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Biogeosciences Discussions

Interactive comment on "Dissolution of coccolithophorid calcite by microzooplankton andcopepod grazing" *by* A. N. Antia et al.

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This manuscript addresses the control of zooplankton grazing on the dissolution of pelagic calcium carbonate from marine phytoplankton. This is a poorly understood topic and yet one which the research community must improve its understanding if it is to better quantify the role of pelagic calcifiers in the marine carbon cycle and pertinently, how changes in ocean acidification, through changes to carbonate pump, may feedback on atmospheric carbon dioxide. The paper used two approaches: the first compares the grazing impact of two microzooplankton and a copepod on a monoculture of Emiliania Huxleyi; whilst the second approach studied the potential effects of varying carbon dioxide concentrations on calcification rates and calcite dissolution due to grazing within a natural pelagic ecosystem during the PeECE III study.

I found the paper very difficult to read. The paper lacks coherence and the writing style is often confusing. The methodologies are poorly described and the monoculture experiments have methodological flaws (see also the other reviews the details of which I need not repeat here) which without better qualification deem the research non-publishable in its present form. The representation and interpretation of the PeECE III results must also be improved.

The paper requires a significant rewrite if it to published in this journal. As a guide, but this is no means a comprehensive review as it is not possible to fully understand the work done, I include some specific comments to the paper.

The introduction requires more substance (supported by a greater use of references) to give a solid reasoning for performing this research in the first place – ecological and biogeochemical consequences of a better understanding of the topic. It would also be useful to have a short review of the processes that control calcite dissolution in the ocean: it is only by Page 13 that other processes (e.g. aggregates) are mentioned. There is no mention on ocean acidification and yet this was the main reasoning behind the PeECE III experiment.

Material and Methods

P4 L9 How was the seawater buffered? And what was the original total alkalinity of the seawater after buffering? This is important for future comparisons with this research. At 14 degrees C and a pH 8.15 (assuming a laboratory pCO2 of 750ppm results in an omega for calcite of over 6.3 which is very high and may result in a greater production of CaCO3 than found in natural waters. Varying calcite saturation states may result in different coccolith shapes and thicknesses with different susceptibility to grazing and dissolution. P6 L1 I would like to see a description of the methodology detailed in Saffrian et al (2007) also included here. P6 L5 I doubt the method can estimate the production and loss of ANY (i.e. every) variable that is of autotrophic origin. P6 L6. You refer here to calcite measurements. But how were the rates of production and

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dissolution calculated? P6 L9 as for Chl a? You have not said how this was taken previously P6 L15-18 This really says nothing. What rates? I presume you mean calcite dissolution of production but it could be copepod growth rate, nutrient uptake rates, Chl a production rates etc

Results

More information on the seawater composition is required for any work on calcification as the calcite saturation state is required to compare results with future experiments. For a control run to be applicable to the experimental runs it is a requirement that the conditions are identical at the beginning of the experiment. The concentrations of Emiliania Huxleyi at the beginning of the experiments are all different (Figure 1). If the coulter measurements were done in triplicate then error bars should be given in Figure 1.

P7 L3 we saw - Should be written were seen/observed (preferable to keep in the third person) P8 L14. This chosen value from unpublished data on the Ca content of the isolate is critical to the results of this work. I would like to see not just the average of the measurements but the range Discussion

P12 L23. It is stated that there is close coupling between the specific constants of dissolution and calcification and that there were not seen any inter CO2 treatment differences. However, Figure 3 shows quite distinct differences between the treatments with only in the high CO2 treatment is the system in steady state. However, this does raise a question mark over the methodologies employed to calculate the rates. If the high CO2 treatment was indeed in steady state throughout the experiment then there would not be the changes see in total Ca (Fig 4).

It should be noted that the total Ca particulate standing stock fits very well with the cumulative particulate inorganic carbon estimates shown in Bellerby et al, (same issue). However, I am uncomfortable with the use of different experimental results together in Figure 4. The different treatments were sampled on different days and the devel-

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opments within treatments should be shown separately. Again, error bars should be shown on the data points. Similarly, the SScells plot cannot be assumed to be the same in all treatments as used in Figure 4.

P13 L12. It is suggested here that the range of dissolution is similar in the PeECE experiments to the culture work. On P9 L19 it is written that coccolithophores are not preferentially digested. One would imagine that in a natural system the grazers would then choose an alternative food source if were true.

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