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The Cryosphere Discussions

Interactive comment on "Remapping of Greenland ice sheet surface mass balance anomalies for large ensemble sea-level change projections" *by* Heiko Goelzer et al.

Anonymous Referee #2

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General comments

This paper describes a method for adjusting a surface mass balance (SMB) anomaly field that has been calculated with reference to a specific ice sheet topography, such that it may be applied to an ice sheet model with a different topography, minimising unphysical impacts in the target model that may arise purely from this difference in initial height. In other words, it aims to estimate, from a single base field, the SMB anomaly that would be physically consistent with any given surface, without explicitly recalculating the climate and SMB anomalies on that surface. Such a method is desirable in a multi-model ice sheet comparison project such as ISMIP6, where a single future sce-

nario forcing needs to be applied consistently across a spectrum of ice sheet models that have significantly different representations of the present day ice sheet state.

Evidence from preliminary work in ISMIP (initMIP) shows that such a method will make comparing the ISMIP6 experiments across the different participating models very much more robust, so this is in principle a worthwhile contribution to the field, and may well prove useful beyond the immediate scope of the ISMIP6 experiments themselves. The method described is sensible and the paper is written carefully, and addresses the main questions that arise regarding its application and the degree to which it may inherently distort the input fields.

This is a methods paper, describing a method that I believe has already been used by a number of groups conducting the ISMIP6 experiments, so there's no real scientific interpretation to quibble over and the main goal of the paper is to document what was done, say why certain decisions were made how they were and enable others to reproduce the method. The only real fault I find with the current state of this paper then is the derivation of the time and height dependent remapping procedure, section 4.2. There's clearly some subtlety in how to remap the real changes in SMB that come from a changing climate (simulated in the RCM) at the same time as estimating what would be expected to physically occur as the ice sheet height evolves (not simulated in the RCM) along with applying the numerical dSMB/dz remapping to account for the initial state mismatch, but I found 4.2 a very confusing way of trying to explain this. This is perhaps due to the notation used - for me the summary in words at the end was much clearer than the form of the equations used to derive it. This section, alone, doesn't do a great job of allowing others to understand and reproduce the method for themselves.

Further, given that the method may have use beyond the current ISMIP6 effort, it might be useful to future readers to highlight things that could have been done better in hindsight, or that could be applied if a reader's individual use case isn't subject to some of the (wide-ranging) restrictions implied by the ISMIP ensemble. So, whilst it is stated (pg5, line 13) that other choices of hc and R might be appropriate for a non-

MAR forcing product, it might guide future applications for the authors to note some more detail as to why they decided their choice was "sufficient". In this vein, whilst there is attention on the needs of different ice sheet models as targets for the method, perhaps the authors could speculate on issues that might arise from using a different source climate model - eg a GCM with a lower horizontal resolution than MAR.

Lastly, playing devil's advocate (and supporting writing for maximum clarity, and defensively) readers not familiar with ISM modelling might question whether what's being done here is a fudge of the "right" boundary condition purely for the sake of convenience for modellers who haven't initialised their ice correctly and don't want it forced into correctness by these "right" boundary conditions. I would thus recommend being careful in outlining the motivation and the scientific intent of the adjustment. As an example, pg 2, l28: "appropriate" carries an ambiguous meaning here. I think there are a couple of other places terms like this are used too. For me it would be better to be very explicit and stress that the method is intended to transform the climate forcing so that it has more physical consistency with the ice sheet state it will be used with. So, in section 2 "this effect we are trying to capture" could be made more explicit along the lines of: here is a physical relationship between ice geometry and boundary conditions we need to be able to honour in each model that uses our forcing set, because the two things are not independent and blindly applying the same set of absolute boundary values to every ISM would impose an artificial inconsistency.

Detail comments

page 4, line1: "fixed function of observed surface elevation" could add "sampled across the entire ice sheet"

pg4, l5: "apply the remapping" could add "separately"

pg4, l13: it's not obvious to me why the median is used, rather than any other average

pg4, l16: is there any possibility at this point to recalculate/merge the drainage basins

for ISMs that might have very different/coarse geometries? Who does each part here? Do ISM groups remap themselves based on the lookup table?

pg4, l33: What were the MAR boundary conditions - ERA?

pg5, fig 2: The choice of colours is a bit random, some are indistinguishable from each other - does this invite unnecessary use of printer ink!?

pg5, l12: "judged sufficient" is not very precise - if you're going to say other intervals might be appropriate for other products, could you give some kind of guidance as to why you felt this choice was appropriate for this product?

pg9: might be a good place to note the effect of the remapping on (integrated) SMB conservation? One would not expect it to conserve, of course - and probably you actively don't want it to, again within a framework of transforming the SMB forcing so it gains physical consistency with the state you're applying it to rather than preserving numerical neatness for its own sake.

pg13, l12: notes "where the relationship between surface elevation and aSMB breaks down". I think this could ideally be expanded and come much earlier in the paper, as a general caveat to the applicability of the whole approach. Could you add an estimation of where/how badly this affects things, or how far the target topography can be from the original before this sort of method is not worth applying?

pg 14: whilst I got the principle fine, I found the derivation of the form of the time- and height-dependent anomaly on page 14 (ultimately, eqs (10) or (12)) to be very confusing. Not sure how best to suggest clarifying, but some points to consider: - Line 10, I didn't find the omission of hˆbar from the R(...) operator and elsewhere helpful for clarity, I ended up writing it back in everywhere it was not explicit to remind myself that these terms originated at hˆbar rather than any other h. For consistency throughout the uses of h, would hˆbar be more clear as h_RCM, h_0 as h_ISM(t=0), and h(t) as h_ISM(t)? - Lines 13-17 seem to be there primarily to illustrate what *not* to do, as a

misleading false start. Is this part really needed at all? - Line 18, and eqs (9) and (10) are the fairly straightforward aim of it all - could all of lines 10-17 actually be left out, and the two terms on the RHS of (10) just be explained as representing the explicit climate change dependence and the height-dependence of the SMB respectively (if that's what they are)? - The alternative form in eqs (11) and (12) is not uninteresting, but since it's ultimately not used I'm afraid it contributed more to my initial sense of confusion than my education. Could make it a footnote? - Line 6: I additionally wasn't clear how the various d(SMB)/dz terms were in practice derived for the ISMIP forcing product - via a local spatial SMB gradient from MAR, from the basin-scale SMB vs height lookup tables described in section 2.1 or one of the other methods noted in the references on lines 7 and 8?

pg16, l5: wasn't clear to me how the physical and unphysical biases in the sea-level contributions were being discriminated between, unless it's simply that the remapping is a good thing, so the biases left after applying it must be physical, and the difference between that and what was there before are unphysical?

pg17, l31: Are the scripts to do the remapping also going to be made available (with long-term storage) somewhere - perhaps as part of the TC submission?

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