The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-216-RC1, 2018

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.

Gwyther (Referee)

david.gwyther@gmail.com

Received and published: 29 November 2018

In this brief communication "PICOP, a new ocean melt parameterization under ice shelves combining PICO and a plume model", the authors Pelle et al, introduce a method for parameterising basal melting which will very likely prove useful in ice sheet models. An existing basal melting parametisation (PICO; Reese et al) is coupled to an existing plume model (Lazeroms et al), to produce estimates of basal melting that better consider ocean dynamics.

This manuscript presents a nice evolution in technique that I expect will prove useful. As such, I believe it is suitable for a brief communication-type paper in The Cryosphere.

My main concerns with this paper are that it properly recognises the original cavity and plume models that it couples. While this recognition occurs throughout the paper, it would be better suited right at the start, such as in the abstract. Secondly, comparison is made to glaciological/satellite-inferred estimates, which is fine, but observations and regional models also exist and would serve just as well for comparison. Thirdly, and the biggest issue in my opinion is how the ice velocity in the calculation of the plume. I believe the physical basis for this choice requires more justification.

I have several minor specific issues which I believe should be addressed. If these are addressed, I would recommend this manuscript for publication.

For reference, Page 5, Line 16 to 18 is referred to as P5L16-18.

Kind regards,

David Gwyther

University of Tasmania

Australia

Specific issues:

~~~~~~~~~~~~~~~~~~

Proper recognition: This paper is presenting a coupling procedure between existing models. As such, and considering that the authors of the original models are not co-authors, I think effort should be made to properly recognise the original model authors. One way to do this would be to cite the original model studies in the abstract. I note that both models are properly cited throughout the paper, but doing this early and upfront will better recognise the original studies.

Comparison to R2013: For specific locations, comparison should be done to actual observations, not just glaciological/satellite-inferred estimates. You refer to them as "observations", but there are existing observations (e.g. APRES) which resolve basal
~~~~~~~~~~~~~~~~~~

melting better than a continent wide estimate/survey. Even though this is a brief communication, it will not take much space to compare to existing studies. For example,

Davis et al, 2018, "Variability in basal melting beneath Pine Island Ice Shelf on Weekly to Monthly Timescales"

Stanton et al., 2013 "Channelized ice melting in the ocean boundary layer beneath Pine Island Glacier, Antarctica"

Jenkins et al., 2010, "Observation and Parameterization of Ablation at the base of Ronne Ice Shelf, Antarctica"

Likewise, there are many regional modelling studies which possibly provide more consistent estimates for comparison with. For example, many studies exist for your target ice shelves, for Totten these are:

Khazendar et al., 2013, Observed thinning of Totten Glacier is linked to coastal polynya variability

Gwyther et al., 2014 Simulated melt rates for the Totten and Dalton ice shelves

Plumes coincide with ice velocities: Sometimes it's necessary that in order to overcome technical limitations, decisions must be made which may not be realistically accurate; this is a parametisation after all. However, there should be some realistic basis to coinciding plumes with high ice velocities. I think this should be explained more in depth. For example, it should be made clear whether the relatively good agreement with Rignot 2013 is because you expect actual plumes (and hence the high velocities and turbulence which drive melt rate) to be located in the region that your parameterisation predicts. That stability can not be achieved with plumes being initiated with high basal slope is disappointing; perhaps other initiation methods you tested could also be mentioned. Essentially what I'm saying is: are you getting the right answer for the wrong reasons, and in which case, would you expect these results to hold for future scenarios with evolving cavity geometry and a different ice velocity. This choice requires more justification and physical reasoning, including when this assumption could be expected to break down.

Technical corrections:

∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼∼

P1L8: capitalisation of Plume is not necessary.

P1L10: 'a wide variety' -> 'several'. You only showed us 3 regions.

P1L12: 'able to reproduce'. What you're referring to is 'able to match Rignot et al., 2013', which I think could be more accurately stated by saying 'able to reproduce inferred high melt rates beneath'...

P1L21: periphery OF the AIS

P1L21-P2L1: long sentence, consider splitting in half.

P7L2: "better agreement with observation than PICO, because PICOP..."

P7L19-23: Sentence is way too long; split in 2 or 3.

Figure 1: "on" in green box has mismatched font.

Table 1: in the External Quantity section, you have many "-" in the value column. I would change the column header to "Source", and then cite the study where you pull the quantities from e.g. Far-field ocean temperature, etc.

Table 1: missing '\epsilon' value?

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-216, 2018.