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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of HNO_3, ΣAN, ΣPN and NO_2 fluxes:evidence for rapid HO_x chemistry within a pine forest canopy" *by* D. K. Farmer and R. C. Cohen

T. Foken

thomas.foken@uni-bayreuth.de

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I apologise for my late answer, but I was involved in field work. I see some significant problems in the micrometeorological approach, which may influence the chemical interpretation of the experimental results.

p. 7092, line 25ff: I think for the eddy covariance method some more information is necessary in addition to the reference to Farmer et al. (2006). Their method seems not to be state of the art (Lee et al., 2004) because of the reference to McMillen (1988). The data selection for night time, calm and other situations should be repeated or explained - or is the paper restricted to conditions at noon (Tables)?

p. 7094, line 15-16: If you use the temperature measurements from the sonic anemometer (sonic temperature) you measure the buoyancy flux and not the sensible heat flux (Liu et al., 2001; Schotanus et al., 1983). The calculation of the stratification has an error up to 20 %.

p 7097, line 11ff: The method described is extremely unclear. You do not use the Bowen ratio method but the modified Bowen ration method (Businger, 1986). The method works only if both fluxes are similar. This is valid for the carbon dioxide and water fluxes in the case of well transpirating canopies, but for most of the fluxes is not (Ruppert et al., 2006). Furthermore, both scalars must be inert gases. This is probably also not the case.

p. 7097, line 25 ff: If I understand the paper correctly, you have carbon dioxide measurements up to 10.5 m and have extrapolated these up to 14.3 m. Furthermore, you have concentration measurements at 14.3 m for different chemical species and assumed a similar gradient as the carbon dioxide concentrations to calculate the concentrations of the chemical species for 7 m. Firstly, was the scalar similarity controlled and the fluxes identically, otherwise the gradients differ for different fluxes and scalars, which must be furthermore inert gases. Secondly, how can you extrapolate a profile in the roughness sublayer (canopy height 7 m), as the available approaches are not very accurate.

p. 7098, line 4 ff: For the gradient see my comment to p. 7097, line 25ff and for the determination of the Obukhov length my comment to p. 7094, line 15-16. In eq. (4) you present a new form of the integrated profile equation. I am missing an error analysis to the original form (Paulson, 1970), see also all textbooks. Furthermore, you use the universal function of the Kansas experiment (Businger et al., 1971), but with the wrong von-Kármán constant. Nowadays a different function is used (Högström, 1988).

p. 7098, line 16ff: The use of the surface renewal method is very questionable: firstly, the determination of the gradient (see above) and secondly the transition time. Nor-

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mally typical ramp structures (Finnigan, 2000) are used (Snyder et al., 1996). This time is much shorter than your resistance time of 400 s.

p. 7100, line 12: For forests, coherent structures are very typical (Collineau & Brunet, 1993a; Collineau & Brunet, 1993b; Finnigan, 2000; Thomas & Foken, 2005; Thomas & Foken, 2007). The typical time scale is about 30 s. Therefore the mixing occurs much faster than you calculated. Resistance times of about 400 s correspond to smaller circulation systems like between forest and clear cuts (Zhang et al., 2007). More important than the separation between winter and summer time are the time of the day and the coupling situation between the atmosphere and the canopy to interpret the data (Göckede et al., 2007; Thomas & Foken, 2007).

Finally, I think that the investigated fluxes of chemical species need, because of changing micrometeorological and reactive conditions, that means the Damkoehler number, a much higher time resolution and the averaged data presented in the Tables are less helpful.

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