The Cryosphere Discussions

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Comment on tc-2022-145

Anonymous Referee #1

Referee comment on "Effects of extreme melt events on ice flow and sea level rise of the Greenland Ice Sheet" by Johanna Beckmann and Ricarda Winkelmann, The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-145-RC1, 2023

Review of “Effects of extreme melt events on ice flow and sea level rise of the Greenland Ice Sheet” by Beckmann and Winkelmann

The authors present a set of ice-sheet model simulations to 2300 that explore the impact of extreme events of varying frequency and intensity relative to a simulation with a baseline climate forcing. They construct the baseline temperature forcing using regional climate model estimates of Greenland surface temperature and an emulated global mean temperature time series. They then calibrate a positive degree day model for this temperature time series in order to attain surface mass balance forcing to 2300. They construct nine scenarios for combinations of periods of 5, 10, and 20 years and relative intensities of 1.25, 1.5 and 2, relative to the running decadal mean temperature in the baseline forcing. Running the ice sheet model under these forcing scenarios, they find that including extreme warm events can have a significant impact on long-term mass loss for events with high frequency and intensity. They find a 14% increase relative to the baseline scenario for the most extreme scenario that includes ice dynamics and SMB-elevation feedback. For a case that considers only surface mass balance (i.e., neglecting ice dynamics changes), they find a 16% increase in mass loss.

Overall, I think the study is interesting and the paper is well written. I have two primary concerns with the paper, however, which cause me to recommend major revisions.

The first concern is that the initial condition is not a good representation of the present day ice sheet, with many major outlet glaciers over- or under-estimating observed velocities by a wide margin. It is not possible to discern from the figures just how far from the modern state the initial condition is, but some major outlets seem to differ from observed velocities by >100% (estimating from Fig S2 by eye). Some misfit statistics are reported in the text, but these are skewed by the very large slow-flowing part of the ice sheet and so the reported average misfit of 9 m/yr is not terribly relevant. It is unclear why this initial condition was used, when there are other PISM initial conditions that look much closer to observations. The present initial condition makes it difficult to interpret the results, as I would not expect this model configuration to respond to external forcing in the same way as a configuration that is closer to observations. I recommend improving the initial condition (possibly just using one that has been published) and repeating these simulations and analysis, or at least somehow demonstrating that the initial condition does not significantly bias the results relative to a more realistic initial condition.

My second concern is that the experimental design is to essentially add extra temperature forcing, which leads to the conclusion that extreme events are important. But applying these extreme events to the baseline temperature forcing time series results in a stronger average temperature forcing than the baseline. Thus, there is no way to determine how much of the excess mass loss is really due to the extreme events and how much is due to this increase in average temperature forcing. It seems that the proper methodology would be to ensure that the baseline and extreme scenarios have the same mean temperature forcing over some long-term average (probably a few decades to a century). This would much more clearly show the impact of variability vs mean forcing.

I also found the Discussion section to be rather limited in scope. I have added a few suggestions below of topics to enhance the Discussion. A number of more specific edits, comments, and questions that are also listed below. There are a number of mis-referenced figures, especially in the very long supplement, so that should be checked carefully during revision.

Specific comments:

L 73: "Consider changes in ocean melt or sliding due subglacial or subglacial processes" needs revision

L 88: submarine melting is kept constant, but how is it calculated, or what dataset is used? Is the melt rate constant for each glacier for all time, or does each cell have an associated melt rate that is applied when the glacier terminus is in that cell? Are there different treatments for floating and grounded ice? Please provide more information about this.

Is there any calving law or criterion used here?

L 123–124: By this logic, Humboldt Glacier should have a fairly good match to observations because its width is large compared with the model resolution. However, the fit is very poor there.

L 125: There should be similar statistics reported for just the fast-flowing part of the ice-sheet (e.g., where speed > 100 m/yr or some other reasonable threshold), where the velocity and thickness are much more relevant than over the ice-sheet interior.

Specify which version of BedMachine is being used. Presumably v3? Citing the paper rather than the dataset (ie., the NSIDC page) is a bit ambiguous because the Morlighem et al (2017) should be cited when using v4 or v5 as well.

Fig 1B: Subplot title missing a letter?

Figure S1: please add a panel showing thickness misfit as a fraction of observed (BedMachine) ice thickness.

Figure S2: Color bars on all plots are too narrow, resulting in very large areas of saturated color that make it impossible to judge the fit to the observations. Please use wider color bars; 10^4 m/yr would be a more reasonable upper limit for panels (a) and (b). Also consider using a signed log-scale (e.g., -10^3, -10^2, …, 10^2, 10^3) for panel c to aid with visualization. There should also be a plot that shows the misfit as a percentage of observed velocity. Some of the velocities at these large outlets (notably Humboldt, NEGIS, most of the NW sector, and potentially others, but hard to tell on this color scale) are very far from the observed velocities, which will significantly bias model results in these regions. This makes interpreting the results rather difficult, as the modeled ice-sheet state is quite far from the true modern state.

L 138–140 and Fig S3: The agreement between the modeled and observed mass balance from 1972–2017 seems overstated to me, given that the slope of the observed mass balance is almost twice the slope of the modeled mass balance from 2000–2017.

L 157–162: Difficult to understand. I don't understand how the anomaly years contain the monthly anomalies, for instance. Please revise these lines for clarity.

L 164: Is Figure S9 the correct figure to reference here? I don't see how it relates to the text here. Seems like it should be Figure S5

L 181: Should be I1.5f5?

Figure S10: There is only one tick on the vertical axis here, which makes it impossible to determine the vertical scale.

Section S2.1: “ Figures S5 and S6 show that the extremes would increase…” These don’t look like the correct figures. Should be S8 and S9? Also, the brown curve is not defined in S8 and S9.

Figure S12: Why are the two MAR curves here so different over most of the century? I don’t fully understand what is meant by: “SLR from the original MAR data set (Miroc5) of 1km resolution was derived from the â□□SMB”, so perhaps that can be phrased more clearly, with a reference to another figure if relevant.

L 205: It would be helpful to remind the reader in this sentence of what the scenarios are.

Figure 2 and in general: It seems strange that only extreme warm events are included in these scenarios, rather than including both extreme cold and extreme warm events. By including only warm events, you’ve essentially just increased the decadal (or multidecadal to centennial) average temperature by a few degrees, which will of course lead to correspondingly more mass loss. It seems that the proper comparison would be to make temperature time-series that have the same multidecadal average, so that the impact of variability is actually quantified, rather than to add extra temperature forcing to a baseline temperature time series as is done here.

Figure S13: The vertical axis label should be dST/dz, correct?

L 210: In the SMB-only experiments, does ice thickness change due to SMB? Or is ice thickness held constant in time? Or is advection active, but velocity is held constant? Please add a bit more detail about this set of experiments.

Figure S14: Text seems to reference something that isn’t present in the figure: “he

corresponding ice sheet extent in 1971 (i) and the emerging ice retreat in years 2100 (ii), 2200 (iii) and 2300 (iv) are given in light blue and

red shading, respectively.”

L ~245: Is this shown in a figure or table anywhere?

L267: typo: Mirco5

L 300: Could it also be that CW is the only one that continues to accelerate because Jakobshavn remains a marine-terminating outlet, and that’s not the case for most other large outlets? From Fig1, it looks like the only other outlets that remain in contact with the ocean are Petermann, maybe NEGIS, and maybe Humboldt.

What basal friction law is used here? I see that you use an exponent of 0.6, but what is the form of the law? That could have an effect on the slow-down you observe while driving stress decreases.

The Discussion section is very short and the Conclusions section reads like it should be in the Discussion. Consider expanding the Discussion and including more of a summary of your findings in the Conclusions. Particularly, the discussion should touch on the impact of the initial ice sheet state on these results, as the spun-up initial condition is quite far from the observed modern ice sheet state (Fig S2). This initial condition should be compared with other model initial conditions for Greenland, at least with the initial condition from Aschwanden et al. (2019). Another topic to touch on in the Discussion is that temperature extremes will in reality increase the flux of meltwater to the bed and thus affect ice dynamics through subglacial hydrology, which is not accounted for in these simulations. Finally, some discussion of the full dynamics runs vs the runs without SMB-elevation feedback would be good.

There is no equivalent of Fig 4 given for the dynamic case without SMB-elevation feedback. Overall, it seems like those runs were ignored compared with the SMB-only and full dynamics cases. There should be another subsection analogous to 3.2 in which the full dynamics and no-feedback runs are compared in more detail.

I have rated Presentation Quality as "Fair" because there I think the manuscript relies too heavily on the numerous figures in the Supplement, while there are only a few figures in the main paper.