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The Cryosphere Discussions

Interactive comment on "GrSMBMIP: Intercomparison of the modelled 1980–2012 surface mass balance over the Greenland Ice sheet" *by* Xavier Fettweis et al.

Anonymous Referee #1

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The following is a review of "GrSMBMIP: Intercomparison of the modelled 1980-2012 surface mass balance over the Greenland Ice sheet" By X. Fettweis, et al.

The manuscript presented describes the Greenland Ice Sheet surface mass balance (SMB) model inter-comparison results for the historical period 1980-2012. The authors assess the ability of different types models (including regional climate models, radiation balance models, positive degree day models, and general circulation models), 13 in all, to estimate the surface mass balance over the Greenland continent. Skill criteria for the models are derived from observational datasets, including MODIS bare ice extent, ice cores/snow pits/ in-situ observations, and the calculation of regional SMB as the

difference between GRACE estimates of mass and previously-published ice discharge into the ocean. A large amount of effort is taken to design the experiment and compile model submissions that have the same source of forcing, the same spatial resolution, and cover the same overlapping time periods. Through this comparison, the authors derive an ensemble mean and standard deviation of Greenland SMB over the 1980-2012 period, as well as trends. The authors find the largest model discrepancies are along the ice sheet margins, and it is the increase in meltwater runoff along the margins that drive the prevailing negative trend in Greenland mass balance over the study period. Results suggest that regional climate models have strong skill in matching observed patterns of SMB, though computationally expensive compared to the positive degree day or radiation balance models. Overall, the authors find that it is the ensemble mean that best matches observations, meaning that errors from the various models balance each other out and do not convey any obvious systematic biases.

The work presented here is critical for cryosphere scientists, especially to the scientists interested in quantifying and simulating the evolution of ice sheets (including atmosphere/surface/ice sheet modelers). This is clearly a massive effort, and as observation of SMB in many areas are quite sparse, the authors have done a very nice job of compiling meaningful comparison criteria as a first attempt at this type of exercise. Such an effort is quite necessary to build a SMBMIP community and launch similar efforts in the future. The work presented here is especially a nice basis on which to build future comparison efforts that may focus on sea level projections. This is especially true considering the conclusion that the current compilation of models does not show systematic bias. For these reasons, publication of this work is timely and critical.

That being said, the manuscript in its current form needs a lot of work, especially the text which requires major revision. The tables and figures, in general, are adequate for conveying the discussion and conclusions of the manuscript. However, the model descriptions take up most of the text, and the rest is very concise. I think expanding upon the scientific results would make this manuscript much less of a technical paper

and much more appropriate for publication in The Cryosphere. Such improvements would also help broaden the audience for this paper. As is, the manuscript is difficult to digest by other cryosphere scientists, and the authors do not make it immediate clear to the reader why these impactful results may be of interest to their research.

Below, I outline my general comments to the authors:

Introduction – In general, the introduction should be expanded to discuss more clearly the topics of observed variability in SMB over the historical time period assessed and why it is important and/or difficult to capture them with models. In addition, an introduction to the types of models that are assessed should be given, since those reading this manuscript might not be familiar with how and why these types of models differ. This could be a good way to let the reader know about the general advantages and disadvantages of each model type also. Another helpful topic to cover would be a short discussion on why this effort is so important and what the authors are aiming to learn about model bias (i.e. why a historical assessment is helpful to complete before assessing projections from this group of models). This pertains to statements that are made in the conclusion section of the paper, especially those about implications on model coupling and about quantification of uncertainties in sea level rise projections. Introducing these concepts before mentioning them in the concluding remarks would help highlight their importance and future inter-comparison goals.

Additionally:

- The first paragraph of the introduction mentions glacial water storage, and notes that this is the first time it has been evaluated. However, the GS term is not included in Equation 1, and GS is not discussed explicitly anywhere else in the manuscript. Please be more specific here about how GS is evaluated, and how it is being included in this analysis.

- Line 70-71, This line might be better coming after Eq. 1.

- Line 78, Please specify the type of variability you refer to here

Model Section –

- Maybe it would be helpful if there was an overarching Methods section, since the Model section would really benefit from a short introduction describing what your methods in general are, and what you plan to do as an inter-comparison exercise. If the Model, Observations, and Evaluation all came under a larger section, it might be a good way to add some explanation prior to the reader going through all the details right away without understanding what type of inter-comparison is being presented.

- As it is, it is very difficult and quite boring for the reader to be introduced to a list of models and their descriptions up front with no introduction to them. Maybe a table of model names, types, native resolution, downscaling type, etc., could help serve as a reference/summary to this section. Such a table/figure might help the reader to have something to refer to while looking through the tables and the figures. Easier access to model type (by a table or color coding in the figures?) would help make the results easier to read.

- If at all possible, it also might be helpful to push this list deeper into the section – maybe with the observations or data described first? (Though this might be fixed by section summary I mention earlier).

- It is also important to note, that many model descriptions refer to RACMO within their write-ups, but no reference for RACMO, what it is, or what it stands for has been included prior to these sections.

GRACE estimation Section

- It could be helpful to include the equation of glacier mass balance here, so that it is clear to the reader how SMB is calculated from GRACE and ice discharge.

- These last two sentences can probably be simplified to just say that you are using the methods of King et al. (2018), but instead of RACMO, you use each of your different

SMB products. These sentences are awkward that way that they read currently.

Comparison with GRACE measurements Section – Results here are very interesting and there is plenty to point out to the reader. In general, I don't think there is any advantage to being extremely concise. It would be nice for you to lead the reader from figure to conclusion for some of the statements made in this section.

- The discussion about the seasonal cycle here is a nicely suggested by your presented results. Could you please add some more explanation in order to lead the reader a bit more on why the RMSE from your Supp. Fig. 2 would imply how the seasonal cycle is modeled?

- For discussing the GCM's, could you please be explicit about the difference between the forcing of variability on these models by ERA-Interim, and how it pertains to the RMSE?

Conclusion Section – Here, some of new concepts that were not brought up earlier in the manuscript are mentioned. This includes the mention of coupling with an ice sheet model (i.e. a topographic feedback scheme was not used, maybe add a reference to a paper that shows the feedback may be important) and quantification of uncertainty in climate projections. I think the manuscript would be improved if some space was taken in the discussion section to mention more of how results presented here do have implications for these other applications. Implications of interest could range from estimates of historic sea level contribution to forcing of ice sheet models for historic and future simulations, and the ability to now give those applications error bars. I would even say that bringing these applications up in the Introduction as justification for conducting this MIP could help improve the manuscript's impact.

Below, I offer some more specific comments/suggestions:

Page 2, line 62: Please rephrase, "of the same order as RCMs compared with observations and therefore remain useful tools. . ." or something similar

Page 3, line 98: maybe, "although each model prescribes the reanalysis forcing in different manner". Please refrain from referring to the forcing as data.

Page 3, line 99: "(EMBs)"

Page 11, lines 1-2: A reference to Fig. 1 would be helpful here

Page 11, line 336-337: "This allows. . ." Please rephrase this sentence. It is very awkward and difficult to understand.

Page 12, line 359: maybe, ". . .compared to the resulting mass balance estimates from the GRACE product".

Page 13, line 411: "with the GRACE-derived. . ."

Page 15, line 452: Instead of mainly, maybe "largely"?

Page 15, line 457: Not sure what you mean by "oscillates" in this context. Maybe "deviates from the mean"?

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