Biogeosciences Discuss., 5, S430–S433, 2008
www.biogeosciences-discuss.net/5/S430/2008/

Biogeosciences Discussions

Interactive comment on "Microzooplankton grazing and phytoplankton growth in marine mesocosms with increased CO_2 levels" *by* K. Suffrian et al.

K. Suffrian et al.

Received and published: 25 April 2008

Interactive comment on “Microzooplankton grazing and phytoplankton growth in marine mesocosms with increased CO2 levels” by K. Suffrian et al. Anonymous Referee #1 Received and published: 14 March 2008

Dear referee,

Thank you very much for the valuable comments and suggestions. Reworking the manuscript, in our opinion, lead to a much more focussed and readable version. The restructuring and reformulating turned into a major revision, and we hope, that you will appreciate this. We hope to convince you, that the manuscript has improved much.

Thank you for your effort, Kerstin Suffrian on behalf of all authors.

I have some specific comments that should be considered by the authors prior to publication.

Methods: 1. Did the authors forget to include the equations on how to calculate grazing and growth rates for the dilution approach? They describe the variables but do not show the equations. Please add , or -even better- plot an example.

The equation was in, I have to say on behalf of our defence, but got lost somewhere in the typesetting process. Of course this was corrected. It was explained now, too, how the growth rates are derived.

2. By looking at the results, I realized that the bottle incubations for the different CO2 treatments were not performed at the same day, but within a time span of 3 days. This needs to be stated more clearly in the method section. Moreover, the authors should comment on the potential variability of other parameters within the 3-days time span, e.g. irradiance.

Thank you for pointing out to this important point. The consecutive experiments were stated clearer and a discussion was included on how results have to be read.

3. Initial nutrient addition to the mesocosms was performed using a DIN to DIP ratio of 25, likely resulting in P-limitation of the system. During the grazing experiments nutrients were added in a ratio of N:P of 10. Why? Wouldn’t this affect phytoplankton community composition in a different way than in the mesocosms?

At the beginning of PeECE III a N:P ratio of approx. 25:1 was used to stimulate growth of the coccolithophorid Emiliania huxleyi (Egge, 1993; Egge & Jacobsen, 1997). When nutrients were depleted in the mesocosms, they were added to our experimental bottles to ensure unlimited algal growth rates. During the post-bloom phase if any nutrient NO3- would have been limiting. As nutrients were never completely depleted in our experimental bottles (Table 2), nutrients were added in a more natural N:P ratio of

BGD

5, S430–S433, 2008

approx. 10:1. Nevertheless, although growth rates stayed low during this phase of the experiments, they have to be considered maximal potential growth rates. The addition of nutrients in a different ratio does not have changed the species composition by favouring other algae than before. This is in agreement with data from Paulino et al., (2007) and Schulz et al., (2007), which show the same species composition as our results.

Results & Discussion: 4. The authors chose to quantify phytoplankton standing stock and community composition by pigment analysis. However, zooplankton graze on cells rather than on pigments. Can the authors give an estimate for the size range of cells within each phytoplankton taxa, and how it changed during the experiment? This would be important to get an idea on the predator to prey size ratio within each group. Also, what was the contribution of the specified phytoplankton groups to the total community (give at least % Chla)?

Phytoplankton cells were not analyzed quantitatively during this study. Data on this is unpublished yet (V. Martin-Jezequel). From optical impression phytoplankton cells in the experimental bottles (pre-filtered at 200 μm) were well in a size-range, considered to be prey of dinoflagellates as well as ciliates (see discussion, 4.3).

5. I think that the authors can potentially valorise their study by analyzing the size distribution of microzooplankton during the experiments. Since they made the effort to count and seize the cells of the microzooplankton community, they should present and discuss this information. Information on the microzooplankton biomass in carbon units would be more interesting when related to other carbon based data of the study, i.e. macrozooplankton and POC (Schulz et al.).

Size range of the μZP was added and μg C was also given as μmol C, to enable a comparison with other studies. For single taxa also the development of their size over the course of the bloom was added.

6. I suggest that the authors discuss the results of their study in more detail. Some

BGD

5, S430–S433, 2008

findings are not easy to understand, e.g. for some incubations growth as well as grazing rates are negative, or close to zero. How reliable is such a result? Does this indicate that the assumptions for estimating g and k with the dilution technique were not fulfilled?

Thank you very much for these remarks. We made a major revision of the discussion, and find that it has improved significantly.

Minor comments: 7. I don’t think that the arrows in figure 2 help to understand the dynamics of growth versus grazing, especially since the values are heavily fluctuating between dates. I suggest removing the arrows as well as the labelling of dates.

We have already worked quite a bit on these graphs, and found the arrows to be helpful for many people. We thus decided not to change this figure. We do certainly understand your point though.

8. Table 1: 1xd13, PO4 concentration correct?

This value has been checked quite a few times. We do not know, why this value is so high, we thus indicated it as an error in the measurement.

9. Page 413: line 2: change to ’.. increased ocean acidity with a pH drop of 0.1 in the surface ocean since...’ Page 413: line 24: change to ’..and its potential feedback effects on carbon cycle.’

Done.

Interactive comment on Biogeosciences Discuss., 5, 411, 2008.