Review for '*Microwave Radiometry Improves Modelling of Surface Melt Processes of Antarctic Firn* by Colliander et al. for TCD

Summary

This paper integrates the firn model GEMB with the microwave emission model MEMLS to simulate brightness temperatures and compares them with SMAP (L-band) and AMSR2 observations over Shackleton Ice Shelf (SIS), Antarctica. The authors show that L-band brightness temperatures are highly sensitive to liquid water content and that vertical model resolution strongly affects simulated meltwater percolation and refreezing; consequently finer vertical model resolution is recommended. The study argues that radiometric observations can help to constrain firn model physics in data-sparse Antarctic regions.

I think this paper has the potential to be strong, but I currently have a number of concerns, outlined below. As such, I have mainly focused on giving general comments. I would also like to note that I am not an expert in the technicalities of microwave radiometry, and therefore have written this review from a broader, process-oriented perspective.

General/major comments

The approach is interesting and potentially important. The integration of microwave radiometry directly into firn model evaluation goes beyond simply validating melt days, for instance, and instead attempts to constrain internal meltwater processes. As such, I think the Introduction could benefit from acknowledging prior studies that have used microwave radiometer data with either firn models (e.g. Banwell et al., 2023) or more simple energy balance models (e.g. Zheng et al. 2025) to analyze melt and melt processes on ice sheets/ice shelves.

Based on the current Introduction, the goal of the paper is not clearly articulated. Based on the conclusion stated in the penultimate sentence of the abstract (and related text in the Discussion and Conclusion), I assume that the goal is to improve the simulation of meltwater processes in firn models. However, parts of the Introduction — including the relatively lengthy paragraph about the study site — suggest that the focus is instead on better understanding melt processes on the Shackleton Ice Shelf (SIS) specifically (e.g. line 78 refers to investigating how "seasonal surface melt effects on the SIS over multiple seasons"). The authors should clarify whether the primary aim is methodological (firn model improvement) or site-specific process understanding.

Related to my point above, the study focuses on a single East Antarctic ice shelf with episodic melt, but the results— that finer resolution firn models are required— appear to be framed broadly (all of Antarctica? Greenland also?). Given that only SIS is examined, the authors should clarify the extent to which results are transferable. E.g., how might the conclusions differ for colder locations with minimal melt, or for warmer regions with more sustained melt and deeper percolation?

Given that a key conclusion is that "realistic simulation of meltwater percolation and refreezing requires substantially finer vertical resolution than typically employed," the manuscript would benefit from briefly describing how other commonly used firn models (e.g. the Community Firn Model, e.g., Medley et al., 2022; SNOWPACK, e.g., Wever et al., 2014, 2015) are typically configured in terms of vertical resolution. This context is currently missing and would help readers understand the practical implications of the findings.

Much of the evaluation is presented visually through time series comparisons of data in the three figures. I suggest adding more quantitative diagnostics, (e.g. RMSE and bias between observed and simulated TB, melt event detection metrics, melt onset/cessation timing errors, comparison of retrieved vs modelled LWA statistics etc). This would allow readers to more easily assess how much improvement is achieved by the finer vertical layering scheme.

The Discussion and Conclusion states "Fine layering offers the most accurate depiction but is computationally expensive" but no detail (here or in the Results) is provided as to how computationally expensive the differing vertical resolution layers are that are tested in the study. Some explicit detail would be very helpful.

Are LWA and LWC different? They appear to be being used interchangeably, at least in many instances.

The Discussion and Conclusion are currently combined. I recommend separating these into distinct sections. Even after reading the current section twice, the key take-home conclusions were not entirely clear to me. A concise, focused Conclusion section would help.

The figures are currently had to interpret as there lots of data is being presented (and much is small), especially in Figs 1 and 3.

Please check that all cited references paper appear in the reference list.

Specific comments to the 'study site' section:
Lines 91 – 94: The general description of what ice shelves are and why they are important is not necessary here, especially as similar text is already present in the Introduction.

96 – 106: I don't think all this detail about the study site is required, especially if the results of the study are to be broadly applicable and not about Shackleton specifically. What could be helpful to put in this section is WHY Shackleton specifically was chosen, what area of Shackleton was studied/modelled (whole ice shelf or subset?), and where ERA-5 data and AWS data are from. A map could be helpful.

References not currently cited

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