Response to the comments of reviewer #2

Manuscript se-2021-98, Igor Ognev et al.

"Crustal structure of the Volgo-Uralian subcraton revealed by inverse and forward gravity modeling"

Dear Reviewer,

We express a sincere appreciation for our manuscript's critical analysis. Your comments certainly allowed us both to enhance the quality of our manuscript and present the results of our work more clearly. Please find below our responses to all your comments on a point-by-point basis. We use the blue color to distinguish our responses from your comments. The same blue color is used in the updated manuscript to show the corrected and added text.

Main questions and comments

1. In the two step inversion procedure, it is sometimes difficult to follow which parameters are inverted from which data and where the seismic constraints are used. The schematic workflow of figure 2 could be completed by some additional details (Moho depth z_{ref}, $\Delta\rho$ range, a priori information, inverted parameters, hyperparameters...).

Thank you for your comment. Yes, we agree that it is better to complement the schematic workflow with more input information. We did it in the current version of the manuscript.

2. l. 145: "In addition, topographically corrected GOCE...": What about the data on satellite height? Aren't they topogrpahically corrected as well?

Yes, they are also topographically corrected. We made the writing clearer.

3. l. 156-157: For the LAB, could the highest density contrast boundary significantly differ from the isothermal boundary and to which extent could the depth difference impact the results?

The highest density contrast boundary could also be linked to the compositional boundary in case we have a significantly different composition between the lithospheric mantle and the asthenosphere. This could happen for example due to mafic depletion or on the opposite enrichment of the lithospheric mantle relative to the asthenosphere. To take this effect into account another approach for modeling is required which would allow the lateral density variation of the lithospheric mantle.

We would still expect the compositional boundary to be linked with the temperature, and thermal expansion phenomena, but it might be a lower temperature than what we used. According to our thermal calculations, a 100 ºC degree temperature change would result in ca. 5 kg m^{-3} change of LAB density contrast. A much greater impact is caused by the

SPT density of the lithospheric mantle, which is ca. 10 kg m^{-3} of LAB density contrast change for the same change of SPT density. So, potentially if we have a lithospheric mantle relatively enriched with mafic components (e.g. with SPT density of 3370 kg m^{-3}), our LAB density contrast can be greater than 30 kg m^{-3} which would impact our model in the area of such a compositional change.

In any case, this requires a different modeling approach with laterally variable lithospheric densities which was out of the scope of the current study.

4. l. 178: Could you add any reference for the value of the thermal expansion coefficient?

Yes, a reference is added to (Artemieva, 2007, 2019).

5. l. 179: "Slightly modifying Eq. (1)": Could you explain a little bit more how you go from Eq. (1) to Eq. (2)?

Yes, thank you for pointing this out. We added the explanation to the manuscript: "As the temperature at the Moho boundary does not contribute to the thermal expansion of the asthenosphere, we can slightly modify Eq. (1) to get in situ density of the asthenosphere by taking asthenosphere temperature as equal to LAB temperature".

Also, two errors were corrected in Eq. (1):

1) Eq.(1) had a typo: it should have a plus sign instead of a minus in the last member.
2) The initial calculations were performed with a different thermal expansion coefficient for the upper mantle and the asthenosphere. In the updated version of the manuscript, the same thermal expansion coefficient was used for these layers. This gives a LAB density contrast of 5 kg m^{-3} instead of initially used 10 kg m^{-3}.

6. l. 220-222: Does one tectonic region correspond to a spatially constant Δρ?

Yes, it does. Now this point is specifically highlighted in the manuscript.

7. A few details on the inversion are missing and could helpful to the reader (eg. l. 230-231). What is the Δρ step? On what are the Δρ and depth ranges based? Could you provide any references? What is the grid spacing (see. point no 9)?

Yes, we added all this information in the manuscript at the end of the section 3.2.2.

8. I do not fully understand how the trade-off is made between the gravimetric inversion residuals and the fit to the seismic data (eg. l. 231, this is related to my point no 1). How is the Moho adjusted to the seismic data (eg. l. 253)? On which criteria? How do you deal with the uneven spatial distribution of the seismic data (eg. l. 277-278)? For this last question, I understand that you do not take it into account but that you are aware of this in the discussion of your results which seems fair to me.

1) The Moho was adjusted to fit the seismic data using two criteria:

A) When the Moho adjustment led to the enhancement of the gravity fit to both components of the gravitational potential (gravity and gravity gradient) or one of the components without significantly losing the fit to the other.

B) when the seismic data was showing consistently different Moho depths compared to the inverted Moho like on one of the digitized seismic profiles (see TATSEIS-2003) which had to be respected.

2) In our updated model we do take uneven data coverage into account by decreasing the resolution of the traced seismic profiles (TATSEIS-2003, URSEIS-95, ESRU, UWARS) from 10 to 40 km to make it comparable to the one of the USGS seismic catalog. In line with the addition of the complementary 4th tectonic unit (see Precaspian Basin at the end of section 3.2.2) in our gravity inversion, it led to a slightly different result in terms of the density contrast lateral distribution (Fig. 5). But overall the newly obtained inverted Moho model remains very close to the initial one.

9. l. 238 "extended by 2500 km": This value is to be compared to the extent of the study area and the size of the elements of the model.

Added.

10. l. 240-241: "triangulated polyhedrons in-between vertical cross-sections": I am not familiar with kind of "hybrid" modelling. Out of curiosity, what are the advantages?

The main advantage is a fast feedback loop. A user can go on each of the sections, modify the positions of the polyhedrons' vertexes and immediately see the change of gravity fit of measured and calculated anomalies on this section or the entire map. Another advantage of the software is its capability of displaying multiple additional data on your sections like seismic constraints on depth to Moho in the form of points or interpreted seismic sections as georeferenced images. You can then easily adjust your interfaces' geometry to better fit all the constraints.

11. Fig. 6-7: It seems that there is a long wavelength signal left in the residuals. Has a regional/long wavelength signal been removed from the data? If not, why not? I think that it could help but the relevence of such correction might be questionable at this scale. What do you think about this long wavelength signal and the (ir)relevance of a correction of the data before the inversion?

Such a long-wavelength signal in the residuals may be a result of a lateral density heterogeneity of the East European Platform (Artemieva, 2003). This lateral density heterogeneity of the EEP can be is linked to its compositional change. So it is probably a good idea to work with the data as it is, because introducing a correction for not-fully known compositional variation of the crust can be rather problematic.

12. l. 321: "also manifested a considerable misfit": I am confused with this statement while the fit was described as "acceptable" a few lines before.

We agree it may seem confusing. This sentence was rephrased to be clearer.

13. The discussion and conclusion mention that "the 3D forward gravity modeling revealed a considerable gravity misfit in the central part of the study area" (l. 408-409). However, I could not see this feature on the figures shown... until I saw the supplementaries. In think that figure S2 deserves to be moved to the main article as the maps show features that are widely discussed. You might combine fig. 10 and S2b if it is not too small to remain readable.

We modified the S2a by adding differences of Moho depth between the inverted and seismically obtained Moho, combined it with S2c, and added this figure in the manuscript as Fig. 9 along with respective discussion in section 4.2.

14. Fig. 9: I do not understand this figure with negative thicknesses. Are these variations?

To obtain this figure we performed isostatic calculations of mass imbalance from Eq. (5) and assumed that the areas of negative imbalance correspond to the high-density material in the lower crust. Then we found the thickness of this material by dividing the obtained mass imbalance by the density difference between the regular lower crust and the assumed high-density portion of it. It was 2900-3100=-200 kg m^{-3}. It was done to estimate the possible thickness of the underplated body assuming the studied region is isostatically compensated.

15. Fig. 10: The map would be more readable if the same information and colorscale was shown as background and in the triangles. If you showed the seismic Moho depth in the triangles, the reader would more easily see the areas where the color of the triangle significantly differs from the background color.

We tried this way of presenting the result as well (see figure below). In this way even when the obtained model differs from the seismic constraints, the colors of triangles and the map still remain very close on a color pallet. It makes it hard to notice the places of difference. We would prefer to keep our initial way of presenting the differences between seismic data and the obtained model as it seems that in this case, the differences are a little more conspicuous.

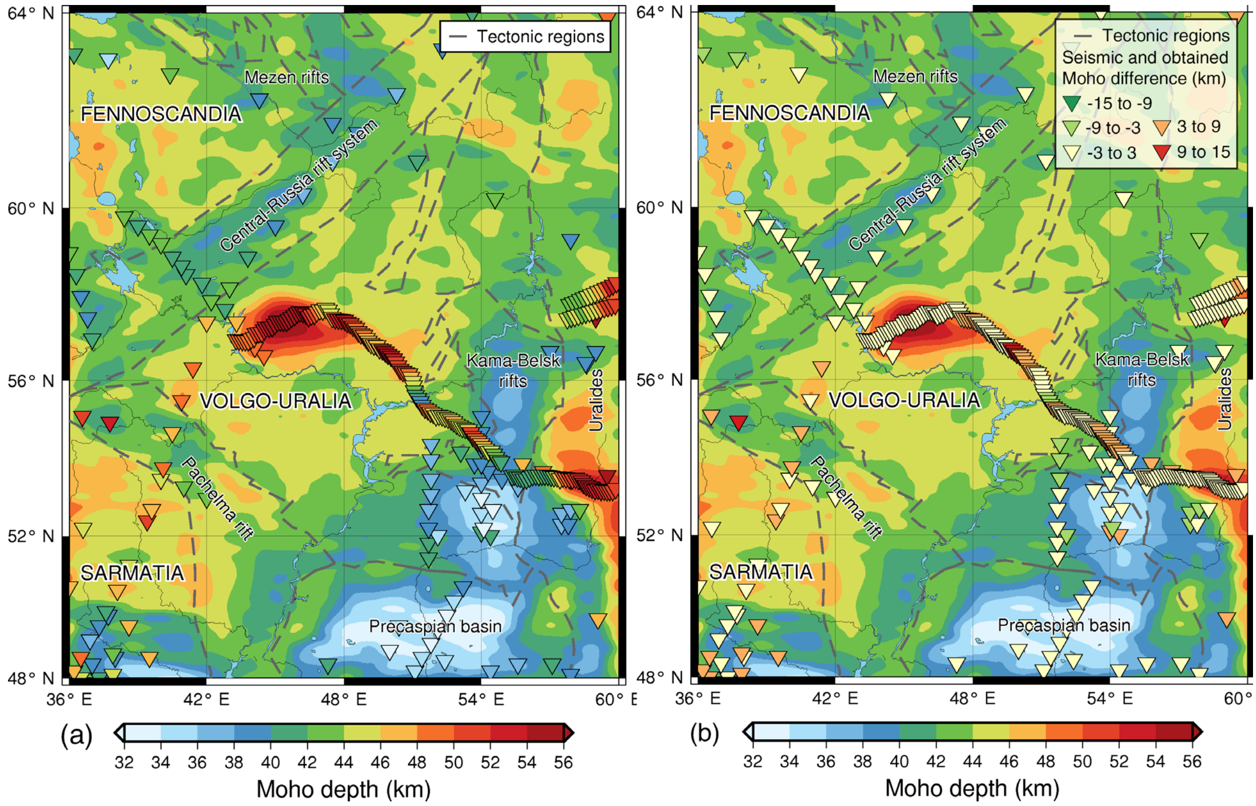

Fig. 1. Moho depth map with both seismic and modeled Moho depth presented by the same colorbar (a) and with differences between the modeled and seismic data using another colorbar (b).

Minor corrections

- l. 64: add comma before "at 1.8 Ga"`

Corrected

- l. 68-69: unexpected change of paragraph

Corrected

- l. 144: remove comma after "That is why"

Corrected

- l. 144-145: "on satellite height" -> Could you precise what is the satellite height ("ie. xx km of altitude") so that we can more easily compare to the other data?

Yes, we specified the height in the text as 225 km.

- l. 159: "The main petrophysical parameter": Why "main"? What other petrophysical parameter do you have in mind?

It could be porosity, degree of fracturing, or different fluids' saturation for the sedimentary section. Of course, all of these are expressed in density, so we think that the word "main" was used here appropriately.

- l. 165 and 208: "1/2 of the depth of sedimentary strata in km": not clear enough. What is the depth of the strata? Top, bottom, middle?

It is ½ of the bottom depth of the sedimentary cover. The corrected description says: "½ of the sedimentary cover bottom depth in km".

- l. 264-266: This sentence is quite complicated. Could you rephrase (split) it for more clarity?

Agreed. We split it.

- l. 279-280: "were assigned" makes me think the value is fixed beforehand while it is a result -> "resulted in"?

Yes, it's clearer to say "resulted in". Thank you.

- l. 299: "The main product": What are the other products?

Agreed. Changed to "The final product".

- l. 318 (caption of fig. 8): "6th later" -> "6th layer"

Corrected.

- l. 337: "fit of" -> "fit between"

Corrected.

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We thank the reviewer for the thorough analysis of the manuscript and hope that we successfully addressed all the arisen comments and questions.

With best regards on behalf of all the co-authors,

Igor Ognev.

References

Artemieva, I. M.: Lithospheric structure, composition, and thermal regime of the East European Craton: implications for the subsidence of the Russian platform, Earth and Planetary Science Letters, 213, 431–446, https://doi.org/10.1016/S0012-821X(03)00327-3, 2003.

Artemieva, I. M.: Dynamic topography of the East European craton: Shedding light upon lithospheric structure, composition and mantle dynamics, Global and Planetary Change, 58, 411–434, https://doi.org/10.1016/j.gloplacha.2007.02.013, 2007.

Artemieva, I. M.: Lithosphere structure in Europe from thermal isostasy, Earth-Science Reviews, 188, 454–468, https://doi.org/10.1016/j.earscirev.2018.11.004, 2019.