

ON THE EXCITATION OF THE EARTH'S POLAR MOTION

B. Fong Chao

Geodynamics Branch, Goddard Space Flight Center, Greenbelt, Maryland

Abstract. One of the conclusions reached by recent studies of Barnes et al. and later Hide was "that atmospheric excitation alone was sufficient to account for the observed polar motion over (the studied) period, that there is apparently no need to invoke substantial excitation either by the fluid core or . . . earthquakes." The purpose of the present paper is to point out that their argument that led to the above conclusion is unjustifiable (hence whether the conclusion is in reality true or not is still an open question). I demonstrate this through a physical "thought" experiment and a numerical simulation. In essence, they show that if we want to compare a geophysically observed excitation function $\psi_a(t)$ with the excitation function deduced (via deconvolution) from the polar motion observation $m(t)$, we should do so directly (the "direct approach"). To compare $m(t)$ with the polar motion computed (via convolution) from $\psi_a(t)$ (the "integration approach"), as Barnes et al. and Hide did, is misleading.

Introduction

The motion of the Earth's rotation axis with respect to the geographical reference frame, known as the polar motion, has been observed for nearly a century now. It consists mainly of an annual wobble and a 14-month Chandler wobble. The annual wobble is a forced motion believed to be caused by seasonality in the atmosphere and hydrosphere. The Chandler wobble is a mode of the Earth's free oscillation which has been, and is being, continually excited. The evidence is two-fold: (i) without excitation the Chandler wobble would have died away a long time ago due to energy dissipation in the Earth, and (ii) the observations do show changes in the amplitude and phase of the Chandler wobble. However, despite decades of effort by many investigators, the major excitation source(s) for the Chandler wobble still remain a mystery.

In a recent study, Barnes et al. [1983] made, among other things, a detailed comparison of the polar motion with global meteorological data for the period 1/1981 - 4/1982 (about 1.1 Chandler periods in length). One of the main conclusions they reached was "that atmospheric excitation alone was sufficient to account for the observed polar motion over that period, that there is apparently no need to invoke substantial excitation either by the fluid core or . . . earthquakes" (henceforth referred to as the CONCLUSION). Hide [1984] later extended this analysis to include 12/1979 - 2/1984 (about 3.6 Chandler periods in length), endorsing the same CONCLUSION. The importance of these work is quickly being recognized. Unfortunately, due to a stumble in

their reasoning, the CONCLUSION is unjustified. My argument is as follows.

A "Thought" Experiment

Let us consider the following "thought" experiment. Suppose we place two identical, heavy pendulums some distance apart in a turbulent wind. We set the pendulums in motion starting from the same initial conditions and record their motions $m_1(t)$ and $m_2(t)$ for a few cycles. Upon close examination, we notice that the two functions $m_1(t)$ and $m_2(t)$ are not exactly the same — they have been perturbed by "random" excitation functions $\psi_1(t)$ and $\psi_2(t)$ of the wind, respectively. Yet by and large $m_1(t)$ and $m_2(t)$ look rather alike (and incidentally, rather smooth). Can we then conclude that the two excitation functions $\psi_1(t)$ and $\psi_2(t)$ are the same, at least approximately? The answer, of course, is "no" because the observed motions $m_1(t)$ and $m_2(t)$ are predominantly a free motion set off by the initial conditions. As long as the initial conditions are the same, we will always have $m_1(t) \approx m_2(t)$ regardless of how different $\psi_1(t)$ and $\psi_2(t)$ are. In other words, the observed motion $m(t)$ is insensitive to the excitation function $\psi(t)$. Mathematically, $m(t)$ is the convolution of $\psi(t)$ with the pendulum's free motion $m_o(t)$. No matter how "rugged" $\psi(t)$ is, its convolution with $m_o(t)$ will yield a rather smooth $m(t)$ which, within a few cycles at least, does not differ much from $m_o(t)$ itself (for details see discussions pertaining to Equation 1 below). With known $m_o(t)$, we can recover $\psi(t)$ from $m(t)$ via deconvolution. The point here is that if we want to compare $\psi_1(t)$ with $\psi_2(t)$, we should do so directly. Comparing $m_1(t)$ and $m_2(t)$ instead is, to say the least, misleading.

Back to Polar Motion

The swing of the pendulums in the above example is physically analogous to the Earth's polar motion. The latter is being excited (by some unknown means) just as the pendulums were perturbed by the turbulent wind. The only (formal) difference is that the functions are now complex-valued functions reflecting the 2-dimensional nature of the polar motion. To be more specific, the observed polar motion $m(t)$ can be expressed [c.f. Munk & MacDonald 1960, p. 46] as the sum of an "initial condition" term (see below) and the convolution of some (unknown) excitation function $\psi(t)$ with the free Chandler wobble $m_o(t)$, which is the impulse response of the Earth filter:

$$m(t) = m(0)\exp(i\sigma t) + \psi(t) * m_o(t). \tag{1}$$

where $m(0)$ is the initial position $m(t)$ at $t=0$, $\sigma = 2\pi/(435\text{days})$ is the Chandler frequency, and the asterisk denotes temporal convolution. Note that for simplicity we have taken σ to be real-valued since the energy dissipation within a few cycles is

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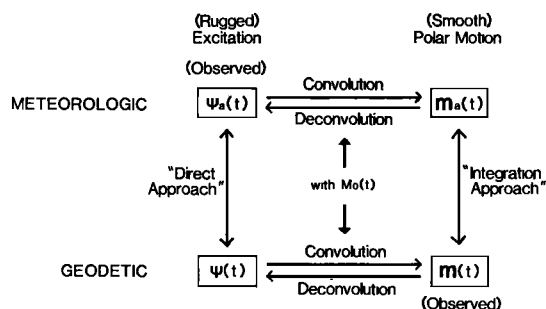


Fig. 1. A summary of procedures and terminology used in comparing meteorologic and geodetic observations.

negligible; hence $m_a(t)$ is taken to be a circular motion (around the North Pole) with angular frequency σ . The first term on the right-hand side of Equation (1), $m(0)\exp(i\sigma t)$, corresponds to the complementary (or “transient”) solution to the equation of motion, whereas the second term $\psi(t)*m_a(t)$ corresponds to the particular (or “forced”) solution. Loosely speaking, as long as $\psi(t)$ is random and not exceptionally large (c.f. the numerical experiment below), what we see in $m(t)$ is mostly (the smooth) $m(0)\exp(i\sigma t)$, with “jiggles and wiggles” arising from (the rugged) $\psi(t)$ in the form of $\psi(t)*m_a(t)$. Thus the point to stress once again is that, as in the above though experiment, $m(t)$ is insensitive to $\psi(t)$.

The excitation function $\psi(t)$ can be deduced from the observed $m(t)$ via deconvolution [see, e.g., Lambeck 1980, p. 61]. The geophysical problem is to compare this deduced $\psi(t)$ with geophysical events/variations. What Barnes et al. [1983] and Hide [1984] did was an attempt to identify $\psi(t)$ with the atmospheric excitation function $\psi_a(t)$ obtained from global meteorological data (where the subscript a denotes “atmospheric”). Here we shall not distinguish between our ψ function and their χ function since the difference is numerically trivial. In keeping with their terminology, I shall make use of the terms “direct approach” and “integration approach”. The former refers to the comparison between $\psi(t)$ and $\psi_a(t)$, whereas the latter refers to the comparison between $m(t)$ and $m_a(t)$, $m_a(t)$ being the computed polar motion for $\psi_a(t)$ using Equation (1). Figure 1 summarizes the procedures and terminology. Unfortunately, the direct approach conducted by Barnes et al. [1983] and Hide [1984, see also IUGG/IAU SSG 5-98, Bulletin No. 1, 1984] are far from conclusive (also see below). So they turned to the integration approach. By empirically choosing an initial condition for $m_a(t)$, they found $m(t) \approx m_a(t)$. Then, based on this fact, they reached the CONCLUSION which essentially states that $\psi(t) \approx \psi_a(t)$. Now, in light of the thought experiment and the above arguments, it should be clear that, while the integration approach is itself legitimate, to draw the CONCLUSION from it is unjustifiable because, to reiterate, $m(t) \approx m_a(t)$ by no means warrant $\psi(t) \approx \psi_a(t)$.

The analogy between the polar motion situation and the thought experiment is actually not all that straightforward. This is because the atmospheric $\psi_a(t)$ is not random — it has a strong annual component (and perhaps a semi-annual component as well). Consequently we should not expect $m(t)$ and $m_a(t)$ to simply resemble the circular motion $m(0)\exp(i\sigma t)$. Instead, the system being linear, they should

both resemble the sum of $m(0)\exp(i\sigma t)$ and an annual wobble. This is exactly what Barnes et al. [1983] and Hide [1984] found. In particular, the large change in the amplitude of the polar motion during the studied period is simply part of the ~ 6.4 -year beating phenomenon between the annual wobble and the Chandler wobble. However, this does not alter the above physical argument about the Chandler wobble. The resemblance between $m(t)$ and $m_a(t)$ says little, if anything, about the excitation of the Chandler wobble, and certainly does not lead to the CONCLUSION in any case. It merely means that it is the annual wobble that can be largely accounted for by the atmospheric excitation (a well-known fact) and that the initial conditions $m(0)$ chosen empirically by Barnes et al. [1983, for their Figure 10] and by Hide [1984, for his Figure 4] were numerically proper. Incidentally, exactly how much of the annual wobble can be accounted for by the atmospheric excitation during the studied period is not clear from their analysis, however. The conventional method is to compare the two annual excitation vectors deduced respectively from $\psi(t)$ and $\psi_a(t)$. The difference vector between them then indicates the contribution to the annual wobble excitation from other sources (for example, the hydrosphere).

Now let us come back and discuss the direct approach which does compare $\psi(t)$ and $\psi_a(t)$ directly. In the time domain, both $\psi(t)$ and $\psi_a(t)$ have clear annual signals (especially in the y-component) — this is expected as said above. But as

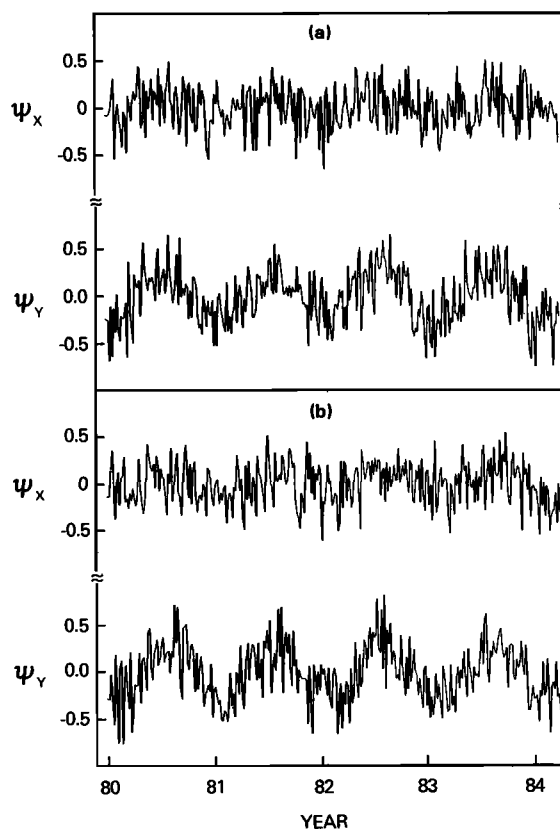


Fig. 2. Two sets of computer-generated excitation functions used as input to generate the polar motions in Figure 3. They contain identical annual terms but independent zero-mean, Gaussian random noise series.

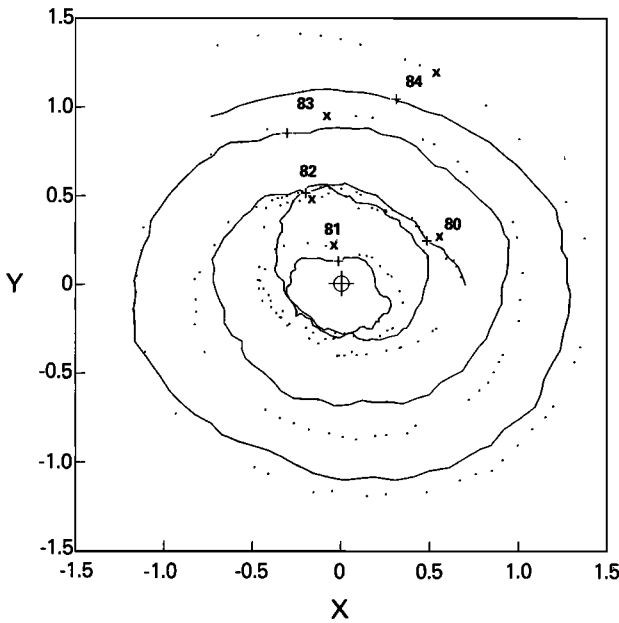


Fig. 3. The polar motions generated (using Equation 1) by the excitation functions in Figure 2 [solid curve for Fig 2(a), dotted curve for Fig 2(b)].

Figure 9 of Barnes et al. [1983] shows, the resemblance stops there. That is, apart from an annual trend the resemblance between $\psi(t)$ and $\psi_a(t)$ is actually rather poor. In a sense, this should be no surprise, either, because even if $\psi(t) = \psi_a(t)$ in reality, their time-domain comparison in practice is unlikely to be fruitful. The reason is the following. $\psi_a(t)$ may have, for example, strong semi-annual and even “7-week” signals whereas (the deduced) $\psi(t)$ does not. The latter is because these components, unlike the annual component which is close to the 14-month resonance, have been dynamically suppressed and observationally overwhelmed by noise and, hence, cannot be recovered to above the noise level by deconvolution. Further, but no less serious, problems arise from the noise (especially high-frequency noise) being greatly enhanced by deconvolution. These, however, should not intimidate us in the search of correlation between $\psi(t)$ and $\psi_a(t)$ for Chandler wobble, as long as we do it in the frequency domain. The latter calls for a coherence study concentrated on the Chandler frequency band. Unfortunately, the time series of Barnes et al. [1983] and Hide [1984] are much too short to allow reasonable frequency resolution because a complete resolution of the annual and the Chandler wobbles requires at least 6 years of data. The work by Dickey et al. [1984], using 8-year worth of data, may shed some light in this regard. However, without removing the annual component from the data, their results concerning the Chandler wobble are still inconclusive.

I should point out that a similar argument has been alluded to in an earlier work by Wilson & Haubrich [1977], where they questioned the validity of the conclusion of O’Connell & Dziewonski [1976]. O’Connell & Dziewonski [1976], by means of an integration approach, reached the conclusion that the Chandler wobble can be accounted for by, ironically, earthquakes. Wilson & Haubrich [1977] examined their excitation functions in a direct approach (in the frequency domain), and found no evidence to support such an asser-

tion. The moral here is, again, the integration approach is misleading.

A Numerical Experiment

Finally, I shall now present a numerical simulation, the purpose of which is to show explicitly the “indifference” of $m(t)$ toward the perturbing $\psi(t)$. I constructed a hypothetical excitation function $\psi(t) = (\psi_x(t), \psi_y(t))$ in the form

$$\psi_x(t) = A_x \cos(\omega t + \theta_x) + N_x(t), \quad (2)$$

$$\psi_y(t) = A_y \cos(\omega t + \theta_y) + N_y(t).$$

In (2), $N_x(t)$ and $N_y(t)$ denote two computer-generated, zero-mean, Gaussian random series with standard deviations S_x and S_y , respectively, and the sinusoidal terms represent the annual excitation [$\omega = 2\pi/(365 \text{ days})$]. In order to have my $\psi(t)$ closely resemble Hide’s Figure 3 (although this is by no means essential in making the point), I liberally chose $A_x = 0.08$, $A_y = 0.3$, $\theta_x = \theta_y = 5\pi/6$, $S_x = S_y = 0.23$, and a total time span of 4.2 years (at 5-day intervals). Some slight editing on $N_x(t)$ and $N_y(t)$ were made as follows. First, I reduced those points with absolute values exceeding 2.3 standard deviations by a factor of 2.3. This alleviates the extremely out-lying points in $N_x(t)$ and $N_y(t)$. Secondly, I least-squares fitted a mean and a linear trend to $N_x(t)$ and $N_y(t)$ and subsequently removed them to make certain that $N_x(t)$ and $N_y(t)$ are truly zero-mean and trendless. The resultant $\psi_x(t)$ and $\psi_y(t)$ are displayed in Figure 2(a). Note that Figure 2 does grossly resemble Hide’s Figure 3, except for a static term (especially in $\psi_y(t)$) which has no physical importance. Then, with the initial condition $m(0) = (0.7, 0)$ which I again chose liberally, I performed the computation (1). The resultant polar motion $m(t)$ that the hypothetical excitation function (2) would generate is shown as the solid curve in Figure 3. Then I repeated the same procedure with a completely independent set of $N_x(t)$ and $N_y(t)$, similarly edited, while everything else stays the same (see Figure 2b). The polar motion $m(t)$ thus generated is shown as the dotted curve in Figure 3. Notice, incidentally, how well Figure 3 takes after Hide’s Figure 4 (apart, again, from a trivial static offset). The important point, however, is that the solid curve and the dotted curve in Figure 3 follow each other closely. In fact, as is evident from the time marks in Figure 3, the closeness is no worse than that in Hide’s Figure 4. Yet another feature shared by both Figure 3 and Hide’s Figure 4 is the gradual departure of the two curves (in both amplitude and phase angle). This is consistent with the random-walk nature of the polar motion arising from integration of random excitations. In any event, this numerical experiment undoubtedly confirms my earlier assertion that one always obtains similar polar motion curves despite the completely uncorrelated excitation functions (except for the common annual excitation term whose sole function in the present case is to cause the beating in the amplitude).

Summary

In summary, the work by Barnes et al. [1983] and Hide [1984], contrary to what they claim, do not lead to the CONCLUSION (that the atmospheric excitation can account for

the Earth's polar motion during the studied periods) because the information about the Chandler wobble excitation that can be deduced from their study is, at best, insufficient. Whether the CONCLUSION is in reality true or not is thus still an open question, as the search for other excitation mechanisms for the Chandler wobble proceeds.

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B. F. Chao, Geodynamics Branch, Goddard Space Flight Center, Greenbelt, MD 20771.

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