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Discussions

Interactive comment on "The effect of overshooting 1.5 °C global warming on the mass loss of the Greenland Ice Sheet" *by* Martin Rückamp et al.

Anonymous Referee #1

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— Summary —

The response of the Greenland ice sheet (GrIS) to a RCP2.6 global warming scenario is studied with an ice sheet model forced by a combination of climate models. The output from existing Global Coupled Climate Model (GCM) simulations is further processed with a surface energy balance model of intermediate complexity to generate surface mass balance and temperature forcing for the ice sheet model. While a feasible two-way coupling strategy between GCMs and ice sheet models remains unavailable, this study applies anomaly forcing and a number of corrections to estimate the future sea-level contribution from the GrIS.

The full potential of the high-resolution, higher-order ice sheet model is not realised due to a lack of important forcing mechanisms (ocean) and a rather crude climate forcing. This leaves the application of the surface energy balance model of intermediate complexity as the main novelty compared to state of the art projections. Nevertheless, this component has not been treated with sufficient detail and its output requires more analysis and a better comparison with observations. The description of the experimental setup and processing of the forcing data is not always easy to follow and also needs more precision. I therefore suggest major revisions along the lines of my comments given below.

— General comments —

The SMB forcing is clearly the most important ingredient for this type of projection, in particular since the study does not consider any oceanic forcing. Consequently, more effort has to go into understanding and discussing the SMB product resulting from a chain of different models and processes. What is missing entirely is a (spatially resolved) validation of the used SMB forcing compared to observations and other modelling results.

The modelling approach of using the intermediate complexity model SEMIC to calculate SMB based on GCM input for projections of the GrIS sea-level contribution is one of the new and interesting aspects of this study and should receive much more attention. SEMIC is treated in the description and analysis practically as a black-box element, but should instead have a much more prominent place. The key question this study should be in the position to answer is if and why SEMIC is an improvement to, or similarly suited as other methods that are used to produce SMB forcing based on GCM output. The current alternatives include e.g. regional climate models (which are hardly mentioned in the manuscript) and models based on the positive-degree-day method.

The authors rely on the parameter settings of the SEMIC model, which have been optimised for a different climate model input (Krapp et al., 2017). The Krapp et al.

study shows that the SEMIC model can well approximate the MAR SMB results given MAR climate input. It must however be expected that the parameters that were chosen for a completely different climate input (different model, RCM vs GCM) are not optimal. Unless evidence can be provided that the applied parameters are indeed suited for the GCM forcing used in the present study, the model parameters should be optimised. Discussion on differences to other results (e.g. as done compared to Fürst et al., 2015) hinges on the implied sensitivity of the SMB model, which is currently not possible to be judged.

Modelling decisions, in particular those concerning the chain of processing used to arrive at the SMB and temperature forcing have to be better explained and motivated. In the current manuscript, some of the modelling choices appear arbitrary and it is not clear if they are optimal, possible to improve or just used in absence of better options.

The organisation of the material in the manuscript is not optimal and could profit from a reorganisation. To name just a few examples, some aspects belonging to model setup and initialisation appear too late in the text, while some results first appear in the conclusions after they have already been discussed. The ice sheet model is introduced first (2.1), while it is the much less important component for the projection compared to the SMB forcing. See also specific comments below.

There may be a problem with the thermodynamic model used to spin up the temperature as presented in Table 2. I suggest to thoroughly check and verify that aspect of the modelling.

The manuscript is so far rather short and could easily accommodate additional material that would be required to respond to the issues raised above and below.

— Specific comments —

p1 l6 Not clear why a threshold of 1.5C is relevant when calculated regionally for Greenland. To start with, the global threshold of 1.5 is a political target and is not directly

related to a real threshold in the climate system. Locally, a 1.5 degree warming has no specific meaning at all. Over which area is the Greenland wide average calculated?

p1 l8 How is plausibility of the future forcing assessed? This has to be made clearer and the wording should be changed accordingly.

p1 l14 It is not well documented what the reason for the loss of floating ice tongues really is. In the absence of ocean forcing this should be explained by interaction with the SMB. Or are part these changes related to the unforced response of the ice sheet model?

p1 l14 What values? Greenland sea-level contribution? Elevation changes? Clarify

p1 l14 A lower bound of what? The actual future sea-level contribution of Greenland? The contribution under forcing scenario RCP2.6? I think you cannot make a meaningful statement about a lower bound based on the results of this study. There is a combination of missing important processes (ocean forcing) and uncertainties about the climate forcing (intrinsic and not properly studied) that make a quantitive statement very hard to justify.

p1 l19 Repeated "past decade". Compare also "past decades" in l21. More precision needed.

p1 l22 Remove "Obviously"

p2 l2 To asses *all* "the impacts of global warming of 1.5C ..." is a huge aim. Be more specific about the aims of this study in particular.

p2 l3 RCPs were not designed for a specific warming level. Reformulate.

p2 l5 "are not passing the limit". Which limit, be more precise.

p2 l6 Remove "potential". If the effect is return to below the threshold, it is an actual overshoot.

p2 l9 Repeated "response"

p2 l9 Maybe "GCM" is better than "atmospheric model" here.

P2 l10 Maybe "surface mass balance changes".

P2 l12 Replace "uncoupled" by "one-way coupled".

p2 l13 The causality in this sentence is not clear. What does higher-order physics have to do with corrections of atmospheric forcing?

p2 l16 "the low computation cost"

p2 l17 Why is high resolution a requirement for higher-order physics? Also, for this study, representing the SMB forcing accurately should be the most important aspect where computational resources should be directed to.

p2 l19 "anomalies *of*"

p2 l21 More precision needed to replace "obtain these anomalies from the GCM"

p2 l24 Consider describing the ice sheet model later since it is the least important component in this study.

p2 l25 I suggest a less technical description here, e.g. "Ice flow and thermodynamic evolution of the GrIS are approximated"

p2 l28 It is not the elements themselves (as in finite elements) that have these characteristics (SIA to FS). Reformulate. Which approximation is finally used?

p3 l1 The reader does not necessarily know what "the balance equations" refers to.

p3 l5 Better to describe how basal melt rates are calculated before saying that they are held constant during the experiment.

p3 l7 "Under grounded ice"

p3 l7 Melting is not *due to* frictional heating. Frictional heating and geothermal heat

flux warm the ice that may eventually melt. More precision needed.

p3 23 "shearing"

p3 26 Remove "fields".

p4 l1 Replace "or" by "and".

p4 l1 "All methods are suitable ...". I don't think this represents the conclusions of the study very well. There are clearly methods that are more suitable than others and a combination between different methods may be needed, is how I would put it.

p4 l5 What exactly is initialized over 50 years? Is the geometry relaxed? What constant temperature is used? Be more precise in your description. The aim should be to make the model setup reproducible for other modellers.

p4 l7 Why is the spinup done to 1960, and the reference period 1960-1990. Motivation needed.

p4 l7 "basal-friction inversion" requires some additional description and references to place what is meant here in the context of state of the art techniques. What is inverted for and by optimisation of what precisely?

p4 l9 "mesh refinements are made at certain points during the initialization ..."

p4 l10 Explain better the sequence of runs. Is the forcing over 125 kyr repeated several times? The number of years add up to 290 kyr, but the forcing is supposedly only for 125 kyr.

p4 l20 What precisely is taken, thickness and bedrock data? Removes "bed from". Add "data set" after "BedMachine Greenland"

p4 l21 This belongs to the description of basal-friction inversion that should be added in the section before.

p4 l23 Add "spatially constant" before "surface temperature anomaly". Describe better

what "based on" means. Supposedly the present day RACMO temperature is offset by a spatially constant temperature anomaly?

p5 l4-7 Reformulate this sentence, too long.

p5 l10 Motivate the choice of models. Why these three GCMs?

p5 l14 Specify the reference period against which the change is calculated.

p5 l19 Could give a more specific reference here, i.e. a specific IPCC chapter.

p6 l2 Why would polar amplification only have consequences in extreme years? Or does it have an impact on the amount of extreme years? Clarify.

p6 l2 Add reference to figure 2 at end of sentence.

p6 l3 Add "amplification" after similar.

p6 l4 Polar amplification is not the same as Greenland amplification. Consider and discuss the difference and similarities if any.

p6 l7 "A striking feature" in which model?

p6 l9 "lower bound" and "upper bound" is the wrong wording for this case. Use "the highest" and "the lowest forcing" or similar.

p6 l11 "might be different across the GrIS". Why "might", you have the data to check that and make an informed statement.

p6 l13 How does a model "best" represent overshooting. Either temperature overshoots or it doesn't. Reformulate.

p6 l15 Specify what you mean by "ice sheet specific quantities".

p6 l15 It would be useful to describe the SEMIC model in coarse lines here, since it is an important ingredient to the simulations. In my opinion it represents one of the interesting new aspects in the presented simulations. Based on this description you

should judge the advantages and shortcomings of this approach and compare it to other used methods like positive-degree-day models, RCMs and other intermediate complexity models (e.g. REMBO, Robinson et al., 2010).

p6 l18 As mentioned before, SEMIC has been tuned to reproduce MAR SMB given MAR climate forcing. It cannot be expected that the model tuning translates to another model like the GCMs used here. The ultimate test is if the SMB produced for the recent past compares well against observations. This should be shown for the three GCM models and eventually it requires returning of SEMIC for that purpose.

p6 l18 Not clear what the shortcomings of the Krapp method to treat albedo were and neither how this has been improved for the present study. This requires some additional description. Extending on the last comment, changes to the albedo scheme likely also have an impact on the SMB and would lead to different tuning even for the same climate model input.

p6 l24 Motivate why this two-step procedure is necessary.

p6 l28 Add "(.)" after "quantities".

p6 l30 In my understanding hsˆISSM-pd should be replaced by hsˆSEMIC-pd. Or are they both considered the same? Please clarify.

p6 l31 What (and when) exactly is the present-day surface elevation referred to here?

p7 l4 The following three paragraphs are only remotely related to the atmospheric forcing and would fit much better with 2.2 about the initial state of the ice sheet model.

p7 l5 This is confusing. Before ISSM is run forward in time, wouldn't it have exactly the geometry that you have prescribed? A good match with the observed geometry is therefore not a result. Reformulate?

p7 l7 Remove "perfect" before equilibrium.

p7 l10 Not clear why the models have to be "run on the same ice sheet mask". Clarify.

p7 l15 Replace "ice sheet models" by "initial states".

p7 l16 Shouldn't the imbalance be subtracted to counteract it? See also equation (3), which should have a minus sign before SMB_corr.

P7 l17 The SMB correction method has been used by other modellers before (nevertheless, it is not unproblematic), which calls for adding some references (e.g. Price et al. 2011, Goelzer et al., 2013). The magnitude of the required correction should be quantified (see references above for comparison) and the shortcomings of the method should be discussed.

P7 l17 It is not clear to me why SMB_corr is time dependent here. In my understanding, the most effective method should be to subtract the imbalance diagnosed for t=1 for each year of the forward experiments (unless an iterative procedure is used). What SMB_corr is used after the end of the relaxation run from 2060 onwards? Please explain this better.

p8 l3 "GCM" does not appear in the formula.

p8 l4 I thought RCP2.6 was only defined until 2100. Describe how it has been prolonged if that is what has been done here.

p8 l5 Maybe "albeit without a correction term"?

p8 l9 What does "bias corrected onto the [..] grid" mean exactly?

p8 l14 "respectively".

p9 l16 In my understanding h_fix should be the modelled present-day surface elevation, not the observed. This would result in corrections for the actually occurring elevation changes. Or are they (modelled and observed) identical?

p9 l25 These gradients were found as best fit to SMB simulated by a specific RCM (MAR) at different elevations. Applying these in your setup may be better than nothing, but for a consistent picture, these should ideally be recalculated based on your own

model setup (SEMIC). Maybe, if you can run SEMIC at different elevation, you could get a feeling for the implied differences. At the very least this inconsistency should be recognised and discussed as a shortcoming.

p9 l6 replace "reveals" by "shows" or "exhibits"

p9 l12 What criteria are used to judge plausibility of the warming patterns?

p9 l15 Same problem here. What criteria are used to judge implausibility of the warming patterns?

p9 l16 Add "as Figure 3" after "in a similar fashion".

p9 l16 Remove "as" before "as" or "as" after "as".

p9 l22 Reformulate "extreme pattern".

p9 l33 Validation of the SMB for the present day has to come much earlier to give confidence in SEMIC and should include analysis of the 2D pattern, not only total numbers.

p10 l2 All of this suggests that the confidence in the derived SMB forcing (and consequently the resulting SL numbers) is rather low, something that should be discussed in the end of the paper. However, ultimately you are using anomalies with respect to 1960-1990, so maybe that looks better. To be shown.

p10 l11 Is it important which model is used? If not, make that clear.

p10 l15 These results are difficult to see in Figure 6. It could help to plot velocity differences or ratios instead. Zooming in on some important regions could also give the interpretations more substance.

p10 l19 This paragraph should start with a motivation before going into technicalities on how things are calculated.

p10 l24 It seems like a strange choice to not correct the reported SL changes for the

model drift. I interpret all the corrections that go into the method as an attempt to produce a steady state at 1960. Or are you suggesting that the model drift should represent some natural background evolution? In my understanding the (negative) SL response in an unforced forward experiment is purely an artefact of the initialisation method and should be corrected. Another motivation would be to be transparent about the remaining model drift, which I could appreciate. However, in this case the results of a full control experiment should be presented alongside with the SL numbers of the forced experiments so that the actual magnitude of the projection can be easily judged by the reader.

p10 l25 As mentioned in the general comments, I am not convinced that the timing when Greenland mean temperature changes cross 1.5 degree is a very meaningful diagnostic, in the light of spatially divergent warming trajectories. What interpretation are you hoping to derive from this analysis?

p11 l4 "This is potentially an effect of ice dynamics"? You are running an ice sheet model, which should put you in the place to make an informed statement about what is going on here.

p11 l9 Reformulate "false trend".

p11 l18 What are these "errors in vertical ice velocities"? If this is a shortcoming of your ice sheet model, that should be discussed at some place in the model description. Does the same problem occur in the unforced control experiment? Again, being in full control of the ice sheet model in use here, you should be able to diagnose exactly what the problem is.

p11 l27 Why is this section called "Acceleration" when some of the glaciers see deceleration? I suggest rewording to "Dynamic response" or similar.

p11 l32 I am wondering in how far a detailed analysis of individual glaciers is justified given that an important aspect of the forcing in form of interaction with the ocean and

sub-glacial hydrology is missing. The comparison suggests that we could hope to get the behaviour of individual glaciers in line with observations, which I consider very unlikely given the steady-state initialisation, coarse GCM-based forcing and lack of important forcing mechanisms.

p12 l2 You could speculate that you could maybe reproduce observed acceleration of Jacobshavn Isbrae if calving rates are forced like in Bondzio et al (2017). If this is really the case in your model is not clear until you have tried it. Reformulate.

p12 l7 What is generally the magnitude and pattern of the SMB correction, average, largest magnitude, overall positive or negative? Where is it particularly prominent? What does that mean for ice dynamics and SMB, which fail to generate or export enough ice from a given region?

p12 l7 Replace "undermining" by "underlining"

p12 l10 What does "geometric settings at their base" refer to? Clarify

p12 l10 Why does alternation between acceleration and deceleration mean the model is able to "resolve glacier valleys well"? What does it mean to resolve glacier valleys well? The geometry, the velocity structure within the valleys?

p12 l14 Sea-level contribution is in mm not mm a-1

p12 l31 These numbers should be given before, when the results are being discussed, and as mentioned earlier, together with the model drift of an unforced control experiment.

p13 l2 This paper requires a dedicated discussion section before the conclusions that serves to discuss the advantages and shortcomings of the models and processing steps needed to arrive at the final numbers.

p13 l4 "switching between spin-up and RCP forcings" A correctly applied anomaly method should not lead to any additional model drift, other than the imbalance re-

sulting from imperfection of the data assimilation process. Possibly the SMB implied during initialisation differs from the one used further on? Often modellers use a (short) relaxation run as part of the initialisation to avoid too large model drift in the forward experiments, possibly combined with a correction method as applied here. At any rate, the uncorrected model drift of as much as 50 % of the signal by 2100 (MIROC) and the corrected model drift of still 30 % of the signal seems pretty large given the low magnitude RCP2.6 forcing applied here. This should be discussed in the paper at some point.

Table 1 Not clear which actual years are covered by these spin-up runs. Clarify.

Table 2 - What does it mean when a temperature of 0.00 is indicated as modelling results? The -2.4 at NGRIP means that the temperature is at the pressure melting point (PMP). Is that the case for the simulated temperatures for p-cl,Gr and pd-cl,Gr? - Basal temperatures of $\sim$-20 seem to be extremely low compared to the observed ice core temperatures (nowhere below -14) and are at odds with my own experience in thermodynamic modelling of the GrIS. The results should raise some doubts about the correctness of the applied thermodynamic model. - Typically, one would expect the pd spinup to result in generally warmer basal temperatures throughout, because of the lack of glacial signatures in the evolution. This is not confirmed in some cases. Why is that? - Could add the NEEEM ice core to the list of constraints

Figure 1 Add what area is used to calculate GrIS warming. All land area, observed ice sheet mask? b) Include GrIS in y-label.

Figure 2 Caption: "The grey line depicts the identity" Also describe here which range of years are plotted and from what product (grid). Add what area is used to calculate GrIS warming.

Figure 3 Colour bar labels are not well readable at this size. Could remove identical colour bars per row of figures and have one big one.

Figure 4 Colour bar labels are not well readable at this size. Could remove identical colour bars per row of figures and have one big one.

Figure 5 The forcing that the ice sheet model actually sees and that goes into the SL projections is based on anomalies of the SMB with respect to 1960-1990. How does figure 5 look like and how does the constructed SMB compare to observations when this anomaly calculation is applied? Caption: Is there a paper reference available for the SMB observation product?

Figure 6 Figure colour bar labels are not well readable at this size. Could remove one of the identical colour bars per row of figures. Should add contour lines in panel c and d. Caption: (a) simulated horizontal velocity magnitude, (b) observed horizontal velocity magnitude (Rignot and Mouginot, 2012), ...

Figure 7 Figure labels are not well readable at this size. Labels should be increased to be readable in the final two-column layout. Caption: Add what area is used to calculate GrIS warming. You should note here that the relaxation run differs in setup from the other experiments.

— References —

Price, S. F., Payne, A. J., Howat, I. M., and Smith, B. E.: Committed sea-level rise for the next century from Greenland ice sheet dynamics during the past decade, Proc. Nat. Acad. Sci. U.S.A., 108, 8978-8983, doi:10.1073/pnas.1017313108, 2011.

Robinson, A., Calov, R., and Ganopolski, A.: An efficient regional energy-moisture balance model for simulation of the Greenland Ice Sheet response to climate change, The Cryosphere, 4, 129-144, 2010.

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