

Atmospheric rivers and winter sea ice drive recent reversal in Antarctic ice mass loss

Corresponding Author: Dr Marlen Kolbe

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

**** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors ****

Dear Dr. Kolbe,

Your manuscript titled "Atmospheric rivers and winter sea ice drive recent reversal in Antarctic ice mass loss" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised substantial concerns that must be addressed. In light of these comments, extensive revisions will be required before we can further consider the manuscript for publication. We would, however, be interested in considering a revised version that fully addresses these serious concerns.

In particular, please ensure that the revised manuscript meets the following editorial thresholds:

1. Present a compelling case that your conclusions regarding the impacts of sea ice on the surface mass balance are robust and fully supported by your analyses.
2. Ensure that you fully describe and justify your climate model experiments regarding the influence of sea ice on the surface mass balance.
3. Ensure that your manuscript demonstrates a clear and significant contribution beyond the scope of existing literature, highlighting its novelty and relevance within the field.

We hope you will find the reviewers' comments useful as you decide how to proceed. If additional work allows you to either incorporate or refute these criticisms, we will be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Shin Sugiyama, PhD
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Nicola Colombo, PhD
Associate Editor, Communications Earth & Environment
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of "Atmospheric rivers and winter sea ice drive recent reversal in Antarctic ice mass loss" by Marlen Kolbe et al.

Communications Earth & Environment: COMMSENV-25-4371

General comments:

In this paper, the authors utilize the gravitational mass balance data derived from GRACE/GRACE-FO satellite data provided by TU Dresden, and report that the Antarctic ice sheet (AIS) has been gaining snow and ice mass since 2020 with a significant 5-year (2020-2024) mass gain trend of 67.53 ± 31.4 Gt/y, although AIS has lost substantial mass at a near-linear rate from 2000 to 2020. The authors attempt to clarify the causes of the recent mass gain through the following three steps:

1. The authors investigate data from Davison et al. (2023, cited in this paper) on ice discharge from AIS and find that dynamically driven ice loss by discharge has continued to increase over the past five years.
2. The authors examine SMB data derived from the ERA5 reanalysis, the regional climate model RACMO2.4p, and the regional climate model HCLIM-AROME, and confirm that SMB has been increasing in the recent five years due to increased precipitation linked to increased atmospheric river (AR) activity. In this part, HCLIM-AROME model simulations were performed by the authors, and the AR detection method (ANTIS-AR) was also developed by them.
3. The authors investigate the impacts of the recent reduction in sea ice area extent around AIS on the increase in precipitation by conducting idealised numerical model simulations with HCLIM-AROME. This is because the loss of sea ice around AIS can provide more moisture to the atmosphere from the ocean surface, which is expected by many model-based studies. Based on the idealised model simulations, the authors conclude that the impacts of surrogate sea ice are minor compared to remote large-scale processes such as AR.

Based on the considerations mentioned above, the authors conclude that the recent increase in AR activities around AIS is the key process driving the recent AIS mass increase.

Overall, the topic presented in this paper is timely and new. The manuscript is generally easy to follow. The recent increase in the AIS mass balance from 2020 to the present could impact the community because it substantially affects the global sea level, although it is uncertain whether this trend will continue in the near future. For Steps 1 and 2 indicated above, I think the authors' arguments are robust because the data and analysis methods are solid. However, for Step 3, I have some concerns about the methods employed by the authors as listed below (Please also see my specific major comments). I agree with the authors that it is imperative to consider the possible effects of surrogate sea ice on the recent rapid increase in precipitation over AIS to obtain robust conclusions of this study. Therefore, I think Step 3 cannot be removed, and it is necessary to revise the step substantially.

Specific comments (major):

L. 89 “short-term, high-impact events”: Can the authors define them quantitatively? I think such quantitative information can be useful for readers.

L. 157 “Idealised Experiments”: In my humble opinion, the authors do not provide sufficient information about the model settings employed in the idealized experiments. Without such information, readers cannot assess the validity/reliability of the idealised experiments and their qualitative and quantitative results (L. 166 ~ 239). I have the following questions about the model settings:

- In the experiment with added sea ice (exICE, where sea ice concentrations are increased to 100% up to 5°S north of the monthly mean SIE), how did the authors assign sea ice surface temperature for the added sea ice? Did the authors modulate the ocean internal water temperature below the added sea ice? Why did the authors select the value of 5°S north?

- In the experiment with completely removed sea ice (noICE), how did the authors assign the SST and albedo values for the areas originally covered with sea ice? Did the authors modulate ocean internal water temperature as well?

- What kind of boundary conditions did the authors choose for the HCLIM43-AROME? Are they the same as the CTRL run? If so, the model must be strongly constrained by the ERA5 reanalysis, meaning that the responses of atmospheric conditions to changes in sea ice conditions can be weak.

- Do the authors think that only two idealised simulations are enough to obtain a robust qualitative conclusion about the impacts of sea ice on the AIS SMB? The results presented in this paper can be coincidental due to the initial conditions. I think it is necessary to perform more simulations where initial sea ice physical conditions and distributions are slightly modified, and then present the ensemble mean of these simulations.

- I know the study by Kittel et al. (<https://doi.org/10.5194/tc-12-3827-2018>), where Kittel et al. investigate the direct sensitivity of the AIS SMB to sea surface conditions perturbations around the Antarctic. The authors must discuss the differences in the methods between Kittel et al. and this study. It seems to me that the method employed by the authors is much simpler than the method by Kittel et al. Therefore, I think the authors must justify their simple method.

L. 196 ~ 197 “higher annual mean land temperatures result primarily from increased net solar radiation (Figure A14) due to lower albedo”: Please provide information about the albedo scheme employed in HCLIM43-AROME for this study.

L. 341 “convection-permitting”: In general, a “convection-permitting” atmospheric model requires a horizontal resolution of the model < 4 km (Prein et al., 2015, <https://doi.org/10.1002/2014RG000475>). I understand that the horizontal resolution of HCLIM43-AROME can be enhanced as high as < 4 km; however, in this case (horizontal resolution is 11 km), the term “convection-permitting” should be removed.

L. 358 ~ 364: How did the authors set the snow and ice internal physical conditions over the ice sheet? What ice sheet mask did the authors utilize? Please explain.

Specific comments (minor):

L. 146 ~ 147 “We find that SIC correlations with SMB are generally stronger over ice shelves (Figure A10)”: Compared to the inland ice sheet? Please clarify. Also, is this the case for MAM? For SON and DJF, it is difficult for me to see the statement in Fig. A10.

L. 310 ~ 311: Please indicate the “top” value used in Eq. (2) to allow readers to apply the method to other datasets. Related to this point, Mattingly et al. (2018, <https://doi.org/10.1029/2018JD028714>) used P-level values over the 1000-200 hPa atmospheric layers to detect Atmospheric Rivers in the polar regions. I think the authors must explain and justify why they use the model-level data instead of the P-level data and set the “top” value in this study.

L. 348 ~ 357: This list, which indicates the HCLIM43-AROME model physics, is too simple; therefore, it is not helpful for readers who are not familiar with the HCLIM43-AROME model. Please describe the key features of each process briefly.

Technical corrections:

L. 67: It seems to me that “GMB” has not been defined in the running text yet. Please introduce the definition of GMB,

“Gravitational Mass Balance,” at an appropriate place in the running text.

L. 87 “(Zwally drainage basins 19-21)”: Where are the areas? A figure that shows the Zwally drainage basins “19-21” is needed.

L. 94 ~ 96 “The summer increase is most pronounced over West Antarctica and the AP, whereas winter SMB has risen mainly along the East Antarctic coastline and tip of the AP.”: This is an interesting and meaningful message in my opinion. I would start a new paragraph with this sentence to highlight the message.

L. 139: Please define “SIC” here. In the current version of this paper, I cannot find the definition of SIC in the running text.

L. 303: The definitions of ANTIS-AR and EDARA are missing.

Figure 1a: It seems to me that an explanation of the solid gray line is missing.

Figures 1c and 1f: Which SMB data did the authors use for these figures?

Figure 2: The data source for daily precipitation over the AIS should be explained in the caption.

Figures A3, A9, A10: The data source for SMB should be explained in the caption.

Figures 3, A11 ~ A19: It is better to indicate in the captions that the contents are based on the HCLIM43 experiments.

Figure A10 caption: Suggest rephrasing “Yellow patterns over land and blue over ocean ~ ” to something like “Yellow patterns over ice sheet and blue over ocean ~ .” Ice shelves are not on land.

Reviewer #2 (Remarks to the Author):

This manuscript investigates the potential causes of the recent slowdown in Antarctic Ice Sheet (AIS) mass loss after 2016. The authors use reanalysis data to link AIS mass balance anomalies to the increased frequency of atmospheric rivers (ARs) since 2020. In addition, climate models are employed to quantify the role of sea ice in ice-mass gain through its influence on precipitation. The study provides useful insights into the role of atmospheric rivers over Antarctic ice shelves and their interactions with sea ice.

Overall, the topic is of high scientific interest and relevant to the journal’s readership. However, several aspects require clarification and improvement before the manuscript can be considered for publication. Specific comments are provided below.

Major Comments

1. Lines 33–60:

The authors are encouraged to clarify the knowledge gap between this study and previous work. It appears that earlier studies have already discussed the slowdown of AIS mass loss and its linkage with ARs. Does this study extend the analysis period, introduce the recent increasing trend observed during the past five years, or provide the first quantitative connection between AIS mass gain and sea ice? Clarifying this will help position the novelty of the research more clearly.

2. Figure 1:

Figure 1 effectively illustrates the recent increase in AIS mass, but it currently contains limited information, while too many key details are placed in the Supplementary Information. This separation may be inconvenient for readers. The authors are encouraged to overlay the increase in AR frequency (2020–2024) and the westerly trend on the AIS mass time series and to elaborate on their consistency and respective roles in the “Recent Increase in Antarctic Ice Mass” section. Including regional divisions in this figure would also improve clarity.

3. Section “Regional Accumulation Patterns Shaped by SAM, ENSO, and ARs”:

a. This section could be improved by explicitly showing the regions discussed. The authors might consider dividing Antarctica into several regions and presenting the correlations between SAM/ENSO and SMB in a table, together with their trends and relationships, to make the results more straightforward. Including this table in the main text would enhance readability.

b. Atmospheric river (AR) activity in East Antarctica is known to correlate strongly with SAM (Pohl et al., 2021; Shields et al., 2022). The relatively low correlations reported here may arise from a lead–lag relationship between SAM and ENSO, as noted by King et al. (2023). The authors are encouraged to reexamine this relationship.

Reference:

Shields, C. A., Wille, J. D., Marquardt Collow, A. B., MacLennan, M., & Gorodetskaya, I. V. (2022). Evaluating uncertainty and modes of variability for Antarctic atmospheric rivers. *Geophysical Research Letters*, 49, e2022GL099577.

<https://doi.org/10.1029/2022GL099577>

Pohl, B., Favier, V., Wille, J., Udy, D. G., Vance, T. R., Pergaud, J., et al. (2021). Relationship between weather regimes and atmospheric rivers in East Antarctica. *Journal of Geophysical Research: Atmospheres*, 126, e2021JD035294.

<https://doi.org/10.1029/2021JD035294>

King, M. A., Lyu, K., & Zhang, X. (2023). Climate variability: A key driver of recent Antarctic ice-mass change. *Nature Geoscience*, 16, 1128–1135. <https://doi.org/10.1038/s41561-023-01317-w>

c. The logic regarding sea ice in this section is somewhat unclear. It would be helpful to strengthen the rationale for including sea ice here and to explain why its contribution is emphasized, given that its influence may be smaller compared to large-scale circulation changes.

4. Figures A9–A10:

These figures serve as the basis for the modeling section. However, regional variations in sea ice are likely to contribute mainly to regional changes in SMB, and these effects may vary considerably among basins. This spatial heterogeneity could explain the limited correlations. The sparse significant signals observed may not sufficiently justify the subsequent modeling. The authors may consider applying methods such as Singular Value Decomposition (SVD) to identify coherent SIC–SMB patterns, and then relate the resulting time series to large-scale circulation indices (e.g., SAM and ENSO).

Minor Comments

1. Lines 90–91: Showing the winter and summer averages for the two periods would make the conclusions clearer.

2. Figure 3d–3f: Consider adjusting the color bar to a narrower range to highlight smaller-value distributions.

3. Line 244: The conclusion that summer SMB increase is associated with positive SAM is valuable but not sufficiently emphasized in the results section. This connection should be more explicitly stated earlier in the manuscript (see Major Comments).

4. Figure A10: The correlation color bar over Antarctica is not clearly distinguishable, particularly for negative values. Please enhance its visual clarity.

Reviewer #3 (Remarks to the Author):

This manuscript investigates the recent slowdown and reversal of Antarctic Ice Sheet (AIS) mass loss observed in satellite data. The authors analyze reanalysis products and regional climate model simulations to identify the drivers of this reversal. Their analysis attributes the positive anomalies in surface mass balance (SMB) to enhanced precipitation, which is closely linked to intensified atmospheric river (AR) activity and reduced sea ice extent. The manuscript is clearly structured, well written, and offers valuable insights on the mechanisms driving recent AIS mass changes. Only the analysis on the role of sea ice requires better justification. Beyond that, I only have a few minor comments for improvement.

L20-23: The causal chain in this sentence could be clarified for better logical flow. From my understanding, I suggest rephrasing it as: "...this reversal is primarily due to positive surface mass balance anomalies, which result from increased precipitation driven by enhanced atmospheric river activity, strengthened westerlies, and reduced sea ice extent."

L27: Should "the effect of sea ice" be "the effect of sea ice loss"?

L42: Similar to the comment above, the relationship between "increased precipitation" and "surface mass balance anomalies" is unclear here.

L67: "GMB" is not defined.

L73-76: Considering the known biases in ERA5 precipitation, it would be better to briefly explain in the main text how SMB is computed here (as described in the Methods) and the rationale of the approximation used. Additionally, since you do not clearly introduce what type of datasets RACMO2.4p and HCLIM43 are, it would be helpful to provide a very short description of each when first mentioned.

L85: The numbers of the supplementary figures are out of order throughout the manuscript.

L97-98: There's a lack of explanation here of how do strengthened westerlies impact the SMB.

L133: The transition from the previous section is not smooth. At L125, it states that "SAM and ENSO variability alone cannot fully explain the enhanced accumulation", which naturally leads the reader to expect a discussion of the drivers behind East Antarctic precipitation, SMB, and GMB changes. However, this section primarily focuses on the contribution of sea ice, even though the low correlation between inland East Antarctic SMB and local sea ice has already been noted. It is important to add clear transitional sentences to explain why the role of sea ice is being examined here.

L169-170: I didn't see the mean SIC in the figure.

L166-182: It is not clear whether these results explain the positive mass anomalies in East Antarctica, as the signal appears to be mostly appearing in West Antarctica.

Figure1: Why does the grey linear regression line "twist" during 2016-2020?

L282-285: Would be better to add some citations to support the choice of ERA5 for the analysis.

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Version 1:

Decision Letter:

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Dear Dr Kolbe,

Your manuscript titled "Atmospheric rivers and winter sea ice drive recent reversal in Antarctic ice mass loss" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Shin Sugiyama, PhD
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Nicola Colombo, PhD
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of "Atmospheric rivers and winter sea ice drive recent reversal in Antarctic ice mass loss" by Marlen Kolbe et al.

Communications Earth & Environment: COMMSENV-25-4371A

General comments:

I would like to thank the authors for considering my review and revising the paper. The authors' responses to my earlier concerns are convincing. Therefore, I recommend this paper's publication in this journal.

Reviewer #2 (Remarks to the Author):

The authors thoroughly addressed all my comments and suggested revisions. I would be happy to see this manuscript published in its current form.

Reviewer #3 (Remarks to the Author):

I appreciate the authors' efforts in addressing my previous comments. The transitions are now smoother, and the role of sea ice is better justified. Overall, the manuscript is well structured, clearly written, and provides valuable insights into the mechanisms driving recent Antarctic Ice Sheet mass changes. I am satisfied with the revisions and believe the manuscript now meets the standard for publication.

EDITORS' COMMENTS:

Line 34-37: I think it makes more sense if these two sentences are swapped. Because there is redundancy, they can be combined into one sentence.

Line 40-41, 62, 68, 125: Please spell out acronyms for the first time (GRACE, GRACE-FO, EAIS, SMB, IVT).

Line 59: "11km" >> Put space between 11 and km. Please check out other places.

Line 69-71: What about "Kittel et al. (2018) [21] have assessed Antarctic SMB responses to sea ice concentration (SIC) and sea surface temperature (SST) by performing multi-decadal sensitivity experiments with a hydrostatic RCM with a 50 km resolution."?

Line 62-67: This part explains the novelty of this work. It is unusual to read about your own work in the middle of the introduction. What about combining these sentences with the last paragraph (Line 80-87)?

Line 190 and 219: I think these subheadings are not necessary.

Line 229-230: "SIC are increased to 100% up to 5 S north of the monthly mean SIE" >> I do not understand this procedure. According to the Method section, maybe "5 degrees S" should be "5 degrees"?

Figure 1a and b: Quantities are missing from the vertical axis labels. I understand they are indicated as legends and in the plot titles, but a more usual manner is to indicate them next to the axes with units (e.g. Cumulative mass balance anomaly (Gt), Zonal wind speed (m s⁻¹), AR frequency (yr⁻¹)). Please consider revision for these and other plots.

Figure 5a and f: The numbers next to the color scale bar are odd (e.g. 22, 788, 1554 ...).

Line 330 and 336: The GIA is spelt out twice.

Line 378-382: Variable definitions are repeated several times.

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Reviewer #1

General comments

In this paper, the authors utilize the gravitational mass balance data derived from GRACE/GRACE-FO satellite data provided by TU Dresden, and report that the Antarctic ice sheet (AIS) has been gaining snow and ice mass since 2020 with a significant 5-year (2020-2024) mass gain trend of 67.53 ± 31.4 Gt/y, although AIS has lost substantial mass at a near-linear rate from 2000 to 2020. The authors attempt to clarify the causes of the recent mass gain through the following three steps:

1. The authors investigate data from Davison et al. (2023, cited in this paper) on ice discharge from AIS and find that dynamically driven ice loss by discharge has continued to increase over the past five years.
2. The authors examine SMB data derived from the ERA5 reanalysis, the regional climate model RACMO2.4p, and the regional climate model HCLIM-AROME, and confirm that SMB has been increasing in the recent five years due to increased precipitation linked to increased atmospheric river (AR) activity. In this part, HCLIM-AROME model simulations were performed by the authors, and the AR detection method (ANTIS-AR) was also developed by them.
3. The authors investigate the impacts of the recent reduction in sea ice area extent around AIS on the increase in precipitation by conducting idealised numerical model simulations with HCLIM-AROME. This is because the loss of sea ice around AIS can provide more moisture to the atmosphere from the ocean surface, which is expected by many model-based studies. Based on the idealised model simulations, the authors conclude that the impacts of surrogate sea ice are minor compared to remote large-scale processes such as AR.

Based on the considerations mentioned above, the authors conclude that the recent increase in AR activities around AIS is the key process driving the recent AIS mass increase.

Overall, the topic presented in this paper is timely and new. The manuscript is generally easy to follow. The recent increase in the AIS mass balance from 2020 to the present could impact the community because it substantially affects the global sea level, although it is uncertain whether this trend will continue in the near future. For Steps 1 and 2 indicated above, I think the authors' arguments are robust because the data and analysis methods are solid. However, for Step 3, I have some concerns about the methods employed by the authors as listed below (Please also see my specific major comments). I agree with the authors that it is imperative to consider the possible effects of surrogate sea ice on the recent rapid increase in precipitation over AIS to obtain robust conclusions of this study. Therefore, I think Step 3 cannot be removed, and it is necessary to revise the step substantially.

[Thank you for reviewing our article and giving valuable feedback. We have addressed the concerns and suggestions below.](#)

Specific comments (major)

1. L. 89 “short-term, high-impact events”: Can the authors define them quantitatively? I think such quantitative information can be useful for readers.

Yes, this is a good idea, we have added the following to the previous sentence (ll. 123) :

“Annual precipitation over the AIS is dominated by short-term, high-impact events, often defined as daily precipitation extremes exceeding the 95th–99th percentile and associated with ARs identified using IVT percentile thresholds (we use the 95th percentile), which typically produce 1–3-day landfall episodes [27]. ARs alone are estimated to contribute on average 10–30% of Antarctic precipitation [9, 28, 29].”

2. L. 157 “Idealised Experiments”: In my humble opinion, the authors do not provide sufficient information about the model settings employed in the idealized experiments. Without such information, readers cannot assess the validity/reliability of the idealised experiments and their qualitative and quantitative results (L. 166 ~ 239). I have the following questions about the model settings:

(Thank you for these questions. We have now answered each of the three points below in the Methods section of the manuscript and have referred to these details in the main manuscript).

3. In the experiment with added sea ice (exICE, where sea ice concentrations are increased to 100% up to 5°S north of the monthly mean SIE), how did the authors assign sea ice surface temperature for the added sea ice? Did the authors modulate the ocean internal water temperature below the added sea ice? Why did the authors select the value of 5°S north?

We include this now in ll. 458 :

“In the exICE experiment, where SIC is increased to 100% up to 5° north of the monthly mean SIE, the sea ice surface temperature is computed interactively by the thermodynamic sea ice scheme, SICE, implemented in HCLIM. SICE calculates the temperature profile within the ice by solving the heat-diffusion equation through a slab of prescribed thickness. SICE explicitly couples the ice to the snow layer through heat and momentum fluxes at the snow–ice interface. We chose 5° north of the monthly mean ice edge to represent a physically plausible but substantial perturbation to the sea ice boundary (see Figure 5a) [80].”

4. In the experiment with completely removed sea ice (noICE), how did the authors assign the SST and albedo values for the areas originally covered with sea ice? Did the authors modulate ocean internal water temperature as well?

We include this now in ll. 457:

“In the noICE experiment, the SSTs over areas originally covered by sea ice represent SSTs from ERA5 (below the sea ice cover), and the albedo was set to that of the open ocean (0.07-0.09).”

Adding a perturbation to the internal temperature structure would greatly increase the complexity of the experiment and introduce additional sources of forcing. We now also explain why we did not change SSTs in ll. 85:

“We chose not to modify SSTs to avoid adding an extra source of noise and an additional SST-driven thermodynamic signal.”

If we also increased the SSTs, the evaporation and precipitation would likely be higher, which is why we also add this part to the conclusion (ll. 313):

“Our experiments still confirm that Antarctic SMB does increase due to sea ice loss, even if relatively small, and we note that larger SST increases than imposed here could lead to more precipitation (and also enhanced ice melt).”

5. What kind of boundary conditions did the authors choose for the HCLIM43-AROME? Are they the same as the CTRL run? If so, the model must be strongly constrained by the ERA5 reanalysis, meaning that the responses of atmospheric conditions to changes in sea ice conditions can be weak.

We answer this in ll. 466:

“For all experiments (CTRL, exICE and noICE) we use ERA5 as lateral boundary conditions. This allows us to isolate the local atmospheric response but means that the large-scale atmospheric state is constrained for the two extreme perturbations.”

6. Do the authors think that only two idealised simulations are enough to obtain a robust qualitative conclusion about the impacts of sea ice on the AIS SMB? The results presented in this paper can be coincidental due to the initial conditions. I think it is necessary to perform more simulations where initial sea ice physical conditions and distributions are slightly modified, and then present the ensemble mean of these simulations.

We agree that multiple ensembles are needed to sample internal variability. Here however, our goal was to test the sensitivity of Antarctic precipitation to two extreme SIC states under the same boundary conditions. The Kittel et al. (2018) study mentioned below has already shown that the Antarctic climate responds to SIC/SST anomalies in a physically consistent way (warmer surfaces lead to higher moisture availability and more precipitation). Our two idealised experiments build on this result and represent two “what-if” extremes to capture the range of possible responses. The fact that the simulated changes indeed largely follow the expected physical relationship also suggests that the results are not due to initial-condition noise. We now clarify this motivation in the manuscript. As Kittel et al.s experiments showed that additionally increasing SST by a

substantial amount leads to higher AIS precipitation than just sea ice loss alone, we now also include the above-mentioned statement in the conclusion (last statement from question 4).

7. I know the study by Kittel et al. (<https://doi.org/10.5194/tc-12-3827-2018>), where Kittel et al. investigate the direct sensitivity of the AIS SMB to sea surface conditions perturbations around the Antarctic. The authors must discuss the differences in the methods between Kittel et al. and this study. It seems to me that the method employed by the authors is much simpler than the method by Kittel et al. Therefore, I think the authors must justify their simple method.

Thank you for bringing this study to our attention, we apologize for not having seen it earlier. We now introduce it in the Introduction section and explain the differences between their method and ours (ll. 69):

“Kittel et al. (2018) [21] have performed multi-decadal sensitivity experiments with a hydrostatic RCM at 50km using present-day modified sea ice concentration (SIC) and sea surface temperature (SST) to assess Antarctic SMB responses. The authors applied perturbations to SIC and SST, both separately and in combination, using ERA-Interim data. SIC was perturbed based on the minimum or maximum of the 3- and 6-cell neighborhood, and SST was perturbed by 2°C and 4°C for ice-free grid cells. The total SMB changes (including ice shelves) due to sea ice perturbations alone ranged from -6.6% (for higher SIC) to +3.5% (for lower SIC), increasing to +12.7% when combined with a +4°C SST perturbation. They also ran experiments prescribing SIC and SST fields from CMIP5 models, which resulted in similar magnitudes of SMB change.

In our experiment we explore two extreme sea ice states outside present-day variability in a non-hydrostatic high resolution (11km) RCM, driven by ERA5: a completely ice-free Southern Ocean (noICE) and a high-SIE scenario with 100% sea ice extending 5° equatorward of the ice edge (exICE). These experiments are conducted over a single year (July 2021 - June 2022), which includes four precipitation-heavy AR events, and we examine how these events are affected by the sea ice changes. We chose not to modify SSTs to avoid adding an extra source of noise and an additional SST-driven thermodynamic signal.”

Kittel et al. used more “samples,” which gives us additional confidence that our results are not random since the outcomes are qualitatively similar (note that they did not decrease or increase SIC as much as we did). Instead of running several years and many experiments at coarser resolution (50km in Kittel et al.), we used a 11km non-hydrostatic model.

8. L. 196 ~ 197 “higher annual mean land temperatures result primarily from increased net solar radiation (Figure A14) due to lower albedo”: Please provide information about the albedo scheme employed in HCLIM43-AROME for this study.

Thank you for suggesting this, we have added more information about the model physics including the albedo (ll. 434):

“Snow albedo is computed following the CROCUS method [77, 78] in three spectral bands ([0.3–0.8], [0.8–1.5], and [1.5–2.8] μm), which are combined to provide total broadband albedo. The default albedo varies with snow density, optical grain size, and snow aging, and sea ice thermodynamics are treated using the SICE module, which solves heat diffusion through a prescribed ice layer and couples to the overlying multilayer ISBA-3L snow scheme to represent vertical temperature gradients, metamorphism, accumulation, melt, and compaction.”

9. L. 341 “convection-permitting”: In general, a “convection-permitting” atmospheric model requires a horizontal resolution of the model < 4 km (Prein et al., 2015, <https://doi.org/10.1002/2014RG000475>). I understand that the horizontal resolution of HCLIM43-AROME can be enhanced as high as < 4 km; however, in this case (horizontal resolution is 11 km), the term “convection-permitting” should be removed.

Thank you for suggesting this, we have removed the term convection-permitting.

10. L. 358 ~ 364: How did the authors set the snow and ice internal physical conditions over the ice sheet? What ice sheet mask did the authors utilize? Please explain.

We have added this information to the Methods section (ll. 441:)

“The ice sheet mask was derived from BedMachine Antarctica [79]. The snow and ice internal physical conditions over the ice sheet were initialized according to the SURFEX/ISBA-ES land-surface scheme, which simulates the thermal and physical properties of the snow and ice layers (including snow temperature, density, compaction, and optical grain size) [75, 76].”

Specific comments (minor):

11. L. 146 ~ 147 “We find that SIC correlations with SMB are generally stronger over ice shelves (Figure A10)”: Compared to the inland ice sheet? Please clarify. Also, is this the case for MAM? For SON and DJF, it is difficult for me to see the statement in Figure A10.

Yes, compared to regions further inland SMB; we now rephrase this part and have changed the colorbar so that the correlations are clearer (ll. 207):

“We also find significantly positive SIC–SMB correlations over some ice shelf areas (especially from March to August; Figure A10), which suggests that some of the additional moisture from sea ice loss is precipitated out over the ice shelves before reaching the grounded AIS (this is also supported by our experiments below; Figure 5f).”

12. L. 310 ~ 311: Please indicate the “top” value used in Eq. (2) to allow readers to apply the method to other datasets. Related to this point, Mattingly et al. (2018, <https://doi.org/10.1029/2018JD028714>) used P-level values over the 1000-200 hPa atmospheric layers to detect Atmospheric Rivers in the polar regions. I think the authors must explain and justify why they use the model-level data instead of the P-level data and set the “top” value in this study.

We apologize that this was not clear enough. Yes, IVT is either calculated using selected pressure/model levels or using pre-integrated variables if they are available as for ERA5. Similar to other studies (two examples below; Krishna & Mager 2024 and Gimeno-Sotelo 2023) we used the two ERA5 variables “vertical integral of eastward/northward water vapour flux” (we define them as F_u and F_v before the mentioned equations), therefore the top value corresponds to the top of the model atmosphere (~0.1 hPa) and is not chosen by us. We tried to make this more clear by adding the part in bold (ll. 371:):

“For ANTIS-AR we use the vertical integrals of eastward (F_u) and northward (F_v) water vapour fluxes, which represent the total column-integrated moisture flux (in $\text{kg m}^{-1} \text{s}^{-1}$) from the surface to the top of the atmosphere. IVT is then calculated as

$$\text{IVT} = \sqrt{F_u^2 + F_v^2}$$

F_u and F_v are derived internally by ECMWF from model-level winds and specific humidity as:

$$(F_u, F_v) = -\frac{1}{g} \int_{p_{\text{sfc}}}^{p_{\text{top}}} (u, v) q dp$$

where u and v are the horizontal wind components, q is specific humidity, p is pressure, g is gravitational acceleration, and **p_{sfc} and p_{top} correspond to the variable surface pressure and the top (~0.1 hPa) of the model atmosphere. We preferred this method to integrating fixed pressure-level data because ERA5 extrapolates humidity and wind fields to the surface, which, if not adjusted to actual surface pressure, can lead to overestimation of IVT [61].”**

- Krishna, N., Kingston, D.G., & Mager, S.M. (2024). *Climatology and trends of atmospheric water vapour transport in New Zealand*. Theor Appl Climatol 155, 7757–7772. <https://doi.org/10.1007/s00704-024-05072-9>
- Gimeno-Sotelo, L. (2023). Where does the link between atmospheric moisture transport and extreme precipitation matter? Weather and Climate Extremes, 39, 100536. <https://doi.org/10.1016/j.wace.2022.100536>

13. L. 348 ~ 357: This list, which indicates the HCLIM43-AROME model physics, is too simple; therefore, it is not helpful for readers who are not familiar with the HCLIM43-AROME model. Please describe the key features of each process briefly.

We agree and have added a more detailed short explanation of the model features (ll: 419):

“The model employs a non-hydrostatic dynamical core with deep convection explicitly resolved. Shallow convection is parameterized using the EDMFm scheme [64, 65], which incorporates a dual mass-flux framework to represent both dry and moist updrafts [66]. Cloud processes are simulated using the statistical cloud scheme of Bechtold et al. (1995) [67]. Microphysics follow the ICE3 single-moment bulk scheme [68], predicting cloud water, rain, cloud ice, snow, and graupel. For cold conditions, ICE3 is used with the OCND2 modifications [69], which improve the representation of supercooled liquid water, fog, and mixed-phase cloud layers. Radiative transfer is handled using RRTM for longwave radiation [70, 71] and the six-band SW6 shortwave scheme [72], a simplified version of the ECMWF radiation scheme [73]. Turbulence is represented using the HARATU scheme [65, 74]. Surface processes are represented with the land-surface model SURFEX [75], which simulates snow on ice using the 12-layer Interactions Surface Biosphere Atmosphere Explicit Snow (ISBA-ES) model [76]. The topmost snow layer is always ≤ 0.05 m thick, and the scheme calculates snow thermal conductivity, radiative fluxes, accumulation, melt, and compaction across the full multilayer snowpack. Snow albedo is computed following the CROCUS method [77, 78] in three spectral bands ([0.3–0.8], [0.8–1.5], and [1.5–2.8] μm), which are combined to provide total broadband albedo. The default albedo varies with snow density, optical grain size, and snow aging and sea ice thermodynamics are treated using the SICE module, which solves heat diffusion through a prescribed ice layer and couples to the overlying multilayer ISBA-3L snow scheme to represent vertical temperature gradients, metamorphism, accumulation, melt, and compaction. The ice sheet mask was derived from BedMachine Antarctica [79]. The snow and ice internal physical conditions over the ice sheet were initialized according to the SURFEX/ISBA-ES land-surface scheme, which simulates the thermal and physical properties of the snow and ice layers (including snow temperature, density, compaction, and optical grain size) [75, 76].”

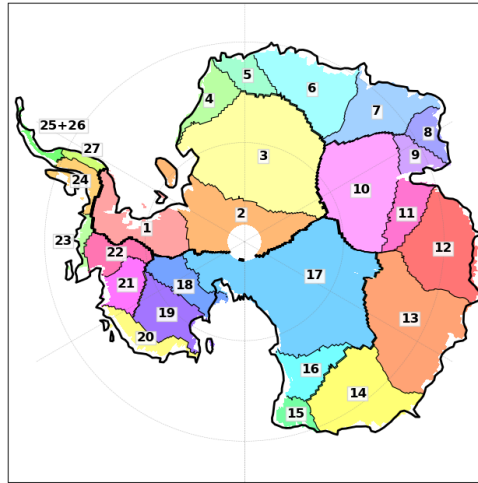
Technical corrections

14. L. 67: It seems to me that “GMB” has not been defined in the running text yet. Please introduce the definition of GMB, “Gravitational Mass Balance,” at an appropriate place in the running text.

Thank you for noticing this, we define GMB in l.94 now.

15. L. 87 “(Zwally drainage basins 19-21)”: Where are the areas? A figure that shows the Zwally drainage basins “19-21” is needed.

Thank you for this suggestion, we have now added Figure A9 that shows the individual basins:



In the manuscript we note that basins 19–21 are located near the Amundsen Sea, and since we do not refer to the basin numbers elsewhere in the text, we chose to include this figure only in the Appendix.

16. L. 94 ~ 96 “The summer increase is most pronounced over West Antarctica and the AP, whereas winter SMB has risen mainly along the East Antarctic coastline and tip of the AP.”: This is an interesting and meaningful message in my opinion. I would start a new paragraph with this sentence to highlight the message.

This is a good suggestion, we now start a new paragraph at this point.

17. L. 139: Please define “SIC” here. In the current version of this paper, I cannot find the definition of SIC in the running text.

Thank you for noticing, we now define SIC in I.70.

18. L. 303: The definitions of ANTIS-AR and EDARA are missing.

Thank you for noticing, we now define both catalogues in II. 369-371.

19. Figure 1a: It seems to me that an explanation of the solid gray line is missing.

The solid grey line corresponds to the linear trend over the full period, which is described in the caption as “Light straight lines indicate linear trend slopes from 2003 to 2019, and 2020 to 2024 (2023 for RACMO2.4p), with trends printed in the respective colour”). We added “solid” after “straight” which hopefully clarifies this more.

20. Figures 1c and 1f: Which SMB data did the authors use for these figures?

(Now Fig 1d and 1e) We apologize that this was not indicated in the caption, we now clarify that it is based on ERA5.

21. Figure 2: The data source for daily precipitation over the AIS should be explained in the caption.

We apologize and now clarify that it is based on ERA5.

22. Figures A3, A9, A10: The data source for SMB should be explained in the caption.

(Now Fig A3, A10, and A11) We apologize and now clarify that it is based on ERA5 or downscaled HCLIM43 experiments driven by ERA5.

23. Figures 3, A11 ~ A19: It is better to indicate in the captions that the contents are based on the HCLIM43 experiments.

We agree and have now added data sources to all figures.

24. Figure A10 caption: Suggest rephrasing “Yellow patterns over land and blue over ocean ~ ” to something like “Yellow patterns over ice sheet and blue over ocean ~ .” Ice shelves are not on land.

Thank you for this suggestion, after changing the colour following the comment #10, we have now changed this to: “Orange patterns over the ice sheet and ice shelves, and blue over ocean thus indicate co-occurring high SMB and low SIC.”

Reviewer #2

This manuscript investigates the potential causes of the recent slowdown in Antarctic Ice Sheet (AIS) mass loss after 2016. The authors use reanalysis data to link AIS mass balance anomalies to the increased frequency of atmospheric rivers (ARs) since 2020. In addition, climate models are employed to quantify the role of sea ice in ice-mass gain through its influence on precipitation. The study provides useful insights into the role of atmospheric rivers over Antarctic ice shelves and their interactions with sea ice.

Overall, the topic is of high scientific interest and relevant to the journal's readership. However, several aspects require clarification and improvement before the manuscript can be considered for publication. Specific comments are provided below.

Thank you for reviewing our article and giving valuable feedback. We have addressed the concerns and suggestions below.

Major Comments

1. Lines 33–60:

The authors are encouraged to clarify the knowledge gap between this study and previous work. It appears that earlier studies have already discussed the slowdown of AIS mass loss and its linkage with ARs. Does this study extend the analysis period, introduce the recent increasing trend observed during the past five years, or provide the first quantitative connection between AIS mass gain and sea ice? Clarifying this will help position the novelty of the research more clearly.

Thank you for this suggestion. Previous studies have identified the recent slowdown in AIS mass loss between 2020 and 2022 (namely Zhang et al. 2024, and Wang et al. 2023) and attributed it to increased precipitation, but none have linked these SMB anomalies to changes in AR activity. Here we extend the analysis through 2024, provide a link to intensified AR frequency and use the RCM experiments to isolate the role of reduced sea ice. We have made that more clear in ll. 62:

“Our study is the first to demonstrate a connection between the recent increase in surface accumulation through 2024 and intensified AR frequency. We also perform non-hydrostatic RCM experiments with a completely sea ice free Southern Ocean and a high SIE counterpart similar to conditions during the Last Glacial Maximum [19, 20] to isolate how major SIE changes influence AR-driven precipitation and SMB over Antarctica.”

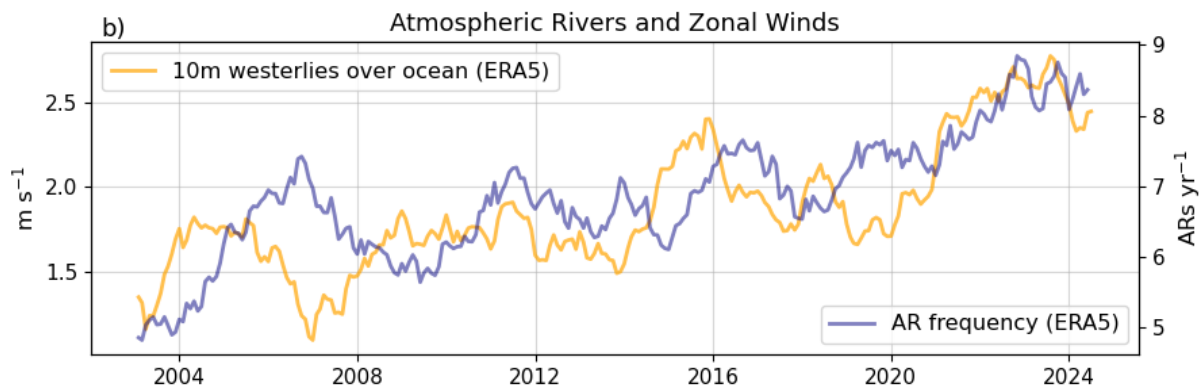
We then further discuss the differences between the experiment setup of Kittel et al. and ours, who also studied the SIC–SMB relationship (ll. 69-87).

2. Figure 1:

Figure 1 effectively illustrates the recent increase in AIS mass, but it currently contains limited information, while too many key details are placed in the Supplementary Information. This

separation may be inconvenient for readers. The authors are encouraged to overlay the increase in AR frequency (2020–2024) and the westerly trend on the AIS mass time series and to elaborate on their consistency and respective roles in the “Recent Increase in Antarctic Ice Mass” section. Including regional divisions in this figure would also improve clarity.

This is a great suggestion, we added a new panel (b) to Figure 1 (overlying it made it too chaotic because Figure 1a is quite dense already; also note that the time series in (a) show accumulated anomalies):



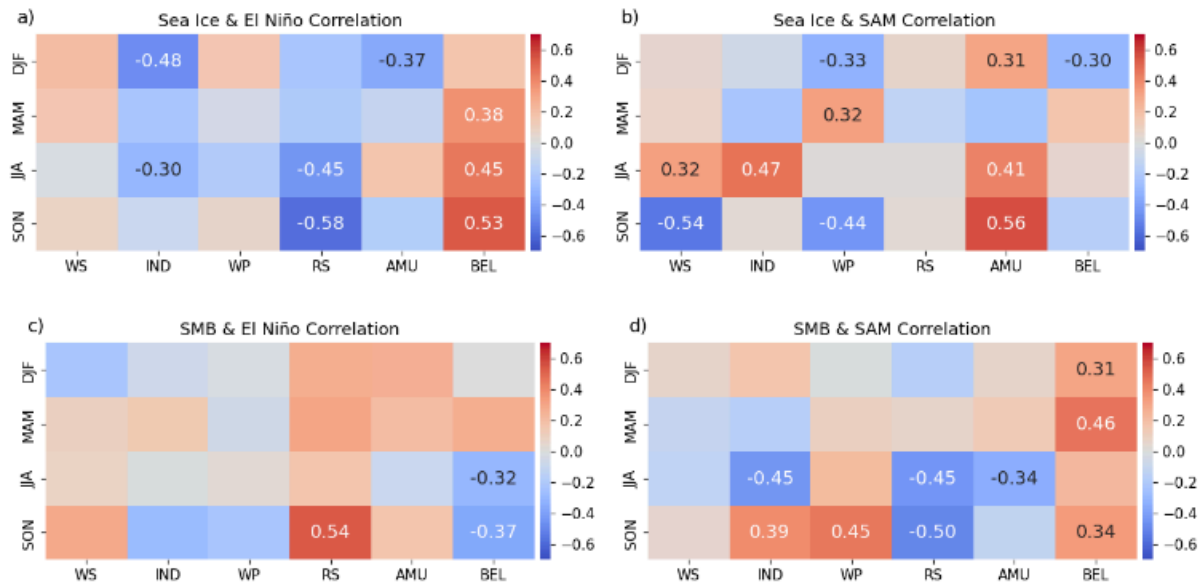
We also added the following in the “Recent Increase in Antarctic Ice Mass” section (II. 136):

“These patterns coincide with regions experiencing the highest AR activity (Figures A5 and A6) and IVT (Figure A7), as well as strengthened westerlies over the past five years (Figures A8 and Figure 1b). Particularly after 2020, both AR frequency and westerly winds intensified, but their strong variability leaves it unclear how long this pattern will continue (Figure 1b). We find that high AR activity and strengthened zonal winds do not always coincide, and hence their recent synchronisation is likely driving the accelerated increase in Antarctic SMB.”

3. Section “Regional Accumulation Patterns Shaped by SAM, ENSO, and ARs”:

a. This section could be improved by explicitly showing the regions discussed. The authors might consider dividing Antarctica into several regions and presenting the correlations between SAM/ ENSO and SMB in a table, together with their trends and relationships, to make the results more straightforward. Including this table in the main text would enhance readability.

We agree that it helps to show the regions in the main paper. In the previous version we included the map for the sea ice+land region divisions in the Appendix. We now moved it to the main paper as Figure 3 and also added a new Figure 4 as shown below (“tables” for the ENSO and SAM correlations):



We add references to these figures in the section “Regional Accumulation Patterns Shaped by SAM, ENSO, and ARs”. (Note that Figure A2 shows that there is no significant long term trend in either mode).

b. Atmospheric river (AR) activity in East Antarctica is known to correlate strongly with SAM (Pohl et al., 2021; Shields et al., 2022). The relatively low correlations reported here may arise from a lead–lag relationship between SAM and ENSO, as noted by King et al. (2023). The authors are encouraged to reexamine this relationship.

Reference:

Shields, C. A., Wille, J. D., Marquardt Collow, A. B., MacLennan, M., & Gorodetskaya, I. V. (2022). Evaluating uncertainty and modes of variability for Antarctic atmospheric rivers. *Geophysical Research Letters*, 49, e2022GL099577. <https://doi.org/10.1029/2022GL099577>

Pohl, B., Favier, V., Wille, J., Udy, D. G., Vance, T. R., Pergaud, J., et al. (2021). Relationship between weather regimes and atmospheric rivers in East Antarctica. *Journal of Geophysical Research: Atmospheres*, 126, e2021JD035294. <https://doi.org/10.1029/2021JD035294>

King, M. A., Lyu, K., & Zhang, X. (2023). Climate variability: A key driver of recent Antarctic ice-mass change. *Nature Geoscience*, 16, 1128–1135. <https://doi.org/10.1038/s41561-023-01317-w>

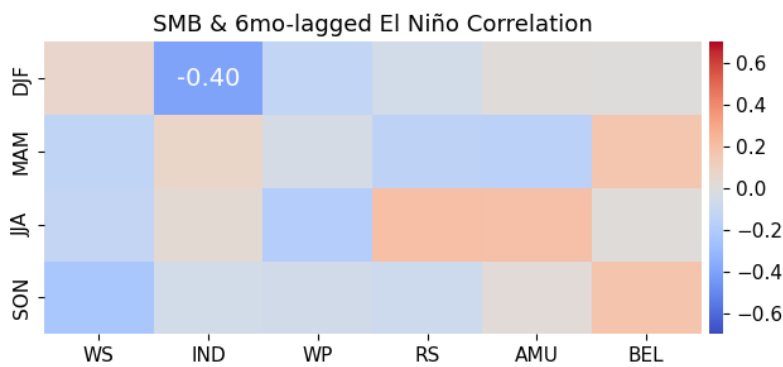
Thank you for the references. We added a statement about the linkage of ARs over East Antarctica and SAM/ENSO in ll. 175 (where [38-40] are the 3 mentioned studies):

“While previous studies have shown that SAM and lagged ENSO can have an influence on coastal East Antarctic AR frequency and SMB [38-40], the relationship is weaker than in West Antarctica and the peninsula.”

Pohl et al (2021) and King et al (2023) do not seem to assess ARs, only ice mass and weather regimes. Note that in our study we also do not report low correlations between East Antarctic AR activity and SAM/ENSO but show that East Antarctic **inland SMB** shows low correlations with local sea ice and large-scale climate modes (Figure A3). This is in

agreement with Shields et al (2022) who also report weak precipitation influences of climate modes in the East, except for some key regions which align with our map correlations (Figure A3). Their key point also suggests this: “Modes of variability hold greater influence over West Antarctica than East Antarctica and are consistent across most AR detection tools”.

We repeated the El Niño correlations with a 6-month lag as in King et al. (2023) which resulted in even lower correlations:



Our findings are consistent with the previous studies in that ARs are a dominant contributor to East Antarctic SMB, but in agreement with Shields et al. (2022) suggest that the statistical relationship between AR-related mass input (i.e. a large part of precipitation) and SAM or ENSO variability is weak.

c. The logic regarding sea ice in this section is somewhat unclear. It would be helpful to strengthen the rationale for including sea ice here and to explain why its contribution is emphasized, given that its influence may be smaller compared to large-scale circulation changes.

We agree that it makes sense to stress the motivation behind sea ice, so we added the following at the beginning of section “The role of sea ice in modulating SMB and AR strength” (ll. 192):

“Although large-scale circulation changes are the primary driver of Antarctic precipitation variability in most regions, local sea ice conditions can also strongly affect precipitation by modulating air–sea heat fluxes and moisture availability [21,42–44]. The fact that Antarctic SIE reached several record lows since 2017 raises questions about a possible linkage between reduced sea ice and enhanced surface accumulation.”

4. Figures A9–A10:

These figures serve as the basis for the modeling section. However, regional variations in sea ice are likely to contribute mainly to regional changes in SMB, and these effects may vary considerably among basins. This spatial heterogeneity could explain the limited correlations. The sparse significant signals observed may not sufficiently justify the subsequent modeling. The authors may consider applying methods such as Singular Value Decomposition (SVD) to identify coherent SIC–

SMB patterns, and then relate the resulting time series to large-scale circulation indices (e.g., SAM and ENSO).

(Figure A9 is now Figure 3).

Yes, we agree that the influence of sea ice on SMB is strongly regional. This is why we present the experiment results not only as time series but mainly as spatial maps of precipitation, SMB, etc (Figure 5 and Figures A11–A15). The ERA5-based SIC–SMB correlations (Figures 3 and A10) highlight where local sea ice conditions co-vary with SMB. We also note in the manuscript that these relationships are not necessarily causal everywhere, which is why we performed the sea ice sensitivity experiments, which allowed us to isolate the physical contribution of reduced sea ice (and showed that especially in winter the sea ice contribution is quite substantial in regions such as the AP and Dronning Maud Land (Figure 5)). We agree that further analyses based on both observational datasets and complementary modelling approaches would be valuable for future research.

Minor Comments

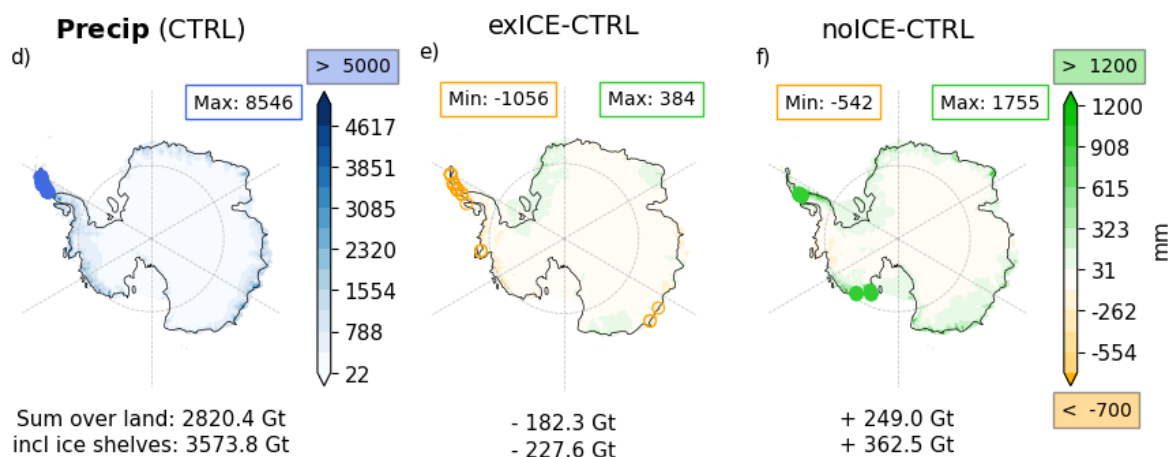
1. Lines 90–91: Showing the winter and summer averages for the two periods would make the conclusions clearer.

Thank you for this suggestion. Figure 2 shows a climatological “time series” from Jan to Dec, so the request from the reviewer about winter and summer averages for the two periods is difficult to understand. We refer to the seasonal (winter and summer) SMB mean changes between the two periods which are presented as spatial maps in Figure A4. We probably should have referred to this figure and have added it now to the statement (now ll. 128):

“Recent precipitation and SMB increases are evident across most days of the year, particularly during summer (Figure 2b and A4). [...]”

2. Figure 3d–3f: Consider adjusting the color bar to a narrower range to highlight smaller-value distributions.

This is a good idea, we have adjusted the colour bars to a narrower range of max 5,000 (instead of 7,000) mm/day for total precipitation, and -700 to 1,200 mm/day for the difference:



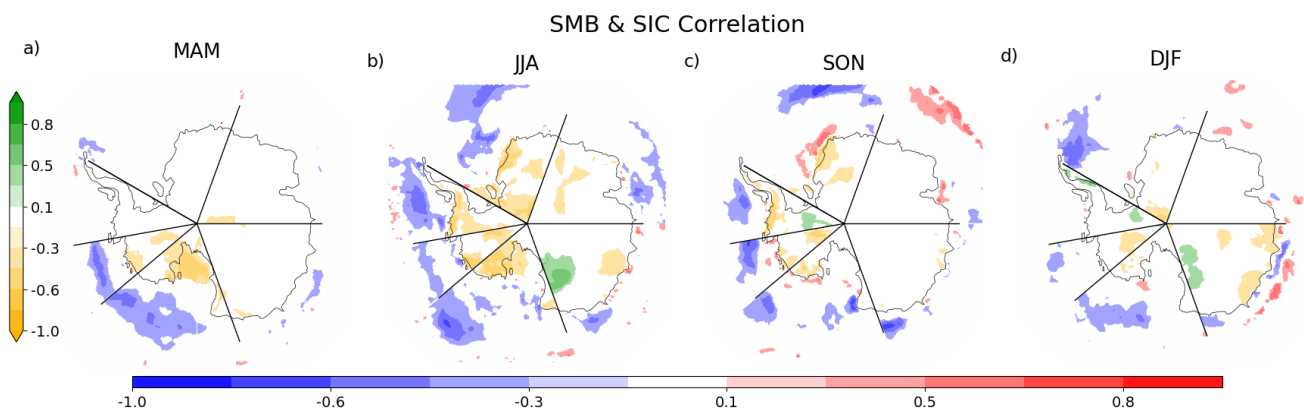
3. Line 244: The conclusion that summer SMB increase is associated with positive SAM is valuable but not sufficiently emphasized in the results section. This connection should be more explicitly stated earlier in the manuscript (see Major Comments).

This is a good point and we added the following text to the “Regional Accumulation Patterns shaped by SAM, ENSO, and ARs” section earlier in the manuscript (ll. 162):

“The recent sea ice decrease in the Bellingshausen Sea, and the SMB increase on the AP and western Weddell Sea sector (near the Ronne-Filchner Ice Shelf), are thus very likely enhanced by negative ENSO and positive SAM anomalies respectively (Figures 4 and A3). As the ENSO influence on SMB appears weak during summer, we conclude that the accelerated summer accumulation over the AP and western Weddell Sea sector is largely driven by a positive SAM. “

4. Figure A10: The correlation color bar over Antarctica is not clearly distinguishable, particularly for negative values. Please enhance its visual clarity.

Thank you for noticing. We have adjusted the yellow tone (now more orange) for negative values for better readability:



Reviewer #3

This manuscript investigates the recent slowdown and reversal of Antarctic Ice Sheet (AIS) mass loss observed in satellite data. The authors analyze reanalysis products and regional climate model simulations to identify the drivers of this reversal. Their analysis attributes the positive anomalies in surface mass balance (SMB) to enhanced precipitation, which is closely linked to intensified atmospheric river (AR) activity and reduced sea ice extent. The manuscript is clearly structured, well written, and offers valuable insights on the mechanisms driving recent AIS mass changes. Only the analysis on the role of sea ice requires better justification. Beyond that, I only have a few minor comments for improvement.

Thank you for reviewing our article and giving valuable feedback. We have addressed the concerns and suggestions below.

1. L20-23: The causal chain in this sentence could be clarified for better logical flow. From my understanding, I suggest rephrasing it as: "...this reversal is primarily due to positive surface mass balance anomalies, which result from increased precipitation driven by enhanced atmospheric river activity, strengthened westerlies, and reduced sea ice extent."

This indeed makes it sound clearer, we have revised this sentence of the abstract to (ll.20):

"Here we use a suite of reanalyses and regional climate models to show that this reversal is primarily due to positive surface mass balance anomalies, which result from increased precipitation driven by enhanced atmospheric river (AR) activity, strengthened westerlies, and reduced sea ice extent (SIE)."

2. L27: Should "the effect of sea ice" be "the effect of sea ice loss"?

Yes, Thank you for reviewer for pointing this out, we have changed this accordingly (ll. 26):

"High-resolution regional climate model simulations with modified SIE show that the effect of sea ice loss on enhancing precipitation through increased evaporation accounts for around 10% of the winter increase, but is overall minor compared to remote large-scale processes."

3. L42: Similar to the comment above, the relationship between "increased precipitation" and "surface mass balance anomalies" is unclear here.

We have revised the sentence to clarify the distinction between precipitation and SMB anomalies. We now state that the interruption is linked to anomalously high snowfall and the resulting positive SMB anomalies are a result of the increase in snowfall (ll. 42):

"This interruption in the downward trend has been attributed to anomalously high snowfall

and resulting positive SMB anomalies, which were unprecedented in both West and East Antarctica [5, 7].”

4. L67: “GMB” is not defined.

Thank you for noticing this, it is now defined in l. 94.

5. L73-76: Considering the known biases in ERA5 precipitation, it would be better to briefly explain in the main text how SMB is computed here (as described in the Methods) and the rationale of the approximation used. Additionally, since you do not clearly introduce what type of datasets RACMO2.4p and HCLIM43 are, it would be helpful to provide a very short description of each when first mentioned.

This is a good suggestion. We now include in the main text how SMB is computed in our study and also added short descriptions of RACMO2.4p and HCLIM43 (ll. 99):

“Instead, the recent mass gain is driven by an increase in SMB, which is the sum of precipitation, evaporation and sublimation, and surface runoff. For ERA5 and the RCM HCLIM43, we approximate SMB as the sum of precipitation and net evaporation–sublimation. Over Antarctica, this approximation compares well with observations and the more advanced SMB calculations of the RCM RACMO [25] (we use RACMO2.4p; see Methods).”

6. L85: The numbers of the supplementary figures are out of order throughout the manuscript.

We arranged the Supp. Figures in a logical way for the reader (e.g. grouping all SMB comparison maps together, or related time series as in A1 and A2). In the main text, the figures are therefore not always cited in numerical order. As far as we are aware, the journal does not require supplementary material to follow the order of first citation. We are open to reorganize them if still requested, but it would make the Supp. Information less structured.

7. L97-98: There’s a lack of explanation here of how do strengthened westerlies impact the SMB.

Thank you for pointing this out. We now mention in the text that strengthened westerlies are associated with enhanced moisture transport toward Antarctica, and therefore more potential AR landfall and general precipitation which increases SMB (ll. 136):

“These patterns coincide with regions experiencing the highest AR activity (Figures A5 and A6) and IVT (Figure A7), as well as strengthened westerlies over the past five years (Figures A8 and Figure 1c), which enhance moisture transport toward the continent and increase the likelihood of (AR-induced) precipitation.”

8. L133: The transition from the previous section is not smooth. At L125, it states that “SAM and ENSO variability alone cannot fully explain the enhanced accumulation”, which naturally leads the reader to expect a discussion of the drivers behind East Antarctic precipitation, SMB, and GMB changes. However, this section primarily focuses on the contribution of sea ice, even though the low correlation between inland East Antarctic SMB and local sea ice has already been noted. It is important to add clear transitional sentences to explain why the role of sea ice is being examined here.

Thank you for noticing this. We now clarify that the low correlations refer only to inland East Antarctica, and we state why sea ice is still relevant for coastal SMB and AR changes. We also suggest that the inland SMB increase in East Antarctica is likely AR-driven (ll. 178):

“East Antarctic precipitation is more strongly influenced by synoptic-scale moisture transport from northerly sources, largely due to the higher elevation of the plateau [10, 41]. This explains the lower correlations of inland East Antarctic SMB with the climate modes (Figure A3a–f) and local sea ice (Figures 4 and A10), and suggests that recent episodic AR events have instead increased precipitation in these inland areas, especially during winter (Figure A5d and Figure A6d). That said, sea ice loss can still influence many coastal regions, including parts of East Antarctica, both by increasing local heat and humidity fluxes and by intensifying individual ARs. These mechanisms are examined in the following section.”

9. L169-170: I didn’t see the mean SIC in the figure.

Thank you for pointing this out, we removed this from the caption (we forgot to update it after including a separate figure just for the sea ice experiments (Fig. 5a-c)).

10. L166-182: It is not clear whether these results explain the positive mass anomalies in East Antarctica, as the signal appears to be mostly appearing in West Antarctica.

Thank you for this comment. However, the modelling results do show a clear coastal SMB increase in East Antarctica, especially in Wilkes Land, and also in Victoria Land, and the Dronning Maud Land sector. We acknowledge that our original text only emphasized the AP and Marie Byrd Land, which may lead to misunderstanding. To clarify this, we have added “Wilkes Land” to the following sentence (ll. 241):

“The areas most affected are Marie Byrd Land, Wilkes Land, and the AP, with over 1,700 mm year of increased precipitation near Larsen C (Figure 5f). Correspondingly, precipitation is most reduced in these areas if SIC are increased (Figure 5e).”

11. Figure1: Why does the grey linear regression line “twist” during 2016-2020?

The slight change in the angle of the regression line between 2016 and 2020 is caused by the end of the original GRACE mission (mid-2017) and the start of GRACE-FO (mid-2018). The regression uses the actual time stamps and the 1-yr gap stretches the axis without

adding new data points. We added this to the caption: “The small change in the grey line’s angle around 2017 reflects the 1-year GRACE – GRACE-FO gap.”

12. L282-285: Would be better to add some citations to support the choice of ERA5 for the analysis.

Thank you for this suggestion and have added more references to support our use of ERA5 (ll. 345):

“We use precipitation and evaporation fields from ERA5 [55] to estimate SMB from 1985 to April 2025, calculated as precipitation minus evaporation and sublimation. Although additional processes influence the final SMB, this provides a close approximation when compared with more advanced SMB estimates [25]. ERA5 shows reasonable performance for Antarctic climate and synoptic systems [56–58], and offers relatively high horizontal resolution compared to other reanalyses (31 km). “

Response to Editor and Reviewers

Manuscript: COMMSENV-25-4371A

Journal: Communications Earth & Environment

We thank the reviewers 1-3 for the positive evaluation and recommendation for publication.

We thank the Editor for their careful assessment of our manuscript and their positive feedback. Below we provide brief responses to the remaining editorial comments. All changes have been implemented as requested.

(Line 34–37):

I think it makes more sense if these two sentences are swapped. Because there is redundancy, they can be combined into one sentence.

Revised accordingly: “Antarctica holds the largest reservoir of freshwater on Earth; therefore, understanding the drivers of mass gains and losses of the Antarctic Ice Sheet (AIS) is crucial for predicting future global and regional sea level rise and associated changes in ocean circulation.”

(40–41, 62, 68, 125):

Please spell out acronyms for the first time (GRACE, GRACE-FO, EAIS, SMB, IVT).

Revised accordingly.

(Line 59):

"11km" >> Put space between 11 and km. Please check out other places.

Revised accordingly.

(Line 69–71):

What about "Kittel et al. (2018) [21] have assessed Antarctic SMB responses to sea ice concentration (SIC) and sea surface temperature (SST) by performing multi-decadal sensitivity experiments with a hydrostatic RCM with a 50 km resolution."?

Revised accordingly.

(Line 62–67):

This part explains the novelty of this work. It is unusual to read about your own work in the middle of the introduction. What about combining these sentences with the last paragraph (Line 80–87)?

Revised accordingly: “In this study, we use a non-hydrostatic, high-resolution (11 km) regional climate model driven by ERA5 to explore two extreme sea ice states outside present-day variability: a completely ice-free Southern Ocean (noICE) and a high SIE scenario with 100% sea ice extending 5° equatorward of the present-day ice edge (exICE). These experiments are conducted over a single year (July 2021–June 2022), which includes four precipitation-heavy AR events. By examining how these events respond to large sea ice changes, we isolate the influence of SIE on AR-driven precipitation and Antarctic SMB. Our study demonstrates a link between the recent increase in Antarctic surface accumulation through 2024 and intensified AR frequency. To avoid introducing an additional thermodynamic signal, sea surface temperatures are not modified.”

(Line 190 and 219):

I think these subheadings are not necessary.

Revised accordingly.

(Line 229–230):

"SIC are increased to 100% up to 5°S north of the monthly mean SIE" >> I do not understand this procedure. According to the Method section, maybe "5 degrees S" should be "5 degrees"?

Revised accordingly: “5° north of the monthly mean SIE”

(Figure 1a and b):

Quantities are missing from the vertical axis labels. I understand they are indicated as legends and in the plot titles, but a more usual manner is to indicate them next to the axes

with units (e.g, Cumulative mass balance anomaly (Gt), Zonal wind speed (m s^{-1}), AR frequency (yr^{-1})). Please consider revision for these and other plots.

Revised accordingly.

(Figure 5a and f):

The numbers next to the color scale bar are odd (e.g. 22, 788, 1554 ...).

Revised accordingly.

(Line 330 and 336):

The GIA is spelt out twice.

Revised accordingly.

(Line 378–382):

Variable definitions are repeated several times.

Revised accordingly.