

Ocean Sci. Discuss., referee comment RC1
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Comment on os-2021-111

Carolyn Begeman (Referee)

Referee comment on "Evaluation of basal melting parameterisations using in situ ocean and melting observations from the Amery Ice Shelf, East Antarctica" by Madelaine Rosevear et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-111-RC1>, 2021

General comments:

Rosevear et al. present a rare and valuable dataset of concurrently observed ocean properties and melt rates. Such datasets are critical for constraining melt parameterizations and that is a major focus of this study. The authors performed some of the expected comparisons of parameterizations from their data and also went above and beyond in synthesizing observational agreement to date with a common parameterization. The main question that remains unaddressed is whether the authors could get a good fit for all three/four time periods with a single parameterization if $c_d^{1/2} \cdot \Gamma_T$ were tuned. This is of great relevance to determining whether MK18 really is the best choice at this site. I'd also like to see some discussion of the relationships between thermal driving and melt rate (and u_{mean} , u_{tidal} and melt rate) for each of the four periods analyzed. I imagine there is some reason the authors have omitted this discussion (nor plotted dT vs. melt rate), but this reason should be stated.

There were clearly some difficulties processing the ADCP dataset but the authors seem to have approached it rigorously. I have a few minor comments on this, detailed below.

I would also have appreciated more contextualization of the meIt seasonality with, e.g., seasonal variability in AASW and HSSW properties and whether the phase relationships are compatible with advection timescales from the ice-shelf front to the observational site. I see that you cite ISW concentrations but do you have a hypothesis that would explain the timing of those changes?

Overall, I think the manuscript is excellent and well-written and I believe if the authors address my comments here and below it will be suitable for publication.

Specific comments:

L5: Later, explain how your application of the ADCP is novel

L34: I'm not sure what exactly you mean by intermediate depths, but I'd say that for warm water cavities mCDW can also drive melting at deep depths.

L36: This section heading was unexpected. Attempt to offer transition at the end of the previous paragraph.

L119: "During this period..." implies that $U_{7-19} > U_{19-91}$ from Aug-Dec but this appears to only be the case from Oct-Nov

L126: more details about the mooring needed, particularly given that the ADCP swings. By how much is the ADCP and other sensors moving? Is the mooring end affixed to the seafloor? How might this motion contaminate the measurements? How have you accounted for this motion?

Section 2.1: I found the relationship between the morphology determination and the melt rate determination confusing here. I understand the space and time bins you've used for the melt rate determination but not those for the basal morphology shown in Figure 7. In addition, since you say the BT is noisy, what is the uncertainty on the morphology shown in Figure 7?

L148: It's unclear how this heading range is chosen. Is this the range that is closer than 100m from the sensor?

Table 1: Why doesn't interface pressure have a start date? Isn't this measured at the time of the CTD casts?

L171: Is this slope compatible with the eddy diffusivity of heat being greater than salt? If so, state that.

L190: "This indicates..." Logic of this sentence is unclear. I think you're talking about meltwater accumulating as mean flow is roughly from AM02 to AM06. Please clarify. In general, your explanation of why freshening occurs in spring wasn't clear to me.

Figure 5. Is the grey an uncertainty envelope? Explain in caption.

Figure 6. I assume the current direction is measured from north? Specify in caption. In addition, You don't explain why the RMS data is missing until later. Please state in the caption or when Figure 6 is first referenced.

L205: "consistently biased" Have you ruled out instrumental bias? If so, please state in the manuscript.

Figure 8: Why are the horizontal lines 2 months long if the measurements are at monthly intervals?

Section 3.5: Is there a reason why you don't have a panel similar to those in figure B1 but with the annual average melt rate? Can you identify significant spatial variation in melt rate? If not, why not?

Section 4.1: It would be helpful to include in this section the explanation for why you average melt rates over 3 months.

Section 4.4: Readers should be reminded that MK18 does not depend on current speed (residual or tidal)

Table 3: I'd like to see the melt rate uncertainty over these 3 month windows

L289: Can you remind us what the upper bound on the microcat depth is, here or on the lines when you discuss sensitivity to this choice? Can you also indicate to us that you are going to discuss sensitivity to depth later?

L305: Can you include a figure in the supplement or appendix that shows us that the upper water column velocity structure is relatively depth independent?

L307: This missing data should be mentioned earlier.

Figure 9: I really appreciate that the authors have computed the conductive flux, as most studies neglect this term. However, I think that this figure would be more appropriate for the appendix given the relative importance of the conductive heat flux sensitivity to melt rate when compared with the other figures and results.

L335: Here, it's unclear whether "different averaging periods" refers to averaging periods of different durations or different start/end times.

L336: "The periods were..." I would have rather had this information when you were introducing data choices.

L350: Remind readers that MK18 is a convective parameterization

L361: This is confusing: Malyrenko et al. suggest a critical Re_{Δ} for horizontal ice settings yet "it's not clear that it can be applied to our (horizontal) site"

L362: In this paragraph, it would be good to get a sense for how much getting the local slope right might matter. That is, how much would the predicted melt rate change if the large-scale slope of 0.1deg were used instead of the local 9deg slope?

L427: Here or elsewhere, clarify that what you hypothesize is a mostly barotropic flow bringing both ISW and HSSW from shallower depths near the ice-shelf front to your observational site.

Technical comments:

L1: Phrasing here is convoluted. Would be clearer to write "is accelerating loss of grounded ice" or similar.

L9: Might as well express the seasonal variability as a percentage of the mean here

L11: Can remove ~ since you've already specified "typical" speeds

Figure 1: Specify what dashed line is. Schematic arrow for the sub-ice-shelf circulation would also be helpful.

L23: demonstrated >> demonstrated that

L27: melts >> melt

L30,31: watermasses >> water masses

L43: The ISW >> ISW

L66: In the >> The

L71: roughness >> ice shelf basal roughness

L74: parameterised >> parameterised in these ocean models

L83: Gamma_T >> Gamma_S

L92: Use $T_b - T_{ML}$ instead of T' or specify what T' refers to

L105: and with \gg and scales as

L110: the tendency of \gg found that ... tended to reproduce

L113: the questions \gg these questions

L118: ice shelf \gg ice shelf there

L148: Can you choose a different notation for heading or ice shelf slope so that they cannot be confused?

Figure 7: Make explicit that the black box is the $-30 < \theta < 46$ in caption or text.

L235: Remind us that AM06 is on the eastern flank

L248: "The maximum..." The logic of this sentence is unclear.

L296: I'm not sure why you use the LOW abbreviation as I don't think it appears again.

L327: Since the equations are in the appendix, the readers need to be reminded what the variables refer to if it's been a while since you introduced them. Here, remind them what Q_T is.

L436: bases \gg basal topography

L454: delete "the"