"Effects of upper mantle heterogeneities on lithospheric stress field and dynamic topography" by Anthony Osei Tutu et al.

Referee #2 T. Gerya (Editor) taras.gerya@erdw.ethz.ch

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This is an interesting and timely paper presenting results of modeling global topography and stress distribution based on a coupled numerical model of mantle convection and lithospheric dynamics. The upper 300 km shell of the model with free surface is modeled using realistic visco-elasto-plastic rheological model for the mantle and crust. The paper is of broad interest but I think the quality of presentation could be improved by addressing several discussion points listed below.
Taras Gerya, Zurich, 15.01.2018

We are very much appreciate Taras Gerya time taken to look at our manuscript, the comments and suggestions to help us improve the manuscript. In the paragraphs below, we have carefully considered each comment and provided the response. Also we have accounted for the required modification in the revised manuscript were relevant.

Ref. Points 1-Page 1:
We show that lateral density heterogeneities in the upper 300 km have a limited influence on the modeled horizontal stress field as opposed to the resulting dynamic topography that appears more sensitive to such heterogeneities. There is hardly any difference between the stress orientation patterns predicted with and with- out consideration of the heterogeneities. . .". This low sensitivity in term of stresses seems unfortunate. Is there any way to increase sensitivity? Changes in the dynamic topography should typically result in notable changes of bending stresses inside plates. Perhaps the method of comparing simulated and observed stresses should somehow try to isolate better the bending stress component?

Although the overall lower sensitivity of the modeled lithospheric stress field to the upper mantle heterogeneities (here the top 300 km) marks one of the findings of this study, we find some significant regional influences such as in the Andes, due to the crustal thickness variations and upper mantle heterogeneities. These regional effects together with global stress pattern when compared to the observed stress show the range of impacts of the deep mantle flow.
We also find that using different data to describe the upper mantle structure, either based on seismic topography or heat flow data, did not significantly impact stress field magnitudes and horizontal directions. This is illustrated in Fig. R1 where we estimate the differences in the respective modeled stress magnitudes and orientations as well as their corresponding dynamic topographies. In Fig. R1 (a & b) we show the differences between the modeled stress magnitude with crustal thickness variations (Fig R1a) and without (Fig. R1b) and the corresponding stress orientations differences (fig R1c and d).
However, in Fig. R1e the difference in dynamic topography models shows very large amplitudes in cratons, which is not the case for the corresponding differences in the stress magnitudes (fig R1b) or orientations (fig. R1d). This may suggest that changes

in topography may not readily translate into bending stresses inside the plate interior. Also, as we mentioned in the study lateral viscosity variations in the crust and lithosphere could be one major influence on the stress sensitivity controlling how the lithosphere strength responds to the mantle flow below. However, in this study we concentrate on the thermal-density structure of the upper mantle without exploring variations in the strength of the lithosphere and a rheological parameter space to understand how the stress will be transmitted elastically over long distances.

A study of different crustal and lithosphere lateral viscosity variations should be the focus of a future study, now that the dependency dependence of the stress field on the upper mantle density heterogeneities is established.

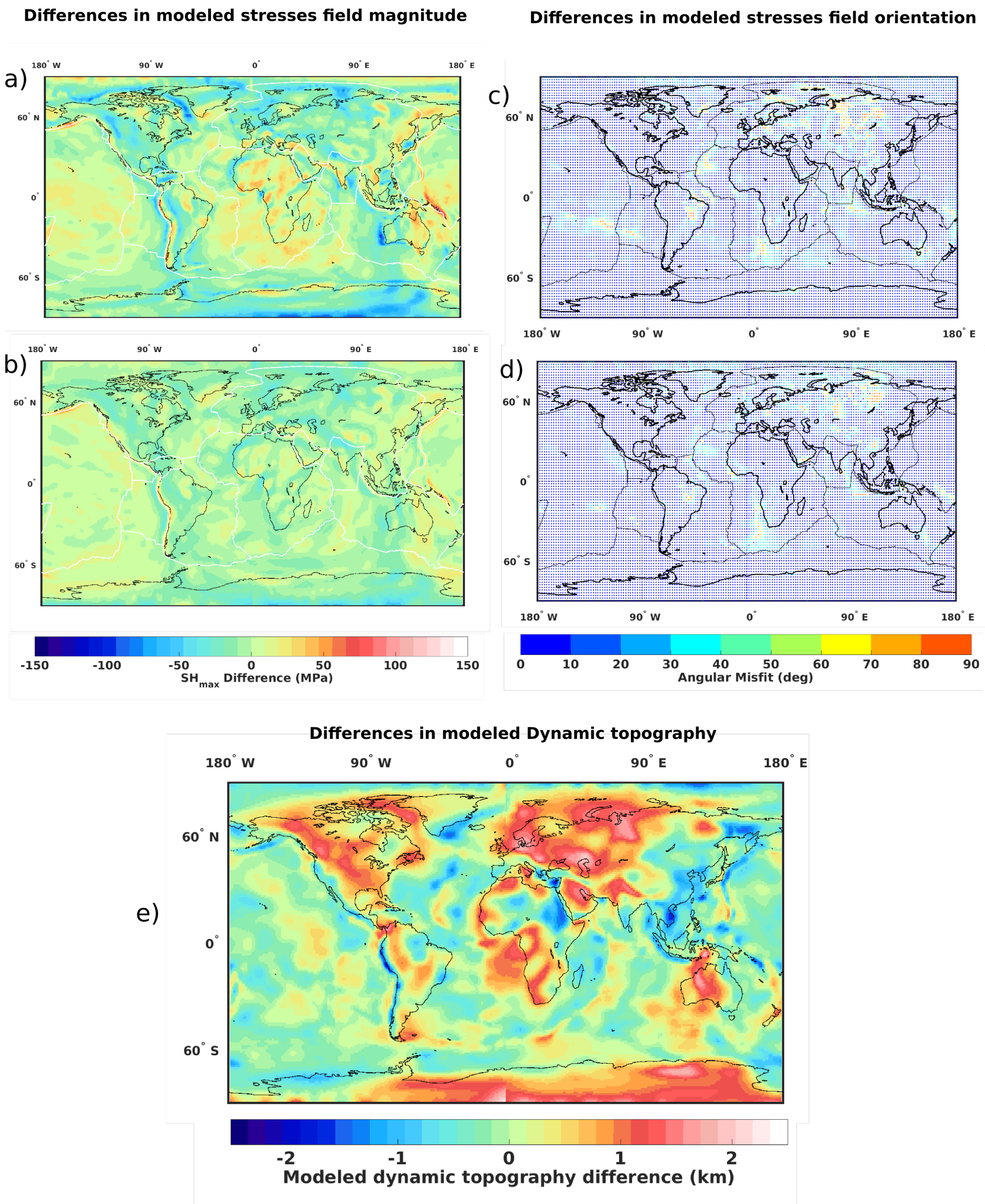

Figure 1R: Estimates of the differences between modeled stress (a) magnitudes and (c) orientation of main manuscript Figure 6 (a&b). Also similar exercise is shown in (c) magnitude difference and (d) orientation difference of the main manuscript Figure 7 (c & d). In (e) is the corresponding dynamic topography difference between main manuscript Figure 7a and 7b.

Ref. Points 2-Page 1:
"After correction for the chemical depletion of continents, the TM2 model leads to a much better fit with the observed residual topography giving a correlation of 0.51 in continents, but this correction leads to no significant improvement in the resulting lithosphere stresses." Same as above. Would be good to understand better where major discrepancies for stresses are coming from – missing slabs? data inaccuracy?

To add to the above response, Naliboff et al., (2009) showed the influence of the cratonic roots on lithosphere stress field in both magnitude and pattern is small compared to cratonic influence on the changes in mantle tractions generating the stresses in the lithosphere plate. The regional influence on topography due to realistic treatment of the cold craton is not apparent in the respective stress field probably due to the integrated effect of large mantle driving forces transmitted elastically from far field through the lithosphere and thus overwhelming the local change in the mantle lithospheric structure.

Taking for instance, the IBM region, when we consider lithosphere stresses due to only the viscous mantle flow, we a predict purely compressional regime (Figure 3a). However, in the scenario considering only the crust, the lithosphere and a part of the asthenosphere above 300 km (with muted contribution of the lower mantle), we predict an extensional stress regime for the IBM subduction region (Figure 3b). Their combined contributions to the stress field make the IBM regions compressional, showing the influence of the integrated traction from the viscous mantle. When we explicitly include slabs in the top 300 km (TM1), the compressional regimes overwhelms the observed extensional back-arc IBM subduction system but the stress pattern does not seem to significantly change. * We further comment on this in Ref. Points 9-Page 17*

We can attribute some of the discrepancies between the model and the observational data (WSM2016) to the interpolation method, the search radius for interpolation and the upper mantle structure considered. However, there is a relatively good agreement in most regions between the modeled and observed stress fields with some misfit in some regions due to the thermal density structure considered in the upper mantle. For instance, in the Tibet region, considering heat flow-based thermal structure (TM1) give a better fit for the to the observed stress field compared to the modeled stress field with the S-wave model. Also, the inclusion of slabs in TM1 gives a better fit in the Sumatra subduction compared to the slabs implicitly included in the seismic model (TM2).

There are some regions such as the Colorado Plateau that will still need further investigation with regards to the disorientation of the stress patterns between the model and observations. This will be appropriate for future studies using recent high-resolution seismic tomography from the US Array to help understand that anomaly.

Ref. Points 3-Page 3:
"The residual topography is here defined as the observed topography corrected for the variations in the crustal and lithosphere thickness and density variations and for subsidence of the sea floor with age." One could also mention here strong influence of the complex brittle–ductile rheology and stratification of the continental lithosphere result in short-wavelength modulation and localization of deformation induced by mantle flow (Burov & Guillou-Frottier, 2005).

Yes, this is very true with regards to topographic change in the lower crust and the lithosphere. We have included the sentence below:

"Also at the tectonic-scale topography is influenced by the elastic-brittle–ductile layered crustal-lithospheric layered structures underline by the viscous mantle convection (Burov and Guillou-Frottier 2005)."

NB: The introduction paragraph containing "The residual topography is here defined as the observed topography corrected for the variations in the crustal and lithosphere thickness and density variations and for subsidence of the sea floor with age." has been removed per the suggestion of the first referee to shorten the introduction.

Ref. Points 4-Page 6:
"A forward model is run for half a million years with a time step of 5kyr, 5 and at each time step tractions in the lower mantle due to density heterogeneities are computed using the spectral mantle code and then passed across the coupling dynamic boundary to the top component SLIM3D. Within the upper domain (SLIM3D), the flow velocities are then computed and passed back across the coupling boundary as an upper boundary condition to the spectral mantle code, with the method convergence estimated by comparing the velocity and traction norms of two successive iterations." This approach does not seem to account for continued slabs crossing 300 km depth level. It has been demonstrated by Stadler et al., (2010) that having such continued slabs is essential for properly reproducing surface plate motions.

AU: Here, in our calculation within the mantle above the 300 km we explicitly consider slabs in the TM1 thermal structure and below 300 km depth we assume density heterogeneities corresponding to slabs and upwellings captured by the Smean seismic tomography without explicitly including slabs. Furthermore, in our plate motion paper (Osei Tutu et al. 2018), we show that with this approach when used to predict global plate motions, we obtained a good fit to the observed plate motions NUVEL-1A (DeMets et al., 2010) with rms velocity of 3.75 cm/yr.

Ref. Points 5-Page 6:
"Within the upper mantle, our crustal rheology is taken from Wilks (1990) and below the crust we have considered dry and wet olivine parameters in the lithosphere and sub-lithospheric mantle layers, respectively, modified after the axial compression experiments of Hirth and Kohlstedt (2004) (shown in the appendix, Table A1. Adopted from Osei Tutu et al. (2017) for studying the influence of both the driving and resisting forces that generate global plate velocities and lithospheric plate net rotation)." Reference to Osei Tutu et al. (2017) is for a paper in revision, which is not accessible.
AU: This referenced referred paper (Osei Tutu et al. 2018) is now published. We presumed it would be out before the review process of this paper has been finished; that is why we referred to it instead of repeating the exercise in this manuscript.

Ref. Points 6-Page 6:
"Here the topographic signal induced by the layers below 300 km is assumed to be due to convection in the viscous mantle, although cold rigid subducting slabs (Zhong and Davies, 1999; Faccenna et al., 2007) and possibly also the deepest cratonic roots (Conrad and Lithgow-Bertelloni, 2006) extend deeper than 300 km." Do you account for slabs at <300 km depths? Does not seem to be the case in Fig.2, but Fig. 3d shows some slab-like features along the western margin of South America. What moves plates in the absence of slabs and prescribed surface velocities (free surface boundary condition is used) – mantle drag only? How realistic is this approach for the global

plate tectonics of modern Earth, which is assumed to be predominantly driven by the slab pull? Would be good to discuss this in some more details.

AU: Yes, we account for slabs at depths above 300 km in the TM1 upper mantle structure while we assumed that slabs are picked by the seismic tomography the S-wave structure TM2. Likewise in previous Figure 3d (now Figure 2c) the slab signal under South America is coming from the Smean tomography without explicitly including slabs below the 300 km. As we have shown for different depth slices for TM1 on the left column.

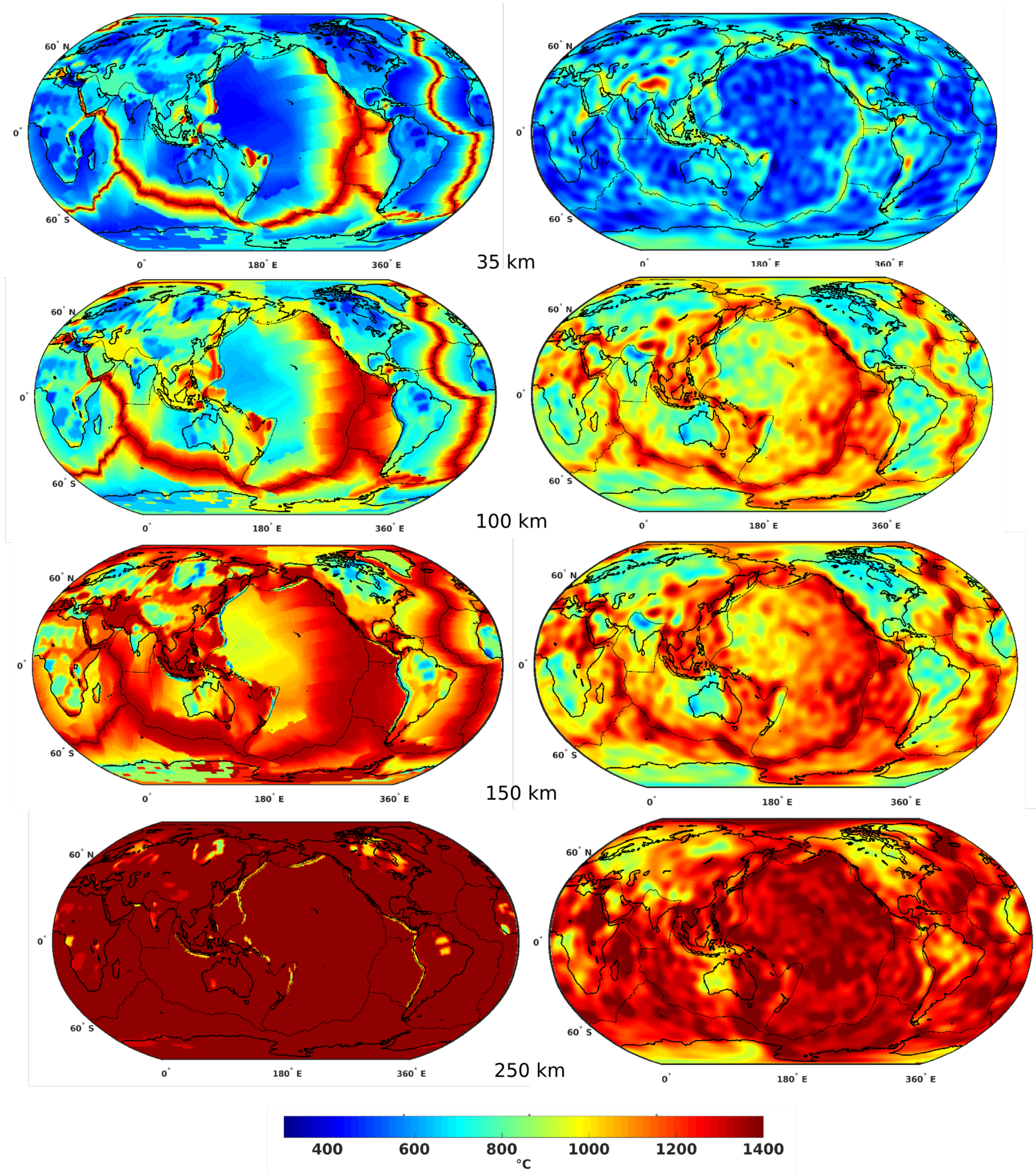

Figure S1: The thermal structure of the upper mantle at a depths of 35, 100, 150, and 250 km from the two reference thermal models adopted in this study, TM1 (left column) and TM2 (right column). TM1 is derived from the thermal structure TC1 of Artemieva (2006) in the continents and the sea floor age model of Müller et al. (2008) in the oceanic areas, while the TM2 model is based on the S-wave tomography-model SL2013sv from Schaeffer and Lebedev (2013) for inferring thermal structure in the upper 300 km. A detailed description is given in the main text

Ref. Points 7-Page 10:
"Since the focus of this study is to investigate the effect of the upper mantle lateral density variations on the horizontal stress field and dynamic topography, an assessment of the influence of the plate boundary friction and water content in the asthenosphere on plate velocities has been carried out in a separate study (Osei Tutu et al., 2017). Hence, in the present work, we constrain our resulting creep viscosity with a cutoff for extreme viscosity values in the upper mantle by setting permissible minimum and maximum viscosity values similar to Becker (2006) and (Osei Tutu et al., 2017), with this approach yielding a good fit between the observed and modeled geoid." Would be good to give some summary of plate velocity modeling results since the referred paper (Osei Tutu et al., 2017) in review is not accessible. For example, Stadler et al. (2010) suggested that prescribing slabs in the upper mantle is essential to reproduce global plate motions. Can you confirm this?

AU: Our analysis in Osei Tutu et al., (2018) with explicit inclusion of slabs in the top 300 km and viscous mantle drive from seismic tomography (Smean) below confirms the suggestions of Stadler et al. (2010). In instances where slabs were not considered, the fit to the observed plate motion deteriorates. However, the benefits of considering slabs in the stress field studies are arguable. An inclusion of slabs improves the fit in the Sumatra subduction area, while contributing to a strong compression regime in the IBM extensional subduction area.

Ref. Points 8-Page 15:
"Cammarano et al. (2011) showed that correction for the depletion of the lithosphere increases the inferred temperature of a cratonic root by about 100K and decreases density by about 0.1 gcm^{-3}, and fits observations well compared to models assuming pyrolitic composition." Depletion-related density decrease of the cratonic mantle is age-dependent and increase from 30 to 80 kg/m^3 (i.e., 0.03-0.08 g/cm^3) with increasing the age from the Phanerozoic to the Archean (Djomani et al., 2001)

AU: Thank you for the spotting this! Indeed, there was a mistake in the density difference value, which is assumed in the study (i.e. 0.01 gcm-3) as opposed to the wrong value mentioned in the text (0.1 gcm-3), which would be very low. In our qualitative analysis we do not distinguish cratonic regions based on age progression from the Phanerozoic to the Archean, but rather considered a single value for all cratons.
We have taken your suggestion into account and use the mean value of 40 kg/m^3 (0.04 gcm-3) for a density correction of depletion-related density in cratons. We know that a proper treatment of different ages of cratons may improve the modeled topography, but our analysis shows there seems to be no significant impact on the stress field. We therefore have reformulated the relevant part as of the manuscript as:

"Previous studies of cratonic mantle depletion in- relation to density and temperature inferred from S-wave models (for example, Cammarano et al., 2011) identified composition as the key dominant agent for the low modeled topography. They showed that with the assumption of only 100 K hotter mantle combined with lateral variations in composition resulted in a difference in density of about 0.1 gcm-3 compared to models assuming pyrolitic composition. The depletion-related density in cratons is age-dependent and considered to be increasing from 30 to 80 kgm-3 (i.e. 0.03 to 0.08 gcm-3) for the Phanerozoic through Protozoic to Archean platforms ((Djomani et al. 2001). Here we aim at a qualitative first order analysis imposing an additional mean density value of 0.04 gcm-3 (about 300 K) as a correction in TM2 cratons to compare with models using realistic corrections. Also, following the realistic compositional correction in cratons by Cammarano et al., (2011) we adopt two additional thermal structures from different seismic tomography models SAW24B16 (Mégnin and Romanowicz 2000) and S20RTS (Ritsema et al. 2007) with corrections applied to the depleted mantle based on the thermodynamic model Perple_x (www.perplex.ethz.ch, Connolly, 2005) and compare with our results."

Our increase in density/temperature for the cratonic regions as a correction in the TM2 decreases the correlation from 0.512 with Steinberger (2016) for the 100 K to 0.50 for the 300 K considered, while the correlation with Hoggard et al., (2016) increases the correlation from 0.180 to 0.192, respectively, since the free-air gravity used by Hoggard et al., (2016) creates positive topographic anomalies across most cratons.

Ref. Points 9-Page 17:
"We predict normal faulting mostly in regions above upwellings (mostly extensional regions) such as the Icelandic swell, Eastern African rift, or along divergent plate boundaries, while thrust faults are mainly predicted in compressional regions such as subduction zones and some other tectonically active regions in continents. In continental areas, few regional variations occur in South America, West Africa and on the Eurasian cratons. In oceans we see variations in the North Atlantic around the Icelandic swell, at the east Pacific Rise and around the southern African plate region." Strong compression seems to be predicted in the extensional backarc of the IBM subduction system (Fig. 6) – this seems problematic to me. Perhaps having continued deep and dense slabs in this region would change this?

AU: Our inclusion of explicit slabs in the top 300 km (TM1) seems to contribute to this strong compressional regime, which is not the case when we rather consider the rather slabs captured by the s-wave model (TM2). The IBM subduction system is shown as compressional when we consider only density heterogeneities below 300 km (figure 3a), similar to what Steinberger et al., (2001) obtained for calculations with viscous mantle flow and half the speed of the free plate motion prescribed as top boundary condition. Having continued slab into depths below 300 km might influence the strong compressional regime we have predicted but our coupled numerical model accounts for realistic crustal-lithosphere structure separately from the viscous mantle and does not allow for the continuity of the slab material. Nonetheless, the implemented continuity of velocities and tractions is very robust, helping us understand stress pattern and regimes in the lithosphere as it is for plate motions.

Also, we do not account for melting and fluid releases in our calculations, which are predominantly the cause of upwelling in the IBM region due to the interaction between subducting and overriding plates. This may contribute to the extensional stress regime of the IBM subduction system (Arculus et al. 2015; Brandl et al. 2017). Hence, such study considering melting and fluid release in future probe is encouraged to shine some light on the dominance of the lower mantle compressional regime in the IBM subduction zone reported here in this study.

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