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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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This paper compares the global and local impacts of 3D viscosity structure on the computation of RSL fingerprints for 21st century ice loss. The basic outline of the results in the low viscosity regions will be no surprise to anyone who has worked on regional GIA models within the low-viscosity regions, but I think that these impacts have not been broadly appreciated among all researchers in GIA. Also, this is the first study that I know of that has made a global quantitative comparison between 1D and 3D models for this purpose, so their evaluation of the worldwide impact (or lack of impact in this case) is new information. Overall, it is good to have a careful quantitative comparison, and I think this paper will have a valuable impact on the field.

The biggest question I have raised by the discussion in lines 235-247. When does the model computation start, and does it include in any way the viscoelastic effect of the 20th century ice loss? It is not explicitly stated, but it appears that the model run starts in 2010, and I suspect that the assumption is that there was no mass change before 2010 (again, this was not explicitly stated, but is implied). That is actually a substantial problem for the featured areas of Alaska and Patagonia, because the viscoelastic effects of the 20th century ice loss will be substantial. I don't think that this invalidates the comparison between the 1D and 3D models in general – if anything, the ongoing viscoelastic response to the 20th century changes will amplify the differences in the near field – and I don't think that this will impact their conclusion that the impacts of the low viscosity regions are essentially local. But it will complicate the discussion that centers around Figure 5. In particular, the position of the hinge line will be affected, so while the trends in the hinge line migration will be largely accurate (especially in the later time periods of the model when the pre-2010 changes are less important as they will be a few relaxation times in the past), any statements about the position of the hinge line may not be accurate without an adequate model for the pre-2010 changes. In particular, the statements about the changes in sign of the RSL trend at Sitka and to a lesser extent Vancouver, Victoria and Rio Grande may or may not be correct here – they depend on the magnitude and sign of the response to the 20th century loss.

I don't see any easy way to deal with this aside from re-running all of the 3D models (for the 1D models the viscosity is so high these changes hardly matter). They could incorporate information from previous modeling studies for Alaska and Patagonia, but those model predictions are only available for the present and thus only define the trend at 2010, not the response over the whole 2010-2100 interval). As a first order approximation it may be good enough to run the 2010-2100 model forward in time to 2210 (with no further changes after 2100), and then add a re-scaled and time-shifted version of this to approximate the 20th century effects. It may also be enough to avoid saying "transitions from a fall to a rise", and instead refer to more positive and more negative RSL rates.

Section 2.1, Ice Model. Can you add something about how this 21st century load differs from the 20th century changes? This should be feasible in the case of Alaska (e.g., Berthier et al., 2010, Nature Geosciences), and Patagonia, which should be sufficient examples. For the world as a whole, the total projected mass loss can be compared with existing global estimates from mountain glaciers. I think it would be adequate to give a percentage change for each region – there will be finer-scale spatial detail changes but they are not important here.

Minor points

Abstract, line 17. The differences 1 cm and 10s of cm don't mean very much without the context of the size of the signal, especially given that the ice load model is a future projection. I suggest at a minimum comparing these to the barystatic sea level rise (10.8 cm) for better context.

Line 39. Add "assumed to be" before "dominantly elastic". This assumption is fine in many cases, although not in the examples this paper focuses upon.

Line 52. Spelling correction: Freymueller

Line 62. This sentence largely repeats the previous one.

Lines 163-168. Same comment as on the Abstract above. In the case of the far-field regions, I wonder if "<1 cm" is the best way to describe it, as it looks on the figure like «1 cm might apply for large areas. I suggest computing a simple statistic like the median absolute deviation for the regions that they define as far-field in panel c, and reporting this quantitative measure.

Line 192. While "greater" and "less than" are mathematically correct, I suggest using the wording "more negative" and "more positive" instead. A similar wording is used a few sentences later. One reason to suggest this change is that it discourages a potential misinterpretation that a reader might make about the absolute value of the RSL change. (I suggest this because my own brain briefly interpreted "less than" as

implying "closer to zero" before I caught myself and realized that the signal in question was already negative.

Line 207. Delete the comma after "change"

Line 265. Spelling correction: Hu, not Huy

Line 274. Spelling correction: van der Wal

Line 279. Do you mean "viscoelastic" rather than "viscosity elastic"?

Lines 287-291. Clarify here whether you incorporated the 20th century changes in your models. My reading is that you did not.

Figure 1. It is really hard to see the colors on the thickness changes. I suggest using a much thinner black line for the coastline, and/or less detail applied for the coastline. Make sure that the resolution of the figures is good enough that readers can zoom in and see the thickness change projections.

Figure 2. The regions are a bit arbitrary here, but I think they are basically defensible. I infer from the text that this was a (former?) students' project and thus not likely to be easily tweaked, but I don't see any clear reason to have a region of high viscosity between the red and green regions in Figure 2a. This will only matter locally and does not impact the main conclusions, so I don't object if it is not easy to change this and it is left as it is.

Figure 5. Some of these curves will be substantially affected by the inclusion of the 20th century ice loss, while others will have only minor impacts. See comments above.

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