We thank the reviewers for their constructive feedback on the manuscript. In the following, we provide a short joined response to all reviewers. Thereafter, a response to the specific comments of reviewer 1 is given.

Joint response to all reviewers

On the scope of the manuscript and request to add one-way coupled simulation for the 4x to 1xCO2 reduction scenario : the scope of the manuscript is to examine Greenland ice sheet and climate **interactions**. Feedbacks are one specific type of these interactions, namely those that involve a bi-directional coupling (initial process is augmented or reduced through the feedback). We will make the interaction-feedback distinction more explicit in the introduction of the reviewed manuscript. Quantification of the albedo feedback for 4xCO2 has been done in previous work (Muntjewerf et al, 2020) by examining the contribution of absorbed solar radiation to the total melt energy and a dedicated simulation is not necessary. For this reason, here we focus on the elevation feedback. Since elevation does not change in the mitigation scenario (mass balance becomes approximately zero), we find that it is unnecessary to explore elevation feedbacks there with a one-way coupled simulation.
In addition, we want to clarify that the primary goal of the manuscript is ***not*** to quantify the difference in melt projections for ice-sheet-only and coupled models. We do this only for our model, and the results will be different for other climate models and surface mass balance schemes. In our paper, this numerical comparison makes one part of the manuscript, with the main focus being the physical ***processes*** of ice sheet and climate interaction, and how our model represents them in the one-way and two-way coupled flavors. We will make this more explicit in the reviewed manuscript.

Suggestion to run more simulations: Here we present a set of multi-century "IPCC-type" Earth System Model simulations with a 1 degree atmosphere and dynamical ocean components. This type of model is extremely complex and simulations are computationally very expensive (3,600 core hours are required to run one simulation year). To our knowledge, here we are presenting the first comparison of one-way to two-way simulation with an IPCC-type model. In addition, we present the first assessment of the coupling of global climate, ocean circulation and GrIS snow/firn evolution with an IPCC-type model for a scenario of mitigation. We don't have the means to run more simulations.

Suggestion to eliminate or move the CO_2 reduction simulation to a different paper for consistency or to highlight results separately: we consider this unnecessary as the common theme here is the assessment of processes of ice sheet and climate interaction. The current structure of this manuscript around the theme of ice sheet-climate interactions first shows the effect of elevation feedbacks by looking at an extreme warming scenario and comparing a set-up with and without evolving GrIS topography, and thereafter addresses other interactions (ocean, snow pack) in the light of a mitigation scenario, aiming to quantify the effect of different interactions and feedback on the GrIS mass balance. Besides, the use of different simulations to address one research question (In our case: "Which interactions between the GrIS and the climate affect the GrIS mass balance?") is not uncommon (e.g., see Gregory et al. (2020), analyzing one 1-way and

several 2-way coupled simulations for different warming scenarios and for multiple mitigation scenarios, around the theme of irreversible mass loss). We propose to make some changes to emphasize more on the common theme in this manuscript (interactions and feedbacks) and the connection between both parts.
To make the common theme clearer we propose to change the title to: "Role of elevation feedbacks and ice-climate interactions on future Greenland melt"

Request to run more simulations to provide a "one-fits-all" seasonally varying lapse rate for one-way simulations: we believe this lapse rate will depend both on the modeler choice of climate model forcing and surface mass balance calculation. In this manuscript we do provide a seasonally varying estimate of the temperature lapse rate by comparison of two-way and one-way simulations in CESM. To our knowledge, nobody has provided this sort of estimate. We expect estimates from other models to follow. Crow et al. (2024) is a different type of assessment, where they try different prescribed lapse rates and see which one/type results in a better fit to proxy records.

Request to clarify one-way simulation design: the one-way simulation has evolving albedo as this is calculated interactively in the land component. Ice sheet area and elevation are not evolving in the climate components. Meltwater fluxes to the ocean are not evolving. They are prescribed to those calculated in the pre-industrial simulation. We will clarify the simulation design (choices) further in the reviewed manuscript.

Request to provide justification of fixed lapse rate choice in one-way simulation: a fixed lapse rate was chosen for consistency with the standard design for sub-grid surface mass balance simulation (downscaling) through elevation classes. Other state-of-the-art downscaling techniques suggested by reviewer 3 are not applicable to an Earth System Model as they are based on high-resolution regional modelling at the scale of 10 km.

Questions about albedo feedback: the albedo feedback has been already quantified in a previous study (Muntjewerf et al., 2020). This can be done by looking at the energetic contribution of albedo change (in W/m^2) to the total melt energy. That is, there is no need to perform dedicated sensitivity simulations to quantify this feedback. We will make this more explicit in the revised manuscript.

Response to specific comments of reviewer 1

On the use of acronyms and sentence structure: We agree that "the Greenland and Antarctic ice sheetsyou're your example is more pleasing, however, we feel that the use of the acronym GrIS is so common that this should not be a problem. Regarding our model component acronyms, we will revisit the text and change these to "land/ocean/… model" from section 2.2 forward. Next to that, we will have a look at the connections between the sentences and chosen words, especially for the conclusions and the sentences describing the timing within our simulations.

Response to minor points that are not answered above

Referee comments in black, authors' response in red

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 […]".

Thank you, we will follow this suggestion.

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 […]".

We will include your second suggestion.

Line 52: which period?

We will change this to: "during an overshoot period"

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

Feasibility in the light of policy-making. To answer the question of whether mitigation after a temperature overshoot period can be used to reverse any "damage" that has been done.

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.

Yes it is a conclusion from the modelling studies. We will change line 56 to: "Model-based studies suggest that, if temperature overshoots are limited, …"

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.”

We will include your suggestion, thank you.

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.

We will consider your suggestion for the revised manuscript and rewrite this paragraph to make it less model-specific.

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.

Thank you for your suggestion. First of all, to make more clear that the current description in Section 2.2 is about bi-directional coupling, we propose to change line 111 from “By coupling CISM2 with CESM2, …” to “By applying a 2-way coupling between CISM2 and CESM2, …” , following a suggestion of referee 2. Then, we will move the 1-way and 2-way design to the end of Section 2.2 as you proposed.
The design of 2-way is taken from Muntjewerf et al. (2020). The 2-way simulation is an extended version of the simulation in Muntjewerf et al. (2020), we will add this in the revised manuscript.

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

Until we reach year 500, we will add this.

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

Yes we did that, we will change the wording “extent of the ablation area” to “percentage of the GrIS that experiences ablation” to make this more clear.

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

66% more in 2-way than in 1-way, we will make this more clear and have a check throughout the manuscript for other mentions of percentages.

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

Thank you for pointing out, we will correct the units.

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

Yes, we will indicate that it is the real rate in the revised manuscript.

References

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