Interactive comment on "Overcalcified forms of the coccolithophore *Emiliania huxleyi* in high CO$_2$ waters are not pre-adapted to ocean acidification" *by* Peter von Dassow et al.

Anonymous Referee #2

Received and published: 7 November 2017

The article 'Overcalcified forms of the coccolithophore Emiliania huxleyi in high CO2 waters are not pre-adapted to ocean acidification' by van Dassow et al. is presenting environmental data and its relation to coccolithophorid species composition, in particular Emiliania huxleyi morphology, in the Eastern South Pacific. Furthermore, the theory is tested that over-calcified morphotypes of Emiliania huxleyi, apparently found in high CO2/low pH waters, might be pre-adapted to such conditions in terms of negative effects on cellular calcification rates. The manuscript is well structured and written, and the conclusions backed up by the results. Hence, I support its publications, having only some minor comments and suggestions.

C1

General comments and suggestions:

1) some of the figures, such as Fig. 7 and 8, but also 6, are not essential for the conclusions drawn, thus they could be presented in the supplement.

2) You could consider presenting carbonate chemistry speciation climatologies like the one you have for temperature in Figure 1 (but also see my comment #8 below).

3) Please include carbonate chemistry speciation data from the experiments, potentially in a table (coming to the end of my review I did realise that carbonate chemistry speciation data is indeed included in table 2, but not referenced or discussed). Also, you should report measurement uncertainties for all parameters. Furthermore, did you use certified reference material for the spectrophotometric pH measurements, or how were potential dye impurities quantified?

4) Concerning adjusting pH/pCO2 in the cultures, was the aeration done at the incubation temperature of 15 degrees Celsius? Otherwise, there will be off-sets to target levels.

5) The experimental setup description in the methods section could be more detailed. For instance, it was not clear to me whether there was replication (although I think it is mentioned somewhere in the discussion). Then, it appears that there was a large headspace of about 4 liters in the experimental bottles. That should significantly affect seawater pH/pCO2 over time. Or were the bottles continuously aerated?

6) In Figure 3 there should be no connecting lines for the measurement parameters shown in panel a) and b)

7) The discussion is sometimes difficult to follow when you refer to certain locations, rather than station acronym. So either add these locations to the map or always have a station acronym next to them in the text.

8) What are the implications of your experimental results for observations (and how do they compare to) that coccolith CaCO3 content appears to vary with seawater CaCO3

saturation (e.g. in Beaufort et al. 2011). In this respect, you should also be more precise when you talk about 'levels of calcification' on P2, L32. Along those lines, temperature might be a better candidate (compare Fig. 3a).

5) In the experiments, how do hemocytometer cell counts compare to those done by flowcytometry? And why did you opt to use the former for calculating growth rates rather than the latter?

6) How do PIC based calcification rates and quotas compare to those you could calculate by using the change in alkalinity (potentially corrected for nitrate and phosphate uptake)?

Minor comments and suggestions:

1) P1, L42: The notion that the dissolution of $CaCO_3$ at depth can consume more CO_2 than is released at the surface during production is misleading (and I am also not sure what you are aiming at with this statement). It is only the case if you take into account CO_2 uptake by photosynthesis at the surface. And if you do that, then you should consider CO_2 production through respiration at depth.

2) P3, L30: Replace 'In the 29...' with 'On the 29...'.

3) P5, L25-33: Parts of this could rather go into the discussion.

4) P6, L13: How exactly were POC and TPC measured?

5) P7, L30: What does the notion that 'Emiliania huxleyi abundances correlated with diversity' mean? Also, if that is the case the sentence on P7, L20 should be re-phrased.

6) P8, L11: Looking at Figure 5a, it appears that there was not a statistically significant effect of pCO2 on growth rates in all strains. How does this compare to Table 3?

7) P8, L19: How can POC and PIC quota correlate with strain type in a single strain, i.e. CHC342?

C3

8) P9, L8: How did you distinguish between calcified and naked cells, by flowcytometry?

9) P9, L28: 'exceptionally robust' in which sense?

10) P9, L30: Maybe use the term 'coincides' rather than 'correlates' (see also my general comment #8 above).

11) P10, L 15: How do you explain the differing results found here and in Mueller et al. 2015 regarding the tolerance of over-calcified strains in response to OA?

12) P9, L20: If increased TEP production would be responsible for increased measured cellular POC quotas, one would expect that cell size would not be affected. Do you have evidence for that?

13) P9, L30: Cellular PIC/POC ratios in coccolithophores are not what impacts the biological carbon pump, as most of the POC in the ocean is not produced by diatoms. Re-formulate.

14) P10, L32: Looking at Figure 5d it appears that the decrease in PIC/POC was statistically significant in only one strain. Is that really correct? If so, you should make amendments to the text.

15) P10, L 36: It should read 'quota' not 'quote'.

16) P11, L1: Maybe 'comparison' instead of 'consensus'?

17) P11, L5: do you mean 'percentages'?

18) P11, L8: It should read 'effect'.

19) P11, L15: How could high cell densities and ammonia concentrations explain these differences?

20) P11, L32: 'Resistant' in terms of what?

21) P11, L34: What is your notion, that populations might be already 'near the limit'

C4

based on?

C5