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Interactive comment on "Effect of enhanced pCO_2 levels on the production of DOC and TEP in short-term bioassay experiments" *by* G. A. MacGilchrist et al.

Anonymous Referee #1

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General comments

This paper addresses an interesting and still puzzling issue, i.e. what will be the effect of increasing pCO2 and, thus, ocean acidification (OA), on the production and fate of DOC and TEP, and on potential feedbacks to rising atmospheric pCO2? It has been suggested that OA could install a negative feedback loop on atmospheric pCO2, via the enhanced production of DOC and TEP, which in turn was assumed to increase vertical export of particulate matter (Riebesell et al. 2007, Arrigo 2007). On the opposite, it has been suggested that OA could install a positive feedback loop on atmospheric pCO2 via an enhanced production of TEP and an alteration of their sticking properties (Mari 2008). Such antagonist conclusions were discussed in review papers (Weinbauer et al.

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2011, Passow and Carlson 2012). More recently, Passow (2012) showed that abiotic TEP formation from their dissolved precursors was not affected by OA, but instead was sensitive to changes in total alkalinity. This latter study demonstrated that the use of acid was inappropriate to mimick future pH. In brief, some studies show that OA should enhance TEP production and hypothesized a subsequent enhanced vertical export, other rather suggest a diminution of vertical export while also observing an enhanced TEP production, and finally, the last set of studies show no effect of OA on TEP formation.

The present study thickens the plot as it shows that OA could either enhance or decrease TEP production, while no consistent or strong effect of OA was observed on DOC. Instead of a direct effect of OA on TEP production rate, the present study concludes that increasing pCO2 rather plays indirectly on TEP production depending on environmental parameters (community structure, nutrient availability and stage of phytoplankton growth).

While I agree that environmental parameters may be the cause of the observed results (this assumption is discussed convincingly), in my opinion, it omits one scenario that could explain not only the observed highly variable effect of pCO2 on the production of TEP, but also the variability between the studies conducted so far. It has been showed that TEP have a density much lower than that of seawater (Azetsu-Scott and Passow 2004), and that this low density tended to bring them at the surface and to fuel the surface microlayer (Azetsu-Scott and Niven 2005, Wurl and Holmes 2008, Wurl et al. 2009). If such an ascending flux of TEP occurred in the incubation bottles during the present experiments (actually, there is no reason why TEP should not also ascend during these bioassay experiments), one could expect a vertical heterogeneous distribution of TEP in the bottles and an accumulation at the surface. This heterogeneity of distribution may cause a high level of heterogeneity in the measured TEP concentration depending on the sampling procedure and replicability. It is to be noted that DOC concentration should not be influenced by the same density-driven mechanism.

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Unfortunately, the Material & Methods section (presented in Richier et al. same issue) does not give any details on the sampling procedure, and thus it is not possible to evaluate this potential source of variability in TEP concentration. Therefore, I suggest providing details about the sampling procedure inside the bottles (e.g. sampling depth, sampling equipment, volume sampled) and incubations method (agitation or not), and discuss the possibility of an ascending flux of TEP, likely to introduce a certain level of heterogeneity in the vertical repartition of TEP and subsequently some uncertainty in the measurement of TEP concentration. Until this ascending mechanism is well taken into consideration, any attempt to decipher the effect of OA on TEP production and vertical flux may continue to generate contradictory and puzzling results.

Specific comments

- If the TEP concentration measured in the bottles is converted in terms of TEP-C concentration using the conversion factor (i.e. TEP-C = 0.75 TEPcolor; μg C L-1) provided by Engel and Passow (2001), for the measured range of TEP concentration (Fig. 3b), the TEP-C concentration should range between about 5 to 12 μM. This range for TEP-C concentration is about the same as the measured total POC, i.e. from 8 to 20 μM (Richier et al. same issue). While the TEP-C concentration is only an indirect estimation, I think TEP-C and POC should be compared in the light of the respective sampling procedure for TEP and POC, as it may help understand the fate of TEP in the bottles, and probable different repartition patterns of POC and TEP in the bottles.

- Fig. 2c, experiment E2: The results of the chlorophyll a concentrations in the small size fraction are different from those presented for the same experiment in Richier et al. (same issue).

- Fig. 2 and 3: insert time scale on the x-axis.

- I suggest adding Mari et al. (2001) in the carbon overconsumption citation lists (line 469) as they first described the role of TEP production in the carbon overconsumption process.

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Technical corrections

Reference list has been checked. No error detected.

References cited

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Interactive comment on Biogeosciences Discuss., 11, 3701, 2014.