nature portfolio

Peer Review File

Shoaling of abyssal ventilation in the Eastern Indian Sector of the Southern Ocean

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

1st Sep 21

Dear Dr Shimada,

Your manuscript titled "Rerouting of Abyssal Ventilation in the Indian Sector of the Southern Ocean" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

Specifically, for publication in Communications Earth & Environment to be appropriate, we will need you to restructure the manuscript to improve its readability. In addition, we encourage you to clarify the main findings in terms of changes in water supply and formation rates, taking into account the reviewer comments.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Regina Rodrigues, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-8010-4018

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-

specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of the MS
Rerouting of Abyssal Ventilation in the Indian Sector of the Southern Ocean by Shimada et al.

The present work investigates changes in water mass properties in the deep layers of the southeastern Indian Ocean portion of the Southern Ocean, its impacts on the sea level rise, and possible drivers. The study is based on hydrographic in situ observations from GO-SHIP/WOCE sections and Japanese occupations. While the freshening and warming of the bottom waters of the southeastern Indian Ocean are already known from other studies in the literature, the present work gives a step forward in trying to understand their causes. The authors removed the possible effects of the shifting of the Antarctic Circumpolar Current (ACC), and the temperature/salinity changes were persistent. Thus, these changes seem independent from an ACC shift. From that point, the MS results are not entirely clear, as I will describe in more detail below. The conclusions are based on speculations that were not straightforward to follow (I have some suggestions to make reading easier by modifying the way the MS is structured). Said that I am sympathetic that it is tough to use a quantitative approach in studying the Antarctic Bottom Water (AABW) variability, given the few observations we have and the many unknowns. We do not know the AABW formation rate in detail (for all formation sites), the abyssal pathways (our view is only pictorial), the air-sea-ice interaction variability triggering AABW formation, to cite a few unknowns.

I believe the MS has a potential for publication as there is some novel results but a revision is required prior publication. The figures are overall good and clear.

A first suggestion is about the title. After reading the MS, I am unsure what ‘rerouting’ means in this context, as the word was not used or defined anywhere in the MS. Second, this MS is only about the Eastern Indian Ocean sector of the Southern Ocean. The western part is not discussed in the MS. The

western Southern Ocean is supplied by Weddell Sea bottom water, and according to Purkey and Johnson papers, the bottom in the southwestern is cooling and not warming.

The main criticism I have is about the way the MS is structured. In several parts of the MS, the reader is pointed for a supplementary section, sometimes with information crucial to understanding the core of the work. Arriving at the supplementary section, the reader is sent (again) to other supplementary sections/figures/tables (which sometimes can lead to the other supplementary sections/figures/tables). I had to open three different computers at the same time to be able to follow this maze. I guess other readers will have the same problem, making it hard to focus on the science part. I understand that supplementary sections/figures/tables may be a way to give more information than the maximum words allowed by the journal. However, the authors need to put some effort into improving the readability of this MS. My suggestion is that the authors reduce the number of supplementary sections by putting together all information in fewer sections, so they do not point out to any other supplementary section. Also, it would be much better if supplementary figures and tables that belong to these supplementary sections were embedded in the section and not as separate entities (The authors need to consult with the editors if this would be possible). My suggestion is to have self-contained supplementary sections.

Another criticism is that the most important result and conclusion of the study are not entirely clear, even in the abstract. After my first reading, I couldn't pinpoint the central message of this study. From the abstract, we could infer that a 'weakened AABW supply' in L#30 is likely the main result. However, the word 'supply' is ambiguous (the authors will only be defined it later on). For instance, we could easily understand it as a weakening in the AABW formation rate. But that is not what the results suggest, as no formation rate was measured or inferred. Thus, 'supply' is not the most appropriate word for describing the study's main result. I would suggest the authors find another word that captures the main result and avoids misunderstandings. Maybe something similar to L#62-65 (but without the other 'supply').

The authors introduced a terminology in the MS, ‘intermediate density layer’ (L#115). Sincerely, I found it confusing as the ‘intermediate density layer’ gives an idea that the layer is much shallower in the water column when it is indeed a deep density layer (even the bottom layer in the South Australian Basin). Independent of that, I could not quite follow the arguments from L#130-166, which I believe is the core of the present work. I would suggest the authors draw a schematic diagram to explain the mechanism they are suggesting (this would help visualize it). I also think a model simulation may be a good addition here (but not required). For instance, two runs: one a control and another with changes in DSW. Would that alter the type of AABW formed in the model?

After reading the MS several times, I am still unsure if the authors are arguing that the rate of AABW formation has not changed, but what is happening is that a light version of AABW is being formed due to changes in source region properties. With that, a light AABW would reach the SAB much easier, crossing the Southeast Indian Ridge. If this lighter layer is being ventilated (by contact with the atmosphere in the formation region), then we would expect to see that in CFC/SF6 measurements, much is a better tracer than AOU for ventilation. I know that in GO-SHIP, these variables are collected. Could the authors say something about the CFC/SF6 signal in the I09S 2012 (SAB) in relation to 2005 and 1995? Is there increasing in CFC/SF6? Another suggestion is the authors take a look at the Labrador Sea Water literature. In that case, there is the Classical LSW and the upper LSW, which is lighter because less saline. Perhaps, there is some similarity here in which we have different classes of AABW and their prevalence change in decadal-to-long time scales (huge

speculation here). The authors could denominate the IDL layer as upper AABW instead of 'intermediate density layer'.

As pointed by Purkey and Johnson's studies, salinity needs to be correct for the different batches of standard seawater. In some WOCE/GO-SHIP lines, this correction can be of the same order as the detectable salinity changes. I could not find anything about this kind of correction on the data description, i.e., whether corrections were applied or not. Also, there is no information on if the salinity (CTD) data were calibrated against in-situ rosette bottle observations. This is crucial for deep ocean studies. I guess the authors have done that, but this needs to be documented in the paper.

Other Comments
L#35: "proved" is a too strong word given the few observations and no modeling simulations. I suggest the authors be careful with words.
L#46-47: According to Figure 1b, the AAB is not the 'greatest SLR' adjacent to Antarctica. Indeed, most of the Indian Ocean sector (including the western) and the Ross Sea show SLR as high as the AAB. I suggest the statement be modified to reflect the cited Figure. Also, it would be good to say how much this change in SLR is due to changes in deep-bottom T-S properties.
L#49: Not only WOCE, but authors also use GO-SHIP data according to Figure 1a (see cchdo webpage)
L#50-51: Section S1a does not present a detailed description of the data. Indeed, it sends the readers to supplementary Table S1. Even there, the description is not detailed as there is no information, for instance, about vertical resolution, calibration against in-situ data, whether salinity was corrected to differences in standard water, whether the Adhoc correction from Purkey and Johnson (based on CFC) was applied, the spatial resolution of all sections, etc.
L#52: In the caption of Fig.1, there is no indication of which inset is AAB and SAB. The caption states, "Upper, middle, and lower insets are the time series of the bottom (averages and standard deviations of the properties within 100 m from the bottom) potential temperature (red), salinity (blue), and neutral density". But is the upper SAB, the middle AAB, and the lower?
L#55-56: Is the DSW not warming only freshening? More information is needed here
L#58: I did not understand what is "warm northerly water masses". Please rewrite. Also, it would be good to explain how the ACC shift would impact the bottom water variability.
L#63: Need to define what is the southern ACC front (maybe cite Fig. 1b)
L#72-75: I think this unnecessary here as L#47-51 have already described the data used in the presented study and cited a supplementary section.
L#77: It seems there is some slightly salinification after 2015. Is that true?
L#80: Would it be below the CDW?
L#91-93: Could the authors elaborate on why the statistical significance would be greater in deep layers? Why this increase in statistical significance would lead to the conclusion that "deep layer components contributed substantially to the multi-decadal trends"?
L#106-107: It would be worthy to explain the AOU as a tracer for ventilation here.
L#108-110: I don't get the point here. I understood that the authors have removed the effect of AAC shift so why return to this. Or the SLR is the highest or is not. It is a quantity. You can't 'reasonably interpret as the highest'. It is not open to interpretation.
L#110-113: Same here. I don't get the point the authors are trying to make. I am unsure what 'consistently confirmed' would mean because I wouldn't know what is 'inconsistently confirmed'.
L#115: It is awkward that 'Intermediate Density Layer' can reach the bottom in the SAB. I think a better term could be used. See my previous comments.
L#117-119: As the 28.3 isopycnic doesn't exist in the SAB and the near-bottom density is 28.19, it is

hard not to get confused with Figures 3a and 3c. I suggest the authors express changes in properties in function of density and not pressure. There is also something strange in this phrase “In the mid to deep layer of the SAB, abyssal warming and AOU increase were reverted.” In Figure 3c, the abyssal water (> 28.19) is still warming (not cooling; no reversal there). The layer above, not the abyssal, is the one having a tendency for cooling, although uncertainty is too large to say that it is really cooling.
L#124: Please delete consistently
L#127-128: southward shift of what? Of the ACC front? If yes, something is contradicting L#105-107 in which affirms that the ACC front shift effects were removed from the data. I’m unsure whether the southward ACC shift effect has or not has been removed from the data.
L#130-131: I think 'reversed' is not an adequate word to describe the difference in the vertical structure. In both basins, the bottom layer is warming and freshening (although freshening is weaker in the SAB), so no proper reversal there in trends. This is also why I think it would be better to express the changes in terms of density instead of pressure, as we could see how the properties changed along isopycnal from one basin to another since the densest AABW layers don't exist in the SAB. As the AABW moves northward, it mixes (horizontally and vertically) with environment waters losing its T-S characteristics, with the bottom density being progressive lighter.
L#132-133: I suggest the authors define what they mean by ventilation here. Ventilation can have different meanings in the literature. Was the water that supplies the IDL in contact with the atmosphere around Antarctica as the classical AABW? Do they originate in the same sites?
L#135-136: please explain ‘this connection becomes wider in the shallower layer’. Is the Australian−Antarctic Discordance (AAD) narrow in the bottom and wide at shallow depths? What is the depth of the AAD?
L#136-137: Is there no diapycnal mixing within AAD?
L#137: Why in contrast?
L#143-144: please express changes in isopycnal (not pressure) to check if this argument makes sense as the text has previously stated that the transport from AAB to SAB is along isopycnal.

Reviewer #2 (Remarks to the Author):

Review of “Rerouting of Abyssal Ventilation in the Indian Sector of the Southern Ocean” submitted to Communications Earth & Environment by Shimada K. et al. (2021).

Based on numerous cruise data obtained from 1995 to 2019, this manuscript delineates decadal changes in the water properties in a region of the Southern Ocean (Australian-Antarctic basin and South Australian Basin) which is a crucial location with regards to the ocean’s Meridional Overturning Circulation (MOC). In particular, the authors confirmed that a main cause of warming and freshening trend in the abyssal layer (where Antarctic Bottom Water-AABW is located) is a reduction in the supply of AABW from upstream regions. The authors also demonstrated that the intermediate-deep layer is freshening and cooling, which imply that the ventilation in this layer is increasing, whereas that of the abyssal layer is decreasing. The authors also delineated how these changes in water properties affect the variations of the mean sea level in the study region.

Given the importance of MOC in regulating earth’s climate, and the public’s interest in climate changes in polar regions and sea level variations, I think that this manuscript is worthy of publication in Communications Earth & Environment. However, major modifications, especially in the article structure and writing, will be needed before this article can be published.

In terms of science, I find that the analysis methods and the general approach are sound and very pertinent. In particular, the authors took great care in the analysis of their dataset, making the best use of just a dozen cruise data via mapping. The authors were also able to distinguish those warming or freshening trends which were due to change in ventilation vs. changes in the location of the Southern Antarctic Circumpolar Current Front, thus solving a major issue in polar oceanography. My only concern with the analysis resides on the interpretation and lack of discussion of Fig. 3 (see major comment), and maybe in some missing discussions at some points.

However, I am much more concerned with the writing of the manuscript at this point. Overreliance on the supplementary materials, lack of context, too indirect writing, and too frequent use of abbreviations, make for a laborious reading. This is the main reason why I recommend a major modification. At this point, the authors are not very kind to the readers, and I would very much like them to keep this notion (kindness to the reader) in mind when revising the manuscript. Also, note that the quality of Fig. 3a, c is quite poor, and this figure absolutely needs to be made more readable before publication.

MAJOR COMMENTS

1) The article relies too much on the supplementary materials.

This is a problem because in some cases, the main findings of the authors seem hidden in 2 sentences within a larger paragraph, whereas the bulk of their finding is in the supplementary materials. Because there are so many references to the supplementary materials, it becomes difficult to understand what really is important to remember and what is secondary. Considering that the rule of thumbs for the number of words is 500 word per display item (Table or Figure), and that a maximum of up to 10 display items is accepted, I think the authors can make a much better use of the words and figures allocations. I would therefore suggest a serious rewriting of the manuscript, in order to make a better use of the figure and text allocations. At the very least, I suggest that the main part of Supplementary sections 3 be included in the results section, and all of the supplementary sections 7 and 8 be included in the Methods section.

2) Tedious, repetitive explanations, unclear writing.

Although the article's main text is short, it was not an easy read. While there are only very few grammatical mistakes, I found that the sentences structure, or choice of words were not straightforward enough. Subject was sometimes repeated where it shouldn't be, and omitted where it should have been written. These are just examples among several others, but the following sentences are either not very palatable, or just very unclear. (L108-110) "Furthermore, multiple factors, which induce sea level change, influence the AAB, such as warming induced by both the southward shift (***repetition from what was written just above***) in the ACC and weakened AABW supply (***repetition from what was written just above***), and widespread freshening (Supplementary Section S3). Thus, SLR in the AAB can be reasonably interpreted as the highest." or (L119-120): "Further, the vertical structure in the freshening trend was reversed from that of the core AAB, taking the maximum in the mid to deep layer with a reduction toward the bottom in the SAB.".

Also, sometimes, all explanations were made within the same paragraph, without break in between. This made it also difficult to follow well the logic or understand where the focus was supposed to be. I therefore recommend the authors to go through every sentence of the article and rewrite them, if necessary, by always asking themselves:

- Is the context provided in the sentence sufficient for the reader to understand?
- On the contrary, is the context superfluous?

- Is this sentence written in a way that it conveys the authors idea in the clearest, most straightforward way possible?
- Is there a need to make a separate paragraph at the end of this sentence?
More generally, rewriting should be focused on a good compromise between clarity of the message and supplying sufficient information to (1) inform the reader of what type of analysis was done, and (2) provide readers who may not be too familiar with physical oceanography with enough context so they can well understand the importance of this paper. Lastly, (3), please make sure that, within a paragraph or chapter, the authors' logic is explained in the most straightforward way possible.
In the "other comments" section, I included numerous comments about unclear sentences. These are not exhaustive though.

3) Discussion of the results of Fig. 3.
L138-141: One thing remains unclear with the results of Fig. 3. In the AAB, we see a trend around 0 °C/decade or a very slight warming trend between 900 and 1700 m depth. So, basically a 0 trend over a 800 m-width column. In the SAB, we see a cooling trend of -0.01°C/decade within a 1800 m-width column (from 2400 m to 4200 m). Therefore, if integrated over depth and distance, the heat deficit in the SAB seems quite larger than that in the AAB. Could the authors clarify this? Is this simply due to the large uncertainty of the data? Could there be any other reason explaining the stronger cooling and freshening in the SAB than in the AAB?
Lastly, we can see from Fig. 3b that between 1994-1995 and 2019, the layer thickness of the IDL does not change at all. But since the ventilation in the IDL intensifies, shouldn't we expect a larger volume of waters between 28.02 and 28.19 kg. m-3? Could the authors then discuss whether it is possible that the IDL cools in the AAB even though the layer thickness does not change?

4) Possibly a missing discussion on the extent of the freshening in the AAB.
How do the authors results compare with the observations by Purkey and Johnson (2013) that the AABW freshening in the Indian Ocean sector of the Southern Ocean is smaller than expected from the amount of glacial meltwater input into the Ocean? In that context, Haumann et al. (2016) suggested that the glacial meltwater input on the shelf is partly compensated by an increase of the offshore freshwater flux (increased equatorward transport of sea ice). It is not a strong comment, but I wonder whether mentioning about this paper could be pertinent in this manuscript.
Haumann, F., Gruber, N., Münnich, M. et al. Sea-ice transport driving Southern Ocean salinity and its recent trends. Nature 537, 89–92 (2016). https://doi.org/10.1038/nature19101

OTHER COMMENTS
L44-46: I think that a little bit more context and explanation are needed here. The authors should first explain how sea level rise around Antarctica is driven by the melting of Antarctica's grounded ice sheet and ice shelves. And that this increase in SLR is due in larger part to the steric contribution of the added freshwater than to the barystatic effect. Then, it could be further explained, why, in this context, it is necessary to study about SLR in the AAB, where dense water and AABW are formed.

L49: I would suggest changing (Fig. 1a) to (East Antarctica, Fig. 1a)

L52-56: I think that this part is slightly confusing because it is not entirely clear whether this statement is entirely inferred from the existing literature, or if this also includes some of the authors confirmed findings.
If this statement applies entirely to the literature and not the author's findings, then it may become

clearer if the authors move the preceding sentences (L47-52) to the beginning of the next paragraph (L62-67).
If this statement applies to both the existing literature and confirmed findings by the authors, then I would recommend to rephrase this paragraph so what is coming from the literature and what is confirmed by the authors is clearly identified.

L69-93: For this section, the analysis and Fig. 2 and Fig. S3 are pertinent and informative. However, I think that Fig. S3 needs to be included in the main manuscript. The main reason is that it introduces steric and (by deduction) non-steric changes of the Sea Surface Height. Non-steric or barystatic changes are non-negligible, which seems like a new result compared to Rye et al. (2014). And then, I think that that the source of the non-steric SLR should be mentioned. Likely, this should be the advection of additional (fresh) water generated by ice shelve melting in the Ross sea and other upstream regions.

L105-107: The authors rely too much on the supplementary materials here, even though the results described are interesting and significant. I know that there is a word limitation, but I think that the content in L105-107 should be changed into a proper paragraph (maybe a dozen lines or so) summarizing in a clear way the results of Supplementary section 3.

L108-110: I find these 2 sentences (until "the highest" L110) hard to follow somewhat. Could you please rephrase, and maybe also modify or remove the mention of "such as warming induced by both the southward shift in the ACC and weakened AABW supply", which is a repetition of what is written in L103-107.
L110 and elsewhere: In general, there may be too many abbreviations, which makes the reading uneasy at times. Please consider removing some of the less used abbreviations.

L117-118: Because the vertical grid is in depth unit and not in density unit in Fig. 3, the authors should also add the depth range (2400-4200m) between the brackets.

L117-118: ""In the mid to deep layer (density range; 28.02 to 28.19 kg/m3) of the SAB, abyssal warming and AOU increase were reversed, revealing cooling and AOU decrease". I find the phrasing of this sentence awkward. Please rephrase in a more direct, less confusing way. For example, "The mid to deep layer (density range; 28.02 to 28.19 kg/m3) of the SAB presented cooling and AOU decrease trends, i.e. opposite to the abyssal trends."
As indicated in the major comments, I think it would be better to focus on writing in a clearer, more direct way. It is important to avoid any formulation which may be confusing to the readers.

Figure 3: Panels a) and c) should be modified because the shaded, checkerboard-like confidence intervals obscure the actual data. At this point, this figure is not readable. My suggestion would be to use a plain color shade, setting transparency to something like 0.2 (on a scale with 0 being fully transparent and 1 being fully opaque). Also, consider smoothing the AOU curve because its noisy appearance probably makes the figure uneasy to decipher. An alternative would be to attach a small panel just to the left or right of the main panel and plot only the AOU curve on this attached panel.

L116-123: The organization of this paragraph could be improved. It was also quite confusing. First, the authors start talking about the opposite sign of the Temperature and AOU trends in mid to deep layer vs. abyssal layer in the SAB. This was fine, but just after, and without any transition, the authors mention about the salinity trends in the AAB and compare this with that of the SAB.

Here too, some context is missing. The authors should first explain briefly what they are doing, and explain the significance of comparing data in the SAB vs. AAB. Then only can they present the results. But when they do so, the authors should really focus on what is the clearest way to introduce the data. For example, first talking only about the temperature and AOU data. And once this subject has been covered, talking only about the salinity data. And then maybe explain the relationship between these 2 different data, if any. This is just an example, but overall, I would very much like to see an improvement both in the logical development of the authors ideas, and in the clarity of the writing.

L119-120: "Further, the vertical structure in the freshening trend was reversed from that of the core AAB, taking the maximum in the mid to deep layer with a reduction toward the bottom in the SAB.". What does this actually mean? If the salinity freshening maximum trend in the SAB is in the mid-to deep layer, does it mean that in the AAB the salinity freshening trend is minimum in the mid-to-deep layer? This is what this sentence actually implies, but that is not what I see in the figure. And to talk about "reversed" trends, is there really any sort of symmetry between the salinity trends in the AAB and SAB in the first place?
Therefore, I am very confused, and I suppose that many readers will be too. Could you please rephrase this?

L121: Please consider removing the IDL abbreviation. Too much abbreviations in the same sentence make for a difficult reading.

L135: Please remove the SEIR abbreviation, which is only used 4 times throughout the paper.
L136: The abbreviation AAD should also be removed. It is only used 2 times throughout the paper.

L133: Please start a new paragraph for the sentence starting with "First".

L134: I suppose there should be no comma after isopycnal.

L134-135: For clarity, I would suggest modifying this sentence into something like "The inter-basin connection at the lower boundary only occurs in the region near the Southeast Indian Ridge (Fig. 4), whereas the connection is fully open for all layers with density less than 28.19."

L138: "hence, transport would depend on the densest transport through the AAD". This sentence is unclear, so I do not understand what the author means. But from L134-138, I do understand that basically the shallow bathymetry of the Southeast Indian Ridge prevents most of the deep layer changes in the AAB to be transmitted to the SAB. Then, what about the Australian Antarctic Discordance? Is it an opening within the Southeast Indian Ridge, which let some of the densest water go through? This is not entirely clear from Fig. 1a and the sentence of L138 is not clear enough to fully understand the concept. I would advise the author to rework the writing of L134-138. Please try to rewrite this so the explanation is the clearest, most straightforward possible, with the help of the figures 1 and 4.

L138: What signal? The reversed signal in the IDL? The trend signal in general throughout the water column? I am sorry if my comment may seem picky, but I really want to point out that, at this point the writing is not very clear, and this lack of clarity prevents the scientific message to be passed effectively to the reader. Because this is one of the most important part of the manuscript, I would very much appreciate if an extra effort could be spent on clarity.

L143: To understand the connection with the next sentence, I think the sentence should be changed to “with the relatively warmer and oxygen-rich AAB”.

L145: maybe it should be “was observed in the AAB”.

L155: A new paragraph should be started from “To simply”.

L185: The whole Supplementary section 7 should be moved integrated into this chapter.

L201: The whole Supplementary section 8 should be moved and integrated into this chapter.

Supplementary section S1a: It would be better to state in a clearer way, from the beginning, that data were mapped to combine the results from the 2 sections, and that prior to this mapping, the applicability of combining the 2 sections’ data together was assessed.

L16: Is the mention of bottom depth criterion referred to Reference 34 (Pedlosky?). If so, could you please add the equation necessary to understand what is being done here, and what the bottom criterion is. This comment is probably relevant to the next comment as well (L18-19).

L18-19: This part is difficult to follow: What is the logical link between the bottom depth range and the fact that sea ice extension has interannual variability. Please explain in the text so a reader not familiar with the mapping methodology can understand.

L57: Please add a comma after “steric SLR”.

Supplementary section 2: Overall, it is somewhat hard to follow and some rewriting of the whole chapter may be needed.

L90: What is the significance level for the correlation? Given the number of years when CTD data were collected, I assume there are 10 degrees of freedom, which induces a 95% significance level for the correlation of 0.58. In this case, it would be good to add in Fig. S4a, d, a line to indicate the significance level. And please consider high negative correlation as anything greater (in absolute value) than 0.58 rather than 0.5.

An additional figure (or extra panel in Fig. S4) showing the time series of SACCF and changes in temperature at a certain depth level would be very helpful.

Supplementary section S3:
- L130: replace “are” by “is”.
- L158-159: For clarity, consider moving the sentence “The meridional shift component (1.2 ± 0.9 mm/y) accounted for the remaining 46 %.” To L161 after “compensated for each other”.

Reviewer #3 (Remarks to the Author):

This is a review for the manuscript titled 'Rerouting of Abyssal Ventilation in the Indian Sector of the Southern Ocean' by Shimada et al.

The study describes and lays out in detail- observational evidence suggesting changes in the lower Meridional Overturning Circulation (LMOC), an important component of the global Meridional Overturning Circulation. The LMOC is driven by the formation of Antarctic Bottom Water and its northward flow. Using data primarily from repeat hydrographic sections between 1995 and 2019, the study suggests that changes in watermass properties on interannual to decade timescales are occurring the formation regions of Antarctic Bottom Water in the Australian-Antarctic Basin region, affecting its rate of formation in the basin. The changes are not isolated but also affect the density and watermass properties in the basin downstream in the South Australian Basin.

The conclusions in the paper are supported by a rigorous error analysis of the variables presented and sufficient care has been taken to demonstrate that the conclusions are not as a result aliasing of natural variability in the system. While most of the observations come from a small region near the continental margins of Antarctica, the implications of the study are important and are also supported by other studies in different ocean basins as well. Overall, the manuscript is well-written and I commend the authors for putting together this work. Here are some comments which are mostly minor-

Comments:

1) I would suggest to name, abbreviate and delineate the neutral density extent between the different watermass layers earlier in the manuscript perhaps somewhere in the Introduction (Line 62 onwards) e.g. densest portion of the LMOC (> 28.30 kg/m 3) could be called the dense layer (DL) or something along those lines and the IDL (as introduced in Line 115) could be introduced earlier in the same paragraph so that its clear from the beginning which layers the results are referring to. It was slightly confusing to read 'less dense' ,'denser', 'densest' referring to the different watermass layers and so the delineation in the Introduction section should clear that confusion.

2) This might be a nit-picky issue concerning Figure 3- if possible I would suggest using a 'cleaner' shading for the confidence intervals in both Figure 3a and Figure 3c. There is quite a bit of overlap between the lines and therefore make it difficult to view. If there is a way to make it cleaner similar to Figure S7, that would be great. I would leave this to the discretion of the authors.

Other Comments:

Line 108-113 – These lines contain some critical information and inferences drawn from the data. However, the phrasing is somewhat confusing and the sentences are not grammatically sound. It would be best to re-phrase these lines in a slightly different way perhaps.

Line 116 – Not necessarily needed as information is the same as the last line in the previous

paragraph.

Line 119-121 – This sentence does not make much sense and needs re-phrasing. The intermediate density layer can be introduced along with the other layers in the Introduction- per my comment number 1).

Line 154 – Re-write as ....increased (decreased) transport in the IDL (abyssal) layer...'

Line 156 – I am not sure what is meant by interpolate/extrapolate here. If the meaning of the sentence is simlar to that of Line 154, then I would suggest using parentheses such as - interpolate (extrapolate) etc. to make it clearer.

Line 169 – phrase as … 'and weakening in the densest portion of the LMOC'.

Line 179-181- Adding 2-3 lines explaining in greater detail some of the expected implications from either modeling or theoretical perspective would help add context to these results about the observed shoaling in the ventilating layer.

Response to reviewer #1:

We thank the reviewer for thorough reading of our manuscript and positive assessment with very constructive comments. All your points helped us a lot, especially on increasing understandability of the manuscript. We believe that it was impossible to improve our manuscript without them.

Because the manuscript revision was extensive and substantial, we could not keep all changes in the manuscript to be highlighted. Instead, we will provide bullet points of our major changes to the manuscript in addition to point-by-point response to reviewer's comments.

Major changes:

- Title of the manuscript was changed to "Shoaling of abyssal ventilation in the in the Eastern Indian Sector of the Southern Ocean"
- Salinity correction for batch-to-batch offsets was newly applied. The presented results were, however, essentially unchanged.
- The abbreviation "D-LMOC" for "densest layers in the LMOC" was introduced to increase clarity.
- The abbreviation "L-LMOC" for "less dense layers of the LMOC" was introduced instead of intermediate density layer (IDL) introduced in the original manuscript.
- The manuscript was substantially restructured. Especially, contents in the supplementally information were moved to main text as much as possible. Major moved contents are Figs. 3 and 4, table 1, main part of Supplementary sections 3, all of the supplementary sections 7 and 8.
- The result section "Signals of intensified ventilation in less dense layers of LMOC", which describes main result of this study was extensively re-written to increase understandability.

Our response follows, one by one, the list of the reviewer's comments.

General comments

A first suggestion is about the title. After reading the MS, I am unsure what 'rerouting' means in this context, as the word was not used or defined anywhere in the MS. Second, this MS is only about the Eastern Indian Ocean sector of the Southern Ocean. The western part is not discussed in the MS. The western Southern Ocean is supplied by Weddell Sea bottom water, and according to Purkey and Johnson papers, the bottom in the southwestern is cooling and not warming.

Authors: Thank you for pointing this out. The title was modified as follows, accordingly.

"Shoaling of abyssal ventilation in the in the Eastern Indian Sector of the Southern Ocean"

The main criticism I have is about the way the MS is structured. In several parts of the MS, the reader is pointed for a supplementary section, sometimes with information crucial to understanding the core of the work. Arriving at the supplementary section, the reader is sent (again) to other supplementary sections/figures/tables (which sometimes can lead to the other supplementary sections/figures/tables). I had to open three different computers at the same time to be able to follow this maze. I guess other readers will have the same problem, making it hard to focus on the science part. I understand that supplementary sections/figures/tables may be a way to give more information than the maximum words allowed by the journal. However, the authors need to put some effort into improving the readability of this MS.

My suggestion is that the authors reduce the number of supplementary sections by putting together all information in fewer sections, so they do not point out to any other supplementary section. Also, it would be much better if supplementary figures and tables that belong to these supplementary sections were embedded in the section and not as separate entities (The authors need to consult with the editors if this would be possible). My suggestion is to have self-contained supplementary sections.

Authors: To reduce the readers to be sent around the sections, we moved contents in the supplementally information as much as possible to main text. Also, we combined the relevant contents which were distributed over multiple sections as much as possible. While supplementary figures and tables were not embedded in the section, we expect that readability of revised manuscripts much increased.

Another criticism is that the most important result and conclusion of the study are not entirely clear, even in the abstract. After my first reading, I couldn't pinpoint the central message of this study. From the abstract, we could infer that a 'weakened AABW supply' in L#30 is likely the main result. However, the word 'supply' is ambiguous (the authors will only be defined it later on). For instance, we could easily understand it as a weakening in the AABW formation rate. But that is not what the results suggest, as no formation rate was measured or inferred. Thus, 'supply' is not the most appropriate word for describing the study's main result. I would suggest the authors find another word that captures the main result and avoids misunderstandings. Maybe something similar to L#62-65 (but without the other 'supply').

Authors:
I used relatively ambiguous word “supply” because it is not clear whether there was reduction in AABW formation rate with constant temperature (and AOU) or there was warming (and AOU increase) in the formed AABW with constant formation rate, even if we confirm warming or AOU increase in abyssal layer after removing the effect of the SACCF. We expect that there was reduction in AABW formation rate, however, we have no evidence to differentiate these two. We thus consider that some ambiguity is inevitable.

The sentence which defined weakened AABW supply was modified to reduce ambiguity.

(original manuscript)
“here, the weakened supply includes both warming in the supplied AABW, with a relatively constant volume transport[6,9], and a reduction in the supplied AABW volume with relatively constant temperature[1,7–9],,

(Revised manuscript L 49-51; main point is that “supplied AABW” is replaced by “AABW formation rate”)
“here, the weakened supply includes both reduction in the AABW formation rate with relatively constant temperature[1,6–8], and warming in the formed AABW, with a relatively constant formation rate[8,9],,

The authors introduced a terminology in the MS, ‘intermediate density layer’ (L#115). Sincerely, I found it confusing as the ‘intermediate density layer’ gives an idea that the layer is much shallower in the water column when it is indeed a deep density layer (even the bottom layer in the South Australian Basin). Independent of that, I could not quite follow the arguments from L#130-166, which I believe is the core of the present work. I would suggest the authors draw a schematic diagram to explain the mechanism they are suggesting (this would help visualize it). I also think a model simulation may be a good addition here (but not required). For instance, two runs: one a control and another with changes in DSW. Would that

alter the type of AABW formed in the model?

Authors: Thank you for pointing this out.
Pease see our response for the next comment, rearding terminologies of intermediate density layer.
As for the arguments in L#130-166, We extensively re-written the section to increase understandability.
Regarding the model simulations, we do recognize importance of utilizing model simulation which reproduce response against the changing properties in DSW. However, model which reproduce realistic AABW formation process, including vertical mixing, is required for this purpose. We consider that such model study is beyond the scope of this manuscript and left for future work.

After reading the MS several times, I am still unsure if the authors are arguing that the rate of AABW formation has not changed, but what is happening is that a light version of AABW is being formed due to changes in source region properties. With that, a light AABW would reach the SAB much easier, crossing the Southeast Indian Ridge. If this lighter layer is being ventilated (by contact with the atmosphere in the formation region), then we would expect to see that in CFC/SF6 measurements, much is a better tracer than AOU for ventilation. I know that in GO-SHIP, these variables are collected. Could the authors say something about the CFC/SF6 signal in the I09S 2012 (SAB) in relation to 2005 and 1995? Is there increasing in CFC/SF6? Another suggestion is the authors take a look at the Labrador Sea Water literature. In that case, there is the Classical LSW and the upper LSW, which is lighter because less saline. Perhaps, there is some similarity here in which we have different classes of AABW and their prevalence change in decadal-to-long time scales (huge speculation here). The authors could denominate the IDL layer as upper AABW instead of 'intermediate density layer'.

Authors:
As mentioned above, our point for change in AABW formation rate is that we have no evidence to differentiate between reduction in AABW formation rate with constant temperature and warming in formed AABW with constant formation rate.

Regarding CFC/SF6, it is reasonable to expect signal of intensified ventilation to be found in L-LMOC. Along I09s section, both CFC-11, CFC-12 are available for observation in 1995 and 2005 (unfortunately no CFC/SF6 data was not sampled in 2012). We thus tried to estimate change in the ventilation age using partial pressure (Fine et al. 1988, Doney & Bullister 1992). However, we consider that obtained CFC data cannot resolve ventilation change during the observation period because increase rate of CFC-11 and CFC-12 in atmosphere during the target period (the period that the water in the layer had lastly contact with atmosphere; ~1950) is small compared to accuracy of CFC-11 and CFC-12 observation for sea water (Approximately 1-2%, Hood et al, 2010).

Details are as follows. pCFC-11 (CFC-11) and pCFC-12 (CFC-12) in L-LMOC (neutral density 28.02-28.19 kg/m^3) observed in 1995 are 1.4 (0.034 pmol/kg) and 3.5 (0.021 pmol/kg), respectively. These two imply that the water in the layer had lastly contact with atmosphere in the year 1953.2 and 1948.5, respectively. Increase rate of atmospheric CFC-11 and CFC-12 in those year are, however. 5.8 and 10.16 ppt/decade which are respectively comparable with accuracy in pCFC-11 (5.5) and pCFC-12 (11.9). Hence, change during the observation period between cannot be resolved and this topic remains as the future study.

As for terminology Intermediate Density Layer (IDL), we would like to introduce less dense layers of LMOC (L-

LMOC), as we mentioned as the major changes to the manuscript. Proposed terminology “upper AABW” is sounds nice and appropriate on point of analogy with Labrador Sea Water. However, we consider that defining new water mass in the Southern Ocean here is too much challenging for us.

As pointed by Purkey and Johnson's studies, salinity needs to be correct for the different batches of standard seawater. In some WOCE/GO-SHIP lines, this correction can be of the same order as the detectable salinity changes. I could not find anything about this kind of correction on the data description, i.e., whether corrections were applied or not. Also, there is no information on if the salinity (CTD) data were calibrated against in-situ rosette bottle observations. This is crucial for deep ocean studies. I guess the authors have done that, but this needs to be documented in the paper.

Authors: Thank you for pointing this out.
We revised manuscripts so that all the data details to be provided in supplementary section S1. As for salinity, we applied salinity calibration against in-situ rosette bottle observations in the previous version of the manuscript. Correction for batch-to-batch offsets in the Standard Sea Water was not applied and hence, we re-calculated in the revised manuscript. This application is expected to influence most of the presented results, however, were essentially unchanged as confirmed in revised Fig. 1-5.

Other Comments
L#35: “proved” is a too strong word given the few observations and no modeling simulations. I suggest the authors be careful with words.

Authors: Thank you for pointing this out.
The word “proved “ was replaced by “suggested”, accordingly.

L#46-47: According to Figure 1b, the AAB is not the ‘greatest SLR’ adjacent to Antarctica. Indeed, most of the Indian Ocean sector (including the western) and the Ross Sea show SLR as high as the AAB. I suggest the statement be modified to reflect the cited Figure. Also, it would be good to say how much this change in SLR is due to changes in deep-bottom T-S properties.

Authors: Thank you for pointing this out.
As pointed out, SLR in the AAB is not greatest among the Southern Ocean south of 62° S. Because that the word “greatest” was intended to be applied for whole the basin scale, we denoted so in the revised manuscript. Also, please note that 4000 m isobath and 62° S latitude line were added to Fig. 1b.

L#49: Not only WOCE, but authors also use GO-SHIP data according to Figure 1a (see cchdo webpage)

Authors: Thank you for pointing this out.
“/GO-SHIP” was added after WOCE, accordingly.

L#50-51: Section S1a does not present a detailed description of the data. Indeed, it sends the readers to supplementary Table S1. Even there, the description is not detailed as there is no information, for instance, about vertical resolution, calibration against in-situ data, whether salinity was corrected to differences in standard water, whether the Adhoc correction from

Purkey and Johnson (based on CFC) was applied, the spatial resolution of all sections, etc.

Authors: Thank you for pointing this out. We revised manuscripts so that all the data details to be provided in supplementary section S1. As for adhoc salinity correction introduced by Purkey and Johnson 2013, however, we did not apply this because we consider possibility that ventilation is reaching CDW; which was assumed to be the oldest water that in the Southern Ocean.

L#52: In the caption of Fig.1, there is no indication of which inset is AAB and SAB. The caption states, "Upper, middle, and lower insets are the time series of the bottom (averages and standard deviations of the properties within 100 m from the bottom) potential temperature (red), salinity (blue), and neutral density". But is the upper SAB, the middle AAB, and the lower?

Authors: Thank you for pointing this out.
We revised the caption so that it indicates the region for respective inset in the Fig. 1a. (Upper inset: SAB, middle inset: Northern AAB, lower inset: core AAB).

L#55-56: Is the DSW not warming only freshening? More information is needed here

Authors: This is reasonable.
Based on discussion by Shimada et al. 2012, temperature change in HSSW in the Ross Sea, which is major source water of AABW supplied to the AAB, is small (2×10^{-3} °C decade^{-1}). We thus modified the relevant sentence in the revised manuscript so that this point to be implemented.

L#58: I did not understand what is "warm northerly water masses". Please rewrite. Also, it would be good to explain how the ACC shift would impact the bottom water variability.

Authors: Thank you for pointing this out.
The sentence was re-written as follows.
"Another potential cause of the warming is the southward shift in the Antarctic Circumpolar Current (ACC) fronts, which also induce southward shift in warm water masses locate in the north[13], although the robustness of this phenomenon and its overall effect on the Southern Ocean remain under debate[14]."

L#63: Need to define what is the southern ACC front (maybe cite Fig. 1b)

Authors: Thank you for pointing this out.
We modified the sentence so that Fig. 1 to be cited.

L#72-75: I think this unnecessary here as L#47-51 have already described the data used in the presented study and cited a supplementary section.

Authors: Thank you for pointing this out.
Along with some modification in L#47-51, we deleted L#72-75, accordingly.

L#77: It seems there is some slightly salinification after 2015. Is that true?

Authors:
While this is not main topic in this manuscript, it remained after batch-to-batch offsets correction (Fig. 2a in the revised manuscript) and thus we consider that he slight salinification after 2015 is true.
Perhaps, it corresponds to reversed signal of persistent freshening in HSSW in the Ross Sea (Aoki, S. et al. 2020, Castagno, P. et al., 2019, Silvano, A. et al. 2020). This point is mentioned in the section "Implications of the change"

L#80: Would it be below the CDW?

Authors: Thank you for pointing this out.
Based on Orsi et al. (1999), the salinity maximum layer corresponds to lower circumpolar deep water. We modified the sentence to include this.

L#91-93: Could the authors elaborate on why the statistical significance would be greater in deep layers? Why this increase in statistical significance would lead to the conclusion that "deep layer components contributed substantially to the multi-decadal trends"?

Authors: It is likely that our point did not convey well owing to relying too much on supplementary section.
Statistical significances for multi-decadal trends in deep layers were greater in deep layers because those of shallow layers were reduced by evident interannual fluctuation. Also, it is based on these greater significances that we concluded that deep layer components contributed substantially. We modified the sentences so that they include these contexts.

L#106-107: It would be worthy to explain the AOU as a tracer for ventilation here.

Authors: Thank you for pointing this out. We added explanation for AOU in the revised manuscript, accordingly.

L#108-110: I don't get the point here. I understood that the authors have removed the effect of AAC shift so why return to this. Or the SLR is the highest or is not. It is a quantity. You can't 'reasonably interpret as the highest'. It is not open to interpretation.

Authors: It is likely that our point did not convey well owing to relying too much on supplementary section. Also, the sentence was ambiguous. After division of steric SLR into frontal shift and water-mass change components (Supplementary Section S4 in the revised manuscript), we argue that both southward shift and water mass change component are important.
Please also note that the section is extensively re-written to increase the understandability.

L#110-113: Same here. I don't get the point the authors are trying to make. I am unsure what 'consistently confirmed' would mean because I wouldn't know what is 'inconsistently confirmed'.

Authors: Thank you for pointing this out.
The sentences were modified as follows, accordingly.

"The southward shift in the ACC fronts is not evident in the SAB, whereas warming and AOU increase in the deep and abyssal layers below 4,000 m were confirmed based on observations on the WOCE/GO-SHIP I09s (Fig. 5d, 5e and Supplementary Section S5). Hence, ventilation in the D-LMOC is consistently weakened. "

L#115: It is awkward that 'Intermediate Density Layer' can reach the bottom in the SAB. I think a better term could be used. See my previous comments.

Authors: As commented previously, we introduced "less dense layers of LMOC (L-LMOC)" instead of "Intermediate Density Layer".

L#117-119: As the 28.3 isopycnic doesn't exist in the SAB and the near-bottom density is 28.19, it is hard not to get confused with Figures 3a and 3c. I suggest the authors express changes in properties in function of density and not pressure. There is also something strange in this phrase "In the mid to deep layer of the SAB, abyssal warming and AOU increase were reverted." In Figure 3c, the abyssal water (> 28.19) is still warming (not cooling; no reversal there). The layer above, not the abyssal, is the one having a tendency for cooling, although uncertainty is too large to say that it is really cooling.

Authors: We have extensively re-written the section to increase understandability. Also, the word "cooling" was modified to "cooling tendency", accordingly. Regarding the vertical coordinate, please see our response against the comment L#130-131 listed below.

L#124: Please delete consistently

Authors: Thank you for pointing this out. Owing to extensive re-writing of the section, however, the relevant sentence was deleted.

L#127-128: southward shift of what? Of the ACC front? If yes, something is contradicting L#105-107 in which affirms that the ACC front shift effects were removed from the data. I'm unsure whether the southward ACC shift effect has or not has been removed from the data.

Authors: Thank you for pointing this out.
Our explanation was insufficient. We removed the effect of southward shift of ACC front from estimated trends in the core AAB. However, it was difficult to remove them from the trends estimated for the entire AAB. This is because that these trends are based on observations along the WOCE/GO-SHIP sections (I09s, I08s, and SR3), which are respectively repeated for three times, and it was difficult to find clear relation between meridional shift of ACC fronts and observed variation in these observations (e.g., correlation coefficient between temperature variation and SACCF position; Fig. S4a in the revised manuscript).

To increase understandability, we modified the sentence as follows and modified relevant part of the Supplementary Section S5 to include the above contents.

"Linear trends of the entire AAB, estimated using three sections (I08s, I09s, and SR3), similarly revealed reduced warming and AOU decreasing tendency in the L-LMOC (please note that it was difficult to remove the effect of southward shift in the ACC fronts in the case for the entire AAB; Supplementary Section S5)."

L#130-131: I think 'reversed' is not an adequate word to describe the difference in the vertical structure. In both basins, the bottom layer is warming and freshening (although freshening is weaker in the SAB), so no proper reversal there in trends. This is also why I think it would be better to express the changes in terms of density instead of pressure, as we could see how the properties changed along isopycnal from one basin to another since the densest AABW layers don't exist in the SAB. As the AABW moves northward, it mixes (horizontally and vertically) with environment waters losing its T-S characteristics, with the bottom density being progressive lighter.

Authors: We have extensively re-written the section to increase understandability.
Regarding the vertical coordinate, we do recognize advantage of density coordinate especially for following spatial propagation of properties and trends, however, we consider that depth coordinate better for following relationship with bottom topography (especially the Southeast Indian Ridge). We also expect that the revised manuscript provides understandable context compensating the disadvantage of depth coordinate. Also, mean neutral density added to fig. 3 (Fig. 5 in the revised manuscript).

L#132-133: I suggest the authors define what they mean by ventilation here. Ventilation can have different meanings in the literature. Was the water that supplies the IDL in contact with the atmosphere around Antarctica as the classical AABW? Do they originate in the same sites?

Authors: We agree that we should provide more information for the ventilation, which is discussed here, although identifying the source region is still difficult at this stage. The supporting evidence for intensified ventilation was observed as increase in the intrusion of cool, fresh, and oxygen-rich water, of which properties are consistent with those of DSW. We thus mentioned in the revised manuscript that increased intrusions were likely derived from formation site of DSW although identification is yet difficult in the revised manuscript (second paragraph in the section "Signals of intensified ventilation in less dense layers of LMOC").

L#135-136: please explain 'this connection becomes wider in the shallower layer'. Is the Australian−Antarctic Discordance (AAD) narrow in the bottom and wide at shallow depths? What is the depth of the AAD?

Authors: Thank you for pointing this out.
We denoted in the preceding section ("Robust signals of weakened AABW supply and cause of extensive SLR") that Australian−Antarctic Discordance is relatively narrow deep zone in the Southeast Indian Ridge. We added depth of Australian−Antarctic Discordance (exceed 4000 m) in the revised sentence. Also, please note that we denoted in the revised figure caption for Fig. 3 (Fig. 5 in the revised manuscript) that schematized topography and isopycnals (c) reflect condition in the Australian−Antarctic Discordance.

L#136-137: Is there no diapycnal mixing within the AAD?

Authors: We expect that there is diapycnal mixing within the Australian−Antarctic Discordance, although lack in direct measurement.

Although we could not get what reviewer intended, we considered that the sentence, which sounds too much assertive (only the changes above isopycnal of 28.19 kg/m^3 can be transported to from the AAB to SAB), was inappropriate and thus modified. We expect that this point is solved in the revised manuscript.

L#137: Why in contrast?

Authors: The sentence was deleted in the revised manuscript. Corresponding context appears in the third paragraph of the section and we expect that inappropriate choice of word is removed.

L#143-144: please express changes in isopycnal (not pressure) to check if this argument makes sense as the text has previously stated that the transport from AAB to SAB is along isopycnal.

Authors: This is reasonable. We confirmed that warming and AOU increase are not observed also on isopycnal surfaces of current target range (28.02 to 28.19 kg/m^3) and the sentence is re-written as follows.

"However, this is not likely because no corresponding AOU increase on both isopycnal and isobaric surfaces were observed in the AAB. "

Response to reviewer #2:

We thank the reviewer for thorough reading of our manuscript and positive assessment with very constructive comments. All your points helped us a lot, especially on increasing understandability of the manuscript. We believe that it was impossible to improve our manuscript without them.

Because the manuscript revision was extensive and substantial, we could not keep all changes in the manuscript to be highlighted. Instead, we will provide bullet points of our major changes to the manuscript in addition to point-by-point response to reviewer's comments.

Major changes:

- Title of the manuscript was changed to "Shoaling of abyssal ventilation in the in the Eastern Indian Sector of the Southern Ocean"
- Salinity correction for batch-to-batch offsets was newly applied. The presented results were, however, essentially unchanged.
- The abbreviation "D-LMOC" for "densest layers in the LMOC" was introduced to increase clarity.
- The abbreviation "L-LMOC" for "less dense layers of the LMOC" was introduced instead of intermediate density layer (IDL) introduced in the original manuscript.
- The manuscript was substantially restructured. Especially, contents in the supplementally information were moved to main text as much as possible. Major moved contents are Figs. 3 and 4, table 1, main part of Supplementary sections 3, all of the supplementary sections 7 and 8.

・The result section ”Signals of intensified ventilation in less dense layers of LMOC”, which describes main result of this study was extensively re-written to increase understandability.

Our response follows, one by one, the list of the reviewer’s comments.

MAJOR COMMENTS

1) The article relies too much on the supplementary materials.

This is a problem because in some cases, the main findings of the authors seem hidden in 2 sentences within a larger paragraph, whereas the bulk of their finding is in the supplementary materials. Because there are so many references to the supplementary materials, it becomes difficult to understand what really is important to remember and what is secondary. Considering that the rule of thumbs for the number of words is 500 word per display item (Table or Figure), and that a maximum of up to 10 display items is accepted, I think the authors can make a much better use of the words and figures allocations. I would therefore suggest a serious rewriting of the manuscript, in order to make a better use of the figure and text allocations. At the very least, I suggest that the main part of Supplementary sections 3 be included in the results section, and all of the supplementary sections 7 and 8 be included in the Methods section.

> Authors: To reduce the readers to be sent around the sections, we moved contents in the supplementally information as much as possible to main text. Also, we combined the relevant contents which were distributed over multiple sections as much as possible.

2) Tedious, repetitive explanations, unclear writing.

Although the article’s main text is short, it was not an easy read. While there are only very few grammatical mistakes, I found that the sentences structure, or choice of words were not straightforward enough. Subject was sometimes repeated where it shouldn’t be, and omitted where it should have been written. These are just examples among several others, but the following sentences are either not very palatable, or just very unclear. (L108-110) “Furthermore, multiple factors, which induce sea level change, influence the AAB, such as warming induced by both the southward shift (***repetition from what was written just above***) in the ACC and weakened AABW supply (***repetition from what was written just above***), and widespread freshening (Supplementary Section S3). Thus, SLR in the AAB can be reasonably interpreted as the highest.” or (L119-120): “Further, the vertical structure in the freshening trend was reversed from that of the core AAB, taking the maximum in the mid to deep layer with a reduction toward the bottom in the SAB.”.

Also, sometimes, all explanations were made within the same paragraph, without break in between. This made it also difficult to follow well the logic or understand where the focus was supposed to be. I therefore recommend the authors to go through every sentence of the article and rewrite them, if necessary, by always asking themselves:

- Is the context provided in the sentence sufficient for the reader to understand?
- On the contrary, is the context superfluous?
- Is this sentence written in a way that it conveys the authors idea in the clearest, most straightforward way possible?
- Is there a need to make a separate paragraph at the end of this sentence?

More generally, rewriting should be focused on a good compromise between clarity of the message and supplying sufficient information to (1) inform the reader of what type of analysis was done, and (2) provide readers who may not be too familiar with physical oceanography with enough context so they can well understand the importance of this paper. Lastly, (3), please make sure that, within a paragraph or chapter, the authors’ logic is explained in the most straightforward way possible.

In the “other comments” section, I included numerous comments about unclear sentences. These are not exhaustive though.

Authors: Thank you very much for your thorough advise for us to revise the manuscript. After reconstructing the contents of the manuscript, we checked every sentence and paragraph so that our points convey well.

3) Discussion of the results of Fig. 3.

L138-141: One thing remains unclear with the results of Fig. 3. In the AAB, we see a trend around 0 °C/decade or a very slight warming trend between 900 and 1700 m depth. So, basically a 0 trend over a 800 m-width column. In the SAB, we see a cooling trend of -0.01°C/decade within a 1800 m-width column (from 2400 m to 4200 m). Therefore, if integrated over depth and distance, the heat deficit in the SAB seems quite larger than that in the AAB. Could the authors clarify this? Is this simply due to the large uncertainty of the data? Could there be any other reason explaining the stronger cooling and freshening in the SAB than in the AAB?

Lastly, we can see from Fig. 3b that between 1994-1995 and 2019, the layer thickness of the IDL does not change at all. But since the ventilation in the IDL intensifies, shouldn’t we expect a larger volume of waters between 28.02 and 28.19 kg. m-3? Could the authors then discuss whether it is possible that the IDL cools in the AAB even though the layer thickness does not change?

Authors: Both points are reasonable.

Regarding the first point, we agree that cooling tendency in the SAB (-0.01°C/decade) cannot be explained by propagation of potential temperature trends in the core AAB (neutral or slight warming). We consider that negative heat advection can also be induced by intensified transport from the AAB to the SAB because temperature in steady state is cooler in the AAB than in the SAB, although conclusive discussion is difficult due to large error. This point is implemented in the revised manuscript (Please see the fourth paragraph in section “Signals of intensified ventilation in less dense layers of LMOC” in the revised manuscript).

Regarding the second point, we totally agree that there must be volume increase in the L-LMOC (IDL in the original manuscript) under increased ventilation. Point is that the L-LMOC is not closed in the study area and widely distributed over the Southern hemisphere (Fig. S12; Fig. S11 in the revised manuscript). Such an L-LMOC should be comprehensively analyzed to answer this question, which exceed the scope of this study. We thus consider that the topic will remain as the future study.

4) Possibly a missing discussion on the extent of the freshening in the AAB.

How do the authors results compare with the observations by Purkey and Johnson (2013) that the AABW freshening in the Indian Ocean sector of the Southern Ocean is smaller than expected from the amount of glacial meltwater input into the Ocean? In that context, Haumann et al. (2016) suggested that the glacial meltwater input on the shelf is partly compensated by an increase of the offshore freshwater flux (increased equatorward transport of sea ice). It is not a strong comment, but I wonder whether mentioning about this paper could be pertinent in this manuscript.

Haumann, F., Gruber, N., Münnich, M. et al. Sea-ice transport driving Southern Ocean salinity and its recent trends. Nature 537, 89–92 (2016). https://doi.org/10.1038/nature19101

Authors: Thank you for pointing this out.

We agree that understanding hydrological cycle is important because salinity change in the Southern Ocean is

prominent. We believe that we have updated and improved understanding for multi-decadal changes by separating ACC shift component and water mass change component. However, our results are derived from core AAB, which is relatively small are in the AAB, and hence, estimation may be influenced by large interpolation error if they are applied to the overall basin. We thus consider that comparison with the basin scale budget will not be the main topic of this study.

Even though, speculation is possible and interesting.
We estimated freshwater input to explain basin wide freshening in the AAB by extrapolating the observed trends in the core AAB zonally (80–140°E) and meridionally. For meridional direction, integration was limited to lower- to mid-slope region (in the bottom depth range from 2600–4400 m) in accordance with the core AAB. Please note that integration area is roughly a half of estimation of Purkey and Johnson (2013).
For the layer below salinity maximum (~900 m; I selected this layer because freshening becomes significant below this layer), required freshwater input was estimated as 32 $\pm$ 22 Gton/y. For the water mass freshening (effect of southward shift is removed), it was modified to 49 $\pm$ 21 Gton/y (153% of the observed input). These estimates are comparable with that of Purkey and Johnson (2013; 48 $\pm$ 36 Gton/y), although ambiguity remains in contribution from the northern region of lower slope in the AAB.

Hereafter we follow discussion in Purkey and Johnson (2013). The freshwater flux into the South Pacific and south Indian Oceans is estimated to be 140 Gt/y over recent decades (Rignot et al. 2008) and freshwater input for the deep Amundsen–Bellingshausen basins is estimated as 25 $\pm$ 9 Gton/y (Purkey and Johnson, 2013). Suppose that freshwater input estimated from slope region will be doubled to include the contribution from northern region of lower slope in the AAB (namely, 98 $\pm$ 42 Gton/y), total freshwater input in the ocean will be 123 $\pm$ 51 Gton/y. In this case, the glacial meltwater input and freshwater input in the ocean are assessed to be consistent within error.

I consider that freshwater input for the AAB in above discussion is overestimated because freshening reduces to the north. I thus consider that compensation by increased equatorward transport of sea ice suggested by Haumann et al. (2016) is important. However, conclusive discussion is difficult owing to lack in information for water mass change in the north region of the slope.

OTHER COMMENTS

L44-46: I think that a little bit more context and explanation are needed here. The authors should first explain how sea level rise around Antarctica is driven by the melting of Antarctica's grounded ice sheet and ice shelves. And that this increase in SLR is due in larger part to the steric contribution of the added freshwater than to the barystatic effect. Then, it could be further explained, why, in this context, it is necessary to study about SLR in the AAB, where dense water and AABW are formed.

Authors: We agree that more context and explanation is needed here. We re-written the sentences as follows.
Please note that we did not mention about barystatic effect in the revised manuscript because we considered that barystatic effect discussed in Rye et al. is not directly related to the results presented in our manuscript. Pease also see our response to your comment (L69-93:) for detail. Also, please note that 4000 m isobath line indicating 62° S is added to fig. 2b.

"Sea level rise (SLR) in the high latitude region south of 62° S exceeds the global mean by 1–5 mm/year[5], and SLR in the Australian-Antarctic Basin (AAB) is the highest at basin scale among the major basins adjacent to Antarctica (Fig. 1b). Not only magnitude but these spatial variations are also critical because they may reflect source and path of accelerated freshwater supply from the Antarctic Ice Sheet and variation in formation and circulation of AABW[5].
"

L49: I would suggest changing (Fig. 1a) to (East Antarctica, Fig. 1a)

Authors: This is reasonable. We added "East Antarctica", accordingly.

L52-56: I think that this part is slightly confusing because it is not entirely clear whether this statement is entirely inferred from the existing literature, or if this also includes some of the authors confirmed findings. If this statement applies entirely to the literature and not the author's findings, then it may become clearer if the authors move the preceding sentences (L47-52) to the beginning of the next paragraph (L62-67). If this statement applies to both the existing literature and confirmed findings by the authors, then I would recommend to rephrase this paragraph so what is coming from the literature and what is confirmed by the authors is clearly identified.

Authors: This is reasonable.

Because L52-56 in the original manuscript applies entirely to the literature, the preceding sentences (L47-52) was moved to the beginning of the next paragraph (L62-67).

L69-93: For this section, the analysis and Fig. 2 and Fig. S3 are pertinent and informative. However, I think that Fig. S3 needs to be included in the main manuscript. The main reason is that it introduces steric and (by deduction) non-steric changes of the Sea Surface Height. Non-steric or barystatic changes are non-negligible, which seems like a new result compared to Rye et al. (2014). And then, I think that that the source of the non-steric SLR should be mentioned. Likely, this should be the advection of additional (fresh) water generated by ice shelve melting in the Ross sea and other upstream regions.

Authors:

We agree that Rye et al. (2014) concluded from simulation that barystatic SLR is negligible, however, they referred to contribution to local anomalies (barystatic response would be spatially uniform and not likely contribute to local anomaly).

The barystatic SLR of 0.5 mm/year, which is deduced in our study from the SLR difference between satellite (3.2 mm/decade) and steric (2.7 mm/decade), is absolute value for SLR and its contribution is expected to be spatially uniform.

Thus, we consider that conclusion of ours and Rye et al. (2014) are not directly related, although estimated barystatic SLR of 0.5 mm/year is not negligible (equivalent to 16 % of steric SLR)

Please note that we moved Fig. S3, table S2 and the supplemental section S1d (Observed SLR and its characteristics) to main text with some modification to increase understandability.

L105-107: The authors rely too much on the supplementary materials here, even though the results described are interesting and significant. I know that there is a word limitation, but I think that the content in L105-107 should be changed into a proper paragraph (maybe a dozen lines or so) summarizing in a clear way the results of Supplementary section 3.

Authors: Thank you for pointing this out. This part is much complemented in the revised manuscript by moving most part of supplementary section S3 in the original version of the manuscript.

L108-110: I find these 2 sentences (until “the highest” L110) hard to follow somewhat. Could you please rephrase, and maybe also modify or remove the mention of “such as warming induced by both the southward shift in the ACC and weakened AABW supply”, which is a repetition of what is written in L103-107.

Authors: Thank you for pointing this out.
We revised the sentence to increase understandability.

L110 and elsewhere: In general, there may be too many abbreviations, which makes the reading uneasy at times. Please consider removing some of the less used abbreviations.

Authors: Thank you for pointing this out. We removed uncommon or less used abbreviation as much as possible.
As mentioned in major changes, however, we introduced L-LMOC (less dense layers of LMOC; IDL in the original manuscript) and D-LMOC (densest portion of LMOC) to reduce ambiguity induced by appearance of too many words relevant to density (less dense, denser, densest) in the manuscript.

L117-118: Because the vertical grid is in depth unit and not in density unit in Fig. 3, the authors should also add the depth range (2400-4200m) between the brackets.

Authors: This is reasonable. We added depth range between the brackets.

L117-118: ““In the mid to deep layer (density range; 28.02 to 28.19 kg/m3) of the SAB, abyssal warming and AOU increase were reversed, revealing cooling and AOU decrease”. I find the phrasing of this sentence awkward. Please rephrase in a more direct, less confusing way. For example, “The mid to deep layer (density range; 28.02 to 28.19 kg/m3) of the SAB presented cooling and AOU decrease trends, i.e. opposite to the abyssal trends.”
As indicated in the major comments, I think it would be better to focus on writing in a clearer, more direct way. It is important to avoid any formulation which may be confusing to the readers.

Thank you for pointing this out.
As noted as response to your comment for L116-123 listed below, we have extensively re-written the section (L. 116-155) to increase understandability. We believe that awkward phrasings were appropriately modified in the revised version of the manuscript.

Figure 3: Panels a) and c) should be modified because the shaded, checkerboard-like confidence intervals obscure the actual data. At this point, this figure is not readable. My suggestion would be to use a plain color shade, setting transparency to something like 0.2 (on a scale with 0 being fully transparent and 1 being fully opaque). Also, consider smoothing the AOU curve because its noisy appearance probably makes the figure uneasy to decipher. An alternative would be to attach a small panel just to the left or right of the main panel and plot only the AOU curve on this attached panel.

Authors: We agree that vertical profiles in shown in Fig. 3a and 3c are difficult to view.
After testing different shading patterns, we considered that it is difficult to display four vertical profiles with their errors in a single panel. Thus, the combined vertical profiles in Fig. 3a, 3c are divided into upper (theta and salinity trends) and lower panels (neutral density and AOU trends), respectively. Also, please note that the same changes are made for Fig S8 (Fig. S7 in the revised manuscript).

L116-123: The organization of this paragraph could be improved. It was also quite confusing. First, the authors start talking about the opposite sign of the Temperature and AOU trends in mid to deep layer vs. abyssal layer in the SAB. This was fine, but just after, and without any transition, the authors mention about the salinity trends in the AAB and compare this with that of the SAB.
Here too, some context is missing. The authors should first explain briefly what they are doing, and explain the significance of comparing data in the SAB vs. AAB. Then only can they present the results. But when they do so, the authors should really focus on what is the clearest way to introduce the data. For example, first talking only about the temperature and AOU data. And once this subject has been covered, talking only about the salinity data. And then maybe explain the relationship between these 2 different data, if any. This is just an example, but overall, I would very much like to see an improvement both in the logical development of the authors ideas, and in the clarity of the writing.

Authors: Thank you for pointing this out.
We tried to explain all the characteristics of obtained trends at once in the original version of the manuscript, but it is likely that this attempt made the organization poor. Thus, we have extensively re-written the section (L. 116-155) to increase understandability.

L119-120: “Further, the vertical structure in the freshening trend was reversed from that of the core AAB, taking the maximum in the mid to deep layer with a reduction toward the bottom in the SAB.”. What does this actually mean? If the salinity freshening maximum trend in the SAB is in the mid-to deep layer, does it mean that in the AAB the salinity freshening trend is minimum in the mid-to-deep layer? This is what this sentence actually implies, but that is not what I see in the figure. And to talk about “reversed” trends, is there really any sort of symmetry between the salinity trends in the AAB and SAB in the first place? Therefore, I am very confused, and I suppose that many readers will be too. Could you please rephrase this?

Authors: Thank you for pointing this out.
We tried to explain difference in spatial structure of freshening between the AAB and the SAB (freshening increases toward bottom in the AAB but freshening reduced from mid to deep layer to bottom), but the wording was not precise.
During the revision relevant to previous comment (L116-123), however, the sentence was deleted in the revised version of the manuscript.

L121: Please consider removing the IDL abbreviation. Too much abbreviations in the same sentence make for a difficult reading.

Authors: We agree that too many abbreviations reduce the understandability.
However, we considered that abbreviation “L-LMOC (less dense layers of LMOC)”, which was introduced as IDL (Intermediate density layer) in the original manuscript, was inevitable to reduce ambiguity induced by appearance of

too many words relevant to density (less dense, denser, densest) in the manuscript. Also, we added abbreviations D-LMOC (densest portion of LMOC) due to the same reason. Instead, we removed uncommon or less used abbreviation as much as possible.

L135: Please remove the SEIR abbreviation, which is only used 4 times throughout the paper.

Authors: This is reasonable. The abbreviation SEIR was removed in the revised version of the manuscript.

L136: The abbreviation AAD should also be removed. It is only used 2 times throughout the paper.

Authors: This is reasonable. The abbreviation AAD was removed in the revised version of the manuscript.

L133: Please start a new paragraph for the sentence starting with "First".

Authors: Thank you for pointing this out. The sentence was, however, removed in the revised version of the manuscript.

L134: I suppose there should be no comma after isopycnal.

Authors: This is reasonable. The sentence was, however, removed in the revised version of the manuscript.

L134-135: For clarity, I would suggest modifying this sentence into something like "The inter-basin connection at the lower boundary only occurs in the region near the Southeast Indian Ridge (Fig. 4), whereas the connection is fully open for all layers with density less than 28.19."

Authors: Thank you for very helpful comment.
We adopted the proposed modifications, accordingly. Please note that the word "Southeast Indian Ridge" is replaced by "Australian-Antarctic Discordance" considering the context.

L138: "hence, transport would depend on the densest transport through the AAD". This sentence is unclear, so I do not understand what the author means. But from L134-138, I do understand that basically the shallow bathymetry of the Southeast Indian Ridge prevents most of the deep layer changes in the AAB to be transmitted to the SAB.
Then, what about the Australian Antarctic Discordance? Is it an opening within the Southeast Indian Ridge, which let some of the densest water go through? This is not entirely clear from Fig. 1a and the sentence of L138 is not clear enough to fully understand the concept. I would advise the author to rework the writing of L134-138. Please try to rewrite this so the explanation is the clearest, most straightforward possible, with the help of the figures 1 and 4.

Authors: Thank you for pointing this out.
In the 133-138, we tried to mention that, in deep layers below the L-LMOC, isopycnal transport would be interrupted by the Southeast Indian Ridge and deep transport through the Australian-Antarctic Discordance[22], which composes densest portion of the LMOC, would contribute to cross-basinal transport. We expect that this point will convey well in the revised manuscript.

L138: What signal? The reversed signal in the IDL? The trend signal in general throughout the water column? I am sorry if my comment may seem picky, but I really want to point out that, at this point the writing is not very clear, and this lack of clarity prevents the scientific message to be passed effectively to the reader. Because this is one of the most important part of the manuscript, I would very much appreciate if an extra effort could be spent on clarity.

Authors: Thank you for pointing this out. The signal in the L 138 of original manuscript, referred to cooling tendency and AOU decreasing trend observed in the IDL of the SAB. While the sentence is deleted, we expect that overall clarity is much increased in the revised version of the manuscript.

L143: To understand the connection with the next sentence, I think the sentence should be changed to "with the relatively warmer and oxygen-rich AAB".

Authors: Thank you for pointing this out.
This is reasonable.
However, I have removed "warming or" from the next sentence to increase understandability of connection with the former sentence. This is because that we considered that detailed discussion for temperature trend in the IDL is inappropriate due to large error.

Before revision.
"Intensification of mesoscale eddy activity[23,24] may also decrease AOU in the SAB by increasing the exchange with the relatively oxygen-rich AAB (Supplementary Section S6). However, this is not likely because no corresponding warming or AOU increase on isopycnal was observed in the AAB."

After revision.
"Intensification of mesoscale eddy activity[24,25] may also decrease AOU in the SAB by increasing the exchange with the relatively oxygen-rich AAB (Supplementary Section S7). However, this is not likely because no corresponding AOU increase on both isopycnal and isobaric surfaces were observed in the AAB. "

L145: maybe it should be "was observed in the AAB".

Authors: Thank you for pointing this out. This point in implemented in the revised sentence.

L155: A new paragraph should be started from "To simply".

Authors: Thank you for pointing this out.
We have corrected accordingly.

L185: The whole Supplementary section 7 should be moved integrated into this chapter.

Authors: The whole Supplementary section 7 is moved to "Methods" section in the main text, accordingly.

L201: The whole Supplementary section 8 should be moved and integrated into this chapter.

Authors: The whole Supplementary section 8 is moved to “Methods” section in the main text, accordingly.

Supplementary section S1a: It would be better to state in a clearer way, from the beginning, that data were mapped to combine the results from the 2 sections, and that prior to this mapping, the applicability of combining the 2 sections’ data together was assessed.

Authors: Thank you for providing appropriate way.
The section S1a (S2a in the revised manuscript) is extensively re-written, accordingly.

L16: Is the mention of bottom depth criterion referred to Reference 34 (Pedlosky?). If so, could you please add the equation necessary to understand what is being done here, and what the bottom criterion is. This comment is probably relevant to the next comment as well (L18-19).

Authors: Thank you for pointing out.
It is likely that our point did not convey well. The sentence was re-written as follows.

“Thus, horizontal resolution among Umitaka-Maru and WOCE/GO-SHIP sections remained relatively comparable using data that meet the bottom depth range.”

L18-19: This part is difficult to follow: What is the logical link between the bottom depth range and the fact that sea ice extension has interannual variability. Please explain in the text so a reader not familiar with the mapping methodology can understand.

Authors: Thank you for pointing out.
It is likely that our writing was not straightforward. The relevant sentences were re-written as follows.

“However, especially in shallower southern region, meridional extent of the sections is influenced by sea ice extension, which presents inter-annual fluctuations. Thus, the shallower limit was modified to 2,600 m to avoid being influenced by sea ice extension in the gridding procedure, the product of which was used for quantitative analysis such as the sea-level rise (SLR).”

L57: Please add a comma after “steric SLR”.

Authors: Thank you for pointing this out.
The sentence was, however, deleted in the revised manuscript.
(Please note that relevant section was moved to main text in the revised manuscript.)

Supplementary section 2: Overall, it is somewhat hard to follow and some rewriting of the whole chapter may be needed.

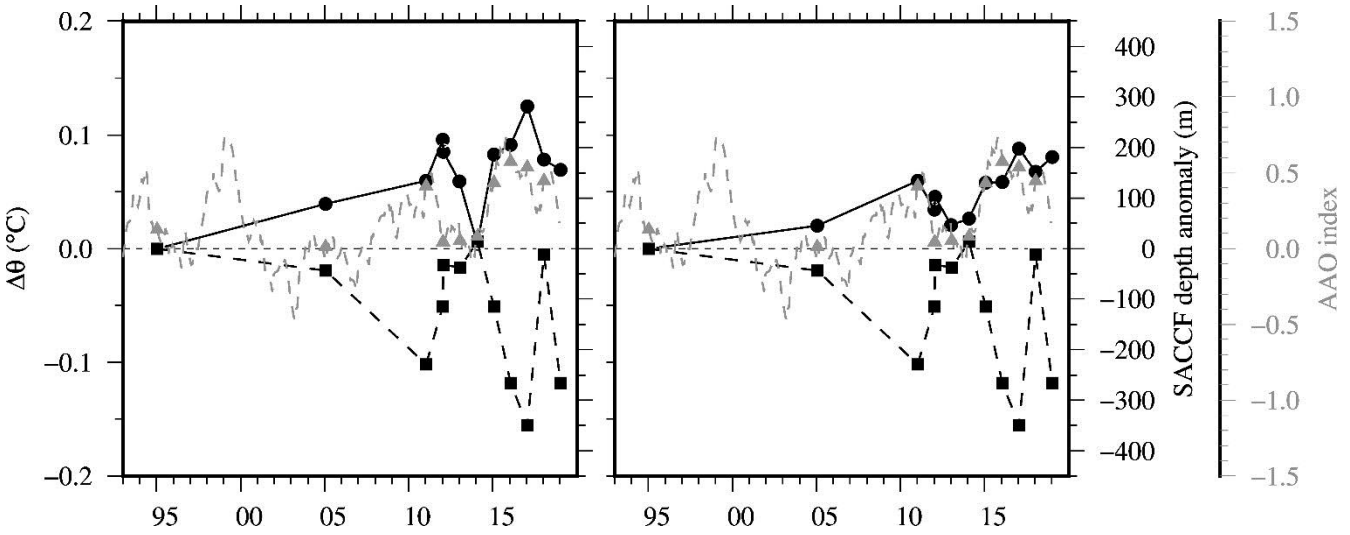
Authors: Thank you for pointing this out. The Supplementary section 2 was extensively re-written to reduce unclear sentences.

L90: What is the significance level for the correlation? Given the number of years when CTD data were collected, I assume there are 10 degrees of freedom, which induces a 95% significance level for the correlation of 0.58. In this case, it would be good to add in Fig. S4a, d, a line to indicate the significance level. And please consider high negative correlation as anything greater (in absolute value) than 0.58 rather than 0.5.

Authors: This is reasonable. In our case, 95% significance level for the correlation is 0.58. Thus, lines in Fig. S4a, and d were moved to point of correlation coefficient 0.58.

An additional figure (or extra panel in Fig. S4) showing the time series of SACCF and changes in temperature at a certain depth level would be very helpful.

Authors: This is reasonable, however, we consider that timeseries of temperature changes at certain depths are essentially same with that of thermo-steric SLR shown in Fig. S5 in the revised manuscript. Shown below are the time series of potential temperature change at 1,000 (left) and 3,000 m depth (right), respectively. In both panels, potential temperature change, SACCF depth anomaly, and AAOI (smoothed by the two-year moving average) are shown by black circles with solid lines, black squares with dashed lines, and grey triangles with dashed lines, respectively.



Supplementary section S3:
- L130: replace “are” by “is”.

Authors: Thank you for pointing this out. The sentence, however, was deleted during the revision (major part of section S3 is moved to result section).

- L158-159: For clarity, consider moving the sentence “The meridional shift component (1.2 ± 0.9 mm/y) accounted for the remaining 46 %.” To L161 after “compensated for each other”.

Authors: Thank you for pointing this out. The sentence was moved accordingly.

Response to reviewer #3:

We thank the reviewer for thorough reading of our manuscript and positive assessment with very constructive comments. All your points helped us a lot, especially on increasing understandability of the manuscript. We believe that it was impossible to improve our manuscript without them.

Because the manuscript revision was extensive and substantial, we could not keep all changes in the manuscript to be highlighted. Instead, we will provide bullet points of our major changes to the manuscript in addition to point-by-point response to reviewer's comments.

Major changes:

- Title of the manuscript was changed to "Shoaling of abyssal ventilation in the in the Eastern Indian Sector of the Southern Ocean"
- Salinity correction for batch-to-batch offsets was newly applied. The presented results were, however, essentially unchanged.
- The abbreviation "D-LMOC" for "densest portion of the LMOC" was introduced to increase clarity.
- The abbreviation "L-LMOC" for "less dense layers of the LMOC" was introduced instead of intermediate density layer (IDL) introduced in the original manuscript.
- The manuscript was substantially restructured. Especially, contents in the supplementally information were moved to main text as much as possible. Major moved contents are Figs. 3 and 4, table 1, main part of Supplementary sections 3, all of the supplementary sections 7 and 8.
- The result section "Signals of intensified ventilation in less dense layers of LMOC", which describes main result of this study was extensively re-written to increase understandability.

Our response follows, one by one, the list of the reviewer's comments.

Major Comments:

1) I would suggest to name, abbreviate and delineate the neutral density extent between the different watermass layers earlier in the manuscript perhaps somewhere in the Introduction (Line 62 onwards) e.g. densest portion of the LMOC (> 28.30 kg/m 3) could be called the dense layer (DL) or something along those lines and the IDL (as introduced in Line 115) could be introduced earlier in the same paragraph so that its clear from the beginning which layers the results are referring to. It was slightly confusing to read 'less dense' ,'denser', 'densest' referring to the different watermass layers and so the delineation in the Introduction section should clear that confusion.

Authors: We agree that layer definition is a little bit confusing.

As stated in first paragraph of section "Robust signals of weakened AABW supply and cause of extensive SLR" in the original manuscript, however, definition of densest portion of the LMOC is too long to be introduced in the "introduction" section, as it includes slightly less dense water (~28.22 kg/m3) transport from the AAB to the SAB through the Australian-Antarctic Discordance. Thus, we will not change defining section. Instead, the abbreviations "D-LMOC" for densest portion of the LMOC, "L-LMOC" for intermediate density layer are introduced for increasing clarity.

2) This might be a nit-picky issue concerning Figure 3- if possible I would suggest using a 'cleaner' shading for the confidence intervals in both Figure 3a and Figure 3c. There is quite a bit of overlap between the lines and therefore make it difficult to view. If there is a way to make it cleaner similar to Figure S7, that would be great. I would leave this to the discretion of the authors.

Authors: Thank you for pointing this out. We agree that vertical profiles in shown in Fig. 3a and 3c are difficult to view. The combined vertical profiles in Fig. 3a, 3c are divided into upper (theta and salinity trends) and lower column (neutral density and AOU trends), respectively (Fig. 5 in the revised manuscript). Also, please note that the same changes are made for Fig S8 (Fig. S7 in the revised manuscript).

Other Comments:

Line 108-113 – These lines contain some critical information and inferences drawn from the data. However, the phrasing is somewhat confusing and the sentences are not grammatically sound. It would be best to re-phrase these lines in a slightly different way perhaps.

Authors: Thank you for pointing this out. We believe that the sentence were appropriately re-written in the revised manuscript.

Line 116 – Not necessarily needed as information is the same as the last line in the previous paragraph.

Authors: This is reasonable. Line 116 in the original manuscript was deleted.

Line 119-121 – This sentence does not make much sense and needs re-phrasing. The intermediate density layer can be introduced along with the other layers in the Introduction- per my comment number 1).

Authors: Thank you for pointing this out. The sentence was deleted owing to extensive re-writing of this section. Regarding layer definition, please see our response against major comment 1).

Line 154 – Re-write as ....increased (decreased) transport in the IDL (abyssal) layer...'

Authors: This is reasonable. However, the sentence deleted in the revised manuscript owing to extensive re-writing of this section.

Line 156 – I am not sure what is meant by interpolate/extrapolate here. If the meaning of the sentence is simlar to that of Line 154, then I would suggest using parentheses such as - interpolate (extrapolate) etc. to make it clearer.

Authors:
Wording "interpolate/extrapolate" is ambiguous, as pointed out.
A relevant part of the sentence was re-written as follows.

"we estimated the total AOU changes in the respective layers by using the linear trends obtained in the core AAB for

the AAB and by using those obtained along the WOCE/GO-SHIP sections for the SAB (Fig. 6)."

Line 169 – phrase as … 'and weakening in the densest portion of the LMOC'.

Authors: Thank you for pointing this out. The sentence was re-written as follows.

"We examined the intensified (weakened) ventilation in the L-LMOC (D-LMOC)."

Line 179-181- Adding 2-3 lines explaining in greater detail some of the expected implications from either modeling or theoretical perspective would help add context to these results about the observed shoaling in the ventilating layer.

Authors: Thank you for pointing this out. We agree that adding detailed explanation for expected implications would benefit the readers. We added following sentences.

"Especially, long-term sink of anthropogenic carbon dioxide, which is an important aspect of AABW formation, is concerned to be weakened because shoaling should also occur in anthropogenic carbon transport path."

1st Dec 21

Dear Dr Shimada,

Your revised manuscript titled "Shoaling of abyssal ventilation in the in the Eastern Indian Sector of the Southern Ocean" has now been seen by our original 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are still raised. We continue to be interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

Specifically, for publication in Communications Earth & Environment to be appropriate, we will need you to revise the manuscript to improve clarity and readability while lessening the reliance on supplementary information. The latter can be achieved by integrating Supplementary Information text and selected Figures into the main article within the limits of 5,000 words, 10 display items, and unlimited space in the Methods. We also need you to explain plainly how the estimates of required changes in deep shelf water supply, which involves the decomposition of the effects of the meridional shift of the Antarctic Circumpolar Current fronts and water-mass property changes, are obtained considering the reviewer comments. We also encourage you to reduce the number of acronyms particularly those not mentioned as often, such as “SACCF” and “AAD”.

We invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees’ comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren’t able to submit it within this time so that we can discuss how best to proceed. If we don’t hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these

revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Regina Rodrigues, PhD
Communications Earth & Environment
orcid.org/0000-0001-8010-4018

Heike Langenberg, PhD
Chief Editor
Communications Earth

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of the MS
Shoaling of abyssal ventilation in the Eastern Indian Sector of the Southern Ocean by Shimada et al.
This is the second revision of the MS by Shimada and colleagues. The present work investigates changes in water mass properties in the deep and abyssal layers of the southeastern Indian Ocean portion of the Southern Ocean, its impacts on the sea level rise, and possible drivers of these changes. The study is based on hydrographic in situ observations of temperature, salinity, and dissolved oxygen from GO-SHIP/WOCE sections and Japanese occupations. The new results suggest a decline in Antarctic Bottom Water (AABW) supply since 1995 and an increase in ventilation of the immediate-above layer (28.02 to 28.19 kg/m3), which the authors referred to as the less dense layer of the lower limb of the meridional overturning circulation L-LMOC. These changes may have implications, for instance, heat and carbon transport and storage-- variables intrinsically connected with the Earth's climate.

The current MS reads much better than the original version, and I congratulate the author's effort in such a major revision. Despite the effort, the MS largely relies on supplementary materials. Normally, supplementary material is not something the readers must read to understand the paper content, as it is in this case. However, I am not sure if this issue is due to the journal format. I will let this task be for the editors.
A major comment I have is on the part the authors describe the decomposition of the effects of the meridional shift of the ACC fronts and water-mass property changes (L#122-131). The text is overall confusing, and I could not follow it. In multiple instances, the authors say 'meridional gradient,' but meridional gradient alone does not mean much as we need to know of which quantity. Thus, the

methodology is not reproducible. Because one of the key findings of the paper is based on the results of employing methodology, I don't have the confidence to evaluate the results and the conclusion based on them. I suggest re-write this part paying attention to details. For instance, 'then the multidecadal trends were modified to reveal the water mass change.' How were the trends modified?

In addition, the MS still have an excessive number of acronyms which make reading certain passages very hard as the readers need to remember all defined acronyms (there are ACC, AAB, SAB, AAD, AOU, AABW, LMOC, MOC, L-LMOC, D-LMOC, SACCF, SLR to cite a few). I would suggest using fewer acronyms to improve the readability. For instance, there is no need to use a long acronym for the southern ACC front (SACCF) or define an acronym for Australian-Antarctic Discordance (AAD, L#144) as most times the authors use (as they should) Australian-Antarctic Discordance.
Line by line comments

L#1: 'in the' is typed twice in the title
L#65: It would enrich the paper if the authors show how much the ACC front has shifted over the period analyzed in Fig. 1
L#68-69: I understood that these are the results of the study; thus, past tense would be more appropriate "has weakened …. has intensified."
L#108: I didn't find a Fig. 6K in the manuscript. Please check.
L#114: AAD acronym is only used here. No need to be defined
L#136: add meridional shift 'of the southern ACC front'
L#132-141: While the reasoning for the warming is given, nothing is said about the freshening. If I read the results correctly, all the freshening in both layers could be interpreted as due to water-mass properties changes. The frontal shift would be causing salinification as the results show> 100% contribution for the water-mass property changes. Is that right?
L#143: Could the authors point for a figure where the readers could see that the ACC fronts didn't change in the SAB?
L#145: 'has' instead of 'is'
L#168-175: I did not understand where is the 'inter-basin connection at the boundary.' Are the authors talking about the southeast Indian Ridge? As boundary is used multiple times after that, I was confused by the jargon. For instance, 'for the layers above this boundary' (L#172) would it means for the layers above the southeast Indian Ridge depths? What does 'below the boundary' (L#173) mean? I believe this paragraph (L#168-175) could be re-written straightforwardly.
Fig 1 caption: add (practical) salinity (L#408). Are the blue and green curves time-average positions of the AAC fronts? If yes, add the info to the caption with the respective period.
Fig 3: in the x-axis, 92 and 93 are displayed together 9293. Consider omitting 92.
Fig 4: What are the maroon curves? In some subplots, the maroon curve is thick, and in others thin. I have no idea what they mean.

Reviewer #2 (Remarks to the Author):

I appreciate the clear response by the authors and the reorganization of the paper structure. The manuscript is now clearer (though some improvement is still needed to my opinion) and its structure much improved. After moderate modifications, I think this manuscript would be an excellent addition to COMMSENV. I have one major comment (1) regarding the science, about the estimates of required changes in DSW supply, which I am dubious about at this point. If the authors can clarify

this point, then I will have no further reservations about publications of these results. My other major comment (2) is about the writing, which still can be made better, although there have already been large improvements.

Major comment:

1. It seems to me that there is still an important missing piece in the puzzle, with regards to increased ventilation of the L-LMOC. The authors estimated that +0.69 Sv of DSW supply is required for explaining the AOU changes in the study region. This is quite a considerable amount, especially when considering that the decrease in DSW supply in the abyssal region is only -0.13 Sv.

- First, Intuitively, shouldn't we expect the mid-depth intensification and deep weakening of DSW supply to be balanced? At this point, we have a net increase in DSW production by 0.56 Sv. This seems unlikely.
- Second, I think that the authors need to clarify what the change in DSW supply means. My understanding is that the DSW would be freshening (as mentioned in L217-218), therefore it would become less dense, and instead of flowing all the way to the abyssal depths to form AABW, it would flow down to the shallower layer in the L-LMOC. Is this what the authors imply? If so, could they explain this clearly in the manuscript? Because if confirmed, that is a finding very much worth mentioning.
- Third, if the above is correct, then how much density loss of DSW is needed for DSW to flow down to the L-LMOC rather than to the D-LMOC? For downflowing DSW to reach equilibrium to less than 28.19 (lower limit of L-LMOC) instead of more than 28.32 kg.m-3 (upper limit of D-LMOC) would require a considerable change in DSW density, possibly in the order of -0.5 kg.m-3. Therefore,

o Can the authors provide an estimate of the required change in DSW density for reaching +0.69 Sv in added supply?

o An alternative way could be to estimate the sensitivity of equation (3) to a change in the density of DSW, from 28.74 kg.m-3 to (for example) 28.64 kg.m-3, 28.54 kg.m-3, etc. Then the authors can draw their conclusions from the low or high sensitivity.

o (optional) Can the authors, using historical/instrumented seal data provide a rough estimate of the observed changes in DSW properties over the past 2 decades?

o If the required change in DSW density seems too large to be true, can the authors provide some sort of correction to the +0.69 Sv change in DSW supply for the L-LMOC layer? Given the large uncertainty in this number (+-0.65 Sv) and the smaller amount of DSW supply decrease in the D-LMOC (-0.13 Sv), I would suggest that + 0.69 Sv is a non-negligible overestimation. In this case, maybe the alternative estimate in L283-287 would make more sense and could very well replace the -0.13 Sv, +0.69 Sv estimates in the main text. This would also make more sense in terms of balance between the reduced and increased DSW supply.

2. I would not have added this comment if the manuscript had been submitted to a journal specialized in physical oceanography. However, since COMMSENV readership is not restricted to physical oceanographers or oceanographers in general, I think that the writing can still be improved for clarity. The aim is to tell a good story in a compelling way, which may also attract non-oceanographers, or at least non-physical oceanographers to read it. It is difficult to provide concrete examples of how the writing can be improved, but the guideline would simply to make sure that the story is understandable and easy to follow by a marine biologist, a glaciologist, or a specialist in atmospheric science. Reading through the manuscript, I think that it becomes clearer from the chapter "Signals of intensified ventilation in less dense layers of LMOC" (L146) until the end. Conversely, from L70 to L146, the reading is still a little hard to follow at times. The manuscript could really become excellent with an extra effort on clear writing from beginning to end.

Minor comments:
L47: Please remove “that” after “Because”.
L47-48: Maybe there is a more simple way to express this, such as “Because warming in the dense shelf water (DSW), which is the source water of AABW, is weak, rapid freshening of DSW is the likely cause for the weakened AABW supply”.
L67-68: “ to be weakened” and “to be intensified”. Shouldn’t it be “has weakened” and “has intensified”?
L89 and after: I think that mention of “total steric SLR” is confusing. I think the authors should talk about “total SLR” (=including both steric and non-steric components) and “steric SLR”, to avoid confusion.
L92: “which were omitted”. Rigorously, it is not that the barostatic component is “omitted” in the steric calculation, it is that it “cannot be estimated”.
L101: Consider changing “significantly evident” with “more evident”.
L104-108: In this case, it is appropriate to put this part in the Appendix, because this part does not contribute directly to the storytelling. Consider putting the following sentence at L103 “Overall, the multi-decadal trends estimated above are consistent with those of previous studies.” With a few citations after “previous studies”, and then something like “ (the reader is referred to Supplementary Section S2 for more details.)”.
L113: To avoid confusion, I would advise to change this sentence to “with slightly less dense water (~28.22 kg/m3) transportED from the AAB to the SAB through the Australian-Antarctic Discordance (AAD)22, …”.
L137: It would be good to explain that in this case, AOU increase indicates less ventilation (because less dissolved oxygen is available, and in proportion, more is then utilized) and decrease indicates higher ventilation.
L139: “Please remove “, it” before is shown. That being said “is shown to be increased” is verbose. Please consider rephrasing in a clearer way.
L140: I am not 100% sure, but I do not think that the grammar from “that … to be highest” is a proper use of English.
L140-141: Actually, I do not understand how the authors reach this conclusion since the study focuses on the AAB and SAB only. Could the authors elaborate? Or remove the statement if there is no sufficient justification.
L180: I think it should be “partly owing to compensation for the reduced AABW supply”. That being said, I think this sentence is a little convoluted and can be improved. Consider rephrasing.
L175-195: I think those explanations are satisfactory. However, I wonder that there could be a clearer way to present this. In this case, the authors first explain the results of AOU, and then those of salinity (and potential temperature). In both cases, they explain these results by the possible intensification of the ventilation in the L-LMOC, which may seem a bit repetitive.
Another way to do it would be to start by presenting the hypothesis that ventilation has increased in the L-LMOC, and then use the results in salinity and AOU as support elements for this theory. In this way, I suppose the whole explanation can hold within 1 rather than 2 paragraphs and be clearer and more compelling. But I let it to the authors to decide which way seems better for them.
L196: Shouldn’t it be “To simply describe THE magnitude of the intensified, etc.”. I am not a native English speaker, but it seems to me that the use of articles (or its absence) is a bit off in some cases. Could the authors go through the manuscript for minor grammar mistakes, or get it checked by an English editing company?
L199: Consider rephrasing by avoiding the repetition of “changes in DSW supply”.

Reviewer #3 (Remarks to the Author):

The authors have substantially revised the contents of the manuscript and addressed all of my comments and suggestion made to the previous iteration of the manuscript. I recommend the manuscript for publication.

Response to reviewer #1:

We thank the reviewer for thorough reading of our manuscript and positive assessment with very constructive comments. All your points helped us a lot, especially on increasing understandability of the manuscript. We believe that it was impossible to improve our manuscript without them.

Our response follows, one by one, the list of the reviewer's comments.

Major comments

A major comment I have is on the part the authors describe the decomposition of the effects of the meridional shift of the ACC fronts and water-mass property changes (L#122-131). The text is overall confusing, and I could not follow it. In multiple instances, the authors say 'meridional gradient,' but meridional gradient alone does not mean much as we need to know of which quantity. Thus, the methodology is not reproducible. Because one of the key findings of the paper is based on the results of employing methodology, I don't have the confidence to evaluate the results and the conclusion based on them. I suggest re-write this part paying attention to details. For instance, 'then the multi decadal trends were modified to reveal the water mass change.' How were the trends modified?

> Authors:
> Thank you for pointing this out.
> We modified the manuscript so that methods for estimating multidecadal trends and methods for dividing them into meridional shift and water mass change component are given in the method section of the main text.

In addition, the MS still have an excessive number of acronyms which make reading certain passages very hard as the readers need to remember all defined acronyms (there are ACC, AAB, SAB, AAD, AOU, AABW, LMOC, MOC, L-LMOC, D-LMOC, SACCF, SLR to cite a few). I would suggest using fewer acronyms to improve the readability. For instance, there is no need to use a long acronym for the southern ACC front (SACCF) or define an acronym for Australian-Antarctic Discordance (AAD, L#144) as most times the authors use (as they should) Australian-Antarctic Discordance.

> Authors:
> Thank you for pointing this out.
> After consideration, we removed acronyms "AAD", and "CDW". We also modified acronyms "L-LMOC" and "D-LMOC" to "less dense LMOC" and "densest LMOC", respectively.

Line by line comments

L#1: 'in the' is typed twice in the title

> Authors:
> Thank you for pointing this out. 'in the' is deleted, accordingly.

L#65: It would enrich the paper if the authors show how much the ACC front has shifted over the period analyzed in Fig. 1

> Authors:

Thank your pointing this out.

We modified the sentences in L. 64-66 so that they include meridional shift during the observed periods spanning 25 years (55±30 km).

L#68-69: I understood that these are the results of the study; thus, past tense would be more appropriate "has weakened …. has intensified."

Authors:

Thank you for pointing this out.

Past tense was adopted to the relevant part, accordingly.

L#108: I didn't find a Fig. 6K in the manuscript. Please check.

Authors:

Thank you for pointing this out.

We intended to refer fig. 6K of ref. 1 (Purkey and Johnson, 2013) and the sentence was modified accordingly.

Please also note the relevant paragraph was moved to method section, as it does not contribute directly to the story.

L#114: AAD acronym is only used here. No need to be defined

Authors:

Thank you for pointing this out. The acronym “AAD” is deleted, accordingly.

L#136: add meridional shift 'of the southern ACC front'

Authors: Thank you for pointing this out.

We added ’ of the southern ACC front’, accordingly.

L#132-141: While the reasoning for the warming is given, nothing is said about the freshening. If I read the results correctly, all the freshening in both layers could be interpreted as due to water-mass properties changes. The frontal shift would be causing salinification as the results show> 100% contribution for the water-mass property changes. Is that right?

Authors:

This is reasonable.

We added following sentences in the revised manuscript, accordingly.

“Based on positive meridional gradient in salinity, southward shift of the SACCF would cause salinification and hence, observed freshening is due to water mass change component; namely, freshening in the source regions and waters.”

L#143: Could the authors point for a figure where the readers could see that the ACC fronts didn't change in the SAB?

Authors:

Thank you for pointing this out.

The discussion regarding southward shift of ACC fronts in the SAB is given in Supplementary Section S5.

Hence, we modified the sentence so that this point to be clear.

L#145: 'has' instead of 'is'

Authors:

We modified the sentence accordingly.

L#168-175: I did not understand where is the 'inter-basin connection at the boundary.' Are the authors talking about the southeast Indian Ridge? As boundary is used multiple times after that, I was confused by the jargon. For instance, 'for the layers above this boundary' (L#172) would it means for the layers above the southeast Indian Ridge depths? What does 'below the boundary' (L#173) mean? I believe this paragraph (L#168-175) could be re-written straight forwardly.

Authors:

Thank you for pointing this out.

We used the word “boundary” for isopycnal of 28.19 kg/m^3.

In addition to replacing “boundary” by “isopycnal”, we modified this paragraph substantially to increase clarity.

Fig 1 caption: add (practical) salinity (L#408). Are the blue and green curves time-average positions of the AAC fronts? If yes, add the info to the caption with the respective period.

Authors:

Thank you for pointing this out.

We added “practical” before salinity.

The blue and green curves indicate climatological position of the ACC fronts estimated using data obtained prior to the year 2013, as described by Shimada et al. 2017 (ref. 33). This point is implemented in the revised manuscript.

Fig 3: in the x-axis, 92 and 93 are displayed together 9293. Consider omitting 92.

Authors:

Thank you for pointing this out.

We removed “92” from x-axis of Fig. 3, accordingly.

Fig 4: What are the maroon curves? In some subplots, the maroon curve is thick, and in others thin. I have no idea what they mean.

Authors:

Thank you for pointing this out.

Thich black and red lines indicated the observed and water-mass change components of the linear trends, respectively. We used thin curves to indicated higher (lower) boundary of 95% confidence intervals for both black and red plots. We deleted the thin curves because they contain no additional information more than the shaded areas. We also modified

the figure caption, accordingly.

Response to reviewer #2:

We thank the reviewer for thorough reading of our manuscript and positive assessment with very constructive comments. All your points helped us a lot, especially on increasing understandability of the manuscript. We believe that it was impossible to improve our manuscript without them.

Major comment:

1. It seems to me that there is still an important missing piece in the puzzle, with regards to increased ventilation of the L-LMOC. The authors estimated that +0.69 Sv of DSW supply is required for explaining the AOU changes in the study region. This is quite a considerable amount, especially when considering that the decrease in DSW supply in the abyssal region is only -0.13 Sv.

Authors:

Thank you for pointing this out.

We agree that the gap between increased ventilation of the L-LMOC ($+0.69 \pm 0.65$Sv) and decrease in DSW supply in the D-LMOC (-0.13 ± 0.23Sv) is quite considerable and likely unrealistic.

We consider that the gap derives from ambiguity in estimation for the AAB contribution. Namely, the estimation may be influenced by interpolation error induced by applying the results derived from the core AAB, which is relatively small area in the AAB, to the overall AAB (a currently adopted estimate which gives $+0.69 \pm 0.65$Sv for DSW supply in L-LMOC; Table S2a) and by the errors induced by south ward shift in ACC (an alternative estimate which gives $+0.11 \pm 0.59$ Sv for DSW supply in L-LMOC; Table S2b).

While alternative estimate is moderate and may be realistic on point of balance between the increased/decreased ventilation of the L-LMOC/D-LMOC, validity of the estimations should also be assessed based on methodologies. Having no such evidence at current stage, we would like to propose adopting the averaged value of two estimates, which would also give a moderate estimate.

- First, Intuitively, shouldn't we expect the mid-depth intensification and deep weakening of DSW supply to be balanced? At this point, we have a net increase in DSW production by 0.56 Sv. This seems unlikely.

Authors:

It is reasonable to consider that mid-depth intensification and deep weakening of DSW supply to be balanced. A net increase in DSW supply is implied for the layer below LCDW when the mid-depth intensification is larger than deep weakening. Under rapid freshening and density decrease in DSW, this increase is not reasonable.

- Second, I think that the authors need to clarify what the change in DSW supply means. My understanding is that the DSW would be freshening (as mentioned in L217-218), therefore it would become less dense, and instead of flowing all the way to the abyssal depths to form AABW, it would flow down to the shallower layer in the L-LMOC. Is this what the authors imply? If so, could they explain this clearly in the manuscript? Because if confirmed, that is a finding very much worth

mentioning.

Authors:
Thank you for suggesting straightforward expression. We implemented the expression in the end of the introduction section.

- Third, if the above is correct, then how much density loss of DSW is needed for DSW to flow down to the L-LMOC rather than to the D-LMOC? For downflowing DSW to reach equilibrium to less than 28.19 (lower limit of L-LMOC) instead of more than 28.32 kg.m-3 (upper limit of D-LMOC) would require a considerable change in DSW density, possibly in the order of -0.5 kg.m-3.

Therefore,
o Can the authors provide an estimate of the required change in DSW density for reaching +0.69 Sv in added supply?

Authors:
Change in DSW density will also change mixing rate with ambient water masses (e.g., Smith, 1975). Hence, we consider that a model which reproduces realistic mixing under realistic geographical settings, which also influence mixing, would be required for commented purpose. While we recognize importance of clarifying the offshore response to change in DSW density, we consider that such model study is beyond the scope of this manuscript and left for future work.

Smith, P. A streamtube model for bottom boundary currents in the ocean. Deep Sea Res., Oceanogr. Abstr., 22, 12, doi:10.1016/0011-7471(75)90088-1 (1975).

o An alternative way could be to estimate the sensitivity of equation (3) to a change in the density of DSW, from 28.74 kg.m-3 to (for example) 28.64 kg.m-3, 28.54 kg.m-3, etc. Then the authors can draw their conclusions from the low or high sensitivity.

Authors:
Thank you for providing an alternative way.
However, change in density from 28.74 to 28.64 and to 28.54 kg/m3 correspond to density change of 0.01% [(1028.74-1028.64)/1028.74] and 0.02 % [(1028.74-1028.54)/1028.74], respectively. Therefore, we consider that sensitivity of equation (3, equation 5 in revised manuscript) to density change of DSW is negligible.

o (optional) Can the authors, using historical/instrumented seal data provide a rough estimate of the observed changes in DSW properties over the past 2 decades?

Authors:
HSSW in the Ross Sea and DSW in the Mertz Polynya region, which are the major DSW source for the Australian Antarctic Basin, are relatively well studied.
As for HSSW, freshening rate of －0.03 PSS-78/decade with relatively constant temperature is obtained for the period before 2010 (Jacobs and Giulivi, 2010[12]). As for DSW in the Mertz Polynya region, rapid salinity drop after 2008 is

confirmed (Shadwick et al., 2013[10]). Salinity changes in both DSWs are roughly 0.1 PSS-78 and would induce density decrease of -0.11 kg/m^3.

o If the required change in DSW density seems too large to be true, can the authors provide some sort of correction to the +0.69 Sv change in DSW supply for the L-LMOC layer? Given the large uncertainty in this number (+-0.65 Sv) and the smaller amount of DSW supply decrease in the D-LMOC (-0.13 Sv), I would suggest that + 0.69 Sv is a non-negligible overestimation. In this case, maybe the alternative estimate in L283-287 would make more sense and could very well replace the -0.13 Sv, +0.69 Sv estimates in the main text. This would also make more sense in terms of balance between the reduced and increased DSW supply.

Authors:
As we responded to the first comment, we agree that the gap between increased ventilation of the L-LMOC ($+0.69 \pm 0.65$Sv) and decrease in DSW supply in the abyssal region (-0.13 ± 0.23Sv) is quite considerable and likely unrealistic. Having no evidence to conclude which estimate is more realistic at current stage, however, we propose adopting the averaged value of two estimates, which would also give a moderate estimate. Averaged estimate will give values of $+0.40 \pm 0.62$ Sv for increased ventilation of the L-LMOC and -0.19 ± 0.25Sv for decrease in DSW supply in the D-LMOC.

Accordingly, we modified the relevant part in the Results and Discussion section (line 196-206 in the revised manuscript) and method section (line 290-317 in the revised manuscript).

2. I would not have added this comment if the manuscript had been submitted to a journal specialized in physical oceanography. However, since COMMSENV readership is not restricted to physical oceanographers or oceanographers in general, I think that the writing can still be improved for clarity. The aim is to tell a good story in a compelling way, which may also attract non-oceanographers, or at least non-physical oceanographers to read it. It is difficult to provide concrete examples of how the writing can be improved, but the guideline would simply to make sure that the story is understandable and easy to follow by a marine biologist, a glaciologist, or a specialist in atmospheric science.
Reading through the manuscript, I think that it becomes clearer from the chapter “Signals of intensified ventilation in less dense layers of LMOC” (L146) until the end. Conversely, from L70 to L146, the reading is still a little hard to follow at times. The manuscript could really become excellent with an extra effort on clear writing from beginning to end.

Authors:
Thank you for your advice.
By effort of authors and English editing company, we have modified the manuscript as much as possible to increase the clarity and understandability.

Minor comments:
L47: Please remove “that” after “Because”.

Authors: thank you for pointing this out.
We removed “that”, accordingly.

L47-48: Maybe there is a more simple way to express this, such as “Because warming in the dense shelf water (DSW), which is the source water of AABW, is weak, rapid freshening of DSW is the likely cause for the weakened AABW supply”.

Authors:
This is reasonable.
The sentence is corrected accordingly.

L67-68: “ to be weakened” and “to be intensified”. Shouldn’t it be “has weakened” and “has intensified”?

Authors:
Thank you for pointing this out.
The relevant parts were modified, accordingly.

L89 and after: I think that mention of “total steric SLR” is confusing. I think the authors should talk about “total SLR”(=including both steric and non-steric components) and “steric SLR”, to avoid confusion.

Authors:
Thank you for pointing this out.
We agree that “total steric SLR” in this context is confusing. We thus, replaced them by “steric SLR” throughout the manuscript.

L92: “which were omitted”. Rigorously, it is not that the barostatic component is “omitted” in the steric calculation, it is that it “cannot be estimated”.

Authors:
Thank you for pointing this out.
We agree that “omitted” in this context is not straightforward. We thus replaced it by “not included”.

L101: Consider changing “significantly evident” with “more evident”.

Authors:
This is reasonable.
We replaced “significantly evident” by “more evident”, accordingly.

L104-108: In this case, it is appropriate to put this part in the Appendix, because this part does not contribute directly to the storytelling. Consider putting the following sentence at L103 “Overall, the multi-decadal trends estimated above are consistent with those of previous studies.” With a few citations after “previous studies”, and then something like “(the reader is referred to Supplementary Section S2 for more details.)”.

Authors:
This is reasonable.
We moved the paragraph to method section (please note that relevant part which was in Supplementary Section S2 was

moved to method section) and following sentence added, instead.

"Overall, the SLR estimated above are consistent with those of previous studies [1, 5] (Method Section provide more details on comparison)."

L113: To avoid confusion, I would advise to change this sentence to "with slightly less dense water (~28.22 kg/m3) transported from the AAB to the SAB through the Australian-Antarctic Discordance (AAD)22, …".

Authors:
Thank you for pointing this out.
We consider that the sentence was too long, and our point did not convey well. We divided the sentence as follows.

"The densest portion of the LMOC in the study area consists of AABW (> 28.30 kg/m^3) supply to the AAB and slightly less dense water (~28.22 kg/m^3) transport from the AAB to the SAB through the Australian-Antarctic Discordance [22], which is a relatively narrow deep zone in the Southeast Indian Ridge between 120 and 128° E (Fig. 1a). The major AABW sources supplying the AAB are the Ross Sea[15], Adélie/George V Land coast[18-20], and Vincennes Bay[21]."

L137: It would be good to explain that in this case, AOU increase indicates less ventilation (because less dissolved oxygen is available, and in proportion, more is then utilized) and decrease indicates higher ventilation.

Authors:
Thank you for pointing this out.
We added explanation for meaning of AOU variation as follows.

(AOU; a parameter which is used as a tracer for ventilation[23] and its increase/decrease indicates less/higher ventilation)

L139: "Please remove ", it" before is shown. That being said "is shown to be increased" is verbose. Please consider rephrasing in a clearer way.

Authors:
Thank you for pointing this put.
We removed ", it", accordingly.

L140: I am not 100% sure, but I do not think that the grammar from "that … to be highest" is a proper use of English.

Authors:
Thank you for pointing this put.
Please see our response to the next comment for the revised sentence.

L140-141: Actually, I do not understand how the authors reach this conclusion since the study focuses on the AAB and SAB only. Could the authors elaborate? Or remove the statement if there is no sufficient justification.

Authors:

May be there was overstatement, as pointed out.

We thus modified the sentence as follows.

"Considering the contribution of multiple factors, conditions in the AAB are favourable for an increase in SLR."

L180: I think it should be "partly owing to compensation for the reduced AABW supply". That being said, I think this sentence is a little convoluted and can be improved. Consider rephrasing.

Authors:

Thank you for pointing this out.

We modified the sentence as follows.

"For the layers below the less dense LMOC of the SAB, AOU increased because signals of intensified ventilation were trapped in the AAB partly owing to compensation for the reduced AABW supply (please note that depth of lower boundary of less dense LMOC kept constant while top layer of AABW declined)."

L175-195: I think those explanations are satisfactory. However, I wonder that there could be a clearer way to present this. In this case, the authors first explain the results of AOU, and then those of salinity (and potential temperature). In both cases, they explain these results by the possible intensification of the ventilation in the L-LMOC, which may seem a bit repetitive. Another way to do it would be to start by presenting the hypothesis that ventilation has increased in the L-LMOC, and then use the results in salinity and AOU as support elements for this theory. In this way, I suppose the whole explanation can hold within 1 rather than 2 paragraphs and be clearer and more compelling. But I let it to the authors to decide which way seems better for them.

Authors:

Thank you for pointing this out.

In fact, similar explanations can be found in results for AOU and salinity (and potential temperature) and thus it would be better to be combined if possible.

However, there also are non-negligible differences between these parameters (e.g., freshening and warming in L-LMOC of the core-AAB are weak while AOU decrease in largest) and explanations would be convoluted if they are combined.

We thus leave these paragraphs as they were.

L196: Shouldn't it be "To simply describe THE magnitude of the intensified, etc.". I am not a native English speaker, but it seems to me that the use of articles (or its absence) is a bit off in some cases. Could the authors go through the manuscript for minor grammar mistakes, or get it checked by an English editing company?

Authors:

Thank you for pointing this out.

"the" is added before magnitude, accordingly.

Please also note that the manuscript is checked throughout by an English editing company.

L199: Consider rephrasing by avoiding the repetition of “changes in DSW supply”.

Authors:

Thank you for pointing this out.

We deleted phrase after “,” because it was logically repetition of preceding phrase.

Response to reviewer #3:

We thank the reviewer for thorough reading of our manuscript and positive assessment.

Reviewer’s comment.

The authors have substantially revised the contents of the manuscript and addressed all of my comments and suggestion made to the previous iteration of the manuscript. I recommend the manuscript for publication.

Authors:

Thank you for positive comment. Your comments truly encouraged us for improving our manuscript.