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***Interactive comment on* “Changes in coccolith calcification under stable atmospheric CO₂” by C. Bauke et al.**

C. Bauke et al.

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Author response: “Changes in coccolith calcification under stable atmospheric CO₂” by C. Bauke et al.

Dear Editor, we have read the two reviews of our paper with great interest and were surprised to find two very contradicting opinions. We are, however, convinced that we can show that the concerns raised by Reviewer #2 are mostly not justified. His main concern is the method of estimating coccolith weight. The reason for this is a discussion paper submitted to BGD after our own paper. Therefore, we were obviously not aware of this paper when we submitted our manuscript. We would like to emphasize, that the method used by us is well-established and has undergone peer-review several times over the last years, including very high-ranked journals such as, e.g. Nature. It seems

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that the opinion of Reviewer #2 is a little bit biased. Still, we have checked the method suggested by Bollmann in BGD, and we can show that the concerns of reviewer #2 are not justified, as the new method proposed by Bollmann in BGD does not affect the quality of our data (see below for details). Reviewer #2 states that “...the results could be a very important step forward in our understanding of coccolithophore ecology and the affect of ocean acidification on coccolithophore calcification.” We agree with this statement, and as we can show that the concerns of Reviewer #2 are not justified, and we are confident, that our paper will make an important contribution.

We appreciate the interactive comments and the suggestions of the two reviewers of this manuscript. The reviews are included below along with responses to each comment.

1) Reviewer #1: Throughout the entire manuscript the term ‘calcification’ is used without clear distinction between calcification rates, the process itself, cellular calcium carbonate quotas and so on. As for instance calcification rate can change without affecting cellular quotas, the authors should be more specific.

Response: In order to clarify, we added a paragraph about calcification to the introduction: “The degree of coccolith calcification can be reflected in the exocytosis rate, in the size of coccoliths and in the weight per coccolith (Bach et al., 2012). Bach et al. (2012) showed a correlation between calcification rates and coccolith weight in *E. huxleyi*, expressed in a simultaneous change in coccolith weight and exocytosis rate and suggested the use of single coccolith weights as a potential indicator for calcification rates.”

2) Reviewer #1: Physiological studies on the effects of changing carbonate chemistry (on the scales of glacial/interglacial variability) suggest that it is rather pH than CO₂ or carbonate ion concentration affecting calcification rates in coccolithophores. The authors and other paleoceanographic studies, however, focus on carbonate ion. This should be discussed in more detail.

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Response: We agree with Reviewer #1 and extended the discussion about the carbonate system in chapter 6.3.3: “Different factors of the oceans’ carbonate system are discussed to affect calcification rates of coccolithophores such as pH levels, carbonate ion or bicarbonate ion concentration (Bach et al., 2012). During the Holocene the atmospheric CO₂ concentration changes only minor and consequently the reconstructed carbonate system of the ocean as well. As we focused more on environmental factors than on the oceans’ carbonate system, we decided to show the parameter of the carbonate system which attracted attention in the study of Beaufort et al. (2011) to compare our results with the large data set of the authors.”

3) Reviewer #1: This and previous studies rely on reconstructions of ancient seawater carbonate chemistry speciation. One of the assumptions (not to mention uncertainties in temperature estimates and reconstructions of alkalinity from proxy-derived salinity) is air/sea gas equilibrium. This is, however, not the case in large parts of the ocean where seasonal seawater CO₂ variability, exceeds atmospheric variability on glacial/interglacial time scales. An exception are the super-oligotrophic regions of the central subtropical gyres, but many sediment cores are outside these areas. This issue should be discussed.

Response: We agree with Reviewer #1 and extended the discussion about the carbonate system in chapter 6.3.3: “Despite the minor changes of the atmospheric CO₂ concentration in the Holocene, the CO₂ solubility in the ocean changes geographically and seasonally due to the dependence on parameters such as SST, salinity, alkalinity and productivity (Feely et al., 2001; Zeebe & Wolf-Gladrow, 2001). Nowadays, the average CO₂ uptake of the ocean is smaller in the Azores region than at the Rockall or Vøring Plateau (see map of Takahashi et al., 2002). Our reconstructions of the Holocene carbonate system show a regional difference, e.g. the decrease of the carbonate ion concentration over the Holocene at the Vøring Plateau is stronger than at the Azores or the Rockall Plateau. The influence of the biological productivity, which strengthens the biological pump and leads to a decrease in CO₂ of the surface ocean

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and thus to changes in the oceans' carbonate system (Sigman and Haug, 2003), are not included in the reconstructions. It is therefore likely that the rising productivity at the Vøring Plateau enhanced the biological pump and led to a decrease in CO₂ of the surface ocean. Possibly, the decrease in carbonate ion concentration is weaker than in our reconstructions. Vice versa, the decreasing productivity at the Azores could lead to a stronger decrease in carbonate ion concentration than reconstructed. But despite these uncertainties, the Holocene changes in the oceans' carbonate system still are much smaller in comparison to e.g. the glacial/interglacial variability of the carbonate system. Further, it should be mentioned that our selected sites are located in areas affected by seasonal variations in e.g. productivity, temperature and pCO₂ (Nilsen, 2003; Rios et al., 2005; Tans and Conway, 2005; Schwab et al., 2012). If coccolithophore weight would be e.g. influenced by a seasonal change in the oceans carbonate chemistry (Smith et al., 2012), which differs from the overall mean trend over the Holocene, these seasonal signals in coccolithophore weight would not be comparable to the Holocene mean trend of the carbonate system. But as we consider multi-annual Noelaerhabdaceae mean weights, seasonal responses are averaged and can be neglected."

4) Reviewer #1: At the end of the discussion a paragraph on the most likely explanations, derived from direct measurements (e.g. species composition or morphotypes) as opposed to speculation, for changes in mean coccolith weight at each site could be presented.

Response: We rewrote the last paragraph of the discussion but could not go into further details, as this would be too speculative.

5) Reviewer #1: P9416, L8: 'increasing pCO₂ and carbonate ion concentration', changes actually go in opposite directions.

Response: We thank Reviewer #1 for spotting that mistake and changed "increasing pCO₂ and carbonate ion concentration" to "increasing pCO₂ and decreasing carbonate

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ion concentration”

6) Reviewer #1: P9416, L11: What is meant with 'realistic analysis'?

Response: We changed the sentence to “For an analysis on changes in major components of Holocene coccolithophores under natural conditions, the family Noelaerhabdaceae was selected, which constitutes the main part of the assemblage in the North Atlantic.”

7) Reviewer #1: P9416, L15: Here and throughout the manuscript, what is meant with 'calcification'? See also general comment #1.

Response: see the answer to comment 1) of Reviewer #1

8) Reviewer #1: P9416, L17-18: Looking at the figures, it is rather 7 to 6 pg at the Azores and 7 to 10 pg at the Vøring Plateau.

Response: We changed the sentence to: “During the Holocene mean weight (and therefore calcification) of Noelaerhabdaceae (*E. huxleyi* and *Gephyrocapsa*) coccoliths decreases at the Azores (Geofar KF 16) from around 7 to 6 pg, but increases at the Rockall Plateau (ODP Site 980) from around 6 to 8 pg and at the Vøring Plateau (MD08-3192) from 7 to 10 pg.”

9) Reviewer #1: P9416, L28: Higher productivity would potentially lead to higher CO₂ disequilibrium between seawater and air, i.e. lower seawater pCO₂. See also general comment #3.

Response: see the answer to comment 3) of Reviewer #1

10) Reviewer #1: P9417, L19-20: A direct connection between sinking speed of marine aggregates and size/weight of coccoliths is not shown in Rickaby et al. (2007).

Response: We changed the sentence to: “Thus, variations in coccolith calcification affect the sinking velocity of the aggregates and consequently the organic carbon export to depth (Beaufort et al., 2007; Ziveri et al., 2007; Zondervan et al., 2011).”

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11) Reviewer #1: P9418, L1: Beaufort et al. (2011) doesn't really confirm the observation that ocean acidification can influence coccolith size and weight as glacial/interglacial changes in carbonate chemistry speciation are much smaller than those expected for future ocean acidification. Furthermore, the current study challenges the conclusions drawn in Beaufort et al. 2011.

Response: We changed the sentence to "A recently conducted study on sediment samples in different ocean basins presented a correlation between decreasing coccolith weight and declining carbonate ion concentration during the CO₂ increase of the last deglaciation (Beaufort et al., 2011)."

12) Reviewer #1: P9418, L3: The term 'driven' is not justified, as it was a correlation, not a cause-effect relationship.

Response: we changed this sentence, see answer to the comment 11) of Reviewer #1.

13) Reviewer #1: P9418, L10: What do the authors mean with 'pH calcification optimum'?

Response: We added the information and changed the sentence to: "As calcification in coccolithophore species and even strains shows an optimum response with respect to various carbonate chemistry parameters (Krug et al., 2011; Bach et al., 2011), these results may indicate that this morphotype has its calcification optimum at a relatively low pH."

14) Reviewer #1: P9418, L17: Nutrient utilization changes have direct influence on seawater carbonate chemistry speciation. See also general comment #3.

Response: see the answer to comment 3) of Reviewer #1

15) Reviewer #1: P9418, L24: I would re-phrase to 'The probably relatively minor changes in... ', but see also general comment #3.

Response: We changed the sentence to "The probably relatively minor changes in

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the carbonate system over the last 10,000 years allow testing the influence of environmental conditions (temperature, coccolithophore productivity) on coccolithophore calcification.“

16) Reviewer #1: P9419, L4: 'Allowing to assess' is a statement a bit too strong.

Response: We changed the sentence to “The diverse trends and gradients of temperature, carbonate ion concentration and coccolithophore productivity in the three studied cores allow to gain insights which of the factors are reflected in coccolith weight and therefore in their calcification.“

17) Reviewer #1: P9420, L26: The potential bias of counting 100 coccoliths in some and 3500 in others cases should be discussed in more detail. Adding detailed information to the graphs (or in a table) on the actual amount of coccoliths counted in each sample could be beneficial.

Response: We agree with Reviewer #1 that a mean coccolith weight composed of 3500 measurements is more accurate than of 100 measurements. But 100 measurements are a high number in comparison to other studies (Bollmann, 2013), further, we show error bars for each sample in Fig. 2 and 5 - 10, so the statistical significance of each data point can be assessed. A table of the single measurements from each sample will be added in the supplementary information. We changed the paragraph to “Depending on coccolith content in the sediment sample and the preparation of the smear slide, roughly between 100 and 3500 Noelaerhabdaceae coccoliths were measured in each sample, in total 89,160 single coccoliths (see supplementary information). Error bars with 95 % confidence intervals are shown in Fig. 2 and 5 - 10 to assess the statistical significance of each data point.“

18) Reviewer #1: P9422, L15-20: Reconstructed changes in sea surface temperature are opposite to what I would have expected. If the Azores Front with warmer waters in the South shifted northwards during the Late Holocene, I would have thought sea surface temperatures to increase at ODP Site 980.

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Response: Geofar KF 16 is located at the outer rim of the subtropical gyre where frontal movements are connected to a change in the faunal composition of foraminiferas and coccolithophores rather than to a change in temperature (Schwab et al., 2012 and references therein; Repschläger, personal communication 2013). Temperature changes occur during more southern positions of the gyre as during Heinrich event 1, the Younger Dryas or the 8.2 event. ODP Site 980 is far off to be influenced by the shift of the subtropical gyre, at this site (as well as at the Vøring Plateau) the decrease in temperature reflects the overall cooling trend of the North Atlantic during the Holocene (Leduc et al., 2010).

19) Reviewer #1: P9424, L14: Change to 'reported'.

Response: We thank Reviewer #1 for spotting that mistake and changed "report" to "reported".

20) Reviewer #1: P9425, L25: Change to '...the reconstructed carbonate ion...'. See also general comment #2 and 3.

Response: We changed the sentence to "Despite the rather stable atmospheric CO₂ conditions, the reconstructed carbonate ion concentration in all studied cores decreases over the Holocene (Fig. 3)."

21) Reviewer #1: P9427, L3: Why should the, probably more obvious, explanation that changes in coccolith weight are the combined result of changes in temperature, nutrient availability, and species and strain composition, rather than relatively small changes in carbonate ion concentrations alone, be restricted to the North Atlantic?

Response: We are cautious with an overgeneralisation of our results since e.g. Beaufort et al. (2011) showed that even within one ocean basin deviations from general responses can be observed (e.g. R-Type). We changed the sentence to "Despite the decreasing trend in all sites, the coccolith weight reacts differently and leads to the assumption that other factors have a stronger influence on mean Noelaerhabdaceae

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coccolith weight in our studied sites of the Holocene North Atlantic.“

22) Reviewer #1: P9430, L9: Change to '... the CO2 solubility in the ocean depends on parameters such as SST and alkalinity.'

Response: We rewrote the sentence to: “Despite the minor changes of the atmospheric CO2 concentration in the Holocene, the CO2 solubility in the ocean changes geographically and seasonally due to the dependence on parameters such as SST, salinity, alkalinity and productivity (Feely et al., 2001; Zeebe & Wolf-Gladrow, 2001).“

23) Reviewer #1: P9430, L11: 'strong decrease' in what?

Response: We changed the paragraph to: “Nowadays, the average CO2 uptake of the ocean is smaller in the Azores region than at the Rockall or Vøring Plateau (see map of Takahashi et al., 2002). Our reconstructions of the Holocene carbonate system show a regional difference, e.g. the decrease of the carbonate ion concentration over the Holocene at the Vøring Plateau is stronger than at the Azores or the Rockall Plateau.“

24) Reviewer #1: P9430, L11: change to '... within rather stable atmospheric CO2...'

Response: we changed this part of the paragraph 6.3.3.

25) Reviewer #1: P9432, L15: In my understanding, the study of Lohbeck et al. (2012) detected changes in strain abundance as the most important adaptation, over new genetic mutations.

Response: We changed the sentence to: “A recently conducted long term study of Lohbeck et al. (2012) revealed an adaptation of *E. huxleyi* calcification to enhanced CO2 due to genotypic selection as well as mutations within the population.“

26) Reviewer #1: P9433, L8: This study not only did not detect changes in coccolith weight, previously ascribed to changes in carbonate ion concentrations, it even found the opposite in two sediment cores.

Response: We changed the sentence to: “Our results show weight changes during the

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Holocene of the same amplitude than previously reported for the CO₂ increase of the last glacial to interglacial change, but with opposing trends in different regions.“

RESPONSES TO COMMENTS MADE BY REVIEWER #2

1) Reviewer #2: 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 were 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).

Response: The concern raised by Reviewer #2, i.e. not citing Bollmann 2013 is unfair and incorrect. Our paper was submitted to Biogeosciences Discussion and put online before Bollmann (2013) was published in Biogeosciences Discussion. Moreover it is difficult to follow the logic of Reviewer #2 to take Bollmann (2013) into account (as well as Horigome et al., 2013, see comment 3) of Reviewer #2), which has not been reviewed yet, while on the other hand he blames us for citing “secondary” literature (see comment 7) of Reviewer 2#). If the citation mentioned would be secondary, would Bollmann 2013 then qualify as “premature”? Yet, we will take the publication into account, since there is no influence on the measurements of Noelaerhabdaceae, which could have easily been detected by Bollmann and Reviewer #2 themselves. As described in the method of Beaufort et al. (2005), the thickness of calcite is proportional to its brightness in a range of 0 to 1.5 μm . Very few extant coccoliths are as thick or thicker

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as 1.5 μm (i.e. *Pontosphaera discopora*, *Coccolithus pelagicus*, *Ceratolithus cristatus*) (Beaufort et al., 2005, and references therein), but not the coccoliths of the family Noelaerhabdaceae we focus on. We use the exact same microscope, and the exact same light settings as Beaufort et al. (2011). Our calibration gives a slightly different factor (0.0016 vs. 0.0013 in Beaufort et al., 2011), which may be due to a different camera or glass slides/cover slips or the smaller size of the calcite powder we used (see below). Still, the method is exactly the same as in previous studies and therefore highly comparable and reproducible. For the calibration slides we used calcite powder in a size range of 1-2 μm to approach our transfer function to the valid range of calcite thickness. We added the information to chapter 2.3.1. Even if the Noelaerhabdaceae mean coccolith weight of our estimations is slightly overestimated, the weight trend over the Holocene would remain constant as there is no mistake within the single coccolith measurements (see above) and therefore our trends are well comparable to other results.

Light control: In order to clarify the method of the light control we extended this paragraph in chapter 2.3.1 to "The light of the Microscope bulb was continuously controlled to avoid a decrease in light intensity over time. Every 10 smear slides, each 200 images of two control-slides were taken and the brightness was checked with the program ImageJ and compared with the previous results. In addition a weekly control with the highest possible light conditions of the microscope was conducted. Both methods revealed brightness changes of less than 2 % and are therefore negligible.

For a statement on the comparability of mean weights composed of 100 versus 3500 coccolith measurements see the answer to comment 17) of Reviewer 1#.

2) Reviewer #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 slides were made (page 9420, line 8-9). However, Henderiks & Törner (2006) and Blaj & Henderiks (2007) already reported that the smear slide method is not suit-

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able 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.

Response: Maybe smear slides are suboptimal, in relation to standard counting procedures, where a much smaller number of fields, and individuals is taken into account, yet the strength of the SYRACO is the much larger number of individuals and area screened. Due to the high number of measured coccoliths a size fractionation is irrelevant as we focus on the mean weight of species and not on the counts or relative distribution of coccoliths within a sample. As mentioned in our text we used SEM counts for a quantitative analysis. Additionally, SYRACO detects and measures coccoliths that coincide with the trained shape of individual species, aggregates or stacked coccoliths are not identified and have therefore no negative influence.

3) Reviewer #2: 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 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 microscope. However, all assemblage counts were done using scanning electron mi-

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croscope (SEM) images and simple size measurements of species and morphotypes on SEM images would have provided at least some insights.

Response: We agree with Reviewer #2 that a separation into different species would give more insights into the weight response of the family Noelaerhabdaceae. As Reviewer #2 already stated is the separation of morphotypes and species difficult and to treat with caution. We partly noticed so-called invaders (coccoliths from a different species invading the output files of other species). This is mostly due to missing bridges in the lightmicroscopic images. These invaders may have different weights than the original species and therefore we decided to assemble all species of this family to avoid mistakes due to misidentification. We checked all output files of the single species to ensure the ascertainment of all Noelaerhabdaceae species. Furthermore we think that the response of a dominant coccolithophore family is highly informative as we include the competition of species and morphotypes of a coccolithophore assemblage in their natural habitat what is rather difficult in laboratory conditions. Regarding for example future changes in the ocean, in the end the overall response of the coccolithophore community to changing environmental conditions is at least as important as the response of a single species. In addition, another advantage is the possibility to compare our results with the large data set of Beaufort et al. (2011), as the authors presented Noelaerhabdaceae mean coccolith weights and used the same calibration method as we did. We disagree with Reviewer #2 that our statement “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.” is not supported by any data because we show single species data from SEM and if changes in the weight are not induced by relative abundance shift of species it is very likely that changes in calcification or morphotype variability change the Noelaerhabdaceae mean weight. As Reviewer #2 already mentioned, it is difficult to separate morphotypes in light microscopy. The SEM counts we used for the relative abundance of the species within Noelaerhabdaceae are from other authors who did not regard morphotypes. As mentioned in our discussion, we counted the uppermost sample of

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the Vøring Plateau under SEM and focused on species as well as on morphotypes.

4) Reviewer #2: 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.

Response: Reviewer #2 obviously did a mistake here, as the trend from core Geofar KF 16 is the same as previously published from a different core from the Azores region by Beaufort et al. (2011). This is clearly shown in Fig. 4. This effect is discussed in detail and has nothing to do with the issued raised by Bollmann.

5) Reviewer #2: 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. muelleriae* and *E. huxleyi* is not reflected in the coccoliths weight at all. The average coccolith weight (about 7 pg) remains constant/unchanged regardless of abundance change of *E. huxleyi* from about 60% to >90%.

Response: In order to clarify we rewrote this paragraph: “At the Vøring Plateau, Noelaerhabdaceae mean coccolith weight is controlled by two species, *E. huxleyi* and *G. muelleriae*. Interestingly, and as previously mentioned, the shift in the relative abundance of the species between 7 - 8 ka BP is not reflected in the mean weight as well as the increase in weight in the late Holocene. This leads to the assumption of changing calcification of the species or abundance shifts of morphotypes.”

6) Reviewer #2: Page 9428, line 8-11: The manuscript reports less than 1% of *G. muelleriae* in samples from a core previously studied by Giraudeau et al. (2010). In contrast, Giraudeau et al. (2010) reported high numbers of *G. muelleriae* for the same

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core/interval/samples. This is an alarming finding and questions light microscope studies of Holocene/Quaternary coccolith assemblages. It has major consequences of the reliability of the conducted work and interpretations of Giraudeau et al. (2010) and the presented study. The manuscript needs to discuss and explain this problem in greater detail instead of just mentioning this fact in one simple sentence.

Response: We studied one sample under SEM to get an indication about the species and morphotype composition at the Vøring Plateau. It might be valuable to count more Holocene sediment samples of this area under SEM in the future, but we think it is rather inappropriate to challenge the work and interpretations of Giraudeau et al. (2010) with a data base of our single measurement. Further Giraudeau et al. (2010) only reports absolute values of *G. muelleriae* and therefore cannot be compared to our data.

7) Reviewer #2: 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 influ-

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ence 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.

Response: We revised the following sentences: Page 9417, line 19-21: see the answer to comment 10) of Reviewer #1 Page 9429, line 18-20: “The distribution of *Gephyrocapsa* species in the Holocene correlates with environmental gradients (Bollmann, 1997), e.g. *G. muellerae*, which occurs in moderate productive cool surface waters (Winter et al., 1994) and is thus often used as a cold water indicator (Weaver and Pujol, 1988), whereas *G. oceanica*, which has an affinity to warm and nutrient rich waters (Kleijne et al., 1989; Giraudeau et al., 1993; Ziveri and Thunell, 2000) and is therefore reported to be abundant in temperate and tropical waters as well as equatorial and upwelling areas (Okada and McIntyre, 1979; Bollmann, 1997; Ziveri et al., 2004).” Page 9416, line 1-2: we agree with Reviewer #2 and added the sentence: “The response, however, is different between species and strains, and for the relatively small changes observed in natural environments a uniform response of the entire coccolithophore community has not been documented so far.” Page 9416, line 2-5: We changed the sentence to “Moreover, previous studies basically focus on changes in coccolith weight due to increasing CO₂ and the resulting changes in the carbonate system and only few studies focus on the influence of other environmental factors.” Page 9417, line 7; page 9418, line 23: We extended the reference but still included Raven et al. (2005) as it one of the most comprehensive reports about ocean acidification.

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8) Reviewer #2: Page 9416, line 9: Please explain in greater detail what a predominantly stable carbonate system means.

Response: We changed the sentence to: “The pre-industrial Holocene with its predominantly stable atmospheric CO₂ provides the conditions for such a comprehensive analysis.”

9) Reviewer #2: 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).

Response: To complete our data we added a small chapter about salinity to the discussion (chapter 6.3.2): “Reconstructed paleo-salinity changes at our coring sites are rather small at the Rockall Plateau (35.9 - 36.2), slightly higher at the Azores (35.9 - 37.4) and show high fluctuations at the Vøring Plateau (34.6 - 38.2). Despite some studies showed correlations between size and weight of *E. huxleyi* in plankton and culture studies with salinity (Bollmann et al., 2009; Fielding et al., 2009), there is no correlation between Noelaerhabdaceae mean coccolith weight in our results and the reconstructed paleo-salinity (not shown).“ However, the study by Bollmann et al. (2009) shows a modest correlation of coccolith diameter to salinity, but besides temperature no other factors have been analyzed, which could have influenced this value as well.

10) Reviewer #2: 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.

Response: See response to comment 6) of Reviewer #1

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11) Reviewer #2: Figure caption of Figure 2: Please explain the meaning of Noelaerhabdaceae coccoliths within weight bins.

Response: we changed the figure caption to: “Figure 2. a) Azores, core Geofar KF 16 b) Rockall Plateau, ODP Site 980 c) Vøring Plateau, core MD08-3192. Abundance of Noelaerhabdaceae coccoliths within weight bins (colours) and Noelaerhabdaceae mean coccolith weight (black line). For the weight bins the data set has been divided into classes (bins) with a step of 0.4 pg, each bin contains the frequency of data points in % of each sample from the Holocene data set. Error bars indicate 95 % confidence intervals of the mean weight. Abbreviations: early Holocene (EH), middle Holocene (MH), late Holocene (LH)“

12) Reviewer #2: 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.

Response: Beaufort et al. (2005) is listed in the reference list! The message of our paper is about the response of a dominant coccolithophore group to environmental conditions in the Holocene North Atlantic. The focus lies on the assemblage, not on single species as these results are as important as the response of single species if not even more important, because assemblage responses represent the total carbonate production from coccolithophores. We do not agree that an assemblage response is a step backwards as it represents responses of a coccolithophore community which is adapted to their habitat and includes shifts in species and morphotype composition (see response to comment 3) of Reviewer #2). Further, we do not ignore facts, we

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focus on facts which are more important and not on factors which do not seem to have an influence on the mean weight of Noelaerhabdaceae coccoliths as e.g. the salinity which has no influence.

REPLIES TO SHORT COMMENTS:

Short comment from M. N. Müller: I would like to draw the author's attention to the following references dealing with coccolith geometry. Considering the findings of these studies might provide additional input for the discussion section of the manuscript. Gibbs, S. J. et al. (2013). Species-specific growth response of coccolithophores to Palaeocene–Eocene environmental change. *Nature Geoscience* 6, 218-222. Müller, MN et al. (2012). Influence of CO₂ and nitrogen limitation on the coccolith volume of *Emiliana huxleyi* (Haptophyta). *Biogeosciences* 9, 4155-4167. Triantaphyllou, M et al. (2010). Seasonal variation in *Emiliana huxleyi* coccolith morphology and calcification in the Aegean Sea (Eastern Mediterranean). *Geobios* 43, 99–110. Henderiks, J (2008). Coccolithophore size rules - Reconstructing ancient cell geometry and cellular calcite quota from fossil coccoliths. *Mar. Micropaleontol.* 67, 143–154.

Response: We thank M. N. Müller for drawing our attention to the references and included Müller et al. (2012) in our discussion.

Short comment from J. Bollmann: 1) From my point of view, the method used by Bauke and colleagues to estimate the weight of coccoliths leads to incorrect weight estimates. In fact, all weight estimates using the method first reported by Beaufort (2005) appear to be incorrect and coccolith weights from different studies can not be compared with each other because of a flawed calibration method. Please find a detailed description of the basic problem in Bollmann (2013).

Response: see the answer to comment 1) of Reviewer 2#.

2) The paper does not provide any information about size calibration, the precision/accuracy of the size measurements and error of the weight estimates.

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Response: We do not discuss size since no size data are presented in our paper. For a discussion about the weight estimates and the accuracy of the weight see the answers to comment 17) of Reviewer #1 and comment 1) of Reviewer 2#.

3) How was the light intensity controlled and measured over time?

Response: see the answer to comment 1) of Reviewer #2.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/10/C4841/2013/bgd-10-C4841-2013-supplement.pdf>

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