Biogeosciences Discuss., 8, C3485–C3488, 2011
www.biogeosciences-discuss.net/8/C3485/2011/

Biogeosciences Discussions

Interactive comment on "Organohalogen emission from saline environments – spatial extrapolation using remote sensing as most promising tool" *by* K. Kotte et al.

Anonymous Referee #1

Received and published: 6 October 2011

General comments: The manuscript by Kotte et al. concerns the growing research field of emission of natural organohalogens (VOX) to the atmosphere, which is an interesting subject clearly within the scope of Biogeosciences. The manuscript is (despite some grammatical errors) well written and novel in the sense that not much has been published on VOX emission from terrestrial saline environments.

The manuscript provides some data that clearly shows that the investigated areas may release significant amounts of organohalogens, although the data appear rather preliminary. The largest focus in the manuscript is on remote sensing as a tool to predict landscape changes and the idea is then that these changes may be used to predict VOX

emissions within the areas. The mix of presenting novel laboratory data and reflecting on tools for quantifying landscape changes and hence changes in VOX emissions appears at first interesting to the reader. There is a great risk, though, of confusion and wrong conclusions in linking very small-scale lab measurements and very large scale landscape data. I think the authors are aware of this since they discuss future needs of quantifying things like small-scale spatial variation and field measurements of actual VOX-emissions before actual conclusions may be drawn. The question is if this will be clear to a reader who is either not very deep into the subject already or who does not read the paper very thoroughly. There is a clear risk that this will be another paper that is cited for providing general estimates of VOX-emissions from a quantitatively important environment in general instead of what it actually does: Provide evidence that there may be an emission and provide suggestions as to how reliable estimates may be achieved in the future. I think the authors should try to rewrite parts of the manuscript bearing this risk in mind, e.g. by decreasing the focus on their VOX formation measurements.

Specific comments:

P. 7527 l. 11. What semi-volatile compounds? Chloromethane seems to be mentioned as an example but would be classified as very volatile I think?

P. 7528 l. 27 to P. 7529 l. 1. Please provide (a) reference(s) for this statement.

P. 7530 l. 2-3. Do you actually know that this extrapolation is possible?

P. 7530 l. 16. Do you mean sea or rather salt lake here?

Section 2.3 The section on especially the VOX production seems extremely short compared to the value the data obtained are given in the later Results and Discussion sections.

P. 7533 l. 2-4. The procedure of freeze-drying and milling the soil samples might not change abiotic formation to a very large degree (although probably some) but the biotic formation will be greatly affected. Since this paper is sent to the Journal Biogeosciences I assume that the authors also expect biotic activity to be important in regards to VOX formation. The authors need to address this issue in detail also within the manuscript, and to discuss to a greater detail how reliable their estimates of emission from these soils are! If the formation experiments only serve as examples that VOX may be formed from these soils and as an example to demonstrate how the subsequent calculations may be performed, the authors should write this very clearly and then put less emphasis on their emission estimates in the Results and Discussion sections.

P. 7534 l. 24. Which of the three isomers of DCE?

P. 7536 l. 17. Please define typical. Your observation? Data from literature (I assume it is your data, but there is nothing about sampling at these sites in the Methods section).

P. 7536 l. 21. Does $CHBr_3$ occur consistently (it does not in Table 3)?

P. 7537 l. 3. Why especially for southern African environments?

P. 7537 l. 5-13. I agree that extrapolation from small to large scale is essential. I agree that the remote sensing approach using satellite information might work to estimate changes in landscape. I am however not convinced at all that you can then extrapolate from lab- or even small scale field measurements to general emission rates for a certain landscape. As long as the mechanisms of formation of VOX in these environments are unknown and great variations in VOX-emissions are evident but not understood (as also indicated with the variation in table 2 and 3) in my opinion, one should be careful to conclude that this might actually work in practice.

Table 2. I believe that presenting emissions estimated from experiments with a few grams of soil with the units g/km^2 tends to completely ignore the great spatial variability that may exist even on a small scale (e.g. as was recently described for $CHCl_3$-emissions from temperate forests (Albers et al. 2011)

Table 3. Something must be wrong with the units (g/m^2). Furthermore, it is not clear to me, if the minus-sign indicates no emission (which would in some of the cases be unexpected with the very large emissions at other sites, even if the unit is g / km^2 as in table 2) or if it indicates that the study is ongoing, as is indicated in the Table caption.

Table 3. Did you also determine the fourth Br/Cl-trihalomethane ($CHBrCl_2$)? It could be interesting to compare with the others. . .

Table 2 and 3. What are the detection limits of the emissions? It is surprising that you in some cases see no formation while in others a large formation (actually very large compared to any previous published VOX-emissions). What is your suggestion to explain this observation?

Reference: Albers, C. N., Jacobsen, O. S., Flores, É. M. M., Pereira, J. S. F., & Laier, T. 2011. Spatial variation in natural formation of chloroform in the soils of four coniferous forests. Biogeochemistry 103: 317-334

Interactive comment on Biogeosciences Discuss., 8, 7525, 2011.