

Interactive comment on “Environmental controls on the *Emiliana huxleyi* calcite mass” by M. T. Horigome et al.

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(Please note: Several people contributed to this comment, including (alphabetically): Chris Daniels, Jason Hopkins, Sarah O’Dea, Nicola Percival, Rosie Sheward, and Helen Smith).

GENERAL COMMENTS The paper by Horigome et al. examines coccoliths from Holocene (~last 12,000 yrs) sediments in the context of present day surface water chemistry in order to test which environmental factors are key to the calcification state of the coccoliths. Although the conclusion that ‘coccolith calcite mass should not be used as a straightforward proxy for the response of coccolithophores to atmospheric CO₂’ is interesting, there are a number of serious issues that should be asked of the data and manuscript beforehand:

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1) What evidence exists that these are Holocene sediments? The assertion that the sediments used in this study are ‘pre-industrial/late Holocene/modern age’ is fundamental to the conclusions and methods applied. However, no age-models or dating of the sediment is presented in the paper, only a reference to a book chapter by one of the co-authors (Baumann et al. 2004). When looking at this book chapter, the methods only state: “All sediments are assumed to be of Holocene age. However, only a few of these surface sediments have been dated, nor is the exact sedimentation rate known at most sites. The ages of surface sediments may vary from decades to several hundreds, or even up to several thousands of years, depending on the local sedimentation rate.” This highlights several issues in terms of the age of the sediments used in the paper by Horigome et al., such as the assumption that the sediments are pre-industrial in origin. How confident can the authors be that these are pre-industrial Holocene sediments? How do they know what proportion of the sediment is post-industrial vs. pre-industrial? This is a key potential weakness of the study, that without being addressed may undermine the entire work – when were these coccoliths deposited? Also, things in the ocean do not sink straight down, there is considerable lateral movement in the upper ocean. Hence, is it suitable to correlate point X on the sea floor with a source region directly above it, especially accounting for the strong oceanic gradients of the study areas – what evidence do the authors have for little or no lateral movement of their coccoliths? Additionally, bioturbation of the surface sediment by burrowing organisms is likely to have mixed the top sediment to varying degrees at different sites. This also affects the age of the coccoliths, much older sediment could potentially be mixed up to the surface.

2) The paper describes changes in coccolith size, not mass. The calcite content of *Emiliana huxleyi* coccoliths is directly proportional to their size (see Young and Ziveri 2000 Deep-Sea Research II). Hence this manuscript shows a clear change in coccolith size rather than (just) mass. For example, Fig. 2b shows the linear relationship between size and mass – it is obvious from this that what the study by Horigome et al. shows is a primary change in coccolith size across biogeographic regions.

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3) Methodology. A recent paper by Bollmann, also in *Biogeoscience Discussions* (10, 11155-11179, see full reference below) now appears to question the underlying methodology used in the study by Horigome et al. – specifically the calibration used and the ease of identifying *E. huxleyi* coccoliths using this method. The authors will need to address the methodological issues raised by Bollmann (2013) to validate their own findings. (Bollmann J (2013) Technical note: Weight approximation of single coccoliths inferred from retardation estimates using a light microscope equipped with a circular polariser – (the CPR Method), *Biogeoscience Discussions* 10, 11155-11179).

4) What about morphotypes? It is well recognised in the literature that several morphotypes of *E. huxleyi* exist, and these appear to have clear biogeographical distributional patterns with variations in coccolith size and calcite content. There is also considerable intra-type variability in coccolith size (within one cultured strain) which has been examined in culture studies (e.g., Paasche 2002 *Phycologia*). The authors extensively list how morphometry (coccolith biometry) has been linked with environmental factors (pg 9297, In 29 – 9298, Ins1 - 3). However, only two of these references include field data and there are several key missing references on morphotypes in this section (e.g., Cubellis et al. 2007 *MEPS*; Cook et al. 2011 *J Phycology*; Poulton et al. 2011 *MEPS*; Smith et al. 2012 *PNAS*; Hendericks et al. 2012 *MEPS*).

pg 9298, In 3-4: “the observed changes in *E. huxleyi* calcite mass distribution could be controlled by ecological preference of the different morphotypes”. Presently, the literature would support this observation, but the authors do not link the changes in coccolith size they detect with the distribution of morphotypes. If the authors have access to the SEM images from the sites (as shown in Fig. 1) why is this not examined in more detail or the study’s findings presented in this context? These patterns potentially explain the variations in coccolith size that they observe. Fig. 1 has images of different morphotypes from the different areas, but there is very little discussed in the paper recognising these differences.

5) Have the oceanographic conditions over the last 12,000 yrs been stable in the study

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area? The authors provide no evidence or literature supporting the stability of temperature, salinity, nitrate and phosphate over the Holocene in the study area. Is it correct to assume that these have been stable in concentration and magnitude over this period? Have the frontal boundaries between the regions studied also stayed the same? As such, is it suitable to correlate annual values of these against an integrated Holocene record of coccolith size? Were the coccoliths deposited at the same rate throughout the Holocene in these areas? Also, why is chlorophyll-a included? Has chlorophyll-a stayed stable for the last 12,000 yrs?

6) Have the statistics been correctly interpreted? The cluster analysis of just coccolith mass (Supplementary Fig. S2) shows a very different pattern to that including both coccolith mass and environmental parameters (Fig. 3a). Fig. 3a shows a strong match to the distribution of water masses through the study area, indicating that the clusters are driven by hydrological factors, and that mass is a minor factor. No-where are the factors driving this clustering examined (e.g., Eigenvectors). The PCAs (Fig. 3c, 3d) show strongly how temperature (and salinity) is negatively correlated with nutrient concentrations (warm waters have low nutrients), but do not “reveal that more than 83% of the *E. huxleyi* calcite mass variance is explained by two factors” (pg 9296, In 21-22). The PCAs show that 83% of the variance between stations can be explained by two factors, which are an amalgamation of temperature-salinity-nutrients-chlorophyll-a-coccolith mass-carbonate ion and pH-pCO₂. The relative degree to which coccolith mass (size) influences the PCAs is not shown.

Furthermore, salinity and nitrate (and chlorophyll-a) appear as strong factors in the PCA, comparable with temperature, phosphate and pH-pCO₂, but are surprisingly overlooked in the discussion. Why are these factors ignored? Where is the data to support the statement “carbonate ion concentration was also correlated with the first PCA factor, although according to our analysis it represents a minor contribution” (pg 9297, In 5-6)? How do the authors get to the conclusion (pg 9298, In 8-9) that “mass distribution is linked to surface water phosphorus and [the] temperature where coccol-

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ithophores calcify”? What happened to salinity, nitrate and chlorophyll-a?

7) Where are the results? Relative to a 2-3 page introduction, a 4 page methods section (including a lengthy discussion of the birefringence method, which is published, and has been applied in numerous studies) and a 3.5 page discussion – why is the results part so short (1.5 pages). There is also no presentation of the raw (pre-statistical) results, apart from the difficult to interpret bubble plots in Fig. 1. Are these mean values?

8) What is the comparison with the Beaufort et al. (2011) paper all about? The authors state (pg 9297, ln 9) that the data compilation of Beaufort et al. (2011) was based on living coccolithophore water samples. However, only ~24% (180 of 735 samples, not the number of coccoliths analysed) of the data in Beaufort et al. (2011) are water column samples, the other 75% are sediment trap material. Also, where did the authors get the Beaufort et al. (2011) data from? Some acknowledgement should be included?

9) What about light? The authors do not include any information on relative light levels in the environments examined. This is despite several studies showing the importance of light on coccolithophore distribution (e.g., Charalampopoulou et al. 2011 MEPS), growth rates and cellular PIC:POC (Muller et al. 2008 L&O), and irradiance also modulates their response to pCO₂ (Zondervan et al. 2007 DSRIII). For a full examination of the environmental factors influencing coccolithophore growth and calcification, all factors should be included.

SPECIFIC COMMENTS

Abstract No quantitative results presented in the abstract, despite the strong conclusions. Where is the environmental data from?

Introduction pg 9286, ln 26 – pg 9287, ln 1: Coccolithophores drive very little of the organic production in phytoplankton communities, and hence their photosynthesis is not the important term driving the PIC:POC ratio – it is their calcification.

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pg 9288, ln 13-16: So diagenesis only occurs below the lysocline? The selection of sites situated above the modern lysocline may reduce the post-depositional effects of diagenesis, but certainly doesn't prevent them. Citing Boeckel et al. (2006) instead of Boeckel and Baumann (2008) would be more appropriate here. It should also be noted, however, that Boeckel et al. (2006) refer to Biscaye et al. (1976) for the position of the lysocline in the South Atlantic. Furthermore, Boeckel et al. (2006) clearly state that 'sediment assemblages preserved in samples from water depths of less than 4000 m might also be affected by dissolution due to change in alkalinity at the sediment-water interface' (section 3.3). SEM images are required to verify sample preservation.

pg 9291-9292, lns 28-1: *Gephyrocapsa oceanica* is not a "well studied common species" in the modern ocean. What is the argument for combining two species with coccoliths of very different masses?

Methods pg 9291, Equation 1 – where does "2275.14" come from? All the other terms are defined but not this one.

pg 9292, ln 24: Why include chlorophyll-a? This is surface chlorophyll-a, so lacks any depth resolution and misses potential deep chlorophyll maxima, which are important in the subtropics.

pg 9293, ln 3-5: What control does chlorophyll-a have over coccolith ecology or calcification?

pg 9293, ln 13-14: How has *E. huxleyi* mass been included in the cluster analysis? As binned data, average values? Was it transformed/standardised?

Table 1: Have all of these been sampled? More than one of these samples has a depth greater than 4400 m, despite the statement on pg 9290, ln 13-14.

Results pg 9294, ln 23-24: How does lower coccolith calcite mass equate with lower abundances of *E. huxleyi*?

pg 9297, ln 2: What is *E. huxleyi* production? Is it Primary production? Calcification?

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Calcite production? Production of cells (i.e., growth)? Coccolith production?

pg 9297, ln 16: What is a 'fairly good correspondence' in the Boeckel and Baumann (2008) reference? Does this cover both subtropical and Antarctic comparisons?

pg 9297, ln 10-11: Where are the statistics to prove that the data "clearly indicates very different trends"?

Discussion pg 9298, ln 24-26: What evidence is there that calcification rate determines thickness and growth rate determines size? It is not mentioned in Muller et al. (2008).

Figures The figures are generally difficult to interpret/see. Fig. 1. Differences in bubble sizes almost impossible to see. Why include the SEM images when morphotypes are not classified or quantified? Technically the 'coccoliths' mentioned are coccospheres. How did the authors quantify a 'typical' coccolith? Fig. 3. Very difficult to see colours and labels. What are the error bars in 3b? How do we interpret Fig. 3c? What are the important things to notice? Why is the cluster plot of coccolith mass (which the paper is all about) relegated to the Supplementary material?

Interactive comment on Biogeosciences Discuss., 10, 9285, 2013.