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Biogeosciences Discussions

Interactive comment on "Effects of ultraviolet B radiation on (not so) transparent exopolymer particles" *by* E. Ortega-Retuerta et al.

Anonymous Referee #1

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Ortega-Retuerta et al. present data of the decomposition and production of transparent exopolymer particles (TEP) in seawater through the exposure of visible and UVB radiation. The paper address a relevant scientific question as previous studies have shown that TEPs plays an important role in marine biogeochemical cycles, including oceanic CO2 uptake and sequestration. A similar study has been conducted earlier by Orellana and Verdugo (2003) as cited by the authors.

Material and Methods

The design of the lab experiment could have been improved as TEPs have been exposed to radiation for 18 hours. The period of exposure exceeds a day cycle which limits the natural exposure of TEP on the ocean's surface, e.g. about 12 hours. Through this overexposure, I expect that the rates of decomposition are overestimated. Addi-

tionally the PAR was 66 times lower in the lab experiments compared to the outdoor experiment. It needs to be explained, why such much lower PAR has been applied for the lab experiments.

Furthermore, the design of the experiments limits conclusions about decomposition/production rates of TEP within the first 10 cm of the ocean's surface through the direct exposure of the glass bottles. A better approach would have been to do an incubation experiment similar to the approach measuring primary production rates, either incubation in the ocean at different depth's or in an incubation tank with bottles covered with light-shaded foils to simulate light exposure at different depths.

Further problems with the experimental design:

P7/L13 I would expect that material with a surface-active character stick to the bottle wall during the incubation. Surprisingly, the authors claim that they did not find significant differences in the concentration of the model substances before and after vigorous shaking. I need to question the author's interpretation of the data. First of all, how do the authors perform statistical testing on significance (see P13/L21) in duplicate samples (P13/L15)? Such tests require at least triplicate samples to calculate standard deviation, and therefore the data interpretation is incorrect and no conclusion can be drawn from this tests. Secondly, according to Fig. 1c, the bottles used for the dark experiments contained more of the model substances than the initial concentrations. How can this be? Pls explain. The authors claim that the concentrations of model substances were not significant different after incubation, but for the alginic acid in the dark exp. it seems the concentration were significant higher than initially (I estimate that by looking at the standard error). Again, the authors need to explain how a standard error is calculated on duplicate samples (see Figure 1 C). Thirdly, the concentration levels chosen for this test are much higher than typical concentration of surface-active substances in natural seawater. Consequently, significant losses through adsorption on the glass wall, e.g. binding to active sites in the glass, at natural concentration levels may be masked through the high concentrations used in the test. Last but not least, the

concentration of alginic acid after shaking is about half (estimated through Fig 1c) of the concentration measured without shaking. It is not clear to me why losses through shaking can occur? Unless concentrations have been measured in filtered samples after separation of TEP, but it seems that was not the approach.

Unfortunately, test on losses through adsorption on glass wall is important to interpret the further results, and I consider the resulting uncertainties as rather high.

Chemical and Biological Analyses

P8/L12 Samples for dissolved mono- and polysaccharide have been filtered through a GF/F filter. GF/F filter do not have a well-defined pore size as filter membranes (such as polycarbonate membranes). I am concern with the use of GF/F filter in this study, as TEP may not retain on this filter, and be part of the filtrate and therefore contribute to the mono- and polysaccharide concentration. Consequently, the concentrations of TEP pre-cursor material may be overestimated.

Results

Through the lack of the estimation of adsorption of surface-active substances on glass walls, the interpretation of the results is challenging. Before publication, I strongly suggest to repeat the test on glass wall adsorption.

P9/L19 I disagree as according to Fig.2, the TEP concentration in Exp.1 with +UVB increased from a incubation time of 1.5 days to 3 days.

P9/L21 The sentence does not make sense as it says that by excluding UVB TEP decreased at lower rates in Exp. 2 but no significant changes were observed in Exp. 2 and 3. Pls clarify.

Discussion

P12/L9 “We obtained an average TEP photolysis rate of 31% d , which would yield complete TEP photolysis in ca. 3 days under UVB radiation. However, TEP photolysis

rates were even faster, at least 69–71% per day in 2 of our experiments, leading to a complete loss of TEP in around 35 h or less. “

This statement is misleading as in natural seawater decomposition and production of TEP may occur under different conditions. For example, TEP is produced at depths without significant exposure to radiation, and ascend to surface through positive buoyancy or adsorption on bubbles. That creates a upward flux of replenishing TEP reservoir in surface water, which likely exceeds decomposition as TEP has been found to accumulate in microlayers (as stated in the introduction of this paper). So I feel above statement is misleading and requires further discussion of TEP cycling.

P12/L18 “Consequently, our results indicate that photolysis may at times be a potentially signiïňĄcant loss process of TEP, which should be included in future TEP budgets and carbon cycling scenarios.”

That is indeed true and important for a better understanding of carbon cycling. However the application of the presented data is limited to the upper 10 cm water layer (estimated through bottle diameter). Incubation at different depth/using light-shaded foil would have been useful to obtain similar data for the depths at which penetration of radiation is still significant.

P13/l18 “In the experiment testing TEP formation in the presence of microorganisms, TEP increased greatly under UVB radiation suggesting that UV promotes the production of TEP by organisms.”

Further discussion on TEP accumulation in the microlayer seems to be important here, as microlayer are often enriched in bacteria abundances. See also (Cuncliffe & Murrell, ISML Journal, doi:10.1038/ismej.2009.69)

In the present form, I can not recommend this paper for publication, but would like to encourage the author to re-checked the test on wall adsorption, clarify and if necessary to re-do the test with more replicates. I am open for further discussions and clarification

on how the test was performed. Getting further data on different exposure level (to simulate radiation levels at different water depth) would improve the relevance of the study.

Interactive comment on Biogeosciences Discuss., 6, 7599, 2009.