

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

**Götze**

hajo@geophysik.uni-kiel.de

Received and published: 8 January 2019

First of all I would like to thank the authors for their stimulating manuscript about 3D constrained gravity modelling in/around the Marmara Sea, as well as for the comments of the two reviewers. It is important to note first of all that the 3D modelling was done in a group with a high international standing in geophysical modelling of static as well as dynamic and kinematic processes. The same applies to their competence in geological interpretations of complex areas.

Their paper describes the results, the data processing, constraining information used and the 3D modelling process in this endangered area very competently and precisely.

C1

The software tools used are based on proven and recognized numerical methods and procedures. The 3D lithospheric density model is constrained by seismic tomography and geological findings. Gravity data are based on the Förste et al. (2014) gravity model EIGEN6C4 and the 1 arc. min. global relief model. These are data sets which were extremely helpful in the modelling and interpretation of static and dynamic Earth problems - as in this case.

Against this background and a careful study of Gholamrezaie et al's manuscript, which is up for discussion, the conclusion of Reviewer 2 seems a bit overdone to me. I even find it quite unfair, as the reviewers' criticism is not really based on numerical findings made with the data sets under discussion, but on "vague visual impressions". This becomes clear in Gholamrezaie's reply, where they pointed out that differences between the two data sets in question (EIGEN 6C4 and Kende et al. 2017 data) leave no general doubt as to the validity of the published interpretation. There are two other observations concerning the "Kende et al. 2017" gravity data set and the EIGEN 6C4: (1) the reviewer 2 mentioned a data set which is not fully available to the public and (2) After the study of the Kende et al. publication several aspects remain unanswered regarding the achieved accuracy/uncertainty in data processing (topographic correction) and 3D modelling.

In my opinion the manuscript should be published soon, since it contains concepts which contain valuable hints for future work in further works (3D modelling). A rejection, as demanded by Reviewer 2, is not really justified - and my evaluation corresponds rather to the vote of Reviewer 1.

Finally, I would like to conclude a very personal remark based on my long experience as an author and reviewer of international journal articles. As a reviewer, I have always been careful not to "rewrite" the manuscript available for the review: the decisive factors for the quality of a manuscript are the handling of the data/information, the actuality of the data processing and numerical interpretation as well as its evaluation. In this sense the manuscript in question can be accepted - in this case with references to the

C2

otherwise still available data sets and their quality.

Hans-Jürgen Götze, Institut für Geowissenschaften, CAU Kiel, Geophysik

---

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