

Interactive
Comment

Interactive comment on “Strain-specific responses of *Emiliana huxleyi* to changing seawater carbonate chemistry” by G. Langer et al.

Anonymous Referee #2

Received and published: 24 June 2009

The manuscript entitled ‘Strain-specific responses of *Emiliana huxleyi* to changing seawater carbonate chemistry’ by Langer et al. touches an interesting aspect of ocean acidification, i.e. the possibility that different strains of the same organism might respond differently to the same changes in seawater chemistry. This could have important implications for future predictions of species performance in a high CO₂ world. In principle I am in favour of publication, however, there are several important issues to be resolved.

General comments and suggestions:

1 Although I did not find a clear notion on the method employed to manipulate the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



carbonate system in the methods section, it seems from the discussion and the data presented in table 2 that alkalinity was the parameter being modified. While this approach should be fine within the presented CO₂ range simulating human induced ocean acidification, I was noticing that measured DIC was not the same in all CO₂ treatments. Manipulation of alkalinity with acid or base in order to achieve different CO₂ levels should leave DIC unchanged. However, in three out of four experiments there is a DIC gradient from higher towards lower values with increasing CO₂. One explanation could be ingasing or outgasing of carbon dioxide at seawater pCO₂ lower or higher than that in air, respectively. It would be important to know whether this was happening during seawater manipulation and preparation prior to incubation or afterwards when filtering, storing and measuring the DIC samples. And secondly, why did this happen in three but not in one of the experiments? In this context it should be mentioned whether the DIC and TA values presented in table 2 are from samples taken prior to incubation or at the end.

One has to be certain that the measured DIC corresponds to that during incubation. If the CO₂ exchange occurred during sampling, storage and measurement, DIC would have been the same in all incubations and calculated carbonate chemistry would differ significantly from presented values. Assuming an initial DIC value of about 2065 $\mu\text{mol kg}^{-1}$ for all incubations (calculated as the mean from the NS10Y incubations where DIC is comparatively similar in all CO₂ treatments), the CO₂ range would be considerably broader in all but this experiment, ranging from about 185 to up to 2000 μatm . The highest values would also correspond to that experiment where the most pronounced effect on growth rate was measured (RCC1256). As data interpretation, comparison and conclusions crucially depend on the actual CO₂ levels, this issue should be resolved.

2 The authors' chose to incubate the four different strains at two different temperatures. Although I do understand the reasoning behind this I would not like to exclude the possibility that this could have caused certain differences between strains. I think the

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

claim that 'the responses between strains that were in a physiologically similar state in relation to their optimum growth rate', should have been tested for. For instance, strain RCC1256 was grown three degrees above maximum annual sea surface temperature at the location of isolation while strain RCC1238 was grown five degrees below that value (table 1). This eight degrees difference from a potential optimum temperature for growth could have significantly influenced the measured responses. *Emiliana huxleyi* growth rate, for instance, has been shown to decrease to about half the maximum value when shifting growth temperatures about eight degrees from optimum conditions (Buitenhuis et al., 2008).

3 The classification of the physiological responses presented in table 4 appears sometimes a bit subjective. For instance, if growth rates are assumed to decrease in RCC1216 which is basically based on a decrease at the highest CO₂ level only, then why is growth rate of RCC1238 classified to increase slightly, if again growth rates at the highest CO₂ level are seemingly lower? There are more examples and it appears to be an intrinsic problem to check for trends in curves with four data points and sometimes relatively small differences, and I do not have a solution either. I recommend interpreting the data with more caution.

4 The two different ways to reference the four strains is sometimes confusing. There should be only one throughout the manuscript.

Specific comments:

1 P. 4363, L.11: I do not see any indication for a CO₂ sensitivity of *Emiliana huxleyi* at low light conditions in Feng et al. 2008 and therefore no contradiction to Zondervan et al. 2002.

2 P. 4363, L.19-24: The use of different strains is probably not the sole explanation for

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

apparently contradictory results between experiments as there are several with different *Emiliana huxleyi* strains but the same physiological responses.

3 P. 4366, last paragr.: I would be more cautious with the classification of three distinct response types of *Emiliana huxleyi* (see comments above).

4 P. 4369, L21-27: In the studies by Brand he found not only differences between coastal and oceanic strains but also differences between spring and fall isolates. So it seems that there is more to the story than just the distinction between coastal and oceanic.

5 P. 4371, L9-10: The effect on atmospheric CO₂ of this potential feedback will be relatively small for the next centuries.

6 P. 4371, L16-22: Whether different responses to elevated CO₂ in terms of calcification of different coccolithophore species will result in dominance shifts crucially depends on the function of calcification. In other words, what are the physiological drawbacks on species fitness when cells are less calcified? An important factor in possible dominance shifts could be different responses in growth and nutrient assimilation rates.

7 P. 4380, 4381, 4382, 4383: The x-axis in all graphics should read μatm .

References

Buitenhuis, E. T., Pangerc, T., Franklin, D. J., Le Quéré, C., and Malin, G.: Growth rates of six coccolithophorid strains as a function of temperature, *Limnol. Oceanogr.*, 55, 1181–1185, 2008.

Interactive comment on Biogeosciences Discuss., 6, 4361, 2009.

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