Biogeosciences Discuss., 12, C5867–C5869, 2015
www.biogeosciences-discuss.net/12/C5867/2015/

Biogeosciences
Discussions
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Interactive comment on "Vanishing coccolith vital effects with alleviated CO_2 limitation" *by* M. Hermoso et al.

Anonymous Referee #1

Received and published: 29 September 2015

The study: "Vanishing coccolith vital effects with alleviated CO2 limitation" by Hermoso and co-workers contains highly interesting results on growth, and stable carbon/oxygen isotope fractionation in coccoliths of four different coccolithophore species. The laboratory work makes a very good impression although I cannot really comment on the isotope methodology. I have, however, one major and one minor concern with the data interpretation. I will try to explain these concerns in the following.

Major concern: A core parameter in your study is DCUt. To calculate this parameter you assume that "passive influx of CO2 constitutes the only source of carbon to the cell". I have very strong concerns with this assumption (which seems to be central to many of your interpretations and hypothesis) and worry that it is not valid. You underline this assumption with studies by Sekino and Shiraiwa (1994) and Kottmeier et al., (2014).

However, Sekino and Shiraiwa (1994) stated in the abstract that "HCO3- was utilized mainly for production of CaCO3 and accumulation of internal inorganic carbon" which contradicts your assumption. Kottmeier et al. (2014) indeed showed that CO2 is the dominant DIC source under high DIC but this finding is only true for photosynthesis. Kottmeier et al., (2014) did not investigate the carbon source for calcification.

Furthermore, there are a large number of studies with different methodological approaches which have shown that HCO3- is a (or even the) key source ion for photosynthesis (e.g. Rost et al., 2003, 2006; Schulz et al., 2007) and calcification (e.g. Sikes et al., 1980, Nimer et al., 1993, Buitenhuis et al., 1999, Bach et al., 2013).

Please clarify this issue because if this assumption is not true then DCUt cannot be interpreted in the way you do in this paper.

(Please have a special look on lines 22-27 on page 15846, lines 23-29 on page 15849, and lines 17-18 on page 15855.)

Minor concern: DIC concentrations in the highest treatment were $\sim$12000 μmol/kg. When I calculate Omega_calcite for this concentration (assuming pH 8.2 (pH scale missing! See comment 4), S=35 (not given, why?), T=15, K1/K2 by Mehrbach et al.1973 refitted by Dickson and Millero 1987) I get values of 26 (pH on free scale) or even 30 (pH on total scale). At such high Omega_calcite values there is a high potential of inorganic CaCO3 precipitation. Could this interfere with your results? And to some extent explain the absence of vital effects at high DIC? I noticed that you seem to address this issue at the beginning of section 4.1 but I did not understand your argumentation here.

Specific comments:

1) Page 15838 line 13: What do you mean by "primarily CO2"? Changing DIC at pH 8.2 primarily affects HCO3-.

2) Page 15838 line 17: The term "static vs. dynamic" is unclear in this context (at least

for the reader not experienced with isotope geochemistry and vital effects).

3) Page 15840 line 6: Perhaps a question which is a bit difficult to answer but do you expect that there is an effect of N2 purging on cell physiology? I mean, you effectively removed O2 and all other trace gases as well. I wonder if this makes a difference to the cells. (Since your growth rates are fine I don't think it does but I am just wondering.)

4) Page 15840 line 11. Please give the pH scale. This is absolutely essential in carbonate chemistry experiments.

5) Page 15840 lines 14-15. What do you mean by successive alterations of the carbonate chemistry. Please try to be less cryptic.

6) Page 15846 line 25. Bach et al., 2014 does not exist. Do you mean 2013 or 2015?

7) Page 15850 lines 13ff. Langer et al., (2009) only showed this for a much narrower range of carbonate chemistry conditions. I doubt that no changes in PIC/POC would occur in your experiment because your DIC range is huge.

8) Page 15851 line 11ff. More recent results showed that another strain of C. pelagicus changes PIC/POC in response to changing carbonate chemistry (Bach et al., 2015).

9) Page 1854 Lines 18-21. I wonder: Isn't it a bit too optimistic to make this suggestion based on the current evidence?

10) Page 15855 Lines 1f. This would only work if coccolith size is bound to cell size. However, there are also very large species with very small coccoliths (e.g. Pleurochrysis carterae).

I hope my comments help to improve the manuscript further.

Interactive comment on Biogeosciences Discuss., 12, 15835, 2015.