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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.

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Dear reviewer #2,

Thank you for your close inspection of this manuscript. Based on your comments, we have revised the manuscript to provide 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 improved figures and table. 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 #2 comments & responses:

"Vertical mixing correction using N2O concentrations: given that the shallow mixed layer depths represented only a fraction of the euphotic zone, I wonder whether the N2O correction is suitable in this environment to estimate vertical mixing. Showing the N2O and O2 profiles (even if in the supplementary material) would be useful to assess this. As nitrification is photoinhibited, N2O concentrations typically start increasing below the euphotic zone. In addition, in the coastal experiment, I wonder if there could be other sources of N2O such as denitrification or lateral inputs."

As suggested, we have added N2O and O2 profiles to the supplement (Fig. S1), which indicate the depth of 1% of surface PAR as the base of the euphotic zone relative to the mixed layer depth. At drifter site 1, the average mixed layer depth (zmld = 19 m) is $\sim$14 m shallower than the average euphotic zone depth (zeu = 33 m). Regardless, the new figure shows that mixing ratio [O2]bio/[N2O]bio (a key term of the mixing correction in Eqs. 5-6) remains relatively constant with depth below the mixed layer, suggesting that it is not significantly altered by biological processes between the zmld and zeu depths. We excluded two additional CTD casts during which N2O and O2 concentrations were measured on the reasoning that they were likely influenced by an external water mass. Denitrification is an unlikely additional source of N2O because measured water column O2 concentrations during CTD casts never fall below $\sim$50 mmol kg-1 in the upper 100 m of the water column, well above the denitrification threshold (e.g., Hopkinson and Barbeau 2007). Notwithstanding the caveat that N2O profiles were only measured once per 24 hours, we neglect lateral advection of N2O into the drifter site because there is little change in [O2]bio/[N2O]bio between the two CTD casts used to apply a vertical mixing correction during drifter deployment 1, suggesting a water masses with similar N2O signatures. We discuss these assumptions in Sect. 2.6 of the revised manuscript.

“POC vertical mixing correction: the correction for the vertical mixing of POC uses POC concentrations at the DCM to estimate the gradients, even though the mixed layer is much shallower. Please explain how this might affect the flux estimates.”

Our correction for vertical mixing of POC uses a gradient between the mixed layer depth and the shallowest depth at which transmissivity profiles, collected by the CTD, reach their maximum values (i.e., minimal particle concentrations), below which there is little change in apparent particle concentrations. We acknowledge that there is an error associated with this assumption, as beam transmissivity does not equate to POC concentration. Specifically, a different ΔPOC/Δz gradient term affects the magnitude of the POC correctionÂă(Eq. 8), and thereby POC-derived NCP estimates. Although we do not account for this in our error propagation of NCP calculations, we have added an explicit assessment of this uncertainty in the methods and discussion.

“In general, I think the authors should provide the air-sea and vertical flux terms used to estimate NCP so that the reader has an idea of the magnitude of the corrections (for example, the magnitude of these corrections could be included in Table 1).”

We agree with the reviewer and have added these terms to Table 1.

“To better understand the discrepancies between both methods, I recommend including in the introduction a more comprehensive description of the assumptions required for each method.”

As part of our response to reviewer #1 we have elaborated on the assumptions and limitations of the methodologies in the introduction. Several limitations were already explained in the discussion (Sect. 4.2).

“The diel approaches have been mostly used to estimate GPP and respiration (R). To estimate NCP it is probably more appropriate to use the real-time changes in O2/Ar or POC (as per Hamme et al., 2012). I recommend including NCP calculated this way. “

It is problematic in our dataset to calculate instantaneous NCP because of noisiness

in the time series. We calculated NCP at three-hour resolution, and have found values to generally approximate our daily NCP values (Table 1). We have stated this in the methods section 2.6. Hourly resolution was insufficient for removing noise because the average data measurement interval after quality control (Sect. 2.1) was $\sim$15 minutes. Thus, we maintain that there is value to calculating NCP over daily time scales using daily rates of change in ΔO2/Ar and [POC] (Claustre et al. 2008; White et al. 2017).

“Also, I really encourage the authors to report GPP and R estimates based on the diel O2 and POC cycles, as these rates could provide some insight about the source of the discrepancies observed.”

As requested by reviewer #1, we have added GPP and R estimates to Table 1, and a discussion of how these separate estimates have contributed to the calculated NCP discrepancy in Sect. 4.1.

“Again, a better description of how different processes might affect the diel cycle of POC would be useful.”

We include a brief sentence describing the diurnal variations that would affect POC concentrations in the discussion Sect. 4.1.

“The authors argue that NCP estimates from the different methods agree during the second experiment, even though they even show opposite directions and POC-NCP is <5% of O2/Ar-NCP. Are the uncertainties so high that the difference between both methods is within error? Again, I would compare GPP and R, as they will have lower uncertainty.”

As Table 1 shows, the differences between NCP and GPP estimated by ΔO2/Ar and POC time series generally exceed the propagated uncertainties, while the differences between R estimated by both approaches were generally smaller than the uncertainties in R. We have added statements about NCP differences relative to error to discussion section 4.1.

“It is unclear to me why the drawdown of nutrients represents C export rather than NCP. Please elaborate on this. Also, do you expect the vertical mixing of nutrients to affect this estimate?”

We have corrected the manuscript, equating nutrient drawdown to NCP. As we wrote in our response to reviewer #1, vertical mixing dampens the magnitude of nutrient drawdown during drifter deployment 1. We have now added a vertical nutrient mixing correction to the results and discussion sections of the revised manuscript. The NCP estimates, derived from mixing-corrected nutrient drawdown, show good coherence with our $\Delta O_2/Ar$-based estimates, which is encouraging.

“Given the wide range in the reported fraction of NCP that goes into the DOC pool, I doubt that using a value of 40% to estimate DOC production and C export is justified.”

We agree with that there is considerable uncertainty associated with the assumption that 40% of NCP is transferred to DOC, which affects our POC export calculations. We have added additional discussion to Sect. 4.1 addressing the implications of this uncertainty on POC export calculations.

“L47-49 I agree with reviewer 1 that the diel cycle is not needed to estimate NCP, but rather it is useful to estimate GPP and R.”

As in our response to reviewer #1, we maintain that utilizing sub-daily underway $\Delta O_2/Ar$ and cp time series to calculate NCP is advantageous over extracting time points every 24 hours to estimate NCP because the former approach reduces error in ΔO_2 or ΔPOC (Claustre et al. 2008; White et al. 2017). This is expressed in methods section 2.6, as well.

"L50-51 In oligotrophic regions a significant fraction of phytoplankton total production goes to the DOC pool (Karl et al. 1998)"

We amended this sentence as suggested by the reviewer.

"L60-61 “ providing and indirect measure of carbon export out of the mixed layer”. NCP

is only equivalent to C export at steady state and over long timescales"

We removed this clause from the sentence.

"L197-198 do you mean 0.125 kg/m3 instead of 0.25 kg/m3?"

No, we did mean 0.25. We found that a higher critical density difference matched with the visible inflection points in density profiles from CTD casts during drifter deployment 2. Smaller differences were insufficient to capture the depth of inflection. We have clarified this in the methods section 2.3.

"L299-300 was there a gradient in N2O below the mixed layer? If not, the lack of supersaturation might not be a good indicator of the absence of mixing."

There is a vertical gradient in N_2O from surface to below the mixed layer (Fig. S1 in revised manuscript). According to Izett et al. (2018), the sub-saturation of $[N_2O]_{bio}$ in the mixed layer at drifter site 2 invalidates the vertical mixing approach in this environment. Furthermore, satellite-derived and underway temperature imply little upwelling at drifter site 2 (Figs. 1, S2). This observation is consistent with expectations of more stratified offshore water columns with limited vertical mixing.

"L591-592 This sentence is unclear."

Reviewer #1 had similar thoughts, and in response we changed the word “concentration” to “accumulation”, attempting to clarify that a higher DOC/NCP ratio would still result in little absolute DOC accumulation rates at drifter site 2.

"Table 1 Why are the NCP-POC results not included in the table? I suggest adding to this table all the terms used for the calculation of NCP, that is, the corrections for vertical mixing and air-water gas exchange"

We have added all these components to Table 1.

"Figure 1. The resolution of this figure is not very good. The “x” symbol indicating the initial release of the drifter is hard to see."

We have fixed a typo in the description for this figure. The diamond symbol shows the location of initial drifter release in the center panel; there is no “x” symbol. We have made the diamond red and increased the font size of the color bar to make this figure clearer.

References in author response

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.

Hopkinson, B. M., and Barbeau, K.A.: Organic and redox speciation of iron in the eastern tropical North Pacific suboxic zone,ÂăMar. Chem.,Âă106(1-2), 2-17, 2007.

Izett, R. W., Manning, C. C., Hamme, R. C. and Tortell, P. D.: Refined estimates of net community production in the Subarctic Northeast Pacific derived from ∆O2/Ar measurements with N2OâĂŘbased corrections for vertical mixing, Global Biogeochem. Cycles, 32(3), 326–350, 2018.

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-AC2-supplement.pdf

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

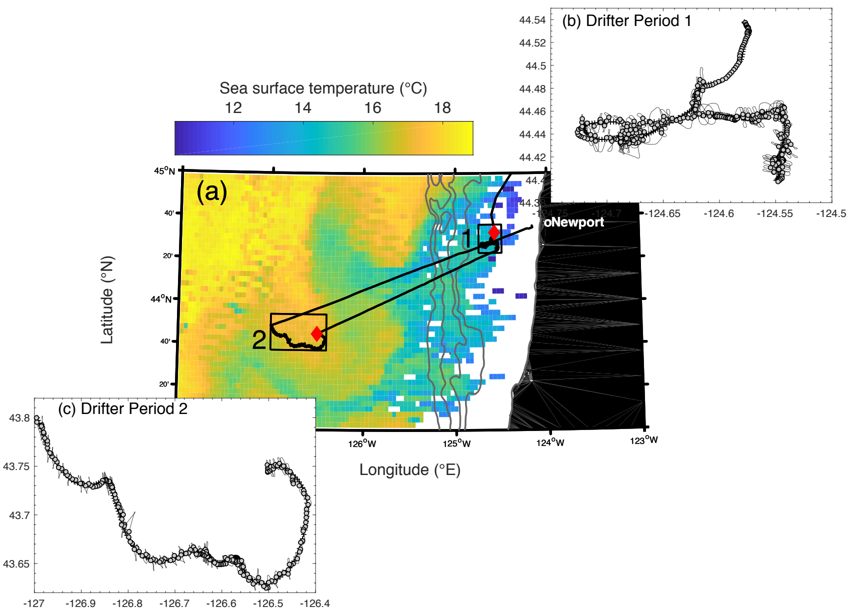

Fig. 1. Drifter deployment map

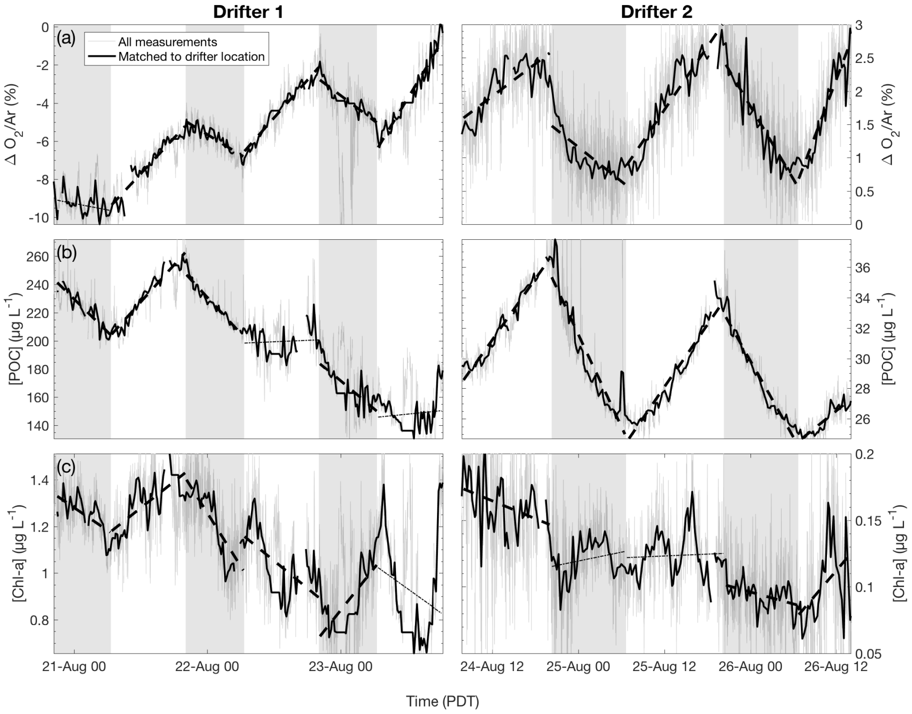

Fig. 2. Time-series of △O2/Ar, POC concentration and Chl-a concentration

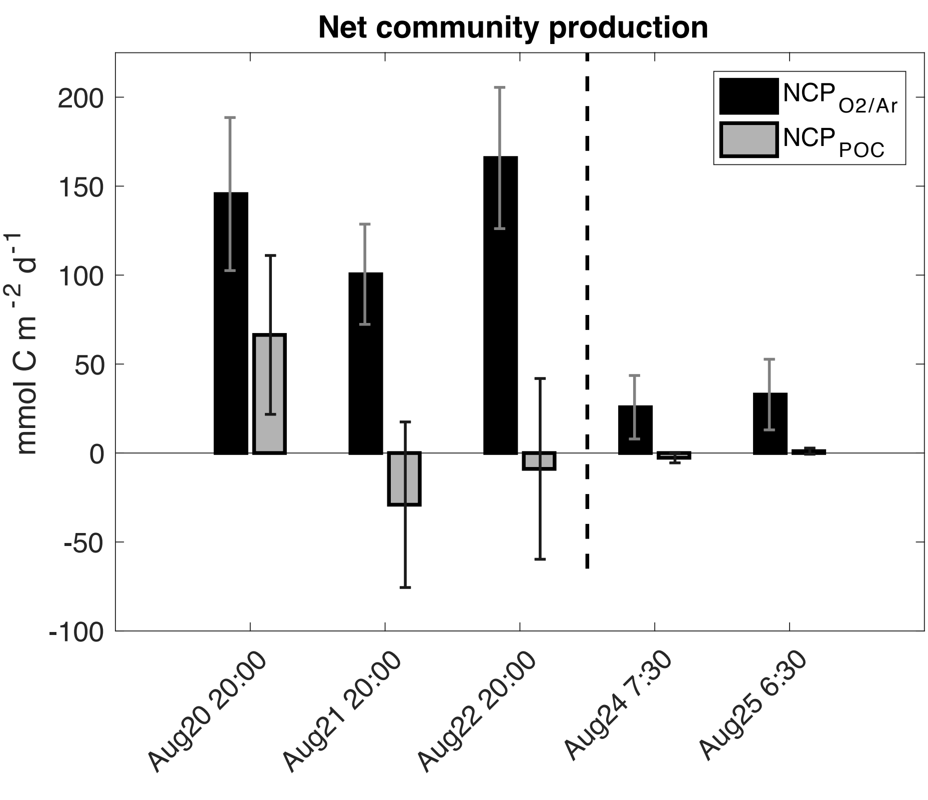

Fig. 3. Net community production (NCP)