

## Response to editor:

Topic Editor Decision: Publish subject to minor revisions (review by editor) (08 Dec 2020) by Mario Hoppema

Comments to the Author:

Dear Dr. Zhang and co-authors,

Thank you for submission of your revised manuscript. Referee #1 is generally satisfied with your revisions, though some minor issues remain. The referee described those in the attached referee report.

I agree with the referee and think the manuscript is almost there. I did go through the manuscript and provide minor and technical comments below. Please account for the comments by the referee and me, and submit your final version of the manuscript

**Response:** Thank you very much for your constructive comments. We have considered all suggestions and incorporated them into the revised version. In the following we answer to your comments point by point and indicate how the manuscript is going to be revised.

You are using data from other sources, e.g. satellite data from Copernicus and NASA. Please check and confirm whether you fulfill the fair data use statement and whether the acknowledgment of the data use is according to the request by the data providers.

**Response:** Thanks for this suggestion. We have added related statement to the Acknowledgement and citation to the reference list following the requests of Copernicus and NASA.

L42-44 “NCP is equivalent to the rate of organic carbon export and transfer up the food web, which can quantify the strength of biological pump (Lockwood et al., 2012).” It is not clear to me what “transfer up the food web” means, as this is also compared and stands besides carbon export. After the comma, I suggest: and is a measure for the strength of the biological pump (Lockwood et al., 2012).

**Response:** Sorry for the confusion we made. NCP is the difference between gross primary production and respiration, and can also represent net organic carbon production. At steady state, organic carbon mainly has two fates: being directly exported to the deep water and involved into the food web. That’s why we write this sentence. But after reconsidering, we think the organic carbon that’s involved into the food web will finally be exported to the deep water through the sinking of fecal pellets and dead creatures. Thus we decide to delete “transfer up the food web”.

L44-45 “Dissolved oxygen-to-argon ratio ( $O_2/Ar$ ) has been developed as a proxy for NCP ...” I think this requires some more explanation why this is the case.

**Response:** We agree with you. Prior to this sentence, we have added an explanation that “*NCP effectively couples carbon cycle and oxygen ( $O_2$ ) production through photosynthesis and respiration in the euphotic layer, thus many previous researches measured the mass balance of  $O_2$  to quantify NCP (e.g., Emerson et al., 1991; Hendricks et al., 2004; Huang et al., 2012; Reuer et al., 2007). Argon ( $Ar$ ), a biological inert gas, was commonly used to normalize the  $O_2$  concentration in these researches. Based on the similar solubility properties of  $O_2$  and  $Ar$ , oxygen-to-argon ratio ( $O_2/Ar$ ) can remove the influences of physical processes (i.e., temperature and pressure change, bubble injection) on the mass balance of  $O_2$  (Craig and Hayward, 1987).*”

L56-57 “in global NCP data sets” It was not clear to me that these exist. Please add a reference.

**Response:** Sorry for this inappropriate expression. The “global NCP data sets” don’t exist. Here we want to emphasize that coastal and shelf regions still suffer from low resolution measurements. We have revised this sentence as “*However, these regions still suffer from low resolution measurements that can’t provide representative high-resolution NCP data.*”

L59 “extremely complex ecological characteristics” It is not clear to me what that implies. Why is it more complex than other regions? Please explain or tone down.

**Response:** We agree that “extremely” is exaggerated. We have deleted it.

L72 “during its long residence time in the SCS” Please be more specific; how long?

**Response:** We have clarified in the text that the residence time can be about 40 years.

L127 If you measure concentrations, please indicate that here.

**Response:** Thanks for the suggestion. We have changed this sentence to be ‘*Continuous measurements of dissolved gases (O<sub>2</sub>, Ar, and CO<sub>2</sub>) were obtained using membrane inlet mass spectrometry*’. But we think it’s not appropriate to indicate concentration or partial pressure here. MIMS just provides the signal intensities of the dissolved gases instead of the actual concentrations. O<sub>2</sub>/Ar-standards were not available to calibrate the oxygen and argon signals from the MIMS. However, the O<sub>2</sub> signal was normalized to Ar to yield a biologically relevant parameter, i.e. O<sub>2</sub>/Ar, which can be directly calculated using the signal intensities of O<sub>2</sub> and Ar. Temperature-controlled seawater standards were used to calibrate *p*CO<sub>2</sub> and biological O<sub>2</sub> saturation ( $\Delta$ O<sub>2</sub>/Ar) measurements. Signal intensity can be a reflection of the concentration or partial pressure of the dissolved gas; thus we can use a calibration curve to quantify *p*CO<sub>2</sub>.

L152 Please indicate what is measured here of O<sub>2</sub>, Ar and CO<sub>2</sub>: concentration, partial pressure?

**Response:** Due to the same reason as mentioned above, we mean the long-term signal stability for O<sub>2</sub>, Ar and CO<sub>2</sub> here, hence it’s not necessary to indicate concentration or partial pressure.

L129-131 Are there any cruise reports that could be cited? Do the cruises have EXPO codes?

**Response:** Sorry, there’s no available report that could be cited here. And the cruises don’t have EXPO codes.

L156 delete “down during this cruise”

L264 delete “In addition,”

L295-296 Change to: ... of the two cruises are shown in Tables 1 and 2.

L299 delete “reasonable”

L303-304 delete “shallower than that of October 2014”

L530 I suggest: The amount of light may also play a role in the extent of primary production. (that light play a role in primary production is trivial)

**Response:** Thanks for your suggestions. We have done these.

L357-358 Please explain why you took these transects. Why not take transect 4, which was sampled

in both years.

**Response:** There are several reasons. Both selected transects are uninterrupted, with relatively short time for CTD casts, which can give a good view of the zonal/ meridional variations of parameters. In addition, highest DIN and associated spike of NCP occurred in Transect 5 in 2014, thus we wanted to highlight this result. Transect 4 in 2015 was chosen to discuss the influence of upwelling, which is quite meaningful to refine our NCP results. Transect 4 in 2014 was partly interrupted by an obvious ship's drift during the period for CTD casts (Figure 2, 4) and the DIN concentration in this transect was not very significant, so we didn't take this transect.

Table 1 Please use data format like 13 Oct 2014

Figure 2 Please delete PSU at the salinity panel

Figure 3 Please delete PSU at the salinity panel

Figure 5a Please delete PSU at the salinity panel

Figure 6a Please delete PSU at the salinity panel

Figure 7 The labels at the axes are too small. Please make them larger for better readability.

Figure 8a Please delete PSU at the salinity panel

Figure 9b and 9d Please delete PSU at the salinity panel

**Response:** Thanks for your suggestions. We have done these. All the dates in Table 1, 2, 3 and 4 have been revised to the format like 13 Oct 2014.

## Response to reviewer 1:

### General comments:

Qin et al have thoughtfully revisited their manuscript and made a number of revisions that have strengthened the paper overall. In particular, their analysis of the timing of shelf water intrusion into the SCS versus the residence time of their O<sub>2</sub>/Ar measurements, supported with satellite chlorophyll data, provides convincing and valuable support of their conclusion regarding the contribution of shelf water intrusion to NCP.

The authors' more detailed analysis of average mixed-layer PAR and their revised conclusion that light does not limit mixed-layer NCP in the study region is also an important improvement.

Generally, the revisions have demonstrated care and critical thought in reevaluating the interpretation of this study's findings. The changes made have satisfied this reviewer's original criticisms of the manuscript.

The new assessment of the impact of the shelf water intrusion upon observed NCP rates is also quite clever and an interesting scientific contribution.

**Response:** Thank you very much for your constructive comments. We have considered all suggestions and incorporated them into the revised version. In the following we answer to your comments point by point and indicate how the manuscript is going to be revised.

### Specific comments:

Figure S3 is quite nice and I'm very tempted to recommend that this be included as a main figure. I certainly think it adds more value to the main article than Figure 9, for instance.

**Response:** Thanks for your suggestion. We have set this figure as Figure 10.

Line 15: The statement that NCP is a proxy of carbon export is slightly too strong, as NCP is more accurately a metric of export potential (excess organic matter production available for export to depth).

**Response:** Thanks for your suggestion. We have revised this sentence as "*.....is a measure for the strength of the biological pump.*".

Section 2.3: How many replicates for nutrient analysis were collected at each CTD station?

Line 376-378: Upon further reflection, this statement reads as attributing somewhat too strong of a causal relationship. I find the NH<sub>4</sub> measurements, sparse though they are, to be useful evidence of ammonium contributing to the peak in NCP on this transect, and the residence time at these stations was quite short which further supports this, but at the end of the day these are just two stations. This also further emphasizes the importance of replication of nutrient sampling as noted above. If these are only single measurements, then only a very weak statement can be made here, and the associated discussion should be reconsidered more thoroughly.

Figure 5c and Figure 6c: As noted above, if the NH<sub>4</sub> nutrient sampling includes multiple measurements, the individual replicates in addition to the mean might be shown.

**Response:** We only made single measurement for the nutrients. Our method has a good precision of 3 %, ensuring the reliability of the single nutrients data. Therefore, we didn't make multiple sampling at each CTD station. We agree with your concern that the statement here is too strong, since there were only two data points of NH<sub>4</sub> in this transect. So we have toned down the statement that "*However, we only got nutrient data at two CTD stations in this transect, thus the result we obtained here just*

*indicated that high NCP occurred at the station with relatively high  $\text{NH}_4^+$ , but couldn't be a strong evidence that  $\text{NH}_4^+$  was the main factor influencing NCP in this transect."*

Lines 385-387: I would cite Figure S3 here, as this clearly shows the influence of shelf water.

**Response:** Thanks for your suggestion. But we think after we set S3 as a main figure (Figure 10), citing it here may make the order of the main figures a bit chaotic. Because if we cite it here, it should be "Figure 7" based on the order of being cited. But the paragraph that discusses this figure is near the end of the *Results and Discussion* section where "Figure 10" is much more appropriate.

Line 421 (and line 27 in abstract as well as line 559 in the conclusion): The figure of 376% is a little overly precise, especially given the variance in the NCP of the background and intrusion-influenced water masses. I would replace with a more general statement like "by potentially more than threefold" or similar, following the convention the authors have adopted in lines 492-495.

**Response:** Thanks for your suggestion. The variance in the NCP of the background and intrusion-influenced water should be considered. We have replaced "376 %" with "potentially more than threefold" in these sentences you mentioned.

Lines 510-529: this new passage and the associated new data figure and table are very strong additions. Again would make the case for Figure S3 to become a main figure given its importance to the manuscript's conclusions regarding the July cruise.

**Response:** Thanks for your suggestion. We have set Figure S3 as Figure 10 in the main text.

Throughout: I would recommend that the authors double-check the manuscript text for minor grammatical errors, particularly in the newly-added text.

For instance, in "Dissolved oxygen-to-argon ratios ( $\text{O}_2/\text{Ar}$ ) in the oceanic mixed layer has been widely used" (Line 13), "has" should be replaced with "have".

Similarly, "Despite the coastal waters such as shelves and estuaries only account for 7 % of the global ocean surface area" (line 52) should be revised to something like "Despite coastal waters such as shelves and estuaries only accounting for 7 % of the global ocean surface area", etc.

**Response:** Thanks for your suggestion. We have revised the grammatical errors.