The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-324-RC1, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

The Cryosphere
Discussions

Interactive comment on "ISMIP6 Antarctica: a multi-model ensemble of the Antarctic ice sheet evolution over the 21st century" *by* Helene Seroussi et al.

Anonymous Referee #1

Received and published: 6 March 2020

The manuscript describes the Ice-Sheet Models Inter-comparison Project for Antarctica. In addition to presenting results, the manuscript also documents various aspects of the project itself. Undoubtedly, it will be published, at some point. The current version, however, requires modifications, restructuring and potential additional analysis (I will come to this later).

The most general comment is that it is not entirely clear who is the intended audience for this manuscript. If it is aimed at wider, more general audience, it is full of jargon and unstated assumptions (e.g. that the ocean temperatures simulated by climate models can be used as a proxy for the sub-ice-shelf melting). If it is primarily aimed at ice-

sheet modellers, it is a little bit thin on results. It would be beneficial for the manuscript if the authors write it with a specific audience in mind. Regardless of that, the text has to be much more clear that the described results are results of simulations, and are not expected contributions of Antarctica to sea level. This seems like an obvious, and redundant comment, however, considering a high profile of this manuscript (a most like reference for the next IPCC report), its language and wording has to be precise. I would recommend to modify all statements similar to "The contribution of the Antarctic ice sheet…" (p.2 line 6), "East Antarctica mass change…" (p.2 line 9), etc. to "The *projected* contribution of the Antarctic ice sheet…", "*Simulated* East Antarctica mass change…" (I'm not going to mark all such phrases, but please correct them all).

Another general comment, which is easy to address, is that it'd be better to use CMs (climate models) instead of AOGCMs. "CM" in CMIP5 stands for "Climate Model", additionally later in the text (lines 85-95) "CM" and "ESM" in names of the models, which outputs were used, indicate the type (complexity) of the model - either a Climate Model or Earth System Model.

Overall, the text has too much jargon (e.g. SMB; it's not clear what "idealized surface mass balance" means). The titles of sections and subsections are too cryptic (e.g. "ctrl_proj", "NorESM1-M RCP 8.5 scenario"). They need to be informative enough to give a general idea of section or subsection content. The subsection "2.1.4 Ice shelf collapse" is misleading in both its title and justification of the experiment. Perhaps it should have quotation marks to indicate the name of an experiment. Lines (157-159), state that hydrofracturing is the main mechanism that leads to an ice-shelf collapse. Though collapse of the Larsen B ice shelf preceded by surface melting, and hydrofracturing was specifically proposed to explain its collapse, it is not the only ice shelf to collapse, and collapses of other ice shelves, for instance Willkins Ice Shelf, were most likely unrelated to hydrofracturing or surface melting (it happened during austral winter). Because hydrofracturing is essentially the only mechanism that can be parameterized in an ice-shelf model, it does not mean that it is the only possible mechanism to trigger an ice-shelf disintegration. This part of the text needs clarifications.

As mentioned above, the manuscript documents experimental protocols and describes projected Antarctic contributions to sea level. It is unclear whether there will be in-depth analyses of this MIP. Considering the diversity of participated models, it would be interesting to know whether some useful lessons (apart from projected sea-level magnitudes and their spreads) could be learned from this exercise. For instance, because the initial ice sheet geometry affects ice flow and and ice discharge through the grounding line at the later times, it would be interesting to know whether ice-sheet models that use long spin-up as initialization, simulate significantly different ice discharge compared to ice-sheet models used present-day configurations as initial conditions. The same question applies for parameterizations of basal sliding - do models that use inversions of the present-day observations produce different results compared to models that don't employ inverse methods? These are rather suggestions, and it up to the authors to decide what is the scope of the manuscript.

Similarly, the structure of the manuscript is the authors' decision. The current version has a fairly lengthy description of sub-ice-shelf melting simulation. It is not entirely clear why this process has such a prominence compared to other, no better constrained processes (e.g. basal sliding, calving, etc.). As a suggestion, the authors might consider moving details to an appendix, and the current appendix (C at least) to supplemental online materials. The manuscript will benefit from streamlining. Currently, section 2 combines together various unrelated aspects (i.e. kinds of forcing, experiments, etc.). Having a better structure will improve the manuscript readability.

Figures could be more illustrative. Overall, bar figures are difficult to read, simply displaying them in the models' alphabetical order is not informative. The authors might consider modifying the time axis in Fig. 1 (e.g. having uneven spacing prior to 1990s or so) to focus more on a period of time that have results from more models. Panels (b) and (c) in Fig. 3 seem to show the same field but in different units, the purpose of that isn't clear. Perhaps using log scale (for negative values it could a different colormap of

the absolute vales) in figs 6 and 14 might show better spatial variations on the grounded ice sheet, as the largest magnitudes are on ice shelves or in their immediate vicinity. It is unclear how to read fig. 15, its caption does not help with that.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-324, 2020.