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 2 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 2

Referee comments in black, authors' response in red

Have there been other similar simulations done by other CESM2 users you could include as comparison? Or could this be part of a larger study involving, e.g an overshoot scenario study with CESM2-CISM2?

There have not been run more climate mitigation scenarios with the CESM2-CISM2 set-up. Although our simulation could be of interest for a larger study about overshoot scenarios, our objective is not to only look at the mass balance (and therefore SLR projections), but to look at the processes and interactions involved. Although a larger number of simulations with different mitigation scenarios would be very interesting for SLR projections, at this point it would not add significantly to the story of ice sheet-climate interactions that play an important role in mitigation scenarios, especially compared to the computational costs of our model.

Response to minor comments

General comment on figures: some figures might benefit from being slightly wider (e.g. maps). I liked the consistent use of colours but the shades of blue, red and green were sometimes difficult to tell apart, especially in figure 2d and again in figures 7b and 9b. Could you keep the same main colours but change the tint/shade (i.e. make darker tones even darker and lighter ones lighter)?

Thanks for bringing this to our attention, we will change the colors in fig 2d, 7b and 9b and have a look at the other figures as well. We will make fig 1, 3, 8 and 10 wider.

Abstract
p1, line 10: I suppose the lapse rate you're mentioning is the lapse rate used to downscale temperature from the elevation tiles to the ice sheet grid. It would be useful to add that information.

Yes, we will add that.

p1, lines 12-13: add that it is for the 2-way coupled simulation

We will change this to: "Furthermore, we analyze a simulation branched in year 350 from our 2-way coupled simulation in which we annually reduce atmospheric CO2 by 5% until PI concentrations are reached."

1. Introduction
p2, line 52: what do you mean by "such a period"? A certain length of time or simply that it's a warmer period?

A period in which a certain temperature threshold is surpassed. We will change this part to: "The rapidly increasing global temperatures call for the investigation of 'overshoot' scenarios, where this temperature threshold is surpassed. Such an overshoot could have large implications for the evolution of the GrIS-induced SLR, as GMSL could rise substantially under the larger temperatures during an overshoot period." (after following some suggestions from referee 1 as well).

2. Method

p4, lines 96-97: I'm not sure I understand these 2 sentences. Is the snowpack thickness reset to 10m at the beginning of every year? Meaning that if the thickness at the end of the year is 10+X m, X m is the positive SMB that is transferred to CISM2? And, in the second sentence, what do you mean by "further melt"?

The maximum snow thickness is 10 m. When the snowpack exceeds 10m (10 + X), then X m will be transferred to CISM as positive SMB to increase ice thickness (ice accumulation).
We will change "further melt" to "further melt of ice", as this concerns the melt after the 10 m w.e. has already melted (ice ablation).
As the snowpack is only part of CLM, changes that only occur in the snowpack (meaning no ice ablation/accumulation), will not be communicated to CISM.

p4, line 119: I don't think the definition of refreezing capacity is necessary here, as you only mention it much later in the manuscript (p19, end of section 5). Instead, I would just change line 376 (p19) in "The refreezing capacity (amount of refreezing divided by the amount of available water) peaks earlier..."

Thank you, we will follow your suggestion.

p5, line 126 (then p6, line 146 and p11, line 252): The way you wrote these different sentences, I am not sure whether the fixed lapse rate of -6K/km is used in both the 1- and 2-way simulations. From line 126, I think yes but then I was a bit puzzled when reading "as is done in the 2-way coupled configuration" in line 252. Finally, in lines 250 to 254, you mention a computed lapse rate that you compare to the fixed lapse rate of the 1-way simulation.

Yes the fixed lapse rate is used in both the 1-way and 2-way set-up to interpolate from CLM to CISM, using elevation classes. In the 2-way set-up, this lapse rate is only used to interpolate from the coarser CLM grid to the finer CISM grid and allows for taking the nonuniform topography within the larger CLM grid cells into account. In 1-way, the lapse rate is used to describe elevation changes due to melt as well, as the CLM topography is fixed. We will change line 251-252 from "... we compute lapse rates..." to "... we compute the lapse rates resulting from elevation change..." to make this more clear.

If I understood correctly, the lapse rate used for downscaling the temperature from CESM's to CISM's grid is fixed in both simulations. I'd add in line 126 that it is the case in both simulations, to make it clear the first time it's mentioned. And I'd remove "as it is done in the 2-way coupled configuration" entirely in line 126.

We propose to change line 111 from "By coupling CISM2 with CESM2, ..." to "By applying a 2-way coupling between CISM2 and CESM2, ..." to point out that the coupling description in section 2.2 is about bidirectional coupling. We will remove as it is done in the 2-way coupled configuration".

Then, in section 4, lines 250-254 I think the lapse rates that you mention you're computing are computed offline, in the same way as you computed the lapse rates for the ME feedback. I'd add here for clarity that those are computed offline I would then add, either in line 252 or 253, something along the line of "compared them with the fixed applied lapse rate used to downscale temperature and LW during the simulations". I'd also remove the 1-way simulation mention if the 2-way simulation also uses a fixed lapse rate for temperature and LW. I think adding that would make the reader know immediately which lapse rates you are referring too and would make the reading easier.

The lapse rates computed are the 'real' changes of temperature and LW when using 2-way coupling and are computed by comparing the changes in temperature and LW fields with the changes in elevation.

p5, line 137: You're mentioning the fact that, since the coupling from POP2 to CISM2 is not implemented yet (presumably because you need a way to downscale ocean temperatures onto the ice sheet grid in order to be able to resolve the fjords), there is no direct influence of the ocean on the ice sheet via it's forcings on marine terminating glaciers. Could you add a few words about the potential biases this could lead to and the processes involved?

As increases in sea surface temperature are not communicated back, we will not see increases in ice discharge resulting from this. We will add a bit on that: "Therefore ocean-forced melting of marine-terminating glaciers is not accounted for, which could lead to biases in the computed ice discharge." This is however not of great importance for the simulations done in this study, as ice discharge will go to zero when the ice sheet becomes land-terminating under an extreme warming scenario.

p6, line 165 to p7, line 193: Section 2.4 (metrics definitions) I don't think this section is necessary in this form. Some of the concepts are useful to define in a thesis but are well known to the readers of scientific papers (e.g. the definition of ELA) and others are only used much later in the manuscript and should, in my opinion be moved there. I'd keep the definitions of lapse rates, GBI and the moving average and remove the ELA, NAO and IVT.

- Lapse rates: I am not entirely sure what the lapse rates refer to here. I think you're using them to isolate the melt-elevation feedback as this would be the only way to evaluate that the ME feedback leads to more melt in the 2-way simulation since the SMB doesn't decrease as much in the 2-way simulation and the snowfall doesn't differ much. I'd move the definition to section 4.1.
- GBI: I'd keep the definition in the manuscript as you're using the modified GBI proposed by Hanna et al. (2018), which they call GB2. I'd move the definition to section 4.2 and would add that what you're using is called GB2 in Hanna et al.

- Moving averages: I'd keep that one to remind the reader that the length of the moving average changes during the simulations but I'd put it at the end of section 2.3 (simulation design).
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Thanks for the suggestion, we intent to make your proposed changes in the revised manuscript.

3. Simulated mass loss

p8, lines 216-218: Is there a numerical threshold in the NAMOC index for you to consider it to collapse or are you looking at increased rates of change?

We look at increased rates of change.

4. Climate feedbacks

p12, line 262: I'd add orange dashed line after Figure 5c so the reader can spot the point without reading the caption.

Good suggestion, we will add that.

p11, caption figure 4: the blue line shows the monthly mean surface temperature, not the red line.

Thanks for spotting this mistake, we will change it.

6. Discussion

p21, line 425: By considering the surface temperature ? What does considering mean here?

The lapse rates are strongly influenced by surface temperature, as for a melting surface, much of the available energy will be used to melt the surface instead of heating the atmosphere, leading to smaller lapse rates. We will change this line to "...by considering whether the surface temperature has reached melting point." to make this more clear.

Typos, spelling, punctuation

p4, line 103: ice thermodynamicS model instead of thermodynamic? Like in ice dynamics model?
p4, line 106: same p6, line 140: simulationS design?

p6, line 146: -6 K km-1 instead of K/km (as p5, line 126)
p6, line 148: "In contrast, the 2-way coupled run..". to the 1-way coupled run in not necessary here as you were just talking about the 1-way coupled run in the previous sentence.
p6, line 157: I'm not sure the last part of the sentence (after which) is grammatically correct. I would write "the 4xCO2 scenario is an extreme warming scenario and, after the year 140, has a similar radiative forcing to that of the SSP5-85 scenario at the end of the 21st century".

p7, lines 197&203+p8, line 206: 20-year centered moving average (with a hyphen)

p8, line 207: returns within one standard deviation OF the PI mean.

p10, line 244: no comma after we account for this
p10, line 245: comma after while if you put one after 1-way simulation

p12, line 264: no comma after simulation

p15, line 311: not significantly instead of not significant

p16, line 335: here you express SLR in cm but in line 321 you express it in m. Can you check throughout the manuscript and pick one?

p18, line 372: no comma between forcing and are

p21, line 395: as opposed to?
p21, line 399: comma before although
p21, line 403: comma after cloud cover
p21, line 404: the sentence is a bit long so I'd start a new one after transmissivity (This aligns…)

p22, line 433: tens of thousands of years
p22, line 448: no comma after level
p22, line 449: no comma after small
p22, line 450: if we were extending I think

Many thanks for reading the manuscript so thoroughly, we will incorporate you proposed changes regarding spelling, typos and punctuation.

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.

Gregory, J. M., George, S. E., and Smith, R. S.: Large and irreversible future decline of the Greenland ice sheet, The Cryosphere, 14, 4299–4322, https://doi.org/10.5194/tc-14-4299-2020, 2020.

Muntjewerf, L., Sellevold, R., Vizcaíno, M., Ernani da Silva, C., Petrini, M., Thayer-Calder, K., Scherrenberg, M. D. W., Bradley, S. L., Katsman, C. A., Fyke, J., Lipscomb, W. H., Lofverstrom, M., and Sacks, W. J.: Accelerated Greenland Ice Sheet Mass Loss Under High Greenhouse Gas Forcing as Simulated by the Coupled CESM2.1-CISM2.1, Journal of Advances in Modeling Earth Systems, 12, e2019MS002 031, https://doi.org/10.1029/2019MS002031, 2020.