

Blue means comments have been addressed

We sincerely thank the editor and the reviewers for their attention to our manuscript and their helpful suggestions. We synthesize the reviews and summarize the overall areas where our manuscript needs work in the following list:

- We will improve the citations of prior literature and the presentation of these references in the introduction and discussions.
- We will clarify our method for partitioning the transport into two layers and better explain the cumulative nature of the streamfunction minimum as a metric of transport.
- We will decompose the surface-salinity-driven transformation into a sea-ice driven component and a component from direct E-P-R (as done in Abernathey et al. 2016).

In addition, we will make many smaller changes based on the reviewers' helpful suggestions.

Below is a point-by-point response to each reviewer comment.

RC1 (Anonymous Referee #1)

Major Comments

1. You conclude in the Discussion section that none of the reanalyses capture realistically AABW formation, export and variability due to their coarse resolution. This should be included in the abstract, before describing the loss of AABW found in SOSE, not at the end as a reason for no relationship with large-scale climate oscillations.

Agreed. Will include our finding that the reanalyses used did not realistically capture AABW formation, export and variability in the abstract, BEFORE mentioning the findings from each model.

2. It is not clear how AABW is defined in this paper. In line 287 defines it based on TS boundaries, in line 319 it is all the waters denser than CDW (without clarifying which density this is), and in line 364 there are different density criteria for each reanalysis. Since the paper is focusing on AABW, its definition should be clearer to the reader, with supporting references if it is based on prior studies, or adequate justification if it is a new definition for the purposes of this work.

Agreed. Will clearly state that the definition of the AABW depends on each model's annual mean overturning streamfunction (when the line switches signs, essentially); and this single value shall represent the range between the densest value up to the value at the boundary of the

overturning circulation demarcating deep and bottom water masses.

3. The study region is not defined. Defining the study region is vital for the reader to interpret the physical meaning behind inflows/outflows. For example, is the shelf region included? Is the northern boundary set by topography or an arbitrary latitude?

Agreed. Boundary will be explicitly defined by these coordinates [65°W, 30°E, 78°S, 62°S] (Will cite Gordon et al 2007, DOI: 10.1175/JCLI4046.1 for these boundaries)

4. The Introduction is devoted mostly to a description of the global MOC. I suggest shifting to a more regional focus that is more relevant to the paper. For example, what is already known about water-mass transformations and inflows/outflows of AABW. Some works that could be useful on this regard are:

This comment is a very similar comment to Céline Huezé, so we will condense the introduction to focus more on the study region and consider incorporating these suggested works into the text.

- a. Couldrey, M. P., Jullion, L., Naveira Garabato, A. C., Rye, C., Herráiz-Borreguero, L., Brown, P. J., ... & Speer, K. L. (2013). Remotely induced warming of Antarctic Bottom Water in the eastern Weddell gyre. *Geophysical Research Letters*, 40(11), 2755- 2760.
- b. Meredith, M. P., Gordon, A. L., Naveira Garabato, A. C., Abrahamsen, E. P., Huber, B. A., Jullion, L., & Venables, H. J. (2011). Synchronous intensification and warming of Antarctic Bottom Water outflow from the Weddell Gyre. *Geophysical Research Letters*, 38(3).
- c. Jullion, L., Naveira Garabato, A. C., Meredith, M. P., Holland, P. R., Courtois, P., & King, B. A. (2013). Decadal freshening of the Antarctic Bottom Water exported from the Weddell Sea. *Journal of Climate*, 26(20), 8111-8125.

We have already cited this work in the discussion.

Also, there is a lack of references in the Introduction and Discussion sections, including but not limited to:

- d. Line 28: "... forming Antarctic Bottom Water (AABW) [reference]"
We will cite Talley 2013.
- e. Line 31: "The global impacts of this circulation system on biological productivity and carbon and heat uptake [reference] ..."
We will cite Vernet et al 2019 and highlight circulation in the WG (study region) within the context of the MOC is worth studying.
- f. Lines 32 to 35.

We will cite ~~Talley 2013~~ Gordon 2020 and Vernet et al 2019.

g. Line 40: "... interior mixing [reference]"

We will cite Nikurashin and Vallis (2011,2012)

h. Line 46: "... cascading down continental boundaries [reference]"

Line 46 also comes from Williams, 2001. We will cite Williams once at the end of this sentence instead of the sentence prior in line 45.

i. Line 55: "... storage of large amounts of carbon [reference]"

We will cite Ito et al 2015:

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015GL064320>.

j. Line 73: "Even if physical processes such as coastal polynyas are not always represented accurately in ocean reanalysis [reference] ..."

Even though it is within this study's own experience with such reanalyses, we will find a reference to this statement.

And finally, please double check the relevance of citations. For example:

k. Line 24: Purkey and Johnson 2013, Vernet et al. 2019 are not relevant citations regarding the MOC's inter-hemispheric transport

Instead we will cite Talley 2013.

l. Line 463: Armitage et al. 2018 don't study AABW production/export

We will remove Armitage et al 2018 from line 463.

5. In the model assessment section, in order to make a fair comparison, please select the same time periods for model/observations. Your comparison could be biased because you are comparing, for example, bottom temperatures from ECCO for the period 1992 to 2015 with WOA for the period 1981 to 2010. This applies to the entire section.

This is a reasonable suggestion. However, not all of the reanalyses cover the climate normal period, so it is not possible. Thus we prefer to keep the comparison as is; however, we will add text noting the possible issues with this comparison.

6. It's not clear to me that bottom properties in the reanalysis products are similar to the observed. What does this mean for the reliability of AABW production and representation in reanalyses?

No direct observational estimates of AABW production exist. But direct observations of bottom water production do. In this section, we are using bottom water properties as a metric (clearly imperfect) for overall model accuracy. None of the reanalysis products provides a formal quantification

of estimate uncertainty, so we use the model standard deviation instead.

7. Lines 318-325: it is confusing the grouping of CDW/WSDW. Did you mean CDW/WDW? Warm Deep Water is the local term for CDW in the Weddell region, whereas Weddell Sea Deep Water is a different water mass altogether, with a different origin and characteristics.

That is a typo, thank you!

8. In the same paragraph, you cite export values from Figure 5. To get export values, shouldn't you get the cumulative sum for the density range corresponding to your AABW definition? If I interpreted correctly, that minimum is the cumulative sum of export of all the lighter densities, and is in fact excluding export from denser waters. And how are you obtaining the transformation values afterwards?

The other reviewer had similar questions, so clearly we need to improve the description of our method. Our budget coarsely separates all water into two classes: bottom water (denser than σ^{cross}) and everything else (by volume, mostly CDW and its regional variants; thus the name "deep"). Due to volume conservation, in this construction, the transport of bottom water across the basin boundary is equal and opposite to the transport of deep water. The dividing isopycnal σ^{cross} is *defined* via the minimum of the overturning streamfunction. So one single transport value represents both the deep outflow and the equal-and-opposite inflow.

9. Line 326: Kerr et al (2012) obtain this value from a transect at the tip of the Antarctic Peninsula. Your calculation encompasses outflows for the entire region, so I'm not sure it is a valid comparison. This comparison is repeated in other places in the text.

We acknowledge that the locations differ. However, Kerr does attempt to capture the dominant outflow of the Weddell sea AABW. We think it is important to attempt to compare with observations somehow. In our revision, we will note this caveat in the discussion of Kerr.

10. Paragraph starting in line 330: you are describing Figure 7 and your conclusion is that both models agree with past studies. However, I look at the Figure and ECCO and SOSE look completely different. They show different water mass transformations (different signs for the same density ranges). And again, how are you obtaining the precise values for transformation of bottom waters?

In our revision, we will modify this language to be more nuanced, rather than just saying "agree". (I assume you're meaning Fig 6.) What we mean is that the different

components of transformation in each model align qualitatively with established understanding of how the circulation works. Each component of transformation (surface salt, surface temp, and mixing) has the correct sign (i.e. mixing acts to lighten the wm, brine rejection acts to increase AABW, etc). However, it is not possible to make a *quantitative* comparison of individual components, since no such prior calculations have been published; that is part of the novelty of this study.

11. In a couple of occasions, you mention that the salinity driven WMT is due to brine rejection, with no influence of ice melt, runoff or precip (such as line 357). How do you verify that?

Similar comment to Céline's major comment and specific comment #30. Similar response here: in our revised manuscript, we will quantify the role of sea ice vs. evaporation / precipitation / runoff using model diagnostics to decompose the surface salinity contribution to transformation.

12. In line 363 you define CDW as the inflowing water and AABW as the outflowing waters. There are several studies that show that there is significant inflow of AABW into the region. Have you verified how sensitive your results are on this definition?

Our transport values are the net sum of all transport across the boundary, including both inflow and outflow, for a given density range. We only choose the separatrix density based on the inflow / outflow criterion. We will clarify this in our revision.

13. Also, if I interpret your figure correctly, the time tendency in SOSE is <0 year-round. Wouldn't this mean that there is a loss of volume throughout the year? This contradicts your statement in line 368.

Yes, that is the correct interpretation of Fig. 7. We agree that the statement in line 368 needs to be revised. The seasonal cycle in SOSE includes a large negative offset due to the time-mean volume loss.

14. Are the correlations between terms and the correlations between climate indices significant? With what confidence level?

In our revised manuscript, we will include confidence levels for these reported correlation values.

Update: updated fig 12 with error bars and included p values and confidence intervals as well.

15. In the Discussion section you mention discrepancies with mooring records. Please cite them and describe how your analysis is different.

Mooring records were cited in line 496 and mentioned again in line 536. In our revision, we will make these citations more prominent and collocated with relevant discussion in the text. Our analysis is very different from those papers because it focuses on reanalysis products, not direct observations. 1:1 comparison between moorings and reanalysis is difficult because of the highly local nature of mooring observations.

Minor Comments (general)

1. Correct figure captions throughout the text. For example: add units in Figure 1 and 2, add description of density contours in Figure 3 and 4 as well as labels for panels.

Agreed. We will add units to Figure 1 and 2 ($^{\circ}\text{C}$ and psu, respectively) and to Figure 5 (kg m^{-3}); and will add description of sigma2 density contours in Figs 3 and 4.

2. Please use the same axis for different panels. For example, Figures 5 to 8. Figure 10 is ok since it would make visualizing difficult.

We will not change - x axis label was left to be only at the bottom to make comparative visuals easier for the reader.

3. Sections 4 to 5 read a bit confusing. A suggestion for improvement is to try to make paragraphs focused on just one topic, which could be, for instance, describing one figure. Avoid one-sentence paragraphs as in line 326 and 460. Lastly, paragraphs in the Seasonal Climatology section could be organised better: you go from describing Figure 7 for all 3 models, to a paragraph each describing ECCO and SOSE in Figure 8 and 9, back to SODA in Figure 7 for the last paragraph.

We will focus on each section to be clearer. The purpose of inserting a paragraph about ECCO and SOSE before going back to SODA in Fig 7 is because we are describing each model in the order displayed in Fig 7; and Figs 8 & 9 give a deeper look into ECCO and SOSE.

Update: Did not change text order of Sects 4.1 and 5. The lead author found the analysis in the text in a good order for the reader to digest the figures. Changed order of text in Sect. 4.2.

4. There is no need to completely describe a figure in the text since it is already in the captions. For example, from line 310 onwards you describe all components of Figure 6 and 7, when you could limit the paragraph to (for example): The time evolution of the cumulative water mass volume distribution is not completely balanced by the total inflow/outflows and mean transformations, indicating that the system is not in steady state balance but is subject to low frequency variability or model drift".

Will get rid of text from 310-316 and follow suggestions.

5. Avoid subjective judgements. For example: line 359: "It is interesting to note"; line 447: "it gets worse, not better"; line 505: "take these correlations with a grain of salt".

Will adjust the statements in line 447 to: "...and this trend ~~persists~~ amplifies..."; and line 505 to: "However, SOSE is very short, so these correlations are not definitive."

Minor Comments (line based)

1. Line 13: add period where SOSE shows loss of AABW.

"...loss of AABW during 2005-2010, driven..."

2. Line 25: "... transported to the northern hemisphere ..."

Will add suggestion to line.

3. Add 1000m isobath in Figures 1 and 2 to aid in visualization.

Yes we will do this in our revision.

4. Consider changing salinity colorbar range or colormap to better visualize the region's bottom salinity, Figure 2.

We will use a better colormap in our revision.

5. Add delimiting lines of AABW according to working definitions in the TS diagrams, Figures 3 and 4; as well as in Figures 5 and 6.

We will have this range highlighted with a box in figs 3 and 4, and we will add delimiting lines of AABW in Figs 5 and 6.

6. Change units in all figures from [x] to (x).

Will change unit brackets.

7. Figure 9: change colorbar label from Sverdrup (m³/s) to Transport (Sv).

Will change label to Transport (Sv).

8. Number figures in order of appearance in the text (Figure 9 is described before than Figures 6 and 7)

Will reorder the text to mention Figure 9 after explaining the layout of the figures 7 & 8 to be similar to that of Figs 5 & 6.

Update: Instead we just switched the order of figs 8 & 9 and kept the mentions in the text the same.

9. Line 275: the region you are indicating is not beneath the Filchner-Ronne Ice Shelf (I think none of these reanalyses include the FRIS).

Thank you for catching that. We will adjust the text accordingly since the ice shelf ends more south than where the salt plumes are appearing.

10. Line 351: why do you refer to psi (inflow/outflow from the region) to overturning?

Psi quantifies the strength of the overturning.

11. Line 383: why tertiary and not secondary?

Because its magnitude is less than the mixing component of transformation.

12. Line 478: change wording. For example, "there is little agreement between reanalysis products regarding variability in seasonal to interannual timescales"

Will change to: "...there is little agreement between these reanalysis products on interannual timescales".

13. There are several syntax errors throughout the text, including but not limited to:

Will fix syntax errors.

- a. Line 438: Fig. 2 is not the one you wanted to reference right?

Fig. 5!

- b. Line 439: forgot parenthesis closure

Will close parenthesis.

- c. Line 457: and instead of AND

Will change to "and".

- d. Line 465: sea-ice concentration (SIC).

Change abbreviation to "(SIC)".

- e. Line 522: starts with Purkey and Johnson (2012) and ends with (Hellmer et al 2011).

"In Hellmer et al. (2011), it is..."

14. Correct citations throughout the text. For example, line 549: (e.g. Small et al (2014); Morrison et al. (2016); Kiss et al. (2020)) should be (e.g. Small et al. 2014; Morrison et al. 2016; Kiss et al. 2020). This is probably a latex typo, please check throughout the text.

We will correct citations where multiple papers are cited.

15. Similar to the comment above, verify throughout the text the figures you are referencing. For example, in line 517 you reference Fig. 2, but I don't think that is the figure you wanted to refer to. If so, I don't see how in bottom salinity you can identify loss of AABW properties.

We meant Fig. 5; we will double check each fig referenced in text.

16. Check text for unclosed parenthesis (I found several).

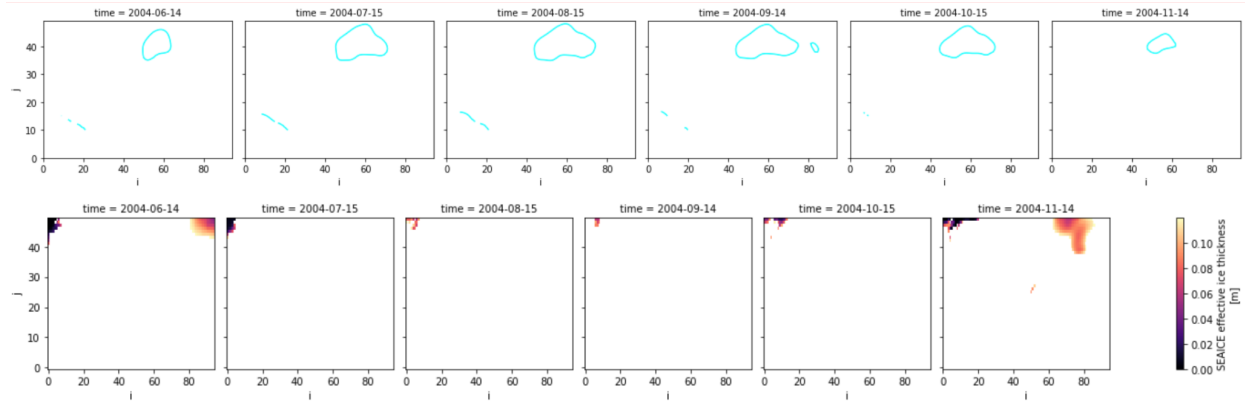
Will do, thank you!

RC2 (Céline Heuzé)

General Comments

My first issue is your definition of polynyas. [Disclaimer: The following contains references to papers from my team. I am not mentioning them to pressure you into citing me. These are the studies I know of that illustrate the point I am about to make.] On several occasions in the text, you discuss how some signals look like polynya signatures. I would add that the surface salinity flux of SOSE on Fig 9 suggests this further. The issue I have is that you rule out polynyas based on sea ice concentration. Globally, and especially so over Maud Rise, there can be polynyas with a near-100% sea ice concentration... but with a very thin ice. That's why many polynya detection algorithms use a thickness threshold instead of a concentration one. A threshold of 12 cm is standard; see e.g. the latest Nakata et al. (doi: 10.1029/2020GL091353) for coastal polynyas, and Mohrmann et al. 2021 (doi:10.5194/tc-15-4281-2021) for all types of Antarctic polynyas, in models and in observations. That is particularly crucial because there was some halo / small polynya activity in the region in 2004 and 2005 (table B1 of Heuzé et al. 2021, doi: 10.5194/tc-15-3401-2021). I would therefore like to see you redo your "polynya analysis" with the sea ice thickness instead of the concentration, and if the thickness falls below the threshold or simply decreases, rewrite your discussions accordingly. An extra (supplementary?) figure to compare the sea ice in all three models would be most welcome. If they assimilate sea ice instead, do describe the data source, frequency, whether they even assimilate thickness or only concentration, etc.

We did preliminary "polynya analysis" with ECCO's sea ice thickness diagnostic and defined the threshold as suggested (0.12m). We found no link between the second type of polynya (below the threshold thickness) and the AABW density contour outcropping between 2004-2007. Below we have attached the first year of the outcropping (2004). Similar patterns are observed for the latter years.



We will conduct a similar analysis with SOSE's diagnostic for the year 2005 to see if it agrees with Huezé et al. 2021's finding. Also, to address this major comment and a specific one further down (#30), we will examine the role of sea ice vs. evaporation / precipitation / runoff using model diagnostics to decompose the surface salinity contribution to transformation.

Update: We conducted similar analysis with SOSE and found minimal contribution of E-P-R to surface salt fluxes, and dominant brine rejection activity throughout most of the season until summer when there were spikes in ice melt during a few summer months (amplitude usually matching the highest brine rejection spike). The new manuscript includes several descriptions of this new analysis in relevant locations (beginning in lines 164 and 346, for polynya analysis line 445). We also found no link between the second type of polynya and the AABW density contour outcropping. These findings were included in the relevant sections where the text talks about possible polynya activity and considering sea ice as the only influence on transformation due to the surface salt fluxes.

My second issue is from line 365 onwards: I do not understand why you are conducting the analysis on sigma cross, the boundary between CDW and AABW, rather than on AABW itself. Especially since you say line 366 that you are using this level to study AABW. I would like to see a clearer explanation of why this particular level can be representative of AABW, and not, as I first expected, be where the signal is most dampened. I would also like to see that this choice is robust, either by providing a supplementary version of Figs 7, 8, 10 and 11 created at a denser level, or by adding "denser" lines to these figures.

The key point here is that, because the streamfunction is a cumulative integral quantity (eg. 8), we are not conducting an analysis "on sigma cross". This framework defines AABW transport

as the flow across the boundary of *all water denser than sigma cross*. Choosing a denser value would only damp the signal we are interested in, since the transport values decline monotonically as density increases from σ^{cross} (fig. 5). This was clearly not explained well in our manuscript, and in our revision we will clarify this important point.

Specific Comments

1. The introduction up till line 45 is quite repetitive. I would merge these paragraphs and keep only the key points, notably the very last line (first time you do not mention only air-sea interactions but also cryosphere-sea interactions).
Will condense introduction to focus more on the study region.
2. The paragraph line 53-59 is out of place. That should be among the very first things to write about, this overall “why should anyone who is not an oceanographer care?”. By this point, I would rather you explain why the seasonal to interannual variabilities are important to study, which processes they impact, etc.
Will consider integrating this paragraph to the one below or eliminate it altogether.
3. Line 89, for context, provide the depth range that you are looking at in this study (should also be mentioned in the introduction)
Our study uses density coordinates and consequently does not specifically address a depth range. In our revision, we will note the approximate depth ranges associated with our water masses.
4. Line 91-93: Discuss whether these limitations are problematic to study the Weddell Gyre region. Again, no pressure for citations, but Mohrmann et al. 2022 (doi:10.1029/2022GL098036) suspects that some mixing signals we see in observations in the region are the result of cabbeling and thermobaricity.
Thank you for pointing out this interesting new study. We will include this in the discussion in our revised manuscript. We note that we are not necessarily *neglecting* these thermodynamic processes; we are simply not explicitly diagnosing their contribution to WMT. They are lumped into the mixing terms.

5. Line 103: typo I suspect, vertical is diapycnal? Also, since you called this term G_h , I would write “horizontal” instead of “lateral” in the description, to help the readers.

We will change “lateral” to “horizontal”, and “isopycnal” to “diapycnal” for G_{vdiff} term.

6. Having the reanalyses introduced earlier would make section 2.1 easier to picture, to know which variables are available at which resolution, esp. vertical. Consider whether to swap sections 2 and 3.

While we appreciate this suggestion, we prefer to lead with the theory if that is okay.

7. Line 136 onwards: your cumulative sums, bottom up or surface down?

We use a bottom-up implementation. However, we also note that all results are insensitive to this choice, provided care is taken with the signs. The theoretical foundation for the cumsum is in eqs. 6 and 8, via the Heaviside function. One can always change the convention by multiplying everything by -1 .

8. Equation 14: double minus = typo?

Typo indeed! We will fix it.

9. Section 3: for all products, specify whether σ_2 is provided or whether you had to compute it, and if so, how.

σ_2 was computed using a python package called JMD95. When introducing σ_2 , we will say σ_2 was computed using the Jackett & McDougall (1995) ocean equation of state and will [cite](#) accordingly.

10. Line 194, ECCO: Vertical grid type? Resolution (in m/dbar)?

In meters.

11. Throughout the manuscript, for example lines 205 and 207, check your citation styles (citet vs citep, if using LaTeX)

We will recheck every citation and fix the style accordingly.

12. Line 2012, SOSE: Number of vertical levels? Type of vertical grid? Vertical resolution? Also, please write more clearly what daily values of 5-day averages mean: each day is the 5-day mean centred on that day?

The model was constructed in spherical coordinates with 42 vertical levels of varying depth (m). 5-day averaging starting 1-5 January 2005 (the centering window is unclear). We will add the info to text.

13. Line 226, SODA: Vertical grid type? Vertical resolution?

The vertical level is in z^* coordinate. Vertical levels are at telescoping depths.

14. Line 232: how many ensemble members?

Not explicitly told (DOI: 10.1175/JCLI-D-18-0149.1); just states, "growing ensemble".

15. Line 245-246: I do not understand this description; it is nudged? If so, how often? Towards which variable(s)?

Not sure how often, but from what we came across for the budget analysis the salt field looked to be nudged.

16. Figure 1 / line 258 onwards: is the bottom at the same depth for all products? In particular, is the shelf-break shifted N-S?

The bottom is the same depth for the three products.

17. Lines 262-263: I do not understand what the standard deviation and spatial variability mean. Throughout the manuscript, use consistent terms such as "temporal standard deviation", "horizontal standard deviation", etc

We agree the terminology is unclear. In this case, we are referring to the temporal standard deviation, which we use as a proxy for the (unknown) reanalysis uncertainty. In our revision, we will use consistent terminology.

18. Figure 1: fonts are too small on the colorbars

We will make cbar fonts larger.

19. Figure 2: fonts are too small there as well, and adjust the caxis ranges on most panels, but especially the first column, so that we see more details

We will make cbar fonts larger, and consider changing the cbar range.

20. Line 288: Help the reader by having this range highlighted with a box on Fig3.

Good suggestion. We will implement this in our revision.

21. Line 291: you eventually give this information several pages later, but give the T-S range of CDW now or on line 288, and not just that of AABW.

Might disregard the density range for CDW altogether in the later sections.

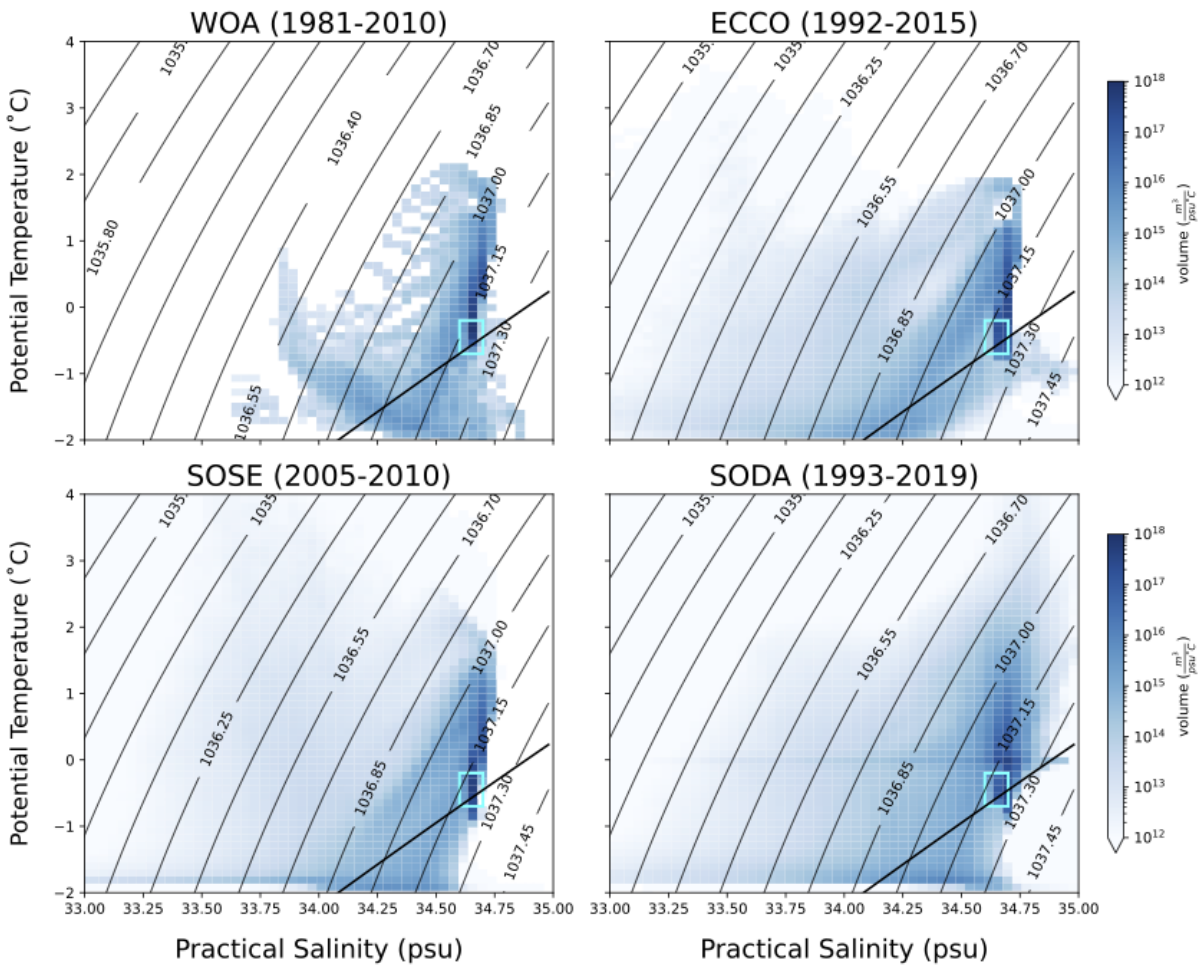
Update: We never provided the TS range for CDW.

22. Figure 4 deserves way longer a description than this short line 294. Comment on the overestimation (?) in CDW, or on how the Gade line really sticks out, which suggests some sea ice / ice shelf interaction misrepresentation.

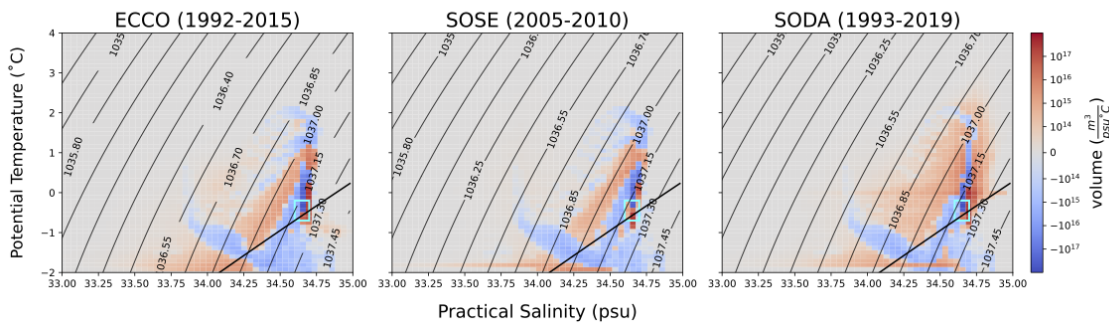
Good suggestion. We will do that.

Update: We commented on the CDW distribution in Fig 4. We also computed and plotted the Gade line in Figs 3 & 4 but struggled to see the misrepresentation you suggested; therefore, we did not feel comfortable including it in the paper. Please see the aforementioned Gade line in the figs below.

TS Diagrams



TS Diagrams Model minus WOA (1981-2010)



23. Figure 3: something is off with SODA at T=0 deg C. Explain why it has a larger volume than expected, or correct if that is artificial.

Will look into it (might be another missing data (with wrong time date) that we missed to omit that's throwing it off).

Update: Added text to talk about how we cannot explain this feature.

24. Figure 4: The caption and titles are reversed, so write more clearly “model minus WOA” (or the opposite)

We will change the caption and title for Fig 4 according to suggestion.

25. Line 318: you eventually give the densities in the different models many pages from here. You should give them here instead. Because you just finished showing how biased they are in T-S, yet here it looks like you do not account for their biases in density.

We do not account for the biases when defining each model's density, we define the lowest density value of AABW for each model based on the overturning streamfunction in Fig 5 (i.e. on the boundary between deep and bottom water).

26. Line 325: what does a volume gain mean in practice? Takes over other water masses higher up in the water column? Wider branch?

Volume gain means other water masses are transformed into AABW - they add to the volume of the AABW mass class.

27. Line 331: Fig 6 does not show brine rejection, only surface salinity fluxes. Could be P-E. This joins my major comment: your study of sea ice needs to be more extensive, and to be shown.

We are currently working on breaking down the salinity-driven transformation into a component based on sea ice and a component based on direct E-P-R, as was done by Abernathy et al. (2016). These results will be included in the revised manuscript.

28. Line 336-337: are these small values a typo? If not, which density interval am I supposed to look at right now? Because I did not notice this order of magnitude difference from ECCO.

These values are not a typo. The values of transformation components are computed on the boundary between deep and bottom water masses. As per previous suggestions, we will put delineating lines to denote the boundary between deep and bottom so that it may be easier for the reader to discern these values in the figure.

29. Line 347: I guess you do not use LaTeX after all. Please do not comment on Fig 9 before Figs 7 and 8, it is quite uncomfortable to have to go back and forth between the pages.

We did use LaTeX to prepare the manuscript. In our revision, we will make sure to present and discuss the figures in the correct order.

30. Line 358: see major comment, surface cooling + freshening suggests sea ice melt (from below) to me.

Please see the response under the major comment.

Update: changed 'corresponding to surface freshening' to 'corresponding to ice melt'.

31. Line 420-421: have you tried contacting them? They may not have checked AABW, but maybe they've investigated NADW for the AMOC and that would give you clues.

We tried contacting them on a different matter, but received no response.

32. Figure 10: to help with the comparison of the variabilities, have the same width as time interval for the three panels (e.g. 1 cm per year). Ideally, align them even, so that we can directly compare the models to each other.

For our revision, we will make this figure and see how it looks. Our concern is that the SOSE time period is very short compared to the others.

33. Figure 11: same comment as Fig 10

Same response as above.

34. Line 541: there's an Heuzé 2021 on CMIP6 (doi: 5194/os-17-59-2021). More models, more recent, same conclusions. As previously: no expectation of citation, just for your information.

We will include. Thank you!