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Interactive comment on "Comparison of the glacial isostatic adjustment behaviour in glacially induced fault models" *by* Rebekka Steffen et al.

Anonymous Referee #2

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Authors of the manuscript "Comparison of the glacial isostatic adjustment behaviour in glacially induced fault models" present a comparative study of two approaches to modeling of the glacial isostatic adjustment (GIA). As I've understood from the text, it is a continuation of debates between two groups of authors according reliability of methods they use.

In the introductory part (55, p.2) authors declare that the aim of his study is to compare two approaches (abbreviated as "WU" and "HA") based on benchmarking of two typical setups from the previous studies. However, authors also notes that they aim "... to verify (1) if the HA approach is suitable for GIA investigations and (2) if GIF results based on the HA approach are reliable in view of GIF activation due to GIA". This phrase might indirectly point on prejudice of the authors that won't pass for a comparative benchmarking study.

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Discussion paper

Main complaints of the authors concerning the HA models are: (i) the HA model does not take into account viscosity of the whole mantle using the dashpot approach instead, (ii) the HA models neglect an effect of the free surface and topography of the boundaries between internal layers caused by loading (as far as I understood from Eq.3 and Eq. 4) , and (iii) the bottom boundary condition (10, p.1). I'm totally agree with the authors that the using more realistic lithospheric structure and boundary conditions allow for more natural behaviour of the model. It is obvious and do not needs additional proofs. However, it makes numerical models unreasonably complex in some cases. That is why we use often such a simplified approaches like the WU and HA and both of them might be valid under certain conditions or not, and that is why I can not support the conclusion of the manuscript that the HA modeling approach is unappropriate.

In my opinion, comparison of two different approaches with the same model parameters like the mantle viscosity is unacceptable because of different limitations for both models. For instance, the WU model fits to observed data (Fig. 5) only for the particular viscosity structure but it does not mean that the used values of viscosity correspond to real ones. There are plenty of published radial viscosity profiles based on GIA studies and the geoid inversion. Using any of that within, say, WU or HA approach gives sufficiently different response on surface loading as well as including in the model such important factors as dependance of viscosity both on stress and temperature, changing of elastic thickness of the lithosphere under loading, compressibility, dynamic pressure caused by convection in the mantle due to inhomogeneous density structure, etc.

A conclusion that " we see that the HA models with and without dashpots show no difference" is also very strange to me. Let's consider an end-member example: a dashpot with infinite viscosity (fixed boundary). It must change the solution, otherwise calculations are wrong.

I realize, that the objective and impersonal benchmarking study that includes at least three different approaches is really important for further advances in the GIA modeling, but not a criticism of the particular modeling approach (HA) that I can see from the manuscript.

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