Reply to Referee #2

Thank you very much for your constructive comments. In the following we answer to your comments point by point and indicate how the manuscript is going to be revised.

General comments

In their manuscript "High-resolution distributions of O_2/Ar on the northern slope of the South China Sea and estimates of net community production", the authors report continuous net community production (NCP) estimates in the mixed layer of the northern South China Sea (SCS). The study makes a clear contribution to understanding of productivity in marginal seas like the SCS, where prior NCP estimates are limited. To a lesser extent, the study also advances a relatively novel method to estimating NCP through continuous observations of ΔO_2/Ar. My major critique is that the authors do not connect back to these original objectives in their paper. What new information have they gathered about the SCS as a result of this continuous method of measuring NCP, and how does this relate to past measurements of NCP in this region? Which methodological and/or environmental factors cause their estimates to compare or differ from past estimates? It is clear why their study is significant, but explicitly tying the discussion of results to these objectives will strengthen the scientific contributions of this paper.

Response: Following your suggestion, we amended the manuscript and described the importance of shelf water intrusion in the study region from line 84 to 88 that *"The northern slope of SCS is an important transition region between coastal area and SCS basin. In the summer, the shelf water intrusion is an important process changing the nutritive state in the northern slope region of SCS (He et al., 2016; Lee Chen and Chen, 2006). But so far, the NCP enhancement caused by this process is still unknown."* Besides to figure out the regulating factor on NCP, we also set quantifying the contribution of shelf water to the NCP enhancement in the study region as one of our main objectives. This can be a new finding achieved by our O_2/Ar method.

By comparing our NCP result with previous results, we emphasized the important influence of shelf water intrusion in the summer. In addition, we highlighted that our high-resolution observation can catch the rapid NCP variation more effectively than previous methods. Related content can be found in section 3.3 and 3.4 (lines 346 to 354, lines 409 to 417).

In the revised conclusion section, we pointed out that nitrogen is the main regulating factor on NCP in the study region and reported that *"the summer shelf water intrusion may significantly promote NCP by 376 %"*, connecting back to our objectives.

Specific comments

In the title: It is more accurate to write ΔO_2/Ar rather than O_2/Ar?

Response: We have changed O_2/Ar to $\Delta(O_2/Ar)$

Line 35: Clarify what "indicator" means in this context, and in which conditions this assumption holds true (e.g., NCP may be partitioned into DOC production, particle export, zooplankton grazing, etc.).

Response: NCP here represents the net organic carbon production in the mixed layer, corresponding to the difference of phytoplankton photosynthesis and respiration. Thus we can use the mass balance of biological O_2 to quantify NCP. NCP can be regarded as a sum of biological POC, DOC and the organic carbon involved in the food wed. O_2/Ar-based NCP used to rely on the steady state assumption that the productive rate and wind speed keep constant. We have incorporated these into the revised manuscript from line 39 to 44 that *"Net community production (NCP) corresponds to gross primary production (GPP) minus community respiration (CR) in the water (Lockwood et al.,* *2012; Stanley et al., 2010) and is an important indicator of carbon export. At steady state, 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).*"

But recent researches pointed out that the O_2/Ar-based NCP estimate could be a time-weighted NCP over the residence time of O_2, weakening the need for the steady state assumption. We have clarified this in section 2.2.

Line 55: The water classifications are a bit confusing here. Perhaps it would be clearer to say that SCS water is a mix between two end-members: freshwater runoff from rivers and North Pacific offshore water.

Response: Sorry for the confusion caused. The SCS water is not just a mix between freshwater runoff and North Pacific offshore water. Because of its long residence time in the SCS region, its property has been changed a lot by heat exchange, precipitation and mixing processes. Li et al. (2018) regarded the SCS water as one of the end-members of water masses on the northern slope region of SCS. We have revised the manuscript to make this classification clearer. Now this content reads *"The surface water masses on the northern slope of SCS can be categorized into three regimes: shelf water, offshore water (e.g., the intruded Kuroshio water), and the SCS water (Feng, 1999; Li et al., 2018). The shelf water is mixed with fresh water from rivers or coastal currents and thus usually has low salinity (S < 33) and low density (Uu and Brankart, 1997; Su and Yuan, 2005; Cheng et al., 2014). Both offshore water and SCS water originate from the Northern Pacific. Thus offshore water has similar hydrographic characteristics of high temperature and high salinity as the Northern Pacific water. But the SCS water has changed a lot in its hydrographic property because of the mixing processes, heat exchange and precipitation during its long residence time in the SCS (Feng et al., 1999; Li et al., 2018).*"

Reference: Li, D., Zhou, M., Zhang, Z., Zhong, Y., Zhu, Y., Yang, C., Xu, M., Xu, D. and Hu, Z.: Intrusions of Kuroshio and Shelf Waters on Northern Slope of South China Sea in Summer 2015, J. Ocean Univ. China, 17(3), 477–486, doi:10.1007/s11802-018-3384-2, 2018.

Lines 70-84: It is unclear what the aim of listing these numbers is? Do the authors wish to convey that NCP is variable across SCS studies? It would be useful to reference these numbers again in the discussion for comparison. In any case, when reporting NCP and export, use both O_2 and C units so that the numbers are comparable. The authors can perhaps apply the photosynthetic quotient used in the method to do this conversion to keep units consistent (line 174).

Line 79: Should the units here be sˆ-1 rather than aˆ-1?

Response: Here we want to report the previous researches about carbon export in the SCS. The POC just occupies a portion of NCP, and its value may not be very comparable with NCP. Hence we deleted the description about previous POC export and mainly focus on the previous NCP researches in the SCS in this paragraph. We have cited the previous NCP values to compare with our NCP results in this study. We have also converted the unit of all NCP values mentioned in the text to "mmol C m^{-2} d^{-1}".

Lines 85-86: Describe the potential inaccuracies of each discrete method so that it is clearer how this study benefits scientific understanding of the SCS.

Response: Thanks for your suggestion. We revised the manuscript to describe the potential inaccuracies and shortcomings of discrete methods in the following paragraph as that *"Discrete sampling suffers from low spatial resolution, and cannot adequately resolve variabilities caused by small-scale physical or biological processes in dynamic marine systems. In addition, each of the three methods for NCP estimate mentioned above has its limitation. DIC-based NCP estimate is not suitable for the coastal region, because instead of biological* *metabolism, the terrestrial runoff can be the strongest factor influencing the DIC in the coastal system (Mathis et al., 2011). The inavoidable difference between in situ circumstance and on deck incubation condition can introduce uncertainties to the NCP derived from light/dark bottle incubation (Grande et al., 1989). Though Argo profiling float partly gets rid of the limitation of discrete sampling, it's hard to control its movement in the study region. However, no high-resolution measurement of NCP has been reported for the SCS so far."*

Section 2.2: Explain how the 5-minute NCP values are scaled up to daily estimates.
Lines 179-183: As written, these two sentences imply that the authors do not know whether their NCP estimates represent daily or monthly signals. If they represent the latter, would this not defeat the purpose of the study, which is to resolve "highly dynamic environmental fluctuations of coastal systems" (line 87) in shorter than monthly time scales?

Response: The "5-min" here is not a timescale for monitoring the net change of a biological production tracer during this period to calculate the daily NCP, but the time interval of our underway data. The 5-min interval is usually along with a spatial interval of 500 m to 1 km because the ship is moving, thus it can also be regarded as the spatial resolution of underway sampling, which is much higher than that of discrete sampling (e.g., CTD cast).

The "over the past month" used in the previous version is not very accurate, and we have revised it to "*during the residence time of oxygen in the mixed layer*". The $\Delta(O_2/Ar)$ we obtained is a cumulative result that had been influenced by the physical (e.g., air-sea exchange, water mixing) and biological processes (e.g., respiration and photosynthesis) during the residence time of oxygen in the mixed layer. The influence of "environmental fluctuations" during that period could be reflected by the physical and biological processes mentioned above, which certainly had a contribution to the final NCP values we estimated. That's why we can catch the dramatic high NCP and negative NCP resulted from shelf water intrusion and upwelling respectively in the June cruise.

Here we have intended to clarify that our NCP estimate is a time-weighted result instead of a daily average result. If the environment is at the steady state (e.g., constant productive rate and constant wind), our estimate can be an actual daily NCP. But in reality, steady state is always violated because of the variable wind-speed (or gas transfer velocity) over time. We apply a time-weighted scheme to calculate the gas transfer velocity following Reuer et al.(2007) and Teeter et al.(2018), more heavily weighting recent periods and storm periods to erase the importance of earlier states. As a result, though our NCP result is in the unit of $mmol\ C\ m^{-2}\ d^{-1}$, it's not the actual daily NCP but represents an estimate of time-weighted sea-to-air biological oxygen flux over the residence time of oxygen before our observation of O_2/Ar.

Reference: Reuer, M. K., Barnett, B. A., Bender, M. L., Falkowski, P. G. and Hendricks, M. B.: New estimates of Southern Ocean biological production rates from O_2/Ar ratios and the triple isotope composition of O_2, Deep Sea Res. Part I Oceanogr. Res. Pap., 54(6), 951–974, doi:10.1016/j.dsr.2007.02.007, 2007.

Teeter, L., Hamme, R. C., Ianson, D. and Bianucci, L.: Accurate Estimation of Net Community Production From O_2/Ar Measurements, Global Biogeochem. Cycles, 32(8), 1163–1181, doi:10.1029/2017GB005874, 2018.

Lines 412-416: Clarify how the DIN and NCP criteria were chosen for each cruise.
Fig. 10b is not very compelling as the lowest MLD - highest volumetric NCP data point seems to drive the negative correlation. Thus, the authors should consider removing their analysis of June 2015 data from Fig. 10b, and just discuss the analysis in the text in relation to the much stronger relationship between MLD and volumetric NCP during October 2014. Another related analysis that may be interesting is comparing NCP values at stations where MLD is deeper than the euphotic zone depth, to NCP at stations where the MLD is shallower

than the euphotic zone depth.

Response: Thanks for your suggestion. The relationship shown in figure 10b is not convincing enough because of inadequate data points. In addition, during the June 2015 cruise, the euphotic zone was 2-7 times the depth of the mixed layer, thus it's not meaningful to discuss the light limitation in the summer. We selected 9 stations of October cruise where surface DIN concentration in the range of 0.10—0.17 μmol L^{-1} to make the analysis of NCP and light. Because of your comment and the comments of reviewer 1, we have noticed the limitation of our analysis between MLD and NCP_{vol}. The negative correlation between NCP_{vol} and MLD we obtained may partly result from that NCP_{vol} is calculated by NCP/MLD. Thus we calculate an average surface PAR by integrating the daily satellite-PAR data obtained from NASA ocean color website (https://oceancolor.gsfc.nasa.gov/l3) over the residence time of O_2 at each selected station in October 2014. 8-day PAR data were used to estimate the missing daily data. Then we use light attenuation coefficient (K_d) to calculate an average PAR in the mixed layer to make a correlation analysis with NCP. The results were shown in a new table (Table 4). This new analysis gives a result that light availability is not a limitation on NCP in the SCS, much more convincing than the former analysis just based on MLD.

The calculation of K_d basically based on Lambert-Beer law (Kirk 1994; Jerlov 1976):

$$K_{\mathrm{d}} = -\frac{1}{z}\ln\frac{E_d(z)}{E_d(0)} = \frac{4.605}{Z_{eu}}$$

Where K_d (m^{-1}) is the light attenuation coefficient in the euphotic layer; $E_d(0)$ is the PAR at the surface, integrating an average over the residence time of O_2 before our observation, in the unit of mol m^{-2} d^{-1}; z represents a depth (m) and $E_d(z)$ is the PAR at this depth; Z_{eu} is the euphotic depth (m).

Table 4. Satellite-PAR data and NCP at selected stations in October 2014

Station	Date of observation	MLD (m)	Z_{eu} (m)	Surface PAR[a] (mol m^{-2} d^{-1})	K_d (m^{-1})	ML PAR[b] (mol m^{-2} d^{-1})	NCP (mmol C m^{-2} d^{-1})
O-01	2014/10/13	58	82	42.0	$5.6 * 10^{-2}$	12.0	3.0
O-02	2014/10/13	64	74	42.0	$6.2 * 10^{-2}$	10.0	15.1
O-03	2014/10/14	56	84	41.1	$5.5 * 10^{-2}$	12.4	10.1
O-08	2014/10/21	49	72	38.7	$6.4 * 10^{-2}$	11.4	15.7
O-10	2014/10/15	68	81	40.0	$5.7 * 10^{-2}$	9.8	4.4
O-13	2014/10/16	48	52	39.2	$8.9 * 10^{-2}$	8.7	15.3
O-15	2014/10/22	49	68	38.6	$6.8 * 10^{-2}$	10.8	16.3
O-20	2014/10/18	35	61	39.2	$7.5 * 10^{-2}$	13.3	16.4
O-22	2014/10/17	76	102	42.2	$4.5 * 10^{-2}$	11.6	15.7

[a] Average surface PAR over the residence time of O_2 in the mixed layer. [b] Average PAR in the mixed layer.

References: Kirk, J. T.: Light and photosynthesis in aquatic ecosystems, Cambridge university press, UK, 1994.

Jerlov, N. G.: Marine optics, Elsevier, Netherlands, 1976.

Technical comments

Figure 1: Explain what the dots/markers in the panels represent. Are they the locations of the CTD casts? If not, it is worth adding the locations of the CTD casts to this figure so that readers may better understand the interpolation of MLD between casts for underway data.

Response: Yes, the dots and stars are the locations of the CTD casts. Following your suggestion, we have added an explanation of these markers to the caption of figure 1, "*the black dots/stars represent the locations of the CTD casts*".

Figure 3: Why were there more variables in the June cruise? This is not clear in the methods.
Response: Sorry for that. During the cruise in October 2014, the DO sensor of RBR broke down, and we did not make the standards for CO_2 calibration. Thus there are no data of DO and pCO_2 in that cruise. We have clarified this in the section 2.1 which reads:
"We didn't obtain continuous DO data in October 2014 because the DO sensor of RBR broke down during this cruise."
"The instrumental CO_2 ion current was calibrated at about 12–24 h intervals using equilibrated seawater standards as per Guéguen and Tortell (2008) during the survey in June 2015."

Figure 5: Write in the salinity units. It is worth clarifying somewhere in the figure text, as well as the main text referencing Fig. 5, that the temperature fluctuations shown here are too small to reflect upwelling.
Figure 6: Write in the salinity units.
Figure 8: Write in the salinity units.
Figure 9: Write in the salinity units.
Response: Thanks for your suggestions. We have added the units of salinity on these figures as well as figure 2 & 3. We have also clarified in the main text that the temperature fluctuations shown in figure 5 are too small to reflect upwelling.

Figure S1: This actually is referenced after Fig. S2 so consider switching the figure order. Why is [NO_3^-] omitted here?
Response: Thanks for your suggestion. We have switched the order of figure S1 & S2. The surface NO_3^- concentration was below the detection limit at all sampling stations during the cruise in 2014, thus we didn't make the plot for [NO_3^-]. We had clarified this in the figure caption that *"The surface concentration of nitrate (NO_3^-) at all sampling stations was below the detection limit during this cruise."*.

Figures S3-S7: These are not referenced in the text, but they should be if they are to be published. Otherwise, it is not clear what the significance of showing these data are, as they could just go on an online repository which gets referenced in the text.
Response: Thanks for your suggestion. These transects are not very representative, so we didn't discuss them in the text. We decided to delete these figures from the supplementary. But the data of these transects can be easily downloaded from the online repository we shared.