

Interactive comment on “Eddy covariance VOC emission and deposition fluxes above grassland using PTR-TOF” by T. M. Ruuskanen et al.

Anonymous Referee #3

Received and published: 22 October 2010

General comments: The manuscript is a mostly well written and consistent contribution to the growing data base of biogenic VOC emissions. The measurements are novel, carefully conducted, and for the most part well analyzed. They should be published after probably minor revisions.

Suggested Improvements

1. It should be pointed out more clearly that the measurements were not intended as a longer term study of grassland VOC emissions, which had been done at this site before, but rather as a test of a novel instrument for performance and consistency with previous measurements. I believe the authors can then cut the introduction into half.
2. There is some confusion about what the tubing lag time actually was. The statement

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“Our true lag was around 0 s ...” is odd because of a finite travel time in the sampling line. I presume there was an offset between the mass spec’s timeline and the data-logger’s timeline that recorded the wind signals? Passages explaining this part of the work should be clarified.

3. The measured monoterpenes fluxes are not supported by the literature or independent verification. There is mention of some GC-MS measurements of air samples adsorbed on cartridges, but no mention of monoterpenes in that respect. The listed species are not known to be significant emitters (with the possible exception of clover) or their emissions are simply unknown. Please refer to and reference Hewitt and Street, “A qualitative assessment of the emission of nonmethane hydrocarbon ...”, Atmospheric Environment 1992). Neither of the onsite species is known to store terpenes.

It would be an important finding if monoterpenes had a significant deposition rate. The authors’ data in Figure 3 and Table 1 suggest an average flux of -1 to -3 nmol m⁻² s⁻¹ at an abundance of 1-2 ppb monoterpenes. That translates into a deposition velocity of several cm per second, comparable to ozone and NO_y deposition rates. Lacking a similar surface destruction or removal mechanism compared to these species, this result is physically implausible. I wonder if the valley location of the grassland in between monoterpene sources leads to a strong horizontal advection of these species towards the site that invalidates the EC assumptions and creates this spurious signal. I suggest starting with an analysis of the meteorological conditions during the days prior to harvesting (maybe all met data should be shown anyway).

Unless the authors can demonstrate through field or laboratory testing, preferably using flux chambers, that this grassland location is depositing terpenes at such large rates, the terpene flux finding should be omitted or used to demonstrate the site’s limitations for EC measurements. Tables and Figures need revisions accordingly.

4. Methanol being the dominant emission species found, the authors may want to read

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and cite Custer and Schade, "Methanol and acetaldehyde fluxes over ryegrass", *Tellus* 2007, alongside the citations on page 21092.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 21077, 2010.

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