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Biogeosciences Discussions

Interactive comment on "Snowpack concentrations and estimated fluxes of volatile organic compounds in a boreal forest" *by* H. Aaltonen et al.

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The manuscript by Aaltonen et al. describes measurements of volatile organic compounds like isoprene, monoterpenes, and sesquiterpenes in the interstitial air of a seasonal snowpack below a canopy in a boreal forest in Finland. They performed measurements during two winters at two or three different depths in the snowpack and estimated fluxes of the determined compounds based on the observed vertical concentration gradients. The measured concentrations and estimated fluxes are significant and indicate that processes in the wintertime can be important regarding the exchange of VOCs between the forest and the atmosphere. The authors further show

that processes related to the damage of forests can significantly enhance the VOC concentrations. These results may have important implications regarding the sources of biogenic VOCs and their impact on physicochemical processes in the forest atmosphere. Therefore, the manuscript covers important subjects warranting publication after considering the comments listed below. I understand that the manuscript is a contribution to the iLEAPS special issue, which is a joint special issue of Biogeosciences and Atmospheric Chemistry and Physics. With respect to the addressed subjects I believe that the manuscript would be better off in Atmospheric Chemistry and Physics than in Biogeosciences. I leave this decision to the editors.

Comments Ch. 2.2: Correct measurements of concentrations in the interstitial air of snow are extremely difficult to obtain. Due to large concentration differences between the interstitial air and the atmosphere (or in different layers of the interstitial air) observed concentrations cannot directly be allocated to a certain snow depths as soon as air from different layers or from the atmosphere is mixed into the sampled air. In my opinion, the set-up used by the authors is novel and was used for the first time. At least for the VOC sampling it seems like a good technique to avoid the mixing in of air from different layers and the atmosphere. Since used for the first time, I recommend to elaborate a bit more on the technical details of the set-up. What is the estimated residence time in the collectors? How much time is needed to get homogeneous concentrations in the collectors between the sampling periods? Can the sampled area or volume be estimated? How are the adsorbent tubes accessed and replaced? How is the pump connected to the tubes?

Ch. 2.3 and 4.4: The advantage of using the SNOWPACK model remains unclear to me. According to the presented equations used for the estimation of the fluxes, all needed parameters were measured. Therefore, the fluxes could be estimated using only the observations. As far as I understand SNOWPACK was used to determine the structure of the snowpack to determine the vertical profile of the snow water equivalent (SWE), which was subsequently used to derive different diffusion coefficients in

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the snowpack between the different levels of the concentration measurements. Is that correct? How large are the differences between the diffusion coefficients (or the fluxes) obtained with a total SWE and with the model-derived SWE? How do these differences compare to other errors and uncertainties? For example, the authors state that the middle collector was installed at a height of 10 to 15 cm. This uncertainty directly causes an error in the estimated flux according to equation 1. Modeling the snowpack properties below a canopy is still a challenging task. How confident are the authors in the simulated results? Have they been validated (beyond the comparison of the snowpack temperatures as mentioned in the manuscript)? What meteorological data were used to run the SNOWPACK model? Are there data measured underneath the canopy? In summary, it may well be that using the SNOWPACK model helped to improve the estimation of the fluxes. However, the authors need to give more information clearly demonstrating this point. If using the observed SWE results in comparable fluxes these results should also be included in the manuscript.

Editorial comments: Introduction, first paragraph: After reading the first lines, I got the impression that so far only finish scientists have been working on biogenic VOC emissions from forests. It is certainly okay to refer to own previous studies, but the authors should acknowledge also the studies of other groups.

P. 529, l. 7ff: "The air chemistry ... wintertime forest floor VOC processes." This statement remains unclear to me. As far as I understand Kulmala et al., 2000, there is no indication of the role of wintertime processes. The statement is too general and should be clarified.

P.531, l. 15: The thinnest snow cover recorded in Table 1 has only 8 cm of snow, not 10 cm.

P. 534, equations 2 and 4: A common definition of the tortuosity is the ratio of the path length in a porous medium over the direct path length. Based on this definition the tortuosity should always be equal or larger than 1. I believe that equation 4 rather gives

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the inverse of the tortuosity. If this is corrected, equation 2 also needs to be changed to D = phi*Di/tau * (P0/P)(T/T0)ˆ1.75 (see for example Domine et al., Atmos.Chem.Phys. 8, 171-208, 2008 without the corrections for p and T).

P. 535, first paragraph: Please list the used diffusion coefficients for the single species. They could be added to Table 2.

P.539, l. 26: "... the O3 reactions are negligible, ... released to the atmosphere." Why is that?

P. 544, l. 25f: "These new results ... VOC emissions more accurately." Based on the presented results I do not understand how that should be possible at this stage. For example, the estimated concentrations and fluxes vary by orders of magnitude. How can they reasonably be used in ecosystem modeling or atmospheric chemistry modeling? I am convinced that there is a multitude of parameters that impact the resulting VOC fluxes at the snow surface. The authors have addressed a few of them with a limited number of observations. I do not think such a general statement is warranted here. The authors should rather try to give more precise advice of what could be the most important parameters.

Fig. 2 to 4: In the printed version, the figures are impossible to read (lines, error bars too thin; labels too small; ...).

Interactive comment on Biogeosciences Discuss., 9, 527, 2012.