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Comment

Interactive comment on “Estimates of micro-, nano-, and picoplankton contributions to particle export in the northeast Pacific” by B. L. Mackinson et al.

B. L. Mackinson et al.

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We would like to thank the reviewer for the careful review and thoughtful comments. The reviewer’s comments are included here with our responses.

GENERAL COMMENTS Topic of this paper ‘contributions of nano- and picoplankton to export flux’ is an interesting and important topic. Quantitative study on this issue is still limited in marine ecosystems, although this is not the first paper to deal with this issue. Thus, present paper is worth being published in Biogeosciences. However, I have some points which need to be addressed before publishing on BG. Especially, authors need to make more suitable and careful discussion on 1) accuracy of the con-

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clusion from this study, 2) possible underestimation of contribution of nanopicoplankton to export flux, and 3) conversion factors of Chlorophyll a to POC for micro- nano- picoplankton in this study area.

SPECIFIC COMMENTS

INTRODUCTION

It is better if authors could clearly point out their study purpose.

***The purpose of the study as stated in the introduction is to “build upon prior investigations of phytoplankton community composition and export production along Line P by examining the distributions of organic carbon, phytoplankton indicator pigments, and ²³⁴Th in three particle size-fractions,” (p. 12634, line 4-6).

P. 12634 L.9-12: Authors can not say like that since authors do not show any data on chlorophyll a-carbon ratio for micro-, nano- and picoplankton.

***The focus of this paper is on pigment analysis and taxon-specific carbon was not determined in this study, and therefore, we are unable to calculate POC:pigment ratios. However, pigment concentration is commonly used as a proxy for biomass. “POC export” has been edited to read “particle export” in the passage noted by the reviewer to avoid confusion.

DISCUSSION

P.12648 L27-29: ‘zooplankton grazing and cell degradation’ may also contribute to POC loss. Then, this sentence is not suitable to explain the low pigment and high POC in the trap compared to pumping.

***We do not feel that acknowledging a possible source of error in the methodology, that grazing and cell degradation within the trap tube could slightly reduce the pigment concentration of trap samples over the 3-day deployment, invalidates the observed trends.

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CONCLUSIONS

P.12649 L23-25: Do authors want to say their methodology is not reliable to quantify contribution of micro- nano- picoplankton to the export flux, and finally authors have wrong data set? If this is the case, this paper is totally useless.

***Once again, we do not feel that acknowledging a sampling bias invalidates the results of the study. However, given the sampling bias and the fact that our methodology did not account for all pathways of export, we do not believe that our results disprove the hypothesis put forth by Richardson and Jackson (2007).

P. 12650 L2-16: Authors should show and discuss conversion factors of Chlorophyll a to POC for micro- nano- picoplankton in this study area. Contribution of each phytoplankton category to 'POC' export can be changed due to the factors.

***As stated previously, taxon-specific POC:pigment ratios could not be calculated in this study. Given that POC:pigment ratios are strongly dependent on phytoplankton growth conditions, the use of literature values would be problematic. However, by comparing pigment fluxes with pigment standing stocks it is possible to estimate how efficiently cells of different size-classes are exported from surface waters. While this is by no means the only factor influencing POC export, it does suggest the relative contributions phytoplankton of different size-class make to the biological pump via aggregation and sinking.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C8376/2015/bgd-11-C8376-2015-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 11, 12631, 2014.

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