

Dear Editor, dear Reviewers,

the authors wish to thank both reviewers for the detailed review containing many helpful remarks and constructive criticism! We do appreciate very much the time spent on getting down to the study. Before answering the points raised by both reviewers, we want to inform that we have detected an application error when calculating the SMB with SEMIC. This has a significant effect on the results, for instance the SLE contribution (Figure 1).

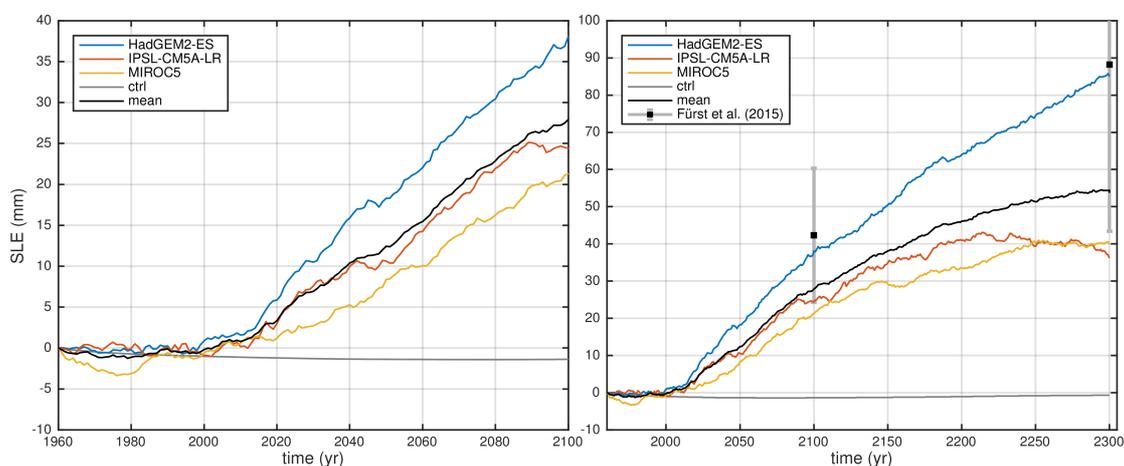


Figure 1: Sea level equivalent until the year 2100 (left panel) and 2300 (right panel) for all GCMs. Additionally, the control run and the mean of all GCMs are shown. The right panel shows additionally the mean and standard deviation at the year 2100 and 2300 by Fürst et al. (2015).

Beside the updated results (which are now fit better to observations and previous studies) and the corresponding changes to the text, we have performed the following major changes - that are all also documented below in detail:

- As both reviewers suggested re-structuring the manuscript, the material in the revised manuscript is presented as follows:
 1. Introduction
 2. Model description
 - 2.1 SEMIC
 - 2.2 Ice flow model
 3. Results
 - 3.1 Forcing fields
 - 3.2 Present day state
 - 3.3 Projections
 4. Discussion
 5. Conclusion
- We have improved the control run (see Figure 1).
- SEMIC and the calculation of the SMB are explained in more detail and became more prominent in the manuscript.
- Although the point was not raised by the reviewer, we will focus more on “The effect of overshooting 1.5°C” as mentioned in the title

Technicalities: below we answer each point raised by the reviewers and mark our answer in blue color. Point raised by both reviewers are answered at one location and referenced at the second one. 'Done.' denotes that this point would be solved in the revised version of the manuscript. This could be that it will be either done directly, or that due to other changes the point does not arise any more, or that the point has been answered at another place in this text already.

Reviewer #1

— 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.

Yes, indeed this is an important point and we followed the reviewers suggestion. With using the parameters of Krapp et al. (2017) the direct output of the SMB from SEMIC has a misfit of about $\sim 2\text{m/a}$ and a correlation of $\sim r^2=0.5$ by comparing `SMB_RACMO_1960-1990` and `SMB_SEMIC_1960-1990` (almost similar for all GCMs used). However, recalling Equation 3 and 4 from the manuscript,

$$\text{SMB}(x, y, t) = \overline{\text{SMB}}_{\text{RACMO}}^{(1960-1990)}(x, y) + \Delta\text{SMB}(x, y, t) + \text{SMB}_{\text{corr}}(x, y, t), \quad (3)$$

$$\Delta\text{SMB}(x, y, t) = \text{SMB}_{\text{SEMIC}}(x, y, t) - \overline{\text{SMB}}_{\text{SEMIC}}^{(1960-1990)}(x, y), \quad (4)$$

we do not use the direct output of SEMIC, but apply anomalies computed using SEMIC. The benefit of our approach is, that only the GCM trends of SMB changes are added to the RACMO SMB reference field, which represents the real SMB distribution very well. If we compare the computed SMB to RACMO (according to Eq. 3 and 4 without the synthetic SMB_{corr}), for instance for the HadGEM2-ES year 1990, it shows a very good agreement (Figure 2). See also answer to specific comment “p10 l2” below. In the revised manuscript we dedicate an own section to this issue.

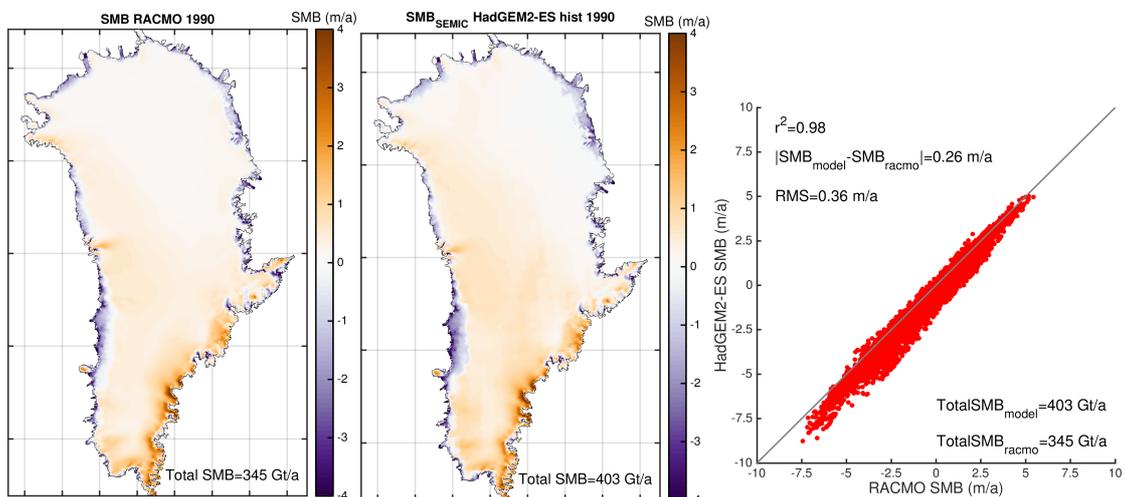


Figure 2: (left panel) surface mass balance of RACMO2.3 (Noel et al., 2016) for the year 1990; (middle panel) surface mass balance for HadGEM2-ES for the year 1990 according to Eq. 3 and 4 in the manuscript (without SMB_{corr}); (right panel) scatter plot of both fields.

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.

We expand the section about the SEMIC model in order to give the reader a better understanding of the model. In the new version of the manuscript we also review in the introduction section briefly the already existing alternatives used and relate the discussion section accordingly. The reason we have not included too much detail on that issue previously is, that we basically apply SEMIC and that the model in itself and all the parameter tuning is work done by Krapp et al., 2017. The advantage of using a semi-complexity model is indeed its simplicity and cost efficiency, which would allow ice sheet modellers to also run computation up to time scales of thousands of years (e.g. until 5000) studying long-term commitment of various emission scenarios and hence not be limited by the availability of regional climate model output. However, regional climate models having the clear benefit for representing snow and firn layers with all melt and refreezing processes by far more realistic than any semi-complexity model will ever do. For the future, we plan a study on comparing the difference in ice sheet model response to three different types of forcings, PDD, SEMIC and a regional climate model forcing.

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 Frst et al., 2015) hinges on the implied sensitivity of the SMB model, which is currently not possible to be judged.

We have chosen the same parameters of SEMIC as Krapp et al., 2017, due to the following reason: the parameter tuning procedure performed by Krapp et al., 2017 aimed to find a parameter set which gives a best fit between SMB and skin temperature T_s of SEMIC with only a limited number of processes and simpler parameterisations than a regional climate model with full complexity would derive. As a regional climate model is typically validated against reanalysis data and observations, the best match between SMB and T_s of SEMIC and regional climate model (in that case MAR) is the best way to represent the processes and their parameters in SEMIC. We see it thus as a tuning of the parameterisation of the processes. Once the process description in SEMIC is optimised, any type of input, either GCM or reanalysis data fields, will lead to the best possible SMB and T_s fields that SEMIC can produce. Still, the GCM will lack the best atmospheric fields over the ice sheet, as it is limited in resolution compared to a regional climate model. Given experiences we made from these three GCMs used in this study, which are all have different drawbacks, which would mean to have a tuning for each of them and this tuning would then make the whole benefit of having a semi-complexity model with low costs meaningless. Furthermore, it would basically mean to compensate far too low near surface temperatures with SEMIC parameters, which would offset the whole comparison of GCM forcing. Therefore, we have chosen a different approach: we compensate for this by using the SEMIC output only as an anomaly.

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.

We can understand this and follow the reviewer's recommendation and try to describe the processing of SMB and Temperature product better in the revised manuscript.

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.

We do not agree that an ice model is much less important for the projection compared to the SMB forcing. In order to estimate the SL contribution from the ice sheets an appropriate ice flow model (resolution, ice dynamics (and response), grounding line migration, etc.) is necessary. The main novelty in this study is from our point of view, the derivation of an appropriate initial state, which is also stressed in Goelzer et al. (2018) and given that our way to derive an initial state became a recommendation from a community benchmark experiment, this is indeed clearly the benefit of the study presented here. However, as about 50% of the current mass loss of Greenland is due to changes in SMB and, as the reviewer claims that the SEMIC is the main novelty of this study, we will follow the recommendation and first introduce the SMB forcing and describe the ice flow model afterwards. We agree that some information was not placed optimal in the manuscript and we will follow the concerning specific points raised by the reviewer below. We will also provide a separate discussion section on this issue in the revised manuscript.

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. We can prove that the thermodynamic model is correct as the numerical code is verified against analytical solution (Kleiner et al., 2015). Furthermore, the application to Jakobshavn Isbræ gives reasonable results for the thermodynamic model (Bondzio et al., 2017). There, the simulated temperatures show a good match to measured temperature profiles at the fast flowing area of the ice stream.

From our point of view the selected scenario (p-cl, Gr) as initial state for the projections from our sensitivity study shows a reasonable match to the observations, given the lack of knowledge of the geothermal heat flux, which affects any type of ice sheet modelling of Greenland independent of atmospheric forcing. At least the GRIP location with T_{sim} of -18°C is too cold ($T_{obs}=-8^{\circ}\text{C}$). Due to the fact, that the applied inversion technique for the friction coefficient allows sliding everywhere, the portion of deformational shearing may be underestimated, which cannot be proven without any observations of basal velocities that are unfortunately not existing at all. However, for our projections on centennial timescales this is a negligible effect.

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.

We agree and provide additional material.

— 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. Instead of referencing the years when 1.5 warming is reached in the GCMs, we refer to the different warming trends in the GCMs.

Over which area is the Greenland wide average calculated?

We have used the ice sheet mask provided by the BedMachine dataset (Morlighem et al., 2014).

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

You are right. Our “plausibility-check” is very subjective. In the section “Forcing fields” lines 1-15 we discuss the forcing fields, in particular, the temperature distribution and its change over time. From our point of view the temperature field should reveal a higher warming in the North

(polar/arctic amplification) and overshoot the mean global warming value. Both aspects are only fulfilled for HadGEM2-ES. The plausible DSMB pattern (p9 l28) is a consequence from the temperature. We take care of this in the new version of the manuscript.

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? In the new simulations not all floating tongues are lost. We will explore this in more detail.

p1 l14 What values? Greenland sea-level contribution? Elevation changes? Clarify
See next comment.

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 quantitative statement very hard to justify.

The sentence is rewritten to: *"The sea-level contribution estimated in this study may serve as a lower bound for RCP2.6 scenario, as processes proven to play a major role in GrIS mass loss are not yet represented by the model."*

Regarding the missing processes: It has been shown by the SeaRISE effort (Bindschadler et al., 2013) that increased oceanic melting and increased sliding will lead to a further mass loss of the GrIS. Both aspects are very likely within a warming climate. Therefore, we assume that our values are a lower bound.

About the uncertainties in climate forcing, we hope to convince the reviewer with the new version of the manuscript that contains additional material.

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

p1 l22 Remove "Obviously"
Done.

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.

We delete "aim of this study here" and moved it to the end of the section "introduction".

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

p2 l5 "are not passing the limit". Which limit, be more precise.
Done. "Limit of 1.5°C or 2°C" is added.

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

Done.

p2 l9 Repeated "response"
Done, first appearance is deleted.

p2 l9 Maybe "GCM" is better than "atmospheric model" here.
Yes, you are right. Atmospheric model is replaced with GCM.

P2 l10 Maybe "surface mass balance changes".
Done.

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

p2 I13 The causality in this sentence is not clear. What does higher-order physics have to do with corrections of atmospheric forcing?

Done. The sentence is partly removed.

p2 I16 "the low computation cost"

Done.

p2 I17 Why is high resolution a requirement for higher-order physics?

Compared to Shallow Ice Approximation the higher-order physics include transversal and longitudinal stress gradients. If the resolution is low, the gradients in the geometry are decreasing and therefore the influence of these stress gradients. In order to resolve the geometry and the stress gradients a high resolution is needed.

We have not discussed that in the text so far, as this is well known in ice modelling, however, we can include a paragraph on that on editor request.

Also, for this study, representing the SMB forcing accurately should be the most important aspect where computational resources should be directed to.

Done.

p2 I19 "anomalies *of*"

Done.

p2 I21 More precision needed to replace "obtain these anomalies from the GCM"

Done. We have rewritten the "Aim of the study" at the end of the Introduction (see also comment to p2 I2).

p2 I24 Consider describing the ice sheet model later since it is the least important component in this study.

See comment to major point above.

p2 I25 I suggest a less technical description here, e.g. "Ice flow and thermodynamic evolution of the GrIS are approximated"

Done.

p2 I28 It is not the elements themselves (as in finite elements) that have these characteristics (SIA to FS). Reformulate. Which approximation is finally used?

The sentence is rewritten and we give a reference to the Blatter-Pattyn approximation.

p3 I1 The reader does not necessarily know what "the balance equations" refers to.

We drop "balance equations" here.

p3 I5 Better to describe how basal melt rates are calculated before saying that they are held constant during the experiment.

Done.

p3 I7 "Under grounded ice"

Done.

p3 I7 Melting is not *due to* frictional heating. Frictional heating and geothermal heat flux warm the ice that may eventually melt. More precision needed.

Only geothermal heating and internal deformation warms the ice. Once the ice temperature at the base reaches the pressure melting point sliding occurs and with that melting takes place. The boundary condition is switched from a Neumann-type to Dirichlet-type condition and all excessive energy is used for melting. According to Aschwanden et al. (2013) melting a_b is defined as:

$$a_b = F_b - (q - q_{geo}) * n_b / (L * \rho_i),$$

where F_b is the frictional heating, q the heat flux into the ice and q_{geo} the geothermal flux entering the ice at the base (n_b is normal vector, L is latent heat and ρ_i the density of ice). Once the pressure melting point is reached frictional heating and geothermal flux is only used for melting. However, we rewrite the sentence to: *“Once the pressure melting point at the grounded ice is reached melting is calculated from basal frictional heating and the difference in heat flux at the ice/bed interface.”*

p3 23 "shearing"
Done.

p3 26 Remove "fields".
Done.

p4 l1 Replace "or" by "and".
Done.

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.
We replace "suitable" with "required".

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.
Yes, "initialized" should be replaced by "relaxed". We update the description of the initial state by giving more details.

p4 l7 Why is the spinup done to 1960, and the reference period 1960-1990. Motivation needed.
The spin-up is done to 1960 in order to start the projections before the tipping point of GrIS mass balance (Noel et al., 2017). The reference period 1960-1990 is chosen as we assume the ice sheet close to steady state in this period.

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?
We add: *“The inversion approach infers the basal friction coefficient k^2 in Eq. 1 by minimizing a cost function that measures the misfit between observed and modelled horizontal velocities (Morlighem et al., 2010).”*

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

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.
We rewrite this paragraph to better explain our spin-up strategy. To make it clear the spin-up is only run over 125kyr before present. The mesh sequences just repeat a certain period of the spin-up by subsequently refining the mesh.

p4 l20 What precisely is taken, thickness and bedrock data?
Bedrock and thickness is taken from BedMachine Greenland. We added that to the text.

Removes "bed from". Add "data set" after "BedMachine Greenland"
Done.

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

Done.

p4 I23 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?

Done.

p5 I4-7 Reformulate this sentence, too long.

Done.

p5 I10 Motivate the choice of models. Why these three GCMs?

We add a motivation for the selection of the three GCMs: *"We have selected GCMs which covering the CMIP5 historical scenario, the RCP2.6 scenario until 2300 and reveal an overshoot in annual global mean near-surface temperature change relative to pre-industrial levels (1661–1860)."*

p5 I14 Specify the reference period against which the change is calculated.

We add that the reference period is the pre-industrial level from 1661–1860.

p5 I19 Could give a more specific reference here, i.e. a specific IPCC chapter.

Done.

p6 I2 Why would polar amplification only have consequences in extreme years? Or does it have an impact on the amount of extreme years? Clarify.

We rephrased this, as we did not intend to make any statement on the amount of extreme years, nor on the amplification having only consequences in extreme years. We intended to say that the interannual variability is larger if looking at temperature time series over Greenland compared to global average.

p6 I2 Add reference to figure 2 at end of sentence.

Done.

p6 I3 Add "amplification" after similar.

Done.

p6 I4 Polar amplification is not the same as Greenland amplification. Consider and discuss the difference and similarities if any.

We switched the wording to the terminology Arctic amplification.

p6 I7 "A striking feature" in which model?

All models show the higher variability. Compare Figure 1a and b in the manuscript.

p6 I9 "lower bound" and "upper bound" is the wrong wording for this case. Use "the highest" and "the lowest forcing" or similar.

Done.

p6 I11 "might be different across the GrIS". Why "might", you have the data to check that and make an informed statement.

Done.

p6 I13 How does a model "best" represent overshooting. Either temperature overshoots or it doesn't. Reformulate.

Done.

p6 I15 Specify what you mean by "ice sheet specific quantities".

Done.

p6 I15 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).

See answer to major points above.

p6 I18 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.

See answer to major points above.

p6 I18 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.

We agree with the reviewer. We expand the section about the SEMIC model. In order to be consistent with parameters provided by Krapp et al. (2017) we switched back to the albedo scheme used by Krapp et al. (2017) for the new simulations.

p6 I24 Motivate why this two-step procedure is necessary.

Usually this two-step procedure is not necessary. One would interpolate the GCM data from the original 1° grid directly to finite element grid of ISSM. Without going into the details, it is technically the easiest way. However, for future applications we aim to avoid the intermediate interpolation. We add a sentence to the revised version of the manuscript.

p6 I28 Add "(.)" after "quantities".

Done.

p6 I30 In my understanding $hs^{ISSM-pd}$ should be replaced by $hs^{SEMIC-pd}$. Or are they both considered the same? Please clarify.

In general $hs^{ISSM-pd}$ and $hs^{SEMIC-pd}$ at the initial state are the same. However, to be more precise we change hs^{ISSM} to hs^{SEMIC} .

p6 I31 What (and when) exactly is the present-day surface elevation referred to here?

In the revised version of the manuscript we make a clear distinction between initial state (1960; end of spin-up) and present day (~2000).

p7 I4 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.

Done. As mentioned above, we restructure the revised manuscript.

p7 I5 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?

Yes, you are right. Our initial state is exactly the geometry that we prescribed from observations. We rephrase this paragraph.

p7 I7 Remove "perfect" before equilibrium.

Done.

p7 I10 Not clear why the models have to be "run on the same ice sheet mask". Clarify.

As RACMO and our model are run on the same ice sheet mask and geometry the forcing fields of RACMO could be used. A model that was run with evolving geometry and calving

front during a paleo spin-up and ends with a significant different ice sheet mask and geometry at present day could not easily utilize the RACMO data. We add a sentence to clarify this.

As RACMO and our model are run on the same ice sheet mask and geometry the
p7 l15 Replace "ice sheet models" by "initial states".

Done.

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

Yes, you are right. "Add" is replaced with "subtracted".

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. We agree - in the new version we add a figure and give numbers of the applied SMB correction. The method will also be discussed but very briefly. Our SMB correction is in the interior of the ice sheet close to zero but dominant at fast flowing outlet glaciers.

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.

This was indeed not explained in sufficient detail. However, for the revised version of the manuscript we have re-run the simulations by using the imbalance at $t=1$ from the relaxation run. The time-varying SMB correction is dropped for the new version of the manuscript. With the new SMB correction, the model drift (i.e. SLE; see Figure 1) is close to zero. We will introduce a paragraph to the SMB_{corr} (see comment to P7 l17).

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

Done.

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.

We did not prolong RCP2.6 ourselves but there are official extended RCP2.6 scenarios, see e.g. Meinshausen et al. (2011), based on which climate modelling centers carried out extended future climate projections within CMIP5, bias-corrected versions of which were used here.

p8 l5 Maybe "albeit without a correction term"?

Done.

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

Thank you for pointing out this imprecise wording. In the revised manuscript version we will write "*The ISIMIP2b atmospheric forcing data are CMIP5 climate model output data that have been spatially interpolated to a regular $0.5^\circ \times 0.5^\circ$ latitude-longitude grid and bias-corrected using the observational dataset EWEMBI (Frieler et al., 2017; Lange, 2017).*"

p8 l14 "respectively".

Done.

P8 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?

This was not well explained in the manuscript. The terms of present-day and initial state were mixed up and not properly defined. In our case h_{fix} is identical to the surface elevation used in SEMIC and to the surface to the end of the spin-up.

p9 I25 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.

This would be an interesting study. But for our application we follow the same argumentation above to the major point "parameter tuning". The parameters found by Edwards et al. (2014) are the most physical reliable and additionally we don't want to have different parameters between the three GCMs.

p9 I6 replace "reveals" by "shows" or "exhibits"

Done.

p9 I12 What criteria are used to judge plausibility of the warming patterns?

See comment to p1 I8.

p9 I15 Same problem here. What criteria are used to judge implausibility of the warming patterns?

See comment to p1 I8.

p9 I16 Add "as Figure 3" after "in a similar fashion".

Done.

p9 I16 Remove "as" before "as" or "as" after "as".

Done.

p9 I22 Reformulate "extreme pattern".

Done.

p9 I33 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.

We will introduce a new section on this issue. See answer and Figure 2 to major point above.

p10 I2 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.

Due to the error we made when running SEMIC (see preamble of this document), the time series improves very much. Annual variations of the calculated SMB are now in the order of the DMI/polarportal and RACMO2.3 data.

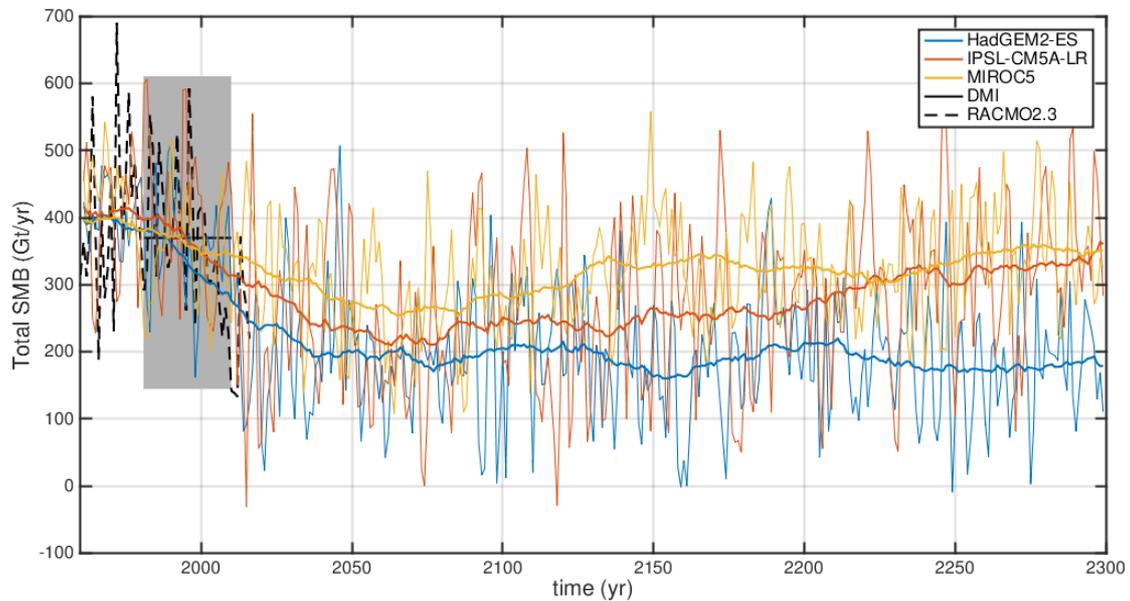


Figure 3: Time series of SMB according to Eq. 3 for all three GCMs under RCP2.6 forcing scenario (thick line is a 50-year running mean applied to the GCM data). In grey colour and black line the range and mean of SMB between 1981–2010 from DMI/Polarportal is marked (polarportal.dk). The dashed line shows the SMB time series of RACMO2.3 (Noël et al, 2016) from 1958-2016.

p10 I11 Is it important which model is used? If not, make that clear.

The behaviour is for all models similar. We wrote that now explicitly.

p10 I15 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.

In the new version of the manuscript we will provide a scatter plot of observed and simulated velocities.

p10 I19 This paragraph should start with a motivation before going into technicalities on how things are calculated.

We rephrased this section.

p10 I24 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.

This point is solved. The new control run shows almost no SL contribution (see Figure 1).

p10 I25 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?

The story line of the project started with global overshooting scenarios, so scenarios which fulfil the Paris agreement, but are overshooting the 1.5° before 2100 and cool down to 1.5° globally by 2100. The science question arising from this was for us, if the society chooses this pathway to 1.5°, how does Greenland mass loss develop? So, indeed the timing of Greenland crossing 1.5° is not that meaningful, we just used this as a kind of further proxy to assess the

GCMs. A GCM that crosses 1.5° late is suspect to underrepresent the overshooting effect on Greenland.

p11 I4 "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.

The paragraph will be rewritten due to the updated results.

p11 I9 Reformulate "false trend".

Done.

p11 I18 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.

As the high elevation decline does not appear anymore in the new simulations, this sentence is dropped.

p11 I27 Why is this section called "Acceleration" when some of the glaciers see deceleration? I suggest rewording to "Dynamic response" or similar.

Done.

p11 I32 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.

This is indeed a good point raised. It is certainly true, that important forcing mechanisms like the oceanic forcing and subglacial hydrology are missing in this study, however, representing the dynamics of a glacier in the narrow fjords of Greenland well or representing the large NEGIS well, is only achieved with sufficient grid resolution and physics in the model, which our model both fulfils. This is indeed assessed by comparing individual glacier drainage basins with observation, like the surface velocity field. We are concerned about the statement 'given the steady-state initialisation' – we do not perform a steady-state initialisation at all, in contrast, we perform a complex initialisation procedure with mixture between inversion and paleo-spin ups. This procedure has been the top procedure in an international benchmark assessing the ability of models to achieve a good initial state (Goelzer et al., 2018). The reviewer seems to have overlooked this substantial part of this study. The coarse GCM-based forcing is subsequently processed in SEMIC is improving the resolution and the anomaly forcing is making sure, that the SMB in individual glacier basins is in high resolution – so the glacier basins are forced on high resolution.

p12 I2 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.

We reformulated this sentence. It is obvious, why we did not try it: there are no observations of calving rates in the time period 2018-2300 available in 2017.

p12 I7 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?

See Answer to comment P7 I17 above.

p12 I7 Replace "undermining" by "underlining"

Done.

p12 I10 What does "geometric settings at their base" refer to? Clarify

With geometric setting we refer to bed topography.

p12 I10 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?

To resolve glacier valleys well means that the velocity field within a glacier valley is reasonably well representing the observed velocities. If a glacier is narrow, e.g. 3km wide, a coarse resolution ice model, e.g. running on 5km, will never be able to represent this glaciers dynamics or contribution to mass loss, as both velocity field and elevation change will lack sufficient resolution. If your grid resolution is too coarse, a narrow glacier would entirely accelerate or decelerate, as you would not have enough elements within such a narrow valley. This is what we had in mind when we formulated this sentence, but we rephrased it to avoid any confusion.

p12 I14 Sea-level contribution is in mm not mm a-1

Done.

p12 I31 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.

Done.

p13 I2 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.

Please see also comments above.

p13 I4 "switching between spin-up and RCP forcings" A correctly applied anomaly method should not lead to any additional model drift, other than the imbalance resulting 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.

Due to the improved SMB_{corr} for the new version of this manuscript, this point is dropped.

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

See answer to specific comment p4 I10 above.

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?

We do apologize - the observed values are PMP. Ours were provided pressure corrected. This will be corrected in the new manuscript version.

- Basal temperatures of ~ -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.

See answer to major point above.

- 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?

See answer to major point 5 of Reviewer 2.

- Could add the NEEEM ice core to the list of constraints

Done.

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

We have used the ice sheet mask provided by the BedMachine dataset (Morlighem et al., 2014). b) Done.

Figure 2 Caption: "The grey line depicts the identity"

Done.

Also describe here which range of years are plotted

Done.

and from what product

It remains unclear what the reviewer means with product/grid? This is just a plot of the global data versus a sub-dataset over Greenland of the GCMs with different GCMs denoted in color.

Add what area is used to calculate GrIS warming.

Done.

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.

We apologize the bad quality of the figures. Figure will be updated as suggested.

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.

We apologize the bad quality of the figures. Figure will be updated as suggested.

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?

This must be a misunderstanding. Figure 5 is exactly the figure that you want to see. As stated in the caption the plotted SMB is according to our SMB anomaly equation (Eq. 3) which is imposed on the ice surface.

Caption: Is there a paper reference available for the SMB observation product?

The webpage give in the text (p9 l34) is added here, but to our knowledge there is no paper reference available. The new figure also provides the RACMO time series and reference (Figure 3).

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.

Done.

Should add contour lines in panel c and d. Caption: (a) simulated horizontal velocity magnitude, (b) observed horizontal velocity magnitude (Rignot and Mouginot, 2012), ...

Done.

Figure 7 Figure labels are not well readable at this size.

Labels should be increased to be readable in the final two-column layout.

Done.

Caption: Add what area is used to calculate GrIS warming.

Done.

You should note here that the relaxation run differs in setup from the other experiments

In the new figure the relaxation run is not shown, as it was only run for 1 year.

Reviewer #2

General comments:

This manuscript presents future volume evolution scenarios of the Greenland Ice Sheet under three different surface mass balance forcings. Atmospheric forcing is provided by three global climate models and the surface mass balance is computed with a relatively simple surface energy balance model. The ice-sheet model employed, is the state-of-the art ISSM model with higher order ice physics. The sea-level rise projections from surface mass balance perturbation alone are between 46-71 mm by 2100 and 114-189 mm by 2300.

The topic of the manuscript is of interest to ice-sheet modellers as well as the wider cryospheric community. The overall structure of the paper is logical but some sections would benefit from a tidy-up and the language is hard to follow in some places. While the results are certainly not groundbreaking and omit any contributions from ice dynamics, I think the manuscript presents enough novelty and hence merits publication subject to consideration of my comments listed below.

We are concerned about the statement that we omit any contributions from ice dynamics. The study presented is solving the higher order approximation of the momentum balance of ice sheets, which is not standard for all ice models performing projections, as quite many of them are relying on shallow ice / shellfish stream approximation. Not only do we solve the higher order momentum balance, but we do so in high resolution, hence the benefit of this level of approximation is not suppressed by coarse grid resolution.

Specific comments

The study's strong point from an ice-sheet modelling perspective is the model initialisation which combines the two commonly employed spin-up and data assimilation techniques. The main focus is, however, on the surface mass balance forcing with the SEMIC model. In the light of this and the importance of the surface mass balance forcing, for someone that is not familiar with the SEMIC model, I am missing a succinct description of the model fundamentals and the configuration used in this manuscript. Furthermore, the entire manuscript would benefit from some reordering and substantial improvements to certain sections and improvements in readability of some figures (detailed below). My main concern is with the calculation of the surface mass balance anomaly for the projections. Please find below my main concerns, followed by specific comments.

Main concerns:

1. My main concern is the calculation of the surface mass balance anomalies. First of all, I understand that you account for the model drift by adding a synthetic SMB correction term (SMB_{corr} in Equation 3). But what dh/dt is applied – an average of your unforced relaxation run from 1960-2060 or the last or first time step of this relaxation simulation? How can this term be time-varying in your projections? On page 9 line 20 this time-varying SMB_{corr} term is used as an explanation for spatial differences in the SMB pattern. Maybe I missed it, but it would help if you clarified this.

The SMB correction was probably not explained with sufficient detail – as also mentioned by Reviewer #1. The relaxation run for measuring the SMB correction was run from 1960 to 2060 exactly on the same time steps as the subsequent climate forcing runs. From the relaxation run we have taken the dh/dt values from every time step $t_1, t_2, t_3, \dots, t_{end}$ and prescribed these as a SMB correction (after 2060 the SMB correction is held constant). The dh/dt values during the first time steps are rather large compared to the later ones as the ice sheet approaches equilibrium. Therefore, the SMB correction in the year is 2000 much larger as in 2100 or 2300. These you can see in Fig. 4. However, in the new version of the manuscript the time varying SMB correction will no longer appear as we have modified the SMB correction towards an time independent correction in our new

simulations. The SMB correction is taken from the first year and held constant in time (see answer to Reviewer #1, comment to P7 I17).

2. The more critical point is how you compute your SMB in Equation 3. The way I understand it and please correct me if I am wrong, Equation 3 states that SMB_{RACMO} plus your correction for the model drift should give you an SMB that keeps your ice sheet close to steady state (or at least present geometry).

It is actually not about keeping it close to steady state, it is keeping the SMB distribution as close to the realistic one, which we assume RACMO – or any other validated regional climate model – to be.

The applied perturbations are however calculated with respect to the SEMIC model baseline. If you use your SMB_{RACMO} to keep your ice sheet in steady state, you should also calculate your anomalies with respect to your SMB_{RACMO} field. If not, your perturbations to the surface mass balance appear a bit arbitrary. Would it not be more consistent to use the SEMIC output? The argument that your model drift gets larger is rather weak, considering that you would just get a larger SMB_{corr} term from the unforced relaxation simulation.

We are confident that the computation of the SMB is correct and consistent – the anomaly approach is widely used in ice flow modeling (e.g. Goelzer et al., 2013, 2018). If we assume the ideal case in Eq. 3 and 4 the reference terms $+SMB_{RACMO_{1960-1990}}$ and $-SMB_{SEMIC_{1960-1990}}$ will cancel out and the climatic forcings from the $SMB_{SEMIC}(t)$ remain. This is certainly not the case and the equation must be interpreted as having the RACMO reference field – with a good spatial distribution – as a background field where the trends from SEMIC are added.

For the relaxation and control run we have used a simplified form of Equation 3 and 4 by neglecting the input from SMB_{SEMIC} . Of course, we could use a GCM forcing for the relax/control run from the so-called pre-industrial run from the ISIMIP2b project, but the results are unlikely affected by it. The temperature changes in the pre-industrial run are so small, that a $\Delta SMB_{SEMIC_{pre-industrial}}$ according to Eq. 4 will be negligible.

You are right, one could drop both reference terms and put all the model drift in the SMB correction term and use the SEMIC output directly. In this case we would lose information on climatic forcing versus synthetic forcing, which are an additional quality measures. The Total SMB for $SMB_{RACMO_{1960-1990}}$ is ~ 400 Gt/a, for $SMB_{SEMIC_{1960-1990}}$ ~ 500 Gt/a (dependent on GCM input) while the SMB_{corr} is ~ 100 Gt/a.

In the new version of the manuscript we give a better motivation for the choice of our SMB calculation and the assumptions we made for the relaxation and control run.

3. I think the section “Input data” should be removed as this mostly repeats earlier statements (e.g. Greve 2005 dataset). The basal drag inversion should be moved to the “Initial state” section as this is where it is most appropriate.

Done.

I would introduce a section “Results” which would start with the subheading “Forcing fields” and continue with “Present day elevation and velocities”. The heading “Projections” followed by “Present day ...” was confusing. I would suggest to add “projections” where appropriate e.g. Mass loss projections, Speed up projections etc.

We have restructured the manuscript accordingly. As mentioned in the preamble of this document, we have introduced a separate results and discussion section.

4. Please provide a more complete description of the SEMIC model than the few lines provided on P6 L15-22. You also claim to have improved the albedo parameterisation, but to me it is not clear how or to what extent. Please expand on this.

We agree with the reviewer. We expand the section about the SEMIC model. In order to be consistent with parameters provided by Krapp et al. (2017) we switched back to the albedo scheme used by Krapp et al. (2017) for the new simulations. As also replied to Reviewer #1, we also introduce a validation of the applied SMB fields (see first major points of Reviewer #1).

5. I am certainly not an expert on ice temperature, but to me the following questions came up when looking at Table 2. Are there no temperatures from observations for EastGRIP? At EastGRIP there is not temperature available yet. But we expect to get a temperature profile in the near future as the coring project is currently ongoing (<http://eastgrip.org>). Why are there such large differences in basal temperatures between the Greve (2005) and Shapiro and Ritzwoller (2004) maps at the selected locations? Does this mean that temperature in these regions is dominated by the geothermal heat flux and that this heat flux is that different at these locations? Why do the simulated temperatures do not agree with GRIP temperature observations?
- The large differences between the four set-ups arise from the different geothermal flux maps used and from the imposed surface temperature forcing. The Greve (2005) geothermal flux is generally larger, particularly in the northeast and at the Dye3 location (South Dome), than the Shapiro & Ritzwoller (2004) fluxes. Generally, the Greve (2005) geothermal flux leads to a warmer ice base compared to Shapiro & Ritzwoller (2004). The present-day climate forcing would generally lead to a warmer ice, as the history from the paleo conditions are missing. Consequently, the different combinations lead different thermal states. In the new version of the manuscript we have dropped these sensitivity study, as it is not relevant for the paper, and describe only the setting that we have used.

Technical corrections

Abstract

L2 "...sea-level change under different atmospheric forcing scenarios from ..."

Done.

L11 Sentence starting with "Simulated an observed sea-level rise..." That makes no sense to me. Is it simulated or observed? I believe you are trying to say that your simulated sea-level rise for the period 2002-2014 matches sea-level rise from observations in magnitude? Please clarify.

There was a typo: "An" changed to "and".

P1L19 delete second "past decade"

Done.

P1L22 Delete "Obviously, ..."

Done.

P2L1 „engaged“? Do you mean encouraged?

Indeed, we mean encouraged.

P2L20 "...provided by ..."

Done.

P2L27 replace "." with ","

Done.

P2L27 Sentence starting with "ISSM is designed to ..." Is this really important for the paper? Also while I welcome the fact that the authors kept the details of the ice-sheet model brief, I would appreciate if you could add what higher-order physics you used (Blatter-Pattyn, Stokes or SSA)? Please add to ice-flow model section. Also, can elements be either Stokes or SIA? Do you mean that for each element you can choose what force balance is solved?

The sentence "ISSM is designed to ..." is shortened. We give a citation to Blatter-Pattyn. The paragraph is slightly rewritten.

P3L2 "...surface mass balance and climate forcing"

The sentence is rewritten to: "The upper boundary incorporates the climatic forcing (i.e. the surface mass balance and ice surface temperature)."

P3L19 “compensates”

[Done.](#)

P3L20-21 “...according to a sub-grid parameterization scheme,...”

[Done.](#)

P3L24 “... towards the base where vertical shearing becomes more important.”

[Done.](#)

P4L4 Delete sentence starting with “Furthermore, the thermo-mechanically ...” I think it is obvious that if you simulate ice temperatures that your simulations are sensitive to temperatures.

[Done.](#)

P4 L5-13 and Table 1 I do not completely understand when you start your mesh refinements? The way I understand your initialisation method is that you run your temperature spin-up with mesh sequence 1, then you do an inversion for basal friction parameters and run your temperature spin-up again with a refined mesh before you do another inversion on the refined mesh? Please describe this more clearly.

[As also the other reviewer suggests giving more details, we have rewritten this paragraph \(see answer to Reviewer #1, his comment p4 l10\).](#)

P5L4-7 Please reformulate this sentence. It is too long. Also please delete “aim” as this implies that you are not sure it is going to work. Your results show that it clearly does work.

[Done.](#)

P5L10 Could you explain why the three GCMs were selected as forcing? So far this choice appears a bit random.

[See answer to Reviewer #1; his comment p5 l10.](#)

P5L20 This sentence is unclear. It reads like Greenland warms above 1.5°C but you are talking about T I believe. Also, could you state more clearly that you are comparing it to the global temperature increase in the GCMs.

[We rephrased this sentence.](#)

P6L3 Sentence starting with “While HadGEM2 ... “ makes no sense to me. Leading to similar factors? What factors?

[The warming of IPSL-CM5A-LR and HadGEM2 is of the same magnitude. We rephrased this sentence.](#)

P6L8 “reaches”

[Done.](#)

P6L8-9 This sentence has to come earlier as it is indeed very striking, but also expected.

[Done.](#)

P6L9 Please delete “Summarizing”

[Done.](#)

P6L17 Please delete “Due to the fact that Krapp et al. (2017) performed calibration over GrIS”

[Done.](#)

P6L28 “We follow ...”

[Done.](#)

P7L4 Here and throughout “the ISSM”=“ISSM”

[Done.](#)

P7L5 very well = well
[Done.](#)

P7L14-16 This statement needs a citation. Is this true for Greenland? I doubt that every data assimilation initialization leads to a 3% ice volume gain.
[We removed this statement.](#)

P8L5 By doing so = This ensures that
[Done.](#)

P8L14-15 espectively=respectively
[Done.](#)

P9L4 “leads to an increase in temperatures ...”
[Done.](#)

P9L5 “exceed 2°C of warming”
[Done.](#)

P9L6 and P9L8 Be more specific. By how much? Numbers please!
[We give numbers in the new version of the manuscript.](#)

P9L13-15 Please explain this. Why is this the most plausible? It is not apparent to me.
[See answer to Reviewer #1, his comment p9 l12.](#)

P9L16 delete first “as”
[Done.](#)

P9L19 here and throughout vallies=valleys
[Done.](#)

P9L20-21 See main comment above. How can this be time-varying?
[See answer to major point 1 above.](#)

P9L27 “The magnitude of SMB is far less in the period 2300-2000...”
[Done.](#)

P9L31 which pattern? Spatial or temporal or both?
[We are interested in the spatial distribution of the SMB and its change over time. However, in order to illustrate what the ice sheet’s total surface gains and losses have been over the year from SMB we show the integrated SMB in the time series. We rephrased this sentence.](#)

P11L14 Again why is this the most plausible pattern? Please elaborate.
[See comment to p1 l8.](#)

P11L33 “... experience acceleration across all simulations.”
[Done.](#)

P12L8 levelled out = balanced
[Done.](#)

P12L17 “...ice sheet loses contact with the ocean.”
[Done.](#)

P12L17 resolution = grid resolution
[Done.](#)

P12L28 “considerably large”. What does this mean? Be more specific!

Done. We add "... their temperature variation is considerably large".

Figures:

We apologize the bad quality of the figures. After a major storm in northern Germany two days prior to submission our computing cluster had power failure due to fire and we were unable to update the figures at the time of submission.

Figure 1: Can you make the line for 1.5°C bold to aid visibility when the models pass this threshold?

We added a dashed bold line.

Figure 3: Question mark before "C" symbol in Figure. Colour bar is too small. As it is the same magnitude for all panels one big colour bar should suffice.

Figure will be updated as suggested.

Figure 4: See comments for Figure 3

Figure will be updated as suggested.

Figure 6: Again, use one colour bar per panel. Also, please have colour bar labels on the same side of the colour bar and avoid overlap of axes labels with main Figure. Please align top and bottom panels properly.

Figure will be updated as suggested.

Figure 7: Again bigger axes labels and legends.

Figure will be updated as suggested.

Figures 8 and 10: See comments for Figure 3

Figure will be updated as suggested.

References (only new ones compared to the manuscript)

Lange, S.: Bias correction of surface downwelling longwave and shortwave radiation for the EWEMBI dataset, *Earth Syst. Dynam. Discuss.*, <https://doi.org/10.5194/esd-2017-81>, in review, 2017.

Meinshausen, M.; Smith, S. J.; Calvin, K.; Daniel, J. S.; Kainuma, M. L. T.; Lamarque, J.-F.; Matsumoto, K.; Montzka, S. A.; Raper, S. C. B.; Riahi, K.; Thomson, A.; Velders, G. J. M. & van Vuuren, D. P., The RCP greenhouse gas concentrations and their extensions from 1765 to 2300, *Climatic Change*, **2011**, *109*, 213