Review of the manuscript “Positive feedbacks drive the Greenland ice sheet evolution in millennial-length MAR–GISM simulations under a high-end warming scenario “, by Chloë Marie Paice et al.

Paice et al. present the first coupled simulations of an ice sheet model and a regional climate model on a millennial timescale. To this end, they coupled the ice sheet model GISM to the regional climate model MAR, forcing it with IPSL-CM6A-LR global climate model output under the SSP5-8.5 scenario. They compare a zero-way (elevation changes are not taken into account), one-way (offline correction to consider elevation changes), and two-way (elevation changes are communicated between GISM and MAR) simulation, spanning the period 1990-3000. The analysis focuses on feedbacks arising from Greenland ice sheet elevation changes. The authors discuss several positive and negative feedbacks that play a role over different time scales and relate this to the surface mass balance and ice mass loss.

This study builds on the work done by Le clec’h et al. (2019) and Delhasse et al. (2024), in which MAR was coupled to the ice sheet models GRISLI and PISM. This study brings something new by presenting simulations spanning over a millennium. Over longer timescales, the effects of coupling complexity on ice mass loss become more pronounced, and feedbacks that play a role on longer timescales can be identified. This study presents a sound methodology and brings new and interesting results that will be valuable to the ice sheet modelling community. The paper is well written, and the results are presented in clear figures. My comments mainly concern the attribution of the differences between the simulations to the different feedbacks and the interpretation of climate interactions. After addressing these, I would recommend this manuscript for publication in The Cryosphere.

References:

Le clec'h, S., Charbit, S., Quiquet, A., Fettweis, X., Dumas, C., Kageyama, M., Wyard, C., and Ritz, C.: Assessment of the Greenland ice sheet–atmosphere feedbacks for the next century with a regional atmospheric model coupled to an ice sheet model, The Cryosphere, 13, 373–395, https://doi.org/10.5194/tc-13-373-2019, 2019.

Delhasse, A., Beckmann, J., Kittel, C., and Fettweis, X.: Coupling MAR (Modèle Atmosphérique Régional) with PISM (Parallel Ice Sheet Model) mitigates the positive melt–elevation feedback, The Cryosphere, 18, 633–651, https://doi.org/10.5194/tc-18- 633-2024, 2024.

We would like to thank the reviewer for their dedicated time thoroughly reviewing the manuscript and for their useful and constructive suggestions. We have carefully addressed all comments by the reviewer (in blue, with track changes below when applicable), and the manuscript will strongly benefit from the proposed changes.

Major comments

The choice of the ice sheet model: To me, it is unclear why you couple MAR to another ice sheet model, rather than using the existing MAR-GRISLI or MAR-PISM. Is this for computational reasons, or to explore the sensitivity to the choice of ISM? Can you add something about the choice for this model at the beginning of section 2 or in section 2.1?

Thank you for notifying us that this was indeed not addressed. GISM is the ice sheet model developed at the Vrije Universiteit Brussel (VUB) to which the 1st and 3rd author are affiliated. Besides, there are several other reasons why we opted for GISM as the ice sheet model in the coupling framework. We will clarify this by adding the following paragraph at the end of section 2.1:

Compared to the previous two ice sheet models that were coupled to MAR, the advantages of GISM are its (new) possibility to combine a glacial-interglacial spin-up and data assimilation for its initialization, as explained below, and the fact that the higher-order model version can still run at relatively high resolution for the envisioned millennial timescale. Additionally, by coupling MAR with another ice sheet model, we can evaluate the robustness of the results compared to those obtained with GRISLI and PISM (Le clec'h et al., 2019b; Delhasse et al., 2024).

Section 3.3 on wind speed changes: In this section, the reason for a different pattern in SMB changes between the simulations is attributed to changes in katabatic and barrier winds. Although I find these results very interesting, I do not think you can attribute all these differences to differences in wind patterns. I would also suggest looking into the effect of your offline correction on the differences in the near-surface air temperature changes at the margin. In your offline correction, you use an SMB gradient based on a climate computed at a higher elevation (since the MAR topography does not change). Using these SMB-elevation gradients, you implicitly assume some sort of relationship between temperature and elevation. When a grid cell becomes part of the ablation area in GISM, but not in MAR, the relationship you find based on the MAR SMB will not hold for GISM, because the energy required for melting reduces temperature increases. This (combined with the decreased winds) might lead to a too strong SMB-elevation gradient around the margins. Hence, your corrected SMB might correspond to a higher temperature than you would find in two-way coupled. You can also see this behaviour in Fig. 7a, where the strong increase in temperature at the high elevations is due to the initial global warming until 2300, the slower, but rather linear increase thereafter results from the temperature-elevation feedback, and the constant temperatures around zero for the lower elevations are because of the thermodynamic limit of melting ice. The timing of changing towards a new type of temperature-elevation relationship would then likely not be the same between MAR and GISM, leading to differences in SMB. I understand that you do not downscale temperature separately for your offline correction, but I would suggest thinking about this at least in a conceptual way.

Thank you for this interesting question and explanation. However, we do not think that the explanation regarding the temperature–elevation gradients applies here, as the largest SMB differences take place at the (current) ice sheet margin where the vertical SMB gradients are fully based on SMB values over the (present-day) ablation zone. Moreover, the ablation zone rapidly expands over the whole ice sheet after

2100, even in the offline simulation, and follows the same evolution on both the MAR and GISM grid for all three simulations as a result of the high-warming GCM forcing. We will update Figure 8 to show the ablation zone expansion on both grids. Therefore, the SMB–elevation gradients remain based on SMB values computed in the (offline) ablation zone where energy is used to melt the snow in MAR, even after a significant retreat of the ice sheet.
Hence, we believe that what we explain and show in this section and Figure 7 is sound, namely that the offline extrapolation does not remain valid at the ice sheet margins, even before 2300, and that ultimately this is due to wind changes in the 2wC simulation, that in turn lead to changes in other variables. After 2300, this effect or feedback effect persists, and additionally, the extrapolation starts to fail overall for the 1wC simulation because of the increasing differences between the MAR and GISM topographies in this simulation.

Regarding your statement on the increase of cloudiness derived from the increased long- and decreased shortwave radiation, I agree that the radiation changes are likely because of an increase in cloudiness, but you should consider a few other processes as well. The increase in water vapour in the column due to both the increase in temperature (Clausius-Clapeyron) and the elevation decrease leading to a larger atmospheric column will also lead to a decrease in shortwave and an increase in longwave radiation. Additionally, for the longwave radiation, increasing air temperatures affect the Stefan-Boltzmann relationship as well. Then, you attribute this increase in cloudiness to decreasing wind speeds, resulting in “cloud accumulation”. Although this is a likely explanation for at least a part of this increased cloudiness, you should not forget about the increase in cloudiness due to a warming atmosphere (Clausius-Clapeyron), which you do mention in section 3.6.1, but should be mentioned here as well.
We fully agree with the reviewer regarding these other processes influencing the radiation balance and observed changes. However, the changes that we describe in this paragraph only concern the changes or differences that we observe at the ice-marginal zone compared to the ice sheet interior or area(s) further away from the margin. We are explaining the inversion of the SMB–elevation gradient at the margin, not the changes in cloudiness itself. What we meant here is that changes in cloudiness at the margin are a potential side-effect of the observed changes in wind speed. We do not observe abrupt changes in the LWD and SWD radiation (and therefore clouds) at the margin, as for wind speed. Therefore, we are confident that the changes in the radiative components are caused by changing wind speeds. We will rephrase our text to reduce ambiguity:

As demonstrated by a transect from the ice sheet interior to the margin (Fig. 6), these changes in wind speed impact the surface energy balance and result in lowered runoff and a reduced or even inversed SMB–elevation gradient in the ice-marginal zone in the 2wC simulation. Figure 7 disentangles the impact of these changing wind speeds on the different radiation balance components. ~~This is because the changing wind speeds~~Moreover, the increased katabatic winds lead to an accumulation of cold air along the ice-marginal zone (~~sudden~~ slightly decreasing temperature and strongly decreasing sensible heat flux) and potentially clouds (continuously increasing~~ed~~ long- and decreasing~~ed~~ ~~short wave~~shortwave downward radiation towards the margin) ~~along the ice-marginal zone~~, as well as deposition or condensation onto the ice sheet behind the ice-marginal zone (increased latent heat flux). Decreasing barrier winds on the other hand reduce runoff in the ice-marginal zone ~~(Fig. 7)~~.

For this section, it is also not completely clear to me whether you are only considering the period up to 2300 or the whole period. Initially, I was thinking about the whole period, since the figures 6 and 7 show this as well, but then lines 353-354 contradict this. So please state more clearly which period you are considering and if this is only a shorter period (e.g., till 2300), where to look in the figures.

We are indeed considering the whole period. The Figures show that the observed patterns and negative wind feedback at the margins persist for the entire period, i.e. 1990-3000. Yet, what we meant is that over time, throughout the 2wC simulation, the positive feedbacks become more important in terms of ice mass loss (i.e. the negative wind feedback does not counteract the observed positive feedback effects). We will clarify this in the text:

After 2300, this negative wind feedback at the margins persists as Figures 6 and 7 show, but over time nevertheless ~~is overruled by~~ the positive melt–elevation feedback and related changes in the atmospheric circulation amplify the ice mass loss everywhere in the 2wC simulation, as explained in the next sections.

Section 3.6.1 on clouds: This section discusses the changes in cloudiness and cloud phase and their effect on the shortwave and longwave radiative effect. I am happy to see that you separate solid and liquid clouds and consider their distinct effects on radiation. I do think you are missing a few things regarding the cloud effect on radiation and the claim that clouds act here as a positive feedback. That there is no net effect on shortwave radiation does not mean there is no feedback effect of clouds on shortwave radiation. In fact, I would say that the negative feedback of clouds on shortwave radiation counters the positive melt-albedo feedback by reducing incoming shortwave radiation.

We agree with the reviewer regarding the (balancing) effects of clouds and albedo on shortwave radiation, and we feel like we had addressed this in sections 3.4, 3.6.1, and 4.1 albeit less explicitly than stated here. We will update the text to better disentangle and address the effects of these different processes (see also comment hereafter).

Section 3.6.1:

Regarding the increase in cloudiness, and the changing ratio of solid and liquid clouds with differing longwave emissivity, several~~two positive~~ feedback effects can be observed over time, that are stronger for the 2wC compared to the 1wC and 0wC simulations. Moreover, following the Clausius – Clapeyron relation, specifying that a warmer atmosphere can hold more water, the rising air temperatures lead to an increase in water vapour and clouds in the Arctic. Together with the decreasing ice sheet surface elevation and subsequent thicker atmospheric column, ~~t~~this leads to a negative feedback on or decrease in incoming shortwave downward (SWD) radiation. ~~b~~But~~,~~ due to the increasing number of bare ice days per year and the reduced surface albedo, the absorbed SWD at the surface remains relatively stable (Fig. 11~~0~~).

Additionally, the observed changes in the radiative fluxes are the sum of changes in more components than just clouds. To make a definite statement towards the sign of the cloud radiative effect and feedbacks, you would need to disentangle the effects of clouds, water vapor, temperature and albedo, which would be very complex using the set of simulations you did and involve a lot of assumptions. Therefore, I suggest toning down and mentioning that there are more components influencing the radiation budget. Also consider this in lines 542-548 in the discussion. Specifically, “longwave warming outweighing cooling associated to the reduced incoming shortwave radiation” would be a too strong statement since you cannot separately compute and compare the longwave and shortwave radiative effects from your results.

We would like to thank the reviewer for this useful comment, that serves to disentangle different feedback effects and better explain them in the manuscript. We agree that the changes in the radiative fluxes are the result of more components than just clouds and that it is very difficult to quantify all the separate effects. We have therefore better addressed this in our discussion. Nevertheless, we believe that it can still be stated based on our results that the warming effect of the increased LWD radiation outweighs the cooling effect of the decreased SWD radiation in our 2wC simulation, though it is right that these changes cannot only be attributed to the changes in clouds. In total however, when considering all the above-mentioned effects on the radiative budget, the changes in clouds and the melt–elevation feedback do amplify one another here. Hence, we suggest to rewrite this paragraph in the discussion to nuance our previous statements.

Section 4.1:

In addition, ~~the runoff is also further amplified through increased liquid clouds that raise the LWD radiation, with a higher longwave emissivity than solid clouds.~~ important changes are observed in the radiative budget, whereby a distinction can be made between the effects on the SWD and LWD radiation. Firstly, our results show that the negative feedback of increased clouds and water vapour (through the Clausius-Clapeyron relation and thickening atmospheric column) on the SWD radiation balances the positive melt-albedo feedback by reducing incoming shortwave radiation. In other words, the net absorbed shortwave radiation remains stable as the albedo decreases. Thus, the cooling effect from the reduction in SWD radiation is outweighed by the warming effect of the stronger increase in LWD radiation. The latter is the combined result of the increasing atmospheric temperatures (Stefan-Boltzmann relation) and the increase in water vapour and mainly liquid clouds with a higher longwave emissivity than solid clouds. ~~Hence, also clouds serve as a positive feedback, with long wave warming outweighing cooling associated to the reduced incoming short wave radiation.~~ Hence, though it is difficult to quantify all the effects separately, altogether the changes in clouds and the positive melt–elevation feedback amplify one another in our 2wC simulation. Though this seems to contrast with other long-term modelling studies that do not regard the impact of cloud changes on the ~~long wave~~longwave radiation (Gregory et al., 2020; Feenstra et al., 202~~5~~4), it aligns with earlier findings regarding the impact of clouds on the Greenland near-surface climate and surface energy balance (Franco et al., 2013; Vizcaíno et al., 2014; Van Tricht et al., 2016; Hofer et al., 2019; Lenaerts et al., 2020).

And additionally, we will rewrite the conclusions accordingly:

In addition, the rising atmospheric temperatures, ~~decreasing surface elevation~~and increasing water vapour coincide~~s~~ with an increase in mainly liquid clouds that further increase the ~~, that act as an additional positive feedback for~~ runoff through amplified LWD radiation. Though at the same time the increased clouds and water vapour act as a negative feedback effect on the SWD radiation, this effect is balanced by the positive melt–albedo feedback. Hence, altogether the positive feedback effects on the radiative budget prevail.

Minor/technical comments

p1, line 9: “predicting” -> “projecting”
Ok.

p2, line 56: remove “and winds”, since you only discuss this in the next paragraph.
We will.

p3, line 84-85: “It is the first time ice sheet–atmosphere interactions are accurately accounted for on the millennial timescale.” I would tone down here. As you mention, there are a number of studies that presented two-way coupled simulations on millennial timescales before, but with lower resolution or complexity. Therefore, these MAR-GISM simulations with higher resolution and/or complexity than used in these other studies will add a lot to our understanding of feedbacks, but 30 km is still a rather coarse resolution (as you mention in the discussion), and both MAR and GISM have their limitations as well. I would suggest highlighting that your simulations are run on the highest resolution or that your model has the highest complexity, rather than stating that they “are accurately accounted for”.
We thank the reviewer for their rightful comment and will update the text accordingly:
It is the first time ice sheet–atmosphere interactions are ~~accurately~~ accounted for using an RCM on the millennial timescale.

p4, line 109: “using the higher-order approximation”, what kind of approximation is this? Could you mention it briefly?
Thank you for stipulating that this was not described in the text. We will therefore clarify it further (and will update the reference list):
Though ~~it considers~~ different approximations to the force balance equations governing ice flow can be considered, the presented simulations are performed using the higher-order approximation, which includes multilayer longitudinal stresses and lateral horizontal shearing. It is classified as a LMLa higher-order model or Blatter-Pattyn model (Blatter et al., 1995; Pattyn, 2003; Hindmarsch, 2004) and is described in detail in Fürst et al. (2011). It is~~,~~ complemented by a simplified equation to describe the basal resistance (called SR HO in Fürst et al., 2013) in the basal sliding formulation.

p4, line 116-117: What do you mean by “acceptable level of discrepancy between the model resolutions”? Do you mean that the resolutions should not be too different, and it would not be ideal to run GISM on a much higher resolution?
That is indeed what we mean. We will make this clearer as follows:
Besides, ideally the GISM resolution should not be too high with respect to the MAR resolution. ~~Additionally, it is important~~ to maintain a~~n~~ ~~acceptable~~reasonable level of discrepancy between ~~the GISM and MAR~~both model resolutions~~,~~ ~~both~~ throughout the coupled simulations, and to facilitate the efficient initialization of the ice sheet and coupled model into an equilibrium state resembling present-day observations.

p6, line 170-174: Can you briefly explain what these optimization and relaxation steps are?
Thank you for pointing out that we did not explicitly mention the purpose of both steps. We will add the following brief explanations:
It consists of an optimization step, during which the basal sliding coefficient (BSC) and

enhancement factor (EF) are updated to match the modelled ice thickness to observations. This is followed by a relaxation step, during which the ISM is run in free geometry mode with the inferred BSC and EF fields, to minimize any remaining model drift. ~~that~~ Both steps are repeated until the root mean square error (RMSE) between the modelled and observed ice thickness no longer improves significantly (~~Leclec'h~~Le clec'h et al., 2019a).

p10, line 266: several hundred metres around the margins, where the ice thickness is small, is quite a large relative difference. Do you know what causes these differences (particularly in the central east)? How does it affect your results?

We agree that several hundred metres is indeed a relatively large difference. However, this is in line with expectations and can be explained as follows:

As can be seen on Fig. 2, the differences between the initialized and observed present-day topography (Morlighem et al., 2017) are generally less than 40 m and up to several hundred metres around the steep margin, where a minor misalignment of slope can cause large differences between the modelled and observed ice thickness. Besides, the observed topography is very irregular around the ice sheet margins and the central east in particular, which is marked by coastal mountain ranges with deep valleys in between, and an ensuing high spatial variability in ice thickness. As also reported by Le clec'h et al. (2019a), it is therefore most difficult to obtain a well-constrained ice thickness in these areas.

This does not significantly influence our results, since the overall present-day ice sheet geometry is well-represented (without the need for flux corrections or forced ice removal at the ice sheet boundaries) and the MAR SMB at the start of our simulations is the one over this initial GISM topography thanks to our iterative MAR-GISM initialization.

p12, line 275-276: I am a bit confused about the definition of the GBI region. It makes sense to show the temperature evolution over this region, but since you are not considering the GBI in your analysis, I think it only adds confusion. You could just refer to it as "Greenland region" or something similar, with the definition of the coordinates.

Thank you for indicating this confusion. We will change it with the following replacement in the text and in the subscript of Figure 3:

Greenland Blocking Index (GBI) region → larger Greenland region

with indication of the coordinates in-text.

p13, fig 3: same for GBI region, and add that the MAR 2m mean annual air temperature is for the two-way simulation.

Ok. The latter was implied by "the retreating ice mask" but we will explicitly mention it for clarity:

… the MAR 2 m mean annual air temperature over the retreating ice mask (in °C), i.e. for the 2wC simulation, …

P15, line 310-313: I do not see a clear overestimation of SMB in one-way over the interior.

We agree that this overestimation is not very apparent in the Figure. This is because the order of magnitude is much smaller (~10 to 20 mm w.e.) than for the underestimation at the edges (order of 1 to several metres w.e.). We will address this in the text as follows:

Especially around 2200 it becomes clear that a spatial compensation of differences within the SMB fields is at play, with ablation in the 1wC simulation being overestimated (by 1 to >3 m w.e.) compared to the 2wC simulation within 60 km from the ice sheet margins (i.e. two MAR grid cells), referred to hereafter as the ice-marginal zone, and slightly underestimated *over the interior compared to the 2wC simulation (by generally 10 to 20 mm w.e.).*

(…)

In other words, the~~is~~ over- and (weaker but more widespread) underestimation of SMB on the 5 km GISM grid can be linked to the offline extrapolation that falls short at the ice sheet margins over time.

And further down:

This explains the over- and (slight) underestimation of ablation with respect to the 2wC simulation and the ensuing similar ice sheet contribution up to 2300.

In addition, note that we have also referred to it in our description of Figure 6, where it is easier to see:

In addition, Fig. 6 demonstrates that even throughout the 2wC simulation, the extrapolated SMB on the 5 km grid is too negative compared to the SMB on the original 30 km grid when the grid cell is at the ice sheet margin (reduced colour saturation) and slightly overestimated when it is part of the ice sheet interior (full colour saturation).

p15, line 313-315: I had to read this multiple times to understand what you are referring to. Can you rephrase in such a way that it is clear that you mean the change of sign of the difference between the GISM SMB between one-way and two-way, which does not occur for the MAR SMB?

Thank you for indicating that this part was difficult to understand. We will reformulate it more straightforwardly:

However, as Fig. 5b demonstrates, the magnitude and sign of this ablation difference between the 1wC and 2wC SMB on the 5 km GISM grid ~~over- and underestimation of ablation~~ not only vary~~ies~~ spatially but also vary~~ies~~ over time. ~~Besides, it clarifies that this is only the case for the SMB extrapolated onto the 5 km GISM grid, not for the original SMB on the 30 km MAR grid.~~On the other hand, on the 30 km MAR grid, the ablation is always more negative for the 2wC simulation and the difference with the 1wC simulation is ever larger over time. ~~-~~In other words, the~~is~~ over- and (weaker but more widespread) underestimation of SMB on the 5 km GISM grid can be linked to the offline extrapolation that falls short at the ice sheet margins over time.

p16, line 329: decreased barrier winds, compared to what? I assume this is a decrease over time and only observed in two-way?

Indeed, that is exactly what we mean. We will specify it as follows:

Similar to Delhasse et al. (2024), over time we observe decreased barrier wind speeds in the 2wC simulation within the (30 km to 60 km broad) ice-marginal zone and increased katabatic wind speeds behind this zone further inland because of the retreating margin and steepening slopes.

p16-17, line 326-335: How do you define barrier and katabatic winds here? You describe the maximum changes in wind speed; are these the locations with the largest change? I think it could be very interesting to add some plots showing the (changes in) katabatic and barrier wind speeds over the ice sheet. Or otherwise, at least describe where these changes are most pronounced, or whether the behaviour is very similar along the whole Greenland margin.

Katabatic winds consist of cooled dense air masses above the ice sheet surface that accelerate down the ice sheet slopes. Barrier winds arise when the geostrophic wind at the top of the atmospheric boundary layer is directed from the warmer tundra towards the colder ice sheet surface and is confined by the ice sheet acting as a topographic barrier, resulting in a jet blowing (clockwise) along the ice sheet edge. The katabatic and barrier winds can thus be distinguished based on their direction

relative to the ice sheet.
The described maximum changes in wind speed indeed refer to the locations with the largest change. We will add a map showing these overall wind speed changes over the entire ice sheet for the 2wC versus the 1wC and 0wC simulations and refer to it in the text.

p17, line 339-340: I do not see this sudden decrease in temperature in Fig. 7a.
We agree that the decrease in temperature is not very large for most depicted grid cells and that the "sudden" or substantial decrease mainly applies to the sensible heat flux. We will therefore remove the word "sudden" here.

p18, Fig. 6c: It is not clear to me what all these lines are. If you plot one line for the MAR SMB and one for the GISM SMB for 9 locations, you would end up with fewer lines than you have now. Also, the reduced colour saturation makes the lines with small markers quite hard to see on the left side.
These lines depict the values for all the corresponding GISM grid cells, which are more numerous than the number of MAR grid cells, as one 30 km grid cell corresponds to roughly six GISM grid cells at 5 km resolution. To avoid confusion, we will explicitly mention this in the figure description:
For the SMB (c), both the values for the 30 km MAR grid cells as well as the extrapolated values for the corresponding 5 km GISM grid cells (small markers) are shown. Note that the number of corresponding GISM grid cells is higher than the number of MAR grid cells, as one 30 km grid cell corresponds to roughly six GISM grid cells at 5 km resolution.
To improve the readability of the figure, we will increase the size of panels b and c with respect to a, and we will slightly increase the size and saturation of the small markers.

p19, Fig.7: longwave and shortwave instead of long wave and short wave. There are also multiple instances of this in the text (e.g., line 340 and section 3.6.1).
We will update all instances.

p20, line 373: Please state whether you consider the MAR or GISM SMB and refer to Fig. 11.
We would like to thank the reviewer for their observant comment. The SMB on both the MAR and GISM grid is indeed displayed in Figure 11. We will move it upwards in the text to refer to it here, and will change the figure numbering of Figures 8-11 accordingly.

p20, line 376-379: Is the bare ice albedo in MAR a fixed value?
The bare ice albedo in MAR is not exactly fixed, though the variations are small:
This implies that there is practically no difference in the positive melt–surface albedo feedback between all three simulations, as most of the ice sheet surface consists of bare ice during the Arctic summer in all three simulations (Fig. 10~~9~~). ~~and t~~The difference in albedo (that varies between 0.50 and 0.55 for bare ice in MAR depending on the presence of surface meltwater) and absorbed incoming solar radiation at the ice sheet surface between simulations is therefore negligible (Fig. 11~~0~~).

p23, Fig. 10: I would suggest showing the summer fluxes instead of the annual mean, since the melt mainly happens in summer.

We agree to show the summer (June-August) fluxes and will update the figure.

p24, line 411: mean surface elevation -> mean surface elevation decrease

Ok.

p24, line 413: "The time of this intensification", I assume this is 2500?

Indeed. We will add this to the sentence for clarity:

At the time of this intensification, around 2500, the mean annual 2 m air temperature…

p25, line 425: What is meant by "entire Arctic summer"? From the context, I would understand this is >150 days. Please define Arctic summer or mention that you refer to the yellow line in the plot.

We were referring to the (extended) summer period of 120 runoff days earlier in the sentence, but agree that it may sound ambiguous and will therefore remove it:

Around 2500, an accelerated expansion can be observed in the ice sheet area exhibiting runoff during at least 120 days per year ~~, or, during the entire Arctic summer~~ (Fig. 12).

p25, line 428-429: Do you know why the areal expansion for runoff days is so different from the number of bare ice days?

Thank you for this interesting question. Based on the observed changes in precipitation and density of the upper ice layers we can conclude that the rapid areal expansion of bare ice days is linked to changes in snowfall. Already by 2200 most of the snowfall melts before densifying to firn in all simulations (section 3.4), and there is hardly any (summer) snowfall remaining (at most 4.5 % by 2200 and 2.4 % by 2300 in all simulations) (section 3.6.2). Nevertheless, the slower expansion of runoff days is linked to the available energy for melt, that remains too low over most of the ice sheet area to generate melt and runoff throughout a large part of the year due to the low solar elevation angle, especially during the winter months (section 3.5).

p25, line 431-436: I do not think this is very new. With no incoming shortwave radiation for a large part of the ice sheet during winter, it is no surprise that there is no energy available for melt. Therefore, I would focus more on the extension of the melt season, which you did in the previous paragraph, and consider shortening or removing this paragraph.

A low amount of incoming shortwave radiation will indeed naturally lead to a low amount (of energy available for) melt. However, what we wanted to stress is that though the melt season keeps expanding in the 2wC simulation, this expansion is physically constrained by the low solar elevation angle in winter. We suggest to rephrase the paragraph accordingly:

Nevertheless, the proportion of June–August runoff remains 68 % or more of the annual total, compared to 94 % at the start of the simulations and 76 % by 2300 in the 2wC simulation. This is because even after 2300 the available energy during the Arctic winter months is hardly enough to melt the fresh snow layer over most of the ice sheet area. In other words, though the melt season keeps expanding, the number of runoff and bare ice days per year and therefore the strength of the melt–elevation feedback remain physically constrained by the low solar elevation angle (Fig. 10 and 12).

p22, Fig. 9 and p26, Fig. 12: Please make the colours consistent between the plots. Now, in Fig. 9, the shortest number of days is in red, and the longest in blue, while for Fig. 12, it is the other way around.

Thank you for pointing out that the colour scale of both figures was accidentally still each other's inverse. We will rectify this.

p27, line 459-460: A thicker atmospheric column also results in changes in shortwave radiation. Additionally, you should consider the effect of the Clausius-Clapeyron relationship here as well. It does not only result in an increase in cloudiness, but in water vapour as well, influencing radiation. For longwave, temperature changes can have an influence as well through the Stefan-Boltzmann relationship.

We agree with the reviewer that we could have been more specific here and should have mentioned the impact of the Stefan-Botzmann relation on the longwave radiation. We will update the paragraph accordingly (same as above):

Regarding the increase in cloudiness, and the changing ratio of solid and liquid clouds with differing longwave emissivity, several ~~two positive~~ feedback effects can be observed over time, that are stronger for the 2wC compared to the 1wC and 0wC simulations. Moreover, following the Clausius – Clapeyron relation, specifying that a warmer atmosphere can hold more water, the rising air temperatures lead to an increase in water vapour and clouds in the Arctic. Together with the decreasing ice sheet surface elevation and subsequent thicker atmospheric column, this leads to a negative feedback on or decrease in incoming shortwave downward (SWD) radiation. ~~b~~But, due to the increasing number of bare ice days per year and the reduced surface albedo, the absorbed SWD at the surface remains relatively stable (Fig. 11~~0~~).

(…)

As can be seen on Fig. 13, after 2300, the increase in longwave downward radiation continues and by 3000 it has increased 26 % more compared to the 1wC and 0wC simulations over the same retreating ice mask. Again, ~~T~~this is due to both the increase in cloudiness and the reduced surface elevation that results in a thicker atmospheric column above the surface. In addition, the ~~and the consequently~~ enhanced longwave radiation can also be attributed to the Stefan-Boltzmann relation, stating that the total radiated energy by a body or matter is directly proportional to its temperature to the fourth power.

p28, Fig. 13: Do you show the increase in cloud cover or cloud water path here? Please mention this. In case you used cloud cover, I would suggest looking into the water path, since this likely tells you more about the amount of clouds and their radiative effect than just cloud cover. An increase in cloud water path might not be reflected in the cloud cover, but it still has large effects on radiation.

Thank you for mentioning that this was not specified. We are showing the changes in cloud water path and will update the axis label and figure description:

Figure 13: Percentual i~~I~~ncrease in solid clouds (ice water path), liquid clouds (water vapour path) and the sum of both (a) ~~(solid, liquid and the sum of both)~~, percentual increase in ~~long wave~~longwave downward (LWD) radiation (b), and multiplication factor of runoff (c) with respect to the 30 year mean at the start of the simulations (1990–2019). Depicted is the 30 year running mean over the retreating ice mask for the 2wC simulation, and over the fixed as well as the same retreating ice mask for the 1wC and 0wC simulations.

p30, Fig. 14: Why do you show the grey area in this figure specifically, but not in many of the others? I would either show this in all timeseries or only in Fig. 3.

We had included the grey area in this figure specifically to highlight that the large variability in precipitation stems from the large variability during the randomly sampled period to prolong the simulations (2250-2300). We will remove it from the figure and will only mention this in the figure description:

Note that the large variability after 2300 results from the large variability in precipitation during the ~~The grey area indicates the~~ years that were randomly sampled (2250 to 2300) to prolong the IPSL-CM6A-LR forcing for MAR ~~after 2300 and highlights the variability in precipitation during this period~~.

p30, line 507: "This corresponds to a reduction of -42 % in total snowfall and -38.4 %" I think you can get rid of the minus signs, since a reduction already implies it is negative.

We agree that the minus sign is in fact redundant and will rephrase this:

This corresponds to a reduction of ~~-~~42 % in total snowfall and ~~-~~38.4 % in total precipitation over the ice sheet between 2300 and 2500, ~~that decrease~~ with further decreases reaching ~~towards -~~90 % and ~~-~~86 %, respectively, by 3000.

p33, line 531-537: Are there other studies that looked into the inland displacement of precipitation and the diminishing orographic barrier? I particularly find your results on the diminishing orographic barrier very interesting, and I would be interested in knowing whether this was found before.

We sincerely thank the reviewer for their question and interest. Thus far, we are not aware of other studies that have reported a decrease in total precipitation due to the diminishing orographic barrier effect of the GrIS on the continent-wide scale. The inland displacement of precipitation on the other hand has often been reported before. We should indeed have mentioned this.

Besides, beyond 2300, in the 2wC simulation the precipitation ~~(and associated cloudiness)~~ also continues to move further inland following the retreating ice sheet margin, as often reported before (Toniazzo et al., 2004; Ridley et al., 2005; Vizcaíno et al., 2010; Solgaard and Langen, 2012; Gregory et al., 2020; Andernach et al., 2025; Feenstra et al., 2025).

p33, line 539-540: I do not think that the fact that the melt-elevation feedback is constrained by the solar elevation angle is a new finding of your study. The same comment for the conclusion, p36, line 650-652. There, you also state that this is even the case under high-warming scenarios, but the solar elevation angle is independent of the warming scenario.

Of course, the solar elevation angle is independent of our applied warming scenario. What we mean however, is that even for this high-warming scenario, wherein the annual mean temperature rises by 15°C already by 2300, less than 30 to 40% of the (remaining) ice sheet experiences (more than) 150 runoff days. And that this is due to the low solar elevation angle, which is thus a physical constraint on the strength of the melt–elevation feedback. We will rephrase the respective paragraphs:

~~Though our simulations clearly indicate that even under high-warming scenarios the strength of the melt–elevation feedback is constrained by the low solar elevation angle during Arctic winter months, a~~Around 2500, though its strength remains constrained by the low solar elevation angle during the Arctic winter months, the melt–elevation ~~the~~ feedback intensifies.

Around 2500, the positive melt–elevation feedback intensifies. ~~, even though our results also reveal that the strength of this feedback is restricted by the low solar elevation angle during the winter months at high latitudes, even under high-warming scenarios. Around the same time, w~~We observe a decrease in (solid) orographic precipitation and the summer runoff expands more rapidly in both space and time once (more than) 40 % of the remaining ice *sheet area experiences runoff during at least 120 days, or a prolonged summer period.*

p33, line 544: associated to -> associated with
Ok.

p34, line 567-570: “not including the melt-elevation feedback” Are you referring to the zero-way or one-way simulation? Also, you find a larger underestimation than the three other studies. Is this because you have stronger warming by 2200 in your simulations? Or could this be attributed to the choice of ISM? If the warming is very different, you could consider comparing SLR contributions linked to a certain amount of warming instead of timing?
We are referring to the 0wC simulation and have now specified this. Our underestimation is indeed larger than for the other three studies because in our simulations the warming continues up to 2200 as opposed to these other studies, wherein the warming only continues up to the year 2100 with a constant climate after that time. Given the continued strong warming in our simulations after 2100, we consider this as the main reason for the difference rather than the ISM uncertainty. We will reformulate this part in the text for unambiguous interpretation and will leave out the reference to Edwards et al. (2014), since in hindsight not only the applied warming but also the different methodology makes it difficult to compare the numbers as is.
We find an underestimation of the sea level contribution of 10.41 % by 2150 or 14.41 % by 2200 when not including the melt–elevation feedback (i.e. 0wC simulation).~~,~~ ~~which~~This is somewhat higher than the ~~~10 %~~9.3 % by 2150 reported in Le clec’h et al. (2019b), and the 10.5 % by 2200 reported in Delhasse et al. (2024), since in these ~~these three other~~ studies ~~in which the~~ a stabilized climate was assumed to extend the simulations ~~did not continue to warm~~ beyond 2100~~up to 2200 (Edwards et al., 2014; Leclec’h et al., 2019b; Delhasse et al., 2024)~~.
The suggestion of linking the SLR to a certain amount of warming would be very interesting. However, this is not straightforward either, since the sea level contributions for the different experiments are not reported for or linked to different temperature forcings in the different studies, and neither are the forcing temperatures reported in a consistent fashion, over a comparable mask or at the same atmospheric level for example.

p34, line 574-575: I suggest rephrasing this. Since you do not downscale winds, it makes sense that your offline correction does not capture this feedback. I would rather say something about the fact that you need two-way coupling to include this effect.
Thank you for mentioning this. In a way this indeed makes sense. Yet, when annually recalculating local SMB–elevation gradients, to some extent one would expect to capture indirect effects on the melt or SMB during the downscaling. But this was shown not to be the case, already for the first part of the simulation. We thus suggest to rephrase our sentence such that it better reflects what we mean:
In addition, our results illustrate that the Franco et al. (2012) method for extrapolating the SMB ~~between the ice sheet and RCM grid~~ from the lower resolution RCM grid to the higher resolution ISM grid by means of locally derived SMB–elevation gradients does not fully capture indirect effects on the SMB, like the effect of changing winds, that act as a negative feedback in our 2wC simulation~~the negative wind feedback~~.

p34, line 585-586: How well can you resolve wind speeds at 30 km resolution? Is this resolution high enough to capture the katabatic and barrier winds realistically?
Yes, the 30 km resolution is high enough for this. As for example shown in Delhasse et al. (2020), winds are well resolved by ERA5 (~31 km) and ERA-Interim (~41 km) reanalysis products, with similar spatial resolutions as our MAR simulations. Furthermore, the katabatic and barrier winds were already resolved using a simplified 2D version of MAR by van den Broeke and Gallée (1996).

Delhasse, A., Kittel, C., Amory, C., Hofer, S., van As, D., S. Fausto, R., and Fettweis, X.: Brief communication: Evaluation of the near-surface climate in ERA5 over the Greenland Ice Sheet, The Cryosphere, 14, 957–965, https://doi.org/10.5194/tc-14-957-2020, 2020.

van den Broeke, M.R. and Gallée, H.: Observation and simulation of barrier winds at the western margin of the Greenland ice sheet. Q.J.R. Meteorol. Soc., 122, 1365–1383, https://doi.org/10.1002/qj.49712253407, 1996.

p35, line 593-598: This paragraph is a bit vague, especially the last sentence. I would consider removing this paragraph entirely and maybe moving the first sentence to the paragraph before.
Thank you for this suggestion. We will remove this paragraph and incorporate the first sentence with the previous paragraph.

p35, line 601-603: I would not consider the increasing difference between the MAR and GISM SMB when using a higher GISM resolution a big problem. I think the computational constraint justifies the choice for the 5 km resolution already well enough.
We agree that the computational constraint is the main reason for the 5 km GISM resolution and justifies it. However, based on our results it seems reasonable to assume that (with the applied or similar methods for extrapolation) the discrepancies between the original and extrapolated SMB will increase with increasing difference in model resolutions. It is thus an additional rationale for our and potential future coupled model set-ups that we would like to mention.

p35, line 609: remove “though”
Ok.

p35-36, line 620-626: I would consider moving this to line 606, since this paragraph discusses limitations of your ISM.
Thank you for this suggestion. We will move this paragraph upwards below the former paragraph that discusses the ISM limitations.

p36, line 622~~3~~: remove the comma here.
Ok.

p36, line 636-642: Are winds the only reason for this difference?
Assuming that the reviewer refers to the spatial differences in SMB, we feel like we have addressed this in detail in section 3.3, where we argue that despite the similar integrated ice mass loss between simulations, clear spatial differences in SMB can be observed, that we could indeed relate to the changing wind speeds (and the ensuing changes in surface energy fluxes).

Regarding the similar integrated ice mass loss, in section 3.4 we presented the lack of difference in the positive melt–surface albedo feedback between simulations as another cause for the small difference in integrated ice mass loss. We will add this to the paragraph, as we did not repeat it here:

We find that the ice sheet contribution by 2300 differs by only 46.9 cm s.l.e. or 2.36 % between the simulations with different coupling complexity. This small difference can in part be attributed to the similar bare ice cover during the melt season, and the ensuing lack of difference in the positive melt–albedo feedback between the simulations. Nevertheless, in addition, distinct spatial differences in SMB were observed between the 1wC and 2wC simulations, that largely compensate one another and thus lead to a similar integrated ice mass loss. ~~that~~ These SMB differences can be attributed to changing near-surface wind speeds that reduce ablation along the ice sheet margin in the 2wC simulation, and the fact that this negative feedback effect is not adequately represented by the offline extrapolation.

p40, line 741-743: Feenstra, et al. (2024) is not in discussion anymore: https://doi.org/10.5194/tc-19-2289-2025

Thank you for pointing this out, we will update the reference list accordingly.

References

Below, we have inserted the references that we will add to our reference list based on the updates above.

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