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Biogeosciences Discussions

Interactive comment on "Metabolic balance of a plankton community in a pelagic water of a northern high latitude fjord in response to increased $p\mathrm{CO}_2$" *by* T. Tanaka et al.

Anonymous Referee #2

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Overall assessment

This article is a valuable contribution to the special issue reporting the results of a project that addressed the effects of pCO2 on the biogeochemistry of microbial plankton communities enclosed in mesocosms. The article is well-written and the description of methods and results is (with some omissions) largely correct. The authors are careful in presenting their main conclusion, that high pCO2 leads to low net photosynthesis, with caution. However, there are additional discrepancies and oversights which may further weaken such conclusion. The discrepancies between the present results and those reported by Engel et al. need to be resolved, as well as possible methodological

biases in the calculation of NCP to nutrient consumption rations. There should also be more connection with the results reported in other articles of the same issue, regarding for instance elemental composition of particulate matter. Excessive fragmentation of closely related results into different articles may have prevented the emergence of a clear picture from this project.

Main comments

As with all short-term experiments looking at the biological effects of high CO2, all the papers in this special issue suffer from the same limitation: to which extent do short-term responses to sudden changes in pCO2 (hundreds of ppms in 5 days) inform us about the real effects of high CO2, which is increasing at a rate several thousand times slower? The authors should acknowledge this limitation in the discussion of their results.

There are well-known issues with long (24-48h) in vitro incubations, which however are not mentioned in this article: it has been shown that important changes in the abundance, activity and composition of phytoplankton and bacteria take place during in vitro incubations (e.g. studies by Pomeroy et al AEM 60, 1994, Calvo-Diaz et al. AEM 77, 2011 and references therein), which are likely to affect the estimates of both photosynthesis and respiration. The authors should discuss the possible impacts of these artefacts for their results and conclusions.

What are the main differences/similarities with the paper by Silyakova et al in the same issue, which seems to address a very closely related topic using data from the same experiments? This should be clarified at the end of Introduction.

It is not clear if the different temporal phases used to analyse the results can be interpreted ecologically. Certainly the authors do not interpret them ecologically. If the different phases do not represent clear ecological situations (e.g. the onset of a bloom, or a post-bloom phase), they are arbitrary and therefore it becomes difficult to translate the conclusions to the real world.

The authors calculate ratios of NCP to consumed nutrients in units of molC:molN and molC:molP. To do so, they converted the original NCP data (in O2 units) to C units, using a PQ of 1 (page 11023). PQ is strongly dependent on the nitrogen source (e.g. nitrate vs ammonia), which means that using a constant PQ may have led to significant biases, since nutrient concentrations changed widely during the experiment, and perhaps even among treatments. For instance, as added nutrients become exhausted, the importance of ammonium as a nitrogen source is likely to increase relative to that of nitrate, leading to a decrease in PQ. So to which extent the NCP decrease relative to N and P consumption (Fig. 3) has been, at least in part, caused by the use of a constant PQ? This is vaguely referred to in the Discussion but a more careful discussion of the issue is required.

If pCO2 does affect the stoichiometry of organic matter production and remineralization, this should be observed in the chemical composition of particulate and dissolved matter. However, and rather surprisingly, there is no mention to measurements of C, N and P in organic matter in the Results, and only very briefly in the Discussion. The data seem to be available (Schulz et al.) and should be discussed here in more detail.

Another article in the same issue (Engel et al.) reports a positive relationship between pCO2 and C14-based productivity. The present article does not really address the reasons for this discrepancy, but limits itself to discuss why 14C-based production is higher than NCP. The fact that opposite CO2-dependent trends are observed in NCP and 14C-based production must be addressed here. At first sight, the explanation given by Engel et al (that high CO2 induces high 14C-PP but also high release of DOC which in turns enhances microbial respiration, thus leading to lower NCP) is not convincing, since CR seemed to be independent of pCO2.

Specific comments

The title can be improved. It now includes superfluous content (in a pelagic water, northern) but misses the important fact that the studied community was enclosed in-

side mesocosms. I would suggest something like 'Effect of increased pCO2 on the planktonic metabolic balance during mesocosm experiments in a high-latitude fjord'

Somewhere (end of Introduction, beginning of Methods) a brief description of the study site is needed, so that the reader knows the context (especially, as far as seasonal variability is concerned) of the experiments. This is also important to assess whether the nutrient addition mimics something that does happen in this system, or else if is not realistic. If an increase in nutrient supply during summer never takes place in this system, then the experiment is studying an artificial community, and the extrapolation of the results to the real world becomes even more difficult.

A more complete description of mesocosms is needed: material, volume, dimensions, etc. Was there any recirculation/stirring of water? Sedimentation must have been intense, particularly after nutrient addition, and vertical gradients very sharp. The vertical profile of irradiance must have changed dramatically after the onset of the nutrient-induced bloom – however BOD samples were incubated at a constant depth of 4m.

Chla levels are important to understand what's happening in the different mesocosms and in fact are used to define temporal phases. They should be shown in a plot similar to Fig. 1.

Minor points

Given that NCP measurements based on 13C-accumulation and DIC changes are also used in the Results section, they should be briefly mentioned in Methods.

Table 3, Fig. 4. Three methods are used to determine NCP, not four as stated.

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