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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.

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Anonymous Referee #2

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Response: We would like to thank each of the three reviewers for their comments and have responded to each point raised below.

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 ef-

fect 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.

Response: A lack of clear focus for the abstract and conclusions was a common comment across the three reviews and we have now rewritten these sections to highlight the main findings of the study (i.e., strong in situ relationships to light and not nutrient and carbonate chemistry; strong response to nutrient addition in the shelf E. huxleyi dominated communities but not in the oceanic mixed species community; depression of growth rates in elevated pCO2 treatments in two of the bioassays as well as changes in cellular levels of calcification. We will respond to comments on the interpretation of the manipulation experiments in the relevant sections.

Main comments: 1. One main concern is the calculation of cell specific calcification 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.

Response: The influence of species other than E. huxleyi on cell-CF is something we fully acknowledge and the reason why we present an indication of the relative abundance of E. huxleyi in the in situ stations (Table 3). C. pelagicus was only seen at two stations (C43 and C71), deep in the water column and in abundances <0.2 cells mL-1 (we have now added this information) whereas E. huxleyi was hundreds of times more abundant at these stations. In terms of the bioassays, we did not include information on species composition and have now rectified this to show how bioassay 2B had a

70:30 percentage split of E. huxleyi and G. muellerae while the other two bioassays were dominated by E. huxleyi.

2. The manuscript present various light parameters (e.g. Kd, Ed[Ml], Ed[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.

Response: These parameter are key characteristics of the light field and we have now clearly stated their importance and linked to the results in the discussion. They were mentioned in the discussion previously, but we had dropped the abbreviations to aid the reader. We have now included the abbreviations. The 'hydrographic data' describes the growth environment for the coccolithophore communities in NW European shelf waters and parameters of the coccolithophore dynamics such as cellular calcification are shown in the paper to correlate with elements of the water column structure (mixed layer depth) and irradiance. Therefore, we have not removed this section from the revised manuscript.

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.

Response: Issues around coccolith calcite content and changes in shape constants of coccoliths are now covered in the complementary paper from the NW European Shelf by Young et al. (BGD) and hence we have removed this data and its discussion from the current paper.

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.

Response: In agreement with the reviewer we have now highlighted this pattern in the rewritten abstract, discussion and conclusion. However, with such a small dataset (n = 14) we are limited in doing too much exploration of this issue and would need a better balance of shelf and offshore samples and measurements to focus on this issue.

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.

Response: We have rewritten the conclusion sections and moved the further discussion and references to the discussion.

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.

Response: Actually, we did not measure chlorophyll a as one response variable – we measured size-fractionated chlorophyll-a, so we have three different types of chlorophyll a data: <10 um, >10 um and total. This is why we use the term 'total chlorophyll' rather than chlorophyll a; to specify to the reader which dataset we are discussing and so they do not get confused when we discuss chlorophyll a in a general sense or total chlorophyll in a specific sense.

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 umol values. I also assume that the precision of the Apollo AS-C3 was better than 99.9% rather than 0.1%.

Response: We have reworded this section to assist the reviewer, as well as correcting

the precision and accuracy statements to umol values.

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

Response: We have also rewritten this sentence to clarify our meaning – “In the nutrient manipulated treatments, there was very little drawdown in terms of NOx, PO4 or dSi over the 48h of the incubation, apart from in the ambient +NP treatment, where NOx concentrations were reduced to 1.3 umol N kg-1 and PO4 concentrations to 0.1 umol P kg-1 relative to the additions (Table 5).”.

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

Response: What drives this relationship is an increase in cell-specific calcification with deepening mixed layers (and visa versa). But we believe this is not what the reviewer is questioning and rather than the review is looking for the mechanistic (physiological) reason. It is not that clear cut, most likely it is an element of the irradiance conditions experienced by the cells – deeper mixed layers will experience lower average irradiance conditions and this may then reduce cell-CF.

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 concentration have a significant influence on coccolith calcite content? Does Si concentrations influence biogenic calcification in coccolithophores?

Response: As pointed out by reviewer 1, correlation does not mean causation and hence this is not a direct relationship. Rather it is a function of the environments we have sampled. We have now removed the coccolith calcite content from the manuscript as this is covered in the complementary paper by Young et al. (2014).

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.

Response: We are not sure what the reviewer expects us to learn from such a comparison – laboratory experiments expose single strains to changes in pH over different time-scales, whereas in the natural environment the communities are not composed of single strains (or species) and have often gone through some temporal succession and acclimation. Indeed, most ocean acidification experiments examine differences across more extreme pH gradients than 0.2 units.

7. Page 2713, Line 4-15: This statement and argumentation is a bit tedious. Certainly, all experiments dealing with ocean acidification have to 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 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).

Response: This argument is key to our framing of the field bioassays as examining coccolithophore sensitivity to sharp changes in carbonate chemistry rather than anything else. We also differ in the reviewers assertation that there is a general consensus over the physiological effect of ocean acidification on the (laboratory) physiology of coccolithophores (see Langer et al. 2006, Langer et al. 2009; Langer et al. 2011; Benner et al. 2013). However, our point here is not that there is/or isn’t a consensus on the coccolithophore responses to OA, but rather that our induced rapid changes to carbonate chemistry conditions experienced by natural communities of coccolithophores should not be viewed as directly informative over what will happens to such mixed communities during future ocean acidification.

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.

Response: We have revised it in the new version of the paper.

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

Response: We have now replicated the information from the methods section explaining the repeated sampling of these locations into the figure caption (and Table 1 legend).

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