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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hydrocarbon fluxes above a Scots pine forest canopy: Measurements and modeling" *by* J. Rinne et al.

Anonymous Referee #1

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General Comments

The paper by Rinne et al. is a straightforward first paper of VOC fluxes at the Finish Hyytiala forest site by disjunct eddy covariance using a proton transfer reaction mass spectrometer. It makes a significant contribution to the scientific literature. The paper is clearly laid out, well written and easy to follow.

The authors use a state-of-the-art measurement technique, with analysis approaches which are sound and have quickly become a standard in the community. The modelling component makes a useful addition, to help estimating how representative the measurements are for the surface flux. The manuscript is sometimes brief in describing the details of the analysis technique and most of my minor comments below are related to

this. I was a little disappointed to see only four days of data. The fact that calibrations were performed on a weekly basis (page 2363, line 7) implies that the total measurement period was much longer. However, I am looking forward to reading a paper on a more extensive dataset later.

Specific Comments

1. Page 2361, lines 20-27. I found this paragraph confusing. The authors relate to their analysis technique as a DEC technique, but appear to follow the recipe of Ammann et al. (2006) which they describe as a continuous sampling eddy covariance technique. This requires clarification. Also, if Ammann et al. (2006) use the same approach as Karl et al. (2002), why not reference the earlier paper?

2. Page 2362, lines 18-21. Could the authors expand their paragraph on the error calculation slightly? Where does the factor 1.96 come from?

3. Page 2362. Eq. (2). Why was the concentration normalised by the isotope M39 although M37 was measured directly (according to Table 1)? The first cluster only reacts with some of the compounds. Surely the others should be referenced to M18 only?

4. Page 2365, line 19. What kind of model is the model by Boy et al. (2005) that was used to predict OH concentrations? The text seems to imply that the concentrations of OH, NO3 and O3 were assumed to be constant with height and that above canopy concentrations were used in estimating the chemical reaction. Could the authors speculate on the impact of vertical gradients? Some studies have indirectly suggested potentially large OH concentrations within the canopy (e.g. www.atmos-chem-phys.net/6/3471/2006/acp-6-3471-2006.pdf).

5. Page 2366, line 27. Not all footprint models integrate to unity and for good reasons. In the general case, this analysis needs to reference the footprint with chemistry to the one without chemistry, not to unity (even though it may be unity for the model used by

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the authors).

6. Page 2367, line 12. The difficulties of sampling the highly reactive (and sticky?) sesquiterpenes into the ptrms is also worth mentioning.

7. Page 2367, line 18. Since this is the first place where the disagreement between the current and the former measurements is mentioned it would be helpful to cite the numbers of the old results. How large are the errors relative to the measured fluxes?

8. Page 2368, lines 12-18. A little table comparing the different fluxes (ranges, average etc.) from the different studies and ecosystems would help.

9. Page 2369, line 27. Clearly, the discrepancy between the ptrms measurements and the gradient fluxes cannot fully be resolved until both measurements are done simultaneously. Could the time of the year of the measurements have played a role? Presumably, the ptrms is only calibrated for one monoterpene, while M81 and M137 are fragments from a range of monoterpenes, which also fragment differently. What is the error introduced. Would it be large enough to explain a factor of two difference with the former measurements?

Technical Comments

Page 2360, line 6. Would “Until now” or “So far,” be better than “This far”?

Page 2364, line 5. Use italics and subscripts for u*.

Page 2366, line 10. The text talks about different masses than shown in Fig. 4, which is slightly confusing.

Page 2367, line 6. Suggest changing “to be equal to” to “to be a good approximation of”

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