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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.

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We thank the reviewer for their useful and constructive comments on this manuscript which helped in particular to focus and refine the discussion. Our responses to each comment from Reviewer #2, including modifications to the manuscript, are detailed in the following:

REVIEWER #2, COMMENT 1: 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.

Author response: As the reviewer highlights, this manuscript contains a lot of data and figures. We believe that these provide important biogeochemical, chemical and physical information which together build a solid picture of the study. The influence of CO_2 on particulate and dissolved matter pools and fluxes, analysed in details in this manuscript, sets the scene for more specialised manuscripts which are currently under preparation (see Table 3). Nonetheless we agree with the reviewer that some parts of the Discussion would better fit in the Results section. For example, as suggested by the referee, we have shifted the 'environmental' statements regarding the initial conditions in the Tvärminne Storfjärden from Discussion section 4.1 to the Results section 3.1. We have also condensed the discussion by approximately three pages to focus more on the mesocosms and removed redundant environmental statements as well as removed two figures.

REVIEWER #2, COMMENT 2: Phase 1 (section 4.2), there are no differences in contrast to expectations, don't need 1+ pages to say that.

Author response: We have now restructured and condensed this section to focus the discussion on the flux of carbon into the DOC pool and into sinking particle flux in Phase I.

REVIEWER #2, COMMENT 3: Section 4.3 ends with picoplankton were always affected by CO_2 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 increase relative importance, with a lower C:N, accounted for even the higher

C:N in the total?

Author response: As the reviewer correctly states, picoplankton appeared to be affected (positively) by CO_2 from early on in the experiment but were not abundant enough to influence particulate or dissolved matter pools. We are not sure if we correctly understood the reviewer's comments here, but as this is unclear we decided to remove this statement from the discussion (P6889, L28/29; P6890, L1) to avoid confusion and instead show TPC concentrations for the total and < 10 μm size fractions (Fig. 12).

REVIEWER #2, COMMENT 4: 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?

Author response: Bacterial production rates and respiration rates will be presented and discussed in accompanying papers (Hornick et al., in prep; Nausch et al., in prep; Spilling et al., in prep., Table 3 in the revised manuscript).

REVIEWER #2, COMMENT 5: While DOC concentrations are higher in the CO_2 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?

Author response: This is a valuable point that the reviewer raises about DOC bioavailability that was not explicitly considered in the manuscript. Higher DOC concentrations may have been, at least in part, due to a reduction in bioavailability. Unfortunately we have no information about DOC lability and so we cannot confirm this with the available data from this study. However, lower respiration and bacterial remineralisation rates (observed during the experiment, data presented in Spilling et al. in prep. and Hornick et al. in prep.) under elevated CO_2 could also explain the measured higher DOC concentrations in the higher CO_2 treatments. We have included this point in the discussion (p. 21, lines 15 – 17).

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REVIEWER #2, COMMENT 6: The discussion seems to focus on the channelling of carbon from POM to DOM cycling but isn't really clearly presented.

Author response: The discussion was focussed on CO2-related differences in particulate and dissolved matter pools and fluxes with reference to the size structure of the plankton community. We have reworked and restructured the discussion and hope that this message is now more clearly presented in the revised version of the manuscript.

REVIEWER #2, COMMENT 7: Section 4.5, seems like it should be in the conclusions more than its own stand alone 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.

Author response: We thank the reviewer for highlighting this. We tried to integrate this information into the conclusions, as suggested, however felt that the discussion about this point was too detailed to be included solely in the conclusions and thus justify inclusion as a separate section in the discussion.

REVIEWER #2, COMMENT 8: 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.

Author response: We thank the reviewer for their encouraging thoughts on this study and its potential contribution to the literature on the responses to ocean acidification. Please also see response to reviewer #2 comments 1, 2, and 6.

REVIEWER #2, COMMENT 9: 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 a belief that this was a sampling issue? Should we be concerned about the broader dataset or is this telling us something?

Author response: In many variables no outliers were removed, for example Chl a, pH, DIC, dissolved silicate, sinking particle flux. The decision to remove a small number of outliers in some data sets was made carefully and based on a statistical test for outliers (Grubb's test). While in a few data sets numerous outliers were removed (e.g. dissolved NO3- + NO2- , see Fig. 10A), these variables are often prone to errors in measurements because concentrations are low (nanomolar range) and are challenging to measure (e.g. dissolved inorganic nutrients). Other data sets are the result of a mass balance or calculation (e.g. dissolved organic nutrients, particulate matter stoichiometry) which combines the errors of two measurements. However we do not believe that these select examples bring the whole data set into question nor compromises conclusions presented in the manuscript.

Limited specific comments:

REVIEWER #2: Table 1: Lomas et al. reference is North Atlantic, not Pacific.

Author response: This has been corrected accordingly.

REVIEWER #2: Is Figure 1 really necessary – information in there seems tangential at best to the story.

Author response: Figure 1 was included as this gives a clear depiction of natural variability in pH in the Baltic Sea compared to other oceanographic regions. However, in light of the reviewer's criticism concerning the large number of figures and tables, we have removed this from the revised version of the manuscript.

REVIEWER #2: Figure 4, useful but not really necessary.

Author response: Here, we disagree with the reviewer that Fig. 4 (experiment timeline, now Fig. 3) is not really necessary as this provides an important and coherent overview of various manipulations and see this as a useful element in the manuscript. In particular, this manuscript is considered an 'overview paper' guiding the other publications in this special issue with background experimental design, sampling regime

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Interactive Comment

and biogeochemical setting.

REVIEWER #2: Figure 6, symbols horizontally – issue in upload or trying to show something?

Author response: These horizontal symbols are the values of average water column salinity, temperature and density from the CTD profiles. These have now been removed from Fig. 5 (was Fig. 6) and Fig. 8.

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