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Interactive comment on "Coccolithophores on the north-west European shelf: calcification rates and environmental controls" *by* A. J. Poulton et al.

Anonymous Referee #1

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The manuscript 'Coccolithophores on the north-west European shelf: calcification rates and environmental controls' presents an extensive data set including in-situ measurements and manipulative incubation experiments. The focus is on the response of coccolithophores to environmental conditions such as nutrient availability and carbonate chemistry speciation. The manuscript is generally well written, the conclusions, however, are vague and interpretation of the manipulative experiments are, in my opinion, problematic (see below).

General comments and suggestions:

1: It seems that most calcification related data, such as coccolith calcite or cell specific

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calcification, is based on the dominant species *Emiliania huxleyi*. Given a contribution of up to 30% by other coccolithophores than *Emiliania huxleyi* (page 2702), what are the associated uncertainties?

2: Using the data on pH from table 1 and of *Emiliania huxleyi* dominance from table 3, I wasn't able to verify 'a significant ($p < 0.01$) inverse correlation between pH_T and *E. huxleyi* dominance' (page 2712). I would suggest to show all statistically significant correlations in a separate graph. In this respect it was confusing that in the results section '*E. huxleyi* abundance was negatively correlated to Ω' while in the discussion 'the only one relationship to a parameter of the carbonate chemistry' was the one mentioned above (pH and *E. huxleyi* dominance).

3: The title suggests that there are new insights into environmental control on coccolithophorid calcification rates/abundances and several significant correlations are presented in the results. In the discussion, however, no clear conclusions are drawn with this respect, although it is highlighted that 'no co-variability of pH/Ω was observed with other growth limiting factors' which 'is key to interpreting coccolithophore ecophysiology in relation to growth-limiting factors and needs to be carefully considered in future studies'. Why not exploring this further in this study?

4: For the incubation experiments I would have liked to see data on initial cell numbers and calcification rates to compare with final ones. Looking at the nutrient data (phosphate and nitrate) it seems that hardly anything was utilized in the high CO_2, in sharp contrast to the ambient treatments, even when nitrate and phosphate were added but also in the controls without additional nutrient addition. The only exception is the high CO_2+NP treatment were there was at least some nitrate consumption although also considerably less than in the ambient+NP treatment. The lack of significant nutrient utilization in all elevated CO_2 treatments is also reflected in the lack of significant chlorophyll buildup during the two days of incubation, again in contrast to the

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ambient ones. Thus, it seems that community calcification rates at elevated CO_2 did not decrease but rather that community calcification rates at ambient CO_2 increased. The communities at elevated CO_2 just did not grow, which is strange. However, this could well be a stress related response to the acid/$NaHCO_3$ addition, leading to an extensive lag phase with no growth. Thus, the the short-term bioassays do not seem suitable to infer physiological responses to ocean acidification. This potential issue should also be considered for the accompanying paper by Richier et al.

Specific comments:

1: P.2687, L.20 Here a correlation is described which is not necessarily the actual cause.

2: P.2688, L.7 There are many coccolithophores significantly larger than 10μm.

3: P.2688, L.18 The author's could also include the Sarsia paper by Egge et al. (1994) for the influence of nutrient availability on coccolithophore blooms. If I remember correctly, this paper identified rather high nitrate to phosphorus conditions favorable for blooms than low nitrate to phosphorous as speculated on the first line of the following page.

4: P.2690, L.1 It should read '$< 2\mu$m'.

5: P.2690, L.5 Which effect?

6: P.2691, L.3 Light profiles were taken for the pre-dawn stations, i.e. during the night?

7: P.2694, L.17 I would suggest to rather cite the original Welschmeyer paper here.

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8: P.2696, L.13 What was the rational to always add silicate together nitrate or phosphorus?

9: P.2703, L.12 A coccolith production rate of eight per hour is clearly too high for *Emiliania huxleyi*.

10: P.2710, L.3 Light availability should also affect primary production. Was this the case here?

11: P.2710, L.8 Why and how did mixed layer irradiance influenced community size and CP, while water column structure had a n influence on cellular calcification? Also, a correlation should not be confused with an cause/effect relationship.

12: P.2712, L.8 Coccolithophores have probably a bigger effect on pH than pH on coccolithophores.

13: P.2715, L.6 and P.2716, L. 22 It seems that there was no negative but rather no response to decreasing pH (see also comment above).

Interactive comment on Biogeosciences Discuss., 11, 2685, 2014.