Biogeosciences Discussions, 2, S790–S794, 2005
www.biogeosciences.net/bgd/2/S790/
European Geosciences Union

Biogeosciences Discussions

Interactive comment on "C_2-C_{10} hydrocarbon emissions from a boreal wetland and forest floor" *by* H. Hellén et al.

Anonymous Referee #3

Received and published: 14 December 2005

The manuscript describes C2-C10 and halogenated hydrocarbon emissions from a boreal peatland and forest floor. The subject is relevant and well in the scope of Biogeosciences. However, the paper suffers from limited data to support the main conclusions. Below, I have tried to explain what I see as the weakest points in the manuscript. To tackle these well, more data are needed. To tackle them somewhat, the manuscript has to be extensively revised, including the main conclusions.

One of the major findings is that monoterpene emissions from the forest floor were highest in the spring and autumn and very low during the summer. This statement was based on measurements conducted once in April, twice in May, not at all in June, once in July, not at all in August, twice in September and twice in October (Fig. 4, 2004). I do not see how anything can be stated about seasonal variation in the emission based on these measurements; one measurement between mid-May and September is not

enough to prove that the summer emissions are negligible. This applies to the discussion on the seasonality of emissions throughout. A further problem with generalization of the forest floor results comes from the fact that the emissions were only measured on two plots. As the variation in VOC emissions from ecosystem plots is very high, two plots cannot give a reliable estimate of the forest floor emissions in the studied forest, not to mention boreal forests in general.

Discussion on the needle litter being the probable source of the forest floor emissions is largely based on speculations. My guess is that litter may, indeed, be a significant source for the measured emissions, but your reasoning is based on the seasonal variation and is therefore not logical. Since you have no data showing that litter was the source, you have to be careful with the wording.

Another replication-related problem can be found on page 1802, lines 1-6. Sedges were removed from one of the plots at the peatland site, and both green mosses and sedges from another plot. This type of manipulation cannot be used to show anything about contribution of sedges or mosses to the total emission without replicate plots, because other differences between the plots alone can confound the plant removal effect. Furthermore, there is no control on the effect of the mechanical effects the removal process may have caused. Sedge and moss removal would have been a very interesting additional part to the present study had it been adequately replicated (let's say four plots of each of the treatments). In the current situation I would recommend being very careful with the conclusions drawn from the manipulation. It is not possible to state that Sphagna were the main source of isoprene from the wetland.

The same problem is faced with the chloroform emissions (page 1803, line 20 - page 1804, line 28). As the authors noted (page 1804, lines 23-28), there is high spatial variation in the efflux of chloroform even among similar-looking microsites. To cope with this high variability, lots of independent samples (from different places in the forest) are needed if the aim is to estimate chloroform emissions from the forest under investigation. Measurements from two collars are clearly inadequate to permit discus-

BGD

2, S790–S794, 2005

sion on magnitude of chloroform emissions from boreal forest floors. Boreal forest floor is very heterogeneous, and if the aim is to quantify the flux, measurements have to be conducted on a number of replicated patches. How where the collars placed (randomly or on similar-looking microsites)?

It is in contrast with earlier research, and therefore interesting, that no emissions of halogenated hydrocarbons from the peatland site were detected. The short mentioning about this on page 1801, lines 24-28 would benefit of some further discussion on what could be the reason for the lack of these emissions in the present study. Could the difference be explained e.g. by different methodology, vegetation or water table depth?

More specific comments:

- Introduction lacks a clear focus. Although some related literature is listed and some hints of reasoning are added, publishing of the present data set is not clearly justified. What were the aims of the study? What were the hypotheses to be tested?

- Method description in the Results section (e.g. page 1801, lines 1-3) belongs to the Materials and methods.

- The measurement sites should be described a bit better. What was the wetland type and how high was the water table? Was the vegetation similar in all the plots? (In general I do not like the term “wetland”, when more specific terms are available.) Where there any shrubs inside the collars at the forest site? According to the Results section, there was some litter on these plots. This could be mentioned already in Materials and Methods.

- Correct the misspelled scientific names of the plant species.

- Please give some climatic data to show how rainy and cold the summer of 2004 was compared to average summers.

- Some further details to the description of sampling: What material were the chambers made of? How did the samples get from the chamber into the adsorbent tubes? Was

BGD
2, S790–S794, 2005

EGU

air in the chamber mixed? Was temperature always monitored inside the chamber during the measurements?

- Use a few more words to describe the blank test. Now the sentence hardly makes sense.

- What does “seasonal development” (page 1800, line 26) mean? What does the “water content” (page 1800, line 26) refer to? Water content of the plants, soil, atmosphere?

- How can the larger area in REA measurements vs. chamber measurements explain the difference in fluxes (page 1801, lines 12-14)?

- When did the snow melt (page 1802, line 11)?

- References to figures and tables are partly inadequate and partly wrong (e.g. page 1802, line 11). - this should be checked throughout.

- What does Table 1 show? Ranges? Please, correct the table caption so that it corresponds to the content (e.g. 2004 vs. 2005).

- Fig. 1. is not needed.

- I assume Fig. 2 shows data from the peatland site. Clarify this in the figure caption. - Figs 3-4: In each figure caption, specify what the dots/bars (and error bars) represent. Change the x-axes from category to value axes so that they show actual dates, and the differences between the measurements can be seen. Label dates according to the journal instructions.

- In Fig. 4 you should make a clear distinction between the data presented in the current paper and the Scots pine data used as a comparison. This should be done at least in the figure caption, but could also be clarified in the figure itself using clearly distinct ways to show the two data.

- I would omit Fig. 5., because the data can be presented in a small piece of text.

BGD

2, S790–S794, 2005

Interactive Comment

- The English language should be carefully revised throughout the manuscript. As a non-English speaker I am unfortunately not able to help.

Interactive comment on Biogeosciences Discussions, 2, 1795, 2005.