

We would like to thank again the reviewers for their helpful comments. We reply to each individual comment in the following.

Anonymous Referee #1

I thank the authors for their thoughtful consideration of the comments and the improvements they propose to make to the paper. I appreciate the perspective they have added in the introduction, and the clarification of the decomposition into SMB and dynamic contributions. I have a few more comments on their new draft which they may wish to consider.

page 1 line 16-17. "and is now larger than"

Done.

page 4 line 6. If I've understood this correctly, you call them "iterations" because you do them sequentially. "Iteration" usually has the idea of repeating the procedure but starting from where the previous iteration ended, whereas your "iterations" all start from the same state. Therefore I find "iteration" slightly misleading. You could call them an ensemble, except that you also use the final state to modify the next "iteration". Perhaps it would be better to call them something like "short transient experiments", which distinguishes them from the long equilibrium experiments.

The procedure intends to infer the basal drag coefficient that leads to the best match with observed present-day ice thickness. After each short transient experiments we update the basal drag coefficient, starting from an initial guess. With these updates, we are converging towards the minimal ice thickness error with respect to the observations (reachable to our model given the forcings and initial state). It seems that this is in line with your definition of an iterative procedure (the basal drag coefficient is updated at each iteration). What might be confusing perhaps is that the thermal state and the geometry are not changed from one iteration to an other. This is because they are not the variables that we intend to infer during this step.

The procedure also includes a simple way to compute the initial thermal state as well (long simulation with fixed geometry). Here again, it is iterative since from one long iteration to an other the temperature is updated.

We slightly rephrase a sentence:

“Each 200-yr iteration uses the exact same initial condition for the ice thickness and temperature but have a different initial basal drag coefficient.”

page 6 line 16. "Instead" doesn't sound quite right to me. You don't mean a replacement, but a comparison. I would say, "On the other hand" or "By contrast".

We now use “By contrast”, thanks for the suggestion.

page 8 line 19. I don't think it's correct that "CMIP6 models show generally a much larger" climate sensitivity than CMIP5. There are certainly some models which a considerable larger sensitivity, but the ranges given on the following lines for ISMIP6 are not representative of CMIP6 in general.

We agree that the subset used for ISMIP6 is not necessarily representative for the whole ensemble.

We simplified to:

“The CMIP6 models used in ISMIP6-Greenland have an Earth climate sensitivity from 4.8 to 5.3 °K, i.e. larger than the CMIP5 models used here, which show a range from 2.7 to 4.6 °K (Meehl et al., 2020).”

page 8 lines 20-21. Units of climate sensitivity are K.

Corrected, thanks for noticing.

page 10 line 27. I think you mean it's the same range of colors, don't you? I would not say it's the same color scale; it's an order of magnitude different, which is the point you are making, if I understand correctly.

In fact the colour scale is mirrored, i.e. opposite values and invert colour gradient, since the dynamical contribution is generally negative (resp. positive) where the ice thickness change is positive (resp. negative). For positive values, the dynamical contribution is one order of magnitude larger than the ice thickness change, when it is one one order of magnitude lower for negative values. We corrected the sentence:

“[...] we show the ice thickness change in 2100 with a similar colour scale (opposite values and invert colour gradient) in Fig. 10c.

page 11 line 9. I would say "inaccurate" rather than "unjustified", which is unfair to yourselves. It was a justifiable choice, on grounds of simplicity.

Thanks, corrected.

Fig 11. It would be helpful if you could put a % change on the right-hand axis of these figures.

We are not sure of what you want us to do. It seems rather unusual to put the unit of an axis variable at the end of this axis. The units (here %) are specified with the axis label for all the plots.

page 12 line 2-15. This point seems important to me and perhaps deserves to be mentioned in the conclusions and maybe the abstract.

The last sentence of our conclusion now reads:

“Finally, the initial condition chosen for the ice sheet model remains an important topic for ice sheet modelling. In particular, assuming an ice sheet in equilibrium with present-day climate for the initial condition, as done here but also in most ISMIP6 participating model, could lead to an underestimation of the future mass loss.”

page 12 line 5. loosing -> losing (you can't be blamed for surprising English spellings)

Thanks, corrected.

page 13 line 18. loses -> loses (same comment)

Corrected.

page 13 line 17-27. As I commented last time, I think you could draw attention in the conclusions to some more of the findings from the large range of experiments you are able to carry out (as well as adding the point of the first paragraph of page 12). You could state more clearly that the dynamical contribution is generally much smaller (by an order of magnitude) than the SMB contribution (which is implied by the final sentence of the conclusions, but clearer earlier in the text), that

consequently the uncertainty in ocean forcing has a relatively small effect on the spread of projections, which mostly comes from the SMB, and that the basal drag is rather poorly constrained but doesn't much affect the projections to 2100. Perhaps some of these might be added in the abstract too.

We have followed your suggestions and expanded the conclusion section with the following:

“The oceanic forcing contributes to ice loss by about 10~mm~SLE in 2100. In addition, the time integral of the surface mass balance is generally much larger than the dynamical contribution to ice thickness change (by an order of magnitude). This suggests that the Greenland ice mass loss in the future is mostly driven by surface mass balance changes, in particular through a larger ablation at the ice sheet margin. This process should thus be carefully implemented in ice sheet models aiming at simulating the Greenland ice sheet evolution at the century scale. With additional sensitivity experiments, not included in ISMIP6, we have also shown that the choice of uncertain mechanical parameters (i.e. flow enhancement factor and basal drag coefficient) has only a small impact on the spread of mass loss. Finally, the initial condition chosen for the ice sheet model remains an important question for ice sheet modelling. In particular, assuming an ice sheet in equilibrium with present-day climate for the initial condition, as done here and in most ISMIP6 participating models, could lead to an underestimation of the future mass loss. ”

In addition, the end of the abstract now reads:

“The oceanic forcing contributes to about 10~mm~SLE in 2100 in our simulations. In addition, the dynamical contribution to ice thickness change is small compared to the impact of surface mass balance. This suggests that mass loss is mostly driven by atmospheric warming and associated ablation at the ice sheet margin. With additional sensitivity experiments we also show that the spread in mass loss is only weakly affected by the choice of the ice sheet model mechanical parameters.”

Anonymous Referee #2

Overall, I think that the authors have done a reasonably good job revising the paper, and I only have some remaining issues.

Most importantly, I take it from the authors' reply that they have not followed my suggestion that "the entire manuscript should be very carefully proofread by a (near-) native speaker or a professional language editing service." As a result, there are still a considerable number of language issues, especially in the newly written parts. I am aware that, as the authors write, "The Cryosphere journal includes a language editing service for all accepted manuscripts". However, in my understanding, this service must not serve as an excuse for taking a relaxed stance on the quality of the writing. Just being "generally understandable" is not sufficient! Rather, authors of papers in high-quality journals like The Cryosphere should make every effort to deliver their contributions in a near-perfect condition.

We understand. However, we do not have the fundings in our projects to use a professional language editing service for our papers. Some native speaker colleagues offer some help occasionally, but it cannot be on a regular basis since it is time consuming. As such we are somehow limited by our English skills.

Below, I will suggest some corrections (the majority of all comments). However, I neither have the time nor the motivation to proofread the entire manuscript carefully (especially because I'm a non-native speaker either...), so that these corrections have no claim for completeness.

Thank you very much for your valuable suggestions.

Page/line numbers refer to the diff version of the manuscript that came attached to the response letter.

P. 2, l. 28:

"(CMIP6 forcing, separate effects of the atmospheric and oceanic forcings)"

Done.

P. 3, l. 26: "ice sheet model_s_"

Corrected.

P. 3, l. 26:

This sounds as if the flow enhancement factor favours only selected deformation modes., which is wrong. Rather "factor that increases the ice fluidity in the SIA".

Thanks for the suggestion.

P. 4, l. 9: "consists _of_ finding"

Corrected.

P. 4, l. 27: "Greenland_'s_ floating ice tongues, _the_ sub-shelf melting rate"

Corrected.

P. 5, l. 5: Delete "in the atmospheric model"

Done.

P. 7, l. 12: Unit of "about 0.55"? Log of velocities in m yr⁻¹, I suppose?

Correct. Precision added.

P. 7, l. 18: "the _south-eastern_ glaciers"

Corrected.

P. 7, l. 19: "small_,_ meaning"

Corrected.

P. 8, l. 14:

"sharp inflexion" sounds strange either. What about "we can not discern any sudden change ... century that may indicate a tipping point".

Again, thank you for your suggestion.

P. 8, l. 16: "forcing_s_"

Corrected.

P. 8, l. 16: "mass balance _becoming_ negative"

Added.

P. 8, l. 17: "2060_,_ while"

Added.

P. 8, l. 23: "_the_ SSP585 scenario"

Corrected.

P. 8, l. 26:

The authors may consider comparing their findings re CMIP5 vs. CMIP6 to those described in Sect. 4.2 of the Technical Report by Greve et al. (2020, Zenodo, <https://doi.org/10.5281/zenodo.3971251>).

Thanks for the suggestion. We have added:

This has also been reported by Greve et al. (2020) where the use of the CMIP6 model ensemble under the SSP585 leads to an ice sheet contribution to sea level rise increased by at least 70 % with respect to the contribution simulated using the CMIP5 ensemble.

P. 8, l. 32: "mass loss _compared_ to the"

Corrected.

P. 9, l. 1: "_compared_ to the"

Corrected.

P. 10, l. 3: "of the _combined_ forcing"

Corrected.

P. 10, l. 15: "integrated _divergence of the ice flux_ can be"

Corrected.

P. 10, l. 21: Delete "(ice flux convergence)" [unnecessary duplication]

Done.

P. 10, l. 22: Delete "(ice flux convergence)"

Done.

P. 10, l. 26: "change_,_ we show"

Added.

P. 10, l. 27: "values_,_ suggesting"

Added.

P. 11, l. 5: "south-eastern and central western regions"

Corrected.

P. 11, l. 27: "_for_ the control experiment"

Corrected.

P. 12, l. 3: "optimally tune_s_"

Corrected.

P. 12, l. 4: "with _the_ present-day"

Corrected.

P. 12, l. 6: "by construction, our simulations"

Corrected.

P. 12, l. 7-9: 6.5 cm is not "large" compared to 3.5-14 cm. Rather "it is comparable to the spread"?

You are right. This sentence now reads:

“This number is comparable to the GRISLI spread discussed in this paper, and more generally to the spread amongst ISMIP6 models (3.5 to 14 cm SLE, Goelzer et al., 2020)”

P. 12, l. 10: "largest source_s_ of uncertainty"

Corrected.

P. 12, l. 11: "_an_ ice sheet at equilibrium"

Corrected.

P. 12, l. 19: Not only diffusion. Except for the near-basal parts of an ice sheet, downward advection is actually more efficient. Perhaps more generally "since the information|memory of the low temperatures of the glacial period in the ice sheet"?

Right. We have followed your suggestion, thanks.

P. 12, l. 31: "ISMIP6-Greenland _accounts_ for the vertical"

Changed.

P. 12, l. 34: "under _the_ RCP8.5 scenario"

Added.

P. 13, l. 1: "smaller compared to"

Corrected.

P. 13, l. 2: "respectively)_,_"

Added.

P. 13, l. 3: "smaller _than that of_ Vizcaino"

Corrected.

P. 20, Fig. 2, caption: "_with respect to_ the end"

Corrected.

P. 21, Fig. 3, caption: "for _the_ velocity difference_s_"

Corrected.