Bernhard Steinberger

One addition is the model MF*. From what I understand, in this model the positions of plates and boundaries are rotated into the paleomagnetic reference frame, but rotation rates are additionally modified to change them into a no-net-rotation reference frame. But these additonal rotation rates are not integrated to change the positions of the plate. At least, I think this would be the most sensible way of doing things, because it would maintain the paleomagnetic reference frame, yet lithospheric net rotations would not cause a net rotation of the entire mantle. Yet it is not entirely clear that this is your procedure, so I think you should make it explicit. A few more points in this context:

The MF* alternative case is not a new convection model, it is simply the MF model rotated in the paleomagnetic reference frame. In this reference frame, the lithosphere undergoes net rotations. Because we simply rotate the outputs in the paleomagnetic reference frame, the net rotations of the lithosphere also apply to the underlying mantle. We explained our procedure more clearly in section 2.3 lines 242-266 (in the trackchanges version).

The net rotations of the deep mantle in the MF* case can be seen in the supplementary video *qcmb_MF.mp4*. A significant eastward rotation notably occurs between -700 Myr and -580 Myr. This net rotation is due to the coupling with the surface net rotation. Similar rotations of the deep mantle are visible in the supplementary videos to Dannberg et al. (2024) (such as the eastward drift between -700 Myr and -580 Myr). Though our approach is different from the one of Dannberg et al. (2024), the strong coupling between the lithosphere and the deep mantle in their models seems to produce very similar net rotations of the deep mantle.

Around line 92: One should mention that net rotation can also be a problem in mantle reference frames: Plate reconstructions in a (e.g. hotspot-based) mantle reference frame do contain a net rotation, but when these plate reconstructions are imposed as boundary conditions, they will usually not yield the same net rotation relative to the deep mantle - e.g. in the case without lateral viscosity variations, the deep mantle will show the same net rotation, and there is no relative net rotation. So even in the case of the mantle reference frame, it would probably be better to remove net rotation from the rotation rates (but without modifying the positions of plates and boundaries by integrating rotation rates). As an alternative procedure, I change from free-slip to fixed CMB at degree-1 toroidal (the net rotation component) flow, in my computations, in this way substantially reducing the net rotation of the deep mantle and introducing a net rotation of lithosphere vs deep mantle even without lateral viscosity variations.

We are aware of this potential net rotation of the deep mantle when the plate reconstruction model itself contains net rotations. We agree that it would be interesting to run an end-member MF model in which net rotation would be removed from the plate reconstruction and at depth, because net rotation depends on lateral viscosity variations that are limited in model MF. In our MF* case, all the surface net rotations are instead applied to the deep mantle. Some of these net rotations are expected, while some shouldn't exist (if they are differential rotation of the lithosphere relative to the deep mantle). We have stated in the text that the rigid rotations that arise in model MF are a combination of the imposed boundary conditions and considered lateral viscosity variations (lines 249-259).

Line 104/105: "This alternative correction is equivalent to what seems to be done": I think it would be beneficial to figure out how Dannberg et al. (2023) handle this net rotation issue, i.e. if they remove net rotation the same way you do it. If they keep the large amount of net rotation, which is then transferred to the mantle, then this would indeed be somewhat problematic in their computation.

Following the comment by Juliane Dannberg, we explained more clearly the difference between their approach and ours in the manuscript (lines 242-266).

Also, I find lines 124-126 still a bit confusing. From what I understand, they start with a paleomagnetic reference frame, which they convert, at least since 500 Ma, into a "TPW corrected mantle reference frame" (see their Table 1), yet this is not (at least not necessarily) a no-net rotation reference frame. So, conversion into a no-net rotation reference frame is something you would still need to do (not sure whether you do this). What is further confusing is that on line 322 you write that your reconstruction is identical to the "no net rotation" plate reconstruction of Müller et al. (2022). As said, I think it would be most suitable to change rotation rates to a no-net rotation frame, but not to rotate plate positions into it, i.e. keep them in a mantle frame.

The net rotations are indeed removed from the plate reconstruction model, which is identical to the NNR case of Müller et al. (2022). This plate reconstruction corresponds to the one of Merdith et al. (2021) from which all the net rotation has been removed. The plate reconstruction is thus given in a no-net-rotation reference frame which assumes no net rotations of the lithosphere relative to the underlying mantle. We changed the lines you referred to (lines 113-116) by making it clearer that we use the plate reconstruction of Merdith et al. (2021) rotated in the no-net-rotation mantle reference frame as in Müller et al. (2022).

Lines 261 ff: Again (see above comment on lines 124-126) I am not sure whether you are doing (or describing) this right. Merdith et al. use at least since 500 Ma a "TPW corrected mantle reference frame", so in order to get the reconstruction into a paleomagnetic reference frame, one would presumably just have to undo their TPW correction, but that presumably does not remove all net rotation. So, after doing the TPW correction, do you additionally remove any remaining net rotation? But if you do that also for the cumulative rotations, then you would no longer be in the paleomagnetic reference frame. Sorry, I am still confused.

See lines 242-266 for the description on how the MF* case is obtained.

Lines 13/14: "The average TPW rates ..." - I think it should be clarified that this sentence refers to the alternative TPW correction according to Fig. 7, not to your model.

The average TPW rates we mention here are the ones obtained for cases MF1, MF2 and MC1. We mentioned average TPW rates ranging from 0.4°/Myr to 1.8°/Myr, with MF2 being the lower value and MF1 being the higher value.

Line 206: "densities and flow velocities are first set equal to zero" - are you sure you are setting flow velocities equal to zero? Because density variations below 300 km will also drive flow above 300 km, and setting flow above 300 km to zero would presumably introduce a flow discontinuity which I don't think you want.

This sentence was indeed misleading. As we use a Stokes flow solver to compute the geoid, the velocities in the mantle only depend on the density. There is thus no need to set the flow velocities equal to zero above 350 km. We removed this sentence from the manuscript.

Table 1: I don't understand the sentence "The value of delta rho_c ... is an average over all continents". Do you mean, you use different densities for different continents? Or you use a constant value which is the value obtained by averaging observationally-derived densities for all continents?

Different types of continents are used, with different densities (see Flament et al., 2014). We added a reference to Flament et al. (2014) in the caption of the figure (the reference was already given in the text line 120).

Your new Fig. 1: I don't really understand why the temperature profiles and CMB temperatures are so different (differing by ~1000 K in the lowermost mantle) in the two models. Is that because your model MC does not include adiabatic compression and heating, whereas model MF does? Although the temperature drop in the the bottom thermal boundary layer looks more or less realistic in both cases, I think absolute temperatures are way too low in the model MC. I think this should be explained to avoid confusion.

MC uses the Boussinesq formulation, while MF uses the extended Boussinesq formulation, in which the adiabatic compression effects are accounted for. Those precisions have been added to the description of the models lines 111-112 and line 140.

Line 387: Perhaps "diverge back in time"?

This has been modified

Line 432: Why limited to 9° to 18°? In figure 4 right, it looks like the curve exceeds 20°/Myr.

The TPW velocities are limited between 9°/Myr and 18°/ Myr in the MC1 case because of the time step between two successive geoid snapshots. Maybe you refer to the figure 4 of the initial submission version, in which the geoid was incorrect and the TPW velocity indeed reached 20° /Myr.

Minor comments:
line 82: typo, should be "inertia"
line 271: typo, should be "displacements"
line 410: "as it implies"
A number of references have https://doi.org/ twice

This has been modified

Your response #3:
I did not find the sentence “The density and viscosity distribution in the mantle below 350 km is not modified.” in your text.

This was an oversight, it has been added lines 195-196.

Shijie Zhong

I still have some concern on MF model’s geoid, i.e., the difference between “No LVVs geoid” and “Total geoid”, as I pointed out in my first review. The authors explain the difference as a result of lateral variation in viscosity due to slabs. While this may indeed be what is going on, my experience with this sort of model calculations still makes me concerned. Presumably, “No LLVs geoid” calculation should have converged easier because of the removal of lateral variation in viscosity. The effect of lateral variations in viscosity on the geoid is an old topic, and our recent paper [Mao and Zhong, 2021], using the plate motion history for the last 130 Ma and similar temperature- and depth-dependent viscosity to MF models, reproduced the observed geoid from degrees 4 to 12 reasonably well. Admittedly, MF models include much longer plate motion history, and the results could be quite different. I do not want to further delay the publication of this paper, knowing that their calculations prove their main idea.

Viscosity variations are only removed in the top 350 km for the ‘No LVVs geoid’, which generally converge faster, and occasionally do not converge. We note that the viscosity of the asthenosphere in model MF is not as low as that preferred by Mao and Zhong (2021), which could partly explain the difference in geoid calculation.

line 190, “Viscosity lateral variations are also removed above 350 km …”. Can the authors clarify how they did this? What is the viscosity used for the top 350 km then? Is it the horizontally averaged viscosity (like in Fig. 1) for the top 350 km used here?

The ‘No LVVs’ geoids were obtained by solving the instantaneous Stokes flow in a CitcomS restart from ‘velo’ files in which the temperature was set equal to ambient mantle temperature above 350 km depth.

Equation 1 for the geoid anomalies. First, l=1 should not be included in this equation, because by definition, there can not be degree 1 gravity or geoid anomalies. Geoid and gravity anomalies start with degree 2. Second, the geoid spherical harmonic expansion coefficients c and s in this equation are dimensionless, and they are scaled by radius of the Earth R to get the geoid anomalies. However, this equation is a bit strange to me (not necessarily incorrect, because one can scale the geoid in anyway). Perhaps, the authors should double check on it.

Equation 1 has been corrected to start the sum at l=2. The same equation with the same scaling was used in Phillips et al. (2009), Greff-Lefftz and Besse (2014) or Rouby et al. (2010).

MF* case with a paleomagnetic reference frame is a good addition. However, given that degree-1 toroidal plate motion (or net rotation of lithospheric shell) is present in present-day plate motion in hotspot reference frame, and that it can be dynamically generated using proper plate boundary viscosity [see Mao and Zhong, 2021], the reference frame seems always an open question. Perhaps the so-called geologically inferred TPW is actually not TPW. I am not sure what I expect the authors to revise on this point, rather than to remind them the complexity of this sort of issues.

We are aware of the complexity of this subject. For our MF* case, we chose to simply rotate the outputs of the MF model in the paleomagnetic reference frame. By doing so, we imposed that the net rotations of the deep mantle were a combination of the net rotation of the deep mantle in the MF model and the net rotation of the surface in the paleomagnetic reference frame. Though this is clearly not an ideal solution, we chose this approach as it only required rotations of the MF model outputs. As you mention, it is difficult to decipher the amount of the surface net rotations due to a solid-body rotation of the mantle. In our approach, we consider that all the surface net-rotations are due to a solid-body rotation of the mantle. A discussion of this problem has been added (section 4.4.2).

Reply to Juliane Dannberg’s public justification

Dear Juliane,

Thank you for the explanations you provide in your comment, which clarify the differences between your approach and ours. In addition to the differences between the mantle convection models used in our study and in Dannberg et al. (2024), our MF* case differs by the coupling between the net rotations of the surface and the deep mantle. In our model, all the net rotations of the surface are solid rotation of the whole mantle-lithosphere system, while the coupling with the deep mantle is part of the model in Dannberg et al. (2024).

You mention that the differential rotation between the lithosphere and the mantle is probably underestimated in the models you use. The net rotations of the deep mantle relative to the surface can thus be expected to be relatively similar between the two approaches. Our MF* case assumes that all the net rotations of the lithosphere in the plate reconstruction of Merdith et al. (2021) are due to a solid-body rotation of the mantle. Though this is indeed what is expected in the case of TPW, it is less clear whether rotations in longitude should be treated this way. Though the approach in Dannberg et al. (2024) has the advantage of being consistent with the tectonic reconstruction in the paleomagnetic frame of reference, it requires running a specific model, and we instead chose to use our approach which only requires rotations of the outputs of model MF.

We agree with your statement that the net rotation of the mantle does not affect the power of different spherical harmonic degrees of CMB heat flux, nor your q* parameter. It does however play a role in the latitudinal heat flux distribution (your Figure 9b), which is found to strongly affect the geodynamo behaviour (Olson et al, 2010; Zhang and Zhong, 2011).

Best regards,

Thomas Frasson, Stéphane Labrosse, Henri-Claude Nataf, Nicolas Coltice and Nicolas Flament

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