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Interactive comment on "Halogens in porewater of peat bogs – the role of peat decomposition and dissolved organic matter" *by* H. Biester et al.

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This is an interesting paper dealing with the natural cycling of halogens in the environment with the focus on peat bogs with differing atmospheric deposition patterns. The data is original and holds the potential of bringing further light on the natural cycling of halogens and the paper is to a major extent based on sound reasoning. However, it has some major weaknesses which make it impossible to evaluate the data as it stands now. I suggest major revision.

The general assumption that chloride is inert in soil and thus can be used as an internal tracer of water movement is commonly used in hydrological and hydrochemical studies. The past years of research has revealed that chloride participates in a complex biogeochemical cycle - hence the inertness of chloride is questioned. The present paper takes its point of departure in these findings and argues that the fact that the concentration of

chloride (and the other halogens) is higher in pore-water than in rain proves that these elements originate from decomposing peat. However, without knowledge of evapotranspiration and run-off, it is not possible to determine how high the concentration of the halides ought to be if they were to originate solely from precipitation. It is well known that high evapotranspiration in combination with low run-off renders a considerable increase in chloride concentrations. I assume that the investigated bogs do have outlets since they otherwise would be prone to saltification.

It is quite possible that the authors are right in their conclusion, but the data does not stand for scrutiny without information on evapotranspiration and run-off, hence basic hydrological information should be included and internalised in the text.

The distribution between organohalogens in peat, halides in peat, organohalogens in pore water and as halides in the pore water at any site in the investigated bogs is bound to be the result of at least four processes: 1. input by vertical movement, 2. output by vertical movement, 3. halogenation 4. dehalogenation. (input and output by horizontal movement can probably be omitted). As the text stands now, the authors mainly confine the discussion to dehalogenation. Halogenation is brought in on page 1469 (page 10 in the ms) and vertical transport is concluded to be of minor importance on unclear grounds. The input of organohalogens by atmospheric deposition can most likely be assumed to be of minor importance based on the reasoning in Asplund and Grimvall, 1989. However, vertical transport within the peat-bog is likely to occur (as I assume that the investigated bogs do have outlets for reasons given above). The authors should interpret their results in the light of the complexity guiding the halogen cycle rather than focussing on one out of four major processes. If this is done, the paper has the potential of becoming truly interesting.

Redox does most likely play a crucial role in the natural halogen cycle. The authors touch upon this issue and do, between the lines, conduct an interesting discussion. However, the line of reasoning disappears in the overwhelming and unstructured body of text. I suggest that the authors refine this discussion and make it a major point of

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the paper and omit as much as possible of other more speculative threads.

The text is a bit raw and repeats itself several times. It should be structured such that repetition is avoided.

Details:

It is confusing that the authors repeatedly stress that “organohalogens in porewaters are not considered in this study” (e.g. line 25) at the same time as one of the major issues discussed is organohalogens in porewater based on data from the present study (?).

The method section should be more elaborated such that it is possible to evaluate the results without having to scrutinize previous papers. No need to go into details but more information than presently given should be given. It is a bit difficult to understand from the information given, what was measured and how. This is of crucial interest since the major focus deals with speciation and distribution patterns among organically bound, adsorbed in peat, dissolved organic and ionised halogens.

The authors do not take into account previous data suggesting that iodine changes speciation and thus solubility with redox (mainly particle bound at higher redox and as free iodide at lower redox).

When discussion enzymatic halogenation, reference should be given to

Asplund G et al. 1993 A chloroperoxidase-like catalyst in soil: detection and characterization of some properties. Soil Biol Biochem 25:41-46

Ortiz-Bermudez et al. 2003. Appl. Env. Microbiol. 69:5015-5018

When discussing halogens in peat, reference should be given to

Asplund and Grimvall, 1989

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