Response to reviewer comments for the article “Sub-seasonal variability and multi-year trends in glacier area change and ice speed on the Antarctic Peninsula”

We thank the editor and the reviewers for taking the time to read the article and for their thoughtful assessment of the manuscript. This document compiles all reviewer comments together with our responses, so that each reviewer may see the others. Each review is reproduced here in full. Responses to general comments are addressed directly and are indicated in blue, while responses to specific comments are presented in tabulated form.

Following the reviewers’ general comments, we have made substantial changes to the seasonality and correlation analyses. To strengthen the seasonality analysis, we now assess the timing of annual peak velocities and terminus area changes for the entire AP, and separately for eastern and western glaciers. This addition allows for improve comparison and for supporting a more detailed discussion of potential environmental drivers. We also revised the correlation analysis in response to the concerns raised by both reviewers. Both pointed out that correlating first-order differenced time series is not well suited for assessing the coupling between terminus area changes and ice velocity dynamics without further concerted efforts to separate and understand the processes at play, because the two variables may be linked across a variety of different timescales. We agree with this criticism. A full separation of the different processes involved would require much more process-specific analysis (as done by Ultee et al. (2022)), and would go beyond the scope of this study. We therefore followed the suggestion of Reviewer #2 and repeated the analysis using detrended time series, which retain the seasonal signal. This allowed us to focus more directly on potential seasonal coupling between terminus area and ice velocity. We now present the full correlation results, including both the sign of correlation and the associated lag, and discuss them in much more detail. In particular, we now distinguish more clearly between relationships that may reflect causal links, shared seasonal forcing, or responses to isolated extreme events followed by dynamic adjustment.

There was general agreement between the reviewers that the manuscript did not make the novelty of the study clear enough, especially in the abstract and conclusion. We revised the manuscript accordingly. The new version states much more clearly that our observations complement existing studies while also providing new insights into longer-term glacier dynamics in specific regions, such as Larsen A, and into the timing and magnitude of seasonal changes, particularly for ice velocity and on the eastern AP and terminus area changes across the entire peninsula. We also substantially expanded the discussion of the seasonal coupling between terminus area changes and ice velocity dynamics, and now emphasize more clearly that, to our knowledge, this is the first assessment of this relationship across a larger set of glaciers in this region. While this first assessment needs further research to disentangle this relationship across different timescales and include other forcing mechanisms, it provides a general overview of potential seasonal coupling and lead-lag behavior between the two variables for AP glaciers.

To reflect the revised analysis and address specific comments, we also updated some of the figures. The original Figure 4 has been split into two separate figures. One figure (Fig. 4) now presents the results of the correlation analysis results, including the sign and lag of the correlations for all investigated glaciers, while the other (Fig. 5) contains the maps of seasonality strength in an enlarged and more readable form with adjusted color scale. We also added panels showing the timing of annual peak velocity and terminus change. In addition, we added a plot (Fig. 6) of the fast ice extent in the Larsen A embayment, which mas manually mapped using the Google Earth Engine Digitizing Tool (GEEDiT) (Lea 2018), to support our discussion of long-term glacier dynamics on the eastern AP.

Finally, following the suggestions of Reviewer #1 to show the major processing steps more clearly, we added plots of all glacier time series to the appendix. For ice surface velocity, these plots show the individual image-pair measurements with the velocity tracking error, the Kalman smoothed speed estimates, as well as the monthly aggregated time series. For terminus area, the plots visualize the original and monthly aggregated time series. In addition, they contain the calculated linear and break-point trends, which were relevant for the categorization of the glaciers. The assigned category is also indicated in each plot to make the grouping more transparent. In addition, we included plots of the timing of annual minimum and maximum terminus extent and velocity for

eastern and western glaciers, as well as temperature and precipitation records for Marambio and Vernadsky stations, to support parts of the interpretation.

Responses to comments from Reviewer #1

In this manuscript, Leibrock et al. analyze time series of glacier area and velocity along the Antarctic Peninsula. The manuscript is well written and easy to read. The authors' rationale for each step of the analysis is explained clearly and the authors have nicely placed their results in the context of previous work via a thorough literature review. Despite its virtues, I do see opportunities for significant improvements to the manuscript.

We thank the reviewer for the very detailed and constructive review of our manuscript. We highly appreciate the time and effort invested in the assessment and welcome the suggestions for improvements. In response to the comments, we substantially revised several parts of the manuscript, including the seasonality and correlation analyses, the discussion, and conclusion, in order to better highlight the main findings and clarify the novel contributions of this study.

After reading the paper, I am not sure what the main finding is. I know what the paper is *about*, and I think I understand the methods that were used in the analysis, but I could not confidently recall the main takeaway if someone asked me about the manuscript tonight at a cocktail party. To bring the main findings into clear focus, I recommend reducing the length of the abstract by about 50% and focusing on one main finding. Possibly include a secondary point or two if they're profound or give helpful context, but keep the messaging tight. The journal Nature provides a formula for writing an abstract that works remarkably well, and a similar approach might benefit the present manuscript (https://www.nature.com/documents/nature-summary-paragraph.pdf).

Thank you for this helpful comment. We agree that the abstract should more clearly communicate the main takeaway of the study. Following your suggestion, we refocused the abstract around the central finding of the paper, while briefly mentioning supporting results for context, following the guidelines of Nature. The new version is not significantly shorter; however, we believe that it now highlights the main takeaways, while still keeping the message tight.

The Introduction section motivates paper, saying observation-based studies like this one are needed to understand ongoing changes in the AP, but the Discussion says that understanding any correlations or lack of correlations between velocity and glacier area is beyond the scope of this study, and the Conclusion states that future work is needed to understand the observations reported here. The glacier area and velocity data are both from other studies, and no new understanding have been derived from them, so I am wondering what exactly is new here.

We agree that the manuscript did not clearly enough communicate what the main new contribution of the study is. We have substantially revised the discussion and conclusion sections to better highlight the novelty of the work. In particular, in Sections 5.1 and 5.2 we added a specific focus on the discussion of Larsen A glaciers in comparison to Larsen B, which have not been studied in recent years. Additionally, we completely reworked Sections 5.3 (“Seasonal changes in ice speed and terminus area”) and Section 5.4 (“Correlation between terminus area changes and ice velocity dynamics”) and there clearly discuss the observed seasonal patterns, potential environmental drivers, and disentangle the calculated correlations and their corresponding lags. In terms of data, we use the same velocity data as Davison et al. (2025), but extent it to glaciers on the eastern side of the peninsula. Their paper focused on an ice discharge dataset, for which these velocity measurements served as an input, whereas our study directly examines ice speed, its seasonal variability and long-term evolution. Thus, while the underlying methodology is not entirely new and some of the measurements overlap, the main findings presented here are largely new. The terminus area dataset was introduced in a previous study (Loebel et al. 2025) that concentrated on dataset development rather than on using it to investigate glacier behavior across multiple timescales, which is the focus of the present work. To our knowledge, this is the first published dataset of sub-seasonal terminus area measurements for the region. This made it possible to carry out the first analysis of seasonal terminus variability for glaciers on both sides of the AP. In addition, this study presents the first correlation analysis between terminus area changes and ice velocity for a larger sample of glaciers in this region. Although this analysis does not allow

firm conclusions about the underlying mechanisms, we believe that it still provides a valuable first assessment of the potential seasonal coupling between terminus area changes and ice velocity variability.

Overall, I find it difficult to fully understand or believe in the conclusions, because I haven't been able to see where the conclusions come from. Readers are told about the data, we're told about the analysis, and we're told what the authors' conclusions are, but we're not *shown* much along the way. As a result, we're in the position of trusting the authors' interpretation of the data, rather than being able to see the data and come to the same conclusions on our own. To give readers an intuition for where the conclusions come from, consider adding a plot that shows the raw data, the processed data, and the natural grouping of the different glaciers. Consider showing all 42 time series of velocity and area, with the raw data (i.e., the image-pair level velocity values) in a light color, overlaid with the filtered monthly average time series in a heavier, darker color. And identify, perhaps with line color, the Category that each glacier is determined to be. Adapt these suggestions as necessary, but the main idea is to bring readers along and show us the major steps from raw data to final conclusions.

You are completely right that the original manuscript did not show the path from the underlying data to the final conclusion clearly enough. To make the results and interpretations easier to follow, we added plots of all measurement locations to the Appendix (Fig. A1), as 53 plots of an appropriate size would have needed too much space in the actual manuscript. These plots show the velocity tracking measurements from the individual image pairs with the velocity tracking error, the Kalman smoothed speed estimates, as well as the monthly aggregated time series. In addition, we added the original and monthly aggregated terminus area time series and calculated linear and break-point trends, which were relevant for the categorization of the glaciers. We also indicate the category of each glacier in these plots to make the grouping more transparent. In addition, we revised the main text to point readers to these figures. We hope this makes it easier to follow the main steps of the analysis and understand where the interpretations come from.

I find the seasonality strength metrics to be somewhat suspect. It's hard to be certain, because we are not shown most of the data, but in the four Area time series that are shown in Figure 3, I see no indication of seasonal variability whatsoever, yet if I am reading Figure 4 correctly, it indicates all of these glaciers have "moderate" seasonal strength. Is a residual from the once-in-a-decade calving events of 2022 being interpreted as a seasonal signal?

Thank you for rising this important concern. We agree that a seasonality metric is difficult to evaluate without access to the underlying time series, and we therefore added plots of all velocity and terminus area time series to the appendix (Fig. A1) to allow direct visual inspection. The STL decomposition method is commonly used for environmental time series analysis because it can handle complex seasonal and trend components. We chose this approach because the terminus area time series are incomplete and contain gaps during the austral winter months due to polar night, meaning that methods relying on complete 12-month records (e.g. the autocorrelation analyses conducted by Wallis et al. (2023b)) cannot be applied consistently across both variables. The seasonality strength metric is subsequently used to quantify how much of the variability of the detrended time series ($S_t + R_t$) is explained by the seasonal component (S_t) rather than the remainder/noise (R_t).

Regarding the four terminus area examples shown in Figure 3, we agree that seasonal fluctuations are not visually apparent for most of these glaciers. For HGE, Jorum, and Crane Glaciers, the strong retreat in the beginning of 2022 dominates the plotted range, which compresses the smaller-amplitude intra-annual variability and makes it difficult to identify by eye at this scale. At the same time, such an abrupt shift in the time series is difficult for a smooth trend component to capture perfectly, particularly around break point. As a result, part of this signal likely remains in the remainder component, increasing $\mathrm{Var}(R_t)$. Since seasonality strength is defined as $F_S = 1 - \mathrm{Var}(R_t) / \mathrm{Var}(S_t + R_t)$, this would reduce F_S. Therefore, the most likely effect is that these once-in-a-decade calving events lead to an underestimation of the seasonality strength.

I don't think the simple correlation analysis is appropriate for this work, because it conflates many different potential seasonal signals with response to rare calving events. Seasonal velocity variability may be driven by seasonal calving, or by seasonal basal melt near the grounding line, or by surface melt that impacts basal hydrology, particularly at these northern latitudes. Longer term changes in velocity might be driven by major calving events or by gradual changes in driving stress due to surface elevation or SMB. Simply correlating two time series--one that accumulates over time and another that represents an instantaneous response to forcing--without consideration

of different timescales or driving mechanisms, is unlikely to reveal any meaningful patterns. The manuscript points to Ultee et al., 2022 as justification for doing a similar analysis here, but an important difference is that Ultee et al. made a concerted effort to separate and understand the processes at play, while the present manuscript ends the analysis when no clear pattern is discovered in a simple correlation analysis.

Thank you for this comment. We understand your main point that a simple lagged correlation analysis has clear limitations and cannot fully separate the different processes and timescales that may link glacier velocity and terminus change. In response, and considering the suggestion of Reviewer #2, we substantially revised this part of the analysis to now use detrended time series for the correlation analysis to focus on a potential seasonal coupling between the two variables (see Section 3.3, 4.4, 5.3). This adapted version of the correlation analysis still cannot distinguish between seasonal forcing, responses to calving events, or shared dynamics driven by external forcing on its own. Therefore, we now describe the results more transparently including the sign of correlation as well as the observed lags and expand the discussion to critically evaluate our results, connect the findings of seasonality and correlation analysis, and highlight where causality is likely missing. Overall, we have revised the manuscript to better frame that the correlation analysis as a first-order assessment of temporal association, while acknowledging that are more process-specific treatment would be needed to fully disentangle the drivers of the observed variability.

The conclusions are framed around the authors' own classification scheme that categorizes glaciers as either accelerating, decelerating, advancing, retreating, fluctuating, or "change point". I understand the need to make sense of the world by clustering similar behaviors together, but these categories are somewhat squishy, they rely on arbitrary thresholds of statistical significance rather than incontrovertible properties of the glaciers, and without seeing the data, it's hard to know how distinctly clustered these categories really are, or how easily a glacier's identity might switch between categories with one calving event. If these categories truly represent distinct characteristics that might indicate common driving or governing mechanisms within each category, then I'd like to see evidence for that and a discussion of the physical processes that might be at play. If not, I'm not sure what value the categories provide.

We appreciate the reviewer's comment and agree that these categories should not be interpreted as fixed or incontrovertible glacier characteristics, as they can change in response to future events such as major calving or speed-up. However, this was not their intended purpose. Rather than representing permanent physical properties or common governing mechanisms, the categories were introduced do describe the long-term dynamics of the time series during the study period and to group similar patterns in ice velocity and terminus area change across the glacier sample. We also note that the classification is not based on arbitrary thresholds chosen specifically for this study, but instead uses a commonly applied t-test for a linear trend, which evaluates whether the slope of a fitted linear regression differs significantly from zero, using a significance threshold of $p < .05$. We suspect that part of the concern arises from how the purpose of these categories was understood. Their intention is to provide a practical framework for clustering observed time series and therefore glacier behavior during our observation period, which makes it easier to summarize the evolution of the two variables across many glaciers without having to describe each glacier individually. To clarify this point and to make the distinction between categories more transparent, we added the categories to the newly created plots of ice velocity and terminus area time series for all measurement locations and glaciers (Fig. A1) and added a note to Sections 3.1 and 3.2 referring the readers to these plots. This allows the readers to directly assess the behavior represented by each category.

Reviewer #1		
ID	Reviewer Comment	Response
1	L20: I think "inter-annual variability" should be "intra-annual variability"?	The abstract has been completely revised and this has been removed.
2	L23-26: The last two or three sentences of the abstract essentially say that nature is complicated and future work is needed to understand it. This sentiment makes the abstract fall flat and implies the present study does not offer any satisfying conclusions. Reword the end to come to a stronger conclusion about what this study DID find rather	We agree, the abstract was not stating clearly enough the main findings of the study. As suggested in your general feedback we completely rewrote the abstract, focusing on the main finding and one to two supporting results following the Nature guidelines to now show what this study did and found rather than what needs to be done in the future.

	than what it didn't find, and delete the vague statements about the need for future work.	
3	L49: Check those units. Davison et al. presented units of Mt yr^-2, not Mt yr^-1 in their abstract. In my opinion, presenting units of acceleration in the Davison paper wasn't a great choice on their part because interpreting changes in acceleration rates isn't very intuitive, and it's easy to mistake for simple flow rates (as has happened here!). I recommend rewording in a way that doesn't repeat Davison's units of acceleration.	Thank you for pointing this out. We misread the units in the Davison paper, mistaking their rate of change of acceleration (Mt yr^{-2}) for a discharge rate (Mt yr^{-1}). Following your suggestions, we changed the sentence in question to: "More recently, grounding line ice discharge from western glaciers increased by 7.4 % (7 Gt yr^{-1}) between 2017 and 2023, marked by a widespread and quasi-synchronous acceleration since 2021, likely in response to ocean warming (Davison et al. 2024)."
4	L56: The acronym EO is defined here, but never used.	The acronym EO has been removed.
5	L87-90: I think this entire paragraph can be deleted because the sentence that begins "This study focuses on 42 marine-terminating glaciers" is very similar to the sentence on Line 64 reads "In this study, we...investigate...42 outlet glaciers" and the "Of these glaciers..." sentence doesn't lead to anywhere.	We agree, this paragraph was largely redundant with the earlier description of the study scope. We have therefore deleted the paragraph and moved the study area map to appear before Section 2 ("Study area"), where the region and the glaciers are first introduced.
6	Figure 1: * The caption says units are km yr^-1, but the colorbar says m yr. * It is common to show ice velocity in a log color scale. I'm not suggesting it here, but only noting a potential for ambiguity that could be mitigated by including more tick labels on the colorbar to show linear increments.	Thank you very much for pointing this out. We have corrected the units in both the caption and the color bar – the correct unit is m yr^{-1}. We agree that adding more tick labels could reduce potential ambiguity in the interpretation of the color bar. However, implementing this change would mean reworking the whole map to then adjust the color scale layout. Since the map is mainly included to provide general spatial context and does not show main results of the study, we think that the current version is sufficient for this purpose. Therefore, we have retained the existing color bar.
7	Fig 1 caption, L127, and L143: Reword to remove references to Slater (In Prep). If any details in the unreleased manuscript are necessary to understand the present study, describe those details here.	We agree that referring to Slater (In Prep.) was not informative for the reader. We have removed this reference as no essential methodological details from the unreleased manuscript are required to understand the present study.
8	Table 1: It would be helpful to actually see all 42 time series, either colored or somehow clustered by Category. Because the table provides a suitable reference, but doesn't provide intuition for what shapes of time series to imagine when we think of each category.	Thank you for pointing this out, we agree that it is useful to show all time series to get a feeling of the different categories and to bring the readers along on the main processing steps (see general comments). Including all 53 time series (one for every velocity measurement location) in the main manuscript would take too much space, which is why we added plots for every glacier to the appendix (Fig. A1) and included the category of each glacier there. We also added the description "The categories refer to terminus area and ice velocity separately and do not describe their combined behaviour. Therefore, each variable is classified into four categories." to the table to make it clearer that these categories refer separately to terminus change and velocity, and not to their combined behavior.
9	L153-163: The description of the processing would be easier to understand if we could see what is being described here. Point readers to Figure 3 here (and reorder the figures as needed) so we can see the 3x3 km boxes, etc.	We agree that without being able to see how the data was processed, the description can be a bit abstract. As described earlier, we have now added plots of all glacier time series and their main processing stages to the appendix and added a note in the text that

		points readers to this figure (Fig. A1) and Figure 3, where the placements of the 3 x3 km boxes is shown.
10	L171: "Seasonal" and "Trend" don't need to be capitalized.	This has been corrected.
11	L183-197: Show, don't tell. At a minimum, point readers to the time series shown in Figure 3, but to understand where the conclusions come from, it would also help to see the normalized cross correlation analysis.	We thank the reviewer for this suggestion. To make the basis of our interpretation more transparent, we now refer readers to Figure 3 of exemplary time series as well as to the full set of plotted time series in Figure A1. We are not entirely sure what was meant by "see the normalized cross correlation analysis". However, we added to every plot in Figure A1 an inset with information about the correlation, lag, and statistical significance of the correlation. This should make it easier for readers to directly follow how the conclusions are derived from the underlying time series and cross-correlation results.
12	L206: "initially advanced" is not very descriptive. Upon first read, I thought it meant a rapid acceleration that served as a precursor to significant retreat, but after looking at Figure 3, I think it means a steady advance rate over a decade?	Thank you very much for drawing attention to the misleading description of the terminus advance process. We changed this part to now read: "steadily advanced during the first years of the study period", which should more clearly describe the observed processes.
13	L256 and Figure 4c: Why doesn't the correlation coefficient include the sign of the correlation? The strength of the correlation is meaningless without knowing the sign.	We agree that the sign of the correlation (together with the corresponding lag), is highly relevant for interpreting the results. As part of the revised correlation analysis (see our response to the general feedback), we have reworked this section completely and now include a dedicated figure presenting the correlation results in two maps: one showing the positive and negative correlations (Fig. 4a) and the other showing the corresponding lags (Fig. 4b). In addition, both the sign of the correlations and the lag information are explicitly described in the text of Section 4.3.
14	L289: Typo "norther" should be "northern".	This has been corrected.
15	L379: This may be a term I am not familiar with, but I'm not sure what is meant by a "consolidated" ice front.	We thank the reviewer for pointing this out. We agree that the term "consolidated" may be unclear in this context. We have therefore replaced it with "stabilized" to clarify the intended meaning. Specifically, we refer to the role of landfast sea ice in maintaining the structural coherence of the ice-front region (including mélange and fractured ice) and suppressing calving activity during its persistence.
16	L330: There is no need to define the acronym SIE, as it's only used in this sentence.	The acronym has been removed.
17	L340: The new findings don't necessarily "contrast with" Seehaus et al. Perhaps "update" or a similar sentiment would be more precise?	This is a good point as the study period of Seehaus et al. (2018) ends where ours begins. We have revised the sentence accordingly, and now state that our findings "update" those of Seehaus et al..
18	L372: Same typo "norther" should be "northern".	The discussion of the seasonal dynamics has been substantially revised, and therefore this typo has been removed in this process.
19	L379: I'm not sure what it means to "consolidate" a glacier terminus.	Similar to comment 15, we have replaced "consolidate" with "stabilized" to prevent confusion.
20	L642: The Wallis citation is incomplete. Needs a doi, journal name, or other identifiers.	Thank you for pointing this out. We have corrected the citation to now include a doi.

Response to specific comments from Reviewer #2

We thank the reviewer for taking the time to read and comment the manuscript and for providing valuable feedback. Their review is divided into two parts: a written section with general comments, to which we respond directly in blue, and a series of specific remarks, which we address in tabulated form.

The authors carried out an analysis of spatio-temporal variability of glacier area and velocity changes along the AP (north of 70°S). They relied on already published datasets (and maybe an owe velocity data set, clarification is needed, see comments below) and carried out various analysis on seasonality and correlation.

Various studies have already analyzed these aspects for the AP on different spatial or temporal scales.

In general, the analysis is well written, and the methods and results are clearly described. However, a more detailed discussion is desirable (see comments). Moreover, often the manuscript reads like a summary of already existing data, and the focus on what is new is missing, in particular in the Abstract and Conclusions.

Thank you for this helpful comment. Following the suggestions from you and Reviewer #1, we substantially revised the manuscript to strengthen both the analysis and the discussion, and to make the novel contribution of the study clearer. In particular, we extended the seasonality analysis to include the annual timing of peak velocity and terminus change, and we completely reworked the correlation analysis to use detrended time series of both variables in order to focus more specifically on their seasonal coupling. We also expanded the corresponding discussion sections to better highlight the new findings of this study, place them in the context of previous work, and discuss possible environmental factors that may contribute to the observed patterns. In addition, we rewrote the abstract and conclusion to communicate the main takeaway of the study more clearly, while briefly mentioning supporting results for context, following the guidelines of Nature.

Additionally, a correlation (either quantitatively or qualitatively) to potential forcing factors like ocean temperature or atmospheric variables (e.g. temp., run-off,….) would strengthen the analysis and probably support the proposed chains of effects.

We agree that including a correlation to potential forcing mechanisms would strengthen the analysis significantly. However, adding this type of analysis would require a substantial extension of the study and is beyond the scope of the present manuscript. Instead, because we completely revised the discussion of the observed seasonal patterns and correlations between the two variables, we now include a more comprehensive discussion of the environmental drivers that may contribute to the observed glacier dynamics and of the possible process chains linking these forcings to the glacier response. We also added a plot showing the evolution of the fast ice extent in Larsen A during our study period (Fig. 6) to support the discussion of glacier dynamics and potential environmental drivers in this region. In addition, we looked at station data for temperature and precipitation measurements on the western side (Vernadsky station) and eastern side (Marambio station), and included the plotted data in the appendix (Fig. A3).

Reviewer #2		
ID	Reviewer Comment	Response
1	Abstract: What are your key findings? Any quantitative measures? e.g., how strong are fluctuations….. (per glacier, region vs. region?…..). The abstract does not provide any new information. To me, the main message is that the glaciers' dynamics are heterogeneous, which was already reported by other studies.	Thank you for this helpful comment. We agree that the abstract should more clearly communicate the main takeaway of the study. Following your suggestion, we refocused the abstract around the central and new findings of the paper, while briefly mentioning supporting results for context.
2	Section 3.2. Please clearly state if you took velocity data from the study by Davison et al. 2024. If so, just link to this study and do not explain the processing again in full detail. If not, Why don't you take the data from them to avoid doubling the work? Moreover, the authors are similar in both studies!	We use the same velocity data as Davison et al. (2025); however, that paper focusses on describing an ice discharge dataset which uses the same ice velocity measurements as an input. The description of this specific ice velocity method is not a focus of this paper and is found in an appendix but provides technical detail regarding thresholding and filtering of our ice velocity observations. The manuscript

		has been updated to direct readers to the appropriate appendix, however, as our paper focusses on ice speed measurements, we feel it is appropriate to document the key steps in the processing chain such that the reader has clear access to this information. With respect to the common coauthorship, A. E. Hogg is the PI of the group and is the only common coauthor on both studies. We follow standard co authorship principles that credit all authors involved in each publication.
3	L136: Table 1 contains more than 5 combinations? Which ones did you use?	Thank you for highlighting this discrepancy between the manuscript and Table 2. It is a formatting mistake as the first three rows of the table were added erroneously. These rows correspond to the three window and step size combinations used for processing ice velocity across the rest of the Antarctic Ice Sheet, outside of the Peninsula. This has now been corrected in the manuscript by removing the first three rows to leave the correct 5 window and step size combinations.
4	L143: Why did you order the S1 data using the mid-date of image pairs? Not meaningful.	We use the mid-date as a representative time for the ice speed measurements as we observe only the displacement between two dates and cannot resolve speed variability on time periods shorter than the 6/12-day image separation. This is in line with the convention of other published ice velocity datasets such as NASA's ITS_LIVE and GEUS's PROMICE.
5	L145: On which scales did you apply the outlier filtering? For each pixel, individual? Some sampling regions/points?	Temporal outlier filtering was performed pixelwise on our temporally-chunked data. The manuscript has been amended to reflect this.
6	L150: What is sigma_window?	sigma_window is the outlier-robust standard deviation for the 90-day window of the Hampel filter. Like sigma_ts it is calculated by scaling the median absolute deviation of the window by k=1.4826. A definition for sigma_window has been added to the manuscript, and the paragraph has been reworded to improve readability.
7	L153ff: Did you extract the velocity time series on a daily or monthly base? Unclear.	Thank you for pointing this out. We have clarified in the text that we extracted the monthly median velocity time series for each measurement location.
8	L174: Check formula. Seems to be incomplete?!?!	You are completely right, there was a missing closing parenthesis and the period between 0 and 1 should have been a semicolon. We have corrected the formula accordingly.
9	L178: Any reference for this threshold? My first impression would be that it seems to be a bit low.	The threshold was not intended to represent a level of statistical significance, but rather to provide the reader with transparency on how glaciers were selected for the subsequent, more detailed analysis. That said, we agree that the originally applied threshold was likely too low, and that including glaciers with such weak seasonality is not particularly informative for an in-depth assessment of intra-annual dynamics. We therefore revisited this part of the analysis and now focus on eight highlight glaciers per variable that exhibit strong to very strong seasonal fluctuations ($F_S \geq 0.6$ for

		terminus area and ≥ 0.8 for ice velocity). The manuscript has been updated accordingly.
10	L179: Unclear sentence. What is a seasonal median?	This part in the method section has been revised and this sentence has been removed since we replaced this part of the analysis with an assessment of annual absolute variability (month of minimum and maximum) which is more analytically valuable, especially for comparison with other studies.
11	L201: Fig 2b.	We have corrected this.
12	L203: Since you studied different amounts/areas of glaciers on both sides, it would be more meaningful to provide relative change information. E.g. relative to the total glacier area	Thank you for this valuable suggestion. However, because our analysis focuses only on terminus area change, we do not have data of the total glacier area. We believe that obtaining these data solely to express terminus change relative to total glacier area would add little value in the context of this study. Instead, we already report the relative contribution of glaciers on each side to the total observed area change. The purpose of these values is to show that, although fewer glaciers were analyzed on the eastern side, they still accounted for most of the total retreat. We believe this is already clear from the current presentation of the results and therefore do not consider further changes necessary here.
13	Fig2. Splitting the relative change (%) and classification in individual panels would make the figure clearer (4 panels in total)	Thank you for this valuable suggestion. We agree that the current configuration is quite information-dense, and that splitting the figure into four separate panels could reduce the visual load for the reader. However, we tested this layout and found that it made it much harder to directly relate each glacier's category information, and thus its long-term behavior, to the corresponding relative changes. In our view, this separation leads to a loss of context that outweighs the gain in visual simplicity. We therefore chose to retain the current figure design as a compromise.
14	L256: Why does it not distinguish between +/- correlation for all glaciers and just for "most"?	We agree that the sign of the correlation (together with the corresponding lag), is highly relevant for interpreting the results and should be included for all glaciers. As part of the revised correlation analysis (see our response to comment 33), we have reworked this section completely and now include a dedicated figure presenting the correlation results in two maps: one showing the positive and negative correlations (Fig. 4a) and the other showing the corresponding lags (Fig. 4b). In addition, both the sign of the correlations and the lag information are now explicitly described in the text of Section 4.3.
15	L263: How did you do the correlation analysis for glacier complexes like HGE? I guess you got only one glacier area time series but 3 velocity time series. Which ones did you analyse? All? If so, which one did you select for e.g. Fig. 4. Please clarify here and in the methods section.	The correlation analysis was carried out separately for every velocity measurement location, and the full results for all locations can be found in the Supplements. Figure 4 has been revised (see comment 14) and now includes inset maps for glaciers with more than one measurement location. We added the following clarification to the figure caption rather than the methods section, as this makes it easier for the reader to understand the figure directly without having to move back and forth through the manuscript. This is the text we added: "For glaciers with more than one velocity measurement location, the figure is presented as

		follows: if only one location showed a significant correlation, that value is mapped; if multiple locations showed significant correlations, inset maps are used to display the individual measurement locations and their respective values; and if none of the locations showed a significant correlation, the glacier is colored white (0) and marked with hatched outlines to indicate non-significance."
16	Fig. 4: It shows the most relevant outcome of your analysis. However, the figure panels are rather small and individual catchments are difficult to recognize. I would recommend changing the color ranges and boundaries of the categories. And maybe increase the figure size (see also comment further down, to include also other panels) to 2-column panels.	Thank you for this suggestion. We have revised the original Figure 4 by splitting it up into two separate figures: Figure 4 now shows the results of the correlation analysis (see comment 14 and 15), and Figure 5 shows the results of the seasonality analysis. By doing this, the figures now consist of two-column panels and enlarged maps to improve readability. We also adjusted the color ranges and replaced the discrete categories with a continuous color scale.
17	L277: Include the thresholds of F_s also in Table 1. Did you use similar thresholds for area changes or just by visual interpretation?	We removed the part of the analysis in which this threshold was used (see response to comment 9). In terms of Table 1, we assume you are referring to the category "Fluctuating". This category does not imply that a glacier exhibits seasonal variability. Instead, it refers to glaciers that do not show a statistically significant long-term linear trend and are primarily characterized by short-term fluctuations. Whether these fluctuations follow a seasonal pattern is assed separately in the seasonality analysis and by computing the seasonality strength F_S.
18	L285: A figure which shows the spatial distribution of the seasonality (timing of peak velocities and area changes) would be helpful and interesting, like Boxall et al. 2022 but just for each glacier. (maybe combined with Fig. 4)	Thank you for this valuable suggestion. We have now added two plots showing the annual timing of maximum and minimum ice velocity and terminus area change for all glaciers as well as for eight highlight glaciers. These plots are integrated with the two maps of the spatial distribution of seasonality strength in the revised Figure 5. A figure directly analogous to Boxall et al. (2022), in which the timing of seasonal extremes is shown for every individual time series, was not feasible in our case because of the large number of series considered, including two variables across 42 glaciers and 53 ice velocity measurement locations. However, we believe that the newly added plots provide a clear and compact overview of the temporal structure of seasonal dynamics across the AP. In addition, we plotted the monthly extremes for eastern and western glaciers separately to assess regional differences. These plots can be found in the appendix (Fig. A2).
19	L290: Why don't you report also the area change "peak" month?	This is a good point. The month of maximum terminus area extent has now been explicitly reported. In addition, we specified the exact months of minimum and maximum ice velocity and terminus area, and included quantitative estimates of the magnitude of intra-annual speed-up and terminus fluctuations to provide a more complete characterization of the seasonal dynamics.
20	L301: Report also relative area changes (see comment above)	Thank you for pointing this out. Our area measurements are given as relative changes,

		referenced to the fist measurement of each glacier, because the underlying dataset only provides relative values. As a result, we cannot directly calculate quantities such as percentage change, which limits a more direct comparison with the results reported by Seehaus et al. (2018).
21	L317: At which water depth?	Good question. We added information about the water depth at which the warming was observed most strongly: “upper-ocean temperatures (approximately -1.8°C above 500 m)”.
22	L318: Source of the bed slope or lit. ref. of this fact is missing	Thank you for pointing this out. We added a literature reference for this fact.
23	L326: “cooling influence” seems not to be meaningful and sounds awkward. Check your wording and description of the process.	We have removed this part of the sentence and it now reads: “In this sector of the AP glacier dynamics appear to be governed by the warmer waters of the Bellingshausen Sea, which has undergone significant warming since the early 2000s.”. This wording describes the process more clearly and avoids confusion.
24	L332: delete “CDW”, it is included in “ocean temp. regime”	This has been corrected.
25	L342: Provide an explanation on why your observations are different.	Thank you for pointing out this previously uninterpreted result. After revisiting the comparison between the two studies, we found that the main difference for western glaciers lies in the magnitude of acceleration, which is likely explained by differences in glacier selection, measurement location, and the longer observation period used by Seehaus et al. (2018). We have now added this explanation to the manuscript. In addition, we expanded the comparison for eastern glaciers north of Larsen B, where Seehaus et al. (2018) has already noted deceleration and this pattern continues in our results. We now also provide a brief discussion of potential drivers for this behavior.
26	L345: Comparison with other studies in the South-western section is missing? How did the trends change/not change? Did you observe some differences, something new?	You are right, a discussion for this region of the AP was missing. We have now added a paragraph, comparing our results with those of Friedl et al. (2018) and Walker et al. (2017) for Fleming and Prospect Glaciers, showing that our findings are broadly consistent and discussing the observed differences between glaciers in Marguerite Bay and potential drivers of the observed dynamics, as well as possible implications for future glacier change.
27	L351: Is this just a hypothesis or did they really prove that CDW entered the trough? Please clarify.	Thank you for raising this point. Wallis et al. (2023a) did not provide direct measurements of Brescochea Bay, but inferred from their analysis that warm CDW likely entered the trough. We have clarified this in the text. The sentence now reads: “At Cadman, Wallis et al. (2023a) concluded that a 400 m deep channel enabled the intrusion of warm CDW, leading to persistent thinning and ungrounding from a bedrock ridge, which likely triggered this rapid acceleration.”.
28	L356: “first eight years” of what? Of your study period? Please be more precise also at other locations in your manuscript.	Thank you for pointing out this lack of clarity. We have revised the sentence to make clear that we are referring to the first eight years of the study period. We also reviewed the rest of the manuscript and

		clarified the wording in other places where the description was too vague.
29	L355ff: Did you discover something new? Something which was not already reported by the numerous studies on the Larsen-B sped-up? Otherwise, it is just a summary of existing reports. Did you discover something in Larsen-A? Was this also affected?	We agree that this part of the discussion can be improved by focusing what new findings this study added to the existing ones for Larsen B, which we now provide. In addition, we added an entire section about our findings for Larsen A glaciers and what their dynamics indicate in the context of fast ice modulated dynamics.
30	L370: Highlight here what is new? Did Wallis cover the East Coast? As far as I know, not! So point out the differences	We substantially revised the entire discussion of seasonal changes. At the beginning of the section, we now clearly highlight where this study provides new insights beyond previous work.
31	L405: I guess this is in contrast to Seehaus et al. 2015, who discovered a correlation between retreat and speed-ups at Dinsmore-Bombardier-Edgeworth Glacier.	Thank you for these helpful comments. We agree that the original approach and the presentation of results were not sufficiently clear. In response, we substantially revised the correlation analysis as you suggested. Instead of using first-order differenced time series, we now correlate detrended time series that retain the seasonal signal, which allows us to assess potential seasonal coupling between terminus area changes and ice velocity. As a consequence, we rewrote large parts of the methods (Section 3.4), the results (Section 4.3), and discussion (Section 5.4) to reflect this updated approach and to better explain both the analysis and its limitations. Regarding the findings of Seehaus et al. (2015), our revised analysis does show relatively high negative correlations for DBE Glacier, with lags indicating that terminus changes led ice velocity changes. Therefore, the two studies do not contrast as they also observed velocity responses to terminus retreat.
32	L417: Maybe it advanced even for a longer period! Please be more precise regarding timing information.	
33	L420: I am not sure if first order differencing is suitable for the subsequent analysis of correlation, since you are actually trying to figure out if the terminus changes and velocity changes (both got seasonality) are correlated. So, it would be interesting if these seasonal signals are correlated. So maybe use the de-trended time-series instead of the first-order differences.	
34	L440: Not just the seasonality, but also ice discharge and ice mass loss.	Thank you for pointing this out. We agree that the environmental drivers highlighted in that paragraph are amplifying ice discharge and ice mass loss as well. We have therefore revised this part of the conclusion to explicitly include these processes.
35	Conclusion: Similar to the abstract, the authors should more clearly pronounce which new findings were revealed by their analysis and not just repeat what others already figured out. There is some potential in their data, but it needs to be unveiled.	We agree that the original conclusion did not sufficiently highlight the new insights provided by our analysis and instead placed too much emphasis on findings that are already relatively well established. In response, we revised the conclusion to more clearly emphasize the key results that emerge from our analysis, in particular the new insights into seasonal patterns, their potential coupling, and the environmental drivers that may underlie them.

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