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# *Interactive comment on* "Cross-basin differences in the nutrient assimilation characteristics of induced phytoplankton blooms in the subtropical Pacific waters" *by* Fuminori Hashihama et al.

**Anonymous Referee #1**

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Cross-basin differences in the nutrient assimilation characteristics of induced phytoplankton blooms in the subtropical Pacific waters

by Hashihama et al. submitted to Biogeosciences

Review: The manuscript by Hashihama et al. reports on the results of a set of shipboard incubation experiments in which surface seawater collected at seven stations across the subtropical North and South Pacific ocean was amended with deep water. The additions induced phytoplankton blooms and concomitant nutrient drawdowns in all seven experiments. The novelty of the study resides mainly on the use of nutrient nanomolar measurements to reveal regional patterns in nutrient drawdown ratios. The



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authors use also the obtained drawdown ratios in an attempt to contribute to current knowledge on the factors that control phosphate distribution in the subtropical Pacific Ocean.

Overall, the manuscript is very well organized and written. The introduction is fairly complete and presents nicely the context of the study. The methodology is well developed and it did not raise any major concerns from my side. The outcome is very interesting and will certainly contribute to the field of nutrient biogeochemistry in the oligotrophic ocean. The study constitutes a nice illustration on how nanomolar measurements of nutrients can give exciting insights on nutrient cycling.

I do have a few comments that may contribute to clarify some aspects of the manuscript.

General comments:

1. A recent paper published by the authors (Hashihama et al. 2020, GBC) show data from the same cruises presented in this study. If I am not wrong, this paper is referenced as Hashihama et al (submitted) at some points in the discussion section. Now that this paper has been published, and given the complementarity with this one, I strongly recommend the authors to refer to it to better put into context their outcomes. For instance, it would be interesting to check the influence of the experimental bloom-derived drawdown ratios on regional patterns of P cell quotas, addressed approximately through chlorophyll or phytoplankton biomass normalized POP data.

2. The only methodological issue that raised some concern to me was the differences in incubation times among the experiments since they can affect the drawdown ratios. This is particularly true when DIN and PO4 decreases are not linear (i.e. station 15, figure 3). I suggest the authors to conduct their calculations using only data obtained during the first 48h to see if the outcomes still stand. Otherwise, please add a statement in the discussion on the fact that different incubation times might have affected the obtained differences in nutrient drawdown ratios.





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3. The differences among experiments in nutrient drawdowns and their ratios are sometimes small. In order to add robustness to the interpretation of these differences, the authors should calculate the errors associated to these observations (errors of the slopes of linear regressions in Figs 3a and 3b and errors of calculated ΔDIN and ΔPO4). More generally, I missed standard errors and/or estimated uncertainties of all data and/or calculations throughout the manuscript.

4. I would like to share with the authors some thoughts that came to my mind when reading their manuscript related to the influence of their results on P* regional distribution. I am not sure of the pertinence and accuracy of these thoughts, so I share them just in case they can feed the interpretation of their outcomes. In 4.4 section and in Figure 6, the authors make a parallelism between ambient P* and "bloom P*". This exercise is interesting and confusing at the same time. The authors state that the observed negative values of bloom P* (not observed in in situ P*) imply the presence of alternative P sources. But isn't this statement only true under the assumption of 16:1 drawdown ratios used to calculate ambient P*? Wouldn't the observed differences in ambient and bloom P* values question the use of 16:1 to estimate ambient P*? This would also be illustrated by the much higher variability in ambient P* compared to bloom P*, wouldn't it?

Specific comments: - Page 5, lines 125-126. When available, it would be useful to give the standard deviation of the triplicate analytical data. - Page 6, lines 168-169. The observed trend would be due to the low variability in DIN concentration rather than to low DIN concentration itself. - Page 11, line 219. The term 'net assimilation rates' is normally associated to tracer incorporation measurements which is not the case. Please, stick to the term drawdown rates. - Page 11, line 220. Please, add estimated errors of these slope values (cf general comment 3 above) - Page 11, lines 225-226 and page 13, line 253. I do not understand what these R2 values mean, please clarify.

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