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The Cryosphere Discussions

Interactive comment on "Brief communication: PICOP, a new ocean melt parameterization under ice shelves combining PICO and a plume model" *by* Tyler Pelle et al.

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In this brief communication, the authors present a new parameterization of ocean induced ice shelf basal melting based on a quite simple numerical model. PICOP is a combination of the simplified box model (PICO) and a 2-d plume model. The authors claim that the model is more appropriate to use for transient coupled ice sheet – ocean model runs, since it resolves many of the problems caused by the simplifications/ assumptions made in the original models.

General comments:

A Brief Communication in TC might allow for a more superficial description of the various model components, but to gain a thorough insight the reader is forced to read the original publications of Reese et al. (2018) and Lazeroms et al. (2018). Without doing so, one has to trust that the model set-up is correct, the right parameters have been used, and a realistic forcing is applied. Thereby, the only way left for evaluation is the comparison of basal melt rates (Fig. 5) for three different ice shelf regions at the rim of the Antarctic Ice Sheet. The latter, however, bears some subjectivity and might have tempted the authors to use the term 'in excellent agreement'. However, first, the authors 'only' compare their results with one observation (Rignot et al., 2013), which actually is not an in-situ observation. Second, it is not obvious to the reader which set of parameters has be used, what tuning has been applied to reach this agreement. Third, the agreement for Filchner-Ronne Ice Shelf (FRIS) is difficult to assess since an inappropriate color scale is used. Various approaches show, and the results of Reese et al. (2018) greatly exaggerate, that refreezing occurs below FRIS. I hope it is only a matter of the color scale, since the pattern shows promising. However, melting at the ice shelf front can also not be resolved by PICOP, since it is a different process which drives this frontal melting.

Therefore, I urge the authors to spend more time on validation of the PICOP results, but if done, I recommend publication in TC after consideration of the comments/ corrections listed below.

Specific comments:

P1L18: The term 'upwelling' is not correctly used. In Physical Oceanography, 'upwelling' means the vertical displacement of deep water masses towards the surface, caused by the prevailing winds, e.g., at the Antarctic Divergence, off Namibia, etc. Here, it is the spreading of mCDW onto the continental shelf and, sometimes, into the fringing ice shelf cavities.

P2L16: The name might be misleading, but PICO was NOT designed to reproduce the density driven overturning circulation within sub-ice shelf cavities. The latter, however, was the intention of the box model developed by Olbers & Hellmer (2010).

P4L03: It is unclear where the ice depth-averaged velocity 'v' is actually coming from – either from the ice-sheet model coupled to PICOP or from observations. In addition, 'epsilon' is not listed in Table 1.

P4L07: Same for z_b – assumed to be taken from Bedmap2.

P4L27: The assumption that the ambient temperature in the ice shelf cavity T_a is equal or above the surface freezing point is unfortunate, at least for cold cavities like FRIS and Ross Ice Shelf, since the observations show temperatures well below T_f characteristic of Ice Shelf Water. This assumption might lead to an exaggeration of basal melting in these cavities.

P5L12: The mean salinity for the Amundsen Sea of 34.86 is wrong, e.g., Dutrieux et al. (2014), and seems to be a transposed digits since 34.68 makes more sense. Such high salinity causes a stronger overturning circulation and thus higher basal melting. Same for the southern Weddell Sea continental shelf, where a salinity above 34.8 is only observed in the far western corner, which – I have to admit – provides most of the shelf water fueling basal melting underneath Ronne Ice Shelf.

Table 1: Checking the constant parameters used in Reese et al. (2018) one realizes that they sometimes differ from those used for PICOP, which leaves the impression that Table 1 shows a mixture of parameters used in the original models. In addition, the overturning strength C used in Reese et al. (2018) – Table 1 was a best-fit, which does not necessarily mean that a strength of 1x10ˆ6 is also the best choice for PICOP. Finally, please explain why most units include '°C' but the 'freezing point-depth coefficient'.

Technical corrections

P1L01: 'basal melt' is the liquid resulting from ocean induced ice shelf 'basal melting'

– please use the latter term.

P1L11 - and the following: The term Filchner-Ronne Ice Shelf is widely recognized, e.g. in Reese et al. (2018).

P1L21: "... along the periphery OF the AIS....; sentence too long, split in two.

P3L08: Temperature 'T' and salinity 'S' are already used for the far field temperature and salinity. I suggest omitting "with temperature T, and salinity S".

P7L21: "... are projected to change within the coming century, ... " needs a reference.

References

Olbers, D. and Hellmer, H. H. (2010) A box model of circulation and melting in ice shelf caverns. Ocean Dynamics, 60, 141-153, https://doi.org/10.1007/s10236-009-0252-z.

Dutrieux et al. (2014) Strong sensitivity of Pine Island Ice Shelf melting to climate variability. Science, 343, 174-178, https://doi.org/10.1126/science.1244341

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