nature portfolio

Peer Review File

The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics

Corresponding Author: Professor Minghu Ding

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts:

Review of Zhang et al. manuscript “The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics“ submitted to Nature Communications

This manuscript investigates surface melt across the Antarctic Peninsula (AP) and its ice shelves from 1979 to 2022. The authors document a marked intensification of surface melt after 2009 in two regional climate models (RCM), following more than a decade of predominantly reduced melt. They show that this recent increase is statistically significant and is primarily related to thermodynamic changes, while it cannot be explained with the variability in atmospheric circulation alone.
For this the authors apply the method of self-organizing maps (SOMS) to identify distinct circulation patterns and identify the contribution to surface melt from dynamics (frequency of individual circulation patterns), thermodynamics (intensity of melt events) and the contribution of interaction between both.
Detecting a recent reversal in regional surface melt trends is of major scientific interest, especially when linked to shifts in the surface energy balance. Furthermore it is highly relevant for interpreting recent observations, which typically provide data on the frequency and spatial extent of surface melt, while observation-based estimates of melt intensity remain sparse and poorly constrained.
The motivation of this study is clearly laid out, the applied method are sound and the analysis leads to a plausible conclusion.
I recommend publication after some mostly minor revisions. In some places however I would appreciate a deeper discussion and some additional information. Also I would like to raise a few questions which relate to the method and interpretation thereof.

General comments:

At the end of the discusion the authors come to the conclusion that there is “a growing influence of thermodynamic drivers on surface melt” (l. 240). This should be substantiated or formulated differently, as Figs. 3 and 4 seem to indicate that thermodynamics were also the dominant driver in the first part of the period of investigation. Only the negative 2003-2015 trend appears to be predominantly related to dynamics- this should be pointed out, discussed and put into perspective.

As there are trends which are driven by thermodynamics and others which are driven by dynamics, it might be worthwhile to discuss whether there are implications for the interpretation of satellite observation.
It would be for example interesting to compare the simulated melt with simulated melt extent and melt days, i.e. with that kind of information which is easily available from satellite observations. Could the recent intensification of surface melt be underrepresented in satellite observations?

I recommend to provide in the supplement some timeseries related to the surface energy balance from the atmospheric forcing, e.g. 2m air temperature, longwave and shortwave radiation averaged over the melt period and weighted by melt.

Specific comments:
l. 22: Please specify the period of investigation
l. 61: Is “greenhouse gas forcing” here meant to include clouds and water vapor? Maybe better: concentration of greenhouse gases and clouds
l. 65: To which period does this refer?
l. 78: SEB has not been introduced yet
l. 79: Is it possible to show these measured melt rates over time?
l. 80: How good is the agreement, if only the melt period (NDJF) is considered?
l. 88: Maybe name filter method and motivate the choice of the 5 year low pass filter here
l. 101: Solid line -> solid red line
l. 102: Typo: 1981-2010
l. 103: Additional information would be helpful on how to read Fig. 1c here or in the text. The text identifies a significant negative trend for 1992–2005, but the figure indicates significance from 1992-2003 (following significance diagonally) or 1992-2021 (vertically). Also please add more details in the Methods: is this linear trend fitting accounting/correcting for auto-correlation before performing the significance test and are residuals checked?

l. 106: Please provide the confidence level of the significance test also in the caption.
l.139-140: This sentence is better moved to the Discussion.
Fig.2: Maybe also provide the temporal mean Mi,t as text in the time series panels.
l.166: Change to: dictated by thermodynamical forcing
l. 185: Better: large positive anomalies in melt
Fig 4: What is Intensity here exactly, what is the reference and how is it to be interpreted. Why not show absolute rates here?
l. 280: The method was introduced by Kohonen T (1990) The self-organizing map. Proc IEEE 78(9): 1464–1480, however the reference given here provides a good overview. Maybe cite both.
l. 281: method with unsupervised classification -> method for unsupervised classification
l. 288: The term "node size" may be confusing, as it might be read as the actual size of each node, whereas here it means the total number of nodes in the SOM.
Also it would be helpful to briefly explain why the number of nodes is so important in an SOM and how it influences the output, as this differs from supervised neural networks.
Something like "In a SOM the number of nodes defines how many patterns the input data can be classified into. Based on iteratively adjusting the similarity between each node's reference vector and the input data, the final classification is obtained. The number of nodes therefore defines the number of final classes, a very import hyperparameter of the SOM. If the number of nodes it too small...."

l.308: Is this simply the number of days?
ll.313-315: The notation here is confusing as bars seem to indicate both spatial and temporal means and it is not explained what the i without a dot signifies. We propose to introduce Mi,t as the spatial average (without bar but normal “i”), and then use bars for temporal means only.
l.344: Similar to earlier comment: why is it necessary to standardize the extreme melt rates? How is this to be interpreted?

Reviewer #2

(Remarks to the Author)
Significance of the paper:
The manuscript presents the recent change in melt on ice shelves on the Antarctic Peninsula and points out the significant increasing trend in the last 15 years. While similar published articles remained obscure with respect to the changing amount of the melt, this contribution presents numerically clear results of the decadal change in the melt. On this account, this reviewer finds the presented paper contributing for our present knowledge in the environmental change in the Antarctic.
The paper is, however, not easy to follow. It can be greatly improved by reorganizing the sections and also by adding some key information. These points will by detailed below:

1) With regard to the structure of the paper, “Methods” should come before “Results” and “Discussion”.
2) “Definitions of dynamic and thermodynamic” must be explained more concretely. This reviewer understands that the authors mean under “dynamic” a circulation pattern. In comparison, “thermodynamic” is left short of the clear explanation. If authors can present how the “thermodynamic” is related to each component in the energy budget equation, such as solar global radiation, albedo, longwave atmospheric radiation, sensible heat flux, and heat for the melt, the readers can follow the discussion more easily. This step will also present what could have been the cause of the recent increasing melt. There is also a possibility that solar radiation was increasing besides longwave atmospheric radiation, which the well-known direct consequence of the greenhouse effect.
3) To augment the easier understanding by readers, timeseries of each component of the energy budget should be presented, very much like Fig. 1 (b). This figure is very illustrative but confined only to the melt. Similar diagrams presenting the time series of the main energy components must be presented.
4) The amount of the melt is the key contribution of this paper. Therefore, the melt should also be presented in unit of mean (regional) specific melt, in addition to the regional total melt this paper is presenting. The specific melt will convey readers quickly to energy budget, while total melt requires information of the surface area the authors worked.
5) Fig. 1 (b) shows an interesting fluctuation of the melt. The readers will notice the earlier peak of the melt around 1990-

1995. CO_2 concentration was then about 70 ppm smaller than the recent years this paper is dealing. It is nice if the authors could explain how it then happened. This step will enhance the importance of the present melt increase also.

Reviewer #3

(Remarks to the Author)
The paper addresses a relevant issue, however the methodology they followed is not doing justice to the dynamics involved. For example, for a southward./northward flow in the Antarctic peninsula a warming/cooling and surface melting/freezing can be expected, which is not the case here for the first two modes. The authors are arguing that this is due to the thermodynamic effect. I can agree with that, provided that some computations regarding the heat advection and how significant or nonsignificant it is. If it is thermodynamic, what are the thermodynamic variables involved.
For the remaining modes wind direction and melting more or less agree.

Had the authors checked the atmospheric circulation patterns during the maximum and minimum of the melting in the AP? Does that compare with the SOM analysis?

The downward radiation components in the supplementary diagrams show a zonal sea –saw pattern for mode 1 and 2, which is also not reflected in the melt patterns for these nodes.

I find the analysis is poorly explained and the non association with the dynamic and the association with the thermodynamic processes are not convincingly presented.

I recommend a major revision with computations explaining dynamical/thermodynamical processes along with the SOM for robust interpretations.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Review of Zhang et al. manuscript “The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics“ submitted to Nature Communications
This manuscript investigates surface melt across the Antarctic Peninsula (AP) and its ice shelves from 1979 to 2022. The authors document a marked intensification of surface melt after 2009 in two regional climate models (RCM), following more than a decade of predominantly reduced melt. They show that this recent increase is statistically significant and is primarily related to thermodynamic changes, while it cannot be explained with the variability in atmospheric circulation alone. For this the authors apply the method of self-organizing maps (SOMS) to identify distinct circulation patterns and identify the contribution to surface melt from dynamics (frequency of individual circulation patterns), thermodynamics (intensity of melt events) and the contribution of interaction between both.

The revised manuscript has been carefully reviewed, and I am satisfied that the authors have adequately addressed all of my previous comments. The quality of the paper has improved accordingly, and I believe it is now suitable for publication

Reviewer #2

(Remarks to the Author)
(Concerning with the Structure)
This reviewer suggested earlier to place "Methods" before the "Results". The authors did not take the advice. As a result you can imagine the surprise in future readers of this paper. They will be confronted by "Results" immediately after "Introduction". The authors' response states "In accordance with the formatting guidelines of the journal, the detailed Methods section is placed at the
end of the manuscript." This part of the instruction was misunterstood by the authors. This part of the guideline is a suggestion concerning with the long and detailed description of the methods. The manuscript needs a breif presentation of the "Methods" before the "Results". The problem of integrating the presentation of the methods in the results is obvious. The "Results" has become too long, to call it the result. Usually, the "Results" should be brief and concise. The detailed presentation of the methods can still be placed near the end or in the Supplementary material.

(Sientific Part)
The following comments are rather minor, but certainly help future readers.
1) Presenting the amount of melt, specific melt is necessary in addition to the regional total. The specific melt allows the

direct comparison with the past and also with other regions. This suggestion applies both for the text and figures.
2) Line 108: Caption of Figure 1a. State the concerned period. This is stated in the text. However, in principle, the text and figures should be regarded independent of each other.
3) Energy balance components presented in this paper are the result of computations. It is necessary to compare the computational results against actual measurements? This may be too demanding for turbulent fluxes. The comparison with the radiation fluxes must be possible. The Antarctic is well covered with stations with well equipped radiometers.

Reviewer #3

(Remarks to the Author)
Its happy to see that the authors have carefully and comprehensively addressed the queries. My main concern was a lack of a convincing explanation of the significance of thermodynamic over dynamic factors controlling Antarctic surface melt. Largely, the authors use frequency of specific SOM patterns and melt frequencies obtained from satellite. My suggestion was to examine the heat advection vis a vis thermodynamic effect. Though the authors have gone at great length rebutting the document banking on SOM analysis and radiation, still, I find an analysis of heat advection missing. A discussion, at least, comparing the SOM and satellite derived dynamical effect to computed heat advection using temperature and winds will clear any ambiguity brought by the categorisation based on an increase in brightness temperature for melt anomalies, frequency of specific patterns in SOM and real advection. The authors need not calculate this for every circulation pattern, but at least, a theoretical explanation would suffice. Also across the Antarctic Peninsula, foehn effect warms the surface. These factors may also be included in the discussion. I recommend this revision before the paper is published. This could be a minor revision for the authors.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
This reviewer (Reviewer 2) is impressed by the degree of efforts the authors went to revise the manuscript. It reads well. This reviewer recommends the manuscript for acceptance. If I am allowed to comment on a very trivial point (Lines 470, 472, and 556), it is how to write a Dutch family name. For example, it should be "van den Broeke" and not "Van Den Broeke". Even though the name may appear at the beginning, "van" does not become "Van". The way Line 553 presents is correct. In any case, it is better to type in a homogenous manner.

Reviewer #3

(Remarks to the Author)
The authors have addressed the queries on the relative role of dynamics and thermodynamics. I recommend the manuscript for publication in nature communication.

The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics

Response to reviewers

Reviewer 1:

General comment	(R1, GC)
Summary I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts: Review of Zhang et al. manuscript "The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics" submitted to Nature Communications This manuscript investigates surface melt across the Antarctic Peninsula (AP) and its ice shelves from 1979 to 2022. The authors document a marked intensification of surface melt after 2009 in two regional climate models (RCM), following more than a decade of predominantly reduced melt. They show that this recent increase is statistically significant and is primarily related to thermodynamic changes, while it cannot be explained with the variability in atmospheric circulation alone. For this the authors apply the method of self-organizing maps (SOMS) to identify distinct circulation patterns and identify the contribution to surface melt from dynamics (frequency of individual circulation patterns), thermodynamics (intensity of melt events) and the contribution of interaction between both. Detecting a recent reversal in regional surface melt trends is of major scientific interest, especially when linked to shifts in the surface energy balance. Furthermore it is highly relevant for interpreting recent observations, which typically provide data on the frequency and spatial extent of surface melt, while observation-based estimates of melt intensity remain sparse and poorly constrained. The motivation of this study is clearly laid out, the applied method are sound and the analysis leads to a plausible conclusion. I recommend publication after some mostly minor revisions. In some places however I would appreciate a deeper discussion and some additional information. Also I would like to raise a few questions which relate to the method and interpretation thereof.	

We sincerely thank the reviewer for the constructive comments and suggestions, and positive feedback. We are very happy to get comments from you and another reviewer which are helpful and have improved the quantity of our manuscript greatly. We have carefully to check and revised our manuscript according to your comments and feedback from other reviewers. Our replies are highlighted in blue text, while all corresponding changes in the revised manuscript are marked in green, with revised line numbers (of the version with track changes) indicated where relevant.

The detailed responses to each comment are as below.

Reviewer 1 — General Comment 1	**(R1, GC1)**
At the end of the discusion the authors come to the conclusion that there is “a growing influence of thermodynamic drivers on surface melt” (l. 240). This should be substantiated or formulated differently, as Figs. 3 and 4 seem to indicate that thermodynamics were also the dominant driver in the first part of the period of investigation. Only the negative 2003-2015 trend appears to be predominantly related to dynamics- this should be pointed out, discussed and put into perspective.	

We thanks for your thoughtful comments. Indeed, our description is not precise and only the negative 2003-2015 trend is predominantly related to dynamics. For most periods with significant trends of AP surface melt changes, the thermodynamics dictate the changes in surface melt. During the 2003-2015, the dynamics dominate the decrease in the AP surface melt. In this period, the frequency of patterns related to negative (positive) melt anomalies show an increase (decrease) (Fig. 2). The initial motivation here is to emphasize the importance of the thermodynamics to the enhancement of surface melt. Therefore, we have reformulated this sentence in lines 284-288 of the revised manuscript:

“*Our results show the dynamics only dictate the significant decreasing trends during 2000s, when there is a decline in the occurrence frequency of circulation patterns related to negative surface melt anomalies (Fig. 2). Our discovery underscores the strong influence of thermodynamic drivers on surface melt across the AP, especially for the recent enhancement since 2009.*”

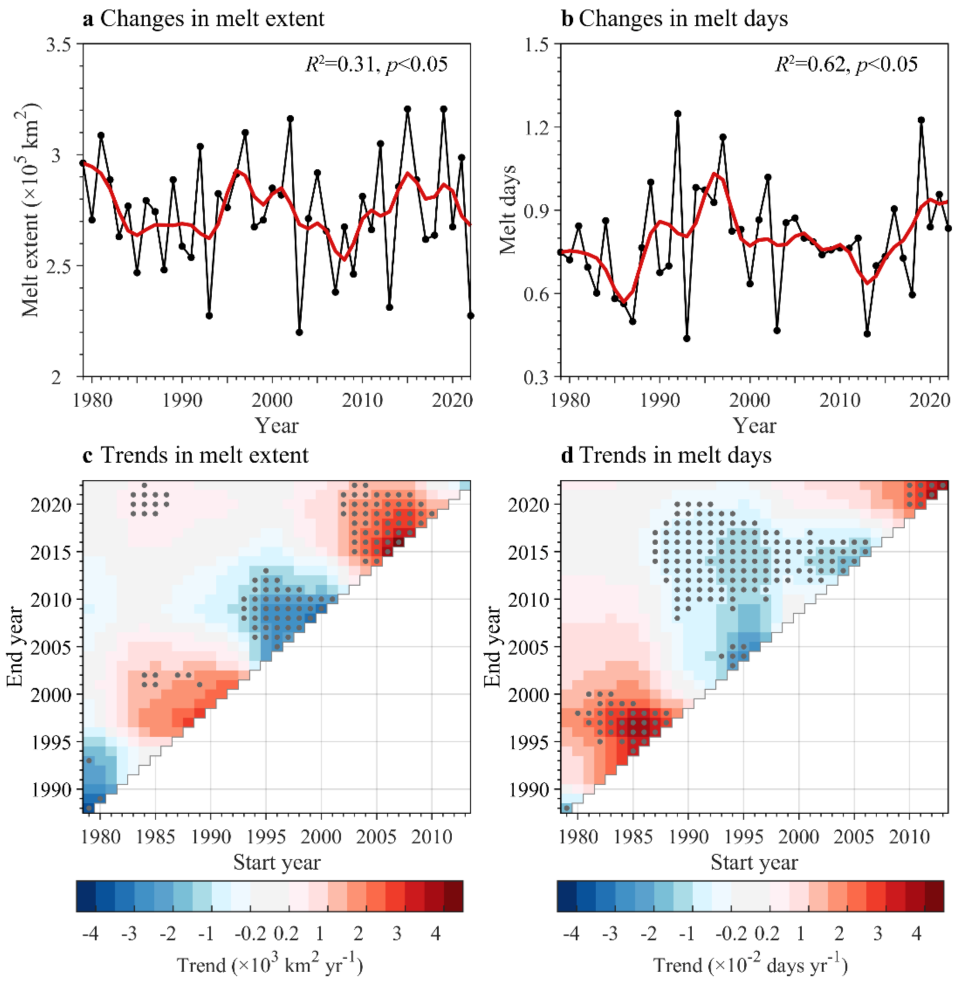
Reviewer 1 — General Comment 2	(R1, GC2)
As there are trends which are driven by thermodynamics and others which are driven by dynamics, it might be worthwhile to discuss whether there are implications for the interpretation of satellite observation. It would be for example interesting to compare the simulated melt with simulated melt extent and melt days, i.e. with that kind of information which is easily available from satellite observations. Could the recent intensification of surface melt be underrepresented in satellite observations?	

Thanks for your excellent suggestions. In the new manuscript, we have used brightness temperatures from NASA passive microwave satellite (25 km×25 km) to quantify the NDJF cumulative melt extents and averaged melt days over the Antarctic Peninsula since 1979 (new supplementary figure 11). We find that the changes in the melt extent and rates are not highly consistent (R^2=0.31), although the statistic is significant. The melt rates show a significant increase in the early and late periods of our study period and decrease in the other periods. However, the melt extent exhibits alternating increases and decreases. Particularly, the change in the melt extent does not show a recent enhancement. Instead, the change in the melt days is highly correlated with the melt rates (R^2=0.62, p<0.05). The melt days also showed a significant NDJF increase during 2009-2022 with trend value of 0.02 d yr^{-1} (p<0.05). This difference between the satellite observations and models highlights the necessity of quantifying the melt rates. Relying solely on satellite-derived melt extent may lead to an underrepresentation of AP surface melt. Our results of quantifying melt rates is an effective supplement to the signals captured by satellite observation.

We have drawn new supplement figures in the revised supplementary file and added relevant description in lines 251-257 of the revised manuscript:

"*Compared with the results of satellite observation, passive microwave data show that melt days remains consistent with our modeled melt rates (R^2=0.62, p<0.05; Supplementary Fig. 11), with an increasing trend of 0.02 d yr^{-1} since 2009 (p<0.05).*

However, melt extent trends tend to underestimate the recent intensification. This discrepancy suggests that the severity of AP melting may be underestimated using satellite observations alone. Our results of quantifying melt rates is an effective supplement to the signals captured by satellite observation."



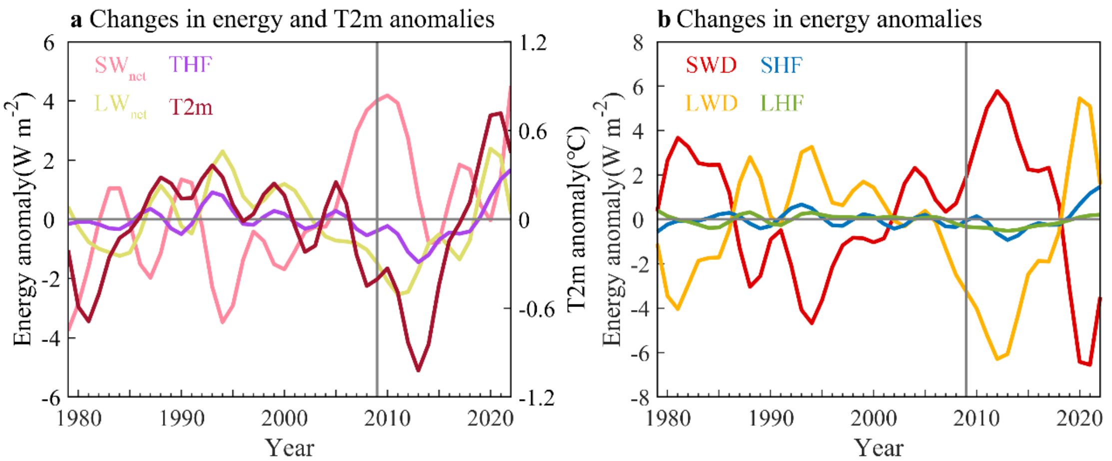
Supplementary Figure 11: Changes and trends in AP NDJF surface melt extent and averaged days from Passive microwave satellite. **a, b** Changes in AP NDJF surface melt extent and averaged days. The solid red line represents the results after the five-year low-pass Butterworth filtering. **c, d** Trends during different periods. Linear trends were computed based on the time series represented by red lines in **a** and **b**. Correlations between satellite indices and melt rates are also listed in **a** and **b**. The grey dots in **c** and **d** represent passing the (Hamed-Rao modified) Mann-Kendall significance test of the confidence level of 95%.

Reviewer 1 — General Comment 3	(R1, GC3)
I recommend to provide in the supplement some timeseries related to the surface energy balance from the atmospheric forcing, e.g. 2m air temperature, longwave and shortwave radiation averaged over the melt period and weighted by melt.	

Thanks for your insightful and thoughtful comments and suggestions, which were also recommended by another reviewer. Therefore, combining all reviewers' comments, we added a new figure in the supplementary file to show the changes in surface energy heat fluxes from the atmospheric forcing and 2 m air temperature anomalies from RACMO2.3p2 in NDJF over the AP from 1979 to 2022, relative to the 1981-2010 mean (New Supplementary Figure 3).

We find that the changes in surface melt rates are related to the changes in net longwave radiation (R^2=0.53, p<0.05) which show a significantly increasing trend during 2009-2022 (0.31 W m^{-2} yr^{-1}). Additionally, turbulent heat fluxes also show significantly increase during 2009-2022 (0.16 W m^{-2} yr^{-1}) due to increased sensible heat fluxes.

Moreover, we have also added the total and detailed surface energy flux anomalies during each SOM node (New Supplementary Figure 4 and 5).



New Supplementary Figure 3: Changes in AP NDJF average surface energy heat flux and 2 m air temperature anomalies. (a) Time series of NDJF average surface net shortwave radiation (SW_{net}), net longwave radiation (LW_{net}), turbulent heat fluxes and 2 m air temperature (T2m) anomalies from 1979 to 2022, relative to the 1981–2010 mean. The lines represent the results after the five-year low-pass Butterworth filtering. (b) Same as (a) but for downward shortwave radiation (SWD), downward longwave radiation (LWD), sensible heat flux (SHF) and latent heat flux (LHF).

Reviewer 1 — Specific Comments 1	**(R1, SC1)**
l. 22: Please specify the period of investigation	

Thanks for your suggestions and we have revised it in lines 21-23 of the revised manuscript:

"*Here, using 1979-2023 daily surface melt rates from regional climate models, we show that this declining trend has reversed,…*"

Reviewer 1 — Specific Comments 2	**(R1, SC2)**
l. 61: Is "greenhouse gas forcing" here meant to include clouds and water vapor? Maybe better: concentration of greenhouse gases and clouds	

Thanks for your comments. Indeed, your suggestion is better and we have rewritten it in lines 60-63 of the revised manuscript:

"*Key drivers include long-term near-surface warming driven by global climate change, variations in longwave radiation associated with the concentration of greenhouse gases and clouds, and changes in sea-ice extent and sea-surface temperatures (SST).*"

Reviewer 1 — Specific Comments 3	**(R1, SC3)**
l. 65: To which period does this refer?	

Thanks for your comments. The period analyzed for changes in the melt season in the western AP covers 1978 to 2022. Additionally, we have corrected an error in the original text: the decrease in sea ice concentration leads to a delayed end of the melt season, rather than an overall lengthening of its duration. We have revised this in lines 65–67 of the manuscript as follows:

"*Additionally, reductions in sea ice concentration in the Bellingshausen and Amundsen Seas from 1978 to 2020 have delayed the end of the melt season in West AP by enhancing turbulent heat fluxes.*"

Reviewer 1 — Specific Comments 4	**(R1, SC4)**
l. 78: SEB has not been introduced yet	

Thanks for meticulous reading and comment. We have rewritten this sentence and added the full name of "SEB" in lines 79-81 of the revised manuscript:

"*The regional climate model RACMO2.3p2 faithfully represents AP surface melt volume/rate, compared with both daily and monthly melt estimates from surface energy balance model*[35] *in four automatic weather stations*"

Reviewer 1 — Specific Comments 5	**(R1, SC5)**
l. 79: Is it possible to show these measured melt rates over time?	

Thanks for your suggestion. We agree that presenting measured surface melt rates during the study period would enhance the interpretation of our results. However, the temporal coverage of these observations is quite limited. As shown in the figure below, which illustrates the time series of surface melt flux derived from an SEB model forced by AWS observations in the AP, the longest record spans only five years (at AWS18). Such limited observations are insufficient to characterize long-term trends in AP surface melt rates or to substantiate a recent enhancement. While these data are valuable for evaluating climate model performance, we have ultimately decided not to include these measured melt rates over time in the manuscript or the supplementary material.

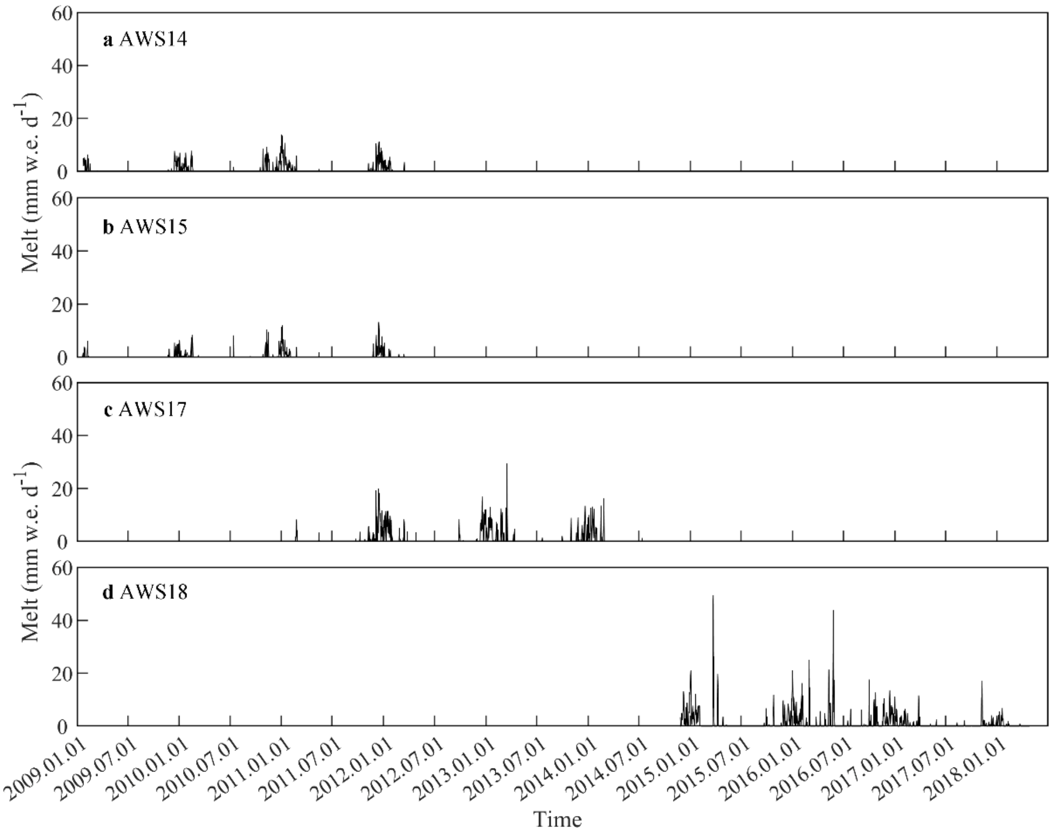
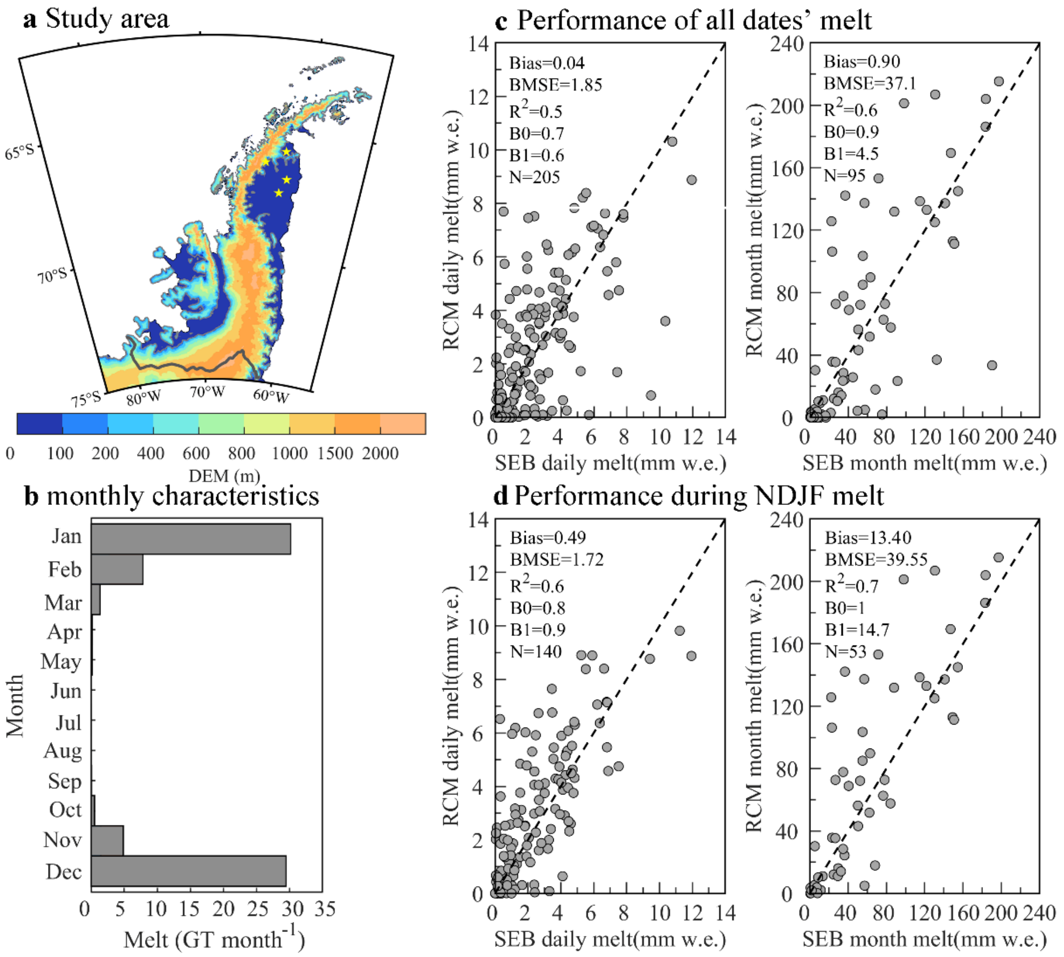


Figure. Time series of surface melt flux from surface energy balance model forced by observations from automatic weather stations in AP.

Reviewer 1 — Specific Comments 6	**(R1, SC6)**
l. 80: How good is the agreement, if only the melt period (NDJF) is considered?	

Thanks for your comments and suggestions. Indeed, it is essential to demonstrate the performance of RACMO2.3p2 in simulating surface melt rates during the melt season (NDJF). We have now included these evaluation results in the supplementary material (Supplementary Fig. 1d). Compared to the results encompassing all dates, RACMO2.3p2 shows a higher daily bias but a lower RMSE and a higher R^2 (0.6) during NDJF. Regarding monthly performance, the bias is higher, which can be attributed to the high absolute values of surface melt rates during this period. Similarly, the R^2 value is also higher (0.7). Overall, these results indicate good agreement between the RCM and the SEB models forced by observations, further increasing our confidence in analyzing the changes in AP surface melt, particularly during the NDJF period. We have rewritten lines 79–83 of the revised manuscript as follows:

"*The regional climate model RACMO2.3p2 faithfully represents AP surface melt volume/rate, compared with both daily and monthly melt estimates from surface energy balance model35 in four automatic weather stations (stars in Supplementary Fig. 1a) ($R_{daily,NDJF}^2$ = 0.6, p < 0.01; $RMSE_{daily,NDJF}$ = 1.72 mm w.e. day^{-1} , Supplementary Fig. 1c and d)*"



New Supplementary Figure 1: Study area and the performance of RACMO2.3p2 of AP surface melt. **a** Study area and the distribution of AWSs used to forced surface energy balance (SEB) model. **b** Monthly distribution of AP surface melt in RACMO2.3p2. **c** and **d** Performance of all dates' and NDJF surface melt rates from RACMO2.3p2, respectively. Shading in **a** indicate the altitude in model and yellow stars represent automatic weather stations used in the surface energy balance model. The bold black line indicates the extent of the AP. Statistics in **c** and **d** show the number of data points (N), mean bias (Bias), root mean squared error (RMSE), correlation coefficient (R^2), regression slope (B0) and intercept (B1).

Reviewer 1 — Specific Comments 7	(R1, SC7)
l. 88: Maybe name filter method and motivate the choice of the 5 year low pass filter here	

We thank the reviewer for this suggestion. A 5-year low-pass Butterworth filter was applied to the NDJF surface melt time series to suppress high-frequency interannual variability and emphasize low-frequency, multi-year variations relevant to this study. Shorter cutoff periods do not sufficiently reduce year-to-year fluctuations, whereas longer cutoff periods lead to excessive smoothing and a reduction in statistical degrees of freedom. Therefore, we have ultimately opted for a 5-year low-pass filter. We have rewritten this sentence in lines 91–93 of the revised manuscript as follows:

"Given the strong interannual variability, the 5-year low-pass Butterworth filtered time series highlights multi-year variability (red line in Fig. 1b) and long-term trends (Fig. 1c)."

Reviewer 1 — Specific Comments 8	(R1, SC8)
l. 101: Solid line -> solid red line	

Thanks for your comments and we have revised it in lines 109-110 of the revised manuscript:

" *The solid red line represents the results after the five-year low-pass filtering*"

Reviewer 1 — Specific Comments 9	(R1, SC9)
l. 102: Typo: 1981-2010	

Thanks for your carefully review and we have revised the reference period in lines 111-112 of the revised manuscript:

"*The changes are also shown as a percentage (right y-axis, relative to the 1981–2010 mean)*"

Reviewer 1 — Specific Comments 10	(R1, SC10)
l. 103: Additional information would be helpful on how to read Fig. 1c here or in the text. The text identifies a significant negative trend for 1992–2005, but the figure indicates significance from 1992-2003 (following significance diagonally) or 1992-2021 (vertically). Also please add more details in the Methods: is this linear trend fitting accounting/correcting for auto-correlation before performing the significance test and are residuals checked?	

We thank the reviewer for these insightful and constructive comments. Regarding the issue of autocorrelation, we apologize for not accounting for it before performing the Mann-Kendall (M-K) significance tests in the previous version. Following your suggestions, we examined the residuals using Durbin-Watson (DW) statistics and found evidence of positive autocorrelation (DW < 0.5) across all decadal windows. In the revised manuscript, trend significance is now assessed using both the original M-K test and the Hamed-Rao modified M-K test to account for serial correlation. Specifically, the DW test is performed first; if the *p*-value is less than 0.05, the Hamed-Rao modified M-K test is applied. Otherwise, the original M-K test is used.

Compared with our previous results, we found that significance levels were somewhat overestimated. For instance, some signals in Fig. 1c were previously misclassified as significant. Regarding the various time windows, we now find that all long-term trends (ending in 2022) are not statistically significant. However, the positive trends during the early and late stages of the study period remain significant, while significant decreases occurred during the 1990s and 2000s. Importantly, the primary conclusion regarding a recent enhancement remains unchanged: all time windows starting in 2009 exhibit significant positive trends. We have added this detail to the Methods section (lines 432–435).

"*For linear trends, the Durbin-Watson test was used to examine the first-order autocorrelation of linear regression residuals. When time series has significant auto-correlation, the trends was assessed using the Hamed-Rao modified Mann-Kendall* *test*[67]."

Additionally, we apologize for the confusing description of Fig. 1c. In this study, the start and end points of a trend are defined as the period during which all overlapping 10-year windows exhibit a consistent and significant trend. Consequently, based on the new results, the periods with significantly negative trends in NDJF melt are 1992–2004 and 2003–2014 (Fig. 1c). To ensure clarity for the reader, we have added explanatory information to lines 93–96 and 113–115:

"*AP experiences two periods with significantly negative trends in NDJF melt: 1992–2004 and 2003–2014 (Fig. 1c), when consistent significant negative trends were observed across all overlapping decadal windows.*"

"*The start and end time-points of a tendency are defined as the period during which all overlapping 10-year windows exhibit a consistent, significant trend.*"

Reviewer 1 — Specific Comments 11	**(R1, SC11)**
l. 106: Please provide the confidence level of the significance test also in the caption.	

Thanks for your suggestion. The confidence level of the significance test is 95%. In lines 116-117 of the revised manuscript, we added:

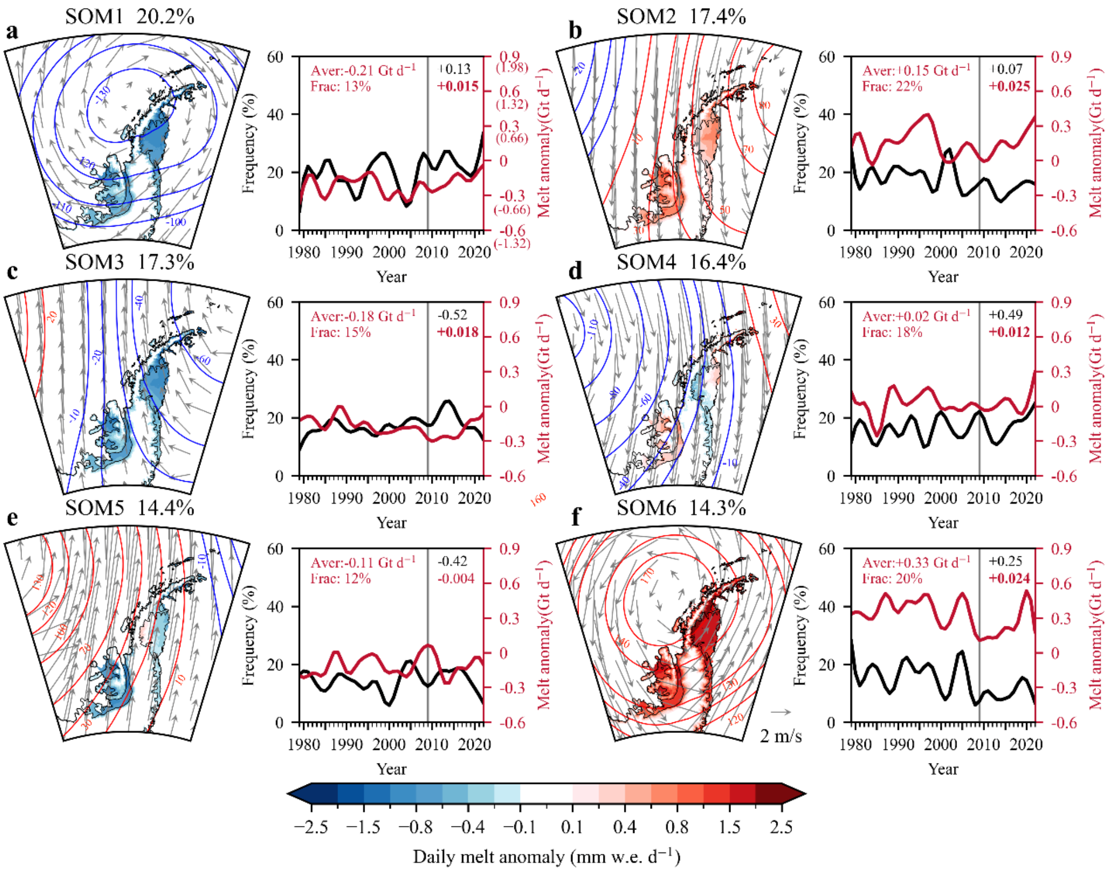
"*The grey dots in (**c, d**) represent passing the (Hamed-Rao modified) Mann-Kendall significance test of the confidence level of 95%.*"

Reviewer 1 — Specific Comments 12	**(R1, SC12)**
l.139-140: This sentence is better moved to the Discussion.	

We thank the reviewer for this suggestion. We agree that this sentence is misplaced and would be more appropriately situated in the Discussion section. Given that a similar description already exists in the Discussion, we have ultimately decided to delete this sentence from the revised manuscript to avoid redundancy.

Reviewer 1 — Specific Comments 13	**(R1, SC13)**
Fig.2: Maybe also provide the temporal mean Mi,t as text in the time series panels.	

We thank you for your suggestions and helpful comments. We have added the temporal mean anomalies of daily surface melt rates (Mi,t, indicated by the red "Aver" values). For instance, in the case of SOM6, the corresponding mean daily surface melt anomaly is 0.33 Gt d^{-1}. The revised Fig. 2 is provided below:



New fig. 2 Circulation patterns and corresponding daily surface melt anomaly

Reviewer 1 — Specific Comments 14	(R1, SC14)
l.166: Change to: dictated by thermodynamical forcing	

Thanks for your comment. We apologize that the specific revised description is now reflected in the new phrasing because the relevant section of the manuscript was extensively restructured during the revision.

Reviewer 1 — Specific Comments 15	(R1, SC15)
l. 185: Better: large positive anomalies in melt	

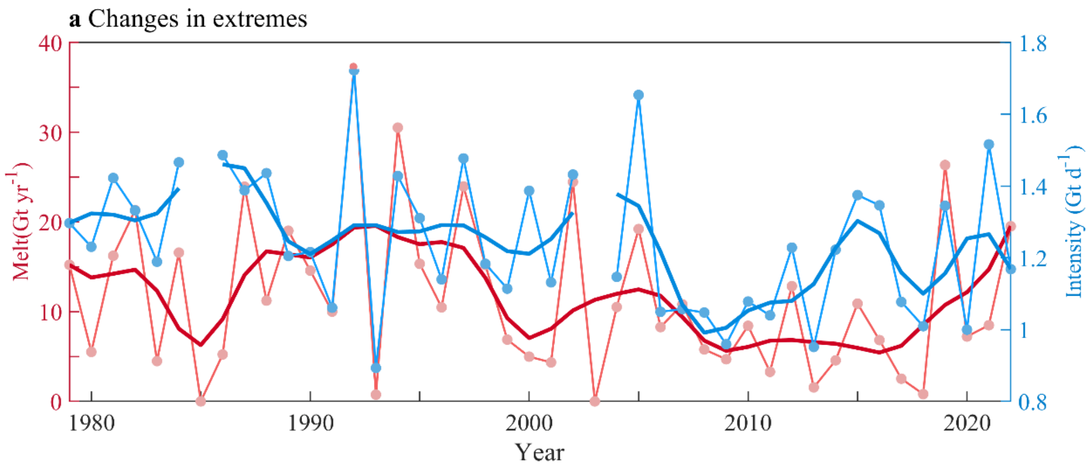
Thanks for your suggestion and we agree to changed it as follows in lines 219-221 of the revised manuscript:

"*Compared with non-extreme surface melt, these events are primarily characterized by large positive anomalies in melt (>200%) over the north AP (Supplementary Fig. 10).*"

Reviewer 1 — Specific Comments 16	(R1, SC16)
Fig 4: What is Intensity here exactly, what is the reference and how is it to be interpreted. Why not show absolute rates here?	

We thank the reviewer for this comment. In this study, "intensity" refers to the standardized average daily value of extreme melt events each year, facilitating the intercomparison of these events. We agree that using absolute values is more effective than standardization, as it allows readers to intuitively understand the extreme melt rates. Following the reviewer's suggestion, we have now used absolute values to represent intensity. Specifically, we calculated intensity as the average surface melt anomaly of extremes relative to the 1979–2022 NDJF mean. We have updated Fig. 4a in the revised manuscript and added the corresponding description in lines 237–239:

"*The intensity represents average daily extreme melt anomalies, relative to the mean the average daily melt during the NDJF period spanning from 1979 to 2022.*"



New Fig. 4a Long-term changes in total extreme surface melt in NDJF and intensity of extremes. The intensity represents average daily extreme melt anomalies, relative to the mean the average daily melt during the NDJF period spanning from 1979 to 2022.

Reviewer 1 — Specific Comments 17	(R1, SC17)
l. 280: The method was introduced by Kohonen T (1990) The self-organizing map. Proc IEEE 78(9): 1464–1480, however the reference given here provides a good overview. Maybe cite both.	

We thank the reviewer for this suggestion and have included the reference to Kohonen (1990) in the revised manuscript.

Reviewer 1 — Specific Comments 18	(R1, SC18)
l. 281: method with unsupervised classification -> method for unsupervised classification	

Thanks for your comment. In lines 339-340 of the revised manuscript, we have changed it as follows:

"*The self-organizing maps*[59,60] *were used to classify atmospheric circulation patterns, which is a neural network method for unsupervised classification.*"

Reviewer 1 — Specific Comments 19	(R1, SC19)
l. 288: The term "node size" may be confusing, as it might be read as the actual size of each node, whereas here it means the total number of nodes in the SOM. Also it would be helpful to briefly explain why the number of nodes is so important in an SOM and how it influences the output, as this differs from supervised neural networks. Something like "In a SOM the number of nodes defines how many patterns the input data can be classified into. Based on iteratively adjusting the similarity between each node's reference vector and the input data, the final classification is obtained. The number of nodes therefore defines the number of final classes, a very import hyperparameter of the SOM. If the number of nodes it too small...."	

We thank the reviewer for pointing out the potential ambiguity of the term "node size". We agree with the importance of clarifying the influence of the number of nodes in an SOM. Consequently, we have replaced "node size" with "number of nodes" throughout the manuscript. Additionally, we have included a concise explanation regarding the key role of the node count in an SOM, as recommended. This revision has been made on lines 347-353 of the revised manuscript:

"*In a SOM, the number of nodes determines how many distinct patterns the input data can be represented by*[59,60]*. Based on iteratively adjusting the similarity between each node's reference vector and the input data, the final classification is obtained. The number of nodes therefore defines the number of final classes. If the number of nodes is too small, different patterns may be merged and the diversity of the data cannot be fully captured; if it is too large, the resulting map becomes excessively detailed, making the classification difficult to present and interpret*[30,64]."

Reviewer 1 — Specific Comments 20	(R1, SC20)
l.308: Is this simply the number of days?	

We thank the reviewer for this comment. Indeed, "occurrence frequency" in this study refers to the total number of days associated with each circulation pattern. Here, we separated the total NDJF melt anomaly to the sum of the product of the days of each pattern and the corresponding average daily surface melt anomaly. For total melt, the frequency here represents the number of days rather than the ratio. We have added the corresponding unit on line 369 of the revised manuscript for clarity:

"*…the occurrence frequency (days) of daily circulation patterns in a cluster*"

Reviewer 1 — Specific Comments 21	**(R1, SC21)**
ll.313-315: The notation here is confusing as bars seem to indicate both spatial and temporal means and it is not explained what the i without a dot signifies. We propose to introduce Mi,t as the spatial average (without bar but normal "i"), and then use bars for temporal means only.	

We thank the reviewers for their comments and suggestions. We apologize for the confusion caused by the notation and the inconsistent appearance of the dot on the letter "i" . The disappearance of the dot on the subscript "i" was a font rendering issue in Microsoft Word; specifically, when an overbar (e.g., $\bar{\imath}$) is applied, the dot is automatically suppressed. To clarify, our notation refers strictly to the temporal average, as spatial averaging was not performed in this study. To avoid further confusion, we have replaced the variables "i" and "K" with "n" and "N" in all formulas throughout the revised manuscript. The updated formulas are provided below:

$$M_t = \sum_{n=1}^{N} \overline{M_{n,t}}\, f_{n,t} \ , \tag{1}$$

$$M_t = \sum_{n=1}^{N} \left(\overline{M_n} + M'_{n,t}\right)\left(\overline{f_n} + f'_{n,t}\right) \ , \tag{2}$$

$$\frac{dM_t}{dt} = \sum_{n=1}^{N} \left(\overline{f_n}\frac{dM'_{n,t}}{dt} + \overline{M_n}\frac{df'_{n,t}}{dt} + \frac{d\left(M'_{n,t}f'_{n,t}\right)}{dt}\right) \ , \tag{3}$$

We also further explain the $\overline{M_{n,t}}$ in lines 372-375 of the revised manuscript:

"Here, we defined the total surface melt anomaly as the sum of the product of the frequency of each pattern $f_{n,t}$ and the corresponding temporal-average daily surface melt anomaly $\overline{M_{n,t}}$:"

Reviewer 1 — Specific Comments 22	(R1, SC22)
l.344: Similar to earlier comment: why is it necessary to standardize the extreme melt rates? How is this to be interpreted?	

We thank the reviewers for their comments. Indeed, considering your earlier point, we agree that it is unnecessary to standardize the extreme melt rates. Consequently, we have revised the definition of "intensity" (as detailed in our response above). In the Methods section, we have updated the corresponding description on lines 415–417 of the revised manuscript:

"*We also further defined the intensity as average daily extreme melt anomalies of extremes, relative to the mean the average daily melt during the NDJF period spanning from 1979 to 2022.*"

Reviewer 2:

General Comment	(R2, GC)
Summary The manuscript presents the recent change in melt on ice shelves on the Antarctic Peninsula and points out the significant increasing trend in the last 15 years. While similar published articles remained obscure with respect to the changing amount of the melt, this contribution presents numerically clear results of the decadal change in the melt. On this account, this reviewer finds the presented paper contributing for our present knowledge in the environmental change in the Antarctic. The paper is, however, not easy to follow. It can be greatly improved by reorganizing the sections and also by adding some key information.	

We thank the reviewers for their comments and positive feedback, which have greatly helped us improve the quality of our manuscript. We have carefully revised the paper in accordance with your suggestions and the feedback from other reviewers. We agree with your recommendation to reorganize the text and add key information to make the manuscript easier to read for both researchers and the public. Our replies are highlighted in blue text, while all corresponding changes in the revised manuscript are marked in green, with revised line numbers (of the version with track changes) indicated where relevant.

Our detailed responses to each comment are provided below:

Reviewer 2 — General Comment 1	(R2, GC1)
1) With regard to the structure of the paper, "Methods" should come before "Results" and "Discussion".	

We acknowledge the reviewer's comment regarding the structure. In accordance with the formatting guidelines of the journal, the detailed Methods section is placed at the end of the manuscript. However, we recognize that the initial submission may have lacked sufficient methodological context within the main text to support a smooth transition into the Results. Therefore, to improve readability and address your concern, we have revised and strengthened the methodological descriptions within each Results section, ensuring that readers can clearly understand the basis for our findings.

Reviewer 2 — General Comment 2	(R2, GC2)
2) "Definitions of dynamic and thermodynamic" must be explained more concretely. This reviewer understands that the authors mean under "dynamic" a circulation pattern. In comparison, "thermodynamic" is left short of the clear explanation. If authors can present how the "thermodynamic" is related to each component in the energy budget equation, such as solar global radiation, albedo, longwave atmospheric radiation, sensible heat flux, and heat for the melt, the readers can follow the discussion more easily. This step will also present what could have been the cause of the recent increasing melt. There is also a possibility that solar radiation was increasing besides longwave atmospheric radiation, which the well-known direct consequence of the greenhouse effect.	

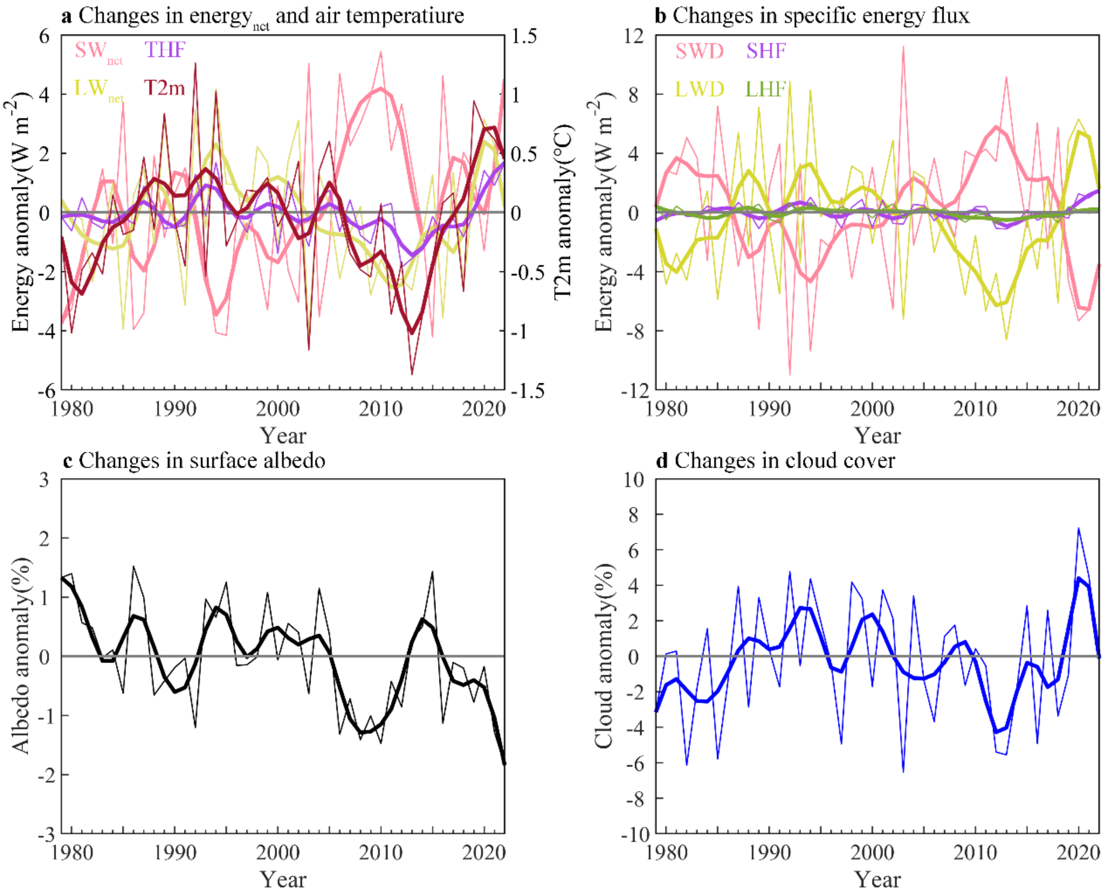
We thank the reviewer for these insightful and thorough comments. We apologize for the lack of clarity in our initial explanation of "thermodynamics". Here we define the thermodynamics as the response of surface melt intensity to changes in the background field temperature and local surface energy budget, independent of changes in atmospheric circulation patterns. To make this distinction more concrete for the reader, we now explain the thermodynamic drivers through the following SEB components. For example, under a constant circulation frequency, these drivers include changes in cloud cover, LWD, SWD, and THF (SHF+LHF) due to background field warming and increased atmospheric water content caused by global warming; changes in SWnet due to changes in surface albedo, such as snow grain degradation or meltwater accumulation; and changes in SHF and LWD due to the reduction of surrounding sea ice and rising sea surface temperatures.

Based on reviewers' insights, we have added anomalies and variations in shortwave and longwave radiation. We find that LWD and THF show a recent increasing trend in most patterns, but the increase in solar radiation also plays a significant role under certain circulation patterns. In SOM5, changes in LWD and THF lead to reduced melt, but SWnet shows an increasing trend, attributed to albedo. We believe these revisions will make it easier for readers to understand the driving factors behind the AP surface melt. We have revised relevant explanation of thermodynamics in the section of method, in the lines 388-398 of revised manuscript:

"*For the thermodynamic contributions, here it refers to the fact that the response of surface melt intensity to changes in the background field temperature and local surface energy balance, independent of changes in atmospheric circulation patterns. For example, under a constant circulation frequency: changes in cloud cover, longwave radiation, downward shortwave radiation, and turbulent heat flux due to background field warming and increased atmospheric water content caused by global warming; changes in net shortwave radiation due to changes in surface albedo, such as snow grain degradation or meltwater accumulation*[65]*; and changes in sensible heat flux and longwave radiation due to the reduction of surrounding sea ice and rising sea surface temperatures.*"

Reviewer 2 — General Comment 3	**(R2, GC3)**
3) To augment the easier understanding by readers, timeseries of each component of the energy budget should be presented, very much like Fig. 1 (b). This figure is very illustrative but confined only to the melt. Similar diagrams presenting the time series of the main energy components must be presented.	

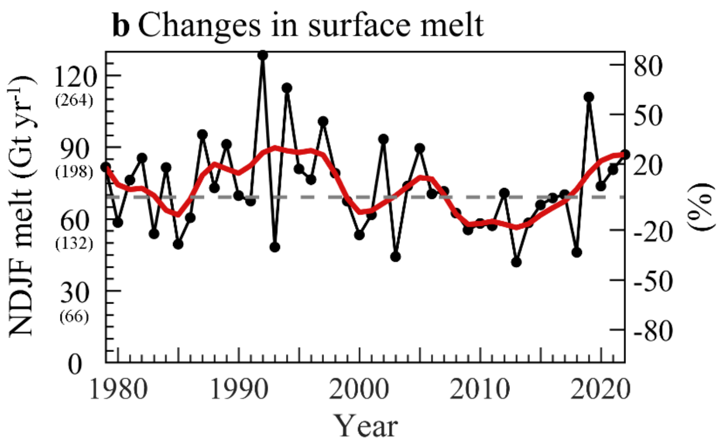
We thank the reviewer for these constructive and thoughtful comments and suggestions. Similar points were also raised by another reviewer, who recommended providing time series of the surface energy balance components related to atmospheric forcing in the supplement. Therefore, by synthesizing the comments from all reviewers, we have added a new figure to the supplementary information (New Supplementary Figure 3). This figure illustrates the changes in surface heat fluxes driven by atmospheric forcing and 2-m air temperature anomalies from RACMO2.3p2 during NDJF over the AP from 1979 to 2022, relative to the 1981–2010 mean. The ground heat flux (G) is not shown, as its magnitude and variability are generally small (Kuipers Munneke et al., 2012). The new supplementary figure is provided below:



Supplementary Figure 2: Changes in NDJF average surface energy heat flux, air temperature, albedo and cloud anomalies in AP. (a) Time series of NDJF surface net shortwave radiation (SW_{net}), net longwave radiation (LW_{net}), turbulent heat fluxes and 2 m air temperature (T2m) anomalies from 1979 to 2022, relative to the 1979–2022 NDJF calendar daily mean. The bold lines represent the results after the five-year low-pass Butterworth filtering. (b) Same as (a) but for downward shortwave radiation (SWD), downward longwave radiation (LWD), sensible heat flux (SHF) and latent heat flux (LHF). (c, d), same as (a) but for surface albedo and average total cloud cover. All data in (a-c) comes from RACMO. The albedo is computed based on SW_{net} and SWD. The cloud cover data is from ERA5.

Reviewer 2 — General Comment 4	(R2, GC4)
4) The amount of the melt is the key contribution of this paper. Therefore, the melt should also be presented in unit of mean (regional) specific melt, in addition to the regional total melt this paper is presenting. The specific melt will convey readers quickly to energy budget, while total melt requires information of the surface area the authors worked.	

We thank the reviewer for this comment. Region-integrated surface melt rates are a common way to present results in most studies, as they inform readers about the total volume of surface melt produced across the AP. However, since our study links surface melt to the surface energy budget and circulation anomalies, we agree that providing mean specific melt rates, as you recommended, enhances the analysis. Accordingly, we have added values below the y-axis tick labels at the same time, to represent the spatially averaged surface melt rates (e.g., in Fig. 1b and Fig. 2a2).



New Fig. 1b Long-term NDJF surface melt change from 1979 to 2022. The solid red line represents the results after the five-year low-pass Butterworth filtering. The values in parentheses below the left y-axis tick labels represent the spatial-averaged melt rates (mm w. e. yr^{-1}). The changes are also shown as a percentage (right y-axis, relative to the 1981–2010 mean).

Reviewer 2 — General Comment 5	(R2, GC5)
5) Fig. 1 (b) shows an interesting fluctuation of the melt. The readers will notice the earlier peak of the melt around 1990-1995. CO_2 concentration was then about 70 ppm smaller than the recent years this paper is dealing. It is nice if the authors could explain how it then happened. This step will enhance the importance of the present melt increase also.	

We are grateful for the reviewer's constructive suggestions. Indeed, Fig. 1b shows the earlier peak of the AP surface melt rates around 1990-1995 and readers will notice this interesting point. In our study, we think the thermodynamic contribute the enhancement of the surface melt during the early and last period. Meanwhile, we try to explain the possible thermodynamic factors like the "greenhouse gas forcing" favoring the longwave radiation. However, this inevitably leads readers to think about the smaller CO_2 concentration then.

We are grateful for the reviewer's constructive suggestions. Indeed, Fig. 1b displays an earlier peak in AP surface melt rates around 1990–1995, an interesting point that readers will likely notice. In our study, we suggest that thermodynamics contributes to the enhancement of surface melt during both the early and recent periods. While we attribute these changes to thermodynamic factors such as "greenhouse gas forcing" favoring downward longwave radiation, this may lead readers to consider the lower CO_2 concentrations present at that time.

Firstly, our finding of an earlier surface melt peak around 1990–1995 is consistent with previous studies based on models and satellite data (Tedesco and Monaghan, 2009; Costi et al., 2018). Regarding the explanation for this peak, New Supplementary Fig. 3 illustrates changes in surface energy fluxes, albedo, and cloud cover anomalies. We find that net longwave radiation and total turbulent flux both exhibited earlier peaks during that time, coinciding with a peak in air temperature. Furthermore, net shortwave radiation showed a positive anomaly in 1990–1993 due to low surface albedo anomalies. Although CO_2 concentrations were lower than in recent years, the cloud cover anomaly reached an earlier peak, resulting in positive downward longwave radiation.

Compared to this earlier peak, the recent enhancement of surface melt shows similar surface energy flux anomaly conditions. We observe an increase in downward longwave radiation driven by both increasing cloud cover and sensible heat flux. Both the increased cloud cover and rising CO_2 concentrations contribute to the intensification of downward longwave radiation.

"*Our findings indicate that the thermodynamic signals are related to the recent increase in longwave radiation and turbulent heat flux. In addition, we also note that there is a surface melt peak in the early 1990s, similar highly positive longwave radiation and turbulent heat flux are also found along with early-peak of cloud cover anomalies.* "

Reviewer 3:

General Comment	(R3, GC)

Summary

The paper addresses a relevant issue, however the methodology they followed is not doing justice to the dynamics involved. For example, for a southward./northward flow in the Antarctic peninsula a warming/cooling and surface melting/freezing can be expected, which is not the case here for the first two modes. The authors are arguing that this is due to the thermodynamic effect. I can agree with that, provided that some computations regarding the heat advection and how significant or nonsignificant it is. If it is thermodynamic, what are the thermodynamic variables involved.

For the remaining modes wind direction and melting more or less agree.

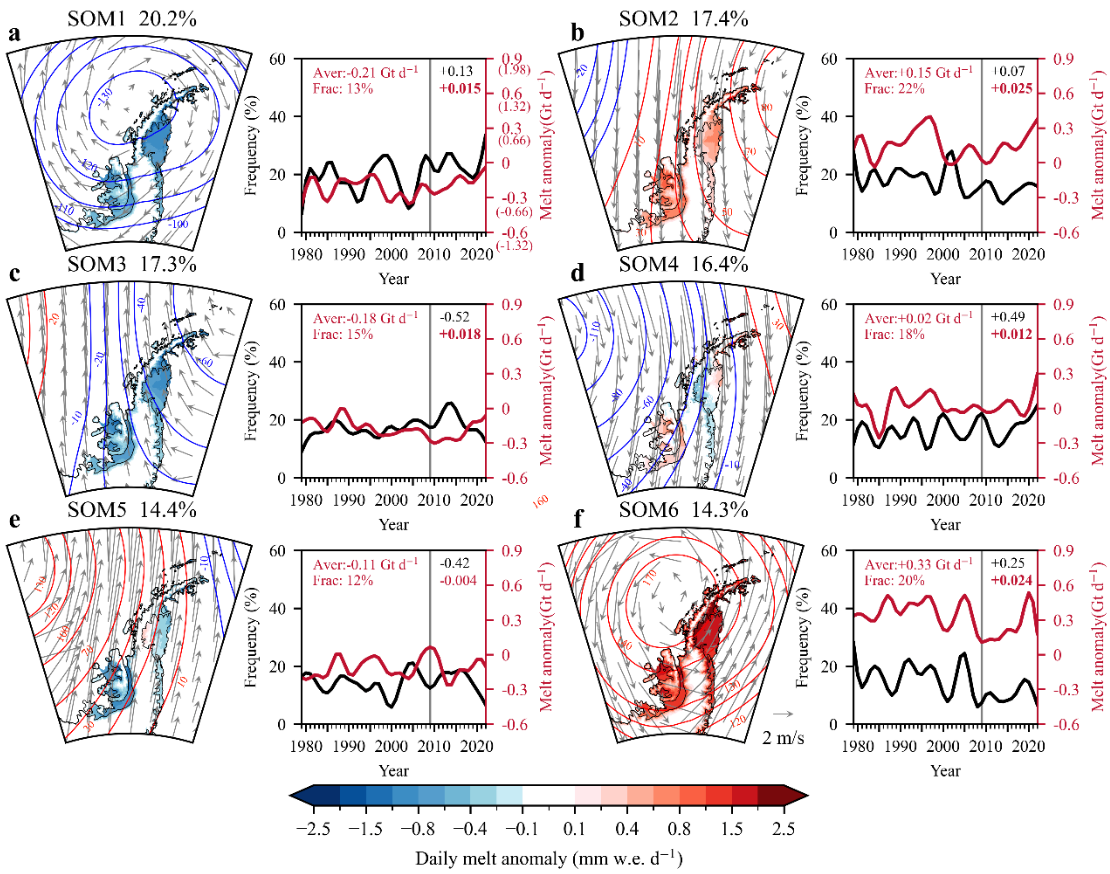
We thank the reviewer for these constructive comments and thoughtful feedback. In response, we have carefully revised the manuscript by incorporating the suggestions from all reviewers. Firstly, we appreciate your concerns regarding the physical interpretation of the first two SOM modes. In the previous version, we used wind and cloud cover to explain these modes but realized that the underlying logic was not fully articulated. We have re-examined our classification method, surface energy balance (SEB) anomalies, cloud cover, and other contributing factors, providing a more robust explanation for all modes. Additionally, we have quantified horizontal and vertical heat anomalies and expanded the descriptions of the thermodynamic variables involved.

Detailed answers and further explanations are provided in the following responses. Our replies are highlighted in blue text, while all corresponding changes in the revised manuscript are marked in green, with revised line numbers (of the version with track changes) indicated where relevant.

Reviewer 3 — General Comment 1	(R3, GC1)
The downward radiation components in the supplementary diagrams show a zonal sea –saw pattern for mode 1 and 2, which is also not reflected in the melt patterns for these nodes.	

We thank the reviewer for their meticulous reading of our manuscript and for raising this important point. In the previous version, we attributed melt anomalies under individual patterns to the wind field and cloud cover; however, the underlying physical logic was not clearly articulated.

In the revised manuscript, we have refined our classification method. Previously, we applied the SOM to standardized variance data, which failed to preserve the spatial structure of the variance and could not capture regional characteristics with strong signals. Following established studies (Hewitson and Crane, 2002; Rogers et al., 2022), where SOM inputs are typically climate anomalies or raw data, we have re-classified the 500 hPa geopotential height anomaly maps from the ERA5 reanalysis into six patterns, ordered by their frequency.



New fig. 2 Circulation patterns and corresponding daily surface melt anomaly

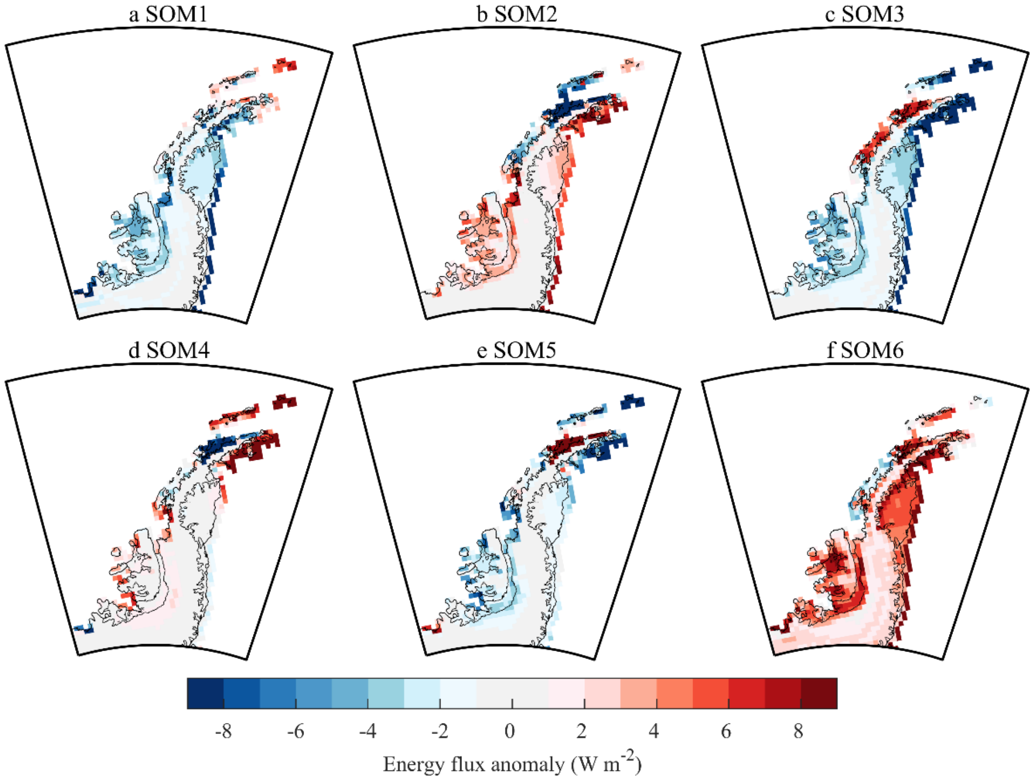
Regarding these six circulation nodes, negative surface melt anomalies occur in SOM1, 3, and 5, while positive anomalies are found in SOM2, 4, and 6. Since surface melt is a result of surface energy budget (SEB) anomalies, it is essential to first examine radiation and turbulent flux anomalies. New Supplementary Fig. 4 and 5 illustrate the anomalies of the total SEB (excluding ground heat flux) and its specific components. We categorize these SOMs into two groups for further discussion: SOM2, 3, 5 and SOM1, 6.

For SOM2–5, positive SEB anomalies are primarily driven by downward longwave radiation (LWD). These four modes are characterized by strong meridional wind anomalies: southward (northward) winds over the AP lead to negative (positive) cloud cover anomalies (New Supplementary Fig. 6), resulting in decreased (increased) LWD and corresponding melt anomalies. In this category, although cloud cover also impacts downward shortwave radiation (SWD), the stronger LWD anomalies mask the opposing SWD signals.

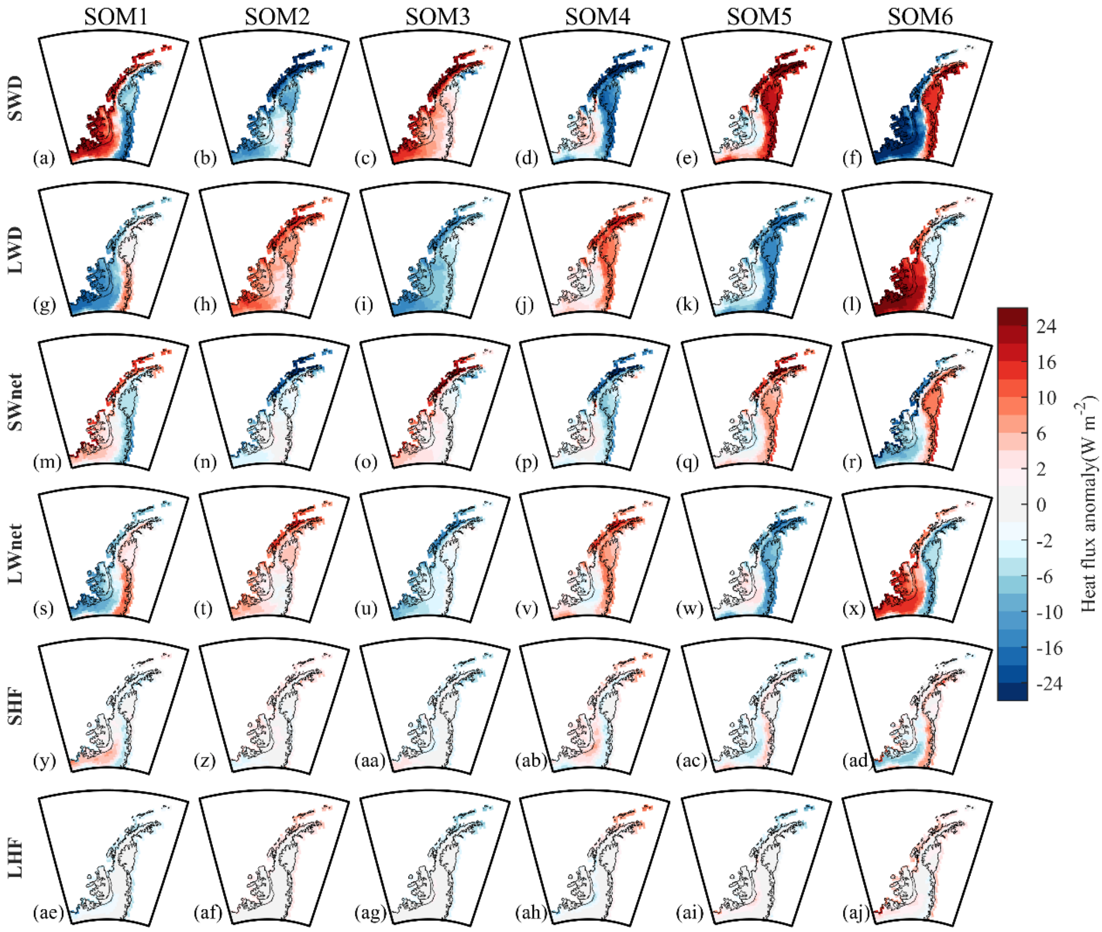
SOM1 and SOM6 are characterized by strong zonal winds, which induce opposing wind patterns—and thus opposing cloud cover and radiation anomalies—between the east and west AP. When air masses cross the high mountain ridge south of the AP, moisture decreases on the leeward side, causing negative cloud cover anomalies. In case of SOM6, increased LWD masks decreased SWD in the west AP, while decreased LWD is masked by increased SWD in the east, ultimately leading to positive total SEB anomalies across the entire peninsula. This explains why surface melt anomalies do not exhibit a zonal “seesaw” pattern despite the strong zonal winds.

Generally, LWD dominates surface melt anomalies in patterns without strong zonal winds, whereas both SWD and LWD influence the net SEB in the west and east AP, respectively, under strong zonal flow. We acknowledge that fully explaining melt anomalies under all SOM nodes—especially those with strong zonal winds—is challenging due to the complex topography of the AP. While other potential factors may play a role, an exhaustive explanation of every mechanism is beyond the scope of this study. Instead, we focus on the total SEB anomalies as the primary driver.

Finally, we thank the reviewer for suggesting the analysis of heat advection. Using ERA5 data, we calculate the vertical and horizontal heat advection at 850 hPa over the AP following Xu et al. (2021). We find that under strong zonal winds (SOM1 and 6), vertical heat anomalies show opposing signs on windward and leeward slopes (e.g., over the Larsen C Ice Shelf in SOM6). However, on the windward slopes (west AP in SOM6), positive horizontal heat advection cannot fully offset the negative vertical heat anomalies. Given that heat advection does not adequately explain low-level temperature and surface melt anomalies of each SOM modes in this topographically complex region, we have chosen to use SEB anomalies—the most direct driver—to interpret the melting anomalies in each circulation mode.



New Supplementary Figure 4: Surface energy anomaly under each pattern. Shading in maps represents daily surface energy anomalies (SW_{net}+LW_{net}+THF) relative to the average calendar daily melt during the NDJF period spanning from 1979 to 2022.



New Supplementary Figure 5: Surface energy and air temperature anomaly under each pattern. Shading in maps represents daily surface energy anomalies (SW_{net}+LW_{net}+THF) relative to the average calendar daily melt during the NDJF period spanning from 1979 to 2022. The low-pass filtering curve graphs in right illustrate the changes in average energy anomalies and air temperature during 1979-2022.

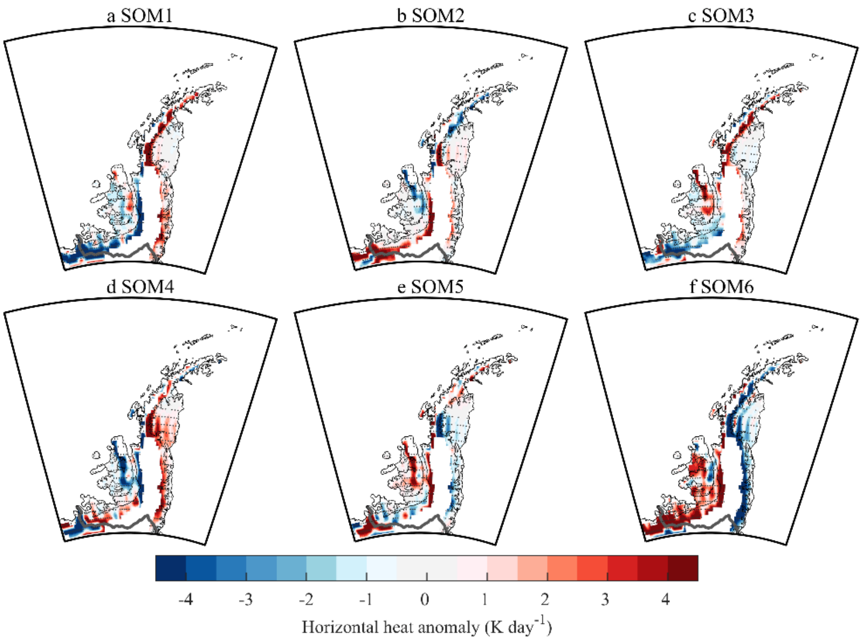


Figure: 850 hPa horizontal heat anomaly under each pattern. The Monte Carlo permutation test (randomly permute the order of years) is used to check the significance. The dots represent passing 95% confidence level. The AP regions with mean surface pressure lower 850 hPa are masked.

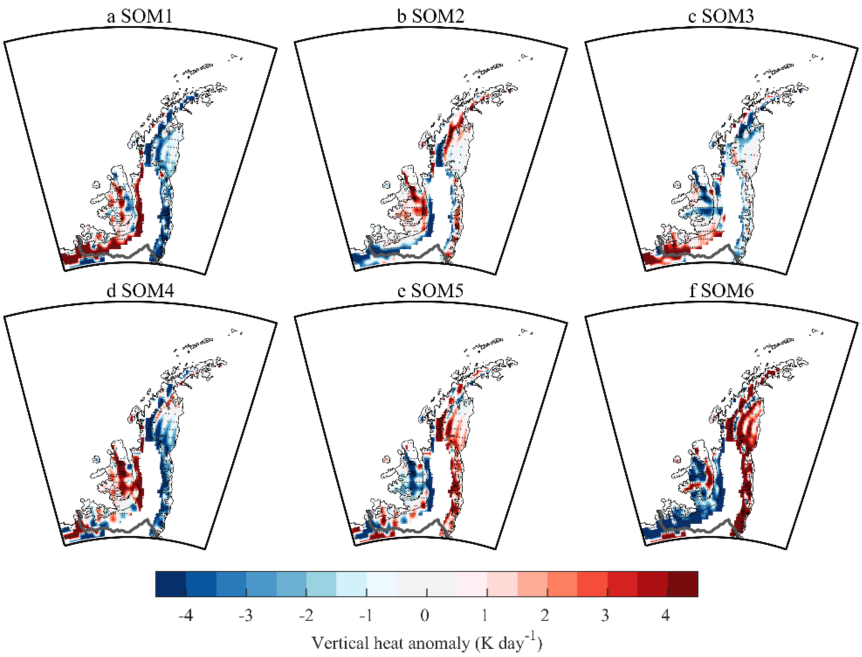


Figure: 850 hPa vertical heat anomaly under each pattern. The Monte Carlo permutation test (randomly permute the order of years) is used to check the significance. The dots represent passing 95% confidence level. The AP regions with mean surface pressure lower 850 hPa are masked.

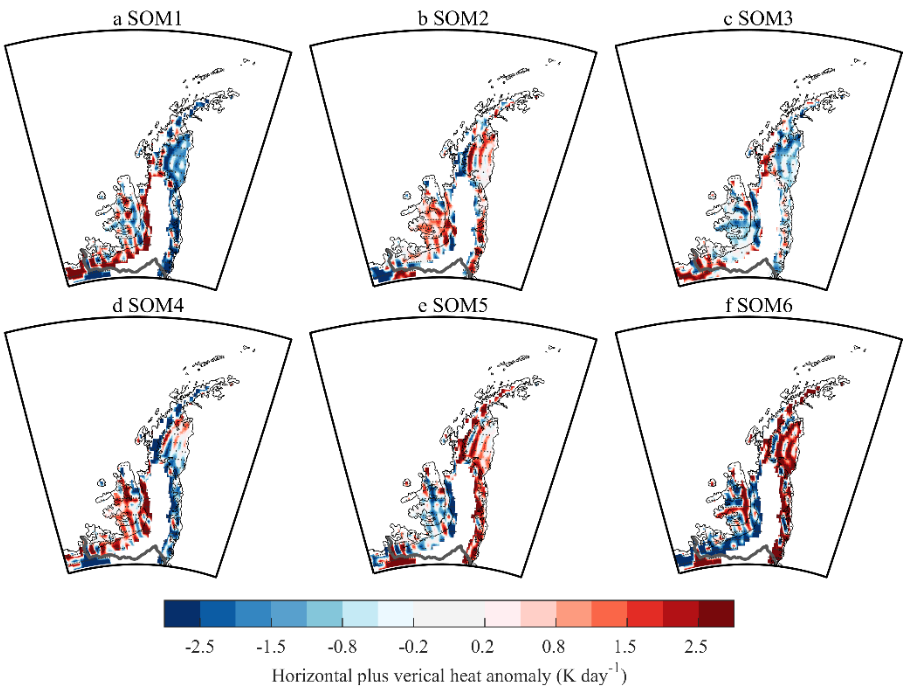


Figure: 850 hPa Horizontal plus vertical heat anomaly under each pattern. The Monte Carlo permutation test (randomly permute the order of years) is used to check the significance. The dots represent passing 95% confidence level. The AP regions with mean surface pressure lower 850 hPa are masked.

Reviewer 3 — General Comment 2	**(R3, GC2)**
Had the authors checked the atmospheric circulation patterns during the maximum and minimum of the melting in the AP? Does that compare with the SOM analysis?	

We thank the reviewer for their very helpful comments. Following this suggestion, we have examined the atmospheric circulation patterns and corresponding surface melt anomalies during periods of maximum and minimum melting in the AP.

First, we statistically analyzed the top and bottom 10th percentiles of daily NDJF melt anomalies. We investigated the mean surface melt and circulation patterns associated with these high- and low-melt events and calculated the occurrence probability of each event type across the six SOM nodes.

Our analysis shows that 31.6% and 30.4% of low-melt events occurred in SOM1 and

SOM3, respectively. Conversely, 49.0% and 30.4% of high-melt events were associated with SOM6 and SOM2, respectively. The average surface melt and circulation anomalies for these extremes, as shown below, are highly consistent with their corresponding SOM nodes. In summary, our circulation classification effectively captures both low-melt and high-melt events.

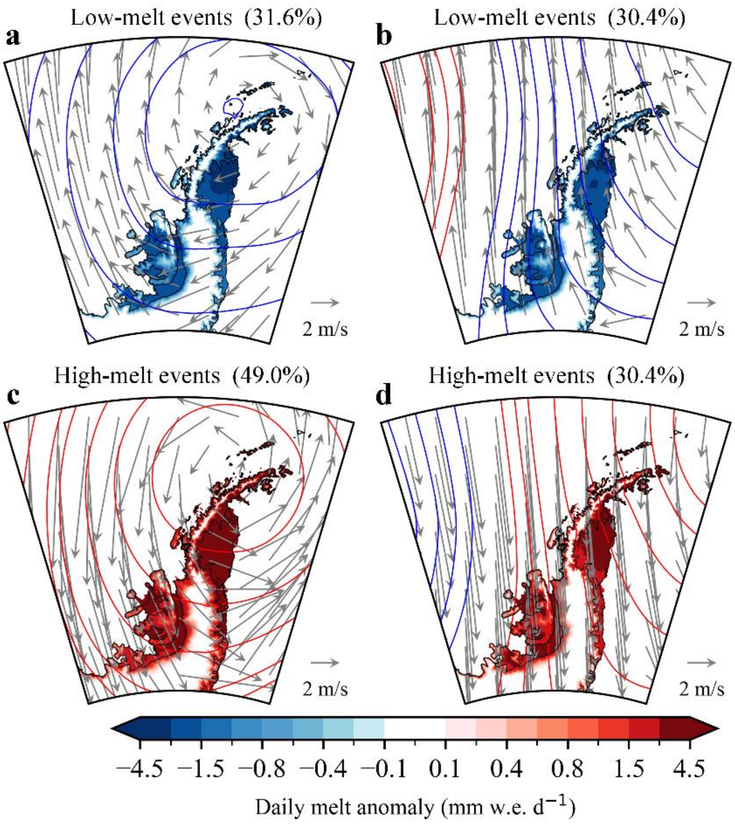


Figure: Average circulation and surface melt anomalies during two most numerous types of high and low melt events.

Reviewer 3 — General Comment 3	(R3, GC3)
I find the analysis is poorly explained and the non association with the dynamic and the association with the thermodynamic processes are not convincingly presented.	

We thank the reviewer for the insightful comment. We apologize for the lack of a comprehensive explanation regarding the non-association with dynamic processes and the association with thermodynamic processes. In the revised manuscript, we have reorganized the relevant sections and incorporated statistical calculations to clarify this point.

First, we decomposed the total surface melt variability into two components: changes in melt frequency (linked to the occurrence of distinct circulation patterns) and changes in melt intensity (represented by mean melt anomalies). We define the “dynamics” of surface melt change as variations in atmospheric circulation patterns, while “thermodynamics” refers to factors affecting melt intensity independent of circulation changes. Consequently, the dynamic contribution represents the portion of the trend explained by changes in the frequency of each circulation pattern, assuming a constant mean surface melt intensity.

Based on Fig. 2, we examined the changes in circulation frequency and surface melt intensity, which are linked to dynamics and thermodynamics, respectively. During the 2009–2022 period of enhanced surface melt, we found no significant trend in the frequency of individual circulation patterns. In contrast, the average surface melt intensity showed a significant increase across five patterns (e.g., 0.024 and 0.025 Gt d^{-1} $year^{-1}$ for SOM 2 and 6, respectively).

Hence, the recent enhancement in AP surface melt cannot be fully explained by changes in the frequency of individual patterns, implying the critical role of other thermodynamic factors influencing melt intensity. Following the methodology of Horton et al. (2015), we quantified the specific contributions of thermodynamics and dynamics. We found that thermodynamics contributed 80% of the total trend in RACMO2.3p2, a trend evident across the entire AP (Fig. 3).

In addition, we have analyzed the specific surface melt trends resulting from thermodynamics, dynamics, and their interaction within each SOM mode, as recommended. We also find that thermodynamics have resulted in significantly increasing trends in five patterns like SOM2 (0.43 Gt $year^{-1}$, $p<0.05$). Ultimately, we conclude that thermodynamics dictates the recent enhancement of AP NDJF surface melt rates.

We have revised the descriptions in the “Circulation classification for AP surface melt” section to make this explanation clearer for the reader (lines 136-144 of the revised manuscript):

"Here, we check the changes in circulation frequency and surface melt intensity, respectively. During 2009–2022—the period of significantly increasing melt—no significant trend is observed in the frequency of any individual circulation pattern. Instead, the average surface melt intensity shows a significant increase in five patterns, for instance 0.024 Gt d^{-1} $year^{-1}$ for SOM 6 ($p<0.05$, equivalent to 6% $year^{-1}$ relative to the 1981-2010 mean). This suggests that the recent enhancement in AP surface melt cannot be explained by shifts in atmospheric circulation frequency alone, highlighting the critical role of the intensification within-pattern which influences the surface melt intensity."

Based on these new results, we have added descriptions regarding the relative contributions of thermodynamics and dynamics, as well as the relevant thermodynamic processes. The following text has been added to lines 172–202 of the revised manuscript:

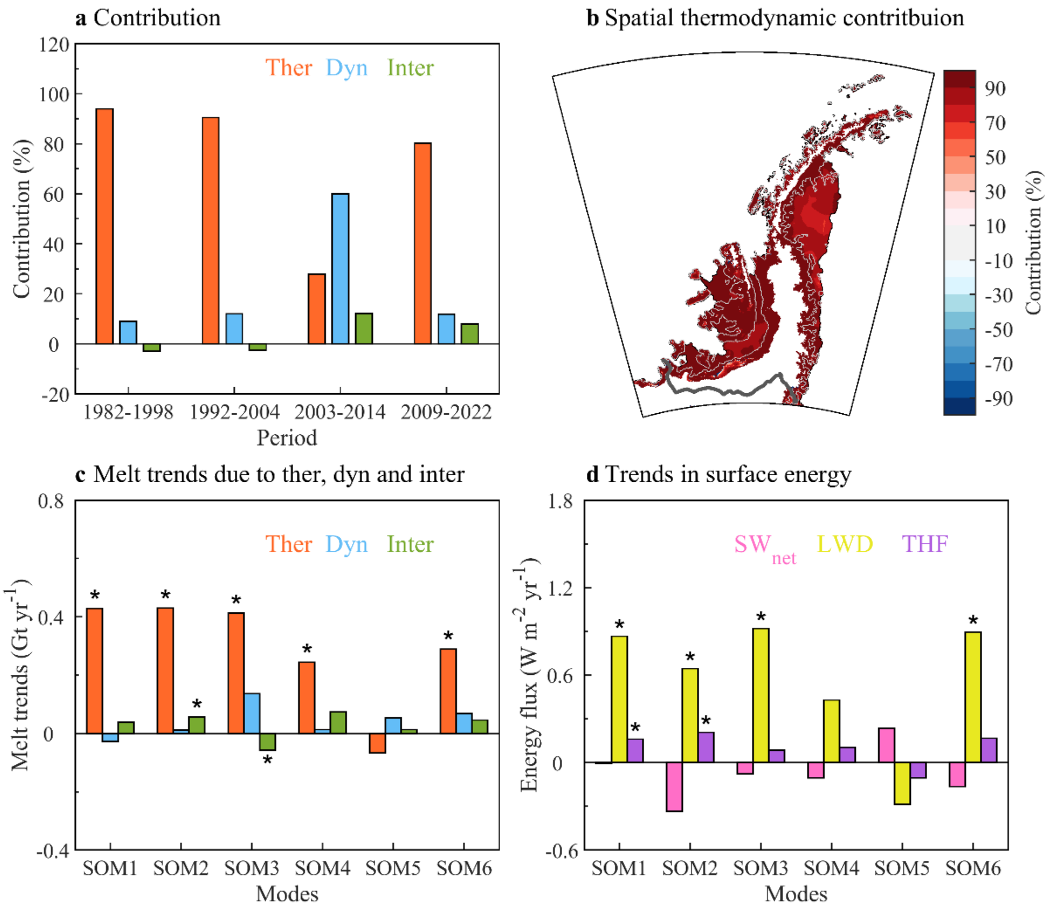
"Across most periods characterized by significant AP surface melt trends, thermodynamic processes—rather than large-scale circulation shifts—dominate, typically accounting for over 50% of the total trend (Supplementary Fig. 7). For the four typical periods with significant trends (Fig. 1c), Fig. 3a specifically shows the contributions of three components, respectively here. While dynamics were responsible for 60% of the melt changes only during the 2003–2014 period, thermodynamics have dominated the post-2009 intensification, contributing 80% of the trend in RACMO and ~85% in MAR v3.12 (Supplementary Fig. 8a). Meanwhile, thermodynamic dominance is evident across the entire AP in RACMO and MARv3.12 (Fig. 3b and Supplementary Fig. 8d) . This strong inter-model agreement underscores the robustness of the thermodynamic forcing in driving recent melt enhancement across the entire AP. Decomposition of individual SOM modes since 2009 further confirms this point (Fig. 3c). Thermodynamics have triggered significantly increasing trends in five patterns,

such as SOM 2 (0.43, $p < 0.05$). Overall, these results indicate that thermodynamics dictate the recent acceleration of AP surface melt. The surface melt trends of all SOM nodes since 2009 have been decomposed in the same way. Thermodynamics have resulted in significantly increasing trends in five patterns like SOM2 (0.43 Gt year^{-1}, $p<0.05$, 86% of total melt trend in SOM2). Overall, we conclude that thermodynamics dictate the recent enhancement of AP surface melt.

To elucidate the specific thermodynamic drivers, we analyze trends in surface energy budget anomalies for each circulation modes since 2009 (Fig. 3d). The results show significant increases in downward longwave radiation across four patterns, notably SOM 2 (0.8 W m^{-2} year^{-1}, $p<0.05$) and SOM 6 (0.9 W m^{-2} year^{-1}, $p<0.05$). Turbulent heat fluxes also exhibit upward trends, reaching statistical significance only in SOM 1 and SOM 2. In contrast, net shortwave radiation does not contribute to the surface energy balance increase, primarily due to a decline in downward shortwave (Supplementary Fig. 3b). These results suggest that thermodynamic intensification—modulating the increase in longwave radiation and turbulent heat flux over the AP—results in the recent enhancement of surface melt since 2009. This process is likely driven by atmospheric warming and cloud-radiative forcing, potentially amplified by positive feedback involving regional sea-ice loss and SST) warming, which likely intensify turbulent heat fluxes (Supplementary Fig. 9).”

Reviewer 3 — General Comment 4	(R3, GC4)
I recommend a major revision with computations explaining dynamical/thermodynamical processes along with the SOM for robust interpretations.	

We are grateful for your constructive suggestions, which have significantly improved the quality of this paper. We have carefully revised our manuscript by incorporating your comments alongside those from the other reviewer. As recommended, we have calculated the horizontal and vertical heat anomalies for the six SOM nodes; please refer to our previous response for the relevant results and detailed explanations. More importantly, we have examined the specific surface melt trends resulting from thermodynamics, dynamics, and their interactions based on our SOM results. We find that thermodynamics contributed to the increase in surface melt during 2009–2022, with significant positive trends observed in five SOM modes. In this study, we define thermodynamics as processes—largely independent of atmospheric circulation variability—that affect surface energy balance anomalies. We have also analyzed the specific surface energy anomalies for the six SOM modes (Supplementary Fig. 3–5) and their respective trends during 2009–2022 (Fig. 3d). Our findings indicate that downward longwave radiation and turbulent heat flux are critical thermodynamic processes, exhibiting significantly increasing trends in nodes such as SOM1, 2, 3 and 6. Overall, our descriptions and analyses have been improved by benefiting from your insightful feedback, and we have incorporated additional results alongside the SOM analysis.



New Fig. 3 Contribution of thermodynamic, dynamic and interaction processes melt trend. **a** Contributions of individual components to surface melt trends during four typical periods. **b** Spatial thermodynamic contributions during 2009-2022. **c** Melt trends due to thermodynamics, dynamics and the interaction in each SOM node. **d** Trends in mean anomalies of net shortwave radiation (SW_{net}), downward longwave radiation (LWD) and turbulent heat flux (THF) in each modes during 2009-2022 NDJF. The stars in (**c, d**) represent the passing the (Hamed-Rao modified) Mann-Kendall significance test of the confidence level of 95%.

Reference:

Hamed, K. H. & Rao, A. R. A modified Mann-Kendall trend test for autocorrelated data. *J. Hydrol.* **204**, 182-196 (1998).

Kohonen, T. The self-organizing map. Proc. IEEE 78, 1464-1480 (1990).

Kuipers Munneke, P., van den Broeke, M. R., King, J. C., et al. Near-surface climate and surface energy budget of Larsen C ice shelf, Antarctic Peninsula. *Cryosphere*, **6**, 353–363, 2012.

Costi, J. et al. Estimating surface melt and runoff on the Antarctic Peninsula using ERA-Interim reanalysis data. *Antarct. Sci.* **30**, 379-393 (2018).

Tedesco, M. & Monaghan, A. J. An updated Antarctic melt record through 2009 and its linkages to high-latitude and tropical climate variability. *Geophys. Res. Lett.* **36**, L18502 (2009).

Hewitson, B. C., & Crane, R. G. Self-organizing maps: applications to synoptic climatology. *Clim. Res.* **22(1)**, 13-26 (2002).

Rogers, C. D., et al. Sixfold increase in historical Northern Hemisphere concurrent large heatwaves driven by warming and changing atmospheric circulations. *J. Clim.* **35(3)**, 1063-1078 (2022).

Ryan, J. C., Cooper, M. G., Cooley, S. W., Rennermalm, Å. K. & Smith, L. C. Meltwater ponding has an underestimated radiative effect on the surface of the Greenland Ice Sheet. *Nat. Commun.* **16**, 8274 (2025).

Xu, M. et al. Dominant role of vertical air flows in the unprecedented warming on the Antarctic Peninsula in February 2020. *Commun. Earth Environ.* **2**, 133 (2021).

The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics

Response to reviewers

Reviewer 1:

General comment	(R1, GC)

Summary

Review of Zhang et al. manuscript "The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics" submitted to Nature Communications.

This manuscript investigates surface melt across the Antarctic Peninsula (AP) and its ice shelves from 1979 to 2022. The authors document a marked intensification of surface melt after 2009 in two regional climate models (RCM), following more than a decade of predominantly reduced melt. They show that this recent increase is statistically significant and is primarily related to thermodynamic changes, while it cannot be explained with the variability in atmospheric circulation alone. For this the authors apply the method of self-organizing maps (SOMS) to identify distinct circulation patterns and identify the contribution to surface melt from dynamics (frequency of individual circulation patterns), thermodynamics (intensity of melt events) and the contribution of interaction between both.

The revised manuscript has been carefully reviewed, and I am satisfied that the authors have adequately addressed all of my previous comments. The quality of the paper has improved accordingly, and I believe it is now suitable for publication.

We sincerely thank the reviewer for the positive feedback. Thanks for your previous careful review and all helpful comments and suggestions.

Reviewer 2:

General Comment	(R2, GC)
Summary (Concerning with the Structure) This reviewer suggested earlier to place "Methods" before the "Results". The authors did not take the advice. As a result you can imagine the surprise in future readers of this paper. They will be confronted by "Results" immediately after "Introduction". The authors' response states "In accordance with the formatting guidelines of the journal, the detailed Methods section is placed at the end of the manuscript." This part of the instruction was misunderstood by the authors. This part of the guideline is a suggestion concerning with the long and detailed description of the methods. The manuscript needs a breif presentation of the "Methods" before the "Results". The problem of integrating the presentation of the methods in the results is obvious. The "Results" has become too long, to call it the result. Usually, the "Results" should be brief and concise. The detailed presentation of the methods can still be placed near the end or in the Supplementary material.	

We sincerely thank the reviewers for their comments and positive feedback, which have greatly helped us improve the quality of our manuscript. We have carefully revised the paper in accordance with your suggestions and the feedback from other reviewers. Our replies are highlighted in blue text, while all corresponding changes in the revised manuscript are marked in green, with revised line numbers (of the version with track changes) indicated where relevant. Our detailed responses to each comment are provided below.

For the txt structure, we realize the reviewer's concerns and agree to briefly present the methodology in this study. Given the strict journal formatting guidelines (https://www.nature.com/documents/ncomms-formatting-instructions.pdf), please forgive us for not adding a new section of “Methods” before the “Results”, and we finally present a brief description of methodology at the end of section of “Introduction” as follows. Meanwhile, we have removed some relevant detailed presentations of methodology in the section of “Results” to shorten the text length. Additionally, we put the detailed methods in the end of manuscript after "Discussion" and other methods used in supplement computation in the end of supplementary document.

"*In this work, to confirm the potential recent increase in AP surface melt rates, here we employ output from the advanced polar regional climate model (RCM), RACMO version 2.3p2*[33]*, statistically downscaled from 27 km to a 2 km × 2 km grid*[34]*. RACMO2.3p2 faithfully represents AP surface melt volume/rate, compared with melt estimates from surface energy balance model*[35] *in automatic weather stations (R_{daily}^2 = 0.6, $p < 0.01$; $RMSE_{daily}$ = 1.72 mm w.e. day^{-1} between November and February). Firstly, in conjunction with another RCM (MAR v3.12), we quantify changes in AP surface melt rates. As a second step, we separate total surface melt variability into two components: changes in melt frequency (related to dynamics) and melt intensity (related to thermodynamics), which is separated by a spatial cluster method self-organizing map (SOM). Finally, we further decompose the melt trends into the dynamic and thermodynamic contributions by holding frequency and intensity constant, respectively*[30,36] *(See Methods section).*"

Reviewer 2 — Specific Comment 1	**(R2, SC1)**
(Sientific Part) The following comments are rather minor, but certainly help future readers. 1) Presenting the amount of melt, specific melt is necessary in addition to the regional total. The specific melt allows the direct comparison with the past and also with other regions. This suggestion applies both for the text and figures.	

We acknowledge the reviewers' comment and apologize for the previous omission of the specific melt data in the main text. To rectify this, we have thoroughly updated the revised manuscript: the exact melt values are now explicitly provided in the text whenever surface melt is mentioned, and they have also been added to Fig. 3c and 4a for clarity.

Reviewer 2 — Specific Comment 2	(R2, SC2)
2) Line 108: Caption of Figure 1a. State the concerned period. This is stated in the text. However, in principle, the text and figures should be regarded independent of each other.	

We thank the reviewer for this comment, and we have added the period for Figure 1a in line 113 of the revised manuscript as follows:

"***a*** *Spatial distribution of multi-year average surface melt from 1979-2022 November to February of the following year (NDJF).* "

Reviewer 2 — Specific Comment 3	(R2, SC3)
3) Energy balance components presented in this paper are the result of computations. It is necessary to compare the computational results against actual measurements? This may be too demanding for turbulent fluxes. The comparison with the radiation fluxes must be possible. The Antarctic is well covered with stations with well equipped radiometers.	

We thank the reviewer for these constructive and thoughtful comments and suggestions. In the previous version, we added a series of SEB results from RACMO2.3p2. We agree to introduce the reliability of regional climate model to readers in aspect of surface energy balance. In fact, previous studies have assessed the performance of RACMO2.3p2 in SEB components using observed shortwave radiation and longwave radiation in Antarctic (Van Wessem et al., 2018; Jakobs et al., 2020; van Dalum et al., 2022). Their results have proven that RACMO2.3p2 can accurately capture the Antarctic radiation fluxes and surface energy balance well. We have added relevant descriptions in lines 296-298 of revised manuscript:

"Overall, RACMO2.3p2 can present Antarctic near surface climatology and surface energy balance conditions well[33,35,51]*."*

Reference:

Jakobs, C. L. et al. A benchmark dataset of in situ Antarctic surface melt rates and energy balance. *J. Glaciol.* **66**, 291-302 (2020).

van Dalum, C. T., van de Berg, W. J. & van den Broeke, M. R. Sensitivity of Antarctic surface climate to a new spectral snow albedo and radiative transfer scheme in RACMO2.3p3, *Cryosphere* **16**, 1071–1089 (2022).

van Wessem, J. M. et al. Modelling the climate and surface mass balance of polar ice sheets using RACMO2 – Part 2: Antarctica (1979–2016), The *Cryosphere* **12**, 1479–1498 (2018).

Reviewer 3:

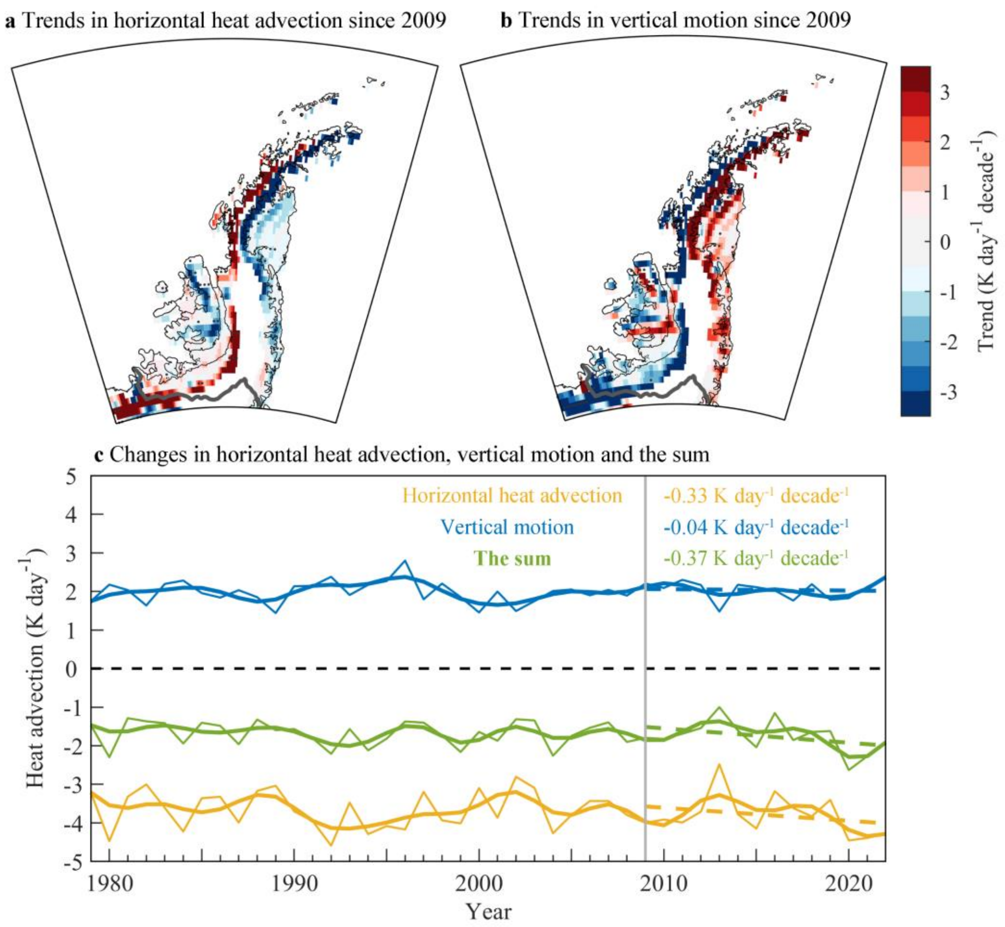
General Comment	(R3, GC)
Summary It's happy to see that the authors have carefully and comprehensively addressed the queries. My main concern was a lack of a convincing explanation of the significance of thermodynamic over dynamic factors controlling Antarctic surface melt. Largely, the authors use frequency of specific SOM patterns and melt frequencies obtained from satellite. My suggestion was to examine the heat advection vis a vis thermodynamic effect. Though the authors have gone at great length rebutting the document banking on SOM analysis and radiation, still, I find an analysis of heat advection missing. A discussion, at least, comparing the SOM and satellite derived dynamical effect to computed heat advection using temperature and winds will clear any ambiguity brought by the categorisation based on an increase in brightness temperature for melt anomalies, frequency of specific patterns in SOM and real advection. The authors need not calculate this for every circulation pattern, but at least, a theoretical explanation would suffice. Also across the Antarctic Peninsula, foehn effect warms the surface. These factors may also be included in the discussion. I recommend this revision before the paper is published. This could be a minor revision for the authors.	

We sincerely thank the reviewer for the constructive comments and thoughtful feedback. In response, we have carefully revised the manuscript by incorporating the suggestions from all reviewers. Our replies are highlighted in blue text, while all corresponding changes in the revised manuscript are marked in green.

For the main concern of the reviewers, we have calculated the average horizontal heat advection ($-V \cdot \nabla T$) and vertical motion (vertical adiabatic warming and vertical advection, $\left(\frac{RT}{C_p p} - \frac{\partial T}{\partial p}\right)\omega$) in NDJF over the Antarctic Peninsula from 1979 to 2022 (New Supplementary Fig. 12). Given the AP altitude and narrow terrain, we chose 850 hPa as target pressure level to check the dynamical tendency. Through computing horizontal heat advection and vertical motion, we try to check the heat advection and föhn winds effect.

Finally, we find that average NDJF horizontal heat advection and vertical motion over the AP do not show obvious enhancement during the recent period of melt-increase (2009-2022), with opposite decreasing tendency. Spatially, the horizontal advection and vertical motion show decreasing trends in eastern and western AP, respectively. For those increasing dynamical trends, the regions with significant signals are very few. Therefore, heat advection and föhn winds cannot explain the changes and trends since 2009. We fully acknowledge that dynamics play critical role in occurrence of strong surface melt events over the AP and influence thermodynamics. The dynamics can trigger AP high melt events like foehn effect in the east AP, Larsen C ice shelf. However, for the changes and trends of AP surface melt, it is explained by the changes in melt intensity, i.e., thermodynamics effects, which we want to clarify. We have added relevant descriptions in section of discussion as follows:

"The dominance of thermodynamic forcing does not imply that circulation variability is unimportant. Our decomposition associates daily melt with concurrent atmospheric circulation states, yet thermodynamic anomalies can also arise as a lagged response to prior circulation events. For the occurrence of strong surface melt over the AP, atmospheric circulation play critical role. Recent findings support that atmospheric rivers, föhn winds and teleconnections trigger AP surface melt and climate[20,27,46,47]. Circulations influence cloud cover anomalies and radiation budget as well as cause horizontal and vertical heat anomalies, consequently triggering melt anomalies. However, these dynamics cannot explain the recent increase and melt trends in AP surface melt from 2009 to 2022 fully. Specifically, for example, average NDJF 850 hPa horizontal heat advection and vertical motion don't show obvious enhancements over the AP since 2009 (Supplementary Fig. 12). However, thermodynamics like downward longwave radiation and turbulent flux contribute to the recent enhancement of surface melt through increasing melt intensity (Fig.3 and Supplementary Fig. 2). Our results show the dynamics only dictate the significant decreasing trends during 2000s, when there is a decline in the occurrence frequency of circulation patterns related to negative surface melt anomalies (Fig. 2)."



New Supplementary Figure 12: Changes in average NDJF dynamical tendency (K day^{-1}) over the Antarctic Peninsula from 1979 to 2022 from ERA5. **a** Trends in average 850 hPa NDJF horizontal heat advection over the AP during 2009-2022. **b** same as **a**, but for 850 hPa vertical motion (vertical adiabatic warming and vertical advection) over the AP during 2009-2022. **c**. Changes in average 850 hPa NDJF horizontal heat advection, vertical motion and their sum over the AP from 1979 to 2022. The bold lines represent the results after the five-year low-pass Butterworth filtering. The regions with surface pressure lower than 850 hPa are masked and not used for computing regional mean. The numbers in **c** indicate the linear trends during 2009-2022 NDJF of regional-average dynamical tendency. All trends in **c** are not significant in statistic. The black dots in **a** and **b** represent passing the (Hamed-Rao modified) Mann-Kendall significance test of the confidence level of 95%.

The recent enhancement of the surface melt over the Antarctic Peninsula dictated by thermodynamics

Response to reviewers

Reviewer 2:

General Comment	(R2, GC)
Summary This reviewer (Reviewer 2) is impressed by the degree of efforts the authors went to revise the manuscript. It reads well. This reviewer recommends the manuscript for acceptance. If I am allowed to comment on a very trivial point (Lines 470, 472, and 556), it is how to write a Dutch family name. For example, it should be "van den Broeke" and not "Van Den Broeke". Even though the name may appear at the beginning, "van" does not become "Van". The way Line 553 presents is correct. In any case, it is better to type in a homogenous manner.	

We sincerely thank the reviewer for the positive feedback. Thanks for your previous careful review and all helpful comments and suggestions. We have revised the writing type of the Dutch family name.

Reviewer 3:

General Comment	(R3, GC)
Summary The authors have addressed the queries on the relative role of dynamics and thermodynamics. I recommend the manuscript for publication in nature communication.	

We sincerely thank the reviewer for the positive feedback and previous constructive comments which have greatly improved the quality of our manuscript.