NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

Technical Report 32-1247

Proceedings of the JPL Seminar on Uncertainties in the Lunar Ephemeris

Held at the Jet Propulsion Laboratory
Pasadena, California
July 20, 1967

Edited by
J. Derral Mulholland

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JET PROPULSION LABORATORY
CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA, CALIFORNIA

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Approved by:



W. G. Melbourne, *Manager*
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JET PROPULSION LABORATORY
CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA, CALIFORNIA

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Preface

Although recent changes in the computational base of the lunar ephemeris have improved its accuracy considerably, certain ambiguities remain. The final resolution of these uncertainties will very likely come about through the use of both optical and radar data, with the classical astronomers and the astrodynamicists playing complementary roles. The JPL Seminar on Uncertainties in the Lunar Ephemeris was organized so that representatives of these two groups could discuss their own work and become acquainted with that of others, in the hope that such contact would help scientists and engineers find common points of interest. The attendees were concerned not only with work directly on the Lunar Theory or the ephemeris, but also with research that is strongly affected by the ephemeris and which might be expected to verify corrections already made to it or point to corrections yet required. At the time these words are being written, events have already shown that this contact has, indeed, been fruitful.

The papers that follow, while they have been subjected to some editing, are basically informal discussions. With the exception of the first paper, they have been prepared from tape recordings, with the editorial cooperation of the speakers. Reference citations have been inserted by the editor where it seemed appropriate.

I thank Dr. Charles L. Lawson for sharing the chairmanship with me and Jay Lieske for assisting with physical details. Special credit goes to Mrs. Gale Storm for laboriously transcribing the tape recording of the seminar. Amateur astronomer S. J. Warkoczewski very generously supplied his occultation slides (Figs. 2 through 6) for use at the seminar and in this report.

J. Derral Mulholland
Editor

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Proceedings of the JPL Seminar on Uncertainties in the Lunar Ephemeris

I. Welcoming Remarks, *William G. Melbourne, Jet Propulsion Laboratory*

I am here to welcome you on behalf of JPL to the Seminar on Uncertainties in the Lunar Ephemeris. I would like to take just a few minutes to make some remarks.

I think that we are privileged to be able to hold such a meeting at the Laboratory. It is an appropriate time for such a meeting, because it seems that many things are coming to a head now. Many different data types are becoming available, and a great deal of increased interest in the motion of the moon is apparent. I think it is appropriate the meeting be held here at JPL for reasons that I will try to describe.

Insofar as JPL is concerned, we are sponsoring this meeting because (1) certainly there is a purely scientific importance attached to this work, and we do have an interest in that here, and (2) JPL has an even more pragmatic motivation, however, and that is because we are tied to the concept of earth-based radio guidance for navigational purposes. Interestingly enough, the lunar ephemeris plays a very important role in our capability of carrying out this activity. One of the things that limits us in the execution of precise navigational measurements is the knowledge of the locations of tracking stations on the surface of the earth. We are now engaged in a program to improve these locations, and, for this purpose, one needs an accurate lunar ephemeris. Let me give you a "back of the envelope" kind of argument.

If the station position is uncertain by an order of 5 m in longitude on the surface of the earth, then this uncertainty magnifies at the lunar distance by about 60, to the order of 300 m. Now, if one goes into the Solar System, using the astronomical unit scale, this magnification increases to something on the order of the inverse of the solar parallax in radians – something like 20,000. Then, a 5-m error magnifies into something on the order of 100 km. I am not suggesting that this simple-minded idea is to be entirely satisfied when one actually puts the uncertainties into a sophisticated orbit determination process, with all of the forces, motions, etc. Nevertheless, one finds that uncertainties of the order of 5 m on the surface of the earth create an ambiguity in our ability to pinpoint the spacecraft at planetary distances which is of the order of magnitude of 100 km.

One hundred kilometers at planetary distance has been adequate for missions up to the present time, but guidance requirements are becoming more stringent, because the scientific goals are becoming more ambitious.

We are attempting to improve the precision with which we can guide spacecraft. We would like to improve the uncertainty by a factor of 5 or more, so as to reduce that 100 km down to something on the order of 10 or 20 km. This requires a knowledge of station locations to within 1 m. One must know the difference between Ephemeris Time (ET) and UT1 to the order of 1 ms. This, in turn, means that one must know the lunar positions to within 30 to 50 m.

Justification for all of this is, I think, the dramatic improvement in the precision of radio tracking technology. Transmitter frequency oscillator stabilities have improved remarkably in the last few years; our rubidium standards have a stability of 5 parts in 10^{12}. It appears that even greater stability will occur with the application of hydrogen masers. To put the situation in more picturesque terms, a year or two ago the lake looked pretty placid, with most of the error sources below the surface. Now, the surface level is dropping and all of these dangerous rocks start appearing. The rocks are numerous; the lunar ephemeris is only one. There are also such things as earth pole wandering, plasma effects, tropospheric models, to name just a few. Unmodelled spacecraft low-thrust forces are another; 10^{-10} g acceleration on a spacecraft is the kind of thing that we worry about.

I have tried to put the problem into perspective. JPL is interested in a program to improve the structure of the theory describing the motion of the moon, as well as to improve the parameters associated with that theory. I hope that today's gathering, and the interchange that results from it, will help us in the future.

II. Remarks on Current Lunar Theory,

G. M. Clemence, Yale University Observatory

(read by Douglas O'Handley, JPL)

It must now be well known to all students of the Lunar Theory that W. J. Eckert has succeeded in bringing the solar portion of the theory to a high degree of perfection. Indeed, with the possible exception of one minor blemish arising from our lack of knowledge of the physical constitution of the moon, I am not able to think of any way in which Eckert's work could be improved upon.

It should not, however, be forgotten that the planetary portion of the Lunar Theory remains still in the condition in which Brown left it, and that Brown's treatment of the subject was less ambitious than his own work on the solar portion; whereas in the solar portion he generally included coefficients as small as 0".002, in the planetary portion there is no coefficient smaller than 0".003.

The planetary perturbations, at least in longitude, are as important as the solar perturbations as regards the number of terms, as may be seen from the following tabulation.

	Solar	Planetary
Number of terms in longitude	312	414
Number of terms in latitude	295	117
Number of terms in sine parallax	158	31

The number of solar and planetary terms are those in Brown's tables of the moon. Eckert's solar terms are of course much more numerous than Brown's. It should be remembered that Brown contented himself with lower precision in the parallax than in the other two coordinates, since he did not anticipate the possibility of radar observations.

Brown gives 83 planetary terms in the longitude having coefficients of precisely 0".003, and 23 in the latitude. The neglected coefficients of 0.002 and 0".001 must be numerous. By inspecting the rate of convergence of Brown's tabulations, I estimate that, in the longitude, the neglected coefficients of 0".002 may number 150, and neglected coefficients of 0".001 may number 250. In the case of the latitude, the corresponding numbers are 40 and 60. Thus, although the neglected terms tend partially to cancel one another at a given epoch, their algebraic sum usually amounts to a few hundredths of a second of arc. Furthermore, many of Brown's coefficients must be subject to end-figure uncertainties of at least a unit. Therefore, I judge that the precision with which the coordinates of the moon can be evaluated at present is not higher than a tenth of a second of arc, and probably worse.

It would be instructive and interesting to calculate the planetary perturbations of the moon by step-by-step numerical integration for a period of, say, 10 years, to combine the results with Eckert's solar perturbations, and to fit the sum as well as possible to the current lunar ephemeris. The discrepancies would show the deficiencies of Brown's planetary perturbations more easily than

can be ascertained by any other procedure. Of course, at this stage, it would be desirable to use the same values of the planetary masses used by Brown. Furthermore, the result would show precisely how urgent it is to recalculate the series-expressions for the planetary perturbations.

Discussion

None, because of the absence of the author.

Editor's Note

Dr. Paul Herget has observed that the procedure suggested amounts to an Encke scheme, with the earth–moon–sun system taken as the intermediary or reference orbit.

III. The Use of Occultation Data in the Study of the Lunar Motion, *Charles Martin, Aeronautical Chart and Information Center, U. S. Air Force*

My discussion of the use of occultations is directed primarily at long-term effects in the lunar motion. Briefly, I am going to analyze the occultation data from about 1627 through 1858 initially. Of course, Simon Newcomb (Refs. 1 and 2) published extensive analyses, not only of all the occultation data then available, but also the eclipse data and even the ancient observations. Following this, Spencer-Jones (Ref. 3) did exhaustive work in this area. Why am I repeating some of this work?

There are several reasons to justify such repetition. One of the principal reasons is the topic before us today, the accuracy of the lunar ephemeris. Newcomb was handicapped in that he was forced to use the Hansen Lunar Theory, which was the best available to him. He recognized some of the shortcomings in it, and his first effort was to modify the Hansen Theory. Thus, Newcomb's results are associated with a modified Hansen theory. Brown's *Tables of the Motion of the Moon* (Ref. 4) were available to Spencer-Jones, and he used them in determining differential corrections to the Lunar motion. It would have been better had he evaluated the full Brown Lunar Theory, but this would have required an impossible amount of hand computation. One of the major justifications for the present work is our capability through electronic computers, of evaluating the complete Improved Lunar Theory, augmented by the additional corrections that have been determined by Dr. Eckert (Ref. 5).

Another area of justification is in the accuracy of the stellar positions which were used. Those of you who are familiar with occultations know that the three most important aspects of the observation are timing, the lunar position, and the star position. It is true that serious errors and inaccuracies exist in the old observations. One hopes, however, that the time errors are generally quite random, as seems to have been partially shown to be the case by Newcomb (Refs. 1 and 2). While the errors are quite large, we assume that their randomness will still allow meaningful determinations. On the other hand, all of the stars used by Newcomb and Spencer-Jones had rather sharp observational histories, which created uncertainties in the accuracies of the star positions. We now have the zodiacal catalog, which can be reduced rigorously to the FK 4 system. This represents another major source of improvement in the data.

These two data improvements, combined with the capacity of electronic computers for performing much more extensive analysis than our predecessors would have considered, justify the reworking of these old data.

What have we accomplished with this work? We do not yet have any definitive results, so I will just sketch the pattern that the work is taking. We have obtained the U. S. Naval Observatory Lunar Ephemeris Program, which contains the complete series expressions given in the Improved Lunar Ephemeris (ILE) (Ref. 6). Eckert's corrections have also been programmed. For anything as extensive as the ILE, one needs to make some independent checks on the resulting values. These checks are being provided by a similar program which has been made available by Dr. Broucke (JPL). I have obtained agreement between the programs to within 0″.001 in longitude, latitude, and sine parallax. With slight modifications to Broucke's program, Mr. Van Flandern (U. S. Naval Observatory) has achieved even better agreement. These checks have been carried out all the way back to 1627, but do not yet include Eckert's corrections. One of the advantages of Dr. Broucke's program, incidentally, is that it can be used as a very neat subroutine and fits into the complete computation package with no difficulty.

For the stellar positions, we have already updated the zodiacal catalog to the FK 4 system. We intend to compare some of these positions with the Smithsonian Astrophysical Observatory catalog, which might help with some problematical identifications of stars with events.

All of this work is being facilitated by Newcomb's ingenuity and his conviction that someone might take up this work again. After some searching, we were able to find documentation for the basis of the time corrections that he applied to individual observations.

What do we expect to accomplish? The use of old observations is made more difficult by the lack of good corrections from Universal Time (UT) to ET. One of the purposes of the present work is to derive ET-UT differences over this time span, to facilitate the discussion of observations over long intervals. The late Dr. Brouwer made determinations of this quantity from 1820, but for dates prior to this, only estimates are available. Another aspect of the work (perhaps not so important, but interesting) concerns the long-term effects in the lunar motion. Specifically, one might expect to obtain some equinox and obliquity corrections to the star system over the interval concerned. Further, there is the question of the effect of Watts' limb corrections (Ref. 7), a factor unavailable to previous investigators. These provide timing corrections which take into account the irregularities of terrain on the limb of the moon. Watts indicated that he felt there was a bias of about 0".6 between the center of figure and the center of mass. I believe we will be able to study this.

We expect to have some preliminary results from this work within 3 or 4 months.

Discussion

Question: I am still not clear on what you are going to obtain finally. Do you expect to correct coefficients in the Lunar Theory or to apply numerical corrections?

Answer: We do not expect to correct the coefficients of the Lunar Theory. We expect primarily to obtain ET corrections for the period mentioned. Secondarily, the idea is to look for any indicated equinox and obliquity corrections to the star system; to look for any corrections that might be indicated to the node and perigee. One could not expect this analysis to affect the Lunar Theory, except for very long-period effects.

Question: So, primarily, you just want to extend the Lunar Ephemeris Time correction back before 1900?

Answer: Yes.

Question: Have you been able to program Watts' charts on the computer?

Answer: We made a start on this. As you may know, Watts' charts number about 1800, one for every 0.2 deg in position angle, so that we were not too enthusiastic about it. We now feel that we can mechanize the process with much less effort. There is a type of electronic device with which one can trace contours and generate coordinates directly onto punched cards or magnetic tape. Both the U. S. Naval Observatory (USNO) and the Aeronautical Chart and Information Center (ACIC) are presently considering obtaining an automatic device of this type which generates output at pre-selected intervals. ACIC already has a manual device like this, where you have to physically punch a button when you want a recording made. Thus, I can say definitely that these charts will be mechanized soon.

Comment (Van Flandern): I would like to add to the answer to the first question that, by obtaining accurate values for the orbital elements so far back in time, we will then have accurate values for the secular motions of these elements. Thus, we will be revising the fundamental arguments that go into the Lunar Theory.

Comment (Mulholland): This means that it would have a definite effect on the ephemeris; it would *not* affect the Lunar Theory.

Question: If you had enough confidence in the Lunar Theory to use it to determine ET corrections for as far back as 200 years, would this also mean that you could determine proper motions of stars in the same way?

Answer: Of course, any corrections to ET that we could get back in that time period would be more beneficial than what we have at the present time. I would be very reluctant to think that we could improve individual stellar positions, even though we may get corrections to the system — the equinox and obliquity.

Question: Is it not true that, to get meaningful results from such a study, you have to assume that you know the star positions accurately by other means, that you know it photometrically?

Answer: Yes, this is the basic assumption.

Question: Since you are getting ΔT, you have to have UT. How do you find some mean UT from all the observations? How accurate is UT required for a stated accuracy in the ET correction?

Answer: The UT times are available for all of the observations that I am using. This is a benefit that we have from Newcomb's work, and I do not think that we could improve on this part of his effort. Newcomb spent several years working with this problem. He visited observatories in France, Russia, etc., where the data were taken. We have the records that as many observations as he used he threw away, with the stated reasons for throwing them away. Beyond that, we must make the basic presumption that the remaining time errors are random. If they are random, even though they may be large, then we can expect to obtain something meaningful. Should there be bad systematic effects, we have problems.

Question: How did Dr. Brouwer arrive at his UT scale when he initially projected his values of ΔT back? I think his graph actually goes back to 1600, just extrapolated in some manner.

Answer: It was extrapolated from 1820; prior to 1820, the values are not right. In addition, he was not making an effort to re-evaluate the work of Newcomb and Spencer-Jones.

IV. The Grazing Occultation Observation Program, *David W. Dunham, Yale University Observatory*

There are three lunar coordinates that we can observe to determine or improve the moon's position: (1) the range, (2) ecliptic longitude, and (3) ecliptic latitude. The first coordinate has only recently been measured accurately by means of radar. The longitude has been determined primarily from timings of stellar occultations, which Mr. Martin just discussed. These observations have been made for over 300 years. The latitude can be measured by observing grazing occultations of stars by

the moon, which I will discuss. The observations of these events have been made only during the past few years, when the increasing availability of high-speed electronic computers has made it possible to compute the large volume of predictions needed to support an extensive observing program. The result has been to "turn the tables" on the relative accuracies of longitude and latitude observations. Simple occultation observations *can* be used to determine the moon's latitude, but not with very high precision, and very large numbers of observations must be combined statistically to get results. With grazing occultations, however, we can get much higher accuracy in latitude by means that I will describe in more detail. Latitude measures are now more accurate by an order of magnitude than those in longitude.

First, I will show just what a grazing occultation is. We see the moon moving through a star field. An occultation occurs when a star is eclipsed by the moon; the star disappears at one limb and then, approximately 1 hour later, reappears at the opposite limb. During a graze, though, the observer is situated so that the relative path of the star is tangent to the moon's edge. Thus, one observes the star disappearing and reappearing several times among the mountains and valleys along the limb of the moon. This is the phenomenon that we are after.

The idea is to observe the successive passages behind the lunar mountains from a number of stations just bracketing the graze, so that we can observe the profile of these mountains. We must get many data points, which can be compared with the profile predicted from Watts' limb correction charts (Ref. 7). Consider the geometry of the earth, the moon, and a star an "infinite" distance away. An image of the moon (the occultation shadow) will be cast on the earth by the star, and one finds a region of the earth's surface from which the star will be hidden behind the moon. As the moon moves in its orbit, this shadow sweeps across the earth's surface to describe a region of visibility of the occultation. Along the advancing edge of the shadow, the star will disappear; along the receding edge, it will reappear. The graze occurs at the northern and southern limits of the region of visibility. The lunar ephemeris, the star position, and a mean lunar radius are used to calculate these limits.

In setting up for a graze, we plot the predicted limit line on an accurate topographic map. Watts' charts are used to determine what the limb profile is likely to be. A series of observers are posted along a line crossing the predicted limit line, so that each observes a slightly different path adjacent to the limit. That is, there is a band parallel to the predicted limit line in which the star will disappear and reappear several times. This band depends on the terrain itself; if it is a low area on the moon, this band will be farther south than the predicted (northern) limit; if there are high mountains, we might have observers well to the north of the limit. The band from which these multiple disappearances and reappearances are visible is only 1 or 2 mi wide. It is much narrower than a total solar eclipse track, which is typically 50 to 100 mi wide. Thus, we get a much higher "leverage" with grazes than with solar eclipses. Furthermore, we usually know the star positions much more accurately than the solar position.

Next, I will show several series of figures. The first series shows the closest thing we have to a photograph of a grazing occultation. It is not exactly a grazing occultation; in this case the observer was about 5 mi south of the predicted northern limit. The photographs were taken with a 16-in. reflector owned by Stan Warkoczewski of Kansas City, Missouri (Fig. 1). The graze occurred in August 1964, and involved the 6.6 magnitude star 88 Virginis. The star at the top (Fig. 2) will graze the northern limb. Since the observer is so far south of the limit, he will see an ordinary occultation of very short duration. The star will disappear once and reappear about 5 min later.

The star is getting closer (Fig. 3), and you can just begin to see the dark edge of the moon illuminated by earthshine. Do not be confused by the illuminated mountain peaks, which appear just beyond the terminator.

The star has disappeared in Fig. 4. The pictures are being taken at increasingly longer exposures, so that the earthshine will stand out more. This, of course, washes out the detail on the bright side. Notice the two mountain peaks just beyond the northern crest of the moon.

In Fig. 5, you will notice that there seem to be three peaks; the farthest "peak" on the left is the star, which has reappeared. In a true graze, this will happen three or four times.

In the last photograph of this sequence (Fig. 6), the star has passed over the terminator and is just above the bright limb.

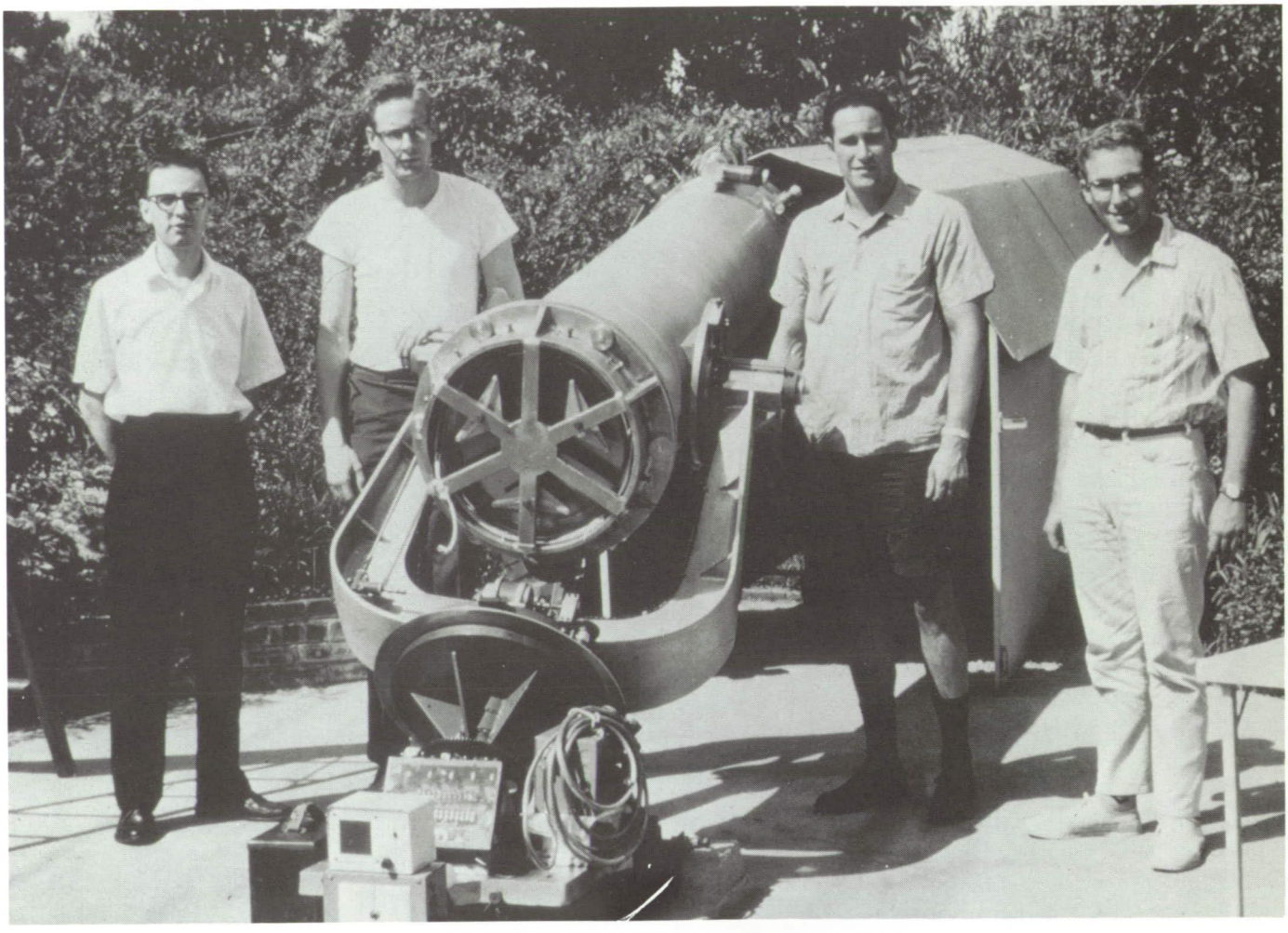


Fig. 1. Graze expedition of August 1964

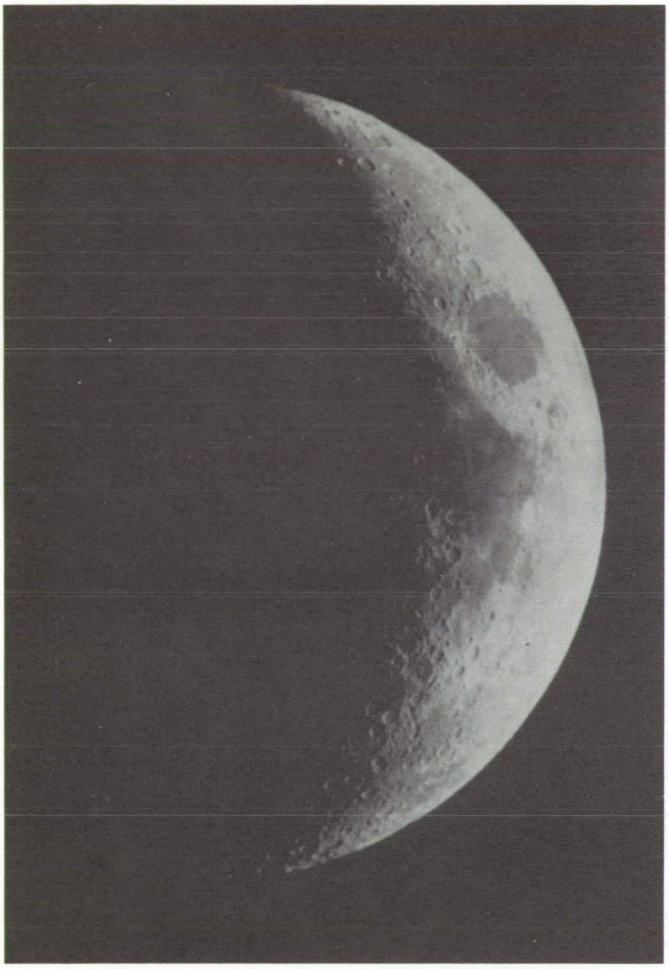


Fig. 2. Occultation of 88 Virginis (prior to occultation)



Fig. 3. Occultation of 88 Virginis [star approaching dark limb (top left)]



Fig. 4. Occultation of 88 Virginis (star occulted)

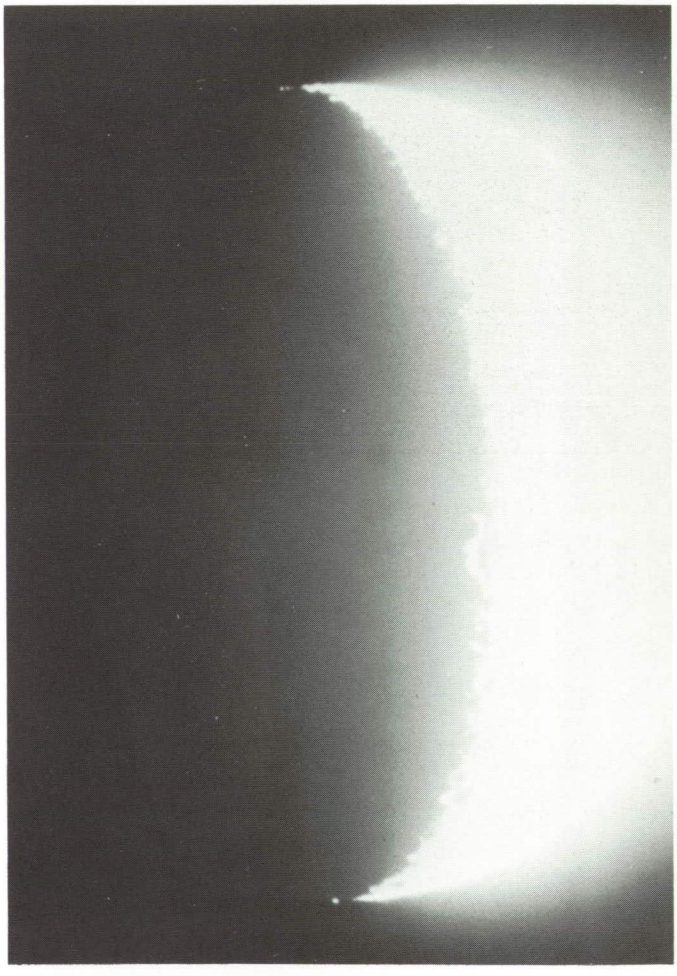


Fig. 5. Occultation of 88 Virginis (emergence to right of mountain peaks)

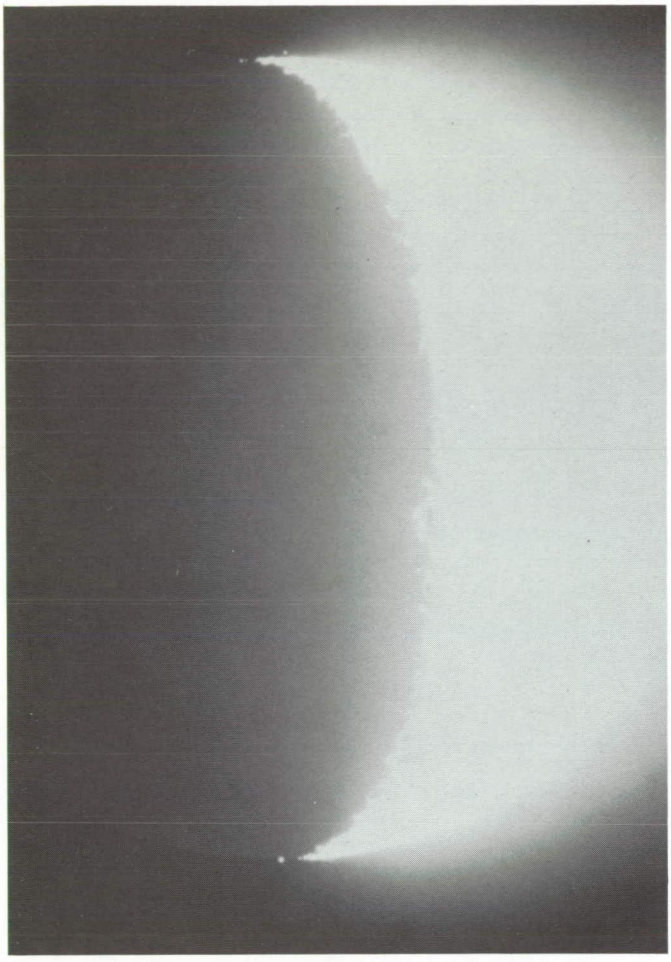


Fig. 6. Occultation of 88 Virginis (post-occultation, star over bright limb)

Figure 7 shows one of the predicted profiles based on Dr. Watts' charts. An observer at the predicted limit would see the star move on a horizontal path on this chart. In this particular case, the terrain is such that, for an observer on the predicted limit line, the star would just miss the highest peak completely. Observing at the predicted limit is not enough. One has to determine the band of the graze. In this case, it is a region 1.5 to 3 mi south of the predicted northern limit.

When the observations from such a graze are reduced, we get a whole series of data points that describe the profile. The observer makes a timing record each time the star disappears and reappears. If one has ten observers along the observing "fence", each one making perhaps six or seven timings on the average, then one gets about 60 contact timings for one graze. Each of these contact timings represents an observed point on the lunar profile. For each observer, the star will appear to follow a straight line, with contact timings that can be compared with the predicted lunar profile. The observed profile is fitted as well as possible to the profile from Watts' charts.

Some people get the impression that we are mapping the moon. We definitely are not doing that, except in a crude sense for certain regions of Watts' charts. These are the regions that are blank because they correspond to regions on the lunar limb which are never presented to view from earth except when they are dark, as a consequence of Cassini's third law (librations).

The accuracy that we get from these observations comes from the ability to compare with a predicted profile. With an ordinary occultation, only one or two timings are obtained, and one is never sure of the exact nature of the terrain. With a graze, we have whole series of timings providing a detailed profile which can be fitted to the predicted profile with an accuracy in latitude on the order of 0.02 or 0".03. This corresponds to about 30 or 40 m. I wish to emphasize that this is possible even though the individual timings may be accurate only to 1 or 2 s. The accuracy is supplied by the observer's position. The timings merely establish whether or not an observer sees a particular mountain occult the star.

At this time, there are numerous groups in the U. S. and several overseas that are actively involved in this program. The development of the program is briefly as follows: Jean Meeus began predicting grazing occultations in Belgium in 1957. In 1959, he was fortunate enough to have one of the predicted limit lines fall only 400 m from his private observatory. He had clear weather and observed the event, the first grazing occultation to be both predicted and observed. I began in 1962, and the first observation from my predictions was made by Leonard Kalish of the Los Angeles Astronomical Society, from near Castaic Junction in September 1962. This was the first time that a portable telescope was transported to a predicted limit line for an observation.

The first well-observed graze occurred in February 1964, when I led an expedition near Davis, California. We set up 12 stations bracketing the southern limit and used stop watches, short-wave radio for WWV time signals, and tape recorders. The ideal arrangement, used at some of the stations during this graze, is for the observer to call off events, with WWV in the background for time reference, and recording on magnetic tape.

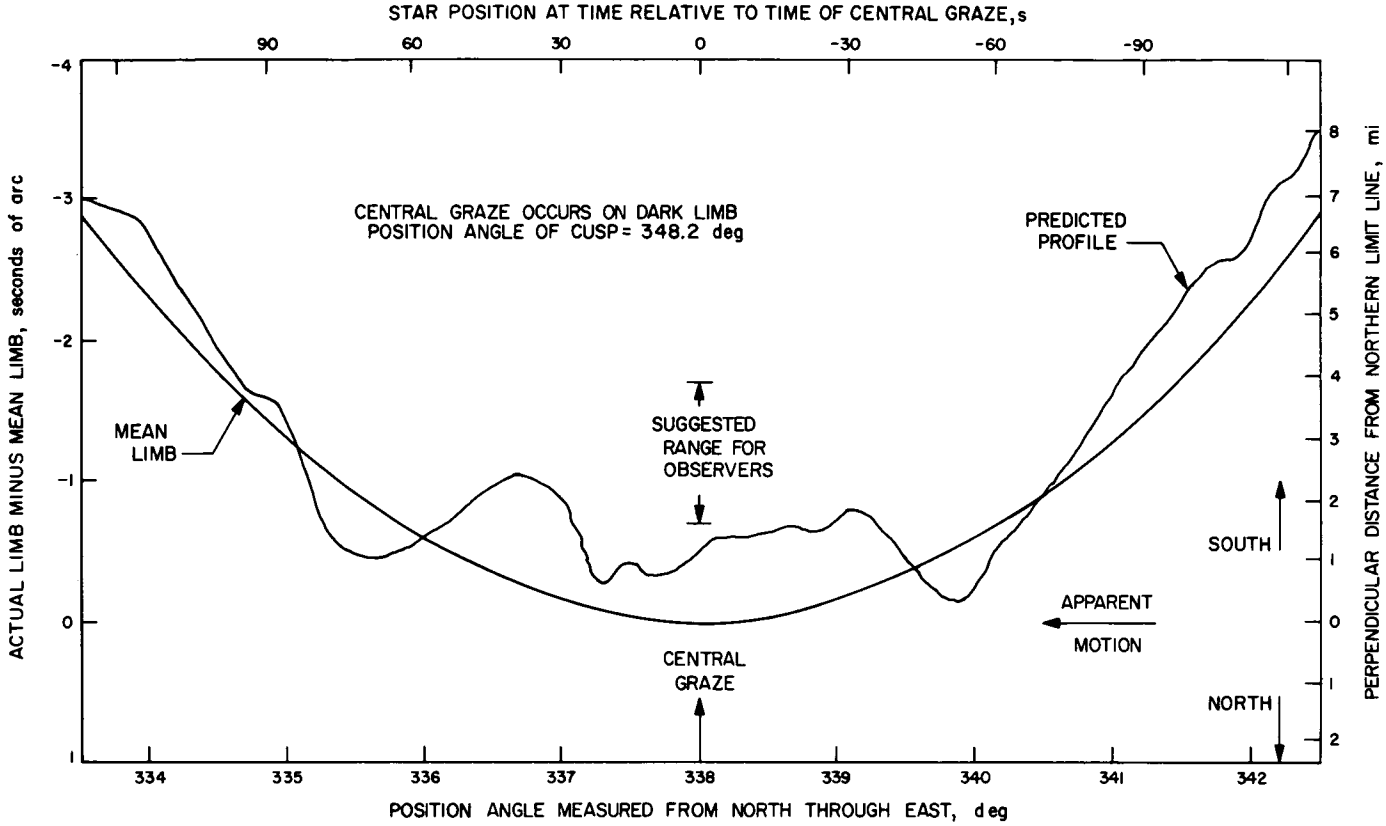


Fig. 7. Predicted profile for grazing of ZC 0614, July 24, 1965

With subsequent playback of the recorder, timings may be determined to the order of 0.1 or so. Figure 8 shows the results of the Davis expedition. The moon's limb has been straightened out and Watts' profile is plotted as the dashed line. The dotted lines indicate the path of the star relative to the various observers; the dots are joined by solid lines where the star was visible. The actual contact timings are indicated by the slashes. In this particular case, the contact timings agreed with the predicted profile to within the accuracy of the star position, but only after the Watts profile was shifted by 0.2 deg in position angle. This horizontal shift that we see in the observed features is an interesting result of the grazing occultation program. A shift of this magnitude corresponds to about 8 s of time, which is certainly within the observational capability of the program.

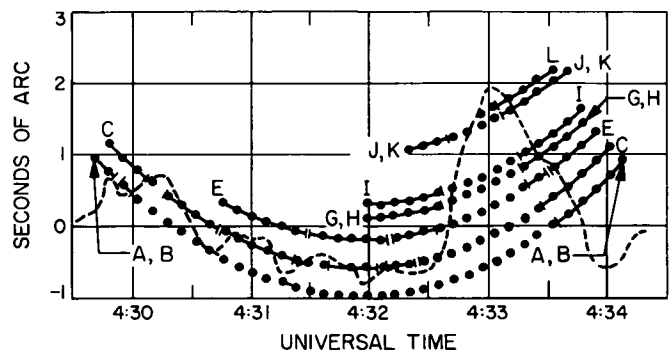


Fig. 8. Occultation of ZC 398, February 19, 1964

The next figure (Fig. 9) shows another reduction profile. In this case, the graze occurred in one of the blank areas of Watts' charts, so that all we could do was to fit a profile to the observations as best we could. There were some difficulties; some observers were apparently on the wrong minute on their stopwatches. One can see tracks where, judging from the other stations, the star must have been occulted.

In 1965, the USNO undertook to design and build a cable to facilitate these observations. The idea was to produce a piece of equipment sufficiently simple and inexpensive that amateur groups could duplicate it and join in a nationwide program. The cable is 8000 ft long, with electrical breakouts at 400-ft intervals. When a graze occurs, the cable is laid across the observing band,

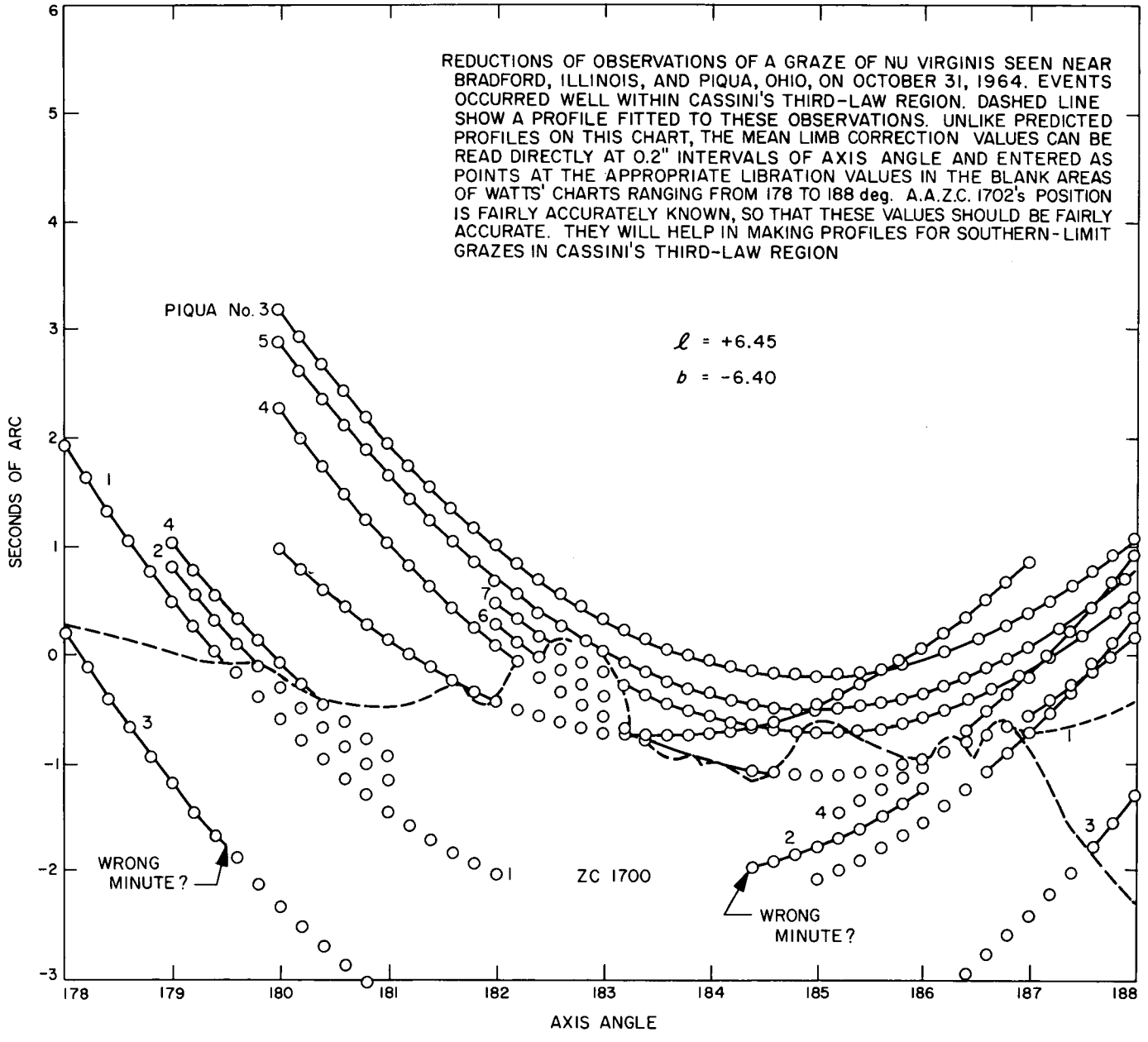


Fig. 9. Occultation of η Virginis, October 31, 1964

and observers are stationed at the breakouts. Each observer plugs a different frequency tone generator into the cable at his station, and the events are noted by pressing the button on the generator – once for a disappearance, twice for a reappearance. All of these signals are recorded on a single tape recorder at a central location on the cable, with simultaneous recording of time signals from either a portable chronometer or WWV.

The USNO had a very successful expedition to Virginia Beach, Virginia, where the city granted permission to lay the new cable along the boardwalk. They even turned off all of the lights along the boardwalk for an hour bracketing the graze, so that the observers could get dark-adapted. Twenty-four stations were manned (three off the cable), and something on the order of 100 timings were recorded. We are still trying to untangle the signals and reduce the data.

In 1966, Mr. Van Flandern and I led the first international graze expedition, from Yale University to near St. Hyacinthe, Quebec. Seven Americans and four Canadians manned eleven stations along the cable. We had a little difficulty on this trip, because we found that the trans-Canadian highway had been built across our proposed site since our maps were published. Fortunately, highway builders provided a bridge, so that we were still able to use the site. We did have to get the engineering plans for the bridge, however, to use in the determination of observing positions. Figure 10 shows the results of this graze. The highest mountain shown is about 4000 ft from base to peak. In one case, the observer bumped his telescope and missed two events, but he included this information in his report and it did not cause any problems. The recording of the sharp valley shown was especially interesting, because we had several signals coming nearly at once. Separating the timings was so difficult that we have named that feature Nightmare Valley. Of course, as before, the figure shows a tremendous horizontal-to-vertical scale distortion, about 40:1 in this case.

In the past year, a number of amateur groups have been building cables, following the USNO example, and have begun to secure observations.

Discussion

Question: In one of the earlier plots of a graze, there was a definite bias between the predictions and the observations. What is the magnitude and meaning of this bias?

Answer: It amounts to a shift around the limb of the moon of about 0.2 to 0.3 deg, which is very large. We are still studying it, trying to find a cause, and to correct for it. Shifts of this size are significant in applying Watts' data to regular occultations as well as grazes. Studying the systematic shifts such as this is one of the major goals of the grazing occultation project, along with the determination of latitude fluctuations of the moon.

Question: That is 0.2 deg at the center of the moon?

Answer: Yes, 0.2 deg in position angle from the center of the moon.

Question: Could this be due to incorrect positioning of these regions by Watts?

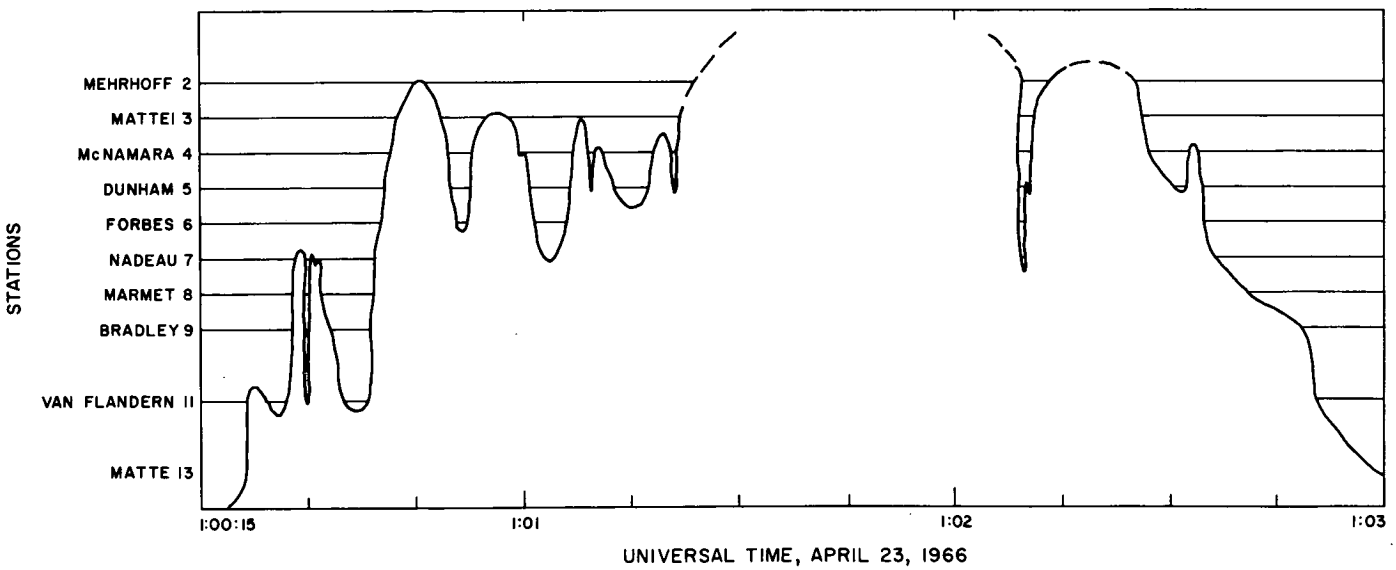


Fig. 10. Observed profile grazing occultation of 13 Tauri, St. Hyacinthe, Quebec, Canada

Comment (Van Flandern): That is a possibility. The magnitude, projected on the celestial sphere, of this discrepancy is about 4″, so it certainly is *not* in the lunar ephemeris. Just what this discrepancy is cannot be said yet, but it is being examined very carefully.

Question: Could you give an approximate figure for the accuracy of measurements of the latitude of the moon's center of mass?

Answer: To the accuracy of the star positions. The best star positions that we have are the FK 4 stars, but we do not have many observations of these stars. A single grazing observation can be fitted to an accuracy of 0.02 to 0″03, but the star positions are seldom that accurate.

Question: Is there a systematic shift all around the limb, or is it confined to one area only?

Answer: We do not know yet. We need more observations and we need to reduce more of the observations that we do have.

Question: Have you seen it only with one star?

Answer: No. We have seen this systematic shift in several grazes; the best-observed grazes show it time and again. It shows up most markedly in the southern regions of the moon. We do not yet have good enough data on the northern limb to confirm it there.

Question: Is it always in the same direction?

Answer: Yes.

Question: What do you mean by "same direction", rotational or lateral?

Answer: In the sense that, for southern limit grazes, the observed times are always earlier than predicted.

Question: Do you have any special interest in observing grazing occultations of binary stars, possibly observing the occultation of only one component?

Answer: Occasionally, we do have grazes of double stars. During the expedition last August, which several of those present attended near Castaic, a 5th magnitude star was observed under excellent conditions. The observer with the largest telescope (a 12-in. reflector) saw a faint star remain visible for a few seconds after the bright star disappeared, and then it reappeared a few seconds before the bright one. We can tell from the data that he observed a faint companion (8th magnitude) about 0.2 to 0″3 north of the primary. I have checked the catalogs and find no record of its being a binary. This observation was confirmed by Jim Young, using a 16-in. telescope at Table Mountain Observatory (JPL), who saw the companion star just before reappearance of the bright star from an ordinary occultation. Thus, by blocking the bright star, we can get information on unknown binaries or refine separations of known ones.

Question: What is the maximum number of disappearances and reappearances?

Answer: Eighteen. On two or three occasions, nine lunar features have occulted the star for some observers. I was fortunate enough to experience this during the Quebec graze. It is conceivable that ten or twelve features could occult during one graze with a very rugged profile, but four is probably an average number.

Question: How many foreign observers do you have?

Answer: We have quite a few in Canada. Excluding Canada, there are about 15 active observers and groups from whom we are getting observations.

Question: How do you measure the observer's latitudes and how accurately?

Answer: We take the observers' positions from the best topographic maps available. This should give an accuracy on the order of 100 ft, which is equivalent to 0.01 to 0″02 at the moon's distance. We are not concerned with refinements of the geodetic system via satellite observations, for example, although this surely will be done in the future. At this time, the inaccuracies in the star positions are much greater than those due to the observers' positions.

V. A Preliminary Report on a Lunar Latitude Fluctuation, *Thomas C. Van Flandern, U. S. Naval Observatory*

The past five years have seen a breakthrough in the study of the lunar motion and in the accuracy of the lunar ephemeris. Contributions have come in several different forms, some of which affect the observations and some the theory itself. The principal contribution has been C. B. Watts' *Marginal Zone of the Moon* (Ref. 7), a large volume which contains 1800 charts of the elevations and depressions at the edge of the moon for nearly all combinations of the libration arguments. One of the main advantages of Watts' work over previous charts is that, for the first time, corrections to the limb of the moon have been related to the center of mass of the moon with some degree of consistency. In the past, it was assumed that the geometric center of the moon was determined by the best fit to the limb. It was thought that this was approximately the same from night to night, and remained in some constant relation to the center of mass. Watts did not make these assumptions; in fact, the charts indicate that the geometric center shifts back and forth with respect to the center of mass with a maximum amplitude of about 1″5.

In addition, we now have an atomic standard of time which has a linear relationship to ET over periods of a few decades. This enables one to study the fluctuations in longitude of the moon, as well as those in latitude. In the very recent past, any observation of the lunar longitude was considered to be an observation of ET, no error being admitted in the ephemeris of the moon itself.

We also have W. J. Eckert's revision (Ref. 5) of the main part of Brown's Lunar Theory, and the International Astronomical Union (IAU) has recently adopted a new set of astronomical constants (Ref. 8). With all of these

factors, we should not be terribly surprised if a new investigation produced results that did not entirely agree with those of earlier investigators, particularly the results of Brown (Ref. 4), Newcomb (Ref. 2), Brouwer (Ref. 9), and Spencer-Jones (Ref. 3) earlier in this century.

Optical observations of the moon are being processed by three different divisions at the U. S. Naval Observatory. Time Service Division processes the dual-rate moon camera observations. The Six-Inch Transit Circle Division is undertaking a new reduction of their meridian circle observations – fundamental positions of the moon over 42 years. The Nautical Almanac Office is analyzing occultation observations, both regular and grazing occultations, in an attempt to get very accurate longitude and latitude measures. Today, I would like to report the very preliminary results from this last analysis. Only about 4% of the available data have been rigorously analyzed at this time. A report is being made now only because the implied changes in the constants of the Lunar Theory, and in the orbital elements of the moon, are surprisingly large, and disagree with those of previous investigators. It is important that the corrections to the constants are consistent. In the past, conflicting results were obtained from different types of observations, the results differing by several times their probable errors. Coefficients were found with periods for which there was no theoretical basis. We are now finding real coefficients for theoretically expected periods and near-zero coefficients for periods with no theoretical basis. We are getting consistency between various kinds of observations. Thus, we believe that we are approaching a theoretical basis for the reductions that is sufficiently accurate to give meaningful preliminary results at this time.

Based upon 4% of the observational material, we obtain the following corrections to the constants of the Lunar Theory adopted by Brown:

Parameters	Correction, second of arc
Inclination	$\Delta I = -0.33 \pm 0.1$
Eccentricity	$\Delta e = +0.33 \pm 0.02$
Longitude of perigee	$\Delta\omega = -0.8 \pm 0.1$
Obliquity of ecliptic	$\Delta\varepsilon = -0.18 \pm 0.1$
Equinox	$\Delta E = -0.36 \pm 0.2$
Longitude of node	$\Delta\Omega = +4 \pm 1$

These are for a current epoch, approximately 1960.

In addition to these solutions, an attempt was made to use meridian circle observations and occultation observations to determine the systematic drift between ET and Atomic Time (A.1). If the adopted value of the atomic second were fortuitously chosen, these two time scales would remain nearly coincident for several decades at a time. Since the length of the atomic second was based on just three years of observation, one would expect the difference to become observable in perhaps three additional years, or around the beginning of this decade. The latest determination, however, indicates agreement of the two time scales to within an accuracy of $1^s.5$ per century, based on 12 years of data (1955 through 1967). This is the probable error of the determination, *not* a correction; there is *no observable difference* between the rates of ET and A.1. This agreement has to be considered a remarkable coincidence in the original definition of the atomic second, since the new result includes all of the recent corrections, including the Eckert corrections and the IAU constants. For those of you who are using A.1, the latest determination of the constant, including all of the above corrections to be added to A.1 to get ET is $32^s.05$ to meet the definition of ET, $30^s.71$ for interpolation in the lunar ephemeris. The reason for the difference is familiar to most people who work in this area. The quantity $\Delta T = ET - UT$, as defined by the Paris Conference, is referred to the equinox of Newcomb. The lunar ephemeris, on the other hand, is referred to the equinox of the FK 4 system, requiring the use of a corrected value for ΔT. This distinction will be clarified in the National Almanacs beginning with the 1970 editions.

Some comment needs to be made about the correction to the node. We do not actually see residuals in either coordinate that are of the order of 4″. The principal effect of such a correction would be a fluctuation in the latitude with an amplitude equal to $\Delta\Omega \sin I$. Since $\sin I$ is about 1/11, the indicated fluctuation is about 0″.5. One needs some explanation why so large a discrepancy between the observed and theoretical values exists in the lunar ephemeris. Brown's theory and the observations that he used are presumed to be much better than that. I will present a hypothesis that will try to account for this correction. More numerical work is required to verify this hypothesis, but this is presented as a demonstration that a possible explanation does exist and that the size of the correction can be explained.

Figure 11 is a view of the mean outline of the moon as seen from the earth, which is presented in the

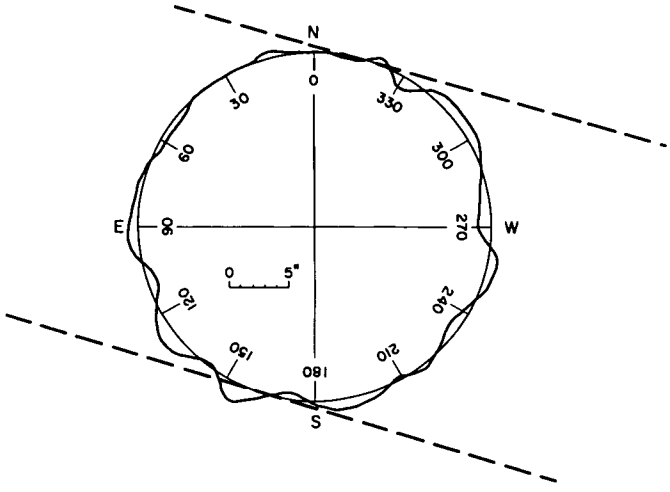


Fig. 11. Mean outline of the moon presented to the earth, motion indicated at ascending node

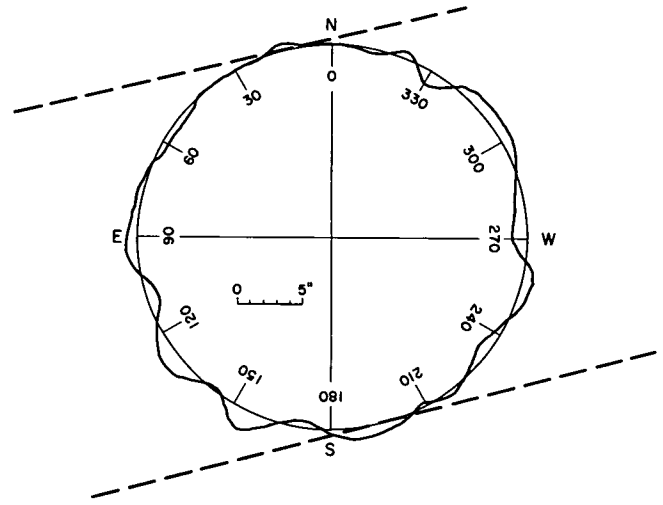


Fig. 12. Mean outline of the moon presented to the earth, motion indicated at descending node

summary at the end of Ref. 7. The southern portions of the moon are seen to be considerably more mountainous than the northern regions. The angles shown are measured from the rotation axis, and are called "axis angles" in the graze program, since they differ from position angles. The dotted lines represent the direction of motion of the moon near its ascending node on the ecliptic. The moon would be moving upward, relative to the ecliptic, at an angle of about 5 deg. Stars occulted by the moon would encounter it on a track from upper left to lower right. On the average, stars occulted near the south limb will encounter mountainous areas with steeper contact angles and will contact a mountain before reaching the mean limb. Without limb corrections, this would give the impression that the center of the moon was somewhat lower when at the ascending node than was really the case for the center of mass. The effect near the smooth north limb would be almost negligible. Before Watts' corrections, the observed moon would have been south of the true moon at the ascending node.

Figure 12 shows the same outline, but with the lines corresponding to motion at the descending node. Here, a star will encounter the southern mountains more directly. There will be much shorter duration between contact and reaching the mean limb. The situation is similar at the north limb. The net effect is that the observed moon would still seem to be south of the predicted one, but not so far south. The result of these circumstances is that, for over half a century, it was believed that the geometrical center of the moon was a constant 0".6 south of the center of mass. In fact, the average distance was 0".6 south but fluctuating with the period of the nodes. The geometrical center is much farther south at the ascending node than at the descending node.

Unaware of this, investigators who compared theory with observations accidentally introduced a term into the lunar theory with the period of the nodes (27.3 days), an amplitude of about 0".5 and with maximum values at the two nodes, a term like $+0''.5 \sin F$, where F is argument of latitude. The value derived for the location of the ascending node would have been 5" ahead (advanced in longitude) of the true position. Based on the considerations above, one would expect the present work to yield a correction to the nodes of about $-5''$ today. Instead, we find approximately $+4''$, nearly the opposite of what is expected.

One additional idea needs to be introduced to account for this situation. It has long been known that a discrepancy exists between the observed and theoretical rates of motion of the nodes which has not yet been resolved in a reasonable way, on theoretical grounds alone. Its size is about 10" per century. Based on limb correction arguments alone, there is no reason why the rate should be determined erroneously; the locations of the nodes would be determined 5" in error at any epoch, so the rate between epochs would be correctly determined. However, the nodes are determined with much less accuracy than the mean longitude, due to the factor of 1/11 mentioned earlier. This leads to the circumstance that the mean epoch of Brown's determination (using observations from 1750 to 1910) is approximately 1870,

while the mean epoch of the mean longitude determination is about 1830. This would affect the problem in the following way: speaking roughly, Brown would have determined the rate of motion of the mean longitude by the relation

$$L = (L_{1910} - L_{1750})/(UT_{1910} - UT_{1750})$$

In effect, this quantity defines the rate at which ET will will advance. It represents a mean fit of ET to UT over the period 1750 to 1910. The rate of motion of the argument of latitude of the moon (again speaking roughly) would have been determined by

$$F = (F_{1910} - F_{1840})/(UT_{1910} - UT_{1840})$$

since the mean epoch is 1870.

Both of these determinations would be consistent with one another and in agreement with modern observations if, indeed, the time scales defined by L and F were both consistent with ET. The L scale is consistent by definition. However, the quantity $(UT_{1910} - UT_{1840})$ differs from $(ET_{1910} - ET_{1840})$ by approximately 14 s. Brown did not know about the fluctuations in the rotation rate of earth, or at least he did not know how to account for them. The result would be the introduction of an error of approximately 10″ per century in the rate F.

If one combines the two types of error factors, one can conclude the following. From the relation

$$F = L - \Omega$$

one sees that, if F is in error by 10″ per century, so is Ω. Thus, if Brown's determination of Ω were −5″ in error at 1870, we would have to expect it to be in error by about +4″ at 1960. This is also consistent with results derived by Spencer-Jones and Brouwer around 1930, indicating a near-zero error in Ω at that time.

Tentative though this hypothesis is, it appears, from the preliminary work, that the corrections to the constants of the lunar theory and to the orbital elements of the moon are going to prove surprisingly large. These corrections may, in fact, turn out to be the largest yet to be introduced into the lunar ephemeris.

Discussion

Question: How would these apparent corrections affect the discrepancies in Dunham's graze observations?

Answer: They have no effect at all on position angles. Mr. Dunham did not say much about the discrepancies that we have found in the latitude, which are quite large. The graze observations, which are very sensitive to latitude, are especially suited to showing this correction.

Question: Are these corrections based on graze observations or on normal occultation observations?

Answer: Normal occultations, on the average, have only about one tenth the weight of grazing occultations in determining the latitude. The figures given for the nodes and inclination are based primarily on grazes. The correction to the nodes really is a compromise value between that indicated by graze observations and that indicated by meridian circle observations. At this stage of the investigation, we cannot be quite sure that all systematic effects have been removed from either or both determinations.

Question: Are the probable errors based only on the 4% of the observations that you have processed thus far? Do you expect them to decrease much?

Answer: Yes, we should see about a factor of 5 reduction in the probable errors when the final solution is complete.

VI. Interaction Between the Lunar Ephemeris and the Solution for the Lunar Potential, *Jack Lorell, Jet Propulsion Laboratory*

I have not yet had time to digest the meaning of the errors discussed by the previous speakers, but they seem to be a few hundred meters in size. I have been working on the problem of using *Lunar Orbiter* data to derive the lunar potential. The question arises: how do the uncertainties in the ephemeris affect this work? To anticipate the discussion that follows, I will say that the uncertainties in the ephemeris have comparatively little effect on the determination of the potential.

The procedure that is used to determine the gravity field of the moon is as follows. From tracking stations on earth, we observe the motion of *Orbiter* about the moon. The tracking data are used to determine a selenocentric orbit, and the observed deviations of the spacecraft motion from this orbit, which are due in part to the nonsphericity of the potential. We have both range and range-rate observations available, but we find that the use of the range data does not strengthen the determination of the selenocentric orbit. The primary contribution of the range data is to give the distance to the moon, which is not very important in the orbit determination.

What is the effect of lunar positional uncertainties when the primary data type is earth-based range rate measurements? First, let us look at the kinds of errors

that can be introduced in the range-rate data. Errors induced in range rate by inaccuracies in longitude or latitude will come about primarily because the earth–moon direction is not what the ephemeris says it is, so the vehicle is computed to be in the wrong part of its orbit. The error is small and is largely removed in the orbit fitting process. For example, the doppler signal due to gravity harmonics that we expect is on the order of 5 Hz, with a 0.1-Hz noise level. An error of 200 m in the longitude of the moon will affect the doppler with a maximum amplitude of about 10 Hz, which could presumably be detected in the data. When the orbit is fitted, however, this is reduced to 0.004 Hz, which is below the noise level. Similar arguments hold for latitude. In range, the initial error may be 20 Hz and will reduce to 0.001 Hz. Thus, the harmonic estimates will be very insensitive to errors of position.

The ephemeris error of greatest concern is the ephemeris error in range rate itself, since this would appear directly in the measurements. The effect of such an error, over the time span of our fits, would appear as a more or less constant bias in the residuals. Thus far, the signatures of all of the residuals that we have seen contain no discernible constant component.

Discussion

Question: I do not see how you can claim that an error in range rate would give a constant bias.

Answer: I guess I did not make myself clear. I am talking about estimating an orbit over about 3 to 6 h, so the errors in the ephemeris may be treated as nearly constant. Of course, this cannot be done over longer periods.

Question: You are essentially saying that you can determine the error in the range rate of the moon and separate it out. Did you determine it?

Answer: You should be able to see it in the residuals. We do not see it; the inference is that any such error is below the noise level.

Question: How many meters per second?

Answer: The noise level of the doppler data is 0.7 mm/s.

VII. Lunar Orbiter Ranging Data Analysis,

William L. Sjogren, Jet Propulsion Laboratory

We have now tracked *Lunar Orbiters* for nearly a year, and have obtained ranging data from each of the four vehicles. The geometry is shown in Fig. 13, with tracking station designated R, the earth–moon distance $A_{\mathbb{C}}$, and the position of the spacecraft r. Uncertainty in

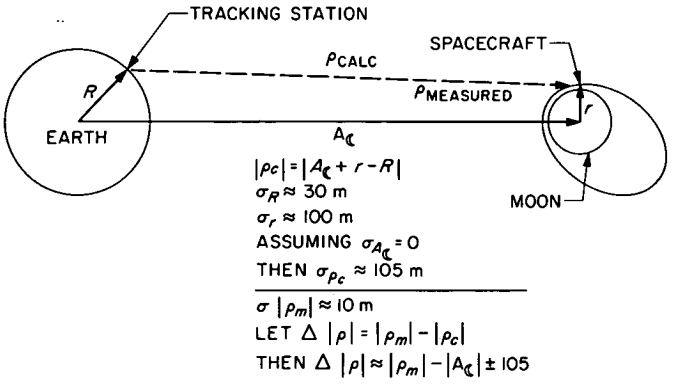


Fig. 13. *Lunar Orbiter* geometry

the position of the station is on the order of 30 m. The position of the spacecraft in its orbit bears a nominal uncertainty of 100 m due to the potential model, but this is open to question. The selenocentric position uncertainties due to ephemeris error are much smaller, as mentioned by Mr. Lorell. Using two orbits of simulated data about the moon, and assuming a particular set of initial conditions and a potential model, the moon's positional error was studied. We arbitrarily moved the moon in one coordinate, and then converged to a solution of the selenocentric orbit. One can see in Table 1 that the maximum residuals before the fit is 10 Hz for a 200-m perturbation in longitude and 20 Hz for a perturbation of 400 m in radial distance. After convergence, the residual in range rate (D) was 0.04 Hz in the longitude case, 0.001 Hz for the radial direction case, corresponding to thousandths of a meter per second. Similarly the selenocentric conic elements show little change, with agreement in the semimajor axis to eight decimals.

Returning again to the bottom of Fig. 13, we see that a $1-\sigma$ uncertainty in calculated range, assuming a perfect lunar ephemeris, is approximately 105 m. The uncertainty in the direct range measurement of the spacecraft is, we feel, about 10 m. For variations larger than this, there is only $A_{\mathbb{C}}$ left to accommodate the differences, which indeed exist. We can illustrate this with Fig. 14 (Ref. 14). In this graph, the circles indicate the residuals ($\rho_m - \rho_c$) that we obtained using the old ephemeris. They obviously show a systematic trend. At about the time the first of this work was being done, a new ephemeris was being generated by the application of Eckert's corrections. The correction in radial distance, supplied by Dr. Mulholland, is shown by the dark line. When the corrected ephemeris was applied to the data, the residuals indicated by the crosses resulted. Notice the second group of residuals, those for *Lunar Orbiter II*.

Table 1. Perturbations caused by lunar ephemeris errors

	(1) Parameters	(2) Δ Longitude ≈ 200m (do not estimate REM)	(3) Δ REM ≈ 400m (do not estimate REM)	(4) Δ Longitude ≈ 200m Δ REM ≈ 400m (estimate REM)	(5) Δ REM ≈ 400m no range point (do not estimate REM)	(6) 1σ Uncertainty
Selenocentric	ΔX m	1.	12.	0.2	0.0001	21.
	ΔY m	0.5	12.	1.	0.0002	114.
	ΔZ m	2.	9.	1.	0.0030	202.
	$\Delta\dot{X}$ m/s	0.0008	0.026	0.001	0.0002	0.016
	$\Delta\dot{Y}$ m/s	0.0024	0.11	0.005	0.0003	0.065
	$\Delta\dot{Z}$ m/s	0.0021	0.18	0.007	0.0004	0.089
Geocentric	ΔX m	+90.	−328.	98.	−343.	
	ΔY m	−166.	−226.	−158.	−166.	
	ΔZ m	−94.	64.	−97.	−54.	
	$\Delta\dot{X}$ m/s	0.0007	0.018	0.00009	0.0005	
	$\Delta\dot{Y}$ m/s	0.0015	−0.065	0.0035	−0.001	
	$\Delta\dot{Z}$ m/s	−0.0003	0.088	−0.0031	−0.0003	
	ΔREM			6.71		
Selenocentric conic	Δa m	0.1	0.6	0.5	0.00000	
	Δe	0.00000003	0.0000021	0.00000008	0.00000002	
	Δi deg	0.000084	0.007	0.00019	0.00002	
	ΔΩ deg	0.00019	0.004	0.00028	0.00001	
	Δω deg	0.00024	0.004	0.00030	0.000006	
	Δν deg	0.00000007	0.000003	0.00000003	0.000001	
	Maximum residual before convergence	D[a] 10. Hz R[b] 168m	D 20. Hz R 35m	D 30. Hz R 144m	D 20. Hz	
	After convergence	D 0.004 Hz R 12m	D 0.06 Hz R 375m	D 0.03 Hz R 4m	D 0.001 Hz	

[a]D = Doppler.
[b]R = Ranging.

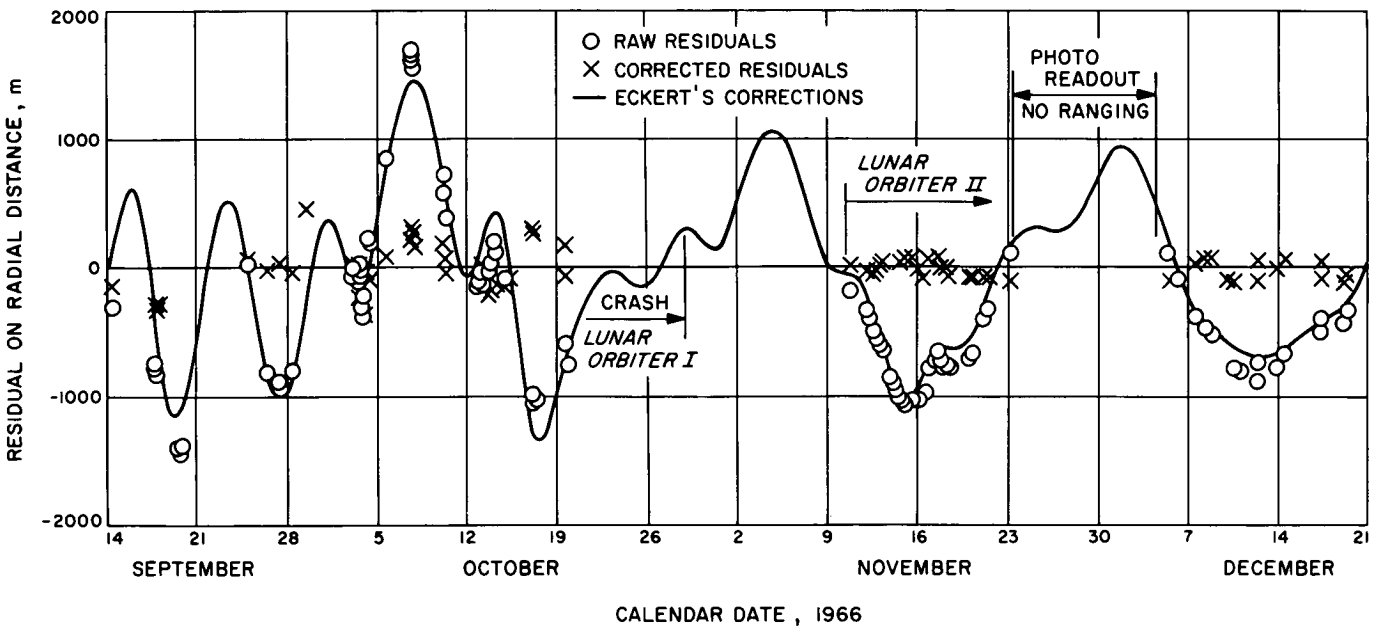


Fig. 14. *Lunar Orbiter* ranging residuals, September 1966 to December 1966

The residuals look very good, due possibly to a better potential model obtained from the calculation made on *Lunar Orbiter I* doppler data. (*Lunar Orbiters I* and *II* had the same inclination and geometry.)

Figure 15 shows the residuals that we have obtained during the period from December through June 1967. We had thought that the corrected ephemeris might take the residuals to about 100 m, but it seems to be more like 200 or 300 m instead. These points represent a lot of different sets of harmonics, and it is possible that some of this noise is caused by coefficients from low-inclination orbits in processing the high-inclination orbit data as in *Lunar Orbiter IV* (i = 80 deg). Hopefully, when the doppler data are all processed by Mr. Lorell's selenodesy system and a consistent potential model is obtained, we will be able to determine if there are still large systematic effects that have not been accounted for.

Discussion

Question: In your analysis, did you notice a tendency for the range residuals to become very large around pericentric passage, as they do for range rate?

Answer: Yes. The ranging residuals show the corresponding variations. The effects are of the order of 50 m at closest approach.

Question: Is it not true that there was a change made in the *Lunar Orbiter* orbit determination process that would tend to make the data of Fig. 16 not directly comparable with those of Fig. 15?

Answer: Well, no more so than what is shown in Fig. 15 for *Lunar Orbiter I*, for we had no idea of what the coefficients were, and arbitrarily picked Goudas model 2.

Question: No, I mean in the fitting procedure. Didn't they change the length of the data arc?

Answer: That is true, they increased the length of the data arcs. Actually, they did this to some extent for *Lunar Orbiters I* and *II* also, but after that they used the longer arcs consistently.

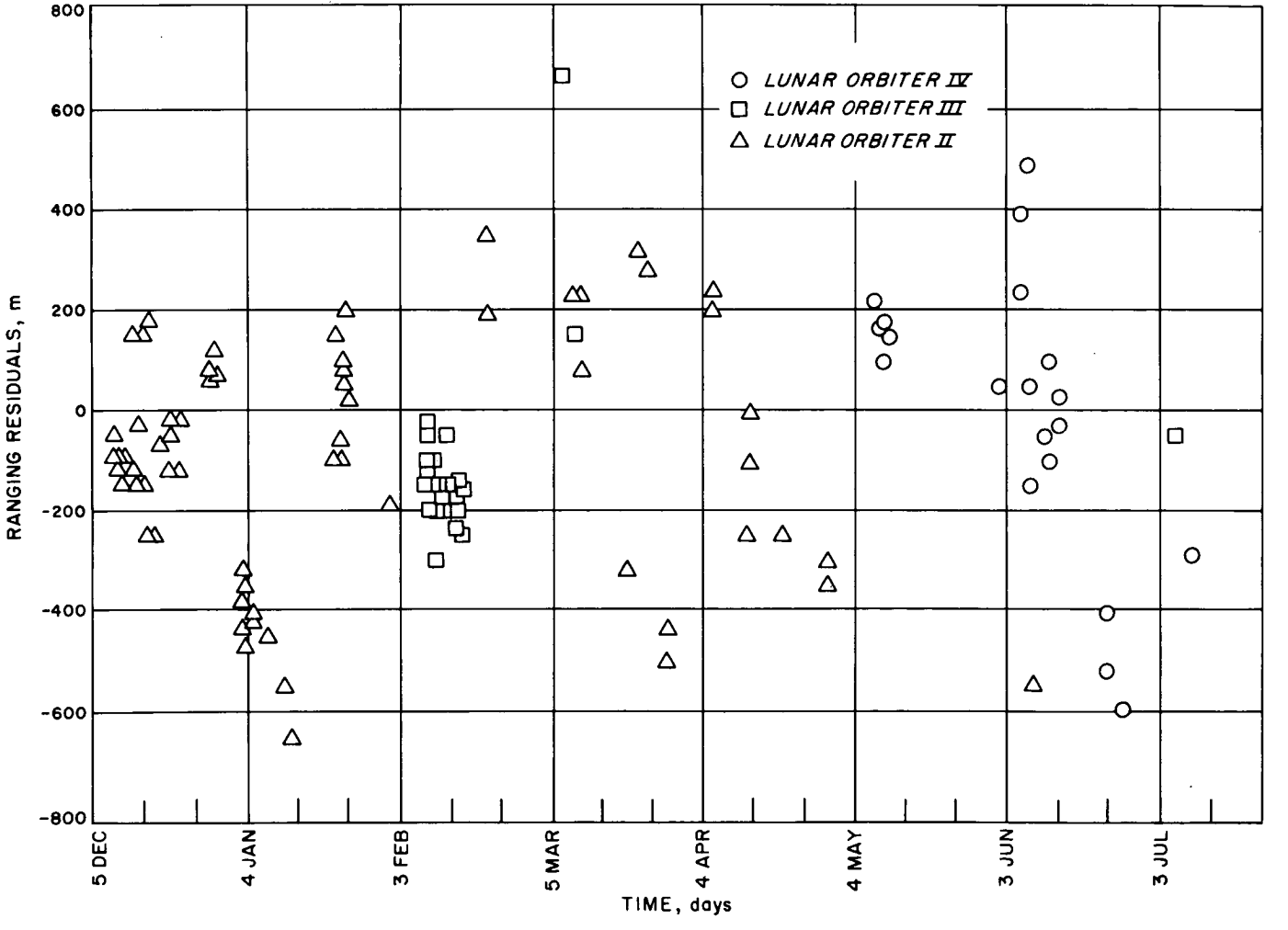


Fig. 15. *Lunar Orbiter* ranging residuals, December 1966 to July 1967

Question: I am concerned about the very broad spread of residuals over short time intervals. For example, in June you see a spread of something like 600 m for *Lunar Orbiter II* over a couple of days, with a maximum near +500. At about the same time, the maximum residual for *Lunar Orbiter IV* is bigger than −500 m. I think you are seeing something that is very large that is *not* ephemeris error.

Answer: Right. I intended to indicate that, until the lunar potential model is pinned down, at least a magnitude better than it is now, we are not going to find any significant improvements to the ephemeris from this approach. I said about 100 m for uncertainty due to the lunar potential, but it might well be 300 m.

Question: Did you change the lunar potential model?

Answer: Yes, these results are based on all kinds of potential models.

Question: Did you always use the same terms?

Answer: No, the data represent different terms, different orders. Some include up to fifth order; some include solutions for the model, some do not. The last graph shows that the potential model is still too uncertain to permit nice, clean results from the ranging data. Over one fitting arc of three or four orbits of data, the ranging residuals are clean, with no extraneous noise.

Question: In Fig. 15, which is essentially the one published several months ago, was there this experimentation with the potential model in the reductions?

Answer: Yes, but for *Lunar Orbiter II*,[1] they used some of the results on the potential model determined from *Lunar Orbiter I* and, since *Lunar Orbiters I* and *II* were in similar orbits, there must have been some benefit reaped there.

Question: Is the ranging restricted to any one tracking station?

Answer: All three stations range: Woomera, Madrid, and Goldstone.

VIII. Precision Time Synchronization Experiment,

John R. Smith, Jet Propulsion Laboratory

The Jet Propulsion Laboratory operates a network of tracking stations that are strategically located around the world, to enable 24-h tracking of any spacecraft. To make the tracking data as useful as possible, it is necessary to ensure that the time tags applied to these data be as consistent as possible. Recent studies have shown that, in the past, inadequate time tags represented the largest single error in the tracking data.

Studies for the quality of orbit determination, and particularly for the *Lunar Orbiter* selenodesy experiment, indicate that relative clock accuracies at the tracking stations of 20 μs are required, and 5 μs would be desirable. The station clocks are driven rubidium standards. These clocks have drift problems and occasional failures, so that it is sometimes necessary to restart or resynchronize the clocks at the stations. Classically, the information used for restarting has been high-frequency WWV. Our experience indicates that, at overseas stations, this is accurate to approximately 5 ms, which is not good enough for our purposes.

We examined some possible systems for obtaining better time correlations between stations. It is possible to use low-frequency VLF transmission for perhaps 3 ms accuracy. Recently, the National Bureau of Standards introduced a technique of using simultaneous or near-simultaneous VLF transmissions at two different but close frequencies. This can produce accuracies of about 50 μs. We found, however, that some of our stations cannot receive WWVL, so that we cannot get timing by this method. Alternate methods of satisfactory accuracy include the transportation of a portable cesium clock between stations. JPL has such a clock and has used it for this purpose. The method has an accuracy within 5 μs, but it is rather expensive and it can only be used on an occasional basis due to traveling schedules. In addition, a technique has been used with the *Lunar Orbiter*, using the ranging equipment, which has a nominal accuracy of 5 μs. The true accuracy may be more like 1 μs. The technique works very well but can only be used after a spacecraft is in flight. Furthermore, the spacecraft will not always be available due to project requirements.

We have chosen to investigate a system that is very similar to the spacecraft ranging method, except that the moon is used as a passive reflector (Ref. 11). Figure 16 shows a simple representation of this system. In principle, one could send a single radar pulse from the transmitter at Goldstone and monitor the time of its arrival at the distant station. In practice, this is difficult to do because of the signal/noise ratio. We have used, instead, a pseudo-random code together with correlation techniques. The receiver compares the received signal with the local model and produces output power (the indication of a received signal) only if the received code is exactly in alignment with the local model. Thus, we can transmit energy, reflect it from the front cap of the moon, and determine its time of arrival at the point where the correlation occurs. In practice, we account for the round-trip time of the signal by advancing the transmission time by the predicted travel time. The signal advance is controlled by a computer at the transmitting station; this computer also points the antenna and

[1]The Boeing Company was responsible for the real-time data reduction from which these results were obtained.

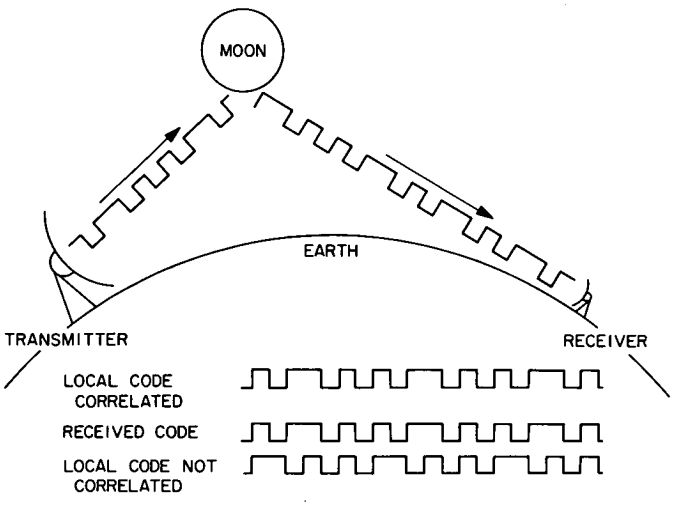


Fig. 16. DSN synchronization concept

controls the transmitted frequency. For reasons of economy, the receiver is fixed-tuned and the doppler shift due to the moon's motion is corrected at the transmitter.

Figure 17 represents a typical output from the receiver. We actually begin transmitting 30 μs ahead of the predicted time that would cause correlation if the clocks were exactly synchronized. At 1-s intervals, the transmission is retarded by 1 μs. Thus, we try successive apparent round-trip times, corresponding to different apparent clock differences, until the first correlation occurs. Our code has a 10-μs bit, so that it takes 10 s (on the figure) to reach full correlation. The system is so designed that the round-trip time to the moon's front cap is exactly corrected when correlation first occurs. Any offset from the predicted time of first correlation is, then, receiver clock offset. Since we shift the code at 1 μs/s, Fig. 17 shows a magnification factor of 10^6. By extrapolating down the curve to the point of first correlation, we can determine the clock error. For the case shown, the error was 8 μs. As we shall see later, the system has an apparent accuracy of 5 μs.

The basic block diagram of the system is shown in Fig. 18. Essentially all of the complications of the system are in the transmitter, because the system requires only one transmitter, while there may be receivers at many locations. The receivers are very simple. The station clock drives the local code generator, which is correlated against the received signal in the receiver proper. The output is monitored on a recorder. At the transmitter, timing is provided by the master clock, which at the moment is indirectly set to the Naval Observatory by

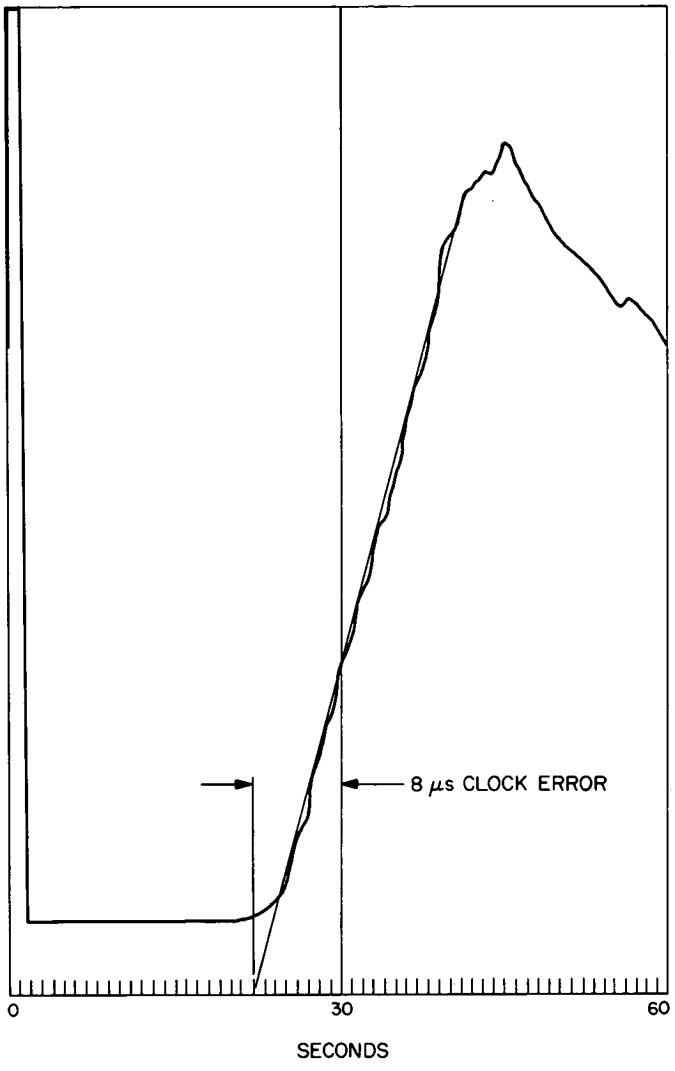


Fig. 17. Typical calibration curve for clock synchronization experiment

means of the portable standard. The time and the lunar ephemeris are input to a station control computer, which predicts the round-trip time, advances the coder accordingly, controls the transmitter frequency, and points the antenna. The transmitter operates at X-band with 10 kW of power, providing more than adequate signal strength at the receiver. Table 2 presents the transmitting station parameters, Table 3 those for the receiver. The carrier frequency must be suppressed in transmission, because it would always be correlated; 45 dB is considered to be very good. The code length of 2047 bits, each with a 10-μs period, means that we have repetition rate of about 20 ms. This represents a 20-ms ambiguity in the system; if the clocks differed by that much we could not detect the error with this system. An error of this size is easily resolved by other systems, however.

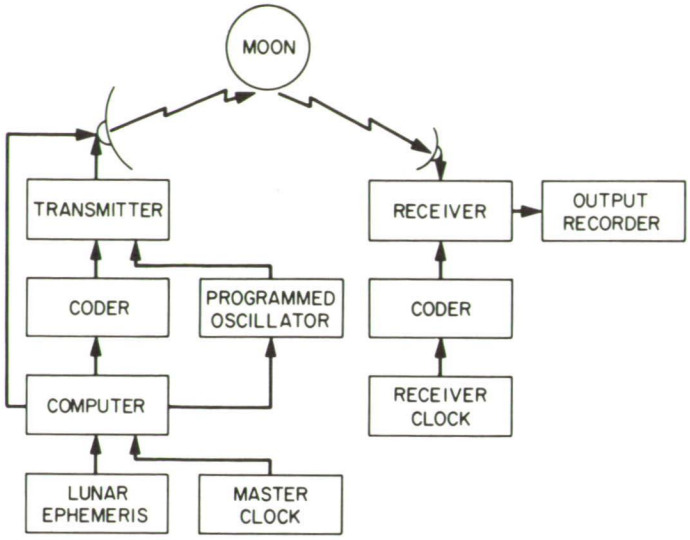


Fig. 18. DSN time synchronization system

Table 2. System parameters—transmitting station

Parameters	Requirements
Frequency	8450.1 MHz ± required doppler offset
Power	10 kW
Carrier suppression	Greater than 45 dB
Code length	2047
Code digit length	10 μs
Station controller	SDS 910 computer
Antenna	30-ft parabola—azimuth elevation mounts
Antenna gain	56 dB
Antenna beam width	0.27 deg

Table 3. System parameters—receiving station

Parameters	Requirements
Frequency	8450.1 MHz
Noise bandwidth	10.94 Hz
Noise figure	6.1 dB
S/N ratio	9.4 dB
Antenna	4-ft parabola
Antenna gain	38 dB
Antenna beam width	2.1 deg
Post-detection time constant	10.5 s

The receiving antenna is shown in Fig. 19. It is a commercial 4-ft dish antenna with a tunnel diode amplifier mounted at the feed. The base structure, which was made at JPL, is a polar mount, manually controlled in declination, and motor-driven in hour-angle. The drive rate is stepwise variable, controlled from the base

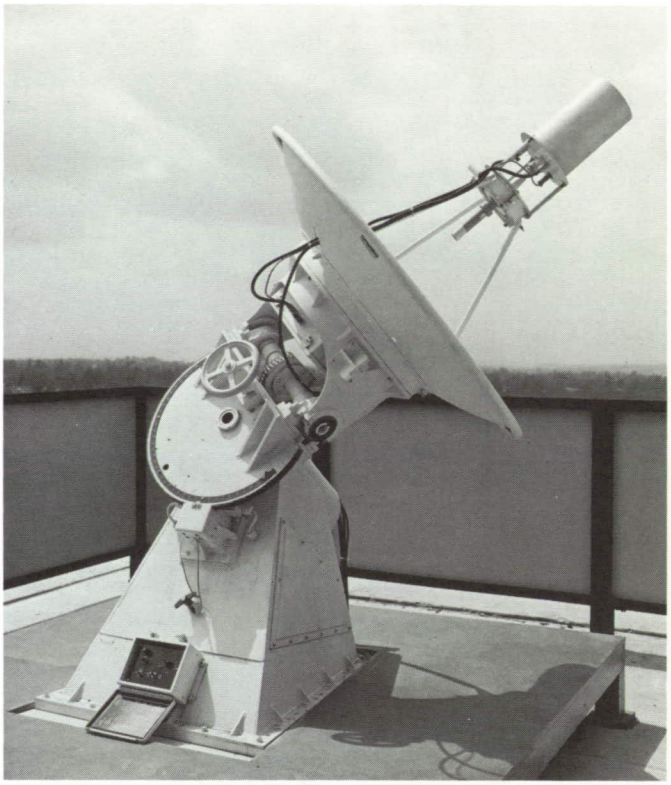


Fig. 19. Receiving antenna for clock synchronization experiment

panel, to provide for the range of hour-angle rates required in tracking the moon. We can track in hour-angle satisfactorily over an entire pass. The fixed declination limits us to about 6 h but, since a common view period with the transmitting station is typically only about 4 h, the antenna can track virtually unattended for an entire transmission period.

The transmitting station (Figs. 20 and 21) uses a 30-ft parabolic antenna located at the Venus site, Goldstone. The antenna is on an azimuth-elevation mount. The control equipment is sufficiently automatic so that the operator has virtually nothing to do but sit in front of the computer and watch the lights flash. The computer drives the oscillator at X-band frequency with such precision that we are unable to measure the error. The entire station is regarded, at this time, as experimental equipment. We hope to replace it with a permanent facility in the near future.

Table 4 lists the potential error sources in what we estimate to be their order of significance. The highest is lunar ephemeris error, which for our purposes includes

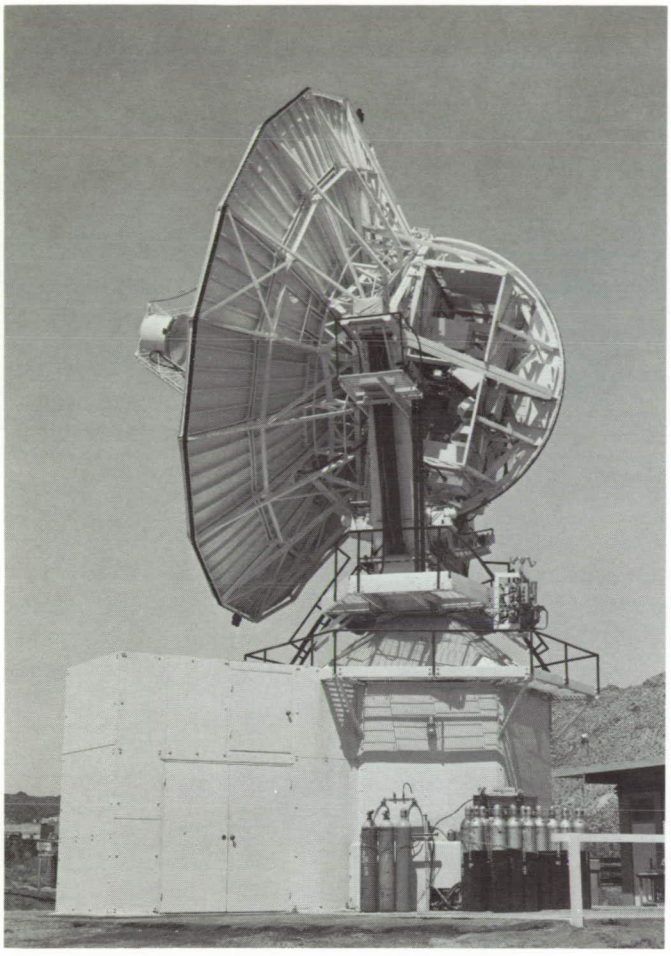


Fig. 20. Transmitting antenna for clock synchronization experiment

the errors in assumed station locations and moon reflection points. After that comes the multiple-path reflection effects. The remainder are the various equipment-dependent errors, which appear to be negligible for our system. A measure of these possible errors is given by the data of Fig. 22. This group of points was taken, 1 point/min for 8 h 30 min, with the receiver located at JPL. The skewed mean indicates some rather significant clock drift during this period. The 3-σ is about 5 μs, most of which is contributed by the yet unexplained dip near 2 h.

The only implication of the skewness of the data is that one clock was drifting relative to the other. Independent measurements have shown that the receiver clock was doing most of the drifting, while the transmitter clock was drifting in the opposite direction at about 0.2 μs per day.

Table 4. Sources of errors

No.	Source
1	Lunar ephemeris error
2	Range error due to rough surface and variation from a true sphere of the moon
3	Variations in delay time within the transmitter
4	Station location error
5	Error in curve interpretation due to roughness caused by lunar multiple path reflections
6	Code jitter
7	System noise

I wish to emphasize that this project is an experiment. We are in the process of equipping the stations of the Deep Space Network with receivers. Another station is being installed at the National Bureau of Standards, Boulder, Colorado, which will enable us to tie in with their clock. Within a year, receiving stations will be located at all major tracking complexes around the world, providing them with 5-μs time correlations. We currently are using this technique to obtain time correlations at a receiver at Pioneer site, Goldstone, on a daily basis. Because the Venus and Pioneer sites are only a few miles apart, we can make independent measurements of the clock differences between the two stations. In effect, we monitor the apparent accuracy of the lunar ephemeris. With our 5-μs timing resolution, we are limited to about 400-m accuracy in the measurement of overall ephemeris accuracy, including the transformation process. To this accuracy, we have been unable to identify any ephemeris error at all.

Discussion

Question: You said that within a year you will have receivers at all stations. When will this be operational?

Answer: Within a year, we will have receivers at all major complexes, not all stations. For example, only one receiver will be installed at Madrid, where there are two stations. The system will be operating at that time, but it will still be classified as experimental, because we will still be using the research and development transmitter at the Venus site. The Venus station is an experimental system.

Question: When will it become an operational system?

Answer: If we receive funding to build an operational transmitter, the system will be operational when the transmitter is completed. We think it will take two years to build after we receive authorization.

Question: You stated that, with your 5-μs resolution for individual points, corresponding to 400 m in range, you have seen no ephemeris error. Have you seen anything that you suspect to be ephemeris error when dealing with the normal points, composed of 10 individual points?



Fig. 21. Control room equipment for clock synchronization experiment

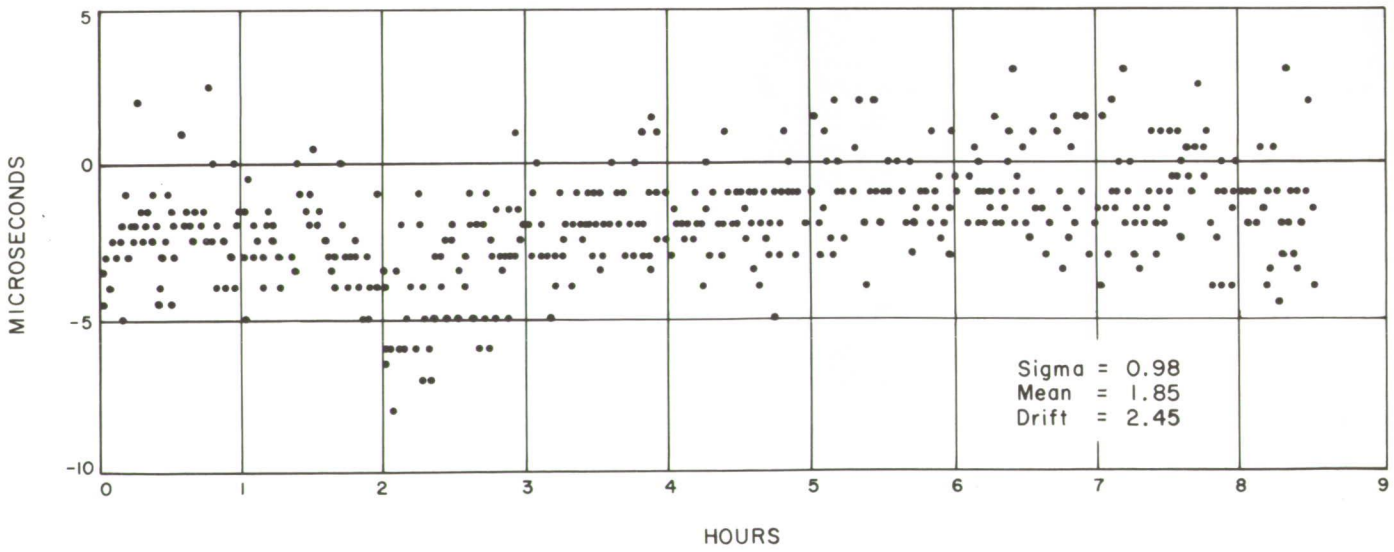


Fig. 22. Clock synchronization measurements (512 over 8 h 30 min period)

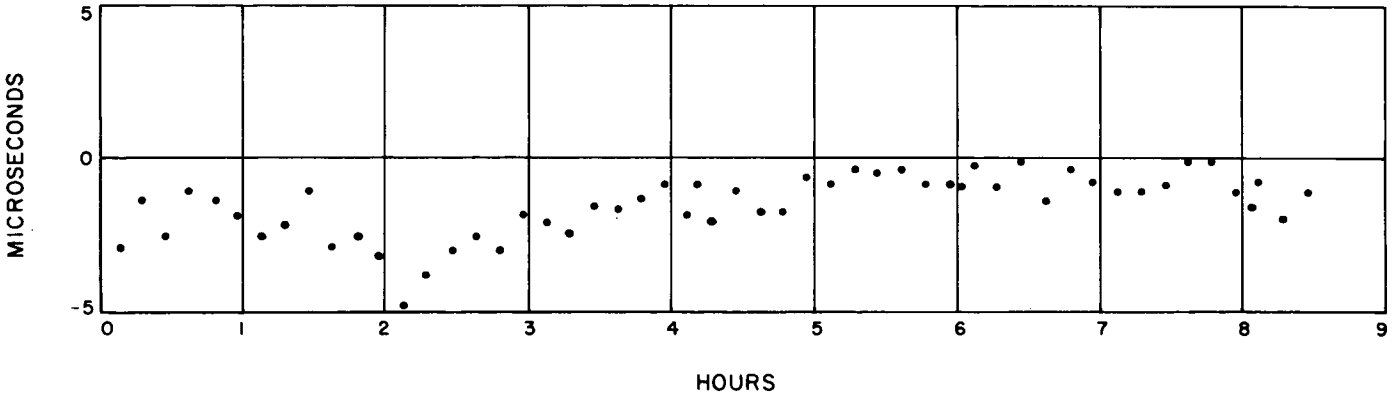


Fig. 23. Clock synchronization measurements (512 over 8 h 30 min period — points averaged in groups of 10)

Answer: I have no indication at all of any errors in our ephemeris, which includes the latest corrections by Dr. Eckert. The "jitter" in the measurements is the largest contributor to the system inaccuracy.

Question: You take out the doppler shift at the transmitter station?

Answer: Yes.

Question: You correct the doppler with respect to a particular receiving station?

Answer: Yes, we do — we must. We can transmit to only one station at a time both because we must correct for the doppler shift to that station and also because we must correct for the time of flight of the radio signal to that particular station. I might point out that this system is capable of providing time synchronizations to each of our Deep Space Stations every day, with the possible exceptions of Madrid and South Africa. For these two stations there will be one or two days each month when the common view period with Goldstone is only a few minutes, which may not be enough to provide a quality time synchronization measurement.

Question: How do you know that you are reflecting only off of the front cap of the moon?

Answer: We monitor the arrival time of the first signal, and that can come only from the front cap.

Question: What about the radius of the moon used for the prediction?

Answer: This is a possible error source in the system, and I have included it in ephemeris error. The predict program uses a constant radius, determined from earlier lunar mapping done at JPL. While we reflect off a relatively small area of the moon, the signal is averaged over a large area relative to the surface features, so that small scale local variations do not show up.

Question: How does your lunar radius compare with the average for that area as given by ACIC or the U. S. Army Map Service (AMS)?

Answer: They are essentially equivalent.

Question: To what level?

Answer: To the accuracy that we are interested in.

Question: To about 0.5 km?

Answer: To about 100 m.

Comment (unidentified): I think you are inconsistent with your own group here. I think the *Ranger* series indicated a radius of 1736 km, while ACIC and AMS give 1738, so you have a 2-km systematic error there. Besides, I think you are really underestimating the errors due to topography by a sizable amount.

Comment (Knowles): At the Naval Research Laboratory, we have been doing fairly similar work, except that we use a shorter pulse length. We have actually gotten heights of certain specific areas on the lunar surface. We *do* find that, using a 1-μs pulse, we get differences at different times of the month of up to 1.5 km. We have ascribed these to topography and not to the lunar ephemeris.

Answer: We have monitored these signals ourselves over long periods of time. The radar reflection point describes a complex, nearly elliptical figure on the surface, with a period of about 1 month. You would scan everything of interest in 1 month. We have not seen any variation over several months. However, we are averaging over a large enough area that it might account for the differences in our results.

Question: Was any consideration given to using the 24-h satellites for this purpose?

Answer: This has been considered, but it presents the same basic problem as does the use of other spacecraft. We, at the tracking complex, tend not to have adequate control over the scheduling of spacecraft functions. The spacecraft may not be available when we need it.

Comment (Clarke): It seems to me that a space company would be interested in selling this type of service, just like calling on the telephone to get the time. It would be an international service that everyone could take advantage of.

Answer: If we could predict the time delay through such a system, this might be a very desirable way to handle it.

Question: If you were to use another satellite, wouldn't the receiving equipment have to be more complex?

Answer: Perhaps, but perhaps not. A transponder sitting on the moon, an *active* reflector, would allow about an order of magnitude improvement in our accuracy without any change to the transmitter or receiver. Some programming changes would be necessary.

Question: You have 5 μs from one of these signals?

Answer: That is 3 $-$ σ. I base that on consistency. I really do not have enough data to make a statistically significant estimate of the real accuracy of the system. Based on the data we have, the accuracy is 5 μs, but that is still a preliminary figure.

Question: Could you make any comparisons of these results with the other two high-accuracy systems?

Answer: We have used a portable clock to transfer time from the transmitting station at Goldstone to the receiver, when it was here at JPL. The two methods agreed consistently to within 1 μs. We have made one very careful measurement of the agreement between the time-synchronization method and the spacecraft ranging system, using *Mariner V*, which is fairly insensitive to the lunar ephemeris. These data differed by about 5 μs, with about 8 μs total uncertainty.

Question: I understand that you originally used a second receiver near the transmitter as a self-calibration device. Is that no longer used?

Answer: We use it for self-calibration in the sense that we check the system with the receiver at Goldstone. It is on the basis of the data from this receiver that I base the statement that we have seen no ephemeris error. We have not had to correct any errors, so we have not actually *done* any calibration, but we do *check* the calibration daily.

IX. Position Anomalies of Landed *Surveyor* Spacecraft, *Clifford Cary, Jet Propulsion Laboratory*

We have been working with the tracking data obtained from the *Surveyor I* spacecraft after it landed on the moon. The spacecraft was tracked continuously for about 2 weeks after landing, before going into shadow. Although it survived for several months, only a few sporadic data points were obtained after this first period. Thus, most of our work has dealt with only these 14 days of data from June 1966. Since the spacecraft carried no ranging transponder, these are range-rate measurements only. We are trying to use these data to obtain the best estimate of the location of the spacecraft landing site. We have used least-squares regression on the data, but have not yet been very successful. We believe that possibly some of our trouble may be due to errors in the lunar ephemeris.

We must represent the motion of a point on the lunar surface — the spacecraft. For the motion of the lunar center of mass, we use the current standard JPL lunar ephemeris, which includes Eckert's corrections. It is, as far as I know, the best center-of-mass ephemeris now available. We have used very precise models for rotation of the earth and the moon, including physical librations. While the *accuracy* of the rotation model may be questionable, we have not been able to explain the discrepancies in our results in any way other than to infer that they are due to errors in the lunar ephemeris.

Various methods have been used in several attempts to determine the position of *Surveyor I*. The results are indicated in Fig. 24, where the legends mean the following:

(1) SU-1 Photographs. Obtained from triangulation using surface features shown in photographs from the spacecraft itself.

(2) LO Photographs. Obtained by triangulation using surface features shown in a *Lunar Orbiter* photograph.

(3) Pre-T/D Tracking. Obtained from extrapolation of the flight path to its point of intersection with the lunar surface.

(4) Post-T/D Tracking. Obtained by fitting the doppler signals from the landed *Surveyor* spacecraft.

This figure gives an indication of the errors involved in these determinations. The location deduced from doppler tracking after touchdown is not a unique point; use of selected subsets of the data will cause this point to shift around. The figure does not show the radial distance, which can only be obtained from post-touchdown

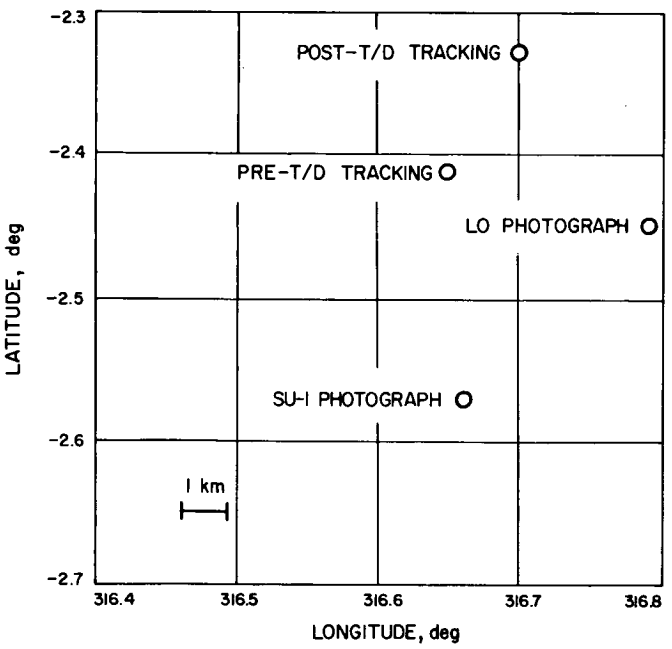


Fig. 24. *Surveyor I* location estimates

tracking. We are getting quite unreasonable results for this coordinate. The best fit to date gives a value which is several kilometers too small; it seems clear that we are seeing the effect of some large systematic error.

Figures 25 through 28 show the actual doppler residuals obtained from these four locations. Each discrete collection of points represents a day of tracking, the 12-h view period for the antenna. The most striking feature here is the approximately sinusoidal trend which appears in each figure. The residuals from the photographic positions appear worse than those for the other two. Figure 28 presents the best fit to the post-touchdown data. The offset visible in the other plots has been removed, but the general sinusoidal character remains; we have had no success in trying to remove it. It has an amplitude of about 1 mm per second and a period of about 10 days. If one assumes this to be a range-rate error and integrates it, one obtains range anomalies of 400 to 500 m. The work of Sjogren and Mulholland (Ref. 10) gives the impression that such errors do not remain in the radial component of the moon's position. The later ranging residuals were that large, certainly, but it is not clear that these residuals are entirely due to the ephemeris.

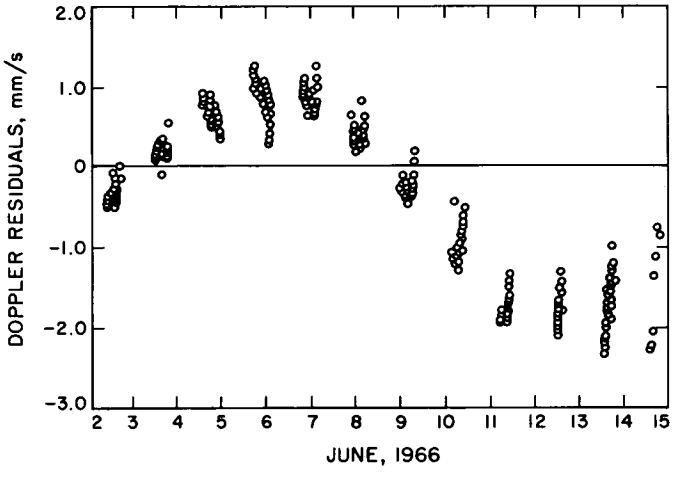


Fig. 25. *Surveyor I* residuals based on position determined from pre-touchdown tracking

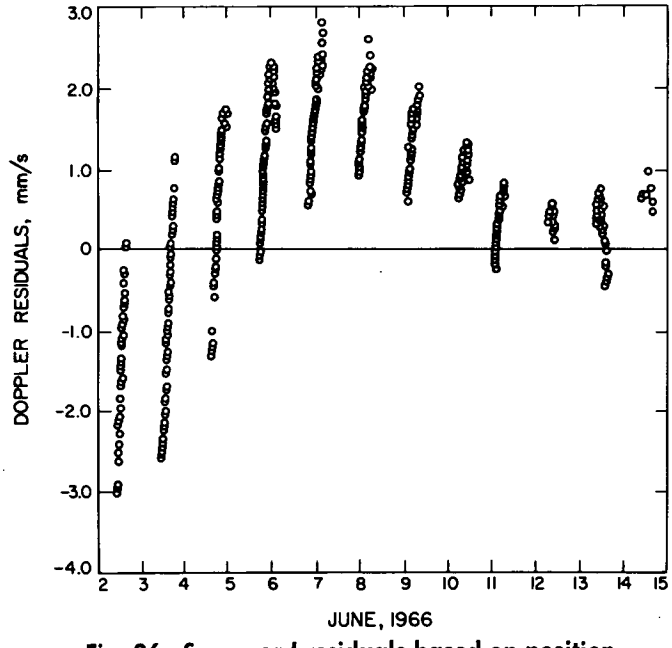


Fig. 26. *Surveyor I* residuals based on position determined from *Surveyor I* photographs

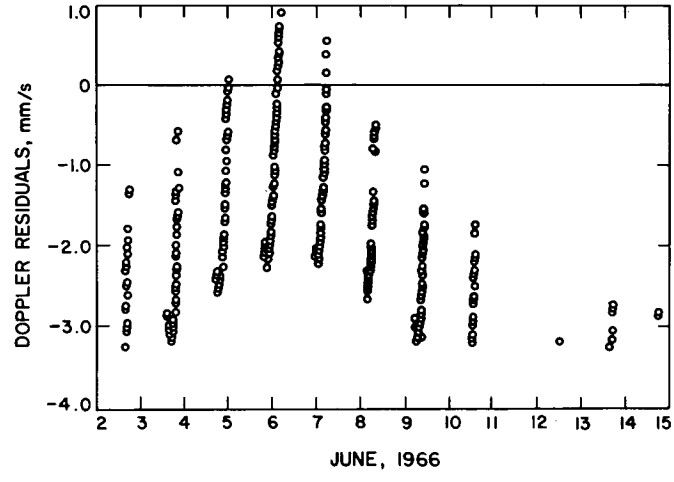


Fig. 27. *Surveyor I* residuals based on position determined from *Lunar Orbiter* photographs

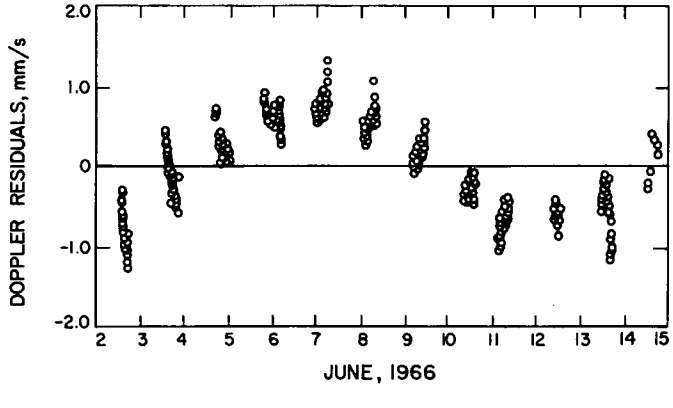


Fig. 28. *Surveyor I* residuals based on position determined by post-touchdown tracking

On the other hand, our doppler calculations use the lunar velocities, which are derived numerically rather than being given by the theory. It could be that the relative velocity errors are larger than the relative position errors; I know of no information that might resolve this question. We would be interested in seeing some numerical integration of the lunar equations of motion, in the hopes of getting a velocity ephemeris that is more consistent with the position ephemeris than is currently the case.

One may ask if it is really obvious that our residuals arise from the ephemeris; it is not. There are other possibilities, but none that we have considered are capable of producing effects of this size. We examined the physical librations, since we only include the largest terms in our model. The error in physical librations would have to be several hundred percent. We have examined effects that could modify the doppler signal, such as ionospheric perturbations. Again, this seems too small. Lunar tides are very small. We considered the possibility of an anomaly in the spacecraft electronic equipment which would cause a phase shift as the sun rose. There is no evidence that would support effects of the necessary size.

Returning to Fig. 24, I will remind you that the amplitude is about 1 mm/s *after* a least-squares fit. The fit moves the spacecraft to the position which best fits the residuals. This location is unreasonable, so the error indicated by the residuals must be considered as a lower bound on the real error. If it is indeed a velocity effect, we must expect the real error to be on the order of 1 to 3 mm/s.

Discussion

Question: How big are the error bounds in Fig. 24?

Answer: On the pre-touchdown prediction, a distance of about 5 to 6 km is claimed as the $1-\sigma$, which is fairly large. The other methods claim much smaller probable errors, certainly not overlapping ones. In the case of the post-touchdown tracking determinations, the calculated probable errors are clearly unrealistic, since the solutions vary by more than the probable errors.

Question: It escapes me why the two photographic determinations are so far apart. Are there resolution or identification problems?

Answer: I do not know. It is possible that they may not be using the same selenodetic control systems.

Question: Isn't there a question about what they were really seeing when *Surveyor* was in TV mode?

Answer: There may have been. Mr. Whittaker (University of Arizona), who made that determination, seemed fairly certain of his determination, quoting uncertainties of 0.02 deg in latitude and longitude.

Question: Did you try picking an intermediate point between the locations shown in Fig. 23?

Answer: Presumably, a location chosen by a least-squares fit to the data should be better than a randomly chosen point. There should be no other point that can give a better residual plot than Fig. 28.

Question: Do you solve for three components of position or just two?

Answer: Three. The plots do not show radial distance, but this tends to be down around 1728 km, which is not very reasonable.

Question: What is the inherent precision of a single doppler measurement?

Answer: On the order of 0.1 mm/s.

Question: The two dynamic positions are both north of the photographic positions. Is there any possible center of figure offset effect?

Answer: The adjustments are not on these photographs, and they would be about 0.04 deg on the Lunar surface. That still would not explain why the photographic determinations are so far separated in latitude from one another.

Question: Don't you have a figure showing the residuals using the old ephemeris?

Answer: No, unfortunately. Those residuals look quite a bit different from Fig. 28; the general trend had another peak. The new ephemeris caused corrections, particularly on June 5 through 7, on the order of 1 mm/s. However, the residuals that remain are of about the same size, so it seems that another correction comparable to Eckert's is needed.

Question: The magnitude range of the residuals using the old tables is roughly the same as the range shown here?

Answer: Yes, that is right.

Comment (Mulholland): That is definitely a selection effect due to the date. There are times when the velocity corrections induced by the corrections made to the position ephemeris amounted to more than 10 mm/s.

Question: How do your later tracking data fit with your least-squares position?

Answer: After June, we have perhaps 2 or 3 days of data in July and then only a few points in October, November, and January. We have only used the July data, and they seem to be very nearly consistent with the June data. If they are included in the fit, only a small correction to the location is indicated, and there is no apparent increase in the size of residuals.

Question: Did you find that 10-day period throughout the data?

Answer: We had only a small amount of data after June. It is not possible to tell.

Question: Each pass shows a variation of about 1 mm/s. Would adjustment of station locations on earth have any effect on variation?

Answer: No. This is the best set of residuals that we have been able to get, even when the station coordinates are corrected. It does not seem possible to remove the daily trends.

Question: Do you have any explanation for such a daily effect?

Answer: Any long-term error will usually show a daily effect, imposed by the motion of the tracking station. A systematic daily variation does not necessarily mean that the error source has a 1-day period.

X. Effects of Recent Corrections to the JPL Lunar Ephemeris, *J. Derral Mulholland, Jet Propulsion Laboratory*

Early in 1966, the need was recognized of modifying the JPL Lunar Ephemeris, to account for several deficiencies then known in the existing ephemeris. Several corrections were applied, some based on theory, others on spacecraft data. Full documentation of these changes is given in JPL Technical Memorandum 33-346 (Ref. 12). The modifications were applied in the form of Fourier series of the same form in which the Lunar Theory is given. The largest terms in each of the three coordinates had the following coefficients:

Coordinates	Coefficients, seconds of arc
Longitude	0.1683
Latitude	0.0154
Sine parallax	0.005507

corresponding to linear distances of about 340, 31, and 617 m, respectively. Figure 29 shows the effects of the corrections over the interval 1962 to 1967. As one can see, the maximum values are roughly

Coordinates	Values
Range	2000 m
Longitude	0".3
Latitude	0".15

with the extreme values being rather uncommon. Figures 30 and 31 give the corresponding corrections to the rectangular coordinates and velocities. The units are earth radii and earth radii per day (1 $R_e \approx 6.4 \times 10^6$ m, 1 $R_e/d \approx 72$ m/s). The fine structure of the corrections over an arbitrarily selected interval is shown in Figs. 32 through 34. Most of you, however, will be more interested in the effects on range and range rate. A fairly severe example of these effects is shown in Figs. 35 and 36, covering a 20-day period last April. Near the end of the interval, one sees a range correction of nearly 2 km. More typical is the example of Figs. 37 and 38, which cover the 20 days beginning 1967 July 12.5 UT.

Really, though, we are not so much interested in what changes have been made to the ephemeris as we are in how good it is. I will lead up to this question by first dealing with two related ones.

The discussions that we have heard this morning concerning the lunar occultation program certainly indicate that considerable error may exist in the ephemeris due to incorrect orbital constants. These results are not yet definite; we must admit to total uncertainty for the present.

Professor Clemence's remarks, indicating possible positional errors of the order of 200 m, I interpret to be a "worst-case" estimate of the neglected planetary terms.

Finally, publication of the 1968 Nautical Almanac (Ref. 13) brought to our attention the circumstance that the present JPL Lunar Ephemeris (LE 4) is still based on an archaic value of the earth's flattening, so there is one more major correction that we know must be made. The appropriate corrections have leading terms that are quite large in longitude (0".189) and latitude (0".215), but virtually zero in sine parallax.

It is somewhat more satisfying, and perhaps more meaningful, to consider the new ephemeris in the light of its performance in use. At present, we have data of two different types: Direct ranging measurements from *Lunar Orbiter* spacecraft and doppler measurements from landed *Surveyors* and from *Orbiters*.

Preliminary results from the *Lunar Orbiter* data were reported by Bill Sjogren and me last January (Ref. 10). Figure 39 is reproduced from that work, showing the effect of the new ephemeris in reducing the range residuals on *Lunar Orbiters I* and *II* by nearly an order of magnitude. The final residuals shown for *Lunar Orbiter I*

still contain the effect of the nonsphericity of the potential field on the spacecraft trajectory, hence is not directly indicative of the ephemeris error. As Mr. Sjogren indicated earlier, the *Lunar Orbiter II* data were reduced using the best potential derived from the *Lunar Orbiter I* data. The standard deviation of a range point indicated in this figure is about 100 m, with the later residuals being much smaller. *Lunar Orbiters III* and *IV* do not seem quite so well behaved, but there are additional factors which seem to play a role in those cases.

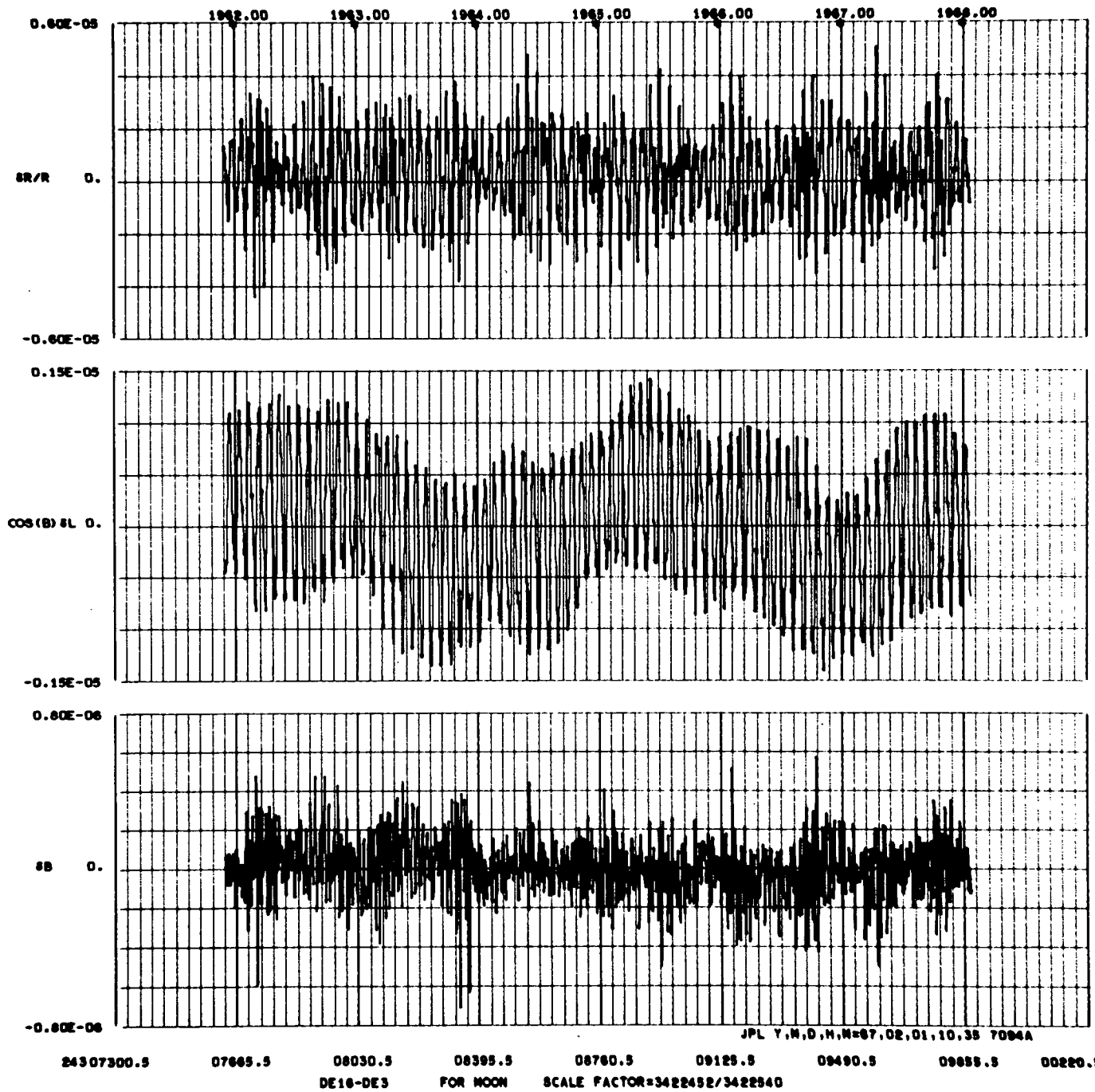


Fig. 29. Spherical coordinate differences (LE 4 − LE 2), 1962 to 1968

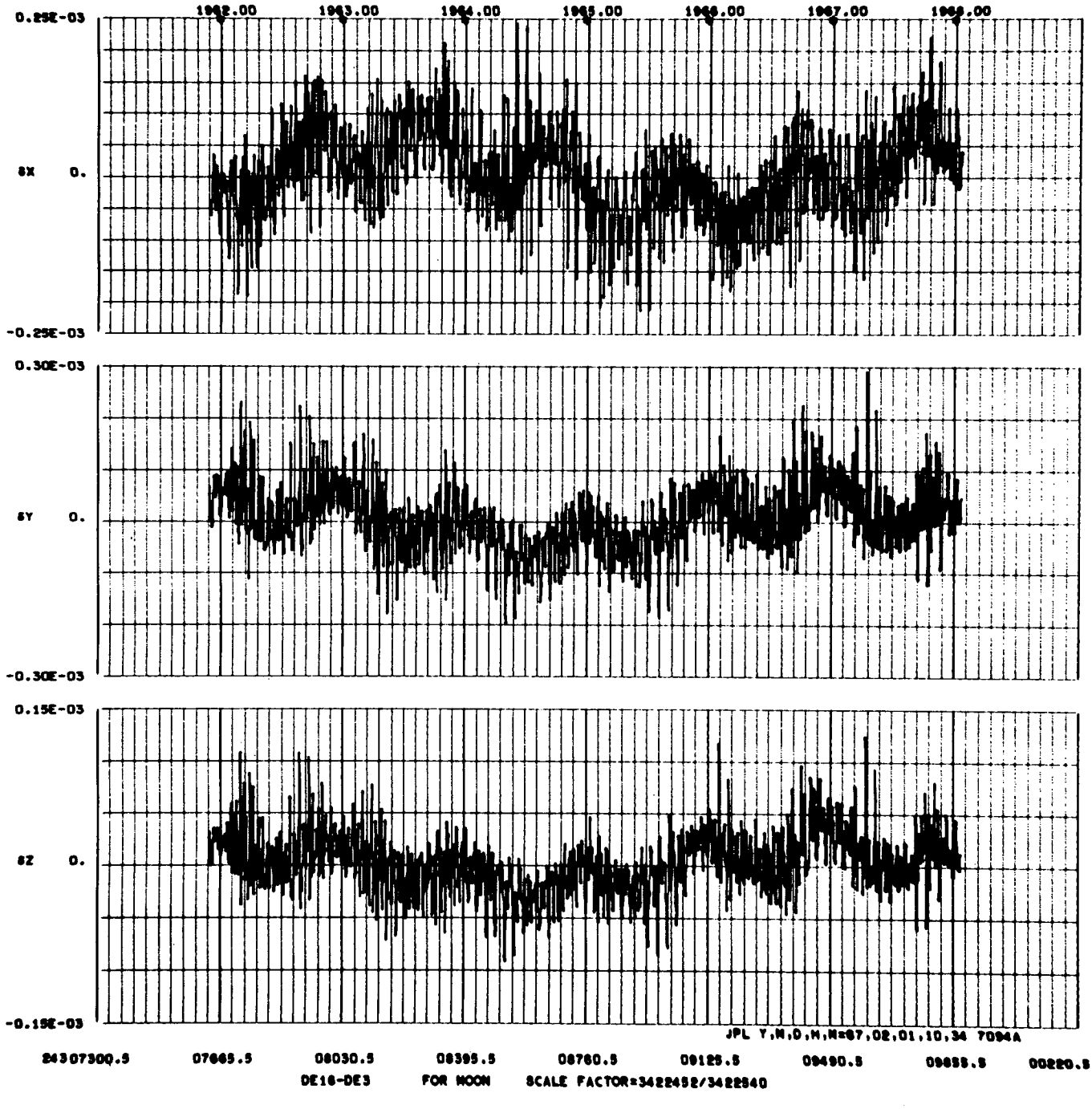


Fig. 30. Rectangular coordinate differences (LE 4 — LE 2), 1962 to 1968

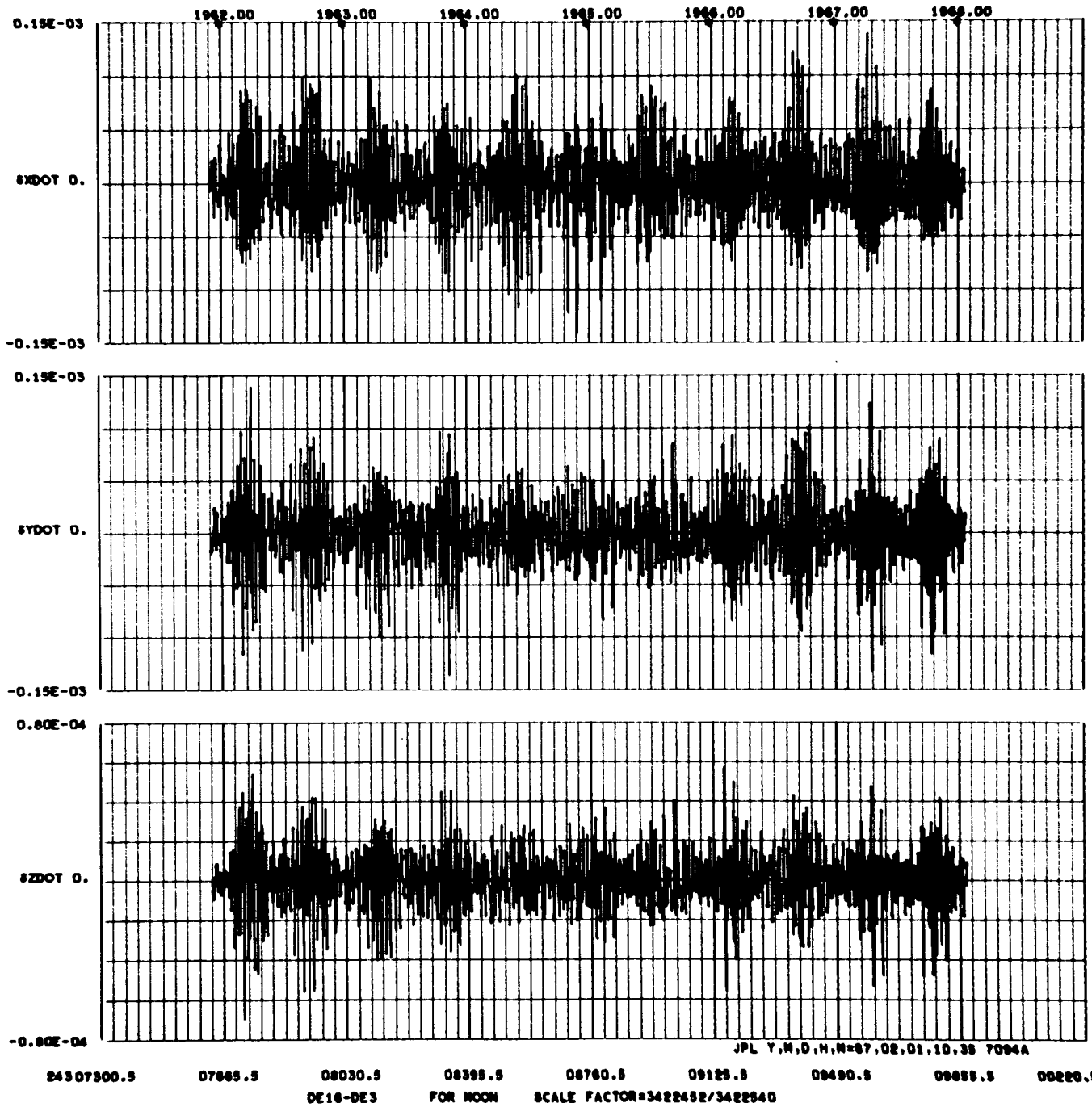


Fig. 31. Velocity corrections (LE 4 − LE 2), 1962 to 1968

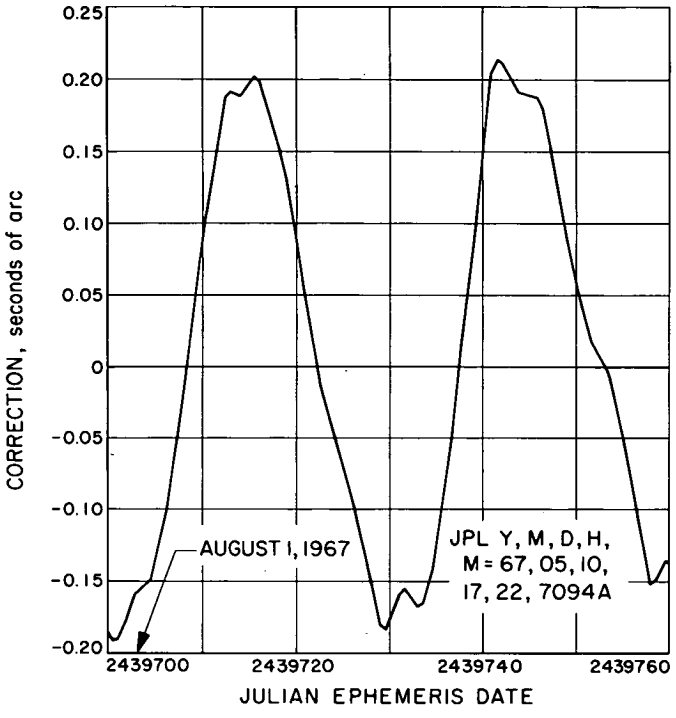


Fig. 32. Longitude corrections to the lunar ephemeris (LE 4 − LE 2)

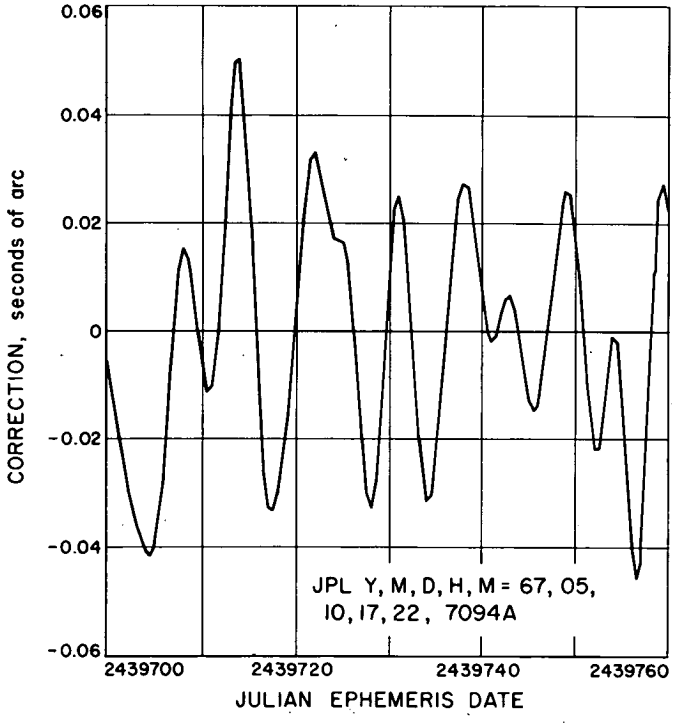


Fig. 33. Latitude corrections to the lunar ephemeris (LE 4 − LE 2)

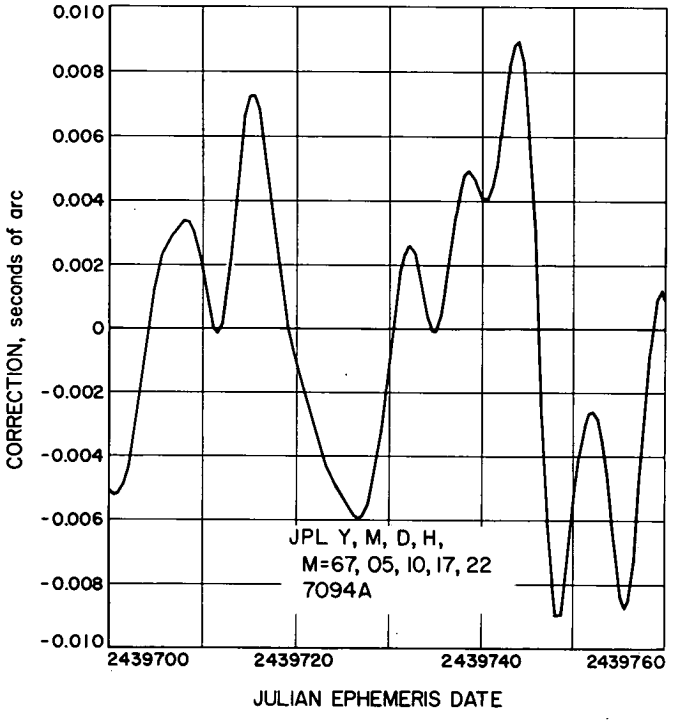


Fig. 34. Sine parallax corrections to the lunar ephemeris (LE 4 − LE 2)

The ephemeris group is just now turning its attention to the *Surveyor* beacon data, which were discussed by Mr. Cary. These data indicate range-rate residuals of about 1 mm/s. We also heard Jack Lorell speak of residuals of about 0.7 mm/s from *Lunar Orbiter* data.

From the material on which the preceding remarks are based, we can infer the following as probable $1-\sigma$ uncertainties in the position ephemeris:

x,y,z coordinates	150 to 200 m
range	50 to 60 m

These numbers were based on the assumption that the time interval September 14 to November 24, 1966 was reasonably typical. The meaning of the later data, which I have not seen before today, is unclear at this point. These figures also assume that the orbital constants of the moon are essentially correct. If this is not true, the estimate for the coordinates will have to be substantially increased, but not the range estimate.

We have not yet seen enough data to get a good "feel" for the adequacy of the velocity ephemeris. There are two ways that one might project a guess, based on the position ephemeris. First, one could hypothesize that

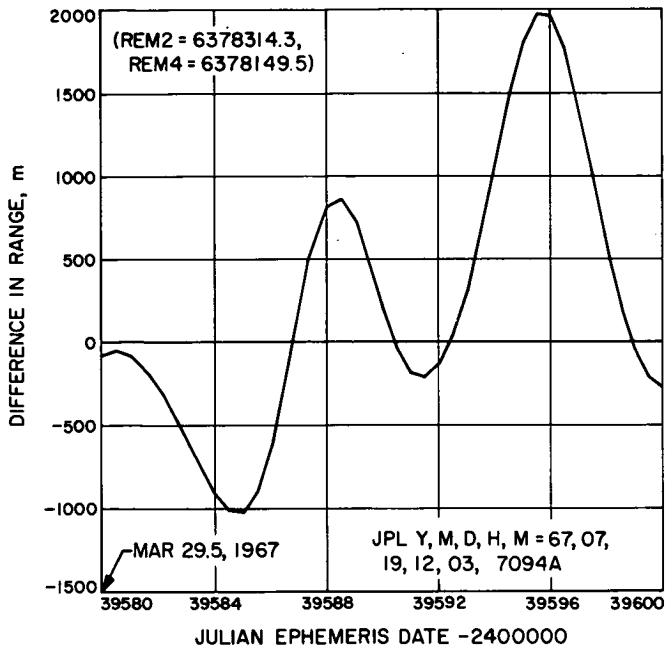


Fig. 35. Range differences, LE 4 — LE 2 (JED 243 9580 — 243 9600)

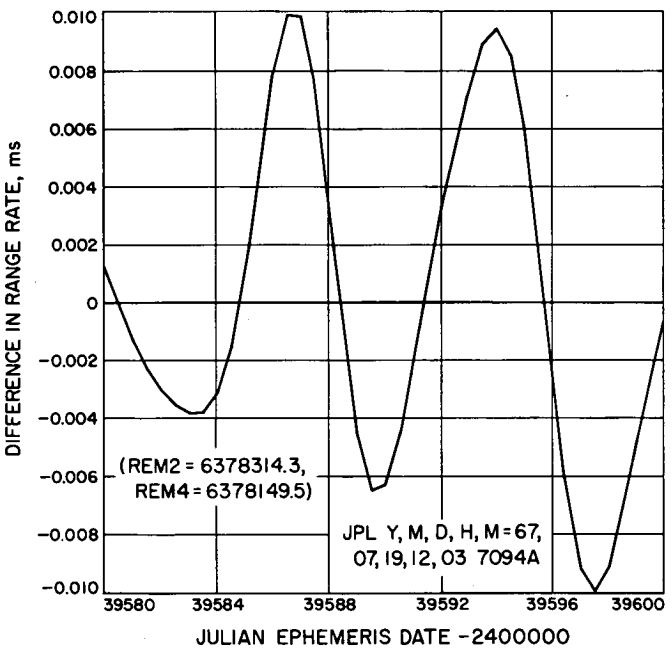


Fig. 36. Range-rate differences, LE 4 — LE 2 (JED 243 9580 — 243 9600)

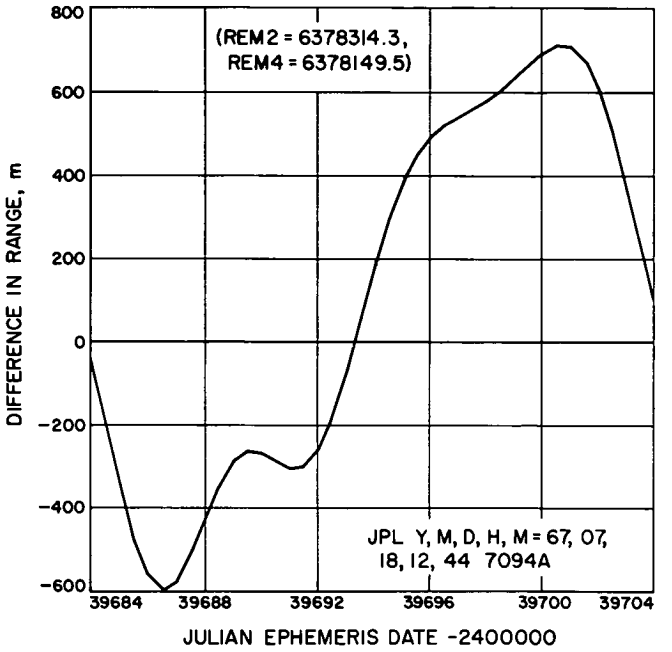


Fig. 37. Range differences, LE 4 — LE 2 (JED 243 9684 — 243 9704)

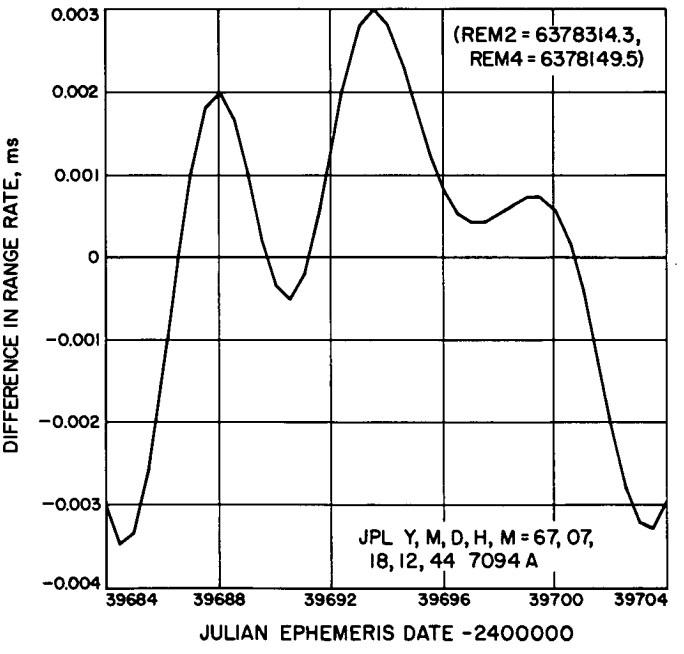


Fig. 38. Range-rate differences, LE 4 — LE 2 (JED 243 9684 — 243 9704)

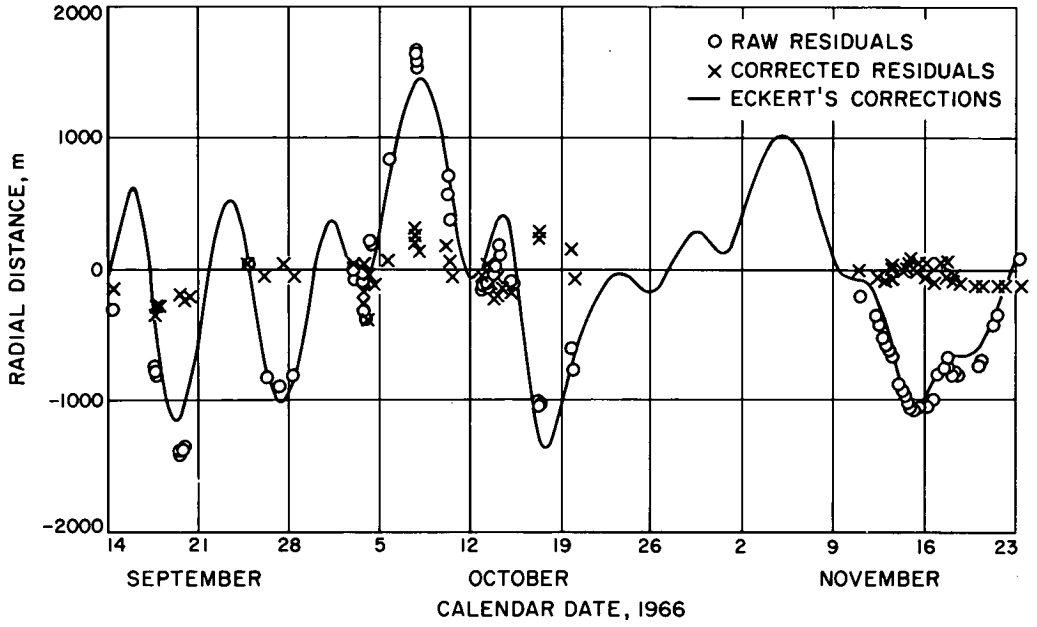


Fig. 39. *Lunar Orbiter I* and *II* residuals with LE 2, compared with Eckert's corrections

the relative uncertainty in the velocities is the same as that in the positions. That would indicate that the probable error should be on the order of 0.1 to 0.3 mm/s. I do not believe this, and I am sure the *Surveyor* people do not either. On the other hand, one might assume that the ratio of maximum correction (LE 4 − LE 2) to new probable error is the same for position and velocity. If one does this, then one concludes the following $1-\sigma$ uncertainties:

x,y,z velocities	1.2 − 1.5 mm/s
range rate	0.4 − 0.5 mm/s

I wish to emphasize that there is no firm basis for these numbers, but they do not seem unreasonable. They are at least of the same order of magnitude as the observed residuals. Further study of the observational data is necessary.

Now that we have generated and used a new lunar ephemeris, we do have a rather good and well-founded idea of the reliability of the *old* one. Our present beliefs about the *new* one are based largely on what a lawyer would term "circumstantial evidence". The basis for a good estimate of the uncertainties will grow stronger only with the extension of the discussion of the pertinent observations.

Discussion

Question: Are the corrections that you presented the differences between the two JPL ephemerides?

Answer: Yes, they are the corrections that were applied to Lunar Ephemeris Number 2 to get Lunar Ephemeris Number 4.

Question: Are the data in Figs. 1 through 3 Eckert's corrections?

Answer: These represent several corrections, the primary of which were Eckert's corrections. In addition, there are corrections to update the constants, and a change was made in the treatment of aberration. The corrections due to Eckert account for perhaps 80% of the changes in the coordinates.

Question: Should you now be consistently referred to the IAU System of Astronomical Constants?

Answer: With the exception of the earth flattening parameter, yes.

Question: Are the corrections due to J_2 periodic?

Answer: Yes, the leading term in longitude has the period of the lunar node, about 19 years. The leading term in latitude has a period of roughly 1 month.

Question: Do you have any plans to implement the suggestion by Clemence to compare the ephemeris with a numerical integration?

Answer: We do intend to do such a thing, but we have no plan in the sense that we have a schedule.

XI. General Discussion

Comment (Lawson): Since there has been an increase in ephemeris activity at the Laboratory, we have instituted a system of nomenclature for our ephemerides. It is, after all, a little disconcerting to refer to "the new one and the old one and the old one before the last new one before that old new one." The ephemeris that most people refer to as the "old" JPL ephemeris, first distributed in March 1964, is now designated Development Ephemeris

Number 3 (DE 3). This is not found in the original documentation, because the labeling problem had not been appreciated at that time, but the first BCD record on the tape contains the number 3. It will be helpful if the identification DE 3 is used in future communication. The lunar data carried on that tape, the Brown Improved Lunar Ephemeris, is now designated Lunar Ephemeris Number 2 (LE 2). The current export version of the JPL Ephemeris Tape System is Development Ephemeris Number 19 (DE 19), and it contains Lunar Ephemeris Number 4 (LE 4). This lunar ephemeris contains Eckert's corrections, as well as others, and is the ephemeris that has been used by all JPL speakers here today.

Question: Is DE 19 identical with LE 4?

Answer: DE 19 is an ephemeris of all the major planets, plus the moon. The lunar ephemeris that is included in it is identified as LE 4. The term Development Ephemeris is reserved for a complete set of lunar and planetary data.

Question: Mr. Cary indicated that he accounted for physical librations. Is that included in the ephemeris?

Answer: No, that enters into the JPL trajectory programs as a separate subroutine.

Question (van Flandern): I would like to ask the people working with range and range-rate data if they have seen anything in those data which contradicts or conflicts with the optical data that I discussed this morning.

Answer (Knowles): I would say not, but I just got Tom's (van Flandern) numbers this morning and I would like to check them.

Answer (Sjogren): No, I do not think we can say that we have seen any contradiction.

Question: I would like to know what Mr. van Flandern intends to do with his corrections. Will he program Eckert's new theory and apply his corrections to determine a new theory? How will these corrections be applied to the ephemeris, from the Naval Observatory's point of view?

Answer (van Flandern): Once the results become final, rather than just preliminary, some of them will cause changes in the fundamental arguments that enter into the theory, and others will change the actual numerical values of the coefficients.

Question: Then a new ephemeris will be generated?

Answer: Presumably, yes, but ideally it should be re-fit to observations instead of this constant differential correction process. After a while the ephemeris begins to lose contact with reality.

Question: When is JPL going to integrate the equations of motion, at least over short arcs? It seems that one could get a better hold on the velocities that way.

Answer (Lawson): A computer program for integrating the motion of the planets and the moon is in preparation at the Laboratory. The integration part has been completed since last summer. The segments for comparison with observation are just now being completed. This will be a research tool that will be available for this type of work. We have made tests that indicate the integration is feasible. The tool is nearly available. It becomes a question of priorities in the ephemeris development work.

Question: How do you represent earth and moon, as point masses?

Answer: The J_2 term is included for earth, but not for moon.

Question: Is that adequate?

Answer: Probably so, yes. It will certainly be enough for matching or exceeding the accuracy of the corrected Brown theory.

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List of Attendees

Dr. John D. Anderson, Jet Propulsion Laboratory
Mr. Donald R. Anselmo, Bellcomm, Inc.
Mr. M. Bennett, Aerospace Corporation
Mr. Dale G. Bettis, Yale University*
Mr. Joseph P. Brenkle, Jet Propulsion Laboratory
Mr. Dan L. Cain, Jet Propulsion Laboratory
Dr. James O. Cappellari, Jr., Bellcomm, Inc.
Mr. Clifford N. Cary, Jet Propulsion Laboratory
Miss Laura G. Charron, U. S. Naval Observatory
Mr. Victor C. Clarke, Jet Propulsion Laboratory
Mr. Dan Dannenfeldt, International Business Machines Corporation
Dr. André Deprit, Boeing Company Scientific Research Laboratory
Mr. David W. Dunham, Yale University**
Mr. Peter Dyer, Jet Propulsion Laboratory
Mr. Pat B. Esposito, Yale University*
Mr. R. Grauer, Northrop Corporation
Mr. Tom W. Hamilton, Jet Propulsion Laboratory
Mr. J. P. Hogan, Hughes Aircraft Company
Mr. Arthur W. Johnson
Mr. N. Johnson, Aerospace Corporation
Mr. W. D. Kahn, NASA/GSFC
Dr. William M. Kaula, University of California, Los Angeles
Mr. Stephan Knowles, U. S. Naval Research Laboratory
Dr. Bodo Kruger, NASA/GSFC

*Temporarily at JPL
**Temporarily at U. S. Naval Observatory

Dr. Charles L. Lawson, Jet Propulsion Laboratory
Mr. A. Lee Laxdal, Jet Propulsion Laboratory
Mr. H. Fred Lesh, Jet Propulsion Laboratory
Mr. Jay Lieske, Yale University*
Mr. Jack Lorell, Jet Propulsion Laboratory
Mr. Anthony Liu, Jet Propulsion Laboratory
Mr. Don E. Marshall, Brown University
Mr. Charles F. Martin, Aeronautical Chart and Information Center, U. S. Air Force
Mr. Phil M. McKown, Purdue University*
Dr. William G. Melbourne, Jet Propulsion Laboratory
Mr. R. J. Mercer, Aerospace Corporation
Mr. Theodore Moyer, Jet Propulsion Laboratory
Dr. J. Derral Mulholland, Jet Propulsion Laboratory
Dr. Claus Oesterwinter, U. S. Naval Weapons Laboratory
Dr. Douglas A. O'Handley, Jet Propulsion Laboratory
Mr. Gerald E. Pease, Jet Propulsion Laboratory
Dr. David A. Pierce, Yale University*
Mr. Rex Reed, International Business Machines Corporation
Mr. J. Ribaret, Hughes Aircraft Company
Mr. Paul E. Russell, University of Texas*
Mr. William L. Sjogren, Jet Propulsion Laboratory
Mr. Carlton B. Solloway, Jet Propulsion Laboratory
Dr. Henry Thacher, Notre Dame University*
Mr. Don William Trask, Jet Propulsion Laboratory
Mr. Thomas C. Van Flandern, U. S. Naval Observatory
Mr. Richard A. Wallace, Jet Propulsion Laboratory
Mr. Michael R. Warner, Jet Propulsion Laboratory
Mr. Benjamin Yaplee, U. S. Naval Research Laboratory