**Response to the comments of reviewer #1**

**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 of our manuscript's critical analysis and your valuable comments which allowed us to enhance the quality and the clarity of our manuscript. 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.

**Major comments**

1.      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.

We extended the tectonic setting chapter with a description of major evolutional steps of the large sedimentary structural units with an emphasis on both North and South Tatar arches. We also added a figure in the supplementary material which is showing the location of the major Paleozoic tectonic structures of Volgo-Uralia.

The mentioned study of (Plotnikova, 2008) is indeed providing important insights on the fluids' circulation in the sedimentary cover and crystalline basement of the South Tatar arch and possible degassing of the basement. However, the measured density decrease of the oil deposits in the South Tatar arch is reported to be only ca. 2-5 kg m$^{-3}$ (fig. 12 in Plotnikova (2008)) which results in approximately 1-2 kg m$^{-3}$ of bulk rock density decrease within the oil-bearing part of the sedimentary section.

Such a small density change would not be reflected in the satellite gravity data or a large-scale global gravity model as XGM2019e. Even if we assume that such a density deficit can potentially be observed not only in the oil fields but also within several km of the crystalline basement section, it would still not be significant enough to be effectively represented in our lithospheric-scale model. Thus, even though the phenomenon of fluid circulation is important, it should be addressed in a specific local-scale study possibly with terrestrial gravity field measurements as constraints. At this point, it is out of the scope of the current study.

2.    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.

Agreed. We mention this feature of the Precaspian basin in section 3.2.2. And later-on distinguish the Precaspian as a separate tectonic unit with a unique density contrast.


3.    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?

We added information about the model's lateral extension as compared to its dimensions and how the extension was performed in the section 3.3.

We did not model the tectonic structures outside the area of interest. In our case, the extension has been done solely to minimize edge effects. Practically it means that all the layers' thicknesses were extended by 2500 km from the area's of interest edges (see figure below).
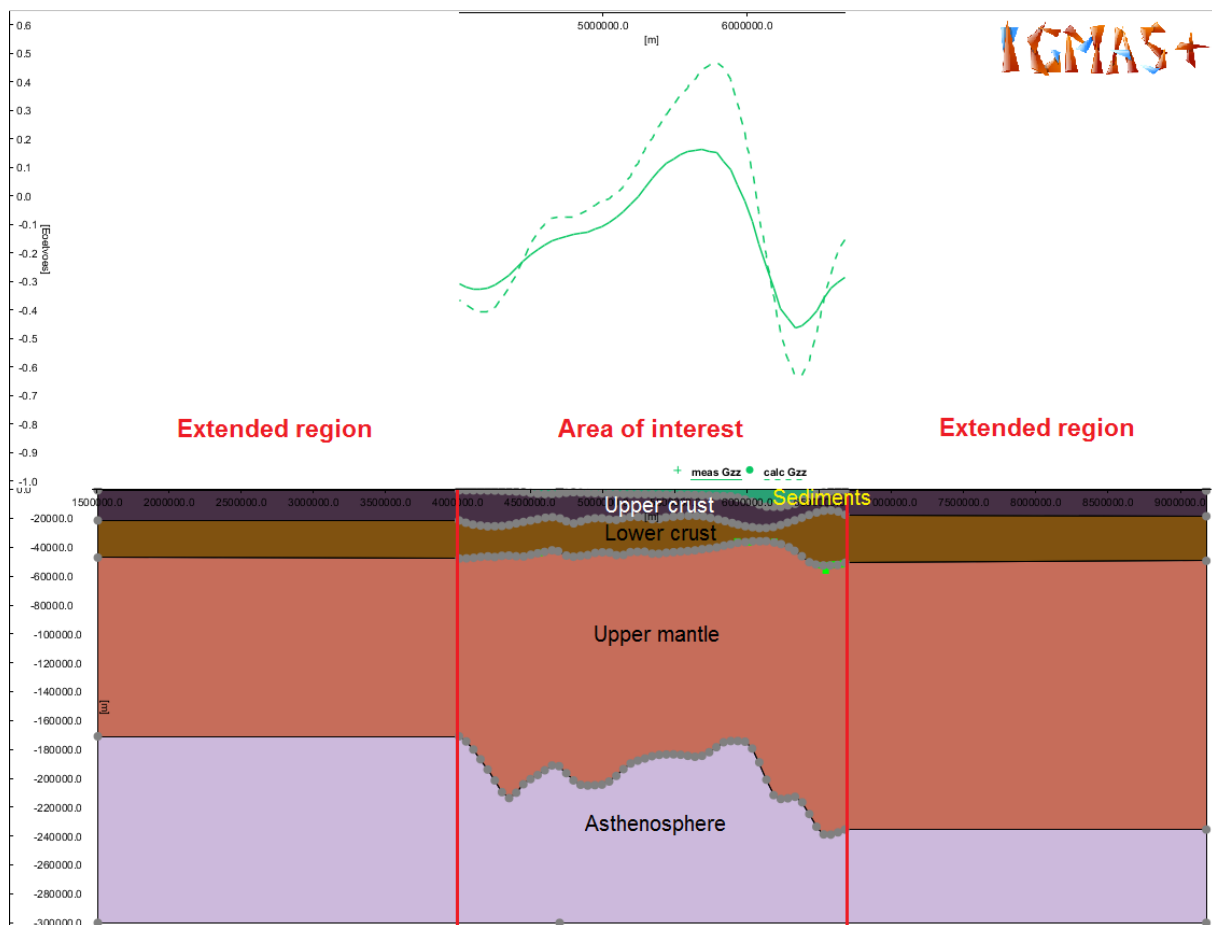


Fig. 1. Lateral extension in IGMAS+.

4.      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.

Thank you for a reasonable question. Initially, we did not distinguish the Precaspian basin as a separate tectonic unit and practically treated it as a part of Volgo-Uralia. If we do distinguish it, we will have a slightly different density contrasts' lateral distribution with the 350 kg m$^{-3}$ density contrast in the Precaspian basin, and 500 kg m$^{-3}$ in paleorifts. This approach gives us the inverted Moho depth which is 1-5 km shallower in the Precaspian basin and 0.5-1 km deeper in the paleorifts as compared to the initial result.

Even though overall it gives a very similar Moho model for the whole study area, still the newly obtained model is closer to the seismic constraints in the area of the Precaspian basin and the finally obtained model in IGMAS+. We think, that it would be fair to distinguish Precaspian as a separate additional tectonic unit and keep this gravity inverted model.

5.      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.

Agreed, changed.

6.      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.

Yes, the body that we are defining has revealed itself on the TATSEIS-2003 seismic profile as a relatively acoustically transparent region. Mints et al. (2010) interpreted it as high-density metamorphic rocks of mafic composition at the base of the so-called Vetluga synform. We added this information into section 4.2.

The shape of the body in our case is dictated by the observed gravity misfit that was compensated by the body with the corresponding shape. Potentially it could stretch a little further as a thinner layer for a better gravity fit, but we decided to keep it realtively isometric as there are no further seismic indications of this body apart from the TATSEIS-2003 profile.

7. 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.

We agree with the reviewer's concern about a certain discrepancy of seismic and modeled Moho beneath the thick sedimentary section and we appreciate the possible solution that the reviewer has proposed to this issue. Nevertheless, we must admit that such a discrepancy has arisen due to the kind of sedimentary model that we used.

During the review, it was found that our sedimentary model has approximately a 1-degree lateral North-East-to-South-West shift as compared to the EUNAseis original sedimentary model. Such a shift was a result of incorrect gridding at the initial stages of modeling. The updated model uses a correctly gridded EUNAseis sedimentary model. Eventually, we managed to keep the Moho depth as shown on the TATSEIS-2003 without having a significant gravity misfit in this area. Thus we did not need to add a high-density body in this region. See Fig. 12.

8. Besides, the names of the tectonic units must be shown along the profile in Figure 11 in addition to names of the seismic profiles.

Agreed, added.

9. 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.

Agreed, added as Fig.13.

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

Added.


11.    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".

Ok, thank you, we changed the "Pericaspian Basin" to the "Precaspian Basin" both in the figures and respective text.


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


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

Igor Ognev.

**References**

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Plotnikova, I. N.: New data on the present-day active fluid regime of fractured zones of crystalline basement and sedimentary cover in the eastern part of Volga-Ural region, Int J Earth Sci (Geol Rundsch), 97, 1131–1142, https://doi.org/10.1007/s00531-007-0274-z, 2008.