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 all reviewers 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.

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).
-

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.

Response to specific comments of reviewer 3

Abstract

- P1. L6-8: Please add a time reference for your simulations and specify the level of CO2 at which you start.

We will add that these are multi-century simulations and that they start from PI CO2 conditions.

- 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.

You are correct that the overestimation in 1-way is not a new result of our study. However, the novelty here is the attribution of the physical processes causing this overestimation. To make this more clear, we will change this line to: "We also attribute part of the overestimation of mass loss in the 1-way coupled simulation to an overestimation of melt in the ablation area, caused by the use of a uniform temperature lapse rate."

- P1 L12-13: precise that you extend your 2-way coupled simulation instead of a new experiment.

We will change this line 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."

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?

In this coupled set-up, the snow layer is only part of CLM, meaning that changes in only the snowpack will not be communicated to CISM. Only ice ablation (ablation occurring when the snow and firn layer in CLM have completely disappeared) and compaction of snow and firn into ice (when the snow layer exceeds 10 m w.e.) are communicated to CISM.

- P4, L119: Please add equations for SMB and refreeze capacity to illustrate and summarize how SMB and refreeze are considered.

We will add the equations for SMB and SEB here. We will leave the refreezing capacity out in this part and add it in Section 5, where refreezing is discussed (as suggested by reviewer 2).

- 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?

As evaluated by van Noël et al. (2020) and Kampenhout et al. (2020), CESM2 yields a realistic SMB, compared to in-situ observational data and RACMO model data. Since the GrIS topography we get after the spin-up procedure is slightly different than the present-day topography, the SMB will be biased to this as well. As for ice discharge, there is no increased ocean forcing from increasing sea temperatures, since coupling to the ocean modelling is uni-directional, which introduces a bias as well. 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.

- 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?

For every grid cell in CLM, a set of atmospheric variables (a.o. temperature), are computed for 10 different elevations by means of a lapse rate. Then, using these atmospheric variables, the SMB and SEB for all elevation classes is computed. The CLM output is then the area-weighted average of these elevation classes. The CISM SMB is then computed by interpolation using the elevation of the grid cell in CISM. More on this method can be found in Sellevold et al. (2019), but we will also add a brief explanation to the manuscript.

- 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?

The topography of the 1-way and 2-way simulation are the same in year 0, this topography is the one computed from the model spin-up as described by Lofverstrom et al. (2020),

and is slightly different from the present-day topography. Initial freshwater fluxes are computed during the spin-up procedure as well.
The CAM, CLM and CISM topographies are connected in a 2-way coupled set-up. Using the downscaled SMB, every model year a new CISM topography is computed. Then, a new CLM topography (on a coarser grid) will be computed from the CISM topography for every model year as well. Besides, CAM receives an updated topography from CLM every 5 or 10 years (simulation dependent).

- P6 L.152-153: "In contrast to the surface topography, the surface albedo is update 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)?

Thank you for pointing this out, we will change this to: "In contrast to the surface topography, the surface albedo is updated in both the 1-way and 2-way coupling. The model allows for the surface albedo to change. Therefore, as snow and ice melt or snow accumulates, the albedo will be updated."
Regarding the mitigation of the albedo-feedback, yes this is the case, however only later (starting around year 450), this plays a role. Looking at fig 2h, you can see the percentage of area that is ablation area stabilize, which is due to the melting of the margins.

- 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.

Yes, we will add that, and we will follow your suggestion to refer to the figures.

- 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 CO_2 level?

No, we did not do a 1-way control simulation. In lines 200-202, we explain a bit about the choice for this. As in the control simulation the mass balance is nearly zero (0.03 mm/yr), the melt-elevation feedback will not play a role, like you said. As the only difference is that the surface topography in CLM and CAM can evolve when using 2-way coupling, we expect a very similar control run when done with 1-way coupling. Considering how computationally expensive our model is, we do not see enough added value in a 1-way coupled control simulation, and therefore only use the 2-way coupled control simulation.

- 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?

Yes, that is correct, we use the variability from the control run for the normalization. We will change lines 179-180 from “normalize with respect to the control simulation” to “normalize using the variability of the control simulation”. The same holds for line 182.

- 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?

Here, we chose to stick to the 20 years used in earlier studies of CESM2-CISM2 (e.g. Muntjewerf et al. (2020)). As changes in the CO_2 ramp-up and during the CO_2 ramp-down are relatively fast, a 20-year mean might capture strong trends better. However, we do not expect a that using a 30-year mean would influence our results strongly.

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.

Thanks for mentioning this, you are correct. Fig 3c shows the melt differences between 1-way and 2-way (which are then caused by different elevation feedbacks), but this is not the same as the elevation feedback. We will change the title of Fig 3c to “Difference” to make sure there is no confusion about this.
For the topography differences we would like to refer to Fig A1a-e (ice thickness maps from CISM), we will add a reference to this in the text around Fig 3.

- P11, L251: Please justify why you also look at the LW down (and not LWup, SWdown, or up).

The downscaling of LW_down using elevation classes has a large influence on the uncertainty of the SEB (Sellevold et al., 2019). As LW is also dependent on temperature, it would be interesting to see how the lapse rates of the LW and near surface air temperature relate. As for the upward LW component, this will be largely surface temperature dependent, meaning that attributing changes in LW_up to elevation change is difficult.

- P12 L 273-275: I suggest at least adding these references to describe atmospheric blocking: Hanna et al. 2014, McLeod and Mote 2016.

Thank you for the suggestion, we will add these references.

- 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 anticorrelated for the current period (Hanna et al., 2015).

Yes, they are related to height changes. As you say, the 500 hPa geopotential will indeed lower when the surface height is lowering. The way we see it, the lowering of such a big atmospheric 'obstacle' as the GrIS, will result in less persistent high-pressure fields, as these are able to travel more easily. Mullen,1989; Narinesingh et al., 2020; Sellevold et al., 2022 found a positive relationship between orography and blocking events, indicating that a significantly lowering topography can change the blocking regime. In our view, the cause of the changes in blocking is the lowering of the topography, and therefore correcting for the changes in topography would not make that much sense to us. We will however nuance our results a bit more, as it is possible that, as you say, we do not only tackle the changes in blocking but in other circulation patterns as well. We will change the many uses of the term "atmospheric blocking" to "Greenland blocking index". We will add to line 277: "As the large changes in GrIS topography might have large influences on the atmospheric circulation around Greenland as a whole, it is possible that our computed changes in blocking index do not only consist of changes in blocking but are influenced by other changes in atmospheric circulation as well."

- 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.

For the part about the results section, we would like to refer to the answer above. Regarding the discussion, here we would like to change the term "blocking" to "blocking index" as well. Besides, we would like to change lines 410-411 to: "However, further investigation into the causes of these changes in blocking and their relationship with melt is necessary to make a definite attribution, especially since the robustness of the blocking index towards large topographic changes has not been evaluated."

- 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.

Here, we would like to refer to line 400 of the discussion and Fig A3, stating that the contribution of the larger snowfall in 2-way is limited. We will add "limited (..), *compared to other feedbacks.*" to this line.

- 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.

Thank you for pointing out that this is unclear, we will indicate that this is compared to PI and add the years in the comparison.

- 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 CO2.

For us, the point of showing this comparison, is to make clear that, despite the similar degree of warming, the ice sheet is in a completely different state. We will make this more clear. We agree that a spatial representation of these two states would definitely be interesting to add, therefore we intent to add this to the revised manuscript.

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.

Thank you for this suggestion, we will add this reference.

- P22 L432: Please specify that you're mentioning 1.1m of SLR contribution.

Thanks for spotting this mistake.

Typo

- P2 L51: Please define GMSL;

We will define it in the revised manuscript.

- L672: The reference of Sellevold et al. 2019 should be updated as the paper is not in discussion anymore.

Thank you for finding this mistake, we will update this reference.

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