Some Remarks on Polar Wandering

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This paper lends fresh support to the hypothesis that large angular displacements of the earth's rotation axis relative to the entire mantle have occurred on a geological time scale, owing to the gradual redistribution (or decay or manufacture) of density inhomogeneities within the earth by the same convective processes that are responsible for continental drift. The first of our three contributions is a pedagogic theorem that rigorously illustrates this mechanism of polar wandering for a 'quasi-rigid body.' That theorem states that any slow changes in shape of such a body preserve as an adiabatic invariant the solid angle traced out by its angular momentum vector as viewed from its principal axes. Thus, if the body were once set spinning about the axis with the greatest moment of inertia, it would always continue to spin almost exactly about the same principal axis no matter how that axis moves through the deforming body. The second and main contribution is our refutation of the widely accepted notion that the earth's figure shows unmistakable signs of the faster spin rate of the past. If correct, the degree of permanence of the rotation bulge so inferred by G. J. F. MacDonald (1963, 1965) and D. P. McKenzie (1966) would have been an effective impediment against any significant polar wandering of the earth as a whole. However, we show here that, after subtraction of the hydrostatic flattening, the remaining or nonhydrostatic part of the earth's inertia ellipsoid is distinctly triaxial. Such a triaxial shape, as well as the coincidence of the present rotation axis with the principal axis having the largest of the nonhydrostatic moments of inertia, is indeed to be expected of any randomly evolving, nearly spherical object without too much 'memory' for its past axis of rotation. Finally, we discuss briefly some statistical aspects of polar wandering on the assumption that the earth is such an object.

INTRODUCTION

The wealth of recent evidence on the drift of the earth's continents relative to one another seems to have obscured the role of polar wandering as a distinct though complementary phenomenon. One is tempted to say: If the continents do drift, won't the same mechanism displace them also with respect to the rotation pole?

Such intuition, it seems to us, is at once a truism, a fallacy, and a fairly profound observation. (1) It is a truism in the sense that, as reckoned from any given continental block sliding relative to its neighbors, some shifting of the pole position is almost inevitable. (2) It is a fallacy because, as is likewise well known, such ambiguities are far from the whole story: The pole position averaged over several continents appears to have moved roughly 90° during the past 300 or 500 m.y., whereas the angles through which most continents have drifted or turned relative to each other are not necessarily so large. We find it difficult to regard this gross polar wandering simply as an aggravated version of the ordinary continental drift, for that would require the mantle convection to possess an additional component of truly global amplitude and scale of coherence. (3) And yet, both the major polar wandering and the relative drift of the continents probably do share a common explanation. That basic cause indeed seems to be the limited mantle convection that is usually associated only with the *differential* motion of the continents!

To be specific, we suspect that the pole wanders chiefly because of slight rotational imbalances resulting from gradually changing den-

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sity inhomogeneities within the convecting parts of the mantle. This suggestion is by no means novel. In principle, at least, the same has already been expounded by *Gold* [1955], *Burgers* [1955], *Munk* [1956, 1958], *Inglis* [1957], and *Munk and MacDonald* [1960*a*], and by *Darwin* in his classic paper of 1877.

Even the extensive summary of such previous studies in chapter 12 of *Munk and MacDonald's* [1960*b*] book, however, preceded (1) most of the geopotential information now available from satellites, and (2) the increasingly persuasive arguments in favor of mantle convection to which we have already alluded. There has been surprisingly little emphasis on how well these additional facts complement the earlier theoretical considerations, and vice versa. On the contrary, the excess of the measured second zonal harmonic of the geopotential over its hydrostatically required value has since been repeatedly interpreted [*MacDonald*, 1963, 1965; *McKenzie*, 1966; *Kaula*, 1967] as indicating that the mantle possesses something like a 10^7-year 'memory' for the faster rotation rate of the past. As implied already by *Munk and MacDonald* [1960*b*] and noted explicitly by *McKenzie* [1966], such a degree of permanence of the main rotational bulge would indeed have precluded any significant polar wandering by the major part of the mantle. Thus, whoever accepts the memory is left with the unpalatable task of explaining how a relatively thin shell carrying the various continents and ocean basins has managed to slide, comparatively intact, a full 90° with respect to the underlying mantle.

The main objective of the present paper is to challenge this 'fossil bulge' interpretation and thereby reopen the theoretical discussion. Before doing that, however, let us review the basic mechanism for a type of rotating object whose polar wandering can be demonstrated rigorously.

Quasi-Rigid Body and Its Polar Wandering

Presumably, any convection within the nearly solid mantle of the earth occurs without much regard for the geographic orientation of its rotation axis. There might be a slight bias because this convection takes place within a spheroid rather than a sphere, but all Coriolis forces must be utterly negligible. For this reason, it is instructive to consider the polar wandering of an idealized body whose shape or configuration is completely unaffected by its rotation, but which nonetheless evolves with time owing to prescribed internal processes or motions. For brevity, we will call such an object a *quasi-rigid body*. In what follows, we assume that its rate of evolution is very slow in comparison to both its rotation and its free nutation, and that it experiences no net external torque.

On any short-term basis, our quasi-rigid body is almost indistinguishable from a truly rigid object in free rotation. Like the truly rigid body, it is capable of Eulerian nutations about either the principal axis with the largest of the three moments of inertia, or that with the smallest. What concerns us here, however, is the long-term behavior represented by the question: If the quasi-rigid body had a possibly finite nutation amplitude at the outset, and if subsequently its shape changed gradually, what would that amplitude have become at some later time?

This question is analogous to the classical problem of a particle oscillating in a slowly time-varying one-dimensional potential well [cf. *Kulsrud*, 1957; *Lenard*, 1959; *Landau and Lifshitz*, 1960]. As in that problem, our answer involves an 'adiabatic invariant,' namely some property of the motion that is conserved to exponential accuracy in the small parameter representing the typical rate of change of the gross properties of the body. The adiabatic invariant in the present case turns out to be the solid angle swept out by the angular momentum vector as viewed from the instantaneous principal axes of inertia. This implies that, if the axis of rotation of the quasi-rigid body did once coincide with its major axis of inertia, then (subject to certain restrictions to be stated later) it will always continue to do so to high accuracy, regardless of where that principal axis may have shifted relative to the 'geography.' And that indeed is an example of polar wandering.

Six independent functions of time suffice to describe the entire dynamical effect of the internal deformations of our quasi-rigid body. Three of those functions are the principal moments of inertia, $A(t) \leq B(t) \leq C(t)$, reckoned from the center of mass. They and the respective axes are determined by the mass distribution at every instant. The other three functions, $h_1(t)$, $h_2(t)$, and $h_3(t)$, are the components of any excess or 'relative' angular momentum associated with material motions relative to these mobile principal axes [cf. *Munk and MacDonald*, 1960*b*, pp. 9–11]. They can likewise be regarded as prescribed functions of time in the present context. They arise because the moments A, B, C, and the components $\Omega_i(t)$ of the angular velocity of rotation of the aforementioned principal axes do not alone define the angular momentum when the body is not truly rigid. For instance, there may be internal motions, and consequently a net angular momentum, even when the principal axes happen to be stationary.

The components of the total angular momentum $\mathbf{H}$, resolved with respect to the trio of principal axes, are thus

$$H_i(t) = I_i(t)\Omega_i(t) + h_i(t) \qquad (1)$$

if we write $I_1 = A$, $I_2 = B$, and $I_3 = C$, for short. As viewed from space, $\mathbf{H}$ remains invariant, but in this rotating system

$$d\mathbf{H}/dt = \mathbf{H} \times \mathbf{\Omega} \qquad (2)$$

Now let

$$\begin{aligned} x_i(t) &= H_i(t)/H \\ \alpha_i(t) &= h_i(t)/H \end{aligned} \qquad (3)$$

where $H = |\mathbf{H}|$. The elimination of $\mathbf{\Omega}$ from equations 1 and 2 then yields the following version of what Munk and MacDonald call the Liouville equation:

$$d\mathbf{x}/dt = \mathbf{x} \times \mathbf{D}(t) \cdot [\mathbf{x} - \boldsymbol{\alpha}(t)] \qquad (4)$$

Here $\mathbf{D}(t)$ represents a diagonal tensor with components

$$[\mathbf{D}(t)]_{ij} = [H/I_i(t)]\,\delta_{ij} \qquad (5)$$

For arbitrary distortions of the quasi-rigid body, these two equations describe fully the trajectory of the direction vector $\mathbf{x}(t)$ on the unit sphere affixed to its principal axes.

We would now like to point out that the same dynamics can also be described via certain conjugate variables and a Hamiltonian and that therefore the motion of $\mathbf{x}(t)$ belongs in a class of one-degree-of-freedom problems whose adiabatic invariants have already been established generally by *Lenard* [1959] and *Gardner* [1959]. The Hamiltonian is

$$\mathcal{H}(q, p; t) = (H^2/2)\mathbf{x} \cdot \mathbf{D}(t) \cdot [\mathbf{x} - 2\boldsymbol{\alpha}(t)] \qquad (6)$$

and the generalized coordinate q and its conjugate momentum p are

$$q = \lambda \qquad p = H(1 - \cos\theta) \qquad (7)$$

in terms of a co-latitude θ and longitude λ such that

$$\begin{aligned} x_3 &= \sin\theta\cos\lambda \\ x_1 &= \sin\theta\sin\lambda \\ x_2 &= \cos\theta \end{aligned} \qquad (8)$$

We leave it for the reader to verify that the canonical equations

$$dq/dt = \partial\mathcal{H}/\partial p \qquad dp/dt = -\partial\mathcal{H}/\partial q \qquad (9)$$

are then completely equivalent to vector equation 4.

As Lenard and Gardner do, we suppose $\mathcal{H}$ to be differentiable to all orders with respect to q, p, and t. Also, as they do, we assume that $\mathcal{H}$ is time-independent at least for short intervals immediately before $t = 0$ and following $t = T > 0$. We further assume that $\mathcal{H}(q, p; t) = F(q, p; t/T)$, or that the *manner* of change of the parameters may be divorced from its rate. And finally—although not without qualification—we note that Lenard's explicit requirements that 'the $\mathcal{H}$ = constant curves form a family of concentric simple closed curves in the (q, p) plane' and that '$\mathcal{H}$ is a monotone increasing [or decreasing] function along the outward normal to the curves' (for any fixed t) are also met here.

This last sentence must be qualified slightly because our (q, p) coordinates refer to locations on a sphere. The present $\mathcal{H}$ = constant curves are in fact the intersections of the (constant energy) ellipsoids

$$(x_1 - \alpha_1)^2/A + (x_2 - \alpha_2)^2/B + (x_3 - \alpha_3)^2/C = \text{const.} \qquad (10)$$

with the unit sphere $x_1^2 + x_2^2 + x_3^2 = 1$. If only for this reason, they must divide into at least two families of simple closed curves.[1] In a local sense, each of those encircles one stable fixed point on the sphere. Such multiplicity was not explicitly foreseen either by Lenard or by Gardner. Their proofs still apply, however, provided that we limit ourselves to motions that remain associated with only a given 'fixed' point, even though that point itself may shift with time. Fortunately, this requirement is best checked retrospectively.

The only other qualification is almost trivial: Both Lenard and Gardner stipulated 'closed curves in the (q, p) plane.' This is not quite the same as closed curves on the sphere, for q or λ would change by 2π around any curve separating the two coordinate poles. That difficulty can, however, be circumvented (at least for small enough nutation amplitudes) simply by a different choice of the main axis of reference (cf. equation 8).

With these reservations, the adiabatic invariance proved by those earlier authors can now be paraphrased as follows: Let J_0 and J_1 denote the values of the action integral $\oint p\,dq$ associated with some arbitrary trajectory $\mathbf{x}(t)$ of the quasi-rigid body before $t = 0$ and after $t = T$, respectively. Then, regardless of any further details of the evolution of that body during $0 < t < T$, the difference

$$|J_1 - J_0| = O(T^{-n}) \qquad (11)$$

for every positive n as $T \rightarrow \infty$. In other words, although $\oint p\,dq$ may not be an exact invariant, it is conserved to remarkable accuracy when a prescribed total change of body parameters occurs sufficiently slowly. (Unfortunately, like most other adiabatic invariants, this asymptotic result does not itself define 'sufficiently slowly.' It seems clear, however, that the implied comparison here refers to the *slowest* of the nutations experienced at any stage of a given evolution.)

In the present context, the nearly invariant

$$\oint p\,dq \equiv H \oint (1 - \cos\theta)\,d\lambda \qquad (12)$$

is simply H times the solid angle enclosed by the motion $\mathbf{x}(t)$ when periodic. This simple result does more than just comment on the nutation. It applies equally to polar wandering, as emphasized by its following corollary: If a once rigid body, initially in pure rotation about its principal axis with the largest of three unequal moments of inertia, were transformed gradually into a new rigid shape by internal processes alone, its subsequent rotation axis would still coincide almost exactly with that 'most principal' axis of inertia, *wherever that may have shifted*.[2] In other words, if no Eulerian wobble had existed at the outset, and if all changes occurred slowly, hardly any wobble would have been excited in the process. Yet the rotation pole itself could have been displaced through an unlimited angle relative to most of the material.

We end this rather abstract discussion with an offshoot of *Gold*'s [1955] example of a beetle on a perfect rotating sphere. (That original example is slightly singular, since the two larger moments of inertia are then equal.) Instead of the single beetle, we suppose here that a colony of N beetles inhabits the surface of the sphere and that each beetle crawls along some random path determined only by itself. These goings-on are caricatured in Figure 1, where the gridwork on the sphere represents meridians and circles of latitude dating from some past epoch. That combined system is indeed a quasi-rigid body by our definition.

The most interesting thing about this picturesque example is *not* that the axes of inertia (and therefore the rotation axis) will shift with time. It is rather that the rate of movement of such axes, as viewed from the sphere, exceeds the net speeds of the beetles themselves by a factor of the order of $N^{1/2}$. This estimate stems from the fact that much of the correlation with

[1] In any mantle convection, the dimensionless 'relative' momenta α_i would be only of order (1cm/yr)/(300m/sec) $\cong 10^{-12}$. This is much less than even the smallest relative difference of order 10^{-5} between the various moments of inertia I_i of the earth. With such parameters, the $\mathcal{H}$ = constant curves for the quasi-rigid body would be only slight variants of the usual angular momentum paths found during the free rotation of a strictly rigid asymmetric body [cf. *Landau and Lifshitz*, 1960, Fig. 51], and the curve families would consist of the usual four.

[2] Strictly speaking, this application of our theorem requires that a finite difference be maintained at all times between the largest and the intermediate moments of inertia, lest the relevant nutation become unduly sluggish. This limitation is probably only formal.

the gross properties of an earlier bug distribution should be lost by the time a typical creature has covered the distance to its neighbor; by then the 'most principal' axis ought to have shifted through an angle of the order of 90°. For large N, this is an important reminder that even very modest relative displacements of material in (or on) an almost spherical object can result in a truly large-scale polar wandering. (Needless to say, the movements of the beetles must be very slow, if their cumulative effect on the rotation is to be significant. To establish a criterion, let β be the ratio of the masses of a typical bug and of the sphere. Then the instantaneous moments of inertia of the sphere plus passengers will differ from each other by fractions of the order $N^{1/2}\beta$, and the typical nutation period will be $(N^{1/2}\beta)^{-1}$ times that of the rotation. If even this time is to be short in comparison with the time-scale of drift of the axes of inertia, the hypothetical insects must be forbidden to crawl more than a fraction of a radian during β^{-1} revolutions.)

The polar wandering of our quasi-rigid body has been intended mainly to illustrate the presumed fundamentals of the polar wandering of the earth itself. The earth differs from a quasi-rigid body in that it is both dissipative and flabby. Yet, those differences may not be significant. The dissipation (coupled with the ability to flex on a rotation time scale) would obviously tend to damp the Chandler wobble, but we have already seen that hardly any such nutation about the C axis is excited during the slow

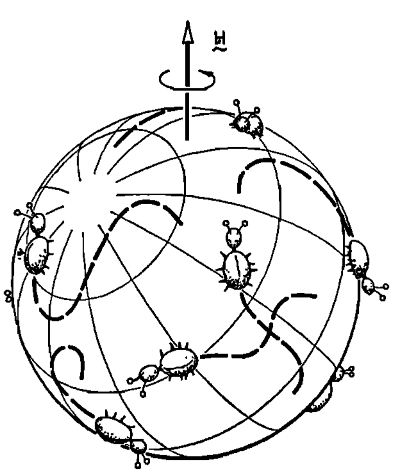

Fig. 1. A quasi-rigid body.

evolution of the quasi-rigid body. Likewise, any rotational bulge involving only elastic or truly hydrostatic departures from a sphere should be relatively unimportant, since it seems clear that 'a rapidly spinning rubber ball would orient itself relative to the rotational axis in accordance with tiny surface markings,' to quote *Munk* [1958].

The only major uncertainty, as the same authors have abundantly emphasized, arises from those long-term properties of the mantle which determine the ease with which its rotational bulge could have adapted itself to a gradually changing axis of rotation. (This very question represents a departure from the concept of a quasi-rigid body, for which it made no sense to ask whether a given change of shape was 'easy' or difficult. In the case of the earth, however, where only mass inhomogeneities in excess of the dynamically required flattening seem a plausible cause of polar wandering, the degree of permanence of any given—and roughly 100 times larger—equatorial bulge is clearly of great interest.) Unfortunately, the evidence relating to this question is still very sparse. For instance, since any mantle convection or modal damping may be—and every example of post-glacial uplift certainly is—relatively shallow, it is not at all clear that the values of viscosity, etc., so deduced apply to the major parts of the mantle. Nor has solid-state theory yet provided sufficiently firm parameters; arguments have been advanced both for and against the deep mantle possessing a finite permanent strength. Only satellite measurements of the geopotential have furnished significant information. But even that information, as we are about to show, has been somewhat misinterpreted.

Nonhydrostatic Moments of Inertia of Earth

The bit of mischief we would now like to rectify concerns data about which there remain little disagreement. These data consist primarily of the coefficients

$$\left.\begin{aligned} \bar{C}_{20} &= -484.2 \\ \bar{C}_{22} &= 2.4 \\ \bar{S}_{22} &= -1.4 \end{aligned}\right\} \times 10^{-6} \qquad (13)$$

[cf. *Kaula*, 1966; *Wagner*, 1966] of the three significant $l = 2$ terms in the familiar spherical harmonic expansion

$$V = (GM/r)\left\{1 + \sum_{l=2}^{\infty} \sum_{m=0}^{l} (a/r)^l \bar{P}_{lm}(\sin \phi) \cdot [\bar{C}_{lm} \cos m\lambda + \bar{S}_{lm} \sin m\lambda]\right\} \quad (14)$$

for the geopotential, as determined from satellite orbit perturbations. Here ϕ, λ, and r are the latitude, longitude, and radial distance, M is the earth's mass, a is its mean equatorial radius, G is the gravitational constant, and the $\bar{P}_{lm}$ are associated Legendre functions normalized so that

$$\int_0^{\pi/2} [\bar{P}_{lm}(\sin \phi)]^2 \cos \phi \, d\phi = 2 - \delta_{0m} \quad (15)$$

or that the integral of the square of every surface harmonic over the unit sphere equals 4π.

The uncertainties in the above values of $\bar{C}_{20}$, $\bar{C}_{22}$, and $\bar{S}_{22}$ do not exceed one unit in the last decimal place. (To the same accuracy, the measured $\bar{C}_{21}$ and $\bar{S}_{21}$ are both zero, consistent with the smallness of the observed Chandler wobble.) These coefficients are intimately related to the differences

$$[C - \tfrac{1}{2}(B + A)]/Ma^2 = -5^{1/2}\bar{C}_{20} = J_{20}$$

$$(B - A)/Ma^2 = (20/3)^{1/2}(\bar{C}_{22}^2 + \bar{S}_{22}^2)^{1/2} = -4J_{22} \quad (16)$$

between the principal moments $A < B < C$ of the present mass distribution of the earth. They also imply that the axis with the least moment A coincides roughly with the intersection of the equatorial plane and the 15°W–165°E meridional plane.

Likewise well established by now is the difference between the polar and the equatorial moments of inertia that the earth should exhibit if it were in complete hydrostatic equilibrium at its present rate of rotation. It is known theoretically [e.g., *Jeffreys*, 1959] that this flattening depends strongly on the assumed mean moment of inertia and only weakly on higher-order moments of the mass distribution. Seismic models have long sufficed for the latter, but the former became known to sufficient accuracy only with the satellite determination of J_{20}. This, combined with the precessional constant

$$H = [C - \tfrac{1}{2}(B + A)]/C \cong 1/305.6 \quad (17)$$

gave

$$C/Ma^2 = J_{20}/H \cong 0.3308 \quad (18)$$

Henriksen [1960] appears to have been the first to use essentially this datum to estimate the hydrostatic flattening. He assumed that the ratio C/Ma^2—and, of course, the mass and volume, but not necessarily the polar moment C nor the equatorial radius a nor the coefficient J_{20} nor even the precessional constant H—of such a body would be the same as that for the existing earth. (This assumption is defensible because (1) the effect of mass irregularities on that ratio should be only of $O(10^{-5})$, and (2) even a complete subsidence of the full rotation bulge should affect only its last digit.) Henriksen's prediction was corrected slightly by *Jeffreys* [1963], who concluded in effect that the coefficient of the second zonal harmonic for a truly fluid earth should be

$$(\bar{C}_{20})_{\text{equil.}} = -(479.5 \pm 0.2) \times 10^{-6} \quad (19)$$

The question has been further examined by *Caputo* [1965]. His $(\bar{C}_{20})_{\text{equil.}} = (1082.7 - 9.05) \times 10^{-6}/\sqrt{5} \cong 480.15 \times 10^{-6}$, obtained by using the same reasonable assumptions (as opposed to cases like J_{20} = fixed that were also considered by him), is probably a better indication of the remaining uncertainty than Jeffreys' error estimate. In what follows, we use Jeffreys' value of $(\bar{C}_{20})_{\text{equil.}}$ because Caputo's value would only strengthen our case.

This, then, is the evidence from which stems the widely held notion that even the nonhydrostatic part of the earth's figure consists predominantly of an equatorial bulge, i.e., that it is reasonably approximated by an oblate spheroid.

That characterization is, however, false. To see this, just imagine that the earth's rotation were halted and that the present rotational bulge, but nothing else, were allowed to subside. The coefficients $\bar{C}_{20}$, $\bar{C}_{22}$, and $\bar{S}_{22}$ would then become

$$\left.\begin{aligned} \bar{C}_{20}' &= \bar{C}_{20} - (\bar{C}_{20})_{\text{equil.}} = -4.7 \\ \bar{C}_{22}' &= \bar{C}_{22} = 2.4 \\ \bar{S}_{22}' &= \bar{S}_{22} = -1.4 \end{aligned}\right\} \times 10^{-6} \quad (20)$$

except for negligible higher-order corrections, and the corresponding moments of inertia A', B', C' would differ by the almost *equal* increments

$$\begin{aligned} C' - B' &= 6.9 \times 10^{-6} Ma^2 \\ B' - A' &= 7.2 \times 10^{-6} Ma^2 \end{aligned} \tag{21}$$

From those differences alone, one would be hard pressed to describe the hypothetical nonrotating earth as either an oblate or a prolate spheroid. In fact, it is no spheroid at all, but a good example of a *tri*axial object!

This conclusion would have come as no surprise if we had instead been conditioned to regard the nonhydrostatic earth as a collection of more or less random density inhomogeneities. As an illustration, consider again the example of Figure 1. Suppose there are ten beetles on that sphere, distributed independently with uniform probability. Treat them as equal point masses of randomly chosen sign (this treatment to avoid any bias of dealing only with positive mass 'inhomogeneities') and the sphere as homogeneous and of vastly larger mass. It is then a well-posed problem to inquire about the probability distribution of the ratio $f = (C - B)/(C - A)$ of the principal moments of the sphere plus beetles about their joint center of mass. The histogram in Figure 2 answers this question numerically; it is a record of 2000 such random realizations performed on a computer. Figure 2 emphasizes that it is only the nearly oblate ($f \cong 1$) or the almost prolate ($f \cong 0$) random bodies that are comparatively rare.

It is amusing that even the moon, with $f \cong 0.63$ [cf. *Koziel*, 1967], is proportionately more oblate than the nonhydrostatic earth, whose $f' = (C' - B')/(C' - A') \cong 0.49$ (or whose $f' \cong 0.43$, using Caputo's estimate of the hydrostatic flattening).

In short, the only remarkable thing about the object described by equations 21 is that its actual rotation coincides with the axis with the largest of the residual moments of inertia. At first thought, either of the other two principal axes might have seemed an equally valid alternative, since the much larger hydrostatic bulge is yet to be superposed.

But even this does not imply that the earth exhibits a clear excess of *rotational* flattening. Admittedly, the positive difference $C' - \frac{1}{2}(B' + A') = 10.5 \times 10^{-6} Ma^2$ is still something like an oblateness, and this indeed exceeds the difference $B' - A'$ between the two equatorial moments. Yet, by the same reasoning, one would also conclude that the excess of the 'prolateness' $\frac{1}{2}(C' + B') - A' = 10.7 \times 10^{-6} Ma^2$ over $C' - B'$ indicates a similar deficiency of matter all around the 115°W–75°E meridional plane. No one has yet proposed that such a relative depression has anything to do with some past rotation about the corresponding equatorial axis.

It seems to us that a much sounder explanation of that preference of axis is the old suggestion that the nonhydrostatic density inhomogeneities (just as the beetles on the sphere in our quasi-rigid body discussion) always 'steer' the rotation axis so as to maximize the resultant polar moment of inertia. In other words, like *Munk and MacDonald* [1960*a*, p. 2171], we believe that

> the simplest hypothesis is that the present pole has in fact a position in accordance with these unknown features [i.e., the inhomogeneities]. If during the geologic past these unknown features were differently distributed, presumably the pole was in a different position as well.

Of course, unlike the beetles, those inhomogeneities have also to contend with the deforma-

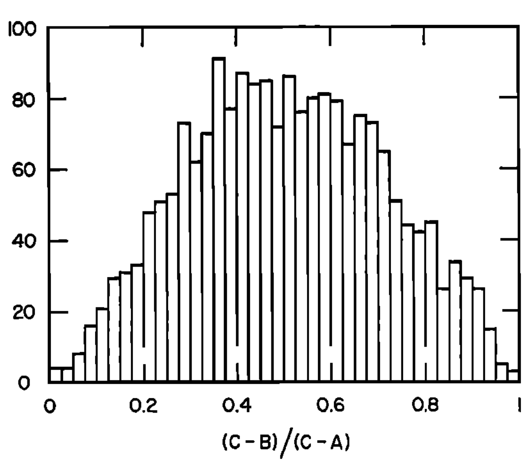

Fig. 2. Distribution of the relative differences of the moments of inertia of 2000 nearly spherical random bodies. Counted vertically is the number of occurrences of the ratio $(C - B)/(C - A)$ in each interval.

tion of their carrier by its rotation. But once the 'fossil bulge' argument is discarded as highly suspect, there remains no empirical evidence to indicate that the mantle possesses either sufficient viscosity or permanent strength to prevent such polar wandering.

In a sense, our story ends here. However, because the excess bulge hypothesis has become rooted rather firmly in geophysical thought and because the errors involved seem in retrospect both interesting and instructive, we conclude with a review of its history.

To our knowledge, the suggestion that the earth's figure shows evidence of a faster rotation from the past was first put forward by *Munk and MacDonald* [1960*a, b*] shortly after the first satellite determinations of J_{20}. At that time, J_{20} was the only reliably determined coefficient of the geopotential. Values of some of the other zonal harmonic coefficients ($m = 0$) were only beginning to be obtained, and as yet there were no determinations of the tesseral or sectorial coefficients ($m \neq 0$). Thus it was not immediately possible to test whether the excess symmetric flattening about the rotation axis is really much greater than the other departures of the earth's figure from hydrostatic equilibrium.

When some moderately good estimates [e.g., *Kozai*, 1962] of those other coefficients had become available, it was *MacDonald* [1963, 1965] who concluded that the nonhydrostatic equatorial bulge is indeed the principal distortion. He used essentially the present J_{20} and $(\bar{C}_{20})_{\text{equil.}}$, and a J_{22} only slightly smaller than that of equations 13 and 16. The ratio of the nonhydrostatic $|J_{20}'/J_{22}| = 12.0/1.6 = 7.5$ cited by MacDonald seemed amply to prove his point.

What then is the source of the disagreement? Principally, but not exclusively, it is a matter of normalization: The coefficients J_{20} and J_{22} refer to an expansion of the geopotential that is similar to equation 14, but in which the Legendre polynomials are the standard $P_{20}(x) = \frac{1}{2}(3x^2 - 1)$, $P_{22}(x) = 3(1 - x^2)$, etc. The mean square value of $P_{20}(\sin \phi)$ over the unit sphere is $4\pi/5$, whereas that of the surface harmonic P_{22} $(\sin \phi) \cos 2\lambda$ is $48\pi/5$, or 12 times larger. To remove this prejudice, MacDonald's coefficient ratio must be reduced by a factor $(12)^{1/2}$, or to about 2.2. For this very reason, we ourselves have used Kaula's 4π-normalized harmonics (cf. equation 15) from the outset; those and the more modern values cited in equation 20 determine the above ratio as

$$\bar{C}_{20}'/(\bar{C}_{22}' + \bar{S}_{22}')^{1/2} \cong 1.7$$

Even this deflated ratio still implies that the second zonal harmonic has an amplitude fully 2.4 times as great as the average (in the rms sense) of the two sectorial harmonics. It is thus not surprising that *McKenzie* [1966], using *Guier and Newton*'s [1965] coefficients, estimated the respective energies to be in the ratio $112/17.8 \cong 6.3$ (or roughly $(2.4)^2$), or that *Kaula* [1967] still felt that the excess $\bar{C}_{20}$ probably 'has a special explanation associated with the rotational deceleration.'

All the above estimates failed to reckon, however, with another and more insidious source of bias that is inherent in every spherical harmonic representation. This is the bias introduced by the choice of the axis.

To explain this concisely, let us just postulate that the distortional energy of a triaxial ellipsoid of semiaxes $(1 + \epsilon_1)$, $(1 + \epsilon_2)$, and $(1 + \epsilon_3)$ is

$$E = \epsilon_1^2 + \epsilon_2^2 + \epsilon_3^2 \qquad (22)$$

in appropriate units, provided that the strains ϵ_1, ϵ_2, and ϵ_3 are infinitesimal and $\epsilon_1 + \epsilon_2 + \epsilon_3 = 0$. The departures of this ellipsoid from the unit sphere can obviously be described entirely in terms of second-degree surface harmonics, and so, of necessity, can the energy.

Within this framework, consider how the total energy $E = 2\epsilon^2$ of an ellipsoid of semiaxes $(1 + \epsilon)$, 1, and $(1 - \epsilon)$ should be apportioned between the zonal and the other harmonics. First, pick the eventual $(1 - \epsilon)$ axis as the main reference axis for the harmonics, and imagine that the deformation of the original sphere into the ellipsoid has taken place in the following zonal (i.e., axisymmetric) and nonzonal stages:

$$\text{Ellipsoid} = \text{Sphere} + (\tfrac{1}{2}\epsilon, \tfrac{1}{2}\epsilon, -\epsilon) + (\tfrac{1}{2}\epsilon, -\tfrac{1}{2}\epsilon, 0) \qquad (23)$$

The sequence of those changes does not matter. As they are written, however, the first involves an expenditure of energy equal to $(\frac{1}{2}\epsilon)^2 + (\frac{1}{2}\epsilon)^2 + (-\epsilon)^2 = 3\epsilon^2/2$ and the second only $\frac{1}{2}\epsilon^2$.

From this, it is indeed correct to conclude that such a zonal harmonic of the deformation contains 3 times as much energy as the harmonic involved in the second transformation, or *6* times as much as the average of both sectorial harmonics. Yet, to see that even this factor of 6 has nothing like the significance purported by McKenzie, now view the deformation instead as

$$\text{Ellipsoid} = \text{Sphere} + (\epsilon, -\tfrac{1}{2}\epsilon, -\tfrac{1}{2}\epsilon) + (0, \tfrac{1}{2}\epsilon, -\tfrac{1}{2}\epsilon) \qquad (24)$$

Then the first change again involves a zonal harmonic with energy $3\epsilon^2/2$, and the second change requires only one-third as much energy. Note, however, that this predominant zonal and the lesser sectorial harmonic are now reckoned from the eventual $(1 + \epsilon)$ axis!

Discussion

It is understandable that the current wave of enthusiasm for continental drift has not yet embraced the closely related hypothesis of polar wandering: There has been much temptation to regard the polar wandering as merely a scaled-up version of the continental drift.

The burden of this paper has been to reemphasize that large relative displacements of material need not be invoked to account for large shifts in the location of the pole. As we have reviewed by means of the theorem and example in the second section, quite modest redistributions of mass within the earth can give rise to large excursions of the rotation axis relative to the entire crust and mantle. On that hypothesis, the wandering rotation axis always approximates the 'most principal' axis of inertia of the density inhomogeneities in the convecting parts of the mantle. (It should be recalled that one of the earliest triumphs of satellite geodesy was the demonstration by *Munk and MacDonald* [1960*a*] that the differences among the earth's nonhydrostatic moments of inertia exceed by more than an order of magnitude the contributions calculated from the ocean-continent distribution. Thus the inhomogeneities of the crust, and in particular any relative displacements of isostatically compensated continents, may be safely ignored in this context.)

The latter type of polar wandering, as opposed to the sliding of crust over mantle, of course presupposes that it is not too difficult for any given rotational bulge to flow into conformity with a newly desired axis of rotation. The recent acceptance of the 10^7-year fossil bulge and of the 'viscosity' of the deeper mantle that it implies have caused this mechanism for polar wandering to fall into disfavor. But now that the bulge hypothesis itself has become very suspect, this second mechanism deserves to be resurrected.

The following numerical experiment illustrates better than words alone the degree to which such a hypothesis complements the impression that the mantle convection is (1) relatively *disorganized* or chaotic on a global scale, and (2) *unsteady* in the sense that a typical 'eddy' or anomaly evident in the geoid [cf. *Kaula*, 1967, Fig. 1] persists only for a limited time.

In this experiment, we simulated a randomly evolving, almost spherical body by first generating a long sequence of random points on a unit sphere (just as for Figure 2). We then assigned to the nth point a supposedly minute reference 'mass' M_n^0 chosen from a Gaussian distribution with zero mean. Finally, each mass was turned on and off gradually according to the rule

$$M_n(t) = M_n^0 \exp\left[-\pi\left(t - \frac{n}{25}\right)^2\right] \qquad (25)$$

This rule was meant to idealize the growth and decay of any single density anomaly only as it affects the moments of inertia; hence, we did not bother to conserve total mass or to spread the disturbance over a finite area. The constants were so chosen that the equivalent duration

$$\int_{-\infty}^{\infty} [M_n(t)/M_n^0]\, dt = 1 \qquad (26)$$

for every n and that roughly twenty-five mass points were 'on' with at least half-intensity at any given instant. By using a computer, it was a simple matter to follow the evolution of the moments $A \leq B \leq C$ and of the axes of inertia of such an object with 'time' t. A fairly typical (but obviously subjective) sample of the results is contained in Figures 3 and 4. Figure 3 shows the continuous random walk of the C axis relative to the underlying sphere, the coordinates on that sphere having been redefined post facto so as to place one end of that axis at the 'north pole' at $t = 0$. Figure 4 shows the correspond-

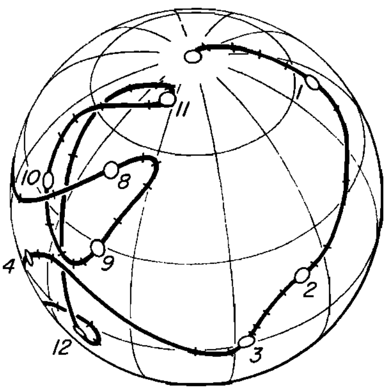

Fig. 3. A simulated curve of polar wandering. The meridians and the circles of latitude on the sphere are both drawn 30° apart. The markers along the path denote 'time' $t = 0.2, 0.4, 0.6$, etc.

ing history of the moments A, B, and C relative to their mean, which was held fixed for the purpose of plotting.

A striking feature of Figure 3 is again the large amplitude of such polar wandering, but the time scale is just as important. As explained by the fact that successive versions of our random body are almost totally uncorrelated beyond intervals $\Delta t = 2$, we see that the C axis moves, say, 1 radian in typically 1 or 2 time units. This impression is reinforced by the table

Δt	1	2	3	4
$\langle\Psi\rangle$	54°	71°	77°	81°
$(\Psi)_{\text{median}}$	56°	67°	75°	81°

Cited there as functions of Δt are the mean and the median values of the *net* angular separations

$$\Psi = \cos^{-1} [\mathbf{k}(t)\cdot\mathbf{k}(t + \Delta t)] \qquad (27)$$

of the unit axial vectors $\mathbf{k}$ observed for the entire run from $t = 0$ to $t = 30$ (or considerably farther than is shown in either figure).

Translated into geological terms, the above illustrates that, *on the average,* the earth's rotation pole should have wandered 90° in a time slightly longer than the duration of a typical convection element. The actual time scales seem indeed to be so related to each other: On the one hand, paleoclimatic data [cf. *Blackett,* 1961], the combined European and North American paleomagnetic data [cf. *Runcorn,* 1965], and studies such as *Creer*'s [1965] reconstruction of the large movement of the south pole relative to three southern continents hint that the last 90° of polar wandering occurred during the past 300 to 500 m.y. On the other hand, the opening up of the Atlantic and the closing of the Tethys Sea since the middle Mesozoic, the rates of recent sea-floor spreading of the order of several centimeters per year, and the 100 m.y. subsidence of the Darwin rise [*Menard,* 1965] all point to significant convection 'cells' or ridge systems of the recent past as having endured at least 150 m.y. but not necessarily longer than 250 m.y.

Also to be stressed, however, is the great variability in Figure 3 of the instantaneous (and even of some average) rates of wandering of the C axis, despite the fact that the rate of evolution of the mass irregularities themselves is here statistically quite uniform. This is not a peculiarity of the chosen example. It was observed in all other calculations as well. As the reader himself can verify by referring also to Figure 4, those rates show a pronounced negative correlation with the difference $C - B$. That inverse relationship merely reflects the greater ability of slight additional perturbations to turn the axes when the moments of inertia are almost equal than when they are not. Nevertheless, the likelihood of occasional large

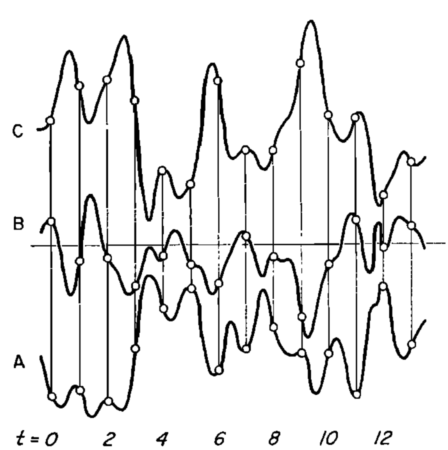

Fig. 4. History of the moments of inertia of the body referred to in Figure 3.

and rapid swings like those near $t = 3.5$ and $t = 11.8$, representing virtual swaps of the C and B axes, seems decidedly greater here than in any ordinary random walk. In fact, the full calculation over 30 time units revealed no fewer than six distinct instances where $\Psi > 45°$ for $\Delta t = 0.2$.

On the other hand, the same variability also warns that the pole may on occasion have lingered in some vicinity without implying that the mantle convection had then become either steady or non-existent (although the converse *is* true in the present scheme of things). This is another reason why polar wandering of the over-all sense considered here seems preferable to the alternative of a really large-scale (meaning $n = 1$ or $n = 2$ harmonic) component of mantle convection [e.g., *Runcorn*, 1968]: Even if such a vast streaming were established, it seems hard to understand why it should be as erratic both in its rate and in its direction as is suggested by Figure 3 and also, as Runcorn himself seems to concede, by the paleomagnetic data.

Is there any geophysical evidence of the expected inverse correlation of the rate of wandering with $C' - B'$ (in the terminology of the preceding section)? Unfortunately, we do not know the past history of that difference of the nonhydrostatic moments of inertia. However, judging from the present $f' \cong \frac{1}{2}$ and especially from the fact that the present rms value

$$\sigma_2 = [(\bar{C}_{20}'^2 + \bar{C}_{21}^2 + \bar{C}_{22}^2 + \bar{S}_{21}^2 + \bar{S}_{22}^2)/5]^{1/2} \tag{28}$$
$$\cong 2.4 \times 10^{-6}$$

of the second harmonic coefficients of the geopotential matches *Kaula*'s [1967] 'rule of thumb'

$$\sigma_n \cong 10^{-5}/n^2 \tag{29}$$

for the coefficients of higher order, one would guess that the rate of polar wandering in, say, the last 100 m.y. has perhaps been slightly less than the historical average. This, too, is consistent with paleomagnetic data [cf. *Runcorn*, 1965] as far back as the Cretaceous. At any rate, there is no geologic evidence of any recent pole travel as vigorous as the 90° motion over roughly a 200-m.y. span from the Silurian to the Permian, which *Creer* [1965, Fig. 9] deduced relative to Africa, South America, and even Australia.

Finally, let us reconsider what this presumed mobility of the pole actually demands of the long-term mechanical properties of the mantle. As we said before, the great uncertainty that still plagues any dynamical description of polar wandering is the choice of an appropriate stress-strain relation. Does the earth possess finite strength? If not, does the rate of strain depend linearly on the stress?

As yet, there are no thoroughly reliable answers to these basic questions. Nevertheless, let us assume, as *MacDonald* [1963], *McKenzie* [1966], and others have done, that at least the major parts of the mantle consist of a homogeneous classical fluid of constant kinematic viscosity ν. If such an 'earth' were uniform throughout, its time constant for the adjustment of any second-harmonic distortions would be

$$\tau_M = (19/2)(\nu/ga) \tag{30}$$

where g and a are the surface gravity and radius, respectively. According to McKenzie, the inclusion of a core of realistic proportions shortens this time by a factor 1.4, and the assumption that the top 300 to 1000 km can likewise support no sensible viscous stress reduces it further by as much as an order of magnitude. To err on the safe side, however, we adopt

$$\tau_M \cong 2(\nu/ga) \tag{31}$$

where ν now refers only to the deeper parts of the mantle.

This time constant is relevant to polar wandering because the distortion resulting from a change of the axis of rotation (like that from a change of the rate of spin) involves surface harmonics only of degree two. It is not sufficient, however, that this value just be shorter than the time scale of the contemplated polar wandering. A much more stringent requirement, as *Gold* [1955] and others have noted, is that the angular rate of viscous polar wandering,

$$\Omega_{pw} = I_{13}/[\tau_M(C - A)], \tag{32}$$

that would result from torques associated with the largest possible *product* of inertia, say I_{13} (or $ma^2 \sin\theta_0 \cos\theta_0$ in McKenzie's notation), of the density inhomogeneities themselves must at least equal the desired rate Ω_{pw}.

It seems unreasonable to expect I_{13} to have exceeded $\frac{1}{2}(C' - A')$ from equation 21. Hence,

$$\begin{aligned} \tau_M &< \tfrac{1}{2}[(C' - A')/(C - A)]\Omega_{pw}^{-1} \\ &\cong 6 \times 10^{-3}\Omega_{pw}^{-1} \end{aligned} \tag{33}$$

and it follows from equation 31 that

$$\nu < 1.5 \times 10^{25} \text{ c.g.s.,} \tag{34}$$

if it is assumed that $\Omega_{pw} = 1.3 \times 10^{-16}$ sec^{-1}, which corresponds to 90° of travel in 400 m.y.

This new upper bound to the effective kinematic viscosity of the lower mantle is approximately a factor 40 lower than McKenzie's estimate, which assumed that $\tau_M = 10^7$ years. Our reduced value would, however, permit only a very sluggish and delayed response of the rotation axis to the wandering of the 'most principal' axis defined by the evolving inhomogeneities. The apparent non-uniformity of the recorded rate of polar wandering therefore argues that a reasonable maximum to the effective ν may be yet another order of magnitude smaller. Even the latter limit still exceeds by more than two orders of magnitude the viscosity estimated for the upper mantle from the rebound of Fennoscandia [*Haskell*, 1937; *McConnell*, 1965].

These reduced estimates of the viscosity also remove McKenzie's objection to the thermal convection of the lower mantle. But it is not for us to speculate whether such deep convection does in fact occur.

Acknowledgments. This study grew out of discussions between L. N. Howard, S. A. Orszag, and ourselves, begun during the 1966 summer program in Geophysical Fluid Dynamics at the Woods Hole Oceanographic Institution.

This work was supported in part by grants from the National Science Foundation and the National Aeronautics and Space Administration.

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(Received September 2, 1968.)