Biogeosciences Discuss., 9, C2521–C2523, 2012
www.biogeosciences-discuss.net/9/C2521/2012/

Biogeosciences Discussions

Interactive comment on "Technical Note: Enhanced reactivity of nitrogenous organohalogen formation from plant litter to bacteria" *by* J. J. Wang et al.

Anonymous Referee #1

Received and published: 17 July 2012

Review of "Enhanced reactivity of nitrogenous organohalogen formation from plant litter to bacteria" by J.-J. Wang, T.W. Ng, Q. Zhang, X.-B. Yang, R.A. Dahlgren, A.T. Chow, and P.K. Wong

General Comments

The paper by Wang et al. deals with natural halogenation processes of bacteria and the release of C1/C2 organohalogens. The authors suggest that bacteria may constitute an important precursor material for naturally produced organohalogens. In this review I will not comment on the quality of the bacterial culture experiments because my knowledge in this field is rather limited. I will comment on the paper from an en-

vironmental chemist's perspective. The study is of interest because it adds information about possible new pathways of organohalogen formation in the environment. I found the manuscript for most parts to be well structured and clearly presented. In particular the introduction adequately details the current state of our knowledge about organohalogen formation in soils. Figure 4 shows a conceptual model that nicely summarizes the results of this work. Furthermore, the study by Wang et al. raises some interesting questions which certainly need to be answered in the future. I support publication of the manuscript, however, I have several comments which I hope the authors might consider in their revised manuscript.

Below, in no particular importance order, are my specific comments:

Specific comments

Page 6780, line 18, Introduction: The applied concentration of 50 mmol is very high. Please explain and justify why 50 mmol per liter NaOCl solution was used for all experiments. Why were experiments not conducted at different concentrations of NaOCl?

Page 6781, line 24/25, Material and methods: "EPA Method 551 was adopted for the organohalogen quantification." A short description of the method should be added. Please give also details of the analytical system that was used for separation and analysis of the organohalogens. How were the different organohalogen compounds identified? Only using GC-ECD? Did the authors confirm the eluted compounds using a dissimilar column or by the use of GC-MS. Did the authors also search for formation of polar organohalogens such as halogenated acetic acids?

Page 6784, line 18: . . .contributed to global C1/C2 organohalogen budget. . . Please be more specific and refer to the compounds that were actually measured in this study. These results might be not applicable to other C1/C2 compounds such as monohalogenated alkanes.

Page 6785, line 2, suggest to modify sentence ". . .and the presence of different

halides. . .” into “. . .and the presence of halide ions at different concentrations. . .”. I would like to add that the pH value might be also a crucial parameter affecting formation of organohalogen formation from bacteria. The authors should mention the potential role of the pH value in the Result and discussion section.

Page 6789: Figure 1. No error bars are shown for the lower panel (b). Why? Give the number of replicates for results shown in (b) in the legend.

Technical corrections:

Page 6781, line 10: “bromide’s” not “briomide’s

Page 6784, line 21: “caused” not “casued”

Interactive comment on Biogeosciences Discuss., 9, 6777, 2012.