Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-113-AC1, 2018

Interactive comment on "Crustal Density Model of the Sea of Marmara: Geophysical Data Integration and 3D Gravity Modelling" *by* Ershad Gholamrezaie et al.

Ershad Gholamrezaie et al.

egholamrezaie@uni-potsdam.de

Received and published: 20 December 2018

Dear members of the Editorial Board,

We thank reviewer2 for his/her comments and would like to directly respond to his/her critical remarks. We agree that there is a mistake concerning the correct referencing of the Kende et al. paper and apologize for this.

The correct reference of course is:

Kende, J., Henry, P., Bayrakci, G., Özeren, M.S. and Grall, C.: Moho depth and crustal thinning in the Marmara Sea region from gravity data inversion, Journal of Geophysical

Research: Solid Earth, 122(2), 1381–1401, doi: 10.1002/2015JB012735, 2017.

We also thank reviewer2 for pointing us to the discrepancy between different gravity data sets. We however strongly disagree with the reasons reviewer2 gives for rejecting the paper. In the following, we address the different concerns of reviewer2.

(1) The first major concern of reviewer2 is that we have used the wrong gravity data set. In particular, the reviewer mentions a higher resolved data set introduced in the Kende et al. (2017) paper. Here we have to state that we have used the publicly available data set EIGEN-6C4 (Förste et al., 2014) because it covers the onshore and offshore parts of the study area. The higher resolved dataset the reviewer recommended to use instead and presented in Kende et al. (2017) is not publicly available. We have managed to access the mentioned satellite data shown in their Figure 2 (Sandwell et al., 2014; http://topex.ucsd.edu/WWW_html/mar_grav.html), but not the shipboard data for the offshore parts. Anyway, the "improved" data set of Kende et al. (2017) differs only by max a few mGals in the offshore domain from the satellite data (Sandwell et al., 2014), but has the same general characteristics. So the problem raised by the reviewer relates to the difference in two different versions of satellite gravity data published in the same year by two different well-known and experienced teams.

As the scope of our paper is not to quality check satellite gravity data, but to make use of it together with other observations, we decided for the EIGEN-6C4 free-air gravity anomaly data set.

Nevertheless, we thank the reviewer for pointing out this discrepancy between the different gravity data sets and we carefully have carried out a sensitivity analysis to test which difference we obtain between our model adjusted for the EIGEN-6C4 and a model adjusted to the data set of Sandwell et al. (2014). The results of this sensitivity analysis are enclosed to this response as a supplement file (Figure. 1). Accordingly, the two different satellite gravity data sets are almost identical over the onshore domains where the absolute difference is less than 10 mGal. Offshore, a notable difference of 30 to 50 mGal is evident along the bending segments of the Main Marmara Fault, the regions below which we have modelled the high-density bodies in the lower crust. It is clear that the difference between the two data sets is smaller than the gravity response of the high-density bodies. Thus we can confirm that the high-density bodies are still required, though the alternative data sets (Sandwell et al., 2014 and the one including the shipboard gravity of Kende et al., 2017) would require the high-density bodies to be slightly smaller in size/or density (non-uniqueness of gravity). The quantitative comparison enclosed shows that the top of high-density bodies would be 5 km deeper for a model consistent with the data set of Sandwell et al. (2014) than for our best-fit model consistent with the data set of Förste et al. (2014). The fit with the data set of Sandwell et al. (2014) could be even further improved by considering a density of 2990 Kg/m3 for the high-density bodies. We suggest to include this comparison in the paper in the revised version and thus document the related uncertainties. In addition, we could propose which additional (seismic) data could help to discriminate between these endmember configurations derived mainly from gravity.

Apart from the two different data sets, there are consistent findings in our study and the study of Kende et al. (2017). In particular, the latter also shows the need for deep compensation of the sedimentary fill, however, the authors propose to solve the problem with an uplift of the Moho in the domains of our lower crustal high-density bodies. In detail, they propose local shallowing of the Moho – and therewith also high-density bodies that are 5 km thick with a density of 3330 kg/m3 (compared to up to 18 km of density 3050 kg/m3 in our model) –, assuming a laterally uniform density of the crystalline crust. This is confirming our results rather than discarding them.

Seismological data used for model construction (Becel et al.,2009; Hergert et al., 2011) indicate that no such pronounced Moho uplift is present in the domains of our high-density bodies, a point also admitted by Kende et al. (2017). They critically review this misfit with their model and mention uncertainties in the seismic data as possible reasons for the misfit. However, if these uncertainties in the seismological constraints

are small, the derived Moho uplift may not be there and the crystalline crust may not be as uniform as suggested by Kende et al.'s gravity modelling.

Moreover, the limited available seismological observations (Becel et al., 2009; Karabulut et al., 2013; Bayrakci et al., 2013; Yamamoto et al., 2017) indicate that seismic velocities vary within the crystalline crust, in particular, an increase in seismic velocities (Bayrakci et al., 2013) is found for the uppermost part of the high-density bodies modelled in our study. Yamamoto et al. (2017) present results from a tomography study indicating a zone of higher seismic velocities in the areas where the modeled western high-density body cuts the interface between upper and lower crystalline crust.

Finally, the locations of the lower crustal high-density bodies also correlate spatially with a positive magnetic anomaly (Ates et al. 2003) which indicates that some mafic lithology is present below the non-magnetic sediments. Thus, assuming a uniform density and a +/- constant thickness of the upper and lower crystalline crust separated by an interface running parallel to the Moho is hard to justify.

(2) The other main point of the reviewer is that we do not consider the right bathymetry, in particular, the one presented in Kende et al. (2017) or in more detail in Le Pichon et al. (2001). Here we would like to clarify that this is a question of the horizontal resolution of our model. As we analyze a lithosphere-scale model, its horizontal resolution cannot resolve small-scale details. Nevertheless, we consider the main characteristics of the bathymetry, in particular, the location of maximum seafloor depth, only the steepness of the present-day basin margins is not resolved to the same level of accuracy. More specifically the reviewer claims that not properly considering the steep slopes resolved in the high-resolution bathymetry may question our general results concerning the deep crustal structure. This is simply impossible because such differences would result in a few mGals maximum difference with very local and short wavelength characteristics in the gravity response, but would not question the presence or absence of deep bodies causing a response of at least several tens of mGals and tens of km in wavelength. This can be demonstrated easily with a comparison of the grav-

ity response of respective models. We therefore clearly disagree with the reviewer's judgement concerning this point.

In summary, considering the higher resolved bathymetry and the higher resolved gravity data may help in defining sharper boundaries of the high-density bodies but would not question their presence and therefore our main results.

(3) The reviewer also asks for more discussion of the geological implications of our results. We indeed have been rather reluctant here as we wanted to avoid going too far with the interpretation, but could easily provide a few lines of thoughts here. We can add discussion on the consequences of the different interpretations for the deep structure of the Marmara Sea against the background of previously proposed concepts. This would touch hypotheses for the deformation mechanism that created the Marmara Sea and for the present day distribution of strength in the crust.

We hope these arguments will convince the editors that the work is worth publishing and will contribute to increasing our understanding of continental transform faults.

Best regards,

Ershad Gholamrezaie

On behalf of all co-authors

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2018-113/se-2018-113-AC1-supplement.pdf

---

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2018-113, 2018.