Review for 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 (bg-2020-448).

This manuscript present results of O_2/Ar ratios and $^{17}\Delta$ (^{17}-O excess) in water samples collected in profiles on the SEATS (SouthEast Asian Time-series Study) station located in the South China Sea. This method has been widely used in the ocean to determine aquatic primary productivity. The net oxygen production (NOP) is determined by discriminating the physical contribution to the dissolved oxygen concentration through measurements of the O_2/Ar ratio, and determine solely the biological input. The gross oxygen production (GOP) is determined by the quantification of the stable isotopic abundance in dissolved oxygen (^{16}O, ^{17}O and ^{18}O) and on the basis of the mass fractionation identify the presence of biotic (from photosynthesis) or abiotic sources (from the exchange with the atmosphere) of O_2 in the sample. The authors aimed to study the influence of seasonal monsoon forcing to the local aquatic primary productivity and to better identify oligotrophic and eutrophic stages.
For this, vertical profiles were collected to represent three seasonal phases: 1) during the transition of the monsoon seasons (16th October,2013), 2) during the southwest monsoon (SWM) in summer (June to September; samples collected in 5th and 6th August, 2014), and 3) during the northeast monsoon (NEM) in winter (November to April; samples collected in 24th and 25th April, 2015). Complementary measurements of dissolved oxygen and fluorescence were collected to support the observations from the isotopic analysis. The authors found that winter conditions allow for a shift from net heterotrophy to net autotrophy in the course of 24 h due to the influences of colder temperature and stronger winds. Although the authors did not quantify specifically vertical exchanges between the mixed layer and deeper waters, their vertical profiles hint to the exchange of productive deeper waters with less productive mixed layer water from summer. However, the highest productivity estimates were found during the inter-monsoon sampling in October 2013. The NEM might play an important role to control the metabolic balance at the SEATS in the South China Sea by shifting to net autotrophy mode, in contrast to the predominant net heterotrophic state in summer.

Although the O_2/Ar and TOI methods have been widely applied in ocean research, few studies in the past have focused on vertical sampling as the authors pointed out well (e.g. Juranek and Quay, 2005; Wurgaft et al., 2013) in the water column, despite several discussions have focused on the relevance of vertical influences to the mixed layer productivity determination. In this regard, the contribution of the vertical profiles from this study, and the focus on analyzing the changes in the different seasons, made a nice short manuscript that certainly falls within the scope of BG. My major concern is that the authors draw conclusions to characterize seasons (or annual trend) from scarce time data points (one profile per day in max. two days in summer or winter), and other previous observations should be used to better understand the SEATS station in the context of aquatic productivity.

In addition, the way that the manuscript is currently written does not meet yet the quality necessary to accept it for publication, and major changes are needed. Besides it requires a thorough language editorial review, it is oddly arranged with paragraphs that are included in the discussion but that actually belong to results, and vice versa. To be able to consider this manuscript for publication in BG, I encourage the authors to improve the current version substantially. I list below general and specific comments with the hope that these can support the authors to improve their current version.

General comments.

- The text requires a thorough English editorial check. In many places, the articles are missing to refer to e.g. "THE" SCS, or "THE" limit of the photic zone. I encourage the authors to let the revised version be read by a professional English editor or a native speaker to correct this recurrent grammar errors. Also, some sentences are hard to understand or the fluency of the text is hard to follow. This can also be alleviated by the contribution of a professional editor.
- The authors sampled in October 2013 (intermediate season), August 2014 (summer) and April 2015 (winter), and discuss these as a sequential annual set up, but do the authors think they would have found the same pattern if e.g. August 2013 and April 2014, or simply October 2015, were instead sampled instead? How the results could have changed if a true sequential year was sampled? Are there any anomalous events in between the presented sampling dates that could indicate that the results are not representative of true sequential seasons in a "normal" year? For example, the 2014-2016 ENSO influence?
- Aren't there any historical records in the SEATS station that could support the observations presented in the current study to better characterize the seasonal changes? For example, nutrients distribution, record of dissolved O_2 profiles? The authors made references to other studies, so they could make better use of the past data to interpret their current results.
- The authors discuss their results in the context of steady state mixed layer, but clearly one of the major contributors to their observations seems to be the diapycnal mixing and contribution from deeper water to the mixed layer primary productivity. The authors could estimate this mixing as it has been done previously (e.g. Castro-Morales et al., 2013; Seguro et al., 2019) on the basis of their vertical profiling data and quantify the vertical contributions.

Introduction.

The authors need to sustain their arguments with numerical evidence from the listed references. I make some specific cases in the list of comments below.

Methods.

There is no indication as to how Chlorophyll a was measured in the vertical profile. Also, a lot of relevant information such as: how many samples were collected from each depth (i.e. duplicate analysis)? They should also at least mention briefly the sample processing (preservation, extraction of gases).

How was the PAR obtained to define their PML depth?

The authors mention repeatedly "daily variation" in their results, but the methods description hint to a one-time sampling of each profile in one sampling date, so how are they able to discuss a time change variation during a day?

Discussion

All the paragraphs in lines 211-220, 221-228, and parts of the "October 2013" should belong to results and not to discussion section. Also, it is oddly arranged, since I would start chronologically from October 2013, August 2014 and April 2015.

How can the authors interpret an increasing value of $^{17}\Delta$ with depth while $\Delta(O_2/Ar)$ is decreasing? So apparent net heterotrophy by the consumption of O_2 is assumed due to negative $\Delta(O_2/Ar)$ (also vertically exchanged O_2 depleted deep water), but at the same time, increasing photosynthetic production of O_2 is assumed due to increasing $^{17}\Delta$ with depth? I think this is the representation of the vertical contributions to the ML both in summer and winter.

Use of notation.
There seems to be a mix up in the definition of the parameters. The variable $^{17}\Delta$ is the notation to represents the ^{17}O-excess or the triple oxygen isotopic anomaly of dissolved oxygen, defined with the Eq. 1. However, in some places the authors refer to $^{17}\Delta$ as the "triple oxygen isotope composition technique" (L62), the "triple isotopic analyses of dissolved O_2" (L70), or simply the "triple oxygen isotope composition" (L101). The authors should be careful on how they make use of the notation and how they refer to this parameter in the text.

List of detailed comments.

L22 – I wouldn't call $^{17}\Delta$ a "conservative" tracer since it is influenced by photosynthetic or atmospheric sources.

L25 – How is the SCS influencing the global biogeochemical cycling of C and O_2? Can the authors sustain this statement with some references? Especially in L29 the authors mention that the properties in marginal seas cannot be extrapolated to global scales.

L29 – which range of latitudes represent "mid-latitude shelves"?

L33 – this statement lacks of numerical evidence, how much is "high" annual surface temperature?

L37 – can the authors elaborate what do they mean with "unusual seasonal pattern in phytoplankton biomass"?

L36 – providing the values of the CO_2 fluxes will be helpful

L44 – "Owing to its **geographical** position …."

L45 – "the weaker southwest monsoon (SWM) in summer"

L46 – "the strong northeast monsoon (NEM) in winter"

L49 – How much are "medium chlorophyll a" concentrations?

L66 - which other sources?

L67 – "enables **an estimation** of the integrated gross productivity…"

L69 – “In order to evaluate the photosynthetic O_2 production, **and its contribution to the local** carbon balance…”

L72 – how are the authors defining here “deep water”? as of below the seasonal mixed layer depth?

L76 – “on board” instead of “aboard”

L99 – The authors provide here a very specific detail in the pre-processing of samples, but lack of more general and necessary information on the method, so either elaborate and complete this sentence or simply remove the detail.

L104 - Equation 1 should have $^{17}\Delta$ on the left to the equation

L124 – what are the mean ± 1 standard deviation values of the $^{17}\Delta$ in the equilibrated water samples?

L129 – Throughout the manuscript the authors refer to gross oxygen production as GP, so please also change it in Equation 2.

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

L135 – how much is “high” wind speeds? Also, these lines are hard to follow. They mention “O2 production time”, wouldn’t they mean O_2 residence time or O_2 production rate? By adding the units of the O2 concentration and GP, as well of K it will help the reader to understand this calculation.

L139 – here needs to refer for the first time to Table 1.

L141 – throughout the manuscript the authors refer to net oxygen production as NP, please change here NOP.

L154 – “the standard approach”? I am not sure there is a standard approach in this, this line might be rephrased.

L160 – I don’t think is useful to start the description of results by contrasting SEATS to HOT. Also, further comparisons to HOT should be done with caution since it is a tropical station with very different seasonal characteristics.

L166 – here it should be “August 2014”

L169 – What depths refer to a “shallow” mixed layer?

L176-181 – what is the reference value to say that Chl-a was generally low? With respect to what? Can the authors include here some reference values for this statement? Also, there are no uncertainty values added to these measurements? If the authors compare a range of 0.2-0.3 mg m^{-3} to 0.5 mg m^{-3}, are these values really different based on the uncertainty given in the measurements? Maybe also some statistical analysis will help here to understand if these values are significantly different or not.

L184 - how can the authors define “a daily component” if only a one-time point sampling was done?

L185-186 – how much is “low” $^{17}\Delta$, which depth range is defined for “upper $^{17}\Delta$ profiles”, which “values below”? the “mixed layer values” of what?

L190-197 – this paragraph is very hard to follow

L199-206 – this paragraph should belong to discussion and still I am not sure that a comparison to HOT brings something substantial to the results of this manuscript.

L215 – So far, the authors did not provide anywhere the typical $^{17}\Delta$ values for atmospheric O_2 (or at equilibrium) and for photosynthetic O_2, this will help the readers to understand the shift in values. Also, referring to Table 1 here will help to the reader and many of these lines can be skipped in results and only focus on the discussion.

L254-261 – This paragraph should rather belong to introduction

L263 – This paragraph should come earlier in the discussion and contains data that should be moved to results or referred in results to table 1.

L265 – Directing the reader to Table 1 should become a lot earlier in the manuscript in the results section. In this table the authors present production rates in terms of mg C m^{-2} d^{-1}, whereas in the text the present the results in terms of g C m^{-2} d^{-1}. Please decide one or the other for both presentations of data. Table 1 will also benefit if uncertainty values are added in the form of e.g. ±standard deviation if possible

L262-279 – How these results can be compared to the CO_2 fluxes reported by Tseng et al., 2005 (and briefly mentioned in the introduction?).

L268 – I think that to say that two days of sampling with little variation between measurements might be representative of summer is an overstatement.

L275-278 – the authors could estimate the vertical contributions of oxygen on the basis of their vertical profiling data, in this way they can rule out deep local production or vertical transport. Which processes can sustain new production at 30-50 m depth? What role can play processes such as photoinhibition and phytoplankton vertical migration due to enhanced mixing to the observed subsurface Chl-a maximum and deep higher productivity?

L281 – If the mixed layer depth is defined solely on the basis of temperature gradient, this can lead to shallower layers than in contrast are very different to mixed layers defined on other parameters related to metabolic processes. A mixed layer defined on the basis of density gradients or dissolved oxygen (e.g. Castro-Morales and Kaiser, 2012), might support better the mixed layer productivity observations, and will be perhaps be more similar to their photic layer depth. The authors might consider changing their ML definition as test.

L299-300 – Besides that the comparison between ^{14}C and the O_2 isotope is limited due to their methodological nature, the authors hint to the relevance of the seasonal shifts in the region to the local productivity, hence they should look at the sampling periods when the published ^{14}C incubations were made before considering a general decline in productivity rates.

L316 - something seems to be missing in the finishing sentence

L320 – I think this section in the discussion must be the center of the discussion since is the most important contribution in the manuscript (vertical $^{17}\Delta$ quantification), hence this should be the beginning of their discussion section.

L330-338 - This paragraph is oddly written with some missing or wrongly placed words. It needs to be improved, but also the placing within the manuscript should be considered. It provides a lot of details on the physics in the SCS, and it should be moved to a site description section at best. Throughout the manuscript, the authors did not present any data that intended to identify the water masses at SEATS in relation to the productivity data, so trying to interpret their results in this context is a stretch towards the end of the manuscript.

L340-354 – Following the above comment, this paragraph should be moved to an earlier part of the manuscript if the authors think it contributes substantially to their productivity observations. Further, the authors suggest that $^{17}\Delta$ might be a useful tracer of water mixing processes, hence the interpretation of their results in this regard should be made prominent in the manuscript. However, I think the data set is still too limited for this purpose.

L359 – Was not the highest GP recorded during the inter-monsoon period?

L360-362 – I think the authors cannot yet conclude this statement with the few data points they present, especially if no other data from traditional conservative tracers for this purpose is discussed in this manuscript.