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Biogeosciences Discussions

Interactive comment on "Synergistic effects of $p\mathrm{CO}_2$ and iron availability on nutrient consumption ratio of the Bering Sea phytoplankton community" *by* K. Sugie et al.

Anonymous Referee #2

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This manuscript presents results from a CO2 and Fe manipulation experiment performed on plankton communities in iron-limited waters of the Bering Sea. The authors found that the main effects of increased CO2 were evident in the Fe-limited condition, as after Fe is added, the large phytoplankton response likely over-shadows any minor effects of variations in CO2. In the Fe-limited treatments, increased CO2 resulted in variations in the elemental ratios of Si:N and Si:C with higher ratios under the higher CO2 conditions. The authors speculate this to be due to increases in CO2 (and corresponding decreases in pH) resulting in a further reduced iron availability which would then increase S:N (and Si:C) mainly as a consequence of reduced N and C cellular contents. Another interesting result in the Fe amended treatments were the minor

shifts in diatom compositions after day 4, when nutrients were fully depleted. Higher CO2 treatments contained reduced cell densities of Rhizosoleniaceae diatoms relative to the Fe-amended lower CO2 treatments.

The authors performed a very comprehensive number of measurements to not only determine the effects of increased CO2 concentrations on nutrient consumption ratios, but also how CO2 would influence phytoplankton physiology and community composition. The examination of variations in CO2 under both Fe-replete and Fe-limiting conditions was key to the success of this study. In hindsight, given the majority of the CO2 effects where found under Fe-limiting conditions, it is unfortunate there were not as many CO2 treatments under these conditions as with the Fe-replete conditions. Nonetheless, the effects are clear. Overall, I found the manuscript very well written and the results clearly presented with appropriate interpretations. I only have a few minor comments/suggestions for the authors to think about.

a) Title: The study presents much more than just the effects on the nutrient consumption ratio. I realize the focus is on the nutrient dynamics but the changes in community composition are just as important. Therefore, I would suggest the following revised title: "Synergistic effects of pCO2 and iron availability on the phytoplankton community and nutrient consumption ratio dynamics in the Bering Sea".

b) Particulate nutrients: In addition to the ratios, it would be nice for the authors to present the actual concentrations and how they changed over time. If not incorporated in the discussion, this data could be included in the supplemental.

c) Pg. 4340: Is the PDMPO fluorescence intensity normalized to a specific area? In other words, are the measurements independent of the actual size of the cell?

d) Pg. 4342: I found the lack of response by the Rhizosoleniaceae diatoms in the Fe-amended high CO2 treatments rather intriguing. However I was left a bit unsatisfied as there is little discussion as to why they did not respond. Could the authors speculate how these diatoms may have been negatively affected by high CO2 compared to the

other diatoms groups that seemed to be less affected? Is it because they are much larger cells?

e) Pg. 4343 (and Figure 7): Overall I find this figure not very informative and a bit misleading. Through plotting the changes in nutrient concentrations over chl a, I do not get the sense this ratio will provide a measure of the nutrient requirements per unit phytoplankton biomass. This is due to the significant changes in cellular chl a after iron addition. As shown in the figure, this implies a much larger consumption of nutrients (per unit phytoplankton biomass) in the controls versus the iron amended treatments, which I don't think is actually the case. I realize POC concentrations are not algal specific however the differences from the initial concentrations to day 4 are likely to be dominated by increases in the phytoplankton biomass. Therefore, to get a sense of the changes in cellular Chl a quotas, could the ratios of Chl a/POC provide a good measure? Also, did the authors try plotting changes in nutrient drawdown normalized to POC concentrations? Although it is important to note that the dramatic changes in phytoplankton species composition in response to iron addition need to be considered as well as changes in individual cell physiology.

f) Pg. 4347: The authors claim the main effects of CO2 levels on species composition are under Fe-replete but (macro)nutrient-depleted conditions. This is an important result, but a bit confusing. How does the species composition still change after macronutrients are depleted? Shouldn't this terminate growth? That is, unless there is significant nutrient storage right? Do the authors think CO2 could be affecting their nutrient storage capacities? I would be interested in further discussion about the actual mechanism behind this affect.

g) Pg. 4347: In addition to the mechanisms listed, the authors also cannot rule out variations in the phytoplankton composition (and their intrinsic nutrient requirements) between the control treatments as an explanation for differences in the Si:N and Si:C ratios. As shown in figure 5, although very similar, the diatom composition of the control treatments was not exactly the same. In addition, there is no description of possible

differences in the non-diatom plankton communities that could be affecting the nutrient consumption ratios (although likely in a very minor way).

A few technical comments:

a) Pg. 4341, Line 28: Use "lower" instead of "small" b) Pg. 4346, Line 26: Use "less" instead of "minor" c) Pg. 4348, Line 26: Instead of "Our recent study reported...", should this be "Our other recent studies reported..." d) Pg. 4349, Line 25: This sentence is a bit awkward. What does "when the nutrient remained in conditions" mean? Please rephrase.

Interactive comment on Biogeosciences Discuss., 10, 4331, 2013.