Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-257-AC1, 2019

Interactive comment on "Decoupling of net community production and particulate organic carbon dynamics in near shore surface ocean waters" *by* Sarah Z. Rosengard et al.

Sarah Z. Rosengard et al.

srosengard@whoi.edu

Received and published: 22 October 2019

Dear reviewer #1,

Thank you for your close reading of the manuscript, and for the depth of your feedback. Based on these comments, we have revised the manuscript to provide: (1) greater focus in the results and discussion sections, and clearer articulation of the study's contribution to the scientific community, (2) a more detailed quantification of the diurnal balance among the organic carbon (POC) and dissolved O2 sources and sinks (primary productivity, respiration, vertical mixing and gas exchange), and (3) a more conservative approach to interpreting estimates of organic carbon partitioning into DOC production and particle export. In the following, the reviewer comments are shown in quotation marks followed by our response. In general, we have tried to respond to the comments in order; however, when several comments in one review are connected, we address them together. References cited in the responses are provided at the end of this document.

Reviewer #1 comments & responses:

"... I think that the manuscript needs to be better focused on clearer conclusions that arise directly from the results of this study. Put another way, after reading the manuscript, I am not sure exactly in what way the authors think that the specific results of this study have advanced scientific knowledge... Also, please be specific about why the findings are important."

We believe that the main contributions of the work are as both a methodological "proof of concept" and as a source of new information on mixed layer carbon dynamics in the Subarctic Pacific. With the growing use of autonomous measurements of POC from optical beam attenuation (cp) and dissolved O2 on ships, moorings and gliders, there is greater potential to quantify ecosystem metabolism and carbon flows in the ocean mixed layer, with high spatial / temporal resolution, and without potential artifacts associated with incubations. Understanding the differences between O2-based and C-based estimates is critical in this regard. As these autonomous measurements have seldom been compared directly in the same time span and location, our study advances understanding of the ecological conditions in which they agree or diverge. In particular, our study directly expands the analyses reported by Alkire et al. (2012) and Briggs et al. (2018) from the North Atlantic Bloom to a new ocean environment, in which seasonal upwelling plays a key role in the productivity of several important fisheries. This upwelling environment presents an interesting methodological challenge because vertical mixing can affect cp and $\Delta O_2/Ar$ time series. Thus, our study further illustrates a new approach to applying mixing corrections to POC and O2 mass balances.

Based on the reviewer's comments, we have made an effort to emphasize the importance of these key results in the revised discussion and conclusion sections of the manuscript. Specifically, we have rewritten parts of the introduction to emphasize the goal of this study (to compare two autonomous measures of NCP in a new environment), and have rewritten parts of the discussion and conclusion to demonstrate how the results support this objective. We hope that these amendments clarify how our work advances scientific knowledge of ecosystem metabolism in the Northeast Pacific Ocean, and the methodologies used to quantify these metabolic rates.

"The interpretation of the various optical proxies should be tightened up, clarifying the extent of empirical support for each proxy, the use of the term "diel cycles" should be clarified. . ."

In the Sect. 4.2, we have added a paragraph discussing the uncertainty associated with several more optical proxies than discussed in the original manuscript, to ensure that readers understand both the strengths and limitations of our interpretations. As part of our response to reviewer #2 we have also made efforts to elaborate the assumptions and limitations of these proxies in the introduction.

The term "diel cycles" refers to a daily pattern of change. Because our measurements do not consistently display cyclical behavior, we realize that the term "diel cycles" may be misleading in this study. Therefore, we have rephrased the term diel cycles as "diurnal variations" in all instances. We feel that this term is appropriate for our continuous measurements made across several day – night transition periods.

". . .one of the conclusions seems to be that this method promises to expand the coverage of export estimates. How exactly (autonomous or ship-based), and what accuracy can we expect?"

We acknowledge that there are several limitations to accurately estimating POC export in our study, especially as a result of the uncertainties in vertical mixing correction and POC loss to the DOC pool. We have expanded uponÂăthese limitations in the discussion and conclusion sections to remind readers of the caveats associated with our export estimates. As a result of these caveats, we removed POC export as quantity plotted in Fig. 4. In the conclusion we have further clarified how our work can inform future export estimates from autonomous or ship-based platforms.

"1. Is the main advance a "proof of concept" of the approach (simultaneously tracking net O2 production and net POC accumulation to constrain export) in two additional environments? If so, what are the criteria for success of this "proof of concept"? 2. Is the main advance some new knowledge about the functioning of the specific two ecosystems studied (i.e. Upwelling and offshore N. Pacific)? If so, what exactly have we learned and how does it differ from (or strengthen) previous understanding?... ...the only conclusion in the conclusions section that clearly comes from the data presented in this manuscript is that O2 and POC cycling is more "coupled" offshore than in the upwelling region. But to me this statement is vague; I don't understand exactly what it means or why it's important. It would be much clearer to say something like "we find that a higher fraction of production is exported in region X than region Y" or "O2/Ar-based NCP can be used as a proxy for carbon export in region X but not region Y"."

The aim of our study is to show how simultaneous ΔO2/Ar and POC time series measurements can provide quantitative estimates (with some caveats) of carbon export in marine regions of high vs. low productivity, and to suggest how such approaches may be valuable additions to more labor-intensive techniques such as the 238U- 234Th disequilibrium method. Specifically, we have endeavored to show that this approach is useful in an environment different from the North Atlantic bloom region. The major criterion for success of this "proof of concept" is the consistency of our measurements with what is known in upwelling and oligo/mesotrophic environments in the Northeast Pacific Ocean (for which there is a significant scientific literature). We expected more export in the upwelling site compared to the offshore waters, and therefore greater discrepancy between POC and ΔO2/Ar diurnal variability, as observed in Alkire et al. (2012) and Briggs et al. (2018). We have made changes in the manuscript discussion

Printer-friendly version

Discussion paper

and conclusions to make these criteria for proof of methodology more transparent, and have made our hypothesis regarding the differences in NCP comparisons in both drifter sites more explicit in the introduction. In particular, we have made efforts to explain why the contrasting results at the two drifter sites support the case for a proof of concept.

While we acknowledge that findings add little fundamental new knowledge to understanding of these two environments, this is not a primary conclusion of our study, as we have now clarified in the revised manuscript.

"For example, the authors tentatively conclude that their high O2/Ar-NCP estimates, if accurate, imply that both the widely-used CbPM model and the even more widely-used C-14 incubation method might be substantially under-estimating NPP in this environment. Even if this conclusion is not certain, if the authors believe that this is the most likely interpretation of their results, this conclusion is worth highlighting, because the accuracy of these NPP methods are of broad importance to the field."

We agree that this is an important result, and have emphasized it further in Sect. 4.3. But, because we do not think this is a main contribution of the study, given a limited data set to compare different NPP estimates, we did not add this result to the conclusions section.

"For NCP calculation, diel cycles (sub-daily data) are not really needed at all. All you need is the net change from the beginning (or end) of one day to the next. So the repeated claim that diel cycles are used to calculate NCP does not make much sense to me. Diel-cycle-based gross production estimates have their own uncertainties, but I think that they would be very valuable for the interpretation of the results of this study. The authors hypothesize that both NPP methods may be biased low. A finding that diel cycles-based gross production of O2 and POC are substantially higher than the NPP estimates would increase support for this hypothesis. A finding that gross production agrees with NPP would weaken support and suggest alternative interpretation (e.g. O2/Ar-NCP is over-estimated)."

NCP represents the difference between gross primary production (GPP) and community respiration, which we have calculated from the best-fit slope from linear regression of [O2]bio and POC against time within each day/night interval of both drifter deployments (Equations. 2, 8). Utilizing these sub-daily data maximizes the signal to noise ratio in the time series, minimizing the error of the best-fit slope. The reviewer's suggested approach is equivalent to taking the slope between two points at 24 hour intervals, which would introduce greater uncertainty into the NCP calculation. We have clarified this in the methods Sect. 2.6.

We have calculated two separate GPP approximations per drifter deployment day (White et al. 2017; Claustre et al. 2008), and have added them to a revised Table 1. In Section 4.3, we compare GPP and net daily [O2]bio accumulation to NPP.

"I don't find the authors' analysis of NCP to be completely clear. First of all, I don't think that the net accumulation of POC should be called "NCP". I think that it would be clearer to call it "net POC accumulation" or "NCP minus export and DOC production" or something else."

In regions where POC production and loss are in close balance, NCP has been derived from diurnal variations in cp-derived POC (Claustre et al. 2008; White et al. 2017). For consistency with these prior studies, we have chosen to use the term NCPPOC, but we now indicate in the revised methods section 2.6 that this NCPPOC is more accurately net POC accumulation.

"Second of all, I am not sure why the authors do not attempt to estimate NCP from nutrient drawdown. For example, the authors find a 0.9 _M drawdown of ML nitrate/nitrite over 3 days at site 1. If we naively convert that to carbon via redfield ratio and ignore mixing, doesn't that imply NCP of 2_M C per day or 38 mmol/m2 over a 19 m ML? If so, why is this number so much lower than the mean daily O2/Ar-NCP of 150 mmol/m2 over this period? Am I missing something? Can nutrient supply from below plausibly explain the difference? Regardless, a second calculation of NCP, using NO3

Printer-friendly version

Discussion paper

drawdown, would be very helpful in interpreting the main results of this study, which all hinge on the accuracy of NCP via a single method (O2/Ar)."

We thank the reviewer for this useful suggestion. As suggested, if we assume a Redfield ratio for organic matter production, cumulative nutrient (phosphate, silica, nitrate + nitrite) drawdown based on observations would equate to 36 to 50 mmol m-2 d-1 of organic carbon, which greatly underestimates NCPO2/Ar (Fig. 4). Most likely, upwelling of nutrient rich deep seawater has dampened the nutrient drawdown signal. Similar to our POC vertical mixing correction (Eq. 8), we have now applied the kmix term from Eq. 6 and calculated vertical gradient terms from nutrient profiles to correct for the effect of mixing on the apparent magnitude of nutrient drawdown. This calculation yields higher values, 101 to 132 mmol m-2 d-1 C, which are closer to NCPO2/Ar values, and thus provide support for our approach (Table 1). We address these calculations in the discussion section 4.1.1.

"So I would recommend removing or de-emphasizing the interpretation of bbp vis-à-vis phytoplankton carbon outside its more established use in the CbPM model."

We have removed all calculations of NCP based on diurnal variations in phytoplankton carbon, and have focused less on interpretations of diurnal variability in this backscatter-derived metric. We still report Cph concentrations in Fig. 2 to show the differences between drifter sites, but explain the limitations of interpreting such values in the discussion Sect. 4.2, as suggested by this reviewer.

"However, it is my understanding that the fraction of DOM that absorbs light at 440 nm is relatively small, and that this fraction consists mostly of refractory, humic-like substances, whose dynamics are driven primarily by circulation and photodegradation, not necessarily by recent in-situ production. As such, the authors' claim that CDOM absorption measurements can be useful for tracking the partitioning of fixed carbon into DOC (line 766) is not really supported by the literature."

Absorption at 440 nm by chromophoric dissolved organic matter has been observed

Printer-friendly version

Discussion paper

to track seasonal variations in [Chl-a] in several locations, and a number of studies have suggested that absorption is impacted by microbial degradation of organic matter in the mixed layer, in addition to abiotic processed like photooxidation (e.g., Nelson et al. 1998; Organelli et al., 2014). However, we do agree that measurement of CDOM absorption at one wavelength (440 nm) is limiting, and that absorption at lower wavelengths (e.g., <400 nm) and the spectral slope of CDOM absorption over a range of lower wavelengths (e.g., 275 – 295 nm) would shed more light on the sources and sinks of CDOM in the mixed layer (Del Vecchio and Blough 2004; Grunert et al. 2018). We have thus amended the conclusion section to clarify that both CDOM absorption and spectral slope measurements would improve understanding in NCP transfer to the DOC pool on daily to seasonal time scales.

"The authors interpret changes in bbp spectral slope as indicative of changes in particle size. While this interpretation does have a clear theoretical basis, and while this interpretation is widely used in the literature, the authors should be cautioned that, again, there is no strong empirical support for this relationship. In fact, a recent extensive study of bbp and particle size spanning a wide range of spectral slopes (-3 to 0) found no correlation with size at all."

We acknowledge that there are many limitations to interpreting the particulate backscatter slope in terms of particle size distribution and have revised the manuscript to better address these limitations. However, we find that the link between slope and size is generally sensible in terms of differentiating both drifter sites. Moreover, the conclusions we derived about particle size distributions from the back-scatter slope were supported by results from size fractionated [Chl-a] measurements, and HPLC pigment-derived phytoplankton size distribution.

"Line 51: Claustre et al. 2007 (a Biogeosciences discuss paper) citation should be replaced with the 2008 peer-reviewed Biogeosciences citation."

We have changed the citation date to the 2008 Biogeosciences paper in all instances.

"Line 592: Do you mean "low overall DOC accumulation", rather than "low overall DOC concentration"? Most DOC is highly refractory with long residence, so we don't really know much about total DOC concentration from recent DOC production."

Yes, thanks for pointing this out. We have changed "concentration" to "accumulation".

References in author response

Alkire, M. B., D'Asaro, E., Lee, C., Jane Perry, M., Gray, A., Cetinić, I., Briggs, N., Rehm, E., Kallin, E., Kaiser, J. and González-Posada, A.: Estimates of net community production and export using high-resolution, Lagrangian measurements of O2, NO3−, and POC through the evolution of a spring diatom bloom in the North Atlantic, Deep Sea Res. Part I Oceanogr. Res. Pap., 64, 157–174, doi:10.1016/j.dsr.2012.01.012, 2012.

Briggs, N., GuÃřmundsson, K., Cetinić, I., D'Asaro, E., Rehm, E., Lee, C. and Perry, M. J.: A multi-method autonomous assessment of primary productivity and export efficiency in the springtime North Atlantic, Biogeosciences, 15(14), 4515–4532, 2018.

Claustre, H., Huot, Y., Obernosterer, I., Gentili, B., Tailliez, D. and Lewis, M.: Gross community production and metabolic balance in the South Pacific Gyre, using a non intrusive bio-optical method, Biogeosciences, 5, 463-474, 2008.

Del Vecchio, R., and Blough, N. V.: Spatial and seasonal distribution of chromophoric dissolved organic matter and dissolved organic carbon in the Middle Atlantic Bight,ÂăMar. Chem.,Âă89(1-4), 169-187, 2004.

Grunert, B. K., Mouw, C. B., and Ciochetto, A. B.: Characterizing CDOM spectral variability across diverse regions and spectral ranges,ÂăGlobal Biogeochem. Cycles,Âă32(1), 57-77, 2018.

Nelson, N. B., Siegel, D. A. and Michaels, A. F.: Seasonal dynamics of colored dissolved material in the Sargasso Sea,ÂăDeep Sea Res. Part I Oceanogr. Res. Pap.,Âă45(6), 931-957, 1998.

Organelli, E., Bricaud, A., Antoine, D. and Matsuoka, A.: Seasonal dynamics of light absorption by chromophoric dissolved organic matter (CDOM) in the NW Mediterranean Sea (BOUSSOLE site), Deep Sea Res. Part I Oceanogr. Res. Pap., 91, 72–85, 2014.

White, A. E., Barone, B., Letelier, R. M. and Karl, D. M.: Productivity diagnosed from the diel cycle of particulate carbon in the North Pacific Subtropical Gyre, Geophys. Res. Lett., 44(8), 3752–3760, 2017.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-257/bg-2019-257-AC1-supplement.pdf

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-257, 2019.

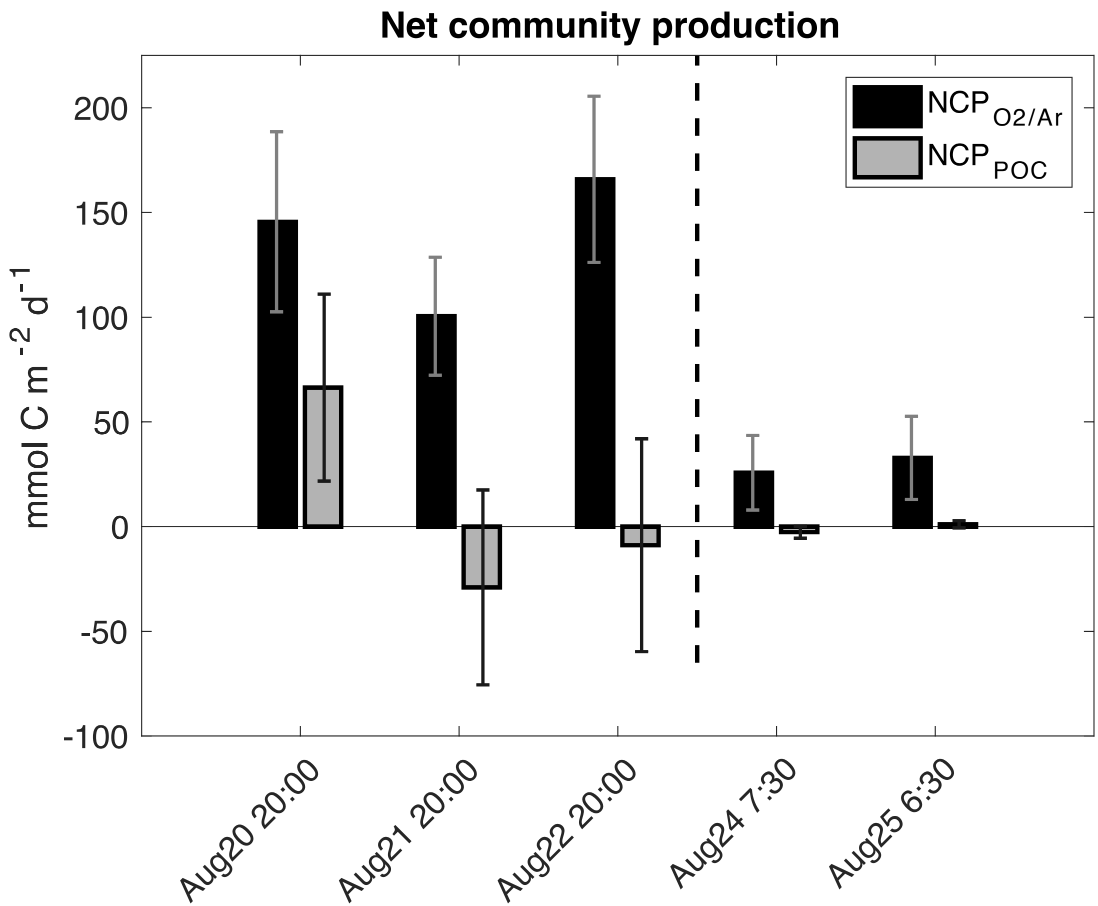

Fig. 1. Daily net community production (NCP)

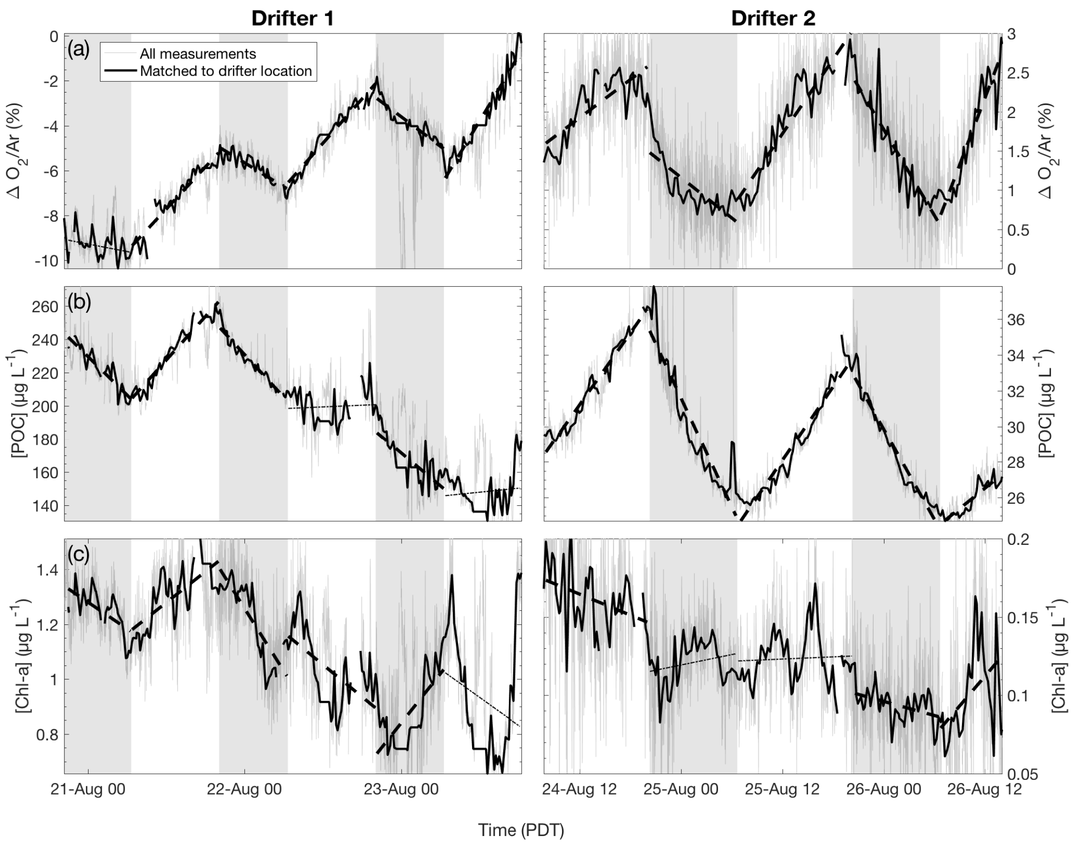

Fig. 2. Time-series of ΔO2/Ar, POC concentration and Chl-a concentration

Printer-friendly version

Discussion paper