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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 #1

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This article presents the results of field study examining CO2 effects on phytoplankton processes in the nutrient limited waters of the western Subarctic Pacific. The main conclusion reached is that diatom abundances and DOC production rates are enhanced in the lowest pCO2 manipulation treatment, contrary to what has been observed in previous studies with nutrient enriched phytoplankton assemblages.

While I agree with the authors that CO2 effects on nutrient limited communities remain poorly understood, I feel that this current study suffers several drawbacks. First, it was not clear to me whether the waters sampled were chronically nutrient limited (as in a tropical gyre), or whether the study took place at the end of a phytoplankton growth period. What are the maximum nutrient concentrations in the study area at the beginning of spring phytoplankton growth period? Are we looking at a post-bloom community? If so, it seems to me that the bulk of the biological signal has already occurred and that further production is dependent upon a renewed source of nutrients into the upper mixed. In such a post-bloom community, it would seem to me that CO2 effects become secondary. To understand how C cycling and primary productivity in this system is affected by CO2, it would seem necessary (to me) to study the effects of CO2 during the phase of phytoplankton growth when most net production / C export occurs. Did this phase of active phytoplankton growth occur well before the authors conducted their field study, or does productivity in this system always occur under nutrient limited conditions?

Secondly, it is clear from the results of this study that the sampled community is one in which phytoplankton growth is tightly coupled with grazing. Indeed, in all treatments, there was a net decrease in phytoplankton standing stocks (i.e. negative net phytoplankton growth rates). Given that grazing was such a dominant structuring factor in the ecosystem, I was surprised that there was no attempt to quantify grazing rates across the CO2 treatments, nor any mention of grazing in discussing the results. I don't see how we can fully interpret the resulting community shifts without a consideration of grazing processes and their CO2 sensitivity.

Specific comments:

First para. Introduction: The work of Riebessel and colleagues on CO2 and calcification should be cited.

Materials and methods:

The authors sampled very late in the growing season (end of Aug.) I suspect that by this time all of the nutrients had been consumed by phytoplankton growth earlier in the year. See comment above.

Results and Discussion:

It would be helpful to see maximum (i.e. winter time) nutrient concentrations and seasonally integrated nutrient drawdown. Again, are we seeing a post-bloom community?

p. 6, para, 3. Did Liu et al. sample at the time of the season? Same para: Measurements of chlorophyll concentrations provide limited information when there is simultaneous phytoplankton growth and grazing. I believe that the data set would have been significantly strengthened by the inclusion of primary productivity data (e.g. 14C experiments). These experiments could have been designed to measure POC production, PIC production and DOC production.

p. 7, bottom of first para: It is possible that fuco/chl ratios are sensitive to pCO_2.

p. 8, bottom of first para: Increased photorespiration and enhanced glycolate excretion could increase DOC production under low CO_2 conditions.

p. 8, last few lines. I'm a bit uncomfortable with this sweeping extrapolation given the limited data set.

p. 9, top para: It's important to keep in mind that nutrient limited ocean regions contribute much less to oceanic C sequestration than high productivity nutrient replete areas.

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