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The Cryosphere Discussions

Interactive comment on "Uncertainty in future solid ice discharge from Antarctica" *by* R. Winkelmann et al.

Anonymous Referee #2

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–General comments–

Winkelmann et al. present a study exploring uncertainty in solid ice discharge from the Antarctic Ice Sheet in coming centuries arising from ice sheet model parametric uncertainty, and uncertainty in future predictions of Antarctic climate change. This is an interesting manuscript that tries to deal honestly with the range of uncertainty associated with Antarctic sea level rise predictions, given present modeling technology – it is a nice accomplishment. My impression is that the results presented here could change markedly with different or improved models and/or model optimization techniques. That being said, this manuscript provides a good reference point for any such future work. I do have some concerns with some of the methodology and interpretation, that I would like to be addressed by the authors prior to publication. I have suggested some more simulations or re-analysis in a few cases. However, in lieu of a re-run of the whole experiment/analysis, in these cases I'd certainly like a much more detailed discussion of the implications of not including my suggested simulations/re-analysis.

Specific comments:

P674,L7: define ECP acronym above

-Introduction: paleo-observations (e.g. Naish et al., 10.1038/nature07867) should definitely be discussed here, as these provide some real-world evidence of AIS responses to warmer conditions. In particular, the apparent loss of some/all of the WAIS during Pliocene interglacials, in which temperatures apparently were only moderately warmer than today, should be discussed as an obvious 'real-world' analogy and a form of validation for the modelling presented here. This discussion will frame the validity of your results. It needs to be more explicit earlier in the manuscript that the surface temperature only affects the ice temperature and thus ice flow behaviour – one is not actually modelling any melting or SMB-temperature effects.

-It is noted that temperature won't have a role in the SMB in the future. However, the role of melting over the big AIS ice shelves in the future isn't mentioned – this may be a mechanism by which surface melting could dynamically affect future dynamic behaviour (eg Scambos et al, 10.1016/j.epsl.2008.12.027, Fyke et al, 10.1175/2009JCLI3122.1), that isn't modeled here.

-How long do you spin up the AIS models to equilibrium? State this explicitly.

-Since the internal temperature field has no memory of past glaciations, I presume it is on the warm side. This important caveat should be highlighted, if this is the case. It seems to me that this doesn't help the noted discrepancy that the model appears to exhibit a slow response to the transient climate forcing –all else being perfect, I would expect a warm ice sheet to respond faster. This slow response despite warm temperatures may be due to your parameter 'optimization' procedure, which automatically compensates by choosing ‘too-stiff’ parameters that artificially ‘prop up’ a too-warm ice sheet. This might be ‘fixing an error with an error’ and should be explored in model runs or discussed, in the manuscript or in a reply to my comments (if you think I'm off-base). It may help explain the delayed response.

-More generally, I think the validity of using a steady-state preindustrial AIS should be questioned. You do allude to this, but it bears explicit discussion, especially regarding how it could strongly (dominantly?) impact the transient future behavior you present.

P677,L14: for clarity, I suggest briefly reviewing the Le Brocq/Martin boundary conditions/methods you used to spin up the ice sheet – this is pretty important information that one should have easy access to here.

-Do you have a rigorous basis for stating: “Most important with regards to the transport across the grounding line . . . are . . . E_SIA, E_SSA, and F_p”? This statement could use some justification, aside from the fact that others have used these parameters.

-Just two diagnostics were apparently used – WAIS volume and shelf area. This seems an incomplete set of diagnostics to me; there are several other obvious choices that would likely quickly trim down your ‘good models’ and probably much more strongly constrain your parametric uncertainty. There may be a real danger that you have included parameter sets that do well for these two diagnostics, but grossly miss other basic diagnostics – this is not touched on at all. Without actually re-running your experiment with a more comprehensive set of diagnostics, more comment is definitely needed on why you limited yourself to these diagnostics, and how that might strongly (dominantly?) influence your results.

-P677,L18: “Aiming at maintaining a large range of representations of the ice dynamics, we exclude only those of all possible parameter combinations for which the. . .” -> “In order to identify a range of model parameter combinations which generate reasonable equilibrium Antarctic Ice Sheets, we exclude all parameter combinations for which the. . .”

-P677,L22: “The deviation of the . . .” -> “In addition, we exclude models for which the anomaly of the ice shelf area is greater than 10% of the observed area, for each major shelf (??), in order to constrain grounding line positions.”

-P677,L26: “The remaining parameter combinations are marked. . .” -> “The remaining parameter combinations that result in reasonable Antarctic Ice Sheets are marked. . .”

-I am quite concerned/convinced that structural (not parametric) uncertainty in the model could have a large effect on your results – you allude to this briefly when discussing the need for a better basal sliding model. For a study like this, one needs to probe in greater depth the possibility that the basic design of the model may be lacking something critical (i.e. high enough resolution for resolving grounding lines, sufficient skill at generating sub-shelf melt distributions, etc.), and interpret how this lack might skew the results. As it is, it is mostly implied that the model is perfect, but with uncertain parameters – this is definitely not the case.

-The method of turning the global mean temperature timeseries of Meinshausen into an evolving Antarctic surface temperature field should be better explained – it is quite confusing.

-I find the description of the method for getting ocean temperatures to force the Olbers-Hellmer model even more confusing. One thing I don’t understand – do the CMIP3 runs go to 2500? If not, how do you get ocean temperature forcing out that far? Do you get it from Meinshausen?

-Generally, why did you not use spatially-varying air/ocean temperature from simpler climate models (that can run well past 2500) directly?

-I like the inclusion of the ‘time-dependent’Olbers-Hellmer model that evolves with evolving geometry. However, especially given it’s simplicity compared to the real world, how confident are you that the Olbers-Hellmer model responds realistically to perturbations which are far from the present state for which it has been validated, especially as the ice shelf geometry changes dramatically? Also, are there parameters within the sub-shelf model that could be as important as parameters within the ice model itself? Greater exploration (ideally explicitly in the form of more simulations, or at least in the discussion) of the role of the Olbers-Hellmer model in influencing these results is important, because I would intuitively expect your results to be quite sensitive to parameter/structural changes to this nice, but highly parameterized, model. In general I think using global ocean temperatures (?) to derive specific patterns of subshelf melt is questionable give the complexity and locality of melting processes.

-P681,L7: "...we compute the likely and very likely ranges of ice loss, defined by the 33rd and 66th percentiles and the 5th and 95 percentiles, respectively" -> add "for each ECP scenario." How is the precipitation increase calculated?

-"exemplarily" – not sure what this word means!

-Anti-correlation between initial equilibrium ice sheet size (here represented by WAIS above MSL) and sea level response (pg 683) is a really nice insight – and a cautionary note for other modellers – different model parameters can result in 'reasonable' ice sheets (based on a few diagnostics) that respond very differently to perturbations. However, as noted above, I suspect use of additional diagnostics in your model 'throw-away' procedure besides WAIS size and shelf area would do a better job at weeding out unrealistic models, to arrive at a smaller range of 'realistic' models with a smaller range of future behavior.

-Please describe the parameterization of precipitation (Huybrechts/Wolde) in Section 3 – it appears to be introduced for the first time in the results section.

-Label axes on Figure 11.

-Fig: 17: is the large change in dynamic ice loss between this figure and Figure 12a due to addition of precipitation? Or in other words, is Figure 16a the result of Figure 17 minus Figure 15a? I believe this is the case... this could be more clarified in the text.

Interactive comment on The Cryosphere Discuss., 6, 673, 2012.