Reply to Referee #1 comment made on bg-2020-300, “Cross-basin differences in the nutrient assimilation characteristics of induced phytoplankton blooms in the subtropical Pacific waters ” by Hashihama et al.

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 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.

Our reply (in italics hereafter): We appreciate your constructive comments on our manuscript. We will address your comments as seen below.

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.

Yes, we will refer Hashihama et al. (2020, GBC) in the revised manuscript. For the suggested analysis using POP data, we have looked at the ambient data for Chl a and POP (TPP) in the study area of Hashihama et al. (2020, GBC). However, data on Chl a-normalized TPP in the mixed layer did not show a clear regional pattern (no distinctly high TPP/Chl a values in the western North Pacific). Since cell-specific content of Chl a varied depending on seasonal and/or regional light intensity, the use of Chl a-normalized TPP data is unsuitable for the suggested analysis. We consider that direct

observation of particulate C-N-P variability response to deep water addition will be necessary soon.

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.

We have compared slopes of the nutrient decreases during the first 36-48 h, and for the full incubation periods (52-96 h), and both the slopes were not significantly different for DIN or PO_4 (paired t-test, $p>0.05$). We will add the related statement in the revised manuscript.

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.

We will add the errors (95% confidence interval) for the slopes and intercepts of regressions and the Δ values and its ratios in the revised manuscript. The errors of the drawdown ratios can be estimated based on the error propagation rule (Miller and Miller, 1993 Statistics for Analytical Chemistry, 2nd edn).

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?

Apologies, we had stated a slightly confusing discussion in 4.4 section. In the revised manuscript, we will state a simple discussion as "The difference between the bloom P and ambient P* largely depends on the different N:P consumption ratios of <16 and 16, respectively. If the low N:P consumption ratios (<16) are consistently dominant in the PO_4-depleted WNP, alternative P sources*

other than PO_4 are required to fully exhaust DIN. Since lower DOP concentrations and higher alkaline phosphatase activity were observed in the WNP, compared to other subtropical Pacific regions (Hashihama et al., 2019; Hashihama et al., 2020), active DOP utilization in the WNP likely contributes to the DIN exhaustion. These perspectives suggest that, in the studies on subtropical nutrient biogeochemistry using N:P stoichiometry, the bioavailable fraction of DOP could be an important factor as well as DIN and PO_4."

Specific comments:

- Page 5, lines 125-126. When available, it would be useful to give the standard deviation of the triplicate analytical data.

The standard deviations will be indicated in Table 2.

- 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.

We will add "the consistently low concentrations of DIN" in the revised manuscript.

- 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.

We will use "drawdown rates" throughout the text.

- Page 11, line 220. Please, add estimated errors of these slope values (cf general comment 3 above)

Yes, we will do as mentioned above.

- Page 11, lines 225-226 and page 13, line 253. I do not understand what these R2 values mean, please clarify.

These values were derived from the linear regression analysis. We will add "r^2=xxx in linear regression" in these parts.