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.

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?

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.
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.
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.

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. 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, “long wave warming outweighing cooling associated to the reduced incoming short wave radiation" would be a too strong statement since you cannot separately compute and compare the longwave and shortwave radiative effects from your results.

Minor/technical comments

p1, line 9: "predicting" -> "projecting"

p2, line 56: remove "and winds", since you only discuss this in the next paragraph.

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".

p4, line 109: "using the higher-order approximation", what kind of approximation is this? Could you mention it briefly?

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?

p6, line 170-174: Can you briefly explain what these optimization and relaxation steps are?

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?

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.

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

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

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?

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

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.

p17, line 339-340: I do not see this sudden decrease in temperature in Fig. 7a.

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.

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).

p20, line 373: Please state whether you consider the MAR or GISM SMB and refer to Fig. 11.

p20, line 376-379: Is the bare ice albedo in MAR a fixed value?

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

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

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

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.

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

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.

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.

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.

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.

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.

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.

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.

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.

p33, line 544: associated to -> associated with

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?

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.

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?

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.

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.

p35, line 609: remove “though”

p35-36, line 620-626: I would consider moving this to line 606, since this paragraph discusses limitations of your ISM.

p36, line 623: remove the comma here.

p36, line 636-642: Are winds the only reason for this difference?

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

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.