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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.

Anonymous Referee #2

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Formal review of manuscript "Modelling sea-level fingerprints of glaciated regions with low mantle viscosity" by Alan Bartholet et al, published as open discussion paper in ESD.

The authors build a 3D viscoelastic model to study the effect of low-viscosity regions in glaciated areas on sea level fingerprints between present day and the end of the century.

They focus on two regions, namely Alaska-SW Canada and the southern Andes, and find that including viscoelastic relaxation in the upper mantle produces large near-field effects, in comparison with results obtained by modelling a purely elastic earth.

The paper is nicely written, clearly and thoroughly explained, and it addresses an issue

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Discussion paper

that, though well known to the GIA (and to part of the geodetic) community, has not yet been explicitly discussed. There has been a number of studies about viscoelastic effects of recent ice melt (mainly post-LIA) in several regions worldwide (Alaska, Patagonia, Antarctica, Iceland), but when it comes to sea level fingerprints and especially their use for sea level projections, most studies are still based on the elastic approximation, following the work published by Mitrovica and colleagues almost 20 years ago.

The authors show both the importance of local viscosity structures in the near field and the practical validity of the elastic approximation in the far filed, which are both very interesting and useful result.

Hence, I find this work very timely and I strongly recommend its publication in ESD.

Apart from a few minor comments listed below, I have only one somehow major concern.

As the authors explicitly discuss, one of the main differences with respect to an elastic model is the presence of side lobes (or peripheral budges) that move vertically in opposite direction with respect to the areas directly below the ice load. Even though those lobes can only be expected to appear next to the uplifting region, while comparing Figure 4b with Figure 2a I cannot help but noticing that the shape of the subsiding region very closely resembles the shape of the low-viscosity region. That makes me wonder how Figure 4 and 5 would look like, if the low-viscosity region were wider (which I think is a plausible assumption). I understand that the main purpose of this paper is a proof of concept, and the authors already discuss many limitations of their study. Nonetheless, I think that especially the results shown in Figure 5 are very specific and should better be supported by some minimal sensitivity study. A relatively simple option would be to model an end member, i.e., a 1D-model with the same vertical stratification as the center of the low-viscosity zone. That would provide a reasonable lower bound to the effect of local viscosity structures.

Minor comments:

l.11: my -> by (typo).

l.46: mantle -> shallow mantle.

l.53: could you add a short explanation of why plate subduction would change the viscosity structure? It might not be obvious to all readers.

l.62: the sentence is redundant, if layer thickness is already mentioned in the previous line.

l.86: is it 2100 CE or 2090 CE? The caption of Figure 1 seems to suggest the latter.

l.217-218: I wonder if it is realistic not to differentiate between asthenosphere and upper mantle. I understand that this is the model obtained in the reference paper, still it seems a little effort to introduce some reasonable discontinuity.

Figure 4 seems to have a wrong caption (the panel labels in the text do not match the actual panels in the figure).

Figure 5: in all panels, please add a thin horizontal line to indicate RSL=0.

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