

## ***Interactive comment on “From laboratory manipulations to earth system models: predicting pelagic calcification and its consequences” by A. Ridgwell et al.***

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The paper by Ridgwell et al. addresses a widely debated issue in current ocean acidification research. The authors review the “highly incoherent picture of the pH-dependent calcification responses of coccolithophores” and offer a potential solution by introducing an “Eppley curve” for calcification. Overall, the paper is very well written and clearly structured. The review of currently available literature on laboratory and mesocosm manipulation experiments as well as the demonstration that “the differences in carbonate parameters between the different manipulations are not critical.” and that “...a reversed (calcification) response seems unlikely” already justifies publication in BG be-

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cause it represents an important contribution to the current debate. However, there are a few comments and some fundamental issues regarding the development of a “unified ecosystem calcification curve” that need to be addressed:

1) The title is misleading and should be changed: a) the paper does not deal with “pelagic calcification” as such but with one, albeit important, group of pelagic calcifiers, the coccolithophorids. Planktonic foraminifers, pteropods, calcareous dinoflagellates and other groups of pelagic calcifiers are not addressed. Hence the paper needs to address the partitioning of the coccolithophorids in the global pelagic calciumcarbonate production. Although several numbers exist on the total pelagic  $\text{CaCO}_3$  flux or accumulation in the sediments, to my knowledge, there are no firm numbers on the partitioning of the pelagic carbonate production between the different taxa but 50% seems to be a acceptable number for the purpose of the paper. b) Instead of “predicting” I would prefer the more cautious word “projection”. Hence, I suggest to change the title to “From laboratory manipulations to earth system models: projecting coccolithophorid calcification and its consequences”.

2) I have my doubts that the Eppley concept is applicable to “pelagic calcification”. The original Eppley curve is “the envelope of a sufficiently large number of individual growth-temperature response curves” and each of the individual growth-temperature curves has the shape of a (more or less skewed) Gaussian distribution. The shape of these curves is caused by the physiological response of all organisms to temperature, which is basically determined by the combined temperature optima of all the enzymes. By analogy, the authors suggest to replace the growth-temperature response curves by calcification-pH response curves. However, there is no physiological basis that I can think of, that forces calcification-pH response curves to follow a Gaussian distribution. The authors recognize themselves “...that calcification optima, if generally existing, are relatively broad compared to existing growth-temperature relationships in the ocean and may have a highly protracted (or non-existent) “tail”. . . . .”. In fact, a calcification optimum has only been demonstrated for one species (Langer et al.,

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2006). For photosynthetic calcifiers, one could argue, that there is a tradeoff between photosynthesis and calcification. Even though Trimborn et al. (2005) have demonstrated that photosynthesis in *E. hux* does not benefit from calcification, one could still argue (in principle) that, if photosynthesis is CO<sub>2</sub> sensitive, that at higher [CO<sub>2</sub>] (and lower [CO<sub>3</sub><sup>2-</sup>]) photosynthesis is enhanced while calcification is reduced. Vice versa, at lower [CO<sub>2</sub>] (and higher [CO<sub>3</sub><sup>2-</sup>]) photosynthesis would be reduced while calcification might be enhanced. Hence, one could argue that there is an optimum pH at which the organism operates best overall. However, current evidence suggests that most photosynthetic calcifiers do not show a Gaussian calcification-pH response curve. It seems that marine calcifying heterotrophs will simply show an increased (degree or rate of) calcification at higher pH and decreased calcification at lower pH. Hence, the question must be raised how an “Epply envelope” can be generated when the individual calcification-pH response curves are not Gaussian distributed. Is it possible to use something like a “half P<sub>max</sub> value” as in Michaelis-Menten kinetics? For instance, the point of 90% of the calcification maximum (either rate or degree of calcification)

3) Although, I'm confident that the authors are aware of this, they should strongly voice that the Epply concept as such might be useful for a biogeochemical understanding of ocean acidification but in order to understand the impact of ocean acidification on the marine ecosystem, detailed knowledge of species specific responses cannot be replaced by a general calcification response curve.

4) Many readers will not be familiar with the oceanic carbonate chemistry. I think that a short paragraph about the natural and the anthropogenically perturbed ocean carbonate chemistry, their typical drivers and timescales of change might be helpful. This will also allow to put the different experimental manipulations into perspective. For instance, something along the following lines: “On glacial-interglacial timescales (several thousand to 100 of thousands of years), the ocean is titrated by the weathering products of rocks (adding carbon and alkalinity) while the biological pump removes carbon and alkalinity from the surface to the ocean interior. The imbalance between these two

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processes is compensated by a net accumulation or dissolution of sediment on the ocean floor and is reflected by a shoaling or deepening of the lysocline, respectively. Because the DIC content of the oceans is ca. 60 times larger than the CO<sub>2</sub> concentration in the atmosphere is the latter determined by the average dissolved CO<sub>2</sub> concentration in the world ocean surface waters. On the contrary, today, and on timescales of 10 to a few 100 years, is the ocean slave to the rapidly increasing atmospheric CO<sub>2</sub> concentration. Because the ocean circulation is slow, carbonate compensation does not play a role on those timescales and, as a consequence, DIC increases, while alkalinity remains constant. In terms of the two widely applied experimental manipulations, one could argue that adding acid or base is more representative of longer timescale processes in the natural system while bubbling with CO<sub>2</sub> is a closer analog for the Anthropocene.” However, at the same time it should be noted that the differences in final chemistry over a small range of change are little (see under 5).

5) With regard to the different carbonate chemistry manipulation experiments it may be useful to add something along the following lines: “Any set of two carbonate system parameters (e.g. DIC and alkalinity) determines the other four ([CO<sub>2</sub>], [HCO<sub>3</sub><sup>-</sup>], [CO<sub>3</sub><sup>2-</sup>], pH). It turns out that bubbling with CO<sub>2</sub> increases [CO<sub>2</sub>], [HCO<sub>3</sub><sup>-</sup>] and DIC, leaves alkalinity unaffected, and decreases pH and [CO<sub>3</sub><sup>2-</sup>]. Adding acid, decreases alkalinity, pH and [CO<sub>3</sub><sup>2-</sup>] and, increases [CO<sub>2</sub>] and [HCO<sub>3</sub><sup>-</sup>] but leaves DIC constant. As a consequence of both manipulations, the saturation state also decreases. Consequently, in both manipulations [CO<sub>2</sub>] and [HCO<sub>3</sub><sup>-</sup>] increase, while pH, [CO<sub>3</sub><sup>2-</sup>] and the saturation state decrease.” Following this, the authors could then show that, starting with the same batch of seawater and targeting the same final e.g. pH, the final carbonate chemistry parameters are not very much different.

Trimborn, S., Langer, G. and Rost, B., 2007. Effect of varying calcium concentrations and light intensities on calcification and photosynthesis in *Emiliania huxleyi*. *Limnol. Oceanogr.*, 52(5), 2007, 2285–2293

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