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Discussions

Interactive comment on "Analysis of the Surface Mass Balance for Deglacial Climate Simulations" *by* Marie-Luise Kapsch et al.

Anonymous Referee #1

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This paper presents a downscaling energy balance surface/snow model, intended to derive more detailed surface mass balance fields for glacial regions in climate models. The downscaled surface mass balance may be analysed in its own right (as done here), or used as a boundary condition for driving an ice sheet model. The downscaling model is described, and its performance evaluated with reference to a regional climate model simulation of Greenland's recent past. The final section describes their calculation of surface mass balance fields for the northern hemisphere hemisphere ice sheets, derived offline from a climate simulation of the last deglaciation conducted with the coarse resolution version of the MPI-ESM. Running ice sheet models more closely with climate simulations is a growing field, and this model represents a very useful contribution to area. The paper is generally carefully written and covers the all the

necessary topics, although there are quite a few specific places I think it could do with a little clarification, as detailed below. More generally I think sharpening up the framing and motivation - especially for the paleoclimate section - would be valuable.

General comments

I did enjoy reading this, and it feels like I've spent a lot of time writing it up - apologies for the lateness - I found lots of small things to ask about, see below.

I felt there was one wider, general issue that hopefully would be easy enough to address - as I got to the paleoclimate section I didn't think that a solid motivation had really been given for /why/ a simulation of deglacial SMB was being done, and thus I wasn't really sure what to be looking for in how it's been described and analysed. Even by the end this isn't clear, as the Conclusions don't conclude anything that sounds obviously new about the glacial climate system or the model itself. In a couple of places it's said that the SMB fields will be made available to the ice sheet community for their use, but the first part of section 4 says that the simulation isn't going be evaluated, so I don't really buy this as sufficient motivation for the whole exercise - are these modelling groups going to want to use a new SMB product from a climate simulation that hasn't been evaluated for how it compares to evidence of what actually happened?

There is some interesting decomposition of how accumulation and melt factors balance differently in different periods and how the (perhaps non-obvious) relationship between total SMB and ELA changes too, but only for GrIS - is it different in different regions? - and no attempt is made at general statements about the global implications or testable hypotheses for underlying principles. Personally, I wanted to know more about how some of the details of the snow physics were being forced by the climate through the deglaciation - albedo, refreezing etc, rather than just the resulting SMB, but that's just one idea.

I think the authors should either decide on a clear main goal that's stated at the front of the paper and runs all the way through, or be more clear about the separate aims of each part - either way that should make it easier to see what the paper as a whole is working towards, and clearer what should be in the final conclusions.

Detailed comments:

line 4: "deglacial climate" - quite specifically the /last/ deglaciation

line 5: "allows to resolve" isn't grammatically great - 'allows the resolution of' perhaps?

line 8: The flow of ideas implied by the: 1) <multiple sentences> 2) structure isn't very clear, I'm not sure it's needed

line 10 (and throughout): I found the dating notation used throughout off-putting, personally, but that might just be me. In my experience, the convention I've seen most is that kyr = "thousand years" and ka = "thousand years ago" with 1950 usually taken as the reference for 'ago'. "ka BP" thus feels to me like it's mixing conventions and providing both an 'ago' and a 'before'. I could live with the use of "ka BP" if defined here carefully (and used consistently throughout, but sometimes just "ka" seems to be used to indicate an event date, not a period of time - I'm looking at line 447 as I type this, and table 2 has "kas BP"), but I'm afraid the use of just "a" as the shorthand unit for years (and is that actually meant as 'years before 1950'?), spaced away from the number it's attached to so it can be mistaken for the indefinite article 'a', just grated horribly for me - eg line 455, line 359. It looks OK for eg the SMB numbers as (Gt aˆ-1) - to avoid confusion perhaps write the full word 'years' in cases like line 359 where "a" is currently used on its own? Related to the date convention, the only place I saw the 1950 reference date stated for the BP 'present' is in the caption to figure 3, and since this isn't a paleoclimate-specific journal I think it would be helpful for the readership to note this much earlier and more plainly.

line 11: having said that melt dominates the changes at 13ka, it would be helpful to say which component causes the increase in SMB at 9ka - is it an increase in melt or decrease in accumulation?

line 12: would be helpful to note the timescale of this AMOC/SMB variability here, it it related to eg Heinrich events or slower/different variations

line 13: not sure a statement on data availability needs to be part of the abstract does it, that intent is implicit in EGU journals now?

line 25: a lack of local horizontal resolution is far from the only reason ESMs struggle with ice sheet SMB - large-scale background climate and circulation biases play a big role, and increasing resolution (horizontal and vertical) certainly doesn't solve everything for either Greenland or Antarctic surface simulations in global models.

line 28: this paragraph is still supposed to be about ice sheets generally, but all these references are for Greenland SMB. You could throw in some Antarctic studies too to be a bit more general - eg Agosta et al., TC 2019

line 29: I spent a while wondering which ice sheets were going to be included in this study, and it's not clarified until much later in the paper. This would be a good opportunity to say this - 'extends the analysis of northern hemisphere SMB changes', perhaps? - and also a good place to note why it's of interest to have a detailed SMB product / analysis for this domain.

line 37: references for what different proxy studies suggest about AMOC variation during these periods would be useful

line 45: I think this statement is too strong - Bauer and Ganopolski show that using fixed parameter values throughout in their simple PDD model does a poor job, but inverting that to say that *only* EBM models can produce realistic ice volume changes seems too much. I'm a little wary as well of the way the term "EBM" might be taken by readers too, whilst we're on the subject - in my (climate modeller) experience it's most often used in the sense of a much-simplified model of radiation balance for an atmospheric column, but here (I think) its being used in a much more general way for any model that explicitly calculates the components of the energy budget and how they affect temperature and the phase of water at the surface, however complex or simplified. This potential confusion could be eliminated by a disambiguation sentence?

line 53: typos: "a EBM" and "Two kind of simulations"

line 62: this is OK for present-day Greenland (although the accumulation isn't a simple function of height), but should this relationship be taken to be equally strong for all ice sheets, all the time? For example, variation in present-day Antarctica SMB components is more significantly controlled by dynamic atmospheric conditions, rather than a local scaling with topographic height I'd say. Might be worth a caveat here, and a sentence in the discussion about the limitations of this sort of downscaling

line 78: can the intervals/level heights be detailed in a table, maybe supplementary material?

line 84: it would be helpful to note in this list that the phase of precipitation may be changed too

line 105: does the rain that potentially refreezes have a temperature, - perhaps that of the surface - changing the amount of energy required to refreeze it?

line 141: I found it confusing to say "the aging process starts", when so far the only aging process described is for fresh snow. 'Another aging process starts', perhaps? Your aging of the surface albedos is purely a function of time, when quite often the rate of increase of snow-grain size and thus lowering albedo is dependent on temperature and density. Do you know how sensitive your results are to the timescales you've chosen, and whether you would expect them or your results to be different for LGM temperature and accumulation rates?

line 155: Going on with the thread of the previous comment, any table of tuned values almost begs the question of how sensitive your model is, and conclusions are, to the choices you have made, especially when they have been tuned to match one type of climate (the present GrIS) and the model is then applied to very different ones. Do

you have any insight from your undocumented(?) tuning process that you could use to comment on this sensitivity? More work, but maybe possible if you have already done (or could easily do?) the simulations, would be to not only compare your results with MAR SMB for the present, but also for what other models find for GrIS SMB at 2100, or under some other idealised climate forcing. Freely downloadable results do exist for MAR run to 2100 with the climates from various CMIP6 models, from the ISMIP6 preparations. That wouldn't be an evaluation in the same way, but would give your readers some useful reference points for how sensitive your EBM tuning is to the climate it's in.

line 161: I had trouble following this section, but I think I more or less got there in the end. Might be worth thinking about rephrasing. The two cases handled are labelled 1) and 2), but that numbering isn't used again, and it seems like the first case described ("The inflow of snow and ice from above") is actually case 2). How does the temperature profile shift - in proportion to the amount of mass (solid and liquid?) that has been moved between layers? In the case of surface melting (line 165), the mass has not actually left the snowpack until the percolation/refreezing procedure has been completed, so is the change in density profile delayed until that has been done? Although, surface melting lifts the profile /up/, which sounds like melting at the surface makes the top layer /more/ dense than the reference. How does the surface layer ever reduce again to the reference profile, since more snow falling should further increase the layer density (line 163) or be directed to other layers underneath? Is it said anywhere what the top/bottom densities in this profile, or the density of ice in the virtual under-layer, are?

line 190: is even the deep ocean in a true steady state for your LGM initial condition, or are there drifts there to worry about? If it's all a steady state, why are first 5 kyr considered a discardable spin up?

line 195: Freshwater input forced AMOC variability comes up as a feature later - it would be good to mention specifically how glacial runoff/outburst water is treated in their protocol

line 200: From figure 4 etc, it seems like the ELAs talked about here are not a simple contour on the ice surface demarking where ablation and accumulation balance, but a /potential/ ELA calculatable for the climate in each land surface gridbox regardless of what the actual altitude (or SMB) of the ice sheet was. I'm still not sure what I think that means - precipitation, for instance, is at least partly orographically controlled and either falls in one place or another so it does matter where the surface really is, so not every potential ELA calculated can simultaneously be true...maybe?...but anyway, it would be worth another sentence to explain that the ELA that will be talked about from here on is not a simple contour line on the ice.

line 202: since the ELA is defined as the height where SMB=0 I don't really see what is meant by saying it's less sensitive than SMB

line 232, 242: structurally, do these short paragraphs need their own section headings? Thinking about it, would it make more sense to put the paleo experiment design section later, next to the paleo results, rather than right up front before all the recent history stuff is described and analysed?

line 238: not just cloud, all precipitation in ERA - and thus your EBM-ERAI GrIS accumulation - is also purely a product of their model physics run at global resolution, so I wouldn't expect that to make for a great comparison with the local MAR precipitation, regardless of how well EBM-ERAI can do the surface energy balance.

line 252: from here on, neither refreeze nor runoff is mentioned, only "melt". Since we're often talking about total SMB, I'm not sure whether they really mean surface melt (that might refreeze), or the actual liquid mass leaving the snowpack (runoff). Or are they talking about runoff-precipitation - so the proportion of the original mass that has now been lost? I'm interested to see what their refreezing does anyway, and it's a shame that that's not mentioned, but if they really are talking about just surface melt from here on then I really think the refreezing needs to be shown too.

line 269 (and I elsewhere, like line 316): there's a tendency here to simply blame the biases on in the MPI-ESM runs and the EBM SMB derived from them on inadequate spatial resolution and the local topography. That will undoubtedly be part of it, especially at ice sheet margins, but higher resolution doesn't fix everything in complex climate models, in fact it can make it worse (see Kampenhout et al, TC, 2019 for a very geographically relevant case-study). Biases in the large-scale climate, inappropriate initialisation, internal variability, missing physics - the list is of course endless, so it's irksomely simplistic to keep saying that the model resolution is too low and no more. It's not like the EBM_MPI-ESM results would = EBM_ERAI if you just ran MPI-ESM at ERAI resolution.

line 275: RACMO hasn't been introduced yet in any way, as a model or an acronym

line 276: You have some tunable control over the rain/snow partitioning in your EBM, so could this MPI-ESM bias be fixed for your purposes?

line 281: I would have thought that larger-scale moisture transports over the ice sheet, which are also quite resolution dependent, would be likely to play a role here too. Low resolution atmospheres often just drizzle too far north and over too-wide areas because they're overly diffusive everywhere, it's not just a local thing you would fix with a higher ice sheet. Again, maybe this is an area where things in the climate models other than simply local grid resolution/topography play a significant role

line 295, 300: I didn't follow the logical thread through here. First MPI-ESM-CR has a higher topography than MAR and the EBM produces too much melt, then later the area in the south is higher than the ISMIP topography and there is less melt. Less melt in which model? Is there a consistent physical link being drawn between these two cases?

line 299: your model has a pretty direct control on how much melting you get when you adjust for a new elevation through the lapse rates - does this point to the lapse rate changes being too high?

line 323: I think this statement on the scope of the paper should be in the Introduction, not all the way back here

line 329: The Antarctic ice sheet has yet to collapse, thankfully

line 332: It would be good to say why these particular time-slices are chosen for analysis.

line 344, 396: the impact of the changes in circulation and the different realisations of AMOC history on the ice sheet SMB sound interesting, it would have been good to hear more about them and how that might have influenced the ice sheet deglaciation. There is some decomposition of whether accumulation or melt is dominating the SMB/ELA trend for the main Glac1D run at different periods - does all this still hold for the ICE6G run? If so, that's an interesting conclusion perhaps that you can say is independent of the ice sheet reconstruction used.

line 394: the final figure of 550 Gt/a doesn't match the values in your table /very/ well. Does the 550 at the end of the transient correspond to the year 1950, rather than $\sim$2000? Your recent history run does go through 1950 doesn't it, even if only 1980-2010 were analysed - does this 550 match what the historical run did at that point?

line 403-405: references needed for the decline history of these glacial ice sheets

line 450: this information about the meltwater forcing should be in the experiment description

figure 1: is this surface melt, or runoff: the caption says ACC = SMB - MELT? It also says that MAR does not provide accumulation, which I don't understand - MAR gives you snowfall and rainfall? The ELA is just the contour of where the SMB on the surface is 0, isn't it? Is it said in the text why CR has so much more melt in far north

fig 2: (top, middle) to denote the second row and (bottom, middle) to denote the third row is not very clear. Ditto (center, left) and (center, right). With this many panels may just label them a)-p)?

fig 3: Paleoclimate figures often stack different proxies on the same horizontal time axis but offsetting the vertical axes, rather than overlaying everything completely like this - that might work well here too.

fig 4: the 2D ELA surface idea might need explaining in the caption here too. Is the EBM run everywhere? Is there +SMB in the MPI-ESM climates for other places, eg a Siberian ice cap that's just not shown here because masking teh results onto the Glac1D glacier mask?

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