Biogeosciences Discussions

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Comment on bg-2021-220

Anonymous Referee #2

Referee comment on "Contrasting patterns of carbon cycling and DOM processing in two phytoplankton-bacteria communities" by Samu Markku Elovaara et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-220-RC2, 2021

The manuscript determined the carbon cycling and subsequent effect on bacterial community composition using an experimental approach based on monoculture of two common phytoplankton species in the Baltic Sea, A. malmogiense and R. marina. The manuscript found the clear differences in carbon cycling including the DOM composition and degradability which cause predominance of different bacterial species. The conclusion was derived from comprehensive experimental data, i.e., carbon cycling with 14C tracer experiments, DOM degradability with its composition, bacterial community composition etc. This manuscript is novel and contains some very exciting results that I think will be of interest to readers of Biogeosciences. However, I have some major comments which will improve the manuscript.

(1) The discussion, conclusion, and implication to natural environments are generally well written based on the experimental results. However, I do have one concern about this point. The authors added natural bacterial inoculum to phytoplankton incubations. However, the major bacteria in the DOM consumption experiments was derived from non-axenic phytoplankton cultures but not from natural seawater inoculum. In this case, what is the source of bacteria in non-axenic phytoplankton cultures? Furthermore, is the production and degradation of DOM by these bacteria comparable to the bacterial community in natural seawater? The authors should describe/discuss these issues in the revised manuscript.

(2) The current manuscript, in particular the method section, is hard to follow. One of the reasons is likely that some information is only available in figure caption (e.g., Fig. A1). It is recommended that the authors move all information about the methods described in the figure captions to the methods section of the text. The other reason is probably that the manuscript contains many experimental lines having diverse chemical and biological analyses. I guess it's better to combine Fig. 1 and Table A1 to overcome this issue.

(3) I could not understand the reason why the authors set the DOM line mix. In addition, the authors seemed to determine the cell specific BP by tracer experiments of production line mix and bacterial abundance of DOM line mix. Are these two results directly comparable to determine cell specific BP?

(4) It is recommended that a subsection regarding with carbon cycling (e.g., points described in graphical abstract) is added to the beginning of discussion section. Some descriptions in the result section and the 4.3 subsection may be able to move this new subsection.

Specific minor comments

L 131-133: Please add this information in Fig. 1.

L 679-383: I could not understand how the authors determined the incorporation of 14C-labled DOC and bacterial production by 3H-thymidine and 14C-leucine and. Please explain it with more details. Again, please move these descriptions in the method section of the text. In addition, it is recommended that the meaning of ratio of leucine to thymidine incorporation is explained here.

Figure 2 and other similar figures: In Figure 2 and other similar figures, the results of A. maim. located left side in the time series (Fig. 2a) but that located right side in the box and wisher plot (Fig. 2b). I was sometimes confused by this difference. I think it is better to place the A. main. data on the right (or left) side of every panel in all figures.

Figure 5: I assume that the bacterial community composition of DOM release experiment and that of Day1 in DOM consumption experiment should be similar, because the major fraction of bacteria in these experiments was derived from the same non-axenic phytoplankton culture. However, bacterial community compositions seem to be largely different between two treatments in Fig. 5. Why?

L443-445: It is recommended that the DOC data is added in Fig. B1.

L482-484: I don’t agree with this conclusion. The qualitative parameters of DOM, namely S275-295, HIX, BIX etc, changed significantly with incubation time and differed among KPI3 1-3 (Fig. 7B).

L495-496: “are be able to”. Please rephrase it.