Author’s response to comments from X. Asay-Davis (Referee 2)

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.

We thank the reviewer for the helpful comments. We have now revised our manuscript in light of these and other comments that we have received. A point-by-point reply is given below.

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).

Thank you for the detailed comments on the figure. They have been cleaned up accordingly.

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.

We have altered the manuscript to address your specific comments. However, we are currently not able to be more specific on the way in which the standard ocean forcing will be implemented, as the final choice will depend on the evaluation of CMIP6 ocean runs that have not yet been made by the climate centers. Our goal was therefore to provide a variety of options. This said, ISMIP6 is committed to obtain the best possible forcing for ice sheet models with the help of atmosphere and ocean experts, and in the coming two years, we are organizing a series of workshop to address this issue. The first of these workshops will be held in San Francisco in December 2016, just before AGU.

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.

We have altered the manuscript to clarify why it is challenging for standalone ice sheet models to have the same time range as the standard CMIP6 runs, and by including the modified ISM time range in the Table. Indeed the issue primarily applies to the historical run, which in standard CMIP6 runs start from the pre-industrial spin-up or control run. The reason is that pre-industrial spin-up is challenging for ice sheet models, so ice sheet models generally initialize at present day, or in the late 1990s.

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.
This is a great advice! We will do so in future Copernicus manuscripts.

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.
We have now defined SRES and RCP the first time that we refer to them.

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.
We used Reviewer 1's suggestion to add the sub-clause "where XXX stands for different forcing scenarios as described later".

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.

Following a comment from V. Eyring, we have now moved up the description of the methods used to assess and evaluate AGCM and AOGCM results (initially in section 4) to this section.

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.

We have changed the sentence "The Tier 2 experiments" to "Another set of experiments". In addition, following a comment from the CMIP panel, we have now indicated the Tier for each experiment in the experiment tables.

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".

We have replaced "It may" by "Accurate treatment of ice-ocean interactions 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.

We have modified the text as suggested.

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"?

We have modified the text as suggested.

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?

Indeed, the use of RCMs introduces additional ambiguity about biases, and is a motivation for avoiding the use of RCMs. We have added a sentence to state this.

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.

Indeed, we agree with the reviewer that an ideal effective melt parameterization would be more complex than the ones described in our manuscript. Given that it is still a topic of ongoing research, we are limited in our manuscript to propose solutions that have been used by the community. This said, ISMIP6 is engaging with the ice-ocean community (for example with the upcoming pre-AGU workshop) with the goal of identifying a better way to provide oceanic forcing.

The paragraph on the oceanic forcing has been expanded into four new paragraphs in light of the comments that you make below. The discussion now ends with "Ice-ocean interactions are an active area of research, and more complex parameterizations are being developed (e.g. Asay-Davis et al., 2016). ISMIP6 will organize workshops with the polar ocean community to investigate how to best derive oceanic forcing for ice sheet models, such that by the time the CMIP6 ocean models are evaluated, ISMIP6 may adopt a method that is distinct from those described above."

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).

Thank you for pointing out the mistake of the use "surface" for the ocean temperature and the deficiency of the use of this simple parameterization. Indeed, Rignot and Jacob (2002) sentence "Delta T is the difference between the nearest in situ ocean temperature measurement and the seawater freezing point (*43*) at a depth of 0.88 H_{p} [Table 1, (*21*)]", which suggests an ocean bottom temperature. We have removed the word "surface" and added a sentence about the problem of using this relationship.

The new text now reads:

One possibility is to calculate melt rate anomalies from changes in the nearest ocean temperature using an observationally derived relation of 10 m yr^{-1} $°C^{-1}$ (Rignot and Jacobs, 2002). However, this linear relation between ocean temperature and melt rates is calibrated for melt rates at the grounding line, and likely missing important non-linearities (Holland et al., 2008).

p. 10 l. 29-30: "...that depends on the ocean temperature 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.

The parameterizations of Martin et al. (2011), Pollard and DeConto (2012), and DeConto and Pollard (2016) are all fairly similar and based on Beckman and Goose (2003). The oceanic melt rates are linked to ocean temperature using a relationship that takes the form of:
Melt = constant (To – Tf) for Martin et al (2011)
Melt = constant (To – Tf) | To – Tf | for Pollard and DeConto (2012), and DeConto and Pollard (2016)
Where To is a specified ocean temperature and Tf the ocean freezing point temperature at the ice shelf base, and the constant being a combination of density of the ocean water, density of the ice, specific heat capacity of ocean mixed layer, latent heat capacity of ice, thermal exchange velocity…

The specified ocean temperature, To, is different for each studies:
For Martin et al. it is set to -1.7C, a value that correspond to the Ross Ice Shelf from the work of Beckmann and Goose (2003), which according to Beckmann and Goose (2003) correspond to the "average temperature between 200 and 600m depth" for the Ross Ice Shelf.

For Pollard and Deconto (2012): "the ocean temperature To is specified differently for various Antarctic sectors, based on observations but mainly aiming to produce realistic ice shelf extents and grounding line position"

For DeConto and Pollard (2016), To is initially introduced in the method section as "ocean temperature interpolated from the nearest point in an observational (or ocean model) gridded dataset" and later defined as "the 1 degree resolution World Ocean Atlas temperatures at 400m depth"

We have turned the sentence in question into a paragraph to be more specific about the different approaches, and the various definition of ocean temperature. We also agree with the reviewer that ideally the ocean temperature would be a function of depth, and the revised manuscript reflects this.

The paragraph now reads:
"An alternative approach is to parameterize melt rates as proportional to the difference between ocean temperature at the shelf break and the freezing temperature at the ice shelf base. Beckman and Goosse (2003) developed such a scheme for ocean models, and similar schemes have been applied in offline ice sheet model simulations with idealized ocean forcing (e.g. Martin et al., 2011; Pollard and DeConto, 2012; DeConto and Pollard, 2016). In those studies, the ocean temperature is set to the average temperature between 200 and 600 m depth (Martin et al., 2011), or the temperature at 400 m depth (DeConto and Pollard, 2016), or specified differently for specific Antarctic sectors (Pollard and DeConto, 2012). Depending on the evaluation of the CMIP6 models, ISMIP6 may adapt one of these choices, or could prescribe depth-varying profiles of ocean temperature (and possibly salinity). The dependence of melt rates on thermal driving ranges from linear (Martin et al., 2011) to quadratic (Pollard and DeConto, 2012; DeConto and Pollard, 2016). Since the freezing temperature at the ice

base decreases with depth, the melt rates in all schemes tend to be higher near grounding lines, as found from observations."

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)?

As mentioned in our earlier responses, the text that described the evaluation of CMIP6 models based on observations and regionally focused ocean models has now been moved ahead of this section. At a minimum, the CMIP6 ocean models will need to capture the broad scale polar ocean characteristics, and the ocean temperatures. However, at this time it is not possible to be more specific on a definition for "suitable", as the field is progressing rapidly, so new metrics may become available and the CMIP6 ocean models may have improved on the CMIP5 ocean models. We have replaced "suitable" by "can accurately capture the broad-scale polar ocean circulation or produce realistic near-shelf temperatures."

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.

We agree that this is not the ideal choice, and that the difference between cold and warm shelves is important to capture. We have altered the manuscript to stress that if we have to use this method, the parameterization will not be uniform over the whole Antarctic, but will vary from one basin to the next, taking into account warm and cold shelves.

The manuscript now reads:

"If none of the CMIP6 ocean models can accurately capture the broad-scale polar ocean circulation or produce realistic near-shelf temperatures, an alternative is to prescribe a melt rate that simply depends on the ice shelf draft (e.g. Joughin et al., 2010a; Favier et al., 2014). This approach is less satisfactory, however, as it ignores temporal changes in ocean conditions, and typically uses coefficients calibrated to local thermal conditions. If ISMIP6 uses this approach, the provided coefficients would not be uniform, but would take into account that ocean waters reaching ice shelf cavities or fronts differ regionally. In Antarctica, for example, the ice shelves of Pine Island Glacier and Thwaites Glaciers lie in "warm" water, while the Filchner-Ronne or Ross ice shelves reside in "cold" water. Ocean temperatures reflect the dominant water sources, with warm waters dominated by circumpolar deep waters (Jacobs et al., 2011), while cold waters typically correspond to high salinity shelf water (Nichols et al., 2001)."

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.

This is a very good point, and parameterization that varies in time seems like a good idea, except that it might be difficult and time consuming for some groups to implement. At the same time, if our goal is to have all the models apply a similar anomaly (which will never be exactly the same as ice shelf areas vary from one model to the next), we will have to ignore the shelf draft and base it on something like the average depth between all the models, or the observed values. As stated above, the final decision on the oceanic forcing will be made after evaluation of the CMIP6 models, and after community workshops. The goal of the sentence was simply to state that we will distribute the forcing (or how to compute the forcing) via the ISMIP6 website. This sentence is not crucial to the text, so has been removed.

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?
We have reordered the manuscript so that initMIP is now already described, and slightly changed the wordings of the paragraph in response to your comments below and additional comments that we have received.

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?
The forcing corresponds to the climate conditions at the end of the present-day initialization method. Present-day is defined differently for each model, and it also dependent on whether the initialization is an interglacial spin-up or whether it is mainly based on data assimilation (when data assimilation is used, present day also depend on what observations have been used). Therefore for one model, present day is year 1990, but for another model, present day will be year 2014. We have altered the manuscript to clarify these sentences.

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.
The quantity of accurate, high-resolution data available during the satellite era far exceeds that available for pre-industrial and historical periods. The majority of ice sheet models use these data in sophisticated initialization and assimilation procedures such that the present-day state of the ice sheets is simulated with a very high degree of fidelity. The lack of suitable data means that no such accuracy can be assumed for simulations of the historical periods. This becomes an issue because such inaccuracies are known to have a large effect on projections. For instance, discrepancies between projections can often be attributed to slight differences in the geometry of the ice-sheet margin assumed in a model (e.g., Shannon et al).

We have altered the manuscript to expand the discussion on the challenges of initializing ice sheet models to pre-industrial condition.

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.
Because it is not possible for ice sheet models to initialize at a time that correspond to pre-industrial conditions (defined as year 1850 in the CMIP6 climate models) using data assimilation methods, the historical run for ice sheet models cannot start at year 1850. The historical run for ice sheet models can therefore only start from the "present-day" year that the ice sheet model was initialized at, which as clarified in an earlier response could be 1990, but also later. We hope that the rewritten version of this manuscript makes this clearer and we have updated the tables to show the distinctions between the start time of the CMIP6 and ISM runs.

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?
Because of the difference in ice shelf extent between the different models, the abmb anomaly is prescribed to be constant for each basin. This scalar value is different for each basin, and derived from the mean value of the ice shelf melt observed by Rignot et al. (2014) and Depoorter et al. (2014). The abmb anomaly is applied in the models everywhere where the ice is floating, so that the ice shelf area is the only parameter that impacts the amount of basal melt anomaly applied. For the SMB forcing, the procedure is the same for both Greenland and Antarctica. The schematic SMB anomalies are defined everywhere on the model grid and are therefore applicable for models with varying ice sheet extent.

We have modified the text to include this information.

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.
Indeed, ideally there will be no lag at all. However, we acknowledge that the climate modeling centers will be busy running simulations for other MIPs of CMIP6, hence we expect a time delay. We have however used your suggestion and rephrased so that the sentence now reads "Ideally, the XXX-withism and ism-XXX-self experiments would follow the corresponding AOGCM experiments with no more than 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?
We have modified the sentence as suggested.

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)
The start and end year for each ISM simulations have now been included in the table (now Table 2).

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).
The figure has been cleaned up, and we are confident it is now ready for publication.

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.
The figure has been cleaned up, and we are confident it is now ready for publication.

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..."
We have done the suggested changes.

p. 6. l. 18: “four ISMs simulations” → “four ISM simulations”?
We have corrected the typographical mistake.

p. 9 l. 22: “AOGCM simulation” → “AOGCM simulations”
We have corrected the typographical mistake.

p. 9 l. 28: “...cannot be made however we list...” → “...cannot be made. However, we list...”
We have corrected the typographical mistake.

p 10 l. 5: RCMS → RCMs. Also, a verb is missing in “to SMB”, perhaps, “to simulate SMB”?
We have corrected the typographical mistake, and added the missing verb, so it now reads “to simulate SMB”.

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

p. 11 l. 8: “the dynamic response output” I would remove the word “output”.
We have removed the word “output” as suggested.

p. 11 l. 11: “Ice Sheet-Ocean-Model” → “Ice Sheet-Ocean Model”
We have corrected the typographical mistake.

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?
We have corrected the typographical mistake (cleaned up extra punctuation and changed the mistake in the Sect reference, which should indeed have been 3.3.2).

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

p. 11 l. 32: “ice sheets evolution” → “ice sheet evolution”
We have corrected the typographical mistake.

p. 12 l. 1: “at least by 4 meters” → “by at least 4 meters”
We have corrected the typographical mistake.

p. 12 l. 2: “from the ism-lig-std” → “from ism-lig127k-std”
We have corrected the typographical mistake.

p. 12 l. 30: “Antarctic Ice Sheets” → “Antarctic Ice Sheet”
We have corrected the typographical mistake.

p. 15 l. 28: “As regional” → “Just as regional”
We have done the suggested changes.

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)
We have done the suggested rephrasing.

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.
We have done the suggested update.

p. 31 l. 5 and 18: In 2 references, Vizca.no 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.
We have done the suggested changes, namely to spell "Vizcaíno" consistently in both the manuscript and references.

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