

## ***Interactive comment on “Perturbing phytoplankton: a tale of isotopic fractionation in two coccolithophore species” by R. E. M. Rickaby et al.***

### **Anonymous Referee #2**

Received and published: 18 August 2010

This is an excellent contribution to our understanding of how coccolithophore calcify in response to changing ocean carbonate chemistry. This is now a hot topic since it relates to ocean acidification. A lot of unknowns remain in this young field. What is very interesting and original in this study is that strains from two distinct coccolithophore lineages have been grown in medium with constant pH but increasing DIC and pCO<sub>2</sub>. This study is centered on isotopic fractionation with important results on how coccolithophore utilise bicarbonate ions. This has consequences on subject as estimated past CO<sub>2</sub> from alkenones, coccolith size in fossil records... The results are sound, significant and are worth published in Climate of the Past. The paper is well written, the figures appropriate. I therefore recommend it for strongly publication. However I have

some remarks related to the clarity of the work in very few part of the manuscript :

1- At the end of the introduction I have been frustrated to not have seen the reason why they studied coccolith calcification in changing DIC at constant pH. I understand this is important to navigate through the matrices of factors that can affect cell physiology to understand which aspect of carbon chemistry appears to be so detrimental to the coccolithophores, but it would be interesting to know exactly why. In the abstract we see this sentence to understand different species response to changing pH and DIC but the pH is not changing.

2- Also it would be informative here to tell if the ocean chemistry used in this work can be found in a real ocean (in the past and/or in the future?). If not in the discussion it would be important to mention that the strains survived in a truly artificial conditions.

3- They found the calcification differences between two coccolith strains. They extrapolate those differences at a high taxonomic level saying that Noelarhabdaceae are different from coccolithaceae. Strain-specific differences have been found for the Noelarhabdaceae (e.g. Langer, et al., Strain-specific responses of *Emiliana huxleyi* to changing seawater carbonate chemistry. *Biogeosciences Discussions*, 2009. 6: p. 4361-4383.). They should add a caution note in their discussion, stating that they indeed studied only two strains.

4- I found this section 3.4 very difficult to read by a non specialist. Many parts of the demonstration should be explain in length. For example why is it important to plot epsilon versus  $\mu/\text{CO}_2$  ? The reason of using the growth rate divided by  $\text{CO}_2$  instead of only growth rate should be explain.  $\mu$  should be redefine there (it is only at the beginning of the paper).

5- It is particularly intriguing that *C. braarudii*, being the coccolithophore that should be the most adapted to high DIC as other coccolithaceae of large size since they were thriving in an high DIC world (e.g. Paleogene), are showing malformation at high DIC. Is it not the contrary that was expected ? Why the size of *C. braarudii* diminishes with

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increasing pCO<sub>2</sub> when larger Coccolithus were existing during higher pCO<sub>2</sub> times ? The author say that for this taxa does not respond to changing pH and CO<sub>2</sub> (e.g. p275 line 12 or in the conclusion) which is apparently not demonstrated by their experiment here. Could they clarify that point ?

Also some very minor things : Title : I do not feel that the title is informative, and although funny it is abstruse. P.257 Line 1 - "No two..." strange wording. P.257 Line 8 - G. should be Gephyrocapsa because it is not correct to start a sentence with an abbreviation. This is made several time (P.262 Line 2, P.265 Line 5....).

P.268 Line 1-3 : "But the trend in isotopic values (Fig. 5) under stable growth rates at the lower DIC levels implies that the isotopic shift cannot only be due to decreased growth rate at high DIC." I do not understand the "only". This is not the proof that this "shift cannot" be stricly "due to decrease growth rate".

P269 Line 18 d13Cpocg: delete the g.

P270 Line 7 : Before Pagani et al 2005, at least there is Jasper, J.P., et al., Photosynthetic fractionation of <sup>13</sup>C and concentrations of dissolved CO<sub>2</sub> in the central equatorial Pacific during the last 255,000 years. Paleoceanogr., 1994. 9(6): p. 781-798.

P 271 Line 26 gn -> onBibliographie La bibliographie est vide car le document ne comporte pas de citation. Pour ajouter une citation, choisissez Insérer > Citation EndNote.

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Interactive comment on Clim. Past Discuss., 6, 257, 2010.

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