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Interactive comment on "Large and irreversible future decline of the Greenland ice-sheet" *by* Jonathan M. Gregory et al.

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This study presents new simulations of the Greenland ice sheet under long timescale climate change to assess its stability. A new assessment of this nature is long overdue, since the widely cited threshold estimate is based on the results of only one study (Robinson et al., 2012). The work here is innovative primarily because the simulations are performed using an ice-sheet model that is fully (bidirectionally) coupled to a low resolution GCM climate model. The process-based analyses presented are a very valuable contribution to our understanding of this complex coupled system. Nonetheless, I believe significant revisions are needed to improve the clarity and precision of the study (in particular, because of the extra scrutiny and attention that this paper will likely receive in the context of AR6).

Major comments

Threshold or no threshold. The statements in this paper regarding the existence of a threshold for large-scale melting are framed as rather strongly, in a way that greatly contrasts with the results of Robinson et al. (2012) and previous work. However, I feel that this rather binary framing is not really warranted, nor does it help the community gain clarity on the issue.

- First, I would argue that the experimental setup here simply does not allow such a definitive conclusion to be made. Only 47 experiments are performed with rather arbitrary levels of SST warming applied based on available GCM experiments. This leaves some conclusions open to interpretation. For example, in Fig. 2c, it could be argued that the low-albedo model (red points) shows a roughly linear reduction in equilibrium volume as a function of temperature anomaly, while the high-albedo model (blue points) shows a threshold at $\sim$2 °C.

- Second, it is clear from this and previous work that strong, positive feedbacks do exist that give the system the potential for self-sustained melting of the ice sheet (albedo, elevation feedbacks). This work shows that there are additional important negative feedbacks (circulation changes leading to increased cloudiness and precipitation) that can serve to counterbalance the positive ones. Given this, I think the binary framing of "threshold or no threshold" is rather misleading.

- Third, this work will clearly feed into the upcoming IPCC report. Simply including the headline statement "There is no threshold" implies that these results run completely counter to previous work. But one could also make the following statement: "Above 2 °C, all simulations show retreat of the ice sheet to less than half of its current size." This statement is actually quite consistent with previous results, with the difference being how far the ice sheet retreats.

For the reasons above, I would suggest a general reframing of the discussion of these results in relation to previous work to highlight the continuity in our growing understanding of this complex system.

Section 2 ("Conceptual basis for the existence of a threshold warming"). Related to the point above, I don't quite see how this section adds value to the manuscript, as it is currently framed, especially since later it is stated that the "the conceptual basis for its existence is incorrect". It feels somewhat like a straw-man argument. The simple equations described in Section 2 are useful for conceptualizing the possibility of a runaway feedback leading to the complete melting of an ice sheet. But I think it is by now clear to the community that an ice sheet like Greenland is a large, complex system with processes coupled to atmospheric circulation and a wide range of acting timescales. It is clear, for example, that Δs, A and f(ΔM) will all change over time. Therefore, I would suggest to the authors that, rather than framing this as the current paradigm that should be rejected, it would be more valuable to highlight, conceptually, what could happen when some of those terms vary (i.e., when A becomes smaller, but Δs increases, or to expand f(ΔM) into the multiple contributions that may exist, like f_{albedo}, which is most often positive in a warming climate, and f_{cloudiness}, which is found here to be an important negative feedback). Because I think the authors would agree that, ignoring possible climate feedbacks like cloudiness for a moment, as done in the uncoupled experiment, the theoretical basis for a threshold for ice sheet retreat (i.e., the small ice cap instability) still applies here. It is just mitigated by additional feedbacks/factors that are not accounted for in this simple equation.

Along the same lines, I don't think it makes sense to summarize the study of Robinson et al. (2012) at the end of this section as estimating ΔM. In that study, as in this one, a fully coupled climate – ice-sheet system is simulated with a dynamically evolving ice sheet and topography. The results of such experiments allow later comparison with expectations from this conceptual framework, but this equation is not used at all for any quantitative analysis.

The scatterplot of Fig. 2c is indeed interesting. While above 2 °C, only rather low-volume states appear to be accessible, from ~0.5-2.0 °C, a wide range of intermediate

states are accessible. It appears that the low- and medium-albedo model versions particularly allow access to volume distributions between 3-6 m sle. In contrast, the high-albedo model version mainly shows states with a large volume or a much smaller volume. Is there a reason that the high-albedo model may exhibit more threshold-like behavior than the low-albedo model? I think a discussion around this point would be a valuable addition to understanding the physics of the system.

The model setup is innovative and well-described. In particular, the use of elevation classes (i.e., tiles) is a proven method to improve the downscaling of smb to the higher resolution ice sheet model from low-resolution GCMs. Nonetheless, I have two key concerns:

1. I am very surprised to see an SIA model applied here, and at only 20km resolution. At a minimum, some justification of this relatively low grid resolution should be made (I would not expect computational cost to be an issue for such a model in this framework). More importantly, the authors should acknowledge and discuss the possible impact of a lack of fast ice dynamics in their simulations. For example, is basal sliding parameterized in some way, or is no basal sliding allowed? If it is not, the model is likely underestimating the dynamic adjustment of the ice sheet to the area retreat, which is an important positive feedback on ice decline on these timescales.

2. The FAMOUS atmosphere is necessarily low-resolution for computational speed, but 7.5° lon x 5.5° lat corresponds to roughly 7 grid points east-west and 5 grid points north-south over Greenland. Given that this study highlights the importance of atmospheric circulation changes impacting Greenland stability, such a low atmospheric resolution here seems problematic. Have the authors considered running a short experiment with a higher resolution equivalent of the AGCM with the same boundary forcing and a reduced ice-sheet configuration, but no active ice-sheet model, to see if the atmospheric state is similar to that predicted by the very low-resolution version? Such an experiment, if possible, would go very far towards understanding the possible uncertainties related to these non-linear feedbacks with the atmosphere.

It would also be valuable to see a figure showing the forcing applied to the model. For example, how do the present-day SST fields compare to reanalysis or observed SSTs? What are the future patterns of warming? Also, it generally seems that the simulations forced by NorESM1-M stand apart from the other two with lower ice volumes predicted for the same level of global warming. Is this reflected in the SST warming patterns in some way?

Finally, in terms of style, I find that the use of abbreviations for different variables throughout the text makes the manuscript harder to follow. For example, on L30, the phrase "The increase in D is probably the ice-dynamical response. . ." would be more straightforward replacing "D" with "discharge". Perhaps the authors could consider only using the variable abbreviations (P, D, R, M, etc.) when the text is related to specific equations that use them, and otherwise use the actual names in sentences. Some other abbreviations could be avoided all together (BCs, GMSLR, etc.).

Minor comments:

L11-12: "This is because the dominant effect is reduction of area, not reduction of surface altitude, and the geographical variation of SMB must be taken into account." <= This sentence could be more precise. In previous work geographical variation of SMB was also considered, even if in a simpler way. Nor does it seem that the dominant effect is the reduction in area. Rather, it seems that changes in atmospheric circulation act to mitigate the warming via increased cloudiness. The next sentence is already clearer, so I would suggest deleting this one.

L15: "owing to such effects," <= This reference is not very clear, as increased cloudiness and precipitation would, in principle, help the ice sheet regrow. Consider rephrasing, or simply removing.

~L118, Section 3.1: Other boundary conditions aside from topography and SSTs should be explicitly mentioned here. Are greenhouse gas concentrations applied in FAMOUS-ice to be consistent with the applied SST fields, for example?

Fig. 3: This is an important figure, but feels a bit busy. Perhaps the color bars of each row could be placed vertically on the side? This would clean it up a bit and make more room for the panels themselves.

Fig. 4: This figure is hard to follow, as there is a lot of information. I would suggest revising colors and symbols to provide more clarity. For example, I think the colors for different forcing scenarios should be substantially different from the colors delineating model versions (low, medium, high albedo).

L269-270: A stable but diminished ice sheet is consistent with previous work. Robinson et al. (2012) found that 10% of the ice sheet remained even above the tipping point for large scale melting (see state E3 shown in Fig. 4 of that reference for an approximate picture). It is clear that at some point retreat of the ice sheet to high elevation zones may lead to restabilization. I suggest reframing here.

L337-345: Consider reframing title of this section and first paragraph along the lines of earlier comments.

L387-388, L395, etc: I would remove the terms WOWS and NON, as they do not help comprehension beyond the already defined EWNS terms, and are used rather rarely in any case.

L411-412: I would point out that this seems to be an example of a tipping point being activated. This is, of course, not starting from present day, and so is not the same as what has been discussed until now. But it does show that the mechanisms for triggering self-sustained decline are present in the system.

L458: Greenland or global warming => regional or global warming

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