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Interactive comment on "High-resolution distributions of O_2/Ar on the northern slope of the South China Sea and estimates of net community production" *by* Chuan Qin et al.

Anonymous Referee #1

Received and published: 16 September 2020

General comments:

This manuscript reports high-resolution measurements of net community production in the coastal South China Sea in summer and fall, as estimated using the dissolved O2/Ar technique. The authors compare measured NCP rates against physical parameters and nutrient concentrations to assess factors influencing the spatial distribution and magnitude of productivity.

These new NCP data are a useful contribution in that productivity measurements in this region to date have remained relatively sparse and low-resolution, but apart from presenting this new data the manuscript offers few broader or novel conclusions and

the wider scientific significance is limited.

The field, experimental, and statistical analyses appear to have been rigorously conducted largely according to current best practices. In particular the methods indicate a good experimental setup and attention to potential sources of error in field O2/Ar measurements.

In some cases, however, the conclusions regarding the influence of light and nutrients upon productivity patterns that the authors draw from their data and results are not fully appropriate or justified.

The presentation of data in the manuscript and figures is generally good, but leaves some room for improvement. The text could have benefited from better description of some data and variables, while Figure 1 is difficult to interpret, in turn impacting interpretation of other figures and results throughout the paper.

The major point this reviewer would like to stress is that the authors should pay careful attention to qualifying the caveats to some conclusions, while reconsidering other conclusions if they are not fully backed by the presented analyses.

1. The conclusion that NCP is subject to nitrogen limitation based on correspondences between elevated NCP rates and DIN concentrations needs further justification. Nutrient concentrations reflect the marine environment at the moment of sampling, while the O2/Ar method integrates over the residence time of biological oxygen in the surface ocean. The residence time is a particularly important factor for the authors to highlight in the manuscript, as it carries important implications for how far temporally removed the measured productivity signal is relative to the cruise measurements. At a minimum, additional discussion of the residence time of the oxygen signal on both cruises and potential associated considerations is necessary. Given the availability of satellite data as presented in He et al., 2016, discussion of the history of the measured water masses should be quite feasible. The reviewer also notes that the nitrogen limitation in this region is hardly a novel finding, as the authors themselves mention in the

introduction.

Mixed layer depth ranges and O2 surface layer residence time should be directly reported in the text of the results.

2. Similarly, assertions regarding the influence of light availability on NCP are questionable. Are MLDs around the time of these cruises really deep enough for light to influence mixed-layer productivity? In June of 2015 the data indicate that the maximum MLD value was just 30m. Water column PAR and Chl profiles are undoubtedly available from the CTD casts and should be presented and discussed in the context of such claims. A June 2015 Chl-a maxima of 0.6 ug/L certainly suggests that biomass-induced light attenuation in the mixed layer wouldn't be an issue, etc... Furthermore, residence time should once again be discussed, as light limitation is a factor influencing the time-integrated productivity signal over the relevant wind speed history at the measurement sites. Figure 10 also seems to suggest that the strength of MLD relationships with NCP are dependent on a relatively low number of data points.

3. The claim at lines 426-428 that "there was no significant productivity below the mixed layer that was missed by underway sampling" also seems unjustified and contradicts earlier results and text. Earlier for instance, the authors note that a subsurface oxygen maximum is a characteristic feature in the South China Sea, and significant subsurface productivity is observed in many oligotrophic regions globally such as the Sargasso Sea. Vertical Chl profiles would again be important evidence to present in support of the claim that underway measurements did not miss significant subsurface production.

4. In general, since Delta O2/Ar is directly used to calculate NCP, reporting and discussing relationships between Delta O2/Ar and other parameters provides little value. This reviewer would recommend removing discussions of Delta O2/Ar versus Chl, MLD, and so on from the manuscript.

5. The authors should also consider whether alternative hypotheses (nutrient co-limitation, limitation by a non-measured variable such as Fe, etc...) could potentially

present alternative explanations for observed patterns/relationships.

Specific comments:

Lines 13-14: it should be clarified at the very start of the abstract that the O2/Ar ratio refers to dissolved gases in surface seawater.

Line 32: Rather than oceanic CO2 uptake, which is strongly dominated by physical factors, it might be more appropriate to say that oceanic carbon sequestration is regulated by primary production and export.

Line 38: No longer recent... quite an established technique at this point.

Line 41: No longer clear that coastal O2/Ar-derived estimates of NCP are sparse.

Line 97: This is minor, but the original publication describing the MIMS technique (Tortell, L+O: Methods, 2005) should be cited here.

Line 158: Clarify units.

Lines 170-175: For this section, units for density should be in kg/mˆ3 to arrive at the correct units for NCP, or the appropriate conversion factor

Line 181: remove "excellent"

Lines 213-220: Encourage authors to also display the ranges for SSS and Chl-a for the June 2015 cruise using the same format employed in the paragraph above for the October 2014 cruise, for consistency and clarity.

Lines 225-227: Elaborating on the assertion that high O2/Ar and low pCO2 signify a strong biological CO2 sink may be useful. The pCO2 values are indeed considerably low and I would be curious about the associated residence time of O2/Ar on Transect 3.

Line 236 and elsewhere: Here and throughout the paper, you are reporting averages as ##.## +/- ##.##. You should specify that these represent avg +/- std dev... etc...

Lines 299-301: Keep it clear in this sentence that upwelling does not necessarily produce an underestimation of NCP. It is more accurate to say that subsurface waters may have different (either more positive or more negative) O2/Ar signatures that could produce either an underestimation or an underestimation of NCP, as you explain a few sentences later in the context of subsurface O2 maxima in oligotrophic regions.

Lines 313-315: The cutoff for salinity to account for waters influenced by shelf water injection is justified, but the cutoff intended to exclude regions with upwelling seems somewhat arbitrary.

Line 330: Consideration of other explanations is needed in this section as described in the general comments.

Lines 412-418: As mentioned in the general comments, is it even meaningful to analyze the influence of light limitation on NCP when the euphotic zone is 2-7 times the depth of the mixed layer in this region? Much more discussion is needed to back this light limitation idea. Cassar et al 2011 makes the point for instance that MLD is not the only factor affecting light availability.

Lines 426-428: "This result implied that there was no significant productivity below the mixed layer that was missed by underway sampling." As mentioned in the general comments, you need to address the contradiction between this and subsurface O2 maxima and potential deep chlorophyll maxima.

Figure 1, lines 681 - 683: please describe what the point and star symbols represent. Are these the locations of CTD casts? The fact that the color scale in (a) and (b) represents bathymetry should also be stated as well. The cruise path in (a) is also difficult to interpret based on the arrows. This figure is critical for the interpretation of subsequent figures. Perhaps numbering of points on the cruise plan would convey the path of travel better.

Technical corrections:

Lines 310-318: Here and elsewhere throughout the manuscript, you use sentence structures that rely too heavily on parentheses, which can cause confusion for readers. These sentences in particular are very difficult to interpret.

General: There are some grammatical errors in the conclusion

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