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Earth System Dynamics Discussions

Interactive comment on "Modelling sea-level fingerprints of glaciated regions with low mantle viscosity" *by* Alan Bartholet et al.

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Dear Gerrit,

The authors discuss the impact of low viscosity spots in the asthenosphere, associated with tectonic activity, on the sea-level predictions due to glacial changes between 2020 and 2100 CE. While the study is fairly well structured and written, I ended up with a number of concerns regarding their conclusions.

1. From the given figures, I got the impression that the impact of viscoelasticity is important only in regions of low viscosity coinciding with ice-mass loss, whereas the global sea-level fingerprints are merely affected. This aspect is from my point of view the most important result. The phrase "This comparison indicates that the error incurred

by ignoring the non-elastic response is generally less than 1 cm over the 21st century but can reach magnitudes of up to several 10s of centimetres in low viscosity areas" does not reflect this finding properly. Inside the manuscript the authors use stronger expressions.

2. The authors state, that higher resolved load distributions are demanded for reconstructing the spatial pattern of the sea-level fingerprints in the surrounding of such glacial changes. This is expectable due to the thinner lithosphere and viscous response considered in these regions. But a sensitivity study undermining this conclusion is missing. Futhermore, they smoothened their forcing using a Gaussian filter, so reducing lateral variability.

3. The importance of structural features of the asthenosphere are mentioned in the conclusions but not repeated in the abstract as a finding. The authors cite Austermann et al. 2013 and Klemann et al. 2007, but do not deliberate about whether earth structure or load distribution impacts the derived pattern more.

My suggestion is, the authors should discuss Point 2 in more detail, may be by presenting a proper sensitivity study with different details regarding the load distribution. A discussion of Point 3, would be great, but this might be bejond this study. But in this case they should relate to such a future extension. They should also compare their results to a 1D ve earth model in order to prove the reliability to compare their 3D results with those of an elastic model.

Regarding the setup of the manuscript, I must confess that I am not a native speaker. Nevertheless, I had the impressions, that some statements can be expressed more precisely and, at some places, the discussion can be sharpened.

Some details: The authors should care that all abbreviations are explained on first use and should care about the common use of hyphenation. Further things I placed in the annotated manuscript I attached as a supplement.

Best regards, Volker

Please also note the supplement to this comment:
https://esd.copernicus.org/preprints/esd-2020-72/esd-2020-72-RC1-supplement.pdf

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