

Interactive
Comment

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

Anonymous Referee #1

Received and published: 12 July 2014

The manuscript by Daniels et al. features an interesting and timely research question, an appropriate experimental setup, a sufficient dataset, sound conclusions, and a concise writing style. The relevant literature is, with one exception, cited, although not always discussed in the desirable detail. Consequently, I will only comment on a few things which should be improved. These are: P 10517, L 18: Give actual numbers for cell density rather than just saying “low” P 10520, L 16: Define “steady state”. I assume it means $N = \text{constant}$. P 10521, L 2: Define “dominate”. Does it mean $>50\%$? 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? P 10521, L 20-25: Although the overall purpose of these

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

[Interactive
Comment](#)

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. P 10521, L 28: Please make clear that the standard deviation is taken from Table 1. P10523, L 4: Which “2 samples”? P10523, L 9: Better “. . .in 5 out of 29 samples”. Otherwise “5” could mean “a lot” as well as “very few”. P10523, L 14: The parameter discussed here is growth rate, but Fig. 3d displays quota change. Does that make sense? P 10524, L 15: Data for *C. braarudii* and *C. pelagicus* do exist. See Gerecht et al. 2014, Biogeosciences 11, 3531- 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?

Interactive comment on Biogeosciences Discuss., 11, 10513, 2014.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)