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PAGES = 35

N 67-34627
(PAGES)

TMX-60226
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30
(CATEGORY)

TELEPHONE 961-1111
AREA CODE 415

To: Distribution

Subject: NASA Symposium on Trajectory Estimation held at Ames Research
           Center, October 18-19, 1966.

The subject symposium was attended by some 55 individuals, principally
specialists in the theory and application of space vehicle trajectory
estimation. There were representatives from eight NASA organizations,
nine NASA contractors, and three universities. Names and addresses of
the attendees are listed in Attachment A of this letter.

Purpose of the meeting was to discuss the present state of the theory
and practice of trajectory estimation. There were thirteen presentations,
abstracts of which are contained in Attachment B, and an informal open
forum discussion, a transcript of which is given in Attachment C of this
letter.

The content of the meeting may be summarized as follows:

The basic theory of data processing is fairly well understood, although
there are always new developments as discussed in the presentations of
Curkendall, Pfeiffer, and Smith. However, application of the theory is
handicapped by inadequate probabilistic modeling and definitions of the
environment, which make it difficult to make satisfactory statements
regarding the accuracy of trajectory estimation. The need for better
measures of trajectory estimation performance was indicated at the
meeting (1) for real-time problems in Schiesser's talk, and (2) for the
post-flight analysis of data in scientific investigations in the presen-
tations by Mayo, Fisher, and Bourke. For pre-flight analysis and mission
planning the need is also apparent, as indicated by Friedlander's problem
in comet orbit determination. Some of the approaches to modeling were
discussed directly in Pfeiffer's and Woolston's papers and were implicitly
involved in all the other presentations. Modeling was also a principal
subject of discussion in the open forum. Development of computer programs
for data processing has followed a number of different lines, reflecting:
(1) the various alternatives which exist, as outlined by Morrison; and
(2) the need to meet operational requirements, as indicated by Schiesser
and Dungan. Some ideas on how to handle certain practical problems due to
modeling inadequacies and the need for efficiency were presented by Smith,
Ditto, Schmidt, and Woolston.

RQ7-44557

1. FACTORS DESCRIBING AND INFLUENCING THE ACCURACY OF DISTANT SPACE
   PROBE NAVIGATION USING EARTH-BASED TRACKING DATA
   David W. Curkendall, Jet Propulsion Laboratory

   The concept of "velocity parallax" is introduced and it is shown
   that this is the principal contributor to the doppler data partials
   with respect to the out-of-plane components for distant, non-accelerated
   probes. Using this concept, a simple three-dimensional straight line
   motion with a rotating earth model is constructed and the information
   content in a single pass of doppler is determined. This information
   content is displayed in terms of data's ability to determine (1) geo-
   centric range-rate, (2) right ascension and declination of the probe
   or alternately, (2') the station's distance off the spin axis and
   station longitude.

   The data noise model is discussed and the accuracies for alternate
   models are charted. Non-gravitational forces are briefly considered
   and the effects on the estimation accuracies of unknown probe accelera-
   tions are calculated.

2. SOME TRW EXPERIENCE WITH SEQUENTIAL PROCESSING OF TRACKING DATA
   Dr. David D. Morrison, TRW Systems

   TRW has been involved in the process of sequential processing of
   tracking data since 1958. Among the problems discussed are:

   a) The derivation of sequential processing equations.

   b) The theoretical and practical divergence of some sequential
      processing techniques and discussion of methods for avoiding
      divergence.

   c) Equivalence theorems which relate least squares and sequential
      processing techniques with and without state noise.

   d) A discussion of alternative methods available in sequential
      processing techniques, with arguments for and against various
      alternatives.

3. ON THE STOCHASTIC MODELING PROBLEM IN ORBIT DETERMINATION
   Carl G. Pfeiffer, Jet Propulsion Laboratory

   The philosophy of minimum variance estimation is discussed, and
   criteria are suggested for constructing a "correct" stochastic model
   of the system. The linear dynamic process is discussed, and two

alternative models of data noise are suggested, both leading to a
sequential estimation technique. An interpretation of noise of uncor-
related increment is presented. Deep space orbit determination based
upon counted doppler data is discussed. Various treatments of the
nonlinear problem are outlined. It is pointed out that practical
techniques for treatment of nonlinearities depend upon the assumption
of small amplitude noise. It suggested that the presently employed
iterative approach to nonlinear estimation appears to be adequate
for most applications, but there remain questions of convergence and
uniqueness of the resulting estimate.

4. SEQUENTIAL ESTIMATION OF MEASUREMENT ERROR VARIANCES
   Gerald L. Smith, Theoretical Guidance and Control Branch, ARC

   A method is presented for relaxing the usual assumption in sequential
Bayesian or minimum variance estimation that the distributions of the
observation errors are known. The approach used is to regard the
distributions as normal but with unknown variances. It is assumed that
the system equations are linear, that the distribution of the system
state vector is normal, and that the unknown variances can be repre-
sented as random variables having inverted-gamma distributions.
Application of Bayesian estimation theory in a multi-stage process
then yields recursive equations for estimating simultaneously the
system state and the variances. The equations, in effect, are like
those of the Kalman filter but with additional equations adjoined to
produce the running estimates of the unknown variances. Results are
given for application of the method to a simulated trajectory estimation
problem for an interplanetary vehicle. It is shown that when there is
substantial uncertainty in the observation error variances, there is a
possibility of significant improvement in performance as compared to
that of the conventional approach which assumes the variances to be
known.

5. ORBIT AND TRAJECTORY DETERMINATION FOR SCIENTIFIC SATELLITES
   David Fisher, Goddard Space Flight Center

   Scientific satellites lead to increased efforts in determining
both gravitational and non-gravitational forces acting on these
satellites. Additional efforts are being made to improve the
mathematical models for satellites of extremely high and low
eccentricities.

6. ORBIT DETERMINATION FOR LUNAR ORBITER
   Alton P. Mayo, Langley Research Center

   The basic structure and solution capabilities of the lunar orbiter
orbit determination program (ODP-L) are discussed. The elements of the
lunar orbit after deboost are presented. The mean square of the doppler
residuals of the data fit during the translunar orbit and upper lunar

orbit are shown to be about 0.1 cps. The estimates of the spacecraft cartesian state after deboost are shown to vary about 1/3 of a kilometer for x component and about 1 kilometer for the z component as the data arc processed was varied from one to three days. The effects of spacecraft pitch maneuvers and undetermined perilune effects are shown to appreciably affect the doppler residuals. The solution for the spacecraft cartesian state is discussed and was observed to experience no numeric difficulties. The orbit determination program provided adequate information for fairly precise mission control.

7. OPERATIONAL PROBLEMS IN GEMINI TRAJECTORY DETERMINATION
Larry J. Dungan, Manned Spacecraft Center

The Bayes Method of trajectory determination used for Gemini missions has proven itself satisfactory. Many of the operational problems which were expected did not occur or have been eliminated by operating procedures.

Problems which have occurred during the Gemini missions which affect the quality of the determined trajectory are as follows:

a) Proper adjustment of a-priori weighting on post maneuver data.

b) Evaluation of vectors computed from data immediately following a maneuver.

c) Random thrusting as a result of mission experiments.

d) Radar data received from some sites not of high quality due to station coordinates and radar or beacon problems.

Some of the problems experienced in the Gemini missions are expected to remain in the Apollo missions. It is anticipated that until data is received and processed from the USB tracking sites, the operational Apollo trajectory determination program will not attain the accuracy results predicted by error analysis.

8. PRAGMATIC PROBLEMS OF TRAJECTORY ESTIMATION
Frank H. Ditto, IBM Coporation/RTCC

Some of the practical problems of the batch sequential trajectory estimation process developed for Gemini are discussed. The peculiar advantages for Gemini are presented and consideration is given to data culling. A new approach to batch data weighting which has shown promise with Gemini data is presented. Means of determining meaningful error estimates are presented along with a method for evaluating the quality of the fit.

9. KALMAN FILTERING APPLIED TO REAL DATA
   R. K. Squires, H. Wolf, D. Woolston, Special Projects Branch, GSFC

   The Special Projects Branch at Goddard Space Flight Center has
undertaken a study of application of Kalman filtering from the
following point of view:

   a) An orbit determination scheme performs adequately only if,
      in addition to an estimate of spacecraft position, it provides
      a measure of how well that position is known in the form of a
      realistic covariance matrix.

   b) The performance of an orbit determination program should be
      judged not only on its ability to fit a given block of tracking
      data but also on its ability to predict ahead to subsequent
      blocks of data. While accurate prediction depends on accurately
      representing the dynamic and environmental models, in the
      orbit determination phase any reasonable model should work
      provided one adequately accounts for the uncertainties in
      the model.

   The Goddard Kalman filtering program uses the approach of accounting
for, but not solving for, various model uncertainties generally following
the formulation given by Schmidt. A recent preliminary attempt to open
the covariance matrix to uncertainties which lead to in-track errors is
described. This approach indicates quite favorable results.

   Examples of applying the Kalman filter to data for the first IMP
satellite are presented and discussed. Favorable performance of the
filter is shown although the need for further refinements and the use
of double precision in some areas of the program are indicated.

   The paper represents not really a demonstration of the capability
of the Kalman filter but rather a sharing of Goddard experiences in
working toward an operational program based on it.

10. PLANNING APOLLO NAVIGATION PROCEDURES
    Emil R. Schiesser, Manned Spacecraft Center

   As the basic capabilities of the navigation complex nears its
final stages of definition, even greater attention is being given to
plans for its use.

   The use of onboard navigation capability was introduced in the
Gemini project. This and the inclusion of frequent maneuvers for
rendezvous led to the establishment of the first ground/onboard navi-
gation procedures. For Apollo, the roles of ground and onboard navi-
gation systems change for the different mission phases. In fact, even
the navigation complex itself changes with mission phase.

The purpose of this discussion is to informally indicate some of the current procedures for the various navigation systems during the earth orbit, translunar and lunar orbit phases of the planned lunar landing mission.

In earth orbit this involves the ground, command module, and the S-IVB navigation systems.

In lunar orbit the command module and ground systems will again be considered. The procedures for the lunar module phases (rendezvous, descent, and ascent) may be mentioned, time permitting.

In earth orbit, the S-IVB system will normally be the prime source of navigation data; however, the ground may replace the S-IVB position and velocity values with the ground estimate.

In lunar orbit, the ground will generally be the prime source for free flight and the command module will be prime during powered flight. What this means will be discussed.

11. IDENTIFICATION OF RANDOM FORCES ON INTERPLANETARY SPACECRAFT
Dr. Roger D. Bourke, Jet Propulsion Laboratory

This paper describes current work at the Jet Propulsion Laboratory devoted to the analysis and modeling of random forces on spacecraft and eventual inclusion of their effects into the orbit determination scheme. Forces of this type can be generally classed into two categories: spacecraft generated forces (eg. those arising from the attitude control system), and spacecraft interactions with the environment (eg. solar radiation pressure). Several potential sources of translational force are listed for each category. Attitude and tracking data from Mariners II and IV indicate that forces of this type were indeed acting on the spacecraft. Some attitude data from Mariner IV is presented in the paper and explained. A method for reducing these data to deduce bias torques, cross coupling between axes, impulse variations and misalignments is outlined. From this torque information it is possible to infer translational forces if certain assumptions are made. The basis for several possible sets of assumptions is discussed and preliminary results are given.

12. COMET ORBIT DETERMINATION
Alan L. Friedlander, IIT Research Institute

Optimal linear estimation theory is applied to the problem of determination and prediction of cometary motion and, in particular, to the short period comets, Encke and D'Arrest. These comets are studied with a view toward obtaining the most representative orbit and its probable uncertainty based on observations of right ascension

and declination made in previous appearances. This information is then used to predict the future motion of the comet and, specifically, to estimate the comet's ephemeris errors which are relevant to the guidance accuracy and fuel requirements of a spacecraft intercept mission.

The numerical study of cometary motion is facilitated by a high precision Orbit Determination Program developed for use on the IBM 7094 computer. The computer program, apart from the observational data processing section, is basically an N-body trajectory integration code which includes the gravitational perturbation effects of all the solar system planets and also non-gravitational or secular perturbations unique to the nature of comets themselves. Numerical integration is accomplished by Cowell's method using a fourth-order Runge-Kutta procedure with variable step size control.

Past observations of comet Encke are obtained for seven appearances over the period 1931 - 61 with no less than three observations in each appearance. Results of data fitting show strong evidence of secular acceleration of mean motion which is in close agreement to that found in previous investigations. The average effect over the interval studied causes a decrease in the orbital period of about -0.02 day/orbit. This seemingly small secular acceleration, if not accounted for, would result in large spacecraft miss distances at some future date.

In the case of a 1974 mission to Encke and a 1976 mission to D'Arrest, it is shown that miss distances under 10,000 km cannot be achieved unless the comets are observed in the year of launch. Even then, to achieve a desirable miss distance of 1,000 km, the observation period must extend almost to the time of intercept, thereby implying a late midcourse or terminal maneuver with its inherently larger ΔV requirements.

13. ESTIMATION OF STATE WITH ACCEPTABLE TOLERANCE CONSTRAINTS
Dr. Stanley F. Schmidt, Philco Corporation WDL

Many papers have described difficulties in obtaining a good estimate of state with the Kalman filter when time arcs spanned by the observations are very large. Some of these difficulties are numerical while others are a result of imperfect modeling. These problems are not necessarily inherent in the Kalman filter but also exist in the weighted least squares or the maximum likelihood filter. The undesired characteristics generally found are a growth of residuals (differences between computed and actual observations) with time.

This paper describes a non-optimal filter which has much better behavior than the Kalman filter in the presence of numerical and modeling errors. The filter design is based on the philosophy that current observations should be weighted more heavily than past observations. As a result, residuals in the near past are smaller than those exhibited by other filters.

The filter design approach is to define an acceptable tolerance on the accuracy one expects an observation can be estimated. This tolerance leads to a gain constant associated with the use of an observation in obtaining an estimate of state.

Example problems with modeling errors are shown which compare the Kalman filter and the new filter. The results indicate the new filter has considerable merit for certain estimation problems.

TRANSCRIPT

Panel Discussion at NASA Symposium on Trajectory Estimation

October 18-19, 1966

The following transcript has been edited only to the extent of eliminating (1) extraneous words and phrases, and (2) remarks which were unintelligible on the tape. In general, the conversational tone has been preserved, and the grammar was corrected only where necessary to convey intended meanings. Identification of speakers may not always be correct, and also certain words and phrases may have been incorrectly interpreted. Therefore, no part of this transcript should be taken as a direct quote without express authorization by the speaker(s) involved.

GERALD L. SMITH: I would like to open the meeting to a discussion and anybody who has something that they would like to start out on, go ahead. Perhaps the discussion we were already engaged in, in lively fashion, this morning, somebody would like to continue.

STANLEY F. SCHMIDT: Well, actually I'd like to propose at least that consideration be given to defining -- I made one attempt to define -- a nonoptimal filter. In other words, open up the problem to defining acceptable constraints on what accuracy you can achieve rather than saying that the thing must go to zero, and I would like to have other (suggestions). I think it is a good idea. I have only looked at one little thing about it, and it gives you a lot of freedom, except you don't necessarily know how to use this freedom, see? And I've shown one way that intuition and perhaps engineering sense (indicates) maybe this ought to be done. It seems to me like its the kind of freedom that we have been looking for, and it seems like it ought to be able to be used in a better fashion than what I have proposed. I mean, I would like to hear anybody else's comments on this, but it seems like it's a sort of -- it's a correct kind of direction.

SMITH: Well, my feeling about that, at least in part, is that we don't take this to mean that we stop our modeling efforts. The modeling of errors will always be important. As we attempt to get more and more information out of the vast quantities of data that we are getting, we absolutely require that we model the uncertainties that are present as accurately as possible. This means bringing all of our knowledge to bear on the modeling problem all the time, but in the meantime, we are faced with these real problems as has been very well pointed out here. In the real time trajectory estimation problems you can't wait to do an ex post facto analysis. You have to have answers now for your mission, and the kind of scheme that Stan has suggested represents one approach to handling this kind of practical problem. Somebody else? Sam?

SAMUEL PINES: I was thinking that maybe a good direction that the effort might go in, may be one that might prove a lot more practical, would be the following: That you have a mission; and you set yourself the objective -- what kind of a filter shall I use? What kind of orbit determination shall I use, as a function of what I am going to do with the answer in the mission? And I think too often we tend to simply use a program that is available without really thinking about what we're going to do with the data and what part is a must and what part is a luxury, and so on. So that maybe there is an analytic way of going at the thing, in the mission design, to make specific changes to the programs from the point of view of where I'm going to use the data in the operational sense.

SMITH: This sounds like it's kind of related to Stan's philosophy of a constraint on accuracy. You can -- you're perfectly willing to accept certain, uh . . .

PINES: It is a reasonable problem. I am talking about a problem that analytical people can tackle to assist project engineers and personnel in carrying out the mission. I mean, right now some people are interested in data and constants and various forces and so on; but there may be very specific things that we have to do at different times and maybe the filter analysis can be designed specifically for it, to some advantage.

SMITH: Carl, do you have any . . . ?

CARL G. PFEIFFER: I guess you're asking about the other techniques than minimum variance, and there's one which came out of the old game theory. The old notion of a game against nature. You might sit down and say, well, there is a random force in my equation and I don't know what it is. Let's assume that it's the worst possible thing it could be. Let's assume that it acts in the worst direction. And look for this kind of a model, which says that if my estimate is acceptable under this circumstance it will always be acceptable under any other. This may be reasonable.

SMITH: Well, that is the old minimax philosophy. Personally, I don't like the minimax too much. I don't know how other people feel about it.

SCHMIDT: I mean that it's basically one of our problems -- at least you see it all the time -- you perform an error analysis and a real mission comes along and it doesn't agree very good. Now somehow you have got to introduce something. You would like to perform a correct error analysis of the problem before, and when the thing really occurs then it does agree. Something is wrong in what we are doing because seldom until after the thing has occurred can we make an error analysis that agrees with what happened. And that doesn't seem like a very good approach. We will have to introduce something like what Carl said. It happens in the worst possible way somehow.

PFEIFFER: Of course, this may not be. There is a lot of objection to this, as Jerry points out. Even if you can do this you are going to end up with something foolish. Something which is absolutely meaningless and won't work at all.

SMITH: Well, of course, there is nothing wrong with ex post facto analysis as such because you can always have additional information that might not have been really terribly important for the original objective of the mission and still teaches you something extra.

SCHMIDT: Yeh, but take, for example, the anchored IMP. I think that's something like 80% or 90% probability to get into a successful orbit about the Moon. It didn't.

CARLETON B. SOLLOWAY: That is just one case, though.

JEROME BARSKY: That was a succession of 1 1/2 sigma errors. All additive in the same direction. That's a quote.

SOLLOWAY: I think there is a difference between studying stochastic processes as a group before a mission and worrying about one specific time sequence during a mission. And we've all done things on the average, but that hasn't helped us in a specific problem. When you have one flight and one thing happening this is not a random process.

SCHMIDT: No, I agree. Except that seemingly -- although some disagree-- I have seen a lot of three sigma flights. Usually the first ones always are. Then later on, things are modified. But, just like Schiesser was speaking about -- I mean, you've done something, you actually to your knowledge believe this, but later on it proves that you were optimistic. Now why are there so many things that generally go this way? Is it just -- I mean, we need some way of making it so that there aren't so many three sigma flights, because there shouldn't be. Should there? Or does every-body agree there ought to be lots of them.

PFEIFFER: There really aren't so many. The Centaur went extremely well. The Surveyor mission, for example, the first Centaur shots went very smoothly.

SMITH: Dave?

DAVID W. CURKENDALL: Yeh, I want to say we're kind of changing the subject in a way. But I don't think you're right, Stan, that there are a lot of three sigma flights. All the Rangers, once the spacecraft worked at all . . .

SCHMIDT: Well, after about no. 4, I think, is what you mean.

CURKENDALL: Well, wait a minute, wait a minute, obviously if something won't turn on, you are certainly out of the limit of reason.

SCHMIDT: What about no. 3? This had actually a fuel depletion on the Atlas followed by an Agena mis-setting on an accelerometer followed by JPL making the correction in the wrong direction. Isn't it? I mean, obviously --

SOLLOWAY: I would have personally been inclined to call the first Surveyor a three sigma flight.

CURKENDALL: Well, you know we have a name for that. They're referred to as . . . On the basis of the earlier Ranger experience, they constructed this very elaborate model of noncatastrophic failures in a launch vehicle and included the actual Atlas-Agena experience up to that time. They were incurring these noncatastrophic failures, that is, actual hardware failures. Something failed. But the thing did not blow up. The thing still injected. And they were occurring at the rate of about one per flight, at that time. So they built this great big model to account for them, and succeeded in rationalizing the fuel loading of the Ranger. And moved it from, what, 60 m/s -- no, from 45 meters per second to 60 meters per second, I believe, based on that analysis. And from that time on, I don't believe we've ever had a noncatastrophic failure. And from Ranger 5 on -- 4 and 5 didn't work at all -- we exceeded what the error analysis said we were going to do on every single flight. But we really changed the subject when we talked about how well the guidance worked. You're trying to talk about how well does the model work. It is a different animal. I was thinking about this at lunch, and I want to make a comment that if we were listening to what you were actually saying when you were talking, Stan, rather than what we were afraid you were saying, I don't think there would have been half so much disagreement. We would agree. Here's a way -- Look, you are throwing out the data anyway. For God's sake throw it out smoothly. You know, in a manner that will impress the project manager with your expertise. And I consider that much better than one of the things we now do, which is off-weight the data -- we say we don't believe the data. And I think that's wrong, and that's a lot worse than what you're suggesting.

EMIL R. SCHIESSER: Well, you can look at it differently, you know. You can say you don't believe that portion of the model that's associated with, like, the station locations. That enters into the weight you give the data.

CURKENDALL: Yeh.

SCHIESSER: The instrumentation is always perfect, right? That is, it measures doppler perfectly, and all that jazz. The fact that a truck runs into the station and moves it is a model error because you didn't model where the truck hit it.

SCHMIDT: Well, I think another subject that I would like to bring up is that seemingly there ought to be a way by which you can take residuals, bad as they may be, and compute an error -- a reasonable error -- in state,

based on the residual behaviors themselves. I'd like to -- I know in the past what I've tried to do is take mean-square values of residuals across a fit and use this as a means of trying to estimate how accurate is this estimate of state. And seemingly . . . I mean, it looks to me like it ought to be that there's room there for work to be done, and actually, given that you have something, given the behavior of something, what is its accuracy based upon the data itself as opposed to something based on, well, some number that you want to throw in? I would like to hear other peoples' comments on this.

ROGER D. BOURKE: Are you in effect saying that you should be able to build the model from the data by itself? Start with a rather cursory model and then correct this model on the basis of the residuals? Is that . . ?

SCHMIDT: No, what I would like to see is a means by which you could estimate the accuracy of an estimate, based upon the residuals themselves.

SCHIESSER: There's a number of ways to do that, but . . .

SCHMIDT: I know, but how good are they?

SCHIESSER: Well, they're not too bad, in some sense. Like for example, you take Earth orbit. How accurately are you really able to get the position and velocity, in Earth orbit for Gemini flight, say? Well, how do you know something like that? You'd like to know, in a way, what is the error in the orbit but all you've got to really tell what is the error in orbit is the things that you compute the orbit with. So what do you do? Well, you've got to somehow come up with a boot-strap technique which says something like this: If I have ten sites and three revs of fairly solid tracking, this permits me to find out what is the error in the orbit as computed by a single site because the accuracy of a multiple -- three revolution -- fit with a fairly good model is at least an order of magnitude better than the accuracy you can achieve by the data from a single site. This permits you to say, okay, for a single site I can process that data and get the orbit to so many feet and so many ft/sec. Okay, well that's useful information. Then you can say can I also go ahead now and get the accuracy of a two-station solution? And I say, yeah, it's a little less. The degree to which I can convince myself that the comparison of that R and V vector with my multiple fit is a little less than for a single station; but, yeah, I have quite a lot of confidence in that that difference is for real. See? So you can keep on doing that but then you get toward the end there and you say well what about the confidence in my three-rev fit? Well there's no way, you see. You've had it. But at least you can say maybe you get confidence through a two-station, maybe a three-station fit, or maybe even a four-station fit. Or maybe groups of four-station fits. And by screwing around a lot, and maybe by starting to look also about your ability to predict forward you can maybe come up with the accuracy of the local position and velocity vector. Now you say, well, right away, well what about a drag error? Well, if I get my BET* over a

*Best Estimate Trajectory

rev and a half and the altitude is so and so, well that's not a long enough arc, so that the drag didn't really come in. See? And so you throw out your model errors that way maybe. And if you don't try to do something foolish like fit 12 revs with maybe a maneuver in the middle and try to compare it with vectors from single station, that'll probably turn out pretty good, even so. Then also, you can get a general feeling about the accuracy of a single radar because you have a three-rev fit. And the residuals from any particular radar get set to the trajectory. They're going to be fairly large and fairly representative of the overall quality of that site. And in fact, you can double check -- then you say, well, if the residuals are there, if I get a single station fit, I'm going to fit those data and I'm going to get hardly any residuals or what residuals I do get is beating of the range measurements against the angles and if I've weighted my range heavier I get angle biases. Can't fit 'em all simultaneously. I know something is wrong with the instrumentation. It's not self-consistent, you see. I learn something about the angles that way, expecially by confidence in the range. And I can beat that out by comparing different systems against the BET and back again, you see. What I'm saying here is there is a way of bootstrapping yourself through to get overall accuracy in R and V vectors themselves as determined during the flight, which is the answer, really, that you're using during the flight, and you can also learn something about the systems. Now when you do this that still doesn't solve the error analysis problem because here you have a tool which isn't predicting these kind of accuracies. Well, there the idea is: don't worry about it too much, just get a shoe box, put five knobs or six knobs on it, where each knob represents the general family of errors and, oh, and maybe do a first cut as to setting these knobs and then you feed these actual errors in there and calibrate the thing, you see. And then hope that when you run another mission's geometry to it that you haven't experienced the model's sufficiently flexible that it adjusts. Well, you can do that for translunar and lunar orbit, like -- what was your JPL experience? -- on the translunar you can calibrate your S-band model. Hopefully, it'll carry over to lunar orbit geometry. But you're not guaranteed. At least that's the best you can do; that's the only thing you can do.

SMITH: I might say this business of studying residuals can be a very exhausting task because what you are really doing is you're trying to determine something about the distributions of the random variables which contributed to these errors -- observed errors. (Residuals are always such.) In order to do this task, one still needs to make some kind of assumptions about the nature of such distributions because you're looking. . . , you're always trying to. . . , well, you have to generally represent these distri- butions by some finite number of parameters so that you can have something to write down. I think that you always in the end are wondering about what kind of errors remain in your . . . Well, let's put it this way: You don't know how accurate your estimates of these distributions are once you've finished studying the residuals. So you still have errors remaining of a perhaps higher order.

JURIS VAGNERS: Well, this is true but in any given physical problem hopefully you have built-in natural weighting factors. In the Earth satellite problem for instance, and this is true in interplanetary trajectories, you have the physical model that you set up, the representation that you're going to use -- for instance, spherical harmonic field -- as a weighting factor in it (like a radius over a base radius to some power) to separate coefficients, for instance. Or ratios of mean motions. That's one sort of a thing that you have that must be considered and quite often isn't. And then another fact is that if you analyze analytically the the difference influences, you have a weighting factor separation due to their effect on the trajectory. For instance, separation of in-plane and out-of-plane motions. For instance, I believe Iszak several years ago at SAO* used the, what he called, rotated residuals, which means you look at the particular residuals you got in a problem and you separated out the effects and the parameters and the residuals. In other words, you didn't just look at a whole mess of residuals and say I have 22 parameters to fit, let's juggle until our residuals go to zero and that way modify the model. I think that drives you directly to madness. In a hurry.

SMITH: Other comments along this line?

PFEIFFER: One thing I might say in line with different forms of the estimator, there has been a technique suggested -- I think it was a paper by Bellman and Kalaba and someone else in the AAS Journal. Something like nonlinear filtering and invariant inbedding. They introduced a . . . they don't know the model so they introduced an unknown function and then they did a least squares, but they threw a weighting, they put an additional term in there. They threw in a, not just the integral of the residuals squared, but also a weighting function times the integral squared of the unknown function. If people are interested they might look that up.

CURKENDALL: I have a kind of general kind of question. It kind of seems that the closer the people here are to the data the more optimistic they are. JPL can kind of wallow around in this kind of stuff, and its like Alice in Wonderland. Everybody's happy and they think all things are possible. Stan hasn't seen any data in five years and he's kind of pessimistic. Emil has never seen the S-band data and I get the impression maybe he'd rather just hold his breath and hope for the best. Golly, I don't know who's right. But, I wonder why that is -- the closer you get to the data, the more optimistic everybody seems to be.

SCHMIDT: Well, I've got a paper actually that essentially is from Bellcomm, and I looked at some residuals here. And they do the same thing now that they did five years ago. They're not random. They're random for short term and for long term they're not. This is the only thing I worry about. And that problem hasn't changed in five years, I don't think.

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*Smithsonian Astrophysical Observatory

CURKENDALL: I think it has, I think it has. It is now possible to take data from the Earth to the Moon and get a perfect fit on that data, all the way to the Moon. And that wasn't possible five years ago.

SCHIESSER: Yes, but take the following view, though. Say, with the current error model, they estimate that in lunar orbit maybe we can get position to a kilometer, and -- I don't know about the velocity -- maybe a couple of meters per second. So, now what we're talking about is R and V; we're not talking about how small the residuals are any more. Actually I really don't care how big the residuals are as long as I am confident that R and V is good, right?

CURKENDALL: Yes.

SCHIESSER: And, so I'm talking about R and V most of the time, although I have to use the residuals to tell how good things are. Well, okay, but now if you compare your small sample solutions in lunar orbit with your multiple orbit fits you're getting a km and 2 m/sec, so I'm justified in being conservative, you see? In other words, it's coming out that way. Now if I had built an error model that says that doppler has just got noise on it -- that is, I have very few model errors -- I wouldn't have got the result that we get with 2 cm/sec bias. That's the only justification we have. It's not that we're . . . In a way we aren't looking at the data too closely. We're more looking toward that R and V vector. The data residuals at times are very small. That doesn't necessarily mean that the orbit's all that good. Well, of course, they usually do go hand in hand, don't they? On the translunar, for example, you could get the residuals almost to zero but the lines point the wrong direction.

CURKENDALL: What's that? The lines?

SCHIESSER: Well, the vehicle's moving in a straight line, practically. We can get all the residuals to have a consistent tracking at one site -- the fact that all the residuals are fit well doesn't necessarily mean we know that R and V vector, right? I mean, the line could be pointed in the wrong direction and still fit a line to the data.

SCHMIDT: You say you fit everything to the moon and there's absolutely no mean in any of the data, huh? I mean the residuals have no mean?

CURKENDALL: Well, if you check, if you take the Ranger tracking reports -- I want to suggest an answer to the question posed -- but, yeah, if you take your residuals and do a hypothesis test on them, hypothesize that they came from a distribution whose sigma you know is equal to the sigma of the residuals you see and whose mean is zero, then the mean of a sample, the sample mean that you compute by looking at the data, is small enough so that you would accept the hypothesis. I can't say that very clearly but I mean obviously in any random sample of even mean zero variables you're going to get some mean, and so there's some mean in our Ranger data. But that mean is consistent with our hypothesis that it came from a population whose mean was zero.

SCHMIDT: This is three tracking stations and they all agree?

CURKENDALL: Excuse me?

SCHMIDT: Three tracking stations and they all agree?

CURKENDALL: Well, I gotta admit the one hole here is that those reports
are out before we ever published a final value of the position locations
of the tracking stations, and then went back and fit all the data. There's
a possibility that the tracking stations are enough unknown just on the
way to the moon to take out whatever anomalies there are in the data.
Understand? Whereas, if I constrain my solution to use one set of station
locations for all the Ranger experience then I might not do so well.

SCHIESSER: Yes, you see if you only solve for 6 x 6, you'll see residuals
maybe, where if you've got enough parameters in your solution you can just
wash them all out. In other words, I won't allow you to do a 12 x 12
solution and say you have zero residuals. You've dumped those residuals
into some kind of elements there, and I have no guarantee you've dumped
them in the right elements, you know. I'd rather see you do a 6 x 6
solution -- well, or generate residuals off of a best trajectory, generate
residuals with your nominal model without having a model intermediate --
what you might call an intermediate model -- left over from your 12 x 12
fit, you see. In other words, you can do a 12 x 12 fit -- that's all
right -- then throw away the solved-for elements in that thing. All you
allow us to use legally is the initial R and V vector plus you pre-mission
model to generate your residuals.

PINES: That is to say once you've determined the mass of the earth, you're
not allowed to go back.

CURKENDALL: That's what I'm saying we've got to do with the Ranger stuff.
It works all right for the masses, but I don't know about the station
locations. Station locations move around from flight to flight within
the sigma tolerances quoted.

VAGNERS: I have some questions. First of all, when you gather the data
are there spans of data when more than one station is receiving the signal
at the same time?

CURKENDALL: Well, you won't find any three-way data.

VAGNERS: Okay. Second question is when you take the station locations and
you say okay now you've adjusted station locations and you're going to
process this whole set of data again, how do these station locations . . .

CURKENDALL: Same data:

VAGNERS: Yes. This seems to me what Emil was saying, is what you've done
is taken the inconsistencies, dumped them in the station locations, which
you now . . .

CURKENDALL: Yes, that's a possibility.

VAGNERS: Well, this is what . . . I think it's more than just a vague possibility, I think Earth-based orbit tracking of earth satellites has shown this to be the case. Unless you can pick up three stations sighting at the same time any consequent juggling of the station locations to within a number of meters will give you a better fit on the data. Then the next one goes along and it doesn't. The only way that you're going to separate out and say that you're not doing this, sweeping the troubles under a different rug, is if they have a simultaneous sighting. Then you can hope to improve consistently and realistically something called an uncertainty in the station location but not otherwise. See?

CURKENDALL: Well, I don't really think that's true. Remember, you get several passes of data from a given station on a given flight, and . . .

VAGNERS: Okay, well, here's a check, . . .

CURKENDALL: You gotta use one station location for a whole flight.

VAGNERS: Okay, but here's a check. How do your adjusted station locations fit with the . . . some current model of the geoid.

CURKENDALL: The previous one?

VAGNERS: No. With some current model of the geoid. Otherwise. Independent of the flight.

SOLLOWAY: But that may or may not be significant. It depends on whether you're interested in doing geodetic work or whether you're trying to accomplish a specific mission. And granted you may have the wrong model. But if you can dump errors fictitiously into this model and still come out with an adequate solution to your overall mission, that may be all right for that mission. You just don't want to confuse it with doing geodetic work.

VAGNERS: You don't want to do it again on the next mission.

SOLLOWAY: Hopefully not, no. You'd like to know more about the process.

VAGNERS: Right. This is what Emil said and I tend to agree with him that if you're going to do it I'd rather see him work with a 6 x 6, forget the other 6 x 6 which you can dump some of your problems into.

SOLLOWAY: No, I don't think I would. I think I'd like to accomplish the mission first and then go back and find out why my model was wrong. If I get answers for station locations which I know are definitely wrong, I'd like to know what was wrong with the original model. But the first thing is to accomplish the mission. And we've thrown an awful lot of dependence

on these residuals and I don't think we should. And we keep coming back
to the question of the size of the residuals as adequacy of the model, and
that doesn't always make sense. It doesn't always tell us what's going on
either, because certainly you can keep throwing parameters into this model
and reduce the residuals without knowing what's going on.

VAGNERS: Well, somewhat of a problem is eventually ... in other words, you
can take that point of view and continue it, on every flight -- but
eventually, hopefully, by analytics we should be able to begin to separate
out and apportion these problems into their various properly labeled boxes.
That's a secondary objective I'll grant you. It's true that you want to
accomplish the mission first -- you want to get a good analysis of the
mission -- but if each time that you're going to put up a Mariner you're
going to start -- or put up a Ranger, or any spacecraft -- and you're
going to start reanalyzing your problem in this same manner, I think you
are just back-tracking and doing the same work over and over again. The
question of always the separation which I think is somewhat obscured by,
as a matter of fact, things like station juggling, will plague you to the
end of your days, until you can begin to separate out -- is it really
station problems or is it something else? Sooner or later you have to
draw this line.

SMITH: The only way to determine that is to test your determined --
supposedly better -- station locations against the results of subsequent,
preferably different, kinds of flights.

PINES: Well, we have to get back and talk about what the objective is.
In other words, if you are really interested only in the success of this
mission then certainly -- what Carl says is enough -- you should go ahead
and finish. However, once you look at the data and you see that the actual
statistical variation of data is down to .1 - .2 cm/sec and you realize
that there are forces that you are interested in that could have produced
results 10 times as big as this that you can get information on, then you
sort of feel silly that you didn't push it a little further. And so, it
would seem to me that once again, once you set the objective, it ought to
be possible to go ahead and develop a method to get the maximum information
out of the data depending on what you want to do. Now for the success of
each mission -- all you want to do is take a picture of Mars or send some
TV back or something like that -- maybe this approach of sweeping under
the rug is good enough. But certainly we could do more. It's obvious.
I mean, from the data presented you can tell when a vane turns around or
something like that. That's a lot of information. And, it seems to me
that we ought to take a good look at some of that -- there's information
there and we ought to see what it is. For that you need a much better
determination program than we've discussed here -- increase your model
accuracy and pick this stuff up.

SCHMIDT: The only confidence, though, that you have, Sam, is in terms of:
It works on this flight and it works on this flight and it works on this
flight. Now if it doesn't work on the next flight, well then there's still

something wrong. Like, for example, changing station locations. What I wonder is do you change station locations during a flight? Like, you process one batch and the stations are one place and then you process another batch and let the stations move in between batches. Do you do this? And I suspect that you might do this because it'll make the residuals look a little better and then you're happier, but really that isn't the right thing to do.

CURKENDALL: No, . . . then you're wide open.

SCHIESSER: . . . because if there's something wrong you'd like to see it in the residuals so you can properly weight that data. So it's to your advantage if you know that station locations can't move but within a certain range. You're better off to do what JPL does -- that is, use the nominal pre-mission constants and then you're in a position to -- you've got a model there that's set -- and you have some past flight experience with it which you can get by re-running the old data if you want to. Then if the next flight comes -- if you only solve for 6 x 6 -- then if there's anything wrong with that site you know it's not going to be from the station creeping. You know what general behavior this error has. Something else, it's going to show up in the residuals, and you can do something about it. Of course, you can do something about it the other way too, but then you have to monitor how much the location changed. If it changed beyond some bound, well then you say well there's something wrong, so you could look at that, but then let's suppose . . .

VAGNERS: I don't think that anybody's shifting station locations around to fit. This is something which was done five or six years ago, but you leave yourself wide open when you do this. You shift station locations as a batch from a given flight through a flight maybe, but not . . .

CURKENDALL: You certainly don't want to sweep anything into the station locations. A good example of that is -- I mentioned before, the Pioneer VI data has a gas leak. And you can actually make the data fit by just opening up the sigma on the station locations, and the trajectory and all this gas leak will fall into the station locations. And when you do that you get a worse -- a much worse -- trajectory determination than had you left the station locations alone and let the data fit poorly. So there's two dangers: (a) You're happy when that happens to you and (b) you're worse off than if you were sad. But I wanted to suggest the answer to the question that I asked. Maybe its . . . the reason we're more optimistic is the kind of flights that are facing us. Because we're not faced . . . if we were faced with lunar orbiters, we might not be so optimistic. And it isn't really a question of revs here. That we've got a model that seems to fit really very well from here to the Moon. And if we could get a model that fit for one revolution of the Sun, or out to any particular planet to which we wanted to go -- and fit very well -- that would be kind of the next step. And, gee, if you make kind of a goal, if we can build a sophisticated enough orbit determination program to get from here to Mars and make the residuals lay down all the way without throwing away data earlier, gee, I think we could be pretty happy for the next flight. Right now the lunar

orbiter problem looks pretty . . Doesn't it look kind of hopeless? You can't fit lunar orbiter data for a year.

ALTON P. MAYO: Give us time. Wait for another year then say that.

SCHIESSER: It depends on what you're trying to do. You're really not trying to make all the residuals lay down. All you want is an accurate enough vector to make that next maneuver. And well that's pretty easy to accomplish. That looks like we're in pretty good shape there, generally speaking. In other words, . . .

SOLLOWAY: Well, for some things we are. I think Sam and I were both agreeing with Dave here when we said that what you're trying to do is build something that will work, to satisfy your mission requirements.

SCHIESSER: Yes, right.

SOLLOWAY: And it isn't even a question of making the residuals lie down, at the beginning or the end even, as long as you can do what you're trying to do. And it's true we have a pretty good model for Earth-Moon systems. We don't have for planetary missions yet, nor the lunar orbiter.

PFEIFFER: I think we're a lot closer on planetary than we are on lunar orbiter.

MAYO: Wait until lunar orbiter has as many years as the planetary mission, and I think . . . .

SOLLOWAY: We could give Jack Lorell away and avoid the problem.

PINES: Actually, though, isn't this what you expect? You never know the problems until you look at them, and the only thing that's happened is that we have a successful lunar orbiter and now we really see the stark truth and we realize we're missing something and we're going to find it eventually and plug that hole up. And then we'll be down once again to the 0.2 cm/sec kind of residuals. You really won't know what the interplanetary problems are until you try to get some decent pictures of Venus, and you find that you're going to miss it by 20,000 kilometers or something, and then we'll be back again -- I'm not saying we will, but if you do -- worrying about it. Right now I feel that one of the few things we ought to undertake is to examine the adequacy of our present ephemeris. I keep harping on this because I really think that it ought to be re-examined very very critically before we undertake some additional missions.

WILLIAM G. MELBOURNE: I would like to say a few words on that. We are.

(??): We're spending a fair amount of time processing radar bounce data off of the (???) and occasional spacecraft data, primarily with respect to Venus and Mars, right now.

PINES: Well, I'm thinking specifically of lunar orbiter now. Here's a situation with, uh . . . . we have these residuals of 2 cm/sec, now what's going on? Maybe there's something in the basic ephemeris that could account for that. And we ought to be thinking about it. Because it is a real first-order effect, you know, it's nothing . . .

VAGNERS: On this lunar orbiter, are there any occultation-type experiments that could be carried out?

MAYO: The occultation data is available. Some occultation data.

VAGNERS: Well, did somebody look at this to see whether or not they can pick out any dust or gas -- atmosphere?

MAYO: No, they haven't.

VAGNERS: Because this thing -- it was probably commented on yesterday -- this type of an error looks like an air drag type of an error. This is the sort of thing that you would expect, and trying to hide it amongst gravitational coefficients -- I don't think this, uh . . .

MAYO: Well, you've got to realize that the lunar orbiter data has only been available about a month now. . . and the analysis, the post-flight analysis, will probably last somewhere about that order of magnitude. In that length of time you just haven't had time to analyze it . . . .

(??): Do the residuals right before occultation look screwy?

PINES: They look all right. The edges always look good. It's the middle that's bad. From what I saw the residuals don't look too bad.

MAYO: Well, in the lower orbit let's say, you have perilune quite often right before it occulted and we were upset by this oscillation. In the upper orbit I think there was some effect right as it occulted, but again that's one of those undetermined . . .

CURKENDALL: Gee, there's going to be a Pioneer VII occultation of the Moon next January, and Stanford's got their two-frequency experiment on that. You might be able to see something.

(??): Yeh, only charge time . . .

(??): It might give you an indication . . .

BOURKE: Have you calculated what those residuals are in terms of acceleration?

MAYO: What residuals?

BOURKE: Well, the ones I saw yesterday. Those wiggles there at perilune.

MAYO: Yes, I don't recall what it came out to be, but there was a
possibility of a higher harmonic of the lunar gravitational field.

PINES: Well, its 2 cm/sec over a half-hour . . . .

VAGNERS: What higher harmonic are you talking about? You mean a longitude
dependence? Well, you can pick out . . . . you can analyze the resonance.
It would only have some sort of a resonant or non-resonant effect. And you
should be able to pick out that thing analytically to see whether or not
you would get that effect. It would depend on the mean ratio of the mean
motion, right? So 15 would sound like a logical number or any one of those
other ones. You should be able to pick up the effect analytically.

MAYO: Right. I understand also you can change the shape of that curve
by changing the gravitational parameters in the solution.

VAGNERS: Yes, but I think that might be hiding the problem.

PFEIFFER: Why do you say that's hiding the problem? If it's possible to
explain what happened by a very simple explanation . . .

VAGNERS: Well, I think they did -- I think they have looked at it to see
what the change in gravitational parameters does and what . . . . certainly
you can make things go away, but then if you try to take and use these
gravitational constants for reconciling other well-known phenomena, and
other observations, and find that you come up with a discrepancy in those,
I think the answer was that you can't. You see we have an independent
check on lunar orbiter. We do play around with gravitational constants
to fit these lunar orbiter data. We have an independent check -- we have
all the JPL data of transpace, and we have lunar motion type . . . . .
Quite a number of theories depend on these constants, independent of the
lunar orbiter, by which we can check. This is the point. And I think you
wind up with a discrepancy. At least this is what was told to me.

PFEIFFER: I don't know much about it. My understanding is though that
it is possible to explain these 7-second periods near perilune -- 7-minute
periods -- by means of the higher harmonics, and . . .

VAGNERS: Oh yes, yes, very definitely.

PFEIFFER: And this would be consistent with all . . .

PINES: That's not true, because, no, because those would show up over very
long periods. These higher harmonics cannot produce this result in a short
time. They can only show you stuff, you know, if you wait a long time.

VAGNERS: This is speaking of the resonant situation and then it's entirely
true; it takes a long time to build up. That's true. But I think the
hypothesis is that there is a large anomaly of that order such that that's
a first-order effect, not a long period, not a resonant effect.

PINES: That's what I'm saying. My point is this, that if you drop out the higher harmonics, you'll get a single bump. If you throw in the higher and higher harmonics, it just takes that energy and throws it into each one of the coefficients -- you get a lot of wiggles. But it indicates that that is not the explanation.

PFEIFFER: I guess -- we know -- that we have to do a lot more work.

PINES: No, it's not gravitational. I mean it's not due to higher harmonics. ?? I say it is, and you say it isn't.

VAGNERS: I think the resonant situation you can probably look at quite readily, to see whether or not it's a higher-order harmonic resonance.

SOLLOWAY: Well, I take your stand, Sam. I don't think it is either, but then somebody asks me, well then what is it?

PINES: I got a clue. I think it's something like this, that when you freeze the ephemeris tape, what you're really locking in is the orientation of the rotation vector of the Moon with respect to the Earth. And if that were all, then no matter how you redistribute the masses you'll never wipe that out, see? It might be just a geometric effect. Say just a whole rotation of the moon around. That's rigidly linked in your tape. Once you decide you know what the geometric librations are, that fixes the way the Moon moves with respect to the Earth. I'm not saying that's the answer, but I think it's something like that.

VAGNERS: We've got two weeks -- at least two weeks -- of data. We should be able to pick it up if that should turn out to be periodic. Unless you say it has a mean of zero.

PINES: This has a mean of zero. All that happens is that if you try to do it over a longer and longer time the oscillations will get bigger and bigger, but they're all zero mean. You look at . . .

CURKENDALL: If NASA should offer a prize for the best solution, you'd have everyone in the country working on it.

MAYO: I think your problem is that you have so many people talking about it and not enough working on it.

CURKENDALL: That's true.

VAGNERS: Of course, then there's the question that I understand was raised the last time this was discussed in Los Angeles. When do some of the rest of us get a chance to lay our hands on some of the data? You see, we can hypothesize without looking at the tracking data where we can't really offer concrete evidence backed up by observation. That's really the . . . .

SCHIESSER: Yes, but even if you had it, it would take you six months to get a program that . . . .

VAGNERS: It may or may not.

SCHIESSER: Well, there's a real world out there, my gosh, and if I manage to get all the constants fed in there, that takes time.

VAGNERS: Well, let's say it may or may not.

SOLLOWAY: Of course, I think that before a lot of programs are built, it would be nice to get some positive suggestions as to what it might be. Whether you had the data or not.

SMITH: Well, does anybody have anything else that they'd like to bring up for discussion? I would kind of like to have some summary-type remarks if it were possible -- if anybody has given some thought to that -- as to where we stand in the field of trajectory estimation, what we know now that we didn't know say five years ago. Or is everybody too much lost in the details of trying to get the current-day jobs done?

SCHMIDT: I think Carl's . . . for example, he said I want to go to Mars and I want to make sure I miss Mars with some probability. Now, what . . . I mean, it seems to me like we're not really in a good position to make an accurate calculation of this type as yet, are we? Do you really feel that you can compute something of this sort that is real meaningful? Or would you really say, well, I want to make sure I don't run into Mars, so I'll deliberately off-aim it and make a last correction when I get close and that's how I will ensure that I won't hit Mars? I mean, do we have enough confidence ..... in other words, I wonder today, I think we know a lot more than we did five years ago in terms of modeling, but to what degree of confidence can we predict, I think, on a given flight as to how it will behave? Like, for example, the probability of something like this happening, that you don't want to hit Mars, so you go through and calculate and sure enough according to your calculations you wouldn't, but now what degree of confidence do you have in those calculations.

PFEIFFER: That's the whole question. The degree of confidence is a philosophical question. In making some calculations how seriously we take them depends on your point of view. I think that the model we almost have for interplanetary missions looks very good - we're very optimistic. But still I would have to admit that's not the perfect answer. But you have to do something, and we should recognize that maybe what we're really searching for is some rational way to do the problem. A figure of merit, if you will. Any probability number which you come up with is really a figure of merit. It's some way of controlling what you're trying to do and describing what you're trying to do and that's all you can say, ultimately.

SMITH: Well, I think unless somebody has something else they want to bring up -- we've spent about an hour on this discussion -- I would like to declare the open meeting adjourned. And thank all of you for attending and helping out a great deal in making this meeting successful.

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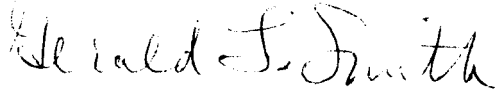University of Texas

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    Mr. Juris Vagners

It is concluded, on the basis of the meeting presentations and discussions that NASA trajectory estimation research and development efforts in the future can be profitably directed along the following lines:

    (a) New developments and refinements of basic theory as new problems are uncovered through experience in mission operation.

    (b) Concentrated research on definition of the environment and probabilistic modeling.

    (c) Continued development in the area of practical computer implementation.

    (d) Occasional specialist meetings at the working level for the interchange of ideas and new developments.

Sincerely,

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Attachments
  As stated above

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