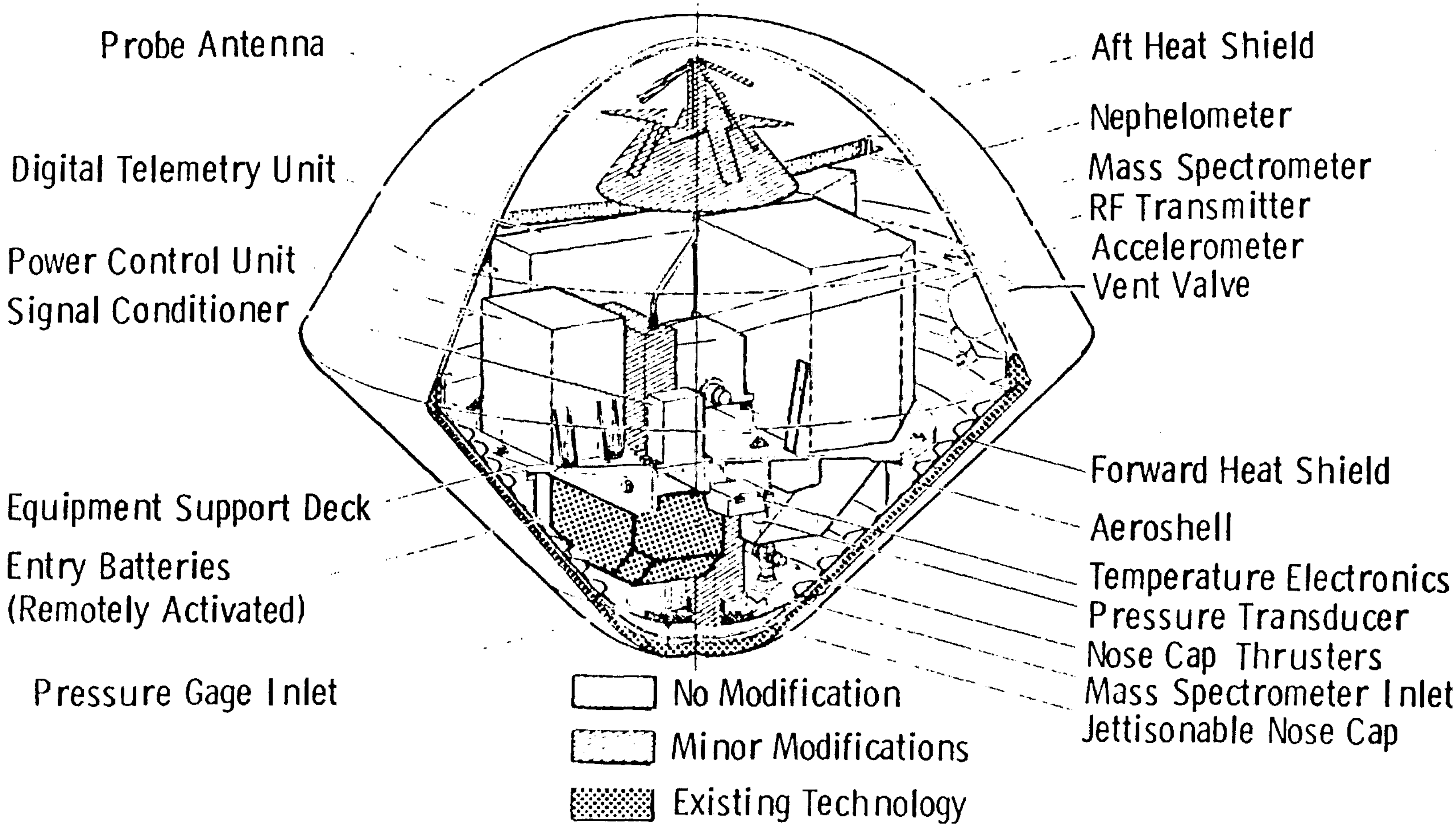


FIGURE 4-39

possibility of using off-the-shelf or existing hardware; or in the case of science, the hardware that is being developed and specified for the Pioneer-Venus program.

The two items that are not existing hardware are the heat shield and the batteries. For a Uranus probe mission with a seven year duration, you will need remotely activated batteries.

There has been much discussion today about heat shield technology. It does appear that the carbon phenolic type heat shield may be sufficient for the Uranus probe design. The development of this heat shield in the Pioneer-Venus program will provide design technology for the Uranus probe. Hopefully, if some of the uncertainties in the Uranus atmosphere are reduced further, then possibly even more efficient heat shield materials might be sufficient. We have looked at quartz nitrile phenolic heat shield material and it may be a possible candidate.

Most of the general communications type hardware with some modification, can be used directly in the Uranus probe design.

Figure 4-40 presents a summary of science equipment adaptable to a Saturn-Uranus probe. I won't dwell on all of the points, but we did evaluate the specified science for the Pioneer-Venus program. I might say that with no modification or minor modification, you would have to requalify the system for the higher G loads. The design G-level remains to be seen, but is generally going from, say 400 to 600 G's for requalification.

The accelerometers would require modification for greater range because of the higher G's; temperature and pressure essentially can be used as is. The upper range on the pressure scale would not be required.

EXISTING HARDWARE ADAPTABILITY TO PIONEER S/U PROBE

INSTRUMENT	SOURCE	REQUIRED MODIFICATION
ACCELEROMETER TRIAD	PV, LARGE PROBE	MODIFICATION FOR RANGE
TEMPERATURE GAUGE	PV, EITHER PROBE	MODIFICATION FOR RANGE
PRESSURE GAUGE	PV, EITHER PROBE	NO MODIFICATION; EXCESS RANGE CAPABILITY
NEPHELOMETER	PV, EITHER PROBE	NO MODIFICATION
NEUTRAL MASS SPECTROMETER	PV, LARGE PROBE	MASS RANGE MODIFICATION INLET LEAK REPLACEMENT. OUTGASSING VENT TUBE. OTHER SOURCES CONSIDERED.

FIGURE 4-40

MARTIN MARIETTA

The nephelometer requires no change. The mass spectrometer does present some specific problems. Because of the different atmospheric environment, one would have to change the inlet leak size. For a seven-year period, the outgassing problems just within the instrument would require some sort of venting to obtain the initial vacuum so that the ion pumps would activate. An approach we considered was simply a vent tube that could be opened prior to entry and then sealed off to clear out the ion pump section.

It is more difficult to measure the helium, and a little higher voltage is required to ionize the gas. So there are enough modifications to the mass spectrometer that it is reasonable to consider some other sources; and there are a couple of other mass spectrometers that could be used.

The major modifications are the inlets, the addition of better pumps, and the increased voltage to the ion pump.

Figure 4-41 presents the availability of electrical/electronic components. The main item I want to point out here, is the battery system. As can be seen, we considered various hardware programs that use the typical type of equipment that will do the job for the Uranus probe. However, the battery is a new design and build; and, again, you do need to use a remote activation type battery.

As far as the G loading is concerned, Martin has tested batteries up to 750 G's under electrical load with no ill effects.

We chose a Viking type antenna which required modification to accommodate the frequency change.

Figure 4-42 presents structural/mechanical component availability. The most significant item here is the heat shield design. It would require a new design and build. However, by using the carbon phenolics, it will be based on existing technol-

HARDWARE AVAILABILITY FOR ELECTRICAL/ELECTRONIC COMPONENTS

ELECTRICAL & ELECTRONIC DESIGN	SOURCE	REQUIRED MODIFICATION
DATA		
DTU	PIONEER F & G	MODIFICATION TO REMOVE REDUNDANCY, CHANGE ROM's AND ADD COAST TIMER.
SIGNAL CONDITIONER	PIONEER F & G	NO CHANGE
POWER		
PCU	MARINER	MODIFY CONTROL CIRCUITS AND UPDATE UNIJUNCTION TRANSISTORS. MODIFY WIRING TO ADD G SWITCH.
BATTERY	NEW DESIGN & BUILD	EXISTING TECHNOLOGY
COMMUNICATIONS		
TRANSMITTER	TELEDYNE	STANDARD UNIT MODIFIED FOR MODULATION CHANGE
ANTENNA	VIKING LANDER	MODIFIED FOR FREQUENCY CHANGE.

MARTIN MARIETTA

FIGURE 4-41

HARDWARE AVAILABILITY FOR STRUCTURAL/MECHANICAL COMPONENTS

MECHANICAL & STRUCTURAL DESIGN	SOURCE	REQUIRED MODIFICATIONS
HEAT SHIELD	NEW DESIGN & BUILD	EXISTING TECHNOLOGY
MECHANISMS		
CABLE CUTTER PIN PULLERS	TRW PROGRAMS	NO MODIFICATION
BALL-LOCK RELEASE PINS	TRW PROGRAMS & MINUTEMAN	NO MODIFICATION
PYRO THRUSTER	HI SHEAR	NO MODIFICATION
THERMAL CONTROL		
ISOTOPE HEATERS THERMAL BLANKET	PIONEER SPACECRAFT	NO MODIFICATION
FOAM INSULATION	SATURN II	NO MODIFICATION
NITROGEN GAS ASSEMBLY	NEW DESIGN & BUILD	EXISTING TECHNOLOGY

FIGURE 4-42

MARTIN MARIETTA

ogy. That might be a little optimistic in that earth reentry testing may still be required to qualify the heat shield material. Langley people have been talking of this test which would use a launch vehicle with upper staging and provide test data that more nearly fits the conditions that are required.

The other item, thermal control, includes components that were incorporated in our design and no new technology is involved.

The nitrogen gas assembly is a thermal control concept in which gas is released into the entry vehicle internal system during descent to keep out the atmospheric gases up a few bars of pressure. This subsystem is simply an engineering design-and-build effort.

Figure 4-43 summarizes our study conclusions: design of a common Saturn-Uranus probe is feasible and practical and this includes design for the extreme atmospheres of both planets. In the case of this study, with the Pioneer spacecraft, and by comparing item for item, it appears that approximately 85 percent of existing hardware can be used in the Uranus probe design. Now whether or not that is the best design remains to be seen. The only qualification to the 85 percent figure is that the components would have to be requalified for the higher G's and any unique temperature environment combination. However, based on discussions of atmospheric uncertainties at this meeting, it appears likely that the design entry G levels may be reduced from current requirements somewhat.

It can be expected that a reasonably low-cost program can be developed using this approach. In fact, it is necessary that we keep the cost down because of the constrained budgets of today. However, there are some things that should be done and should be done soon to enhance the mission reliability and further reduce the cost of these programs.

CONCLUSIONS

o DESIGN OF A COMMON SATURN/URANUS PROBE IS FEASIBLE AND PRACTICAL.

o PROBE HARDWARE COMMONALITY WITH EXISTING FLIGHT SYSTEMS CAN BE AS HIGH AS 85%.

o PROBE HARDWARE COMMONALITY CAN RESULT IN A LOW COST SATURN/URANUS PROBE PROGRAM.

o ADDITIONAL DEVELOPMENT EFFORTS:

1. HEAT SHIELD ANALYSIS AND TEST
2. REMOTELY ACTIVATED SILVER-ZINC BATTERIES
3. MASS SPECTROMETER INLET AND PUMPING SYSTEM
4. THERMAL INSULATION MATERIAL TESTS.
5. HIGH g PACKAGING CONCEPT TESTS.

FIGURE 4-43

MARTIN MARIETTA

These additional development efforts are listed. The first is the heat shield analysis and test. Additional analysis is required and the upgrading of the test facilities and the flight-type entry testing are certainly desirable, if not required. Again, the remotely activated silver-zinc type batteries for the seven-year mission duration for Uranus are required as well as the mass spec items that were discussed including the inlet and pumping systems. Thermal insulation materials should be investigated within the hydrogen-helium type environments, for applications where they may be exposed at the higher pressures. The environment would certainly tend to affect the thermal insulation characteristics. Finally, the high G packaging concepts proposed for this design should be tested.

SESSION V - ENTRY AERODYNAMICS AND HEATING

Dr. Walter Olstad, Chairman

NASA - Langley Research Center

MR. VOJVODICH: We are very fortunate in having Dr. Walter Olstad of Langley to chair the entry aerodynamics and heating panel. I am not going to go into Walt's background. He is well published in this area and without further delay, I will turn the proceedings over to Dr. Olstad.

DR. OLSTAD: Yesterday we heard some discussion about technology for the probes being pretty much in hand. Today we have some surprises for you. The technology isn't all that well in hand, and we have some genuine concerns about which you will be hearing today.

Before launching into the talks by the panel, I would like to give a brief overview of some of these problems.

Looking first at the problem of entry aerodynamics and heating, Table 5-1, we ask: What are we supposed to do? The first and obvious answer is to assure survival of a probe, which gets us into the heating problem. But, beyond that, mere survival of a probe isn't sufficient. It doesn't guarantee any data coming back; or if data does come back, it doesn't guarantee that you can interpret that data. So it is very important that we be able to predict performance and that performance be reliable.

Figure 5-1 presents some of the challenges to making predictions for a probe entering a severe environment. We always have the problem of transition from laminar flow to turbulent flow. And, as those of you who know anything about the transition problem are aware, the only way to learn about it is through experimentation. It is not something you can calculate. Unfortunately, our ground facilities don't provide the conditions that will be encountered during entry in the outer planets. And so, we have to extrapolate from experiments and ground facilities.

Furthermore, we must be able to predict the turbulent

ENTRY AERODYNAMICS AND HEATING PROBE REQUIREMENTS

- SURVIVAL
- RELIABILITY OF PERFORMANCE
- PREDICTABILITY OF PERFORMANCE

Table 5-1

TECHNICAL CHALLENGES

- TRANSITION
- TURBULENT HEAT TRANSFER
- RADIATION BLOCKAGE
- CHEMICAL STATE
- AFTERBODY HEAT TRANSFER
- ASYMMETRIC ABLATION
- REAL-GAS AERODYNAMICS

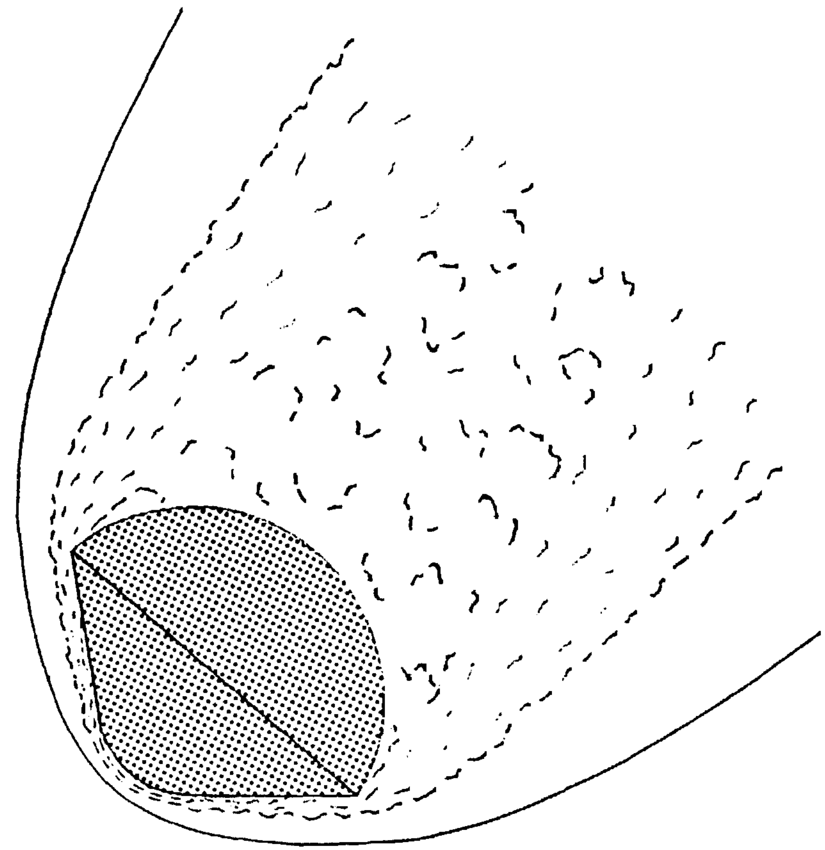


Figure 5-1

heating. Turbulent heating is also an area where empiricism is necessary. Once again, we have to extrapolate from ground facility experience, and that is a long and uncertain extrapolation.

The third area is one that I have labeled radiation blockage. The ablation products which are injected from the vehicle's surface tend to absorb some of the radiant energy from the shock layer. This is generally a beneficial effect which heats up those ablation products which are then swept into the wake. But we have a difficult time predicting how much absorption or blockage we get. One of the big problems is that we don't know the radiative properties of some of the heavy molecules which are constituents of the ablation products. Further, we don't know really what the chemical state of the ablation layer is. We don't know if it is in chemical equilibrium or not. That makes quite a difference in any calculation.

As you will hear a little later in this session, there is some question about the chemical state of the shock layer itself, and this, again, relies on experimentation. Fortunately, we can do a good bit of the necessary experiments in shock tubes.

Another problem area is that of afterbody heat transfer. Generally, it is not large enough to significantly affect the design of a probe but the greater confidence we have in predicting afterbody heating, the less will be the margin of safety we have to put into heat shield design and the more weight can be allotted toward increasing the science payload or enhancing system reliability.

Asymmetric ablation may be something of a problem. It can affect the aerodynamics for the rather blunt vehicles that we are talking about. Our intuition tells us it is not too much of a problem. There is some experience which shows that it can be a rather severe problem for slender vehicles. It is an area that hasn't been looked at very carefully, as yet, for blunt vehicles and requires some attention if we are to have full confidence in our ability to predict the performance of a probe.

The last area is real-gas aerodynamics. We have lots of wind tunnels, lots of ground facilities in which we can study aerodynamics, but generally we don't get real-gas effects which can play an important role during planetary entry.

So these are some of the technical challenges that still remain. They are being worked on, and I am reasonably confident that we will have the right kind of information at the right time. But it is not all in hand right at the moment.

On Table 5-2 I have listed some of the major obstacles that must be overcome to achieve technology readiness. We have to extrapolate our experience from ground facilities to the flight environment, and that extrapolation is very lengthy and uncertain in terms of heating rate experience; it is an order of magnitude or more that we are extrapolating. I am sure you will hear more about this problem in the second session this morning.

There is a lack of flight experience. The flight experience that we have now is in the regime of Apollo entry. With Pioneer Venus we will gain some flight experience at more severe conditions. But when you talk about outer planet entries, even the Saturn and the Uranus entries, we are talking about potential heating rates, an order of magnitude larger than the Venus heating rates. So we will be lacking any real flight experience, and there is bound to be some kind of risk associated with undertaking a mission without it. At the present time, I am not sure we know how to assess that risk. It is important that we be able to assess it and to quantify if as best we can so that the mission planner can then make his decision as to how much of a risk he is willing to accept.

MAJOR OBSTACLES

- MAJOR EXTRAPOLATIONS FROM GROUND TESTS TO FLIGHT
- LACK OF FLIGHT EXPERIENCE
- LACK OF PARAMETRIC DATA
- UNCERTAIN KNOWLEDGE OF ATMOSPHERES

Table 5-2

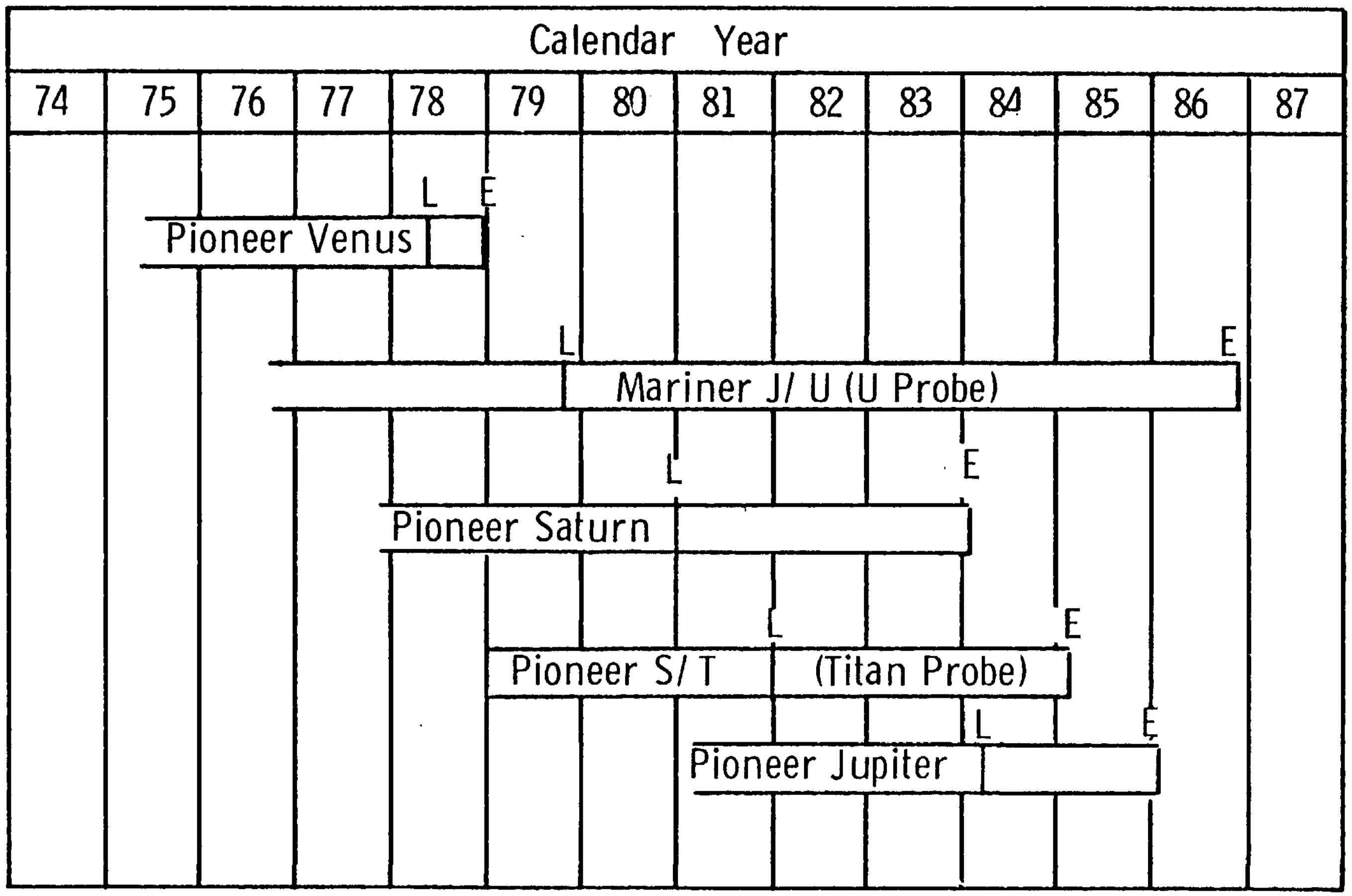
There is a lack of parametric data, as well. If you look at the information available to a probe designer, it is limited to a rather small family of sphere-cone vehicles and a small family of spherical segment vehicles, like an Apollo shape, and that is about it. And as you will hear a little later, even that information leaves a lot to be desired, at least in terms of predictions of heat transfer.

Finally, lets address the area that was talked about yesterday, the uncertain knowledge of the atmospheres. I heard what I thought were two stories that were somewhat conflicting. I heard one story that said the upper and lower bound atmospheres, or the cold and warm atmospheres, were probably too far away from the nominal; that if you applied some statistics and asked about three sigma errors and things like that, you could close in on the nominal atmosphere. But then I heard that the nominal atmosphere wasn't necessarily the most probable atmosphere. We also heard a good bit about the Pioneer 10 results, and the question which has arisen as to how to interpret those results and what they mean in terms of an atmospheric model. Think back to our experience with the Martian atmosphere; what we know as the Martian atmosphere now falls completely outside of the bounds that we had placed on the Martian atmosphere prior to any information gained from Martian orbiters. So I am not all that confident that we can squeeze down on the nominal atmosphere because I am not all that sure the nominal atmosphere is the proper one.

We need some good information on what really are the bounds of the atmosphere. Obviously, the scientists can't tell us precisely what the atmosphere is. That is one of the reasons we are going there. But anything they can tell us about what really are the upper and lower bounds on the atmosphere will be very helpful in probe design.

I wish to elaborate a bit more on the lack of flight experience, and what it really means. This Figure 5-2 is labeled as

CURRENT OSS MISSION MODEL*



L - LAUNCH E - ENTRY

*APRIL 1974

Figure 5-2

the current OSS mission model. That is the model which Dan Herman came up with yesterday. Let's look at what kind of flight experience will be generated by the current series of proposed missions. The schedule shows the Pioneer Venus multiprobe mission with launch in May, 1978, two Mariner Jupiter/Uranus spacecraft (possibly with Uranus probes) with launch late in 1979, two Pioneer Saturn probes with launch late in 1980, two Pioneer Saturn/Titan spacecraft (possibly with Titan probes) early in 1982, and two Pioneer Jupiter probes with launch early in 1984. At first glance this may appear to be a reasonable sequence in (roughly) increasing order of difficulty. However, when trip times are considered the sequence becomes rather distorted. The first probes to enter are the Pioneer Venus probes late in 1978, only one year prior to the Mariner Jupiter/Uranus launches. The next probes to enter are at Saturn in early 1984, only a few months before the Pioneer Jupiter launches. All other probes enter the target atmospheres after 1984. As a result, the only real flight experience which can impact outer planet probe design must be gained from the Pioneer Venus multiprobe.

So with this kind of schedule, we face the possibility of committing ourselves to a series of probe experiments without really gaining any flight experience. This may be all right, but we have to assess the risk associated with this kind of operation. I don't think we have as yet. Instead, we rather hopefully claim that the technology is in hand. As I said earlier, I think you will hear this morning that it is not that well in hand.

I'll now introduce our first speaker, Donn Kirk of Ames who will discuss the effect of initial conditions on the deduced atmosphere for Uranus and Jupiter entries. This relates to our ability to reconstruct an atmosphere based upon the data we get from a probe considering the uncertainties in entry conditions and aerodynamics.

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EFFECT OF INITIAL CONDITIONS ON DEDUCED ATMOSPHERE FOR URANUS AND JUPITER ENTRIES

D. Kirk

NASA Ames Research Center

MR. KIRK: I want to discuss atmosphere reconstruction and what I mean by that is the determination of the density, the pressure and the temperature as functions of altitude. I want to discuss how this determination is affected by errors in the initial conditions.

The initial conditions I am talking about are the entry velocity and the entry flight path angle. There are two distinctly different kind of errors that I want to distinguish between before proceeding. One is the navigation kind of error where you try to enter at a flight path angle of minus 30 degrees and because of various tipoff errors and so forth, you can only guarantee that you will enter minus 30 plus or minus 10 degrees. And this is an important kind of error in designing the actual probe, because it affects the peak heating and peak deceleration. But it doesn't affect the atmosphere reconstruction at all.

The error that affects the atmosphere reconstruction is that you really enter at 32 degrees flight path angle and you are told that you entered at 30 degrees. This 2 degree error does have a significant impact on the determination of the atmosphere structure.

Table 5-3 is a summary of the cases that I am going to talk about this morning. The Saturn mission is also included here to give kind of a complete idea about the outer planets.

What we have here, let us just go down the column. Under Jupiter, this is a reasonable entry velocity. Entry flight path angle of -9.5° indicates a very shallow entry to cut down on the peak heating. And let me point out that these numbers are all relative, relative to the atmosphere. They are not inertial numbers.

		Jupiter	Saturn	Uranus	(PAET) Earth
V_E	km/sec	47.4	28.9	24.8	6.6
γ_E	deg	-9.5	-39.5	-40	-40.9
$z = 0$		$p = 1$ atmosphere $\longrightarrow$			$\longrightarrow$
z (1g)	km	244	358	341	76
z (peak g's)	km	102	148	139	35
peak g's		328	360	255	76
z (M~2)	km	60	99	87	26

results using "Nominal" atmospheres

Table 5-3

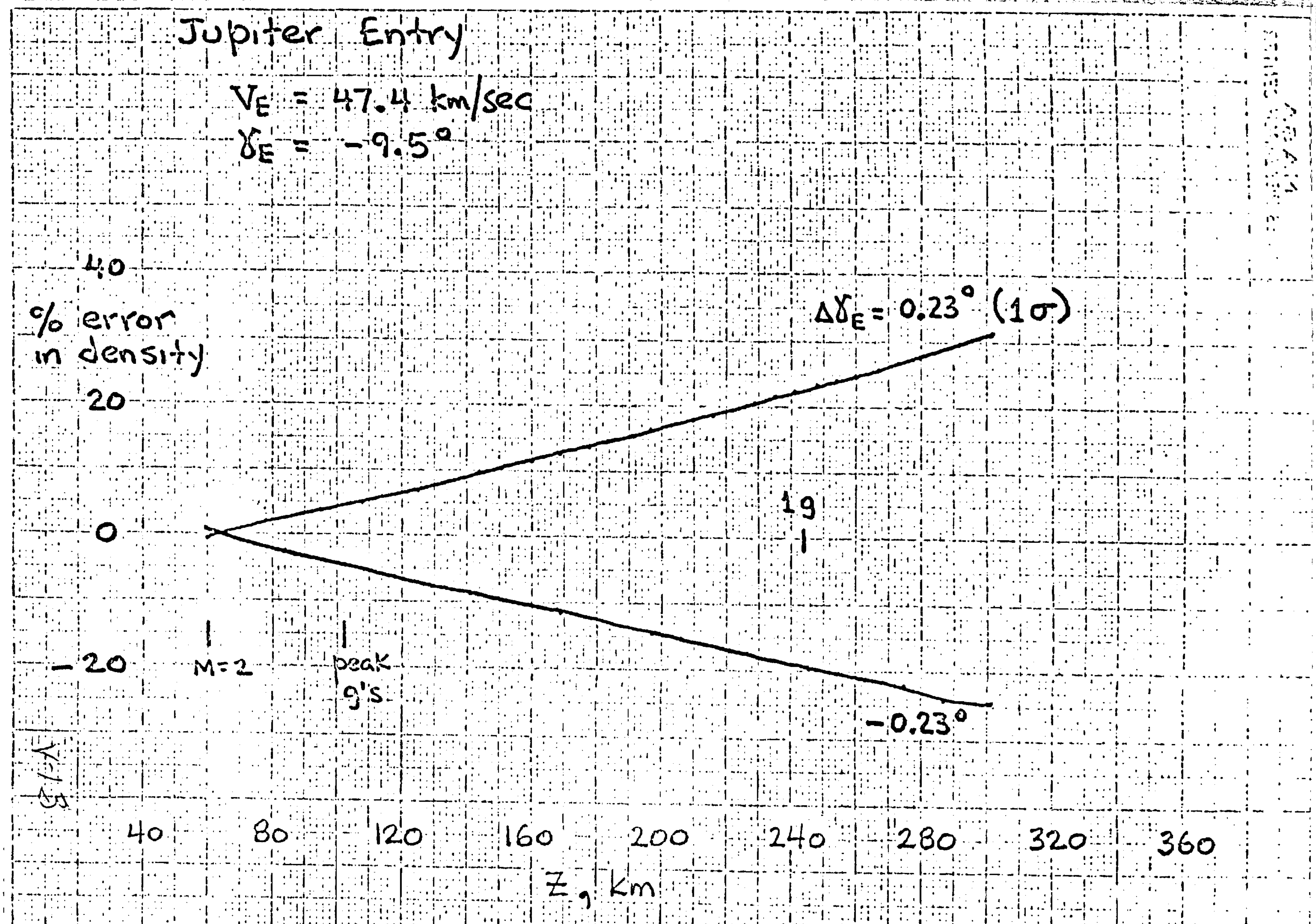
For all the cases, the zero altitude is where the pressure is one atmosphere, just arbitrarily. And I have listed here where the probe first experiences one G deceleration where it reaches peak G's, what the peak G's are and where it reaches a Mach number of two; and for the high speed part of the entry, all you are relying on is an accelerometer to determine the structure of the atmosphere. And this is where the errors in the initial conditions come into play quite strongly.

You will notice for Saturn, the altitude range is roughly the same. For Uranus, the altitude range is roughly the same. We are talking about roughly 300 kilometers down to 100 kilometers for each of the three planets.

All of these results are using the nominal atmosphere, but we did do cases with the extreme atmosphere and it does not affect what I am going to say.

I included, here, the PAET flight from three years ago into the Earth's atmosphere where we demonstrated this concept of high speed determination of the atmosphere. The peak deceleration was only 76 G's and the altitude range was from 76 kilometers down to 26 kilometers. Over that range, we feel that we determined the density profile well within ten percent of its true value, and that would be a reasonable goal that we would like to achieve for the outer planets if at all possible.

On Figure 5-3 I have the Jupiter entry with the flight path angle of nine and a half degrees. What is shown here is the percent error in density as a function of altitude, and this altitude is from the pressure equals one atmosphere level. Shown here are two curves, one for an error in the flight path angle of plus about a quarter of a degree and one for minus of about a quarter of a degree. Notice that this error is about two and a half percent of the initial flight path angle. It is not a very sizeable error, and is the one sigma, not three sigma, error from navigation that is assumed right now.



Jupiter Entry
V_E = 47.4 km/sec
γ_E = -9.5°
40
% error
in density
20
0
-20
$\Delta\gamma_E$ = 0.23° (1σ)
1g
M=2
peak
g's
-0.23°
40
80
120
160
200
240
280
320
360
Z, km

Figure 5-3

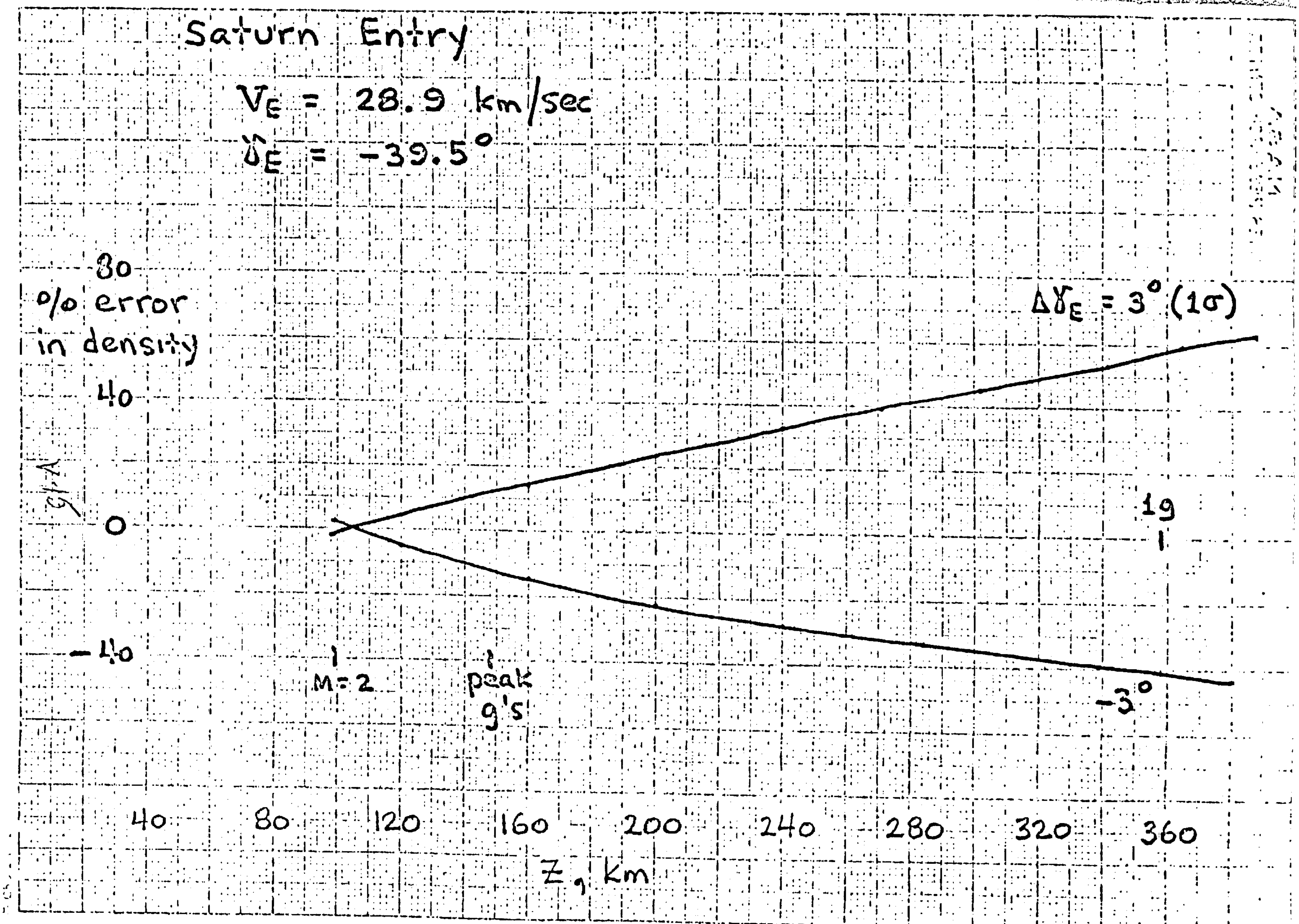
After the fact, we should be able to do better in knowing the entry flight path angle. How much better, nobody seems to know. But you will notice that for this kind of error, you are talking about errors in the density of 30 or 40 percent at the altitude where the probe is experiencing more than one G deceleration and where you had hoped to have a very good handle on the atmosphere. And this error is only due to this initial condition error. Everything else is completely exact.

Figure 5-4 is the same kind of plot for entry at Saturn. Again, this is the one sigma error that is assumed right now as far as navigation is concerned. They claim that they can enter at thirty-nine and a half degrees plus or minus three degrees one sigma. So, again you see that through a large part of the altitude range, you are talking about sizeable errors that could be introduced by an error in the initial flight path angle.

Figure 5-5 shows the same thing for Uranus. And here I don't know what the one sigma or three sigma errors in navigation are, but shown is the result if there is an error of one degree. It is similar to the previous plots, a ten or twenty percent error in the density is introduced by this one factor.

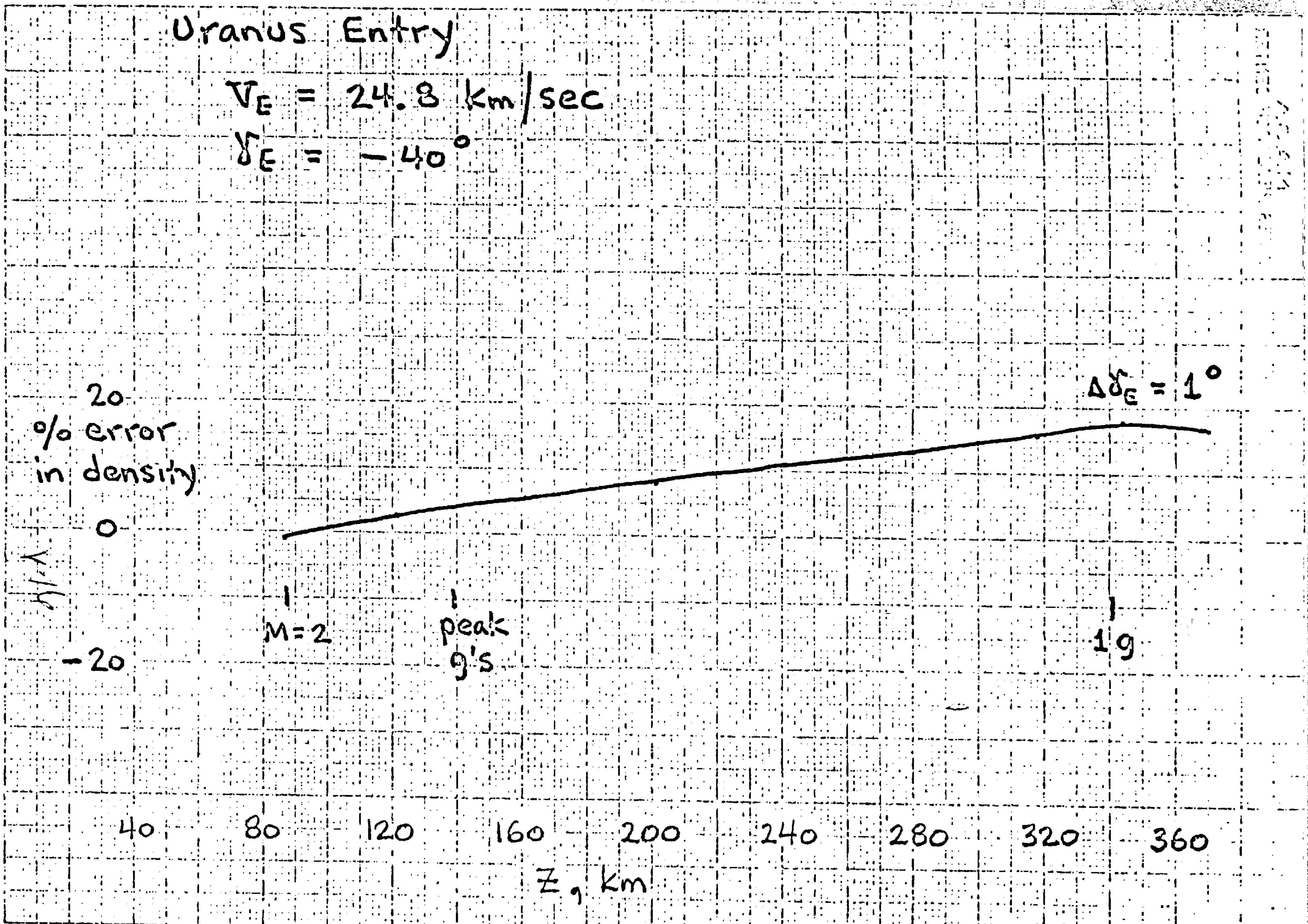
I want to point out one thing: to get the pressure in this high altitude region, you essentially integrate the density so the same kind of error that you get in the density shows up in the pressure. What this leads to is a surprising thing, that the temperature that you get by just dividing the two comes out quite good. For this particular case, the temperature error over that entire altitude range was less than five degrees kelvin. So you can get sizeable errors in density, sizeable errors in pressure, but small errors in the temperature.

Everything I have done so far has been for errors in the flight path angle. Figure 5-6 shows the effect of errors in the initial entry velocity, and this is for the Saturn entry. You remember



Saturn Entry
V_E = 28.9 km/sec
γ_E = -39.5°
80
% error in density
40
0
-40
$\Delta\gamma_E$ = 3° (1σ)
1g
M=2
peak g's
-3°
40
80
120
160
200
240
280
320
360
Z, km

Figure 5-4



Uranus Entry
V_E = 24.8 km/sec
γ_E = −40°
$\Delta\gamma_E$ = 1°
20
% error
in density
0
−20
M=2
peak
g's
1g
40
80
120
160
200
240
280
320
360
Z, km

Figure 5-5

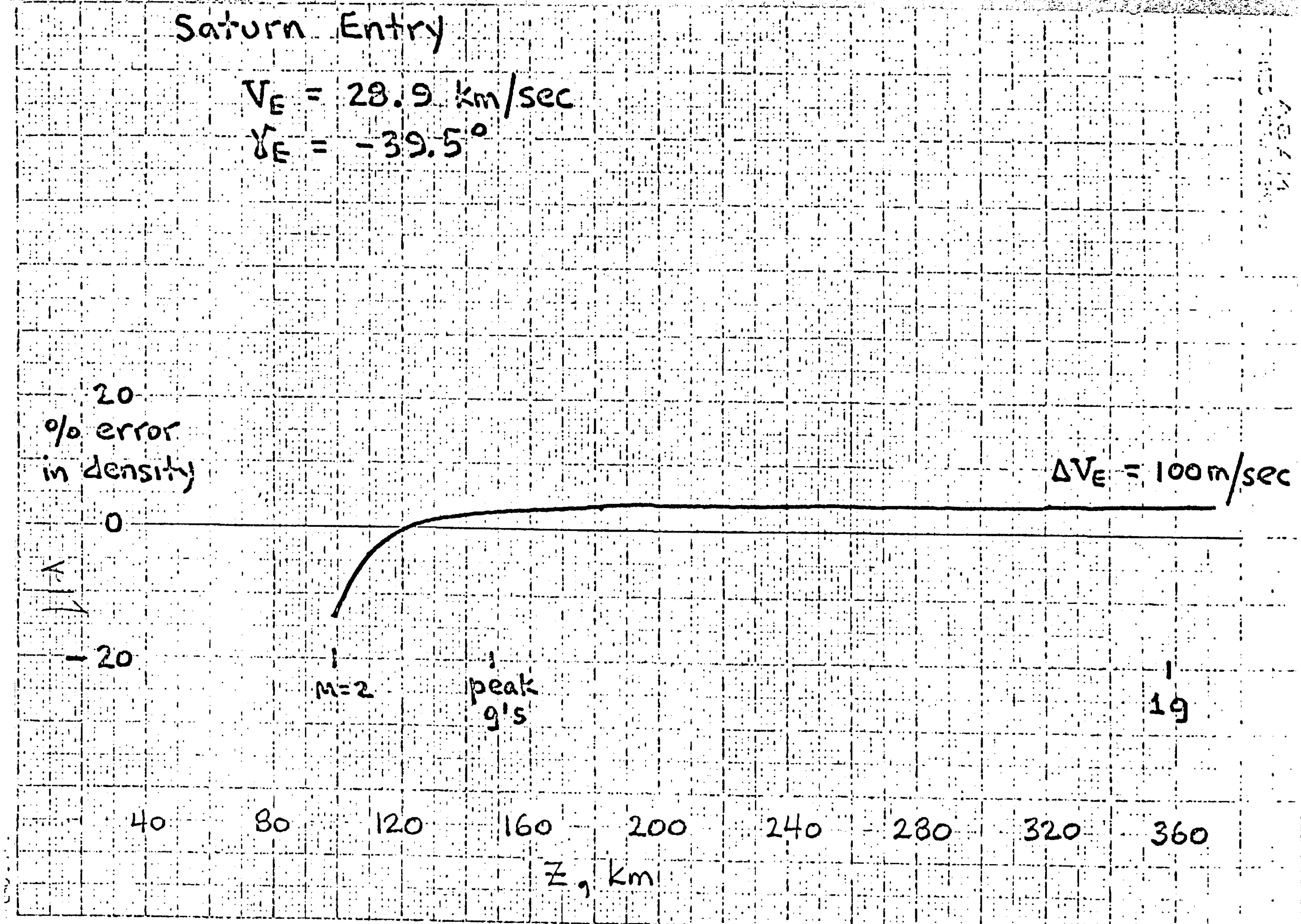

Saturn Entry
V_E = 28.9 km/sec
γ_E = −39.5°
20
% error in density
0
−20
ΔV_E = 100 m/sec
M=2
peak g's
1g
40
80
120
160
200
240
280
320
360
Z, km

Figure 5-6

how all the flight path angle errors were relatively linear and came down to a value that was very small. This shows that at high altitudes, a 100-meter per second error, that is one hundred of 28,900, introduces about a four percent error in the deduced density. This four percent stays constant through most of the altitude range, and then switches sign near the end of the high speed experiment. At this point, you are going to deploy a temperature sensor, and from then on you are going to actually measure the temperature, measure the pressure. So, from then on, the atmosphere reconstruction is extremely accurate.

The funny thing here is that if you corrected this value of density to the value you get from a low speed experiment, in other words, push the entire curve up, what you would be doing is throwing the rest of the atmosphere up to about a ten percent error.

I want to conclude by saying that my feeling is that it is a shame to introduce sizeable errors like this in the atmosphere reconstruction. What I hope is that people who are knowledgeable in tracking can come up with ways to get errors in the initial velocity and initial flight path angle down to an abosolute minimum.

MR. FRIEDMAN: That was error that was associated with the a posteriori effect.

MR. KIRK: Yes, that is correct

MR. FRIEDMAN: That is a knowledge error that you can obtain through solving.

MR. KIRK: We don't care anything about real time, necessarily. Two weeks after the fact, what is the best estimate that people can come up with?

MR. RON TOMS: I am not sure I quite understood how many readings you need in order to get those kinds of accuracies that you are showing. I have heard people say that the Uranus descent may be competent of reading all the way down to the surface.

MR. KIRK: No, you have to get a number of readings during the high altitude part and these readings would be put into a storage during the entry and then played back during the low speed descent.

MR. TOMS: So the errors you are showing had nothing to do with the number of readings that are taken.

MR. KIRK: I have assumed exact acceleration readings throughout the entry. Only the initial conditions have affected the accuracy of the atmosphere reconstruction. When I ran the case with no errors in the initial conditions, I deduced the atmosphere within a tenth of a percent through the whole altitude range.

UNIDENTIFIED SPEAKER: Just a comment. I think your Jupiter numbers probably more than any others look very optimistic. You are hoping for a lot to get a determination that good. The other numbers, I think may be somewhat more reachable.

RADIATIVE RELAXATION RATES AND INTENSITIES DURING OUTER PLANET ENTRIES

Dr. L. Leibowitz
Jet Propulsion Laboratory

DR. LEIBOWITZ: This morning I would like to give you a review of the gas properties which can affect outer planetary entry probe radiative heat transfer.

The goal is to be able to predict the effect of processes such as radiative relaxation, radiative cooling, and equilibrium radiation intensities on entry. The purpose is to better quantify these processes in order to avoid overestimating the radiative transfer by an over simplified approach to the problem. By reducing these uncertainties in the knowledge of these processes, we hope to minimize the heatshield weight by reducing safety factors and performance limits that might otherwise have to be put in.

Figure 5-7 is a schematic diagram that roughly shows flow regions for an outer planetary entry probe. The atmosphere of the outer planets, as you know, is molecular hydrogen and helium, for the most part. Through the shock layer these gases are transformed into hydrogen atoms, ions and electrons. You can basically think of the shock layer in terms of three regions, neglecting the boundary layer. First we have a weakly radiating non-equilibrium layer. In this layer the shock heated gas undergoes chemical reactions and is transformed as it flows into the ionized species. Then we have the equilibrium layer where the gases are considered in local thermodynamic equilibrium and the radiation transfer can be calculated accordingly. Finally we have a high-temperature radiative cooling region where the hot gas radiates much of its energy away into the outer flow and by loosing that energy the temperature falls and it, therefore, radiates considerably less energy to the wall, thus causing lower heat transfer.

These three regions represent areas of separate topics of study. The non-equilibrium layer is the one that we have been emphasizing. In this region the radiation is proportional to the electron concentration. The electron concentration is initially

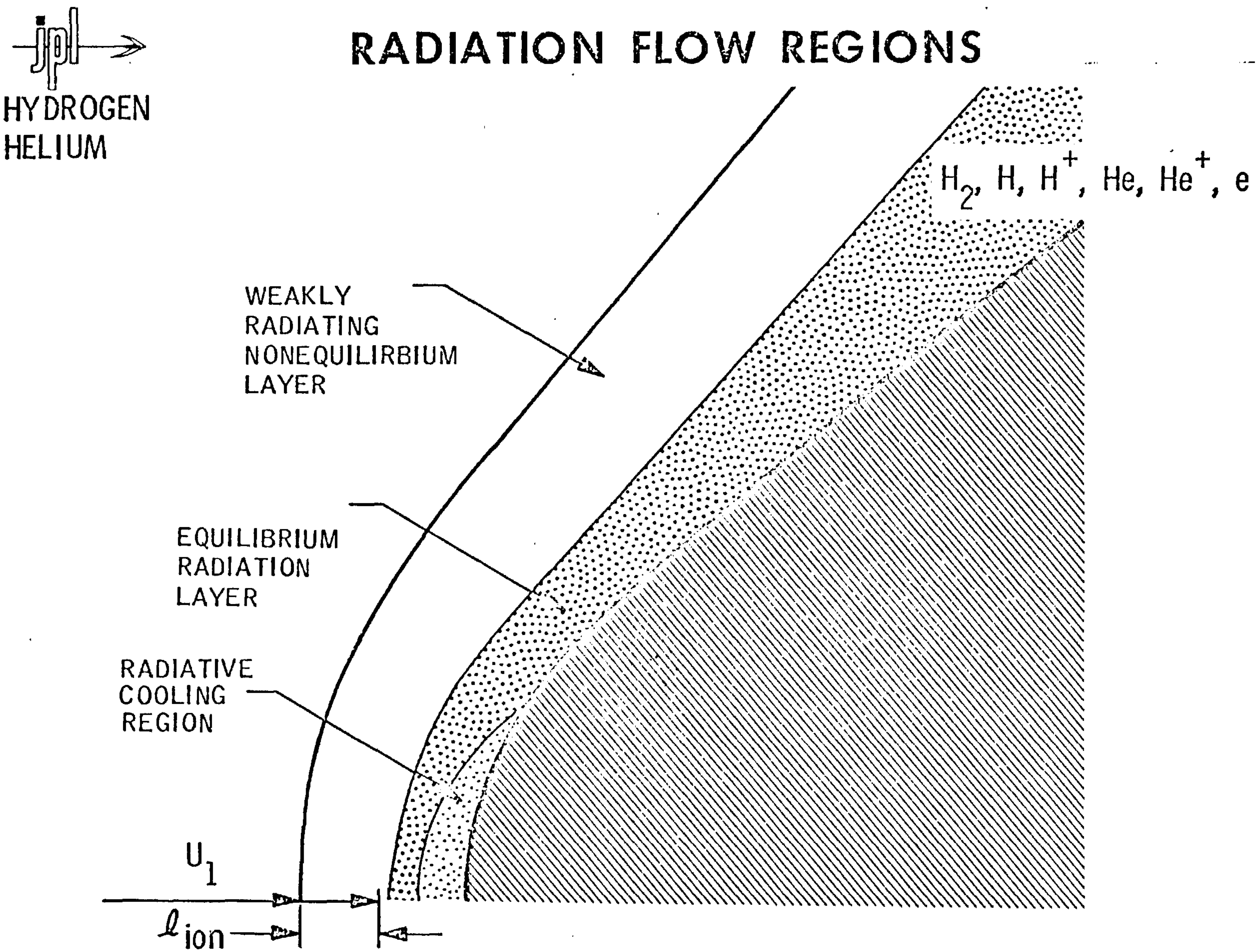

RADIATION FLOW REGIONS
jpl
HYDROGEN
HELIUM
H_2, H, H^+, He, He^+, e
WEAKLY
RADIATING
NONEQUILIRBIUM
LAYER
EQUILIBRIUM
RADIATION
LAYER
RADIATIVE
COOLING
REGION
U_1
ℓ_{ion}

Figure 5-7

zero at the shock wave and then as the reactions take place it increases to an equilibrium value; therefore, when the electron concentration is much below the equilibrium concentration the radiation is much below the equilibrium radiation. So, in the case where the relaxation distance is long compared with the standoff distance you have a large region of virtually radiation free gas.

This is considerably different from the case of non-equilibrium radiation for Earth and Venus, where the non-equilibrium overshoot of molecular species behind the shock wave resulted in an increase in radiation over what the equilibrium theory would indicate.

Our approach has been to develop shock tubes which produce conditions as close as possible to entry, then to make measurements of the radiative and kinetic properties of the shock heated gases and finally, the experimental data is applied to flow field calculations in order to obtain entry heat flux. Data has been obtained both in a conical arc driver, shown in Figure 5-8 and a newly-developed annular arc driver, called ANAA shock tube. The ANAA shock tube deposits energy of a capacitor bank into a flowing gas which then immediately expands and cools before it can lose energy to the walls of the shock tube while it waits for a diaphragm to open. With this new shock tube, Jupiter and Saturn entry velocities and pressures, for the most part, can be simulated.

In the diagram of Figure 5-8, we see a capacitor bank which discharges a spark into a gas. The heated gas then rushes down the tube driving a shock wave ahead of it. The radiation emitted behind the shock wave, then, is measured by a series of spectrometers and monochromators. Hydrogen line and continuum channels are detected, including the profile of the H Beta line using a fiber optics slit system which can be used to get electron densities and temperatures directly.

EXPERIMENTAL APPARATUS

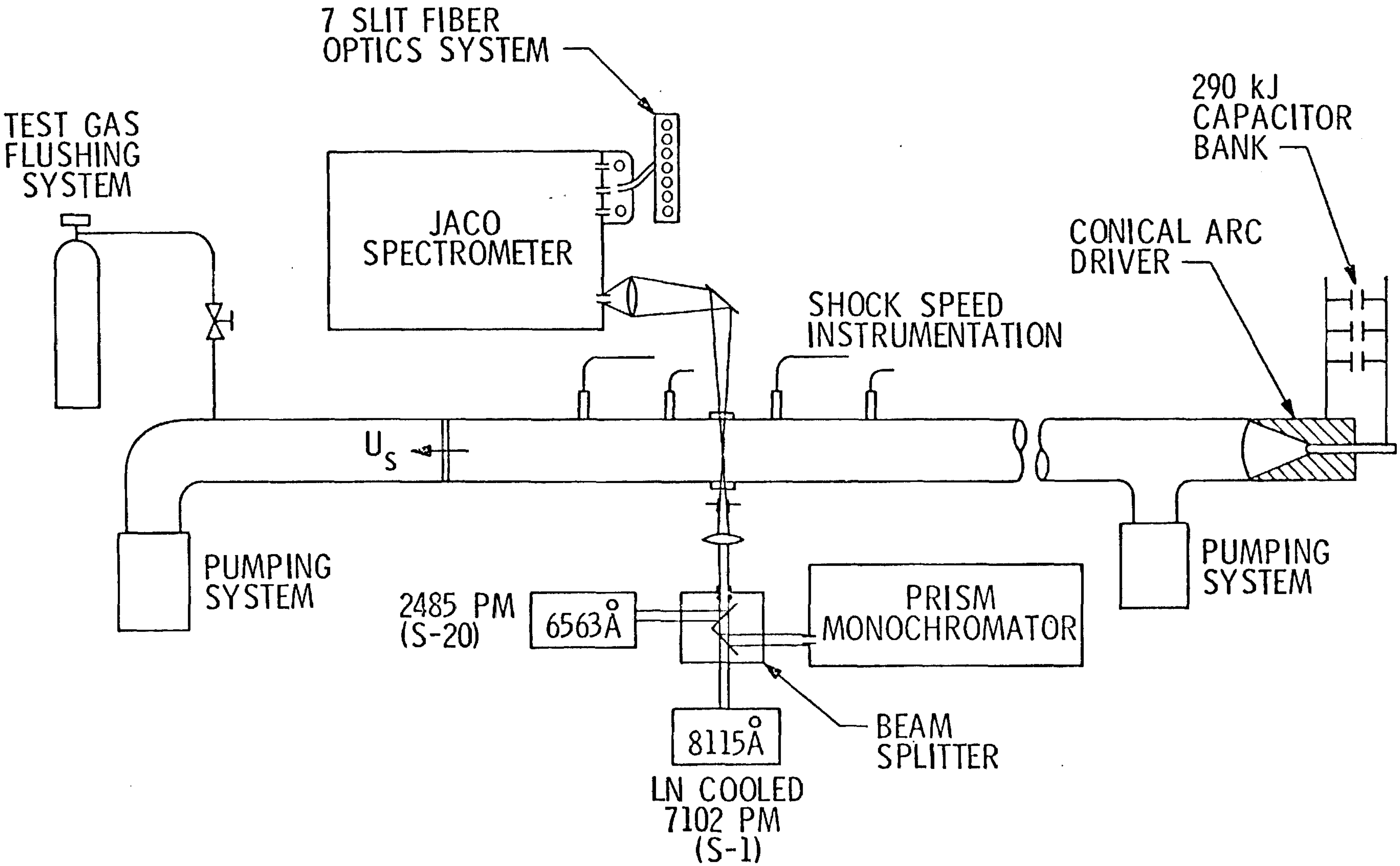


Figure 5-8

ORIGINAL PAGE IS OF POOR QUALITY

Figure 5-9 is our latest trace obtained last Friday and it is our closest attempt at simulating outer planetary entry conditions. This is for an initial pressure four torr and 26 kilometers per second. This roughly approximates peak heating for a Saturn entry. We have measured here the intensity of the H Beta line as a function of time. This is a magnified version. Intensity is down so, initially at the shock arrival, the intensity is virtually zero; then, as the chemical reactions take place and the electrons begin to be formed, the intensity suddenly jumps and then rapidly reach an equilibrium value. The relaxation distance is the distance between the shock arrival and when equilibrium is achieved. It is rather substantial: four centimeters compared with standoff distances. We will see that a little later.

Figure 5-10 is a plot of relaxation distance times the initial pressure in the shock tube as a function of the shock velocity. The dark points are the higher pressure data obtained with the ANAA shock tube. The solid line is a curve fit obtained from numerical integration of the ionization and dissociation reaction kinetics. By adjusting rate parameters one can see that there is rather good agreement on the dependence on the part of both the data and the calculations. The squares represent data obtained at a much lower pressure in the conical driver and while the data agrees very well at the higher shock velocities, it diverges somwhat at the lower velocities which seems to indicate the possibility of test time limitations in these low velocities.

With the kinetic data obtained by fitting the experimental results we can apply the kinetics program to the flow field case. This is the subject of the next talk by Dr. Kuo. It is with data such as this that we will be able to quantify the non-equilibrium effect for outer planet entry conditions.

SATURN ENTRY RADIATION RELAXATION

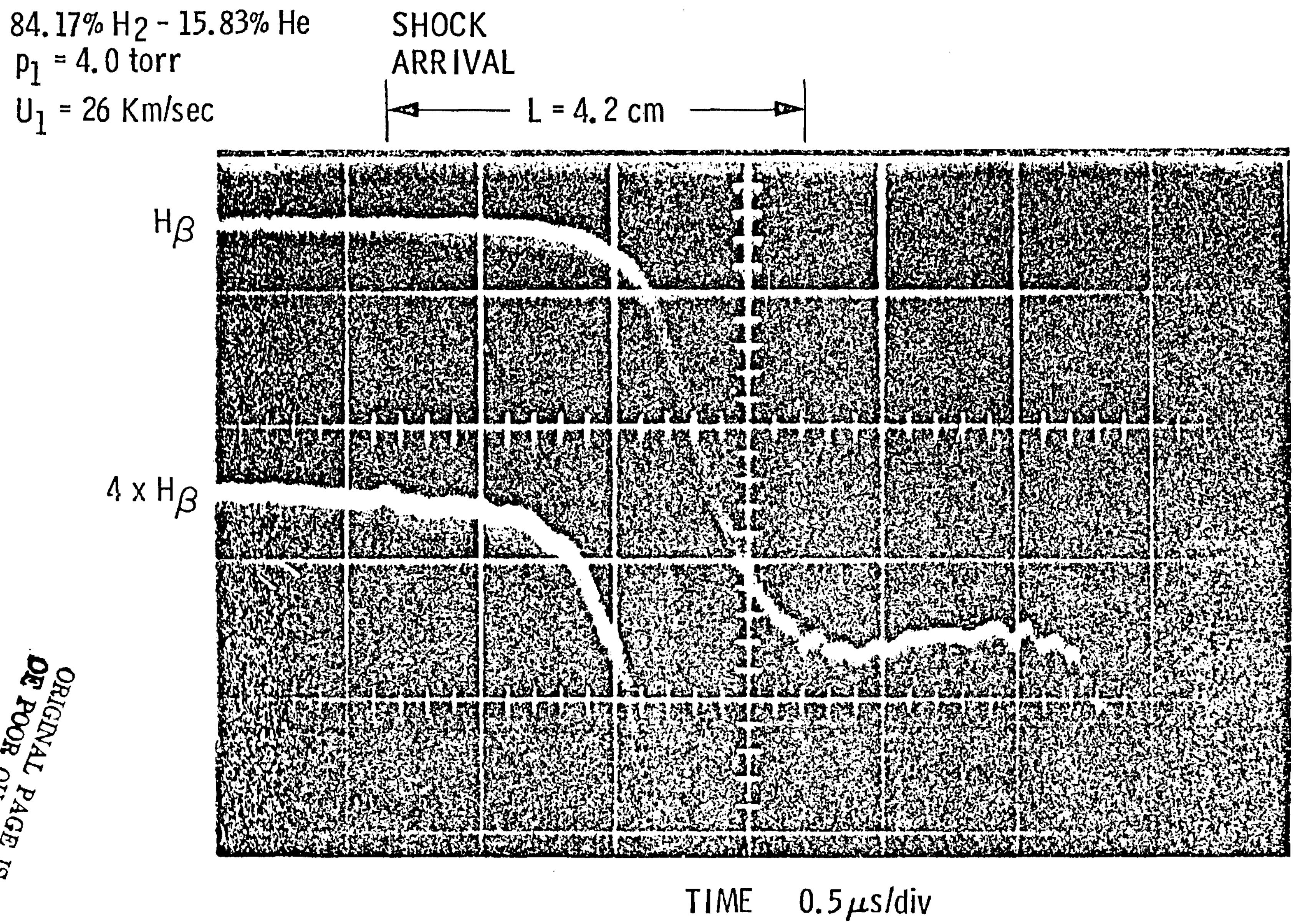


Figure 5-9

RADIATION RELAXATION DISTANCE

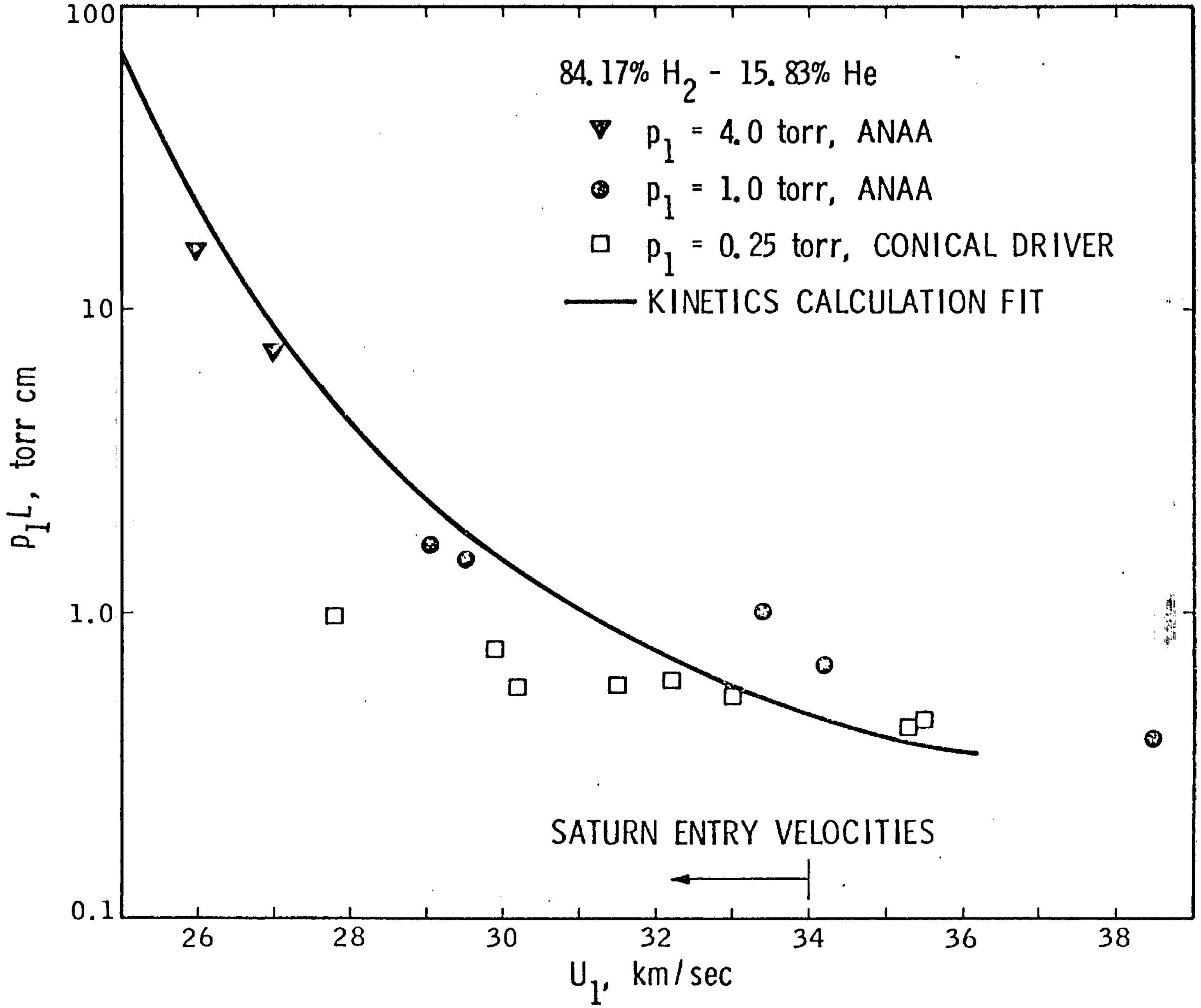


Figure 5-10

We have made a rather comprehensive comparison of equilibrium hydrogen and helium radiation measurements with theory. We have covered ranges of temperatures from 10,000° to 20,000° Kelvin and electron densities that cover the full range of Saturn and Jupiter entry conditions. Figure 5-11 is a sample of some of the typical agreements that we have obtained. This is hydrogen line radiation and these are hydrogen continuum channels over a wide range of temperatures. As you can see, for the equilibrium calculations, we are very well able to predict what we measure in the shock tube. Throughout the full range of all conditions that we have covered we get a twenty-five percent agreement with the theory.

Concerning radiative cooling measurements, we've just begun to use the capabilities of the ANAA shock tube for this study. Radiative cooling could result in up to a seventy percent reduction in radiative heating during portions of Jupiter entry trajectory. Initial experimental data is in reasonable agreement with simplified calculations. This work is now being continued.

In conclusion, due to recently improved simulation facilities that are able to produce Jupiter and Saturn entry conditions, and the development of the non-equilibrium flow programs, we are in a good position now to accurately assess the effect of each of these radiative processes on the entry trajectories themselves and on the heatshield requirements.

UNIDENTIFIED SPEAKER: On that final chart, the curve you labeled as a function of lambda. It goes eight tenths, point three, and point sixty-five. Is that a peaking situation and, if so, what would cause that peaking?

MR. LEIBOWITZ: The top curve is line radiation which is considerably more intense than the continuum. The bottom two traces are continuum which increases with decreasing wavelengths.

C-5

EQUILIBRIUM INTENSITY

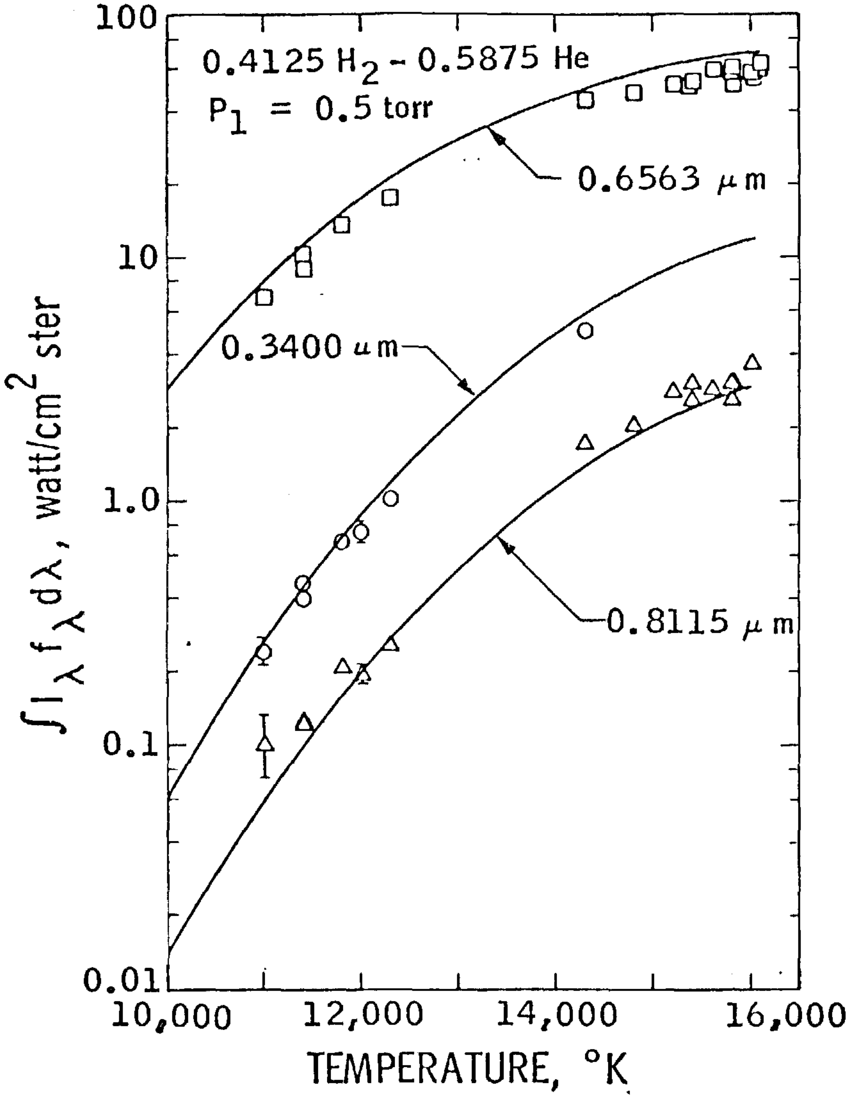


Figure 5-11

ORIGINAL PAGE IS OF POOR QUALITY

UNIDENTIFIED SPEAKER: Lew, you said something about a seventy-percent reduction in radiative heating for Jupiter; would you expand on that a little bit: for what conditions?

MR. LEIBOWITZ: The question was under what conditions do you get a seventy-percent reduction in radiative heating due to the radiative cooling effect. That's a rough number. That would correspond, probably, to close to a worst case. I think that's a rather severe entry of, like, entry angles of greater than ten degrees. I don't have the exact numbers.

I don't claim that that would be an integrated value.

UNIDENTIFIED SPEAKER: I see; because the thing that strikes me is that Jupiter is such an energetic entry and the temperatures are so high and I think it would drive us towards equilibrium much better than the other planets.

MR. LEIBOWITZ: This is a different phenomenon when we talk about radiative cooling. That's not non-equilibrium. It's true that we expect the non-equilibrium effect to be much more significant, I think, at Saturn than for Jupiter.

UNIDENTIFIED SPEAKER: Could you make some kind of a comment about the sensitivity of this to the presence of those heavy elements we heard about yesterday.

MR. LEIBOWITZ: Yes. I haven't looked into that personally. Some work has been done here, I think, by Bill Page. His data that I have seen seems to indicate that it's not that sensitive. We haven't gone through this but our physical intuition seems to indicate that the heavy elements should be at the lower altitudes and one wonders whether it percolates up to the altitudes of severe entry.

UNIDENTIFIED SPEAKER: I thought Jupiter's peak heating was located at about the same height as the Pioneer 10 occultation data controversy.

MR. LEIBOWITZ: Yes?

UNIDENTIFIED SPEAKER: Howard, what is the pressure level at which peak heating occurs?

MR. LEIBOWITZ: All I know is about 10^7 dynes per square centimeter if that tells you anything. I think there is a two-fold problem. We can answer that question in a shock tube. The work has already been started at Ames on that. This can be continued. It is fairly easy to make shock tube measurements of what a little bit of one thing and another does. As I say, the initial indications are that it may not be that important. Hydrogen has always been an impurity that causes more problems in measurements of other compounds.

MR. SEIFF: Here is a comment. I have been working on this problem actively as I think everybody knows. There are two things that these gases can do. In the first place, I think their presence was a presumption. If they are present, they can do two things. One of them is they can absorb energy by dissociating - in trace constituents that will not be an important effect. The other thing that they can do is introduce line radiation in other locations than those that are being studied here. Again, with minor constituents, this should not be an important effect.

MR. LEIBOWITZ: I think all these species are present, probably, as ablation products, in much higher concentrations in the shock layer, than they would be in the atmosphere.

MR. OLSTAD: For the case of the Jupiter entry, a steep entry into a cold atmosphere which is the worst case in terms of heating rate, the shock layer is essentially optically thick. If you put any other radiators in there it doesn't matter unless it affects the temperature. The trace constituents won't affect the temperature too much. In that case, they shouldn't be too severe. It can have some effects on the non-equilibrium chemistry. As Lew mentioned, there have been some tests here at Ames which have introduced trace amounts of methane and ammonia. They have found, essentially, no effect on the amount of heating. But these were really trace amounts. I think there is some evidence that, in the Uranus atmosphere at least, they may be more than just trace amounts.

UNIDENTIFIED SPEAKER: At pressures of less than about a tenth of a dyne per square centimeter you are above the photochemical level and you will just have a hydrogen atmosphere, basically. There isn't even any methane to make photochemical products.

MR. SEIFF: Could we see that chart again that shows the relaxation lengths? (Figure 5-10)

I presume those were relaxation lengths - that would be the products of pressure and the relaxation lengths. That capital L there is the distance behind the shock wave? How was that defined? Is that when the radiation peaks?

MR. LEIBOWITZ: Yes. It is defined on the sample oscillograph. It's the distance to approach of equilibrium.

MR. SEIFF: For example: at one torr ambient pressure, at 32 kilometers per second, you might expect to get, say, one centimeter of relaxation distance?

MR. LEIBOWITZ: That's right. A flow-field case will be shown in the next talk for an entry velocity of 28 kilometers with a calculated length of four centimeters.

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NON-EQUILIBRIUM SHOCK-LAYER COMPUTATION FOR SATURN PROBES

TA-Jin Kuo
Jet Propulsion Laboratory

DR. KUO: This study actually is a joint effort by Dr. Lewis Leibowitz and myself.

Figure 5-12 gives the objective and the approach of the shock layer analysis. The objective is to develop physically sound methods for computing the flow field, energy fluxes and heat shield requirements. The justification of the approach is, as we just heard Walt comment this morning about the technical challenges, that total simulation is not feasible; at least as of now.

So it calls for an analytical approach, first carefully examining the governing mechanisms and then seeing how far we could go by uncoupling them, if possible. Then we would study those governing mechanisms separately. Finally, by putting them together and, by synthesizing experimental and theoretical inputs we would provide necessary information for the heat shield computation.

Figure 5-13 gives the approach for the shock layer analysis. First we are going to make a statement that radiation can be uncoupled in the shock layer, an effect which will be ascertained in the subsequent slide; which means then, that the aerothermochemistry of the inviscid shock layer can be uncoupled from radiation as if it is radiatively adiabatic or inert. So, by solving the aerothermochemistry of the inviscid shock layer, we will obtain the constituent densities, N_J, the heavy particle temperature, T_I, and the electron temperature, T_E. With this, it provides sufficient information for the computation of the radiation of the shock layer as if it is a static layer of radiating medium. That is what is meant by the uncoupling.

So, eventually, from both of these then, we will obtain the boundary conditions at the boundary layer. I want to point out here, that the uncoupling, first of all, greatly simplifies the analysis of the problem, and secondly, it allows the shock layer radiation characteristics to be studied in full spectral detail.

- OBJECTIVE

  TO DEVELOP PHYSICALLY SOUND METHOD(S) FOR COMPUTING THE FLOW FIELD, ENERGY FLUXES AND HEAT SHIELD REQUIREMENT.

- JUSTIFICATION

  TOTAL SIMULATION NOT FEASIBLE. CALLS FOR AN ANALYTICAL APPROACH BY PUTTING TOGETHER IMPORTANT MECHANISMS, AND BY SYNTHESIZING EXPERIMENTAL (E.G. REACTION RATES) AND THEORETICAL INPUTS.

Figure 5-12

FLOW CHART OF SHOCK LAYER ANALYSIS

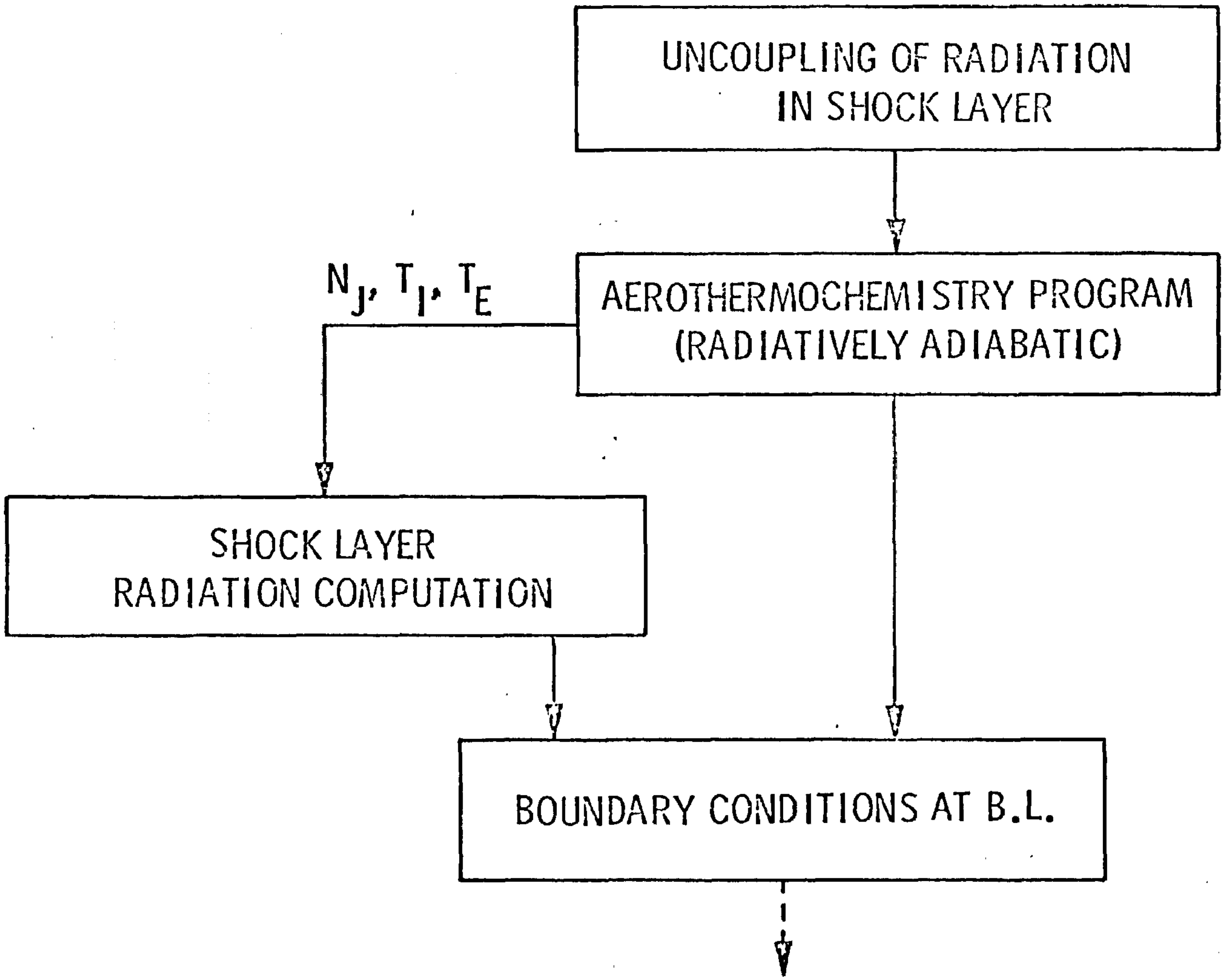


Figure 5-13

The graph of Figure 5-14 is taken from Angus McRonald's trajectory computations which shows that radiation can be uncoupled from the aerothermochemistry, at least for the Saturn probes. The ordinate here represents the ratio of two fluxes. F_{eq}^{SL} is the radiative flux from the inviscid shock layer evaluated under equilibrium conditions towards the edge of the boundary layer. The denominator, $\frac{\rho U^3}{2}$, represents the enthalpy flux as convected by the mass flow.

Now this dimensionless quantity appears as a multiplier in the non-dimensionalized shock layer energy equation. So that, physically, what it shows is the relative importance of the radiative flux term versus the convection term on the left hand side of the energy equation. If this non-dimensional quantity is small, the radiative flux can be ignored in the first order of consideration, which is the case of practical importance.

The abscissa of this represents the time of flight in seconds so the curves actually show the time history of this non-dimensional parameter. We know that for cases of Saturn probes, the cases of interest, the entry angle would be bounded above by forty degrees or fifty degrees. This peaks around two percent, actually slightly less than two percent in the case of a forty-degree entry angle with a probe of 0.7 meters. We can say for sure prior to actual computation, that for the fifty degree angle case, this would be somewhere around 2.5%.

So this number, actually, is small and radiation can be uncoupled from the aerothermochemistry in the first consideration, at least for the Saturn probes. Furthermore, because this is based on the evaluation of tangent slab equilibrium conditions, and we know that under non-equilibrium conditions the radiative flux would be still less, this actually gives an overestimate of what the parameter actually should be.

THE UNCOUPLING OF RADIATION FROM AEROTHERMOCHEMISTRY FOR SATURN PROBES

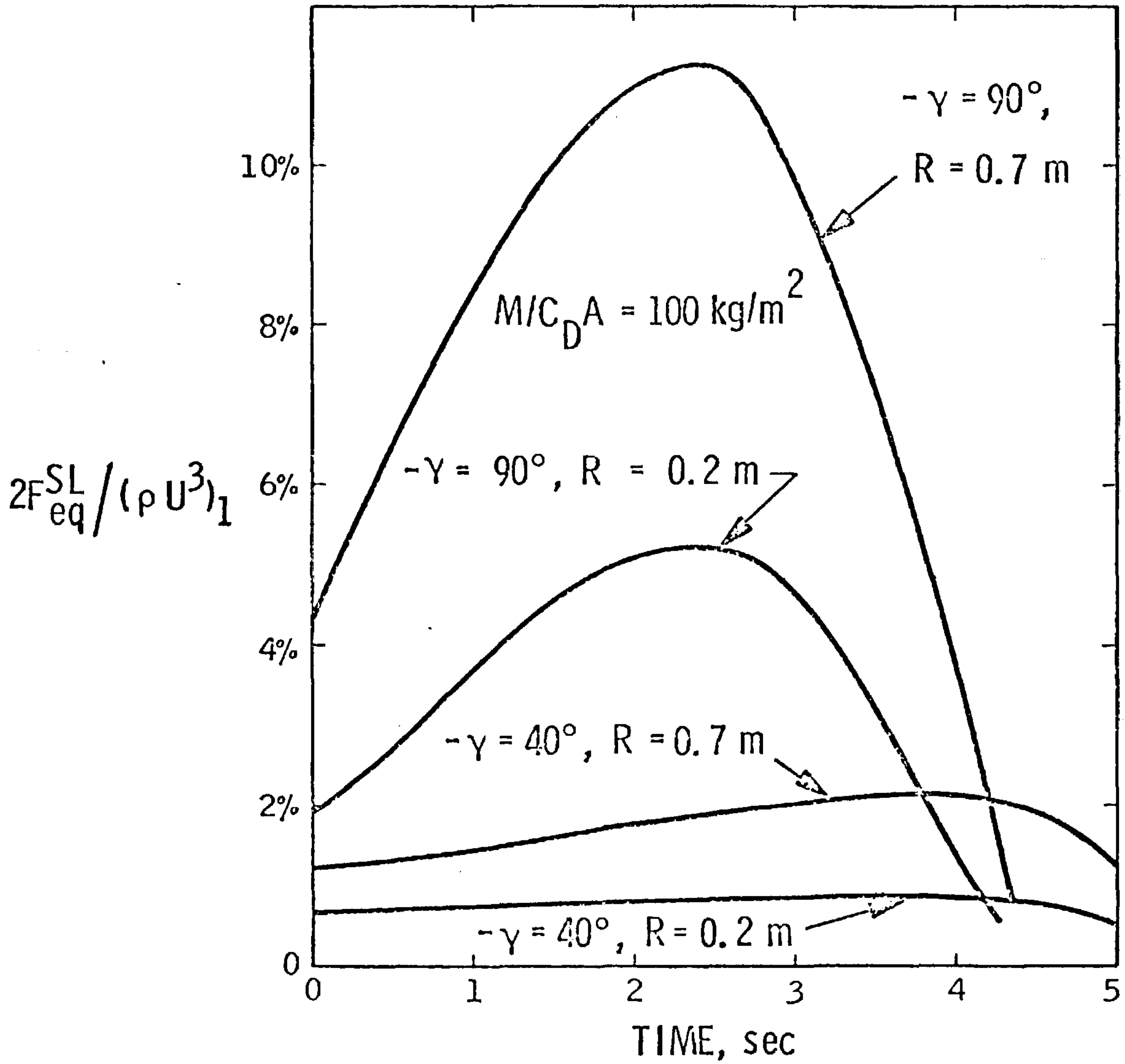


Figure 5-14

As shown in Figure 5-15, with the radiation uncoupled from the aerothermochemistry, we can tackle the inviscid shock layer separately without consideration of radiation. On the right, which gives the geometry for the shock layer analysis, a simple analysis actually, R_b is the body radius, Δ_0 the stand-off distance, δ the displacement of the shock center from that of the body center, and R_{os} the radius of the shock front at the axis.

On the right are the formulas actually used in the computation to get the stand-off distance, its relation versus the density-compression ratio. The quantity ε is the compression ratio which is the ratio of the free stream density to the mean density in the shock layer. These formulas are good over a wide range of ε.

The approach to tackle this problem is, first of all to define a quantity, Ω, which is in essence, the characteristic fluid mechanical time over the characteristic ionization relaxation time which Lewis just talked about a moment ago. This is used to obtain the stand-off distance and to give the shock shape in a manner which Hornring described in his paper which was published in JFM in 1972.

The second point is that the pressure along the boundaries is prescribed because along the body surface we can assume that it follows the modified Newtonian model and along the shock front obeys the oblique shock relation. In between we use a certain interpolation formula so that the pressure field of the entire flow field is obtained.

Thirdly, we use a constant density model to obtain streamlines so that the streamline configuration is thus determined. Finally, we use the reaction rates as taken from Lewis Leibowitz'

AEROTHERMOCHEMISTRY PROGRAM

- APPROACH
  - $\Omega = \frac{R_b}{U_1 \tau_{ion}}$ USED TO OBTAIN Δ_0 AND SHOCK SHAPE
  - PRESSURE ALONG THE BOUNDARIES PRESCRIBED
  - CONSTANT DENSITY MODEL USED TO OBTAIN STREAMLINES AND VELOCITY
  - CHEMICAL KINETICS MARCHING FROM SHOCK FRONT

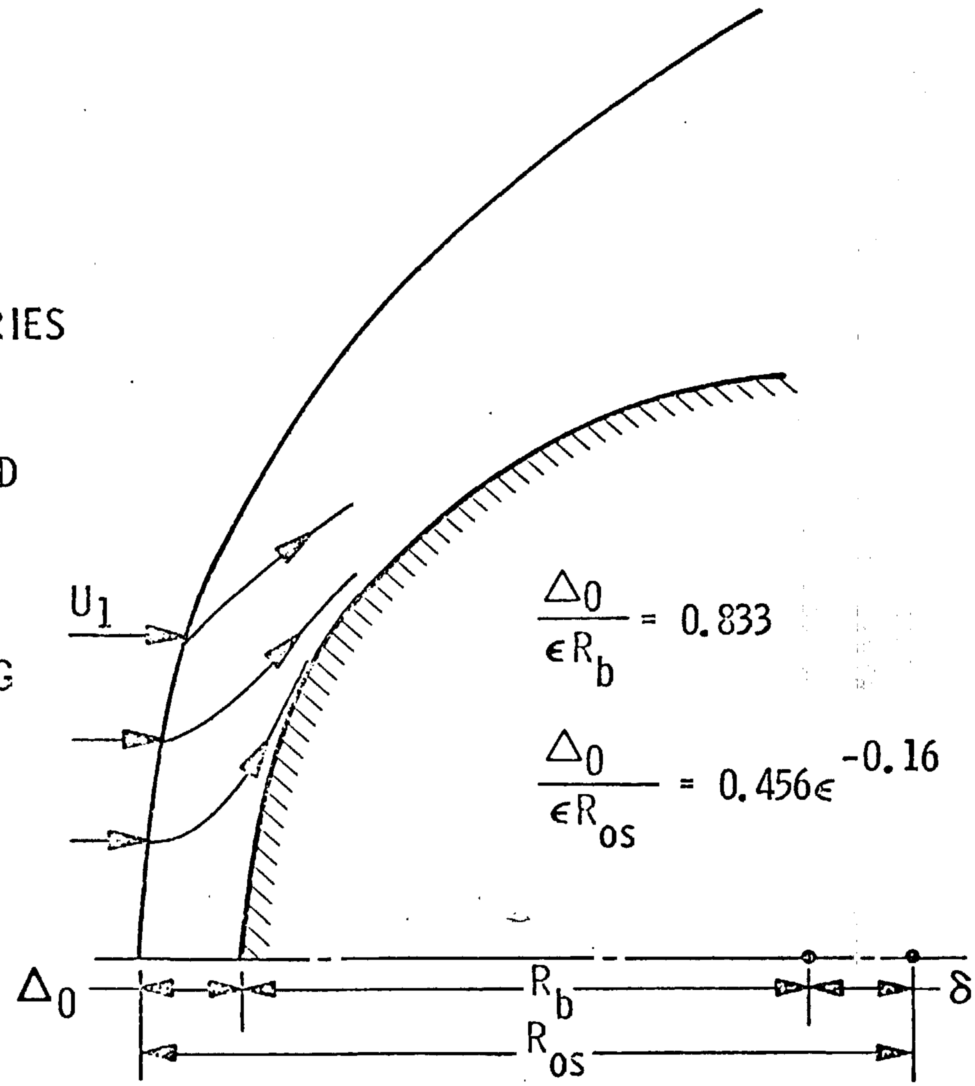


Figure 5-15

shock tube data to compute the chemical kinetics. First of all we march ahead from the shock front and then, step by step, march downstream until the solution is carried far enough. Then we shift to another streamline and, again, march ahead. So, first of all, it is station by station along a streamline and then streamline by streamline until the entire flow field is covered.

By this, then we obtain the chemistry as well as the aero-thermodynamics of the entire flow field.

Figure 5-16 presents the actual computation which we obtained some time ago for the parameters as shown for a Saturn probe, forty-degree entry angle case. The ballistic coefficient is 100 kg/m^2, the reaction rate parameter is given here - about seven - and the probe diameter is 0.7 meters. The probe is at the critical altitude where the heat flux is about at its peak.

Now, we note very briefly that there is a demarcation line between the non-equilibrium zone and the equilibrium zone that Lewis just talked about a moment ago. On the left of this line is the relaxation zone, and on the right of the line is the equilibrium zone. We can see that particularly in the stagnation region the majority of the shock layer gas is actually relaxing, so if we use the equilibrium approach, then, it would be far from the truth, at least in the stagnation region. Please note that for certain cases that the shock layer is not optically thick so this would result in a considerable reduction of radiative flux to the body, at least in this stagnation region.

The next figure, Figure 5-17, shows some later results that we just completed which give the shock layer electron concen-

EFFECT OF IONIZATION RELAXATION

RECENT COMPUTER RESULT SHOWS AT LEAST 60% OF THE STAGNATION REGION SHOCK LAYER IS RELAXING. HEAT SHIELD WEIGHT SIGNIFICANTLY REDUCED

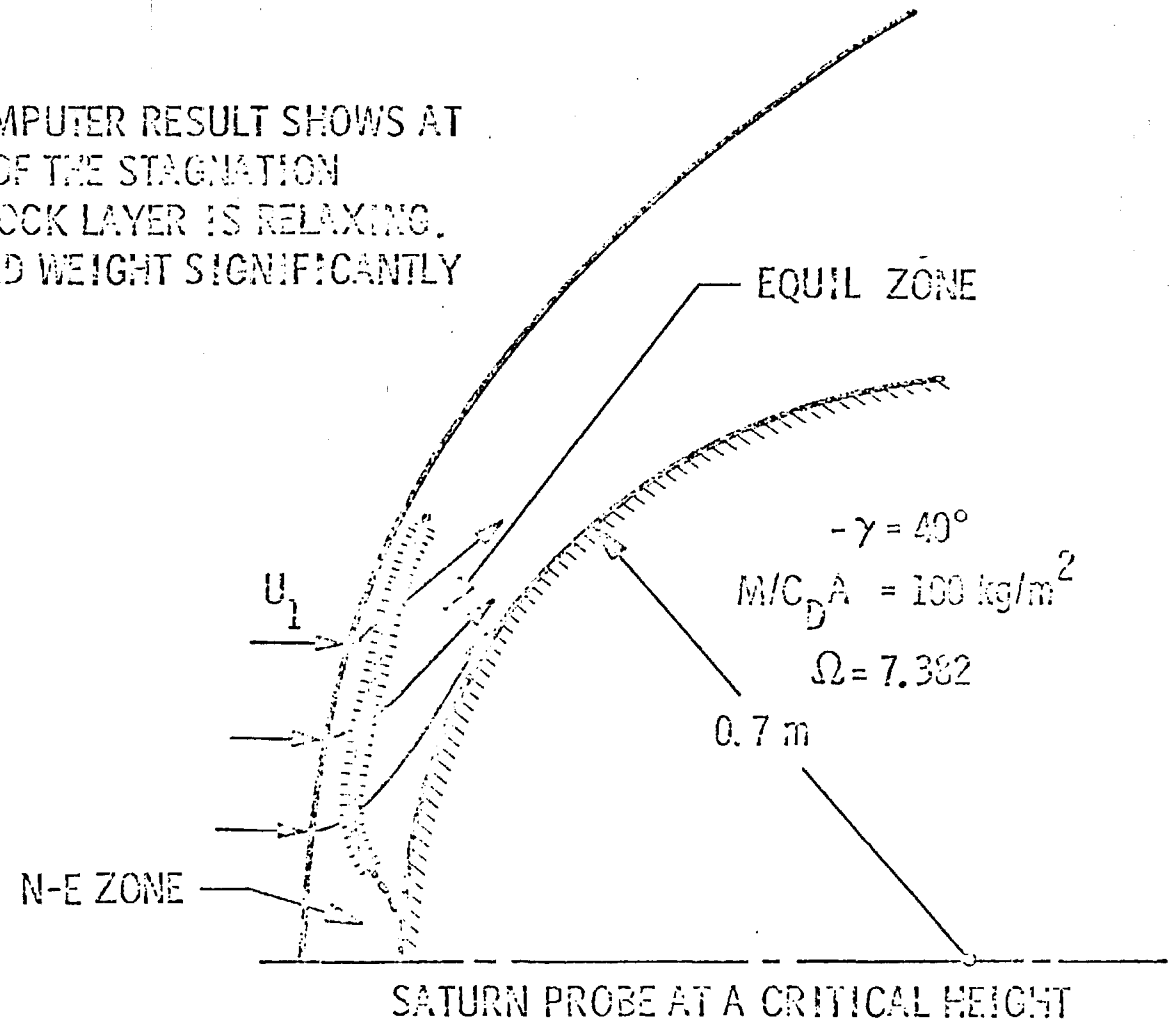


Figure 5-16

SHOCK LAYER ELECTRON CONCENTRATION

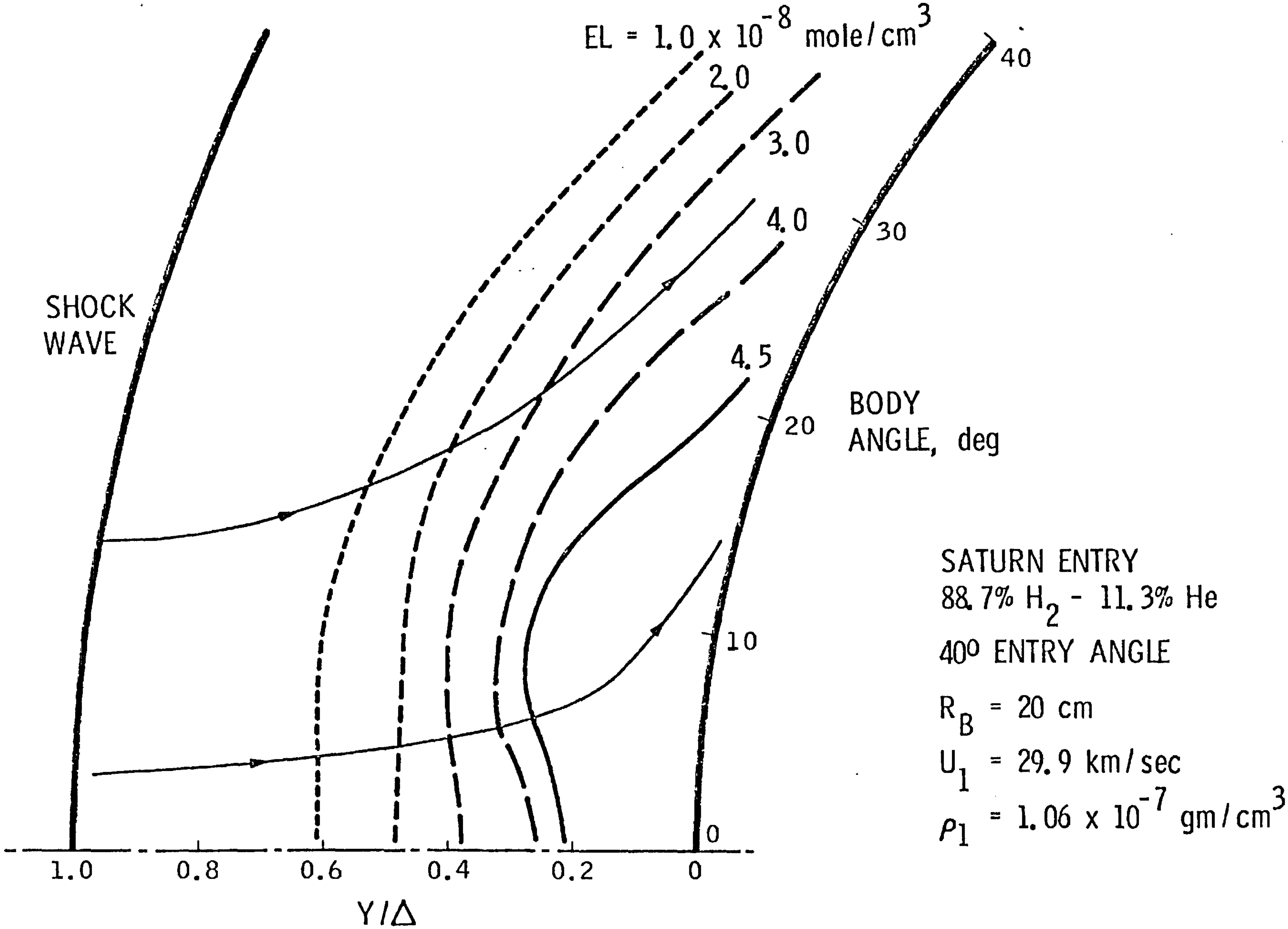


Figure 5-17

tration profile. The conditions are given on the right of the figure. We can see that electron densitites are plotted so that the first line is 10^{-8} grammoles per cm^3. In increasing order, the next line is 2×10^{-8} grammoles per cm^3; the next ones are three, four, and 4.5. Here the shock layer is enlarged out of proportion so that it will give more details of the profiles. Also, the shock layer thickness should increase as we go further down the streamline. Please note that the profiles are essentially parallel to the body. In other words, the gradient is, basically, normal to the body surface instead of along the streamlines.

Next, in Figure 5-18, we are going to bend the shock layer, pull this over so that the body line will be a straight line and then turn it 90°. That is a different representation. This one is a computation under identical conditions which gives the electron temperature within the shock layer. Again, the parameters are given on the right. The other parameters were already given in the previous figure. The body line is transformed into a straight line, and we see that because the shock layer thickness increases, the shock wave bends upwards as we go downstream. Now, regarding the electron temperature profile on which the radiative properties are dependent, we see 13,00°K, 12,000°K, 11,500°K and 11,000°K lines. Again, essentially, they are parallel to the body so the gradient is, basically, pointing towards the normal direction.

With these preliminary computations completed, we are going to talk about our longer-range studies (Figure 5-19). First of all we are going to compute in great detail the radiative flux to the boundary layer when radiative transport is important. This is being studied by Dr. Peter Poon. First of all, it is a non-gray gas and, secondly, he is going to use a tangent slab model. This is valid because the shock layer thickness is very small and, as we have just seen, the gradients of the profile are, basically, along the normal direction.

SHOCK LAYER ELECTRON TEMPERATURE

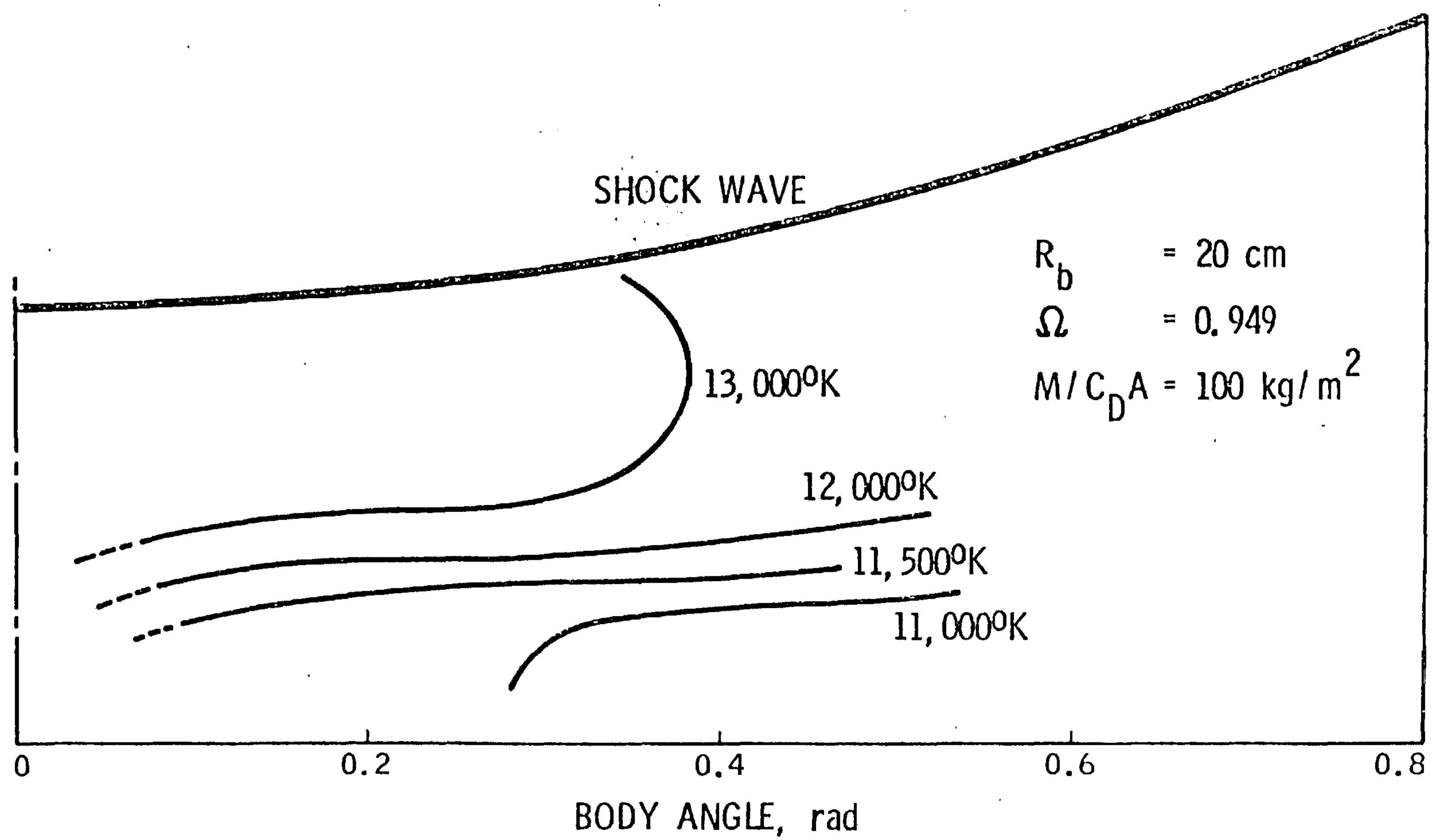


Figure 5-18

LONGER RANGE STUDIES

- DETAILED COMPUTATION FOR RADIATIVE FLUX TO THE BOUNDARY LAYER WHEN RADIATIVE TRANSPORT IS IMPORTANT (WITH Dr. Peter T. Y. Poon)

- INCORPORATE BOUNDARY LAYER STUDIES, SHOCK LAYER ANALYSIS AND MATERIAL RESPONSE INTO A UNIFIED COMPUTATION SCHEME

Figure 5-19

Secondly, we are going to incorporate, eventually, the boundary layer, the shock layer analysis and material response into a unified computation scheme. Gil Yanow of our group is now studying the boundary layer transition problem in actual experiments.

MR. SEIFF: Do you have a figure for the actual level of the radiative heating in this case where the probe energy is weaker than two percent? The reason for my question is that ordinarily when that number is small the radiative heating is not likely to be an overpowering thing and so I think that the conditions that you are relying on to perform your analysis automatically puts you into the range where the problem is not important.

DR. KUO: Yes. First of all, I don't have the figure with me, but it has been computed. Angus McRonald took the computation from George Stickford's previous isothermal slab computation. At peak heating, radiative transfer is of the same order as convective transfer.

MR. SEIFF: My point is that when the assumption is valid, the problem may be unimportant.

MR. OLSTAD: I think that is not the case here, because when you do compute one half ρU^3, you come out with a very large number. When you calculate the adiabatic heating rate, you come out with a substantial heating rate. You will see some numbers later when Bill Nicolet gives his paper. Dr. Kuo was just saying that under those conditions the cooling parameter is not a particularly large number.

MR. SEIFF: If I may, I would like to make one other comment, again harking back to the work of Bill Page, he discovered that even when the fraction is small, as for example, for Apollo, that the effect on the radiative heating can still be an interestingly large one; that is like, twenty or thirty percent reduction in the radiation even when the full energy fraction is as small as one or two percent.

MR. OLSTAD: Right. You have a significant amount of radiation from the ultraviolet where the optical pathlengths are short. A small radiation cooling parameter means that the cooling just has to take place close to the body. That is where the ultraviolet radiation comes from, and that is important.

Now, we are going to hear about Viking entry aerodynamics and heating. The problems of entry heating for Viking are not particularly severe but they do have to be predicted and there are some interesting aerodynamics that must be predicted. Bob Polutchko from the Martin Marietta Corporation will speak on Viking Entry Aerodynamics and Heating.

VIKING ENTRY AERODYNAMICS AND HEATING

Robert J. Polutchko
Martin-Marietta Corporation

MR. POLUTCHKO: Entry into the relatively thin Mars atmosphere is pretty straightforward compared to some of the more exotic outer planet entries you have been hearing about. Figure 5-20 describes the characteristics of the Mars entry including the mission sequence of events and associated spacecraft weights.

The Viking spacecraft is comprised of a modified Mariner Orbiter and the Viking Lander Capsule. The Mars Orbit insertion weight is 5189 pounds. After separation of the entry vehicle, the de-orbit maneuver is performed by a low thrust, long burn time (15 minutes) propulsive maneuver. This propulsion system is a mono-propellant hydrazine system that is also used for reaction control during entry. During the coast period (3 to 6 hours) after de-orbit, the entry vehicle is oriented to an angle of attack of -20 degrees in order to align several entry experiments with the free-stream velocity vector. I will describe the locations of the entry science sensors in a moment.

Atmospheric entry is arbitrarily defined as 800,000 feet and the entry vehicle weight is 2060 pounds. At 0.05 G's deceleration the entry vehicle reaction control is switched from pitch, yaw and roll attitude hold into a rate damping mode for pitch and yaw. The Viking entry vehicle flies a lifting trajectory so roll attitude hold is maintained to control the lift vector.

Parachute deployment is provided by the guidance and control system radar altimeter at 24,900 feet. Depending upon the atmosphere encountered the mortar fire Mach number will be between 0.6 and 2.1. The aeroshell/heat shield is aerodynamically separated 7.0 seconds after mortar fire. The terminal propulsion engines are ignited at 3565 feet above the surface and the parachute and base cover are separated 2.0 seconds after engine start. The terminal propulsion system is also mono-propellant hydrazine and the engines are differentially throttled for pitch and yaw

VIKING ENTRY THROUGH LANDING SEQUENCE

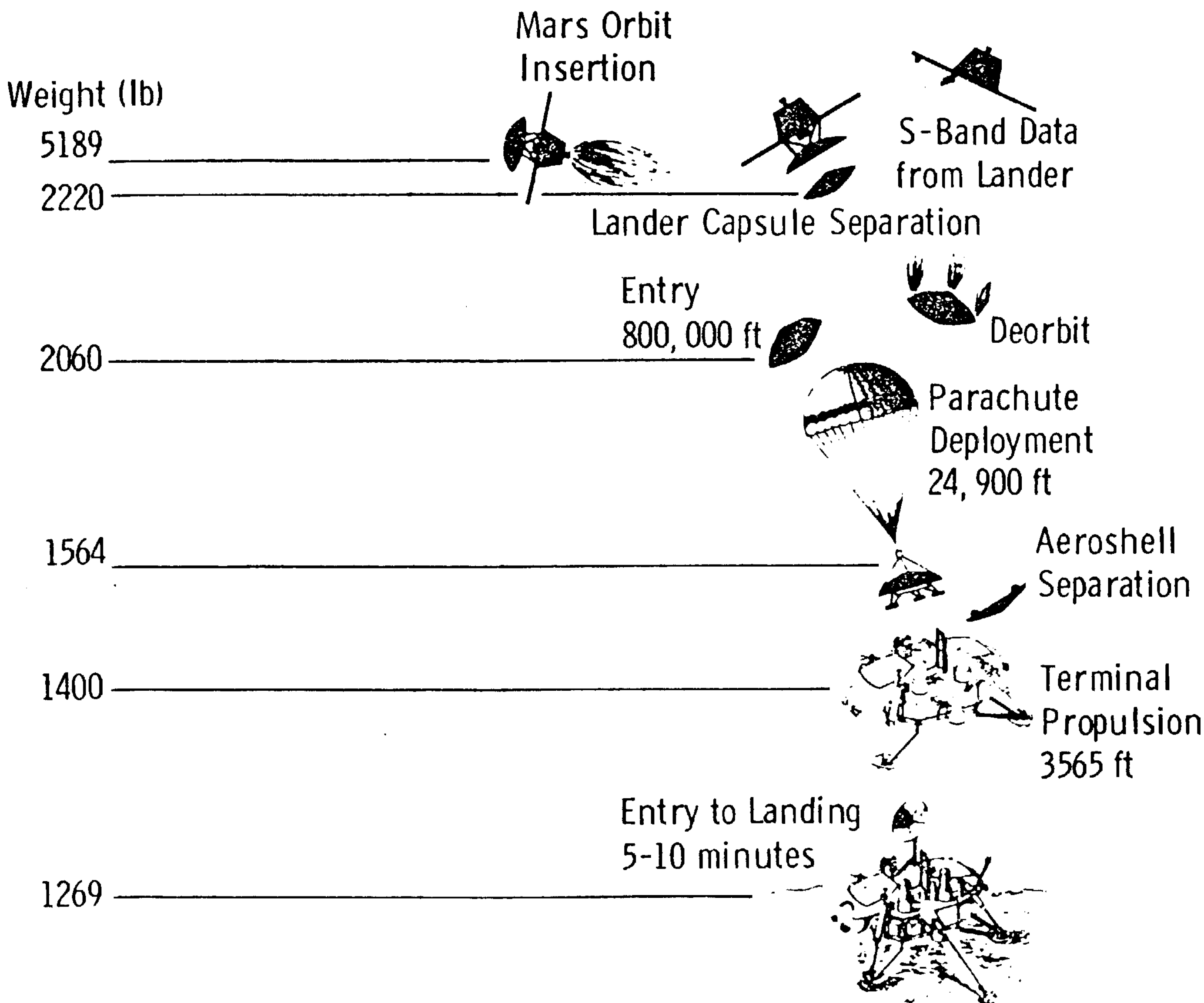


Figure 5-20

control. Roll control is provided by small roll engines mounted on the terminal propellant tanks. A constant velocity descent contour is reached above the Mars surface and the Lander engines are cut-off at surface contact. The touchdown velocity will be approximately 8.0 feet/second.

The Viking entry into a relatively thin atmosphere is critically dependent upon high drag. The configuration as shown in Figure 5-21 is a 140-degree included angle cone with a base cover. There was, of course, considerable concern with the aerodynamic stability of very high drag configurations but we will discuss the stability characteristics in more detail later. The entry configuration is eleven and one-half feet in diameter. On the windward meridian several entry science instruments are located - an upper atmospheric mass spectrometer, a retarding potential analyzer and the stagnation pressure port. A stagnation (recovery) temperature sensor is located on the leeward meridian and is deployed through the heat shield at a velocity of 1.1 km/second (Mach 4.0). We also have some engineering measurements located on the heat shield (four diametrically opposed pressure ports) and one base cover pressure port.

Sometimes the more simple points are overlooked. For a very blunt vehicle lift is obtained from the high axial force. The body force diagram is shown in Figure 5-22. In order to obtain a positive lift from the axial force, a negative angle of attack is required. The normal force is also negative but is a small contributor to the resultant lift vector. For the Viking configuration the lift to drag ratio is given approximately by -0.015α. For a c.g. offset of -1.84 inches the trim angle of attack is -11.2 degrees and the L/D is 0.18.

Figure 5-23 presents test data for the aerodynamic characteristics of the entry vehicle showing trimmed alpha, drag coefficient and trimmed lift to drag ratio versus Mach number. The MD requirements here refer to the mission definition requirements for

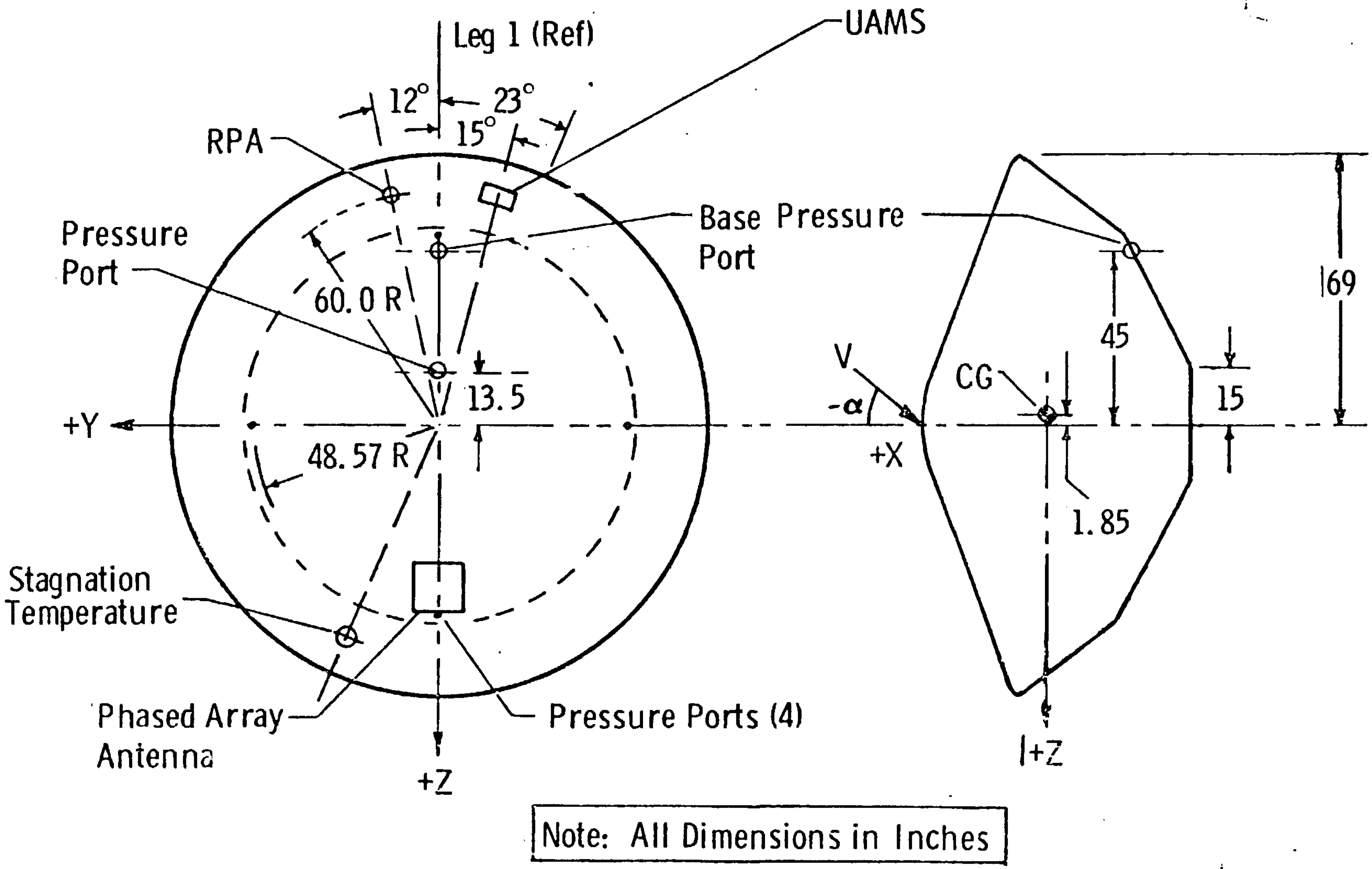
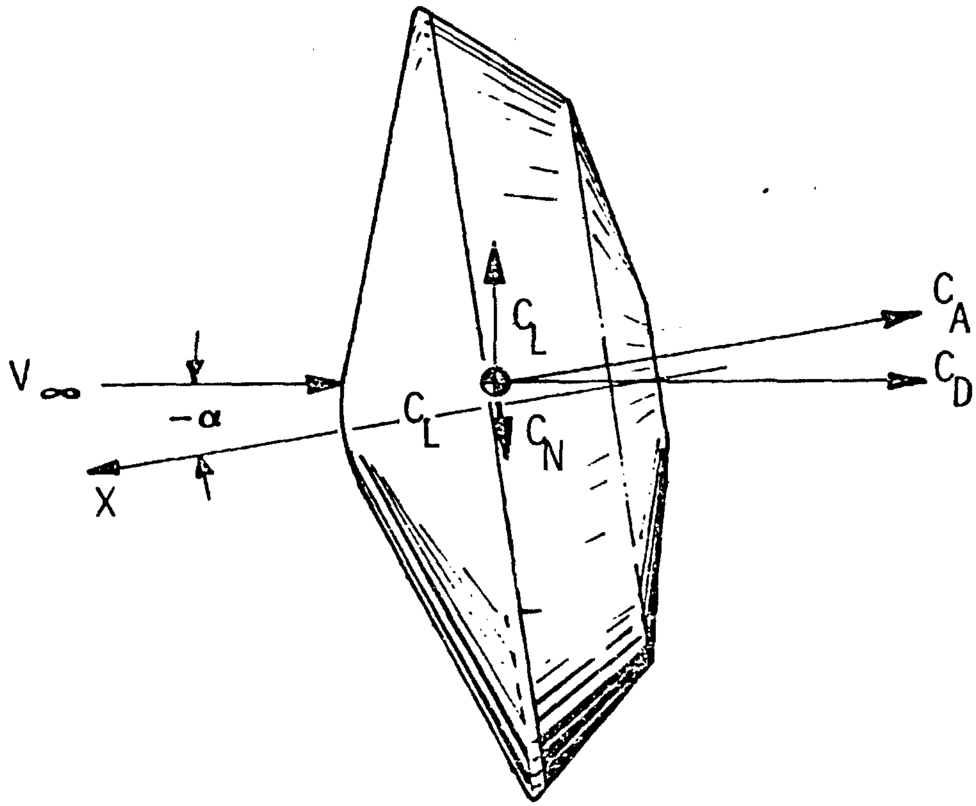

Leg 1 (Ref)
UAMS
12°
23°
15°
RPA
Base Pressure Port
Pressure Port
60.0 R
13.5
+Y
48.57 R
Stagnation Temperature
Phased Array Antenna
+Z
Pressure Ports (4)
V
-α
+X
CG
45
69
15
1.85
+Z
Note: All Dimensions in Inches

Figure 5-21

ENTRY VEHICLE BODY FORCES



$$\left.\begin{aligned} C_L &= C_N \cos\alpha - C_A \sin\alpha \\ C_D &= C_N \sin\alpha + C_A \cos\alpha \end{aligned}\right\} \frac{L}{D} \approx \frac{C_{L_\alpha}(\alpha)}{C_D} \approx -0.015\,\alpha$$

- Negative α Required for Positive Lift
- Negative α Obtained by CG Offset Above ℄

Figure 5-22

VLC WIND TUNNEL RESULTS

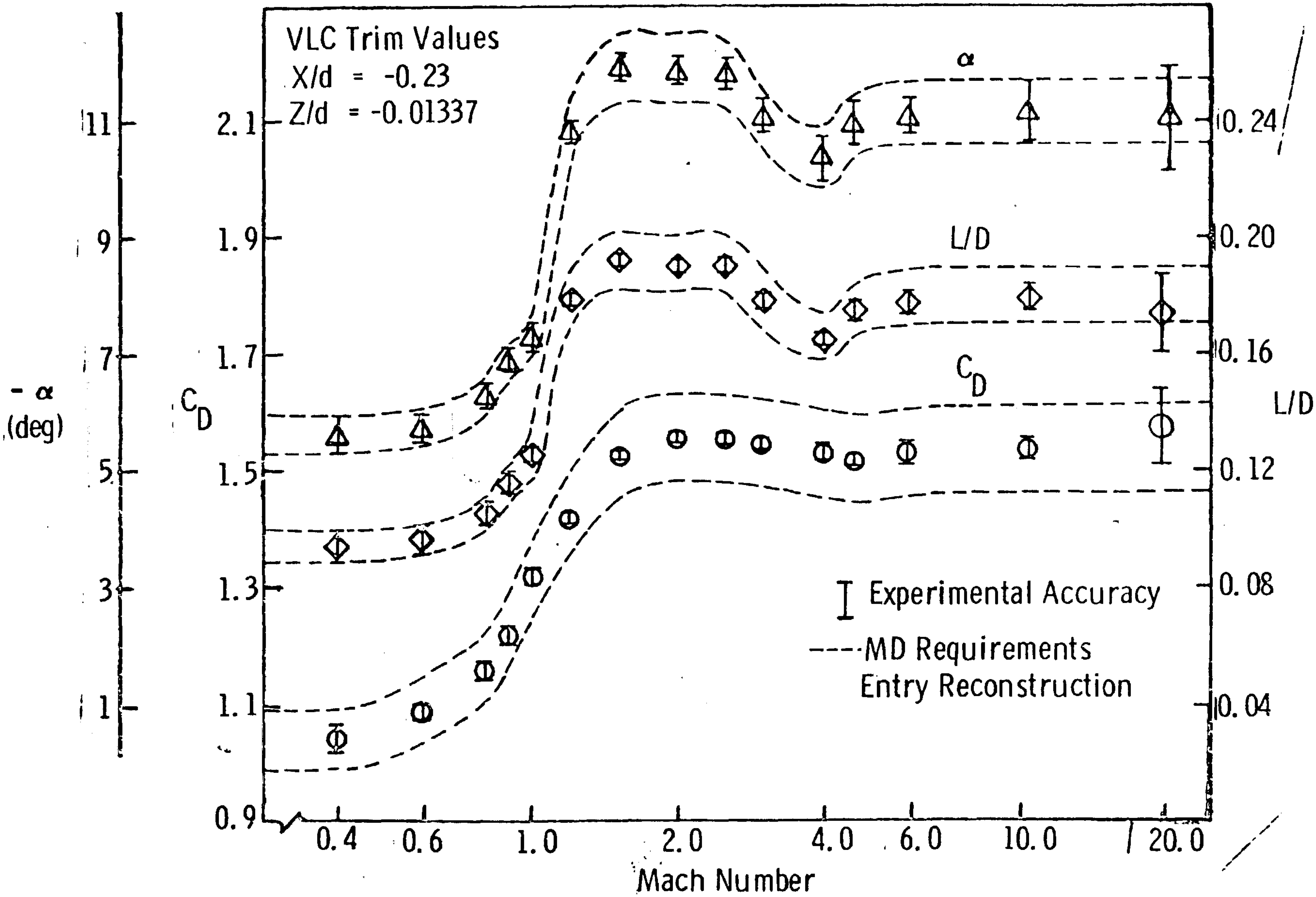


Figure 5-23

atmospheric reconstruction. The specification requires a priori aerodynamic coefficients within ± 5% and the test data certainly falls within the indicated tolerance. These test data were obtained using conventional wind tunnels and fairly straightforward testing technology.

Figure 5-24 shows the damping characteristics of the entry configuration. These data were experimentally derived utilizing forced oscillation and free oscillation testing techniques. This figure shows the basic negative damping at low angles of attack for very blunt configurations. The plots of C_{m_q} plus $C_{m_{\dot{\alpha}}}$ versus α and the same parameter versus Mach number show that there are two Mach numbers (about 1.2 and 2.0) where we have negative damping at low angles of attack. It should be noted, however, that for a trim angle of attack of -11.0 degrees that the Viking configuration has positive aerodynamic damping at all Mach numbers. Also note the relative insensitivity of longitudinal c.g. position on the pitch damping values.

Al Seiff (NASA/ARC) is currently in the process of obtaining ballistics range (free flight) test data for the Viking configuration. Comparisons of foreced and free oscillation data with the free flight data should provide additional assurance of the predicted vehicle motions.

On Figure 5-25 the angle of attack time history is shown for several Viking entries. Again the entry altitude is defined as 800,000 feet above the mean surface level. As I mentioned earlier, the nominal trim angle of attack is -11.2 degrees when Viking enters the sensible atmosphere. At the end of the long coast period following the de-orbit maneuver the guidance and control uncertainty (worst case) in angle of attack is ± 10 degrees. For entry science reasons we have a pre-programmed attitude hold mode prior to entry into the atmosphere. The angle of attack will be -20 degrees which orients the windward meridian directly normal to the velocity vector for the mass spectrometer and RPA data. In the worst case then, alpha could be either -30 degrees or close

PITCH DAMPING CHARACTERISTICS

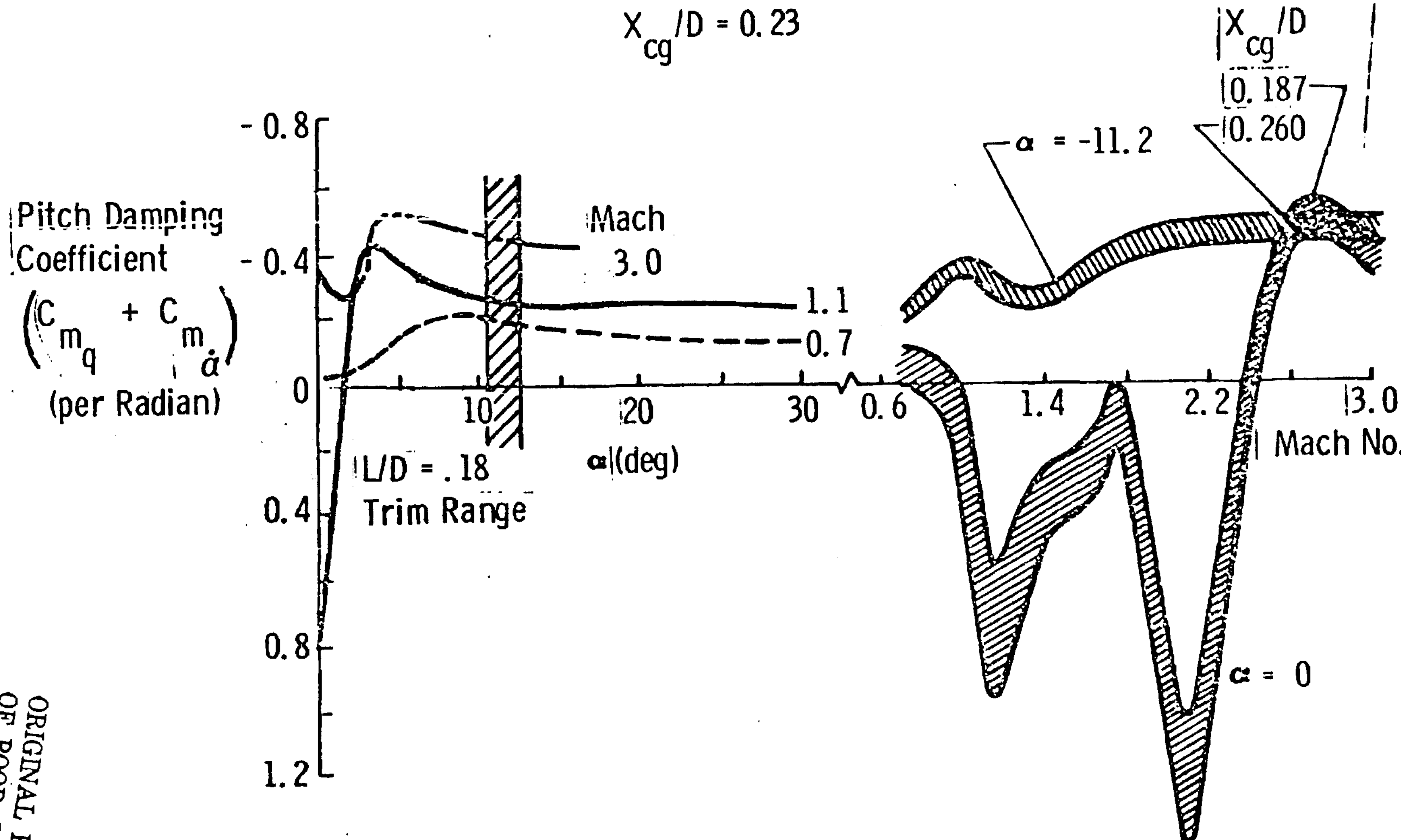
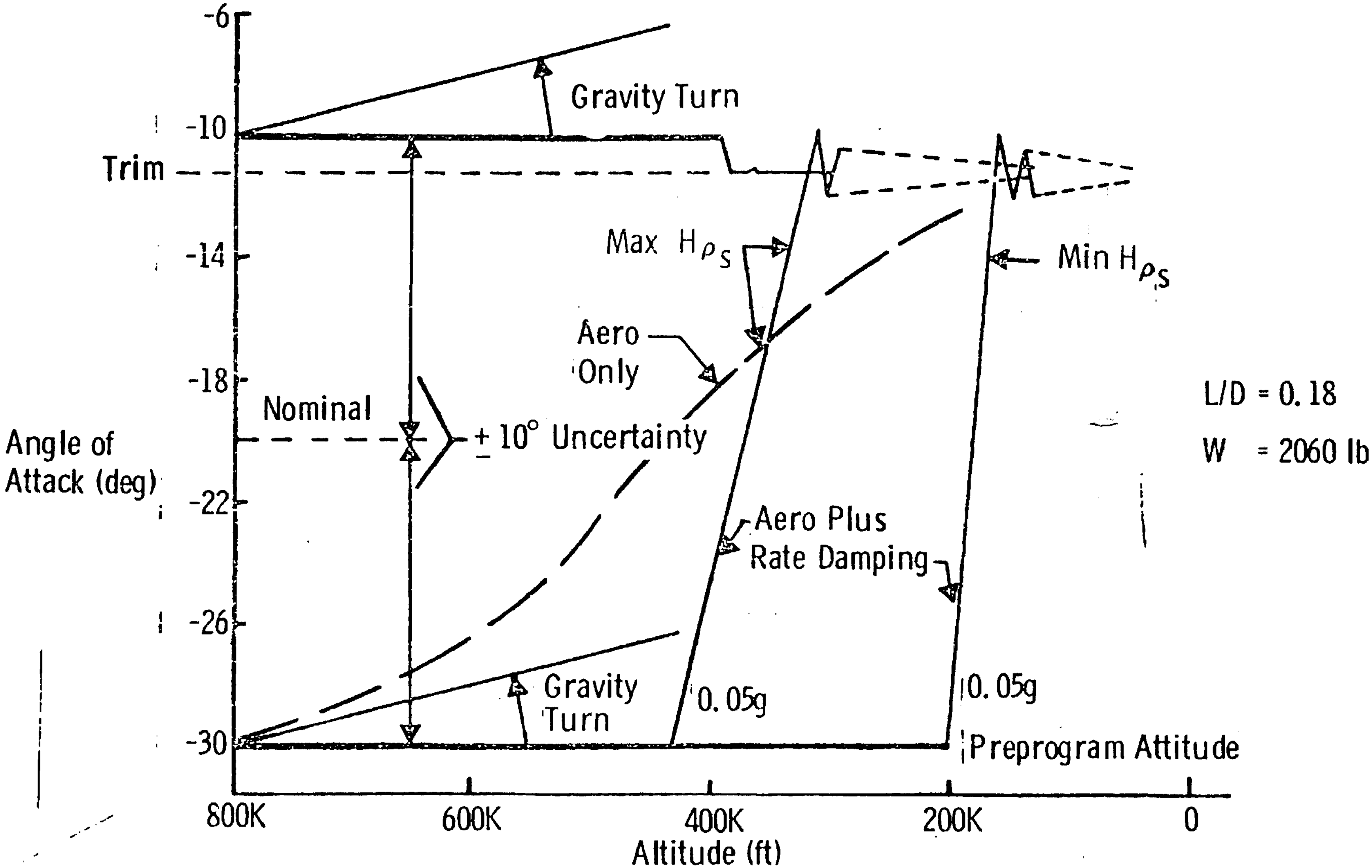


Figure 5-24



-6
-10
-14
-18
-22
-26
-30
Trim
Gravity Turn
Max H_{ρ_S}
Min H_{ρ_S}
Aero Only
Nominal
$\pm$ 10° Uncertainty
Angle of Attack (deg)
L/D = 0.18
W = 2060 lb
Aero Plus Rate Damping
Gravity Turn
0.05g
0.05g
Preprogram Attitude
800K
600K
400K
200K
0
Altitude (ft)

Figure 5-25

to the trim angle. Our discussion here will be limited to the -30 degree case.

A normal gravity turn will change the angle of attack as indicated. At 0.05 G's we switch to rate damping and combined with the natural aerodynamic damping characteristics the vehicle motion rapidly converges to the trim alpha. Shown on this figure are two atmospheric extremes and the convergence associated with only natural aerodynamic damping (i.e., reaction control system inoperative). It should also be noted that the reaction control system is operating in opposition to the aerodynamic damping forces in order to maintain the pre-programmed angle of attack. These engines are 4 pounds of thrust each (4 engines). After reaching the trim angle of attack maximum excursions due to spec gust profiles (20 meters per second) show maximum excursions of 3 degrees to vehicle attitude.

Figure 5-26 presents the relatively mild stagnation heating and pressure time histories. The curves are the worst case design limit values and represent atmospheric, entry angle and lift to drag ratio extremes. The stagnation heating values are calculated using a Newtonian pressure gradient and the Marvin and Pope correlation with real gas effects included. This relatively mild environment allows us to use very lightweight structures and heat protection and, therefore, the normal care of design and test must be exercised to provide a minimum weight entry vehicle.

Figure 5-27 presents the aeroshell heating distribution as obtained in tests run in the NASA Ames 42-inch Shock Tunnel for various gases. We also have obtained equivalent data in CF_4 at NASA Langley and in air at Cornell. The solid curves are our predictions of a heating distribution using the Aerotherm BLIMP C program. All our data and predictions have correlated quite well and an example of the agreement is given here. This high heating rates at the corner of the aeroshell are caused in part by the sharp radius - 1 inch full-scale. The differences indicated

DYNAMIC PRESSURE AND STAGNATION HEATING HISTORIES

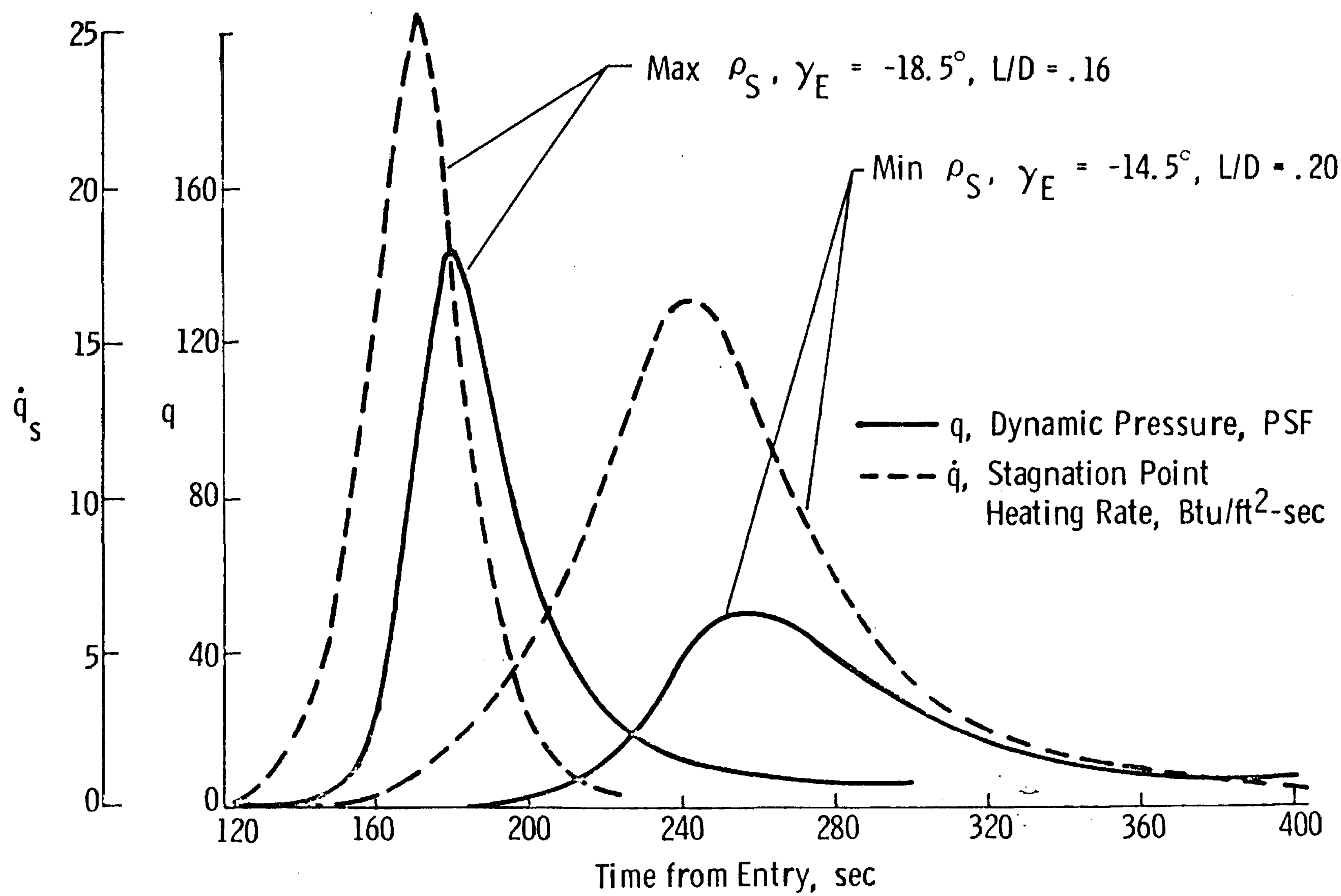
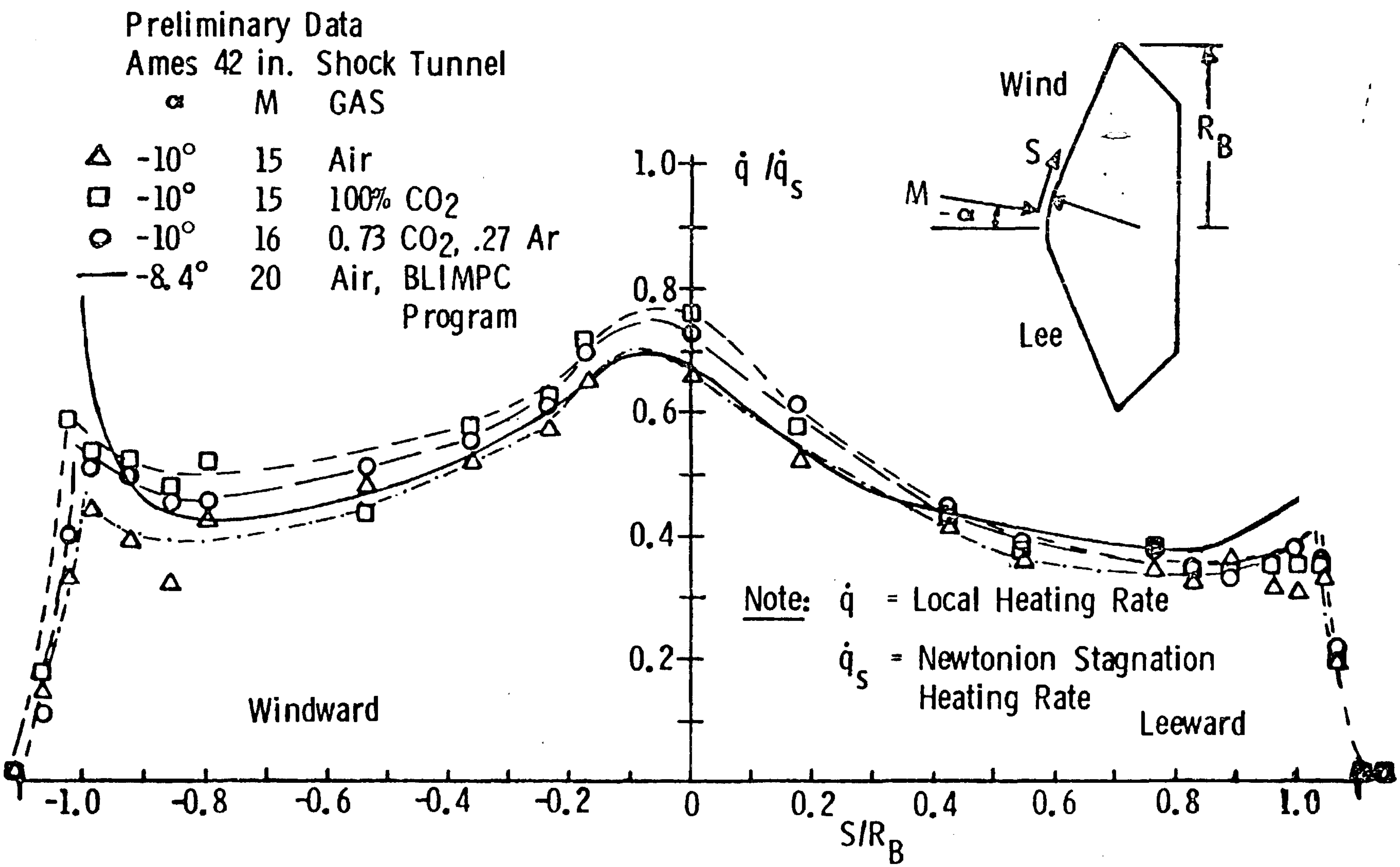


Figure 5-26



Preliminary Data
Ames 42 in. Shock Tunnel
α M GAS
△ -10° 15 Air
□ -10° 15 100% CO_2
○ -10° 16 0.73 CO_2, .27 Ar
— -8.4° 20 Air, BLIMPC Program
$\dot{q}/\dot{q}_s$
Wind
Lee
S
M
-α
R_B
Note: $\dot{q}$ = Local Heating Rate
$\dot{q}_s$ = Newtonion Stagnation Heating Rate
Windward
Leeward
S/R_B
1.0
0.8
0.6
0.4
0.2
-1.0
-0.8
-0.6
-0.4
-0.2
0
0.2
0.4
0.6
0.8
1.0

Figure 5-27

between the BLIMP C prediction and the data is test model peculiar. We have obtained data on a model constructed to emphasize specifically the instrumenting of the sharp corner. These data indicate that the BLIMP predictions shown here are accurate. These predictions here are based on the pressure distribution data from that special model and the heating rates indicated by the test data shown here are, in fact, in error.

On Figure 5-28 is presented some heating data from the Variable Density Tunnel at Langley at Mach 8.0 in air. Also shown are BLIMP laminer and turbulent heating rate predictions. The leeward side of the aeroshell seems to experience a transition to turbulence at Reynolds numbers between 3 and 4 million. We artificially tripped the boundary layer and experienced additional increases in the local heating rates which seem to show a good resemblance to the turbulent predictions. The Viking Reynolds number at the peak heating point in the worst case trajectory is about 3 million and the evidence seems to indicate that we could expect transition on the leeward side. This Reynolds number translates to a momentum thickness Reynolds number of about 140.

Precise transit criteria is not the point here since many factors influence determination of such a specification. However, this wind tunnel test, in fact, was a very close flight simulation for Viking and in the same facility Apollo tests showed remarkable correlation with flight test data. The Viking heat shield was designed to handle the situation indicated by these data. We also placed the entry science recovery temperature sensor on the leeward meridian to take advantage of the higher local Reynolds numbers at that location.

The curve of Figure 5-29 presents the design values selected for the heat protection system based upon all the test data and analyses we have performed. Basically, we have taken a conservative approach that calculates the expected heating rates in the

VIKING TRANSITION STUDY

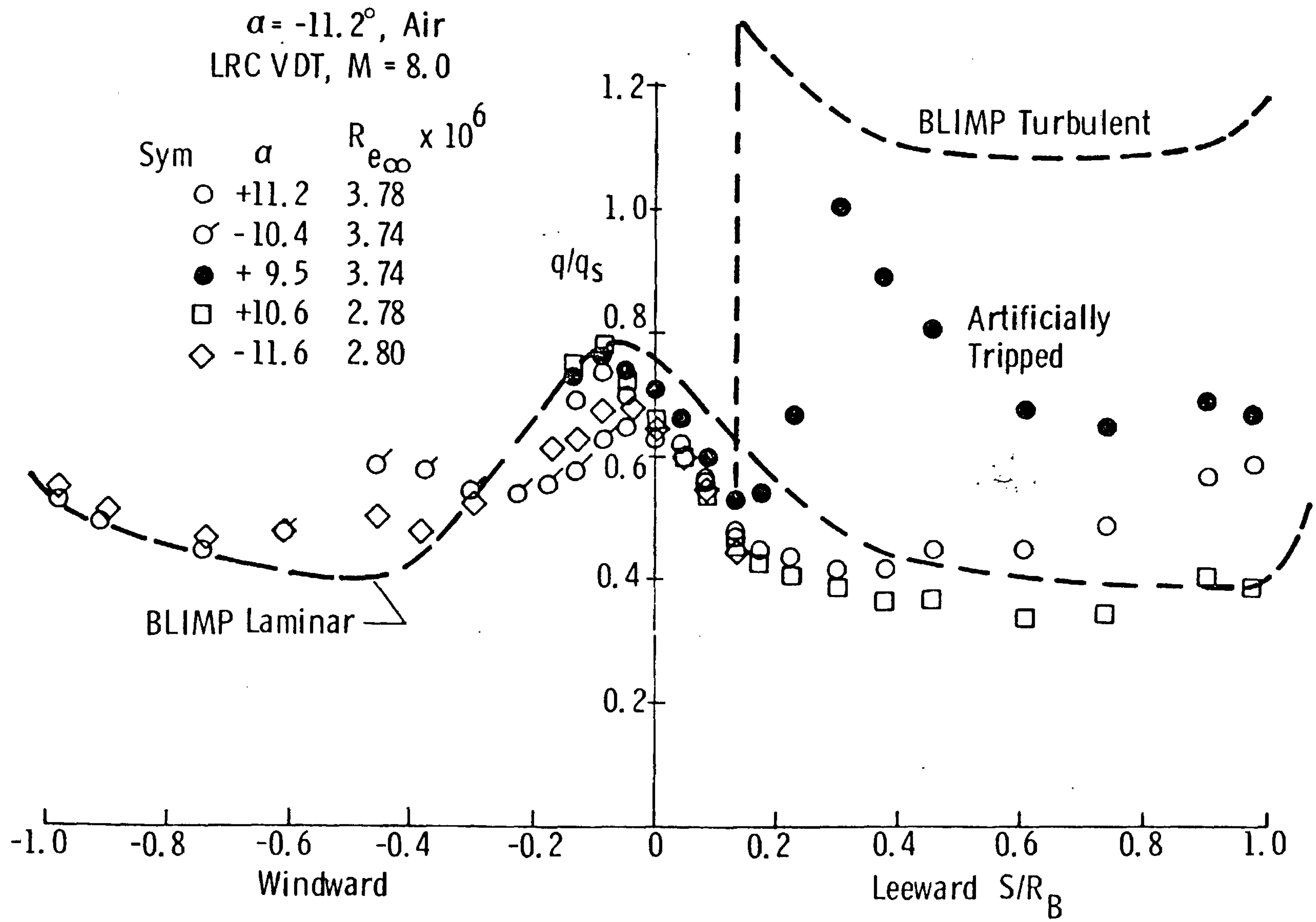


Figure 5-28

AEROSHELL DESIGN HEATING PITCH PLANE DISTRIBUTION

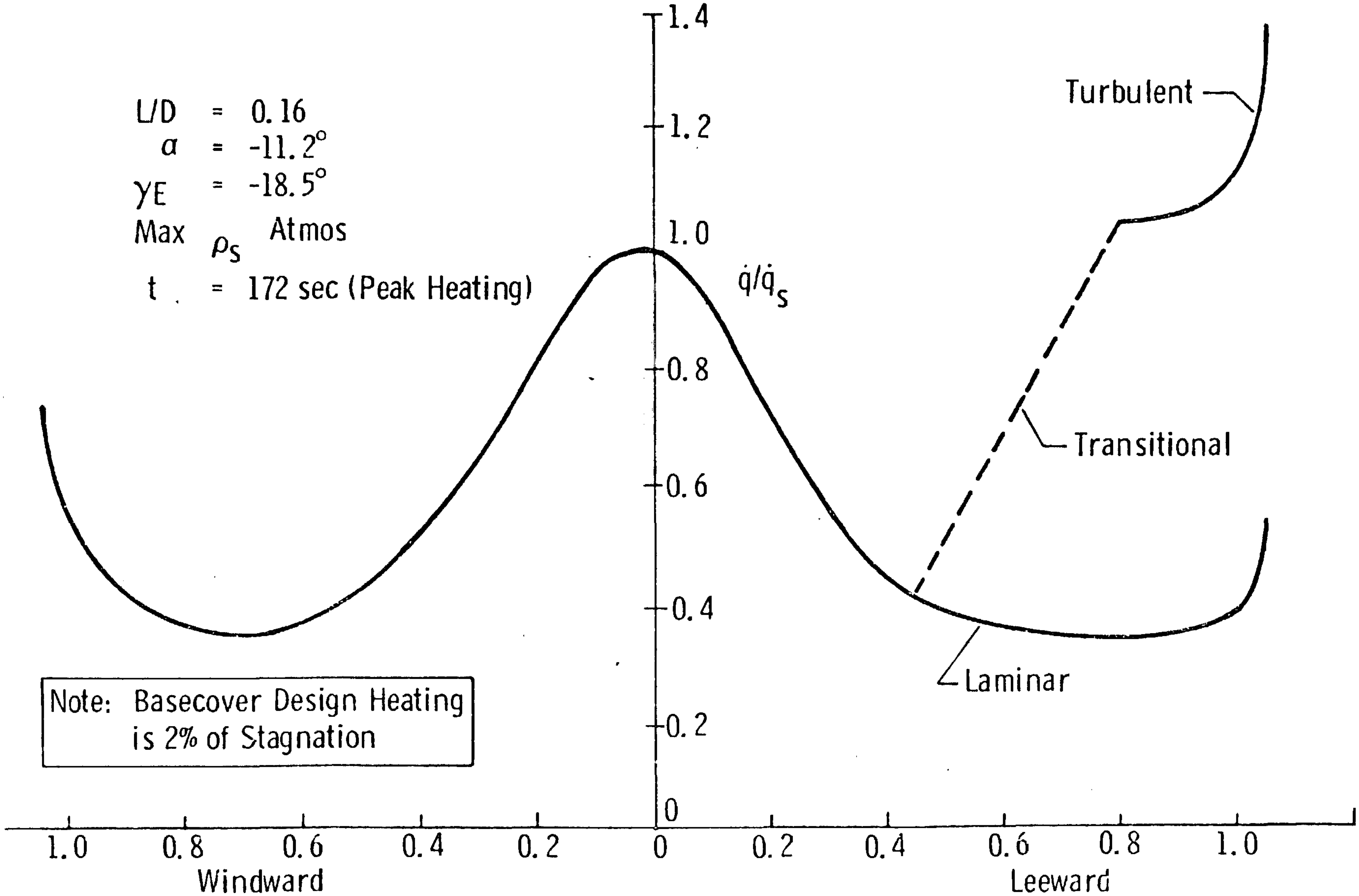


Figure 5-29

Mars CO_2 atmosphere by using a measured freon pressure distribution. The shock density ratio basically governs the pressures and the values for freon and CO_2 are very similar. For the turbulent areas we have modified the heating values using BLIMP rather than, for example, the Harris model at LRC. BLIMP gives a factor of three increase in this area while the Harris model shows about a factor of two. The stagnation area does not really experience a "Newtonian" stagnation heating rate but we have used the full stagnation value for design.

Figure 5-30 shows some test data we obtained on protuberances. The case shown here is the mass spectrometer cap which is potentially the largest if it failed to jettison prior to entry. The interference factor above the local "smooth" heating rate is plotted versus streamline direction. It can be seen that a factor of about 3.0 increase in heating rate could be expected. We have locally protected these areas with a high density ablative material that was previously flown on the USAF PRIME vehicle.

Figure 5-31 presents the real gas effects on the entry vehicle aerodynamics based on CF_4 data we measured at NASA-LRC and some preliminary data measured at NASA-ARC. You will note the slight increase in drag and the more non-linear nature of the pitching moment with alpha. However, the trim angle of attack for all three test gases is virtually the same for the Viking configuration at -11.2 degrees and the lift to drag ratio is virtually identical. We don't anticipate any problems for the lifting entry aerodynamic performance in the Mars atmosphere.

Figure 5-32 summarizes several of the design values and design factors for the Viking entry mission. The heat shield is basically an insulator and is, therefore, total heat rather than heating rate sensitive. The base cover is designed for 2 percent of stagnation heating based upon test data. The maximum base cover heating rate that was measured was 1.5 percent of stagnation. We have applied a design factor of 1.5 to all heating rates for smooth areas and a factor of 4.0 to all protuberances areas. Shear

PROTUBERANCE HEATING EFFECT ON AEROSHELL

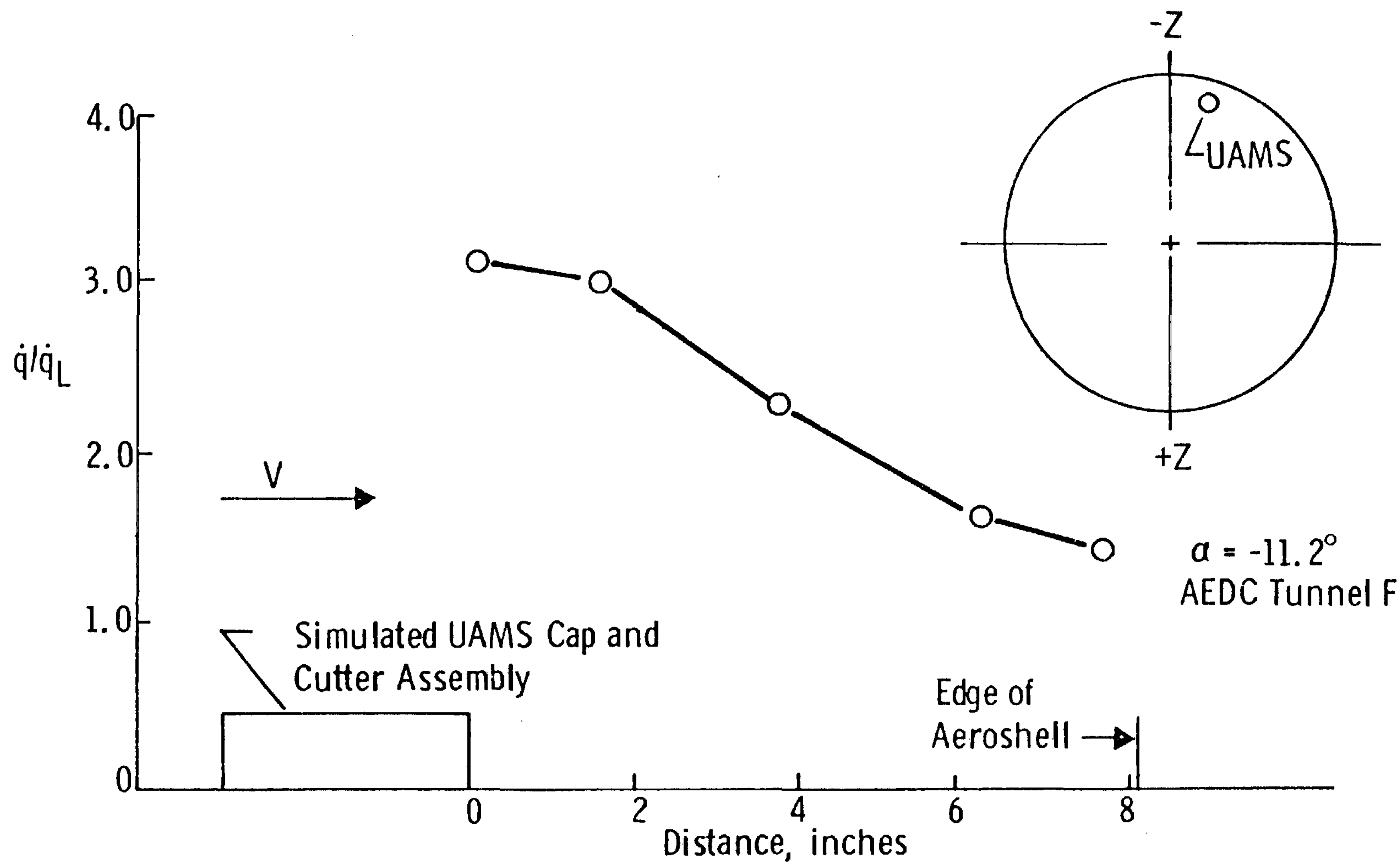


Figure 5-30

EXPERIMENTAL REAL GAS EFFECTS ON AERODYNAMICS

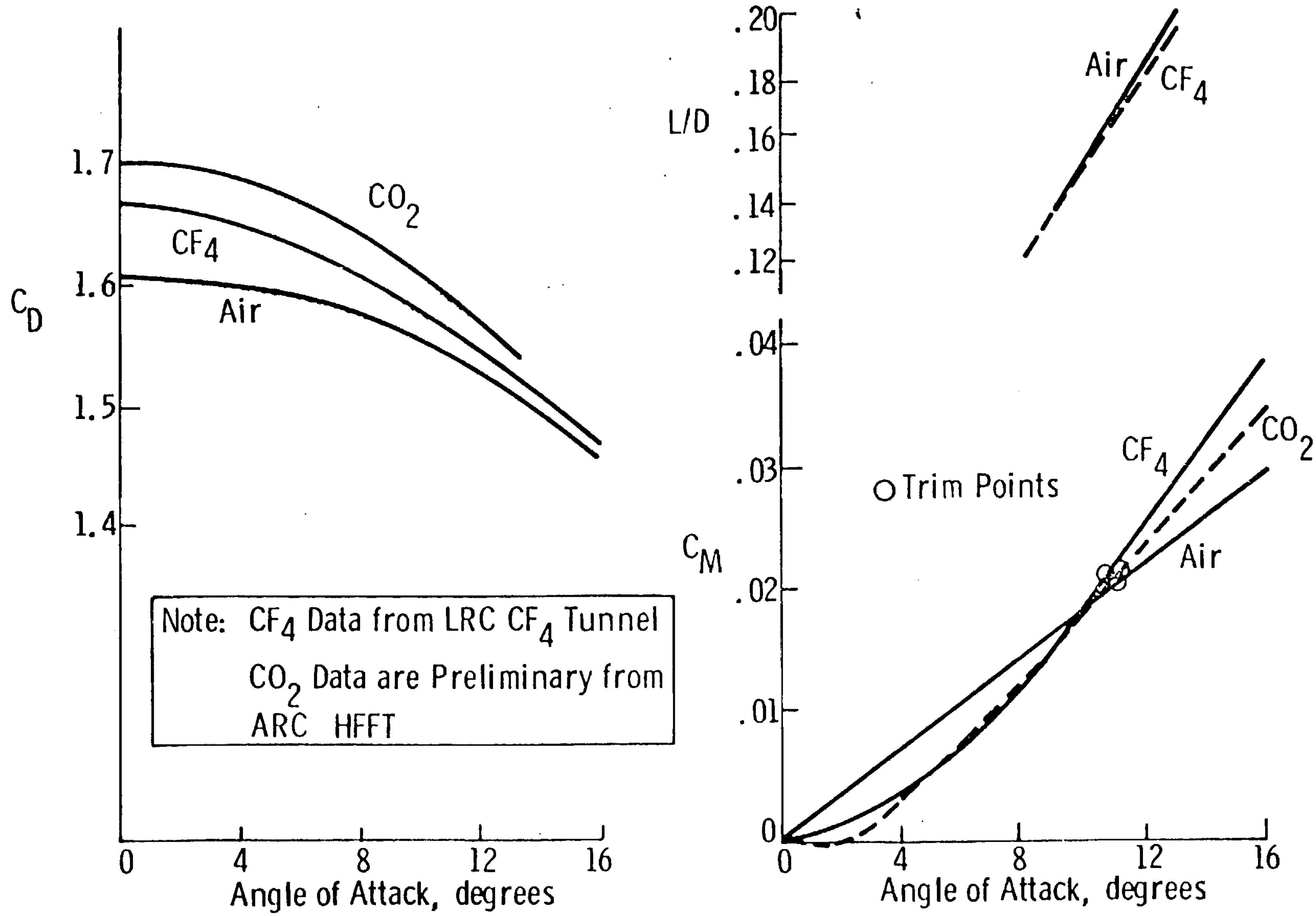


Figure 5-31

ENTRY DESIGN VALUES AND DESIGN FACTORS

PARAMETER	AEROSHELL	BASECOVER	UNITS
Heating Rate (Smooth)	25.6	0.512	Btu/ft^2 - sec
Total Heat	1240	24.8	Btu/ft^2
Max Shear Stress	2.6	0	PSF
Dynamic Pressure	144	-	PSF
Load Factor	12.7	12.7	Earth G
Collapse Pressure	280.0	16.5	PSF

DESIGN FACTORS	
Heating (Smooth Areas)	1.5
Heating (Protuberances)	4.0
Shear Stress	1.5
Collapse Pressure	1.25

CONSIDERATIONS

Atmosphere Extremes

$\gamma = -14.5^\circ$ to -18.5°

L/D = 0.16 to 0.20

W = 2060 lbs

V_E = 15,175 FPS

Figure 5-32

stress factor is 1.5 and aerodynamic loads factor is 1.25. These factors are applied to worst case combination of atmosphere model, entry angles and lift to drag ratios.

I would now like to show you a five minute film clip of the qualification flight test program of the Viking decelerator system, the Balloon Launched Decelerator Tests, BLDT. As summarized on Figure 5-33, the program consisted of four tests conducted at the White Sands Missile Range (WSMR) in New Mexico and were designed to span the extremes of the worst case conditions on Mars. These flight tests also demonstrated the aerodynamic separation of the full-scale aeroshell and the flying qualities of the entry configuration in an uncontrolled mode.

The parachute is a disk-gap-band configuration 55 feet in diameter, mortar deployed in a single stage with a mortar ejection velocity of about 100 feet per second. Tests were conducted at Mach numbers of 2.2, 1.2 and 0.5 and dynamic pressures of 14.5 and 4.5 pounds per square foot. The full-scale Viking test vehicle was carried to 120,000 feet by a helium filled, 34 million cubic foot balloon. The test vehicle was dropped from the balloon and rocket boosted to the test altitude and Mach number. All tests were successful and demonstrated a 35% structural margin above the worst case expected at Mars.

BALLOON LAUNCHED DECELERATOR TESTS (BLDT)

Five Minute Test Sequence Film

Four Decelerator Qualification Flights over White Sands Missile Range (WSMR)

Mach Numbers from 0.5 to 2.2

Dynamic Pressures from 4.5 to 14.5 PSF

Test Altitudes from 90K to 150K feet

Balloon Altitude 120K feet

Aeroshell Separations at Mach $\approx$ 1.0

Figure 5-33

CALCULATION OF DOWNSTREAM RADIATIVE FLOW FIELDS WITH MASSIVE ABLATION

G. Walberg
NASA Langley Research Center

MR. WALBERG: I would like to give you a rather broad-brush picture of the state of the art in radiative flow field calculations for downstream flows with massive ablation as viewed from the Langley Research Center. Why downstream flow fields? Well, that is where most of the heat shield weight is and that is also where our theoretical descriptions are the shakiest.

Let me quickly contrast the situation, as I see it, between the stagnation region analyses and the downstream analyses. Now, over the past several years a lot of people have done a lot of work on stagnation region radiative flow fields. A number of researchers now have developed analyses which appear to incorporate all the important phenomena. I don't mean to say that these stagnation point analyses have been verified as being correct; they have not. We don't have the experimental data to accomplish such a verification, but the analyses are self-consistent and do appear to account for the important phenomena as we understand them.

The downstream situation is a bit more complicated. In the first place, the gas dynamics of the problem are basically two-dimensional rather than one-dimensional. This means that the computer storage requirements and computing times are much greater than those required for the stagnation region. Most important of all, we have to consider the possibility, as we go from the stagnation point downstream, of transition to turbulent flow, which is probably the biggest single unknown in downstream radiative flow fields.

The first figure (5-34) shows some typical downstream radiative flow fields. I just want to point out the major characteristics. There are two bodies shown here: a 60° cone and a 45° cone. I have done this because the nature of the flow field and the problems that you encounter in the solution are very much dependent on the cone angle; in particular, the location of the sonic line in the inviscid flow. I will come back to that in a moment.

TYPICAL DOWNSTREAM FLOW FIELDS

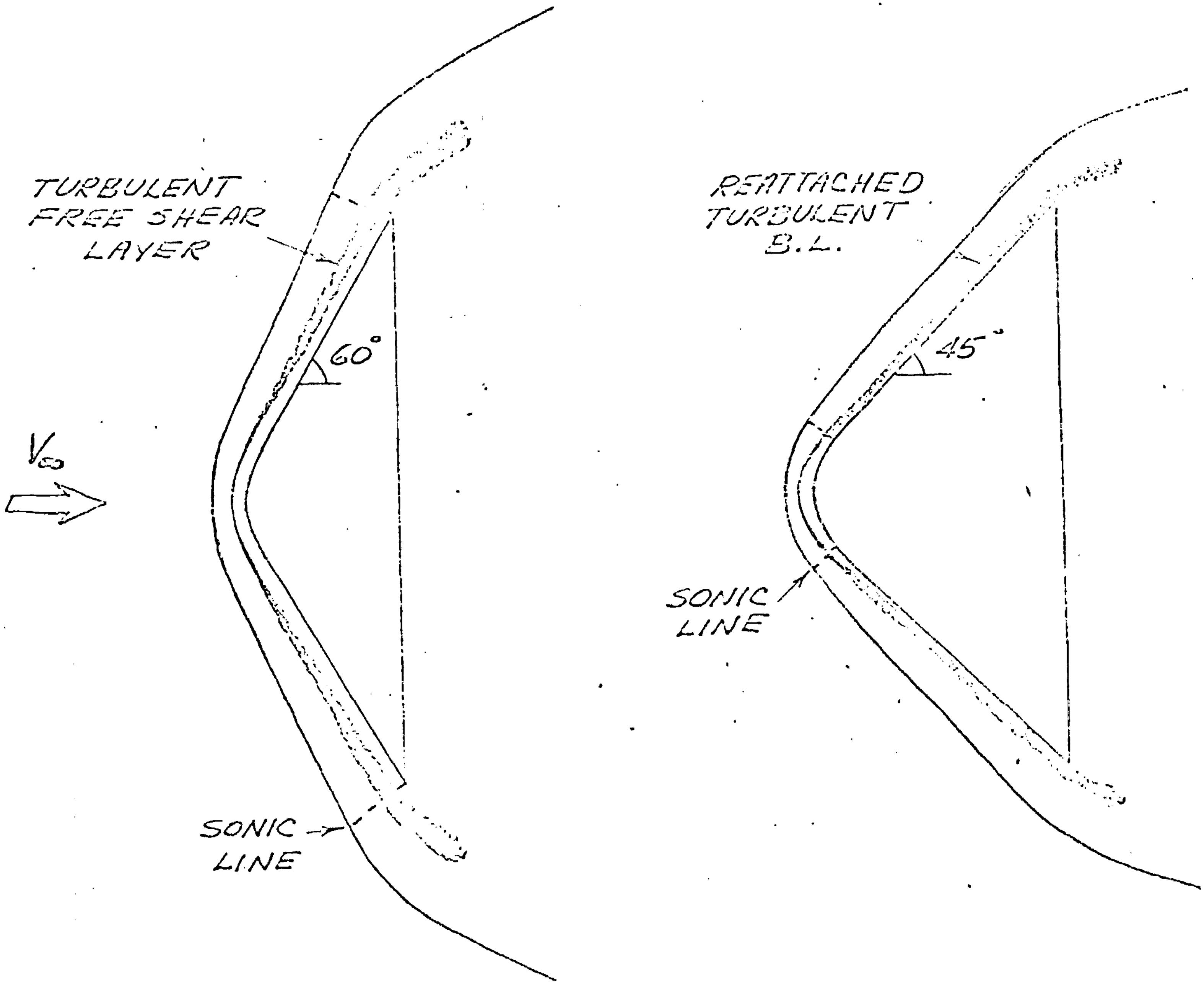


Figure 5-34

In the first place, we are talking about entry into the giant planets so the radiative heating rates are high. At the stagnation point we are dealing with massive ablation; so, rather than having an attached boundary layer in the normal sense, the ablation rates are sufficient to blow the boundary layer off the surface and we have, instead, a free shear layer.

As we progress from the stagnation point downstream, the question is: Will that initially laminar layer undergo transition to turbulence? Nobody really knows, of course. We don't have dependable transition criteria for this type of a mixing layer. Most people think the answer is "yes". So let's assume that it does undergo transition. Now, how fast will that layer grow in extent? Will it reattach to the surface of the vehicle? Or will it stay off the surface and just be dumped into the wake? This is important because there is a good likelihood, particularly for the Jovian entries, that this mixing layer will absorb a lot of the radiant energy coming from the inviscid shock layer and, so it will be carrying a lot of energy and it will be a turbulent layer. If it attaches to the surface of the vehicle the local heating rates could be very high.

What I've shown here is sort of a scenario of my guess at what will happen. If it's a 60° cone, our calculations of inviscid radiative heating rates say that the radiative heating will still be relatively high on the flanks. The ablation rates will be high and so, perhaps, the mixing layer will not reattach to the surface. For the 45° body on the other hand, the radiative heating rates - at least the inviscid rates - are predicted to drop off. So, the ablation rates on the flanks will not be so high and, in this case, perhaps there will be a reattachment of the free shear layer.

Finally, the question of sonic line location must be answered. For the 45° body the sonic line, at least in the inviscid part of the flow, will almost certainly be near the sphere-cone juncture. Most of the analyses that have been developed for downstream flows, so far, really handle this situation better than the one where the sonic line is near the aft edge of the cone. The worst situation you can be in, from an analytical standpoint is a cone angle where the sonic line is just on the verge of moving from the sphere-cone juncture to the base; and you can actually encounter the situation where, during an entry, the sonic line moves along the flank of the cone.

So, these are the important aspects of the downstream flow problem, as I see it. Now, let me describe two analyses that are presently under way at Langley. They are differing approaches, with different problems and promises.

On Figure 5-35 I have labeled these approaches as rigorous analyses. The intent is rigor; the result is far from being rigorous. We still can't account for everything that we know is important here. They are ambitious analyses. I have listed the characteristics of these analyses and, as you can see, they allow arbitrary, multi-component gas; a detailed radiation model is used; the intent is to include laminar or turbulent mixing layers; they do assume equilibrium, and this harks back to Lou Lebowitz' point. For these really detailed flow field calculations, nobody that I know of has been brave enough to include non-equilibrium chemistry in addition to all the other complicated phenomena.

The first approach is that by Ken Sutton. Here, the inviscid outer flow field is calculated using a time asymptotic solution and that's matched to a first-order boundary layer solution calculated along the vehicle surface.

The second approach, by Jim Moss, is a viscous shock layer analysis where the viscous shock layer equations are solved throughout the entire flow. Sutton's analysis, is to my knowledge, the only analysis that has been carried out to date where the radiatively coupled flow field all along the surface of a conical entry

RIGOROUS ANALYSES

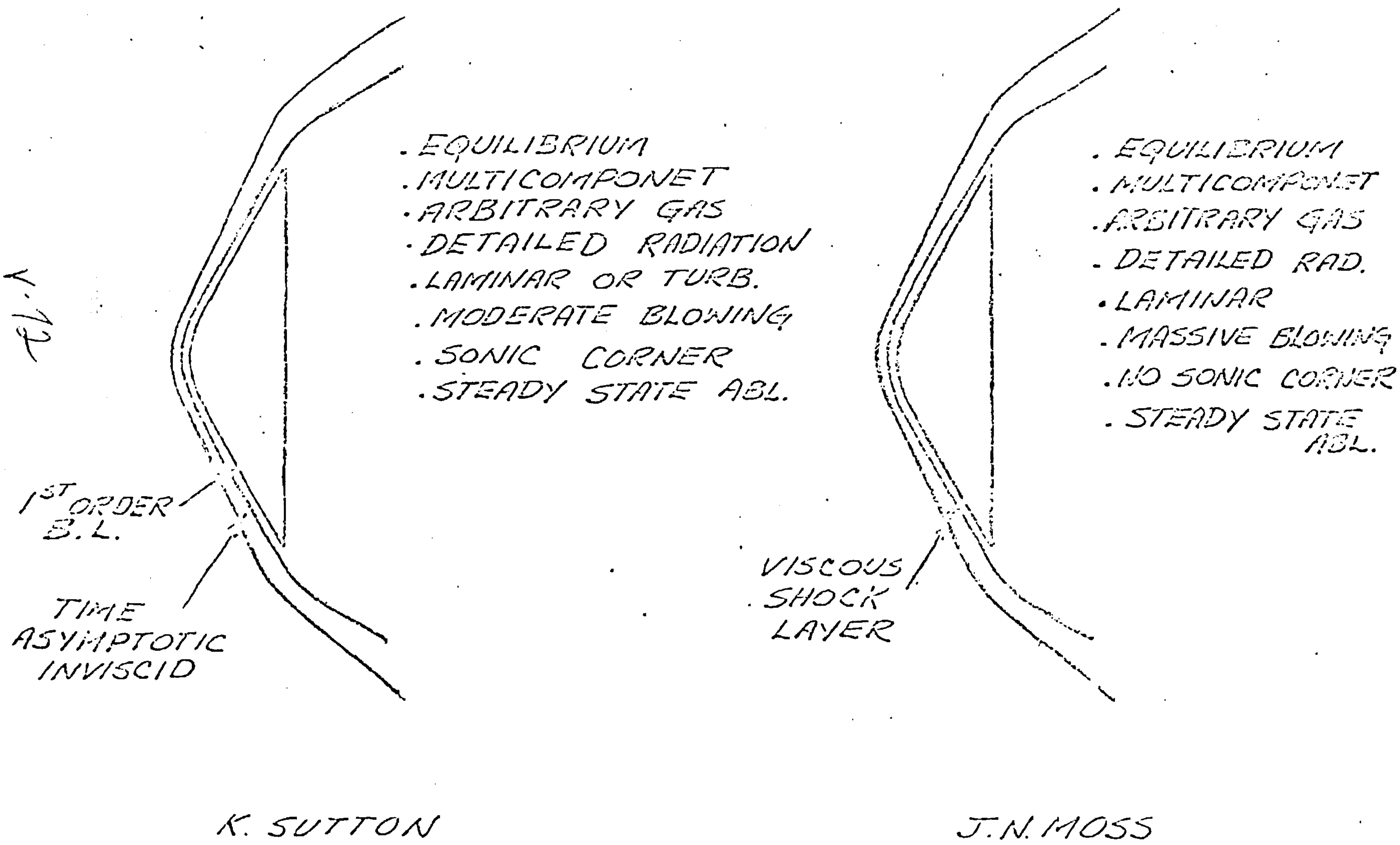


Figure 5-35

probe has been calculated with a turbulent boundary layer. Unfortunately, the boundary layer solution that is used in this analysis becomes unstable at massive blowing rates and, so, the analysis presently is limited to moderate blowing rates.

The viscous shock layer solution, on the other hand, has been demonstrated to be stable at very high ablation rates but, at the present time, it is only formulated for a laminar flow. Dr. Clay Anderson at Old Dominion University is in the process of incorporating various turbulence models into this viscous shock layer analysis but, at the present time, no results are available.

Let me show you some results from these two analyses to demonstrate their capabilities. I would point out that the results you will see will not be for the giant planets. You will see some results for Venus; you will see some results for Earth entry. The fact is there are no downstream rigorous analyses for the giant planets, yet. We are still working on them.

Figure 5-36 presents some of the results that Ken Sutton obtained for the large Pioneer Venus probe when it was assumed to be a 60° cone. This analysis is as far as I know the only one that's been presented with a detailed coupled radiative solution and a turbulent boundary layer. The solution is obtained for the entire surface of the conical vehicle. The solid line denotes convective heating; the dashed line denotes radiative heating. Transition was assumed at a momentum thickness Reynolds number of approximately two hundred.

Notice that there is only one curve for radiative heating. The reason for this is that the same answers were obtained for both laminar and turbulent boundary layers. This is sort of surprising but the next figure will clarify the situation.

What happened is illustrated in the plot of radiative flux to the wall presented in Figure 5-37. This is a spectral distribution of

SOLUTION FOR LARGE PIONEER VENUS PROBE

STEADY-STATE ABLATION OF CARBON – PHENOLIC HEATSHIELD

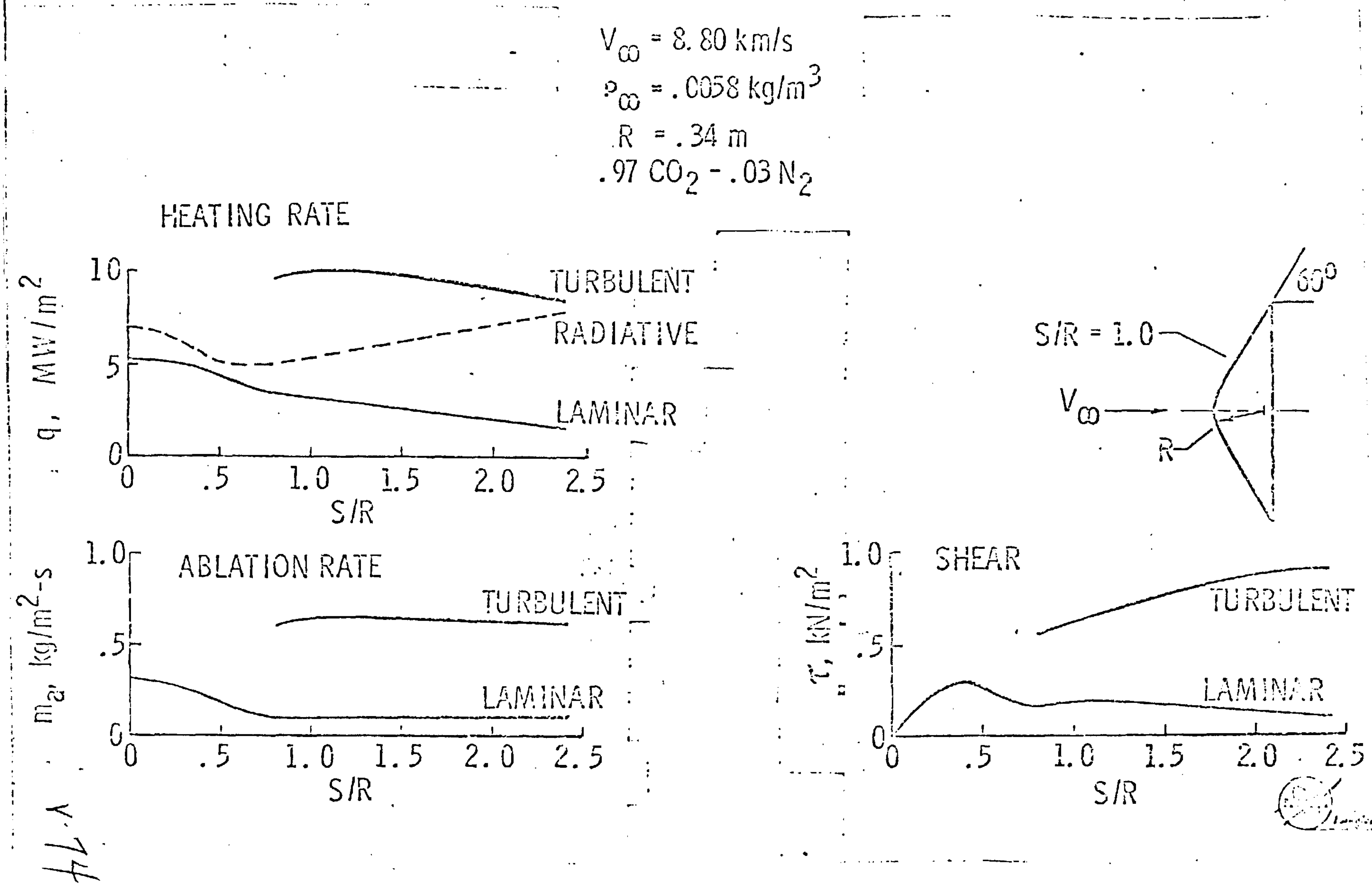


Figure 5-36

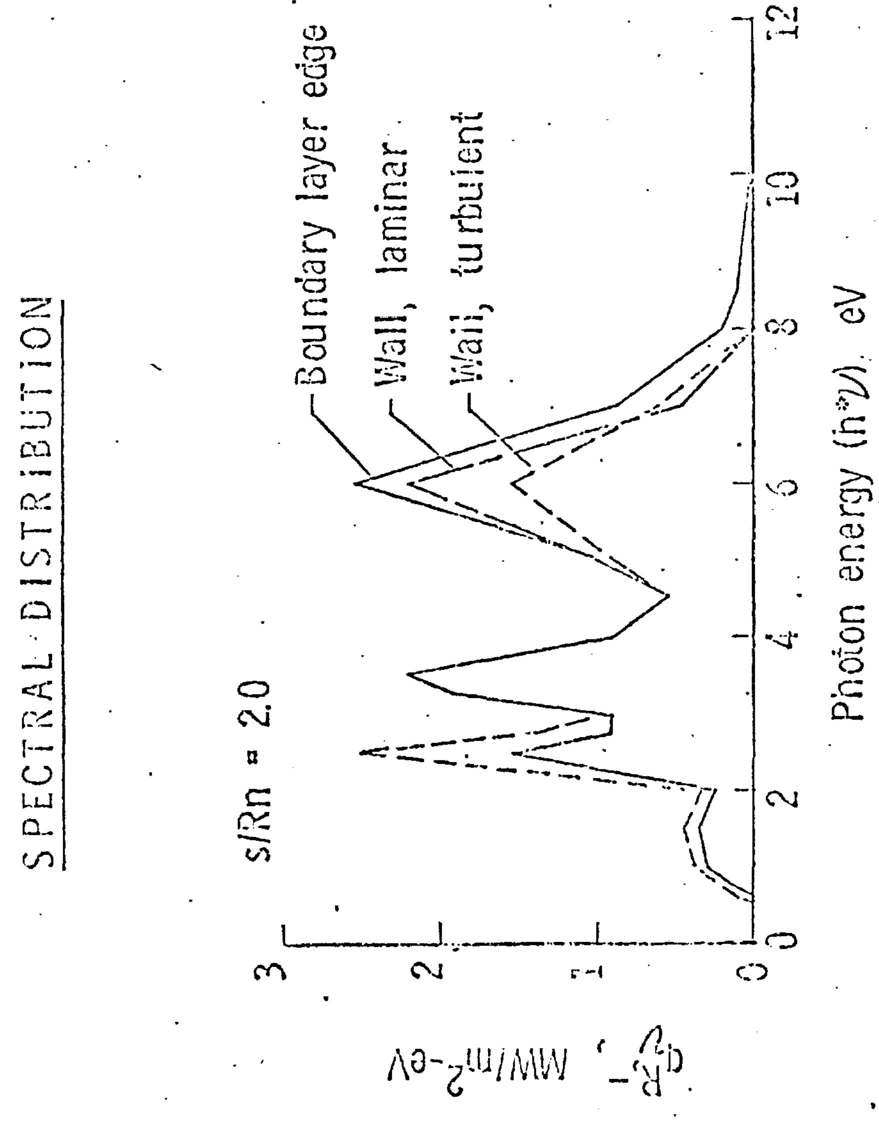

SPECTRAL DISTRIBUTION
s/Rn = 2.0
Boundary layer edge
Wall, laminar
Wall, turbulent
Photon energy (hν), eV
MW/m²-eV
0
2
4
6
8
10
12
0
1
2
3

Figure 5-37

radiative flux as a function of photon energy. The solid line indicates the flux to the outer edge of the boundary layer. The long-dash line is the flux to the wall when the boundary layer was laminar and the short-dash line is the flux to the wall for the turbulent boundary layer. For the laminar boundary layer there was some absorption at uv wavelengths from five to eight eV. When the boundary layer was turbulent there was more significant absorption in this range but, in addition, there was emission in the visible and IR end of the spectrum. It is just a coincidence that the two cancel each other in this case, yielding virtually the same answers for laminar and turbulent boundary layers. These results show significant differences in the spectral distribution of radiative heating depending on whether the boundary layer is laminar or turbulent, and I feel that, in general, you should expect differences in the magnitude of the frequency-integrated heating as well.

Now, a couple of viewgraphs to demonstrate the capabilities of the viscous shock layer solution of Jim Moss. As I said, Sutton's solution is presently limited to moderate blowing rates, so we can't really tackle the giant planet entries with it. Figure 5-38 presents some stagnation point results that Jim Moss obtained for earth entry. These are temperature distributions through the complete layer - both what amounts to a boundary layer and the inviscid layer - for various dimensionless ablation rates. The highest value of this dimensionless ablation rate that Sutton has managed to get a solution for is approximately 0.2. Here you see answers for 0.6 which really is massive ablation; and yet the viscous shock layer solution did remain stable and give answers for this case. It promises that if we can incorporate all the other phenomena that we would like to account for, perhaps this approach will handle the massive blowing.

Figure 5-39 shows some downstream solutions that Jim Moss obtained for an Earth entry case with the viscous shock layer solution. Basically, what this shows is that the thing does, indeed, calculate

STAGNATION TEMPERATURE PROFILES FOR VARIOUS INJECTION RATES

ALT = 62.2 km; $U^*_\infty = 15.24$ km/s; $T^*_S = 14{,}700$ K, $P^*_S = 0.5$ atm; $R^*_N = 0.305$ m

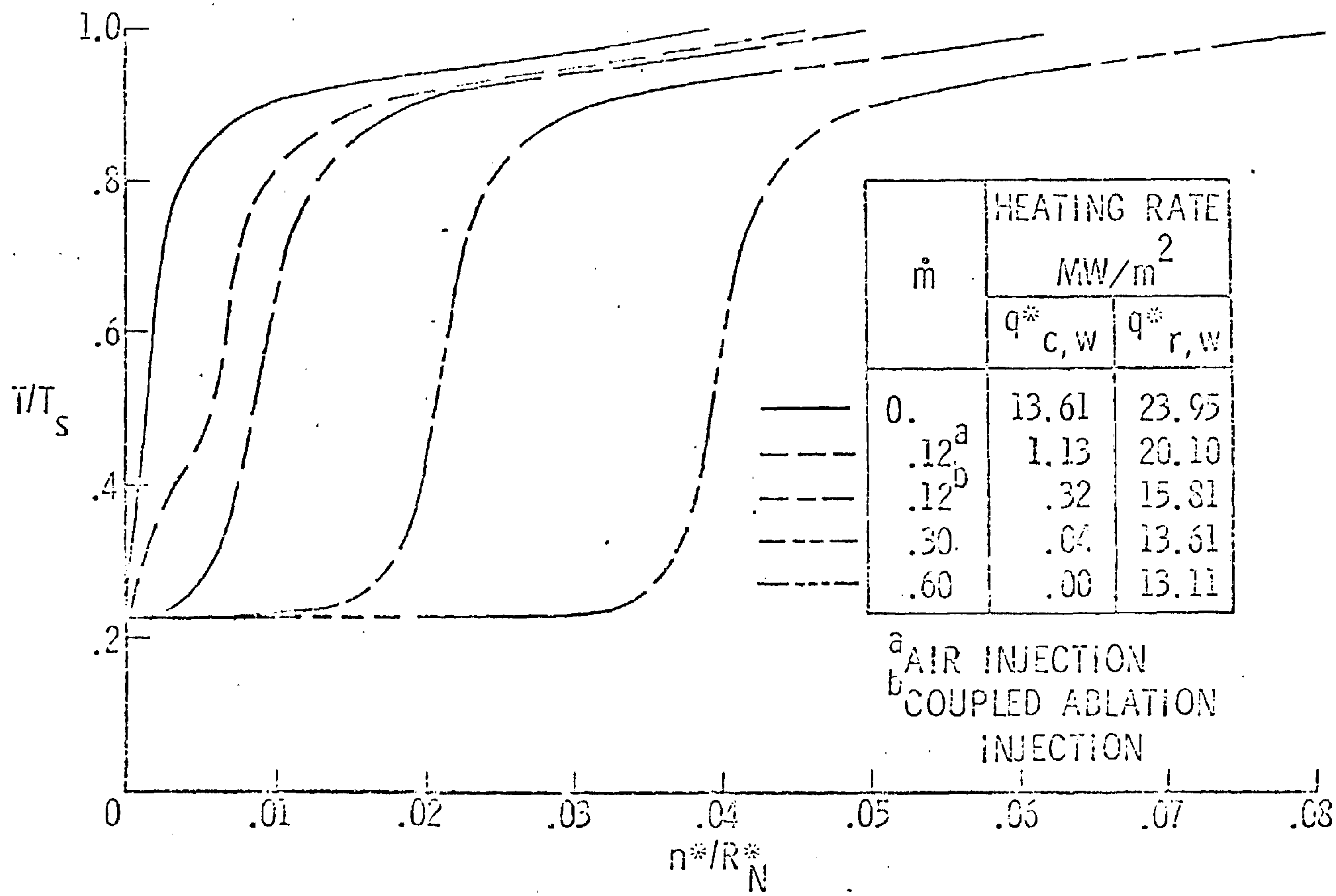


Figure 5-38

RADIATIVE VISCOUS SHOCK-LAYER RESULTS WITH COUPLED ABLATION INJECTION

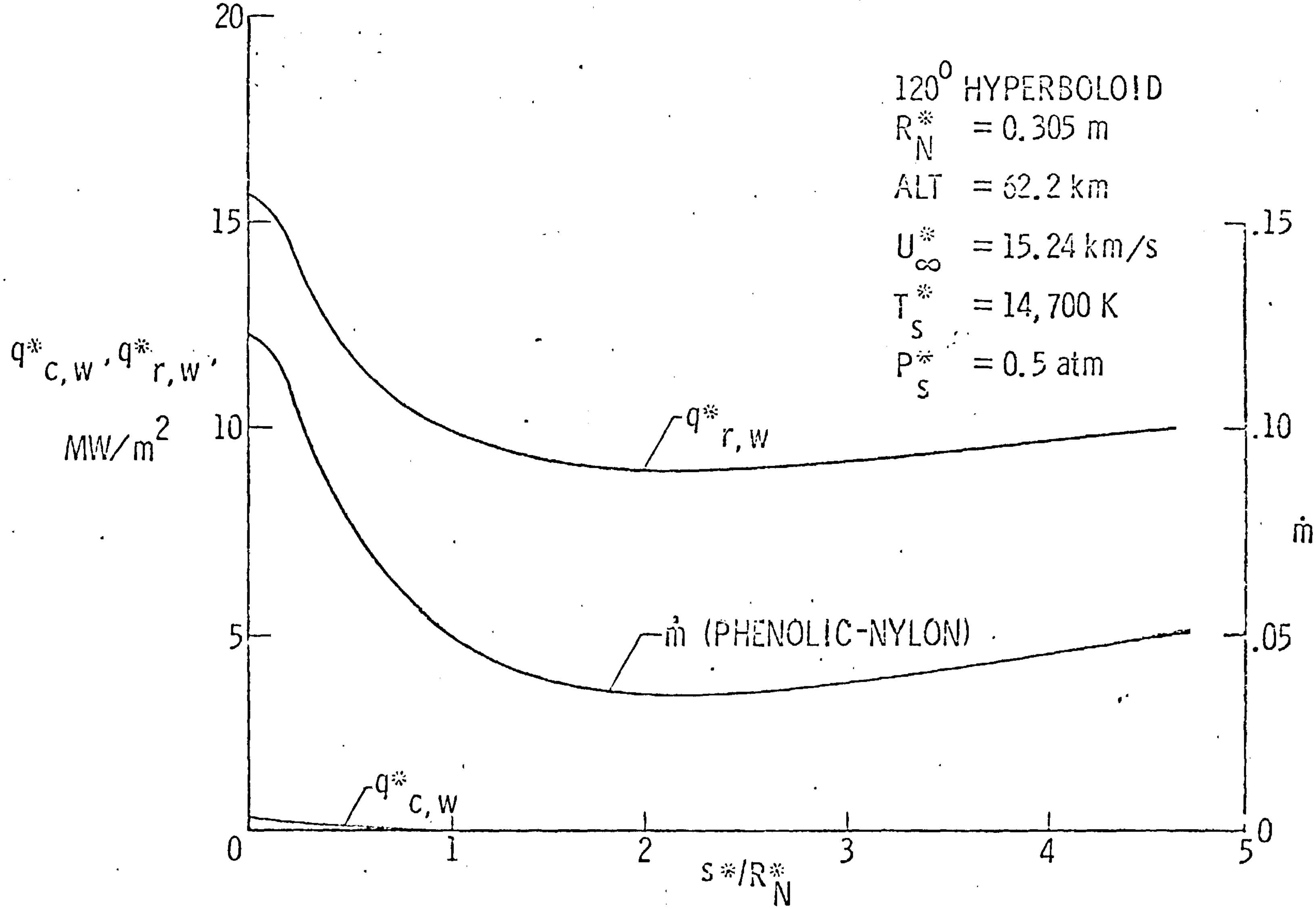


Figure 5-39

all the way around the body and this is a radiatively coupled downstream solution; albeit for Earth entry, and a laminar boundary layer.

The biggest shortcoming for both of these analyses really is the description of the turbulent mixing layer. While Sutton has obtained answers for the turbulent mixing layer, it really amounted to an attached turbulent boundary layer and, in this case, we have turbulence models that we can use with some confidence. For the massively blown free mixing layer I'm not sure anybody knows what the proper turbulence model is. This is really the big thing that we need to know. We need a turbulence model that we can include in these flow field analyses with some confidence.

Even if we have the turbulence model, and if we include all the other good things that we have to in these detailed rigorous solutions, the computing times required are still going to be so large that I doubt we will ever use them for parametric studies or mission analysis studies. So, there is a need for an approximate solution and there is a real possibility that you can develop an approximate solution if you have a detailed solution to sort of calibrate the approximate solution with.

Figure 5-40 shows a couple of approaches that have been taken at Langley toward producing these approximate solutions. The first is due to Walt Olstad. It's a two inviscid layer model, really most applicable to the massively blown situation where a Maslen-type inviscid flow field is assumed in both layers. The second is an approach due to Louis Smith where a one strip method of integral relations approach is used in the outer inviscid layer and a simplified integral boundary layer solution for the inner layer.

Here, again, the location of the sonic line starts to be important because at its present state of development, anyway,

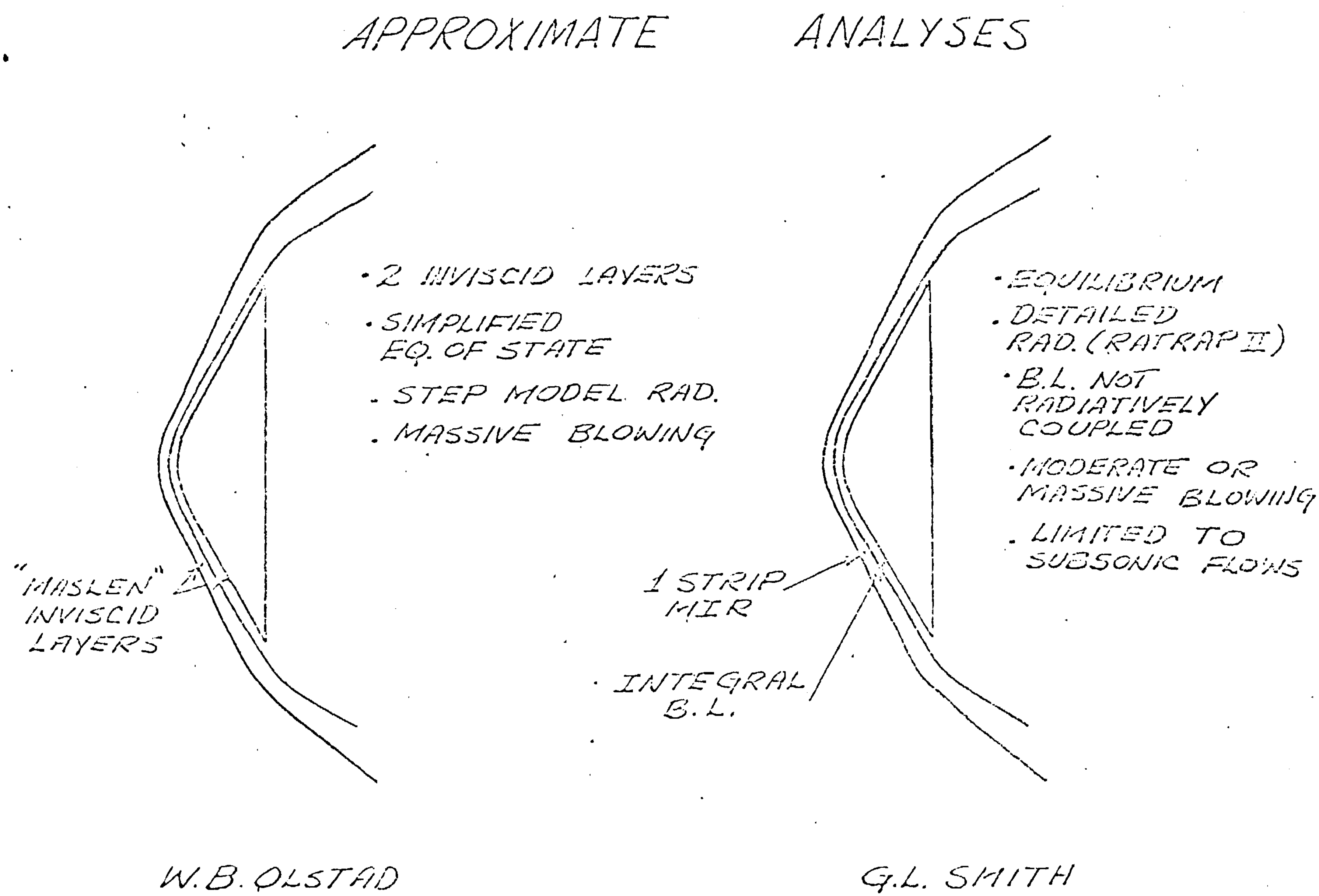

APPROXIMATE ANALYSES
•2 INVISCID LAYERS
•SIMPLIFIED
EQ. OF STATE
. STEP MODEL RAD.
. MASSIVE BLOWING
"MASLEN" INVISCID LAYERS
W.B. OLSTAD
•EQUILIBRIUM
. DETAILED
RAD. (RATRAP II)
•B.L. NOT
RADIATIVELY
COUPLED
•MODERATE OR
MASSIVE BLOWING
. LIMITED TO
SUBSONIC FLOWS
1 STRIP
MIR
. INTEGRAL
B.L.
G.L. SMITH

Figure 5-40

Olstads' analysis can't handle the sonic line at the aft corner; and in the Smith method's present state of development, it can't handle it anywhere else. It only works for a subsonic flow field. So, the sonic line location determines which of these approximate analyses you want to use.

Just to show you what you can do with an approximate solution, if you have a good rigorous analysis with which to calibrate, Figure 5-41 and 5-42 show some inviscid radiative heating rates computed for two proposed Pioneer Venus probes. Radiative heating rates are plotted as a function of dimensionless wetted length from the stagnation point. The solid curve is Ken Sutton's very detailed solution; the dashed curve is an Olstad-Maslen type solution worked out by Ralph Falanga at Langley. The agreement is very good but before you can get this type of agreement you really need a benchmark to compare with the approximate solution when you are working up the radiation step model and the thermodynamic approximations in the solution.

In summary, then, our present situation is that while we are attempting to develop rigorous flow field models for downstream radiative flows of massive ablation and we are making progress, there are significant unknowns. The biggest of these is the turbulence model for the mixing layer. For engineering calculations for trade-off studies, there really is a need for approximate solutions. It appears that there are several promising avenues to follow in developing these, but you do need the rigorous solution, or experimental data, to calibrate the approximate approaches.

RADIATIVE HEATING DISTRIBITION

V_∞ = 11.18 km/s

ρ_∞ = 0.00285 kg/m^3

0.90 CO_2 - 0.10 N_2

Rn = 0.305 m

θ_c = 60^0

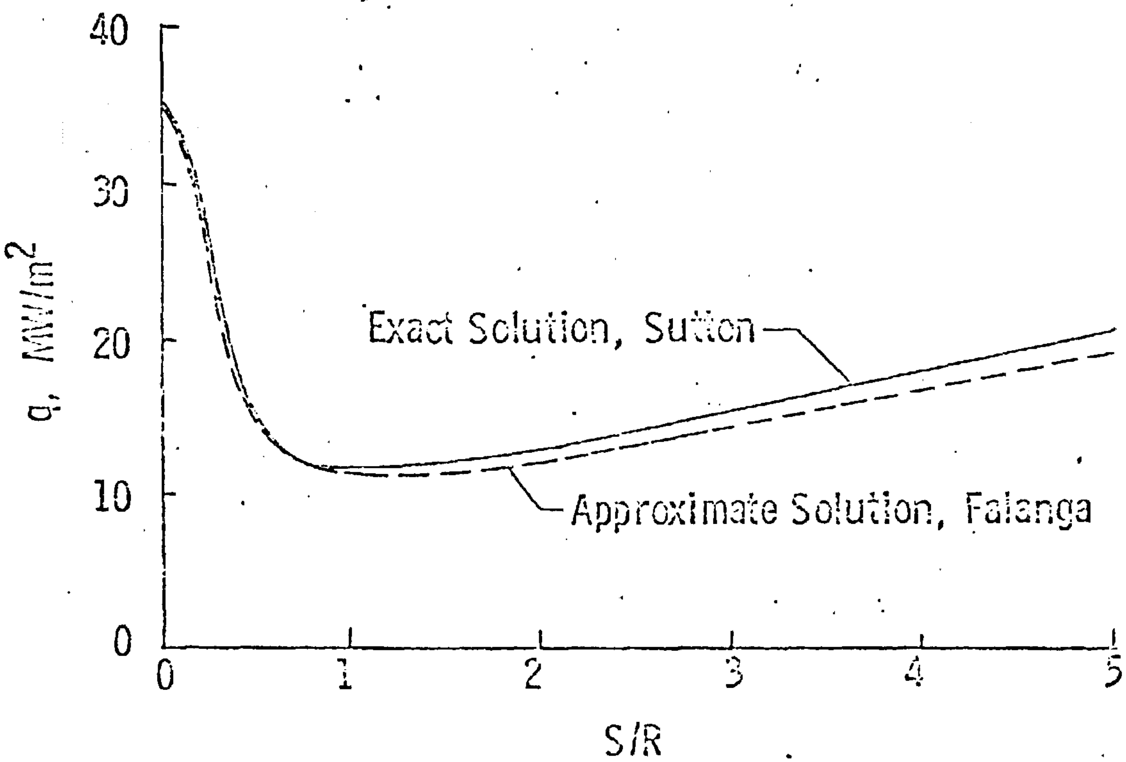


Figure 5-41

RADIATIVE HEATING DISTRIBUTION

$V_\infty = 9.14$ km/s

$\rho_\infty = 0.0091$ kg/m^3

$0.90\ CO_2 - 0.10\ N_2$

$Rn = 0.181$ m

$\theta_c = 45^0$

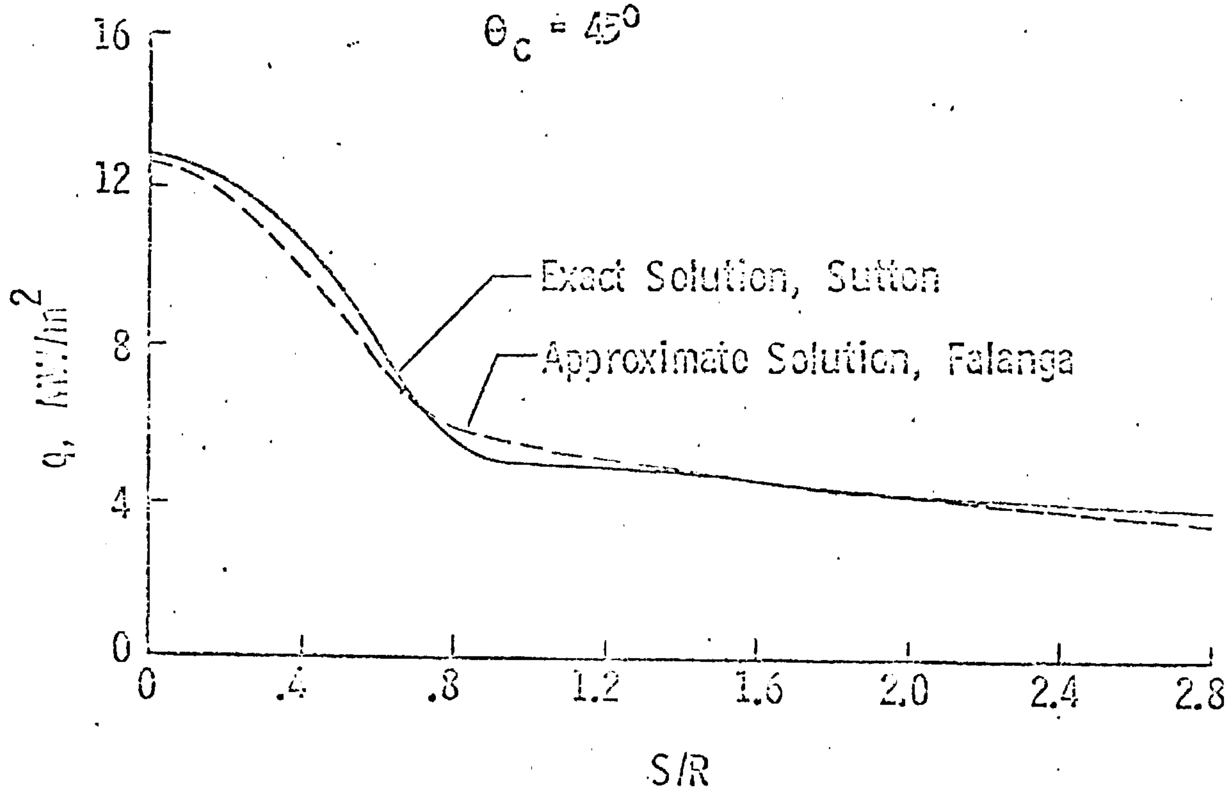


Figure 5-42

UNIDENTIFIED SPEAKER: In the rigorous analysis by Jim Moss you have this shock layer analysis which is split into two parts: one is inviscid. I believe the energy transport is important but not the momentum transport. Is that the case?

MR. WALBERG: I think I don't understand your question, you should ask it again.

UNIDENTIFIED SPEAKER: There is a viscous shock layer so - this is a generalized term. It implies that energy transport is important. You said viscous, and then you said something about an inviscid shock layer. Did you say that?

MR. WALBERG: First of all, in Jim Moss' analysis of the viscous shock layer you have one set of governing equations that apply uniformly throughout the entire flow field. I may have referred to the outer flow as effectively inviscid or inviscid. If I did, I meant what you are saying that the energy transport is more important.

UNIDENTIFIED SPEAKER: My questions actually are, is the Reynolds number or the Péclet number that important, to justify this complicated approach as versus the other approach; that is the viscous shock layer, because it is much hotter?

MR. WALBERG: The question is, in view of the Reynolds number that we encounter, do we have to go to a complicated viscous shock layer solution, or could we use a simpler analysis.

The answer is in many cases we could use a simpler analysis, but the objective here is to develop a rigorous solution that can be applied to many different entry situations and it should have wide applicability rather than one that's limited to a particular planetary encounter.

MR. OLSTAD: Our next speaker is Bill Nicolet, from Aerotherm Acurex Corporation. I think maybe, finally, you will se some numbers on heating rates for the outer planet entry. Bill's topic is Aerothermal Environment and Material Response, A Review.

N75 20385

NOTE: This paper is as it was presented during the workshop. The Author's review and editorial comments were not received. His slides and figures appear at the end of this session.

THE AEROTHERMAL ENVIRONMENT AND MATERIAL RESPONSE, A REVIEW

William E. Nicolet
Aerotherm Acurex Corporation

MR. WILLIAM NICOLET: Thank you. In response to the letter of invitation, as I recall, the wording was that we were invited to review and assess current states of technology and make recommendations, and so I addressed myself to that, rather than giving a lot of numbers. It seems like I've been promised, repeatedly, to give numbers. I did give a few just to orient the audience, but this will not be a presentation oriented to that end.

In addition, in the initial response to the letter, I promised to review both aerothermal environments and material response. After looking at the time allocation, I decided I'd better delete material response and leave that to this afternoon's sessions and to other people. So, the focus of this particular talk will be a review of the aerothermal environment.

Figure 5-43 - I'm going to end up duplicating some of the material that Jerry presented, clearly, but let's start off by looking at the flow and the material response as Aerotherm sees it as opposed to how Langley sees it. There are, pretty clearly, a lot of overlaps here.

To begin with, you have a normal shock wave with some relaxation zone behind it, usually of some maximum thickness, at the stagnation point. Typically, there is the hot shock layer of gases behind it emitting radiation to the body. There is some type of a mixing region, hopefully out in the middle of the shock layer, bounded by ablation gases flowing inviscidly out from

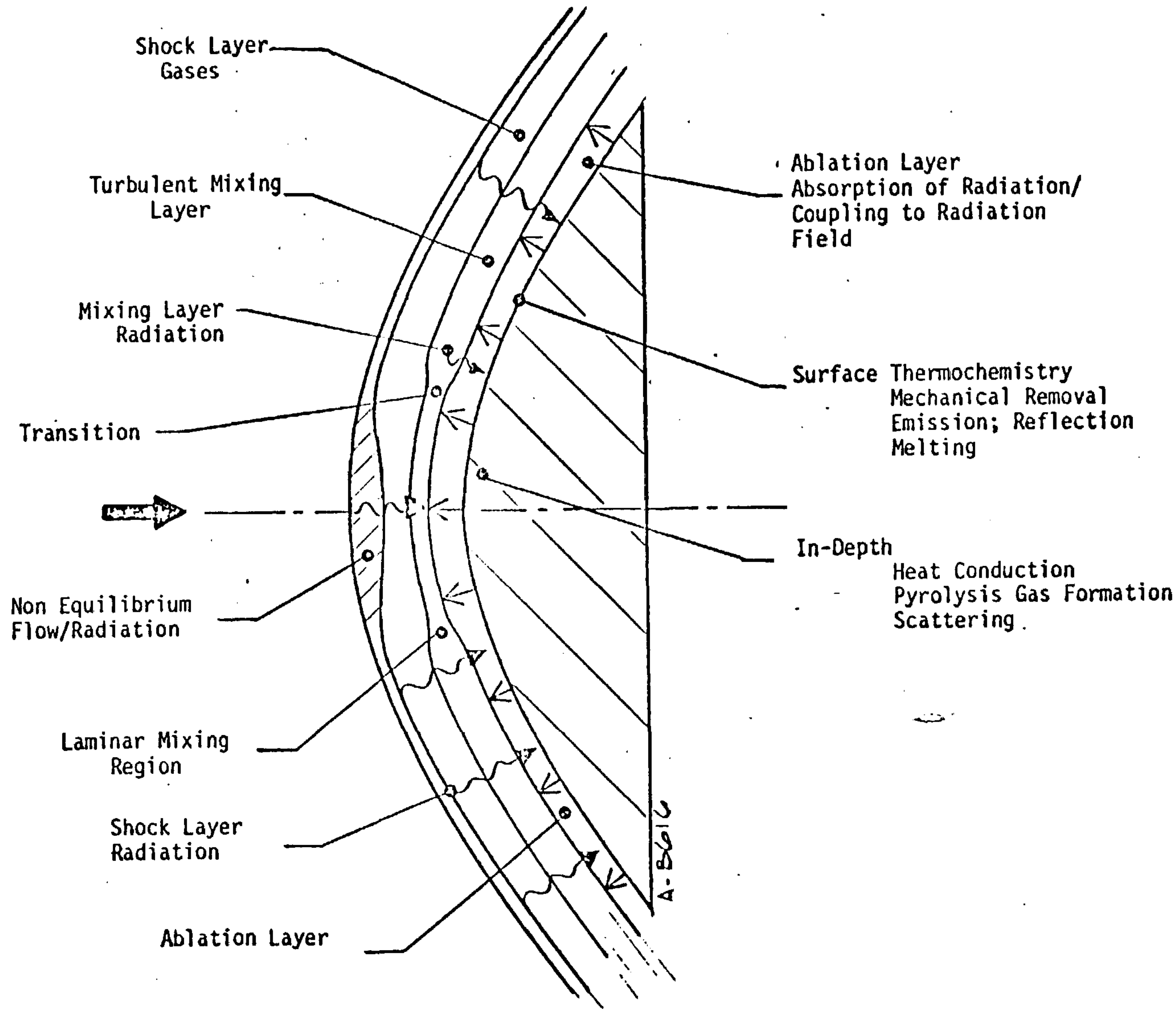


FIGURE 5-43

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OF POOR QUALITY

the body on the inside, and by environmental gases flowing more or less tangentially on the outside. One might expect this to be laminar in the region near the stagnation point with transition of turbulence back further. There will be absorption of radiation in the mixing layer and in the ablation layer. In addition, there appears to be important radiation components emitted by the mixing layer itself. If one goes over and looks at the other end - and I am just going to touch on this - as I said, important absorption in the ablation layer; important events going on at the surface: thermochemical events, mechanical removal, radiation emission, reflection, melting, depending on the type of ablator selected; important events going on in depth: heat conduction, pyrolysis gas formation, scattering; again, depending on the material selected.

Figures 5-44, 45, and 46 are three slides that I will put up here really just to allow us to focus down to some numbers. To begin with, note that the solid lines are for Saturn, and the dashed lines are for Uranus. This is the stagnation-point radiative heating flux as a function of time.

To begin with, two different atmospheres are considered here; the cold dense and warm atmospheres for both planets. Also two different body shapes were considered. Most of the data is for a 60° aft angle cone, but the very high radiative flux (above 60 kW/cm^2) was computed for an Apollo-type configuration. The convective fluxes show slight quantitative differences but, qualitatively, are very similar. In contrast, the radiative fluxes are vastly different, with the Uranus cold-dense fluxes being nearly an order of magnitude greater than those for the Saturn cold-dense entries. Moreover, entries into cold-dense atmospheres have radiative flux levels which are at least an order-of-magnitude greater than the corresponding entries into the nominal or warm entries for the same planet. This point will be made over and over again, but has to do with the composition of the atmospheres and almost nothing else.

COMPARISON OF SELECTED SATURN AND URANUS STAGNATION POINT HEAT PULSES WITH NO ABLATION

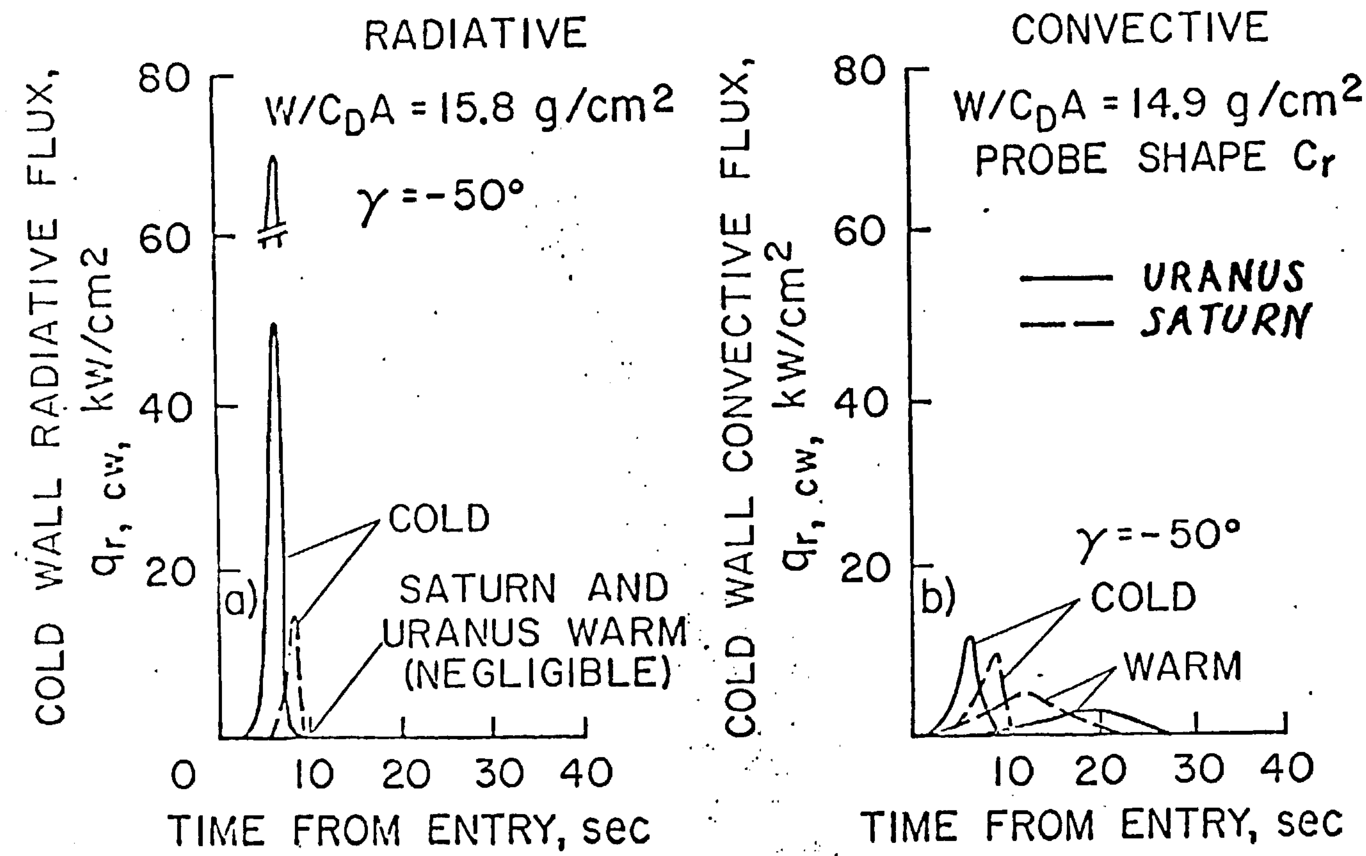

FIGURE 5-44

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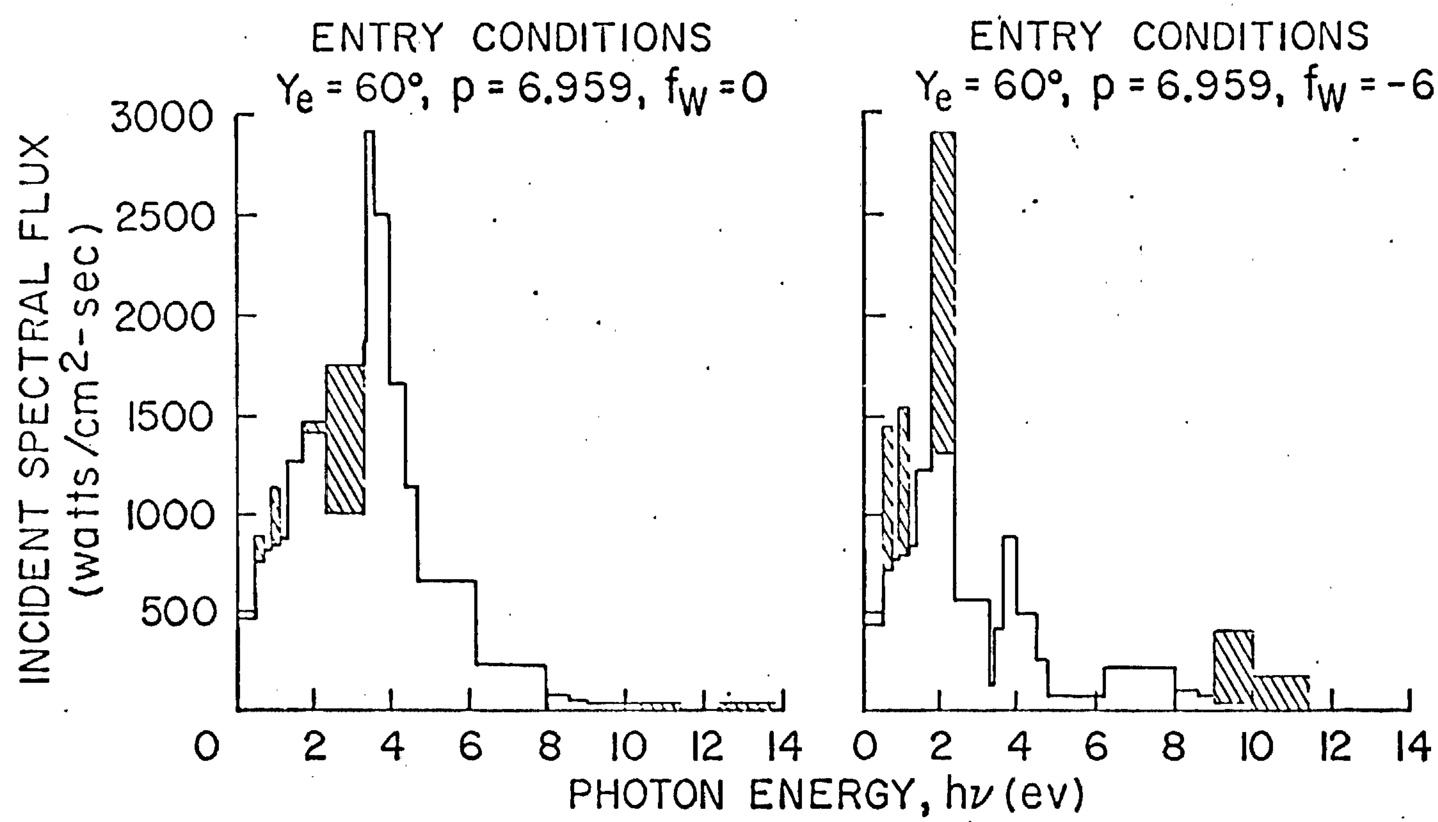
IMPACT OF CARBON ABLATION ON STAGNATION REGION RADIATIVE HEAT FLUX TO PROBE SURFACE - SATURN ENTRY



(a) NO ABLATION (b) WITH ABLATION

FIGURE 5-45

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RADIATIVE HEAT BLOCKAGE CORRELATIONS FOR CARBON AND SiO_2 ABLATION

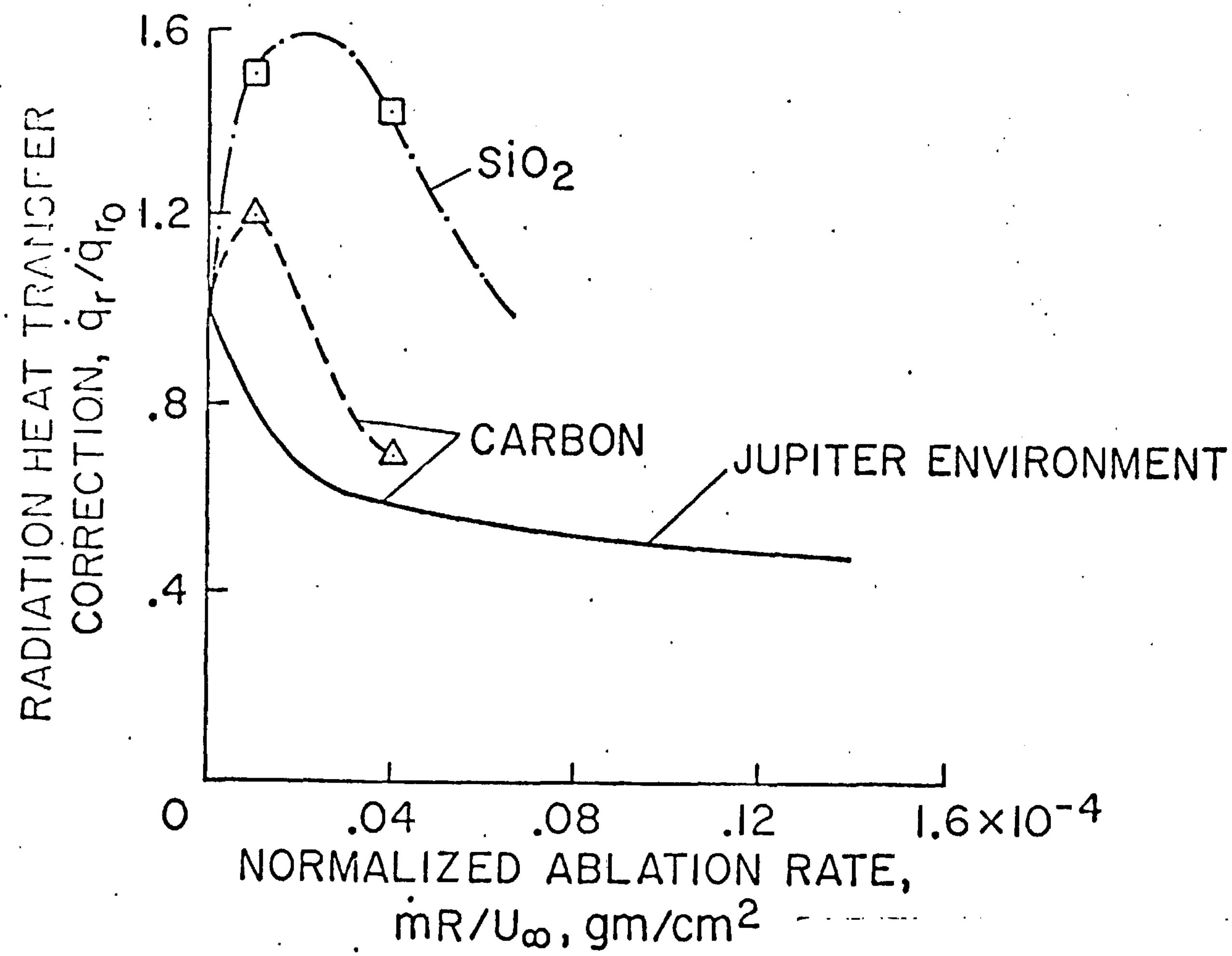

FIGURE 5-46

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Figure 5-45 presents the spectral distributions of the incident radiation flux. Typically, these are calculated from our computer program. These would be for the Saturn nominal entry, a relatively steep entry into the Saturn nominal atmosphere. The plot on the left shows the radiation coming from the shock layer alone.

On the right we see the radiation with ablation products. Note that we have cut out a good part of the radiation in the U.V. Simultaneously, we added radiation in the visible portion of the spectrum. This is the radiation coming directly from the mixing layer. In both of these figures, the clear parts refer to continuum radiation; the slashed parts refer to line radiation.

One might hope to get from a detailed calculation of the radiation heat transfer correction and blockage correlation of the nature shown in Figure 5-46. The solid line represents work that was done in support of a Jupiter entry study. It was done three or four years ago by Ken Wilson, and subsequently correlated by Bill Page. The focus there was for large blowing rates. The Jupiter entry case was very severe. Typically, we would see it reduce the radiation flux by about a factor of two. My point in doing additional calculations for smaller blowing rates which is important in the Saturn-Uranus nominal type entries was to investigate the effects due to the mixing layer radiation that I discussed in the previous slide. As you see, typically, we have important additive effects.

These types of correlations developed from stagnation-point solutions, are generally used for the whole body. The objection Mr. Walberg was making a few minutes ago was that, in fact, these might change shape as you go around the body. John Howell and C. H. Liu, here at Ames, are greatly expanding the matrix of calculations on this particular subject. Again, it is focused primarily on the stagnation point, but it, supposedly, would do a lot to firm confidence in this type of calculation and the correlations of it.

Hopefully, I have set the stage for the type of numbers, the type of effects, the type of events we are dealing with, I would like to now review calculational and experimental approaches to solve the problem, focusing on areas where there are uncertainties and suggesting, as a last item, ways to reduce the uncertainties.

The most uncertain item - and notice on Figure 5-47 I have listed input conditions now - the most uncertain item in the whole analysis is the atmospheric composition and, particular, elemental mass fractions of helium. My calculations jump up and down and go all over the place, depending on what we assume there. In particular for the Uranus case we can find fantastic radiation fluxes for a high-helium-content mixture; and almost none for a low-helium-content mixture.

The elemental mass fraction of the primary radiating species in the environment is important, as are the atmospheric scale heights. But they certainly take a distant second place in importance to the elemental mass fraction of helium.

I am going to briefly run down some of the calculational methods. To begin with, all of my focus will be on the 2-D flow capabilities. There is a figure in the handout dealing with 3-D capabilities but, for those of you who are interested in the angle of attack, I suggest you look at that and perhaps, talk to me. I will not discuss it as part of the oral presentation.

Let's start with the inviscid type of calculations (Figure 5-48) applicable right behind the shock front. There are a number of finite - difference, time dependent or integral relation methods - Jerry Walberg alluded to some - focusing primarily on situations where there is no radiation coupling. These would be used for basic studies or pressure or boundary layer edge velocities, and the like. They would be applicable in the cases where the radiation is not important. If we add radiation coupling on the second line (indicating), we find that there are a couple of calculations that can be done. There are a couple of codes available

REVIEW OF INPUT CONDITIONS

ATMOSPHERIC COMPOSITION:

ELEMENTAL MASS FRACTION OF HELIUM

ELEMENTAL MASS FRACTION OF IMPORTANT RADIATING SPECIES, E.G., H_2, C, O

ATMOSPHERIC SCALE HEIGHT

FIGURE 5-47

REVIEW OF 2-D FLOW CAPABILITIES

TYPE	APPROACH	USE	STATUS
INVISCID	FINITE - DIFFERENCE, TIME DEPENDENT OR INTEGRAL RELATIONS (REAL GAS - NO RADIATION COUPLING)	BASIC STUDIES OF PRESSURE, B.L. EDGE VELOCITIES	OPERATIONAL AT MANY ORGANIZATIONS
	SAME AS ABOVE (RADIATION COUPLING)	BASIC STUDIES OF SHOCK SHAPES, RADIATION COOLING FACTORS	OPERATIONAL AT ONE OR TWO ORGANIZATIONS
	CORRELATIONS OF SHOCK SHAPE, PRESSURE DISTRIBUTIONS, ETC. (NO RADIATION COUPLING)	SUPPORT OF HEAT SHIELD SIZING STUDIES	OPERATIONAL AT MANY ORGANIZATIONS
	CORRELATIONS OF SHOCK SHAPE, PRESSURE DISTRIBUTIONS, ETC. (WITH RADIATION COUPLING)	SUPPORT OF HEAT SHIELD SIZING STUDIES	NOT AVAILABLE

FIGURE 5-48

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with selected organizations. If we go on down to talk about what would be required to support heat shield sizing studies, correlations of shock shape, pressure distributions, and the like are certainly vital inputs. They are generally available for the shapes of interest for the non-radiation coupling situation.

Let's go on to boundary or merged layers, Figure 5-49. Here my terminology for merged layer is the same that Jerry was using for viscous shock layer, that is, a boundary-layer-type calculation extending from the shock wave all the way to the body. I tend to use them interchangeably since the mathematics tends to be quite similar.

If we talk about a finite difference method coupled to ablation chemistry, with the laminar or turbulent flow, but without radiation, we have such codes as the BLIMP that has been discussed previously. It is operational without radiation coupling. There are other codes like it, provided that the blowing rates remain modest. If we add radiation coupling; same types of codes, same types of restrictions. If we reduce, or subtract off, the turbulent flow requirement we have codes that are applicable for all blowing conditions, and this is certainly the situation for the typical outer planetary entry of interest.

If we go on down and ask about a finite difference approach considering finite rate chemistry, even without radiation coupling and without turbulence, we find that this type of approach has generally not been used in the planetary entry situation, although the RV community has developed that type of code and some capability does exist. I point out that this type of discussion was made before I was aware of the most recent presentation of the people from JPL.

Figure 5-50 continues and gets more into intermediate or tool-type things that would support heat shield sizing, there are various stream tube methods. I would consider Olstad's method a

REVIEW OF 2-D FLOW CAPABILITIES

TYPE	APPROACH	USE	STATUS
BOUNDARY OR MERGED LAYER	FINITE DIFFERENCE, ABLATION COUPLED, EQUILIBRIUM, REAL GAS CHEMISTRY, LAMINAR OR TURBULENT, WITHOUT RADIATION	BASIC STUDIES, SURFACE HEATING, BLOWING CORRECTIONS, ETC.	OPERATIONAL FOR MODEST BLOWING
	SAME AS ABOVE BUT WITH RADIATION COUPLING	SAME	OPERATIONAL FOR MODEST BLOWING
	SAME AS ABOVE BUT WITHOUT TURBULENT FLOW	SAME	OPERATIONAL FOR ALL BLOWING CONDITIONS OF INTEREST
	FINITE DIFFERENCE. FINITE RATE CHEMISTRY NO RADIATION, LAMINAR	SAME	NOT GENERALLY IN USE IN THE PLANETARY ENTRY COMMUNITY ALTHOUGH SOME CAPABILITY EXISTS

FIGURE 5-49

REVIEW OF 2-D FLOW CAPABILITIES

TYPE	APPROACH	USE	STATUS
BOUNDARY OR MERGED LAYER	STREAM TUBE, WITH RADIATION, WITHOUT ABLATION	INTERMEDIATE METHOD, RADIATION COOLING FACTORS, COLD WALL HEAT FLUX DISTRIBUTIONS	OPERATIONAL
	SAME AS ABOVE BUT WITH ABLATION	BLOWN WALL FLUX DISTRIBUTIONS	OPERATIONAL
	INTEGRAL METHOD, REAL GAS EFFECTS, RADIATION COOLING. FACTORS, BLOWING CORRECTIONS, LAMINAR OR TURBULENT	SUPPORT OF HEAT SHIELD SIZING STUDIES	OPERATIONAL
	CORRELATIONS, BLOWING CORRECTIONS FOR CONVECTION, RADIATION	SUPPORT OF HEAT SHIELD SIZING STUDIES	OPERATIONAL FOR CARBON SiO_2

FIGURE 5-50

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stream tube method. Typically, they can't handle radiation. They will do things like radiation cooling factors or cold wall heating distributions. If you add ablation they also have some capability, but it is more limited.

If we go into straight integral methods, we can handle most of the items of interest, provided we restrain ourselves to cold wall events - no blowing or whatever - and we have to resort to other types of approximate methods to get the radiation fluxes. These are the methods that I have typically used in support of heat shield sizing.

Finally, we have correlations which, again, will be required for the heat shield sizing. That would include blowing corrections for the convection and the radiation and would refer back to the figure I showed before. Some are available for such ablation species as carbon and SiO_2 and there are efforts underway right now to expand the correlation base.

I would like to go now to Figure 5-51, review of transport properties. This is with application to input to the flowfield calculations. To begin, there is a total properties approach, and this is a classic approach that has been used for years. It was originated by Butler and Brokaw. The entry calculations that have been done with it are almost without number. It is very simple, however, it is restricted to non-varying elemental composition across the layer. And that, in effect, restricts the calculations to no ablation or to ablation of a gas which has the same elemental composition as the environmental gas. So, with that restriction, that approach is losing favor.

There is a series here of three successively more complicated approaches, namely: correlations for such properties as viscosity, diffusivity, thermoconductivity plus equal diffusion; coefficient approximation, bifurcation approximation, actual solution to the first order Chapman; Enskog solutions. These successively

REVIEW OF TRANSPORT PROPERTIES

APPROACH	COMMENT
TOTAL PROPERTIES (BUTLER AND BROKAW)	REQUIRES FIXED ELEMENTAL COMPOSITION (NO ABLATION) RESTRICTED APPLICATION TO PLANETARY PROBLEMS
CORRELATIONS FOR PROPERTIES PLUS EQUAL DIFFUSION COEFFICIENT APPROXIMATION OF LEE'S	ADEQUATE UNTIL SUBSTANTIAL IONIZATION OCCURS
CORRELATIONS FOR PROPERTIES PLUS BIFURCATION APPROXIMATION WITH MULTICOMPONENT DIFFUSION	BETTER THAN EQUAL DIFFUSION APPROXIMATION AT LOW LEVELS OF IONIZATION, BUT ALSO FAILS WHEN SUBTANTIAL IONIZATION OCCURS
CORRELATIONS FOR PROPERTIES PLUS FIRST ORDER CHAPMAN - ENSKOG SOLUTION TO MULTICOMPONENT DIFFUSION	SAME AS ABOVE
HIGHER ORDER SOLUTIONS OR IMPROVED CORRELATIONS	REQUIRED IF TRANSPORT PROPERTIES ARE TO BE SIGNIFICANTLY IMPROVED AT HIGH LEVELS OF IONIZATION

FIGURE 5-51

increase the accuracy of the solutions to the diffusion equations. They all run into trouble when ionization begins to be important. This would, typically, be in the 8,000° to 10,000° Kelvin range. It is my feeling that higher order solutions or improved correlations are required to go above that substantially.

Figure 5-52 reviews the radiation transport codes or properties that are available, detailed codes have been generated and are available from several organizations. They are used in support of basic studies, reduction of experimental data. There is at least one that is available that will support flow coupling calculations. Typically, they are also used to define multi-group radiation models.

Concerning the properties, these have been the subject of a recent review at Langley. It is my feeling - although I haven't read the report yet - that the environmental gases are in good shape; the ablation type gases are somewhat uncertain.

On Figure 5-53 I am going to touch briefly on the status of the experiments. Basically, in terms of laboratory experiments - now this is only in terms of the aerothermal environment simulation and not the material response - certain aspects can be simulated with shock tubes, arcs, lamps, and combined arc-lamp facilities. There is no known facility that will do a full job of just covering the important parameters that exist. Flight experiment feasibility studies indicate promise but a lot of expense. It has been suggested that we consider shuttle as the launch vehicle which may help with the cost problems.

On the final figure, 5-54, I have selected some priorities as to what I think should be done; pretty much in the order that I think they should be done, although, for example, the first one is certainly just a wish, namely, obtain better input on atmospheric composition. I am somewhat in agreement with Jerry; I think we ought to do some fundamental work in upgrading the turbulent model. I think we ought to make an effort to continue

REVIEW OF GAS PHASE RADIATION TRANSPORT

CODES

TYPE	USE	STATUS
DETAILED	BASIC STUDIES REDUCTION OF EXP. DATA	SEVERAL OPERATIONAL
DETAILED	COUPLING TO FLOW FIELD CODE	ONE OPERATIONAL
MULTI-GROUP	COUPLING TO FLOW FIELD CODE	MANY PROPOSED

PROPERTIES

SYSTEM	STATUS
AIR $CO_2 - N_2$ $H_2 - He$	GOOD SHAPE
C SiO_2 TFE	UNCERTAIN

FIGURE 5-52

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EXPERIMENTAL VERIFICATION

LABORATORY FACILITIES

SPECIFIC ASPECTS OF THE AEROTHERMAL ENVIRONMENT CAN BE SIMULATED IN SHOCK-TUBE, ARC, LAMP, AND COMBINED ARC-CAMP FACILITIES

NO FACILITY EXISTS WHICH WILL PERFORM A FULL SIMULATION OF ALL THE PARAMETERS VIEWED AS BEING IMPORTANT

FLIGHT EXPERIMENT

FEASIBILITY STUDIES INDICATE PROMISE BUT COST IS VERY HIGH FOR RV LAUNCHED EXPERIMENTS

PROSPECTIVE USE OF SHUTTLE AS LAUNCH VEHICLE MAY REDUCE COST PROBLEMS

FIGURE 5-53

SUGGESTED PRIORITIES

1. OBTAIN BETTER INPUT ON ATMOSPHERIC COMPOSITION
2. UPGRADE TURBULENT MIXING LAYER MODELS
3. UPGRADE RADIATION TRANSPORT MODELS TO BE CONSISTENT INDUSTRY - WIDE AND CONSISTENT WITH RECENT LANGLEY REVIEW
4. OBTAIN BETTER RADIATION PROPERTIES FOR ABLATION PRODUCTS
5. CONTINUE GENERATION OF BLOWING REDUCTION CORRELATIONS
6. DEVELOP CORRELATIONS OF INVISCID PARAMETERS INCLUDE EFFECT OF RADIATION
7. PERFORM VERIFICATION TESTS
8. UPGRADE TRANSPORT PROPERTY CORRELATIONS
9. UPGRADE CODES TO CONSIDER NONEQUILIBRIUM EFFECTS

FIGURE 5-54

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making the radiation models that are used throughout the industry consistent so that we can talk about apples and apples instead of apples and oranges. I would like to see something done better with the radiation properties for ablation products. I think we ought to continue worrying about blowing reduction correlations. That is certainly important in terms of planetary entries. I would like to see some development of correlations of the inviscid parameters which include the effect of radiation. That capability exists; it seems a shame it is not being exploited. I think we have to worry the verification tests business further. I would like to see some upgrading of the transport property correlations, and I think that there ought to be some attention given to the non-equilibrium effects.

SESSION VI - HEAT PROTECTION

Chairman: Dr. Phil Nachtsheim
NASA Ames Research Center

MR. VOJVODICH: This is kind of like one of the old western movies where you can tell the antagonists and the protagonists as the guys who wear the black hats and the guys who wear the white hats. We have two different view points here: the traditional approach to the black, carbon phenolic type of heat shield and the white, reflecting heat shield.

DR. NACHTSHEIM: In this session we are going to talk about the evaluation of heat shield materials, development of new heat shield materials, and then the question of simulation. The evaluation will be concerned with the heat shield materials that are very well characterized: the carbon phenolic and graphite heat shields. Those evaluations will be discussed in terms of what was done at the HIP facility in St. Louis and the high-powered laser which is here at Ames. In other words, existing materials with existing facilities. We will talk about the developmental effort on the reflecting heat shield. This concept was introduced several years ago, and most people agree that it's a good idea. The question remains: how do you do this? So, we will be addressing the development of the reflecting heat shield, the silica heat shields; and we will have two papers discussing that. Then, finally, we will discuss the question of simulation. Whether the heat shield be a black heat shield or a white heat shield, we do feel that in order to flight qualify it, it should be evaluated as closely as possible in the environment that we would expect for a planetary entry.

With that, I would like to introduce Sam Mezines from McDonnell-Douglas who will talk about the work he's done on sizing the heat shields for Saturn and Jupiter, and some tests he performed in the HIP facility.

CARBON PHENOLIC HEAT SHIELDS FOR JUPITER/SATURN/URANUS ENTRY PROBES

S. Mezines

McDonnell-Douglas Astronautics Company

MR. MEZINES: I am going to limit my talk to carbon phenolic heat shield technology. As you probably know, these materials have been around for a number of years and we have assimilated a lot of fabrication and flight experience on these materials from our numerous RV programs.

In this presentation I am going to cover three areas. First of all, I will summarize the heat shield results from the outer planetary probe mission studies that we've done in the last couple of years. Secondly, I will attempt to demonstrate the applicability of missile flight data to planetary entry conditions; and finally, I will summarize the results of some recent plasma jet testing of carbon phenolic conducted in our ten megawatt facility.

Figure 6-1 illustrates the common probe design that we have developed for exploration of the outer planets. We propose to use a carbon phenolic heat shield material and tailor the thickness of the material to accommodate each of the probe missions.

We have selected an integral heat shield approach over concepts utilizing an intermediate insulation layer in order to eliminate a high temperature interface problem and permit direct bonding of the carbon phenolic to the structural honeycomb sandwich. The sandwich is filled with a very fine powder to minimize degradation of its insulation properties by the high conductive hydrogen/helium gases during the long atmospheric descent phase. The inner portion of the forebody heat shield has been hollowed out to reduce both weight and heat conduction.

The afterbody heat shield is made of a low density elastomeric material which is light-weight and RF transparent.

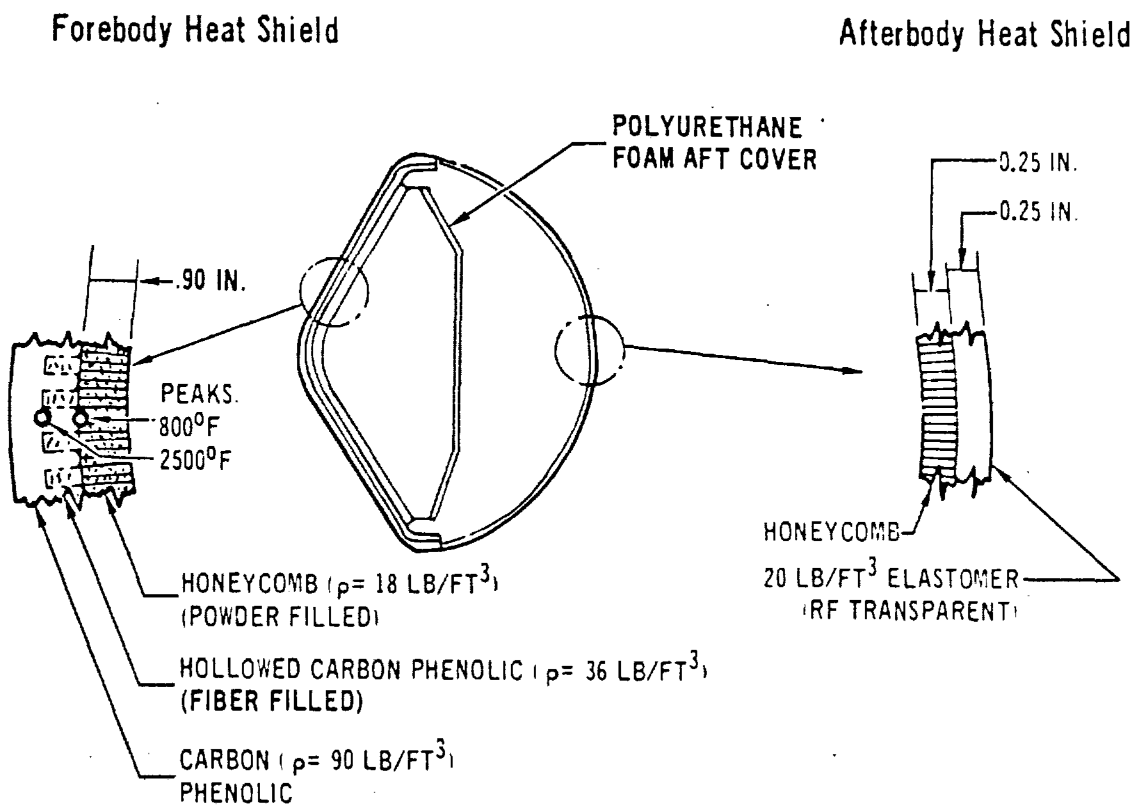

Figure 6-1. Planetary Probe Heatshield

Figure 6-2 depicts the convective and radiative heat flux associated with entry into each of the planets. As indicated, the fluxes are very high, in the 40,000 to 50,000 BTU/FT^2 sec range, and predominantly radiative. These fluxes and the heat shield requirements to be shown later were computed by Aerotherm Corporation under contract to NASA Ames.

The magnitude of heating associated with each planetary entry is very strongly influenced by the initial entry angle and atmospheric mode/assumed. For instance, steep entries into the cold atmospheres of Saturn and Uranus result in heating rates as high as those encountered in a shallow entry into the Jupiter nominal atmosphere, even though the entry velocity at Jupiter is 50 percent higher than entry into the other planets.

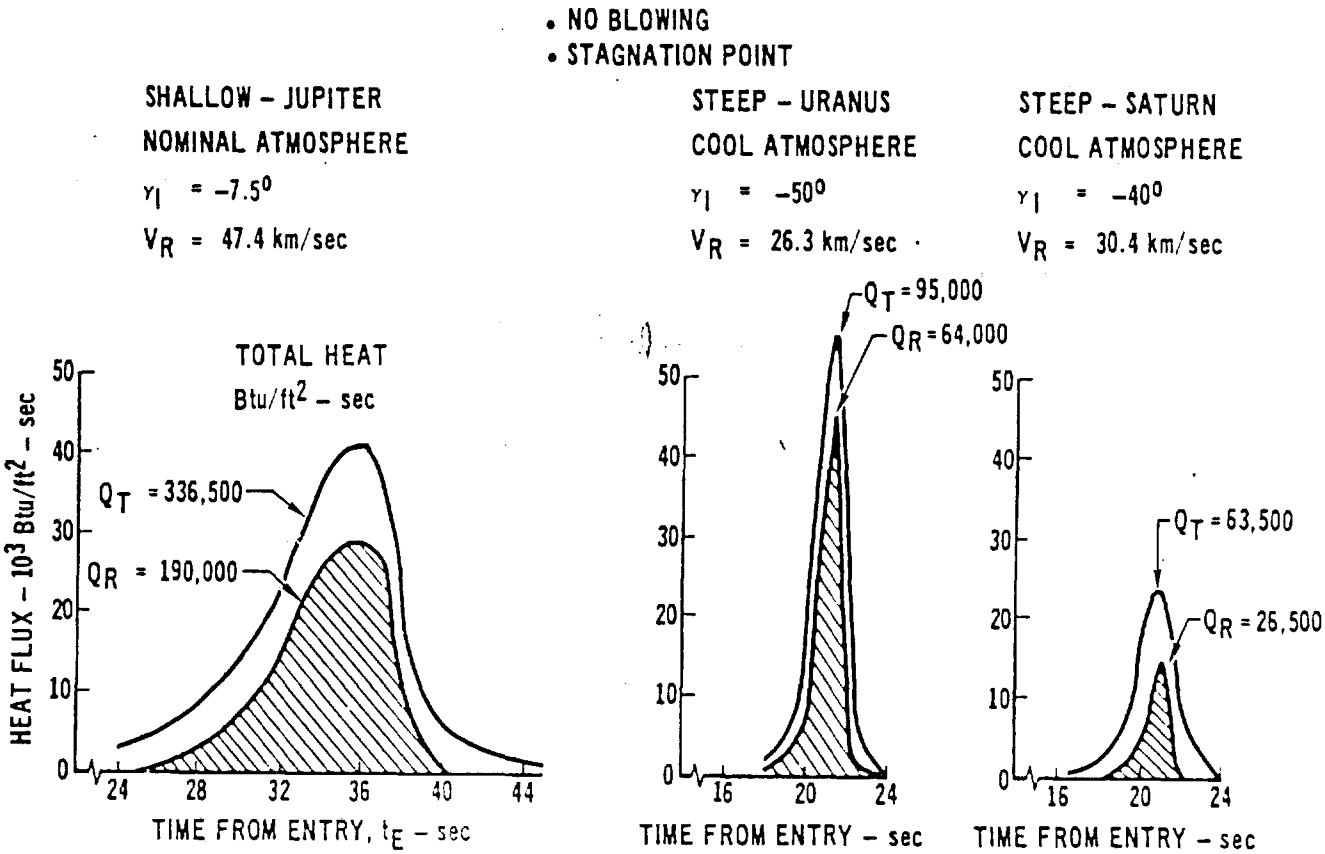

Figure 6-2. Planetary Entry Heating Environments

The high heating rate for the Uranus entry is due to the large proportion of helium dictated by the cold atmospheric model. The high helium/hydrogen ratio results in not only a higher deceleration load and stagnation pressure but also in higher shock layer temperatures and much higher radiation fluxes. Selection of the shallow Jupiter entry condition was made on the basis of the preliminary Pioneer 10 data which indicated that the atmosphere composition is near the solar abundance ratio (nominal model) and better knowledge of the planet's ephemeris data permit shallow entry with very small uncertainty in entry angle.

Heat shield thickness requirements for each of the outer planets is established by analyzing a number of critical entry trajectories which bound the entry envelope and atmospheric model uncertainty. In general, steep entries coupled with the cold atmospheres model definition results in high heating rates and high surface recession rates whereas shallow entries and warm atmosphere lead to milder heating rates but longer durations and higher insulation requirements.

For Saturn, the shallow-warm atmosphere entry sized the heat shield even though the peak heat flux was only 2300 BTU/FT^2-sec and practically no surface recession occurred. Conversely, entry into the Uranus cold-dense atmosphere model results in very high heating rates so that material recession sizes the heat shield thickness requirements. For Jupiter, we have purposely limited the entry angle to very shallow values (about 7.5°) in order to alleviate the heating and heat shield requirements. Furthermore, the Pioneer 10 data indicate an atmosphere composition corresponding to the current nominal atmosphere.

The heat shield thickness shown in Figure 6-3 is based on 2000°F backface temperature. A number of insulative approaches can be used to reduce the temperatures below the 2000°F level. For Saturn/Uranus, our baseline approach is to hollow-out the carbon phenolic below the 2000°F isotherm whereas for the Jupiter heat shield we have elected to forfeit the weight savings provided by the hollowed-out layer in order to increase the inherent safety margin.

Figure 6-4 illustrates the similarity in entry heating and pressure between planetary probe and mission flight entries. The missile body point of interest is the control surface that was protected with a carbon phenolic heat shield. Heating rates on the missile nose tip are even higher but stagnation pressures are sufficiently high (above 100 atmospheres) to exclude the applicability of these data for planetary heat shield designs.

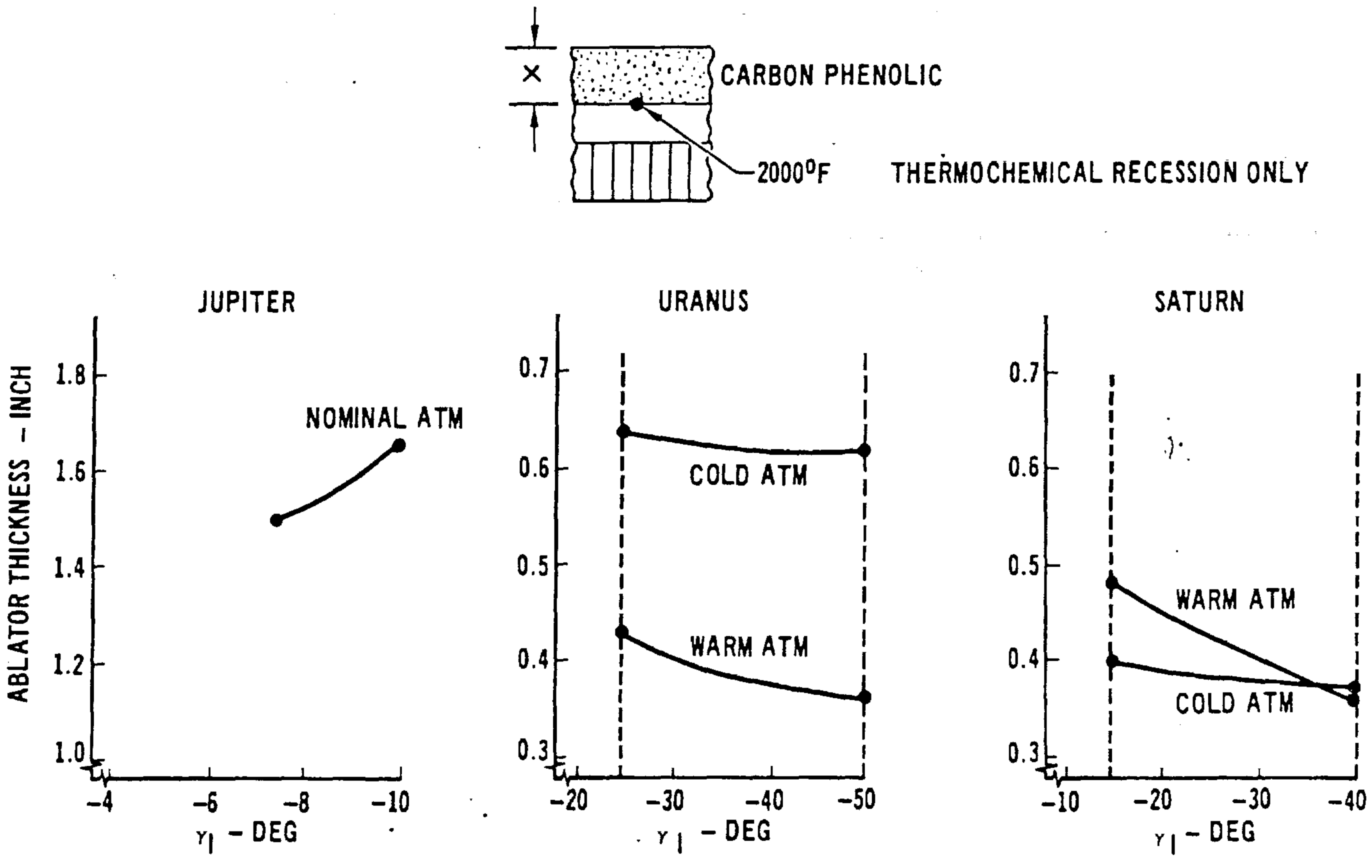

Figure 6-3. Heat Shield Requirements

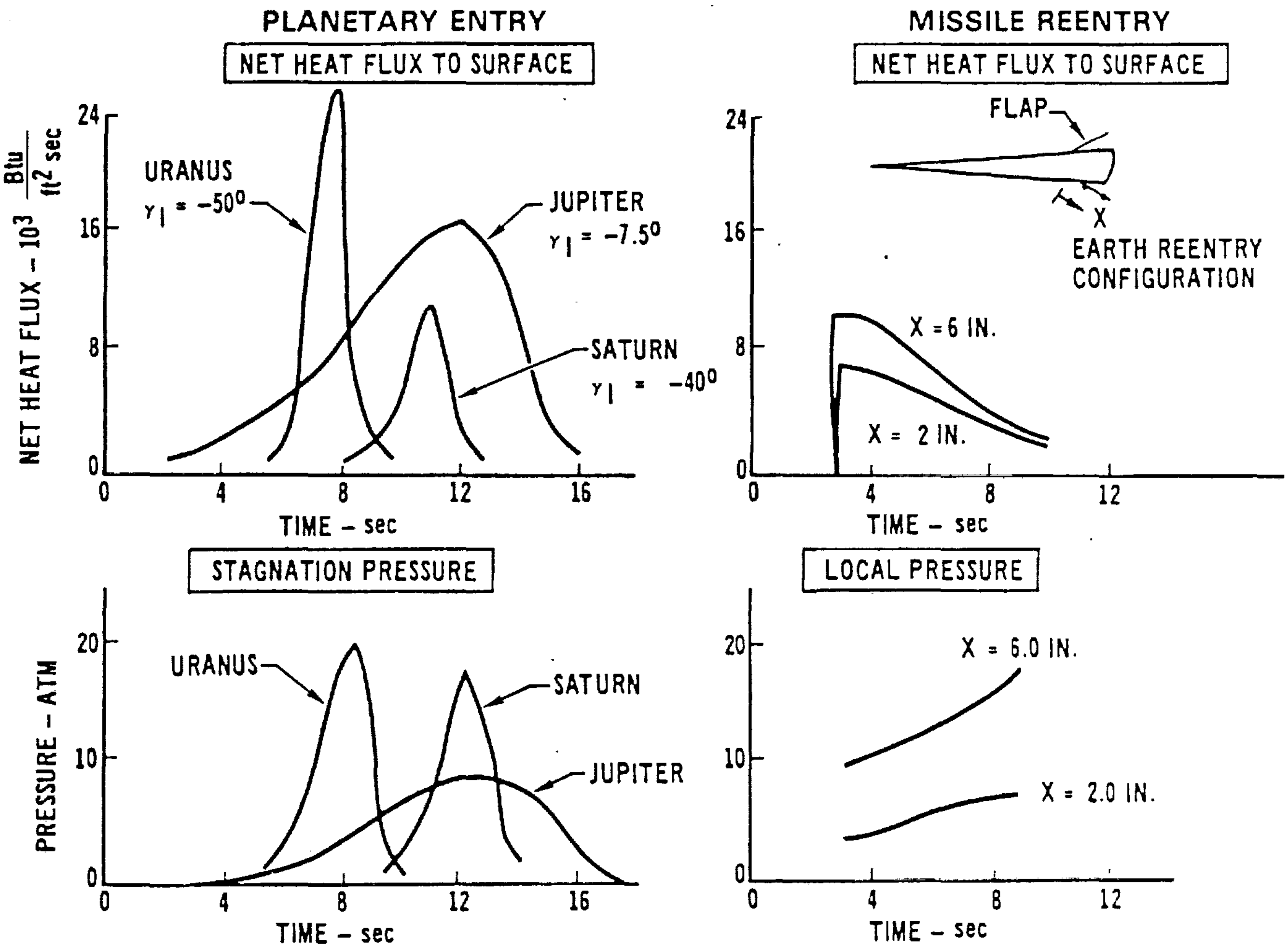

Figure 6-4. Planetary Entry Environments

The comparison in heating is in terms of net heating to the surface; i.e., the reduction in heating due to ablation blowing and hot wall correction has been applied. The comparison is made in this manner since blowing greatly reduces the planetary heat flux but only slightly affects the turbulent heating on the flap. Furthermore, it is presumed that there is no effect in material performance between convective and radiative heating for Carbonaceous materials since the incident radiant energy is absorbed on the surface. If one accepts this assumption, then they could use the missile flap data to base the probe heat shield design. Note, that the pressure levels between planetary and missile entries compare favorably. Pressure is important since mechanical erosion for carbon phenolic ablators has been correlated in terms of this parameter.

Mechanical erosion represents the greatest uncertainty in predicting material performance during planetary entry. The central question is how the material recedes, does it recede primarily due to chemical reaction and sublimation (thermochemical recession), processes that absorb large amounts of energy per pound of material consumed; or is there a large fraction of material removed by bits and pieces (mechanical erosion) resulting in a reduction of material effectiveness. Causes for mechanical erosion have been attributed to preferential oxidation of the binder, high surface temperatures with large temperature gradients and high aerodynamic shear and large pressure gradients. For lack of adequate analytical techniques, we have resorted to empirical correlation of ground test or preferably flight data. The correlation shown in Figure 6-5 is based on the missile flight data discussed earlier. The correlation is in terms of measured total recession rate, mechanical and thermochemical included, ratioed to the predicted thermochemical recession rate versus surface pressure and net heat flux to the surface.

A high degree of uncertainty is present in the application of this correlation to planetary entries, primarily because of

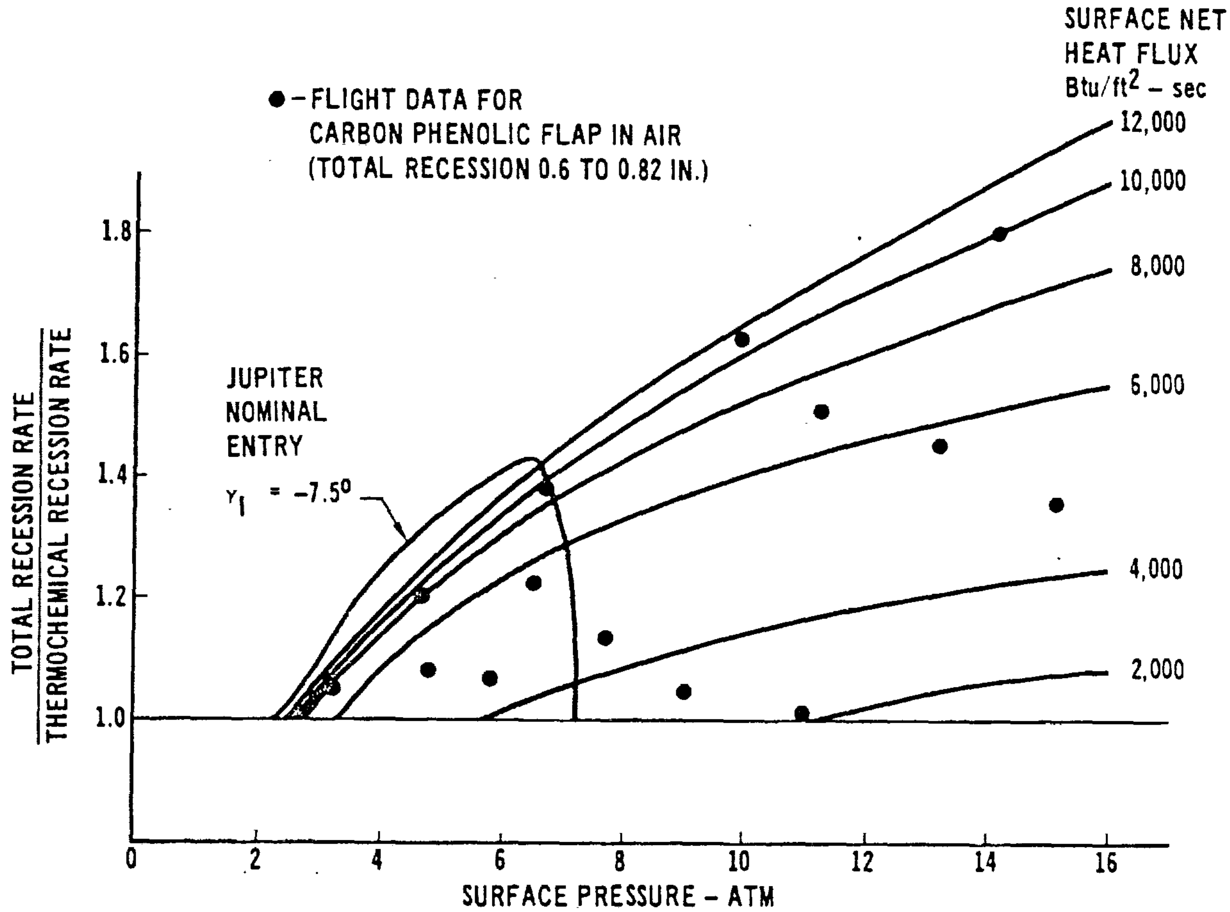

Figure 6-5. Mechanical Erosion Correlation of Missile Flight Data

the difference in environments. However, the correlation is presumed to yield conservative estimates of mechanical erosion since the aerodynamic shear levels were much higher than those expected on the probe.

The Jupiter heat shield thickness based on computation of the thermochemical and mechanical recession and insulation requirements for an 800°F bondline temperature are illustrated in Figure 6-6. Assuming a constant forebody ablative thickness and adding the honeycomb and powder insulation weight results in an aeroshell mass fraction of about 53 percent. Although this is a relatively high weight penalty, it is within the probe weight allotment.

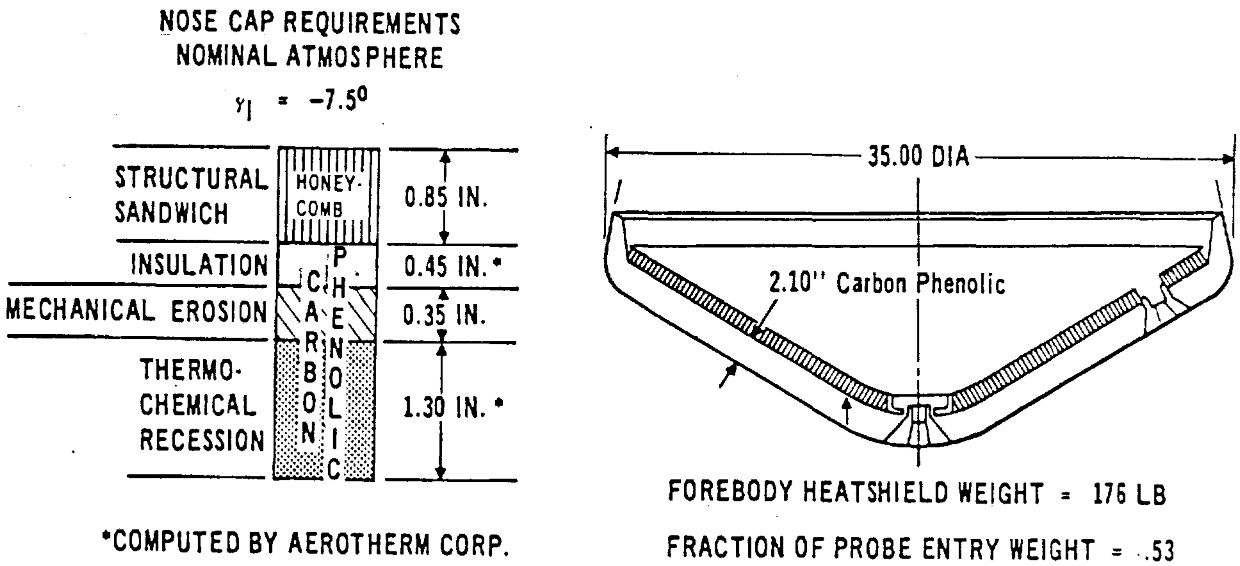

Figure 6-6. Carbon Phenolic Heatshield for Jupiter Entry

A plasma jet test program (Figure 6-7) was conducted in the MDRL 10 Megawatt Facility to obtain performance data on carbon

10 MW MDRL FACILITY

NOSE TIP MODELS	WEDGE MODELS	ENTRY FLIGHT PROBE
q_{CONV} = 8000 Btu/ft^2-sec	q_{CONV} = 3600 Btu/ft^2-sec	q_{RAD} = 5 TO 45,000
P = 10 AND 20 ATM	P = 10 ATM	P = 2 TO 15 ATM
H = 5000 Btu/lb	H = 3400 Btu/lb	H = 2×10^5 TO 5×10^5 Btu/lb
AIR AND N_2	AIR AND N_2	H_2/H_e ATMOSPHERE

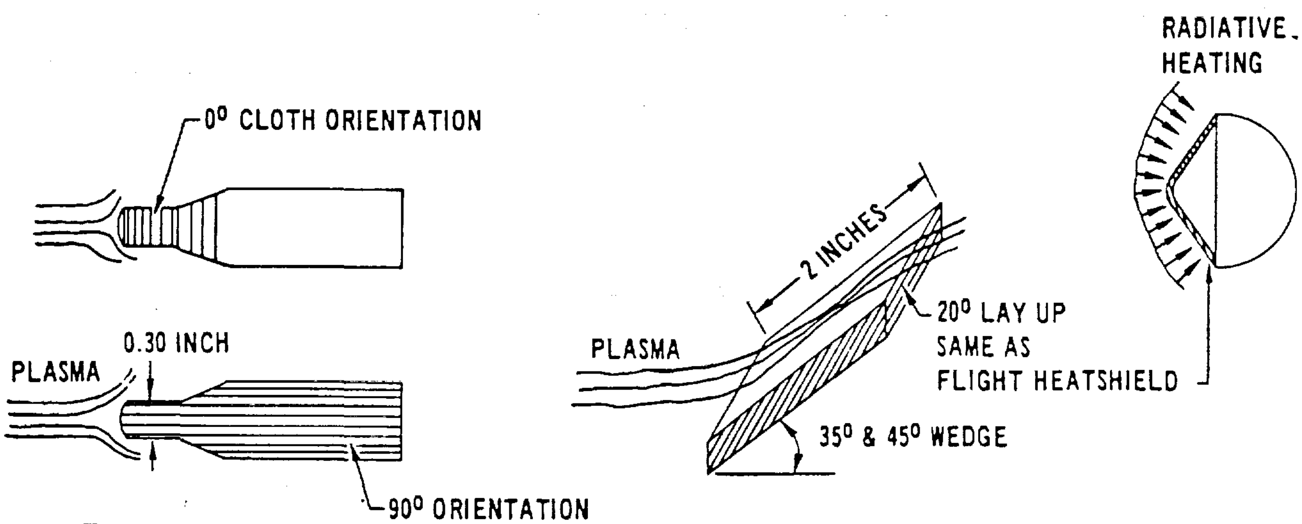

Figure 6-7. Plasma Jet Test Program

phenolic at the possible highest heating rates but at moderate pressures between 10 and 20 atmospheres. A key objective was to evaluate the mechanical erosion phenomena in an oxidizing (air) and an inert environment for possible extrapolation to the hydrogen helium planetary atmospheres. Both nose tip and wedge models were tested in air and nitrogen plasma streams. Much higher heating rates are feasible with the nose tip model, however, the wedge model besides providing a larger test specimen, is also more representative of the flight heat shield in regards to the cloth orientation with the boundary layer flow.

Theoretical ablation predictions have been made to correlate the measured recession rate data. As shown in Figure 6-8, a fair degree of matching the data was achieved in our initial analytical effort and work is continuing in this area to resolve some of the discrepancies. A major problem is the uncertainty in the nose tip recession rate measurements. Contributors to the uncertainties are the relatively small total recession experienced, the initial swelling of the material and the lack of sufficient data points to provide a good average value. Recession measurements were obtained from measurements of the before and after test specimen thickness and from motion picture views of the receding surface. The nose tip motion pictures showed small flakes of carbon phenolic laminates being removed (mechanical erosion) in both the air and nitrogen runs but at a higher rate in air tests. The small nose tip size and the flat laminate lay-up contributed to this mechanism of removal.

Although a number of discrepancies are indicated by the data, the trend of the data indicates a higher mechanical erosion in air than in nitrogen and higher erosion rates in the turbulent higher shear wedge environments.

MR. VOJVODICH: Sam, I think this will probably be a question of general interest, and that is: In the Saturn and Uranus cases you show, as you decrease the entry angle, the heat shield

GAS STREAM	CLOTH ORIENTATION (DEG)	COLD WALL HEAT FLUX Btu/ft^2–sec	SURFACE PRESSURE ATM	MEASURED RECESSION RATE (INCH/SEC)	PREDICTED THERMOCHEMICAL RATE (INCH/SEC)	MEASURED THEORY
			NOSE TIP DATA (LAMINAR)			
AIR	0	6300*	10	0.058	0.057	1.01
N_2	0	6300*	10	0.025	0.035	0.72
AIR	0	6800*	20	0.087	0.070	1.14
N_2	0	6800*	20	0.022	0.031	0.70
			WEDGE DATA (TURBULENT)			
AIR	20	4560**	13	0.064	0.050	1.28
N_2	20	2630	13	(+ 0.005) (SWELLING)	0.006	–

*HEAT FLUX MODIFIED BY NOSE TIP SHAPE CHANGE
**HEAT FLUX BASED ON CUSPED CALORIMETER DATA

Figure 6-8. Preliminary Comparison of Plasma Jet Test Data with Theory

weight goes up. In the case of Jupiter, as you are decreasing the entry angle, the heat shield weight is going down; will you comment on that.

MR. MEZINES: The total heat shield thickness is the sum of the recession thickness plus the insulation thickness needed to limit the backface temperature to a certain value. In general, increasing entry angles result in higher recession but lower insulation requirements. The total thickness or the sum of these two thickness may or may not increase with higher entry angles but will depend on which mechanism predominates. For Jupiter entries, recession is the dominant mode, thus total thickness requirements are higher with increasing entry angles. Conversely, for the Saturn/Uranus entries, the insulation requirement sizes the heat shield thickness; thus higher entry angle entries require less thickness to achieve the same backface temperature.

DR. NACHTSHEIM: Our next speaker is John Lundell who will describe the evaluation of graphitic materials in the Ames high-powered gas dynamic laser.

N75 20387

TESTS OF HEAT SHIELD MATERIALS IN INTENSE LASER RADIATION

John Lundell
NASA Ames Research Center

MR. LUNDELL: As shown above, I have changed the title of the talk from what's listed in the program for several reasons. First of all, I don't think in fifteen minutes we can review the work that's been done on the behavior of graphitic materials in intense heating environments. Secondly, I thought you might be more interested in some very recent results we got testing heat shield materials under intense radiation in our gas dynamic laser.

Figure 6-9 schematically presents our gas dynamic laser. The facility was funded by Paul Tarver, at Headquarters, several years ago when it became apparent that the only way we would get radiative rates of interest for planetary entry - particularly Jovian entry - was to have a laser. It is a gas dynamic laser in which we burn CO to CO_2. It lases at 10.6 microns and produces a continuous output at powers up to about 45 kilowatts. For the test I'll describe today we focused the beam with a one and a half meter focal length mirror and simply re-imaged its focal point on the target, which is sitting out in a room environment. We did have a nitrogen jet blowing in front of the target. It was spaced away from the target such that it was not impinging on the target to cool it. The motive here was to try to blow the plume away.

In some early work we did on graphite in the laser, we found that at low intensities the plume could effectively block about two thirds of the incident radiation, so we wanted to blow it away and let as much radiation get to the target as possible. Thus, the beam impinges on the target, and what we do is measure the time from the moment it impinges until it first burns thru. That is, we are measuring burn-thru time. We do that with either TV or movie cameras, and we also measure the surface temperature

C-6

EXPERIMENTAL SET-UP

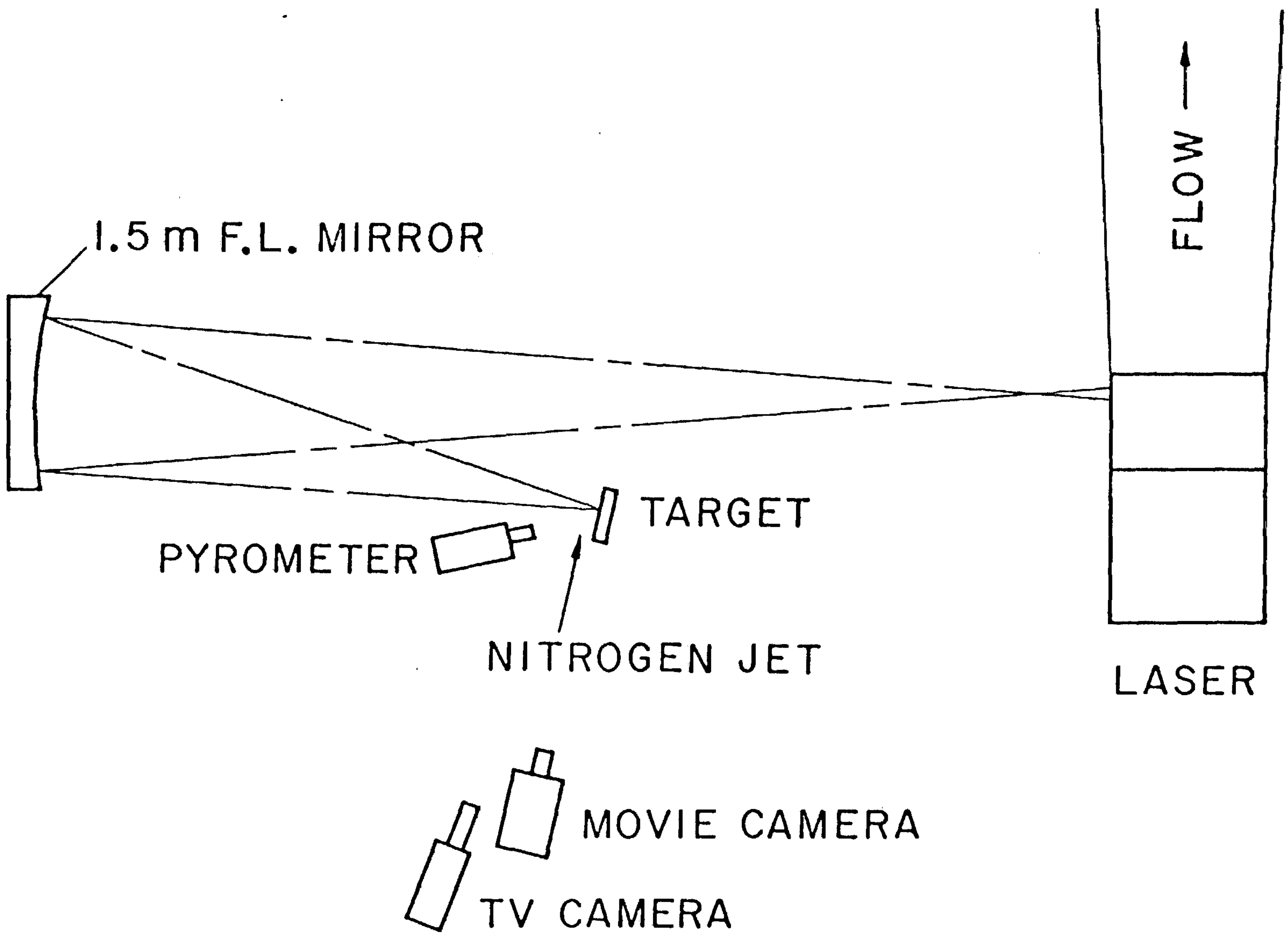

Figure 6-9

by focusing an automatic optical pyrometer on the irradiated spot on the target.

Figure 6-10 shows the test conditions. We looked at three different materials: ATJ graphite, which is a representative, fine-grain graphite typical of what's being used for ballistic missile nose tips today; Carbitex 100 is a carbon-carbon composite which is made by Carborundum Corporation. We found in our preliminary survey of a lot of different materials, in the laser, that Carbitex was the best carbon-carbon composite that we tested. The third material is a phenolic carbon. This is representative of what's being used as a heat shield material on ballistic missiles today. These models were furnished by McDonnell Douglas, St. Louis. Carbon phenolic is simply made by stacking up layers of carbon cloth and then, essentially, gluing them together with a phenolic resin.

We placed the models in the laser beam at a point where we had about a third of a square centimeter irradiated spot. We had to go to that small a spot in order to get intensities of interest. So, what we did, then, was to leave the models at the same point in the beam and vary the output power of the laser from essentially four to 35 kilowatts. If we divide these power numbers by the area of the irradiated spot, we come up with the indicated average intensities: from ten to 92 kilowatts per square centimeter; in English units, from 9,000 to 81,000 BTU's per square foot per second. Now I want to emphasize that these numbers are the average intensity. The laser does not have a spatially uniform output beam; it's more Gaussian. So, the peak intensity may be a factor of two or more above the average intensity; at this time, I don't know the ratio of the peak to average intensity. You should note that the burn-through time is probably more closely related to the peak intensity than the average intensity.

Incidentally, we selected these conditions so that the lowest intensity would represent entry into Jupiter using the warm at-

TEST CONDITIONS

MATERIALS: ATJ GRAPHITE

CARBITEX 100

MDAC PHENOLIC CARBON

EXPOSED AREA: 0.377 cm^2

POWER	AVERAGE INTENSITY	
3.86 kw	10.2 kw/cm^2 ;	9,000 Btu/ft^2-sec
19.2	51.0	44,900
34.7	92.1	81,200

Figure 6-10

mosphere, at about a six-degree angle. The intermediate intensity represents a nominal atmosphere, going in at about seven degrees; and the highest intensity represents the cold atmosphere, going in at about nine degrees.

Figure 6-11 shows the results we obtained at the lowest intensity, namely, an average intensity of 9,000 BTU/FT^2-sec. What I am plotting here then is, essentially, the target thickness against the burn-through time. For each of these materials we ran three or four different thicknesses from about an eighth of an inch up to in excess of a half inch. As you can see, the curves, then, to obtain a burn-through velocity or the velocity at which the beam penetrates into the material.

We find that for this condition ATJ graphite has the lowest penetration velocity, about an eighth of an inch per second; and the carbon phenolic was in excess of a half inch per second; and the carbitex fell in between.

Figure 6-12 shows the results we obtained at the intermediate intensity. Here I am plotting the same coordinates. The relative ranking in the materials is the same: ATJ has the lowest velocity, then the Carbitex, and then the Phenolic carbon. Note that we are up to penetration velocities in the order of one to almost two inches per second.

Figure 6-13 shows the results for the highest intensity; up around 81,000 BTU/FT^2-sec. The relative ranking in the materials is still the same: ATJ is the lowest and phenolic carbon the highest. However, you will note now that the materials are all kind of coalescing together as far as performance goes. We have penetration velocities from 2.2 up to about two and three quarter inches per second. For the carbon phenolic point, for example, the thickest model was 1.08 inches and the beam penetrated that in about .39 seconds.

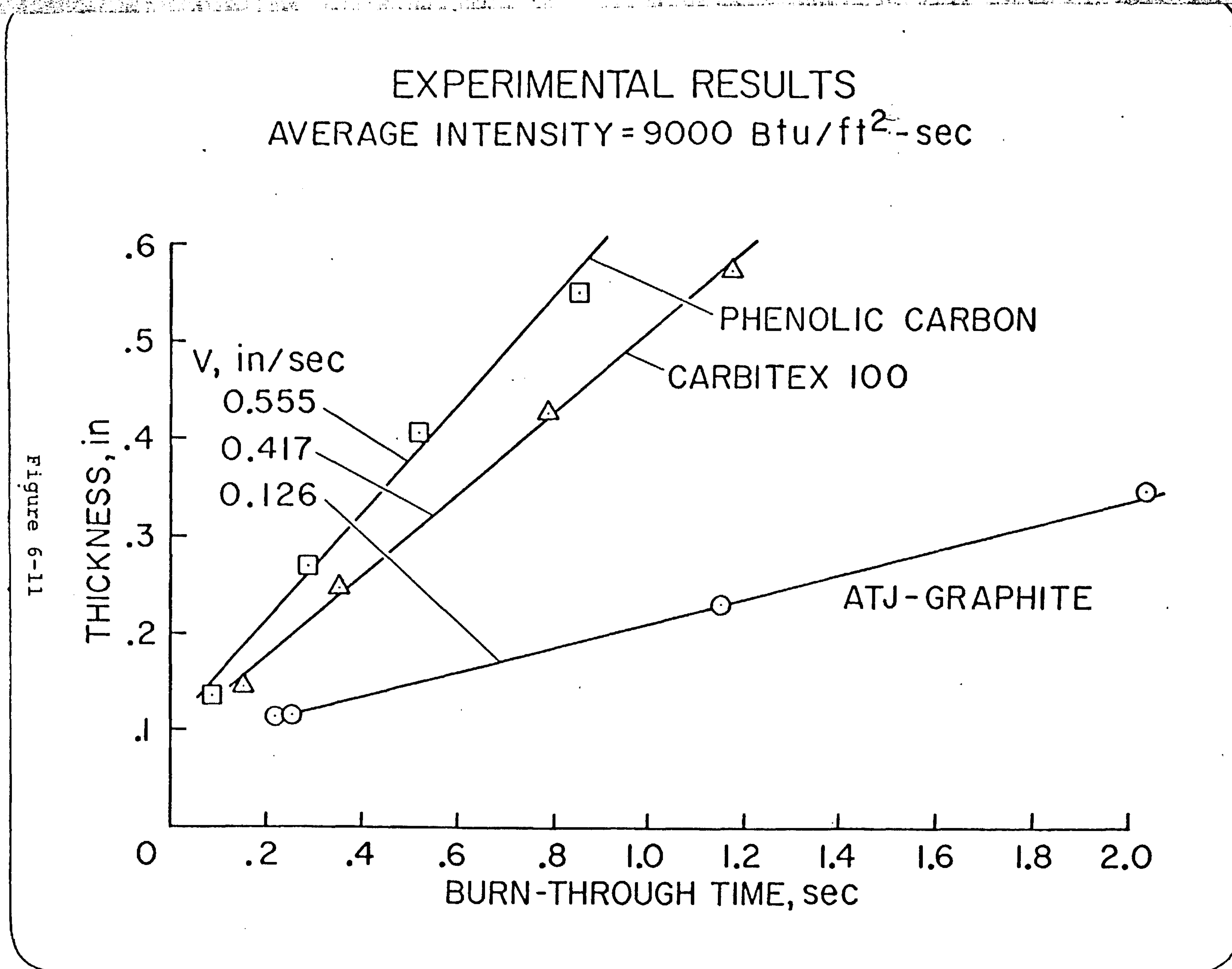
EXPERIMENTAL RESULTS
AVERAGE INTENSITY = 9000 Btu/ft^2-sec
V, in/sec
0.555
0.417
0.126
PHENOLIC CARBON
CARBITEX 100
ATJ-GRAPHITE
THICKNESS, in
.1
.2
.3
.4
.5
.6
0
.2
.4
.6
.8
1.0
1.2
1.4
1.6
1.8
2.0
BURN-THROUGH TIME, sec

Figure 6-11

EXPERIMENTAL RESULTS

AVERAGE INTENSITY = 44,900 Btu/ft^2-sec

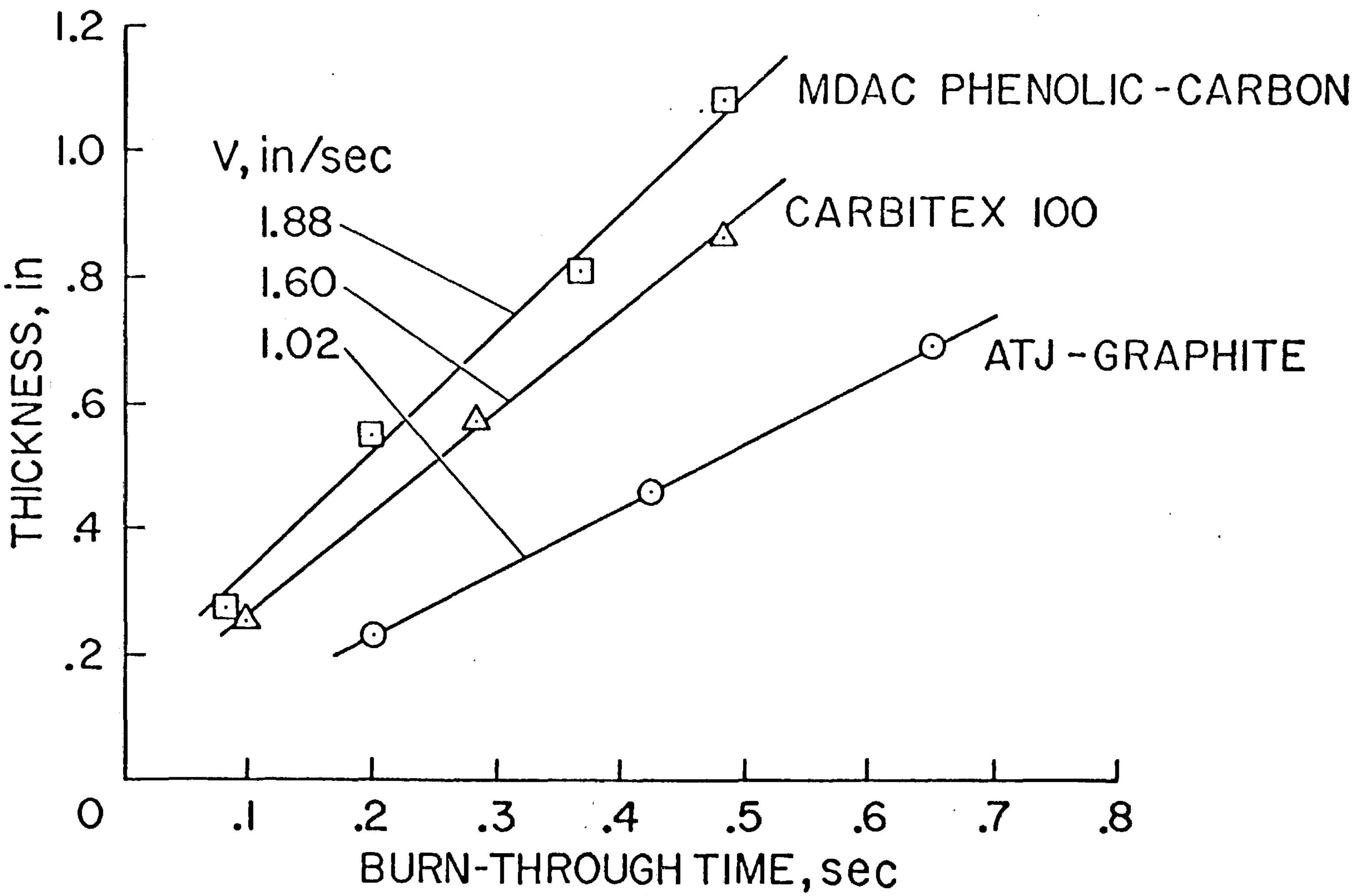

Figure 6-12

VI-20

EXPERIMENTAL RESULTS

AVERAGE INTENSITY = 81,200 Btu/ft^2-sec

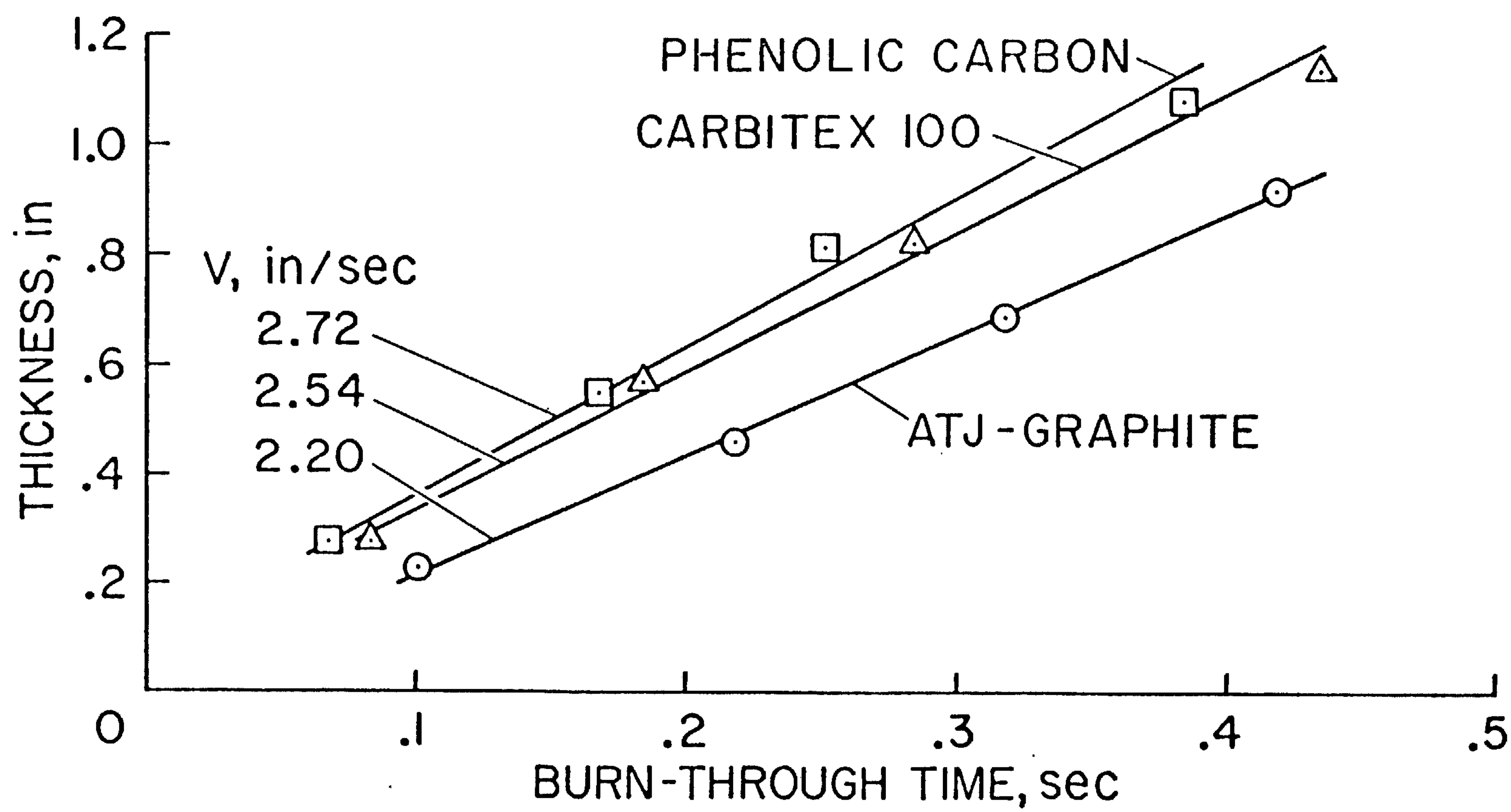

Figure 6-13

I thought it might be of interest to show you very briefly a film clip of the test of that particular model to give you an idea of what these things look like when they get hit with very intense radiation. (Film clip shown)

MR. LUNDELL: We shot these pictures at 600 frames per second and they are being projected at 24, so we are slowing it down by a factor of twenty five.

The film indicates that the carbon phenolic puts on quite a fireworks display at this intensity level. The other materials give you about the same amount of plume, but you don't see as much evidence of particulate mass loss as you see with carbon phenolic.

Figure 6-14 summarizes the results in terms of mass loss rates. The quantity we were determining from the previous slides was the recession velocity. If you multiply that by the density of the material, you can get a mass loss rate. So, that is what we have here for the various average intensities and the three different materials: ATJ, Carbitex and carbon phenolic. As you can see, at the lowest intensity we've got almost a factor of four to one difference in the mass loss rate between the graphite and the carbon phenolic. When we get to the intermediate intensity, this ratio drops to about 1.5. They got about 50 percent more mass loss rate for the carbon phenolic. And when we get to the highest intensity, they are all pretty comparable: from about 18 to 21 lbs/ft^2-sec, which was a pretty good mass loss rate. To give you an idea of what that compares to in our convective tests, I think the highest ablation rate I ever obtained in a convective test on graphitic materials was about a half pound per square foot per second.

These results are shown graphically on Figure 6-15, where I'm plotting the mass loss rate against intensity. As you can see, and as I noted before, down at the lowest intensity we have

SUMMARY OF EXPERIMENTAL RESULTS

	MASS-LOSS RATE, lb/ft^2-sec		
AVG INTENSITY Btu/ft^2-sec	ATJ GRAPHITE	CARBITE × 100	PHENOLIC CARBON
9000	1.13	3.02	4.21
44,900	9.18	11.6	14.3
81,200	19.8	18.4	20.7

Figure 6-14

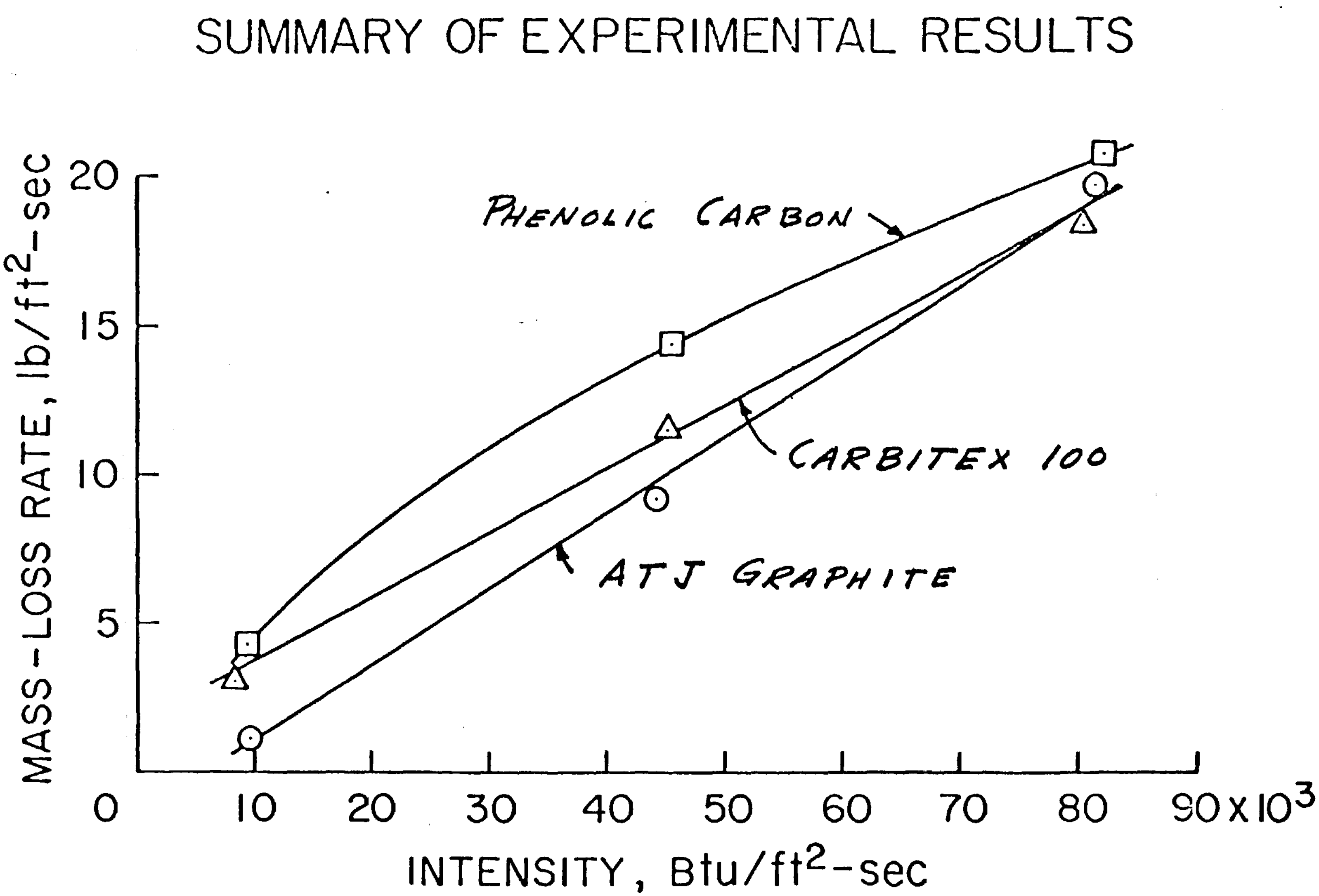
SUMMARY OF EXPERIMENTAL RESULTS
MASS-LOSS RATE, lb/ft²-sec
INTENSITY, Btu/ft²-sec
0
5
10
15
20
0
10
20
30
40
50
60
70
80
90 x 10³
PHENOLIC CARBON
CARBITEX 100
ATJ GRAPHITE

Figure 6-15

the largest difference on a relative basis between the materials; and when we get up to the highest intensity, they are all running about the same.

The thing to note here, however, is that the two all-carbon materials are performing better than the phenolic carbon; and this isn't too surprising. A predominant heat accommodation mechanism under these severe heating conditions is sublimation and, in that case, you want as much carbon up front as you can get. These curves do turn out to be linear and if you take the slope of this curve for ATJ you will come up with an effective heat of ablation of about 4,000 BTU's per pound, which is about half the heat of sublimation if one assumes that the specie being sublimed is C_3.

The curvature in the phenolic carbon curve, I think, is probably due to the fact that we've got the phenolic there complicating things when it pyrolyzes.

In conclusion I'd like to say that it does appear as though the heat shield problem is going to be rather severe for entry into the outer planets but, with the laser and the up-coming arc-jet facilities which are going to be developed here at Ames and which Howard Stine will describe shortly, I think we will be able to do a pretty good job of simulating entry into the outer planets and we will be able to determine why these materials perform the way they do under these intense heating environments. Then, we will be able to design the flight heat shield with a great degree of confidence.

UNIDENTIFIED SPEAKER: Because of the linear relationship in your last chart there it seems fair for an actual entry case where the heating intensity reaches a peak and then comes down to just integrate the area under it and make the thickness proportional to that?

MR. LUNDELL: Yes, I think that would be a pretty reasonable thing to do, for a first approximation, based on what we know now. In other words, I think even though the heating rate is varying very rapidly with time, you are going to stay pretty close to thermal equilibrium.

UNIDENTIFIED SPEAKER: Did you get any surface temperature measurements?

MR. LUNDELL: Yes, we did. They are running about 7400° Rankine; that's about 4100°K.

UNIDENTIFIED SPEAKER: Do you consider your monochromatic results reasonably applicable to the real case?

MR. LUNDELL: That's the real question in using the laser as a simulation facility for planetary entry. In a planetary entry case we expect radiation in the visible and the UV and, of course with the laser we are way out in the infrared. In answer to your question, I think it's okay for graphitic materials, or black materials. It certainly would not be for the reflective materials.

UNIDENTIFIED SPEAKER: Is that 2.2 inches per second some sort of a world's record?

MR. LUNDELL: It is for me.

DR. NACHTSHEIM: The next speaker will be Bill Congdon from Martin Marietta and there is a slight discrepancy in the program: he will be describing Dave Carlson's work, which is the applicability of the Pioneer Venus hardware to Saturn probes, and he will also be discussing Martin's efforts on the development of silica heat shields. So, in his talk he will essentially make two talks, and make the transition from the evaluation of heat shield materials to the development of heat shield materials.

N75 20388

MAJOR UNCERTAINTIES INFLUENCING ENTRY PROBE HEAT SHIELD DESIGN

W. Congdon
Martin-Marietta Corporation

MR. CONGDON: I'm going to start out wearing, appropriately, a gray hat this morning as I present Dave Carlson's paper, but as I move on to the second paper, I think you will notice the hat becoming progressively whiter.

As Phil just mentioned, the first paper discusses major uncertainties influencing the design of an outer planet probe heat shield; these uncertainties were ones which were considered most critical in our recent study effort on the adaptability of existing Pioneer Venus hardware to a Saturn/Uranus probe. The second paper gives some of the accomplishments and interesting results which we at Martin-Marietta have seen so far in our effort to develop a high purity silica reflecting heat shield for outer planet missions.

Most of the material that I planned to present in this first paper on probe heat shield design uncertainties has already been discussed in considerable detail this morning by other speakers. Therefore, to cut down on a lot of redundancy, I will go through these view graphs rather rapidly and just re-emphasize major points.

As you have seen several times this morning, there is quite a large range in the entry heating environments to be expected for an outer planet probe (Figure 6-16). This is due primarily to large uncertainties in composition and scale height of the planet atmospheres. This Figure shows analytically predicted convective and radiative heating rates vs. time, covering the cool, nominal and warm atmosphere extremes for a Saturn entry probe. For the cool dense atmosphere, entry heating consists of very intense convective and radiative fluxes for very short time periods. For the warm atmosphere extreme there are long convective and radiative pulses of relatively low intensity. Also, it is very evident that the importance of the radiation component changes significantly in going from the cool atmosphere to the warm atmosphere, which has a

Range of Predicted Entry Heating for Saturn Entry Due to Uncertainty in Atmospheric Composition

MARTIN MARIETTA
DENVER DIVISION

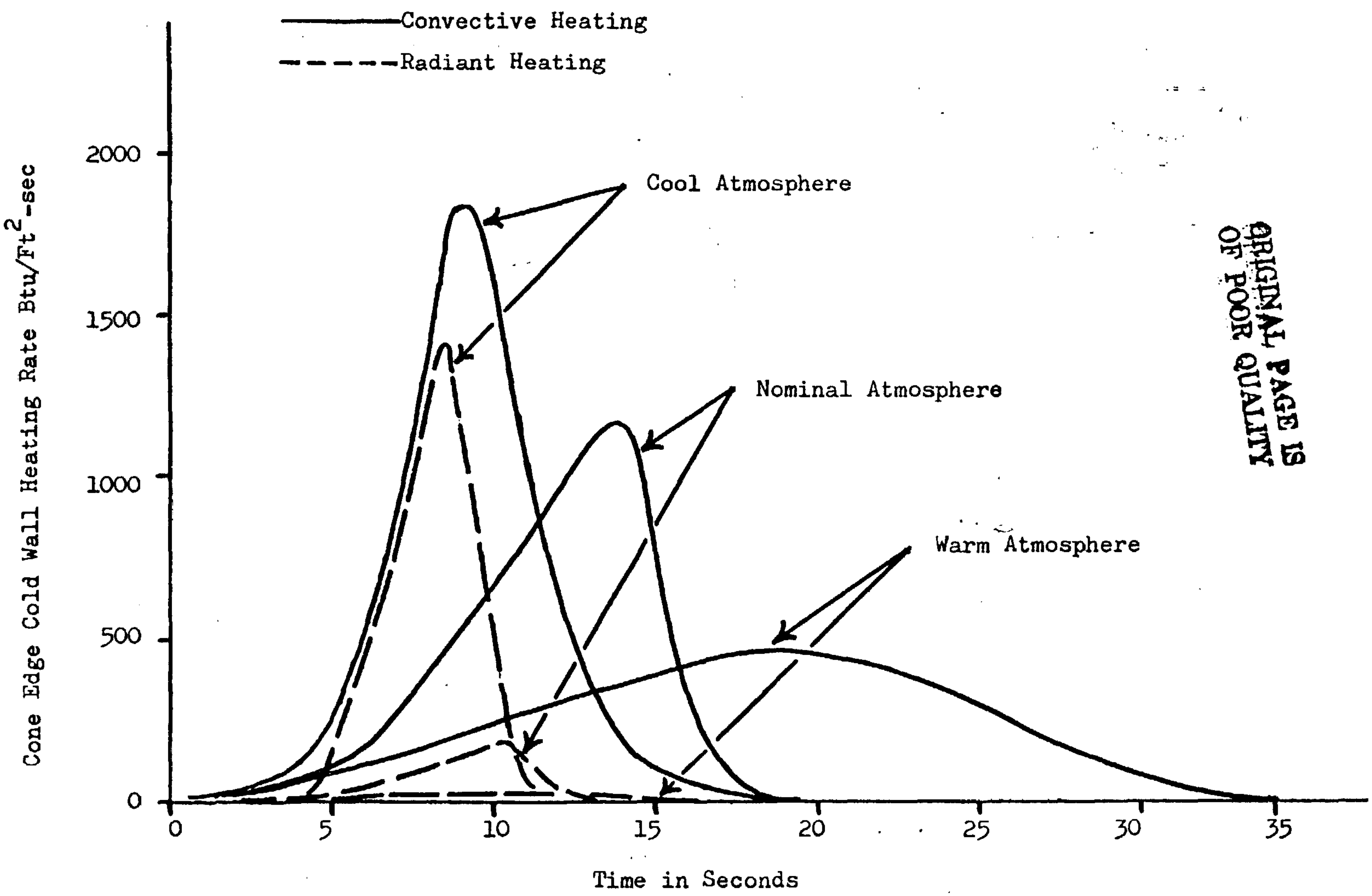

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Figure 6-16

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bearing on reflecting heat shield use; the cool to nominal range is the range where a silica heat shield could be used most effectively.

Now when you size a heat shield, you have to cover the extremes in the entry environment. For the engineer, it is very difficult to design the most efficient heat shield for such a wide variation in the anticipated entry environment as shown in this typical case; on the one hand, the heat shield is designed for high surface recession and, perhaps, spallation, while on the other hand, the heat shield is designed for thermal soakback. Unless such large uncertainties can be narrowed, the heat shield system cannot be fully optimized.

A second item in this first category of heat shield uncertainties (Figure 6-17) - a category which we could label as "Entry Heating Uncertainty" - is the uncertainty of the effects of ablation species on entry heating. This slide shows radiative flux correction vs. mass injection rate and convective flux correction vs. mass injection rate. One would expect, normally, that the radiative flux would be attuned or blocked by ablation species. Analytical predictions recently performed here at Ames and at Aerotherm have shown that for Saturn/Uranus entries, using carbon and silica based heat shields, there is an augmentation of the radiation flux at lower values of the mass injection rate parameter. This is shown in this first graph at values on the abscissa less than one. The ablation species themselves are radiating. More computer analyses are needed to further definitize the shapes and values of these curves - as you can see in this graph, both curves are based, essentially, on only three points.

In the second graph are shown a curve of analytically predicted convective blocking plotted out to high mass injection rates expected for Saturn/Uranus entry, and a curve of

Uncertainty in Entry Heating Blocking for Massive
Blowing Conditions During Outer-Planet Entry

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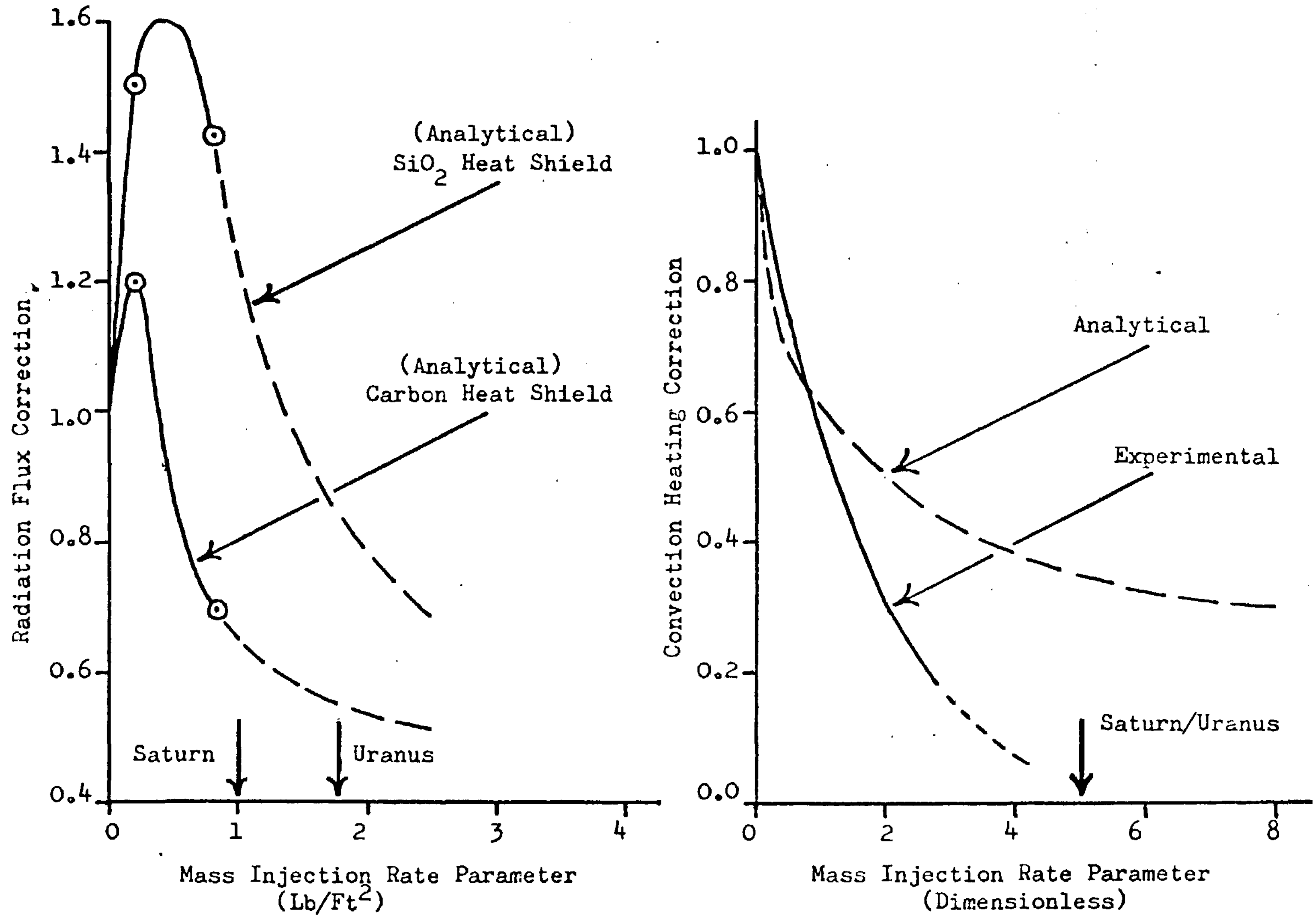

Figure 6-17

convective blocking based on a correlation of some earth re-entry and ground test data for relatively low injection rates. At higher values of the mass injection rate parameter there is disagreement between the two curves. As addressed by several speakers earlier this morning, this is not necessarily an analytical shortcoming, but rather, a consequence of radiation/convection interaction at such high entry velocities. The point of this graph is that there is considerable uncertainty in the magnitude of convective blocking for Saturn/Uranus and other outer planet entries and the heat shield sizing strongly depends on degree of blocking. More computer work should be performed to further definitize convective blocking as well as radiative blocking and, wherever feasible or possible, tests should be conducted to confirm the analytical predictions.

A second category of major uncertainties influencing entry probe heat shield design is uncertainty in material performance (Figure 6-18). For the carbon based ablators, probably the biggest uncertainty is the uncertainty of spallation under intense heating. This was discussed earlier by John Lundell and other speakers. Spallation is difficult to model analytically and, in addition, adding extra thickness to the heat shield to prevent spallation failure modes can lead to an excessively heavy heat shield. Tests and flight experience with carbon phenolic have shown that this material is susceptible to char cracking and spallation. At Martin Marietta, research has been performed to come up with an improved carbon phenolic, one less prone to spallation, and some progress has been made to date in this area. Shown in this slide are two different formulations of carbon phenolic tested under the same conditions, radiation exposures at 1500 Btu/ft^2-sec for 3 seconds. The formulation on the left was found to spall consistently, while the one on the right was very resistant to spallation under these test conditions. More development is needed on carbon ablators to further reduce spallation problems.

Uncertainty of Carbon Phenolic Spallation
Under Intense Heating

MARTIN MARIETTA
DENVER DIVISION

Models Viewed After Exposure to Xenon-Arc Lamp Radiation at 1500 Btu/Ft^2-sec

3 Sec. Exposure	3 Sec. Exposure
Char Cracking	Char Cracking
Spallation	No Spallation

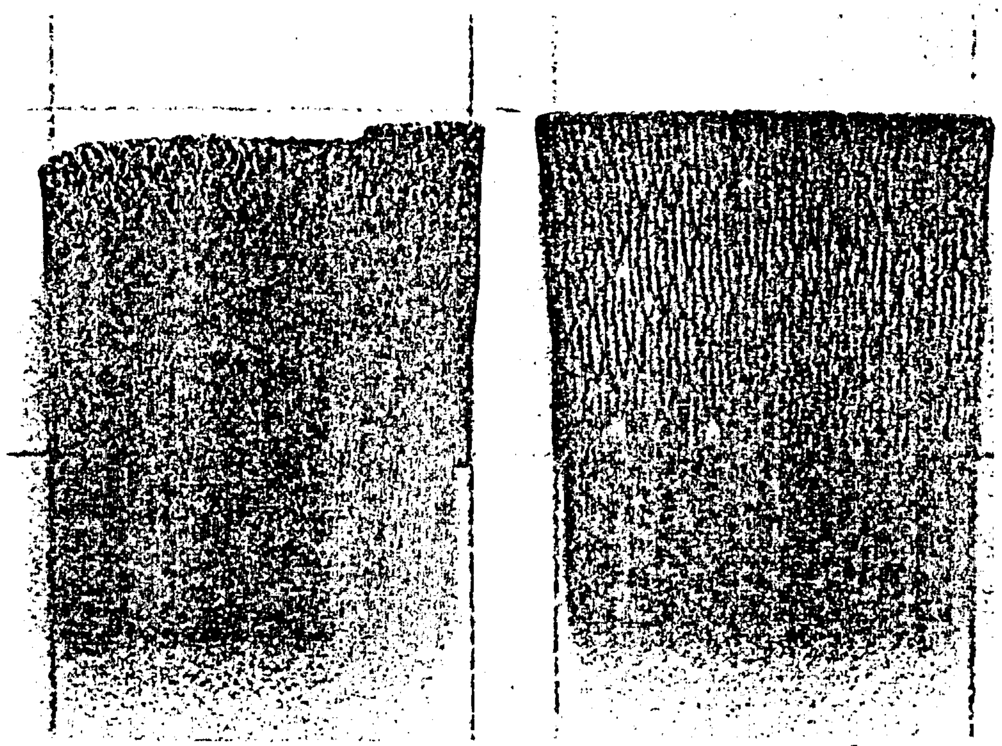

Figure 6-18

Moving on to the white reflective materials, fuxed silica in particular; when a silica heat shield reaches temperatures in excess of approximately 1700°C, the particles begin to coalesce, voids are destroyed and the heat shield begins to become transparent. This bulk vitrification event is a severe failure mode because the radiation can be transmitted directly to the substructure. The presence of impurities in the silica matrix, especially alkali metals, enhances vitrification, primarily because the alkali metals cause stronger absorption of short-wavelength visible and ultraviolet radiation and the heat shield heats up more rapidly. We at Martin Marietta have made progress in developing a silica heat shield which is resistant to bulk vitrification under high intensity radiation. This was accomplished primarily by going to higher purity fused silica powders. Figure 6-19 shows a material which we fabricated and tested last year under our IRAD program. The material could withstand high intensity xenon-arc lamp radiation of about 1000 Btu/ft^2-sec for times in excess of 25 seconds. This model was one that was exposed for 25 seconds. Except for a thin layer of powdery silica on the surface, the model was not degraded in any obvious way by the exposure. The model shown here on the right was exposed for 30 seconds and it did vitrify. These models, by the way were about 0.2 inch thick. For comparison, some commercial materials that we tested, for instance some Glasrock products, vitrified in about 3 seconds under the same radiant flux. So we have made noteworthy progress in developing an improved silica reflector, we have delayed the occurrence of bulk vitrification out to relatively long time periods. The fused silica configurations that we are presently working on are even better performers than this IR&D-developed configuration; this is the subject of the next paper. An uncertainty with a fused silica reflecting heat shield is this: we must be certain that we have a material that can withstand the combined radiative and convective pulses without becoming transparent at a critical moment causing failure; we must be certain of the conditions at which bulk vitrification occurs.

Uncertainty in Silica Vitrification
Under Intense Heating

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Models Viewed After Exposure to Xenon-Arc Lamp Radiation at 1000 Btu/Ft^2-sec

25 Sec. Exposure — No Vitrification

30 Sec. Exposure — Vitrification

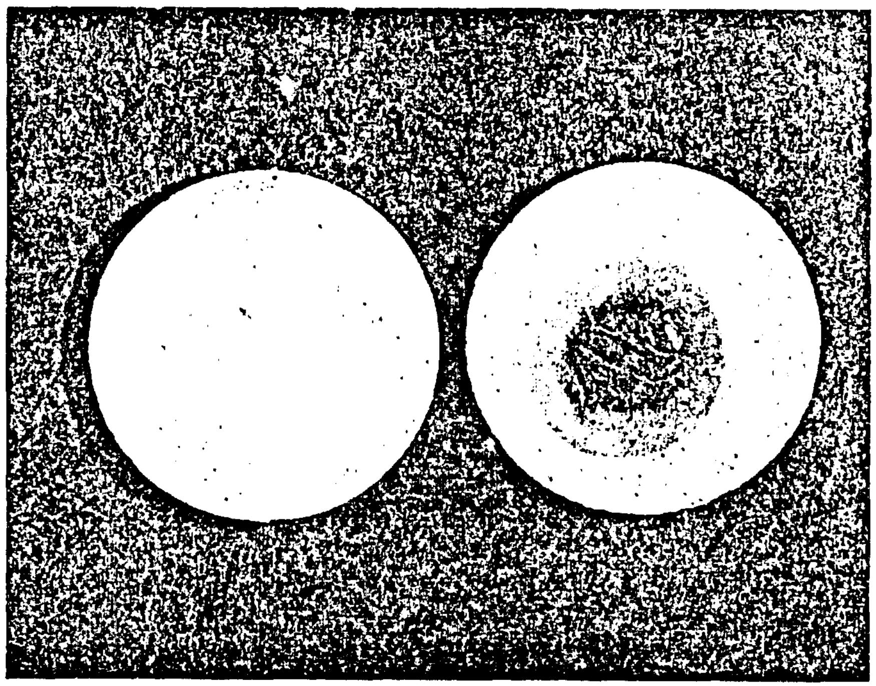

Figure 6-19

Summarizing, briefly, some of the major uncertainties which I have discussed in this paper; the outer planet entry environments are not well defined because of uncertainties in composition and scale height of the planet atmospheres; the augmentation/attenuation of entry heating by ablation products requires more computer study and testing where possible; carbon heat shields, especially carbon phenolic, possessing improved resistance to spallation need developing, and white silica reflecting heat shields with improved resistance to bulk vitrification need further developing.

That wraps up, essentially, the points that I wanted to cover in this first paper.

DR. NACHTSHEIM: Before you move to the second paper, I think it is appropriate to note that for the technology that is in hand, aside from Jupiter, the biggest uncertainty in sizing the heat shield, from this study, is apparently what is the atmosphere; whether it is the cold or warm atmosphere. And that, coupled with the severe problems for Jupiter - that problem also persists here - I think it is appropriate to draw that conclusion to conclude this talk. And if there are any other questions at this time, before Bill goes on, I would like to entertain them now.

DR. JOHN LEWIS: Just a brief comment: there is reason to anticipate that the blips on these model atmospheres will be brought down closer to the nominal models, most especially the helium rich Uranus model atmospheres and I think it would be very hard to find anywhere models which look like those engineering models of the atmosphere generated as extreme cases with engineering problems in mind and the penalties that were being paid to meet them are obviously out of proportion to the probability that they are real.

UNIDENTIFIED SPEAKER: What was done to the silica materials that you developed to retard bulk vitrification, the models shown in the last slide?

MR. CONGDON: The primary emphasis of this work was just going to higher purity materials and using non-contaminating processing techniques.

UNIDENTIFIED SPEAKER: The models shown in the last slide, are those two the same materials that you have there?

MR. CONGDON: Yes

UNIDENTIFIED SPEAKER: And it takes about thirty seconds to vitrify them?

MR. CONGDON: Let us say something in excess of 25 seconds. When we originally started developing and testing fused silica reflectors, some of the moderate purity materials would vitrify in, say, ten seconds for this exposure. So by going to higher purity materials - materials containing lowered levels of alkali and alkaline earth metals, especially - we were able to delay that bulk vitrification event out to longer time periods.

HIGH PURITY SILICA REFLECTING HEAT SHIELD DEVELOPMENT

WILLIAM CONGDON
Martin-Marietta Corporation

MR. CONGDON: I think most of you here today are familiar with the basic principles of the reflecting heat shield concept. But, just as a brief review, a reflecting heat shield is composed of highly transparent materials with differing refractive indices. Reflections and refractions occur at the interfaces between these materials and the macroscopic result is diffusely scattered radiation. This is the geometrical optics interpretation of scattering. In a reflecting heat shield, the scattering is sufficiently intense to reject the shock layer radiation, reflecting it back through the front surface of the material. If the materials were not highly transparent, the radiation would be absorbed within a few scattering events. In a fused silica heat shield, scattering results from the refractive index mismatch between silica particles and the voids introduced during the fabrication process.

An important consideration in the selection of materials is what is the spectral distribution of shock layer radiation to be scattered? As you can see, Figure 6-20 gives the predicted spectral distribution for entry into the Saturn nominal atmosphere. Radiation intensity is plotted vs. wavelength in eV and microns. I tend to think in microns but both are given. The major portion of the radiation for this non-ablating wall spectrum is between about 0.7μm and 0.2μm, which is essentially, the visible and near ultraviolet. When the heat shield ablates, this, of course, will be perturbed due to absorption and emission by the ablation species. There are analytical indications that silica ablation species shift the spectrum to longer wavelengths and this is a favorable trend. However, as mentioned earlier, at some mass injection rates, the net radiant flux to the wall is increased by silica ablation species. But the increased radiation is mostly at wavelengths where silica is a very efficient reflector.

Predicted Radiant Heating Spectral Distribution for Saturn Entry
(Incident Flux to Non-Ablating Wall)

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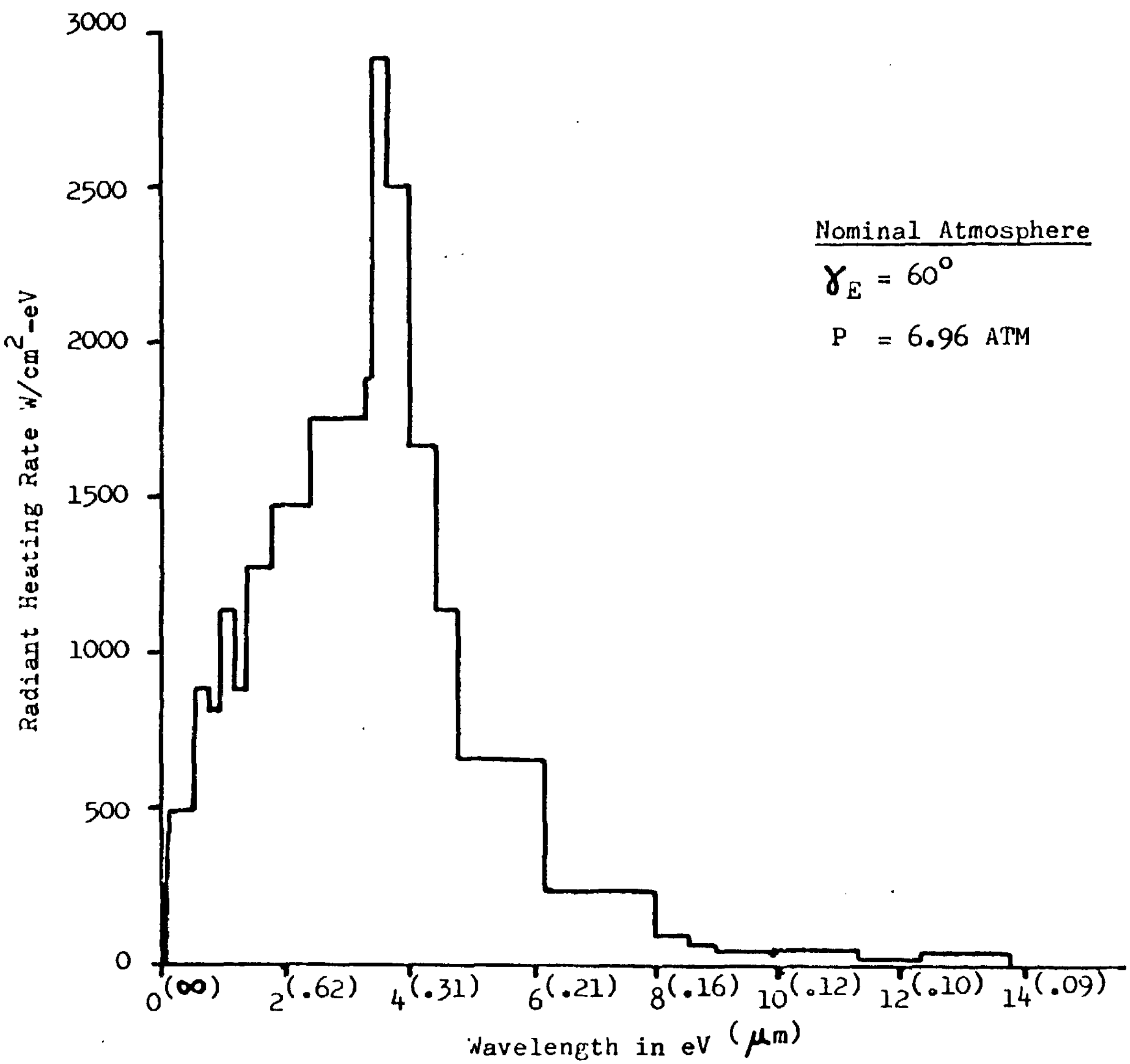


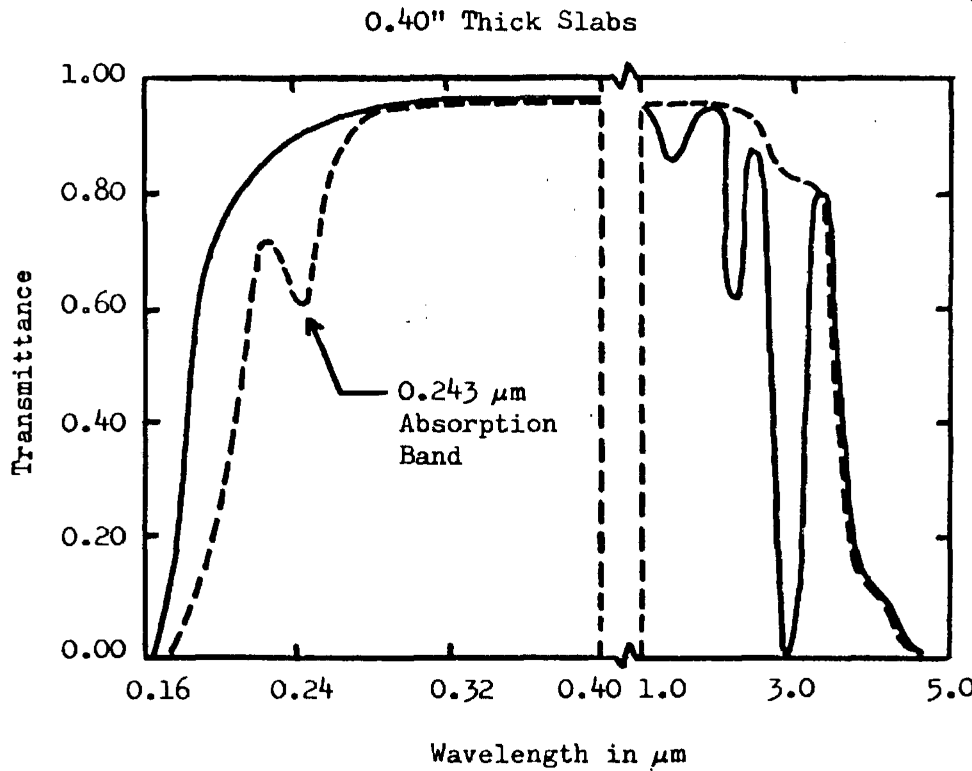
Figure 6-20

Figure 6-21 shows transmittance vs. wavelength for 0.4 inch thick slabs of 100% dense clear fused silica. For our purposes, fused silica materials can be classified into two general categories. Type A fused silica is a synthetic material, usually prepared by vapor phase hydrolysis of silicon tetrachloride. This ultra-high-purity material contains characteristic absorption bands shown in this slide at 1.38, 2.22, and 2.73μm - infrared absorption bands, which deserve little concern because they are at wavelengths longer than the bulk of predicted shock layer radiation. This synthetic material has very high transparency down to the 0.16μm cut-off. The second category, Type B fused silica, is an upgraded and fused natural quartz capable of very high purity. This type has a characteristic absorption band at 0.243μm - the cause of this absorption band is not fully understood. The material is not as transparent in the ultraviolet as the Type A material but is still very transparent. Recalling the spectrum of the previous figure, the synthetic fused silica would be the preferred material to use for a reflecting heat shield because of its higher transparency at shorter wavelengths. A disadvantage of Type A silica is that it is approximately two orders of magnitude more expensive than Type B silica.

I want to point out that this slide shows room temperature transmittance. At higher temperatures, there is a significant shift of the ultraviolet absorption edge of these materials to longer wavelengths. Some of you are familiar with an article by Beder, Bass, and Shackleford, which showed that at 1500°C, the shift for the Type A fused silica is up to about 0.24μm. Silica ablates at about 2800°C, so the location of the absorption edge could be expected to be at even longer wavelengths at ablation temperatures. Therefore, reflectance falls off at shorter wavelength visible and ultraviolet regions for a silica reflecting heat shield during entry. Anything that can be done to improve reflectance - such as tailoring the morphology; void size, particle size, volume density - even by relatively small amounts, could be of significant benefit in terms of overall heat shield performance.

Spectral Transmittance for Thin Slabs
of Two Types of Clear Fused Silica

MARTIN MARIETTA
DENVER DIVISION



Type A Fused Silica

Synthetically produced ultra-high purity fused silica, usually prepared by vapor phase hydrolysis of silicon tetrachloride. Contains no UV absorption band at 0.243 μm, but does have IR absorption bands at 1.38, 2.22 and 2.73 μm.

Type B Fused Silica

Produced by upgrading and fusing natural quartz, capable of very high purity. Contains a UV absorption band at 0.243 μm, but does not show IR absorption bands at 1.38, 2.22 and 2.73 μm, common to high-hydroxyl content material.

Figure 6-21

So, one of the ways to improve reflectance is to go to higher purity materials. Preferably, the Type A synthetic fused silica should be used because of its higher transparency. Purity effects were discussed in the previous paper. What degree of improved reflectance can be obtained by tailoring the morphology? This is one of the quesitons being addressed in our present effort at Martin Marietta and is the main subject of what I want to cover in this presentation.

To start, we addressed the question of morphology analytically, using a radiation scattering computer program. This program dubbed MSAP for Multiple Scattering Analysis Program, couples the exact Mie solutions of Maxwell's equations for single particle scattering with the phenomenological equations of Kubelka-Munk and predicts scattering performance based on intrinsic material properties and relative sizing parameters. The next three slides show MSAP predictions. I would like to point out that the important thing of these figures is not the absolute values of reflectance but the indicated trends.

Figure 6-22 shows hemispherical reflectance vs. wavelength, void size and volume density for a Type A fused silica heat shield at room temperature. Void size, by the way, is a function of particle size - the voids are basically the interparticle interstices. This figure shows that for larger void radii you get increased reflectance. And, for a given void radius, you get higher reflectance by increasing the volume of void phase, which is, essentially, decreasing the density of the material by increasing the number of voids. Also, the increase in reflectance by increasing the number of voids is less for the larger voids than for the smaller voids.

Figure 6-23 - what happens at 1500°C? Well, as you can see, the larger void radii have a decreased reflectance in the ultraviolet region of the spectrum - more of a decrease than the smaller void radii. This is due to increased absorption and the changed scattering cross sections due to increased absorption at this high temperature.

Effect of Void Size and Volume Density on Hemispherical Reflectance of High-Purity Fused Silica at 20°C as a Function of Wavelength

MARTIN MARIETTA
DENVER DIVISION

(Predicted by MSAP Computer Program)

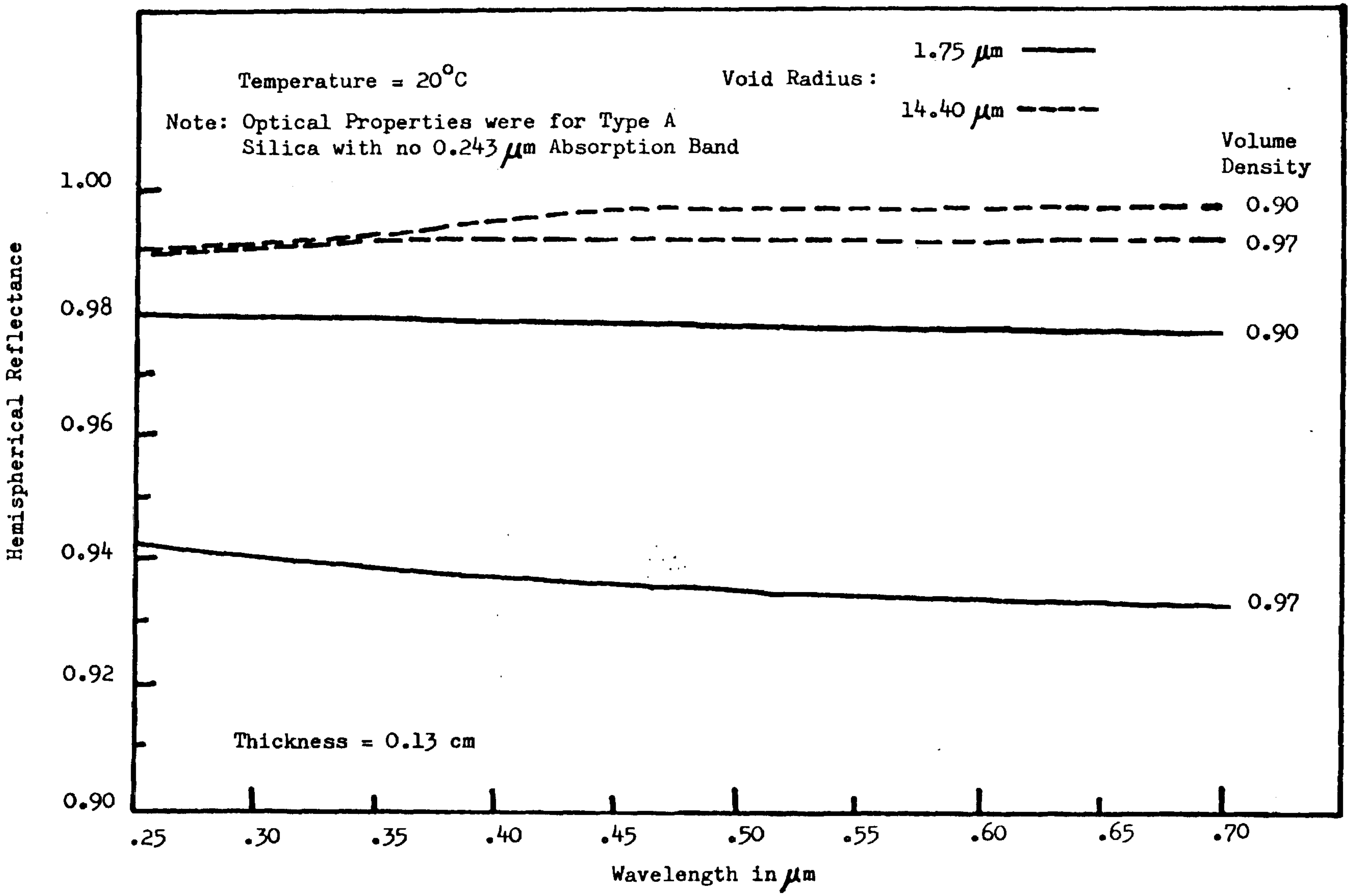


Figure 6-22

Effect of Void Size and Volume Density on Hemispherical Reflectance of High-Purity Fused Silica at 1500°C as a Function of Wavelength

MARTIN MARIETTA
DENVER DIVISION

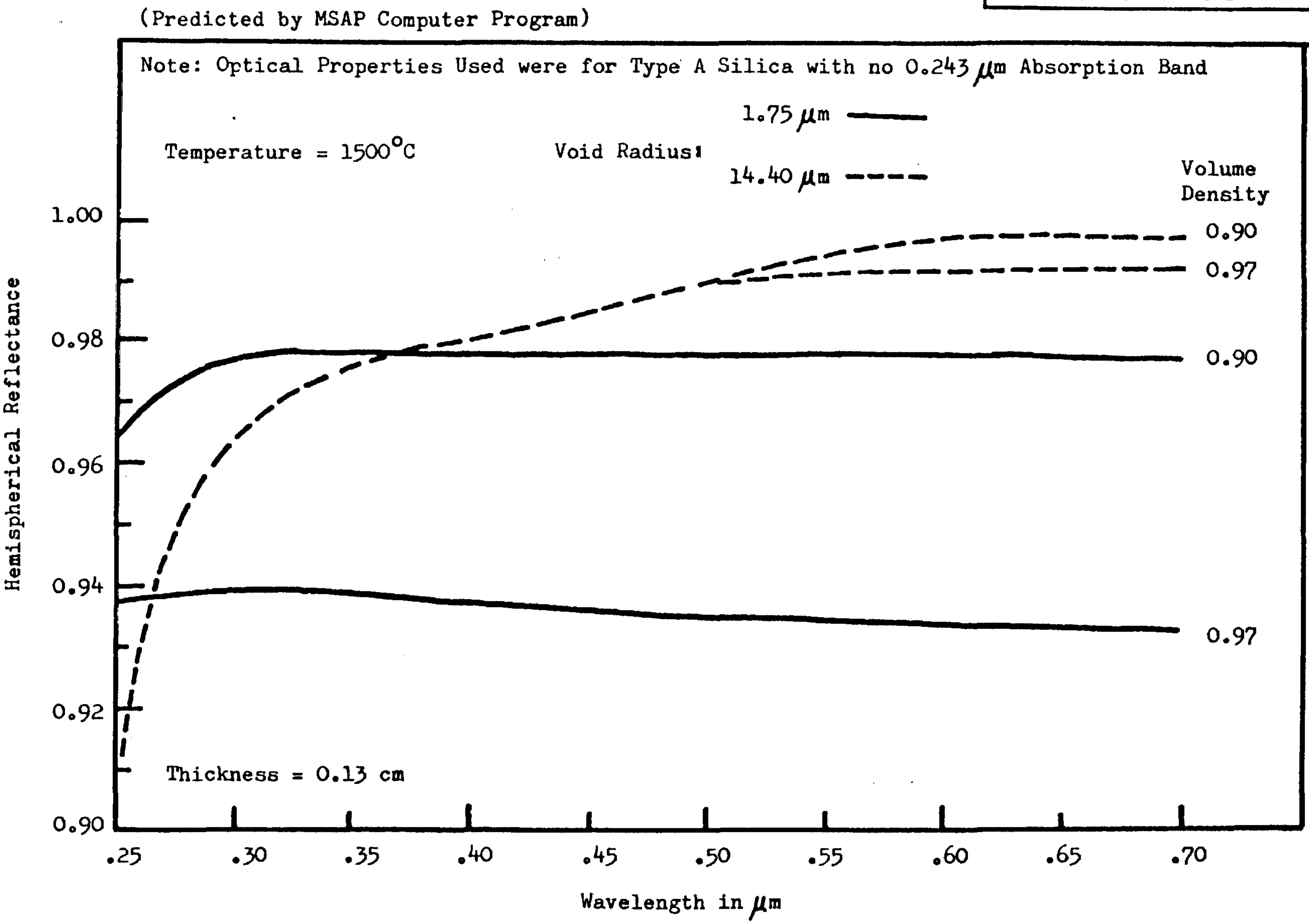


Figure 6-23

This phenomenon is significant because the surface of a silica reflecting heat shield will, of course, achieve this temperature, 1500°C, very rapidly, and the larger reflectance of the smaller voids could prevent or delay the occurrence of bulk vitrification just that much more by decreasing absorption.

Just briefly, we used MSAP to calculate total hemispherical reflectance relative to the predicted Saturn entry shock layer radiation spectrum that I showed you in Figure 6-20. For this spectrum we calculated, as shown in Figure 6-24, reflectance vs void size and volume density at 1500°C. As you can see, for a 70% dense material, optimum reflectance is achieved by a void radius, essentially, in the 2 to 3 μm region. For higher density configuration, optimum reflectance requires larger voids. Again, I mention that the important thing of the MSAP results is the trend rather than the absolute values listed on the axes. We would hope, but really we don't expect, to build a heat shield with a 98 to 99% total reflectance.

So what we have done on our development program is mill our high-purity silica material and then classify it into different and discrete particle size distributions. Then we made test samples from the different particle sizes and studied spectral reflectance vs particle size. The fabrication method that was used was slip casting. Incidentally, we used a high-purity Type B fused silica for this effort because a large amount of material was required and the expense of using Type A was prohibitive. Figure 6-25 shows the size distributions of the particles we used. The Y axis in the slide shows weight percent smaller than a particular particle size, which is given on the X-axis. The usual particle size distribution used in slip casting is the continuous one shown in this figure - approximately 100% of the material is smaller than, say, 60μm, while about 20% is smaller than 2μm. The three monodisperse particle sizes that we studied were 20 to 40μm, 10 to 21μm, and 5 to 11μm. The particle sizes are referred to as I, II, and III.

Total Hemispherical Reflectance of High-Purity Fused Silica at 1500°C Relative to Predicted Saturn Entry Shock Layer Radiation.

MARTIN MARIETTA
DENVER DIVISION

Note: Optical Properties Used were for Type A Silica with no 0.243 μm Absorption Band (Predicted by MSAP Computer Program)

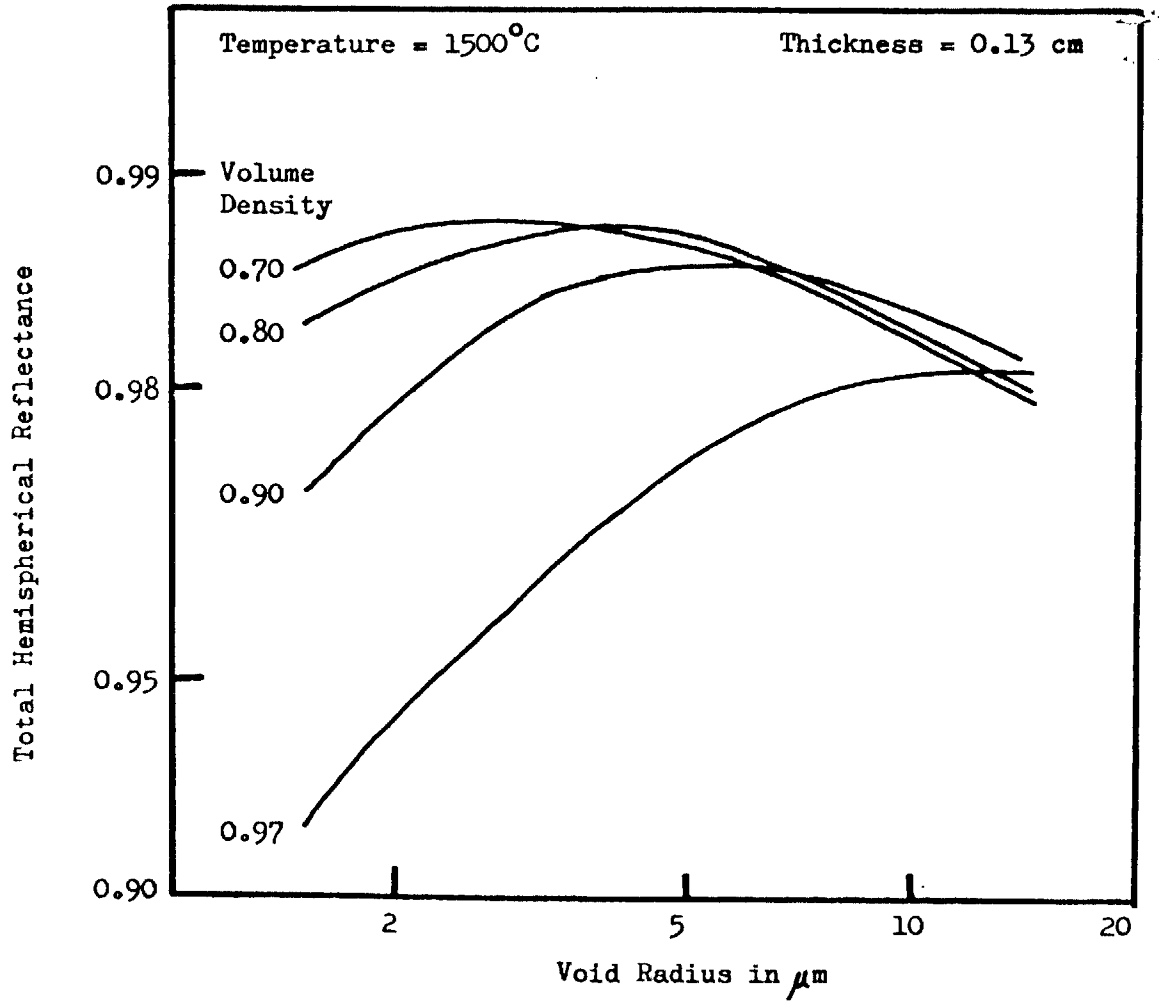


Figure 6-24

Particle Size Distributions of Silica Powders
Used in Silica Reflecting Heat Shield Development

MARTIN MARIETTA
DENVER DIVISION

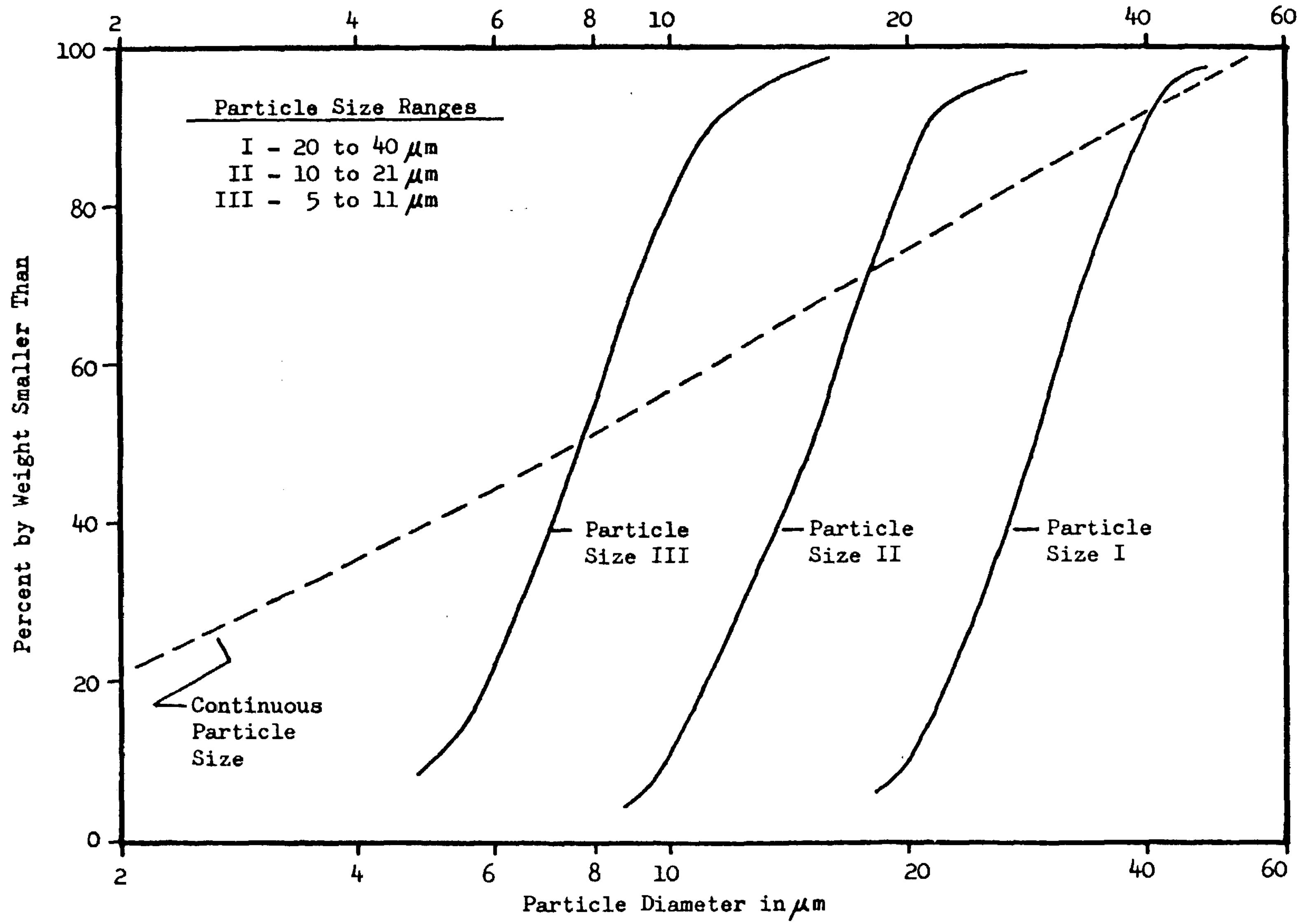


Figure 6-25

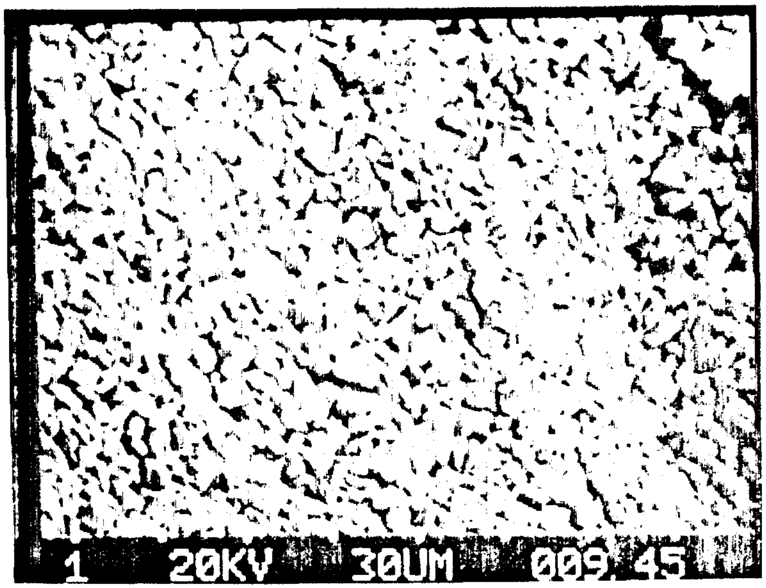
On the left side of Figure 6-26 is a SEM photograph of a slip cast configuration made from particle size III, the 5 to 11μm diameter particles. The scale on the photo shows that the distance between hash marks is 30μm. The surface is a uniform scattering matrix - a uniform and narrow distribution of particles and voids. The SEM photo on the right of Figure 6-26 is of a configuration made from the continuous particle size distribution. The surface is irregular and the distribution of particle and void sizes is wide. One would predict that, because of its uniformity, the particle size III configuration would have a higher reflectance than the continuous particle size configuration. Testing has proven this prediction to be true and I will discuss this later on.

Figure 6-27 contains SEM photographs of slipcast configurations made from particle sizes I and III to provide a comparison between the two. On the left are the 20 to 40μm diameter particles and on the right are the 5 to 11μm particles. Incidentally, as you can see, it is difficult to ascertain the quantitative relationship between void size and particle size - one can only consider qualitatively, that the larger the particles the larger the voids. Also, these samples deliberately have been slightly underfired to make the particles easier to see and distinguish in these particular photographs.

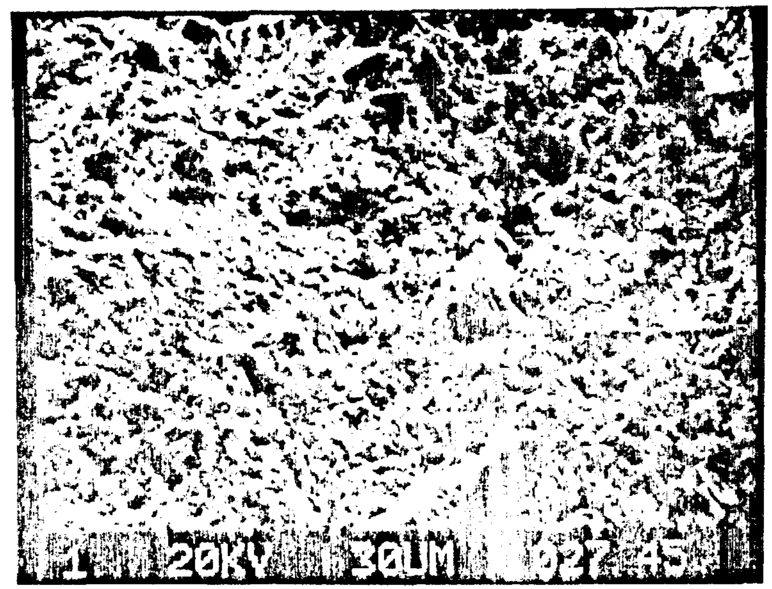
Now, as I mentioned a few minutes ago, we ran tests of reflectance and transmittance on slip-cast configurations made from the three monodisperse particle sizes and the continuous particle size and the tests did show differences between them. Figure 6-28 shows hemispherical reflectance vs wavelength obtained using our Beckman spectrophotometer with an integrating sphere attachment. The figure shows that each of the monodisperse particle sizes, sizes I, II, and III, have higher reflectances than the continuous particle size configuration in the important spectral region, that is, in the visible and near ultraviolet at wavelengths shorter than about 0.7μm. Also, the

Comparison of Fused-Silica Configurations
Made of Particle Size III and a Continuous Particle Size

MARTIN MARIETTA
DENVER DIVISION



Particle Size III (5 to 11 μm)
500X Magnification

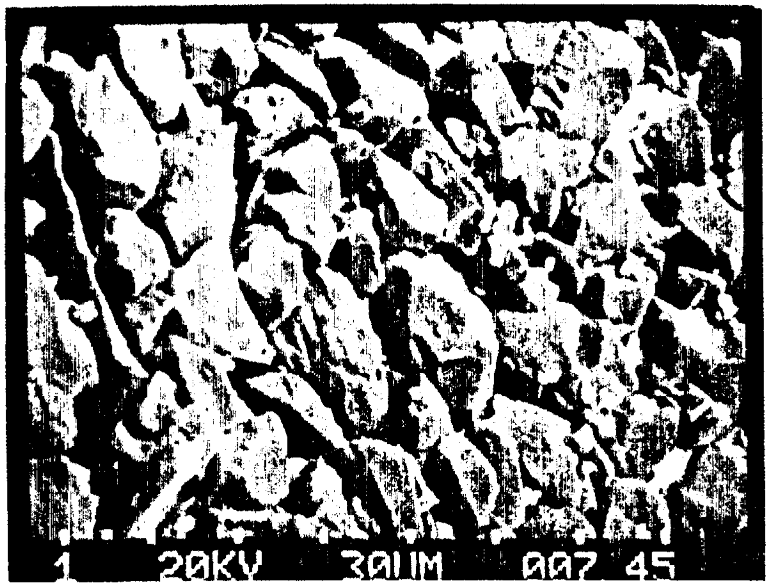


Continuous Particle Size (100 to 0.01 μm)
500X Magnification

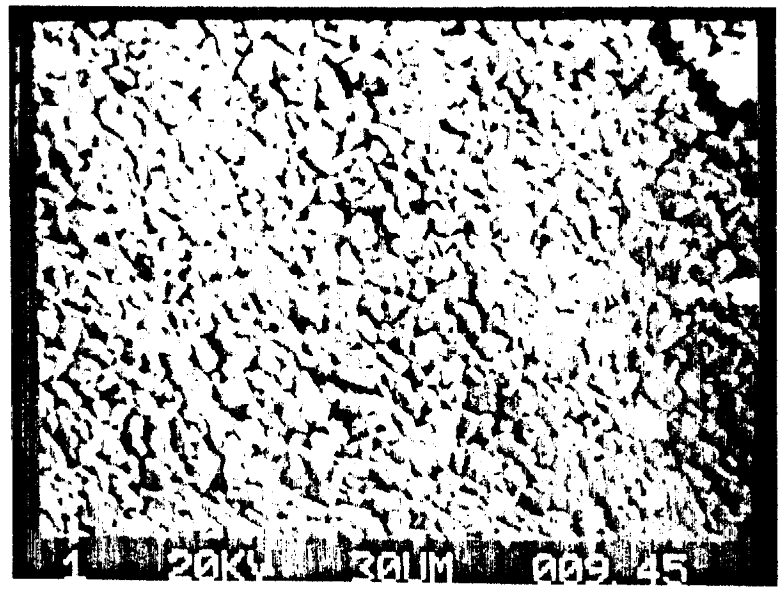
Figure 6-26

Comparison of Fused-Silica Configurations
Made of Particle Size I and Particle Size III

MARTIN MARIETTA
DENVER DIVISION



Particle Size I (20 to 40 μm)
500X Magnification



Particle Size III (5 to 11 μm)
500X Magnification

Figure 6-27

Hemispherical Spectral Reflectance for 0.20" Thick Fused-Silica Discs Made from Different Particle Sizes

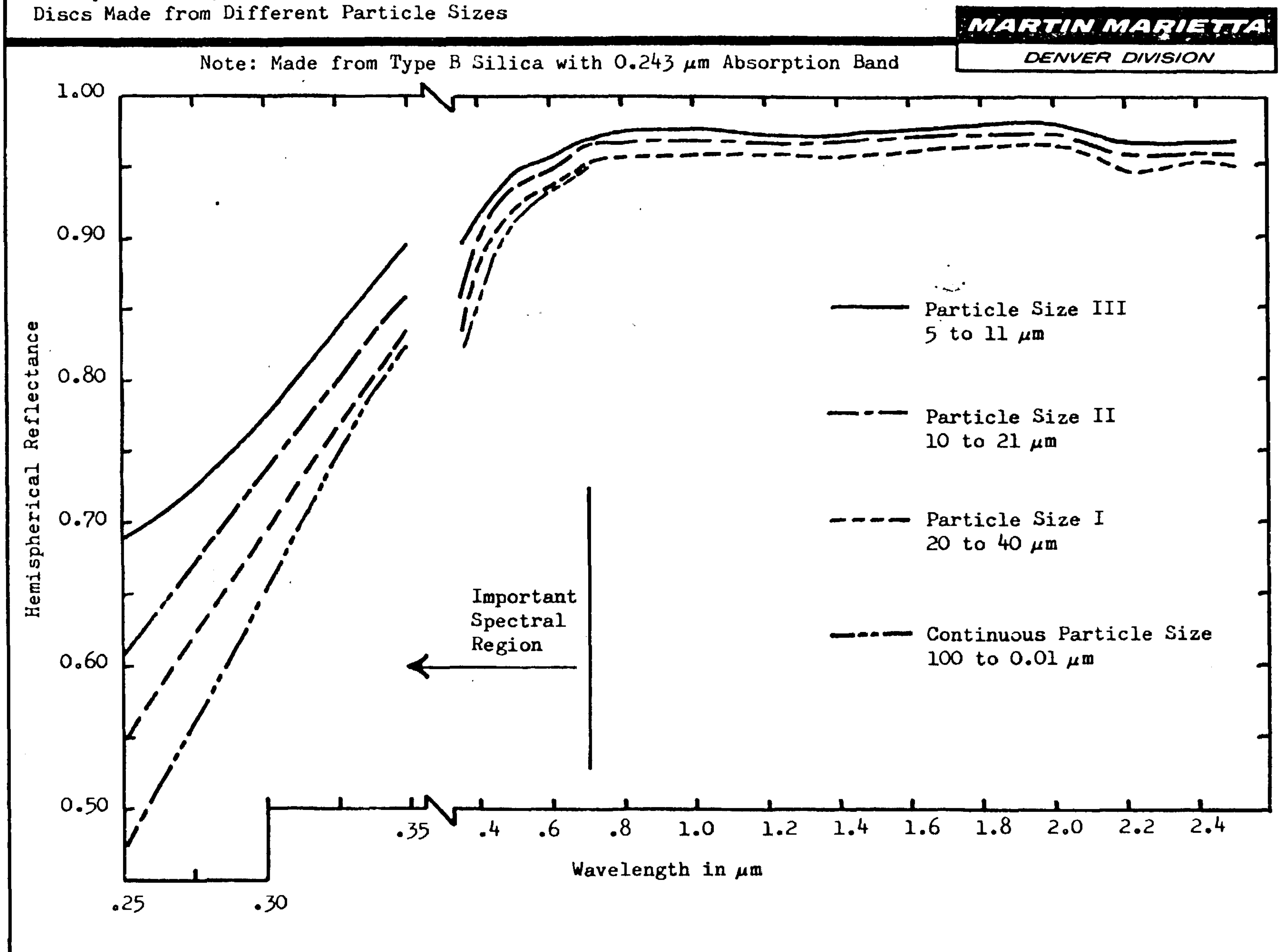


Figure 6-28

smaller monodisperse particle sizes have higher reflectance than the larger ones. At first glance, it might seem that these test results are not in complete agreement with the MSAP predictions. However, the test configurations have higher absorption than the theoretical configuration used in the MSAP analyses - higher absorption because they were made from Type B fused silica rather than Type A, and because inevitable contamination is introduced during milling, classifying, and processing. Thus, the slide of spectral reflectance at 1500°C - the MSAP predictions for the case of increased absorption - which shows higher reflectances for smaller voids and particles is consistent with the test results.

Monodisperse particle sizes and, especially, smaller monodisperse particle sizes produce higher reflectances. This is important because, even for the highest-purity synthetic fused silica - a material that has a total metal contamination well below 10 ppm - and assuming no introduction of impurities during processing, reflectance decreases at higher temperatures and a tailored morphology can lessen this decrease and inhibit the occurrence of bulk vitrification.

Figure 6-29 sums up some of the things we have discussed here: the best material to use in a silica reflecting heat shield is Type A, which is capable of ultra-high-purity and which does not show the 0.243μm absorption band; the reflection efficiency of fused silica is decreased at higher temperatures due to the bathochromic shift of the ultraviolet cut-off; for a given silica material, over the wavelength region and particle sizes that we have tested, the monodisperse particle size configurations; and the smaller monodisperse particle size configurations give higher reflectance than the larger ones. By tailoring the matrix for optimum scattering and using an ultra-high-purity material, we should be able to achieve a reflecting silica configuration that is truly an efficient reflector of shock layer radiation even at high ablation temperatures.

I. To reflect radiation in the visible and ultraviolet spectral regions, synthetic fused silica, Type A, is the best silica to use for a reflecting heat shield.

II. The reflection efficiency of all silica materials is decreased at higher temperatures due to increased absorption.

III. For a given material, over the particle size ranges and spectral regions which we studied:

A. monodisperse particle size configurations produced higher reflectances than continuous particle size configurations;

B. the smaller monodisperse particle sizes produced higher reflectances than the larger monodisperse particle sizes.

IV. By using Type A silica and optimizing the scattering characteristics of the matrix (i.e., particle size, void size, volume density), it should be possible to build an efficient high-temperature radiation reflector for outer-planet missions.

Figure 6-29

UNIDENTIFIED SPEAKER: No matter how pure you get silica, you are limited. Wouldn't you do better doing the same kinds of studies with magnesia?

MR. CONGDON: With magnesia? Well, there are certainly a large number of materials that are good room-temperature reflectors of low intensity radiation. Yes, magnesia does have a high reflectance down into the ultra-violet region of the spectrum, but at higher temperatures I think you will find that the reflectance of magnesia falls off more significantly than that for silica. There are many materials that you could look at: alumina has a very good reflectance. But alumina has severe thermal stress problems, so does magnesia. That's the problem with quite a few good reflectors that would otherwise be heat shield candidates. We are putting, essentially, all of our effort into fused silica at this time because it has high reflectance, has a large heat of sublimation, and has very low thermal expansion - very good resistance to thermal shock. So we are looking for two things, actually, one is a high reflectance, and the other is a good response to convective heating; that is, a high sublimation energy. Silica has both of those; magnesia doesn't have as high a sublimation energy and that is one reason we are not as interested in it.

MR. SEIFF: Bill, you may have mentioned this and it slipped by me, but the thickness of those specimens clearly affects the amount of reflection that you get from them.

MR. CONGDON: It doesn't necessarily - you're talking about very small changes. Because of its large refractive index mismatch, about 1.5, slip-cast fused silica is an intense scatterer of radiation. Reflection actually takes place within a very short distance beneath the surface of the material. That is to say, very thin samples are optically very thick for shock layer radiation, which is mostly visible and ultraviolet. You rapidly reach a point of diminishing returns in terms of improving reflectance by going to greater thicknesses. We have tested models with thicknesses from fifty-thousandths inch to one-half inch and found that for wavelengths smaller than about 0.7μm, there is no detectable increase in reflectance for thicknesses greater than about one-tenth inch. For a material like Teflon, of course, there is a strong sensitivity of reflectance to thickness. Incidentally, the spectrophotometer data shown in Figure 6-28 was for two-tenths inch thick models for fused silica.

MR. SEIFF: Well, that which is not reflected, then, ultimately, you will have to account for all of the energy requirements. So, what happens in the case of thicker specimens? If the same fraction is reflected, is the remainder of that absorbed?

MR. CONGDON: Yes. Because a one-tenth inch thick sample is optically very thick to visible and ultraviolet radiation, it has essentially no transmittance. Therefore, what is not reflected is absorbed. And absorptance and reflectance remain essentially constant for greater thicknesses. At wavelengths outside the region of the bulk of predicted shock layer radiation - wavelengths longer than about 0.7μm, infrared radiation - there is some noticeable sensitivity of reflectance, transmittance, and absorptance to thickness. Because shock layer radiation will have a small infrared tail, there may be some very slight transmittance of this radiation, depending on the heat shield thickness.

MR. SEIFF: The application, that is the end goal of this thing, is you don't want that radiation leaking through onto the lower structures. What thickness must be provided in order to accomplish that?

MR. CONGDON: A silica heat shield is sized by other considerations, primarily surface recession. Current computer analyses indicate that a thickness of an inch or more will be required for outer planet entry, varying between Jupiter and Saturn/Uranus. There should be very little transmittance of radiation for such a thick heat shield. Exactly how much hasn't been determined at this time. You're talking about numbers that are a very small fraction of a percent. To detect this with the correct spectral distribution and the correct thickness, you need very intense incident radiation - a facility that doesn't exist. In our xenon-arc lamp tests, where the spectrum contains large infrared components, we have measured transmittance of roughly one-half percent for high density slip cast silica models of three-tenths inch thickness. It should be possible to take into consideration the spectral distribution differences between predicted outer planet entry radiation and xenon lamp radiation and devise a test. Probably the best way would be to correlate the test data, construct an analytical model of radiation transfer for slip cast silica and run computer analyses. We have done this sort of thing for Teflon but not for silica.

MR. VOJVODICH: Bill, from a designer's standpoint, we're interested in what the payoff is in obtaining better performance. Is there a one-to-one correspondence between increased reflectance, decreased transmittance, and the heat shield weight, or - what I guess I am asking is what are the parametrics associated with change in performance in terms of what the impact on the heat shield is?

MR. CONGDON: This is the sort of thing that has to be determined by computer analysis. Our present effort is directed entirely to materials development. A detailed parametric computer sizing study needs to be performed and we have developed the analytical tools to do this, but it is not a part of our present effort. I believe that John Howe has done some work in this area and he may be including it in his talk.

DR. NACHTSHEIM: Thank you, Bill. Our next speaker is John Howe who will discuss some of the advantages of this type of heat protection system, based upon analytical calculations.

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PERFORMANCE OF REFLECTING SILICA HEAT SHIELDS DURING ENTRY INTO SATURN AND URANUS

John Howe
NASA Ames Research Center

MR. HOWE: I just want to take a moment to orient some in the audience who may not be familiar with the reflective heat shield concept.

The idea is that you take a material that doesn't absorb radiation, like a window (Figure 6-30) - the radiation will go through it - and you pulverize it and you then use that as your heat shield; that is, you put it back together somehow. But now it's in finely divided particles, and voids, as Bill Congdon just talked about. It still is not absorbing radiation significantly, but it is also not transmitting it. It's back scattering it, reflecting it. This is the whole idea.

We've tried to analyze the performance of silica heat shields in the outer planet environments. Very briefly, I want to show you what's in this analysis (Figure 6-31). This is a picture of the front end of an entry probe, and one has incident radiative flux and convective flux and the surface is ablating. One can divide the radiation into an inward intensity and an outward, backscattered intensity, and one can have a mirrored surface on the back if he wants. For boundary conditions, we insulate the back to see that no heat gets through. This system is described in a set of differential equations: an energy equation, that is the usual heat conduction equation - unsteady - with terms having to do with absorption - where K is the absorption coefficient - the absorption of the outward intensity and the inward intensity; and the emission of radiation because the material gets hot.

These intensities are obtained for each spectral band, "m" for a pair of equations: one having to do with the outward intensity and the other having to do with the inward intensity. All of the properties are temperature dependent; that is thermal conductivity, density, specific heat, absorption coefficient, scat-

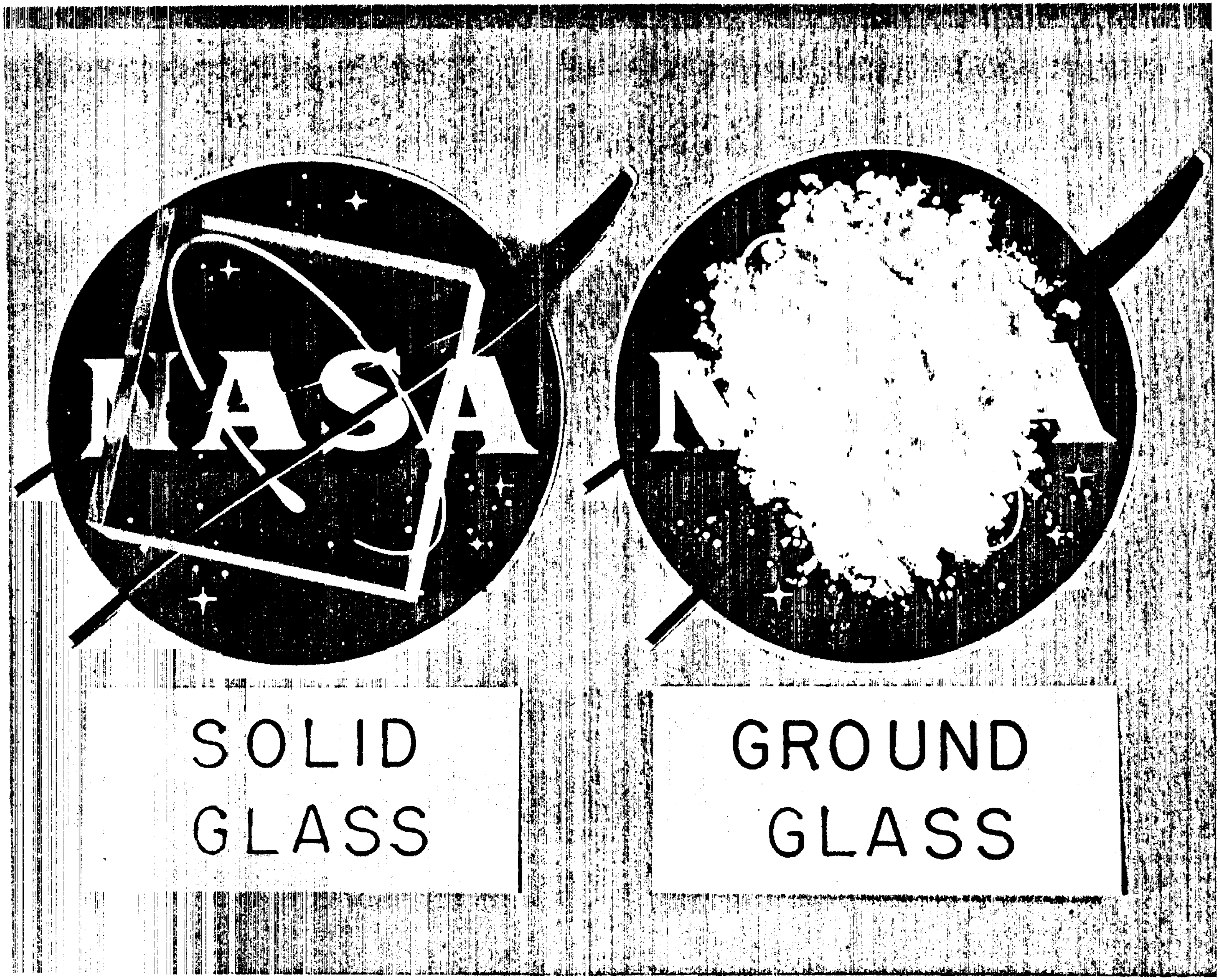
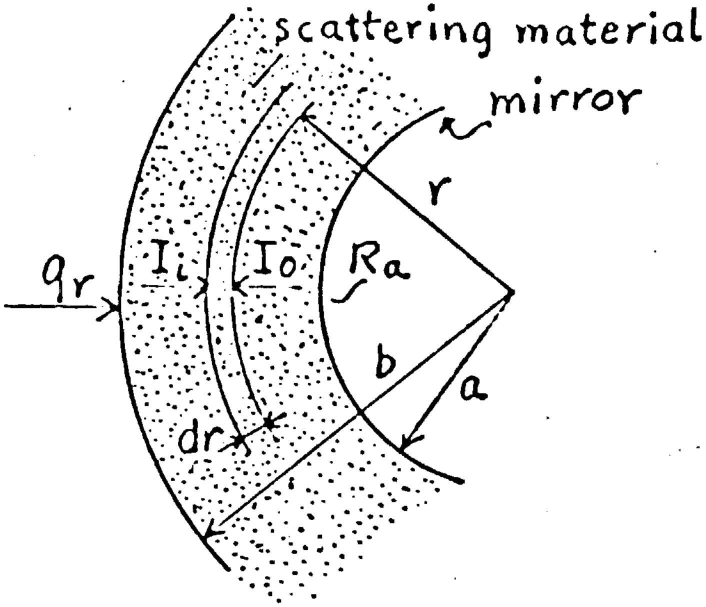


Figure 6-30



I = bidirectional intensity

K = bidirectional absorbtion coefficient (cm^{-1})

S = bidirectional scattering coefficient (cm^{-1})

differential equations:

$$\left.\begin{aligned}\frac{1}{r^2}\frac{d}{dr}\left(r^2 I_o\right) &= -(S+K)I_o + SI_i + n^2 K I_B \\ \frac{1}{r^2}\frac{d}{dr}\left(r^2 I_i\right) &= (S+K)I_i + SI_o + n^2 K I_B\end{aligned}\right\} \text{2 equations for each frequency band "m"}$$

$$\rho c_p \frac{\partial T}{\partial t} = -\frac{1}{r^2}\left[\frac{\partial}{\partial r}\left(-r^2 k \frac{\partial T}{\partial r}\right) - r^2 \sum_{m=1}^{M} K_m \pi \left(I_{o_m} + I_{i_m}\right) + 2r^2 \pi \sum_{1}^{M} r_m^2 K_m I_{B_m}\right]$$

FIGURE 6-31

tering coefficient. The optical properties are not only temperature dependent but they are wavelength dependent. So one has to solve this mess. It's non-linear, and it's coupled,and it's transient; and we've got a scheme for solving that.

I want to show you the results for that. First of all, let me just tell you what some of the properties are that have to go into this. The absorption coefficient is very important, and we want to know what it is as a function of wavelength and temperature. Figure 6-32 shows what we have been able to get out of the literature for a material called Ultrasil. There is data in the catalogs at room temperature. There is some data in a narrow temperature band region near 1600°K by a man named Rupp for certain wavelengths; and then Spivak, in the infrared end of the spectrum, has some data that goes out fairly uniformly over a broad temperature range. But, clearly, we need more data in the intermediate range, and we need some higher temperature data on absorption coefficient. You can see that the absorption varies wildly with temperature - orders of magnitude - and this is built into our code.

The scattering coefficient shown in Figure 6-33 tells you how much radiation is reflected. With high scattering, there is high reflection. The bottom curve is the scattering coefficient for a fibrous astroquartz laminate - fibre size of five or six microns - and it's really not a very high scattering coefficient - something like 40 reciprocal centimeters - at the most. The upper curve is a slipcast silica, Glasrock. It's a commercially available fused silica and it's not particularly good, either, but we are going to use both of these and show the effects.

Theoretically, I think that one can come up with a scattering coefficient that's about twice as good as this Glasrock, depending on the void size, and so forth, as Bill Congdon just talked about. You can see that the Glasrock is far better than the fibrous material.

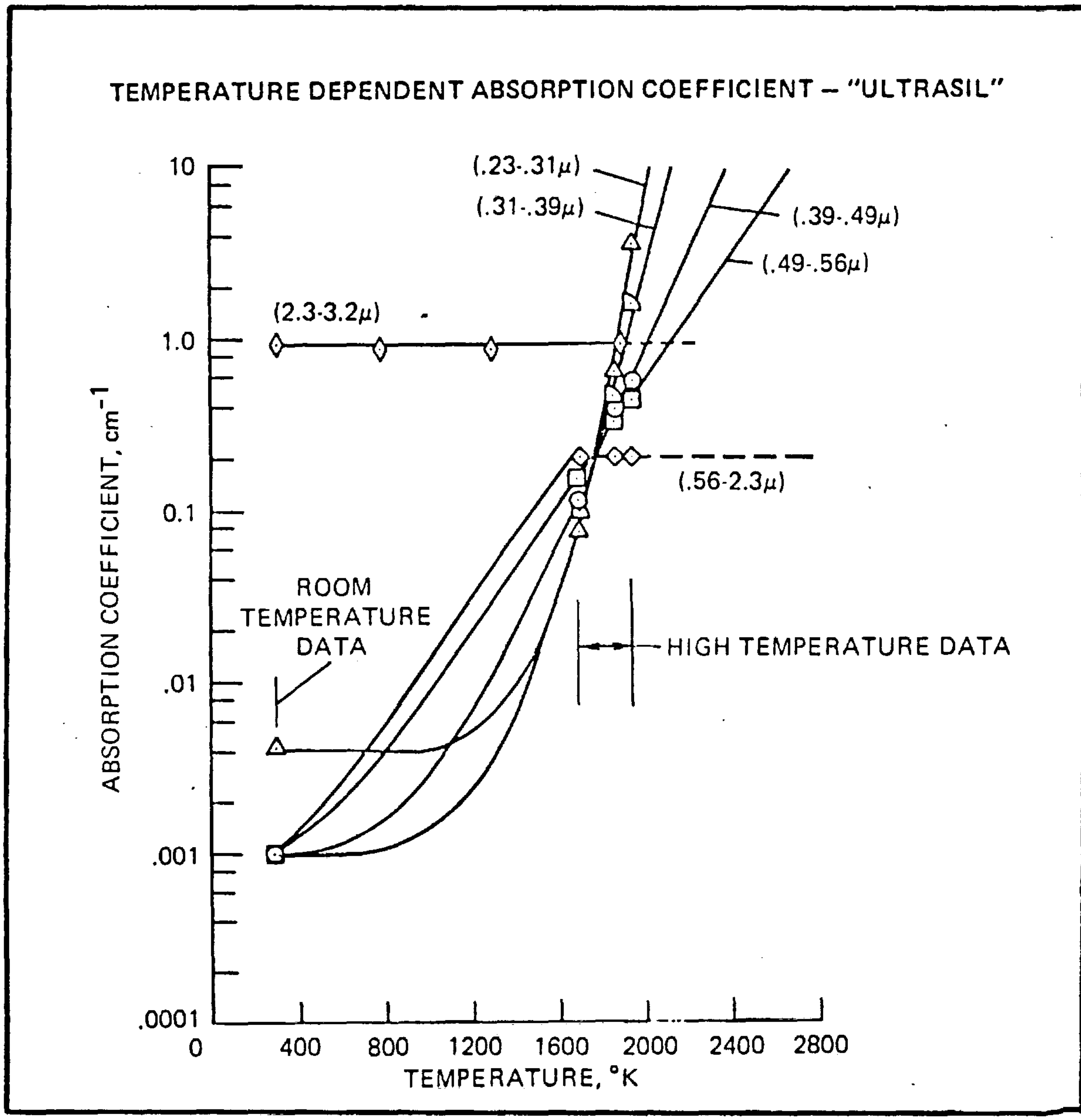


FIGURE 6-32

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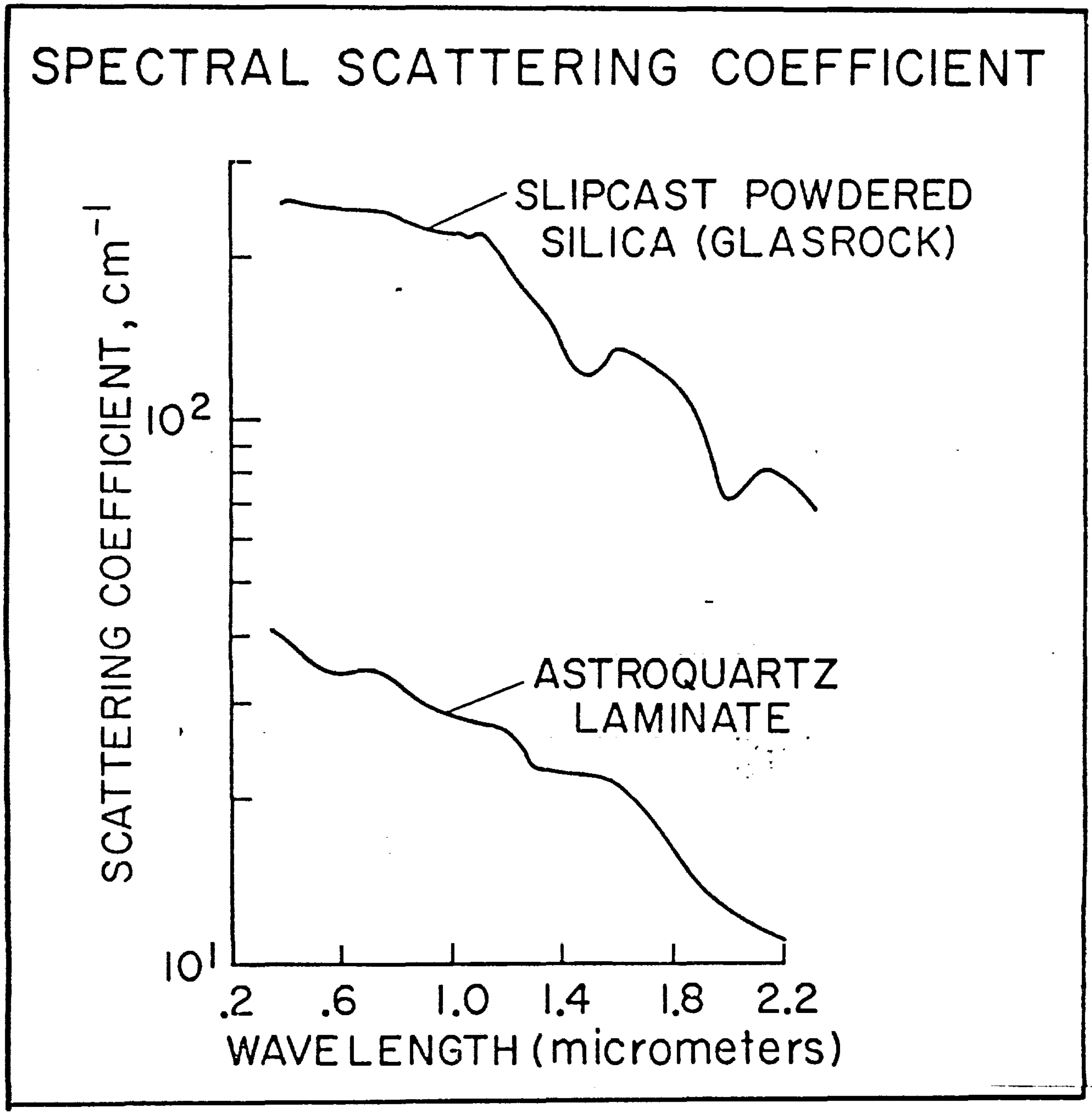
SPECTRAL SCATTERING COEFFICIENT
SLIPCAST POWDERED SILICA (GLASROCK)
ASTROQUARTZ LAMINATE
SCATTERING COEFFICIENT, cm⁻¹
10^2
10^1
.2 .6 1.0 1.4 1.8 2.2
WAVELENGTH (micrometers)

FIGURE 6-33

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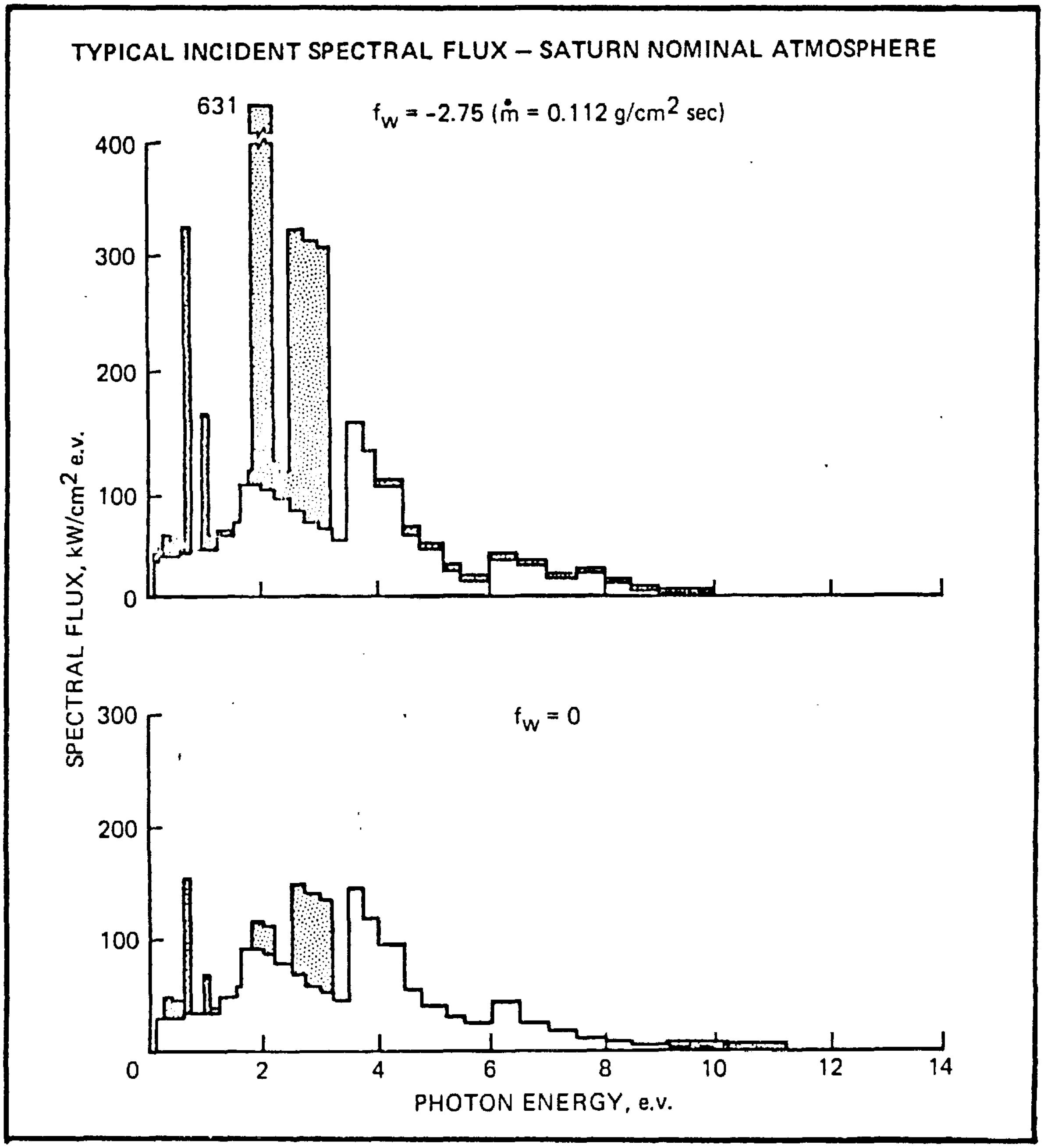

TYPICAL INCIDENT SPECTRAL FLUX – SATURN NOMINAL ATMOSPHERE
631
f_w = -2.75 ($\dot{m}$ = 0.112 g/cm^2 sec)
400
300
200
100
0
SPECTRAL FLUX, kW/cm^2 e.v.
300
f_w = 0
200
100
0
2
4
6
8
10
12
14
PHOTON ENERGY, e.v.

FIGURE 6-34

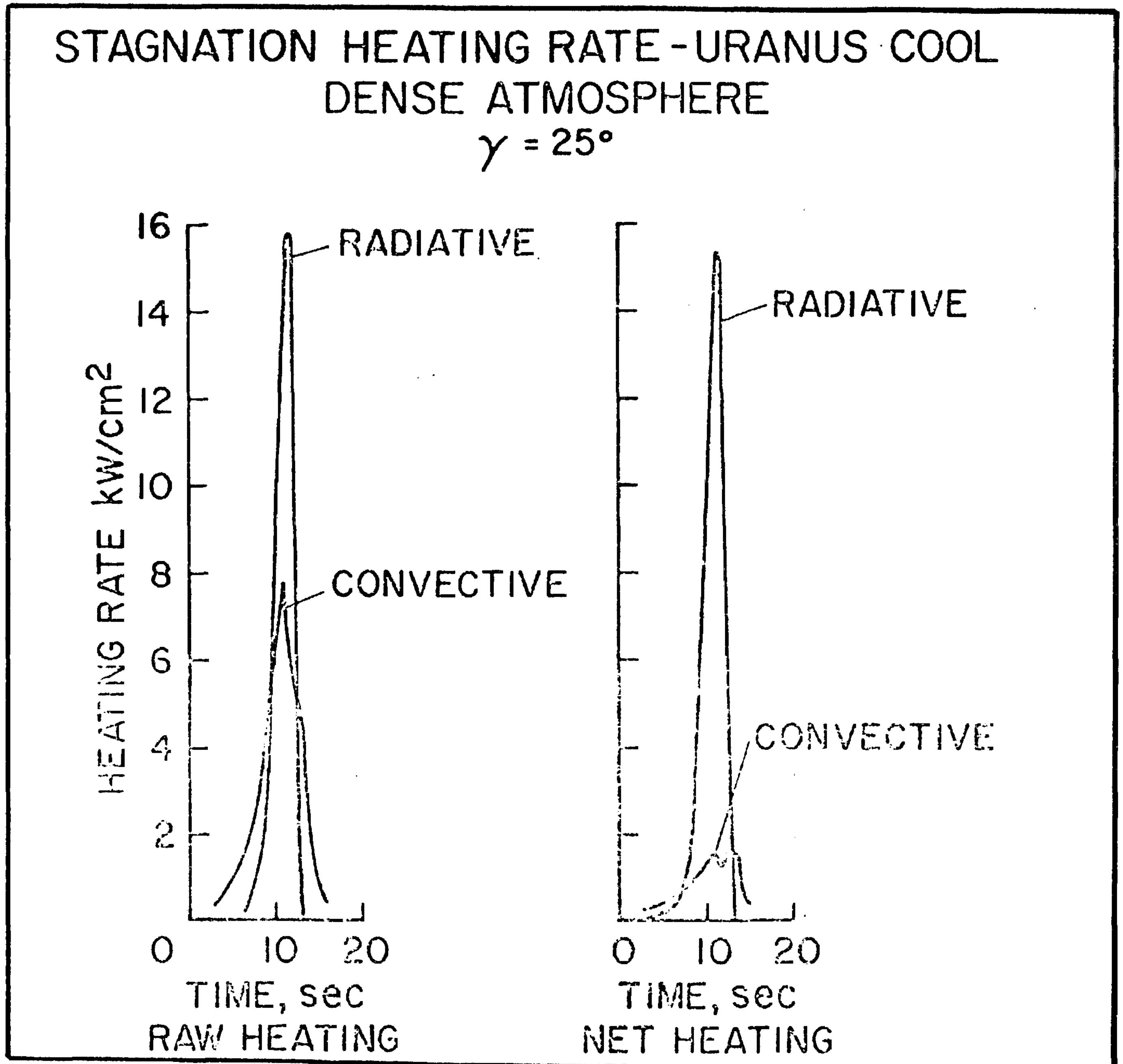
STAGNATION HEATING RATE - URANUS COOL
DENSE ATMOSPHERE
$\gamma = 25°$
HEATING RATE kW/cm^2
16
14
12
10
8
6
4
2
0
10
20
TIME, sec
RAW HEATING
RADIATIVE
CONVECTIVE
0
10
20
TIME, sec
NET HEATING
RADIATIVE
CONVECTIVE

FIGURE 6-35

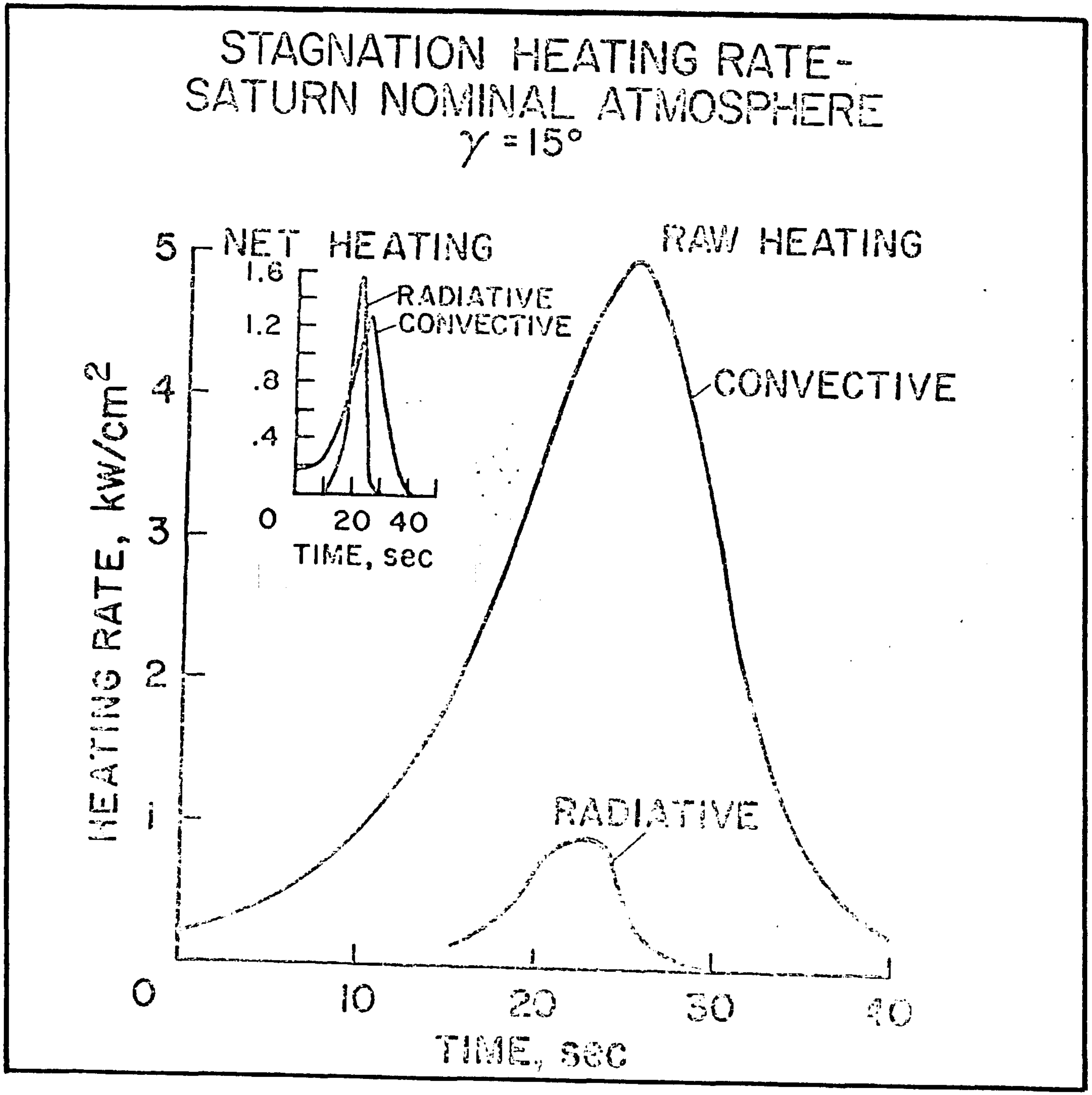
STAGNATION HEATING RATE-
SATURN NOMINAL ATMOSPHERE
γ = 15°
NET HEATING
RAW HEATING
1.6
1.2
.8
.4
0
20 40
TIME, sec
RADIATIVE
CONVECTIVE
CONVECTIVE
RADIATIVE
HEATING RATE, kw/cm²
5
4
3
2
1
0
10
20
30
40
TIME, sec

FIGURE 6-36

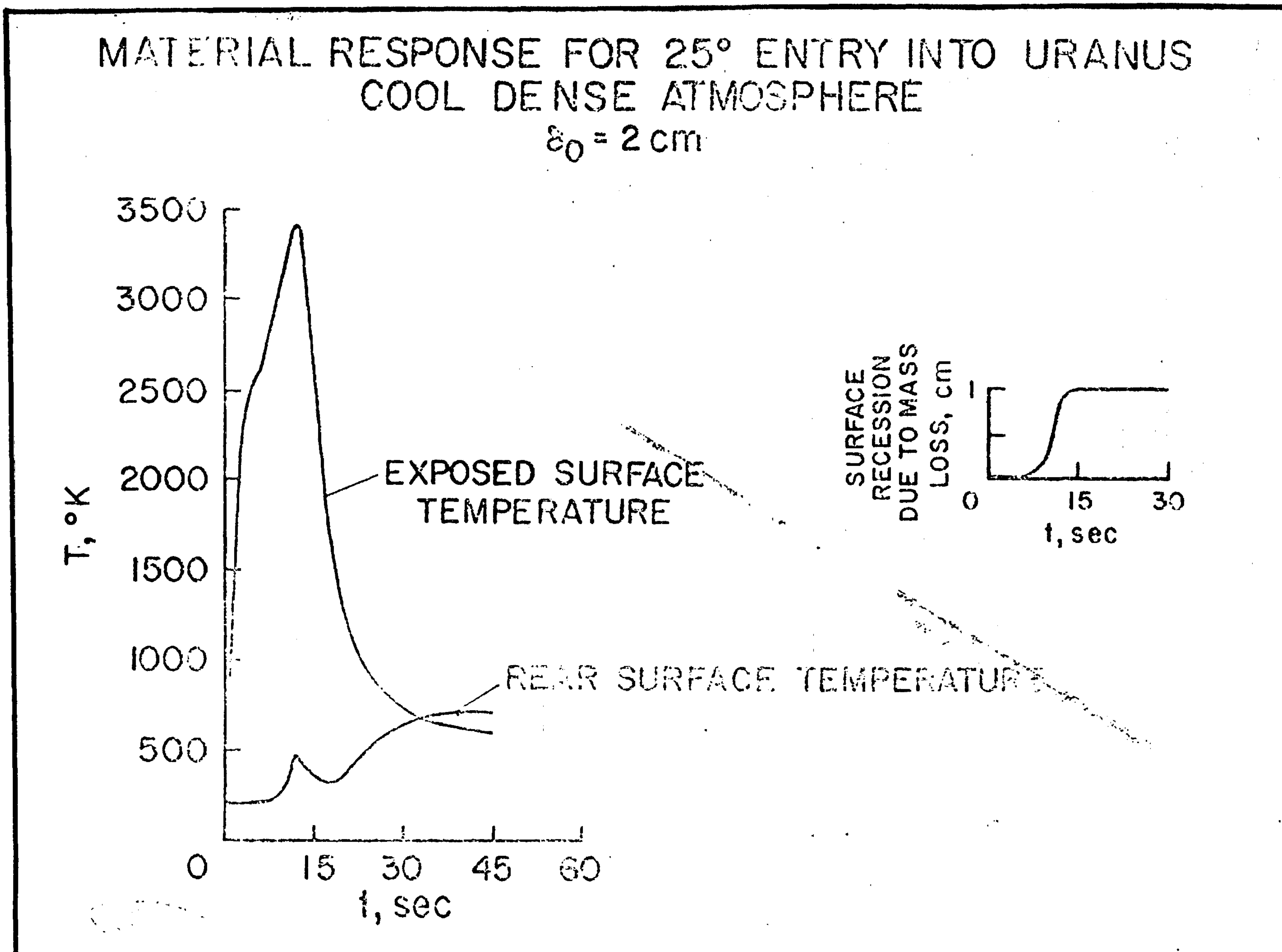
MATERIAL RESPONSE FOR 25° ENTRY INTO URANUS
COOL DENSE ATMOSPHERE
$\delta_0 = 2$ cm
T, °K
3500
3000
2500
2000
1500
1000
500
0
15
30
45
60
t, sec
EXPOSED SURFACE TEMPERATURE
REAR SURFACE TEMPERATURE
SURFACE RECESSION DUE TO MASS LOSS, cm
1
t, sec

FIGURE 6-37

the front surface temperature rises rather sharply because some of the radiation bands are absorbed right on the surface. So the front surface temperature passes through a peak corresponding to both the radiation and the convection pulse and then begins to cool down so that when a subsonic mach number (of 0.7 here) is reached, the front surface is really quite cool. It's being cooled by the atmosphere flowing over it.

The interesting thing is that the rear surface temperature doesn't really see any heat at time zero; but, when that radiation pulse comes on, some of that gets through instantaneously to the back surface. This is a finite thick slab. Not all of the radiation is reflected, some of it is deposited there. So, this shoots the rear surface temperature up to a little peak and, as the light goes out, or the radiation diminishes, that begins to drop down. But, then conduction from the hot front surface finds its way through the material and the back surface temperature begins to rise again. For this particular case we lost about a centimeter of material due to ablation for this 2 centimeter thick shell.

Figure 6-38 shows the corresponding temperature profiles at various points in time. The peak temperature was at about twelve seconds, and it's dropping at sixteen seconds; at twenty-three seconds there is some heat flowing toward the front as well as heat flowing toward the rear; and at forty-six seconds it's a fairly uniform temperature - everything is over.

Figure 6-39 shows a thicker slab going into the same atmosphere, but I want to show you something. The previous two figures use the Glasrock scattering coefficient, and I noted that there was just a small temperature rise at the back surface when the radiation came on. But if we use the Astroquartz scattering coefficient, which is nowhere near as good, the rear surface temperature really shoots up. So, you see how important this is. The rear surface temperature, essentially, designs the heat shield. If you are going to have a low rear surface temperature,

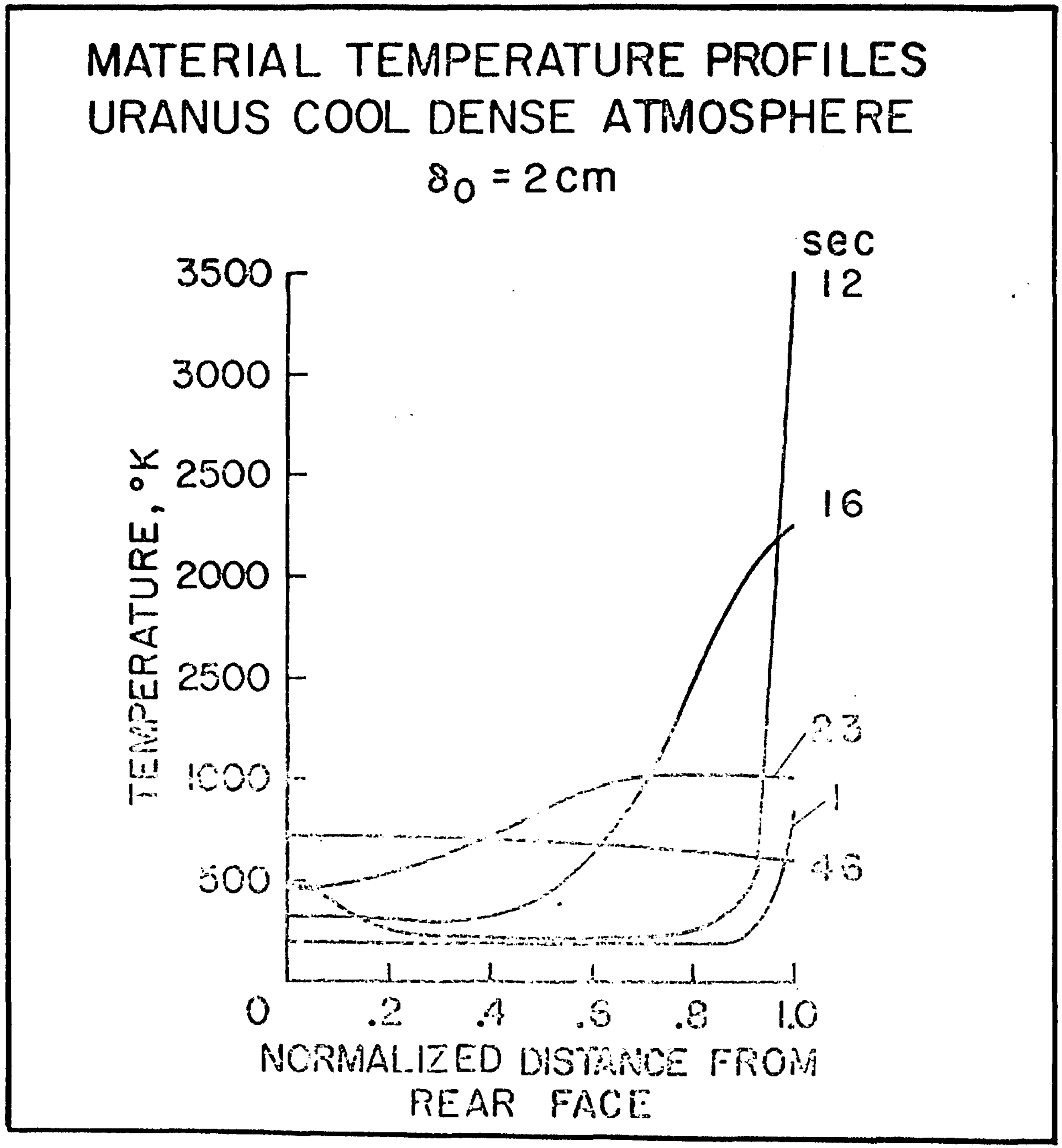
MATERIAL TEMPERATURE PROFILES
URANUS COOL DENSE ATMOSPHERE
$\delta_0 = 2$ cm
sec
12
16
23
1
46
3500
3000
2500
2000
2500
1000
500
TEMPERATURE, °K
0
.2
.4
.6
.8
1.0
NORMALIZED DISTANCE FROM REAR FACE

FIGURE 6-38

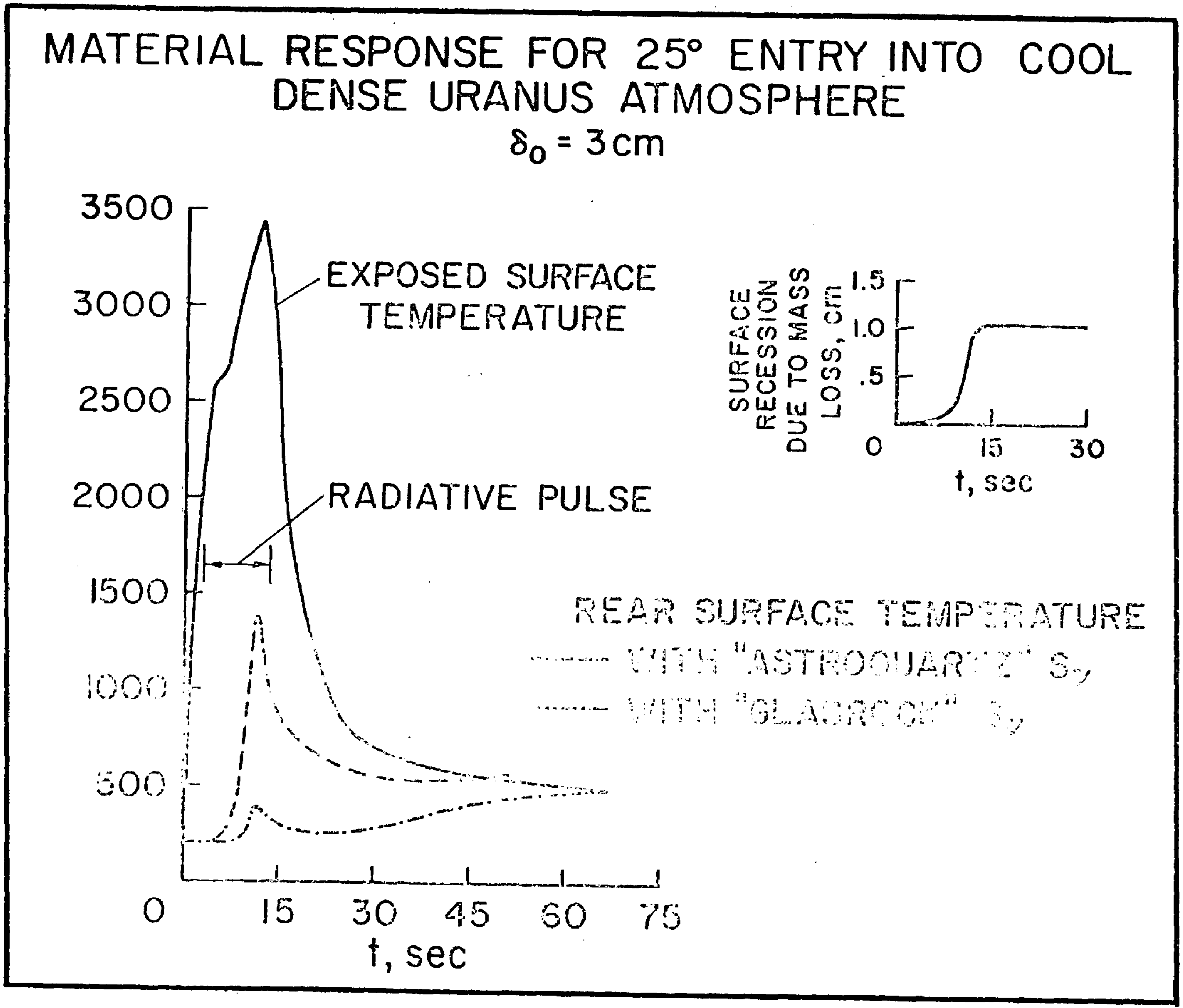

FIGURE 6-39

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you have to have a high scattering coefficient. Going from Astroquartz to Glasrock, effectively knocks that rear surface temperature by a thousand degrees, Kelvin. So, this is really great; you can get by with thinner heat shields with the higher scattering coefficient. If we can, indeed, double the scattering coefficient above Glasrock, we can go thinner yet.

Figure 6-40 shows a result for the nominal Saturn atmosphere at a 15° entry; we saw the environment in Figure 6-36. This is just a one and a half centimeter thick shell; not thick enough it turns out, because the rear surface temperature goes up to almost 1700° or 1800° Kelvin. This is essentially due to the conduction from the front face through. On this one we lost about three quarters or eight tenths of a centimeter of thickness due to ablation.

A Jupiter case is shown in Figure 6-41. It is five centimeters (original thickness) shell, and you can see the case wasn't quite finished. It's been finished since this slide was drawn. The surface temperature goes up quite high - around 3500°K. This is a 20" entry into Jupiter, which is very severe. The radiative heating is something like a hundred kilowatts per square centimeter - up in the extreme range that John Lundell talked about - so this is really a very hard entry. We lose about two and a half centimeters of material. The rear surface temperature doesn't go very high, something around 400 or 500 degrees Kelvin. So this is thicker than we need.

We have made quite a number of runs for these three planets, one entry angle for each planet. We haven't really run any extensive parametric studies as yet, but we have summarized the results of these studies on Figure 6-42.

As I mentioned, the backface temperature essentially designs the heat shield. This Figure is for a heat shield density of 1.49 grams per centimeter cubed which is about the same as the carbon phenolic density that Sam Mezines discussed. The figure is for the Glasrock scattering coefficient.

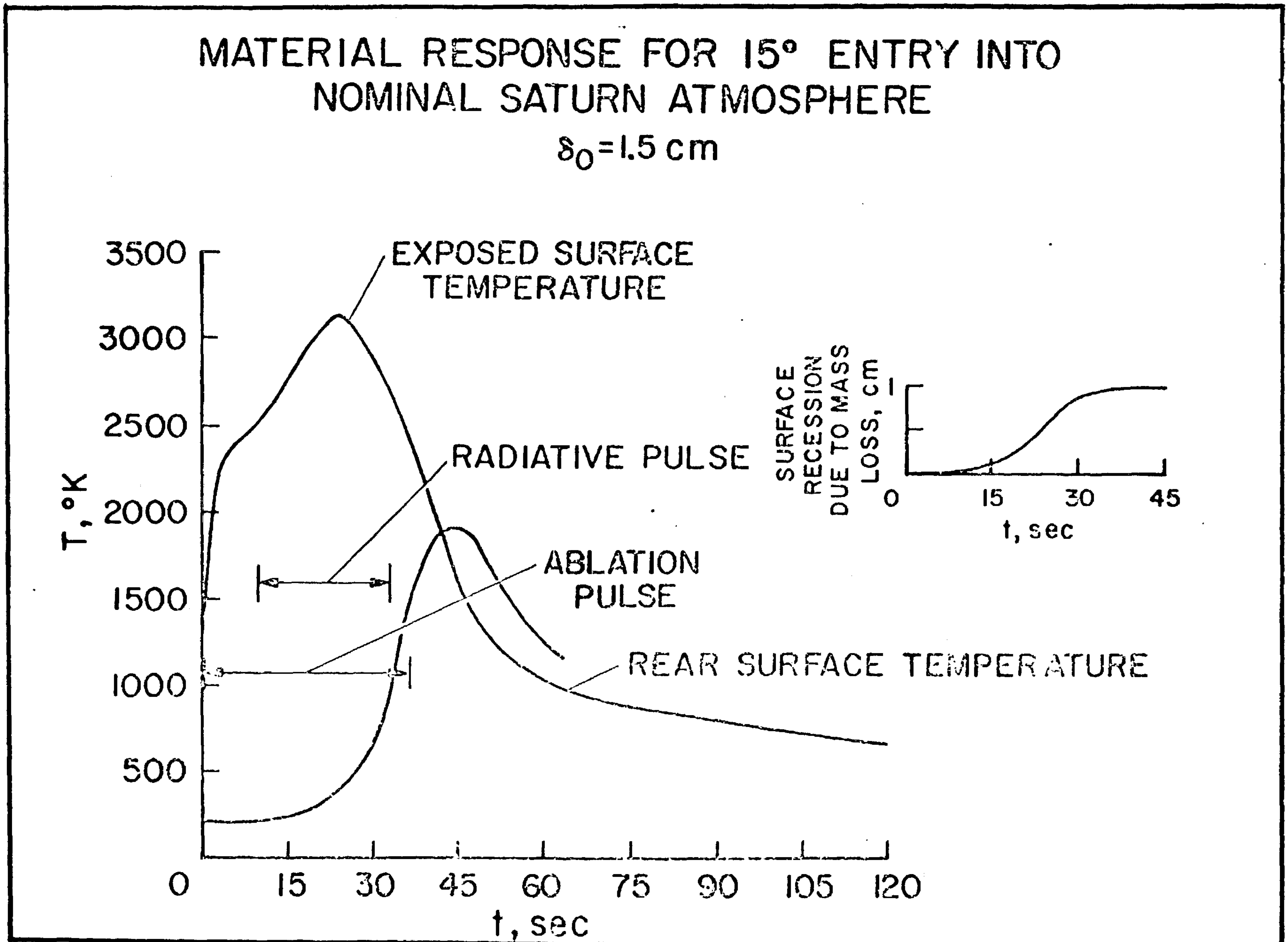

MATERIAL RESPONSE FOR 15° ENTRY INTO
NOMINAL SATURN ATMOSPHERE
δ_0=1.5 cm
EXPOSED SURFACE
TEMPERATURE
RADIATIVE PULSE
ABLATION
PULSE
REAR SURFACE TEMPERATURE
T, °K
3500
3000
2500
2000
1500
1000
500
0
15
30
45
60
75
90
105
120
t, sec
SURFACE
RECESSION
DUE TO MASS
LOSS, cm
1
0
15
30
45
t, sec

FIGURE 6-40

MATERIAL RESPONSE FOR 20° ENTRY INTO JUPITER NOMINAL ATMOSPHERE

$\delta_0 = 5$ cm

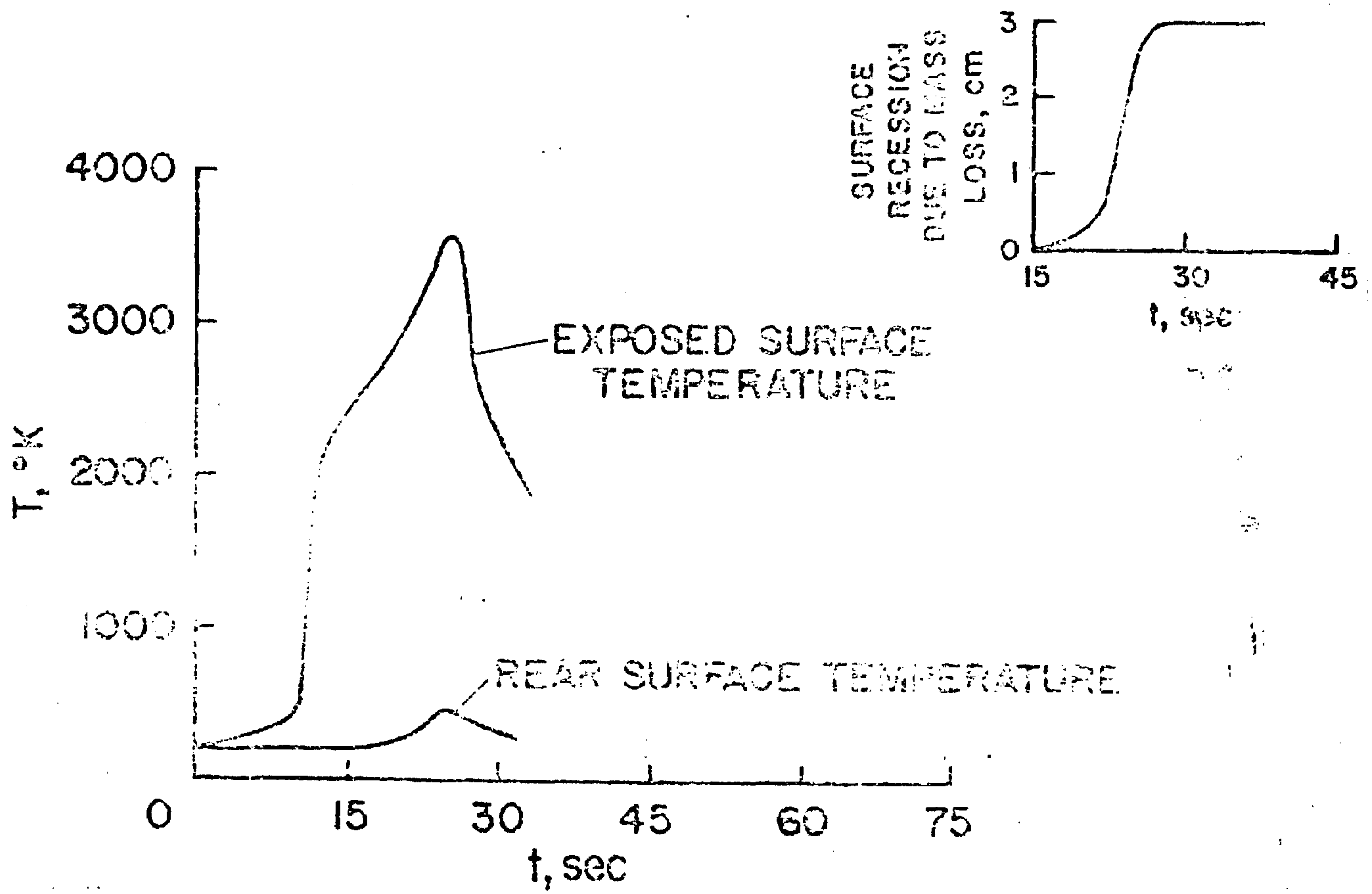


FIGURE 6-41

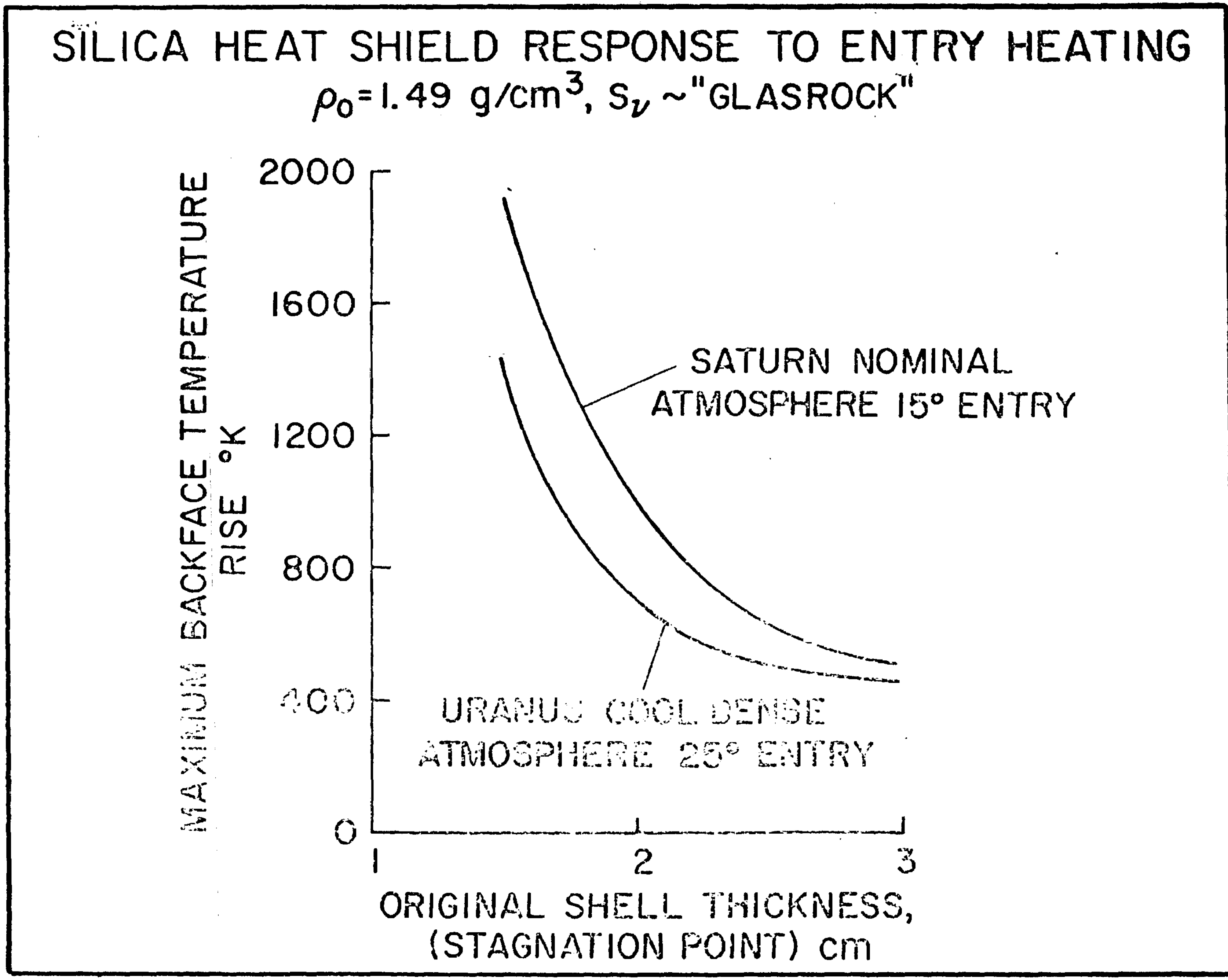

SILICA HEAT SHIELD RESPONSE TO ENTRY HEATING
ρ_0=1.49 g/cm^3, S_ν ~"GLASROCK"
MAXIMUM BACKFACE TEMPERATURE RISE °K
2000
1600
1200
800
400
0
SATURN NOMINAL ATMOSPHERE 15° ENTRY
URANUS COOL DENSE ATMOSPHERE 25° ENTRY
1
2
3
ORIGINAL SHELL THICKNESS, (STAGNATION POINT) cm

FIGURE 6-42

The figure shows rather surprisingly, perhaps, that it is easier to go into the Uranus cool dense atmosphere than it is into the Saturn nominal atmosphere; the reason being, this is essentially a radiative environment and this heat shield just doesn't accept the radiation. It back-scatters it. So it is pretty easy in terms of backface temperature rise. What it tells us is that if we are limited to, say, 700 degrees Kelvin backface temperature, (that corresponds to Sam Mezines' 800 degree Fahrenheit interface temperature), you can get into the Uranus cool dense atmosphere with a less than two centimeters thick shell, into the Saturn atmosphere with a little over a two centimeter shell, and into Jupiter with four centimeters. That comes out to about 1.56 inches of heat shield for the 20-degree Jupiter entry. And that is really a severe environment. So silica really looks very attractive for severe radiative environments.

I think one thing that this silica heat shield could do is broaden the entry envelope into these planets, if that is of any interest for other mission considerations.

So these are the results so far, and we are busy trying to extend these results to other entry angles, other atmospheres.

UNIDENTIFIED SPEAKER: The vitrification process, is that as simple as surface melting that then propagates back through the silica material?

MR. HOWE: We don't have that modeled in great detail. What we have is a density change; that is, when we reach a certain temperature we say the material from then on is transparent; it no longer scatters. So, we have that built in, but we don't have it modeled in any great detail.

UNIDENTIFIED SPEAKER: Isn't the temperature like 3200° or something like that? I thought maybe your peak temperature would have melted the surface.

MR. HOWE: Oh, yes; there's a region at the front of it that's melted and is no longer back scattering. The scattering is being done in the depths.

UNIDENTIFIED SPEAKER: A question of clarification: Were you limited to 800°F temperature, or temperature rise, because you have plotted there a temperature rise?

MR. HOWE: That 800°F was a design temperature that McDonnell Douglas used for the interface temperature between insulation and the heat shield. The ordinate on Figure 6-42 is really absolute temperature, I shouldn't have said temperature rise.

MR. LEIBOWITZ: Does the performance of this heat shield change dramatically for a very intense Jupiter entry where the peak radiation falls below two thousand angstroms?

MR. HOWE: That is about 6 e.v. at the peak. It will reflect effectively in wavelengths between about 0.5 and 6.0 e.v. Those are the constraints for this. So, if it falls into the vacuum ultraviolet - I guess that's what you are thinking?

MR. LEIBOWITZ: Yes.

MR. HOWE: Well, then, it's not going to reflect. Actually, my own opinion is if you have radiation in the vacuum ultraviolet the material won't see it anyway; it will be absorbed by all the molecular species in the gas phase - in the boundary layer.

GEORGE DEUTSCH: I notice that you dealt with an appreciable thickness above the liqueous temperatures of the silica; what's to keep that from simply flowing away?

MR. HOWE: I think that a melt layer that flows along the body would be very thin, George. Bill Nicolet took a look at that in an earlier stage of the Saturn-Uranus studies and concluded that it was really a thin melt layer. These temperature profiles are quite sharp, and the material is eroding at a great rate; so that the primary mass loss is due to the thermochemical erosion normal to the surface.

UNIDENTIFIED SPEAKER: It looks like the equations you first showed were spherical coordinates. Are all of these spherical coordinates?

MR. HOWE: Oh, there's a little exponent in there. If you set it to one it's a spherical geometry, and if you set it to zero it's a slab.

UNIDENTIFIED SPEAKER: But it's either a slab or a sphere?

MR. HOWE: Yes, if we want to do a cone, those are essentially slabs. That is, these are very thin shells, with large internal radii. So, I think a slab would do us well anywhere except at the stagnation point. And even there it is pretty accurate.

SAM MEZINES: Based on the fact that you got most of the heat shield requirements from the shock layer radiation contributions, would a shallow Jupiter entry with higher convective heating require more heat shield than you have shown here?

MR. HOWE: I don't really know, Sam; it's a possibility. We would like to try that seven and a half degree angle Jupiter case to find out. These are not trivial things to run, I might mention. In order to get one case, somebody has to stay up all night - me. We are trying to improve that situation.

N75 20391

HIGH PURITY SILICA REFLECTIVE HEAT SHIELD DEVELOPMENT

James Blome

McDonnell-Douglas Astronautics Company

MR. BLOME: I would like to very briefly describe to you the development program that we have with NASA Ames on the high purity reflective heat shield material.

As summarized on Figure 6-43, we selected the SiO_2 material primarily because it is very highly reflective in the wavelength band of interest. Also, it is shock resistant, has good ablation characteristics, and we feel that the cost would be competitive with other materials.

The major factor, as I discussed, is the fact that it is highly reflective in the correct wavelength band. The factors that influence the reflectance, we feel, are purity and morphology. By morphology, we mean the internal nature of the particles, the shape, size, and void size.

I would like to thank Aerotherm for the use of their spectral flux data which I have plotted on Figure 6-44 for a twenty-degree entry into the Jupiter atmosphere. I said that purity is very important, and this slide primarily addresses the purity effect. We have determined reflectance for three different purity levels of material. The five thousand ppm material, which we feel is quite impure has an SiO_2 binder which contains most of the impurities. Commercially pure, slip cast material, which was Glasrock, has about a 3,700 ppm. These are the total metallic ion concentrations.

This top curve on the figure is for a slip cast part, similar to the one I passed around. In the fabricated state, it has approximately twenty-four ppm. We start with a material that has about 1 ppm total metal impurity ions.

What we did next is to take this spectral flux and integrate it with the three reflectances for these three different purity levels. This is shown on Figure 6-45, which shows how much energy

- REASONS FOR SELECTION
  - HIGHLY REFLECTIVE IN THE CORRECT WAVELENGTH BAND
  - GOOD ABLATION CHARACTERISTICS
  - EXCELLENT THERMAL SHOCK RESISTANCE
  - READILY FABRICATED TO FULL SIZE AT REASONABLE COST
- FACTORS THAT INFLUENCE REFLECTANCE
  - PURITY
  - MORPHOLOGY

Figure 6-43. Silica Selected as the Reflective Heat Shield Material

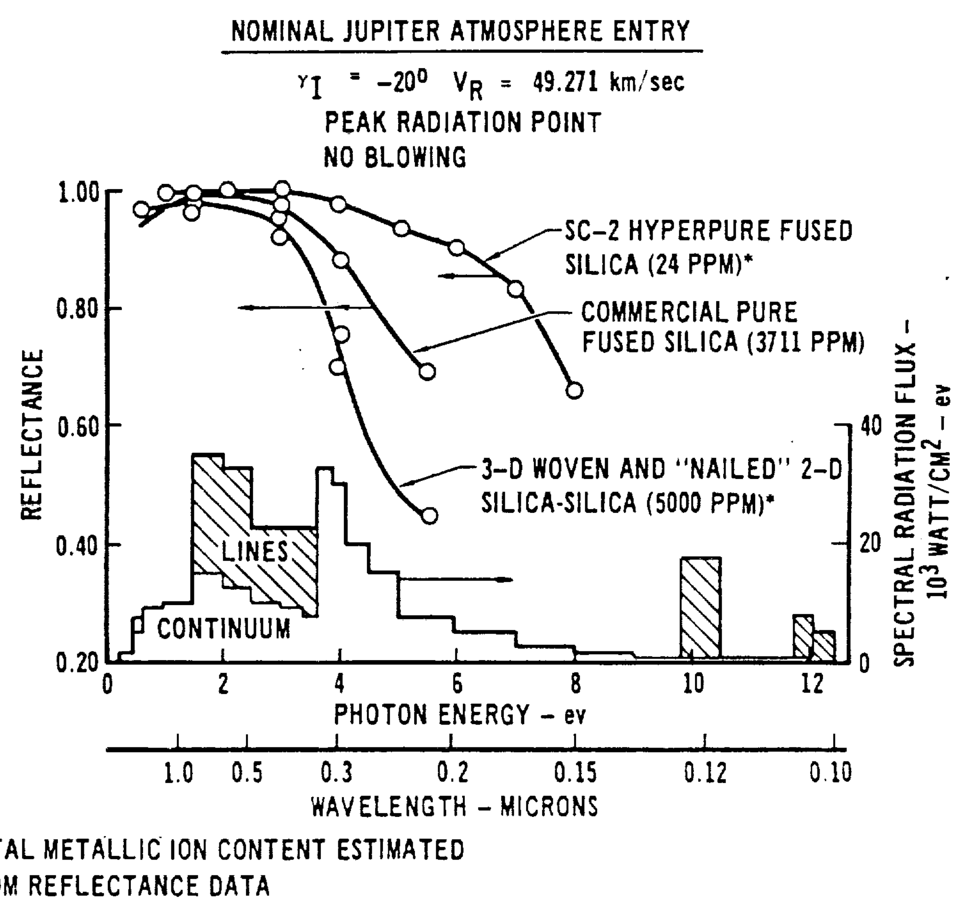


Figure 6-44. Purity Affects Reflectance

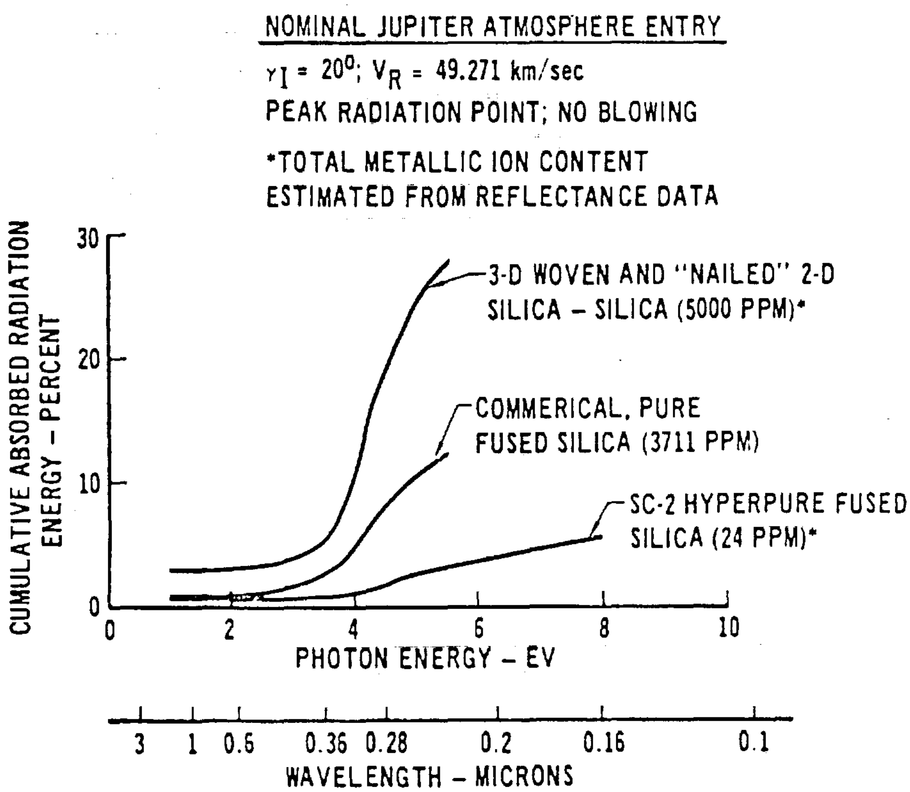
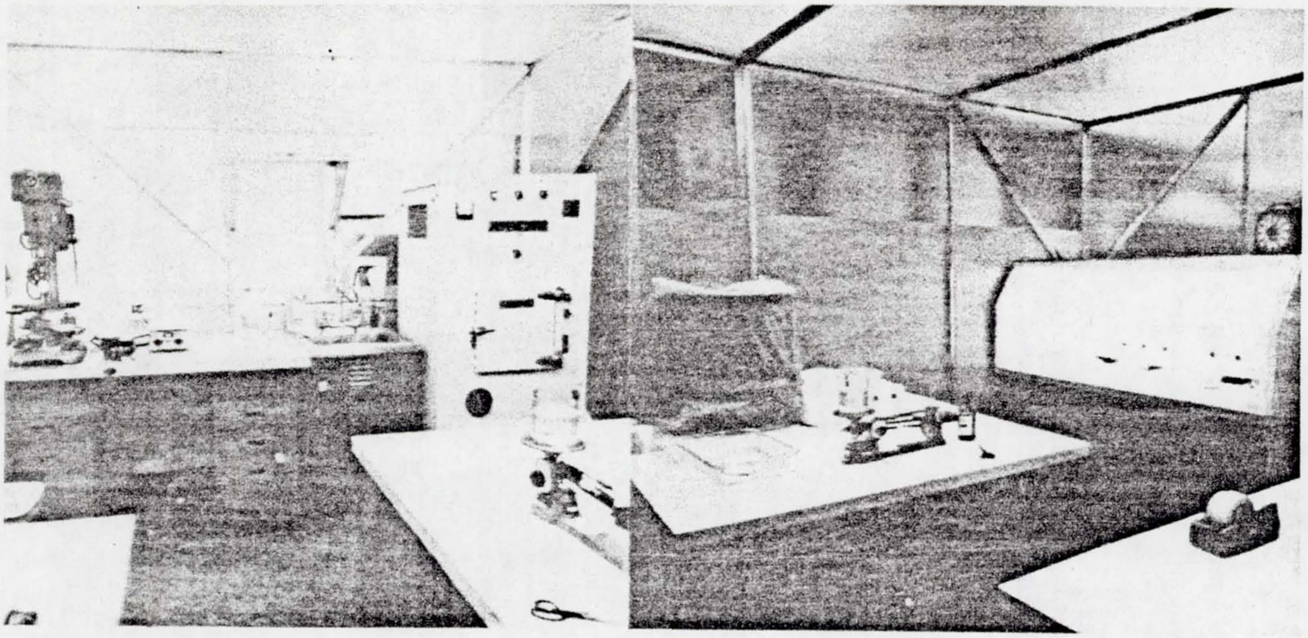


Figure 6-45. Purity Important to Amount of Energy Absorbed

is absorbed, for a given atmospheric entry for the three different purity levels of material. For example, the cumulative amount of energy absorbed up to about 5.5 ev, is approximately three percent for the hyperpure material, about twelve percent for the commercially pure slip cast material, and for the least pure material, about twenty-eight percent of the energy is abosrbed.

We have had some doubts, and people ask us, "How can you maintain this degree of purity?" It's really not that hard once you establish an area that you set aside and use only for this purpose. Figure 6-46 shows a room we put together with plastic film over some structure with normal laboratory equipment inside. There is no special equipment other than a few little items. For example, we can't let metallic materials come into contact with the SiO_2, so we coat metal components with plastic coatings.



CONTAINS: DUAL FILTERED, PRESSURIZED AIR
TEMPERATURE CONTROL (SEPARATED FROM CENTRAL AIRCONDITIONING)
LAMINAR FLOW BENCH
WET DIAMOND MACHINING FACILITY
MICROWAVE & AIR OVENS
SINK WITH DISTILLED WATER

Figure 6-46. High Purity Processing Room/Equipment

Other than that, just a normal, clean room environment. Again, we process only the very high purity SiO_2 material in this room.

In Figure 6-47 we will discuss a little about the morphology aspect which as you recall, has a large impact on reflectance. We have found that probably the most important processing variable which affects morphology is the degree of firing to which you subject the material. We want the reflectance to be as high as possible, and the density we want to be high for ablative reasons and strength reasons. What we have here is data for two different particle sizes of materials, both being hyperpure materials, made two different ways. The data at the left is for a material made by a normal ceramic process called dry processing. The data at the right is for a material made by the slip casting

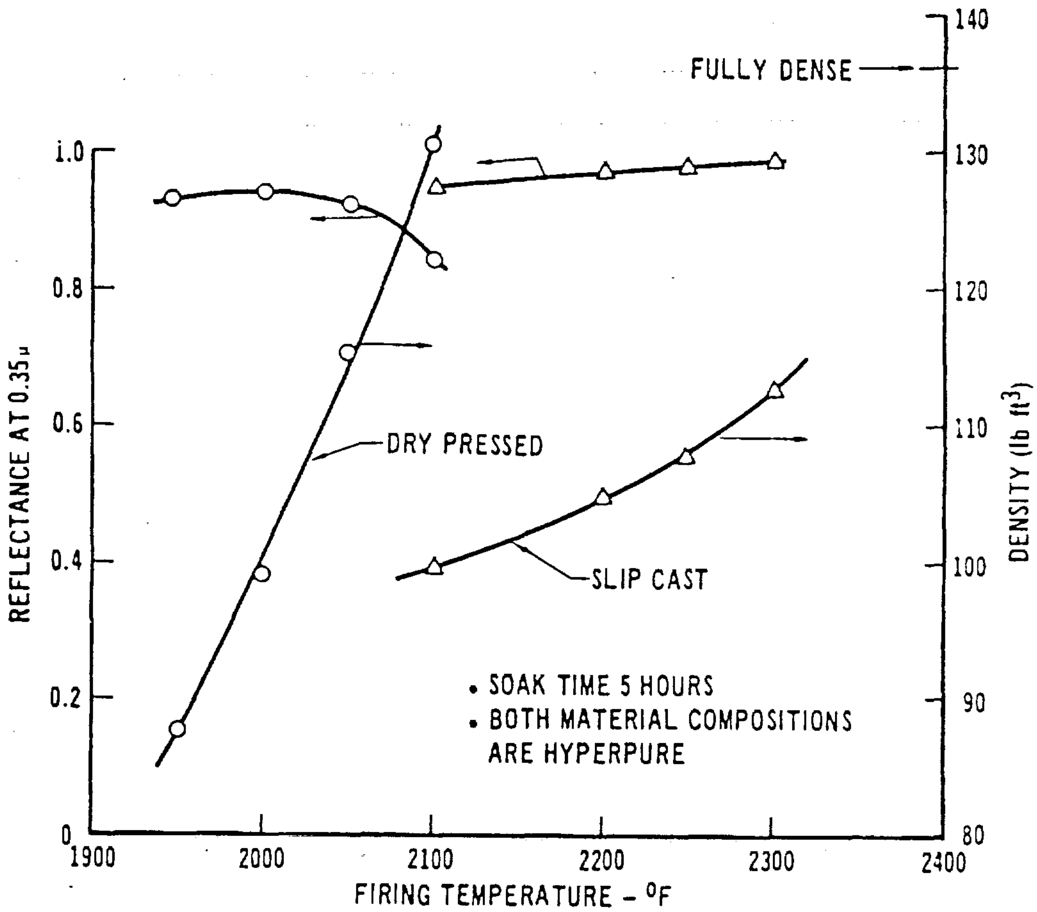


Figure 6-47. Reflectance and Density Change with Firing Temperature

process. We show data here for a dry pressed formulation containing a very small particle size, approximately .2 microns in diameter, silica as part of the charge. As we fire this material to higher temperatures, the density increases very rapidly and as it approaches the completely dense state, that is to say clear the reflectance begins to drop off. Plotted here is reflectance at 0.35 microns (we also have curves for other wavelengths). The slip case material has an average grain size of ten microns. The firing temperature has not yet been reached where we start to see a decrease in the reflectance at 0.35 microns. I think the proof of the material is in these two items reflectance and density.

Morphology can also be studied using the scanning electron microscope and we find this to be a very helpful tool, as Bill has discussed earlier. In Figure 6-48, the top row of pictures are 500x SEM's with firing temperature shown at the top of each picture. You can see a decrease in the size of the voids as temperature increases. The material is much smoother in texture as you proceed to the right. By viewing the same three specimens at approximately 10,000x (lower row) you can see the ultimate particles. As the firing temperature is increased, you can note a decrease in the angularity; the particles are becoming smoother. The sizes of the voids are diminishing.

In order to size these scattering type heat shields, we determine reflectance on a very thick sample and then a very thin sample, on the order of 0.750" and 0.020" respectively.

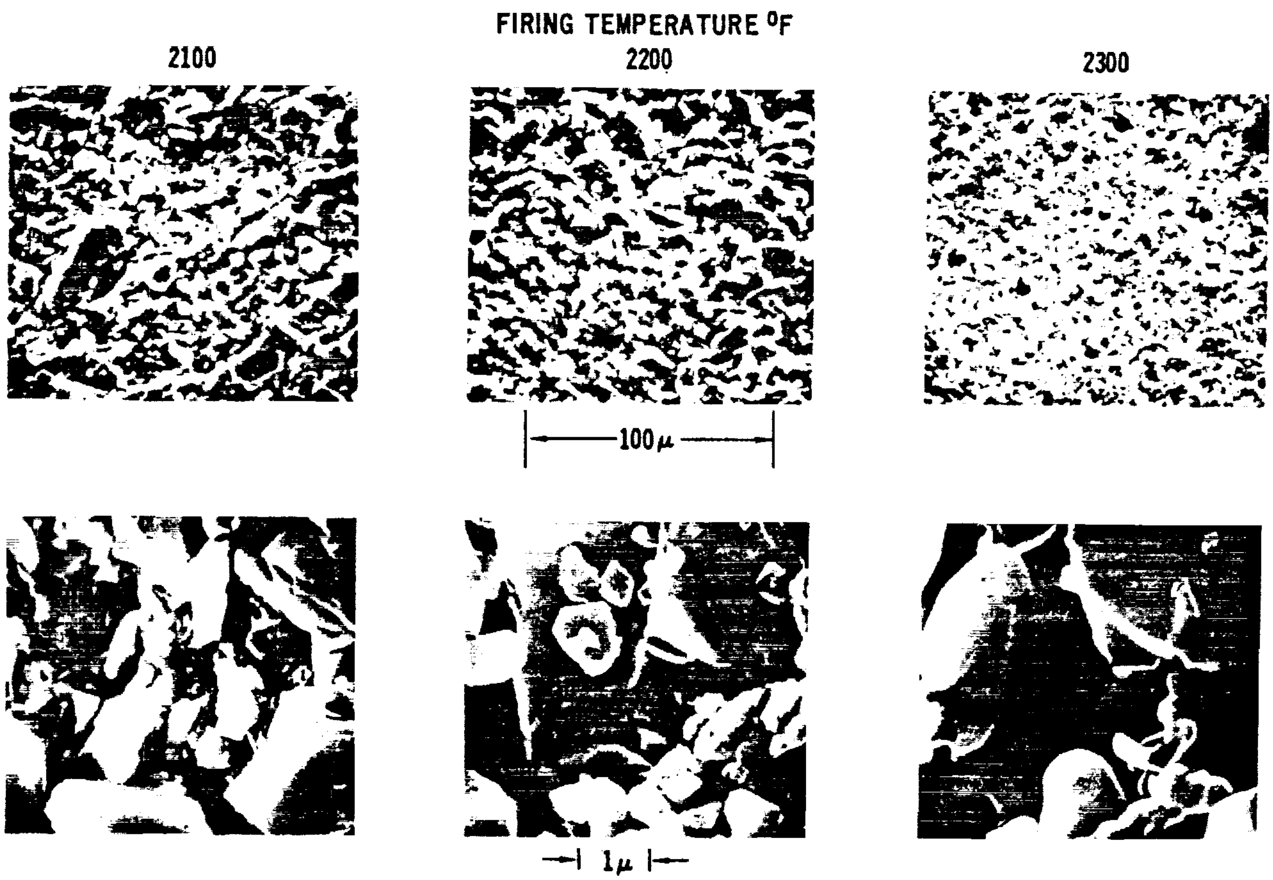


Figure 6-48. Morphology (Microstructure) Helps Explain Properties and Effects of Processing Variables.

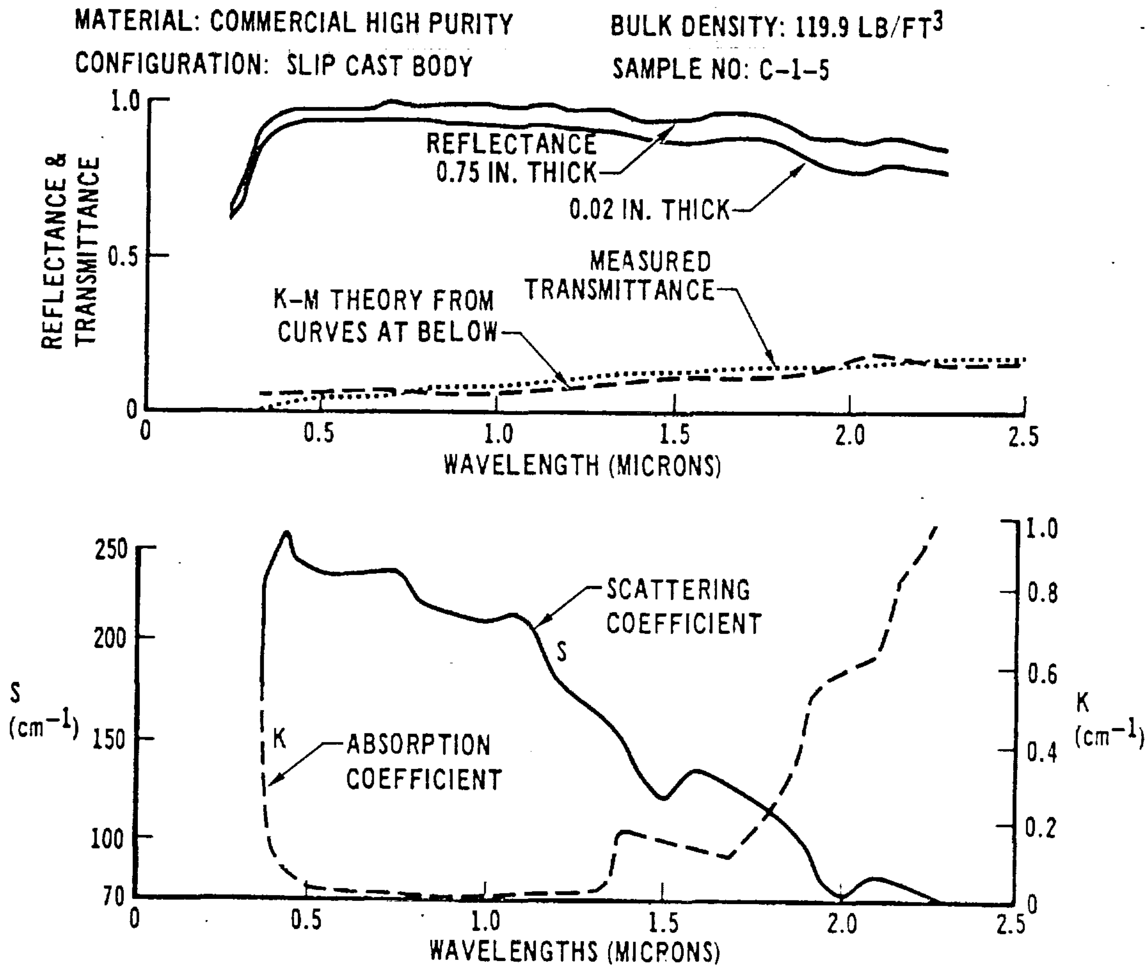


Figure 6-49. Kubelka-Munk Scattering Theory Used to Define Heat Shield Thickness

On the thin sample, we are getting some energy through. Then from that, we can calculate the scattering (S) and absorption (K) coefficients from reflectance data which then can be used in the computer program as John Howe has described. Typical data curves are shown in Figure 6-49.

Conclusions to date on our program are summarized in the table of Figure 6-50: purity and morphology are very important; that pure materials are available under one part per million from three suppliers; that required purity and morphology can be maintained. We feel that quite a high percentage of our steps in how to make this material are now understood. We have determined

- PURITY & MORPHOLOGY IMPORTANT FOR MAXIMUM REFLECTANCE
- PURE MATERIALS AVAILABLE ($\approx$1 PPM METALS)
- REQUIRED PURITY & MORPHOLOGY CAN BE MAINTAINED USING REASONABLE CARE
- 90% OF PROCESSING STEPS NOW DEFINED
- HIGH REFLECTANCE: 0.99 FROM 0.4 TO 1.2μ
0.90 FROM 0.24 TO 1.88μ
- READY TO BE SCALED UP TO FULL SIZED HEATSHIELDS
- READY TO CHARACTERIZE MATERIAL
- APPEARS TO BE COST EFFECTIVE

Figure 6-50. Conclusions

reflectance, 0.99 from 0.4 to 1.2 microns. We feel like our materials are developed to the point when we should talk about scaling up and producing samples of some size and should characterize the material, which we are doing now, in determining strength and stiffness. Cost appears to be in line with other heat shield materials.

UNIDENTIFIED SPEAKER: You speak of maintaining the purity. How far through the whole process of building this heat shield, putting it on the vehicle, having any number of mechanics and so on handling the thing all the way out to the salt water Cape, do you mean maintaining or do you mean achieving cleanliness in your environment?

MR. BLOME: Well, you obviously have to maintain purity. We found some real interesting things in this material. This high purity material opens up an entire new area of interest. You can take this material and fire it up to twenty-three or twenty-four hundred degrees Fahrenheit, and this is just not done now in the state-of-the-art. With other pure materials you start getting devitrification and things like that happening. Really, I think that if you can keep the purity internally or in other words if you can maintain a high purity inside the material, perhaps by sealing, by firing, or even packaging it can be maintained. It has to be done. You have to maintain the purity. We haven't taken any great pains, just the normal procedure in our R and D effort. We have made reasonably large sizes. This is a sample that we core drilled out some specimens for John Lundell at NASA Ames and this is the size that we have been able to make with good success.

DR. KLIORE: Looking at the plasma jet sample you passed around here, I notice some cracks in your surface.

MR. BLOME: That is in the glassy layer, yes.

DR. KLIORE: In connection with remarks made previously about good thermal shock resistance, do you have any comments on that?

MR. BLOME: I think those cracks that you see in the glass are from cool-down and from the contamination of the arc jet. It is a fact, we do get some contamination from the jet. That was exposed to a flux of about 3,600 $\frac{BTU}{ft^2\text{-}sec}$. So that specimen did have a good thermal shock load on it, and it did not come apart. Had we done that with an MgO or Al_2O_3 ceramic specimen, the pieces would be throughout the room, fractured from shock, I am sure. I have seen that happen.

QUESTION: How does the efficiency of the reflective heat shield compare to the black type? Let's say you encountered some warm atmosphere and you didn't have any radiation, or at least you had a low rate. Will it perform fairly comparable to the other type?

MR. BLOME: I think John Howe would be more competent to answer that then I would.

MR. HOWE: In a thoroughly convective environment, it doesn't perform as well as the carbon phenolic, that is, aside from the spallation effects, we don't really know. Silica has a very high sublimation energy, but it is only about half of that of carbon phenolic. So you would expect, in a purely convective environment, that you would need more silica than you would carbon phenolic.

N75 20392

AMES FACILITY FOR SIMULATING PLANETARY PROBE HEATING ENVIRONMENTS

Howard A. Stine

NASA Ames Research Center

MR. STINE: I wish to bring you up to date on what has been done at Ames Research Center in recent years in development of arc-jet entry simulation apparatus, what we are now doing, and what we are planning to do. Along the way, I will attempt to make you aware of the rationale for our activities and try to acquaint you with our schedule for accomplishing this work.

The first illustration, (Figure 6-51) is a sketch of the only piece of arc-jet apparatus ever built at Ames that came anywhere near generating an environment corresponding to a giant planet entry. Its performance is described in Reference 1.* Essentially, it is a long, skinny, tube chopped up into segments. Each segment is made of a good heat conducting material, namely copper. It is water cooled. The segments are spaced with electrical insulation so that the whole device can support the voltage gradient of an electric arc which is established within the tube. At the ends of the tube, are arrays of electrodes, the number being picked to limit the amount of current that each element has to handle to a value that will permit the machine to survive. Remember that this apparatus in itself, is exposed to the same environment that we are trying to simulate, within a factor of two or so. It is a real challenge to assemble such an apparatus so that it will remain intact long enough to accomplish its purpose. Unfortunately, this device is unsuitable for heat shield materials testing because its run duration is only 1/2 sec at most.

The next figure, (Figure 6-52) is a table that shows, historically, Ames arc-jet facility development activity during the last few years. The top two entries in the table list Ames facilities

*Shepard, Charles E.: "Advanced High-Power Arc Heaters for Simulating Entries into the Atmospheres of the Outer Planets" AIAA Paper No. 71-263. AIAA 6th Aerodynamic Testing Conference; Albuquerque, New Mexico/March 10-12, 1971.

6 cm PULSED CONSTRICTED ARC JET

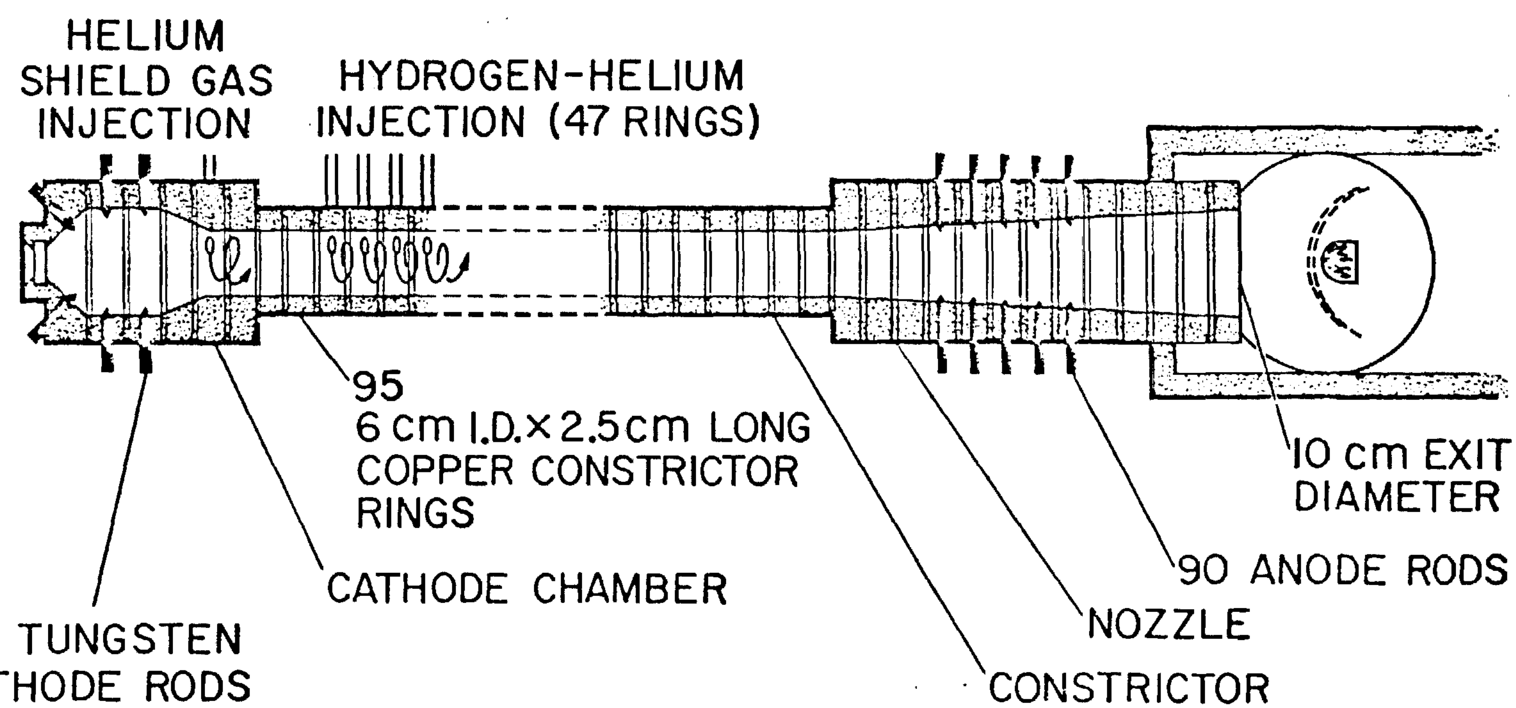


Figure 6-51

TPS FACILITY DEVELOPMENT

AMES RESEARCH CENTER

Name	Power (MW)	Impact Press (Atm)	Enthalpy (MJ/KG)	Stream Area (CM^2)	Gas	Gas Flow Rate (KG/Sec.)	Purpose	Status
TPS Pilot Facility	20	.12 .36	32 32	1135 710	air air	1.25 1.25	RCC Char. & Devt. HRSI Char. & Devt.*	Operational Shakedown
Interaction Heating Facility (CofF '72)	60	.12 .36	32 32	8500 2550	air air	2.5 2.5	RCC Dev. & Qual. HRSI Devt. & Qual.*	Under Const. Under Const.
Giant Planet Pilot Facility (CofF '74)	110	6	600	95	H_2+He	0.1	Arc Technology Devt. Giant Planet Entry Simulation	Design
Trans. & Turb. Flow Test Apparatus (CofF '75)	110	20	4.6	314	air CO_2+N_2	14	Turbulent Flow with Massive Ablation	In Budget
Giant Planet Facility (CofF '77)	160	10	600	113	H_2+He	0.15	Jupiter Entry Simulation	Proposed

*Semi-elliptic-duct nozzle
(all other conical)

Figure 6-52

5/74 - NASA-Ames

dedicated to space shuttle TPS testing. The first is a twenty megawatt machine now in operation, for cyclic testing of high temperature reusable surface insulation. The second, called "interaction heating facility," is in shakedown status. Construction began in 1972 with C of F funding. It consists of a sixty megawatt arc heater and associated D.C. power conversion equipment, and it is nothing more than a scaled-up version of the 20 mw pilot facility.

Finally, of more interest to the people here are the remaining entries in Figure 6-52. For a number of years a need has been recognized for a facility to simulate entry into giant planet atmospheres. Just two months ago authority was received to construct what is called a giant-planet pilot facility. It is expected to operate at a power level of 110 megawatts delivered to the arc heater, to generate impact pressures of six atmospheres at an enthalpy of up to 600 megajoules per kilogram. These conditions are close to those expected at the peak heating point for a shallow entry into the atmosphere of Jupiter. The stream will not be large; only an area of ninety-five square centimeters would be possible without additional electric power. Mixtures of hydrogen and helium will be used as the working gas, at very low flow rates. Two purposes will be met by building this pilot facility. One is to advance the technology of arc heater development to permit operation in the giant-planet entry regime; the second is to at least come close to being able to simulate, if not Jupiter entries per se, then those of Saturn or Uranus probe missions. As I said, we have been authorized to go ahead with the giant planet pilot facility. It is at present under design.

In the fiscal year 1975 budget is an item (Figure 6-52) to produce another arc heater in the 100 MW class. This device, called "Transitional and Turbulent Flow Test Apparatus," is nothing more than an upgraded Linde arc heater that will be used to produce very large flow rates of moderate enthalpy gas.

It can be operated with air, or CO_2 but it could for that matter accept mixtures of hydrogen and helium. It's purpose is to produce flows in which transition to turbulence will occur simultaneously with massive ablation from heat shield materials. It will not produce appreciable radiative heating, but will rather produce very high convective heat transfer rates.

Finally, it is in our plan, which is based on a 1984 Jupiter probe mission, to build a more powerful giant planet facility that would achieve the full Jupiter entry simulation assuming the nominal Jupiter atmosphere. It would produce impact pressures up to ten atmospheres, the same enthalpy as the pilot facility, be somewhat larger, but not very much. I will try to point out why it is the large increases (from 110 to 160 MW) don't permit much increase in size.

Figure 6-53 shows domains of enthalpy, or energy content per unit mass as a function of impact pressure for probes that enter giant planet atmospheres. On it one can conveniently also plot the corresponding performance domains of such simulation facilities as exist today. Notice that their operating domains lie very close either to the ordinate or the abscissa. Close to the abscissa and continuing out to even much higher impact pressures than those shown (of the order of two hundred atmospheres) the RENT and the HIP facilities, by nature very low enthalpy devices, can operate. The crosshatch band adjacent to the ordinate corresponds to the performance domain for the six-centimeter pulsed device shown on Figure 6-51. It has, indeed, generated enthalpies that correspond to Jupiter atmosphere entry, close to 10^9 joules per kilogram, but only at impact pressures of less than one atmosphere.

As I said, peak heating for Jupiter entry lies at enthalpy and pressure values of 600 MJ/kg and 10 atm., respectively for a fifteen degree initial entry angle. Saturn and Uranus entry

ENTHALPY VERSUS IMPACT PRESSURE FOR GIANT – PLANET ENTRY AND EXISTING ARC-JET FACILITIES

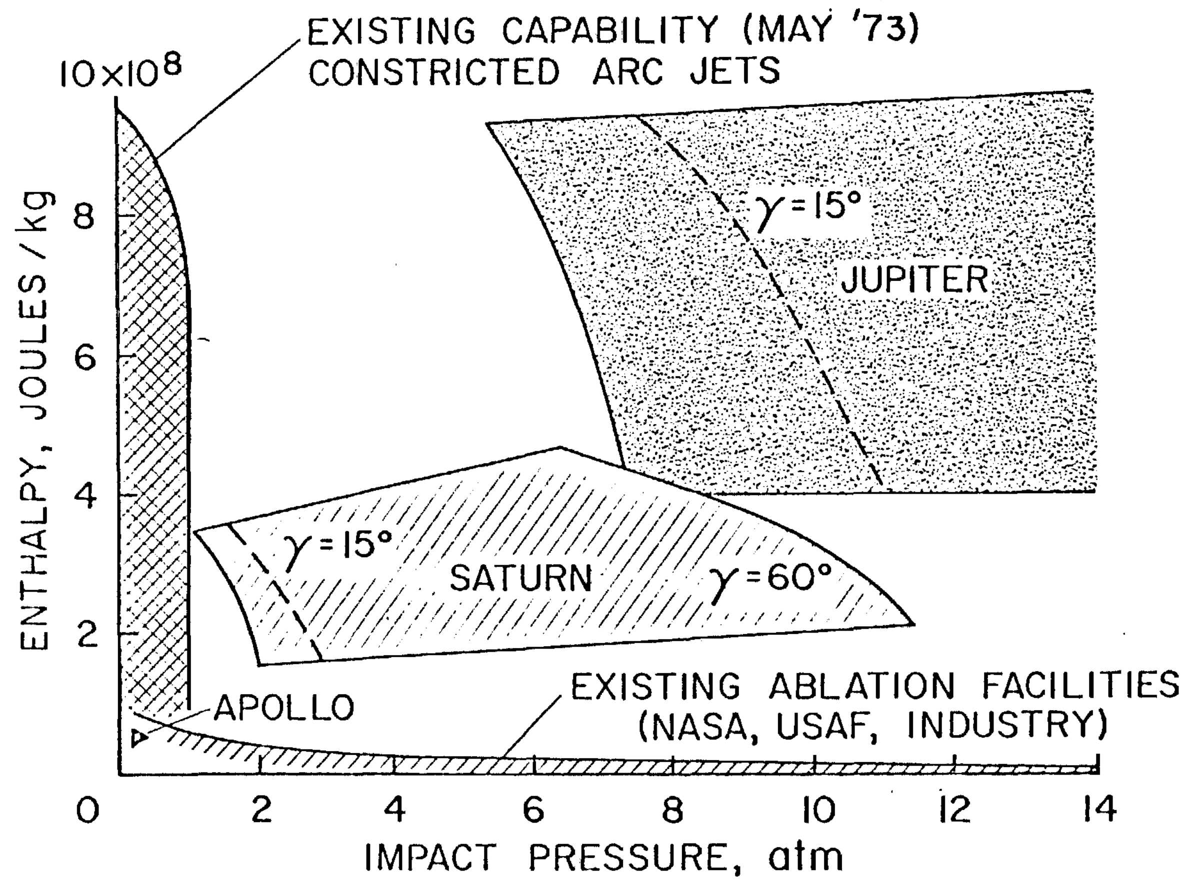


Figure 6-53

domains lie below and to the left of that for Jupiter, and one may note that existing facilities are very close to being able to simulate these entries now.

Why is it that arc jets and other facilities as we know them have operating domains that lie close to the axes in this plot (Figure 6-53)? The reason is a simple one, namely that the stream power density required to produce the Jupiter entry environment is very large, (see Figure 6-54). Figure 6-54 shows essentially the same information as Figure 6-53, but with the addition of lines of constant stream power density. For example the line that lies closest to the Jupiter entry trajectory for an initial entry angle of 15 degrees corresponds to a stream power density of one and one half megawatts per square centimeter of stream area impinging on the heat shield nose. Present arc heater technology is such that only two-tenths megawatt per square centimeter has been achieved at Jupiter-entry enthalpy. I should also point out that the shuttle TPS devices that are described in Figure 6-52 are creampuffs by comparison. Their operating domains all lie very close to the origin of Figure 6-54 (32MJ/kg; 0.2 atm).

Figure 6-55 is a plot that shows the present arc heater power supply capability at Ames Research Center. The supply will produce an output, under ideal conditions, as a function of run duration along the top curve on the graph. For shuttle TPS testing, it will generate up to seventy-five megawatts for periods of 1/2 hour if an exact match between arc heater and power supply were achieved. Because a perfect match is not ordinarily possible, one must take a small penalty as shown by the cross-hatched band below the line of ideal output. Thus, our shuttle arc is designed for sixty megawatts, and will operate in the cross-hatched band near 2,000 seconds. For short run times, like the ten seconds corresponding to entries into giant planet atmospheres, we expect that the power supply will, under ideal

STREAM POWER DENSITY FOR GIANT PLANET ENTRY

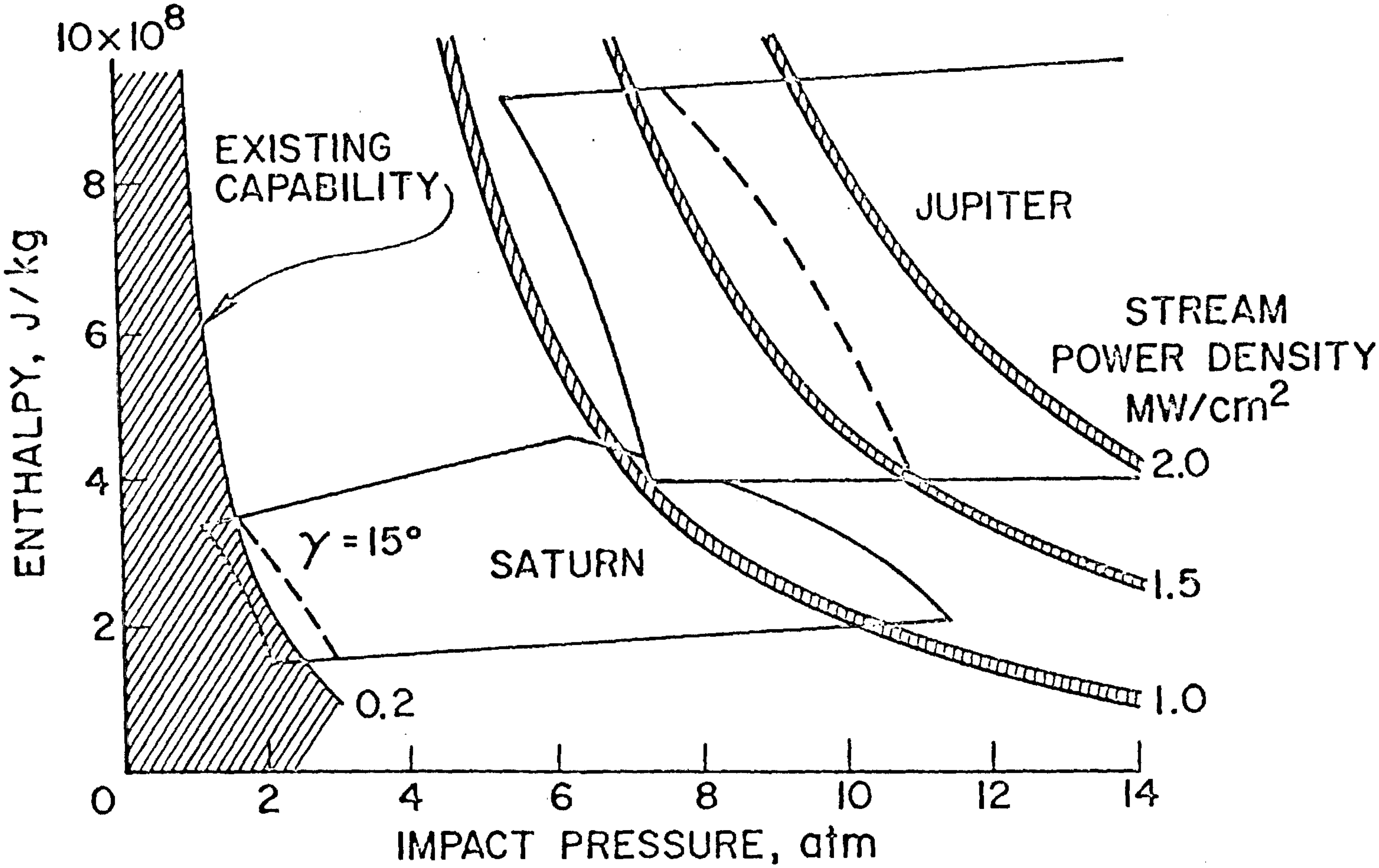


Figure 6-54

AMES DC POWER SUPPLY COMPARED TO GIANT-PLANET ARC REQUIREMENTS

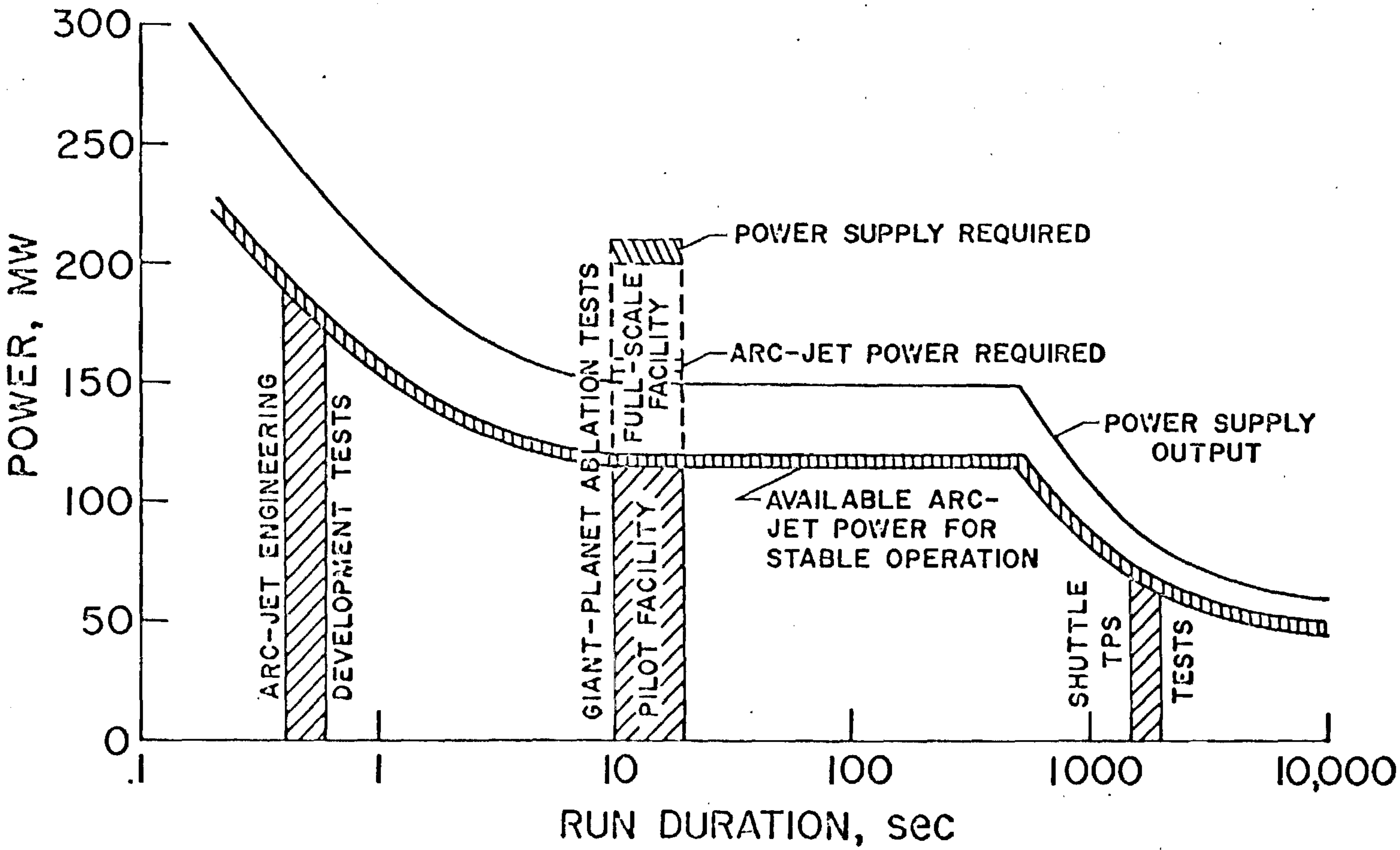


Figure 6-55

conditions, produce one hundred and fifty megawatts of D.C. power. We believe we can certainly deliver one hundred and ten megawatts to the giant planet pilot facility. If we elect to operate the pilot facility in a heat-sink mode for times less than one second, we can perhaps deliver as much as one hundred and seventy-five megawatts to the heater.

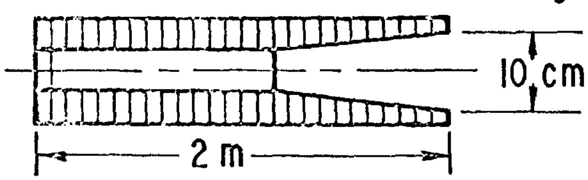
To accomplish a Jupiter entry simulation, we estimate that it is necessary to deliver 160 MW to the arc heater, as is also shown on Figure 6-55. Even the present power supply would not be sufficient to do this task if the atmosphere model of Jupiter remains as it is thought to be today.

Figure 6-56 shows our giant-planet facility development plan in terms of arc heater performance. The device shown in Figure 6-51, representative of present technology, can generate a little over one atmosphere impact pressure at twenty megawatts, with corresponding cold-wall heating rates of fifteen kilowatts per square centimeter. The giant-planet pilot facility, as I said, is also a 600 megajoule per kilogram device. We will attempt to generate impact pressures up to six atmospheres at 110 megawatts, with corresponding combined heating rates up to thirty-five kW/cm^{-2}. With 160 megawatts available, impact pressure can be raised to ten atmospheres at a slightly higher heating rate. But stream size, as is shown, can be increased only slightly.

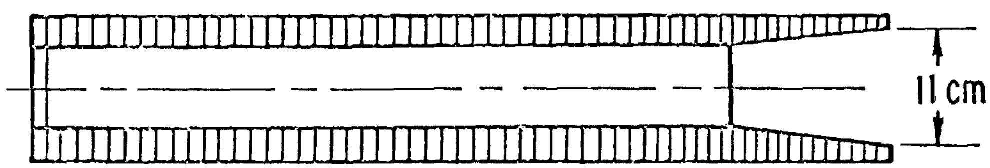
Owing to the present lack of definitive information both as to the character of Jupiter's atmosphere and to the behavior of heat shield materials at Jupiter entry conditions, it is believed that a probe mission to Jupiter involves several steps which must be taken in sequence, Figure 6-57. First, some arc heater development is necessary to find out whether the required facility can be built. Second, we have to build the facility. Third, we have to find out whether or not a viable heat shield can be built. Only then do we know whether or not a Jupiter

GIANT-PLANET ARC FACILITY DEVELOPMENT PLAN

	ENTHALPY	IMPACT PRESSURE	ARC-JET POWER	COLD-WALL HEAT RATE
PRESENT TECHNOLOGY	600 MJ/kg	1.2 atm	20 MW	15 kW/cm^2



	ENTHALPY	IMPACT PRESSURE	ARC-JET POWER	COLD-WALL HEAT RATE
PILOT JUPITER ARC	600 MJ/kg	6.3 atm	110 MW	35 kW/cm^2



	ENTHALPY	IMPACT PRESSURE	ARC-JET POWER	COLD-WALL HEAT RATE
JUPITER ARC FACILITY	600 MJ/kg	10 atm	160 MW	48 kW/cm^2

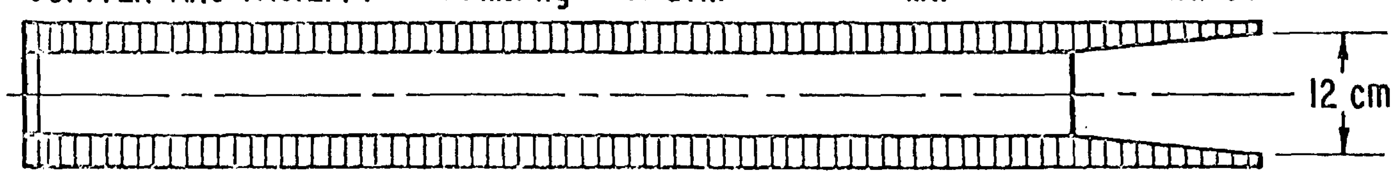


Fig. 10

Figure 6-56

SEQUENTIAL STEPS FOR JUPITER PROBE MISSION

1) ENGINEERING ARC DEVELOPMENT

2) BUILD FACILITY

3) HEAT SHIELD DESIGN

MISSION FEASIBILITY ESTABLISHED

4) SPACECRAFT DESIGN

Figure 6-57

probe mission is feasible. Finally, assuming successful completion of all foregoing steps, we can start designing a space craft.

Figure 6-58 shows our time schedule. Actually, design work was started on the pilot arc about two months ago, so we are now slightly ahead of schedule. We expect the pilot facility to be operational in the middle of fiscal year 1976. Thereafter, both arc development testing and some heat shield materials testing will be carried out. If it turns out that the Jupiter entry environment is more benign than is now thought, it may develop that the pilot arc facility can simulate the Jupiter probe entry environment as well as those of Saturn and Uranus. Otherwise, we will have to go through the complete cycle shown on Figure 6-58 which would permit us to say whether or not we have a viable heat shield design sometime during the middle of 1980. Thereafter, mission approval and probe construction would consume the remaining time prior to spacecraft launch in 1984.

DR. NACHTSHEIM: Questions?

MR. SEIFF: Howard, is any attention being given to using this existing facility to achieve 600 megawatts?

MR. STINE: Megajoules per kilogram

Mr. SEIFF: -- per kilogram?

MR. STINE: It is not water cooled, Al. You can't run it more than one-half second at a time.

MR. SEIFF: It doesn't get the right pressure; but is there any attention being given to evaluating materials in there? It has the correct enthalpy, apparently.

GIANT-PLANET ENTRY ENVIRONMENT FACILITY

TIME SCHEDULE, FISCAL YEAR

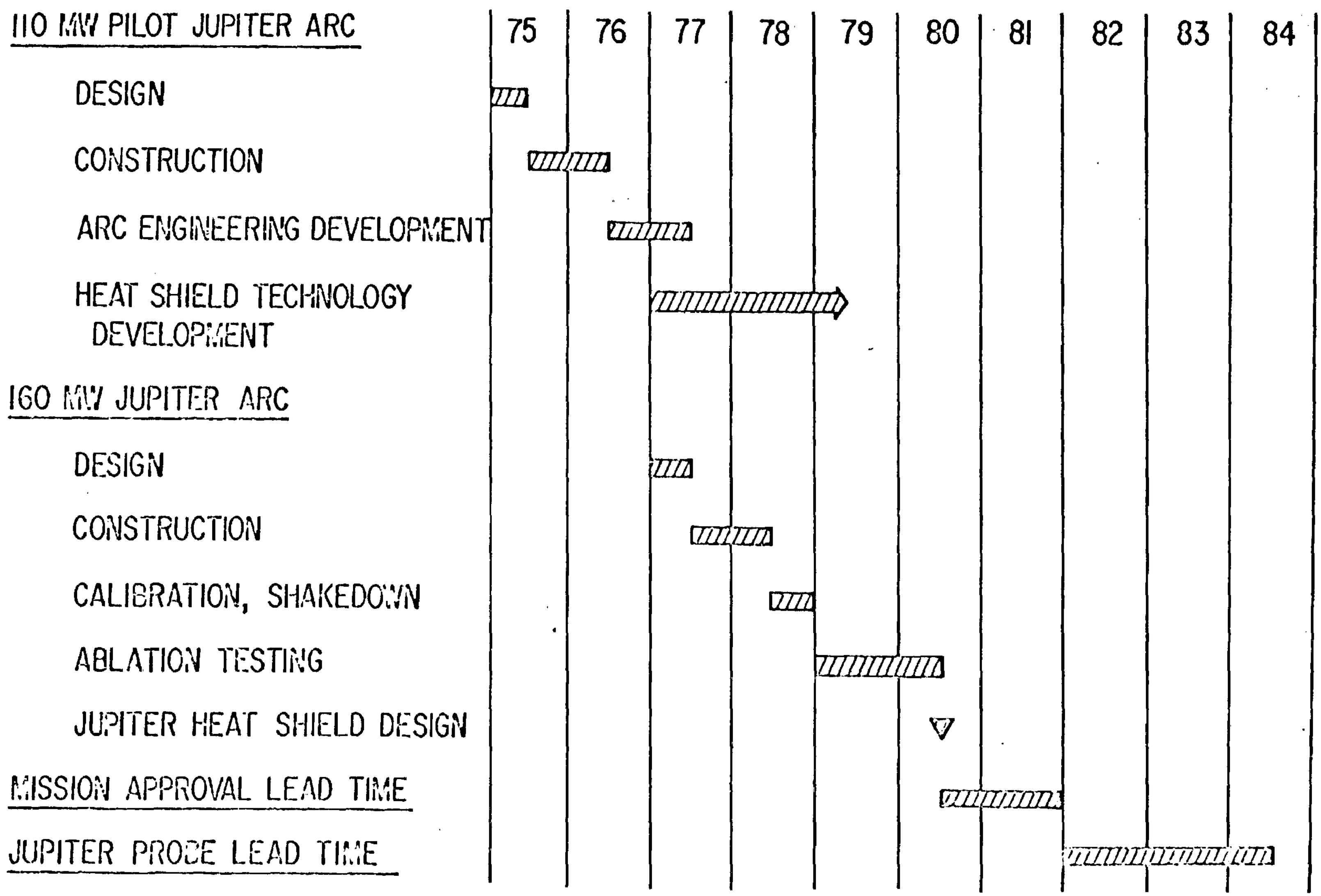


Figure 6-58

ORIGINAL PAGE IS OF POOR QUALITY

MR. STINE: Well, it will sickle through a piece of aluminum bar four inches thick in a half a second, but it won't quite get a piece of graphite hot enough to start ablating. It just barely starts, and then the run is over.

MR. SEIFF: Oh; the run time is too short. That is its limitation.

MR. STINE: Longer than a shock tube but shorter than the time it takes for the material to respond.

MR. NICOLET: What about the possibility of looking at aerothermal environments with that. Would it take a sizable model?

MR. STINE: It's got a ten centimeter diameter nozzle exit. Yes, we did do that, actually.

MR. NICOLET: You did look at aerothermal environments?

MR. STINE: Yes; well, we tried to determine what the device was putting out. We measured the heating rates: convective and radiative; we measured enthalpy, of course, impact pressure, and things of that nature; hydrogen-beta line broadening, things of that sort; some spectra.

N75 20393

SECTION VII - COMMUNICATIONS AND DATA HANDLING

T. L. Grant
NASA - Ames

MR. GRANT: This session is on communications and data handling. Before I introduce the speakers that are listed, I would like to say a few words about the communications system in general, just to give you an outline of the objectives, some of the problems, and an idea of our approach.

The obvious objective of the communications system is to return science data. But aside from that, we are concerned not only with basic science information for the first missions but also with considerations for follow-on missions. At the same time we want to minimize the technology development and achieve some commonality between the missions. The last two objectives are important in this era of low cost emphasis because the communications system has historically represented about 30 percent of development costs for a mission.

On Figure 7-1 I have a cartoon on communication problems. You have seen this a couple of times before in past sessions, but it helps to illustrate where the basic problems are for this communication link.

First of all, shown schematically, are a couple of lines representing the atmosphere and ionosphere and reminding us that we really don't know through what kind of environment we have to propagate in order to communicate with the entry probes.

The other constraint is a common one for all space vehicles. We have a power, weight, and volume limit constraint. But the big difference between communicating from a probe entering at the atmosphere to a flyby spacecraft and communicating from a space-

COMMUNICATION PROBLEM

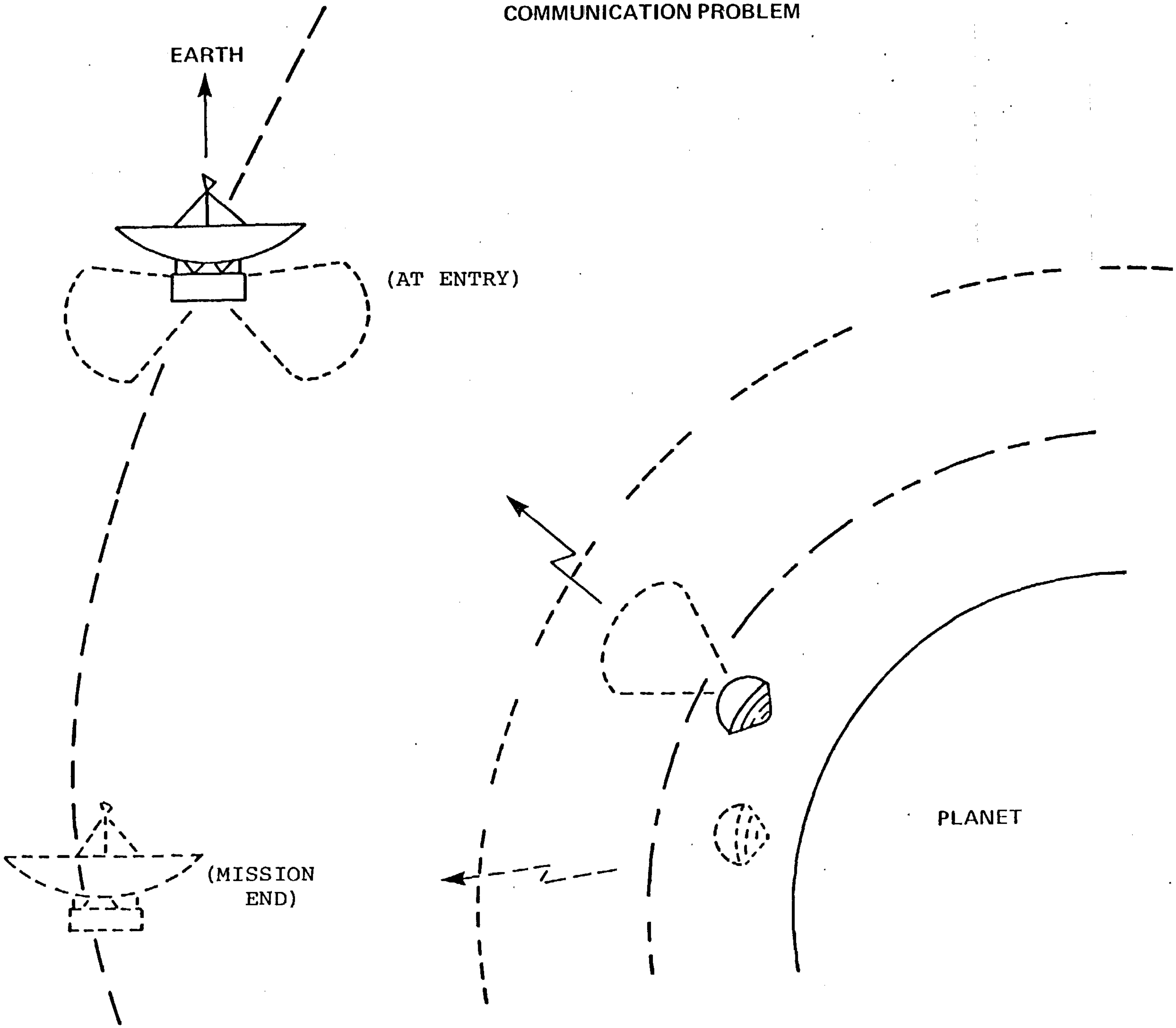


FIGURE 7-1

craft to Earth is that first we have a very limited amount of time to communicate and second we have a large geometry change over the communication time. For the Pioneer-type of mission, we have established a baseline design that accommodates this geometry variation, or change in aspect angles, by using broad-beam, axially-symmetric antennas.

That outlines the basis of the problem, and as you know, the method of solution has been to begin with the current models of the atmosphere environment and through a feasibility study, come up with a baseline design which we expect to evolve as our studies continue.

Figure 7-2 shows the pertinent points of the baseline design for Pioneer. The first thing to note is that our baseline design provides for pre-entry data storage and not transmission. The McDonnell-Douglas Saturn-Uranus study proposed a design with 15,000 to 30,000 bits of pre-entry storage, primarily accelerometer data.

The second important point is that all events are timed in sequence or are activated by a G switch, i.e. there is no command link with the probe, and this is an important consideration as we review the baseline design.

We have a relay link because in order to accommodate most of the missions, a direct link was not felt to be feasible and would constrain the mission design severely. Therefore, telemetry is transmitted only during the descent phase of the probe entry and for this baseline the rate is 44 bits per second over a time interval from about 25 to 70 minutes. This encompasses not only different atmospheric entries for different planets, but also the different models of the planetary atmospheres and allows for dispersion in the entry angle and phasing.

BASELINE DESIGN (PIONEER)

- PRE-ENTRY DATA STORAGE
- TIMED SEQUENCE + 'G' SENSE
- RELAY LINK
- TELEMETRY TRANSMISSION DURING DESCENT
- 44 BPS/25 TO 70 MINUTES
- AXISYMMETRIC, LOW GAIN ANTENNAS
- 400 MHz CARRIER
- NARROW-BAND PCM-FSK MODULATION
- CONVOLUTIONAL CODING

FIGURE 7-2

As previously mentioned, this design utilizes axially symmetric low-gain antennas for both the transmitter and receiver namely a micro strip antenna with a gain of about 7 db on the probe transmitter and a loop vee antenna with a gain of about 2.5 db on the bus receiver.

The baseline carrier frequency is 400 MHz with a modulation scheme that is narrow band binary frequency modulation with convolutional coding, and we haven't as yet decided exactly what decoding method would be used. We are still doing trade-offs to determine the code constraint length and whether to use maximum likelihood or sequential decoding.

Figure 7-3 shows one of the prime problems in the communication link, the radio frequency environment. I will speak briefly about the ionospheric absorption and turbulence models.

Figure 7-4 - the turbulence model is considered to be a weak homogeneous turbulence in most of the atmospheres. This implies that the amplitude modulation of the signal is the important effect of the turbulence.

The amplitude has a narrow band spectrum with a log normal probability density. The standard deviation of this statistic is proportional to the structure factor in the atmospheric turbulence. It is also proportional to the frequency of the carrier to the 7/12ths power and the length of propagation, L, to the 11/12ths power. The problem here is we currently have virtually no information from which to decide on the structure factor or the propagation length that we have to deal with as the probe enters.

The turbulence induced modulation bandwidth is estimated to be proportional to the perpendicular wind velocity and inversely propor-

RADIO FREQUENCY ENVIRONMENT

- CURRENT MODELS -

- NOISE SOURCES
- ATMOSPHERE ABSORPTION

R. COMPTON

- IONOSPHERE ABSORPTION
- TURBULENCE FADING

FIGURE 7-3

TURBULENCE EFFECTS

- o WEAK - HOMOGENEOUS
- o AMPLITUDE MODULATION
- o NARROW BAND
- o LOG NORMAL
- o STD. DEV. $\propto C_N F^{7/12} L^{11/12}$
- o MODULATION BANDWIDTH

$$F_{3DB} = 0.3 \, V_{\perp}/L_0$$

- o MODEL: STD. DEV. $\leq$ 0.23

$$F_{3DB} \leq 2 \text{ Hz}$$

- o NEED PIONEER 10/11 OCCULTATION ANALYSIS

FIGURE 7-4

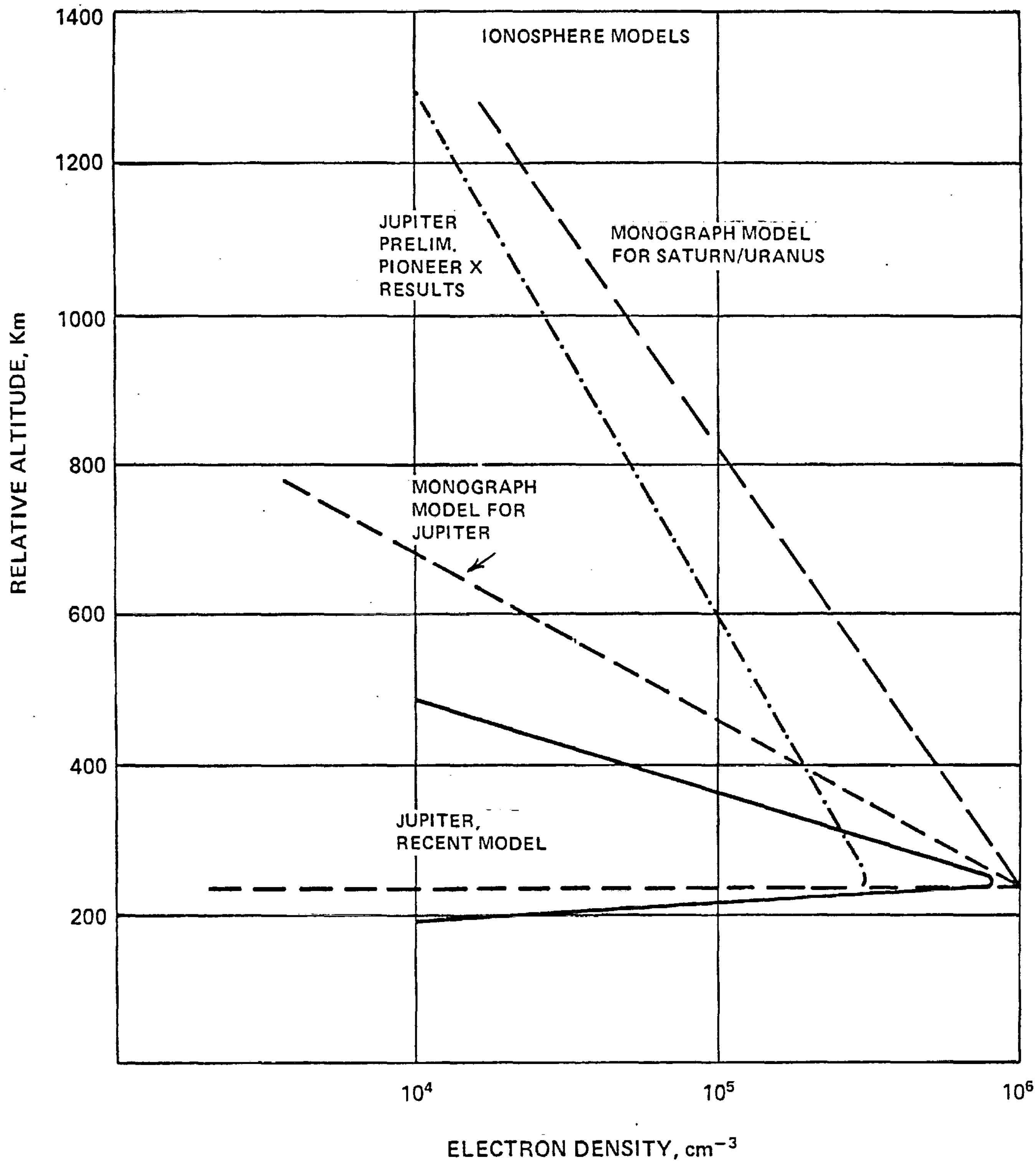
tional to the largest scale size of the turbulence. Here, again, we don't have very good measures of either of these parameters. although the wind is modeled for Jupiter as being something on the order of 100 meters per second. Comparing it with other turbulent atmospheres, like Earth, which is our only other real model, it is estiamated that the scale factor of the turbulence could be on the order of about 50 meters to perhaps 150 meters.

This gets us to the model that we are currently using for the amplitude modulation. We are using a standard deviation of about .23 or less on the amplitude modulation, and a bandwidth of less than two Hertz. But we need some real data to verify these assumptions and that points out the need for some analysis of the Pioneer 10 and 11 occultation data. We are hoping that we can have some of this analysis done by Richard Woo of JPL who has done similar work for the Pioneer-Venus project.

The other factor in the link analysis is ionospheric loss. Here, there are two important considerations; the peak density of the ionospheric electron density and the scale height. Figure 7-5 shows (with a little bit of license from communication engineers point of view) a model of the ionospheres as if they started at the same relative altitude. Each density model is still quite different, depending at whose model or what data you look. As you notice on the figure, the NASA Space Vehicle Design Criteria monograph of Saturn-Uranus ionospheric density has a peak electron density of 10^6 and a fairly large scale height.

The Jupiter preliminary Pioneer 10 results shows a scale height that is a little larger but a peak electron density of only about 3×10^5. The monograph for Jupiter, in contrast, shows a considerably lower scale height.

Plotted for reference, from a recent article in Science, is a projected possible profile with a very low scale height and a peak electron density of about 10^6.



IONOSPHERE MODELS
JUPITER PRELIM. PIONEER X RESULTS
MONOGRAPH MODEL FOR SATURN/URANUS
MONOGRAPH MODEL FOR JUPITER
JUPITER, RECENT MODEL
RELATIVE ALTITUDE, Km
1400
1200
1000
800
600
400
200
10^4
10^5
10^6
ELECTRON DENSITY, cm^{-3}

FIGURE 7-5

An important factor to note is that the integral over the altitude of this electron density is what really determines the attenuation. Thus, if we use the most extreme model, the one for the Saturn-Uranus ionosphere, to determine attenuation, we will have a conservative estimate. Figure 7-6 shows the attenuation versus frequency for this extreme model and predicts the attenuation of the ionosphere to be less than a 10th of a db at 400 megaHertz. Please note, however, that the NASA monograph allows the peak electron density for the Saturn-Uranus ionosphere to be as much as an order of magnitude higher than this, even though thus far there is no firm scientific rationale for that. So I feel that the attenuation versus frequency profile of Figure 7-6 is realistically conservative, but not an absolute worst case.

Our first speaker, Reavis Compton, is doing telecommunications work for advanced programs at Martin-Marietta and has been involved with advanced programs for the past four years or so. He will talk about mirowave propagation in the atmospheres of the outer planets.

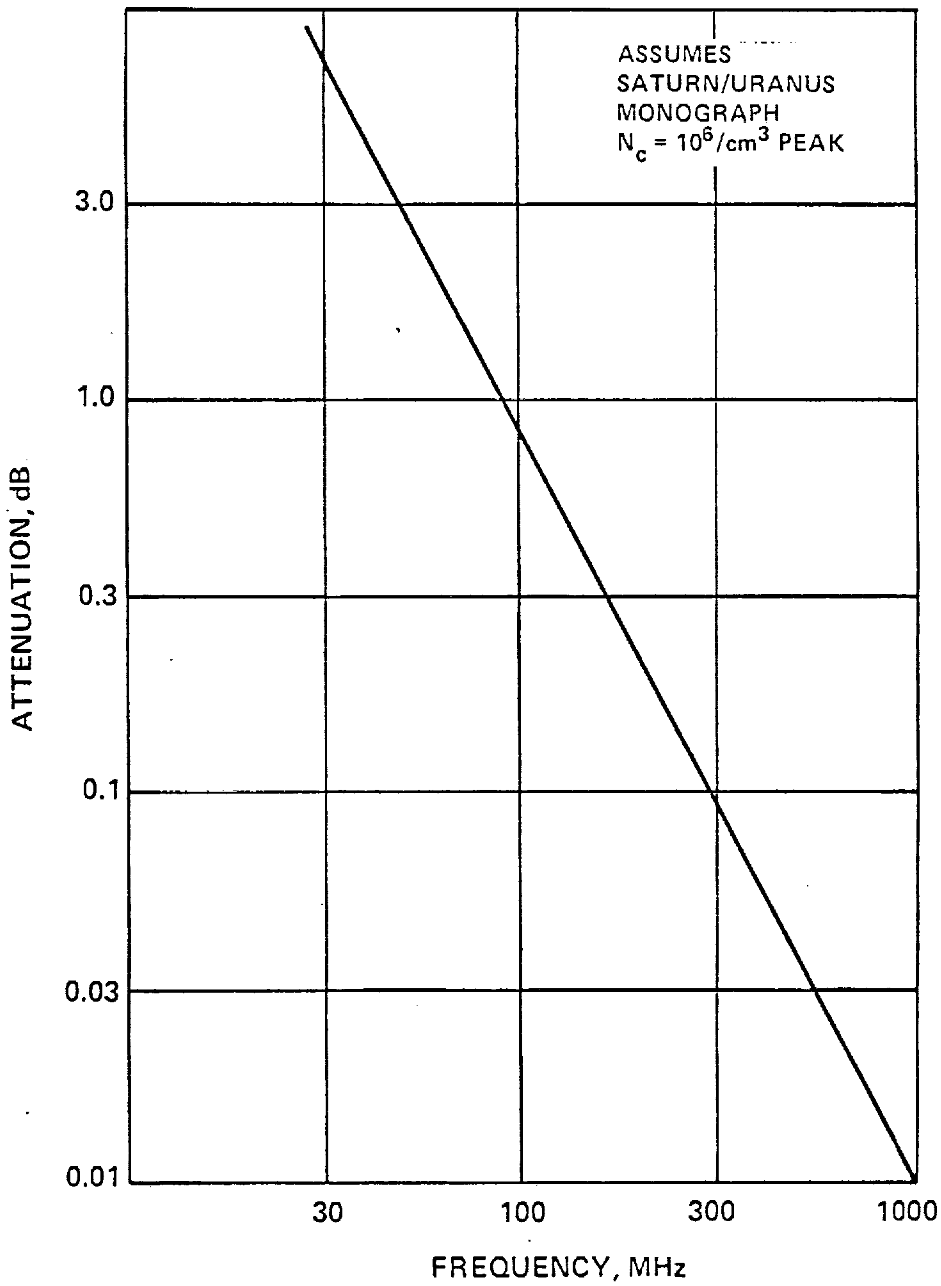

ASSUMES
SATURN/URANUS
MONOGRAPH
$N_c = 10^6/cm^3$ PEAK
3.0
1.0
0.3
0.1
0.03
0.01
ATTENUATION, dB
30
100
300
1000
FREQUENCY, MHz

FIGURE 7-6

MICROWAVE PROPAGATION IN THE ATMOSPHERES OF THE OUTER PLANETS

R. E. Compton

Martin Marietta Corporation

N75 20394

MR. R. E. COMPTON: First of all I will discuss the atmosphere absorption that exists in the atmospheres of the three major outer planets, Jupiter, Saturn, and Uranus; then I will discuss system noise temperature problems at Jupiter.

As we know, the atmospheres of the outer planets are very similar in content, being comprised mainly of hydrogen and helium. There are three principle sources of microwave absorption: the ammonia and water content, and ammonia clouds, if present. Microwave absorption; therefore, is proportional to several factors: the elevation or depth that we go into the atmosphere; the probe aspect angle at which we transmit from the probe to the spacecraft; the operating frequency at which we operate the RF link; and also the models that describe the various atmospheres for the three planets.

Figure 7-7 shows, for instance, the calculated zenith absorption for the Jupiter cool/dense atmosphere which is the worst-case model. It has the highest ammonia mass fraction of the three atmosphere models. The position of the ammonia/water solution cloud is shown and you see from the curves the variation in absorption as frequency and depth are increased. Shown are the values for propagation directly up through the atmosphere, normal to the surface sphere.

Figure 7-8 shows how the absorption varies with the atmosphere models, the dotted line being the nominal model and the solid line the cool/dense. As seen, there is a large difference between the models at higher frequencies. But as we lower the frequency to the UHF region below 1 GHz, the curves converge. The atmosphere effects are not as significant as they could be at higher frequencies and greater depths.

ZENITH ABSORPTION FOR THE JUPITER COOL/DENSE ATMOSPHERE

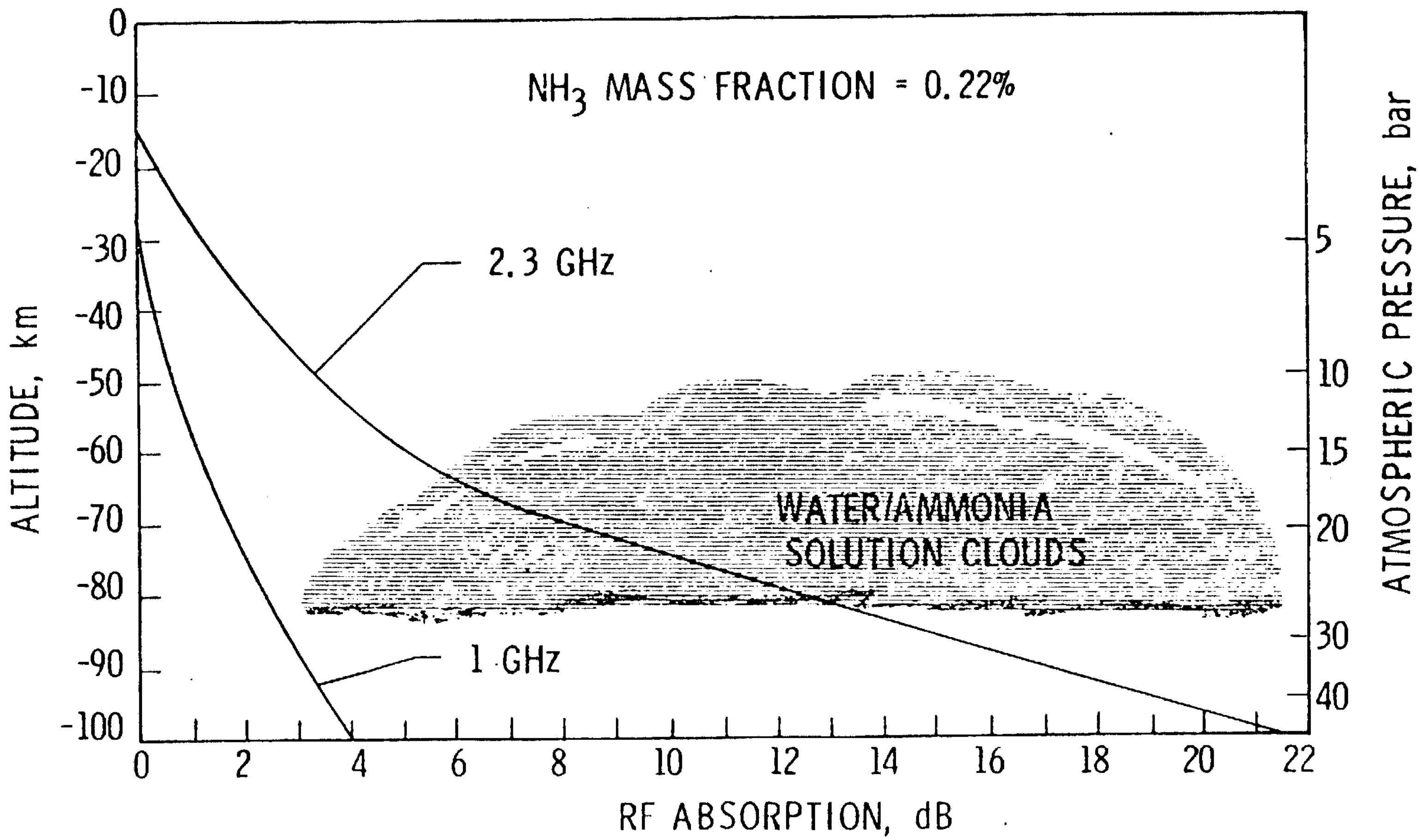


FIGURE 7- 7

ZENITH JOVIAN ATMOSPHERE ABSORPTION

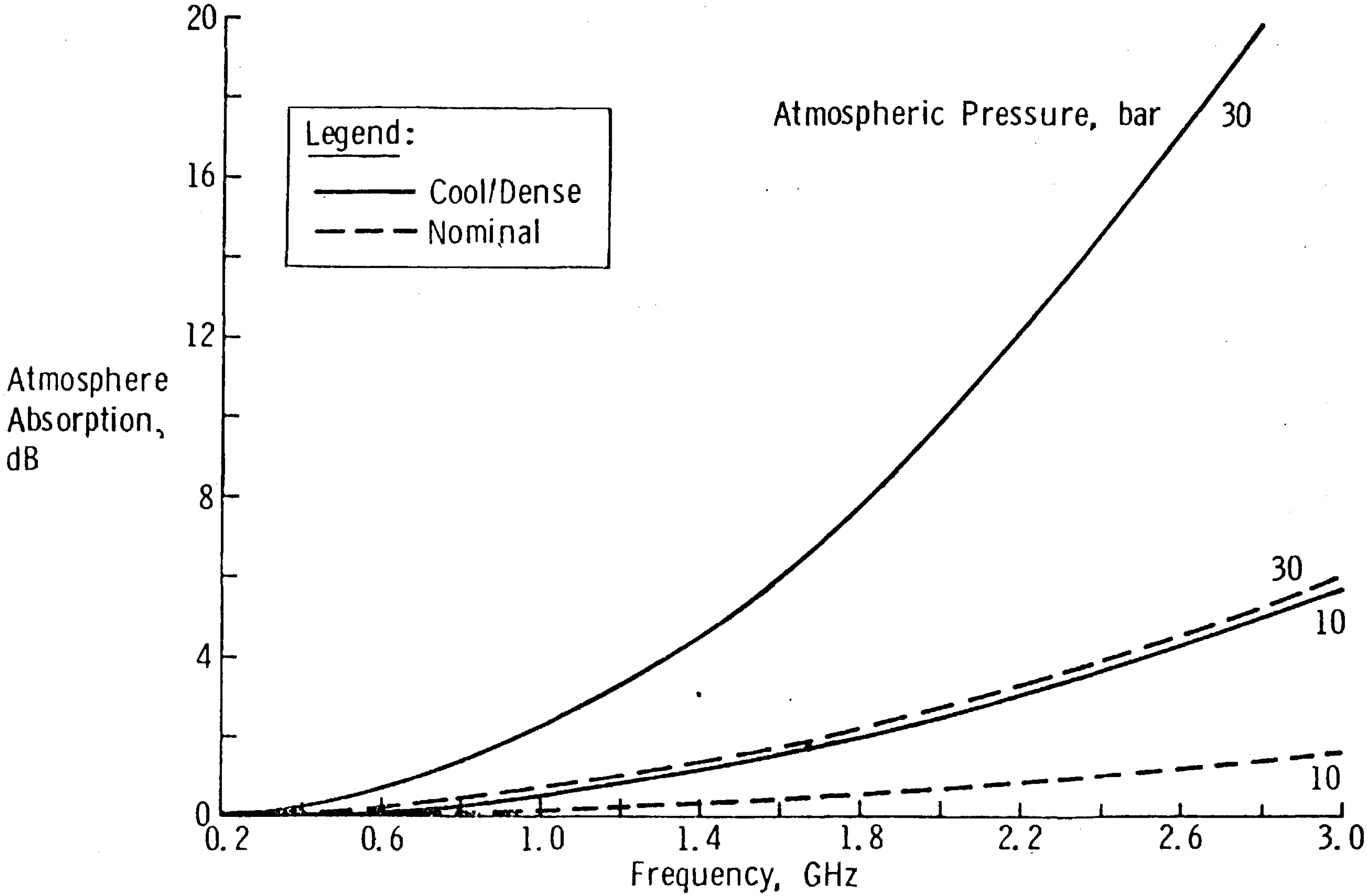


Figure 7-8

Moving to Saturn, Figure 7-9 shows the zenith absorption that is calculated from the worst-case atmosphere, which is the cool model. Again, we are below the ammonia ice cloud and the effects of propagation to the clouds enhances the curves by increasing their slopes. Again, for operating frequencies on the order of 400 MHz and for a depth of 10 bars, we are only talking about 0.5 dB of absorption due to the atmosphere.

A similar condition exists at Uranus, as seen in Figure 7- 10 The worst case is the nominal atmosphere because for the cool model the cloud level is well below 50 bars. Therefore, for a 10-bar probe mission, we have the nominal case and we have also penetrated through the ammonia ice cloud. The RF absorption is less than 0.5 dB for 400 MHz.

Figure 7-11 shows what happens as the probe aspect angle increases. This is strictly the refraction effect that occurs in the atmosphere, and does become quite severe for a probe aspect angle approaching 90 degrees - in other words, if we were propagating out towards the local horizon. For probe aspect angles on the order of 45 degrees or less, refraction losses can be approximated very well by the secant of the angle.

UNIDENTIFIED SPEAKER: Is this a function of frequency?

MR. COMPTON: The defraction effect is not a function of frequency. It is only a function of the probe depth and the probe aspect angle.

Moving on to the next subject of the system noise temperature, Figure 7-12 shows the various thermal noise components of the receiver system that is on the flyby spacecraft. The system noise temperature is a value that is used in the link analysis, and it determines the threshold noise level in the receiver. It is comprised of three components: (1) the antenna noise temperature (T_A), (2) the feed line (T_F), and (3) the front end of the receiver (T_R).

ZENITH ABSORPTION FOR THE SATURN COOL ATMOSPHERE

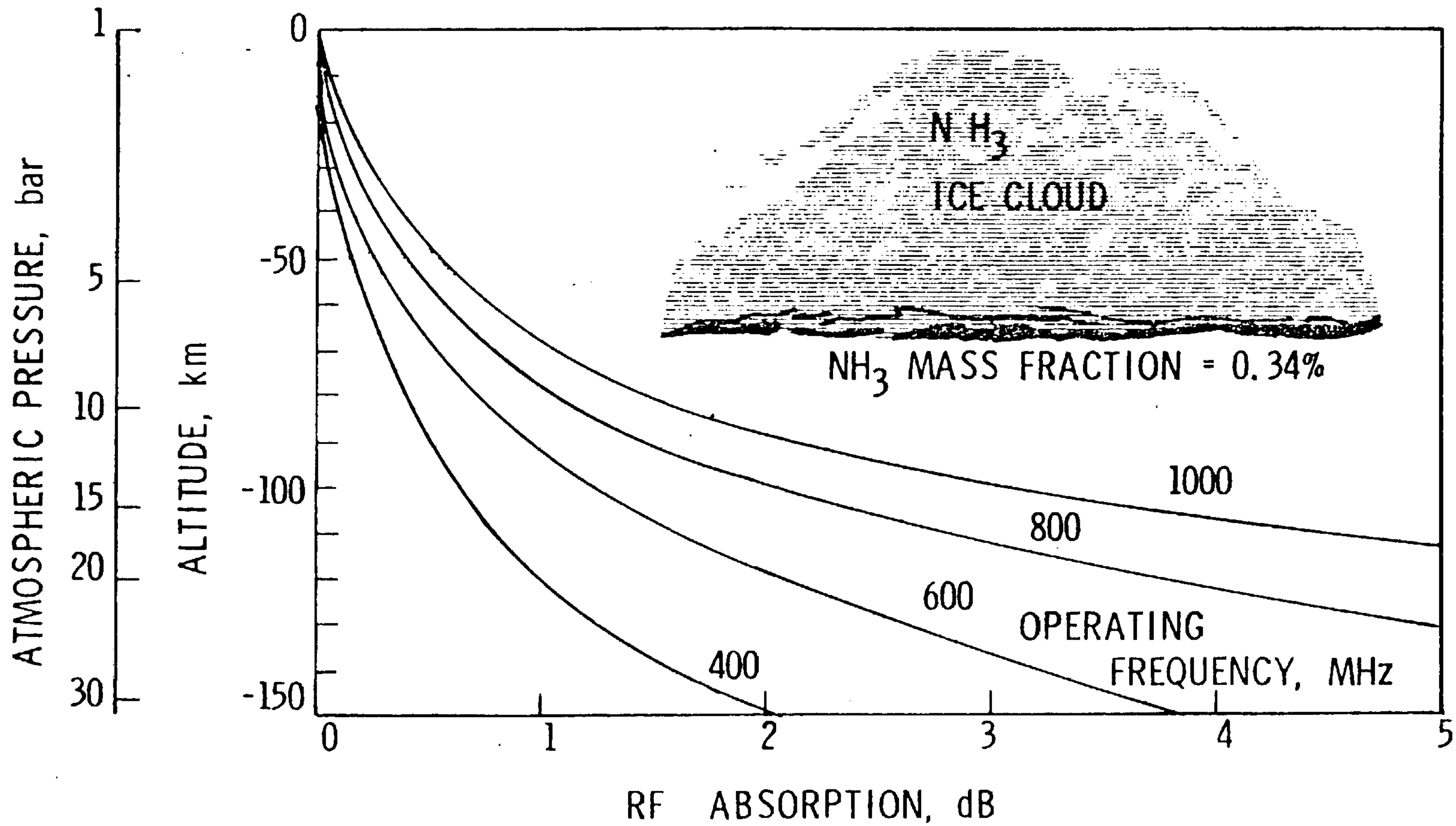


Figure 7-9

ZENITH ABSORPTION FOR THE URANUS NOMINAL ATMOSPHERE

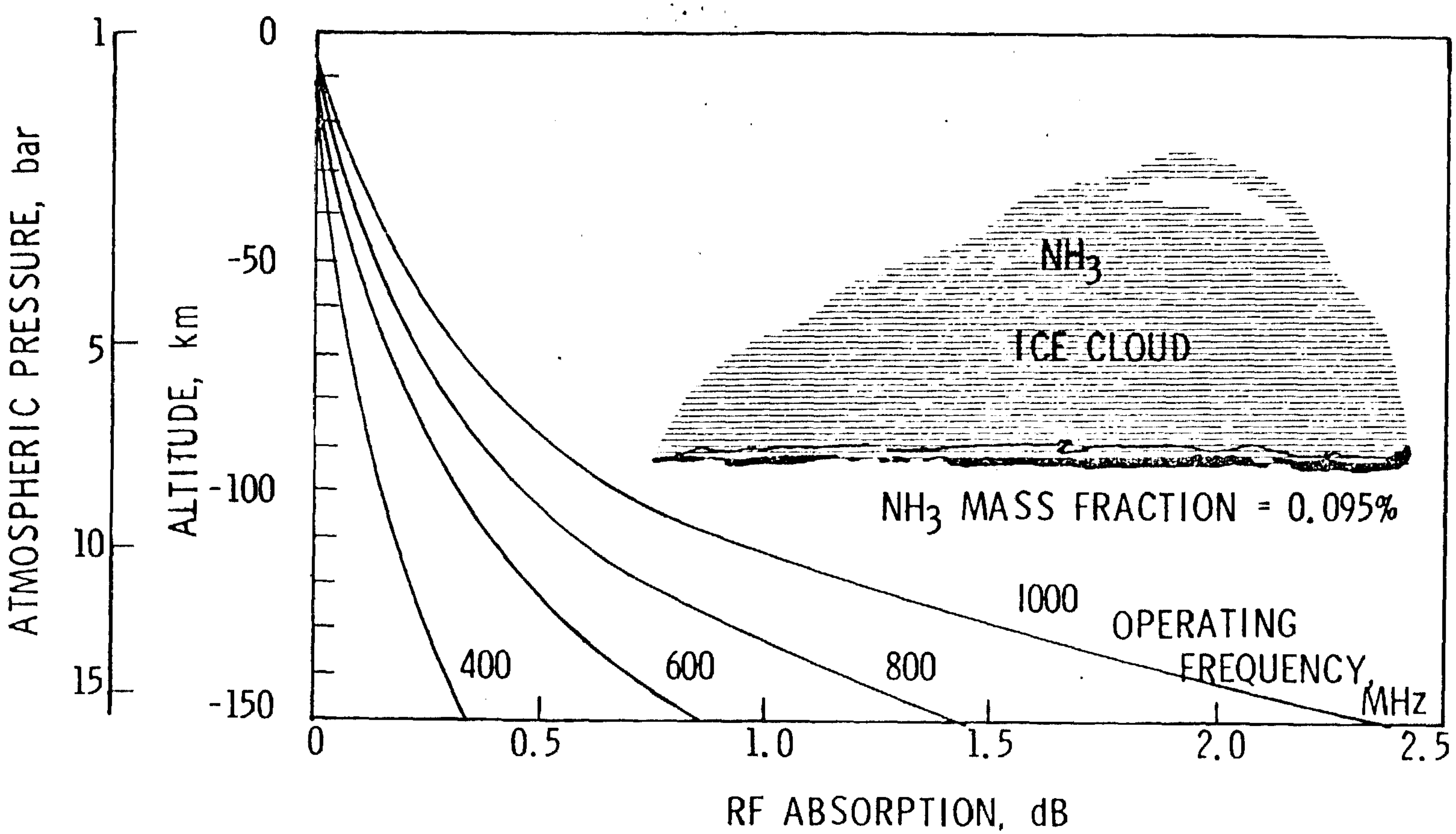


Figure 7-10

DEFOCUSING LOSS FOR JUPITER COOL/DENSE ATMOSPHERE

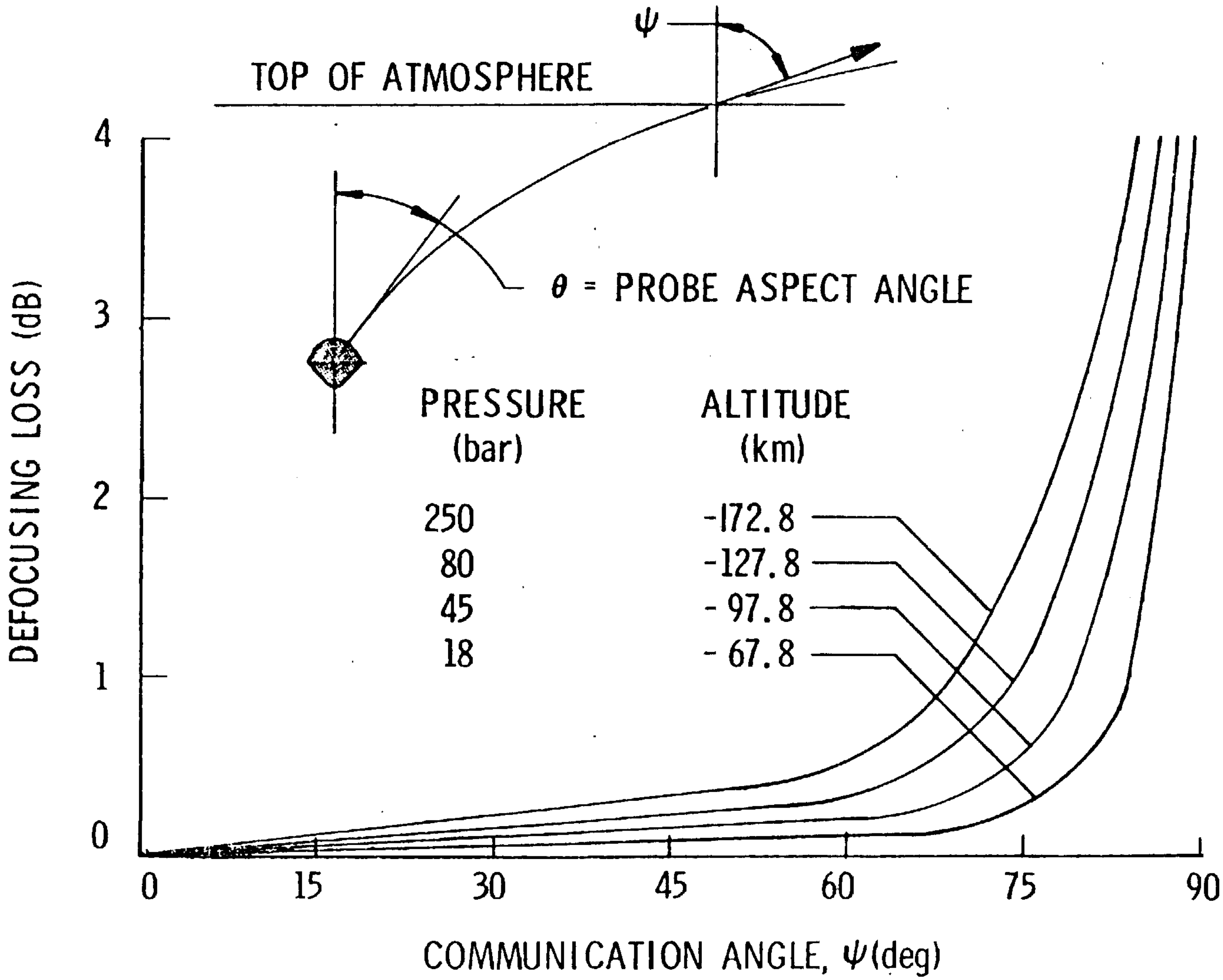
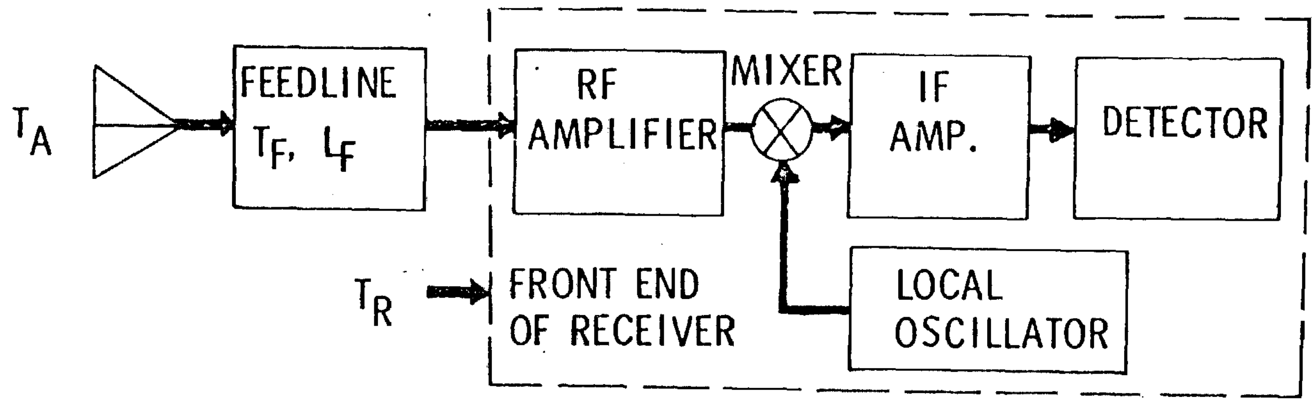


Figure 7-11

THERMAL NOISE COMPONENTS OF THE RELAY RECEIVER



$$T_A = T_G + T_{BS} + T_{BD} \text{ as applicable}$$

$$T_S = T_A + T_F + L_F T_R$$

$$T_F = 290^0 (L_F - 1)$$

Figure 7-12

The antenna noise temperature (T_A) is comprised mainly of three parts, depending upon the type of pattern we have chosen for the antenna that is on the spacecraft. Galactic noise (T_G) is always present in the background of the antenna pattern. We also have the synchrotron brightness temperature (T_{BS}) from the magnetosphere, if one is present at the planet. Jupiter and Saturn have magnetospheres; Uranus does not. We also have the disc brightness temperature (T_{BD}), which is present for all of the planets. So the system noise temperature is the sum of the noise temperatures of the antenna, the feed line, and the front end of the receiver itself.

Figure 7-13 shows typical solid state microwave receivers and their noise figures, which can also be converted to noise temperatures as shown on the right. I averaged the various noise figures for three different types of solid state receivers and the average ranges from 2.5 to 3.0 dB. This corresponds to the receivers noise temperatures shown on the right of the curve that would typically be used for the relay link receiver.

Figure 7-14 shows the synchrotron noise model for Jupiter that is in the present monograph. Also given in the monograph is an equation to calculate the synchrotron noise temperature as a function of the wavelength and distance in the model penetrated by a ray vector. Since this model is a function of the amount of the model that we intercept, it is very dependent upon the type of antenna that is used on the flyby spacecraft and whether or not all the antenna pattern is directed at the planet. If we had an axisymmetric (butterfly) pattern on a Pioneer spacecraft, only a portion of the magnetosphere would be in the antenna beam. So the magnetosphere's influence is different, depending upon the geometry and the antenna pattern shape. The amount of beam which intercepts the model determines how much brightness temperature we have from the magnetosphere. As the mission progresses and we have the probe descending towards the planet, we have primarily the noise coming from the planet disk itself with a small contribution from the magnetosphere. So we can see that the synchrotron

NOISE FIGURE FOR MICROWAVE RECEIVERS

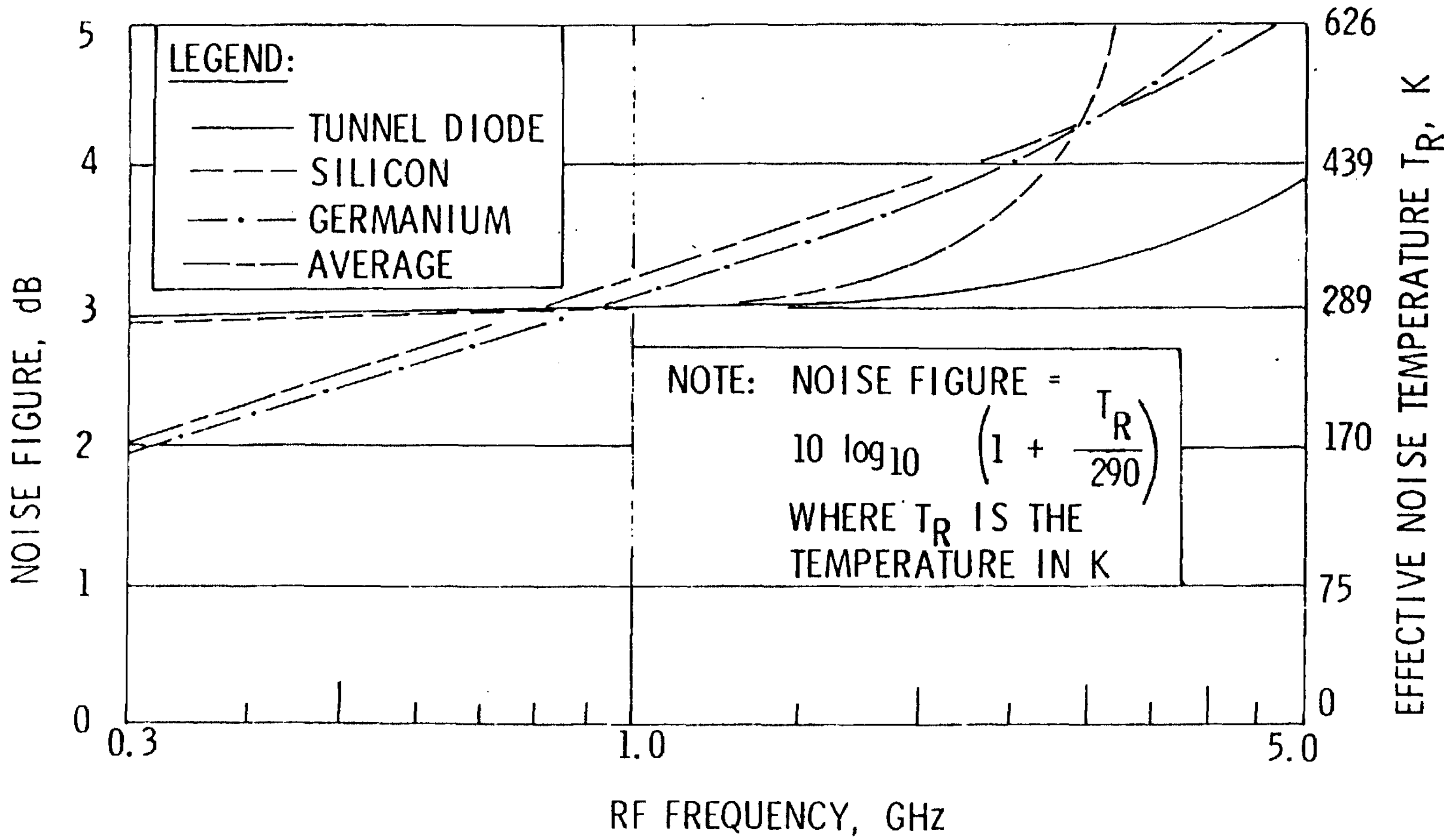


Figure 7-13

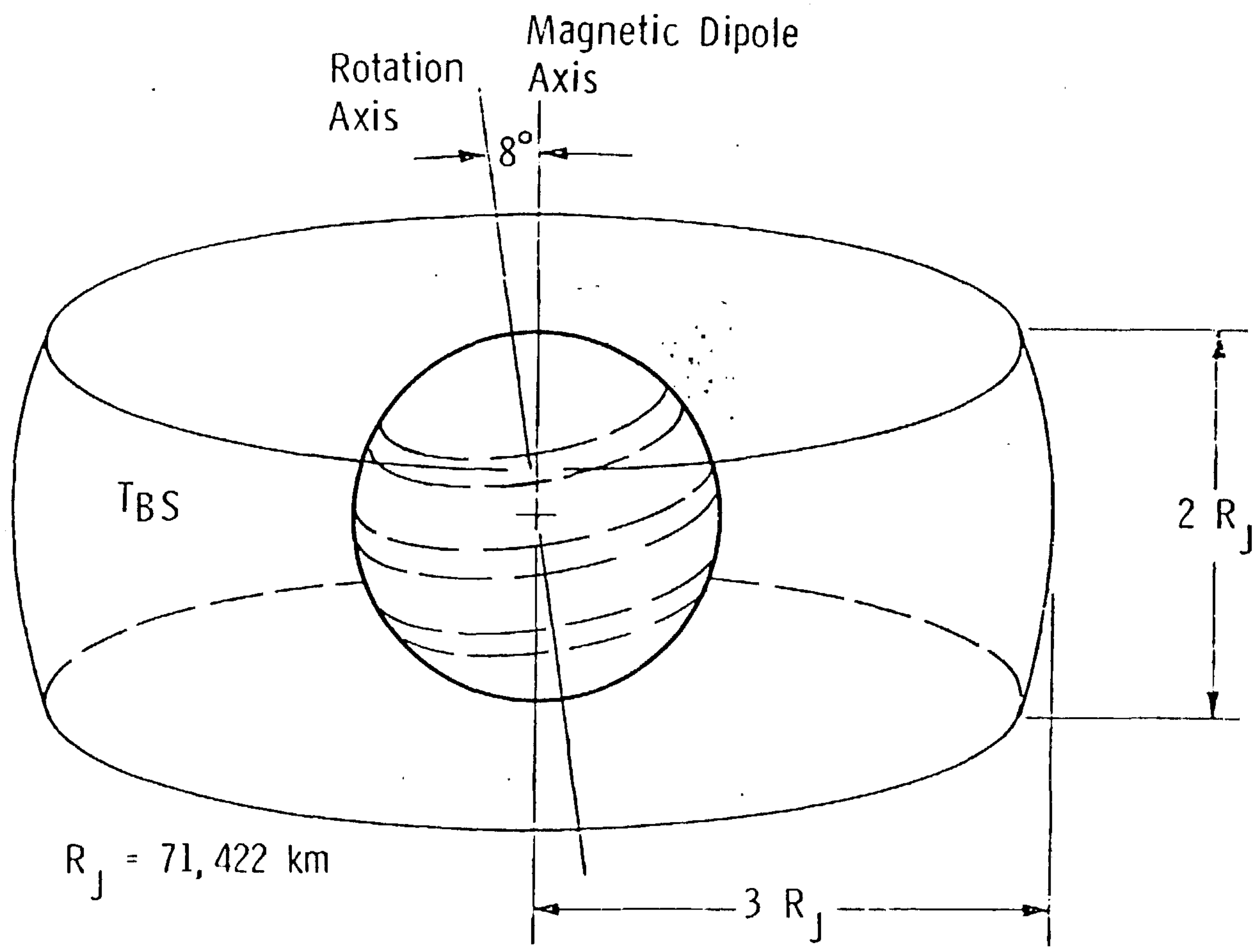


Figure 7-14

noise temperature varies as we progress through the mission from entry to the end of the mission. For Saturn, the noise synchrotron temperature is only a function of the wavelength and we do not have a model like Jupiters. Figure 7-15 shows the disk brightness temperature taken from the Jupiter monograph. The grey areas are the ranges of observed brightness temperatures that have been measured on Earth and the upper limit below 1 GHz is less than 500 kelvin. The upper limit curve was used for the disk temperature in the calculations.

The next three figures are the calculated antenna and system noise temperatures for the three planets of interest. Figure 7-16 shows the noise temperatures for Jupiter. The lower curves show the antenna noise temperatures for two types of antenna patterns, the solid curve being for a dish antenna on a Mariner 3-axis stabilized spacecraft and the dotted curve for a split antenna beam as required by a Pioneer spin-stabilized spacecraft. As seen by the curves, the antenna noise temperature, which is the major contributor to the system noise temperature, and the total system noise temperatures can range above 1,000 kelvin. As seen, the temperatures increase as the frequency is lowered. So this is one parameter that does get worse when lowering the operating frequency. The noise temperature of the system does tend to increase as a result of the planet's influence within the antenna pattern.

Figure 7-17 shows the same calculations for Saturn. The effects are very similar, but they are more pronounced due to the arbitrary equation given in the monograph for Saturn's synchrotron noise. The difference between the antenna noise temperature and the total system temperature is about 1,000 kelvin at 1 GHz.

Figure 7-18 shows Uranus which does not have a synchrotron source of noise. We only have the background galactic noise and the planet disc noise present plus the feedline and receiver noise temperature. All of the temperatures lie below 1,000 kelvin, so

VII-24

RANGES OF OBSERVED JOVIAN DISK BRIGHTNESS TEMPERATURES

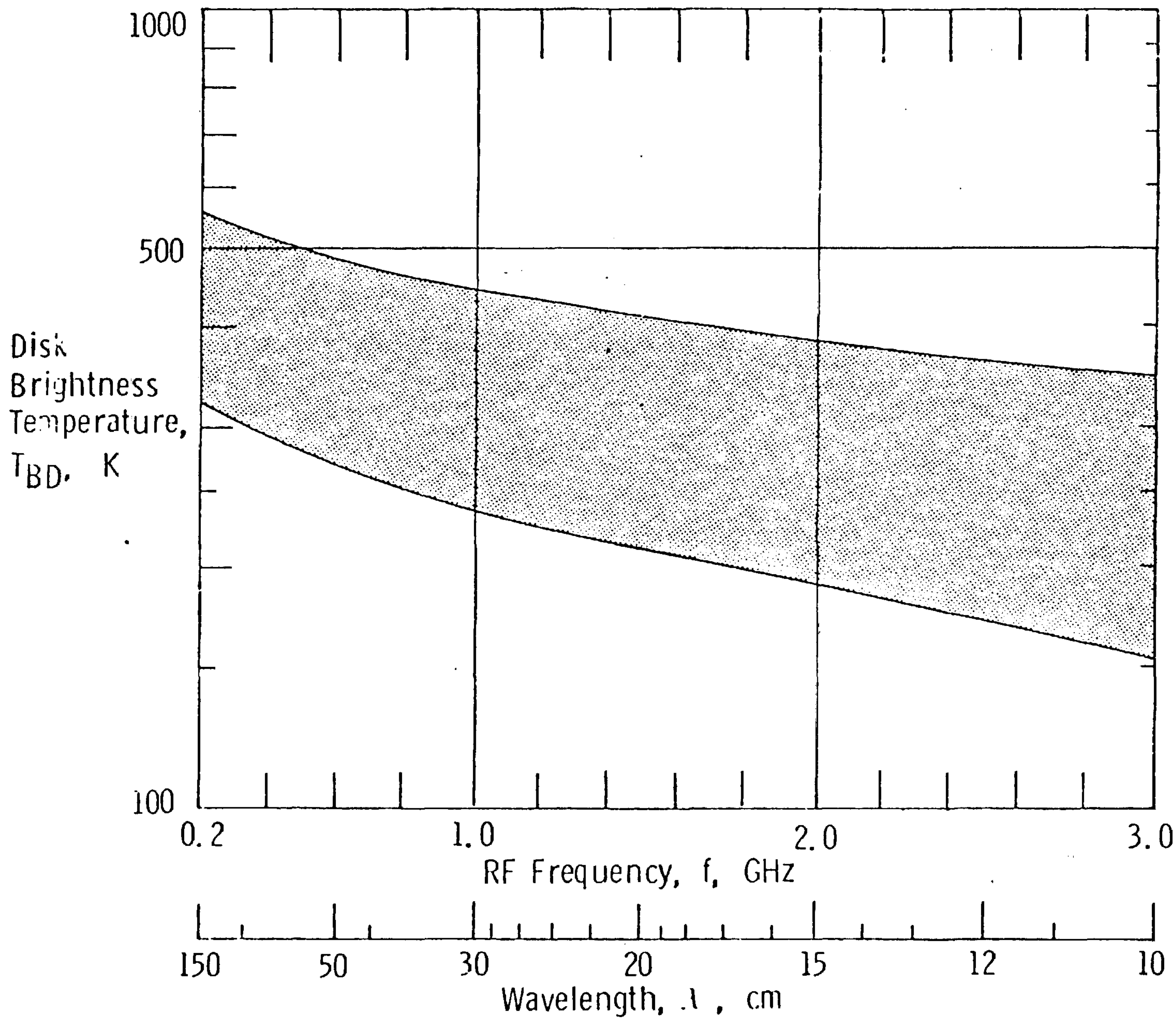


Figure 7-15

NOISE TEMPERATURE FOR JUPITER

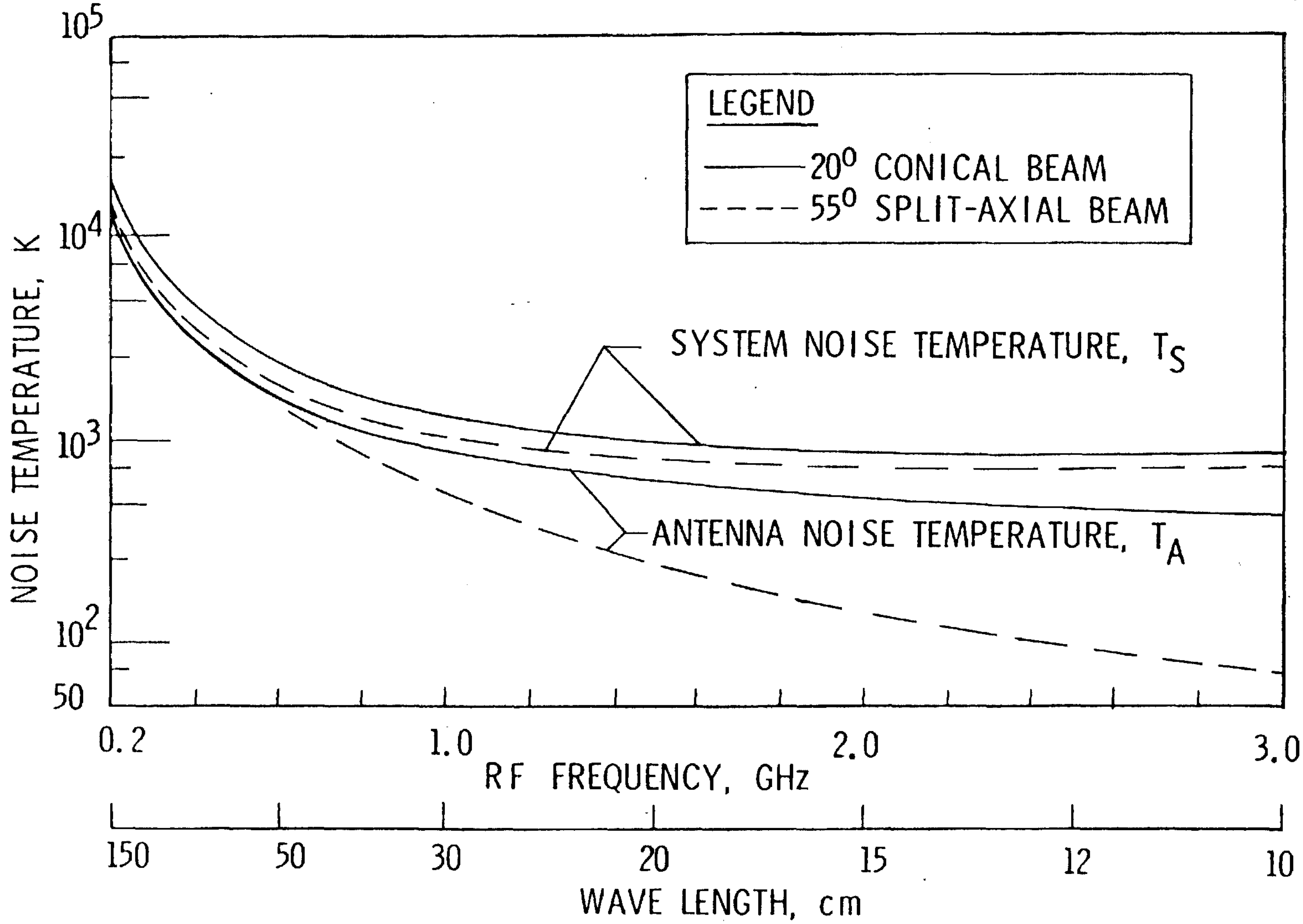


Figure 7-16

NOISE TEMPERATURE FOR SATURN

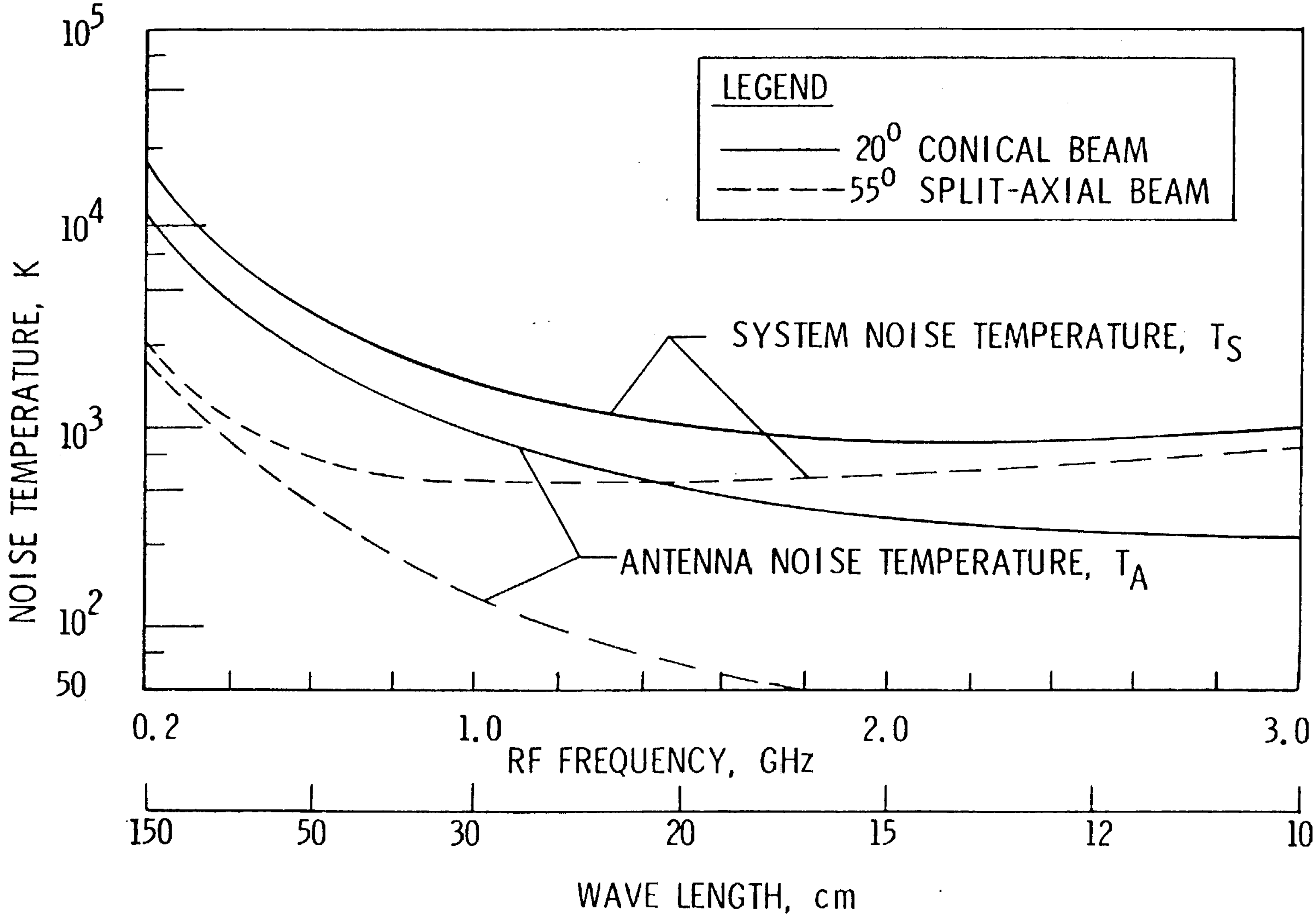


Figure 7-17

NOISE TEMPERATURE FOR URANUS

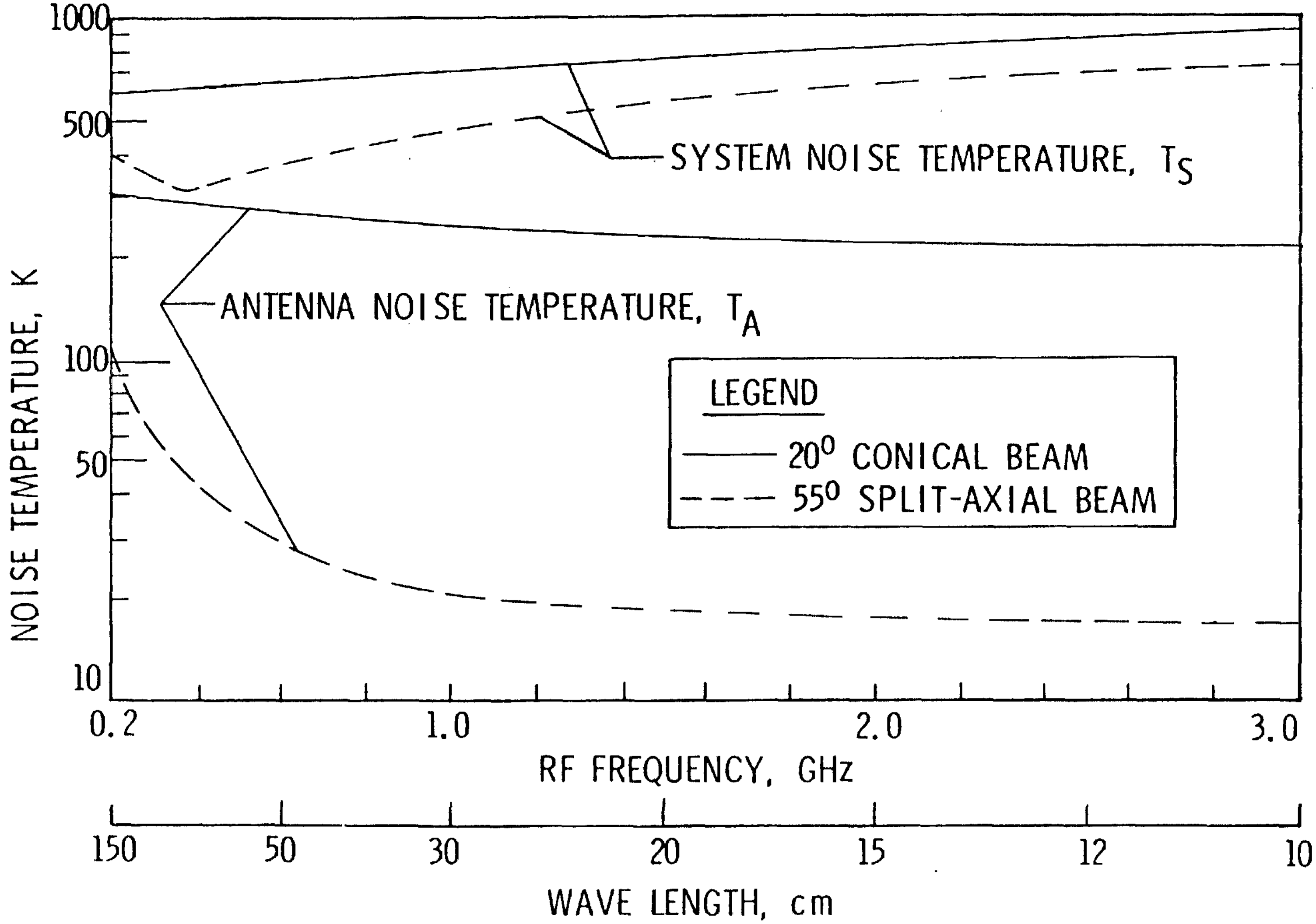


Figure 7-18

the effect is not as predominant as it is for the other two planets. For Uranus the system temperatures generally increase with increasing frequency, in contrast to the curves with negative or zero slope for the other two planets.

Figure 7-19 has some conclusions to outer planet atmosphere propagation. As shown previously, the Jupiter cool/dense atmosphere is the worst-case model and atmosphere absorption can become quite significant and must be considered in determining the effects of propagating through the atmosphere. In order to minimize the atmosphere effects, one should be concerned with keeping the probe aspect angle as small as possible during the mission, the RF frequency as low as practical, and the depth of descent less than 20 bars. The atmosphere losses for Saturn and Uranus are not significant for a typical 10-bar mission using UHF transmission.

Thermal noise in the communication system places a limit on the minimim detectable signal present in the receiver to operate with and the noise effects change as the mission progresses from entry to the end of the mission. Jupiter is the worst of the three planets with its very noisy synchrotron source.

MR. L. FRIEDMAN: I would like to make a comment. I think this analysis shows how a lot of effects vary with the frequency of the transmission; but it assumes antennas of fixed beam width. Actually, your antennas are generally space limited; so I think, if you let the beam width also be a function of frequency and put the whole RF link together, you might get a more realistic picture of how the whole system performance varies with frequency.

MR. COMPTON: Yes, I agree with you. The problem in letting the beam widths vary is that in doing so, you are assuming as the beam widths become more that you are going to somehow track the aspect angle changes.

MR. FRIEDMAN: The beam width can only vary subject to the mission requirements. But you showed 55 and 20 degrees.

OUTER PLANET ATMOSPHERE PROPAGATION RESULTS

- THE JUPITER COOL/DENSE ATMOSPHERE MODEL IS WORST CASE.
- MINIMIZE THE DEPTH, ASPECT ANGLE, AND RF FREQUENCY TO MINIMIZE THE ATMOSPHERE EFFECTS.
- ATMOSPHERE LOSSES AT SATURN OR URANUS ARE LESS THAN 2 dB AT UHF FOR A 10-BAR PROBE MISSION.

- THERMAL NOISE IN THE COMMUNICATION SUBSYSTEM PLACES A LIMIT ON THE MINIMUM DETECTABLE SIGNAL.
- PLANET NOISE EFFECTS CHANGE DURING A PROBE MISSION WITH GEOMETRY AND ANTENNA BEAMWIDTH.
- JUPITER HAS THE LARGEST THERMAL NOISE EFFECT.

Figure 7-19

MR. COMPTON: Right, they were for two entirely different types of antennas and flyby geometries.

MR. GRANT: One question I had, Revis, was that a 20 degree half angle or beam width?

MR. COMPTON: It was a 20 degree beam width antenna.

MR. GRANT: I agree that you might get more insight than we have here, especially for the Mariner, to see how, if you change the beam width, you could come up with a more optimum operating point.

MR. FRIEDMAN: I think that this ultimately ties into battery weight on the probe and the variation of the transmitter efficiency is very small.

MR. COMPTON: That particular trade-off was included in the Saturn Uranus studies that McDonnell covered. Actually, I am not sure if the antenna beam widths were ever factored in as a variable directly with everything else, but, except for Jupiter, the net effect of the noise and the atmospheric attenuation tended to be small over the frequency range that we are considering.

MR. GRANT: Our next speaker is Paul Parsons who is an engineer in the applied communications research group at JPL. He has been working on advance studies related to the Mariner project, and he will speak about data relay design.

DATA LINK RELAY DESIGN

Paul Parsons

Jet Propulsion Laboratory

MR. PAUL PARSONS: We have analyzed the data link for the Ames baseline probe as applied to the MJU spacecraft specifically with an entry at Uranus. I am going to cover four general areas. I will have a few introductory remarks and discuss a bit about the link, look at the effects on the spacecraft and, then, just briefly, touch on the aspects of the two-way link.

We have been studying effects on the link design and what happens to the spacecraft; and, as I said, we are looking at the effects of a two-way link. I will get into the reasons for that in just a moment.

The first thing to look at in this link design is the Frequency Aanlysis. (Figure 7-20). There is a relatively small choise in frequency. You can have UHF or perhaps L-Band. S-Band is conceivable, but it doesn't have very many advantages.

We noted that the atmospheric absorption increases with frequency. The receiver and planet noise increase with frequency. In most cases the planet noise decreases with frequency, or at least levels off, but at Uranus it increases slightly.

We noted that the baseline probe is designed to operate at 400 MegaHertz and we are concerned here with a couple of things: partially, the transmitter, but mainly the antenna pattern. The antenna pattern from this probe is basically that of an open-end wave guide coming back along the longitudinal axis. And the lower frequencies make it a bit easier to get a wider beam width. We will see in a few minutes a wide beam width pattern from the probe is very important.

FREQUENCY ANALYSIS

- ATMOSPHERIC ABSORPTION INCREASES WITH FREQUENCY
- RECEIVER AND PLANET NOISE INCREASE WITH FREQUENCY
- PROBE IS DESIGNED FOR 400 MHZ
- VIKING ORBITER RECEIVER IS BEING BUILT AT 400 MHZ

Figure 7-20

The last major aspect we examined is the Viking orbiter receiver, which is now being built, and is to operate at about 398 MegaHertz. One of the advantages of using this receiver is that all of the EMI work has been done. We know where the interference frequencies will fall, and they will not interfere with the other receiver or with the science; at least the science on the Viking orbiter.

The next major area to get into is the trajectory. There are several parameters here that are of major importance. (Figure 7-21).

The first is the range and shown on the figure in megameters or thousands of kilometers. The first column is the R_U, the periapsis distance in Uranus radii. R_I is the range from the spacecraft to the probe at the entrance into the atmosphere. R_F is the range from the spacecraft to the probe at the termination of transmission.

Notice that at a periapsis of two radii, the range varies from about 95 megameters down to about 38. The 95 megameters correspond to about 184 db path loss at UHF, and you can see that there is about a 5 db change in path loss, reduction in path loss throughout the life of the probe.

We also looked at the case of 1.1 radii, which is perhaps better from a celestial mechanics view point. They get closer to the planet and perhaps a little more sensitivity to some of the J factors in the expansion of the gravity field, but the range is quite short there. The disadvantage of that and the reason I did not show it is there is such a range of cone angles on the spacecraft that we should be very hard pressed to follow it with the antenna.

The second factor in trajectory parameters is the track on the spacecraft. This is the track that the probe would trace out as

TRAJECTORY PARAMETERS

- RANGE Mm

R_U	R_I	R_F
2.	94.5	38.5
3.5	111.	76.6

- S/C TRACK

R_U	$CONE/CLOCK_I$	$CONE/CLOCK_F$
2.	151/277	84/252
3.5	131/271	84/258

- ANGLE FROM PROBE AXIS

R_U	I	F
2.	15	46
3.5	34	42

Figure 7-21

it enters the atmosphere. Figure 7-21 has this listed in cone and clock. For those of you who are not familiar with this system, it is a coordinate system on the spacecraft in which two coordinates describe the entire sphere. Zero degrees cone would be pointed at Earth, and right at encounter the planet would be about 90 degrees cone. Prior to that, it would be close to 180. Clock is measured from the South celestial pole, or Canopus, clockwise, looking at Earth. So you can see that for the two R_U entry, we are looking just a little below horizontal. If it were over 270, it would be horizontal looking off toward the right; it would be 7 degrees below that and at the end of this would be a 252, which would mean we had moved up a bit.

The cone angle starts about 150, which is near the antisolar point, and goes to just a little bit on the sun side of the 90-degree point.

It is interesting to note that the latter portion of the entry is closer to what might be considered the equator of the spacecraft, if you consider the cone the pole. And this has quite an effect on the antenna pattern that we would develop.

If we were to go at 1.1 R_U, we would wind up with a final cone angle of about 50 degrees. That would be on the other side of the 12-foot antenna which would make it a little difficult for the relay antenna to follow it in.

The most important difference here in these flyby periapses is the angle from the probe axis. Now I have said this probe antenna pattern has a maximum on the longitudinal axis and falls off fairly slowly, and at 50 degrees I believe it is down to about 0 dB.

We see on Figure 7-21 that the two R_U case starts out at about 15 degrees which is very good, and winds up at about 46 as a final angle from the axis, which is not too bad. The 3.5 case starts out at about 34 and winds up at 42. In neither of these two cases is the change anything like monotonic. It gets down to a minimum of about 8 degrees in one case, and I believe 12 in the other. It does not exceed 46 for the 2 R_U case or 42 for the 3.5 R_U.

Because of this variation, we do want to keep the antenna beam width as wide as possible; and also this would accommodate any oscillations that will occur in the spacecraft due to the dynamics of entry.

Figure 7-22 covers the dispersion of the probe. It is easy to get shot down on the subject of dispersions, because there are so many different factors entering it. In this case, we have assumed that the Uranus ephemeris has been improved to be more in line with the knowledge of the ephemeris of Jupiter and Saturn. Right now the ephemeris is more unknown or known to a lesser degree. If we have to live with the ephemeris as it stands now, I am afraid our dispersion would be much worse and we would have to revise our analysis.

The entry dispersion analysis I have done so far assumes that the only error is in entry angle. We have assumed a nominal 40 degree entry angle, and we have looked at the difference in parameters that you get with a 30 and a 50 degree entry angle. As you might expect, the 30 and 50 degree entry angles move most of these parameters in opposite directions.

The range will vary by a maximum of five megameters from the nominal case of 40 degrees entry, which would amount to approximately 0.5 db, path loss, which is negligible. It will move the probe trace on the spacecraft by a maximum of three degrees, which is a small amount. However, it can affect the probe axis angle by ten degrees. The angle off the axis can get up to around 55 degrees or so. At this angle, we have not only reached

DISPERSION

- ASSUMES IMPROVED URANUS EPHEMERIS
- ASSUMES ONLY ERROR IS IN ENTRY ANGLE
- EFFECTS

RANGE	5 Mm
S/C ANGLE	3°
PROBE AXIS ANGLE	10°

Figure 7-22

a region of decreased gain, we have reached a region of some lobing in the pattern. This will, obviously, give you some scintillation of the received signal, something we would rather avoid.

That pretty well covers what we have done on the trajectory analysis. I would like to go into the spacecraft design, Figure 7-23.

The required view region comes directly from the probe trace on the spacecraft, and we see that it covers a region of roughly 30 degrees by 80 degrees. Now that 80 degrees is in cone. This is a fairly narrow trace going along what we consider the 270 degree longitude line. The required gain is about 6 db. Most of this is concentrated at the initial portion of the pattern, which is around 150 degrees cone.

The receiver we see is a modification of the Viking orbiter receiver to include AFC because of the requirements of tracking the dynamics of the frequency as required by the low data rates. In detection, of course, we see a detector, some sort of symbol synchronizer, and we see probably a decoder being built into the spacecraft. The probe would have convolutional encoding and we would expect that we would decode that and send just the bits down rather than the entire symbols.

I would like to just touch very briefly on the two-way considerations (Figure 7-24) and show a block diagram (Figure 7-25). The reason for thinking about two-way is that it could provide Doppler data if we could find some way of breaking this off out of the receiver, that could give some scientific data and perhaps something about the atmosphere on entry.

The problems are two phase locked loops cascaded and you are going to have some noise, additional noise that you would not have normally. The real big problem is in acquisition, and

SPACECRAFT DESIGN

- ANTENNA

  VIEW REGION ~ $30^{\circ} \times 80^{\circ}$

  REQUIRED GAIN ~ 6 DB

- RECEIVER

  MODIFICATION OF V. O. TO INCLUDE AFC

- DETECTION

  DETECTOR

  BIT SYNC

  DECODER

Figure 7-23

TWO-WAY CONSIDERATIONS

- COULD PROVIDE DOPPLER DATA
- PROBLEMS
  - NOISE FROM CASCADED LOOPS
  - ACQUISITION
  - TRACKING

Figure 7-24

TWO WAY DIAGRAM

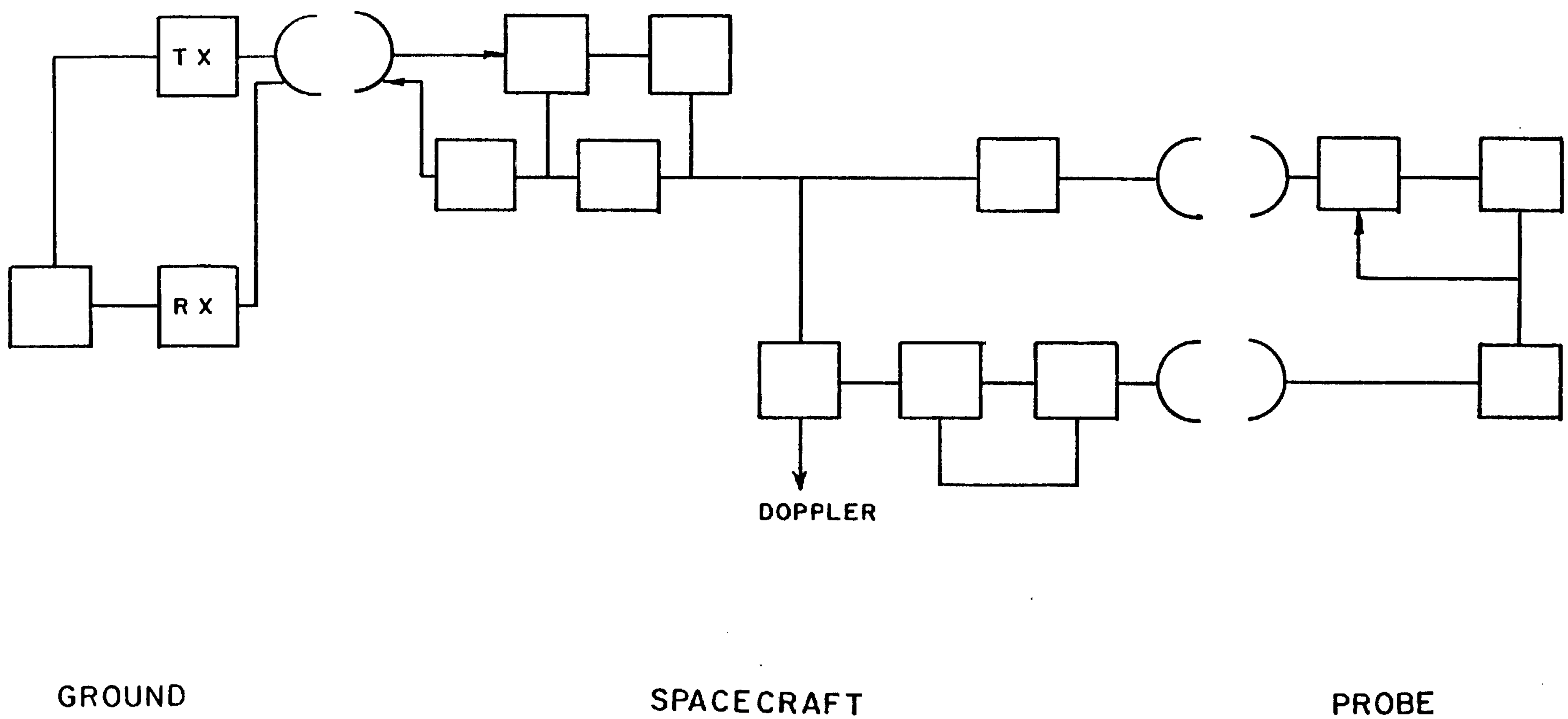


Figure 7-25

there is some problem in tracking and re-acquiring.

On the block diagram, Figure 7-25, we have the normal link with the spacecraft, the ground transmitter out through a phase lock loop, a multiplier, the down link receiver, and the Doppler extractor.

On the probe we have to have a different multiplier out to the second antenna to the probe. The probe would lock up to the received signal, then another offset - transmitting a slightly offset signal back to the spacecraft. The spacecraft would now have to lock up to this signal from the probe and then there would be a Doppler extraction and this would have to be read out and sent down on a telemetry link.

You see we have complicated the relay link greatly. Instead of a simple transmitter on the probe, we now have a transponder that has to lock to the signal from the spacecraft, and instead of a simple receiver on the spacecraft, we now have to have another phase lock receiver.

We are quite concerned about the two-way acquisition aspects of this. Thank you.

MR. GRANT: The next speaker is Mr. Carl Hinrichs, senior engineer at the McDonnell-Douglas Corporation. Mr. Hinrichs will report on a digital receiver simulation study recently concluded at MDAC.

N75 20396

DIGITAL RECEIVER SIMULATION

Mr. Carl Hinrichs - McDonnell-Douglas Corporation

MR. HINRICHS: The simulation is summarized on Figure 7-26 and was for the Saturn-Uranus design that you have heard so much about in the last day and a half. This design is 40 watt, 400 MegaHertz, 44 bit-a-second link and, as has been pointed out, is a power starved link and uses convolution coding. As far as the simulation itself goes, parameters such as the power level, the bit rate, and the range are relatively insignificant. These are taken into the simulated signal-energy-to-noise-density ratios. The center frequency is, in the simulation, relatively unimportant because the simulation is entirely in complex amplitude so that the center frequency is just a normalization.

As was pointed out, we were interested in encoding this link and this is one of the reasons that the simulation became particularly attractive. For convolutional codes we do not have to concern ourselves with some typical simulation problems such as very low symbol error rates. We will be looking primarily for symbol error rates that are around .05. And if we get down to .01 or .001, this is very solid for the code. This makes simulation quite attractive.

Fine, it is attractive but why simulate this particular link? As we have heard from the previous speakers, this link has several unique aspects. First of all, atmospheric scintillation. We are in an atmosphere here today, we transmit radio waves back and forth, why don't we have that problem? Well, primarily because we are not at a ten or thirty bar level. We are only in a one bar level here. If the pressure were higher, we would start seeing scintillation problems.

Secondly, the center frequency certainly enters into this, our Doppler to data rate ratio is very high. What I mean by this is that relative to the bandwidth of the data, the

DIGITAL RECEIVER SIMULATION

TYPICAL LINK
- 40 WATTS
- 400 MHz
- 44 BPS
- 10^5 km RANGE
- CONVOLUTIONAL CODE

UNIQUE ASPECTS
- ATMOSPHERIC SCINTILLATION
- DOPPLER/DATA RATE $\approx$ 450

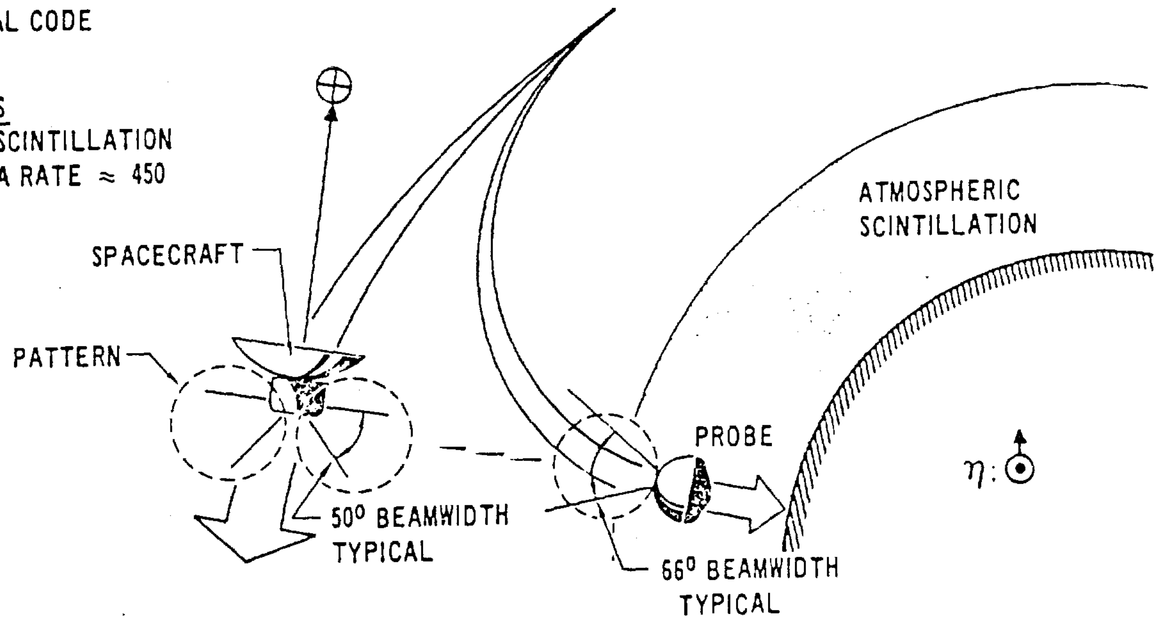


Figure 7-26

frequency uncertainties due the Doppler are quite wide. So we have a unique aspect in this sense. Because of the uniqueness of the link, the unique problems and because we are only looking for fairly high symbol error rates as opposed to an uncoded system, simulation appears to be a good technique to determine the applicability of candidate designs.

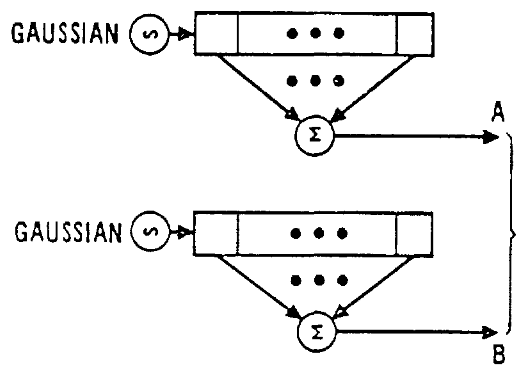
Now in the next chart, (Figure 7-27), I would like to review a little bit about atmospheric scintillation. Sometimes we tend to say that these problems are non-analytic. Certainly in the past, there have been a number of articles, at least that I am familiar with, that deal with fading. In the bulk of the fading articles, the amplitude is generally considered Raleigh or Ricean and the phase is assumed to be uniform. In atmospheric scintillation, neither of these is necessarily the case.

Atmospheric scintillation arises when one has a blob, as it is called in the literature, of atmosphere with an index of refraction slightly different from the remaining atmosphere. This blob may have been generated in a number of ways but generally, it is some form of thermal instability that creates it. The blob is unstable and breaks into smaller blobs. The smaller blobs continually break until the Reynolds number is finally sufficient and it can dissipate. So there is a range of inhomogeneities in the index of refraction.

As an electromagnetic wave passes through this range of inhomogeneities, the larger inhomogeneities tend to affect the phase of the signal and the smaller inhomogeneities tend to affect the amplitude of the signal. Thus, we see the amplitude in the phase characteristics of the signal are independent.

As Mr. Grant pointed out, for this simulation we have modeled the scintillation amplitude as some value A, with a 4/3rds foll off

SCINTILLATION FILTERING



AMPLITUDE: $A\,(1/(1 + j\,f/f_A))^{4/3}$

$\mu_A = 0;\ \sigma_A = 0.23;\ f_A = 2.$

PHASE: $B\,(1/(1 + j\,f/f_B))^{4/3}$

$\mu_B = 0;\ \sigma_B = 0.47;\ f_B = 0.2$

Figure 7-27

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at a corner of two Hertz. This amplitude is modeled, in this case, as having a zero mean and a root variance of .23.

The phase, the other independent variable, again, has a 4/3rds filter roll off. Four-thirds is basically from the Russian Tatarski. The phase has, again, a zero mean and a root variance of .47 radians and rolls off at a much lower corner, 2/10ths of a Hertz.

Typically, in digital simulations, we like to use Z transforms but as one can fairly readily show, when one has a non-integer number of poles, the Z transform series doesn't collapse into a closed form. So we spent a fair amount of effort in modeling the exact characteristics of the scintillation in terms of tapped delay lines. We took independent Gaussian numbers and ran them through the delay lines to form the amplitude and the phase. For this simulation we modeled the amplitude as simply unity plus the Gaussian number. A better simulation might utilize a log normal.

Given the problem, we need a candidate design. In the first portion of the Saturn-Uranus study, TRW supported McDonnell Douglas in defining the hardware impacts of various candidate system designs. In the latter portion of the study, they took the resultant system design and performed a detailed receiver design. That receiver design is shown on Figure 7-28.

In the receiver, the lower loop is the frequency tracking loop. This loop tracks the tones of the transmitter. It is a continuous phase, FSK transmitter. The upper loop is the automatic gain control loop which serves to hold the voltage for the AFC loop at a constant value. The automatic gain control loop provides a signal strength indication from the coherent amplitude detector. If it is not locked to the signal, it can initiate the sweep circuitry.

CANDIDATE DESIGN (TRW)

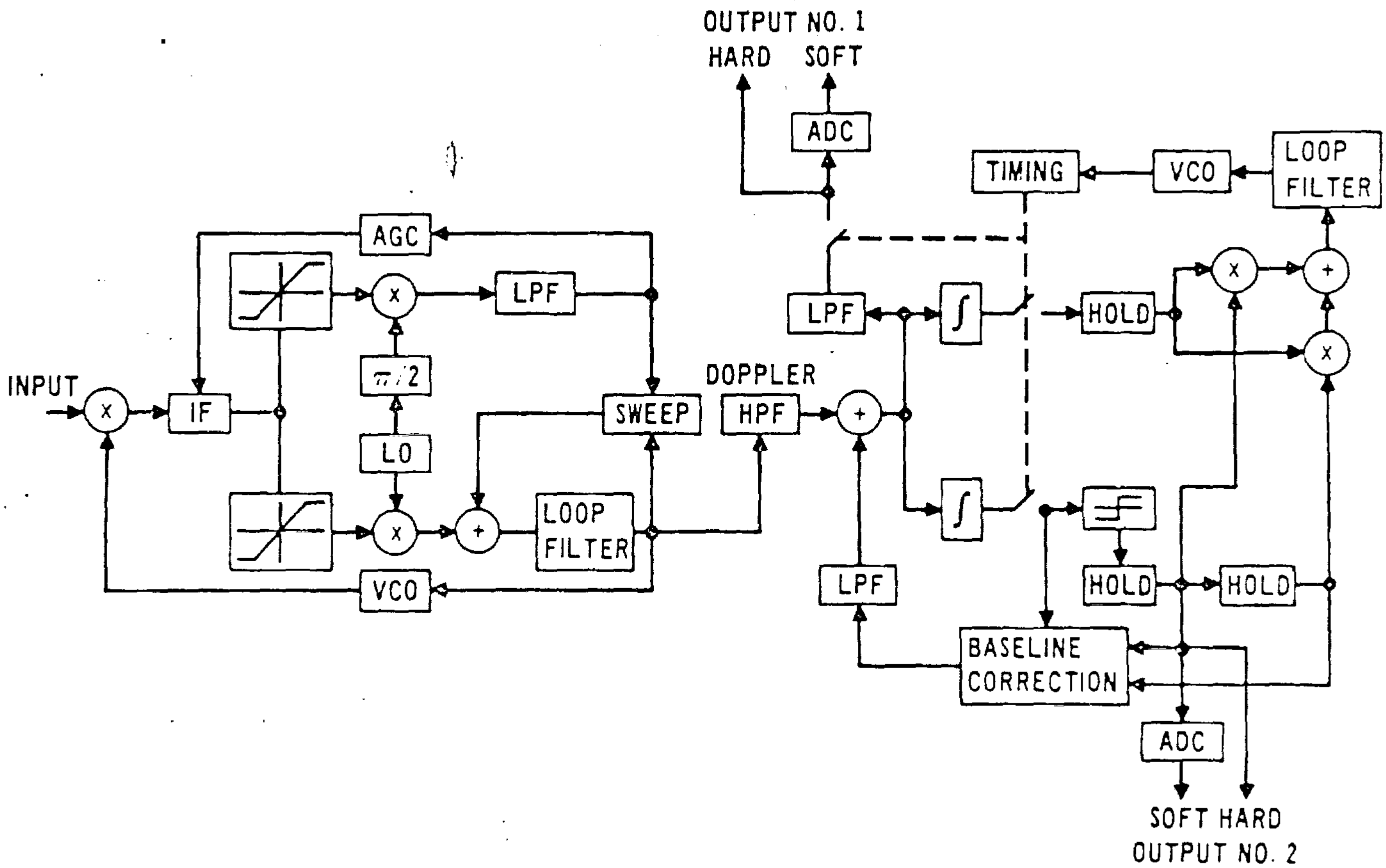

Figure 7-28

The signal feed back from the tracking loop filter indicates when it may have gone beyond the specified sweep or anticipated Doppler range. It will then reverse the sweep direction.

The bit synchronizer, is a relatively straightforward in-phase, quadrature phase, bit synchronizer. It has a baseline correction circuit to correct for "drifts," i.e., long successive strings of either plus ones or minus ones.

Fairly early in the simulation efforts, it appeared that it would be easy in the simulation, since the bulk of the work in a digital simulation is in the receiver (relatively little of the work in terms of computing time takes place in the bit synchronizer) to look at two different type of detectors: a sampled filter detector and the in-phase integrator (as a detector). For both of these detectors, we look at both a hard decision; (that is either a plus or minus one) or soft decision (the relative level of confidence of a level). This is the candidate design that we have investigated.

This chart (Figure 7-29) represents an abbreviated computer flow diagram. We actually generated two routines, one for the error rate and one for the acquisition. Unfortunately, we never got a set of curves of the acquisition probabilities as every time we tried to acquire, we did. Perhaps if we go lower in E/No (we only went down to 7 db) we could start to define the curve. Above 7 db, the receiver acquired every time.

Basically, in the computer flow after initializing the problem, we may or may not step the scintillation. We are taking approximately 40 samples per bit in the simulation. Because the scintillations are only two Hertz and 2/10ths of a Hertz compared to 88 symbols per second, it was not necessary to step the scintillation lines every time that we stepped a sample for a bit. Thus we saved some time here. The simulation data is a 63 bit PN sequence.

ERROR ROUTINE FLOW CHART

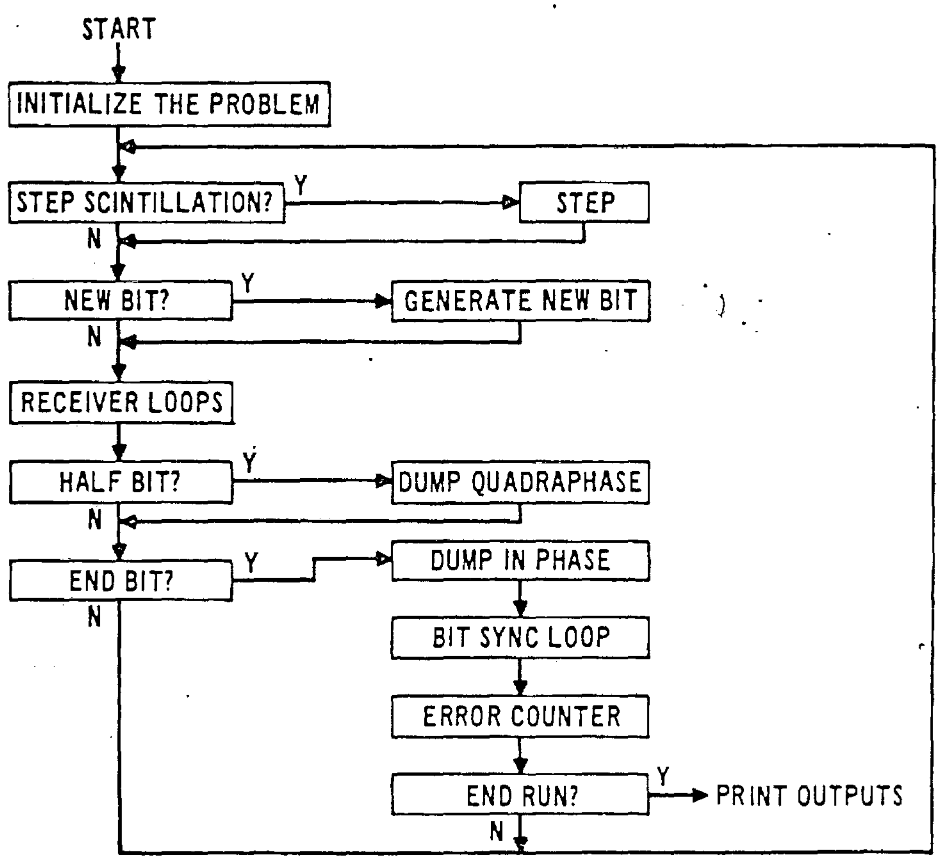

Figure 7-29

One of the interesting things in the simulation was that we simulated to the lowest feasible component in the receiver. Each filter in the receiver, the band-pass filters, the tracking filters, were individual Z transforms, the gain constant of the VCO's were independently variable; each multiplier occurred (the front end of the receiver) as a complex amplitude multiplication.

We ran some interesting parametrics, Figure 7-30. We looked at varying the modulation index and, the old 7/10ths modulation index still holds good. The initial design was for a 1,000 Hertz IF. It looks like slightly larger IF's might be more advantageous. In the future we will be looking at 1,500 or 2,000 Hertz. The IF has to be wide enough so that there won't be any phase distortion in the receiver; but if it is very wide, it is not necessary. One curious thing that we discovered was that the dynamic range of the automatic gain control could be increased somewhat. By this, the AGC tries to keep the voltage level to the AFC loop constant. What one normally does in a design is when the signal hits the threshold, the gain stops. If the gain were a bit greater, the performance improves.

Finally, looking at the two different types of detectors, in all of the runs that we made, the integrator detector - that is, the in-phase integrator in the bit synchronizer, out performed the sample filter detector. It appears that the integrator detector is the best design.

One of the things that we always like to look at is error rate. The No scintillation and scintillation data shown here are compared to the original specification which was an FSC BT=2 receiver. The candidate design is performing well within that bound.

In conclusion (Figure 7-31), in terms of the mean error rate, this is an acceptable design. However, when considering convolution

PARAMETRICS

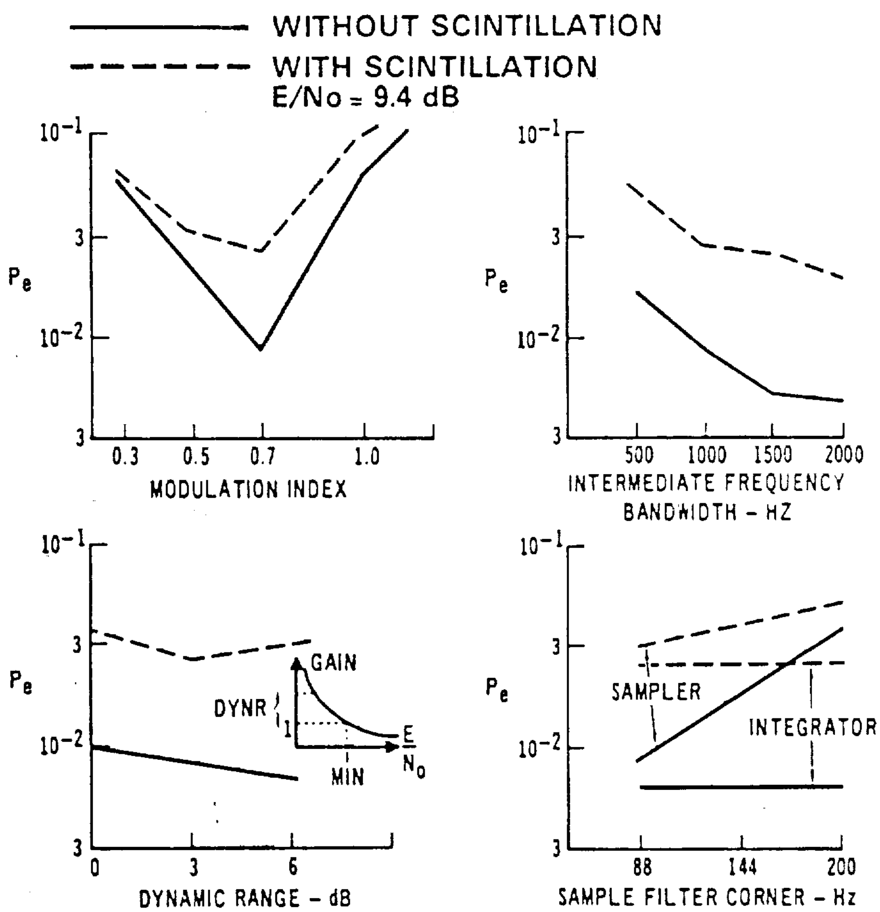

Figure 7-30

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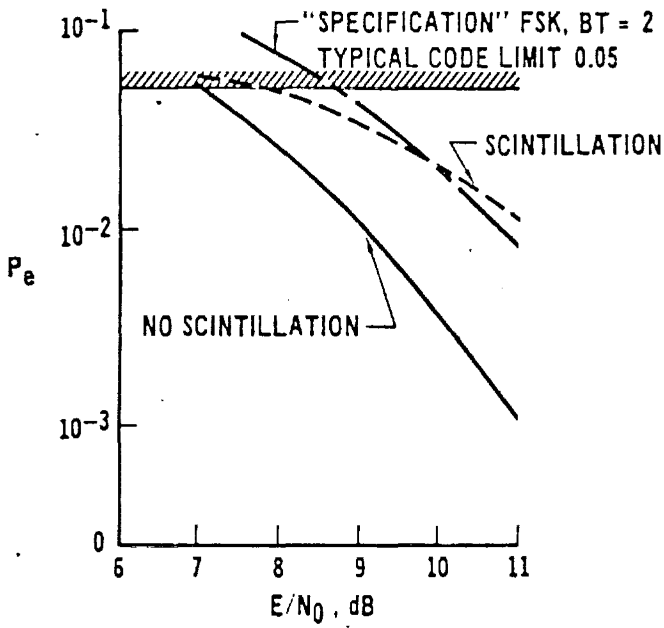
CONCLUSIONS



CONCLUSIONS –

- CANDIDATE ACCEPTABLE FROM THE MEAN ERROR RATE VIEWPOINT
- COMPLETE VERIFICATION AWAITS ARC CODING ANALYSIS

RECOMMENDATIONS –

- INTERMEDIATE FREQUENCY BANDWIDTH ≈ 1500 Hz
- AUTOMATIC GAIN CONTROL DYNAMIC RANGE ≈ 3 dB BELOW " MINIMUM SIGNAL"
- USAGE OF INTEGRATION RATHER THAN SAMPLING FILTER DETECTOR
- FURTHER EXPLORATION OF SCINTILLATION MODEL EFFECTS

Figure 7-31

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codes, the mean error rate is only one of the criteria. The code is sensitive to not only the distribution of errors but the actual pattern of the errors. At the conclusion of this study, we cut magnetic tapes for ARC to analyze for different coding algorithms. The tape records the different detector performance via soft decisions.

We recommend an IF frequency a little bit greater than 1,000 Hertz; an AGC something below the usual definable minimum signal, and integration detector rather than a sample filter detector, and now that we have the tools available to us, investigate a variety of scintillation models.

Thank you.

MR. GRANT: Our next speaker is Dr. James Modestino, Associate Professor in the Systems Engineering Division at Rensselaer Polytechnic Institute. Dr. Modestino will report on convolutional code performance in fading channels.

N75 20397

CONVOLUTIONAL CODE PERFORMANCE IN PLANETARY ENTRY CHANNELS

Dr. James Modestino
Rensselaer Polytechnic Institute

DR. MODESTINO: I would like to spend some time this afternoon talking a little bit about the performance of convolutional codes in a fading channel which would be typical of a planetary entry mission. What I would like to talk about in particular is but one small aspect of some on-going work that is being conducted at RPI under NASA support. I might say at the outset that the primary motivation underlying our work has been in support of Pioneer-Venus, although we do expect that the results have much more general application to the planetary entry mission in general.

In the first table (Table 7-1), I have indicated some of the tasks that have recently been completed. The first task has been the modeling of the planetary entry channel for communication purposes. Here, we are primarily interested in representing the scintillation or the turbulent atmospheric scattering effects experienced on a planetary entry channel. A second task has been the investigation of the performance of short constraint length convolutional codes in conjunction with coherent BPSK modulation and Viterbi maximum liklihood decoding. The third task has been the investigation of the performance of selected long constraint length convolutional codes in conjunction with, again, coherent BPSK modulation but now sequential decoding. We have been looking at both the Fano and the Jelinek algorithms for sequential decoding. Our interest here has primarily been in the computation and/or storage requirements as a function of the fading channel parameters. Finally, we have been concerned with the comparison of the performance of the coded coherent BPSK system with that of the coded incoherent MFSK system.

TABLE 7-1

Tasks Recently Completed
• Modeling of the planetary channel for communication purposes.
• Investigation of the performance of short constraint length convolutional codes in conjunction with coherent BPSK modulation and Viterbi maximum liklihood decoding.
• Investigation of the performance of selected long constraint length convolutional codes in conjunction with coherent BPSK modulation and sequential decoding.
• Comparison of the performance of coded coherent BPSK system with that of coded incoherent MFSK system.

The next table indicates very briefly how we are going to model the fading channel. The transmitted signal s(t) is expressed in terms of a complex signal representation. Here u(t) is the complex envelope of the transmitted signal and it can be expressed simply in terms of successive translates of a basic channel signaling wave form, $u_0(t)$. The quantity T_s which appears here is the basic channel signaling interval. We have, of course, modulation by the binary information sequence to be transmitted represented by the sequence $\{x_i\}$ of $\pm$ 1 values. We will assume that the received signal v(t) is again expressed in complex signal representation. The complex envelope w(t) in this case looks like that of the transmitted signal except for the presence of a modulation factor $[\Gamma + a(t)]$ and the addition of a white Gaussian noise component n(t). The quantity Γ appearing in the modulation factor can be expressed as $\Gamma = \gamma e^{j\psi}$. Here the amplitude γ is a fixed deterministic quantity to be specified while the phase ψ is a random variable uniformly distributed over $[-\pi, \pi]$. The quantity a(t) is a complex zero-mean Gaussian process which represents diffuse scattering. It is completely described either in terms of a frequency dispersion function $\tilde{\sigma}$ (f) or in terms of an autocorrelation function $R_{aa}(\tau)$.

TABLE 7-2

Fading Channel Characterization

- <u>Transmitted Signal</u>

$$s(t) = \mathrm{Re}\{u(t)\ e^{j\omega_o t}\}$$

with

$$u(t) = \sqrt{2E_s}\ \sum_i x_i u_o(t - i\ T_s)$$

$\{x_i\}$ ∿ binary ($\pm$ 1) information sequence

$u_o(t)$ ∿ complex envelope of channel signaling waveform

- <u>Received Signal</u>

$$v(t) = \mathrm{Re}\{w(t)\ e^{j\omega_o t}\}$$

where

$$w(t) = [\Gamma + a(t)]\ u(t) + n(t)$$

Here

$n(t)$ ∿ AWGN process with noise spectral density $N_o/2$ watts/Hz.

$\Gamma \triangleq \gamma\ e^{j\psi}$ ∿ γ fixed deterministic quantity and ψ uniformly distributed over $[-\pi, \pi]$

$a(t)$ ∿ complex zero-mean Gaussian process representing diffuse scattering

<u>Frequency Dispersion Function</u>

$$\tilde{\sigma}(f) = \frac{\sigma_a^2}{2\pi}\ \frac{B_o}{B_o^2 + f^2}$$

B_o ∿ channel coherence bandwidth in Hz.

<u>Autocorrelation function</u>

$$R_{aa}(\tau) = \sigma_a^2\ e^{-2\pi B_o |\tau|}$$

In our work we have made use of a particularly simple choice for $\tilde{\sigma}(f)$ as indicated in the slide by the first-order Butterworth spectra. Here the frequency dispersion function $\tilde{\sigma}(f)$ is completely described in terms of a scale parameter $\sigma_a{}^2$ and a quantity B_O measured in Hertz which we will call the channel coherence bandwidth. The coherence bandwidth B_O, or more precisely its reciprocal, is a measure of the amount of memory on the channel. Thus, in terms of this particular model, there are three quantities we have to specify; the amplitude term γ, the scale parameter $\sigma_a{}^2$ of the diffuse scattering component a(t), and the channel coherence bandwidth B_O. Actually, with respect to this last quantity, it will prove more convenient to specify the dimensionless quantity $B_O T_s$ which represents the coherence bandwidth normalized to the signaling rate of $f_s = 1/T_s$. The appropriate specification of these parameter values, of course, depends heavily upon mission parameters and, in particular, the communications geometry.

I would like to mention at the outset, and I think this was brought out in the previous talk, that some of the theoretical propagation studies result in a channel model which differs somewhat from that which I have described. In particular, the amplitude of the fading signal component as I have described it possesses a Rayleigh-Rice distribution while the propagation studies predict a lognominal distribution. For a number of reasons which I don't really want to get into at this time we have found it much more convenient to make use of the model I have described. In any event, in the regime where the lognormal result can be justified, there is close agreement between the two distributions. Furthermore, it is important that the parameters in the model described here can be related quite easily to the results of the theoretical propagation studies.

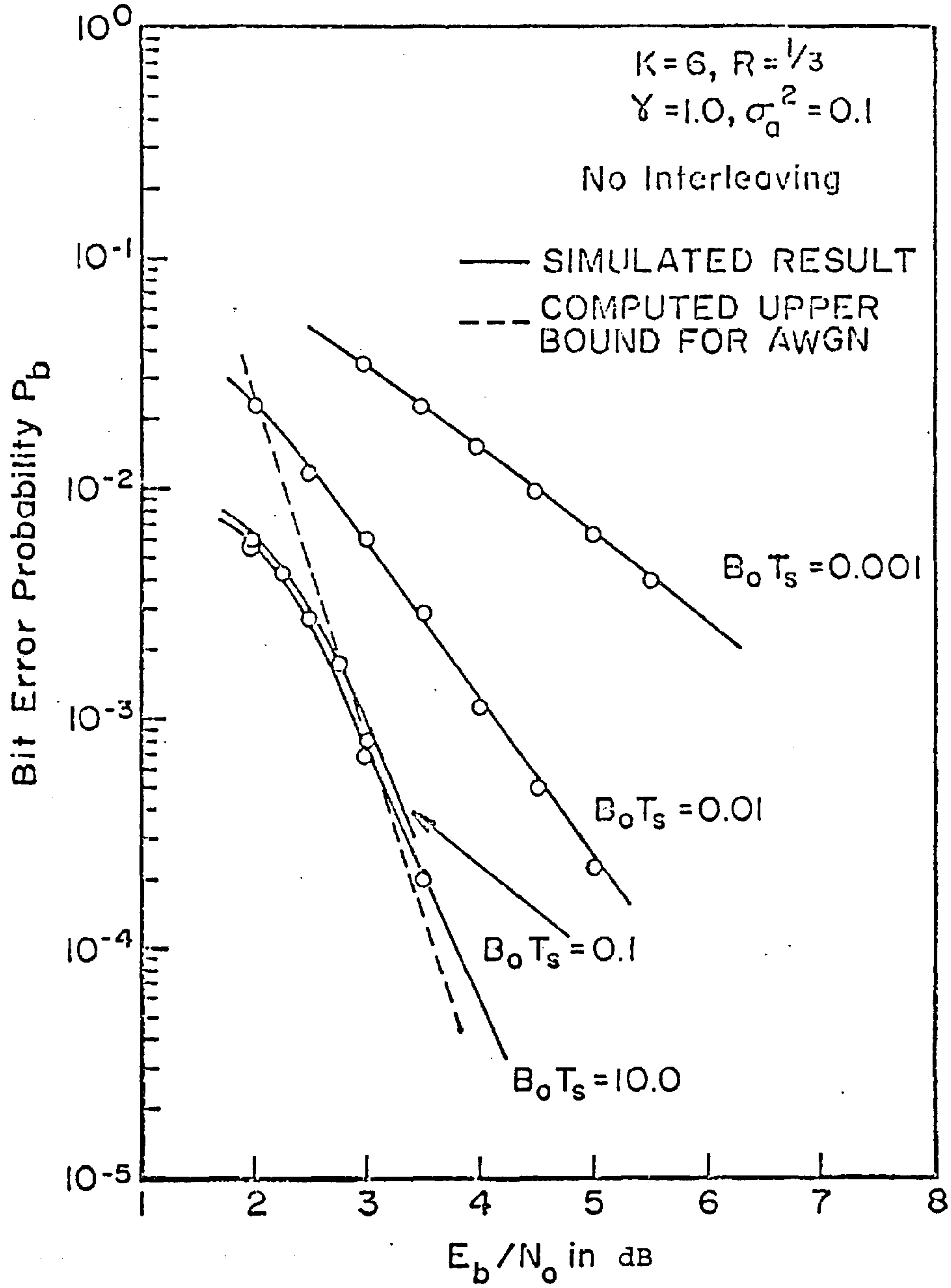
In the table below I have indicated some typical channel model parameters. These data are derived from a paper by Woo, et al., from JPL and are for a Venus mission. The quantity L, here is the depth of penetration into the Venusian atmosphere, σ_χ^2 is a scale parameter representing the variance of the log-normal amplitude component and B_χ is the corresponding bandwidth of this component. We have developed techniques which allow the parameters B_0, σ_a^2 and γ to be related to σ_χ^2 and B_χ allowing completion of the table as indicated. Observe that for a depth of penetration of 55 kilometers a value for B_0 of 0.146 Hz is appropriate. The location parameter γ and scale parameter σ_a^2 can similarly be determined. The case γ=1.0 represents the best fit to the theoretical propagation results and we have in addition carried through the case γ= 0 as somewhat of a worst case.

Table 7-3
Summary of Fading Channel Model Parameters

L*, km	σ_χ^2	B_χ, Hz	$B_0=\sqrt{2}\,\sigma_\chi B_\chi$, Hz	σ_a^2 $\gamma=0$	σ_a^2 $\gamma=1$
55	0.056	0.436	0.146	1.118	0.112
30	0.018	0.59	0.112	1.037	0.036
10	0.0025	1.02	0.071	1.005	0.005
5	0.007	1.45	0.054	1.001	0.001
1	4×10^{-5}	3.23	0.029	1.000	-

*L is depth of penetration into Venusian atmosphere

Figure 7-32 indicates some typical results. In this case we consider a constraint length K=6 code with rate R=1/3. The location parameter γ= 1.0 and σ_a^2 = 0.1 which would correspond approximately to the top line of the preceding table



Simulated Performance of K=6, R=1/3
Code for Different Values of B_0T_s
and with γ =1.0, σ_a^2 = 0.1

Figure 7-32

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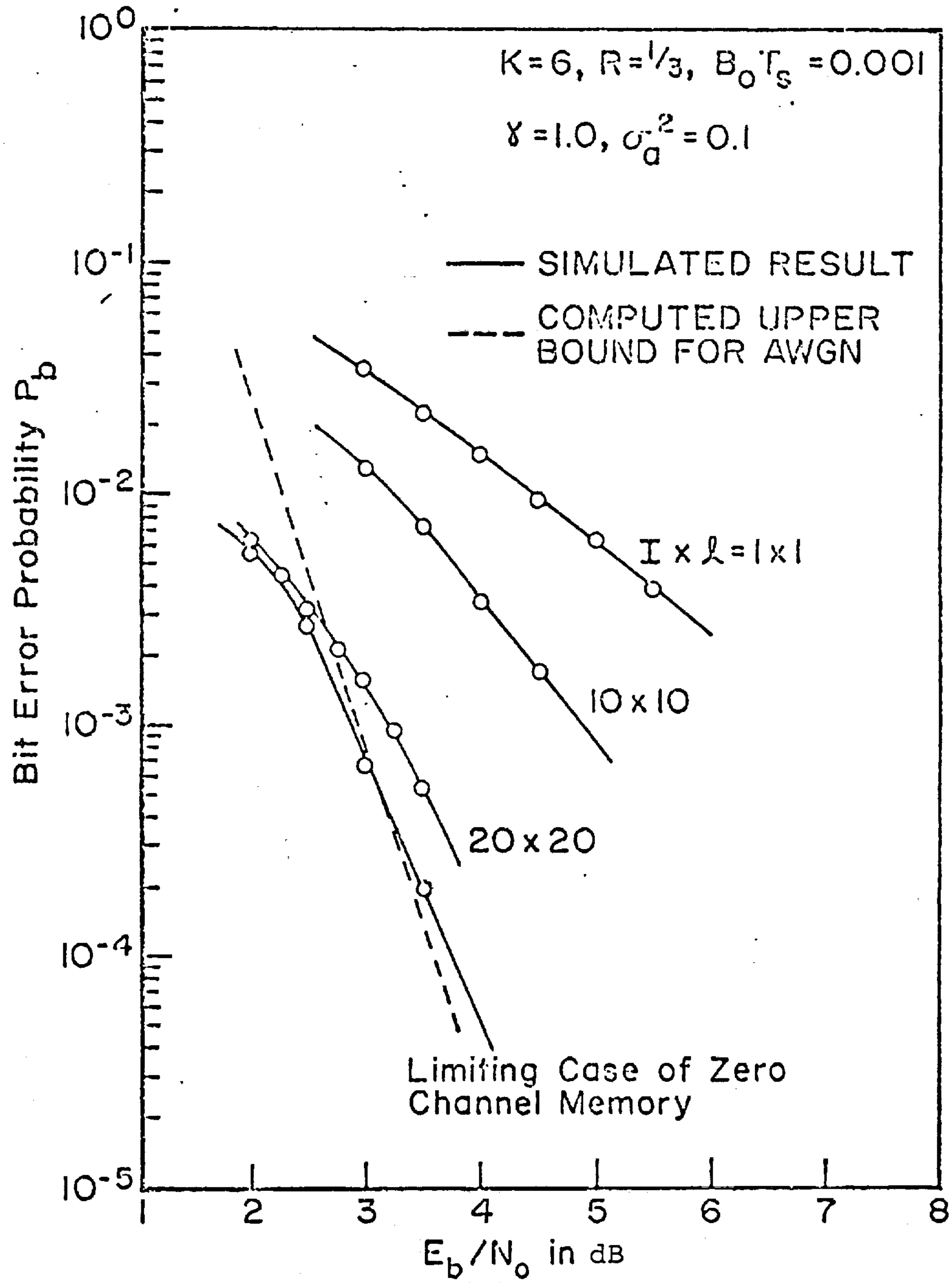
indicating a depth of penetration into the Venusian atmosphere of 55 kilometers. The resulting bit error probability P_b as a function of E_b/N_0 is indicated for several values of B_0T_s. If B_0T_s is small this would indicate considerable channel memory while large values of B_0T_s indicate little or no channel memory. The dotted line illustrated in this figure represents the performance that would be obtained on the additive white Gaussian noise (AWGN) channel. It represents a computed upper bound which we know to be extremely tight on the tails. As the figure indicates, the presence of memory on the channel results in severe degradation in performance over that which would have been obtained on the AWGN channel.

The easiest way to combat the effects of the channel memory is by the use of some form of interleaving. In Figure 7 - 33 we indicate the performance obtained with a very simple square block interleaver for the same code and channel parameters. Here, again, the dotted line represents performance on the AWGN channel. We see that using a 20 x 20 interleaver with $B_0T_s = 0.001$ we can obtain performance relatively close to that predicted by the AWGN results. The solid line, here, is labeled "limiting case of zero channel memory," and represents a large B_0T_s value say 10.

It is clear then that some form of interleaving is required to combat the memory of the channel. On the basis of a large number of simulation results it has been concluded that the amount of interleaving required is quite insensitive to the code constraint length and/or rate. In Table 7 - 4 we indicate in tabular form the required interleaver size as a function of B_0T_s to achieve performance within a few tenths of a db of the limiting case of zero channel memory.



Effects of Block Interleaving for
K = 6, R = 1/3 Code With B_oT_s = 0.001
and γ = 1.0, σ_a^2 = 0.1

Figure 7-33

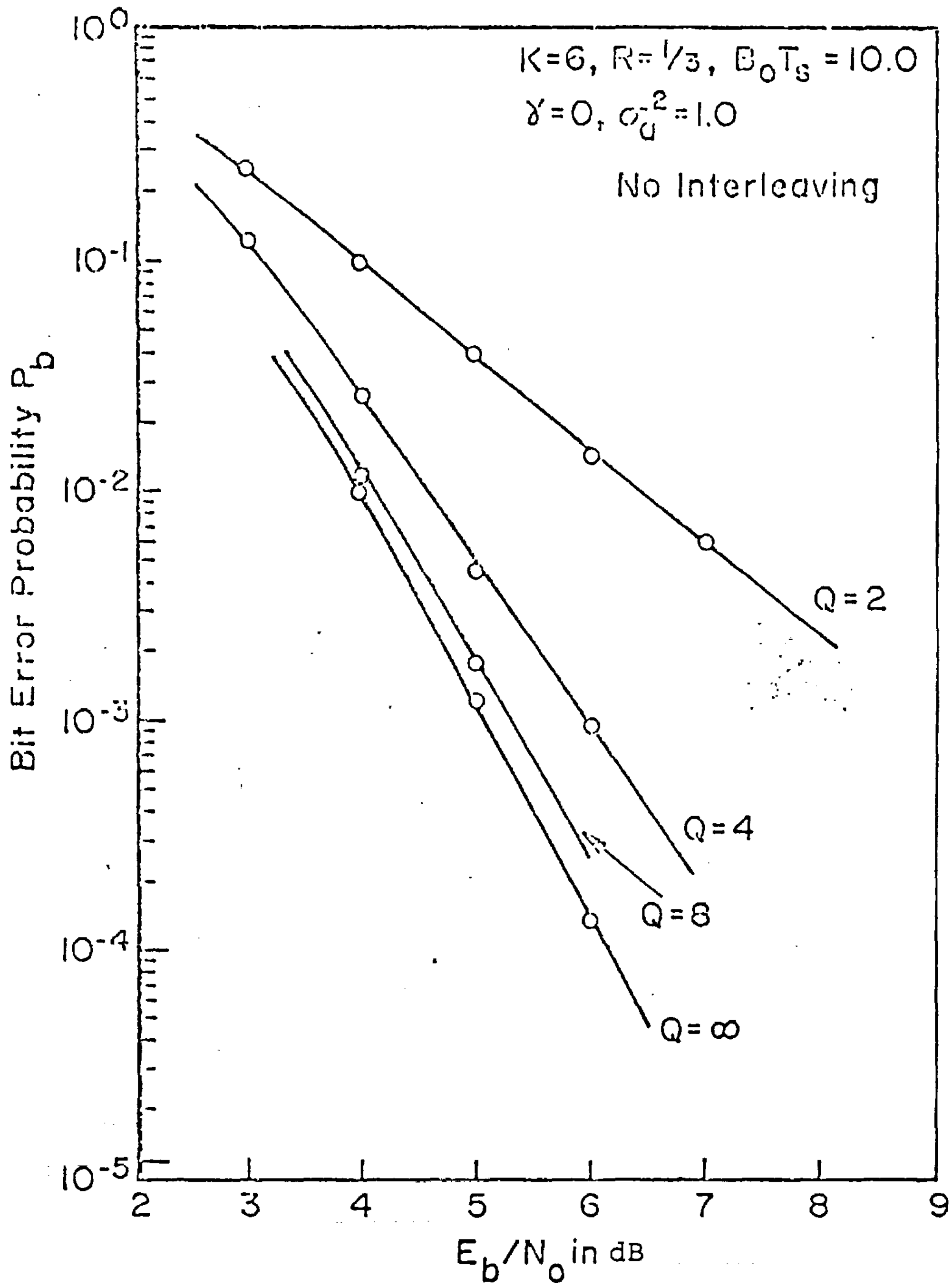
TABLE 7-4

B_oT_s	Required 1 x ℓ
.1	1 x 1
.01	10 x 10
.001	100 x 100

Summary of Interleaving Requirements as a
Function of B_oT_s to Obtain
Performance Within a Few
Tenths of a db of Limiting Performance

In the simulation results reported so far we have assumed infinite quantization of the receiver output. Typical performance as a function of the number Q of quantization levels allowed at the receiver output is illustrated in Figure 7-34. We see that Q=8 level quantization results in performance within a few tenths of a dB of the performance with infinite level quantization.

It would appear at this point that, if we were to make use of the simple interleaver structures described here and Q=8 level receiver output quantization, performance within a few tenths of a dB of that predicted for the AWGN channel can be achieved. Unfortunately, the results have all assumed perfect phase tracking and, of course, this need not be the case. Since we are considering a coherent BPSK system we must address the effects of imperfect phase tracking. Recall that in the case of amplitude fading along, the channel memory really bothered us. If we look now at the case of phase tracking it is possible that we can exploit the channel memory to estimate the signal phase. In particular, with appreciable channel memory (i.e., B_oT_s<<1)



Effects of Quantization on K=6,
R=1/3 Code with B_oT_s = 10.0
and γ=0, σ_a^2 = 1.0

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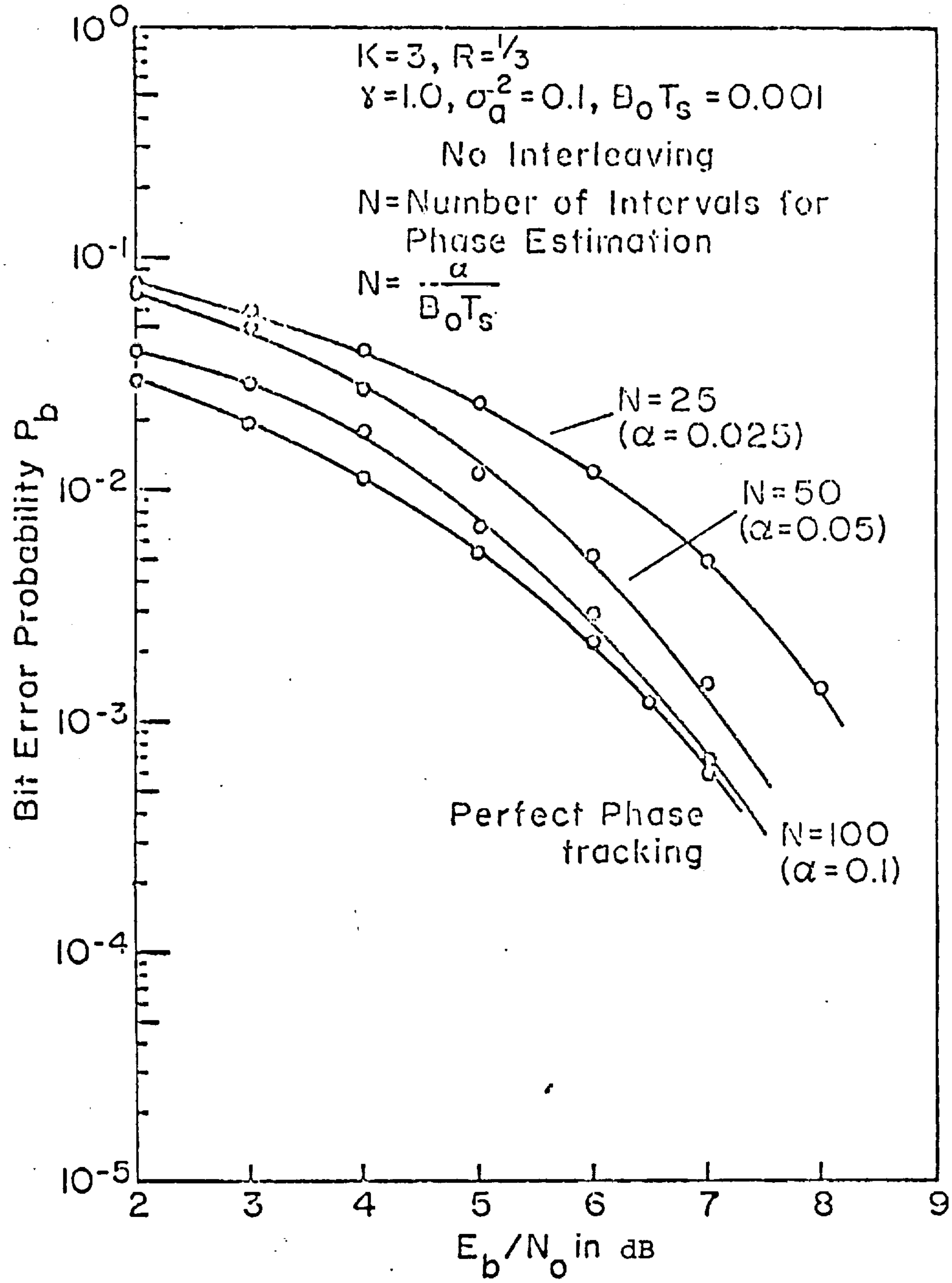
Figure 7-34

the channel changes very little over many successive signaling intervals. It is possible then to make use of past receiver outputs to estimate the phase during the next signaling interval and use it for coherent local oscillator injection. Typical performance obtained with such a phase estimation scheme is illustrated in Figure 7-35. In this case the constraint length K=3 the rate R=1/3 and $B_O T_s = 0.001$. The quantity N is the number of past signaling intervals used for phase estimation. We expect the received signal phase to change very little over a number of channel signaling intervals which is approximately $1/B_O T_s$. As a result, the curves in this figure are parameterized by $N = (\alpha/B_O T_s)$ where $0 < \alpha \leq 1$ represents the fraction of the total possible signaling intervals used for phase estimation. The phase estimator utilizes the in-phase and quadrature matched filter outputs during N past intervals to predict the phase during the next signaling interval. We see from the figure the performance obtained with N=25, 50 and 100 compared with that which we would have obtained with perfect phase tracking. With N=100 (i.e., $\alpha = 0.1$) it is possible to obtain performance which is again within a few tenths of a dB of that obtained on the AWGN channel.

The conclusions to be drawn from these simulation studies are summarized in Table 7-5. Finally, Table 7-6 indicates the future work to be performed under this program.

Thank you.

MR. GRANT: The last speaker of this session is Dr. Thomas Croft of Stanford University. Dr. Croft is a Senior Research Associate in the Center for Radar Astronomy and a member of the radio science teams for the Pioneer Venus and Mariner-Jupiter-Saturn missions.



Effects of Imperfect Phase Tracking
for K=3, R=1/3 Code With γ =1.0
σ_a^2 = 0.1 and B_oT_s = 0.001

Figure 7-35

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Table 7-5

Conclusions

- Even in the absence of phase tracking errors some degree of interleaving is required to combat time correlated fading of channel.

- Simulation results have indicated only modest amounts of interleaving are required to approach performance of memoryless channel.

- Additional propagation results are required particularly on the phase perturbation process.

- More recent results have indicated the definite superiority of noncoherent MFSK system when phase tracking errors are considered.

Table 7-6

Future Work

- Additional Modeling of Phase Tracking Errors in Coherent BPSK System
- Investigate the Performance of Coded Incoherent MFSK System
- Investigate the Performance of Coded PCM/FM System
- Explore the Desirability and/or Feasibility of Concatenated Coding Schemes
- Investigate the Frequency Tracking and/or Acquisition Problem Associated with PCM/FM

RADIO FREQUENCY SCIENCE CONSIDERATIONS

Dr. Thomas A. Croft
Stanford University

N75 20398

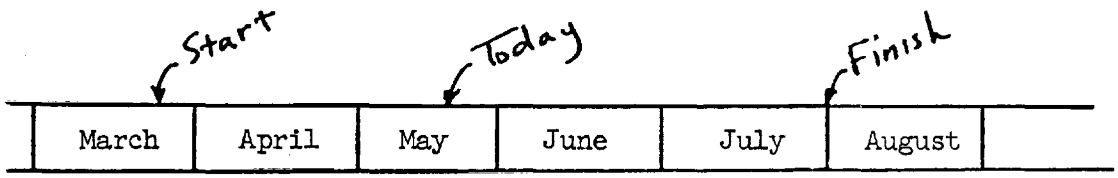
DR. CROFT: Many scientists have been waiting a long time to get access to a radio link that is completely outside the atmosphere, and I would like to talk about how we might use the 400 MHz link to do some scientific research at the same time we use it for telecommunications. There hasn't been much mention of this subject thus far in this meeting and in part, that lack is due to our tendency to think in terms of just the sensible atmosphere, the lower part. However, the ionosphere and the magnetosphere form the top of the atmosphere; and one can't hope to understand the atmosphere without knowledge of the exosphere. As a result, I like to include the ionosphere when I speak of the "atmosphere."

Figure 7-36 is an outline of my activities relevant to this subject. One of my objectives is to compose a consensus, not just my views, so if any of you have suggestions to be included in the final report, please contact me.

There are three areas of investigation listed in Figure 7-36. First, what can we do to get new scientific information by using the 400 MHz link by itself; second, what can we do to back up the experiments that are flown and, third, how can we help in the design of follow-on probes? We are going to be designing more probes in the future and, eventually, we will want to know what happened on this set for the purpose of engineering the next set. So what should we be looking for to meet these three areas?

A study previously conducted by Coombs of Ames led to a list of recommended objectives which are summarized on Figure 7-37 and present some very good ideas. One of the most straight forward goals is the measurement of the strength of the signal and serves to get both the measurement of absorption as a form of scientific information about the atmosphere and to provide

FIGURE 7-36



to consider 3 adjunct uses of the 400 MHz telecommunications system:

1. obtain new scientific information
2. provide backup information for the experiments flown
3. obtain measurements which aid in designing future probes

FIGURE 7-37

Coombs' suggested starter list:

1. Measure 400 MHz amplitude to determine absorption and perhaps scintillation (if data rate permits)
2. Measure noise strength near 400 MHz to reexamine 400 MHz choice and to observe thermal, cosmic and local synchrotron noise trends
3. Probe VSWR sensing to monitor integrity of system, icing, and possibly plasma effects
4. After probe is finished, have the bus radio occultation in the same region where the probe fell - primarily to evaluate the occultation

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Other ideas briefly mentioned

-dual frequency from the probe
-high-gain tracking antenna on the bus
-two-way communication, bus to probe and back
-more than one probe, or an auxiliary space-deployed unit
-sensor antennas on the probe
-additional DSN facilities

data which will aid us in the design of future systems. Scintillation could be observed by this same means if we incorporate sufficiently rapid sampling of the amplitude.

This is a good time to bring up a point concerning the telemetry system; it has a "soft decision" feature, that is we won't get on-off decisions from it, but rather we are going to get a bit (i.e., a decision) and some measure of the confidence in that bit. The coding engineers who are trying to optimize this system should keep in mind the scientists' need for a good quantitative measurement of the amplitude variations. It might be possible to kill two birds with one stone in this case. That is, if we measure atmospheric scintillation as an adjunct to the telemetry code, we would come out with good scientific understanding of the planet's atmosphere and with a good set of data for designing future probes. We would get our confidence measure for this telemetry string at the same time, provided that we do the coding right. I haven't seen any mention of this kind of reasoning in the literature.

The second suggestion of Coombs which is also very natural is that we should measure the noise. I will have more to say about that in connection with subsequent figures.

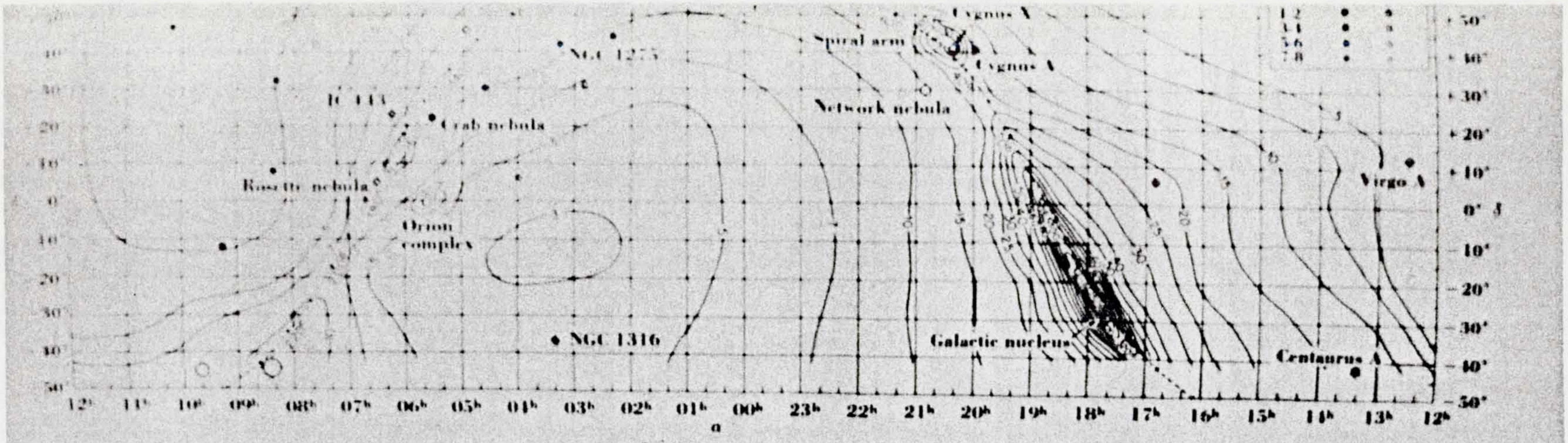
The third suggestion was to measure the standing wave ratio on the antenna. He points out that we are going to have these probes descending into some extremely unearthly atmospheres, and for example, the antenna elements might ice up; some kind of material might be physically deposited on the antenna that would cause a loss of telemetry. If the telemetry fades out, we would want to know the cause. It would be very illuminating to know the standing wave ratio, for that purpose alone. If something breaks, the standing wave ratio is a good diagnostic indication. If the telemetry weakens and the standing wave ratio goes bad, you would have a good clue as to why it went bad.

If we do telemeter the VSWR, while the probe is in the ionosphere, we could measure the ionospheric plasma effect. In this case, the effect is somewhat masked by the local ionization induced by the vehicle itself, but nevertheless, this idea merits further consideration.

Coombs suggested that after probe descent the bus perform an S-band Occultation in the same region of the planet where entry occurred. His objective was to shed some more light on the occultation method itself.

I won't go into those last items on Figure 7-37 because of time. With regard to measuring noise; because we have selected 400 MHz, we are in a frequency regime where, for the various missions, the cosmic noise, the planet disc temperature and the synchrotron emissions are of comparable magnitude. We inherently measure their sum. (This isn't true, however, at Jupiter, where the synchrotron radiation overwhelms everything else, but on the other missions we are going to be measuring the sum of several comparable sources.) I think it would not be productive to measure the noise unless we can somehow identify the relative strength of the components.

I have included Figure 7-38, a radio map of the sky, because it shows the distribution of cosmic noise at 250 MHz. The situation at 400 MHz is similar. The lower portion of this figure is a representation of the same data in shades of gray. The white dots are the radio star sources and the light-band is the spatially diffuse emission; it is probably synthrotron emission from electrons in our own galazy. You can see there are large areas of comparative quiet. If we can manage it, we might enter the planetary atmosphere on a side that faces a quiet area and thereby eliminate a lot of this source of noise. That should be one of the things considered in entry-point selections, albeit, a minor point.



In the plot above, to get temperature in °K at 250MHz, multiply by 6 and add 80°K.

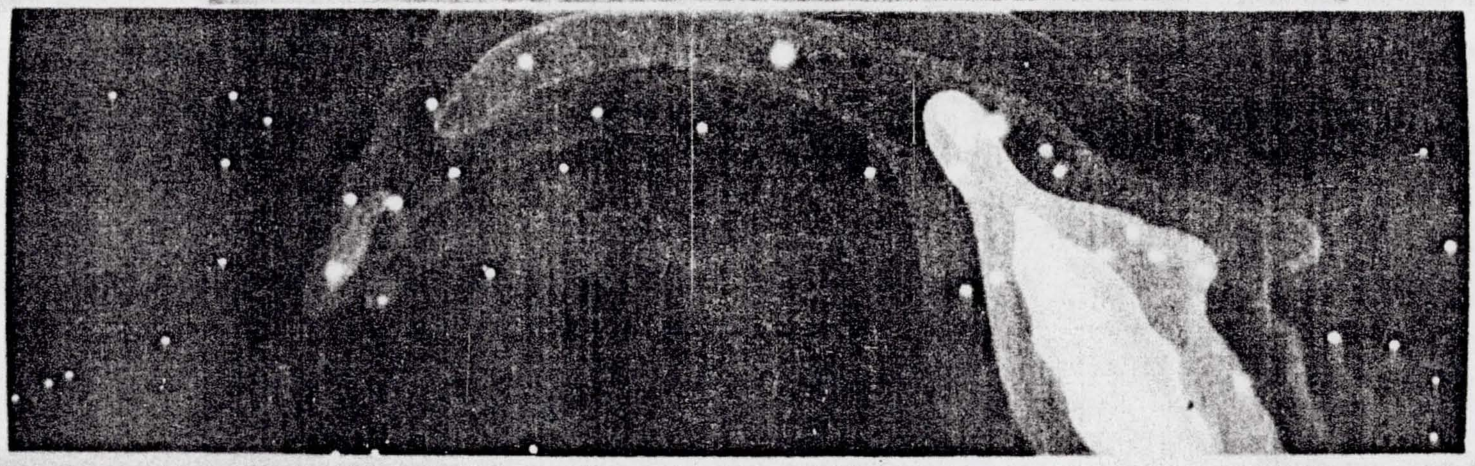


Fig. 8-2. Radio-sky panorama obtained by converting the contour map of Fig. 8-1 to shades of black and white. The figure gives an impression of how the sky would appear if our eyes were sensitive to radio waves instead of to light.

Figure 7-38

Microwave absorption spectroscopy is often used to identify molecules but examination of the frequency axis of Figure 7-39 reveals that most of the identifiable absorption bands are in the 10 GHz region or higher. Ammonia has a 23.5 GHz absorption band, but it is pressure-broadened to such an extent that it is a major absorber even down at 400 MHz. This figure indicates that there is not much hope of measuring individual absorption lines and thereby doing any kind of molecular species identification unless we venture into the S, X and K bands.

Ammonia is an unusual molecule in that the three hydrogens lie in a triangle and the nitrogen atom forms the peak of a pyramid shape as shown in Figure 7-40. Classical mechanics

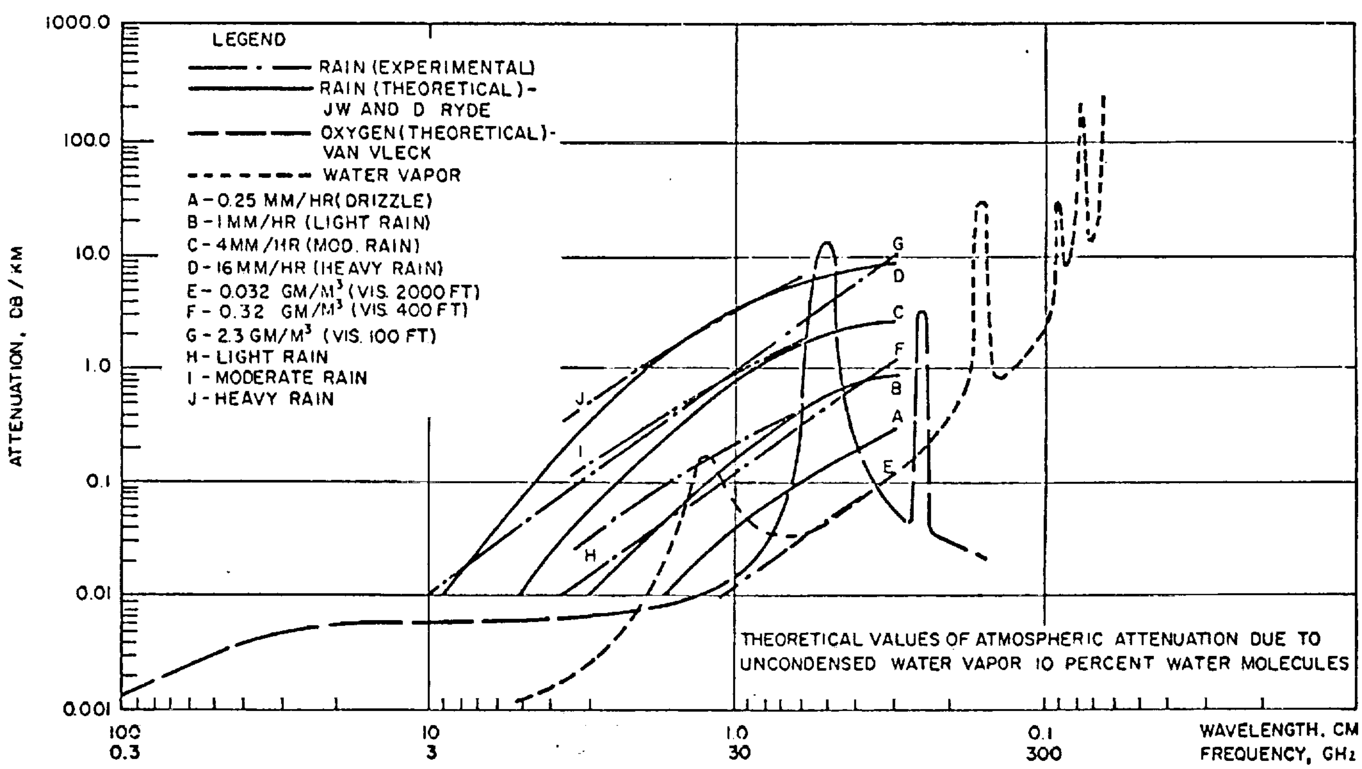


Figure 7-39. Atmospheric Attenuation Summary

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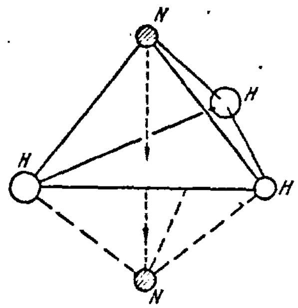


Figure 7-40

leads you to conclude that the nitrogen must remain on the top, but quantum-mechanically it is found that the atom can tunnel through that potential barrier at the center of the triangular base and get down to the bottom position. That oscillation from the top to the bottom is called "inversion" and the fact that it has to tunnel makes it "hindered inversion;" this slows the natural frequency somewhat. As a result of tunnelling, the nitrogen atom oscillates at 23.5 GHz but pressure broadening causes it to be effective even at 400 MHz. At Jupiter, absorption by ammonia is a major factor but this doesn't appear to be the case at the other planets of interest.

There is a mention in the literature that water droplets with ammonia in solution in the droplets might be a major absorber even down at 400 MHz, at least in Jupiter's atmosphere. I don't know how serious this problem is, but it may be the limiting item determining how deep we can go in the Jupiter atmosphere.

Figure 7-41 is calculated for Earth, but it shows the general trend that ionospheric absorption is not a problem on Earth and my calculations to date indicate that similar absorption (or less) occurs on the outer planets considered. The absorption is on the order of a tenth of a db. The measurement of absorption would not reveal anything about the ionosphere nor would it be a problem. I don't see anything of significance here for us.

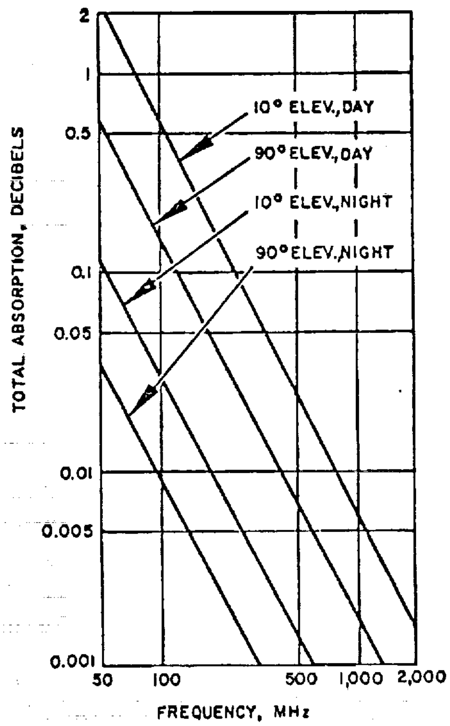


Figure 7-41. Ionospheric Attenuation for a Source at 1,000-km Height

There is some possibility of equipping the probe with a sensor for measuring capacitance; with this, we might determine the ionospheric density. By using the 400 MHz antenna standing wave ratio, we might get the same kind of data. Such a measurement would be scientifically interesting and also useful to the engineers who design future probes.

Figure 7-42 is a photograph of Saturn and I have indicated the probe approaching along the inner white line and the bus on the outer white line. I am trying to show that the bus could observe the direct signal from the probe to get the telemetry, and it could also simultaneously observe the Doppler-shifted echo reflected off the ring. I can assure you that if that could be received, this signal could be very informative to scientists. Right now, this concept isn't in the baseline design because the 400 MHz transmitter doesn't operate until the probe descends into the atmosphere. I do not yet know if the reflected signal would be strong enough for such an obser-

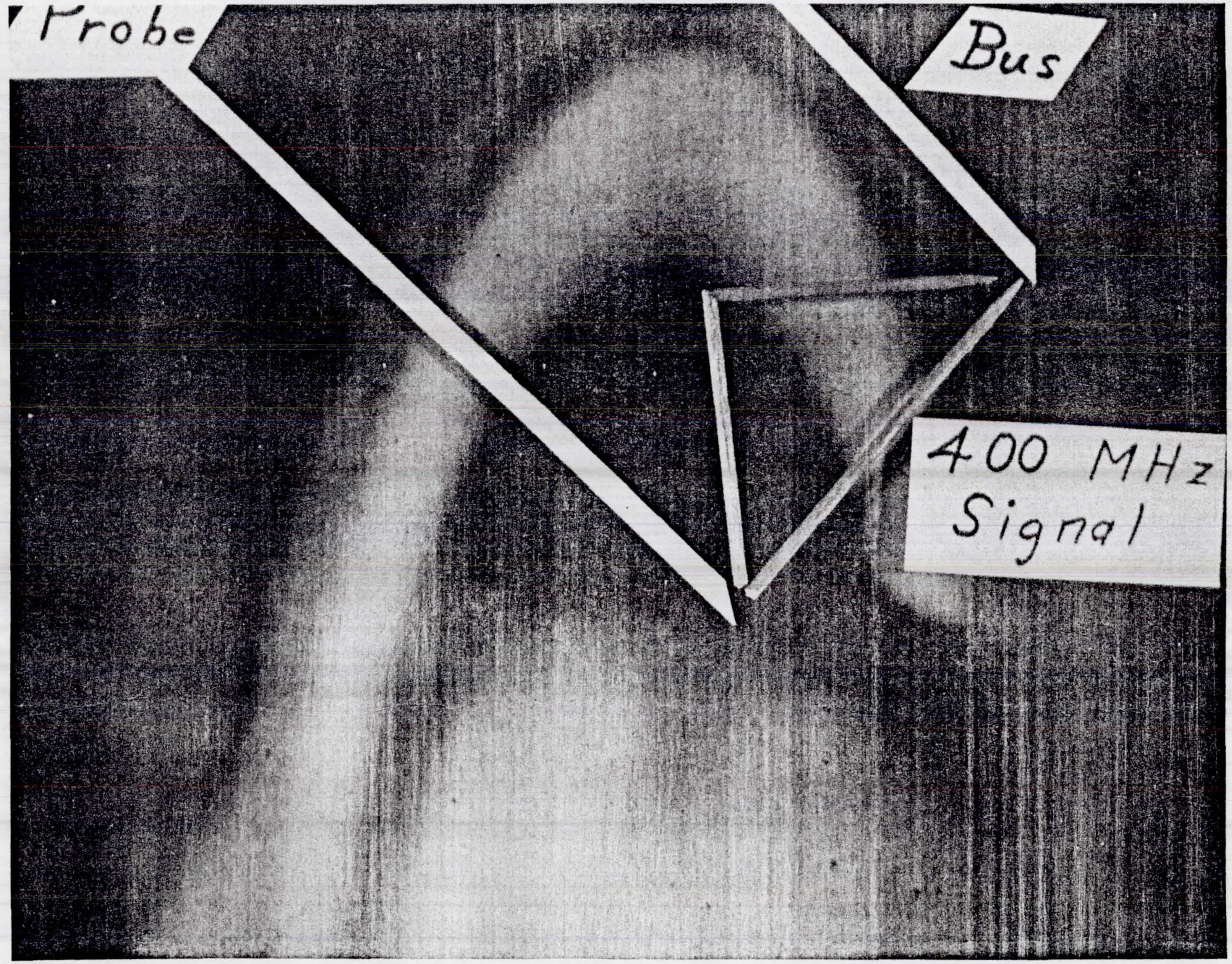


Figure 7-42

vation with the baseline design, and I will do some more study on this. There is a debate concerning the cause of the observed radio scattering off the rings, and different models explain it in different ways. Some models lead to the prediction that scattering as shown in Figure 7-42 would be very weak; others indicate this would be a strong echo. It would be very informative if we could see it.

Figure 7-43 shows an exciting concept I haven't heard mentioned earlier. If we operate this radio system before entry, then it is feasible to orient the bus and probe so that there is a brief period during which the 400 MHz signal goes through the rings of Saturn. A ring occultation at this low frequency would provide additional data about the structure and composition. (Prior S-band occultations will have occurred.) It appears possible to perform and complete this occultation experiment before probe entry (Figure 7-44). Therefore, it appears this experiment wouldn't conflict with the other requirements of the 400 MHz system.

For Saturn, Jupiter, and probably Uranus, there is virtually no chance of seeing the reflection off the surface as shown in Figure 7-45. For Titan, however, this is a reasonable possibility. If we build the capability into the bus receiver of looking for Doppler shifted echos well away from the direct signal, then we should look for this reflection from Titan. It could tell us a great deal about the atmosphere and the surface.

For Uranus, at the time of these probe missions, the planet's spin axis will be within about 10° of the direction to the sun. In Figure 7-46 the sun is to the left and the probe and bus are approaching Uranus. If we have two-way Doppler, as Paul Parsons mentioned, we could measure the Doppler shift and perhaps obtain an indication of the north-south winds. Because of the near-alignment of sun, spacecraft and planet, there is comparatively

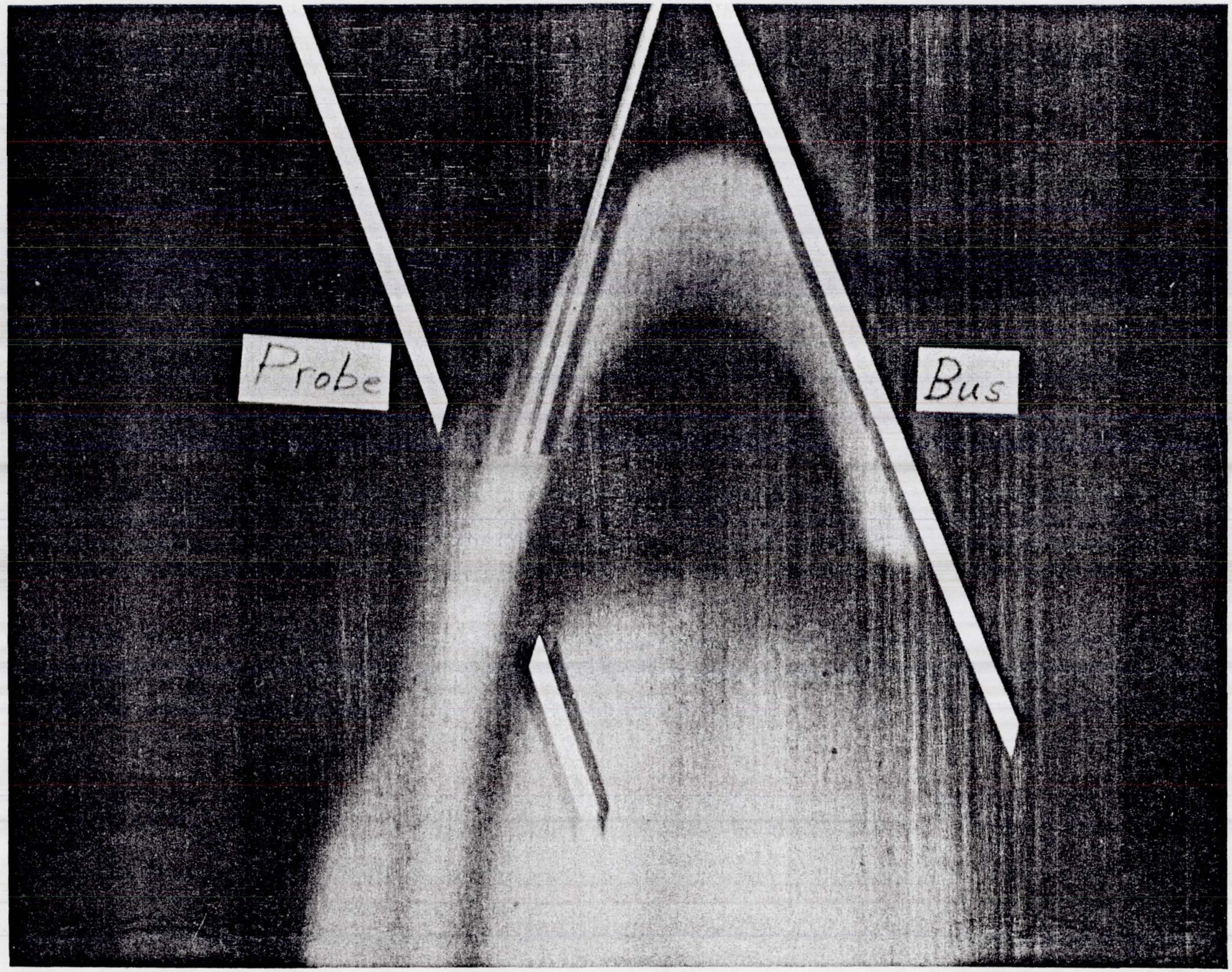


Figure 7-43

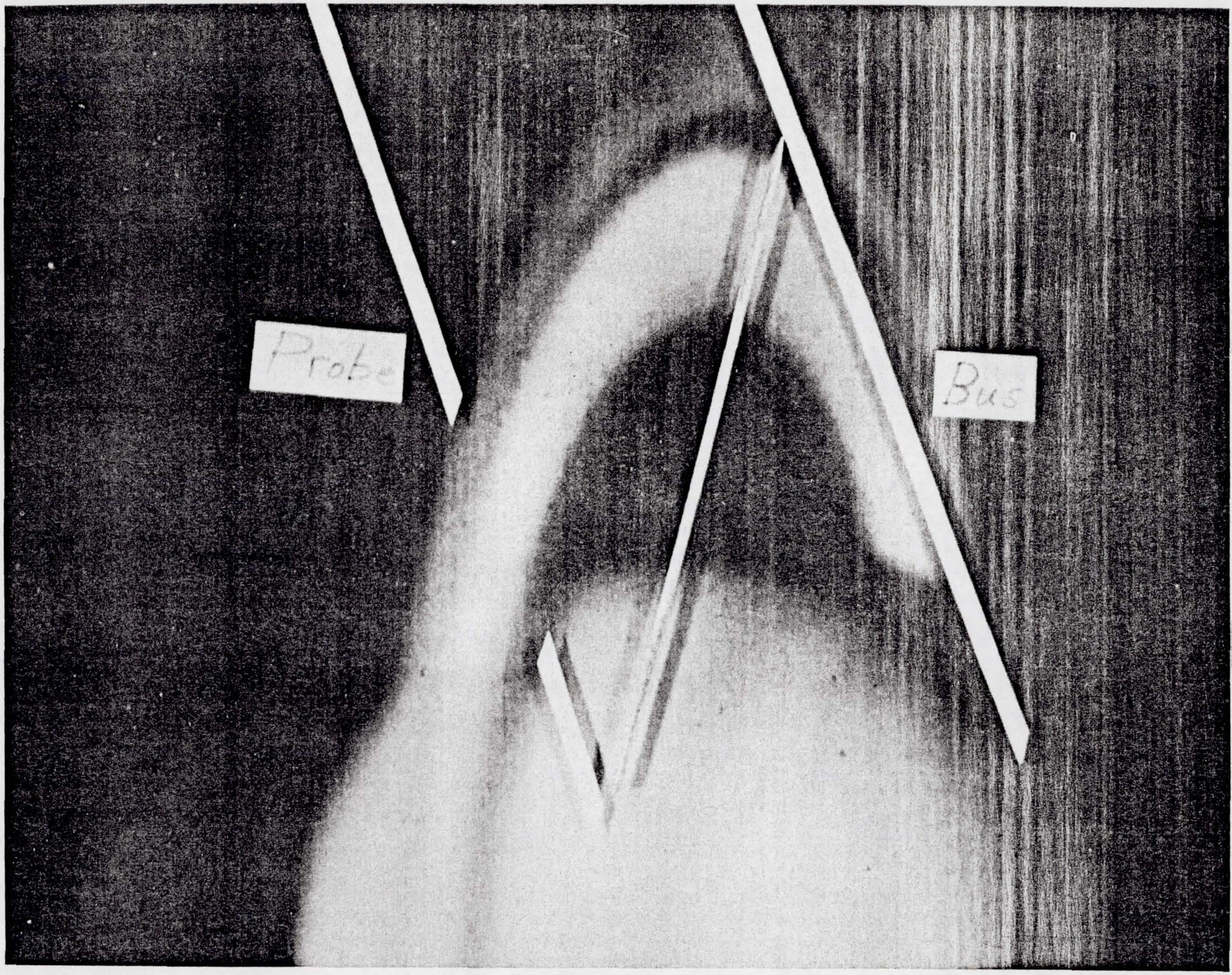


Figure 7-44

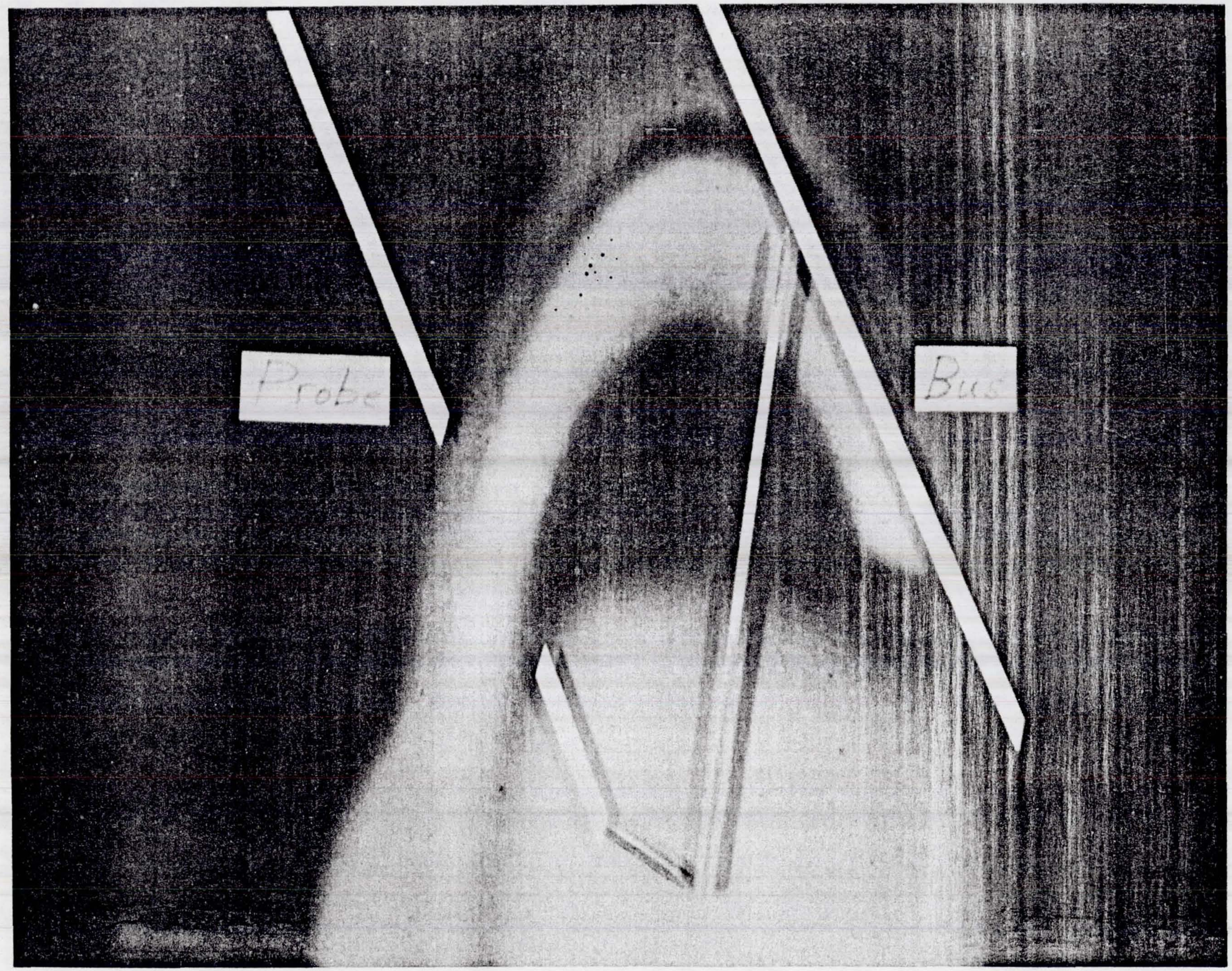


Figure 7-45

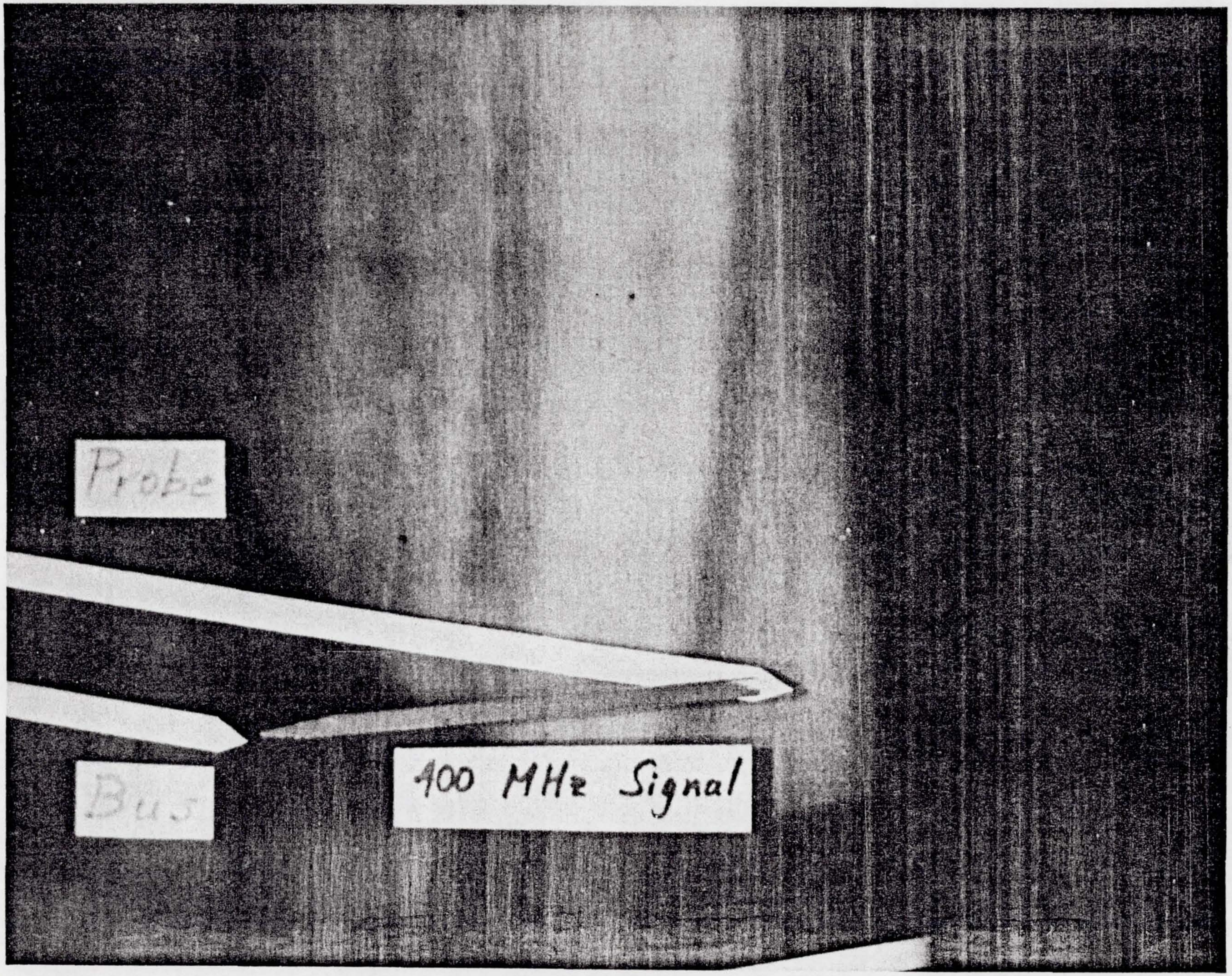


Figure 7-46

little parallax involved. All vectors are almost in a straight line, and we may be able to resolve the north-south wind by measuring the motion of the probe's terminal descent.

Before entry, the measurement of Doppler would permit accurate tracking and would solve one of the problems that the entry people are worried about; namely, where did the probe actually go in. With Doppler measurements at 400 MHz, we could reconstruct the final pre-entry track and find out where it went. That would be a very valuable adjunct to other experiments.

I had planned to carry you through a dual-frequency calculation, but for lack of time I'll only show the result, in Figure 7-47. If we transmit two frequencies and measure differential group delay, we can determine the electron content, I, which is the electron density averaged along the path multiplied by the length of the path. If the frequencies differ by two to one, we obtain a total effect three-quarters as large as would be obtained if the highest frequency were infinite. The message here is that if you had two frequencies which differ by 2:1 or even $\sqrt{2}$:1; we would get a measurable delay difference from which we could infer the electron concentration along the path. In turn, this would provide the electron content of the ionosphere and possibly the magnetosphere if one exists. So here is still another valuable radio measurement prior to entry.

If we operate the radio system prior to entry, it may be possible to occult a satellite as depicted in Figure 7-48. The occultation at the satellite would be interesting to scientists and it would also give trackers an accurate measurement of the probe location. As with the Doppler tracking, this helps determine where the probe entered the planet. I think a satellite occultation experiment would benefit navigation and science. It would be of particular interest to navigators if two-way doppler cannot be incorporated.

FIXED-FREQUENCY $\frac{\text{PHASE}}{\text{GROUP}}$ DELAY

$$T_1 \cong \int_D \frac{ds}{C} \mu^{\pm 1} \cong \frac{1}{C} \int_D \left(1 \mp \frac{40.3\ N_e}{f_1^{\ 2}}\right) ds = \frac{D}{C} \mp \frac{40.3}{cf_1^{\ 2}} \underbrace{\int_D N_e\ ds}_{I}$$

DUAL FREQUENCY DELAY DIFFERENCE

$$\Delta T = T_1 - T_2 \cong \mp \frac{40.3\ I}{cf_1^2} \pm \frac{40.3\ I}{cf_2^2}$$

$$\Delta T = \mp \frac{40.3\ I}{C} \left(\frac{1}{f_1^{\ 2}} - \frac{1}{f_2^{\ 2}}\right)$$

Figure 7-47

If we operate the 400 MHz transmitter during entry, we could determine the radio blackout point. With a dual frequency link in operation, we would get the blackout at two different frequencies and that ought to be useful to the physcists for identifying species. Different atoms ionize at different vehicle speeds or mach numbers.

I have mentioned several experiments that would be possible if we operate at 400 MHz system before entry, although that is not presently in the baseline design. Figure 7-49 summarizes and emphasizes this area of consideration. I feel that these observations would be very valuable to all scientists; not just radio scientists and, therefore, I recommend pre-entry transmissions from the probe be considered. I would summarize this partially completed study as follows: the idea of transmitting 400 MHz (perhaps two-way transmission, perhaps dual frequency

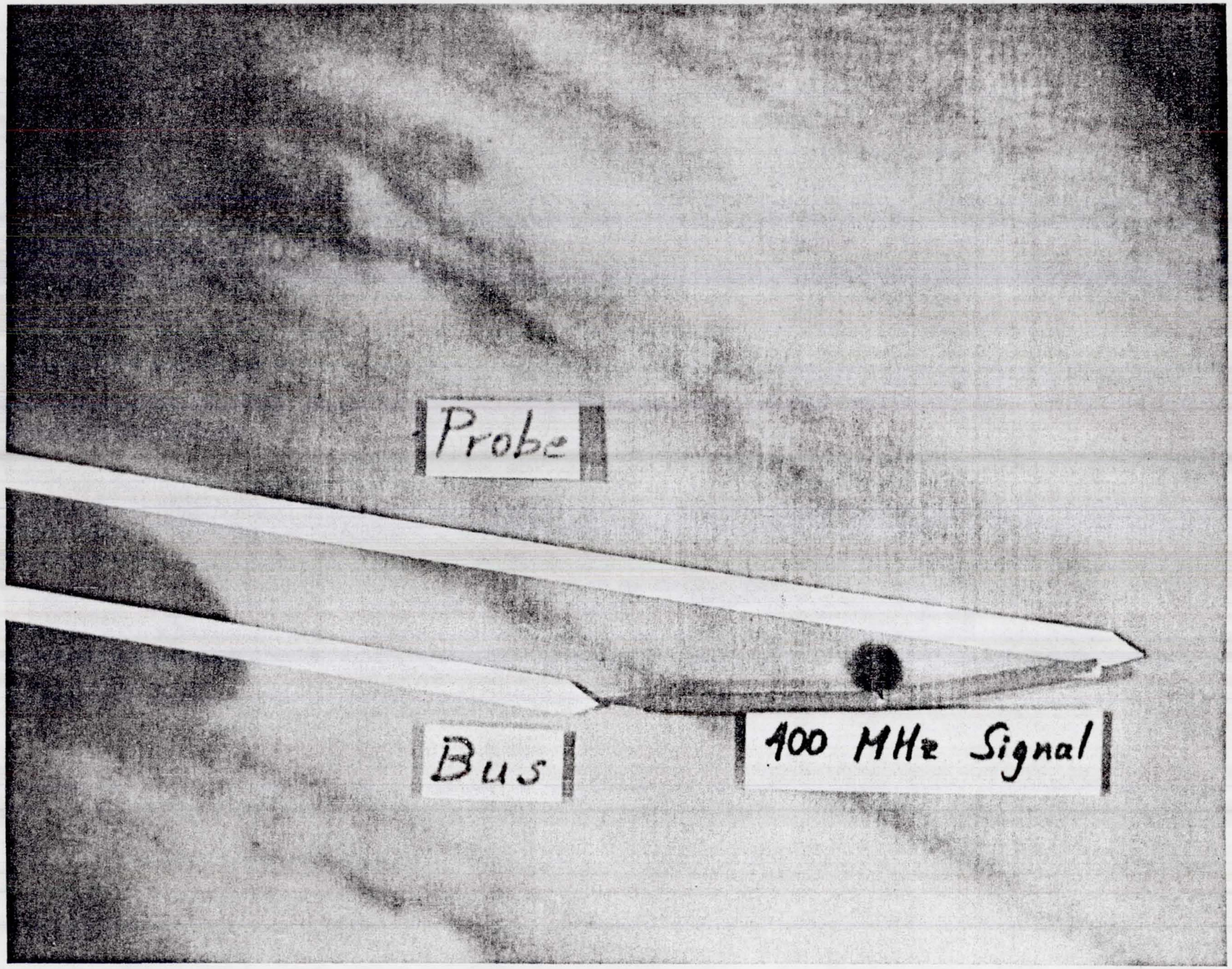


Figure 7-48

transmission) before entry has many striking advantages and yet presently is not being considered. I think the reason is because it is so costly to put items inside the probe's heat shield and protect them during entry. However, it seems to me that there are a number of seemingly unconventional ways to circumvent this cost. For one example, portions of the equipment could be ejected from the probe in the last minute before the entry. There is no need for two-way tracking or dual frequency tracking during final descent so that part of the apparatus, including a battery to run it, could be kicked off before entry. (At Uranus, we might wish to retain two-way tracking.) This is the concept I would like to suggest; an innovative approach to permit productive 400 MHz transmission outside the dense atmosphere.

Thank you.

FIGURE 7-49

Possible 400 MHz Observations BEFORE Entry

A grazing reflection from the rings of Saturn, and perhaps an occultation

Monitor electron concentration during approach by the dual-frequency method

Occult a satellite

Look for reflections from the planet (unlikely to be seen, but very informative if they are measured.)

Monitor the radio blackout at the entry

Observe ionospheric and possibly magnetospheric scintillation

Measure Faraday rotation to determine magnetic field strength

Doppler tracking to determine entry point accurately

UNIDENTIFIED SPEAKER: I think what you mentioned represents a viewpoint that we have not heard very much about in our science advisory committee on J/U, and I would suggest that in the interest of representing the radio science desires, that it would be appropriate for you to discuss this problem somewhat with John Lewis, so that we can get some inputs into the Van Allen Committee and have a better opportunity to evaluate it. We have been operating this committee for about four months and we have not talked about many of the things that you have proposed. We are going to have this continuing interaction with the science team and we would like very much for you to bring this to their attention.

DR. CROFT: I will definitely do that.

MR. SEIFF: I want to make sure I understood this suggestion for a Doppler tracking of the entry probe. You are talking about tracking it during the period prior to entry from the bus vehicle, whose position can then be established after flyby by the perturbation of the trajectory due to the planet.

DR. CROFT: Yes, just like they do the normal trajectory.

MR. SEIFF: That sounds like an extremely valuable idea to me.

MR. GRANT: I don't know what the cost of it is. Of course, everything always has its cost. But the return from it is certainly beneficial.

DR. CROFT: Each pound within the probe body costs you so much, but what would it cost if we kicked off part of that probe? That ejected part would be the cheapest element of the whole bus-probe combination. You don't have to pay for decelerating that mass on the bus, so it is cheaper than a pound of bus equipment. And it is certainly cheaper than a pound of gear inside the probe.

MR. SEIFF: There is another possibility in this same class. You are able to track the probe very accurately by inertial instruments, as a perturbation to the bus trajectory as a result of the Delta V impulse that is applied to it. All of this requires accuracy now, but if that could be done accurately, then I guess the same scheme could be applied; namely, of using post-flyby knowledge of the bus trajectory plus the perturbation that has been applied directing the bus away from the trajectory that the probe is following.

UNIDENTIFIED SPEAKER. The trajectory is going to be known after the fact.

MR. SEIFF: The bus trajectory will be known.

UNIDENTIFIED SPEAKER: What you have to do is somehow get that tied back to the probe.

MR. SEIFF: I am just suggesting that it could be done inertially as well as by radio.

UNIDENTIFIED SPEAKER: In the mission analysis splinter group yesterday, we also wanted to strongly suggest this idea of having communications on the way in because certainly at Jupiter and Saturn, this will be the only data we can get from the probe.

You pointed out in the very first figure that such operation is ruled out; in the baseline there are no communications on the planetary approach prior to entry.

DR. CROFT: I pointed that out specifically for contrast because I also said that we are looking for new views with regard to the baseline. One of the main topics I would like to question in the splinter session is the possible removal of this restriction against pre-entry transmission.

UNIDENTIFIED SPEAKER: Was that brought out because of the power limitation or an antenna problem?

MR. GRANT: I think the question is more broad than that. There is the problem of determining the position of a probe which is always moving relative to the planet. There is going to be an extremely large desire on the part of the science community to have pre-entry transmission for the particles and fields kind of experiments at Jupiter and that requirement ought to be on the table and looked at to see just what the problems are going to be.

We appreciate your comments about it here. But let's be careful because we are talking already about fairly extensive missions and fairly expensive probes. When we start talking about dual frequencies and a two-way Doppler link between the spacecraft and the probe, you are talking about some pretty tough problems. They won't come cheap.

DR. CROFT: I was going to read, as a closing point, a quotation from Admiral Rickover* in 1953 about the gap between an engineers' view and an academic outlook as to the practicality of what could be done by advanced technological systems. It was closely relevant to your point, with which I concur.

UNIDENTIFIED SPEAKER: We certainly want your ideas brought into the discussion we are going to be having in the next couple of years, and we can consider the problems.

UNIDENTIFIED SPEAKER: I think there are a number of interesting concepts that he proposes can be achieved from an analysis of a one-way, noncoherent signal. I would be a little concerned, though, that some of them may be too subtle to appear to have the kind of frequency stability that we expect on a probe, particularly with a transmitter that is going to be on for an hour as its whole life. I think you have to look at that to see if it is going to rule out some of these fairly subtle effects.

* From journal "Nature", volume 243, June 1, 1973

DR. CROFT: If we had a signal going to this spacecraft for the purpose of tracking, then we have the ability to command the probe. Is there any need for this?

UNIDENTIFIED SPEAKER: That is not in the baseline. There is no commandlink capability on the bus, neither the Pioneer nor the Mariner.

DR. CROFT: I realize it is not in the baseline but the baseline is something that you people have to work to. If we had two-way for the purpose of tracking, then commanding the probe is relatively straightforward.

SESSION VIII - SCIENCE INSTRUMENTS

Chairman: Mr. Joel Sperans
NASA Ames Research Center

Because this session was beginning later than planned, Mr. Sperans deleted his planned introductory remarks and introduced the first speaker, Professor A. Nier of the University of Minnesota.

N75 20399

DETERMINATION OF THE COMPOSITION OF RARIFIED NEUTRAL ATMOSPHERES BY MASS SPECTROMETERS CARRIED ON HIGH-SPEED SPACECRAFT

Professor A. Nier
University of Minnesota

INTRODUCTION - A. NIER:

As all of you know, mass spectrometers have been used in the laboratory for many years for analyzing mixtures of gases and it has been possible to analyze rather complex mixtures if one has calibrations for the individual gases. There have been dozens of sounding rocket flights which have carried mass spectrometers to the thermosphere region of our atmosphere, and there have been at least a half dozen satellites which have carried mass spectrometers for making analysis of the neutral atmosphere as well as the ionized atmosphere. On the Viking mission there will be two mass spectrometers on each of the landers. One will make measurements in the upper atmosphere and the other will be on the lander itself. The latter will make atmospheric analyses once the lander touches down on the surface and will also look for organic compounds and volatiles in the soil. The Pioneer Venus program will have mass spectrometers both on the entry vehicle and large probe, and on the orbiter. As can be seen, mass spectrometers are playing an important role in the space program, and for good reason, especially in those missions where one doesn't know what is present. Where there are unknown mixtures,there's probably not a more versatile tool than a mass spectrometer. One has enormous dynamic range and can detect very rare constituents in the presence of much more abundant ones. Unlike many methods which may be sensitive for particular classes of compounds, the sensitivity is roughly the same for all compounds.

Today I want to talk about the use of mass spectrometers carried on high speed vehicles through rarified atmospheres. Following what Don Hunten mentioned yesterday, if you are going with a probe to a planet surface, one should take advantage of the opportunity to make measurements as one approaches the planet. Tying measurements in the thermosphere to those in the lower atmosphere provides valuable information concerning atmospheric processes.

Making quantitative measurements with mass spectrometers carried on high speed vehicles poses certain problems. I want to discuss solutions to some which arise as one passes through a rarified atmosphere. Other speakers will discuss measurements in more dense atmospheres.

THE OPEN SOURCE ATMOSPHERE EXPLORER MASS SPECTROMETER

In our work we have been using magnetic deflection instruments for performing mass analysis. The ion sources are of our own design and the mass analyzer employs the familiar Mattauch-Herzog geometry. Figure 8-1 is a schematic drawing of the instrument we have provided for the Atmosphere Explorer satellites C, D, and E. Ions are produced by an electron beam moving perpendicular to the figure. It is represented by the black dot between the two bar magnets M which collimate the beam.

If the instrument moves to the left, the ambient gas entering the instrument is equivalent to a beam toward the right as shown. For an earth satellite such as Atmosphere Explorer-C the beam consists of a stream of particles having a unidirectional component of velocity of 8.5 km/sec to the right and an omnidirectional component corresponding to a Maxwell-Boltzmann distribution having an average speed of about 1 km/sec. Particles entering the region between the magnets M will be ionized, some directly as they pass through the electron beam, others after they have struck surfaces and are slowed down. Ions formed are accelerated toward the slit S_1, in part due to a repelling field between grid 3 and the assembly Sh, and in part due to an attracting field between Sh and the focusing plates J_1 and J_2.

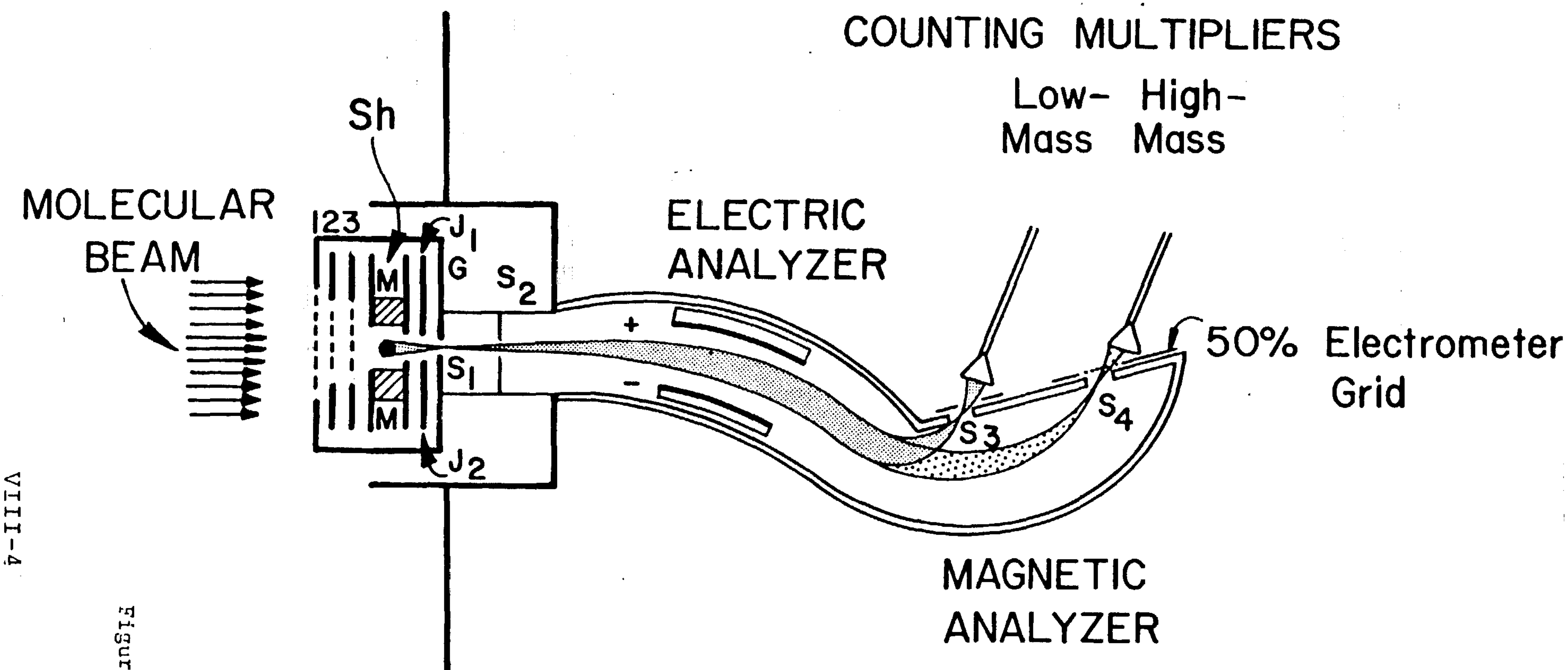


Figure 8-1

ATMOSPHERE EXPLORER OPEN SOURCE
MASS SPECTROMETER

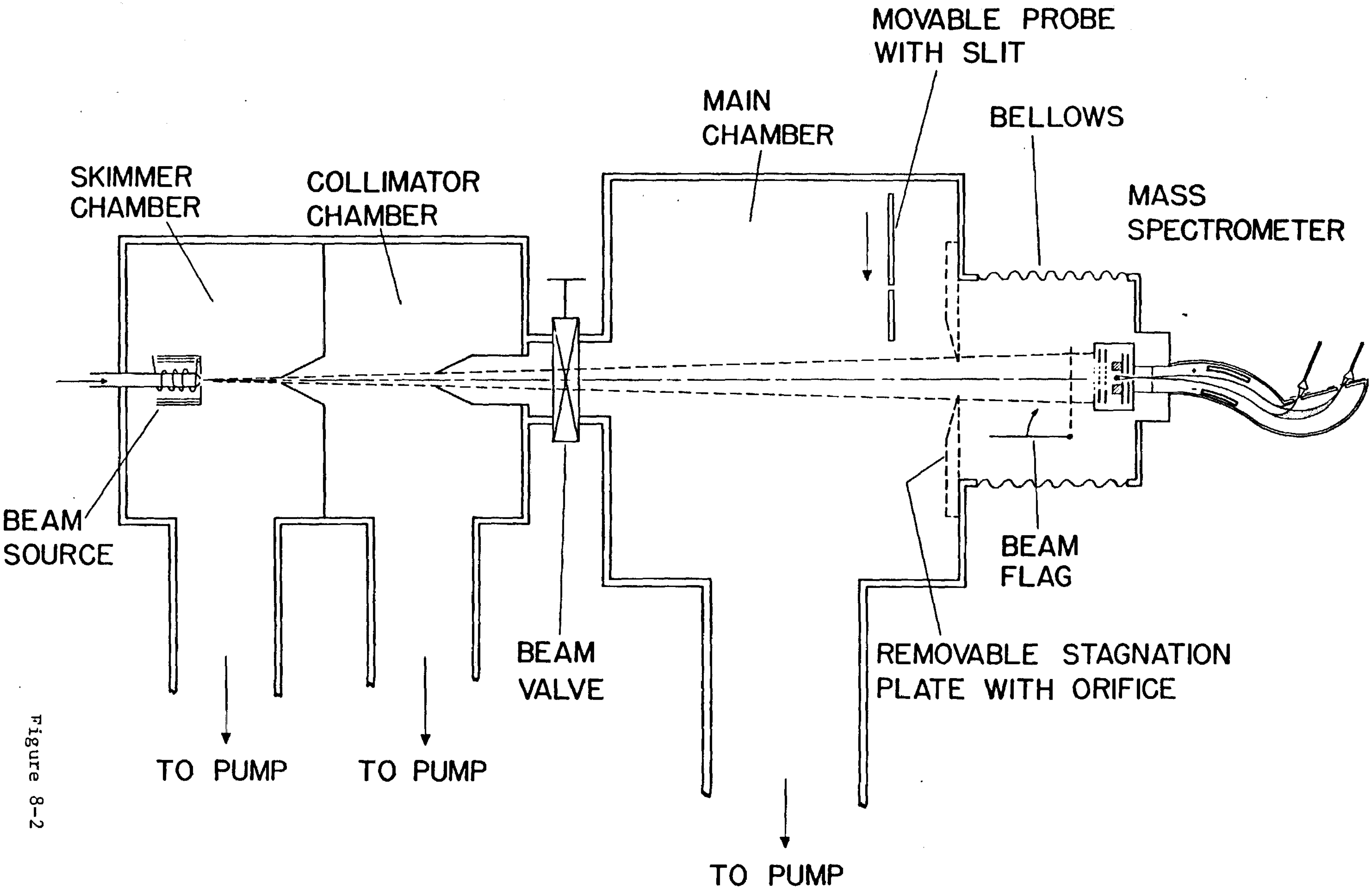
In the instrument shown there are two collectors beyond the slits S_3 and S_4, making possible the simultaneous collection of ions differing in mass by a factor of 8. Mass spectra are swept by changing the total accelerating potential applied to the ions along with the field in the electric analyzer. In the case of missions to comets where the atmosphere is extremely tenuous the two multipliers could be replaced by a channel multiplier array, making possible the simultaneous collection of many masses. A practical instrument such as discussed can be built to weigh 6 kg or less and consume under 5 watts of power.

MASS SPECTROMETER PERFORMANCE IN HIGH SPEED MOLECULAR BEAMS

Last year (thanks to the cooperation of Prof. J. B. French and his colleagues) we had occasion to test one of our Atmosphere Explorer open source instruments in the high speed molecular beam facility of the Institute for Aerospace Studies of the University of Toronto. Figure 8-2 is a schematic view of the test facility. The high speed beam is produced by the free expansion of a low molecular weight carrier gas (helium in our case) seeded with a small amount of argon and CO_2, the gases of interest to us in our tests. The mixture leaves the heated ceramic tube through a pinhole as shown to the left in the figure. After passing through a skimming and collimating chamber, the beam impinges on the mass spectrometer attached to the main chamber as shown.

The response of the instrument to different angles of attack could be checked by bending the bellows. When the beam flag was rotated into place, the background in the chamber could be measured. When the stagnation plate was slid into place, the bellows chamber became an idealized stagnation chamber, making possible a check of the extent to which the ion source departed from an idealized closed source.

C-8



MOLECULAR BEAM FACILITY

VIII-6

Figure 8-2

CurveA of Figure 8-3 os a typical mass spectrum obtained when a 3.9 kg/sec beam impinges on the ion source. One sees peaks corresponding to CO_2 and Ar as well as impurities such as O_2, N_2, H_2O and some hydrocarbons. When the beam flag is placed in front of the source, one obtains curve B, corresponding to the background in the chamber due to impurities present in the system as well as the scattered molecular beam. As can be seen, for beam particles the background accounts for only about 20 percent of the readings. It is interesting to note that for O_2, H_2O and the hydrocarbon impurities the A and B curves coincide, showing that these gases are due entirely to impurities in the chamber, none being present in the beam.

From a comparison of the A-B difference and the spectrum obtained when the stagnation plate was in place while the flag covered the source, it was possible to show that for Ar and CO_2 the ion source itself behaved as if it were 96 percent stagnated. In other words, the laboratory measurements predicted that the source as designed, when exposed to an ambient atmosphere of heavy gases, would give essentially the same readings as an ideally closed source with a knife-edged orifice. For helium the readings were somewhat lower, showing that this light gas is not completely accommodated upon collision.

MASS SPECTROMETER PERFORMANCE IN FLY-THROUGH MODE

The availability of the high speed molecular beam made possible tests not previously undertaken. In particular, if grid 3 and the focusing plates J_1 and J_2 are tied to the assembly Sh, there is no field drawing ions out of the region where they are formed, and the instrument is in the retarding potential, or fly-through mode. In this case, incoming gas molecules which strike the ion source and are accommodated have only the energy characteristic of the ion source surface temperature, a few hundredths of an electron volt. On the other hand, those particles which have not struck surfaces have an energy characteristic

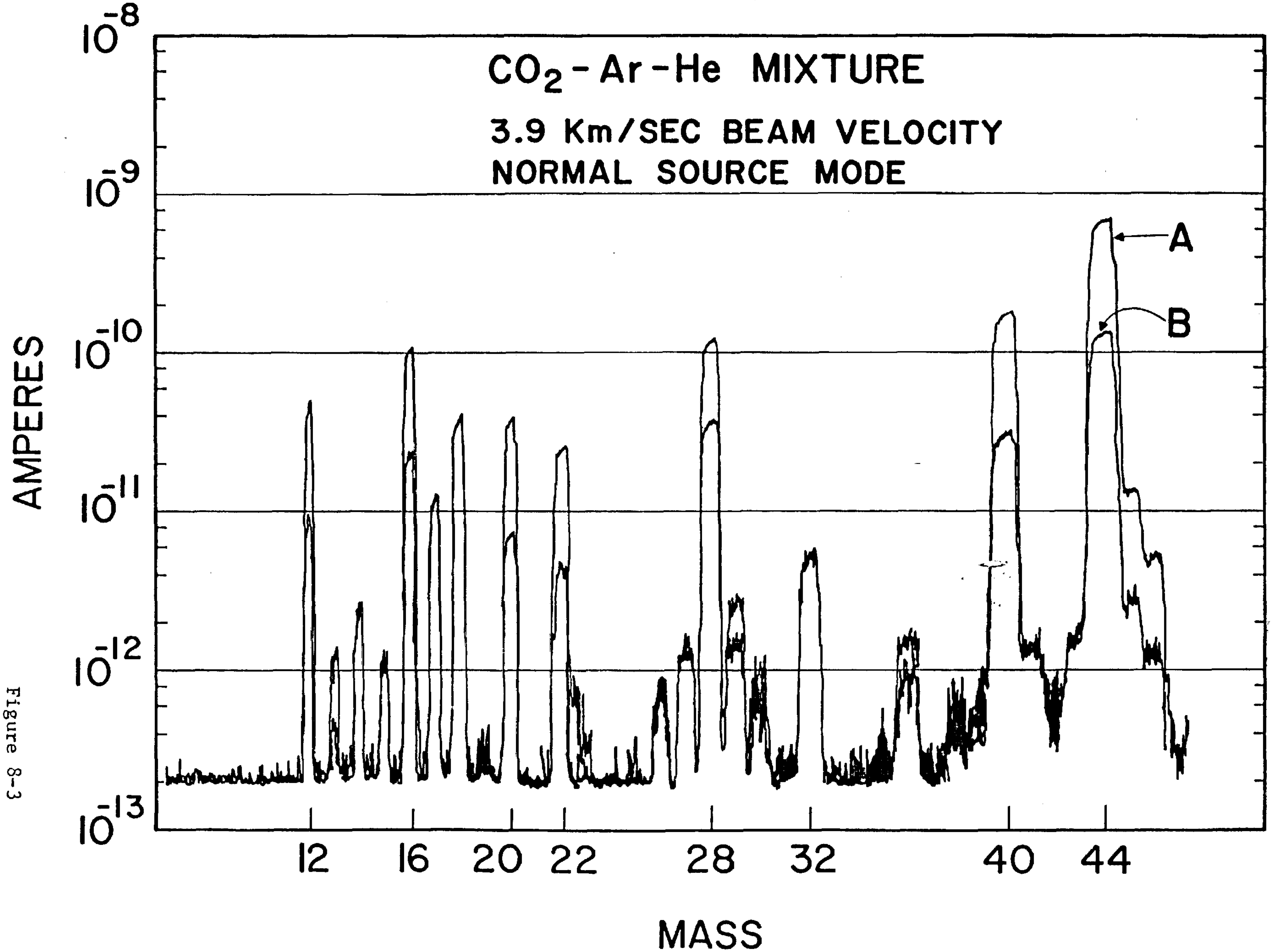

CO$_2$-Ar-He MIXTURE
3.9 Km/SEC BEAM VELOCITY
NORMAL SOURCE MODE
A
B
AMPERES
10^{-8}
10^{-9}
10^{-10}
10^{-11}
10^{-12}
10^{-13}
12
16
20
22
28
32
40
44
MASS

Figure 8-3

of their velocity in the beam, about 0.09 eV per atomic mass unit of mass for a 3.9 km/sec beam. This initial energy is enough to permit them to pass into the accelerating region after being ionized by the electron beam. In other words, in the fly-through mode the instrument, when carried on a high speed spacecraft, has the capabilities of distinguishing between true ambient particles and ones which have hit the instrument's surfaces and become thermalized or altered in nature.

Figure 8-4 shows spectra corresponding to those shown in Figure 8-3 obtained when the instrument was in the fly-through mode. It is interesting to note that except for the 12, 16 and 28 peaks the background curve F_B is zero, showing that the instrument indeed discriminates sharply against particles which do not have the energy of the beam. The fact that the background at 12, 16 and 28 is not zero comes about because these peaks are fragment ions produced by the dissociation of the background CO_2. In the ionization process they acquire kinetic energy. Hence these fragments are not excluded. The 14 peak is due entirely to background N_2 in the chamber and as in the case of the CO_2 fragments, acquires kinetic energy in the dissociation and ionization process. As will be discussed later, ambiguities due to the energetic fragments can be eliminated.

The beam tests just discussed were conducted in time to include the fly-through feature in the instrument carried on Atmosphere Explorer-C launched in December 1973.

APPLICATION OF FLY-THROUGH FEATURE TO ATMOSPHERE EXPLORER MEASUREMENTS

The determination of the absolute densities of atomic and molecular oxygen by mass spectrometers carried on sounding rockets and satellites has been the subject of some controversy. In the case of open source instruments carried on rockets, it was recognized that atomic oxygen was lost by reactions with instrument

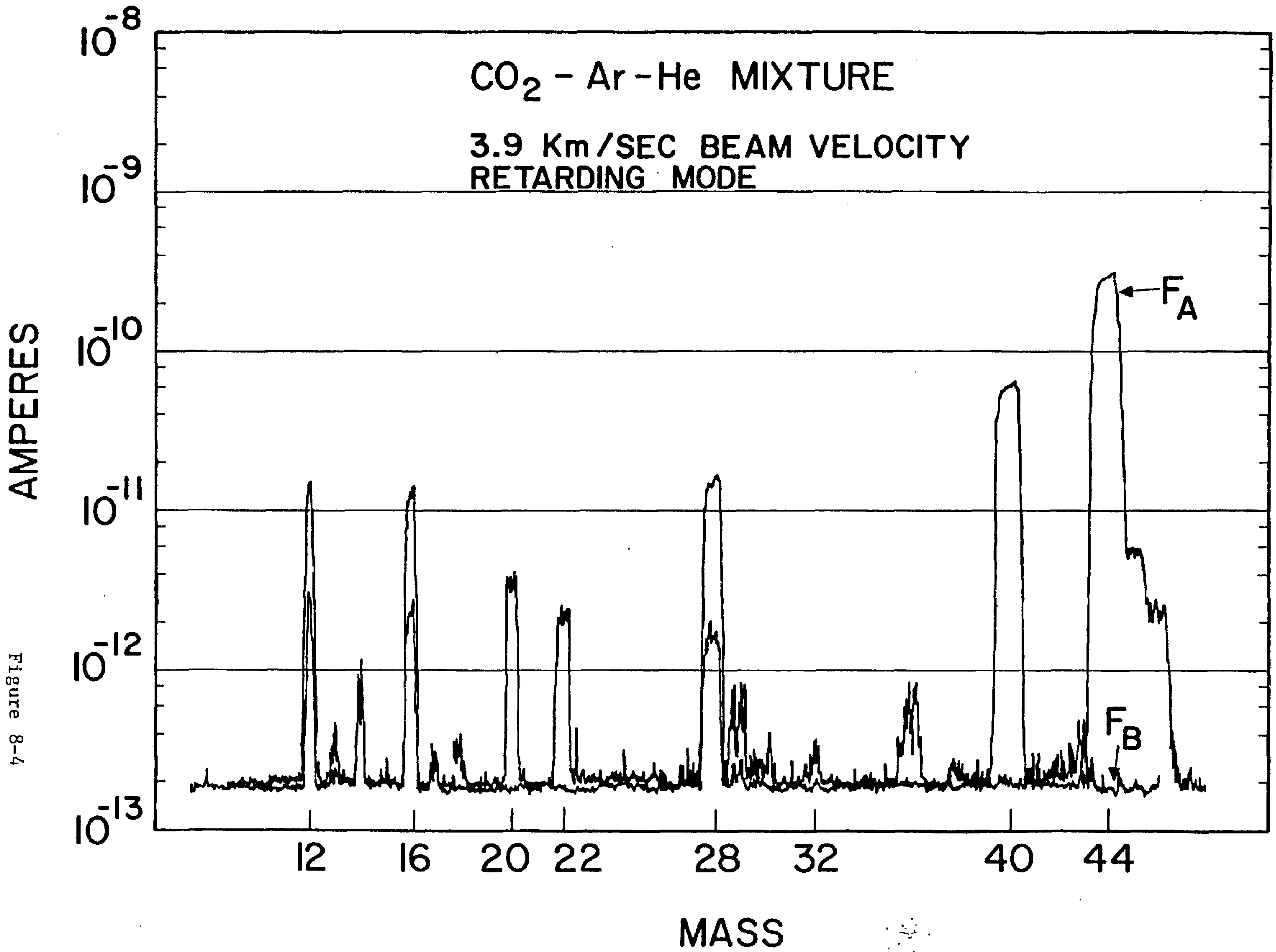

CO2 - Ar - He MIXTURE
3.9 Km/SEC BEAM VELOCITY
RETARDING MODE
AMPERES
10-8
10-9
10-10
10-11
10-12
10-13
FA
FB
12
16
20
22
28
32
40
44
MASS

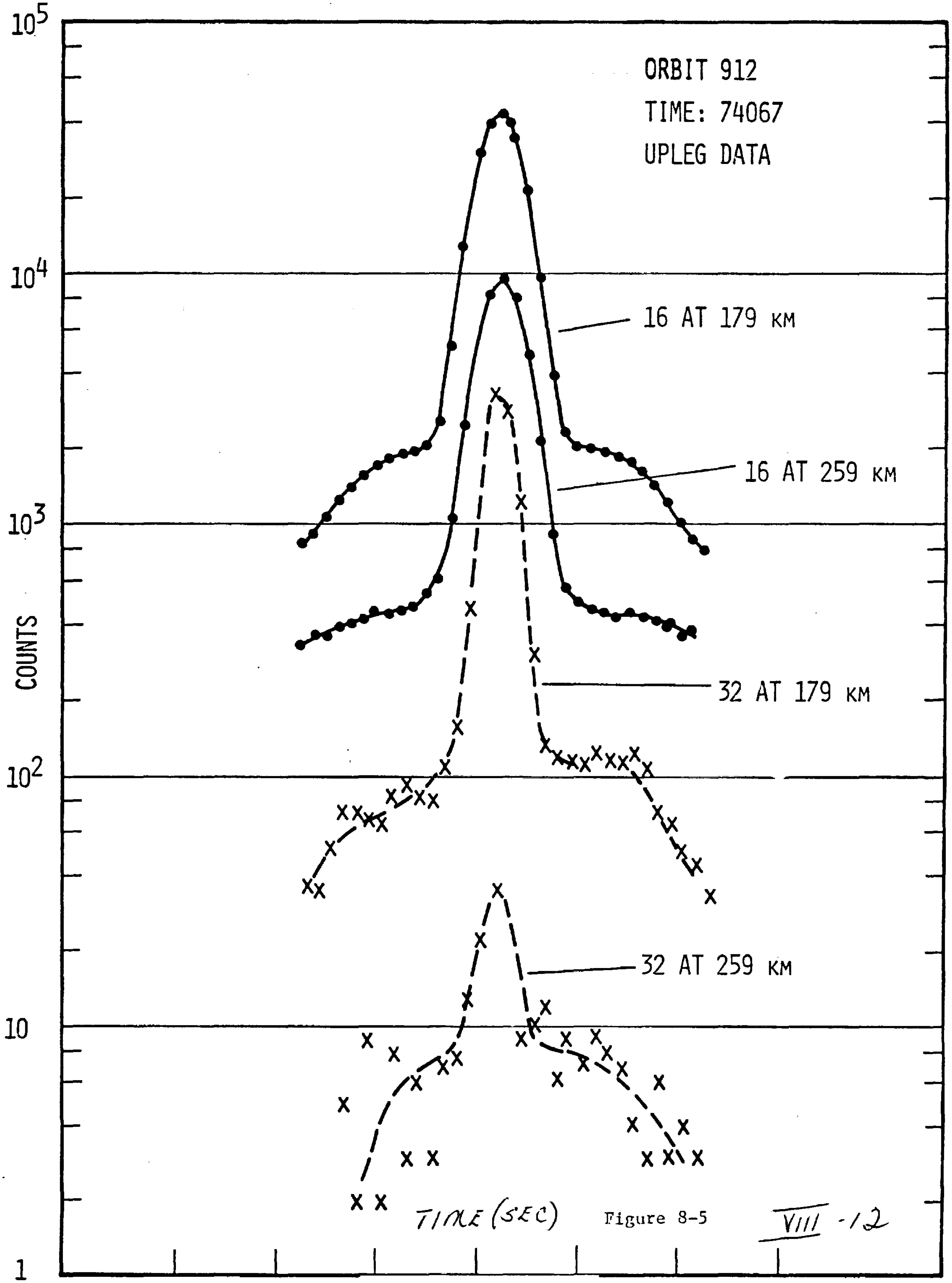
Figure 8-4

surfaces but the extent of the loss was not clear. In the case of closed source mass spectrometers carried on satellites, it was found that after several orbits all of the atomic oxygen was converted to molecular oxygen by reactions on the walls of the cavity enclosing the source. While this made possible the quantitative measurement of atomic oxygen at high altitudes where ambient molecular oxygen was negligible, the method merely gave total oxygen in the interesting region of the atmosphere where atomic and molecular oxygen have comparable abundances.

As has already been mentioned, in its normal mode of operation the Atmosphere Explorer open source mass spectrometer performs essentially as a closed source instrument. In this mode it gives quantitative values for number densities of N_2, Ar, He and total oxygen. When switched to the fly-through mode, it distinguishes between atomic and molecular oxygen, giving absolute number densities for each. Figure 8-5 illustrates the performance in this mode. The mode is particularly applicable when the spacecraft is spinning at its normal spin rate of 1 revolution per 15 seconds and the instrument is set to toggle back and forth between masses 16 and 32 rather than look at a large number of masses.

Multiplier counts are accumulated for 1/16 sec while the instrument is set to collect mass 16. It then counts mass 32 ions for 1/16 second, switches back to mass 16 for 1/16 second, etc. The results are shown for two different altitudes of orbit 912 as the instrument passes through the forward looking direction as the spacecraft spins. Particle densities are roughly proportional to count rates.

The figure illustrates a number of interesting points: (1) the 16 peak is always greater than the 32 peak at the same altitude, as it indeed should be in the altitude range shown, (2) in going from 179 to 259 km the 32 peak falls



ORBIT 912
TIME: 74067
UPLEG DATA
16 AT 179 KM
16 AT 259 KM
32 AT 179 KM
32 AT 259 KM
COUNTS
10^5
10^4
10^3
10^2
10
1
TIME (SEC)

Figure 8-5

off very much faster than the 16 peak, as it should due to the large difference in scale heights, (3) the background of a few percent of the peak values near the "forward" direction is due primarily to incomplete rejection of slow particles. By operating the instrument with low electron accelerating potentials, 25 volts in our case, the production of energetic fragment ions such as O from O_2 is reduced. Since they are made from O_2 produced in part by chemistry in the source, they do not contribute to the sharp peak when the instrument looks forward, and merely add to the background, (4) while part of the width of the peaks is due to the finite acceptance angle of the instrument, the largest part is caused by the fact, which was mentioned earlier, that the "beam" seen by the instrument has a unidirectional component having the spacecraft velocity of 8.5 km/sec and an omnidirectional component of roughly 1 km/sec average velocity corresponding to the Maxwell-Boltzmann distribution at the thermospheric temperature of about 900°K. As the spacecraft spins, particles can thus enter over an angle of a number of degrees. It is interesting to note that at half height the width of the 16 peak is approximately 2 1/2 wider than the 32 peak, as it should be because of the difference in average Maxwell velocities of the two species in the atmosphere. It appears that with proper calibration, the width of the peaks can be used to deduce *in situ* atmosphere temperatures.

Figure 8-6 gives the plot of peaks such as shown in Figure 8-5 as a function of altitude, and from the relative scale heights provides additional proof that the peaks as read in the fly-through mode are indeed due to the ambient atmosphere uncontaminated by wall collisions effects. The count rates are reduced to ambient number densities through laboratory calibrations supplemented by calibrations in orbits where fly-through readings are interspersed with readings in the normal mode.

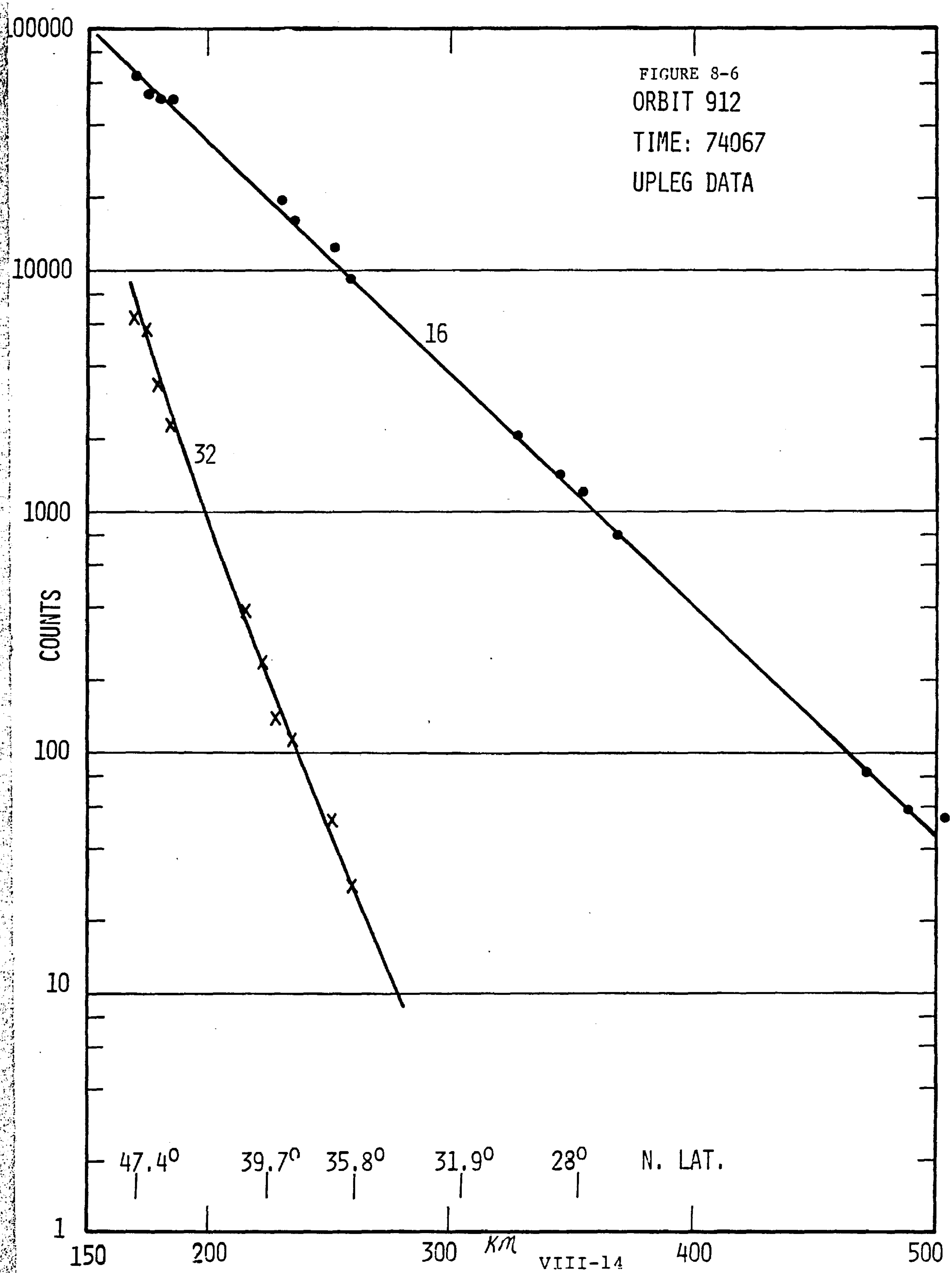


FIGURE 8-6
ORBIT 912
TIME: 74067
UPLEG DATA
16
32
COUNTS
100000
10000
1000
100
10
1
47.4°
39.7°
35.8°
31.9°
28°
N. LAT.
150
200
300
KM
400
500

CONCLUSIONS

In a properly designed open source mass spectrometer one can operate in both a "normal" mode and in a mode in which particles arising from collisions with instrument surfaces are excluded. In instruments carried on high speed spacecraft such as will be sent to the unknown atmospheres of other planets or comets this feature is of considerable importance in making a distinction between the true ambient atmosphere and gases which arise as the result of chemical reactions on instrument surfaces.

An example is given in which atomic and molecular oxygen are distinguished by the open source mass spectrometer carried on the Atmosphere Explorer-C satellite.

A MASS SPECTROMETER CONCEPT FOR IDENTIFYING PLANETARY ATMOSPHERE COMPOSITION

Dr. Nelson W. Spencer
NASA Goddard Space Flight Center

N75 20400

DR. SPENCER: Professor Nier has introduced the subject very nicely and told you a lot of details of these systems. I'd like to use my few minutes to speak to a few principles pertinent to some of the considerations which guide people in using mass spectrometers for atmospheric measurements.

The basic problem is not a new one. It is to try to get a sample of the atmosphere and measure it without modifying it. Atomic oxygen is a good example. In most instruments it recombines on the surfaces and is measured as O_2, which is acceptable if the ambient O_2 is negligible (true for most cases). Thus, if you didn't know that there was atomic oxygen up there in the first place, you might conclude that molecular oxygen was present until your fundamental physics told you otherwise. That's fine for the earth, but when we go into other atmospheres, we don't really know what is there, and then it is not quite so obvious. I think that the discussions yesterday, particularly those concerning Jupiter and the trace constituents emphasized the point and illustrate the situation that we find ourselves in, and that is how do we really analyze a sample of the atmosphere in a rather brutal way, which is what the mass spectrometer does, without changing its composition. So, getting a sample is a challenging task, a concern, a consideration that one must be aware of. Obviously, the other things that are a little more apparent in considering a design are the dynamic range that the instrument must have, the mass range that the instrument must cover, the precision of the measurements that are necessary for example, to confirm isotope rations.

A number of systems have evolved, and I want to use some of our more recent work on Pioneer Venus as an example to illustrate some of the problems.

This diagram may not surprise you very much, but it is quite fundamental. (Figure 8-7). Basically, we need some arrangement to sample the atmosphere. We use the term "sample" in the very broadest sense; whether you take a parcel of gas and bring it into the instrument and analyze it or whether it flows through the instrument in the sense that Professor Nier was speaking about really depends on the particular application. Fortunately, in the upper atmosphere, in satellite usage, one can take a sample directly into the ionization region of the mass spectrometer without it having experienced surface collisions and analyze it with perhaps what you might consider the minimum amount of modification. However, the atmospheric sample is not the only gas observed in the source because the surfaces produce gases as well. If you can use the energy of the particles as a differentiator, then you have a very nice tool for differentiating between the particles which are of spacecraft origin or mass spectrometer origin and atmospheric origin. When one goes to lower atmospheres, and I am going to speak generally about more dense atmospheres, then that tool is not available and the chemical effects in the ion source are more difficult to avoid. The sample inlet system that is represented in this block diagram reflects those portions of the system which conduct a sample of the atmosphere, whether it be a batch or a continuous flowing gas, into the ion source of the instrument.

These systems will in general have pumps. There are a variety available, the kind to be used depending upon the particular atmosphere. For Venus, where the atmosphere is dominated by other than inerts, Getter pumps are very handy devices. Ion pumps are useful as well for controlling the inerts.

Most of our activities concentrate on guadrupole analyzers as shown. The rest of the figure should be quite familiar to you.

GENERAL SYSTEM OUTLINE

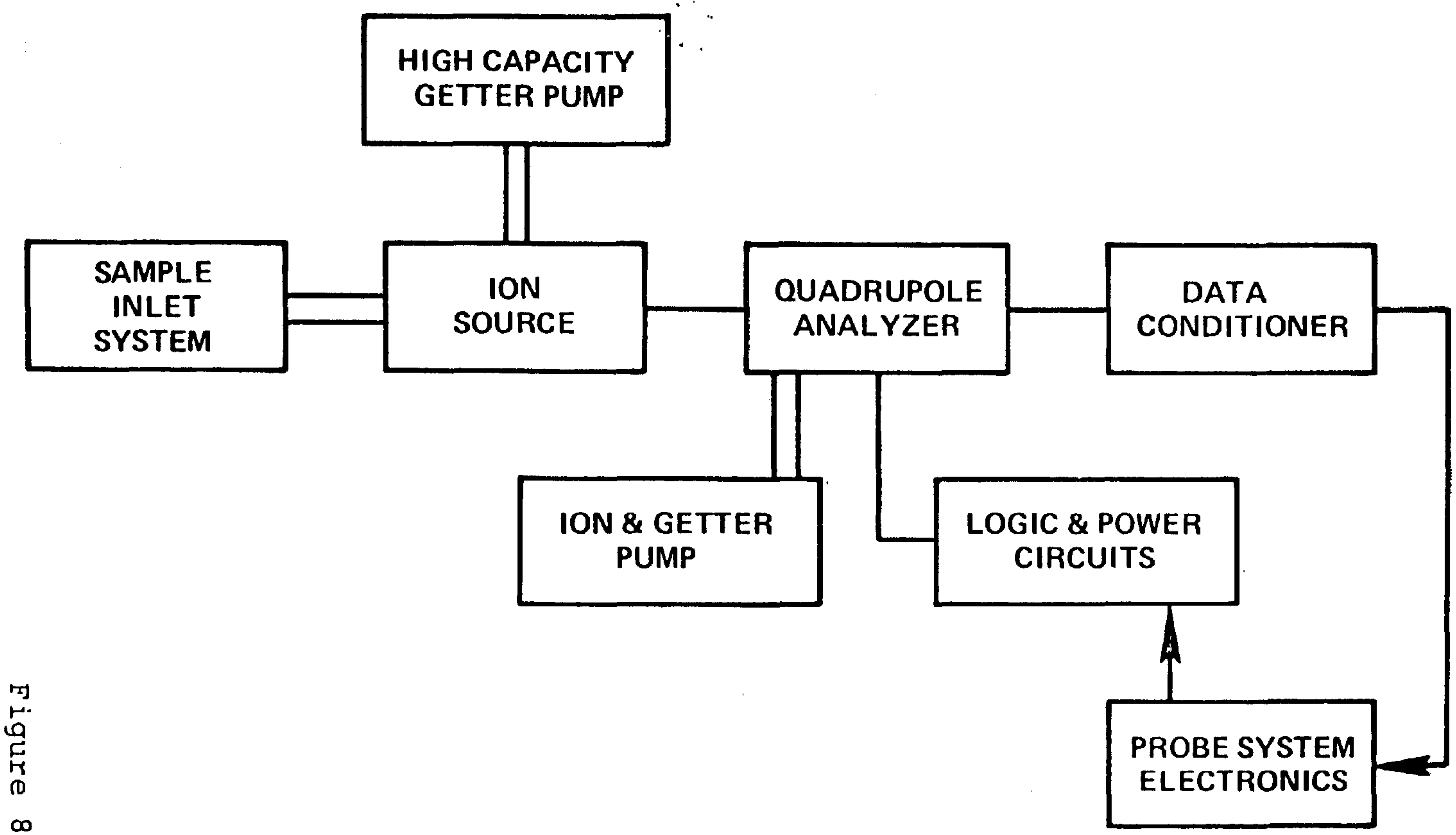


Figure 8-7

The next figure (Figure 8-8), shows what a typical system may look like. The quadrupole and the other elements of the analyzer are shown. In general, the analyzer portion is separated from the ion source region by a relatively low conductance ion orifice. It has its own pumping system to maintain the background gas at a suitably low level. The left portion of the slide shows the ion source and the inlets which are closely associated physically because it's desireable to minimize the amount of surface that is exposed to the gas. The pump (and leaks) are sized to maintain an adequate flow of gas through the ion source. Also shown are three inlets which will be discussed later.

The next slide shows typical weights corresponding to the block diagram (Figure 8-9). It must be noted, however, that these weights are mission dependent. For example, the structure that is required to support the various elements of the instrument will vary from mission to mission and is necessarily closely associated with the sample system.

Although the next figure (Figure 8-10) is a rather poor reproduction, it illustrates a typical instrument installation with sample tubes projecting through the probe wall. In Pioneer Venus, there are some particular temperature and structural problems which require special consideration. The acceleration forces must be supported in some manner by the elements of the system, and that's where some of the weight appears that is not particularly defineable, but which I classify as mission dependent weight.

(Figure 8-11). I mentioned that it is necessary to accommodate to a rather wide range of pressures in the instrument when descending through an atmosphere to the surface. At the same time, it is necessary to optimize, for dynamic range purposes, the pressure in the ionization region. There are a number of devices that can be used to reduce the atmospheric sample to an acceptable pressure level for the mass spectrometer. At the same time, one is concerned about the particular material that

BLOCK DIAGRAM OF PUMP SYSTEM

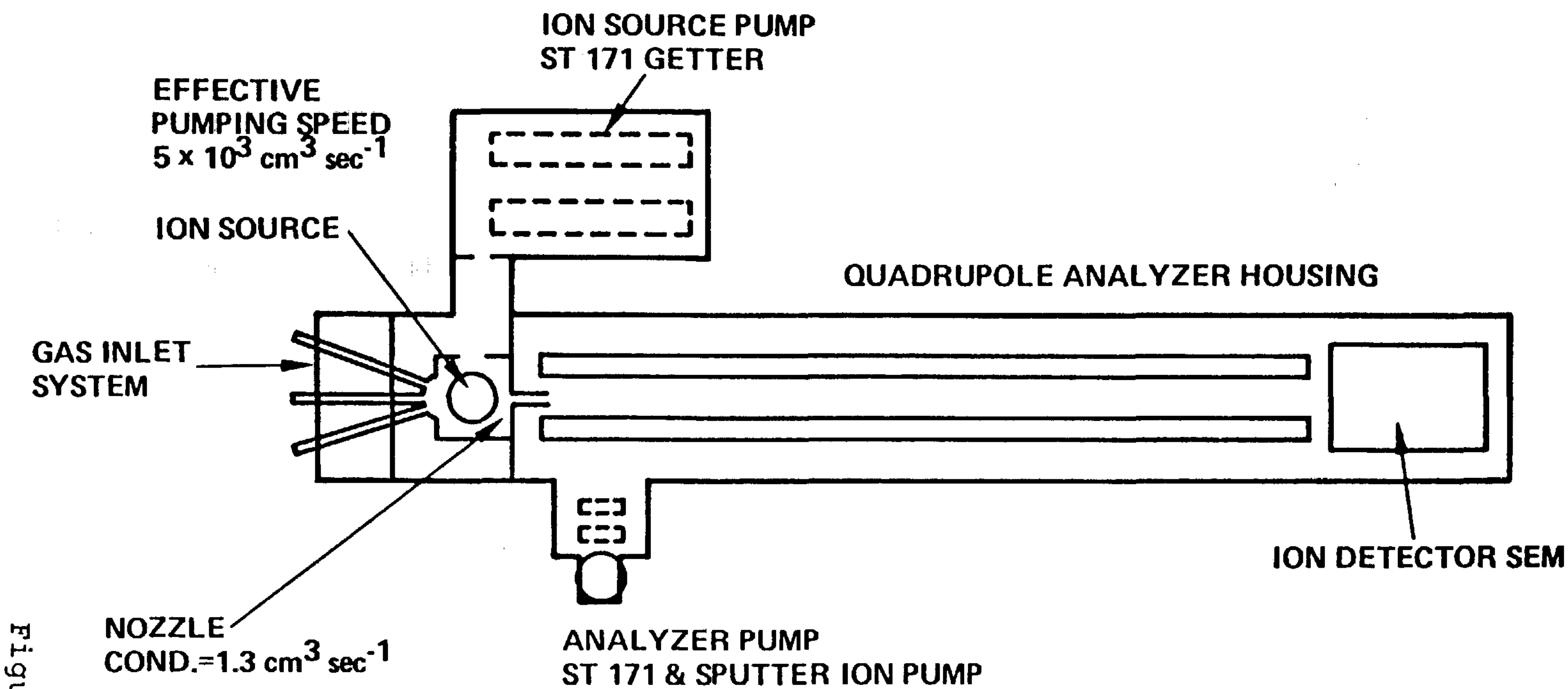


Figure 8-8

INSTRUMENT WEIGHT BREAKDOWN

	KG	LBS
SAMPLE INLET SYSTEM	0.9	2.0
MASS SPECTROMETER	0.5	1.1
ELECTRONICS	1.0	2.2
MECH. STRUCTURE	1.0	2.2
PUMP	≥1.0	2.2
	4.40	9.70

Figure 8-9

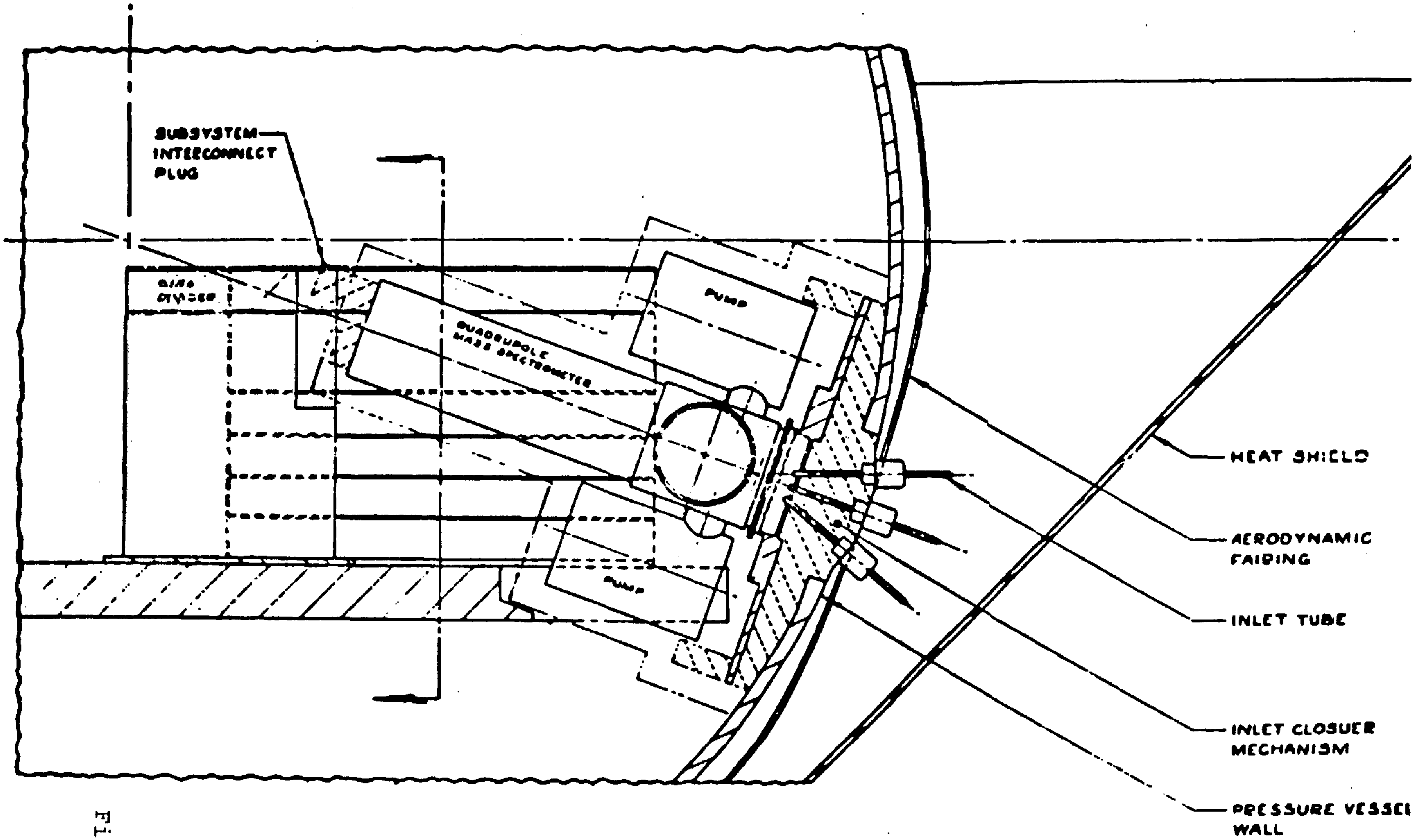

SUBSYSTEM INTERCONNECT PLUG
PUMP
QUADRUPOLE MASS SPECTROMETER
PUMP
HEAT SHIELD
AERODYNAMIC FAIRING
INLET TUBE
INLET CLOSUER MECHANISM
PRESSURE VESSEL WALL

Figure 8-10

INLET SYSTEM OUTLINE

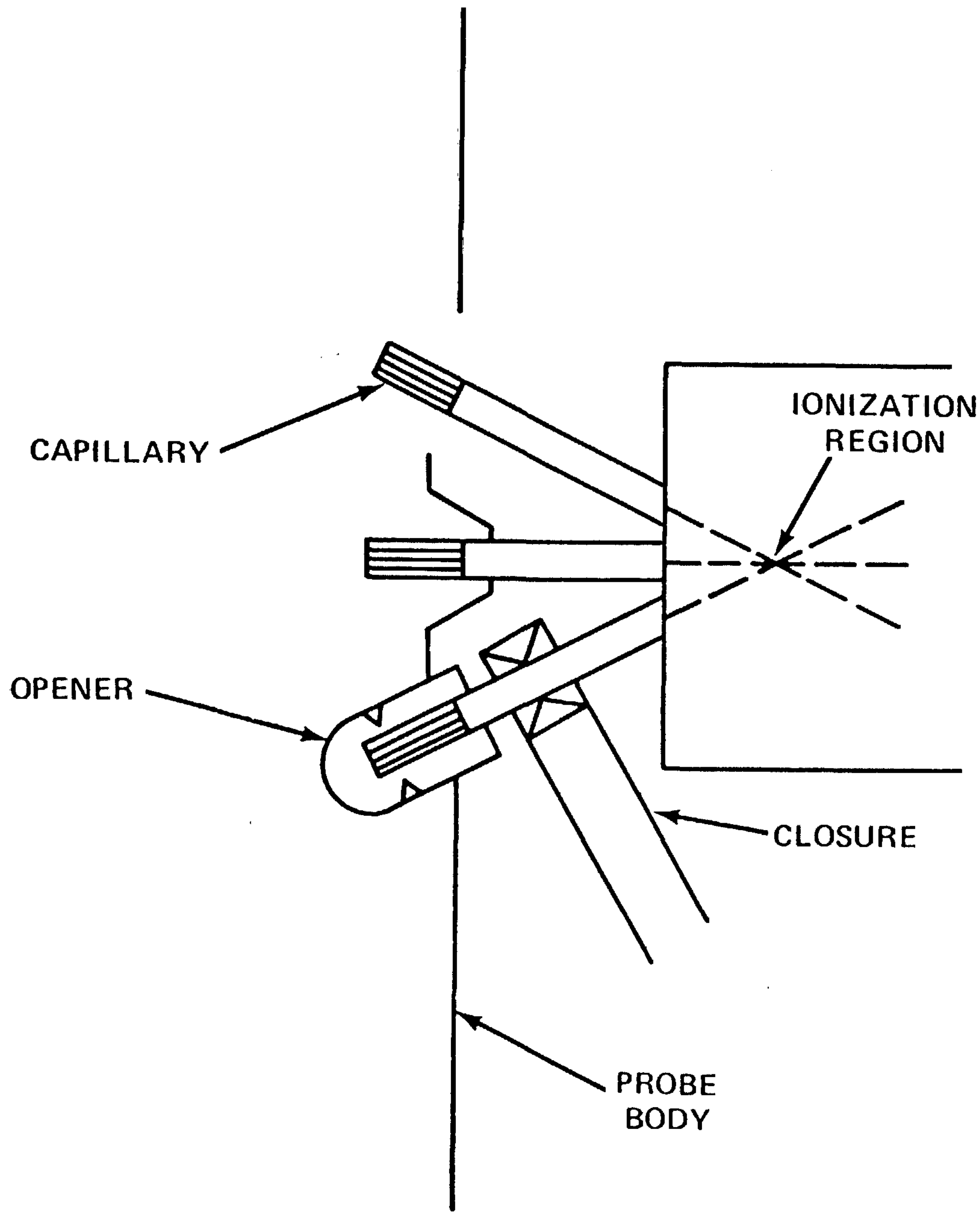


Figure 8-11

the glass is exposed to. Again, one has a choice. One can use glass, quartz, ceramic devices or metals, but the basic consideration is how do these materials react with the gases in the atmosphere.

The approach that we have taken for atmospheric probes is to provide what we believe is a fair amount of redundancy. We take a number of different batches of atmospheric gas during the descent of a probe into the atmosphere and analyze each of them individually. Three channels are shown in the slide, however, for Venus, eight is an appropriate number. The amount of time that the flow can take place for the particular sample really depends upon the particular mission. It could be either nearly continuous or brief. The system must also be very clean so one can have confidence that gases are not being carried there which will alter the analysis. The example illustrated here shows three capillaries with an opener for uncovering and exposing each. There is also a device which terminates the sample by sealing off the tube at the end of a selected flow period. Considering a number of sample tubes, the times of the various samples can be spaced through the atmosphere to accommodate for example the considerations that John Lewis was speaking about yesterday where different strata in the atmosphere might prompt one to look for different groups of gases.

Figure 8-12 illustrates one measurement scheme during a particular sample. The vertical scale represents the operating pressure level in the ionization region. In general, it is not constant, but for the purposes of this discussion, it makes little difference. I think you can see essentially what happens; at some time through an internally generated signal, the device is exposed to the atmosphere and the gas permitted to flow into the instrument. One can select, depending upon the particular altitude range or the particular localized study, scans of selected mass numbers. Scans can be continuous, where you look at

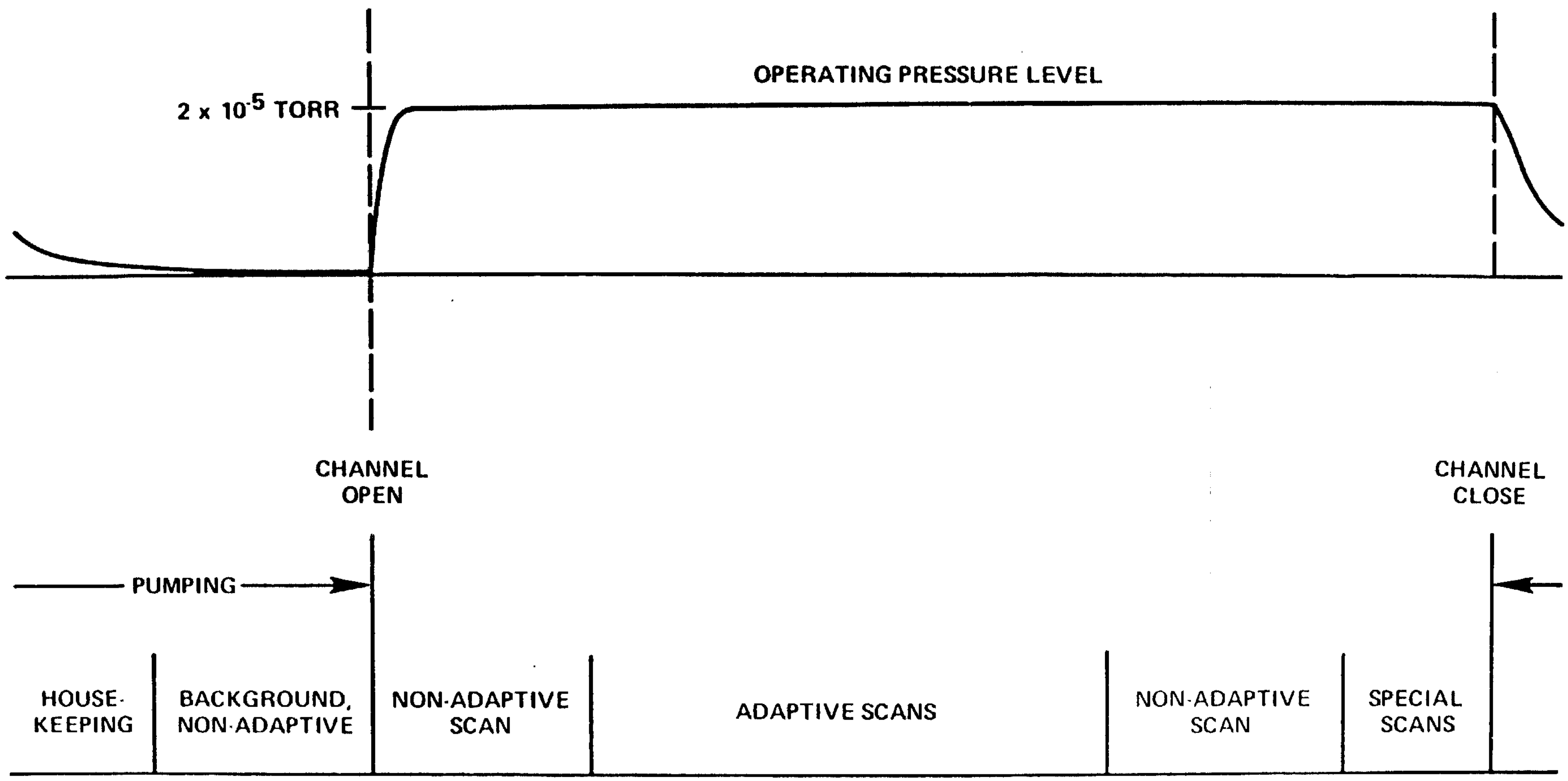

OPERATING PRESSURE LEVEL
2 x 10-5 TORR
CHANNEL OPEN
CHANNEL CLOSE
PUMPING
HOUSE-KEEPING
BACKGROUND, NON-ADAPTIVE
NON-ADAPTIVE SCAN
ADAPTIVE SCANS
NON-ADAPTIVE SCAN
SPECIAL SCANS

Figure 8-12

every mass, which I have labeled here non-adaptive, for purposes of identification. You may want to study the altitude distribution of the gases. The data system can be used a little more effectively by using an adaptive approach that looks at pre-selected masses.

At the end of some period of time, the channel itself can be closed and the instrument then sealed off from the atmosphere. The capillary in this case, or whatever the leak happens to be, is sealed off and the high-pressure gas that is now remnant in the capillary and ion source is removed. The pump system is thus able to reduce the remaining gas in the system to a background level. This is a particularly important concept, because the surfaces of the instrument of the ion source do retain gases, which must be expected, especially in an unknown and hostile atmosphere such as Venus; and presumably for other planets where there may be a number of exotic components in one form or another. They may react with and be retained by the surfaces of the ion source. One would like to know, for example, that one doesn't carry a particular gas that may result from some surface chemical reaction at one altitude to some lower altitude. This arrangement permits one to look at that background.

I included the last but didn't really intend to talk about it (Figure 8-13); however, a talk about mass spectrometers would not really be complete without showing a spectrum. People would not think that you were being very honest. This is a nest spectrum from a laboratory study that we have done that illustrates the capabilities of small quadrupoles. You can see the typical things - the number of gases and the resolution. It gives you a feel for the dynamic range of instruments and peak shapes.

I think I will close, then, with just one remark. I have been speaking to you about things that are real in terms of instruments. We, collectively, have done a lot of development over the years towards these instruments, and I think we have

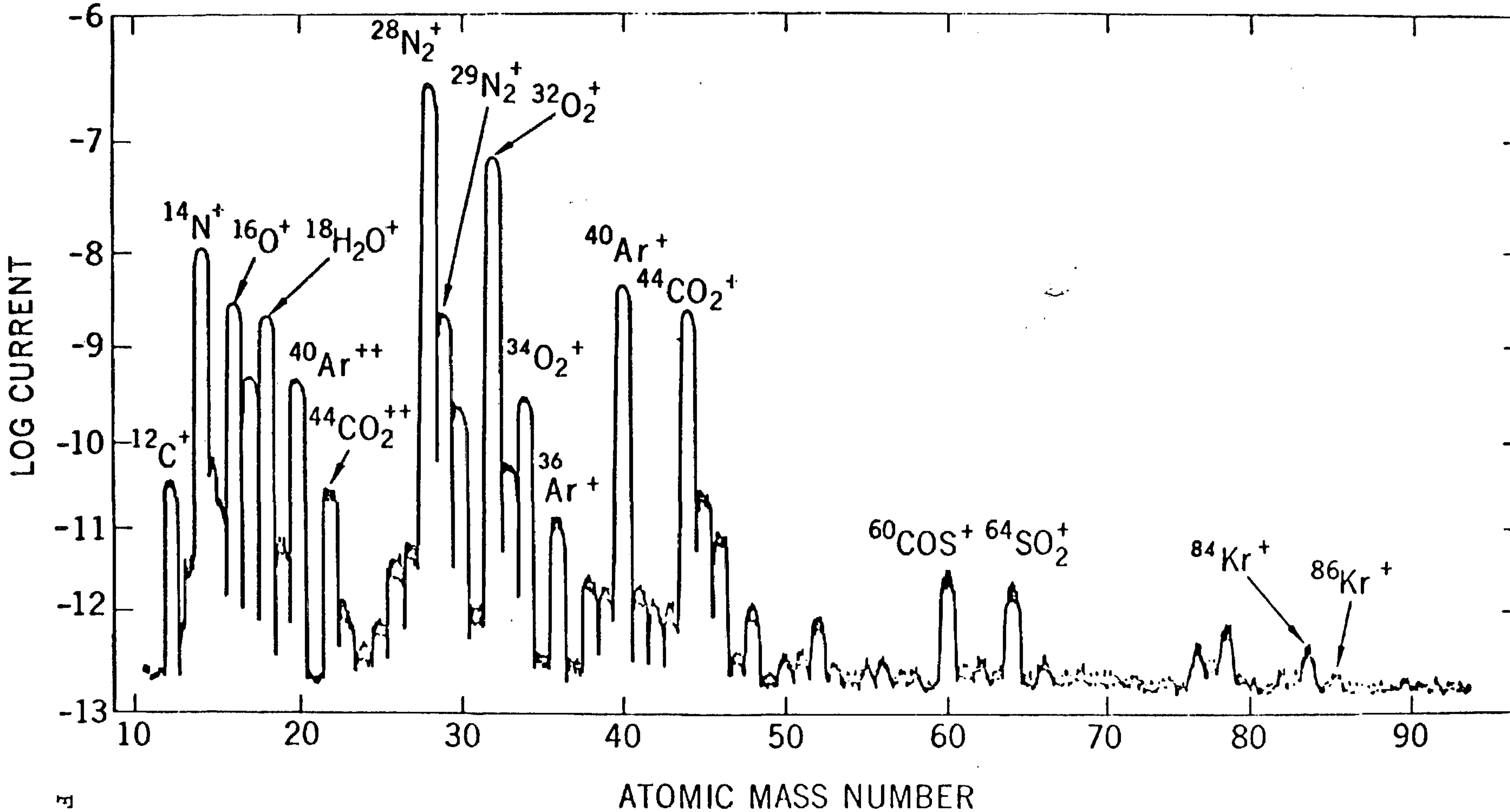

LOG CURRENT
-6
-7
-8
-9
-10
-11
-12
-13
ATOMIC MASS NUMBER
10
20
30
40
50
60
70
80
90
$^{12}C^+$
$^{14}N^+$
$^{16}O^+$
$^{18}H_2O^+$
$^{40}Ar^{++}$
$^{44}CO_2^{++}$
$^{28}N_2^+$
$^{29}N_2^+$
$^{32}O_2^+$
$^{34}O_2^+$
$^{36}Ar^+$
$^{40}Ar^+$
$^{44}CO_2^+$
$^{60}COS^+$
$^{64}SO_2^+$
$^{84}Kr^+$
$^{86}Kr^+$

Figure 8-13

come to quite an advanced state. I think we are ready for missions to the planets. These instruments are in many cases built and operating. Many of them are tested. Many of the principles have been tested and have been found lacking in some regards. The test that Professor Nier speaks about on A.E. will be carried forward also. We too will be doing a similar, but somewhat more advanced experiment on the next A.E. satellite with a system that is particularly designed for planetary upper atmosphere use. We are not speaking about what might be, we are speaking about what in fact can be, and what is being done.

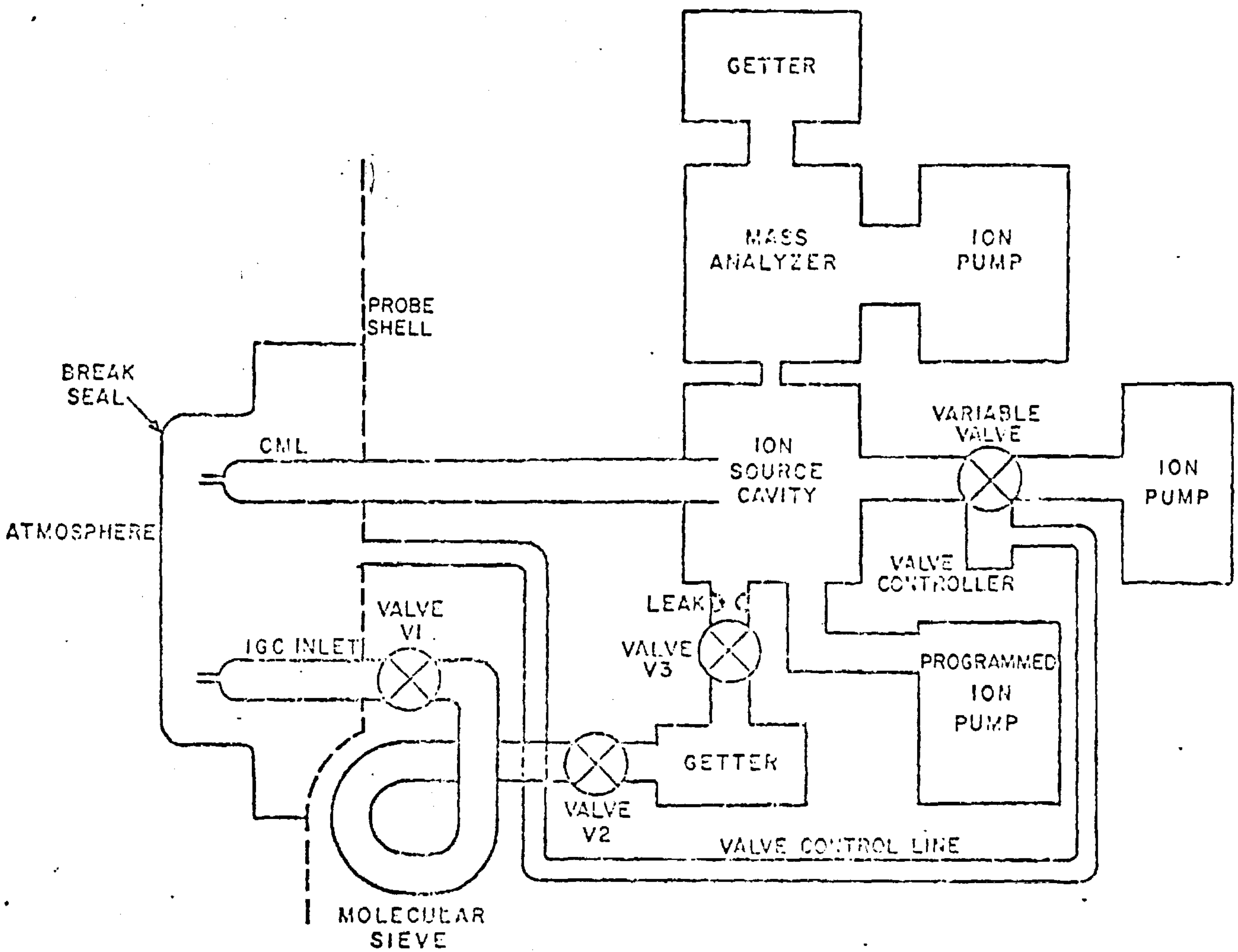
MASS SPECTROMETRIC MEASUREMENTS OF ATMOSPHERIC COMPOSITION

Dr. John H. Hoffman
University of Texas at Dallas

DR. HOFFMAN: The previous two speakers have given you two views of the usage of mass spectrometers in atmospheric studies. In addition, Dr. Spencer has spoken about various concepts of sampling the lower atmosphere from probes that descend into planets. I would like to continue in the vein that he has started and show you another system which we have been developing also for the Pioneer Venus program, and how it might be adapted to the outer planet probe studies which this conference is discussing.

Figure 8-14 shows a schematic drawing of such a system. The basic parts are the inlet, the pumping, and the mass spectrometer systems. We have proposed for Pioneer Venus and do so here, a continuous approach to sampling the atmosphere, whereas the previous speaker chose a batch approach, taking one sample, analyzing it and exhausting of pumping it out, and then at some time later taking in another sample. Our approach involves a continuous sampling and analysis of the atmosphere as the probe descends down to the surface. Its basic element is a leak, which is called a ceramic micro-leak, or CML, which protrudes outside the shell of the probe and into the streaming atmosphere as the probe descends to the surface. The gases are admitted through that leak which drops the pressure from the outside atmospheric pressure, which can be as high as ten or twenty bars, or even higher, to that required to operate the ion source in a single stage. In the case of Venus, these devices have been tested up to almost two hundred bars. The gas passing through the leak then travels through a very short, straight tube right into the ion source cavity, wherein ions are formed by electron bombardment. The ion beam is drawn out through a narrow slit into the mass analyzer. In our case, we propose a magnetic sector field analyzer, the same thing as Al Nier has shown you. The mass analyzer gives a quantitative determination of those gases in the ion source cavity.

Some of the characteristics of the leak are: it is made of a flattened stainless steel tube which has been oxidized on the



10 Bar Atmospheric Probe
Mass Spectrometer

Figure 8-14

inside; it can be pressed or forged together to get any given leak rate that you wish, between ten to the minus one to ten to the minus nine cc per second. This means, of course, that the leak rate can be tuned to whatever depth of the atmosphere you wish to fly. Of course, this cannot be done inflight. It must be adjusted in the laboratory ahead of time. In-flight the bases pass between the two parallel platelets of oxidized material, an oxidized metal, which is essentially a ceramic material and, therefore, the name ceramic micro leak. Owing to the inertness of the surface, there is a minimum change in the composition of the gas as it passes through the leak. The volume of the leak and its surface area are very small making the time response of the leak very small compared to the settling time of the probe in the atmosphere or the time of the sweeping of the mass spectrum, which will be discussed later.

Another part of the system consists of a pumping mechanism which in this case is an ion pump because of the expected large amount of helium in the entry planet atmospheres whereas on Venus the rare gases seem to be a very negligible part of the atmosphere. These gases do play an important role, but are negligible from the pumping standpoint. We have chosen here to use a constant speed pumping system and a variable valve, or variable orifice, which is controlled by the atmospheric pressure being fed in through a control line. It is sort of a pneumatic type valve. The conductance of the valve is directly proportional to the atmospheric pressure. This, then, maintains a constant pressure in the ion source, which has the great advantage of giving a wider dynamic range to the measurements. We actually obtain about ten to the seventh in dynamic range.

In addition to those parts, there is, of course, the mass analyzer, which I'll discuss more later. It is pumped separately by an ion pump which is used during entry and during pre-launch activities here on Earth, and a getter, which keeps the entire system, the vacuum part of this system, evacuated during pre-launch phase and the cruise phase to the planet, the seven years or whatever it may take to get to some of the outer planets. The capacity of this getter is quite adequate, even against small leaks into the system, even if the probe were pressurized to one atmosphere, but I understand that the plans in general call for a nonpressurized probe. I think that eliminates the need for having vent tubes from the analyzer to the outside of the probe and the complications involved there in having to close these vents reliably so that you don't get the ten to twenty bar pressure leaking back into the instrument which would be a wipeout if that happened. We have a self-contained vacuum system here which takes no power, because these getters are room temperature operated getters. They are activated prior to launch in the laboratory, prior to the delivery of the instrument to the spacecraft.

Figure 8-15* is a photograph of an analyzer that was built for another purpose, but this is just to orient you to the size and shape of instruments that are being flown these days. This is a small sector field instrument. It consists of a magnet which bends the ion beam through different allowed trajectories through the magnet. This happens to be a three-channel instrument. By that we mean that, as ions are formed up in the ion source and pass down this inlet drift tube into the magnet, three different beams are identified coming out of the magnet. In this particular case, the mass ranges of one to four, to sixteen and sixteen to sixty-four atomic mass units are scanned simultaneously by a single sweep of the ion energy as the ions are formed in the ion source. By this means, of course, one can scan a wide mass range with a very small change in the voltage of the ion source itself, namely, in this case a factor of four rather than a factor of sixty-four. The instrument that we would propose for an outer planet mission would probably have two channels instead of three, and it would scan the mass range of one to four and twelve to forty-eight and perhaps on to mass sixty if we wish to cover iron. That extra mass range is essentially full. An even wider mass range is possible but these are some of many options that are available.

*Notavailable for inclusion in these proceedings

Figure 8-15

The instrument is packaged inside an eight inch diameter circle, and you can see it takes a very small total area of that circle. It can be packaged very readily, I would say, inside of the probe, as we saw yesterday.

The dynamic range, as I mentioned before, is approximately ten to the seventh. This is obtained as follows: we use an ion counting technique to detect the ions. We use electron multipliers which could be spiraltrons, magnetic strip type or venetian blind or Allen type multipliers. The counting rate that one can obtain effectively from these devices is something a little over 10^5. That is the dynamic range of counts. In order to increase this to ten to the seventh, we use a little trick in the ion source. Where we find that we are coming up on a peak with a very high counting rate, one that is over some preset threshold, we automatically decrease the sensitivity of the ion source, cutting this by two orders of magnitude, and then count that specific peak at the lower sensitivity. This then expands the dynamic range and we can get seven decades. We can very nicely see one part per million species.

Figure 8-16 gives a few of the specifications of the mass spectrometer. I have talked about some of these already. We use a dual filament arrangement in the ion source just for redundancy. We have a multi-electron energy capability here whereby we can bombard the gases in the ion sources with different energy electrons. I will show you the effect of that a little later. The detectors have been discussed already. The ion source pressure is maintained in, say, the high ten to the minus six torr range, because this is a good range to get the sensitivity we mentioned and does not produce too much pressure scattering of the beam in the ion source. The analyzer is maintained at a very low range so that the peak shapes are very well confined. There are no significant tails, and one can effectively use the dynamic range that is available.

NEUTRAL MASS SPECTROMETER

DESIGN SPECIFICATIONS

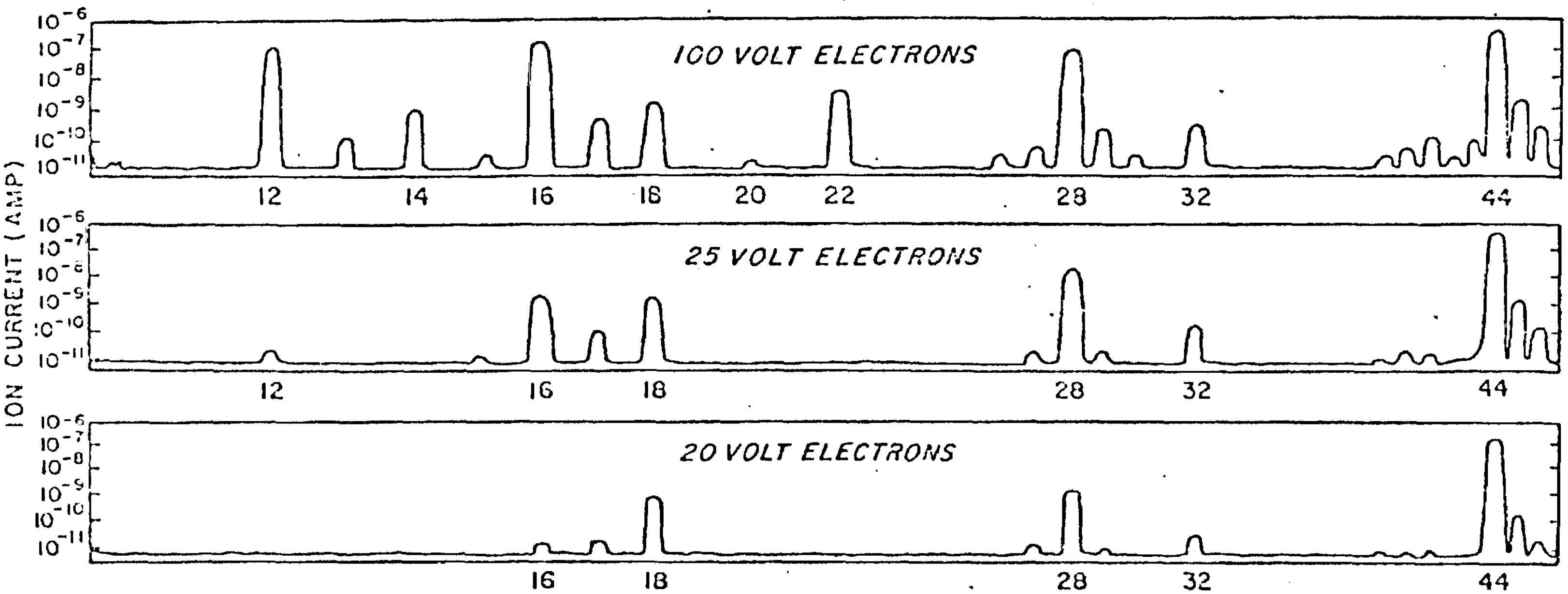
- MAGNETIC-SECTOR MASS ANALYZER.
- MASS RANGE 1 TO 48 AMU.
- TWO CHANNELS: 1 TO 4; 12 TO 48 AMU.
- DYNAMIC RANGE 10^7 - EMPLOYS ION COUNTING TECHNIQUES.
- TWO ION SOURCE SENSITIVITIES - AUTOMATICALLY TRIGGERED BY ION BEAM COUNTING RATE.
- ION SOURCE: DUAL FILAMENTS
  MULTI ELECTRON ENERGY.
- DETECTORS: ELECTRON MULTIPLIERS.
- ION SOURCE PRESSURE: 7 X 10^{-6} TORR.
- ANALYZER PRESSURE: 10^{-8} TORR RANGE (ION AND GETTER PUMPED SEPARATELY FROM ION SOURCE).
- SPECTRAL SCAN TIME: 35 SECONDS AT 14 BPS.

Figure 8-16

The scan time of the mass spectrum is dependent, of course, upon the telemetry bit rates that are available. For this discussion, I have assumed the fourteen bits per second that are given in the little blue booklet of the ten bar probe summary. This gives a scan of about thirty-five seconds for the mass spectrum, which is repeated continuously as the probe descends through the atmosphere.

Now Figure 8-17 shows how one might utilize the different electron energies that are used to ionize the gas molecules in the ion source itself. These are three spectra here of carbon dioxide, and if you note very carefully here, there are about five decades of amplitude range compressed on these scales. What we are talking about here is a large peak amplitude difference that is compressed down to a very narrow range. Carbon dioxide has a parent peak at mass forty-four, has isotopic peaks of carbon and oxygen at forty-five and forty-six, and that might be a good way of determining what the isotopic ratios of carbon and oxygen are although I am not sure there would be enough CO_2 in the outer planet atmospheres to do that. This is more specifically related toward Venus. At one hundred volt electrons, or even seventy volt electrons, which is the range that is normally used in mass spectrometers flown on earth satellites, one has a multitude of peaks that are formed by dissociatively ionizing or by doubly ionizing complex molecules. You have a rather complex, a busy sort of spectrum here. At mass 44 we have the parent peak; at mass 28 we have the CO peak with perhaps the addition of a little nitrogen from air leakage into the system when this spectrum was taken. We have a doubly charged CO_2 peak at mass 22. The mass spectrometer measures the mass to charge ratio of an ion, so an ion with two charges will effectively appear in the spectrum at one half its mass, so that is CO_2 double plus. The sixteen is 0 and the twelve is C, from CO_2, all torn out of the original molecules by the hundred volt electrons. Also, the fourteen peak seems to be significant here, which may indicate that there is some nitrogen in the mass twenty-eight peak.

Figure 8-17



Mass spectra of CO_2 produced by electron beams of 3 different energies.

Now, if we drop down to twenty-five volt electrons - these values are arbitrary, we can choose anything we wish - you see that the parent peak has not changed. In fact, it may have increased very slightly, indicating a slightly higher efficiency of ionization of the CO_2 at this level. The twenty-eight peak has decreased quite a bit. You will note that the twenty-two peak is absent completely, so one can eliminate from the spectrum there all doubly charged species. The eighteen has not changed - that's a water vapor impurity in the vacuum system itself. The sixteen has decreased significantly while the twelve has come down a real bunch. Therefore, the spectrum is much cleaner.

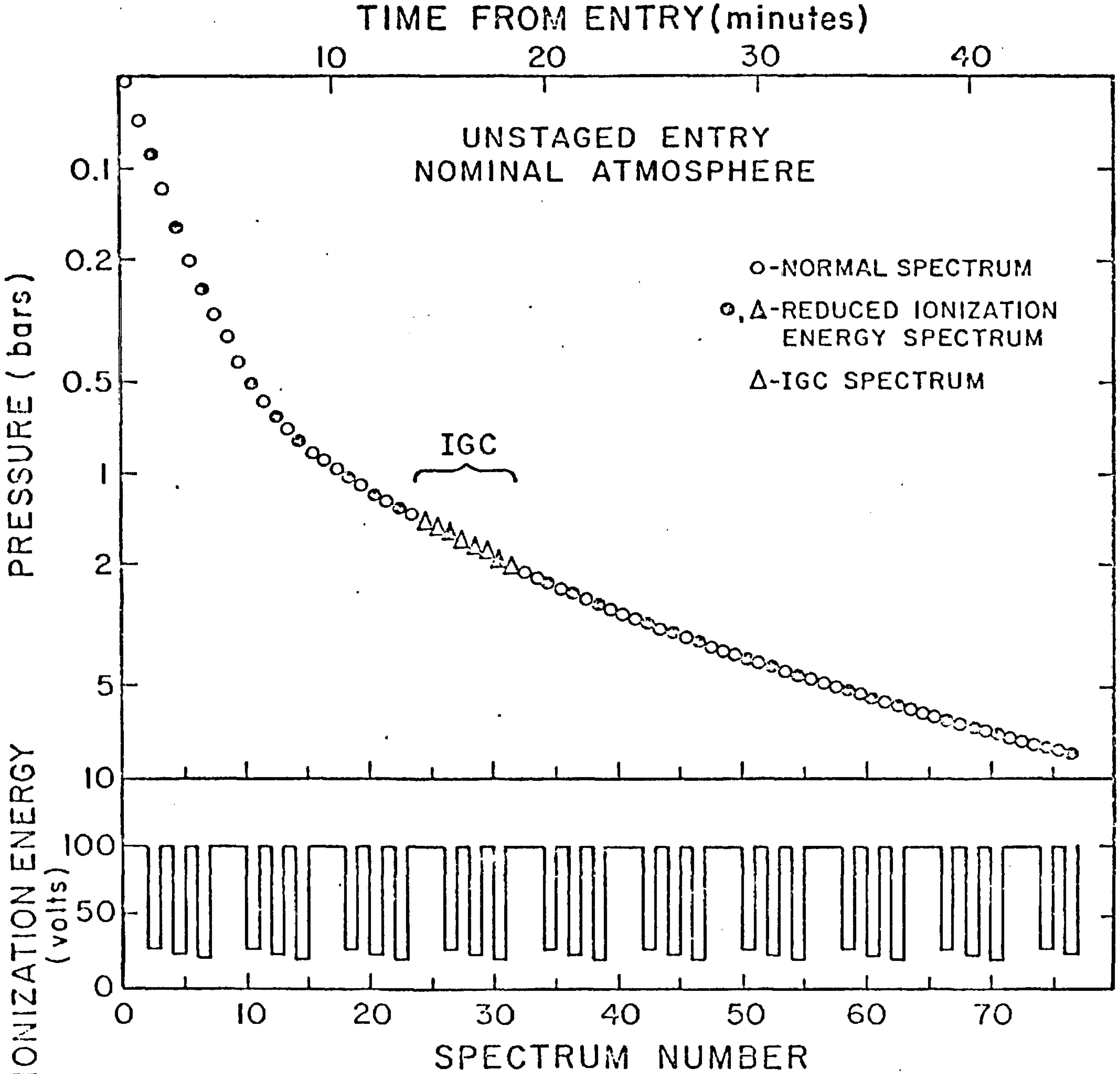
As we come on down now to the twenty-volt electrons, we find that, indeed, just about everything at the low end of the spectrum has been eliminated. The sixteen peak is almost gone. One thing to notice here is that the seventeen peak which is made in the ion source from the dissociation of water vapor - it is the OH ion and usually exists at something like one third the amplitude of the eighteen peak - has dropped almost two decades here; therefore, by using this technique, one could make a direct measurement of ammonia, which is at mass seventeen, without any significant interference of the OH from water vapor. One could make separate identifications of ammonia and water by this technique. Also, one might be able to measure the neon isotopes by the elimination of the doubly charged peak at mass twenty-two. The neon twenty-two, if there were enough CO_2, is certainly going to be masked, but the CO_2^{++} can be eliminated from the spectrum by the lower energy. Incidently, these doubly charged peaks tend to disappear at about 35 electron volts, which is well above the neon ionization potential.

This is actually a powerful tool that can be used for sorting out complex spectrums to identify the parent peaks and perhaps measure the isotopic ratios of a number of the different constituents, such as oxygen, nitrogen, and so forth, carbon.

Figure 8-18 gives an operations plan for entry into an outer planet atmosphere. From the time of entry, we have assumed

Figure 8-18

MASS SPECTROMETER OPERATIONS PLAN



a forty-five minute time to descend to about the ten bar level from fifty milibars. The circles indicate the time of each spectrum, which is thirty-five seconds. Again, this is assumed and is purely arbitrary and strongly dependent upon the telemetry bit rate that is available to us. The open circles are at one hundred volt electron energies; the solid circles at lower voltages. They are set up in blocks of eight. There are two sweeps of the spectrum at one hundred volts, then one at a lower voltage, which could be thirty, one again at one hundred volts then at twenty-two, one hundred, twenty, and one hundred. This grouping of eight, then, is repetitive as the probe descends. Now, this gives us a very good height profile of all the different constituents and enables one to make scale height determinations and study the variations as one goes through the cloud levels and that sort of thing.

There is one thing that I neglected to mention in the Figure 8-14. That is the IGC which stands for "inert gas cell." What we effectively do is to collect a sample of the atmosphere at the high level just after entry, just after the cap has been broken off, and the leaks have been exposed to the atmosphere. This sample is collected through another leak which has quite a bit larger conductance than the ion source leak. This sample is fed into a molecular sieve which purifies this gas sample of any active gas species, such as hydrogen in these planets. This sample is then transferred into a getter where it is further purified and sometime later in the flight, such as is shown by the triangles in the profile on the last slide, is transferred into the ion source. At that time, the programmed ion pump is operated which reduces the residual gas in the ion source. One can use this method to make isotopic ratio measurements of an enriched sample of the inert gases. One place where this might be very important is the situation that John Lewis mentioned yesterday, where one has normally an interference at mass three between HD, the molecule formed with the deuterium isotope of hydrogen, which comes in at mass three, and the helium three. In the mass spectrometer there is no way to distinguish between those two. Both of them appear at mass three and they add, so we don't know the amount of each one. But if one had a purified sample of the atmosphere in which the hydrogen was essentially eliminated and then measured, one would have essentially no contribution from the H_D peak and could get a good measurement of the helium three, helium four ratio. This is then one of the little tricks that John referred to yesterday that can be used to determine the various isotopic ratio measurements of the inert gases and perhaps some of the active gases as well.

Figure 8-19 gives some mass spectrometer interface specifications. Again, these are somewhat subject to adaptations depending upon which type of a probe we are flying on, but basically we have a mass analyzer and base plate that weigh something like two kilograms. The magnet itself weighs less than one kilogram. The weight here is sort of dependent upon the strength of the base plate one might need to mount the instrument on to withstand the entry G's into those planetary atmospheres. The inlet assembly is fairly lightweight. It is a single CML and has one break-off seal which is kicked off just after entry. The inert gas cell is fairly light. The pumps are approximately a kilogram. The electronics depend a little on what mass ranges we would cover and its degree of sophistication. Three kilograms is a good value giving a total weight of close to seven kilograms. The volume is around seven and one half liters, and this is again somewhat adjustable. The shape is certainly adjustable, as one can package electronics different ways and make this thing adaptable to the different probe designs. The power is around eleven watts. This is a rather steady power, because there are no pyrotechnic devices in the instrument after the initial ejection of the cap. There is some power reserve in here for heating of the inlet devices to prevent condensation on the inlet tube or on the leak itself.

The telemetry format depends upon what is available to us. We are assuming a fourteen bit per second read out rate. Each spectral scan, in the particular design that I showed you requires about four hundred ninety bits, that is out to mass forty-eight.

NEUTRAL MASS SPECTROMETER

INTERFACE SPECIFICATIONS

- WEIGHT (KG)

ANALYZER AND BASEPLATE	2.0
INLET ASSEMBLY	.4
INERT GAS CELL	0.3
PUMPS	1.2
ELECTRONICS	3.0
	6.9 KG

- VOLUME 7500 CM^3
- POWER 11.5 WATTS
- TELEMETRY

490 BITS PER SPECTRAL SCAN (49-10 BIT WORDS)

TELEMETRY WORDS SHOULD BE EQUALLY SPACED IN TIME.

SPECTRAL SCAN TIME = 35 SEC.

READOUT RATE: 14 BPS

Figure 8-19

We use ten bit words, of which one bit is the sensitivity flag, and nine bits contain a sort of a pseudo-logarithmic format to the base two. This format gives a 6-bit accuracy over the entire dynamic range. This information is telemetered back to earth along with about eighty bits of overhead during each of the sweeps of the mass spectrum; overhead being status flags, housekeeping data, and that sort of thing, engineering type units.

What I have tried to show you is one system which we could use to sample and measure the atmosphere of the outer planets. It is adapted from our Pioneer Venus instrument. All the parts of this system have been tested in the laboratory and have been shown to be within the "state-of-the-art" of space mass spectrometry.

L. POLASKI: I think we have time for a quickie question. Joel kind of played it smart. He didn't allow the other fellows to get questions. If you have a question for any one of the first three, throw it out.

QUESTION: How long does it take you to completely evacuate the chamber for a new sample gas and how completely do you get rid of all the previous molecules when you get a new sample in there to analyze?

DR. HOFFMAN: We have done some tests along that line and we show that in about a two-second time frame, we can pump out a gas like argon with an ion pump to about four percent of its original level. Argon is notoriously slowly pumped by ion pumps; all the rare gases are. The active gases will pump out much faster than that. Two seconds to get down to about the four percent level for argon is an actual test number that we have done in the laboratory. I would say in a few seconds between each scan of the spectrum, we would have the system pretty well evacuated so that we would have very little cross contamination of the different sweeps. In other words, we would really be looking at a fresh spectrum of gas, a fresh sample of gas, each time.

QUESTION: Are there any entry velocities on the breakdown system in the operation of the instrument itself?

MR. SEIFF: Are you talking about high velocity penetration at high velocity entry?

MR. HOFFMAN: This type of a system is not the type that Al Nier was talking about where one uses the ram energy due to the motion of the vehicle itself through the medium to bring the gas samples in. Here, the gas is sampled as it slips past the probe as it is settling through the atmosphere after entry. The curve I showed you is for non-staged type entry. This is just the settling rate of the probe itself through the atmosphere. It is a terminal velocity. It is not particularly critical in that case.

QUESTION: Do you propose using getter material for keeping out gases on this cruise? For the outer planets, we are talking about a rather long cruise. Is it still possible to use getter material?

MR. HOFFMAN: I did a calculation knowing the actual tested capacity of the getters that we are proposing here. If we had an atmospheric probe that had one atmosphere of a gas in it, say nitrogen, or it doesn't matter which gas particularly, as long as it is not a rare gas, we could pump for, like, ten years against a leak of ten to the minus ten cc per second, and that is readily achievable with today's techniques of building vacuum systems. We can also absorb in this getter a number of monolayers of gas off the internal surface of the instrument. I think we have more than adequate capacity without having to resort to vent tubes that stick outside the probe.

COMPARATIVE ATMOSPHERE STRUCTURE EXPERIMENT

S. Sommer

NASA Ames Research Center

N75 20402

MR. SOMMER: We have heard quite a bit about pressure, temperature and accelerometers being used for probes for the outer planets. I thought I would take this opportunity to just review very briefly how we use these instruments to determine atmosphere structure, spend a few minutes to review very briefly the results that we have obtained from our PAET earth entry, and to then describe, again very briefly, some of the instruments that we have flown on PAET, that we will fly on Viking, and that we hope to fly on Pioneer Venus.

As indicated on Figure 8-20, in order to describe atmosphere structure determination, we divide the entry into two regimes, high speed and low speed. We measure acceleration and from the acceleration we determine density as a function of time. We integrate the equations of motion to determine velocity, flight path angle, and altitude as a function of time. Then we determine density as a function of altitude from the previous determinations of density and altitude as a function of time. We assume hydrostatic equilibrium to determine pressure as a function of altitude. Finally, we apply the equation of space to determine temperature as a function of altitude, if we know the mean molecular weight. We obtain the mean molecular weight independently from either the low speed experiment or from the composition experiments.

During the low speed portion of the flight, and by low speed I mean somewhere around mach one or two or where you can deploy a temperature sensor without destroying it, we measure pressure, temperature, and again, acceleration. We correct pressure and temperature to ambient values. We solve the equations of hydrostatic equilibrium and vertical motion, and obtain altitude and velocity as a function of time and mean molecular weight.

We compute pressure and temperature as a function of altitude, and we apply the equation of state to obtain density as a function of altitude.

NASA - Ames

ATMOSPHERE - STRUCTURE DETERMINATION

HIGH SPEED

- MEASURE A_X TO DETERMINE $\rho(t)$
- INTEGRATE EQS. OF MOTION TO DETERMINE $V(t)$, $\gamma(t)$ AND $z(t)$
- DETERMINE $\rho(z)$ FROM $\rho(t)$ AND $z(t)$
- ASSUME HYDROSTATIC EQUILIBRIUM TO DETERMINE $P(z)$
- APPLY EQUATION OF STATE TO DETERMINE $T(z)$

LOW SPEED

- MEASURE P_T, T_R, AND A_X
- CORRECT P_T AND T_R TO AMBIENT VALUES
- SOLVE EQS. OF HYDROSTATIC EQUILIBRIUM AND VERTICAL MOTION TO OBTAIN $Z(t)$, $V(t)$ AND μ
- COMPUTE $P(z)$ AND $T(z)$
- APPLY EQUATION OF STATE TO OBTAIN $\rho(z)$

Figure 8-20

The next figure, Figure 8-21, indicates what we hope to obtain if we flew more than one probe at the same time, as Pioneer Venus does. I added molecular weight to the chart because this independent measurement can be used to compare with measurements made by the composition experiments. We hope to be able to get some insight on circulation of the global scale. We intend to be able to make some vertical wind determinations. We will attempt to measure atmospheric turbulence in the lower part of the atmosphere. If any of the four probes on Pioneer Venus survive impact, we hope to make some seismic measurements.

Now what I would like to do is run through some of the results that we have obtained from our PAET experiment. The first is a trajectory determination. Plotted on Figure 8-22 is velocity as a function of time from lift-off. The dots shown here are experimental points determined from the method that I showed you on the first slide, and is compared to radar tracking data obtained both from Bermuda and from Wallops. I have indicated the division between the high speed experiment and the low speed experiment. Velocity up to about this 576 seconds was determined solely from acceleration and from about 576 seconds on, from acceleration, pressure, and temperature measurements.

You will note that we have reasonably good agreement. The next Figure 8-23 shows altitude as a function of density. This is one of the primary measurements. The region above about twenty-six kilometers, where we reached a mach number of about two and deployed our temperature sensor, density was determined solely from the accelerometers whereas at lower altitudes, density was determined by using accelerations, pressures and temperatures. You will notice that the data covers over five decades of density. Since this is a log plot, we have plotted the difference between the measurements and meteorological data on the right hand side of the figure. Although local differences approach 20 percent, it turns out that meteorological data has much more uncertainty than this particular experiment.

ADDED EXPERIMENT CAPABILITY

- ATMOSPHERIC MEAN MOLECULAR WEIGHT
- CIRCULATION AT GLOBAL SCALE
- VERTICAL WIND DETERMINATION
- MEASURE ATMOSPHERIC TURBULENCE
- SEISMIC MEASUREMENTS

Figure 8-21

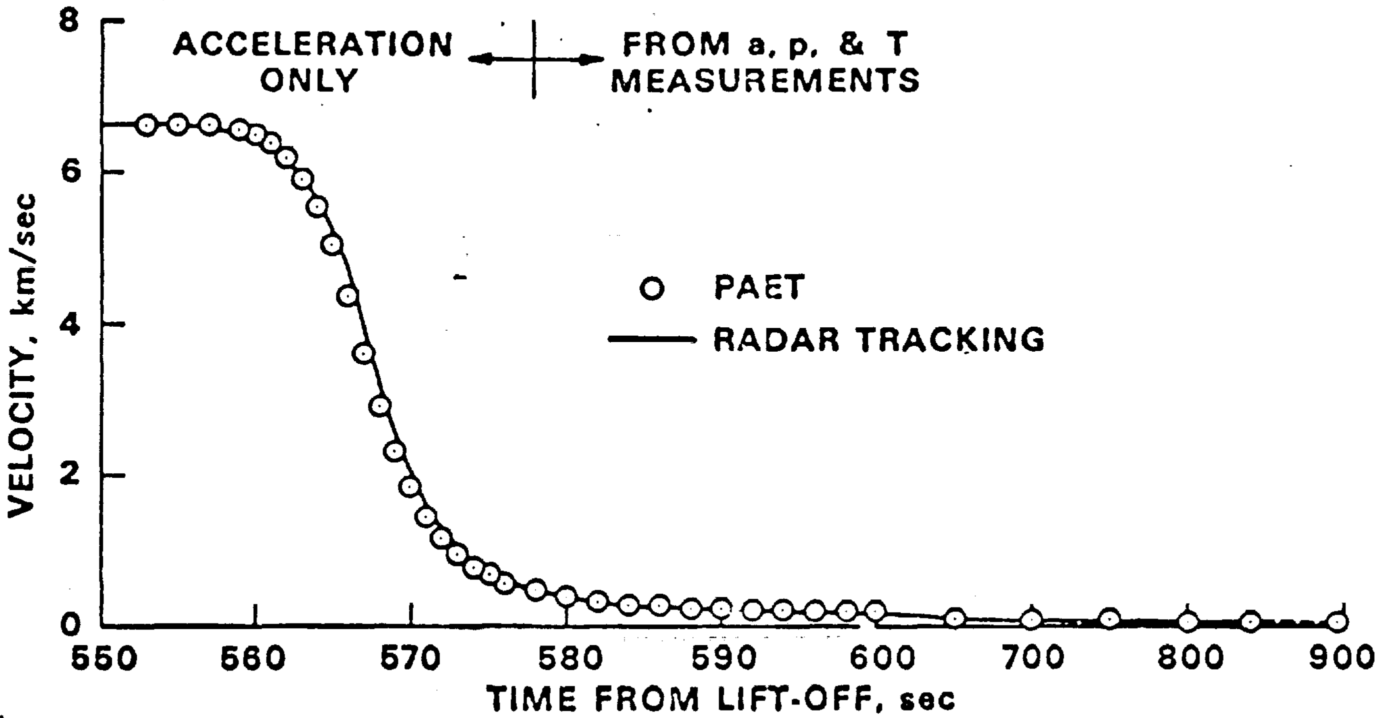


Figure 8-22

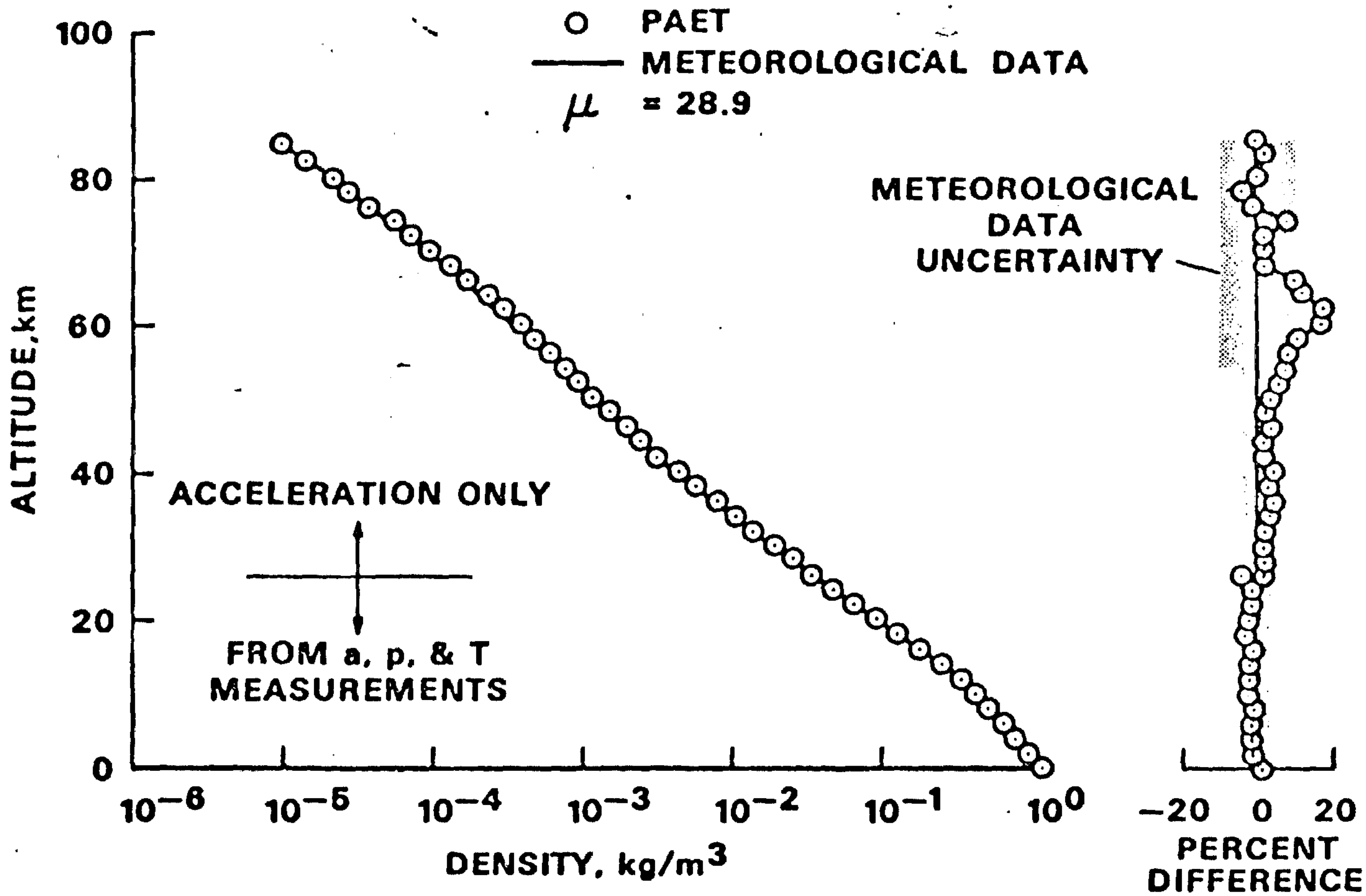

PAET
METEOROLOGICAL DATA
μ = 28.9
100
80
60
40
20
0
ALTITUDE, km
ACCELERATION ONLY
FROM a, p, & T
MEASUREMENTS
METEOROLOGICAL
DATA
UNCERTAINTY
10^{-6} 10^{-5} 10^{-4} 10^{-3} 10^{-2} 10^{-1} 10^{0}
DENSITY, kg/m^3
−20 0 20
PERCENT
DIFFERENCE

Figure 8-23

The last of the data plots, Figure 8-24, from PAET is deduced temperature as a function of altitude, where temperature is determined from readings of the accelerometers. Essentially, from 26km up, the temperature data is deduced solely from the accelerometers and is compared to meteorological data from Viper Dart firings made about one hour before and one hour after PAET. Notice the similarity between the two sets of data and the almost perfect agreement with the meteorological data where direct measurements of temperature and pressure were made.

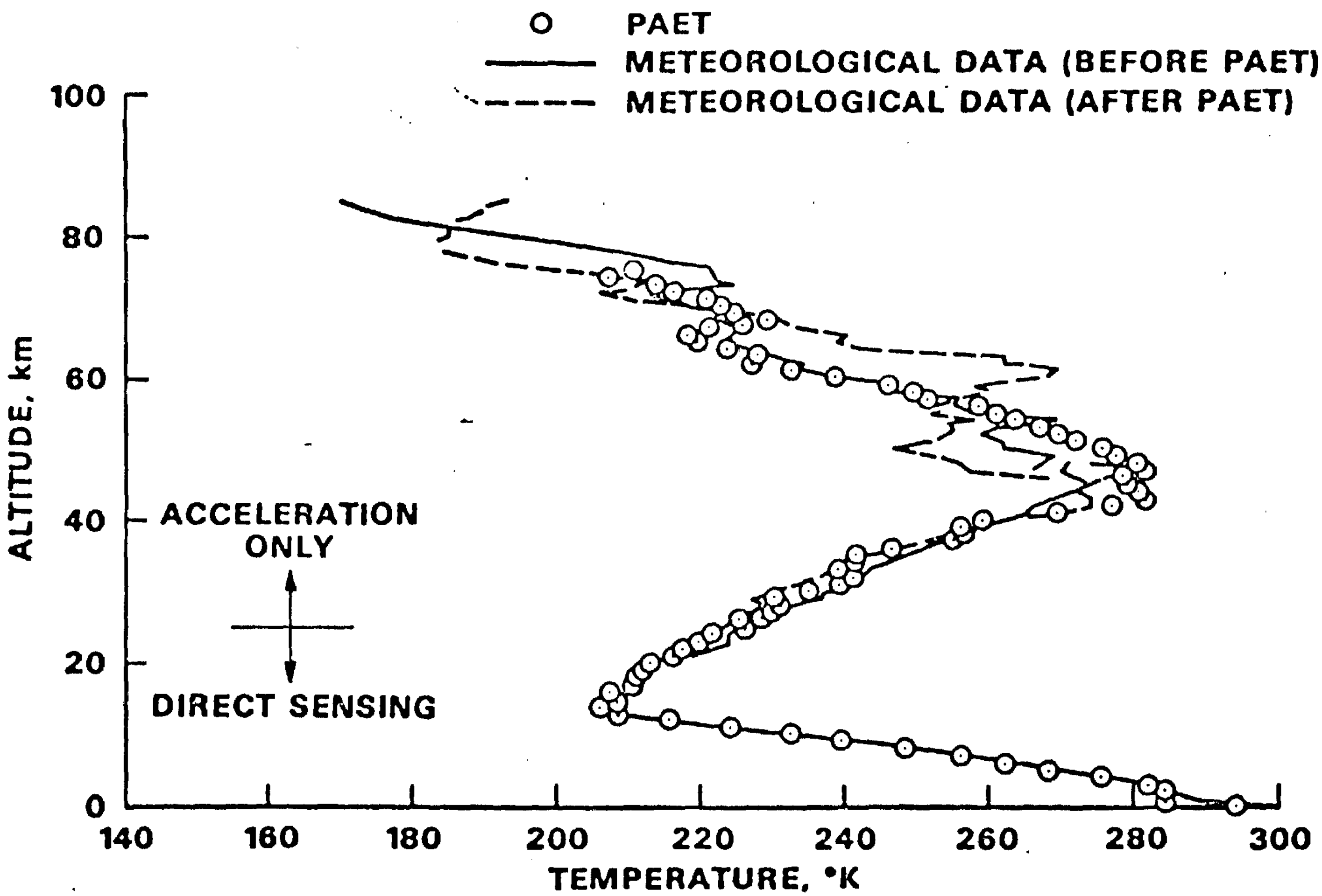
Let me spend the rest of my few minutes comparing the instruments on the three missions that the Ames group has been, and is involved in; PAET, Viking, and Pioneer Venus. Figure 8-25 is a comparison of atmospheric temperature sensors for the three missions, comparing type, range, accuracy, and weight.

For PAET, we used chromel-alumel thermocouples, the range from 200 to 660 degrees kelvin. We had an accuracy of about one degree. We had two sensors that deployed through the heat shield, each weighing about six tenths of a pound.

Viking is carrying two temperature sensors for us, and the one that I am describing here is the one that comes out through the aeroshell before separation. It is also a chromel-alumel thermocouple with a range from 100 to 700 degrees kelvin. The accuracy is three and one half degrees plus the one percent of reading, and it weighs about one pound.

On Pioneer Venus, we are planning to use a resistance thermometer. The range, again, is very similar - 200 to 800 degrees kelvin. The accuracy requirement is much more severe. We feel that the temperature differences around the globe are small, and we are trying to determine what those are, thus the 1/4 degrees accuracy requirements; total weight is about 1/2 pound.

The way we plan to deploy the temperature sensor for Pioneer Venus is illustrated in the next two figures. Figure 8-26 shows a



PAET
METEOROLOGICAL DATA (BEFORE PAET)
METEOROLOGICAL DATA (AFTER PAET)
ALTITUDE, km
100
80
60
40
20
0
ACCELERATION ONLY
DIRECT SENSING
140
160
180
200
220
240
260
280
300
TEMPERATURE, °K

Figure 8-24

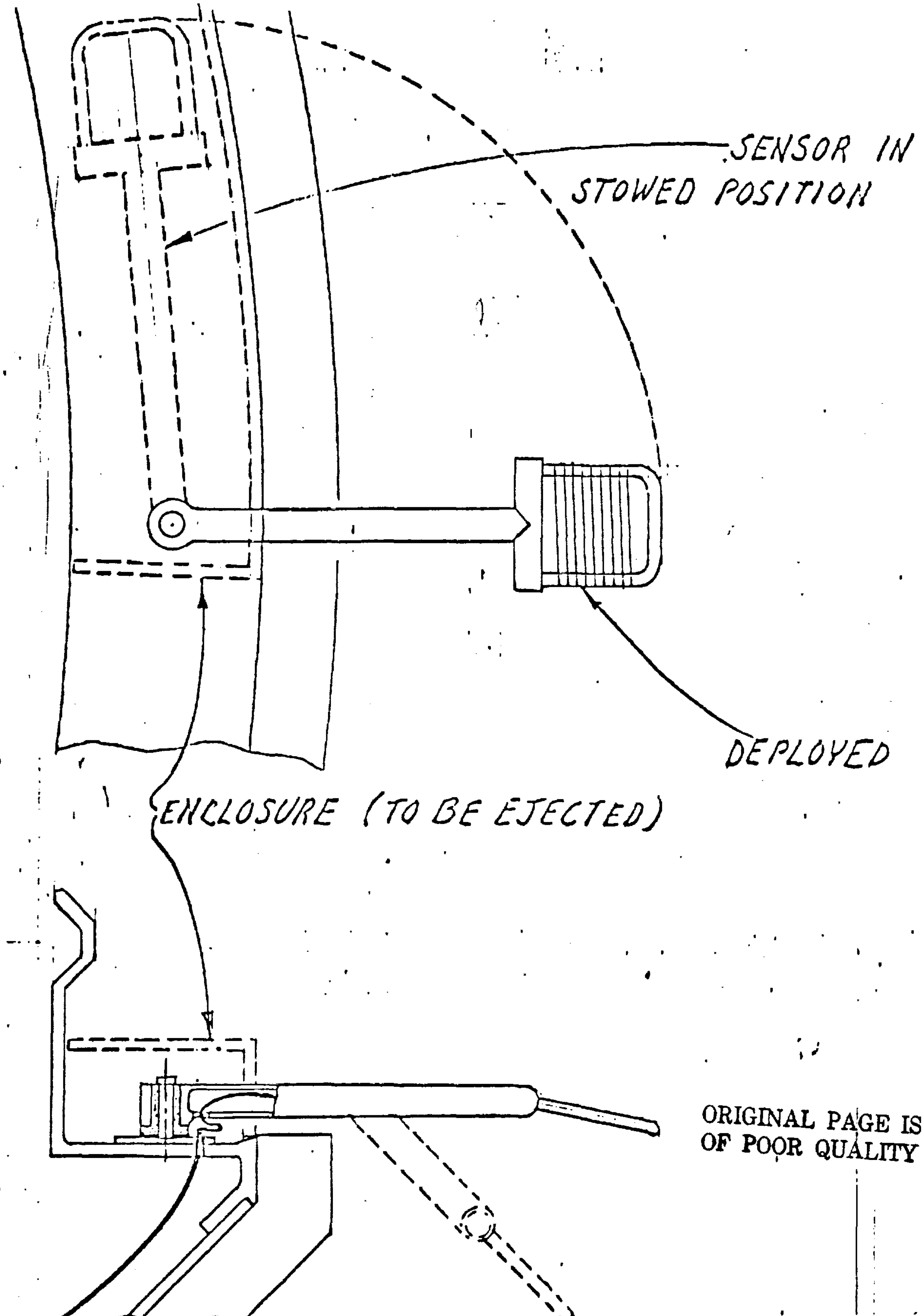
NASA - AMES

COMPARISON OF ATMOSPHERIC TEMPERATURE SENSORS

ON THREE MISSIONS

MISSION	SENSOR TYPE	TEMP. RANGE, ^{o}K	ACCURACY	WEIGHT, lbs.
PAET	THERMOCOUPLES CHROMEL-ALUMEL	$200^{o} - 660^{o}$	$\sim 1^{o}K$	0.64 (ea) 2 SENSORS
VIKING	THERMOCOUPLES CHROMEL-ALUMEL	$100^{o} - 700^{o}$	$<[3.5^{o}K + 1\%RDNG]$	0.96
PIONEER VENUS	RESISTANCE THERMOMETER	$200^{o} - 800^{o}$	$<\frac{1}{4}^{o}K$	0.52

Figure 8-25



ORIGINAL PAGE IS
OF POOR QUALITY

TEMPERATURE SENSOR IN SMALL PROBE

Figure 8-26

plan view of a deployable arm that is located within the afterbody cover. The arm comes out and bends down so that the sensor sees the flow around the body outside the boundary layer before it comes to the afterbody. This is one of the concepts that we are contemplating.

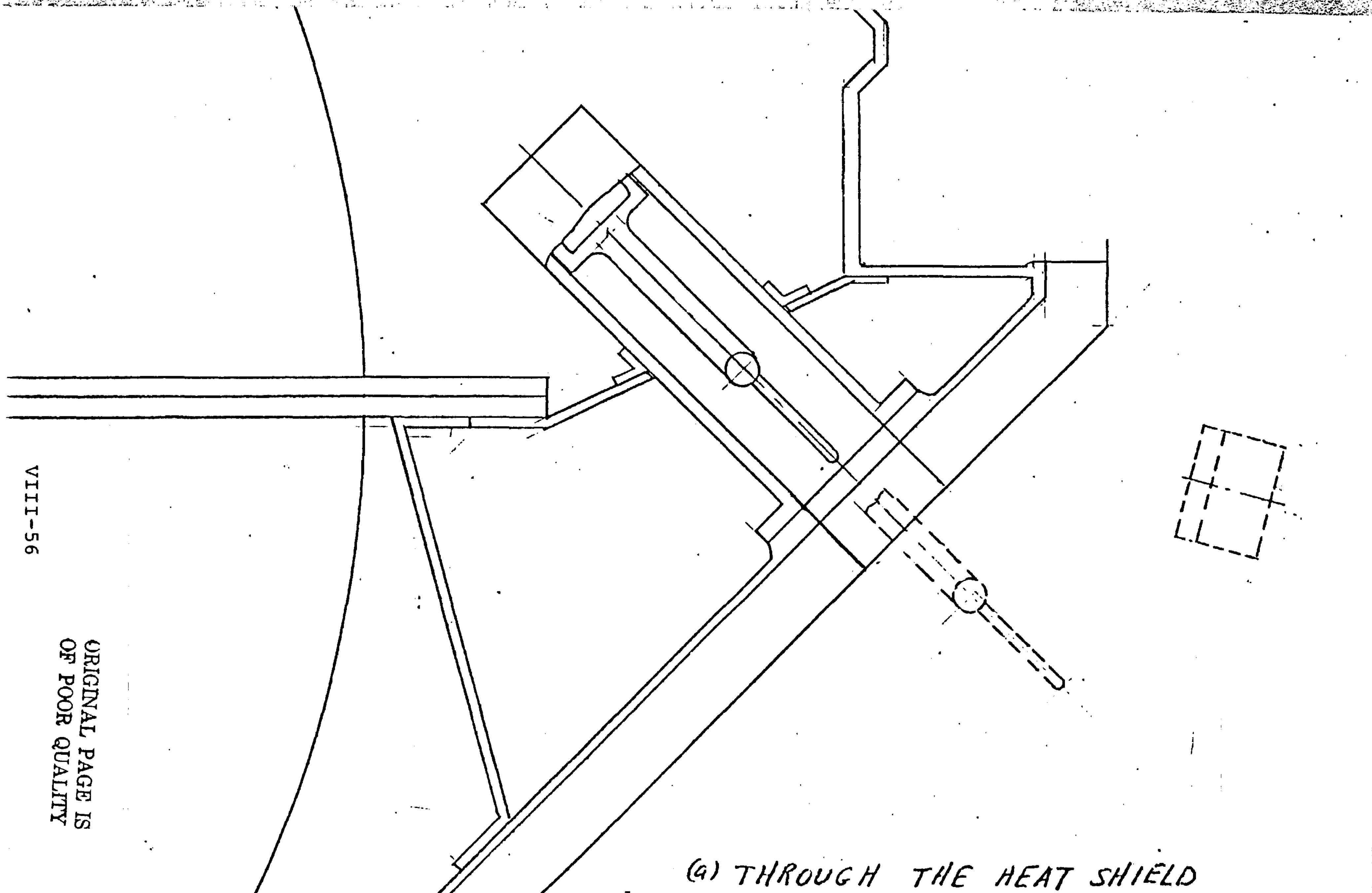
The other is illustrated on Figure 8-27, which is the concept that we have used on PAET that will be used on Viking and could be used on Pioneer Venus and/or any of the other planets.

Next, Figure 8-28 contains a similar comparison of the pressure sensors. For PAET, we used a vibrating diaphragm pressure sensor which measured pressures from a .001 to one atmosphere with an accuracy of about one percent of reading. There again, we carried two sensors, each one weighing about seven tenths of a pound.

On Viking, for the entry vehicle, we are carrying a stainless steel, conventional type diaphragm pressure sensor. The pressures to be measured are from .001 to only .15 atmosphere. Accuracy is about 2 percent of reading and weighs very close to a pound.

For Pioneer Venus, we are planning to carry a number of miniature silicon diaphragm diffusion-bonded wheatstone bridge-type sensors. They are sensors about a quarter of an inch in diameter, weighing a few grams. We are contemplating carrying anywhere from six to twelve in order to cover the range from 30 millibars to about 100 atmospheres. The goal is an accuracy of about 1/2 percent of reading. The weight of that entire system, including electronics, is on the order of 0.8 pound.

Figure 8-29 illustrates how we intend to sample the pressure, either through the heat shield at the stagnation point or through tubing opening adjacent to the temperature sensor. When the temperature sensor is deployed, then that pressure sensor will make its readings starting at that time.



(a) THROUGH THE HEAT SHIELD

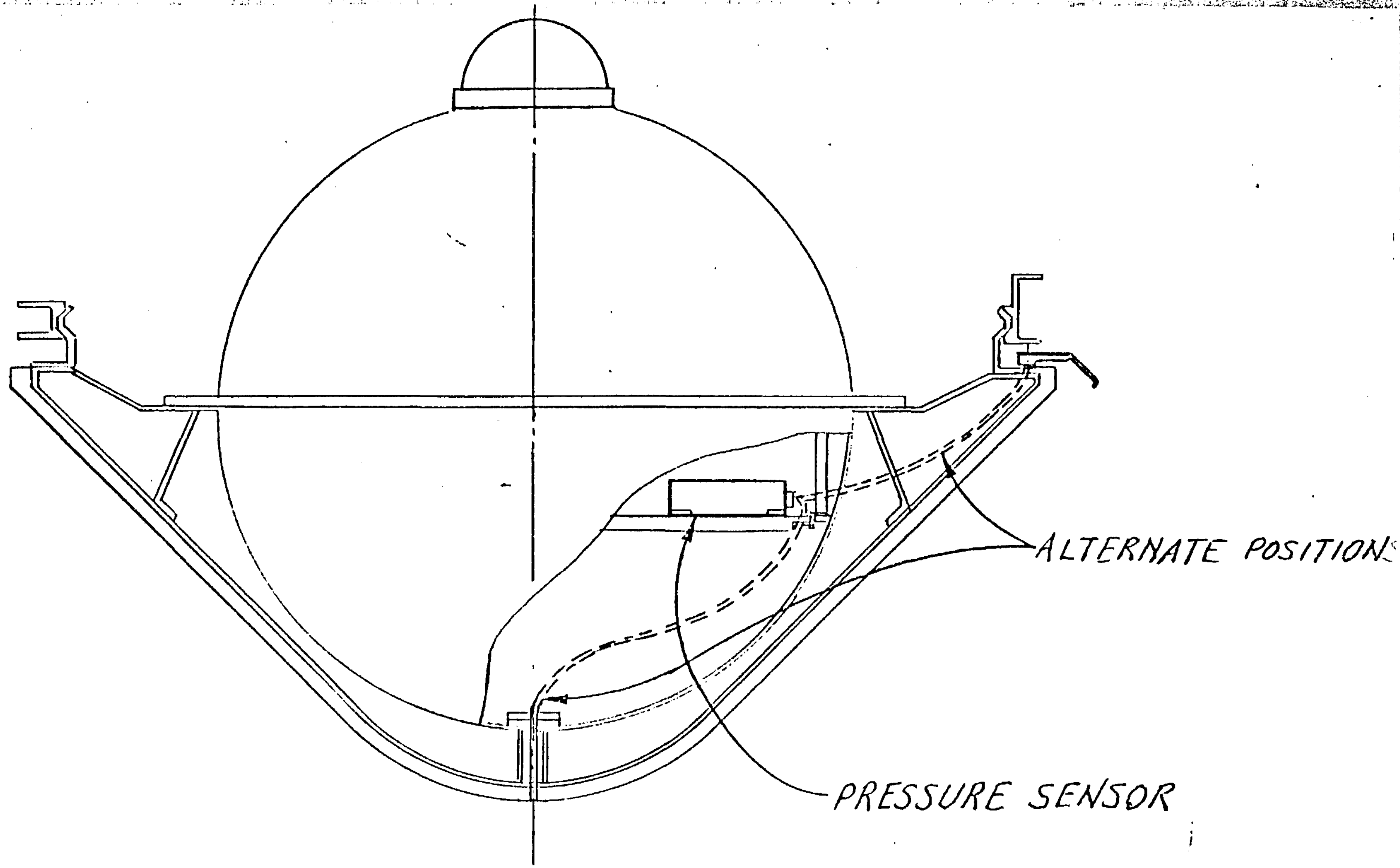
FIGURE 8-27. POSSIBLE DEPLOYMENT MECHANISMS FOR THE TEMPERATURE SENSOR ON THE SMALL PROBE

NASA - Ames

COMPARISON OF PRESSURE SENSORS ON THREE MISSIONS

MISSION	SENSOR TYPE	PRESSURE RANGE, ATM.	ACCURACY, % OF RDG	WEIGHT, lbs.
PAET	VIBRATING DIAPHRAGM	0.001 TO 1.0	~1	0.73 (ea) 2 SENSORS
VIKING	STAINLESS-STEEL DIAPHRAGM	0.001 - 0.15	~2	0.93
PIONEER VENUS	MINIATURE SILICON DIAPHRAGM (DIFFUSION-BONDED WHEATSTONE BRIDGE)	0.03 TO 100	~0.5	0.80

Figure 8-28



SMALL PROBE INLET TUBE LOCATIONS

Figure 8-29

Finally, Figure 8-30 compares the accelerometers. Again, I have compared the PAET, Pioneer Venus, and Viking sensors. They are all force rebalance type sensors. The electronics are not shown. The accelerometer that we used on PAET, which was designed for a range of about 80 G maximum on the axial accelerometer, was capable of up to several hundred G's. It weighed about 0.4 pound. The Viking instrument, where the maximum acceleration expected is less than 22 G's, is shown in the middle figure. Since this instrument is already developed, it is the leading candidate for Pioneer Venus. The people who have built, designed, and flown these instruments have been working for about the past year and a half on a sub-miniature instrument that has exactly the same capabilities, weighing about fifteen grams. When this instrument is qualified, it will be a leading candidate for planetary entry acceleration measurements.

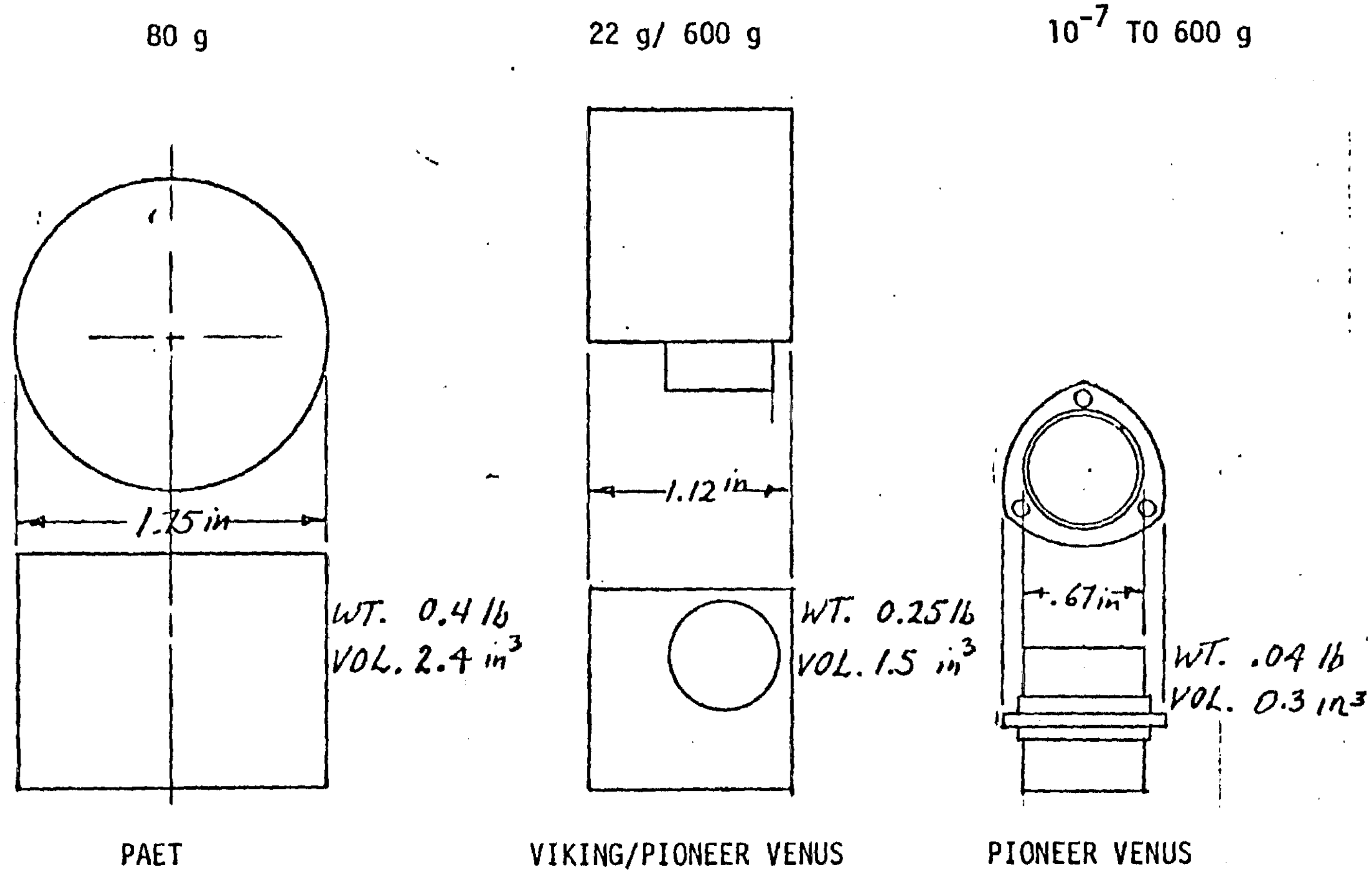
Figure 8-31 shows a blow-up of the Viking instrument. It has over one hundred parts including alnico and magnet housing. I want to compare that to a schematic (Figure 8-32) of what the accelerometer manufacturer calls the model eleven, that has about nine parts. The primary reason for the simplicity, they say, in this is in the magnet. It is made out of a rare earth material, samarium cobalt. An instrument of this type has been built, and is ready for test.

In conclusion, I would like to say that instrumentation for atmosphere structure determination is available with very little modification for application to outer planet exploration.

QUESTION: What is the name of that vendor with the super-light instrument?

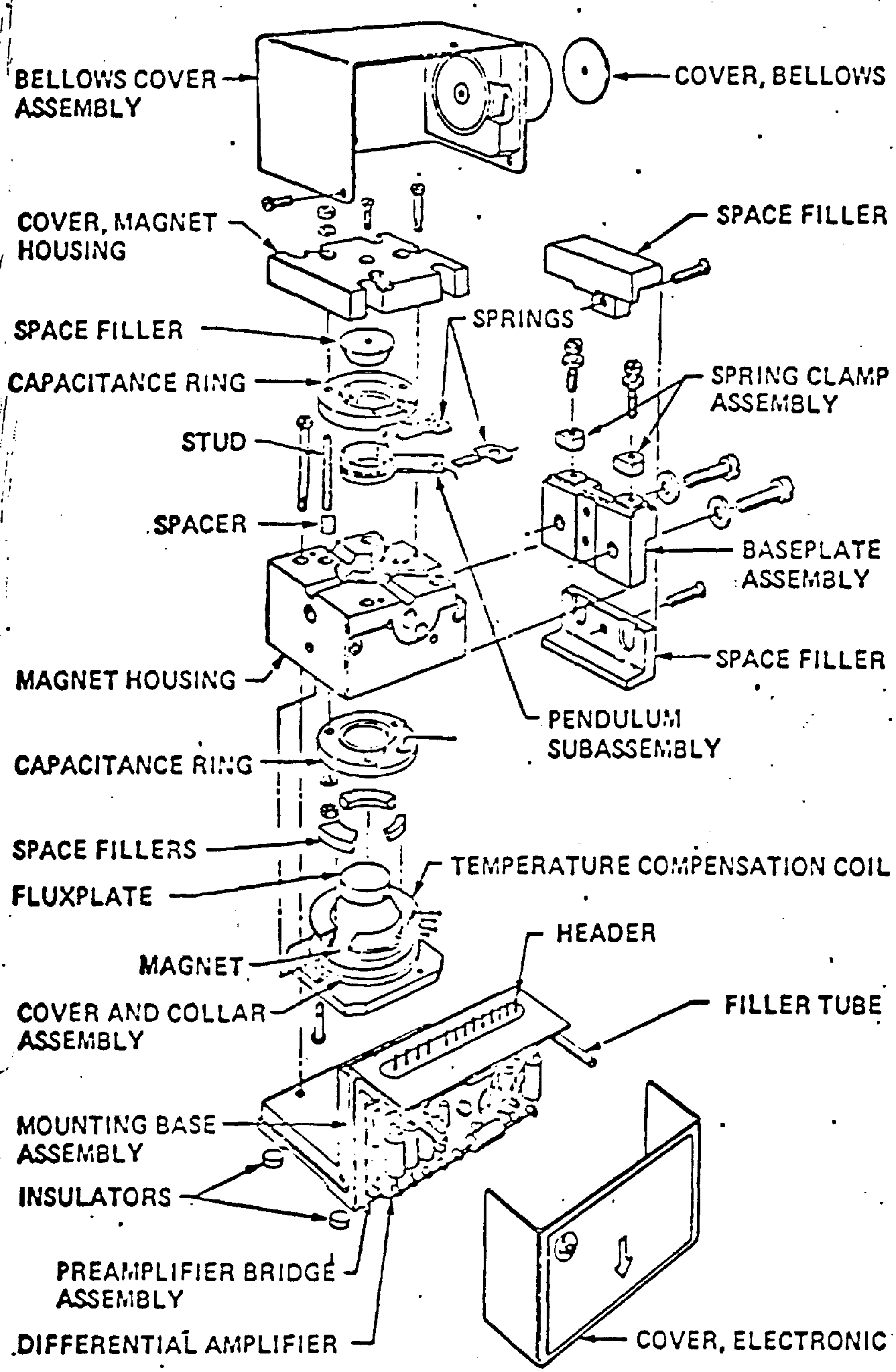
CHAIRMAN: Bell Aerospace

QUESTION: What is the altitude range you hope to get turbulence measurements on?



COMPARISON OF ACCELEROMETERS ON THREE MISSIONS

Figure 8-30



ACCELEROMETER MODEL IX

-7 CONFIGURATION

Figure 8-31

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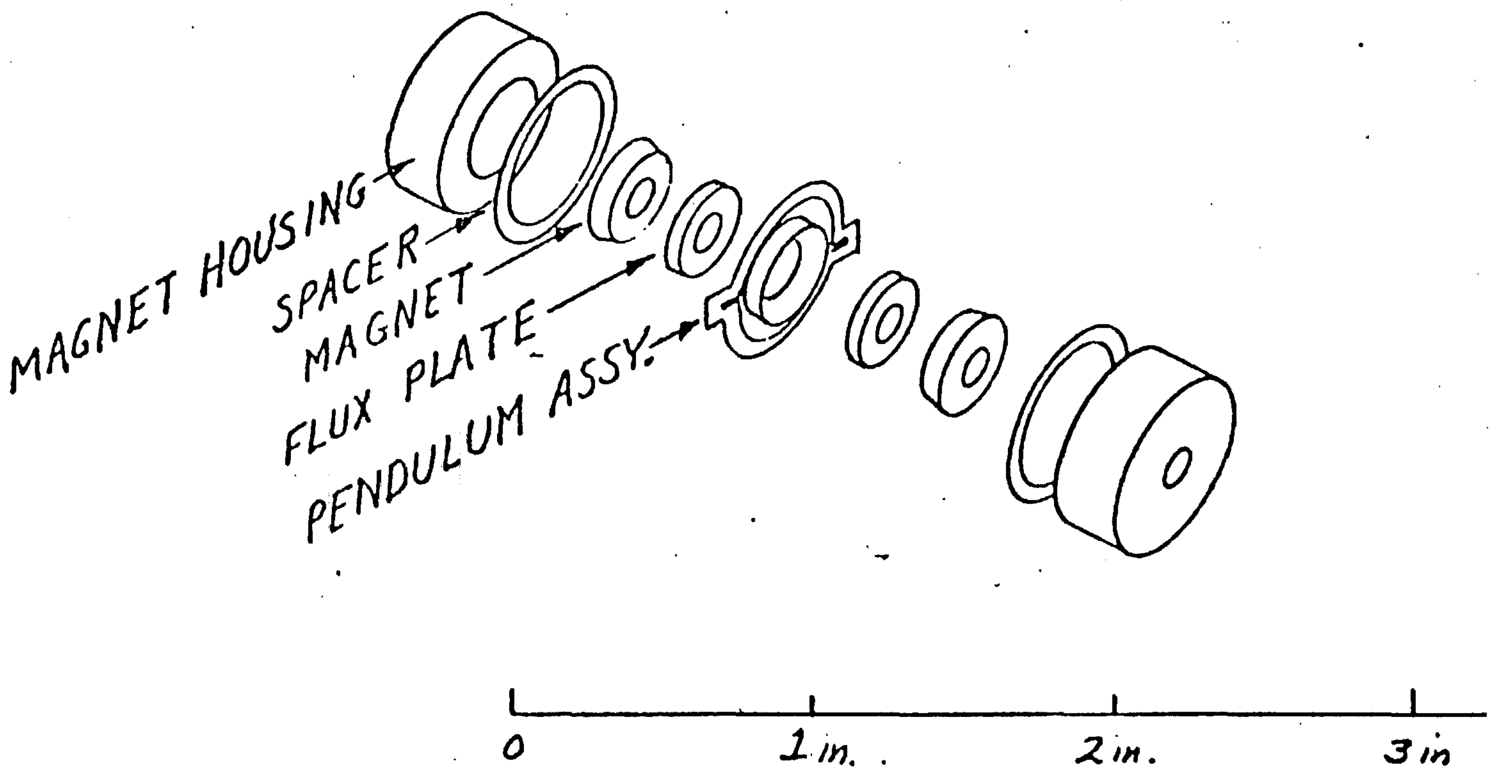


MODEL XI ACCELEROMETER

Figure 8-32

MR. SOMMER: Anywhere during terminal descent for Pioneer Venus, from around 70 km to as close to the surface as we can go. That kind of turbulence measurements we hope to make are really statistical measurements. In other words, we are going to try to count the number of times that the vehicle will feel accelerations above pre-selected values. We will sum those up over a period of time, transmit those back, and then analyze the data. That is the only kind of data capability that we have available for that experiment.

N75 20403

IMPACT OF THE RETAINED HEAT SHIELD CONCEPT ON SCIENCE INSTRUMENTS

W. Kessler

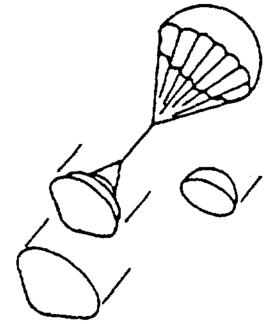
McDonnell-Douglas Astronautics Company

MR. KESSLER: The preceeding speakers in the science session have discussed the design and the operation of a specific science instrument. This presentation will consider the associated interface problems between the mass spectrometer and the actual probe design and consider the problem of providing a clean sample to the gas detection instrument.

McDonnell-Douglas has adopted the retained heat shield concept (Figure 8-33) where the heat shield is retained throughout the entire descent trajectory, in the design of an outer planet probe. This was done because of potential high reliability and savings in development costs as well as an associated lower weight. Once the peak deceleration and peak heating environment have been traversed and the probe reaches subsonic velocity, it becomes necessary to expose the scientific instruments to the ambient atmosphere. This is accomplished in the probe design by penetrating the heat shield with sampling tubes.

Of particular interest is the penetration of the heat shield by the mass spectrometer sampling tube, because not only do we have to demonstrate that the sampling tube can penetrate the heat shield but also that the mass spectrometer can be supplied with a contaminant-free gas sample, free of contaminants from out-gassing of the heat shield.

These two shadow-graph photographs (Figure 8-34) were obtained in the pressurized ballistic range facility at NASA Ames. The ballistic range models incorporate an extended tube at the stagnation point to simulate the sampling tube for the mass spectrometer. The tests were conducted at a Mach nine-tenths condition to match the actual flight deployment conditions for the sampling tube. These flow field visualization pictures illustrate basic flow field features that cannot be duplicated by computational techniques. Note that right around the base of the sampling tube there is a small



	RETAINED HEATSHIELD (BASELINE)	JETTISONED HEATSHIELD
PRIMARY ADVANTAGES	• SIMPLE PASSIVE SYSTEM	• M/C_DA IS MORE ACCURATE
PRIMARY DISADVANTAGES	• CANNOT TAILOR DESCENT RATES, IF REQUIRED	• ΔWt = 33 LB (13% INCREASE OVER BASELINE) • COMPLEX ACTIVE CHUTE STAGING
QUESTIONS REQUIRING PROOF OF CONCEPT	• DEMONSTRATE INHERENT DYNAMIC STABILITY • DEMONSTRATE HEATSHIELD PENETRATION TECHNIQUE • ENSURE CONTAMINATION FREE ATMOSPHERIC SAMPLES	• DEMONSTRATE HEATSHIELD RELEASE AND CHUTE DEPLOYMENT SEQUENCE • DEMONSTRATE SPACE STORAGE LIFE OF CHUTES • ENSURE CONTAMINATION FREE ATMOSPHERIC SAMPLES

Figure 8-33. Basic Concept

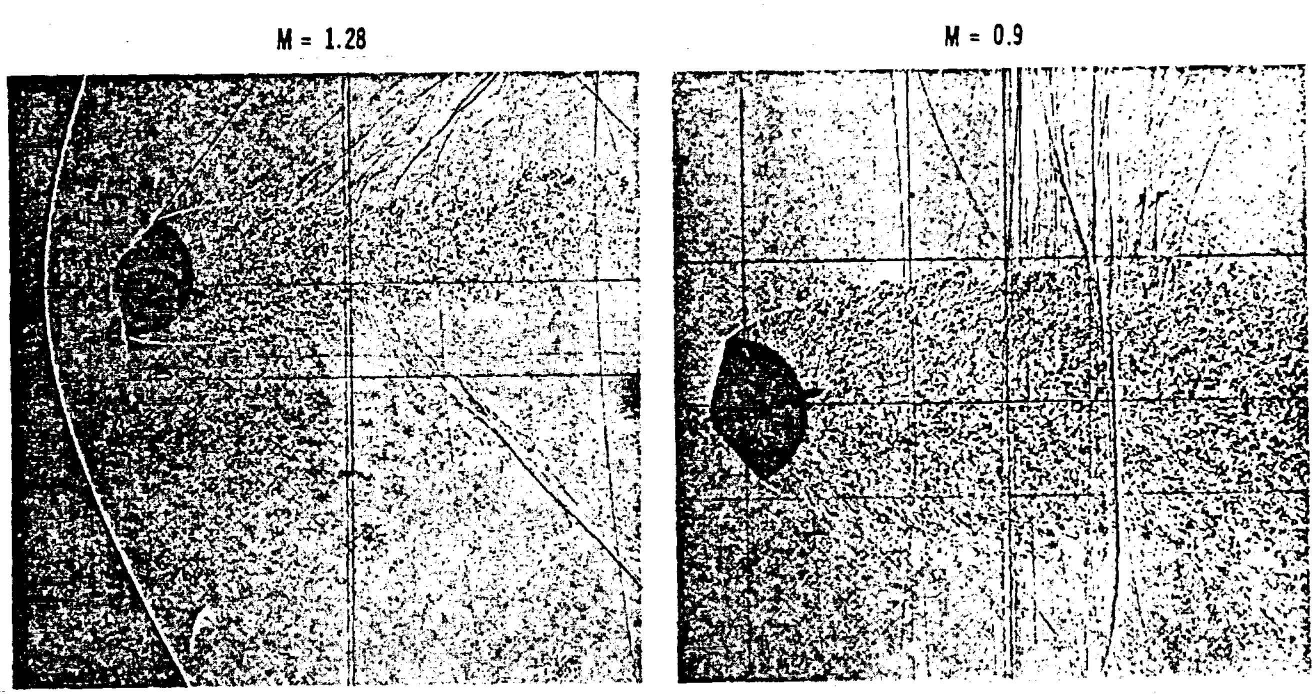


Figure 8-34. Ballistic Range Tests

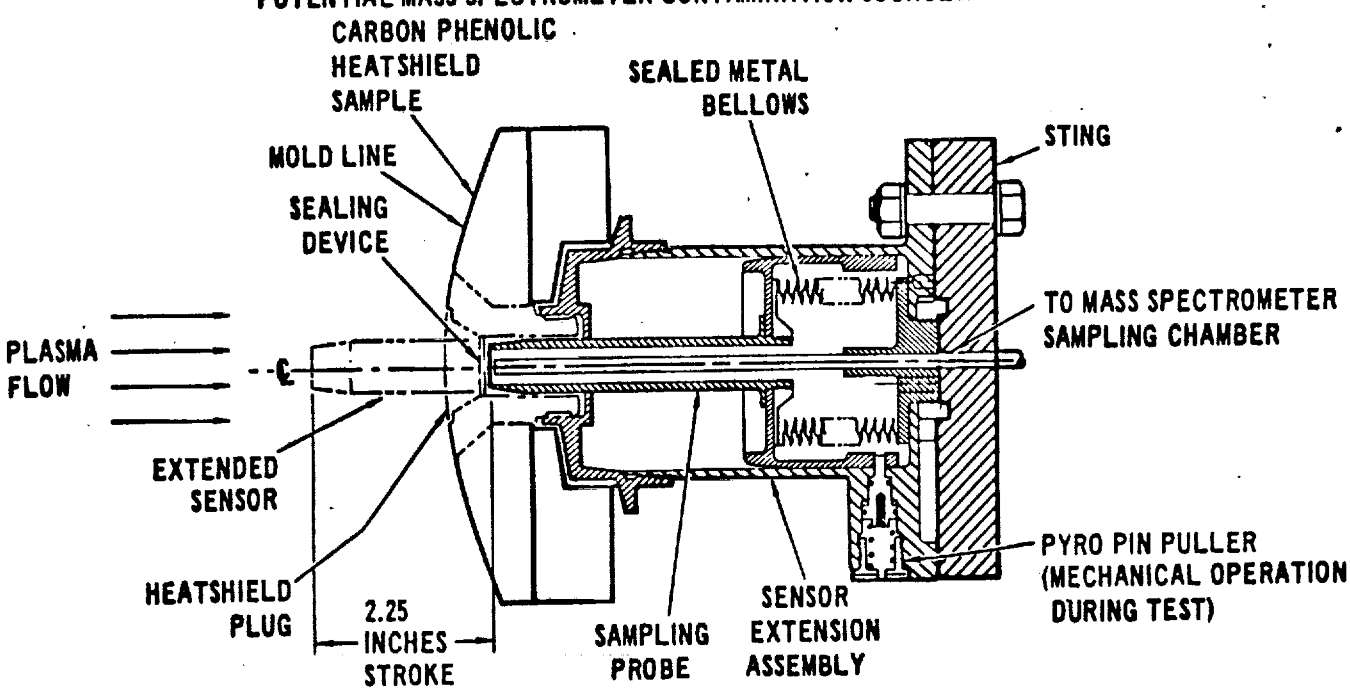
region of separated flow. It is also noted that locally the sampling tube appears to trip the laminar boundary layer.

The remaining charts review two "proof-of-concept" test programs that will be conducted in the near future at the NASA Ames Research Center. The first test will determine the feasibility of penetrating the charred heat shield with a sampling tube and collecting a clean sample for the mass spectrometer analysis. The second test will determine whether or not any contaminants from the out-gassing of the charred heat shield are ingested by the sampling tube.

The first test is to verify the feasibility of penetrating the charred heat shield. The interface between the mass spectrometer sampling chamber and the ambient atmosphere is the sensor extension assembly (Figure 8-35). Within the sensor extension

PURPOSE

VERIFY CAPABILITY TO PENETRATE HEATSHIELD AND EVALUATE POTENTIAL MASS SPECTROMETER CONTAMINATION SOURCES.



TEST RESULTS

- HIGH SPEED MOTION PICTURES OF THE HEATSHIELD PENETRATION
- HEATSHIELD TEMPERATURE TIME HISTORIES
- CONTAMINATION MEASUREMENTS BEFORE, DURING AND AFTER SENSOR DEPLOYMENT

Figure 8-35. Test 1: Sensor Extension Test Program assembly there is a sealed metal bellows which is in a compressed condition. Once the peak deceleration and peak loading regime has been traversed and a subsonic environment encountered, the energy in the compressed bellows is released and the carbon phenolic plug and the sealing device are pushed out into the main stream of the flow. The sampling tube extends two inches in front of the charred heat shield ablator and is used to bring samples of the atmosphere into the mass spectrometer.

This test will be conducted in the plasma arc facility at NASA/ARC. High speed motion picture data will be used to determine the trajectory of the plug as it comes out of the heat shield.

The tests will be conducted at two extreme conditions: one, typical of a shallow entry into a warm atmosphere; and the other a steep entry into a cool atmosphere. The solid lines on Figure 8-36 indicate the actual conditions along the descent trajectory, the dashed lines indicate the simulating test condition. During

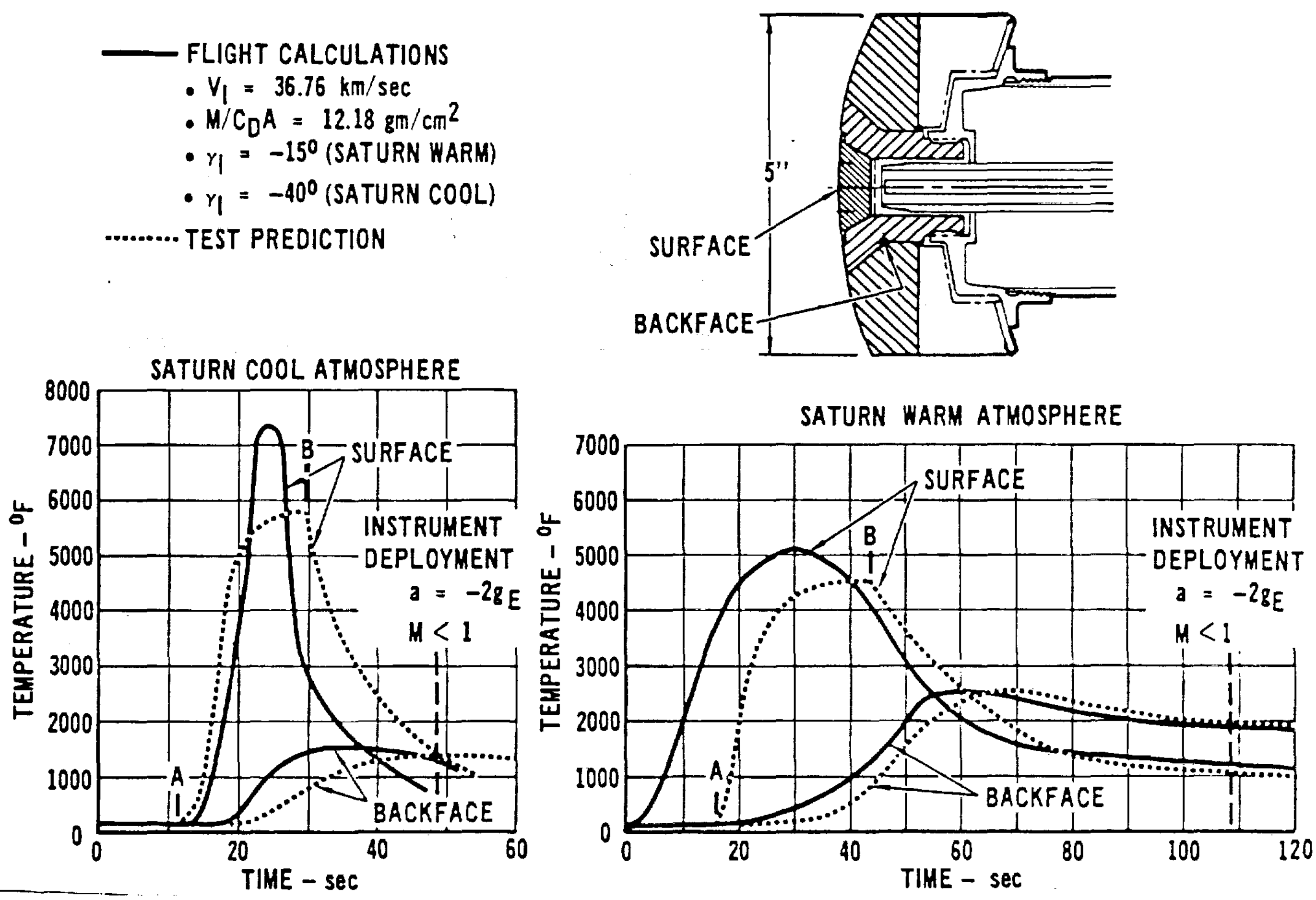


Figure 8-36. Test 1: Plasma Jet Simulation of Entry Environment

the test, the backface temperature at the deployment conditions and the total heat flux underneath the curves will be matched.

The second program to be conducted will determine if any contaminants from the heat shield outgassing are ingested by the sampling tube. The tests will be conducted for the worse case flight conditions for outgassing (Figure 8-37). These worst case conditions are the shallow entry into the warm model atmosphere. The trajectory point being the deployment conditions for the mass spectrometer sampling tube. This point is where the outgassing mass flow rate is still high. Setting the worst case conditions for outgassing determines the local free stream conditions - a Mach number of nine tenths, and a Reynolds number based on the probe diameter of one and one half million. Also, at this point the ablator characteristics and the wall conditions are known from heat shield analysis. The test program, to be defined here, considers methods of scaling these flight conditions to a wind tunnel test program to obtain parametric data on outgassing contamination.

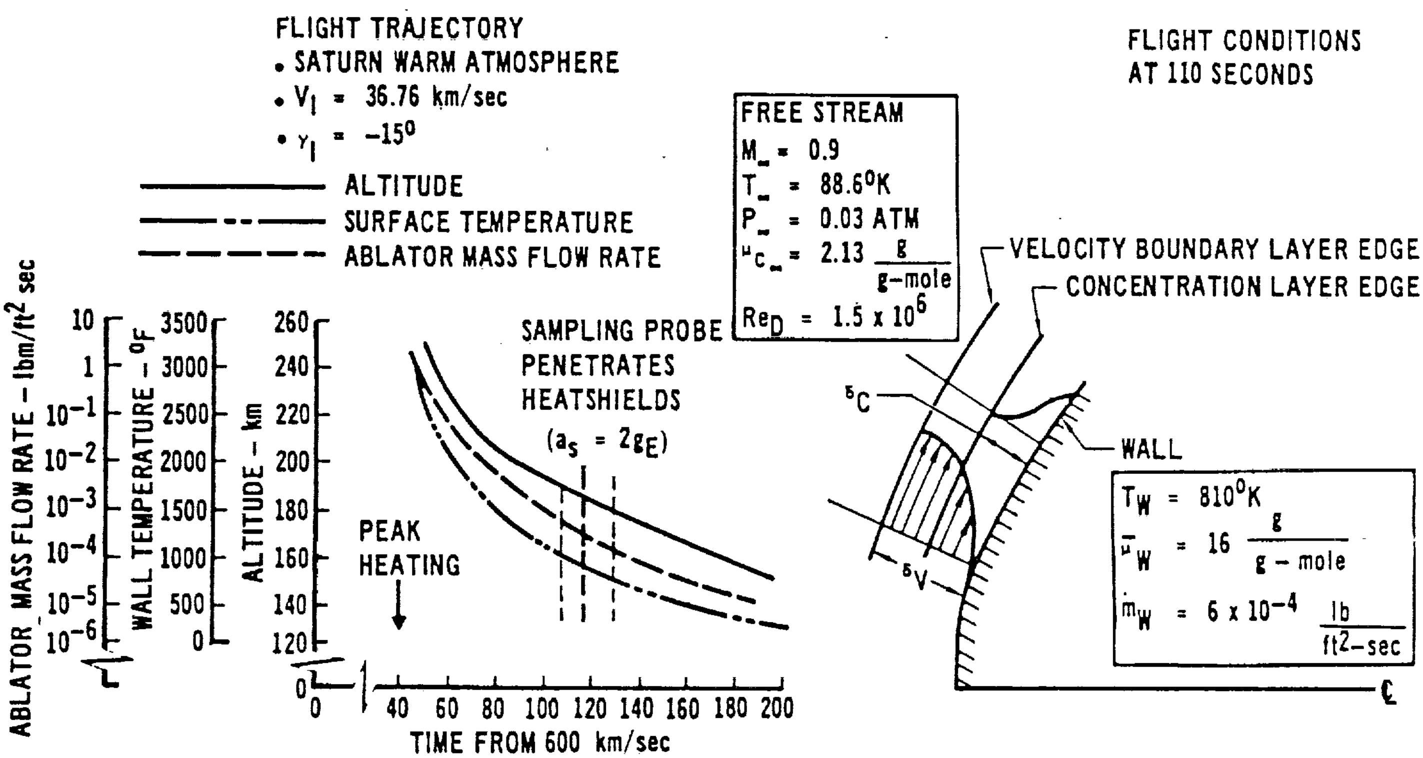


Figure 8-37. Test 2: Flight Conditions

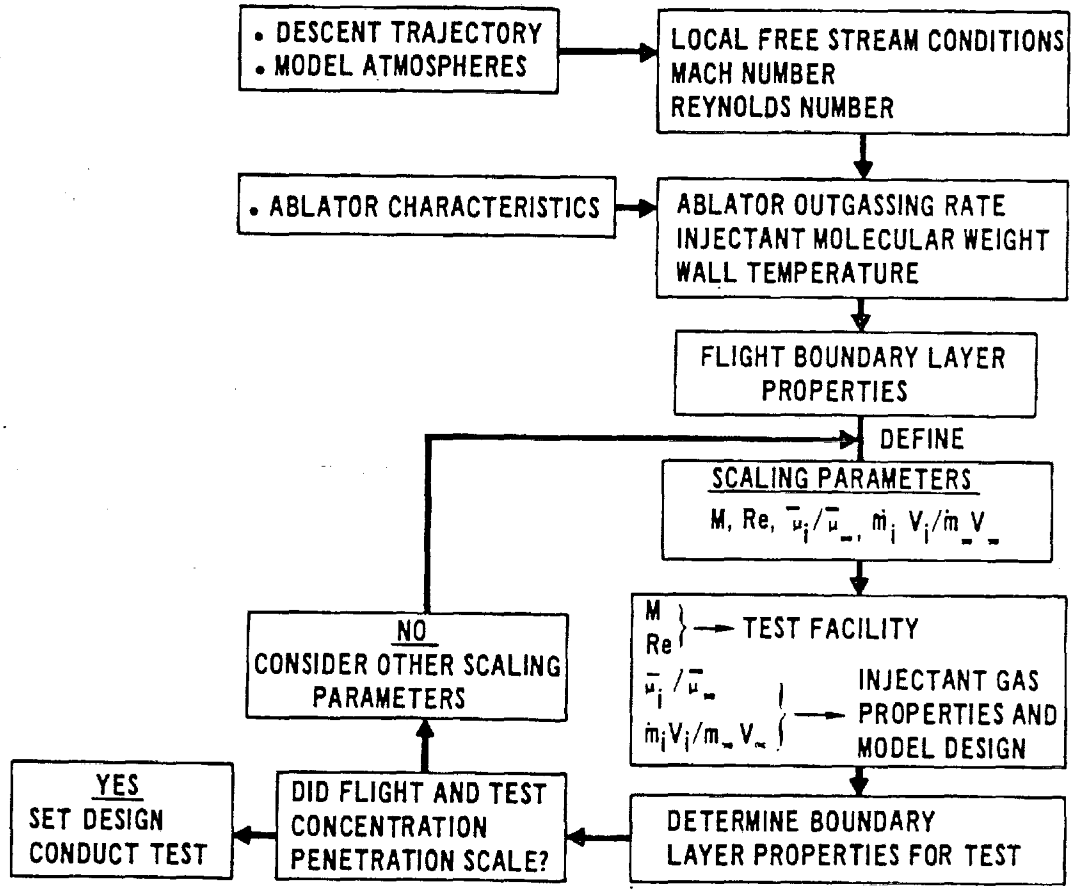


Figure 8-38. Test Definition Flow Diagram

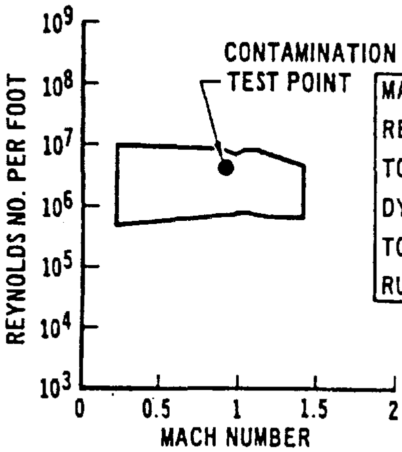
The technique used in the test definition is to define the descent trajectory and the heat shield characteristics (Figure 8-38) so that the flight boundary layer properties can be determined. The objective then becomes scaling these parameters to an inexpensive wind tunnel test program. The Mach number, the Reynolds number, the ratio of the injected gas to free stream molecular weight, and the momentum flux ratio of the injected gas and the free stream are the flight parameters matched in the test. The Mach number and the Reynolds number define the test facility which for these conditions will be a transonic test facility. The molecular weight ratio and the momentum flux ratio determine the injected gas and the mass flow properties of the injected gas. Boundary layer calculations are made for the probe without a sampling tube at the stagnation point and the flight and test boundary layer profiles compared to determine if a simulation was achieved.

In comparing the results, the determination if the contaminant gas (the one that is injected) penetrates the same distance through the velocity boundary layer as it did in the flight case is considered to be the criterion for simulation. These boundary layer computations have been completed and the indicated scaling parameters were found to be the test for simulating the flight conditions.

The test program will be conducted in the NASA Ames two-foot by two foot transonic test facility. Figure 8-39 illustrates the envelope of the test conditions and where the contamination test point is located. The schematic on the right is the test model. The model has a permeable forebody, the center is the plenum chamber for the contaminate gas. The plenum will be supplied with a heavy molecular weight gas that diffuses through the permeable forebody and into the boundary layer to simulate the heat shield out-gassing under flight conditions. Parametric data will be obtained in the program by varying the angle of attack range from zero degrees to twenty degrees, the sampling tube length from zero to twice nominal, and the injected mass flow rate by a factor of five (greater and less) about the nominal.

An on-line mass spectrometer will measure the presence of the contaminant gas in the sampling tube.

In conclusion (Figure 8-40) the retained heat shield concept requires various proof of concept tests to demonstrate the feasibility of penetrating the heat shield and the cleanliness of the mass spectrometer sample. Test programs have been defined to demonstrate these points and we are currently in the process of conducting these tests.



MACH RANGE:	0.2 TO 1.4
REYNOLDS NUMBER ($\times 10^6$/ft):	0.5 TO 8.7
TOTAL PRESSURE (psia):	2.3 TO 44.1
DYNAMIC PRESSURE (psf):	60 TO 2175
TOTAL TEMPERATURE (°R):	580
RUN TIME:	CONTINUOUS

FLIGHT CONDITIONS MATCHED IN WIND TUNNEL TEST:

- MACH NUMBER
- REYNOLDS NUMBER
- MOLECULAR WEIGHT RATIO
- MOMENTUM FLUX RATIO

TEST PARAMETERS

ANGLE OF ATTACK: 0° – 20°

SAMPLING TUBE LENGTH: 0.0 – 0.2 INCHES

INJECTANT MASS FLOW RATE: $0.5 \times 10^{-2} - 3 \times 10^{-2}$ $\frac{lb_m}{ft^2-sec}$

MEASUREMENTS

PRESENCE OF CONTAMINATE GAS

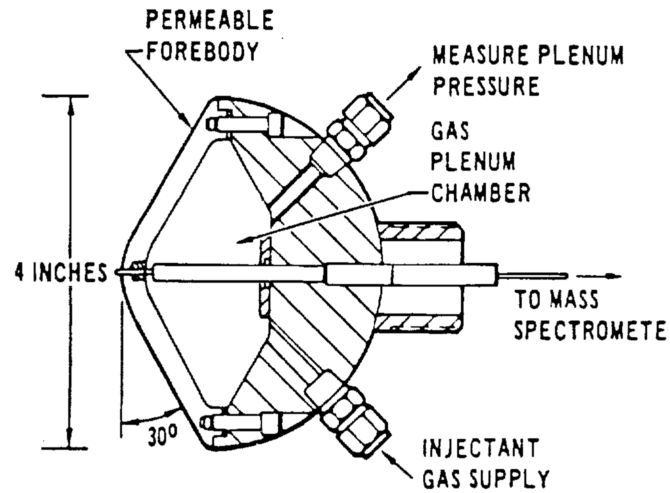


Figure 8-39. Test 2: Transonic Wind Tunnel Test Program

- THE RETAINED HEATSHIELD CONCEPT POTENTIALLY PROVIDES A HIGHLY RELIABLE MINIMUM WEIGHT ENTRY PROBE DESIGN.

- PROOF-OF-CONCEPT TESTING IS REQUIRED TO DEMONSTRATE HEATSHIELD PENETRATION AND TO ENSURE AGAINST SAMPLING CONTAMINATION.

- TEST PROGRAMS HAVE BEEN DEFINED AND WILL BE CONDUCTED TO PROVIDE THE NECESSARY DATA FOR EVALUATING THE RETAINED HEATSHIELD CONCEPT.

Figure 8-40. Summary

CLOUD DETECTING NEPHELOMETER FOR THE PIONEER-VENUS PROBES

Boris Ragent
NASA Ames Research Center
Jacques Blamont
University of Paris

N75 20404

MR. RAGENT: I would like to describe for you our experiences in developing a cloud detecting nephelometer for the Pioneer-Venus probes. Since this effort is still in progress, this is in the nature of a preliminary report and we are still involved in testing and proving the apparatus. Obviously, the nephelometer on the Pioneer-Venus probe will have a great deal in common with the nephelometers that have been suggested for the outer planet probe missions. Many of the problems to be faced on Pioneer-Venus are very similar to problems that will arise on the other planetary entry probes.

The presence of clouds in the Venus atmosphere, as well as in the atmospheres of the outer planets, has been well documented and the importance of these clouds in affecting the energy balance on the planet's surface and its atmosphere, as well as in strongly affecting atmospheric dynamics, has been extensively discussed. During the early spring of 1972, a Science Study Group attempting to define the experimental payload for the Venus mission strongly recommended that a cloud detecting nephelometer be investigated for possible inclusion into the small probe experiment package. A nephelometer is a device for measuring cloudiness or documenting an aerosol from a measurement of the amount of light scattered from an illuminated volume containing a sample of the cloud or aerosols. The purpose of this equipment was to be to document the presence of clouds, their vertical structure or extent, and from the multiple probe data, to provide some guides as to the global variability of this cloud structure. In their deliberations, the SSG considered a number of alternative approaches to cloud measurement and the recommendation for a nephelometer resulted. This was because only the nephelometer appeared to offer the promise of cloud detection without radically altering the design of the pressure shell of the probes, or requiring the erection of external equipment, while conforming to the requirements imposed by the mission constraints.

At that time, there was, and still remains, considerable doubt as to the composition of the clouds of Venus. The thin upper hazes, extending from altitudes of about 63 to 68 kilometers exhibit a layered structure, as shown by the Mariner 10 results. The uppermost cloud layers, starting at about 60 kilometers, appear to be composed of very concentrated sulfuric acid particles of modal radius about 1.0 microns, index of refraction 1.45 and concentrations estimated at anywhere from 50 to 500 per cubic centimeter, whereas particle concentration estimates for the hazes range from 1 to 100 particles per cubic centimeters. Conjectures about the composition of the deeper clouds involve, for example, such unpleasant compounds as various halides and sulfides of mercury, antimony and ammonia, carbonyl sulfide, and even extend to suggestions of clouds of pure mercury droplets.

In any event, the specifications for the instrument were, very severe, involving detection sensitivities for particulates from what, on Earth, would be called "clean room" conditions, corresponding to visibilities of 10 km or greater, all the way to cloud conditions which may be denser than any known on Earth. Because of the mission constraints, any such instrument would have to be capable of operation on probes entering in either sunlit or dark regions of the planet, be limited to mission physical constraints, including a launch weight of about 500 grams, an average power consumption after atmospheric entry and during the one-hour descent, of about one watt, a volume of about 500 to 700 cubic centimeters, be capable of surviving the severe entry environment into the Venus atmosphere involving decelerations of 400 to 500 G's, and to continue functioning as deep into the ambient atmosphere as possible, preferably to the surface, where conditions are approximately 750°C and 90 to 100 atmospheres. A summary of the required specifications is shown in Figure 8-41.

DESIGN GOALS

Total Instrument

Weight	454 grams
Volume	524 cm^3
Power	1 watt (average)
Data Transmission Rate	$\leq$ 16 bps (large probe) $\leq$ 16 bps above 30 km a (small probe) $\leq$ 4 bps below 30 km (small probe)
Internal Calibration	Must check instrument calibration during entry

Backscatter Channel

Least Count	$\leq$ 10%
Signal/Electronics Noise	$\geq$ 1 for 3 particles/cm^3 1.1μ radius, n = 1.45 (high altitude haze layer) >> 1 for 700 particles/cm 1.1μ radius, n = 1.45 (visible cloud tops)
Signal/Particle Shot Noise	$\geq$ 1 for 3 particles/cm 1.1μ radius, n = 1.45, unattenuated sunlight (high altitude haze layer)
Background/Signal	< 10^6 (limited by saturation of detector)
Dynamic Range	Detector: 10^6 Backscatter Channel: 10^5
Altitude Resolution	$\leq$ 300 meters

Background Channels

Wavelengths	Near UV Visible Near IR (if possible)

Monitor Channels

Window Contamination	Must monitor optical quality of windows
Temperatures	Must monitor temperatures of critical components

Figure 8-41

A very heavy emphasis in the Pioneer-Venus program has always involved reliability coupled with low cost and the assurance of low risk for cost overruns. These ground rules lead to a derived emphasis on off-the-shelf types of proven hardware or components where possible and a somewhat greater reluctance to rely upon long lead time development items or unproven approaches. We first conducted a feasibility study that convinced us that the desired instrument was within the state-of-the art, subject to all of the above constraints involving the mission costs and time.

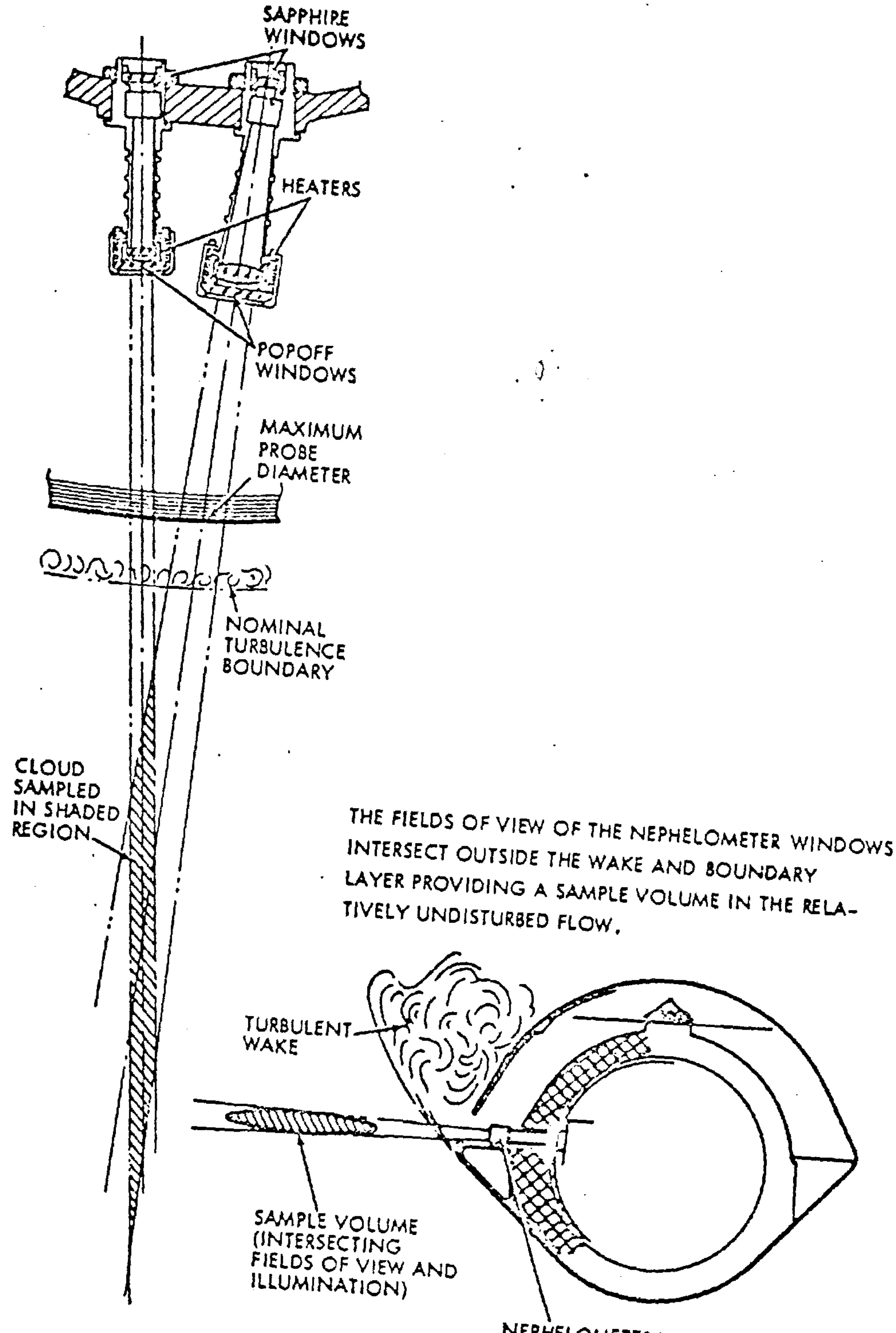
A number of conceptual designs were initially considered. Early ground rules based upon the above thoughts led us to de-emphasize concepts which involved the mechanical erection of any structures outside of the pressure vessel after the very severe deceleration and heating pulse associated with entry into the Venus atmosphere, and structural considerations for the probe made the construction of a "sampling" or reentrant design undesirable. We were, thus, faced with attempting to measure clouds from roughly within the available configuration of the pressure vessel. Since some of the probes were to enter on the dark side of the planet, it was necessary to include a light source as an essential component rather than relying upon ambient sources of radiation. The on-board source would then have to illuminate a sampled region and light-scattered from this region be detected on-board. Our self-imposed proscription against reentrant geometries, pumping samples on-board, or the erection of mirrors, or other optical elements, thus, limited us to scattering in the rearward direction at angles greater than 145° from the direction of incidence of the illuminating light. Again, availability of components and sensitivity considerations led us roughly to choose the visible range of wavelengths for consideration. Further investigation of the information to be obtained from multiple wavelength or polarization measurements made in the restricted range of available scattering angles (within the types of projected accuracies obtainable) led us to the conclusion that very little additional information was to be obtained about the nature of the clouds from multiple wavelength or polarization measurements. As a result, we chose to work at a wavelength of about 9000 Å, for which convenient, powerful solid state sources and sensitive solid state detectors are available and at a scattering angle near 180°, at which angle the scattering is greatest for backward scattered radiation.

Since some of the probes would be entering in the sunlight, a very high level of ambient light would be expected in the visible wavelengths, especially high in the atmosphere. As a result discrimination between ambient background and the on-board light source was necessary, leading to the requirement for a narrow wavelength band source and filtering for the detector. Even with optical filtering, because of the high possible background light levels, as well as for electronic considerations, a pulsed light source and synchronous detection techniques were essential in order to encompass the enormous range of expected signals and to provide the required stability. Since the expected range of signals extends at least over a range of 10^4, a dynamic range of 10^5 was the design goal.

From the start it was evident that sensitivity at the low end of the range was the major problem. Limitations on the available power and on the light sources made it mandatory that we design for the highest possible sensitivities from our detector, and as a corollary, the lowest electrical noise level in our electronics. The optical design, also, had to be very carefully considered with a view toward signal maximization. Low f/number optics are essential in order to collect as much of the light from the source as possible and focus it into the required sampling volume. The effective magnification of the source determined the size of the source beam at the sampling volume. Maximum signal considerations, then, dictated that the image of the detector at the sampling volume be of about the same size as the source, leading also to a low f/number optical system. Further, the size of collecting aperature had to be as large as possible, so as to effectively collect the scattered light. The physical configuration of the nephelometer and the entering probe is shown in Figure 8-42.

The actual limitation on the optics apertures was set by considering the power required to heat the sindows in order to isolate the instrument from the outer environment. A study performed by the Pioneer Office showed that because the probe surface is cool with respect to the atmosphere, condensation of the atmosphere onto a probe window is to be expected, unless the window surface is maintained at a temperature somewhat above the ambient. Because the window heating power is so large and goes as some power of the window diameter, it was desirable to minimize the window size. Considerations of signal-to-noise dictated a large window so that a compromise value had to be established. At this time, a value of 2.5 centimeters has been chosen for both the source and detector apertures. Further development in sources may allow us to reduce at least the source aperture.

For the typical configuration shown in Figure 8-42, an analysis of signal-to-noise was made using quoted source and detector characteristics, the geometry and a postulated aerosol haze composed of a narrow size distribution of spherical particles of modal radius 1.1 microns and index of refraction 1.45. The ambient background light was also calculated as a function of the angle of scatter from the sun into the detector (assuming only single scatter). The nosie contribution was calculated as coming from both electrical noise (Johnson noise, shot noise and 1/f noise) and noise due to functuations in the ambient background signal due to statistical fluctuations in the sampled volume caused primarily by the motion of the probe in moving the sampled volume. This latter noise is obviously dependent on the phase angle of the sun relative to the viewing path. These calculated values of signal-to-noise and background showed that the required values of sensitivity could be achieved.



Nephelometer Field of View and the Turbulent Wake

Figure 8-42

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It was now necessary to actually build a laboratory instrument to demonstrate the feasibility of the proposed design. A crude breadboard instrument was constructed and tested. The design for this breadboard was based on an initial, hurried design study which included recommendations for component hardware, and which was later verified by a more detailed study conducted by TRW Systems Group. A typical breadboard device is shown in Figure 8-43. The units consist of solid state light source, a solid state detector, source and detector optics, an optical filter in the detector channel, appropriate driver and signal processing electronics and a mechanical structure to properly contain and orient the components.

Two versions of the initial device were built, the first using a novel (but space-unqualified) double heterostructure GaAs solid state laser, capable of operation at peak powers of several hundred milliwatts with microsecond pulses at duty cycles of 5 to 10%q and a second using a space-qualified, high powered GaAs light emitting diode. Both units used a silicon PIN photodiode as a detector. Appropriate electronics using synchronous detection techniques were developed and tested. In this mode of operation, the detector output only contributes to the output of the detector when the light source is pulsed. It is, thus, possible to use the output of the detector when the light source is off as a measure of the ambient light striking the detector. This feature was also built into the design.

The first breadboard was crudely tested on the laboratory bench by mapping out the extent of the sampling volume and attempting to use targets with roughly known scattering cross-sections and a bench type of small fog chamber. It was then tested in a better defined fog environment in the fog chamber at the University of California, Richmond Field Site. Figure 8-44 shows such a test in progress. The instrument is attached to a boom ahead of the cab vehicle and is then "flown" into a precalibrated fog of known characteristics. In another type of test,

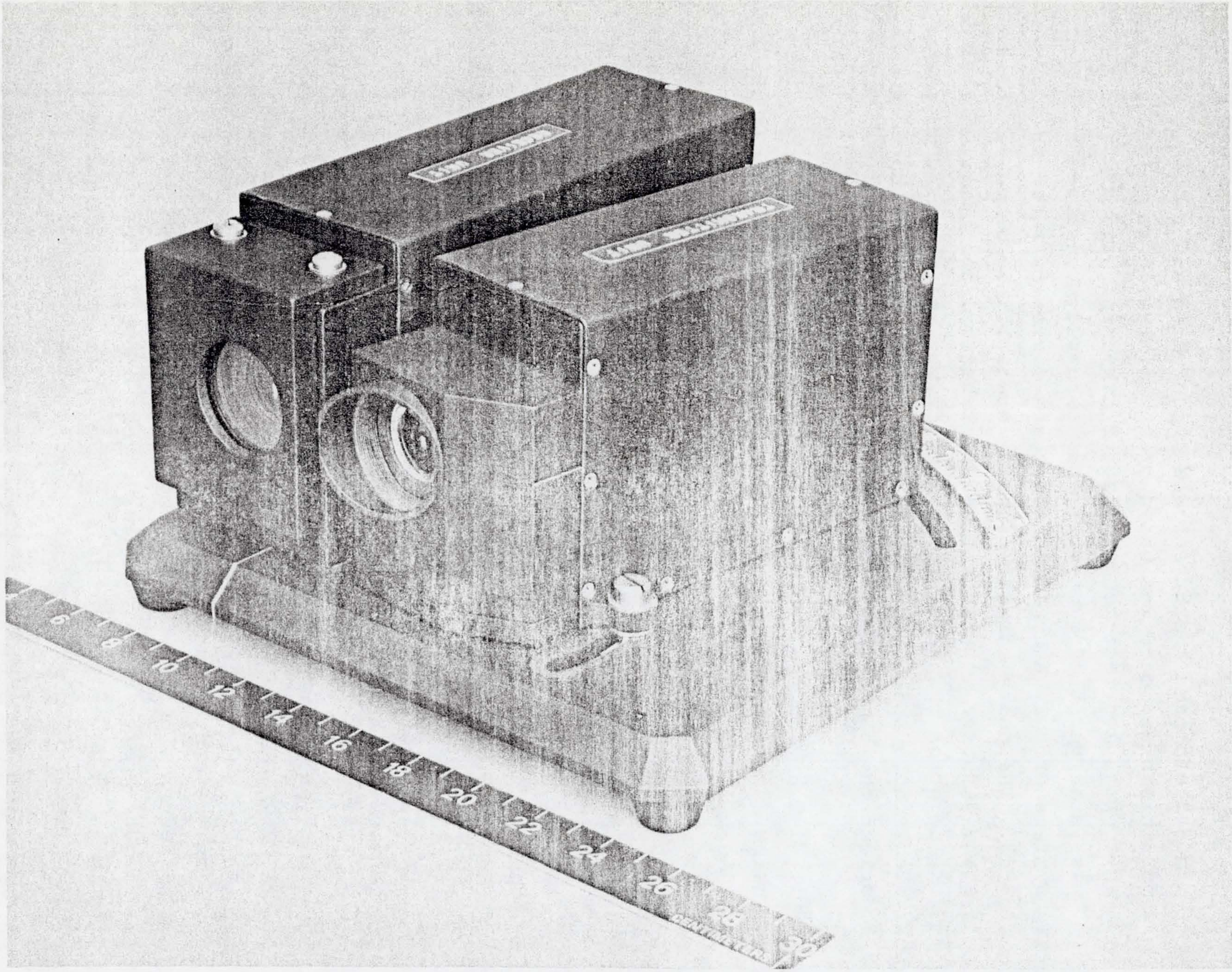

Figure 8-43

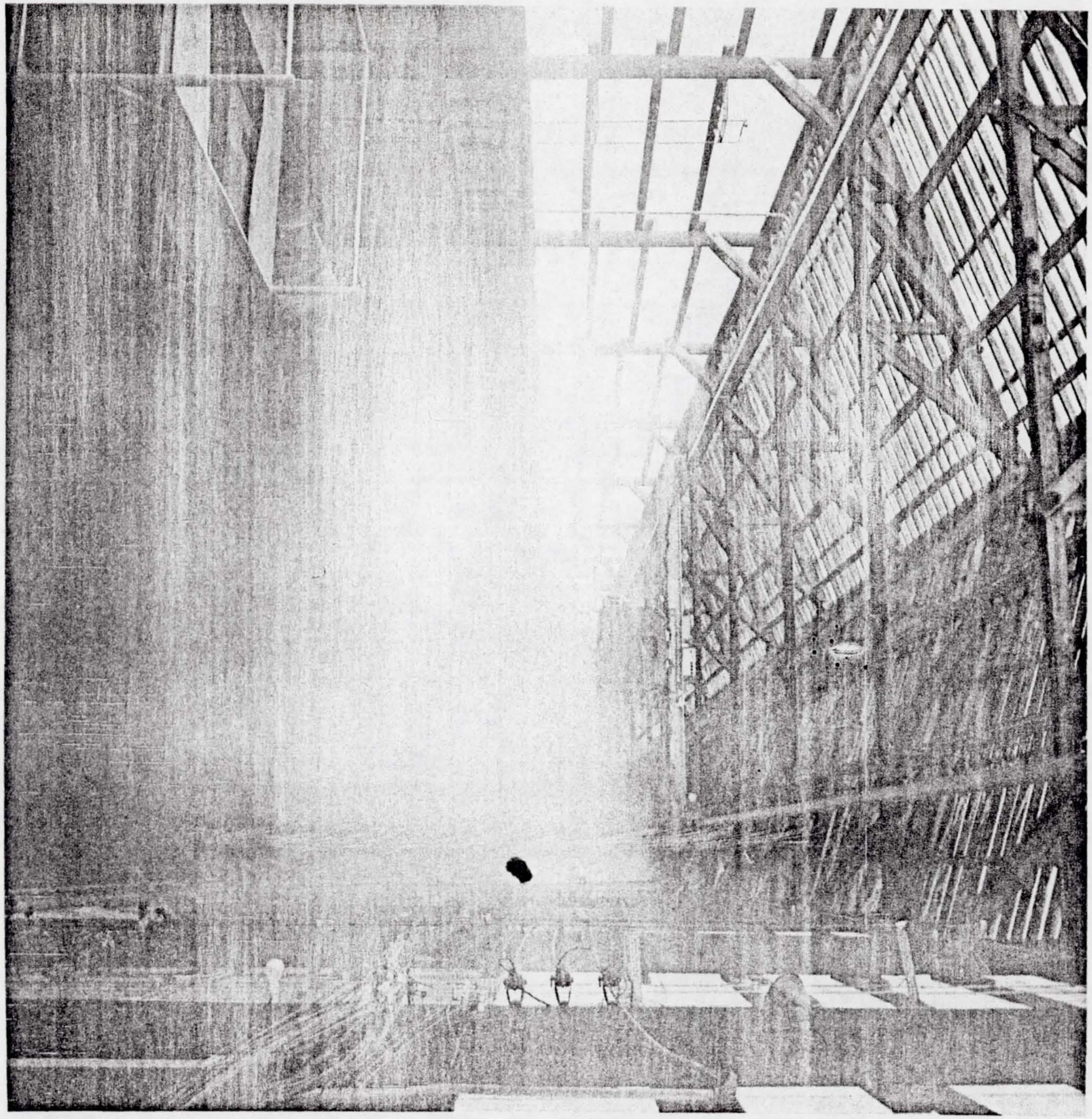

Figure 8-44

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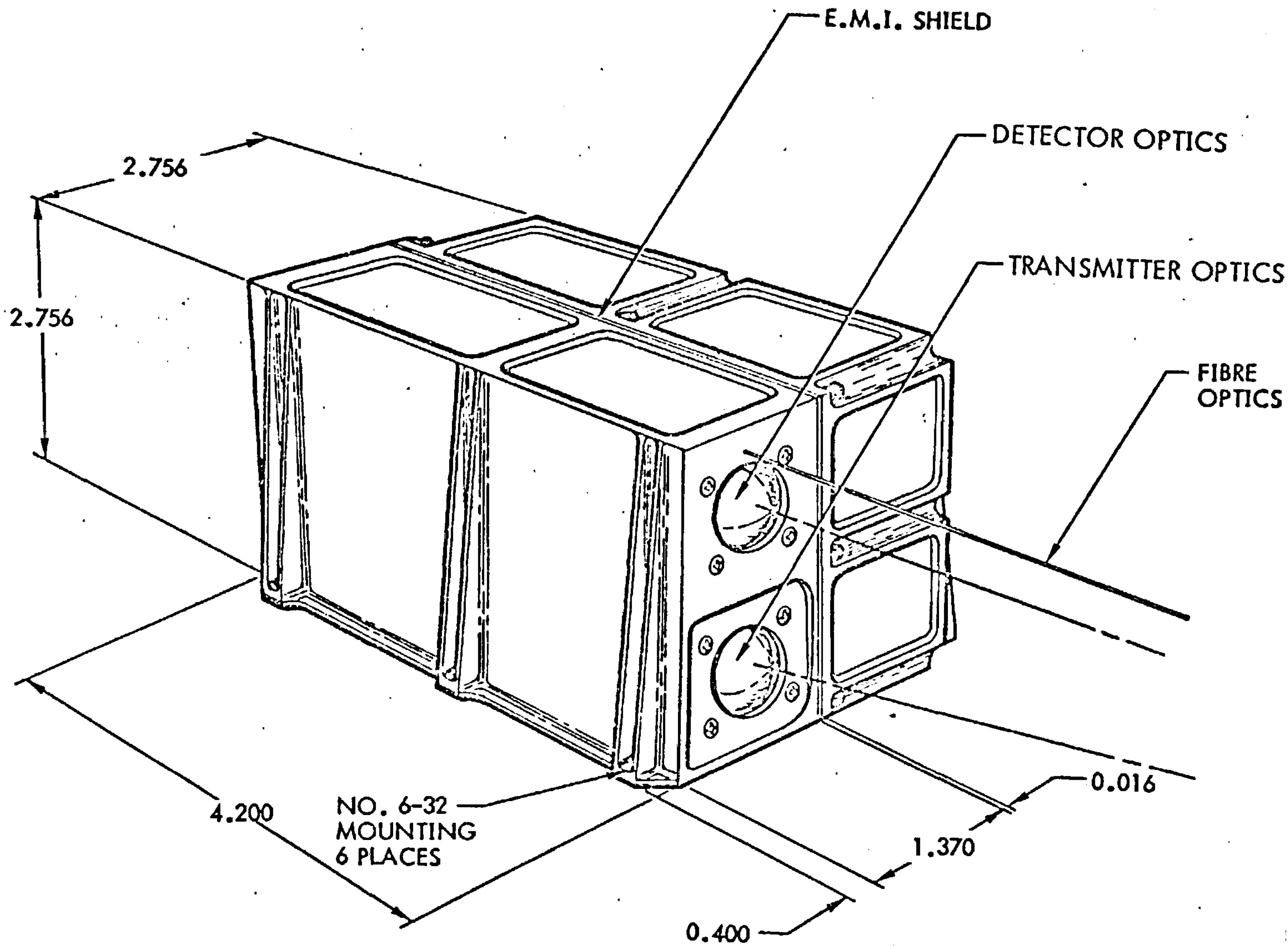
the unit was mounted on the top of an automobile and driven through a naturally occurring fog on the Northern California Coast.

The breadboard model constructed by TRW was the result of a much more extensive study than our early one and involved careful consideration of the optical design, component selection, component performance and component environmental tests, the electronics system design, and mechanical design. Actual cloud measurements using this unit are now being planned in conjunction with a Colorado State University Flight Research aircraft which has been instrumented for cloud and other atmospheric measurements. We also hope to fly this breadboard on the same flights with an instrument being developed for particle size analysis on the Pioneer-Venus large probe. We hope to fly these tests in June and July.

Finally, the specific implementation of such a nephelometer for use aboard the Pioneer-Venus small probes was considered. Packaging, including minimization of weight and volume, power, monitoring of major components and window conditions, data formatting and other necessary parameters were carefully considered. A concept of the final flight package is shown in Figure 8-45. Because there must be a very intimate interfacing of our instrument with the probe window structure to be provided by the probe contractor, the final design, especially of the interfaces, must await final decisions on probe configurations.

I also wish to mention that in this experimental package, we have incorporated a small subsidiary experiment. We have added two additional off-axis detectors and filters to the detector package. These will be used to measure the ambient light level in ultraviolet and visible spectral regions in order to provide some data on the optical thickness of the atmosphere at these wavelengths. Mariner 10 pictures and Earth-based observations have indicated upper atmospheric structural features, but showed none in the visible.



PIONEER VENUS NEPHELOMETER

Figure 8-45

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The design status of the instrument, as compared with the originally drawn set of requirements, is shown in Figure 8-46. The weight and power are somewhat larger than our original estimates, but are subject to possible downward revision, depending on probe interfacing questions.

Figure 8-46. Nephelometer Design Status

Quantity	Requirement	Status	Determined by
Weight	454 grams	600 grams	Packaging Design and Analysis
Volume	524 cm^3	524 cm^3	Packaging Design and Analysis
Power	1 watt (ave)	1.33 watts (ave)	Analysis
Data Transmission Rate	$\le$ 16 bps (large probe) $\le$ 16 bps above 30 km } small probe $\le$ 4 bps below 30 km } small probe	16 bps (large probe) 16 bps above 30 km } small probe 4 bps below 30 km } small probe	Design
Internal Calibration	Must check instrument calibration during entry	Relative calibration of all detectors and LED source strength checked approximately every 10 minutes	Design

Figure 8-46 **Nephelometer Design Status**
(Continued)

Backscatter Channel

Quantity	Requirement	Status	Determined by
Least Count	$\leq$ 10%	< 10%	Design of Coding Schema
Signal/Electronics Noise	> 1 for 3 particles/cm^3 1.1μ radius, n = 1.45 (high altitude haze layer)	$\geq$ 1.07 ~ 0.8	Analysis Test*
	>> 1 for 700 particles/cm 1.1μ radius, n = 1.45 (visible cloud tops)	$\geq$ 251 ~ 180	Analysis Test*
Signal/Particle Shot Noise	> 1 for 3 particles/cm 1.1μ radius, n = 1.45, unattenuated sunlight (high altitude haze layer)	> 1 for at least 1/2 of the azimuth	Analysis
Background/Signal	< 10^6 (limited by saturation of detector)	< 10^6 for at least 2/3 of the azimuth	Analysis
Dynamic Range	Detector: 10^6 Backscatter Channel: 10^5	> 10^6 10^5	Test Design of Data Processing System, Test
Altitude Resolution	$\leq$ 300 meters	< 300 meters	Selection of Sampling Rates

Figure 8-46. Nephelometer Design Status
(continued)

Background Channels

Quantity	Requirement	Status	Determined by
Wavelengths	Near UV	3200 Å to 3900 Å	Absorption Corning Glass 7-37
	Visible	4600 Å to 5900 Å	Filter Selection Corning Glass 4-64
	Near IR (if possible)	Eliminated	Trade-off of Science Return Vs. Weight, Volume, and power penalties

Monitor Channels

Quantity	Requirement	Status	Determined by
Window Contamination	Must monitor optical quality of windows	Cleanliness of LED window monitored	Design*
Temperatures	Must monitor temperatures of critical components	Monitor three temperatures corresponding to detector block, LED heat sink, preamplifiers	Design

*Two different techniques for monitoring window contamination have been proposed. The technique used in the flight instrument will be determined, in part, by the spacecraft contractor.

AN APPLICATION OF GAS CHROMATOGRAPHY TO PLANETARY ATMOSPHERES

Dr. Vance Oyama
NASA Ames Research Center

N75 20405

DR. VANCE OYAMA: I guess the best way to start on a subject that is relatively new to the physical world but has been practiced for years in the chemical world, is to start with something that people can easily relate to. Let us take a Coke bottle and shake it up. When it is cold, very few bubbles occur in that Coke bottle. When you take this Coke bottle and you shake it up when it is warm and open the cap, out comes your Coke in a sudden burst of energy. Essentially, I am talking about the process of partition in a two phase system, a gas and a liquid.

In the liquid system, you have dissolved carbon dioxide in the above case. When the dissolved carbon dioxide escapes from the liquid, it causes the ebulition.

In gas chromatography, the same kind of phenomena occur except not so violently. In a system in which you may have stationary phases of liquid, semi-liquid, a polymer or a solid as one phase, and in the gas phase a dissolved solute, the gas tends to move into the solid or the liquid phase until there is an equilibrium set up between the gas and the liquid phase in which the concentration in both phases is a function of the parameters of the system - temperature, phase, gas, etc.

Now suppose that you transfer this gas in the head space to another portion of this system in which you have the stationary phase, but have no solute gas. That gas then re-equilibrates with the new stationary phase and it sets up this particular partition coefficient. This is essentially like saying that there is a certain concentration in the head space and a certain concentration in the liquid phase. Now consider a movement of a stream of gas such as helium moving across the stationary phase. The solute gas tends to move out of the stationary phase and move into the gas phase. The solute gas in the gas phase moves down into the liquid and similarly, along the train, as you can see. If a gas has a strong affinity for the liquid, it will be retained and slowed down in the process; whereas a gas that has limited affinity for the liquid phase will move along the train very rapidly. There is a separation of the two phases.

Figure 8 - 47 is an example of the process I am talking about. On top we have a column coated with a stationary phase of some sort, that has different affinities for X and O molecules introduced into it. A carrier gas, such as helium, drives the binary gas mixture to the right. This gas plug moves along and in the second display the components begin to separate. In the ideal system, the components are separated and you are ready to sample. You want a detector at the column outlet that is able to distinguish from the carrier system - a particular peak has arrived - and is able to quantitate it over a large dynamic range. This is, in essence, gas chromatography. It is a very simple process.

Now how does this differ from mass spectrometry, which is the other mode of composition analysis? Gas chromatography is obviously a high pressure system. It is a high pressure system that can take a high pressure gas, introduce it into the system, and come out with an answer. It does not require a pump. All it requires is some pressurized gas source.

How, again, does this differ from the mass spectrometer? The mass spectrometer impels electrons against the molecules of interest these molecules are fragmented and ionized imparting a characteristic to it that allows fractionation by an electric field and/or a magnetic field. The difference is that the gas chromatograph separates components without changing the structures. The retention time helps to identify the molecule.

Now, the resolution capability of the gas chromatograph will depend primarily upon what you want out of the system. If you want to measure something of low molecular weight, you devise or

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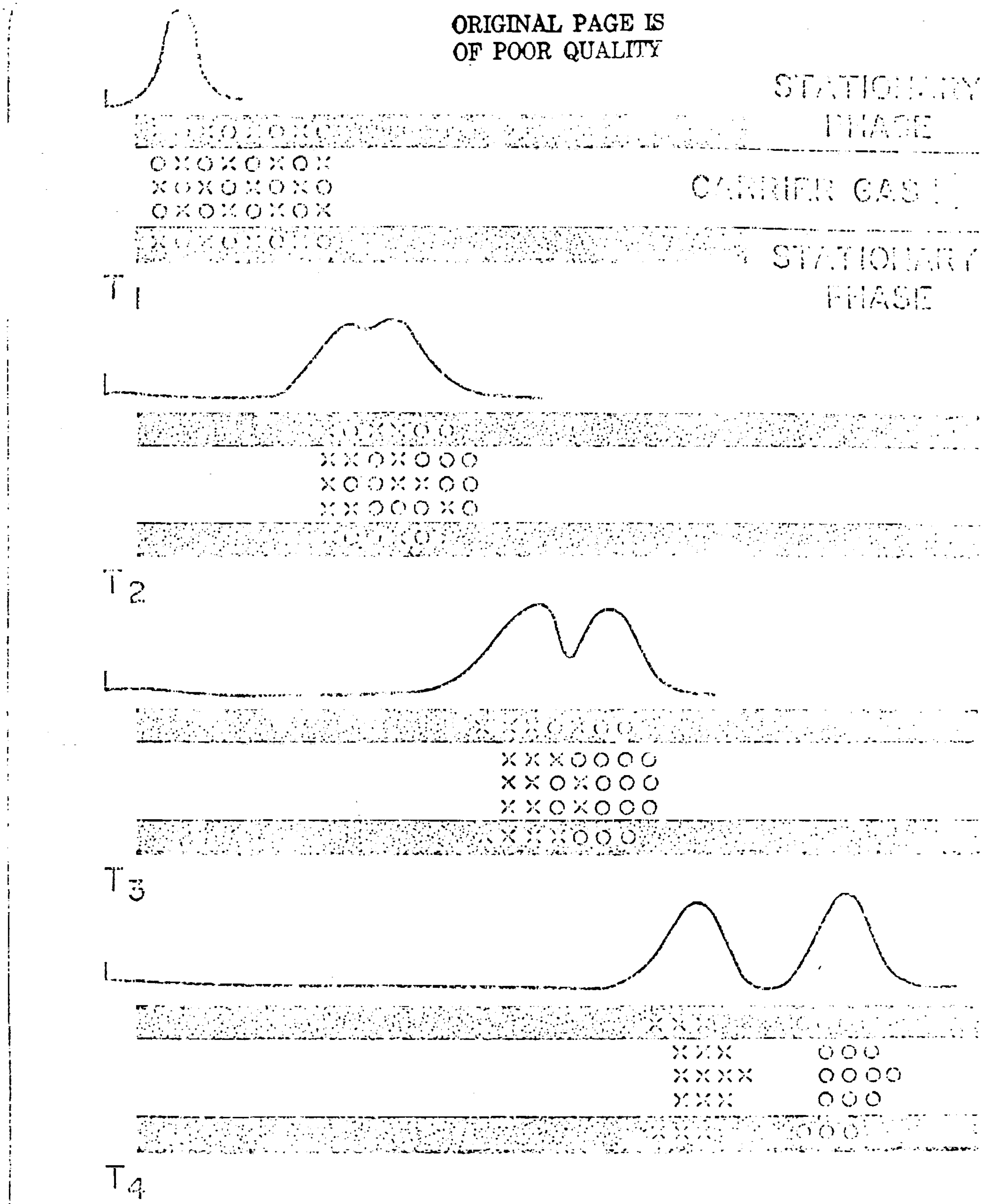

STAGES OF CHROMATOGRAPHIC SEPARATION

Figure 8-47

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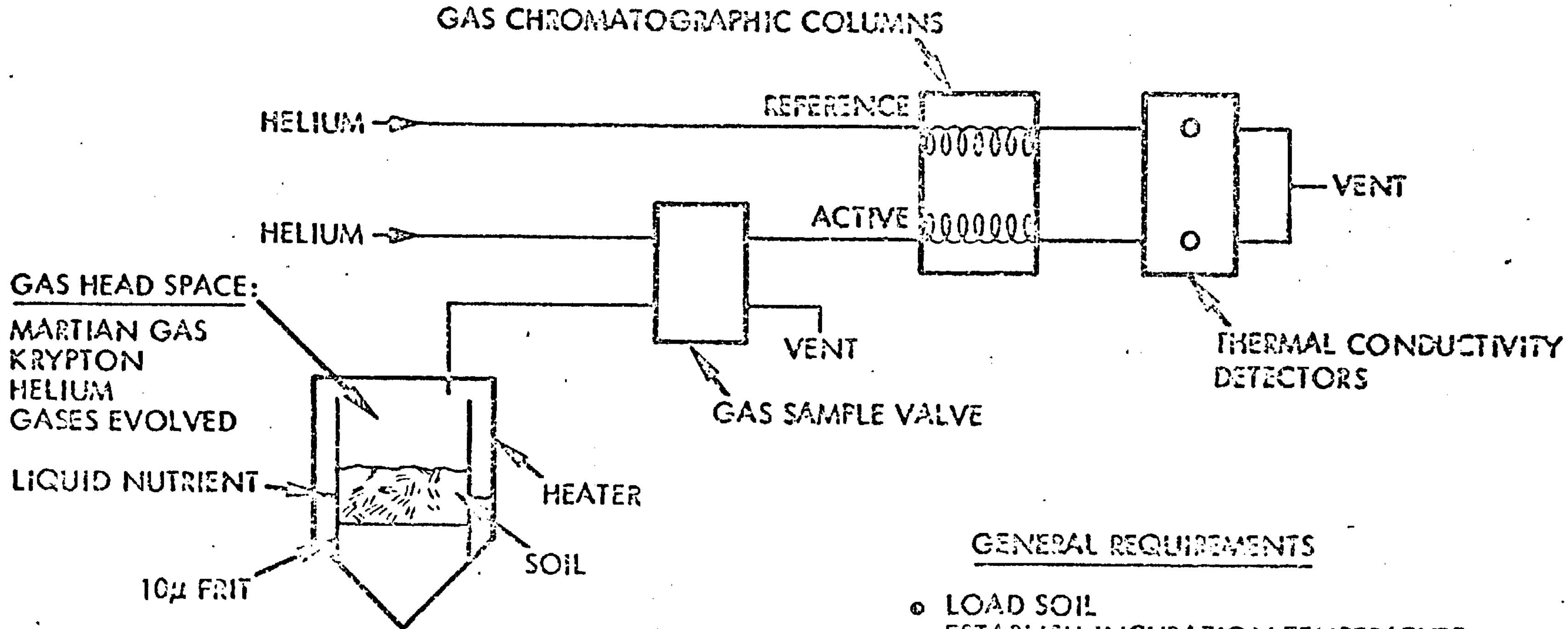
tailor-make your system to get the fastest analysis consistent with the degree of resolution required for the gas species likely to be present. Say you want to measure a cc and you want to do it rapidly, you design a particular system to do just that. The gas chromatograph can be ultimately made to do all the gas analyses one requires. For example, the gas chromatograph can separate isotopes. Contrary to belief, the reason that these processes have not been performed routinely on a laboratory scale is simply because the conditions for these analyses are not usually attainable. For example, it is possible to separate molecular hydrogen from HD by running the column of, say, aluminum oxide at temperatures of about minus seventy degrees centigrade. If you want to take a spacecraft and go through space, cool it down, and run these columns at the temperature, you can make these kinds of separations. So, it is really what the particular people want out of the system that we can design to.

In the case of the Viking experiment, a gas chromatographic system is provided which measures the head space in a chamber. We hope to find biological activity present there.

The system as shown on Figure 8-48 consists of a chamber, which provides the head space. Soil is introduced into the chamber and gas and liquid nutrient added. A sample of the head space fills the sampling system by utilizing the martian ambient pressure. The greater head pressure of the chamber allows us to move gas through the sampling assembly by appropriate valve actuations. The sampler then injects into the carrier stream the sample of gas. This is a volumetric sample and is not something that is measured because of the capillary flow. Having a volumetric sample allows us to estimate the concentration of every gas that is in that volume provided pressure is known and all gases that enter the column enter the detector.

In the Viking GEX a thermoconductivity detector - thermistor heads - are used. The helium flow in the reference leg going

GAS EXCHANGE EXPERIMENT



GENERAL REQUIREMENTS

- LOAD SOIL
- ESTABLISH INCUBATION TEMPERATURE
- ESTABLISH MARTIAN ATMOSPHERE
- ADD KRYPTON AND HELIUM
- ADD LIQUID NUTRIENT
- SAMPLE AND ANALYZE HEAD SPACE FOR CHANGES IN COMPOSITION 7 TIMES DURING INCUBATION PERIOD

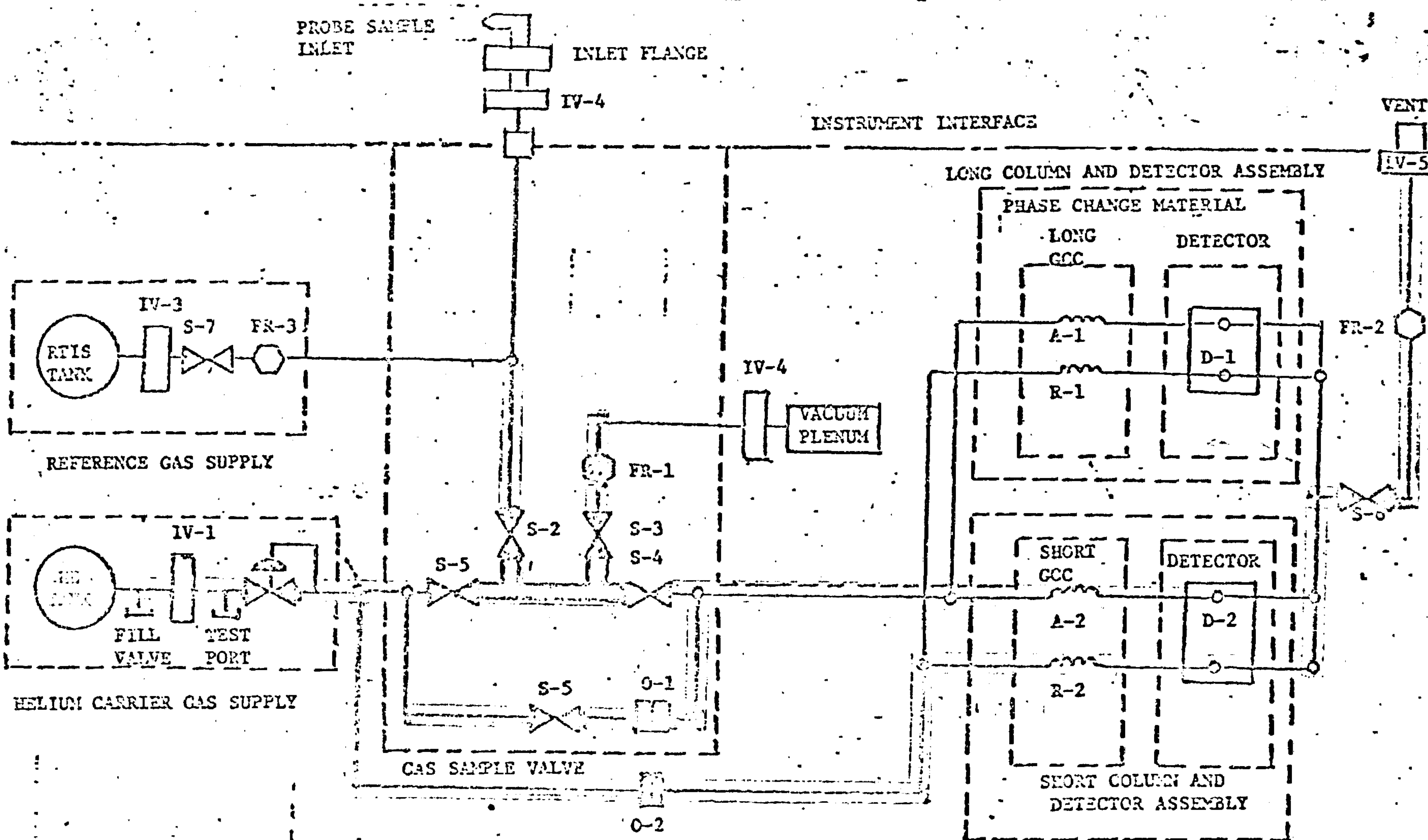
Figure 8-48

through an identical column allows us to balance temperature fluctuations which would normally make a thermister type detector unstable. With this system we are able to separate such gases as hydrogen, nitrogen, oxygen, methane, krypton, and carbon dioxide. The reason we have put krypton into this system is to provide an internal standard to the entire system. The internal standards allow us to make corrections of the time in which a gas arrives at the detector to compensate for changes that might have occurred in the system. From this exact measurement of krypton, we are able to get relative retention times. These retention indices are required if we are to define a particular substance in the head space.

The reason we require this is because the thermoconductivity detector is basically a catholic detector, it is a universal detector. It measures everything that has thermoconductive properties that differ from the carrier gas, helium. Since the Viking GEX utilizes only one way of identifying the substance, i.e. thru its retention index, we must be very careful to establish a standard known substance that the retention time is relative to. We have provided krypton as our internal standard.

Figure 8-49 is a schematic of the Pioneer Venus gas chromatograph and because of the basic economy of the mission, emphasis was placed on adopting Viking GEX features. We incorporated the thermister systems to monitor the output of two pairs of columns. We have a single sampling device which allows Venus atmosphere to pass through the sample loop into a plenum continuously during descent of the large probe. The plenum is the simple, enclosed volume of about thirty cm^3. Before entering the atmosphere, the thermoisolation valve is open, exposing the sampling system to the atmosphere of Venus.

Now, the use of two columns in the Venus probe emphasizes the concept of tailor making a system for a particular job. Two columns were required to separate the wide range of gases likely



PVGCAA FLIGHT DESIGN SCHEMATIC

Figure 8-49

VIII-94

NOTES

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to be in the Venus atmosphere and in addition there was a complementary need to support the mass spectrometer.

As the previous speakers have pointed out, in order to get a good analysis of a particular sample gas, you break it down so you can see its fragments. Now, a number of gases are associated together, the resulting fragmentation patterns with coincidental M/e could confuse the analysis. It is for this reason that we felt that it was necessary for us to develop columns which will allow us to make separations that could pose a problem for the mass spectrometer. Therefore, for the short column in this assembly, we designed the column to make the separation of carbon dioxide, hydrogen chloride, water, hydrogen sulfide, carbonyl sulfide and sulfur dioxide, (Figure 8-50). The long column was designed to separate such gases as neon, hydrogen, nitrogen, oxygen, argon, carbon monoxide, methane and krypton (Figure 8-51).

In the long column, not all the bases introduced will traverse the column during the descent period but are retained in the columns. These gases on shorter columns and/or higher temperatures could very well be detected in the period of analysis.

If you will note, although we have tailor-make the columns to make these separations, there are plenty of spaces for unknown objects to appear in our particular system. The virtue of GC in the low molecular weight range is the fact that there is only a limited number of low molecular weight substances. Consequently, we can provide for any vacancies that might occur in our particular system.

Because the major component in the atmosphere of the planet Venus is carbon dioxide, the question is could one really detect the other minor and trace components of interest? Figure 8-52 shows that at 10 bars we have this immense peak for carbon dioxide (top chromatogram), upon which these various components at relatively low levels are detectable.

Now, how do we go to the outer planets? If we take a look at the planet Jupiter, or Saturn, or any of these larger planets, the major components may be helium and hydrogen. If one assumes that

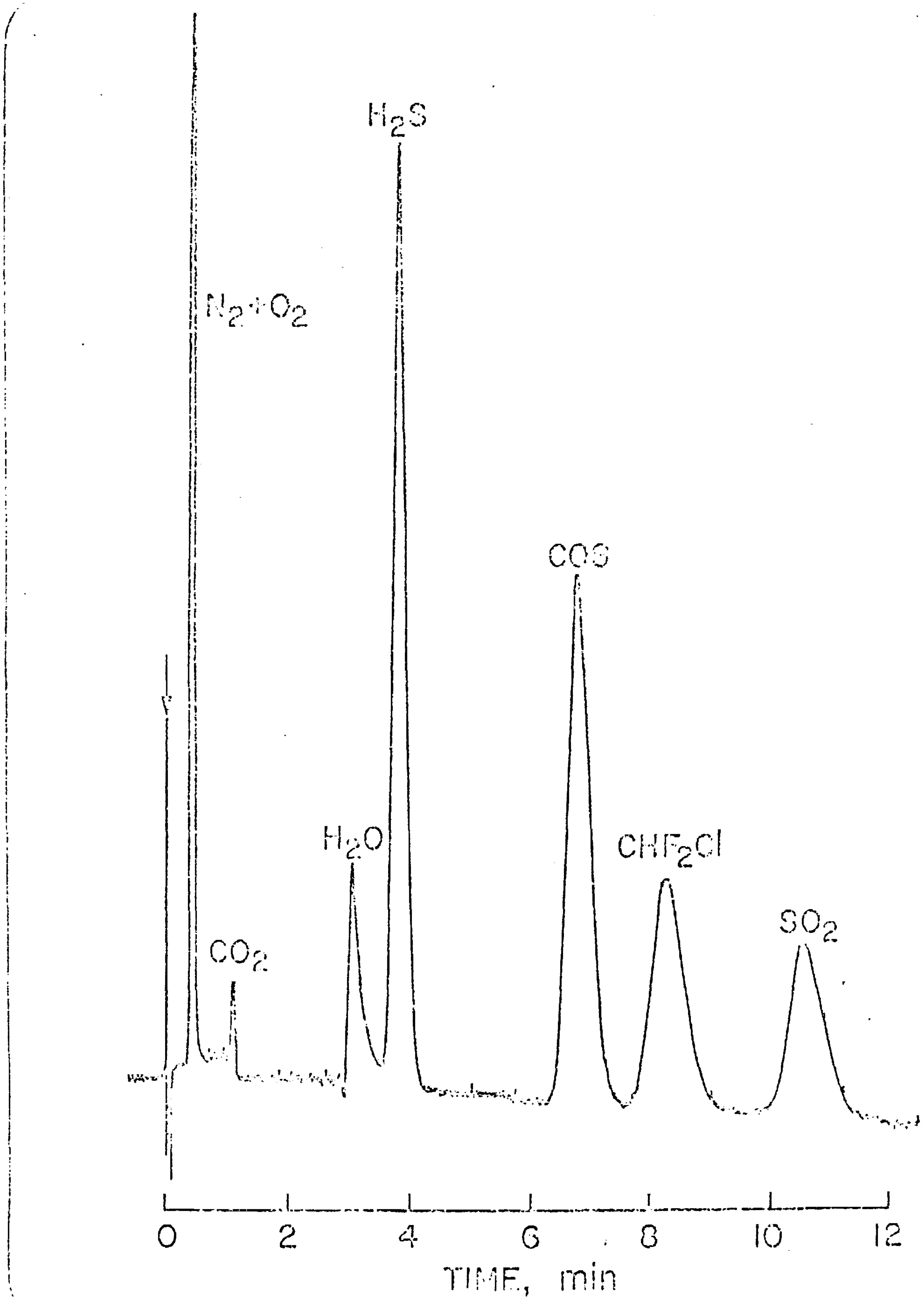
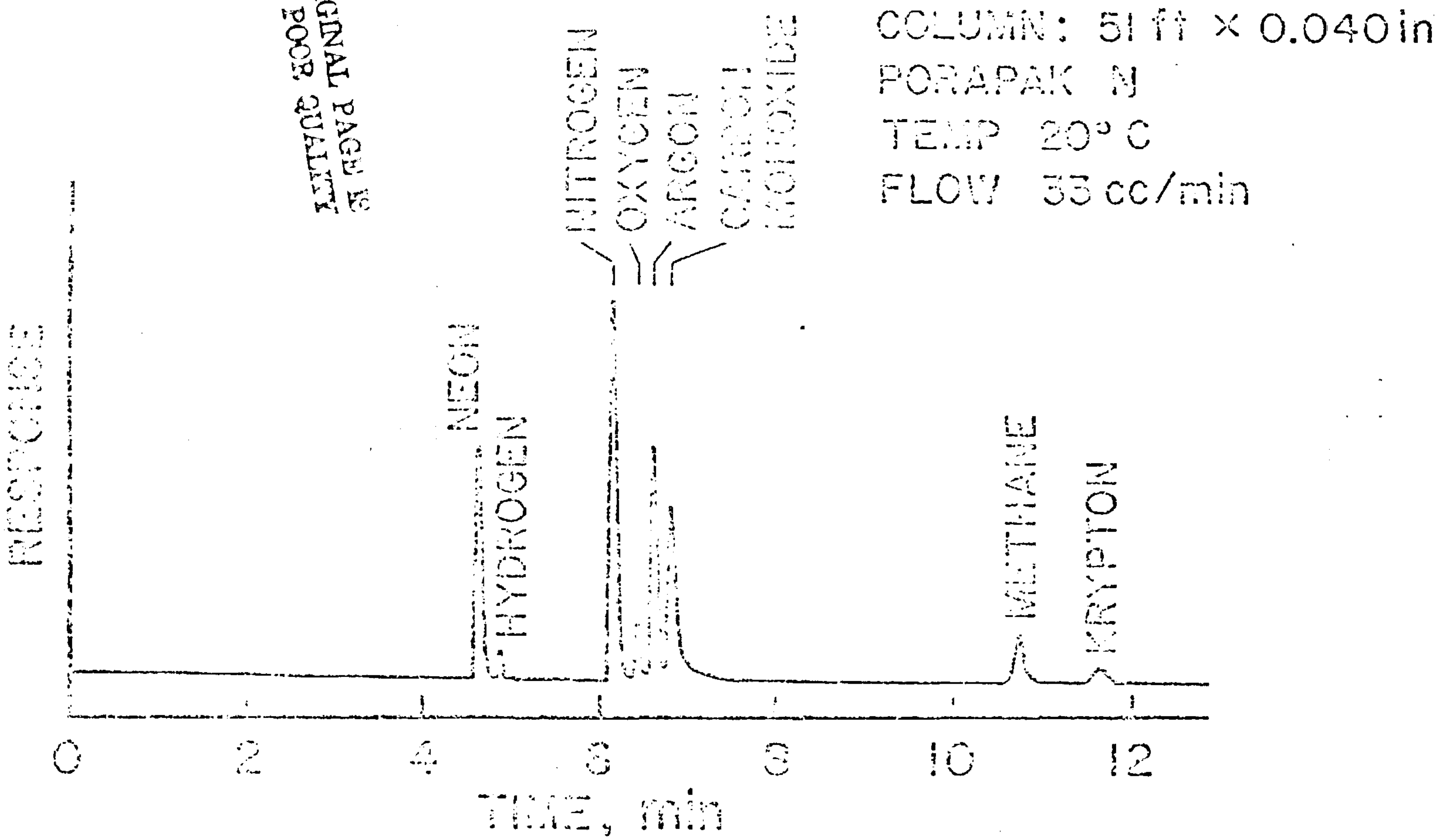
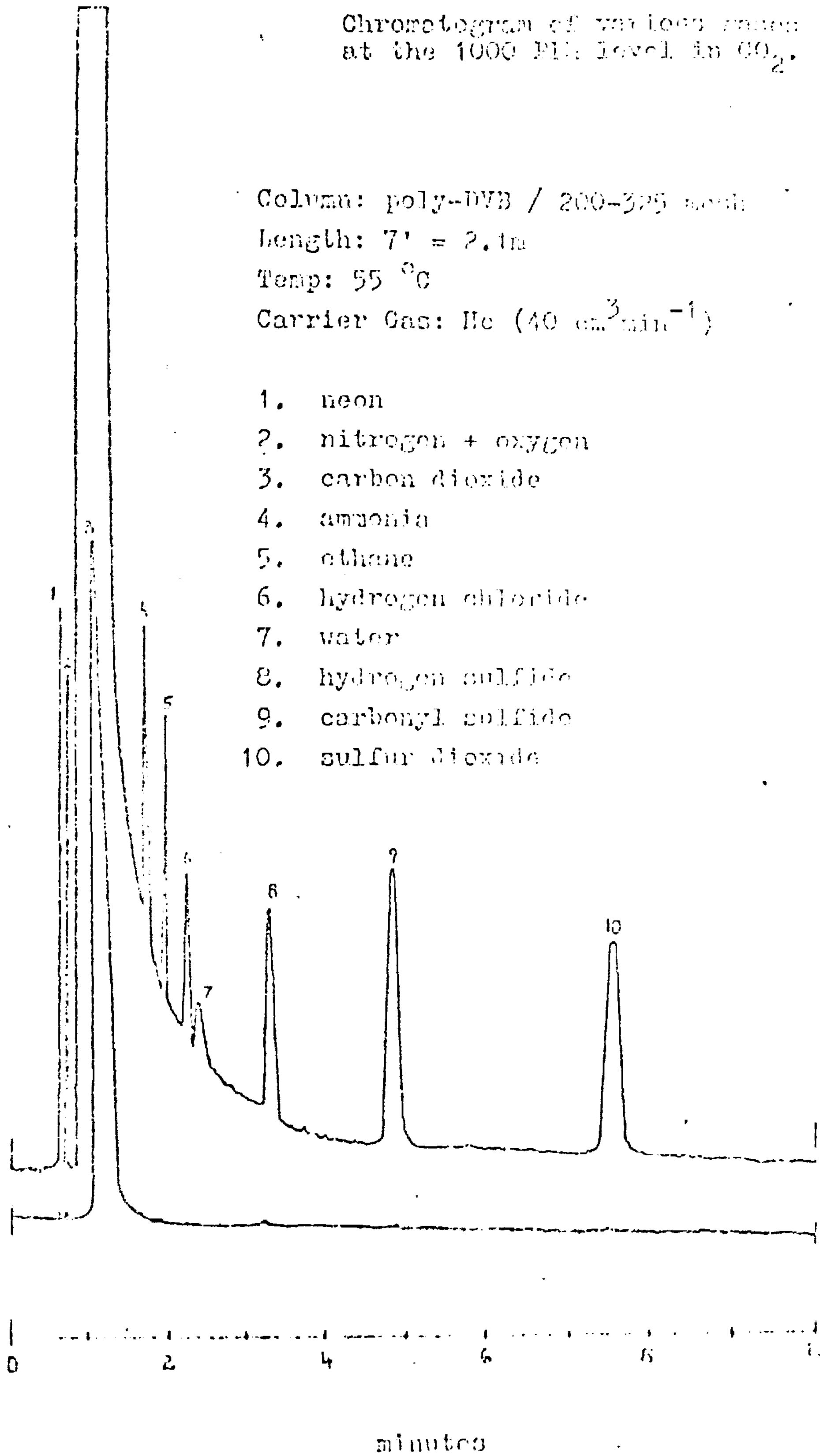


Figure 8-50



CHROMATOGRAPHIC SEPARATIONS ON POROUS CROMATIC HYDROCARBON COLUMNS

Figure 8-51



NOTES

Figure 8-52

these are the major components and they represent 95% of the atmosphere, if you have a known volume, you have a known pressure that you measure during that sampling interval and you know what the temperature of that particular sampling system is then, simply by the gas laws, it's easy to compute what remains in your system. So, it is possible to measure hydrogen and assume the concentration of helium simply by using the helium carrier system. It is not necessary to measure all the components in such a system.

The systems we were talking about are systems that have already been built or are being built. We have talked about the carrier gas supply which is something that is on the Viking mission. I have not talked about the regulator but there is a regulator. We have the sampling gas assembly system and, of course, these valves are all miniature latching solenoid valves that are space qualified. In Figure 8-53, is a schematic of an outer planets gas chromatographic system. It has additional valves and three separating columns. We have lost the column pairs here because what we are now proposing for the outer planets are detectors which are not influenced by temperature and pressure changes and no reference flow is required. Basically, we are talking about the inclusion of ionization detectors. What are ionization detectors? Ionization detectors are detectors which utilize radioactive sources such as strontium 90 or nickel 63, in an electric field sufficient to ionize gases of interest in the carrier stream. These radioactive sources provide electron current which is on the order of about 2×10^{-9} amps upon which currents of 3 to 4 orders of magnitude can be read.

With this steady background, one which provides for a fairly constant flow of electrons, one can essentially excite molecules and ionize them by providing a variety of electric fields. With high electric fields, one can cause a great agitation but it is not really important in this case because we don't care how much we fragment, we only care that we get a signal; and that this signal has a relatively useful range. We have sequenced detec-

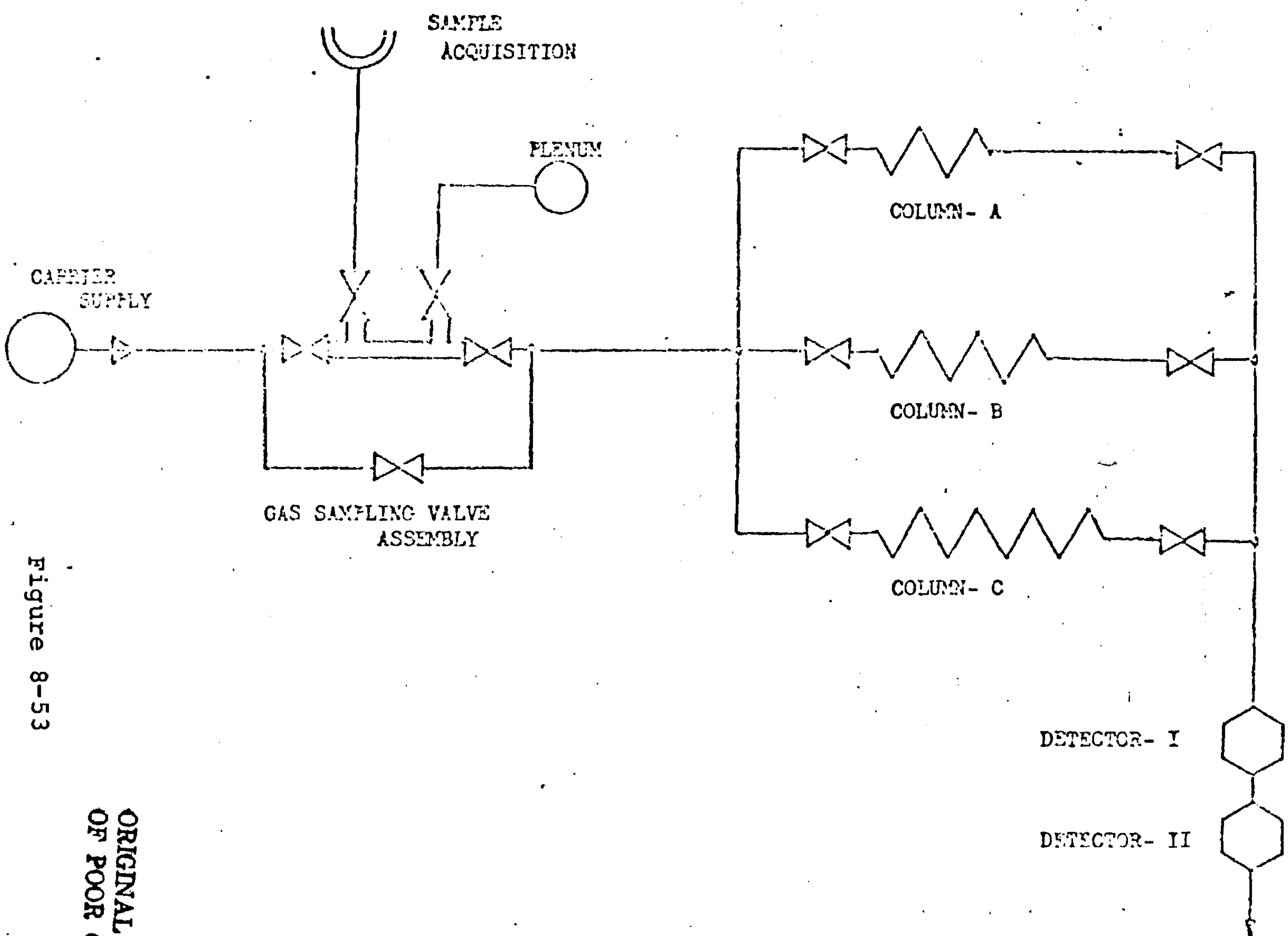

SAMPLE ACQUISITION
PLENUM
CARRIER SUPPLY
GAS SAMPLING VALVE ASSEMBLY
COLUMN- A
COLUMN- B
COLUMN- C
DETECTOR- I
DETECTOR- II

Figure 8-53

tors in series to compensate for a very important program. The compensation is the fact that in the Pioneer Venus mission, we have deliberately required that the retention index be the parameter of interest because we decided to go with the economy of the thermistor detector system.

Because we have taken that turn, we can now re-analyze the situation. We can see that if we can apply, in tandem arrangement, two detectors of unique quality which depend upon independent physical properties, we can therefore qualitatively identify a pure substance, analagously to a mass spectrometer's dependence upon fragmentation patterns, except that we would require some other technique in which high pressure could be used. The ionization detectors are the things that I am referring to.

With this type of system, we can now coast to the outer planets. As the temperature rises entering the atmosphere,the columns equilibrated at the colder cruise temperature will follow. We can take advantage of this rise in a very clever way. We can use the same column material, or various column materials, in various lengths. We can have a long column, a medium column, and a short column. The column lengths will then provide us with the kind of approximation which will give us the answers on the integral components, that is, the ones that are in the particular atmosphere.

For example, at the high altitude, the main interest might be the very light gases - helium and hydrogen, maybe argon and nitrogen. We can expect to make separations of these components with a long column very adequately.

The next sampling point is taken at a lower altitude. The sample is introduced into the medium sized column. Again, we will get a separation. Now, however, the light components come out unresolved. Their resolution will not be as good, but the moderate gases will come out and they will be nicely separated. Residual gases remaining in the long and medium columns remain trapped.

For the last case, we could use a very short column. Meanwhile, the temperature of the probe has gone up to, say, seventy degrees centigrade. This allows us to make very nice separations of such polar gases as water, carboneal sulfide, or whatever you want to consider in this particular system, even such gases as acetylene and benzene, if these may be there.

What I have talked about here is the system which we think is quite flexible, allows us to work with a high pressure system, and allows us to take volumetric samples and make analyses. One thing I want to point out is this sample acquisition system.

The sample acquisition system here is one in which there is dynamic flow. If you have dynamic flow, you have many components in this gas flow making contact with absorbing surfaces. If you only take in a very small portion of the gas molecules, like when you are talking about a vacuum system, then you have a big problem. In the system described, we are talking about a large number of molecules, which are in equilibrium with all of these surfaces. Virtually, we have a non-discriminating sampling device.

Figure 8-54 shows the detectors that we have in mind, which we are presently studying. There are about twenty-five more classes of detectors that could be added. Mainly, these are ionization detectors and they all have their particular virtues. The interesting part here is that the thermoconductivity detector that we have called the nominal one, relative to some sensitivity scale, (and that would be equivalent to five parts per million of nitrogen detection at ten bars, something along that order), you can see the kind of sensitivity increases that are afforded by an ionization detector.

As you can see, the physical properties we can talk about are various. We can take these combinations, and we have, essentially, an orthogonal approach to qualitatively identifying a particular substance. Two detectors in series, in which one

G. C. DETECTORS

DETECTOR	CARRIER GAS	RESPONSE TO PHYSICAL PROP.	LINEARITY	SENSITIVITY (REL. TO THERM.)
CROSS-SECTION	He	CROSS-SECTION FOR IONIZATION	4	0.1
DISCHARGE (RF,DC)	H_2	NON DISCRIMINATING	4	0.1
THERMISTOR	ANY	THERMAL CONDUCTIVITY	4	1
DIELECTRIC	He	DIELECTRIC	4	1
ELECTRON MOBILITY	He	NON DISCRIMINATING	2-3	10
ARGON IONIZATION	Ar	IONIZATION AT LESS THAN 10.6 eV	5	100
PHOTOIONIZATION	He(RF)	UV ABSORPTION (ALL GASES)	4	10^3
ELECTRON CAPTURE	Ar	ELECTRON AFFINITY	4	10^3
HELIUM IONIZATION	He	NON DISCRIMINATING	3	10^4
ELECTRON IMPACT*	He	NON DISCRIMINATING	6	10^3
FIELD IONIZATION*	He	RELATIVE TO FIELD STRENGTH	6	10^6

Figure 8-54

sample is traversing, will obviously give you answers which are fairly respectable.

SESSION IX

SPECIAL SUBSYSTEM DESIGN PROBLEMS

Chairman: Ronald Toms
Jet Propulsion Laboratory

MR. VOJVODICH: As usual, we saved the best until last. This morning's session, Number IX on Special Subsystem Design Problems, will be chaired by Ron Roms from the Jet Propulsion Laboratory, and his session will deal mainly with the area of planetary quarantine. He does have a couple of papers that fall in a specialized category on radiation effects as well as thermal control. So, without further delay, and hoping that this morning maybe we can stay on schedule and possibly start our afternoon session a little early, let me introduce Ron.

MR. RONALD TOMS: Thank you Nick. As Nick said, this morning's session has, perhaps an emphasis on planetary quarantine. It was kind of a catch-call session for those special problems that come up in the design of probes, and in designing the overall mission that might be very important to be thinking about because of their impact, in particular, on cost.

Planetary quarantine is one that would have a serious impact on cost and complexity if we have to adopt it. It still isn't clear, of course, whether we need planetary quarantine on the outer planet probes. NASA Headquarters has been talking a great deal about having a big get-together to discuss "the planets of biological interest." That's supposed to be a topic of a seminar that was to be held in mid-August. But the latest I have on it is that they haven't picked a date yet and it is not certain that that particular seminar will ever be held. The problem has been to try to get people like Horowitz, Liederberg and Carl Sagan all available at the same time to get together. A decision is eventually going to have to be made on whether we have to adopt planetary quarantine for the outer planets.

I will now call upon our first speaker, Mr. Al Hoffman of JPL, for an overview of planetary quarantine.

AN OVERVIEW OF PLANETARY QUARANTINE CONSIDERATIONS FOR OUTER PLANET PROBES

Alan R. Hoffman

Jet Propulsion Laboratory

MR. HOFFMAN: You have given a brief introduction to the problem of planetary quarantine and I will discuss today an overview of this subject as it pertains to the outer planets. To that end, I will be covering the topics that are listed below:

TOPICS

- BACKGROUND
- PLANETARY QUARANTINE CONSIDERATIONS
  - PRELAUNCH
  - LAUNCH AND SPACECRAFT
  - BASIC CONTAMINATION EQUATION
- CONCLUSIONS

I will start with an introduction and give some background relative to where we receive our planetary quarantine requirements, the international and national policy and how a flight project gets those requirements and what a flight project does with them. Then I will trace the planetary quarantine considerations through the life of a flight project assuming that a planetary quarantine requirement has been imposed. We will mention the considerations that pertain to the pre-launch phase, the launch-and-space-flight phase and then comment on the basic differences between a Mars lander and an outer-planet probe, and relate that to the basic contamination equation. And, finally, draw some conclusions relative to the significance of planetary quarantine for outer planet probe missions.

Turning to the background, Figure 9-1, as many of you are aware, the international policy for planetary quarantine is established in the Outer Space Treaty that was signed in January of 1967. In that, there is a phrase that states that the participating nations flying missions to the planets will take measures to prevent their harmful contamination.

PLANETARY QUARANTINE BACKGROUND

- INTERNATIONAL POLICY
  - OUTER SPACE TREATY
  - COSPAR
- NATIONAL POLICY
  - NASA
  - SPACE SCIENCE BOARD
- PROGRAM POLICIES
  - NASA PQ OFFICER
    - PQ PROVISIONS (NHB 8020.12)
    - P_C AND P_G FOR PLANETS
  - PQ PLANNING
  - ANALYSIS AND DOCUMENTATION

FIGURE 9-1

The International Council of Scientific Unions has established a Committee on Space Research, COSPAR, that establishes the guidelines and passes resolutions that relate to the international policy. The policy, as far as the United States is concerned, is established by NASA. NASA establishes that policy based on recommendations of Space Science Board. One of the purposes of having the seminar that Mr. Toms was referring to relative to the outer planets is to determine those planets of biological interest so that the Space Science Board can provide recommendations to NASA to establish the national policy relative to outer planets.

As far as program policy and how it is transmitted to a flight project, the NASA Planetary Quarantine Officer, at Code SL, provides to the Program Manager the PQ provisions document (NHB 8020.12), and two parameters for each planet or satellite of biological interest; one, a probability of contamination number (PC) and a probability of growth number (PG).

Then the flight project, based on the information that has been provided, begins its planning function and generates a planetary quarantine plan and, as appropriate, any subsidiary plans, such as a microbiological monitoring plan and a sterilization plan and, if necessary, a decontamination plan.

Then a flight project proceeds into the implementation phase of the planetary quarantine effort. The project performs some analysis; documents the results of that analysis and the microbiological monitoring; and generates such documents as a pre-launch analysis document and following the launch of the spacecraft, the post-launch analysis document.

On Figures 9-2 and 9-3 I will walk you through the life of a typical flight project, starting with the pre-project planning. (Outer-planet probes are currently in the pre-project planning phase.) In the pre-project phase we evaluate the effects planetary quarantine will have on the mission strategy, trying to formulate any impact that PW would have on these mission constraints. We try to determine what planetary quarantine analytical tools are

PLANETARY QUARANTINE CONSIDERATIONS

Prelaunch Phase

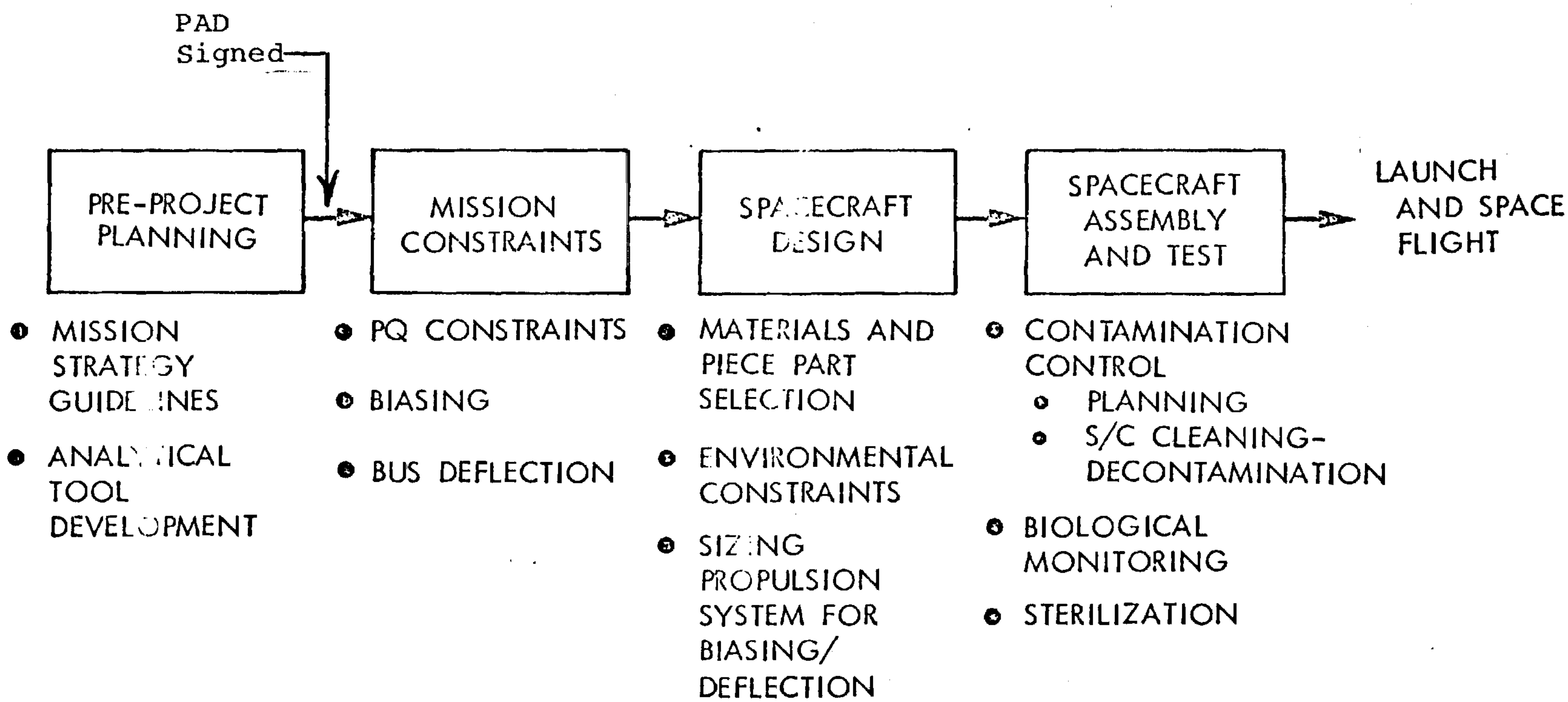


Figure 9-2

PLANETARY QUARANTINE CONSIDERATIONS

Launch and Spaceflight Phase

LAUNCH AND INJECTION → INTERPLANETARY → PLANETARY ENCOUNTER

LAUNCH AND INJECTION

- BIASING
- RECONTAMINATION OF STERILE HARDWARE

INTERPLANETARY

- BIASING
- EXPOSURE TO NATURAL SPACE ENVIRONMENTS
  - VACUUM
  - SOLAR IRRADIATION
  - SOLAR WIND
- RECONTAMINATION OF STERILE HARDWARE

PLANETARY ENCOUNTER

- BIASING
- BUS DEFLECTION
- ORBITAL LIFETIME
- EXPOSURE TO NATURAL SPACE ENVIRONMENTS
- RECONTAMINATION OF STERILE HARDWARE
- ENTRY HEATING

Figure 9-3

lacking and need to be developed for such things as bus deflection, biasing, and other navigation and trajectory considerations.

The project approval document (PAD) is signed at the point indicated. The project then proceeds into developing mission constraints and spacecraft design. Listed are some of the PQ considerations that are considered during these phases. For example, for biasing and bus deflection, planetary quarantine has an effect on how one sizes the propulsion system; i.e., the weight penalties that are attributable to planetary quarantine for performing these types of maneuvers.

If the project is going to have a sterilization or, a microbical reduction of some sort, then materials and piece part selection becomes a very important part of your spacecraft design.

Also considered during this portion are the environmental constraints that have a bearing relative to planetary quarantine. For example, as will be noted later, the natural space environments and, in particular the encounter environments, can have a reduction effect on the number of micro-organisms on the spacecraft arriving at the planet. These environments should be considered in the spacecraft design and can influence the stringency of the sterilization cycle.

Going into the spacecraft assembly and test operations, the contamination control planning effort, one looks at the considerations relative to the facilities that are needed to assemble the spacecraft and also, the personnel constraints and any special cleaning and decontamination methods.

If biological monitoring is required, it would te performed during this phase and then, as I have mentioned earlier,a terminal sterilization (i.e. microbial reduction process) of some sort may be required.

The next phase is the launch and spaceflight. On Figure 9-3 I have divided this into three areas: launch and injection, interplanetary, and planetary encounter. The biasing for planetary quarantine reasons has been a mode that we have been using on the majority of our missions that have been flown in the past where we biased the aim point away from the planet and then, by subsequent trajectory correction maneuvers, correct back to the desired aim point. This has a certain delta-V and weight penalty associated with it.

Also, if we are dealing with sterile hardware, (i.e. a probe sterilization has been performed), then the recontamination from a non-sterile bus is an important consideration in all three of these phases.

During the interplanetary phase, we have reduction techniques from the natural space environment that may reduce the viability of the micro-organisms that are on spacecraft exposed surfaces; such things as vacuum, solar irradiation, and solar wind.

Then finally, at the planetary-encounter stage, the things that need to be considered are the bus deflection, and if we are flying an orbiter, orbital lifetime. Then the exposure to natural space environments such as the trapped-radiation belt at Jupiter, may reduce the number of viable micro-organisms as well as entry heating. Recontamination I have already discussed.

What is uniquely different relative to Mars landers and outer planet probes is the planetary-encounter phase; in particular, the degree of entry heating that one would encounter. This can best be illustrated by Figure 9-4. This figure gives the basic contamination equation given entry and it applies for either inadvertent entry or the entry of a probe. It gives the number of viable organisms on the body at the time of entry times the probability of surviving atmospheric entry, the probability of release, and the probability of growth. This is important, during the planetary-encounter phase because if the probability of surviving atmospheric entry is very small that means that the number of viable organisms can be large at the time of encounter which in turn, maps back to what the launch burden can be, which in turn maps back to the stringency of the sterilization requirement.

BASIC CONTAMINATION EQUATION GIVEN ENTRY

$$P_{C|E} = n \; P_{SA} \; P_R \; P_G$$

WHERE

$P_{C\|E}$	IS	PROBABILITY OF CONTAMINATION GIVEN ENTRY
n	IS	NUMBER OF VIABLE ORGANISMS ON BODY AT TIME OF ENTRY
P_{SA}	IS	PROBABILITY THAT A GIVEN ORGANISM SURVIVES ATMOSPHERIC ENTRY HEATING
P_R	IS	PROBABILITY THAT A GIVEN ORGANISM IS RELEASED IN A REGION OF BIOLOGICAL INTEREST IN THE ATMOSPHERE
P_G	IS	PROBABILITY OF GROWTH

Figure 9-4

So, if P_{SA} is sufficiently small, the stringency of the sterilization requirement may be considerably less than what it is for Mars if there is any sterilization requirements imposed on outer planets probes.

Finally, Figure 9-5 has the three messages that I would like to leave with you today, as an overview: for the time being the planetary quarantine provisions of NHB 8020.12 are applicable to probes; as far as the interaction between the encounter environments and the stringency of the pre-launch sterilization requirements, this should be taken into account during early probe studies; and the information that we have learned during the course of doing the planetary quarantine work forthe Pioneer, Mariner, and the Viking programs, forms a basis for doing planetary quarantine work for the outer-planet probes.

This has been an overview of the planetary quarantine as it currently exists. There are unknowns relative to which planets are of biological interest to us. I understand that at some point in the near future that a position paper will be released by or through the NASA Planetary Quarantine Program Office on the outer planets. And that would be made available to the aerospace community.

MR. TOMS: Our next speaker is Bob DeFrees from McDonnell-Douglas who is going to talk about the impact of planetary quarantine on probe design.

CONCLUSIONS

- PQ PROVISIONS OF NHB 8020.12 APPLICABLE TO PROBES

- INTERACTION BETWEEN ENCOUNTER ENVIRONMENTS AND STRINGENCY OF PRELAUNCH STERILIZATION REQUIREMENTS

- PIONEER, MARINER, AND VIKING PQ EXPERIENCE AND ANALYSES FORM BASIS FOR QUARANTINE WORK

Figure 9-5

PLANETARY QUARANTINE IMPACTS ON PROBE DESIGN

N75 20407

Robert E. DeFrees
McDonnell-Douglas Astronautics Company

MR. DEFREES: The switch in order was especially advantageous because a lot of the things that I had to presume have already been explained by Alan Hoffman. The planetary quarantine program, as far as probes are concerned, progressed in the following fashion. We designed a probe under contract to Ames for entry into Saturn and Uranus. We were asked at the start of the design to hold off any provisions for planetary quarantine, specifically. Subsequently, after completing the basic contract, we were given a contract to determine the incremental effects of imposing planetary quarantine on the probe design that we had evolved. Quite frankly, the changes are small in scope and few in number. The business of planetary quarantine begins with a probability analysis. An analogy I would like to draw is: Walt Disney usually referred to his work as an examination of plausible improbabilities. The planetary quarantine business is the inverse of that, in that it is the examination of plausible probabilities. We are constantly setting standards and, as engineers, trying to live with them. The standards that are set here are on Figure 9-6, the probability of contamination and the probability of growth.

NASA Headquarters, in particular the planetary Quarantine Officer, sets these probabilities. They have been set for each of the planets and for some of the missions. In general, the probability of contamination value is the same for these planets, including all four of the giant planets. Pluto is still expected - as is Mercury - as being of little biological interest. In effect, the probability of growth is the more significant number because a probe is intended to go into the planet; and if it does, it has a chance of releasing organisms which can grow. Therefore, this number is divided up according to the number of missions, number of times you expect something to have the potential for contaminating that planet, and the transit survival potential. A flyby can contaminate it in one of two ways, (1) by direct entry or (2) by ejecta from part of the entire launch vehicle or spacecraft. Also,

Figure 9-6. Probability of Contamination and Growth

PLANET	PROBABILITIES CONTAMINATION, P(c)(1)	GROWTH, P(g)(2)
VENUS	1×10^{-3}	1×10^{-9} (ATM) NIL (SURFACE)
MARS	1×10^{-3}	1×10^{-6}
JOVIAN PLANETS	1×10^{-3}	1×10^{-6}
MISSIONS		
1975 VIKING (ORBITER AND LANDER)	7.2×10^{-5}	1×10^{-4}
PIONEER F AND G (EACH)	6.4×10^{-5}	1×10^{-4}
PIONEER G (SATURN)	1×10^{-4}	
OUTER PLANET MISSIONS (PER FLIGHT, PER PLANET)	7.1×10^{-5}	1×10^{-4}
SATURN AND URANUS(3) (SUAEP STUDY)	2.5×10^{-5}	1×10^{-6}

1) STAVRO AND GONZALEZ, PLANETARY QUARANTINE CONSIDERATIONS FOR OUTER PLANET MISSIONS.
2) PLANETARY QUARANTINE SPECIFICATION SHEETS, FOR NASA BY EXOTECH SYSTEMS, INC., ISSUED 12 1 73
3) STUDY OF THE EFFECTS OF PLANETARY QUARANTINE ON THE DESIGN OF AN OUTER PLANETS ATMOSPHERIC PROBE, MDC E1053, 29 MARCH 1974; INTENTIONALLY MORE CONSERVATIVE.

the other factor involved is time. In the case of Mars mission, there is a fifty-year time period of reasonable non-contamination involved. In general, for the outer planets, the time span is set at about twenty years and then one has to determine how many times American, U.S.S.R., or some other country is going to send something to the vicinity of the planets. From this you get the probability of contamination and, also, fairly arbitrarily, you establish the growth probability for each of those planets.

Now, Pioneer 11, originally Pioneer G, is interesting in that it will go past Jupiter, having the potential for contaminating it, and then go on to Saturn. The analyses for both of the flights, F&G, were performed some time ago (before launch) by Ames Research Center and then the Pioneer G was extended to the Saturn case (before Jupiter encounter). This was of interest to us on the Saturn-

Uranus probe study, because our Saturn-Uranus probe has a similar mode of operation: a flyby of one planet and a deposition of a probe into the second. In general, as you can see, the value for the probability of contamination at the second planet is given a little relief (lowered) from that of the first.

We have chosen a deliberately more severe requirement than have some other authors simply because the number of flights is not well established yet, over this twenty-year time period, and we felt it was appropriate to establish the more stringent requirement on our own studies.

The classic requirement for sterilization has been established in the Viking program and you will hear a good deal more discussion about that in a few minutes from Bob Howell. But, classically, it is a matter of saying that if you heat something at a temperature above a hundred degrees Centigrade, you will enhance the probability of decreasing the microbe load; and, in fact, plotted on a semi-log paper it is a straight line. In effect, if you hold a certain temperature for a period of time, you will decrease the number of microbes on that object from 100% to 10% to one percent to one tenth of one percent, and so forth. This is usually referred to as decimal reduction time (D - value) and it is also sometimes referred to as decades or logs. (See Figure 9-7)

The standard D-value that is used is that for bacillus subtilus variant niger, as supplied by the U.S. Public Health Service. The temperature that was initially set for Viking was 125°C. This was later changed to 113°C. On the outer planet probes, we now understand it may go back to 125° because there tends to be more probe equipment available that has been tested at the higher temperatures. This has to be a consideration in the costing. It conceivably could be a requirement for more testing of a probe, even though there is a tremendous fund of knowledge already available in the Viking program.

In addition to that, the life of a planetary quarantine engineer is a little bit complicated by the discovery that not all microbes are willing to die at the same rate that bacillus subtilus does. This leads to a problem wherein some will follow a more-or-less

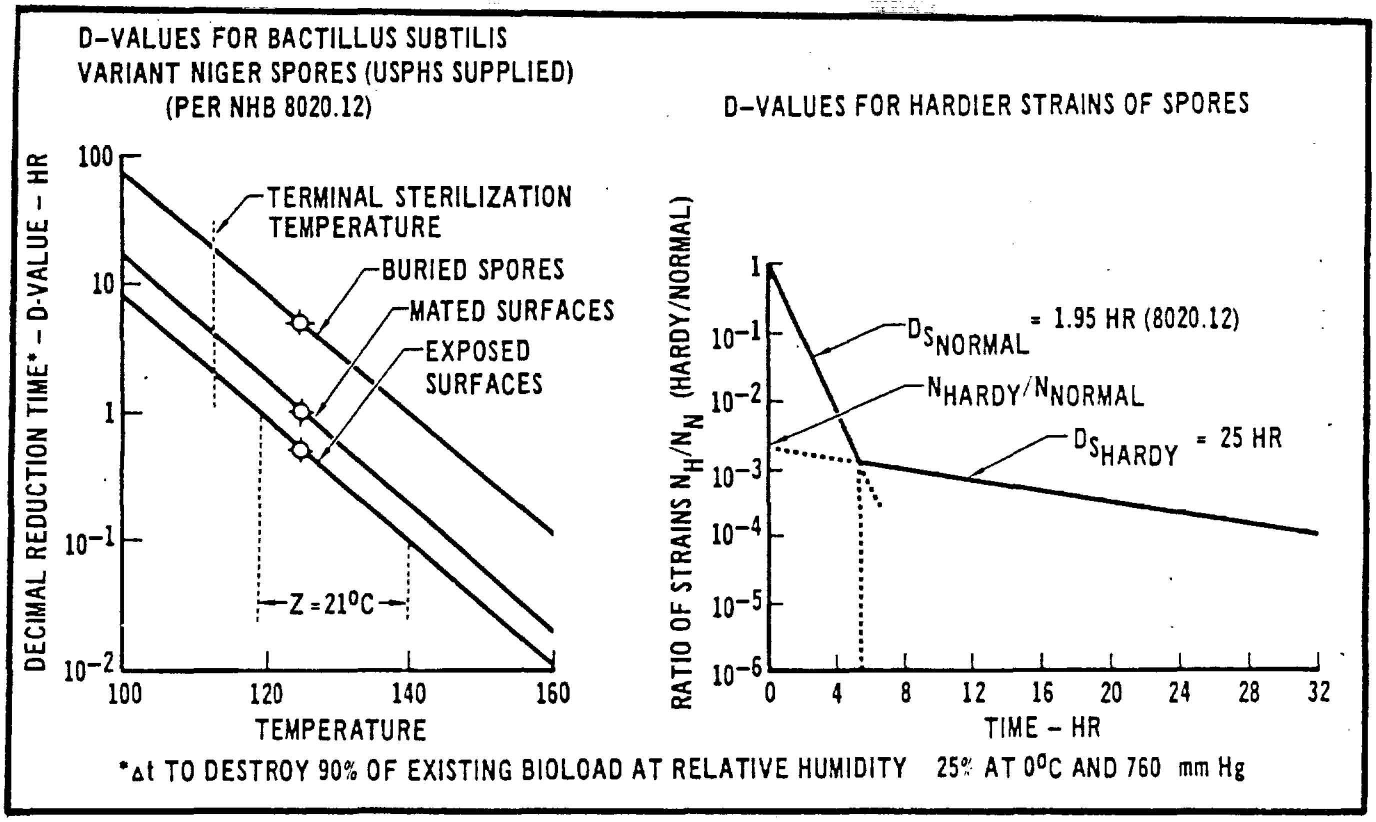


Figure 9-7. Decimal Reduction Times

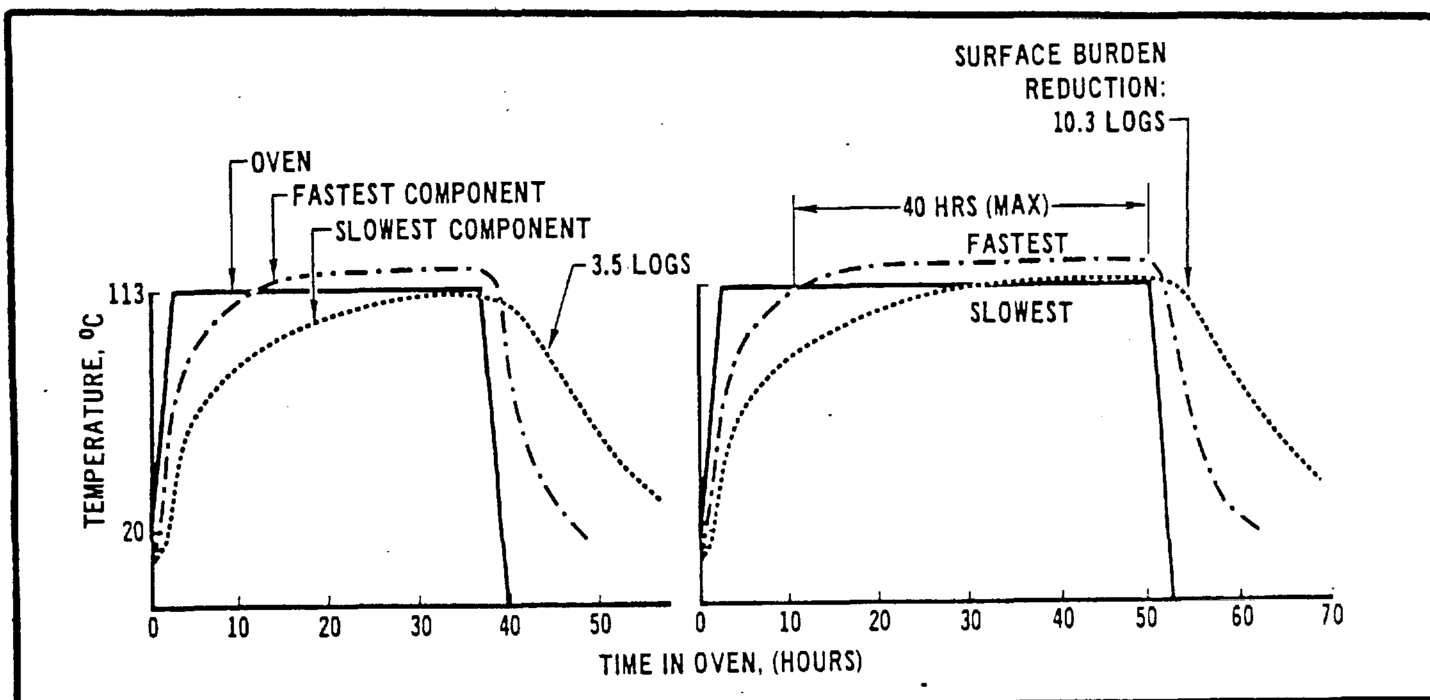


Figure 9-8. Estimated Thermal Response Characteristics

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OF POOR QUALITY

normal decay rate, whereas, others have a very prolonged decay rate. An example is on Figure 9-7. It is not the only example, others have even shallower slopes. In this example, the times were chosen fairly arbitrarily and the ratio between the two types was shown. The net effect of this is that instead of periods of the order of forty or fifty hours of terminal sterilization, we might be forced to go to longer periods to guarantee that these hardy ones are killed off. The obvious requirement on the part of cleanliness engineers and their staffs is to find out whether that type of microbe is prevalent in clean rooms. It is analogous to the problem in surgical situations after World War II where they suddenly found tremendous quantities of staphylococcus showing up in operating rooms: a rather horrible concept that they had to lick rather quickly due to excessive dependence on antibiotics and relaxed cleanliness procedures.

The requirements for heat sterilization are shown on Figure 9-8 as they affect the equipment designer, the man who provides the oven, and also the design engineer, who is designing the probe. If you make a probe to go through space where there is very little sunlight, it is going to get cold. So, in general, we have provided a rather effective barrier to reduce the rate of loss of heat in space. The net effect of this as far as an oven is concerned is that you can turn the oven on and run it up to 113° Celsius in a matter of hours. Some of the components will heat up rather rapidly. This is shown as exceeding the oven line. Obviously, it wouldn't exceed the oven unless it is something like the radio isotope heater unit inside which would go beyond the oven temperature and will get to that temperature rather quickly.

Other components in the case of the probe, the battery is a good example, are buried inside of multiple-layer insulation and inside some foam insulation on one side or some powder insulation on the other. It may also have deliberately poor heat conductive paths to the framework. The net effect is that some component is going to take a long while to get up to this temperature. But, if you determine this fact by analysis and confirm it later by tests, that this particular component only go to 113°C at the time you shut off the oven, you still can expect some reduction in microbes by the fact that it exceeded 100°, more particularly 110°, before the oven was turned off. But the problem as far as the probe designer is concerned is how long will it be subjected to that temperature and how frequently. Again, this goes back to the fact that most units are designed to the qualification test requirements and not to the true environment; thus, you have to determine the total length of time this temperature exposure is held if you wish to calculate microbe kill capability.

The classic equation was inferred by Al Hoffman when he showed that the probability of contamination is a function of the number that is present at the start of the terminal sterilization period, divided by the probabilities for survival, for release, and for growth. This determines the number of microbes that will remain when the probe enters the planet.

Now in a forty-hour period we can decrease the number of microbes from, say, three and a half, typically, to ten logs. This is in effect even if you start with a million microbes onboard, you cut them to 10^5, 10^4, 10^3, 10^2, 10^1, and even below; to get a probability less than one that there are any living microbes.

A further reason for doing this is that we are looking for the flight acceptance test requirements, trying to set them for the components and for the probe itself. This branched system, Figure 9-9, shows components on the upper branch and the assembled probe with a presumed bioshield and test requirements requiring from fifty-four hours of exposure at 110° to 113°C. In a discussion yesterday with Bob Howell and Leo Daspit of Langley, the acceptance test temperature for components is usually the upper limit of 125°. What we are after is a determination of how many

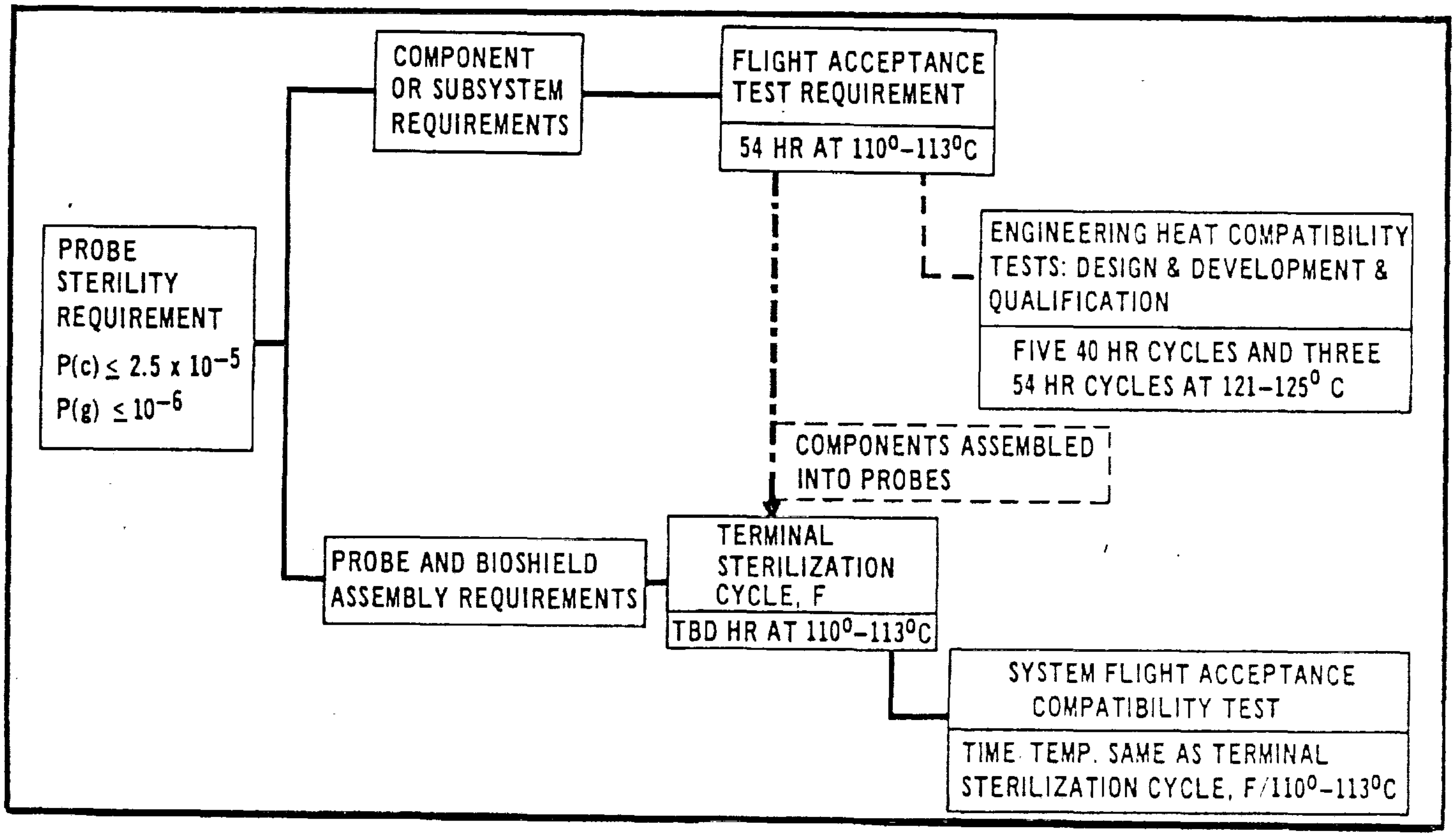

Figure 9-9. Sterilization Development Testing

times will the component be subjected to the worst case terminal sterilization cycle. This is of interest because one of the side benefits of going through this type of cycling is that you performed an excellent accelerated life test, because you have raised the component or probe to a high temperature repeatedly. That, of course, is deleterious to plastics, to rubbers, and to other materials whose physical properties are temperature dependent.

A total of eight cycles was negotiated in the Viking program. We initially adopted this in our probe studies. We feel the number is a negotiable item relative to a probe design. A lower number of cycles are preferred simply because the probe is orders of magnitude less complex than the Viking lander.

For internal equipment sterilizations, we have to determine a time. This is performed at 110° to 113° on Viking. It may go back up to 125°C on the probes, according to Larry Hall, and if so, that fact will have to be taken into account both in writing of procedures and in the costing of the probe.

The net result of all this is that there are changes that were required in probe configuration. The accompanying figure, Figure 9-10 lists them. The significant ones are that a bio-shield is necessary or some other form of prevention of contamination after the unit is assembled. There may be changes in the adapter. Inside the probe, the chief changes are in thermal control (a substitution of one plastic for another); the electrical

• STRUCTURAL/MECHANICAL	– BIOSHIELD (IN NEW ADAPTER) – FIELD JOINT (IN NEW ADAPTER) – DESIGN FOR 1 ATM DIFFERENTIAL PRESSURE – SEPARATION OF BIOSHIELD COVER AT EARTH – HONEYCOMB THAT IS SELF-VENTING IN CHANGING PRESSURES
• THERMAL CONTROL	– KAPTON SUBSTITUTED FOR MYLAR INSULATION BLANKET – SILVERIZE RATHER THAN GOLDIZE THE EXTERNAL MLI
• ELECTRONICS	– EQUIPMENT LIMITS ARE 160°F (OPERATING) – SOME WEIGHT AND COST PENALTIES
• ELECTRICAL	– MAIN BATTERY UP 33% IN WEIGHT – MAIN BATTERY UP 28% IN VOLUME – CELL CASES MUST USE HI-TEMP PLASTICS – NEW SEPARATORS REQUIRED – PLATE POROSITY CHANGES IN NiCd BATTERIES – SUBSTITUTION OF KAPTON OR TEFLON INSULATION ON WIRES – CLAMPS CUSHIONED BY TEFLON
• SPACECRAFT	– CABLE CUTTER MOVED INSIDE BIOSHIELD – CHANGES IN WEIGHT: SEQUENCING EQUIPMENT
• MASS PROPERTIES	– 16.5 LB INCREASE, MOSTLY IN BIOSHIELD AND POWER SUBSYSTEM

Figure 9-10. Design Impact Summary

system, (the batteries tend to get bigger, which means heavier); and very little change for the electronics. The chief reason for the increased battery weight is that silver peroxide will break down to silver oxide at the temperatures involved, so you can't count on that particular fifty-percent plateau of energy. Thus, the size of the plates just about double. There are some other changes in the spacecraft, which are not too significant. The result is an increase in the case of a Pioneer-attached probe of about sixteen and a half pounds. In a Mariner installation this could be a little bit heavier because we have built the bio-shield into the adapter and taken advantage of that structural unit. So on Mariner the increment would be about eighteen and a half pounds.

There are some cost increments involved. The cost estimates that were made were based on contractor-furnished science instruments and, also, they pertain only to the direct costs of planetary quarantine related to the cost of the probe itself, and not to the overall program costs which would include spacecraft, launch and NASA mission operations costs. The analyses showed that most of the increase is in the design analysis and in the test phases. The basic probe cost is $40 million and the cost increment equals $13 million. This incremental increase is about twenty-one percent of direct contracted probe costs (about 5-6% of all costs).

In conclusion, there are really only two overriding conclusions, although I have included a list of some general and specific ones on Figure 9-11. The overriding ones are: (1) that a probe can be built in a sterile condition with no insurmountable problems to the design engineer, and (2) that the cost increments are predictable, which usually means that they are controllable. It is usually only unpredictable ones that are uncontrollable.

MR. TOMS: Our third speaker will be Bob Howell from Martin who has been working on the Viking Program and will show us just how the implementation problems have been solved for Viking.

- PLANETARY QUARANTINE REQUIREMENTS DO NOT AFFECT TIME NEEDED TO DEVELOP THE PROBE; BUT DO INCREASE MANUFACTURING STEPS AND HANDLING DIFFICULTY
- COSTS WILL INCREASE ABOUT 21% DUE TO MINIMIZING CONTAMINATION AT EVERY STAGE OF FABRICATION AND PRE-LAUNCH OPERATIONS
- DRY HEAT STERILIZATION WITHIN A BIOSHIELD IS COMPATIBLE WITH PLANETARY QUARANTINE OBJECTIVES AND WITH THE CURRENT STATE OF TECHNOLOGY. STUDIES OF OTHER TECHNIQUES ARE UNDERWAY FOR ATMOSPHERIC PROBE MANUFACTURE TO LOWER THE INCREMENTAL COSTS FURTHER.
- THE PROBE COULD BE ASSEMBLED IN A LARGE LAMINAR FLOW BENCH FACILITY AND, THEREBY, LIMIT MICROBE GROWTH, A CLASS 100 ROOM, IF AVAILABLE, FACILITATES ACCESS.
- RETAINED (AFT) PART OF BIOSHIELD CAN BE INTEGRATED INTO A NEW SPACECRAFT-TE364-4 ADAPTER; FORWARD COVER CAN BE RELEASED ALONG WITH THE JETTISONED TE364-4 STAGE AFTER IT INJECTS THE SPACECRAFT AND PROSE INTO A TRANSIT ORBIT.
- PROBE COLLAPSE IS NOT IMMINENT AT PRESSURES UP TO 30 ATM; ELECTRICAL EQUIPMENT IS DESIGNED FOR OPERATION IN A 160°F AMBIENT TEMPERATURE ENVIRONMENT. FAILURES WILL OCCUR PROGRESSIVELY AS THE FORWARD COMPARTMENT TEMPERATURE EXCEEDS THIS VALUE.

Figure 9-11. Summary of Conclusions

VIKING PLANETARY QUARANTINE PROCEDURES AND IMPLEMENTATION

Dr. Robert Howell
Martin Marietta Corporation N75 20408

DR. HOWELL: As the previous two speakers have mentioned, there has been a great deal of activity in planetary quarantine for a number of years, and there is still a great deal of interest in the subject for the outer-planet probes. Many of the implementation techniques and methodology that was discussed by Mr. Hoffman from JPL has been used on the Mariner programs and applied to the Viking Project.

I would like to share with you some of the techniques and methodology that have been used on Viking at the Martin Company to implement the planetary quarantine requirements. As you well know, Viking is the first U.S. project required to satisfy the full intent of the international agreement, both from a sterile-lander concept and planetary quarantine requirements on the orbiter.

Implementation starts with requirements that are imposed by NASA Headquarters and the Viking Project Office (Figures 9-12 and 9-13). These requirements establish the necessity to sterilize in an inert gaseous environment; that the affluent gas coming from the vehicle during the terminal-sterilization cycle be equal to or less than twenty-five percent relative humidity at zero degrees centigrade, 760 millimeters of mercury, and that lethality may not be counted until the humidity requirement is achieved, and the minimum lethal temperature is one hundred degrees centigrade.

As Mr. DeFrees from McDonnell Douglas indicated, additional information is provided on the accepted standard test organism, D values and Z values the probability of growth, probability of release, lethality of ultraviolet radiation, the microbial density in non-metallic materials, and probably most important, the allocation for the mission in question, all of which are needed to determine the implementation approach for building and sterilizing a vehicle.

PROJECT REQUIREMENTS AND CONSTRAINTS

DEFINITION

NASA Headquarters

NHB8020.12 (April 1969)	"Planetary Quarantine Provisions for Unmanned Planetary Missions"
NHB5340.1A (October 1968)	"NASA Standard Procedures for the Microbiological Examination of Space Hardware"

Project

M75-127 (March 1970)	"Viking 75 Project Planetary Quarantine Provisions"

Statement of Work

M75-123	"Viking Mission Definition"

Figure 9-12

REQUIREMENTS AND CONSTRAINTS

Sterilization Environment

Inert Gaseous Environment

Humidity $\leq$ 25% at 0°C and 760 mm

Minimum Lethal Temperature (100°C)

Standard Test Organism

"D" and "Z" Values

Logarithmic Death Model

Allocation

Planetary Quarantine and/or Biology

Other Parameters

Probability of Growth, Release, Ultraviolet, etc.

Microbial Density of Nonmetallic Materials

Figure 9-13

In addition to Planetary Quarantine, there may be a requirement or an allocation for biology. In the case of Viking there is such a requirement and we must satisfy a probability of contamination of the biology instrument on-board by terrestrial organisms.

The basic approach for implementing Planetary Quarantine is the same for any vehicle, Figure 9-14. You must start out with the mission allocation and determine the potential contaminating events associated with that mission. For Viking we must consider sterilization, recontamination prior to launch, and recontamination after launch, from the launch vehicle or orbiter. Some of the contaminating events prior to launch include propellant loading of the vehicle, bioshield pressurant gas, propellant pressurization, and the RTG cooling water which is used to cool the thermoelectric generators.

I will discuss only one of these events with you today - the techniques and approaches we have implemented on Viking for sterilization.

There are three types of burden which must be considered when sterilizing the lander: the organisms which are on the exterior surfaces of the hardware, the organisms which are between mated surfaces, and organisms within the materials that the components in the system are constructed of. The latter is called "encapsulated burden." Each of these different burden types have different thermal death characteristics. The encapsulated burden is the most resistant to dry-heat sterilization and requires the longest period of time for reduction. Our approach is to achieve the required encapsulated burden reduction at the component level and to track the reintroduction of this burden type during the assembly and buildup of components and the system. We have integrated the planetary quarantine heat requirements with engineering requirements for heat-compatibility testing on components to achieve this reduction. (Figure 9-15 and 9-16)

There is information which is required before one can determine or specify the appropriate heat cycle for the hardware

DERIVED REQUIREMENTS AND CONSTRAINTS

PROBABILITY OF CONTAMINATION - LANDER SOURCES

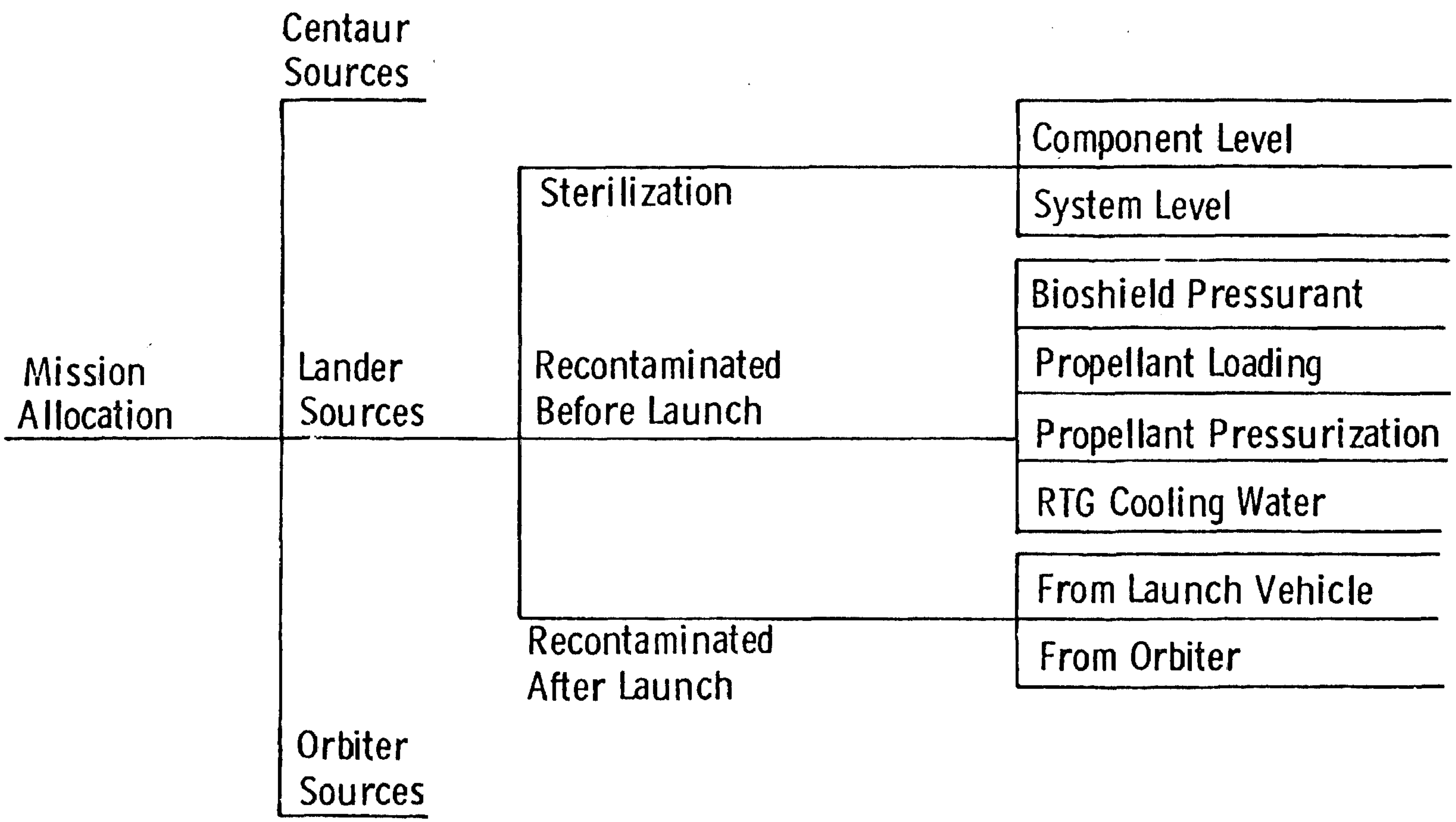

Figure 9-14

DERIVED REQUIREMENTS AND CONSTRAINTS

Terminal Sterilization Cycle Defined to Achieve Required Reduction in Mated and Surface Burden

Component Flight Acceptance Temperature Nominally that of Terminal Cycle at Sufficient Time to Achieve Encapsulated Burden Reduction

Terminal Sterilization Cycle Refined not to Exceed any Component FA Level

Figure 9-15

DERIVED REQUIREMENTS AND CONSTRAINTS

COMPONENT REQUIREMENTS

Flight Acceptance

Designed to Kill Encapsulated Burden

Temperature Based on Terminal

Qualification

Designed to Qualify Hardware

Temperatures and Time Increased for Margin

Development

Same Temperatures and Time as Qualification

Thermal Analysis Verification

System Requirements

Total Cycle Time Limitation

Time at Temperature Limitation

Temperature Level

Figure 9-16

(Figure 9-17). To gather this information, thermal analyses are performed to determine the slowest-responding point within that component, the time lag between this point and the exterior of the case, and the instrumentation required to verify the thermal analyses during development testing. This information is used to establish the component flight acceptance heating time required to achieve the required encapsulated burden of reduction.

As shown on Figure 9-16, the development times and temperatures are the same as those for qualification and are elevated both in time and temperature over that which we expect flight hardware to experience.

We use the terminal sterilization process to achieve the necessary reduction of the surface and mated burden. Flight components experience approximately the same cycle as they saw during their flight-acceptance component heat-compatibility. System level constraints of time and temperature have been established to ensure this is the case.

This process is shown schematically in Figure 9-18. A thermal analysis is performed which establishes the requirements for component testing. The component-development test results are used to verify the thermal analysis and make corrections as necessary. And then we perform the component flight acceptance heat-compatibility test on flight hardware to kill the encapsulated burden.

We use the component thermal analysis information and test data to feed back into our system analysis to predict the response of these components at the system level. We then built and tested a Thermal Effects Test Model which is a simulated Viking lander with non-functional components to verify that the system thermal analysis and the component analysis which were performed previously are in fact correct.

Finally, we test our qualification vehicle which is called the Proof Test Capsule, refine our thermal test data and, qualify

VERIFICATION

COMPONENT ANALYSIS

Identification of "Coldest" Contaminated Point

Identification of Instrumentation

Establishes Lag Time

SYSTEM ANALYSIS

Identification of Component Response

Identification of Instrumentation

Identification of Design Changes

First Level Verification of Component Requirements

Identifies Terminal Cycle Operational Constraints

Figure 9-17

VIKING THERMAL ANALYSES & HEAT REQUIREMENTS

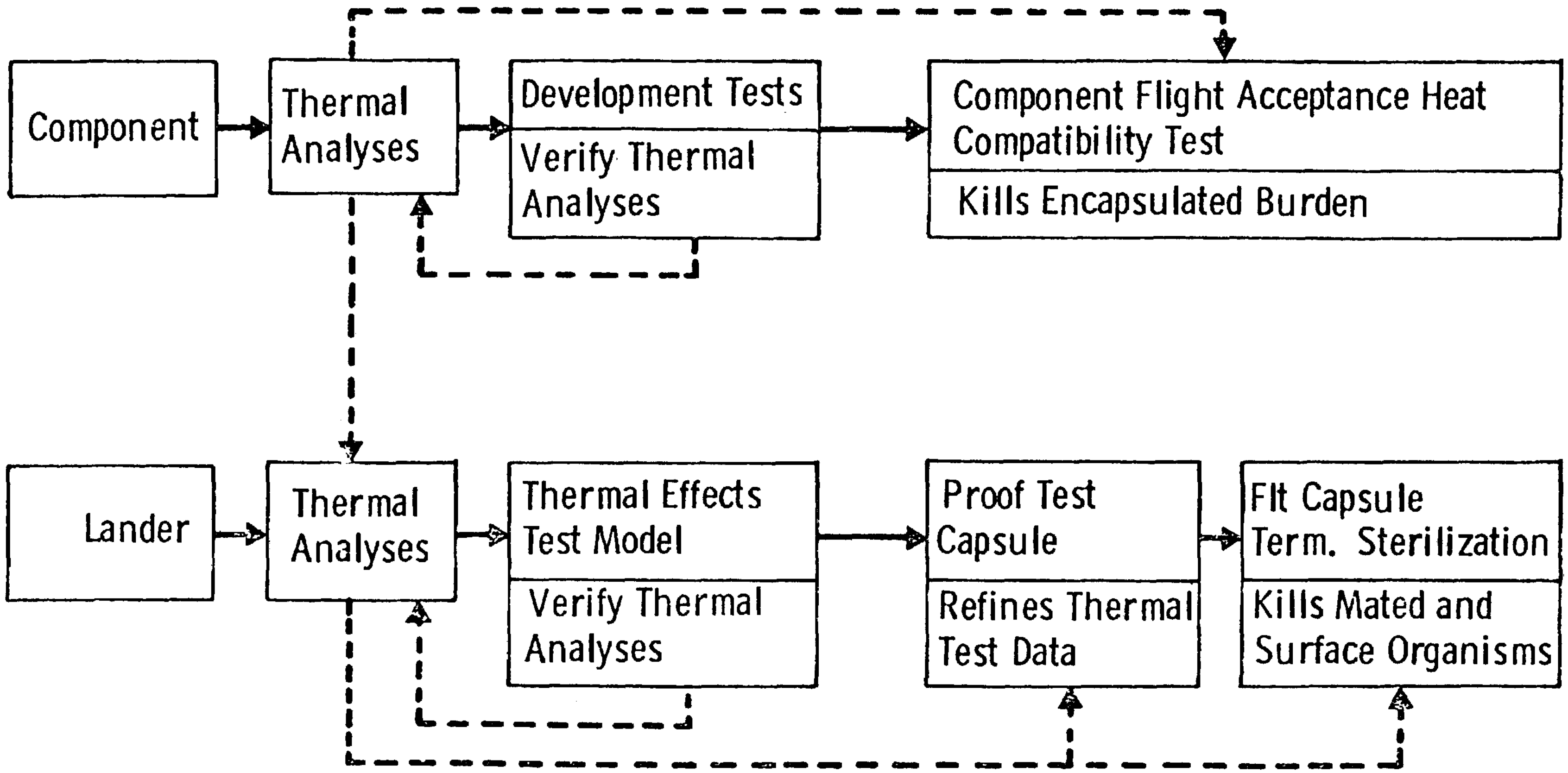

Figure 9-18

the cycle to be used during the terminal sterilization process for the flight landers.

We have completed the Thermal Effects Test Model testing. The results gathered during that test are shown in Figure 9-19. There is an engineering constraint of forty-hour time-at-temperature maximum after the first component reaches its lower flight acceptance level temperature. The camera was the component which reached its lower flight acceptance level temperature first. There are many components which reached 110° to 113° before the camera did, however, their flight acceptance level temperature requirements are higher and did not constitute start of the cycle.

The slowest responding component during this test was the biology mechanical subsystem, and it achieved the terminal sterilization temperature at approximately twenty-four to twenty-five hours after start of ramp-up. There is a 2.4 hour internal lag in the biology instrument between the exterior of the case and the coldest point in the instrument. Since our approach is to place the burden at the coldest responding point in the vehicle and sterilize to that response we must incorporate this 2.4-hour lag time before we can start counting lethality.

As I stated earlier, lethality can't be counted until the humidity requirement is met. On the first cycle this time was approximately twenty-nine hours into the cycle. Therefore, any integration of lethality earlier had to be excluded. The purge rate on the first cycle was 2.75 scfm. Analyses were performed to determine if an increased purge rate would shorten this time. During the second cycle on the Thermal Effects Test Model we increased the purge rate to 4.75 scfm. The humidity requirement was achieved in approximately ten hours. However, there was some question as to whether this shortening of time was actually due to the increased purge rate or that we had heated the vehicle for a second time. We postulated that if we maintained a purge rate of 4.75 scfm, we could probably expect a worst-case situation of approximately

THERMAL EFFECTS TEST MODEL TEST RESULTS

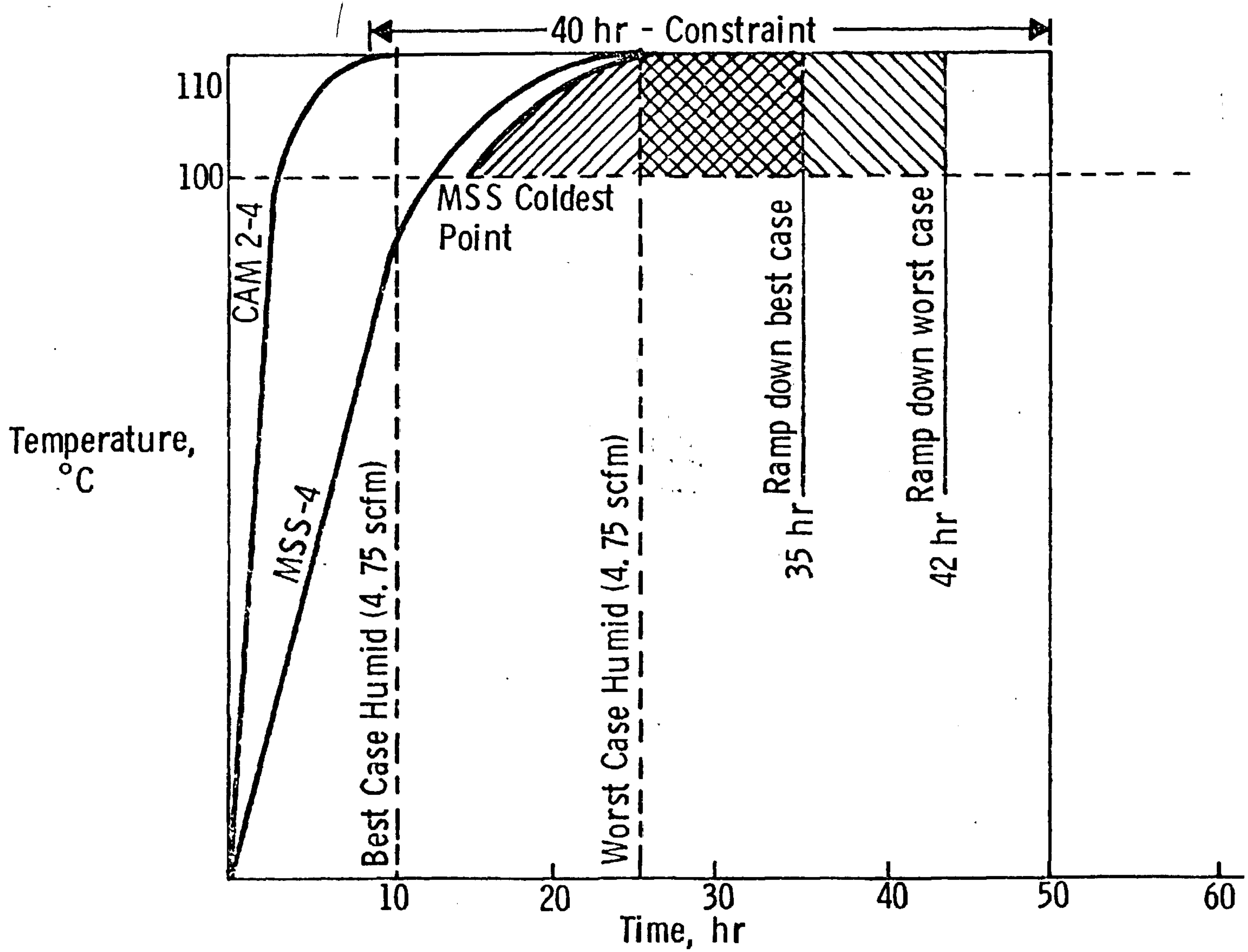

Figure 9-19

twenty-five hours, therefore, to achieve the required kill to meet the planetary quarantine and biology requirements would require forty-two hours heating time from the start of heat-up to the start of ramp-down.

The next view graph* will show you a picture of the Thermal Effects Test Model used during this testing. The TETM's very similar in nature to a flight-type Viking Lander, however, it had thermal simulator instead of functional components. As you will see later, the information we gathered from this vehicle was quite similar to that gathered on the Proof Test Capsule.

Here is another picture* of the TETM inside the sterilization chamber with the bioshield inflated. The vehicle that you just saw in the previous view graph now is enclosed in the aeroshell base cover and bioshield. The bioshield is inflated to a minimum of five inches of water pressure during terminal sterilization, and this picture was taken through the window of the oven during the actual sterilization process.

The next vehicle we have sterilization testing on is the Proof Test Capsule. The objectives of this testing are shown in Figure 9-20 and were completed earlier this year. Results are plotted on Figure 9-21.

The radar altimeter electronics was the first component to reach temperature. Camera number two got up to its lower flight acceptance temperature first, however, it was only the exterior of the insulation and thermal concluded that the interior of the camera, or the electronics had not reached temperature yet, so therefore, we were able to extend the cycle start time by approximately an hour. The radar altimeter electronics reached its lower flight acceptance level temperature in approximately eleven hours. Again, as with TETM, the biology mechanical subsystem was the slowest responding component in the vehicle.

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* Not available for inclusion in these proceedings

PROOF TEST CAPSULE (PTC) STERILIZATION

Objectives:

1. To Provide Verification That the Sterilization Requirements Can Be Met On a Functional Flight Type Vehicle

2. To Qualify the Processes Used to Accomplish Number 1. Above

Figure 9-20

PROOF TEST CAPSULE TEST RESULTS

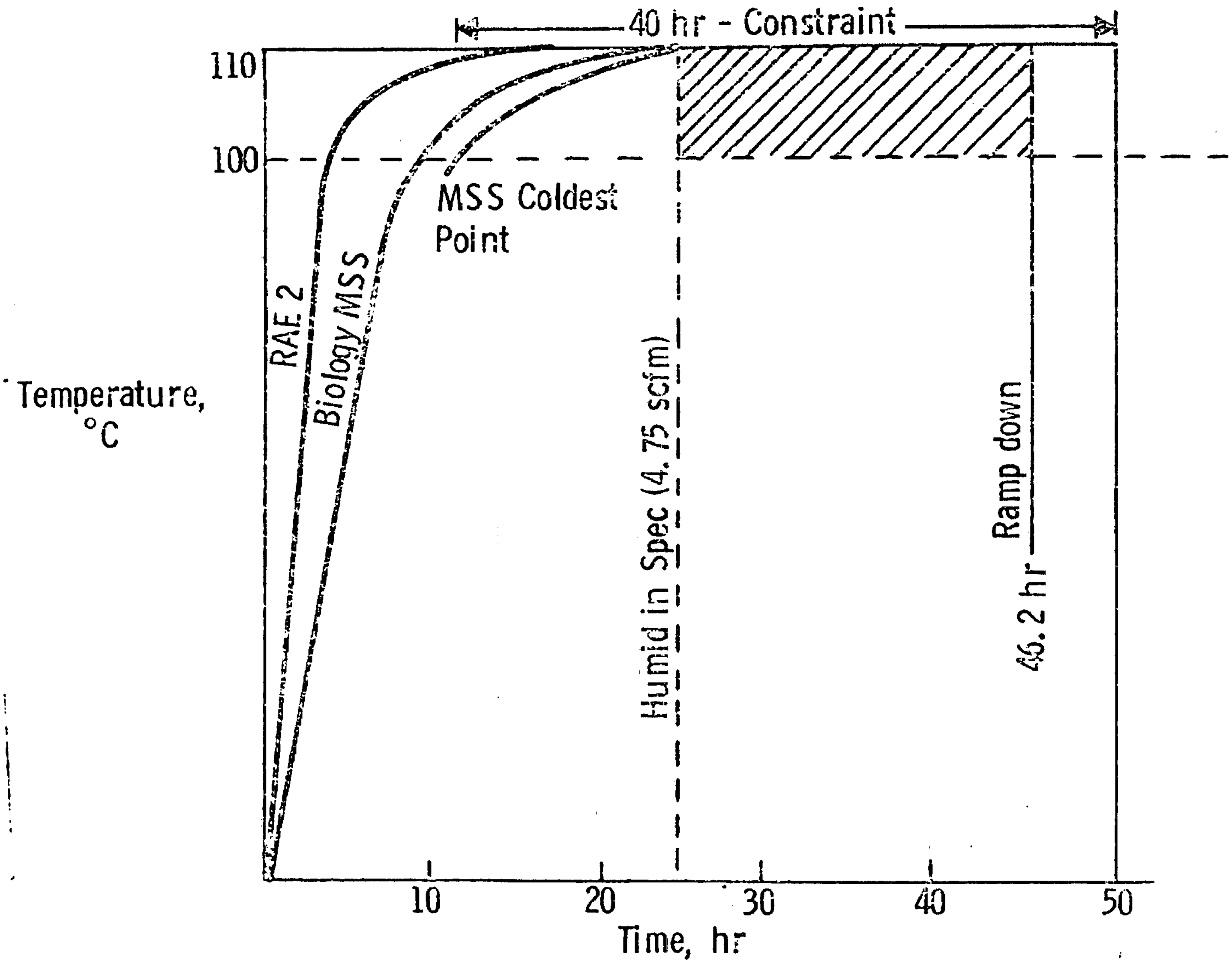


Figure 9-21

We had a great deal more information when we conducted this test than we did on TETM. We had microbiological sampling data gathered during the assembly of the vehicle. We had the TETM experience and had gained a great deal of knowledge from the time that we had heated the TETM until we heated the Proof Test Capsule.

We calculated the lethality required to satisfy both planetary quarantine and biology requirements, and based on these calculations, vehicle was ramped down at 46.2 hours after the start of heating. The humidity requirement was achieved at 25.17 hours.

Here is an earlier picture* of the Proof Test Capsule. As you can see, many of the components do look different from those you saw on the Thermal Effects Test Model. These are functional components. There were some simulators but very few.

In summary, Figure 9-22, we have taken the requirements which have been imposed on us by the Viking Project Office and by NASA Headquarters, and converted these into engineering requirements. We have imposed these requirements and constraints on ourselves and our suppliers, and have been able to produce hardware which will satisfy these constraints. The hardware has been designed and developed. Our thermal data base has been established, both from the component and system thermal analysis work, from the Thermal Effects Test Model data and now from the Proof Test Capsule data. We have designed, built, and tested a sterilizable vehicle which satisfies planetary quarantine.

(Mr. Toms opened the session to questions to any of the three prior speakers.)

MR. T. C. HENDRICKS: I have a question, I guess for Dr. Howell, and that is: Previously we saw estimates of the cost impact of getting this planetary quarantine requirement on the probe. I was wondering if, in the earlier days of Viking, you made these

*Not available for inclusion in these proceedings

SUMMARY

Requirements and Constraints Established and Imposed

Hardware Designed and Developed

Thermal Data Base Established

Component Verification of Thermal Data Base Completed

System Verification of Thermal Data Base Complete on TETM

Qualification with PTC Completed

Figure 9-22. Summary

cost estimates and now that you are almost done with your program, how close were you able to make these estimates, how good were the cost estimates?

DR. HOWELL: Well that is very difficult to say because from Viking we have not really sat down and separated out all of the costs that have been associated with planetary quarantine. There was a decision early in the project not to do this. The costs associated with some of these things are very easy to obtain, like the cost of developing the bioshield, et cetera. Some of the costs associated with the selection of hardware and so forth become very difficult, become very program dependent and there was a conscious decision made early in the Viking project not to track the specific costs associated with planetary quarantine. So it's very difficult, if not impossible, to answer your question because I don't know what the actual costs were or have been associated with planetary quarantine on the Viking Project.

MR. TOMS: Dan Herman made a comment in the introductory session that for the outer-planet program, for the outer-planet probes, we would not include planetary quarantine in our present thinking. And I asked him the other day if he could give me more justification than just a whim on that. He says that there is a letter in existence - many of you may know of this - letter that was written to the Space Science Board (in fact it was more in the form of a paper by Dick Goody and Leibowitz and Others) that, in fact, made such a recommendation, and I think that was done more than a year ago. Until that is acted upon by the Space Science Board, it does at least give us a reason for working on the assumption that perhaps planetary quarantine for the outer-planet probes and for the outer-planet spacecraft wouldn't be necessary.

Of course, it is not only the probes themselves but the overall mission design, including such things as the economics of using a bus deflection maneuver and then not sterilizing the bus. They are all part of the same quarantine problem.

MR. DEFREES: What class clean rooms do you use for assembly and test operations?

DR. HOWELL: I'll let Al Hoffman from JPL talk about the orbiter.

For the lander we use a class one hundred thousand clean room environment for the assembly and testing of the Viking lander.

MR. DEFREES: Bob, do you use anything more stringent than that for components?

DR. HOWELL: No, Sir.

MR. DEFREES: You use the one hundred thousand throughout?

DR. HOWELL: In some cases the component assembly areas are equal to or less than a hundred thousand. In some cases we don't even require a hundred thousand environment for the assembly of the components. The basic requirement, for component assembly, is dictated by the functional requirements of that component. If, in fact, there are functional reasons why it should be assembled in a very clean environment, then it will be. So the component assembly spans a range from not fitting into one of the federal standards, 209(a) or (b), categories, to a flat one hundred.

MR. TOMS: Fine, well, I think we'll close that subject. Some of the authors have brought copies of papers with them. There are not enough to make a general distribution of them, but you can ask the authors themselves for copies, if you are interested.

MR. HYDE: Yes, I have a question. Al, would you sum up for me in one sentence your posture about the outer planets, on just the quarantine?

MR. HOFFMAN: On the Quarantine? I think there are considerable unknowns. As far as long-term planning, the picture is cloudy, as to the degree of stringency of the planetary quarantine and sterilization requirements. I feel that as long as there are biologists that are interested in exobiology for the outer planets, there will be some sort of quarantine constraint. The degree of that is unclear at this point. I think we would be amiss at this early stage in our planning to completely neglect it. We should factor it into some of our thinking. And, we have a good basis to start from, our Pioneer, Mariner and Viking experience.

MR. HYDE: I want to expand my question just to say outer planets and all their satellites?

MR. HOFFMAN: Yes, as you are well aware, Titan is of considerably more interest than some of the primaries. And the problem that I was addressing earlier, the reduction in the stringency of the sterilization requirements because of entry heating, may be going for us at Titan. Titan may be, indeed, the one that will dominate our sterilization and quarantine.

MR. KANE CASANI (JPL): The thing I was going to say that I think is important is that your point is well taken, that we ought to assume that there is going to be some quarantine requirements and whether or not those requirements have to be satisfied by actually heat sterilizing the probe is the uncertainty. In other words, it is on these that we can satisfy the requirements without having to heat sterilize the probe and in some cases we may have to heat sterilize. That is the thing that I think is of general interest here. I think we are certainly going to have the requirements.

MR. HOFFMAN: Yes, that point is well made.

J. HYDE: I would only add to that the question of the bus deflection maneuver versus the probe deflection maneuver. It is a crucial issue in this whole thing. If we have to turn around and make the probes, intelligent probes, capable of doing their own deflection, we are not talking about the same kind of probes we have been talking about the last couple of days. We are not talking about the same kind of money. So I think maybe you should start looking at the numbers game on this whole thing. Pay attention to the implications of putting a requirement on the probe to do the deflection maneuver. If you do that, I think we may be out of business.

MR. HOFFMAN: Let me make a comment relative to that first, Jim. I think, as you are well aware, up until 1971 there was an unwritten policy in the United States that bus deflection was not a mode that would be used for planetary missions. Then, after that time, if we can demonstrate that the planetary quarantine requirements can be satisfied using a bus deflection, that mode is an option that's available to us. And that is NASA policy. One concern relative to that is to demonstrate four or five nines reliability. Many of us get a little uneasy when we must demonstrate reliability greater than two or three nines with that type of operation. But I think it's a problem that can be addressed and worked.

MR. SEIFF: This will agree a little bit with what you said about the gravity of the change in the probe if the probe has to be deflected. Earlier studies have been performed based on that presumption that this was the way that it was to be done and it doesn't have as major an impact on the probe design as you are suggesting.

MR. HYDE: I don't agree with that at all, because I don't think that we are talking about probes in the price category that we have been discussing. If we have to talk about the intelligence required to perform the attitude stabilization maneuver and the deflection maneuver on the probe, I don't think we are talking about the same kind of numbers.

MR. SEIFF: I think the system that you are envisioning is more complex than what is needed to do the job.

MR. HYDE: Well, the issue is going to be bucks. And that is what we've got to address here. What I am trying to poke at is the money that is going to be associated with the impact on the design activity related to incorporating that capability into the probe, and I don't think we want to do that.

MR. TOMS: Let's hear from Bob DeFrees.

MR. DEFREES: I was going to make the same comment that Al just made to Jim relative to the NASA policy that is written into one of the specifications that the bus deflection is an acceptable, in fact, the preferred method of entry. The only thing you have to do is guarantee the probability or reliability of those things are at least as good, and that means, essentially, a reliability of 10^{-4}, that it will not contaminate the planet with the bus. With redundancy, that is fairly easy to accomplish. But I just wanted to interject that.

MR. SEIFF: The only thing I would like to emphasize in closing the discussion is there are studies on the record in which probe deflection maneuvers have been incorporated as part of the study. And I was just looking around the room to try to find some of the older characters who might have been involved in this; Steve Georgiev, for one. He did a study on a Mars probe that dates back about eight years, by now, I guess, in which that was considered to be the standard approach and it doesn't throw the kind of major monkey wrench into the works that has been suggested here.

MR. HYDE: We might want to take this up outside of this room. I think I need a parting comment too. We are not talking about studies, we are talking about MJU '79 with a probe. We have got to look at the problem of the bus-deflection maneuver, the reliability of that relative to the quarantine, very specifically. I think the cost...

MR. SEIFF: I don't disagree with that, that is fine.

MR. TOMS: Dan Herman wants both JPL and Ames to look more closely at the quarantine problem during the coming months and, of course, we are trying to get Larry Hall and his group back at Headquarters to bring the whole issue to a head, get a ruling on it we can live with, and go ahead from there. It's going to be quite a change of pace.

Now to the other design problems we want to talk about. We have two papers that include discussion of radiation effects. The speaker I want to bring up now is going to talk about not only radiation effects but also long-life batteries. These are two of the problem areas that he has been looking at. Lloyd Thayne from Martin Marietta Corporation.

RADIATION EFFECTS N75 20409

Lloyd Thayne
Martin Marietta Corporation

MR. LLOYD THAYNE: Gentlemen, I was preparing to present two papers here from the very beginning, during the seminar, and the other day in conversation with Ron, I was instructed that I had fifteen minutes to cover both of them. So if you see skeletons here, it is the skeletons of what was initially intended to be presented. Let me very quickly run through some areas. Because other speakers are covering radiation and long-life problems, I don't think it is necessary for me to go into any great depth.

Let's quickly go through a couple of areas that we have to be aware of with respect to radiation. Our colleague, Mr. Divita will cover in more detail the radiation effects problems that we are faced with in probes.

This graph (Figure 9-23) is related to cosmic radiation. It is in terms of displacement equivalents of 3 Mev electrons and 20 Mev protons, if they were to impinge on the components in question, i.e., the transistors, et cetera, that are inside of the boxes. It is assumed here the cosmic radiation is in the greater-than-100-Mev category. Notice that the shielding has very little effect. You get maybe a factor of two at the most and probably about a factor of one and a half change from no shielding to 225 mils of aluminum, assuming a spherical shielding condition. But note that the equivalent fluence is not high enough to be of concern.

Notice Figure 9-24 with respect to the problem of solar flares, the energies are somewhat lower and the effect of distance from the sun has a strong effect on total dose. The chart shows the equivalent 20 Mev proton displacement fluence in protons/centimeters squared/year. Here because of the low level of the particles in question, shielding, comes into effect quite significantly.

Shown in Figure 9-25 are some points I have taken from Pioneer 10 data. The projected impact on the probe missions with respect to going into Jupiter is quite encouraging. The actual measured

MAXIMUM EQUIVALENT FLUENCES DUE TO COSMIC RADIATION

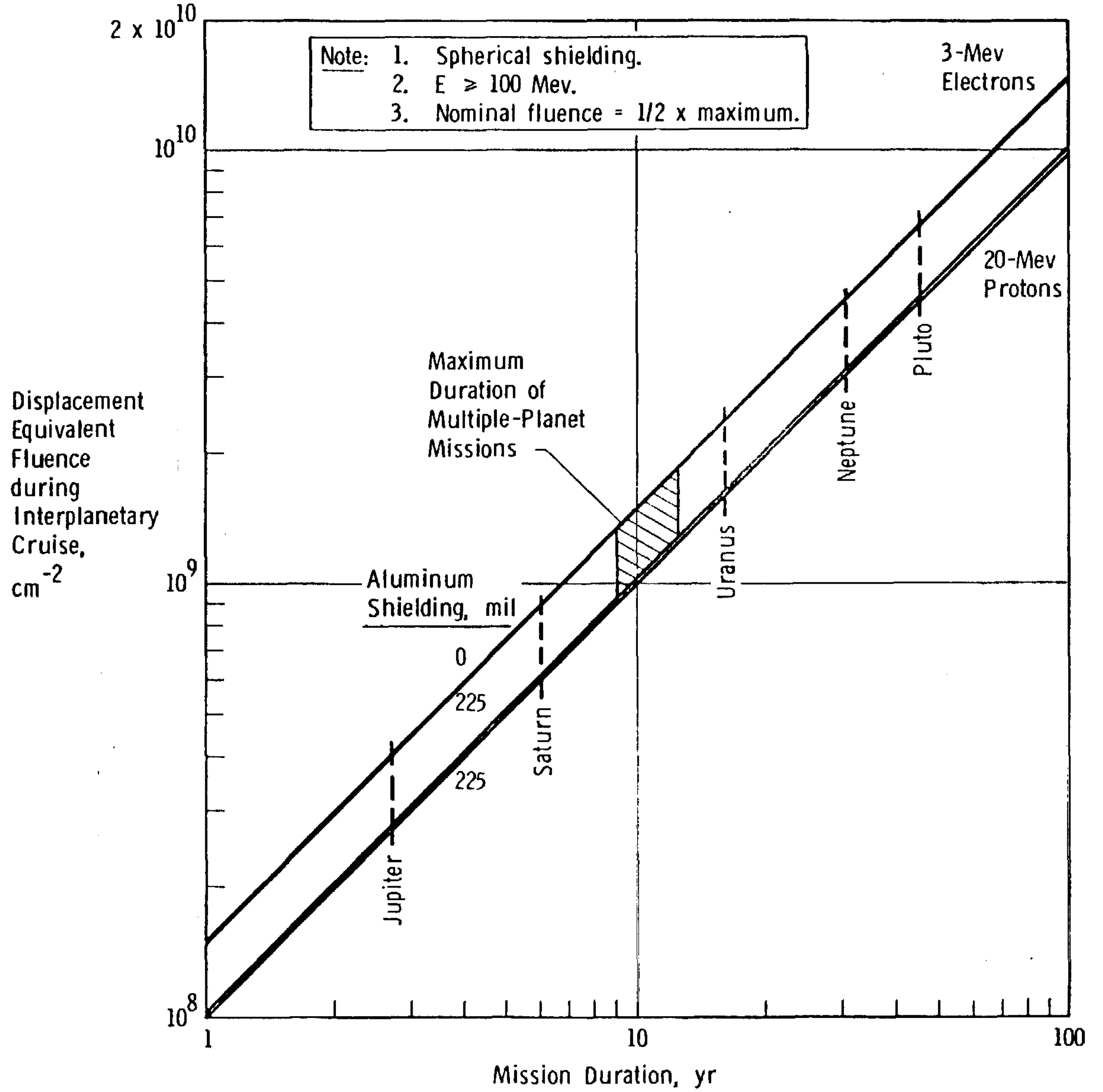


Figure 9-23

MAXIMUM EQUIVALENT FLUENCES DUE TO SOLAR FLARES

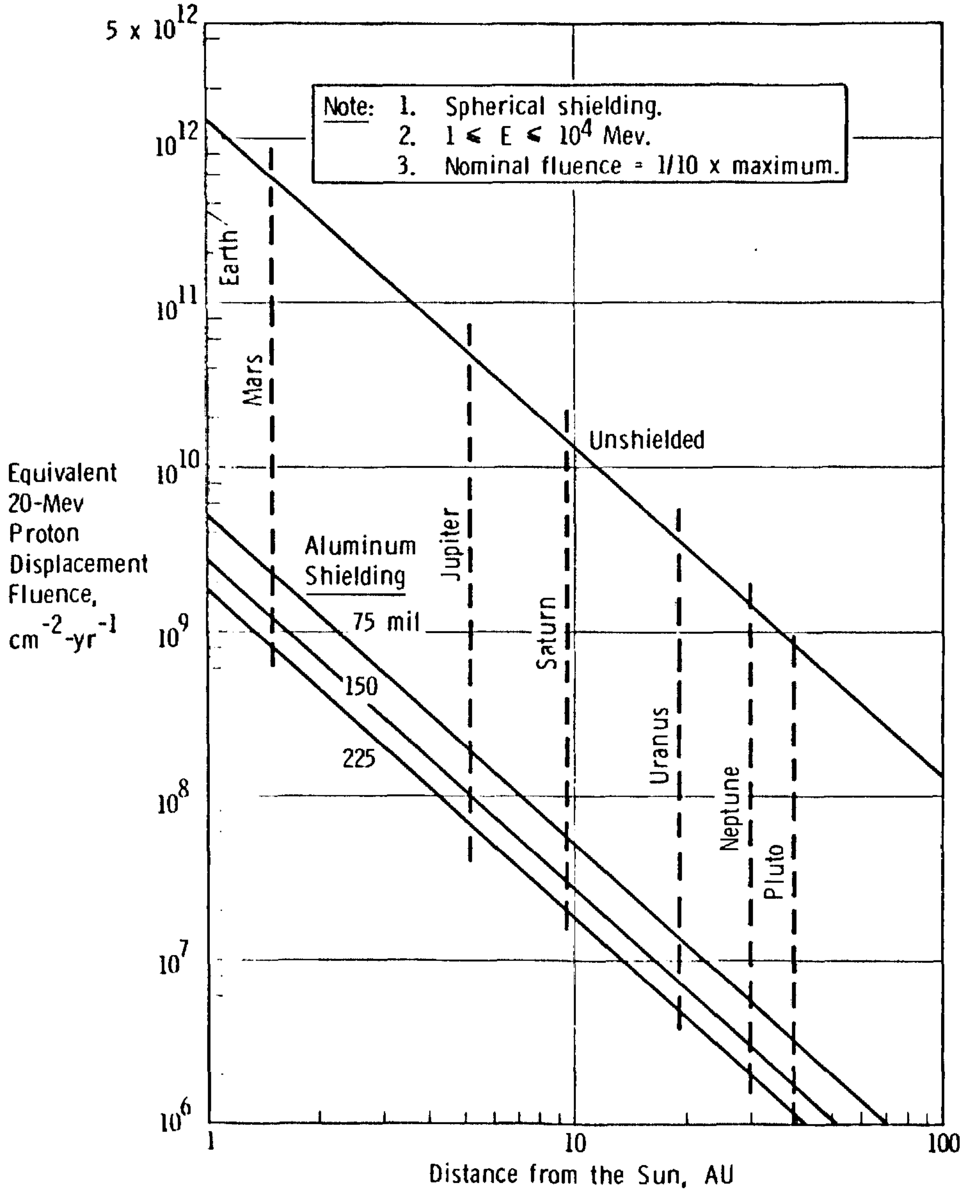
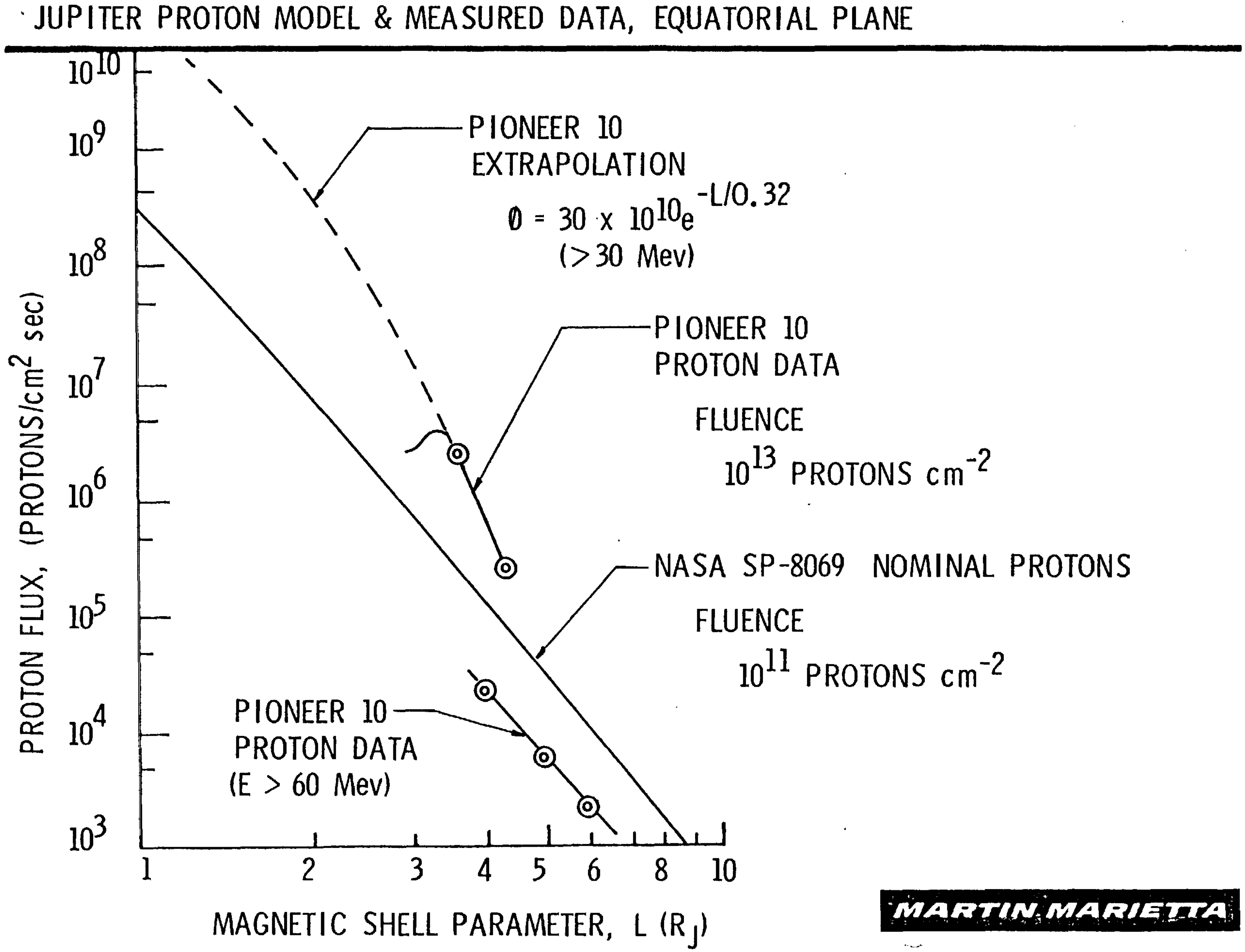


Figure 9-24



MARTIN MARIETTA

FIGURE 9-25

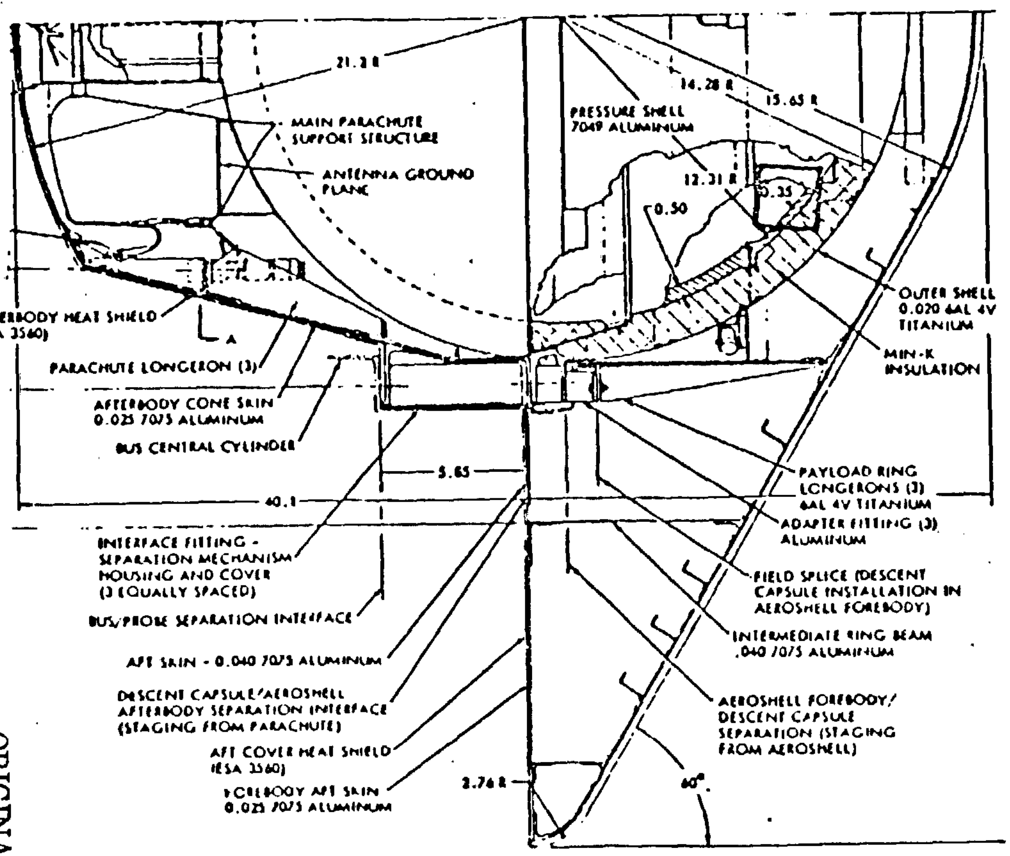
points are shown (circles) and it was noted that there was a tail-off at 3.6 R_J, approximately the orbital position of one of the satellites. The 60 Mev protons are slightly below the nominal proton fluence projected by NASA 8069.

The significant part is that if one were to integrate under the 30 Mev curve extended (dotted line), and assume that all protons below the 30 Mev level are removed, one still ends up with about 10^{13} protons per square centimeter by the time the probe enters. That is not quite acceptable, I think Mr. Divita will indicate later on that 10^{13} is probably a little more than we would care to have with respect to protons, since that is equivalent to probably 3×10^{14}. We don't really care to design probes to that level.

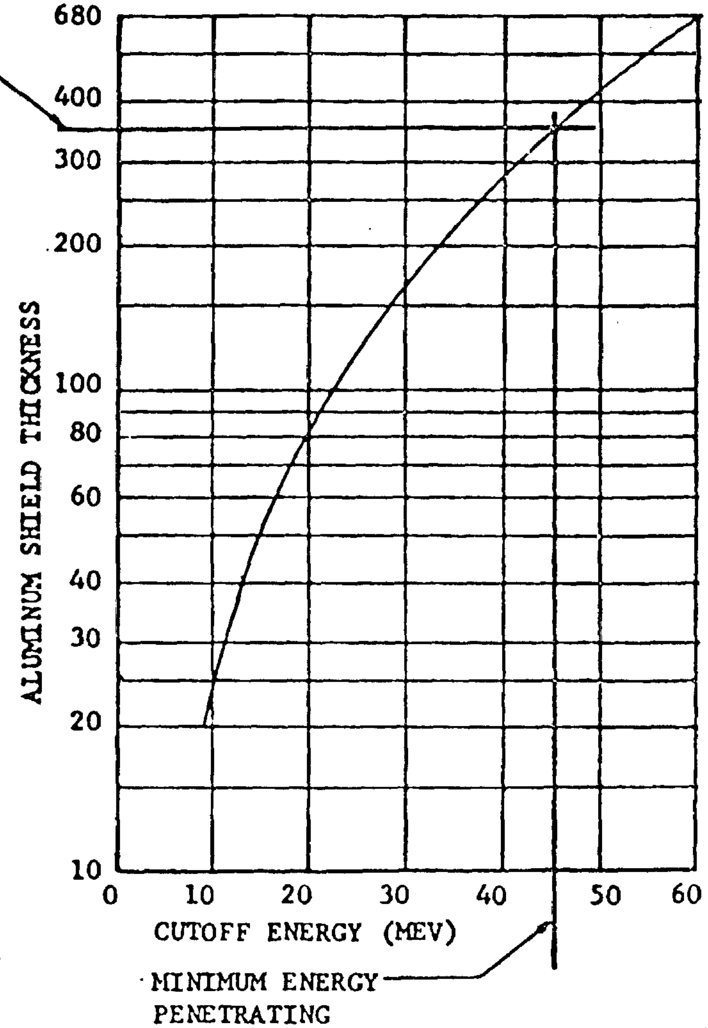
The 60 Mev proton fluence is somewhat below the NASA nominal model. If you were to take the nominal curve and assume that the probe goes into one R_J, then it ends up with about 10^{11} protons per square centimeter. I think we can live without any serious impact with that two orders of magnitude of improvement. One point of interest is that as you integrate under these curves, you find out that you can forget everything far out because it is only the last half of an R_J that is going to provide about 90 percent of the fluence anyway. So, integrating under the curves is kind of a waste of time and effort. You might as well just pick a point at 1.25 R_J and assume you are going to be in that area for the period of time it takes to go from 1.5 R_J to 1.0 R_J and that will either frighten you away or solve the problem for you.

I looked at the projected large-probe Pioneer-Venus version that was presented to Ames by Martin Marietta and I think that the Hughes large-probe is going to be similar in that in both cases you have to have a pressure vessel. This is the MMC hundred-bar probe, Figure 9-26 which has to have a pressure vessel. In this case, I found that the minimum thickness of the pressure vessel was about 350 mils of aluminum. I am not sure what it is for the

PROBE PROTON SHIELDING



LARGE PROBE CROSS SECTION



MARTIN MARIETTA

Figure 9-26

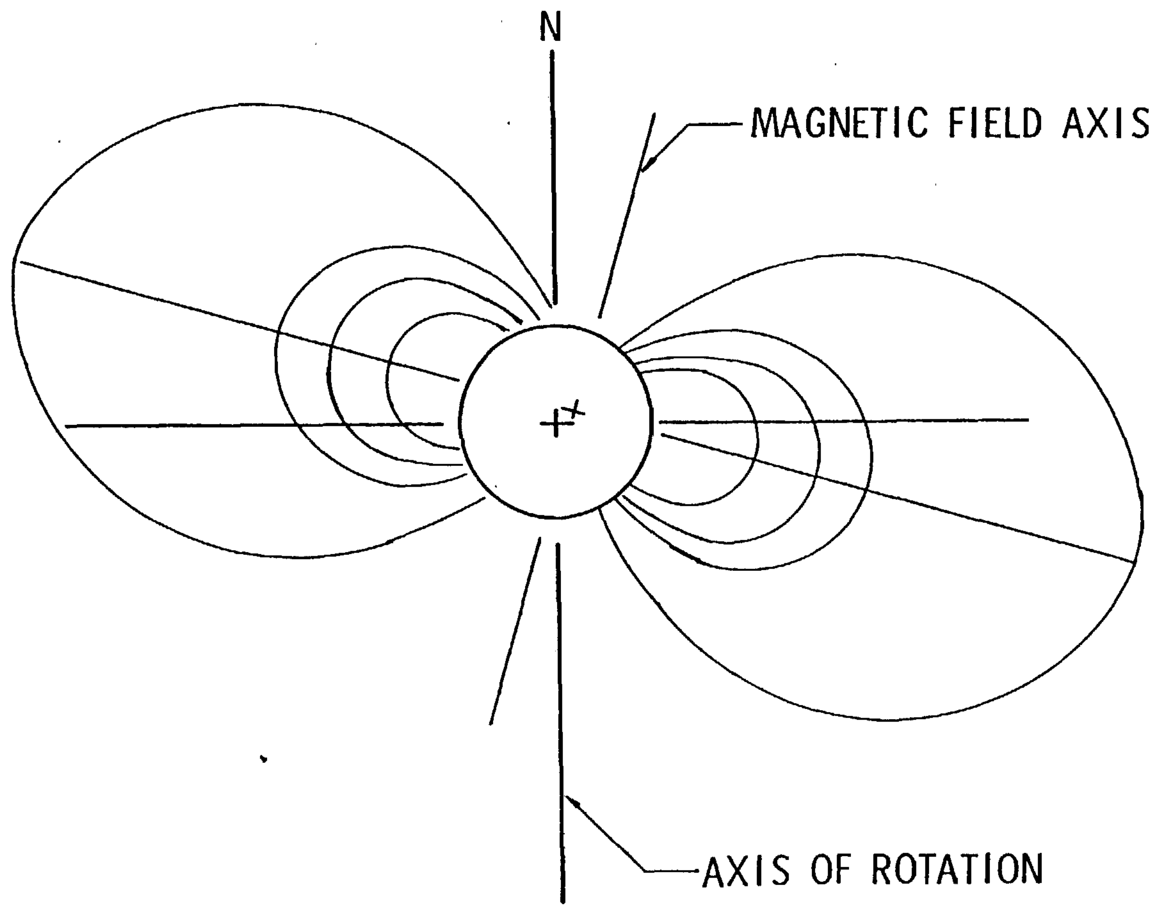
ORIGINAL PAGE IS OF POOR QUALITY

Hughes probe but you can translate from 350 mils to any other point.

From the curve on the right, you will find that as the shielding thickness goes up, the minimum energy of the protons that get through the shield,and are, therefore, capable of doing damage to the electronics, increases. For the 350 mils thickness, essentially no protons with energies less than about 40 Mev are going to get through the shield. If you recall, from the previous chart, the 30 Mev and the 60 Mev proton levels essentially bracketed the NASA nominal model. If you could translate that 40 Mev to the nominal model we are talking about approximately 10^{11} protons per square centimeter as that which is projected to get inside of the pressure vessel. That is going to be reduced even further by the fact that you have all the ballistic paraphernalia on the outside; the heatshield and so forth are going to add additional shielding to the system.

Assuming then that we can get in with the type of trajectory that Pioneer 10 took, there is some capability of increasing our chances even more by taking advantage of the fact that the centroid of the magnetic field is offset from the center of the planet and tilted by some fifteen degrees in the nominal model from Pioneer 10. Notice Figure 9-27 -that the latest projections, that I have found at least, indicated that the centroid was offset about $0.2\,R_J$ from the center and up towards the northern pole by about $0.1\,R_J$. This gives us a little bit of help in getting the field off to one side. If one were to consider an entry in the southern hemisphere, assuming the same latitude on either side, one can see that you can save quite a bit by coming in on the side opposite the centroid. This isn't a matter of going in posigrade versus retrograde, it is a matter of timing as to what the position of rotation of the planet is at the time the entry takes place. There can be possibly as much as an order of magnitude but more probably a factor of two to five, improvement in the radiation expected by selecting the time of arrival of that probe with respect to the rotation of the planet.

JUPITER TARGET PATTERN



MARTIN MARIETTA

Figure 9-27

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OF POOR QUALITY

This is kind of a composite curve (Figure 9-28) because we are not presently talking about being able to drop in a 100-bar probe and then also go into orbit with our present payload capabilities unless one takes advantage of the Mars swing-by talked about the other day. (I am not really proposing that, but it is a possibility. If one were to take that course you could not only get a large probe into Jupiter, but you could also have sufficient capability to go into orbit with the bus.) But the point I wanted to show here was that once one has dropped off a probe or gone into orbit, that you can improve your radiation protection if you make the bus orbit such that it is an integer multiple of the rotational period of the planet; so that it always comes back at the location of minimum radiation.

That's basically the comments that I wanted to make with respect to radiation. Now let me tell you just a little bit about another problem I am concerned with, that of long-life batteries for these probes.

We've done a little testing on some batteries we have designed at Martin Marietta taking basically an Eagle Picher silver zinc cell, modifying the size of the plates, the separator material, the number of wraps, and so forth, in order to learn more about the critical areas that are involved. The standard cell starts with forty-eight watt-hours per pound and drops rapidly (Figure 9-29), which isn't very useful in any of these probe missions because we are beyond the twelve-month period on just about all of them.

From the modified cell we now have test data out beyond twenty-months with cells that still give us, at 30°F storage, right at forty watt-hours per pound in all three test modes: discharge, charge and float-stand.

MINIMUM RADIATION JUPITER ORBITER MISSION

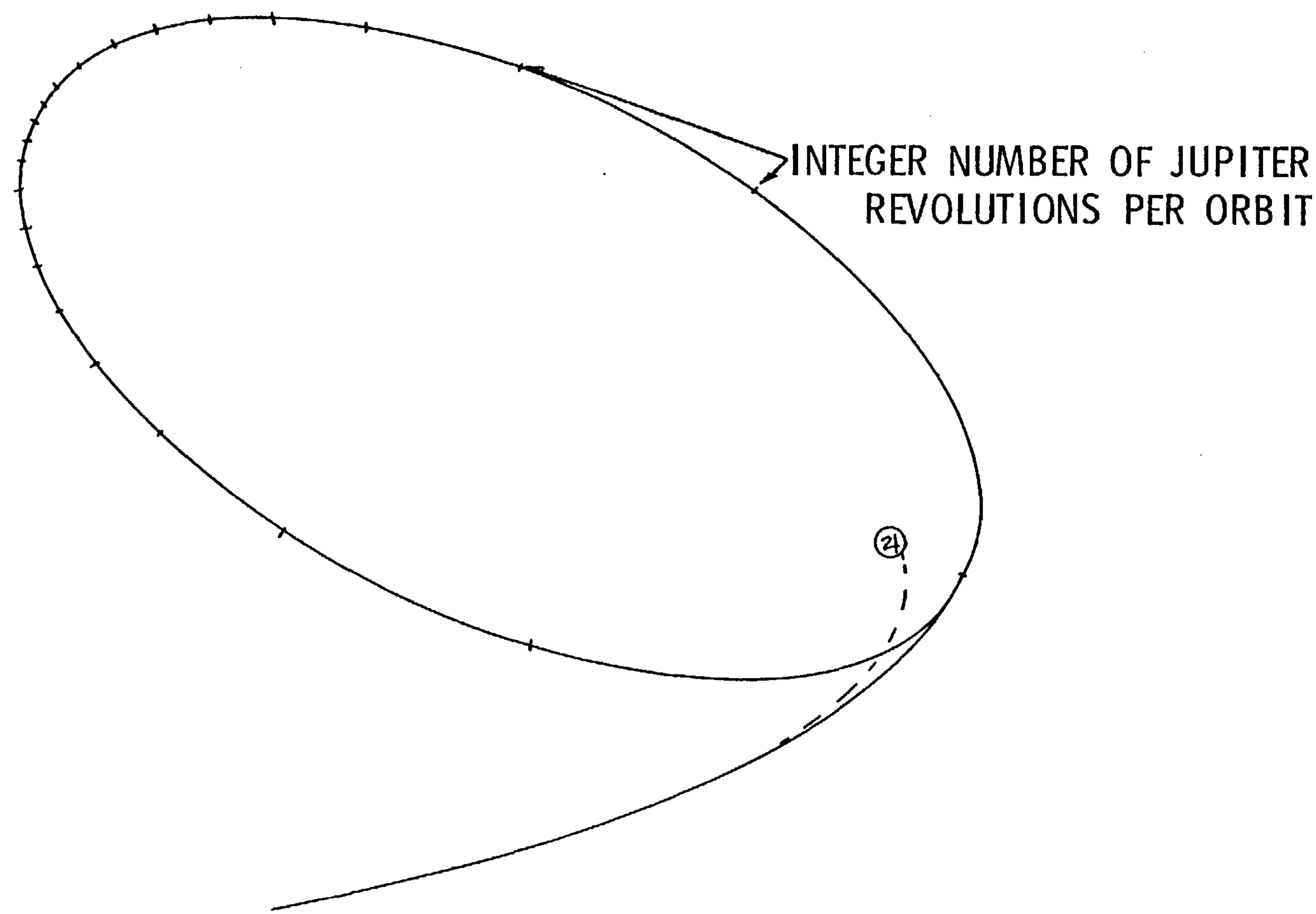

Figure 9-28

MARTIN MARIETTA

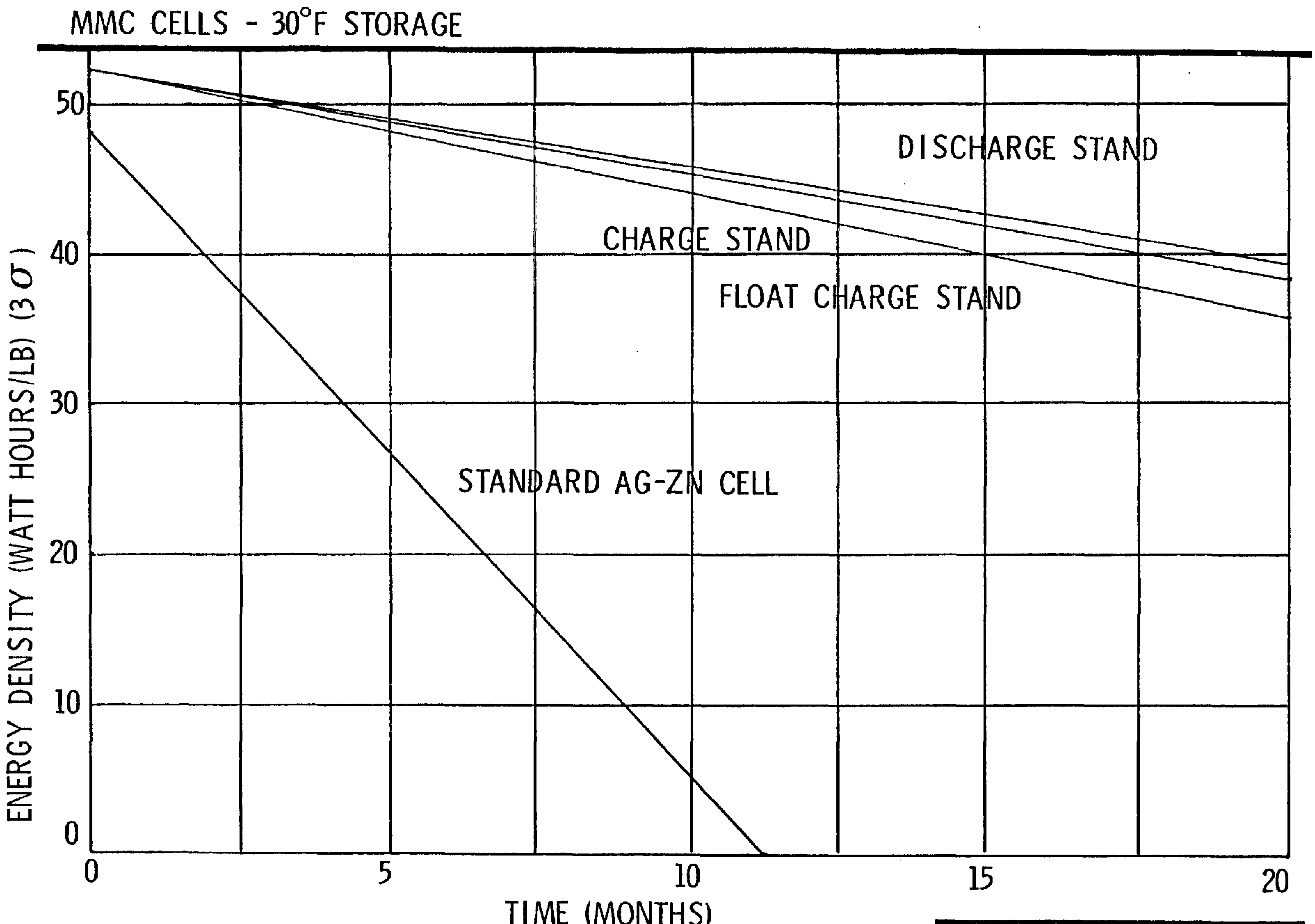
MMC CELLS - 30°F STORAGE
DISCHARGE STAND
CHARGE STAND
FLOAT CHARGE STAND
STANDARD AG-ZN CELL
ENERGY DENSITY (WATT HOURS/LB) (3 σ)
TIME (MONTHS)
0
10
20
30
40
50
0
5
10
15
20

Figure 9-29

If we store them at about 55°F (Figure 9-30) we find that we improve that slightly over what we had at 30°F. I don't have a curve on the cells at 75°F, but we got less capacity out of the cells at room temperature than we did at either 30° or 55°. It just turned out that 55° is about the optimum temperature. At the colder temperatures we had charge problems on the cycles, and at the hotter temperatures, the degradation in the cells occurred faster.

I might make a comment before I go into the next slide. Those groups of cells that have had failures have shown no failure indication at all for some extended period of time and then suddenly the whole group goes in a very short period of time. The separators fail in essentially the same mode. It is a chemical oxidation of the separators that has occurred so far. We have had, to date, no shorting between the plates due to dendrites.

We talked to a few people about sterilization (that is a problem that we have been talking about here this morning) and some of the comments that have been made with respect to sterilization are shown on Figure 9-31. They are taken out of context. You don't see the question that was asked and you don't see the whole conversation that was held. So please consider that fact as you read them. It is obvious that some have done no sterilization work; some have found failures. For instance, Tom Hennigan at GSFC indicated that they had had some mechanical problems with the ESB units. You talk to Al Jordan at ESB and he likes to talk about the success they had on their Viking test. Sandy Seidman at Yardney says they have been successful.

But what it boils down to as you really dig into it is you find that all of them have problems. They all have, basically, the one problem and that is that when you heat these filled cells you have extreme gas pressures produced and you have structural failures of the cells. Now, they have done some work at Stanford,

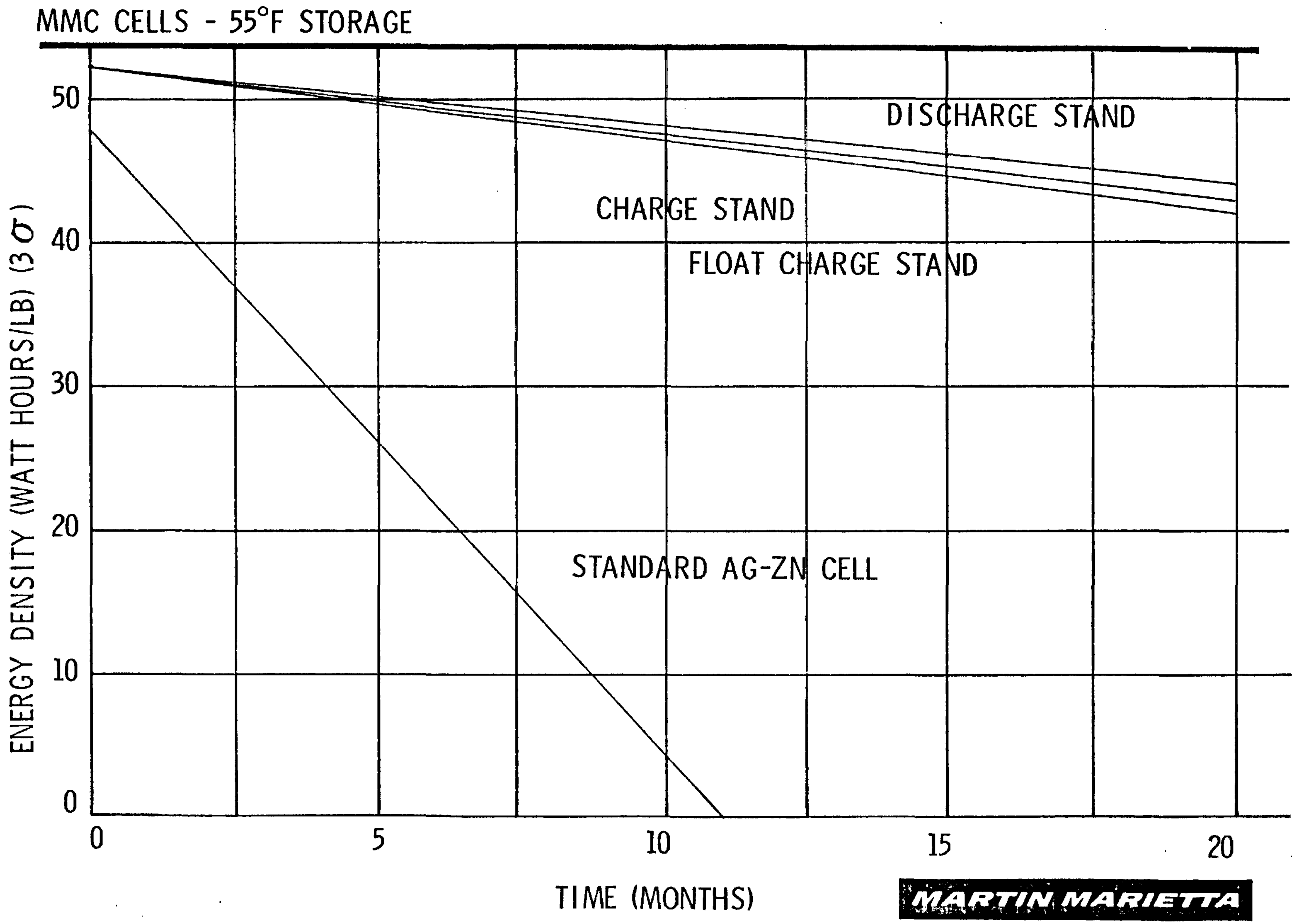
MMC CELLS - 55°F STORAGE
DISCHARGE STAND
CHARGE STAND
FLOAT CHARGE STAND
STANDARD AG-ZN CELL
ENERGY DENSITY (WATT HOURS/LB) (3 σ)
0
10
20
30
40
50
0
5
10
15
20
TIME (MONTHS)
MARTIN MARIETTA

Figure 9-30

STERILIZATION - AG ZN

CONTACT	COMMENTS
VERN BJORK - POWER SOURCES	NO STERILIZATION WORK
TOM HENNIGAN - GSFC	NOT TOO SUCCESSFUL, MECHANICAL PROBLEMS ON E.S.B. UNITS, NO WORK ON DRY CELLS.
JOHN BOZEK - LeRC	SUCCESSFULLY PASSED STERILIZATION (WET) (YARDNEY DESIGN - WORK AT STANFORD)
SANDY SEIDMAN - YARDNEY	SUCCESSFUL - POLYETHYLENE, ALSO PREDICT SUCCESS ON CERAMIC SEPARATOR CELLS.
JEFF WILSON - EAGLE PICHER	SUCCESSFUL - MECHANICAL SEAL BIGGEST PROBLEM
AL JORDAN - ESB	SUCCESSFUL ON VIKING TEST.

Figure 9-31

MARTIN MARIETTA

supported by Lewis, where they have beefed up the cell structure and have been able to solve some of that problem but it costs you quite a bit in energy density. No one who we talked to had done sterilization work on dry cells.

Long-life wet stand is discussed in Figure 9-32. We have found that we can get higher energy density for short periods of time but if we want them for any extended period of time, it drops off rather rapidly. Yardney has indicated that they are working on a ceramic separator cell that they are predicting will have a seven-year wet stand life. This would solve most of our headaches, but, unfortunately, we haven't got seven years to wait for them to prove it.

There is a great deal of difference of opinion as to whether or not there is in existence today a silver zinc cell that will last seven years in the dry stand to be activated after you get out there. (Figure 9-33). There are even concerns that you can put an active small secondary battery with it and have it work to activate the dry one when you get out there. Both McDonnell-Douglas and Martin have proposed a remote-activated battery for these deeper space probes but there are still a lot of problems that have got to be solved. It isn't something that we can say it is there, whenever we get around to using it we can use it. There are some problems that have got to be worked out. The one that comes up more frequently than anything else is that they don't know what happens in a vacuum with the plates. Some have mentioned that we ought to put some kind of an hermetic seal around it to avoid drying out the plates and the cracking that follows because you have got to band the plate edge so that when you go into the high-g forces, you don't tear them up.

So, those are just some points in passing. It is not a simple problem, it is not a solved problem, we have got to work it.

MR. TOMS: Thank you, Lloyd. Does anyone have questions for Lloyd? Bill Dixon?

LONG LIFE WET STAND - AG ZN

JOHN BOZEK - LeRC	LEAKAGE PROBLEM AFTER 21 MONTHS SOLVED BY MECHANICAL REDESIGN SOLVED PLATE SLOUGHING PROBLEMS.
EAGLE PICHER TEST @ MMC	40 WH/# @ 2 MO. 20 WH/# @ 7 MO.
MARTIN REDESIGN	40 WH/# @ 20 MO.
SANDY SEIDMAN - YARDNEY	WORKING ON CERAMIC SEPARATOR CELL THAT SHOULD HAVE 7 YEAR WET STAND LIFE. (For LeRC)

Figure 9-32

MARTIN MARIETTA

LONG LIFE DRY STAND - AG ZN

JEFF WILSON - EAGLE PICHER	CONCERNED ABOUT PLATES DRYING, SHRINKING, AND CRACKING. (CAUSED BY BINDING OF PLATE EDGES TO WITHSTAND ENTRY g FORCES). NO VACUUM DATA AVAILABLE.
SANDY SEIDMAN - YARDNEY	7 YEAR DRY STAND LOSS, 25-30%. NO VACUUM PROBLEMS WITH HERMETICALLY SEALED OUTER CASE.
AL JORDAN - ESB	NEED SEALED CONTAINER TO AVOID VACUUM PROBLEMS. VACUUM EFFECTS UNKNOWN.
VERN BJORK - POWER SOURCE	NO PROBLEM WITH 7 YEAR DRY STORAGE.
BILL ROBERTSON - LeRC	NO PROBLEMS UP TO 8 YEARS DRY STORAGE.

Figure 9-33

MARTIN MARIETTA

DR. DIXON: Yes, I think there are a few points that he made that deserve some comment. This all has to do with the radiation portion of his talk. The first was I concur on the probe that the most significant part is the innermost L shell but I think with regard to the bus that goes by that is not necessarily true. Particularly if electrons are the problem rather than protons they seem to slope off more gradually with L shells. So, therefore, you are interested in things farther out for that purpose.

MR. THAYNE: Yes, my comments applied to the probe itself, and not necessarily to the bus. It's a whole new ball game when you are talking about the bus.

DR. DIXON: Also, with regard to the offset effect of the magnetic dipole, radiation fields are most likely symmetric with respect to the magnetic equator. It doesn't necessarily mean you want to land the probe on the side opposite the offset. You may want to land it on the other side and take advantage of a sweeping effect, sort of like the South Atlantic anomaly, it may lead to voids near the planet.

The third one has to do with the comment about the probe-orbiter mission. I think with the sort of probes we are talking about here, 350 pounds or so to Jupiter, we have shown that the Pioneer on the Titan launch vehicle can do both the probe and the orbiter missions.

MR. THAYNE: I think I agree with you if you talk about that size probe. My comments applied to the hundred-bar probe with the large shielding capability which is not in the three-hundred pound class but upwards of six-hundred to a thousand-pound class of deep-entry probe. If you get the probe small enough and the booster large enough, you can handle both or either problems. It is just a trade-off you have got to work.

MR. TOMS: Did Kane Casani want to make a remark?

MR. CASANI: Yes, I think your point about the battery life time, what happens to that battery during the seven years, is really going to be a problem. It is probably going to be one of the toughest problems that we are going to be confronted with on this probe. The thing I was wondering is, you showed a lot of data but you didn't show any specific energy numbers. What are we talking about in power densities of those batteries. Do you have any feel for that? What watt-hours per pound?

MR. THAYNE: You mean the earlier curves that I showed there?

MR. CASANI: On those last two you showed on wet and dry batteries.

MR. THAYNE: Okay. Right now for the wet batteries there is no way to predict how you would end up at seven years because we can't get much beyond two, if that, before we get total failure of the cells. And it looks like even without failures, it's sloping off to the point where you're down to maybe ten to fifteen watt-hours per pound for the wet cells.

For the dry ones, the bulk of the people that I talked to are projecting only five to ten-percent loss due to the seven-year stand. Some are projecting as much as twenty-five or thirty percent. You also get a projection of thirty to thirty-five percent due to sterilization, which, if you activate the battery while it's still on the bus, can be recovered by recharging the battery; so you can recover everything you lost in the sterilization of the dry cells in that mode. But if you use a remotely activated battery we are talking about twenty watt-hours per pound, because about half of the weight of the battery is going to be eaten up by the activation system. If you are lucky, you can micro-miniaturize it to that degree. We are talking of a forty watt-hour per pound battery and that much more weight in activation system.

MR. TOMS: Our next paper is concerned with the Jupiter radiation environment which an outer-planet probe will have to go through if it is on a Jupiter swing-by to Uranus. Ed Divita from JPL is going to talk about the kind of materials and hardware effects produced by the Jupiter radiation environment.

JUPITER RADIATION BELT ELECTRONS AND THEIR EFFECTS ON SENSITIVE ELECTRONICS

E. L. Divita
Jet Propulsion Laboratory

MR. DIVITA: I will discuss specifically the electron environment trapped at Jupiter; testing performed to simulate the effects of electrons on MJS77 (Mariner Jupiter-Saturn 1977) sensitive piece parts, and test results from those simulations.

I was pleased to see a preliminary analysis presented on the proton radiation effects because I am not going to address protons. However, I think the proton environment eventually may have a significant impact on the design of Jupiter probes.

The data base used which is now a significant data base is from the Pioneer 10 observations. At this point in time the emphasis is predominantly on electrons. The proton data base which includes protons above 35 Mev, protons above about 65-70 Mev and lower energy protons (∿1 to 20 Mev) are currently being developed into an engineering model. Considerable uncertainty exists in both low-energy protons, below 35 Mev, and their extent. Therefore, I will specifically address the electron problem. The Pioneer project is providing a current summary of the low-energy protons observations.

Figure 9-34 is an introductory slide which will give you a reference to the spatial distribution of the trapped electrons. The reference is a set of isoflux contours mapped on a Jupiter fixed-dipole coordinate plot using the magnetic polar, Z, axis measured along the planet offset dipole and the L-shell, R_J, axis measured along the magnetic equator in the radial direction.

We have taken the model from the February, 1974 Workshop, which was held at ARC by the Pioneer 10 Project. This map is for electrons having energy E greater than 3 Mev. The workshop data allow us to map as is done for the Earth Van Allen Belts,

CONTOURS OF CONSTANT FLUX J (CM^{-2} S^{-1}), 1974 WORKSHOP ELECTRON DATA, E_e >3 MeV;

JOVIOMAGNETIC COORDINATES (ASSUMING LONGITUDINAL SYMMETRY)

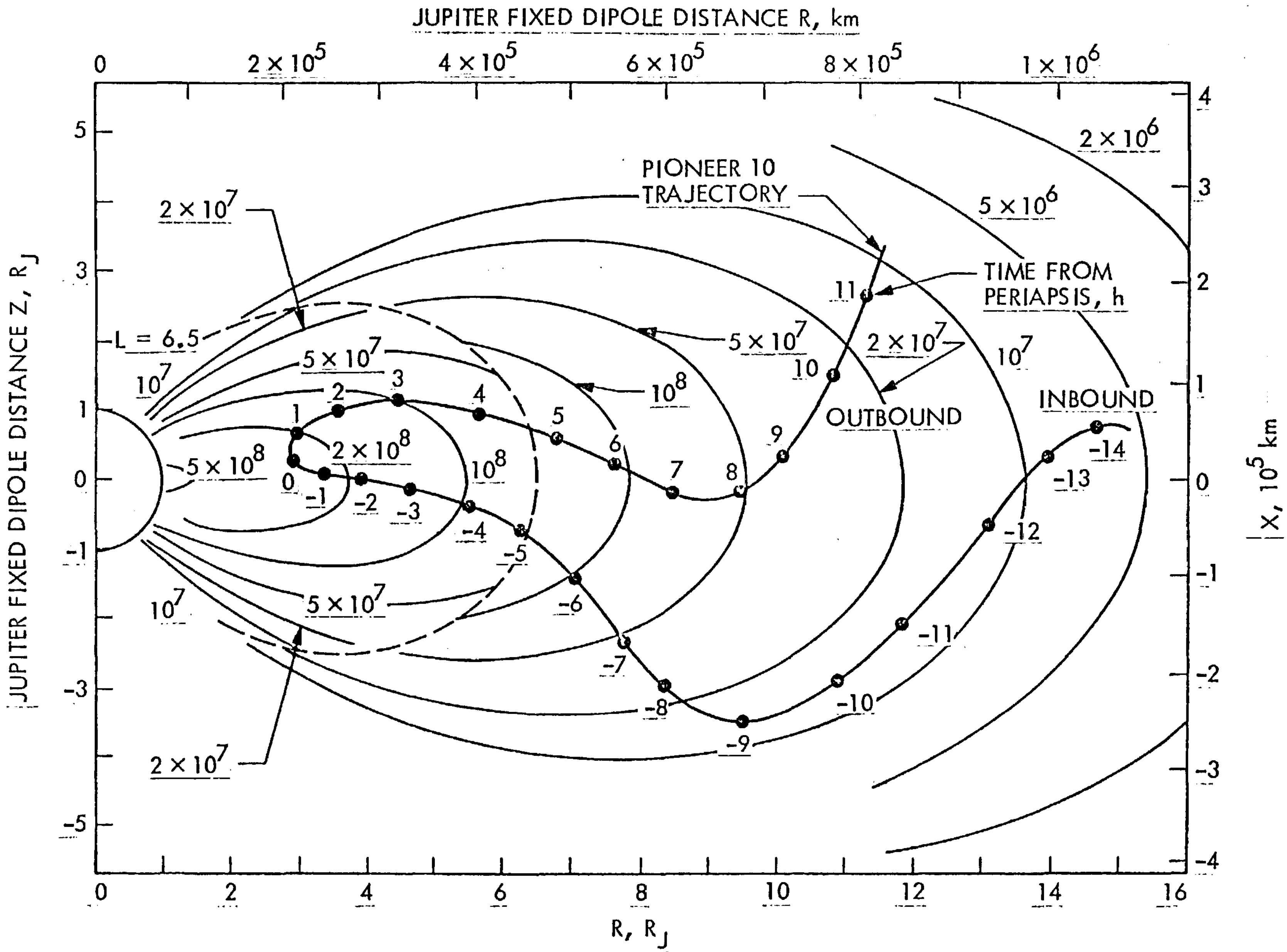

FIGURE 9-34

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with symmetry, a set of contours about the planet Jupiter. Based on available observations, we can map for lower energies down to 550 Kev, and for higher energies up to 31 Mev.

The contour map in Figure 9-34 is used to address some of the important features of the Belt. These electrons peak near a little more than on R_J from the center of the planet at 5 x 10^8 electrons per square centimeter per second above 3 Mev. This level is a significant flux and it potentially can interfere with sensitive science instruments and sensitive materials. From about 3 R_J to about 12 to 14 R_J the reduction is about a factor of 1/50 decrease in flux along the magnetic equator - this small decrease emphasizes the extensiveness of the trapped radiation belt.

The next feature in Figure 9-34 is the fluence accumulated by Pioneer 10. The flight path shown indicates that it was significant with a peak flux of 3 x 10^8 e/cm^2-s. Science measurements taken along this flight path allowed good mapping of the trapped particles.

The flux and fluence data presented for candidate MJS '77 flybys are determined as described for Pioneer 10. A family of flight paths with various perijove distances were used to evaluate fluences accumulated along those flight paths. Figure 9-35 shows the results of this evaluation as a set of accumulative fluences based on using several contour plots corresponding to different integral energies. The integral fluence is given as a function of energy for selected perijove distances, 5.0, 8.8, and 12 R_J. This range essentially encompasses the region of interest to MJS '77.

An important feature is the significant change in slope of the integral fluence at 3 Mev. For the 5 R_J perijove case the fluence level is about 5 x 10^{12} electrons per square centimeter above 3 Mev. Pioneer 10, based on using the same model, and the flight path shown in Figure 9-34 encountered about 7 x 10^{12} elec-

INTEGRAL ELECTRON FLUENCE ENERGY DISTRIBUTION FOR SELECTED PERIJOVE DISTANCES

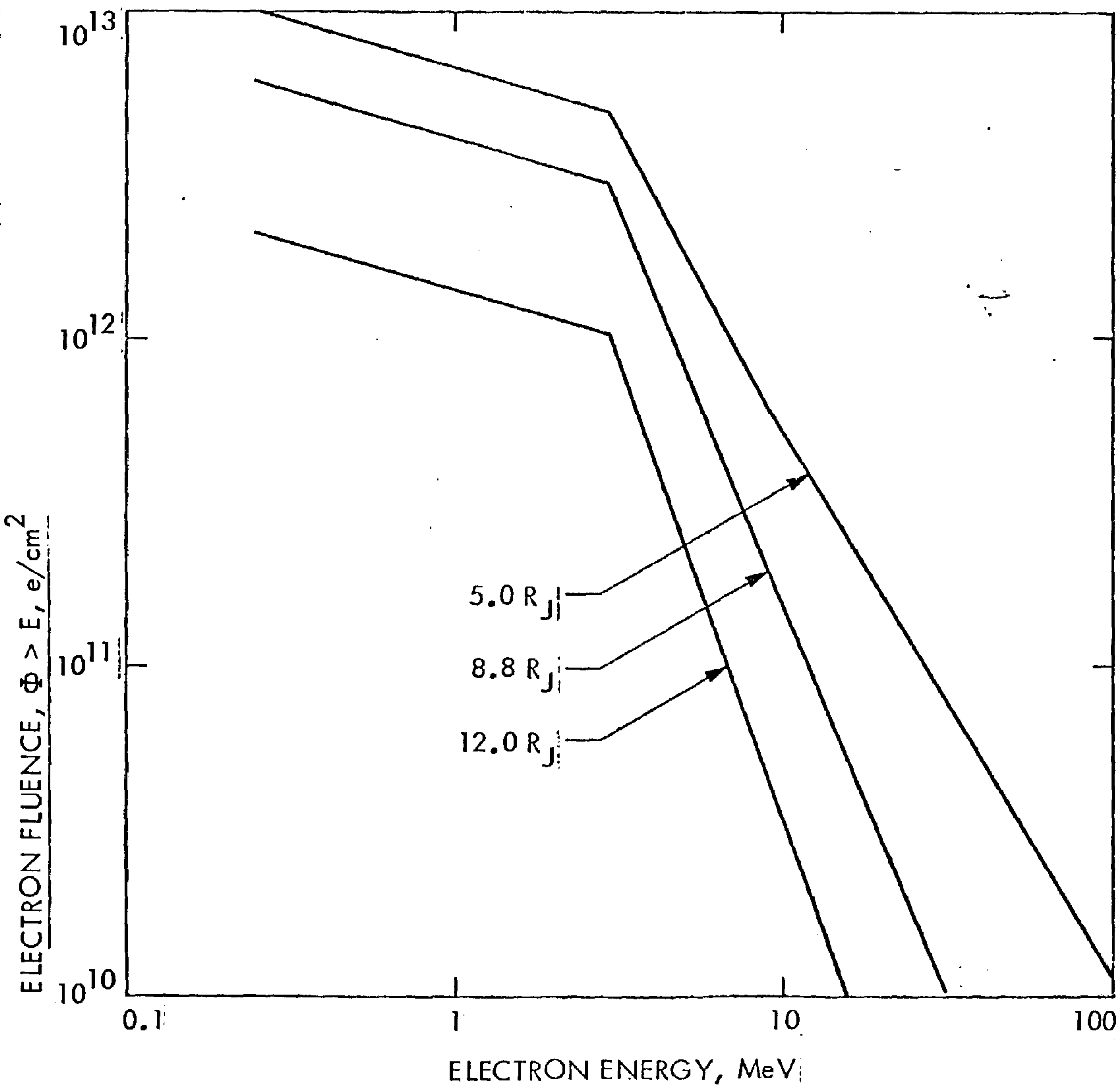

FIGURE 9-35

tron/cm^2 (E>3 Mev). Therefore, at 3 Mev the integral fluence for a 5 R_J perijove encounter is essentially the same as that which Pioneer 10 accumulated.

To specify test levels based on these spectra it is necessary to collapse the spectra to single energy equivalents of the spectra. This is accomplished by accounting for either one or both of the two major types of damage resulting from radiation: one, ionization; the other, displacement. To perform reasonable and practical tests, and to test with the facilities that are available, it is necessary to use cyclotrons (D.C. steady state or pulsed accelerators) to produce the desired high-energy electrons. In either case, using a mono-energetic electron is a practical simulation. The use of gammas as a substitute for electrons to simulate ionization is also generally acceptable provided that only ionization degradation is expected to dominate. Gamma substitution is the most practical test method. The predominant degradation mechanism for electrons at these fluences is ionization. The equivalency for ionization is performed on a total dose basis.

Figure 9-36 displays a plot of the fluence-to-dose conversion for the ionization produced by electrons as a function of energy. This dose conversion is an absolute conversion and it was evaluated using the energy loss dE/dx (Mev-cm^2/gm) in silicon.

Figure 9-36 also contains a curve which defines the other type of degradation displacement damage. In order to generate a set of test levels to simulate displacement requires energy equivalencing. This is required because displacement varies significantly with energy and depends on the types of materials and, as well, what happens in the material itself. The displacement damage curve in Figure 9-36 is specified as a relative displacement damage because it is the relative differences between energies that validate the assumption for its use. The spectra (see Figure 9-38) are weighted by the normalized values to yield a spectrum equivalent the 3 Mev level.

ELECTRON IONIZATION ENERGY DEPOSITION

AND

RELATIVE DISPLACEMENT DAMAGE IN SILICON

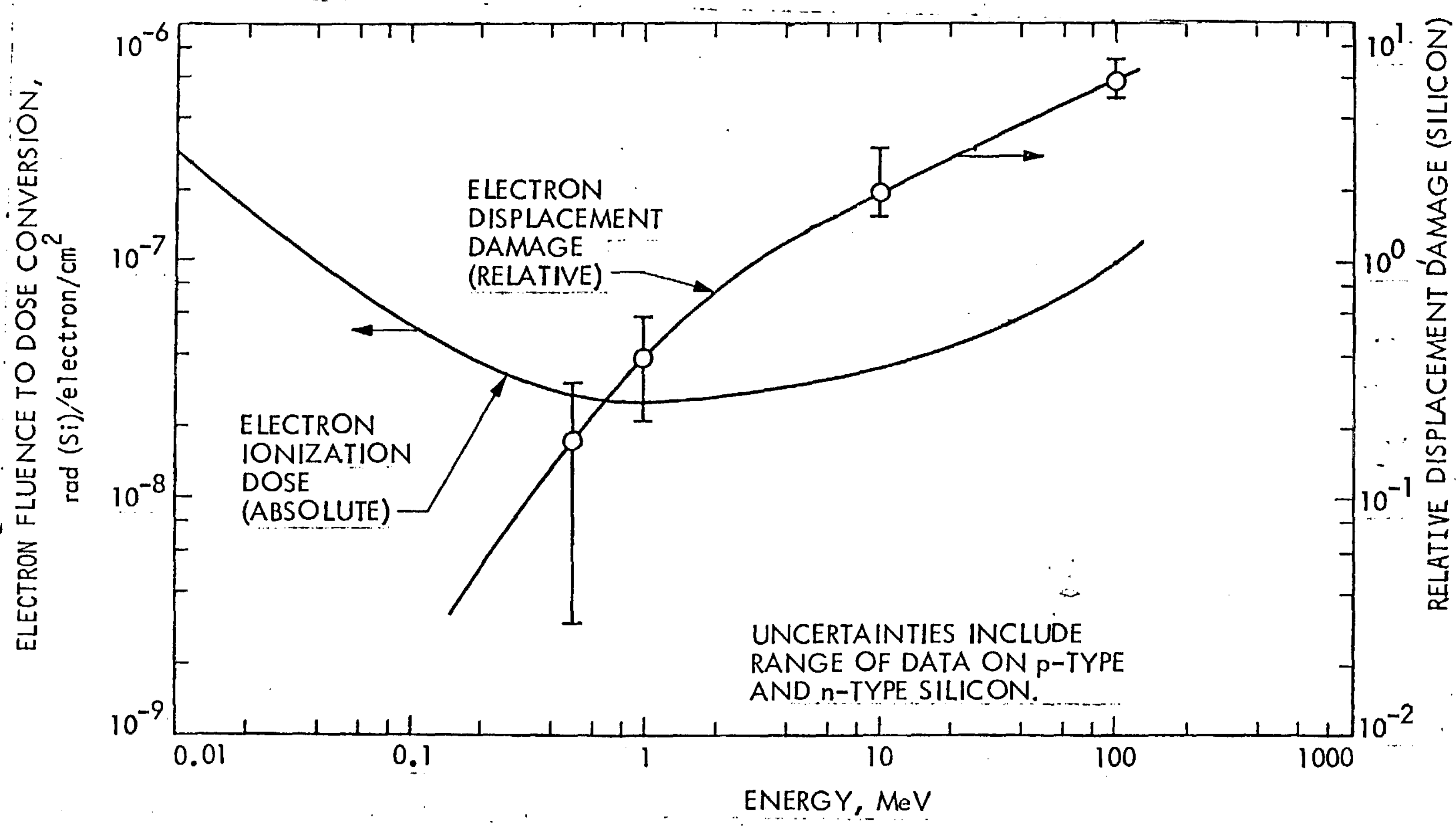

FIGURE 9-36

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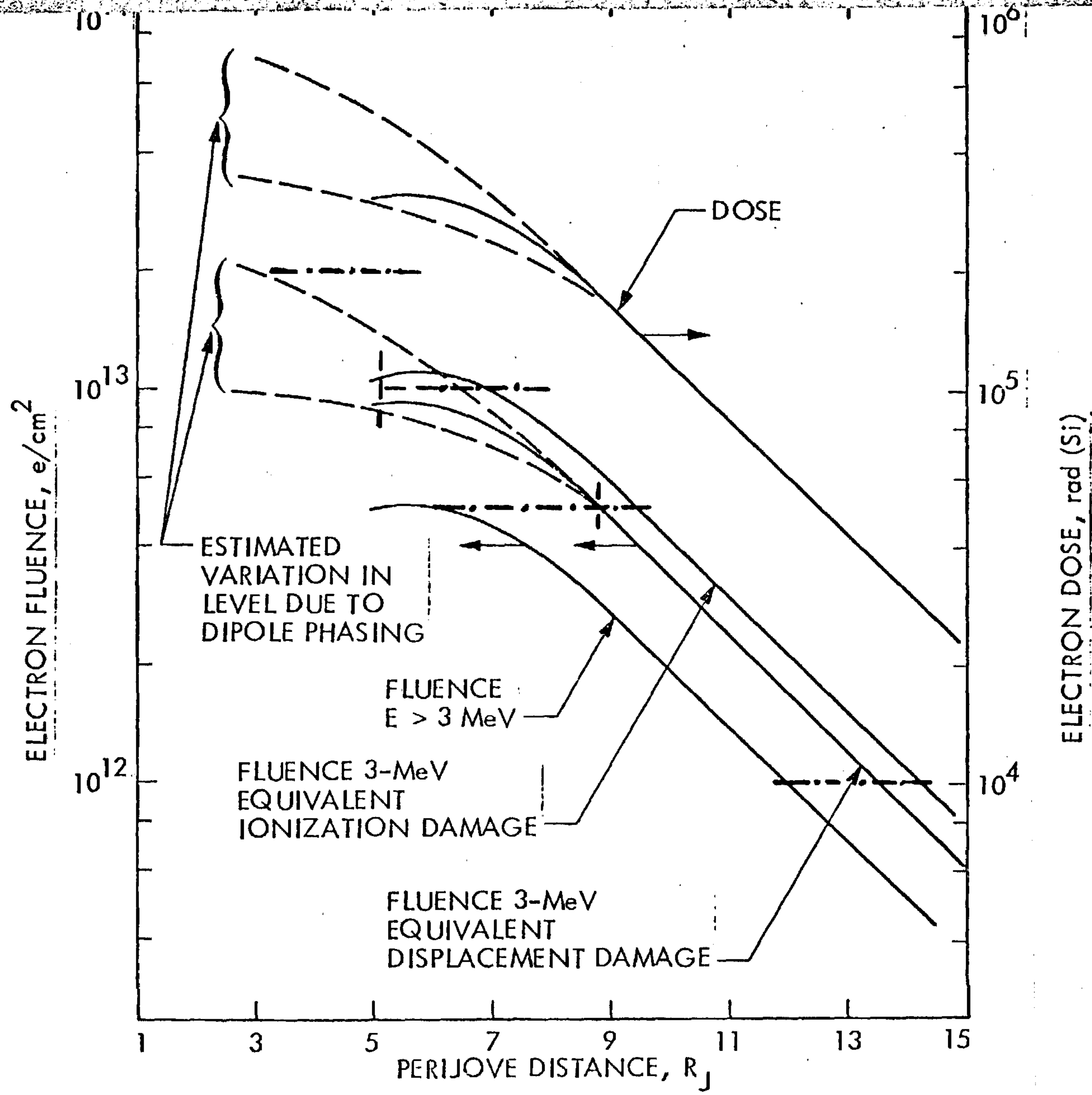
These uncertainty bars in Figure 9-36 simply have to do with whether the material is P-type or N-type silicon. There are other uncertainties that should be factored in but the important feature of this curve is its relative distribution. The slope and not the absolute amount is the important parameter at this stage; however, variation in slopes should be anticipated.

Note in Figure 9-36 that the low-energy contribution of the electrons has very little influence on the accumulation of displacement degradation. However, the low-energy ionization dose contribution has a sizable influence on ionization dose. Our problem, with sensitive electronics on MJS, is primarily an ionization problem.

For comparison purposes, the MJS '77 proton environment, if as large as expected, will not achieve as much ionization as expected with the electron environment as defined. However, the displacement from protons would be at least as much as the electron environment. As a result, the displacement problem may be twice as large which is still not as critical, from our understanding of the sensitive electronics, as is the ionization. Proton ionization at exposed surfaces are expected to be significant.

Figure 9-37 displays the results of folding the energy and dose equivalent degradation data (see Figure 9-36) into the spectra in Figure 9-35. The results include 3 Mev equivalent fluences, 3 Mev equivalent doses, and E>3 Mev fluences. A major feature displayed in Figure 9-37 includes phasing of the flyby with planet rotation and magnetic axes. For the current model no significant variations in phasing occur beyond about 6 R_J. Probe mission design, therefore, should consider this feature as significant and more detail study should be followed and correlated with Pioneer 11 data.

The curve of fluence with E greater than 3 Mev is constructed using data points taken from Figure 9-36 at the integral fluence points at E greater than 3 Mev. The fluence, curve of 3 Mev equi-



ELECTRON FLUENCE AND DOSE ACCUMULATION IN A POINT DETECTOR FOR FREEFIELD ENVIRONMENT AND FOR SELECTED TRAJECTORIES VERSUS PERIJOVE DISTANCE

FIGURE 9-37

valent ionization damage, is constructed using the ionization data normalized to a 3 Mev equivalent. The fluence, 3 Mev equivalent displacement damage, is constructed using the displacement data normalized to a 3 Mev equivalent. The difference in levels between these two curves, the one for ionization and the other for displacement, 3 Mev equivalent for comparable R_Js are essentially insignificant.

Furthermore, the total ionization dose is used to simulate the ionization radiation environment. Because ionization degradation can be effectively evaluated by assuming that the dose-damage concept applies, the influence of electron energies can be neglected within the first approximation. So the tests simply use the total dosage due to the spectrum taken at 3 Mev. With this assumption we can account for both ionization and displacement in the same test and as well provide test data as a function of R_J for mission design assessment.

Four fluence levels on Figure 9-37 are highlighted with dash-dot lines to indicate derived test levels. The levels, 2×10^{13}, 1×10^{13}, 5×10^{12}, and 1×10^{12} are the test levels used for our quick-look tests. An extention of the quick-look tests is planned for parts identified as significantly influenced by this test environment. The evaluation will be made: (1) to determine whether the parts are potentially usable, which means more radiation data as a function of critical parameters are required, and (2) to determine whether the parts will work in circuits having specific input/output characteristics.

Table 9-1 contains a tabulation of a set of qualified test results. The qualifiers are: (1) these are quick-look test results of limited measurements and interpretations; (2) degradation is rated slight, moderate and critical, and should be related to statements: about parameter changes as noted, e.g., slight: component/circuit operates within specification limits, application should be reviewed. Moderate: significant parameter shifts, one parameter out of specification, component/circuit

TABLE 9-1

TEST RESULTS

Components / Type		3 MEV e/cm^2		20 MEV	Comments
	1×10^{12}	5×10^{12}	2×10^{13}	3×10^{12}	
Integrated Circuits					
★DGM 111	Critical	Critical	Critical	Critical	All devices catastrophic failure if used in neg current drain mode
DG 125	Moderate	Critical	Critical	Critical	Same as above at 5×10^{12} and 2×10^{13}
★LM 108A	Moderate	Critical	Critical	Critical	Slew rate okay; gain severe degrad
HA 2520	Slight	Slight	Critical	Slight	Catastrophic failure at 2×10^{13} in gain and off-set and bias currents
HA 2700	Slight	Slight	Moderate	N.T.	
DAC-01	Slight	Slight	Slight	Slight	
AD-550	Slight	Slight	N/A	Slight	

TABLE 9-1

TEST RESULTS (CONT)

Components / Type	3 MEV e/cm^2			20 MEV	Comments
	1×10^{12}	5×10^{12}	2×10^{13}	3×10^{12}	
CMOS					
CD4012AD	Slight	Moderate	Critical	N/A	
CD4049AD	Slight	Moderate	Critical	N/A	
CD4014AD	Slight	N/A	N/A	N/A	Still analyzing results
★ CD4011AK	Moderate	Critical	Critical	Slight	
CD4061A	Slight	Critical	Critical	Critical	Some devices survived and operated within spec at above 2×10^{13} (670K rads)

Note: All CMOS devices have shown a significant dependence on date code with respect to their sensitivity to radiation. Later devices appear to be significantly softer. Under investigation by Sandia and RCA.

TABLE 9-1

TEST RESULTS (CONT)

Circuits / Types	10^7 e/cm²/sec	10^8 e/cm²/sec	10^9 e/cm²/sec	1×10^{12}	5×10^{13}	2×10^{13}	Comments
IRU Integrators	Slight	Slight/Mod	Critical	Slight	Moderate	N/A	
Power Shunt Regulators	Slight	Slight	Slight	Slight	Slight	N/A	
Power Under Voltage Det	Slight	Slight	Slight	Slight	Slight	N/A	Tested at 20 MEV to 5×10^{12} with only slight effects
FDS Master OSC	N/A	N/A	N/A	Slight	Slight	Signifi-cant	Failed at 2×10^{13} and then recovered 18 hrs. later; same results at 20 MEV
FDS Countdown Ckt	No results due to test equipment malfunction						

still operates, applications of component/circuit must be checked. Critical: two or more critical parameters out of specification, failure may be catastrophic, all applications must be reviewed, circuits utilizing components should be tested. These qualifiers are important because generally the worst-case measurement condition was followed.

The simulations were performed using a LINAC. It is used to produce the accumulated test fluence only, because it is a pulsed accelerator. Rate interference testing is not performed with a LINAC. All rate interference test data presented was accomplished using a continuous-wave DC machine (Dynamitron) producing electron energies between 2 to 3 Mev.

Test levels identified in Table 9-1 are 1×10^{12}, 5×10^{12}, 2×10^{13}. For some piece-parts a 20 Mev electron simulation of the spectra was performed to make sure that we didn't have a significant difference in the 20 Mev displacement compared with 3 Mev displacement. The displacement curve was larger at 20 than at 3 Mev, resulting in an equivalent amount about 2/3 of the equivalent amount at 3 Mev.

The starred entries include transistors which are low power and potentially low current usage devices. The 2N2484 was identified as critical at all fluences indicating a very sensitive part showing DC current gain out of spec at all levels. However, proper interpretation is required because the device was tested in a low-current mode, 10 microamps. When the device was operated at higher currents, then only moderate degradation occurred. Moderate degradation is, typically, acceptable within the gain change. Note that the degradation which occurred at low current is estimated to be practically all ionization degradation. The displacement degradation which occurred throughout but is dominant at the higher current level was not significant enough to fail the 2N2484. The same kind of appraisal applies to the other transistors (typically, these devices are general-purpose transistors). At the higher fluence levels, the critical parameters have moderate degradation.

Sensitive Integrated Circuits which are starred in Table 9-1 e.g., the analog switches, are devices which are tentatively identified as critical: these switches showed catastrophic failure when used in a negative current drain mode. That simply says that you can't turn the device on, so it can't be used in a bilateral switching mode.

The LM 108A is an operational amplifier whose characteristic offset voltage may be the critical parameter. It was identified as moderate degradation at 1×10^{12} and critical at the higher levels 5×10^{12}, to 2×10^{13}. The point made using LM 108 data is that there is a tremendous spread in the amount of degradation in that device for a given level. Therefore, applications in circuits, especially at 5×10^{12} and higher should be properly designed to accommodate radiation.

The CMOS devices, for example, the CD 4011 Dual Quad Nand Gate, essentially contains two P-channel and two N-channel type transistors. It was rated as moderately damaged at 1×10^{12} e/cm^2; but critically damaged at the higher levels ($\geq 5 \times 10^{12}$ e/cm^2, 3 Mev equivalent) as shown on Table 9-1. For 20 Mev electrons the damage assessment at 3×10^{12} which is assumed equivalent to the 3 Mev fluence of 5×10^{12} e/cm^2 indicated less degradation. Therefore, we assume the degradation to be dominated by ionization degradation.

The point in this assessment is that 4011's are ionization damage sensitive; and, as well, the range on degradation levels is wide and the degradation depends on part type, process and the manufacturer. There are a number of things that are being done to close-up the uncertainty range on the damage level as well as to harden the devices. Manufacturers, processes and controls are being reviewed and, as well, some of the available "hardened" CMOS is being evaluated.

Notice what happens to circuits which use these parts. The IRU integrator shown in Table 9-1 uses the LM 108 operational amplifier. The rate interference was slight to moderate at rates as high as 10^8 e/cm^2-s. For the MJS '77 mission, the rate is more like about 5×10^8 e/cm^2-s (see Figure 9-34). At this design rate there is a concern about rate interference. The detailed test data indicate that there is an adequate function at 5×10^8 e/cm^2-s. Only moderate damage to the IRU occurred at 5×10^{12}, which satisfies the MJS '77 design requirement. These quick-look test results help us identify those parts that are potentially too sensitive to the Jovian electrons, allow us to selectively generate characteristic performance data for the sensitive parts, and circuits that use the sensitive parts. In addition, the test results will be used to do radiation design analysis on the circuits.

The test results and design analysis will be used in spacecraft design trade-offs. Spacecraft design trade-offs include the use of inherent shielding, location and orientation of sensitive devices and, as well, the use of some additional shielding. Mission trade-offs, essentially, include selecting the perijove flyby distance and satellite positions most compatible with science objectives.

THERMAL CONTROL FOR PLANETARY PROBES

Dr. Robert McMordie
Martin Marietta Corporation

DR. MCMORDIE: Now, the area that I want to focus in on is the thermal control of the probes, and particularly the descent phase of the mission.

Notice that I will not be addressing the entry problem, rather strictly the descent problem.

Now, if you ask ten thermal control engineers to devise a chart describing the technique for the development of a thermal control subsystem, you would probably get ten different graphs, or charts. Figure 9-38 illustrates one of these approaches, and I think it is fairly representative. You are given temperature limits; equipment limits; constraints such as power, volume, weight; and the environments that your equipment must survive in. Then you perform analyses, starting with studies on your conceived design, and often you will need to perform some development tests to support your trade studies.

For a probe mission you might conceive of a design that has insulation on the exterior of a pressure vessel, or the interior of a pressure vessel, or a vented design. In the case where you have the exterior insulation, or a vented design, you need to know how the insulation performs in the environment. In the case of the planetary-probe mission, you need to know how the insulation performs when subjected to hydrogen/helium atmosphere.

Also, it appears there are some problems in defining the environments and, particularly, the wide variation in the temperatures that you might encounter.

In Figure 9-39 the nominal environments for a nominal descent into three planetary atmospheres are shown. The important point here is the wide variation in temperatures between the Jupiter and Uranus missions. This is not an overwhelming problem, but it certainly has to be considered by the thermal control designer.

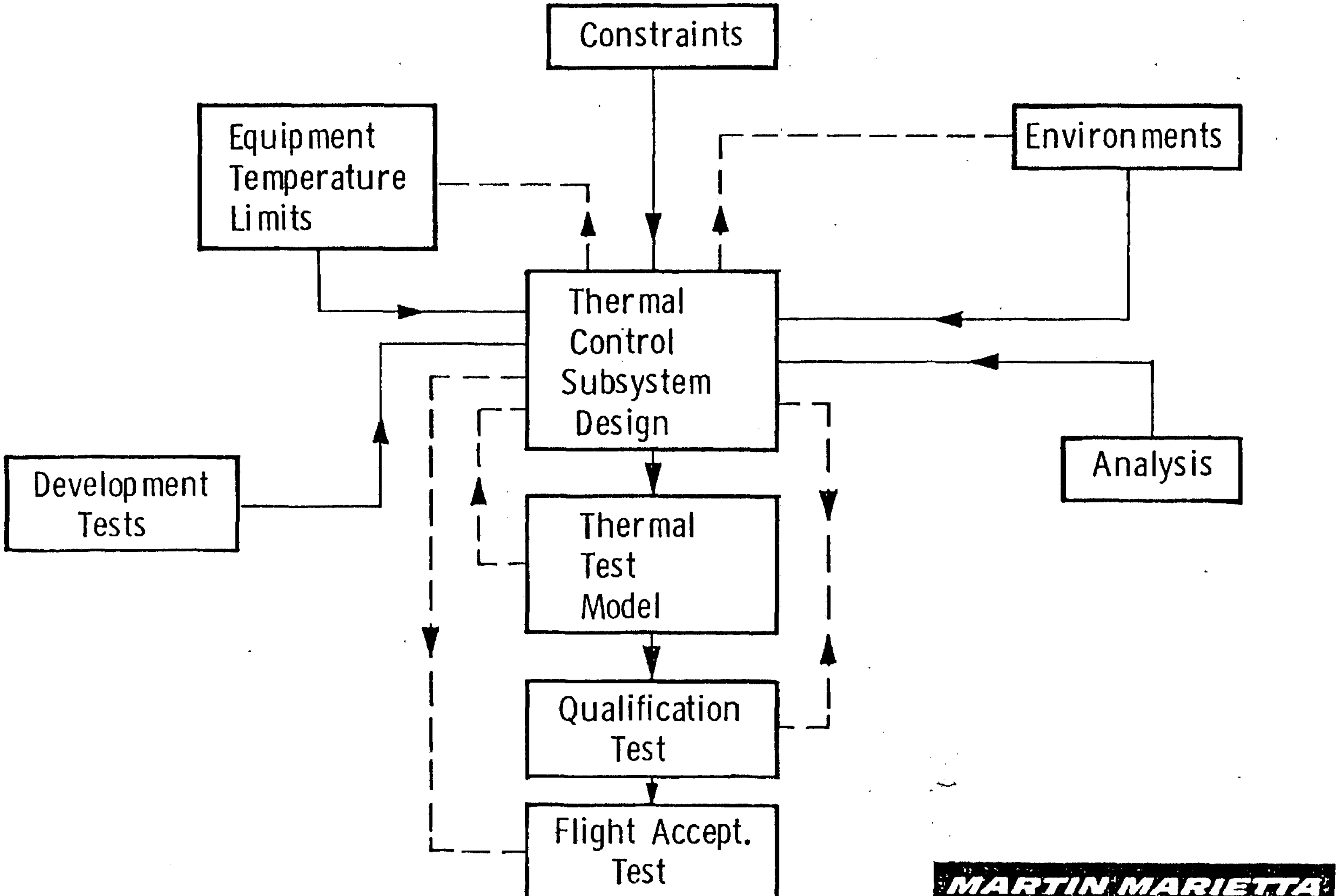

Constraints
Equipment Temperature Limits
Environments
Thermal Control Subsystem Design
Development Tests
Analysis
Thermal Test Model
Qualification Test
Flight Accept. Test
MARTIN MARIETTA

FIGURE 9-38

COMPARISON OF PLANETARY DESCENT TEMPERATURE PROFILES

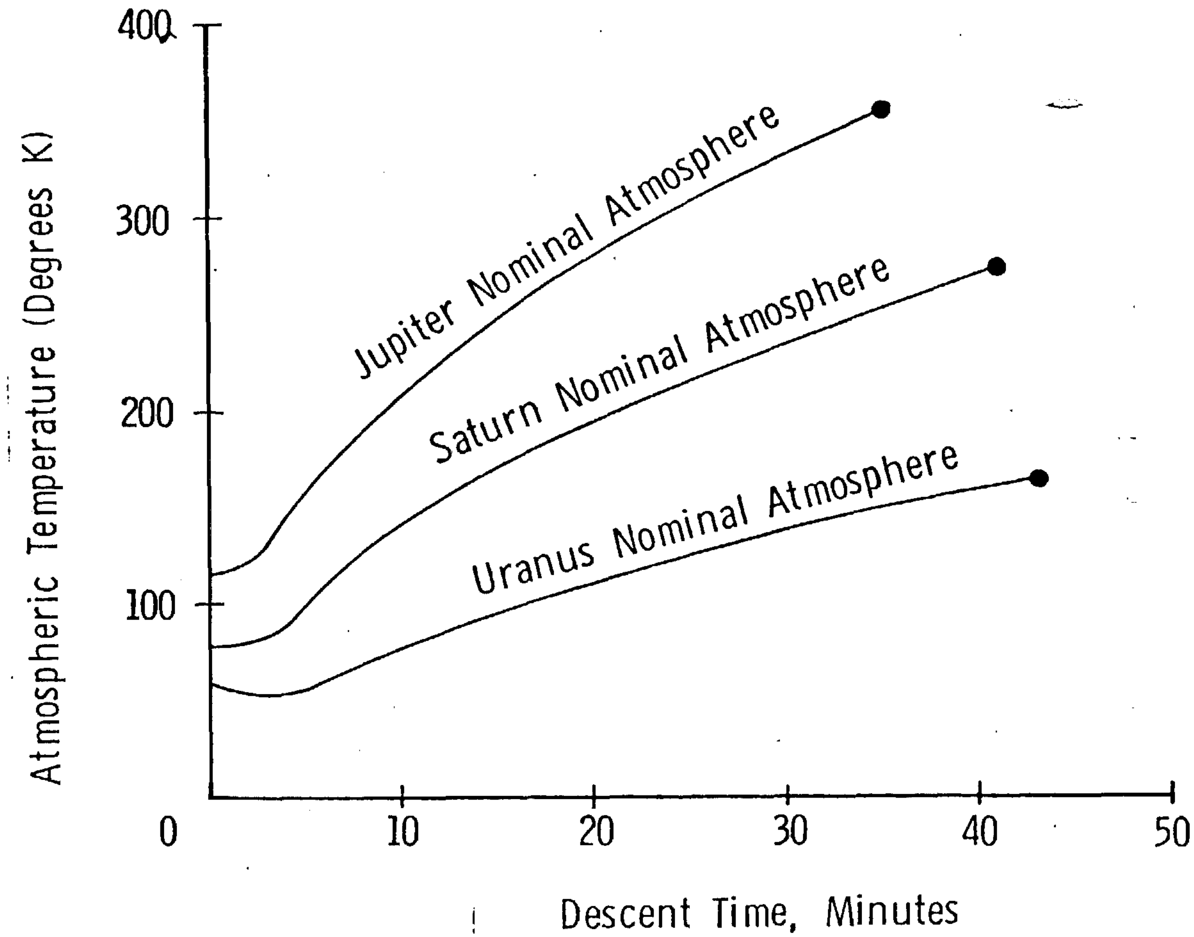


FIGURE 9-39

Figure 9-40 shows temperature vs. time plots for Uranus descents. Here the temperature difference between a cold and warm model atmosphere is as large (approximately 200°C) as it is between the Jupiter and Uranus nominal descents.

Figure 9-41 shows data for a Venus descent probe. The test article was a solid sphere that was insulated with a fibrous, porous insulation. The test article was placed in a chamber that was controlled to match the pressure and temperature versus time for a Venus descent. The analysis, with and without mass transfer considered, did not match the test data even though experimental valves of the insulation conductivity was used.

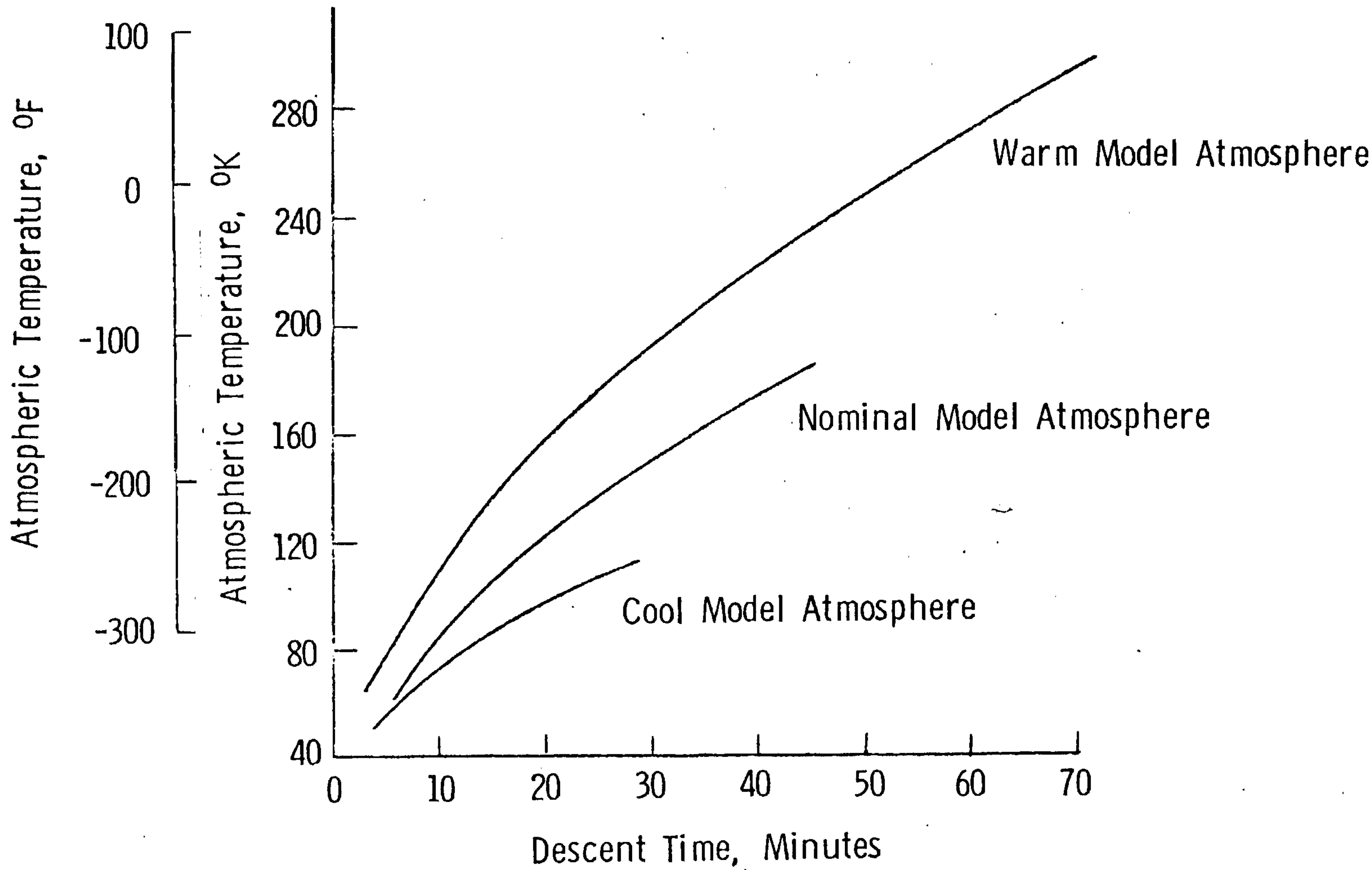
The thing that we discovered was that there were two reasons why our analytical model, using steady-state test data, did not allow us to predict the performance. One was that free convection actually took place within the insulation. This is something that you would never expect, or at least I would never have expected to take place. In an earth environment, with the type of insulation we are using, you would never have any free convection or actual mass movement within the insulation.

The second thing that occurred that we feel accounts for some of the differences is that during a descent, when the CO_2 is moving into the insulation, you get an absorption effect which represents an energy release that caused the difference between the tests and the analyses.

The whole point here, then, is that for a new environment, such as the hydrogen/helium that we will encounter in the outer planets, I think transient tests of candidate insulations should be performed. Then we can perform the trade studies, trading interior, exterior or vented designs and determine the optimum design.

Figure 9-42 is a logic diagram for a generalized descent probe program. This program can be used for any planetary descent

DESCENT PROFILES AT URANUS



MARTIN MARIETTA

FIGURE 9-40

DESCENT SIMULATION CORRELATIONS

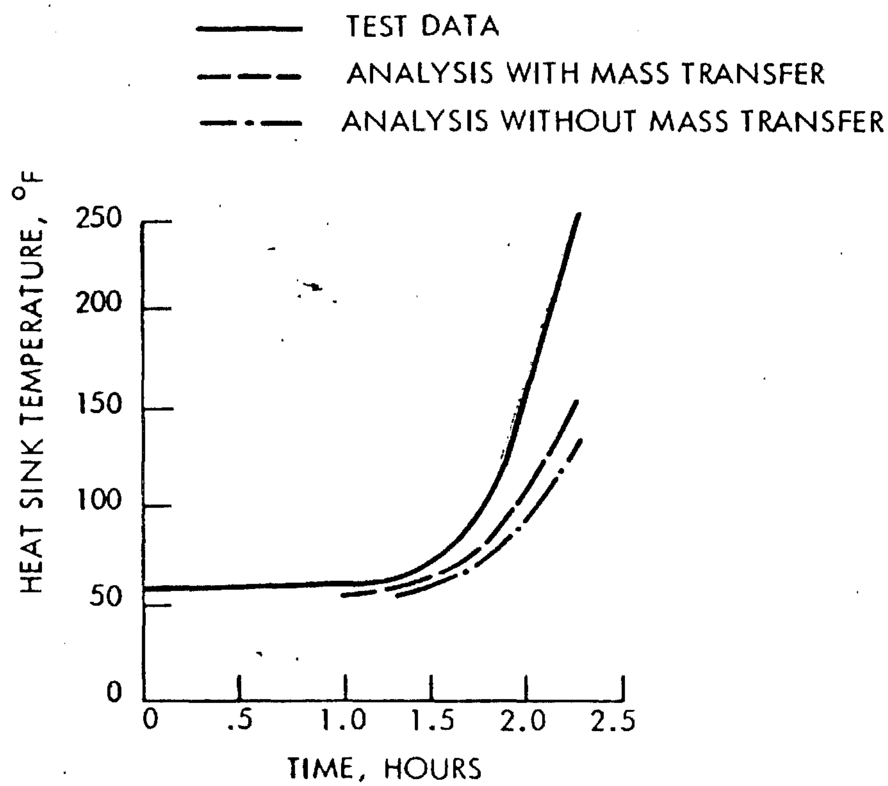


FIGURE 9-41

C-10

DESCENT PROBE THERMAL AND STRUCTURAL DESIGN PROGRAM

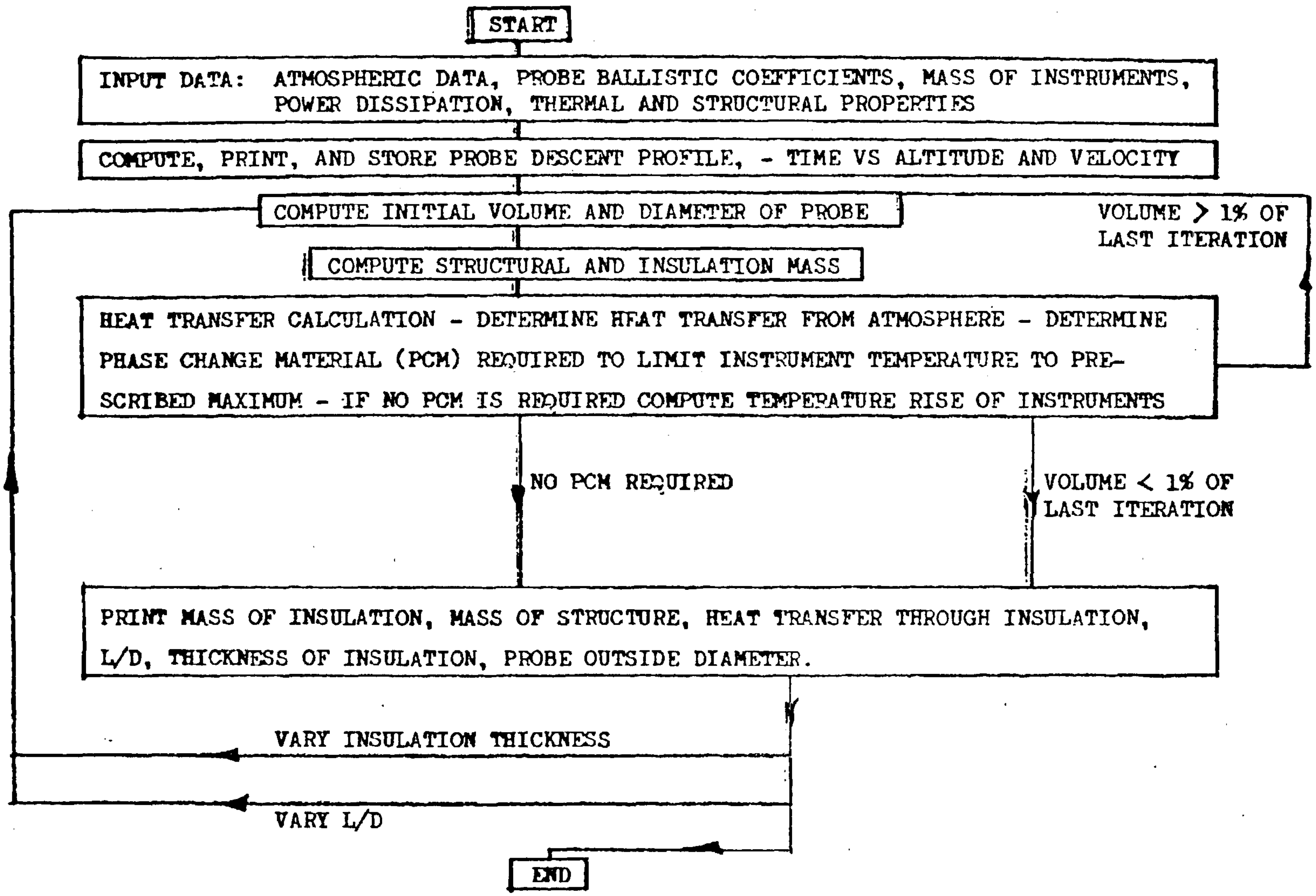


FIGURE 9-42

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and couples the structural and thermal aspects of the problem. At the same time, it performs weight calculations and analyzes the need for phase-change material, if needed. Aerodynamic equations are also used to compute the time-temperature and time-pressure profiles which would, in turn, define the environment for the probe.

In summary, this program provides a powerful tool to perform trade studies for planetary probes.

Figure 9-43 shows diagram of a test fixture that has been used to test an almost full-scale Pioneer-Venus large probe. The diameter of the test article was twenty-two inches. This facility was used to perform a test matching the Venus descent profile, both pressure and temperature in a CO_2 environment.

The problem areas relative to the thermal control of an outer planet descent probe are given in Figure 9-44. Relative to insulation performance, I would suggest that we perform transient tests on the candidate insulations in a helium/hydrogen atmosphere so that we can, in turn, perform trade studies, looking at various probe designs.

MR. TOMS: I think Bob McMordie made an important point about the atmospheric uncertainties. Particularly with the Uranus probe, the atmosphere definition needs to be refined if we are going to get a design we can live with.

If there are no more questions, I want to thank the speakers for being so well prepared and for giving us a good session this morning.

ATMOSPHERIC DESCENT TEST CHAMBER

MARTIN MARIETTA
DENVER DIVISION

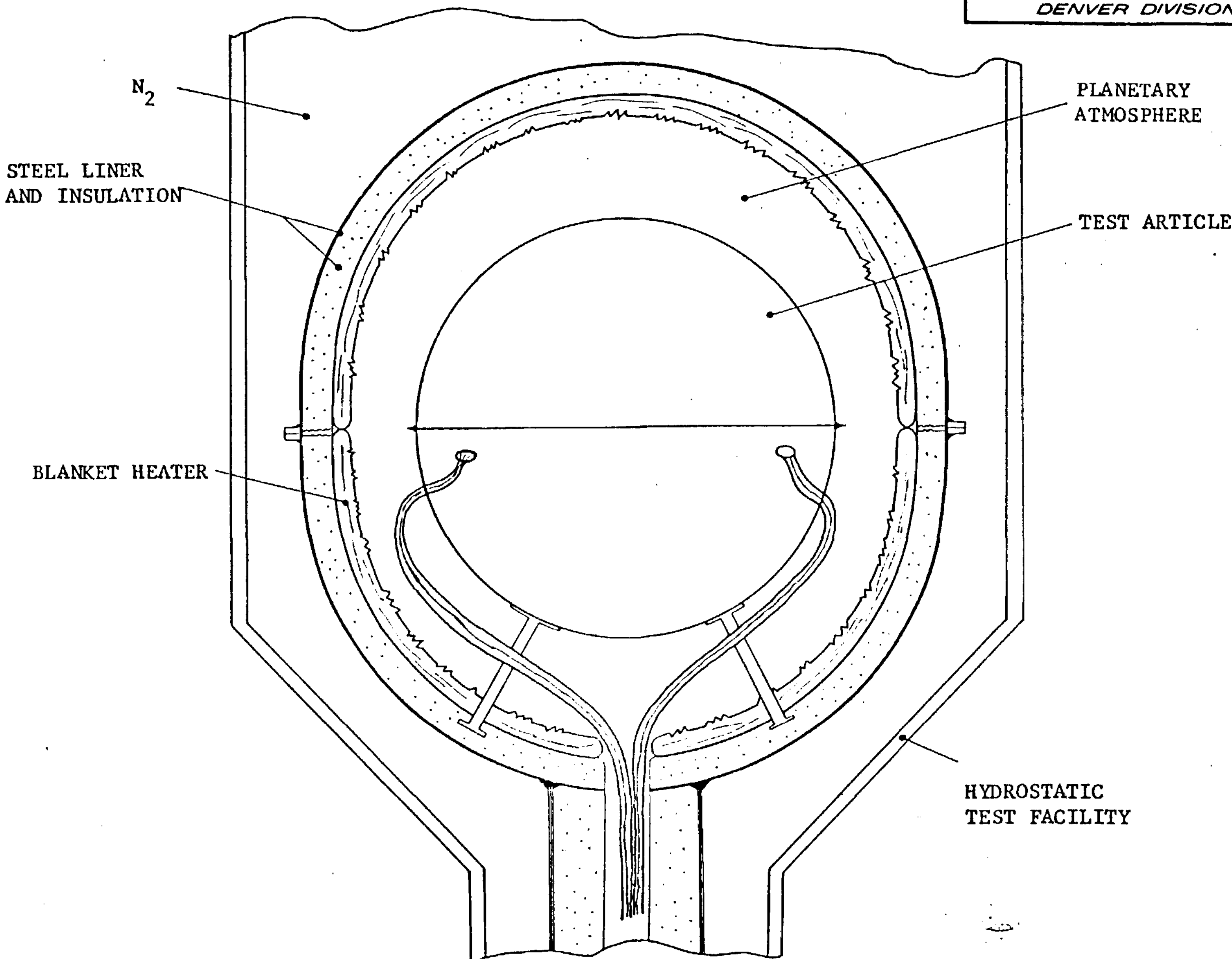

FIGURE 9-43

THERMAL CONTROL STATUS FOR PLANETARY PROBE MISSIONS

PROBLEM AREAS:

- o Atmospheric Uncertainties
- o Insulation Performance

FIGURE 9-44

MARTIN MARIETTA

SESSION X

MISSION COST ESTIMATION

Chairman: N. Vojvodich
NASA Ames Research Center

MR. VOJVODICH: I would like to welcome you to the last session which is, in many respects, probably one of the most important sessions because it deals with the question of cost. It is not necessary for me to remind you that because of NASA's constrained fiscal situation, technical feasibility, which has been discussed for the past two and a half days, is certainly necessary but, unfortunately, not a sufficient condition for us to undertake these missions. More than ever before we are going to have to do them in a cost-effective manner if they are going to be, in fact, accomplished.

Now, as many of you know, the art of cost estimation has evolved over the years to become a relatively sophisticated combination of analytical capability and what I call black art, or a certain amount of magician's quality to it.

We have three distinguished practioners here. Unfortunately, one of the practitioners, Steve Duscai of Martin Marietta, could not make it because he is home in Denver costing out a new proposal, actually working a problem from the standpoint of a cost estimator.

We have changed the order of speakers around. Instead of having Bill Ruhland of JPL speak third, he will speak second, and Fred Bradley from McDonnell-Douglas will speak third.

The first speaker that we have on the agenda is eminently qualified to address the question. He is John Niehoff, Senior Engineer with the responsibility of planetary program manager with Science Applications, Inc. He is in the process of working parametric cost estimates for many of the outer-planet mission options under contract to Dan Herman at NASA Headquarters.

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OUTER PLANET PROBE COST ESTIMATES - FIRST IMPRESSIONS

John Niehoff
Science Applications, Incorporated

MR. NIEHOFF: The purpose of this paper is to examine early estimates of outer planet atmospheric probe cost, evaluating these estimates by comparison with past planetary projects. Of particular interest is identification of project elements which are likely cost drivers for future probe missions. Discussion is divided into two parts: first, the description of a cost model developed by SAI for the Planetary Programs Office of NASA, and second, use of this model and its data base to evaluate estimates of probe costs. Several observations are offered in conclusion regarding the credibility of current estimates and specific areas of the outer planet probe concept most vulnerable to cost escalation.

Cost Model

A cost model has been developed by SAI for the Planetary Programs Offices as an estimating tool for long-range mission planning. The model is based on cost data from seven lunar and planetary unmanned spacecraft projects completed (or in progress) between the ten-year period 1964-1974. The model input requirements are matched to the level of mission definition available from pre-Phase A studies. The basic estimation parameter is direct labor hours. The labor estimating relationships (LER's) are primarily a function of subsystem weights due to the limited detail of pre-Phase A data.

At the present time the cost model can be applied to flyby, orbiter, atmospheric probe and soft lander mission concepts. Features include non-recurring and recurring division of cost, specified fiscal year dollars, project inheritance, and cost spreads of estimates. The model will reproduce the costs of the data base projects with a mean absolute error of 10%. The error goal for future program estimates is 20%. Initial test results, shown below, indicate that this accuracy is achievable. A detailed description of the cost model is given in Reference 1.

For the purpose of this paper it is instructive to take a closer look at the cost model data base, the method for translating labor hours into cost, and overall estimation accuracy. The roots of any cost estimation procedure are buried in its data base. The seven projects comprising the SAI cost model data base are listed in Table 10-1. The list includes almost all the lunar and planetary unmanned spacecraft flown between 1964 and 1974, as well as Viking which will be launched next year. With these data, it has been possible to construct a model capable of estimating flyby orbiter and soft lander mission costs. Atmospheric probes are also modeled using Viking entry system cost data, although this single project data point is considered tenuous and mismatched to smaller entry probe concepts for Venus and the outer planets.

TABLE 10-1

SAI COST MODEL DATA BASE

- Programs in Current Model
  - Mariner Mars '64
  - Surveyor
  - Lunar Orbiter
  - Mariner Mars '69
  - Mariner Mars '71 (FY '72 status)
  - Pioneer F/G (FY '72 status)
  - Viking '75 (FY '72 status)
- Programs Under Evaluation
  - Mariner Mars '71 (complete)
  - Viking '75 (FY '74 status)
  - Mariner Venus/Mercury (complete)
  - Mariner Jupiter/Saturn (FY '74 status)
- New Programs to be Added
  - Pioneer Venus '78

Also shown in Table 10-1 are programs currently under evaluation for updating and expanding the data base. The first two programs, Mariner Mars '71 and Viking '75, involve updates to estimated run-out costs for these programs in the original data base. The Mariner Venus/Mercury program is a new addition which not only will expand the data base, but is also proving useful for modeling inheritance savings. Mariner Jupiter/Saturn, a program just getting under way, will further expand the data base and permit refinement of model inheritance factors.

An important future addition to the cost model data base is the Pioneer Venus '78 project. Cost data from this program are of interest for the following reasons: (1) it is the first planetary program involving atmospheric probes, (2) it will be only the second program in the data base for spin-stabilized spacecraft, and (3) it is the first planetary program evolved under low cost (expanded weight) guidelines. The Pioneer Venus '78 data represent a significant improvement in the data base for estimating probe costs. The evaluation of current probe estimates (presented below) is only preliminary in nature as indicated by the title of this paper. Low cost (expanded weight) program philosophy, and its impact on cost modeling, will not be discussed further here. Although a potentially significant alteration to traditional estimating procedures, it is not immediately relevant to the subject of this paper and must be treated in detail to be properly understood.

Within, then, the cost model data base, manpower and dollar costs are broken down into elements of two basic categories: support categories and subsystem categories. The various elements within each category are itemized in Table 10-2. Elements within the support categories relate to project functions and non-flight hardware. Elements within the subsystem categories are flight hardware. Table 10-2 illustrates how data base project resources (dollars and manhours) are allocated across these elements. The data are averages of all seven projects in the data base.

TABLE 10-2

SAI COST MODEL ELEMENTS

(Comparison of Dollar* and Labor Hour Distributions**)

		Cost	Man Hours
• Support Categories			
o	Program Management	5.3%	5.4%
o	Systems Analysis/Sys. Eng.	4.0	4.3
o	Test	7.0	7.2
o	Quality Assurance & Reliability	4.7	5.3
o	Assembly & Integration	2.8	2.8
o	Ground Equipment	9.0	8.1
o	Launch/Flight Ops.	10.0	10.0
	Subtotal	42.8%	43.1%
• Subsystem Categories			
o	Structure	8.9	9.0
o	Propulsion	5.2	4.5
o	Guidance & Control	9.2	9.1
o	Communication	13.9	14.7
o	Power	4.1	4.7
o	Science	15.2	14.0
o	Miscellaneous	0.7	0.9
	Subtotal	57.2%	56.9%
	Total	100.0%	100.0%

*w/o fee
**all-project average percentages of totals

Several important observations should be noted from Table 10-2 relevant to NASA's planetary flight projects in general, and the cost modeling procedure in particular. Some subsystem category elements contain more subsystems than their names imply. Structure, for example, is actually a conglomeration of structure, mechanisms, landing gear (when applicable), thermal control, pyrotechnics and cabling. The reasons for combining subsystem hardware are two-fold. First, certain component costs are difficult to separate from available project financial records. Second, some hardware element costs can be modeled (with pre-Phase A definition) better in combination than separately. Note in Table 10-2 that less than 1% of the total project man hours and cost are unaccounted for (miscellaneous subsystem category element) using the described element breakdown.

Direct labor hours, while an intrinsic understandable unit of cost, is only part of a project's total cost. Material, burden, ancillary support, and fee make up the remainder of required project costs. Fortunately, due in part to NASA's rigid contracting requirements, direct labor hours consistently accounted for 30% of total costs within the seven-project data base. This result has a maximum deviation of less than 3%. The close comparisons between labor hour and dollar percentages, evident in Table 10-2 further illustrate that this is true at the project category level as well as on totals.

Finally, note that the subsystem categories, science and communications, are comparable in cost, and are the largest single cost elements in automated lunar and planetary projects. This point will be readdressed in the discussion of atmospheric probe cost estimates below.

A schematic diagram of the SAI Cost Model, illustrated in Figure 10-1 summarizes the cost estimation process. Subsystem direct labor hours are estimated, using the cost model LER's from mission definition input parameters. These estimates can be re-

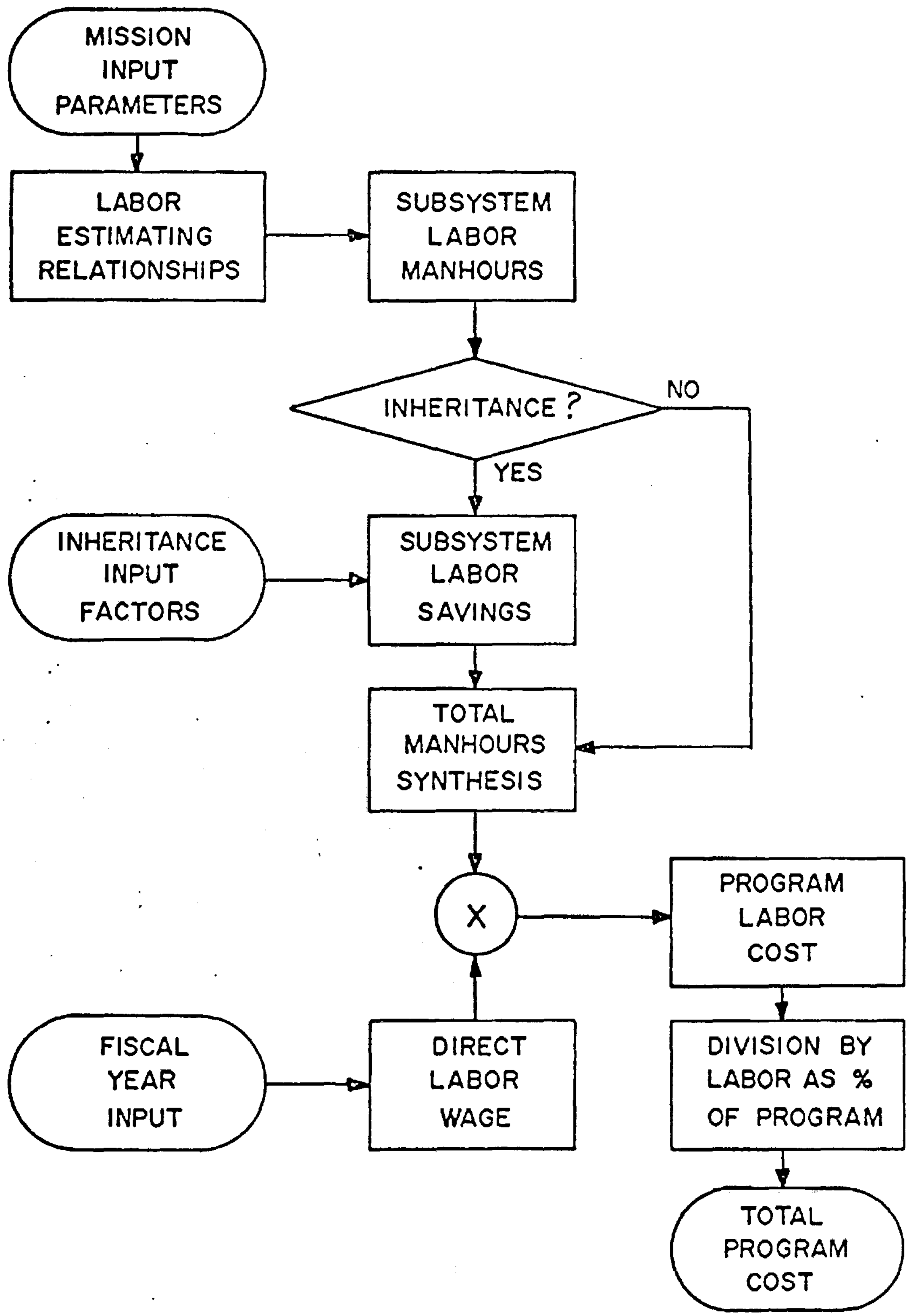


FIGURE 10-1 COST MODEL SCHEMATIC

duced if subsystem hardware inheritance from a previous project is applicable. Total direct labor man hours are synthesized from the subsystem labor estimate using additional LER's for the project support category elements. The total direct labor hours are converted to dollars by applying estimated labor wage rates for the fiscal year cost output of interest. It is only at this point the inflation factors are added to the estimate. The total program cost (less fee, NASA management, and contingencies) is computed by assuming that direct labor accounts for 30% of the total cost.

An accuracy of $\leq$10% error has been demonstrated[1] by the cost model in reproducing the project costs of the data base. This result involved the estimation of 88 individual cost elements. A statistical histogram of the 88 element errors is presented in Figure 10-2. Ideally one would like the density function to have a sharp spike entered around zero error and a relatively rapid tail-off such that the probability of exceeding 2 or 3 mean absolute errors is essentially zero. The actual distribution has a sharper peak (greater density) within one mean absolute error of zero than a Gaussian function, but the tail-off is slower than desired. Estimation errors associated with the Surveyor Project in the data base are mainly responsible for the negative bias in the distribution. The mean error and mean absolute error taken over the remaining six projects in the data base are only -$0.4M and $2.3M, respectively. The mean absolute error of all seven projects is just 10% of total cost.

An error goal of $\leq$20% on cost model estimates of projects not in the data base has been realized from limited applications to date. Some test comparisons with completed projects and independently estimated future projects are presented in Table 10-3. On this sample of six cases the maximum error estimate is under 12%. Note that the six projects vary considerably in mission concept, total dollar level, number of spacecraft, and period of performance. The results are indeed encouraging. The negative

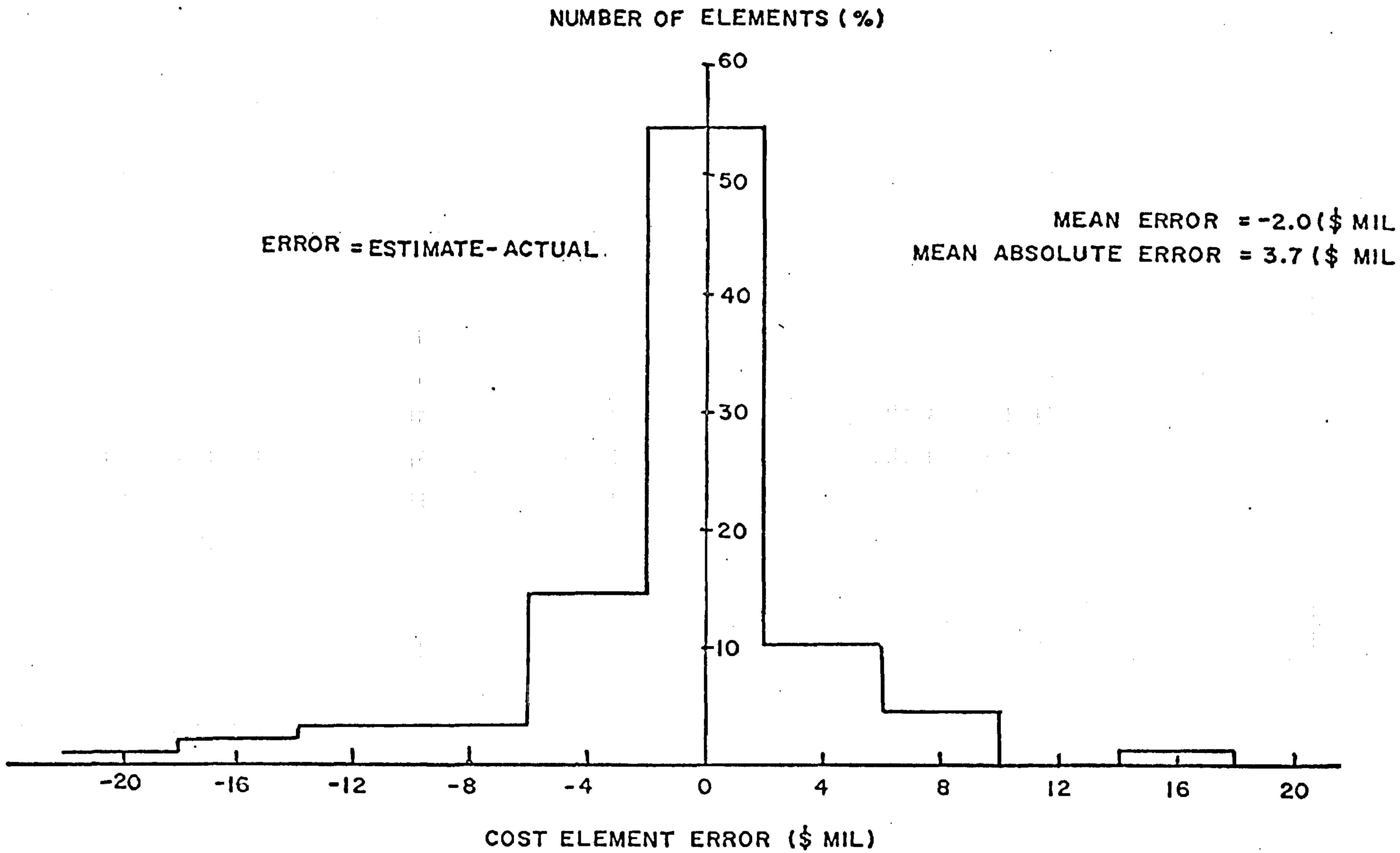


FIGURE 10-2. DISTRIBUTION OF ESTIMATION ERRORS FOR 88 COST ELEMENTS

TABLE 10-3

SOME COST MODEL TEST COMPARISONS

Missions	FY $	SAI Cost Estimate* ($M)	Comparison		% Difference
			Cost ($M)*	Basis	
Pioneer (A-E)	1965	55.8	58.7	Actual	-4.9
ATS (A-E)	1966	133.1	137.3	Actual	-3.1
Planetary Explorer Bus	1970	63.2	65.2	GSFC 3/71 Est.	-3.1
MJS-77	1972	187.4	210.0	JPL/SAG 2/72 Est.	-10.8
MVM-73	1973	93.2	94.1	Actual	-1.0
Mini-MSSR	1973	455.3	515.0	JPL 5/73 Est.	-11.6

*Excludes contractor fee, NASA mgmt., and contingencies bias in all the estimates, however, indicates some necessary refinement required in the estimating procedure.

Probe Cost Estimates

A certain degree of ambivalence exists with respect to planetary entry probe cost data. On the one hand, considerable data exists from earth reentry programs including test programs, military applications, and NASA manned projects. On the other hand, atmospheric entry is only one function of planetary entry probes; many of its systems and operations differ markedly from past reentry programs. To date the only planetary entry probe missions flown have been the Venera and Zond series launched by the U.S.S.R. Hence, despite the undeniable feasibility of planetary entry probes, there is little or no historical data directly applicable to the cost estimation of such probes. The situation is not altogether hopeless, however, and a start must eventually be made somewhere. The preliminary cost evaluation of outer planet entry probes which follows, is presented with these thoughts in mind.

Considerable Phase A level analysis has been performed in the last several years on the definition of a first-generation outer planet entry probe concept. This effort includes several contractor studies as well as NASA in-house work at both JPL and ARC. For practical as well as programmatic reasons, the options have been narrowed to a Saturn-Uranus common probe design capable of atmospheric penetration to at least 10 bars. The cost of three flight articles and one spare is currently estimated at $40M (FY'74 dollars). This estimate is sufficiently detailed to be compared with the cost model described above. Such a comparison should highlight similarities and differences in cost between future planetary probe missions and past automated lunar and planetary spacecraft experience. It should also contribute to the process of firming up the cost estimate of this outer planet probe concept.

A category element comparison of cost between the Probe Study Estimate, PSE, and the SAI Cost Model data base (presented in

Table 10-2) is illustrated in Table 10-4. The clear bars are PSE cost percentages and the hatched bars are data base cost percentages. It is apparent from a comparison of the individual bar sets that the probe support category costs are less (by %), and the probe subsystem category costs are more, than the averages from the cost model data base. The ratio of subsystem/support cost for the PSE is 2.6, whereas the data base indicates a more equal distribution of 1.3. This difference is probably due more to the fact that the PSE is only part of the cost of a complete probe mission than to any intrinsic difference between the construction of entry probes and spacecraft. Adding the probe carrier bus estimate, and non-probe launch and flight operations costs should bring the subsystem/support ratio for the complete project in line with the data base.

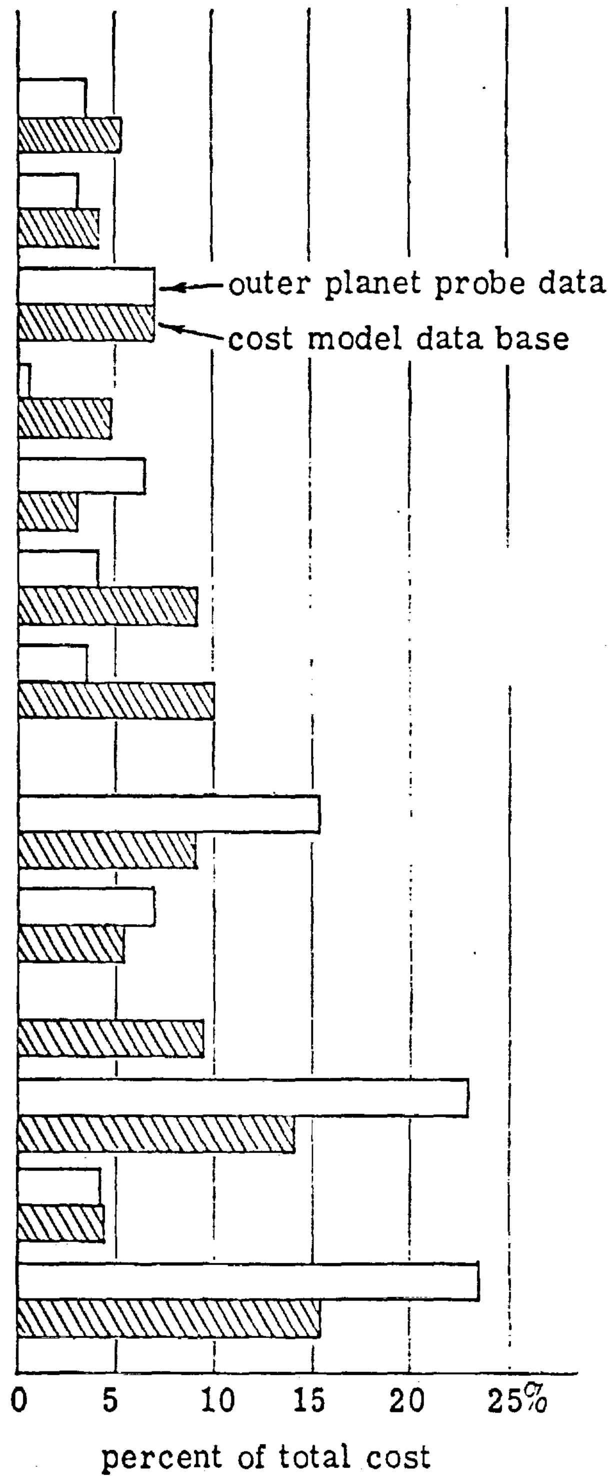
There are, however, some real differences in cost distribution within the subsystem category elements. Since the outer planet probe concept is a passively stabilized device guided by the carrier bus no costs appear for guidance and control. However, significant instrument and electronics packaging constraints must be imposed to insure stability during entry and descent. Packaging costs, precipitated by stability control, show up in the structure element and, indeed, increase the structure cost percentage above the average data base value.

Two other subsystem elements are also considerably above the data base averages - science and communications. The differences are reconcilable if one accepts the notion that these subsystem elements are more dependent in definition and cost on mission objectives than on the specific mission mode (flyby, orbiter, probe or lander). In particular, there is no reason to believe the cost of science and communications for probes should be any less than non-imaging science and communications of a flyby spacecraft. Since the total PSE is less than the cost of, say, a Pioneer flyby mission to Jupiter, the science and communication cost percentages for the probe will, therefore, be higher even considering the

TABLE 10-4

COST DISTRIBUTION COMPARISON

- Support Categories
  - o Program Management
  - o Systems Analysis and Engineering
  - o Test
  - o Quality Assurance and Reliability
  - o Assembly and Integration
  - o Ground Support Equipment
  - o Launch and Flight Operations
- Subsystem Categories
  - o Structure
  - o Propulsion and Aerodeceleration
  - o Guidance and Control
  - o Communication
  - o Power
  - o Science



- Subsystem/Support Ratio
  - o Outer Planet 10-Bar Probe ______ 2.6
  - o SAI Cost Model Data Base ______ 1.3

additional cost of imaging on the Pioneer spacecraft. Hence, where these two subsystems were seen to be the largest cost elements in the cost model data base, they become even stronger cost drivers of atmospheric entry probe costs.

As a second point of this assessment, the cost of the 10-bar outer planet entry probe was reestimated using the SAI Cost Model. The same assumptions of three flight articles and one spare, and FY '74 dollars were used in making the estimate. Applying the cost model without modification yielded a first estimate of $64.9M compared to the PSE of $40M. After examining the estimates of the individual subsystem elements, it was found that the costs for the aero deceleration and power subsystems were too high for the probe concept. The aero deceleration system LER was based on only one data point, that being the much larger Viking lander aeroshell. The power system LER was developed from data which always included solar arrays or RTG's. The probe, of course, only has a battery power source. Adjustments to these two LER's yielded a lower second estimate of $58.8M.

One more necessary change was found in a further review of this second estimate. The cost model assumes that what it's estimating is a complete program, which, of course, is not true for the probes. As a result of the costs for ground equipment and launch and flight operations charged to the probes was unrealistically high. Modifying the ground support equipment and operations cost to match the requirements of the probes part of a total flight project, yielded a third and final estimate of $48.0M. A comparison of this estimate with the PSE is presented in Table 10-5. The agreement between the two estimates on a percentage basis is quite good. The SAI cost model estimate, however, is 20% higher than the PSE on a total dollar basis. In view of the paucity of actual probe cost data available, it seemed prudent to conclude the comparison and estimation exercise at this point.

TABLE 10-5

PROBE DATA/COST MODEL COMPARISON

	Probe Data	Cost Model
• Total Cost for Three Probes	$40M	$48M
• Distribution of Cost*		
o Management/Design	6.3%	7.5%
o Science Instruments	23.4	23.3
o Probe	63.0	62.9
o GSE and Operations	7.3	6.3
Total	100.0%	100.0%

*Percent of Total

Summary

The most important point to be stressed, is the lack of any directly applicable data base with which to compare present cost estimates of the 10-bar outer planet common probe design. There are similarities with past projects on a subsystem level, and the Pioneer Venus Probe mission, just getting started, should provide relevant cost data in the near future. But for the present, the estimation and validation process of outer planet probe costs is in an embryonic stage.

Still, the similarity between two estimates presented here is encouraging. Based on the available definition of the probe design with the SAG recommended baseline payload, a reasonable preliminary estimate of the probe cost for three closely spaced missions is \$50M $\pm$ 10M (FY '74 dollars).

This investigation of outer planet probe costs has also brought out several interesting points relevant to the continued development of the present 10-bar common design concept. Using the carrier bus for targeting the probe to the correct entry conditions largely eliminates the cost of guidance and control, traditionally 9% of a total project. The savings, however, is largely offset by the difficult packaging of instruments, batteries and electronics in the probe for atmospheric stability. The two most costly subsystems of the probe are science and communications. This has been true in past lunar and planetary automated missions, and appears to be even more apparent in the probe cost estimates. There has already been discussion in this Workshop about expanding the capability of the probe's science and communications. In pursuing those suggestions, one should recognize that these may well be the cost drivers of probe missions. Finally, the cost of the aero deceleration system seems quite reasonable, provided, of course, that entry conditions remain within the bounds of current and near-future laboratory simulation test facilities.

In conclusion, the concern over the lack of an adequate data base from which to evaluate probe cost estimates is restated. The necessary alternative is to closely monitor the developing definition of outer planet probes, so that significant excursions in cost from present estimates are immediately identified.

References

1. Pekar, P.P., Friedlander, A. L., and Roberts, D. L., "Manpower/ Cost Estimation Model for Automated Planetary Projects," Science Applications, Inc., August 1973.

MR. HERMAN: One comment with regard to why the cost model is useful to you and why we need this kind of study.

The Space Science Board is holding is a summer study to assess what can be done in the next five or ten years and to recommend to NASA the optimum series of programs which yield the greatest degree of science value per dollar. In order to provide meaningful data to the summer study we have to have estimates of the programs that are in a relatively nebulous state. Some of the studies conducted were only Phase A, and some were not even Phase A studies.

In order to define the important costs per fiscal year, the nature of the summer study, by the way, is such that the Space Science Board is going to look at several funding levels for the Office of Space Sciences and on the basis of the various funding levels, determine towards what series of programs we should provide assistance in our planning. On that basis, the closer our estimates come to the actual cost of the program, the less problems we will have when we have to fight for the new program with the Office of Management and Budget and the Congress. So it is a rough job that we have and the data are being used for that purpose. It is not just an endeavor to see how close we can come to making a profit.

The other point I wanted to make, the thing that bothered me about John's model is the fact that the Pioneer-Venus philosophy is not factored to date. That is, you must rigorously constrain your payload and yet allow yourself plenty of weight and volume, but use the weight and volume margins to bail yourself out of trouble rather than a million dollars, as is the case with Viking. That experience does not seem to be factored into your particular model.

MR. VOJVODICH: Do you want to comment on that, John?

MR. NIEHOFF: Yes. Dan and I have talked about modeling "low-cost" projects at some length. This is one reason why we are very anxious to see the Pioneer-Venus project cost data. We feel that by comparing PV '78 data against our existing data base, we can determine to what extent low-cost expanded-weight concepts really work. We do, indeed, expect to see differences in the Pioneer-Venus data if money is being saved by removing weight constraints.

MR. CANNING: Do you plan also to add as available on missions the planning for the space shuttle, which, presumably, is on the same basis of unlimited weight?

MR. NEIHOFF: Yes. As Dan Herman implied, one of the criticisms of the current model is that it is embedded in history and does not reflect many new cost-saving ideas, particularly those motivated by the space shuttle. We are very anxious to incorporate data that is designed for shuttle launches. I am also anxious to see how significant proposed cost savings will be with the Space Transportation System.

MR. GEORGIEV: John, on the cost data that comes from the ten-bar studies and in comparison to your cost model, are there any particular elements of the cost that are significantly farther out of bed than the twenty-percent differential that you show? Are there any particular elements of the costing system that are much different?

MR. NIEHOFF: Yes. The cost model estimate almost exactly replicated the subsystem costs, but more than doubled the estimate of support category costs. The largest dollar difference was in the estimate of assembly, integration, test and quality assurance - $6.2M. We were unable, however, to determine whether this was a real difference or largely due to differences in bookkeeping cost allocations. You will recall that the percentage comparison between the two estimates presented in Table 10-5 showed much better agreement than this using a coarser distribution of costs.

MR. SWENSON: Is the data handling system lumped into science or communications?

MR. NIEHOFF: Communications. It includes transmitter/receiver assembly, data handling, storage, and antenna assemblies.

MR. HERMAN: I was going to say that the SAI results suggest that in programs where we are going to use these payload effects maybe a better variable than weight would be science weight since no data is derived from communications inherently. Actually, that was a suggestion made by SAI.

MR. NIEHOFF: That is right. At the present time, the communication system is based only on communication weight parameters and evidence exists that science weight has an impact on the cost of the communications system.

MR. HYDE: John, would you care to speculate, with regard to forty-eight-million-dollar figure that you have shown up there, if we had to incorporate the capability of the capsule deflection maneuver and also sterilization?

MR. NIEHOFF: We saw some numbers earlier, by Bob DeFrees of McDonnell-Douglas, on sterilization which I think were on the order of eight million dollars, and we do not have sterilization in this estimate. We have looked at sterilization costs in other programs and the $8M figure compares favorably with those results.

As far as the probe deflection goes, the cost model does have an estimating relationship for guidance and control. There is no money in that category in our estimate, since the ten-bar probe is a passive device. I really have no idea what the additional cost would be since I haven't seen any proposed hardware for intrinsic probe guidance. A rough guess would be about the same percentage of the total cost as reflected in the cost model data base for this subsystem element. From Table 10-2 that percentage is 9.1% which would raise the cost by $4.8M to $52.8M.

We are talking about putting deltas on an estimate that I have said is very preliminary. I think we have a forty-million-dollar estimate and a forty-eight-million-dollar estimate at the present time, but the data base is so small that I don't believe these kinds of extrapolation are realistic.

MR. VOJVODICH: I would like to reflect on what Dan Herman said, too. Although we are talking about pre-project or phase zero type cost estimates, as you know, the planning cycle is one in which we frequently get locked into a number that we have to live with based on these types of numbers. So it is important that the data reflect as much reality as possible.

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COST MODELING TECHNIQUES FOR DESIGN MATURITY

E. W. Ruhland
Jet Propulsion Laboratory

MR. RUHLAND: It is a pleasure to be here and not have something controversial to talk about, because I didn't have anything to do with generating any numbers here. So I'm just talking philosophically.

The first point I'd like to make is I'm in a very delightful position at JPL: I'm never right and I'm never wrong, because before a project comes in, my estimate is always too high; once it's through the door, it's too low. But, then, I'm never wrong because they never do the project that was estimated.

As a matter of information, Figure 10-3 shows some things that we have available at JPL. I'd like to say that they're only available to the government. They are not available to contractors; maybe they are lucky.

The first one is a model on re-entry heatshields, aerodynamic decelerators, and the integration problems particular to that. The model only works with the second model shown on the figure and I would like to point out the date, 1970, which makes it old. I would also like to point out the development of this model was funded by Dan Herman, as a matter of fact, in one of his studies, and we did a grand total of two man-months of effort on it and now use it as a guide for trade-offs.

In general, I want to talk about what drives subsystem costs and how maturity affects it (c.f. Figure 10-4). Basically, given a technology base, the cost is driven by the number of interfacing subsystems. The more interfacing subsystems you have, the higher the price tends to go. Subsystem costs are also driven by: the design and software maturity and I am using "maturity" the way some people might use inheritance; the test effort; and changes in the interfacing subsystems. And, cost avoidance items are hardware

RE-ENTRY VEHICLE COST ESTIMATING

AVAILABLE REPORTS AT JPL

- RE-ENTRY SUBSYSTEM COSTS (REF. 1) INCLUDE
  - HEAT SHIELD
  - AERODYNAMIC DECELERATOR
  - VEHICLE INTEGRATION (R/V TO S/C)
- TO BE USED IN CONJUNCTION WITH BASIC COST MODEL (REF. 2)

REFERENCE

1. F. E. Hoffman, "Cost Prediction Model for Unmanned Space Exploration Missions -- Entry Structure Cost Parameters," R-1434, dated April 24, 1970.
2. F. E. Hoffman, et al., Cost Prediction Model for Unmanned Space Exploration Missions, PRC R-1298, dated December 15, 1969

Figure 10-3

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COST MODELING TECHNIQUES FOR DESIGN MATURITY

SUBSYSTEM COST

GIVEN A TECHNOLOGY BASE

SUBSYSTEM COST DRIVEN BY:

- -NUMBER OF INTERFACING SUBSYSTEMS
- -DESIGN MATURITY
- -SOFTWARE MATURITY
- -TEST EFFORT
- -CHANGES IN INTERFACING SUBSYSTEMS
- -HARDWARE AVAILABILITY
- -SOFTWARE AVAILABILITY
- -SUPPORT EQUIPMENT AVAILABILITY
- -LEVEL OF DOCUMENTATION

COST AVOIDANCE

Figure 10-4

WER

availability, software availability, and support equipment availability. These are the true inheritors. Finally, the level of documentation has a cost impact. For example, we can track in Viking some of the effects of the level of documentation. All these things have to be considered if we are ever going to have a low-cost approach in doing business.

At the laboratory, we have developed internal to the cost-estimating people - we tried to hide this from the JPL'ers so I hope they are not taking notes - a maturity index as shown on Figure 10-5. The basic concept of the maturity index is to bracket the level of the subsystem, its status. It begins at the highest index represented by existing, qualified hardware, or that which is in active production, i.e. you are going to do the same thing the same way. It proceeds then, the next step down, to either a modification of the hardware or which you have to qualify because there is a new environment that's different in some way, or you have to replicate the qualified design. We find when we analyze the cost data that if you can't get onto an existing line you can't achieve the inheritance that you would like.

Next, down the maturity index scale is extending the subsystem capability using qualified piece parts. For example, making a bigger computer out of the same piece parts. Or either of the items from the index above, where you have to qualify the modification or extend the time. As you spread out in time, you pick up more cost and this starts getting subjective. There is no question; trying to cost maturity or to take into account maturity requires subjectivity, it requires a great deal of understanding of what you are trying to do, and it takes a great deal of open-channel communication with the technical and project people to keep you informed of the actions and status.

The lower end of the maturity index is zero, where we have never done the subsystem before, new technology is required and we bracket to that level.

COST MODELING TECHNIQUES FOR DESIGN MATURITY

DESIGN MATURITY (HARDWARE)

SUBSYSTEM STATUS

MATURITY INDEX (MI)					
100	EXISTING QUALIFIED HARDWARE OR ACTIVE PRODUCTION (A)				
.75	MODIFICATION OF A (B)		A, BUT REQUIRING QUALIFICATION	OR	REPLICATE QUALIFIED DESIGN (C)
.50	EXTENDED CAPABILITY USING QUALIFIED PIECE PARTS	OR	(B) OR (C) REQUIRING QUALIFICATION	OR	WITH TIME
.25	KNOWN TECHNOLOGY	OR	MI=50 WITH TIME		
0	NEVER BEEN DONE BEFORE/NEW TECHNOLOGY				

Figure 10-5

WER

Figure 10-6 shows the effect of maturity on cost. One of the major points is that design changes flow toward the less mature subsystem. This is not really a continuous curve, it is a continuous curve for purposes of modeling. Actually, there are great pressures not to change the design if you have a high maturity level. There are great pressures at the very low levels of maturity (and high cost amplifier levels) to force the design toward higher maturity indices, so you tend to have a cost amplifier that goes up from the lower right and it sort of settles toward the middle. The most important point is that the inflection point tends to move with the changes in the interfacing subsystems. For example, if you have an existing computer but you are changing everything around it you are going to have to spend more money on the computer anyway.

This is the basic approach, philosophically. We have developed the CER's on this and we have tested it and it seems to be working fairly well.

The second most important cost driver that we tried to model is the test effort as shown on Figure 10-7. The test program is structured by the mission and the design complexity, the mission and program risk avoidance (or acceptance) and by design maturity of the system and subsystems. Someone, at some level, has to say, "I will accept less testing and more risk to save cost." I can verify for example that, with time, at JPL we have been willing to do less testing on the Mariner machine because we understand it better. The designers understand it better. The general structure of a test program tends to be directed towards detecting design and fabrication defects at each level. You test at the vehicle level, the system, subsystem, assembly, and at the piece part level with a minimum of some kind of screening. And accepting, or neglecting testing at any one of those levels is a major cost driver. It is a programmatic decision, a risk.

COST MODELING TECHNIQUES FOR DESIGN MATURITY

EFFECT OF DESIGN MATURITY

- CHANGES FLOW TOWARDS LESS MATURE SUBSYSTEMS
- ABILITY TO RETAIN DESIGN DEPENDS ON MATURITY OF INTERFACING SUBSYSTEMS
- COST AMPLIFIER RELATIVE TO MATURITY

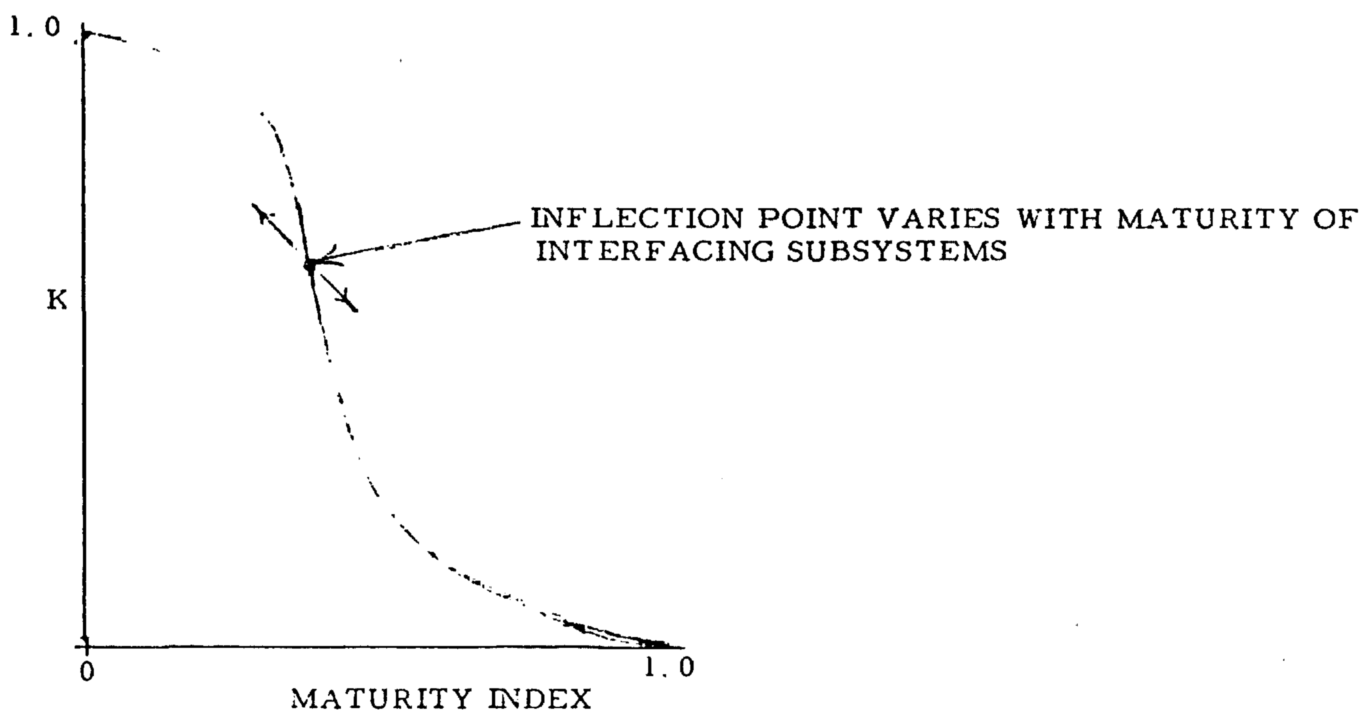


Figure 10-6

COST MODELING TECHNIQUES FOR DESIGN MATURITY

TEST EFFORT

O TEST PROGRAM STRUCTURED BY:

- -MISSION AND DESIGN COMPLEXITY
- -MISSION AND PROGRAM RISK AVOIDANCE (OR ACCEPTANCE)
- -DESIGN MATURITY

O GENERAL STRUCTURE TESTS DIRECTED TOWARDS DETECTING DESIGN AND FABRICATION DEFECTS AT EACH LEVEL

- -VEHICLE
- -SYSTEM
- -SUBSYSTEM
- -ASSEMBLE
- -PIECE PART (SCREENING)

Figure 10-7

WER

We have also developed a CER on which we have been working with regard to the effect of test level on cost. I show a sanitized picture of this on Figure 10-8. Basically, it results from the following things: As the number of test levels increases, the number of interfaces, the number of tests and support equipment increases. That is a direct cost driver. The impact is directly dependent on design maturity and, in general, it tends to look like the two curves on the figure. As the number of tests and test levels increase, the cost amplifier goes up exponentially because you pick up increasing integration costs, increasing support equipment costs, etc. Design maturity, however, can lower the cost amplifier as shown. But also you must not forget that with maturity the number of tests also comes down. You can't forget that this tendency to push down is directly affected by constraints and risks in management. To lower the cost for example by removing subsystem people from the project before you complete system testing, you are accepting a risk. If you don't want to accept the risk, you have got to expend the money necessary to continue to support the subsystem people.

That is really all I had to go over in a general presentation. I didn't realize this was to be an open meeting and I had prepared a presentation containing numbers that I am unable to release to an open session.

MR. VOJVODICH: Thank you very much, Bill. Do we have any questions?

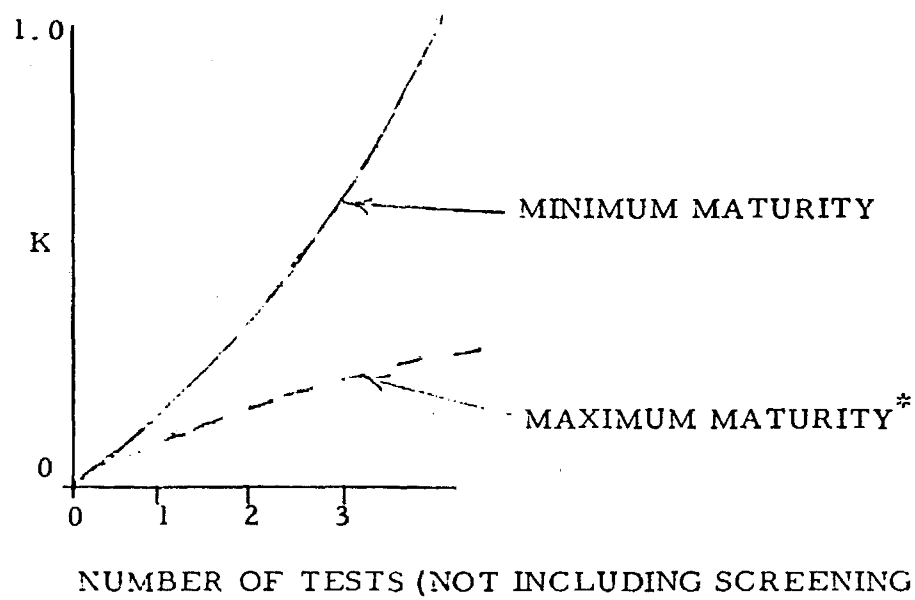
MR. CASANI: Yes, Let's see, Bill, you made a comment on the reduced level of testing you have experienced at JPL. Could you be more specific? We have looked at the series of Mariner programs. Is the percentage of total dollars that is being spent on testing decreasing?

MR. RUHLAND: The percentage has not been decreasing because as the design goes down, the testing goes down somewhat in parallel. So you might say that it's tending to stay a constant

COST MODELING TECHNIQUES FOR DESIGN MATURITY

EFFECT OF TEST LEVEL

- AS NUMBER OF TEST LEVELS INCREASE THE NUMBER OF INTERFACES, THE NUMBER OF TESTS AND SUPPORT EQUIPMENT INCREASE
- THE IMPACT IS DIRECTLY DEPENDENT ON DESIGN MATURITY
- COST AMPLIFIER GENERALIZE PROGRAM COST



*THIS CAN BE CONSTRAINED BY RISK AND MANAGEMENT

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Figure 10-8

percentage of a reduced number. Because of maturity, you have an interaction in the cost of the design and the testing: You do less design and you do less testing. So the testing costs have gone down on a normalized basis, but they tend to stay the same percent because the design cost comes down.

MR. SEIFF: I found your presentation to be really a dream because it looks like you are trying to quantify something that has been generally something of a black art, you know, sort of a guessing game. I was just wondering how far this quantification goes in terms of - take a new program like this one that is being discussed here. Do you actually proceed from a set of charts? The ones you showed us were qualitative; they had no numbers on the axes.

MR. RUHLAND: I painted the numbers off.

MR. SEIFF: You take a set of charts and apply them, subsystem by subsystem, and end up with a final estimate. Do you then try to bring judgmental factors in at that point or how do you actually do this; and what has been your experience in predicting the accuracy of the end result?

MR. RUHLAND: We try to push the subjectivity to the farthest front point that we can, and we quantify all the operations thereafter. The subjectivity comes from a dialogue with the technical people, trying to understand what they mean when they say they are inheriting this or they are expanding that, and to turn it into an input factor. But we have been doing this for six years now. We started trying to track inheritance six years ago and we have been learning. We did terribly for a while and we finally got down to something that I think is working right now. A priori it tracks about thirty missions, when you use the maturity factor, within about a three-percent band. On a new project it's probably tracking twenty percent but how much of that is the model and how much of that is understanding of the project? What I said in the beginning wasn't a joke; it's literally true. The project I estimate with a model, before there is a project office is never done. When you get a project office and they see the problems they've got and they really try to buy the hardware that they can't get now, the project is restructured. So, literally, they never do the project that is estimated by the model before the fact. Now, at JPL I track every project until completed. I continuously re-estimate and I can see it coming in. They change and we converge. If you don't know the project, you can't get the cost.

MR. HERMAN: Just one question: On MJS what was the variance between __________'s estimate and the estimate as submitted by your model?

MR. RUHLAND: We came within plus or minus five percent on the mean. I don't remember precisely. I think it might have been plus or minus four percent, so that is an eight percent band width.

MR. HERMAN: Is the project that you modeled the project that Boyster and Meyers are implementing now?

MR. RUHLAND: Pretty much. I have done a couple more model runs since. I do a minimum of one model run on every project a year, and the last model run I did, I talked to Hickock and the people and we got the deltas and changes, and we went through there. There were not many changes in the assumptions to make a model run, number one; they were very close. The numbers still tracked about the same way.

DESIGN-TO-COST

Fred E. Bradley

McDonnell-Douglas Astronautics Company

N75 20414

MR. FRED BRADLEY: As I go through this, I think you will see a lot of correspondence between what you've been talking about and what's involved in design-to-cost. For instance, Dan Herman mentioned something Tuesday about giving a contractor a baseline design and seeing what he can do with it. You'll see that in this presentation.

Many of the rest of you have been talking about how much science in terms of number of instruments, number of samples, things like that. The amount of science costs money. In a design-to-cost project, there will be a relationship between science and cost.

The cost of weapon systems and space systems has been steadily escalating. This has caused great concern in the government, and has caused them to throw us the challenge of designing to cost. The idea behind design to cost has been stressed in a number of ways, such as eliminate the gold plating, get rid of the frills or, more positively, provide the most for the money or the best buy. I am going to follow a best-buy approach.

As shown, Figure 10-9, the intent behind design to cost appears to be quite clear but whether a given design approach to a particular program is, in fact, providing the best buy may not be so clear. The reason for that is that known costing methodologies do not permit inputting a cost and backing out a best design to do that job. Instead, it is necessary to take a design and its characteristics, input the cost model and get a cost. Mathematically, the cost model plays the part of a many-to-one transformation between the characteristics of the deisgn and a single cost number and, therefore, does not have an inverse. So, then, how are you going to do it?

"PROVIDE THE MOST FOR THE MONEY" (BEST BUY)

OR

"ELIMINATE THE GOLD PLATING"

BUT HOW?

Figure 10-9. Design-to-Cost Intent

In this context it is well to express for you all, in the context of the talk today at least, what design-to-cost is not. (Figure 10-10). It is not streamlined management, value engineering, cost reduction, skunk works, or any of these techniques. Why is that? It's because, given a set of requirements, a contractor can and should provide the lowest cost design that he knows how, using any of these techniques that are permissible with the customer.

WHAT IT IS NOT:

- VALUE ENGINEERING
- COST REDUCTION
- SKUNK WORKS

CONTRACTOR CAN, AND SHOULD, PROVIDE LOW-COST DESIGN TO REQUIREMENTS

WHAT'S LEFT?

REQUIREMENTS – COST TRADE-OFFS

- SYSTEM
- SPECIFICATIONS

Figure 10-10. Design-to-Cost (DtC)

At any rate, whichever ones are permissible, the contractor should use. So what's left? The only thing that appears to be left anyway is requirements-cost trade-offs. And they fall into two categories: the system level requirements, that is the mission description and functional requirements and so forth, requirements documents; and, also, invoked specifications. I'll discuss these two separately, starting with the system requirements.

To do a design to cost analysis in the context that I'm talking about, it is best accomplished in five steps: a requirements analysis, definition of a mission baseline design, a benefit and a cost analysis, and then a benefit-cost analysis. I'll discuss each one of these separately.

<u>Requirements</u> <u>Analysis</u> - Figure 10-11

- MISSION DESCRIPTION
- NASA/USER/CONTRACTOR ESTABLISH

  MANDATORY REQUIREMENTS

  DESIRABLE "REQUIREMENTS"

In the requirements analysis one starts with the mission description. NASA and the user, in the case of the probes the scientific community, and the contractor need to establish a minimum set of mandatory requirements: minimum requirements, mandatory requirements. Because to do any mission at all there have to be some requirements, some place to start from. And then list, hopefully in a prioritized order, the desireable requirements or desirements. The next step is to define a baseline system that meets those mandatory requirements and it may not make a lot of difference what that baseline is, assuming that you use low-cost design approaches. At any rate, it's a concept of the best design, or the minimum design, to meet the minimum baseline requirements. That is your starting point to make the trade-offs of requirements design and cost.

Benefit Analysis - Figure 10-12

- ESTABLISH BENEFIT SCALE
  - QUANTIFY BENEFIT OF EACH OPTIONAL "REQUIREMENT"
    - ANALYTIC
    - "COOPER RATING"
- REQUIRES CLOSE WORKING RELATIONSHIP
  - NASA/USER/CONTRACTOR

BENEFIT ANALYSIS – A MUST

In the benefit analysis it will be necessary to establish a benefit scale to quantify the benefits; in the case of the probe, the amount of science. Sometimes it will turn out that there is a directly-perceivable obvious analytic measure of benefit and I will show you an example of that a little later. In other cases and, unfortunately, frequently such a direct-benefit scale is not available. Judgment is involved, opinion and prejudice. It will be necessary in that case to establish a so-called "Cooper rating" type scale that will vary from zero to one or zero to a hundred or whatever you want and rank each desirement in terms of its benefit. "Cooper rating" scales are used in pilots' judgments of the flying qualities of aircraft relative to their stability parameters or other parameters. Again, a close relationship between NASA, the scientific community and the contractor is going to be involved. We have to all talk the same language or there is no way to do this design approach. It appears to me that a benefit analysis is a must despite the difficulty, perhaps, of quantifying it, because if you don't do it you will tend to be driven to the vicinity of the lowest-cost design, which might be the baseline design. In all likelihood that is not the best buy.

Cost Analysis - Figure 10-13

- ESTABLISH AGREED UPON COSTING METHODOLOGY

  NASA/CONTRACTOR

- USED TO COST THE BASELINE AND COST INCLUSION OF OPTIONAL "REQUIREMENTS"

- ACCOUNT FOR INTERACTIONS

To do the cost analysis itself it will be necessary for NASA and the contractor to agree upon a methodology early, day one. Again, we have to talk the same language. Once that is done we cost the baseline itself and cost the inclusion of each additional desirement. We have to account for interactions in that process and I'll explain that a little more fully on the next chart.

Benefit-Cost Analysis - Figure 10-14

Having gone through all this you can determine the change in benefit for each desirement and the change in cost, and you can tabulate or plot or however you want to do it, the ratio of change in benefit to the change in cost. Then you can make a plot of benefit versus cost and what you do is you order these and you add the thing that gives you the most benefit for the least cost, first. Then you take the second one, the third one, the fourth one and the fifth one. Then, depending on your cost goal, which is qualitatively illustrated on the figure the point

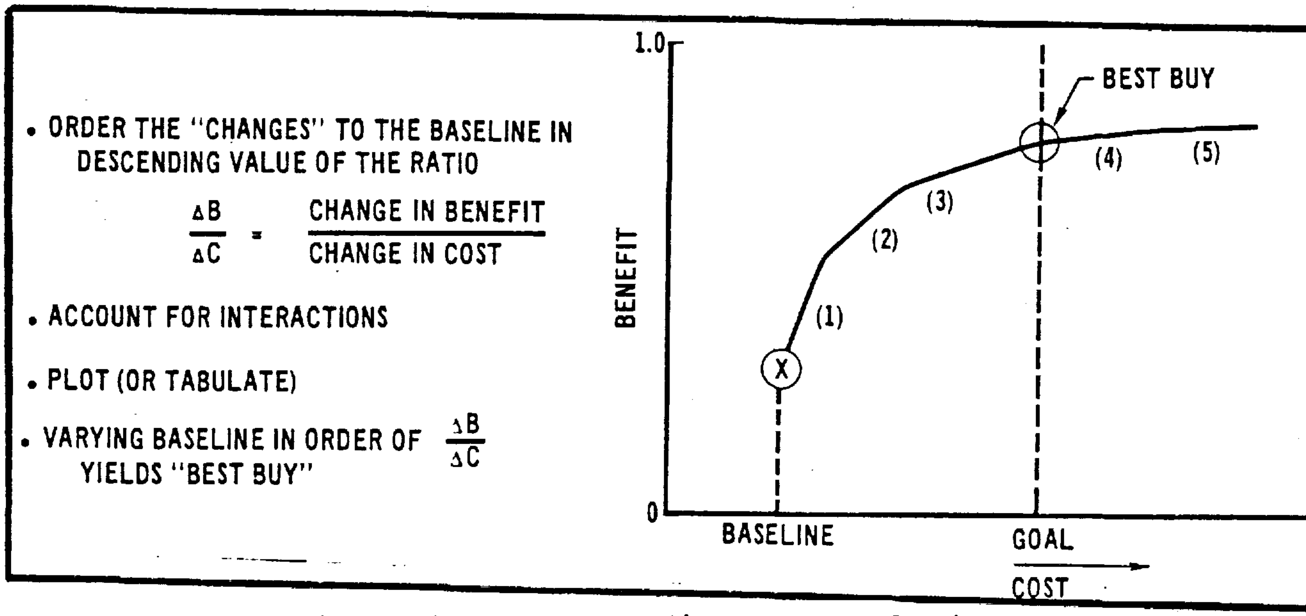

Figure 10-14. Benefit-Cost Analysis

shown would be the best buy. In this case, of the five potential desirements that might be incorporated in the baseline, you would add the first, the second and the third, but not the fourth and fifth. Now you can get some idea here of the idea of eliminating frills and gold plating, it says, "Get off the upper tail, there are diminishing returns out there."

Now I mentioned accounting for interactions. The benefit-cost relationship, in general, will not be independent of the order in which the changes are made. So you will need, probably a complex computer program that has the interactions built in, to test out various orders and find the best one. For example, Wes Cowan told you Tuesday that the design of the probe model that you saw was dominated by the mass spectrometer. Once it is put in, there is quite a bit of volume, and it's thirty-five inches and those things, for the other experiments. Now were that not in there you could start, then, with a smaller probe and then putting the mass spectrometer in would be a big step. The point is, the order in which you incorporate the things that you want causes you to have to account for that in making this plot. That is the basic idea of how to approach, in a systematic fashion, a design-to-cost program. This dealt, so far, with the system-level requirements.

Now invoked specifications are another kind of requirement. They can be a most insidious cost driver because frequently they are rather slavishly invoked. So they should be critically examined in whole and in part and unnecessary items eliminated. Mr. Gansler who is Deputy Director of the Department of Defense Research and Engineering had an interesting example of that. There was a spec requirement invoked against an airplane. It required that all systems on the airplane be operable, not survivable, but operable at seventy thousand feet. One of these was the instrument landing system. Those kinds of things should be eliminated.

If the specs are analyzed in great detail, there will be some questionable ones. They can be subjected to benefit-cost tradeoffs. An example of that might be the structural factor of safety, amount of testing, uncertainty of the atmosphere, confidence of being able to penetrate successfully, and things of that nature. So, these need to be very carefully examined.

There is a potential effect on contractor selection in the competition in a design-to-cost program and if you go that way on the probe you might want to think about this. These are compared on Figure 10-15. In the older present method, the requirements are fixed, the contractors design to them. If they've done their

OLD METHOD:

FIX THE REQUIREMENTS – VARY THE COST –
MATCH DESIGN TO TOTAL REQUIREMENT.
ALL DESIGNS WOULD DO THE JOB.
SELECTION BASED ON COST, A MORE SUBJECTIVE PARAMETER

DESIGN-TO-COST METHOD

FIX THE COST – VARY THE DESIGN COMPATIBLE WITH VALUE/BENEFITS OF REQUIREMENTS.

RESULT: CONTRACTOR SELECTION BASED ON TECHNICAL PROPOSAL; WITH MOST VALUE FOR THE COST GOAL

Figure 10-15. Potential Effect on Contractor Selection homework, the designs will tend to be very similar, and in the evaluation of the technical proposals, the point spread quite close. Therefore, frequently the selection is based on other factors or cost, which is a more subjective parameter. Unfortunately, some people think that our cost predictions are in the same category as your atmospheric predictions, which tempts me to term costing methodologies as scientific.

Well, at any rate, given additional data in the case of either atmospheres or costing, the costs do converge and as the two previous speakers talked about, given enough definition, enough understanding of the program and enough time to understand it, we can do a good job.

So, in the design-to-cost method, the contractor, via the program manager, will have his eye on the cost ball or at least the relationship between the cost ball and the design. And, in particular, he will have to be very careful in his proposal as to what he promises that he will give for a given cost goal. He will plaster the cost model on the wall and understand, to the detail that he can in the time available, those things that are driving that model and will be very specific about what he says he can do. Now, that should have the effect of spreading the difference in the technical proposals and, therefore, the technical proposals should become the primary SEB-type evaluation article which most of us would like for it to be in the first place.

After the hardware development is initiated, one still has to keep the cost goal in mind. It isn't going to automatically come out what we all think it will. So, now one apportions cost goals. In the past the tendency has been to apportion weight, power and so forth goals. There will, of course, always be some constraints but, nevertheless, the idea here is to apportion cost goals and give the subsystem designers rules of thumb or some means of running the whole system model, as the case may be, to make his trade-offs to stay within his cost goal.

Involved in that is managing effectively, after the hardware is let. That may sound trite but that is what it boils down to, and different companies and different centers have their own ideas of how to do that. At any rate, if one continues - and one should - to actively use benefit-cost analyses in the decision-making process during the hardware phase at least our eye will be on the ball and we'll always be converging in the right direction.

I would like to run through an example. I wanted to get one that directly related science to cost and so I selected a hypothetical orbiting telescope. Why, you will naturally ask, didn't I use the probe? The reason is that in the case of the orbiting telescope there is a ready-made measure of benefits. In the case of the probe, and I feel even more strongly after listening to you all, we didn't have that measure and we haven't been able to sit down with you and come up with this benefit scale. In this case, it is fairly straightforward. What we are going to do is orbit a telescope and systematically stare at the sky in wavelengths filtered by the Earth's atmosphere. So it's fairly easy to quantify this case (c.f. Figure 10-16).

> LAUNCH A SCIENTIFIC ORBITING TELESCOPE WHOSE PURPOSE IS COLLECTION OF INFORMATION BY SYSTEMATICALLY SEARCHING THE SKY WHILE VIEWING IN WAVE-LENGTHS FILTERED BY THE EARTH'S ATMOSPHERE. THE PROGRAM IS TO FOLLOW THE DtC APPROACH.

Figure 10-16. Example-Orbiting Telescope

<u>Requirements Analyses</u> - Figure 10-17

I am going to go through the steps that I outlined that you should go through. This is very simplified, of course. We are going to launch it on the shuttle. The program life is a total of eighteen years: three years for design, development, testing and engineering, and fifteen years on orbit. There is a ground rule of no single point failures as the minimum level of redundancy.

1. THE TELESCOPE IS TO BE LAUNCHED ON THE SPACE SHUTTLE.
2. THE PROGRAM LIFE CYCLE IS 18 YEARS. (THREE YEARS DDT&E AND 15 YEARS OPERATIONAL)
3. THE TELESCOPE MIRROR IS TO BE THE LARGEST DIAMETER COMPATIBLE WITH A SINGLE-LAUNCH IN THE SPACE SHUTTLE.
4. NO SINGLE POINT FAILURES.
5. ONE TELESCOPE IS TO BE PROCURED. IF A DISABLING FAILURE OCCURS ON ORBIT, THE TELESCOPE IS TO BE RECOVERED FROM ORBIT, AND RETURNED TO EARTH BY THE SPACE SHUTTLE, REFURBISHED, AND RELAUNCHED BY THE SPACE SHUTTLE.
6. A DUE EAST LAUNCH FROM ETR.

Figure 10-17 - Requirements Analysis

Coupled with this is the idea that if we get a failure on orbit we will go up with a shuttle, get the telescope, bring it down, refurbish it, re-launch it with a shuttle - that is two launches - and put it back in orbit. Now those are the requirements. All those are considered to be minimum or mandatory.

<u>Minimum</u> <u>Baseline</u> <u>Design</u> - Figure 10-18

1. A MEAN MISSION DURATION (EXPECTED ON-ORBIT LIFE) OF 2.5 YEARS.
2. A SUBSTANTIAL WEIGHT MARGIN ON THE SPACE SHUTTLE.
3. A COST OF UNITY WHICH IS BELOW THE GOAL, G.

From that emerges a baseline design which we don't have to go into the details of for our present discussion, but it turns out that with no single point failures you get a mean mission duration, which is the expected life on orbit - the mean time between failures, it's called a lot of things - but it's the average length of time it will last before it fails and has to be brought back, of about two-and-a-half years.

There is a weight margin and the weight margin in design-to-cost that you were discussing earlier may be more important in the context that I am going to talk about, than the one in which you were talking about it. Then, I have normalized all costs to the total life-cycle costs of the baseline. That is the total eighteen years.

Benefit Analysis - Figure 10-19

BENEFIT = VIEWING TIME ON ORBIT

$$= P \left(1 - \frac{R}{MMD + R}\right)$$

P = PROGRAM OPERATIONAL LIFE = 15 YEARS

MMD = MEAN MISSION DURATION IN YEARS

R = TOTAL TURN-AROUND TIME = 1/3 YEAR

Now, what is the benefit? Well, we want to stare at stars and getinformation, or stare at places where there aren't any stars and see if there are any in these wavelengths. So, a direct measure of benefit, assuming you get the data back, is viewing time on orbit, which is equal to the fifteen years that you would be without any failures diminished by the amount of time that the thing is being turned around. This is the time from the detection of a failure, bringing it back, refurbishing it, and relaunching it. In other words, the recycle time, times the number of failures you get, which is the program duration on orbit, divided by the mean mission duration plus the recycle time.

So, this is a direct measure of benefit and you can see that increasing the mean mission duration increases the scientific benefit. However, building in more mean mission duration costs money. I have plotted on Figure 10-20 unit cost of the telescope as a function of the amount of mean mission duration built in. This is done by increasing redundancy. We get the left hand curve on the figure and it's fairly steep. It's essentially exponential through any range that you would be interested in. There is also a weak effect on the design, development, test and engineering costs and it appears to be linear. It is weak, but it is there as shown on the right hand curve of the figure.

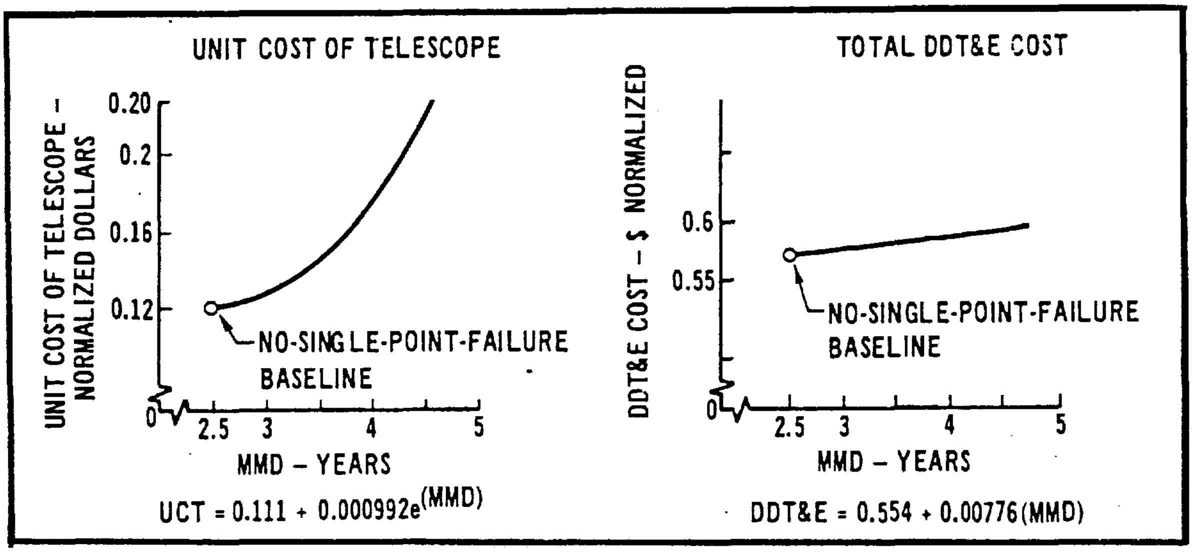

Figure 10-20. Cost Relationships

Figure 10-21 presents a simple cost model written from those previous curves. The total life cycle cost is the DDT&E of the baseline plus any increment to run up the MMD,* the unit cost of the baseline of the telescope plus any increment to run up the MMD, plus the refurbishment cost, which is equal to the percent it costs to refurbish the telescope - I used ten percent - times the cost of the telescope, times the number of times you have to refurbish

$$
\begin{aligned}
LCC &= DDTE_{BL} + \Delta DDTE + UCT_{BL} + \Delta UCT \\
&\quad + \frac{P}{MMD + R} \left(k_1 \left[UCT_{BL} + \Delta UCT\right]\right) + \left(1 + \frac{2P}{MMD + R}\right) CPL_{SS} \\
&= 1.46 + 0.00776\,(MMD) + 0.000992\, e^{(MMD)} \\
&\quad + \frac{0.7825}{MMD + 1/3} + \frac{0.00149e^{(MMD)}}{MMD + 1/3}
\end{aligned}
$$

LCC	=	Life Cycle Cost, total program
$DDTE_{BL}$	=	Baseline Design, Development, Test, and Evaluation Cost
$\Delta DDTE$	=	Incremental Cost in DDT&E to provide an increment in MMD
UCT_{BL}	=	Unit Cost of the Baseline Telescope
ΔUCT	=	Incremental Unit Cost of the Telescope to achieve an increment in MMD
k_1	=	Percent Unit Cost of the Telescope to perform one refurbishment = 10%
CPL_{SS}	=	Cost per Launch of the Space Shuttle

The fifth term in the equation accounts for the number of refurbishments to be performed and the sixth accounts for the number of shuttle launches to be performed.

Figure 10-21. Life Cycle Costs

*Mean Mission Duration

it, which is the number of failures, which is P over MMD plus R, as I already mentioned then, plus the launch costs, which is the cost per launch of the space shuttle times the number of launches. You have to have one to get up there in the first place. For every failure you have two launches, so that is the factor of two and, again, the number of failures. So that is the total cost.

That all boils down to this relatively simple expression on Figure 10-21. Combining the benefit model and the cost model you can plot benefit versus cost as on Figure 10-22. There are several interesting things about this.

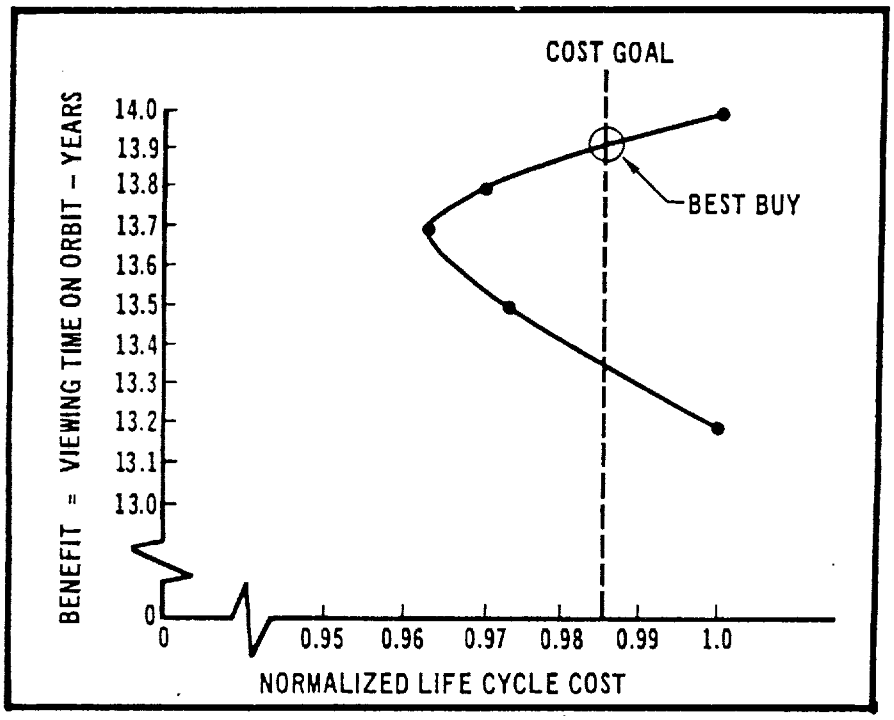

Figure 10-22. Locating the Best-Buy

The ordinate is viewing time and the abscissa is normalized life cycle cost. The baseline is shown. It neither provides the most benefit nor is it the lowest cost. So, as you add redundancy you not only increase benefit but you make the program get cheaper. The reason that it does go in that direction is that you are reducing launches faster than you are adding cost to the telescope itself, until you get to the point at the knee of the curve. As you continue to add redundancy you still reduce the number of launches but now the cost of the telescope gets to you, and the curve turns around and goes the other way.

If your cost goal were as shown, then your best buy would be at the circle on the curve. So this is a systematic way of approaching design to cost in this particular example.

Now let's take a look at the probe. As an example, you might investigate commonality in terms of the number of planets to be visited. In other words, do you design it to visit one, two, three, or four planets? We have plotted in Figure 10-23 cost in millions of dollars, with and without planetary quarantine to do that. Now, there are two effects in this curve. Notice these go the other way instead of bending over. There are two effects in developing these curves. One is you are buying two probes per planet; and that is in there. But, also, if you design it to go to more than one planet there is an increase in engineering and development cost of a commonality-type probe. And that's in here, too. However, although we don't have it plotted on here, it's a straight line, that's going to be less expensive than designing independent probes for each and every planet. So, given a particular program cost goal, you can come in here at your goal and figure out how many planets you might want to design for.

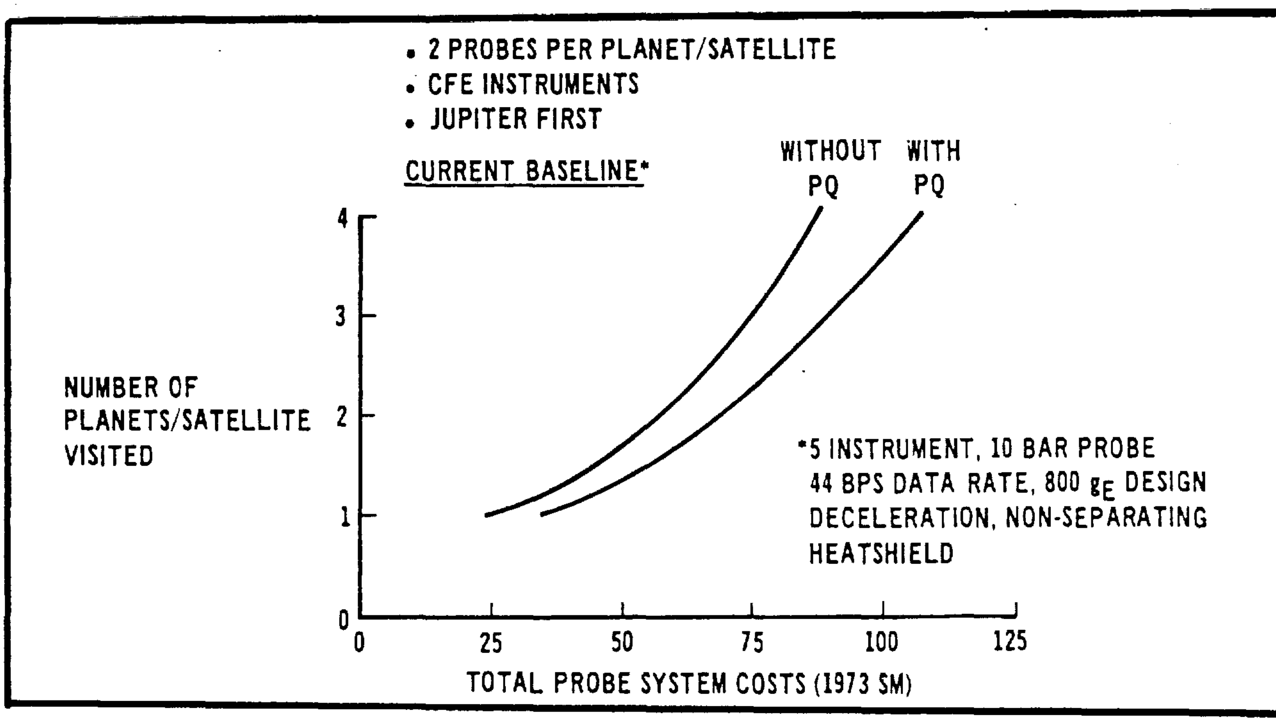

Figure 10-23. Potential Probe Applications

Now that's just one example. Other things that could be traded off are how many instruments, maybe the amount of data, and maybe the number of samples that are taken as you come down through the atmosphere. One nice thing about the method that I have presented to you is that you can intermingle all these apples and oranges. You can investigate the increment in benefit by going to different planets, the increment in benefit by adding, or subtracting, for that matter instruments, playing with the data rate, the number of samples as you come down thru the atmosphere, even contending instruments on that basis, and make a plot. The first step might be go to another planet, the next step might be add another instrument, the next step might be get more data, and so forth. Then you can come in and figure out what you ought to do.

Now, conversely, if you don't know what the cost goal ought to be, you use this same technique backwards and find out what the cost goal ought to be.

My conclusions are summarized on Figure 10-24: design-to-cost is a practical approach and it can be approached systematically. It's very obvious to me, or at least I feel confident about it, that close liaison between NASA, the scientific community and the contractor is required to follow this approach. We've just got to be talking the same language or the problem isn't tractable. The technical proposals will become of increasing import-

DESIGN-TO-COST

- CAN BE APPROACHED SYSTEMATICALLY
- REQUIRES CLOSE NASA/USER/CONTRACTOR LIAISON
- PROBABLY LEAD TO INCREASED IMPORTANCE OF TECHNICAL PROPOSALS
- WILL YIELD THE BEST-BUY

Figure 10-24. Conclusions

ance and, probably to the benefit of all of us, it will yield the best buy.

MR. VOJVODICH: If there are questions here we do have some time for some questions from the floor.

MR. GEORGIEV: Would you put on that slide, Mike, that shows a very strong cost trend, at least between the one and two, and I'm not clear exactly what you are constraining. This is the same instrument payload on both probes? (Figure 10-23).

MR. BRADLEY: Yes, five instruments

MR. GEORGIEV: With the same data rate?

MR. BRADLEY: Correct

MR. GEORGIEV: Why is there such a strong cost difference?

MR. BRADLEY: A lot of the slope is due to buying two more probes. If you would subtract out the cost of the hardware of the probes, what was left would be the cost of engineering and testing and so forth commonality.

MR. TOMS: It still looks very, very steep because it is steeper than the lines of the origin.

MR. BRADLEY: If we work on it, maybe we will get them down some. These are pretty first-cut estimates on this.

MR. CANNING: These viewgraphs that you showed just before this one, the ones with the double value (Figure 10-22) - I sort of question the idea of locating the best buy this way. It would seem to me that you can conclude, perhaps, the best buy is that left-handed point. It just depends on whose money you are spending. When I go to buy a car, for instance, I don't say, "I am going to spend $3,692," and then go out and find the fanciest car that I can get for that. I go out and get the car that I need.

MR. BRADLEY: I wasn't really going to get into this, but the way to answer your question, I guess, is I will have to get into what is the difference between cost effectiveness and design-to-cost. We think design-to-cost is new. Well, the facts are there isn't any difference. What you would do in cost-effectiveness is look for the most cost-effective point. You would look for the knee in the curve, if there is one. And that would be as shown on Figure 10-22. This would be the least expensive and somewhere in here would be the most cost-effective, that is, if you plotted benefit over cost as a function of cost, it will have a maximum and it will look like half a banana, which is similar to this one. So if you envision this translated into benefit over cost as a function of cost, then its maximum point is the knee of the curve. Beyond that you have reached diminishing returns.

Now what it would do, it would loop back around like this - this point would be the lowest cost program. And then, the horizontal tangent, as it loops back over is the knee or the best buy from a cost effectiveness standpoint. But, now, suppose the guy says, "I don't care about that. I've go so much money to spend and I want to spend it in the best way I can." Then, if it is that much, he will pick that point. So the real difference between design-to-cost and cost-effectiveness is not formal at all, there isn't any. <u>It's in the eyes of the beholder</u>. The cost-effectiveness advocate will pick the most cost-effective point; the design-to-cost person, whether he is below or above, will pick the best buy.

MR. NIEHOFF: Fred, I think you will also want to be very careful about evaluating best buys on the basis of the shapes of the curves because shapes of curves are very easily manipulated by the scales you are applying them to. In this particular case

I think that curve would be very different in shape, almost a vertical line if you changed your abscissa here which is very, very, very fine, within hundredths of a percent of total cost. So it is important that you say the thing that you are really evaluating, in this case, would be real dollars and probably months of time on orbit would be the sets of parameters, and that could change what you are willing to call the best buy. So, you can get all kinds of shapes by varying the scale and you have to be careful.

MR. BRADLEY: What you say is true. However, these are real dollars. I have just normalized them; and these are real years.

MR. NIEHOFF: No, I am not questioning the variables, abscissa, or ordinate. I believe them, but it is the scale that is being used.

MR. LIPSON: I would suggest, also, that one other factor is the factor of technical risk. The technical risk may be different for these points and you may feel a lot more comfortable going with the lower technical risk even though it may not have the best scientific payoff. You may feel at least that you are sure you can satisfy that particular configuration by that particular launch window.

MR. HERMAN: A comment: You know design-to-cost can also be a way of changing your philosophy rather than exact numerical procedures as to how you come up with a baseline design. And the best example I can give you is Pioneer-Venus and, specifically, the report issued by the Science Steering Committee where they, in effect, said that if that program can be brought in for, say, in the order of a hundred and fifty million dollars: It is the highest priority program of all the programs that NASA presented to the Space Science Board. They went one step further in that they said if that program escalates, say, beyond two hundred million dollars, it is no longer of that high priority because there are other programs that have the science potential, you know, for the dollars expended that are more worthy of consideration than Pioneer-Venus. So, on the Pioneer-Venus program there is a point where if we can determine that the runout costs may exceed the prior reports, there would be consideration given to cancelling.

MR. VOJVODICH: Well, we are running up on a bind here with respect to lunch and our next presentation which are in the afternoon. Many of you won't be around here for this afternoon's roundtable and, on behalf of John Foster, Director of Development and Ben Padrick, Chief of the Advanced Space Projects Office I would like to thank you personally for participating in making the workshop something of what I feel has been a success.

SESSION XI - SUMMARY ROUNDTABLE DISCUSSION

MR. SEIFF: We plan for the next two hours to try to sum up what has happened here during the two-and-a-half days of meetings. In view of Dan Herman's announcement at the outset that the planning for Uranus probe missions was becoming more firm in the sense that Phase B studies are to be undertaken, the panelists are going to each put a special emphasis on the feasibility of the Uranus mission and to comment on problems that they see remaining; things that should be done to solve those problems and to bring the technology up to the state where it is ready. If, indeed, it is not now ready, as I think it is in many of the sub areas.

We are also going to try to limit ourselves to something like five minutes each in the opening remarks on each subject area so that we can allow some time for interchange between the panel and the audience after we make the rounds. I think I prefer to let the panel's statements be uninterrupted in the sense of going from subject to subject until we complete all summaries. At that point in time, however, we are going to declare open house and we are going to receive comments from you. Or, if you would like to augment something that a panelist has said, or agree with something, or disagree with something he has said or raise questions, any of those things will be in order.

The order of the panel chairmen speaking will be the same as that used in the original program, with the exception that Larry Colin will speak for Ichtiaque Rasool who had to leave. We will proceed on through the sequence, and we will close with remarks from John Foster and Paul Tarver, representing Ames management in the probe area and Headquarters NASA management respectively.

DR. LARRY COLIN: In case anybody is confused, I was not a member of the panel. All the panel members from the first session, Science Rationale and Objectives, left early and I happened to be walking down the hall and they asked me to summarize what they said. Since I didn't listen to all of them, I will make some comments of my own as well.

The point that they wanted me to stress was that exploration of the outer planets and their satellites by in-situ measurements is absolutely required if the major questions about the outer solar system are going to be answered. This is not to say that orbiter and flyby remote sensing isn't important. Certainly, they are important from the point of view of helping to understand some of the ground-based observations which have been collected over many, many years now. But there is no question that in-situ probing will be necessary in the long run.

Interest ranges over a wide spectrum of missions from simple missions of the kind that were mentioned consisting of simple temperature, pressure, and accelerometer instruments, plus the comparative atmospheric structure experiment (a payload which may be of the order of two kilograms), up to a full-blown entry probe mission of the order of the Pioneer Venus large probe mission, which contains about thirty kilograms of scientific payload weight.

The panel was very much interested in the proposal put forward by John Wolfe of a Pioneer-Jupiter orbiter dropping off a small probe which would be capable of carrying about ten kilograms of science. Ten kilograms fits nicely within the two-to-thirty spectrum that I mentioned. The experiments that are on the Pioneer-Venus large probe are, in fact, those which are in the primary payload including options mentioned at these meetings. Included are: (1) the atmospheric structure experiment (temperature, pressure, acceleration and, hence, density, of course, which results from these),

(2) for measuring the composition of atmospheres, both the mass spectrometer and gas chromatograph and their combinations, of course, are of interest, (3) for studying the cloud structure, the cloud particle size spectrometer and nephelometer, and finally, (4) for studies of thermal balance of the planets, devices like net solar flux radiometers and net IR flux radiometers would be very important in outer planet missions.

The question arose about payload commonality for Uranus, Saturn and Jupiter missions. The panel members definitely feel that trade-off studies are required immediately to determine the question of whether such commonality is desirable. Certainly, commonality sounds good, but it should be looked at from a scientific point of view for each of these outer planets and their satellites. As I understand it, NASA Headquarters has taken up this suggestion of a trade-off study and one will be set up this summer. Don Hunten will be organizing the summer study.

The panel wishes also, to endorse for outer planet science the basic approach which has been used for Pioneer Venus. That is, complete iteration and reiteration of the science objectives and instrumentation and spacecraft capabilities so that one can optimize and balance the scientific payload against the spacecraft design with the viewpoint of keeping as low a cost approach as possible.

John Lewis made a special plea in the area of composition measurements. Chemical analyses of the planets appears to be a relatively easy thing to do with the kind of instruments that are at hand today. The measurements of isotopes, clearly of importance in solar evolution theory, is the thing which is most difficult to do. The idea of a separate gas chromatograph and a separate mass spectrometer is certainly a desirable thing to have. The question of combining them, a la Viking, as a single instrument is something that he endorses for continued development.

Along this line, I would like to urge NASA Headquarters that they generally maintain a strong SR&T program for advance development of long lead time instruments.

Don Hunten cautioned that we should not overlook the importance of the upper atmospheres and ionospheres of the outer planets. After all, we do fly through them getting into the lower atmosphere, if for no other reason. But they are important for their own sake, and we have a ready collection of in-situ measurement devices: neutron and ion mass spectrometers, retarding potential analyzers, electron temperature probes, and airglow and dayglow devices, which would be very useful on outer planet missions.

With regard to Uranus, John Lewis stressed that it is the logical first choice; and the panel also feels it is the logical first choice for outer planet entry missions. They caution that the Pioneer 10 thermal results from the occultation experiment, which appear helpful from system design, are quite contradictory with regard to all other measurements that have ever been collected across the spectrum. They feel that all the conflict that has arisen makes it impossible to use the Pioneer 10 results as a basis for spacecraft entry designs in the future. Those results have to be understood if they are correct.

MR. BYRON SWENSON: The Mission and Spacecraft Design Constraints panel had roughly ten major points that they would like to make. They divide themselves roughly equally into comments regarding navigation and comments regarding systems.

With emphasis on Uranus, the first and probably the foremost is a plea for an improved ephemeris of Uranus. We estimated that we could obtain this for a very modest expenditure; I believe about $250,000. It seems that there is a real requirement that something be done along this line.

The second point also deals with navigation relative to Uranus. We have seen that optical measurements were required because of the ephemeric uncertainty of Uranus, but there is a question relative to the real-time processing of the optical measurements when you have something like a five-hour light time from Uranus to the Earth. And the software that goes into processing that type of data and the real value of that data is still in question.

The next major point is a systems oriented point relative to Uranus. There is concern by several members of the panel as to the system interactions and implementation of deploying a spinning probe off a 3-axis stabilized Mariner bus. The problems do not seem entirely insurmountable, but there are a lot of things that have not been investigated: tip-off errors, the implementation of the deployment; whether we should have a spin table; whether we should go to the difficulty of putting a spin table on the spacecraft; and so on.

The final systems oriented point relative to Uranus was the question of how much commonality should be carried in the probe design. Previously in the Saturn-Uranus probe studies where we deployed it off the Pioneer spacecraft, we did find that we could employ a great deal of commonality. But now introducing the Mariner into this and not only do we require commonality between the planets, but we must now require commonality between spacecraft. This implies some penalties associated with the probe when flown on a Mariner.

For example, the frequency that was chosen for the Pioneer was 400 megahertz and I believe that 800 megahertz would be a more reasonable center frequency if you were flying off a 3-axis stabilized machine which had a highly directional antenna.

And, of course, a change in the communication system cascades itself right on through the system, and I am sure there are penalties here that we have not completely understood.

So we have the whole question of how much commonality is desirable and cost-effective.

Moving on to the Saturn and Titan missions, which were to be Pioneer launched, we saw that the capability to obtain a Titan intercept and the subsequent Titan occultation was indeed uncertain with the V-slit navigational sensor.

However, the point was raised that the tests that TRW has made on the V-slit have indicated a greater accuracy than was used in the calculations that resulted in the previous conclusion.

So it appears that if we are going to fly a Titan mission using a Pioneer spacecraft, there is more work to be done on the V-slit sensor to verify this greater accuracy.

For Jupiter probes, one of the major questions which has not been addressed sufficiently in the conference is the radiation hardening of the Jupiter probe. The probe does have to get in close to the planet by definition and it will encounter a great number of protons if the current models are correct. Some more light should be shed on this question with the Pioneer XI passage, which will give us much closer passage and a much better model of the proton belt.

A question was raised relative to pre-entry science data particularly at Jupiter. It was felt that the scientists - and I believe Don Hunten mentioned this - would eventually request pre-entry science. A dramatic impact is noted when you require pre-entry communications from the probe. I just want to highlight this because if you do put on pre-entry science you are going to really change the probe design.

And finally, there was a feeling that we should re-examine the depolyment strategy for all these missions. They appeared to be common but there were slight differences. Nearly everyone is using deployment at 27 days prior to encounter. However, we saw some numbers slightly different from that, and it was felt that these factors do have some fairly sizable impact upon the systems, and we should, if we are going to have a common probe, standardize some of those factors.

MR. SEIFF: If I may exercise the Chairman's prerogative here, I would like to ask you one question. The suggestion that was made by Tom Croft, when coupled with the problem that was described by Donn Kirk, namely, the need for accurate initial conditions for reconstruction of the atmosphere - these seemed to couple together. He is proposing that the relative velocity between the probe and the bus be accurately determined prior to entry - after separation but prior to entry - and that the bus trajectory be accurately documented from its perturbation in flying by the planets which, coupled together, leads to a very accurate information, presumably, on the initial conditions for entry.

MR. SWENSON: I can't really comment on that. The only thing I can say is that the Mariner with its full optical systems will be able to deliver the probe to a much smaller entry angle corridor than the Pioneer can, for example, at Saturn. And this, too, of course has impact on the probe design and the question of how much commonality should be provided and the quality of the science you will get at Saturn versus Uranus.

MR. SEIFF: Tom Canning is next, to speak on the subject of the probe design.

MR. CANNING: Most of the things that I will comment on are concerned with probe <u>system</u> designs. There will be others talking about the sub-systems of probes, and I will try not to spend too much of my time on them.

With regard to the draft "10-Bar Probe" book that was sent out with invitations to this meeting, one point was emphasized through the study DYNATREND did with and for us, but may not have been amplified on adequately here; and that is in that book and in discussions during the last three days we see very different system designs to do the expected missions at Saturn and Uranus. This serves a purpose, namely, it tells you that either there is no single, unique design that will do the job, or these differences might imply that somebody is off on the wrong track in his design.

One of these designs was done essentially on the basis, "no-holds barred, re-package your payload, do everything necessary to design the system for the mission." The other approach which received a lot of attention was, "Here are a bunch of boxes and designed systems from a similar investigation, do this outer planet mission with them modified as little as possible." There were other minor differences in ground rules, but that really was the driver to produce the very different designs presented.

During this meeting all of the designs we have discussed in detail for the Saturn-Uranus entry and descent were <u>unstaged</u> designs, that is, they did not have a parachute stage to delay the descent at high altitude. One of the panel members urged, and I repeate his urging, that we really must not consider this to be a closed subject. We have to expect continuing evaluation by the engineering and scientific communities on the impact and value of obtaining high altitude measurements. And an input to these trades would be the designs for staging via parachute-type systems.

Along the same lines of the continuing interest and influence from the scientific community, we clearly should keep a very active participation of a nucleus of scientists. During the formative phases of the project, we would like to know as accurately as possible what the scientific requirements are going to be when the mission is approved for execution. At that point, or shortly thereafter, we would like to have some way of finalizing on these science requirements, turning the scientists off, if you will, to let us get on with the system design in accordance with the requirements as have been established. And this always presents a problem.

In the middle of that problem is the establishment of priorities, or of principal goals in the case of a probe mission going to any of these planets. This usually manifests itself in the competition for weight, dollars, data, or any other measurable quality, between the probe that goes into the planet and the spacecraft which flies by. I think that this is a question which should be settled by the concensus of the scientists ahead of time; i.e. establish these priorities, and then stick to them. I can see grave difficulties and costly perturbations to a program if those priorities are not carefully settled in advance.

Another comment that came from this discussion was concerned with schedules and that we should do our best to pace the program very carefully in accordance with what we are able to do. That is, to base the next program, or perhaps the next two programs, on what we are quite confident we can start out to do right now. Perhaps, even restrict these programs to things that we know damn well we can do. The danger of that approach, however, is that we would be neglecting the long-distant program; obviously, in this case a Jupiter probe mission which presents a major step in difficulty from the other outer planets.

We certainly would like to consider the possibility of what one might call a revolutionary advance for that program, even though we don't demand or we would not even intend to use such advances for earlier programs unless they came along very rapidly. An example of this advance could be the continued development and availability of a characterized reflecting heat shield.

Another point should be made: several speakers indicated that Jupiter entry is now so much easier with the improved ephemeris, improved navigation and so on based partly on Pioneer 10 data. This discussion was very optimistic. On the other hand, not sufficiently emphasized is the point that the heat shield of this Jupiter-entry vehicle does not change much. Even with shallow entry, the probe is going at 50 kilometers per second and has to be slowed. The heat shield will remain to be the design driver.

My group then discussed the philosophy of the control of system design for long term missions, and this is in the area of the reliability of the hardware produced. We typically characterized the hardware that we have used, the subsystems and the total systems, by reliability numbers. Analyses should be continued with regard to the cost-effective approach to reliability for long-term missions: redundancy of equipment vs. high reliability demonstration projects; reliability analyses, failure analyses, and the examination of the consequences of failures. The JPL approach to this subject should be examined since it apparently works well as demonstrated by the Mariner-Venus-Mercury; Mariner X mission. There were equipment failures and yet the mission was a fantastic success.

MR. SEIFF: The critical areas of heating estimation and heat protection will be covered next and Dr. Walter Olstad will address the first of those subjects.

DR. WALTER OLSTAD: From the point of view of entry aerodynamics and heating, being asked to focus on Uranus really doesn't restrict me at all because we know so little about Uranus. What we know about the atmosphere is that there is some hydrogen in it and there is some methane in it. And if we design for what is now considered the worst case, the entry in terms of heating rate is about as severe as the nominal Jupiter entry. Thus, if Uranus rather than Saturn or Jupiter is chosen as the first target for an outer planet probe, the problem of entry heating is not greatly simplified.

And that brings up the first point. We need a good handle on the range of possible atmospheres. We'll let someone else worry about what the probabilities are but let us know what the range of possible atmospheres are and we'll exercise our predictions over that range. Then the decision makers can work with those numbers as they will.

An interesting feature about outer planet probe missions is that we are going to have to rely much more heavily on analytical and computational predictions without backup experimental verification than ever before unless we undertake a fight experiment which could be a very costly thing. So we need to assess the risks, and we must assess them quite carefully. This is something we should get on with right away.

Now, let's look at our ability to predict heat transfer for probes entering the atmospheres of the outer planets. Most of the analyses have been confined to the stagnation region. They are quite sophisticated and we feel quite confident we can come up with a conservative number and one that is not so far out of the ball park that you are really compromising probe design. However, we have no real experimental verification. Any verification we have is a partial verification under conditions much less severe than required.

As we go away from the stagnation point on the probe, things get worse. At the present time, we have just a few analyses, a few analytic tools available and there are some serious deficiencies in these tools. These deficiencies have to do with things like predicting transition, determining turbulent heat transfer and determining the chemical state of the ablation products. These deficiencies are going to remain because the only way we can get at them is experimentally under the same conditions the probe will experience. It is not easy to extrapolate from experimental experience when you are talking about transition and turbulence. What we do now is take a lot of data and fit curves through it. The curves are not based on any physical reasoning so when you try to extrapolate a long distance from the original data base you can be badly misled. There are plenty of examples of just this sort of improper extrapolation throughout our short history of entry vehicle design.

So we are going to be faced with considerable uncertainty, and it is important that we try and quantify the uncertainty so that a proper assessment of risk can be made. Furthermore, we need to improve the analyses in the down-stream region as much as we possibly can. We are working at that right now.

If we go farther back on the probe to the probe base area, again we depend almost entirely on experimental numbers for base heating. That is not anything that is really going to make or break a mission, but there is a lot of area back there and the heat shield weight is significant. So, again, I think we are faced with an uncertainty and it is important that we try and quantify that uncertainty.

In general with regard to heating, if we find after trying to quantify uncertainties, that the risk looks pretty large, it might make sense to try and get some experimental data. The only way I know to do it now is a flight experiment, and that could be very costly. So the risk-cost trade off is a very serious one.

It is interesting that, for the Viking mission, where the heating is not very severe and where ground facilities are adequate, the Viking people are putting a 1.5 factor on all of their heating predictions. If we start putting a 1.5 factor on heating predictions for the outer planets, we are liable to put ourselves out of business. And yet, the uncertainties are probably going to be a lot greater for these outer planets than for Mars. So, again, it is extremely important that we try to quantify these uncertainties.

In addition, we need to perform a number of parametric studies over the range of possible atmospheres. All we have looked at are a small family of blunt cones and Apollo shapes and the so-called model atmospheres. Furthermore, most of these parametric studies were performed some time ago. Now our prediction methods, while still far from adequate, are much improved. Perhaps through proper studies we can identify a better configuration.

With regard to aerodynamics, stability, of course, is an important problem. We want to know what orientation the probe is in at all times. We feel quite confident that we can guarantee a stable design although there are some problems having to do with large blowing rates, axisymmetric ablation, things of that sort, but they don't seem to be particularly serious. They are problems we are going to have to work out, but will not require any unusual effort.

With regard to performance, the Viking people say that they would like to know their aerodynamic coefficient within five percent in order to get good information on reconstruction of the atmosphere from accelerometer data. Here, again, I think with some work, with some studies in facilities that we already have, complemented by some analytical work, we can probably achieve that level of accuracy.

MR. SEIFF: Thank you. Inasmuch as there were very few results given in the meeting on heating on the probes for Uranus, I took the liberty of looking in some old publications that are in my office to get some numbers and I saw in a study that Mike Tauber did about four years ago a value of the mean heating rate of six kilowatts per square centimeter for a body somewhat blunter than the ones that are now being considered.

I think one of the McDonnell-Douglas people showed values equivalent to twenty-four kilowatts per square centimeter. These values are, by comparison with those that have been computed for Jupiter entry, quite modest.

DR. OLSTAD: But if you look at the worst case, the radiative heating rate goes up to fifty kW/cm^2 and that coincides with a nominal Jupiter entry. Now unless we learn that the worst case is highly improbable, we must design for it. Furthermore, we don't really know that the current so-called worst case is the real worst case.

MR. SEIFF: What does that worst case correspond to?

DR. OLSTAD: That is the cold dense atmosphere and a steep entry.

MR. SEIFF: What does that imply with respect to sixty percent helium?

DR. OLSTAD: The cold dense atmosphere assumes 60 percent helium by volume.

DR. NACHTSHEIM: The heat protection group organized their work into an assessment and recommendations and they also made an observation focusing in on the question of Uranus.

As far as the assessment went, there were five points that were made. The first one had to do with the characterization of carbonaceous heatshield materials. The group felt that the thermochemical prediction of graphite and carbonaceous material was predictable. Particulate removal could be handled within the range of our experience by applying a design factor. Two different studies have used a design factor of 1.3.

The third point under the characterization of carbonaceous material was that there was no agreed-upon particulate removal mechanism.

The second main point made in the assessment was that the silica-silica heatshield needs further characterization. However, it was pointed out that there is a wealth of knowledge on the convective performance of pyrex and quartz heatshields that dates back to the 1960's and that many missile radomes are made out of this material. This information should be looked into.

The third main point of the assessment was that all possible mechanisms of ablation and intense heating are not known at this time. They are undefined.

The fourth point under the assessment was that present facility capabilities exist to verify heatshield designs, on a small scale of course, for Venus and that such capabilities do not exist for the outer planets. In other words, Venus is the limit of our capabilities with existing facilities, at the present time.

The fifth and final assessment point was that our flight experience with radiation present is the Apollo experience.

There were six recommendations. The first dealt with carbonaceous materials. Under this topic, one point is that we should characterize carbonaceous materials at the highest heating level possible. Second, we feel that we should increase the laser power so that we can get larger heating areas. The third point under this main topic of carbonaceous materials is that we should combine the laser with an arc jet and get combined heating. The fourth point under carbonaceous materials would be that we should exploit graphite performance, and we should start studying the graphite-insulation system as a heatshield. Graphite by itself is not a heatshield material. It requires an insulator. Another possibility is to look into the concept of a hot bondline.

The second recommendation deals with silica-silica heatshields. There are several points under this. One is, development should continue. Second, the silica material should be exposed to the solar spectrum at high heating rates. There are some facilities that utilize the sun with huge arrays of reflectors to get heating levels on the order of six kilowatts per square centimeter. The silica material should be exposed to that environment. Third, another suggestion was to design a material to reflect laser radiation. In other words, the technology is understood to reflect visible radiation. Since our intense source of radiation is the laser, you should be able to demonstrate reflection at 10.6 microns if you understand the problem well enough.

The third recommendation had to do with a design philosophy. It was the consensus that we should exert every effort to verify heat shield design in ground-based facilities before flying a mission. That is the recommended design philosophy.

The fourth recommendation had to deal with the engineering flight experiments. We feel that these should be studied in terms of earth entries, looking at the Langley proposal of a rocket-launch experiment. And in the 1980's, possibly a shuttle-launched experiment should be considered.

Also, in the way of an engineering experiment a planet should be considered. What we suggest is to put the question the other way around. If you could optimize the heatshield design to go to Jupiter, do so; and then ask yourself what science could you take along with that. This would be a feasibility study to determine the engineering feasibility of sending a probe into Jupiter. The Jupiter entry engineering experiment would be comparable in cost to earth entry experiments. This is not unlike the Apollo experience. Before we put a man in the Apollo vehicle, a whole class of vehicles were flown. This suggestion says, "Let's build an engineering probe with modest science, demonstrate the feasibility, then have the elaborate science." There, we would be simulating everything in full scale. It is a serious suggestion.

The fifth recommendation is to continue development of the giant planet arc, and this is being driven by a Jupiter 1984 launch.

The sixth recommendation is to accelerate development of the giant planet arc, and this would be driven by the Uranus 1979 launch. At the present rate of development, it could not assist that mission.

Then, finally, we made an observation that the life style of the NASA entry technology personnel will change if the support of the Uranus probe increases for the 1979 mission. The personnel currently at Langley and at Ames are only skeleton crews compared to that which will be necessary to support the Uranus mission.

MR. SEIFF: The subject of communications is equally critical because without communication all is for naught. So, Terry, would you give us your appraisal of that situation?

MR. TERRY GRANT: I think the first item that can be derived from our splinter meeting is that, by virtue of the absence of discussion, we should conclude that there were no problems uncovered in the Probe-to-Bus communications for a Pioneer Saturn-Uranus mission with the present science requirements. In other words, the baseline design with the ground rules that were originally given does not appear to have any technology problems associated with it. If new science requirements are added, however, the baseline design will have to change. The first requirement and the one which was discussed most was the requirement for pre-entry transmission. The consensus at the splinter meeting was that the communications required for this could be accommodated, but that it is impossible for us to assess at this point the complexity of that communication system, or the costs related to it, until we have some more details about this requirement.

For instance, we really need to know what kind of frequency stability is required for pre-entry transmission, since one of the criteria for an experiment using pre-entry transmission is to measure the electron density along the propagation path.

Also, we need to know what data rates are required. If it is postulated that there is a small amount of science and it has a low data rate, this pre-entry transmission might be relatively easy to accommodate.

Of course, an important parameter of pre-entry transmission is the time required. The transmission time and the data rate are more related to total system requirements than to communications. Once you build a transmitter it can provide transmission time in direct proportion to the battery and thermal capacity of the probe.

That was one point that we wanted to emphasize; that the pre-entry transmission is also a systems requirement and that it would impact the systems design as much or more than communications. Therefore, trade-off studies of the complete system are required in order to come up with an efficient new baseline design.

The other point with regard to science requirements was that there seemed to be an indication that additional scientific data would be required during the descent portion of the mission. This, again, would impact the baseline design for communications.

MR. SEIFF: What, specifically?

MR. GRANT: Well, I was thinking specifically of the interest in the gas chromatograph and I can see that the data rate originally defined is likely to be considered sparse if the gas chromatograph is an added instrument.

I point this out because while the baseline design accommodates the relay link at 44 bps, it doesn't do that with a large amount of margin. Furthermore, the baseline design cannot be extended very far to accommodate higher data rates by simply adding power, for instance. It will require extensive re-design if we require much higher data rates.

Going on to particular comments relative to the Uranus mission with a MJU probe, it is important to realize that the commonality considerations in this baseline design keeps it from being optimized for a Uranus mission, particularly for a Uranus mission with a Mariner-Jupiter-Uranus/probe.

First of all there is no turbulence proposed in the modelings for the Uranus ionosphere, or atmosphere. Therefore, we might achieve more efficient communications by going to a phase-modulated signal rather than a frequency-modulated signal as we have now.

Secondly, with the Mariner three-axis stabilized vehicle, the use of the pointing antenna would make a higher carrier frequency more optimum; I think Tom Canning or Byron Swenson pointed this out earlier. We recognize that a commonality of communications design for outer planet entry probes does make the design sub-optimum for a Uranus mission.

Another point that came out perhaps more rapidly than we would have liked was one that Kane Casani brought up in another presentation. That is, there are conflicts between the flyby bus and the probe priorities and they showed up in the papers that were presented; particularly, in the paper that was presented by Paul Parsons. There are a few interface problems that show up immediately. One is that the optimum probe antenna beamwidth for the presently-envisioned Mariner-Jupiter-Uranus trajectory is wider than the probe beamwidth that we have in our baseline design. This problem is not inherent in the Uranus mission but it is inherent in the considerations that were given to the Uranus trajectory. I believe the trajectory was set up so that the bus science would be free to operate without interference from probe transmissions during the closest approach to the planet and, therefore, the probe communication range and aspect angles were non-optimum.

Another interface problem relates to the allowed storage on the bus for probe data and the rate at which probe data can be relayed in real-time to the Earth. If bus storage up to a million bits and real-time transmission of 264 bps can be allowed, an efficient code can be used for the relay link by taking advantage of a complex decoder on the ground. However, if the storage and transmission rates are appreciably less, decoding on-board the bus may be required, resulting in more weight and cost for the probe communications subsystem.

The other factor that requires a technical decision on the interface is whether or not some amount of antenna steering should be provided for the relay receiving antenna on MJU. The current baseline for the MJU bus is to have a fix-mounted antenna. So here again we have an interface where, obviously, from the bus point of view a fixed antenna is desirable but if you look at the overall mission priorities you might want to allow the antenna some degree of mobility in order to optimize the relay link.

The last factor is one that goes along with what I said earlier, that the baseline as it now stands does not have much margin for increasing its capability. There is a possibility, however, that within the next year further information on the turbulence models for the outer planets, and also on the expected modem and coding performance, could conceivably improve the link capability over what we now use as our baseline. I think that there will be new information incurred in the short run that will bear on the baseline design for communications.

MR. JOEL SPERANS: The Science Instruments Group, by contrast to what I have been hearing the last few minutes, tended to take a very conservative point of view with regard to the outer planets missions.

We concentrated on the baseline programs and I think at this point we would have to say we will give Terry Grant very few communications problems of the sort that he suggested.

The opinion in general was that we should concentrate on doing one job and doing it well, and that the baseline job in this case is the lower atmosphere. From that it followed that we felt that by a combination of atmosphere-structure experiments and a combination of mass spectrometer and gas chromatographs, both of which are in a fairly high state of development at this point, we could do a pretty effective job with the payload capabilities that we have available to us today.

We did consider a number of specific problems in areas in which more money and more effort should be put. In general, they are relatively minor. Certainly more emphasis needs to be put on the study of the problem in operating in a helium environment and pumping helium in the mass spectrometers. These studies are being funded now, are going on and appear to be very successful. The consensus was that this did not represent a great problem in the long run.

An issue that has not had much emphasis put on it so far is the question of survival and operation of some of the basic instruments after a shelf life of seven years. Most of our instruments are ready to fly but they are not necessarily ready to fly all the way to Uranus. It is going to take a while for us to be sure that after seven years of sitting around on a spacecraft, or on the shelf, these things will operate in a way in which we can understand them. Again, these aren't expensive tests but they are tests which I think should be initiated very quickly.

I think the most significant outcome of our discussion was the emphasis that we all place on the need to put more time and more consideration into the application of the gas chromatograph family of instruments into the outer-planet instrumentation.

We would like to enthusiastically endorse the removal of the stigma of the so-called "ten-bar probe" that we see on a lot of the documentation which seems to be coming out of Ames and a lot of other places in the last few years. In the view of the instrument people, this is not a ten-bar probe; it is an outer-planets atmospheric probe and we will get information as far down into a planet's atmosphere as the spacecraft can provide us with communications.

There are one or two other minor tests that we would like to see; that we would like to endorse: such as the trade-offs between pressurizing the entire vessel or spacecraft versus trying to build instruments that can operate in unpressurized atmospheres. These are things that should be undertaken and will be undertaken in the near future. I don't think they represent large investments of money or talent.

Other than that we felt that the basic instrumentation for the lower-atmosphere science was in pretty good shape. Certainly by the time the instruments fly on Pioneer-Venus we will be in very good shape in those areas.

Because of its composition, this particular group, felt that it did not really have the mandate to consider to any great extent the apparent lack of emphasis to date on the middle atmosphere measurements. Larry Colin brought this out quite effectively in his opening remarks and I am sure Don Hunten too would emphasize these to a great extent. We haven't paid sufficient attention to the problems of making measurements in the so-called middle atmosphere.

One possibility for doing these in a low-cost way is the shock-layer radiometer or some derivation of it. This instrument is reasonably well-developed and reasonably inexpensive, but again, we did not feel this to be within the province of our particular group. Although we are not endorsing it strongly at this point, we feel that a lot of serious thought should be given to considering the shock layer radiometer as a fairly low-cost, easily-accommodatible addition to the outer-planets payload.

I think that about concludes what we discussed.

MR. VOJVODICH: Did your instrument group address the operational question of penetrating heat shields and getting a resultant clean sample of gas to analyze?

MR. SPERANS: Yes, we did. We discussed that at some length. The reason I didn't mention it was that it did not appear to be a problem. We discussed several options: several ways to do it. In general, if we can poke a big enough hole through the heatshield and get a decent size sample to carry enough gas inside to where the gas chromatograph and/or the mass spectrometer can operate on it, the problem of working through the heatshield doesn't appear to be formidable.

MR. SIEFF: Okay, thank you very much, Joel.

MR. SEIFF: The next technical category is that of Special Subsystem Design Problems which, in our meeting here, turned out to be primarily sterilization and radiation effects. Ron Toms of JPL will give us the summary group report.

MR. RONALD TOMS: Well, in fact, the session we had did not include a splinter group meeting. We had such a diversity of topics that it didn't seem particularly appropriate to break out into a splinter group.

The particular topic of planetary quarantine is one, of course, that has been worked on a great deal. We started off by hearing the ground rules of the game that we are supposed to play. Next we heard about the way in which we would do quarantine for the outer planets, and the effects on probe design. Then we heard a horror story of what Viking has to do to meet the kind of requirements imposed upon Viking. We don't know the cost of that; and Viking is not, in fact, making an effort to keep the costs of providing planetary quarantine as a separate, recognizable item.

I think we are a bit comforted though by the hope that heat sterilization requirements of outer planet probes will be unnecessary. Those of you who were here on Tuesday morning and heard Dan Herman's statement of his position on this heard that (for the time being at any rate) in our mission designs, in our cost estimates, and in the way we plan the mission we won't include planetary quarantine, even though we will also do studies to find out what it would cost and how it could be implemented.

On the radiation environment and its effects, I think I could summarize best by saying that the MJS spacecraft is solving the problem for the MJU mission of what you do about flying past Jupiter to carry a probe that would go on an MJU mission to Uranus. A seven-year flight to Uranus, flying past Jupiter, would go by at $12R_J$ which is a fairly modest radiation dosage compared with some of the cases that MJS itself is looking at (which go all the way in as close as $5R_J$ and pass out to 8.5 or 9.) So as MJS solves the problem it will, in a way, get solved for Uranus. Nevertheless, the probe itself has to be designed to meet the particular environment.

The Jupiter entry is another problem, and a probe that goes into Jupiter will have to be designed to meet the environment which by then we hope will be much, much better known not only from the later Pioneer data but from the MJS data itself.

The other two topics we tackled were battery life and thermal design: battery life for a seven-year class of mission and thermal design for the kind of conditions met in going out to the outer planets. Some significant problems were stated, and some adequate-looking solutions were discussed and given quite a good airing here.

I have a couple of comments on the MJU mission itself. It seems to me that it clearly is time to open up the probe-science question and then to optimize the probe design for the Mariner as a probe carrier. The other item is that I feel it very important that you all recognize that the MJU performance was not well reflected in the draft document that was sent out to everybody. I don't want anyone to go out from here thinking that MJU mission carrying a Uranus probe can only be flown off the shuttle, so that won't be happening in 1979. The performance capability is available with the Titan, and corrections of the document will be made before it is used in presentations to the SSB, OMB and Congress.*

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*(Updated information has been received and included in the August, 1974 issue of the document "Atmospheric Entry Probes for Outer Planet Exploration - A Technical Review and Summary" Ed.)

MR. SEIFF: Now that brings us to the cost session, which was the most recent one this morning, and Nick Vojvodich will summarize that.

MR. NICK S. VOJVODICH: Since the cost session was held so recently, we changed the order around and our splinter group actually met before the general meeting. We had about an hour and all the cost session speakers sat around the table and dissected program cost estimating from the standpoint of whether it is a black art or whether it is a science or indeed a combination of the two. I have some random thoughts that I jotted down during the splinter session that might be of general interest.

One of the reasons we had so many questions at the end of the open session presentations is that, as Steve Georgiev of DYNATREND was saying, in technical areas some people always feel uncomfortable; however, when it comes to cost, everybody is an expert. That observation was reflected in both the nature and extent of the comments and I hope we get into this cost area a little bit more as the discussion that is to follow this round-table summary develops.

One of the critical points that was made during our splinter discussion by all speakers was that low cost methodology must truly be specified at the beginning of a program. That is a procedure must be set up to: monitor and to control the costs; reduce the required paper work; and minimize tests and development costs wherever possible. Namely, achievement of low cost goals is not obtainable by applying cosmetic changes to a "business as usual" approach.

Another important point that was brought up is that inherent in the traditional way of looking at the cost-weight sensitivity of a subsystem namely, the cost of subsystems grow with weight - is that the functional performance also usually goes up.

We are in a situation now, though, that if a system has excess weight capability, and if, in fact, low cost and design-to-cost are constraints, fix the performance requirements and take advantage of the weight contingency to realize the cost savings. This is opposed to the historical approach of letting somebody come in and say, "If I could only get two more bits of data," or, "If I could only have one more sensor or more dynamic range capability." Probe entry systems are not linear so that a small change in one subsystem tends to perturb the system as a whole, and you have an uncontrollable growth situation. As somebody once said, "sometimes the spacecraft is growing so fast that one wonders if the launch vehicle will have enough boost capability to get it off the ground."

The question, of course, of inheritance was addressed during all of the talks and it is at this point that we get a direct interplay between technology and cost in some of the areas we were discussing earlier. John Niehoff of Science Applications Inc. emphasized that programs which push the frontier of technology run the risk of encountering potential problems that may require a substantial number of additional tests and thereby become susceptible to significant cost overruns. Therefore, early attention to technology development and assessment and working the identified problems by doing the appropriate SR&T, can significantly impact the program cost, schedule and technical achievement.

Specifically, in the area of the heat shield, we recognize that there is a quantifiable risk that one can handle by application of a conservative margin of safety to the design. Regarding this point, Fred Bradley from McDonnell-Douglas made the observation based on his participation in a number of previous successful flight programs ranging back to Gemini and Apollo, "we've never really started a program where we have had all the technology in hand. We have applied engineering judgment where appropriate and used some of the available weight contingency as a factor of safety and thereby eliminating the necessity of having to go down to the last five percent or ten percent in either the prediction or the simulation of the heating environment." I am sure that we will get into a discussion of that philosophy a little bit later.

From the standpoint of the track record of these costing models that are used in project funding estimation, it appears that by and large they generate predictions that have been found to be within twenty-percent of the actual costs. That was more or less an established goal of these cost models. But if we are really trying to do business in a new way, one wonders whether we should continue to use these cost-estimating models which essentially are mirrors that reflect the past. So this point was also brought up, that we've got to make sure that the cost estimates are realistic, especially the early ones.

I want to close by emphasizing my last statement. That statement coincides with a comment that Dan Herman previously made at the end of the meeting; namely, the early cost estimates, made in a phase zero, or pre-phase A, are most often the costs that both the program manager and the contractor have to live with. It is, therefore, extremely important that the cost people interact with the technical people particularly during the formative stages of a program and get a good, solid, definition of the system so that unexpected surprises are not encountered as the program develops.

The key word here to categorize this aspect of the cost situation is one of credibility. We have to develop a funding estimate that is not only credible but one that is also realistic in terms of existing technology.

That's the end of our cost-session wrap-up. It was a bit disjointed but I feel that it accurately reflects our thoughts. I am hoping that John Niehoff, Fred Bradley, and Bill Ruhland will add to the follow-up discussion.

MR. SEIFF: Now we come to John Foster who is in the enviable position of not having heard the meeting, but being asked to comment on its conclusions.

MR. JOHN FOSTER: I have two points I would like to make from the Ames' management standpoint and, particularly, from the Pioneer view point.

The first point is that we are interested in probe technology because we are interested in future probes. As you know, we are in the middle of the Pioneer-Venus probe mission and Ames and JPL are both looking into outer-planet probe missions. I would like to clarify at least one point on that. There was a recent article in one of the aerospace newsletters that said that NASA plans to do all their outer planet probe missions using the Pioneer Venus spacecraft. It is not true, for a number of reasons. First of all, the Pioneer-Venus probes are 100-bar, hot probes. It is a different mission than the one that we are talking about, which is around ten bars, and at different temperatures. I want to assure all contractors that this is still an open ball game.

The last thing I would like to say is that it is my observation that the time is ripe to look forward to the outer-planet probes, and particularly the Uranus probe. Certainly JPL and we, and I am sure many other people, are very, vitally interested in this coming mission.

MR. PAUL TARVER: John Foster narrowed his comments to three points and I am going to narrow mine to one. If I may, I'm going to deviate a little bit from the chairman's admonition to stick to Uranus.

This is something that has rather strong programmatic implications both as to mission sequence and our SR&T planning for the whole series of outer-planet-probe missions.

You probably noticed in the mission model that Dan Herman showed that the Jupiter-probe mission is scheduled for 1984. This decision was made with the advice of the scientific community, not because it ranked below the other planets in terms of science interest but on the basis of when it was estimated that we'd have the technological capability to do it. This estimate was based on our prior estimates of the nominal or the less favorable Jupiter atmosphere and ephemeris accuracy that was available.

Now, as a result of Pioneer 10, the improvement of the ephemeris and the possibility of a warm, expanded atmosphere, in some respects opened a Pandora's box, which should be opened. There is no complaint about that, but undoubtedly we are going to get pressure to bring a Jupiter-probe mission off sooner. We need to have some better facts, some better assessments than we have now as to whether this is a practical thing to do.

The present structure of outer-planet-probe sequences, is based on the development of a common Uranus and Saturn probe with the first Uranus probe on the MJU, followed by a Saturn probe later.

The question now arises, can we do a Jupiter-probe mission using Uranus/Saturn probe technology? If we can, then I am sure many people will want to do a Jupiter-probe mission sooner.

So, I am making a plea for this: that we do what can be done to get as much narrowing as possible of the uncertainty estimates in the environmental parameters that are involved.

Then, based on that, an assessment in as much depth as we can, of the feasibility of doing a Jupiter-probe mission with Uranus-probe technology. And deriving from that an assessment of the risks involved if we attempt to do a Jupiter probe mission that will employ common technology with the Uranus/Saturn probe.

Obviously, this has to wait for further verification from Pioneer 11. But, when that is available, then I think we need to do the studies to attempt to quantify insofar as we can the risks that would be involved so that we can make the necessary decisions whether it is feasible to move up the Jupiter-probe mission.

MR. SEIFF: We have now reached the point where we are ready to involve the audience in the discussion. We have gone around the table and now is there anyone out on the floor who would like to raise any questions?

MR. NICOLET: I would like to address this comment to Walter Olstad about the heating between the worst case of Uranus entry and the Jupiter nominal situation. If you were comparing the maximum heating levels which occur at one point in time as you enter, in fact I think that is comparable to the maximum heat levels for the Jupiter entry, but that is only a fair comparison. If you look at the Saturn warm entry to explain the worst flux, which is maybe only 5,000 kilowatts per centimeter square, the requirements on the heatshield are almost as severe as for the Uranus probe with its terrible helium content. The point is that the time requirements are there and they are very important; and for either Uranus atmosphere, the heatshields are only slightly different and the requirements on the heatshield are a lot less in the Jupiter case.

(NOTE: The following notation dictated by Mr. Nicolet after the round table session).

My comment was with regard to Walter Olstad's analogy between the most severe Uranus entry heating condition and that for the nominal Jupiter entry. The comparison was between the maximum heating levels which would be encountered at one time on the trajectories, that is the maximum heating levels for an entry. That is not an entirely appropriate comparison as the time integrated heating pulse more directly bears upon the required heatshield thickness. For example, the entry into the Saturn warm atmosphere encountered a heat flux no higher than about 5 kilowatts per centimeter square. However, the heatshield required for that condition was almost as great as that for the Uranus cold dense entry where the maximum heating levels were roughly 50 kilowatts per centimeter square.* (End of dictated notation.)

DR. OLSTAD: There are two aspects to the problem, and one is the total heat load. And certainly, for Uranus, it is considerably less than what it would be for Jupiter and, as you say, a shallow entry into the Saturn warm atmosphere is a severe case. The other aspect is the heating rate and we don't know what is going to happen to a heat shield when it is exposed to very large heating rates. We aren't able to produce these conditions in ground facilities at the present time, and until we have some experience,heat shield behavior will remain a matter of particular concern. So the heating rate is an important factor. Current estimates of heat shield weights for outer planet probes are based on the assumption that the heat shield materials will respond to heat loads in the same way the Apollo heat shields did. This is a very crucial assumption. If we find that heat shield materials respond in a different way to large heating rates than to the smaller rates of current experience then our estimates of heat shield weights may be seriously in error.

MR. SEIFF: One comment that I think Nick made was very interesting to me, and that was to point out the fact that on many of the earlier missions that we have undertaken the uncertainties have been very great.

When John Kennedy stood up in 1960, or whatever year it was, and said, "We shall go to the moon," there was nobody around who really knew that we were going to go to the moon.

So uncertainty in the projections of future missions is by no means a new thing. And, really, what usually happens is that people rise to the challenge. Once the planning is made definite, people rise to the challenge and they do the job that has to be done. I would fully expect the same thing to happen here.

MR. SEIFF: Ron, you have some remarks?

MR. TOMS: I wanted to raise some points where I think the Mariner mission has really not been well understood by this group. In particular, the question of what you do about communications. Now, in flying the Mariner spacecraft and being able to use a body-fixed antenna with an extra five or six db gain, the first thing that you can use the extra db for is to move from the dark-side entry to the light-side entry, which is what the atmospheric physicists particularly want. Flying around on the right side of the planet instead of the left side also allows you to get a very high escape velocity from the solar system, which is what the inter-galactic investigators want.

The next candidate for using some of that db gain is to not have to fly by at some specially-optimized flyby distance from Uranus but to have flexibility, for example, from about 2 to 4 R_U.

And the third thing you can use it for is a somewhat higher data rate, if there is any need on the part of the scientists to increase the data rate above the one that's now being looked at.

A fourth thing, then, is that of taking the probe data a little earlier in order to get better pictures. That doesn't mean to say that one can't take the data at the same time as was previously planned, but if you have the extra db gain then you can optimize a best combination of probe data and picture data.

A fifth way to use that extra gain would be just to lower the probe power by perhaps a factor of two. So there are all those candidates.

Then, there is another way of increasing the db gain in this data link and that is to move to a higher frequency. There is no suggestion that Mariner wants a higher frequency. It doesn't need it, but it would be another point of gain that one could make to move up to 860 kHz or thereabouts.

Now, there were some remarks, too, that puzzled me about whether or not we knew we could deploy a spinner from a three-axis stabilized spacecraft. Certainly we can. There are a couple of very good designs; both of them adequate and both of them quite inexpensive and not costing us very much in weight. There were some numbers in the handout (the Ten-Bar Probe document) which talked about it costing 70 kg to be able to incorporate the probe on the Mariner. It must be a typographical error. It only costs about 10 kg for all the additional things that one would want to do to the spacecraft, including putting the relay-link antenna and receiver on it, plus about 25 kg of propellant for the additional maneuver. The tip-off conditions have been looked at and they are relatively modest. We are even looking right now at a way of getting very, very close tracking of the probe by simply turning the imaging system on to the probe as it leaves the spacecraft. There we would get a very precise way of monitoring the probe trajectory and extrapolating to accurate entry conditions.

I want to take issue with something that Tom Canning said, on a quite different topic. Tom, you said, I think, that you wanted the Science Advisory Committee to be turned off and to have a frozen position on priorities (when the program begins). That would be a disaster for a mission of this kind.

MR. CANNING: I was just trying to avoid those major surprises once one starts the program.

MR. TOMS: I think that is right, but you see there is always the danger there that we either fly the wrong mission or we propose to fly the wrong mission and get turned down because it is the wrong one.

And I think that continuing the Science Advisory Committee at full strength all the way through, is important. No more messing around with AMDO's and all that sort of thing.

MR. CANNING: On the other hand, if you want to control costs, as we are going to have to do, if we make major changes on the demand of the system part way through a design, well, I don't have to state the obvious.

MR. TOMS: No, but we must always be ready to.

MR. CANNING: Even that is expensive.

MR. JIM HYDE: I have a comment. There is a very specific thing to be considered here. For some time Ames and a number of industrial contractors have been studying the probe that we are talking about. Out of that has come a reference payload capability. However, the interaction of these efforts with the science community has not crystalized in the same way that the interaction is now crystalizing with the MJU Science Advisory Committee. I think what has happened is we find ourselves looking at the reference payload as being <u>the</u> payload for this mission. Let us not do that. Let us wait until we get more specific inputs from the science community.

I also heard some very interesting stories about different mechanizations on the mass spectrometer, and it is, obviously, a very interacting instrument with the probe system design. Let's wait until we get the real inputs from the science community before we settle on the specific design of the Uranus probe. I think we need this interaction and I think that we'd be playing the wrong game not to let the scientific community give us their best inputs and their druthers, and then let's look at the probe design and see how best we can accommodate their desires. I think that is what Toms is pushing here.

MR. VOJVODICH: I would like Larry to speak to that issue.

DR. COLIN: I certainly endorse the idea of science groups continually reviewing the situation. We have been pushing for that sort of thing and it hasn't occurred yet. But I am hoping that Ichtiaque Rasool will get it rolling. As far as the model payload is concerned, it is in very fine shape. I personally doubt that there are going to be significant modifications to it.

MR. SPERANS: I think there is a misunderstanding here. I think that if anyone thinks that this payload was derived by a few people from Ames and a few contractors sitting in a back room and deciding what would fit into a probe, they are very much mistaken. We have had interaction with the science community right from the very start, dating back four or five years. We've had science advisors representing a cross section of outer planet scientists all along. And it has been their input which has dictated the sort of payload that we are talking about today. The implication that we have been working without this sort of thing is in error. There is only one difference between this and MJU and that is that as yet we don't have a formal Science Steering Group. And the reason for that is programmatic and I am sure that when the time comes, Headquarters will set one up.

MR. SEIFF: There is, for example, the benefit of the entire process by which the Pioneer-Venus payload was defined, which is the usual excruciating process by which people submit - I think there were 180 proposals submitted to fly experiments on Pioneer-Venus and it got narrowed down to what is now an instrument count of thirty-three but there are actually fewer investigators than that. So that what is being done here is all of this experience is being factored forward. Now you do have to admit the possibility that the selected payloads to the outer planets will differ. But neither should what is being shown here be regarded as something that was selected blindly without guidance.

MR. HYDE: I don't mean to imply that. I was specifically trying to get to this point: Let's not kid ourselves and say that this reference design that we currently have is The Design. We have to remain open at this time.

MR. SEIFF: Yes, I am quite sure that when it is executed, it has to be done that way, because nobody would sit still for any other approach.

MR. SPERANS: Well at the same time we keep talking about trying to do low-cost missions and sooner or later we are going to have to face up to the fact that if you are going to do anything remotely resembling a low-cost mission, you have got to settle on some kind of a fundamental science objective and set out to do it, and stop trying to optimize it right up to the point of launch. I think this is one thing we are going to have to live with from now on.

MR. SEIFF: Howard has been trying very eagerly to get in.

MR. MYERS: I would like to make a few comments about upper-atmosphere versus lower-atmosphere instruments.

I wish to comment on the desire expressed by the atmospheric scientists for upper atmosphere measurements. Under contract to ARC, we studied the accommodation of upper atmosphere instruments to Outer Planet probes. We found that the installation of a simple instrument such as electrostatic probe presented no difficulty. Its data could either be transmitted in real time or stored for postblackout transmission. A neutral or ion mass spectrometer can also be added. However, the problems of calibrating an upper atmosphere mass spectrometer

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described in Dr. Nier's paper are aggravated for the Outer Planets by the high entry velocities. Therefore, in the Science Instruments Caucus, the three mass spectrometrists recommended that mass spectrometry be limited to the lower atmosphere. The most promising additional instrument would be a second rf transmitter; the use of two-frequency radio data in atmospheric characterization was discussed yesterday by Dr. Croft.

A second aspect of obtaining upper atmosphere data deserves attention, that of measurement time. The total time available for upper atmosphere measurements (that is, from onset of a sensible atmosphere at $10^{-7}G_E$ to $10^{-2}G_E$) is 20 seconds for a shallow Jupiter entry and up to 30 seconds for Saturn and Uranus! Therefore, the intrinsic value of 30 seconds of upper atmosphere data must be weighed against the increased complexity imposed upon the probe design.

MR. SEIFF: There is one point that was brought up by Phil Nachtsheim - that I would like to see aired a little bit because I think it is so sensible that it probably would be thrown out without consideration, and that is that since we have problems trying to define the capability of heatshields to survive Jupiter entry by any means here on Earth, one might conceivably undertake something very modest, small in size, carrying a minimum number of instruments and throw it off of some vehicle that happens to be flying by there, such as Mariner-Jupiter-Uranus. And not expect too damn much of it; just use it for a learning experience and if we are estimating forty-eight million dollars for this device, the question that comes into my head is what could be done with five? What could be done with five and how much of a leg up would it give us on this problem to take the risk out of the really more capable mission? Now I would like to hear other people's opinion about this. To me it seems exceedingly sensible.

MR. VIC PETERSON: Al, it is conceivable that with a sum of money much less than five million dollars we could accelerate the development of the Jupiter arc facility. This would enable us to simulate the entry environment here on the ground and be able to run the experiments over and over again rather than depend on a one-shot thing.

MR. SEIFF: That would be delightful if true, but I think Howard Stine's report to us was not one really bubbling over with optimism.

MR. PETERSON: He is trying to be realistic.

MR. SEIFF: He is trying to be realistic and what he is saying is if we can marginally obtain the conditions of interest and rather late in the game, and on a rather small sized specimen. But if your speculation were true, Vic, I think it would be the right way to go. Now I haven't seen evidence that it is correct. That's the thing that's bothering me right now. It looks to me like we can invest that same kind of money and still end up somewhat short of what we would like to have.

MR. PETERSON: It is true, though, Al, that you will always get something out of a facility. With a probe you have a fifty-fifty chance of getting nothing.

MR. SOMMER: If it fails you will get something; you will know that your design was inadequate.

MR. SEIFF: Does anyone else wish to comment on that?

MR. SWENSON: If you forget the launch vehicle, your five million dollars will be all right.

MR. SEIFF: Well, that is what I am saying, that this has to be a piggyback experiment on some other mission.

MR. NIEHOFF: I would like to give you a counterpoint to your five million, based on the forty-eight million that we talked about earlier. That was for three flight articles. And if you remove two of them, you are more like thirty-eight million. If you knock off all the science and all the communication, which is not reasonable - presumably, even with a test you want to get data back after you have entered to find out what has happened - you would knock off another seventeen million, so you are down to about twenty million.

Presumably, this thing would be smaller and there would be some savings associated with that; but I still would have to believe that five million is probably unacceptably small.

In fact, I would propose that we start off with five and the way this meeting is going, we will wind up at baseline payload by just normal procedure.

MR. SEIFF: Yes, but you know how everybody's ruminations, it doesn't mean we are going to have -

MR. NIEHOFF: Be careful, seventeen million dollars of that is in communications and science.

MR. SEIFF: But you can shrink your communication system, too, because if you take out the major part of the science -

MR. VOJVODICH: That is his point.

MR. SEIFF: Is that your point?

MR. NIEHOFF: Yes.

MR. CARL HINRICHS: One should be a bit cautious in scaling the costs of communications systems. Regardless of the data rate or range, the link analyses must be performed, i.e., look angle and range histories, error assignments and modulation/coding investigations. Similarly the procurement cycle costs are somewhat invariant, i.e., assessment of EMC and vibration/shock/ acceleration environments and the associated testing costs. Even with the use of an "off-the-shelf" system, these same steps (costs) must be traversed, although hopefully with some of the steps deleted. It would be interesting to see Mr. Niehoff's data broken into recurring and non-recurring costs on a per link basis.

MR. SEIFF: I'm quite serious in being interested in that idea. I don't know whether anyone else feels that way or not, but to me it seems like a very real suggestion. Any other comments or questions?

STAN LIPSON: Will you make a few remarks concerning what role you see ESRO playing in the Pioneer-Jupiter orbiter mission?

MR. SEIFF: Larry (Colin) can you answer that, or John (Foster)?

MR. FOSTER: That is not an entry mission and I'd just as soon defer that, unless Paul (Tarver) wants to answer. That's a Headquarters problem at the moment.

MR. TARVER: This is one of several possible cooperative missions under discussion with ESRO. Conceivably, one role ESRO might play would be to convert the Pioneer H spacecraft into an orbiter with science instruments supplied by both ESRO and NASA. Again, this is just in the early stages of talking about it. But we have a Pioneer H spacecraft, and if this were to be furnished to ESRO, it could be converted into an orbiter. As to how a probe would be handled if there were a probe, this is totally unresolved.

MR. SEIFF: Was there another question? I think we have wound down. We have been going at it for three days and that point has been reached where nobody can think of anything else to say.

I would just like to say in closing that while I wasn't instrumental in putting this meeting together, I really feel gratified that it was held. I think that it had a number of very positive effects. Some people have been calling for closer interaction between scientists and design groups and we had that here.

I have attended meetings on both sides of that fence, but I have never been to a public meeting where there was really quite as much exchange as I have seen here.

Another thing that I thought was extremely healthy was the fact that we had contractors talking to each other. So we have had contractors and we have had Headquarters people and Center people and scientists all communicating with each other.

To me, the whole thing has been very much worthwhile. I don't feel sorry at all that I spent three days sitting here, and I hope the rest of you feel the same.

And with that, I will declare the meeting adjourned.

OUTER PLANET PROBE TECHNOLOGY WORKSHOP
NASA AMES RESEARCH CENTER

ATTENDEES

L. Acheson
Hughes Aircraft Company

L. J. Alpinieri
Aerotherm

J. Baxter
Martin-Marietta Corporation

S. Belsley
NASA Ames Research Center

W. Berry
NASA Ames Research Center

J. Blome
McDonnell-Douglas Corporation

L. Boyce
NASA Ames Research Center

F. E. Bradley
McDonnell-Douglas Corporation

W. B. Briggs
McDonnell-Douglas Corporation

T. N. Canning
NASA Ames Research Center

G. Carle
NASA Ames Research Center

P. C. Carroll
Martin-Marietta Corporation

E. Kane Casani
Jet Propulsion Laboratory

L. Colin (Dr.)
NASA Ames Research Center

R. E. Compton
Martin-Marietta Corporation

W. M. Congdon
Martin-Marietta Corporation

R. Corridan
NASA Ames Research Center

C. E. Cotoe
NASA Headquarters

K. J. Coughlin
Martin-Marietta Corporation

A. Covington
NASA Ames Research Center

W. D. Cowan
McDonnell-Douglas Corporation

T. A. Croft (Dr.)
Stanford University

L. P. Daspit
NASA Langley Research Center

R. I. Debs
NASA

R. E. DeFrees
McDonnell-Douglas Corporation

C. E. DeRose
NASA Ames Research Center

G. Deutsch
NASA Headquarters/RW

E. Divita
Jet Propulsion Laboratory

W. Dixon (Dr.)
TRW Systems Group

J. H. Duxsbury
Jet Propulsion Laboratory

L. Edsinger
NASA Ames Research Center

T. R. Ellis
DYNATREND INCORPORATED

ATTENDEES (Page 2)

L. Evans
NASA Ames Research Center

J. V. Foster
NASA Ames Research Center

L. Friedman
Jet Propulsion Laboratory

S. Georgiev
DYNATREND INCORPORATED

W. Giles (Dr.)
General Electric Company

J. Goodlette
Martin-Marietta Corporation

T. L. Grant
NASA Ames Research Center

M. Green
NASA Ames Research Center

J. Greene
NASA Headquarters/RV

A. Henderson, Jr.
NASA Headquarters/RC

T. C. Hendricks
Martin-Marietta Corporation

D. H. Herman
NASA Headquarters/SL

C. A. Hinrichs
McDonnell-Douglas Corporation

A. Hoffman
Jet Propulsion Laboratory

J. H. Hoffman (Dr.)
University of Texas/Dallas

G. R. Hook
Martin-Marietta Corporation

J. T. Howe (Dr.)
NASA Ames Research Center

R. D. Howell (Dr.)
Martin-Marietta Corporation

D. M. Hunten (Dr.)
Kitt Peak National Observatory

J. Hyde
Jet Propulsion Laboratory

P. Intrieri
NASA Ames Research Center

R. W. Jackson
NASA Ames Research Center

P. Jaffe
Jet Propulsion Laboratory

M. Jennings
TRW Systems Group

J. L. Johnson
Hughes Aircraft Company

W. Kessler
McDonnell-Douglas Corporation

D. B. Kirk
NASA Ames Research Center

A. J. Kliore (Dr.)
Jet Propulsion Laboratory

M. Kolpin
TRW Systems Group

S. Korn
TRW Systems Group

H. Kubota
NASA Ames Research Center

T. Kuo (Dr.)
Jet Propulsion Laboratory

U. E. Lapins
Hughes Aircraft Company

A. Lauletta
Hughes Aircraft Company

ATTENDEES (Page 3)

K. W. Ledbetter
Martin-Marietta Corporation

L. Leibowitz (Dr.)
Jet Propulsion Laboratory

J. S. Lewis (Dr.)
Mass. Institute of Technology

S. Lipson
General Electric Company

C. H. Liu
NASA Ames Research Center

F. R. Livingston
Jet Propulsion Laboratory

J. Lundell
NASA Ames Research Center

R. F. Lyon
Jet Propulsion Laboratory

L. A. Manning
NASA Ames Research Center

R. R. McDonald
Jet Propulsion Laboratory

R. K. McMordie (Dr.)
Martin-Marietta Corporation

H. F. Meissinger
TRW Systems Group

S. A. Mezines
McDonnell-Douglas Corporation

J. W. Modestino (Dr.)
Rensselaer Polytechnic Institute

J. Moore
Jet Propulsion Laboratory

H. Myers
McDonnell-Douglas Corporation

P. Nachtsheim (Dr.)
NASA Ames Research Center

W. E. Nicolet
Aerotherm Acurex

J. Niehoff
Science Applications, Inc.

H. B. Niemann
NASA Goddard Space Flight Center

A. O. C. Nier (Dr.)
University of Minnesota

L. J. Nolte
Hughes Aircraft Company

R. R. Nunamaker
NASA Ames Research Center

W. B. Olstad (Dr.)
NASA Langley Research Center

T. Owen (Professor)
State University of New York

V. Oyama (Dr.)
NASA Ames Research Center

B. D. Padrick
NASA Ames Research Center

N. J. Paradiso
Martin-Marietta Corporation

R. Park
Hughes Aircraft Company

P. Parsons
Jet Propulsion Laboratory

V. L. Peterson
NASA Ames Research Center

J. Polaski
NASA Ames Research Center

J. A. Polutchko
DYNATREND INCORPORATED

R. J. Polutchko
Martin-Marietta Corporation

E. T. Pounder
Jet Propulsion Laboratory

R. Powell
Jet Propulsion Laboratory

B. R[illegible]agent
NASA Ames Research Center

J. Rahlf
TRW Systems Group

I. Rasool (Dr.)
NASA Headquarters/SL

D. G. Rea (Dr.)
Jet Propulsion Laboratory

A. Reetz
NASA Headquarters/SL

E. W. Ruhland
Jet Propulsion Laboratory

S. Russak
Martin-Marietta Corporation

R. K. Russell
Jet Propulsion Laboratory

S. Sadin
NASA Headquarters/RX

R. W. Schaupp
NASA Ames Research Center

R. J. Schmitt
McDonnell-Douglas Corporation

H. M. Schurmeier
Jet Propulsion Laboratory

A. Seiff
NASA Ames Research Center

G. N. Sherman (Dr.)
Jet Propulsion Laboratory

S. C. Commer
NASA Ames Research Center

N. W. Spencer (Dr.)
NASA Goddard Space Flight Center

J. Sperans
NASA Ames Research Center

I. N. Spielberg
TRW Systems Group

D. S. Stephenson
General Electric Company

J. A. Stern
Bionetics

H. Stine
NASA Ames Research Center

H. Suer
TRW Systems Group

B. Swenson
NASA Ames Research Center

P. Tarver
NASA Headquarters/SL

L. Thayne
Martin-Marietta Corporation

R. Toms
Jet Propulsion Laboratory

S. Traugott
Martin-Marietta Corporation

N. S. Vojvodich
NASA Ames Research Center

G. D. Walberg
NASA Langley Research Center

A. C. Wilbur
NASA Ames Research Center

M. E. Wilkins
NASA Ames Research Center

R. S. Wiltshire
Martin-Marietta Corporation

F. Woeller
NASA Ames Research Center

J. Wolfe (Dr.)
NASA Ames Research Center