

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

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This manuscript is one of a series generated from a mesocosm study conducted in mid- to late summer in 2012 in the Baltic Sea. The time frame selection targeted the period after the spring bloom, when nutrients were expected to be reduced, and the coupling between autotrophic and heterotrophic productivity might be altered. Dissolved CO₂ in each mesocosm was manipulated to produce a range of fCO₂ from 365 to 1231 μatm fCO₂.

One major concern is the confounding of fCO₂ levels and microorganisms added with the CO₂-saturated seawater to adjust fCO₂ levels. According to Paul et al (2015), different volumes of 50 μM -filtered seawater were infused in the mesocosms to achieve

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a gradient of $f\text{CO}_2$. This level of filtration will pass viruses, small grazers, and other microorganisms that can influence trophic interactions. Because the volume of added seawater is correlated with $f\text{CO}_2$ levels, it is not possible to separate the abiotic CO_2 effect from unknown biotic effects. This confounding problem was not addressed in the manuscript and is a serious problem.

Temperature is a major driver of bacterial abundance and production, but it was not included, even as a covariate, for any analysis. Going back to Paul et al (2015), temperature varied nearly 8°C in a non-monotonic fashion over the experimental period. This important variable should not have been ignored.

Given the number of variables and potential interactions, why wasn't multivariate analysis or similar integrative type of analysis used? Identifying relationships through multiple univariate and bivariate patterns is cumbersome and not necessarily clear to the audience.

Throughout the manuscript, there are references to significant differences in values. However, there was only 1 mesocosm per $f\text{CO}_2$ level (except for duplicate controls), and no replicate sampling per mesocosm at each time point. There is no information about variation, and therefore, no statistical basis for making statements about significance. Declared differences are based on subjective assessments, rather than objective data analysis.

The discussion could be more succinct and relevant. Much of section 4.2 can be removed, because it is mostly speculative, and ironically, emphasizes the confounding problem mentioned above. This section also contends that grazing was responsible for the drop in bacterial biovolume at higher $f\text{CO}_2$, but there is no supporting evidence from this study to support a grazing claim. This is an important point, because the claim is repeated in both the conclusion and abstract.

Related to the decline in bacterial biovolume at higher $f\text{CO}_2$ are the actual results, displayed in Figure 2.1.C. Careful examination of that panel in the figure shows that one

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of the control mesocosms (368) exhibited a similar decline, for a slightly shorter period of time. In reality, without any information on variation around the data points, it is dangerous to be developing and discussing elaborate explanations of these patterns, if they are even accurate patterns.

Minor points.

Discussion: Numbering for the sections need to be corrected. There is no number for the first portion, and two sections labeled “4.1”.

Figure 3. y-axis label for Figure 2.I.B should be for cell-specific BPP.

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