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Interactive comment on "Spatial and temporal variations in basal melting at Nivlisen ice shelf, East Antarctica, derived from phase-sensitive radars" *by* Katrin Lindbäck et al.

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Dear Reviewer,

On behalf of all the authors of this discussion paper, I would like to thank you for your comments. Your suggestions have been acknowledged and have improved the paper substantially. Our responses can be found below.

Kind regards,

Katrin Lindbäck

RC1 General comments

RC1.1

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.

Author response:

Thanks for your positive comments, very much appreciated!

RC1.2

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.

Author response:

We have restructured the section about the melt modes, clearly starting with each mode and its definition.

RC1.3

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.

Author response:

We have changed "high melt" to "higher melt" throughout the manuscript.

RC1.4

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 iceshelf 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 conclusionsâĂŤmost 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).

Author response:

We have changed the term “mass loss” to “stability”. We have also moved the paragraph in the conclusions to the discussion. We hope is clearer now the distinction about the current status and potential future change.

RC1.5

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.

Author response:

We have rewritten the last paragraph in the discussion give a better overview.

RC1.6

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.

Author response:

The figure size is fixed to a certain width by TC, if we understood it correctly, and cannot be set by us to fill the full-page width in a pdf. We have, nevertheless, enlarged the text in the figures to make it more readable and will do a final check before publication to make sure it looks ok.

RC2 Specific comments

RC2.1

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

Author response:

We have added surface melting and sublimation to the sentence.

RC2.2

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.

Author response:

We have rephrased the sentence as suggested (melt and calving equal).

RC2.3

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

Author response:

We have clarified that this affects the largest ice shelves.

RC2.4

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?

Author response:

We have removed “only recently”.

RC2.5

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

Author response:

We have removed the sentences.

RC2.6

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

Author response:

We have moved the definition to the suggested section.

RC2.7

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

Author response:

We have rephrased the sentence as suggested.

RC2.8

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.

Author response:

The water is entrained into the Antarctic slope current. We have clarified the sentence.

RC2.9

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

Author response:

We have included the battery capacity.

RC2.10

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 betweenâĂŤessentially a bandstop filter? Are variations with the frequency of other tidal components small?

Author response:

This is not actually a bandstop filter. To get the mean melt rate, we needed to remove the time-average strain rate, which we needed to calculate elsewhere, essentially by comparing vertical profiles throughout the time series to see how the internal reflectors move with respect to each other. That correction sets the level of the melt rate. After that we assume that the main remaining vertical strain signal that needs to be removed is from tidal variation in the semi-diurnal and diurnal bands. So instead of trying to calculate the vertical strain rate at tidal frequencies (very difficult to do precisely because of the weakness of the internal reflections) we throw out all tidal variability (melt and strain) by filtering at 36 hours. That leaves us with the variability of most interest here. The assumption is that there is no significant tidal strain at frequencies slower than diurnal, except for the constant background strain rate. In some large ice shelves, a fortnightly signal is visible in the vertical strain rates, as a result of non-linear interactions between the diurnal and/or semidiurnal tides. That signal was not strong at these sites. We have clarified this in the text.

RC2.11

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

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Author response:

Yes, we have clarified this in the text.

RC2.12

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

Author response:

We have added a reference to the processing and stated that it is static processing.

RC2.13

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.

Author response:

We have added a sentence clarifying this as suggested.

RC2.14

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

Author response:

Bedmap2 is 50-100 m off in this area and indicates an ice-rise like feature that cannot be seen in neither our radar data, our GNSS data nor the new REMA product. The ice-shelf is very flat in this area and a major change over the last few decades is unlikely. We edited the related sentence in the paper as follows: "The broad thickness pattern agrees with the gridded ice thickness of Bedmap2 (Fretwell et al., 2013), except on the western ice tongue (profile C), where the thickness of Bedmap2 is clearly too high (Fig. 2b), possibly due to errors in the input data or the interpolation between them."

RC2.15

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?

Author response:

We have rephrased the sentence and left out the term “vorticity waves” and just described the strong tidal currents in shallow regions (thin water column thickness) around the ice rise that may increase the ice-ocean heat exchange.

RC2.16

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?

Author response:

We have rephrased the sentence and replaced “sustained high melt rates” with “an increased basal melting in the future”.

RC2.17

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.

Author response:

We have removed the word “susceptible” in the sentence.

RC2.18:

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

Author response:

We have updated the figure as suggested by Reviwer #2, with difference between the in situ measured values and satellite or modelled values. The measured values are kept as numbers.

RC2.19

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?

Author response:

For a FMCW radar, the frequency of each component of the data that are acquired represents the range to a reflector via the formula R=T·f·v_i/(2·B), where v_i is the radar speed in ice, f is the frequency associated with the reflection at range R, T is the length of the chirp in seconds, and B is the bandwidth of the chirp. So we have taken the Fourier Transform, and converted to range using the above formula. We have clarified this in the figure caption.

RC3 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 Author response

Author response:

We have corrected all these errors. Thanks for pointing them out!

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