

Review: “Simulating Greenland Ice Slabs and Firn Aquifers with a 1D Firn Model”

by Jovanovic et al.

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1 General

In this paper, the authors analyze the formation of firn aquifers and ice slabs in the firn of the Greenland ice sheet. Their work builds on the water retention model intercomparison project (RetMIP; Vandecrux et al., 2020): they compare their model results to the output of that effort. Then, they extend their model along flowlines and examine the future evolution of firn hydrology under three scenarios. They find differences between West and East Greenland, as well as between the volume of melt. One place that this effort needs work is in distinguishing it from prior work in this area and highlighting the novelty. That said, the text, figures, model development, and analysis are in a good place and I think that the paper could be published after some minor revisions. Below I provide some general remarks on the text and I highlight some specific comments, citing line numbers where possible.

2 Remarks

1. Reading the paper, I expected there to be more discussion of ice slabs and their formation, especially given the title. Ice slab formation modeling is thorny from what I can tell, so I am curious to hear more from the authors about how they are thinking about it.
2. On line 38, the authors mention ‘permeable ice lenses’, which I think is important to flag because ice lenses are very likely impermeable. Instead, ice lenses are laterally discontinuous and water penetrates to greater depths through the spaces between the lenses. This macro-permeability is challenging to capture in one-dimensional models. By discussing this point, it could also connect with the issues around the water penetrating too far into the model domain. Similarly, I think it is important to address the lack of lateral flow in these simulations. By stacking one-dimensional simulations along flow, it can sometimes be easy to forget that they are not connected. As the authors note, Miller et al. (2020) show that the lateral flow can be substantial.
3. Although the paper compares to RetMIP data generally, I think there is an omission of context for what the other models in that comparison do and how this work improves upon what is done there. For instance, there isn’t a citation to Stevens et al. (2020), which I think is an oversight given that it is the logically most similar to the presented model. My understanding is that by changing a few parameters, all of these simulations could have been run in the Community Firn Model.

4. Connecting to the point above: one of the issues of RetMIP was that it focused on what the models did much more than (i) why the models did what they did, (ii) which models were similar and for what reasons, as well as (iii) which models are better/worse. I am not suggesting that the authors correct the problems of the past nor hold the authors to a higher standard than published work, but I do think that it is import component of this paper that could be emphasized, i.e., answering the points (i)–(iii) for their model.
5. I am curious about the model spin-up procedure for the comparison with RetMIP and the future evolution simulations. What is the initialization process? The density at depth in Figure 3A is almost exact - does it see these data before this comparison?

3 Specific comments

1. line 6: I don't know about this statement – there is a cottage industry of firm hydrology models.
2. Given that you are using TFM, which is described elsewhere, how much of the model description can go in the appendix? I assume all of it.
3. line 216-217: what is meant by “aligns well with both the mean RetMIP model output and the measured firn density.” Can that be quantified?
4. lines 334-335: awkward phrasing. I think you could add ‘we show’ somewhere in there.
5. re: Extreme melt, section 4.1.2: other models have found a similar result, so this could be contextualized in the literature.
6. Could you add the surface forcing as a small plot above the depth-time plots so that it is clear how the model is being forced?
7. I am confused about the difference (conflation?) between ice slab formation here and the natural process of forming an ablation zone. It seems like Poinar et al. (2015) is relevant here.
8. line 488: What do the authors mean by ‘degenerative nature’ here?
9. Figure A1 caption: one too many ‘m’s in your Summit.

References

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