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Reply to Comments from Anonymous Referee # 2

We thank the reviewer for careful reading and constructive comments. In this reply, we will list our main responses, point-by-point. The detailed reply will accompany the revised manuscript. Your comments are in *Italic* and our replies are in Roman fonts.

> *The results are a bit spread between different features: surface fronts, deep layers of the Enderby abyssal plain with a diffusive model; changes in time of deep and bottom water masses in this sector of Antarctica, which is more rarely visited than others or very close to Antarctica.*

This point has also been raised by Referee # 1. In response, one of the topics ("decadal changes in water mass properties") is moved to Appendix. We also added subsections to Introduction and added subsection "Organisation of the paper" where we explain that the three topics explored in this paper do not have strong inter-connection and the reader may choose to pick up the sections of interest and skip others. As a result of this notes, we feel it better to briefly mention the summary of findings in each topic.

> *There is actually a repeated station to the northeast of the sector at 56.5S/63°E (OISO station 11). I am not exactly sure of its bottom depth, but over 4900 db, which bottle data are regularly placed in the GLODAP archive.*
>
> *The paper's reference is*
>
> Ocean Sci., 16, 1559–1576, 2020,`https://doi.org/10.5194/os-16-1559-2020` "Variability and stability of anthropogenic CO2 in Antarctic Bottom Water observed in the Indian sector of the Southern Ocean, 1978–2018", Léo Mahieu, Claire Lo Monaco, Nicolas Metzl, Jonathan Fin, and Claude Mignon.
>
> *This may not interest as much the authors, as it is mostly T, S, O2, DIC, TA, and sometimes, NO3, Silicates (rarely PO4). On the other hand, it could be nice to check some of the trends mentioned in the last section.*

When we search the GLODAP database, we had a criteria that the station location be within 20 km from our stations such that we do not worry about the variability caused by the difference in space and focus in the variability caused by the temporal difference. The OISO station 11 did not meet the criterion. This criterion is now added in Appendix A.

In the same biogeochemical community, there is an other paper by:

Zhang, S., Wu, Y., Cai, W.-J., Cai, W., Feely, R. A., Wang, Z., et al. (2023). Transport of anthropogenic carbon from the Antarctic shelf to deep Southern Ocean triggers acidification. Global Biogeochemical Cycles, 37, e2023GB007921. `https://doi.org/10.1029/2023GB007921`

We have looked up the reference but could not find a straight-forward link to our manuscript.

I am a little bit wondering of the interest of the frontal description and the two plots on figure 5 (I believe that one is enough), but as it is not the core of the paper, I dont mind that it is discussed in that part..

The reason why we have two plots on Fig.5 is to show the daily movements of the eddies.

When considering the diffusive model, as well as for the water mass composition of the bottom water, the set of constrains is not that large (as clearly some of the variables used are very cross-correlated, as discussed in the appendix). Thus, the choice is made not to take into account the two AABW water masses that originate from further east, as the authors argue that this water does not make it as far west as this section, as it seems mostly flow as an eastern bonderay current northwards in the eastern Enderby Basin. This prompts my comment: If the diffusive hypothesis is relevant, shouldn't it also include diffusion from the eastern boundary (with the water probably having slight different properties). How are you sure that this does not happen? I am just concerned of the limits of the diffusive interpretation and water mass origin made in the paper (not so sure that it would change much, in fine!). Of course, I am aware that you only have a meridional section, and thus not the zonal variability component within the Enderby basin (the other two sections are further away which makes sense!)

We admit that the diffusive effects from the eastern boundary current along the Kerguelen Plateau is beyond the capability of our simplistic model. A remark is added.

Minor comments:

My other comments are mostly on some details that could be improved or on which I had minor questions.

Fronts on map 1 a bit strange east of Kerguelen-McDonald plateau and Fawn Trough (mostly SACCF not getting in right place?), but

> *no importance for the topic and front names could be overlaid on contours for example in the west of the map (where they are all well separated). Hard to see little crosses, circles, that are small (and at 30°E overlaid on longitude line)*

Our definition of SACCF follows that of Orsi et al. (1995, see their Fig.7). The front names are now printed along the contours except for SAF and SACCF (found no spece for these two). The size of the marks (crosses, circles) is a compromise; if too large, the stations overlap. The 30°E longitude line is removed.

> *42, evidence for eddy activity at 1000 dbar (reference on the product not reported in the text, but in the figure 2 caption, where it seems to 1°x1° mapped Scripps Argo drift data (Katsumata, 2017); would the ANDRO (French) product show the same features?). At first hand, I was surprised that, on figure 2, EKE seems larger at 1000m (but that might be some filtering in the altimetry data). Contours on figure 2 hard to visualize (the 200m and 5000 m contours should be done with different colours)*

We did not try plotting the same figure from ANDRO (`ftp://ftp.ifremer.fr/ifremer/argo/etc/coriolis-custom/argo-andro-data/`), but ther document (Ollitrault and Rannou, 2013, `https://doi.org/10.1175/JTECH-D-12-00073.1`) convinces us that the plot will be similar.
As the title of the figure shows, the EKE at 1000 m is multiplied by 10. We added this to the caption. The contours are now in different colours (black and brown).

> *76 Figure 4 seems to be cited before figure 3 (l. 81). I have also some difficulties seeing the colour curves (too thin) on the lower panel of figure 4 (also, the colour on top panel)/*

The order of Figs. 3 and 4 are changed. The color on (new) Fig.3 are changed for better visibility.

> *78: E instead of S for two latitudes.*

Corrected.

> *Figue 3: in the sections, it seems that there is no station to the bottom near 4000 km. This could be be mentioned in describing the data (as this is one area, where the horizontal resolution indicated is not reached, except in the top 2000 db (XCTDs?))*

Description of XCTDs were missing in the original version. It is now added.

> *Figure 6: gradient reported at $\frac{1}{4}$ degree grid, but results from some spatial smoothing in WOD2023 (typically, on the order of 3°). The maximum gradient reported on line 96 at this location might be due to the stationarity of the front at this longitude. I don't think that the 'instantaneous' gradient is weaker, for example, at locations further east in the Indian Ocean.*

Agreed. A sentence is added to point this out.

> *l.97: 'sality' should be 'salty'*

Corrected.

> *105: 'mesoscale structures at 3000 dbar depth'. I am not sure what is exactly refered to. It is not that clear on Q and S sections (at least to the naked eye). What there is is in O2 some strong spatial variability in this region (and depth). Is there some indication from the current measurements of mesoscale structures at this depth and location. I am not so sure that this is indicative of vigorous isopycnal mixing. Or, at least, what the reasoning for that should be explained.*

The 'mesoscale structures at 3000 dbar depth' were not found in temperature nor in salinity – probably because background gradients are weak in temperature and salinity compared to that of dissolved oxygen. The horizontal currents measured by LADCP showed a turbulent flow similar to those shown in Figure 7. We have added this comment.

> *Figure 7 shows currents integrated over neutral density range 27.9 to 28.27. Unfortunately, figure 3 does not show 27.9 (it starts contours at 28.0). Where is this neutral surface located (or could we instead show currents in 28.0-28.27 ofr LCDW layer?). On this figure does one have an idea of the uncertainty in the velocity profile reconstruction. In particular I was a little puzzled by the strong northward velocity component in LCDW for the northern stations in the basin part of the section. Surprising in the two ellipses presented, it seems that the residual average current is exactly zonal. I find that really surprising, and wondered whether the meridional component is not plotted. I am also not sure on how to read the scale of the vectors presented on the figure. I would help to have an arrow below the plot with its velocity value to report this information.*

The top density contour on (new) Fig.4a is changed from 28.0 to 27.9. The accuracy of LADCP horizontal velocity is difficult to estimate (see, e.g. Polzin, K. L., E. Kunze, J. Hummon, and E. Firing (2002), The finescale response of lowered ADCP velocity profilers, J. Atmos. Oceanic Technol., 19, 205–224). We added, at least, a description of good LADCP data quality. Hints for "the strong northward velocity component in LCDW for the northern stations in the basin part of section" can be found in right panel of Fig.5, where an cyclonic (therefore clockwise in the southern hemisphere) eddy is found around (58°E, 44°S). The strong northward velocity follows this height contour, thus geostrophic. The averaged flow is not exactly zonal (meridional transport of 0.78 $\mathrm{m^2s^{-1}}$ compared to the zontal transport of 14.24 $\mathrm{m^2s^{-1}}$ for LACDW. The values are -27.72 $\mathrm{m^2s^{-1}}$ and $-1.07\mathrm{m^2s^{-1}}$, respectively, for AABW). The meridional component of the transport almost evenly between northward (positive) and southward (negative) transports such that the average is very small. The quantity plotted on the figure is transport (velocity times thickness thus in $\mathrm{m^2s^{-1}}$), not velocity ($\mathrm{ms^{-1}}$). The transport for the vector in the ellipse is found below the ellipse. We made the label larger.

> *113: is the range 10^{-5} to 10^{-4} m^2s^{-1} the overall range for all stations, and all depths, or has there been some smoothing. It would be interesting to see its average profile (with quantiles (maybe 20 and 80%) added to see how significant is the near bottom enhancement.)*

The overall range is wider – it is 10^{-6} to $10^{-3.5}$ $\mathrm{m^2s^{-1}}$. The diffusivity has been estimated by the internal wave parameterisation such that some smoothing is inevitable (roughly the size of the vertical binning of the spectrum estimation, i.e. 320m in depth). The average profile is found in Figure 4 and the bottom enhancement is clear in Figure 5, respectively, of the original reference, Sasaki et al. (2024) `https://doi.org/10.1029/2023JC019847`.

> *l.115: Isopycnal diffusivity estimated from vertical diffusivity? (and/or tracer distribution).*

The equation (1) shows that the isopycnal diffusivity (K) and vertical diffusivity (D) are needed. The tracer distribution (c) can determine only one of them. We know D from a method independent of the tracer distribution so that c can be used to estimate K.

> *l.120-125: here D dependency with depth commented earlier is neglected. This could have some impact on the distribution of tracers and their evolution (as well as one the interior vertical velocity), but maybe it would be a small effect. Can the authors quantify it? Later, I got puzzled as on line 166, mention is made of the spatial variability in diffusivity attributed to figure 8c (but not found on it?)*

It was not possible to estimate the effect of depth dependency of D from this method, as it is treated as constant. If we have more data (say, distribution of independent tracers such as Helium isotope), we might be able to include the effect of the spatial variability of D) but for this simplistic model discussed here, D is assumed constant. The variability of D is thus additional uncertainty not included in our estimate of K. We have added a remark.

> *Figure 8: different convention on distance than in earlier figures (with 0 at southern boundaries). This is no problem for me, but maybe some readers might be a bit surprised.*

This is why we used a different label (Y here, while X was used in Fig.4 $\rightarrow$ Fig.3).

> *Then description of the mechanistic diffusive model. I am a little skeptical, as with the two sources they prescribe, it seems to me that there are too many parameters, and many approximations (such as dilution over shelf when waters formed, with 50% sounding a bit high, even with that specified there are 5 unknowns to specify). On the other hand, diffusivity values are reasonable, so are the a values.*

We admit that the model might be an over-simplification of the real ocean, but we also found it interesting that such a simple model could produce a reasonable value and decided to report it.

> *After, watermass study for gamma > 28.27. Among equations, they have PO*, NO* and even SO*, in addition to T and S (I am wondering how independent are the different constraints; actually this is presented in Figure A1, and indeed they are highly inter-related and linear with T). Analysis based on Johnson (2008).*

> *On figure 9 caption, mention of 107S, but it does not seem that the plot was retained.*

The data from I07S is found as black dots on both plots.

> *Fig. 9: the left panel is hard to follow with the overlaid data from R/V Hakuho cruises and from the 107S 2019/2020 stations. The two LCDW waters specified on plot are mentioned on line 218. What sets the choice of these two values?*

They are the two extreme (warm & salty vs cold & fresh) stations from I08S cruise in 2008.

> *224: '... only at 70°E and not at 60°E'.*

Corrected as suggested.

> *In table 3 caption, mention changes relative to what.... As is it is not clear what is presented (appears in the text of section 5, but should also appear in the caption).*

The caption to Table 3 is modified to add this information (now Table A1).

> *am also not sure why the change in temperature in LCDW/AABW is not presented on the table (I realize that some of the reported changes (stations 115 and 117) are taken on an isopycnal (which should also be mentioned in the table; this is somewhat different than for the other stations)).*

The caption to Table 3 (new Table A1) is now includes "on isopycnals" to indicate this. See also added new paragraph well as Eq.(A1).

> *291, Figure 14 also suggests some SSH increase further south. I guess that the comment on southward motion refers to the dipole in trend between 42°S and 45°S. However, overall, I appreciate the discussion of trends.* This discussion is now found in the Appendix.