Solid Earth Discussions

Solid Earth Discuss., referee comment RC2
https://doi.org/10.5194/se-2021-151-RC2, 2022
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Comment on se-2021-151

Anonymous Referee #2

Referee comment on "Regional mantle viscosity constraints for North America reveal upper mantle strength differences across the continent" by Anthony Osei Tutu and Christopher Harig, Solid Earth Discuss., https://doi.org/10.5194/se-2021-151-RC2, 2022

In this manuscript, Osei Tutu and Harig present a series of calculations to constrain the local radial viscosity profiles beneath the North American (NA) continent, using localized free-air gravity kernels and GIA models for the inversion. Due to the strong tectonic contrast between the tectonically active western cordilleran regions and the eastern cratonic regions across the whole NA continent, the authors also zoom into the eastern and the western part of the continent separately using the same joint inversion technique. The inversion results over the whole NA continent show that the density-to-velocity scaling used (constant at 0.3 or depth-dependent following Simmons et al., 2010) has a strong influence on the resulting radial viscosity profiles. The focus on either the western continent plus Pacific margin or the eastern cratonic continent leads to more robust results. The western localized inversion shows a weak and shallow asthenosphere layer, while the eastern localized inversion shows a strong upper mantle and a weak transition zone. Beneath the western NA continent + Pacific margin, the results of the weak asthenosphere/upper mantle in this manuscript are different from the previous postglacial and mantle viscosity studies (e.g., James et al., 2000, 2009a, b) by ~10 orders of magnitude in light of the predicted uplift rate, as authors mention in the text. Beneath the eastern part of the NA continent, the authors attribute the strong upper mantle to the existence of the stiff cratonic root and the weak transition zone to the hot mantle material and volatiles/fluids associated with the underlying descending Farallon slab. Although this study seems systematic and robust, I have three significant concerns/suggestions.

First, after applying a windowing function to the global GRACE gravity data (Slepian function spectral localization technique presented by Wieczorek and Simons,

2005), the authors use two spherical harmonic filters from degree 2-10 and degree 2-15 respectively to perform the regional inversion using the localized geoid kernels. They found there is no significant difference of the results between the two filters in the regional mantle flow modeling below the NA continent. However, as agreed within the geodynamic community, the long-wavelength geoid (2-8 degrees) is dominated by broader mantle density anomalies including LLSVPs and global subducting slabs (e.g., Liu and Zhong, 2016; Ghosh et al., 2010; Mao and Zhong, 2021). Even if the GRACE gravity data is localized into the NA region using Slepian function, part of the local gravity/geoid in the long-wavelength window may still come from other broader-scale mantle density heterogeneities outside of the NA cap. Therefore, I think the author should filter the gravity signal over the NA continent into a shorter-wavelength window. Considering the scale of the NA continent, maybe spherical harmonic degree higher than 8 would be accepted (e.g., 8 – 20 or 8 - 25). The author can try different window in shorter wavelength. Current window starting from degree 2 and 3 is definitely in too long wavelength for the NA continent. The author could also try decomposing the spatial GRACE gravity field over the NA continent or Hudson Bay to find out the appropriate wavelength for the inversion. In addition, based on the findings in Wieczorek and Simons 2005 (Page 670), after windowing a global field by a taper of bandwidth L, the resultant coefficients are only reliable up to Lf – L (Lf is the maximun degree of the field data), and the first L coefficients exhibit large uncertainties. This study seems using a taper of bandwidth L =15 (from the caption of Fig. 1?), so only the GRACE data higher than spherical harmonic degree 15 can be robustly used for this kind of inversion. The authors should also explicitly denote in the manuscript on the bandwidth taper used in this study and perform the corresponding inversion consistent with that bandwidth. I strongly suggest that the authors perform the whole calculation again using the GRACE data in shorter-wavelengths, which can provide more robust constraint on the viscosity profiles below the NA continent. In addition, the variance reductions (in the longer-wavelengths considered here) of the results from only regional mantle modeling seems poorly fitting the observed GRACE gravity data (line #252 - #254).

Second, the NA plate includes a contrasting tectonic setting between the eastern and western half. The eastern cratonic root is reflected by strong high seismic velocity and the western asthenosphere is featured with strong slow seismic velocity. Thus, in the mantle flow modeling focusing on the North American plate, the lateral perturbations of the velocity-to-density scaling should be incorporated into the density/buoyancy field, as the authors also mention in the Discussion section. For example, Ghosh et al., 2013 use zero velocity-to-density scaling for the high velocity anomalies of the cratonic root. Forte et al., 2010 uses two different density-to-velocity scaling profiles for the cratonic root and the ambient upper mantle. The buoyancy of the cratonic keel is still in debate, but for the viscosity inversion, the authors at least should try a simple isopycnic hypothesis (Jordan, 1978) that assuming a neutrally-buoyant cratonic keel down to 250 or 300 km. In addition to the cratonic keel, the lateral perturbation of the velocity-to-density scaling for the slow seismic velocity below the western NA should also be considered. Below the western NA and the neighboring Pacific margin, significant fraction of the partial melt may be present, which explains the very slow seismic velocity in this region. Liu and King, 2022 tested the lateral perturbation of the velocity-to-density scaling in this local area associated with the

existence of the partial melt and found that this considerably influences the mantle flow patterns beneath the NA continent. They found that the velocity-to-density scaling for the slow seismic velocity in this area should be as small as 0.05 or below. If one only considers a constant velocity-to-density scaling of 0.3 or a 1D velocity-to-density scaling from Simmons et al., 2010 as did in this study, the buoyancy structures below the NA continent cannot be correctly represented. This substantial inaccuracy in the density structures may significantly bias the recovered viscosity profiles as the definition of the geoid kernels (eq. 4b in the manuscript). I think for this regional study specifically focusing on the NA continent, considering the lateral perturbations of the density-to-velocity scaling from the chemically-depleted cratonic keel below the eastern NA and the present of the partial melt in the western NA is crucial to correctly estimate the buoyancy/density structures in the mantle flow modeling. The seismic tomography model from French and Romanowicz, 2015 is used in this study, so the authors should be able to define the geometry of these two local tectonic areas based on the seismic velocity magnitudes in the tomography model. Then the velocity-to-density scaling for each area could be adjusted. I guess the resulting viscosity profiles from this new inversion may be improved to fit GRACE gravity data and RSL data better. The variance reductions of the results from only regional mantle modeling seems poorly fitting the observed GRACE gravity data (line #252 - #254).

Third, due to the strong lateral viscosity variations (LVVs) across the NA continent between the weak western asthenosphere and the strong eastern cratonic keel, the effects of the mode-coupling (for example, see Stewart, 1992) on the geoid/free-air gravity between different spherical harmonic wavelengths may be considerably strong below the NA continent. For example, in this scenario, the geoid/gravity signal in the shorter-wavelength may include a part coming from the contribution of the buoyancy structures in the long-wavelengths through mode-coupling. I noticed that the current inversion based on the geoid kernel (eq. 4b in the manuscript) can only constrain 1D radial viscosity profile. The 3D viscosity structures cannot be incorporated into this kind of inversion. It seems that the author does not mention how they calculate regional mantle flow. I think the author should add another paragraph in Method section to describe the technical detail of their mantle flow modeling, including initial condition set-up, boundary condition, solver (spectral or spatial?), etc. If it’s a code that can handle LVVs, such as CitcomCU, CitcomS, or ASPECT, I suggest the author run another numerical model incorporating LVVs (strong cratonic keel and weak western asthenosphere, temperature-dependence, etc.) for the whole NA region, and then compare the predicted geoid/gravity in the appropriate wavelengths corresponding to the NA continent (see my previous point) with the original model in the manuscript that does not include LVVs. If the authors use a spectral code (propagator matrix technique) that cannot handle LVVs, I suggest they should add a paragraph in the manuscript to discuss the potential bias caused by the mode-coupling due to the strong LVVs. Previous global mantle flow modeling has incorporated the LVVs from strong cratonic root, weak plate boundaries, and temperature-dependent viscosity (e.g., Zhong 2001; Becker 2006; Miller and Becker, 2012; Becker 2017).

Some specific minor points that would help make the manuscript clearer and better for the readers:

- The authors should clarify whether self-gravity is included in the geoid calculation. As Zhong et al., 2008 clearly show, self-gravity has a considerable influence on the long-wavelength geoid. Further considering the effects of mode-coupling in LVVs, the effects of self-gravity may be even larger than expected for the Earth. The geoid predicted from the mantle flow models with the effects of self-gravity is also not comparable with the geoid results without the effects of self-gravity.
- Non-hydrostatic free-air gravity. This is mentioned at line #222. I think the authors should make this point more explicitly expressed. How the hydrostatic flattening is removed? Following which paper? Nakiboglu, 1982 and Chambat et al., 2010 are two references commonly used in the community to perform the correction on the gravity date associated with hydrostatic flattening (e.g., Ghosh et al., 2013).
- Slepian functions. In Method section, although the authors describe the utilization and advantages of Slepian functions as a localization technique in spectral field, the authors should more explicitly describe which kinds of Slepian functions are used in this study around line #105, such as which degree the bandlimited space-concentrated tapers up to and how they are linked to the geoid kernels. In addition, in Fig. 1, what does functions 1, 2, 3, 4, 9 mean? These should be explained in detail as some new texts and equations.
- Letter symbols of subfigures. I notice that the authors put letter symbols for some subfigures but the letter symbols for other subfigures are missing, such as Fig. 2, Fig. 7, and Fig. 8. The letter symbols (a, b, c, d, ….) for each subfigure should be appended for the convenience of the readers.
- Be careful of the explanation of the abbreviation, such as RSL (Relative Sea Level?). The full name should be mentioned first with the abbreviation in the parathesis, and then the abbreviation can be used elsewhere.
- There are some misused phrases in the manuscript. Line #29: Fee-air ---> Free-air;
- Line #66: patial sphere ---> partial sphere;
- Line #193: basin specific multiple 1D Earth models ---> basic specific multiple 1D Earth models;
- Line #238: a relatively a weaker ---> a relatively weaker;
- Line #243 and elsewhere: strong viscosity values ---> strong layer or large viscosity values;
- Line #264: strong viscosity interface. Be careful of the word “interface”. Interface means “boundaries” instead of “layers”. Strong interface is a vague expression. You should either use strong layer or an interface from a strong layer to a weak layer. I notice this problem appears in many places (e.g., line #294, line #347, line #451).
- Line #330: “both” should be deleted?
- Line #333 - #334: why does the western upper mantle dominates? Note that there is also no cratonic keel in the western NA continent.
- Line # 358 – Line # 360: This sentence needs more explanations on how it is more in line with the findings by Hager and Richards, 1989.
- Line #388: serving a a first order ---> serving as a first order.
- Line #398: what are clear geophysical arguments? This place needs a couple of citations.
- Line #424: missing parenthesis for the citation.
- Line #441: conclusion ---> Conclusion

- Line #442: based, created: this is a problematic vague expression.
- Line #450: inversion the entire ---> inversion of the entire.
- Line #458 - #459: This sentence needs to be rearranged.
- Line #460 - #463: This long sentence is vague and needs to be explained in detail.
- A1: “Alpha indicates the eigenfunction number and rank”. Can you explain this definition clearer? What does the eigenfunction number and rank mean?
- Line #323: There is no Fig. 6d.
- In Reference section, the reference papers should be listed in alphabetic order.

Overall, I think this manuscript could potentially become an appropriate paper including significant contributions to the exploration of the lateral strength variations below the NA continent if the authors consider following my three major suggestions above to improve the robustness and quality of this study. The manuscript also needs more careful and clear writing/organization. Considering that ~8 weeks turnaround may be not enough for re-performing the calculations, I suggest the paper should be returned for major revision to the authors. If the authors can address my concerns above, they should be encouraged and welcomed to submit the substantially-revised manuscript back to SE. I will be happy to see the revised manuscript again.

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