

Interactive comment on “Production, partitioning and stoichiometry of organic matter under variable nutrient supply during mesocosm experiments in the tropical Pacific and Atlantic Ocean” by J. M. S. Franz et al.

Anonymous Referee #2

Received and published: 21 September 2012

General comments: The authors test the impact of nutrient supply and nutrient stoichiometry on production, partitioning and elemental composition of phytoplankton-derived dissolved (DOC, DON, DOP) and particulate (POC, PON, POP) organic matter in shipboard mesocosm incubations conducted at three nutrient enrichment concentrations. They present clear observations of a decline in organic matter production of coastal phytoplankton as a result of decreasing N supply and flexibility in the stoichiometry of mineral nutrient consumption. Their results demonstrate how DIN and DIP are consumed over a large range of DIN:DIP. This paper addresses an important

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and timely topic, and I rate the overall quality of this work as good. However, there are a few items that should be clarified, which I will address as Specific Comments below, and a few Technical errors, which I list last as Technical comments.

Specific comments: As correctly mentioned by the referee, pools of DON and DOP can be influenced by phytoplankton and by heterotrophic bacteria. The suggested term “microbial biomass” appears therefore as an appropriate compromise to include both of these groups and will be used in the revised manuscript. Furthermore, information on the contribution of bacteria to the organic carbon pool during the three mesocosm experiments will be added. One of the big problems I had with the manuscript was the lack of data, or mention of any analysis of phytoplankton/microbial abundance throughout the studies. I believe cell counts of heterotrophic bacteria as well as autofluorescent phytoplankton are crucial for any experimental work like the one mentioned in this manuscript, so I definitely think this should be included. I have reasons to believe that the authors did indeed do cell counts, since there is mention of abundance changes here and there, but overall there is no detail or even mention of the analysis being done or what the results were. Furthermore, in their discussion, the authors assume that the effect observed in the mesocosm experiments was driven by the diatom-dominated phytoplankton community. Again, I think it is critical for this kind of manuscript to have abundance data to support such claims, and in this case, some sort of community structure data, even if not high resolution. Even flow cytometry counts of different populations of autotrophic and heterotrophic microbes would help support a lot of the claims made from the results obtained from the experiments.

Interactive comment on Biogeosciences Discuss., 9, 5755, 2012.

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