Citation: https://doi.org/10.5194/egusphere-2024-2237-RC1

Reply to Review of the MS "Hydrographic section along 55°E in the Indian and Southern oceans" by Katsumata *et al*

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.

> *I have three main criticisms, which I believe the authors can solve. One is about salinity changes. Salinity measurements need further corrections when comparing observations from different cruises (see the series of work of Purkey and Johnson), which are particularly critical for the deep/abyssal ocean. I am unsure if the results of salinity changes described in the present MS are robust as they are in opposition with Choi et al. (2022) (see my point below).*

The authors thank the reviewer for bringing up this important matter. Indeed, consideration of the possible offsets in Standard Seawater batches increased the uncertainty significantly and we are no longer certain about the change in salinity that we observed. Those conclusions derived from the salinity changes are now deleted. With consideration for the third point below, the topic of decadal changes seems no longer appropriate in the main text and we have moved the section to Appendix after shortening it.

> *Second, the quality of the LADCP observations has never been mentioned or weighted in some discussions about the deep circulation in the paper.*

This point will be addressed in the revision (see below).

> *The third point is the lack of links between sections. It seems the authors have written different pieces and put them together as MS. A better link between sections would have benefitted the MS. I suggest the authors expand the conclusion/discussion to link the sections and bring some conclusions that move forward the understanding of the deep ocean in the Southwestern Indian Ocean portion of the Southern Ocean.*

In response to this comment, we have added a paragraph in Introduction, which now have subsections to clarify its structure 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.

Line-by-line comments:

> *In the main text, sometimes it is typed "Figure x," and sometimes "Fig. x." Choose one and use it throughout the text.*

We have found out that "Fig." is the correct form (`https://www.ocean-science.net/submission.html#figurestables`) and changed "Figure"'s accordingly.

> *Table 1 is not cited in the main text. Consider citing it in "section 2. Data" I guess the table lists the cruises analyzed in the present work.*

A sentence is added to refer to Table 1.

> *Table 2: The potential temperature units are missing. Units could be added in the caption or table interior.*

The unit (°C) is added in the caption (rather than in the table in order to compress the horizontal size of the table.).

> *Figure 4: The fronts cited in the text (L#75-80) could be added to the figure to make it easier to identify. Consider using a horizontal axis with latitude, which also would help with interpretation. We never know where stations start counting on a cruise, whether at the southmost or northmost point. Is this from I07S? It would be nice to add to the caption. I suggest changing the vertical black lines for something less overwhelming, such as grey.*

(Fig.4 → new Figure 3) The fronts are added in the upper and lower panels. Latitude ticks are added. The station numbering is now remarked in the Data section. The vertical grid lines are now thinnest grey.

> *L#81: Fig. 3 appears after Figure 4, which is confusing. Consider order figures sequentially as they appear in the text.*

Figs. 3 and 4 are swapped.

> *Figure 3: Consider reducing the amount of vertical black lines that make it harder to identify features (it will be even harder when formatted to the published paper). Also, I didn't get the spacing of tick markers between major ticks at the bottom axis. For clarity, consider removing the minor tick markers. The vertical axis is pressure, not depth, as described in the caption. I couldn't find the triangular shapes mentioned in the caption. Are they plotted? In Section 2, it is not mentioned that I07S and I07N data have been merged (or in the Figure 1 map). Have the salinity and dissolved oxygen of both cruises been cross-calibrated (particularly in the deep ocean)?*

The CTD and XCTD traces are all removed and substituted by tick marks on the upper horizontal axis. The tickmarks and labels for latitudes are now in brown

while the tickmarks for distance remain in black. The caption is rewritten to refer to pressure rather than depth. The triangular shapes appear in our PDF renderer but with the hope that they do not appear in the renderers of the readers, the remark on this triangular shapes were removed. The data for I07N and I07S are just plotted side by side and not used for quantitative analyses, thus not cross-calibrated.

> *L#83: XCTD data is not mentioned in Section 2. Please add.*

A paragraph is added to mention XCTD.

> *L#85: The terminology is not adequate. Both are real fronts, but one is associated with a transient feature (mesoscale eddy) and the other with a permanent feature of the ocean circulation.*

Agreed. Changed to "ACC fronts".

> *L#97-99: The argument is unclear. How do the salty waters transported by the Agulhas Current/Agulhas Return Current amplify P-E meridional gradients? It can enhance the haline gradients but not P-E. Or are you arguing that there is some coupling with the atmosphere and salinity would increase or decrease P or E?*

Changed to "the meridional haline gradient imprinted by precipitation minus evaporation".

> *L#103-104: I suggest breaking it into two statements, as there are two distinct pieces of information that are hard to understand in the current grammar structure. One describes the features encountered in the section, and the other is the vigorous isopycnal mixing.*

Rewritten into multiple sentences as suggested.

> *L#108: If the sections are instantaneous snapshots, how would the LADCP show the mean flow in a region dominated by mesoscale eddies (previously shown)?*

The adjective "mean" does not indicate temporal averaging but designates spatial averaging, i.e., large-scale background flow. Changed "mean" to "large-scale background".

> *L#109: What do you mean by "rich eddies"? Strong eddies? Please re-write for clarity.*

Changed "rich" to "numerous".

> *L#110: I am intrigued by how a snapshot could capture a "mean transport." It is mentioned in the text the LADCP "could not capture any mean transport"... I guess the text is trying to say that there is no coherent large-scale pattern in the LADCP data, and the deep circulation is dominated by mesoscale, which is an interesting*

result. Please consider re-writing this part. Question: how is the LADCP data quality in the deep ocean? With fewer scatterers in the deep ocean, LADCP-based velocity profiles are sometimes not of good quality. No info about the LADCP data quality in I07S is given in Section 2

As suggested by "no coherent large-scale pattern" above, we paraphrased "mean flow" to "large-scale background flow". We had two (upward looking and downward looking) sensors and both returned good signals even in the deep ocean. The data quality is now explicitly remarked.

L#115-189: Section 4.1 Isopycnal diffusivity estimated from transient tracer distributions. How are the diffusivity estimates calculated based on the CFC-12/SF6 tracer distribution related to the estimations based on the fine-scale parametrization calculated in section 4? It is unclear to the readers what the aim of obtaining both estimates is. How does the diffusivity link with the rest of the study?

The fine-scale parameterisation gives only **diapycnal** diffusivities, while the CFC-12/SF6 tracer gives only **isopycnal** diffusivities. These two quantities are independent in general and one does not necessarily infer the other, thus we believe it is meaningful to give both two estimates.

Figure 8 b/e and Figure 10: pressure, not depth, as described in the respective captions

The captions for Figs. 8 and 10 are corrected.

L#179: It is unclear what the text meant by "is not unlike the deep-sea value." Please re-phrase for clarity. Is this high diffusivity near any specific bottom topography? Or is it associated with high-bottom roughness?

We meant that the value was typical for deep seas. This isopycnal diffusivity is not particularly high.

L#190-250: Section 4.2 AABW composition. How does this decomposition relate to the Lagrangian simulations of Solodoch et al. (2022)? Are they consistent?

Solodoch, A., Stewart, A. L., Hogg, A. M., Morrison, A. K., Kiss, A. E., Thompson, A. F., et al. (2022). How does Antarctic bottom water cross the Southern Ocean? Geophysical Research Letters, 49(7), e2021GL097211. `https://doi.org/10.1029/2021GL097211`

Solodoch et al. (2022) assumed their artificial tracers at the surface of the shelf regions. On the other hand, our source used in Section 4.2 were sampled near the bottom. Since the dillusion of the surface water at the bottom is not known, it is difficult to compare quantitatively the results from Solodoch et

al. (2022) with out results. Qualitatively, the results do not disagree – for example, the near-bottom water in the Enderby Basin consists of the Weddell and Prydz tracers and negligible contribution from the Ross and Adelie tracers (their Fig.1); vigorous mixing is confined south of 60°S (their Fig.2) and mixing is moderate to weak further north. Our assumption in the mixing analysis that Adelie and Ross waters do not contribute to the Enderby AABW is also consistent with their results.

> *Figures 9, 12, and 13: units for conservative temperature and absolute salinity are missing*

The units are added to Fig 9, 12, and 13.

> *L#252-254: In this section, salinity changes are shown. However, there is no description of salinity corrections applied to the different cruises in Section 2 (e.g., correction for different standard seawater, the ad-hoc correction from Purkey and Johnson, etc.). This is critical when comparing temporal salinity changes in the deep ocean from measurements taken decades apart.*

Agreeing that the reviewer made an important point, we have applied the Standard Sea Water batch correction, where possible. For *Marion Dufresne* cruises, the batch correction was not possible and we had to include the possible offsets into our uncetainty. Given this increased uncertainty, we are no longer confident with the salinity changes and rewritten the discussion and conclusion. The section seems no longer tenable in the main text and thus moved to Appendix.

> *Table 3: Units for conservative temperature and absolute salinity are missing. Since the initial times are distinct for the different stations, it would be much better to express changes by rates (property change per decade), highlighting stations with great changes. The table info confuses the reader as it shows changes in conservative temperature only for the UCDW and salinity only for the AABW/LCDW layer. I only realized that in my third reading. I suggest adding both temperature and salinity for both layers. Another confusing point is the comments. It seems that these annotations are for the authors, not something directly connected with the text. At least, this was my impression. I suggest deleting the column comments or improving the writing there.*

(Table 3 → Table A1) The units are added in the caption. The decadal rates are added for temperature. Since we compare temperature and salinity at the same density, the changes in temperature can be easily calculated from the changes in salinity and vice versa. We have added the explanation. The comment column has been removed as suggested.

> *L#261-265: A possible freshening in the AABW/LCDW layer is discussed here. This freshening is in contrast with salinification,*

as pointed out by Choi et al. (2022), which also uses hydrographic observations, but the I06S and a few other cruises. However, the present MS does not mention Choi et al. (2022). Why are the results so contrasting? Would it be due to differences in methodology to calculate changes? Would there be a (likely) lack of corrections for salinity measurements in the present work? Would it be ocean dynamics? The fact is that the changes led to distinct conclusions in the two papers.

Choi, Y., & Nam, S. H. (2022). East-west contrasting changes in southern Indian Ocean Antarctic Bottom Water salinity over three decades. Scientific Reports, 12(1), 12175. https://doi.org/10.1038/s41598-022-16331-y

As mentioned above, we have removed out discussion regarding the salinity changes as a result of the introduction of the SSW batch correction.