

## Review of egosphere-2024-1770, Bonsoms et al.

### Summary

This paper presents numerical model simulations of a glacier in West Greenland. The authors aim to recreate the maximum position of the Holocene ice, in an attempt to compare ice loss rates since this period of time to contemporary and potential future loss rates. This is an interesting line of enquiry, which utilises the geomorphological/geochronological record and combines it with newly developed GPU accelerated ice flow modelling in a reasonably novel way. However, there are several major shortcomings of this work, outlined below, which I think need addressing before publication. I hope these comments improve the paper, as I like the overall approach, but I believe the following needs to be addressed for purposes of rigour and because you have the opportunity to have a good paper here.

### Major comments

#### *Figures*

Firstly, and hopefully a quick fix, the figures are in general quite poorly designed and some of the figures are indecipherable. This makes it really difficult to follow the paper. The following needs to be addressed as a minimum:

- Fig 1 is ok, if a little crowded.
- Fig 2 is useful, but why are there green ticks and little symbols?
- Fig 3 and 4 ok, but it is never stated what A and c are in the text, or how they work within the model.
- Fig 5 is hard to see differences between the plots – perhaps anomaly plots against both reference datasets would work better?
- Fig 6, text too small.
- Fig 8 is indecipherable – where is the glacier, is the whole page glaciated?
- Fig 10b, what do the circles represent? Should these be points or crosses? The circle represents some sort of uncertainty?
- Fig 11. I can't see anything on here.

Perhaps odd to start a review with the, but the poor quality of the figures really hampered my ability to judge this paper.

#### *Approach*

As said in the summary, the overall line of enquiry is an interesting one. But, this could substantially be improved upon with some alterations to the approach.

The speed of IGM allows you to run numerous ensemble simulations, yet your results are mostly based upon one calibrated simulation. This prevents you from defining any uncertainty in your simulations, which given there is also uncertainty in the observations, diminishes the rigour of your approach. My suggestion would be to base the results on numerous “acceptable” simulations, i.e. those that fit within the uncertainty of the data. This is true for both the PDD factors and the ice flow factors. There is certainly large uncertainty within the PDD factors that would fit the geodetic mass balance observations. Multiple PDD factors could also lead to potentially multiple ice flow parameters that fit the data.

I get that the PDD calibration has to be conducted first in the experimental design, logistically making it difficult to combine the sampling of PDD and ice flow parameters, but ideally you would vary these together. Perhaps sequentially creating a distribution of acceptable PDD parameters then seeing which ice flow parameters fit the data based on that distribution is an acceptable way forward.

A and c are key to the above, yet their definition is never provided in the text. I assume A is part of the flow law, and c a sliding exponent/parameter, but the reader is left guessing. The equation that defines these both is necessary here.

The above would allow for a more Bayesian inference based approach to your results, which would give your study more statistical grounding.

You also have a static approach to climate downscaling. This limitation is sort of covered in the discussion, but given this source of uncertainty, it might be worth exploring a range of lapse rates for both temperature and precipitation.

The past climate also has uncertainty in the data that is not addressed or accounted for.

Finally, the assumption that modern-day parameters represent the past is acknowledged too late in the discussion. It's the final point. I think this needs to come in earlier as well.

#### *Overstating results from one glacier*

There are lots of GICs where the extent during different periods of time is well known (especially little ice age which is very clear in Greenland:

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL103950>

It is unclear to me, and potentially to other readers, why you chose this one glacier. It may have behaved differently to others. I am not saying you should therefore model more than one glacier, but statements like "glacier mass loss will double" in the abstract are hyperbole when based on one glacier. Mass loss of one glacier is projected to double is all you can say based on your work.

The title also needs to reflect this.

#### *Line-by-line comments*

L9. "projected future trends" The whole framing of this question could do with a little work.

L10-11. The way this sentence is phrased, I struggled to see what the "gap" was. Please simplify.

L13. The way this is written, I assumed you did the cosmogenic dating in this paper. Please clarify that this is from previous work.

L35. The glaciers aren't accelerating the mass loss themselves, they are displaying accelerated mass loss.

L73. I didn't understand this line. Please rephrase.

L189. It's not clear what the "shop module" is.

L235. This is where A and c need to be defined, with equations so we can understand their effect.

L237. What size is the ensemble? How were parameters sampled?

L256. I got confused here with the terminology. Is this an ensemble in the way it is commonly used (e.g. 100s-1000s of simulations with randomly perturbed parameters?) or a sensitivity test (systematically changing one variable?). You can do sensitivity analysis on an ensemble. Ensure your terminology is correct throughout.

Results and discussion: In my opinion, the above major flaws need to be considered regarding the approach. Which will likely require a major rewrite of this section.

L430-431. Just one example where you need to be clear that this is just one glacier. Please be consistent throughout.

L500. What are far isothermal conditions?

The paragraphs starting on line 559 and Line 572 are repeats. This repeated paragraph is problematic. Why is a figure not shown? Throughout there is a mix of statements pertaining to one glacier and glaciers in general.

L635. This statement on computational demands, parameters and limitations is a bit strange, your approach also has many of these limitations. I don't think a point is conveyed in this paragraph. I would consider removing or rewriting.

L676. GICs is not correct here. Just one.

Conclusions need a rewrite in light of the above.