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.

We would like to thank the reviewer for taking the time to evaluate our work and for providing helpful and constructive feedback. We have carefully addressed all comments (in blue, with track changes below where relevant) and believe the suggested revisions will significantly improve the quality of the manuscript.

General Comments:

We thank the reviewer for their thorough assessment and general comments. Given that (especially) the first and third general comments are accompanied by or further divided into specific comments, we have carefully addressed these in the next section.

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.
   Thank you for this interesting question, which raises an important point.
   The outlet glaciers and their potential or dynamic retreat are not explicitly considered in our presented simulations. We acknowledge that dynamic outlet glacier retreat is an important part of the Greenland ice sheet contribution at present and in the coming centuries as long as the ice sheet is in contact with the ocean. Yet, given our focus on long-term ice sheet – atmosphere interactions and feedbacks (rather than specific sea level contribution in the coming centuries), the ice – ocean interactions are not of interest for the current study.
   We agree to mention and address this in the methods and discussion sections to improve them (see specific comments below).

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.

Thank you for this suggestion. Since we have addressed this matter both in the Methodology and Discussion sections and have specified in the latter why we think the glacial isostatic adjustment will not substantially impact the rapid ice sheet decline and sea level contribution by the end of the millennium, we believe that this is not a major issue to be addressed in the abstract specifically. Nevertheless, we agree with the suggestion regarding l. 163 to reiterate the disabling of the glacial isostatic adjustment in Section 2.5.

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

"Remaining" uncertainties refers to the uncertainties that persist or are unknown up to present. We feel that we have explained these in the following two sentences, i.e. that it remains difficult to quantify the effects of ice sheet-climate interactions, or identify the positive or negative nature of several feedback effects on ice mass loss.

This can mainly be attributed to ~~the~~ major remaining uncertainties regarding ice sheet–climate interactions and feedback mechanisms, that will determine the ice sheet's long-term mass loss. Although many of these interactions and feedbacks have been identified and characterized for some time, quantifying their effects remains challenging (Fyke et al., 2018). Moreover, for some of them, it is still unclear whether they function as positive feedbacks (i.e., amplifying effects) or negative feedbacks (i.e., dampening effects) in the context of ice mass loss.

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

We will include the following specification:

When the ice sheet surface melts, its surface elevation is lowered and the air temperature increases following the adiabatic lapse rate, thereby inducing even more melt.

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).

Thank you for this suggestion. We will mention this more explicitly here (and also around line 72, see below).

However, it is not straightforward to represent this melt–elevation feedback in ISMs, since ~~In addition,~~ the changing topography of the GrIS can in turn influence the (local) atmospheric circulation and induce changes in the precipitation pattern.

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

We will update "landward" with "inland", but prefer to keep the formulation "contribute to accumulation" to avoid confusion with the remaining part of the sentence:

In many cases, the impact is more nuanced and varies regionally, since the precipitation is advected further ~~landward~~ inland where it contributes to accumulation, but decreases near the margin (Fyke et al., 2018).

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

Thank you for this suggestion. We will update the text here, to explain this more explicitly:

Other ice sheet–atmosphere interactions include changes in cloudiness~~, and winds. It has for example been reported that clouds can enhance meltwater runoff over the GrIS by one-third compared to clear skies, through reduction of the meltwater refreezing capacity (Van Tricht et al., 2016). However,~~ as clouds can alter the surface energy balance and surface melt over the ice sheet in several ways, such as through alteration of incoming ~~short wave~~shortwave radiation, ~~reduction of long wave cooling, or~~ increased ~~long wave~~longwave warming, or reduction of longwave cooling. It has been reported, for example, that clouds can reduce melt (in the ablation zone with low albedo) by blocking the incoming solar radiation, which is the main driver of melt here (Hofer et al., 2017). Vice versa however, it has been shown that clouds can enhance the meltwater runoff over the GrIS by one-third compared to clear skies, by reducing surface radiative cooling (mainly at night) and impeding the meltwater to refreeze (Van Tricht et al., 2016).

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).

Thank you for this suggestion. We agree to mention this more explicitly:

Consequently, RCMs are needed to ~~To better~~ represent such ice sheet–atmosphere interactions, or changes in (local) atmospheric circulation in response to the evolving ice sheet slopes and (local) topography at high resolution (Fettweis et al., 2020). Meanwhile, an ice sheet model (ISM) is needed to represent the ice dynamics and evolving ice sheet topography that are not considered by the RCM. As a result, efforts are currently emerging to couple ~~ice sheet models (ISMs)~~ ISMs to RCMs.~~, which are best at representing these interactions at high resolution (Fettweis et al., 2020). Nevertheless,~~ Yet, to date only two such coupled ISM–RCM simulations have been performed, on the centennial timescale (~~Leclec'h~~Le clec'h et al., 2019b; Delhasse et al., 2024).

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.

Thank you for highlighting the confusion regarding the use of the term "present-day". We used it several times to refer to the (observed) state of the ice sheet at the start of our simulations in 1990, but agree that we could have been more clear. We will be more specific throughout the text:

Besides, the initialized system should represent the ~~present-day~~observed state as closely as possible for an accurate representation of ice sheet–atmosphere interactions and ensuing estimates of the GrIS contribution to sea level.

[…] into an equilibrium state resembling ~~present-day~~ recent observations, as represented by the BedMachine v3 dataset (Morlighem et al., 2017).

For the ISM initialization, we combine a glacial–interglacial spin-up with a data assimilation technique, to capture the ice sheet response to past climatic conditions and represent its ~~present-day~~recently observed geometry as closely as possible.

However, as is often the case, the ~~present-day~~ ice sheet geometry obtained from the glacial–interglacial spin-up is slightly oversized and too thick near the margin […]

[…] to accurately represent the ~~present-day~~ observed ice thickness at the start […]

[…] and once run continuously for the period 1950 to 1990 on the ~~present-day~~recently observed topography (Morlighem et al., 2017) […]

As can be seen ~~o~~in Fig. 2, the differences between the initialized and observed ~~present-day~~ topography (Morlighem et al., 2017)[…]

Therefore, considering the exploratory nature of this study, the timescale and the accurately represented ~~present-day~~observed ice sheet geometry with low remaining model drift […]

3.1 ~~Modelled present-day~~ Initialized ice sheet topography

We explicitly simulate the period between 1990 and present-day by directly applying the IPSL-CM6A-LR forcing to MAR from 1990 onwards, the start of our coupled simulations.

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.

For consistency and to avoid model drift at the start of our model simulations, or at the transition between a historical period and the present-day, our coupled MAR-GISM model is initialized using IPSL-CM6A-LR forcing for the period 1960-1990 during which the ice sheet is assumed to have been in a steady-state. The coupled simulations thus start from this steady state in 1990 and are further forced by IPSL-CM6A-LR output. As such, a historical forcing does not apply here. Given our focus on ice sheet – atmosphere interactions and the millennial timescale of the simulations, we believe this is not crucial here. However, we agree to cite Rahlves et al. (2025) and mention the historical forcing nevertheless (see next comment regarding line 99).

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

What we meant is that since our simulations start from a steady-state in 1990, we do not handle the period between 1990 and 2025 separately but directly apply the IPSL-CM6A-LR forcing to MAR for this period (see also previous comment). We will be more specific:

This way we do not need any ~~additional~~ assumptions regarding the state of the ice sheet between 1990 and present, or a historical run preceding the future simulations as in Rahlves et al. (2025)~~in the 2010's or 2020's~~.

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

Thank you for this suggestion. Given the substantial length of the manuscript and the methodology section in particular, we prefer to refrain from adding the sliding law equation here, as it will not contribute to a better understanding of the presented research and results. For interested readers, we have referred to the work describing the specific higher-order model version and sliding law in detail.

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

Thank you for mentioning that this was not explicitly stated. We will specify this:

The geometric input for the model consists of the BedMachine v3 dataset for bedrock topography and ice sheet surface elevation or ice thickness (Morlighem et al., 2017),…

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

What we mean is that the resolutions should not be too different, and it would not be ideal to run GISM on a much higher resolution. We were referring to common sense rather than a determined discrepancy level. We will explain this more clearly:

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~~ recent observations, as represented by the BedMachine v3 dataset (Morlighem et al., 2017).

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)?

We will correct this (see also answers to comments regarding Sect. 2.3 above, and lines 96 and 99):

[…] to facilitate the efficient initialization of the ice sheet and coupled model into an equilibrium state resembling~~-~~ ~~present-day~~ recent observations, as represented by the BedMachine v3 dataset (Morlighem et al., 2017).

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).

Thank you for this suggestion. We agree that this would be useful, but we do not want to overload the current paper and instead plan to go into more detail in another manuscript in which we will focus more on the ice sheet response and shorter-term sea level contribution, for which this procedure and the initialized ice sheet state are (even) more relevant.

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.

We will add the following sentence to Sect. 2.5.1:

The ice sheet topography is annually updated in GISM for all three coupled simulations, but the glacial isostatic bedrock adjustment is not considered (Sect. 2.3.1).

l. 165: Add a brief explanation of how the positive degree-day approach works.
We will add the following explanation:
With this approach, the amount of (energy available for) melt is determined based on the sum of mean daily temperatures above 0°C, i.e. the number of positive degree days. Accumulation is considered as the fraction of precipitation falling below a certain threshold, denoted as the snow fraction limit, here 1°C.

Sect. 2.3.2: Reiterate here that GISM is forced with MAR SMB during data assimilation (currently mentioned earlier).
We suggest adding the following sentence:
Throughout both steps, GISM is forced by the MAR SMB for our reference period (1961-1990).

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.
Instead of performing the data assimilation for the peripheral glaciers, we adopt their observed ice thickness and apply SMB anomalies. We will better explain this and move it to the end of the paragraph for clarity:
For the detached peripheral glaciers surrounding the ice sheet, identified based on the PROMICE aerophotogrammetric map of Greenland ice masses (Citterio and Ahlstrøm, 2013), the data assimilation was not performed. For these areas the observed ice thickness was adopted and SMB anomalies were applied throughout the coupled simulations.

In section 3.2 it is mentioned that their contribution (total of 2.58 cm s.l.e.) is included in the reported sea level values, but we will repeat this when clarifying how the sea level contribution was calculated (see corresponding comment line 256).

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.
Thank you for pointing out that this term can be misleading. To keep it short and given the explanation in the previous sentence, we suggest rephrasing it to "fixed" parameters:
After the data assimilation, in absence of a better approach, the optimized basal sliding coefficient and enhancement factor are held constant throughout the coupled simulations, as is the geothermal heat flux. In general, these ~~unvarying~~ fixed parameters are justifiable for short-term projections but inevitably become more contestable over the course of time (e.g. Goelzer et al., 2013; ~~Leclec'h~~Le clec'h et al., 2019a, 2019b).
The 'optimal' timing of such fixed parameters is already addressed in a separate paragraph in the discussion (Section 4.3), so we prefer not to repeat this here.
In addition, it is unlikely that the optimized two-dimensional fields for the BSC and EF in the ISM remain valid over a period exceeding several hundred years. Over such timescales their values will likely be impacted by the changing overlying ice thickness and basal hydrological conditions (Leclec'h et al., 2019a, 2019b). On the other hand, the fixed ice temperature is not expected to substantially affect the presented results, as the rate of ice melt in our simulations exceeds the rate at which ice temperature is altered through advection or the propagation of atmospheric

temperature perturbations into the ice.

l. 195: Be more precise: *"… computed on the ice-sheet topography as simulated by GISM."*
Thank you, we will implement this suggestion.

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.
The first year of the coupled simulations is 1990. We will mention it explicitly.
The first year of these simulations is 1990.

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 …"*
This could indeed be interpreted in two ways, so we agree with the suggestion for clarification:
As illustrated in Fig. 1, similar to Le clec'h et al. (2019b) and Delhasse et al. (2024), three different coupling types between the ice sheet and the RCM were considered: a so-called two-way (2wC), one-way (1wC), and a zero-way coupled (0wC) simulation~~, similar to Leclec'h et al. (2019b) and Delhasse et al. (2024)~~.

l. 234: Fig. 2 shows differences in ice thickness, not the initialized topography itself. Please rephrase.
Thank you for noticing this. We will remove the reference to Fig. 2 here.

l. 241 ff.: I find this sentence difficult to understand. I suggest rewriting for clarity.
We thank the reviewer for pointing out that these sentences are difficult to understand and suggest rephrasing the paragraph as follows:
For all ~~four~~ three coupled simulations, after 2300 the GCM forcing for MAR is held constant until the year 3000 by randomly sampling the yearly IPSL-CM6A-LR output ~~forcing for MAR~~ from the period 2250 to 2300, during which the forcing temperature stabilizes (Fig. 3). In any case, as stressed by Delhasse et al. (2024), even when the temperature stabilizes ~~As stressed by Delhasse et al. (2024),~~ it is important to repeat the GCM forcing fields for MAR to prolong ~~for~~ the 1wC and 0wC simulations, rather than repeating the ~~direct~~ MAR output (SMB and runoff) ~~to prolong the coupled simulations~~, since the meltwater retention capacity~~,~~ and thereby the ablation area and SMB still require several decades to stabilize once warming stops.

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.
We will mention that the ice – ocean interactions are not regarded in our coupled ice sheet – atmosphere simulations:
Lastly, given the ice sheet model resolution and the research focus on long-term ice sheet – atmosphere interactions, dynamic outlet glacier retreat is not explicitly considered here.

l. 252: *"…since only SMB was extrapolated..."* What about runoff?
Thank you for noticing that indeed also the MAR runoff was extrapolated to the GISM topography throughout the coupled simulations. We will add this to the sentence here.

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

We will add the following clarification at the beginning of section 2.6:

For the conversion of ice volume change to sea level contribution since the start of our simulations in 1990, we consider only the ice above floatation (Goelzer et al., 2020a), include volume changes from the peripheral glaciers and ice caps, assume an ice density of 916.7 kg m^{-3} and apply a conversion factor of 361.8 Gt per millimetre of barystatic sea level rise (Morlighem et al., 2017).

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

We could indeed have been more specific here and suggest the following clarification:

The ice sheet topography from the fourth MAR-GISM initialization iteration is used as the initial topography for the coupled simulations starting in 1990.

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

We will rephrase this.

During the first ~~part of~~three centuries, the ice sheet – climate evolution in the simulations is dominated by the strong warming predicted by IPSL-CM6A-LR under the SSP5-8.5 scenario (Fig. 3) ~~dominates the ice sheet – climate evolution~~.

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…".

Thank you for the suggestion, we will add some details:

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.).

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

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.

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

Thank you for this suggestion. Yet, since the figure and focus concern the SMB-elevation gradient rather than SMB in itself, we favor a small adjustment:

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, resulting ~~and result~~ in lowered runoff and a reduced or even inversed SMB–elevation gradient in the ice-marginal zone in the 2wC simulation.

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.

Thank you for indicating that this was not clear. The density of the upper snow/ice layers indicates whether the surface is covered by snow, firn or ice. We will be more specific:

Another reason for the very similar ice sheet contribution to sea level by 2300, is the similar SMB evolution for all simulations up to 22~~3~~00 on both the MAR and GISM grids (Fig. 9). Besides, as Figure 10~~8~~ shows, by that time the ablation area already covers 100 % of the ice sheet area ~~by 2200~~ in all simulations. This is also reflected in the density of the upper snow/ice layers up to 10 m depth in MAR that exceeds 910 kg m^{-3} by 2200, ~~which~~ indicating~~e~~ that ~~by that time~~ most of the snowfall melts before densifying to firn in all simulations (not shown here).

l. 403: Remind the reader why low-lying margins retreat less rapidly in the 1wC simulation.
Thank you for addressing the confusion here. This was meant to be a mere observation to accompany the figure, rather than a process of which the reader should be reminded. We will rephrase the sentence to make this clearer.
~~Note that b~~Between 2250 and 2650 the mean surface elevation is lower for the 1wC simulation compared to the 2wC simulation, ~~since~~ because the low-lying ice margin retreats less rapidly in this simulation after 2250. The same applies to the 0wC simulation between 2150 and 2250, yet after this time the higher parts of the ice sheet dwindle less rapidly than in the other two simulations.

l. 411: Insert "loss" after mean surface elevation and change "stronger" to "strong".
We will apply these changes.

l. 413: Specify the year of *"At the time of this intensification …"*
Thank you for requesting this specification, we will clarify this:
At the time of this intensification, around 2500, …

Sect. 3.6: Consider merging 3.6.1 and 3.6.2 instead of using sub-subsections.
Thank you for this suggestion. However, these subsections describe different processes in the climate system, namely changes in clouds (3.6.1) and precipitation (3.6.2). The subsections and their titles are meant to indicate the observed processes and differentiate between them, so we prefer to keep the sections and subtitles as is.

l. 455: Clarify that *"increase"* is relative to the 1990–2019 reference period.
We will add this clarification at the end of the sentence:
By 2300 the increase relative to the 1990-2019 reference period in both solid and liquid clouds together is 73 % higher and the LWD increase is 5.8 % higher for the 2wC simulation than for the 1wC and 0wC simulations, compared over the same retreating ice mask (Fig. 14~~3~~).

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.
We suggest the following rephrasing to clarify both sentences in the text:
After 2300, regardless of the constant climate forcing, these trends continue in the 2wC simulation as a result of the thickening atmospheric column following the melt–elevation feedback.~~,~~ ~~and b~~By 3000 the amount of liquid and solid clouds increase by +529 % and +85 %, respectively, compared to the start of the simulations. Though the increments after 2300 ~~changes due to the melt–elevation feedback~~ are thus smaller than those induced by the climate forcing until 2300, they are still substantial.

l. 481: Omit "important".
We will omit it.

l. 489: Rephrase: *"… and the increase in bare ice exposure."*
Thank you for the observant correction, we will rephrase it as suggested.

l. 491: Add: *"… and the associated increase in near-surface air temperature."*
We agree with the reviewer that as a result of the decreasing surface elevation, also the near-surface air temperature will increase. However, we prefer not to add the suggestion, since this might cause confusion, given that the GCM forcing temperature remains constant after 2300 throughout the prolonged simulations, in all simulations.

l. 503: Replace "snowfall" with "precipitation".
We will replace this.

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."*
Thank you for the suggestion. To our feeling however, the original sentence in essence conveys the same meaning as the proposed revision: namely the incomplete transformation of snowfall into rainfall and the consequent reduction in total precipitation. We will consider splitting the sentences for clarity:
However, as Fig. 154 demonstrates, over time not all snowfall is persistently transformed into rainfall. Instead,, as the decrease in snowfall also leads to a decrease reduction in total precipitation.

l. 526: Rephrase: *"Similar to Delhasse et al. (2024), during the first three centuries …"*
As the simulations by Delhasse et al. (2024) only span two centuries we suggest the following reformulation to improve the textual cohesion:
During the first three centuries At the beginning of our simulations, similar to Delhasse et al. (2024), during the first three centuries, the changes in the near surface wind speed have a negative feedback effect on ablation at the ice sheet margins, an effect that was also observed by Delhasse et al. (2024).

l. 528: Rephrase: *"This results in a similar ice-sheet contribution to sea-level rise across our three coupled simulations up to 2300."*
Thank you for improving the readability with this suggestion, we will implement it.

l. 542: Add "per year" to "120 runoff days".
This paragraph will be rephrased according to a suggestion by Reviewer 1, yet we will update this for other instances throughout the text.

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.
We will rewrite the sentence to make this more clear:
Similar to earlier findings for a high-warming climate forcing (Aschwanden et al., 2019; Gregory et al., 2020), iIn our two-way coupled simulation, the ice sheetGrIS has almost entirely disappeared within the next millennium. This is Ssimilar to earlier findings for a high-warming climate forcing though with a different experimental set-up and without full coupling, using a general circulation

model (Aschwanden et al., 2019; (Gregory et al., 2020) or corrected climatologies from an RCM (Aschwanden et al., 2019).,

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.

Thank you for indicating that this was not entirely clear. We will rephrase the sentence to highlight that in the other studies the simulations were extended by assuming a stabilized climate after 2100.

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)., whichThis 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 2100up to 2200 (Edwards et al., 2014; Leclec'h et al., 2019b; Delhasse et al., 2024).

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.

We appreciate the suggestion and will split the sentences for clarity:

This is consistent with the observations by Delhasse et al. (2024) for their MAR-PISM simulations with different large-scale forcing (CESM2). Though their applied On the one hand their horizontal model resolutions are similar to ours, namely 25 km for MAR and 4.5 km for PISM, and it such that it remains unclear to what extent these resolutions affect the strength of the observed negative wind feedback. Yet, on the other hand, the occurrence of the feedback and its poor reproduction by the offline extrapolation can thus be said to be independent of the coupled ISM and large-scale forcing.

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.

Thank you for pointing this out. We will expand the discussion accordingly and add the following paragraph to Sect. 4.3:

Although ice – ocean interactions are not the focus here, it should be noted that ice discharge at outlet glacier fronts is expected to accelerate due to warming ocean temperatures and increased surface runoff (Slater et al., 2019), at least as long as the ice sheet remains in contact with the ocean, which is up to the 2340s in our 2wC simulation. Since dynamic outlet glacier retreat was not included here, the (rate of) ice mass loss in our coupled simulations might thus be somewhat too low during the first centuries. On the other hand, part of the ice at the ocean boundary might be removed by SMB-driven surface melting before it reaches the calving front, such that the present-day observed discharge rates cannot merely be extrapolated over time (Fürst et al., 2015) and the impact is thus likely more limited than such extrapolations would suggest.

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

We will remove this paragraph and incorporate the first sentence with the previous paragraph:

The applied 30 km resolution for MAR is still relatively coarse, as for example the 30 – 60 km ice-marginal zone wherein the near-surface wind speed changes are observed spans only two MAR grid cells. However, even at this resolution it is clear from the presented simulations that the location, type and amount of precipitation are very strongly topographically controlled, highlighting the need for an RCM to accurately represent local atmospheric dynamics. In fact, the most accurate way to represent all ice sheet–atmosphere interactions would be to run both the ice sheet and RCM at the same horizontal resolution. However, Besides, as with all modelling research, the trade-off between the required computational resources, and the spatial as well as temporal resolution of the model (output) is inevitable. Running the RCM at the same resolution as the ISM thus , this currently remains unreasonable in terms of computational resources for millennial-scale simulations, like the ones presented here.

Besides, as with all modelling research, the trade-off between the required computational resources, and the spatial as well as temporal resolution of the model (output) is inevitable. In this case, we were only able to identify an (accelerated) expansion of the area subjected to at least 120 runoff days, and the solar elevation angle as a limiting factor for further intensification of the melt–elevation feedback thanks to the daily resolution of the MAR output. Consequently, it can be argued that our attempt to balance these elements was fruitful.

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

We agree to include more references, like the one suggested by the reviewer. However, given the already substantial length of the manuscript and the fact that findings related to large-scale atmospheric and oceanic changes following the GrIS decline vary considerably between studies (depending for example on model coupling strategies, model types and resolutions, surface properties,…) we prefer to refrain from discussing these potential effects.

MeanwhileLastly, our coupled model set-up does not represent the impact of the GrIS decline on the large-scale atmospheric and ocean circulation over the northern hemisphere (e.g. Davini et al., 2015; Madsen et al., 2022; Andernach et al., 2025; Haubner et al., 2025) but this is far beyond the scope of the study.

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

We agree and will update this.

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.

Thank you. We will apply these changes to improve the figure.

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

Ok, we will show a colour bar for the surface elevation (grey tones) and 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.

Thank you, we will update the figure and caption to improve its readability according to these suggestions.

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.

Thank you for these suggestions.

We choose not to reduce the number of points along the transects, as they show the continuity of the changes taking place at the ice sheet margin, both through space and time. Reducing the number of points would interfere with this continuity, seemingly minimize the robustness of the findings, and/or further complicate the interpretation of the plots.

We have considered several label placings in- and outside the plots, yet to reduce the amount of whitespace and maximize the subplot sizes the current label positions at the top inside the graph proved to be best.

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

Thank you for this suggestion. However, given that the figure order will slightly change according to a suggestion by Reviewer 1 and for reasons of readability we prefer to keep the figures separately.

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

Thank you, we will remove the title.

Fig. 15: Add descriptions to the color bars (e.g. *"total precipitation," "precipitation change,"* and *"rain fraction (increase)"*).
We will move the titles to the colour bar descriptions, as suggested.

Technical Corrections:

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

l. 130: Change to: *"The main difference to …"*
We will rephrase the sentence.
Compared to ~~The main differences with~~ version 3.11 described in Kittel et al. (2021), the main differences include corrections…

l. 197: Avoid *"… until the differences … no longer change …".* Instead, I suggest rephrasing as: *"until the differences become small/insignificant/approach zero"*
We will rephrase this:
…until the differences in SMB and ice sheet topography between two consecutive initializations ~~no longer change significantly~~become insignificant.

l. 230: (and other occasions): Replace *"on Fig. 2"* with *"in Fig. 2"*
Thank you, we will replace this.

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

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

References:

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

Davini, P., von Hardenberg, J., Filippi, L. and Provenzale, A.: Impact of Greenland orography on the Atlantic Meridional Overturning Circulation. Geophys. Res. Lett., 42, 871–879, https://doi.org/10.1002/2014GL062668, 2015.

Goelzer, H., Coulon, V., Pattyn, F., de Boer, B., and van de Wal, R.: Brief communication: On calculating the sea-level contribution in marine ice-sheet models , The Cryosphere, 14, 833–840, https://doi.org/10.5194/tc-14-833-2020, 2020a.

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.

Madsen, M. S., Yang, S., Aðalgeirsdóttir, G., Svendsen, S. H., Rodehacke, C. B., and Ringgaard, I. M.: The role of an interactive Greenland ice sheet in the coupled climate-ice sheet model EC-Earth-PISM, Clim. Dynam., 59, 1189–1211, https://doi.org/10.1007/s00382-022-06184-6, 2022.

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.

Slater, D. A., Straneo, F., Felikson, D., Little, C. M., Goelzer, H., Fettweis, X., and Holte, J.: Estimating Greenland tidewater glacier retreat driven by submarine melting, The Cryosphere, 13, 2489–2509, https://doi.org/10.5194/tc-13-2489-2019, 2019.