Why is the Earth a poor timekeeper?
Comments on *The Earth's Variable Rotation: geophysical causes and consequences* by Kurt Lambeck

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The Earth is not a smoothly running timekeeper. It wobbles on its axis, and its spin rate fluctuates on all conceivable time-scales. Until 40 years ago the Earth's rotation had been assumed constant (apart from secular acceleration), and clocks served to divide the rotational period into convenient time units. Pendulum clocks gave an early hint of a seasonal variation in the length of day (lod), and this was confirmed in 1950 by crystal clocks. No longer could UT be considered uniform time. Since 1955 the fundamental time-keeping is performed by atomic frequency standards whose stability now exceeds that of the Earth's rotation by a factor 10^5. But one man's noise is another man's signal, and a small but dedicated group of geophysicists have devoted their efforts to see what could be learned from the variable rotation. Kelvin (indeed a geophysicist) and Harold Jeffreys are the pioneers. Kurt Lambeck's book (1980) is the latest and most exhaustive contribution.

Since the excursions from the mean rotation Ω are small, it is convenient to write

$$\omega_1 = m_1 \Omega, \; \omega_2 = m_2 \Omega, \; \omega_3 = (1 + m_3) \Omega$$

for the components of the instantaneous vector of angular velocity; m_1 and m_2 are the components of polar motion (or wobble) and m_3 (or lod) is the fractional perturbation in diurnal rotation. The variable distribution of mass and motion on (and in) the Earth affect all three components of **m**, unless the variation is symmetric about the rotation axis x_3 (in which case only m_3 is affected).

The first four chapters give the background information. The distribution of density and of the elastic parameters in the Earth are now well established, as a result largely of the detection and interpretation of many resonant frequencies, following excitation by major earthquakes. This is not so for the anelastic properties of the Earth's mantle, nor for the density stratification and viscosity in the fluid core. Rotation dynamics for non-rigid bodies is classical but complex. A systematic account is given of how the rotation vector **m** is perturbed by various body and surface forces.

Chapter 5 presents the astronomical evidence. Even the best of the new instruments cannot yield an instant time series, and one must go back to the historial observations. There are many pitfalls. For example, measurements of latitude (hence m_1, m_2) essentially started with the development of the visual zenith telescope in 1899. But here the corrections for the micrometer calibration are of the same order as the fluctuations themselves. I remember a

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review article which cautioned against the use of latitude records by anyone without extensive experience in the micrometer calibration: beware of the screw correction.

Longitude discrepancies of the Sun and Moon, and of Mercury and Venus, have now yielded an acceptable $m_3(t)$ from 1830 to 1980 (fig. 5.3). We owe this to the painstaking work of Spencer Jones and Brouwer. The most dramatic feature is a decrease by 1 part in 10^7 between 1870 and 1900. At one time, the longitude discrepancies were fitted by segments of straight lines, leading to the conclusion that the Earth's moment of inertia I_{33} had undergone sudden changes. Subsequently the curves were fitted by parabolic arcs suggesting discontintuities in dI_{33}/dt. But now that the observational error spectrum is properly taken into account the arguments for the discontinuities have disappeared. I regret that for so many years geophysicists were supposed to (and sometimes did) come up with the required catastrophies when there was really no evidence for it.

After the atomic time-scale was introduced, a new part of the lod spectrum rose unambiguously above the observational noise spectrum. The seasonal term with 50 ms amplitude which had barely been detected with crystal clocks is now 60 dB above noise level! Lunar monthly and fortnightly tidal lines are very clear. But reported abrupt changes in the lod (such as the reported 1 ms change between 1968 March 29 and April 8) are not clearly established.

Chapter 6 is devoted to tidal disturbances in $m(t)$. These arise from tidal perturbations in the planet's tensor of inertia. Solid Earth tides dominate, and the measurements of $m(t)$ help determine the Earth's overall elastic response (the dimensionless Love numbers h, k, l). There is a need to correct for the slight latitudinal and longitudinal tidal displacements of the observatories.

Seasonal variations in latitude (Chapter 7) were attributed by Jeffreys to seasonal shifts in air mass and in the distribution of snow. (An estimate of the seasonal rise of sap in trees (the effect is negligible) provided Jeffreys with the opportunity of applying his training as a botanist.) Here the overall conclusion is still the same as Jeffreys' but agreement between measurement and computation has since deteriorated. The seasonal variation in the lod is the result of the variable strength of the westerly winds, total angular momentum of atmosphere plus solid Earth being conserved. Variable ocean currents are (unfortunately) not important here. The continuum in the spectrum of the lod for frequencies above 1 cycle yr^{-1} is also dominated by motion in the atmosphere, and one can hope that week-to-week variations in $m_3(t)$ will some day be useful to meteorologists. Frequencies below 1 cp yr^{-1} might be useful in climatological studies. But at much lower frequencies (decade time-scales) something other than the atmosphere takes over.

The free wobble (nutation) of a rigid earth would have a period of 305 day (Chapter 8). To test this, Kelvin asked Newcomb to perform a harmonic analysis of Washington latitude for this 305-day period, and reported a non-zero result as confirmation. (It was not known then that neighbouring harmonics exhibit similar square amplitudes.) Not until the latitude measurements were analysed by Chandler (a Boston merchant) without the benefit of preconceived periods, was the peak with a 434-day period discovered. The lengthening from 305 to 434 day was immediately recognized by Newcomb to be the result of non-rigidity. Ocean yield (the 'pole tide') accounts for about 30 days of the lengthening, the rest is due to the yield of the 'solid' Earth. Theory and observation are now in accord (but the fluid core is still a problem). The latitude data are a useful restraint on earth models.

The Chandler peak has a measurable frequency bandwidth from which an energy *dissipation* of 2×10^6 watt is inferred. Probably dissipation takes place in the oceans, though an anelastic mantle may contribute. The *excitation* is not understood. Arguments for and against appreciable excitation by earthquakes have seesawed over the last 10 years (the book devotes 20 pages to this story). I am unconvinced that the case has been made either on the

basis of the expected magnitude or on the time correlation between major seismic events and Chandler amplitude changes during 1900–1970. Atmospheric excitation is also too small, but here at least one finds a significant and correct phase relation between excitation and response.

Fluctuations in the lod with time-scale of decades and above have been known since the early nineteenth century (Chapter 9). Newcomb fitted the hump between 1680 and 1900 with a sinusoid of period of 257 yr, which became known as 'the Great Empirical Term'. The fluctuations are indeed of great amplitude, 10 times that of the annual variation. What is perhaps more important is that they constitute a continuous spectrum which is not meaningfully represented by fitting sinusoids. Here and elsewhere, position astronomers have imposed the language of periodic phenomena (with which they were so familiar) on to processes that are basically noisy and have no sharp resonances.

The decade fluctuations are generally attributed to the fluid core, it alone having sufficient mass to impose the measured perturbations. The question is whether the electromagnetic coupling (viscous coupling is negligible) between core and mantle is sufficiently tight, and this comes down to the conductivity of the lower mantle. The opinion has seesawed for the last 20 yr, as detailed in over 16 pages.

There are other factors, as always; climatic changes in sea-level and winds, as perhaps portrayed by global surface temperatures, bear some resemblance to $m_3(t)$ during 1900–1970, but the temperature actually *lags* the spin (fig. 9.8). One could draw the conclusion that the variable rotation is the *cause* of the atmospheric changes; Lambeck does not do this, but he leaves open the possibility that both are the effect of other causes, such as the modification of the Earth's albedo by volcanic dust. There are many papers proposing a correlation of $m_3(t)$ to one or another set of long-term observations. They have not led anywhere.

The last two chapters are the longest and to me the most interesting. Tidal dissipation takes spin energy and puts it into orbital energy. The lod decreases, the Moon moves away from the Earth, and the month gets longer. The lod changes are but a few ms per century, but small changes in clock *rate* produce appreciable time errors when accumulated over long periods. Over a few centuries the longitude perturbations are by tens of arcseconds, and easily observed with early telescopes. Over milleniums the Earth, Moon and Sun line up differentily than if there had been no tidal friction, and eclipses that could have been observed in London (say) are seen in Babylon. And over geological times the number of days per year changes very significantly, and this can be measured by counting the number of diurnal striations in the seasonal growth cycle of ancient corals.

There is now general agreement as to the *present* rate of tidal dissipation. The evidence comes from diverse sources: telescopic measurements starting in the seventeenth century (3.4×10^{12} W), ancient astronomic records from the sixth century BC to the eleventh century AD (4.5×10^{12} W), numerical models of ocean tides (4.2×10^{12} W), phase of tidal bulge from satellite orbits (3.6×10^{12} W), lunar laser ranging (preliminary) (3.6×10^{12} W).

The foregoing summary is oversimplified, particularly since it omits any consideration of the conversion of the measured parameters to energy dissipation units.

Extrapolating backwards leads to the result that the Moon was within the Roche limit of about 3 Earth's radii 1.5 billion years ago. Neither the Earth's nor the Moon's surface shows evidence for such a geologically recent catastrophic event. If such an event did in fact occur, it was more than 3 billion years ago. It is, of course, rash to extrapolate backwards for a billion years from data extending a few thousand years. This is especially so since the configuration of ocean basins are totally altered in 100 million years, and the present rate of dissipation is enhanced by a resonant amplification of semidiurnal tides in the North Atlantic basin. What is needed is a palaeorecord of $m_3(t)$ extending backwards into geological times.

A palaeorecord of polar motion m_1, m_2 has long been available and goes under the name of polar wander. Originally the record consisted of fossil plant distributions (it was found that Spitsbergen once enjoyed a tropical climate). For a laterally rigid earth with a frozen equatorial bulge, any conceivable change in the mass distribution would budge the pole by only a few kilometres. But for a plastic bulge an arbitrarily small redistribution (Gold's beetle) could turn the pole through a large angle, given the time. Viewed from space, the axes of inertia and rotation remain fixed and the Earth turns until the beetle is on the equator. Whether this is a plausible model is a question of 'finite strength'.

The concept of continential drift introduced many additional degrees of freedom. Both concepts, drift and wander, were in limbo until rescued by two palaeomagnetic discoveries: changes in the orientation of the field, and occasional reverses. In plate tectonics (the modern version of continental drift) polar wander is not an essential ingredient, but may be a useful concept as a designation for the global average of the plate motions.

Now with regard to $m_3(t)$, corals, molluscs and stromatolites have daily increments which appear to be controlled by successive alternation of daylight and darkness. Investigations have been conducted concerning the number of daily growth increments between (1) successive seasonal growth marks, and (2) successive synodic months (because of some lunar influence on growth). There are many difficulties.

The Devonian corals studied by Wells show about 400 daily growth increments between successive seasonal annulations. Molluscs yield results for seasonal and monthly annulations. The reported results of days per year and days per month are in keeping with the *expected* value if the tidal dissipation had remained near 4×10^{12} W over the last 0.3 billion years. (Lambeck expresses concern that the results might be biased towards *a priori* expected values). Taken at their face value, the results do support extrapolation of present rates into the geological past, leading to a 'Gerstenkorn event' a few billion years ago when the Moon was very close to or within the Roche limit. According to capture theory in its simplest form, lunar capture occurs in a retrograde orbit, tidal friction brings the Moon closer to the Earth with an attendant increase in the orbital inclination. At the closest approach the orbit becomes polar and then prograde, and from now on tidal friction increases the radius of the lunar orbit and diminishes the inclination, until the present situation is reached. The energy dissipated during the Gerstenkorn event is such as to lead to the wholesale melting of the mantle and of the Moon. There should be some evidence for this event in the geological record, and one study purports a date of 2.85 ± 0.25 billion yr BP. But Lambeck concludes that 'there is no compelling geological evidence to suggest that the Gerstenkorn event occurred during the last 4.0×10^9 yr and most likely the Moon was already well outside the Roche limit at this time'.

By now it must be clear to the reader that the book involves a vast array of disciplines, from the study of ancient chronicles to the palaeontology of bivalves. Lambeck has made himself knowledgeable in all of these matters, and presents them with clarity and style. In many cases he has made personal contributions, and this shows! It is not easy to get at the diverse facts, and anyone who will want to work in this field will benefit from the book. I, for one, am grateful for Lambeck's labours.

For my taste I should have preferred a less encompassing account, with a position taken more often on controversial issues and less of an effort in detailing of how one got there. The book could have been less voluminous (and easier to review on a rolling ship), and perhaps less costly. I am puzzled about the subtitle *geophysical causes and consequences.* What are the geophysical consequences of a variable rotation? The only examples I could find are the *lag* of released earthquake energy relative to wobble amplitude in fig. 8.2 and the lag of global surface temperature relative to the lod in fig. 9.8. In both cases the obser-

vational evidence is tenuous, and the proposal of rotational strain as a cause is implausible because the rotational strain perturbations are so much smaller than tidal strain.

Twenty years ago Gordon MacDonald and I published a book with nearly the same title, and by the same press. I was curious to find out what had been learned and how some of the issues for presenting the material had been handled. I believe we were the first to organize the subject according to frequency with polar motion m_1, m_2 and lod m_3 discussed side by side. (Previously these had been largely separate subjects.) It is the obvious point of view for the geophysical user of the observations, and it is also the one followed by Lambeck. Less obvious is the choice whether to go from short- to long-period fluctuations (as we all did), or vice versa. I suppose there is some advantage in the former choice as high frequencies can contaminate low-frequency observations, but not the other way around. Further, observations and interpretation of the short-period fluctuations are in rather better shape, and one wants to make a good impression on the reader before going on to more speculative subjects.

Regarding developments since 1960, I find it surprising that so much has been learned by new discussions of old measurements, such as the pole position since 1900, the reinterpretation of Babylonian eclipses and the discovery of Hellenic and Islamic sources. Atomic timekeeping and satellite orbits have, of course, added new vistas, but here I feel that the payoff will come in the next twenty years.

I recall how MacDonald and I would search for possible ways of measuring the palaeo-length of day, in parallel with what had been accomplished for the polar motion from palaeomagnetic evidence. The idea that corals could provide just such a palaeoclock was totally unexpected.

Reference

Lambeck, Kurt, 1980. *The Earth's Variable Rotation: geophysical causes and consequences,* Cambridge University Press, 449 pp.