This paper provides a detailed report on a high-profile Earth systems model's (Community Earth Systems Model, CESM2) contribution to the Ice Sheet Model Intercomparison Project (ISMIP6) for the Coupled Model Intercomparison Project - Phase 6 (CMIP6). Being an ice-sheet model intercomparison, the contribution from CESM2 is provided by the Community Ice Sheet Model (CISM2), a single component of CESM2. Atmospheric and oceanic forcings for the ice-sheet model are uncoupled, and provided by output of six atmosphere ocean general circulation models (AOCMs), including CESM2 (when run, called CCSM4.0). The results of the Antarctic portion of ISMIP6 is to be reported in Seroussi et al. (in review), and include results from CISM2.

The novelty of this paper, over Seroussi et al. (in review) is that it:

1. Concentrates on and expands consideration of the sub-shelf melting parameterization, which is the aspect of modeling that Seroussi et al. (in review) report as being *"The largest sources of uncertainty come from the ocean-induced melt rates, the calibration of these melt rates based on oceanic conditions taken outside of ice shelf cavities and the ice sheet dynamic response to these oceanic changes."*
2. Extends simulation to 2500 by maintaining forcing after 2100 based on late 21st century forcing from high emissions scenarios. These longer runs are intended to
    a. Identify the regions of Antarctica most vulnerable to retreat
    b. Determine the long-term ocean-forced sea-level rise from Antarctica.
3. Unstated by the authors, but important to me is that the paper lays out a number of heuristics and other modeling approaches that were used to bring CISM2 to the level of being a credible participant in ISMIP6. Such information is valuable to other modeling groups. While some of this information does appear in Seroussi et al. (in review), it is in the form of a single, terse paragraph.

In terms of novelty 1, I agree that the ocean-induced melt rates are critical to our projections of Antarctica's response to climate change. However, I see that the entire scheme is based on an unpublished work, Jourdain et al. (in review). I recognize that publication of such a large collaborative project as ISMIP6 is extremely complicated and dependencies such as this do arise. On the other hand, I hope the authors can appreciate that my role as a reviewer is to verify the assumptions made are sound. I can not do this when they are based on unpublished techniques. I am distressed by line 18 of section 2.2, which states that the "*climatology was interpolated to fill gaps and **extrapolated** into ice-shelf cavities"* (my emphasis on extrapolated). Extrapolation is dangerous business and I'm not willing to accept that everything is OK with that until it's been through peer-review. My reservations are even greater when considering this paper, which frames much of its novelty around the topic of sub-ice shelf melting.

Continuing with my assessment of novelty 1, I also see that the authors have deviated from the prescribed ocean temperature anomalies per sector, $\delta T_{\mathrm{sector}}$ in order to assure the thickness in the region of the grounding line is similar to modern thicknesses. OK, but I'm not sure this is a tunable parameter. Jourdain apparently assured that the values of $\delta T_{\mathrm{sector}}$ agreed with the estimated mean melt in each sector (no reference provided). No such assurances arise in the author's method, making δT_{sector} an unphysical parameter that is accounting for other model shortcomings. Page 12-13 acknowledge what has been done, and suggest that the lack of buttressing may be responsible for the mismatch. I'd liked to have seen more here. If the paper is valuable because it is taking a harder look at ocean-induced melt rates, and their coupling to ice dynamics, then it should investigate why unphysical parameterizations are needed for spin-up. Lack of buttressing? Maybe. What about using spin up targets that are in steady state? Plainy the Pine Island/Thwaites region isn't in steady state, why initialize the model as if it is? I believe these δT_{sector} are the richest material coming from these simulations, but they aren't being investigated with enough depth. For example, consider figure 5. Why are many regions, for many simulations in a state that is so negative? The ice near the grounding line is too thin? Surely there is more at play. Also, why are some sectors reaching their constraints in terms of temperature forcing (-2.00). It's as if the model is trying to hold back a strong departure from the steady state. Why take such measures? It seems contrived.

Moving on to novelty 2, I just don't accept that these longer runs have value. The idea that ocean coupling, atmospheric coupling, and basal traction do not change on time scales of half a millennia is pure fantasy. Of course, it's also fantastical to imagine that one could do these couplings well, so the authors aren't to be faulted for not providing the couplings. Rather, I fault them for presenting these results at all. They simply are not credible.

Finally, novelty 3. There is considerable value in simply documenting the complex work that goes into modeling on this scale, at this level of complexity. The paper does that, and does so in a way that is well presented and understandable. I think that the paper is quite honest in that there were no major components of the exercise that were left out, obfuscated, or overstated. A sensible argument for publication is that the cost of publication is low, and the quality of search is high. While the paper is highly specialized, the authors are outstanding professionals in the field and this is a useful report on what they have been doing. I only see one problem with this point of view - there were 15 groups participating in Seroussi et al. (in review). Are all of them to have companion papers detailing the way they handled ISMIP6? That seems really inefficient. I wonder if there couldn't be a single companion paper?

In conclusion, I don't feel that the paper meets its own criteria for novelty because Jourdain is unpublished, because the need to alter the thermal forcing correction isn't explored in enough depth, and because the longer runs don't provide much value. The paper might provide novelty in that the details are useful, but that could be satisfied more efficiently with a companion paper that included more ISMIP6 participants.

Provided Jourdain is published, I could support publication of this manuscript with what I'd call 'minor' revisions, which I hope would include an expanded discussion of the thermal forcing correction, the need to push the model to steady state, and a consideration of thermal forcing that produces refreezing. All of these expanded discussions should come at the expense of the long term runs. That said, the option I favor most would be a companion paper that includes more ISMIP6 participants, and that lays out the details of modeling for the experiments in ISMIP6.

I have a number of comments on individual passages of the paper as well.

Page 1 line 7, ‘nudge’ seems chatty and imprecise. “Constrain to observed values”?

Page 1, line 13: say what the missing physics are.

Page 2, line 34: say what is meant by ‘committed’.

Page 3, line 1: Be more consistent with the purpose of the paper, the abstract makes quantitative claims (“10 cm to nearly 2 m”), I’m not sure this sentence really fits with the approach in much of the rest of the paper.

Page 2 Lines 5-9 it’s as if the authors agree the exercise is pointless, but are rationalizing the CPU allotments. Just drop it.

Page 4, line 1-16, this is really a long road to just stating that the power-law sliding law is used. The ‘nearly all’ statement in line 17 is frustrating too. Where is the relation Coulomb? What’s the point of having effective pressure in relations if one just assumes that it is ice overburden. This entire discussion could be collapsed to something much more concise.

Page 4, line 30 - I’m often confused by the (unpublished) ISMIP6 protocols. Here you say CISM runs are based on the protocols. Page 7, line 5 says your differ from protocols with regard to $\delta T_{\mathrm{sector}}$. That’s a fairly big deviation from protocols. So...be consistent in saying what you’ve done.

Page 5, line 11 - are these mean annual, monthly, daily atmospheric forcings?

Page 5, line 35 - This is odd - I guess you force the thickness near the grounding line to be close to observations by altering $\delta T_{\mathrm{sector}}$, but then protect against the large negative values found for many sectors by zeroing out the contribution to melt? So, you have refreezing (I guess) if it aids thickness on initialization, but don’t allow refreezing in forward runs? This really isn’t clear. Is equation 4 used for spinup, and the modified version used for forward modeling? Or, is the modified eq. 4 used for both? If so, then the negative values of $\delta T_{\mathrm{sector}}$ never have any impact, do they? Eqn 5 also eliminates refreezing.

Here is the big question: Aren’t you introducing a big transient when (if) you jump between the refreezing allowed in the initialization and the melt only situation in the forward runs? Is this transient physical (I don’t think it is)? Again, this is the novelty of the paper, but here is another case where it’s not being treated with enough consideration.

Page 8, line 22: OK - here we learn that this is not what was done as part of CISM2's contribution to Seroussi et al. (in review). This should have been scoped out in the introduction, as part of the novelty, with a clear statement of what the primary difference is.

Page 9, line 17: I'm not aware of an analytical solution to the temperature equation that can accomodate advection of heat. Please provide more detail.

Page 9, line 19, specify the extent of the ice that is considered for each type of 'nudge'.

Figure 5 - for context provide the $\delta T_{\mathrm{sector}}$ from Jourdain.

Page 12, line 6-7. This is important to me, but the treatment not satisfactory.

Page 17, line 5 not really based on the protocols, right? "Loosely based"?

Page 18, line 9-11 - this is frustratingly vague.