

Interactive comment on “

Carbon fluxes in natural plankton communities under elevated CO₂ levels: a stable isotope labeling study” by A. de Kluijver et al.

Anonymous Referee #1

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In the introduction this study is justified by its relevance to a scenario of increasing pCO₂ levels with potential effects on phytoplankton production and growth rates as well as changes in the transfer of freshly produced organic matter to the microbial food web or deeper waters. It is stated that (1) an enhanced coupling between phytoplankton and bacteria would provide a positive feedback to ocean acidification through respiration, while (2) enhanced aggregation and sedimentation would provoke a negative feedback to atmospheric CO₂. As for the first statement, it is argued that this could happen either via reduced time-delay in the coupling between phytoplankton and bacteria or a larger

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proportion of phytoplankton production being channeled to bacteria. Unfortunately, no logical explanation why any of those scenarios would be expected is presented, and I cannot think of any. Why would a greater proportion of phytoplankton biomass be used by bacteria? Or why would the time-delay be reduced? This needs to be motivated. The relevance of this study to feedbacks on the atmospheric CO₂ is best studied by looking at effects of organic matter export into deeper waters, and due to unfortunate circumstances during the course of the experiment this was not really doable. The study may be interesting for other reasons, but I do not think that it helps to answer what the authors state as the main question and justification behind it.

The paper is well written and generally easy to read. However, some of the most basic features of the experiment are not explained in a clear way, such as the stratification. This could be easily changed, but adds some confusion as it is.

Differences in growth rates for different algal groups are shown, but these consider standing stocks of biomasses that may well be affected by differences in loss factors such as zooplankton grazing and sinking. There were no significant differences in growth rates in between the different treatments, and the tiny discrepancy in the average growth rate between the pCO₂ treatments is one of several examples where an insignificant difference is overinterpreted as a tendency - especially in the light of the very strong treatment, i.e. doubling and tripling pCO₂ levels. The same goes for the fraction of bacterial carbon derived from the phytoplankton. This comes back in the final section about implications for ocean acidification and I find it a disturbing over-interpretation of your results. The finding that isotope mixing in the settled material was independent of pCO₂ is as I see it the only finding in the paper that clearly connects to what is stated as the main question, and it is in line with the earlier analyses if e.g. TEP in the same experiment. This however does not warrant the paper being written in this context.

In page 3276 there is a paragraph stating that bacterial turn-over rates based on phytoplankton production is the same as BGE. I don't understand this.

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The discussion is not focussed around the main question stated in the introduction but rather on a number of separate issues.

Finally, the lack of information on the development of zooplankton makes me wonder to what extent they may have been directly affected by the pCO₂ treatments and if that in turn may have contributed to the post-bloom differences in phytoplankton biomass?

In conclusion, I think this is an interesting and well performed experiment, but I do not think that this manuscript can contribute to answering the questions said to justify it in addition to what has been previously published from the same experiment.

Interactive comment on Biogeosciences Discuss., 7, 3257, 2010.

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