THE THEORETICAL DESCRIPTION OF THE NUTATION OF THE EARTH

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1. INTRODUCTION

This talk was intended, I think, to be an opportunity to recite the conventional wisdom of the class of geophysicists who are interested in investigating theoretically the Earth's wobble and nutation. It has become clear since the Kiev meeting in 1977, however, that we have not yet agreed upon the contents of our conventional wisdom and it would be premature, and presumptuous, of me to pretend to recite it. What I can do is to tell you what I think the conventional wisdom ought to be. In doing so I shall give free rein to my prejudices and little consideration to opposing points of view (they must speak for themselves). Nothing in this talk is noticeably original; virtually all of it is extracted from the work of others or from the folklore of this topic.

I am mostly going to discuss how we view the results of theoretical calculations of the Earth's nutation (all of which currently come from computers) and how we try to tie those to observation. I originally claimed that I was going to cover both polar motion and nutation (and most of what follows could be readily extended to polar motion). However since nutation is by far the most predictable disturbance of the Earth's rotation it is presumably the element of principal interest in designing coordinate systems. In deference to time and space limitations, then, we will give short shrift to polar motion (and also to changes in the length-of-day). I should offer a word of warning: my astronomy stops at the Big Dipper and I do not understand geodesy at all. Some but I hope not too much, of what I say will surely be in error.

2. THE ELEMENTS OF THEORETICAL NUTATION AND POLAR MOTION

We shall work exclusively in a reference frame which rotates rigidly with the unchanging angular velocity

$$\vec{\Omega} = \Omega_0 \hat{z} \tag{1}$$

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where Ω_0 is a constant scalar and $\hat{\mathbf{z}}$ is a constant vector. This frame continues its invariant rotation no matter what happens to the Earth. Call this frame M_I. We suppose that we have chosen Ω_0 and $\hat{\mathbf{z}}$ sufficiently cleverly that the Earth, as seen in our frame, deviates only slightly from equilibrium. There is, of course, no such frame and we shall, from time to time, have to adjust both the length and orientation of $\vec{\Omega}$ to account for secular changes in the length-of-day and orientation of the mean Earth (as from the precession). On the time scales of interest to us, however, the pleasant fantasy that such a frame exists is quite useful and very nearly true.

One of the useful features of M_I is that it bears an unchanging relation to inertial space. Consequently the motion of an observatory in M_I is, apart from the easily-handled effects of the unvarying rotation $\vec{\Omega}$, the same as its motion with respect to inertial space. This makes the connection between theory and observation relatively straightforward.

Let $\mathbf{x}$ denote position in our frame. Since we suppose that the Earth has a unique equilibrium position, we can regard $\mathbf{x}$ as also specifying a particle in the Earth. If the Earth is disturbed by some agency, internal or external, then in general it will depart from equilibrium. Each particle $\mathbf{x}$ will move to a new point $\mathbf{r}$ given by

$$\mathbf{r}(\mathbf{x},t) = \mathbf{x}+\mathbf{s}(\mathbf{x},t), \tag{2}$$

where $\mathbf{s}$ is what we call the <u>Lagrangian particle displacement</u>. So $\mathbf{r}(\mathbf{x},t)$ is the current location of the particle which is normally (in equilibrium) at $\mathbf{x}$. Expressing the Earth's state as

$$\{ \text{ current state } \} = \{ \text{ equilibrium state } \} + \{ \text{ perturbation } \} \tag{3}$$

is useful as long as the <u>perturbation</u> is small compared to the <u>equilibrium state</u>. Fortunately that is true for nutation, as well as a whole lot of other things of geophysical interest.

Suppose that given an external tidal potential of frequency ω, say, we can compute $\mathbf{s}$ and to adequate accuracy for geophysically reasonable models of the Earth. The question then arises how we connect our theoretically convenient description to some observationally useful quantity. This seems to be a question of arriving at a suitable decomposition of $\mathbf{s}$.

Suppose that we express $\mathbf{s}(\mathbf{x},t)$ as a sum of two terms.

$$\mathbf{s}(\mathbf{x},t) = \vec{\Theta}(t)\times\mathbf{x}+\mathbf{s}'(\mathbf{x},t) \tag{4}$$

where $\vec{\Theta}$ is a spatially constant vector representing a time-dependent rigid rotation and $\mathbf{s}'$ is simply everything left over. We can think of $\vec{\Theta}$ as being the "nutation part" of $\mathbf{s}$, and $\mathbf{s}'$ as being the "body tide part" (assuming $\vec{\Theta}$ represents something sensible). Let v be some por-

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tion of the Earth (such as all of it, or maybe just the mantle, etc.). Let us choose $\vec{\Theta}$ so that

$$\int_V \rho|\mathbf{s}(\mathbf{x},t)-\vec{\Theta}(t)\times\mathbf{x}|^2 \, dv = \text{minimum} \quad . \tag{5}$$

This is simply a specialized flavor of Tisserand's mean axes of body.

Then, in the particular sense defined by this integral, $\vec{\Theta}$ is the instantaneous mean rigid rotation of v. Clearly, $\vec{\Theta}$ will be different for different choices of v. What choice shall we make?

A number of possibilities immediately present themselves:

(i) v = the whole Earth

This is the most obvious choice but not a very useful one. In many cases of interest, and in particular for the nutations, the core and the mantle undergo greatly different, even opposing, mean rigid rotations. This choice for v leads to a value of $\vec{\Theta}$ which is not the actual mean rigid rotation of anything.

(ii) v = the crust and mantle

This is a pretty good choice. Molodensky and others have used it. The reason it is a good choice for the Earth is because the crust and mantle of our planet very nearly rotate together. I think, however, that this is something of a cosmological coincidence and one which we do not really have to rely on. Suppose that the crust and mantle did not move together. This wouldn't bother us much; we would simply choose that definition of v which avoided the uncooperative (and unnecessary) mantle, to wit:

(iii) v = the crust

By "crust" we in fact mean the Earth's solid outer surface. This is where our instruments reside and this is the platform whose orientation we wish to know. Here I reveal my partisan colors and for the remainder of this talk I shall take $\vec{\Theta}$ to be the instantaneous mean rigid rotation of the Earth's surface:

$$\int_{\text{Crust}} \rho|\,\mathbf{s}(\mathbf{x},t)-\vec{\Theta}(t)\times\mathbf{x}|^2 \, d\sigma = \text{minimum} \quad . \tag{6}$$

The more traditional position is to use the mantle or the mantle plus crust as the reference body. I think that this view is based on the notion that the mantle is a more stable and somehow more fundamental piece of matter than this torn and heterogeneous crust of ours. I claim that this is not so; the most we can say for the mantle is that

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it is bigger than the crust,
it is removed from direct observation,
and its tectonics are poorly understood.

It is true as we have already noted, that its rigid rotation is very nearly the same as that of the crust, but that does not constitute a reason to use the mantle as a reference. For any observational purpose known to me the salient quantity is the motion of the crust.

Theory, of course, delivers the analytically exact mean rotation of the surface while observation delivers the mean rotation of a set of observatories. Between these two quantities lie the effects of station distribution in space and time and, most important, processes in the solid Earth and oceans beyond the reach of our models. The observation and understanding of just those processes is, of course, one of the goals of this province of science.

3. OTHER QUANTITIES

$\vec{\theta}$ is now the instantaneous rigid rotation vector of the crust (assuming you agree with our choice of v). This quantity can be converted to more familiar measures of the Earth's rotation, such as motion of the instantaneous angular velocity vector or of the figure axis. In this section we will make some of those connections.

To be a little more specific, we shall assume that $\vec{\theta}$ has the form

$$\vec{\theta}(t) = \theta_0 \ (\hat{\mathbf{x}}+i\hat{\mathbf{y}})e^{i\omega t} \tag{7}$$

which corresponds to a rigid rotation of θ_0 radians about an axis which is rotating in the Earth's equatorial plane with angular frequency ω. With these conventions, nutation corresponds to $\omega \simeq \Omega_0$, and polar motion (such as the Chandler wobble) corresponds to $\omega \simeq -\varepsilon\Omega_0$ where ε is some measure of the Earth's ellipticity of figure and is small ($\simeq 1/300$).

The geographic axis. Suppose that when the Earth is at rest we define a reference system by measuring the position of a very large number of globally distributed stations and that we use this reference system to define a particular direction which we call the geographic axis and which passes through a large painted X near the North Pole. We call the point where the axis pierces the surface the geographic pole. When the Earth nutates, or whatever it happens to do, all of the reference stations get pushed around. If at some instant we try to determine the geographic axis by measuring the locations of all of the stations we will somehow have to accommodate the fact that in addition to rigidly rotating, our network has gotten all "squished up" and deformed. If we use least-squares techniques to fit our old reference system to the new station positions we will find that the geographic pole has moved by an amount

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$$\mathbb{P} = \vec{\Theta} \times \hat{\mathbf{z}} = \theta_0 (i\hat{\mathbf{x}} - \hat{\mathbf{y}}) e^{i \omega t} \quad . \tag{8}$$

Note that this pole does not in general intersect the X any longer (by an amount given by **s′**). $\vec{\Theta}$, then, corresponds directly to the mean rigid rotation of a dense crustal station network in M_I.

The rotation axis. The instantaneous rotation axis of the crust is offset from $\hat{\mathbf{z}}$ by

$$\mathbb{R} = \frac{i\omega}{\Omega_0} \vec{\Theta} = \frac{\omega}{\Omega} \theta_0 (i\hat{\mathbf{x}} - \hat{\mathbf{y}}) e^{i \omega t} \quad . \tag{9}$$

Note that this quantity is scaled by ω/Ω_0 which is about unity for nutation and very small for polar motion.

The forced nutations are sometimes described in terms of the motion of the instantaneous rotation axis. We actually observe **P**, however, and not $\mathbb{R}$. The difference is sometimes called "diurnal polar motion" and is

$$\text{polar motion} = \mathbb{R} - \mathbb{P} = \left| \frac{\omega - \Omega_0}{\Omega_0} \right| \vec{\Theta} \tag{10}$$

which is small for the nutations ($\omega \simeq \Omega_0$) but unfortunately does not exactly vanish. ($\mathbb{P}$ shows no such motion, of course.) The rotation axis is not directly observable and, so far as I can tell, has usually confused matters when used as an intermediary. It would seem to be more direct to specify nutation in terms of the mean crustal rigid rotation (or equivalently the geographic axis, above). See Fedorov, 1963, and Jeffreys′ foreword thereto for further discussion.

The figure axis. I take this to be the instantaneous axis of greatest inertia of the Earth. This quantity is not usefully defined in terms of a specific region v; we have to compute it for the whole Earth. For the record, the instantaneous figure pole is offset from $\hat{\mathbf{z}}$ by the amount

$$\mathbb{F} = \frac{1}{C-A} [\delta I_{xz} \hat{x} + \delta I_{yz} \hat{y}] e^{i \omega t} \quad . \tag{11}$$

where δI_{xz} etc. are the instantaneous perturbations in the Earth′s inertia tensor due to **s**. (Because of our definition of M_I the δI are partially due to deformation and partially due to rigid rotation.) For a rigid body the figure and geographic axes coincide. For the Earth they do not. As the above equation implies, $\mathbb{F}$ is quite sensitive to the values of δI_{ij}. A perfectly spherical Earth does not have a unique figure axis. Since the Earth is very nearly spherical, its figure axis is quite sensitive to the deformational portion of **s**, and consequently, $\mathbb{F}$ is not a very stable quantity. So far as I can tell, it is not a very useful one either.

The angular momentum axis. The instantaneous angular momentum vector is well-determined without resort to complex calculations from a knowledge of the Earth′s shape and of the external torques exerted upon

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the Earth. It is wholly impervious to internal influence. This quantity is theoretically important, and in fact is computationally useful as a check on our calculations (see Wahr, 1980). It is not observable and its role in discussing nutation is the subject of debate. (See, again, Fedorov (1963) and Jeffreys for two sides of this question.)

4. HOW TO COMPUTE **s**

In order to find **s** for, say, a particular tidal component we have to solve the elastic gravitational equations of motion for a rotating, slightly elliptical, self-gravitating Earth with a stratified, compressible fluid outer core and a stratified, elastic mantle. In general we don't know how to do this.

There have, however, been a fruitful series of steadily improving approximate assaults described by Jeffreys and Vicente (1957a, 1957b), Molodensky (1961), Shen and Mansinha (1976), Sasao et al. (1980), and Wahr (1980). These studies vary in several respects but, so far as I am aware, all seek to model the same physics and all are essentially correct at their various levels of approximation. Apart from an occasional numerical or algebraic error, there have been no great surprises over the two-plus decades covered by these authors. There has been, I think, a general improvement in the accuracy of our theoretical results due to substantial refinements in geophysical Earth models and the availability of more powerful computing machinery, and there has also been an improvement in the clarity and completeness of the theoretical underpinnings of this effort (as I think we might expect from twenty-three years of experience). I would not characterize this process as the development of new and improved nutation theories but rather as the extension and refinement of the theory of the Earth's nutation.

These calculations are, in detail, extremely complex. I shall take the liberty of summarizing the central features which, in my view, are necessarily common to all of them. Those features arise from two obstacles faced by every nutation calcuation:

(1) How can we compute the response of a rotating Earth which is initially in perfect hydrostatic equilibrium to an external gravitational potential?

(2) How can we correct for the fact that the real Earth is not in perfect hydrostatic equilibrium?

The first question arises because the rotating Earth is not spherically symmetric and vector surface spherical harmonics no longer provide separable basis functions for representing **s**. Our only escape from this to date has been to approximate **s**; usually this means representing **s** by a spherical harmonic series which (we hope) will converge fairly rapidly. In fact, as we can show from both a priori argument and a posteriori example, that this seems to be the case. The results of such a calculation give us the response of an Earth model whose equilibrium state is purely hydrostatic.

The Earth is not quite in hydrostatic equilibrium. For a modern geophysical Earth model with the correct mass and moment of inertia, Clairaut's equation yields (Smith and Dahlen, in press)

$$\frac{C - A}{C} = \frac{1}{308.8}$$

while the correct value for the Earth (Kinoshita, 1977) is

$$\frac{C - A}{C} = \frac{1}{305.4} \ .$$

The difference is of order 1 percent and is non-negligible. We have not dealt with this by extending the theory principally because we would have to know (but do not) the internal deviatoric stresses which keep it out of equilibrium. The source, magnitude, and distribution of these stresses is currently a mystery. The precise treatment of this dilemma varies from author to author but they are all logically (and practically) equivalent to a single scheme.

This scheme goes as follows: Let $\vec{\Theta}$ be the quantity of interest given by some theoretical calculation; to be specific we might suppose that it is the surface rigid rotation associated with some circular nutation term. Let $\tilde{\Theta}$ be the corresponding quantity for a model which differs only by being perfectly rigid. Because the rigid but hydrostatic Earth does not have the same value for (C-A)/C as the real Earth, $\tilde{\Theta}$ will differ from the value predicted for the quantity $\vec{\Theta}$ by a modern rigid-body nutation calculation. Let $\vec{\Theta}_R$ be the value predicted by such a calculation (such as Kinoshita, 1977). Our corrected estimate for the Earth's predicted nutational motion is given by

$$\vec{\Theta}_E = \vec{\Theta} + (\vec{\Theta}_R - \tilde{\Theta}) \qquad (12)$$

This achieves a simple correction for the difference between the rigid-body response of the model Earth and that of the real Earth. Some studies have used an explicit form of the Liouville equation but that is simply a different flavor of this same correction process.

Ironically, of all the improvements in the theory which have occurred since Molodensky published his results in 1961 the most important seems to have been the refinement of geophysical Earth models. Wahr (1980) discusses how some results due to Shen and Mansinha (1976), who repeated Molodensky's calculations with a more modern Earth model, may be interpreted to show that the principal difference between Molodensky's original results and those of Sasao et al. (1980) and Wahr (1980) is due to changes in the Earth models used. (This does not, of course, alter the fact that the new calculations are better, but it does reflect well on 1960-style intuition.)

There is, fortunately, some reason to expect that the next twenty years will not rearrange our results as much as the last twenty have. That reason lies in the nature of the great improvement in geophysical Earth models beginning in the late 1960's. Models constructed since that time have been constrained to fit the observed long-period elastic-gravitational normal modes or free oscillations. These observations first became available following the 1960 Chilean earthquake but were not systematically used in constructing Earth models until much later in the decade (see for example Gilbert and Dziewonski, 1975). The gravest of these normal modes has a period of about one hour and describes the global elastic-gravitional response of the Earth associated with spherical harmonic terms of degree l=2. Since it is precisely these terms which dominate the effects of elasticity on the forced nutations and since the observations are not likely to change, I think we have reason to expect that future Earth models will nutate (and wobble) about the same as our present ones do. We would not expect this to be the case for models available in 1960.

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