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Dear editor:

Here is my review of the manuscript entitled " **Projecting Antarctic ice discharge using response functions from SeaRISE ice-sheet models** "

General remarks:
the manuscript relies on the SeaRISE experiments to derive sea level rise projections, along with uncertainty estimates, for the next 100 years. As such, this study has strong implications in that it will enable better estimates of the contribution of ice sheets to the present sea level rise, which has so far been neglected in established projections such as the ones found in the IPCC AR4 report. I recommend publication of this manuscript, after some modifications to the manuscript.
I detail my remarks below, but the main comment I have about the manuscript is the fact that the critical point, Eq(1), is not explained well enough for a non-expert in SLR projections to understand. At p9: l13:20, the authors explain how they will use basal melt-rate as forcing and assess sea-level contribution from ice discharge. I believe this should be justified much more thoroughly. There is also ample room for confusion between basal melt, and basal sub-shelf melting. It would be nice to clearly define both terms, and find different ways of relating to them in the manuscript. In particular, I believe it necessary to explain the Bindschadler et al, 2012 sensitivity experiments a little bit more. It took me a while to understand that there was a distinction being made in the text between basal melt (which refers to the SeaRISe experiments), and basal sub-shelf melt rates, which are required to transmit climate forcing between global ocean temperature rise and increased ice discharge. Maybe the last paragraph of page 11 should not be there, as it tends to introduce the confusion between basal melt rate and basal ice shelf melting.
Another issue I have is the fact that increased basal melt rates are supposed to capture a signal in the atmospheric forcing (from increased lubrication, ala "Zwally" effect). There is no discussion relating this decrease in basal melt rate and the addition of another forcing through the basal ice shelf melting, which could be related. Increases in basal ice shelf melting will definitely impact ice flow dynamics, which in itself will impact basal sliding. Are there any feedbacks that would make using these two forcings redundant? or not self consistent?

In the same line of thinking, it is not clear to me why the manuscript focuses so much on basal ice shelf melting, when at the beginning everything hinges on the response functions from the SeaRISE experiments which are based on a lowering of the basal melt rate. Clearly stating why this is being done at the onset of the manuscript would go a long way in clarifying some of the confusion I had.

Abstract: abstract is clear, concise, and to the point. I would avoid referencing tables, but I'm not sure about the policy of TC on this.

Detailed remarks:

p1. l8: please cite Mitrovika et al, 2009.
p6. l9: use "the" finite difference method
p6. l13: Weertman"'s" sliding law
p6. l14: "the" ice sheet margin ...
p6. l14: rounding line $->$ grounding line
p6. l15: floating condition: may I suggest hydrostatic equilibrium?
p6. l15: lacks ice shelves
p6: overall, the AIF description reads more like a sequence of bullet points. Please put more links between the sentences.

Figures:
the figures are clear and self-explanatory.

Sincerely yours,

Dr. Eric Larour