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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 #2

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General comments: The manuscript deals with an important open question of trace gas emissions, that is the upscaling of trace gas fluxes from small scale experiments to larger spatial and temporal scales. Furthermore the authors present interesting new data on the formation of halocarbons in arid environments, which so far have not been recognized as potentially important source regions for volatile halocarbons. The idea of combining remote sensing data with production rates of VOX from small scale experiments seems to be very promising. Anyhow from the data and results presented here, I have some major concerns whether this approach works out in this special case. Some of my concerns are given below.

1.)The production rates have been obtained from freeze dried and ground milled samples in laboratory incubations. The microstructure of the soils is destroyed during the sample preparation in the lab and thus the production rates reported here may largely differ from production rates under natural conditions. This issue is of even more concern when it comes to emission fluxes as these are known to depend strongly on the texture and porosity of soils. This problem has not been addressed in the manuscript. I suggest that the authors clearly distinguish between the production potential of a soil as determined in lab experiments and emissions. Further the authors should indicate how and to what extent the processing of the samples might affect the production rates.

2.) The authors provide between 1 and 3 data points on DCE production for each land cover class and the production rates within the land cover classes are highly variable. In my opinion the dataset on DCE production presented here is to small to address the variability of the DCE production for the different land cover classes on a geostatistical significant level, which. I suppose, is a prerequisite of any upscaling.

3.)From table 3 it appears that the variabilites within the land cover -classes are larger than the variability in between the land cover classes. Given this, it is unclear to me how the use of land cover classes can improve the quality of the up scaling as compared to simply multiplying the average production rates with the total area. The authors mention some of the current limitations of their approach but miss to provide evidence for it's benefit. I think the authors should clearly state how the use of remote sensing data improves the overall quality of the up scaling as this is the main focus of the manuscript.

4.) In my perception the use of remote sensing for upscaling trace gas fluxes requires to attribute the variation in the flux data to properties, which can be derived from remote sensing data and use these relations for up scaling trace gas fluxes As I understood the manuscript the remote sensing data have been used to derive land cover classes according to the FAO.. But according to the manuscript these land cover classes can only poorly resolve for the main drivers of VOX production in soils which are soil organic content (p. 7534 l. 16), halide concentration (p.7537 l. 11 and soil humidity (p. 7528 l. 29). This again raises the question whether remote sensing can improve the up scaling of the DCE fluxes. In their conclusions the authors cite several papers, which seem to provide more advanced methods for assessing the soil salinity. I wonder why these approaches were not applied in this study?

The manuscript has been carelessly prepared. There are quite a few references in the text, which have not been included in the reference list. Some but not necessarily all are listed below: WMO 2007 Von Glasow & Crutzen 2007 Keppler 2000 Keppler 2003 Keppler 2004 Dimmer Varner Rhew Yokouchi Hamilton Winterton

The introduction may be shortened as it provides many detailed information that is of minor relevance for this paper. This includes for instance the number of known organohalogen compounds (Gribble 2010) and the list potential de novo producers. Furthermore formulations such as “Beside the intensified public discussion ....” (p. 7529 l. 8 – 10) should be avoided. Large parts of the conclusions are written like a proposal (for instance p. 7538 l 10 – 17), which is quite uncommon.

There seem to be severe mistakes in the calculation of the production rates or the units are completely misleading. In the manuscript (page 7534, line 24 and in figure 4 the authors provide a production rate of 100 ng*m-2. This should equal a production rate of about 1000 g*km-2 as given in table 2. After recalculating this production rate I end up with 0.1 g*km-2 and not at 1000 g*m-2. The production units in table 3 [g/m^2] are obviously erroneous. In general the production rates should have a time dimension e.g. ng*m-2d-1 instead of ng*m-2

Specific comments Page 7526, line 11: Please specify the DCE isomer. Page 7528 l. 27: should be drivers instead of driver. This statements about the main drivers should be substantiated by references. Page 7529. l.8-10 Please avoid the statement about the public perception of climate research. Page 7534 l. 16: The statement on the importance of soil organic matter should be substantiated. It is unclear whether

the is a result of this work or the authors refer to the literature. Page 7536 The main purpose of this section seems to be to stress the global relevance of organohalogen emissions from saline environments. The section remains quite speculative due to the use of tendential terms such as “increase dramatically” or “increase exponentially”, which in general should be substantiated from the data. l. 4 – 6. This statement is very speculative and should be substantiated by additional data. The authors mention a strong effect of soil temperature and soil moisture on the VOX production rates several times in their manuscript but do not provide any data on these effects. Furthermore this statement should be substantiated by additional information on the annual variability of the soil moisture and soil temperature regimes. l. 7 – 9 This is somewhat speculative. Is there evidence for DCE-emissions from hyper saline sediments of precipitating lake systems? L 14 Do the authors mean exponentially to the numbers of compounds?

Table 2:Heading: Please replace concentrations by productions in the heading of the table. It seems as the authors have not differentiated between not measured and not detected. I assume that for the soil samples n.d. means not detectable whereas for the reed and water n.d. means not measured. This should be clarified.

Heading of table 3. Please insert “and” between soils and sediments. Check the units.

Fig.4. legend: Should be dichloroethene instead dichlorethen, replace concentration by production and check the units. Use squared brackets for the units on the y-axis.

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