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Biogeosciences Discussions

Interactive comment on "Impacts of elevated CO_2 on phytoplankton community composition and organic carbon dynamics in nutrient-depleted Okhotsk Sea surface waters" *by* T. Yoshimura et al.

Anonymous Referee #2

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This paper addresses a timely and difficult subject, the response of phytoplankton communities from open ocean, low nutrient areas to changes in pCO2. The one problem with their experimental design is that it is almost impossible to keep an oligotrophic plankton community alive and viable over a two week shipboard incubation without adding nutrients. Ambient sub-micromolar nutrient concentrations are of course not adequate to support the phytoplankton for anywhere near that long, and because the cells are isolated from the environment in a bottle, they are cut off from the normal sources of regenerated nutrients (from the grazing activities of zooplankton, etc) on which they usually subsist. In the real ocean they depend nearly completely on this steady flux of regenerated N and P, which is suddenly curtailed when they are placed

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in the bottles. The result is that nearly all the members of low nutrient assemblages inevitably decline when placed in extended incubations, which is what is seen here: all phytoplankton groups more or less declined, regardless of CO2 treatment. The "winners" were just the species that declined the least under the experimental conditions. For this reason, long incubation experiments of any type without any nutrients are problematic and hard to interpret, and this is the reason that very few incubations of this type are in the published literature.

Here are some detailed comments: p. 4145, lines 26-28. I'm not sure why these previous experiments are termed "artificial" algal blooms. Many of them used only ambient nutrients (for instance, several are in HNLC or coastal regimes) without any added N or P. The blooms that happened therefore seem completely natural to me. p. 4147, line 5. The gas flow rates of 100 or 50 ml per minute are very high, these bottles must have been very vigorously bubbled. This could have contributed to the decline in phytoplankton biomass seen in all treatments, as well as the complete lack of available nutrients discussed above. Section 2.2, sample treatments and analyses. The analytical approach of the investigators is particularly meticulous and well thought out, I am impressed with their thoroughness. One question: Since the particulate samples for POC were analyzed on a CN analyzer, they should have PON values as well. Why are these not mentioned or presented? Section 3, results and discussion p. 4149. I have some questions about the measured pCO2 values. First, the authors shouldn't switch back and forth between pCO2 and uatm CO2, pick one convention and stick to it. More importantly, the initial pCO2 values (about 200) are quite low, far out of equilibrium with atmospheric pCO2, and suggest that a major algal bloom could have occurred just before their study. Was this the case, and if not, why was pCO2 so low in the collected water? I have a hard time believing that the reason for the much lower measured pCO2 in all the treatment bottles compared to the putative bottled gas concentrations (sometimes half or less what they should be) is due to "insufficient flow rates of the air bubbling". As I mentioned above, their bubbling rate is actually very high. Did they try analyzing the gas in the bottles directly, or at least bubbling

BGD

6, C271–C273, 2009

a small amunt of filtered seawater vigorously for a long time with their bottled gas? This would tell them whether the CO2 partial pressure values the gas company sold them were actually correct. I suspect they were not, as I have seen even certified gas standards that were far from what was written on the cylinder. Even with some minor photosynthetic CO2 drawdown in the bottles (not too much, since biomass was low and growth negative), these numbers are far too low to explain by inadequate bubbling. They are completely right though that these results "show that direct measurements of at least two parameters in seawater CO2 system are necessary", and I commend them for doing a good job of measuring the carbonate system, even if their results are a little odd. Page 4150, lines 1-2. Are these reported growth rates for the in situ community from this other paper net rates, or intrinsic rates? Either way, an in situ community that is growing at a doubling every two days contrasts strongly with the zero or negative growth rates observed in all their treatments, again emphasizing the points I made above. Page 4150, lines 23 onwards. Once again, I'm not sure it is correct to say that the growth of diatoms relative to other taxonomic groups was reduced. Actually, none of them really grew, the diatoms just declined the most. I don't follow the logic of the next sentence, either. Just because diatoms can fix carbon more efficiently than other species at low pCO2, doesn't mean they will somehow necessarily be less competitive at higher pCO2. Page 4152. This extensive discussion on DOC production starting here and going on to the next page is fairly speculative. My interpretation would be, DOC levels are likely to increase in an incubation with lots of dead and dying, nutrient-limited phytoplankton.

To summarize, the experiment was technically very well performed, but the authors need to think carefully about the problems inherent in long enclosed incubations without any nutrients, and what can be learned from them. Interpreting experiments in which most phytoplankton groups decline in all treatments to a greater or lesser extent is not an easy thing.

Interactive comment on Biogeosciences Discuss., 6, 4143, 2009.