Review of revised manuscript **"New constraints on biological production and mixing processes in the South China Sea from triple isotope composition of dissolved oxygen"** by Hana Jurikova, Osamu Abe, Fuh-Kwo Shiah and Mao-Chang Liang (Biogeosciences Discussions, manuscript bg-2020-448, version 25 September 2021)

Reviewer's note dated January 20, 2022:
I completed the review on Nov 5, 2021, but it appears that due to technical problems the review was not uploaded to the editorial system and this issue was only identified in January. I apologize for the delay in receiving this feedback, and I understand that delays in receiving reviews are frustrating for both authors and editors. I hope that you find my comments helpful despite the unintentional delay.

Review dated November 5, 2021:

I was reviewer #2 of the original manuscript and I am now reviewing the revised version. I acknowledge the considerable effort the authors have made to respond to the comments from all three reviewers. In my opinion, there are some remaining issues that need to be resolved before the manuscript is accepted.

First, the authors are still not in compliance with the journal's data policy. The authors state in their response letter that "the supplement was revised to include all data from this study" but that is incorrect. Additionally, as mentioned in my previous review, the data should be put in a repository and not in the supplement. Please note the Copernicus policy: "Copernicus Publications requests depositing data that correspond to journal articles in reliable (public) data repositories, assigning digital object identifiers, and properly citing data sets as individual contributions. Please find your appropriate data repository in the registry for research data repositories: re3data.org."

I have copied my comments on this topic from the previous review below: my comments are in black and the authors' response is in blue.

> Finally, the authors are not in compliance with the journal's data policy (https://www.biogeosciences.net/policies/data_policy.html) and have not provided sufficient
> details on the GP and NP calculation methods. Stating that the CTD data is available by contacting the authors is not acceptable. All data needed to reproduce the GP and NP calculations should be published to a repository capable of issuing a DOI such as PANGAEA
> or Zenodo.
> Authors should archive the following:
> 1) Date and time of each sample (since they discuss in the paper that time of
> day is important; note the date of the final profile is incorrectly listed as April 25th, 2016; the year should be 2015)
> 2) latitude and longitude
> 3) all CTD data used in the paper: exact depth/pressure, temperature, salinity,
> O2, chlorophyll fluorescence for each sample discrete 17 Δ sample, as well as
> the high-resolution profiles plotted in Fig 4.
> 4) optional: mixed layer depth, wind speed, and k values used to calculate
> GP/NP and the final GP and NP results, and a description of how each was

determined.

5) Define in the metadata how 17 Δ is calculated (what lambda is used)

Reply: We have now included the request data to the supplement.

As far as I can tell, the only change that the authors made was correcting the incorrect date for one of the profiles.

Second, the authors should double check which gas exchange parameterization was actually used. I noted conflicting information in their response to the reviewers and their revised manuscript

Comment from Referee 1:

L133 - Why mentioning Wanninkhof et al., 2009 if in the end they used Ho et al., 2006?

Reply: We believe it is useful to mention both, as the parametrization from Wanninkhof et al., 2009 is commonly used, but in our particular case with high windspeeds we used the one from Ho et al., 2006.

However, the revised manuscript states on line 167 that they used the Wanninkhof et al. 2009 parameterization. The citation to Ho et al. (2006) has been removed from the revised manuscript.

The authors should double check which parameterization was actually used and revise the text if appropriate.

Third, I think that the discussion of Ar correction and size corrections still needs revision.

Line 146: "Similar as in Jurikova et al. (2016) an Ar correction was performed to correct for the distribution of gases between the headspace and water in the sampling flasks and normalised to air. A size correction for the total amount of gas in the sample was not required at our current mass spectrometer setting and hence not applied."

I suggest to revise this text as follows, if I am accurately understanding their process:
"We corrected for the distribution of gases between headspace and water in the sample flask following Luz et al. (2002). Because the measured values of $\delta^{17}O$, $\delta^{18}O$, and $^{17}\Delta$ are affected by the sample O_2/Ar ratio (Barkan and Luz, 2003), we performed an Ar correction that accounts for these dependencies, following Jurikova et al. (2016). However, we did not conduct a size correction or pressure baseline correction to characterize the impact of changing the total pressure or total amount of O_2 in the sample (Stanley et al., 2010; Yeung et al., 2020).

Here are the reasons that I suggest these revisions:

As currently written, I think line 146 is incorrect because the Ar correction as described in Jurikova et al. (2016, section 2.5 of that paper) refers to the sensitivity of the measured isotope values to the sample O_2/Ar ratio. It is unrelated to the fractionation of gases between the water and headspace. Was an Ar correction performed?

I am not convinced by their statement that a size correction "was not required", based on their response to my comments in their response letter. As mentioned in my previous review, the way that researchers typically perform a sample size calibration is by putting the reference standard in both bellows but varying the total amount (volume) of gas in each bellow by adjusting the bellows compression and plotting a calibration curve that spans the full range of sizes in their sample set. Based on the authors' response, it appears that they never characterized the size effect.

They mention in their author comment that "Furthermore, in the deepest samples (3500 m) the O2 concentration was still relatively high (~100 umol/kg for the deepest samples vs. ~190 umol/kg at the surface) and hence sufficient for analyses without sacrificed precision or introduction of analytical artifacts."

In my own experience with this method, a size correction was definitely required when measuring a dataset with O_2 concentrations that vary by a factor of two. Therefore, I was not convinced by this comment without data to back it up.

Additional suggested changes to the text:

Line 10-24: the discussion of conditions that may "shift the metabolism" (line 19) and "shifting the carbon balance" (line 22) is contradictory to line 16 which says "These values indicate slight net heterotrophy but, within the uncertainties/variabilities observed, more likely that the metabolism of the system was in net balance."

Line 154: "The reproducibility (1σ) for the analysis of equilibrated water samples (n = 3) was 0.020 ‰, 0.037 ‰, and 11 ± 3 per meg for δ17O, δ18O, and 17Δ, respectively and 4.6 ‰ for δ(O2/Ar)"

I think that 11 per meg is the mean and 3 per meg is the reproducibility. I suggest rewording as:
"For the analysis of equilibrated water samples (n = 3), the mean $^{17}\Delta$ value was 11 per meg and the reproducibility (1σ) was 0.020 ‰, 0.037 ‰, and 3 per meg for $\delta^{17}O$, $\delta^{18}O$, and $^{17}\Delta$, respectively and 4.6 ‰ for $\delta(O_2/Ar)$"

Line 331: Change from "we refer to the scaled production rates as GP and NP to "we refer to the scaled production rates **in C units** as GP and NP"

Line 338: "Temperature-based mixed layer depth" – what was the temperature criterion?

References:

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Luz, B., Barkan, E., Sagi, Y., and Yacobi, Y. Z.: Evaluation of community respiratory mechanisms with oxygen isotopes: A case study in Lake Kinneret, Limnol. Oceanogr., 47, 33–42, 2002.

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Yeung, L. Y., Hayles, J. A., Hu, H., Ash, J. L., & Sun, T. (2018). Scale distortion from pressure baselines as a source of inaccuracy in triple-isotope measurements. Rapid Communications in Mass Spectrometry, 32(20), 1811– 1821. https://doi.org/10.1002/rcm.8247