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Interactive comment on "A protocol for calculating basal melt rates in the ISMIP6 Antarctic ice sheet projections" *by* Nicolas C. Jourdain et al.

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The authors present a new methodology for calculating melt rates at the base of Antarctic ice shelves to serve ISMIP6 (Ice Sheet Model Intercomparison Project for CMIP6). Based on existing observational data sets (WOA18p, EN4, and MEOP) a present-day climatology has been constructed. The evolution of ocean temperature and salinity is derived from climate models by calculating anomalies as differences between the annual means and the 1995-2014 average, then added to the present-day climatology. The proposed parameterization of basal melting depends quadratically on, due to the architecture of the ISMIP6-ice sheet models, either non-local or local thermal forcing, both constrained by the observed temperature climatology. Two calibration methods are proposed based on (1) the mean Antarctic melt rate and (2) melt rates near the

deep grounding line of Pine Island Ice Shelf, the latter to cover a high melting regime expected to become widespread in a warming climate. The still existing deficiencies of this approach ask for the consideration of more physics related to the cavity processes and more and long-term observations of hydrographic characteristics and basal melt rates.

General comments: Once having proposed a 'simple' box model to provide melt rates beneath Antarctic ice shelves and watched recent efforts with the same purpose, I highly appreciate this kind of approach based on data from ocean and ice shelf observations – though, it is just a step in the right direction and does not mark the end of the effort! My comments/questions are mostly marginal and, hopefully, will not hamper a rapid publication in TC.

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Specific comments (according to page and line numbers):

P05L14: It took me a while to realize that ice shelf draft is not an issue and the open ocean profiles are extrapolated horizontally even into the ice. If I am right, please add a sub-clause for clarification.

P08L07: I still puzzle about the procedure of extrapolating shelf water characteristics into the cavities, especially after having seen the thin lines in Figure 2. Looking at the southern Weddell Sea, one gets the impression that only a narrow band along the Filchner-Ronne Ice Shelf front has been considered, which does not even cover the most western part where High Salinity Shelf Water (HSSW), the fuel for basal melting, exists. Similar for the Ross Ice Shelf, where the most saline HSSW of McMurdo Sound is extrapolated into the cavities fringing the western Ross Sea. In addition, it is still a big unknown and its implementation technically not easy, but one should mention somewhere because of that the extrapolation into the cavities does not follow possible routes of cavity inflows.

P12L06: With regard to the non-local melting parameterization and the thermal forcing averaged over all ice shelves of a particular sector, one wonders how sensitive this approach is to the distribution of ice shelf drafts. I assume thermal forcing to be shifted to higher values for a sector with predominantly deep bases but this overestimates melting at lower (than average) bases.

P13L06: Refreezing – obviously cannot happen for the local parameterization. Reese et al. (2018b) point to the impact of basal melting on ice flux across grounding lines, but I assume that refreezing has the same impact, at least for the big ice shelves where refreezing is widespread.

P13L17: What is the reason for using 10ˆ5 random samples. Why not more or less?

P13L19: The uncertainty of 0.17K comes out of the blue.

P15Tab2: Some 'first-glance-surprising' results are presented without the slight attempt for an explanation. Here is an example: what is the reason for the big difference (~3 times) in the median for non-local and local PIGL?

P16Fig4: It took me a while to realize that the whole gray pattern (upper left) is the PIG with area of highest melt rates in red.

P18L05: First, it is impossible to distinguish the different lines in Fig. 6 on a print. I had to go back to the online pdf-file to follow the writing. Second, 'good agreement' tends to be a self-serving statement, since the good agreement only holds for the depth range 400 – 1000 m, which, for FRIS, is above the depth of most grounding lines of the major ice streams.

P18L10. Since I cannot find any deltaT < 0 in Fig. 5c (non-local PIGL), why 'almost' everywhere?

P18L19: Another example for a banged out result: the significant refreezing in the Bellingshausen Sea. Note, it is produced by non-local PIGL, though Bellingshausen Sea is known for a warm continental shelf. Without further explanation, such features

might cause doubts on the general applicability of the method. It cannot be the goal of this work that at the end of the day the user is forced to apply melt rates from different parameterizations/calibrations to different sectors.

P20Fig7: What is the advantage of presenting the maximum melt rate for a particular sector without knowing its location?

P22L08: Please explain why you chose the output of NorESM1-M for the different projections. Is it a coincidence that melt rates start to increase in the 2070's for RCP8.5 like in BRIOS-A1B and FESOM? Where does it happen - everywhere?

P24L04: Please note that, due to Y. Nakayama's PhD work (Nakayama et al., 2014), FESOM improved significantly in the Amundsen Sea with the latest on the issue facing the review process. While spending a whole paragraph on FESOM's deficiencies, it would be just fair to mention the improvements, same you did for WAO18.

P26Tab3: Isn't it dangerous for the general acceptance by the ice-sheet model community to show how different – orders of magnitude – the gamma_0's and the resulting melt rates at the deep grounding lines are when basal slopes are considered? The reader/user might be confused and should await a final recommendation for the preferred melt rate parameterization.

Technical corrections (according to page and line numbers):

P05L10: Assume that there is a wide spread in the characteristics (not distribution) of water masses simulated by the CMIP models.

P06L06: "..., which represents an acceptable compromise..."

P09L06: It was Jenkins et al. (2010) who first discovered the control of sub-ice shelf bathymetric features on cavity properties.

P10L04: - and the following pages, the term Filchner-Ronne Ice Shelf is widely recognized, e.g. in Reese et al. (2018).

P10L06: “These processes are not represented...”.

P13L20: “...forcing, respectively.”

P24L22: “This data, ...“

P24L30: (Fig.5a) – non-local MeanAnt

P25Fig10: Same problem with the too-thin lines especially in the insert, and it should be mentioned that the scale of the y-axis differs.

References

Jenkins, A. et al. (2010) Observations beneath Pine Island Glacier in West Antarctica and implications for its retreat. Nature Geoscience, 3, 468-472, https://doi.org/10.1038/NGEO890.

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