

## ***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 authors wish to thank Dr. Jacobi for encouraging comments and suggestions to improve the manuscript. We have answered each of the comments below.

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

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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?

- Information about the setup and the sampling procedure is now added to the Ch. 2.2 to answer the questions of the Referee.

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 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

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results should also be included in the manuscript.

- As the SNOWPACK model is not constructed for this kind of environment met in this study and the parameters describing canopy properties are difficult to determine, we decided to use measured parameters instead of those modeled with the SNOWPACK. However, as the measurements of snow properties were not fully comprehensive, the SNOWPACK model was used for complement and validate the measured parameters, especially diffusivity and tortuosity. Model has been validated for SMEAR II stand in reference (Rasmus et al., 2007) mentioned in Ch. 2.3. Explanation about the input data used for the modeling is now added to the Ch. 2.3. The installation heights of the middle collectors were given with some inaccuracy, since the ground is very uneven at SMEAR II stand.

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.

- Agreed, however, VOC measurements from boreal forests, especially during winter and from forest floor are quite rare. One reference more was added.

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.

- Kulmala et al. 2000 was meant to be used as an overall reference for tropospheric air chemistry. Sentence was clarified.

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

- The depth of the snowpack was measured from seven places at the SMEAR II stand

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and the values showed were the average depths of these places. As the collectors were not located in the close vicinity of any of these eight places and the ground is very uneven at SMEAR II, there is some inaccuracy in these depth values. A word "approximately" was inserted to the text.

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 the inverse of the tortuosity. If this is corrected, equation 2 also needs to be changed to  $D = \phi \cdot D_i / \tau \cdot (P_0/P)(T/T_0)^{1.75}$  (see for example Domine et al., Atmos.Chem.Phys. 8, 171-208, 2008 without the corrections for p and T).

- Tortuosity is defined in several ways. This one we used is defined as a proportion of porosity and values are thus always between 0 and 1, 1 meaning no tortuosity. The definition is explained more detail in Duplessis, J. P. and Masliyah, J.H.: Flow through isotropic granular porous-media, Transport Porous Med., 6, 207–221, 1991.

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

- Diffusion coefficients were calculated for each sampling separately, as they are dependent on current temperature, pressure, snow porosity and snow tortuosity. Diffusion coefficient included also molecular diffusion volumes (Fuller et al., 1969), and because all the monoterpenes and sesquiterpenes have the same molecular structure within the group, we had only two values for diffusion volumes and these are now added to the text.

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

- Sentence was clarified and some references added.

P. 544, l. 25f: "These new results: : : VOC emissions more accurately." Based on

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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.

- Agreed and the sentence now reworded.

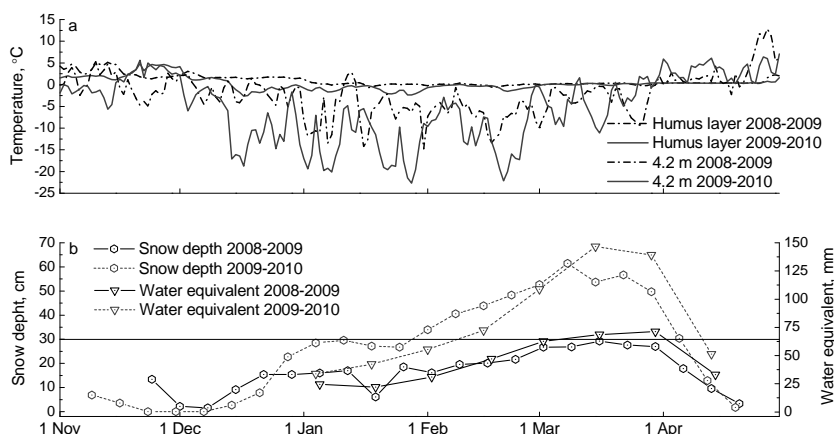
Fig. 2 to 4: In the printed version, the figures are impossible to read (lines, error bars too thin; : : :).

- Figures have been clarified in the revised manuscript. See below.

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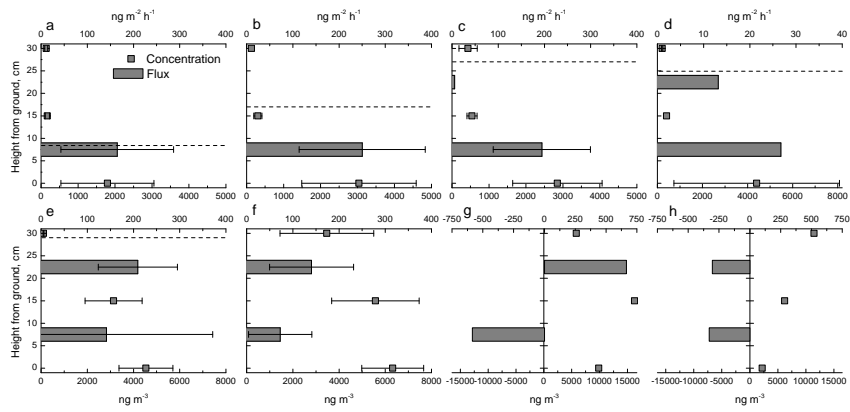
Interactive comment on Biogeosciences Discuss., 9, 527, 2012.

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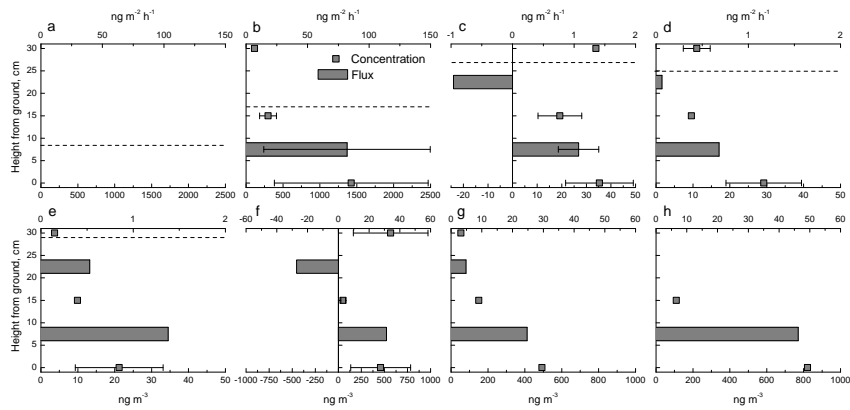
**Fig. 1.** Figure 2. Temperature, snow depth and water equivalent at SMEAR II

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**Fig. 2.** Figure 3. Total monoterpene concentrations and estimated fluxes in the snowpack

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**Fig. 3.** Figure 4. Total sesquiterpene concentrations and estimated fluxes in the snowpack

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