Review of Keisling et al., «**An ice-sheet modelling framework for leveraging sub-ice drilling to assess sea level potential applied to Greenland** »

General comments

This study provides an interesting framework in which an ensemble of ice sheet model simulations is used to (1) estimate which regions of the Greenland Ice Sheet are most vulnerable, and (2) assess which of the uncertain forcings (climate, solid-Earth rebound, etc) and boundary conditions (initial ice sheet geometry) dominate the simulated ice sheet retreat uncertainty in particular regions.

The manuscript is very well written, proposes a novel framework, and is an interesting read. I think that the result can be interesting for a wider community than the submitted title (for example) suggest. Overall, the work is well presented, but there are a couple of inconsistencies that should be addressed and/or discussed, see specific and technical comments below.

Specific Comments (not in order of significance)

1. Title. This work is relevant also for other researchers than those interesting in sub-ice drilling. The current title also makes the scope too narrow for TC. Why not be bold and rewrite to “An ice-sheet modelling framework to determine vulnerable regions of the Greenland Ice Sheet” or similar.
2. Abstract: Lines (L) 25-27. This sentence is very unclear. Please rewrite. L139-143 provides a very clear and nice summary.
3. Keep in mind that the readers might not be very familiar with all ice core locations and names. For example, please change to: “In Greenland, long-archived basal rock and sediment…” (L71-72).
4. Methods: Section 2 starts with a nice summary and Table, but this leaves the reader with many questions on the specific choices of the parameters. Maybe the reader could be guided if in Table 1 you refer to the specific Sections 2.21-2.2.5 for detailed info on the parameter choices. An additional sentence before L157 “We first explain the sea-level potential, and then give details on the model and simulation set-up.” or similar, would also help.
5. Initial climate forcing: Past changes in climate over Greenland were mostly driven by changes in greenhouse gases (e.g. CO2) and changes in insolation. The former causing temperatures in all seasons to increase or decrease, while the latter strongly impacts the seasonal cycle. Are the spatial patterns more important than the seasonal changes (ref: Parameter is called “Spatial climatological pattern” in Table 1; Fig1a showing annual mean (right?) temperatures)? How representative are the early Holocene/Holocene Thermal Max and the PI? Related to this, should they be representative for past interglacials during the entire Pleistocene or for future climate change? Or could they be for both? Is it possible with your modelling framework to discuss/separate the impact of CO2 forcing versus insolation (seasonal impact) forcing? Some more discussion and clarifications regarding the climate forcing is needed.

6. Climate forcing: How do you deal (or not) with the SMB-elevation feedback? Are all ice sheet grid cells always forced by the same initial climate forcing, or is the SMB corrected for the lowering of ice surface elevations during the retreat?
7. L210: TC is read by non-paleo researchers, and PI then does not seem to be the most logical choice to represent “increased atmospheric CO2”. Please rewrite to emphasize that this is the case compared to glacial periods.
8. L213: How do you downscale from a 40 km resolution to the 10 km resolution of the ice sheet model? This might not be trivial for SMB.
9. Consistency: The parameters have various naming in Table 1, the figures, and the main text, please make this consistent throughout the manuscript. Holocene Thermal Max or early Holocene? Lithospheric relaxation time, or aesthenosphere, or mantle relaxation times? Modern transient (Table 1) or deglacial spin-up (Fig 1b)? Etc.
10. Precipitation lapse rate: This is notoriously difficult to account for, so I appreciate the effort. However, precipitation also changes spatially due to atmospheric changes (changing climate), and when the shape/surface topography of the ice sheet changes. I assume that this is not represented in your model set-up? Can you include a bit more discussion on this?
11. Run time: Do I understand correctly that all simulations are ran for 10,000 years, and that most of the analyses are done with the final state of the ice sheet (i.e. at year 10,000)? Using a shorter simulation time (interglacials normally do not last 10,000 yrs), or higher rates of warming, would impact how much of Greenland would be deglaciated in these simulations. Would this impact the calculated sea-level potential and uncertainty? Please discuss why you choose 10,000 years and what the impact of a shorter period would be.
12. Solid-Earth: the values from Le Meur & Huybrechts, and Coulon, are for Antarctica, right? (L241-242) Please make this clear in the text.
13. For the reader it makes more sense to first see and read the results related to Fig. 4 (overall results), and then the analyses related to Fig. 3 (specific impacts of parameters). Is it possible to change the order?
14. L439: “robust constraints”: this is a big claim. I am not sure if this work can really give robust constraints, but I do see its value in pointing out the vulnerable regions and impacts of uncertain forcings…
15. One last thing: Have you compared your simulated rates of change (in mm SLE/yr or similar) to other studies (e.g., Vasskog et al., 2015; Briner et al., 2020)? This could help constraining the rates of warming or simulation length, and/or give some more general constraints to your work.

Technical corrections

1. References: Something seems to have gone wrong with the notation of the references, many commas are missing. It should be (Name et al., year) or (Name and Name, year).
2. Also, sometimes a few references are mentioned, but these are just examples of work. These should include “e.g.,”. For example L104 should be (e.g., Helsen et al., 2023, …)
3. Fig. 1B: should the difference for the LGM not be created on the LGM grid, to emphasize the additional ice present outside of the present area?

4. Fig. 2 caption lacks some info: B) Full ensemble is grey. HTM is in orange (not red). C) Blue is land? Other colours indicate number of simulations predicting deglaciation at the location (right?).
5. L214-215: what does this mean “can be volumetrically”? does this sentence miss a word?
6. L233, should “ramp” be “rate”?
7. L 247: omit “following the LGM”
8. L259: Can you state the ice volume values for these 3 initial states?
9. Fig. 4a: very difficult to properly read the size of the dots. I suggest omitting (a), and add the black outlined dots to the map of (currently) Fig 4b.
10. L 313: add “These regions indicate likely regions for the first 1-2 m of ice loss” or similar, just after “less than 1.5 meters (Figure 4a).
11. L399: “potency”? do you mean “impact”?
12. L438: Green2Ice is a “ERC Synergy Grant funded by the European Union”

References

Briner, et al., Rate of mass loss from the Greenland Ice Sheet will exceed Holocene values this century, Nature, 586, 70–74, https://doi.org/10.1038/s41586-020-2742-6, 2020.

Vasskog, et al. (2015), The Greenland ice sheet during the last glacial cycle: Current ice loss and contribution to sea-level rise from a palaeoclimatic perspective, Earth-Science Reviews, 150, 45-67, doi:10.1016/j.earscirev.2015.07.006