AGU Advances

Authors' Response to Peer Review Comments on

Subantarctic Mode Water Biogeochemical Formation Properties and Interannual Variability

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Author Response to Peer Review Comments on 2022AV000722

Thanks to both reviewers and the editor for their helpful comments and suggestions. We have addressed all specific comments below. The main substantive changes to the manuscript are:

- New key points that better reflect the new knowledge contributed here
- More substantive comparison between float and BSOSE observations (text added in Section 3.1, new supplemental figure)
- Discussion of possible oxygen calibration/accuracy and whether it could impact our results (section 3.2)
- More careful and balanced evaluation of float pCO2 uncertainty and quantitatively how it could impact our SAMW ΔpCO2 estimates

In addition to addressing the comments requesting clarification of text, we made small edits throughout to improve overall clarity and readability. Thank you again for taking the time to critically read this manuscript.

Reviewer #1 Evaluations:

Recommendation: Return to author for minor revisions

Significant: Yes, the paper is a significant contribution and worthy of prompt publication.

Supported: Please Select

Referencing: Yes

Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.

Data: Yes

Accurate Key Points: Yes

Reviewer #1 (Formal Review for Authors (shown to authors)):

This paper documents an analysis of the wintertime biogeochemical properties of Subantarctic Mode Water (SAMW), which are important to understand and correctly model the Southern Ocean carbon sink and associated nutrient and oxygen pathways. Although the paper is largely descriptive, it is clear, well-written and presents valuable diagnostics that are significant to the resolution of ongoing discussions on the amplitude of the Southern Ocean carbon sink and the realism of biogeochemical models in the region. I recommend publication of the paper. I suggest that the authors consider the following minor comments in producing a revised paper draft:

- Line 97: insert "carbon" before "increase"?

Changed

- Line 152: "their" instead of "its"?

Changed

- Lines 253-256: it would be helpful to provide a little quantitative information on what is meant by "more similar" here, as these may guide future comparisons of the float observations to models. How similar? How much less similar was the Indian sector?

-We chose to focus on the Pacific rather than Indian sector for comparison for two reasons: one, the spatial distribution of deep winter MLDs was qualitatively in better agreement in the Pacific sector and two, the control exerted by the major climate modes of variability on the properties of the SAMW in the central (Li and England, 2021) and southeast Pacific (Garabato et al., 2009) is exceptionally strong, also documented by Meijers et al. (2019) for the Argo time period. We were especially interested in the effects of the strong 2015/2016 El Niño event on SAMW properties. We have modified the text to reflect this:

We used iteration 135, covering 2013-2019 at 1/6° resolution, for analysis of interannual variability. Here we only analyze the spatial and temporal variability of SAMW in BSOSE output from the Pacific sector, where the spatial distribution of deep wintertime MLDs and SAMW formation regions in BSOSE iteration 135 agreed well with those from RG-Argo. The Pacific region was additionally of interest because it reflected the impact of the strong 2015/2016 El Niño.

Naveira Garabato, Alberto C., Jullion, Loïc, Stevens, David P., Heywood, Karen J. and King, Brian A. (2009) Variability of Subantarctic Mode Water and Antarctic Intermediate Water in the Drake Passage during the Late-Twentieth and Early-Twenty-First Centuries. *Journal of Climate*, 22 (13), 3661-3688. (doi:10.1175/2009JCLI2621.1).

Li, Q., & England, M. H. (2020). Tropical Indo-Pacific teleconnections to Southern Ocean mixed layer variability. Geophysical Research Letters, 47, e2020GL088466. https://doi.org/10.1029/2020GL088466

- Paragraph starting on line 337: the discussion of subtropical influences on SAMW provided here may be strengthened by contextualising it with the recent findings of Fernandez-Castro et al. (GRL, 2022), also based on an analysis of Argo floats.

-Thanks for pointing out this recent work, we added text about the coherence between the Fernández Castro findings and our own: lines 370 to 374 in the new manuscript:

Using data from biogeochemical Argo floats, Fernández Castro et al. (2022) similarly documented the influence of salty, nutrient-poor subtropical waters on SAMW formation properties which decreases from the Indian to Pacific regions and is an important factor influencing the pre-formed nutrient content of SAMW.

- Paragraphs starting on line 470: I wonder whether a little more can be gleaned about what the new observations of preformed biogeochemical characteristics mean for the question of whether SAMW accumulates anthropogenic carbon through surface or interior processes, by broadly contrasting the observations with the relevant model property distributions. Even pointing to a likely answer, if possible, would significantly increase the interest of the analysis presented.

-We have added more text to this paragraph to indicate what seems likely to us. However, absent a detailed modeling evaluation/study it would be premature to speculate on the partitioning between surface and interior processes. While we do hope to work on this, it is beyond the scope of the current paper.

Reviewer #2 Evaluations:

Recommendation: Return to author for minor revisions

Significant: Yes, the science is at the forefront of the discipline.

Supported: Mostly yes, but some further information and/or data are needed.

Referencing: Yes

Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.

Data: Yes

Accurate Key Points: No

Reviewer #2 (Formal Review for Authors (shown to authors)):

Review for "Subantarctic Mode Water Biogeochemical Formation Properties and Interannual Variability" by Bushinsky and Cerovecki

The authors uses BGC Argo data and BSOSE products to explain the SAMW preformed properties and its IAV in relation to the dominant modes of climate variability. They found that SAM and ENSO leave specific fingerprint patterns on the biochemical properties in the Indian and Pacific sector of the ACC regions, that are in agreement with the previous physical observational analyses. This includes the basin-scale dipolar structures and zonal propagation. The writing style and visualization of the data are excellent and this is clearly publishable result, but it needs some additional clarifications and refinements in the discussions to provide a proper context and broader implications. The conceptual criticisms are provided below, followed by minor, technical points.

Conceptual comments on the main results

I'm not sure if the three key points reflected the most important outcome of this paper. All of the three points are obvious facts. Please reconsider the unique contribution of this work in the key points.

-Thank you for the feedback, new key points have been included that reflect the new contributions of this work.

Section 3.1

I understand B-SOSE should be incorporating the float data in a way that is mathematically consistent with chemical and physical constraints. However, Figure 3ab appear to show systematic differences between B-SOSE and BGC float observations (solid and dash lines), sometimes, outside of the error bars. The current writing of section 3.1 does not discuss this issue. Could you first please describe the differences, and then provide some interpretations?

-We agree that it is important to make sure BSOSE is an appropriate tool for its use in this paper. We have included plots of BSOSE – float differences in the SAMW formation regions (new Figure S2) and described the biases and possible importance as requested. Lines 300-317 in the new manuscript:

Despite capturing the overall relationship between biogeochemical properties and density shown in Figure 3a, the mean BSOSE properties differ from the Argo observations in some density classes. For instance, while BSOSE falls within the observed variability for oxygen, nitrate, and DIC in the Pacific SAMW density range, in the Indian sector BSOSE oxygen and nitrate are both higher than observations for the denser classes of SAMW. Direct comparison of BSOSE sampled at float profile locations and float observed properties for winter waters with deep (>200m) mixed layers within the regional SAMW density ranges indicates mean biases of -0.001 ± 0.72 °C, 0.07 ± 0.12 salinity, -3.4 ± 15.5 μmol kg^{-1} [O_2], 1.5 ± 2.7 μmol kg^{-1} [NO_3^-], and 0.33 ± 12.1 μmol kg^{-1} [DIC] in the Pacific and -0.47 ± 1.19 °C, -0.02 ± 0.19 salinity, 2 ± 12.6 μmol kg^{-1} [O_2], -0.1 ± 2 μmol kg^{-1} [NO_3^-], and 1.6 ± 7.5 μmol kg^{-1} [DIC] in the Indian Ocean (upper 200m average for MLs greater than 200m ± RMSE, Figure S2).

The high RMSE for these comparisons reflects the difficulty for a state estimate to exactly reproduce individual observations, including differences in mixed layer depth at a specific location and the related impact on ML properties. The mean biases are small relative to the magnitude of biogeochemical property changes across the SAMW density range. Additionally, in this study we primarily use BSOSE to explore interannual variability (Section 3.4), for which it is more important to reproduce the large-scale density property relationships than to have perfect agreement between every float profile and BSOSE output.

Section 3.2

Is it fair to say that float O2 calibration can affect the representation of air-sea O2 disequilibrium? Also, is it correct some floats are adjusted to match the WOA climatology in terms of the time-mean for the sampling period? Other floats are using in-air measurement. The former calibration approach assumes that air-sea disequilibrium follows climatology. This means the air-sea O2 disequilibrium derived from the BGC-Argo float are closely linked to the methods used to calibrate the O2 sensor itself. It would be important to provide comments and some perspectives on this.

-Additional details on uncertainty added to methods and we have now included a paragraph discussing how oxygen sensor calibration may or may not influence the results shown here. Lines 446-461:

It is important to consider oxygen sensor accuracy and method of calibration when comparing to shipboard observations as some float oxygen data have been adjusted to match shipboard data. Oxygen data in the SOCCOM dataset (36 out of 53 floats) are calibrated using either atmospheric oxygen as a reference point or using initial shipboard casts if atmospheric data are not available (Johnson et al. 2017; Maurer et al. 2021). Deployments do not occur during the winter and these float oxygen data are therefore independent of the GLODAP shipboard data presented in Figure 4 and Table 2. Oxygen data in the UW Argo O_2 dataset are re-processed using a two-point correction (one near-surface and one deep calibration value). The near-surface values are air measurements (if available) or World Ocean Database (WOD, Garcia et al. 2010) mean values. Surface reference values for the WOD were only used if the water was close to saturation (98-101% O_2 saturation). It is therefore unlikely that any of the shipboard observations shown in Figure 4 were used in the calibration. Therefore, while the uncertainty of oxygen data corrected to WOD is higher than air-calibrated data, these also represent independent observations from the GLODAP data. Uncertainties for the SOCCOM (1-2 μmol kg^{-1}) and UW Argo O_2 (~3 μmol kg^{-1}) datasets are much smaller than the mean offset and SD of the data shown here and are therefore unlikely to be a major factor in the undersaturation found in the pre-formed oxygen estimates.

Section 3.3

The authors provided excellent summary of IAV in relation to SAM and ENSO. Figure 6 is an excellent visualization and summary of basin-scale spatio-temporal variability for the key biogeochemical variables. One comment I have around L549 is that, while the text reads "Physical and biogeochemical properties across the Pacific become uniform during the 2016 El Niño and remain so through 2017, 2018, and 2019.". However, the visualization in Figure 6 and 7 left me a different impression that there is a strong peak in Year 2016, which decayed relatively quickly in the following years with some westward advection. Could you clarify and make the text and visualization consistent?

-We have clarified the text to (a) make it clear that we are referring to the period after the 2016 El Nino, when there is a large peak and (b) are drawing on the regional averages presented in Figure 7.

Comments on the overall discussion

An unresolved discrepancy is the difference between BSOSE and float data as shown in Figure 3. Again, the authors are overly optimistic about the results from BSOSE. I think it is important to address that the BSOSE still does not fully reproduce the float data and it can contain errors. At least, the RMSE of optimized products should be provided for each of the tracers in the iteration 135 used in this study.

-Addressed, as described above.

L588

In this paragraph, the writing of the text appears to be overly optimistic about the data quality of pCO_2 derived from BGC-Argo floats. The SOCAT data reflects direct pCO_2 measurements that are better understood and calibrated than the float-derived pCO_2 data. The authors simply attributed the difference by the sampling pattern, but I don't think that statement is justified. The results from Long et al (2021) suggest that the delta-pCO_2 based on the BGC floats overestimated the out-gassing. Shipboard data as well as aircraft measurements indicate that the float-derived pCO_2 data is overestimating the outgassing, and I don't think this is a simple artifact of sampling pattern differences.

-We agree that we could more explicitly address possible biases in the float pCO_2. To address these concerns we now discuss the impact of a systematic bias on the mean delta pCO_2 in Section 3.2, using a 6 µatm offset as described in Wu et al. 2022. We also have tempered the language in the conclusion paragraph cited here. Overall, the difference between float and ship delta pCO_2 in our study does not seem to be the result of bias, but it could explain part of the offset and we now explicitly make that point in the text.

Minor comments

L104 Ambiguous statement

Water is then ... --> It is then ... or more specifically, SAMW is then ...

-Changed

L162-164 The second part of the following sentence seems to be important but it is not clearly written. "Preformed nutrients are not coupled to regenerated DIC and can serve to draw carbon down from the atmosphere." It reads as if the subduction of preformed nutrient into the SAMW can serve to sequester carbon from the atmosphere. Rather, it must mean that the biological productivity fueled by the upwelling of preformed nutrient can lead to a net carbon uptake and oxygen depletion in the subsurface.

-Thank you, rewritten as: "Preformed nutrients are not coupled to regenerated DIC and can fuel net carbon uptake from the atmosphere when next upwelled to the surface."

L164 I suggest to use "estimate" rather than "calculate"

Preformed nutrients are often calculated ... --> Preformed nutrients are often estimated

-Agreed, changed.

L195 Please indicate what density is used for rho_0. Is it a constant reference density or calculated using temperature, salinity and pressure?

-The density is calculated as a function of temperature, salinity, and pressure. We have changed the symbol to rho, instead of rho_0, to make the text clearer.

L237 It would be nice to spell out and show the calculations to correctly estimate the atmospheric pCO2 from mole fraction to partial pressure. Also, it would be nice to show quantitatively how important these corrections are.

-We have added supplemental text (new section Text S2. Calculation of ΔpCO2) to explicitly describe the calculation of ΔpCO_2 from the atmospheric mole fraction. We also included information on the importance/sensitivity to these adjustments. Additionally, we have included a link in the Open Research section to the code used for this analysis, which includes these calculations as well.

L 247 There is no specific information about the data quality of iteration 135. Please provide RMSE of relevant tracers between this particular BSOSE iteration and BGC floats (e.g oxygen, nitrate, DIC, pCO2).

-Now included as described above.

Table 1. It needs a caption. Please explain the meaning of the uncertainty. It is a confidence interval? How is it computed?

-Table caption added with explanation of how the mean and standard deviations are calculated for the listed variables.

L330 Please explain the meaning of the uncertainty.

-The uncertainties given here are ± 1 SD – text is now updated to state that.