Biogeosciences Discuss., 9, C6125–C6128, 2012
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Biogeosciences Discussions

Interactive comment on "Effect of CO_2 enrichment on bacterial production and respiration and on bacterial carbon metabolism in Arctic waters" *by* C. Motegi et al.

Anonymous Referee #1

Received and published: 1 December 2012

The present manuscript deals with an interesting and so far largely unknown question: Does the increase in pCO2 affect bacterial dynamics and carbon cycling in the future ocean? Hence the manuscript is of great scientific relevance. Although I think that the manucsript is of great interest for readers of Biogeosciences, I have some major concerns regarding methods used and the clarity of the presentation.

First of all, the manuscript is rather descriptive since many relationships, e.g. between the bacterial parameters and those of virus, grazers and phytoplankton, should have been tested by the appropriate statistics. Therefore, I rate large fractions of the discussion highly speculative.

Secondly, the separation between bacterial parameters of free-living and attached bacteria needs some clarification. If I understand it right, bacterial respiration always refers to that of free-living bacteria (0.8 um prefiltered)? Whereas for BP both fractions have been measured... In conclusion also BGE and BCD only include those of the free-living bacteria? Also with flow cytometry only the free-lving bacteria can be counted...

Third, I do not see the ratio of seperating between BP-TdR and BP-Leu. In particular, it has been shown by Perez et al. 2010 (EMI 12:74-88) that uptake of thymidine and leucine is highly species-specific... Hence this ratio also changes with a changing bacterial community composition.

The centrifugation method for bacterial production measurements (due to its small volume) is rather insensitive if the activities are low (as I assume they are at such low temperatures and at the rather short incubation time of 1 hr). This is also reflected by a CV of up to 41%!

Further, the day to day comparison to better resolve for dependencies between bacterial parameters and pCO2 seems to be a bit problematic for me, since the statistical procedures to test for significance are rather limited.

Finally, I think the discussion includes a lot of speculations. To better put the obtained results into context I suggest that the authors give a brief overview on the results of other working groups on viruses, grazers and phytoplankton. As a reader I find it hard to obtain the necessary information from the cited and yet not published references... I strongly recommend some more statistical approaches to proof for dependencies of bacterial parameters on the cited environmental parameters!

Specific comments: P15214, L26: ...conditions...

P15215, LL8ff: references Grossart et al. and Allgaier et al. 2008 are cited in a contrary manner. hence the statement should be more precise. P15215, L21: For nitrification you may also refer to the pCO2 dependence of cyanobacteria (Wannicke et

BGD

9, C6125–C6128, 2012

al. Biogeosciences, 9, 2973–2988, 2012) P15125, LL21ff: pCO2 influence on anabolic and catabolic processes has been studied by the references given above (hence the statement needs some rephrasing...).

P15216, LL3ff: Leu vs. TdR uptake may differ with community composition (Perez et al. 2010) P15216, LL19ff: Taking integrated water samples for activity measurements could be problematic since incubation conditions may not sufficiently well reflect in situ conditions??? Please comment.

P15217, L12: low temperatures and short incubation times may lead to a high CV of up to 41%. This should be mentioned in the method section. P51217, LL14ff: Seperation between free-living and attached bacterial parameters is unclear. When was the filtration done? Before or after the tracer incubation? Did you yield negative values when substracting free-living activities from total activities? Please be more precise in the method description! P51217, LL22ff: Die the 0.8 um prefiltration affect your respiration rtaes, e.g. due to shear stress during filtration? Please specify if this is only the respiration of the free-living bacteria?! P51217, LL22ff: Cell specific activities should be given as cs instead of s

P15218, LL4ff: Did you only include BPfree? P15218, LL10ff: problems, particularly with the TdR incubation, see Perez et al. 2010 P15218, L14: With flow cytometry you only measure the free-living bacteria... Please also mention the high nucleic acid bacteria...

Discussion: it is difficult to put the results into a context without having a brief overview on the most important environmental parameters such as viruses, phytoplankton etc. P15220, L25: see my comment below, how did you measure these? P15220, LL25ff: ... suggestion for viral lysis comes out of the blue!

P15221, LL5ff: relation between bacteria, viruses and phytoplankton should be tested by statistical approaches...

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9, C6125–C6128, 2012

In general, the discussion has lots of speculations!

For the figures I suggest not to use the day to day approach. It would be better to seperate between certain phases during the mesocosm development as has been done in the other publications of the same experiment!

Interactive comment on Biogeosciences Discuss., 9, 15213, 2012.