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Comment

## ***Interactive comment on “Size-fractionated dissolved primary production and carbohydrate composition of the coccolithophore *Emiliana huxleyi*” by C. Borchard and A. Engel***

**Anonymous Referee #2**

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The manuscript presents particulate and dissolved organic production in *Emiliana huxleyi* chemostat culture with interest on the composition of particulate and dissolved carbohydrates for 4 different size classes. As a general comment, the manuscript is well written and the results present a clear scientific interest, as size fractionation and composition of carbohydrates are relatively weakly studied. However, this manuscript “merit much more attention and revisions of the present version”, as well expressed by the other referee. Indeed, as mentioned by the other referee, while reading the M&M we discover that this data set is part of a bigger data set on which the effect of different pCO<sub>2</sub> treatments has been studied on several parameters (Engel et al., 2014). However, nothing is mentioned on this aspect on the introduction as the authors directly

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assumed that there is no effect. But, the results are interesting for: 1) knowledge's on PP, carbohydrates, size fraction, etc and 2) for the possible modification under future pCO<sub>2</sub> levels. The title should therefore be modified to include the pCO<sub>2</sub> aspect. The no OA effect should be clearly and honestly assumed by the authors (from this manuscript and more widely in scientific community), otherwise it will create a publication bias. Also, why does the 180  $\mu$ atm chemostat not taken in consideration here? The no OA effect could be simply shown with one-two Figure(s) (replace Figure 2, see below) to not be repetitive. Apart this OA aspect, the interpretation of the results is, in my opinion, good and discussion related to heterotrophic compartment is interesting. As mentioned by the other referee, the presence of bacteria in the chemostat should be clearly expressed earlier in the manuscript. While reading the M&M we imagine that axenic conditions were maintained, but we discover in the discussion it was not the case and non-axenic conditions have to be considered. The discussion merit more attention and some paragraphs should be either removed or shorten allowing to include OA section that is requested and also avoid repetition. Author should reconsider some paragraphs of the discussion that are more related to results than discussion. Also, the author start their discussion with results from other studies on plankton communities then come to their results and other results on *E. huxleyi* culture. This should be reconsidered in the next version of the manuscript, because from their chemostats on single strain it is not realistic to compare for example PER obtained at community level. Finally some paragraphs of the discussion do not finish on a clear take home message. What CCHO composition of other NSW bring to the discussion? It merit to be related to the rest of the discussion. More specific comments: We don't need stats everywhere but there are results without stats (e.g., size fractionated DO14C production), is there any reason(s)? Does the ER expressed in % is the same as PER? P15291, l.21: "CO<sub>2</sub>" is not defined before P15292, l. 23: "PP" is used but not defined before but after (same page, l.29). P15299: There is no sentence confirming that the processes and parameters measured presented no temporal dynamic (chemostat culture) and therefore that the values expressed are average  $\pm$  SD of the process or parameter during the X days

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of experiment. Therefore, if I understood, the results presented are the average for the X days of experiment and of the 2 pCO<sub>2</sub> treatments? (except for NSW concentration and composition of carbohydrate in Figure 3). P15299, l.9-10: it is expressed that cell densities of *E. hux.* in cells mL<sup>-1</sup> while in Engel et al. (2014) *E. hux.* cell densities are in cells L<sup>-1</sup>, for clarity it could be standardized? Table 1 is cited but do not correspond to the actual Table 1 of the manuscript, as cell abundance data are not presented. P15299, l.17: I would suggest to express the different pCO<sub>2</sub> treatments different way than “337 ± 94 (350) and 623 ± 139 (750) μatm” and use like in P15300, l. 18: value ± SD (present day) and value ± SD (high CO<sub>2</sub>). P15299, l.22: a sentence confirming that PP (PO14C, DO14C) productions were constant during the time of the sampling in stationary phase would be required (see above also). P15300, l.7: “Table 1” should be “Table 2”? P15300, l. 14: “nutrient seawater (NSW)” should be modified to “natural seawater”? P15300, l. 16: Fig. 3a is cited before Fig. 2 has been cited. P15301, l.12: “significant variation in monomeric” is used but no statistics are provided to confirm the “significant”. P15301, l.17: Fig. 4 is also the average of present and high CO<sub>2</sub>? Could be precised here as “(Table 3 and Figure 3 and 4)”. P15301, l.19 and 20: should be “Fig. 3b, right panel” instead of “left”? P15301, l.20: it should be choose between *Emiliana huxleyi* or *E. huxleyi*; there is a “and” that should be removed. P15302, l.24: a new result is provided and there is no information on how the cell abundance were converted to carbon (conversion factor used?). P15303, l.11: Marañón et al. (2005) have also shown relative constant PER over different ecosystem from eutrophic and oligotrophic with field samples. P15303, l.15: What do you mean by culture? Laboratory cultures? Because I don’t read that Marañón et al. (2005) or López-Sandoval et al. (2010, 2011) or Engel et al. (2013) are related to culture but to natural samples samples in field or in mesocosm conditions. P15304, l.10: what is “LMW-DOC”? (not defined before) P15395, l.9: should be “(Fig. 3 b, left and right panels)”? P15395, l.29: should it be “may be related to physiological and ecological functions”? P15307, l.23: I don’t see that large and small fractions have different contributions. Figure 2: is this Figure 2 the same as Figure 3a (right panel) with the NSW concentration removed

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to have the freshly produced component? This figure is cited in the text but not very used, the values could be provided in the text and the Figure 2 could be replaced by a figure showing that there is no effect of increase pCO<sub>2</sub> on some of the parameters and processes measured. Figure 3: cited before Fig. 2 (see above). The a) and b) should be more visible. Figure 4: the text on the right y-axis should be oriented inside like Figures 1 and 2. All: as mentioned by the other referee for each Figures and Table headings, it should be define tCCHO, pCCHO, . . . as well as results represent average between the two pCO<sub>2</sub> treatments over the experimental period. In the present version Figures and Tables are not self-sufficient (reading text is required). However it might change in the future version and new organisation decided for the results.

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