

## ***Interactive comment on “Morphology of *Emiliana huxleyi* coccoliths on the North West European shelf – is there an influence of carbonate chemistry?” by J. R. Young et al.***

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The study: "Morphology of *Emiliana huxleyi* coccoliths on the North West European shelf – is there an influence of carbonate chemistry?" by Jeremy Young and co-workers presents interesting results and conclusions on the environmental control of coccolith size and morphology.

I have two major and some minor suggestions, which may help to further improve the quality of the manuscript.

MAJOR ASPECTS

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1) All bioassays lasted for 96 hours. Carbonate chemistry was manipulated at the beginning and samples for coccolith morphology investigations were taken initially, after 48, and after 96 hours. You found no influence of carbonate chemistry on coccolith morphology. I totally agree with this conclusion for their bioassay E4 where significant increase of *E. huxleyi* cells and coccoliths was observed. I am not sure, however, if this can also be confirmed by bioassays E1 and E5 because (as you say in the discussion on P 4543 L. 15-20) it is unclear how many of the measured coccoliths were actually produced during incubation and how many of them were produced before CO<sub>2</sub> incubations. When I estimate maximum coccolith production rates in E5 (based on values given in the manuscript (0.7  $\mu\text{g C L}^{-1} \text{d}^{-1}$ ,  $\sim 150 \text{ cells mL}^{-1}$ ) and an estimated coccolith weight of 2pg CaCO<sub>3</sub>) results indicate rates of up to 20 coccoliths cell<sup>-1</sup> d<sup>-1</sup>. This is surprisingly high (similar to nutrient replete exponentially growing cultures; Bach et al., 2012) and would mean that there is indeed a high coccolith turnover. I wonder, however, if this is really possible because the amount of coccoliths/mL is so stable throughout the entire experiment (Fig. 3). It would mean that a high production is perfectly balanced by high grazing. This is of course possible but wouldn't it be more likely that there is not much production of coccoliths in the first place? If there was no significant coccolith production then it would be impossible to report CO<sub>2</sub> effects. So I am wondering how reliable your conclusions drawn from bioassay E1 and E5 really are. In order to avoid this problem you could focus on bioassay E4 where it is quite certain that the biggest fraction of measured coccoliths were really produced during the incubation period.

2) You report a negative influence of high CO<sub>2</sub> concentrations on growth rates in bioassay E4. When I look at Fig. 3, however, it seems like cells are not growing under high CO<sub>2</sub> initially (from t<sub>0</sub> to t<sub>48</sub>) but grow equally fast as low CO<sub>2</sub> incubated cells in the second half of the experiment (from t<sub>46</sub> to t<sub>96</sub>). Would it be possible that there was initially an acclimation period in the high CO<sub>2</sub> incubation so that start of the growth was just delayed? And if there wasn't an acclimation phase, why didn't high CO<sub>2</sub> cultures grow from the beginning of the experiment? If there was an acclimation phase, growth

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would actually not be inhibited by high CO<sub>2</sub> concentrations. This would interfere with one of your conclusions (Page 4543 L. 8-11, Page 4544, L. 21-22)

#### MINOR ASPECTS

1) Title: Why do you ask the question: "Is there an influence of carbonate chemistry?" when you can answer the question? Maybe it would be nicer to answer the question in the title already. E.g. "No detectable influence of ocean acidification on morphology of *Emiliana huxleyi* coccoliths on the North-West European shelf. I think it would be also better to call it "ocean acidification" because we have shown that there is an influence of carbonate chemistry on morphology, if conditions are manipulated extremely enough (Bach et al., 2012).

2) Page 4532 L. 7: It may be better not to call it "E4" because the reader does not know what that means.

3) Page 4532 L. 22 and elsewhere: "Calcification" is a vague term. Here you probably mean calcification rates. In other cases (e.g. Page 4533 L. 20) you may mean coccolith size. It would be easier to understand what you mean if you were precise on this.

4) Page 4532 L. 23-26: I do not understand this sentence. How could growth rates obscure these response? Calcification rates are the product of CaCO<sub>3</sub> cell<sup>-1</sup> and growth rates.

5) Page 4533 L. 28-29: What do you mean by "such issues"?

6) Page 4537 L. 18-20: Would you get more useful results if you had normalized number of rays on coccolith size?

7) Page 4538 L. 24: Do you mean x-axis?

8) Page 4542 L. 5-9: I do not really understand why you selected the upper 25%. It would be great if you could explain this in more detail.

9) Page 4542 L. 12: One "E" too much.

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10) Page 4543 L. 23-25: What do you mean by "muted"? By what could it be muted?

11) Page 4545 L. 1-2: What is the difference between the "net effect of ocean acidification" and the "actual effect"?

12) Figure 3: Legend and X-axis label are missing.

13) Figure 8: "Samples" is written twice.

14) Figure 9: I know it could be quite some work but it would look great if you could show individual symbol sizes which are related to the given numbers. That way you would immediately see where you can find large coccoliths.

Best regards, Lennart

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