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Interactive comment on "Biogeochemical implications of comparative growth rates of *Emiliania huxleyi* and *Coccolithus* species" *by* C. J. Daniels et al.

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Anonymous Referee #1

We thank the reviewer for their comments and address them below.

P 10517, L 18: Give actual numbers for cell density rather than just saying "low"

We have now added the range of cell densities observed in the culture experiments to this section of the manuscript.

P 10520, L 16: Define "steady state". I assume it means N = constant.

In the sense that we use the term (and much of the literature), steady state, refers to the period when the 'specific production rates of all cellular constituents are proportional to the rate of cell division' (see Leynaert et al., 2001). Steady state has to be assumed to calculate calcite production from growth rate and cell calcite. This calculation has been used in a large amount of the literature implicitly assuming steady state, whereas we explicitly state it with reference to Leynaert et al. (2001).

P 10521, L 2: Define "dominate". Does it mean > 50 %?

We have now explicitly defined "dominate" as > 50 %.

P 10521, L 10-14: This sentence makes no sense to me if in line 14 it reads "greater than". Should it not be "less than"? This sentence makes for rather difficult reading anyway. Maybe re-phrase?

The reviewer is correct that it should read as "less than". We have now rephrased this sentence to improve clarity.

P 10521, L 20-25: Although the overall purpose of these lines is intuitive, the actual argument is muddled. First, internal consistency between calcite estimates for the three investigated species can as well be achieved by using bulk chemical measurements. Therefore, internal consistency is no justification for using biometric measurements. There is no particular need for such a justification in my opinion, but if the authors feel they need one, they should think of another. Second, what is the "associated error"? I assume it refers to analytical precision. If so, that should be stated explicitly. In any case, please clarify. Third, lines 23 and 24 suggest that differences between studies are due to the "associated error" alone. I'm aware that this is not what is actually said, but I think that most readers will receive that impression. Differences between studies, however, stem not only from measurement errors, but also from real differences, i.e. physiological states of cells. Please make that clear.

We have removed the justification for the choice of method. The associated error does not refer to analytical precision but rather to the inherent errors in the methodologies themselves (Young and Ziveri, 2000; Poulton et al., 2010; Poulton et al., 2011). We have now restructured this paragraph to remove the justification for the choice of method, and now state that the differences between studies is due to both ecophysiological and methodological differences, with appropriate (see above) references.

P 10521, L 28: Please make clear that the standard deviation is taken from Table 1.

We now explicitly state that the standard deviations are from Table 1.

P10523, L 4: Which "2 samples"?

These two samples are indicated in red in Fig. 4 and we have now referenced this figure in this sentence. The raw data containing coccolithophore abundance and sample locations for the in situ samples is also available in the supplementary information.

P10523, L 9: Better " ... in 5 out of 29 samples". Otherwise "5" could mean "a lot" as well as "very few".

We have changed this text as suggested.

P10523, L 14: The parameter discussed here is growth rate, but Fig. 3d displays quota change. Does that make sense?

In this section of the paper we are discussing the relative abundance required such that when *C. pelagicus* has the lower growth rate, it dominates (>50 %) calcite production. Fig. 3d demonstrates the effect of relative growth rates and relative abundance on % calcite production by *C. pelagicus* as discussed in the text.

P 10524, L 15: Data for C. braarudii and C. pelagicus do exist. See Gerecht et al. 2014, Biogeosciences 11, 3531-

We were aware of the Gerecht et al. (2014) paper, although it was only in Biogeo-

sciences Discussions when we submitted the manuscript. We will now include data from Gerecht et al. (2014) concerning cellular quotas, however we do have queries over some of the data in this publication – principally that despite it being well documented that *C. pelagicus* is a smaller species than *C. braarudii*, *C. pelagicus* is reported in Gerecht et al. (2014) to have the higher cellular quota of PIC, POC and PON, with *C. braarudii* having only slightly larger coccospheres and coccoliths than *C. pelagicus*.

P 10524, L 18-24: It would be very interesting to see a comparison of these calculations, with calculations based on experimental data. By the latter I mean PON, POP, and PIC quotas, and cell yield of nutrient limited cells. The data can be found in the cited paper by Langer et al. 2013, and in Gerecht et al. 2014, Biogeosciences 11, 3531-. Such a comparison is interesting because N, P, and C quotas of nutrient limited cells are often different from the respective quotas of nutrient replete cells. So, would using these other, maybe more realistic, data alter the conclusions drawn here?

While we agree that variance in cellular stoichiometry of organic and inorganic components is extremely interesting, the purpose of this section of the manuscript was simply to use the example of a batch culture (i.e. fixed nutrient stock) for a 'thought experiment' to show how this would translate into distinctly different cell and calcite yields between the three species. Even with variability in cellular stoichiometry due to nutrient limitation (with stoichiometry only experienced at the end of the growth phase of the experiment), this would not change our main point – that fixed nutrient stocks result in widely different yields of cells and calcite.

Applying nutrient limited stoichiometry to the next section of the paper (which transfers the 'thought experiment' to the open ocean) would be potentially interesting – however for high cell densities to be experienced in the open-ocean, growth rates need to be optimum and hence we would question whether an actively growing (blooming) community of either *E. huxleyi* or *C. pelagicus* would have nutrient limited stoichiometry (experienced in the stationary/no growth phase of the experiment) rather than the stoi-

chiometry (we estimate from cell size) experienced in the exponential phase of a batch culture. We would also argue that addressing cellular variability in stoichiometry due to resource limitation requires chemostat experiments where growth rates and cellular components are stable (see e.g., Geider et al., 2002; Langer et al., 2013) rather than batch cultures where nutrient conditions experienced by the cells change dramatically with time.

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