Biogeosciences Discuss., 11, C1622–C1628, 2014
www.biogeosciences-discuss.net/11/C1622/2014/

Biogeosciences
Discussions
Open Access

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

A. J. Poulton et al.

alex.poulton@noc.ac.uk

Received and published: 15 May 2014

Anonymous Referee #3

Received and published: 8 April 2014

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

The authors of the manuscript entitled "Coccolithophores on the north-west European shelf: calcification rates and environmental controls" address coccolithophore abundance, primary production and calcification rates at several locations on the north-west European shelf, under the less studied non-bloom conditions. In addition to in situ measurements it also presents data from short-term experiments in which the response of

coccolithophores to nutrient availability and carbonate chemistry was evaluated. The text is in general well written, but the main conclusions and the connection between the two approaches should be more clear. I recommend this manuscript for publication in Biogeosciences after minor revisions.

General comments: 1) The manuscript presents an interesting data set and considers a timely subject. The dual approach provides interesting data. However, the story should be more evident and the main results and complementary information from the two approaches made more clear.

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.

2) Detailed information concerning the in situ manipulations is not available (or I did not find it) since Richier et al (2014) is still in preparation (Page 2696, line 19 to 22). If I understood correctly, the sodium bicarbonate and the cloridric acid were added directly into the incubation bottles. The acid addition might have stressed the cells under high CO2 concentrations, prolonging the lag phase and, as a result, masking the results of this short experiment (48h). Authors should elaborate on that. Perhaps the 96h experiments provide information about the duration of the stress-related lag phase.

Response: The complementary paper by Richier et al (2014) is now available for open review and further describes the manipulation of the carbonate chemistry involving the addition of both sodium bicarbonate and hydrochloric acid. A reduced growth rates is the result of the experimental treatment, rather than masking another result (?). We

fully accept that the acid treatment (by which we assume the reviewer means pH drop associated with elevated CO2) caused the results. Furthermore, in order to test the impact of the direct addition of acid in the incubation bottles, a 2-day gradual increase of the pCO2 was also performed (See Richier et al. 2014). Similar results were obtained giving more confidence that the volume of acid added through manipulation had either an insignificant or no influence on the response observed. The decrease in growth rate observed at high pCO2 is most likely due to the carbonate chemistry manipulation and could result in the increase of the lag phase in the experimental bottles. However the direct impact of carbonate chemistry shift on phytoplankton physiology is strongly suggested, as the responses obtained were not homogenous but rather specific. Indeed a size- and taxonomy-related responses to change in pCO2 in both main (Richier et al. 2014) and additional experiment (this paper) were observed.

3) It would be relevant to provide information about initial cell abundances, growth rates and nutrient drawdown for the incubation experiments.

Response: Initial cell abundances are given in Figure 7 (see dashed line), allowing the calculation of growth rates over the experimental period (there is no initial growth rate data). Similarly, the nutrient drawdown can be calculated from the data presented in Table 4.

4) Finally, there is not a lot of information on relative abundances of coccolithophores for the in situ samplings and the experiments. This would be especially relevant to understand whether the coccolithophore starting communities in the short-term experiments influenced the response to the nutrients and carbonate chemistry, and for the determination of cellular calcification rates.

Response: We do include data on the relative abundances of the main coccolithophore species (E. huxleyi, G muellerae) for the in situ sampling. However, we acknowledge that similar data is missing for the bioassays and have now included this in the revised manuscript.

Specific comments:

Introduction Page 2688, line 9 – Add reference to the chosen interval in the statement "nanoflagellate (<10μm) community".

Response: Stated changed to reflect that this is our operational definition in this study.

Page 2688, line 10 – Add the used abbreviation (E. huxleyi) close to Emiliania huxleyi.

Response: E. huxleyi is not an abbreviation, it is the shortened version of the species name Emiliania huxleyi.

Page 2689, lines 3 to 10 – Improve phrasing.

Response: This is difficult to improve as it is not clear what the reviewer finds fault with.

Methods: Page 2691, line 5 – In the text ". . . macronutrients (nitrate + nitrate, phosphate. . ." should read ". . . macronutrients (nitrate + nitrite, phosphate. . .".

Response: Corrected.

Page 2691, line 15 – The labeling of the sites sampled twice, should be made clearer, perhaps add information on the legend of Table 1.

Response: We have now added information on the legend of Table 1 to highlight what subscript a and b refer to in this context.

Page 2694, lines 5 and 14 – Explain the size interval chosen for micro-phytoplankton and nanoplankton, and add reference to support it.

Response: We have now rephrased this so that it is clear that these are our terminology, not literature derived ones.

Page 2694, line 23 - In the text ". . . macronutrients (nitrate + nitrate. . ." should read ". . . macronutrients (nitrate + nitrite. . .".

Response: Corrected.

Results Page 2697, lines 16 to 18 - Improve writing.

Response: This is difficult to improve as it is not clear what the reviewer finds fault with.

Page 2699, line 20 - Information on how the pH values were obtained should be added in the Methods. If calculated, it should read "Surface water pHt values calculated from...".

Response: The Methods clearly state that pHT was calculated from CT, AT, nutrients, temperature, salinity and pressure data using the CO2SYS program (pg 2695, lns 22-26).

Page 2702, line 9 - Gephyrocapsa muellerae is incorrectly written. It should read "G. muellerae" instead of "G. mellerae".

Response: Corrected.

Page 2703, line 5 and 6 - the reasoning behind choosing a 16h period for the coccolith calcite content calculations is not clear.

Response: This is the length of a day in June during the cruise. As this section has been removed (i.e., all coccolith size and coccolith calcite content) we have not revised this.

Discussion Page 2705, line 24 - The table does not show the concentrations in the text. The text should read "... NOx was reduced by 1.3 uM and PO4 by 0.1 uM..".

Response: The table does show the concentrations in the text, and these concentrations are the ones at the end of the incubation.

Page 2710, line 21 - Elaborate on the coccolithophore responses. Potentially, add information about community composition.

Response: The coccolithophore responses are covered in the lines following on from this opening statement (i.e., pg 2710, lns 24-29). We also now discuss the community

composition.

Page 2712, line 4 - Carbonate chemistry parameters vary with each other. Thus, in situ measurements cannot exclude correlations of more than one parameter.

Response: For carbonate chemistry parameters we chose only to examine pH and saturation state, as these are the two parameters of the carbonate chemistry (in field data) which do not (necessarily) correlate with each other and can vary in different directions.

Page 2713, lines 9 to 11 - Elaborate on potential direct effect of the acid addition.

Response: We are unsure what the reviewer means here – the change in pH through the acid addition had a direct effect on the bioassays – this is what we report.

Page 2714, lines 7 to 11 - The sentence is too long.

Response: We have now split the sentence at the comma so it a second sentence starts “Hence,”.

Page 2714, line 19 - The statement would benefit from additional references.

Response: Such as? We are not aware of any other studies of pCO2 effects on coccolithophore blooms in mesocosms that are independent of the mesocosm experiment described in Engel et al. (2005).

Figures The graphs are in general simple and easy to understand. Table 1 could show pCO2, since it is a variable in the bioassays. Moreover, the meaning of repeated locations should be made clear in the corresponding legend. On Table 4 initial nutrient concentrations or nutrient drawdown should be added and meaning of "2B, 4B and 5B" should be introduced in the corresponding legend.

Response: We have now added pCO2 to Table 1 for the in situ sampling sites. Table 4 does include initial nutrient concentrations (i.e. ambient) before nutrient addition, and we have added to the Methods section the meaning of 2B, 4B and 5B.

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