Review of Nowicki et al., Ice Sheet Model Intercomparison Project (ISMIP6) contribution to CMIP6

The manuscript provides a valuable summary of the set of experiments—in coupled climate models both with and without ice-sheet components and in standalone ice-sheet models—and compelling motivation for why these experiments will be useful for exploring the role of Greenland and Antarctic Ice Sheets in the climate system, particularly as related to sea-level change.

General Comments

The manuscript is well written. Over all, I find the description of the experiments to be quite clear and well thought through. Clearly a commendable effort has gone into designing these experiments. The structure is clear with a few minor exceptions detailed below. The figures provide valuable visual cues to the structure of the experiments as well as the physical processes included in participating models. However, some of the figures are not yet publication quality and could use some additional attention (again, as detailed below).

As my area of expertise is more in ice sheet-ocean coupling and ocean modeling, rather than ice sheet modeling, my most detailed comments relate to ice-ocean interactions. I find that the discussion of potential methods for incorporating melt rates and/or temperature data from the ocean components of AOGCMs as well as the potential used of melt parameterizations needs some further elaboration, as elaborated in the specific comments.

Some of the discussion of how the time ranges of the "XXX-withism" and "ism-XXX-self" and "ism-XXX-std" differ from those of the standard CMIP6 runs they correspond to was not clear to me. I think this issue applies primarily to the historical runs? As I mention below, perhaps this could be clarified better both in the text and by putting the modified ISM ranges into Table 1, rather than having only the standard CMIP6 ranges.

For future Copernicus manuscripts, consider putting the tables and figures inline rather than at the end. This makes the paper much easier to review and is allowed by Copernicus as of January 2016.

My recommendation is that the manuscript be published with minor corrections.

Specific Comments

p. 3 l. 11: You may wish to define SRES and RCP the first time you refer to them, though these acronyms will be familiar to most readers.

p. 4 l. 26: Like Reviewer 1 (Christian Rodehacke), I felt that the XXX convention should be explicitly defined, even though it is likely obvious to the reader.

p. 5 l. 2: It might be worth mentioning here that you will be discussing the method used to assess and evaluate the AGCM results in Sec. 4 (e.g. "...is to assess and evaluate (using metrics discussed in Sec. 4) CMIP atmosphere..."). During my first reading of the manuscript, I missed that the details of the analysis would come later (though you state it on p. 3 l. 19) and I was expecting at least some sense of what fields, metrics, etc. would be used in this analysis.

p. 8 l. 14: “The Tier 2 experiments...” You haven’t yet introduced the tiers for the different experiments at this point in the text. If you can avoid referring to Tier 2 here by giving those experiments some other descriptor, that would save the awkwardness of needing to introduce the tiers here, rather than later where they seem to fit best.

p. 8 l. 30-32: It is not entirely clear what “it” refers to in this sentence, presumably “accurate treatment of ice-ocean interactions”? More importantly, it seems to me that there is little doubt that accurate treatment of ice-ocean interactions requires moving boundaries in the ocean model. Just as parameterizing, rather than explicitly simulation, the circulation in ice-shelf cavities and resulting melt rates leads to inaccuracies, there can be little doubt that ignoring changes in cavity geometry (or parameterizing changes in melt rates) as the ice sheet evolves will lead to inaccuracies. All that is to say that “may” should be replaced with something stronger like “will likely”.

p. 9 l. 21-23: I would suggest moving “based on an initial analysis of AOGCM simulation[s] of ice-sheet climate” to the beginning of the sentence for clarity. That way, it is hopefully clear that you are identifying the experiments based on the initial analysis, rather than that the ISMs are performing experiments based on the initial analysis. Also, maybe again here you could say that the criteria for determining which AOGCM results are “best” (i.e. chosen for the small subset of experiments) will be discussed in Sec. 4.

p. 10 l. 1-2: “...mismatch in spatial resolution over which SMB varies and that is used by AOGCMs”. This phrase is confusing to me. Perhaps “..mismatch between the spatial resolution of AOGCMs and the characteristic length scale of variations in SMB”?

p. 10. l. 8: The use of RCMs as intermediaries between AOGCMs and ice-sheet models also adds ambiguity about which biases are introduced by the AOGCMs and which by the RCMs, does it not?

Paragraph starting at p. 10 l. 26: Presumably, an effective melt parameterization would need to account for both the phenomena you outline in this paragraph (and probably more). It would need to make use of ocean temperature (and probably salinity) as a function of depth somewhere near the calving front each ice shelf and also the depth of the ice draft within the cavity. More sophistication would be nice (e.g. accounting for faster ocean flow with steeper ice-draft slope) but is still a topic of ongoing research.

p. 10 l. 27-28: I do not think that Rignot and Jacobs used surface temperature for their relationship, but rather ocean-bottom temperature close to the calving front. (However, they do not state their method of obtaining the temperature explicitly in their paper, at least as far as I could tell.) Also, this relation is only calibrated for melt rates at the GLs and likely is missing important nonlinearities (Holland et al. 2008).

p. 10 l. 29-30: “...that depends on the ocean temperate at the closest grid cell...” At what depth would the temperature be taken? Hopefully at or near the ocean bottom. Or better yet as a profile of depth.

p. 10 l. 30-31: “If none of the CMIP6 ocean models are suitable” Can you be more specific about how “suitable” is defined (or refer to Sec. 4 and make sure you define there how you determine whether ocean results are suitable)?

p. 10 l. 31-32: “prescribe a melt parameterization that depends simply on the ice shelf draft”. I (and other ocean modelers) feel that this is a poor choice (perhaps very much so) for a couple of reasons: 1)

The thermal forcing (or thermal driving – the difference between the freezing point and the "ambient" ocean temperature, however "ambient" is defined) plays at least as important a role as the depth of the ice draft, so that differences between "warm" and "cold" ice shelves cannot be ignored. 2) Such parameterizations have only been used in small regions, where their coefficients have been calibrated to local thermal conditions, not over the whole of Antarctica.

p. 11 l. 1: "oceanic anomalies (basal mass balance and basal temperatures)" I do not see how these can be generated, independent of ice basal topography (ice draft) if a parameterization is being used. Instead, perhaps coefficients in the parameterization as functions of time could be provided, from which melt rates could be computed given an ice draft.

Paragraph starting at p. 11 l. 20: I found this whole paragraph to be very confusing. Perhaps part of it is that initMIP has not yet been described. Maybe you could consider reordering the paper so initMIP has been described already at this point?

p. 11 l. 21-22: It is not at all clear to me what these two sentences refer to. Is the 1990s to 2014 forcing repeated or is just 2014 repeated? Or something else?

p. 11 l. 27: Please elaborate on the challenges of initializing ice sheet models to per-industrial conditions and how this presents challenges that do not allow for the typical historical run. This is likely not obvious to all of your readers.

p. 11 l 27-29: What does this mean? What period of time is covered? Please consider updating Table 1 so the range of times for the various ism simulations is given separately where they differ from the standard CMIP6 simulations. This would help to clarify the confusing differences in time ranges described in this paragraph compared with those of standard CMIP6.

p. 13 l. 3-5: How will the *abmb* anomaly field be constructed? How will it be made to conform to differences in grounding lines and calving fronts between different models? Similarly, how will the SMB anomaly be made to conform to differences in ice sheet extent between models in *asmb*?

p. 13 l. 19-20: Why would it be ideal for the ism experiments to follow the AOGCM experiments with a six-month lag? Do you perhaps mean "no more than a six-month lag"? I would think ideally the ISM experiments would follow the AOGCM ones without any lag at all, but a realistic (or perhaps somewhat optimistic) time table would be for a six-month lag.

p. 15 l. 29-30: "regional ocean models (e.g. Timmermann et al. 2012)" FESOM, the model that was the primary focus of this Timmermann paper, is actually a global model with high resolution focused in Antarctica. Perhaps "regionally focused ocean models" would be more correct?

p. 33 Table 1: Please consider putting the actual start and end year for each ISM simulation that used a range different from the default CMIP6 (as requested previously)?

p. 41 Figures A1: I feel this figure need some cleanup before they look professional enough for publication. The curves are not lined up very well (black is peaking out from under green). The blue arrows on the lighter blue surface and green base are not very visible. The giant gray error for freshwater flux should be given adequate room so it doesn't overlap the ice berg. Black lines should be anti-aliased and boundaries of the figure should not be jagged (slanted with respect to the figure caption).

p. 42 Figures A2: Blue text (both light and dark) is hard to read on blue background. The phrase “Liquid flux into the snowpack” should ideally either be entirely within or entirely outside the blue region.

Typographical Corrections

p. 5 l. 22: “amip” needs to be punctuated differently. Perhaps “The Atmospheric Model Intercomparison Project (*amip*; Gates et al. 1999) simulation allows...”

p. 6. l. 18: “four ISMs simulations” → “four ISM simulations”?

p. 9 l. 22: “AOGCM simulation” → “AOGCM simulations”

p. 9 l. 28: “...cannot be made however we list...” → “...cannot be made. However, we list...”

p 10 l. 5: RCMS → RCMs. Also, a verb is missing in “to SMB”, perhaps, “to simulate SMB”?

p. 10 l. 18: “the SMB lapse rate obtained” I would remove the word “obtained”. It is not needed.

p. 11 l. 8: “the dynamic response output” I would remove the word “output”.

p. 11 l. 11: “Ice Sheet-Ocean-Model” → “Ice Sheet-Ocean Model”

p. 11 l. 12: “Sect. 3.3).)” there is some extra punctuation here. I think just the one end parenthesis is needed. Also, should this be Sect. 3.3.2?

p. 11 l. 23-24: I would change “our” to “the” at the beginning of both these sentences.

p. 11 l. 32: “ice sheets evolution” → “ice sheet evolution”

p. 12 l. 1: “at least by 4 meters” → “by at least 4 meters”

p. 12 l. 2: “from the ism-lig-std” → “from ism-lig127k-std”

p. 12 l. 30: “Antarctic Ice Sheets” → “Antarctic Ice Sheet”

p. 15 l. 28: “As regional” → “Just as regional”

p. 19 l. 25-26: “not explicitly asked to minimize the data request” → “not specifically requested as an output variable in order to reduce the size of the data files” (or something similar – the original phrasing is not very clear)

p. 20 l. 26-39: Note that Asay-Davis et al. has been accepted in GMD so please don’t forget to update the reference when the time comes.

p. 31 l. 5 and 18: In 2 references, Vizcaíno is spelled without the accent mark while in three it is with the accent mark. This may be an issue with the respective journals but it looks strange when these articles are cited close together.

Reference

Holland, P. R., Jenkins, A., & Holland, D. M. (2008). The Response of Ice Shelf Basal Melting to Variations in Ocean Temperature. Journal of Climate, 21(11), 2558–2572. http://doi.org/10.1175/2007JCLI1909.1