

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

Anonymous Referee #2

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Review of Poulton et al. 2014

The study presented by Poulton et al. describes in-situ measurements of coccolithophore calcification rates on the North-West European shelf. The data set is huge and accompanied by additional short term incubation experiments addressing the effect of changing carbonate chemistry on coccolithophore physiology (bulk calcification rates). The results are presented in an appropriate manner but the manuscript reads very descriptive, lacking an in-deep discussion and the reader is somehow left alone to extract the essential conclusions of the study. I recommend the data set for publication but, in my opinion, the final and revised manuscript would greatly benefit from a careful consideration of the essential message and findings of this study.

Main comments: 1. One main concern is the calculation of cell specific calcification
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rates (cell-CF) by dividing bulk calcite production (CP) by the coccolithophore cell number which also affects other results (e.g. cellular coccolith production rate) and conclusions. The coccolithophore community was not a pure *E. huxleyi* community, thus other coccolithophore species (e.g. *Coccolithus pelagicus*) with cellular organic carbon and calcite content about 100 times higher than *E. huxleyi* might significantly influence these results. This should be considered and discussed in the manuscript.

2. The manuscript present various light parameters (e.g. K_d , $E_d[MI]$, $E_d[0+]$) in the results section. Maybe I missed it but I did not see their importance in the discussion. Is the extended description of these results really necessary or would it be appropriate to summarize those only in tables? In line with this comment, I suggest to condense the result section to the essential findings and present most of the "hydrography data" in tables and figures.

3. The coccolith calcite content is calculated as a function of coccolith distal shield length (DSL). Is it feasible to assume no change in the shape constant under the various conditions (community composition) measured and tested? Another option would be to discuss changes in DSL rather than coccolith calcite content.

4. Page 2711, Line 18 to Page 2712 Line 2: The difference in the response of open ocean and coastal communities is very interesting and I would recommend to explore these differences further. Additionally, these findings should also be stated in the abstract and conclusion section. In my opinion, these differences are an important contribution and finding of this study.

5. The conclusion section needs a better focus and is too long in its current state. I also recommend to exclude further discussion and references from the conclusion section.

Minor comments: 1. The authors measured "chlorophyll a" as one response variable. However, throughout the manuscript the term "total chlorophyll" is used. This might be confusing and I recommend to stick to "chlorophyll a" or its abbreviated term.

2. The method section dealing with the description of the methods used to determine CT and AT is confusing. I am not able to follow which method and which reference material was used. I am certain that this section can be improved. Additionally, please state precision and accuracy either in percentage or μmol values. I also assume that the precision of the Apollo AS-C3 was better than 99.9% rather than 0.1%.

3. Page 2705, Line 22-25: The sentence reads strange. How can nutrient additions see a drawdown?

4. Page 2709, Line 23-25: What is the reason behind the correlation of cell-CF and mixed layer depth?

5. Page 2710, Line 15-18: I think it is confusing to give a correlation between the coccolith calcite content and the ratio of Si and N. This sounds like high Si concentrations have a significant influence on coccolith calcite content? Does Si concentrations influence biogenic calcification in coccolithophores?

6. Page 2712, Line 4-8: It might help to compare the results to findings from laboratory experiments that investigated the response of coccolithophores to changes in pH in the range from 8.0 to 8.2.

7. Page 2713, Line 4-15: This statement and argumentation is a bit tedious. Certainly, all experiments dealing with ocean acidification have to be interpreted with caution because only "real world ocean acidification" simulates ocean acidification correctly (in a strict sense). However, studies from the past decade have produced a good understanding of the response of coccolithophore physiology to changes in carbonate chemistry. Short as well as long-term experiments indicate in general the same trends in the response of coccolithophore physiology to ocean acidification (see Barcelos e Ramos et al. 2010, Muller et al. 2010, Lohbeck et al. 2012).

Table 4: I recommend to abbreviate standard deviation to sd. The unit for the addition of nutrients needs to be checked in the table caption.

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Figure 2: The caption would benefit from a short explanation why some stations have a and b profiles.

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