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 1

General Comments. Thank you for submitting your revised manuscript. You added content to address the comments of the reviewers. However, I still find many places where the manuscript can be significantly improved. Moreover, there is still an important point regarding the explanation of the elevation reduction at CC that is not convincingly treated. After reading your revised manuscript, it became clear that this is more problematic than I recognized in my initial assessment and should be treated carefully. Below I list this, a few other important points, and then several more specific comments. Please address these comments in a revised submission.

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

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

Comment 1

Missing explanation for ice-thickness reduction

You make the argument that the focus of this work is to reproduce the elevation histories at the ice-core locations, and therefore the poor margin-retreat timing is not relevant here. This is potentially a reasonable idea, but in practice, it is not very credible given that you specifically relate a large part of the elevation change at CC to growth of the ice bridge across Nares Strait. For the growth of the ice bridge, this is convincingly treated, and the tests with restricted ice sheets support this argument nicely.

For the retreat, however, you treat this sparingly (L393-396), and this needs much more clarification. It is, at first, wholly unclear how it is possible obtain such good agreement in timing with the elevation reduction at CC, when the ice bridge does not collapse until after -7 ka. One would expect the collapse of the ice bridge to drive the elevation reduction. Nor does "reducing buttressing" make sense since the large ice shelf in Baffin Bay remains until about -7 ka too. This point was noted by Reviewer 1 too.

But I think the SI movie shows what is happening and I believe this deserves much more attention in the results and discussion, because it is very interesting. Namely, just before -9 ka, a very large paleo ice stream forms inland from Baffin Bay and accelerates ice flow all along the northwest coast (this can also be seen comparing Fig. 6a and 6b). It would appear this is the primary reason you can match the elevation reduction timing, and it is quite novel. Furthermore it links nicely with the recent work of Tabone et al (2024) - disclaimer I am a coauthor - who link elevation change to an acceleration of the paleo NEGIS.

Therefore I would propose you analyze and describe the formation of this ice stream and its branches. What triggers its growth? Is ice becoming temperate at the base as it gets thicker and insulated, while the climate also warms? Or, is ice just getting thinner until causing widespread grounding-line retreat, accelerating flow? Did the southern branch of the ice-stream also affect NGRIP (complementing/contrasting with the results of Tabone et al.)? Could there be evidence for such an extensive ice stream? It seems that such an ice stream could only form with the large growth of Nares strait too, so it supports the general message of the paper - that simulating this feature is important.

Perhaps I am wrong, and there is another explanation, but then you still need to convincingly explain how the rapid elevation reduction occurs.

Response: Thank you for the comment.

Thank you for your comments on this, in particular the remarks about the paleo–ice stream in Baffin Bay and its effect on Camp Century thinning. We agree that this provides a convincing explanation for the timing of thinning at Camp Century, and we appreciate you pointing this out.

To accommodate this, we have revised the order of subsections in the Discussion and rewritten the first two subsections. The first subsection now provides a consistent explanation for the two stages of interior thinning, attributed to the formation of the paleo–ice stream and the subsequent collapse of the ice bridge, respectively. We clarify where our model results agree with other data and where they differ, and we explain why our model can reproduce the timing of thinning at Camp Century, even though the retreat timing at the margin differs. We describe how the paleo-ice stream forms as surface melting sets in and the Baffin Bay ice shelf breaks up. We further describe how it is linked to the bedrock topography along the coast. We have also added a reference to Tabone et al. (2024). Overall, we find that this explanation is now significantly improved.

The second subsection has also been revised. It now presents a more detailed discussion of the Holocene evolution of ice volume and spatial patterns, and concludes with a brief discussion of the importance of paleo calibration for constraining modern mass loss rates.

Comment 2

The section "2 Paleoclimatic evidence" does not really belong as it is now. The title is not really clear - evidence of what? The elevation histories were introduced earlier, as were temperature reconstructions. I would propose the following: - Move the content of the first paragraph L58-64 into the Introduction, probably around L22 when "temperature reconstructions vary by several degrees" is stated. - Make a new subsection within the section "3 Model setup", which would be "3.1 Atmospheric forcing". Move the second paragraph there to describe the temperature forcing used. Additionally move the temperature information and precipitation scaling information from "Surface mass balance" to "Atmospheric forcing", for more consistency.

Response: Thank you for the comment.

We agree that it is better to divide Section 2 into the Introduction and the Model Setup sections.

We have incorporated lines 58–64 into the part of the Introduction that introduces the elevation histories. The part describing the temperature forcing has been merged with the "Surface mass balance" section which is now titled "Atmospheric forcing" for improved consistency as suggested.

Comment 3

I acknowledge the simplicity of the model setup here and I appreciate it. However, I think we are now confident that the PDD model lacks realism when insolation changes significantly (see e.g. Robinson and Goelzer, 2014). It is clear that during the early Holocene particularly at high-latitude sites like CC, the additional contribution to melting from increased insolation would be important. There are now multiple approaches available to incorporate insolation forcing, including the dEBM-simple model that is part of the PISM package. You should acknowledge this deficit in the "Surface mass balance" section, and include a paragraph about the possible implications of accounting for insolation changes on melting in the Discussion.

Response: Thank you for the comment.

We agree that the PDD model definitely lacks realism, particularly when insolation changes significantly.

We have acknowledged this in the new section, "Atmospheric Forcing" and include a paragraph about the insolation changes in the "Climatic forcing" section of the discussion.

Comment 4

Note: a hyphen should generally be used when a compound noun becomes and adjective, so "ice sheet" versus "ice-sheet model". I noted several instances below, but please try to check the manuscript throughout.

L2: Ice sheet reconstructions → Ice-sheet reconstructions

L12: ice sheet evolution → ice-sheet evolution

L19: an abrupt warming → abrupt warming

L28: ice sheet model studies → ice-sheet model studies

L32: ice sheet model → ice-sheet model

L34: long term response → long-term response

L39: Before the satellite era → To simulate time periods before the satellite era,

L39: ice sheet modeling → ice-sheet modeling [further instances not noted here]

L43: and significantly → and could significantly

L51: key model parameters → influential model parameters

Response: Thank you for the comment.

We have tried our best to hyphenate all the compound nouns and incorporated all the proposed changes.

Comment 5

L17: Please replace reference to Gulev et al., 2021 with direct reference(s) to support the given range.

Response: Thank you for the comment.

We have replaced the IPCC reference with Lambeck et al. (2014) and Yokoyama et al. (2018)

Comment 6

L22: "which can significantly affect the modeled GrIS" $\leftarrow$ You have not yet introduced modeling. Please try to rephrase here.

Response: Thank you for the comment.

This line has been rephrased.

Comment 7

L109: "We use two constants of proportionality" $\leftarrow$ These should be accompanied by an equation, otherwise it is not clear what they refer to. As they are parameters tested in the ensemble, this is especially relevant.

Response: Thank you for the comment.

We have added equation for the surface melt in the new section "Atmospheric Forcing".

Comment 8

L113: "We looked at three areas for rainfall data" $\leftarrow$ This sentence is not clear to me at all. I understand both temperature and precipitation are obtained from the RACMO simulations. Why is "rainfall data" needed in specific regions? Furthermore, should this be precipitation, or specifically rainfall? If the latter, what is its relevance? Please make this paragraph more clear on these points.

Response: Thank you for the comment.

This is indeed a blunder. It should be precipitation and not rainfall.

The sentence has been rephrased

Comment 9

L121: Explicitly define $\omega_\uparrow$ and $\omega_\downarrow$ as free parameters in the text, since these parameters are also modified in the ensemble.

Response: Thank you for the comment.

We have defined $\omega_\uparrow$ and $\omega_\downarrow$ as free parameters in the text.

Comment 10

L131, Eq. 3: Rather confusing that $\phi_\uparrow$ and $\phi\downarrow$ have the same name as in Eq. 1, but different values. Add a subscript "o" or similar to distinguish.

Response: Thank you for the comment.

We agree that this was confusing.

We have added superscripts "o" for ocean and "p" for precipitation. We have also added the "o" superscript to the ocean melt rate to distinguish it from the surface melt rate $\dot{m}^s$ We also changed the friction angle ϕ to φ to distinguish it from the latitude.

Comment 11

L145: I see that n_sia is fixed to 3, while n_ssa is modified. Changing n_ssa is not so easily done, however, without also adjusting the constant factor A (since its units must be proportional to n). See e.g. Zeitz et al. (2022), who do this in a consistent way using PISM. Is this the method that is used? If so, it should be cited. While it is true that Aschwanden and Brinkerhoff (2022), who you cite, also state that they vary this parameter, it is not explained there either how this is achieved, while maintaining consistency.

Response: Thank you for the comment.

Yes, n_SIA is fixed to 3 while n_SSA is modified. The numerical value of A is not changed, only the units are adjusted, which indeed modifies the viscosity for effective stresses different from 1 Pa. For an effective stress of 80 kPa, a change in n by 0.2 reduces the viscosity by approximately a factor of 10, corresponding to an enhancement factor of 10. We consider this not an inconsistency, but rather a deliberate modeling choice.

We have clarified that A is not varied.

Comment 12

Figure 2: I like this figure as a schematic to show what you did. But I think it could be improved if the red dashed line would point rather to the lower -20 ka dot, to show that this is an iterative loop. Then you could have a black dashed line going from PD to the upper -20 kyr dot, which indicates it is not part of the adjustment process, but rather now you move forward with your ensemble of simulations. This is only a suggestion, of course.

Response: Thank you for the suggestions

We have changed Fig. 2 according to your suggestions.

Comment 13

Figure 2, caption: the last glacial → the last glacial period

Response: Thank you for the comment.

This has been changed.

Comment 14

L153: Please rephrase slightly. The entire equation has been simplified, not just the reference pressure, as the additional term also included a dependence on W_till/W_till_max. Was there a particular reason to simplify the equation in this way?

Response: Thank you for the comment.

The original formulation of Bueler and van Pelt (2015) the effective reference pressure before being capped at P_0 is

$$\hat{N}_{\text{till}} = N_0 \left(\frac{\delta P_0}{N_0}\right)^s 10^{\left(\frac{e_0}{C_c}\right)(1-s)}, \tag{1}$$

which can be written as

$$\hat{N}_{\text{till}} = 10^{\left(\frac{e_0}{C_c}\right)} N_0 \left(\frac{\delta P_0}{10^{\left(\frac{e_0}{C_c}\right)} N_0}\right)^s \tag{2}$$

$$= \tilde{N}_0 \left(\frac{\delta P_0}{\tilde{N}_0}\right)^s, \tag{3}$$

where $\tilde{N}_0 = 10^{\left(\frac{e_0}{C_c}\right)} N_0$. This is what the effective pressure at zero void ratio or saturation would be if it was not capped at P_0. e_0 is the void ratio at the reference pressure N_0 and C_c is the till compressibility. I think that using $\tilde{N}_0$ makes it easier to read and the interested reader can then consult the literature.

We have rephrased the part of the manuscript describing the effective pressure to make it more readable

Comment 15

L157: Please justify these bedrock choices somewhat. Why are neither of these parameters considered in the ensemble? Arguably, they could have an impact on the transient evolution of the ice sheet's elevation history.

Response: Thank you for the comment.

Arguably we could have varied the viscosity and lithosphere flexural rigidity, we would then have had to adjust the bedrock individually for all ensemble members further adding to the computational requirements of the study. We mention this in the discussion section "Bedrock uplift".

Comment 16

L162: "Since we only know the bedrock elevation for the present day and not for the past" ← I think the application of this approach requires a bit more justification than this, especially given that you run Greenland transiently through the glacial cycle beforehand. You could arguably say the same thing about any number of the model choices made, but in some cases you include the parameters in the ensemble, whereas here the bedrock parameters are fixed. Why in particular should the errors associated with the bedrock approach be artificially reduced? Has this or a similar approach been used before? If so please include citation.

Response: Thank you for the comment.

You are right that the errors are indeed artificially reduced, and this should be better justified.

The reason for this approach is that the modeled ice extent, and consequently the surface elevation, is sensitive to ocean melt and sea level forcing. We apply an artificial correction to the bed topography to ensure that the present-day ocean mask aligns closely with observations. This correction improves our ability to reproduce the elevation change at Dye 3, which we were initially unable to match without it, as shown in Fig. A6.

Our approach is similar to the scheme used by van Calcar et al. (2023), whom we will cite.

> We have changed this section to better justify the need to artificially reduce the bedrock elevation error and to refer to van Calcar et al. (2023).

Comment 17

Figure 3: Panel e still shows the 3rd iteration bedrock difference, but I understood this would be replaced with that of the last iteration. Was this an oversight, or a different decision?

Response: Thank you for the comment.

This was indeed an oversight, we forgot to include it.

> Panel e of Fig. 3 now shows the last iteration.

Comment 18

L172: surface elevation → bedrock elevation

Response: Thank you for the comment.

This was correct. Fig. A7 shows the effect on the surface elevation to the bedrock changes

Comment 19

L174: 20 key parameters → 20 parameters

Response: Thank you for the comment.

We have changed this.

Comment 20

L184, Eq. 9: I think it would make more sense to introduce Eq. 11 first, but it is just a suggestion.

Response: Thank you for the comment.

We prefer to keep it this way.

Comment 21

L203, Eq. 12: Is this a standard approach? Perhaps define what it means in words. Also, I note the only place it appears later is at the end of Table 2 without any clarification.

Response: Thank you for the comment.

This is a standard approach for evaluating the efficiency of the sampling. The effective sample size is the number of equally weighted samples that would yield the same variance of the mean as the weighted set of samples. If only one ensemble member has a non-zero likelihood, the effective sample size is one. Conversely, if all members have the same likelihood, the effective sample size equals the actual sample size, namely 841.

We mention the effective sampling size in the discussion "Sampling technique".

We have tried to explain the meaning of the effective sampling size better. The subsection "Sampling technique" has been merged with "Inferred parameters"

Comment 22

L210-214: It is not clear to me from the labels what the difference is between the modeled "individual estimates" and the "combined estimated elevation". The elevations shown are specific to each site. So, does "individual estimates" refer to envelope of simulations that best match the elevation change at that specific ice core (while any mismatch with other ice cores is not accounted for), and the "combined estimated elevation" is then the envelope of simulations that best match the elevation changes of all cores together? If so, please try to rephrase here and in figures. Perhaps this could be something like "Site-specific pdf" and "Combined pdf", or even "Site-specific estimate" and "Combined estimate". Try to make the language precise and consistent between the two cases. And in place of "Ice-core-derived", I would simply put "Reconstruction".

Response: Thank you for the comment.

We agree that "site-specific pdf" and "Combined pdf" is clearer. We prefer however to keep the term "Ice-core-derived" to avoid confusing it with our "reconstruction"

We have changed the labels in the figure and rephrased the text.

Comment 23

L222: RMSEs to → RMSEs associated with

Response: Thank you for the comment.

This has been changed.

Comment 24

L228: "Notably, the estimated enhancement factor of the SIA, E_SIA, differs substantially between the sites." ← Try to improve the wording of sentences like this one, which is currently ambigious. Right now, it sounds like E_SIA is varying spatially with a different value at each ice-core site. I think what is meant is that the most likely enhancement factor value changes substantially depending on which site's elevation history is used as a target. This happens elsewhere, for example, in the very next two sentences too. Please check throughout.

Response: Thank you for the comment.

We agree that the wording was ambiguous.

We have changed the phrasing so it is clear that it is the site-specific estimates that are different and not the parameters that are varying spatially.

Comment 25

L272: Solgaard and Kusk (2023) ← Cite the paper where these data are published: https://essd.copernicus.org/articles/13/3491/2021/

Response: Thank you for the comment.

This has been changed.

Comment 26

L272-280: Please revise these paragraphs to ensure the figures support the text, as opposed to the text supporting the figures.

Response: Thank you for the comment.

We have revised the paragraphs to ensure that the figures support the text and not the other way around.

Furthermore, we have rearranged the figure panels such that the velocity difference, previously shown in Figure 10c, now appears in Figure 9c. Panels 10a and 10b have been removed, as they did not contribute meaningfully to the text. The isochrones that were previously in Figure 9c are now shown in Figure 10, alongside the isochrones from Leger et al. (2024).

Comment 27

Figure 6: Nice figure. It would be instructive to add a panel here showing the distribution at LGM as well, despite the focus of the paper being the Holocene. It is a reference point of interest that is discussed in the text. If you believe it doesn't fit here, then in the Appendix, perhaps together with Fig. A9, which is explicitly related to the LGM. Also, it would make sense to show the dot locations of the ice cores too.

Response: Thank you for the comment.

We have added the ice-core sites to Fig. 6.

We have added the ice sheet configuration at -20 ka to Fig. A9 which is already showing the configuration at -20 ka. This figure will then show the state at the branch-off point.

Comment 28

Figure 7: Please add points that correspond to the estimate of Leger et al. (2024) - see their Figure 16. An explicit discussion of this mismatch with your own should be added (see general comment above).

Response: Thank you for the comment.

Good idea.

We have added the points from Leger et al (2024) to Fig. 7

The discrepancy is now discussed in section 5.1

Comment 29

Fig. 9b: I think the ice deviations are incorrectly limited to the present-day border of the ice sheet. If there is a good reason to do so, this should be clarified in the caption.

Response: Thank you for the comment.

That is correct. There was no good reason for this.

We have plotted the thickness deviation for the entire domain.

Comment 30

L306: "historical calibration" ← Do you mean paleo calibration? Historical typically refers to the current period with direct observations.

Response: Thank you for the comment.

You are right.

We have changed the instances of historical to paleo where it made better sense.

Comment 31

L335: "Lecavalier et al. (2017) presented revised temperature anomalies for the Agassiz ice core that, in turn, increased the ice-core-derived surface elevation at CC by 400 m at the Holocene onset." ← Please then link this to your results. Is the Lecavalier et al. (2017) estimate not plausible and therefore not considered? Or it is plausible, as well as Vinther et al. (2009), and would imply X?

Response: Thank you for the comment.

The Lecavalier et al. (2017) estimate is also plausible and would imply that our model underestimates the elevation which might be remedied by increased precipitation.

We have revised this subsection to link Lecavalier's work to our results.

Comment 32

L337-340: It seems to me that this paragraph should be combined with the first one that also discusses the Vinther et al. (2009) methods and assumptions, and also O-18.

Response: Thank you for the comment.

The paragraphs have been combined.

Comment 33

L339: "The modeled O-18 should then be compared directly with the observed values at the time-dependent ice core site location." ← Same comment. Can you link this back to your work? E.g., Our work rests on the assumption that any potential changes in moisture sources did not materially impact the O-18 fractionation over the ice sheet. [And if you combine this as suggested with the first paragraph, you can still conclude with the comment about total gas content, which would seem to support this assumption.]

Response: Thank you for the comment.

This has been combined with the first paragraph linking it to our work and concluding with the comment on total gas content.

Comment 34

L382: "Once this issue is addressed" ← What issue are you referring to here? The late retreat of your simulations? Please be more explicit.

Response: Thank you for the comment.

It is the issue of having to much thinning at CC for ensemble members with earlier onset of ocean forcing.

We have revised the sentence to be more specific.

Comment 35

L446: Again, is "historical" meant here or "paleo"?

Response: Thank you for the comment.

We have changed this to "paleo".

Comment 36

L455: dynamical response → dynamic response

Response: Thank you for the comment.

We have changed this.

Comment 37

Figure A9: It would be valuable to add a panel which shows the anomaly in bedrock elevation w.r.t. present day to be able to understand more. Furthermore, I would even suggest to add a panel of the anomaly between this LGM bedrock elevation and that of a fully equilibrated bedrock to the LGM ice sheet load. This could be helpful in the discussion of your iterative approach and show how much you estimate the LGM bedrock was in disequilibrium at LGM.

Response: Thank you for the comment.

The modeled bedrock at -20 ka is very close to steady state. We have added a panel showing the uplift rates at -20 ka to show this.

We have added a panel showing the anomaly from 20 ka to present day and a panel showing the uplift rates in what is now Fig. A8.

References

Bueler, E. and W. van Pelt (June 2015). “Mass-Conserving Subglacial Hydrology in the Parallel Ice Sheet Model Version 0.6”. In: *Geoscientific Model Development* 8.6, pp. 1613–1635. DOI: 10.5194/gmd-8-1613-2015 (cit. on p. 10).

Lambeck, Kurt, Hélène Rouby, Anthony Purcell, Yiying Sun, and Malcolm Sambridge (Oct. 2014). “Sea Level and Global Ice Volumes from the Last Glacial Maximum to the Holocene”. In: *Proceedings of the National Academy of Sciences* 111.43, pp. 15296–15303. DOI: 10.1073/pnas.1411762111 (cit. on p. 6).

Tabone, Ilaria, Alexander Robinson, Marisa Montoya, and Jorge Alvarez-Solas (July 2024). “Holocene Thinning in Central Greenland Controlled by the Northeast Greenland Ice Stream”. In: *Nature Communications* 15.1, p. 6434. DOI: 10.1038/s41467-024-50772-5 (cit. on p. 4).

van Calcar, Caroline J., Roderik S. W. van de Wal, Bas Blank, Bas de Boer, and Wouter van der Wal (Sept. 2023). “Simulation of a Fully Coupled 3D Glacial Isostatic Adjustment – Ice Sheet Model for the Antarctic Ice Sheet over a Glacial Cycle”. In: *Geoscientific Model Development* 16.18, pp. 5473–5492. DOI: 10.5194/gmd-16-5473-2023 (cit. on p. 11).

Yokoyama, Yusuke, Tezer M. Esat, William G. Thompson, Alexander L. Thomas, Jody M. Webster, Yosuke Miyairi, Chikako Sawada, Takahiro Aze, Hiroyuki Matsuzaki, Jun’ichi Okuno, Stewart Fallon, Juan-Carlos Braga, Marc Humblet, Yasufumi Iryu, Donald C. Potts, Kazuhiko Fujita, Atsushi Suzuki, and Hironobu Kan (July 2018). “Rapid Glaciation and a Two-Step Sea Level Plunge into the Last Glacial Maximum”. In: *Nature* 559.7715, pp. 603–607. DOI: 10.1038/s41586-018-0335-4 (cit. on p. 6).