Responses to Reviewers

Stirring across the Antarctic Circumpolar Current’s Southern Boundary at the Greenwich Meridian, Weddell Sea

We thank the reviewer for their helpful comments and suggestions that have strengthened our paper. In our responses below, the reviewers’ comments are in black, our responses are in blue and the revised text is in purple.

Reviewer 1 – Kaihe Yamazaki

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 redefined 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). 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.

We respectfully disagree with the reviewer suggesting that the frontal jet is associated with the Southern ACC Front rather than the Southern Boundary. Previous studies (Billany et al. 2010; Swart et al. 2010) focusing on the fronts of the ACC at the Greenwich Meridian identified the Southern Boundary around 55.5 °S (as in our study), whereas the Southern ACC Front was identified around 53°S. Please see Fig. 1,8 and Table 2 from Swart et al. 2010 for further clarification.

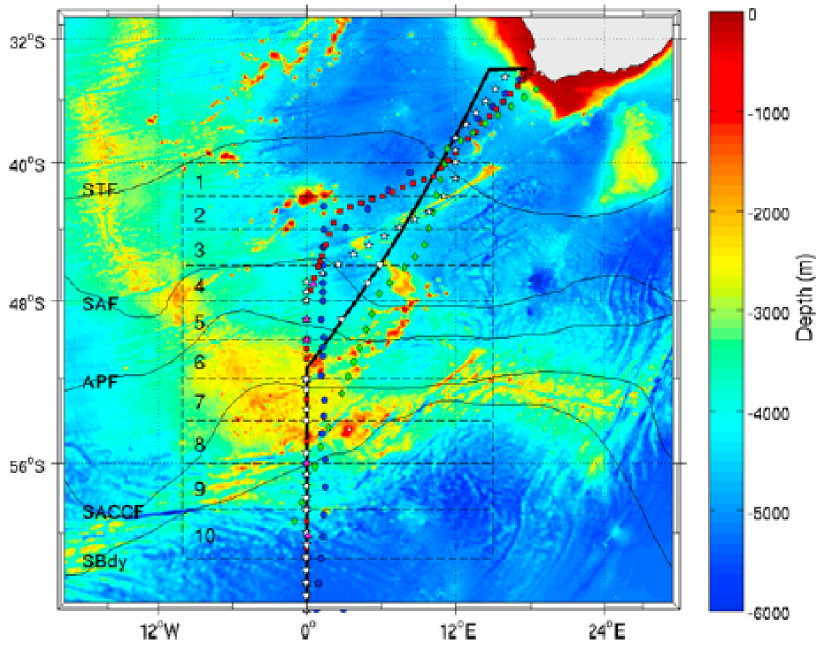


Figure 1. Locations of the eight CTD sections used in this study. The AJAX section (blue circles), A21 section (green diamonds), 1992 A12 section (red squares), 1999 A12 section (magenta triangles), 2000 A12 sections (white stars), and 2002 A12 section (white triangles). The solid black line represents the repeat cruise track of the GH CTD and XBT sections. Traces of the ACC fronts, by *Orsi et al.* [1995], and the bathymetry (in m) has been overlaid. STF, Subtropical Front; SAF, Subantarctic Front; APF, Antarctic Polar Front; SACCF, southern ACC front; SBdy, southern boundary of the ACC. The gridded boxes represent the latitudinal zones from which Argo float data were extracted to derive a seasonal model for the region.

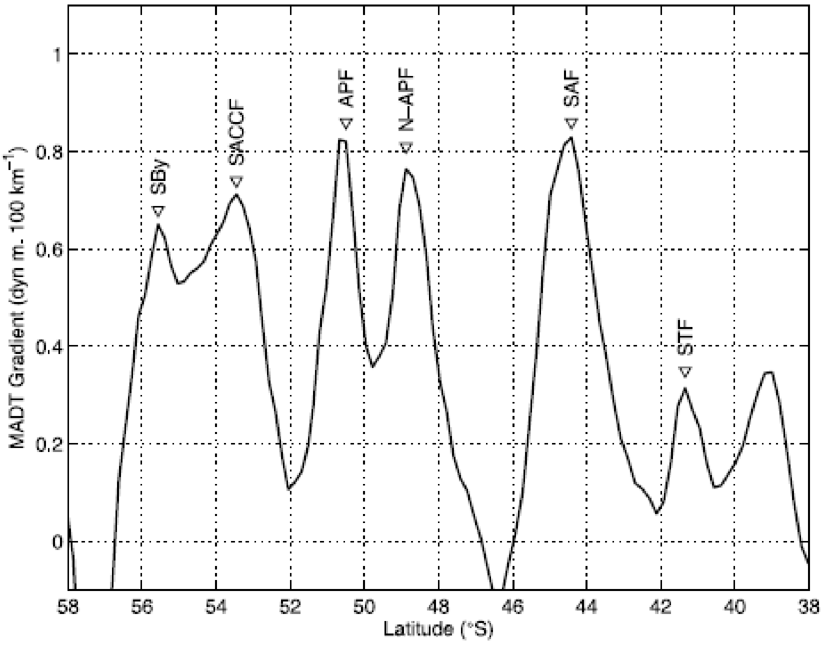


Figure 8. The mean MADT gradient (in dyn m 100 km^{-1}), at the GH line, marks the positions of the ACC fronts (marked and labeled).

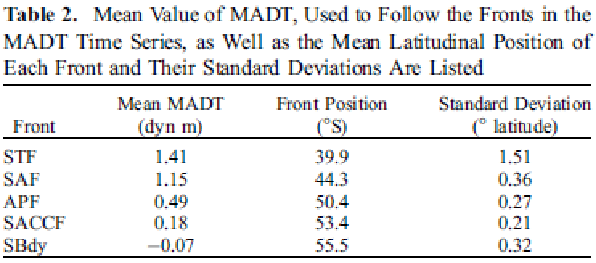


Table 2. Mean Value of MADT, Used to Follow the Fronts in the MADT Time Series, as Well as the Mean Latitudinal Position of Each Front and Their Standard Deviations Are Listed

Front	Mean MADT (dyn m)	Front Position (°S)	Standard Deviation (° latitude)
STF	1.41	39.9	1.51
SAF	1.15	44.3	0.36
APF	0.49	50.4	0.27
SACCF	0.18	53.4	0.21
SBdy	−0.07	55.5	0.32

Furthermore, Billany et al. (2010) reproduced the ACC front locations from Orsi et al. (1995) and identified the Southern Boundary at a location (around 55.5°S) that agrees with Swart et al (2010) and our study. See Table 1 from Billany et al. (2010) for further justification.

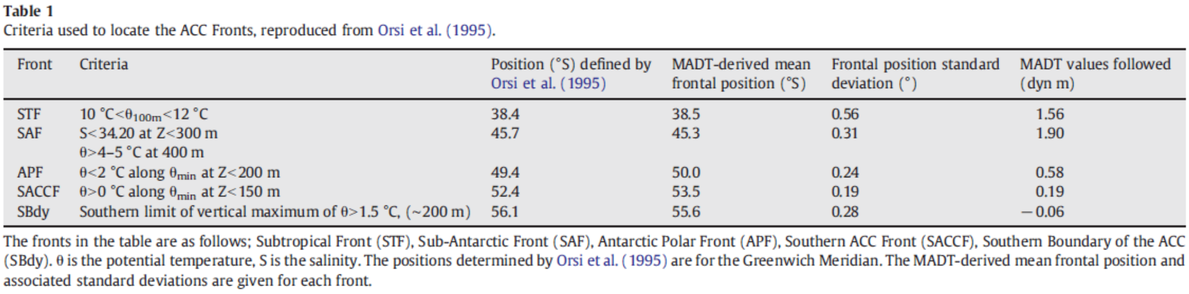


Table 1
Criteria used to locate the ACC Fronts, reproduced from Orsi et al. (1995).

Front	Criteria	Position (°S) defined by Orsi et al. (1995)	MADT-derived mean frontal position (°S)	Frontal position standard deviation (°)	MADT values followed (dyn m)
STF	$10\ °C<\theta_{100m}<12\ °C$	38.4	38.5	0.56	1.56
SAF	$S<34.20$ at $Z<300$ m $\theta>4$–$5\ °C$ at 400 m	45.7	45.3	0.31	1.90
APF	$\theta<2\ °C$ along θ_{min} at $Z<200$ m	49.4	50.0	0.24	0.58
SACCF	$\theta>0\ °C$ along θ_{min} at $Z<150$ m	52.4	53.5	0.19	0.19
SBdy	Southern limit of vertical maximum of $\theta>1.5\ °C$, (~200 m)	56.1	55.6	0.28	−0.06

The fronts in the table are as follows; Subtropical Front (STF), Sub-Antarctic Front (SAF), Antarctic Polar Front (APF), Southern ACC Front (SACCF), Southern Boundary of the ACC (SBdy). θ is the potential temperature, S is the salinity. The positions determined by Orsi et al. (1995) are for the Greenwich Meridian. The MADT-derived mean frontal position and associated standard deviations are given for each front.

The reviewer is correct that the Southern Boundary was originally defined as a water mass boundary. However, a more recent update of this definition clearly showed that the Southern Boundary is associated with the frontal jet at the Greenwich Meridian. Swart et al. (2010) projected hydrographic sections crossing the ACC onto baroclinic stream function space, which provides a two-dimensional gravest empirical mode (GEM). The GEM explained about 97% of the temperature and density variance within the ACC domain. GEM-produced velocities (Fig. 16 of Swar et al. (2010)) compared closely with observations and showed that the Southern Boundary is associated with a frontal jet at around 55.5°S.

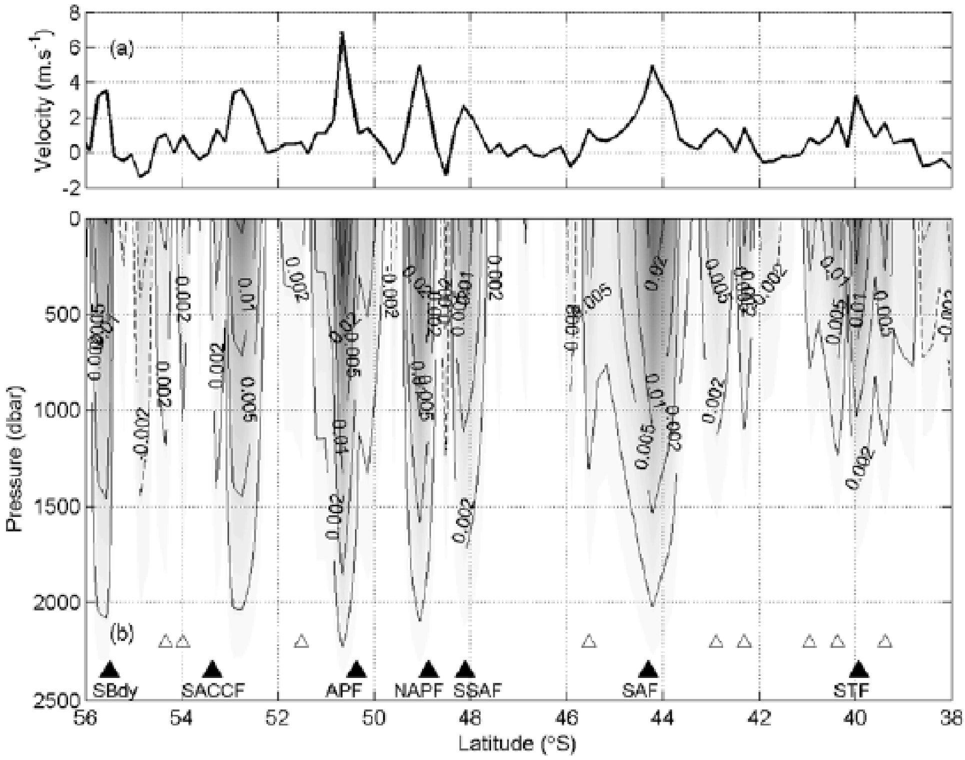
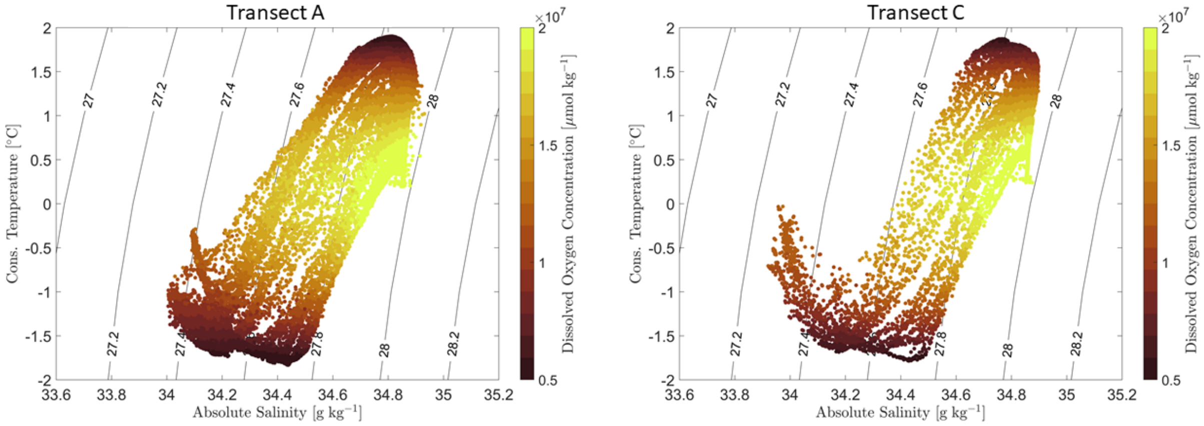


Figure 16. (a) The sum of the time-averaged (1992–2008) latitudinal distribution of the cross-sectional velocities (in m s^{-1}) at the GH line. (b) The vertical distribution of the cross-sectional velocities (in m s^{-1}) are depicted at the GH line. The large arrows show the mean positions of the major ACC fronts, identified by the ADT, while smaller arrows show those additional jet-like structures that are not clearly seen in the MADT velocities, presented in Figure 9.

The reviewer further argued that a characteristic of UCDW is the oxygen-depleted layer. Our glider data provide oxygen data (see examples for Transect A and C below) and show that oxygen is depleted within the UCDW layer.



All above stated findings provide justification that the transition in water mass properties and frontal jet that we discuss within this study, is associated with the Southern Boundary of the ACC.

Additional discussion of the above mentioned citations and definitions of the Southern Boundary have been added to the manuscript for further clarification.

<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 (∇Θ_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, ∇Θ_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 ???

The reviewer is correct that our method differs slightly from the method used by Naveira Garabato et al. (2011). It has to be mentioned here that their study used ship-based hydrographic sections rather than our closely-spaced glider sections. Our study is based on a method for glider data by Dove et al. (2023) & Viglione (PhD Thesis). We have added this reference to our study to further justify the method that we used for glider data. The aim here is to provide a ‘large scale’ temperature field by smoothing over twice the Rossby Radius. Thus, the Θ_m is not based on an average between the transects but rather a smoothed ‘large scale’ version of the high-resolution temperature data, where Θ_rms is the standard deviation between the ‘large scale’ and the high resolution temperature field. Furthermore, the mixing length scale contrast between Transect A and C are sufficiently larger (about 6 times) than the scale of the observations (5 km), indicating the capability of our highly-resolved sections to reveal the mixing length scale contracts between sections.

L35: I assume the authors want to declare the definition of southern boundary in this study?
Yes, the definition of the Southern Boundary is defined in L35 via water mass properties. We now added the additional discussion of the Southern Boundary associated with a frontal jet after Swart et al. (2010) as well as its location at the Greenwich Meridian (around 55.5°S). Please see response to major point 1 for further details.

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.

We refer to the Rossby Radius of deformation (baroclinic deformation radius). Suggested reference has been added.

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

LCDW is not detected south of the Southern Boundary in the observations in this study. The glider transects show that LCDW underneath UCDW north of the Southern Boundary, but not beyond the Southern Boundary. Therefore, mentioning the southward extent of LCDW across the Southern Boundary here would be speculation and has thus not been added.

L111: “28km” – add “spanning over”?
The suggestion has been added to L111.

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?

We thank the reviewer for spotting this error. There has been a typo in the Fig. caption. The cyan colors show the DAC and the magenta colors show the surface drift. This has been corrected in the manuscript.
The geostrophic velocities (Fig. 8) are surface intensified, which suggests that the surface drift should be larger than the DAC (which it is). The winds above the Southern Boundary usually have a west to east orientation, so would tend to further increase the surface drift, which additionally explains the difference between surface drift and DAC.
Therefore the surface drift is influenced by surface currents and winds, whereas the DAC contains information of the deeper water column (1000 m).

L131: “south” – replace with “north”? Perhaps providing the horizontal scale of the bowl structure would help understanding.
The ‘south’ in L131 refers to the location of the bowl structure, rather than the occurrence of warmer waters north of the Southern Boundary. Depending on the defined location of the Southern Boundary (southernmost limit of UCDW) the bowl structure would still be south of the Southern Boundary. This sentence has been edited to improve readability. Horizontal scale description (latitude) of the bowl structure has been added as well.

L133: What is “the coincident changes”?
The ‘coincident changes’ here refer to the characteristics, such as water mass properties and bowl structure south of the Southern Boundary that match for the transects (A, B, D and E) which do not necessarily match for transect C. We have adapted the sentence to clarify.

L143: “40 km” – the baroclinic deformation radius is 10-15km, then we can expect eddy's diameter of 20-30 km?
Yes, apologies for that. The eddies are about 20-30 km in diameter.

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.
We apologize for that. The westward velocities at the eddy’s southern edge are visible in the DAC too. We have adjusted the arrow size in Fig. 4 to increase visibility.

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.
Yes, absolutely. We have added your suggestion to L148.

L150: Then, how sea-level depressions (white contours) larger than the cold eddy can be interpreted?
We are not sure what the reviewer is referring to here? We are assuming that the reviewer is referring to Fig. 4. The other white contours here refer to other cold-core eddies interacting with the Southern Boundary/ SACCF. In Fig. 4 the white contours depict the transition zone from eddy core towards the outside of the eddy. We have added that explanation to the discussion in the text to clarify that.

L161: Absolute salinity needs unit g/kg.
Units have been added to absolute salinity.

L169: Why the DAC is more appropriate as the reference than the surface drift?
Please see the response to L118.

L170: 80 cm/s – this far exceeds the altimetric speed and the surface drift.
Yes, this is quite a common issue. With regards to the surface drift please see L118. Satellite altimetry- derived currents are necessarily temporally and spatially smoothed by the process of creating the gridded product from relatively widely-spaced altimetric tracks infrequently repeated. This may lead to eddies and front being in the correct location, but averaged/smoothed in e.g. current speed so that values from satellite altimetry tend to be smaller than observed current speeds. We have added the following lines to the text to emphasize that in more detail.

L174: “the gradient of ADT (Fig. 8a,c)” – unit is m/m in Fig 8
Yes, thanks for spotting that. Unit has been corrected.

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?
Between transect A and C the frontal jet of the Southern Boundary has shifted meridionally. The location of the Southern Boundary has been discussed in L104-L124. Additional discussion with respect to possible influences on the barrier strength due to the jets meandering has been added.

L184: “strengthens” – does this refers to inverse cascade dynamically?
We are just referring to the changing density gradients here. We have changed L184 to ‘amplifies’ to avoid confusion.

L204: How the temperature fluctuation is calculated? (This would be why I could not fully understand the calculation)
The temperature root mean square Θ_{rms} is calculated as the standard deviation of the temperature anomalies from the mean ‘large scale’ temperature field and the high resolution temperature field ($\Theta_m - \Theta$). This has been added to the method description.

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.
First the glider locations are projected onto a meridional line and then we calculate the geostrophic shear. Thus, we only calculate the zonal velocity component. Furthermore, we find in the key transects (A and C) the flow at the Southern Boundary and over the associated frontal jet is zonal.

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"?
According to the reviewer’s suggestion the sentence has been changed to:
The PV is calculated over potential density surfaces with …

L263: There is section 4.1 but following sections 4.2 etc. are absent.
We thank the reviewer for spotting this error. Section 4 is now divided into section 4.1 and section 4.2.

L275: “The Southern Boundary’s location (determined from the frontal jet)” – I recommend
to replace with the SACCF.
Please see response to major comment 1.

L287: “In summary” – meridional eddy heat flux may be given by -k∇Θ, where k is isopycnal diffusivity associated with the mixing length. Then, how changes in ∇Θ affect the
meridional heat transport? Is it safely negligible even on account of the offshore warming?
This is a really good question that we will give careful thought, when we prepare the revised manuscript.
(The argument is here based on the gradients across the front, which include temperature gradients. Studies (Shi et al (2021)) have shown that due to upper ocean warming the gradients across the main ACC fronts are amplified. In cases

where barrier properties are enhanced (due to amplified gradients) the diffusivities across are near 0, thus the eddy heat transport at least has the potential to become very small).