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 3 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 3

**Abstract**

- P1. L6-8: Please add a time reference for your simulations and specify the level of $CO_2$ at which you start.

We will add that these are multi-century simulations and that they start from PI $CO_2$ 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 $CO_2$ 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 CO2 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.

## 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.

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

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.

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.

Hanna, E., Fettweis, X., and Hall, R. J.: Brief communication: Recent changes in summer Greenland blocking captured by none of the CMIP5 models, The Cryosphere, 12, 3287–3292, https://doi.org/10.5194/tc-12-3287-2018, 2018.

Hanna, E., Cropper, T.E., Jones, P.D., Scaife, A.A. and Allan, R. (2015) Recent seasonal asymmetric changes in the NAO (a marked summer decline and increased winter variability) and associated changes in the AO and Greenland blocking index. International Journal of Climatology, 35, 2540–2554. https://doi.org/10.1002/joc.4157.

Lofverstrom, M., Fyke, J. G., Thayer-Calder, K., Muntjewerf, L., Vizcaino, M., Sacks, W. J., Lipscomb, W. H., Otto-Bliesner, B. L., & Bradley, S. L. (2020). An Efficient Ice Sheet/Earth System Model Spin-up Procedure for CESM2-CISM2: Description, Evaluation, and Broader Applicability. Journal of Advances in Modeling Earth Systems, 12(8), e2019MS001984. https://doi.org/10.1029/2019MS001984

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.

Mullen, S. L.: The Impact of Orography on Blocking Frequency in a General Circulation Model, Journal of Climate, 2, 1554–1560, https://doi.org/10.1175/15200442(1989)002<1554:TIOOOB>2.0.CO;2, 1989.

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.

Narinesingh, V., Booth, J. F., Clark, S. K., and Ming, Y.: Atmospheric Blocking: The Impact of Topography in an Idealized General Circulation Model, Weather and Climate Dynamics Discussions, https://doi.org/10.5194/wcd-2020-2, 2020.

Noël, B., Van Kampenhout, L., Van De Berg, W. J., Lenaerts, J., Wouters, B., & Van Den Broeke, M. R. (2020). Brief communication: CESM2 climate forcing (1950–2014) yields realistic Greenland ice sheet surface mass balance. The Cryosphere, 14(4), 1425-1435.

Sellevold, R., van Kampenhout, L., Lenaerts, J. T. M., Noël, B., Lipscomb, W. H., and Vizcaino, M.: Surface mass balance downscaling through elevation classes in an Earth system model: application to the Greenland ice sheet, The Cryosphere, 13, 3193–3208, https://doi.org/10.5194/tc-13-3193-2019, 2019.

Sellevold, R., Lenaerts, J. T. M., and Vizcaíno, M.: Influence of Arctic sea-ice loss on the Greenland ice sheet climate, Climate Dynamics, 58, 179–193, https://doi.org/10.1007/s00382-021-05897-4, 2022.

van Kampenhout, L., Lenaerts, J. T., Lipscomb, W. H., Lhermitte, S., Noël, B., Vizcaíno, M., ... & van den Broeke, M. R. (2020). Present-day Greenland ice sheet climate and surface mass balance in CESM2. *Journal of Geophysical Research: Earth Surface*, *125*(2), e2019JF005318.