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

Anonymous Referee #1

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This paper presents a new mantle flow model that couples global mantle flow (driven by density heterogeneity inferred from seismic tomography) to a detailed surface model for the upper 300 km of the mantle – this includes more detailed heterogeneity associated with upper mantle viscosity variations and density heterogeneity. The authors use this coupled model to predict both lithospheric stresses and dynamic topography, which are both the result of convective stresses from viscous flow in the mantle. The authors use 2 different predictive flow models (TM1 based on heat flow and seafloor ages and TM2 based upper mantle tomography model) and compare them to the global stress map for stresses and 2 different predictions of dynamic topography. They find that both predictive flow models (which differ only in the upper 300 km) predict similar stress fields but TM2 gives a better prediction of dynamic topography. The authors conclude

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Discussion paper

that dynamic topography is more sensitive to upper mantle structure than lithospheric stresses, that their model does a reasonably good job of predicting the lithospheric stress field, and that dynamic topography is better-predicted using seismic tomography data instead of heat flow and seafloor ages.

I think that the content of the paper useful and innovative – the new models are innovative and are – to my knowledge – the first of their kind to look at the impact of detailed upper mantle heterogeneity on stresses and dynamic topography. The predictions they make are useful for understanding the problem, and should lead to greater understanding of the system a. Thus, I think that the paper is eventually publishable.

The paper has a lot of information and is a bit hard to follow because the authors are comparing 2 predictive models (TM1 and TM2) with 2 output fields (stresses and dynamic topography), one of which has 2 different options (dynamic topography from Steinberger 2016 or Hoggard 2016). Thus, there are several combinations of comparisons, and it is a bit of an effort to follow what is being compared with what. On top of that, the writing is a bit wordy with some repetition. This makes reading and understanding the paper a bit laborious.

I think for this paper to be effective, it needs better organization and structure, and the writing needs to be simplified and shortened. I give some suggestions for this below. Given this, I recommend significant re-revision of the figures and text, although I do not think that significant new analysis is necessary. Thus, I would probably call for "moderate revision". Below is a an overall assessment of the changes that I think would be useful for improving the paper and also some specific points about the paper.

Major Points:

Overall Structure: One problem is that the predictions and analysis of stresses and dynamic topography are intermingled in section 3. I recommend separating this into two sections, one that is devoted to stresses and the other to dynamic topography. Thus, I recommend removing the topography predictions from Figs. 5 and 6 (so these figures are devoted to stresses), and removing the stresses from Fig. 7 (so this figure is for dynamic topography). I also recommend revising the text around these figures to focus specifically on stresses and then on dynamic topography.

Introduction – I think the introduction is a bit too long and rambling, and should get to the main point more quickly – that this paper presents a new method for evaluating the role of upper mantle heterogeneity (both density heterogeneity and viscosity heterogeneity) for mantle stresses. There is much background – condensing it to just the relevant parts will help the authors emphasize their new contribution a bit more clearly.

Conclusions – I think that the paper does a nice job of concluding the major results. But there is no discussion section – it would be helpful to have a short discussion of the implications of this work for our understanding of dynamic topography and stresses, and other factors that might be important to include in the models in the future.

Figures: There are more figures than are necessary. In particular:

Figure 1 and 3 are illustrative about the model, but the information in these figures is not used much – perhaps only the essential components of Fig. 3 could be incorporated into Fig.1? (for example, the geoid, plate velocity, and tractions are not really used here)

Figure 4 shows comparisons for different models of dislocation and diffusion creep, and different water contents – however it is not clear to me that all of this complexity is necessary? Why not simplify by showing only the most relevant models and say in the text what is the impact of changing the rheological parameters?

As I mentioned above, I think that figures 5cd, 6cd, and 7cd are not necessary. Fig. 7 should be moved later into the dynamic topography section.

For the stress comparisons, I mention below that Fig. 8a is not necessary. Also Figures 10 and 11 could probably be combined.

Figures 7ab and 12cd both show dynamic topography for models TM1 and TM2, the only difference being that the effect of seafloor age is removed from 12cd. I think that it is appropriate to only use Figs. 12cd and omit 7ab, because the models 12cd are compared to the “observation” models 12ab.

Figure 13 – I am a little unsure how the amplitudes of spherical harmonic degrees are compared for only parts of the globe (continents or oceans). The spherical harmonics are globally-defined, so does it make sense to compare the spherical harmonic components over the oceans-only or continents-only? How is the rest of the world considered when the spherical harmonic components of the rest of the world are defined? I wonder if it might be better to just do the comparison for the whole globe and avoid this problem?

Figure 14 – perhaps could be added as an alternative predictive model to Fig. 12?

Specific points:

Title – I think the word “the” should be added before “lithospheric stress field”

Page 1 – the statements on lines 7-8 and 9-11 basically say the same thing – I think this sort of repetition is not needed in the abstract

Page 1, line 12 – stating that a correlation has a value of 0.51 doesn't mean too much to a reader – is this correlation good or poor?

P1, L13 – it is not the lithospheric stresses that are being improved, it is the model fit to them.

P1, L15 – the difference in angular misfits reported here (18.3 vs 19.9 deg) doesn't seem very significant – I'm not sure that it is useful to include this information

Abstract – generally, it would be good to close the abstract with an overall general statement about what the reader should take away from this study. Currently the abstract doesn't do this.

P2, L 27 – this paragraph starts “At longer wavelengths,” which seems to differentiate it from the previous paragraph. But both paragraphs are about long wavelengths. It would be better to distinguish this paragraph by stating out the onset that it deals with dynamic topography.

P3, L 27 – Here the Bird (2003) approach is described as using the fit to the observed plate motions – this needs a clearer explanation because it contradicts the statement at the onset of the paragraph stating that Bird (2003) was one of two approaches used to fit the stress field (not the plate velocity field).

P3, L 5 – The paragraph that starts on this line gives many details of the calculations and what was found by them, but these details don't seem necessary for the introduction, and they obscure the description of the main point of what the paper is trying to accomplish.

P6, L 5 – Why does the model have to be run for 0.5 Myr? It seems to me that the authors are basically doing an instantaneous flow calculation, and so advancing in time is not necessary. If the 5 kyr timesteps serve as a way to iterate on the consistency between the upper and lower models, then the authors should state this (and there would be no need to advance the model for any particular length of time).

P8,L24 – the authors call a correlation of 0.82 as "relatively low" but it doesn't seem too much different than the correlation of 0.85 which was viewed more favorably earlier.

Fig. 3a – there is no scalebar for this figure to show how colors relate to geoid height.

P10, L15 – many of the setup details in this section seem to be repeated from previous sections.

Figure 8a – I think this figure is unnecessary, as the data of the WSM have been published elsewhere and are repeated in the interpolation shown in Fig. 8b. Getting rid of Fig. 8a would allow Figures 8b and 8c to be larger, which make the figures easier to see and compare.

P 17 – Some of the discussion on this page about the comparison of the modeled and predicted stresses is more easily discussed in section 3.5.1, which refers to Fig. 9. I suggest to condense the second paragraph on page 9 and combine it into section 3.5.1, which would shorten this section on the overall comparison.

Figures 10 and 11 – Most of the information in these figures is already in Fig. 9, so perhaps these regional details are not necessary? I also think that the discussion of these figures could be shortened.

Fig. 13 – the background grid in this figure is 0.5 harmonic degrees, which is unnecessary. It would make more sense to use 1.0 degrees for the background grid (since half degrees are unphysical).

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