Responses to Reviewers' Comments for Manuscript EGUSPHERE-2024-2223

Modeled Greenland Ice Sheet evolution constrained by ice-core-derived Holocene elevation histories

Addressed Comments for Publication to

The Cryosphere

by

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Authors' Response to Referee 2

General Comments. This manuscript presents simulations of the Greenland ice sheet spanning the last 20,000 years, using elevation histories derived from ice cores as constraints. 20 model parameters were systematically tested using Bayesian inference on an ensemble of 841 simulations. Aside from a glacial spin-up, all simulations use PISM at 10km resolution, which is adequate for this task. The main results is a set of model parameters that was constrained by time-varying reconstructions and the conclusion that a good fit is not possible without allowing the ice sheet to bridge over Nares Strait and connect to the Innuitian ice sheet.

I think this study is timely and highly relevant, even if the data used as constraint is not new (Vinther et al., 2009) and more comprehensive datasets exist. I come back to this point in my comments below. Overall, the manuscript presents work of high quality and the presentation, text and figures, is very good. I agree with the conclusion that long-term, transient trends in ice volume should be considered to accurately and reliably project future mass loss and sea level contribution. This work is a big step in this direction. However, similar points have previously been made MacGregor et al. (2016, doi: 10.1126/science.aab1702) who also point out the importance of the ice bridge across Nares Strait. The manuscript should reference this earlier work.

My criticism is best summarized in four major comments:

Response: Thank you for your feedback and interest in our work.

We will make sure to reference MacGregor et al. (2016) and their work on GrIS's multimillennial-scale response to a collapsed ice bridge across the Nares Strait.

We have carefully addressed all the issues item by item as follows.

Comment 1

1) Use of idealized climate

The simulations use one of five spatially uniform temperature anomalies. Precipitation is based on these anomalies and modified with a simple meridional gradient. I noted, after(!) my own reading, the extensive comments on this issue in the discussions section and will therefore keep my comment brief. However, I believe that a more in-depth discussion of this approach and its implications is needed. For example, most parameter estimates agree within their uncertainty (Table 1), but E_SIA is clearly different for the northern and southern core sites (line 212ff). I believe that this is a symptom of the model fighting systematic bias in the boundary conditions, possibly the climate.

Response: Thank you for the comment.

The uniform temperature anomalies and meridional gradient in the precipitation scaling is indeed an idealization of the climatic conditions. The choice of using the uniform temperature anomalies was to align with the assumption of Vinther et al. (2009) that local temperature offsets were due to a Greenland wide temperature anomaly and local elevation feedback.

We agree that the differences in the estimated model parameters could be a symptom of the model fighting systematic bias. We believe that if the model is fighting systematic biases in the climate it is reflected in the differences in the inferred forcing parameters and optimal temperature anomaly at the site.

The discrepancy between the estimates of E_SIA could also be due to the model fighting systematic biases in the supplied boundary conditions, but we find it more likely to be due to the models inability to correctly model the dynamics as mentioned in section 6.5.

In the revised version of the paper we will include a discussion on the use of uniform temperature anomalies in section 6.6, and how this simplification could possibly relate to the estimate of E_SIA and other model parameters.

Comment 2

2) Comparison with more recent data

I think a revised version of the manuscript should include a comparison with the independent dataset by Leger et al. 2024.

Response: Thank you for the comment.

We think this is a very good idea as getting the timing of the retreat right is the direction in which we think our simulations could improve the most.

In the revised version of the paper we will include a figure showing a comparison between the modeled extent and the extent by Leger et al. (2024).

Comment 3

3) Unclear bedrock optimization

I am not sure if I understood the bedrock optimization routine correctly. Is it correct that it was only performed with one single set of parameters? The modern bedrock topography of the best fit simulation shows a substantially larger deviation from observations (Fig. 11b) and a higher RMSE than the simulation used to initialize the ensemble. Why is this the case and what implications does that have?

Response: Thank you for the comment.

Yes, the bedrock optimization is only done for one set of parameters and at 20 km resolution. It was done because the deviation between the modeled present-day bed topography deviated substantially from the observed one. The modeled bedrock topography deviates from the observed because the parameters are changed and the resolution increased. Ideally the bedrock optimization should be done for each member of the ensemble but this would computationally much more demanding. Updating the bedrock does effect the modeled elevation histories.

In the revised version of the paper we will clarify the bedrock optimization routine described in section 3.5 and include a figure showing the effect of updating the bedrock topography. See further comments to reviewer 1.

Comment 4

4) Conclusions on ice bridge

The two simulations that restrict the GrIS from advancing beyond the present-day coastline or the ECS mask were run with the same parameters as the best fit simulation from the ensemble without any spatial restrictions (line 202f). Can strong conclusions be drawn from such a setup? How can you exclude the possibility that a different climate is compatible with the ice core constraints without the need to limit the ice extent?

Response: Thank you for the comment.

We cannot exclude the possibility that a different climate forcing and different model parameters would be compatible with the surface elevation histories. However, two simulations serve to show the effect of not allowing the ice sheet to advance beyond the present-day land mask or the continental shelf. Neither simulation has an ice sheet that is thick enough at Camp Century before the onset of the Holocene. While this might be remedied by decreasing the viscosity or increasing the precipitation, this would be modeling the correct elevation for the wrong reason as the geological evidence shows that the ice bridge existed. Thus, our simulations propose that the ice bridge formed.

> In the revised version of the paper we will make sure to include this consideration in the discussion, and to clarify the implication of our findings.

Minor comments:

Comment 5

equations 1 and 3: It is not clear how the latitudes were chosen and why they are different.

Response: Thank you for the comment.

The latitudes in equation 1 were chosen to cover most of Greenland, while the latitudes of equation 3 were chosen to be the same as those used by Aschwanden et al. (2019) who used this parametrization based on observations along the western coast of Greenland.

> In the revised version of the paper we will justify the choices of latitudes.

Comment 6

l 140: Please include an explanation why E_SIA and n_SSA are varied but not E_SSA and n_SIA.

Response: Thank you for the comment.

We follow Aschwanden et al. (2022) in the choice of varying E_SIA and n_SSA and not E_SSA and n_SIA as PISM is optimized for n_SIA=3.

In the revised version of the paper we will clarify why we vary E_SIA and n_SSA and not E_SSA and n_SIA

Comment 7

l 221: I think this should read "The northern 'precipitation' parameter, not 'accumulation'.

Response: Thank you for the comment.

In the revised version of the paper we will change all instances of 'accumulation parameter' to 'precipitation parameter'

Comment 8

l 289f: "[..] modeled bedrock topography is very sensitive to the history of the ice load." Please see my comment #3 above.

Response: Thank you for the comment.

As written in response to your earlier comment, we will clarify the bedrock optimization routine described in section 3.5 and include a figure showing the effect of updating the bedrock topography.

Comment 9

section 3 could be more explicit about the model resolution.

Response: Thank you for the comment.

In the revised version of the paper we will make sure to state the model resolutions used in the beginning of section 3.

Comment 10

section 6.6 needs to discuss the implications of using a uniform temperature anomaly forcing, maybe after presenting the findings by Lecavalier et al. (2017) (line 362). Please consider the comment by Jessica Badgeley.

Response: Thank you for the comment.

In the revised version of the paper we will include a discussion on the use of uniform temperature anomalies.

Comment 11

l 355f: I think it is too strong to state that a full coupling of ice sheets to atmosphere and ocean is the only alternative to including spatially non-uniform climate forcing.

Response: Thank you for the comment.

That is correct. There are cheaper options available as also made clear by the other referee.

In the revised version of the paper we will mention this in the discussion in section 6.6.

Comment 12

figure 5 (and others): The line colors, in particular the two shades of blue, are difficult to distinguish.

Response: Thank you for the comment.

The colors used in our figures are taken from the color palette described in Wong, B., 'Points of View: Color Blindness,' Nature Methods 8, 441 (2011). https://doi.org/10.1038/nmeth.1618. This palette offers good variability in lightness, saturation, and hue, making the colors easily distinguishable by individuals with red-green color blindness.

> In the revised version of the paper we will change the sky blue to vermillion in Fig. 5