Review of Trevers et al. "Millennial-scale sensitivity of Pine Island and Thwaites Glaciers to effective pressure and ocean melt parameterisations"

September 14, 2026

General comments:

The manuscript by Trevers et al. presents a number of 1,500 year long ice-sheet simulations with the model BISICLES for Pine Island and Thwaites Glaciers that aim to test the retreat sensitivity of both glaciers with regards to different effective pressure and ocean melt magnitudes. They suggest that Pine Island Glacier is more sensitive to ocean melting and Thwaites Glacier more sensitive to bed weakening or effectively a reduction in basal friction.

When I read the title of the paper, I was quite excited about the longer than usual model integration timescales. However, when I finished reading the paper, I was left wondering why the model was integrated for such a long time and particularly disappointed with the presentation of the results and the discussion. As a result, I could not really tell what the new contribution of the current paper is. Therefore, I recommend a major rewrite of the results and discussion sections to form a coherent story that is currently missing. I also strongly recommend that the authors consider rearranging and combining figures so that the important figures are in the main text and not buried in the supplement. Below I give some advice how this could be achieved, but since these places are numerous, I may have missed some.

Specific comments:

1. My biggest concern is the presentation of the results and discussion sections, which I currently find insufficiently developed to support the scientific conclusions of the paper. The further I read, the more I got the impression that the manuscript currently reads as a collection of results that are described sequentially, rather than as a coherent scientific story. For example, paragraphs starting in lines 110, 119, 135, 143, 157, and 163 all

start with "Figure XXX shows." This makes the Results section read more like a figure-by-figure report than a scientific narrative. The constant referencing to a lot of Figures also makes this section a tedious read. Even worse is that most of them are in the supplement, making the reader go back and forth between two separate documents. The Figures should support the text, but the text should not merely describe every singly Figure in detail. Particularly puzzling are a couple of sentences in lines 173f and 201f that read like Figure captions, but for some reasons seem to merit their own paragraph.

2. This brings me to the number of Figures, especially in the supplement. I understand that the supplement can be used for additional information that is not necessarily critical for the story in the main text. For example, the videos are a worthy addition. The supplement currently contains 21 figures, which seems excessive for a 9-page paper and, more importantly, makes it difficult to identify which results are central to the manuscript's argument. Granted, this number can be drastically reduced if the single panel or double panel figures are combined. I would urge the authors to really think about which Figures are critical to their story. One of my suggestion is to at least combine Figures 3 and 5 as well as Figures 7 and 8. Potentially, you could even combine all four of them. Then the reader could hopefully see instantly which of the glaciers is more sensitive to the respective forcing, instead of comparing separate Figures that have also different y-axis scales.

3. As mentioned in my introductory statement, I was excited to see that the model was run for 1,500 years, but the topic was never picked up again why this might be a good idea and what the benefits of these longer-timescale simulations are. The abstract focuses on the next century, and much of the analysis appears to emphasise responses that could also be investigated with substantially shorter simulations. The authors should therefore explain explicitly what is learned from the centennial-to-millennial evolution that could not be established from existing century-scale studies.

4. The entire discussion section also does not convince me. But this might be partly because I do not seem to recognise what the message of the paper is. Especially, section 4.1. focuses on a limited number of ice-sheet studies that mostly did not even simulate the coupled ice sheet-basal hydrology system. And this is where the millennial timescales could be interesting because in paleo ice-sheet modelling a number of groups have actually investigated the effect of basal hydrology (often in a simplified or parameterised matter), but I feel this should be at least included in the discussion [e.g. Albrecht et al., 2020, Drew and Tarasov, 2023]. There is also a growing number of projections that have looked at the effect of water at the ice-bedrock interface and their ramifications for sea-level rise [e.g. Zhao et al., 2025]. A related question is whether the inversion of basal sliding parameters at the initial state remains appropriate when

these parameters are subsequently held fixed for 1,500 years. This seems particularly important given that the paper's central focus is on evolving basal conditions. Does the chosen height-above-flotation parameterisation provide a sufficient representation of the evolving basal hydrological state over such a long integration, or does fixing the inverted basal friction impose a limitation on the interpretation of the long-term experiments? I am interested to hear the author's opinion, but those are the kind of things that I expect in a discussion section. Because the notion basal hydrology might be important is certainly neither new nor enough substance for a discussion.

5. My last main point is admittedly more about a personal preference. But when I read the paper, there were so many acronyms. I mean everyone that works in ice-sheet modelling and on the Thwaites and Pine Island Glaciers is probably familiar with them, but I would advise to reduce them. Especially, abbreviations like SLC for sea-level contribution I find over-the-top. It is just extremely frustrating as a reader when acronyms crop up and you do not know them. Provided that we live in the age of digital publishing (mostly) and the paper is 9 pages long, we can afford the space to write things out.

Technical corrections:

Title: I disagree that different ocean melt parameterisations are tested. A particular parameterisation is chosen and then multiplied by a constant. As mentioned above, the millennial timescale is hardly mentioned in the paper.

L11: with with; also what is a highly-skilled basal hydrology model?

L20: the marine ice sheet instability

L65ff: How is calving handled in the simulations. Also what about lateral boundary conditions? From the videos, it looks like you are simulating more than just the two drainage basins...

L74: Add some information about the spin-up. At least that it is a present-day, steady-state temperature spin-up.

L80: Why is your u_0 so different from Joughin et al. 2010a. Please elaborate.

L101: Why does PIG respond slower? Slower ice-velocities or something else?

L191: Sentence starting with "At 1000 years (orange lines)..." Unclear what this refers to

L154: This does not belong like this into a paper.

L166: after 500 years

L173f and L201f: Again. Why is this part of the main text?

L235: I thought you just removed the floating ice at the beginning. Does this mean any ice that crosses the grounding line is immediately lost?

L267: What is a "marine ice-sheet model"? Delete marine please.

L271: This optimal threshold appears without sufficient justification. What is it based on, and why is it substantially higher than the value reported earlier from Joughin et al. (2010)?

Supplement: I think for completeness you should mention the magnitudes of your regularisation coefficients and how you determined them.

Figures:

As mentioned above, I think a number of Figures can be combined such that critical Figures from the supplement can be moved to the main text. Also in a number of Figures (e.g. Figs. 3 and 4), the caption is certainly erring on the side of being too short.

Fig. 1: Because the flowlines feature prominently in the paper, it might be worth mentioning how you picked them in half a sentence.

Fig. 2: Can you annotate Pine Island and Thwaites Glacier again for people who may be less familiar with the region? Also why are there so many circular looking features in the height above flotation contours. Are those ice thickness or bedrock topography artefacts? Worth explaining in my view.

Fig. 3: See comment about combining Figures. Axes labels in general are too small. Also I find the abbreviation SLC unnecessary. Just write it out. Also add to the caption in which Figure, I can find the flowline.

Fig. 4: The magenta line is not mentioned in the caption.

Sincerely, Clemens Schannwell

References

T. Albrecht, R. Winkelmann, and A. Levermann. Glacial-cycle simulations of the antarctic ice sheet with the parallel ice sheet model (pism) – part 1: Boundary conditions and climatic forcing. *The Cryosphere*, 14(2):599–632, Feb. 2020. ISSN 1994-0424. doi: 10.5194/tc-14-599-2020. URL http://dx.doi.org/10.5194/tc-14-599-2020.

M. Drew and L. Tarasov. Surging of a hudson strait-scale ice stream: subglacial hydrology matters but the process details mostly do not. *The Cryosphere*, 17(12):5391–5415, Dec. 2023. ISSN 1994-0424. doi: 10.5194/tc-17-5391-2023. URL http://dx.doi.org/10.5194/tc-17-5391-2023.

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