The Cryosphere Discussions

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Comment on tc-2021-230

Anonymous Referee #2

Referee comment on "Basal melt of the southern Filchner Ice Shelf, Antarctica" by Ole Zeising et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-230-RC2, 2021

Review of “Basal melt of the southern Filchner Ice Shelf, Antarctica” by Zeising et al.

In this paper, Zeising et al. present the first ground-based determination of basal melt rates of the southern Filchner Ice Shelf using repeat phase-sensitive radar measurements. They find low spatial variability in melt rates, with net freezing at only three closely spaced sites. They compare their calculated melt rates to those determined by satellite remote sensing and find that the discrepancies can mostly be explained by errors in the velocity field used in the other studies. Thus, they conclude that (1) basal melt rates determined at a single location are likely a good indication of large-scale melt rates, and (2) melt rates determined by satellite remote sensing should use the latest velocity datasets to improve accuracy.

This paper is valuable, well written, and uses novel methods. I recommend publication in The Cryosphere after some minor revisions. I have two main concerns that should be addressed. The rest of my comments are mostly line edits or requests for some clarification of details.

First, I don’t fully understand the analysis presented in Section 3.2 and Figure 4. This could be because there doesn’t seem to be a trend in the data (perhaps simply because of the scale of the vertical axis), and the plot is parametric with two other quantities. My confusion about the plot is exacerbated by the somewhat confusing text in this section. I think what the reader is supposed to understand is that the values are small and there are no discernable trends, but the presentation of the data made it difficult for me to arrive at this interpretation. The authors should consider revising this plot (perhaps with multiple panels to help the reader, rather than putting all of this in one plot) and making the text of this section clearer. There may also be a more suitable choice of independent variable than difference in draft between locations. I go into more specifics in my detailed comments, below.

Second, the Discussion section (Section 4) of the paper is limited to comparing the inferred melt rates with those determined by satellite remote sensing. This is a very useful comparison and leads to practical conclusions; however, I feel that there should be some more discussion of what the melt rates indicate about the oceanographic and glaciological conditions. There is very little context given for these melt rates and the amount of variability. What do the results (especially the low spatial variability and lack of higher melt rates close to the grounding line) tell us about melt-rate parameterizations used by numerical models, like those used by ISMIP6 (Jourdain et al., 2020) or the plume-based parameterization of Hoffman et al. (2019)? I think the impact of this paper would be increased by adding some discussion of how these melt rates relate to physical processes and our understanding of and ability to model this system.

Detailed comments:

Line 42: Unusual use of “benign”. What is meant by this?

L 67: What is meant by “low correlation chirps”?

L 70: Do you have an estimate of the uncertainty in your range estimate based on the Herron and Langway model?

L 80: “plain strain” should be “plane strain”

L 85–86: Add citation for why this is reliable for plug flow.

L ~90: Would it be possible to use the measurements from the 15 excluded stations to calculate minimum and/or maximum melt rates at these locations? What are the possible physical explanations for reasons (1) and (3)?

Fig 3 caption: Would be helpful to give the order of magnitude or the range of aPRES uncertainty

L 103: I only see two “freezing” datapoints in Fig 3. Is this just because two of those stations are very close together (Fig 1)?

L104–108: Is the implication here that the ice temperature gradients are counter-acting the expected variation due to ice draft? Can you make the connection between your results and these last few sentences more explicit?

L 112–113: Presumably BedMachine surface elevation and thickness, not surface elevation alone?

L 115: Both ΔH and Δ□□h are used in this paragraph, and seem to indicate the same quantity.

L 113–114 and Figure 4: I don’t understand what is meant by Δ□□h_b indicating “large scale basal topography for the two locations.” Is this supposed to give an indication of the overall slope or roughness, or is the change in draft really the variable of interest? For a rough ice base, you could have a Δ□□h_b of zero between two points even if there was an overall slope that could drive differences in melting. Is this statement contingent on having a smooth ice base (which the CReSIS data indicate is probably the case)? It seems like either a roughness metric, the mean draft, or the mean slope of the ice shelf base would give a better indication of the large scale basal topography.

L 119: What is “Beside” is supposed to indicate here?

L 121: “many ice thicknesses” might be an overstatement. Based on ice thicknesses in Fig 1, your measurement separations are on the order of 1–3 ice thicknesses.

L: 125: “ice shelf base base”

Fig 4. If the outlier around (11, 0.75) is removed, is there a distinguishable trend here? I’m not sure I totally understand this choice of analysis or what I am supposed to understand from how the data are presented. I think I’m supposed to understand that nearby stations have the same thermal forcing, so this is trying to remove that variability to get at the influence from draft alone, which turns out to be small. However, it seems like the values of Δ□□h_b here are small enough that I wouldn't expect draft to be at all important in explaining the difference in melt rates between sites. I would expect that local oceanographic properties, ice temperature, local ice base slope, or ice base roughness would be more important than a few-meter change in draft. Of course, those quantities are not readily available from existing data, and so it is difficult to determine a relevant independent variable. Would some more meaningful pattern emerge if you plotted melt rate differences as a function of horizontal distance between sites? Then you could include locations outside of just the stations within 2 km of each other. Or alternatively, you could calculate the ice base slope over O(100m) length scales using the BedMachine draft. Or perhaps the text in Section 3.2 just needs to be revised to explain this figure more clearly.

L 177: Can you discuss why you cannot extract a rate of freeze-on? Presumably salty ice with high conductivity and/or low density does not allow for determination of the ice base, but it would be helpful to be more explicit about this. The use of "as yet" suggests that there may be some way around these difficulties. Can you elucidate what these might be?

References:

Hoffman, M. J., Asay-Davis, X., Price, S. F., Fyke, J., and Perego, M.: Effect of Subshelf Melt Variability on Sea Level Rise Contribution From Thwaites Glacier, Antarctica, Journal of Geophysical Research: Earth Surface, n/a, https://doi.org/10.1029/2019JF005155, 2019.

Jourdain, N. C., Asay-Davis, X., Hattermann, T., Straneo, F., Seroussi, H., Little, C. M., and Nowicki, S.: A protocol for calculating basal melt rates in the ISMIP6 Antarctic ice sheet projections, 14, 3111–3134, https://doi.org/10.5194/tc-14-3111-2020, 2020.