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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Uptake and emission of VOCs near ground level below a mixed forest at Borden, Ontario" *by* M. Gordon et al.

Anonymous Referee #2

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The paper present sub-canopy mixing ratio measurements conducted at the Borden Forest Research Station. The aim of the paper was to measure and quantify the uptake and emissions of VOC by forest the floor. As main results and conclusions the authors give diurnal cycles of VOCs and deposition velocities for isoprene and MACR+MVK. The authors relate the forest floor deposition and deposition to the canopy top emissions. The subject of below-canopy processing of reactive compounds is highly interesting and fit the scope of the journal. However, I found the results and conclusion to be rather unsound. My main points of criticism are given below.

The authors use the profile-flux relations derived for well-developed turbulence well above rough surface, to derive fluxes from profiles measured below the forest canopy, thus violating the assumptions behind the theory. The flux-profile relations are known

to be systematically erroneous even above the forest canopy if the measurements are conducted too close to the canopy (e.g. Garratt, 1994). The comparison of the directly measured eddy covariance (EC) fluxes and profile fluxes of water vapor shown in Figure 3 highlights this problem. During daytime the profile fluxes are about three times higher than the EC fluxes, and at night not even the sign of the flux is correct. Thus it is very hard to believe in any quantitative results by the methodology described here.

Also the model the authors use seems to be poorly constrained. The authors mention on page 4518, that they modify the basal emission rates to match the measurements. They do not mention how they did that. If the basal emission rate is matched to the below canopy mixing ratio measurements, the model is very poorly constrained, as many factors (turbulent transport within canopy, chemistry, light penetration into the canopy affecting the vertical distribution of emissions) can also affect the below canopy concentrations. Thus the comparison of below-canopy fluxes to the top of the canopy flux is doubly uncertain as not only are the below canopy fluxes suspicious, but also top of the canopy flux is poorly constrained.

The authors make no mention on the importance of below-canopy chemistry on the fluxes. How sensitive is their model to the changes in below-canopy chemistry due to the changes of e.g. turbulent transport, below-canopy ozone, OH, and NO3? The authors state as one of their assumption inertness of the compound. This may not be a valid assumption as the Damköhler number for e.g. sesquiterpenes may be quite high especially at night.

I also have several minor and technical comments, but I only list below a few main ones.

The paper mentions (page 4508) sub-canopy EC system for VOCs. Why no data is shown and used in the analysis?

Some of the figures could be presented without color, now it is impossible to distinguish different lines in black and white copy in e.g. fig 3. Some of us are still printing in black

ACPD
14, C1032–C1034, 2014

and white so unnecessary use of colors in simple graphs should be eliminated.

Use of UTC time is confusing, please use rather the local time.

The deposition velocity depends not only on deposition flux but also on the height at which the concentration is measured. The height used here is not stated.

References

Garratt, J. R.: The Atmospheric Boundary Layer, Cambridge University Press, Cambridge, New York, 1994.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4505, 2014.