

Interactive comment on “Dissolution of coccolithophorid calcite by microzooplankton and copepod grazing” by A. N. Antia et al.

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Received and published: 12 January 2008

Review of "Dissolution of coccolithophorid calcite by microzooplankton and copepod grazing" by A. N. Antia et al..

This paper deals with the dissolution of coccolithophore calcite ingested by microzooplankton and copepods and its quantification. Microzooplankton grazing on coccolithophores represents an important process in the world's oceans and therewith the authors try to answer an interesting and important question.

Unfortunately, as will be shown in detail below, the authors failed in many instances, to produce a manuscript that can be considered for publication in a scientific journal.

1. Experimental design

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The authors used for their experiments strain CCP 371 of *Emiliana huxleyi* (page 4, line 4). This strain is an open ocean strain from the Sargasso Sea. This strain was used by the authors for batch experiments in brackish water having a salinity of 15 (page 4, line 6). As can be easily calculated from Figure 1 the growth rate in the exponential phase was approx. 0.1 (μ , per day), which is an order of magnitude lower than normally observed under the experimental conditions used. This clearly shows that the culture suffers from salinity stress. It is unclear why the authors used such improper salinities for their culture experiments. By the way, one should never abbreviate a species name. It must read *E. huxleyi* and not *E. hux.* On a minor note, it should read coccolithophores instead of coccolithophorids.

The laboratory feeding experiments were conducted over a 5 to 9 day period (Fig. 1). Such a long time renders it impossible to exclude coprophagy (especially when taking into account that the copepod experiments were conducted using a plankton wheel, which increases the probability of re-ingestion), and it is not known how many times excreted *Emiliana huxleyi* cells were re-ingested. This crucial point is ignored by the authors, who discuss their data as if coprophagy did not play a role in their experiments. On page 10, line 1-5, the dissolution data from the copepod experiment are compared to a model study and it is stated that the experimental data "agree with lower estimates of model studies". Apart from the fact that only one model study is considered, the experimental data do only fit the "constant feeding scenario" of the cited model study (Jansen and Wolf-Gladrow 2001), which does not include coprophagy.

The authors used the Ca/cell value determined from a culture isolate from the PeECE III mesocosm to calculate standing stock of calcite (page 8, line 14). Since it is well known that the Ca/cell values vary in between batches even under similar conditions up to a factor of 10 this method is absolutely unsuited to determine calcite standing stock.

It has to be noted that experimental details are rarely given. If they are given on the other hand, it is not stated how or why this was done e.g. "seawater was buffered to a

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pH value of 8.1-8.2" (page 4, line 9), filters were stored at -20°C (page 5, line 15), filters for Ca measurement were shock frozen in liquid nitrogen and stored at -80°C (page 6, line 13). Freezing of filters for determination of Ca makes absolutely no sense, but rather increases the possibility to lose material from the surface of the filter.

Data treatment / presentation

In the abstract the authors present dissolution rates in %. Obviously rates can not be expressed in % (and they don't mention if they talk about weight or volume percent). Talking about the improper use of units, it has to be noted that it makes no sense to express a standard deviation, as done on page 5 (line 14) in % either. The authors use "sd" for standard deviation without introducing this abbreviation, but on the other hand introduce pointless abbreviations like DAY for day (Table 3) (which strictly speaking is not even an abbreviation). Talking about figure captions, the figure caption for Figure 4 is saying that it is showing "1x, 2x, and 3x", without mentioning what they talk about at all. In the same figure caption the authors state that $SS_{\text{tot}}=SS_{\text{tot}}-SS_{\text{cells}}$ (I think it is not necessary to explain that this makes only sense if $SS_{\text{cells}}=0$, which is not the case). These are only a few examples for the wrong and sloppy style of this manuscript.

The crucial variable of the present manuscript is the percentage of calcite dissolved. The calculation of this variable for the laboratory experiments is described on page 5, line 19-22. Unfortunately, this description is cryptic and cannot possibly be understood by the reader. The same is true for the description of this variable with regard to the mesocosm experiments (page 8, line 20-21): the authors talk about "the coefficients" (which coefficients?), "the experiments" (which experiments?) and "substituting SS-cells" (substituting for what?). For the relevant variables not a single equation is given. In addition to the experimental shortcomings this further decreases the value of the presented results. The interpretation of the SEM data is also not sufficient. On page 7, line 17-18, it is stated that the SEM images show mechanical damage and dissolution of coccoliths. It is not explained how the authors tell mechanical damage from dissolution. A proper explanation is very important, since it is well known that this is a rather

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difficult task.

Discussion

The issue of coccolith dissolution and its quantification during copepod gut passage is difficult to address experimentally. This is presumably the reason why only two experimental studies (Harris 1994, Langer et al. 2007 "Dissolution of *Calcidiscus leptoporus* coccoliths in copepod guts? A morphological study") deal with this important issue. The results of these two studies differ significantly and so do the conclusions drawn by the authors. In this context it is important to note that also the methods used are different. Moreover, the methods do not only differ from one another but also from the method applied in the present study. The same is true for the results. This striking coincidence makes it strictly necessary to discuss the different methods used and the possible impact on the results. The authors do not even mention the methods in the discussion and the paper by Langer et al. (2007) is not cited at all. In view of the very scarce literature on this special issue (the present manuscript being the third one to address it) this is a highly unscientific way of dealing with a problem. While talking about references it can be noted that some references given in the text do not appear in the reference list, e.g. page 9, line 17 and page 13, line 4. Moreover, some important statements lack a reference, e.g. page 3, line 5-7, page 3, line 8, page 3, line 11-14, page 9, line 1-5, page 9, line 7-10. In addition to that, some statements do not fit the papers cited. The most important example is page 3, line 2-4. It is true that Harris (1994) reports significant dissolution, but the data of Harris (1994) do not fit the model calculations by Jansen and Wolf-Gladrow (2001); a fact which is explicitly mentioned by the latter authors. This important fact is ignored by the authors of the present manuscript and it is suggested that both studies (Harris 1994 and Jansen and Wolf-Gladrow 2001) draw the same conclusions.

General

Many sentences in this manuscript are unclear without any clear message at all (and

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are also written in very poor English). I will illustrate this on two examples:

1.) "Since these methods provide a powerful means of estimating the production and loss of any variable that is of autotrophic origin and presuming losses through grazing, we used calcite as the measured variable to estimate its production and dissolution. " (page 6, line 4-7). I. It is not clear what exactly is meant by "any variable of autotrophic origin". II. The authors claim that they measured calcite. They did not present any data in this manuscript of any calcite measurement. I don't even now if they talk about calcite size, weight, colour, concentration or whatever. I can only guess that they talk about the Ca measurement which they used to calculate calcite concentrations. They should be aware of the difference between a calculated and a measured value of something.

2.) "In addition to the many changes in marine food webs and biogeochemical cycles that have been postulated and projected in a changing CO₂ world, it is thus important to understand and account for the large natural background signal of calcite losses in the pelagial against which changes will take place. " (page 14, line 7-10). This is one of the many sentences where the authors obviously tried to make their story sound important by using terms like "changing CO₂ world", without giving any real conclusion or statement. What exactly is "a changing CO₂ world"?

Conclusion

I only gave a few examples, showing where the authors failed to produce a good scientific manuscript. Since the points one could criticise are so numerous, it cannot be expected from a reviewer to waste his time by discussing them all. However, I don't understand why scientists, well known for their good work, submitted a manuscript of such deficient quality (it is difficult to imagine that all authors had a look at the manuscript prior to submission). How can one seriously write about "autotrophic calcite" or the gut of a unicellular organism? I strongly recommend to the authors to withdraw their paper since I don't see any possibility to get this paper into a publishable state.

Best regards,

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