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Comment on bg-2021-116

Anonymous Referee #1

Referee 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-RC1, 2021

Review of Sukigara et al.'s manuscript

Sukigara and co-authors investigate the effect of passing storms on the production of organic matter and its fate once exported into the mesopelagic, using 2 BGC-Argo floats deployed in the western North Pacific. Main conclusions are (1) storms induced a net community production of 126-664 mg C m-2 d-1 and (2) the subsurface deviation of POC/O2/N ratios from Redfield ratios is due to remineralization of DOC which is assumed to be the main substrate. I think that the figures and results presented in the manuscript do not support these conclusions. 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.

(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.

(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.

(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.

Further detailed comments are listed below:

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

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.

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

line 72: “lower depths” or deeper depths?

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.

lines 135-136: not clear to me.

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

line 143: Rembauvile instead of Rambauvill.

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

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

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

line 171: what is the middle layer?

line 177: respiration also occurs in the euphotic layer.

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

lines 200-202: not clear from the figure.

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.

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.

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).

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.

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

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).

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

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

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

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

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

lines 299-302: This sentence is beyond understanding.

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

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