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 Community comment 4

General Comments. This study contributes to the ongoing and necessary efforts to use more data, and a greater diversity of data, to inform ice sheet models. Data constraints are sparse far from the margins of ice sheets. It is therefore especially important to leverage interior data, such as ice core measurements, as has been done in this study. That said, there are many challenges in doing this, some of which I think could be addressed or discussed more thoroughly in this manuscript.

I would be happy to discuss any of this in further detail. Thanks again for your contribution to this rapidly evolving field.

Jessica Badgeley

Response:

Dear Jessica Badgeley,

Thank you for your comments and interest in our paper.

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

Comment 1

Lines 53-59 and 305-320: Estimating elevation from paleotemperature records is sensitive to the choice of lapse rate and any atmospheric circulation changes that occur at the same time as the elevation change (e.g., Forest, 1995; Meyer, 2007; Badgeley et al., 2022). Vinther et al. (2009) discuss this, but my understanding is that neither of these known uncertainties are quantified or included in their estimate of the elevation history uncertainty. If this is correct, are these additional uncertainties quantifiable? How might they impact the findings of this study?

Response: Thank you for the comment.

The uncertainty of the surface elevation histories by Vinther et al. (2009) propagates from the spread of O-18 values of two parallel records from the Agassiz ice cap and the uncertainty in bedrock uplift at the Agassiz and Renland ice cap locations. The surface elevation estimates of Vinther et al. (2009) do indeed use a constant lapse rate of -0.6‰ per 100 m which directly translate the O-18 signal into an elevation signal. The uncertainty in this value is not estimated and introducing an uncertainty would increase the spread of the surface elevation histories and in turn our inferred probability density functions.

In the revised version of the paper we will make sure to include these considerations in our discussion in line 178 after introducing the likelihood function.

Comment 2

Lines 60-70: Do the temperature histories from Nielsen et al. (2018) subtract out the impact of elevation change? If so, it would be helpful to state this. If not, then there appear to be signals in the d18O/temperature records that are being double counted in this study because the Nielsen et al. (2018) temperature anomalies are applied at a constant (modern) elevation. By "double counting" I mean that, in the ice core data, a part of the d18O signals is attributed to elevation change, while in the SMB forcing this same part is attributed to temperature change at a constant elevation.

Response: Thank you for the comment.

Only the temperature reconstruction of Vinther et al. (2009) subtracts the impact of elevation change, but the other reconstructions were calibrated to measured bore hole temperatures or to the measured isotope diffusion record, thereby avoiding assumptions of the isotope to temperature relation. We choose to also sample over the other reconstructions used by Nielsen et al. (2018) to cover the historical temperature uncertainty, despite the limitations in these records.

In the revised version of the paper we will make sure to state that only the temperature reconstruction by Vinther et al. (2009) is corrected for elevation changes in lines 60-70 when introducing the temperature anomalies.

Comment 3

Lines 114-121: There are ice-core informed Greenland paleoclimate reconstructions that provide spatially variable histories of temperature and precipitation (e.g., TraCE-21ka – Liu et al., 2009 and He et al., 2013; Buizert et al., 2018; Badgeley et al., 2020). These reconstructions make it unnecessary to apply a constant temperature scaling or to scale precipitation from temperature. Though these studies show different spatial patterns of temperature change over Greenland, they all show that there is not a single, spatially constant temperature scaling.

Response: Thank you for the comment.

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.

The TraCE-21K climate simulations use the ice sheet configuration of ICE-5G which is not consistent with our simulated ice sheet and does not include an ice shelf covering Baffin Bay. Placing too high confidence in these products introduces a range of additional uncertainties and assumptions. When making a reconstruction of the Holocene evolution, we prefer to keep our method simple, dependent of as few parameters as possible, and independent of any previous reconstructions to avoid any circular conclusions.

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

Comment 4

Lines 178-179: I think this statement needs more explanation or justification. Why not assign a lower uncertainty to present-day data since it is much more certain than the paleo constraints? Separately, why not include other modern and paleo data, such as satellite data and exposure ages of moraines? These datasets have been used before (e.g., Briggs and Tarasov, 2014; Briner et al., 2020) and would provide greater constraints on the model parameters.

Response: Thank you for the comment.

Including present-day observables would improve confidence in the present-day state. However, our focus is on testing the feasibility of using the past surface elevation history to understand how prognostic (future) simulations might be biased by the long-term response of the GrIS to past climatic changes. To judge this, we argue that using this data in isolation makes our results most clear. We therefore also choose to assign equal uncertainty to all points in time as we are interested in modeling the transient response to the past climatic changes and our aim is not to capture the exact present-day state.

Including the exposure ages of the moraines would indeed increase our confidence in this transient response. However, our simulations are systematically late in modeling the lateral retreat. Combining the interior surface elevation records with the exposure ages of the moraines would be a next step.

> In the revised version of the paper we will clarify why we only use the surface elevation histories as constraints in our Bayesian inference and why we assign equal uncertainty to all points in time.

Comment 5

> Lines 202-206: The conclusions drawn from the "restricted to GrIS" and "restricted to ECS" models may be correct, but I believe the comparison of these simulations to the others is unfair. The best-fit parameters for the main model will not necessarily be the best for either of the restricted models. If running a separate parameter calibration for each restricted model is beyond the scope of this study, then, at a minimum, it would be useful to use a smaller ensemble to determine whether the RMSEs for the different parameter combinations correlate across the three models. If they do, then perhaps the conclusions stated in lines 202-206 are justified by the current set of simulations.

Response: Thank you for the comment.

We don't claim that the best-fit parameters for the main model will be the best for either of the restricted models. The point is simply to show the effect of allowing the ice sheet to advance beyond the present-day land mask and form an ice bridge to the IIS. Neither of the restricted simulations 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.

> We will make sure to state that best-fit parameters for the main model will not necessarily be the best for either of the restricted models.

Comment 6

Figure A2: I find the color scheme for this figure to be counterintuitive. Standard practice in my experience is that colder temperatures are shown in blue, warmer temperatures in red, and greater precipitation rates in darker blues.

Response: Thank you for the comment.

We agree that the color schemes can be counterintuitive and will change these.

> In the revised version of the paper the colormaps in Figure A2 will be changed.