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Comment

***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 #2**

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The authors present data generated in a series of nutrient amendment experiments in the Atlantic Ocean. The data demonstrate marked contrasts in the responses of the auto- and hetero- trophic components of the community alongside some interesting large scale spatial gradients in response. Overall the data are interesting and of importance to oceanographers interested in the responses of upper ocean microbial communities to perturbations in nutrient supply, as well as contributing to our understanding of large scale patterns in nutrient limitation. Having previously reviewed an earlier version of this manuscript which was submitted to another journal, many of my

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comments will be familiar to the authors. I note that some, but not all, of my original comments have been taken into account in this revised version.

### Specific comments

The abstract is overlong and includes some material which could be considered introductory. Noting that the BGD format allows me as a reviewer to access other reviews, I would agree with Reviewer 1 that the authors place too much emphasis on atmospheric deposition when this is not directly addressed anywhere in the data presented.

The authors use 2 principal mixed nutrient amendments both alone and in combination. The inorganic amendment consists of the potentially bio-limiting elements N, P and Si. The organic amendment will provide the potentially limiting nutrients N and organic C. Although this is mentioned later in the manuscript, I still feel it needs to be more clearly articulated either in the abstract or introduction/methods, as this information is crucial for understanding both the current results and their relationships to the large scale oceanographic context and prior work.

Page 468. I was glad to see the enhanced discussion of how the artificial light source used was matched to potential in situ light levels. However they should still describe the specific type of light source (e.g. fluorescent tubes? halogen?) and hence it's spectral characteristics. Also it is worth noting that the daily light dose at the incubation depth is not necessarily as relevant as the mean value within the mixed layer and finally that the 'on-off' nature of the artificial light source will differ significantly from the natural situation.

Section 3. Much of the results section is very descriptive and could potentially be reduced. The data are clear for the reader to see within the figures.

Page 479, Section 4.2. The authors briefly discuss top-down control within their experiments. I noted in my previous review that the authors pre-filtered (<150um) the water for their experiments (Page 467, line 10). The effect this would have on microbial pro-

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cesses may be complex (e.g. hypothetically this could increase the grazing pressure on phytoplankton by removing some of the grazing pressure on microzooplankton). Was the pre-filtering protocol they employed also used by the other studies they reference and particularly those which may have produced differing results? Is this a potential reason for contrasting results?

Page 481, line 7 and elsewhere (e.g. page 482 line 19). I would prefer a more careful description avoiding overuse of the potentially ambiguous term 'limitation'. The statement 'co-limited by inorganic and organic nutrients' is imprecise, it gives the reader no indication of which actual nutrients (e.g. C, N or P) might have been 'limiting'. I would prefer the authors to stick to precise statements and then separate these from subsequent inferences. e.g. 'BB and BP were stimulated by organic nutrient addition in all the experiments but only responded to the simultaneous addition of inorganic and organic nutrients at 260N. Given the makeup of our mixed nutrient additions, these responses potentially suggest co-limitation by N and organic C in all experiments, with co-limitation by N, P and organic C possible at 26N'

Page 481, line 17 (and Page 482, line 5). Related to above, arguably all that is demonstrated by both the quoted papers (Fanning, 1992 and Mather et al. 2008) is that phosphorous availability is (relatively) low in the North Atlantic gyre, not necessarily that it is 'limiting', either in the sense of biomass accumulation or some physiological rate.

Page 482, line 10. Again, as stated in my original review, if the aim of the experiments performed was to simulate the effect of atmospheric nutrient inputs (as opposed to other processes which also influence the upper ocean nutrient availability), then this should be stated in the introduction. (see also reviewer 1).

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

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