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The Cryosphere
Discussions

Interactive comment on "Surface mass balance downscaling through elevation classes in an Earth System Model: analysis, evaluation and impacts on the simulated climate" *by* Raymond Sellevold et al.

Anonymous Referee #1

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General comments

This paper describes an analysis of the online climate downscaling scheme used over the Greenland ice sheet in an older version of CESM, alongside a limited study of how sensitive it is to the temperature lapse rate specified, one of the scheme's key parameters. The topic is timely, although I don't think it's totally clear that this belongs in The Cryosphere rather than Geosci. Model Dev., seeing as it doesn't purport to research anything about the real world, rather evaluate the emergent behaviour of a model parameterisation. That's not meant to imply that I don't think it's an important

subject, and it is valuable to highlight these results to a wider audience than those who work on the development of climate models, as it directly bears on the interpretation of model results that are used widely in the cryospheric community. In general I liked it, and as someone working in this area, I found it practically useful.The paper is well structured and clearly written - my main recommendation for an improvement would simply be to show more figures.

One might suggest a number of improvements to the downscaling scheme itself, of course, but those would be outside the scope of the work actually presented here.

Detail

title: the editor's initial comments have already touched on the title, but I'm not convinced the current title is as clear as it could be. The quantities being actively downscaled are "climate" variables, not the SMB itself.

The authors' replies to this comment from the editor also say they'd prefer to leave "Greenland" out of the title, as the scheme is general. Personally I'd put it in. The other obvious ice sheet application for this is for Antarctica, but circumstances there are rather different. Sub-gridscale variation in SMB components there is more dominated by dynamic weather considerations rather than pure elevation, and temperatures are such that the lower, melt/bare ice albedoes - the only means by which sub-grid variation in shortwave radiation can really enter in this scheme - should play a much smaller role. That being the case, the analysis and component gradients here probably *are* only really applicable to Greenland. Additionally, no comment is made of how the scheme might perform for other ice sheets - perhaps if the authors wanted to leave the title as it is they could include some discussion about how the scheme might be expected to perform on Antarctica, or if applied to paleo ice sheets in other regions?

If they wish to keep the scope to just modern-day Greenland, how about "Downscaling climate through elevation classes for Greenland ice sheet surface mass balance in an ESM: analysis [etc...]"?

page 1, line 5: it would be clearer if you note that RACMO is an RCM

p1,l20: "leading" would be better as "which would lead"

p1,l21: "is losing" would be better as "has lost" if you start the sentence with "Since"

p2,l6: I'd say "ESM" deserved a wider definition than 'a climate model with a carbon cycle'. There are many possible physical components in an ESM, and for certain applications I don't think you would necessarily have to have the carbon cycle part active to still call the model an ESM

p2,l8: does the "SMB" contraction need defining in the Introduction proper rather than just in the abstract, which can sometimes stand alone from the main paper?

p2,l10-25: I didn't think the distinction between methods 2. and 3. was terribly clear, or that the "hybrid" variant used by CESM doesn't really sit within method 2. or 3. The section also suggests it's going to list "state of the art downscaling techniques" in general, but this is a wide field and this list seems far from comprehensive - pattern scaling, EOF methods etc

p3,l1: since CESM1 was superseded by version 2 more than a year ago, I think that somewhere in the introduction it would help if you explicitly noted that you're not using the current release version of CESM, and said why. Perhaps in the Discussion you could also note what, if anything, readers might expect to be different in CESM2, based on what you've learnt and what has changed in the model in the meantime

p4,l24: Why did you use a "minimal", 1K/km lapse rate as a control rather than 0K/km which would effectively deactivate the scheme properly and revert to the type of behaviour seen in most ESMs?

p5,l4: I think it's noted later in the analysis, but you're effectively comparing two (likely completely different) realisations of climate variability during a specific historical period by using an ERA-forced RCM vs the GCM. I think it's worth flagging this up, and anticipating the possible impacts here already.

p5,l9 The framework used from here on does rely rather on fitting simple linear relationships to scatter plots of "<variable> vs elevation" from all of Greenland. The apparently wide scatter in the figures often make it look like such a simple relationship really isn't a good way to approach the RACMO data being compared with, although the r values given look higher than the scatter shown in the plot might suggest, so perhaps this is more a presentational issue? Since a universal linear gradient is the paradigm being used in CESM - and the CESM fits do often *look* much more linear - it's not an unjustified way to proceed, but some kind of cautionary note should be put in here that this is a potentially over-simplistic way of looking at regionally heterogeneous data from a much higher resolution study, and that for some variables the scatter makes the fits and gradients reported perhaps more qualitative than quantitative.

p5,l17: I think some 2D plots so that readers can see the regional differences between the RACMO2.3 reference fields you use in this study and your CESM1 SMB would be very useful here. The choice of which figures to put in the main paper in which should be supplementary material will need to be thought about, but in general I think this is the first of a couple of areas in this paper where it would just be useful to be able to see more information than is currently there.

p7,l29: it wasn't immediately obvious to me why the subset of fluxes shown in figure 3 were the "most relevant"

p7,l33: the CESM albedo does not look very sensitive to the lapse rate - probably worth noting that even at the maximum lapse rate you don't even get to half of the RACMO value.

p8,l9: why not actually do the SMB scatter plots and show the gradients? Surely they're important enough to show explicitly?

p8,l31: I really didn't understand the description of the prognostic temperature, or how it was calculated p9,l9: I still don't understand why you used a 1K/km experiment as the control, rather than 0K/km?

p9,l14: it's not clear in which topography the "mean elevation is lower"

p9,l29: the Supplementary info figure is labelled A1 here, but as S1 in one of the links I was given to download

p9,l30: the large discrepancies in the comparison with the reanalysis may be a place where the fact that the reanalysis and the GCM will have different realisations of internal climate variability really plays a role

p10,l29: as previously noted, I think it's worth flagging up differences between your CESM1 and the new CESM2, which is the version new users will likely pick up. Can you say which, if any, of the recommendations you make have been implemented in the current CESM?

p11,l20: "certain lapse rates score better for some metrics than others" is a little disingenuous, really. You've done a great job of showing that that the components being directly downscaled via the lapse rates generally cannot be made to match the physical elevation gradients for any value of the lapse rate, and that the final SMB you get only scores well because of fortunate cancellation of these significant errors. At this point, the "lapse rate" you specify almost loses a physical meaning - it's no longer a parameter you might desire to constrain directly through observations to match reality, rather a model control you can tune directly to get the final (SMB) result you want without worrying about the fidelity of the underlying components that go into that result. Something along these lines should be noted in this paragraph, I think

p11,l24: implies that it's hard to distinguish between the EC6K and EC9.8K SMB gradients, yet two sentences before states that EC6 has a better SMB gradient but EC9 has the best melt. I'm confused as to whether you can really make a robust distinction between the SMB gradients in the two cases - especially since the SMB vs height

scatter plots are not shown for cases other than EC6. Does the r value on the SMB gradient actually justify distinguishing between the two cases? If, in fact, you're only basing that recommendation on the top line of total GrIS SMB in Table 2, given the size of the standard deviation on the RACMO numbers it would seem difficult to justify saying one is better than the other.

Figures ——-

On the whole I feel that the paper could be usefully improved by tweaking the presentation of the figures. Above I've noted that it would be good if 2d plots of the EC6k vs the RACMO2 reference data could be shown, and the actual SMB scatter plots and fits for EC1, EC4.5, EC6 and EC9.8 rather than only summarising this data in a table. It may be that the authors or editor take a view on which figures belong in the main body of the paper and which in Supplementary information, but I do think it would be uesful to show them.

Of all the panels of scatter plots, only Figure 3 includes the useful gradient and r values on the scatter plots themselves - it would be useful if Figures 1 and 2 could show this information as well. In Figure 4, why is the absolute value of SMB shown for the EC1K experiment rather than the more useful difference from EC6K, which is how the information for the EC4K and EC9.8K experiments is shown in panels c) and d)?

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