We thank the reviewer for their careful reading and encouraging comments on our manuscript. We have addressed their comments in-line in bold text. Figure captions and line numbers refer to the revised manuscript.

Review of Keisling et al., «An ice-sheet modelling framework for leveraging subice 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.

Changed to "An ice-sheet modelling framework to determine vulnerable regions of the Greenland Ice Sheet in the past."

2. Abstract: Lines (L) 25-27. This sentence is very unclear. Please rewrite. L139-143 provides a very clear and nice summary.

L25–27 have been updated to read "We map the GIS response to warming, in order to (1) estimate of the region(s) of GIS that are most likely to contribute to the first meter of global sea-level change, (2) guide future sub-glacial access efforts that can provide targeted information about the response of the ice sheet to past warming, and (3) contextualize existing and future datasets within a glaciologically coherent, full-geometry framework to establish the minimum GIS contribution to past sea level when a particular location is ice-free." See revised text lines 26–30.

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).

Changed the wording to what the reviewer suggested (see lines 76–77).

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.

The caption of Table 1 has been updated as the reviewer suggested. We have added a sentence

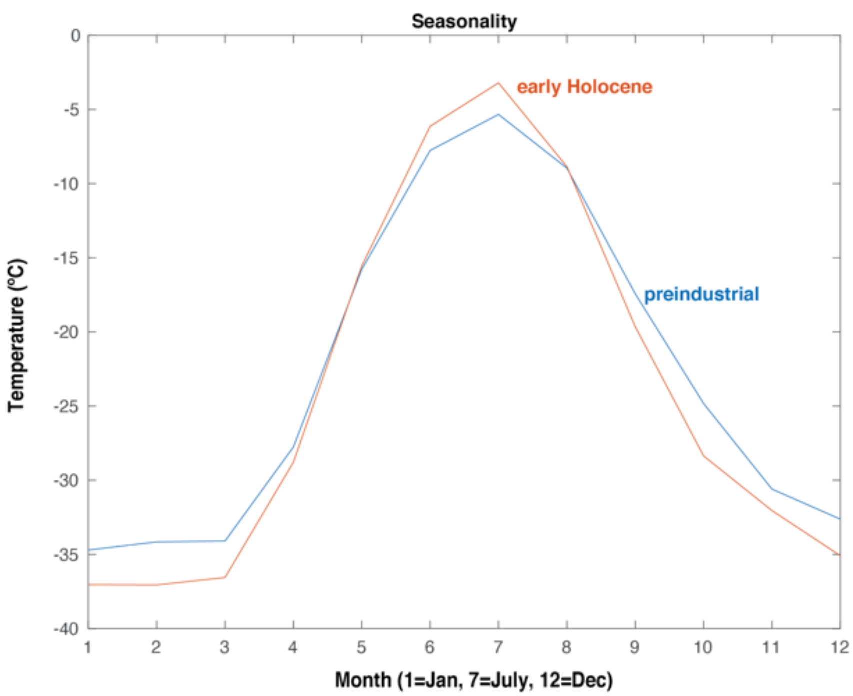
before L157 that reads: "We first provide details on the model and simulation set-up, and then explain our calculation of sea-level potential." The streamlining to keep the definition of sea level potential to section 2.3 was suggested by reviewer #2.

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.

This is correct, and this is indeed reflected in the forcing that we use, because the HTM spatial climatology has relatively warmer summers particularly at higher latitudes (Figure 1A).

Are the spatial patterns more important than the seasonal changes (ref: Parameter is called "Spatial climatological pattern" in Table 1; Fig1. a showing annual mean (right?) temperatures)?

We have edited the figure caption to clarify that these are indeed mean annual temperatures shown. We have also updated the figure to show the difference in summer (July) and winter (DJF) temperatures separately to enrich the discussion about the differences between these climatologies and the ways that they can represent different end-members of Pleistocene climate forcing. Indeed, the Holocene Thermal Maximum climatology is broadly characterized by warmer summers, particularly in the north and west, but colder winters (revised Fig 1). We can see in more detail the differences in these climatologies if we look at the average monthly temperature averaged for central Greenland (here defined as latitude 75.5–80.0ºN, 41.5–61.5ºW):



The HTM is warmer during the summer months, and colder during the winter months, meaning the amplitude of the seasonal signal is enhanced (33.8ºC compared to 29.4ºC for preindustrial). Overall, the mean annual temperature is actually lower during the HTM (-24ºC) compared to the preindustrial (–22.8ºC). This is because the HTM climate changes are responding to changes in insolation but under slightly lower CO_2 concentrations.

It is difficult to disentangle or separate the influences of seasonality versus spatial pattern in the reconstructions using our current methodology, because the reconstruction we used explicitly accounts for changes in both insolation and CO_2 simultaneously. For summer melting, which is a key process for deglaciation, seasonality is likely playing a dominant role because the HTM climatology is several degrees warmer than the PI climatology during the summer months, as shown in the figure above. We agree that seasonality is an important process that could be considered in detail in future work, using the framework we describe here (e.g. by analyzing an ensemble that includes different treatments of seasonality).

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.

To continue on the above response, these reconstructions consider both insolation and CO_2 forcings, so they only represent two scenarios – a "high CO_2/low insolation" scenario (preindustrial) and a "low CO_2/high insolation" scenario (HTM). In this way, they are representative of two different modes of interglacial warmth, and the preindustrial climatology captures the modern-day seasonality well. But, but they do explicity capture a high-CO_2/high-insolation scenario, nor do they capture the potential for a different kind of changing seasonality moving into the future, as has been suggested by some future modelling studies, because these scenarios are not captured in the last 21kyr. We have added an additional discussion on this point in the revised manuscript section 2.2.1, and we thank the reviewer for the inspiration to discuss these interesting points in greater detail in lines 227–231: These timeslices thus represent two end-members: lower CO_2/high insolation (early Holocene) and higher CO_2/lower insolation (preindustrial). In this way, they are representative of two known modes of interglacial warmth, and capture both the spatial and seasonal patterns associated with them. However, they do not capture different spatial/seasonal patterns that might be associated with climates warmer than modern, and we assume both spatial and seasonal patterns stay fixed as we conduct the interglacial warming experiments.

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?

We use a linear temperature lapse-rate correction of 5.0°C km^{-1} to downscale the 40 km^2 climate forcing to the 10 km^2 ice-sheet grid, and to dynamically adjust the ice-sheet surface temperature as the ice geometry evolves (e.g. height-mass balance feedback, Weertman 1961). This sentence has been added to section 2.1 (see lines 205–208) in the revised manuscript.

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.

Added "relative to glacial periods" (line 226).

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.

Downscaling from the 40km resolution climate reconstruction to the 10km ice-sheet model grid is done using a temperature lapse rate correction of 5.0ºC km^{-1}. Melt is then calculated using a positive degree day scheme that applies 5 mm of melt per degree day. Precipitation is calculated assuming no change in precipitation as a function of temperature or a precipitation lapse rate of 2% ºC^{-1}. This has been clarified in Section 2.1 of the revised manuscript (see lines 205–208).

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.

We thank the reviewer for catching this inconsistency. We have revised the manuscript to make these consistent throughout, so that the language in the table is the same as that used through the rest of the manuscript.

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?

This is correct. Our treatment of precipitation lapse rate only accounts for changes in precipitation that are directly correlated with changes in temperature. This is a first-order correction that is consistent with the broad-scale precipitation patterns known from glacial-interglacial temperature changes (e.g. Alley et al. 1993). However, our model set-up does not capture changes in precipitation that would be driven by other changes in boundary conditions, e.g. different moisture pathways due to changes in sea-ice (e.g. Koenig et al. 2015). Due to the difficulty in reconstructing past changes in precipitation, we leave a more complex treatment of potential precipitation changes to future work, but note that our results can guide understanding of which parts of the ice-sheet are most sensitive to changing precipitation during warm climates (our Figure 4). We have expanded the discussion on this topic in lines 412-415 to read: In particular, although the precipitation lapse rate we apply reveals regions where changes in precipitation strongly impact deglaciation, changes in precipitation that are not associated with temperature changes (e.g. changes in moisture pathways associated with changing sea-ice cover; Koenig et al. 2015) are not captured by our ensemble set-up.

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.

Yes, each simulation runs for 10,000 years. However, our analyses are not done with the final state of the ice-sheet, but rather the time that a particular location becomes ice-free within a 10,000 year warming period. We use four rates of interglacial warming (Table 1) to capture different rates of warming. We chose a 10,000 year warming duration because this is a common duration for interglacial periods (reference). Even with 10,000 years of warming, some ensemble members do not completely deglaciate (see our Figure 1). With a shorter duration of warming, we expect that more ensemble members would not fully deglaciate, and some regions would remain ice-covered for

the duration of the experiment (imagine Figure 1, but where warming stopped at 5,000 years, such that some ensemble members still had thick ice-cover). Sea-level potential could still be used in this case, but the histogram we use to produce sea-level potential would not be fully populated, which could alter the median and spread, because we would only be sampling the subset of simulations that deglaciate most quickly. We choose 10,000 years as a representative interglacial length, but acknowledge that the sea-level potential for a site could be slightly different for an interglacial of 1,000 years or 30,000 years duration. We have added the following text to section 2.2.3 to clarify this: We choose to apply this rate of warming for 10,000 years as a representative Pleistocene interglacial length, and note that the sea-level potential for a given site may be different for an interglacial of a different duration. For a shorter-duration interglacial, the subset of ensemble members that deglaciate most quickly could be analysed. For a longer-duration interglacial, the simulations could be run further forward with continued warming.

12. Solid-Earth: the values from Le Meur & Huybrechts, and Coulon, are for Antarctica, right? (L241-242) Please make this clear in the text.

This has been clarified in the revised manuscript, line 267.

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?

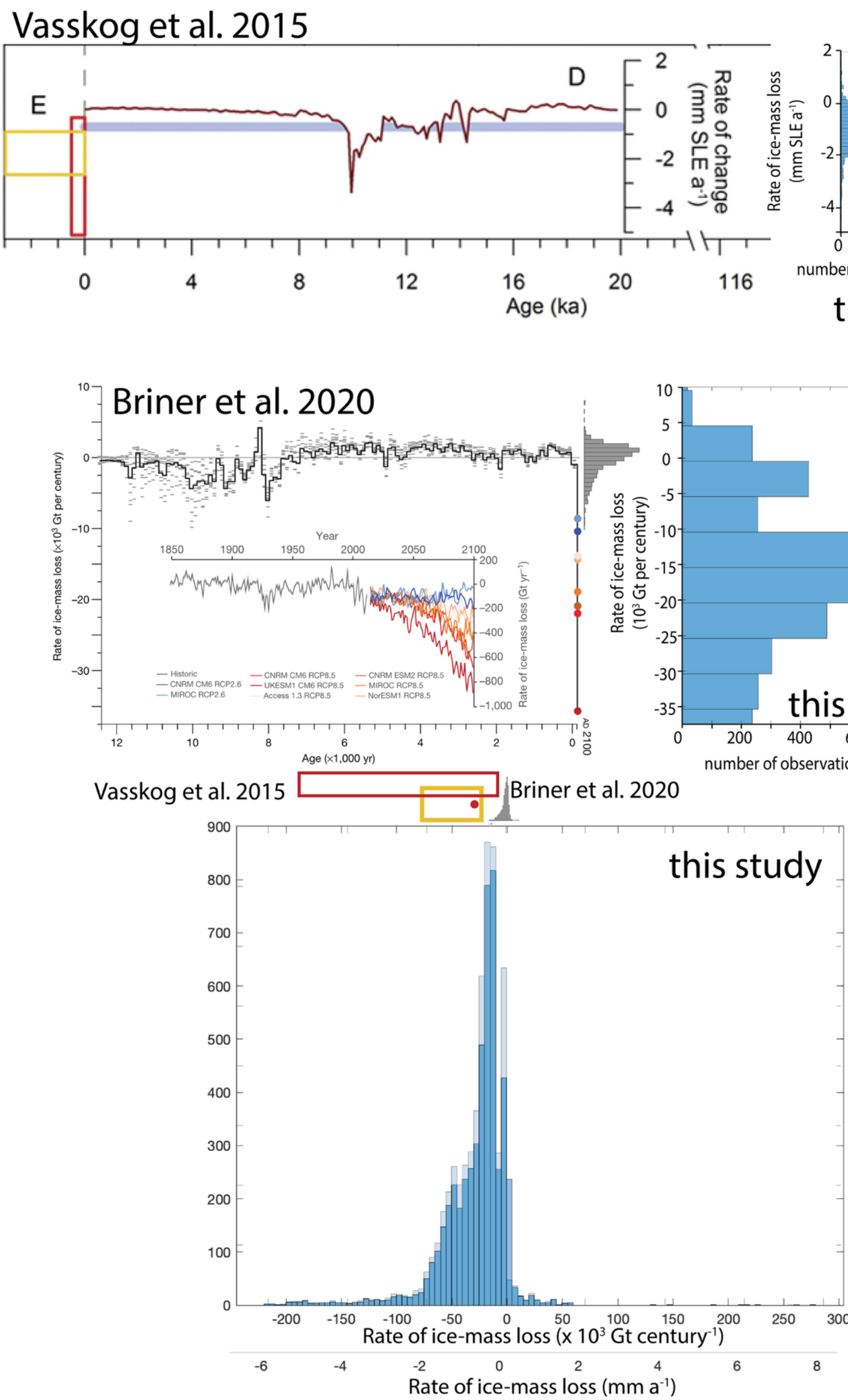
The order of these figures has been changed as suggested by the reviewer, and calls to these figures changed throughout the manuscript.

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…

The word "robust" has been removed. The revised manuscript now reads (lines 477–480): Future programs to collect samples from beneath the ice-sheet margins and interior, including the U.S. National Science Foundation-funded GreenDrill (Briner et al. 2022), and Green2Ice, an ERC Synergy Grant funded by the European Union, in combination with our results, may provide novel constraints on paleo sea level contributions from the GIS.

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.

Although in this manuscript we do not focus on rates, over all of our simulations the rates of sea-level change are broadly similar to those seen during the Holocene and predicted for the future (Vasskog et al. 2015, Briner et al. 2020):



Vasskog et al. 2015
E
D
Rate of change
(mm SLE a⁻¹)
Age (ka)
Rate of ice-mass loss
(mm SLE a⁻¹)
number of observations in bin
this study
Briner et al. 2020
Rate of ice-mass loss (×10³ Gt per century)
Year
Rate of ice-mass loss (Gt yr⁻¹)
Historic
CNRM CM6 RCP2.6
MIROC RCP2.6
CNRM CM6 RCP8.5
UKESM1 CM6 RCP8.5
Access 1.3 RCP8.5
CNRM ESM2 RCP8.5
MIROC RCP8.5
NorESM1 RCP8.5
Age (×1,000 yr)
AD 2100
Rate of ice-mass loss
(10³ Gt per century)
number of observations in bin
this study
Vasskog et al. 2015
Briner et al. 2020
this study
Rate of ice-mass loss (x 10³ Gt century⁻¹)
Rate of ice-mass loss (mm a⁻¹)

Here we have compared the rates of ice-mass loss during our deglaciation runs with both Vasskog et al. (2015) and Briner et al. (2020). We have calculated the rate of sea-level rise in 100-year bins for every ensemble member and made a histogram of results. In the upper two panels, we have adjusted the x-axis of our histogram to match the limits from the other two publications as best as possible. In the lower panel, the full histogram of our results is plotted, and smaller versions of the two other studies are plotted above for comparison. The solid blue bars are for the data binned in the units of 10^3 Gt century^{-1}, whereas the transparent bars correspond to the same data binned in units of mm yr^{-1}. We find that the rates of sea-level change predicted by our ensemble are similar to those predicted for future warming scenarios (red dot from Briner et al. (2020) and colored boxes from Vasskog et al. (2015)) and those experienced by the GIS during the deglaciation and Holocene (red line from Vasskog et al. (2015) and gray histogram from Briner et al. (2020)).

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).

Thank you for pointing this out. We have re-exported the references from Zotero in the Cryosphere format and double-checked that the in-text citations match the reference section.

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, …)

This has been fixed (line 98).

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?

Figure 1 has been updated to reflect this change. Now each panel shows the difference with BedMachine on the model grid, such that the model initialized for LGM conditions has a footprint that extends beyond the BedMachine dataset.

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?).

The Figure 2 caption has been adjusted to contain the additional information and corrections that the reviewer pointed out.

5. L214-215: what does this mean "can be volumetrically"? does this sentence miss a word?

Thank you for pointing this out – the sentence was missing the word "offset" and has been adjusted to reflect this (line 235).

6. L233, should "ramp" be "rate"?

Changed (line 255).

7. L 247: omit "following the LGM"

Changed (line 271–272).

8. L259: Can you state the ice volume values for these 3 initial states?

Added the ice volume for each ice-sheet state to the revised Figure 1.

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.

This has been changed following the reviewer's suggestion.

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).

Added: "These regions indicate likely regions for the first 1–2 m of ice loss in the past."

11. L399: "potency"? do you mean "impact"?

Changed (line 435).

12. L438: Green2Ice is a "ERC Synergy Grant funded by the European Union"

Changed lines 478–479 to reflect the language suggested by the reviewer.

References

Alley, R. B., Meese, D. A., Shuman, C. A., Gow, A. J., Taylor, K. C., Grootes, P. M., White, J. W. C., Ram, M., Waddington, E. D., Mayewski, P. A., and Zielinski, G. A.: Abrupt increase in Greenland snow accumulation at the end of the Younger Dryas event, Nature, 362, 527, 1993.

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Vasskog, K., Langebroek, P. M., Andrews, J. T., Nilsen, J. E. Ø., and Nesje, A.: 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, https://doi.org/10.1016/j.earscirev.2015.07.006, 2015.