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Interactive Comment

Interactive comment on "Flux measurements of biogenic VOCs during ECHO 2003" *by* C. Spirig et al.

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I agreed to review this contribution based on the Title and Abstract which were provided to me, and assumed that the focus of the paper was on the results of eddy covariance measurements made at the ECHO towers. In fact, two-thirds of the manuscript concerns identifying and dealing with the difficulties encountered in applying the eddy covariance technique over an inhomogeneous forest site using a sensor (PTR-MS) with a time resolution (3 s) clearly on the slow end of suitability for eddy covariance. This is not to question the value of the contribution, only my suitability as a reviewer. Indeed, the most significant contribution of the manuscript may well be to raise issues frequently confronted by those attempting to make micrometeorological flux measurements over inhomogeneous vegetation and/or when using sensors somewhat slower than ideal. My fellow reviewers, who are far more expert in this area than I, have of-

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Discussion Paper

fered some comments and constructive criticisms in this area, and I would hope this manuscript would spark some discussion and lead to a greater degree of consensus about how these issues should best be dealt with.

With respect to the flux data itself, there is little that is new or striking, but the data set is a welcome addition to the relatively small but growing group of above-canopy flux measurements which are needed to validate and constrain regional BVOC emission models. In addition, the monoterpene flux data appear to show clearly both a light and temperature dependence, consistent with leaf-level monoterpene emission data from the tree species in question. I have some reservations about how the authors chose to relate above-canopy fluxes and leaf-level emissions, and these are discussed below.

However, the topic is certainly relevant to ACP readership and the Methods and Results are clearly and concisely presented. Given my perception that the paper is dominated by a discussion of the methods used to analyze, and criteria used to accept/reject eddy covariance data, that fact should be more clearly reflected in the Title Abstract. Those of us in the business of measuring above-canopy BVOC fluxes are frequently forced to compromise between sites which are more or less "ideal" from a micrometeorologic or homogeneity standpoint and sites which are practical from a logistical standpoint. A frank discussion of ways to evaluate data under less than ideal site conditions is a useful contribution.

Specific comments

I have left any discussion of eddy covariance "data expansion", cross spectra, time lags and ogives to my learned colleagues. In the area in which I have some experience, i.e., measuring leaf level VOC fluxes and scaling leaf measurements to estimate canopy fluxes, I do have a few specific comments to offer. While the authors discuss in admirable detail their protocol for analyzing their flux data, their description of the model they used to integrate between canopy and leaf-scale fluxes is cursory at best. On p. 6621, the authors demonstrate that the measured isoprene flux is correlated

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with the percent of oaks (the only significant isoprene-emitting species) in the footprint. Although this is hardly a surprising result, demonstrating that it is the case gives us increased confidence in the data set, since much of the variation is explained by changes in the flux footprint at this non-homogeneous site. Clearly, since isoprene fluxes are controlled in the short term by prevailing light and temperature conditions, when one wishes to compare above-canopy fluxes when wind directions differ, it is necessary to normalize those fluxes to a common set of environmental conditions, as done here. Normalizing data collected over a wide range of above-canopy PAR (100 - 2000 umol m-2 s-1) and temperature (16 - 34C) can introduce considerable systematic error if not done carefully, even when normalizing data from a single leaf measurement where conditions are well controlled. Using leaf-level algorithms to correct canopy-scale fluxes is extremely problematic, especially with respect to PAR. This is not to say that it can't or shouldn't be attempted, only that one needs to be careful and owes it to the readership to describe explicitly what was done. For example, what is the justification for using what appears to be, in effect, a big leaf model (i.e., single layer canopy model) but then driving the model with "an average PAR value for the canopy" based on a 3-layer canopy model? Having gone to the trouble to obtain "average PAR" using a 3-layer model, why not use the 3-layer model to normalize the above canopy fluxes? My own quick analysis using a similar 3-layer model suggests that the results of the 3-layer and single layer models will be very similar if isoprene emission capacity at the leaf level is assumed to be constant through the canopy. Under the more realistic assumption that emission capacity decreases by 50 percent from top to bottom of the canopy, the single layer model appears to overestimate the normalized flux relative to the 3-layer model (by 20 to 30 percent). And what exactly is meant by "average PAR"? I suspect that it is the average PAR experienced by all leaves (sunlit + shaded) in the canopy, i.e., the average of the sum of the direct and diffuse fraction of PAR at each of 3 canopy levels, weighted by the fraction of sunlit and shaded leaves at each level. See. . .it's difficult to describe, but "average PAR for the canopy" is inadequate. Perhaps I'm being picky. Whether one uses a single layer or multiple layer model to normalize above-canopy

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flux data is certainly not going to change the qualitative conclusion from Fig. 10, i.e., that isoprene flux scales with the percentage of isoprene-emitting trees in the footprint. But I think the reader should have a better idea of what was done and why. Just out of curiosity, does the normalized monoterpene flux show the opposite relationship. i.e., decreasing as the percentage of oaks increases?

On page 6624, line 2, the mean normalized isoprene flux (presumably normalized as described above) at the west tower is given as 1.5 ug m-2 s-1. Looking at Fig. 10, the mean standardized flux appears to be less than 1.5, and certainly when the percent of oaks is below 60 percent, it appears to be closer to 1 ug m-2 s-1. It's not clear to me how the value of 1.5 can then be combined with an oak contribution of 44 percent to arrive at a leaf-level emission factor?

I'm having trouble reconciling the mean normalized isoprene flux at the west tower (1.5 ug m-2 s-1) with the mean hourly flux data in Fig. 9. Mean midday fluxes seem to cluster around 2.0 ug m-2 s-1 while mean midday temperatures (not given, but estimated from temperature data in top panel of Fig. 8a) seems to be less than 30C. Admittedly, mean midday PAR values are well over 1000 umol m-2 s-1, but should this lead to a 33 percent increase in flux?

Technical corrections/suggestions

p. 6604, l. 25 role of BVOC in "formation" of particulate still somewhat controversial, but they clearly play a role in aerosol growth

p. 6606, l. 19 when giving values for LAI, especially in sites such as this with non-forested areas in the footprint, it's important to specify whether the LAI is that of the forested areas only, or whether it is an average value for the site, including non-forested areas. Please clarify.

p. 6608, l. 22 it seems slightly contradictory that M33 can be confidently allocated to MeOH when in line 7 above, the reader is warned about the significant interferences at

M33, including isotopes of oxygen

p. 6612, l. 10 “aspirating. . .through an inlet line” ?

p. 6613, l. 16 high-frequency

p. 6623, l. 1 In assessing the contribution to the flux of the individual tree species, not only is their

p. 6624, l. 6 please give value of “leaf area per dry weight”

p. 6633 Fig. 1 This is a complicated figure, and I see several shades of yellow, brown and yellowish-brown. More detail in caption would be helpful.

p. 6643 Fig. 10. If the mean area of oak forest within the footprint around the west tower is 44 percent (and around the main tower even less) why is there only one data point in the figure at oak forest fraction below 45 percent?

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6603, 2004.