Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-257-RC2, 2019

Biogeosciences Discussions

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 #2

Received and published: 30 August 2019

This manuscript presents results from two Lagrangian 3-day experiments off the coast of Oregon conducted in two different environmental settings: coastal upwelling and offshore conditions. The authors used the diel cycles of O2/Ar and beam attenuation (as a proxy for POC) to estimate mixed layer NCP, and attributed the large discrepancies between both methods to C export and DOC production. These relatively novel methods avoid the potential artifacts associated with incubations and their application from autonomous platforms (equipped with O2 sensors and transimissometers) have the potential to increase the spatial and temporal coverage of metabolic rates in the ocean, improving our understanding of the oceanic C cycle. Understanding the differences between O2-based and C-based diel rate estimates is therefore critical and as such I

think this manuscript will make an important contribution to the literature. However, I have a few general concerns that I think need to be addressed before publication.

General comments

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.

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.

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).

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.

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. 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. For example, if we can assume that C export and DOC production are constant throughout the day, POC-GPP estimates would not be affected

by this carbon loss (see White et al., 2017, supplementary information) and therefore, POC-GPP should agree with O2/Ar-GPP. Under these circumstances POC-R would be overestimated and NCP underestimated. However, a diel cycle in C export, DOC production/consumption, and grazing would affect both GPP and R POC estimates. Again, a better description of how different processes might affect the diel cycle of POC would be useful.

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.

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?

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.

Specific comments

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

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

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

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

L299-300 was there a gradient in N2O below the mixed layer? If not, the lack of super-

saturation might not be a good indicator of the absence of mixing.

L591-592 This sentence is unclear.

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

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.

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