Comments to RC2: 'Major concerns regarding the gravimetric and bathymetric datasets used in the study cast doubt on the results', Anonymous Referee #2.

We thank reviewer #2 for helpful comments and have earlier directly responded in the open discussion. For completeness we list the main points to the reviewer's comments again here.

The paper addresses the question of the deep crustal structure of the submerged section of the North-Anatolian Fault within the Sea of Marmara, which may have important implications to better assess the earthquake hazard in the highly populated (> 15 Millions inhabitants) Istanbul area. A new crustal-scale 3D density model integrating geological and seismological data is presented, based on additional 3D-gravity modelling. The major result is that the crust appear to be crosscut by two large, dome-shaped mafic high-density bodies (average density of 3050 kg.m-3) of considerable thickness above a rather uniform lithospheric mantle (3300 kg.m-3). It is to be noted here that the location of these two bodies coincides with the location of two major escarpments: below the Tekirdag and the Cinarçik escarpments, respectively (Figure 9c). As a conclusion, the authors then suggest that these high-density bodies control the rheological behaviour along the NAFZ, and consequently, influences fault segmentation and propagation dynamics. The paper is well presented and well written. However, there are major concerns regarding the dataset, both for gravimetry and for bathymetry from the offshore domain in Sea of Marmara.

1) For gravity, the authors use the EIGEN-6C4 dataset (Förste et al., 2014), which is a combined global gravity field model up to degree and order 2190 correlating satellite observations (LAGEOS, GRACE, GOCE) and surface data (DTU 2'x2' global gravity anomaly grid). At the scale and wavelengths concerned by the present study: 1) the DTU 2'x2' global gravity anomaly grid, based on satellite altimetry, is predominant and 2) the density contrast is at the sea-bottom interface is of critical importance. It is highly regrettable that no discussion is presented to compare the free-air gravity anomaly from ship-board gravimeters and the satellite derived gravity data used in the present paper for the offshore domain. In Figure 2 of Kende et al (note missing reference: J. Geophys. Res. Solid Earth, 122, 1381–1401, doi:10.1002/ 2015JB012735), the differences between the two datasets are shown along a 130 km long profile, oriented along the strike of the main fault, following the deeper parts of the Sea of Marmara. This profile represents the most favourable configuration for using gravity from radar altimetry. Still, there are major differences. The N-S profiles (B-B' and C-C') shown in Figures 7 and 8 of the present submission represent the worst configuration for satellite altimetry-derived gravity, as they cross sharp escarpments bordering the Tekirdag and Cinarçik basins, which are expected to produce important effects on the gravity signature. A comparison between satellite gravity and ship-board gravity must be presented and the effects related to the use of altimetry-derived gravimetry must be discussed.

(1) The first major concern of reviewer #2 is that we have used the wrong gravity data set. In particular, the reviewer mentions the higher resolved data set introduced in the Kende et al. 2017 paper. We would like to repeat that it was not the scope of this paper to quality check published and downloadable gravity data, as these went through a review process before publication. We wanted to explore, what additional understanding can be gained if such data is integrated with previous models, other geophysical data and forward 3D gravity modelling. We agree that there is a mistake concerning the correct referencing of the Kende et al., 2017 paper and apologize for this. The reference was corrected and substantial discussion has been added to MS with respect to both the gravity data set as well as the bathymetry presented by Kende et al. (2017).

For the revised version of this paper we have tested the sensitivity of our results with respect to both data sets: Förste et al. (2014) and Kende et al. (2017). We had used the publicly available data set EIGEN-6C4 (Förste et al., 2014) in the initial submission 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) was not publicly available for the initial submission. Fortunately, thanks to support from reviewer #3 P. Henry, the Kende at al. (20179 datasets was made available to us and we could extend our analysis beyond our initial reply to reviewer #2 in the open discussion.

We explored the gravity response of different model configurations with respect to both data sets and present 3 “best fit” endmember models in the revised manuscript that illustrate the sensitivity of the results. In addition, we supply further details in the Supplementary Information.

Nevertheless, we thank the reviewer for pointing out this discrepancy between the different gravity data sets and we carefully have checked which differences we obtain between our model adjusted for the EIGEN-6C4, a model adjusted to the dataset of Sandwell et al. (2014) or a model adjusted for the data set of Kende etal. (2017). We can confirm that the high-density bodies are still required, though fitting the different gravity datasets would require the high-density bodies to be slightly smaller in size or density (non-uniqueness of gravity). We have included a detailed comparison in the paper in the revised version and thus document the related uncertainties. Nevertheless, there are consistent findings in our study and the study of Kende et al. (2017). In particular, the latter also show the need for deep compensation of the sedimentary fill, however, the authors propose to achieve this implementing as 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 3330kg.m^{-3}, compared to +15 km of density 3000 kg.m^{-3} in our initial model, assuming a laterally uniform density of the crystalline crust. This is supporting our results rather than discarding them. We have added a quantitative comparison in the new manuscript in this respect (page 12: Sec 4.2.3. Best-fit models, and page 16: Sec 5.3. Comparison with published 3D density model).

Seismological data used for model construction (e.g. Becel et al., 2009) 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 results. Moreover, the limited available seismological observations (Becel et al., 2009; Karabulut et al., 2013; Bayrakci et al., 2013) indicate that seismic velocities vary within the crystalline crust. In particular, an increase in seismic velocities is found in the regions where the uppermost part of the high-density bodies modelled in our study are located (New Fig.4 and Fig. 10)

Finally, the locations of the lower crustal high-density bodies also correlate spatially with a positive magnetic anomaly (Ates et al., 1999;2003; 2008), also suggested to consult by reviewer #3. This 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 difficult to justify.

In summary all evaluated gravity data sets require the presence of local bodies of higher than average crustal density in the deeper crust. If these are large and characterized by a smaller density contrast to the surrounding crystalline crust or smaller and of higher density remains unclear. Here, additional deep seismic data would help to reduce non-uniqueness.

2) For topography-bathymetry (shown figure 1c), the authors use a dataset exported from 1 Arc-Minute Global Relief Model (Amante and Eakins, 2009), which integrates the 30 arc-second grid obtained from NASA’s Shuttle Radar Topography Mission (SRTM) and a bathymetry dataset from the MediMap Group, 2008. Bathymetric grids from the Medimap group have a 1 km grid-node spacing. Compared to high-resolution grids based on shipboard, multibeam echsounders (e.g. [Le Pichon et al., 2001]), such grids are expected to smooth considerably the bathymetry, when sharp escarpments are present, particularly at the Western Tekirdag and the Northern Cinarcik escarpments. A smoothen bathymetry at escarpments may induce unwanted effects in gravity modelling, by introducing artificially the need of compensating high density bodies at depth. The concerns listed above on both the gravimetry and the bathymetry datasets, cast serious doubts on the reality of the two high density bodies found by the authors. Besides these two major issues, a geological discussion on the implications of the results is cruelly missing (gravity model solutions are not unique; geological criteria represent the best guides for discussing non-unique solutions). In conclusion, for the above reasons, I do not recommend publication of the submitted paper in Solid Earth Discussions. A substantial effort is needed: 1) for testing the relevance of the gravity model they use in the case of the Sea of Marmara (particularly due to the presence of sharp escarpments) 2) for testing the relevance of

the bathymetric grid 3) for presenting an in-depth, geological discussion for discriminating the different (non-unique) results.

(2) This other main point of the reviewer is that we do not consider the right bathymetry, in particular the one presented in Kende et al. (20179 or in more detail in Le Pichon et al. (2001). Though we are sure that this would not be of primary importance, given the horizontal resolution of our lithosphere-scale model we have implemented this bathymetry into the revised models. Again we would like to acknowledge the generous supply of this data set by reviewer #3, P. Henry. The differences with respect to the initial model related to this modification were indeed in the range of a few mGals and thus do 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. Accordingly, considering the higher resolved bathymetry and the higher resolved gravity data has helped in defining sharper boundaries of the high density bodies but their presence was still required.

The reviewer also asks for more discussion of the geological implications of our results, which was also suggested by the other reviewers. We have therefore added discussion on the consequences of the different interpretations for the deep structure of the Marmara Sea against the background of previously proposed concepts (page 13-16). This indeed has sharpened the respective parts of the MS with respect to hypotheses for the deformation mechanism that created the Marmara Sea and for the present day distribution of strength in the crust.