

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.

Anonymous Referee #1

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Summary:

This manuscript analyzes two Lagrangian mixed-layer timeseries of particulate beam attenuation and biological oxygen saturation together with an array of complementary data in order to track the flow of organic carbon in two contrasting North Pacific environments. They find high net primary productivity (estimated via two methods) and net community productivity (estimated from biological oxygen saturation) in a coastal upwelling environment off the Oregon coast and much lower productivities further offshore. In both environments, NCP was similar to or higher than NPP estimates and substantially higher than net POC accumulation (calculated from changes in optical

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beam attenuation). The difference between NCP and POC accumulation, combined with previous results on DOC release, implies that 13-45

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General Comments:

For the most part, the text is clear, and the methodology appears thorough. I agree with the authors' general conclusion that, in principle, the combination of high-resolution POC (via cp) and O₂/Ar timeseries is valuable for understanding carbon flows. However, 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.

Overall, I recommend major revisions before publication. 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, and the manuscript should be refocused around clear conclusions that stem directly from the results of this study. Also, please be specific about why the findings are important. For example, 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? This manuscript estimates export at 13-45

I see two main possibilities for the main conclusion of the manuscript (although I am open to others if they are clearly articulated and supported by the data):

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"? What if all O₂/Ar-NCP values had been 50

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

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we learned and how does it differ from (or strengthen) previous understanding? To me, 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”. To me, it actually looks like O₂-based NCP substantially exceeds POC accumulation in both environments. While the absolute difference is smaller in the low biomass region, it is not clear that the fraction of NCP that ends up as export and/or DOC is lower in the low biomass site.

Of course, the conclusions can be a combination of methodological validation (1) and oceanographic findings (2), but in any case, the conclusions need to be articulated clearly and directly connected to the results. Put another way, much of the conclusions section seems like it could have been written without seeing the results of this study. This makes the value of the work less clear.

In addition to the main conclusion(s), I think that some secondary conclusions could be better highlighted. 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.

Finally, in conjunction with improved focus on conclusions, I think that two analyses should be strengthened and clarified (POC and O₂ Diel cycles, and nutrient draw-down) and three other analyses should be de-emphasized due to large uncertainties (phytoplankton carbon diel cycles, the CDOM-DOC connection, and the bbp spectral

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slope-particle size connection). See details below.

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Analysis of “diel cycles”:

The authors refer many times in the manuscript to their analysis of “diel cycles”. To me, “diel cycle” refers to the change in the balance of production and respiration over the course of a single day (esp. the difference between night and day rates of change). Analysis of diel cycles allows estimate of gross primary production. The authors cite many papers that calculate gross production or a related quantity from both O₂ and/or cp diel cycles, but for some reason, despite independently quantifying night-time and daytime rates of change, the authors do not gross production of oxygen or carbon from these diel cycles. Instead, they add night and day gain/losses together to calculate NCP. 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).

Analysis of NCP:

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. 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 μM drawdown of ML nitrate/nitrite over 3 days at site 1. If we naively convert that to carbon via redfield ratio

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and ignore mixing, doesn't that imply NCP of $2\mu\text{M}$ 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 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).

Phytoplankton carbon diel cycles:

To my knowledge there is no evidence that bbp diel cycles reflect diel cycles in phytoplankton biomass. In fact, I think that both previous literature and the results presented in this manuscript point to the opposite conclusion – bbp is not closely correlated with phytoplankton carbon on a sub-daily timescale. I therefore do not agree with the author's interpretation of diel cycles in bbp as diel cycles in phytoplankton biomass. My reasoning is as follows:

Phytoplankton carbon is produced during the day and consumed at night (through phytoplankton respiration and grazing), so we expect phytoplankton carbon to show a strong diel cycle, with a local maximum near dusk and a minimum near dawn. Repeated studies show that while cp diel cycles follow this expected pattern, bbp diel cycles often do not. During this study, diel cycles in bbp were observed in the upwelling site (similar to cp), but in the offshore site, diel cycles in cp were much stronger than bbp diel cycles. This is the opposite of what we would expect if bbp tracked phytoplankton carbon and cp tracked overall POC.

In addition to this empirical evidence, the latest theoretical work also does not suggest that bbp is more closely connected with phytoplankton carbon than cp. Coated sphere models suggest that roughly the same size classes of marine particles contribute to cp as to bbp (<https://doi.org/10.1038/s41467-018-07814-6>). If this is true, then differences in bbp and cp diel cycles should probably be interpreted as diel changes in particle composition (e.g. refractive index and/or internal structure) rather than a difference

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between phytoplankton and POC diel cycles.

Graff et al. (2015) did find that phytoplankton carbon was better correlated with bbp ($r^2=0.69$) than with cp ($r^2=0.42$) across a range of mostly tropical and subtropical sites. However, these results say nothing about correlation on diel timescales and still imply that bbp is more strongly correlated with POC ($r^2=0.74$) than with phytoplankton carbon. So while bbp may be a useful proxy of approximate phytoplankton carbon for the purpose of productivity modeling, this does not imply that bbp-based phytoplankton carbon (based on a single empirical study and not validated in this environment) is accurate enough track diel or even 2-3 day changes. 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.

CDOM and DOC:

The authors interpret differences in dissolved matter absorption as indicative of patterns in DOC (e.g. line 587). If these absorption measurements are correctly blanked (sometimes a challenge for AC-S data), then absorption of filtrate at 440 nm should indeed represent primarily colored dissolved organic matter (CDOM), which does contribute to DOC. 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.

Bbp spectral slope and particle size:

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

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study of bbp and particle size spanning a wide range of spectral slopes (-3 to 0) found no correlation with size at all.

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Line by line comments:

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

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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.

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

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