

Review of Stephens et al. An upper mesopelagic zone carbon budget for the subarctic North Pacific

This article presents a suite of ship-based and autonomous measurements from the EXPORTS program, examining carbon mass balances in the upper mesopelagic zone of the NE Subarctic Pacific. By combining a range of methods / approaches with different assumptions, strengths / weaknesses and integration time-scales, the authors aim to better constraint on the fate of organic carbon in the top 100 – 500 m of the water column at Ocean Station Papa. Based on a careful and thoughtful analysis of many different data sets, the authors report an apparent imbalance in carbon supply / demand (excess demand), which they justify based on temporal scale imbalances of processes and measurements. Specifically, they suggest that a seasonally-active DOC pool, associated with production occurring prior to their measurements, was required to meet the estimated C demands. Another notable result was the apparent importance of active carbon export associated with zooplankton DVM processes, which have previously been often overlooked in models. Overall, I think this is an interesting and well-executed study, and that the authors do a good job of discussing the limitations, caveats and method-specific assumptions of their work. I do, however, have a few suggestions that I believe would improve the presentation and clarity of the paper.

Specific Comments:

Abstract:

I find the following sentence to be somewhat confusing: *This imbalance could be resolved by particle dynamics influencing timescales of organic carbon utilization prior to the field campaign*

Perhaps simplify / rephrase as: 'resolved by the production and export of organic carbon prior to our measurement period'

Line 45: sentence beginning with NCP is missing a verb. Maybe add 'were' before 'measured'?

Figure 1. Legend isn't clear – I presume that 18 and 19 represent 2018 and 2019, respectively, but that could be more explicit. Small thing, but maybe put in a little ship icon for the OOI cruise, for consistency with the other cruises identified. And maybe stack the bars in the figure by sampling type (cruises together, then mooring, then gliders and float).

Line 163/4. How do you calculate DIC with only pH measured? As I'm sure they know, one other carbonate system parameter has to be estimated or measured. On line 256, the authors mention the CANYON-B algorithm. Is this what was used here?

I find it a bit strange to see a mixture of methods and results in the same section. I realize that the study uses some published data, obtained from methods that have been described previously, but I would have found it useful to have at least a short (but systematic) run-down of the different methods. Given the author's focus on the different assumptions / integration time-scales across methods, it would be nice, I think, to have this spelled out explicitly up front. Later in the text, there is a nice presentation of limitations / caveats (Table 1), and I think it should be referenced here. I still had some questions about the methods (see below), that could have been addressed with a bit more detail on methods.

Figure 2. Maybe I'm missing something obvious, but I don't understand how the POC flux can be 10-times higher than NPP.

Line 260. In the O₂- based NCP calculation, were their corrections made for non-biological effects on O₂ saturation state (e.g. S and T changes and bubble injection)?

Line 295. How was bacterial growth efficiency measured? On line 585, there is a reference to BGE measurements made by Stephens 2020. Is this what is being referred to here?

Figure 4. This is a nice summary, overall, but I don't understand how NCP can be less than NPP. What am I missing?

Line 523 at the end 'base don' should be 'based on'.

END OF REVIEW