

***Interactive comment on* “Primary production during nutrient-induced blooms at elevated CO₂ concentrations” by J. K. Egge et al.**

Anonymous Referee #1

Received and published: 8 January 2008

This manuscript presents results from two years mesocosm experiments examining the impacts of CO₂ levels on primary productivity. The study is part of a much larger body of work dealing with related issues, much of which has appeared in various publications over the past few years. In my view, the current study does add some new information to the picture, but the results would have had more impact and would have been easier to interpret had they been merged with other published data sets. While I agree generally, with some of the main conclusions reached by the authors, I think that there are a number of presentation issues and statistical analyses which could be significantly improved.

General Comments:

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I was a bit concerned about the authors use of terminology with respect to gross and net production, and the potential mismatch of measurement time-scales. I was also concerned about the level replication of certain measurements, and conclusions reached without robust statistical support. In terms of measurement time-scales, the bulk ^{14}C experiments appeared to have been made in both years using 4 hour incubations. Presumably, these measurements would approximate something close to gross primary production. Why weren't the size fractionated measurements made on the same time-scale? In terms of terminology, the authors seem to equate their measured 24 h O_2 changes with gross production. Yet, given the length of these incubations, and the possibility for significant autotrophic respiration, I suggest that these measurements really reflect net primary production. Had the authors used the H_2^{18}O labeling technique, they would have measured true gross production. Similarly, the term net community production is normally taken to include all community respiration including zooplankton. Typically, this measurement cannot be properly assessed using bottles, but would instead be inferred from net O_2 saturation in the mixed layer. Given these caveats, I'm not sure what the authors are really discussing. No explicit statement is made of how the authors calculated community respiration. Is this from bottle incubation data? If so, would it include the contribution of large zooplankton?

Given that size fractionated ^{14}C uptake samples were only collected from a single mesocosm per CO_2 treatment, there would appear to be no reliable way to quantify the true biological variability in the CO_2 dependent response. The variability presented in the graphs (standard deviations) reflects the precision of the ^{14}C method rather than the true variability in the CO_2 response. As such, it would not seem possible to gauge the biological significance of the observed effects, and I feel that the results presented in this section cannot be considered robust. For the oxygen measurements, I was unclear as to whether samples were collected for each mesocosm.

I think it would be easier to follow the graphs and data if basic information were pro-

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vided on nutrient concentrations and phytoplankton biomass during the experiments. While the authors do discuss these data in the text (e.g. phases I – V of the bloom), it would be nice to see these mapped out directly on the figures. This way, one could easily see the period when nutrient depletion set and net phytoplankton accumulation ceased. My other concern regarding presentation is the apparently unbalanced emphasis on the 2005 data relative to the 2003 results. As I understand it, neither of these data sets have been published. If so, why are the 2005 data relegated to a table and only 2005 data presented graphically?

Specific Comments:

Abstract: I found lines 14 – 21 confusing, particular the last sentence in this section.

Introduction: I think the logical flow of ideas might be improved somewhat by grouping together all of the ideas dealing with changes in productivity before the discussion of changes in species composition – i.e. move the material at the bottom of p. 4387 / top of 4388 a littler earlier (line 21, p. 4387).

Bottom p. 4388. I was a bit confused initially which data the authors were going to present (i.e. 2003 and 2005). I think the information about the CO₂ levels used in the experiments (l. 15 – 17) could be moved to the materials and methods.

Materials and Methods:

What is an osmotrophic organism?

I think table 1 should be cited in this section.

p. 4390: It was initially unclear to me whether the size-fractionated measurements were made at a single time point or at successive samplings. This is clear from the associated figure but not from the methods section.

p. 4396, l. 1- 12. I think the information on nutrient enrichment ratios should go in the

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methods section.

With respect to the issue of ^{14}C uptake in the 0.2 -1 μm size fraction, the authors suggest that heterotrophic bacterial uptake could play a role. But wouldn't this be corrected for by the subtraction of dark blanks?

Interactive comment on Biogeosciences Discuss., 4, 4385, 2007.

BGD

4, S2328–S2331, 2008

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