Review of egusphere-2025-2465: “Positive feedbacks drive the Greenland ice sheet evolution in millennial-length MAR–GISM simulations under a high-end warming scenario” (Chloë Marie Paice et al.)

**Summary:**

Paice et al. present a suite of coupled ice-sheet–atmosphere simulations of the Greenland Ice Sheet (GrIS), spanning the period from the present to the year 3000. The regional climate model MAR is coupled to the ice-sheet model GISM. The simulations are initialized through a glacial–interglacial spin-up of the ice sheet model (ISM), followed by a data assimilation procedure to obtain a realistic present-day ice-sheet geometry. Three forward simulations with varying coupling degrees are then performed under a high-emission scenario: (i) a zero-way coupled run, in which MAR-derived surface mass balance (SMB) is applied directly to the ISM without accounting for evolving surface height; (ii) a one-way coupled run, in which the SMB–elevation feedback is parameterized; and (iii) a two-way coupled run, in which annual changes in ice-sheet geometry are passed from GISM to MAR. Results from these experiments are used to systematically assess key atmospheric feedback processes affecting ice-sheet evolution, including the SMB–elevation feedback, wind, cloud, and albedo effects. The authors find that positive feedbacks (e.g. the SMB-elevation feedback) dominate on the millennial timescale and substantially enhance future ice mass loss.

This study represents the first millennial-scale two-way coupling of an ISM with a regional climate model (RCM), making the manuscript both novel and highly relevant for advancing our understanding of Greenland’s long-term atmospheric feedback processes.

**General Assessment:**

This manuscript is highly relevant to the field of ice-sheet modelling and makes an original contribution by advancing our understanding of long-term atmosphere–ice-sheet interactions over Greenland. The paper addresses important scientific questions that fall well within the scope of *The Cryosphere*. The authors present novel methods and results, in particular the two-way coupling of an ice-sheet model with a regional climate model over millennial timescales, which, to my knowledge, has not previously been demonstrated.

The title accurately reflects the content of the paper. The scientific methods and assumptions are valid and are generally well described in the methods section, though I suggest some refinements and clarifications to improve clarity, as well as the addition of

visual aids. The conclusions are substantial and well supported by the presented results. I particularly appreciated the systematic approach to disentangling and presenting the various feedback processes that contribute to future mass change of the ice sheet. The manuscript is well structured and generally clearly written, though in some instances sentences are awkwardly formulated and the logical connections within paragraphs are not always transparent.

I recommend publication after minor revisions, mainly to improve language, grammar, and precision, and to enhance clarity in the methods section.

**General Comments:**

1. Although the methods are generally well described, some details remain unclear. I recommend providing additional precision and detail where necessary (see specific comments). In addition, a visual aid would be valuable to guide the reader through the initialization steps and the interactions between MAR and the ISM (see also comment on Sect. 2.3).

2. While the manuscript focuses on atmosphere–ice interactions, I suggest explicitly addressing the treatment (or non-treatment) of the ice–ocean boundary in both the methods and discussion sections. In particular: How are outlet glaciers and their potential retreat represented in the simulations? If dynamic retreat is not included, how do the authors justify this omission, and how might they expect dynamic retreat to interact with SMB-driven retreat? What limitations does this introduce to the interpretation of results? I believe this point is important, since dynamic retreat of outlet glaciers contributes substantially to present-day mass loss of the GrIS and is expected to remain significant in the future, at least as long as the ice sheet is in contact with the ocean.

3. The manuscript is generally well written and well structured, but some sentences are awkwardly phrased, and the logical connections are not always clear. I believe a careful revision of the language, sentence structure, and causal links within sentences and paragraphs would improve clarity and linguistic precision.

**Specific Comments:**

**l. 22:** To avoid misinterpretation, I suggest to already clearly state in the abstract that glacial isostatic adjustment is not considered, when mentioning numbers of sea-level contribution.

**l. 33:** The expression "*… the major remaining uncertainties…"* is a bit unclear. Please specify what "*remaining*" is referring to.

**l. 43:** Explain why the air temperature increases (changes in air density, adiabatic lapse rate).

**l. 45:** I suggest adding a sentence or two explaining why it is not straightforward to include the SMB-elevation effect in ISMs (see also comment l. 72).

**l. 52:** I suggest replacing "*landward*" with "*inland*" and "*contributes*" with "*increases*".

**l. 58:** I suggest to explicitly explain how clouds affect the meltwater refreezing capacity (via surface energy balance/longwave radiation).

**l. 72:** You could mention why it is not straightforward to represent ice-atmosphere interaction in standalone ISMs and why coupling is needed (see also comment l. 45).

**Sect. 2.3:** If I understand correctly, during the initialization procedure you apply 1961-1990 SMB, but you also assume the ice sheet to resemble present-day conditions at the start of your forward simulations. How do you treat the period between 1991 and present-day? Is this period explicitly simulated? If so, which forcing is used? If not, how do you handle this gap and the observed mass-loss trend? Please clarify.

**l. 96:** Mention that initialization should ideally reflect not only the present-day state but also current mass loss trends in response to recent 'historical' forcing (see also comment l. 118). See for example Rahlves et al. (2025) for including historical trends of ice mass loss in projections.

**l. 99:** To me it is not clear what you mean by "*additional assumptions*". Please clarify.

**l. 110:** Consider explicitly showing the sliding law equation.

**l. 111:** Please specify what is meant by "*geometric input*" (bedrock topography and/or ice sheet geometry).

**l. 116:** Define "*acceptable*" here. How is the acceptable discrepancy level determined, and what happens if it is exceeded?

**l. 118:** You describe the system as initialized *"into an equilibrium state resembling present-day observations."* In reality, the ice sheet is not in equilibrium at present. How do you account for this (see also comment l. 96)?

**Sect. 2.3:** Since the initialization is a complex but central part of your setup, I recommend a schematic figure/flowchart showing the main steps (spin-up, data assimilation, coupled initialization), inputs/forcings, parameters adjusted vs. fixed, and the MAR–GISM information exchange (including frequency).

**l. 163:** Although you mention here that the isostatic bedrock adjustment is disabled after the spin up, I suggest reiterating this in Sect. 2.5 when describing the future simulations.

**l. 165:** Add a brief explanation of how the positive degree-day approach works.

**Sect. 2.3.2**: Reiterate here that GISM is forced with MAR SMB during data assimilation (currently mentioned earlier).

**l. 184**: Regarding peripheral glaciers: if data assimilation is not applied there, does this mean they are initially too large? How are they treated in sea-level contribution calculations? Do they make a difference? Please clarify.

**l. 190:** The phrase *"unvarying parameters"* is misleading. Suggest rephrasing as: *"Holding these parameters fixed is justified over short-term periods …"* Also specify what "short-term" vs. "long-term" means, and where your simulation timeframe fits.

**l. 195:** Be more precise: *"… computed on the ice-sheet topography as simulated by GISM."*

**Sect. 2.5:** What is the first year of the forward simulation (after the initialization procedure is complete)? I suppose it is 1991, but I think it would be good to mention it explicitly.

**l. 216**: Does *"similar to …"* refer only to the zero-way run, or to all three? If the latter, consider: *"Similar to …, we consider three coupling types …"*

**l. 234:** Fig. 2 shows differences in ice thickness, not the initialized topography itself. Please rephrase.

**l. 241 ff.:** I find this sentence difficult to understand. I suggest rewriting for clarity.

**Sect. 2:** In your methods section I am missing a description of how you are treating the ice-ocean boundary. Please include a brief explanation.

**l. 252:** *"…since only SMB was extrapolated…"* What about runoff?

**l. 256:** Clarify how sea-level contribution is calculated and what the reference period is.

**l. 264:** Specify *"fourth iteration"* of what.

**l. 305:** Instead of *"the first part of the simulation,"* write *"During the first X years …"*

**l. 310:** I find the expression "compensation of differences" unclear. Differences of what? Suggest rephrasing to *"spatial compensation within SMB fields."* Also, I think the expressions "over- and underestimation" only work if there is a reference. Consider rewriting to "overestimated compared to…" and "underestimated compared to…".

**l. 335:** Refer to a figure if this is illustrated.

**l. 336:** Clarify causality: e.g. *"Changes in wind speed impact the surface energy balance, reducing runoff and lowering SMB (Fig. 6)."*

**l. 375:** The causal link in this sentence is unclear. Please clarify how the density of the upper ice layers is used as an indicator of snowfall melt. In addition, it would be helpful to specify the actual density values of these layers to support the statement.

**l. 403:** Remind the reader why low-lying margins retreat less rapidly in the 1wC simulation.

**l. 411:** Insert "loss" after mean surface elevation and change "stronger" to "strong".

**l. 413:** Specify the year of *"At the time of this intensification …"*

**Sect. 3.6:** Consider merging 3.6.1 and 3.6.2 instead of using sub-subsections.

**l. 455:** Clarify that *"increase"* is relative to the 1990–2019 reference period.

**l. 466:** Explain why this is specifically a result of the melt–elevation feedback.

**l. 467:** I could not fully follow how this is shown in the text. Please clarify.

**l. 481:** Omit "important".

**l. 489:** Rephrase: *"… and the increase in bare ice exposure."*

**l. 491:** Add: *"… and the associated increase in near-surface air temperature."*

**l. 503:** Replace "snowfall" with "precipitation".

**l. 504:** The two clauses are not logically connected. Consider rewriting to: *"However, as Fig. 14 demonstrates, not all snowfall is transformed into rainfall. Instead, decreasing snowfall also reduces total precipitation."*

**l. 526:** Rephrase: *"Similar to Delhasse et al. (2024), during the first three centuries …"*

**l. 528:** Rephrase: *"This results in a similar ice-sheet contribution to sea-level rise across our three coupled simulations up to 2300."*

**l. 542:** Add "per year" to "120 runoff days".

**l. 557:** When comparing to Aschwanden (2019) and Gregory (2020), stress that setups differ (coupled vs. uncoupled, forcing strategies). This distinction should be made clear throughout the discussion.

**l. 569:** The phrasing *"… in which the climate did not continue to warm up to 2200 …"* is not entirely clear. It would help to clarify whether the intended meaning is that previous studies assumed climate stabilization before 2200, or simply that warming was not extended in their scenarios. Consider rephrasing for precision.

**l. 576:** The sentence *"Though their applied horizontal model resolutions …"* is difficult to follow. I suggest rephrasing or splitting into shorter sentences to make the causality clearer.

**Sect. 4.3:** Briefly discuss missing representation of ice–ocean interactions as a limitation. Outlet-glacier dynamics currently contribute significantly to GrIS mass loss and will likely remain important over several centuries. Also note feedbacks between SMB and dynamic processes (e.g. thinning reduces flux vs. ocean warming accelerates retreat). Even if not in the scope of your study, this deserves a short mention.

**l. 592:** The argumentative line of this paragraph is not clear to me. Can you rewrite to make your thought clearer?

**l. 615:** Consider briefly mentioning expected effects of GrIS on large-scale circulation, and cite relevant studies (e.g. Haubner et al., 2025).

**l. 644:** Since you start a new paragraph here, I suggest opening with *"Beyond the 2300 timescale …"*.

**Figures:**

**Fig. 2:** Omit the title (information is already in the caption) and add a description to the color bar (e.g. *"difference in ice thickness (m)"*). If possible, adjust the color scheme for detached peripheral glaciers to avoid overlap with the palette, where white indicates zero difference.

**Fig. 4:** Consider using a color bar for ice thickness (grey tones) as well as for the bedrock topography.

**Fig. 5:** Omit the title (repeated in the caption) and label all subpanels (a–e). In the upper panels, outline the selected regions in the same colors used for the corresponding SMB values in the lower panels, to improve readability. Add descriptions to the color bars (e.g. *"difference in SMB"*). In the caption, clarify whether the SMB evolution refers to area-

mean SMB or integrated SMB, and specify whether *"mean SMB"* on the y-axis denotes areal mean or annual mean.

**Fig. 6 & 7:** Consider reducing the number of points along the transects to make the plots less cluttered. As a matter of style, I suggest placing subpanel labels outside the plots.

**Fig. 9, 10, 11, 12:** Consider merging these into one figure, since they share the same timescale.

**Fig. 13:** Remove the title; this information is already given in the caption.

**Fig. 15:** Add descriptions to the color bars (e.g. *"total precipitation," "precipitation change,"* and *"rain fraction (increase)"*).

**Technical Corrections:**

**l. 56:** Include commas: *"It has, for example, been reported…"*

**l. 130:** Change to: *"The main difference to …"*

**l. 197:** Avoid *"… until the differences … no longer change …"*. Instead, I suggest rephrasing as: *"until the differences become small/insignificant/approach zero"*

**l. 230:** (and other occasions): Replace *"on Fig. 2"* with *"in Fig. 2"*

**l. 427:** Replace *"a clear link with"* with *"a clear link to"*.

**l. 722:** Add missing *"d"* in *"Land surface induced regional climate change…"*.

**References:**

Rahlves, C., Goelzer, H., Born, A., and Langebroek, P. M.: Historically consistent mass loss projections of the Greenland ice sheet, The Cryosphere, 19, 1205-1220, https://doi.org/10.5194/tc-19-1205-2025, 2025.

Haubner, K., Goelzer, H., and Born, A.: Limited global effect of climate-Greenland ice sheet coupling in NorESM2 under a high-emission scenario, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-3785, 2025.