The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-277-RC1, 2020

The Cryosphere Discussions
Open Access
EGU

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

Anonymous Referee #1

Received and published: 3 January 2020

The authors describe the protocol that will be used to compute melt rates at the base of ice shelves in ice sheet models driven by output of global climate models in the framework of the Ice Sheet Model Intercomparison Project for CMIP6 (ISMIP6). The global climate models included in CMIP6 have generally a too coarse resolution and do not simulate explicitly the circulation and fluxes in the ice shelves cavities. It is thus important that all the groups participating in ISMIP6 use a similar protocol to derive the melt rates from those global model results so that the origin of the differences in their results can be more easily investigated.

The manuscript is very clear. It describes precisely and justifies well the choices performed in the approach. It also proposes several options to sample the uncertainties associated to the computation of the fluxes. This will be very helpful in the develop-

Printer-friendly version

Discussion paper

ment of the intercomparison project. Consequently, I just have minor suggestions for improvements.

I have first two small general points

1/ If I understand well, despite a relatively sophisticated approach to obtain the melt rates for present-day conditions, the warming signal simulated on the continental shelves by global climate models for future conditions is transferred without modifications into the cavities. The warming is also homogenous in the cavities, because of the extrapolation applied. If this is the case, maybe it is good to write it explicitly, for instance in the final section, to avoid misinterpretations.

2/ The computed fluxes can vary by one order of magnitude between the different parametrizations. This is a very large uncertainty and I guess this would have a major impact on ice sheet model results. I know this point is not the topic of this paper but more information on the uncertainties of those fluxes would be very helpful. Results of simulations with FESOM are suggested as a benchmark but I was wondering if other results could be included too to have a broader discussion of this important point.

Specific points

1. Page 4, lines 4-5. I do not understand what is meant by 'coupled ice sheet ocean models are not ready to be used with CMIP boundary conditions'. Is the problem that ice sheet models are not coupled to ocean models for the majority of ISMIP6 models or that those models cannot be used on the spatial-timescales of interest? I guess that, for coupled ice sheet ocean models, a protocol can also be defined to drive them by CMIP boundary conditions (but it is out of the scope of ISMIP6?) – see also page 4, line 20.

2. Page 8, line 17. The authors mention that the errors due to sampling, interpolation/extrapolation are likely much larger than those due to the temporal bias. However, large interannual variability and trends have been observed in several coastal regions

Printer-friendly version

Discussion paper

around Antarctica. That would thus be helpful to quantify the bias associated with the choice of the different periods, maybe using some of the data in the regions with the best coverage or using oceanic reanalyses (which have their own biases too).

3. Page 8, line 24. The dataset proposed is different from the latest release of the World Ocean Atlas that use similar observations as input. I understand the reasons for this choice but, as many scientists will likely use this version of the World Ocean Atlas, it would be needed to highlight the main differences, for instance by showing a few maps in the supplementary material.

4. Section 4.2. Is 'thermal forcing' defined ?

5. Page 13, line 18. The author mention that they take samples in the melt rate and the error in the thermal forcing, using normal distributions. I may miss something but I think they take samples in the distribution of melt rate and thermal forcing (not in the error of thermal forcing). Same for Figure 3.

6. Page 13, line 30. Gamma0 is estimated by sampling the 10 highest melt rates. Would using all the melt rates for the Pine Island ice shelf lead to values that are closer to the ones obtained for the MeanAnt method?

7. Page 15, line 9. If the temperature correction deltaT accounts for 'ocean property changes from the continental shelf to the ice shelf base' (page 12, line 12), I would assume that deltaT should be negative in most regions. Are the positive values obtained for the MeanAnt in many regions a sign that deltaT is rather compensating for a too weak exchange coefficient?

8. Page 18, line 13. Is the underestimation of the melting at surface in the PIGL method a consequence of using constant deltaT on the vertical while the correction may be smaller closer to the surface?

9. Figure 6. The last but one and last but two sentences of the caption are repetitions of the second line.

10. Page 22, line 9. ‘estimated’ instead of ‘reconstructed’?

11. Page 24, line 14. I would suggest ‘selected’ instead of ‘identified’ as the choice is mainly based on past results, not on new analyses performed in the manuscript.

12. Page 24. It is not clear from the discussion if the parametrization with a slope dependency is suggested or not as an option for ISMIP6.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-277, 2019.