This study used three months of high-resolution data from glider transects over the Antarctic Circumpolar Current's Southern Boundary to assess its variability in location and intensity in terms of lateral gradients and velocities. The observation indicates that a mesoscale cold-core eddy influences the Southern Boundary's frontal structure by disrupting the temperature transition zone at the subpolar limb, enforcing stronger density gradients across the front and affecting the frontal jet strength. The authors also showed that small mixing length scale and more pronounced PV gradients at the Southern Boundary were concurrent with the cold-core eddy, and the variability of its barrier/blender nature over a multidecadal timescale was discussed.

The presented observation is very attractive and seemingly provides novel findings about the controlling factors of the frontal structure and isopycnal fluxes in the vicinity of the Southern Boundary, the oceanic gateway to the Antarctic coast. The manuscript is well organized, the logic is clear, and the presentation meets necessary and sufficient. Therefore, I strongly support its publication in the journal.

Before publication, however, I have several recommendations and questions about the manuscript as follows:

<major point 1>

I first want to assure what is the frontal jet focused on this study is. Based on the Orsi's temperature criteria, the authors defined the location of SB, and subsequently the SB was re-defined based on the neighbouring ADT contour and its maximum ADT gradient. However, according to Sokolov and Rintoul (2009a) also cited in the manuscript, the corresponding frontal jet seems to be the Southern ACC Front at 56–57S (see the figure below).

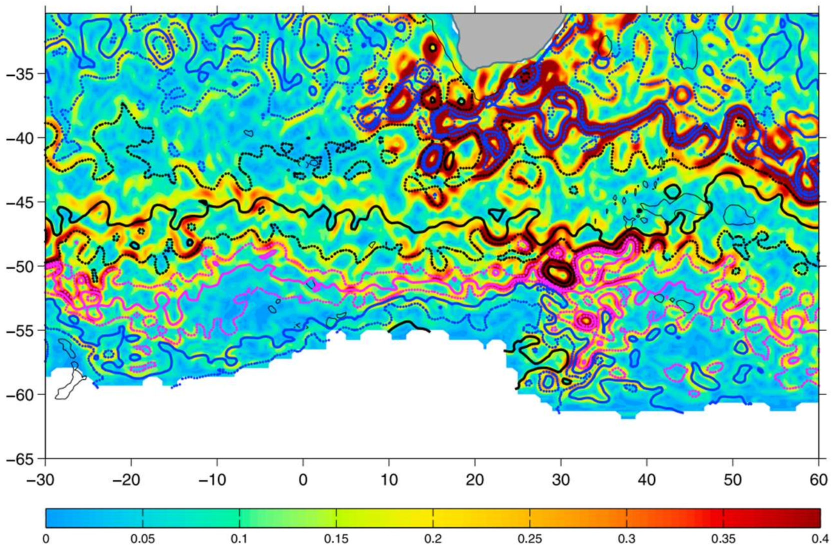
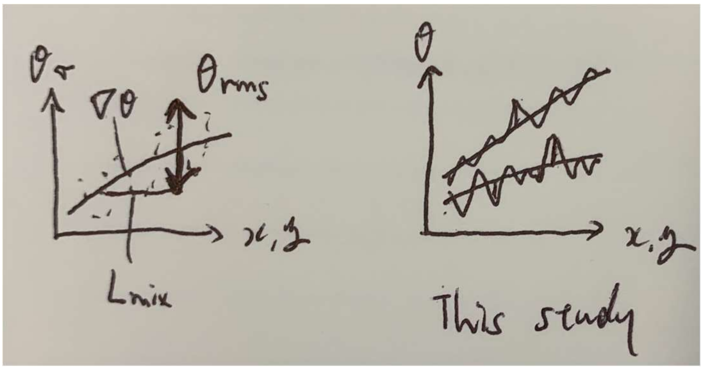


Figure 2. A typical SSH gradient field south of Africa (11 October 2000) overlaid with the synoptic position of the SSH contours associated with each front (the values of the SSH streamlines corresponding to the fronts are derived for the whole period of altimetry observations). The Southern Ocean fronts are color coded from south to north as follows: SB, black; SACCF (-S and -N), blue; PF (-S, -M, and -N), magenta; SAF (-S, -M, and -N), black; SAZ/STZ (-S, -M, and -N), blue. The middle branches of the fronts are shown by solid lines, while the northern and southern branches (where applicable) are show by dotted lines. In the case of the SACCF, the northern branch is shown by solid line and the southern branch is shown by dotted line. The 2000 m bathymetric contour is shown by thin black line.

Then, how can we call the frontal jet of interest? My recommendation is "to use the SACCF instead of SB". Originally, Orsi+1995 defined the SB as 1.5 degC at T-max based on a fact that the isotherm is well aligned with the poleward limit of oxygen-depleted layer, which is characteristic to UCDW in their dataset. Since UCDW conceptually configures the upper branch of the Southern Ocean MOC, it is natural to define UCDW as the oxygen-depleted layer. In other words, without showing the correspondence between the poleward limit of oxygen-depleted layer and the isotherm, it would be non-trivial to define the position of SB using temperature. Strictly speaking, isopycnal poleward migration of UCDW over decades can change the position of the T-max isotherm independent of the frontal shift and the positional relationship between isotherms and dynamical fronts (e.g., Yamazaki et al., 2021), so that the SB's definition introduced by Orsi+1995 based on the pre-1990's data may not be valid at present. Moreover, as mentioned by the authors, the SB is a water mass boundary and not necessarily accompanied with a frontal jet, whereas the SACCF is a dynamical front by its definition.

<major point 2>

I noticed the mixing length calculation shown in Figs 9 and 10 is substantially different from the convention (e.g., as performed in Naveira Garabato, 2011). In this study, the mean tracer gradient ($\nabla\Theta_m$) seems to be calculated from one temperature section smoothed with twice the baroclinic deformation radius horizontally and 0.08 kg/m-3 vertically, whereas it has conventionally been calculated from the averaged tracer field for repeated observations. As for the hydrographic variability (Θ_{rms}), although I could not fully understand the method, it seems like the difference between the original high-resolution section and the smoothed section in this study, whereas it is conventionally the standard deviation of tracer over the repeated observations (see schematic below; left: convention, right: this study). In this way, the difference in mixing length among the two sections can be discussed as in Figs 9 and 10.



This mixing length calculation and the "hydrography-based" mixing length change are new to me, so it would be very helpful if the authors can provide any reference that adopted the same/similar method. Otherwise, I think more explanation for its validity needs to be provided; for example, how many data points are required to quantify the mixing length over the horizontal scale of interest? Comparison to the mixing length calculated from the conventional scheme (in this study, $\nabla\Theta_m$ is calculated simply from the average of five transects, and Θ_{rms} is simply the standard deviation over the five transects) and their physical differences? Sensitivity to the choice of the horizontal/vertical smoothing scale?

Please note, the estimate in this study should be more informative than the conventional estimate in a sense that the estimate is expected to be purely affected by the mesoscale features.

<minor points>

L35: I assume the authors want to declare the definition of southern boundary in this study?

L93: “Internal” Rossby radius or “baroclinic deformation radius”? I recommend adding a reference (e.g., Chelton+ 1998, JPO) here as it is also critical to the mixing length calculation.

L110: LCDW should travel poleward beyond the southern boundary as it constitutes the lower MOC to merge with AABW.

L111: “28km” – add “spanning over”?

L118: Fig. 4 – I wonder that the surface drift (cyan) generally seems weaker than the DAC (magenta) despite of the eastward geostrophic shear above 1000m (Figs. 2 and 3). Can you explain why, and which estimate is more reliable?

L131: “south” – replace with “north”? Perhaps providing the horizontal scale of the bowl structure would help understanding.

L133: What is “the coincident changes”?

L143: “40 km” – the baroclinic deformation radius is 10-15km, then we can expect eddy's diameter of 20-30 km?

L145: I could see westward velocities characteristic to the eddy’s southern edge by the surface drift and the altimetric velocities, while they are unlikely visible in the DAC.

L148: “advected” – it might also be possible that the eddy was merged with a larger structure (probably, jet’s meander) to its west or east.

L150: Then, how sea-level depressions (white contours) larger than the cold eddy can be interpreted?

L161: Absolute salinity needs unit g/kg.

L169: Why the DAC is more appropriate as the reference than the surface drift?

L170: 80 cm/s – this far exceeds the altimetric speed and the surface drift.

L174: “the gradient of ADT (Fig. 8a,c)” – unit is m/m in Fig 8

L177: It also seems like the major front (SACCF-N) and the minor front (SACCF-S) regulate the barrier strength. Can you please provide any effects by jet’s meandering?

L184: “strengthens” – does this refers to inverse cascade dynamically?

L204: How the temperature fluctuation is calculated? (This would be why I could not fully understand the calculation)

L203: Strictly speaking, the cross-section (defined by glider positions), along-stream (defined by the streamline), and zonal components are all different. Please elaborate on it throughout the manuscript or demonstrate these differences do not change the result.

L219: “The PV is further considered along potential density surfaces with…” – Simply, “PV is calculated over”? Or, is this meant to be "potential density surfaces are considered to be isoneutral"?

L263: There is section 4.1 but following sections 4.2 etc. are absent.

L275: “The Southern Boundary’s location (determined from the frontal jet)” – I recommend to replace with the SACCF.

L287: “In summary” – meridional eddy heat flux may be given by $-k\nabla\Theta$, where k is isopycnal diffusivity associated with the mixing length. Then, how changes in $\nabla\Theta$ affect the meridional heat transport? Is it safely negligible even on account of the offshore warming?