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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 #2

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Comments to Author(s): This manuscript by Hashihama et al. describes the variation in macronutrient drawdown among Pacific Ocean surface microbial communities using deep water additions to bottle incubations. The authors present data from seven subtropical gyre sites with distinct nutrient uptake ratios, where the nutrient limiting net biological production is unknown. These observations are linked to pigment proxies for phytoplankton taxa and diatom densities to examine the role of community in nutrient drawdown ratios. The experiments yielded increased phytoplankton biomass at all sites, but varying stoichiometric ratios of uptake for DIN:PO4:Si(OH)4. I feel this manuscript strongly expands upon existing studies on the question of nitrogen, phosphorus and iron limitation in the North and South Pacific Ocean.

The data is presented in a straightforward manner and explained well. All sections are well written and figures are easily digested. I have a few concerns on assumptions made regarding phytoplankton pigments proxies and deep DOM composition, but otherwise recommend the manuscript should be accepted with minor revisions.

General comments

1. Regarding the methods, I request that the deep water collection be clarified slightly. It is not stated if the water was filtered to remove living cells. This is of particular concern at Stations A and 2. If unfiltered seawater was used, both grazers and microbial cells could impact the conclusions at these stations. At other stations, freezing the seawater would remove this concern, but introduce additional nutrients from burst cells. The nutrient composition of cellular detritus is likely different and more bioavailable (urea, NH4, labile DOM) than deep nutrients (NO3, recalcitrant DOM).

2. My largest concern is directly assuming that divinyl chlorophyll A concentrations are representative of Prochlorococcus abundances. While a useful indicator, the concentration of divinyl chlorophyll a could change between sites, season, light/depth level, etc. It is very possible Prochlorococcus cells in the Eastern South Pacific have a lower density of photosynthetic pigments, especially in the summertime at the surface where these cells were collected. In addition, since Nitrogen is limited, the cells may have adapted by lowering the concentration of N-rich photopigments further. This combined effect of photo-acclimation and adaptation to low N could explain the low divinyl chlorophyll A concentrations in the South Pacific subtropical gyre. This caveat should be acknowledged in the Discussion.

3. Regarding DOM, I had two points to consider. The authors mention more bioavailable forms of DOM that may not be present in water at 1500m. Perhaps the DOM added then would not be consumed, leading to no net changes over the incubation period. Alternatively, the balance of net uptake and release could yield no change. This possibility should be acknowledged for silicic acid as well considering the longer

incubation time and high diatom abundances at station 15.

Specific comments Line 47 - Changing ‘alleviates temporarily ‘ to ‘temporarily alleviates’ reads a bit better.

Line 74 - ‘observation’ should be plural

Line 74-75 - I suggest changing ‘, to understand them, experimental validations are required.’ to ‘, and to understand them experimental validations are required’.

Line 95 - See comment on methods

Line 120 - To what extent does the water volume in the bottle change between T0 and the last time point? Also are collection intervals than shorter for the shorter incubation times?

Line 199 - ‘Dominance of Prochlorococcus’ not actually quantified. See comment above.

Line 202 - A brief description for how the Nitzschia longissimi was identified should be included (i.e. by sight, microscope identification?).

Lines 312-315 (347) - See comment on phytoplankton proxies above.

Lines 333-340 - See comment on DOM above.

Line 347 - Based on uncertainty of phytoplankton composition estimates, I don’t believe this phytoplankton uptake theory can be thrown out.

Line 362 - Alternatively, DOP uptake and release is balanced.

Line 396-7 - This point is very interesting and I point the authors to this short compilation reference of nitrogen fixation estimates by Bonnet et al. 2017 in (https://doi.org/10.1073/pnas.1619514114). It is possible that iron (or a trace metal) is limiting, but the microbial population does not have standing stocks of nitrogen fixers.

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