

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

Anonymous Referee #3

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This paper is one of several in a special issue all of which focus on experiments conducted in a series mesocosms with altered pCO₂ levels. The authors present the results of the 14C and O₂ production measurements and try to put these results into the context of the other papers either in press, submitted or in preparation on the subject.

As is the case in many mesocosm experiments the authors have had a hard time finding any significant responses to the CO₂ treatments and this obviously renders the forming of any conclusions rather delicate.

In the methods, it wasn't clear how the authors measured primary production. I was unable to get hold of the reference of Gargas (1975) to check but did the authors take into account the potential differences in inorganic carbon concentration in their calculations

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of uptake? It would be nice to have some details of the ^{14}C uptake methods.

Linked to this, and as pointed out by the authors themselves, they had extremely low values of particulate C to O gross production. I wonder if the light levels in the incubation were the same, or at least, were under saturating light conditions for these two methods? Can the authors comment on this? It could well be that there is a significant loss of DOC produced during primary production, but a loss of up to 75% is rather high. Perhaps the authors can cite some examples from the literature where values of this nature have also been observed.

In the discussion the authors spend quite a bit of time discussing the results of the phytoplankton diversity (paper of Paulino et al. this issue). Similarly, the authors also discuss bacterial abundance and activity in the paper (Allgaier et al in prep) as well as the paper of Shultz et al. (this issue) and I wonder if it wouldn't be better to merge these papers have one condensed paper on this subject addressing the cycling of carbon in the microbial loop. The authors also make a lot of reference to the Riebesell et al paper recently published in Nature. However, other than being from the same experiment, I found it hard to see how this paper added to the previous one and in some places there appeared to be some contradictions between the results.

I may well have missed something but it wasn't clear to me why there are two lines for each treatment in Table 2 and I would like to see the error bars are on Fig. 4.

I therefore recommend that the authors revise the paper completely and think about merging it with some of the other data from the other studies.

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