Review of "Evaluation of basal melting parameterisations using in situ ocean and melting observations from the Amery Ice Shelf, East Antarctica" by Madeleine Rosevear, Benjamin Galton-Fenzi and Craig Stevens.

The authors present an outstanding set of ocean observations beneath the Amery ice shelf. The paper is clearly written, with a very good review in the Introduction and a thorough presentation of the new observations. This is a very useful manuscript as there are relatively few studies questioning the dominant use of shear-controlled formulations in ocean models. I have a single concern about the main conclusion of the paper on the overestimated melt rates in the shear-controlled formulations, as detailed below, but I think this can be addressed in a revised manuscript.

Major comment:

The values of $\sqrt{C_d}\Gamma_{T/S}$ 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). 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 $\sqrt{C_d}\Gamma_{T/S}$, there would be a better match for a majority of ice shelves. Or could the formulation be considered wrong because it requires significantly different $\sqrt{C_d}\Gamma_{T/S}$ values for the various Antarctic ice shelves? To be fair in the comparison between shear-controlled 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)?

Minor comments and edits:

- L. 83: the second Γ_T should be Γ_S
- L. 133: "used map" -> used to map.
- L. 319-320: "by surface accumulation" or ice convergence.
- 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? Could you do something similar to recalibrate HJ99 (e.g. changing ξ).
- 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 $\overline{U}$ but more on something like $\sqrt{\overline{U^2}}$ (which could be roughly estimated from CATS or an equivalent tidal model).