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Interactive Comment

Interactive comment on "Primary production during nutrient-induced blooms at elevated CO_2 concentrations" *by* J. K. Egge et al.

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The submitted paper is an adjunct to the Nature paper of Riebesell et al. examining the effect of elevated pCO2 levels on plankton productivity. The original Riebesell et al paper was based on in situ observations and showed rather convincingly that elevated pCO2 concentrations gave rise to increased net community production, determined by the net changes in both oxygen and total CO2 concentrations. The present paper reports the concurrent in vitro rate measurements. The paper is not easy to assess as it has a rather meandering discussion with, what as far as I can make out, contradictory results. The authors conclude (Abstract, sentence beginning line 12) that: (quote)We found a trend in the 14C-based measurements towards higher cumulative

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primary production at higher pCO2, consistent with recently published results for DIC removal (Riebesell et al., 2007).(unquote) However, they also note (section 4390, line 11) that the 14C observations showed no statistical difference to the treatment. Neither the in vitro rates of gross nor net oxygen production showed any effect of the treatment (their Fig 2). Further they note in the same paragraph the data in Table 2 shows no consistent trend with pCO2 concentrations. They did see a trend in the very small size fraction (Fig 3 and 4) but they are very upfront over the difficulties interpreting what type of organism may be responsibly for this.

It seems to me that if you came to this data set without a predetermined view over the effect of elevated pCO2 levels, one would most likely conclude that in the balance the study had shown there was no effect. That would have been my general conclusion.

The question then is why, when a pretty clear effect is seen with in situ observations, the in vitro observations show no effect or a confused set of results. I cannot claim to know the answer to this, but I can observe that in another context we are having problems reconciling in vitro measurements of community production with the geochemists in situ determined rates: there is a substantial and systematic difference between the two sets of observations. We are coming to a point where we may have to acknowledge major systematic errors of the in vitro methodology. That is all I can offer as an explanation.

My own judgement is the data set offer in the present paper does not lend support to the original Riebesell Nature paper, if anything the reverse and I am unclear what to recommend to the authors and the Journal. My inclination is to suggest they put the data in the drawer and think about their implications, that is not to suggest the authors hide their data but that they mull over them.

There are some small points of detail

1) Poor old Einer Steemann Nielsen, he must be the most misspelt man in science! Here, a paper led by two Scandinavians manages to give two incorrect versions of his name and he is only cited twice 2) Section 4389, line 20. I am sure they used K iodide

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not K iodate to fix the O2 samples and that it is concentration was not 0.003M, my guess it would be nearer 1 molar 3) Section 4389, line 21. It surely is opaque, whether it is photoresistant is largely immaterial 4) Section 4391, line 42 the sentence which starts Based on , does not make sense; it seems incomplete to me.

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