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Biogeosciences
Discussions

Interactive comment on "Effect of elevated CO_2 on organic matter pools and fluxes in a summer, post spring-bloom Baltic Sea plankton community" *by* A. J. Paul et al.

Anonymous Referee #2

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Paul et al. present an enormous amount of data from the KOSMOS mesocosm experiment. In fact they probably present too much data in that this manuscript reads as if it was pulled directly from a PhD dissertation with little distillation. Indeed, a paper with 5 weighty tables and 18 figures is too much. Part of the reason for delay in getting this review turned around is directly related to trying to understand what the story was with the data. Specifically many of the Discussion sections read like rewrites of the results and thus are way too long for what is said. For example, Section 4.1 remove the 'environmental' statements as this is really about closed mesocosms, and the link to the environment isn't that strong and just proves a distraction. Phase 1 (section 4.2), there are no differences in contrast to expectations, don't need 1+ pages to say that. Section 4.3 ends with picoplankton were always affected by CO2 but were not abundant enough early on to impact the total. However, C:N ratio in the total was much greater than C:N in the <10 fraction so how is it possible that their increased relative importance, with a lower C:N, accounted for the even higher C:N in the total? Section 4.4 the discussion of flow into the DOC pool is weakened without rates of DOC production or consumption, which seem like they are presented in a companion paper in this issue? While DOC concentrations are higher in the CO2 treatments how do we know it isn't due to a reduction in its bioavailability, or is this assessment related to the hypothetical reduction in respiration? The discussion seems to focus on the channeling of carbon from POM to DOM cycling but it really isn't clearly presented. Section 4.5, seems like it should be in the conclusions more than its own standalone section as it is all just about the hypothesis that high natural variability has selected for a community that doesn't respond in a dramatic way to CO2 enrichment – no data related to this topic is actually presented. I would strongly recommend the authors refocus the discussion and clearly state the story they are making. I think that the idea of a muted response to OA when nutrients are low is really important and so the basis of their study is really exciting and provides a great 'end-member' to the continuum of OA responses. Specific to other parts of the paper, the methods and experimental design all seem sound. I do have a question about the removal of outliers, specifically that it seems there is a high amount of outlier exclusion. I'm not a statistician, but is it acceptable to remove so many data points? Is there belief that this was a sampling issue? Should we be concerned about the broader dataset or is this telling us something? Limited specific comments: Table 1. Lomas et al. reference is North Atlantic, not Pacific. Is Figure 1 really necessary – information in there seems tangential at best to the story. Figure 4, useful but not really necessary. Figure 6, symbols horizontally – issue in upload or trying to show something.

Interactive comment on Biogeosciences Discuss., 12, 6863, 2015.