# Review Report

**Steinberger et al., Exploring the Origin of Geoid Low and Topography High in West Antarctica: Insights from Density Anomalies and Mantle Convection Models, TEKTONIKA, 2023.**

## Table of Contents

# 1st Round of Revisions

## Comments by Reviewer 1 & Authors' reply

## Section A: Overview of manuscript

### A1) Overall evaluation, general comments & summary

*A1.1) Reviewer's comments*

#### A1.1.1 ) General evaluation and publication suggestion – Required:

***Please use this space to describe, in your own words, the core subject of the submission and your overall assessment of its suitability for publication.***

The paper by Steinberger et al. tries to explain the geoid and dynamic topography signature in Western Antarctica using analytical and numerical convection models. They find a geoid low and topography high that are slightly offset from one another. They place disks of density anomalies in the mantle and explore what size and degree of anomaly is required to match the observation. They next use convection models to explore whether they can produce the observed signals. They find that disks can be used to explain the signal, however it is not clear what dynamics causes them since they are too large to be caused by a mantle plume. Plume simulations produce narrow conduits that don't provide good fits to the observation. Overall this work does not answer the question of what causes the dynamic topography and geoid anomaly, however, it provides some useful modelling that gives further insight into the problem.

I have noticed that this work includes two co-authors who are highschool students. This is very impressive work for a highschool student and I want to applaud them for the research they have performed!

#### A1.1.2 ) What does the submission need to be publishable? (select as needed; comment for all cases)

- ☐ No changes required
- ☒ Rewriting
- ☒ Reorganising
- ☐ More data/figures
- ☐ Condensing
- ☒ Reinterpretation
- ☐ Other

*Comments:*

The content of the paper is useful and interesting. However, I found the presentation lacking. The writing is not very concise, sometimes quite colloquial, and it doesn't give a clear overview of past work in this region. It also brushes over uncertainties yet interprets the final temperature in terms of viscosity (even though the dynamic models do not match the observations). The structure also needs some re-organizing, for example, there are some methods described in the results sections and some results in the discussion. I don't see a fundamental flaw with this work and do believe it's a useful contribution that should be published but encourage the authors to revise the text for clarity and readability.

A1.1.3) Can the submission be improved by reducing/adding any of the following? (select as needed; comment for all cases)

- ☐ Text
- ☐ Table
- ☐ Figures
- ☐ Supplementary material

*Comments:*

I don't think any of the items need to be added or reduced.

A1.1.4) Please complete the following section if you recommend that the submission is NOT appropriate for publication (select as needed; comment if a box is selected)

- ☐ Quality is poor
- ☐ Research is not reproducible
- ☐ Other

*Comments:*

[Free form box]

*A1.2) Author(s) Responses:*

We thank the reviewer for the overall positive evaluation. All specific points of criticism are mentioned again further down, and will be addressed there.

## A2) Summary of main merits and main points of improvement

### *A2.1) Reviewer's comments*

***Please describe below in a few sentences (100 to 300 words) the main merits of the submission and suggestions for improvements.***

#### The main merits I have found are...

This work provides a re-analysis of the geoid signal that is driven by the upper mantle. The analytical and numerical modelling provides insights into the dynamics of the upper mantle and its surface signature. Comparisons are quantitative but first order.

#### The main points of improvement I have found are...

The main component that needs improvement is the writing and structure overall.
I also have some questions on the methods and interpretation of data and results including estimates of uncertainties that I will detail in the later sections.
The title and overall forced connection to Antarctic ice sheet stability should be reduced (more comments on this below).

### *A2.2) Author's responses:*

This will be answered in detail in the later sections.

## Section B: Detailed evaluation of manuscript B1) Title and abstract

### *B1.1) Reviewer's comments*

*These statements are a **guide** to what good* Titles *and* Abstracts include*. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Title* describes the main topic of the manuscript **accurately** — [NO] The

*Title* describes the main topic of the manuscript **succinctly** — [NO] The *Title*

includes **appropriate key terms** — [NO]

The *Abstract* includes a **clear aim and rationale** — [YES]

The *Abstract* supports the rationale with **sufficient background information** — [YES]

The *Abstract* includes a **well-balanced description of the methods** — [NO]

The *Abstract* describes the **main results sufficiently and adequately** — [YES]

The *Abstract* clearly describes the **importance/impact of the study** — [NO] The

*Abstract* clearly states the **conclusions of the study** — [NO]

The *Abstract* is **clear** and **well structured** — [NO]

*Comments:*

I'm sorry but the title reads like clickbait. It currently appeals to ice sheet instabilities of the WAIS, which is really not explored at all in this paper. Viscosities are inferred to very first order and those also only for models that are not dynamically consistent. The work has merit in itself so I suggest revising the title to something that actually represents the content of the paper.

The abstract conveys the main work presented in the paper. However it is quite wordy and long. I suggest revising this, making the language more concise and focus on the main work and outcome. The abstract presents multiple results but it lacks a clear synthesis of the main finding and implication.

### *B1.2) Author's responses*

I'm sorry, but the title represented what got me initally excited in this topic, but in the end I wasn't able to contribute anything really to understand the ice sheet instability. Accordingly, we changed it now to "Exploring the Origin of Geoid Low and Topography High in West Antarctica: Insights from Density

Anomalies and Mantle Convection Models" which we think better represents the content of the paper. We have also made the abstract more concise.

## B2) Introduction

### *B2.1) Reviewer's comments*

*These statements are a **guide** to what good* Introductions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Introduction* provides **sufficient background and context** for the study — [NO]

The *Introduction* describes the **aim/hypothesis/rationale** clearly, providing **sufficient context** — [NO]

The *objective/hypothesis/rationale* **flows logically from the background** information — [YES] & [NO]

The *Introduction* describes the study's **objective and approach** (last paragraph) — [YES]

The *Introduction* contains **relevant, suitable citations** — [NO]

The *Introduction* is **organized effectively** — [NO]

*Comments:*

The introduction does not actually give an overview of geodynamic modelling that has been done in Antarctica. The objective of the work is clear though.
One major issue I have with the introduction is that it already includes a good amount of analysis. The correction of the gravity field for lower mantle convection should in my opinion be placed in the methods and results rather than the introduction. There are also a lot of choices that are being made (e.g. neglect lower mantle flow on DT, approximate slabs and LLSVPs in the lower mantle, etc). that seem a bit ad-hoc and not super well justified. I can't really follow the details of the reference frame rotation for the LLSVP correction.

### *B2.2) Author's responses*

We have now included more information about geodynamic modelling that has been done in Antarctica.

We have moved the correction of the gravity field for lower mantle convection to new first subsections of the methods and results section, and the first paragraph of the discussion. Note that this wholesale restructuring is NOT marked in the "track changes" version, for better visibility of remaining changes, but can be recognized by the new subsection headings 2.1 and 3.1.

Why we do not correct residual topography for the lower mantle (I assume that is what you mean) was explained in the paragraph in lines 112-117 of the previous version, and that paragraph is still there.

An approximate treatment of slabs and LLSVPs in the lower mantle, is adopted from Steinberger and Torsvik (2010), hence it is not discussed in much detail here. However, we have added a bit more discussion: "They use plate reconstructions and geodynamic forward modelling to infer slab distribution

through time, and assume that the LLSVP contribution has remained constant. The geoid due to due to LLSVPs is adjusted such that the fit of the total geoid to the observed geoid is optimized."

We now write that "we first rotate the reference frame by an angle $\varphi_0$ such that the zero-degree longitude line approximately coincides with the LLSVPs centers, instead of Greenwich." and we have somewhat expanded the description of our procedure. The rest is just rather simple math, which one probably has to write down step by step in order to verify it, but this is what is sometimes referred to as an "easy exercise for the reader" and we don't think there is the need to write down every single step in the paper. Moreover, we think the second approach (adopted from Steinberger and Torsvik, 2010) is more adequate, hence we think enough space has been given to the first approach.

## B3) Data and methods

### *B3.1) Reviewer's comments*

*These statements are a **guide** to what good* Method *sections include and good practices for* Dataset *accessibility. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Methods* are described **concisely and with enough detail** for reproducibility — [YES] Necessary

information about **data sources/acquisition/processing** is included — [YES]

**Data used are accessible** via either supplementary files or links in the data availability statement — [YES]

The *Dataset and/or Methods* are **organized effectively** — [YES]

*Comments:*

The methods are described in sufficient detail, as far as I can tell. One component that seems at odds with the study goal is that the time-dependent convection simulations don't seem to reliably calculate the geoid field. They approximate the geoid field, but this is only described in the discussion – since explaining the observed gravity field is a main goal of the paper this seems like the incorrect ordering.

### *B3.2) Author's responses*

Unfortunately we could not compute the geoid for the time-dependent convection simulations. Accordingly, we have added in the methods section
"One drawback of using a box model is that the geoid cannot directly be computed in Aspect, as the geoid postprocessor can only be used if the geometry is a sphere, spherical shell or spherical chunk."
However, we also think that our approach is sufficient, and it would not warrant the major effort of redoing the computations in a spherical shell or spherical chunk geometry, as we don't expect we would gain any major new insights. To explain our approach, we have added in the discussion: "The shallow disk predicted in our dynamic forward models can approximately explain dynamic topography, but with our dynamic forward modelling approach we cannot directly compute the geoid. Instead, we approximate the plume spreading at shallow levels by a cylindrical disk, for which we compute geoid and dynamic topography analytically with the spherical harmonics approach. With a viscosity structure that corresponds to global average (approximately $10^{20}$ Pa s viscosity minimum below the lithosphere) the cylindrical disk required to match geoid and dynamic topography is deeper than in the dynamic forward models. However, with viscosity reduced in parts of the upper mantle, for example 10 times between 350 and 50 km depth or 100 times between 220 and 100 km depth, we can get a better fit for a shallower disk, e.g. 8 degrees radius between 50 and 350 km depth."

## B4) Results

### *B4.1) Reviewer's comments*

*These statements are a **guide** to what good* Result sections *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Results* findings are **supported by data** — [YES]

The *Results* findings are presented **clearly and succinctly** — [YES] & [NO] The

text in the *Result* section **cites tables and figures appropriately** — [YES] The

*Results* directly **relate to the study objectives** — [YES]

The *Results* present **data for all the approaches** described in the *Methods* section — [YES]

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion.* — [YES]

The *Results* section is **organised effectively** — [YES]

*Comments:*

The results section described what is found appropriately.

### B4.2) Author's responses

## B5) Discussion and conclusions

### *B5.1) Reviewer's comments*

*These statements are a **guide** to what good* Discussions *and* Conclusions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Discussion* is **focused on the objectives** of the study — [YES]

The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES]

The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [NO]

The *Discussion* section properly identifies all **speculative statements** — [NO]

The *Discussion* section presents the **implications of the study** persuasively — [YES] & [NO]

The *Discussion* section **highlights novel contributions** appropriately — [NO]

The *Discussion* section **addresses the limitations** of the study appropriately — [NO] The

*Discussion* section is **organised effectively** — [NO]

The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — [YES]

The *Conclusions* are **supported by the data** in *Results* and **follow logically** from the *Discussion* — [YES]

The *Conclusions* are **clear and concise** — [YES]

*Comments:*

There are a lot of estimates that are approximate in this study. For example, the inferred dynamic topography – what is the uncertainty on this? How does this affect the result? How is the best data-model fit found for the disk model? How is it justifiable that the upper 200km of the mantle don't add to dynamic topography contributions? I found a lot of these were not sufficiently addressed in the discussion.

The viscosity estimate that is inferred at the end has so many assumptions in it that I'm not sure it's actually a useful inference.

The discussion also includes some methods (e.g. line 384 onwards) and new model setups (a run with two separate disks). I think all methods should be set up together.

### *B5.2) Author's responses*

We presume that you are talking about uncertainties of observation-based inferred dynamic topography here. Then uncertainties mainly depend on crustal thickness uncertainties, and we have added one sentence accordingly. We do not think we can discuss these in further detail. Moreover, we have already compared to the results of Paxman et al., which should give a further indication of uncertainties.

The assessment of “best-fit” model was merely based on visual inspection. In order to avoid the impression that there is something more behind, we no longer call it that way. See also our response below regarding that issue.

We don’t think we have excluded the upper 200 km anywhere in the dynamic topography computations. I think what you might be referring to is that, in the time-dependent global flow models, we have excluded the upper 200 km of the tomography model when converting seismic velocities to densities that drive flow. The rationale for this model assumption is now explained as follows: “There thermal diffusion plays an important role and hence backward advection, which we use to compute past density and flow, is less appropriate. Also, such a simple thermal conversion is less appropriate in the lithosphere, where compositional variations may affect both density and seismic velocity.”

Our viscosity estimate at the end is merely to show that our results are not at odds with previous estimates. Accordingly we have re-formulated the last two sentences of the discussion, to tone down the relevance of our viscosity estimate. We do want to end with a few thoughts on viscosity, though, since mantle viscosity and its possible relevance for the stability of the West Antarctic Ice Sheet was the initial motivation for this work.

We have moved parts of the discussion to the methods and results sections. Actually we shifted line 373 onwards to the results section (now at the end of the part with Aspect results). The mere shift is not indicated by the “track changes” such as to make it clearer what has actually changed: There are some modifications, as this part is split up between methods and results. Also, we found a mistake in these Aspect disk computations, so all the numbers have changed. Also, we have added results also for the shallower disk and stronger viscosity variations with temperature. More details are given below.

## B6) Figures, tables and citations

### *B6.1) Reviewer's comments*

*These statements are a **guide** to what good* Figures *and* Tables *include and how they are presented. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

*Tables* and *Figures* are **ordered logically** and **numbered sequentially** — [YES]

*Tables* and *Figures* have **captions that explain** all their major features — [YES]

*Tables* and *Figures* have **captions that complement** the information in the main text — [YES]

*Tables* and *Figures* present data that **relate** to the study objective — [YES]

*Tables* and *Figures* present data that are **consistent** with and support the description of results — [YES]

*Tables* and *Figures* have **succinct and informative titles** — [YES]

*Figures* are **accessible** (elements are clearly labelled, accessible colour palettes, colour contrasts, font size legible, etc.…) — [NO]

*Please, check our [Figure guidelines]*

*Figures* with **maps or cross-sections** contain all **elements to be understood** (north arrow orientation, scale, visible coordinates, sufficient coordinate grid intercepts) — [YES]

*Figures* with **maps** have **sufficient location information** (in the map or caption) — [YES]

*Cross-sections* have clear labels for **scale and coordinates** at ends and within-section kinks — [YES]

All georeferenced elements are provided in common format (.shp, .geotiff, .kml) [in an open-access repository] — [YES]

*Citations* throughout are relevant, suitable, and comprehensive — [YES]

*Comments:*

The color palettes should be improved. For example, Fig. 1A uses shades of red for both positive and negative anomalies. When maps are shown that have values divergent from 0 I would recommend a divergent colorscale.

### *B6.2) Author's responses*

We have changed the color scale for Figure 1A to one from Crameri (2018), as for all other color maps as well. When maps are shown that have values divergent from 0 we use a divergent colorscale.

## Section C: Additional comments C1)

## Minor/line-numbered comments

### C1.1) Reviewer's comments

Line 12: levels around -> levels of around
changed
Line 12: Use C rather than K
We find 2°C also confusing in this context as it might indicate an absolute temperature, not a temperature change. We hence opt for "2 degrees" instead of "2 K"
Fig. 1A: I suggest showing the Antarctic 20 model, which is better in this region than SL2013. See earlier comment on the colorscale of this figure
We now show the ANT-20 model of Lloyd et al. (2020) in Fig. 1A. See earlier response on the colorscale.
Fig. 1B: What is the downward axis (1 to 0.5)
We have changed the downward axis labels to km depth. This is now indicated in the figure caption: "Labels on vertical axes are depth in km (10\% Earth radius spacing)"
Fig. 1C: MBL and RS are not clearly labelled. It's very hard to see the continental contour.
We hope is is now clearer, as we have changed the color scale and zoomed in on the Antarctic continent.
Line 48-50: What is means by trough?
What we meant to convey by calling it a trough is that its extent is substantially larger in one direction than in the orthogonal direction. However, we realize that this wording isn't really necessary and now just call it "low geoid". Moreover, by writing "corresponding in position to the areas of Mesozoic subduction surrounding the Pacific" in the following, it should already be conveyed that we imagine the large-scale low geoid to be extended along the strike of the subducted slabs in the lower mantle.
Line 57: what does 'of the subject' mean here? Since this is at the core of the paper I would expect all the relevant material being presented here.
We meant, of the subject of possible mantle plumes beneath Antarctica, and we now include more relevant material, although we cannot offer a complete coverage of the large body of work concerning volcanism in West Antarctica and its origin.
Line 59: Why is this not indicative of lower mantle anomalies?
To explain our reasoning we have first removed "lower" in this sentence, and the following one, but then added this sentence: "Given the spatial extent of about 2000 km for the topography high and that dynamic topography kernels above spherical harmonic degree 12 are probably rather small in the lower mantle (Steinberger et al., 2010), there are very likely density anomalies in the upper mantle." We hope that this phrasing also makes it clear that this does not provide evidence against the density anomalies also extending to the lower mantle.
Line 59: In the residual topography calculation, has the continent been unloaded by Antarctic ice mass first? See Paxman et al., 2022 Scientific Reports for the relevant dataset
We have subtracted crustal and glacial isostatic topography, so if you want to put it that way, the continent has been unloaded first. We have also added "and glacial" to the text which was missing before. We use layer densities and thicknesses from the CRUST1.0 model globally. This is not as accurate as using the dataset of Paxman et al. (2022), however, given that the thickness and density of the ice sheet

is known with much less uncertainty as for the crustal layers beneath, we regard this approach as appropriate.

Line 61: There's a lot of literature on the low viscosity beneath WAIS, which is not cited here. I think this is not very pertinent to the paper anyways, but it might be appropriate to cite some of the relevant work.

We now cite some of the relevant work

Line 64: "The right panel" – of what figure?

of Figure 3 – has been added

Line 65: Fix wording "respective minimum resp. maximum"

We have changed "profiles of geoid and topography, approximately across the center of the respective minimum resp. maximum" to "profiles of the geoid across its minimum and topography across its maximum"

Line 66: I don't think it's useful to refer to the results section in the introduction since there's no context yet for the work that has been done.

We have removed "which can be compared to the profiles in the results section"

Line 66: comparison of what?

Has been removed (see the following response).

Line 66-68: Can this sentence be reworded, it's hard to follow.

We have removed this sentence, as it is not really necessary, and hard to understand at this point in the paper: Regarding the geoid, the mentioned shift of the profiles can be better understood after the effect of lower mantle anomalies is actually introducted, and it is further discussed there. Regarding topography, we discuss a few sentences further down why here we don't consider a possible shift due to lower mantle anomalies.

Fig. 2: What does 'assuming isostatic compensation' mean?

What we meant was assuming crustal isostasy, and we changed it accordingly (we also changed Fig. 3 to "assuming isostasy"). What we meant is, we don't use the actual elevations and depths of crustal layers, but the whole "package" is vertically shifted to correspond to isostasy, before its contribution to the geoid is computed. This vertical "shift" corresponds to dynamic (or residual) topography. But since this step does not change the overall shape and size of the geoid low in a major way, and also, this geoid low is only used for qualitative comparison with modeling results, we think this description is sufficient for the purpose of this paper.

Line 74 onwards: This doesn't seem like introduction to me but rather methods and results

As already said in B2.2, we have moved lines 74-111 of the original manuscript to the methods, results and discussion section (but the mere shift is not shown in the track changes version). However, we prefer to keep the previous lines 112-142 in the introduction: Lines 112-117 and 122-128 give some reasoning for the approach we choose, and 129-142 give an outline for the rest of the paper, hence we believe those lines belong to the introduction. Lines 118-121 and Figure 5 do show a result, however the data for this result have already presented in Steinberger (2016). Although they haven't previously been shown in the south polar map projection, we believe that shifting to the results section could cause to the wrong perception that this is a new result. Hence we prefer to keep in here in the introduction.

Line 93: geographic reference of N and S and E and W are not unique (clear) here

We have added "of Pacific / Panthalassa" after "east and west"

Line 102: That sounds quite vague, what is this based on.

The size of the remaining anomaly is about 7000 km, corresponding to spherical harmonic degrees l>=6. This statement is based that geoid kernels are likely comparatively small in the lower half of the mantle for l>=6. We have modified our statement to clarify this point.

Line 105: Again I would suggest to not refer to the results at this points since it lacks context.

We have removed this sentence. Since we now use the same geoid color scale for all figures, it no longer applies.

Line 145: I suggest not starting a new paragraph with 'These' but actually describing what is refered to.

We have changed the starting of the new paragraph accordingly.

Line 147: Again not clear what 'these' are

This has also been re-worded.

Line 161: What's a sheet anomaly?

This is hopefully better explained now. What we meant is, we consider the effect of these mass anomalies as if they would all occur "condensed" in the middle of each layer.

Line 161-162: The actual setup is not very clearly described

We hope that this is also more clear now, with the above-described modification.

Line 164: What chosen parameters does this refer to?

We have changed to "Considering phase boundaries with parameters from Steinberger(2007)"

Line 168: I think this section should talk about what temperature anomaly such a density anomaly corresponds to and whether that temperature anomaly is realistic.

We had talked about this in the results section on line 292, and we now added a bit here as well. We now write: "For a thermal expansivity 2-3 * $10^{-5}$/K this corresponds to 333-500 K which is rather on the high end of what is realistic. However, results can be simply scaled down linearly to lower temperatures."

Fig. 6: Why does the black line end at 400km depth?

Sorry, this was actually the wrong black line plotted. The two radial temperature profiles with lithosphere and the adiabatic profile should actually converge towards the base of the mantle, and now we plot the right curve.

Line 220: Is 'conductive' thföre right word here?

Sorry, we meant 'conducive'

Line 220: Add brackets around citation

corrected

Line 222: I always thought that the primary driver for Antarctic inception was the opening of the scotia sea and formation of the Antarctic Cicumpolar Current

This is what DeConto and Pollard write, but in any case, our purpose for citing this reference was merely to make clear that we don't suggest that dynamic topography rise was the primary cause for glaciation. But other mechanisms are certainly possible, and we now also cite Kennett (1977) who suggest the mechanism mentioned by the reviewer.

Line 232: How is this viscosity structure consistent with the profile shown in Fig. 6

It is not consistent. The red curve below the lithosphere is similar to the Steinberger (2006) model which gives a similar geoid fit. We have now plotted the profile corresponding to Steinberger (2016) (but with continuous viscosity variations in UM, TZ, LM) along with these curves to show that it is at least "in the same ballpark". To ascertain that this inconsistency does not affect our results, we have now also performed computations with this viscosity profile. The results for this model remain indeed very similar to results shown, and are therefore not shown. In order to better distinguish the effects of global flow (boundary condition) and (local) viscosity structure, we prefer to use the same viscosity structure for the global flow model for all regional viscosity structures. We have added text here, and in the caption of Fig. 6 to explain this.

Line 239: What kind of artifacts are observed if the upper 200km are included and is this an issue?

The artifacts have to do with backward-advecting density anomalies in a region where thermal diffusion substantially contributes to heat transport. For example, if we backward-advect plates at ridges, the negative anomaly at the ridge will simply disappear, as regions with seismically faster lithosphere are back-advected to the ridge. Another issue is that compositional variations in the lithosphere make the velocity-to-density conversion less appropriate. We now write "There thermal diffusion plays an important role and hence backward advection, which we use to compute past density and flow, is less appropriate. Also, such a simple thermal conversion is less appropriate in the lithosphere, where compositional variations may affect both density and seismic velocity."

Line 246: Should the sentence end after plume flux?

We have added “is prescribed” to turn this into a complete sentence, and replaced the comma (which was placed here by mistake) by a full stop.

Line 270: The smoothing is not actually described in the methods.

We now describe this smoothing in the methods section.

Line 270: I would recommend giving a general overview of the results before describing the best fit model.

We now start this section with the introductory sentence “We now show results of our simple models for synthetic disk-shaped anomalies of a given size and depth range.”

How was best fit quantified? How are uncertainties in the observed geoid and DT field included in the model – data fit analysis?

Given the simplicity of the model, none such analysis was performed. The assessment that this is approximately a “best-fit” model is merely based on visual inspection. In order to avoid the impression that there is something more behind, and because the visual comparison is discussed below anyway, we have removed “approximately the best-fit model” here.

Line 290: degree if -> degree disk if

corrected

Fig. 8: I suggest adding the observed curves on top of the right figure to highlight where the prediction fits and where it doesn’t. Is the result sensitive to the chosen viscosity profile?

We have added the observed curves on top of the right figure. Also following the suggestion from the other reviewer, we now also try some different viscosity profiles. The viscosity profile that we had chosen was such that to give a good fit to the geoid globally, but we realize that it would of course make sense to try out a different viscosity profile that corresponds to the viscosity locally reduced by a factor 10-100 in parts of the upper mantle, corresponding to the estimate given in this paper. Accordingly, we have added a new figure. Interestingly, this allows a much better fit to both geoid and dynamic topography even for a rather shallow disk, which is much more similar to our dynamic forward model. In this way, our results become much more consistent and give a better support to the proposed viscosities. We have also re-written parts of the paper to accommodate this improvement.

Line 328-330: This should have been clearly established in the methods and not need repeating here.

We find that lines 328-332 are not needed, as the information given mostly repeats what is already said in the methods section. Accordingly, we have deleted these lines.

Line 336 – 338: Fix grammar

We have added “which is” and hope the sentence in grammatically correct now.

Fig. 9: I suggest separating the geoid and topography predictions

We have separated them now with a dashed line.

Table 2: This should be in the methods, this is part of the model setup!

We have moved this table, and correspondingly the first sentence of the results section, to the methods. However, for better visibility of other changes, this shift is not indicated in the “track changes” version.

Figure 10 & 11: I recommend also showing the predicted gravity anomalies

This does not seem to be possible within ASPECT as it gives me the error message “This postprocessor can only be used if the geometry is a sphere, spherical shell or spherical chunk." Also, we think this would not give meaningful results anyway, for the following reason: With the geoid kernel approach, the modelled geoid critically depends on the viscosity contrast of upper and lower mantle (see the early work by for example Richards and Hager, who concluded a higher viscosity in the lower mantle based on fitting the geoid). Hence, we do not expect to obtain a meaningful result from a model that only includes the upper mantle. We have, however, now also added dynamic topography along the profile where the mantle nonadiabatic temperature cross sections are shown, and a corresponding description in the text. This gives a clearer picture of the magnitude of dynamic topography. We also give a reason for not computing the geoid.

Line 353: on Ross -> on the Ross

corrected

Line 359 – 361: Ant-20 is more reliable than the vote maps. I recommend using this for the interpretation rather than the suite of global models.
We have added a comparison with ANT-20. However, we still think there is some value in comparing with the vote maps, hence we also kept this comparision. We think that a comparison with both ANT-20 and the votemaps broadens the picture, in that it shows that the dynamic models don't provide a good match for either of them.
Line 366: Fix grammar
We were not sure what is grammatically wrong with this sentence. We have now changed "may make sense" to "is warranted".
Fig. 12: Can you also show the DT and geoid?
It would be possible to show dynamic topography here, but we don't think this would add much to the paper. The main purpose of this figure is to show that whole-mantle plumes have a very similar shape in the upper mantle to the upper mantle plumes (so generating the plumes by prescribing an influx into the upper mantle box doesn't lead to major artifacts), and which CMB temperature anomaly is needed to get approximately plume temperature anomalies. Obiously, too high temperature would also translate to too high dynamic topography. Another issue is that for these models the lithosphere has grown thicker.
Hence the plumes look similar in shape to the upper mantle plumes, but beneath a thicker lithosphere. This thicker lithosphere would in turn cause reduced dynamic topography, giving another reason why dynamic topography in these cases is less meaningful than for the upper mantlle cases.
The geoid is equivalent to gravity (apart from different weighting of sperical harmonic coefficients), hence the problem is the same as for Figures 10 and 11 as ASPECT gives me the error message "This postprocessor can only be used if the geometry is a sphere, spherical shell or spherical chunk."
Line 384 onwards: Should go in methods
We have moved lines 384-390 to the methods section, at the end of the subsection "Computations with spherical harmonic code", and somewhat re-written. This paragraph has been shortened accordingly. Line 397: What do they find? Background on past work lacking
We have added "In this way, they are also able to explain a localize geoid low" Line 415: 'one possible way out' is quite colloquial. Improve language throughout We have changed to "One possible explanation could be ..."
Line 417: I think the analysis using two disks should be described in the methods and results rather than just introduced here in the discussion.
We have moved lines 417-426 and (the previous) Figure 13 to the results section. Since the two-disk models are just a linear superposition of models with one disk, we see no need of a separate introduction in the methods section.
Line 441: Their models fit the data somewhat (but not great) and are not dynamically consistent. Is a viscosity inference really sensible here?
This is merely to show that our results are not at odds with previous estimates. Accordingly we have re-formulated the last two sentences of the discussion, to tone down the relevance of our viscosity estimate. We do want to end with a few thoughts on viscosity, though, since mantle viscosity and its possible relevance for the stability of the West Antarctic Ice Sheet was the initial motivation for this work.

### *C1.2) Author's responses*

They are given inline above.

## C2) Other remarks

Comments by Reviewer 2 & Authors' reply

# Section A: Overview of manuscript

## A1) Overall evaluation, general comments & summary

### A1.1) Reviewer's comments

*A1.1.1 ) General evaluation and publication suggestion - Required:*

This manuscript presents a study of the mantle structure able to explain the Earth's deepest geoid low (when referred to a hydrostatic ellipsoid) near West Antarctica, together with a residual topographic high which is shifted towards the south-east with respect to the geoid signal, considering constraints on the mantle sources based on seismology and mantle dynamics models. The authors perform sensitivity tests of the geodetic signals to the spatial structure of the sources, showing that the sources should reach the lower transition zone (650km depth) in order to explain the geoid signal, and that they should not be shallower than ~150km depth in order to explain the dynamic topography. The shift between the geoid and the topography signals can be explained when considering two laterally displaced disks, one at the base of the transition zone and one below the lithosphere, both corresponding to hot and light material.

The seismic tomography is in agreement with a source across the upper mantle, and down to 1000km depth. However, the dynamical plume models are not consistent with the results of the synthetic tests, because they predict the development of a plume with a too narrow conduit compared with the 2000km width of the disks fitting the geodetic data, and spreading only at shallower depths (thus, there is basically no source below 300km depth). Another difficulty is that this shallow spreading of the source (also suggested from seismic tomography) does not seem to be reflected in the two-disks model jointly explaining the amplitude and shifted locations of the geoid and topography anomalies (Fig. 13), if I understand correctly. Finally, an estimate of the viscosity in the hot and light sources below the lithosphere is obtained, for West Antarctica. Such estimate Is important in order to understand how post-glacial rebound may impact the evolution of the ice sheet.

Constraining the mantle structure and dynamics at regional scales from a combination of geophysical observations and dynamical models, as done here, is a very important and active topic. I think it is nicely addressed in this careful and detailed study, which also underlines the challenge of reconciling the different observations and the models. The conclusions call for further research to understand the dynamical origin of a light source in the lower transition zone, suggested from the geophysical data analyses but absent from the mantle flow modelling. I have a few questions in particular regarding the synthetic tests with the disks, but I think this work is very suitable for publication.

*A1.1.2 ) What does the submission need to be publishable? (select as needed; comment for all cases)*

â˜e No changes required
â˜e Rewriting

â˜e Reorganising

â˜‘ More data/figures
â˜ Condensing
â˜ Reinterpretation
â˜‘ Other

***Comments:***

Please see my comments in section A.2.1. A few additional synthetic tests could be interesting, to investigate the effect of a sharp viscosity contrast at the base of a sub-lithospheric low viscosity layer, or of a wider shallow disk in the two-disk model. Some more information concerning cited references for the deep slab signal correction, and the used visocosity profile, could also be added, as well as a few words of discussion on possible thermo-chemical sources in the upper mantle ?

*A1.1.3) Can the submission be improved by reducing/adding any of the following? (select as needed; comment for all cases)*

â˜‘ Text
â˜ Table
â˜‘ Figures
â˜ Supplementary material

***Comments:***

Some minor additional information in the Figures / Figures captions could be helpful, in particular to localize on the maps the different regions discussed in the text. See also my comments in Section A1.1.2.

***A1.1.4) Please complete the following section if you recommend that the submission is NOT appropriate for publication (select as needed; comment if a box is selected)***

â˜ Quality is poor
â˜ Research is not reproducible
â˜ Other

***Comments:***

[Free form box]

A1.2) Author(s) Responses:

We thank the reviewer for the generally positive view on our paper in part 1.1.1. Comments in part 1.1.2 and 1.1.3 are all repeated in more detail below, and will, accordingly be also addressed below.

## A2) Summary of main merits and main points of improvement

### A2.1) Reviewer's comments

The main merits I have found are:
- the multidisciplinary nature of the approach and the effort to compare observation-based, constraints on the upper mantle structure with dynamical simulations of a plume model,
- the sensitivity tests of the geoid and topography signals to the spatial distribution of the mass sources at depth, and characterization of the mass sources able to explain the geophysical observations, which suggest that thermal plume models are not appropriate,
- the detailed investigations, where each step in the analysis, and the impact of a number of parameters, are carefully described and discussed.

The main points of improvement I have found are:
- adding information from the cited references to describe the removal of the deep slab signals from the geoid data (and whether it may explain the shift between the geoid and the topography signals ?) and the viscosity structure used in the sensitivity tests of Fig. 9.
- would the results of Fig. 9 change much if the reference viscosity profile includes a sharper low viscosity layer below the lithosphere (in the cited Steinberger 2016 paper, Fig. 3a, the viscosity profile has a minimum below the lithosphere, and then gradually increases with depth in the upper mantle, instead of a sharp increase at its base) ? Also, the two-disk model in Fig. 13, that explains the shift in the geoid and topography signals, does not seem compatible with the shallow spreading of the sources suggested from dynamical models and seismic tomography. How would this two-disks model perform with a wider shallow disk?
- if I understand well, the dynamical modelling is purely thermal, but I wonder how the results would change in the presence of compositional variations in the upper mantle, and if larger upwellings, better fitting the observational constraints, could be obtained. Maybe a few words of discussion could be added ?

### A2.2) Author's responses:

We thank the reviewer for finding several merits of our work. All the points of improvement are all repeated in more detail below, and will, accordingly be also addressed below.

# Section B: Detailed evaluation of manuscript

## B1) Title and abstract

### B1.1) Reviewer's comments

The abstract could state explicitely that you consider thermal plume models (and not thermo-chemical) .

B1.2) Author's responses

The abstract now states explicitly that we consider thermal plume models

## B2) Introduction

B2.1) Reviewer's comments

In the legend of Fig. 1, could you please add the meaning of the radial numbers in panel b (0.6 to 1, how do they relate to depth ?) Also, it would be nice if you could indicate in a map the location of the discussed regions (Ross Sea, West Antarctica, Mount Erebus, Mary Bird Land). All the corrections applied to the geoid and topography data, to isolate the components related to upper mantle sources, are described in the Introduction. Maybe they should rather constitute the first sub-section of the Section 2 on Methodology, or a new section 2 entitled "Data" ?

B2.2) Author's responses

We have changed the vertical axis labels to 2548 to 0, and write in the caption that these are depths in km, spaced at 10% of Earth's radius. Marie Byrd Land, Mount Erebus and Ross Sea was already indicated in panel D, but we somewhat changed the figure caption now, to make this clearer. West Antarctica is now indicated in panel A. We have moved the correction to isolate the components related to upper mantle sources to new first sub-sections of the methods and results section, and the first paragraph to the discussion.

## B3) Data and methods

B3.1) Reviewer's comments

Please see comments above:
- it would be nice to explicitely describe (maybe adding a Figure) the viscosity structure used in the computations with the spherical harmonics code (section 2.1)
- you could also underline that the plume modeling is purely thermal,
- you could move here the paragraphs of the Introduction which describe the geoid and topography data and the corrections applied ? Could you please briefly recall how you estimate the geoid contribution of the subducted slabs (treated in Steinberger & Torsvik, 2010 and shown in Fig. 4c) ? Could we explain the spatial shift between the geoid and the topography signals by an imperfect correction for the deep slab signal ?

B3.2) Author's responses

The viscosity structures used in the computations with the spherical harmonics code are now shown in the new figure, where results for different viscosity structures are shown.

We have now also added here (at the beginning of the second paragraph of the subsection on Aspect models) that the plume models are purely thermal,

We have moved the paragraph of the Introduction which describes the geoid correction for lower mantle sources to the methods section. In this paragraph, we have now also somewhat extended the description of how Steinberger and Torsvik (2010) compute the slab+LLSVP geoid contribution. However, we keep the description of topography data and the geoid correction for ellipticity, sea floor age and crustal thickness here, as this follows established and previously developed methodology. And, yes, probably the spatial shift between the geoid and the topography signals could be explained by an imperfect correction for the deep slab signal. This is now also briefly discussed in our discussion.

## B4) Results

### B4.1) Reviewer's comments

Please see my general comment above (in Section A.2.1). I wonder if the results of the synthetic tests shown in Fig. 9 would change much if the reference viscosity profile includes a sharper low viscosity layer below the lithosphere (in the cited Steinberger 2016 paper, Fig. 3a, the viscosity profile has a minimum below the lithosphere, and then gradually increases with depth in the upper mantle, instead of a sharp increase at its base)? This would reduce the dynamic topography signals from the sources located below the low viscosity layer, and thus maybe also reduce the maximum depth of the sources fitting the geoid?

Another question concerns the two-disks model shown in Fig. 13. If we increase the width of the shallow disk, to account for plume spreading below the lithosphere, could the laterally displaced two-disk model

explain the shift between the geoid and topography signals ?

### B4.2) Author's responses

The viscosity profile that we had chosen was such that to give a good fit to the geoid globally, but we realize that it would of course make sense to try out a different viscosity profile that corresponds to the viscosity locally reduced by a factor 10-100 in parts of the upper mantle, corresponding to the estimate given in this paper. Accordingly, we have added a new figure. As the reviewer suggests, this allows a much better fit to both geoid and dynamic topography even for a rather shallow disk, which is much more similar to our dynamic forward model. In this way, our results become much more consistent and give a better support to the proposed viscosities. We have also re-written parts of the paper to accommodate this improvement, and we thank the reviewer very much for this question which led, we think, to a considerable improvement of our paper.

Regarding the two-disk model, we have already chosen a width of the shallow disk to match the size of the residual topography high. We have added a sentence to explain that.

Also, the width of the shallow disk approximately matches the plume size of the dynamic forward models, and we also mention that now.

## B5) Discussion and conclusions

### B5.1) Reviewer's comments

Please see my general comment above (in Section A.2.1). If I understand well, the dynamical modelling is purely thermal, but I think it would be interesting to discuss the possibility of compositional variations in the upper mantle, and their impact on the morphology of the sources ?

### B5.2) Author's responses

We have added a bit of discussion and some references, but a more detailed discussion of this would go way beyond the scope of this work.

### B6) Figures, tables and citations

Please see my comment in the Introduction Section about the Figures.

### B6.2) Author's responses

The corresponding response about Figures is given in the Introduction section.

# 2nd Round of Revisions

## Comments by Reviewer 1 & Authors' reply

The authors have thoroughly addressed my comments. I found a few more typos that I note below but otherwise recommend this paper for publication. Thanks for the thorough and positive review!

Line 9: This is an editorial choice but I would suggest writing out "w.r.t." now "with respect to" Several citations are not formatted correctly (see e.g. line 35, 42-46 and elsewhere) corrected Line 85: The text here reads as if Paxman et al. constructed residual topography, which they didn't. They simply calculated the topography after ice unloading. I'm therefore not sure that this can be compared to the residual topography map. Please check that the context here is

appropriate. I think they did construct residual topography. In their Fig. 11 they first show "Rock- equivalent topography", which I think is the same as what you describe as "topography after ice unloading". But then they also subtract isostatic topography from crustal thickness to obtain "Residual topography", similar to how I compute residual topography on continents. So I think it can be compared. Please let me know if you think I misunderstand something here.

Line 91: bee -> been, Line 111: compated -> compared, Line 178: remove duplicate "due to",

Line 210: Remove " at the end of line, Line 258: at bottom -> at the bottom, Line 389: due a -> due to a, Line 495: betwee -> between, Line 512: closing paranthesis missing, Line 546: somewhow -> somehow all corrected

Line 569: verb missing in sentence added "is" Line 602: fo -> for corrected

## Comments by Reviewer 2 & Authors' reply

Thank you for this revised manuscript. The author have well responded to my comments, and they have also strengthened their conclusions on the mantle structure and dynamics beneath West Antarctica as constrained by geophysical observations and dynamical models. So I think the manuscript can be published. Thanks for the positive and careful review!

I only have a few minor questions/remarks, below :
- in the caption of Table 1, you mention for the line « LLSVP-corr: Difference of two previous lines as in Figure 7B » : could you please explicitely state that you mean coefficients corrected for the crust, for seafloor age and for LLSVPs as given by the line just above ? Changed to "Coefficients corrected for the crust, for seafloor age and for LLSVPs as given by the line just above, as in ..."
- line 209-210 (version without track changes) : you mention a temperature contrast of 333-500 K for a 2-3 · $10^5$ /K thermal expansivity, could you please explicitely state that this is for a 1 % density contrast ? Changed "this" to "a -1% density anomaly". Also corrected exponent to -5.
- end of Section 2.2 : could you please add a reference for the calculation of the dynamic topography associated with a cylinder in the presence of lateral viscosity variations, or maybe explain in more detail the principle of the calculation (especially for the calculation of the ratio of geoid to dynamic topography amplitude) ? I added a more detailed calculation.
- References : there is a problem of missing parentheses in the citation of some bibliographic references, for instance see lines 44, 44, 106, 550, ... (version without track changes). corrected
- Textual suggestions (all the line numbers correspond to the version without track changes), in capitalized letters below :

line 91 : bee -> beeN / line 115,292 : regionial -> regionAl / line 201 : as if they would all occur IN THE middle of each layer corrected

line 271-278 : this is a very long sentence, maybe you could split it into two sentences ? Done, by replacing "although" by "However, "

line 343 : The remaining smaller-scale low (Figure 7 D) has an extent of ~ 7000 : the unit is missing. Added "km"

line 456 : (corresponding to the best-fit case in Figure 11 : the end parenthesis is missing added line 599 : the predicted source location at the CMB IS not above LLSVPs corrected

caption of Fig. 7 : contribution in (A) removed from from -> removed from corrected
line 690 : fo -> foR corrected (line 690 in the "track changes" version)