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Reply on RC1

Chiho Sukigara et al.

Author comment on "Geophysical and biogeochemical observations using BGC Argo floats in the western North Pacific during late winter and early spring, Part 2: Biological processes during restratification periods in the euphotic and twilight layers" by Chiho Sukigara et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-116-AC1>, 2021

Reply for Referee #1

Thank you for your kindly comments concerning the manuscript entitled "Geophysical and biogeochemical observations using BGC Argo floats in the western North Pacific during late winter and early spring, Part 2: Biological processes during restratification periods in the euphotic and twilight layer" which we submitted for publication in Biogeosciences. We are studying all your comments carefully and reply to your comments as follows.

Main issues are:

(1) BGC-Argo floats are not Lagrangian floats, so sections of oceanic properties have to be interpreted with caution. Observed changes are not necessarily temporal changes, as the float can move across different water masses. This is particularly true in highly energetic regions such as the Kuroshio Extension region. Calculating production or consumption rates requires that the floats track the same water masse. Here, the authors acknowledge that for 3 of the 4 events analysed, floats may have been tracking different water masses due to the presence of eddies. So how can we trust the production/consumption estimates? Also, when calculating these rates, it is worth mentioning that you neglect diffusive fluxes of O₂ and NO₃.

Author Comment (AC): As you pointed out, we focused on four evens (Case 1 to 4) and discussed their biogeochemical changes. Of those, Case 4 was the only one which we could trace the same water mass. We should have discussed the physical processes of the water mass before we discuss the biogeochemical processes. In the revised manuscript, we will revise it to include an enough physical discussion. Also, we calculated the net flux of O₂ and NO₃. Therefore, we cannot calculate diffusive fluxes of O₂ and NO₃, but we did not ignore them.

(2) POC production is not equivalent to net community production (NCP), as a fraction of the fixed carbon is released as DOC (22 to 40% in the North Atlantic, Alkire et al. 2012). NCP is also different from NPP (NCP=NPP-heterotrophic respiration), so it makes no sense to compare your POC production to NPP. Also, you argue that deviation from the Redfield ratio in the mesopelagic is due to remineralization of DOC (and not only POC). But the

same argument stands for the C/N ratio in the surface layer. Production of POC alone is not supposed to reflect the total N consumption. See also comments on the Redfield ratio in the section below.

AC: Thank you for your comment. I understood that we were estimating POC production and not NPP or NCP. In the revised manuscript, we will discuss the estimate of POC production. And we will compare it to the CN ratios reported in the past, considering DOC production.

(3) The authors refer throughout the manuscript to temperature, salinity, wind, net heat flux and SSH, but none of these variables are shown. I understand that some of these variables are probably shown in the companion paper, but it is a bit frustrating not seeing them. You could at least show temperature and salinity sections.

AC: Thank you for your comment. In the revised manuscript, we will add figures of temporal variation of water temperature, salinity, and heat flux and explain them as well.

(4) Regarding the form, I think the results section contains only 'basic' observations/results, while most important results are drowned in the discussion. The most interesting figure (figure 7), from my point of view, is only introduced and discussed in the conclusion. Also, I think a statement of the objectives of this study is missing in the abstract. I found the quality of the writing to considerably decrease over the course of the paper. I had difficulties to understand some of the discussion/conclusion sentences. The writing clearly needs to be improved.

AC: I appreciate your comments. For figure 7, we will explain the temporal variations about materials (O₂, POC, NO₃) and discuss the degradation process in the twilight layer in the discussion section. We will add the objective of this study in the abstract. We will rewrite the manuscript and make it more readable.

Further detailed comments are listed below:

line 26: How do you calculate the euphotic depth? From what data?

AC: Since our CTD was equipped with a PAR sensor, we used the data to determine the depth of the euphotic layer. We will describe this in the revised manuscript.

lines 27-29: I am not sure to understand this sentence. Do you validate the quality of the sensor by comparing your C/N ratio to the Redfield ratio? If your ratio was significantly different from the Redfield one, would you conclude that the difference is due to the sensor quality or accuracy? Several studies actually demonstrated that organic matter exhibits widely varying proportions of carbon and nutrients, partly reflecting seasonal and spatial changes of the phytoplankton community structure (Green & Sambrotto 2006, Weber and Deutsch 2010, Martiny et al 2013,...). So, I think comparing your local ratio with the global average Redfield one is not very conclusive.

AC: We considered the sensor corrections to be approximately current because the ratio of the decrease in nitrate to the increase in POC obtained by the two independent sensors was on the same order of magnitude as the Redfield ratio when the sensors observed in the same water mass. However, we also understand that CN ratio can vary with region and season. In the revised manuscript, we will compare the CN ratios with those reported in the past.

line 72: add biomass or concentration, "increase in phytoplankton biomass".

AC: We will add "20 ug chl.a m⁻²" to the revised manuscript.

line 72: "lower depths" or deeper depths?

AC: "deeper depths". I will rewrite them.

lines 129-130: the presence or absence of optical spikes depends, among other things, on the vertical resolution of acquisition. It is pretty obvious that POC profiles from discrete water samples will have no spikes. Your sentence makes no sense.

AC: We will delete this sentence.

lines 135-136: not clear to me.

AC: These sentences are not so important. So, we will delete these sentences.

lines 131-150: this paragraph should be moved to the Methods section.

AC: We will move these paragraphs to the Methods section.

line 143: Rembauville instead of Rambauvill.

AC: I am sorry. We will rewrite the name.

line 144: "went south to 32N", not true.

AC: We will rewrite to "32.4N".

line 167: "there was no exposure of", exposure to what?, not clear.

AC: The word "exposure" meant that the 25.3 sigma-theta water mass did not come to the surface of the ocean. We will rewrite this sentence.

lines 170-171: is it temporal or spatial variation? as the floats moved ~300 km northward.

AC: We described this sentence in terms of temporal variation. However, we will add a description of spatial variation as well.

line 171: what is the middle layer?

AC: We wrote "the middle layer" to mean "below the euphotic layer". We will rewrite it as "below the euphotic layer".

line 177: respiration also occurs in the euphotic layer.

AC: It was an inaccurate sentence. we would rewrite "beneath the euphotic zone" as "throughout the water column".

lines 185-190: not clear from the figure. Maybe the colorbar of the figure should be adjusted to better see the variations.

AC: We will redraw figures. The coloring will be adjusted.

lines 200-202: not clear from the figure.

AC: We will add some marks in figures to indicate where we focus.

lines 207-208: "Chl values increased slightly in the surface layer after the deepening of

the mixed layer”, where? it is not clear from the figure.

AC: We will add some marks in figures to indicate where we focus.

lines 208-209: phytoplankton stock can also increase during winter mixing, not only once mixing ceases. This is not visible from Chla concentration records due to dilution when the MLD deepens, but it is from depth-integrated biomass records.

AC: We will integrate Chla to the deepest mixed layer depth during the observation period, and discuss that as well.

lines 218-221: You are comparing local POC to Chla ratios with worldwide Cphyto to Chla ratios. That makes no sense (average phyto contribution to POC is ~30%). It is a weak demonstration that Cphyto is correlated to POC. I recommend the authors to refer to publications that investigated the Cphyto-POC(bbp) relationship (Behrenfeld et al 2005, Martinez-Vincente 2012,2013).

AC: Thank you for the literature review. We will discuss the relationship between Cphyto-POC(bbp) in our results and literatures.

line 242: The link to the Japan Meteorological Agency homepage is useless. It is more appropriate to show direct wind or net heat flux records.

AC: We will add the figures of heat flux.

line 244: “it would shoal rapidly between disturbances”, why? Are net heat fluxes positive during this period? No data shown.

AC: We will add some sentences about the temporal variation of heat flux.

line 254: POC production was 126-664 mg C m⁻² d⁻¹. Does this range of values correspond to the 4 events from both floats? Which one is the most intense? and why? Comparing these values with NPP from another study makes no sense (see my general comments).

AC: 126 mg C m⁻² d⁻¹ corresponds to Case1, and 664 mg C m⁻² d⁻¹ to Case4. However, values of Case1 cannot be compared because the water mass had changed during the observation. We will use only the results of Case4 for this sentence. We will also discuss the results as POC production, except the comparison with NPP from other study.

line 261: “replacement of water masses”, what do you mean by “replacement”?

AC: We used “replacement” in the sense of changing to another water mass. If it is confusing, we will change the word.

line 265: “After each storm, the near-surface layer in Case 4...”. Is it true for each storm or only case 4?

AC: We will remove “After each storm,”.

line 272: What is a “time-series cross-section of nitrate profiles”?

AC: We will remove “cross-section of”. We meant a time-series nitrate profiles as figures 3g and h.

line 275: “a closed environment”. This term is not appropriate.

AC: We will revise this sentence in the new manuscript.

lines 285-286: "the warm water mass on the west side", which one? No temperature data.

AC: I will add new figures which show the horizontal distribution of the surface temperature.

lines 299-302: This sentence is beyond understanding.

AC: We will rewrite the description of the Redfield ratio, so these texts will also be substantially deleted or changed.

lines 352-353: What is a "stable" water mass? What do you mean? Also, see my previous comments about the Redfield ratio.

AC: We used "a stable water mass" to mean "not mixing with other water masses". This sentence will be rewritten. Also, the description of the Redfield ratio will be rewritten.

Most of the sentences in the conclusion are not clear and have to be reformulated.

AC: We will explain Figure 7 in Chapter 4.2. And in the Conclusion, we will write a clear description of what we found out in this study.