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Interactive comment on "Changes in coccolith calcification under stable atmospheric CO_2" *by* C. Bauke et al.

Anonymous Referee #2

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The discussion paper by Bauke et al. aims to untangle changes in coccolithophore calcification due to temperature and/or productivity from changes caused by increasing CO2 and carbonate ion concentrations by studying coccolith weights from three Holocene North Atlantic cores. This is an ambitious goal and the results could be a very important step forward in our understanding of coccolithophore ecology and the affect of ocean acidification on coccolithophore calcification. However, I am concerned that the manuscript fails to meet the set goals mainly because of the chosen approach and applied methods. The overall quality of the presented data is poor and thus the resulting interpretations are not well supported by data, are partly contradictory, and include numerous incorrect and biased citations.

My major concerns are the following:

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1. Page 9420, line 16-28: The authors used the SYRACO-System (Beaufort & Dollfus, 2004) to automatically identify coccoliths of the family Noelaerhabdaceae and to quantify the calcite weight of single coccoliths (Beaufort, 2005). The applicability of these methods has been recently questioned by Bollmann (2013, Biogeoscience discussion paper; see also the comment by Bollmann) because of a flawed weight calibration procedure. This issue needs to be addressed! Furthermore, I suggest providing a more detailed method description including how the light conditions were controlled between different samples/runs, which material was used for calibration and how reproducible the calibration is. I also suggest to provide a table with the original data (single measurements) indicating how many measurements where made per sample. Last but not least, I have difficulty believing that samples with 100 Noelaerhabdaceae coccoliths analyses are comparable with samples where 3500 have been measured (see also reviewer 1).

2. My understanding is that one requirement for a reliable automated analysis and recognition of coccoliths is a homogeneous sample preparation with single isolated coccoliths. The manuscript states in the method section that homogenous smear sildes were made (page 9420, line 8-9). However, Henderiks & Törner (2006) and Blaj & Henderiks (2007) already reported that the smear slide method is not suitable for the preparation of homogenous coccolith distribution (e.g., aggregates can not be avoided) and that there is a potential size fractionation. The manuscript needs to address these issues.

3. Coccolith weights of E. huxleyi and Gephyrocapsa are lumped together and only data for coccoliths of the family Noelaerhabdaceae are shown (page 9423, line 20). The motivation as to why these species were lumped together needs to be explained in greater detail. Several studies (e.g. Beaufort et al. 2011; Horigome et al. 2013, Biogeosciences discussion paper) have shown that it is possible to separate different taxa of the family Noelaerhabdaceae from each other using SYRACO (E. huxleyi and G. oceanica). The separation into different genera, species, or morphotypes is a

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CENTRAL POINT for the interpretation of the presented data because the manuscript states in the abstract (page 9416, line 21-23: “we show that weight changes are partly due to variations in the coccolithophore assemblage, but also an effect of a change in calcification and/or morphotype variability within single species.” These statements are not justified as they are not supported by any data. Neither morphotype data are shown for the species records nor coccoliths weight of E. huxleyi and Gephyrocapsa species are presented. I wonder why these data were not collected. I understand that the separation of different morphotypes and species is not possible on the light microcope. However, all assemblage counts were done using scanning electron microscope (SEM) images and simple size measurements of species and morphotypes on SEM images would have provided at least some insights. Page 9426, line 8-9 (Fig. 4): The statement Quote “decreasing coccolith weight trend of Noelaerhabdaceae observed at the Azores is in line with results from previous studies (Beaufort et al. 2011), see Fig. 4” is wrong as the weight trend of the Noelaerhabdaceae appears to be the opposite trend as reported by Beaufort et al. (2011). This might point to the issues raised by Bollmann (2013, Biogeosciences) using the SYRACO-method for weight estimates. Page 9432, line 10-11 (Fig. 7): The statement Quote “The high relative abundance of E. huxleyi coccoliths at Vøring Plateau reveals that the mean weight of Noelaerhabdaceae coccolith at this site is controlled by this species.” is wrong. In the related Figure 7 the major change in relative abundance between G. muellerae and E. huxleyi is not reflected in the coccoliths weight at all. The average coccolith weight (about 7pg) remains constant/unchanged regardless of abundance change of E. huxleyi from about 60% to >90%.

4. Page 9428, line 8-11: The manuscript reports less than 1% of G. muellerae in samples from a core previously studied by Giraudeau et al. (2010). In contrast, Giraudeau et al. (2010) reported high numbers of G. muellerae for the same core/interval/samples. This is an alarming finding and questions light microscope studies of Holocence/Quaternary coccolith assemblages. It has major consequences of the reliability of the conducted work and interpretations of Giraudeau et al. (2010) and the

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presented study. The manuscript needs to discuss and explain this problem in greater detail instead of just mentioning this fact in one simple sentence.

5. Page 9417, line 19-21: Some of the statements and related references are wrong in the presented manuscript (see also reviewer 1). Besides for example the wrong reference of Rickaby et al. (2007) regarding sinking speed of marine aggregates, there are more statements with wrong references. Page 9429, line 18-20: For instance the statement "the distribution of the different Gephyrocapsa morphotypes in the Holocene is longitudinal, the larger type (G. oceanica) occurs in warmer waters (Bollmann, 1997)" is wrong. In fact, Bollmann (1997) shows a latitudinal distribution of different Gephyrocapsa morphotypes and distinguished two morphotypes of G. oceanica, GE and GL, where GE occurs predominantly in equatorial regions (warm) and GL (large morphotype!) in temperate neritic/upwelling regions. Page 9416, line 1-2: The statement Quote "Coccolith calcification is known to respond to ocean acidification in culture experiments as well as in present and past oceans." is misleading because it ignores other important findings. For instance Langer et al. (2011) reported that the morphological response to changes in seawater carbonate chemistry is strain specific and therefore not straight forward. The statement that previous studies focus on changes in coccoliths weight due to increasing CO2 paying little attention to the influence of other environmental factors is incorrect (Page 9416, line 2-5). For example, Beaufort et al. (2008; 2011) discussed coccoliths weight changes beside carbonate ion concentration also in the light of salinity and temperature. Last but not least, the authors cite Raven et al. (2005) for important statements (page 9417, line 7; page 9418, line 23) for ocean acidification and reliable predictions to future coccoliths calcification etc. From my point of view this reference is "secondary" literature. I suggest citing statements and findings of the original studies instead of citing a REPORT for policymakers from the Royal Society (UK). I did not check every reference and statement but I feel it would be worthwhile to do so to improve the overall scientific quality of the manuscript.

6. Page 9416, line 9: Please explain in greater detail what a predominantly stable

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carbonate system means.

7. Page 9421, line 26: The manuscript states how paleo-salinities were calculated in order to compare salinity trends with coccoliths weight. However, a paragraph of salinity and coccoliths weight trends as well as a figure showing the results is missing from the manuscript. This is surprising because the size and thus weight of E. huxleyi coccoliths in plankton and culture studies have been reported to be significantly correlated with salinity (Green et al. 1998; Phycologia; Bollmann et al. 2009, EPSL; Fielding et al. 2009, Limnology & Oceanography).

8. Page 9416, line 10: What is a realistic analysis of changes in major components of Holocene coccolithophores, the family Noelaerhabdaceae? Please explain the statement in greater detail.

9. Figure caption of Figure 2: Please explain the meaning of Noelaerhabdaceae coccoliths within weight bins.

10. Page 9421, line 1: Beaufort et al. 2005 is not listed in the reference list.

In summary, I feel that merging different genera, species, and morphotypes into the family level of Noelaerhabdaceae is a major step backwards in Holocene nannoplankton research. Important ecological and physiological facts regarding E. huxleyi and Gephyrocapsa species are completely ignored and thus the presented interpretations are strongly biased. Therefore, I doubt that it is possible to untangle changes in coccolithophore calcification due to temperature and/or productivity from changes caused by increasing CO2 and carbonate ion concentrations by studying Noelaerhabdaceae coccolith weights based on applied approach.

11. References

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