Response to Reviewer 2 (Cecilia Bitz):

Reviewer comments are in red font. Replies are in black font.

This paper provides a valuable service by outlining the range of ways that ESMs model the effects of ice sheets and glaciers and by constructing datasets and recommendations to deal with their mass and energy imbalance. There are many positive aspects, like the community effort and presumably consensus behind the work.

Thank you very much for your support and constructive review.

The list of definitions at the start is very good too - though I suggest adding Surface Mass Balance (SMB) to it.

Done

I am grateful to know where these authors "judge" something to be true or offer views. Some of this information would be impossible to know for certain since not all modeling centers answered their survey. The authors provide valuable guidance about simulation assumptions and practices with a summary of choices made by modeling centers with some indication of the consequences. This is helpful for model developers and those analyzing models alike.

Agreed!

Please consider the following specific comments:

1) There are a few occasions when the manuscript needlessly goes into minor issues about conservation. For example, I think the point made about moving snow down hill at line 166 is ridiculous, or needs further explanation. We don't worry about the conversion of PE to heat by raindrops when they hit the ground so why should we worry about the conversion of PE to heat when snow moves downhill. Further if it amounts to 14C it is not a wrinkle that might make a small difference, so something is awry about this paragraph. Another example is at Line 461-3 with regard to icebergs possibly melting a bit due to geothermal heating, etc. While all this may be true, there is little point in itemizing a bunch of stuff that no ESM developer would bother with. It makes the paper longer than it need be.

Fair points, but we are trying to cover all the bases/interests of a diverse group of authors (and readers!). Conservation is important in coupled modeling, and conservation of energy can be pretty subtle in atmospheric models - for instance, many models don't properly account for the specific heat of condensate, nor the changes of potential energy and so rain always falls at 0ºC even in the tropics! As models improve, these assumptions will likely be modified, and so while most coupled models don't currently include PE, that won't be the case forever. (PS. The 14ºC number comes from the simple conversion of $gh = c_i \, \mathrm{delta}(T)$, with h=3000m, g=9.81 m/s2, c_i=2090 J/kg so delta(T) = 9.81*3000/2090 = 14.1ºC though the total energy flux in W/m2 is only about 0.2 W/m2 additional forcing over the ice sheets). These details are included so that we can flag these issues as potentially coming into play at some point.

2) It is good to see some discussion of how transient additions of freshwater will alter the ocean salinity and sea level. Yet, I thought this could be a teachable moment to explain the pros and cons of adding freshwater in volume vs mass conserving ocean models (rather than an oblique reference to there being a difference at line 162). I was intrigued by the discussion that volume conserving models would overestimate sea level changes since most recent ice loss has been from floating ice and models don't do hydrostatic balance of ice correctly. It seems like this should be elevated to a recommendation at the end for modelers to work on.

We agree. However, while we have our preferences we don't think it's the place of this paper to tell modeling groups what ocean models to use. We have added a new subsection in Section 3 to deal with sea level representation.

3) In Fig 1 the Antarctic side shouldn't have surface runoff and much ablation zone at the surface. It is strange to see so much of Greenland below sea level and so little of Antarctica. There are elements that seems reversed between Greenland and Antarctica. Is this really meant to be the final version of this figure?

We've updated the figure to account for this.

4) The figure captions could be improved with citations of sources and a bit more explanation of what the reader should glean from them. An example is Figs 4 & 5 where weighting functions are not defined either in the captions of main text. I would have thought a weighting function would be unitless. The caption should say where such maps are available. A few pages further into reading the manuscript I see the term "Iceber met maps" in the main text but without reference to Figs 4 & 5. Given the units are the same I'm guessing this is what is plotted in Figs 4&5. If so, be sure to reference the figures, use the correct term in the caption, and state in the caption that this is one of your datasets.

Figure captions have been rewritten to provide more explanation of the figure, and explicitly state (when appropriate) that this data is supplied as part of this work with a link to the Zenodo data URL.

Another is Fig 3 which describes data with no citation. Is this one of the products that the authors are providing?

We have added new figures for every data product and explicitly point out these are products provided as part of this work.

If so, I'm concerned about the sudden switch in methods and variability at year 1986 since I expect some users will just prescribe this as forcing to their model without being aware of it and then "discover" an amazing regime change in their model in 1986, similar to the many discoveries in AMIP runs forced with sea ice concentration from HADISST. I recommend providing two separate products: One of the longer period with the same method throughout and a shorter one since 1986. (Again a few pages further into reading the manuscript I see Fig 3 discussed in the main text, which is good. Please at least say in the caption that this is one of your datasets though.)

There are two things going on here, one was described in the submitted ms, the other was not well described. There is both a methods change and a system change near 1990. Specifically:

The source data (Mankoff, 2020) has Greenland-wide spatial resolution from 1840 through 1985 and regional spatial resolution from 1986 onward (Mankoff, 2020a). To provide regional resolution for the entire time series we take the average of the earliest five years of regional resolution (1986--1990) to determine the relative contribution of each region to the whole, and then split the whole by that proportion from 1840 through 1985.

The system change that occurred near 2000 is retreat (Greene, 2024). Zooming in on Fig. 3 (left) the system changes in 1986 - clearly due to methods. But that change appears to be only variability, not magnitude. The magnitude change (i.e., increase in discharge) occurs a few years later, near 2000, which is likely due to the system changing, not our methods.

We think the potential for dramatically different ocean responses as we transition to regional distinct fluxes from regionally uniform fluxes is small, but could be explored in future work.

5) Figure 6. I don't get why the circled 2 is called an "implicit FW" flux and the term "implicit SMB" is used at line 504. What is meant by "implicit" here and elsewhere (the term is used a lot)? It is supposed to mean implied, but I think maybe it is being used to mean derived or diagnosed from other quantities. It seems to me that no prognostic calculations in a model are implicit, so I can't grasp what implicit is supposed to mean.

“Implicit” in these situations means that this is calculated as a consequence of other factors rather than being a bottom-up parameterization or calculation. (2) is referred to as an implicit flux into the ice sheets because there is no explicit ice sheet reservoir that is being passed the local impact of the SMB. Fig 10. (now) is unchanged.

Also in Figure 6, I would think circled 2 should be labeled SMB rather than FW flux since FW flux is not as specific. The lower panel's cartoon of ice sheet to ocean fluxes is unclear and seems inconsistent with the caption. A cartoon should be an aid not a head scratcher.

The net impact of the SMB is a FW flux, and we have now made that explicit in the revised Fig. 10..

6) Lines 111-114 are too hard to follow. Break this up into more sentences and or include a table/equations.

Rewritten as: *The sub-shelf melt anomaly comes from one or the average of Davison (2023) and Paolog (2024) when they overlap, after setting the baseline to 1997.*

7) Line 138-9 Are you saying that some models just disappear the approx 3300 Gt/yr of accumulation on Antarctica? I thought models at least dropped the mass/volume of water into the ocean, as discussed a few paragraphs later. I've never heard of a credible ESM that didn't conserve freshwater at this level at least.

Indeed. This came as a bit of a shock to us too. But at least two models ignore this completely. The resulting salinity drift is small and if groups aren't doing very long runs it might not show up very obviously, but regardless, this will bias the ocean circulation and stratification around the ice sheets. We do not recommend this practice (obviously).

8) The manuscript jumps between modeling approaches and dataset details a bit in somewhat confusing ways. For example, line 379 is probably about a dataset provided, or possibly it is about a particular model. Either way the text should clarify.

We have worked to make the text flow better.