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Biogeosciences Discussions



# *Interactive comment on* "Temporal biomass dynamics of an Arctic plankton bloom in response to increasing levels of atmospheric carbon dioxide" *by* K. G. Schulz et al.

**Anonymous Referee #2**

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Review of Schulz et al., Temporal biomass dynamics of an Arctic plankton bloom in response to increasing levels of atmospheric carbon dioxide. Biogeosciences.

The manuscript seeks to explore the effects of ocean acidification (OA) on phytoplankton community structure and dissolved/particulate organic/inorganic nutrient dynamics in a fjord, the Kongsfjorden, in the Arctic, a region sensitive to rising atmospheric CO2. OA-induced changes in the microbial community have the potential to significantly modify organic carbon and nutrient fluxes to the region's water bodies and also the transfer of carbon through higher trophic levels, which may affect ecologically and economically important taxa.

Overall I am supportive of the manuscript. The research proposed here addresses key issues that have received little to no attention in OA research. First, even though microbial processes control biogeochemical cycling, their response to OA is poorly known, complex, and sometimes inconsistent. Additionally, there is little to no information on how OA will indirectly impact biogeochemistry via direct impacts on phytoplankton assemblages. And what I particularly found interesting was that the phytoplankton in this study location was not dominated by diatoms, but instead small-sized and diverse groups of phytoplankton. Most OA studies on non-calcifying phytoplankton thus far have focused on diatoms and N-fixers, and this study provides much needed insight for how different systems or systems in different seasons will respond to OA. If successful, data resulting from the work presented here would provide much needed insight to predict future changes in microbial community structure and biogeochemistry in the fjord due to ocean acidification, and will serve as a model study for other regions around the globe. I suggest that this paper be accepted after attention to the following revisions.

Major comments:

The intro needs more background information on phytoplankton community responses to OA. The authors reference one review paper on calcifying organisms, but do not give credit to previous studies that have directly examined phytoplankton community responses (specifically, non-calcifying phytoplankton) to OA (authors on this MS, Tortell, Yoshimura, & others). These studies need to be referenced and their results summarized here, as this was the main focus of the paper. And the authors need to end intro with justification for the research.

This manuscript needs a concluding paragraph, summarizing their results in the context of the breadth of knowledge of OA impacts on phytoplankton community structure. What do your results tells us about how the fjord will respond to long-term future increases in CO2? Do you hypothesize potential increasing dominance of prasinophytes? If so, how does this affect food web, biogeochemistry? Where do we go from here?



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Interactive Comment

Specific/technical comments:

Abstract: Name study location here

Intro:

Page 3: “Therefore, the Arctic is projected to be the first ocean region to become under-staturated. . .” I am most familiar with the Orr et al. (2005) model predictions, which suggest that undersaturation will occur in the Southern Ocean first due to the low existing saturation levels of carbonate minerals, uniform temperatures and the extent of mixing in the water column. Please clarify here.

Page 3: “ocean acidification can cause aragonite under-saturation already today (Bates et al., 2009;..” I suggest changing “can cause” to “has caused”.

Page 4: Again, name study location here or at least a more specific one than the current “in the Arctic”.

Methods:

Page 5: “Adult pteropods..” Why did you add different numbers of pteropods and why on those specific days?

Page 6: “t-4 and t4. . .” It would be helpful to add in a one sentence explanation regarding the timeline of your experiment at the beginning of the methods section and move Fig 2 up as well, to aid in understanding of your labeled days "t-4" vs "t4" for example.

Page 7, Section 3.3: You need to, early on in the methods, state which mesocosms served as controls. I assumed it was the 2 lowest pCO2 mesocosms, but I didn’t see this stated until results section 4.2 (discussion). Also, there was nowhere in table 1 with which to compare the pH or pCO2 of any mesocosms to the Fjord water – can you add pH and pCO2 of the Fjord to table 1? Additionally, when I think of a “control” in OA studies, I typically think of somewhere near average ambient atmospheric pCO2 (380-390 uatm), but the controls in this study were at significantly lower CO2 levels (due to



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post-bloom conditions). Is pCO2 of 185 uatm typical for this region? The results are interesting as it represents a true “low CO2” treatment as ambient atmospheric CO2 is now about 390 uatm. It would be nice to include a bit of discussion regarding this issue.

Page 7, Section 3.3: What is the justification for the chosen pCO2 levels? And why a gradient and not replicates of certain preselected pCO2 levels?

Page 7, Section 3.4: What is the justification for these specific concentrations of nutrients?

Page 8, Section 3.5: “water samples were brought back to shore and stored at in situ water temperature in the dark until processing. . .” State the length of time between sampling and processing.

Results:

Page 16, Section 4.5: “While silica concentrations in phase II displayed a statistically significant negative correlation to CO2, those of biogenic silica were positively correlated (compare Fig. 7A and B, and Tab. 2).” Is silica actually supposed to be silicate?

Page 17, Section 4.6: “DOC, starting at about 70-80 $\mu$mol l$-$1 in all mesocosms, increased before nutrient addition during phase 0 and I, resulting in higher concentrations at higher CO2 in phase II..” Did this affect total alkalinity? See Kim and Lee 2009, Kim et al. 2006, Koeve and Oschlies 2012.

Page 20, Section 4.8: “An exception were prasinophytes and dinoflagellates, important contributors to autotrophic standing stocks in all mesocosms during phase II and III, having insignificant contributions in the fjord during this time” Why the discrepancy between mesocosm and field? There are a few within the MS with regards to phytoplankton community and it would be beneficial to see some discussion on this later in the MS.

Page 20, Section 4.9: “ During phase III carbon biomass by diatoms was higher at



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lower CO2 levels, a trend found to be statistically significant" What kind of diatoms? Were they bloom-forming species? Why did diatoms not respond to higher CO2? low nutrients?

Page 20-21, Section 4.9: " An exception were autotrophic dinoflagellates with insignificant and chrysophytes with higher carbon biomass in comparison to the mesocosms at certain times." Again, why the discrepancy between mesocosm and field?

Page 21, Section 4.10: "REALTE" should be "RELATE"

Discussion:

Page 25, Section 5.2.1: "However, its relatively small size (less than 2 $\mu$m in diameter) could make the extensive operation of active CO2 and HCO$-$3 uptake, like in most bigger phytoplankton species (compare e.g. Giordano et al. (2005) and references therein) unnecessary, as the diffusive boundary layer can be considered relatively small (Riebesell et al., 1993)." This is the opposite of what is suggested by Tortell et al. (2008) who states: Larger chain-forming Chaetoceros species may be at a competitive disadvantage for C uptake under low CO2 conditions which induce an upregulation of cellular C transport (Figure 1), and favor small cells such as Pseudo -nitzschia with high surface area to volume ratios." This needs to be addressed, and I am genuinely interested in your thoughts on the matter. I suggest you include your reasoning for the opposing ideas.

Page 26, section 5.2.2: "During phase III of the experiment, after termination of the second bloom by viral infection (see Brussaard et al. (2012) for details)..." Even though you cite Brussaard here, this definitely needs expanded upon because it directly relates to your results. Did you add viruses to the mesocosms or were they natural viral infections. Did the infections affect all phytoplankton taxa similarly?

Page 26, section 5.2.2: "This is most likely an indirect CO2 effect as after the collapse of the second bloom in phase II, more inorganic nutrients were available at lower CO2



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concentrations (compare Figs. 6B and D)." If there were inorganic nutrients in the lower CO2 mesocosms in phase II, then why did the second bloom collapse? Was it due mainly to viral infection?

Page 26, section 5.2.2: It would help to have some background ecological information about this fjord to help interpret some of your results regarding phytoplankton community composition. Are the small phytoplankton species you saw during your manipulation typical? Or in contrast are there typically large diatom blooms in this fjord, but are maybe more prevalent during times of higher nutrient concentrations? Are the diatoms you saw bloom forming species? I am just trying to wrap my head around why higher diatom biomass would occur under lower CO2 conditions.

Orr, J. C., V. J. Fabry, O. Aumont, L. Bopp, S. C. Doney, R. A. Feely, A. Gnanadesikan, et al. 2005. Anthropogenic ocean acidification over the twenty-first century and its impact on calcifying organisms. Nature 437, 681-686.

Kim H-C, Lee K, Wonyong C (2006) Contribution of phytoplankton and bacterial cells to the measured alkalinity of seawater. Limnol Oceanogr 51: 331-338.

Kim H-C, Lee K (2009) Significant contribution of dissolved organic matter to seawater alkalinity. Geophys Res Lett 36: L20603, doi:10.1029/2009GL040271.

Koeve W, Oschlies A (2012) Potential impact of DOM accumulation on fCO2 and carbonate ion computations in ocean acidification experiments. Biogeosci 9: 3787-3798.

Tortell et al. (2008) GEOPHYSICAL RESEARCH LETTERS, VOL. 35, L04605, doi:10.1029/2007GL032583, 2008

Interactive comment on Biogeosciences Discuss., 9, 12543, 2012.