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Discussions

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

Anonymous Referee #1

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Overall impression

This paper was much needed. The topic is very relevant and here it is addressed with a novel approach where a relatively course resolution Atmosphere General Circulation Model (AGCM) is forced with ocean conditions from several Atmosphere-Ocean General Circulation Model, and bidirectionally coupled with an ice sheet model. This permits multiple simulations with models of diverse climate sensitivity and diverse imprints of climate change on the Arctic/Greenland region.

The study provides novel insights into ice-climate feedbacks and the future long-term deglaciation of Greenland.

General comments

The authors present a study on the long-term stability of the Greenland ice sheet, a topic that has been addressed before, but in this case with a novel methodology, with novel conclusions inferred from the simulations, and empirical relationships obtained from these simulations. They use a course-resolution (7.5 degrees lon x 5 degrees lat) atmospheric GCM including SMB simulation on "tiles" with ocean forcing from four different climate models and three choices of albedo. They perform a total of 47 simulations, with 46 coupled to an ice sheet model (shallow-ice approximation, 20 km resolution) and 1 without coupling (no feedbacks from ice sheet change). I find the methodology of this study very efficient in addressing a complex problem with a sophisticated tool (coupled climate, SMB and ice sheet models including SMB downscaling via "tiles") but with adequate simplifications that solve the computational problem.

The paper is well written, with a detailed introduction on the problem of ice sheet stability. In the current form, the theory and conclusions are relatively easy to follow; however I find difficult to navigate the results, e.g., figures (with much dependencies and going forth and back in the text) or trace individually the outcome of the 47 simulations. To this point, I have included some comments regarding figure legends and keys, but if the readers can find further ways to help paper navigation, that would be helpful

I have three major comments/points of discussion regarding the main conclusions of the study:

The authors found no evidence for "warming threshold beyond which the ice sheet would be eliminated". However, one could argue that this depends on how this "threshold" and "eliminated" are defined. The authors based the claim on that "the ice sheet endures, albeit in a much reduced state", others could call this elimination. They use also use the claim that there is a large spread on the final ice sheet mass for the same global temperature change, coming from the albedo and boundary (ocean) conditions. However, if I look at figure 2c, I could draw two perpendicular lines intersecting at (2.6 K, 3 m SLE) and claim that all simulations with warming of more than ~2.6 K would result in very small ice sheets of less than 3 m sea level equivalent, and that no simulation with less than this warming results in final states of less than 3 m sea level equivalent. Yes, for 2 K of warming the spread in the final volume is very large (from almost no change to more than $\frac{1}{2}$ of the original size), but the spread does not include very small ice sheets. Beyond the 2.6 K "threshold" the spread does not include high volume final ice sheets.

The authors label the negative feedbacks found (cloud, precipitation) as "area feedbacks", as opposed to positive "thickness feedbacks" (elevation), but I am not very clear about this separation. For instance, the present-day distribution of solar radiation and precipitation is related not only to the margin position but to the surface topography (or thickness, Ettema et al, 2010; also see Figure 3c3,d3 and Figure 4c4,d4 where solar radiation and snowfall change within the common area as well). Likewise, the albedo feedback is a positive feedback and the albedo effect from area retreat may be included in its definition (besides the melt effects over the remaining ice sheet).

Finally, some of the conclusions of the study come from a empirical relationships inferred from this study's simulations, and not directly from the simulations. The distinction from inferred conclusions and direct conclusions should be made. For instance, when these relationships are applied to present-day rates (as there are actual simulations under present-day climate, but these are "stable" spin-ups) or to results from AR5 (e.g, projections for RCP8.5)

Technical comments/questions

The use of a surface energy balance calculation for melt (as opposed to empirical calculations, e.g., PDDs) should be highlighted more, for instance in the abstract. More detail on such calculation (e.g., energy fluxes, snowpack model, refreezing calculation, albedo calculation) should be given here, instead of only referring to a paper in preparation.

Very little attention is given to refreezing in the manuscript, how is refreezing evolving within the simulations?

Which kind of vegetation cover is simulated under the retreating ice sheet? How different are the properties of this land cover with respect to a glaciated surface?

I find a bit confusing that the spinup is made under 1980-1999 MIROC (ocean) boundary conditions, but scenario climates from other three models are applied without going through the historical period of the corresponding model. For instance, in Figure2a, the change in SMB is referred to historical MIROC, but in the text the historical value for a different model is given for this change in SMB. In Figure 3 historical HadCM3 SMB is depicted in column a, while the text in section 6 indicates that this climate is not used.

Specific comments

Abstract

The abstract is difficult to understand without having read the body text first. Much context is missing on the numerous complex statements. I suggest to give more context and/or reduce/generalize the conclusions.

Line7 – clarify meaning of "initially", and of "warming". A bit of introduction on the simulation design (e.g., "steady state", use of BC from four different models) could be helpful. Line 8 – "for all RCP8.5 climates" – clarify that this means for a steady state climate corresponding to 2081-2100, and not RCP/ECP8.5 up to 2300 as e.g, in Aschwanden et al, 2019, Vizcaino et al, 2015. Line 9 – "if recent climate were maintained" : this is a conclusion not from the simulations, but from empirical relationships obtained from the simulations. Line 11 – "the dominant effect is reduction of area" : effect on what? The statement is very cryptic, why is the area important? Line 12 – "the geographical variation of SMB must be taken into account". This seems to imply that this is not taken into account in previous work, but one could argue that simulation of ablation and accumulation area is a geographical variation already... Please specify further "geographical variation of SMB" Line 14 – "if late twentieth-century climate is restored (...) the ice sheet will not regrow to its present extent" – this would be straightforward after reading line 9, but this line 14 is based directly on the model simulations, and not on the empirical relationships obtained... the mixing of direct and indirect results is a bit confusing. Line 15 “owing to such effects”: which effects? The cloud and snowfall effects were just said to increase the SMB, so they would help to re-grow the ice sheet?

Section 1

L71 “Because of the elevation and albedo feedbacks (...) the present ice-sheet could not be regenerated” – Is a decrease of albedo “irreversible”?

Section 3

Famous-ice AGCM: can you explain how refreezing is calculated? Text in general lacks discussion of effects of refreezing (both in introduction and discussion)

L123-124 Muntjwerf et al., GRL, 2020 also use this approach with same number of elevation classes for CESM2.0 L126 Sellevold et al, TC, 2019 discusses the sensitivity of the “tiles” downscaling method to lapse rate choice L129-L120: which gradients are these, can you specify? L134: “There is an uncertain parameter (...)” please specify L137: since Smith et al is in preparation, could you give here more detail on the albedo modeling? L151-L157: how is sliding parameterized in the ice sheet model? L163: “we run 10 years”, do you mean, each atmospheric year is used 10 times, then next atmospheric year is run, etc,... L165 -> “we have verified” : would it be possible to show some proof of this, e.g., a figure in the supplementary information? Initialization L176 Title “3.3” can be more precise, maybe specify “(Evaluation of) Simulated 1980-1999 surface mass balance”? L193: “similar” is perhaps subjective. Please discuss similarities/differences Table 1, last sentence is unclear (20, 30 or 100 years, “second group”?) The SMB for RCP2.6 is very similar to historical. This makes sense as only the 2080-2099 (steady-state) climate is used, as compared to other SMB estimates with evolving ice sheet topography where the full 21st century climate for RCP2.6 is applied. Maybe good to add some cautionary text to avoid misleading comparisons?

Table 2 Why 402 ppmv for the historical period, isn’t it too high? The mean concentra-

tion was lower than that in 1980-1999.

Section 4. L215 In the legend of Figure 2 it says delta_SMB is referred to steady state under historical MIROC5, but here the HadGEM2-ES value (+307) is given, which one is correct?

L220 – 10% larger -> I get 14%, am I missing something? (0.67*1.50/0.88) L238 “becomes gradually more positive” -> increases

L269 “there are such states” : Could you add detail on those? It is difficult to map them from Figure 2d to Figure2b to follow the SMB evolution.

L292 smaller -> lower L299 “more negative” “4 times more” – confusing, please give values L307-308 precipitation contours are difficult to read or labels are absent

Section 6 L368, L371 “All but one” : are you explaining this “one” somewhere else? L398 “we suppose this dome might regrow in time”: it seems it does not regrow in 20,000 years, when do you expect it will start to regrow? L411 “difference” in what? “infinitesimal” -> what does this mean? Please quantify L435-439 Why 2,500 years at 0.7 mm/yr and 1,700 at more than double the rate (2 mm/yr)? How have you done these calculations? Which GrIS mass are you considering as present-day mass and as NON steady-state mass? L452 “outweighted” : in which sense? L453 “Snowfall”: do you mean ice-sheet-integrated? L466 “If a climate (...)” There is a jump here. The results now are based on the empirical relationships, but not directly on the simulations here as the ice sheet is relatively stable under 1980-1999 forcing. L479-481 What about the course resolution of the atmospheric model, would you include it as a limitation of this study? L485 “sketchy” has a negative meaning in informal American English, maybe replace by other adjective (“gross”)

Figures

Figure 1. White contour in b), is this modelled or observed “ice margin”? The ELA contour of c) makes a strange shape in the NW, any idea why?

Figure 2- It is difficult to read precipitation from the last row of figures. Only one contour line is labeled, there are very few others, and no line interval is given.

Figure 4 – Legend text and keys are confusing. My understanding is that the line color indicates albedo choice. “climates (. . .) indicated by the line colors according to the line key of (c)” would hint to the colors indicating climates, but it is actually albedo? I would take the line key from c) and put it in a common space as it applies to all panels. Same for the symbols in c) The orange dotted line corresponds to offline simulation with low albedo, shouldn’t it be red dotted for consistency? Otherwise, add it to the key for lines, with a “Low albedo, offline run” Legend: “the circles indicate transient and final sites” Panel b- Legend text is not clear. Having circles in the key is confusing, as only the colors are used, and those correspond to the symbols in the key of c).

References

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