DESIGN-TO-COST

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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?

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"PROVIDE THE MOST FOR THE MONEY" (BEST BUY)


            OR


"ELIMINATE THE GOLD PLATING"


        BUT HOW?
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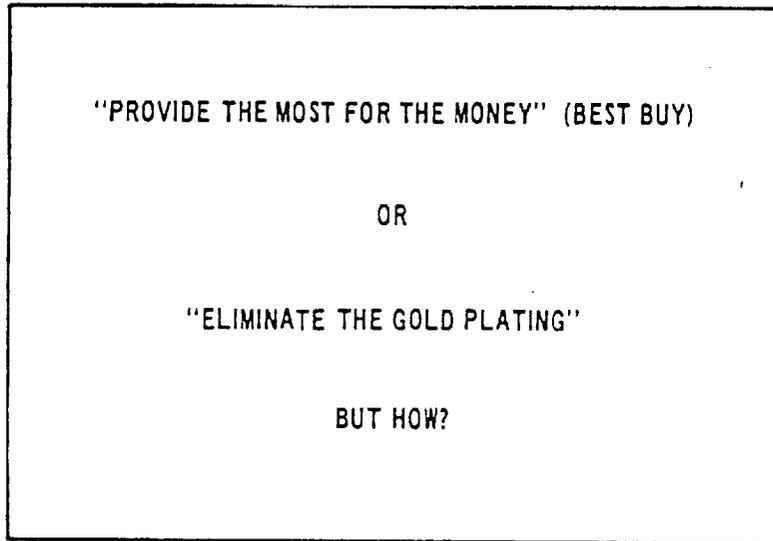
Figure 10-9. Design-to-Cost Intent

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.

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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
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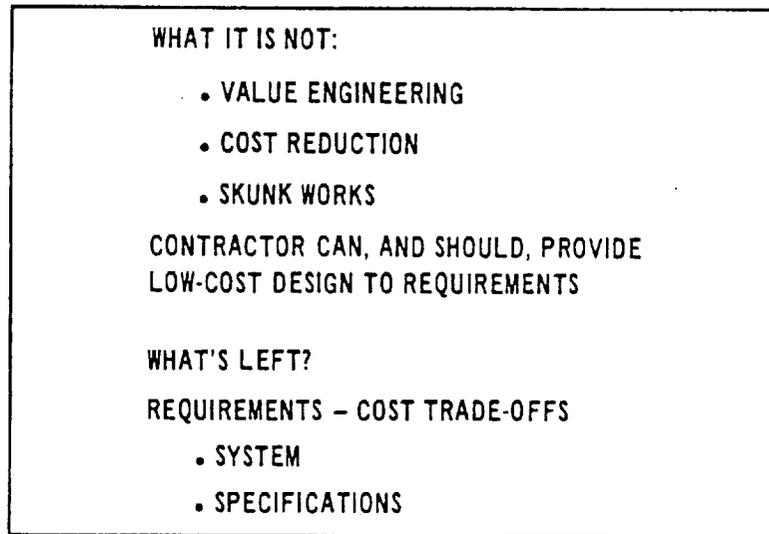
Figure 10-10. Design-to-Cost (DtC)

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.
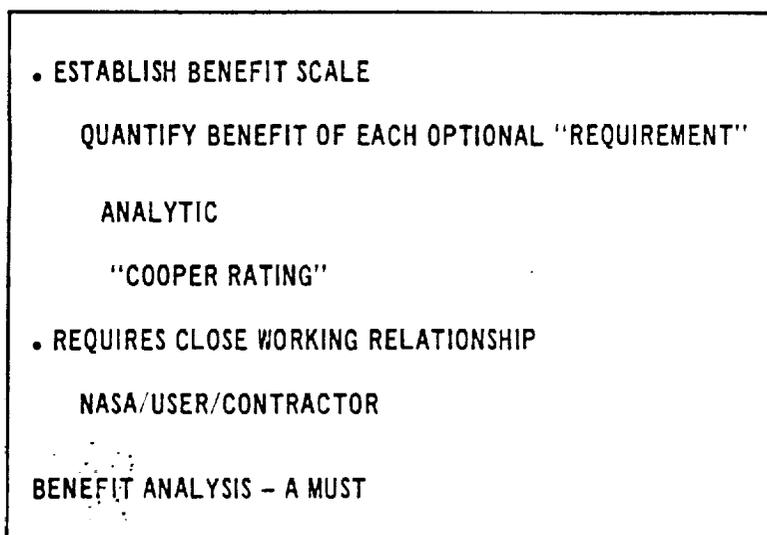
Requirements Analysis - Figure 10-11

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• MISSION DESCRIPTION

• NASA/USER/CONTRACTOR ESTABLISH

    MANDATORY REQUIREMENTS

    DESIRABLE "REQUIREMENTS"
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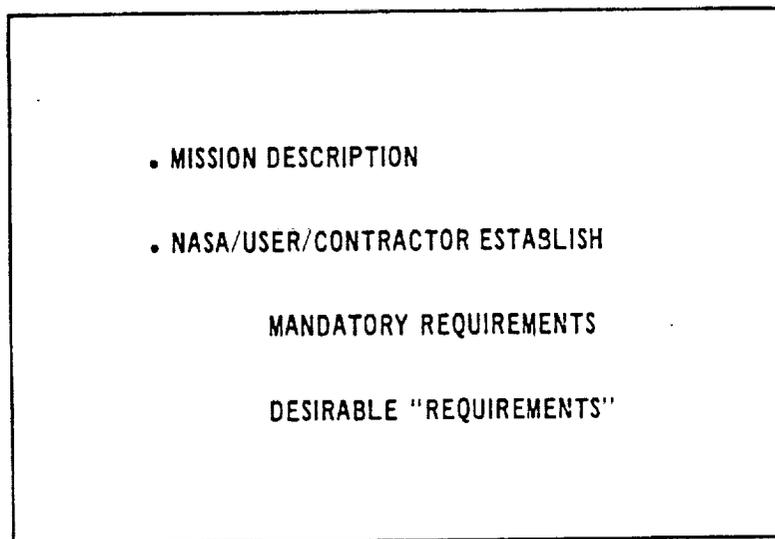
In the requirements analysis one starts with the mission description. NASA and the user, in the case of the probes the scientific community, and the contractor need to establish a minimum set of mandatory requirements: minimum requirements, mandatory requirements. Because to do any mission at all there 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 base-
line 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

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• ESTABLISH BENEFIT SCALE

    QUANTIFY BENEFIT OF EACH OPTIONAL "REQUIREMENT"

    ANALYTIC

    "COOPER RATING"

• REQUIRES CLOSE WORKING RELATIONSHIP

    NASA/USER/CONTRACTOR

BENEFIT ANALYSIS - A MUST
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    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 be-
tween 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

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• ESTABLISH AGREED UPON COSTING METHODOLOGY

     NASA/CONTRACTOR

• USED TO COST THE BASELINE AND COST INCLUSION OF
  OPTIONAL "REQUIREMENTS"

• ACCOUNT FOR INTERACTIONS
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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 addition-
al desirement. We have to account for interactions in that pro-
cess 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

- ORDER THE "CHANGES" TO THE BASELINE IN DESCENDING VALUE OF THE RATIO

$$\frac{\Delta B}{\Delta C} = \frac{CHANGE\ IN\ BENEFIT}{CHANGE\ IN\ COST}$$

- ACCOUNT FOR INTERACTIONS

- PLOT (OR TABULATE)

- VARYING BASELINE IN ORDER OF $\frac{\Delta B}{\Delta C}$ YIELDS "BEST BUY"
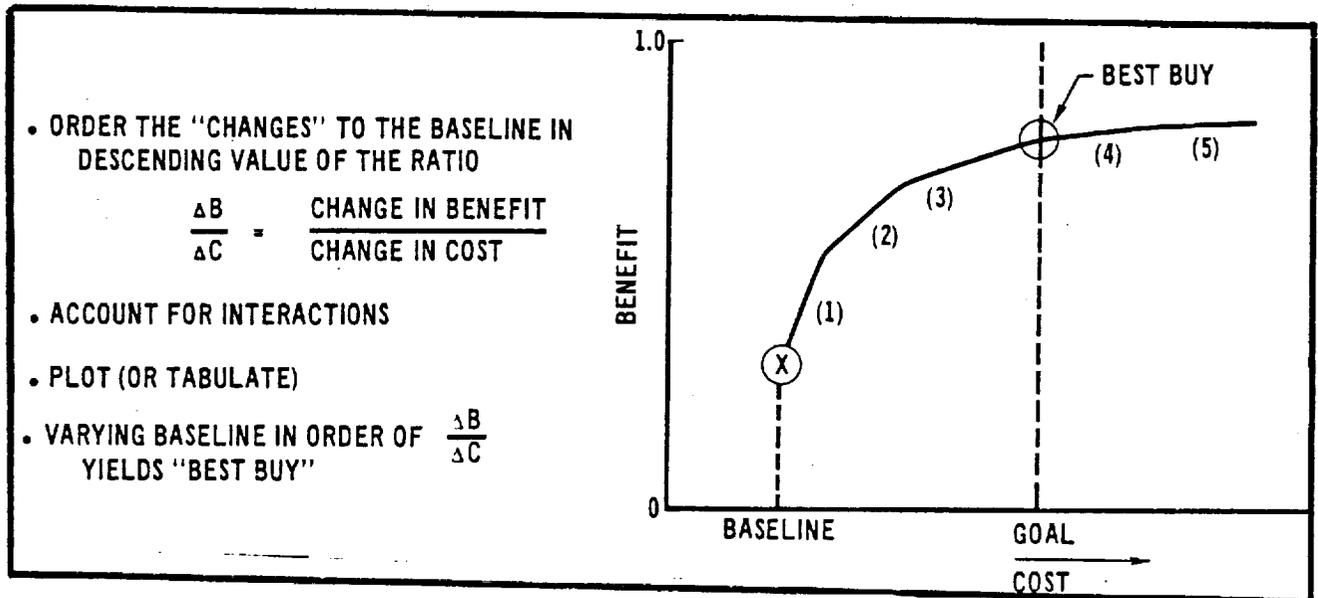
Figure 10-14. Benefit-Cost Analysis

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 trade-offs. 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

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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
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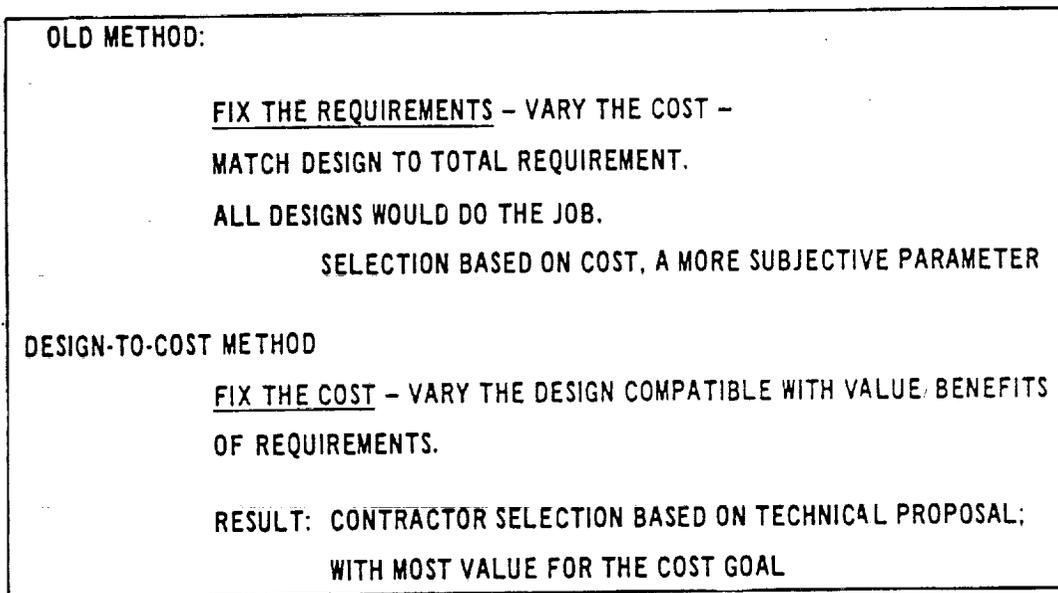
Figure 10-15. Potential Effect on Contractor Selection

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. Unfor-
tunately, 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 pre-
vious speakers talked about, given enough definition, enough un-
derstanding 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

Requirements Analyses - 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.

Minimum Baseline Design - 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 fail-
ures, 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

$$\text{BENEFIT} = \text{VIEWING TIME ON ORBIT}$$
$$= P \left(1 - \frac{R}{MMD + R}\right)$$

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 get information, 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.

UNIT COST OF TELESCOPE

$UCT = 0.111 + 0.000992 e^{(MMD)}$
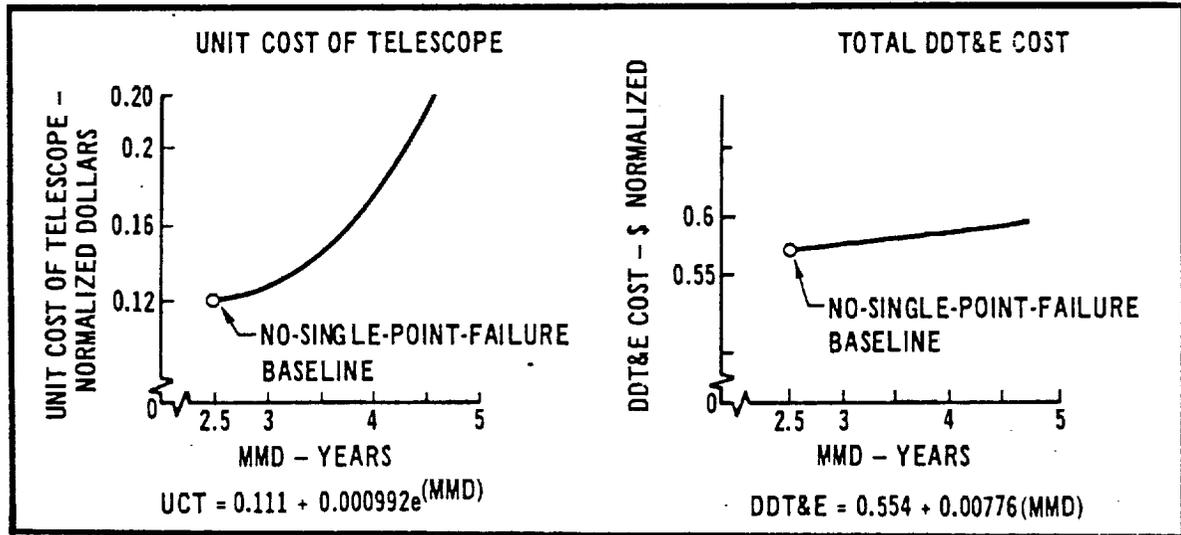
TOTAL DDT&E COST

$DDT\&E = 0.554 + 0.00776(MMD)$

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

$$LCC = DDTE_{BL} + \Delta DDTE + UCT_{BL} + \Delta UCT$$

$$+ \frac{P}{MMD + R} (k_1 [UCT_{BL} + \Delta UCT]) + (1 + \frac{2P}{MMD + R}) CPL_{SS}$$

$$= 1.46 + 0.00776 (MMD) + 0.000992\, e^{(MMD)}$$

$$+ \frac{0.7825}{MMD + 1/3} + \frac{0.00149 e^{(MMD)}}{MMD + 1/3}$$

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.
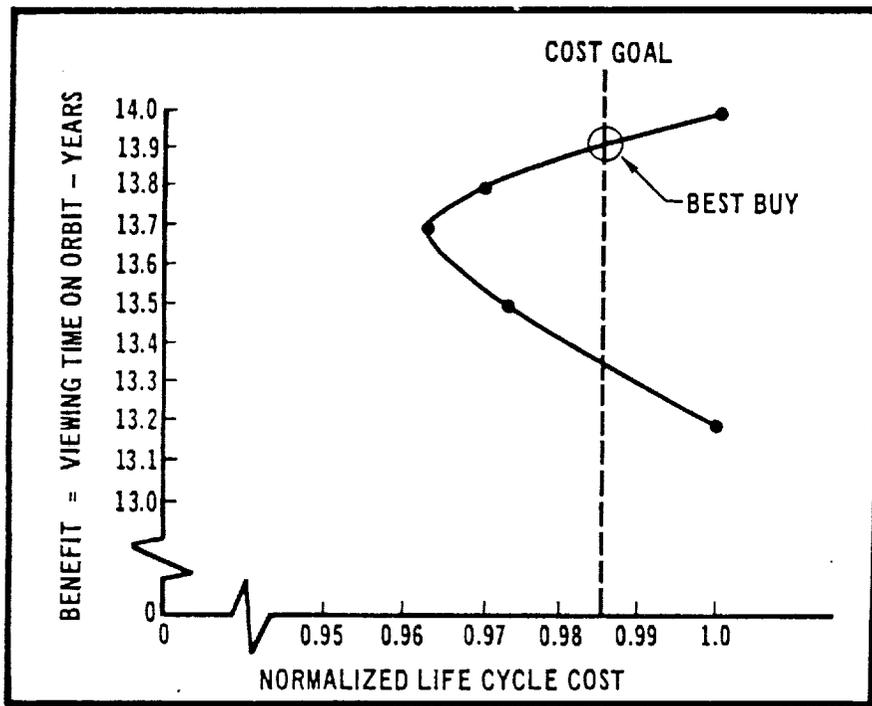
Figure 10-22. Locating the Best-Buy

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.

- 2 PROBES PER PLANET/SATELLITE
- CFE INSTRUMENTS
- JUPITER FIRST

CURRENT BASELINE*

WITHOUT PQ   WITH PQ

NUMBER OF PLANETS/SATELLITE VISITED

TOTAL PROBE SYSTEM COSTS (1973 SM)

*5 INSTRUMENT, 10 BAR PROBE 44 BPS DATA RATE, 800 g_E DESIGN DECELERATION, NON-SEPARATING HEATSHIELD
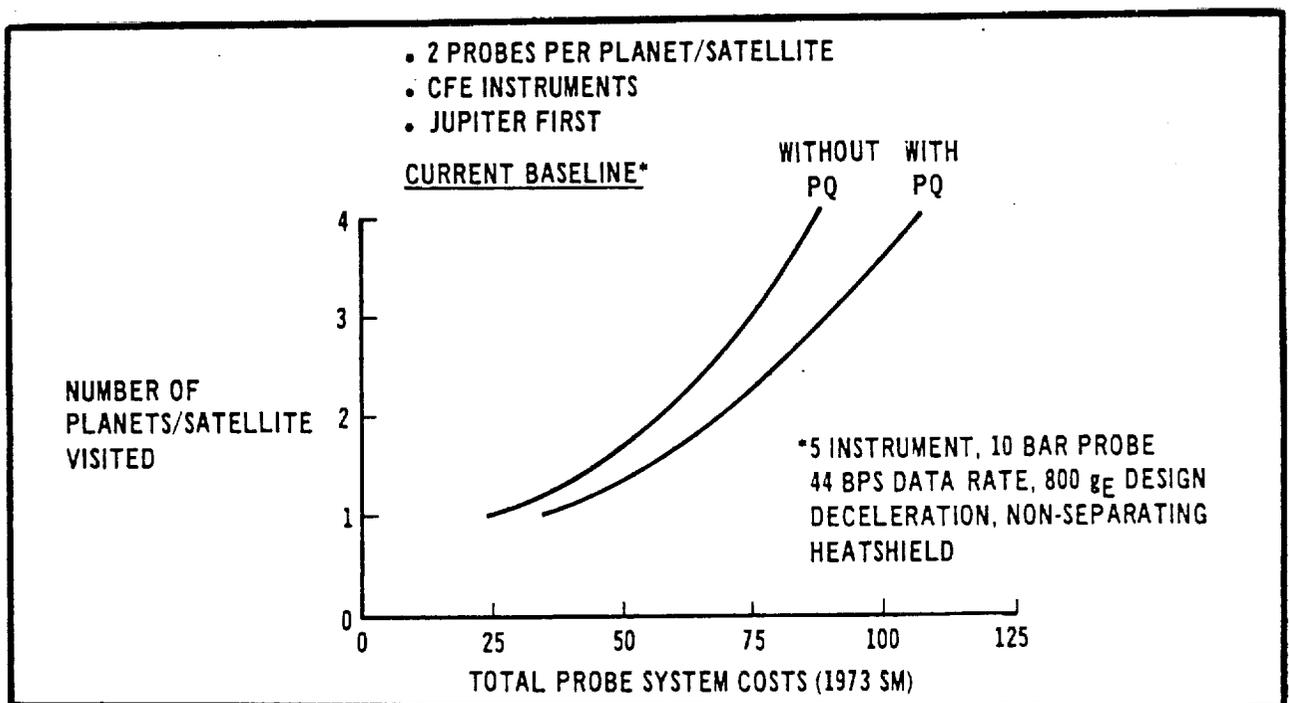
Figure 10-23. Potential Probe Applications

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
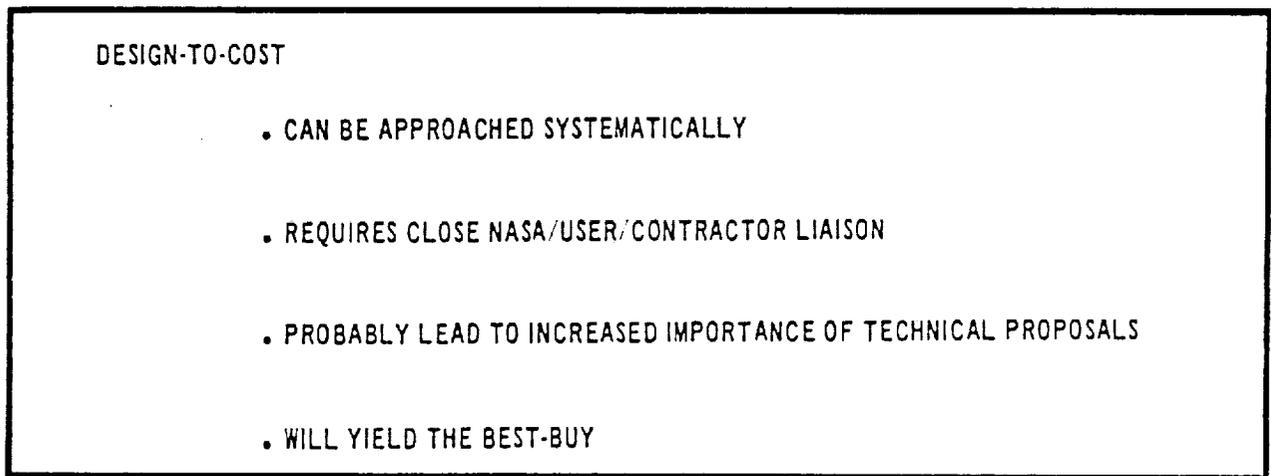
- WILL YIELD THE BEST-BUY

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. It's in the eyes of the beholder. 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, ab-
scissa, 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 differ-
ent 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 par-
ticular 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, specifi-
cally, 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 pre-
sented 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 consider-
ation 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 Develop-
ment 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 ses-
sion, 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 im-
portant 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 tem-
perature, pressure, and accelerometer instruments, plus the compara-
tive 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 con-
tains 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 spec-
trum 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, ac-
celeration 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 opti-
mize and balance the scientific payload against the spacecraft de-
sign with the viewpoint of keeping as low a cost approach as possible.

John Lewis made a special plea in the area of composition meas-
urements. 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 evolu-
tion 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 com-
ments 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 pro-
cessing 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 space-
craft; 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 plan-
ets, 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 cas-
cades 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 uncer-
tain 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 harden-
ing 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 _system_ 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 _unstaged_ 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 repeat 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 in-
fluence 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 ac-
curately 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 final-
izing 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 verifi-

cation 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 differ-
ent 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 con-
vective 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 fa-
cility 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 ex-
perience 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 <u>fourth</u> 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 <u>fifth</u> recommendation is to continue development of the giant planet arc, and this is being driven by a Jupiter 1984 launch.

The <u>sixth</u> 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 heat-shield 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 docu-
ment) which talked about it costing 70 kg to be able to incor-
porate 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 propel-
lant for the additional maneuver. The tip-off conditions have
been looked at and they are relatively modest. We are even look-
ing 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 sur-
prises 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 pro-
pose 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 the 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 specifi-
cally 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 execu-
ted, 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 ob-
jective 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 at-
mospheric scientists for upper atmosphere measurements. Under
contract to ARC, we studied the accommodation of upper atmos-
phere 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 pro-
blems of calibrating an upper atmosphere mass spectrometer

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 say-
ing 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 some-
what 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.

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