Authors' reply to reviewer comments

We address in the following the referee's comments (black) and show our responses (blue) to each comment.

Reply to comments by Anonymous Referee #1

We would like to thank referee#1 for their comments and suggestions that improved the storyline of our manuscript and made it more clear.

General Comments:
The authors present an extended high-emission scenario run with the coupled climate – ice sheet model NorESM2-CISM2.1. The study is the first to use NorESM with an interactive ice sheet model and adds nicely to other coupled climate – ice sheet and extended future warming scenario studies. Uncertainties in future projections of the Greenland ice sheet and its effects on the climate system are large. Therefore, it is desirable that more Earth system models include interactive ice sheet components. Multi-centennial simulations are necessary to account for the long time-scale processes in the land ice system.
The text is well-written and clearly structured.
The figures are good.
Thank you for this nice summary and feedback.
We see the manuscript improving by including and addressing your comments.

However, the text sometimes refers to fields (e.g. barotropic stream function or precipitation) for which no figures are included.
It would be helpful to include such figures, maybe in the appendix.
Thank you for this comment. We added the figures for fields that are discussed or mentioned in the manuscript. Namely: global annual precipitation and ice velocity within the main text and salinity at 400m depth, polar surface air temperature (annual, DJF, JJA), global precipitation in JJA and DJF, and winter snowfall for Artic (DJF) and Antarctic (JJA). The barotropic stream function is only mentioned during the "global average" section and included in Fig. 1h.

The text becomes a bit repetitive sometimes, especially in the discussion. The low climate sensitivity of the model and the corresponding small amount of land ice melting are pointed out too often. I think it is sufficient to do this in the introduction and then in the discussion again when necessary.
We fully agree. We re-wrote the last part of the discussion to avoid repetition and improve the story line.

With respect to the low melt rates, it may be useful to add some words about the relative cooling around Greenland compared to the uncoupled simulation (Fig2) and the potential implications of this relative cooling for the ice sheet melting in the discussion.
This is a very valid point. We added these sentences to clarify:

"The difference in surface air temperature stems from the additional freshwater influx around the coast of Greenland which increases ocean stratification and reduces vertical heat exchange leading to surface cooling. This does not explain the initial lack of melting which is due to the cold initial bias (discussed in Goelzer et al., (Disc.))."

This study provides results with a newly coupled climate – ice sheet model. Substantial conclusions are made. The methods are clearly outlined and the results are sufficient to support the conclusions. Focusing on climate – ice sheet interactions, the manuscript should be suitable for publication in ESD, with minor revisions to be made.
Thank you for this summary. We hope to have addressed your comments appropriately.

Detailed Comments:
L11: “low weak amplification” – Do you mean “weak polar amplification”?
Yes, adjusted.

L39: “own” – better rephrase: “include”.
Rephrased

L47-49: Ackermann et al., 2020 (GRL) did similar simulations with their Earth system model including an interactive Greenland ice sheet. You may consider adding them here.
Yes, but they refer to CMIP5. We added the citation, but changed CMIP6 to CMIP models in the sentence.

L69: Please add a reference for the used configuration “NorESM2-MM”.
Yes, we added a reference. But as mentioned here, this only describes the resolution setting:
“The model version is NorESM2-MM (Seland et al., 2020), equivalent to a nominal 1 degree resolution in atmosphere, land, ocean and sea-ice components resulting in a meridional resolution of 111 km over Greenland while the zonal resolution ranges from 13 to 55 km for atmosphere and land model.”

L94f: Why is the orography updated every 5 years but surface types are updated annually? I understand that this publication focuses on the model results and that a detailed model description is done separately. However, I find it difficult to understand the workflow of the coupling procedure.
These are two separate processes in the model. The update of surface types happens in the land model CLM (at runtime) as soon as an update is available from the ice sheet model (yearly). Update of the surface topography and surface roughness for the atmosphere model CAM is an asynchronous process by modifying the restart files. We added some sentences to the manuscript to make this point more clear:
“The land model CLM experiences changes of ice area annually by adjusting the surface type (e.g. ice or rock) and thereby adjusting the energy exchange within NorESM2 to the current ice extent. The atmosphere (CAM) receives updated ice sheet elevation directly from

CISM2.1 every 5 yrs through updates of topography and surface roughness. This 5 yrs coupling window is a compromise to keep the ice surface elevation updated and reduce the number of time-consuming recalculations of the restart files of CAM needed to set the new ice surface topography. For more information we refer to Goelzer et al., 2025."

L104f: How is the horizontal and vertical spreading of freshwater done? Could you elaborate more on this or provide a reference? A second question: is there a heat flux associated with the solid and liquid runoff?
These are good questions. We have to correct our statements: there is no vertical spreading of the ice discharge. The energy to melt the solid runoff (ice discharge) is taken from the ocean. We clarify this by adjusting the text accordingly:
“In NorESM2 with an evolving ice sheet, snow and ice melt are calculated in the land model CLM and routed to the ocean as liquid runoff with the runoff scheme (MOSART). This melt is communicated as SMB forcing to the ice model CISM which is adjusting the ice mass of the GrIS accordingly.
Ice areas that detach from the GrIS and ice loss due to calving are treated as solid discharge. CISM provides BLOM with annually collected solid discharge that is sent to the closest ocean cell as freshwater and distributed homogeneously throughout the year by BLOM. The energy needed to melt the discharged ice is taken from the ocean heat reservoir.“

L117f: “stand alone” – should be: “standalone”.
Fixed.

L122f: You may consider to move this sentence to the beginning of section 2.
Yes, this is possible. During the writing process, we moved this part of the section around a lot and due to the flow of information it made most sense in the end of the section after all other differences are explained.

Fig1d: You refer to the sea-level curve as “cumulative sea level contribution” but in the caption it says “Greenland ice mass changes.” Does this curve show the ice loss or the actual sea level rise including other effects like thermal expansion? Please clarify.
We understand the confusion. This curve shows the sea-level contribution of the Greenland ice sheet based on changes in mass above flotation. No further calculations were done here. The ocean component is volume conservative as described above and will not allow for thermosteric adjustments. We clarify this and adjust the figure caption to “d) Greenland ice sheet sea-level contribution (m)”

Fig1h: It may be more intuitive to invert the y-axis to see the decline (less negative numbers) in line with the other time series.
Thanks for the suggestion. We decided to keep the y-axis orientation in its usual way. Readers familiar with the barotropic stream function will otherwise likely misinterpret the

figure at first and readers not familiar with this variable will not get more value out of the curve matching the others.

L159: How is ice discharge treated differently in NorESM2fixed compared to the control simulation? I understand the different treatment between NorESM2 and NorESM2fixed but thought that the treatment in NorESM2 is the same as in the control simulation. Please clarify.
Exactly. NorESM2 and control have the same discharge treatment due to the additional ice sheet model coupling. NorESM2fixed is different. We shortened and rephrased the sentence to avoid confusion.

Fig2 third row: With "anomaly" you mean the changes since the 1850s of the respective simulation? Please clarify.
Yes, exactly. We clarified this using your expression: "To cover differences of the initial climate state in NorESM2 and NorESM2fixed, the last row is calculated as double differences, meaning the difference of the changes since the 1850s of the NorESM2 and NorESM2fixed."

L166: It would be helpful to include a figure similar to Fig2 for the barotropic stream function.
The barotropic stream function is shown in Figure 1h as a minimum of an area. We added a reference to the subplot (Fig 1h) in the text. This text is under the headline "Global average changes" and the paragraph only refers to the plot showing globally averaged line plots. We never discuss the spatial evolution of the barotropic stream function as it is not showing any interesting results for this study and further discussion of its evolution is outside the scope of this study. We hope this is more clear now.

L167: Please clarify, that you mean the barotropic stream functions in NorESM2 and NorESM2fixed that diverge from the control run and converge to each other.
Indeed, clarified.

L168: "… lower minimum values …" – better rephrase: "weaker gyre circulation".
Done.

L182: "Around the Greenlandic coastline" – better rephrase: "Around the coastline of Greenland".
Done.

L182-184: Where does the cooling around Greenland come from?
We addressed this in a reply to one of the general comments - see further up.

L185: Should probably be Fig3.
Correct.

L196ff: This sentence seems a bit misplaced here in the results section. I suggest to move it to the introduction.
We keep the sentence here just to explain very briefly why we look into this.

L200: "… is largely staying the same …" – better rephrase: "is largely unchanging".
Done.

L200ff: You may consider to add a figure for precipitation.
Yes, we added a figure for annual global precipitation. We further added JJA and DJF precipitation globally and for snowfall for the both poles in the supplementary and referred to it in the text.

L207: "… during winter …" – which winter? Better use DJF / JJA etc.
Done.

L207: Like above: same for "summer precipitation".
Done.

Fig4: Does the figure show annual mean values? Please clarify.
Yes, it does. We added "Mean annual …" at the beginning of the description.

L214: Do you annual mean sea surface temperatures? Please clarify.
Yes, we added clarification.

L219f: "Between 2100s and 2300s increase by … since 1850s". Unclear formulation: Do you mean temperature increase between 2100s and 2300s or temperature increase since 1850s? Please clarify.
We mean since the 1850s, but most changes happen first in the last 100yrs. We clarified the sentence by removing 2100s.

L221: Remove "still."
Removed.

L224f: "The North Atlantic (above 50°N) and Arctic Ocean are becoming fresher" – To which simulation are you referring?
Still both simulations (like in the sentence before). We added their names for clarification.

L226: " … are visible in a more saline …" – The differences are barely visible. You may think about adjusting the colorbar and adding a north polar stereographic projection, maybe as supplement.
We added a polar projection figure of sea surface salinity in the supplement.

L229: "… advancing divide …" – better rephrase: "increasing meridional gradient".
Done.

L230-232: You may consider to include a figure for salinity at depth.
We added a salinity at 400m depth figure in the supplement.

L238f: "… covers more area …" very difficult to recognize in Figure 6. If I understand correctly, maximum sea ice extent in the Arctic by 2100 is the light blue solid line in the upper panels. By eye it looks to me as NorESMfixed has a larger sea ice area but differences are small anyway. Maybe drop this sentence or include a more distinct figure.
We dropped the sentence.

L242f: "… ice free during summer and nearly all-year ice free… (see Fig1) with a mean annual sea ice area of less than …". Otherwise the next sentence beginning with "The remaining winter sea ice …" sounds contradicting.
We follow the definition of an sea-ice free Arctic where the sea-ice covers an area less than 1 mio km^2 - which we mention in the text. With the sentence "the remaining sea-ice" we aim to mention to the reader where the small area of winter sea-ice is formed in our simulation. We put more emphasis on the definition of ice-free Arctic and add "The **small** remaining winter sea ice …"

L246: What do you mean with changes of GrIS in NorESM2fixed? Greenland does not change in this simulation, does it?
Very true. We corrected this of course and removed NorESM2fixed.

L250: "… additional melt areas starts" – should be: "start".
Done.

L253: From Figure A1 it looks like SMB is always below 8 m/yr everywhere. Do you mean seasonal accumulation rates in the text? Please clarify.
Yes, we mean local maximum accumulation rates and adjust the sentence accordingly: "with a maximum annual mass gain of 7.5\,m\,yr^{-1} locally."

L257: It would be helpful to include figures for ice velocity.
We added a surface velocity into Figure 7

L260: "… direct SMB comparisons …" – should be: "comparison".
Done.

L260-262: "As explained in section 2, … This has been improved …" – consider to move this part to discussion.
We think this short reminder is helpful to explain why we chose a complicated approach to determine the contribution of elevation change to the SMB change.

L268f: Why “lack of SMB classes”? I thought the classes are the same for both simulations.
No. The SMB is only calculated if the ice sheet model CISM is turned on. Since this is not the case in NorESM2fixed, we cannot receive this information from the model.
To avoid confusion, we rephrased the sentence: “due to the missing data output of SMB and elevation classes in NorESM2fixed.”

L270-274: Refer to Figure A1.
Done.

L276f: ablation is less pronounced – To me it looks like the anomaly in 2200 is much more pronounced than in 2100. Please clarify.
Indeed. We went through the paragraph and improved the wording on negative SMB versus positive ablation.

L278: “ there is less or decrease in accumulation…” – I find this sentence confusing. Should this mean “there is less of decrease”, meaning that the rate of change becomes smaller? Please clarify.
L279f: Ablation increases compared to 1850.
Replying to both comments: There was some doubling of words and combined half-sentences likely due to combining comments during the final writing phase.
We rephrase the paragraph to be more clear.

L294: Consider to rephrase: “Arctic amplification factor is 0.2 higher in … than in …”
Done.

L301: Should be “in.”
Yes, corrected.

L307: “… and are hence harder to compare.”
Done.

L307ff: Consider adding “by the year 2300.”
Done.

L310: “lack of calving” – Do you mean an actual calving scheme with an iceberg model? In the method section you mention a calving parametrization for the ice sheet model.
Yes, we mean a more physical or complex calving scheme that would e.g. respond to ocean warming, increased runoff or ice thinning. There is only a simple scheme applied to keep the ice margins within present day ice boundaries and to remove all floating ice.
We state very clearly in the model description: “Calving is implemented as a flotation criterion, removing ice when it becomes floating and routing this mass as water to the ocean.”

L313: “Underestimating”
Done.

L313: What do you mean with “suggested” in this sentence?
We added “suggested as a reason for low impact” to explain that Mutjewerf et al. also discuss that with small (or underestimated) Greenland ice sheet melt there is of course less impact on the climate - as explained in the sentences later.

L323f: “… not be developed enough… “ What development do you mean here?
We rephrased to “to not be large enough”

L343: Remove “and”
No, we need the and there. We added “initialization approach **for CISM with the ESM.**”

L353: For comparison it would be helpful to add the SAT increase in NorESM2 between 2015 and 2100.
True. We added this information to the sentence (for NorESM2 it’s 3.6C).

L355: “ … air temperature changes …” Remove “changes”
Done.

L356f: Repetition: you already mentioned several times the low CS of NorESM. Please consider to point this out only where necessary.
Yes, we restructured the discussion paragraph to combine information to avoid repetition.

L359ff: Maybe the other way around: “Low GrIS mass changes allude to a low Arctic amplification and …”
Done.

L363: “This emphasizes …”
Done.

L363: “… of coupled climate – ice sheet models …”
Done.

L371: “becomes”
Done.

L373: remove “time”
Done.

L379: “ – which contrasts model studies with EMICs”
Done.

REVIEW#2

We would like to thank referee#2 for their comments and suggestions that improved the storyline of our manuscript and made it more clear.

In this paper, the authors analyze the first NorESM simulation including an interactive Greenland ice sheet model. The authors indicate a minor impact of interactive model on the global climate dynamics simulation. They do find differences in Arctic climate by 2100 and a large impact of the elevation-melt feedback on the overall ice sheet mass loss

Thank you very much for this summary and detailed feedback! We are confident we could improve the manuscript by including and addressing your comments.

General comments

The results are very interesting since there are very few examples of this type of coupled ice sheet-climate simulations. However, the analysis of results is difficult to follow as there are too many loose numbers in the text – these could be presented in tables instead – and with inconsistent metrics – sometimes ranges or extremes are presented, sometimes mean values with standard deviations -. The paper would benefit of some assessment of statistical significance of the differences between simulations (e.g, Figure 2), as well of (attempt of) explanations of these differences.

Thank you for this comment. We went through the text and made the numbers more coherent. We do not know of a method to calculate statistical significance based on the difference of two simulations. Any attempt to estimate significance based on, e.g., interannual variance within the individual simulations would be inherently incomplete and potentially misleading.

The main goal of our paper is to demonstrate the impact of the Greenland ice sheet within the NorESM coupled framework and to compare these results with previous studies. We focus on highlighting findings that differ from established results, and have aimed to keep the manuscript concise and focused. Nevertheless, we have added additional figures to support the text.

We show our results in spatial figures (and added more) and use numbers extracted from the figures to support our descriptions and discussions throughout the text.

We do not think that additional tables for some of our results would add more information. On the contrary, they would have to be explained and defined well which areas and which times they refer to. We did this for the Polar amplification because it is relevant for the topic and can be defined.

In the conclusions, the authors highlight how their results differ as those from previous work from EMICs. It would be interesting that the authors provide more detail on that.

The statement in the conclusion merely re-iterates a point made in the discussion. Where it is first mentioned, we elaborate on the nature of the difference and also provide two references to EMIC-based studies. We believe this level of detail is adequate for the present manuscript and that a deeper discussion would be out of scope here. The statement in the conclusions was slightly edited.

The design of the simulations needs some more clarity, taking some parts of the model description (submitted) here where they are relevant to explain the results. For instance, Figure 2 does not provide information about initial NorESM2-NorESM2fixed (temperature, surface elevation) differences (for a pre-industrial or 1850 climate). Also, the treatment of meltwater fluxes in the "fixed" simulation is not clear; perhaps these fluxes could be compared in the manuscript.

We improved the explanation of the simulations in general, also in response to comments from referee #1.

Although we agree that the initialization of the simulations is very important and interesting, we do not think that figures of (very small) anomalies in 1850 would do justice to the problem or allow readers to identify challenges. This is one reason for discussing the technical aspects of the model setup in detail in a separate manuscript (Goelzer et al., 2025). Our text emphasizes specific methodological details as needed for comprehension and, e.g., dedicated several sentences to the meltwater flux . We conclude that the sources and the distribution of meltwater flux are different in the two simulations and any comparison in this manuscript would be misleading. We look forward to comparing the meltwater fluxes for future simulations where they are comparable for coupled and uncoupled setup.

Title and main conclusion

The title can be misleading, as it refers to "Limited global effect of climate-Greenland ice sheet coupling (…)". The conclusions rather refer to the limited effects of Greenland ice sheet change on the global climate. In the title the word climate is first, and the coupling could be uni-directional ("one-way"). Therefore, the title can read as if climate change does not affect Greenland or global sea levels when both climate and ice sheet are modelled together within NorESM … In general, I would highlight more the added value of the coupling in title and conclusions, e.g., along the lines of mapping climate and (land, sea) ice change with a single model that permits to establish direct connections within the Earth System.

This is an interesting point. We want to avoid misleading information. At the same time it is important to not generalize our findings and state that there is never an effect of the added Greenland ice sheet component to the climate. Our results show a first coupling attempt of the Greenland ice sheet with the Earth system model NorESM and within this setup we do not see an effect. We would like to emphasize that our findings are specific for this particular setup.

Abstract

Lines 1-2: it seems from the title that the results do not justify the coupling? I would disagree, as this paper presents a consistent, comprehensive yet fully detailed account of (Artic, global) climate and Greenland ice sheet co-evolution within the same model. Perhaps worth to highlight more the relationships between the timing of NAMOC decline, sea-ice decline, and melt acceleration?

Thank you for this suggestion. Depending on the application, some readers may conclude that a full coupling is not justified. We, however, do not make this conclusion but rather document the limited effect that the coupling has on the large-scale climate. The reasons for this result are identified and we also highlight that this is the first simulation of its kind in NorESM, meaning that improvements in model bias and initialization may yield different results in the future. In other words, we agree with the referee's point of view but think changing the title might lead to conclusions for the wrong reasons..
The manuscript title does not necessarily say "we don't need the coupling" - it states that we show limited effects from the coupling. The manuscript discusses what we see and what is still missing in this coupling setup that might further show further impact of the Greenland ice sheet melt to the global climate (like better ice-ocean interaction which is work in progress). We will leave the introduction statement to introduce the Greenland ice sheet - which is the novum in this coupling.

Line 9: "an important part" -> quantify; e.g., how much is the mass loss enhanced by 2100, how much by 2300
We added to the sentence: " with around 20% of ablation happening due to elevation change in 2200."

Line 11: "1.4 m sea-level rise" -> consider to give this earlier, before line 8 "relatively minor changes"
We moved it higher up and changed the sentence structure.

Line 11: "low weak amplification" -> remove one adjective, amplification of what?
We corrected for "weak polar amplification"

Plain Language Summary
It is not very clear what is happening already and what is expected to happen.
The described processes are stating present processes that will continue in the future. We added "currently" into the first sentence.

Line 20: "is"
Done.

Intro
Line 48: not all citations are CMIP6 models, please check
We changed to CMIP models and added another reference.

Figures
Please add statistical significance. I suggest increasing the size, e.g., until at least page width. For Figure 2, it would be interesting to zoom in on the Greenland area/high latitude differences, and to relate with elevation differences.
For significance, please see answer further up.

We can unfortunately not increase the figure size due to the journal's style guidelines (12 cm width for double column and 8.3 cm for one column figures).
Figure 7 tries to show the elevation and resulting SMB changes. We added high latitude SAT, SSS and snowfall in the supplementary.

Results

Please introduce the structure of this long section. Also, consider adding numbered subsections. The differences in Arctic climate in the *fixed versus coupled simulations are a very interesting result, it would be great if the authors could go into more depth there.
We added numbered subsections and rephrased the already existing introduction to the structure. The manuscript focuses on several sections on the differences in Arctic climate, we added more figures in the supplementary that focus on the polar changes and differences to support the text.

Table 1 – please add standard deviations. Can you indicate which of the coupled versus fixed differences are statistically significant?
We do not see a useful way to apply statistical significance here. See also explanation in general comments further up.

Line 132: "PI" is a bit misleading, perhaps "1870s"/"1870" for consistency ?
Yes, we changed it to 1850s, which is what it was meant to say. - also in the rest of the text

Line 134: usually 2000s would refer to the decade 2000-2009
Yes, this is true. However, we only have simulation results until 2300. We would like to show average values over a 20yr mean period (which only works for 2280 to 2300 for this period) and we would like to be consistent in the naming and decided hence to go with the end of the century average. We hope the explanation of the naming at the beginning makes it clear to the reader.

Global average changes:

Consider adding rates of contribution to sea level rise (mm/yr) as this is an important metric of mass loss (see e.g., AR6).
Good point. We think this is well covered by showing here SMB in Gt/yr and cumulative sea level contribution in combination with a detailed comparison with other publications in the discussion.

Line 135: "Global average changes" -> "Time evolution of climate and ice sheet change"
Thank you for the suggestion. The focus in this subsection is to give an overview of global mean values before going in detail into spatial evolution and difference. We think your suggestion does not show this distinction.

Structure of this part is not straightforward, e.g., global temperature and ice sheet SMB are together in the same paragraph
Yes, but only very briefly. We introduce the results sections with the explanation that we will first give an overall overview of the overall evolution of each climate module. This gives the reader the possibility to see what is happening in general and how is the timing of the different changes in regards to the other climate modules. We afterwards, in different subsections, go in detail into each system.

Line 139: “has a strong impact”-> but temperature change is not per se the cause of SMB change – more melt energy and changes in snowfall are … -
We rephrased the sentence to “is reflected in”, since melt etc define SMB, but snow fall and melt are closely linked to SAT.

Line 141 – “fluctuating around 600 Gt yr-1” -> give mean and standard deviation, e.g., in table
We rephrased this instead.

Line 142 – “SMB with climate forcing” -> ?
Removed.

Line 142 “2000” this time of emergence is an interesting result, perhaps highlight more?
Yes, indeed. We go more into detail during the Greenland ice sheet results and discussion.

Line 146 is ice discharge increasing? Please explain
Yes, but we refrain from any ice discharge discussion because the model outputs are computed very differently (see model description). To avoid confusion, we decided to not give any discharge numbers. It is clear that with increased surface melt and increased ice mass loss, the ice discharge is increasing.
To avoid repetition (like climate sensitivity), we only mention the freshwater flux twice: in the model description and discussion.

Line 146 “200 years” -> shouldn’t it be more?
No, “from the increase in dynamic ice discharge and SMB decrease” which happens after 2100s. We add this number to the sentence to make this more clear.

Line 146 Figure 1d seems to show a trend for the pre-industrial simulation? Can you quantify this and comment on it?
The trend is a desired effect of the initialisation: to reproduce an observed mass loss over the historical period. This is described in some detail in Goelzer et al., (2025.). We added a sentence to say that:
“The sea level contribution at the beginning of the simulation period is influenced by the initialization to keep ice mass loss and margins close to observations and follows the control run with pre-industrial forcing for the first decades. ”

Line 147: “show a similar pattern” -> do you mean a spatial pattern? Or similar time evolution?
Both, but this paragraph described mean values, so we refer to the similar evolution over time. We rephrase the sentence to “similar temporal evolution”.

Line 148 I don’t see a graph or table in support of the global SST analysis
Yes, that is why we only mention it briefly and show and discuss sea surface salinity instead.

Line 148: “discernible” -> what is the criteria you used?
They follow exactly the same trend with only differences in seasonal fluctuation (same as SAT, as described in the sentence). We rephrased the sentence to avoid this word and make it more clear.

Line 149 refer to Fig. 1e in this paragraph
Done.

Line 155-156: range is compared with mean plus standard deviation, please consolidate.
Thank you for pointing this out. We removed the standard deviation here and in the following sentences. Further following your overall comment to be more coherent.

Line 173: here and in other places “up to” is not very representative, please use tables comparing e.g., averages and standard deviations
We rephrased most sentences using “up to”. In regards to tables, we do not think tables would add value. We use values in the text while still showing spatial plots and average line plots. We refer also to our answer on this in the general comments further up.

Lines 200-212: precipitation, is there a figure/table in support of “staying the same”? Also, numbers in the text can be replaced by a table.
We added a figure for annual precipitation.

Oceanic changes: figures are visually described, please add statistical significance, compare areal means, and look for some potential explanations for the results.
Exactly. We describe the changes within the ocean simulated in our setup with focus on how the Greenland ice sheet impacts those. Detailed discussion on the oceanic results is outside of the scope of the study.

Greenland ice sheet changes:
There is not much analysis of surface mass balance or energy components here, is there any reason not to do this?
Thank you for this comment. There is already an evaluation of the SMB in the companion paper Goelzer et al., (2025.) and we did not see an additional value to the story of our manuscript by including a detailed analysis of SMB and energy here. Further, this would

only pertain to the coupled experiment and not to the comparison of NorESM and NorESMfixed, which is our focus in this paper.
We added a sentence in the results “Greenland ice sheet changes”
“For detailed evaluation of the historic surface mass balance we refer to Goelzer et al., 2025.”

Please increase size and add elevation contour lines to Figure 7, to illustrate changes in equilibrium line altitude. Perhaps interesting to add a figure/table with evolution of the mean equilibrium line altitude? (can be calculated from the ablation area % and the function of cumulative area below a certain elevation, or “hypsometric curve”)
Given the fixed figure sizes in Earth System Dynamics, the only way to increase the size of individual panels is by changing their order. We prefer not to do that because the current setup follows a certain logic that we believe is important to keep.
The subplots show mainly SMB anomalies. Adding the ELA would likely add to confusion, because the ELA would be based on the absolut SMB, not the SMB differences shown in the Figure 7. Further, a detailed discussion of the SMB is not the scope of this manuscript: to compare differences between coupled and uncoupled experiments. However, a detailed SMB discussion that you would like to see can only be done for the coupled simulation. A more detailed analysis is done in Goelzer et a., (Disc). We added a comment referring to that paper for the interested reader.

Line 246: here and in other places “visible” -> replace by scientific criteria, such as “statistically significant”, quantification of differences, explanation of mechanisms, etc.
See reply in general comments.

Line 280: “up to “ , not representative of overall change -> use representative metrics, e.g., table with e.g., mean snowfall over ablation/accumulation area
See reply in general comments.

Figure 7h: there seems to be a strange “corner” along the equilibrium line altitude, do you know what causes this?
We cannot identify exactly which strange corner you refer to. In general, there are a few visible artefacts in some of the figures that are due to the downscaling and possibly the anomaly calculations.

Discussion

Please check (and introduce) structure, e.g., line 325 “as mentioned earlier” marks an unexpected break.
We restructured and rephrased the discussion to accommodate the different comments for the discussion.

Line 310 “the lack of calving might underestimate” -> “and ocean forcing to the calving front likely”
Not necessarily. Ocean forcing could also be parameterized and given as a calving law. We only use calving when the ice would reach out further than the initial ice sheet.

Line 353 SAT of 5 C for which greenhouse forcing? Do you mean SSP5?
Yes. We added RCP8.5

Line 355 move this before the previous paragraph?
We restructured and rephrased the entire discussion to accommodate the different comments for the discussion.

Conclusions

Line 368 see -> analyze, assess, etc
Done.

Lines 369-371 This is an interesting result, please quantify, explain (at least tentatively)
This is just the conclusion of the entire manuscript. We write about this in detail during the results and discussion (line 365ff).

Line 372 “major contributor” -> quantify
We added “these elevation feedbacks are making up for almost half of the SMB change what makes them a major contributor to the decreasing surface mass balance.”

Line 373 “longer” with respect to?
We referred to the standard Greenland ice sheet projections that often go until 2100. We rephrase to “encourage simulations beyond 2100 and 2200”

Line 379 interesting result, please specify/quantify
This is only the conclusion. We explain this more during the discussion part.

Line 381 advantage -> added value
Done.

Goelzer, H., Langebroek, P. M., Born, A., Hofer, S., Haubner, K., Petrini, M., Leguy, G., Lipscomb, W. H., and Thayer-Calder, K.: Interactive coupling of a Greenland ice sheet model in NorESM2, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-3045, 2025.