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The Cryosphere Discussions

Interactive comment on "Remapping of Greenland ice sheet surface mass balance anomalies for large ensemble sea-level change projections" *by* Heiko Goelzer et al.

Mario Krapp (Referee)

mariokrapp@gmail.com

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Review

This paper presents a method to correct for unphysical biases in the representation of the surface mass balance (SMB) in ice sheet model. By defining a remapping function for SMB anomalies for different drainage basins and height ranges, mismatches between ice sheet model geometry and climate model topography can be accounted for and thus leading to meaningful smooth and continuous SMB anomaly fields for diffferent geometries across basin divides. As a result of this approach the authors show that the SMB bias is reduced compared to commonly procedure of applying SMB anoma-

lies. The paper focuses on standalone simulations of the GrIS with no interactive ice sheet.

The paper is well written and has a clear message how to address the SMB biases for different ice sheet models, specificically for the Greenland ice sheet. The paper targets a specific audience (ISMIP6 modellers) but as part of a special issues that is understandable.

The paper in its present form has a few shortcomings which need to be addressed before I can recommend it for publication. Find my general comments (and a couple of technical ones) below:

General Comments

- This approach works for relatively high-resolution climate model output, i.e., MAR SMB data (an RCM), but what about coarse-resolution GCMs? What would you regard as (spatial resolution) limit to the approach? E.g., the minimum required number of drainage basins depends on how many samples per basin/height bin your SMB resolution would provide.
- ***Sensitivity on number of drainage basins***:
  What is the minimum number of drainage basins and/or height ranges that would give you an acceptable remapping? What is the overall sensititivy on resolution, i.e., number of basins? From figure 3, I can see that a at least a few different drainage basins could be grouped together, e.g., b3 & b4, b10-b13, b17 & 18, etc. I could see that fewer drainage basisn and fewer height ranges could give you similar I remapping error. To me, 25 drainage basins doesn't sound very "non-local" as is claimed in the abstract.
- ***Uncertainty estimation***:
  The function aSMB=f(hc) isn't well constrained for a few drainage basins. b6, b13, or b25, for example, show substantial variations in each height bin. I don't

think they can be resolved if more basins would be used (also because the mapping becomes more local, which is not the intention) and also not with fewer, unfortunately. So, I if those variations can't be resolved they then needed to be accounted for at least. In b25, the aSMB variations around the median amount to more than 2m/yr. That is substantial. I would like to see how this inherent uncertainty in the remapping function plays out for the future projections. You say on page 8 that these uncertainties are small compared to the uncertainties in the climate forcing. Could you give some figures how large thos uncertainties in the climate model forcing are (in SMB units) and then compare it to you errors due to the remapping? Furthermore, if the remapping errors are relatively small, this could warrant using even fewer drainage basins (as I have suggested before).

- Could you also add the total integrated aSMB to Figure 6?
- On page 10 you say that the rempapping doesn't work well if the modelled surface elevation is too different from the observed. How "close" to observation does the ice modelled ice-sheet elevation need to be?
- ***Sect 4.1 (section naming)***
  If the setup "should not be interpreted as a real projection" (p.13 l.3), then you should revise the section header, as "**Future sea-level change projections**" implies realistic projections.
- ***p.13 ll.12***
  I don't think further refinement would help here. First of all, as you said, if it's a flowline feature than, of course it is a highly localised feature and depends on the representation of the ice sheet model. If you would increase the number of basins, your remapping becomes local, and you need to drop your claim about the key feature being its "non-locality" (see abstract)
- ***Sect 4.2***

I'm slightly confused by what is presented here and I must admit that it is not easy to follow. A little more motivation at the beginning of the paragraph could help to clarify what the actual problem is.

  - I thought that the overall goal of the remapping is to provide a transfer function from (any) h to aSMB. To me this implies that the changing ice geometry h(t) is implicity accounted for, or am I missing something?
  - Also, why is the reader presented with two methods, here?
  - Adding to my confusio, I don't understand what ***Figure 11*** tells me.
  - This section doesn't really "flow" with the rest of the manuscript and needs to be revised substantially
  - The authors favour the "oflline" version of the remapping. Wouldn't the interactive method make more sense as it would nudge the ice sheet towards realistic margins and elevation changes and thus reduce the mode bias on the go? I think that any model bias that is not corrected for (instantaneously) would lead to a model drift and thus ever-increasing errors. So, in my opinion, this method should be the recommended one.

- ***P15 l15*** Which method is now the "proposed method"? Add the relevant reference to the Eq.
- ***Same page, ll19*** "(no ice sheet model is used)" Does this imply (as bofore in Sect. 4.1) that the sea level contributions aren't based on realistic projections? Pleas, clarify.
- ***Sect 5 Discussion and conclusions***

  - As far as I understand it, the remapping depends on the regional climate model used, i.e., MARv.9 forced by MIROC5. For the purpose of ISMIP6, different GCM will be selected for the future projections (https://www.

the-cryosphere-discuss.net/tc-2019-191/). How sensible is the remapping approach to a different configuration of RCM forced by a GCM? While getting the correct figures is difficult, this needs to be discussed at least.
  - How would the aSMB approach play out for “realistic” future projections.
  - The paper is highly specific for a targeted audience, i.e., ISMIP6 modellers. I see that the remapping approach can reduce model biases and it is a quick method to run offline corrections after a standalon ice sheet model run. I can foresee that this method can also be used in a way as to quantify those “unphysical” model biases (by looking behind the reduction of the model bias we see in the integrated SMB responses of each ice sheet model in Figure 12), This is obviously beyond the scope of this paper but can be discussed.

- What can the reader expect for realistic large-ensemble sea-level change projections?
- I would highly recommend to make any scripts or tools that have been used for this paper publicly available.
- The available data are incomplete as are the data availability statements! Therefore, I couldn’t review the associated data that have been produced along this paper. See the "data policy" section of TC (https://www.the-cryosphere.net/about/data_policy.html), for details

Technical Comments

- ***P1 L27***: It is not clear what “observed” is referring to?
- I wouldn’t use colors for the bar chart in ***Figure 12***, as they don’t add any information.

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