Review of “Effect of elevation feedbacks and climate mitigation on future Greenland ice sheet melt” by Feenstra et al.

General impression

This manuscript builds upon previous work on bidirectional coupling between a dynamic, interactive ice sheet model of Greenland and the relevant components of an Earth system model; in this case, the integration of the Community Ice Sheet Model into the Community Earth System Model, respectively. The technical details of the coupling itself and a range of analyses of the performance of the coupled model for the Greenland domain have been previously presented in a set of papers published mainly between 2020 and 2021. The present manuscript seems to be an extension of that set, filling a gap left by and acknowledged in those studies regarding the actual quantification of the differences –for this particular coupled model– between the bidirectional coupling, a.k.a “2-way”, and a simpler unidirectional one, a.k.a. “1-way”, more representative of the trends in previous research at the time. In the latter, the dynamic ice sheet is still forced with an evolving climate from the other Earth system model components, but (in contrast to the 2-way coupling) these components are not informed about the resulting ice sheet changes. Thus, 2-way simulations can explicitly represent feedbacks between ice sheet and climate. Long story short, the authors wonder about the causes and significance of the potential differences between 2- and 1-way simulations.

Although 1-way simulations can be performed “offline” by first running the Earth system model with a fixed ice sheet (i.e. no dynamic ice sheet model) and then using its time-dependent climate output to force stand-alone ice sheet simulations, the authors take advantage of the rather flexible coupled setup that seems to allow for specific interactions to be switched off. In this case, it is the response of the climate to changes in ice-sheet surface elevation, while other interactions (e.g. albedo changes) are still accounted for. Thus, two quasi-equivalent simulations can be directly compared, where the effects of climate-elevation feedbacks can be, in principle, precisely isolated. This itself is already a very interesting option which –assuming other interactions mentioned in the text (e.g. albedo-melt, discharge-melt) can also be toggled-- presents an opportunity to assess the contribution, significance, and dominance of individual processes. So far, I think that such an analysis would be a great contribution to the community and enough material for a publication.

Taking advantage of the climatic context of the experiments analysed in this manuscript, comprising a rapidly warming world where pre-industrial atmospheric carbon dioxide levels are smoothly increased four-fold over the first 140 simulation years, the authors add a second part to this study. In what feels like a left turn from the assessment of the differences between 1- and 2-way simulations, this second part looks at the ice sheet and climate response to a relatively sudden reduction of the increased atmospheric carbon dioxide back to pre-industrial levels over 27 simulation years. This new simulation branches out from the year 350 of the 2-way simulation, keeping the fully coupled setup active throughout. The 1-way setup is not mentioned again during this analysis. I have mixed feelings regarding the contribution of this second part to the clarity, conciseness, and completeness of the manuscript. My concerns are elaborated on in the comments below.

Main points

1. On the scope of the manuscript

As mentioned above, I am not convinced that the addition of the idealised mitigation analysis (in its current form) adds a positive net value to the manuscript. After reading the first 15 pages of the preprint I, as a reader, felt already well immersed on the quantification of the differences between the performances of the uni- and bi-directional coupling approaches under this idealised warming scenario. I was surprised when I found no mention of a 1-way simulation in the climate mitigation sections, mainly because I was curious about the magnitude difference relative to the 2-way simulation under such a setup. Something in the line of, e.g., a branch-out from the 1-way simulation at year 350 that follows the same mitigation protocol, a version of the 2-way branch-out presented where the elevation feedback is turned off, or both. Maybe, I thought, the need for a 2-way coupling –and the added complexity it brings-- is not that clear in a mitigation context and, depending perhaps on the chosen timing and rate of the forcing reversal, the simpler 1-way coupling setup might do a good enough job. It is possible that an expert might rule this as unlikely based on similar studies or even their expert judgment, but I expected to see numbers; after all, quantification is a core theme in this study. In the introduction, the authors themselves seem to describe this as the original point of the experiment: "to assess the impact of ice sheet-climate interactions […] to CO_2 reduction", which happens to be the same wording used in that same paragraph regarding the first half of the study. A bit earlier in the introduction (lines 60-61), the authors mention, without directly providing references, that it is important to account for feedbacks during mitigation studies; well, this paper could be that reference! As it stands now, this manuscript gives the impression of two related but not well connected topics: 1) a quantification of the impact of the elevation feedback, and 2) an analysis of a simulation that mimics 1 potential warming mitigation scenario. Together, the inclusion of these 2 topics affects the conciseness of the paper and consumes resources that could be used instead to fill some of the gaps in either of them (see other comments below for examples of gaps in the former). All in all, I think that the mitigation analysis can be safely removed to make the paper shorter and consistent. I would be happy to see a different manuscript in which the impact of the bidirectional coupling and the hypothesis that "[…] the timing and the rate of the CO_2 ramp-up and ramp-down likely have a large influence […]" (line 429) can be properly evaluated and quantified. Then, the title of the present preprint can be easily updated to something more precise, e.g., "A quantification of elevation feedback effects on Greenland melt under an overshoot warming scenario as simulated by the coupled CESM2-CISM2". In this way, the authors can focus on a single topic that is already interesting enough and comes with nice new findings and confirmations regarding precipitation, air circulation, and cloud feedbacks. Just to be clear, I personally would not support the publication of this manuscript without either an inclusion of 1-way simulations in the mitigation analysis (not preferred), or leaving that second part out (preferred).

2. On the significance of the quantification

The authors find that the "1-way configuration overestimates these [topography-related] feedbacks" (lines 468-469), partially point out to the utilisation of a constant temperature lapse correction when downscaling the climate forcing onto the ice sheet model grid, and then hypothesise that "taking this seasonal dependency [of lapse rates] into account […] could help resolve part of the discrepancy between the 1- and 2-way coupled simulations." (lines 473-475). Later, the authors stress the importance of using bidirectional coupling for projections of future sea level rise (lines 480-481). Now, don't get me wrong; I agree with the core message there. However, I can't help noticing a couple of gaps and leaps in that chain of statements. In this study, the 1-way coupling (i.e. the communication between the climate components and the ice sheet model) is made possible by the temperature lapse rate correction, which seems to do a good job within the context of the elevation classes, but clearly struggles when forced to also cover for the significant departures in ice sheet topography as seen by different model components. In my opinion, presenting a quantification of elevation feedbacks using *only* an homogeneous lapse rate for the latter correction is insufficient to support a call to invest in fully bidirectional coupling setups right away. The authors are aware of work such as Crow et al., (2024), and discuss the possibility that "offline corrections in ice sheet models can be improved by accounting for the temporal and spatial variability of the lapse rate" (lines 424-425). Suppose an ice sheet modeller would like to improve their 1-way projections of sea level rise, but they are not sure about investing in a bidirectional coupling setup. In this case, knowing that a spatially variable lapse rate correction at the core of their 1-way simulations could compensate for a good amount of the differences relative to the not-there-yet 2-way setup --at a comparatively small implementation price-- would surely sound like an excellent option. This study could be that quantitative confirmation if it included that analysis. This way, the quantification presented on this work would get some new context regarding their dependence on an arguably oversimplified approach to very large elevation corrections, shedding some light on how much (or how less significant) the impact is when using a "better flavour" of 1-way. Such addition to this paper would be, in my view, *much* more useful and in-tone with the initial topic of the paper than the mitigation experiments. I believe that the gain in completeness would more than compensate for any apparent loss in novelty that the removal of the mitigation analysis might generate.

3. On manuscript presentation

Although I personally find the quality of the fundamentals (i.e. language and figures) on the high side, I must mention the overuse of acronyms. This seems to be standard practice nowadays, sadly, and at points it makes reading (and reviewing!) a rather painful experience. I imagine it being a bit frustrating for a good-willed general reader that might want to know more about model-based topics in the larger climate change field. I had to go back and forth many times to try and find the meaning of some of them in the text, and the fact that there isn't a summary of acronyms in the shape of a list or table doesn't help. In fact, I think that if such a summary is ever needed, you prolly using too many. Anyway, my point is that a good chunk of the acronyms is likely unnecessary, and to kind of prove it I have so far avoided them altogether in this review. For example, as long as it is clear that your study concerns the Greenland ice sheet, you can safely refer to it as "the ice sheet" or "Greenland" along with "the Greenland ice sheet", which as a bonus add some variation to otherwise tiring/repetitive language choices. If not, a submission to acronyms even ends up creating mildly weird constructions such as "in future sea level rise to the GrIS and the Antarctic ice sheet", which could have used a much more pleasing "the Greenland and Antarctic ice sheets". Similar story with the model components within CESM: other land, atmosphere, ocean, ..., models are seldom mentioned (if ever). Again, you can safely say "our land model" instead of a later and sudden "CLM5" that I then have to search back for. And to be honest, two thirds of typical model names rarely give you any additional information on the nature of the model anyway (particularly true for the Community Models, if I may). Additionally, acronyms can create unnecessary confusion, such in the use of NAMOC instead of AMOC. There I am no expert for sure, but since you still end up using the latter near the end of the paper I stopped to wonder the need for that N, and whether it was worth it to risk a casual misreading for the Northwest Atlantic Mid-Ocean Channel. I have several other examples, but I think and hope I've made my point. Besides acronyms, I think the paper would greatly benefit from a better and smoother connection between the sentences and some work on word choices. For example, at the conclusions I got heavily distracted with the repetitive use of the word "result" in its many uses and conjugations. Another example is the text in lines 341-344, which repeats the phrase "at the end of the simulation" at least 3 times, even twice within the same sentence. I just want to stress that presentation matters, and that this is a low-hanging fruit that can easily elevate the quality of the manuscript: less acronyms (nothing against them per se), more fluidity in the sentences, use of synonyms, a couple of passes with a native speaker (or a certain tech breakthrough for inspiration), ...; all at a negligible cost compared to (re-)running Earth system model simulations.

Minor points

Abstract: connected to my main point above regarding acronyms. Some (e.g. CESM-CISM) are not even used within abstract again (and reintroduced later in the text, so no difference). On that note, if the title is changed to include the acronym of the model, then there's no loss of info at all. Same with using a construct twice within a same sentence (e.g. "losing mass […] lose mass"). To exemplify my point, here's a humble suggestion for the first half of the abstract:

"The Greenland ice sheet stores freshwater equivalent to more than seven meters of potential sea level rise and strongly interacts with the regional and global climate. Over the last decades, the ice sheet has been losing mass at a rate that is projected to increase in the [some future timescale]. Interactions between the ice sheet and the climate have the potential to amplify or reduce its mass balance response to ongoing and projected warming. Here, we investigate and quantify the impact of these interactions using the Community Ice Sheet Model version 2 coupled to the Community Earth System Model version 2. We compare two idealized simulations --containing either a non-evolving or evolving ice sheet topography-- in which we apply an annual 1% increase in CO_2 concentrations until stabilization at four times pre-industrial levels. By comparing the 1- and 2-way coupled simulations, we find that applying a uniform temperature lapse rate to account for elevation differences among model components in our 1-way experiment misrepresents temperature changes in the ablation area, leading to an overestimation of the positive melt-elevation feedback and the resulting mass loss. Furthermore, our analysis reveals significant changes in atmospheric blocking, precipitation and cloud formation over Greenland as the ice sheet topography evolves, which act as important negative feedbacks on mass loss. [...]"

I'll spare the authors of more comments about acronyms and words, unless necessary, but my view stands for the whole manuscript.

Line 50: "overshoot" is introduced in quotes, but a definition of overshoot scenarios is not given. Consider adding it for the general reader, e.g. "[…] investigation of 'overshoot' scenarios, where this temperature threshold is surpassed […]".

Line 50: " Applying a temperature overshoot to the GrIS" is not precise language, consider something like "A climate where global mean temperatures have increased beyond the 1.5°C goal might have large implications for […]". Or even better, and after defining 'overshoot', simply stating "Such an overshot could have important implications […]".

Line 52: which period?

Line 54: feasibility for what? For a recovery under a ramp-down?

Lines 56-57: Is that sentence (which in essence repeats the info from the previous one) also a finding of the studies cited? The next sentence implies that it is a conclusion drawn from modelling studies. If yes, consider rephrasing it to reflect that, e.g. “Model-based results from these studies seem to suggest that if such thresholds are not crossed, ice sheet retreat can be halted or might even be reversible.”, which by the way highlights my observation about repeated info and provides a bridge to the next sentence.

Lines 57-59: Sounds weird. Consider something like “However, a thorough model-based assessment of the role played by ice sheet-climate feedbacks in the reversibility of enhanced deglaciation rates is currently lacking.”

Lines 62-67: This paragraph sounds very model-specific for an introduction section, and ignores existing research with other models (e.g. Madsen et al. 2022). It can be easily rephrased to coupled setups in a general sense and acknowledge other groups worldwide working with bidirectional coupling, plus some context. As a bonus, this would solve the issue of introducing the models (and acronyms) twice in the manuscript. CESM-CISM-specific sentences can be moved to the model description below. The remaining 2 mentions of CESM-CISM can be replaced by “a coupled ice sheet and Earth system model” and removed, respectively.

Line 140: The first half of the simulation design section is just a description of the 1w coupling. I think this could go into the coupling section, which can be divided into 2-w and 1-w for clarity. If the design of 2-w is taken 1:1 from previous studies, I’d like to see it made clear in the text; since at the moment it is somewhat implicit but still unclear. In other words, if any design or simulations are taken directly from previous work, I’d appreciate if there is a clear separation from what is brand new in this study.

Line 143: Does this mean that the freshwater fluxes are fixed and the discharge-melt feedback also removed from the 1-w? If yes, why so? So far the text has given the impression that only the elevation-melt feedback is targeted for analysis. Then why not turning off the albedo-melt feedback as well? I would like the authors to elaborate on the reasoning (or practicalities) behind these choices.

Line 156: kept constant for how long? Would be nice to have the total length of the simulation here as well.

Lines 230-231: Wouldn’t it make more sense to compute then the percentage of the continent/island/whatever that experiences ablation?

Line 467: 66% more melt than what? 1-way? PI melt? Please specify. Same with other percentages elsewhere.

Line 471: missing “/km” in the units of the rate?

Line 472: what do you mean here? You mean the “real” rate?

References

Crow, B. R., Tarasov, L., Schulz, M., and Prange, M.: Uncertainties originating from GCM downscaling and bias correction with application to the MIS-11c Greenland Ice Sheet, Clim. Past, 20, 281–296, https://doi.org/10.5194/cp-20-281-2024, 2024.

Madsen, M.S., Yang, S., Aðalgeirsdóttir, G. et al. The role of an interactive Greenland ice sheet in the coupled climate-ice sheet model EC-Earth-PISM. Clim Dyn 59, 1189–1211 (2022). https://doi.org/10.1007/s00382-022-06184-6

Jorge Bernales, June 2024