OS-2021-111 response to R2

Dear Reviewer,

Thank you for taking the time to review our work, your constructive comments have been extremely useful in improving our manuscript.

In the text below we address all your comments item-by-item, where our responses are in blue.

We have made two small changes that were not specifically requested by either you or R1, but seemed a logical extension when we made the requested changes:

- Firstly, thanks to your question about whether we use the mean+tidal velocities to calculate melt rates, we realized that we had misinterpreted the velocity reported in the text in Jenkins et. al., 2010. They state that the mean current value is 3.6 cm/s, which we misinterpreted as the time-mean of the current speed (i.e. the tidal+non-tidal components) rather than just the non-tidal component. Consequently, we don't know what mean(U) is, and cannot include the FRIS site on Fig. 10 any more. Since the parameterisation being tested Fig 10 (J10) is tuned to this dataset in the first place, this data point is not really needed in any case.
- Secondly, we modified the lengths of the averaging periods used to quantitatively assess the performance of the three parameterisation against observations from 4 months to 2-3 months. This facilitated the discussion that R1 requested around thermal driving, current speed and melting

Finally, we wish to acknowledge your contribution (text starting L554).

Major comment:

The values of $C_d\Gamma_{T/S}$ provided by Jenkins et al. (2010) come from a fit to observations beneath the Ronne Ice Shelf. I don't think that these values should be considered as the best values to parameterize melt rates beneath Amery and I would expect these values to be recalculated for Amery based on the new observations. Similarly, the formulation of Holland and Jenkins (1999) seems to be based on a few days of measurements beneath sea ice in Greenland (Mc Phee 1987) and I would expect the authors to use the new observations beneath Amery to re-calibrate some of the parameters (e.g. ξ).

Regarding the Jenkins 2010 parameterisation, one of the main aims of the paper was to determine whether parameters fit to the Ronne ice shelf could be used for the Amery (and others). We found that they could not, indicating that a parameterisation of this type ($(C_d)^{1/2}\Gamma_{T/S}$ = constant) will be inaccurate if applied everywhere. We think it is worthwhile to have established this. We have presented the best fit $(C_d)^{1/2}\Gamma_{T/S}$ values for the Amery as a point of comparison, with new discussion starting L380.

We have not recalibrated other parameters using AM06 data for a few reasons. Firstly, since turbulent quantities were not measured, the system is undetermined. If u^* were measured (as in Davis et al 2019), then C_d would be known and we could start to look at the transfer coefficients in more detail. Secondly, since the melt rate data has very poor temporal resolution, it is challenging to even test the functional form of the parameterisations (since quantities such as U, T^* and m do not vary much when averaged over the long periods we found to be necessary). Thirdly, we wanted to move away from the approach of tuning parameterisations to each ice shelf, since if a parameterisation needs to be tuned for each ice shelf, or each site, it is not particularly useful for modeling purposes.

In light of this, our approach was to consider which parameterisations could best replicate observed melting at AM06. Where the parameterisations could not replicate the melting, we investigated how the ocean conditions might explain the poor performance (e.g. section 4.4 and section 4.5 from L425 onwards)

In the way things are presented in sections 4.3 and 4.5 and in the Abstract and Conclusion, it is unclear to me whether the J10 and HJ99 parameterizations are highly biased because of a poor calibration or because their formulation is intrinsically wrong. With smaller values of $C_d\Gamma_{T/S}$, there would be a better match for a majority of ice shelves. Or could the formulation be considered wrong because it requires significantly different $C_d\Gamma_{T/S}$ values for the various Antarctic ice shelves?

Thanks very much for pointing out that this isn't clear in the paper at present. For J10, we propose that the formulation is intrinsically wrong, or at least not applicable over the full range of relevant/observed ice shelf conditions. We have now clarified this in section 4.5 (starting L423) and reiterated it in the Conclusions (starting L494).

To be fair in the comparison between shearcontrolled and convection-controlled formulations, would it be possible to do something like Table 4 but for the MK18 parameterization (at least for some of them or based on existing ice topography datasets)?

While this is possible, it would not be meaningful since existing ice topography datasets are too coarsely resolved/"smooth" to capture the local slope, like what we measure at AM06. (see discussion L402). Without this, a convective parameterization will significantly underestimate melting. For example, if we use a basal slope of theta=0.2 degrees rather than ~9 degrees at the AM06 site, the MK18 parameterisation predicts a melt rate of ~3 cm/yr.

<u>Minor comments and edits:</u>

L. 83: the second ΓT should be ΓS

thanks, done

L. 133: "used map" -> used to map.

thanks, done

L. 319-320: "by surface accumulation" or ice convergence.

thanks, done

L. 346-348: If I understand correctly, the last column of Table 3 is a recalibration of J10. Could you compare to the original values of J10?

Thanks, we have added some discussion about this starting L379

Could you do something similar to recalibrate HJ99 (e.g. changing ξ).

See response to major comments.

Does the melt calculation in Table 4 and Figure 11 for the other locations take local tidal velocities into account? The formulation should not be based on U but more on something like U! (which could be roughly estimated from CATS or an equivalent tidal model).

In summary, yes, the melt calculation does take the tidal velocity into account, since we use the measured velocity at each site which is a combination of the tidal and mean components. We use the time-mean current speed rather than the background current speed plus the root-mean-square tidal velocity, since the latter approach was suggested as a way to include tidal effects on melting in ocean models that do not include tides.