

Interactive comment on “Increasing addition of autochthonous to allochthonous carbon in nutrient-rich aquatic systems stimulates carbon consumption but does not alter bacterial community composition” by K. Attermeyer et al.

Anonymous Referee #1

Received and published: 10 October 2013

This manuscript presents an ambitious attempt to understand how bacterial turnover and community structure responds to pulsed additions of fresh autochthonous organic matter. The manuscript is data rich and a large number of methods have been applied. In general, the paper is clearly structured and the language reads well.

The overall aim is to mimic something that is happening in nature - pulsed inputs of phytoplankton derived DOM as blooms crash for example - and to study how the bacteria respond to that. I see two problems with this. First, it is to a large extent descriptive. I lack some clearly stated hypothesis that can serve as a backbone throughout

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the paper. Secondly, I do not think that the experiments mimic natural conditions well. The allochthonous DOM is made out of leaf leachates that are highly labile to bacterial degradation, but rarely enter the aquatic environment. What enters the aquatic environment are the leftovers after the soil microbes have utilized what they can. Also, I am guessing that there was bacterial growth in the phytoplankton cultures that were used to provide autochthonous DOM. If the research question was not so focussed on natural conditions, this would not be a problem. I do think this data can answer some relevant question, but I think it requires a retake on the study as a whole.

Line 15 in introduction. This reference is not a good one to support that there is increasing amounts of terrestrially derived DOM in freshwaters. It is an experimental manipulation. Line 21 Only bacteria and some osmotrophs can directly use DOM. Line 22 There are other processes that are important in controlling DOM turnover, and other factors than concentration and quality that influence bacterial DOM degradation. I don't think it is necessary to point that out in the text, but you cannot write like this. I don't really agree with your interpretation of Langenheders paper, and that has relevance for how you interpret your own data too. That the source of the inoculum is more important than the DOC sources, does not mean that the latter is NOT important. Page 2 line 18. I think this is not a relevant reflection - how could they be.

The experimental design is well explained by figure 1! The way you have designed the experiment makes it difficult to separate the effect of DOM quantity versus quality, since you change both at the same time.

Please state explicitly what is the purpose of the controls. Is "control" really the correct term here, I wonder.

Also, I think it would be more clear if inoculum refers only to the addition of bacteria and that addition of DOM is called "amendment" or similar.

The manuscript is rich in acronyms. I can see it is required, but make it easier for the reader to follow along by reminding us what they mean in results and discussion

C5813

sections.

I think the quality analyses of the two DOM sources illustrates very clearly that this is not mimicing natural conditions.

There are many clever graphs, but I lack a basic one where one can actually follow DOC loss over time. It is fundamental to the understanding of this paper.

I don't follow in the section 3.4 what data you have and don't have. This makes it hard to judge the conclusions made on this data. I don't understand how you separate what is being respired and what is used for biomass.

A general comment for the results and discussion is that it is very data rich and since it is not focused around clearly stated hypotheses it becomes very descriptive and it is hard to see the context. What is the theory that you want to test?

The discussion is to some extent a deeper presentation of the results. I lack the connection to clear questions/hypotheses and reference to literature in large parts. The discussion is not focussed around the question you introduced in the introduction, but discusses results that are not strongly related to that question. For example, the effect on bacterial community is 10 lines in the discussion and lacks references.

Discussion line 10. This is really a non-statement. Be specific. Line 16. This relates to aging and is not applicable here. I guess you have fresh allochthonous DOM but aged autochthonous DOM? If there was bacterial growth in the phytoplankton cultures? Line 25 "higher bacterial DOC degradation" - what do you base that on?

section 4.2 line 10 "the microbially unprocessed and thus bioavailable DOC was related to DOC quantity" - I find this confusing.

4.4. The results on selective utilization bears strongly on findings in Kritzberg et al. 2005 AME and Kritzberg et al. 2006 FEMS and the group around Paul del Giorgio (McCallister) has also studied this.

C5814

Page 14280 line 3 "another reason" it is not clear what the first reason was.

5. Conclusions "Our study highlights the importance of DOC quantity for bacterial DOC consumption and DOC quality for BCC" - is this not at odds with the title of the manuscript?

Again, I appreciate the hard work that went into this, and I think it can be informative if you center it around other, clearly formulated, questions. Best of luck!

Interactive comment on Biogeosciences Discuss., 10, 14261, 2013.

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