Referee #1

Page 2: lines 35-45: perhaps discuss a short part about how we think that the chemical (and therefore negatively buoyant) heterogeneity may be confined to a small region at the base of the LLVPs. See Richards et al (2023; EPSL: "Geodynamic, geodetic, and seismic constraints favour deflated and dense-cored LLVPs").

Thank you for pointing out the recent study of Richards et al (2023). We refer to it in section 1:

“Recent works suggest a chemically distinct composition at the base of these structures, stabilizing them by imposing a negative buoyancy (Richards et al., 2023).”

Page 6: lines 160-165: It would be good to elaborate on why there are two distinct ways to compute the geoid. This study will likely attract a varying audience (e.g., core dynamicists), so some background on this -- even just 2-3 sentences -- would be helpful. What does zeroing out the upper 350 km achieve?

We made the purpose of the No LVVs geoid clearer in the manuscript. We notably included these lines in section 2.2:

“The computation of the geoid is very sensitive to large lateral viscosity variations in the mantle (Čadek and Fleitout, 2003; Flament, 2019). The MF model is driven by a plate reconstruction model, updated every 1 Myr, which notably imposes the positions of viscous slabs. The update of the slab positions strongly affects this “Total geoid”, creating discontinuities in the time evolution of the geoid. To tackle this issue, we compute a second geoid, called “No LVVs geoid”, for which we cancel out density and viscosity lateral variations in the upper 350 km.”

“Radial viscosity profiles are classically used to compute the geoid using geoid kernels (Richards and Hager, 1984; Rouby et al., 2010; Steinberger et al., 2019). In our plate-like models, the largest lateral variations of viscosity happen in the upper mantle. Removing the effects of these lateral variations in the upper mantle thus enables us to compute a geoid that is closer to the one computed from radial geoid kernels.”

Why is the "No LVV" method not applied to the MC model? If this is because it was not calculated in the original study, is there a way to use the output you have access to to apply the same "No LVV" method. It would be better for comparison. Or maybe this is not applicable? If so, please explain why.

An explanation for the reason why a No LVVs geoid was not used in model MC is given in section 2.2:

“The MC model is fully self-consistent without any forcing at the surface by imposing a free-slip boundary condition. The Total geoid computed in MC evolves smoothly, preventing the scattered TPW observed in MF1. It is thus not necessary to remove the effect of lateral variations in the upper mantle to obtain a smoother geoid as it was the case in the MF model, and only the “Total geoid” is computed for this model“

Page 9: lines 250-260. If the variations in the geoid are so large compared to today's actual geoid, what does this mean in terms of how "earth like" the CMB predictions would be? I realize now that you explain this later in the Discussion, so perhaps point the reader to it.

A reference to the discussion has been added in section 3.2:

“The impacts on the CMB heat flux of the different geoid behaviors are discussed in Sect. 4.3”

Page 19: Section 4.2. Is it possible to give some idea of the timescales of the PCs? Perhaps even estimate the frequency content of the time series. Since these undulations time (figs 10-11) reflect the mobility of the piles, can these be related to subducting slabs from above? Can you potentially derive some timescale for surface events to be translated to CMB events? I think this would be very interesting.

We agree that it would be interesting to link the components with specific events in the mantle convection models. This exercise is however challenging and would have significantly complexified the manuscript. We thus decided not to include discussions about how the components relate to the timescales of mantle convection processes in the revised version.

Bernhard Steinberger

My main problem is that the geoid results shown are very different from the real Earth, and also very different from each other. It is not clear to me at all where these differences come from. It is all said in the end of the conclusion: "it would be of great interest to understand where these discrepancies come from". I agree, and I think it should be done in this paper and not in some future work -- among other things, in order to reduce the chance that these discrepancies come from actual errors in the computations.

We indeed discovered an error in the computation of the geoid in model MC. The results and discussion have been updated accordingly in the manuscript. We thank you again for insisting that we clarify this issue.

More specifically, the methods would also have to be better described. It may be possible to extract these from the literature given, but at least some essentials need to be discussed: Particularly, what rheological model is used? Since the geoid strongly depends upon it, in particular on (average) radial viscosity structure. Is it the same in the MF and MC models, or different? If it is the same, why the geoids are so different? Also, the CMB heat flux is different for MF and MC models (line 245); which CMB temperature do they use? Is it the same?

We provide in the revised version the time-averaged profiles of viscosity and temperature in the mantle for both models. A figure showing these profiles has been added to the manuscript in section 3.1.

The two cases MF1 and MF2 start more similar, but then evolve increasingly different. Does the density structure (below 350 km depth) also evolve differently in the two cases, or is it at each time the same density (below 350 km), only the geoid is computed differently? I think this would make more sense, i.e. you always insert slabs at each time step, but why would differences increase with time then?

We clarified the way the “No LVVs geoid” is computed by adding this precision in section 2.2:

“The density and viscosity distribution in the mantle below 350 km is not modified.”

We also mention that the similarity between the two geoids in model MF at the beginning of the simulation might be due to an effect of initial conditions in section 3.2:

“At the beginning of model MF, the piles are associated with positive anomalies in the “Total geoid'”, as it can be seen in the snapshot at -900 Myr. This positive signal above the piles only lasts for the first 150 Myr of the simulation. This change of sign in the geoid anomalies above the piles could thus be an effect of the initial conditions.”

Also, what boundary condition is used for geoid computation in MF? What I usually do is I use prescribed plate motions only for the flow and advection calculation, but free-slip for the subsequent geoid computation at each time step. Because prescribed surface motions are appropriate for flow computations, but may not give realistic surface radial stresses and topography, hence not realistic geoid. This would be important to know in order to understand the geoid in this case.

We mentioned the boundary condition used in model MF in section 2.2:
“Model MF is forced at the surface by the plate model. To compute the geoid at a given time step, we re-start the model at this time-step with a free-slip condition at the surface as usually done in plate-driven models (Steinberger, 2016; Flament, 2019; Mao and Zhong, 2021).”

Regarding results, why the geoid in case MC has such high amplitude and is such strongly correlated with continents? In reality, continents are mostly isostatically compensated at shallow levels and are associated with a very weak signature, i.e. there is hardly any correlation between geoid and the continent-ocean distribution. I think something is wrong here. On lines 286/287 you write that piles are mostly associated with geoid lows, but I don't see this; I see just the correlation with continents.
The correlation between the geoid and continents no longer exists in the corrected geoid. We modified the results and discussion accordingly.

And why there is no such strong continent signal in MF? The difference in results between the three cases MC, MF1 and MF2 is really puzzling and some analysis should be given to understand the differences, e.g. by separating different contributions (topography, Moho, mantle density down to 350 km, mantle density below 350 km, CMB topography.
The difference between the geoid produced by model MC and MF is much smaller after correction of the geoid in model MC. The differences between the Total geoid in models MF and MC are discussed in section 4.3.2.

Shijie Zhong

One class of convection models (MF) presented in this study used CitcomS. Given that CitcomS has been extensively benchmarked for the geoid problems, I would think that the geoid results from this class of models should be okay. However, the geoid results from this class of calculations also raise some concerns to me. For example, I do not quite understand why the geoid would be so different after the removal of shallow thermal structure (i.e., the top 350 km), because the long-wavelength geoid (e.g., at degree-2) often is insensitive to buoyancy at shallow depths where the geoid kernel goes to zero (the geoid kernel concept remains largely relevant even for models with 3D mantle viscosity).

The No LVVs geoid shown in the manuscript is computed by cancelling both the density heterogeneities and the viscosity heterogeneities above 350 km depth. The large differences between the No LVVs geoid and the Total geoid arise from the removal of the lateral viscosity variations rather than from the removal of the lateral density variations. We tried computing a geoid suppressing only the density variations above 350 km depth, showing very few differences with the Total geoid. We added a mention of this third geoid output in the manuscript in section 2.2:
“A third geoid has been computed by cancelling only the lateral variations of density above 350 km depth. The geoid produced in this case is very close to the “Total geoid’and the TPW path does not significantly differ. We thus discarded this case for this study.”

Anyway, it is unclear from the manuscript how the geoid and dynamic topography were computed (e.g., were free-slip boundary conditions used together with some appropriate lithospheric viscosity?). The authors need to describe these issues in the revision. Prescribed surface velocity boundary conditions tend to produce spurious pressure field and hence dynamic topography, with high viscosity lithosphere.

We clarified this point in section 2.2:
“Model MF is forced at the surface by the plate model. To compute the geoid at a given time step, we re-start the model at this time-step with a free-slip condition at the surface as usually done in plate-driven models (Steinberger, 2016; Flament, 2019; Mao and Zhong, 2021).”

The other class of convection models (MC) produced geoid anomalies of several kilometers that are only slightly smaller than that of surface dynamic topography (Fig. 2). These calculations are presumably for Rayleigh numbers that are comparable with that for the Earth's mantle and with that in the first class of convection models using CitcomS. In this type of situation, kilometers of geoid anomalies seem too large to me. Additionally, the geoid to topography ratio in most models in general should be around 0.1-0.2 (like admittance which is the ratio of gravity anomalies to topography, the geoid to topography ratio is only sensitive to viscosity structure, but significantly less sensitive to distribution of buoyancy).

We thank you for pointing out this problem. We indeed discovered an error in the computation of the geoid in model MC. After correction, geoid anomalies are much smaller (while still larger than present-day anomalies), and their pattern is much closer to expectations. We modified the manuscript to discuss the corrected geoid.

Additional modifications

Additional modifications not mentioned above have been made to this revised version. These changes are mostly the results of the addition of a new case, called MF* in the new version, as an alternative to the TPW correction performed in the other cases. In this case, the outputs of the MF mantle convection model were rotated in the paleomagnetic reference frame of Merdith et al. (2021). The addition of this alternative case was motivated by the recently submitted work by Dannberg et al. (2023), in which a mantle convection model is driven by the plate reconstruction of Merdith et al. (2021) kept in the original paleomagnetic reference frame. Our MF model is driven by the same plate reconstruction but in a no-net-rotation reference frame in order to avoid net rotations of the lithosphere. We simply rotated the outputs of model MF in the original paleomagnetic reference frame by considering that the net rotations of the surface in the plate reconstruction are rotations of the whole mantle. This is overall similar to running the mantle convection model in the paleomagnetic reference frame and applying the net rotations of the surface to the whole mantle, as done in Dannberg et al. (2023). We used this case to compare the evolution of the CMB heat flux in the paleomagnetic reference frame with the TPW corrected cases, and we also added a comparison between the positions of the maximum inertia axis and the magnetic dipole axis in the paleomagnetic reference frame of Merdith et al. (2021).

Other minor corrections have also been done in the text and the figures, including:

- Change of colormap for the CMB heat flux and geoid maps (Fig. 2, Fig. 3, Fig. 11 and Fig. 12).
- Change of the color scale of the geoid in Fig. 2 and Fig. 3 to avoid the saturation.
- Addition of the time-averaged spectra of the CMB heat flux in models MF and MC (FIg. 4).
- Correction of the latitudes in Fig. 11: the heat flux patterns were upside down due to a wrong sign in the definition of the latitude for this figure.

References

Dannberg, J., Gassmoeller, R., Thallner, D., LaCombe, F., and Sprain, C.: Changes in core-mantle boundary heat flux patterns throughout the supercontinent cycle, arXiv preprint arXiv:2310.03229, 2023.

Merdith, A. S., Williams, S. E., Collins, A. S., Tetley, M. G., Mulder, J. A., Blades, M. L., Young, A., Armistead, S. E., Cannon, J., Zahirovic, S., et al.: Extending full-plate tectonic models into deep time: Linking the Neoproterozoic and the Phanerozoic, Earth-Science Reviews, 214, 103 477, 2021.