We would like to thank the reviewer for their thoughtful comments and suggestions. NOTE: In the following, we have used blue text to highlight our responses to the Reviewers' comments.

REVIEWER 1

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'

Response: We will revise this sentence based on the reviewer's recommendation.

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

Response: This will be revised to "*Net community production (NCP) rates measured during the preceding spring and early summer of 2018 based on long-term mooring estimates of dissolved inorganic carbon concentrations were higher than those measured during the EXPORTS field campaign.*"

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).

Response: These are good suggestions and will be adapted into a revised Figure 1.

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 alrgorithm. Is this what was used here?

Response: The reviewer's concern here is understood. We will remove the unnecessary phrase, "allowing for the estimation of dissolved inorganic carbon (DIC) concentrations."

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.

Response: We thank the reviewer for this thoughtful comment, it was an issue we discussed at length prior to manuscript submission. We agree that this could also be better addressed up front, prior to presenting the data, for the reader's reference. Starting on Line 165 of the originally submitted manuscript we have not mentioned methods limitations / caveats. Therefore, we have adapted the reviewer's suggestion to more explicitly spell out in this paragraph that we present limitations / caveats later in the manuscript. We will amend the paragraph starting on Line 165 with the italicized text:

> "Several of the datasets presented here have been previously published, and the uncertainties surrounding conversion factors are discussed in those publications (e.g., as highlighted above). Therefore, only a brief description of methods is included here. *However, a detailed comparison of the methods assumptions / limitations can be found in Table 1, along with a brief discussion of how the methods may influence our interpretation of the results.* The present study's goal is to evaluate the combined rates of organic carbon supply and demand and elucidate the implications of carbon conversion factors and uncertainty estimates."

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

Response: Thank you for checking about this. The subplots in Figure 2 are only for the sub-euphotic zone (i.e., deeper than 95 m), where NPP rates are inherently low. Depth-integrated 14C-based NPP rates from 0 to 100 m averaged 13.8 ± 1.9 mmol C m^{-2} d^{-1}, compared with mean ^{234}Th-based POC flux rates at 100 m of 2.0 ± 0.6 mmol C m^{-2} d^{-1}. We will add the average integrated euphotic zone NPP value in the introduction at Line 111 for quick reference for the reader as follows: "*Below the EZ, POC sinking fluxes were also relatively low, with an export efficiency of 10-14% (POC flux of 1.4-2.0 mmol C m^{-2} d^{-1} vs. integrated NPP of 13.8 mmol C m^{-2} d^{-1} over 0-100 m), similar to previous late summer estimates at the study site (Buesseler et al., 2020; Estapa et al., 2021).*"

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?)?

Response: Yes, mixed layer NCP estimates were corrected for non-biological effects. The following sentence will be added at Line 260 to ensure readers are made aware of this: *"Corrections for non-biological effects were applied based on changes in temperature and salinity and due to the effects of bubble injection (e.g., Emerson et al., 2019)."*

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?

Response: Yes, that is correct. The following sentence will be added at Line 296: *"BGE was estimated based on concurrent increases in bacterial cell carbon and decreases in total organic carbon over time in dark incubations conducted throughout the EXPORTS field campaign."*

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

Response: NCP better incorporates the effects of heterotrophic respiration and so is a better "export potential" metric to compare with our combination of measured supply terms.

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

Response: Thank you for catching this error, it will be revised to "*based on*."

END OF REVIEW