

## ***Interactive comment on “Population-specific responses in physiological rates of *Emiliana huxleyi* to a broad CO<sub>2</sub> range” by Yong Zhang et al.***

### **Anonymous Referee #1**

Received and published: 28 March 2018

### Summary

Zhang et al. conducted a series of experiments with multiple strains of *Emiliana huxleyi* isolated from 3 different North Atlantic populations. Each strain was incubated under a broad range of pCO<sub>2</sub> concentrations (about 120-2600 $\mu$ atm) but with constant total alkalinity to discern between effects due to changes in the carbonate systems and changes in CO<sub>2</sub> levels. The physiological responses that Zhang et al measured were growth rates, PIC and POC production rates. They conclude that there were differences among strains and among populations but those differences depended on the physiological rate.

General comments

[Printer-friendly version](#)

[Discussion paper](#)



The manuscript is very well written. The ideas, methods and discussion are also clear and well structured, making the manuscript flow very well. This is high quality and thorough work and it deserves to be published. However, my main comment is perhaps related to the novelty of the work and I will make some suggestions as to how this could be addressed. Zhang et al. do a good job citing some of the previous relevant studies but their work would be better served by emphasizing how their work is significantly different and why this is important. We already know from studies like Iglesias-Rodriguez, Bach, Langer, etc., that there are CO<sub>2</sub> effects in coccolithophore's physiological rates and we also know from Langer et al.'s work that these are species-specific and strain-specific responses, so (in my humble opinion) there is not much surprise in finding that there are population-specific differences. Throughout the manuscript the authors hint at the ideas of phenotypic plasticity and environmental variability. This, on the other hand is not so common, and I suggest that the authors elaborate more on this. They already show the pCO<sub>2</sub> and temperature ranges in those 3 sites and it is used to explain the results. Fully accounting for this variability at the original field site is important and they should emphasize that. Acknowledging this variability is usually not done

### Specific comments

While isolating the effect of CO<sub>2</sub> from changes in TA is a great idea, it also poses the question of whether the same experiment should have been repeated letting the TA change with CO<sub>2</sub> concentration. It begs the question of "how would the results look like if TA could change?". After all, this is a more realistic situation and it would contribute to our understanding of E hux responses to a changing World. While I acknowledge that this would be an entire new project, I think it is my role to bring it up. Perhaps acknowledging the caveat would be enough.

I am a bit confused about how the incubations were done (not saying it is wrong) but perhaps a diagram or flow chart would be helpful. I mention this in the technical comments section as well.

[Printer-friendly version](#)[Discussion paper](#)

Also, how realistic are CO<sub>2</sub> levels greater than 1500  $\mu$ atm?

It is very interesting that they found almost no differences in PIC production rates among populations, yet growth and POC production rates did show differences at the population level. Why do you think this is? One factor that the authors mention briefly is temperature, I think that temperature-adaptation and temperature-CO<sub>2</sub> interactions might have a greater role in explaining the differences than what the authors attribute to it. In some ways the 3 populations sit along a gradient of temperature and CO<sub>2</sub> and depending on which physiological rate is studied, one parameter might be more important than the other. Zhang et al do mention that growing certain cultures under suboptimal temperatures may have set that strain or population at a disadvantage from the beginning. Interactions between temperature and CO<sub>2</sub> effects should not be discarded.

Another consideration is that Zhang et al do a great job by showing that there are different ranges of variability in the places where they were isolated from and they use this argument to explain the differences. However, their cultures are maintained at a constant CO<sub>2</sub> concentration (and light pattern and temperature). As the authors suggest in this manuscript, the next generation of experiments should account for variability at its origin and hence variable environmental parameters (within a given range) in experimental designs. Plasticity and adaptation are key parameters to consider in the future.

Finally, Zhang et al found some very interesting results, some of which were not fully explored. For example, the optimum pCO<sub>2</sub> is higher for Bergen than the other 2 regions, but the temperature optimum in Bergen is lower, what are the implications for future projections? Similarly, all strains but one showed that the pCO<sub>2</sub> optimum for POC is greater than the optimum for PIC and growth rates, how do you think this might affect future PIC: POC ratios? What about the sensitivity constant results? OR Bergen populations experiencing the higher CO<sub>2</sub> optimum and smallest variability between strains vs. Canary islands showing lowest optimums but highest variabilities in CO<sub>2</sub>

BGD

Interactive  
comment

[Printer-friendly version](#)

[Discussion paper](#)



optimums. ... These are just some examples of other interesting avenues to explore in the discussion.

Technical comments Line 39: than that of

Line 44-45: carbonate chemistry responses? Should it say instead "responses to changes in carbonate chemistry changes"?

Line 76: I recommend checking this new publication: Krumhardt et al. 2017. Coccolithophore growth and calcification in a changing ocean <https://doi.org/10.1016/j.pocean.2017.10.007>

Line 135: "consecutive incubations" and then in Line 146 "each strain was grown under 11 CO<sub>2</sub> levels. ..." then in line 150 and 158 "at least 7 generations. . .4-7 days depending on CO<sub>2</sub> concentration ...". can you explain the method in more detail, I am bit confused. Perhaps a supplementary diagram or flow chart figure would help.

Line 202: For Eq 4 and 5, you cited Bach et al 2011, but could you please elaborate on this method. Can you also explain the sensitivity constant a bit more?

Line 207: Do these refer to figure S3?

Line 295: "These findings indicate that the Bergen population may be more tolerant . . ." This is a great result! Environmental variability can tell us something about phenotypic plasticity.

Line 323 "likely causes the lower the carbon. . ." consider moving "the"

Line 343: add and "s" to proton

Line 345: consider adding "and" before "corresponding"

Line 352: this conclusion seems to be out of place and not well justified

Lines 334-372: some very interesting ideas here but these paragraphs needs some tightening.

Line 367-369: do you mean "dominated" or "dominating"? not sure I follow this argument.

---

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-47>, 2018.

**BGD**

---

Interactive  
comment

Printer-friendly version

Discussion paper

