

RESPONSE TO ANONYMOUS REFEREE #1

We thank the referee for the careful assessment of our manuscript and for recognizing the investigation of the relationship between iceberg B09B and marine cold-spells (MCSs) as a worthwhile line of scientific inquiry. We appreciate the concerns raised regarding the treatment of ice-associated summer sea surface temperatures (SST), seasonal variability, the interpretation of the B-SOSE diagnostics, and the presentation of the manuscript. Below, we describe in more detail how we propose to address each comment in a revised manuscript.

Major Comment 1

In Section 2.2.3, the authors apply an additional ice flag to identify cold-spell days occurring under near-ice conditions (MCS_{ice}), and summarize both the total number of cold-spell days and the number of MCS_{ice} days. However, the subsequent results and analysis only focus on MCS_{ice} . The results section only presents the MCS_{ice} cumulative intensity, without showing the standalone temperature anomalies (i.e., the intensity not multiplied by the number of days). Based on the definition of MCS_{ice} , the temperature should not be much higher than the freezing point, as it is calculated under sea-ice presence. The SST data used in this study are derived from OSTIA. Under sea-ice cover, SST is not directly retrieved via remote sensing but is instead estimated based on sea-ice concentration. Therefore, the identified MCS_{ice} events are not based on in situ or directly observed SST, which raises concerns about the reliability of the data.

We thank the referee for raising this important concern. We agree that the submitted manuscript places too much emphasis on MCS_{ice} and that the limitations of interpreting OSTIA SST in ice-associated conditions require clearer consideration.

To investigate this issue, we have conducted preliminary analyses separating MCS_{ice} from MCS_{nonice} . Importantly, the exploratory MCS_{nonice} analysis shows a pronounced post-2011 increase and a spatially coherent concentration around the Adélie Sill–Commonwealth Bay sector. We therefore propose to restructure the analysis so that MCS_{nonice} becomes the principal diagnostic. This would substantially reduce the dependence of our central result on OSTIA SST under near-ice conditions while still allowing the prevalence of ice-associated cold-spell conditions in the region to be documented.

We also agree that cumulative intensity alone does not distinguish the magnitude of individual temperature anomalies from their persistence. In a revision, we would therefore complement cumulative intensity with standalone intensity metrics and event occurrence/persistence statistics, allowing these components of the MCS response to be evaluated separately.

As an additional preliminary quality-control test, we excluded climatological threshold values below -1.8°C and repeated the classification. The overall proportion of MCS_{ice} event days changed only marginally, from approximately 97.42% to 97.39%. This suggests that the dominance of MCS_{ice} in the overall event-day population is not driven

by threshold values below the approximate seawater freezing range. Nevertheless, given the limitations identified by the referee, we agree that MCS_{ice} should not carry the central interpretation in a revised manuscript.

Major Comment 2

In Section 3.3, the authors divide the year into only two periods: "summer" and "winter" (Fig. 3a), rather than the conventional four seasons. This overly simplistic classification may lead to misinterpretation. For example, adjacent months such as March and April, or September and October, would be assigned to different categories. I recommend presenting the conventional month-by-month variations, or at least providing monthly statistics based on the full dataset to capture the true seasonal characteristics. Lines 335–336 state that "very large MCS_{ice} anomalies appear repeatedly and are almost entirely concentrated in the later period," indicating that temporal variations exist even within the defined "summer." Therefore, this issue warrants further investigation.

We agree that the two-period summer/winter classification in the submitted manuscript does not fully resolve the seasonal evolution of the cold-spell signal.

In response to this comment, we have conducted a preliminary month-by-month analysis over the complete 1982–2024 record. This exploratory analysis reveals a clear seasonal distinction between the two MCS categories: MCS_{nonice} is concentrated predominantly during the more open-water portion of the year, whereas MCS_{ice} is much more prominent during the ice season.

We therefore propose to present the monthly analysis as the primary description of seasonality. The broader summer/winter distinction would be retained only as a secondary physical summary where useful, rather than as the sole representation of seasonal variability. This approach would also avoid relying on a conventional four-season classification that may not optimally represent the Antarctic coastal environment while directly addressing the referee's concern regarding transitions between adjacent months.

Major Comment 3

The authors note in Section 3.3 that the variations in MCS_{ice} are more pronounced and prominent during summer (Fig. 3a). However, in Section 3.4, they only discuss the relationship between MCS_{ice} and winter sea-ice and freshwater flux variations, without discussing the summer conditions or the sea-ice state concurrent with the MCS_{ice} events. This represents a biased and preconceived analytical approach, which hinders a comprehensive understanding of the underlying mechanisms.

We appreciate this criticism and agree that the submitted manuscript places disproportionate emphasis on winter conditions. Our intention was to investigate winter sea-ice processes as potential preconditioning of the subsequent surface state, rather than to imply that winter processes alone explain the observed MCS variability. We recognize that this distinction was not sufficiently clear.

The preliminary monthly analysis described above provides a basis for broadening this interpretation. In particular, the concentration of MCS_{nonice} during predominantly open-

water months indicates that concurrent and warm-season conditions should receive greater attention alongside possible ice-season preconditioning.

We therefore intend to broaden the process analysis to consider conditions throughout the year and to distinguish more carefully between possible ice-season preconditioning and the subsequent expression of MCSs under more open-water conditions. Winter sea ice and freshwater-related processes would consequently be treated as potential contributing mechanisms rather than as the sole explanation for the post-2011 change.

Major Comment 4

The analysis in Section 3.5 is based on the B-SOSE model. However, since icebergs are not included in this model, its results cannot reflect iceberg impacts. Using this model to rule out the influence of mCDW is unconvincing. For instance, the statement in Lines 290–291 that "heat becomes less effectively expressed at the surface" lacks supporting evidence, as Figure 3g shows that the surface layer was colder prior to 2010, indicating that the surface has actually warmed.

We agree that B-SOSE cannot directly diagnose the impact of B09B because the iceberg itself is not explicitly represented in the model. We appreciate the referee highlighting that the wording of the submitted manuscript may imply a stronger attribution than B-SOSE can support.

In a revised manuscript, we would clarify the role of B-SOSE as providing context for the contemporaneous three-dimensional ocean state, including the presence and vertical distribution of modified Circumpolar Deep Water (mCDW), rather than as direct evidence of an iceberg-induced circulation response or as a means of excluding mCDW influence.

We would accordingly reconsider statements such as "heat becomes less effectively expressed at the surface" and restrict the interpretation to differences directly supported by the B-SOSE diagnostics. More generally, the revised interpretation would distinguish explicitly between environmental-state diagnostics and evidence that can be used to assess changes potentially associated with B09B.

Major Comment 5

The Discussion section (Section 4) does not introduce new evidence. Instead, it merely combines previously published viewpoints (some of which are not from the study region) to infer the plausibility of the findings. This is not standard practice for a scientific paper, and the credibility of this section is therefore questionable.

We thank the referee for this comment. We agree that the Discussion could distinguish more clearly between interpretations directly supported by our analyses and broader mechanistic interpretations informed by previous studies.

Additional $MCS_{\text{nonice}}/MCS_{\text{ice}}$ and monthly analyses described above would provide a stronger empirical basis for the Discussion. We would also explicitly distinguish observations demonstrated by the present analysis from mechanisms that remain hypotheses informed by previous work. Studies from other Antarctic shelf regions

would be identified as process analogues or contextual evidence rather than as direct evidence for conditions in Commonwealth Bay, and causal language would be moderated where the available observations do not uniquely distinguish among mechanisms.

We respectfully note that comparison of new findings with relevant previous literature is an established function of a Discussion section. We therefore do not propose to remove such contextualization, but we agree that the evidentiary distinction between our results and interpretations based on previous studies should be made considerably clearer.

Major Comment 6

The description of data processing in Section 2.2 is overly detailed, particularly in Section 2.2.4 and subsequent subsections. I recommend removing the descriptions of standard or widely known methods.

We agree that portions of Section 2.2 can be streamlined. We therefore intend to shorten descriptions of standard processing steps while retaining the information necessary to reproduce study-specific methodological decisions, including the MCS definition, baseline period, MCS_{nonice}/MCS_{ice} classification, quality-control criteria, and sensitivity analyses. More routine methodological details could be condensed or transferred to supplementary material where appropriate.

Minor comments

- For the **Abstract (The abstract should include some quantitative descriptions)**, we agree and would include quantitative descriptions of the principal results once the revised analyses are finalized.
- For **Line 3, an event-based detection framework: The meaning of this phrase is unclear**, we agree that “an event-based detection framework” is unnecessarily vague. We would replace it with a direct description of the percentile-based MCS detection approach applied to daily OSTIA SST.
- For **Line 8-9, domain-wide annual cumulative intensity shifting to substantially more negative values: Without knowing the exact definition of "annual cumulative intensity," it is impossible to understand what "more negative values" implies**, we agree that annual cumulative intensity requires an explicit definition. In a revision, we would define it at first use as the sum of daily SST anomalies relative to the seasonal climatology during detected cold-spell conditions, expressed in °C days. More negative values therefore represent a greater accumulated cold anomaly. We would also complement cumulative intensity with standalone intensity and occurrence/persistence metrics, as described in our response to Major Comment 1.
- For **Line 25: Please remove the hyphen in "ice-shelf"**, we agree and would change “ice-shelf” to “ice shelf.”
- For **Line 140: The full name of mCDW is not necessary here**, we agree and would remove the repeated expansion of mCDW.
- For **Line 205, a persistent inflow and recirculation pathway centered on the Adélie Sill: Neither inflow nor recirculation is evident in the figure**, we agree that the wording “persistent inflow and recirculation pathway” is stronger than

can be demonstrated by the figure alone. We would revise the text to describe only circulation features directly supported by the presented diagnostic.

- For **Line 207, eastward toward Mertz Depression: There is no eastward flow shown in the figure**, we agree and would remove or revise the characterization of the flow as “eastward toward Mertz Depression” unless this direction is explicitly demonstrated by the corresponding analysis.
- For **Line 216-219: This paragraph lacks supporting figures and is difficult to understand**, we agree that the paragraph requires clearer support. In a revision, we would either link each statement explicitly to the relevant diagnostic or remove interpretations that cannot be demonstrated by the presented results.
- For the **Caption of Fig. 2: Starting from the end of the second line, the text describes the phenomena revealed by the figure. This information has already been discussed in the main text and should not be included in the figure caption. Please add explanations for the lines and colors in the figure. For example, as mentioned in Figure 1: "The red dashed outline indicates the pre-2010 extent of the Mertz Glacier Tongue before calving." Additionally, the MGT in this figure is shown in two different shades; what do they represent?**, we agree. We would shorten the interpretive portion of the caption and ensure that all symbols, lines, colors, and representations of the Mertz Glacier Tongue are explicitly defined.
- For the **Fig. 2: Yellow triangles are used to represent Iceberg B09B. It is recommended to use the actual iceberg outlines as shown in Figure 1. How were the positions of these icebergs obtained?**, we agree that their provenance and representation need to be clearer. We are currently examining the iceberg-position information and its underlying source in greater detail, including the distinction between iceberg position, spatial footprint, movement, and evidence for grounding. If invited to revise, we would document the source and processing of these positions explicitly and reconsider how B09B is represented graphically. This issue also relates to the second referee’s comments concerning B09B’s trajectory and grounding status and would therefore be addressed consistently across the manuscript.
- For **Line 234, From 2011 onward, annual MCS_{ice} cumulative intensity closely follows the post-calving drift and grounding of B09B: This statement is unclear**, we agree that the statement that MCS_{ice} cumulative intensity “closely follows” the drift and grounding of B09B is insufficiently defined. Based on our preliminary MCS_{nonice} analysis, a revision would instead describe the directly observed spatial and temporal pattern and avoid implying a degree of correspondence or causality that has not been quantitatively demonstrated.
- For **Line 238, consistent with a persistent surface-cold state along the B09B-modified circulation pathway: Is there supporting evidence for this claim**, we agree that “B09B-modified circulation pathway” implies a causal circulation response that is not directly demonstrated by the presented observations. We would remove this terminology and describe the observed spatial concentration of the cold-spell signal without presupposing that B09B modified a particular circulation pathway.

- For **Line 288, after 2011 the cool upper layer extends deeper into the water column: Figure 3g does not support this assertion. Figure 3g shows that the temperature structure before 2010 and after 2014 is not significantly different; the changes only occurred between these two years**, we agree that the statement concerning a deeper cool upper layer after 2011 is too broad relative to the temporal information shown in Fig. 3g. We would revise this passage to describe only the specific differences among the periods represented in the B-SOSE diagnostics and avoid generalizing them to the entire post-2011 period.
- Finally, for **Sections 3.4 and 3.5: Please include the corresponding figure numbers after each argument**, we agree and would add explicit figure and panel references alongside the corresponding results and interpretations.