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Biogeosciences Discussions

Interactive comment on "Influence of changing carbonate chemistry on morphology and weight of coccoliths formed by *Emiliania huxleyi*" *by* L. T. Bach et al.

Anonymous Referee #1

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Review of "Influence of changing carbonate chemistry on morphology and weight of coccoliths formed by Emiliania huxleyi" by Bach et al.

General comments The manuscript by Bach et al. makes a highly interesting contribution to the growing field of Ocean Acidification research in the wider sense. The response of coccolithophores to experimentally induced changes in seawater carbonate chemistry has been a matter of intensive debate for more than a decade. In the last five years or so, the number of publications on that topic has increased tremendously. However, despite extensive research, a number of crucial questions still remain open. Although it is, for instance, well known that the morphogenesis of Emiliania huxleyi coc-

coliths is, at least in some strains and under given experimental conditions, sensitive to carbonate chemistry changes, it is utterly unclear which parameter of the carbonate system causes this sensitivity. This question is of particular importance when the understanding of observed adverse effects is concerned. The data presented by Bach et al. allow the authors to answer that pivotal question. Although there can be no doubt that the dataset is valuable and the conclusion well argued, the presentation of the data falls short of the overall impressive quality of the manuscript. I will specify what I mean by that in "Specific comments".

Specific comments Page 5850, lines 5-6: This is vague and misleading. Vague, because it is unclear what you mean by "understood", and misleading, because the phrase "... how these changes are..." suggests that it is unknown whether morphogenesis is affected at all. But you tackle the more specific, and more challenging, question which parameter affects morphogenesis.

Page 5851, lines 26-27: In your particular strain. With respect to E. huxleyi, this is not the first study to look into this question.

Page 5852, line 13: What was the number of replicates, if any?

Page 5852, lines 16-19: You did not add borate? If there's no borate CO2sys will assume the wrong borate alkalinity. If you did, the TA prior to addition of bicarbonate was not zero.

Page 5852, line 19: 2 ml of NSW per what?

Page 5856, lines 6-7: Why different input parameters? Did the choice of the input parameter affect the calculated CO2?

Page 5858, lines 1-2: This number is useless. Please state how many were analysed per sample.

Page 5858, line 23: Does that equal "per sample"? It is unclear because you do not state whether there were replicates.

Page 5860, line 8: How many liths were analysed per sample?

Page 5863, line 23: What means “more representative”? Give numbers, ie typical sample size used in visual analyses as opposed to your new method.

Page 5863, line 27: Why not possible for type R? Please mention this.

Page 5864, lines 9-10: This statement cannot be made on the basis of Fig. 4 alone. Also Table 1 is not sufficient, because not the complete carbonate chemistry is given. This is crucial. Please provide the complete carbonate chemistry in Table 1. Moreover, Fig. 4 contains error bars; how were they calculated? There is no standard deviation for malformation in Table 1.

Page 5864, line 24: Should read “. . .bound to. . .”

Page 5865, line 2: Replace “factors” by eg “cellular components”.

Page 5865, lines 9-10: The explanation would also be feasible without H+ easily entering the cytosol. See Langer et al. 2006 for a discussion. The study of Suffrian et al. 2011, however, renders this explanation plausible, not merely feasible. Please make this distinction clear.

Page 5865, lines 11-13: Agreed. This was actually hypothesised and argued in detail in Langer et al. 2006. Please cite the paper here.

Page 5865, lines 20-23: This argument was put forth in Langer et al. 2006 for the first time. Please cite the paper here.

Page 5866, line 3: Nor on another C. leptoporus strain.

Page 5866, line 17: Hard to judge because not the complete carbonate chemistry is given in Table 1. See above.

Page 5867, line 6: Replace “co-correlated” by “positively correlated”

Page 5867, line 19: Why nucleation?

Page 5868, lines 5-10: The range in Langer et al. 2006 cannot be narrow and broad at the same time. Please clarify.

Page 5868, lines 10-13: This conclusion is not convincing, because a similar response pattern does not imply a similar coupling. On page 5866, lines 6-8, the authors draw the correct conclusion in a comparably structured argument.

Page 5868, line 17: There are also published data (Langer, G. , Gussone, N. , Nehrke, G. , Riebesell, U. , Eisenhauer, A. and Thoms, S. (2007) Calcium isotope fractionation during coccolith formation in Emiliania huxleyi: Independence of growth and calcification rate, Geochemistry, Geophysics, Geosystems, 8, Q05007). Please cite the paper.

Page 5869, line 9: The term “ecophysiologically” is not ideal, because it usually means “physiologically”. What about “ecologically” or “community”? END OF REVIEW

Interactive comment on Biogeosciences Discuss., 9, 5849, 2012.