The Rotation of the Earth and Polar Motion

REPORT ON THE SECOND GEOP RESEARCH CONFERENCE

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The Second GEOP Research Conference was attended by about 80 persons, 34 invited and 46 observers. The conference was opened by W.H. Munk, general chairman of the conference, followed by M.G. Rochester of the University of Newfoundland, who delivered the keynote address. The keynote address in its entirety is printed on page 769 of this issue. This address and the following summaries of the sessions constitute a report on this conference.

This report was prepared by W.M. Kaula, Kurt Lambeck, Wm. Markowitz, Ivan I. Mueller, and D.E. Smylie. Material contained herein should not be cited.

First Session
Panel on Observations and Coordinate Systems
Chairman: Wm. Markowitz (Nova University)

Members: R. Anderle (U.S. Naval Weapons Laboratory), P. Bender (Joint Institute for Laboratory Astrophysics), C. Coursenman (MIT), B. Guinot (BIH), P. Melchior (Observatoire de Belgique), J. Ramanastray (NASA, Goddard), D.E. Smith (NASA, Goddard), T.C. Van Flandern (U.S. Naval Observatory), G.M.R. Winkler (U.S. Naval Observatory)

The opening session was concerned with observational methods used to determine variations in speed of rotation of the earth and polar motion. The chairman, Wm. Markowitz, remarked that, in addition to the classical optical methods (e.g., use of the photographic zenith tube (PZT), astrolabe, and zenith telescope), techniques had been developed that use Doppler shift, laser ranging of the moon and of artificial satellites, and very long base line radio interferometry (VLBI). Since all these methods had been described during the past few years at symposiums and in journals, the session would be devoted mainly to giving accuracies of measurements recently obtained or expected shortly. He noted that the fundamental polar path, to which other pole paths are referred, has been determined since 1900 by a few stations on a common latitude, designated the International Latitude Service (ILS). Reductions are currently made by the International Polar Motion Service (IPMS) at Mizusawa. The results have been systematically of errors such as those in star positions or in reduction constants. The pole path determined by the Bureau de l’Heure (BIH) at Paris is based on time and latitude observations of a large number of stations.

B. Guinot reported on the accuracy of measurements of the coordinates of the pole and of universal time as obtained by the classical astronomical methods. Two international services are involved in the determination of the rotation vector of the earth, the IPMS and the BIH. Table 1 lists their chief areas of activity in 1973. Table 2 lists the error budgets in the coordinates of the pole, and Table 3 lists the error budget in UT1.

G.M.R. Winkler reported that a new PZT with a 65-cm aperture (20 cm is usual) is scheduled for completion in 1974. The instrument will allow numerous experiments to be performed that will improve the accuracy of PZT observations.

R. Anderle reported on the path of the pole obtained from analysis of Doppler observations made since 1969 of Navy navigational satellites. Sporadic results had been obtained for the period 1965–1969. Currently, pole position is computed on the basis of observations made on at least one satellite during each 48-hour interval. The random error in the range difference (from Doppler integration) is typically 10 cm, which yields a theoretical precision of 30 cm in the latitude of a receiver observing a polar satellite at an elevation greater than 10°. However, uncertainties in the evaluation of the earth’s gravity field produce errors in the computed satellite position of 3 meters amplitude. The fundamental period of the error is about 2
### TABLE 1. Time and Polar Motion Services

<table>
<thead>
<tr>
<th>Sources of observational data</th>
<th>IPMS/ILS</th>
<th>BIH</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw results</td>
<td>x, y*</td>
<td>x, y; UT1-UTC†</td>
</tr>
<tr>
<td>Smoothed results</td>
<td>x, y†</td>
<td>x, y; UT1-UTC†</td>
</tr>
<tr>
<td>Program</td>
<td>Same stars observed; no systematic error due to star position errors.</td>
<td>Different stars observed; errors due to star position errors statistically reduced.</td>
</tr>
<tr>
<td>Common sources of error</td>
<td>Local drifts of the vertical; instrumental errors; random errors.</td>
<td>Local drifts of the vertical; instrumental errors; random errors.</td>
</tr>
</tbody>
</table>

The BIH uses the data of new techniques, but a purely classical solution is maintained for comparison purposes. The subsequent data were obtained with the latter.

* Monthly values.
† Values every 0.05 year.
‡ 5-day values.

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### TABLE 2. Error Budget in x or y

<table>
<thead>
<tr>
<th>Error Source</th>
<th>Error (One Sigma)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>IPMS/ILS</td>
</tr>
<tr>
<td>Systematic errors in drift</td>
<td></td>
</tr>
<tr>
<td>Errors of the proper motion of stars, cm/yr</td>
<td>0</td>
</tr>
<tr>
<td>Local drifts, cm/yr</td>
<td>2</td>
</tr>
<tr>
<td>Annual errors</td>
<td></td>
</tr>
<tr>
<td>Errors of star positions, refraction, cm</td>
<td>30</td>
</tr>
<tr>
<td>Random errors (instrument, refraction, observers, etc.)</td>
<td></td>
</tr>
<tr>
<td>5-day mean, cm</td>
<td>40</td>
</tr>
<tr>
<td>1-month mean, cm</td>
<td>100</td>
</tr>
<tr>
<td>1-year mean, cm</td>
<td>60</td>
</tr>
</tbody>
</table>

The above values were obtained in most cases by a study of the residuals of each series of latitude relative to the evaluated results. Data are given in centimeters at the surface of the earth; 1 arc second ≈ 30 meters. The effect of local drifts on IPMS/ILS data is subject to controversy. Note that the random errors are not a white noise. They are not likely to be reduced by averaging errors more than 1 year.
for the conventional longitudes.

Random errors
- Errors in star positions, refraction, sec: 0.0013
- 5-day mean, sec: 0.0020
- 1-month mean, sec: 0.0014
- 1-year mean, sec: 0.0010

Estimates were made as for Table 2. A constant error results from the adopted values for the conventional longitudes.

hours, but the error includes many superimposed periods of 24/m hours (m = 1, 2, 3, ..). Therefore the actual error in the latitude determination of a receiver based on a single satellite pass is no better than 3 meters. However, since 20 receivers are used and since each receiver observes 4–12 satellite passes per day, the pole position is determined to an accuracy of 60 cm on the basis of each 48 hours of data. During the latter half of 1972, pole position was computed on the basis of observations of three satellites, the standard error of the mean being 22 cm for a 5-day average.

D.E. Smith described preliminary results on variation of latitude obtained from satellite laser ranging. Observations made at several stations well distributed in longitude would provide the motion of the pole. He reported that the present capability of laser ranging to satellites for the determination of polar motions is 1 meter in an interval of 6 hours, as demonstrated in an experiment in 1970 conducted by NASA Goddard Space Flight Center. A single laser tracking station observed the variation in its latitude arising from polar motion over an interval of 5 months, described in Science (170, 405–406, 1972). In essence, the technique uses the orbit of a satellite as its external reference system from which the variation in latitude is determined.

Current limitations of this technique lie in our ability to account for the observed perturbations of the orbit (the reference system). These perturbations are due principally to the higher harmonics of the earth’s gravitational field and to a lesser extent to the earth tides. Present (1972) instrumental capabilities probably limit the technique to about 25 cm from 6 hours of data. However, with the use of future satellites in higher orbits the problem of the gravity will largely be removed, and the limitation will then probably lie with the earth tides (particularly the oceans) and our ability to compute the perturbations of earth albedo radiation pressure. Instrumentation capability, which will be 10 cm in range accuracy in July 1973 and about 3 cm in 1974–1975, is not expected to be a major factor. Consequently, it is anticipated that by 1975 laser ranging to satellites will permit the variation in latitude of a tracking station to be determined to about 10 cm in about 6 hours. By 1978, this value will probably be reduced to 2–3 cm.

Peter L. Bender reported on the use of lunar range measurements for determining polar motion. Measurements with an accuracy of 15 cm are currently being made by the McDonald Observatory of the University of Texas three times per day on about three quarters of the days during the month, weather permitting. A lunar ephemeris and a set of topocentric correction parameters for the problem now exist that fit the observations within about 4 meters over a period of 2.5 years. The lack of a better fit is believed to be due to uncertainties in the lunar libration angles; improved calculations that should remove this problem will be completed before the end of 1973.

A new lunar ranging station in Hawaii with an accuracy goal of 2–3 cm is being constructed by the Institute for Astronomy of the University of Hawaii. The use of a Nd:YAG laser with a 200 ps, pulse length is planned. Four to six additional stations should be in regular operation in other countries by 1975. It should then be possible to begin the regular determination of polar motion and fluctuation in the earth’s rotation rate at intervals down to 1 or 2 days. The accuracy achieved is about that of the basic range measurements.

Charles C. Counselman III reported on the use of VLBI as a potentially important technique for measuring the speed of rotation of the earth, polar motion, and, therefore, precession and nutation. Measurements are made with respect to extragalactic objects that define an excellent inertial frame. Technical improvements expected during the next 2 years should enable measurement uncertainties to be obtained in all three coordinates equivalent to a few centimeters of displacement at the earth’s surface. Delay measurements between stations 5000 km apart expected in the MIT-Haystack VLBI group with a group from NASA Goddard Space Flight Center and the University of Maryland gave a scatter of 0.1 nsec rms, equivalent to 3 cm of displacement, over 6 hours of observations on August 29, 1972.

Separate determinations of UT1 and pole position have not yet been made. However, from four experiments made in 1969 and 1972 it is deduced that the errors in value published by the U.S. Naval Observatory must be smaller than 3 meters, or 0.1 arc second.

Methods are available to enable the effect of the earth’s ionosphere (about 10 cm) and neutral atmosphere (variable by 40 cm) to be eliminated with an estimated uncertainty of less than 5 cm.

Ramsey B. Slopianka discussed modern VLBI experiments carried out by NASA Goddard Space Flight Center and the Smithsonian Astrophysical Observatory (SAO), which included the first measurement of the rotation of the earth through use of VLBI. This was made possible through the availability of (1) hydrogen masers with a frequency stability of 1 part in $10^7$, (2) a catalog of quasar positions with an accuracy of 0.5 arc second, and (3) reasonably accurate estimates of tropospheric and ionospheric effects. The stations at Agassiz, Massachusetts (SAO, 84-ft dish), and Owens Valley, California (Cal Tech, 130-ft dish), operated in the C band (4995 MHz) during February and March of 1972. The mean residual in the time determined, essentially UT1-AT, for 24 hours, was 2.8 nsec (to be regarded as a zero point correction) over an interval of 12 days. This interval was too short to detect any variation in speed of rotation. The rms deviation was 500 nsec which should be reduced as the technique is refined. It is planned to measure both UT1 and polar motion from observations of both quasar sources (at S and C bands) and water vapor sources (at K band).

T.C. Van Flandern discussed modern determinations of the secular acceleration of the earth (and moon). The secular accelerations of the moon’s orbital motion and of the earth’s rate of rotation are closely linked. The accelerations obtained have been changed by large amounts several times in the last 200 years. The earliest
derived value for $\Omega_M^*$, the centennial rate of change of the moon's mean motion per century, was -20°/century$^2$, determined about 1780, before it was realized that the earth's rotation was also varying. Modern determination began with H. Spencer Jones's 1939 value for $\Omega_M^*$ which is -22°/44/century$^2$. Attempts to account for this variation in terms of tidal exchange of angular momentum from earth to moon have not yet succeeded. All determinations of $\Omega_M^*$ for the last 5 years are nearly twice Spencer Jones's value: -52°/21/century$^2$ by Van Flandern, -40°/by L.V. Morrison (using both occultations and the periodic time scale for about 15 years), -38°/60/century$^2$ by C.J. Cohen and C. Oesterwinter (Spencer Jones's method, but modern observations), and R.R. Newton's values for ancient observations of -42° at 200 B.C. and -44° at 1000 A.D. Hence, if we assume Spencer Jones's value to be ruled out by the more recent determination, there is no evidence for any change in $\Omega_M^*$ over the past 30 centuries.

Second Session
Panel on Seismic and Meteorologic Effects on the Earth's Rotation

Chairman: K. Lambeck (Groupe de Recherches de Geodesie Spatiale, Observatoire de Paris)
Members: K. Aki (MIT), M.A. Chinnery (Brown University), F.A. Dahlen (Princeton University), R.A. Haubrich (University of California, San Diego)

One of the outstanding problems of the earth's rotation concerns the nature of the Chandler wobble, the earth's free nutation with a frequency of 0.85 cycle/yr. As the earth is not a perfect elastic body, any such nutation will be damped and will cease to exist after a suitable time interval. Evidence of damping of the Chandler motion comes directly from the broadening of the spectral peaks centered at the Chandler frequency. However, the Chandler model has been observed for more than 70 years, longer than the generally accepted damping time, and there must be some mechanism regenerating the motion. Two such mechanisms, that have been variously proposed are of meteorological and seismic origins.

If the changes in the atmospheric inertia tensor are not purely seasonal, there could be sufficient power in the annual line in the inertia frequency spectrum to sustain the Chandler wobble, as was first suggested by Volterra in 1885 and further detailed by Jeffreys in 1940. The study of Munk and Hansen (Geophysical Journal, 4, 339, 1960), however, indicated that this mechanism failed by 1 or 2 orders of magnitude. At present there is no evidence for revising these conclusions.

The alternative and attractive hypothesis is that seismic activity is responsible for a long history. Kelvin in 1876 (Mathematicat and Physical Papers, 3, 332, 1890) speculated what the effect of sudden changes in mass distribution would be on the as then still unobserved polar motion, Munk and MacDonald (The Rotation of the Earth, Cambridge University Press, 1960) ruled out this possibility as being too small by several orders of magnitude. Peixoto's work (Journal of Geophysical Research, 70, 2395, 1965), however, indicated that the dislocation fields associated with earthquakes were much more extensive than had previously been recognized. Mansinha and Smylie (Journal of Geophysical Research, 72, 4781, 1967) recognized that the changes in the earth's inertia tensor would in consequence also cease to exist after a suitable time interval. Evidence of damping of the Chandler wobble, the earth's free nutation, shows that these solutions agree with one another and with the earlier Smylie-Mansinha calculations to within 10%, except for a special case in the latter. Dahlen and Chinnery indicated that recent unpublished studies by Israel, Ben-Menahem, and Singh, by Saito, and by Dahlen argue that available excitation is too small by an order of magnitude and that these solutions agree with one another and with the earlier Smylie-Mansinha calculations to within 10%, except for a special case in the latter. Dahlen then considers that the mathematical problem of estimating the changes in the inertia tensor resulting from earthquakes is solved, although a number of interesting problems remain.

M.A. Chinnery stated that he had estimated the polar shift that Dahlen's calculations would give for a vertical fault model for the 1964 Alaskan earthquake and found it to be an order of magnitude smaller than the polar shifts deduced by the above authors for dipping faults. He suggested the following possible causes for the differences: (1) the nature of the core-mantle boundary conditions assumed, (2) the choice of the equations of state in the fluid core, (3) the analytical representation of the seismic source, (4) the earth model used, (5) the methods used for estimating the cumulative earthquake effect, and (6) numerical error.

D.E. Smylie and L. Mansinha in their 1971 paper give the first static treatment of the core-mantle boundary conditions, which do not require the Adams-Williamson law to hold throughout the core. Certain aspects of this treatment now appear to be generally accepted, but in any case both Dahlen and Chinnery doubt that the inertia tensor is sensitive to the treatment of the core, since the flexibility of the core as a whole is small in comparison with the inertia of the total earth. Smylie thinks that the anomalous result that Chinnery finds for vertical fault models is due to Dahlen's use of dynamic boundary conditions at the core-mantle boundary. Chinnery doubts whether the free oscillation variable $\psi_r$ is continuous at the core-mantle boundary, and argues that the solution is sensitive to the choice of mantle model (Chinnery was uncertain to what degree).

In particular, he reported calculations that showed that the inclusion of a thin soft model, but Smylie indicated that it must be in order for the normal component of the solenoidal vector (of which $\psi_r$ is the radial coefficient) to be continuous across the core-mantle boundary. According to Chinnery, the Smylie-Mansinha excitation is larger than that computed by others using the same set of fault parameters for the 1964 Alaskan earthquake. If it is presumed that Smylie, is correct only for vertical faults. The results are, however, in agreement for the 1964 Chilean event and the 1964 Alaskan earthquake event for a dipping fault model. Smylie has some doubts whether Chinnery in fact used exactly the same parameters in his comparison and indicated that the solutions can be very sensitive to the source parameters.

Smylie pointed out that the solution is sensitive to the choice of mantle model (Chinnery was uncertain to what degree). In particular, he reported calculations that showed that the inclusion of a thin soft model, but Smylie indicated that it must be in order for the normal component of the solenoidal vector (of which $\psi_r$ is the radial coefficient) to be continuous across the core-mantle boundary. According to Chinnery, the Smylie-Mansinha excitation is larger than that computed by others using the same set of fault parameters for the 1964 Alaskan earthquake. If it is presumed that Smylie is correct only for vertical faults. The results are, however, in agreement for the 1964 Chilean event and the 1964 Alaskan earthquake event for a dipping fault model. Smylie has some doubts whether Chinnery in fact used exactly the same parameters in his comparison and indicated that the solutions can be very sensitive to the source parameters.
Brune, based on a $\omega^2$ dependence of the source spectra, and by Aki, who assumes a $\omega^2$ dependence. According to Aki, recent comparisons of these models with accurate determination of seismic moment appear to support the $\omega^2$ model, although there are some discrepancies when this model is applied to the magnitudes given by Gutenberg and Richter. In some cases this discrepancy appears to be due to an overestimation of the Gutenberg-Richter magnitude. Brune emphasizes the necessity of investigating old seismic records to obtain more reliable estimates of seismic moment for the largest earthquakes. This result is in agreement with the Sanriku earthquake of 1933, whose correct magnitude is 8.3, equal to that of the 1964 Alaskan event. According to Kanamori, however, the observed seismic moment of the law for the 1964 Alaskan earthquake had the largest $M_p$ and that the Sanriku earthquake of 1933 had the ninth largest $M_p$, during the interval 1923–1964. Aki’s results, obtained with Dahlen’s observation that at least 10 Alaskan-sized earthquakes per year are required, do not support the theory of seismic excitation of the Chandler motion. Smylie disagrees with this last statement.

Mansinha and Smylie in their 1967 paper presented a study of cumulative effects based on the theory of random walks and using the Tocher-Press fault length-magnitude relation. In their 1971 paper they used this theory to estimate cumulative effects based on their calculations for the 1960 Chilean and the 1964 Alaskan earthquakes. The Tocher-Press law and Aki’s law give comparable cumulative effects, but Brune’s law gives effects about an order of magnitude smaller, as pointed out by Dahlen. Dahlen in 1973 performed Monte Carlo random walk experiments using Brune’s moment-magnitude relation and Aki’s $\omega^2$ relation and the Gutenberg-Richter seismic magnitudes updated by Duda. These experiments did not alter his basic conclusions.

R.A. Haubrich discussed his reexamination of the latitude data for the likely times of occurrence, the size, and the direction of steps or pulses in the Chandler excitation. The time series of the astrometrically estimated excitation function is noisy. Filtering of the excitation removed frequencies outside a band near the Chandler frequency, and this filtered function was examined for steps or pulses by a combined method of least squares and exhaustive search using dynamic programming. Using four different models of fitting, Haubrich arrives at the times of occurrence of the 10 most likely steps at which breaks occurred in the pole paths as determined by the ILS for 70 years and by the BIH for 10 years. He concludes that there is no correlation between the events determined from two data sets and that neither data set correlates highly with the times of the largest earthquakes, the largest events as deduced from the pole paths appearing to be unassociated with earthquakes. Smylie pointed out that he had done a similar study several years ago and that the pole motion measurements did not seem to be good enough to draw conclusions either way.

In summing up the question of seismic excitation of the Chandler motion, W.H. Munk asked four questions. (1) Are the results of Smylie and Mansinha and of Dahlen in agreement or disagreement? (2) If there is disagreement, can it be due to the difference in treatment of the core? (3) Is the use of Aki’s $\omega^2$ model for the seismic moment a cause for disagreement? (4) What is the situation on the data analysis with respect to the correlation of the pole path with earthquakes?

The answer to the first question is that there is disagreement. Dahlen says earthquakes cannot excite the Chandler motion, whereas Smylie and Mansinha maintain that earthquakes can excite the motion. The differences in the conclusions are largely dependent on whether one uses Brune’s moment-magnitude law or the Tocher-Press fault length-magnitude relation. Aki’s law, mentioned earlier, gives results similar to those of the Tocher-Press law, but this relation predicts earthquake moments at least a factor of ten larger than the moment for the Alaskan earthquake.

In response to the second question, Chinnery, Dahlen, and Smylie agree that the different treatment of the core and mantle has little or no importance. Dahlen indicated that, when he adopted the Smylie-Mansinha conditions, his results changed by only a few percent. Smylie indicated that the core treatment could become important if one were to accept the sludge-like core model proposed recently by Higgins and Kennedy.

Concerning the third question, Chinnery stresses that the cumulative seismic effects have been examined in different ways and that this may be the real reason for the disagreement, although there exists a factor of 10 disagreement in the calculation for an individual earthquake. Dahlen mentioned correlation studies using both Aki’s and Brune’s moment-magnitude relationships and stated that his conclusions do not change.

There is general agreement between Dahlen, Haubrich, and Smylie that the astronomical data are too noisy to excite a high correlation with earthquakes. At the time of Smylie and Mansinha’s 1968 correlation study, only highly smoothed pole paths had been published. It later became apparent in examining the unpublished raw polar motion data that the noise level was substantially higher than was claimed. Mansinha agrees that the correlations exist but that the significance of the correlations in the presence of noise is in question. The decisive proof will be the detection of continuing correlation.

If earthquakes do not excite the Chandler motion, what does? Chinnery, Dahlen, and others have at various times speculated on a related hypothesis that the lithospheric motions that are seismic but ‘jerky’ on a 14-month time scale are more common than earthquakes themselves and that they could provide the excitation in the mechanism. Mansinha and Smylie made the same suggestion in an earlier paper (Science, 161, 1127, 1968).

Meteorological effects on the earth’s rotation were discussed by K. Lambeck. Their influences take two forms: variations in the inertia tensor due to periodic distributions in the atmospheric mass and variations in the relative angular momenta due to the movement of air masses with respect to the earth’s surface. The changes in the inertia tensor affect mainly the polar motion, and this is the principal contribution to the annual period. The inertia tensor affects the motion only if they are geostrophic. Departures from geostrophic motion are, however, small. Winds do modify very significantly the earth’s rate of rotation, and it is the seasonal winds that produce the periodic variations in the earth’s rotation that are not of tidal origin. In particular, the variable quasi-biennial wind oscillation is very clearly reflected in the astronomical data, and thus some conclusions can be drawn about the period and the variable extent of the downwarping of the earth’s rate of rotation.

Concerning the fourth question, Lambeck and Cazenave (Geophysical Journal, 32, 79, 1973) also showed a very high correlation with the short-period variations in the length of day, and there is no need to invoke an interior mechanism to explain these. The wind excitation function has a peak of local time scale of a few days. Analysis of the variations in the length of day for short-period tidal terms will therefore be distorted by the wind contribution. There is also evidence for long-period variations in the excitation function. For the 5-year period of analysis, 1958–1963, the long-period variation in the change of length of day. Thus not all long-term fluctuations such as the decade variations can be attributed to core-mantle coupling.

Third Session

Panel on Core-Mantle Interactions

Chairman: D.E. Smylie (York University)

Members: M. D. Fuller (University of Pittsburgh), D. Gubbins (CIRE/C NOAA), M.G. Rochester (University of Newfoundland), A. Toomre (MIT)

The subject was introduced by a brief review presented by D.E. Smylie, the
Fourth Session
Panel on Long-Term Variations

Panel Chairman: W.M. Kaula (UCLA)

Members: R.H. Dicke (Princeton), C.G.A. Harrison (Miami), P.M. Muller (Jet Propulsion Laboratory), W.H. Munk (University of California, San Diego), H.C. Noltimer (Ohio State), R.J. O'Connell (Harvard), G. Pannella (Puerto Rico), and S.J. Peale (University of California, Santa Barbara)

In an introductory statement W.M. Kaula divided the subject into two parts, observational and theoretical. Extrapolation of polar wander and spin into times before systematic astronomical observations requires special techniques: paleomagnetism for observations with respect to continents, chronological analysis for spin in historical times, and fossil growth bands for spin in geologic times. Attempts to explain the inferred variations include (1) motions in the solid earth for the nondiurnal acceleration of spin (on a 1000-year time scale) and for polar wander and (2) tidal dissipation in the oceans and resonances with Venus for the diurnal nutation of spin throughout geologic time. Oddly, the original causes of the earth's spin rate and obliquity are not customarily included as part of the subject.

H.C. Noltimer summarized the results for virtual paleomagnetic polars. Data now exist for all periods back to Cambrian for North America, South America, northern Europe, Africa, Australia, and India. Rocks of all periods from these continents are radiometrically dated, save for some Mesozoic and Paleozoic sequences in South America and India. Fewer data are known for west Antarctica, Russia, Siberia, and China. These data indicate major breakups between continents since Triassic, 200 m.y. ago. In Paleozoic there was apparently also some coming together of separate Russian, Siberian platform, and China plates. The pattern of virtual pole movement is mainly one of gradual motion with occasional rapid motions such as those in Devonian, Cretaceous, and Miocene. Of these rapid motions all but the late Triassic-Jurassic and Cretaceous are in common between the continents, and thus a motion of the pole of about 0.3°/m.y. rather than tectonic motion is indicated. The Cretaceous pole path, for example, differs between South America and Africa; this difference indicates the rifting that created the South Atlantic. In Cambrian the continents were clumped together in Pangaea, the south pole being in what is now North Africa.

Detailed data for Precambrian exist only in North America. In a study by levying, rapid excursions of the apparent dipole pole, of the order of 60°, occurred 1100, 1300, 1950, and 2500 m.y. ago. Data from other continents or tectonic plates are insufficient to determine whether this motion was in the form of a fluid, or else was emphasized by G. Pannella and Toomre. Rapid excursions of the pole may not require major tectonic activity to increase $^3I_x$ but may only entail a passage of $C_t - A_t$ through zero.

W.H. Munk reviewed tidal dissipation in the oceans. The recent solution of the
global tides by Hendershott obtained a total tidal energy twice that for an equilibrium tide. Integration of the lunar and solar work rates over this tidal surface obtains $3.0 \times 10^9$ ergs/sec as the estimated mean dissipation rate, about 50% of that required to account for the recent determinations of lunar acceleration. An additional increment may come from internal tides. The present arrangement of continents could plausibly lead to a tidal energy anomalously high by a factor of 2; the further factor of 2.7 needed to stretch the moon's orbital lifetime to $4.6 \times 10^9$ years must come from more limited shallow seas in the past.

S.J. Peale discussed the proposal of Hipkin that resonances between the moon's orbit and Venus may have delayed the tidal evolution. Independent detailed examinations by Hipkin and by Yoder conclude that resonances not dependent on lunar orbit eccentricity are at best marginally stable against tidal disruption and have negligible capture probabilities. Yoder further found that lunar eccentricity dependent resonances are an order of magnitude more stable against tidal disruption but are definitely unstable against variations in planetary orbit eccentricities on a $10^7$-year time scale.

W.M. Kaula summarized the integration backward in time of the tidal evolution of the earth-moon system to the early state of a higher lunar orbit inclination, a lower obliquity, and an earth rotation rate half that for instability. The angular momentum, inclination, and obliquity are all compatible with models of planetesimal infall in the later stages of the planetary system formation.

R.H. Dicke reviewed briefly current ideas on $\mathcal{G}$ change in the gravitational constant, and reaffirmed the unlikelihood of its detection in earth rotation phenomena.