Response to Reviewer

Dear Reviewer,

We sincerely thank you for taking the time to review our manuscript and for providing such insightful and constructive comments. Your thorough and professional feedback has been invaluable in helping us improve the clarity, accuracy, and overall quality of our work.

We have carefully considered each of your suggestions and have revised the manuscript accordingly. Below, we provide a point-by-point response to your specific comments, detailing the changes made in response to each one. The text in blue represents the comments.

Major issues:

Altimetry Dataset and Eddy Detection and Tracking (EDT)

Comments: The manuscript uses the C3S DUACS two-satellite L4 SLA product. While a fixed two-altimeter constellation can improve long-term stability/homogeneity (important for climate metrics), the C3S QUID explicitly states that the all-satellite CMEMS products provide better spatial sampling and reduced mesoscale errors and "should be preferred for oceanic mesoscale applications," whereas the C3S two-satellite products are intended for climate applications. The authors should justify this choice in the context of eddy detection/tracking, and ideally add a short sensitivity test (e.g., a representative sub-period) comparing C3S vs CMEMS all-satellite to show that eddy counts/sizes/intensities and the main conclusions are not materially affected.

Response: Thank you for this insightful and critical comment regarding the suitability of the altimetry dataset for eddy detection studies. We fully agree with the reviewer's assessment.

In direct response, we have replaced the previously used C3S DUACS two-satellite L4 SLA product with recommended and more appropriate CMEMS all-satellite L4 product (SEALEVEL_GLO_PHY_L4_MY_008_047). As explicitly stated in the official Product User Manual (Pujol and Grassi, 2025), the all-satellite products provide superior spatial sampling and lower mesoscale errors, and "should be preferred for oceanic mesoscale applications" (Section 2.i), whereas two-satellite products are intended for climate-scale studies prioritizing long-term stability.

Given that we have now adopted the dataset specifically recommended for mesoscale analysis, which represents the current best practice for studies like ours, we believe a direct comparison with the less suitable previous dataset is not necessary. The new analysis, therefore, is based on the most robust and appropriate data foundation from the outset. All eddy detection, tracking, and subsequent analyses in the revised manuscript have been performed using the superior CMEMS all-satellite product.

Comments: Several recent eddy atlases and methodologies increasingly rely on ADT rather than SLA for eddy detection/tracking, as SLA removes the mean dynamic topography and may therefore suppress quasi-permanent features associated with standing meanders, jets, or long-lived structures (e.g., Laxenaire et al., 2018; Pegliasco et al., 2022). Please justify the use of SLA here and clarify whether using ADT would affect eddy detection and the inferred eddy–front relationships.

Response: Thank you for raising this important point regarding the use of SLA versus ADT. We acknowledge that ADT is indeed valuable for capturing quasi-permanent features like frontal jets. However, for the specific objectives of our study, focusing on the properties and cross-frontal behavior of propagating mesoscale eddies, the use of SLA is not only standard but methodologically more appropriate for the following reasons:

First, the core dynamic variable for analyzing propagating eddies is the geostrophic velocity anomaly (u',v'), from which Eddy Kinetic Energy (*EKE*) is derived ($EKE=(u'^2+v'^2)/2$). These velocity anomalies are calculated directly from the gradients of the SLA field via the geostrophic relationship. Therefore, the SLA field is effectively the streamfunction for the anomalous flow associated with eddies. Defining eddy boundaries using closed SLA contours is a well-established and physically robust method, as employed in foundational global datasets (e.g., Chelton et al., 2011) and the specific algorithm we adopted (Saraceno and Provost, 2012). Using ADT, which represents the absolute flow including the large-scale background, would inherently suppress the signal of eddies embedded within strong frontal jets.

Second, to quantitatively address your concern, we performed a comparative eddy detection analysis using both ADT and SLA in an energetic region (the Brazil/Malvinas Confluence and the Brazil Current retroflection system; see Figure R1). The results confirm the expected trade-off: (1) As noted, ADT excellently captures the major jets and large meanders, which are absent in the SLA field. (2) Crucially, however, the SLA-based method detected a larger number of smaller, propagating eddies, particularly at the peripheries of the energetic region. Eddy parameters (radius, amplitude) derived from SLA were also more representative of the mesoscale signal. This demonstrates that while ADT is superior for studying the mean flow, SLA is more sensitive to the transient mesoscale eddies that are the focus of our study.

Third, we wish to clarify that the climatological positions of the ACC fronts used in our study (from Park et al., 2019) were themselves derived from a mean dynamic topography product (MDT, specifically CNES-CLS18), which is the time-averaged component of the ADT. Therefore, the frontal zones against which we measure eddy interactions are already defined using the “absolute” field recommended for identifying quasi-permanent features. Our methodology thus appropriately uses SLA (for the transient eddies) in conjunction with an MDT-based product (for the semi-permanent fronts).

In conclusion, our choice of SLA is grounded in the fundamental physics of eddy dynamics and is the standard for this type of analysis. The comparative test confirms that it optimizes the detection of the propagating mesoscale eddies central to our research questions. We are confident that this approach is the most suitable for investigating cross-frontal eddy transport.

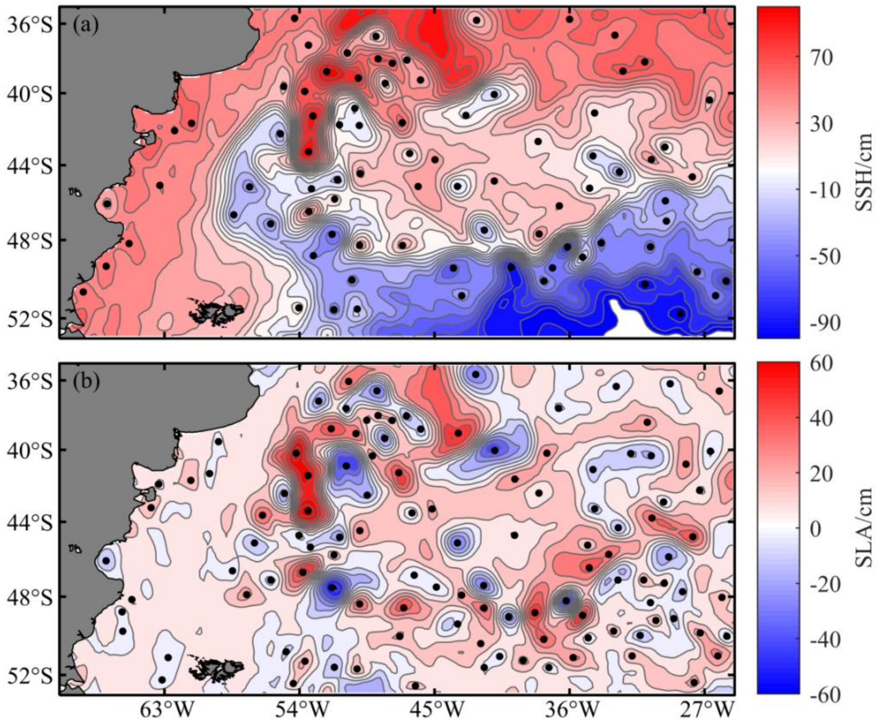

Figure R1. Comparison of eddy detection results based on SSH (or ADT, a) and SLA (b), respectively on Jan. 1st, 2019. Black dots are eddy centers. Contour intervals both for ADT and SLA are 8 cm.

145-147: "A threshold W= −0.2σw was often chosen to identify the outer boundary of an eddy, with being the standard deviation of W over the entire region (e.g., Henson and Thomas, 2008; Isern-Fontanet et al., 2006)."

Comments: The definition of σw is unclear. Is it the standard deviation of W computed over the full spatial domain for each day separately, or over the full domain and the entire study period? Please clarify. In addition, the choice of the threshold needs stronger justification for the Southern Ocean/ACC context. The cited studies (e.g., Isern-Fontanet et al., 2006) applied this criterion in different dynamical regimes and data products, and such a fixed fraction of σw may be product and region dependent. A brief sensitivity analysis (e.g., varying the coefficient around 0.2) would help demonstrate that the results are robust.

Response: Thank you for raising these critical methodological points, which have allowed us to improve the clarity of our manuscript. We apologize for any lack of clarity in the original text.

First, this study used the outermost closed SLA contour to delineate eddy boundaries, not a fixed W= −0.2σw threshold. As validated in previous studies cited, this geometric approach is specifically chosen to define coherent structures.

Second, we concur with your underlying point. As noted in our revised text (and by scholars like Matsuoka et al., 2016, and Saraceno and Provost, 2012), a fixed W threshold can lead to biases such as underestimated eddy areas or the misidentification of meanders as eddies in energetic regions. The outermost closed SLA contour provides an eddy-specific geometric definition that avoids these issues. In direct response, we have revised the methodology section (Lines 145-155) to clearly state our approach, acknowledge the alternative method, and justify our choice, thereby preventing any future misunderstanding.

Since our core methodology is fundamentally based on the geometric SLA contour method, a well-established technique whose parameters are derived from the physical shape of each eddy, a sensitivity test on an unused W threshold coefficient would not yield relevant insights for our specific analysis. The robustness of our approach is inherent in its design and prior validation. We are confident that the revised manuscript now presents a clear, robust, and well-justified methodology.

150-151: "SLA data with 0.25° spatial resolution were first linearly interpolated to 0.125° for better performance in eddy detection."

Comments: I am concerned this is not appropriate, sampling to a finer grid does not add information and may introduce interpolation artefacts (artificial noise/small-scale structure) that can bias eddy detection and tracking. I recommend performing the analysis on the native-resolution SLA.

Response: We thank the reviewer for this important methodological insight. We fully agree that interpolating to a finer grid does not add information and may introduce artifacts. In response, the entire eddy detection, tracking, and all subsequent analyses have been revised using a SLA dataset with native-resolution of 0.125°×0.125° from the Copernicus Marine Service (the CMEMS all-satellite L4 SLA product SEALEVEL_GLO_PHY_L4_MY_008_047), without any interpolation. The data source has been described in Lines 110-114.

152-156: "For eddy tracking, the algorithm identifies eddies at time t+1 that meet the following criteria relative to time t: (1) minimal centroid distance, (2) identical polarity (i.e., rotation direction), and (3) the minimum radius variation. If no eddy at t+1 satisfies these proximity thresholds for a given eddy at t, the eddy is considered dissipated. Conversely, if an eddy detected at t+1 does not match any eddy at t, it is classified as a newly generated eddy. Based on eddy identification and tracking results, this study focuses on eddies with lifespans exceeding 7 days and amplitudes greater than 2 cm."

Comments: The proposed eddy detection/tracking workflow (OW thresholding combined with outermost closed SLA contours and a local nearest-neighbour tracking based on centroid distance/polarity/radius change) appears insufficiently robust, especially for the Southern Ocean, where strong jets, crowding and deformation make eddy identification and track continuity challenging. Such simplified schemes are prone to track switching, fragmentation and sensitivity to thresholds. In addition, the adopted selection criteria (lifetime > 7 days; amplitude > 2 cm) require justification. I recommend either adopting a validated state-of-the-art EDT framework (e.g., Chelton/Schlax-type contour methods, META3.1exp/TOEddies) or providing thorough validation and sensitivity analyses (including reasonable variations of tracking parameters and thresholds) demonstrating that eddy statistics and the derived CFE conclusions are not materially affected by the chosen assumptions.

Response: Thank you for this insightful comment regarding the potential for track fragmentation and spurious detection in eddy tracking algorithms. We agree that this is a critical issue for ensuring the reliability of any eddy dataset. In direct and comprehensive response to this comment, we have taken a multi-pronged, conservative approach to rigorously address this concern:

First, to systematically exclude short-lived, weak, or marginally resolved features that are most susceptible to the tracking artifacts noted by the reviewer, we have implemented substantially more stringent selection criteria. Our analysis now focuses exclusively on eddies that meet all of the following thresholds:

(1) **radius > 30 km**, to guarantee the eddy structure is sufficiently larger than the ~14 km native grid scale;
(2) **amplitude > 5 cm**, to select energetically strong mesoscale signals;
(3) **lifespan > 14 days**, to ensure temporal coherence beyond synoptic noise.

The eddy statistics and the derived CFE conclusions were obtained exclusively from these significant eddies. These thresholds (14 days, 5 cm, 30 km) are substantially more conservative than the initial, more permissive parameters used in the original manuscript. They are designed to **systematically exclude short-lived, weak, or marginally resolved features that are most susceptible to the tracking errors (fragmentation, switching)** noted by the reviewer.

Second, the most decisive validation of our tracking algorithm's reliability comes from independent Lagrangian observations. We identified several long-lived CFEs (with lifespans of 206, 219, and 307 days) in which Argo profiling floats were continuously entrapped for extended periods (e.g., up to 194 days). As shown in Figures R2-R5, the floats remained within the eddy interiors as defined by our algorithm, recording persistent thermohaline anomalies throughout their entrapment. This prolonged, continuous entrainment provides unambiguous evidence that the eddies we track are physically coherent water bodies capable of trapping and transporting water masses over long distances and timescales. It conclusively demonstrates that our tracks represent genuine, material-coherent eddies, not artifacts of algorithmic fragmentation or switching.

We are confident that the combination of (a) conservative data filtering and (b) direct observational validation robustly addresses the reviewer's concern and underscores the reliability of our eddy dataset and the conclusions drawn from it.

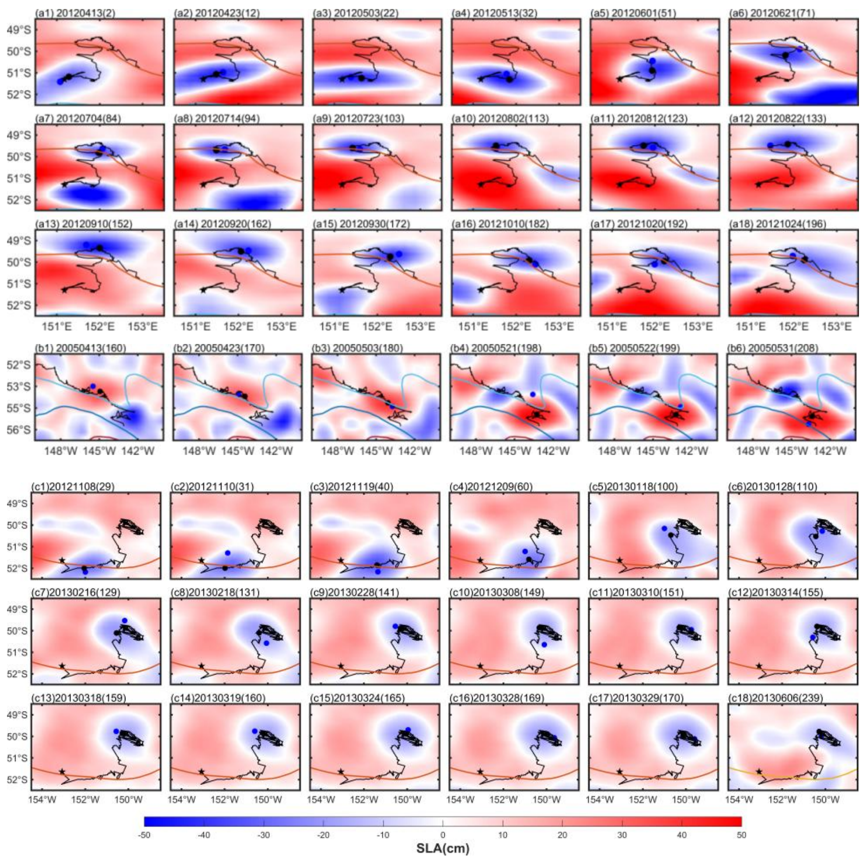

Figure R2. Spatiotemporal evolution of the three cross-frontal eddies (CFEs) and associated Argo float positions. a1–a18: First cyclonic eddy (CE); b1–b6: Second anticyclonic eddy (AE); c1–c18: Third CE. Black and blue dots denote real-time positions of eddy centers and Argo profiles, respectively. Stars represent the eddy genesis points. Black lines are complete propagation trajectory over the eddy's lifespan. Red and blue lines indicate the Subantarctic Front (SAF) and the Polar Front (PF),

respectively. Dates are marked above each subplot, and the number in parentheses represents the number of days since it was generated.

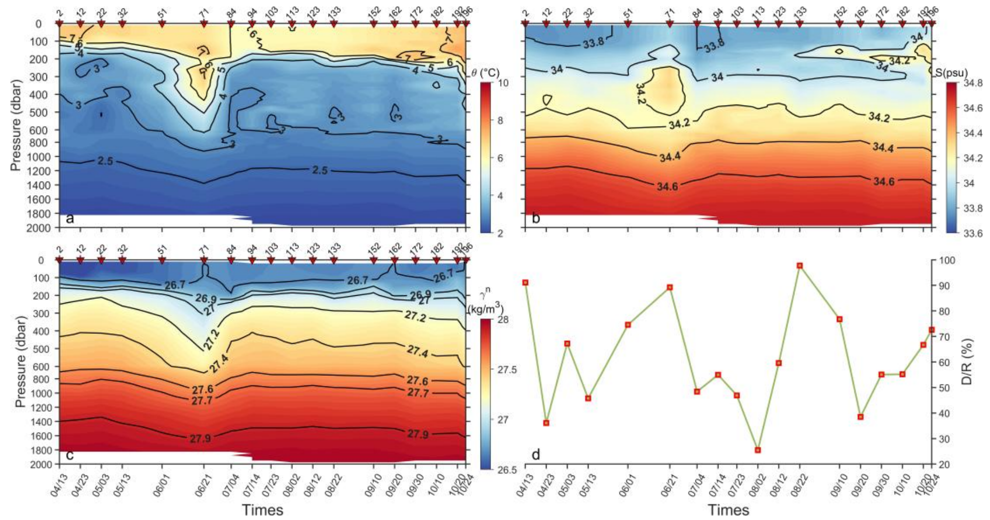

Figure R3. Sectional distributions within the first detected CE in a1-a18 of Figure R2. (a) potential temperature, (b) salinity, (c) neutral density; (d) Time series of normalized distance (Argo distance from eddy center divided by eddy radius).

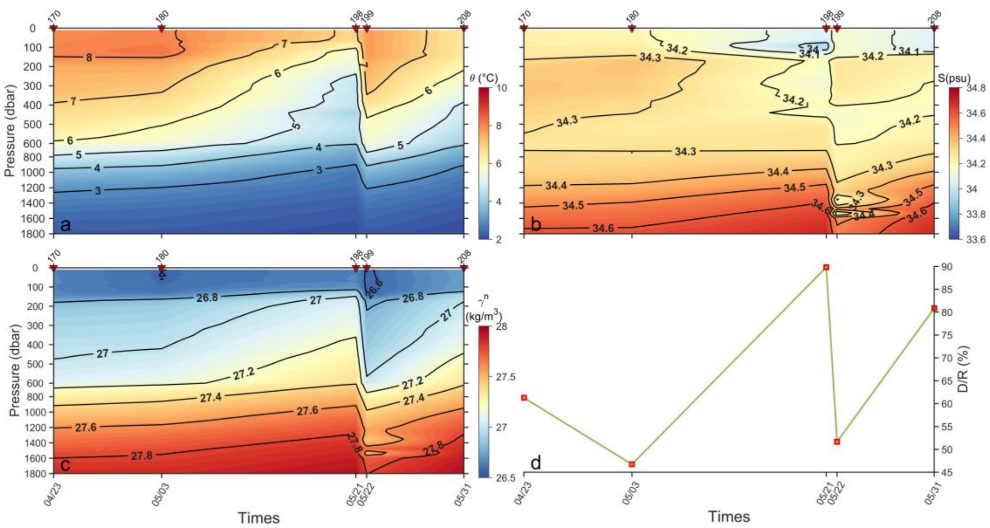

Figure R4. The same as Figure R3, but for the detected AE in b1-b6 of Figure R2.

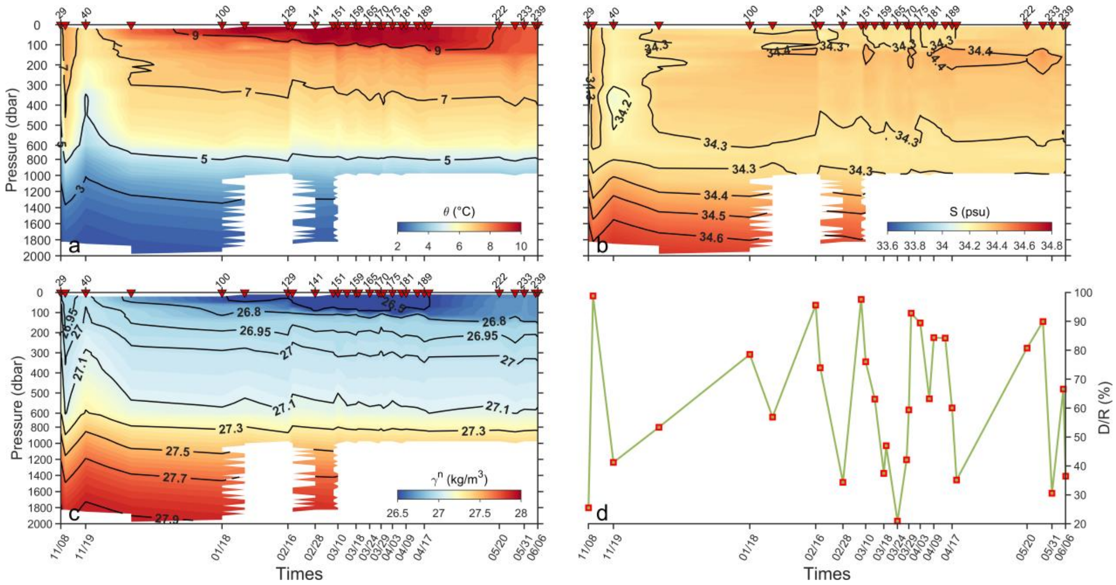

Figure R5. The same as Figure R3, but for the detected CE in c1-c18 of Figure R2.

ACC frontal zone definition

169-170: "To account for topographically induced frontal displacements, we defined frontal zones as a strap ±15 km expanded in the normal directions from each climatological front, consistent with observed SO frontal oscillation area (Kim and Orsi, 2014)."

Comments: It is not clear how the ±15 km band is justified from Kim and Orsi (2014). In that paper, the reported meridional drift of the ACC fronts between 1993 and 2010 is on the order of ~28–46 km (their Fig. 8), and the estimated total drifts assuming a linear trend show sector-dependent values, with the Pacific sector spanning roughly −100 to +20 km depending on the front (their Fig. 9). This suggests that frontal variability/displacement can exceed the adopted 15 km half-width. Because this threshold likely influences eddy-front colocation statistics and therefore the main conclusions, it should be clarified and explicitly justified. A more defensible approach could be to define frontal zones based on the observed latitudinal variability of each front in the study sector (e.g., mean ±1 standard deviation of front latitude), rather than using a fixed ±15 km value. Therefore, this definition should be revisited.

Response: Thank you for this insightful comment. The point you raised concerning the justification of the fixed bandwidth (±15 km) is of great importance, as it directly influences the statistics of eddy-front colocation events and the study's main conclusions. We fully agree that this threshold requires clear and explicit justification.

Based on your suggestions, we have revisited our methodology and conducted additional analyses. Below, we provide a detailed explanation of the rationale behind our approach and outline the corresponding revisions made in the manuscript.

1. Interpretation of Kim and Orsi (2014) and Focus of Our Study

As you noted that, Figure 9 in Kim and Orsi (2014) shows considerable cumulative frontal displacement around the Campbell Plateau and its downstream in the Pacific sector (> 30 km southward, 150°E-170°W), with the largest total drift being approximately 80 km southward at 150°E, while much less total displacement eastward (< 30 km). Figure 10 in this paper also indicates that most of the annual cycles in the Pacific sector are less than 30 km, with "most of the circumpolar regions the amplitude of seasonality is less than 10 km". Thus, the the ±15 km half-width (resulting in a 30 km total strap) is intended to be consistent with the typical long-term displacement and annual cycle for most regions other than the Campbell Plateau. About this critical region, this paper also stated that eddy activity can be a main determining factor for meridional frontal locations downstream of prominent topographic obstacles, such as Campbell Plateau, and raised that the spatial distribution of a streamline becomes greater due to the production of mesoscale eddies through eddy-mean flow interaction processes. These statements suggest that eddy-front interactions should be accounted for during frontal zone determination in this region.

2. Justification for the ±15 km Bandwidth, Physical Criteria, and Robustness Check

Our method does not rely on a fixed ±15 km zone for identifying eddy-front interactions. As described in the subsequent sentences (Lines 171-184), we defined an eddy-front interaction dynamically based on the eddy's instantaneous radius (R). An interaction was recorded only when the eddy's boundary first touches the ±15 km zone. Given that all analyzed eddies have R > 30 km, this created an effective interaction zone with a minimum half-width of (15+30) = 45 km. This 90 km total width is larger than the ~80 km maximum meridional drift near the Campbell Plateau, ensuring robust capture of interactions near the variable fronts.

Following your suggestion, we performed a comprehensive sensitivity analysis by expanding the baseline frontal zone to ± 25 km. The results confirm that the main conclusions of our study, such as the differences between cross-frontal and non-cross-frontal eddies and the overall characteristics of eddy-front energy exchange, remain unchanged. This demonstrates that our core findings are not sensitive to the exact bandwidth within a reasonable range (15–25 km).

3. Consideration of the Suggested "Standard Deviation-Based" Definition

We greatly appreciate your suggestion to define frontal zones based on the observed latitudinal variability (e.g., mean ±1 standard deviation) of each front. While this approach is excellent for climate-scale analyses, for our weather-scale process-oriented study, using a fixed width combined with the geometric criterion offers the following advantages: (a) it avoids incorporating long-term trend and interannual signals into the detection of instantaneous interactions; (b) it provides a consistent and comparable baseline for interaction across all fronts and time periods; and (c) it aligns methodologically with our eddy identification scheme based on instantaneous flow fields.

Once again, we sincerely thank you for your constructive comments, which have significantly helped us improve the clarity and rigor of our work.

Minor issues:

40: Please briefly clarify in the text the distinction between front and jet

Response: Thank you for this suggestion. We have added the description of front and jet in Lines 39-42 as "The ACC comprises multiple zonal fronts, where oceanic jets exit. Here, a front refers to a boundary between distinct water masses, characterized by strong horizontal density gradients, while a jet denotes the narrow, swift current that flows along the axis of such a front. Together, they form the dynamic core of the ACC."

70-73: Sentence is too articulated. I suggest simplifying it, by splitting it into two shorter sentences to improve clarity and flow.

Response: We fully agree with your suggestion. The long sentence has been split into two shorter, more logically structured sentences to improve readability. The revised text is in Lines 71-74 as : "He et al. (2023) demonstrated that nearly half of the subsurface temperature extremes in the Southern Ocean occur within eddies, with cross-frontal eddies (CFEs) generating extremely high-temperature events on the cold side of the ACC and extremely low-temperature events on the warm side. These extremes eventually impact marine organisms and ecosystems."

111: Please provide the DOI of the Copernicus Marine product used, and report the date of access/download

Response: Thank you for the reminder. We have supplemented the description of the dataset in the "Data and Methods" section with the DOI link and the specific access date for the SEALEVEL_GLO_PHY_L4_MY_008_047 product. The revised text states: "The data were accessed/downloaded on 18 December 2025 from the Copernicus Marine Service Information (DOI: https://doi.org/10.48670/moi-00148)." in Lines 112-114.

114-117: Since the horizontal velocities are provided directly by Copernicus, I suggest removing the detailed description of how they are computed. As written, it may imply you derived the velocities yourself. Readers interested in the processing can refer to the product documentation.

Response: Thank you for your suggestion. The detailed formula and description for calculating geostrophic velocities have been removed following your advice. The text now directly states that the velocity anomalies are provided within the dataset and refers readers to the product documentation for processing details. The revised text is: "The SLA represents the sea surface height anomaly... The corresponding geostrophic velocity anomalies (u′, v′) are provided within the same dataset... For detailed processing algorithms, users may refer to the product documentation (Pujol and Grassi, 2025)." (Lines 115-118)

118-126: This paragraph could be shortened for readability. Also, it may help to more clearly separate what is taken from Park et al. versus what is done in this study. For instance, you could briefly state that you used the ACC front positions from Park et al. (altimetry-based), which were validated against available subsurface observations (e.g., Argo over the relevant period and CTD data in 2016–2017).

Response: Thank you for your suggestion. This paragraph has been significantly shortened and rewritten to clearly state that we are using the published, validated synthesis product from Park et al., rather than performing frontal identification ourselves. The revised text reads: "The geographical positions of the ACC's fronts and boundaries used in this study are from the synthesis of Park et al. (2019). This dataset provides the most updated mapping of the ACC frontal system and its associated boundaries, derived from satellite altimetry and independently validated against extensive subsurface observations, including Argo float profiles (2001–2017) and dedicated CTD surveys (2016–2017). As shown in Figure 1, the dataset defines five major streamlines from north to south: the Northern Boundary (NB), the Subantarctic Front (SAF), the Polar Front (PF), the Southern ACC Front (SACCF), and the Southern Boundary (SB). Specifically, the NB represents the northern dynamical limit of the ACC and coincides with the northern expression of the Subantarctic Front system (SAF-N) in this region, while the SAF, PF, and SACCF correspond to the core frontal jets." in Lines 119-126.

167: Replace ":" with "."

Response: Thank you. The colon ":" in the original sentence has been replaced with a period ".".

217-219: Please clarify the percentage values mentioned in the text, as they are not directly reported in the table. As I understand it, they are obtained by summing the corresponding AE and CE percentages for that category and specific front, please state this explicitly.

Response: Thank you for highlighting this point. We have revised the paragraph thoroughly and avoided mentioning the percentages, which are explicitly provided in Table 1. By doing this, the revised version is more focused on the description and is logically smooth for easy understanding. The revised paragraph is in Lines 258-267 as: "Quantitative analysis of CFE types reveals distinct frontal-zone behaviors (Table 1). Transient eddies (Type 3) account for the largest proportion overall (> 40% by summing the transient AEs and CEs at each front), particularly at the two weaker southern fronts (SACCF and SB). Their proportions are lower at the SAF and PF, indicating that these two major fronts host more eddies that interact with areas outside the frontal zone during their lifecycle. For partially front-generated eddies (Type 1), both AEs and CEs exhibit relatively high proportions at the SAF and PF. At the southern SACCF and SB, however, the proportion of AEs drops markedly, whereas CEs show no such reduction. Among partially front-dissipated eddies (Type 2), the three northern fronts consistently show a higher proportion of CEs than AEs. At the two southern fronts, the pattern reverses, CE proportions decline sharply, leading to a higher proportion of AEs. This suggests that in the southern fronts, local cross-frontal CEs are more readily generated and propagated outward, while being relatively resistant to dissipation."

251: ", p=0.08)" replace with "(p=0.08)"

Response: Thank you. We have checked and corrected the parenthesis format for p-values at this location and elsewhere in the text to ensure consistency with the "(p=0.08)" format.

272-273: Consider replacing "Total number of CFEs, divided into types of equatorward and poleward directions;" with "Total number of CFEs, divided into types of equatorward and poleward directions, shown as a function of the different ACC fronts;"

Response: Thank you. Following your suggestion, the caption for Figure 2(b) has been revised to: "(b) Total number of CFEs, divided into types of equatorward and poleward directions, shown as a function of the different ACC fronts;"

379: Do the water-mass core values reported in Table 3 have circumpolar validity for the Southern Ocean? Please clarify the geographic representativeness of the cited values. Since this work focuses on the Pacific sector, you may also consider or discuss Pacific-sector estimates of SAMW and AAIW core properties (e.g., Bostock et al., 2013; Li et al., 2021, 2022).

Response: Thank you for this important comment. We fully agree that the core properties of water masses in the Southern Ocean, such as potential temperature, salinity, and density, are not circumpolarly uniform. Significant sub-regional and inter-basin variability exists across different frontal zones and sectors (Pacific, Atlantic, Indian).

The water mass criteria summarized in Table 3, particularly for Subantarctic Mode Water (SAMW) and Antarctic Intermediate Water (AAIW), are intended to be representative and primarily applicable to the Pacific sector. To clarify this point and enhance the geographical context of our analysis, we have made the following clarifications and additions to the manuscript: (1) A note has been added to Table 3 stating: " Note: The ranges listed, particularly for SAMW and AAIW, are primarily representative of the Pacific sector of the Southern Ocean.". (2) A description about the water masses has been added in Lines 387-394 for better stating the spatial patterns of core water masses as: “Core water masses, especially Subantarctic Mode Water (SAMW) and Antarctic Intermediate Water (AAIW), are not circumpolarly uniform but exhibit substantial regional variability (Bostock et al., 2013; Li et al., 2022). For instance, within the Pacific sector, the salinity minimum of AAIW ranges from ~34.2 in the southeast Pacific formation region to greater than 34.5 in the Tasman Sea after mixing (Bostock et al., 2013); Similarly, SAMW exhibits distinct spatial patterns in its formation and properties (Li et al., 2021). Accordingly, the ranges in Table 3 are intended as a practical guide for identifying water masses within the specific Pacific sectoral context of this study.” .

383: I suggest using a different colour bar for salinity rather than reusing the same colour bar as potential temperature, to avoid confusion and improve readability.

Response: Thank you for this suggestion. Figure 9 has been redrawn following your suggestion by using different color bars for potential temperature and salinity.

465-469: Please provide the DOI of the Copernicus Marine product used and report the date of access/download. Also, the Argo data link does not work, please check and update it.

Response: Thank you for your carefulness. Now, The DOI of the Copernicus Marine product and Argo data link have been updated. The date of access/download has also been added in the Data availability part.