A Review of: "*Effect of elevation feedbacks and climate mitigation on future Greenland ice sheet melt*" Feenstra *et al.* 2024, The Cryosphere Discussion.

Feenstra et al. propose a study about the different feedbacks related to the interaction of the near-surface atmosphere of the Greenland ice sheet with its evolving topography following CO_2 concentration varying in the atmosphere. To represent these interactions, they use the model CESM2 coupled with the ice sheet model CISM2. The study is divided into two main parts depending on the analyzed experiments.

The first part concerns a comparison between 1-way and 2-way coupling method, and more specifically an evaluation of the non-consideration of feedbacks related to the evolving topography of the ice sheet. Components of the mass balance and the surface mass balance are compared, as well as the influence on the GBI and cloud formation. They highlight different negative feedbacks linked to the evolving surface topography that mitigate the mass loss in a climate with 4x the CO_2 concentrations of the pre-industrial period.

Next to that, the extent of the 2-way experiment is realized by rapidly decreasing the CO_2 concentration until reaching the PI concentration. Before the decrease of CO_2, the ice sheet had experienced 350 years of a climate with 4x CO_2 concentrations, resulting in a retreated ice sheet and a largely reduced ice discharge. Combined with a decreased melt at the surface due to a colder climate, the ice sheet presents a limited ice loss, and even a slightly positive SMB when the climate reaches a global warming of 2 K. The oceanic and snowpack conditions limit the possibility for the ice sheet to regrowth under the PI-reconstructed climate.

General comments

This manuscript is well-written and of generally good quality. Scientific approaches are pertinent and consistent. The authors propose a study of a wide range of the topography influences around the Greenland ice sheet, which constitutes both its strength and its weakness. I will have four major comments.

First, on one hand, the study is well complete and treats a lot of possible influences resulting from the interactions between the atmosphere and evolving surface height of Greenland (comparison 1- and 2-way method of coupling, GBI, NAMOC, cloud formation, energy balance and idealized PI CO_2 concentrations restoring and its influences). But on the other hand, all the topics and various processes studied are not deeply analyzed. Moreover, the part of the study concerning the decrease in CO_2 concentrations experiment answers a different question than the part concerning the comparison of 1-way and 2-way coupling. You present in the paper various messages, which could be, to my point of view, better highlighted if they are split. I suggest then dividing this manuscript into two distinct papers: a) 1- and 2-way experiments study, and b) the study of the idealized PI CO_2 concentrations restoring experiment. This way, you could dig a bit deeper into each topic, without presenting a too long and dense manuscript with several messages.

Concerning one of the topics that you should revise, I would like to highlight in a general comment that it seems you miss one thing concerning the evolution of the GBI with the 2-way coupling. As the surface height is lowering with time, the 500 hPa geopotential height is also lowering independently of any atmospheric circulation modification. Even if the index removes the 500 hPa GH of a larger area than the Greenland GBI area to the 500 hPa GH of Greenland, I think the simple lowering surface height have a non-neglectable influence that you should consider in your analysis. I comment on this point in the specific comments.

Finally, I have two comments concerning the methodology.

It seems that the description of the coupling and some details about the models are missing to well understand how the experiments are set up independently of the paper describing the coupling. I wrote specific comments in the next part of the review that you should address to improve this part of your study.

I'm a bit confused about the motivation of the 1- and 2-way simulation comparison, as well as the use of a fixed temperature lapse rate. I'm a bit annoyed by the fact that you explain right from the introduction that using a fixed lapse rate is not at all the best way to represent melt-elevation feedback since this lapse rate varies spatially and in time, and is influenced by these different feedbacks linked to topography. Despite this context, similar and more detailed arguments are repeated in the discussion to suggest that it would be more appropriate to use a non-fixed lapse rate. If I understand well, it seems that you had to use this fixed lapse rate to respect the methodology already used by the coupling and two compare things that are comparable. The fixed lapse rate also helps to highlight processes that are not considered when compared with 2-way coupling. However, none of these justifications appear clearly in the paper. In the introduction, you only mention the need to quantify the incorporation of such a coupling into CESM2 (P3 L67-68). But in the conclusion, you mention the improvement of the 1-way VS 2-way comparison (L 474: "[...] could help to resolve part of the discrepancy between the 1- and 2-way coupled simulations"), which may lead to confusion regarding the motivation of your comparison and the use of such a 1-way method. Especially as various other more efficient methods for representing melt-elevation feedback through offline downscaling of the temperature (Hanna et al., 2005; Gardner et al., 2009; Crow et al., 2024 that you mentioned), or directly from the SMB (for instance: Noël et al., 2016; Goelzer et al., 2020 for ISMIP6; Delhasse et al., 2024) have already proved their effectiveness. Therefore, it would be a good idea to adapt the message to clearly highlight the reasons for such 1-way and 2-way comparisons and the use of this fixed lapse rate.

Specific comments

Abstract

- P1. L6-8: Please add a time reference for your simulations and specify the level of CO_2 at which you start.
- P1, L10-12: "We also find that a uniform temperature lapse rate misrepresents temperature changes in the ablation area, leading to an overestimation of the positive melt-elevation feedback in the 1-way coupled simulation, resulting in an overestimation of mass loss." I think it's inappropriate to consider that the overestimation of the melt-elevation feedback by the 1-way simulation is an original and new result of your study as you already mention in your introduction (P2 L41-43) and in your discussion that other different studies with similar one-way experiments have already the same conclusion. You should rephrase and nuance this sentence.
- P1 L12-13: precise that you extend your 2-way coupled simulation instead of a new experiment.

Method

- P4. L94-99 "The model has a fixed number of vertical layers for the soil, whereas there is a variable number of layers for snow and firn, with a maximum snow depth of 10 m water equivalent (w.e.). The model allows for compaction of snow into firn. Accumulation of snow over 10 m w.e. in a grid cell is transferred as positive SMB to CISM2. If snow and firn are melted away, further melt is transferred as negative SMB (ice ablation) to CISM2." Does it mean that having less than 10 m w.e. of accumulation is not considered in the SMB ? Same for ablation?
- P4, L119: Please add equations for SMB and refreeze capacity to illustrate and summarize how SMB and refreeze are considered.
- P4: In general, has your model and specifically the representation of the SMB, the key feature of your coupling experiment already been evaluated against observation? How robust are your results? What are the range of bias for the different processes you are representing?
- P5 L126 & L.145: "The resulting climate and SMB are downscaled using elevation classes (Sellevold et al., 2019), using a temperature lapse rate of -6 K/km and are interpolated onto the CISM grid". The way you explain how the temperature lapse rate is used to interpolated the SMB is not clear enough for me. Are the SEB and SMB also downscaled with a lapse rate? Or are they calculated on the CISM grid after the temperature has been interpolated using the constant lapse rate? This part of your method should clarify as this is the only way you consider the melt-elevation feedback in your 1-way coupling experiment. Furthermore, could you briefly explain the elevation class method?
- P6, L142-143: How the initial topography of the ice sheet is retrieved? Is it the usual topography used by CAM6? Or is there a step of initialization for CISM? Same questions for freshwater fluxes, from where are they come from? Also, if you use a

different topography than the usual one for CAM6, how the atmospheric module is answering to, sometimes, big differences in elevations?

- P6 L.152-153: “In contrast to the surface topography, the surface albedo is updated as a response to ice sheet melting in both the 1-way and 2-way coupled simulations.” I had to read several times this explanation to really understand what it means. I guess then this is not the best way to explain it. You should more deeply explain what implies this consideration of the albedo. Also, as in the 2-way simulation the ice sheet is retreating, is the albedo-feedback not mitigated by the smaller area of higher albedo (ablation area)?
- P6 L.155: Please specify here how much time your experiments are running. For more clarity, I suggest also to refer to figures 2a and 9a to illustrate you experiment designs.
- P6 L164: Concerning your control simulation, why only have one control simulation with 2-way coupling method? Did you compare it with a similar experiment but with the 1-way method? In other words, does the method to represent the melt-elevation feedback influence the control simulation even if the melt-elevation feedback is supposed to be weak with this CO2 level?
- P7 L179-180 and 183: Could you specify how you normalize with respect to the control simulation? Is it mean you’re considering the variability of the GBI/NAO from the control run to normalize the GBI/NAO from 1w- and 2w-experiment?
- P7 L190-193: Usually, a period of 30 years is considered to talk about climatology mean. Is the choice of 20 years/data point influence your results compared to a 30-year average?

Results

- Fig 3c: The difference between a) and b) gives still a rate per year. This is not sound to the topography feedback to me. I suggest also illustrating the final result in terms of topography with the differences in meters between topography of 1-way and 2-way for year 500.
- P11, L251: Please justify why you also look at the LW down (and not LWup, SWdown, or up).
- P12 L 273-275: I suggest at least adding these references to describe atmospheric blocking: Hanna et al. 2014, McLeod and Mote 2016.
- P12 L276: Are the differences in GBI between both simulations not simply linked to change in height of the surface, at least partly, thus decreasing the geopotential height of 500hPa? If your surface is lowering, the height of the 500hPa geopotential is also lowering, especially as you have differences in surface elevation up to 1000m after 500 years of coupling. And then this GBI decrease will not be entirely due to “real” changes in blocking event regime, and more generally changes in larger scale circulation. Also, I’m surprise to have such differences, even just in winter, in GBI, and not correlated at all with differences in NAO, as these 2 indexes are partly anti-correlated for the current period (Hanna et al., 2015).
- P13 L 281-292: In this part of your results, you should consider the influence of altitude on the GBI computation (as explained in the former comment) before reaching any

conclusions on the relationship between melting and blocking events. Same comment for the discussion (P21 L 406-415) even if this part is already well nuanced.

- P 16 L 315-317: Despite the accuracy of the explanation concerning precipitation, this analysis could be mitigated by comparing the relative importance of the feedbacks mentioned, compared with melt-elevation feedback (Fig 3 and Fig A1h VS. Fig 8) as precipitation is a much lower contributor to the differences between 1- and 2-way experiments.
- P16 L345: "However, the retreated ice sheet margins result in a smaller contribution of ice discharge to the mass balance (Figure 9d)." If I'm right, could you specify that you compared to the PI situation in this sentence? Also, when you consider the "first" state of your comparison (Table 2), please, indicate the years, to be clearer.
- In this comparison (Table 2), you should insist on the fact that, by recovering the same global temperature anomaly, the state of the ice sheet is quite different, as well as the components engaged in the total mass balance. It could be also interesting for your analysis to have a spatial representation of the ice sheet extent for these 2 specific states, and more generally to illustrate what becomes the ice sheet after such a decrease in CO_2.

Discussion

- P21 L412 : "However, Hanna et al. (2018)", Add Delhasse et al 2021, which is the updated version of Hanna et al. 2018 with CMIP6 models.
- P22 L432: Please specify that you're mentioning 1.1m of SLR contribution.

<u>**Typo**</u>

- P2 L51: Please define GMSL;
- L672: The reference of Sellevold *et al.* 2019 should be updated as the paper is not in discussion anymore.

<u>**References:**</u>

Delhasse A, Hanna E, Kittel C, Fettweis X. Brief communication: CMIP6 does not suggest any atmospheric blocking increase in summer over Greenland by 2100. Int J Climatol. 2021;1–8. https://doi.org/10.1002/ joc.6977

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Hanna, E., Fettweis, X., Mernild, S.H., Cappelen, J., Ribergaard, M. H., Shuman, C.A., Steffen, K., Wood, L. and Mote, T.L. (2014) Atmospheric and oceanic climate forcing of the exceptional Greenland ice sheet surface melt in summer 2012. International Journal of Climatology, 34, 1022–1037. https://doi.org/10.1002/ joc.3743.

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McLeod, J.T. and Mote, T.L. (2016) Linking interannual variability in extreme Greenland blocking episodes to the recent increase in summer melting across the Greenland ice sheet. International Journal of Climatology, 36, 1484–1499. https://doi.org/10.1002/joc.4440.

Noël, B., van de Berg, W. J., Machguth, H., Lhermitte, S., Howat, I., Fettweis, X., and van den Broeke, M. R.: A daily, 1 km resolution data set of downscaled Greenland ice sheet surface mass balance (1958–2015), The Cryosphere, 10, 2361–2377, https://doi.org/10.5194/tc-10-2361-2016, 2016.