RC1: 'Comment on egusphere-2025-4140', Anonymous Referee #1, 03 Nov 2025

General remarks

This is an excellent and novel quantitative analysis of the contribution of the local change in background thermodynamic contribution to the Greenland Ice Sheet surface mass balance loss. It effectively separates out the role of sea-surface temperature and sea-ice concentration forcing, which is found (corroborating some but not all previous studies) to be relatively minor. A valuable analysis of the extreme melt case studies of 2012 and 2019, and the difference in terms of climatic forcing between these events, is provided. The analysis is thorough and the paper is clearly written. It will be of wide interest to Greenland climate scientists and ice-sheet specialists. I recommend publication following a minor revision addressing the points below.

We thank the reviewer for their encouraging feedback. Please see our response (in bold) to each of your specific comments below.

Specific comments

Line 26: please add the following recent relevant references:

Otosaka et al. (2023) https://essd.copernicus.org/articles/15/1597/2023/

Hanna et al. (2024) https://www.nature.com/articles/s43017-023-00509-7#publish-with-us

Thank you for suggesting this recent work on the subject. We have added these references in support of the statement on Line 26.

Figure 1: define PGW-1, PGW-2 etc. I the figure caption.

Thank you for helping to make our discussion of the two model experiments clearer. We have also incorporated the suggestion from Reviewer #2 to rename the two PGW experiments to explicitly note the boundary fields that are altered in each experiment, and we have included those definitions in the Figure 1 caption.

Lines 167-179: please add that while GCMs may capture the periodicity of internal climate variability they may not capture the magnitude of such variability. Will taking the CESM-LE ensemble mean be affected by signal-to-noise issues with the GCMs in capturing North Atlantic circulation change?

We have edited the first sentence of the paragraph in question as follows to note that GCMs may not accurately resolve the magnitude of internal variability (changes italicized):

> **"While GCMs aim to capture the periodicity of internal climate variability,** they may not accurately resolve the magnitude of said variability and **the precise timing of a particular mode of variability differs between individual ensemble members."**

We hope that we are correct in interpreting the reviewer's mention of circulation change as referring to atmospheric circulation and not oceanic circulation. If the reviewer is

referring to the failure of GCMs to capture the post-2000 change in North Atlantic atmospheric circulation, then that is precisely what motivated our study design. The use of ERA5 reanalysis in both the control and PGW simulations ensures that the synoptic-scale variability conveyed to MAR at its boundaries is the same in each of the model runs and is representative of the anomalous circulation observed between 2000 and 2019. So, we are not reliant upon the GCM representation of North Atlantic circulation in that sense. We only relied upon GCM output (i.e., CESM-LE) to provide an estimate of the long-term change in the background thermodynamic state of the atmosphere, which was then used to adjust the ERA5 boundary conditions in such a way that the anomalous circulation was preserved between the three model runs. We utilized the ensemble mean to remove some of the noise of internal variability and better isolate the long-term signal of the changing background state. The perturbation fields were computed from the long-term monthly mean for the preindustrial and control periods averaged over the 40 ensemble members of CESM-LE. In total, the monthly means employed in the computation of the perturbation fields represented 800 years of data for each period, which should effectively average out the internal variability.

In response to the suggestion from Reviewer 3, we conducted a comparison of average 500 hPa geopotential heights over Greenland to demonstrate this preservation of the large-scale circulation. This comparison is now included as supplementary figure 1, which we discuss at the end of the second paragraph of section 2.

l.201: Are there issues with the accuracy of prescribed SIC and SST for 1880-1899 that may affected the method used?

Thank you for raising this issue. The Hadley-OI dataset does have certain limitations relative to ERA5 that should be mentioned, and these shortcomings also speak to some of the issues regarding model representation of ocean-atmosphere coupling raised by reviewers 2 and 3. Comparisons between gridded global SST products including the HadISST1 dataset that is the source of SST in the merged Hadley-OI product during the preindustrial period examined here generally show strong spatial and temporal agreement; however, the SST records provided by these datasets are least robust in areas that rely more heavily on spatial interpolation, such as along coastlines and near the sea ice front, where in situ measurements are less frequent. Furthermore, the relatively low spatial resolution of the preindustrial data struggles to capture finer-scale features such as boundary currents, as well as SST and its variability in coastal locations, and acts to smooth sharp SST gradients along ocean fronts (Yang et al., 2021; Hurrell et al., 2008; Hanna et al 2006). However, the disparities among datasets are generally much smaller than the long-term trends (Yang et al., 2021) and, therefore, should exert minimal influence on the signal that we seek to quantify in our analysis. Similarly, SIC from 1880-1899 is inferred from monthly, 1901-1930 climatologies of hand-drawn sea ice charts (Hurrell et al., 2008; Rayner et al., 2003) which should provide a reasonable estimate of the long-term change since the preindustrial period. However, these issues could have some impact on model representation of the local marine influence on Greenland Ice Sheet surface mass balance but are unlikely to have clear, systematic impact on the results for Greenland as a whole.

We have included these points in a broader discussion on potential shortcomings in the model representation of ocean-atmosphere coupling in the second to last paragraph of section 4.

Hanna, E., Jónsson, T., Ólafsson, J., and Valdimarsson, H.: Icelandic Coastal Sea Surface Temperature Records Constructed: Putting the Pulse on Air–Sea–Climate Interactions in the Northern North Atlantic. Part I: Comparison with HadISST1 Open-Ocean Surface Temperatures and Preliminary Analysis of Long-Term Patterns and Anomalies of SSTs around Iceland, Journal of Climate, 19, 5652–5666, https://doi.org/10.1175/JCLI3933.1, 2006.

Hurrell, J. W., Hack, J. J., Shea, D., Caron, J. M., and Rosinski, J.: A New Sea Surface Temperature and Sea Ice Boundary Dataset for the Community Atmosphere Model, Journal of Climate, 21, 5145–5153, https://doi.org/10.1175/2008JCLI2292.1, 2008.

Rayner, N. A., Parker, D. E., Horton, E. B., Folland, C. K., Alexander, L. V., Rowell, D. P., Kent, E. C., and Kaplan, A.: Global analyses of sea surface temperature, sea ice, and night marine air temperature since the late nineteenth century, J. Geophys. Res., 108, 2002JD002670, https://doi.org/10.1029/2002JD002670, 2003.

Yang, C., Leonelli, F. E., Marullo, S., Artale, V., Beggs, H., Nardelli, B. B., Chin, T. M., Toma, V. D., Good, S., Huang, B., Merchant, C. J., Sakurai, T., Santoleri, R., Vazquez-Cuervo, J., Zhang, H.-M., and Pisano, A.: Sea Surface Temperature Intercomparison in the Framework of the Copernicus Climate Change Service (C3S), Journal of Climate, 34, 5257–5283, https://doi.org/10.1175/JCLI-D-20-0793.1, 2021.

ll.252 & 480: please add the following highly relevant reference:

Hanna et al. (2021) https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.6771

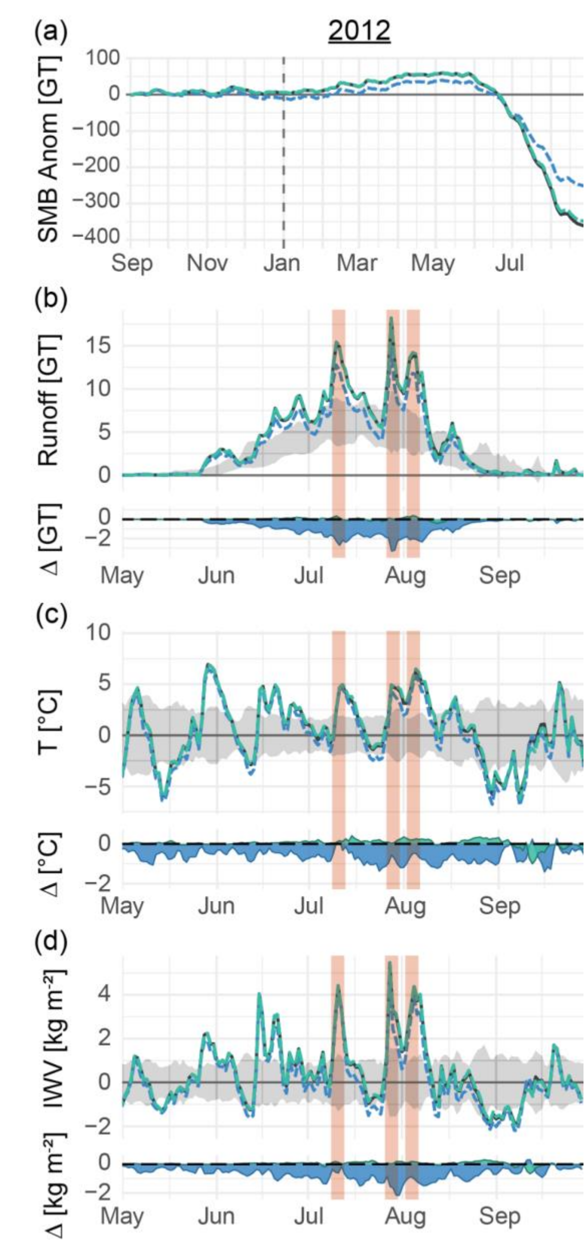
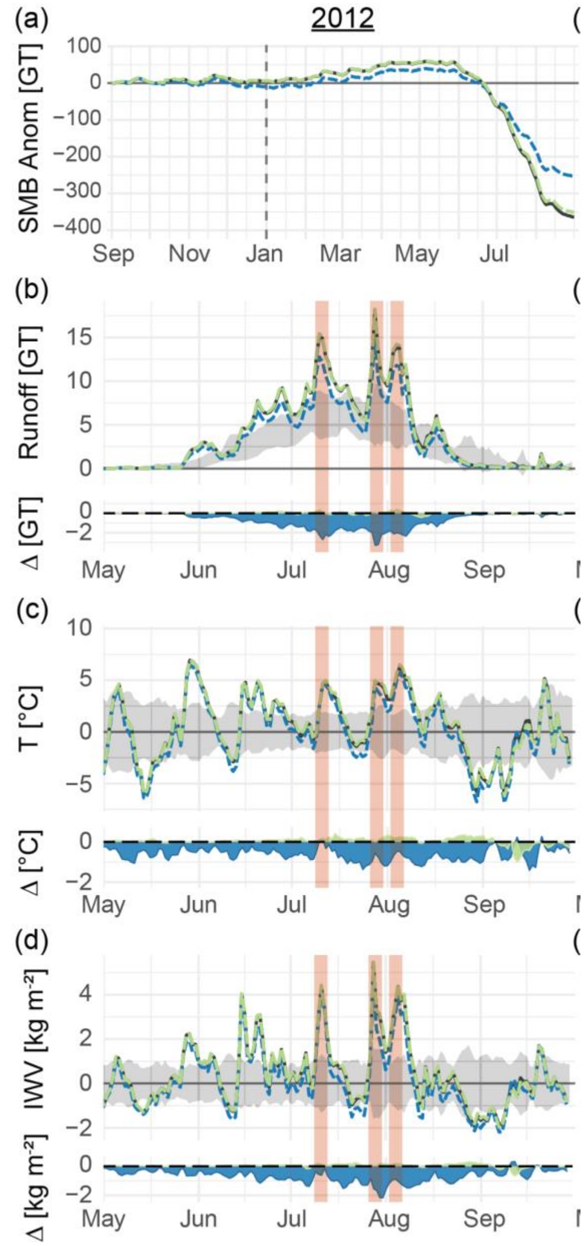
Thank you for highlighting this oversight. We have added this reference in support of the identified statements which are now located in the first paragraph of section 3.1 and the second paragraph of section 3.4.

The green and blue lines on some plots look a bit similar; could they be distinguished more clearly?

Thank you for helping to improve our data visualization. We have applied the changes to the color scheme that are shown in the figures below to all time series throughout the manuscript.

Original Fig. 4

Revised Fig. 4

<table>
<tr><th>Original Fig. 11</th><th>Revised Fig 11</th></tr>
<tr><td>
</td>
<td>
</td></tr>
</table>

l.496: please add the following highly relevant reference to “more persistent circulation regimes under global warming”:

Overland et al. (2012) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2012GL053268

Thank you for highlighting this omission. We have added this reference in support of the referenced statement in the third paragraph of section 3.4.

RC2: 'Comment on egusphere-2025-4140', Jason Box, 30 Nov 2025

Summary
The submitted article is very clearly written and applies a clever pseudo-global warming (PGW) method to estimate of the relative contributions of dynamical versus thermodynamic change to recent Greenland ice sheet surface climatic mass loss.

It's an interesting angle that "the change in the background thermodynamic environment, and its resulting impact on GrIS SMB, represents a more robust signal of climate change than the potential dynamical response

Thank you for the time that you invested on this thoughtful review. Please find our response to each of your comments formatted in bold text below.

high level critique in no particular order of importance…

I think this is an attribution study, and so think the a-word belongs in article s title

We appreciate your perspective here. In fact, this question is one that we also grappled with as authors before submitting for review. While we agree that the approach we took functionally acts to attribute observed mass loss to one effect of anthropogenic forcing, we are not sure that it meets the criteria of an attribution study as more formally defined, particularly as it pertains to the provision of a statistically-based confidence level. Rather, this study leans on prior works that attribute the observed changes in the background climate to anthropogenic causes and builds from there. In other words, our assertion that the thermodynamic change investigated in this study can be more confidently ascribed to anthropogenic warming is based on prior, more canonically-defined attribution work, but our analysis does not directly compare the observed change over Greenland to the fingerprint of anthropogenic forcing versus that of internal variability and natural forcing.

Further, as noted by Reviewer #3, our experimental design is more constrained than typical attribution work. We are comparing the historical record with a hypothetical situation in which the anomalous atmospheric circulation of recent decades had occurred under a preindustrial background state. This falls short of assessing whether observed mass loss is caused by the full effects of anthropogenic forcing, which includes the combined contributions of the background thermodynamic state and any attendant dynamical response.

While some (e.g., Trenberth et al, 2015; Lloyd and Oreskas, 2018) have argued that what qualifies as detection and attribution should be extended beyond the probabilistic assessments that currently define the approach to also include the so-called "storyline" approach that describes our analysis, we have chosen not to label this as an attribution study given the more narrow definition that is widely accepted at this time. We do, however, agree that the title could more clearly define the thermodynamic forcing examined in the study and have edited it as follows:

“Estimating the Thermodynamic Contribution of Post-Industrial Warming to Recent Greenland Ice Sheet Surface Mass Loss”

Trenberth, K. E., Fasullo, J. T., & Shepherd, T. G. (2015). Attribution of climate extreme events. Nature climate change, 5(8), 725-730.

Lloyd, E. A., & Oreskes, N. (2018). Climate change attribution: when is it appropriate to accept new methods?. Earth's Future, 6(3), 311-325.

The article would benefit from stating and focusing some discussion on assumptions, to what extent they may or may not be violated, e.g. coupling between ocean and atmosphere is assumed to be well represented by the approach... the relatively minor SMB response to changing sea ice, line 428 has implicit assumption "is evident that sea-surface conditions alone exert minimal influence on the ice sheet" .. Further discussion lines 434 to 440 rests on the assumption the modeling accurately captures ocean/atmosphere energy exchange.
A sub point, assuming the modeling accurately captures ocean/atmosphere energy exchange, where around Greenland has the greatest sea ice change response? And can that regional signal be credibly interpreted?

We appreciate the discerning and complimentary critiques that you and Reviewer 3 have provided on this issue. It is clear that our framing of the results of the PGW2 experiment should be better constrained to acknowledge the potential shortcomings of our experimental design and general model representation of relevant processes governing connections between the local marine environment and the state of the ice sheet. We wholly agree that the manuscript would benefit from explicit discussion of the assumptions at play, which may explain some of the disparity between observational and modeling work on this subject. In addition to the reviewer’s point regarding representation of ocean-atmosphere coupling, and as noted by Reviewer 3, our experimental design does not allow us to exclude the possibility of nonlinear interactions between combined changes in atmospheric and sea-surface conditions (SSC) leading to a greater impact than changes in SSC alone. Finally, while we deliberately designed the experiment to control for any indirect effects of SSC through modification of the synoptic-scale flow, this represents a legitimate pathway that likely explains some of the statistical relationship between SSC and the ice sheet established in previous work.

Regarding the regionality of the relationship between sea ice change and the ice sheet response, prior work has pointed to sea ice loss in Baffin Bay and the Davis Straight as driving melt over the western ice sheet (Stroeve et al., 2017; Rennermalm et al., 2009). The response in our PGW2 experiment does geographically align with the above-mentioned observational analyses, with greatest mass loss along the western periphery of the ice sheet.

Motivated by your and Reviewer 3’s feedback, we have included an expanded discussion of our results relative to those of Stroeve et al. (2017) as what is now the penultimate paragraph of section 4, where we address potential sources of model bias – including ocean-atmosphere coupling – and note the areas of agreement between the two analyses.

Rennermalm, A. K., Smith, L. C., Stroeve, J. C., and Chu, V. W.: Does sea ice influence Greenland ice sheet surface-melt?, Environ. Res. Lett., 4, 024011, https://doi.org/10.1088/1748-9326/4/2/024011, 2009.

Stroeve, J. C., Mioduszewski, J. R., Rennermalm, A., Boisvert, L. N., Tedesco, M., and Robinson, D.: Investigating the local-scale influence of sea ice on Greenland surface melt, Cryosphere, 11, 2363–2381, 2017.

A recurring question for me:
a) 503-504 "suggests no appreciable contribution by the observed change in local sea-surface conditions to runoff production during these exceptional melt years" and/or the coupling of runoff to sea ice in the modeling is off/muted/incomplete

While it is true that the results of our model experiment suggest no appreciable contribution by local sea-surface conditions, we agree that we should better condition this statement by providing a comprehensive discussion of potential model shortcomings in representing relevant processes. This includes potential biases in energy fluxes from ocean to atmosphere and the possibility of nonlinear interactions between concurrent changes in ocean and atmospheric conditions.

We have made edits throughout the manuscript to more accurately condition our results as informing on the "directed thermodynamic" influence by sea-surface conditions and we have included an expanded discussion of potential model shortcomings as the second-to-last paragraph of section 4.

b) 614 "Even so, this analysis reveals a minimal influence." or the coupling is incomplete/ineffective?

Thank you. We have edited this statement as follows and expound upon potential model shortcomings in the following paragraph.

"Even so, this analysis reveals minimal direct thermodynamic contribution by local SSCs."

c) 626-627 "It should also be noted that" or the coupling here is incomplete/ineffective? ...alternative hypothesis, the not-explicitly-treated interplay between dynamics and thermodynamics is to blame for the counterintuitive result that is to reject the idea "that higher SST and lower SIC may promote earlier melt onset in the spring"?

This is an important point that we agree should be emphasized more within the text. Our study design precludes the possibility of assessing any indirect impacts of changes in sea-surface conditions on ice sheet melt via potential alteration of the large-scale atmospheric flow. We have added a detailed discussion of this point to the penultimate paragraph of section 4 and reference following studies, which present evidence of such a dynamical response to changes in sea-surface conditions around Greenland. After a closer comparison with the results of Stroeve et al. (2017), we believe this may be a critical mechanism at play, as stated in the following excerpt from this newly added paragraph:

“Lastly, our analysis did not examine any indirect effects via alteration of the large-scale circulation by changes in SSCs. Both model- and observation-based studies have yielded evidence of a link between declining sea ice in Baffin Bay and the observed increase in summer Greenland blocking (Liu et al., 2016; Screen, 2013; Sellevold et al., 2022; Wu et al., 2013). Indeed, Stroeve et al. (2017) found that the statistical relationship between ice sheet melt and Baffin Bay SIC weakened considerably and, consistent with the $\mathrm{PGW_{SSC}}$ response in Fig. 5c, became more confined to the periphery of the western ice sheet after correcting for the influence of the GBI. Considered in conjunction with our results, this suggests that this indirect pathway of influence could help explain the statistical relationship between SSCs and early melt onset.”

Screen, J. A.: Influence of Arctic sea ice on European summer precipitation, Environ. Res. Lett., 8, 044015, https://doi.org/10.1088/1748-9326/8/4/044015, 2013.

Liu, J., Chen, Z., Francis, J., Song, M., Mote, T., and Hu, Y.: Has Arctic Sea Ice Loss Contributed to Increased Surface Melting of the Greenland Ice Sheet?, J. Climate, 29, 3373–3386, https://doi.org/10.1175/JCLI-D-15-0391.1, 2016.

Sellevold, R., Lenaerts, J. T. M., and Vizcaino, M.: Influence of Arctic sea-ice loss on the Greenland ice sheet climate, Clim Dyn, 58, 179–193, https://doi.org/10.1007/s00382-021-05897-4, 2022.

KEY: "io" means "instead of", NUMEBRS: line numbers
87-88 the idea of warmer atmosphere, more water vapor appears as a statistically robust feature of observational data, specifically N Atlantic SST and Northern Hemisphere near-surface air temperatures correlation with Greenland snow accumulation, matching theory articulated by Treberth (doi: 10.3354/cr00953), see https://doi.org/10.1175/JCLI-D-12-00373.1 figure 9a, Table 3 and related discussion, e.g. "Paeth et al. (2002) link the increasing precipitation to increasing SST and atmospheric CO2."

Thank you for noting this highly relevant work. We have added some text to the fifth paragraph of the introduction to acknowledge the evidence outlined in the above-mentioned articles which supports the water vapor feedback mechanism that we describe here.

libe 603 "remotely sourced heat" is a point made by https://doi.org/10.1175/JCLI-D-12-00373.1 and that study also examines N Atlantic SST as being less remote than the N Hemisphere

Thank you for suggesting this paper, which helps us anchor our results in the existing literature while highlighting the congruence between our results and observational

evidence. We have added the following text to the fifth paragraph of the introduction in acknowledgement of this work:

“Indeed, Box et al. (2013) linked increased precipitation over Greenland to higher Northern Hemisphere surface air temperature and showed that this statistical relationship was more robust than when considering local near-surface temperatures over Greenland or North Atlantic SST—a result that emphasizes the importance of remotely-sourced heat and moisture to the SMB.”

526 "2019 underwent more frequent melt at lower elevations", 2019 had a low accumulation anomaly across the west, preconditioning the albedo feedback to do its thing alongside the anomalous synoptic forcing

We have acknowledged Tedesco and Fettweis (2020) and Bailey and Hubbard (2025) here to note the role of low snow accumulation in producing the anomalous melt of 2019.

To make the text easier to follow, the abbreviations PGW1 PGW2 should be PGW_T_q PGW_SST_SIC, stacked super sub script, e.g. LaTeX
PGW_{\text{SST}}^{\text{SIC}}
% or shorter
PGW_\text{SST}^\text{SIC}
% or even
PGW_{\mathrm{SST}}^{\mathrm{SIC}}
or HTML: PGW SST^{SIC} or …

Many thanks for this highly effective suggestion. We agree that this will improve readability and havel adopted this into the text with a minor change to clarify that PGW1 includes the sea-surface condition (SSC) adjustments in addition to the T and Q fields:

Replace PGW1 with PGW_{\text{SSC}}^{\text{T,Q}}
Replace PGW2 with PGW_{\text{SSC}}

idea around Figure 7.: The delta LWD correspond to a delta T, if assuming same emissivity for control vs the two experiments (or emissivity as a function of water vapor), is the delta in effective radiative temperature consistent with the observed delta T

We apologize, but we are not sure we understand what the reviewer is suggesting here. We can, however, clarify that while we approximate the thermodynamic response to external forcing via the climate perturbation that was estimated from CESM-LE data and applied at the model boundaries, the external radiative forcing within the model itself is constant between the three experiments. So, any change in downward longwave radiation should be reflective of the integrated contributions of changes in atmospheric temperature, water vapor content, and cloud cover. We are certainly willing to provide additional information or consider additional supporting analysis upon further clarification.

The reader would appreciate capturing the main points with a smaller word count, otherwise I would have rated the article in 4) Presentation quality, as excellent because 4) Presentation quality includes "concise"

We have reviewed the manuscript and reduced the word count by ~800 words and moved one figure to the supplementary info.

** lower level critique **

Main Text

50 statistically "significant"? In any case, the s-word should be reserved for the result of a statistical test.

We have edited the text to clarify that we do, in fact, mean statistically significant in this instance. Thank you for helping us to clarify.

55,396,515 downward io incoming (find and replace); downward is the PMOD World Radiation Center standard

We have replaced all instances of "incoming" with "downward" when referring to energy balance terms.

59-60 (Gallagher et al., 2018; Lenaerts et al., 2019; Noël et al., 2019; Wang et al., 2019). read and cite also http://dx.doi.org/10.1002/2016GL072212

This citation has been added in support of the referenced statement.

62-63 cite also http://dx.doi.org/10.1002/2016GL067720 and http://dx.doi.org/10.3389/feart.2016.00082

We added these citations to the referenced statement.

195 delete un-needed "quite", and in general to avoid ambiguity, avoid adjectives and adverbs

Thank you for this helpful guidance. We have deleted the referenced instance of "quite" and reviewed the manuscript for other instances of unnecessary syntax.

252 delete un-needed "precipitous"

This has been deleted

288 "bouts", have a read and consider incorporating ideas/results from https://doi.org/10.1029/2024GL110121

Thank you for the recommendation. We have incorporated this reference and note the potential competing influence of increased snow accumulation in a warmer climate on the mass balance of the ice sheet in the fifth paragraph of section 3.1.

306 supporting info in https://doi.org/10.1002/met.2134 figs 5 and 6 and related discussion

We have incorporated this reference and noted the congruence between historical trends in rainfall frequency and our modeling results in the sixth paragraph of section 3.1.

312 periphery io terminus

We have replaced “terminus” with “periphery” here.

Fig. 6, aren't the units Gt per day?

Thank you for catching this oversight. Yes, we have changed the units in Fig 6 and Fig 11b,f to read Gt per day.

throughout instead of [] around units, suggest ", unit"

We have implemented this change in formatting.

345 "greatest over the northern ice sheet." consistent with Orsi study?

Thank you for bringing the Orsi study to our attention. It is indeed consistent with our findings and we have incorporated it alongside our discussion of Noël et al. (2019) in the second paragraph of section 4.

448 with io experiencing. The latter is for sentient beings.

We have replaced “experiencing” with “with.”

451 incur (or exhibit) io experience

We have removed this instance of “experience”

491-494 the following main insight deserves highlight in abstract and/or conclusions "This suggests that the relative importance of a changing background state under global climate change may be minimized during periods of strong synoptic-scale atmospheric forcing. In other words, the record melt observed during those two summers is more a consequence of exceptional atmospheric circulation patterns than it is a direct consequence of the long-term warming trend"

We have edited the final sentence of the abstract as follows to more clearly communicate this point (changes in italics):

> **“Furthermore,the thermodynamic contribution to surface mass loss** *during the record* **melt years of 2012 and 2019 was less than half that of the long-term average,** *suggesting that the pronounced mass loss during those two summers was more a result of the anomalous atmospheric circulation than a direct consequence of the long-term warming trend.”*

500 "turbulent heat fluxes (Fausto et al., 2016a, b)." also Box et al 2022 GRL

We have added Box et al. (2022) in support of the statement ending on line 500.

524 remove un-needed "quite"

This have been removed.

534 "anomalously high atmospheric" io "anomalous atmospheric"

We removed this sentence in our efforts to reduce the length of the manuscript.

546 "southwestern ice sheet" io "western slope of the southern ice sheet", elsewhere PLEASE use "ice sheet" instead of "GrIS" once it's obvious the geographic focus is Greenland

We have made the suggested substitutions. Thank you for helping to improve the readability of the manuscript.

537 "it relatively[?] susceptible" io "it more susceptible"

We removed this sentence in our efforts to reduce the length of the manuscript.

566,elsewhere "downward" io "downwelling" (find and replace)

We have replaced all instances of “downwelling” with “downward.”

609 un-needed "It is important to note that"

We have removed this phrasing.

618 nu-needed "It should also be noted that" but seems a new paragraph

We removed the quoted text and the remainder of this paragraph as we revised section 4 based on reviewer feedback.

RC3: 'Comment on egusphere-2025-4140', Anonymous Referee #3, 30 Dec 2025

This manuscript presents an interesting analysis using the so-called ‘pseudo global warming’ approach to quantify the thermodynamic contribution to GrIS SMB changes. The manuscript is very well written, and the analysis is easy to follow. I especially appreciate the excellent Introduction and the Discussion sections. Below, I list some critics of the manuscript, which can be addressed in a revised version. I think these all fall on the minor side of the 'major revisions' and I am supportive of this paper’s eventual publication.

Thank you for your positive feedback regarding the presentation of our study, and for your thought-provoking and constructive feedback on our methodology. Your comments, along with those of reviewer 2, have highlighted a need for more thorough disclosure of the limitations of our study design and the PGW approach more generally. Please see our point-by-point response to your comments formatted in bold text below.

I am raising a potential issue with interpreting the ~62% reduction in anomalous surface mass loss under “preindustrial” conditions. As MAR is forced by ERA5 boundary conditions – and the PGW approach is designed to hold the synoptic evolution as similar as possible between experiments – the “62% statement” is inherently conditional on the imposed large-scale circulation. As such, the results quantify thermodynamic amplification given the observed circulation anomalies, but they do not quantify (and should not be worded as if they quantified) the forced versus internal origin of the circulation anomalies themselves, hence are not to be used as full attribution. This distinction should be made explicit in the abstract and conclusions wherever the “62%” number (and related statements about the “substantial contribution of external forcing”) are presented, to avoid the impression of a full causal partitioning of the observed SMB anomaly into ‘dynamic vs anthropogenic’. I’d suggest adding the phrasing for example ‘conditional on the ERA5 2000–2019 circulation’. In addition, consider revising the terminology around ‘anthropogenic’ versus ‘externally forced’ changes unless the perturbation is explicitly anthropogenic-only.

Thank you to the reviewer for their discerning feedback on this issue. It was certainly not our intention to give the impression that the dynamic and anthropogenic contributions are independent of each other, as we acknowledged prior work in both the introduction and discussion sections suggesting that changes to the earth system in a warming climate may have promoted the recent anomalous circulation that has accelerated melt of the ice sheet. We had stated our intent to incorporate the following references in our final response during the open review process; however, the Preece et al. (2023) paper was already included. We did add the Sellevold reference in support of our discussion in the second-to-last paragraph of section 4.

Preece, J. R., Mote, T. L., Cohen, J., Wachowicz, L. J., Knox, J. A., Tedesco, M., and Kooperman, G. J.: Summer atmospheric circulation over Greenland in response to Arctic amplification and diminished spring snow cover, Nat Commun, 14, 3759, https://doi.org/10.1038/s41467-023-39466-6, 2023.

Sellevold, R., Lenaerts, J. T. M., and Vizcaino, M.: Influence of Arctic sea-ice loss on the Greenland ice sheet climate, Clim Dyn, 58, 179–193, https://doi.org/10.1007/s00382-021-05897-4, 2022.

The basis of our argument is that, unlike any changes in atmospheric circulation, the historical change in the background thermodynamic state that we attempt to isolate here is a robust signal that can confidently be attributed to anthropogenic forcing. While we do not intend to label this as an attribution study because, as noted in our response to reviewer 2, it does not meet the widely accepted definition of detection and attribution work, it is worth noting that others have argued that what does constitute attribution work should be expanded to include a so-called “storyline” approach where, rather than focusing on how climate change may be influencing the probability of realizing a given event or whether or not a given event can be clearly distinguished from internal variability, the analysis should take the occurrence of a historical event as a given and instead seek to estimate how long-term warming influenced said event (see Lloyd and Oreskes, 2018). They argue that, from this perspective, it is appropriate to place more weight on the consequences of anthropogenic forcing that we have the most confidence

in—namely, the first-order thermodynamic response. This is not a competing approach, but rather an alternative and complementary perspective to probabilistic attribution works. While the storyline approach has typically been advocated for use in investigations of isolated extreme weather events, we believe the argument in support of this approach also applies to our case given the large uncertainty regarding dynamic atmospheric response to climate change over the North Atlantic, particularly regarding future trends in Greenland blocking.

None of this is to say that the manuscript would not benefit from the use of more precise language. We wholly agree that the key assumption of the PGW approach concerning the synoptic evolution is something that we want to carefully and deliberately acknowledge in our framing of the results. We have addressed the specific examples cited by the reviewer by either noting that the observed differences are conditional on the ERA5 2000-2019 circulation or by describing the differences under the more constrained phrasing of the "preindustrial background state" rather than the more exhaustive reference to under "preindustrial conditions." We have also closely reviewed the remainder of the manuscript for other opportunities to appropriately constrain our language.

Lloyd, E. A., & Oreskes, N. (2018). Climate change attribution: when is it appropriate to accept new methods?. Earth's Future, 6(3), 311-325.

Second, I have a methodological question related to that the PGW perturbations applied at the MAR boundaries are constructed from long-term monthly-mean CESM-LE differences and then linearly interpolated to 6-hourly forcing. To me, this approach yields a regime-invariant mean-state shift and may therefore miss regime-dependent thermodynamic changes that are particularly relevant for Greenland melt and runoff (often a small number of high-impact events related to blocking or moisture intrusions). A single monthly-mean Δq applied to all synoptic situations can misrepresent how moisture and cloud emissivity change specifically during blocking/moisture intrusion events. In addition, extremes are not expected to scale with the mean: any change in the variance/skewness of specific humidity, temperature or coastal inversion will be missed by a monthly-mean shift. Also, a monthly mean perturbation does not preserve event-specific vertical gradients, which may influence cloud phase, boundary-layer stability and downwelling longwave. If the added/removed moisture in PGW1 is not representative during the high-impact synoptic regimes, then the inferred thermodynamic contribution might be biased high or low to some extent. As the manuscript emphasizes a moisture/downwelling-longwave pathway as a key mechanism, I would suggest demonstrating that the imposed monthly Δq (and associated radiative effects) remains representative during the circulation regimes that dominate extreme SMB anomalies, including the extreme years in 2012 and 2019. The authors could condition key diagnostics (near-surface q, integrated water vapor, downwelling longwave or melt onset) on circulation regimes (blocking days vs moisture-intrusion days) to show that the thermodynamic signal is robust across regimes; or provide sensitivity tests using alternative ways of the perturbation (seasonal mean deltas or smoothed daily climatological deltas) to assess how sensitive the ~62% percent reduction is to the monthly-mean assumption. In any case, some discussion on that the current method does not capture changes in the variance could help.

We agree that the reviewer raises an important point that should be clearly addressed in the presentation of our methodology. The PGW method can be thought of as a hybrid downscaling approach, where the change factor / delta method of statistical downscaling is applied at the boundaries of a limited-area model that is then used to dynamically downscale the adjusted boundary fields. The use of numerical modeling does address some of the shortcomings of the change factor method, including the assumption of stationarity, by explicitly resolving nonlinear processes within the model domain. Our application of the PGW method was somewhat unique and more in line with the so-called storyline approach detailed in Lloyd and Oreskes (2018) in that preserving the 2000-2019 synoptic variability at the lateral boundaries was a priority of our study design so that we could conduct a direct comparison between the historical impact on GrIS SMB and what the impact would have been if it occurred under preindustrial background conditions. So, what is typically thought of as a weakness of the PGW method for assessing future impacts of climate change worked as a benefit for certain aspects of our specific application.

However, the reviewer highlights an important shortcoming inherent to the approach that has implications for the interpretation of our results–while the atmospheric state is free to adjust within the model domain, the application of monthly-mean deltas may not fully capture differences that would be present at the model boundaries during such extreme, high-impact events. For example, the magnitude of ΔQ within the core of an atmospheric river might be expected to be greater than the monthly mean ΔQ that was used to alter the strength of the atmospheric river at the lateral boundaries. With this in mind, we employed a larger integration domain than is typically used for MAR simulations of Greenland Ice Sheet SMB to provide more of a buffer for the model to resolve some of these nonlinear processes while still constraining the model boundaries enough to preserve the large-scale circulation. Despite these efforts, it is likely any biases present at the lateral boundary would be preserved to some extent and be conveyed to the ice sheet, impacting the modeled SMB response.

Only a global model could account for any regime dependent variability in ΔQ; however, the course spatial resolution of GCMs has been shown to cause severe underrepresentation of both the magnitude and frequency of atmospheric rivers (Wang et al., 2023). The PGW approach corrects for this by using a higher-resolution, limited-area model, but this comes with certain trade-offs as noted by the reviewer. This is a criticism that we argue would apply to any application of the PGW or related methods of downscaling. For example, Delhasse et al. (2018) forced MAR with ERA5 data that had been adjusted by applying fixed ΔT perturbations to estimate what future GrIS mass loss would be if the recent increase in Greenland blocking frequency, which is not represented in GCMs, persisted through the mid-21st Century. Others have used the PGW method to estimate future changes in the hydroclimate of western North America,

where annual precipitation is disproportionately dependent on moisture delivered via atmospheric rivers (Ikeda et al., 2021; Li et al., 2019; Musselman et al., 2018). While we agree that a quantitative assessment of the impact of applying a regime-invariant mean state shift for these applications would be an interesting and worthwhile endeavor, we believe that the broad implications of such an analysis would be beyond the scope of this paper.

We are very appreciative of the reviewer's critique, as it motivated us to more thoroughly weigh the implications for our results. We have added the following text to the fourth paragraph of section 4 to explicitly note that the assumption of a mean state shift could lead to biases in our estimate of the thermodynamic contribution to historical mass loss and to acknowledge that this simplification may also help explain the disparity between the thermodynamic contribution over the study period as a whole versus that during the anomalous melt years of 2012 and 2019:

"This signal may be due in part to biases inherent to the PGW approach. The application of a monthly mean climate perturbation may underrepresent the true change in air temperature and water vapor concentration during extreme events such as the bocking episodes and attendant atmospheric rivers that have promoted melt of the ice sheet. While the thermodynamic fields are free to adjust in accordance with any relevant nonlinear processes within the MAR integration domain, it is likely that any biases at the model boundaries would be conveyed to the ice sheet to some extent. Despite these shortcomings, the PGW method of downscaling is recognized as an effective means of isolating the thermodynamic component of climate change (Gutmann et al., 2018; Lackmann, 2015; Mallard et al., 2013; Rasmussen et al., 2020)– an approach that has been advocated, particularly in cases of extreme events for which the governing dynamics are not well represented in the models (Lloyd and Oreskes, 2018; Trenberth et al., 2015), which is true of both atmospheric rivers and atmospheric blocking (Delhasse et al., 2021; Hanna et al., 2018a; Wang et al., 2023; Woollings et al., 2018)."

Delhasse, A., Fettweis, X., Kittel, C., Amory, C., and Agosta, C.: Brief communication: Impact of the recent atmospheric circulation change in summer on the future surface mass balance of the Greenland Ice Sheet, The Cryosphere, 12, 3409–3418, https://doi.org/10.5194/tc-12-3409-2018, 2018.

Ikeda, K., Rasmussen, R., Liu, C., Newman, A., Chen, F., Barlage, M., Gutmann, E., Dudhia, J., Dai, A., Luce, C., and Musselman, K.: Snowfall and snowpack in the Western U.S. as captured by convection permitting climate simulations: current climate and pseudo global warming future climate, Clim Dyn, 57, 2191–2215, https://doi.org/10.1007/s00382-021-05805-w, 2021.

Li, Y., Li, Z., Zhang, Z., Chen, L., Kurkute, S., Scaff, L., and Pan, X.: High-resolution regional climate modeling and projection over western Canada using a weather research forecasting model with a pseudo-global warming approach, Hydrology and Earth System Sciences, 23, 4635–4659, https://doi.org/10.5194/hess-23-4635-2019, 2019.

Musselman, K. N., Lehner, F., Ikeda, K., Clark, M. P., Prein, A. F., Liu, C., Barlage, M., and Rasmussen, R.: Projected increases and shifts in rain-on-snow flood risk over western North America, Nature Clim Change, 8, 808–812, https://doi.org/10.1038/s41558-018-0236-4, 2018.

Wang, S., Ma, X., Zhou, S., Wu, L., Wang, H., Tang, Z., Xu, G., Jing, Z., Chen, Z., and Gan, B.: Extreme atmospheric rivers in a warming climate, Nat Commun, 14, 3219, https://doi.org/10.1038/s41467-023-38980-x, 2023.

Third, I would be cautious about interpreting the results from the current experimental design as sea-surface conditions exert only a minimal influence on SMB. I think so, because of a lack of atmosphere-only runs (retain CTRL sea-surface conditions (ERA5 SST/SIC) rather than swapping to Hadley-OI 1880-1899 SST/SIC). The manuscript interprets PGW1 as the total thermodynamic contribution and PGW2 as the SST/SIC-only contribution. However, without an atmosphere-only PGW experiment (T/q perturbed with modern SST/SIC), it is not possible to say whether marine boundary effects depend on the atmospheric background state (or to quantify the interaction term between marine boundary changes and the atmospheric background state).

Once PGW_atm exists, you can do the separation: CTRL (modern atm + modern SST/SIC), PGW2 (modern atm + PI SST/SIC), PGW_atm (PI atm + modern SST/SIC), and PGW1 (PI atm + PI-like SST/SIC), one can estimate

an atmosphere-only main effect as PGW_atm minus CTRL
a marine-only main effect as PGW2 minus CTRL
a nonlinear interaction term as PGW1 minus PGW_atm minus PGW2 plus CTRL

The interaction term is especially important given the authors current discussion of katabatic wind adjustments and marine-gradient effects (PGW2) versus water-vapor/longwave effects (PGW1): those pathways might not add linearly. Should the full 2000–2019 reruns be too expensive, a compromise is to run PGW_atm for (a) May–Sep only, or (b) a targeted set of years (e.g., 2012, 2019 plus a few 'typical' years) and show whether the inferred “minimal marine influence” and melt-timing conclusions hold once the interaction term is quantifiable.

We are grateful for the reviewer’s acknowledgment of the issue of computational expense. A full PGW_atm run (20 year analysis period plus 5 years of model spin up) would be a substantial undertaking. Unfortunately, running only a subset of months or years would not be possible under our study design because it would not include the requisite model spin-up time, as the state of the modeled snow profile at any point in time is reflective of the cumulative atmospheric forcing up to that point. Referring to the possibility of a single year run of 2012 as an example, we would need to initialize the

snow model at the start of the simulation on Jan 1, 2012. If completing a PGW_atm simulation of 2012 alone, we would need to use Dec 31, 2011 model output from either the control simulation or one of the other PGW simulations, neither of which would be consistent with a snow profile that was allowed to evolve under adjusted atmospheric thermodynamic conditions alone (i.e. PGW_atm). This same limitation applies to the simulation of a seasonal subset.

Our original intent when we designed PGW2 was to build upon previous assessments of the ice sheet's sensitivity to local sea-surface conditions (SSC) by examining the impact of changes in SSC that more closely resembled observed changes since the preindustrial period while also examining impacts on melt timing—an angle that was motivated by the observational results of Stroeve et al. (2017). While the reviewer is correct that we cannot rule out the possibility that nonlinear effects of combined changes in sea-surface and atmospheric conditions may produce a greater response than sea-surface conditions alone, we believe that our analysis does present a valuable addition to the literature that is consistent with a significant body of work demonstrating that the interiors of both the Greenland and Antarctic Ice Sheets are effectively insulated from direct forcing by the local marine environment (Hanna et al., 2009, 2014; Kittel et al., 2018; Noël et al., 2014).

We have expanded our discussion to acknowledge that our results do not directly examine the potential influence of nonlinear interactions between combined changes in sea-surface and atmospheric thermodynamic conditions, nor does it examine the possibility of indirect effects of changing SSC via alteration of the large-scale flow. We have also explicitly noted that these shortcomings – combined with any biases in model representation of ocean-atmosphere coupling as raised by Reviewer 2 – may explain the discrepancy between our findings at that of prior observational work (e.g., Rennermalm et al, 2009; Stroeve et al., 2019). We also noted that our results might not have yielded the same response as was suggested by PGW1 if it had not also included the change in SSCs.

Thanks to your feedback and that of Reviewer 2, we realize that the discussion of our results in relation to those of Stroeve et al. (2017) was too absolute and overlooked important factors that could account for our conflicting results. We have closely reviewed the manuscript to ensure that we properly interpret the output of $\mathbf{PGW_{SSC}}$ (formerly PGW2) as indicating minimal direct thermodynamic contribution of local sea-surface conditions (i.e., absent any nonlinear interactions between ocean and atmosphere or any indirect contributions via alteration of the large-scale atmospheric circulation) rather than the more conclusive phrasing that local sea-surface conditions exert minimal influence on the SMB.

As mentioned in our response to Reviewer 2, motivated by your comments and those of Reviewer 2, our more careful consideration of our results in the context of those of Stroeve et al. (2017) illuminated the congruence between the two studies and highlighted what we believe to be a potential key mechanism in explaining the earlier melt onset

following years of low Baffin Bay SIC. We have copied the relevant excerpt of our added discussion from section 4 below:

"Lastly, our analysis did not examine any indirect effects via alteration of the large-scale circulation by changes in SSCs. Both model- and observation-based studies have yielded evidence of a link between declining sea ice in Baffin Bay and the observed increase in summer Greenland blocking (Liu et al., 2016; Screen, 2013; Sellevold et al., 2022; Wu et al., 2013). Indeed, Stroeve et al. (2017) found that the statistical relationship between ice sheet melt and Baffin Bay SIC weakened considerably and, consistent with the $\mathrm{PGW_{SSC}}$ response in Fig. 5c, became more confined to the periphery of the western ice sheet after correcting for the influence of the Greenland blocking. Considered in conjunction with our results, this suggests that this indirect pathway of influence could help explain the statistical relationship between SSCs and early melt onset."

Hanna, E., Cappelen, J., Fettweis, X., Huybrechts, P., Luckman, A., and Ribergaard, M. H.: Hydrologic response of the Greenland ice sheet: the role of oceanographic warming, Hydrological Processes, 23, 7–30, https://doi.org/10.1002/hyp.7090, 2009.

Hanna, E., Fettweis, X., Mernild, S. H., Cappelen, J., Ribergaard, M. H., Shuman, C. A., Steffen, K., Wood, L., and Mote, T. L.: Atmospheric and oceanic climate forcing of the exceptional Greenland ice sheet surface melt in summer 2012, International Journal of Climatology, 34, 1022–1037, https://doi.org/10.1002/joc.3743, 2014.

Kittel, C., Amory, C., Agosta, C., Delhasse, A., Doutreloup, S., Huot, P.-V., Wyard, C., Fichefet, T., and Fettweis, X.: Sensitivity of the current Antarctic surface mass balance to sea surface conditions using MAR, The Cryosphere, 12, 3827–3839, https://doi.org/10.5194/tc-12-3827-2018, 2018.

Noël, B., Fettweis, X., van de Berg, W. J., van den Broeke, M. R., and Erpicum, M.: Sensitivity of Greenland Ice Sheet surface mass balance to perturbations in sea surface temperature and sea ice cover: a study with the regional climate model MAR, The Cryosphere, 8, 1871–1883, https://doi.org/10.5194/tc-8-1871-2014, 2014.

Rennermalm, A. K., Smith, L. C., Stroeve, J. C., and Chu, V. W.: Does sea ice influence Greenland ice sheet surface-melt?, Environ. Res. Lett., 4, 024011, https://doi.org/10.1088/1748-9326/4/2/024011, 2009.

Stroeve, J. C., Mioduszewski, J. R., Rennermalm, A., Boisvert, L. N., Tedesco, M., and Robinson, D.: Investigating the local-scale influence of sea ice on Greenland surface melt, Cryosphere, 11, 2363–2381, 2017.

Additionally, a comparison between 500hPa geopotential heights or the GBI between the CTRL and all PGW runs would be nice. Just to rule out that by adding a vertically varying T/q perturbation does not influence u/v (by changing pressure gradients) too much (which is assumed in the current manuscript).

The GBI domain does not fall entirely within the lateral boundaries of our MAR simulations. As an alternative, we have included the following comparisons of the area-weighted 500 hPa height between 60-80°N and 20-65°W, which is the latitude-longitude subset of the GBI that does fall within the integration domain as the new Supplementary Figure 1. The comparisons show strong temporal covariability between the model runs, with the most notable difference being that the average 500 hPa height in PGW1 is consistently lower than that of the control, consistent with a colder layer-mean air temperature. We discuss this comparison at the end of the second paragraph of section 2.

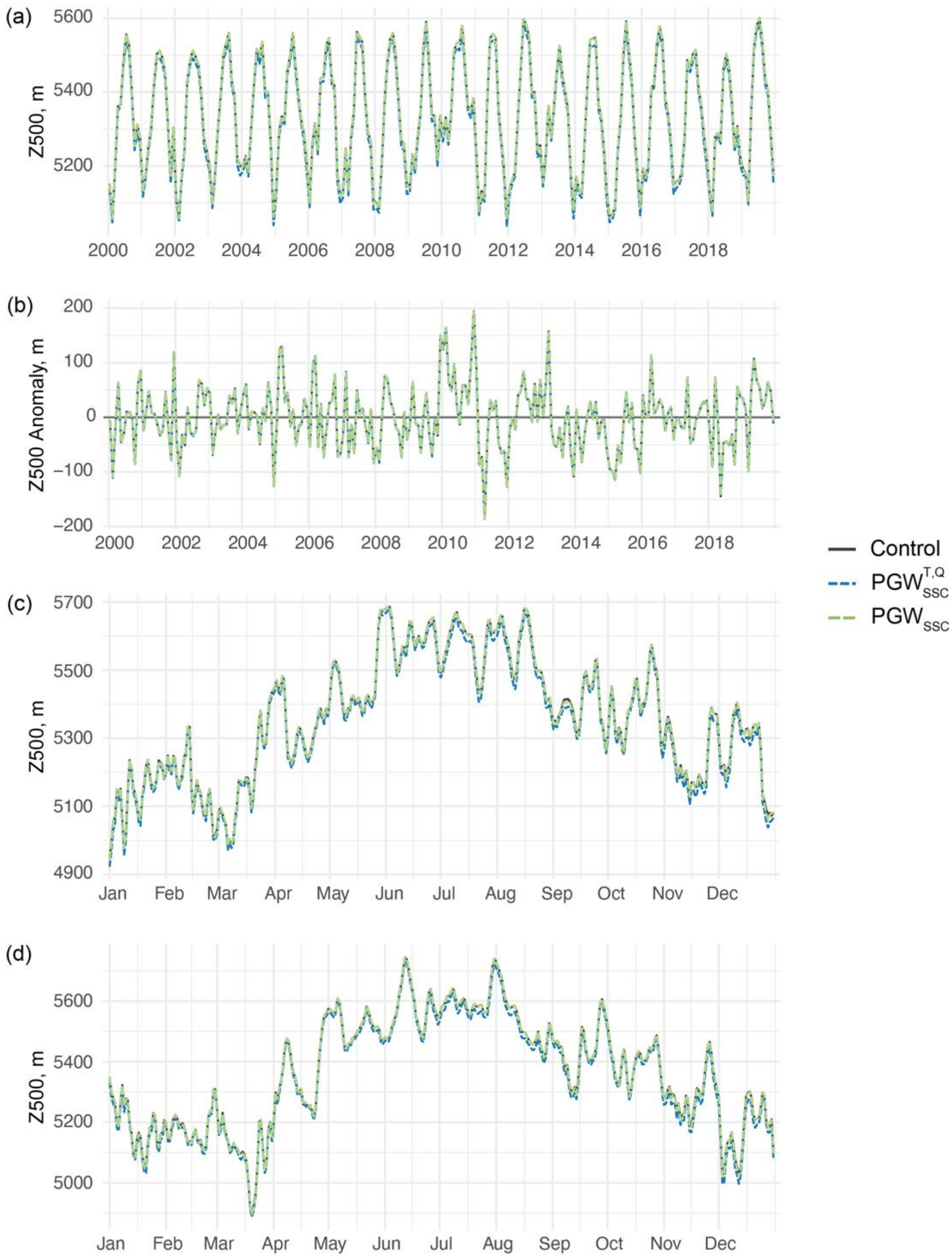


Figure S1. Comparison of the large-scale circulation between MAR simulations. Time series plot the latitude-weighted mean 500 hPa geopotential height (Z500) within a domain spanning 60-80°N and 20-65°W for the control (black, solid), $PGW_{SSC}^{T,Q}$ (blue, dashed), and PGW_{SSC} (green, dashed). Comparisons are shown for (a) monthly mean values of Z500 spanning the full 2000–2019 study period, (b) monthly-mean Z500 anomalies calculated with respect to the 2000–2019 long-term monthly mean for each simulation, and daily values of Z500 for the exceptional melt years of (c) 2012 and (d) 2019.

My last concern is that CTRL uses ERA5 sea-ice concentration as the modern forcing baseline, and any systematic bias in ERA5 SIC relative to satellite-based products (NSIDC) would directly affect the magnitude of PGW2-minus-CTRL (potentially biasing the conclusion toward a small marine effect if CTRL SIC is already too ice-covered, or inflating it if too low). While Fig. 3 usefully motivates why applying CESM SIC deltas directly can create ice-edge artifacts, it does not validate the modern SIC forcing against independent satellite products. I would suggest including a brief evaluation of ERA5 SIC against a satellite benchmark in the key seas and seasons that matter for Greenland moisture intrusions.

Sea ice concentration in ERA5 is prescribed using EUMETSAT's Ocean and Sea Ice Satellite Application Facility (OSI SAF) data (Hershbach et al., 2020). As is the case for the NSIDC Climate Data Record of SIC, the OSI SAF record consists of passive microwave satellite measurements. Thus, we would expect any differences between ERA5 SIC and the NSIDC CDR to be minimal and largely due to differences in the passive microwave algorithms employed by OSI SAF and NSIDC.

Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz-Sabater, J., Nicolas, J., Peubey, C., Radu, R., Schepers, D., Simmons, A., Soci, C., Abdalla, S., Abellan, X., Balsamo, G., Bechtold, P., Biavati, G., Bidlot, J., Bonavita, M., Chiara, G. D., Dahlgren, P., Dee, D., Diamantakis, M., Dragani, R., Flemming, J., Forbes, R., Fuentes, M., Geer, A., Haimberger, L., Healy, S., Hogan, R. J., Hólm, E., Janisková, M., Keeley, S., Laloyaux, P., Lopez, P., Lupu, C., Radnoti, G., Rosnay, P. de, Rozum, I., Vamborg, F., Villaume, S., and Thépaut, J.-N.: The ERA5 global reanalysis, Quarterly Journal of the Royal Meteorological Society, 146, 1999–2049, https://doi.org/10.1002/qj.3803, 2020.