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Interactive comment on "Size-fractionated dissolved primary production and carbohydrate composition of the coccolithophore *Emiliania huxleyi*" *by* C. Borchard and A. Engel

Anonymous Referee #1

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This BGD paper presents particulate and dissolved primary production, and a detailed description of the neutral sugar composition of 4 different size classes of the extracellular release of E. huxleyi grown in chemostat cultures. The paper is overall well written and the results presented in a clear manner. I would like to highlight two statements that both point to issues that, in my opinion, merit much more attention and revisions of the present version of the manuscript. The first statement, on p. 15295, line 10-11, concerns the two CO2 regimes (380 μatm and 750 μatm) under which the chemostat cultures were run. I was surprised that this important aspect was not considered from the beginning. Due to the lack of differences in the parameters determined, the authors treat the results from the chemostats run under contrasting pCO2 as replicates. I

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strongly suggest to change this presentation of the results. I think the authors miss the opportunity to present their data in an ecologically highly relevant and timely context that is ocean acidification. In their actual presentation the results will not be read and viewed by the large scientific community working on the effect of increased pCO2 on biogeochemical processes. Further, these types of experiments are technically quite challenging to run, and they provide very valuable information. The finding that the processes investigated in the present study are not affected by increased pCO2 is important and it should be shared in that way. I therefore would also suggest to add this aspect in the title, similar to the recent publication by Engel et al. (2014). Presenting the results in this context would further render them original with respect to the many studies performed previously on the same topic. The second statement, on p. 15306, line 5-12, concerns the high bacterial abundance (106 cells ml-1) in the E. huxleyi cultures. Given these high abundances, I wonder how representative the production rates of dissolved organic carbon and neutral sugars are? I assume these values underestimate the actual release rates due to the concurrent uptake by heterotrophs. I strongly suggest the authors describe the non-axenic feature of the chemostat cultures in the first paragraph of the Material & Methods Section (Experimental Setup), so that the reader is aware of this fact for the interpretation of the results that follow. I also suggest the authors discuss their results (eg comparison with other studies and natural seawater) with more focus on the potential role of heterotrophic bacteria on the observed chemical signature.

Specific Comments I suggest the authors provide a rationale for the many different size fractions that were examined. It was not clear to me why this was done. I would have been very interested by the % carbohydrates of DOC. Are DOC concentrations available? p. 15290, line 10-11: How can particulate carbohydrates be part of the dissolved pool of extracellular release? Do the authors mean by particulate material the colloidal fraction of E. huxleyi release products? In that case, I suggest to reformulate this term, because many readers will associate with the term “particulate” the E. huxleyi cells and the associated material, in accordance with particulate primary production. Fig. 3. I

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suggest the authors change the heading “E. huxleyi” to “E.hyxleyi exudate”, otherwise it might be interpreted as the E. huxleyi cellular material. Table 3 and corresponding text (p. 15297). I suggest to explain the abbreviations tCCHO, pCCHO and dCCHO in each of the Table Headings and Figure Legends. Can the authors describe more precisely in the text, what the term pCCHO stands for? p. 15305, lines 15-18: This is an interesting observation. But how much is explained by bacterial heterotrophic activity on the release products? p. 15305, line 24: The authors should precise here that Biersmith and Benner (1998) determined the neutral sugar composition also in E. huxleyi cultures. So, why were the concentrations of Ara so different between the two studies?

Interactive comment on Biogeosciences Discuss., 11, 15289, 2014.