

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

Anonymous Referee #1

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This manuscript treats about the acidification effect on the plankton metabolic balance in a northern high latitude fjord. The acidification experiment was conducted on 9 mesocosms establishing 7 enhanced $p\text{CO}_2$ treatments and 2 replicated controls. After 7 days of experiment, the authors induced the development of a phytoplankton bloom to all mesocosms. The authors observed a significant decrease of NCP and GCP with increasing $p\text{CO}_2$. The CR was relatively stable throughout the whole experiment. The authors concluded that elevated $p\text{CO}_2$ influenced NCP of the planktonic community albeit this conclusion had to be taken with caution.

My general evaluation: The manuscript was in my opinion quite confusing and difficult

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to understand. The authors did not explain clearly in the title and the introduction of the manuscript that the experiment did not treat just about acidification effect on planktonic communities but also about the bloom effect on communities receiving acidification treatments. After reading the manuscript, we are quite confused about the results and their interpretations. We don't really know if the results obtained by the authors were caused by an increase of temperature during the experiment, by different incident light receiving at the mooring site in comparison with the sampling site (we don't know because it has not been measured), by the nutrient supply or all together. Furthermore, we do not know if the obtained results were due to the method used by the authors to measure the planktonic metabolism and how could have been the results if the authors had rather used for example the ^{14}C method. Furthermore, the authors concluded about “significant” decrease of NCP with increasing $p\text{CO}_2$ when for the whole experiment, phase 1 and 2, there is no significant NCP increase. I think that the manuscript can be very interesting but important data are missing to improve much better the manuscript (addition of a nutrient control mesocosm, measurement of the planktonic metabolism using a C-labelled method, daily planktonic metabolism, daily pH, $p\text{CO}_2$, Chl, BA, temperature and etc measurements). I recommend this manuscript for publication in Biogeosciences with major revisions.

Specific comments

Title and Introduction:

You should introduce the nutrient bloom of your experiment on the text.

Materials and Methods:

- Why did you choose to make your experiment with post-bloom communities? Don't you think that planktonic communities could respond differently to a $p\text{CO}_2$ increase and nutrient supply with pre-bloom planktonic communities?

- I recommend you to show in table and in a graph the daily evolution of pH, $p\text{CO}_2$, Chl

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a, temperature, NO₃ and PO₄ concentration during the entire experiment.

- Please insert a reference for the nutrient concentrations that you used to simulate a phytoplankton bloom.

- Why did you sample mesocosm water for the planktonic metabolism incubation in the afternoon when it is commonly made early in the morning?

- You should precise in the text that the four bottles immediately fixed with Winkler reagent are considered as control for the DO measurement.

- "NCP and CR were measured every 2 day and every 4 day", I do not really understand. Did you measure NCP and CR at t₂ and t₄ at each phase or at t₂, t₄, t₆, t₈, t₁₀ and etc? Please could you make it clearer?

- Please could you specify that planktonic metabolism was determined at 4m depth because "the incubation was at 4 m depth" means to me that the bottles were incubated at 4 m depth.

- How can you explain that CR measurements did not detect change in DO during 24h incubation in the dark? Did you use dark bottles and put them into dark plastic bags for the incubation? What can be the bottle effect or confinement effect after 48 h of incubation into 60ml bottles? How could those 48h of incubation bias the measured CR rates?

- Please could you also explain how did you calculate the SE of NCP and CR?

- Do you think that 7 days it was enough to observe any effect of pCO₂ increase without any nutrient supply? Do you have any reference that supports your decision to start the phase 2 at t₇? Why did you not measure the planktonic metabolism every day? How do you explain the "second chlorophyll minimum", its origin?

Results: - Why did you use cumulative data rather than means? Any reference?

- Lines 190-192: Is it significantly higher? Please could you improve the different

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comparisons in the results section with statistical test?

- How do you explain that you observed 3 times a peak of chlorophyll in all mesocosms? Which interpretation do you have for this phenomenon?

- Lines 220-222: Did NCP increase significantly after nutrient supply? (statistical test)

- Lines 226-232: Could you please clarify this section?

- Why did you choose a photosynthetic quotient of 1 when the common conversion factor is of 1.25 molar stoichiometry between O₂ and C (Williams 1979; Davies and Williams 1984).

Discussion - Lines 266-267: Does the increase was significant? Please could you add some statistical results?

- How do you explain the second chlorophyll minimum? It can be interesting to add some sentences about this chlorophyll minimum explaining why it occurred and how.

- It could be interesting to see (add a figure) the evolution of the chlorophyll concentration during the global experiment.

- Could you please add some references for the different methods Ct, 13C, 14C.

- Lines 306-307: Do you mean that we can consider the POC from 14C as the NCP from O₂ method and the DOC from 14C as the GPP from the O₂ method? If you mean that, I am not agreeing with you about the comparison between the DOC and GPP. On contrary GPP could be closer to POC+DOC than POC alone or DOC alone. You should revise this sentence. I am also quite surprised that POC from 14C was higher than NCP from O₂ method. Could you add please some statistical information (significant or not, test, P)?

- Why didn't you measure the NCP with O₂ method in the same way than with 13C and Ct (daily)?

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- Lines 331-333: You should give more information about the irradiance, some data.
 - Lines 338-340: You don't know if the planktonic communities received a light-stressed at the mooring site comparing with their sampling site?
 - Lines 359-363: Your conclusion about the decrease of NCP with increasing pCO₂ is maybe a little bit too optimist when you actually see that for the global experiment, for phase 1 and/or 2, there is no significant decrease of NCP with increase pCO₂. Finally, we can observe a NCP decrease just for the phase 3 that it does not allow us to conclude that NCP decrease with pCO₂ increasing. I do not agree your conclusion.
- Table 1: It could be more helpful to add a column on the left specifying which mesocosms are controls, which received low, intermediate and high pCO₂. Please could you add also pH and pCO₂ for phase 2?
- Table 2: I don't know if the table format changed when I loaded it but the heading columns are cut. Please could you remake this table and put in a same line entire words as "Parameter", "Temperature" or "umol Si l⁻¹", 0.05 ± 0.01 ". How did you integrate the temperature data?
- Figure1. Please could you add in the figure some lines defining the beginning of the phase 2 and phase 3? How could you explain that you have more NCP data than CR data and why?
- Figure 3. It is difficult to differentiate which line corresponds to which points. Could you please use different type of line for the different phases?
- Figure 4. Could you please add for each graph line 1:1?

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