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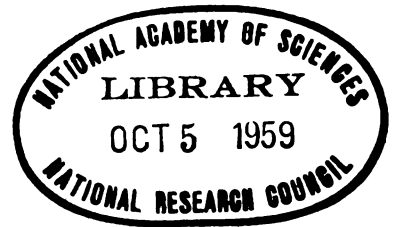
Geophysical Monograph Number 4

Contemporary Geodesy

**Proceedings of a Conference Held at the
Harvard College Observatory—Smithsonian Astrophysical
Observatory, Cambridge, Massachusetts, December 1-2, 1958**

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Edited by
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Charles A. Whitten and Kenneth H. Drummond

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FOREWORD

The successful launchings of high-altitude rockets and artificial Earth satellites have opened a new era in the collection of geophysical data. The application of these new techniques will require the combined ideas, experiences, and services of scientists in a wide variety of disciplines. As an example, any efforts in the collection of geodetic data will necessarily draw together several fields of endeavor outside of geodesy, such as astronomy, rocketry, and electronics, to name a few.

In recognition of this, many American geodesists anticipated the need of a Conference on Contemporary Geodesy for the interchange of knowledge and ideas among scientists in certain fields related to the new space age. The purpose of such a conference was not merely to orient other scientists in modern geodesy, but just as importantly to enlighten the geodesist in those sciences required in space application to his problem.

With this in mind, an Organizing Committee was formed under the Section of Geodesy of the American Geophysical Union. Arrangements were made with Dr. Fred L. Whipple and Dr. Donald H. Menzel, Directors, respectively, of the Smithsonian Astrophysical and the Harvard College Observatories, Cambridge, Mass., to sponsor and act as hosts to such a conference on December 1 and 2, 1958.

Many thanks are due Dr. Whipple and Dr. Menzel for their wholehearted cooperation without which the conference would not have been a success. Thanks are also due the National Science Foundation for its moral and financial backing in this venture.

The Organizing Committee is grateful to the officers of the American Geophysical Union for accepting the proceedings of this conference for publication in its Monograph Series and is very appreciative of the editorial assistance furnished by members of the staff of Smithsonian Astrophysical Observatory and by Waldo E. Smith, Executive Secretary of the A.G.U., and members of his staff.

The list of those in attendance and those actively participating in the conference should alone attest to the merit and success of this undertaking.

Organizing Committee

MILTON O. SCHMIDT, Chairman
KENNETH H. DRUMMOND, Secretary
ROMAN K. C. JOHNS
JOHN A. O'KEEFE
CHARLES A. WHITTEN

Geodetic Fundamentals, Introduction

MILTON O. SCHMIDT

University of Illinois, Urbana, Illinois

The American Geophysical Union, and particularly its Section of Geodesy, feels deeply honored in sharing with Harvard University and the Smithsonian Astrophysical Observatory the responsibility of assembling you and promoting this forum dealing with problems in contemporary geodesy.

Very unfortunately Dr. Walter Lambert is unable to be present because of illness. Also Dr. F. A. Vening Meinesz could not secure travel accommodations to get here. We feel very happy, however, that Dr. Alwyn Robbins of Oxford University, who is now a Visiting Professor at Ohio State University, is able to join us. Dr. Robbins is an assistant to Guy Bomford in Great Britain and is very well qualified to give our keynote address on some historical antecedents of contemporary geodesy. I am very pleased, therefore, to present Dr. Alwyn Robbins.

Evolution of the Geodetic Concept

ALWYN R. ROBBINS

Oxford University, Oxford, England

Gentlemen, I was asked to speak at short notice. I think Mr. Whitten must be short-sighted. He came into the hotel last night with a worried look and said, "I am glad to see you. You are talking tomorrow." If he had looked a little further he would have seen others far better qualified than I. So I hope you will forgive any shortcomings.

As you all know, geodesy is the science of measuring the Earth and thence, of course, to find the size and shape of the Earth. When this departed from what the geodesist thought it would be, he started speculating on the irregularities. Thus he begins to merge into the field of geophysics.

Geodesy is a very old profession. If you look in the Bible, I think the Book of Numbers or Deuteronomy where there is a list of curses, you will find one which in effect says: "Cursed be he who moves his neighbor's boundary stone." So it goes back some way.

Next, the ancient Greeks thought the Earth was a plane supported by four elephants on the back of a turtle. Aristotle went a step further and said it was a sphere. Later Eratosthenes noticed that the Sun shone directly down a well at noon at the summer solstice; he observed the sun somewhere else at the same time, made a traverse by camel caravan, and computed the radius of the Earth.

Then things stood still for a few centuries. With the coming of the telescope and the use of logarithms, triangulation was originated. Finally the size and shape of the Earth was measured by triangulating along meridians to compute the semiaxis and the flattening of the ellipsoid. One measurement appeared to show that the Earth was a prolate spheroid; this was disputed by Newton and others and then we had the famous French arcs in 1735 and 1736 which proved it was an oblate spheroid.

In the nineteenth century, with the realization of the need for maps, many countries observed the national framework of triangulation and some of them determined their own spheroid, or figure of the Earth, that happened to fit their

country best. That was all right but when communications improve, national barriers become meaningless and geodetic networks must be international. The spheroid that fits one country doesn't necessarily fit others.

During this century, the International Union of Geodesy and Geophysics has given geodesy international recognition. During the last thirty years, especially since the last world war, these national triangulations have been linked more and more and many datums have been tied together by triangulation.

When triangulation over large areas is computed on an ellipsoid or spheroid whose size and shape is known, position of the spheroid in relation to the geoid must also be determined. A datum contains seven constants: two, the semi-axis and the flattening of the spheroid, and two additional constants to make the minor axis parallel to the axis of rotation of the Earth. These last two you do not use *per se* but you use them, without noticing as it were, when computing geodetic azimuth from astronomical. As far as these four constants are concerned, you can compute on any spheroid you choose but you still will not necessarily be on the same datum. Finally, you define the latitude and longitude and geoid-spheroid separation at the origin. Now the datum is completely defined. Nothing else can be defined; everything else must be computed. So if you have two disconnected triangulation systems on the same spheroid, they are still on different datums in that they have different origins.

The definition of latitude and longitude and geoid-spheroid separation at the origin is completely arbitrary. You can assume that the separation is zero and that the geodetic latitude and longitude are the same as the astronomical. If you do that and if you happen to be in an unlucky spot where the geoid rises or falls slightly, then as the network extends hundreds of miles, this tilt will become more pronounced and the separation of the two surfaces will increase. Generally, you will reduce your bases to mean sea level but you should to reduce them to the

spheroid. If you have enough deviations of the vertical you can compute along section lines and calculate the separation and its effect on scale. But one seldom has enough information.

There is, however, one way of going about it: You can compute deviations of the vertical on one world datum if you have enough information on the intensity of gravity over the world. However, there is not enough gravity information so there will probably be some residual errors left in computation because of insufficient data. Perhaps some will disagree with that statement.

Be that as it may, it is important to recognize that deviations of the vertical from Stokes' theorem are on one datum, and any others computed on other datums are different. The two can not agree except by chance. So we have a multiplicity of datums. The task of the geodesist is to reduce these and combine them into one world geodetic datum. You can do it by having more observations of gravity and so on, or you can make intercontinental ties between triangulation systems. Then you have the problem of computing the separation of the geoid and spheroid across the sea gaps. One way is to use gravity and interpolate. Ways of doing it are now being studied; some research is being done on that at the moment.

So it does not really matter what datum you have, as long as you have one which fits reasonably well and as long as you have enough observations. It is the lack of sufficient observational information that is holding things up to some extent at the moment. The objective is to

have a world datum and to portray the geoid on it.

We have come a long way since the introduction of the telescope. I do not recall the date of the early triangulation in Great Britain but I remember their geodetic theodolite had a sixty-inch circle and they had to put it on top of St. Paul's Cathedral on a scaffolding. Nowadays one can get better results with a five inch. Of course we also have shoran, and more recently still the satellite.

On the gravity side, for the pendulum we have come up with more accurate timing devices. The gravimeter is being improved and new types are being developed which can be used aboard surface ships as well as under water.

Finally we come to the Earth satellites which are, perhaps, controversial. How much can we get out of them? I would personally like to see it the other way around. How much information can we geodesists give to the physicists? If we know the gravity on the Earth and then tell the physicist what gravity is doing to the satellite, the physicist can determine what the effect of atmospheric drag is and so on. That, again, is the reverse of what many people are thinking. The other way around is to try to find out from the physicist what the drag is doing, whence to determine gravity all over the Earth. It all depends on the relative sizes of the effects we get from one source or the other.

This is a very brief summary. As I say, this was at very short notice and I hope you will excuse any shortcomings.

Geometric Techniques in Geodesy

LANSING G. SIMMONS

U. S. Coast and Geodetic Survey, Washington 25, D. C.

The ultimate goal of the geodesist is, I suppose, to determine the parameters of an ellipsoid of revolution which best fits the figure of the Earth as a whole. But this is not all. He also is concerned with the details of the lack of fit of this ellipsoid to the actual Earth's shape, the geoid.

Consider three surfaces: the actual topographic surface of the Earth, the geoid or sea-level surface, and the reference ellipsoid. Of these three, only one is completely real, the topographic surface. The geoid is considered to be the mean position of the surface of the sea as extended under the continents were it allowed to flow freely with the continental masses still in place. It is an equipotential surface resulting from the Earth's gravitation and rotation, over which the intensity of gravity varies about one-half per cent, and about which an object can be moved without the expenditure of work.

Unlike the topographic surface, which departs from the ellipsoid by as much as five miles at slopes of almost any amount, the geoid hardly deviates from the ellipsoid by more than, say, 100 meters at slopes rarely exceeding one minute of arc. These geoidal slopes, though relatively small, are quite troublesome, since the gravity vector is always perpendicular to the geoidal surface and surveying instruments must be leveled to it.

Astronomic observations, which determine the direction of the gravity vector in relation to the spin axis of the Earth and to some arbitrarily chosen meridian plane within a small fraction of a second, do not position a station with anything like geodetic accuracy. This is due to the unknown geoidal slope or what is the same, the 'deflection of the vertical.' Astronomic parallels and meridians deviate from their geodetic counterparts by as much as several hundred feet, or even a mile, in an unpredictable manner.

In triangulation work, the theodolite measures the angles, both horizontally and vertically. Horizontal angles are measured to an accuracy better than one second of arc, but since the circles are leveled to the geoid, the angles measured are not

quite what they would be if the instrument could be leveled to the ellipsoid. Not much harm is done as long as the distant targets pointed upon are near the horizon. The trouble comes when the targets are at large angles of elevation or depression. In these cases, each pointing should be corrected for the tilt of the horizontal axis. Since the geoidal slope is not known, the tilt is not known and thus, in general, these small corrections remain to be applied at some future time when geoid information becomes available.

In this country, in special cases, we have determined these small corrections to horizontal angles by astronomic observations at the points of observation.

Elevations are determined with high accuracy by spirit leveling, a method in which short and balanced horizontal sights are taken with a level instrument of high precision. Elevations thus obtained are related to the geoid and this is as it should be. Spheroidal elevations would have little meaning to engineers, since they are concerned with the flow of water and other phenomena regulated by the geoid. The precision of elevations by spirit leveling is quite high; the error is probably no more than one or two feet in the middle of the North American Continent.

Vertical angles are measured in connection with triangulation to obtain elevations by another method. Such elevations are quite adequate for most topographic mapping, but, unless controlled by spirit leveling, they are subject to errors of a much higher order. The lines sighted over are long, and the resulting elevations depend on the gravity vector at the two ends of the line only; the averaging process of spirit leveling is almost completely lacking. Moreover, the uncertainty of the refraction of light in a vertical plane contributes just as much or more to errors inherent in the vertical-angle method.

Since triangulation is computed on the ellipsoid, the base lines measured on the topographic surface should be reduced to it. In practice, in this country, base lines have been reduced to the geoid simply because the relation between the geoid and the ellipsoid has not been known

sufficiently well. This refinement remains as a second approximation in a readjustment of the triangulation in this country.

There are those who believe it might be well to dispense with the idea of an ellipsoid entirely and work on the geoid, combining vertical and horizontal angles and astronomic observations in one general adjustment in space. The basic concept of this is excellent, but there are certain obstacles, the principal one of which is the relative inaccuracy of vertical angles as compared to horizontal angles. Weights might be assigned to the two kinds of observations, but the difficulty is, how well these weights would be determined. This matter will probably be taken up by another panel.

Distances are measured on the Earth's surface by the triangulation method. Only an occasional base line need actually be measured and the remaining triangle sides computed. Modern base lines are measured with tapes of invar, an alloy of nickel and steel with a very low coefficient of expansion. Their lengths, 50 meters in this country, are determined by the Bureau of Standards with an error not exceeding two parts per million. The triangulation itself over great distances measures lengths on the Earth's surface to an accuracy of something of the order of four or five parts per million.

In this day of sophisticated contrivances we are able to measure base lines and triangle lengths by electronic means. A Swedish physicist, Bergstrand, in experimenting with the determination of the speed of light, developed an instrument by which the process could be reversed, that is, the determination of distances by essentially timing the passage of light between two points. The instrument is the Geodimeter and its precision is limited by the uncertainty of the basic constant, the speed of light in vacuo. This is probably known to within two or three parts per million.

In practice, a triangulation scheme may be initiated at a single astronomic position and the latitudes and longitudes of the triangulation sta-

tions computed over a large area on some assumed ellipsoid. If additional astronomic positions are observed at well-spaced triangulation stations, the differences between the computed geodetic positions and the corresponding astronomic positions form a clue as to how to shift the triangulation in order to minimize these differences. This is one way to establish a triangulation datum.

Now if, as we recede from the chosen astronomic initial in such a process, we discover a continuous and systematic increase in the differences between the geodetic and astronomic positions, we must conclude that the curvature of the chosen spheroid is in error. This is actually what happened in the eastern United States when the Bessel spheroid was used. It was found to be too small (curvature too great) and this led to the adoption of the Clarke spheroid of 1866.

The process of computing triangulation on an assumed spheroid with astronomical observations to control datum can be reversed. The best fitting spheroid can be computed from the same data. Hayford employed such a method in the United States.

The latest determinations of the Earth ellipsoid have employed triangulation and astronomic observations of a great extent and have included such refinements as the consideration of visible topography and gravity observations in reducing deflections of the vertical. The equatorial radius of such an ellipsoid is almost certainly known to within 200 meters and probably within 100 meters. The flattening is certainly known within one per cent, possibly within one-half per cent.

And now in closing I would like to mention a few new developments. Such things as the modern gravity meter, the Geodimeter and the Tellurometer, the dual-rate Moon camera and not the least, but probably the most spectacular, the Earth satellite, are tools by which the geodesist should attain his goal more expeditiously and with much greater precision.

Discussion

Dr. John A. O'Keefe—Whenever Hellmut Schmid has a problem in the fixing position he just calculates by direction cosines. The photogrammetric people work the same way. We ge-

odesists operate in terms of the ellipsoid and geoid, horizontal and vertical angles. Where does this difference come from: Is it because we cannot trust our vertical angles?

Mr. Lansing G. Simmons—There is no doubt about this unless you are thinking of observations against a star background.

Dr. O'Keefe—We obviously cannot line up against a star background.

Mr. Simmons—This is true. There is a great uncertainty in vertical angle observations.

Dr. O'Keefe—It seems to me this is a fundamental point. The difference between what we geodesists do and what the others do is the result of effort to get away from vertical angles.

Another thing, you remember when Harry Brazier was over here. He was using a system of direction cosines. How did his attempt to calculate geodetic triangulation by direction cosines come out?

Mr. Simmons—This is a subject for a later panel. The method shows promise but the uncertainty of vertical angles is bothersome. You can evaluate the deflections of the vertical to some extent, but I would rather leave this matter to another group which has worked with this problem.

Dr. Raymond H. Wilson—The answer is the lack of knowledge of the vertical.

Mr. Simmons—And of the line of sight in a vertical plane. I would say that whereas the horizontal angles are more accurate than one second, the vertical angles are uncertain to the order of five or ten seconds, and then only when conditions are ideal.

Mr. Daniel F. Seacord, Jr.—Regarding this problem of taking vertical angles, on Mt. Everest and Mt. McKinley, taking the angles all around the mountain, we had some actual evidence.

Mr. Simmons—Oh, yes, in the case of Mt. Everest the lines were so long and the spheroid-geoid relation so uncertain, that we did not know whether we were getting spheroid elevations, geoid elevations, or what.

Mr. J. E. Lilly—I would like to question the matter of vertical angles. I believe Mr. Simmons mentioned the matter of ten seconds. I believe that the variation on different nights can be in the matter of minutes.

Mr. Simmons—Oh, yes, but we do our best work between noon and four in the afternoon when the refraction is less and more nearly constant. I am also assuming simultaneous reciprocal observations.

Dr. Heinrich K. Eichhorn—I am an astronomer and we measure to within two-tenths of a second of an arc down to, say, 50 or 60°. For greater angles the accuracy becomes poor because of refraction. Do you think it would be conceivable that by determining the vertical more accurately with spirit leveling and making an accurate study of the refraction along the length that is measured, that this accuracy could approach what the astronomers reach? There are certain fundamental difficulties which are the principal obstacles.

Mr. Simmons—We are talking in geodesy of angles normally not more than two or three degrees above or below the horizon. There is nothing astronomical about it. We need a way to approach two tenths of a second.

Dr. Eichhorn—Of course, you can not do that.

Some Aspects of Physical Geodesy*

MAURICE EWING, J. LAMAR WORZEL, AND MANIK TALWANI

Lamont Geological Observatory, (Columbia University), Palisades, New York

Introduction—Until about the beginning of the present century, a separate geodetic system was used for each country. As the accuracy of measurement increased it became necessary to connect as many as possible of these systems in order to avoid apparent discontinuities between them at national boundaries. The long-range navigation problems which arose about 1940 made it important to connect geodetic nets across the short water barriers offered by minor seas, and the problems of space navigation and satellite and rocket guidance have now made it very desirable to connect the geodetic networks of all continents and islands into a single unit.

The principal obstacle toward the establishment of the many international ties necessary for a global geodetic system is the difficulty of making geodetic measurements at sea. It is, of course, necessary to determine the shape of the geoid before any such system can be established.

In the present paper we propose methods for the establishment of bench marks in the ocean which would be the basis of intercontinental ties. These would form the base stations for networks of the future which would be used for the location of secondary points anywhere in the ocean. We propose to measure the distance between such bench marks, and it is suggested that this type of measurement can probably be made to one part in 200,000 to give the accuracy required for first order geodetic work. The measurement of the intensity of gravity at a sufficient number of points to permit determination of the shape of the geoid and deflections of the vertical should also be made.

If the difficulties about carrying out fundamental operations of geodesy at sea can be met, it is obvious that the ocean areas are more suitable than the continents for measurements determining the size and shape of the geoid and the relative locations of continents upon it. The measurements are made directly on the surface whose shape is to be determined, there are no unknown densities within several miles of the

point of measurement, and by a simple ratio of areas, the oceanic areas are the typical parts of the Earth's surface.

At the present time great surveys of the oceans are being proposed as national and as international projects. It is timely that these surveys are to be made just when various operations in the outer atmosphere and in space emphasize the need for a global geodetic system. The proposed world-wide network could readily provide a position control for the geophysical and geographical surveys, and these surveys in turn could provide the data and measurements required to establish the geodetic network and give it the first order of precision.

Gravity measurements at sea—The principal uses for measurements of the force of gravity at sea are (1) the estimation of local density anomalies, (2) determination of deflections of the vertical and the shape of the geoid, (3) extrapolation of the gravity field to points external to the Earth, and (4) studying the hydrostatic equilibrium of the body of the Earth.

(1) The estimation of density anomalies in connection with geological and geophysical studies in the upper few tens of kilometers can be made from a study of gravity anomalies. The gravity anomaly may be roughly defined as the discrepancy between the measured intensity of gravity and that calculated on the basis of some assumption about the variation of density within the Earth. It is a classical result of potential theory that a unique solution for the density distribution cannot be obtained from gravity anomalies, but when used in conjunction with other geophysical methods, for example, when seismic-refraction measurements have divided the subsurface into a series of different layers, the gravity anomalies may be used to extrapolate the layers either laterally or vertically into regions where the seismic data are incomplete. Work of this kind has been done along a section along the Puerto Rico Trench [Ewing and Worzel, 1954; Worzel and Ewing, 1954; Shurbet and Ewing, 1956] and across continental margins [Worzel and Shurbet, 1955]. Strong local anomalies are

* Lamont Geological Observatory Contribution No. 357.

of importance in geodesy because they introduce large errors into astronomical measurements of position. The frequent and large changes in reported positions of almost all oceanic islands for which precise locations have been sought are due to the effects of anomalous masses upon astronomical position determination.

(2) Deflections of the vertical may be calculated, and from them the shape of the geoid may be determined. If the gravity field is well determined throughout a sufficiently large neighborhood including the point of observation, this may be done by the use of a theorem of *Stokes* [1849] as modified by *Vening Meinesz* [1928]. The gravity field must be known in great detail in the immediate vicinity of the point at which the deflections are to be calculated, and in lesser detail out to a distance of 2000 km, a distance so great that even for calculating deflection of the vertical at continental stations, knowledge of gravity intensity over certain sea areas is required. The deflection of the vertical gives directly the slope of the geoid relative to the ellipsoid of reference. If the elevation of the geoid is known or assumed at a given reference station, its elevation elsewhere may be obtained by integration of the geoid slope.

(3) The extrapolation for determining the external gravity field of the Earth is best done by expressing the surface values of gravity in spherical harmonics. To do this, it is necessary to have observations of the intensity of gravity spaced more or less equally all over the globe, and observations at sea are particularly important for this type of study. In the past, efforts have been made to predict the force of gravity in the wide, unsurveyed areas of the ocean by extrapolation and from correlation between topography and gravity anomalies, but obviously this procedure is no satisfactory substitute for the measurements.

(4) Deviations from the theoretical value of gravity which are systematically of one sign over a large area indicate an error in evaluation of the theoretical value or a deviation from hydrostatic equilibrium. The latter may be attributed either to rigidity within the crust adequate for partial displacement of the crust from hydrostatic equilibrium over an area corresponding to the size of the anomaly field, or to displacement of matter from its equilibrium position within the interior of the Earth.

The measurement of gravity at sea was long a problem which defied many skillful experi-

mentalists. Until recently, the only gravity measurements made in deep-sea areas with sufficient accuracy for most geodetic uses were those made with the aid of the *Vening Meinesz* pendulum apparatus on board submerged submarines. At the present time there are approximately four thousand such measurements available, but these are not well distributed. Particularly in the southern hemisphere there are large areas in which data are lacking. Figure 1 illustrates the marine gravity measurements which had been made by pendulum up to 1958.

A gravimeter constructed for use in submarines by *Graf* [1958] and adapted for use on surface vessels by *Worzel* [in press] has shown great promise of ability to measure gravity on large surface ships under moderate sea conditions. During 1958 continuous observations have been made over about ten thousand miles of track with this equipment. Attempts to adapt it for use in smaller vessels are now being made. Various other gravimeters are under test for use in surface vessels and even in airplanes, but no definite results are available at the present time.

It seems very clear that with a small amount of additional research, a system will be available which can reliably give a continuous measurement of the intensity of gravity when used on a surface ship. It is mandatory that such equipment be available for use on the numerous large survey vessels which are expected to commence systematic coverage of the ocean in the near future.

Geodetic bench marks at sea—Geodetic bench marks at sea which could be recovered with a position error of the order of a meter are essential to the proposed intercontinental gravity network, and would be of great value for many other purposes. These can be built so that they can be relocated after a lapse of time of many decades. They would be of the greatest value for controlling surveys, engineering operations and secondary geodetic networks.

Such a bench mark can be made by placing a transponder at the corners of an equilateral triangle (Fig. 2). A ship, within the triangle, can transmit acoustic signals which the transponders can repeat back without delay. The bench mark would be defined as the point on the water's surface from which the round trip travel time to all three vertices would be equal. Four points on the ocean floor might be used to define a single bench mark instead of three, providing an assurance against the loss of the position in case

SOME ASPECTS OF PHYSICAL GEODESY

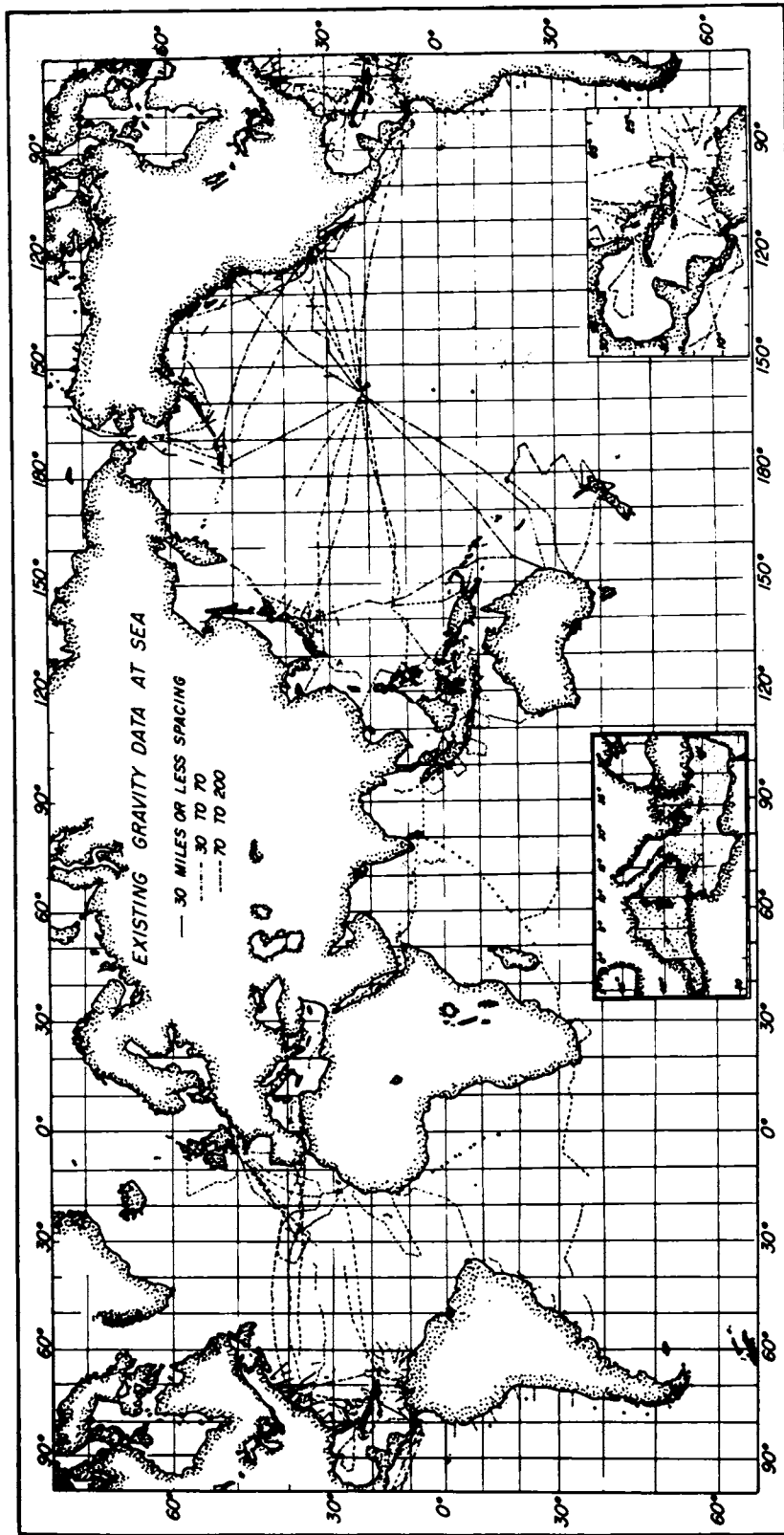


FIG. 1—Submarine gravity pendulum measurements to 1958

one of the transponders ceased to function. These transponders would be placed on a reasonably flat area of the ocean floor, free from great differences in elevation and likewise free from the type of roughness which would produce natural highlights or natural corner reflectors which might cause confusion.

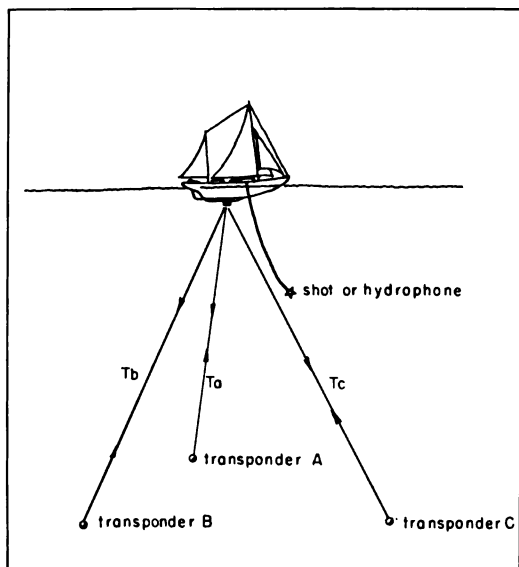


FIG. 2.—Schematic diagram of an ocean bench mark

For most of the operations which will be considered, it would not be necessary for the ship to remain exactly at the bench marks. From experience at holding many types of ships at reference points in many conditions of sea, wind, and current, it is judged that a vessel could easily be held within less than one hundred meters of the bench mark for many hours at a time. Simple acoustical measurements and simple calculations using a templet would permit frequent determinations of the position of the vessel relative to the bench marks (Fig. 3).

If advantage is taken of development of a nuclear power source suitable for unattended operation on the ocean bottom, it would be possible to operate transponders for an almost indefinite length of time.

It is highly probable that corner reflectors mounted on the ocean bottom at the vertices of the bench mark triangle would serve the purpose of locating the ship (or an acoustical instrument suspended from the ship) quite as effectively as a transponder. The construction of suitable corner reflectors seems to offer no very serious problem, and these corner reflectors could reflect sounds back to receivers located either on the ship itself or suspended from it.

The corner reflectors offer the very attractive prospects of an almost indefinite life and very

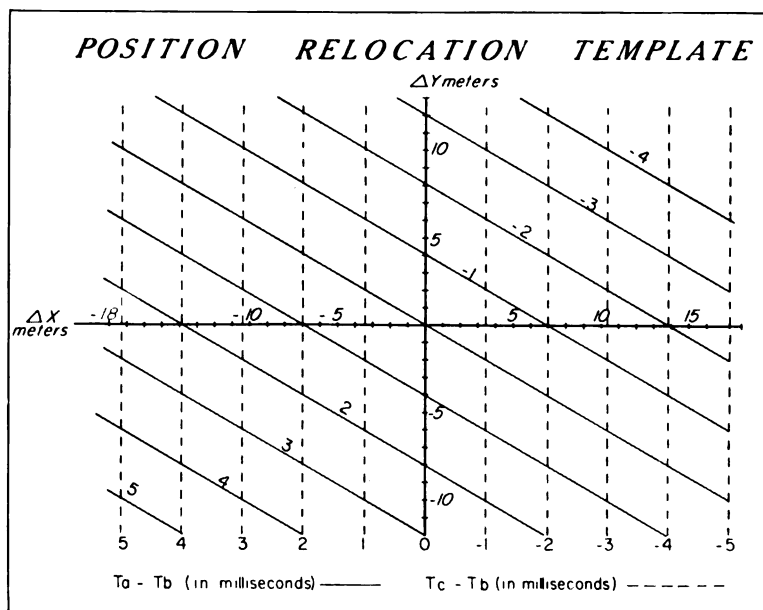


FIG. 3.—Schematic template showing the method for correcting from an actual ship position to a bench-mark position

small cost. When it is considered that the sedimentation rates in the ocean range from a few millimeters to a few centimeters per thousand years, we see that bench marks utilizing corner reflectors would have useful lives perhaps extending a thousand years hence. The life expectancy of such bench marks would so greatly exceed that of any continental markers that the proposed marine bench marks could appropriately be considered as the basic geodetic network of the world.

Geodetic connections between the marine network and geodetic networks on the various continents would be made between land stations near shore and deep sea stations located a short distance beyond the edge of the continental shelf. A number of means exist at present for making connections with first order accuracy between a ship a short distance off shore and a network of shore stations. It is taken for granted that this tie can be made without difficulty.

SOFAR sound transmission for distance measurements—It is proposed that the distance between a chosen pair of bench marks will be calculated from a measurement of the transmission time of SOFAR signals between the two stations. SOFAR sound transmission has been described by *Ewing and Worzel* [1948] and by *Ewing* and others [1946]. SOFAR transmission depends upon the existence of a sound channel in the ocean waters. The axis of this sound channel is the depth at which the velocity of propagation of sound is a minimum. In Figure 4 yearly isotherms are shown for a typical North Atlantic ocean station. It is noted that the temperature drops rapidly for about 1000 meters and then, in general, decreases very slowly from that point to the bottom. In the upper portion of the water mass, the velocity of sound is controlled by temperature changes, but in the lower part the temperature is so nearly constant that the sound velocity depends principally upon the pressure. A general account of the variations of temperature and salinity throughout the oceans has been given by *Sverdrup, Johnson, and Fleming* [1942, pp. 98–152]. This lists many of the original sources of oceanographic data. Several tables for the computation of sound velocity from temperature, salinity, and pressure have been published, for example, those of *Kuwahara* [1939], *Matthews* [1939], and *Del Grosso* [1952].

None of the existing tables provides the accuracy that is required for the proposed geodetic

measurement, but the means for improving the tables by the necessary amount will be described below. The velocity depth relation for typical points in the North Atlantic ocean is shown in Figure 5, and a single velocity depth curve which is taken as typical for the central North Atlantic is shown in Figure 6. In this figure the mean curve has actually been approximated by seven linear segments which may be represented by the data tabulated in the figure. The ray paths for propagation of sound from a source situated at a depth of minimum velocity, that is on the axis of the sound channel, are shown in Figure 7. This diagram has been constructed for the velocity depth relation shown in Figure 6. It is seen that any ray which leaves the sound source at an inclination of twelve degrees or less from the horizontal may be extended indefinitely without encountering either the surface of the water or the bottom, owing to the effects of refraction which bend it back and forth across the axis of the sound channel.

The travel time for sound along various rays such as those shown in Figure 7 will depend upon the inclination of the given ray to the horizontal at the sound channel axis. The travel time will be least for the rays which make the greatest angle and will be a maximum for the rays that are essentially coincident with the axis.

The SOFAR signal for the explosion on the sound channel axis, received at a large distance from the source by a hydrophone also on the axis, will consist of a series of impulses each of which has been propagated along a ray similar to those shown in Figure 7. The time interval between successive impulses will be a maximum in the early part of the signal and will diminish to such an extent that the impulses overlap each other as the abrupt termination of the signal is reached. Figure 8 illustrates the sequence of arrival for these impulses as they would be received at a distance of one thousand miles for the velocity depth relation shown in Figure 6. It is the abrupt termination of this signal corresponding to sound propagation along the rays which are essentially coincident with the sound-channel axis, which permits measurement of travel time with the high accuracy which is required in the proposed geodetic system. Figure 9 is a reproduction of an oscillograph recording made at a distance of three hundred miles off the Bahama Islands. The sound source was four pounds of TNT and the sequence of arrivals can be seen

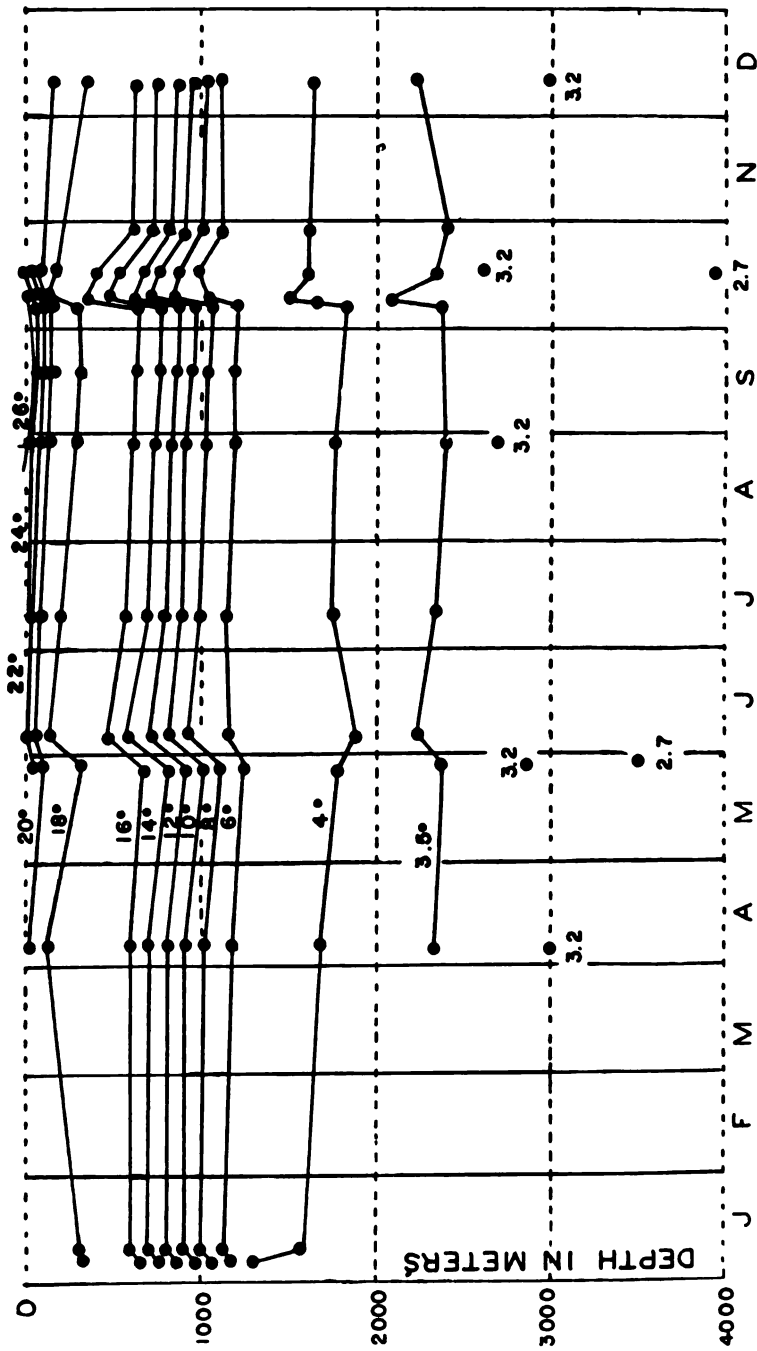


Fig. 4—Seasonal temperature-depth variations; Atlantis station C, 35° 05 N, 67° 06 W, 1937, 1938, 1939

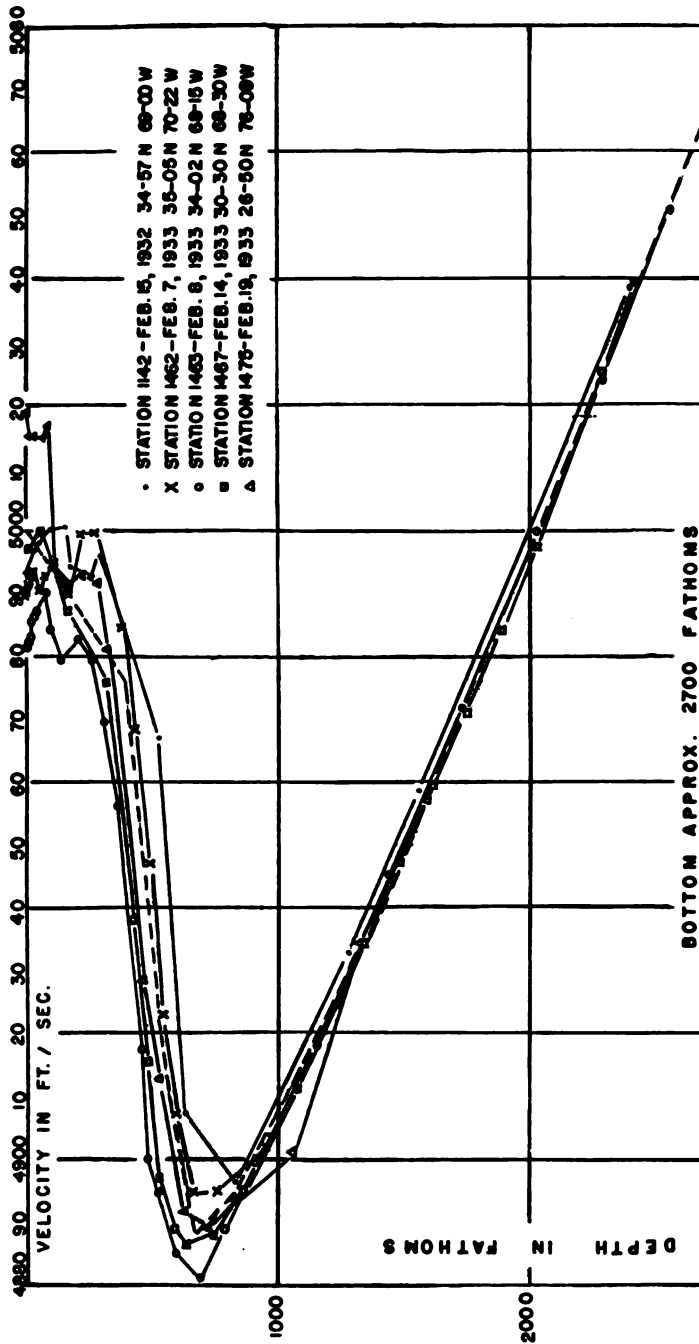


Fig. 5—Velocity-depth curves from five stations in the North Atlantic Ocean

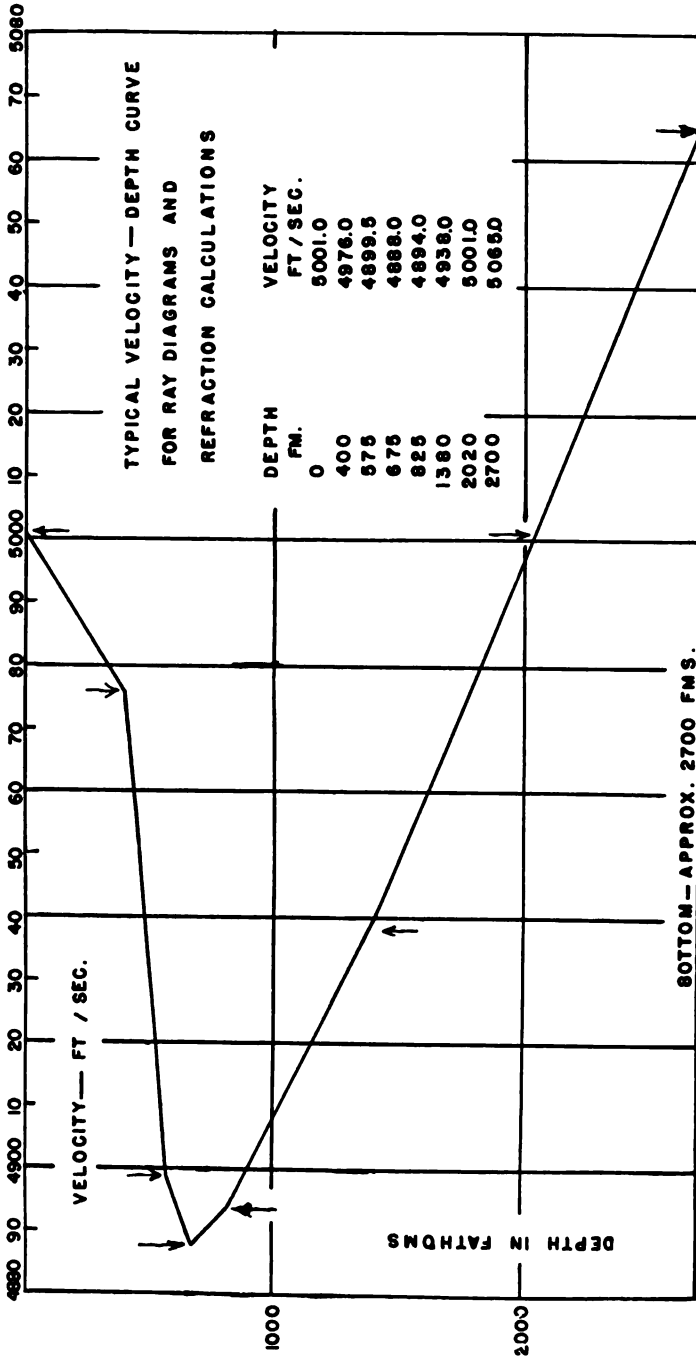
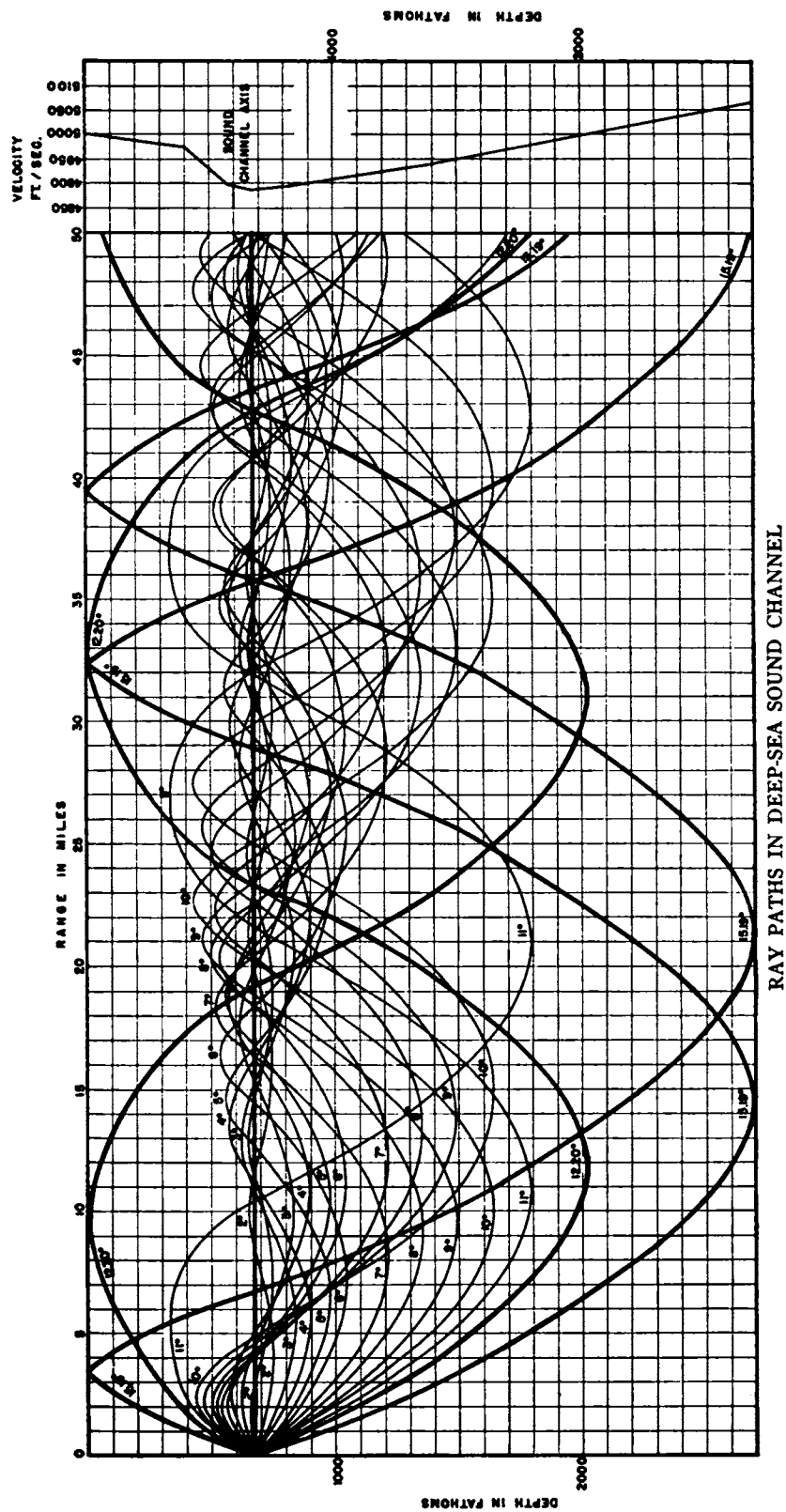


Fig. 6—Mean velocity-depth curve in the Atlantic Ocean

SOME ASPECTS OF PHYSICAL GEODESY



RAY PATHS IN DEEP-SEA SOUND CHANNEL
Fig. 7—Ray diagram for a source located at the axis of a typical Atlantic Ocean sound channel

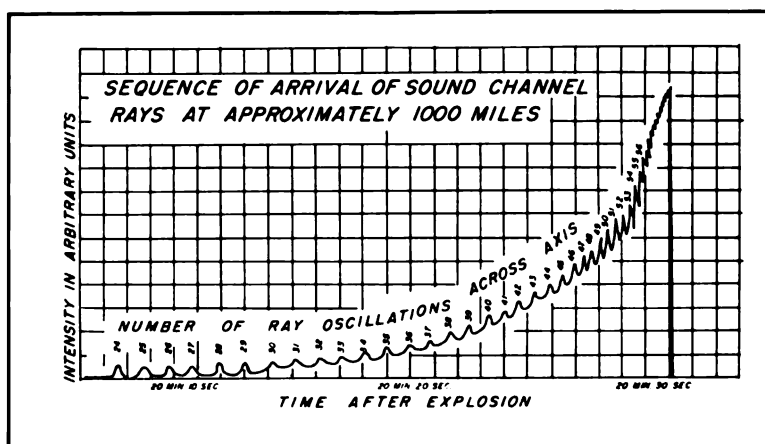


FIG. 8—Schematic diagram showing the sequence of arrivals and the relative signal intensity for a sound channel signal transmitted approximately 1000 miles

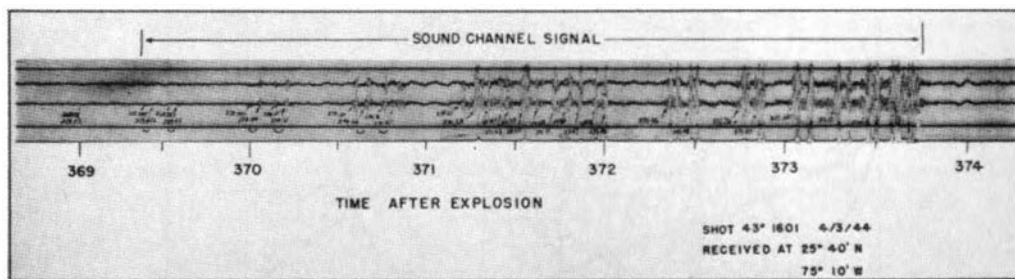


FIG. 9—Seismogram of sound channel transmission at 300 miles; 4-lb bomb, bomb depth 3400 ft; hydrophone depth 3600 ft

clearly. The sharpness of the abrupt end of the signal is seriously reduced by deviations from proper depth of both source and receiver.

No definitive data have been collected to show the precision with which the time of termination of the sound channel signal can be measured, but the general opinion of all those who have had extensive experience with this type of sound propagation is that an accuracy of better than 0.003 second can be easily attained. This value will be adopted as the limit of error throughout the present discussion. Thus, the limit of error imposed by the uncertainty in estimating the time of arrival of the sound channel signal is approximately four meters.

The equipment and procedure used in SOFAR transmission consists essentially of bombs and hydrophones which can be operated at depths up to 700 fathoms. Both will be operated at marine bench marks. The transponders or corner reflectors at the bench marks will be used to lo-

cate the suspended bombs or hydrophones as well as the ships themselves. Amplifiers and oscillograph cameras similar to those used in seismic surveys and capable of measuring time intervals to 0.001 second will be used to record the instant of explosion and the receipt of SOFAR signals. Precision depth recorders [Lusk and Roberts, 1955] could be used to great advantage in recording the sound received from the transponders or corner reflectors which define the bench marks. To insure that the exact time of firing is known with adequate precision, the bombs will be detonated by electrical means, and radio transmission between the ships is required for transmitting the explosion instant. In measuring the distance between two stations the sound transmission will be made in both directions in order that we may make allowance for the effects of ocean current.

The ranges achieved in SOFAR experiments in the past have been of the order of 3000 miles,

and the limit was introduced by the size of the ocean rather than by detectability of the signal. In a test made in 1944, about 50 shots were fired at various points along the coast of Africa. Approximately 60% of these shots were recorded by a hydrophone at Eleuthera Island in the Bahamas after transmission across the entire width of the Atlantic Ocean and the Mid-Atlantic Ridge [Ewing and others, 1946]. In this experiment there is no positive means of deciding which of the failures of reception were due to blocking of transmission by the Mid-Atlantic Ridge, which were due to failure of detonation of the bombs, and which were due to operator errors at the receiving stations. The indication is, therefore, that for the majority of paths, propagation entirely across the Atlantic Ocean will be achieved.

Global geodetic grid, network description—A network of geodetic bench marks is proposed for covering the oceans of the world. Figure 10 is a schematic representation of this network for the Atlantic Ocean. It consists of 33 stations, but the exact locations and the total number of stations would be subject to some adjustment. It is proposed that each of the stations situated near the continents would be tied to stations on the adjacent continents as mentioned above. It is further assumed that the continental stations would have been tied together by conventional triangulation. Stations are shown along the Mid-Atlantic Ridge. It is expected that sound transmission will be used in both directions between station pairs as indicated by the lines drawn in this figure. The marine bench marks shown in Figure 10 are all located in water deeper than 1500 fathoms. (The authors have not investigated the possibility that the needed precision in velocity tables can be obtained by other types of measurement.)

Calibration—In the network shown in Figure 10 there are approximately 20 station pairs situated near the continental margins, between which the distances will be known from ties to the continental triangulation network. The measured travel times between these station pairs may be used for the improvement of the fundamental relation between sound velocity, temperature, pressure, and salinity, provided adequate oceanographic data are available in the coastal areas concerned. It is proposed that after the trans-Atlantic travel times have been measured with velocity tables thus improved, the trans-Atlantic distances can be measured with first order accu-

racy, provided adequate oceanographic data are available along the lines between these stations.

Accuracy—From the estimates given above about the accuracy with which a bomb or hydrophone may be located relative to the bench mark and from the estimates of accuracy of distance measurement, it is estimated that the distances along the lines across the Atlantic Ocean such as those shown in Figure 10 will have probable errors well under 15 meters. This accuracy compares very favorably with all other known methods for making intercontinental ties.

Extension of the network to other oceans—It is considered entirely feasible to extend the proposed network to all the oceans of the world. Many SOFAR sound transmission tests have been reported in the Pacific Ocean by *Condron* [1951] and *SOFAR Research Group* [1950]. Temperature-depth data indicate that a sound channel similar to the Atlantic Ocean exists in the Indian Ocean [Sverdrup and others, 1942].

In the Arctic Ocean and near Antarctica the temperature depth data indicate that the axis of the sound channel is at or near the surface [Sverdrup and others, 1942]. The surface reflection at low frequencies is nearly perfect so that the SOFAR transmission in those areas may be expected to be perfectly satisfactory. Thus this method can be expected to be adequate in all the ocean areas.

The proposed grid of marine bench marks would have great value for controlling positions in future surveys, and in future engineering operations in the oceans. This use of the grid might be almost as important as that of establishing the relative position of the continents on the ellipsoid of reference. The value of recoverable positions at sea, even though the exact coordinate locations of the positions was unknown, has been emphasized by many researchers who have endeavored to extend triangulation nets over moderate oceanic distances. The extensive marine surveys which are being very actively considered today could provide us with a world geodetic network and with world-wide data on the intensity of gravity. The following recommendations are offered:

- (1) The necessary research be done to provide gravity meters for all vessels engaged in wide-range oceanographic surveys.
- (2) Research be done to provide transponders or corner reflectors suitable for constructing and installing marine bench marks.
- (3) The research necessary to achieve the re-

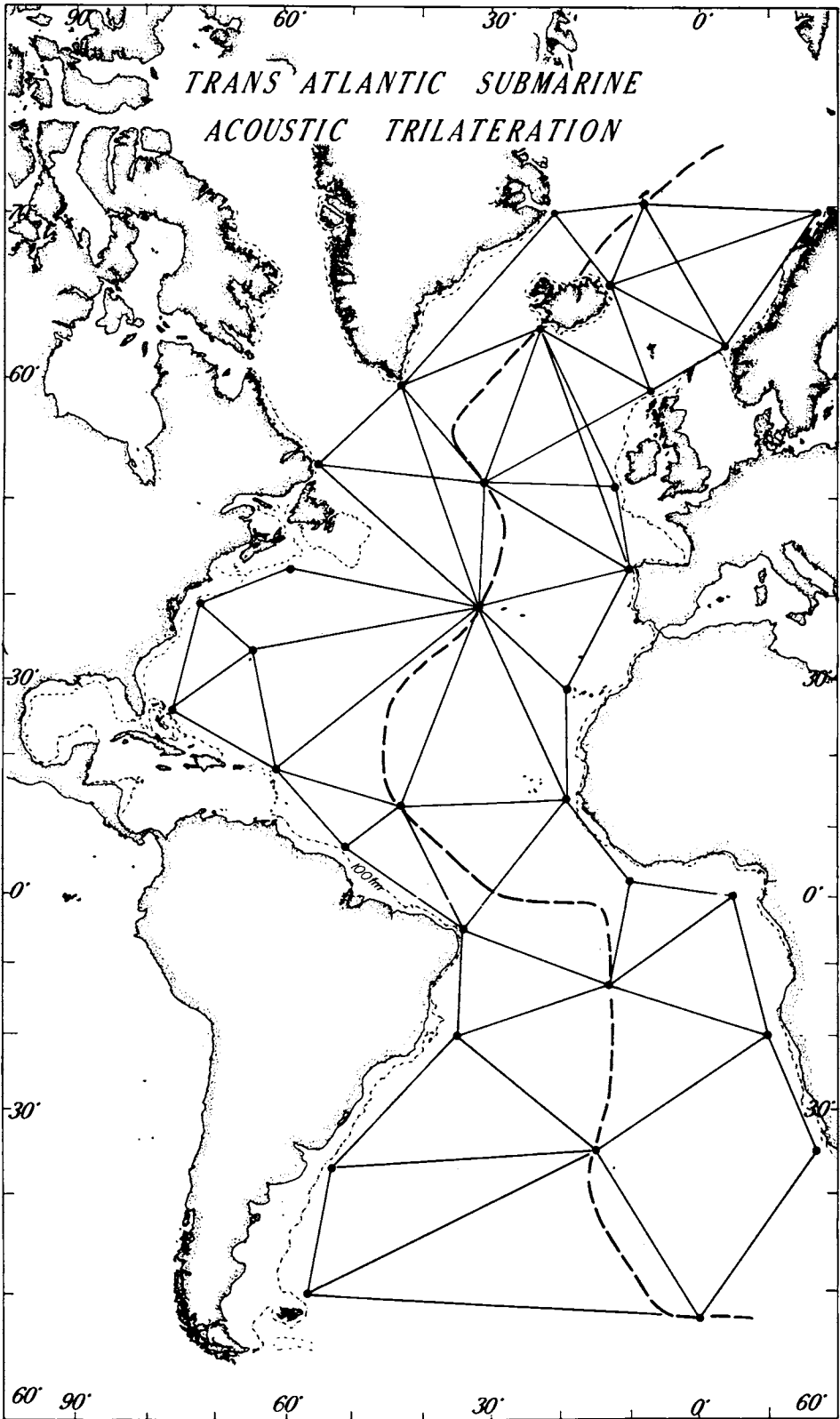


FIG. 10—Preliminary proposed geodetic grid for the North Atlantic Ocean

quired accuracy of SOFAR transmission time measurement be done.

(4) The necessary measurements of oceanographic factors, temperature and salinity, be included in the programs of survey vessels.

(5) Upon completion of suggested researches, the installation of the primary marine geodetic network be made, and the necessary distance measurements and continental ties be carried out.

(6) Second and third order subsidiary bench marks be placed as necessary.

(7) Consideration be given to the best means of adjusting network of measurements such as that represented by the global geodetic grid, and directed ultimately toward the inclusion of these data in a recalculation of the figure of the Earth.

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Discussion

Mr. Lansing G. Simmons—What is the anticipated accuracy for such measurements, one part in a thousand, one part in three thousand or four thousand, or would you want to venture a guess at all?

Dr. W. Maurice Ewing—Well, I think in a thousand miles maybe ten meters.

Mr. Simmons—That is interesting.

Dr. Ewing—It is a very fancy number if it can be made to stick. I believe it can be.

Mr. Simmons—Well, you will have a real tool for the geodesist.

Dr. Ewing—I think it is well worth the consideration of your group.

Dr. John A. O'Keefe—I think it is very exciting that it is now possible to fix recoverable

points at sea. I should think that we might first of all locate those stations by older techniques, such as those developed by the Smithsonian, at sea. We could provide Professor Ewing with accurate distances by which he could get the sound velocity. I think it will be a long time before we can reverse the project. After all, if you have a camera to photograph the satellite it does not matter if the ship is rolling or pitching. You can get a fix even if the ship is rolling at sea by the fix on the star pattern.

What would be the effect of the Gulf Stream? This moves back and forth. Does it alter in a sort of random way the speed in this channel of yours over the place where it goes?

Dr. Ewing—The Gulf Stream is not the most

desirable place in the ocean to measure anything that I know of except the Gulf Stream. You would try to sidestep and dodge the Gulf Stream area as much as you could. If you make measurements in both directions at the same time you can minimize the effect. I believe we can deal with this problem.

Capt. Carl I. Aslakson—In the oceans the salinity is a prominent factor in the velocity. Would not the waves be going through a great many areas of uncertain salinity?

Dr. Ewing—At present there is not enough knowledge about salinities but if you put three or four ships out making measurements for three or four years there will be plenty of information. Changes in the ocean are very, very much slower than changes in the atmosphere and deviations in the mean conditions of the ocean are very much smaller than in the atmosphere. I do not believe that there will be a problem from that source.

There are many, many tests on a scheme like that which is proposed, so that, if there is trouble from some current as yet undreamed of, it will show up and we will not be at the mercy of this difficulty. I believe that by repeated tests and by always reversing the propagation, we can handle any difficulty that I know of in the ocean.

Dr. Daniel Linehan—Of course, as yet, I imagine you have not established as many stations over the oceans as you want but what causes that 700-fathom depth to vary, submarine topography or conditions above the bottom?

Dr. Ewing—It is partly the broad conditions. At the top the decreasing velocity is caused by the upper part of the water which is warmer than the lower part. The lower part is cooled in the high latitudes and sinks to the bottom so you have a great reservoir of cold water at the bottom. We are still debating whether the bottom water is steady or is changing slowly due to its circulation. In general, the climate is warmer at mid latitudes than where the cold water came from. Every oceanographic station in the ocean gives the salinity. In the Atlantic Ocean, at present, there must be 30,000 or 40,000 stations. The sound channel axis rises as you go to the polar latitudes but there have been few tests in that area. The precision may go down in those regions, but they will be supported by connections in more favorable latitudes. I think that also can be handled.

Capt. Aslakson—In the old days, some 35 years ago, the Coast and Geodetic Survey was using

acoustical ranging techniques. The hydrophone was placed near a beach; I do not think the depth averaged more than six or eight fathoms. So probably the problem is quite different at greater depths and the conditions are perhaps more constant. But I know we never figured any such accuracy as you have. I am wondering about that ten meters which is about one in 160,000. That is about as good as you can expect in triangulation. I am just wondering if you really think that is possible. If so, then conditions have certainly improved greatly.

Dr. Ewing—Well, the propagation of sound in the coastal areas where you were doing RAR work is the most difficult of any. There are great changes in salinity because of the seasonal contribution of the rivers. There is a geological difference in the bottom. The reason I am here is that I believe the conditions in the ocean are uniform and change so slowly that this proposal can be made to work.

Dr. O'Keefe—What would happen if you placed your ships, say, four hundred miles apart in a pattern across the Atlantic and triangulated across them by Shoran?

Dr. Ewing—I think this is an interesting suggestion and I know of no difficulties at all.

Capt. Aslakson—Well, of course, it has been proposed for a long time that you use ground stations on ships and have line crossings through those ships moving at slow speeds. We might use airplanes too. If you try to use electronic measurements between ships you are limited but with airplanes in the air measuring lines between these ships you might make rather good connections.

Dr. O'Keefe—Professor Ewing says he can put a little mark in the Atlantic Ocean and come back to it. Therefore you do not have to have the slow moving ship, and the whole problem is changed. It seems to me that when this scheme was previously discussed there were two or three ships and airplanes. It was almost certain that there would be a failure somewhere along the line; but this really has a chance to work, I think.

Rear Admiral Charles Pierce—Is the crest of the Atlantic Ridge in general below the so-called channel?

Dr. Ewing—In general it is.

Admiral Pierce—So you could lay out the lines so they would be above the ridge.

Dr. Ewing—The crest of the ridge is so mountainous that it is hard to generalize on how many more peaks are going to be discovered. In that

one run that was made along the coast of Africa in 1945, we found that for a run on the other side of 800 miles, over half of the signals came through. In the half that did not come through there are several possible reasons. You have the possibility that there was an obstacle. You have the possibility that the bomb did not go off. I am cynical. You have the possibility that the man receiving the event was at lunch. All of these possibilities exist and in spite of these many factors we heard more than half of them.

Mr. Erwin Feuerstein—A minor point. Have any experiments been done on corner reflections?

Dr. Ewing—No experiments have been made on corner reflectors. I would like to volunteer to do so. I think it is entirely feasible.

Dr. Roman K. C. Johns—What about the transponders? How long do you think they would operate?

Dr. Ewing—To date the transponders have all been limited by the power supply. In the brave future that we all think about, people talk about putting nuclear devices on the bottom of the ocean. If somebody does that you are not limited by the battery life. Then the question comes, what will be attracted to these? It could be that some horrible animals living on the bot-

tom will be attracted and come and chew them to pieces. If there are no completely unknown biological causes of trouble, I think with the batteries we can guarantee three to five years. As far as corrosion and troubles of that kind, the first ones might corrode. When that is solved and you have either corner reflectors or other power sources, I think the life of a bench mark is a matter of a decade.

Dr. Johns—Suppose they have this corner reflector on the bottom of the sea but it is covered by some plant organism or something of that sort?

Dr. Ewing—I think there is no problem of plants when you are below the zone where the light does not penetrate. We have photographed the bottom of the ocean thousands of times and see rocks that were dropped by ice, presumably thousands of years before, and they are not covered by anything that looks alarming, perhaps a coating of manganese or something like that but there is no evidence that anything would happen to them.

Dr. O'Keefe—Is the fundamental factor here that you have something like a kilometer of sediment since the origin of the universe? These should be good for a millennium.

Problems of Modern Geodesy, Introduction

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We heard earlier that geodesy is one of the oldest of the sciences. The basic problems of this Earth science do not change appreciably through the years but recent rapid developments which have occurred in other sciences have required the use of geodetic knowledge and geodetic techniques. This new and broader application of geodetic data has accelerated the development of methods and instruments for obtaining this information.

Thus, in this modern era, we do find many problems related to geodesy that command our interest. Geodesy is so broad that attention should be given to each of the special fields of the science. Individuals with considerable experience in the particular field they represent will speak on present day problems in geodetic astronomy, triangulation, leveling, gravity, and studies of the geoid.

Some Remarks on Geodetic Astronomy

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The primary objective of geodetic astronomy is the precise determination of longitude, latitude, and azimuth from observations of celestial bodies. In astro-navigation, the situation is similar; we track the celestial bodies. Processed tracking data coupled with the coordinates of various stars as listed in a catalog yield information about the position of the observer and the direction in question. In general, the navigator, while tracking celestial bodies, will change his position with respect to the Earth. The navigator's motion has a definite effect upon both the astro-navigational technique and the accuracy which may be obtained. The precision of the results is an important aspect of geodetic astronomy, and is usually obtained at the expense of both time and cost.

We may indicate the following principal applications of geodetic astronomy:

Astronomic control of geodetic triangulation and trilateration nets,

Determination of boundaries,

Information about the figure of the Earth,

Detection of crustal movements of the Earth from azimuth observations,

Establishment of the poles' movement.

For the celestial sphere, it makes no difference where we locate the center. We place a sphere of unity radius about the point of observation; the direction parallel to the Earth's axis of rotation, the direction of the vertical, and the direction to a given star will intersect the unit sphere at three points, P, Z, and S, respectively. These three points of intersection then form the spherical triangle PZS as indicated on Figure 1.

Let us review the basic terminology employed in geodetic astronomy. Figure 1 below may help to make the meaning of the symbols employed more clear. All angles are measured clockwise.

Local meridian—The direction of the vertical, which is tangential to the plumb line at a given point, and the parallel to the Earth's axis of revolution form the plane of the local meridian.

Latitude—The tangent of the plumb line at a given point and the parallel to the Earth's axis of rotation through this point enclose an angle of $90^\circ - \phi$, where ϕ is defined as the astronomical latitude.

Longitude—The angle λ enclosed between the planes of the local meridian and an arbitrarily chosen plane of the initial meridian is known as the astronomical longitude.

Azimuth—The horizontal angle A, measured clockwise from the plane of the local meridian to the vertical plane of the celestial body is called the astronomical azimuth.

Zenith distance—The zenith distance Z is given by the angle between the directions of the zenith and the celestial body. Zenith direction is defined by the direction of the vertical; consequently, the position of the zenith is affected by the distribution of both near and distant masses.

Hour angle—The angle t at the pole between the planes of the local meridian and the declination plane of the star is known as the hour angle.

Parallactic angle—The angle p formed at the celestial body between the great circles of the declination and the zenith is known as the parallactic angle.

The measureable elements of the spherical triangle SPZ are: (1) zenith distance, (2) horizontal direction, (3) celestial coordinates of the projection of the target on the celestial sphere, (4) time, and (5) parallactic angle, indirectly.

The position of the zenith may be determined by measuring the target position in two positions of the telescope without any star observations. The pole, however, is not a geometrical point on the celestial sphere which can be directly determined. Therefore, neither the parallactic angle nor the hour angle can be measured directly. Further, the precise direction of the local meridian (and thus, the azimuth) cannot be determined directly and must be deduced from astronomical observations.

We shall now discuss the method utilized for observations of the parallactic angle. To make the problem more clear, we note that to measure the parallactic angle of a star S_1 , another star

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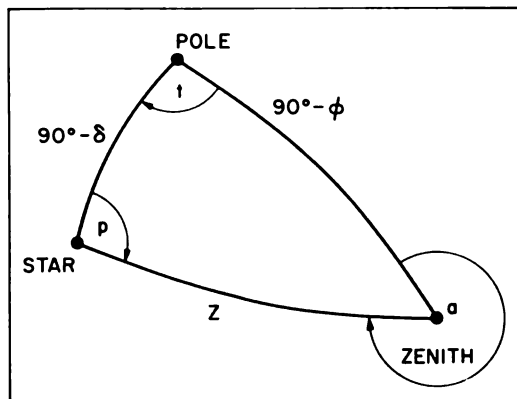


FIG. 1—Definition of terminology

S_2 must be observed simultaneously. The geometrical configuration is illustrated in Figure 2. From Figure 2, we find that

$$p = B_1 - C \quad (1)$$

where the angle B_1 is computed from the star's coordinates. The angle C can be observed directly as is shown below in Figure 3.

Let us assume that the star S_1 is in the center of the field of the telescope. The instrument is set in such a way that the line VV' is vertical and the line HH' is horizontal. The angle C is formed between the lines S_1S_2 and VV' , and may be measured optically or obtained from the photograph.

In addition to the use of stellar methods, measurements may be made with the aid of the Moon. The basic handicap of lunar methods arises from the fact that relatively small distances are utilized in determining larger distances. This process of 'magnification' is detrimental to the accuracy of the results due to the unfavorable law of error propagation. Only a radical improvement of the input accuracies can alter this situation. In addition to this difficulty, lunar methods also suffer from the following inherent difficulties: (1) the present knowledge of lunar topography and profile, (2) the existing precision of lunar ephemeris, and (3) the difficulties in geometric definitions of the geometric and gravity centers of the Moon (and of the Earth as well).

The shape of the lunar profile is projected on the surface of the Earth by parallel rays. An error in the lunar profile will affect the contour of the Moon's shadow on the Earth. The resulting error of the contour will be proportional to

the factor $\sec z$, where z is the Moon's zenith distance at the observing station. This law of propagation requires an accurate knowledge of lunar topography, which cannot be obtained at the present state of the art. It must also be realized that the profile interpretation determines the position of the lunar disc center.

The motion of the Moon is affected by the Sun and the other celestial bodies, the Earth and its potential field, and by the position of terrestrial and lunar centers. These factors make lunar theory quite complex. But in addition, we know that the Earth's rotation is not constant. We

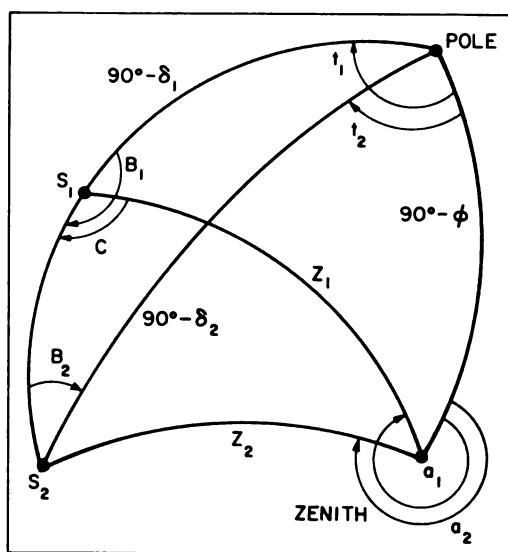


FIG. 2—Determination of parallactic angle

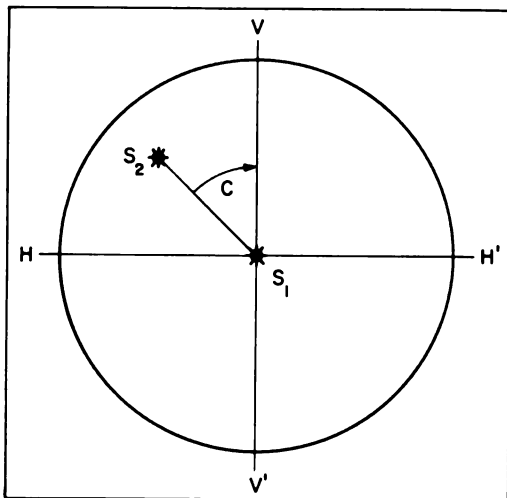


FIG. 3—Observation of angle C

shall hope that improvement in techniques of observation, and continued collection of data concerning the figure and orbital behavior of the Moon—when combined with work on lunar theory—will supply additional information. Even so, there are strong indications that if the problem of lunar theory were solved, there would still remain limitations due to inadequate knowledge of the topography and profile of the moon.

Lunar methods as yet do not produce results satisfying geodetic requirements. However, it must be pointed out that there will always exist situations in which lunar methods will provide the only way to obtain reasonable information about plumb deflection. There are indications that lunar observations may improve existing knowledge of the terrestrial time standard. Lunar observations are useful in many ways; however, we must realize that the results of present lunar methods do not meet the requirements of geodetic control. Consequently, Moon observations are not applicable to first-order geodetic work.

The Moon may be considered as a convenient carrier of a reflector. We may think, for instance, of a corner reflector, or of spherical containers filled with chaff, which would provide a well-defined feature on the Moon and which would increase the precision of optical and radar observations. Greater accuracy could be expected from a beacon system which would make possible distance measurements. Such a transceiver could operate on either solar or nuclear batteries. Simultaneous observations from several stations would eliminate the lunar ephemeris and would also produce valuable information relevant to both lunar theory and time standards.

The artificial satellite creates new possibilities for geodetic astronomy, but this is a subject to be dealt with at this conference on another occasion.

Recently the feasibility of using simultaneous methods has been considered. We mention the following types: (1) from a group of observations completed at the same station—in principle, at different times—two or more unknowns may be determined simultaneously, (2) different stars are observed simultaneously at the same station, and (3) the observations of the same, or different targets are conducted simultaneously at two or more stations.

Such differentiation is necessary. The relationships between the observed and unknown quantities are given in the form of a trigonometric equation; from these equations the propagation

of error and also conclusions concerning the design of observational technique can be deduced. This method of an over-all analysis of geodetic astronomy was initiated by Daniel Bernoulli. His approach was followed by Niethammer in Switzerland, Kempinski in Poland, Beljajev, Vlasov and others in the Soviet Union.

The usefulness of this approach has been noted by the author. Following Bernoulli's technique, the author found theoretically that the precision of the azimuth determination at a station is primarily a function of the zenith distance of a star. The same precision of azimuth determination can be expected from circumpolar and equatorial stars observed at equal zenith distances. These theoretical findings were confirmed experimentally. The observation of stars with large zenith distances has the advantage of decreasing the effect of inclination and pointing errors. On the other hand, the effect of refraction on star observation is increased. However, the effects of refraction upon the target will be considerably greater, since the target, as a rule, will be observed closer to the horizon. Therefore, the precision of azimuth determination will be determined principally by the lateral refraction of the target as opposed to that of the star. This reasoning leads to the conclusion that a decrease in azimuth accuracy cannot be expected when the star is observed far from the zenith.

Usually, astronomical observations are performed at elevated points. Experiments indicate that lateral refraction is affected primarily by atmospheric conditions near the observer. Therefore, if the star and the target are observed in approximately the same azimuth and zenith distance, both observations will be affected to almost the same extent by refraction. Also, since the star and target are observed at about equal zenith distances, and therefore in the same position of the horizontal axis, the effect of pivot irregularities will be minimized. Assuming a certain thickness of the atmosphere (say 10 km), we can find the zenith distance at which the ray path of a star through the atmosphere is equal to the range of the azimuth line. For triangulation of the first order, this requirement is satisfied for zenith distances of about 75°.

Bernoulli's technique leads to a very interesting conclusion concerning the determination of the Laplace equation directly from astronomical observations. The azimuth of a line between two stations, determined astronomically, differs from the geodetic azimuth by the component of plumb

deviation in the prime vertical of the station. We have the relationship

$$A_a - A_g = (\lambda_a - \lambda_g) \sin \phi \quad (2)$$

where

- A_g is the geodetic azimuth
- λ_g is the geodetic longitude
- A_a is the astronomic azimuth
- λ_a is the astronomic longitude
- ϕ is the astronomic latitude

By differentiation of

$$\cos \phi \tan \delta = \sin \phi \cos t - \sin t \cot a \quad (3)$$

the corresponding observation equation can be deduced. Let us further assume the identical preliminary astronomical and geodetic values for the azimuth and longitude. As a result, the following observation equation is obtained

$$V = (\Delta a - \Delta T \sin \phi) \sin z + (\Delta T \cos \phi \cos a - \Delta \phi \sin a) \cos z - L \quad (4)$$

where Δa and ΔT are the corrections of the azimuth and clock, respectively and L is the absolute term.

It can be shown that the term

$$\Delta a - \Delta T \sin \phi$$

is identical with the astronomical correction term of the Laplace equation. Therefore, from at least two observations, the term $\Delta a - \Delta T \sin \phi$ of Eq. (4) can be determined astronomically and introduced directly into the Laplace equation of the geodetic network. Utilizing the notation, $\Delta a - \Delta T \sin \phi = X$, and $\Delta T \cos \phi \cos a - \Delta \phi \sin a = Y$, we have for each star's transit the observation equation

$$V = X \sin z + Y \cos z - L \quad (5)$$

with X and Y being unknowns to be determined. For a star observed on the horizon, $\cos z = 0$; therefore, the unknown X can, theoretically, be determined from observation of only one star on the horizon. From a practical viewpoint, at least two stars' transits are necessary to determine the unknown X .

The Gaussian analysis of errors of the observation Eq. (5) indicates that the precision of the Laplace term X is only a function of the number of observed stars and their zenith distances. Therefore, the astronomical term of the Laplace equation can be determined with the

same precision at all latitudes. The precision of X determination is also independent of the azimuth of the geodetic line and of the azimuth of the great vertical circle of astronomical observations.

The astronomical term $X = \Delta a - \Delta T \sin \phi$ can be determined for all Laplace stations with equal precision by choosing the same number of stars and approximately the same zenith distances of star transits. Therefore the method of direct determination of a Laplace equation has particular significance for triangulation and trilateration networks at high latitudes. Another practical feature of the method is the fact that the total astronomical term of a Laplace equation is determined. Therefore, no separate determinations of azimuth, longitude, and latitude are necessary. This means increased efficiency of work.

Regardless of how strongly I may feel about the advantages of the theoretical approach used by Bernoulli, I also believe that geodetic astronomy is an empirical science. Therefore there will always be a place for differences of opinion about the experimental design, the number and selection of stars, and the number of required observations.

The development of electronic methods has raised the question of astronomic control of nets when direct pointing from one station to another is not feasible. Because of the propagation of errors, it is not practicable to observe azimuth along a short inter-visible line and develop it to a longer line of the trilateration net. It is customary, for the purpose of Laplace control, to use either first-order triangulation or to establish a local net of triangles.

At this time, we can ask ourselves:

(1) Is it possible to obtain a precise azimuth from line crossings of a plane? (Sodano of the U.S. Army Map Service has done extensive research on this problem.) (2) Is it possible to determine the Laplace term, or difference of Laplace terms at two stations from astronomic observations of zenith distances, azimuths, and parallactic angles? (3) How frequently must the Laplace stations be established? Baeschlin did research on the distribution of Laplace stations in triangulation nets, concluding that it is necessary to have one Laplace station for every 10 to 30 directions of the net. This problem should be investigated for trilateration nets.

Astronomic equipment itself needs a great deal of attention. Two items in particular may

be mentioned here: (1) Replacement of the micrometer eyepiece movable wire by a photoelectric device to attain an increase in time determination accuracy. Considerable progress has already been made; however, much more remains to be done. (2) Developing a combination of a radio receiver and frequency standard to eliminate the usual spring clocks. The frequency standard would be automatically synchronized with the radio time signals. When the radio time signals could not be obtained, the frequency standard would supply the time reference for the star observations. After the radio signals are received once more, the time comparison between the frequency standard and radio signals would be made automatically. This combination would enable time references even when time reception is impossible because of unfavorable radio propagation conditions.

In computations required by geodetic astronomy, the following principal types of problem require attention: (1) translation of observation records into numerical values, (2) computation of coefficients, (3) establishing observation equations, and (4) computations of unknowns.

Is there much benefit from the use of high-speed computers as far as time saving and efficiency are concerned? A great deal of work is involved in stages (1) and (2) and this must now be done by 'hand.' We might also ask, should there be special computers for geodetic astronomy?

A few words may be mentioned here about plumb deflection. Because of distribution of masses, the direction of the vertical does not coincide with the normal of the spheroid of reference. This discrepancy can also be represented as the elevation of the geoid above the reference spheroid, and in general, will vary from place to place. It is difficult to envisage that the plumb deflections and geoid elevations will be determined for all stations in the trigonometric net-

work. Supposing the plumb deflections are known at a number of stations, the question of a function arises which would enable the interpolation of plumb deflection and geoid heights. Another related question is that of the curvature of the plumb line, which can be solved by combined gravity and astronomical observations.

The following main fields of research in geodetic astronomy may be indicated: (1) analysis of methods of observation, (2) analysis of program of observations, (3) Laplace control (a) direct determination of Laplace term, (b) frequency of Laplace stations, and (c) azimuth determination between non intervisible stations. (4) determination of plumb deflection and the curvature of the plumb line, (5) study of the figure of the Earth, (6) investigations regarding lateral and vertical refractions, (7) ephemeris research and time signals, (8) improvement of instrumentation, (9) investigation of computing methods, and (10) detection of pole movements.

It is the author's opinion that closer ties and cooperation between gravity work and geodetic astronomy would be beneficial to acquiring knowledge of the Earth's figure. In this case, the Russian example is very instructive.

In the last few decades, the main field of interest in astronomy has been that of astrophysics, and for good reason. Classic and geodetic astronomy have been less fortunate; they have appeared less attractive. Just recently the interest in geodetic and classical astronomy has been intensified. It is of interest that geodetic astronomers have been in the leading positions of the Soviet space activities. Their participation, I believe, contributed significantly to the Sputnik success.

For geodesists in America, it is encouraging and challenging to see this growing interest in geodesy. It is hoped that this development will create more opportunity for research in geodetic astronomy—which is so much needed.

Discussion

Dr. John A. O'Keefe—First, as far as Sodano is concerned, he had essentially two triangulation points. A plane was flown across the line between them and angles were observed to the plane. The error of the azimuth was a matter of two seconds.

The other technique was to put stations at

three points and measure to flares dropped at three points inside the triangle to get the azimuth.

I would like to talk about a more philosophical question. The problem that Johns brought up again and again were these little paradoxes of geodetic astronomy, most of them connected

with azimuths. What are we trying to do? We have the stellar system above us and we are on the ground. We are trying to orient the stellar system with our ground position.

If you have a sphere and want to orient it to yourself, first you have to develop a point on the sphere which is your zenith; then rotate the sphere around that point. The zenith is a perfectly definite point and there is no philosophical trouble about that. The trouble comes when you try to rotate the celestial sphere. This is when the paradox comes into that problem. The conventional way to do it is to start from the zenith pole and run the great circle through the north celestial pole to some point on the ground.

What I contend is that north is obsolete, that the concept of north should not be a part of modern geodesy. The most important things that Roman did in Canada was to adopt this idea of measuring angles not to the celestial pole but to a point along the horizon. If there is any uncertainty about the zenith, and if the zenith and pole are close together, then obviously the direction of the line between them is very poor. Obviously at the north pole of the Earth it is impossible to get anything in this way. Yet at the north pole of the Earth you can measure from a horizon point to a star and can keep track of it and establish how a celestial sphere sits.

In the methods which Roman developed in Canada he was saying, "I will not try to use the line that goes from my astronomic zenith down through the celestial pole and try to measure to the horizon. I will adopt a celestial meridian which is defined geodetically and carry that meridian down to the horizon. In other words, I will adopt an arbitrary reference point around the horizon and then measure."

So you see that the reason why we get all these paradoxes is that we are trying to do something that is a little silly. We are determining the rotation of a celestial sphere around our zenith by means of a point not far from our zenith. We should obviously measure from a point around the horizon. That is the point Johns is talking about.

Now one more point. When Air Force pilots fly in the north pole area they have practically eliminated the north. I believe that the concept of north is perhaps an idea which is of historical and cultural value but should be liquidated in

modern geodesy. In ancient times, even fifty years ago, one determined azimuth from the north pole in an unambiguous way because this was the only point in the sky which could be specified. Since one could not determine the Greenwich time one could not fix this position of other stars from the observer. Now that we have precise radio time we are free to establish a point around the horizon from which to establish our azimuth.

Dr. George Veis—I would like to say that with the same way you get rid of the pole you could get rid of the zenith. If we use the stars as reference we would get the directions in space between our stations and any other stations we want to. This is going to be completely independent of any deflection of the vertical or where our zenith is.

Mr. Charles A. Whitten—How would you propose to survey from one point to another?

Dr. Veis—By photographing the stars as background, the stars themselves define an absolute reference system.

Mr. Whitten—You photograph stars but how are you orienting a network of points on the ground?

Dr. Veis—I am not referring to any azimuth but just direction cosines with respect to a system as defined by the mean astronomical meridian of Greenwich and the axis of rotation of the Earth.

Mr. Whitten—Then you must refer that to some system of points on the ground and how do you propose to do that?

Dr. Veis—The transformation from the sidereal system to the terrestrial system can be made easily by rotation using the result of the International Latitude Service and the Bureau International de l'Heure.

Mr. Whitten—There is still a leakage there in getting the relation to the ground and perhaps we can talk about it later.

Mr. William M. Kaula—The accuracy of the flare azimuths was actually better than the two seconds stated by O'Keefe. The two-second interval was the average of the differences from the US Coast and Geodetic Survey azimuth for two methods of the 'light-crossing' technique, one better than the other. For the better method, the light crossing difference was 1.1", as compared to 1.2" for the same line using the flare-triangulation method, with pointings on flare

drops from three stations. The differences from the US Coast and Geodetic Survey azimuths for the flare triangulation was $0.3''$ and $0.4''$ over the other two lines. So the light-crossing method is probably better than $1.0''$ in azimuth.

Mr. Whitten—Those of you who have had experience in observing astronomical azimuths know it is difficult to get an accuracy of one second.

Mr. Kaula—The point is that there was only one line with a discrepancy of more than half a second.

Dr. Roman K. C. Johns—I would comment on an interesting feature of lateral refraction, namely, that experiments indicate that lateral

refraction depends on the direction of the line. Suppose that we observe in the plane of the local meridian, to the south and to the north. Because of differences in exposure to the Sun, the lateral refraction in the south direction will be different as a rule from the refraction to the north. I always obtained appreciable discrepancies in the results of night observations over a number of nights.

Mr. Kaula—Actually the figures $1.1''$ and $1.2''$ were from pointings in different directions. The figure $1.2''$ was from pointings to flares 30° to 45° to one side of the line. The figure $1.1''$ was obtained from an aircraft right on the line of the azimuth itself.

Geodetic Networks

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The primary purpose of a network of triangulation is to position the network with respect to the Earth. In order to do this it is necessary to have: (1) a point from which to start; (2) a direction in which to proceed; and (3) a surface along which to compute. Having these, a datum is defined.

To establish a national geodetic control system, an initial point is chosen where the deflection of the vertical is not abnormal, then astronomic latitude, longitude, and azimuth are observed at this initial point. These astronomic values and some ellipsoid of reference define our datum.

After angles in the network are observed and a baseline is measured, geodetic positions can be computed based on the provisional astronomic datum. Then, if astronomic positions are observed at other points in the network and these positions compared with the geodetic values, the differences represent deflections of the vertical. This datum is called provisional because it is based on one astronomic position. A more representative datum can be determined by observing several astronomic positions and equating the deflection differences to zero. If the network is then extended based on a datum involving all astronomic positions, and the deflections are abnormal, considering the terrain, the adopted ellipsoid was a poor choice.

A good example of an erroneous ellipsoid is the Everest, on which the surveys of India are based. The extension of these surveys into Thailand shows that the deflections are systematic and are on the order of 30" in longitude.

The extension of our North American 1927 Datum to Alaska and through Mexico and Central America does not show the deflections to be systematic, therefore, the ellipsoid adopted for the North American Datum is a good approximation to the geoid.

Astronomic azimuths are affected by any deflection of the vertical in longitude. If this deflection is known, the geodetic azimuth is obtained by correcting the astronomic azimuth. The correction is equal to the difference between

the astronomic and geodetic longitudes multiplied by the sine of the latitude. From this correction it can be seen that if the adopted geodetic datum contains an error in longitude, an erroneous correction will be applied to the azimuth. This in turn will cause an error in the latitudes and longitudes of subsequent stations. Furthermore, if the dimensions of the ellipsoid do not approximate the geoid, the geodetic positions will accumulate further error. These are prime reasons for the requirement of an accurate datum for extensive networks of triangulation.

The measured baselines in a geodetic network are referred to the sea level or geoid surface. Theoretically, the distances should be reduced to the mathematical surface on which the computations are based. Unfortunately, the true shape and size of the geoid is seldom known when a datum is established and it is necessary to assume that mean sea level is on the surface of the spheroid. If we assume an average geoid height of 50 meters for a baseline, the length reduced to the spheroid would change by about one part in 125,000.

In a network where the differences in elevation are large, the observed directions should be corrected for deflection of the vertical. To compute this correction it is necessary to observe astronomic positions. When the angle of elevation between two points is approximately six degrees and the difference between the perpendicular deflection components is 20", the correction to the observed direction is two seconds of arc. If a baseline in a valley is projected to adjoining stations on mountain peaks, this correction should be applied to the observed directions if precise results are required.

In order to obtain consistent results from the observational data, it is necessary to make a least-squares adjustment of the conditions involved. After a basic network has been adjusted, new surveys are usually controlled by the adjusted results of the basic net. This process is continued as additional surveys are added. Better results could be obtained if a simultaneous adjustment of all observations is made when new

surveys are added to the basic net. This is not practical because of the amount of work involved. Occasionally parts of our basic net are readjusted along with new surveys. This is done to avoid forcing large corrections into the new observations.

Frequently we are asked to give an estimate of the relative accuracy between two points in our geodetic net. An empirical formula devised by Simmons gives the proportional accuracy as one part in $20,000 M^{1/3}$ where M is the distance in miles. Given in meters, the error is $0.059 (K^2)^{1/3}$, where K is the distance in kilometers. This formula is based on position closures from the basic geodetic net of the United States.

Two years ago Hotine of the Overseas Survey Office in London proposed the adjustment of triangulation in space. Using this method, directly observed quantities are used in the observation equations, that is, astronomic values of latitude, longitude, and azimuth. Length control would be furnished by lines measured in space without reducing to sea level. The results would give deflections of the vertical if precise, truly reciprocal, zenith distance observations were made.

Under the direction of C. A. Whitten, adjustments of two nets were made to test this space method. I think some comments from Whitten concerning the results of these tests would be of interest.

Plans are being made through the Interna-

tional Association of Geodesy for a simultaneous adjustment of the triangulation networks of Europe. An adjustment of this size would require the solution of about 10,000 equations. At a special meeting in Munich in 1956, Whitten proposed that equations be included to determine differential corrections to the parameters of the ellipsoid. No doubt this will be done and the results should give a better determination of the ellipsoid.

It would be a fairly simple matter to have most of the geodetic networks of the world directly connected by conventional triangulation. There are direct connections now between North and South America, and between Europe, Africa, and Asia. A connection from Central Europe through Russia and across the Bering Strait to the Alaskan triangulation would connect these five continents.

Since the war the U. S. Air Force has made a connection across the North Atlantic from Canada to Norway. This connection is a network of lines measured by electronic methods. Other trilateration networks connect Venezuela, Puerto Rico, and Cuba to the Florida triangulation. Results of these connections are not available.

By obtaining sufficient ties between continental datums already established, and by cooperation from all the countries involved, a new world datum including all networks would furnish valuable information concerning the shape and size of the Earth.

Discussion

Prof. Frederick J. Doyle—Simmons earlier gave us a tolerance for the dimensions a and f of the ellipsoid. Could you give an estimate of the angle between the axes of the various ellipsoids?

Mr. Charles A. Whitten—We have a panel to give some opinions but I would start out by saying that it might be of the order of three seconds.

Mr. Donald A. Rice—I think it is well to consider the various ellipsoids in terms of the distances between their centers, rather than the angles between their polar axes; by definition all of the polar axes are held parallel as the nets are oriented by the LaPlace condition. The centers of the various ellipsoids may diverge by something of the order of 100 meters.

Mr. Whitten—The longitudes of the various datums may not be coordinated properly so it may be of the order of three seconds.

Dr. Roman K. C. Johns—We do not know how the axes of rotation are situated with respect to each other, except that they are parallel.

Prof. Doyle—Do we know that absolutely?

Mr. Rice—Yes, if the control is adequate.

Dr. Johns—In stellar observations we assume that the observer is in the center of the Earth, however this is not the case.

Dr. John A. O'Keefe—If you convert the latitude, longitude and height to XYZ coordinates you would expect that the difference between what you have done and the truth would be a simple translation without rotation, except for quantities of the order of half a second.

Prof. Doyle—The only angles would be the results of errors in the observations themselves.

Dr. George Veis—I am afraid that this is not correct. It is true that apparently there is no reason for the computation ellipsoid to be tilted. That would be the case if the triangulations were computed with a consistent geometric method. However, this is not so since the measurements are made on the geoid and the computations on the reference ellipsoid. Furthermore, the heights are referred to the geoid. Thus a difference $d\zeta$ between the normal to the ellipsoid and the normal to the mean geoid will introduce a tilt of the same amount (Fig. 1).

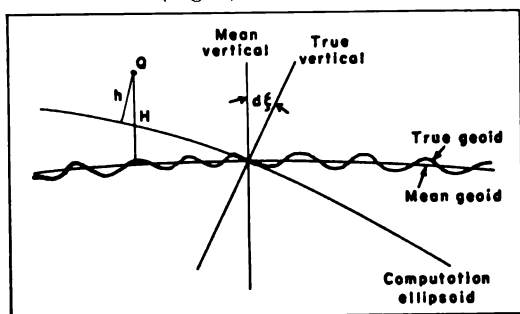


FIG. 1—Relationship of the true geoid, the mean geoid, and the ellipsoid.

This becomes more clear from the fact that although the height of the station Q should be h , the height we give to the same station is H . Since the physical point Q is one and only one we have to rotate the system by $d\zeta$ to be consistent.

Dr. O'Keefe—No.

Dr. Veis—Yes, you have to. There is a residual rotation for this effect. $d\zeta$ is going to be the residual.

Dr. O'Keefe—There are two ways to do that and one is to rotate as you say. The other is a displacement. You determine the geoid by looking at the stars, and hence you should not rotate it.

Dr. Veis—I agree that if you have the heights from the computation ellipsoid by astronomic leveling there is not going to be any question, but if there is a residual $d\zeta$ you have to rotate.

Mr. Whitten—I think this discussion emphasizes that there is some advantage to computing in space.

Dr. Veis—Certainly and that is why I said to get rid of the azimuth, and the zenith point.

(Editorial Insert—In actual practice when computing continental networks the accepted

definitions of the adopted ellipsoid are used. However, because of the lack of information concerning the separation of geoid and spheroid the geodetic positions on a given datum may be systematically incorrect because of failure to project geoid lengths to the spheroid and when extended around a major portion of the Earth's surface may give a computational effect which might be erroneously interpreted as lack of parallelism between the axis of the ellipsoid and spin axis of the geoid. If the lengths of the triangulation are properly computed, the geodetic positions would change, and the resulting space coordinates would be consistent with the assumed ellipsoid. The basic requirements of the datum would be satisfied. C. A. W.)

Mr. Whitten—Earlier I restrained myself when O'Keefe asked about some tests that Harry Brazier helped us with when he visited the United States last spring. Let me give you some of the basic points in Hotine's proposed techniques.

He thinks entirely in terms of space and does not reduce to an ellipsoid of reference because that is an unknown quantity. If the point of observation is $14,000 \pm$ ft above sea level he does not reduce to sea level. The uncorrected or actual observations are used. He had developed the necessary equations to make all the computations in space and then determine corrections of latitude and longitude for points in a network. There are some weaknesses in his system when it is applied to actual observations as was pointed out earlier.

However, there are advantages at the same time. The weakness applies to zenith distances. The function in the equations for that $90^\circ \pm$ zenith angle is the cosine. Being close to zero it cannot have much significance.

We made some computations in Washington for two different areas. First, we tested a net at White Sands, New Mexico, where the observations were made across the hot desert sand. The trigonometric observations were not very good. However, the test confirmed the computing techniques and undoubtedly the results were better for having included all the observations rather than adjusting the horizontal directions alone.

Later, we tested the Pasadena Base net. These observations were made 35 years ago before the day of determining longitude by radio techniques, so there were no astronomic longitudes.

However, there were astronomic latitudes and azimuths. Using these observations we were able to compute the vertical to a reasonable degree of accuracy because the trigonometric leveling was rather good.

First we assigned equal weight to the trigonometric leveling and the horizontal directions. The residuals on the horizontal averaged around a second or so and four to five seconds on the trigonometric. Inasmuch as this was calculated on an electronic computer it did not take long to run it again. We changed the weights to about 1:10 and reran it. The results on the horizontal improved considerably, more nearly what we thought should exist, and the corrections on the trigonometric leveling were still not excessive.

The method has this advantage. In measuring a base line in a valley, projecting that base line up to the mountain peaks, and then using that for the extension of a network of triangulation, considerable error is introduced because we do not know the deflection of the vertical at all points in the scheme. Hotine's method provides a practical method of computing in space and making an accurate determination of the distance between mountain peaks which can be used in extending the network.

It also appears that the method has a further application in determining the best fitting ellipsoid by means of a space adjustment of a world system of triangulation. We might not want to use trigonometric leveling for the height argument. I think we would want to get the help of the gravity people and by working in terms of equipotential surfaces and by using spirit leveling get a better value for the actual point at which we make our observation.

By thinking in terms of the techniques that the photogrammetrists have given us, that is, direction cosines, and if the gravity experts can tell us where the center must be, there may be some further application for computing space coordinates. With the techniques Hotine has proposed and with the ideas presented by others working in the same general field I believe there is some possible application of computing in space.

Dr. Johns—The method of Hotine you mentioned, could yield some information about the plumb deflection. The approach is different from experiments carried out by Kobold in Switzerland.

Mr. Rice—There is no deflection when no ellipsoid is employed.

Mr. Whitten—DeGraaff Hunter and Kobold have made studies for determining deflection of the vertical from trigonometric leveling.

In Hotine's method, equations for the latitude and longitude can be introduced into the network. We can omit these equations. We did omit them on the first test to see what kind of latitude coordinates we would obtain from the triangulation data. Then we compared computed latitudes with observed latitudes and we found them to be in reasonable agreement. In the Pasadena area the deflections are of the order of 25". Later, we introduced all the data we had and then when we projected the line up to the mountain peaks, the result was the most accurate we could obtain.

Hotine concludes that by using all the material you have simultaneously you will obtain the best possible adjustment. The results so far have not been overwhelmingly successful but have been encouraging.

Dr. Johns—What do you think of the feasibility of measuring the ranges instead of angles as it is customary to do.

Mr. Whitten—While we are discussing this I think Aslakson can tell us something about a situation where it was not possible to observe angles but he could measure distances.

Capt. Carl I. Aslakson—Aero Service Corporation was faced with the problem of establishing positions of some pile beacons off shore in the Persian Gulf. These pile beacons were as much as 26 miles off shore and involved measurements up to 26 miles, the average distance measurement being seven or eight miles. Under the conditions of visibility in the desert we could not use triangulation. We would have had to restrict the lengths of the triangulation lines to one or two miles except on rare occasions when the visibility range would be doubled or tripled. Therefore, we established the entire network by tellurometer trilateration on the existing local datum and oriented the scheme from an existing azimuth over one of the shorter lines that was visible. This trilateration network was adjusted by least squares. Our probable error in measuring any one distance as derived from the adjustment was ± 0.206 meters. This was considered adequate inasmuch as the scheme was somewhat weak. However, the probable errors of position exceeded $\pm 0.''007$ or $\pm 0.''008$ of latitude and longitude in only two instances.

I am a firm believer in the use of the Tellurometer for trilateration, and consider it inadequate for traverse if second-order results are desired. There are too many sources of error in carrying angles forward. But I do believe excellent results can be obtained by trilateration methods. One can determine the scale constant in the adjustment if the network has sufficient strength. I am not alone in this belief. Rimington of Australia has done considerable work in Tellurometer trilateration and has compared it with triangulation. His conclusions are in agreement with my own.

Mr. Julius L. Speert—I hate to disagree too strongly with Aslakson as most of my objections were gotten from him. I think most of us recognize the basic weakness of the geometry of trilateration. In fact, that point was brought out earlier in Ewing's figures. The single triangle, while it is a checked figure in triangulation, is not checked in trilateration. Similarly, in a quadrilateral, there are three checks in triangulation, but only one on trilateration. Of course, Aslakson mentions that if one has a strong enough figure, one has good trilateration. I think we cannot overemphasize the need of a strong figure.

My own experience has been not in the theory but in down-to-earth topography. We have all had experience in measuring angles. By proper control of our techniques we can get almost any accuracy we want. We can control the azimuth, and its accuracy, by making an azimuth observation at periodic intervals. My thinking is to get control as fast and as cheaply as we can and still keep it as accurate as we need it. My feeling is that if we run a traverse, measure the angles, control by triangulation or by trilateration when we need to, use the Tellurometer or any other equipment we have available, we have extended our control by running a single chain of lines and gotten from one point to the other point in the fastest most economical way. If we did it by trilateration alone we would have to run an arc with considerable redundancy. Even then our azimuth would be weak because no matter how we measure there is still considerable play in the figures. It is just a simple matter of economics. If we want to get from one point to another in the cheapest way I would prefer to measure with the equipment best suited.

Capt. Aslakson—We are speaking of the same thing. I am talking about main control and you

are talking about supplemental control. I refer to a control network where no triangulation exists.

Some mention might be made of the economy of Tellurometer trilateration versus triangulation. This project in Saudi Arabia contained 59 lines and was observed by two men in one month's time. That is far more economical than triangulation even if we could have used triangulation. If one is faced with the necessity of executing triangulation under these extremely difficult conditions it is often possible to substitute trilateration by Tellurometer and obtain excellent results far more economically. A single three-man party, consisting of a recorder, observer, and ground station operator, can average six to eight distance measurements per day. That is far cheaper than attempting to accomplish the same results by triangulation.

Mr. Speert—Perhaps I should be clearer. I was speaking in favor of traverse. A traverse consists of a single chain of lines. Trilateration consists of at least quadrilateration. I am not so opposed to trilateration that I would refuse to use it where it appears to be the best method. I recognize there could be a good Tellurometer line which could not be observed optically. If it is a problem that can be done cheaper by using a steel tape I would say to do it that way. If it is cheaper by traverse, do it so. There have been cases where we could not measure angles at all but could measure distances electronically. By all means do that. If you have a choice and have the equipment available, it is a question of combining techniques and doing the job the most economical way. Other things being equal, I am inclined to favor traverse over trilateration.

Mr. Whitten—This is an engineering problem, one of economy. Not particularly a scientific problem.

Dr. O'Keefe—I would like to come back to triangulation in space, and just try again to look at this thing. Remember the two desiderates are to find out *where* we are and then to orient. One way of doing this is the photogrammetric approach. This was the essence of Brazier's method. In essence what he did was resect for each position not only to find out *where* he was but even for his orientation. In principle, he did not even level the theodolite at each station.

Mr. Whitten—He did not object to having an astronomic latitude and longitude at each station.

Dr. O'Keefe—Let's take the pure case, in which he was going to determine *all* his unknowns by sighting back on the previous point. In essence it means to ignore what gravity tells us. Why did gravity get involved anyway in a problem of determining where you are? We got into it because of this other question, namely, how on Earth are we going to get good positions by measuring rays when we know that the rays are subject to enormous curvatures?

Now the result that Whitten has laid before us shows in essence that as you go higher above the Earth you get closer and closer to the photogrammetric case. Veis was talking of a problem in space where vertical angles were 60° ; then it is hard to deny that a simple direct solution such as his is the best one. An intermediate case was Pasadena with high mountains. At White Sands where it is fairly level you get to the case of ordinary geodesy.

Mr. Whitten—No, it is not level.

Dr. O'Keefe—All right, let's take Iowa. In these places there is not much doubt of ordinary orientation. On the other hand the vertical angles are extremely bad because you have to be fairly close to the ground. You see, one can have two ideal cases and the actual cases intermediate between them. The extreme cases would be, say, Iowa and a satellite. With the satellite it does not make sense to talk of ellipsoid and so forth. In Iowa if you start to do that you would be in the extreme other end where it would be difficult to measure angles and the information you would get from leveling the theodolite is better than what you get in measuring horizontal angles. We

find that the Pasadena base is somewhat further from the extreme Iowa situation than we thought. It is approaching the case of which Veis was talking.

Mr. Whitten—To comment about Iowa, the zenith distances are all close to 90° . I pointed out that the cosine of the zenith distance appears in the expression of the coefficients of the unknowns. Its effect is zero.

When you are actually going through the mechanics of computing, the problem is very similar to adjusting by variation of coordinates on an ellipsoid. The coefficients are nearly the same. You would not know whether you were using Hotine's or Helmert's method. Where the elevations are all the same, the coefficients are essentially the same. I think that Hotine would object to any suggestion that the astronomic quantities be omitted. He was very emphatic that they be used.

Dr. O'Keefe—Then you are in a different world from that which we actually have to live in geodesy. The crux of the problem is what do we do when we do not have astronomy. What is the best guess of the vertical in a new station? Is it to carry forward by backsight?

Mr. Whitten—The coefficients of the unknown when computing in a flat terrain are essentially the same coefficients as in the classical method of the variation of coordinates.

Dr. O'Keefe—If you have astronomic values I think that is true. But if not, then from the adjustment you get a deflection of the vertical at your new station. You get a weak determination. The question is whether to use it or zero.

Orthometric, Dynamic, and Barometric Heights

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As an initiation stunt at college, I was once asked to speak on the topic, 'How high is up?' Today I introduce a more definite subject of relative height and hope to have more to say. At college, I suppose I considered vaguely as 'altitude' what I now call orthometric elevation and was unaware of a dynamic system expressing height in terms of potential. Both terms 'orthometric' and 'dynamic' may deserve further explanation.

The orthometric elevations used in most engineering work are defined as the geometric heights above mean sea level, or, more technically, above the geoid surface. At the oceans, mean sea level is the surface the ocean waters would assume if there were no disturbing influences of tides, waves, currents, atmospheric and temperature changes, etc. Under the continents, mean sea level is that level surface which still, undisturbed canal waters would assume if a network of closely-spaced very narrow canals connected all oceans. Such a mean-sea-level surface is the same as the equipotential geoid surface.

Offhand, it might seem that perfect leveling starting from mean sea level would always determine true orthometric elevations as observed elevations. This is not true because level surfaces at various altitudes are not parallel. The actual separation between any two level surfaces in different places is inversely proportional to the local intensity of gravity. This intensity increases by one part in 200 from equator to pole. The geoid surface is roughly ellipsoidal, and successively higher level surfaces assume successively greater polar flattening. From a consideration of the theoretical gravity field, a level surface 1000 meters above the sea at the equator is only about 995 meters above the sea at the poles. Therefore, a continental leveling network has to be corrected along the north-south components of the leveling routes so as to obtain truer orthometric elevations. Actually, anomalous local variations in the gravity field cause additional minute variations from parallelism of level surfaces. In general, this effect is minor since it does

not accumulate systematically from equator to pole. In the leveling net of the United States, a simple formula involving latitude and elevation, but disregarding gravity anomalies, enables computations of orthometric corrections to observed leveling differences. In effect, we correct for the major cause of non-parallelism in level surfaces and disregard the minor cause.

To give an idea of size, an orthometric correction of $1\frac{1}{4}$ meters was applied to the observed difference of leveling from San Diego to Seattle, the route having an average height of 1000 meters and a latitude difference of 15° .

The orthometric concept has disadvantages. Under this concept, the mean lake level at the north end of Lake Michigan is 0.07 meter lower than mean lake level at the south end. U.S. Lake Survey engineers, for example, object to this. They want to assign the same elevations to all parts of the lake.

The dynamic system offers an alternative. In considering dynamic heights, we discard the simple concept of measured vertical distances and give to each level surface a number of its own, proportional to the work required to raise a unit mass from sea level to that surface. In geodesy, the dynamic number can be expressed as $(1/g_0) \int g dh$, where dh represents the geometric increments of elevation, g the surface values of gravity, and g_0 an arbitrary constant adopted to make the dynamic number roughly comparable to the orthometric elevation. Gravimeters now allow inexpensive determinations of surface gravity values. When these are determined along all leveling routes, heights in the dynamic sense can readily be computed so as to correct for both major and minor causes of non-parallelism in level surfaces. Each level surface would everywhere have the same dynamic number, and the true dynamic difference between two points would be obtained from perfect leveling, irrespective of the route followed. Though such a system sounds ideal to scientists primarily concerned with equipotential surfaces, the concept of height expressed in terms of a variable geometric-distance unit is not acceptable to

land surveyors, cartographer, and engineers in general.

The International Association of Geodesy has recently recommended that each country compute and adjust its leveling network on a dynamic basis referred to surface measurements of gravity to provide values adequate for scientific work, then convert these to orthometric values to be used as standard elevations by the general public. Unfortunately, to make exact conversions, the average values of gravity from the geoid surface to the points in question must be known. However, even if approximations are used, the resulting orthometric system is improved. The Association adopted a kilogal-meter unit of dynamic measure defined simply as $\int g dh$ and called the geopotential number.

I cannot say when our Bureau will adopt this recommendation. This depends in part on how soon gravity is measured along all our leveling routes. At present, we publish orthometric elevations based on theoretical gravity and can furnish dynamic numbers on the same theoretical basis.

Barometric determinations of elevations have been used very little by geodesists because of lack of precision. When used for approximate purposes and adjusted to known orthometric elevations, the height differences obtained are considered to be orthometric differences. When barometric observations are used to determine heights in the upper atmosphere, however, orthometric differences are not readily obtained. There is a more logical correlation between barometric differences and differences in dynamic number than between barometric and orthometric differences. If *absolute calm* prevailed everywhere in the atmosphere, a particular isobaric surface would coincide with a level surface of a particular dynamic number. Meteorologists, therefore, often determine dynamic heights. First, they determine dynamic differences from barometric observations assuming that some suitable standard atmosphere conditions exist, then they correct such differences for all known departures of temperature and humidity from such standard conditions. Though temperature and humidity represent the dominant variables, the station values are not always representative of the air column, and there are additional factors which affect the difference in dynamic numbers, such as the slope of the local isobaric surfaces, winds,

friction, and other meteorological parameters. The slope of the isobaric surfaces can be taken into account to some degree in multiple-base barometric leveling, but the involved computations required may not be justified in work of such low inherent accuracy.

The geopotential-number unit of the geodesists is 98/100 of the geopotential-meter unit of dynamic height adopted by the International Meteorological Organization in 1947.

A more important problem than the unit of measurement is that of the releveling that should be done. In terms of geologic time, huge changes of elevations of the ground surface are known to have occurred, but in the past it was assumed that crustal movement in any particular 25- or 50-year period could be considered negligible. However, comprehensive releveling programs undertaken by several European countries have shown considerable crustal movements. In Finland, crustal tilting producing a maximum uplift of nine millimeters per year over a 50-year period has been proven. In our own Great Lakes region, an interesting theory of crustal tilt has been deduced from a study of tide-gage readings of lake levels. In Texas and California, our Bureau has made periodic relevelings that show, at some points, settlements of over a foot per year. Though such settlements seem to be due mostly to withdrawals of water or oil rather than to tectonic action which may continue over the years, the point remains that there are many causes of changes in elevation.

As yet, our Bureau has not begun any comprehensive program of releveling on a national scale. We do have a definite plan for such a program.

I firmly believe that our proposed program can be justified from purely practical considerations for its value in strengthening and bringing up-to-date the vertical control net, and for its value to specific engineering applications. I would like to leave before this Conference the question: Is a releveling program to determine the changes taking place in the Earth's crust justifiable as basic research in the Earth's sciences?

If so, our large continent provides an ideal laboratory for such research. Research into upper atmosphere and space is fine, but we also have closer ground for research—the ground under our feet.

Discussion

Mr. J. E. Lilly—Most of our studies have been based on the Great Lakes gage readings rather than precise leveling. We have found a definite tilting in the order of one part in a million, one millimeter in a kilometer per century, or something like that. At that rate in common language they figure the shift has been around six inches between Canada and North Bay in the last 50 years.

Mr. Charles A. Whitten—You do not have a program for releveling?

Mr. Lilly—No definite program.

Mr. Braaten—I think it should be pointed out that if any program is made on a limited scale the most we will get is a relative effect. A releveling program has to be undertaken on a continental scale before we can determine the total overall movement.

Mr. Lilly—The common explanation is that it is a resurgence of the Earth's crust because of melting of ice from the ice age. The center of the old glacier is rising.

Mr. W. L. Berry—Are there any figures available on the magnitude of lateral crustal movements other than obvious faulting?

Mr. Whitten—Observations have been made along fault lines and there is a geodetic program for reobserving networks in California and Nevada along fault zones. Even though there are no earthquakes, there is a slow creeping movement of the land mass on one side of the fault zone relative to the land mass on the other. This slow movement amounts to about one foot in ten years.

Dr. Roman K. C. Johns—Ewing originated a theory of a rising level of oceans. Possibly he would tell us about it.

Dr. W. Maurice Ewing—The question that occurred to me when the gentleman from Canada was speaking was that in the several photographs I have seen of the northern islands, Canadian archipelago, beautiful raised beaches showed and all indications are that the ages of those beaches are about ten thousand years. The new glacial map of Canada just published showed the elevation of a few of these and they are at heights of several hundreds of feet. I think it would be of the greatest interest to find some way to tie this record of a long time into modern day measurement of rate.

When it was believed by everyone that the

greatest thickness of ice was not very far north of the border between Canada and the United States it was expected that most of the uplift due to the removal of the ice load was around the Great Lakes. The possibility exists, and it was sharpened up by the increasing evidence of a greater uplift further north. This ties in with some theories that the greatest amount of ice was farther north.

Mr. James B. Small—The tidal observations show that along the Atlantic Coast mean sea level is rising about 0.011 foot per year. Along the Pacific it is rising about 0.005 foot per year. Our level net was adjusted in 1929 holding mean sea level at zero at 26 tidal stations along the Atlantic and Pacific oceans and the Gulf of Mexico but it is not practical to readjust it to take care of the changes in mean sea level so we just admit there is a difference between modern local mean sea level and elevations in the geodetic net.

Isolated land elevation changes get rather rapid in some areas. We have detected changes of about 20 feet in California. That is our most difficult area to do leveling in because there are so many factors contributing to change, such as earthquakes, removal of oil, removal of underground water for irrigation, fault lines, etc. But those conditions do not exist throughout the country and the changes in California are rather unusual cases.

Mr. Whitten—In one of the valleys of California the nature of the soil is such that it is subsiding when water is added for irrigation purposes.

Mr. Waldo E. Smith—A number of years ago, shortly after the building of the Hoover Dam and the filling of Lake Mead, long lines of levels to nearby bench marks indicated the sinking of the valley up to a foot or more. The question was raised whether it was isostatic in character, deflection of the plumb line caused by the mass of water, or compression of the base. What is the up-to-date thinking on this phenomenon?

Mr. Braaten—I believe it was thought that the cause was the loading of an immense reservoir of water. Theoretical computations were made of the depression of the surface that would be caused by such a reservoir of water. I think it was fairly close to what was actually found from the leveling measurement.

Mr. Small—We established a net of level lines

before the water was impounded and then re-leveled after it was impounded by using tide gauge work to go across the lake and the indication was there had been a settlement of about four tenths of a foot due apparently to the superimposed load.

Dr. John A. O'Keefe—We began by noticing how difficult it was to measure vertical angles. Yet here we can measure from the center of the continent to the coast line with an error of 1.5 feet which means one part in five million. I call your attention again to the way in which the purely geometrical approach was sweetened by the introduction of gravimetric information. In other words, what Braaten has is height which is not height above a reference surface. He could not tell you the angle at the coast line to the ellipsoid at this point. He has managed to solve the problem without measuring vertical angles.

Mr. Braaten—I thought that was covered pretty well by Simmons in saying we use short balanced sights and extremely precise instruments, and correct for all known systematic errors encountered in the differential leveling procedure. I further thought that the ordinary differential procedure would be fairly well known and would not constitute a proper topic for discussion at this time.

Dr. Johns—There are two ways of looking at the variations of sea level. Either the sea level is changing or the ground elevation is varying. I am wondering if there are any more objective

means to establish which level is moving, the ground or the sea.

Mr. Braaten—I would like to know of some if there are. But I am not sure that I have answered Dr. O'Keefe's question.

Dr. O'Keefe—I think it is interesting and fundamental to see how you solve this problem, which would seem on its surface to be insoluble, namely to get an accurate relative elevation by an optical method in the face of the ray curvatures. As I understand it you eliminated the curvature by standing in the middle of the arc so the effect is the same on the foresight as the backsight, provided that the curvature is the same throughout that line.

Dr. Johns—The curvature of the arc is not necessarily identical. As a matter of fact, generally speaking, the curvature of vertical sections of equipotential surfaces is variable. The curvature depends on the equipotential surface of the station. The curvature of the level surface may, under certain conditions, vary appreciably.

Mr. Braaten—I believe if gravity is determined along the leveling route at sufficiently close intervals we need not worry about the irregularities. Once having measured gravity values and the geometric difference in elevation accurately we can determine true values of dynamic elevation. The beauty of leveling is that we do not refer to the spheroid—we could not—but we do refer to the equipotential surface for which a definite knowledge is furnished by the plumb line.

Gravity and Gravity Reduction

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From potential theory it can be shown that under proper conditions a knowledge of gravity intensity over the surface of a body will yield the shape of that body; it will also yield complete information on the gravity field outside the body. The most important conditions are: (a) that the surface be equipotential as generated by combined self-attraction and centrifugal force, and (b) that the surface enclose all attracting matter. A near approximation to such a surface is the geoid or sea-level surface of the Earth. For land areas this surface can be imagined as traced by a network of canals connected to the oceans. It is true that the geoid does not quite enclose all attracting matter, and on land it is not directly accessible for gravity intensity measurement. Various gravity reduction techniques alleviate these difficulties. The shape of the geoid and the associated external gravity field provide the physical framework for most geodetic operations; for example, astronomic positions are convertible to geodetic positions only if we know the local geoid distortion at the observation point; other examples are obvious when we think of the various level-sensitive devices employed in geodetic surveying.

In the 19th century Stokes developed means for finding the shape of the geoid, having gravity-intensity observations on the surface. The first of these requires the expression of gravity anomalies in a series of surface spherical harmonics, the anomalies being differences between observed intensities on the geoid and the theoretical intensities calculated for an ideal model ellipsoid. The second method, and the one usually employed in practice, involves the Stokes formula which permits a numerical integration of the surface anomalies. Both methods yield linear departures of the geoid above or below the model. About 30 years ago Vening Meinesz derived an extension of this formula to obtain directly the geoid slope referred to the model ellipsoid. This is the 'deflection of the vertical' which provides conversion from astronomic coordinates to geodetic or space coordinates on the model.

Although there is a long history of mathe-

matical development, practical application had to await observations over considerable land and water areas of the Earth. This stage was reached in the 1930's, when gravity techniques became an essential element of fundamental geodetic measurement. In favorable areas such as North America and Europe, geoid departures can be calculated with a precision of about ten meters; deflections of the vertical are determinable within one or two seconds of arc on an absolute basis. It must be emphasized that gravimetric techniques alone will not give the scale of the geoid, only its linear departures from the model.

Gravity can now be measured quickly and accurately on land by means of static gravity meters, controlled by relatively few and widely-separated pendulum stations. The Vening Meinesz pendulum apparatus, and the recently-refined versions of conventional gravity meters, are now employed in obtaining coverage at sea. Special remote-reading gravity meters operate very well on the ocean bottom at 300- or 400-foot depths. There is hope for useful gravity measurements in airborne vehicles within the near future.

A gravity measurement *per se* is hardly useful in geodesy. At a given point, gravity can be measured to better than one part in 10^6 on land and about five parts in 10^6 at sea. As gravity changes about one part in 10^6 for ten feet of vertical displacement, clearly the land gravity measurements must always be coordinated with reliable elevation data. This is quite a serious operational problem in isolated or unmapped regions. At sea there is, of course, no elevation problem but east-west velocity of the vehicle must be determined with some precision to eliminate the Eötvös effect. In the air, velocity and long-period accelerations would have to be evaluated either instrumentally or by observations from the ground.

Before the Stokes formula can be applied, the surface gravity measurements must be transferred to the geoid whose shape is being determined. For land stations this is usually accomplished by applying the normal vertical gradient. In all the common methods of gravity reduction, something must also be done about the land

masses projecting above sea level. They can be either abolished, squeezed down flat on the geoid, or transferred within it. The effect of mass elimination or transfer on observed gravity at each point must be calculated from the inverse-square gravitation law. Whatever is done, there is some deformation of the actual geoid and the Stokes formula will give the shape of the resulting artificial geoid. Conversion from the artificial back to the true geoid is carried out as the final step.

Abolishment of external masses is implied in the Bouguer reduction. This reduction, although widely used in geophysical exploration, is not adaptable to the Stokes formula because of the large resulting shift in center of gravity of the geoid. Also, gravity anomalies obtained by the Bouguer system are heavily biased, negatively on high land elevations and positively in deep ocean areas.

The condensation reduction implies a compression of external matter onto the geoid surface without horizontal displacement or change in mass. It is simple in concept and does not require laborious integration of topographic attraction effects. To date, most of the large-scale applications of the Stokes formula have employed the condensation reduction. The chief disadvantage is that gravity anomalies tend to be strongly correlated with topography, and close station spacing is required to obtain good anomaly averages in highly dissected regions.

Various forms of isostatic reductions are also suitable for geodetic purposes. They minimize the anomaly-topography correlation and involve limited mass transfers which can be handled without great theoretical difficulty. However geodetic applications have been somewhat limited, because of the complex nature of the calculations and extensive topographic detail required. From the geophysical standpoint, isostatic theories based on gravity measurements have proved invaluable in defining broad trends in the Earth's crustal structure.

Geodesists are aware of certain theoretical weaknesses in conventional gravity reductions.

Use of the normal gravity gradient introduces some error unless a laborious iteration process is employed. Mass transfers have secondary effects which are difficult to handle with full rigor. Astrogeodetic and gravimetric deflections of the vertical are not exactly comparable, since the one refers to the ground surface and the other to the geoid. In the future, when we have more dense and accurate gravity survey data, theoretical refinements can be introduced to improve the situation. Perhaps the most interesting approach, one which has been intensively studied in recent years, is to use gravity data directly as observed on the physical boundary of the Earth. For convenience, the physical Earth surface probably would be smoothed by some well-defined and simple process without changing the total mass. The anomalies would be gotten by comparison with theoretical gravity as projected upward, employing geopotential height measurements. Under these conditions the integral formulas would ultimately yield the form of the smoothed physical surface of the Earth with respect to the ideal gravity model. The Earth's shape would thus be better defined and gravimetric deflections of the vertical rendered comparable to astrogeodetic deflections as observed on the ground.

Techniques have been developed to calculate the gravity vector at altitudes of some hundreds of miles above the Earth. A convenient method is to express the Earth's disturbance potential in terms of a thin coating of varying surface density on the geoid. Under these conditions the gravity anomaly vector can be determined by integrating the mass attraction effects according to the inverse-square law. Should airborne gravity measurements become feasible it seems theoretically possible to reverse this process. Values of the surface coating, meaned over appropriate area elements, could thus be derived and employed in calculating geoid heights and gravimetric deflections. Such a method would be extremely valuable in extending gravity coverage in the mountains, the polar regions, and over the seas.

Discussion

Dr. Roman K. C. Johns—I think Schneider did some research measuring gravity in space.

Dr. Alan M. Schneider—All of our ideas were based on the classical ideas of the gravitational

field of spherical masses or point masses and our point of view comes more from inertial navigation rather than geodesy. If you measure the space rate of change of the gravitation vector,

you can determine distance and direction to a known heavenly body. We proposed a hypothetical device that might make this measurement by comparison the reading of a pair of accelerometers separated by known space distance. This is the essence of what we did.

Mr. Erwin Feuerstein—What is the magnitude of the gravity anomaly?

Mr. Rice—On the surface?

Mr. Feuerstein—On the surface or fifty miles up.

Mr. Rice—On the surface one should specify the type of anomalies, but let us assume free-air anomalies. Typical values are ten or 20 milligals. Free-air anomalies greater than 50 milligals are quite rare. An anomaly of 50 milligals (50 parts in a million of the total gravity force) would normally require a distance of many miles to build up. There are a few highly disturbed regions where free-air anomalies reach 300 to 400 milligals, for example, in the Puerto Rico trough. Usually there is a well-defined pattern of build-up over substantial distances. There are exceptions to this, however. In certain Pacific island regions a build-up of 200 to 300 milligals may occur abruptly because the island structure resembles a point load on the crust.

Mr. Feuerstein—Are there any time variations?

Mr. Rice—Not that we have been able to detect. Over a period of thousands of years there is probably some variation caused by sinking of large land masses and so on.

Dr. Raymond H. Wilson—I have a question on mean sea level and heights across the continent. Mean sea level, as I understand is approximately the geoidal surface. I heard, for instance, that at the Panama Canal, the sea level at one end is different from the other. I would just like a little further information.

Mr. James B. Small—Mean sea level at the Pacific end of the Panama Canal is about two-thirds of a foot higher than the Atlantic. In the United States for the adjustment of the level net we assumed that it is the same. In adjusting the level net, 26 tidal stations were held fixed. So in effect you can say that the elevation of a bench mark in any locality is based on the weighted mean of all these 26 stations. At this location (Boston-Cambridge area) the tidal station in Boston would have the greatest effect but a west coast station theoretically would have some minor effect.

The indication is that mean sea level is slightly higher as one proceeds north along the Atlantic and Pacific. The greatest difference was from Old Port Comfort, Va., to Prince Rupert, Canada, where there was a difference of about three feet. That was all adjusted into the precise level net. That sounds like a lot but it only meant about two-tenths of a millimeter per kilometer of correction to the leveling.

Dr. Wilson—That would refer to the ellipsoid then?

Mr. Small—No, the geoid.

Mr. Norman F. Braaten—Orthometric elevations are referred to the geoid surface, the surface that still water would assume in narrow closely spaced canals connecting all oceans. Observed elevations carried by three separate leveling lines from local mean sea level values on Pacific, Gulf, and Atlantic Coasts to a point in Iowa would differ by more than a foot even if the leveling were perfect. Of course, actual leveling lines are not perfect, but the orthometric correction applied corrects for the effect of non-parallelism in level surfaces.

Dr. Charles A. Lundquist—The last speaker mentioned a centric geoid ellipsoid. How is this defined?

Mr. Rice—By the center of gravity.

Dr. Lundquist—In the sense of real mass distribution of the Earth or center of mass or how?

Mr. Rice—I think it is most nearly correct to consider the center of volume of the geoid. In the derivation of the Stokes formula the assumption is made that the centers of volume of the ellipsoid and geoid coincide. Therefore, the result of applying the Stokes formula will always be consistent with a centered ellipsoid.

Dr. John A. O'Keefe—And this is the center of mass of the physical Earth.

Lt. Bruce C. Murray—One point about your question concerning the Panama Canal. If the water would flow across, it would mean that the water level is not equal at each end. The adjustment they are making is to the effect that it is not the same equal potential surface on both ends, not that there is not an equipotential surface we can use to talk about. The geoid is off about two-thirds of a foot because of tidal forces.

I was talking to Walter D. Lambert a few weeks ago when he was here in the hospital and he made the point about the validity of the previous use of surface coating. In particular if

one has a varying number of measurements scattered around the surface, one can weight the coating very accurately with the measurements. Therefore, for practical operational uses one can use the coating method in much greater correlation with the data than if the distribution of the data is not taken into account.

Prof. Arthur J. McNair—There are many geologists and physicists who would take issue with a statement that the Earth is homogeneous and that the center of volume of the geoid or of the spheroid is identical with the center of mass of the Earth. Granted that many of the gravity anomalies would produce only secondary effects but would it be proper to state that there is no correction made for variations in the density of the Earth's crust? In other words, let us consider an atoll out in the Pacific made of coral which is somewhat lighter than an equal area in the Mesabi iron range. In each case these are above the mean sea level or above the orthometric datum. Is it not necessary to allow for such differences in density and in gravity?

Mr. Rice—The Stokes formula is based fundamentally on spherical harmonic expansion of the gravity anomalies. The first-degree harmonic is forced to be zero, which means that deviations of the geoid above and below the ellipsoid must be a minimum. In other words the geoid and ellipsoid have the same volume and the same center.

Now let us consider what happens with regard to masses below sea level. In this case it is unnecessary to take account of the problem of external masses. Here is where the geodesists and geophysicists differ in viewpoint. The geodesist does not need to concern himself directly with distribution of the internal masses. If he knows the geoid surface anomalies, he has all the necessary information to determine the geoid shape and the external gravity field. Of course, in practice this is not strictly true, because if surface gravity in a certain area is unknown, one might want to make some sort of guess from knowledge of subsurface structure.

Mr. Charles A. Whitten—Reduction techniques could be planned in such a way that a precomputed density could be assigned to this mass so that the anomaly would be zero. That would satisfy.

From the floor—The geoid?

Mr. Whitten—No. The geodesist would be unable to obtain any information about the geoid.

If we change the density, we would get a curvature exactly the same as the ellipsoid. There would not be any geoid information.

Dr. Johns—Ewing mentioned his great project of a geodetic network across the ocean. Until the advent of satellites, the determination of the geoid and the best-fitted reference ellipsoid was limited to information obtained from continental nets. However we should keep in mind that the surface of the globe consists predominantly of oceans. The theory of potential would indicate that the geoidal curvature under the continents is greater than above the oceans. Let us suppose we were able to establish a geodetic net over the Atlantic and obtain information about the plumb deflection. What is your feeling about the difference between the geoids over the sea and under the continents.

Mr. Rice—There are some pretty good clues as to the geoid in sea areas as a result of the progress in measurement of gravity at sea during the past few years. In fact, there have been some radical changes in viewpoint as measurements taken over certain tracks in the Atlantic showed that gravity was different than previously supposed, especially in the region south of the latitude of Bermuda. Thus the geoid at sea is becoming fairly well defined in certain regions.

Dr. Johns—As I understand it, Worzel carried out an extensive gravity program on the sea and came to interesting conclusions. Dr. Ewing, would you be in a position to give us pertinent information about this research of your Observatory?

Dr. W. Maurice Ewing—I cannot add very much to what Rice said. As you know, when we had very few gravity measurements they were not very favorably situated for giving an average value over the seas. It looked like there was a systematic difference between gravity on the sea and on the land. Even a bigger difference was supposed to exist in the Mediterranean Sea. As we are getting more values the value of that shift is in question and we do not know whether we have a shift in the other direction or whether we are still the victims of insufficient sampling.

From the floor—Did I understand you to say that you used 26 stations? Is this the total number of tidal stations available?

Mr. Small—That was the number we had at that time to which first-order leveling was tied. We have many more now.

From the floor—Was it because you find some stations some three feet above?

Mr. Small—No, 0.4 foot is the greatest present divergence between a local modern mean sea level and elevations from the geodetic level net.

From the floor—I thought you said one station was three feet above.

Mr. Small—Before the 1929 general adjustment was made, a theoretical treatment of the level net was based only on Galveston, Texas. The indication was that the difference between Old Point Comfort, Va., and Prince Rupert, Canada, was about three feet. The final adjustment of the level net was fitted to 26 stations. Because of the changes in sea level there is a maximum difference of about 0.4 foot from the geodetic net as it exists today.

Dr. Nicholas T. Bobrovnikoff—Was there any evidence of the secular variation?

Mr. Rice—I do not know of any reliable indication of secular gravity variation. For some years, there was controversy over apparent changes in the values of gravity at certain base stations. But it has been well established recently that these apparent changes were actually due to errors in the older measurements.

Lt. Murray—Just on that one point, the fact that the areas previously burdened by ice are still coming up shows there is a time lag in compensation, so there is also a secular variation in gravity going on there; similarly, there should

be a change every time a mountain comes out of the ocean. Thus, geologically there are a vast number of changes in gravity.

Mr. Rice—I did not wish to imply that secular changes could not occur, but accurate gravity measurements have not been made over a sufficiently long period to evaluate them.

Dr. Bobrovnikoff—What interval of time exists for which you have comparable data?

Mr. Rice—Something like 50 years, but from the data one could not detect gravity variations of one milligal, which incidentally would be a good-size secular change. The history of precise measurements does not go back 50 years.

Dr. Bobrovnikoff—I would like to ask about gravity at a height of 200 miles or so. It would be interesting to know what we could expect.

Mr. Whitten—In view of the shortness of time now and our program to come, may we defer that till then.

Dr. John C. Rose—I would like to comment on relative changes in elevation by gravity measurements. It is my belief that it has been only in the last ten years that milligal measurements have been reliable. This raises the question about absolute gravity determinations and their relation to sea level. I suggest that when geodesists two hundred years from now redetermine the shape of the geoid, it might be useful to have new absolute gravity determinations as absolute reference points for a geoid.

Ellipsoid Parameters from Satellite Data

JOHN A. O'KEEFE, NANCY G. ROMAN, BENJAMIN S. YAPLEE, AND ANN ECKELS

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I want to bring before you two pieces of research not done by me (O'Keefe) but both having a bearing on geodesy. The first was on the distance to the Moon. Curiously enough this seems to be the best way to determine the mass of the Earth. Nancy Roman and Ben Yaplee are here to discuss this. I will give an outline and they will show how it is done.

We know, from Kepler's law that $a^3/T^2 = \text{const}$ when a is the orbital semi major axis of the Moon and T is the period.

We substitute n , which is $2\pi/T$; and remember that Newton told us that the constant was

$$\text{const} = 4\pi^2 G (M + m) \quad (1)$$

when M is the Earth's mass and m is the Moon's mass. Then

$$a^3 n^2 = G(M + m) \quad (2)$$

We compare this with an imaginary satellite which skims just above the surface of a spherical Earth of radius a_\oplus

$$a_\oplus^3 n_\oplus^2 = G M \quad (3)$$

where n_\oplus is the satellite's mean motion. Dividing (3) by (2)

$$a_\oplus^3/a^3 = (n^2/n_\oplus^2)/(1 + m/M) \quad (4)$$

We know that a_\oplus/a is $\sin \pi$ where π is the Moon's parallax; m/M is usually designated μ ; and to find n_\oplus we know that, since the centrifugal acceleration must equal the acceleration of gravity, g

$$a_\oplus n_\oplus^2 = g \quad (5)$$

or

$$a_\oplus/g = 1/n_\oplus^2$$

Substituting in (4), we find

$$\sin^3 \pi = (n^2 a_\oplus/g)/(1 + \mu) \quad (6)$$

Corrections must be made to (6) to allow for the effects of the Earth's oblateness and the attraction of the Sun; but this is the essence of the

idea. The ratio μ has been determined by Rabe with an accuracy quite sufficient for these purposes.

If we measure a directly then we have

$$\sin \pi = a_\oplus/a$$

Thus we have two equations in the two unknowns, a_\oplus and π . It turns out that the other quantities are so well known that the measurement of the distance to the Moon is in fact a decisive method of determining the size of the Earth. To continue this subject, I would like to introduce Nancy Roman and Benjamin Yaplee.

Dr. Nancy G. Roman—Dr. O'Keefe has made it sound easy but there are many problems in the interpretation of our data. We have used a radar operating near 10 cm to send two-microsecond pulses to the Moon which we then receive as returned echoes.

Figure 1 shows a particularly good sample of the echoes. Even at best our signal to noise ratio is not very high. The base-line markers are separated by ten microseconds, equivalent to a distance of about one mile. If the echo were stable, the distance to its leading edge could be measured to a small fraction of this interval. We think we can measure the position of the first spike in the echo to a fifth of a mile. One of the problems is that we do not get the type of echo we should if the moon were a smooth sphere. In addition to deflections caused by noise, the positions of the peaks fluctuate as a result of constructive and destructive interference of echoes from individual areas. Hence we cannot look at an individual echo and determine the distance accurately. Instead, we have plotted the measured delay against time (see Fig. 2) during the course of one minute. During that minute the Moon is moving. Also during the minute some distances will be larger than others because of the fluctuations. Therefore, we have drawn a line through the minimum distances determined by these points, checked this line by comparing it with plots of the preceding and following minutes to be sure that the slopes are continuous and picked the central point on this line

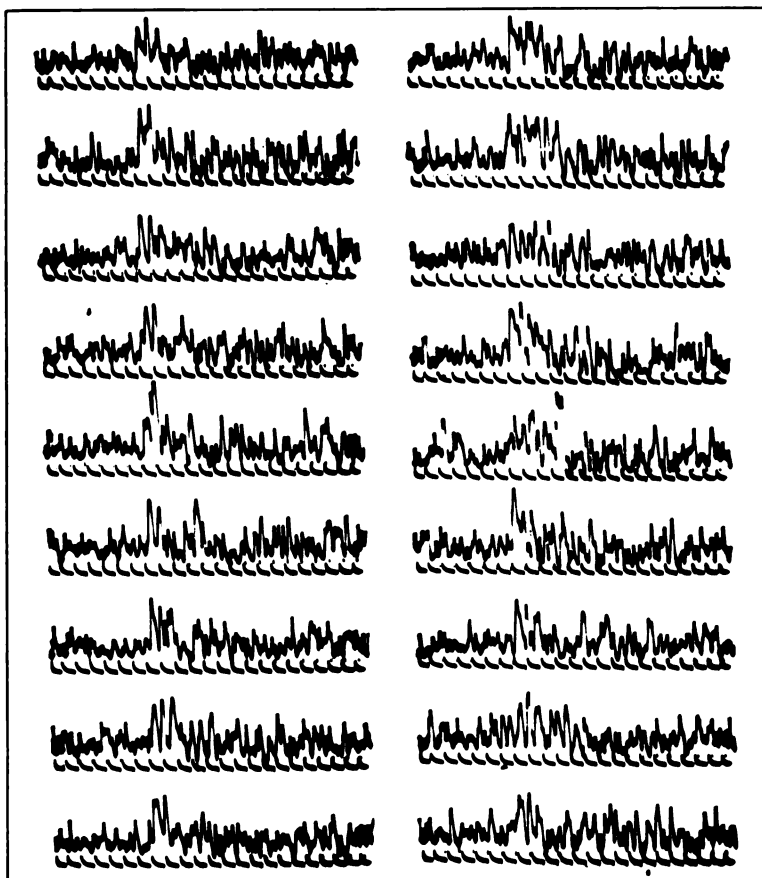


FIG. 1.—Sample of individual return echoes; range marks, 10 microseconds; trace separation, 4000 microseconds.

as the distance to the Moon at this particular time.

The distance to the Moon was computed astronomically (see Fig. 3) and compared with our measured distance. The residuals we have obtained during a month of observations are plotted in Figure 4. The straight lines represent the maximum scatter for an individual day of the residuals determined from the one-minute samples. There is a constant residual of about 45 seconds and, in addition, there is a variation in residuals of the order of 45 microseconds. This can be interpreted as a constant-distance error of about four miles.

The computation of the distance to the Moon is purely geometrical. We measure the distance from a point on the Earth to the nearest surface of the Moon. The problem is to express that distance, marked Δ in Figure 3, in terms of things we know or hope to know. One is, of course, the distance of the observer from the center of the

Earth. We also need, although it does not show on the diagram, the distance of the observer from the axis of the Earth, since we measure only astronomical latitude which we must reduce to geocentric latitude. From astronomical calculations we know the distance from the center of the Earth to the center of the Moon in terms of the radius of the Earth. This is expressed as $a/\sin \pi$ where π , the parallax, is determined by measurements at different points on the Earth's sphere. In order to determine the required equatorial parallax something about the shape of the Earth must again be assumed. Finally, since only the distance to the center of the Moon can be calculated, we must also know the radius of the Moon, which is added directly to the measured distance.

The first equation in Figure 3 expresses the cosine formula for the Earth-Moon-observer triangle. An investigation of the uncertain quantities in this equation proves instructive. In the

first term b the radius of the Moon is uncertain. Astronomically we know the radius of the Moon approximately but the effective radar radius is less certain. The first term on the right depends on the shape of the Earth. The second is the predominant distance term, the determination of which is the main objective of our investigation. It depends not only on the equatorial radius of the Earth but also on the parallax. The variation in π during the month is well known theoretically but the absolute value of this quantity is not as well known. The final term depends on gamma, the angular distance of the Moon from the meridian ϕ the observer's latitude and delta, the declination of the Moon. Since our measurements are basically time measurements, a value of the velocity of propagation of the radiation must be assumed to convert our measured times to distances. Figure 5 is a picture of the central part of the moon. Now our radar echoes indicate we are getting our reflections from a very small area on the Moon, although the signal should be hit-

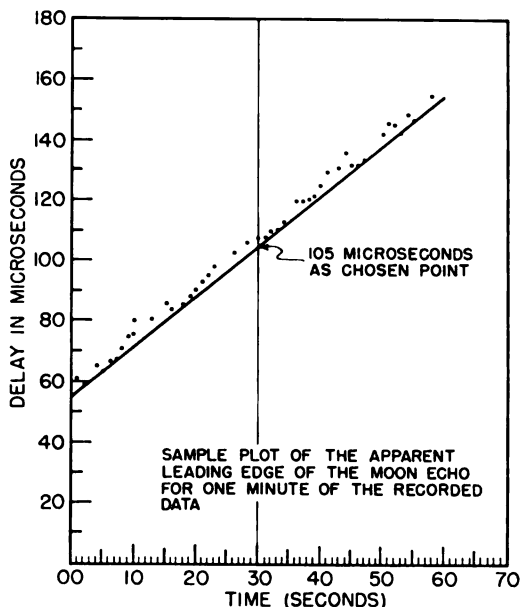


Fig. 2—Measured delay time as a function of time.

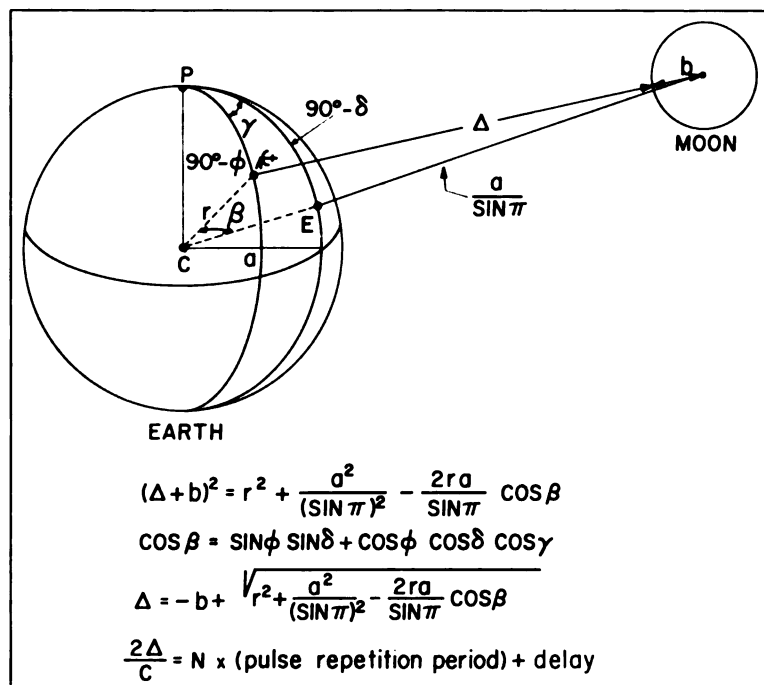


Fig. 3.—Geometry of Earth-Moon system

ting the whole Moon. The question is, are we getting a signal from a high mountain or a valley, or a crater, or just what is the nearest point of

the Moon as seen by our radar? Do we have to go out some distance to accumulate enough area to obtain the return in the background of noise?

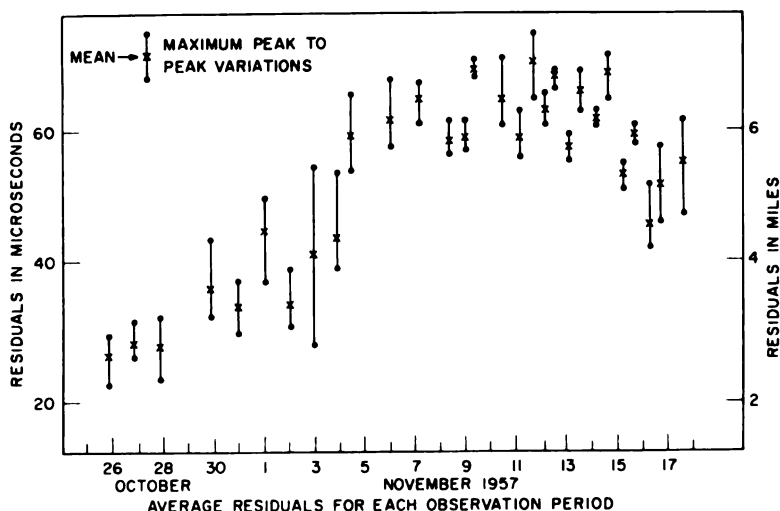


FIG. 4.—Daily residuals in distance

These problems lead to appreciable uncertainties in the interpretation of our data. Ultimately, I think the method has a good deal of potentiality and eventually we should be able to compare our results with O'Keefe's formula and derive some

interesting information. For the present we think the variations over the month look suspicious and interesting. The variations may be due partially to the fact that we do not know from what part of the Moon we are getting our echo.

Discussion

Dr. Roman K. C. Johns—It can be expected that you do not receive the echo from the entire surface of the Moon.

Mr. Benjamin S. Yaplee—It does not reflect as a rough surface, but more than that is needed to explain the residuals.

Dr. Johns—Have you any idea of what percentage of the lunar surface was reflected in the radar beam?

Mr. Yaplee—In terms of the radar cross section, it is an area of a thousand square miles.

Dr. Theodore E. Sterne—We do not know the length of the diameter of the Moon that points to the Earth. We know the angular size of the Moon well. It is nearly round as we look at it, but there is reason to believe that the axis that points to the Earth is longer than the other two. Unfortunately we have no good astronomical information about the distance from the near point of the Moon to its center of mass, that was the point considered in his orbital theory by Ernest W. Brown. We do have a value of C minus A , which is the difference between the moments of inertia about the spin axis of the

Moon and about the line of centers. But that in itself does not let one determine the difference between the radius of the Moon towards the Earth and any other radius. The trouble is that the high harmonic distortions can be important and there is no good way to determine them without looking at the moon from the side, which we can not do.

Dr. Heinrich K. Eichhorn—I wonder about the investigation that was done several decades ago at the Lick Observatory by getting a sort of relative parallax of points on the surface of the Moon with respect to the center of the Moon and different librations of the Moon? Was it Franz or somebody at Breslau where that was done? Their measurements have been newly reduced at Vienna Observatory by Schrutka. I do not know what the inaccuracies are but my feeling is that they are in the neighborhood of a couple of kilometers.

Dr. O'Keefe—All these errors are multiplied by the sine of the parallax in the determination of a , so if we are wrong by a mile in the radius of the Moon for example, it is about 1/60 mile



FIG. 5.—The central area of the Moon (photo from Mount Wilson and Palomar Observatories)

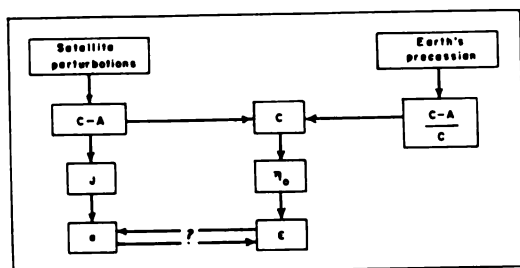


FIG. 6—Relationship of quantities involved in the flattening of the Earth.

in the radius of the Earth. But the other point is that one has an idea of what the possible error could be because of the librations of the Moon which are controlled by the radius of the Moon. Some 20 people have observed these librations, so that the radius cannot be uncertain except in the order of a kilometer. Hence this part is not really dangerous. Mösting A is a small crater on the surface of the Moon, used as a reference point.

Dr. Johns—And they get rather poor observations.

Dr. O'Keefe—Maybe a half a kilometer.

Dr. Johns—One of the difficulties in using the crater as a reference mark is the geometric definition of its center. In addition, any error in the position of the crater enters directly into the position determination.

Dr. O'Keefe—The problem is this. You have the distance of the Moon wrong in the neighborhood of a kilometer. The error here is going to lead to a proportional error in a . So if you have one part in three hundred thousand all these errors we are discussing are insignificant. They will build you up to about thirty meters in the size of the Earth. So I get 6,378,255 meters for the Earth's semi-major axis, taking into consideration all these errors. It is a number obtained by fools rushing in where angels fear to tread.

Dr. Roman—I want to emphasize why I do not want to put too much trust in this value. We can explain the monthly term in the residual or just the constant term but the results disagree. The problem is that things are just not consistent in the picture as it stands now.

Dr. O'Keefe—The most important thing is that we have here a method of very high consistency capable eventually of giving the size of the Earth in an unambiguous way within 25 meters.

Dr. Eichhorn—What does the calibration depend on?

Dr. O'Keefe—Speed of light. The usual number is 299,792.5 km/sec. I would like to have Ann Eckels discuss some results obtained on the flattening of the Earth.

Miss Ann Eckels—From the secular changes in the longitude of the node and the argument of the perigee, the coefficient J in the gravitational potential function

$$U = \frac{\mu}{r} \left[1 + J \left(\frac{a_e}{r} \right)^2 \left(\frac{1}{3} - \sin^2 \phi \right) + \frac{K}{30} \left(\frac{a_e}{r} \right)^4 (3 - 30 \sin^2 \phi + 35 \sin^4 \phi) \right]$$

was determined to be $(1.6232 \pm 0.0005) \times 10^{-3}$. If the bounding equipotential surface of the Earth is assumed to be an ellipsoid of revolution, this value of J implies a flattening f of $1/298.32 \pm 0.05$ [Lecar, Sorenson, and Eckels, 1959].

Dr. O'Keefe—The relation of the various geophysical quantities involved in the flattening of the Earth is shown in Figure 6. C is the moment of inertia of the Earth about a polar axis; A is its moment about an axis in the plane of the Earth's equator.

Before the time of the satellites the most useful piece of observational data was the Earth's precession, from which the quantity $(C - A)/C$ can be deduced. This quantity is called the dynamical flattening and has a value very close to $1/305.6$.

Starting from the dynamical flattening, one proceeded in those hoary times, to guess a value of C . The expected value of the ellipticity ϵ of the Earth, is directly deducible from C . If the Earth is in hydrostatic equilibrium, then ϵ should be equal to e , the actual value of the ellipticity of the Earth. From e , one can deduce the value of J in an unambiguous way and so back via $C - A$ to C again. If the proper value of C was chosen, this loop will close, otherwise not. It was in this way that the accepted value of about $1/297.3$ was deduced for the Earth by many. In practice the business of successive approximations is bypassed by direct formulas; but this is the idea of the computation.

Now the reference cited above, also King-Hele in England, Jacchia at Harvard, Bechar in Czechoslovakia, and Hertz and Marchant at the Army Map Service give measurements of the flattening. They find that $C - A$ does not have the predicted value. From this, it follows unambiguously that the actual external flattening of the Earth is not $1/297.3$ but $1/298.3$.

This undoubtedly means serious deviations from hydrostatic equilibrium. It means, in effect, that in broad lines isostasy does not hold; that there are major, large-scale deviations from isostasy.

This work implies the existence of considerable stress differences in the Earth's interior. These stress differences may be supported by internal mechanical strength. In principle they might also be supported by convection currents. But the physical parameters of the problems are

changed by this work from these which have hitherto been used; and it remains to be seen whether the convection current theory can be adapted to the new data.

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Geodesy and Space, Introduction

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A truly remarkable event happened about a year ago when the first satellite went into orbit. It initiated a new potential, as a number of us had expected, for geodetic research concerning the surface of the Earth and, indeed, the interior of the Earth as well.

Satellites, however, do not have the property of undersea measurement, nor, in fact, can one be put into the sky and held there. So the methodology by rockets and satellites will be different than the older geodetic methods. Nevertheless the potential is very high and we have a panel here this morning who will discuss it. It is not set up as a debate. There are questions whether geodesy could not be done better with natural satellites than artificial only. I hope that during the discussion this point will be taken up to see whether the artificial satellite is indeed unique in its properties to determine geodetic quantities.

Rocketry

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Instead of calling this rocketry it should be called a discussion of what not to use in geodesy. It has been much speculated that one might use rockets to improve our knowledge of the size and shape of the Earth, to improve intercontinental ties, and so forth. I would like to point out why I think this is an unlikely event. The speakers later will probably get to more sophisticated vehicles, satellites and so forth, and show on the other hand how they may be useful. The vehicles I am talking about are vehicles that leave the Earth and re-enter without making a complete revolution around the Earth's surface. The main reason a rocket cannot be useful in geodesy is that a rocket spends very little time in orbit outside the Earth's atmosphere. A rocket may travel about a quarter of the way around the Earth during which time you have very little chance to make observations while it is unperturbed by the atmosphere. Another very good reason why rockets will not be used for geodesy is that there are a very limited number of places from which to launch them and a very limited set of azimuths into which to launch them. It is quite obvious that there are still objections to launching a rocket from, say, the East Coast to the West Coast. A typical situation today for a rocket used geodetically would be that it is launched from a sea coast traveling a large distance over water to an inaccessible land mass. The orbit determination would result entirely from observations near the launch point and would therefore suffer from any inaccuracies made initially.

Consider a practical problem. Suppose one wished to tie Pitcairn Island to the North American Datum. Pitcairn Island is difficult to get to by the usual methods of geodesy and one could logically propose a rocket flight from the West Coast of the United States to somewhere in the vicinity of Pitcairn Island to do the job. The theory of the problem is simple. Describe the properties of the North American Datum as a rectangular coordinate system. The rocket takes off from somewhere in this XYZ system with some known velocity and radius vector from the

origin, traveling along some known orbit and impacting the surface of the Earth at Pitcairn Island. Knowing the orbit conditions leaving the Earth's atmosphere and knowing the gravity field in space, one then can predict where the rocket will impact the datum surface again. Observing where the rocket actually does impact the surface allows a tie of the points of impact and departure. This sounds simple but we shall see it is rather difficult to do in practice.

One can write the ordinary equation for an ellipse as

$$r = \frac{J^2}{GM(1 - e \cos \theta)} \quad (1)$$

where J = angular momentum, e the eccentricity, and θ is measured from apogee. It is useful to rewrite this equation as

$$\frac{r_0}{r_t} = \frac{(1 - \cos \phi)}{\gamma \sin^2 \beta_0} + \frac{\sin (\beta_0 - \phi)}{\sin \beta_0} \quad (2)$$

In this equation we have burnout velocity v_0 , the flight path angle β_0 , the geocentric radius at burnout, r_0 , the angle from burnout to impact measured at the center of the earth ϕ . The quantity $\gamma = r_0 v_0^2/GM$ contains all the dynamic variables of the problem, and GM of the Earth. The radius where the rocket intercepts the Earth's atmosphere again is r_t . This is Eq. (1) for an ellipse rewritten in more convenient form.

You can now use this equation to determine how accurately various quantities have to be known in order to know where Pitcairn Island is in relation to the North American Datum. Letting q_i be any of one of the variables in Eq. (2) then

$$\Delta L_{q_i} = a \left(\frac{\partial \phi}{\partial q_i} \right) \Delta q_i$$

is the error measured in the plane of the trajectory due to an uncertainty Δq_i in q_i . For instance if $q_i = v_0$, the initial velocity then

$$\Delta L_{v_0} = \frac{2a\Delta v_0}{v_0} \frac{(1 - \cos \phi)}{\sin \phi - \lambda \sin \beta \cos (\beta - \phi)} \quad (3)$$

If one puts typical numbers into this expression for a vehicle with range sufficient to travel from the West Coast to Pitcairn Island with reasonable velocity magnitude and angle at burn-out, the result will be that the velocity vector must be controlled to one foot per second in order to predict where the vehicle will land within one mile. Suppose first order accuracy were required, say ± 300 foot uncertainty, then one would have to control the uncertainty in the velocity vector to about five parts in 10^6 at burn-out. This is obviously a rather difficult thing to do.

There are other uncertainties that are important as well. The quantity GM for the Earth is probably known to five parts in 10^6 . Accuracy even of this order would still leave about an 800 ft uncertainty in impact location.

To make the situation more discouraging, it should be pointed out that this is an equation for an ellipse in a central field. Suppose we take into account that gravity anomalies exist along the path that the vehicle is traveling. The anomalous gravity potential U can be expanded in a harmonic series as follows

$$U = \sum_{n=2}^{\infty} \frac{\Delta g_n^m a^{n+2}}{(n+1)r^{n+1}} [p_n^m(\theta, \phi)]$$

where a = equatorial radius, θ and ϕ are latitude and longitude respectively, and g_n^m is the amplitude of the m, n th harmonic. Suppose there exists a longitudinally dependent term p_n^m ; then if g_n^m had a magnitude of ten milligals it would give an uncertainty in impact location of around a thousand feet. Evidently to attain a first order tie we would have to know the gravity field better than it is known now.

Discussion

Dr. Fred L. Whipple—Well, about putting a rocket up over the United States; now that Alaska is in the Union, I understand that the center of gravity of the United States is in Canada. Is that true?

We have here a rather unusual situation in which the protagonist tears down the method. I wonder if anyone would like to defend rocketry from the point of view that the rocket should be capable of putting a marker up in space that could be observed by optical, radio, and possibly other techniques, for triangulation purposes.

Suppose an uncertainty in the flattening of the Earth exists of the order of 1 part in the last unit. This uncertainty leads to a term in the gravitational field as well as a geoidal height uncertainty. These would lead to an uncertainty of 1500 feet in the impact. I conclude that these factors are enough to demonstrate that there probably would be a total uncertainty in the tie obtained this way from these uncertainties alone on the order of a half to three-quarters of a mile.

There are additional problems that I have not talked about. One of which is the problem of finding out where the vehicle would have landed were there no atmosphere. Unfortunately the vehicle does not come in on a vacuum trajectory, but enters through an atmosphere which is a nonstandard, fluctuating phenomenon. It can be predicted how a vehicle would be affected by wind and density fluctuations. The dispersion can be the order of miles, depending on the shape of the vehicle, so that there is another sizeable uncertainty to be added to the list.

As a result of considerations of this sort it seems unlikely that rockets will ever be added to the list of geodetic equipment, particularly since we have satellites and more interesting types of vehicles available now. On the other hand, the interaction of geodesy and rockets in the other direction is quite strong. Rocket designers have been asking questions of geodesy in the past few years that geodesy has not always been able to answer; these therefore have stimulated many programs. Geodetic programs that would ordinarily take generations have been accomplished in a few years and the acceleration may continue for years to come.

Dr. Raymond H. Wilson—You have to have rocketry before you have satellites.

Dr. Charles A. Lundquist—I know of at least a couple of cases where rocketry was used for geodetic purposes. There was one that Hellmut Schmid might comment on better than I. There arose a question of the location of some of the down range islands at Cape Canaveral and some rocket-borne photo flash cartridges were used, I understand, to effect a tie between the down range islands and the mainland.

Dr. Whipple—I might say the method was

used in another case where a rocket put a marker in space.

Dr. Hellmut Schmid—Well, obviously it was unimportant.

Dr. Whipple—The rocket merely carries the flash.

Dr. Schmid—In other words, we have the same accuracy that we have in measuring the trajectory in the first place. Its focal length is up to 300 millimeters. It is in the neighborhood of 1/200,000. There is a wide field of 40° which allows us to fit star observations to refraction expressions. It is a verification of the tie in.

Dr. George Veis—I wonder why rockets would not be able to give any geodetic information. Rockets have the advantage that we can launch them when and where we want. I am not thinking of Cape Canaveral but rockets to be launched from ships. I do not see why they can not be used. The question of accurate timing would not be so important as for satellites. The principle will be similar to the one suggested by Vaisala 12 years ago.

Dr. Mickelwait—What distance are you talking, thousands of miles or hundreds?

Dr. Veis—Not thousands, hundreds.

Dr. Mickelwait—That is a different story. I was really talking thousands of miles.

Dr. Veis—There are a lot of connections, not thousands of miles, that could be made by rockets. For example the connection between Greece and Africa.

Dr. Mickelwait—That can usually be done in other ways.

Dr. Veis—There is a shoran connection but this is different. I think a number of rockets could give a solution.

Dr. Whipple—Does anybody have this answer in mind as to why a rocket shot up over the Atlantic high enough and observed with considerable precision at a number of stations on both sides could not provide triangulation enough to tie the European network and North America. You may wish to use it for smaller distances. That, of course, is where it has already been used with the function of sending up a marker.

Rear Admiral Charles Pierce—There are systems today like Loran C that will give accuracy to 1500 miles. There are systems today for short ranges that are reasonably expensive. So there are systems to take out to more than a thousand miles already developed.

Dr. Veis—Yes, but this is not on the same principle. They do not give the space solution but a surface solution. They measure the distance.

Rear Admiral Pierce—Mickelwait was talking about existing ties to datum in the Earth with systems that already exist.

Dr. Veis—The connection between Europe and Africa as made by shoran does not give the elevations.

Rear Admiral Pierce—It gives potential.

Dr. Veis—This is not a complete geodetic solution.

Dr. Roman K. C. Johns—Speaking about rockets being used for establishing intercontinental geodetic ties, what are your estimates, Dr. Whipple, of the duration of the rocket's visibility? I imagine that the rocket will be sent 400 to 500 miles high.

Dr. Mickelwait—That probably would be about 500 miles. You would probably want to see it from the launching point and the impact point. You probably would not see it over ten minutes simultaneously at the most. During that time there would be probably five minutes for observations.

Dr. Whipple—You could have any number of flashes.

Dr. Mickelwait—There are problems with flashes.

Dr. Heinrich K. Eichhorn—It appears to me that maybe the problem of the refraction has a great complicating influence on that. As far as I know the refraction theory that is used today by the people at the Naval Observatory goes back to 1860. As far as I am acquainted with the literature the constitution of the atmosphere, that is vital for establishing the refraction theory free from hypothesis, has not been sufficiently investigated. I do not know whether the modern observations of, say, the temperature gradient with altitude, have been incorporated into the refraction theory. I do not think that is the case.

On the other hand, if the constitution of the atmosphere were sufficiently known and could be kept sufficiently under control, then it seems if there were position observation made to different rockets from supposed three equal points somewhere in space, that from those position observations on, say, two different rockets, a unique solution could be made if at least there was a decent refraction theory and if the eleva-

tion was high enough so that refraction was sufficiently harmless.

Dr. Whipple—You are worried about the refraction and comparison with the star background, which is usually known better than the secondary arc. There is a correction which Jacchia has used in his reduction of double meteor stations. Not all of you are familiar with it.

Dr. Luigi G. Jacchia—We call the effect 'parallactic refraction.' It may amount to a few seconds of arc for objects at meteor heights when they are observed near the horizon.

Dr. Whipple—If you carry the refracted ray path back you miss the station a little. The correction should not be very large.

Dr. Jacchia—No, but it would presumably become important enough for an object observed at very low elevations as would be the case with intercontinental ties. This effect may reach something like two seconds of arc or so.

Dr. Eichhorn—If one puts a marker up in the middle of the Atlantic I do not know how high one could get it but if one could observe at 20° altitude on any meridian, one would shrink if he had to measure a declination at 20° altitude. I do not know how the refraction would be kept under control.

Dr. Jacchia—That is just it. I think the error caused by the imperfect knowledge of the constitution of the atmosphere, could be of the same order of magnitude. It seems to me if one wanted to apply refraction correctly one should at the same time have a temperature profile along the line of sight.

Dr. Johns—It is customary in geodetic work to minimize the atmospheric effects by randomizing it. The atmospheric errors can be randomized by increasing the number of data, by extending the observations over a number of experiments and by taking continuous pictures of individual firings.

Dr. John A. O'Keefe—I do not agree with Jacchia. I think you will find that what you have to do to take care of this situation is to express the apparent height of the observation station as a fictitious height. I think you will find that the height connection depends on the integral of the density of the atmosphere which is measured by the barometric pressure at the station. This is the so-called refractive height, an added correction which will eliminate the problem.

Dr. Eichhorn—In order to have an hypothesis-free theory of refraction you have to know

something about the density gradient or some other relationship about the atmosphere. Presently, the refraction must be computed from the conditions on the ground without any further safe knowledge on either temperature or density gradient.

Dr. O'Keefe—It is true that you need to know the density gradient when you take the curvature of the Earth into account but it is sometimes surprising how far from the zenith it will work without this complication.

Dr. Johns—We may observe fairly close to the horizon, however under such circumstances considerable effect of refraction can be expected.

Dr. Whipple—Of course this is a second order effect, not first order.

Dr. Veis—The residual refraction is going to be less than two seconds of arc for zenith distances up to 75°. Now if we are 100% off in our theory, the correction by this method would not be more than two seconds of arc which is in the order of the accuracy of our observations.

Dr. Whipple—I think the correction is small if the stars are used as a background. If you come down to 2° of altitude you will not see the stars anyhow.

Dr. Don A. Lautman—One other point concerning the use of rockets is the fact that you do not have to control the velocity within a fraction of a foot per second if you can observe what it is at launching. So if the rocket were launched and could be observed over a reasonable period of time from a number of stations to determine a good orbit, then if it went half way around the world to a point to tie in you would have the orbit determined and could observe whether it was a mile or two miles.

Dr. Whipple—Can we call this dynamic trigonometry? I think we should have a name for it because it will come up again. Kinematic trigonometry, that is a good name.

Dr. Lundquist—In the lunar problem calculations have you investigated how accurately you need to know the orbit of the probe on a near pass to the Moon to start to study lunar geodesy?

Dr. Mickelwait—Yes, we calculated and decided to ignore the whole thing. The big problem is visibility from the Earth.

Dr. Lundquist—Divorced from any practical case in hand, at the right time of the year could we use rockets then for the beginning of the study of lunar geodesy?

Dr. Mickelwait—Yes, if you can solve the problem of visibility in the vicinity of the Moon which is a big problem. It is feasible to control the orbit to get close enough so one could make a slightly better determination, say, of the

Moon's mass. I think, however, a better way to do it would be put a satellite in orbit around the Moon to make continuous observations.

Dr. Whipple—Dr. Sterne makes a comment aside that this is selinodesy.

Satellites

J. ALLEN HYNEK

Smithsonian Astrophysical Observatory, Cambridge 38, Massachusetts

Perhaps, as Mickelwait has said, rockets may not help geodesy much but the long-playing rockets certainly will. When the Smithsonian Astrophysical Observatory went into satellite tracking it was with the avowed belief and intension that satellites would be eventually of prime importance to geodesy and astronomy. The program was set up with this very much in view, not merely as a service organization to be, you might say, 'train dispatchers' for satellites, but to develop a technique that would eventually be useful to both geodesy and astronomy. This program was regarded as an astronomy program even if the IGY satellite got up only into the upper regions where the atmospheric drag would be a serious factor.

The present satellites have perigees that are relatively very close to the Earth (Fig. 1) and hence low in the atmosphere, and, as is familiar to most of you, the net effect is to decrease the eccentricity of the orbit as the satellite goes along, which decrease can be calculated by methods well worked out and published by Sterne and others.

Figure 2 shows this effect. The apogee distance is constantly reduced but not the perigee distance. The net result is a slowly decreasing eccentricity until the satellite spirals in.

I would like to describe the first part of the program of the Smithsonian because I have a feeling that some of you are not aware of what the program is doing. It has concerned itself with satellites of the nongeodetic and nonastronomical kind.

None the less the program was devised to handle the latter kind also. Figure 3 shows the Baker-Nunn-Schmidt telescope designed by Baker and Nunn according to specifications originally set down by Whipple and myself. Note the tri-axial mount of the camera. The fork is to handle azimuth and the gimbal to handle altitude. Then through a third axis the camera is made to track in any direction. The evolution from the cardboard model to the final camera took about a year.

Since July 5, 1958, there have been twelve

of these Baker-Nunn-Schmidt cameras in operation in the network around the world. My purpose here is not to discuss the manner of operation of the camera. If there are any questions later I will be happy to go into that, but mainly I aim to demonstrate what has been done so far and what accuracies have been obtained, and go from that point to show how the program is set up to handle geodetic and astronomical satellites.

Before we leave this, however, if you are not thoroughly familiar with optics, this camera employs a Schmidt system: a spherical mirror (in the base) and a corrector lens element placed at the center of curvature of the spherical mirror. In this case there are three correcting plates, four of the surfaces being aspherical. The focal surface is spherical and the focal length is 20 inches. This gives a scale of $2\frac{1}{2}$ microns to one second in the sky. The telescope drives are such that the camera can be used either as a stationary camera (if the satellite itself is bright enough it 'photographs itself' as it goes along with appropriate chopping by a shutter to provide timing works), or as a tracking camera in which case the camera is made to track the satellite directly, and one merely gets trails of the brighter stars. Or, as designed for its intended use, it has a combination of two rates which, when added, equal the mean satellite rate, and when vectorially subtracted, gives the approximate sidereal rate. The camera follows the satellite, the stars appearing as trails and then on the same film the stars are also photographed as point images. Since a Schmidt system has a focal surface and changing a film would be quite difficult, we have resorted to using a strip film which conforms to a portion of the spherical surface. Also, as the film goes through, when the chopping shutter arrives at a critical point there is a stroblight which is used to photograph a slave clock in the camera which can be read to 0.0001 second.

Figure 4 shows the locations of the stations. The locations have been chosen largely in the good-weather zone around the Earth. This does

not impair their use even for a polar orbit. There is a station at Palm Beach and White Sands; one in a crater in Hawaii; and one each in Peru, Argentina, Spain, South Africa, India, Japan, Australia, Curacao, and Iran.

These stations have been in operation since July 1958, photographing the American satellites primarily. Since July 5, for instance, 1958 α has been photographed at 2.5 transits a day with multiple photographs. The Army Explorer 1958 ϵ has been photographed at 0.81 transits a day. It was largely through these photographs and their reductions that information was furnished to ABMA and through them to James Van Allen.

The stations are essentially identical except for the local color. There is always a sliding roof, a small house, and the camera set up in the manner shown.

Figure 5 shows 1958 α which was about tenth magnitude at the time. The manner of operation is clear. At the time that 1958 α was photographed the camera was in the tracking phase of its cycle. Consequently the satellite was photographed as a point but the brighter stars appear as trails, and the central break was timed to a thousandth of a second. Now, on the same strip of film, as the gross shutter closed for a fraction of a second to reopen when the camera had taken on its stellar rate, the stars appear as point images. With each bright trail goes a bright star image, so those stars too faint to trail before now appear as point images. The camera takes pictures easily to the twelfth magnitude in a one-second exposure. This is precisely why the camera was designed in this way.

The reduction process comes in relating the measured position of the satellite here with a time-displaced position of the break in the brighter stars. You may be interested in some of the accuracies. Henize, who is not here, is responsible for the estimated probable errors and finds that if all the photographs so far reduced are lumped together, the accuracy in the right ascension direction is 5.7 seconds and in declination is 4.0 seconds. When thirteen images of best quality were measured some time ago, Henize derived the following error: In right ascension 1.1 second of arc and in declination 0.5 second of arc.

I feel this represents internal accuracy rather than overall accuracy because of systematic

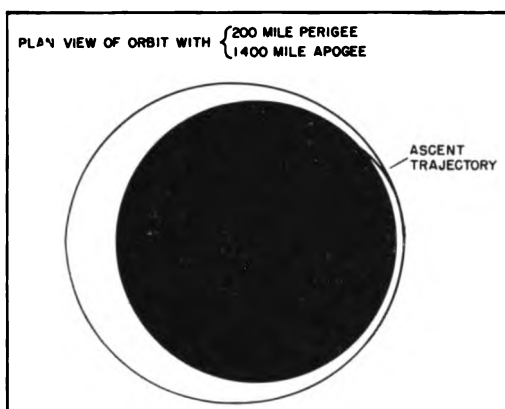


FIG. 1—Typical satellite orbit

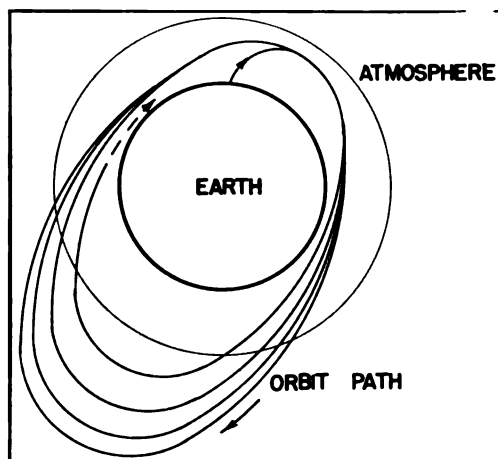


FIG. 2—Decreasing semi-major axis as result of atmospheric drag.

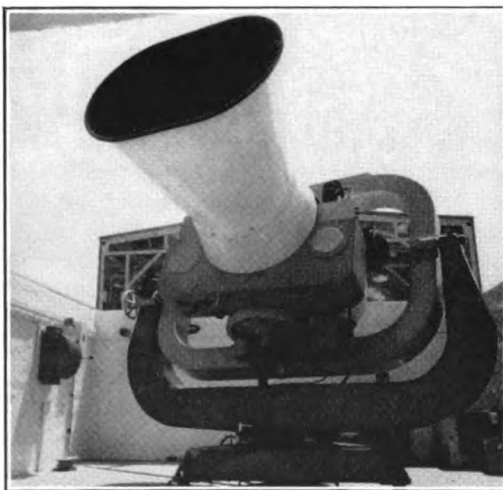


FIG. 3—Baker-Nunn satellite-tracking camera

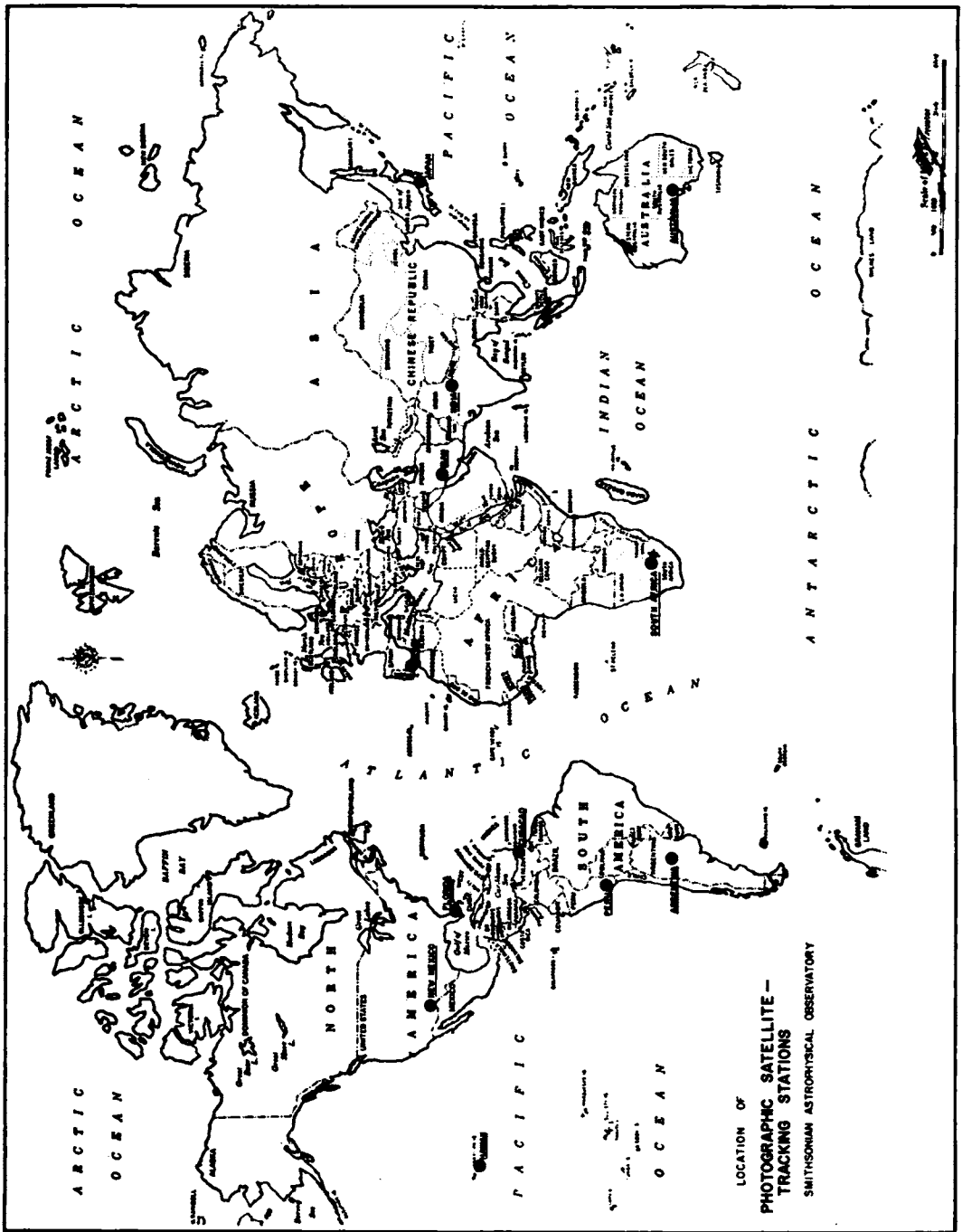


Fig. 4—Smithsonian Astrophysical Observatory network of photographic satellite-tracking stations

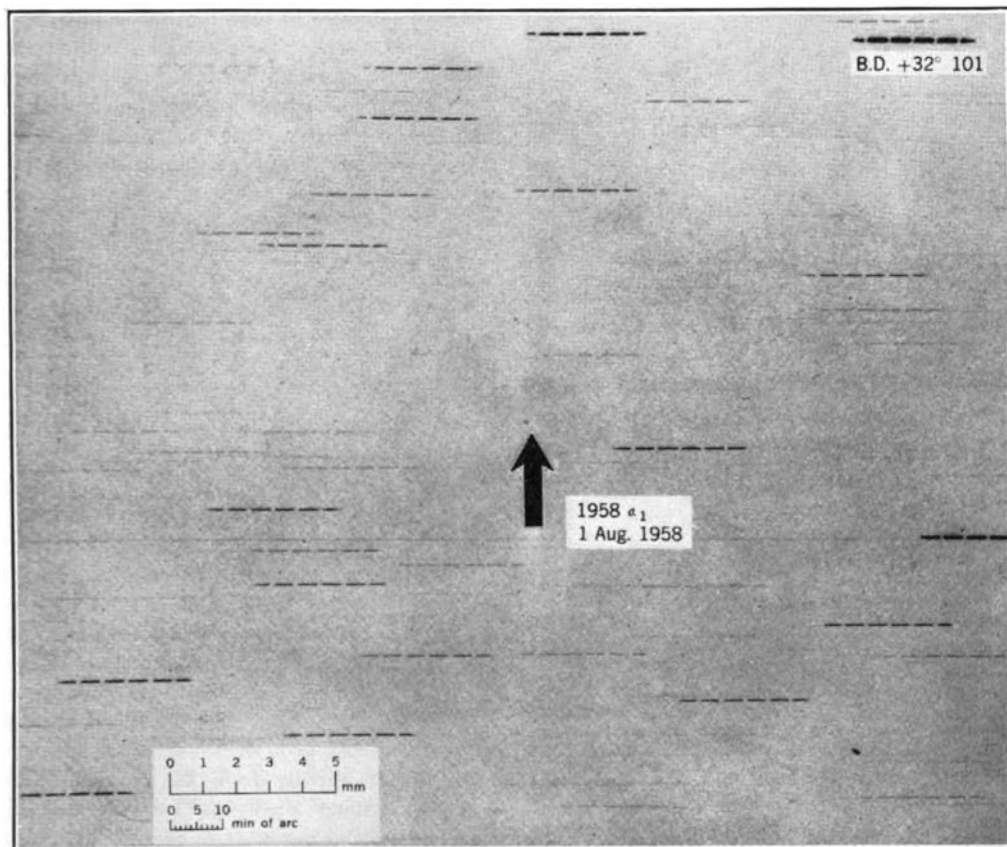


FIG. 5—Baker-Nunn satellite-tracking camera photograph of artificial satellite 1958 α .

errors. One error in particular is the error in the proper motions of stars in the southern hemisphere, where proper motion in many cases is not accurately known; likewise, there is the error introduced by transmission of time signals. I think we will have to solve that problem by eventually having a transmitter, and transponder at each station.

Figure 6 shows the time presentation put on each film strip which gives the minute, second, and hundredths of a second. The pip here whips around a hundred times a second and can be read to a ten thousandth of a second. Each of these markers is one ten thousandth. The timing has turned out not without troubles, of course, but actually better than had been expected. The crystal clocks are working in the field, the great majority of them with very excellent rates.

Up to now I think we have to admit there has not been a geodetic satellite aloft although, as O'Keefe and others showed earlier, satellites

have been able to yield the ellipticity of the Earth better than before. I would call one a geodetic satellite if it has a perigee height greater than 400 miles and less than 1000 miles, and an apogee greater than 1000 miles. The ideal apogee would be about one earth radius.

The function of a geodetic satellite is, of course, to connect geodetic networks and to check densities of distribution of land masses. As to the functions of the astronomical satellite, this would act as a fundamental astronomical reference point. If the satellite is completely above the atmosphere, an ephemeris can be calculated a long time in advance. In fact, I can visualize the nautical almanac having sections on artificial astronomical satellites. Departures from predicted positions would lead to detailed knowledge of the structure of the Earth's gravitational field. Through such astronomical reference points one could obtain better values of some astronomical constants.

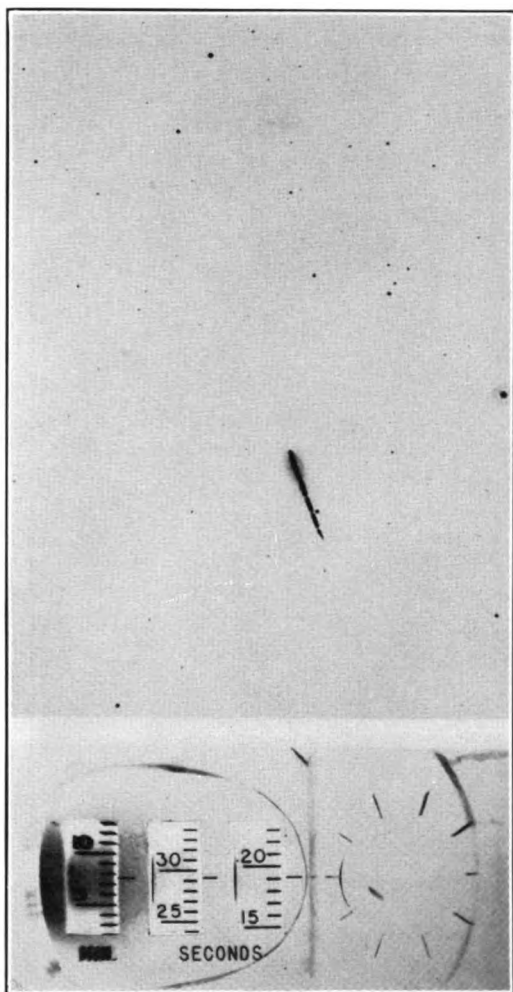


FIG. 6—Baker-Nunn photograph of 195881 taken with camera stationary; the time record imprinted on each frame of camera film is illustrated.

Now of course we can remind ourselves of our own natural satellite, the Moon, but the situation would be improved if the Creator had made it somewhat closer, somewhat less bright, and if it had been a specular reflector instead of diffuse. We could then dispense with highly specialized cameras; both stars and satellites would be point sources and could be photographed very conveniently. Of course, the point is that as we do not have such a satellite we should correct this and put one up ourselves. What are the specifications? They are obvious but some points need study. The Baker-Nunn cameras can be used only in the twilight periods and while accumulated observations over the

years of satellites of long lifetime will serve the purpose of geodesy, the job could be done more expeditiously if satellites carried their own lights and could be observed at any time during the night. So it becomes clear that a satellite carrying its own light source would greatly decrease the time needed to get significant geodetic results.

If a flashing satellite is observed simultaneously from several stations, the time of observation is virtually eliminated. The problems we must consider here are how bright a flash, what frequency, and, for the nonsimultaneous observations, how to time the flashes.

Now Whitney and Veis have looked into these problems in considerable detail with specific reference to what we call here a geodetic satellite as contrasted to the astronomical satellite. They tried to effect a compromise in what the geodesist would like and what the engineer can put up in a year or so. They have kindly allowed me to refer to their paper. They are considering a 100-pound object for the near future.

In the case of the geometric method of observing, the satellite is caused to flash its light either by means of a clock carried on board or by a trigger impulse from the ground. In this case the timing does not have to be very accurate because all that is necessary is to insure the stations are observing the same flash. They have suggested that instead of employing evenly spaced flashes, they be in bursts covering a few seconds time followed by an interval of perhaps a minute and a half before the next set of bursts is triggered.

Another method that they have looked into might be called the orbital. Here the flash is observed in one geodetic net at a time, and the separate observations are connected with each other through dynamic theory. They point out that in the use of this method, because of perturbations even at these heights, the observations should be made within one or two revolutions around the Earth.

Their major effort has been placed on the consideration of what type of flashes, how bright and powerful, etc.

Now the main problem here, I think you can see, is not only the brightness of the flash but the timing of the flash. They have suggested the flash can be timed or triggered from the Earth at which time a corresponding radio pip from the satellite could be also sent to Earth

to assist in the timing. There is, of course, also the possibility, which they have not yet considered seriously, of having a photo-electric method of timing the receipt of the flash. This certainly has an advantage over the receipt of a radio pip in that the profile of the photo-electric receipt of the flash would indicate how the image on the plate had been built up, or whether the intensity peak was to the left or to the right of the time marking. The radio mark would not do this. The obvious difficulty in receipt of a flash from the satellite is that the camera must distinguish between the stars and the flashing satellite. This might be overcome by having a monochromatic source and having the photo-electric system which parallels the

camera use a monochromatic filter which virtually eliminates the background sky and star light and concentrates on the monochromatic flash from the satellite.

I would like to conclude with the statement that I feel that this particular topic might open the discussion. Perhaps the acquisition of a faint flashing satellite might be discussed.

Am I correct, Dr. Whitney, that your calculations indicate a 9th or 10th magnitude object? This would open up the problem of acquisition of such an object and this would lead me to urge a revised system of super MOONWATCH teams, relatively few in number but equipped with visual telescopes to pick up 11 and 12 magnitude objects.

Discussion

Dr. Charles A. Whitney—Yes, the flash energy we suggested would be about equivalent to a tenth magnitude flash of one-second duration. The flash itself would persist for about one milli-second and the eye's resolving power is on the order of one-twentieth of a second. I think that tenth magnitude is about right for flashes.

Dr. Hynek—Have you looked into this physiological system?

Dr. Whitney—No.

Dr. Hynek—I was worried about the problem of acquisition here.

In my own opinion both geodetic satellites and astronomical satellites are needed and are entirely feasible. A great problem is the lifetime of the power source. The real push must be to the direct use of solar power which is still in the embryonic stage.

Dr. Raymond H. Wilson—I carried out some actual tests on the reduction of the magnitude of stars with the duration of flash length and published in *Science* about a month ago. The result is for flashes as short as 0.0002 second you lose only about one magnitude. You say tenth magnitudes at about one second duration. You could multiply the brightness by the 500 which would be around the sixth magnitude.

Dr. Fred L. Whipple—Suppose that is for a milli-second, then would it be 500 times brighter to the eye?

Dr. Wilson—You have a total energy of the flash. You have a 50-watt second of energy.

You cut down to a millisecond and you have a thousand times of brightness.

Dr. Whipple—A single flash would show up relatively brighter to the eye?

Dr. Wilson—It is reduced about one magnitude by that short duration. I measured it by having a disc of a certain size and it gave that effect of looking at stars. If you see a star for only 0.001 second its magnitude is cut down by about one.

Dr. Hynek—In that case you are not really concentrating one second of star light into 0.01 second of eye visibility. This is different from bunching the photons into a more concentrated flash. I am not entirely convinced that the psychological reaction would be the same. It is that point that is worrying me.

Dr. Wilson—It would depend, as I looked at them, whether the flashes would be separate or merged. Well, they were not merged. That is definite. The flash rate was around three per second which would not be merged. They merged only at around ten per second.

Dr. Robert Newton—Would you repeat the frequency of observations you got with the telescope?

Dr. Hynek—A little more than two transits a day for the 1958 α and 0.81 for 1958 β . That is for the network. However, that means multiple photographs for each transit; that is, one transit yields many photographs.

Dr. George Veis—It has been suggested generally that it would be better for a geodetic

satellite to have a circular orbit. However, we came to the conclusion that an eccentric orbit would be better. If we are limited to what would be the minimum height for the satellite it is better to have a satellite to go from 400 miles, let us say, up to 1000 miles so we would get better solutions for the geometric connection of the different nets, always selecting the strongest solution for each connection. On the other hand, using an inclination of 55° as proposed, there is going to be a revolution of perigee in about three months, so practically speaking it will go over all kinds of area of the Earth.

Mr. W. J. O'Sullivan—We have already succeeded in developing a sphere, one of the spheres as Hynek mentioned, that is 100 feet in diameter and the package weighs probably 80 to 90 pounds. I made a few estimates of the lifetime this would have in orbit. At an orbit height of a thousand miles it would have a lifetime of approximately 1.5 years because of the low mass ratio. However, if we raise the altitude to around 1500 miles the lifetime would be approximately 100 years.

So I would like to ask Dr. Hynek if he would consider a lifetime, say, somewhere in the neighborhood of 50 to 100 years as adequate for the stability of the satellite for geodetic purposes. Does that approximate what would be needed?

Dr. Hynek—Whipple and I were wondering what lifetime you mentioned for a thousand miles?

Mr. O'Sullivan—Approximately 1.5 years.

Dr. Whipple—This is a balloon satellite?

Mr. O'Sullivan—A diameter of 100 feet and a weight of 70 pounds.

Dr. Whipple—Whitney has been working on this.

Dr. Charles A. Whitney—I would say 50 years is a reasonable lower limit on lifetime to give the stability needed for geodetic work.

Dr. Whipple—As the orbit changes rapidly kinematic trigonometry would be difficult.

Dr. Luigi G. Jacchia—We have a satellite with that lifetime, the Vanguard. It all depends on what is required. The orbital acceleration varies by a factor of four. If you had to distribute your observations over only a few revolutions you would get into trouble. So it seems for the geodetic satellite that heights of around 400 miles are a little low.

Dr. Theodore E. Sterne—I did not quite agree

with Hynek's belief that an artificial Earth satellite could be a substitute for Eros. On the other hand, I was reminded of the discussion earlier in which O'Keefe brought out that it should be possible to obtain an improved size of the Earth from observations of the Moon. In that, there are difficulties. You do not know what part of the Moon is reflecting, the axis of the Moon not being exactly known. Can you not perhaps get an improved size of the Earth from artificial satellites that will not depend on such uncertainties as the size of the Moon, or what part is reflecting, and on the perhaps unknown radio speed? You can do it all optically with satellites. Careful reductions and studies of residuals from very accurate satellite cameras can be related to the size and surface gravity of the Earth. I wonder how that method would compare with lunar radio reflection.

Dr. John A. O'Keefe—I looked into the question that Sterne is discussing. I think it is a very good thing. It has an advantage over the Moon, beside the ones he mentioned, namely that we do not need to know the mass of the Moon. If we were to get a few decent altitudes for the Vanguard we would be willing to attempt measuring it. Perhaps that is a little too rash but it is certainly true that the effect we are looking for is something in the order of 300 feet.

Dr. Whipple—Then you tie back to the fundamental geodetic network.

Dr. O'Keefe—In essence there are two ways of estimating the semi-axis. A comparison on these two will disclose the GM which can be interpreted as the size of the Earth if we know the value of g . I think it ought to be tried.

Lt. Bruce C. Murray—The question I would like to ask is regarding the timing system of any kind of observation stations that one would have. You mentioned that a millisecond accuracy is necessary and furthermore that such accuracy would have to be valid with respect to each of the stations. That is, there could not be any uncertainty between the exact millisecond of stations A, B, C, and so forth. I have heard that a rather large random time delay might be the case between two separate localities. I wonder whether a simple way to beat such a random time delay has been devised or whether it is a problem at all.

Dr. Whipple—Rather contrary to expectations, at a given station a good clock will keep consistent time with respect to the WWV sig-

nals to about a millisecond a day. We thought it might be worse but it is something of that order.

Dr. Hynek—That is correct.

Dr. Whipple—It would be nice to have them better since we can read the clocks to around a ten thousandth of a second.

Dr. Whitney—We were thinking of putting a clock on the satellite with the radio signal. There are lots of ways of doing it. Also, I do not think we need to consider optical acquisition. I think radio acquisition would be the technique.

Dr. Veis—For the effect of errors in timing, the random errors could be taken into account by taking the appropriate correlation between the observed quantities. The effect of the systematic errors can be considerably reduced by computing the coordinates of the stations with different orbits intersecting at angles of near 90°.

Mr. O'Sullivan—There have been some practical examples of what is feasibly possible at this time on a geodetic satellite. We found vehicular-wise that it would be capable of putting something possibly in excess of a hundred pounds in an orbit of possibly a thousand miles. In regard to the acquisition problem it is now possible to have a quiet satellite which can be commanded by a transmitter on the ground to turn on and make its presence known so it can be acquired by radio tracking and also commanded so that the batteries could be recharged by means of solar cells. This would give the possibility of a satellite that would be in orbit for many hundreds of years and if the power of acquirement of the height is not too great it looks like we may be able to bridge the distances that are being asked here.

Capt. Carl I. Aslakson—I may be speaking from the depths of ignorance but I just want to know why no consideration seems to be given to making distance measurements between points on the Earth with the satellite of the type mentioned here. We should be able to get radar reflections. Why can we not apply the line-crossing technique, which is pretty well established, to get distances between points? For that we do not need to know some of this information with high accuracy. We need to know the altitude above the surface which can be obtained with sufficient accuracy from orbital information. We require the mean velocity of the radio waves but certainly can get good statistical information supplemented by

radio sonde in the vicinity of the tracking stations.

For example, with one station on the bulge of South America and another on the bulge of Africa we should be able to get the distance by the line-crossing technique. Any time a satellite crosses in the middle third of the line between the radar sites, simultaneous observations could be made, and over a period of time an excellent statistical determination of the distance could be derived.

Dr. Whipple—Are you suggesting that it be done by radio or optical?

Capt. Aslakson—By radar. Not by using a transponder but a reflector in the missile or even skin reflection in the case of a large missile.

Dr. Whipple—There is another technique I have heard discussed; a signal from the ground and a transponder in the satellite with the signal pickup by a number of stations on the ground.

Capt. Aslakson—I simply recommend consideration of the Hiran line-crossing technique but substituting radar and reflectors for Hiran transponders. I just wondered why this method has not been considered.

Dr. Whipple—Are there any radar people who have anything to say on this? We need a much larger number of high powered radars capable of accurate distance measurements.

Mr. Erwin Feuerstein—I think it depends on how it is done. If you do it in a very simple manner you need a great deal of power because a satellite moves very rapidly and you want to measure time with a high order of accuracy. You have to accept a wide band of spectrum which means a lot of noise. However, if you do have an orbit and can get the accuracy in a number of ways, then it is possible to operate through an integration technique which takes advantage of, in radar language, the limited information. We know the satellite moves in a smooth trajectory. I think there is an excellent chance of getting position data from a satellite.

In this kind of problem in ordinary radar you see a lot of pictures superimposed on each other. With the satellite you have a problem as it is moving so rapidly and you will not get one picture one on top of another, so to speak. In general terminology allow for the velocity by, let us say, displacing the time base a little bit; advance, if you like, the beginning of the time base so that repeated returns, one after the

other, would settle on top of each other with the object moving quite rapidly.

We have a five mile per second object which increases its time delay about 50 micro-seconds per second. If we get ten returns per second then we have five microseconds difference in round trip time from return to return. If we just change the time base by five microseconds, which we can do if we know the speed, we can interpret returns. We can put one measurement on top of the other. They will tend to build up a triangular pulse shape and you can get the center accurately because you look at the entire return which is more accurate than look-

ing at one pulse at a time. The purpose is to put enough pulses on top of each other rather than a straight pulse by pulse computation.

Dr. Hynek—In closing, I would like to ask Richard Adams if he has any information on the fall of 1958 § 1 and if the time is not too awkward. Is the expected time still seven o'clock tomorrow (December 3, 1958)?

Mr. Richard M. Adams—As far as we know, it is seven tomorrow morning plus or minus a couple of revolutions.

Dr. Hynek—The communications room at 79 Garden Street will be open for any who want to watch the fun.

Optical and Electronic Tracking

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I had planned to give a summary of what had been done in all kinds of tracking, but much of the optical aspect has been covered; at least, there is some understanding of it here.

For one thing, in the title of my subject I would prefer to say 'radio' tracking rather than 'electronic,' because, presumably, the distinction is between the frequencies of the signal from the satellite by which the tracking is done. Electronic problems come also into the optical tracking in the timing and in other ways, and I am going to say some more about that side of the problem.

The definition and purpose of satellite tracking may be stated as follows: To observe positions and motions of a satellite, as well as the times of such observations, with frequency and precision sufficient for developing a theory of such motion capable of predicting future times and positions at least as accurately as they can be observed. Theories and procedures must be based partly on previous geodetic knowledge, and an important feedback from their improvement would be an increase of geodetic knowledge.

I had not thought of the subject as directly geodetic, but as tracking to determine the orbit sufficiently for predicting the future in time and position. Now such a theory involves many geodetic considerations. You have to start out with *some* geodetic knowledge in order to consider the most elementary perturbations in the orbit, and observations of the satellite, when put back into the orbit, will thereby improve geodetic knowledge. That is the standpoint from which I am looking at this question of tracking; however, the problem of tracking as sightings for direct geodetic purposes, as mentioned many times today, could be approached by similar methods.

Now, as I said, there is an electronic operation connected with all kinds of tracking, that is, the time problem. It has been mentioned in questions and discussion that one must have a checking contact with some central standard clock. As I understand it, the Smithsonian method is for each station to have its own clock, which every day is checked against WWV, and that

this station clock in itself keeps time accurately to within a millisecond. The remark was made here awhile back that even WWV, as shown by the Minitrack work, presents problems, such as the fact that, apparently, they set it once a day, and before they set it, it may be off several milliseconds.

Another time problem is that, if the checking contact is by radio, the time of transmission is in doubt by a millisecond or more, because of lack of knowledge of the path through the atmosphere from where the radio signal originated. The signal will vary in velocity and therefore in the time-lag for reaching the observing station. It has been suggested that the only way to get around this problem would be to have wire contact with the checking source. One does know how long it takes a signal to come along a wire, so perhaps you at Smithsonian are going to have to run cables to all your optical stations, as Vanguard is apparently thinking of doing to Minitrack stations. A millisecond variation carries an error of one to four seconds of arc in apparent position of a satellite such as we are considering at a height under a thousand miles or so. Therefore, in a way, that variation represents an accuracy limit which is common to all methods of tracking, either radio or optical.

It has been remarked—I am quoting some Smithsonian people here—that atomic clocks are about ten times more accurate than ones now being used at tracking stations, but that they are very expensive and have short durability, only lasting a few months. Thus it is possible to have time accuracy up to ten times as great as now being done, but only of the same order as the presently available precision of the measured position.

I had hoped that Smithsonian people would say something about the optical tracking, so I would not have all of that problem to discuss. They have indeed, brought a model here to show us how they are doing such work. However, I am going to make a brief outline of the general problem. In particular, all the current methods of optical tracking—that is, before the

discussion today—have used sunlight: the satellite must be sunlit and in a fairly dark sky, except that for visual and photoelectric observations of very bright satellites a brighter sky can be tolerated. As you know, the planet Venus is visible in the daytime with its stellar magnitude of about minus four. So, presumably, a satellite brighter than that could be seen visually in the daytime, and it has recently been demonstrated that photoelectric methods can be used in the daytime on such, or even fainter, satellites.

Now the first type of optical tracking, the most elementary, is that using merely the naked eye—as I heard a Navy man say the other day, 'Mark I eyeball'—or a small telescope. Moonwatch people have done that, and their limit of accuracy is about a tenth of a degree. That does not have to be apologized for, because I have to admit that radio tracking has inaccuracies as great as that. Such visual tracking, however, is of some indirect geodetic value by insuring predictions for pointing more accurate tracking devices. Since it is a fundamental necessity to know where a satellite is at any time, this most elementary type of tracking is of considerable importance. The estimated number of observations from Moonwatch is around 7000 in the past year.

Of course, the Russian satellites, which are so bright you can hardly miss them, account for the majority of those observations. I can say from first hand knowledge, in that I did some of this elementary tracking of them myself, that I do not think this approach needs any apology, since to know when such as the Sputnik III rocket is coming down, you need a lot of observations.

Next there is photography with such as Baker-Nunn-Schmidt cameras, of which I believe Dr. Hynek has said far more than I could. Such tracking which is accurate to less than four seconds of arc and a thousandth of a second in time, is sufficient to get geophysical perturbations of the satellite orbit, both periodic and irregular.

There seems to be two kinds of geodesy, two schools of thought; one thinks in terms of gravity, and the other in kinematic trigonometry. I am thinking here in terms of gravimetric geodesy. The optical tracking, if it is sufficiently distributed around the orbit of the satellite, will detect irregular perturbations in that orbit due to any geodetic or barometric cause that we, up to now, have thought of.

In doing such tracking, the field distortion of these short-focal cameras at large zenith distances is one difficulty in the method of photography. However, photography in astronomy has long established itself as of fundamental importance. After one gets a photograph, one can measure it many times, and thereby add to the accuracy of the knowledge of position of the center of any image on it. This is something that cannot be achieved by methods offering only a single shot at the image, so to speak, such as the visual and possibly some others. The high rate of motion of the satellite does sort of compromise this advantage a bit, which is one reason the accuracy claimed for the method is not as high as it could be if it were applied to an object in a slow motion, such as an asteroid.

Now there is another type of optical tracking which we have to mention, namely, photoelectric detection and position measurement, as described by A. P. Willmore in *Nature* of October 11, 1958. He and his associates have built a photoelectric detector attached to a five-inch refractor for which the accuracy is said to approach two seconds of arc. He has observed the Sputnik III rocket with it, for which a signal record is published with the paper. It shows the light-pulses of the Sputnik, and since the time record goes along with it—the chronograph, so to speak, is at the bottom of the record—you can read off the time of each pulse. Then the question of getting the exact position is handled by having slits properly arranged in the focal plane of the telescope, various schemes for these slits being shown. The key to their method is that, when the satellite crosses the slit, it causes a sudden pulse, thereby representing a point in space and in time.

Of course, we have to tie this position record down to Earth somehow. The slits are oriented by having been previously calibrated either with the star field or, I believe, by an azimuth-setting method. In other words, they have the telescope on an alt-azimuth mounting, and, as the satellite goes along, they make a setting ahead of where it is expected to be. Then, an assistant reads the altitude and azimuth for the field-center, and the photoelectric recorder does its job automatically.

He claims accuracy for this method of around two seconds of arc with only a five-inch, relatively inexpensive, telescope, which is certainly pretty good. There is a very important possibility for this method, which, I think has already

been mentioned. Willmore states that, because of the good signal-to-noise ratio, he is certain they can track a satellite in the daytime. Although they have not yet done this, it is a very important possibility for arranging better distribution of optical tracking observations around the orbit.

Now, apparently, Willmore is not alone in his thinking because, after I devised the outline of this talk, I opened the December *Sky and Telescope*, and it let the cat out of the bag by saying that Smithsonian Astrophysical Observatory has ordered an instrument called the 'Cateye,' which has been devised by Lloyd Wylie at Wittenberg College.

(J. Allen Hynek interrupted to note that the instrument has been delivered and is operating.)

The Cateye shows the planet Venus in the daytime which fact, I would think, indicates that you can hope to track satellites in the daytime. Any further story about such developments I will leave for the Smithsonian people to tell.

What I had better talk most about, since I am, perhaps, the only representative of it here, is radio tracking. This is the observation by radio detection of oscillations between 20 and 1000 per second frequency, that is, wave lengths from 15 meters down to 30 centimeters, either broadcast by the satellite or, possibly, originat-

ing in a ground source and reflected by the skin of the satellite. An example of this latter was mentioned in the previous discussion, as essentially the process called radar. Most of such directional antenna systems are radio interferometers such as Minitrack, although you do not have to use interferometers. You can use a dish, a method which, I am informed here, is being seriously attempted by the Army.

Minitrack (Fig. 1) is a radio interferometer, which means that it receives a beam of the signal from a satellite at two separate points. These beams are then brought together for study of their mutual interference. Figure 1 shows the Minitrack station at Havana, Cuba. The receiving antennas which constitute the reception points for the beams are then brought together for interference, thereby leading to the satellite's position. The north-south line is approximately perpendicular to these two antennas, and here the beams would be brought together. It is similar for the other two antennas, by which we would study the motion of the satellite when it is crossing the prime vertical, that is, the east-west line.

Figure 2 shows a diagram of the general situation, showing the north-south line and the corresponding two antennas. These two antennas receive the beam simultaneously from the satellite and can thereby get the position of it with reference to the north-south line. The total

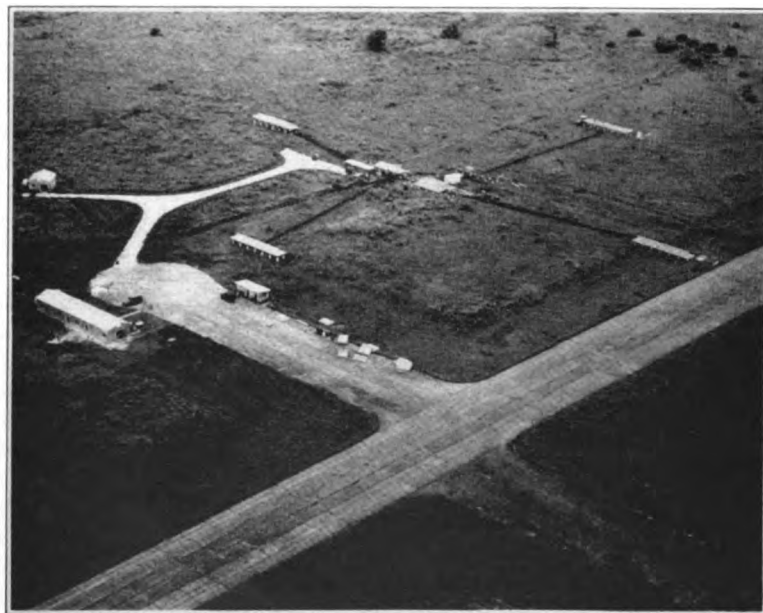


FIG. 1—Minitrack field at Havana, Cuba

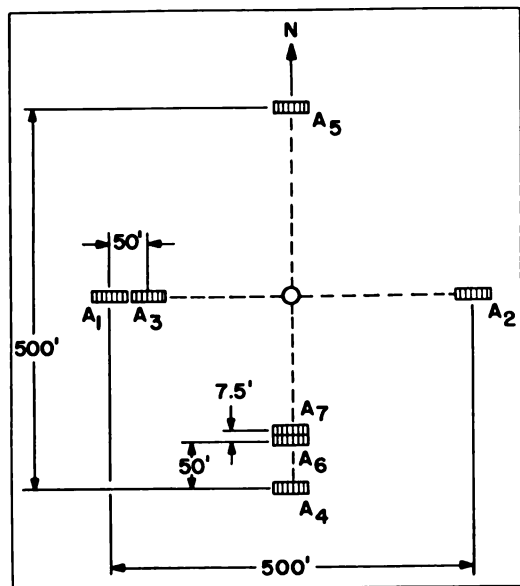


Fig. 2—Minitrack ground plan

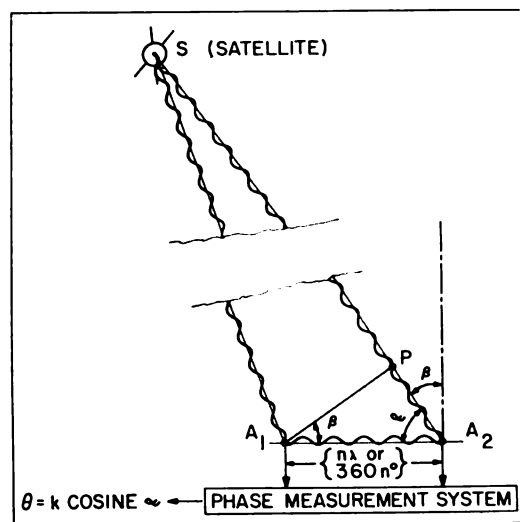


Fig. 3—Minitrack interferometer theory

separation is 500 feet, a distance that is related to the wave length of the signal and the angular size of the lobes of the signal. These lobes in radio work correspond to interference fringes in optical work. The separation of the antennas is related to the angular separation of the lobes, and thereby determines the resolving power, or precision possible from such measurements. Another antenna shown here has to do with determining with which lobe one is dealing. From

the outer two antennas separated by 500 feet one gets a large number of lobes, and to know which one of the fringes, so to speak, is being measured, a third antenna of closer separation is used.

Figure 3 shows the theory of radio interferometry. The beam from the satellite is received here. The separation of the antennas is some multiple of the wave length, and whatever multiple it is determines the angular separation of the lobes. In this case, there is a 500-foot separation of antennas, and the wave length is about ten feet. Hence, the separation of the lobes would be around one fiftieth of a radian, or slightly over a degree. We also have the cosine factor which comes in here if the satellite is some angle off the meridian.

Figure 4 shows the lobes and the interference pattern. If the satellite is on the meridian at the zenith, you would get a maximum in this direction, that is, the two beams received by the antennas would be adding to each other, since they would be in phase. If the satellite is off by a certain angle, which would be half the lobe separation of about 1.2° here, the two beams would be 180° out of phase and the intensity is zero. If this is essentially an interference pattern, the half width of the lobe is a rough order of the resolving power.

However, here is an important point. The techniques of radio interferometry have gone quite a bit ahead of optical interferometry. The Minitrackers are not content with leaving the resolving-power here at 0.6° . They can measure exactly how far off they are from the center of the lobe by measuring the difference in phase of the beams at that point. The measurement of phase difference is an art in radio engineering which I can not explain at length. Suffice it to say they can do it with such accuracy that they claim the positional error by this method would not exceed 20 seconds of arc. In other words, they can determine the phase-difference at particular points on the lobe to within the fraction of about one half of one percent of the total separation of the lobes.

Figure 5 shows a block diagram of the Minitrack system. I am no expert on this, but I point out that they use a standard frequency which helps to make this frequency comparison for getting angular positions to within 20 seconds of arc. The time standard signal from WWV is used too, so they get the time of the observation together with the positional measurement. Various anten-

nas are attached to the receiving system. Finally, the result goes into the recording system and is telegraphed to the Vanguard control center at Washington as an angular position.

In Figure 6 are the two east-west antennas, with the position of the satellite just off the zenith. The calibration of these radio Minitrack stations has to be done by optical means. It is done by airplane, which carries sources of both radio and optical signals. This flies over the Minitrack station while the two signals are given out simultaneously and recorded. The optical signal is photographed on the star background, then the two signal positions are compared for calibration of the radio tracking station. Of course, all kinds of problems come into that operation, for example, the airplane is not above the atmosphere as is a satellite. However, since the ultimate precision claimed is only 20 seconds of arc, we do not have quite the worry about refraction as in the more precise measurements mentioned this morning.

The accuracy of the radio tracking is further lowered to several minutes of arc by ionospheric scintillations. That, indeed is the greatest trouble in radio tracking. This 20 seconds of arc precision, which the instrument makers claim, is correct undoubtedly in the same sense that astronomers claim their stellar position measurements are known to a tenth of a second, let us say. But actually it does not work to this accuracy because of the twinkling of the stars, unless the telescope is on a satellite or on a balloon high in the atmosphere. With the Minitrack observations it is much worse, because the radio waves have not only this tropospheric, but also ionospheric scintillation, which is far more powerful and unpredictable, although in itself it is an interesting problem because related to sunspot activity and other questions of the ionosphere. If this were a

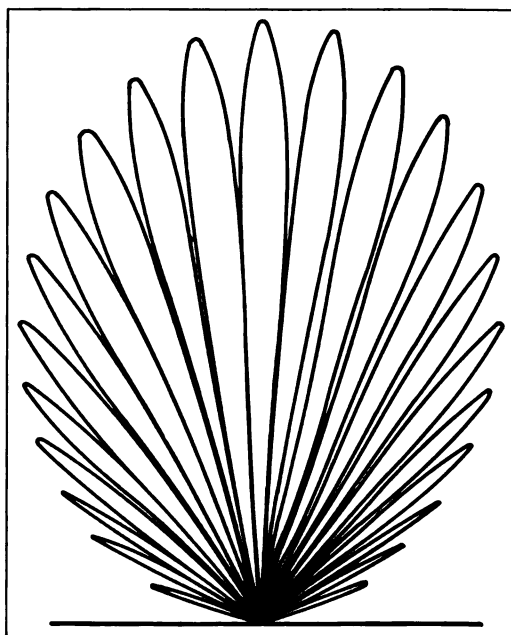


Fig. 4—Minitrack interference pattern

meeting on other aspects of geophysics, I should expand on this question but I will not say more here, except that it presents a great difficulty to the geodesist using radio observations.

However, even so, the Vanguard Satellite I, 1958 β 2 has so far, I believe, contributed almost as much to geodesy as any artificial satellite that has been optically observable. The report by Miss Eckels given earlier on a new derivation of the Earth's figure, showed just a start on the possibilities. But why is this possible, when the accuracy of an individual observation is so low? It is because radio can be used day and night, in fair weather or foul, and the observations are well distributed around the orbit. All these factors compensate for Vanguard's not being observable

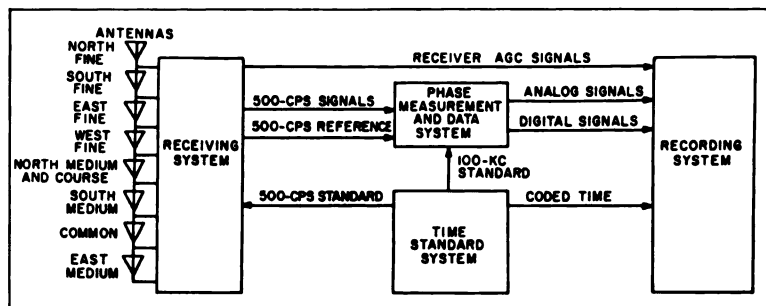


Fig. 5—Block diagram of Minitrack system

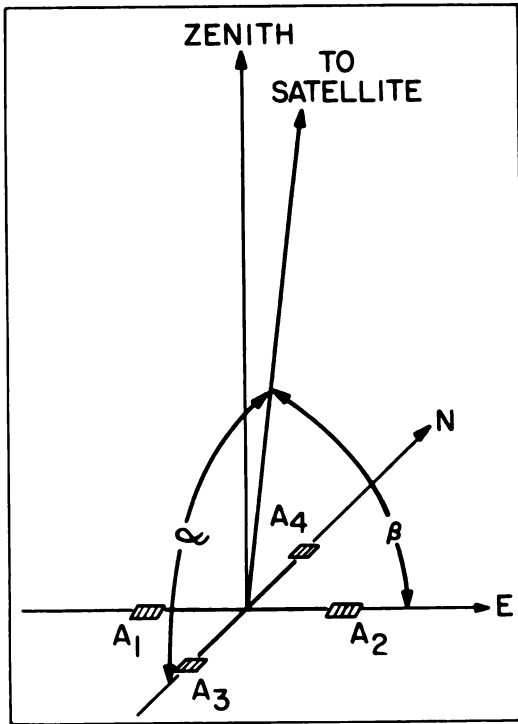


Fig. 6—Minitrack sky geometry

optically. So, even with relatively inaccurate radio observations, these important geodetic results have been announced, and perhaps more are forthcoming.

Figure 7 shows you the geographical distribution of the Minitrack stations. Another has been established in Australia. This distribution gives a good coverage of latitudes north and south. That is why the observations are well distributed around the orbit. All these stations are connected with the central station and thereby report on the satellite at a lot of different positions in its orbit. Radio signals from satellites may be continued for months or years, by the use of solar batteries.

Figure 8 shows how the radio observations are routed into the Vanguard control center. The observations come into the control center and are relayed to various computers, including not only Vanguard computers, but also the Smithsonian.

Figure 9 shows the 1958 β 2 satellite in a cut-away diagram. This is almost an ideal design for a geodetic satellite. Even if you assume only radio observations of it, this is so. It is the only satellite that has ever been up there, or even planned to be up, which has spherical symmetry. That



Fig. 7—Distribution of Minitrack stations

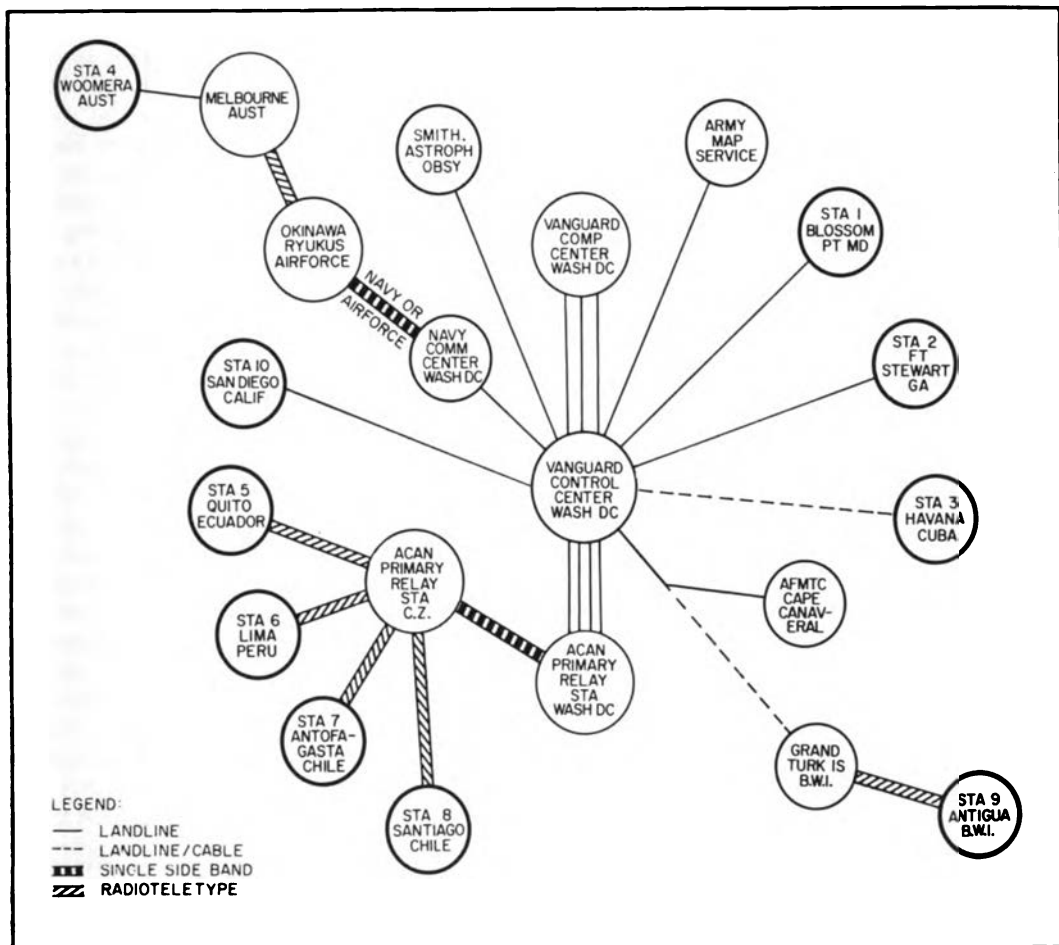


FIG. 8—Vanguard-Minitrack information flow chart

feature is most important for equalizing the atmospheric drag and rotational perturbations. The solar cells are symmetrically distributed and will enable tracking to go on for the indefinite future, at least in the daytime. As of December 1958, I believe the chemical batteries are dead, so there is no signal from the satellite's systems at night. But throughout the daytime they continue to function and will do so for the indefinite future. An immediate test of durability will come by the end of 1958, because one of the difficulties is that, in sunlight, this radio apparatus cannot stand too high a temperature, and, at the solstice, the satellite remains in sunlight all of the time. The winter solstice is the more critical, as the Sun is then nearest the Earth, so, if the radio lives through that period, there is a good chance of its carrying on for many years.

This general efficacy of Minitrack observation has gotten results such as Miss Eckels has reported. There have been many other direct determinations of perturbations having periods of more than two or three weeks, such as one which has been published in the Smithsonian special reports. I think it was Jacchia who discussed there satellite 1958 β 2 and found a monthly perturbation which seems to be related to the tidal effect of the Moon and, possibly, to the Sun.

Now, therefore, the one thing that is lacking in this Vanguard satellite is optical availability for getting observations with the greater accuracy of a few seconds of arc. I have reviewed what has been done in past and present achievements, but there are, both for optical and radio tracking, various weaknesses I have pointed out. The optical tracking lacks good distribution around the

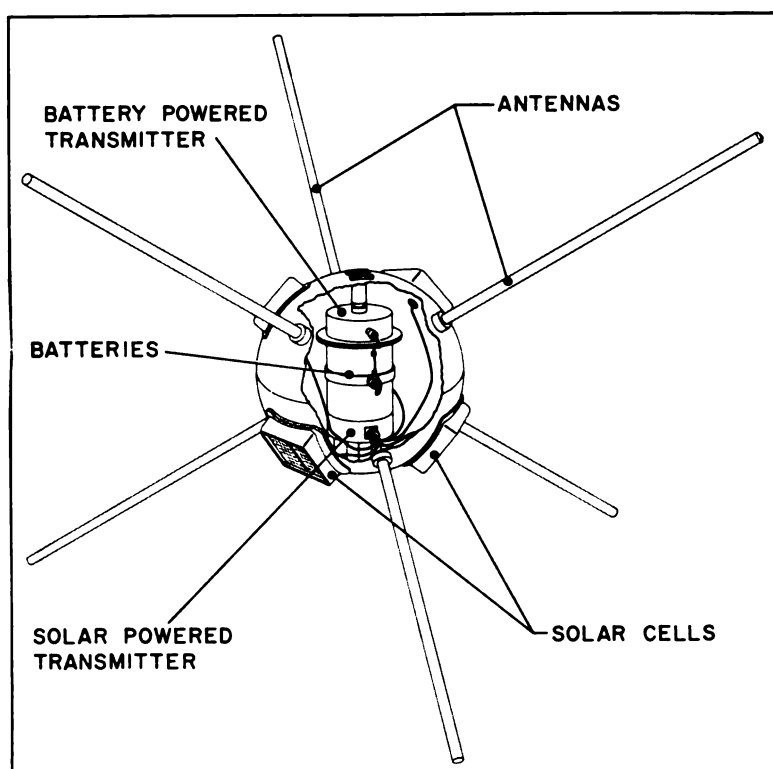


FIG. 9—Cutaway drawing of 1958 β 2, 6.4-inch Vanguard test satellite

orbit, although perhaps, the development of photoelectric detection may amend that by making possible tracking in the daytime. Radio tracking has pretty good orbital coverage (that is the reason so much as been deduced from 1958 β 2), but it lacks angular precision of particular observations.

Hence, I am going to make some suggestions for better possible geodetic tracking. As a start, I suggest that you can have a close satellite orbit which is sunlit practically all the time, that is, an orbit having a rate of nodal regression which equals the annual rate of the eastward motion of the Sun. In that way, optical tracking, even without the further possibility of observing in the daytime, could be extended all around the orbit. This orbit also has the advantage of being almost a polar orbit and thus covers the Earth pretty nicely for geodesy. The theory is that a satellite launched westward at either sunrise or sunset in an orbit inclined slightly over 80° to the equator has motion such that its nodal regression eastward would be about a degree a day, thus exactly keeping up with the Sun.

The theory behind this suggestion, although I will not say it agrees precisely with the new value of J set forth earlier by Miss Eckels, is as follows. Nodal regression period

$$= (9.21 \times 10^{-12})a^{7/2} (\sec i) \text{ days}$$
in which a is in statute miles. For $a = 4500$ miles (mean $h = 540$ miles), nodal regression period is 56.3 days for $i = 0$. Hence the condition for a period of 1 year is that $\cos i = 56.3/365.25 = 0.1541$, or $i = 81^\circ.1$.

The period of nodal regression westward is given in days and it varies as the secant of the orbital inclination. Suppose we assume the semi-major axis at a little over 4500 miles, then the nodal regression period is 56 days for zero inclination. If we want a period of one year we have a solution for cosine i ; and i turns out to be a little over 81° . Such an orbit would be feasible if we could launch a satellite southward and westward, and the expectation is that this will be possible in 1960. It is no longer a secret that a launching base for polar orbits is being constructed in California near Santa Barbara.

With this possibility one could have a more

complete optical tracking around the orbit, because at every sunrise and sunset the satellite would pass most stations at least once. It would always be sunlit and available except that the twilight zone shifts from season to season, as the declination of the Sun varies. However, it turns out that at a mean height of 500 miles, that difficulty practically would not happen, except at the highest latitudes, perhaps 70° latitude north and south. Even there the Sun would still be shining on the satellite, and it could thus be sighted at almost all points around the twilight zone of the Earth. These conditions would continue indefinitely, depending on how accurately one made the inclination so that the period of nodal regression would be exactly a year. Of course, ultimately the nodes would get out of step with the Sun and it would take a long time to get back in step, but I do not think that difficulty would be an immediate worry.

Now another suggestion I am going to make for possible improvement of optical tracking has, I think, already been mentioned. It is to combine photoelectric detection and timing with photography; that is, to use the photography for accurate angular position and photoelectric detection for timing. Such combination of the two would possibly lead to more accuracy and greater flexibility. The flexibility development is an important consideration. This is related to the question of improved visibility of a satellite. Magnification by a reflector of an image is proportional to the radius of curvature of the reflector. But one does not have to make a hundred foot satellite to have a radius of curvature of a hundred feet. One can put a radius of curvature of a hundred feet on a six inch satellite by attaching a little square section of a 100-foot sphere. That section gives much greater brightness than would the six-inch sphere itself and, if there are a few dozen or a hundred such faces in a spherical distribution around the satellite, there would be a sufficient frequency of flashes that one would appear every few seconds to any observer, and thus it could be easily detected, even though a relatively small satellite were used. One of the difficulties to that approach, which the Smithsonian people have pointed out to me before, is on the question of time determination. Their timing scheme assumes a continuous reflection and track of the satellite, so that the track may be chopped at a determinable time. However, with this new suggestion of combining the photoelectric time

with photography I think that such difficulty could be avoided by the timing being done by the photoelectric detector, while the position results from photography of an image which is brighter by having come from a surface flatter and more reflecting than the smooth satellite skin. So even on a satellite only a foot in diameter, the flat faces on it would not have to cover the whole surface. Perhaps some of the rest of the surface would be solar batteries and some of it spherical. Thus a combination of these two methods of detection might solve this dilemma of getting positions from flasher from a satellite having flat faces on it.

I might say (I think this also has been pointed out by others) that, if one sends out a probe to the Moon or another planet, a flat face is the only hope of optical detection of the vehicle out to that distance. You would not have to have a very large flat face to make it quite easily visible optically. Especially, in the case of a lunar probe, the motion would be relatively slow, so one could use a long-focus telescope and usual astrometric methods on it very easily.

Another reason for urging a faceted satellite is the possibility that the word tracking, I believe, has to take on a little broader meaning: not only tracking a space position of the satellite but also its rotational motion. In fact, some experimentalists in satellite work have already been assuming that we have regularly been doing the latter, but, of course, we have to confess that we have not. The latest *Sky and Telescope* had a very interesting report on photometric studies of Sputnik rockets. These brightness peaks are a clue to the rotational motion. One could possibly work out a theory on the axial rotation of these rockets if their exact shape were known. However, if you are launching a satellite yourself, you can choose to arrange the reflection facets so they will disclose the rotation.

The following shows what I mean in this connection. For a specular polyhedron having N faces when complete, the rate of flash-reflection from the Sun for spin-rate ω cycles/sec is n (per second) = $(\omega N \cos L)/273$ where L = latitude of reflecting face, so spin-axis is determinate from flash-rate. Flash duration increases as the secant of the latitude of the face.

Assume a specular polyhedron completely covered with faces, then the number of flashes per second turns out to be divided by this constant (273) which depends on the angular size



FIG. 10—Model of polyhedral satellite constructed by Laddie T. Rhodes (in photo) and colleagues of the U. S. Naval Research Laboratory; 2500 one-inch square glass mirrors have been cemented to a 30-inch fibre-glass sphere; the whole weighs about 25 lbs and cost less than \$50.

of the Sun, and is proportional to the number of faces times the spin rate times the cosine of the latitude on the satellite from which the reflection is received. So you see that, after one got the angle of the satellite from the Sun, immediately the angle of the spin axis to the Sun could be deduced from ground observation where one has merely counted, so to speak, the flash rate. That deduction would assume that there are equal-size faces all over the satellite, as shown in Figure 10. Usually the faces would most conveniently be hexagons. Some wisecracker remarked that if we could only make them pentagons, maybe it would come about sooner.

I brought along a model of such a satellite, a Christmas tree ornament that costs ten cents. It has a latitude-longitude grid of facets. I do not think that such would be the most desirable grid-pattern, but that is a small detail that could be discussed in due time. Such pattern does not work in the above rotation theory because there are the same number of facets at all latitudes. However, one does get a lower brightness when it is reflecting from a facet nearer the pole than when reflecting from the equator, so the latitude of reflection could possibly be determined that way. As I said above, certain experimentalists are thinking of using the attitude of a satellite as a

fundamental datum in their experimental purposes. Thus, flash-counting or photometry, would be an important method of getting the attitude of a faceted satellite from the ground. Of course, one might be able to do this from the satellite itself, but if one has it also from the ground it would always be a useful check on the telemetry data.

If one uses photoelectric detection, there is no reason why one can not go into the infra-red, which is much less interfered with by clouds and haziness. If one goes far enough into the infra-red, the signal would be practically not interfered with at all.

Lastly, in connection with radio tracking: while there seems to be an unsurmountable problem of reducing errors, a study of the scintillation problem would be of interest not only as a study of the ionosphere itself, but also from it one could deduce the complete laws of refraction. There might be an improvement of our knowledge of the tropospheric refraction by studying radio tracking results parallel to optical tracking; in other words, optical tracking would be a continual calibration, one might say, of the radio tracking. Such an aid has been lacking on satellite 1958 β 2, since there have been no optical observations of any value on it up to now.

There was one such observation. L. T. Johnson, using a ten-inch telescope, saw the Vanguard satellite at a time when it was predicted to pass the tracking station, his station being at Welcome, near Blossom Point, Maryland. Although that approach is of little general value since it cannot be done extensively, any comparison with optical positions would certainly help the radio accuracy.

Another approach to increasing radio accuracy, as I pointed out above, is that, in the theory of radio interferometry, there is the dependence of the resolving power on the separation of the an-

tennas. There is improvement both with greater separation of the antennas (I believe some radio observatories have used up to several miles) or with shorter wave lengths (higher frequencies). I think that the latter approach has also been proposed; perhaps somebody in the Army could say more about it than I. At any rate, a higher frequency would decrease the radio tracking errors, since these vary inversely as the square of the frequency: so raising the frequency to a thousand cycles a second from a hundred would theoretically reduce the refraction and presumably also the scintillation by 99%.

Discussion

Dr. Robert Newton—I would like to suggest also, for radio tracking, the analysis of the Doppler signals received. This, in principle, even for a single transit, gives the complete orbit, and again is limited in accuracy by the refraction error. In our studies, we are proposing to use two frequencies on the satellite, and thereby get a refraction measurement at the same time that we determine the orbit.

Dr. Fred L. Whipple—I understand that the Doppler method yields in fact about the same accuracy as the interferometry method. Is that true?

Dr. Newton—We believe that is correct, but we have not been able to get any equipment to test it out. Paper study shows it is as accurate, perhaps even more so.

Dr. Wilson—One of the great difficulties there is that the greatest shifts occur when observation is done near the horizon.

Dr. Newton—This is right, and the accuracy depends on how one corrects for refraction.

Dr. John A. O'Keefe—On this matter of polyhedrons, if I understand it correctly, when one diminishes the time of a flash by a factor of about five hundred one only loses a magnitude in the brightness. From that it would seem to follow if one rotates that little ball, it should look about a hundred times as bright as if it were hanging on a christmas tree. I do not believe that. I think something is wrong.

Dr. Wilson—I did some experiments with short-duration flashes. In the example I mentioned earlier, with a flash duration of only about a thousandth of a second and a flash frequency of two per second, you lose about one magnitude.

I think you would lose less from an actual satellite.

Dr. Fred L. Whipple—May I interrupt to say that Armand Spitz has conducted experiments with a sphere of this sort and the effect occurs only very low in the intensity scale as seen by the eye. Does anybody know the exact numbers of Spitz's experiment? He had two spheres of the type you are both discussing, illuminated at a distance.

Dr. J. Allen Hynek—Hung up on a wire with a flashlight a good many yards away.

Dr. Wilson—You can look at it this way. The reflection image on a satellite you might think of as an area which has an angular size determined either by the angular size of the Sun and the radius of curvature of the specular sphere, or if the sphere is polyhedral, it is limited mostly by the apparent size of a facet. The total intensity will then be proportional to the apparent size of the instantaneously reflecting surface. In other words, the larger the sphere or polyhedral facet at a given distance, the greater will be the satellite brightness. With a specular sphere, the Sun at the Earth's distance is of such angular size that it covers about 1/200,000 of the surface; a specular facet could conveniently be very much larger.

Dr. Whipple—Does this correct your statement that a six-inch sphere could be the equivalent of a 100-foot sphere? It would have to be at least a foot across before it could give one flash equal in brightness to that of a hundred foot sphere.

Dr. Wilson—No, because the apparent diameter of the Sun is 1/100 of a radian, or six inches on a 100-ft sphere.

Dr. Newton—Will you not lose a lot of observations? Would you necessarily gain?

Dr. Wilson—You get the number of flashes per second which varies as the total number of faces on the reflecting object, and as its rate of spin per second. With 450 faces rotating once per second, you would have one flash per second.

Dr. Newton—Do you have to have a regular polyhedron?

Dr. Wilson—No, you would have a little spare area between faces that would be lost, it is true, but a relatively small area.

Dr. Newton—What if you had a polyhedron with the faces all the same size but allow acute angles between faces, as seen by the observer?

Dr. Wilson—I am assuming a convex polyhedron. I should have mentioned the possibility of corner reflectors, since they would have the desirable effect of extending the observations into the night time when the satellite is not sunlit.

Dr. O'Keefe—You can get something like a fourth or fifth magnitude. The intensity dimin-

ishes as the fourth power of the distance. It is a little difficult if you get up to a thousand miles. But if it is up to four hundred or six hundred miles you do not need very many searchlights.

Dr. Nancy G. Roman—How good an ephemeris can you give us at the present time?

Dr. J. Allen Hynek—I think that Richard Adams would not want me to leave unexplained the fact that the Smithsonian ephemeris is 20 minutes off. He finds himself in the position of the irate commuter who misses his train because he has not been notified that the schedule has been changed. The point is that the schedule has to be changed practically every day. He showed me some corrections the other day for particular satellites, particularly Delta. The corrections are available but our funds are limited so we can not telegraph the corrections every day all over the world. So if you are using an ephemeris a few days old it is understandable.

Dr. Wilson—I should have mentioned that 20 minutes error was an extreme case.

Orbits in Contemporary Geodesy

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Satellite orbital theory and launching techniques have been developed sufficiently that consideration of distinctive orbits for particular purposes makes practical sense. Many references to such orbits may be found. Two important aspects of these orbits must be recognized. The distinctive properties of the orbits are functions of the shape and gravitational field of the Earth; and the precision with which special properties may be realized depends upon the accuracy of the launch injections or corrective maneuvers. The study of these matters constitutes a branch of applied geodesy having considerable contemporary importance.

Radiation measurements made during 1958 by the Explorer satellites have detected a region of intense corpuscular radiation around the Earth [Van Allen and others, 1958; Van Allen, 1958]. The lower boundary of this region is not simply specified, being both latitude and longitude dependent, however, an altitude of 1000 km is representative. Hence, to avoid this radiation, an important class of orbits in the future will have an apogee below this altitude. These will necessarily have small eccentricities. Low circular orbits also have been suggested for other reasons. An approximately circular orbit at 480 km has been mentioned as appropriate for meteorological observations of the Earth [Glaser, 1957]. The low altitude of these orbits has the consequence that atmospheric drag will not be negligible and the effects of anomalies in the gravitational field will be larger than at higher altitudes. From the viewpoint of orbit determination, these are not ideal orbits, but practical considerations will dictate their use.

For low orbits inclined to the equatorial plane, the oblate character of the Earth imposes practical limitations on circular orbits. First, if a constant distance from the surface were desirable, say for optical observation of clouds, a circular orbit is at best 22 km closer to the surface at the equator than at the poles, solely because of the shape of the Earth. Local surface elevations add to this difference. Further, the gravitational field of the oblate Earth forces inclined orbits to differ

slightly from circles. The instrumentation to be used on the satellite must, therefore, be designed to at least allow for variations in distance to the surface of these amounts. A nominal specification on launch accuracy corresponding to 10 to 20 km variation from circularity seems reasonable in the light of these facts. The instrumentation would, in this case, be designed to cope with variations of comparable magnitude due to the character of the Earth and launch inaccuracies.

Because ground observation stations rotate with the Earth, satellites having periods commensurate with the rotational period of the Earth have some advantageous properties. Table 1 shows periods and approximate semi-major axes. The most discussed example is the satellite in a circular equatorial orbit with a period of one day which, hence, appears stationary with respect to the Earth. The utility of such a body has been expounded at great length. A disadvantage is its great distance from the Earth.

The motion of an artificial satellite is known to be approximately an ellipse whose plane rotates about the polar axis of the Earth and whose major axis rotates in the plane of the orbit [Roberson, 1954]. The rates of these rotations, given approximately by a first order perturbation theory, are

$$\dot{\Omega} = -3k_e j \frac{\cos i}{a^{7/2}(1 - \epsilon^2)^2} \quad (1)$$

$$\dot{\omega} = -3k_e j \frac{1 - 5 \cos^2 i}{2a^{7/2}(1 - \epsilon^2)^2} \quad (2)$$

for a potential

$$V = -\frac{k_e}{r} \left\{ 1 + \frac{j}{r^2} \left(1 - 3 \frac{z^2}{r^2} \right) \right\} \quad (3)$$

Note that the rates are a function of three of the usual orbital elements, namely; a , ϵ , and i . Hence, by choosing the values of these elements, some adjustment of $\dot{\Omega}$ and $\dot{\omega}$ may be made. For any a and ϵ , the relative values of $\dot{\Omega}$ and $\dot{\omega}$ are illustrated in Figure 1. Any desired ratio of $\dot{\Omega}$ to $\dot{\omega}$ may be obtained. A special case often discussed is that in which the line of nodes rotates once in a year, thus, leaving the orientation of the orbital

TABLE 1 - *Orbital periods and approximate semi-major axes*

Orbital period	Semi-major axis
day	km
1	42,200
1/2	26,600
1/3	20,300
1/4	16,800
1/5	14,400
1/6	12,800
1/7	11,500
1/8	10,600
1/9	9,800
1/10	9,100
1/11	8,500
1/12	8,100
1/13	7,600

plane relative to the Sun unchanged. Another is that in which the value of $\dot{\omega}$ is zero, the line of apsides thus being stationary in the orbital plane. The conditions implied by these two cases, as derived from approximate results of the above equations are

Condition for $\dot{\Omega} = 2\pi/\text{year}$

$$\frac{\cos i}{a^{7/2}(1 - e^2)^2} = -4.73 \times 10^{-16} \tag{4}$$

where a is entered in kilometers.

Condition for $\dot{\omega} = 0$

$$\cos^2 i = \frac{1}{5} \tag{5}$$

the latter being particularly simple. Note that the minus sign in the first of these conditions implies a retrograde orbit.

Many orbits with useful properties may be derived from the relations discussed. As an example, consider a satellite launched to facilitate the determination of the relative positions of European and North American continents. A desirable situation might be that in which the satellite met the following conditions: (1) the satellite should pass the same position over the mid-Atlantic every evening a little past sunset on the east coast of North America, and (2) the satellite should be high during such passes to be in the sunlight and to be visible simultaneously from both continents. The first condition implies that the rotation of

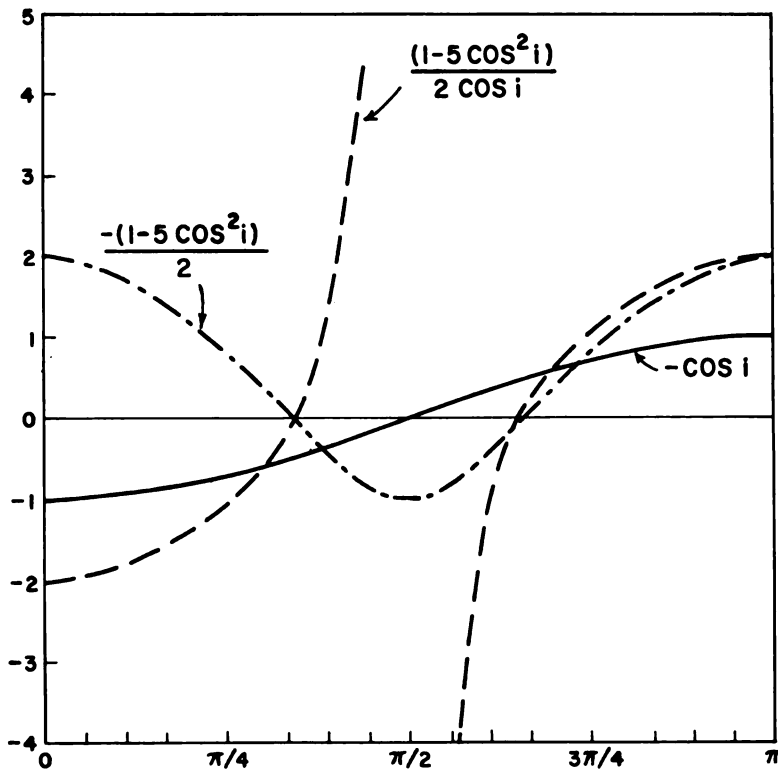


FIG. 1—Relative Values of $\dot{\Omega}$ and $\dot{\omega}$ for fixed a and e

the line of nodes have the same rate as the Earth's rotation about the Sun, and also that the satellite make an integral number of revolutions per day. Thus, the condition for $\dot{\Omega} = 2\pi/\text{year}$, above, must be satisfied with the semi-major axis a given by one of the values from Table 1. The condition that the position be the same on every evening pass either requires that the line of apsides not rotate, or that the eccentricity be zero. Solutions of Eq. (4) and (5) for both possibilities are shown in Table 2. Only those solutions having positions on the orbit with altitudes in excess of 2000 km are listed since such altitudes are required to satisfy the demands on visibility.

Control or, at least, knowledge of the motion of the lines of nodes and apsides is important to the satellite designer for other reasons. In particular, as the orbit orientation changes, the fraction of the time with the satellite in the Earth's shadow changes. This fraction is an important design parameter in the temperature control of the satellite and if solar cells are used in the power supply for the instrumentation.

The precision with which distinctive orbital properties may be realized must be considered. For example, if a high quality guidance system is used in the final stage of a launching vehicle, an uncertainty in injection velocity of five meters per second may be typical. An optimistic accuracy estimate is one part in 10^4 variation of the velocity vector from its desired value. More accurate injection would require a sophisticated correction maneuver with a small auxiliary jet on the vehicle. An estimation of the effect of a five-meter per second deviation on the desired orbital characteristics will be instructive.

For the circular orbit with nine revolutions per day, the velocity is about 6.4 km/sec. The five-meter per second deviation is about 8×10^{-4} of this velocity. This produces about 1.6×10^{-3} relative change in the total energy which, in turn, produces a relative change of about 2.4×10^{-3} in the period. This is a change of some 23 seconds in the period, and is clearly too large a deviation from the desired period to be acceptable for the intended purpose.

The question may arise as to what accuracy of injection velocity would be required. One might arbitrarily require as a minimum acceptable condition that no more than four minutes deviation accumulate in a month. Hence, observation might

TABLE 2 - Distinctive Orbits for Geodetic Satellite

Period	Perigee radius	a	e	i (retrograde)
day	km	km		°
$\frac{1}{4}$	→	11,500	0	38.7
$\frac{1}{8}$	→	10,600	0	55.1
$\frac{1}{6}$	6,960	10,600	.34	63.4
$\frac{1}{9}$	→	9,800	0	65.2
$\frac{1}{10}$	→	9,100	0	70.1
$\frac{1}{11}$	→	8,500	0	74.2

be possible every night for a month, weather permitting. This is an accuracy of about 10^{-4} . Thus, the minimum requirement is on the verge of being possible with good guidance in the last stage of the vehicle. A correction maneuver with an auxiliary jet might be required.

In the examples of distinctive orbits discussed, the condition that the period be $1/n$ days (n , integer) imposes the most severe requirements on the launching and later correction procedures. The requirements imposed by the other conditions may also be estimated easily. The accuracy with which the desired initial position is attained must also be considered. The same relative error in r and v have about the same effect in the period. However, a given relative error requirement is more easily met in r than in v , essentially because the launch from the surface of the Earth begins with a large r .

The examples discussed are only a few of many possibilities. They are, at best, representative of the sort of calculation that must be made in more detail in designing any satellite for a particular objective. The conclusion is clear: the designs and plans for a satellite mission, because of geodetic effects on the satellite orbit, must draw heavily from results of contemporary geodesy.

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Discussion

Dr. A. B. Mickelwait—What about the angular control of the velocity?

Dr. Lundquist—I assumed in looking at this that the variation was a vectorial variation, adding to the velocity a little vector. The condition is not very much different.

Dr. Heinrich K. Eichhorn—This is something that Sterne could answer best. At what height will the Moon come in so it has to be taken into account? If the plane of the orbit is supposed not to change in one year and if you have to get rather large major axis then the moon comes in.

Dr. Lundquist—There are two comments to be made here. All of these calculations have to be done with considerable care. I did them rather roughly for illustration, particularly the high orbits. I can not answer your question offhand as to what altitude the Moon would have to be taken into consideration. The other comment; I think it is really doubtful to consider anything like a year. Something like a month is more reasonable.

Dr. Eichhorn—Could the Moon be used to sort of stabilize those satellites that are far enough off to be in the neighborhood of the libration points?

Dr. Lundquist—I have not looked into that.

Dr. Fred L. Whipple—Does anyone offhand know when the Moon perturbations become serious? (No response) I guess we are going to have the answer later.

There is one other kind of orbit that deserves mentioning. I am not sure if it is of practical value. There are two Lagrangian points in which

a body can be placed so that it will stay more or less in position along the Earth-Sun line. The point at maximum distance needs some added attraction from the Earth to balance centrifugal force. The points are about a million and a half miles from the Earth and to my knowledge nobody has calculated how to keep a body there. One would certainly have to apply some forces from time to time in order to keep a body in place.

From the floor—It has been suggested that we might get something on the Lagrangian Point in the lunar area.

Dr. Whipple—I would not say that I pondered it seriously but the solar perturbations are so large that they effect the Moon's orbit a great deal. The orbit would be extremely unstable. It is possible, however, that you would not have to introduce a great deal of work but I have a feeling you would have to make corrections perhaps monthly to stay within gunshot. Have you thought about it, Dr. Sterne?

Dr. Theodore E. Sterne—No.

Dr. Raymond H. Wilson—There are the points making an equilateral triangle with the disturbing mass. Certainly, on the Earth the Jupiter perturbation point would not stand up. You would also have a point, say, 60° from the Moon, and that might be reasonably stable.

From the floor—How many points to Lagrange are there, five?

Dr. Whipple—There is one the other side of the Sun making three in all. Then there are two triangular points.

Computations

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Since the subject 'Computations' is a broad field and many aspects have already been discussed in the previous four papers, I will remark on only two phases of the subject: (1) What from a geodetic nature can be computed from satellite orbits. (2) How do we go about computing these things.

We have seen from the paper of Miss Eckels that the dynamic flattening of the earth J can be determined. This, of course, is no surprise since the flattening has been determined from the Moon. It is a relatively simple problem to determine J by means of the regression of the nodes in a first-order perturbation theory but it becomes extremely difficult if one tries to achieve the greatest possible accuracy.

The standard method of determining J is to observe the node over a fairly long period of time and use a smooth curve to determine the rate of change. By using a long time base, one is able to ignore the periodic terms.

Jacchia has gone a step further and has determined values of J and K from two satellites using a second order perturbation theory by King-Hele, but there are other things that might enter into these calculations that are unknown now. For example, what effect will higher order terms in the Earth's potential have on the nodal regression. There may be terms which introduce changes in the rate of regression or introduce long-period terms which are not ignorable. It will also be necessary to determine to what extent the short-period terms are ignorable.

Looking at the first order equation for the secular motion of the node

$$\dot{\Omega} = nJ \cos i/a^2(1 - e^2)^2$$

we see that the motion is dependent on the elements a , e , and i which are subject to short-period perturbations and may be subject to long-period perturbations. So suitable mean elements will have to be determined which will give the proper rate. The rate is also directly proportional to the mean motion of the satellite, but which of the three mean motions is to be used?

I do not think the present theories are able to determine this.

As far afield as it might seem, we will have to consider the effect of the Sun and Moon on satellite orbits. Right now we have orbits of small semi-major axis, but, of course, for geodetic work these will have to be increased considerably to get away from the atmosphere. It is practically impossible to do any good geodetic work if we have to contend with the unpredictable atmosphere.

So when we put satellites out farther than the present ones we will expect luni-solar perturbations which are of considerable effect. There is a paper by Lyman Spitzer, Jr. which appeared in the *Journal of the British Interplanetary Society* in 1950. He has gone through this problem in a very rough manner. He first computed the ratio of the disturbing acceleration of the S to the primary acceleration for the Moon and for a satellite 500 miles above the Earth's surface. The ratio for the Moon is about 10^{-2} and for the satellite about 10^{-7} , so we see the solar perturbation of the satellite will be about 100,000 times less than the perturbation of the Moon.

Then using an equation of Hill, he computed the maximum deviation from a circular orbit that would be caused by the Sun and the Moon. This comes out to be about one meter, admittedly a small amount, but one which will increase with more distant satellites.

Spitzer also computed the regression of the nodes caused by the Sun and the Moon and arrived at a period of 2300 years for a 500-mile satellite. This is quite a long time compared with the 100-day regression period caused by the oblateness, but it works out to about one part in ten thousand and should be observable. As the satellites go higher the regression caused by oblateness will decrease as the 7/3 power of the mean motion and the regression from the Sun and Moon will increase as the first power of the mean motion, so the situation can get bad very quickly.

In addition to changing the secular rates, luni-

solar perturbations will introduce long-period terms in the elements of geodetic satellites which will not be negligible. All of these things will have to be investigated in quite a lot of detail before we can make fullest use of artificial satellites for geodetic purposes.

Continuing with things we can determine from satellites, we have heard several times that making intercontinental ties would be a very useful experiment. As a matter of fact, the actual space coordinates of the observing stations could be determined. We should be able to do this with about the same precision as the satellite observations. Two seconds of arc comes out to about 30 meters. I am sure that geodesists would be very pleased to have actual X , Y , Z coordinates on the Earth with respect to the center. Of course, it will be extremely difficult to attain this precision. We could measure the physical flattening f of the Earth from the station coordinates or perhaps by triangulation to determine the satellite heights. With high inclination satellites and high latitude stations quite accurate values of f could be determined and compared with the independently determined J to gain more knowledge of the internal equilibrium of the Earth.

Let us now talk about some things which cannot be computed from satellite orbits. I am quite sure that gravity anomalies cannot be determined. Since anomalies are associated with high inverse powers of r , their effect decreases very rapidly with distance. When a satellite is put above the atmosphere so that you will not have to worry about drag, the effects of the anomalies will probably be negligible.

Another difficulty with trying to determine anomalies is the fact that the individual anomalies act over a short period of time while the observations are made relatively infrequently so that there is insufficient resolution. Perhaps with lighted satellites and more stations, sufficient observations over short periods of time could be made to determine anomalies, but I think the sea-surface gravimeter that Ewing described is the logical method for determining anomalies. Then their effects on satellite orbits could be computed and, if necessary, corrected for.

Another quantity which cannot be determined from satellites is the geocentric constant GM . This quantity can be determined to about the same accuracy as the position of a satellite which is one part in one or two hundred thousand and

GM is already known to better than that accuracy. Perhaps a more accurate value of GM could be determined from observations of distant satellites with long focus cameras or by radar ranges of the Moon.

About methods of computing orbits, there are two which have been used by astronomers, known as general perturbations and special perturbations, but called by others simply perturbations or variations of parameters and numerical integration.

In general perturbations, the equations of motion are divided into two parts. The first part contains the main characteristics of the motion and is solvable analytically; the second part is small compared with the first and is solved approximately or by successive approximations if necessary. The sum of the two solutions then describes the motion of the body.

In special perturbations the equations of motion are integrated numerically to give the positions of the body at any time as a function of an initial configuration. Either the total equations of motion can be solved in this manner, or, as in the case of general perturbations, the departures from an analytically solvable part can be numerically integrated and the two solutions added together to produce the complete solution.

Special perturbations, as the name and method imply, has the great advantage that immediate insight into the character of the motion can be gained once the solution is accomplished. For example, the secular motion of the node or perigee, or periodic terms in the eccentricity can be seen immediately for any given set of orbital parameters. Computing time is significantly less, once the theory is developed, than for numerical integration.

On the negative side, we have the extreme difficulty of developing a perturbation theory which increases many-fold as one strives to increase the accuracy by going to higher and higher orders, and the practical impossibility of handling some kinds of perturbations such as air drag.

Numerical integration has the great benefit that all known effects can be included in the equations with great simplicity. If a general scheme for numerically integrating three simultaneous second-order differential equations is set up, putting in additional terms for new harmonics in the Earth's potential or the disturbing force of the moon would be practically as simple as writing the equation down.

Numerical integration can be made as accurate as one wishes by merely including sufficient figures in the calculations. For long integrations however, many more figures must be carried than the desired end accuracy due to round-off errors and it is difficult to assess properly the exact accuracy achieved.

The principal drawback to the use of numerical integration is the excessive computing time required. However, there have been some very heartening developments in computing machines lately and we can expect commercial computers within a couple of years which are on the order of 100 times faster than the IBM 704.

All things considered, I think numerical integration is to be preferred over general perturbations for very accurate geodetic computation from satellites. There will still be a place for general perturbations, of course, for daily operations concerned with tracking and predicting, and for gaining insight into various aspects of satellite motion. Finally I am sure general perturbation theories will continue to be developed and used if for no other reason than the fact that a well worked out theory is much more aesthetically pleasing than the mechanical process of numerical integration.

Discussion

Mr. Myron Lecar—I might point out some of the problems we had in Project Vanguard. For 1958 β 2, with an arc of about six days, we were able to get the average residual down to about one or two milliradians. When we extended the arc to 90 days, the residuals went up to about 15 mil. However, with the corrections to the P_4 coefficient, we brought the residuals down to $4\frac{1}{2}$ mil. It is interesting that we were not able to bring the residuals down to the one or two mil we had on the shorter arcs. This in part might be due to the nature of the perturbation solution. There is a minimum error caused by the asymptotic nature of the expansions. However, over the short arcs, we were forced to use every observation; we threw out only the very poor ones. Over the 90-day arc, we kept only those observations within five degrees of the observers zenith. So, one would not expect an increase in the residuals because of refraction errors. We seem to be up against determining the higher order moments in the potential. The old moments, that is, the terms that would change sign as one crossed the equator, and the longitude dependent terms. Unfortunately these are all of the same order: the order of K , the coefficient of P_4 .

With the method of general perturbations currently programmed at the Vanguard Computing Center we could only handle the even moments. We could not handle the longitude-dependent terms nor those that changed sign with the latitude. We estimated that it would take 50 hours to do one numerical investigation

(double precision) on the IBM 704. Of course, we would have to determine a number of orbits and perform a number of differential corrections.

This is the point at which the geodesist will have to call on the astronomer for help. We had access to an excellent German paper by Krause with the translated title *The Secular and Periodic Perturbations of an Artificial Earth Satellite*. This carries the secular terms in $\dot{\Omega}$ to the order K but does not include terms of the second order in J . Just recently, we received King-Hele's paper which carried Ω to second order in J . As far as I know, there is no analytical solution currently available which tests the longitude dependent term. That is where the problem stands now.

Dr. Luigi G. Jacchia—The atmospheric fluctuations are quite irregular. They amount to something like 100% of the value of the acceleration but occasionally go up to 400% so I am actually surprised that you got what accuracy you did. In other words, there is no way of depressing the residual beyond a certain minimum. It is useless to go to terms of high order because they would be much smaller than the atmospheric irregularities. There is a limit of accuracy beyond which one cannot go.

The shortest fluctuation which can be detected are of the order of three to five days and occasionally there would be bursts with rapid increase, one of which occurred at the end of August, when there was an increase in the acceleration by a factor of four in two weeks.

Both in 1957 β and 1958 β , there have been

observed changes in the inclination and a rough calculation made on the basis of Sterne's theory of the effect of a rotating atmosphere on a nearby satellite shows that the change in the inclination is too large by something of one to two orders of magnitude because of the rotation of the atmosphere. The change in the inclination of 1957 β was in the order of 0.3° . There is a third satellite, 1958 δ , the one that is coming down tonight (December 2, 1958), which had a change of inclination of nearly a half degree from the beginning to end of its lifetime. The Vanguard satellite has an inclination that decreases rather regularly 0.0001° per day. There must be something else besides atmosphere to reduce these affects.

Dr. Charles A. Whitney—Does the rate correlate with anything?

Mr. Lecar—It depends in part on the method of computation. One thing, if you have an incorrect flattening you are swinging the orbit around at the wrong rate and the errors build up.

Dr. Jacchia—The best way out is to recompute the orbit. The change of inclination I am speak-

ing of, that of 1957 β , comes out of orbit computations by King-Hele and his associates in England, over very short arcs, single transits, so they are completely foolproof.

Dr. Fred L. Whipple—When is 1958 δ 1 coming down?

Dr. Jacchia—Nine o'clock tomorrow morning (December 3, 1958).

Mr. John Ruttenberg—In answer to the earlier question, I did try the calculations as the result of the 1957 β to see where the inclination of the ellipse was and it gives the right direction and right mathematical change but also falls short by one or two orders of magnitude.

Dr. Raymond H. Wilson—It was mentioned that you cannot get them accurate to within a few minutes of arc. Those are all Minitrack observations, and, while we theoretically know the precision is not as great, are the residuals from an orbit smaller?

Dr. Lautman—We do not have any results. The reduction program has just recently swung into high gear and we still do not have enough observations reduced to this high precision.

Space Navigation in the Solar System

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It is difficult to discuss a topic such as space navigation in the solar system when such navigation exists only in theory at present. Guidance systems cannot be tested in outer space until appropriate vehicles are available. It is possible that vehicles with workable guidance systems based on the principles of inertial navigation as they are now known will be developed in the near future.

Inertial navigation and geodesy complement each other, but there are two important differences in approach. First of all, the navigator does not generate information, he uses it. Secondly, the navigator does not need to operate with as great a degree of accuracy as does the geodesist.

Navigation, involving essentially the control of motion of a vehicle, would continue under the same definition whether used in space or near the Earth. Navigation first assumes a vehicle which is composed of a frame, has power to move it, an environment in which to operate, and guidance and control to make it go where it should. In general, the equipment needed for the on-board operation is available now. However, the value of such a guidance system cannot be ascertained until a proven vehicle in which to place it is available.

The first navigational problem is the mid-course, or guidance between the celestial bodies in the solar system. Because the first space vehicles will be greatly underpowered, it will probably be necessary to utilize the minimum energy orbits which have already been worked out for manned or unmanned vehicles. As more power becomes available, the navigator may select more direct paths. The navigation problem in shooting essentially ballistic missiles around the Moon or Mars is the same problem, magnified tremendously, as shooting a stone from a sling. The velocity vector must be aimed very accurately. Here the navigator is going to be very much interested in what the geodesist tells him about the surface of the Earth. A practical aid would be a corrected ballistic path when the firing is near the surface of the Earth and then a vernier control to compensate for the improbability of suffi-

ciently accurate aim at a target 35,000,000 miles away.

Another problem is that of getting off or landing on a planetary body with a manned vehicle. Various landing techniques will be utilized. Any information from the ground or radar measurement for distance would be of direct interest to the navigator. Again some knowledge of geodetic data would be helpful. The problem here is maintaining an orbit, whether for astronomical observation, for the jump off point to another planet, or for making controlled contact by a non-catastrophic impact. This problem has been solved, basically, in other situations such as the so-called collision course in aircraft or missile fire control, or the automatic landing of aircraft. In such control situations, the condition of looking directly at the destination with an instrument has become a problem of determining the rotation of the line similar to the radius vector of the satellite in relation to the Earth. The environment is different, but the problem and the instrumentation for its solution are essentially the same.

The unit on which the solar system is measured is more a problem for the astronomer than the geodesist. The various elements of the solar system are quite well known in terms of the astronomic unit. The equipment that can be placed in today's vehicles would not be entirely suitable for accurately measuring the astronomic unit. However, measurement will be possible in the near future.

In setting up his course, the navigator will depend heavily upon information from the geodesist on the Earth and the astronomer away from the Earth. Once the trajectory or orbit pattern is set up, the navigator must make on-board measurements and verify them with a computer. A sufficient degree of accuracy can be maintained in this case with the equipment now available. J. H. Laning, Jr. and others, of Massachusetts Institute of Technology, presented a paper in the spring of 1958 on a photo-reconnaissance of Mars which appeared to be practical. Acceleration measurements under the circumstances of one

part in ten thousand were discussed. Measurements to such a degree would not satisfy a geodesist, but to a navigator they are good. As the net force in an orbit is zero, the question logically arises as to the meaning of measurements made with an accelerometer. In this case the acceleration measurements give the effect of forces other than gravity. During the time of application of thrust, which is when the orbit is changed, the computer determines the relationship between actual and intended paths.

Heliocentric star-oriented frames for navigation would be logical for solar system work. A replica can be carried in a vehicle in the form of a gyroscope which can give random drift rates of a fantastically small value. It may actually do a great deal better during the orbital transit of a vehicle to Mars or the Moon than it does in vehicles around the Earth. One of the causes of drift is the microscopic shift of mass in millionths of an inch. The resulting drift is due to the effect of the gravitational-force field of the Earth. In

an orbit such a force will not exist. It is not known how well the gyroscope will operate in such a circumstance, but it looks like there will be no loss of a star reference.

Optical measurements will undoubtedly be the principal navigational means for piloting from within the vehicle. The stars and planets used for the tracking references will always be available for such measurements. Equipment now available should not have any trouble operating outside of the atmosphere. It is assumed that radiation and other forces destructive to the navigational system will not be encountered. The main considerations here are man's ability to survive in space, adequate thermal insulation and protection against radiation so that the materials used in the navigating equipment retain their desired physical and chemical properties.

The space navigator is very grateful for the findings of the geodesist, but in the future he is very likely to unearth many new problems for the geodesist to solve.

Discussion

Dr. Fred L. Whipple—I am impressed with the accuracy needed in calculating the velocity. An error of one foot per second in launching gives an error in apogee at the Moon of a thousand miles. We do not know the astronomical unit well enough to hit Mars.

Dr. Wrigley—It is difficult to aim with complete accuracy, but the course can be adjusted in flight.

Dr. Raymond H. Wilson—How about artificial asteroids? Would they help for this discussion?

Dr. Whipple—That is a thought. When we have enough excess in energy and equipment to send one out in a long orbit and can observe it every time it comes around, we might get some useful results.

Dr. Wilson—That would be geodesy for the solar system. I do not know the Greek for it.

Dr. Whipple—We had probably better locate one of the small ones that comes near the Earth and put the equipment on that.

Dr. Roman K. C. Johns—I wonder if there exists a hope that increased knowledge of geophysical phenomena in space may provide a new physical principle of navigation, which we cannot apply on or close to the Earth. I am thinking in particular about radiation.

Dr. Wrigley—What sort of radiation?

Dr. Johns—I have in mind cosmic and solar radiation and possibly ionospheric density. They could provide a new reference, and supply information about our position with respect to a celestial body. As parallels may be given, the application of barometric air pressure is used as an aid to determine the height above sea level.

Dr. Wrigley—The navigator would be well advised, as he has been in the past, to make use of any barometric air-pressure information. Whatever is measured must be related to the navigator in terms of the local information which identifies the navigator or, more probably, the reference frame in which he wishes to navigate. The barometric altitude is useful because a reasonable statistical relation holds between height and pressure. If this relationship did not exist with any sort of regularity then the change in pressure of the atmosphere would be of little interest to the navigator.

Mr. Myron Lecar—I wonder, with the new Maser amplifier techniques, if it is presently feasible to use radio navigation using the stars as sources?

Dr. Wrigley—The on-board material now available is probably satisfactory for solar sys-

tem navigation. The problems are the frame, the power plant, and an understanding of the environment. In flight, wings are used to pass through the dense atmosphere first encountered. Speed becomes more important for propulsion through the less dense atmosphere which is eventually considered as individual particles. The problem of a frame, propulsion, and a proper environment, holds throughout the flight.

The guidepost for interplanetary navigation is similar to the simple problem of taking a sailboat around a lake by seeing the lighthouse. The problem is to make the vehicle go where it should with adequate power.

Some basic reference is always needed for navigation. The stars can provide this reference for solar system travel.

Mr. Arthur S. Cosler—What about the interplanetary inertia?

Dr. Whipple—The solar parallax seems to be between 8.790 and 8.800. Values are given with an uncertainty of one part in 10,000 but the uncertainty is greater. The value is not good to one part in ten thousand. It is better than one part in a thousand.

Dr. Charles A. Lundquist—What is the uncertainty of the mass of the Moon now?

Dr. Whipple—I will be glad to write down estimates. We have 1/81.27.

Dr. John A. O'Keefe—81.45.

From the floor—81.31.

Dr. Whipple—That is the Moon/Earth ratio.

Dr. A. B. Mickelwait—Most people when they talk of accuracies are talking of a minimum ellipse. We have here a minimum-energy orbit and if one considers orbits with energies not much different from this he can find conditions where the guidance requirements are different by orders of magnitude. They are no longer ridiculous but require perhaps 50 feet per second control in velocity. So you have to be very careful of what type of orbit you are talking about. It depends on the take-off velocity and other burnout conditions.

Admiral Paul A. Smith—You mean you can ignore the uncertainty?

Dr. Mickelwaite—You cannot ignore it completely. If you look at the velocity required just to impact the Moon versus the take-off velocity you will find that between minimum energy and something of the order of 36,000 feet a second (if I am taking off at 200 miles altitude) there is a change from ten feet per second to 150 feet per

second in allowable dispersion. It is a little dangerous to make your calculations at one velocity only.

Unfortunately on lunar flights you have to make calculations over a range of 500 feet per second to get the whole picture.

Dr. Nicholas T. Bobrovnikoff—I wanted to comment on an aspect of space travel that has nothing to do with geodesy. It is the problem of Lagrangian points. In the October 1958 issue of *Astronautics* there is an article by two people from Boeing Aircraft in which the problem of Lagrangian points and effect on the Earth and Moon are discussed. There are five points more or less the same distance from the Earth, two of which are triangular. The question is whether they are stable. One cannot say at the present time whether they can be stable in the Earth-Moon system. I know quite a bit of interest has been expressed in them. What is the value of these points?

Dr. Mickelwait—A radio beacon can be put on each one and used for a long base line for radio navigation for interplanetary space. Actually it could be quite an accurate system.

Dr. Bobrovnikoff—Is it being seriously considered?

Dr. Mickelwait—I am merely postulating one use for putting something at such a point.

Dr. Whipple—Dynamically these points are like a small sphere balanced on a larger one. You do not have to do much work to keep it there. It is not going to remain in place but if you keep correcting often enough you can keep it in position. Now putting in an eccentric orbit is like moving the big sphere about in a known fashion and then applying a force to keep the small ball on top. Jugglers can do this sort of thing and it can be done in space, but energy and equipment will be needed to maintain a body in a Lagrangian point. I do not know about the lunar triangular points. I doubt if they are stable. People have searched enough without finding any satellites there.

Prof. Arthur J. McNair—Much has been said about the effect of the Earth's atmosphere on satellites and, of course, the atmospheric effect is obvious. What is the present thought concerning any magnetic effect out in outer space on these satellites?

Dr. Wilson—The magnetic topic has interested me. All the satellites for which any rotation has been determined seem to be slowing down at the

same order of rate. For the Sputnik III rocket, *Sky and Telescope* has published a photometric study which indicates the changing rate of rotation. The Vanguard satellite rotation has been studied by radio, using its dipole aspect variation. The rate of damping is in the same order for both. It was predicted that rotational damping by the magnetic field of the Earth should be a much higher rate. But the present rate could be explained by an effective field out there of about one quarter or one fifth of what was expected.

I was wondering when you mentioned the secular inclination change whether that is not probably caused by the same thing.

Prof. McNair—That is what I questioned.

Dr. Theodore E. Sterne—The forces are so trivial that one is ashamed of himself for having looked into them. They depend on changing magnetic flux. When one enters a region of different magnetic intensity there is a change in magnetic flux and the resulting electric currents can be calculated. They are awfully small currents. Rotation induces electric currents that exert damping torques on the satellite, but the currents caused by changing flux cause very little force. As I remember it was less than one part in 10^{19} of gravity—one part in a million million, which was trivial. It was less than that. It might have been one part in 10^{18} .

Prof. McNair—Would it be fair to say then that this magnetic effect simply damps the rotation of the satellite around its own axis, like the little sphere with the surface reflectors described earlier, but does not damp the rotation of the satellite around the Earth?

Dr. Sterne—Yes.

Mr. Lecar—The satellite does build up a static charge. There is an electromagnetic drag similar to the action of the atmosphere. This has been tested in a paper by Jastrow and Pearse. I remember that this effect was negligible for the satellite 1958 β 2.

Dr. Whipple—I can give observed numbers on the charge. The Russian numbers were minus two volts at an altitude of 200 odd kilometers and minus six volts at an altitude of 700 odd kilometers. I have made some calculations myself for space using photoelectrons and assuming that there is a fairly quiescent space density of protons and electrons in equilibrium at a temperature of a half million degrees. At the Earth's dis-

tance away from the Sun the charge could possibly build up to 50–100 volts negative. Probably the value is only a few volts negative. If the surface is selected to be a good photoelectric emitter the charge can be a volt or two positive in sunshine. The charge would become negative in shadow.

Dr. Sterne—You can calculate the force caused by the motion of that charge in the Earth's magnetic field. Let's multiply the Russian figure by 100; then you come out with 1000 volts. You get a force of four ten-thousandths of a dyne. You have four parts in 10^{11} of the weight. That seems to be my recollection.

Dr. Whipple—You are using a high voltage; you go back to ten volts and you are close to 10^{-12} .

Mr. John Ruttenberg—The charge is so small that for any sizeable body it is small.

Mr. Claude F. Gilchrist—Getting back to your reference system I think it would be interesting to the geodetic people to know the reference system we will probably use in outer space for the positioning of space ships. We cannot use celestial latitude and longitude since the vernal equinox is inherent to the position of the Earth and would be of little use when we are millions of miles from the Earth. We then must adopt another zero point for outer space. I just wondered how much thought you had given to this.

Dr. Wrigley—The reference would probably be Sun-centered with a major star oriented reference. The possibility of interstellar travel is not discussed here.

Mr. Gilchrist—One suggestion would be to adopt the ecliptic as the plane of reference and the star Leonis as the zero point. Leonis is only slightly inclined to the ecliptic and satisfies the condition for a fixed point.

Prof. Milton O. Schmidt—I shall be very brief because I know many of you have plans for early departure from the city.

As spokesman for the organizing committee, I wish to express our thanks to those who have responded so enthusiastically to our invitation. We are extremely grateful to the various panelists and participants. We invite your comment regarding this conference and your suggestions for future ones. I ask that you send such comments and suggestions to Waldo E. Smith, Executive Secretary, American Geophysical Union, 1515 Massachusetts Avenue, N. W., Washington 5, D. C.

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Number 3, Atmospheric Chemistry of Chlorine and Sulfur Compounds—(Publication 652, National Academy of Sciences—National Research Council); Library of Congress Catalogue Card No. 59-60039; about 110 pp., 1959, 7" x 10" **\$5.50**

Based on a symposium held jointly with the Robert A. Taft Sanitary Engineering Center of the U. S. Public Health Service in Cincinnati in November, 1957. Contains 23 papers (some as summaries) with discussion. Preface by James P. Lodge, Jr., (Editor) of the Taft Sanitary Engineering Center and Chairman of AGU's Committee on Chemistry of the Atmosphere.

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