Solid Earth Discussions

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Comment on se-2021-98

Anonymous Referee #1

Referee comment on "Crustal structure of the Volgo–Uralian subcraton revealed by inverse and forward gravity modelling" by Igor Ognev et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-98-RC1, 2021

The reviewed manuscript is relatively well organized in general and is a very important contribution to our understanding of the regional-scale structure of the Volgo-Uralian subcraton. The methodological part and other parts of the manuscript are well written. However, I think that this manuscript requires revision before being accepted for publication. Below, my major comments on how the manuscript can be improved.

The "Tectonic setting" chapter should be extended by describing major evolutional steps of the large structural units. The Tatarian Arch must be mentioned since this structural element is characterized by the presence of large oil fields. Moreover, the Tatarian Arch is characterized by the presence of fluids within the crystalline rocks (e.g. Plotnikova, 2008; Plotnikova, I.N. New data on the present-day active fluid regime of fractured zones of the crystalline basement and sedimentary cover in the eastern part of Volga-Ural region. Int J Earth Sci (Geol Rundsch) 97, 1131–1142 (2008).) I would expect that these fluids should reduce the density of the upper crystalline crust in that region and possibly in other areas of the Volgo-Uralian subcraton. In this case, the influence of fluids should be discussed in the manuscript. Another important point is related to the presence of the numerous huge salt structures in the Precaspian Basin due to mobilization of the Permian salt. Salt has a lower density than the sedimentary cover and, therefore, this feature of the Precaspian Basin must be discussed in the manuscript even if these salt structures are not completely covered by the model. I would expect that, at the regional scale, the influence of the low-density salt in the south should be still recognizable within the model area as well. A more detailed description of the IGMAS model extension out of the main model area must be given in the text. I mean - the lateral extension in order to minimize the edge effect. It is written that model has been extended by 2500 km. However, there is no information on how this extension has been done. Did the authors consider the main tectonic features for the extended parts, especially, towards the south where deep sedimentary basins are present beneath the Caspian Sea?

I have a question - Why the density contrast is also 550 kg/m3 beneath the Precaspian Basin? Even all old rift structures are characterized by the density contrast of 400 kg/m3, whereas the Devonian-Permian Precaspian Basin has the same density contrast as the

Archean cratons.

Another important point is the lower crustal body according to the isostatic calculations in Figure 9. This map reflects the Moho depth in Figure 10: the deep Moho is reflected by the thick lower crustal body and vice versa – the shallow Moho is reflected by this lower crustal body. I would like to admit that it is not a "body" in Figure 9. It is a high-density lower crustal layer which is characterized by the presence of several lower crustal bodies in places where this layer thickens. Therefore, "body" must be replaced by "layer" in Figure 9 and within the respective text. The shape of the almost 17-km-thick solitary lower crustal body within the central part of the model area looks mysterious and must be discussed in more detail. There is a positive gravity anomaly over this body and the authors have mainly associated this anomaly with the lower crustal body. However, the shape of the anomaly (Fig. 6a) is more complex. I expect more discussion on the presence and the shape of this high-density lower crustal body. Is this body traced by the high-velocity body along the TATSEIS-2003 seismic profile? If there is no high-velocity body on the TATSEIS-2003 profile, the authors should explain why this body was not traced by the seismic data.

Figure 11 is a very important figure, showing a difference between the results of gravity modelling and the seismic data along the TATSEIS-2003 and URSEIS profiles. However, I do not understand why the difference is so big beneath the thick sedimentary rocks. The gravity signal is the integral one and requires a differentiation at depth during the modelling. On the other hand, the seismic data are usually considered as a more reliable source since the seismic signal can be much easier localized at depth. I propose to use the seismic Moho configuration from the TATSEIS-2003 profile and add a lower crustal body beneath the thick sediments in order to fit the measured and the modelled gravity data rather than to model the Moho uplift in that area. Otherwise, the authors should explain why they do not trust the seismic data within this part of the TATSEIS-2003 on one hand. On the other hand, they almost precisely retrace the seismic Moho depth at the beginning of this seismic profile in their gravity model. The authors have written that "within the TATSEIS profile seismic Moho has several steep troughs regarded as crustal roots (Artemieva and Thybo, 2013; Trofimov, 2006) which are not reflected in the satellite gravity field patterns. This case led us to a compromise solution: our Moho interface respects the main trends of Moho…". From my point of view, the compromise solution should be to use a smoothed Moho depth, as the authors have done in between the deep Moho beneath the underplating and the modelled Moho uplift beneath the sediments. There are no indications for so strong Moho uplift according to seismic data as it has been modelled by the authors beneath the thick sediments. Of course, the velocity model along the TATSEIS-2003 can be theoretically not the best one in that area. But, in this case, the authors should explain why they think that seismic data are incorrect there. Besides, the names of the tectonic units must be shown along the profile in Figure 11 in addition to names of the seismic profiles.

I propose to show an additional cross-section through the 3D density model from the north to the south to see the transition from the internal cratonic areas towards the marginal Precaspian Basin.

The boundaries of modelled area must be shown in Figure 1.

Precaspian Basin is the more common English name of the “Pericaspian” Basin that has been used by the authors. I propose to use the “Precaspian Basin” rather than the “Pericaspian Basin”.