Review: Spatial and temporal variations in basal melting at Nivlisen ice shelf, East Antarctica, derived from phase-sensitive radars

This paper presents new measurements of sub-shelf melt rates of Nivlisen Ice Shelf in Dronning Maud Land, acquired with ApRES. The survey includes measurements across a broad area of the shelf at yearly resolution and at two points with 36-hour resolution, allowing the authors to study both spatial and temporal variations in melt. The melt rates on Nivlisen are found to be relatively modest, with the highest melt rates in the summer and just behind an ice rumple. These melt rate measurements are compared to a common-offset radar survey of ice-shelf thickness and to atmospheric data. While there is no correlation between ice-shelf thickness and melt rates, the atmospheric data suggest that the highest melt rates may be caused by wind pushing warm surface waters beneath the shelf.

The acquisition of ApRES data to determine melt rates is highly valuable as it allows direct measurement of ice-thickness changes while removing assumptions about firn thickness, strain rates, and/or hydrostatic equilibrium that affect other techniques. The authors have done a careful job in processing the data and assessing the uncertainty in the measurements, and performed extensive and detailed analysis of those results. Relatively few studies have used pRES on ice shelves, and the precision, temporal resolution, and relatively large spatial extent of these measurements make this paper a valuable insight into processes controlling melt, particularly beneath East Antarctic ice shelves. I have a number of comments, primarily focusing on the presentation and discussion, but I think the paper is a nice contribution and will soon be suitable for publication in *The Cryosphere*.

General comments:

The lengthy discussion of Jacobs et al.'s melt modes is too meandering to be easily followed. If this section is retained, I would recommend restructuring to introduce all 3 melt modes with brief definitions first before going on to detail each. In the conclusion, where mode-2 is mentioned again after having been absent since the introduction, it needs redefining. However, I do not find this division of modes to a very clean distinction for the purposes of this study. Instead, perhaps simply say that melt can be driven either by warm summer water near the surface or by warm water at depth and provide citations for each.

It seems like a stretch to call 4 m/yr "high" melt given the rates observed in West Antarctica. Sometimes this melt is described as "high" and sometimes as "higher"—I think remaining consistent calling it "higher" would be most clear.

The distinction between high melt and melt that is in excess of steady state gets a bit muddled here, partly because of the repeated use of the phrase "mass loss" to mean an outgoing flux of ice rather than a loss of total ice volume. I would suggest other terminology, such as "outgoing flux" or something similar, so as to clearly distinguish from a net loss. While I can figure out what is intended, I find the phrasing particularly distracting in the discussion of ice-shelf stability, since the measurements all indicate the melt rates at a particular, with no clear measure of whether those rates are sustainable or "normal". This ambiguity extends into the conclusions—most of the second paragraph of the conclusions is not a conclusion of this work, but more-or-less a hypothesis that "mode-3" melt may affect the stability of some ice shelves. It

is fine/good to make this argument, but I would not consider it a conclusion of this work and would find this paragraph more appropriate merged into the last section of the discussion (and perhaps reiterated in a single sentence in the conclusion).

In section 5.2, it would be nice to see a bit more connection between the different paragraphs. There is a lot of nice, detailed analysis of the phases and spectral power of the melt, but it is hard to know what to make of it in the aggregate. At present, the summary paragraph at the end of this section really just focuses on wind; it would be a huge help to use this paragraph to explain how the phase lead/lag of the seaward/landward sites can be related to the wind forcing, and to whether the spectral power of the melt at each site individually tells us anything about the validity of these conclusions.

All figures except Figure 1 should be enlarged. Simply expanding them to take up the full-page width would help significantly. Even with that expansion, though, some text needs to be further enlarged.

Specific Comments:

L41: For Nivlisen, surface melt/sublimation must be included in the inputs and outputs

L49: Even though Rignot et al. state something similar this, I think this mischaracterizes the results of those studies; they both show calving and melt are equal within error.

L56: This sentence needs the context that this is the mode affecting the largest shelves

L75: What do you mean by "only recently"? Is this a change in occurrence or in observability? Why does this recentness suggest that it is important?

L121-124: Is the inland geography relevant anywhere in the rest of the paper? I think this can be removed

L129: Maybe move ice rise/rumple definition to where they are introduced in L114.

L160: Would be clearer to say "the ice front retreated to its present position by ~11 kyr ago"

L162-165: The wording here makes the meaning unclear—is the entrainment in line 163 the same as in 165, or are two different processes being described? In line 163, the reader needs to know what the CDW is being entrained into.

L198: Maybe mention the battery capacity here, since I'm sure others are considering similar deployments

L237-239: I'm not entirely clear what is meant here. You assume that strain varies either on very long timescales or on timescales shorter than 36H but not in between—essentially a bandstop filter? Are variations with the frequency of other tidal components small?

L271: Do you mean that the effect of horizontal positioning on the error in the vertical is 0.1±0.2 m?

L278: Citation for CSRS-PPP? Static or kinematic processing?

L299-301: I'm guessing you exclude the sites near the ice rumple because you were unable to revisit them? Perhaps mention this explicitly here.

L340: Can you say definitively that Bedmap2 is too high or could the thickness have changed?

L362-364: This sentence seems a bit backwards to me, but I know little about vorticity waves—can you clarify the mechanism for reducing melt rates and restructure the sentence so that cause and effect are clear?

L370-372: The language here should be made clearer. The measurements seem to indicate near perfect balance, so why would anything happen as a result of these rates being sustained?

L559: Based on the evidence provided in the paper, it would be more appropriate to say that the melt rates are susceptible rather than that the ice shelves are susceptible.

Figure 2: The color scales should be changed to match between the point measurements and the rasters in b-d.

Supplementary figure 1: Why is the x-axis in panel a in meters after a Fourier transform? Should it not be in Hz, or is this not the transformed data?

Technical Corrections:

L36: shrinking suggests extent, thinning would be more appropriate
L68: Tottem => Totten
L98: subject/verb disagreement
L103: “to explain them using” is an awkward phrase here
L154-155: This sentence needs a subject
L253: Line spacing of 5 km? Trace spacing of 5 m? I think there is a typo here.
L296: close to => just upstream of?
L305: average rate of thickness change
L561: there is a typo somewhere in “may increase leading”
L566: The first comma should not be there