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Biogeosciences Discussions

# *Interactive comment on* "Sequential Nutrient Uptake by Phytoplankton Maintains High Primary Productivity and Balanced Nutrient Stoichiometry" *by* Kedong Yin and Paul J. Harrison

**Anonymous Referee #2**

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The manuscript by Yin and Harrison measured nitrate and phosphate profiles, along with incubation experiments, to explore the ideas of nutrient drawdown in a coastal ecosystem. The title and introduction bring together ideas about the timing of nutrient uptake, the level of primary production, and how those relate to cellular nutrient stoichiometry. These are intriguing ideas and could shed light on a number of important marine processes and the linkages between them. Unfortunately, I found the presentation of methods and data to be either missing or difficult to follow. The ideas of the introduction didn't necessarily follow the data that was collected. For example, the introduction was mostly about particulate elemental ratios and diversity, but the study itself was about dissolved nutrient ratios of nitrate and phosphorus. No connection was



made between these different types of elemental ratios. Because the methods section was missing many details, it was difficult to follow what the experiments were and when they were done; therefore, it was difficult to assess the interpretation of results. I found the conceptual model presented in Figure 1 to mostly add confusion rather than clarification to the results.

There were a number of more specific issues found in the bulk of the manuscript, which have been listed below.

Suggested revisions

-Redfield is a concept for the open ocean and long-term nutrient balance with deep mixing, that specifically does not account for N-fixation or terrestrial inputs. These are not the conditions here. There is no explanation of other nitrogen forms, like ammonium and DON, which are likely important in a coastal system.

-Line 62: While the Conley et al. paper is about nutrient limitation and eutrophication control, it says nothing about Redfield, nor does it present any data. It is an opinion piece about coastal management.

-Lines 63-66: what about the work by Martiny and co-authors about global patterns of C:N:P and it's connections to diversity?

-Lines 72-75: This sentence was confusing. If the authors are stating that there are no measurements of C:N:P in heterotrophic bacteria, they should take a read through Gunderson et al. (L&O 2002) and Godwin & Cotner (ISME 2015).

-Line 138: What about the uptake of ammonium or dissolved organic nitrogen? This would certainly impact both the uptake rates and the overall drawdown of Si:N.

-The methods state that this experiment was done August 6-14, 1991, but a number of other places in the manuscript refer to additional experiments done on other dates (e.g. data shown in Figures 8 and 9). At a minimum, those additional experiments need to be described.

-For fluorescence (line 151) and nutrients (lines 165-169), more detail is needed on the standards used and detection limits.

-Line 184: Are T1 and T7 referring to time points, or conceptual models?

-Line 199: clear how? Lack of change in ambient dissolved nutrient concentrations does not necessarily imply lack of uptake. It could just as easily be fast turnover rates.

-Line 225-226: Further explanation is necessary to understand which experiments were considered “on-deck” and how that relates to the conceptual model, which is all about mixing events.

-Line 230: Fluorescence does not equal biomass.

-Lines 257-258: there is no data shown on primary production, and thus this statement is difficult to evaluate.

-Lines 269-280: The logic here is quite hard to follow, as each sentence is long and refer to multiple panels of different figures, with limited explanation and/or the use of vague terms (i.e “sitting on top” or “parallel lines”).

-Line 316-317: What is the evidence for higher phytoplankton cell counts?

-Line 318-319: This statement needs to be referenced and further explained.

-Line 335-336: It’s not clear how open ocean internal waves are relevant to this discussion.

-Lines 338-339: Either in this manuscript or in the literature, what evidence is there that phytoplankton are changing position in the water column in the pursuit of nutrients? The work by Bienfang and colleagues in the early ‘80s would indicate that physiological nutrient status does not directly correlate to sinking rates.

-Line 350: POC and PON were not discussed in the methods or results, but introduced in the discussion and figures. In addition, from looking at Figure 10, it would seem that



POC:PON ratio simply did not change, which could be due to any number of reasons, the most likely one being that C:N is a function of cell size and not limitation or luxury uptake. Besides, the introduction spells out all the reasons particulate ratios may be an unreliable measure of cellular nutrient stoichiometry.

-Lines 355-363: The conclusions don’t appear to be related to the primary points in the manuscript.

-Figure 2: an inset of a larger area (zoom out) might be helpful for readers not familiar with this area. Also, the Fraser River location should be highlighted (it’s a bit hard to see) and the approximate plume area/distance/direction should be indicated, as it is mentioned multiple times (e.g. lines 143, 183, 215, Figure 4, etc.) as having an influence on the sampling and results.

-Figures 5 and 6 look like copies of each other. Are the two different stations really exactly the same at all time points? Either way, what is this time series? It was not explained in the methods.

-Figure 7: The time-series results were not explained in the methods. How was this experiment performed? What is the bottom of the axis in the NO3- (middle panel)? It looks like NO3- goes to zero. Was the in vivo fluorescence measure calibrated to a chlorophyll standard, or was it all relative? How do the authors explain a potential lag in uptake of N and P? How would this relate to mixing events, which are presumably short-term?

-Figure 8: Is this station S3? There is no station 3 in the map in Figure 2. Why was this experiment done more than two years before the rest of the experiment? Why wasn’t it explained in the methods?

-Figure 9: Most of the figure blurb needs to be in the methods. Additionally, exactly how the uptake ratios were calculated, and those results, need to be added to the manuscript. Why was this experiment done more than a year before the other experi-

ments described herein?

-Figure 9B: This figure contains the first mention of ammonium. How (i.e. what method) was it measured?

-Figure 9C: What does the terminology of +N/+P and +N/+Si mean?

-Why was this sampling done the year prior to what was explained in the methods?

Technical revisions -Line 57: what is the “stoichiometry of the water column”? Are the authors referring to the dissolved NO3-:PO4 ratio?

-Line 58-59: do the authors mean homeostatic when they say “variable”? That would make the sentence make more sense. Also, is there a reference for this relationship?

-Line 66: typo. . . should read “mechanism proposed is the. . .”

-Line 93: This should probably say that it is a “conceptual model”.

-Line 101: Did the authors mean to say “competition”?

-Line 106: give a reference to Figure 2.

-Lines 113-120: It was confusing to see the conceptual models named T#, because that makes me think of a time-series. In fact, later in the paper (e.g. line 184), this same notation is used for time-series experiments.

-Line 144-145: One citation should be enough to explain station numbers.

-Why are there three figures that comprise Figure 9 given subscripts. This is a bit confusing, as lettering typically implies panels, not separate figures.

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