

RESPONSE TO REFEREE #2

We thank Dr. Anna Olivé Abelló for the careful and constructive assessment of our manuscript and for recognizing the importance of investigating the potential influence of giant icebergs on Southern Ocean conditions and marine cold spells (MCSs). We particularly appreciate the positive assessment of the manuscript's readability and figures, as well as the detailed suggestions concerning iceberg attribution, seasonal processes, sea-ice conditions, and the presentation of the methods. In considering these comments together with those of Referee #1, we have conducted several preliminary exploratory analyses. Below, we describe how we propose to address each of the referee's comments in a potential revision.

Line 18: Consider indicating the SST threshold below which a period is classified as an MCS, or refer the reader explicitly to Sections 2.2.1–2.2.2. This information would help introduce the concept before the detailed methodological description.

We agree that the MCS definition should be introduced more explicitly before the detailed methodological description. In a revision, we would state in the Introduction that MCSs are identified relative to a seasonally varying, grid-cell-specific 5th-percentile SST threshold derived from the 1982–2011 climatological baseline, while referring the reader to the Methods for the complete event-duration and gap criteria.

Line 20: Be more specific about how MCSs may affect upper-ocean stratification, air–sea fluxes, and polar ecosystems. For example, indicate whether they may strengthen stratification, reduce vertical mixing or air–sea heat exchange, and affect biological productivity or habitat conditions. This would better establish both the physical and ecological relevance of MCSs, as is commonly done for MHWs.

We agree that the physical and ecological relevance of MCSs could be introduced more specifically. To improve the manuscript, we would expand this passage briefly to describe how persistent anomalously cold surface conditions may influence upper-ocean stratification, vertical exchange, air–sea heat fluxes, sea-ice conditions, and habitat characteristics. We would distinguish these general potential effects from processes demonstrated specifically by the present study and avoid implying a universal direction of response where this depends on the local hydrographic setting.

Line 31: Consider adding one sentence with further information on iceberg B09, such as its original dimensions, trajectory, previous grounding episodes, etc.

We agree and would provide additional background on B09/B09B, including its dimensions and documented trajectory before and after its interaction with the Mertz Glacier Tongue, as supported by the available observational and published records. We are also examining the available evidence for previous grounding or stationary periods so that these terms can be used more precisely.

Lines 32–35: Clarify which 2010 calving event is being referred to. Did the collision between B09B and the Mertz Glacier Tongue directly trigger the major calving of the glacier tongue?

We agree that the sequence of events surrounding the 2010 Mertz Glacier Tongue calving should be described more precisely. In a revision, we would clarify the timing and documented relationship between the interaction of B09B with the Mertz Glacier Tongue and the subsequent major calving event, while avoiding stronger causal language than is supported by the available evidence.

Lines 30–40: Clarify the reason for selecting this study period. Has B09B remained grounded or stationary in the region since 2011? Why this long period, 1982–2024? Is it to study MCS events before, during and after the iceberg's presence? (The reason appears in Line 53; consider placing it here) Is the hypothesis that these giant icebergs can be related to long MCS events? From my point of view, it is clear in the title but not in the section!

We agree that the rationale for the 1982–2024 study period and the central hypothesis should appear earlier.

Therefore, we would explain that the period is determined primarily by the availability of the OSTIA record and provides a multidecadal interval encompassing conditions before, during, and after the 2010–2011 B09B/Mertz reorganization. This allows the post-2011 state to be evaluated against a substantially longer preceding period.

We would also state the hypothesis more explicitly, namely whether the presence and prolonged residence of a giant iceberg in the Adélie Sill–Commonwealth Bay system could be associated with a persistent change in the spatial and temporal characteristics of MCSs.

The question of whether B09B remained continuously grounded, rather than stationary or resident in the region, would be treated separately and more cautiously in a revision.

Lines 40–45: Consider presenting the analytical sequence more clearly and briefly explaining how the different diagnostics contribute to the study objectives. In particular, justify briefly the reason to use or focus on B-SOSE, and mCDW transport and volume rather than on other water masses, and explain how these diagnostics help assess the proposed mechanism linking B09B to surface cold-spell variability.

We agree that the analytical sequence and the role of the different diagnostics could be explained more clearly.

To improve the manuscript, we would restructure this passage to distinguish among (i) SST-based detection and characterization of MCSs; (ii) sea-ice, open-water, atmospheric, and surface-flux diagnostics used to investigate environmental conditions associated with the observed change; and (iii) B-SOSE diagnostics used to characterize the contemporaneous subsurface ocean state.

Consistent with our response to Referee #1, we would also clarify that B-SOSE does not explicitly represent B09B and therefore cannot directly diagnose an iceberg-induced

91 response. Its role would be restricted to providing context on subsurface hydrography
92 and mCDW rather than demonstrating causation.

93 **Lines 69–73:** I suggest showing the 5th-percentile baseline derived from the 1982–2011
94 OSTIA foundation-SST climatology, either as an additional panel in Figure 2 or as a
95 separate figure in the Supplementary Information. This would provide a useful
96 reference for interpreting the detected MCS events.

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98 We agree that showing the spatial structure of the 5th-percentile climatological
99 threshold would provide useful context for interpreting the detected events.

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101 We therefore propose to include the climatological 5th-percentile threshold as a
102 supplementary figure or additional panel, depending on the final figure organization,
103 together with an explicit description of how it relates to the MCS_{nonice}/MCS_{ice} distinction.

104 **Section 2.2:** Consider consolidating the subsections within the 2.2 MCS framework.
105 Several elements, including the reference to Schlegel et al. (2021) and the use of the
106 5th-percentile threshold, are repeated. A more concise structure would allow the key
107 methodological choices—event duration, permitted gaps, threshold, cumulative
108 intensity, and the -1.7°C MCS_{ice} criterion—to be presented more directly.

109
110 We agree. This suggestion is also consistent with Referee #1's request to streamline
111 Section 2.2. We would consolidate repeated methodological descriptions and present
112 the principal study-specific choices more directly, including the climatological baseline,
113 5th-percentile threshold, minimum event duration, permitted gaps, intensity metrics,
114 and MCS_{nonice}/MCS_{ice} classification.

115 **Section 2.2.4:** Please indicate where the results of the sensitivity experiments are
116 presented. I suggest including a dedicated figure, either in the main text or in the
117 Supplementary Information, showing the effects of the alternative climatological
118 baseline and SST detrending.

119
120 We agree that the sensitivity experiments should be presented more explicitly and
121 would include a dedicated supplementary figure summarizing the alternative
122 climatological-baseline and SST-detrending experiments and refer to this figure directly
123 from the Methods and Results. This would make the robustness tests easier to evaluate
124 than in the present presentation.

125 **Line 124:** Please justify the selection of B-SOSE over other available ocean reanalyses or
126 state estimates, such as GLORYS12v1 or ORAS5. It would also be useful to discuss in the
127 Discussion section the main advantages and limitations of B-SOSE relative to these
128 products, particularly regarding regional resolution, data assimilation, sea-ice
129 processes, freshwater fluxes, and subsurface circulation.

130
131 We thank the referee for this useful suggestion. We agree that the choice of B-SOSE
132 requires stronger justification, particularly given the availability of other ocean
133 reanalysis and state-estimate products.

We would add a concise rationale for using B-SOSE and discuss the relevant strengths and limitations of B-SOSE relative to products such as GLORYS12v1 and ORAS5, including differences in spatial resolution, data assimilation, representation of sea ice and freshwater processes, and subsurface circulation.

We would also make clear that the purpose of B-SOSE in this study is to provide contextual information on the regional ocean state rather than direct evidence of B09B-induced change.

Line 129: Explain why open-water and polynya conditions are represented as 1-fractional sea ice area, rather than using the commonly applied criterion of sea-ice concentration below 0.15.

We agree that this choice requires clarification. The quantity used in the submitted manuscript, one minus the fractional sea-ice area, represents the continuous open-water fraction of each grid cell rather than a binary classification of the grid cell as open water or ice covered.

We would clarify this distinction and, where the purpose is specifically to identify open-water areas spatially, consider additionally applying the commonly used SIC < 0.15 criterion. This would also facilitate comparison with the spatial sea-ice diagnostics suggested by the referee.

Line 149: Specify the potential-density range used to identify mCDW. At present, the definition relies on the upper 0.5% of the annual density distribution, but the resulting density limits are not reported.

We agree and intend to report the actual potential-density limits resulting from the upper 0.5% annual density criterion rather than providing only the percentile-based definition.

Section 2.6 I would rename the section to reflect the ERA5 and Copernicus dataset information, and remove from this section "Bathymetry for the Adélie Sill-Commonwealth Bay region was taken from the Terre Adélie and George..." and place the depth breaks below where they correspond, i.e., the caption of one figure, and remove the domain (140–148°E, 65.5–68.5°S) as it has been repeated throughout the text.

We agree and would rename and reorganize Section 2.6 to reflect the ERA5 and Copernicus datasets more clearly, relocate the bathymetric and depth-break information to the section or figure caption where it is directly relevant, and remove unnecessary repetition of the study-domain coordinate.

Line 262: Discuss what may cause the apparent delay between the winter peaks and the subsequent summer MCSice maxima immediately after 2011.

We thank the referee for highlighting the apparent lag between winter conditions and subsequent summer cold-spell maxima.

Our preliminary month-by-month analysis, conducted in response to both referees, suggests that this issue can be addressed more clearly by separating MCS_{nonice} and MCS_{ice} and examining their complete seasonal cycles rather than relying only on the broad summer/winter categories used in the submitted manuscript.

We would use the monthly analysis to establish the timing of the MCS response first and discuss any apparent winter-to-summer lag as a possible preconditioning relationship only where supported by the diagnostics. We would avoid implying that winter variability necessarily causes the subsequent summer response.

Figure 3: Since the strongest post-2011 MCS_{ice} intensification occurs in summer, why are the freshwater diagnostics restricted to winter? Summer freshwater flux may be more directly related to contemporaneous surface stratification and cold SST anomalies, particularly if meltwater from B09B is proposed as part of the mechanism. Maybe I'm missing something...

We agree. This comment is closely related to Referee #1's concern regarding the winter-focused process analysis.

Our preliminary monthly analysis suggests that MCS_{nonice}, which we propose to use as the principal diagnostic in a revision, is predominantly associated with the more open-water portion of the year. This reinforces the need to examine concurrent conditions rather than restricting the process analysis to winter.

We would therefore broaden the freshwater and surface-state diagnostics beyond winter where the available data support doing so and reconsider whether freshwater forcing should retain the prominence it has in the submitted interpretation.

Line 283: Why would an immediate reduction in deep warm water be expected following the calving event? Please explain the physical reasoning behind this expectation. Increased freshwater input and shelf stratification could instead strengthen or modify the eastward undercurrent at the shelf break, potentially promoting greater mCDW inflow onto the continental shelf instead of reducing its presence.

We agree that the submitted manuscript does not adequately justify why an immediate reduction in deep warm-water presence should be expected following the calving event.

We would remove this expectation as an assumed response and instead use the B-SOSE diagnostics descriptively to evaluate the observed subsurface state. We would also acknowledge that changes in freshwater input, stratification, and shelf circulation could alter mCDW transport in different ways, including potentially maintaining or enhancing rather than reducing its presence.

Lines 392–398: The discussion of Adélie penguins is relevant as a broader ecological implication, but the proposed relationship between stronger MCS_{ice} exposure and reduced open-water access is not supported by the results presented here and appears inconsistent with the reported increase in openwater fraction and decrease in sea-ice volume after 2011. Please either remove this statement or rephrase it.

We agree that the proposed connection between stronger MCS_{ice} exposure and reduced open-water access is not directly demonstrated by the analyses presented.

We would remove this specific inference. Any remaining discussion of potential ecological implications would be clearly identified as broader context and limited to implications supported by the environmental changes demonstrated by the analysis.

Lines 349–354: Please show the spatial evolution of winter sea-ice concentration, ideally in maps comparable to Figures 2 or 5, and indicate areas with $SIC < 0.15$ (commonly considered open water). If possible, also show the spatial distribution of leads. This would better support the interpretation of post-2011 changes in polynya geometry, sea-ice retention, and winter surface conditions than the current domain-averaged indices alone.

We thank the referee for this suggestion. We agree that spatial diagnostics would provide stronger evidence for changes in sea-ice configuration than domain-averaged indices alone.

We propose to examine the spatial evolution of sea-ice concentration, including the distribution of areas satisfying $SIC < 0.15$, and, where supported by the available lead dataset, the spatial distribution of leads. These diagnostics would allow us to evaluate whether changes in open-water exposure and sea-ice retention are spatially colocated with the principal post-2011 MCS_{nonice} signal.

Line 208: Do the authors have satellite-altimetry measurements of B09B thickness to confirm that its draft exceeded the Adélie Sill depth and that the iceberg was therefore grounded? According to Figure 2, its position changes between years before 2019, suggesting that it may not have remained continuously grounded from 2011 onward. Moreover, an iceberg may remain temporarily stationary because ocean currents and winds are too weak to move it, without necessarily being grounded. Please clarify how grounding was distinguished from temporary stationarity, specify its timing, and provide supporting evidence.

We thank the referee for raising this important distinction. We agree that prolonged residence or apparent stationarity alone is insufficient to demonstrate grounding and that the submitted manuscript uses the term “grounding” more confidently than is justified by the evidence presently shown.

We are currently examining the available observational and published evidence concerning B09B’s thickness/draft, bathymetry, trajectory, and reported grounding history. We would explicitly distinguish among documented iceberg position, prolonged residence or stationarity, and independently supported grounding.

Unless continuous grounding can be demonstrated from independent evidence, we would not assume that B09B remained grounded continuously from 2011 onward. Instead, we would adopt more cautious terminology such as “presence” or “prolonged

residence near the Adélie Sill" and specify any periods of grounding only where they can be supported observationally.

This reassessment would also be reflected consistently in the title, Abstract, Results, Discussion, and figures.

Line 230: The manuscript attributes the increase in MCSice cumulative intensity mainly to the grounding of B09B. However, the observed changes may also reflect the broader post-calving reorganisation, including the persistence of other large fragments and the presence of B09B itself. How can these different effects be disentangled? Satellite images showing the evolution of B09B and the absence or drift of other fragments outside the region would help support the proposed attribution.

We agree that the submitted manuscript attributes the observed change too specifically to B09B grounding when the available evidence may also be consistent with a broader post-calving reorganization involving B09B, other iceberg fragments, sea-ice conditions, and changes in regional circulation.

We would therefore broaden the attribution framework. Rather than treating B09B grounding as the sole explanation, we would distinguish between the temporal and spatial association of the MCS change with B09B's presence near the Adélie Sill and the broader environmental reorganization following the 2010 Mertz Glacier Tongue calving.

We would also investigate whether available satellite imagery and iceberg records can better constrain the trajectories and persistence of B09B and other major fragments. Where the available observations do not permit these influences to be disentangled, we would state this explicitly as a limitation rather than assign the observed response uniquely to B09B.

Methods: The source and processing of the B09B position data should be described in the Materials and methods. Although the BYU iceberg tracking database is mentioned in the Code and data availability section, the temporal resolution of the tracking data is not specified, nor is it stated which specific satellites were considered or how uncertainties or gaps in the iceberg trajectory were handled. In addition, I suggest including a two-panel figure showing the track of B09B from the time it enters the study region to its most recent position: one panel showing the trajectory coloured by month/year, and a second panel showing the residence time in each area. This would help assess whether the iceberg became effectively stationary.

We agree that the submitted Methods do not provide sufficient information on the iceberg-position dataset.

We would document the source, temporal resolution, processing, and known limitations of the B09B tracking data explicitly in the Methods rather than mentioning the source only in the Code and data availability section. We would also investigate the underlying observational sources and the treatment of temporal gaps and positional uncertainty before interpreting periods of apparent stationarity.

We appreciate the suggestion of visualizing the trajectory through time and residence within the study region. Subject to the temporal resolution and completeness of the available tracking record, we would consider a trajectory/residence figure or supplementary figure to provide a more transparent basis for distinguishing movement, prolonged residence, and possible grounding.

Results: The persistence of MCSs appears to be associated with increased open-water exposure and reduced sea-ice volume. However, the link between these changes and the presence of B09B remains unclear. Please explain how the giant iceberg could promote more recurrent ice-free conditions or reduced sea-ice retention in the following years.

We agree that the submitted manuscript does not sufficiently demonstrate the physical link between B09B's presence and the observed increase in open-water exposure and reduced sea-ice volume.

We would avoid presenting this relationship as established solely from temporal coincidence. Instead, the proposed spatial sea-ice and lead analyses would be used to assess whether the post-2011 changes have a coherent geographical relationship with the Adélie Sill–Commonwealth Bay sector and B09B's residence.

Any proposed mechanism involving modification of sea-ice transport, retention, or open-water formation would be presented as a hypothesis unless directly supported by the available diagnostics.

Lines 265–276: The reported diagnostics do not show a clear post-2011 increase in freshwater forcing. Sea-ice freshwater flux trend decreases, sea-ice volume is reduced, and lead and open-water fractions increase. Could the lower SSTs therefore be driven primarily by enhanced ocean–atmosphere heat loss through more frequent leads and open-water areas, rather than by freshwater-induced stratification associated with B09B freshwater flux?

We thank the referee for this important alternative interpretation. We agree that the diagnostics presented in the submitted manuscript do not demonstrate a clear post-2011 increase in freshwater forcing and therefore do not justify assigning freshwater-induced stratification a dominant mechanistic role.

The combination of reduced sea-ice volume and increased lead/open-water fractions instead raises the possibility that enhanced ocean–atmosphere heat loss through more frequently exposed surface water contributes importantly to the observed cold surface conditions.

We would explicitly evaluate this alternative and restructure the process discussion so that the mechanisms are guided by the observed diagnostics rather than by an assumed freshwater response. In particular, we would examine the seasonal and spatial correspondence among MCS_{nonice}, sea-ice concentration, open-water/lead occurrence, and relevant atmospheric or surface-flux conditions. Freshwater-induced stratification would be retained only to the extent supported by the revised diagnostics.

Appendix A / Figure A1: Appendix A is not explicitly introduced or discussed in the main text, although Figure A1 is cited. Moreover, the appendix heading appears after Figure A1 and no accompanying methodological or explanatory text is provided, making the appendix appear incomplete. Please move the Appendix A heading before the figure and add a short self-contained description of the sensitivity experiments and their interpretation. Alternatively, if the appendix is intended only to contain an additional figure, consider moving Figure A1 to the Supplementary Information and referring to it accordingly in the main text.

We agree that Appendix A is incomplete in its current presentation.

Our preference would be to move the sensitivity material into the Supplementary Information and provide a concise, self-contained description of the alternative-baseline and detrending experiments, together with an explicit reference from the main Methods or Results. This would also address the referee's earlier request for clearer presentation of the sensitivity analyses.

Title: The title assumes that iceberg B09B was grounded during the whole period, although the manuscript does not provide direct evidence of grounding from prolonged stationarity. Unless grounding can be independently demonstrated through satellite-altimetry-derived thickness, iceberg draft-bathymetry comparison, or other observational evidence, I suggest using a more cautious term such as "arrival", "presence", "collision" or "prolonged residence near the Adélie Sill".

We agree that the title should not imply continuous grounding unless this can be independently demonstrated.

As noted above, we are examining the available observational and published evidence concerning B09B's grounding history, thickness/draft, trajectory, and prolonged residence near the Adélie Sill. If continuous grounding cannot be demonstrated adequately, we would revise the title and corresponding terminology throughout the manuscript to use a more cautious description such as "prolonged residence near the Adélie Sill" or "presence near the Adélie Sill."

The final terminology would reflect the strongest conclusion supported by the available observational evidence rather than assuming grounding from stationarity alone.

Minor comments

- **General: Please standardise the spelling of "cold-spells" and "cold spells". Both forms are currently used:** We agree and would standardize the terminology throughout the manuscript, using "cold spells" except where a hyphen is grammatically required in a compound modifier.
- **General: Use dashes consistently for geographical ranges or linked geographical names, e.g. "Adélie Sill-Commonwealth Bay" and "Adélie-George V Land", rather than alternating between hyphens and en dashes:** We agree and would standardize the use of en dashes for linked geographical names and ranges throughout the manuscript.

- **Line 29: poorly documented and understood:** We agree and would revise the wording to “poorly documented and understood.”
- **Lines 75–76: Replace “more common 10th percentile for cold-spells” with “more commonly used 10th-percentile threshold for cold spells”:** We agree and would replace the phrase with “more commonly used 10th-percentile threshold for cold spells.”
- **Line 79: Please specify the threshold for only the most pronounced cold anomalies:** We agree and would explicitly specify the threshold used to define the most pronounced cold anomalies.
- **Line 84-86: repeated information? Otherwise, clarify better:** We agree that the information is repetitive and would consolidate this passage for clarity.
- **Line 129: Replace “one minus fractional sea-ice area” with “one minus the fractional sea-ice area”:** We agree and would change the wording to “one minus the fractional sea-ice area.”
- **Figure 2: The first panel is labelled “1982–1983”, although the caption states that each panel represents one year. Please either separate 1982 and 1983 or explain why these two years are combined:** We agree that the caption is inconsistent with the combined 1982–1983 panel. If this figure structure is retained in a revision, we would explicitly explain why these two years are combined; alternatively, we would separate them so that each panel represents a single year.
- **Figures 2 and 5: Please consider changing the colour used for the ice shelves/ice tongue, as it is too similar to the shading used for cells with no data to improve the readability of the maps:** We agree and would modify the map colors so that ice shelves and the ice tongue are visually distinct from cells containing no data.
- **Line 268: Delete “respectively” after the list “2015, 2023, 2022, 2014, and 2018”, because the list is not paired with a second corresponding list:** We agree and would delete “respectively.”
- **Lines 280- 285: Sort the years chronologically throughout the text:** We agree and would order the years chronologically.
- **Line 315: Replace “does not indicate disappearance of the corridor signal” with “does not indicate the disappearance of the corridor signal”:** We agree and would revise the phrase to “does not indicate the disappearance of the corridor signal.”
- **Figure 4 caption: Replace “with strongest negative trends” with “with the strongest negative trends”:** We agree and would revise the wording to “with the strongest negative trends.”
- **References: Fischer et al. (2025a, b): These two entries have the same title, journal, volume, article number, and DOI and therefore appear to be duplicate references:** We thank the referee for identifying this duplication. We would remove the duplicate reference and update the corresponding in-text citation(s).

We thank the referee again for the detailed and constructive comments. In combination with the comments of Referee #1, they have helped us identify several areas in which

the manuscript could be substantially strengthened. Our preliminary analyses suggest that emphasizing MCS_{nonice} , resolving the seasonal behavior at monthly scale, broadening the process analysis beyond winter freshwater forcing, and distinguishing more carefully between B09B grounding, prolonged residence, and the broader post-calving reorganization would provide a more robust framework for evaluating the observed post-2011 change.

If invited to revise, we would incorporate these changes comprehensively and ensure that the strength of the attribution to B09B is commensurate with the available observational evidence.