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## ***Interactive comment on “Coupling of heterotrophic bacteria to phytoplankton bloom development at different pCO<sub>2</sub> levels: a mesocosm study” by M. Allgaier et al.***

### **Anonymous Referee #1**

Received and published: 12 March 2008

This manuscript discusses the effects of increased atmospheric CO<sub>2</sub> on bacteria in a mesocosm experiment. The subject is important, it looks at the effect of CO<sub>2</sub>, and thus also of pH, on bacteria in the context of a near-natural ecosystem. The results are mainly "negative" in the sense that central properties like bacterial production and abundance are not significantly affected. This is contrary to results from a previous, similarly designed, experiment published by some of the authors. It should be stressed that this "negative" result is an important result, it seems however (understandably) to have given the authors a presentation problem. My impression is that the important and basically simple message of "minor effects" tends to drown in an overly long and detailed presentation. My main comment is therefore the manuscript could benefit from

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a stricter prioritizing in both introduction and discussion.

Specific points:

The relationship that is stressed in the Abstract is the significant relationship between both bacterial production and cell-specific bacterial production (&#8220;growth-rate&#8221;) and C:N-composition of suspended matter. This is intuitively plausible and seems to relate to the observed change in inorganic C:N drawdown shown in the same experiment. This relationship seems strange, however, considering that neither bacterial production nor C:N in suspended matter are significantly affected by CO<sub>2</sub>-level. Does this mean that bacterial production follows minor changes in particulate C:N within each treatment level or what??? I would have liked more help from the authors to understand this somewhat paradoxical point than what is provided (p.333 l.17)

To test for significant effects, the authors use repeated measures ANOVA, which at first may seem like the appropriate statistical technique. I have a problem convincing myself that it is. There is an obvious autocorrelation in mesocosm data, a high value in one bag at one day presumably increases the probability for a high level the next day. Seems to me to mean that samples are not as independent as I believe is a requirement for a repeated measures ANOVA (or?). The point may not be crucial in the present context since the conclusion for the ANOVA is that there are no significant effects. My intuitive feeling is that the autocorrelation would tend to increase the danger of falsely concluding with a significant difference.

Details: P 319 l. 12. There should be a reference also to the autolysis part as there is to the viral lysis.

p.320 l.20. I think the idea in present models is that grazing can restrict the size of the bacterial community, while viruses only restrict the size of their host-population. As long as there are other hosts to replace those that are controlled by lytic viruses, viruses can probably not limit total bacterial biomass (or?)

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p. 330 I.22: Should not specific viral production be calculated as  $\frac{N(t) + N(t+1)}{2}$  in the denominator? Does probably not have consequences for the regression analysis.

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Interactive comment on Biogeosciences Discuss., 5, 317, 2008.

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