AGU Advances

Peer Review History of

Subantarctic Mode Water Biogeochemical Formation Properties and Interannual Variability

Seth M. Bushinsky and Ivana Cerovečki

[1] Department of Oceanography, School of Ocean and Earth Science and Technology, University of Hawai'i at Mānoa, Honolulu, HI

[2] Scripps Institution of Oceanography, University of California San Diego, La Jolla, CA

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Author Response to Reviewers

Peer Review Comments on 2022AV000722

Reviewer #1

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"?

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

- 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?

- 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.

- 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.

Reviewer #2

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.

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?

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.

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?

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.

L588
In this paragraph, the writing of the text appears to be overly optimistic about the data quality of pCO2 derived from BGC-Argo floats. The SOCAT data reflects direct pCO2 measurements that are better understood and calibrated than the float-derived pCO2 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-pCO2 based on the BGC floats overestimated the out-gassing. Shipboard data as well as aircraft measurements indicate that the float-derived pCO2 data is overestimating the outgassing, and I don't think this is a simple artifact of sampling pattern differences.

Minor comments

L104 Ambiguous statement
Water is then ... --> It is then ... or more specifically, SAMW is then ...

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.

L164 I suggest to use "estimate" rather than "calculate"
Preformed nutrients are often calculated ... --> Preformed nutrients are often estimated

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

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.

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).

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

L330 Please explain the meaning of the uncertainty.

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