Response to referee

Ian Rose and Bruce Buffett

We thank Yanick Ricard for his thoughtful review. In addition to several smaller suggestions and corrections, his review centers on our treatment of the reference frames relevant to the scaling of true polar wander (TPW). We append his review to this response for convenience. We broadly agree with his criticism, and have substantially reworked our discussion of reference frames to address it. Several of the key equations have been corrected, and we have changed some of the notation for clarity, but the overall conclusions of the manuscript are unchanged.

In particular, we have done the following to clarify and correct the treatment of reference frames:

1. We have added a section (Section 2.2) which makes the differences between reference frames explict. We identify three frames which are important to the problem: (1) the inertial, nonrotating frame, (2) A body-fixed geographic frame, rotating with respect to the inertial frame with relative rotation vector $\mathbf{\Omega}$, and (3) The frame described by the principal axes of the convective moment of inertia $\mathbf{E}$, which we call the $\mathbf{E}$-frame. This frame rotates slowly with respect to the body-fixed frame with rotation vector $\mathbf{\Psi}$.

2. We have added a new figure (Figure 1) which illustrates the different reference frames.

3. We have changed the discussion in Section 2.5 to clarify that the rate of TPW is measured in the body-fixed frame. The rate of TPW we denote by $\dot{\Theta}$. The physics describing the magnitude of $\dot{\Theta}$ are more naturally expressed in the $\mathbf{E}$-frame, with colatitude θ and longitude ϕ. For much of the discussion we make the simplifying assumption that $\phi = 0$. If the $\mathbf{E}$-frame is not rotating with respect to the body fixed frame ($\mathbf{\Psi} = 0$) then $\dot{\Theta} = \dot{\theta}$.

4. We have updated the discussion of the time evolution of the mismatch angle θ (Section 4.2) in light of the changes to the reference frames. The characteristic size of θ is set by the competition of its decay via TPW and

its growth via $\mathbf{\Psi}$. This is reflected in Equations (43) and (44). Our scaling furnishes estimates of the sizes of these two processes.

Besides the discussion of reference frames, we have addressed the following other issues raised in the review:

- We have added the clarifying intermediate step in Equation (7).
- We have added Equation (21) on the source of the gravitational field.
- We have changed the notation of the ratio of centrifugal to gravitational forces from the fluid-dynamics-inspired "Froude number" to the symobl m more commonly used in geodesy.
- We have clarified discussion of transforming the linear momentum equation to the angular momentum equation (Equations (25)-(29)). The review criticized this transformation as not very useful. While we agree that these equations are not particularly easy to evaluate in practice, they do serve two useful purposes for our scaling: First, they establish the overall consistency between the approximations made for the linear momentum equation (Section 3) and those made for the angular momentum equation (Section 2). Second, Equations (23)-(29) demonstrate the origin of the nondimensionalization that we use for our scaling. In particular, Equations (28)-(29) shows the origin of $\alpha\Delta T$ as an independent nondimensional number.
- We have added additional note on the effect of dynamic compensation on the value of $(1 + k_f^L)$
- The review suggested that stirring and mixing can populate lengthscales smaller than d in the convecting system, where d is the injection lengthscale, roughly set by the thickness of the boundary layers. While mixing can do this, we argue that those lengthscales are quickly homogenized by thermal diffusion, whereas thermal anomalies at larger lengthscales persist for a much longer time.

REVIEW

Y RICARD

This is a very interesting paper that tries to estimate the TPW rate of a convecting body as a function of the Ra number. This has never been done and I hope to see it published eventually. However in its present form, the paper is not acceptable. As far as I understand, 3 angles enter the problem: the angle between geography (say the no-net rotation plate tectonic frame or the hotspot frame) and the rotation axis (whose variation is the TPW), the angle between geography and the inertia principal axis, and θ the angle between rotation and main inertial axis. The authors are confusing these three quantities. I suggest to encourage a resubmission.

I refer to page and lines as x.y.

2.11 ineratia/inertia

3.40 This expression was in fact published first in Spada et al., Nature, 1992.

4.20 Maybe add that by definition for constant rotation, around z, (6) implies

$$J_{zz} = C = (2/3)(ka^5)/(3G)\omega^2$$

and

$$J_{xx} = J_{yy} = A = -(1/3)(ka^5)/(3G)\omega^2$$

hence (7)

5 Here I disagree for various aspects!

First I compute $E.\omega - (\omega.E.\omega)\omega$ in the inertia frame with vectors k_1, k_2, k_3 where $\omega = \sin(\theta)\cos(\phi)k_1 + \sin(\theta)\sin(\phi)k_2 + \cos(\theta)k_3$ and I get

$$\begin{bmatrix} \left(\lambda_1 - (\sin(\theta))^2 (\cos(\phi))^2 \lambda_1 - (\sin(\theta))^2 (\sin(\phi))^2 \lambda_2 - (\cos(\theta))^2 \lambda_3\right) \sin(\theta)\cos(\phi) \\ \left(\lambda_2 - (\sin(\theta))^2 (\cos(\phi))^2 \lambda_1 - (\sin(\theta))^2 (\sin(\phi))^2 \lambda_2 - (\cos(\theta))^2 \lambda_3\right) \sin(\theta)\sin(\phi) \\ \left(1/2\ (\lambda_3 - \lambda_1)(\cos(\phi))^2 + 1/2\ (\lambda_3 - \lambda_2)(\sin(\phi))^2\right) \sin(2\theta)\sin(\theta) \end{bmatrix}$$

1) This expression is in the inertia frame with vectors k_1, k_2, k_3. Then, if I try to understand what you did, you have probably written $\dot{\omega} = (\cos(\theta)\cos(\phi)\dot{\theta} - \sin(\theta)\sin(\phi)\dot{\phi})k_1 + (\cos(\theta)\sin(\phi)\dot{\theta} + \sin(\theta)\cos(\phi)\dot{\phi})k_2 - \sin(\theta)\dot{\theta}k_3$ *assuming that the k_i are not time dependent (which is obviously wrong!)*. So, identifying the k_3 components you got

$$\dot{\theta} = -\frac{1}{2}\frac{1}{(C-A)T_1}\left((\lambda_3 - \lambda_1)(\cos(\phi))^2 + (\lambda_3 - \lambda_2)(\sin(\phi))^2\right)\sin(2\theta)$$

which is your first equation (13) where you have missed a 1/2 (I can also get your second equation also missing a 1/2).

2) However, *you just cannot forget to derivate the* k_i. Assuming for now that everything occurs along $\phi = 0$ and that the position of the inertia principal axis moves with angular velocity $\dot{\gamma} k_2$, around the y axis then

$$dk_1/dt = \dot{\gamma} k_2 \times k_1 = -\dot{\gamma} k_3$$

So that the correct k_3 component is now

$$\dot{\theta} + \dot{\gamma} = -\frac{1}{2} \frac{1}{(C-A)T_1} (\lambda_3 - \lambda_1) \sin(2\,\theta)$$

Or working with the absolute coordinates $\theta' = \theta + \gamma$

$$\dot{\theta}' = -\frac{1}{2} \frac{1}{(C-A)T_1} (\lambda_3 - \lambda_1) \sin\left(2\,(\theta' - \gamma)\right)$$

So we get the usual "donkey after the carrot" behavior where θ' wants to coincide with γ but γ lives is own life...

3) What really TPW is, is an absolute motion of the geography with respect to ω (or symmetrically the absolute motion of ω), it is not the relative motion of ω with respect to γ (i.e., it is θ' not θ that matters). Paleomagneticiens cannot measure the angle with respect to the inertia frame.

4) If I try to do the math correctly (but so rapidly that you really have to check what I write although the general procedure should be fine), I start with an absolute frame (E_1, E_2, E_3) and I get the inertia frame (k_1, k_2, k_3) by the classical three Euler rotations: α around E_3 to obtain the frame (i_1, i_2, E_3), then β around i_1 to obtain the frame (i_1, j_2, j_3), then γ around j_3 to reach (k_1, k_2, k_3) (see e.g., wiki page on Euler angles). The rotation of the inertia frame is

$$O = \dot{\alpha} E_3 + \dot{\beta} i_1 + \dot{\gamma} k_3 = \dot{\alpha} E_3 + \dot{\beta}(\cos(\alpha) E_1 + \sin(\alpha) E_2) + \dot{\gamma} k_3$$

so that

$$dk_1/dt = O \times k_1 = \dot{\alpha} E_3 \times k_1 + \dot{\beta}(\cos(\alpha) E_1 \times k_1 + \sin(\alpha) E_2 \times k_1) + \dot{\gamma} k_3 \times k_1$$

and (assuming that I did not screw up in expressing the E_i as a function of the k_i)

$$dk_1/dt = (\dot{\gamma} + \dot{\alpha}\cos(\beta)) k_2 + (\dot{\beta}\sin(\gamma) - \dot{\alpha}\cos(\gamma)\sin(\beta)) k_3$$

(I guess these kind of expressions should be found in textbooks about solid rotation) ... The same thing should be done for dk_2/dt and dk_3/dt, then we can redo correctly your (13) and get two equations for $\dot{\theta}$ and $\dot{\phi}$ that should also include the three Euler angle time derivatives. The TPW can then be computed in the absolute frame (E_1, E_2, E_3).

6-35 You should add

$$g = -\nabla V$$

and

$$\nabla^2 V = 4\pi G \rho$$

or remove the centrifugal force from (16), and add

$$\nabla^2 V = 4\pi G \rho - 2\Omega^2$$

It is important that the reader realizes that g is not independent of the rotational dynamic and the internal masses. And as g "includes" classically rotation, my second suggestion might be better.

6-58 Although with my physical background I can accept the "Froude" number, it seems to me that since Clairaut (https://en.wikipedia.org/wiki/Clairaut_theorem), all the geodesists (see Lambeck, Nakiboglu 82, Chambat et al, 2010...) use $m = \omega^2 R^3/GM$. Later, you call Γ something usually named in the convection literature ϵ (Γ is often a Gruneisen parameter).

7-42 You should write or explain how the first terms can be transformed in surface integrals and cancel...

equation 24-25 are not really useful. They look nice but really they are problematic. First ΔV cannot be properly define: when the Earth is above its hydrostatic figure, the ΔV is a real volume, but what is it when the Earth is below its hydrostatic figure? ie, when ΔV is "negative"? Furthermore in (4) E includes the surface deflections due to the convection in a non rotating Earth, what the RHS of (25) does not include it. So the mathematical background of (25) and its connection with (4) are weak.

10-40 I do not want to be finicky but k_f^L is -1 near the surface and the CMB so that $1+k$ is rather 1/2 or below... (at least for a uniform viscosity, you can analytically compute $< 1 + k >$ average in depth, see Hager, Forte or myself, somewhere).

11 although your scaling looks good, the reasoning is strange. d is the lithosphere thickness, so heterogeneities are injected at d and diffusion and stirring should populate the nodes with wavelengths smaller than d. But in fact you consider that only wavelengths larger than d exist.

13 As I disagree with your basic equation I cannot follow. The TPW is not $\dot{\theta}$ (an angle relative to main inertia) but the absolute velocity of ω. The rate $\dot{\theta}$ and rate at which the inertia main axis position moves (what I named O above, or the Euler rate $\dot{\alpha}$, $\dot{\beta}$, $\dot{\gamma}$) are not the same thing.

I stop my reading around p13. I am sorry to be negative but I think the authors have a nice problem in hand to solve. But just now, I cannot agree with their physics.