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The Cryosphere
Discussions

Interactive comment on "Assessment of the Greenland ice sheet – atmosphere feedbacks for the next century with a regional atmospheric model fully coupled to an ice sheet model" *by* Sebastien Le clec'h et al.

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Review of Leclerc et al 2017:

Le clec'h et al present a study that assesses the strength of Greenland ice-sheet-atmosphere feedbacks over the 21st century using a regional model that is coupled to an ice sheet model. I think this is a novel experiment and valuable study and has the potential to be cited extensively as ice sheets are increasingly incorporated into various climate model architectures. My suggestions for improvement, listed in 'order of appearance', are below. My primary general concerns, which I hope the authors can address adequately, involve some apparent inconsistencies in the coupling/spin-up (e.g. use of topography anomalies, and uncertainty on how land surface types change in response to ice retreat, and what happens if the ice sheet wants to expand beyond present-day margins). Finally, please feel free to counter my suggestions if you think I'm in error.

###Comments###

P1L1: "the projected Greenland sea level rise contribution is mainly controlled by the interactions between the Greenland ice sheet (GrIS) and the atmosphere": while I tend to agree, relevant models can't yet fully assess the ocean contribution, so I think this statement is overconfident. Please moderate.

P1L2: "in particular through the temperature and surface mass balance – elevation feedback": no, the atmospherically-driven GrIS SLR contribution is controlled by radiative excess/warming. Feedbacks reinforce this effect but is do not control it.

P1L2: "fine scale processes"->"fine scale dynamical processes" ?

P1L15: "Furthermore, in 2150, using a fix ice sheet mask, as in the no coupling method, overestimates by 24 % the SLR contribution from SMB compared to the use of the ice sheet mask as simulated in the two-way method" this seems counter to the previous statement that SLR from two-way coupling is 9.3% larger than the uncoupled case. Is the difference due to dynamic discharge term?

P2L 4: "The atmospheric conditions control the variability" -> "Atmospheric conditions control variability and change"

P2L7: "SMB directly affect the GrIS total ice mass by impacting its characteristics such as thickness, ice volume and ice extent" - this can occur both directly and via impacts on ice dynamics. Explicitly state the latter (dynamics) for clarity.

P2L9: there are more foundational references regarding the dynamical GrIS impact on atmospheric flow. Suggest to use these in addition/instead. As just one arbitrary example: http://onlinelibrary.wiley.com/doi/10.1034/j.1600-0870.1996.00014.x/abstract

P2L11: “different processes and feedbacks”->”different processes and feedbacks that regulate transient ice sheet change”

P2L16: “The climate models usually represent” -> “For example, CMIP5 climate models unanimously represented”

P2L24: Suggest citing recent Lofverstrom et al. discussion study on resolution dependence of ice sheet conditions in GCMs: https://www.the-cryosphere-discuss.net/tc-2017-235/

P2L35: “the authors only consider a strict linear relationship between topography and SMB changes” - please note more clearly either here or in next paragraph why this is a handicap to these methods, leading to why your approach is better

P2L9: “The second fundamental requirement is to represent the ice sheet topography changes in the atmospheric model by using an ISM instead of the fixed geometry usually used” This sentence is tautological since by definition a fixed geometry will not capture topography changes. Reword sentence.

Throughout text: “developped” -> “developed”

P4L7: 16 km high, from surface? Sea level?

P4L12: “hydrological cycle” -> “atmospheric hydrological cycle” ?

How does Crocus differ/integrate with SISVAT? Please clarify.

In the case where the ice sheet expands or contracts, how is under-snow (or snow free) ice sheet surface exchanged for bare land surface (or vice versa)?

P4L20: “The topography of the GrIS as well as the surface types (ocean, tundra and permanent ice) are provided by Bamber et al. (2013)” -> clarify this is for the NC experiment (presumably)

P5L10: “we have repeated the MIROC5 year 2095 (representative of the years 2090s) for 50 additional years” - this repetition is certainly not representative of this time period due to lack of continued change, and also lack of internal variability. While I don’t think this is a fatal flaw of the study, the authors should clearly note this caveat here and later during discussion of results, so readers clearly realize the effects of this artificial ‘extension’ (probably, fairly strongly reduced overall change, making the results presented here conservative).

P6L11: Why is the annual mean bottom snowpack temperature not used as the boundary condition for the ISM instead?

P6L19: also just due to the long timescale of ice sheet responses?

P7L1: what is meant by ‘vertical fields’? Please clarify.

Spin-up procedure: How does this procedure deal with ice growth outside the observed ice sheet extent? Figure 2 suggests this ice is simply removed? If so, how does this effective strong artificial sink of ice impact all subsequent sensitivity experiments? Please explain the impacts of this clearly in the text, if this is the case.

P8L21: why not simply start the coupling at 2005 (i.e. the end point of the 1976-2005 initialization/spin-up period)?

P9L13: The use of topography anomalies is concerning since it implies the SMB/ST field received by GRISLI is inconsistent with GRISLI’s height (for example, the ELA on the GRISLI grid would exist at a different elevation than if the GRISLI elevation was directly used). Can the authors comment on why this approach does not introduce problems with their experimental design? As it stands, this is not justified adequately. An alternate approach that would have avoided this problem would have been to use the spun-up GRISLI topography as the ‘fixed’ topography instead of the Bamber topography.

Figure 2 and other figures: 5 years is likely not long enough to generate robust climatologies. Suggest using at least 10 years instead.

P11L10: the finding of very strong marginal cooling due to increased katabatics is very interesting and pertinent, and deserves a further explaining. It would be very useful if the authors plotted overlaid near-surface wind anomaly vectors plus ST changes in a 'zoomed-in' plot of a good illustrative portion of the margin.

Similar to above point: it would be excellent to see a quiver plot of wind anomalies over the entire ice sheet, given their importance. Also would it be possible to visualize the increased mixing in the boundary layer, leading to warming in the 2-W coupled case?

P11L23: do authors mean "Following the increase of the ST"?

P11L25: ", there is a decrease of 112 Gt yr−1 25 of ice " -> "112 Gt/yr extra ice ablates"

P11L30: "14 % larger in 2-W" - can an estimate be made of the uncertainty in this value (and others) due to interannual variability? Put another way, can the authors confirm that the changes they see are significant in the face of background noise in ablation area (for example)?

P12L5: "lower surface temperature over these regions" - suggest reinforcing to readers once more here that this is *relative* to the NC experiment.

P12L8/9: what does the +/- indicate here?

P12L13: "become ice or snow-free or snow free, exhibiting bare ice " this is confusing. What happens if the entire GRISLI ice column disappears? Does tundra emerge?

P12L25: Previous studies have highlighted a strong decrease in ice discharge across outlet glacier grounding lines as a consequence of increased surface melting. E.g. Gillet-Chaulet 2012, Goelzer 2013 and others. Is this same effect seen here?

P12L25: Is it completely correct to say the entire SLR contribution is caused by the 'melting contribution'?

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P12L25: Can the authors quantify the reduction in marine margin extent in 2-W?

SP13L1: "This higher integrated SMB, obtained when using no updated ice sheet mask" - do the authors mean "lower"..? This sentence seems to directly contradict the previous sentence. If I'm mistaken here, a clearer description of the processes here is needed.

General: The authors should consider quantifying actual feedback factors associated with the inclusion of elevation feedbacks (see Roe 2009, Reviews of Geophysics). This would be a good benchmark number to produce, for other works to compare to.

P14L11: "As for the ISM, increasing the grid resolution of MAR" - do you mean "as for the regional climate model"..?

P14L35: ". . .underestimated by simulating." Unclear.

P15L1: "surface albedo and strength of katabatic winds." -> "surface albedo and strength of katabatic winds, with a demonstrably strong return influence on SMB"

P15L27: "optimal resolution of the ice sheet and the atmospheric model, for ISM-RCM coupling." While an interesting-sounding statement, I find it also a bit vague: by optimal, do the authors mean something like "of high enough respective resolutions to resolve both important atmospheric and important ice sheet dynamical processes"?

P15L30: "The next step of this study. . ." as described, this is extremely ambitious, with many challenges that outstrip the effort to implement atmospheric coupling. If it is truly a planned next step; great! But if not, I'd suggest not claiming to plan to do this.

General: while the writing is 100% understandable and clear, a final proof-read by a native English speaker would be useful as a final stage, if possible, to clear up remaining small grammar issues.

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