

Interactive comment on “Response of heterotrophic and autotrophic microbial plankton to inorganic and organic inputs along a latitudinal transect in the Atlantic Ocean” by S. Martínez-García et al.

Anonymous Referee #1

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The authors did enrichment experiments along a transect from the North Atlantic to the South Atlantic. They measured several properties of both phytoplankton and bacteria. They found that generally organic enrichments stimulated biomass and activity (production and ETS activity) of bacteria, but these properties were not affected by the inorganic nutrient enrichments. In contrast, phytoplankton biomass and production were not surprisingly affected by additions of either inorganic or organic compounds. This paper has several interesting and potentially important things to say.

Overall I think the paper should shorter. The paper now is 19 pages of single spaced

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text, although a page in this format may not be quite as long as a page in other journals. Anyway, the Introduction is too long with too much extraneous stuff. The Discussion repeats the Results too much.

Another general comment is that the authors have way too much about atmospheric deposition. It may be true that the atmospheric inputs are important for the entire ocean, it is certainly not true for N and P for the upper surface layer (0-100 m). Inputs from the waters below the mixed layer by diffusion and upwelling are much higher.

Even if the authors don't agree with me, the issue is irrelevant for this paper. This paper has nothing on atmospheric deposition, but it does have several important things to say about more general issues. Perhaps climate change and other changes will lead to changes in atmospheric deposition, but the issues addressed by this paper are important even if the world doesn't change in the near future. We know little about bottom-up controls of bacteria, and even the controls of phytoplankton may be less understood than we thought. So there is no need (and it is not appropriate) to have so much about atmospheric deposition.

(As the authors will be able to detect, I reviewed another paper from this project which examined how the additions affected bacterial community structure. This paper on rates and biomass is not as affected by the other paper; the community structure paper needs this paper more so than the reverse.)

Although I think the changes needed address these comments are "major", the authors should find them easy to do.

Specific comments

1. The first few paragraphs (page 465) of this paper should be deleted. This paper has nothing on atmospheric deposition. The paper would be much better if it started on page 466 and discussed in a bit more depth the previous enrichment experiments similar to what the authors did.

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2. Page 469 (and elsewhere): The authors use lots of abbreviations, most of which are not necessary. I can see the need for electron transport activity and for using BGE, but there is no need to use PB, BB, BP and CR. These make it harder for readers to figure out the paper.
3. page 472-473: This rather long section on "initial conditions" could be reduced by 50%.
4. Page 475: The subscript RR is not needed and it's confusing. Just say what changed.
5. Page 476, line 21: BGE is not defined before its use here. The abbreviation was not defined, nor was a brief description of how it was calculated given. Both should be in the Methods and Materials.
6. Page 478, Section 4.1: This section should be deleted as it simply repeats the results. The main point of the paper is about the enrichment experiments, not a survey of in situ conditions.
7. Page 480, line 7: How do the authors' results compare with other enrichment studies that did not look at atmospheric deposition?
8. Page 483, line 14: The data do not allow the authors to say anything about whether bacteria out-competed phytoplankton. In fact, with these data, you could argue just the opposite: phytoplankton get everything they need and are not limited by the substrates; it's only the enrichments that allow the bacteria to increase. The experiments say more about controls of each assemblage and nothing about competition.
9. Table 2 should be deleted. The main points (is an effect significant or not) can be included in the text when discussing effects or in the figures.
10. Figure 7: I think this figure should be deleted. First it is complicated and hard to understand exactly what is plotted; it seems ratios of ratios. Actually, the intent is simple: to compare the bacterial response with the phytoplankton response. But, the

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previous figures gave the answer already: bacteria responded, while the phytoplankton did not. So, this figure isn't saying anything new.

Interactive comment on Biogeosciences Discuss., 7, 463, 2010.