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Discussions

Interactive comment on "Ocean acidification indirectly alters trophic interaction of heterotrophic bacteria at low nutrient conditions" *by* Thomas Hornick et al.

Anonymous Referee #1

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This manuscript addresses an interesting, relevant and timely issue - how bacteria and their C processing may be affected by ocean acidification. As is also pointed out, there are no reasons to expect strong direct effects, while there may be indirect effects channeled through other parts of the food web. This topic is addressed in large scale mesocosms with differing levels of CO_2. Unfortunately, I don't find that the manuscript is very clear or efficient in addressing the issue. It is a difficult approach to study a large suite of variables that are to a large extent interdependent and try to understand what has actually happened. In my view, this study shows very minor (if any) effects of CO_2 on the bacterial variables measured, and it is hard to clearly link those minor effects to any particular process. Linguistically, I think the manuscript is clear, but I think results

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Discussion paper

are overstated and relationships over-interpreted, and that the paper lacks a clear focus and structure.

Specific comments:

It is unclear in the title what "trophic interaction" refers to.

There is too little information given to be able to evaluate the methods applied by reading this paper alone. There is a lot of self-referencing to papers covering the same experiment in all parts of the manuscript and this is problematic. Important information that is missing in the methods is for example the dimensions of the mesocosms and the principles behind measuring physical and chemical parameters.

No information is given on the methds behind the estimation of low and high DNA bacteria. Results are included in the figures on low vs high DNA bacteria, but not mentioned in the results text.

It is unclear how statistics were used to show the relationship between e.g. bacterial variables and CO2 within a given time period - how did you account for time within each period?

There is referencing in the results part. Lines 211-218 should be deleted. This manuscript should be able to stand on its own and not make the assumption that we have or will read the other papers from the same experiment. The motivation for dividing into P1 - P3 should be more explicit.

Lines 228-229 ”During P2, concentrations of Chl a increased again”. I don’t think this concurs with the graph.

Lines 236-237 A Spearman rank correlation does not allow to make an interpretation that distinguishes some treatments from others.

Lines 238-240 This negative relationshop between BV of picos and Chl a is puzzling, especially since BV makes out the majority of phytoplankton biomass during the second half of the experiment.

Since bacteria are the focus of this manuscript (as I understand the introduction), the results regarding bacteria should be placed first, not phytoplankton. The effects of the treatments on the bacterial variables throughout the experiment are very small. The only statistical effects reported are for P1 and by looking at the graphs (Fig. 3), the relationships with CO2 are hard to discern. Then a few time points are selected and emphasized in the results and discussion because they show differences in relation to CO2 treatments, but they make a out a short period of the experiment.

Figure 4 is not commented on in the results text?

The discussion overall is a little tough to follow, since is not very closely aligned to or focused on the main issue. The discussion shows the difficulties in knowing what a statistical relationship means in this kind of study - the relative role of resource abundance, grazing and viral infections can only be speculated around. Still there are plenty of statements like "...revealed several indirect responses to fCO2, resulting from alterations in phytoplankton community composition and biomass". I am not convinced that the data support such statements.

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