Atmos. Chem. Phys. Discuss., 6, S3005–S3008, 2006
www.atmos-chem-phys-discuss.net/6/S3005/2006/

Atmospheric Chemistry and Physics Discussions

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6, S3005–S3008, 2006

Interactive Comment

Interactive comment on "Technical note: Water vapour concentration and flux measurements with PTR-MS" *by* C. Ammann et al.

C. Ammann et al.

Received and published: 10 September 2006

We thank the referee for his very careful reading of the manuscript and the detailed comments. We agree with all technical comments and the suggestions concerning the text formulation, and we will modify the text accordingly. In the following we respond individually to the scientific comments and questions:

1. Page 5331, line 10-12. The authors are correct that there is no eddy-covariance instrumentation available to measure fluxes of all BVOCs detected by the PTR-MS with a higher time resolution. However, there are instruments that target individual compounds, such as the Fast Isoprene Sensor (FIS), and comparisons have been performed (e.g. Jardine and Baker, 2003).

>>> We agree with the referee and will add a reference concerning the FIS technique.

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2. Could the authors please specify how their data were treated prior to calculations of the ogives. For example, the small contribution of low frequencies would suggest that McMillen filtering was applied. If this is the case, the filtering would be partially responsible for the good congruence of the ogives at low frequencies. If filtering were not applied, low frequencies may affect different tracers differently, e.g. under certain conditions deposited species may have a different low frequency contribution to compounds that are ejected from deep in the canopy. How would this affect the empirical spectral correction technique?

$>>>$ We did not apply a McMillen (1988) type filtering or other filtering to the high-resolution time series! While the nice shape of the exemplary ogives displayed in Fig. 2 was frequently observed in our dataset, there occurred also ogives with less regular shape. However, considering the low vegetation and measurement height of the present study, the low-frequency problems have to be attributed rather to instationarity effects (e.g. due to changing wind conditions or passing clouds) than to a vertically different source/sink distribution. However the latter effect might be relevant (and should be checked) when applying the ogive method for EC measurements above high vegetation where deep-penetrating coherent structures are more important for the trace gas exchange and may affect the cospectrum in the low frequency range.

3. It would be interesting to consider whether a mechanistic understanding of the relationship between humidity and m/z 37 counts could be developed. Does the observed relationship follow the theoretical expectation? Is there any chance of predicting the relationship as a function of drift tube pressure and field strength?

$>>>$ We agree that this is an interesting question but it is beyond the scope of this short manuscript focusing on the empirical correction and validation of eddy covariance measurements. We encourage people with more expertise in ion-molecule chemistry to investigate this topic. A complicating effect in this respect might be the partial declustering of m37 ions at the transition between the drift tube and the MS detection unit.

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4. Page 5341. I am doubtful that a sensible recommendation can be made as to the value of f_limit. Surely this value depends of the frequency response of the measurement system and the magnitude of the damping. In extreme cases, frequencies may be damped down to much lower frequencies. How does the recommendation compare to the frequency at which the co-spectrum is expected to have its maximum (or the ogive a deflection point).

$>>>$ We agree that the value of f_limit could be optimized for different conditions. However, with f_limit related to the damped(!) ogive, we already account for different damping intensities. Problems mainly occur when the damping is very strong (e.g. loss of more than 2/3 of the flux). But in such cases, an appropriate correction would be very difficult anyway. The deflection point (spectral maximum) is not always well defined in individual 30-min. ogives (because they can have small local maxima and minima) and it is therefore a less robust criterion compared to the ogive value itself (corresponding to the integral spectral contribution below the respective frequency). On average the deflection point is close to the half height (value 0.5) of the relative ogive.

5. Could the authors comment on the relative merits of empirical spectral correction methods based on the analysis of ogives compared with those based on co-spectra. It seems to me that the former is biased towards fitting of low frequencies, while the latter is biased towards fitting of the peak. Hence the former would be more suitable in conditions of strong damping (where the peak frequency is also affected), while the latter would be more suitable in conditions where low-frequency contributions are important (and may differ between compounds).

$>>>$ There is no theoretical difference between the analysis of cospectra and their ogives (integrals) for the quantification of high-frequency damping, because they contain exactly the same information. However, concerning the practical aspect of fitting the damped cospectrum/ogive to the reference cospectrum/ogive in the lower frequency range, cospectra are more difficult to handle than ogives since they show generally much higher variability.

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6. Page 5341. The authors correctly point out that damping affects water more than other gases that do not interact with walls, and that this effect is not included in Moore's correction approach. However, there are alternative explanations why the empirical method derives larger damping than Moore's approach, depending on exactly how Moore's approach was implemented by the authors. On this aspect, more information should be provided in the manuscript. Firstly, although spectra are calculated on 20 Hz data, they actually represent 1.4 Hz data (due to measurement cycle of 0.7s). Secondly, the tube Reynolds number of 1620 is marginal for the flow to be fully turbulent. Does the implementation of the Moore correction take this relatively low Reynolds number into account?

$>>>$ In the theoretical damping approach according to Moore (1986) we combined transfer functions for (a) sensor separation, (b) sonic path averaging, (c) the 0.7s interval averaging for the PTR-MS signal, and (d) laminar tube flow. Due to the low Reynolds number we assumed laminar flow in the sampling tube, representing a worse case than turbulent flow for the high-frequency damping. We used the respective transfer function given by Lenschow and Raupach (1991). We will add this information in the manuscript. However, the choice of the laminar vs. turbulent flow damping had only a minor effect since the 0.7s signal averaging was usually the dominating effect.

REFERENCES

McMillen, R. T.: An eddy correlation technique with extended applicability to non-simple terrain, Boundary-Layer Meteorol., 43, 231-245.

Lenschow, D. H. and Raupach, M. R.: The attenuation of fluctuations in scalar concentrations through sampling tubes, J. Geophys. Res., 96, 15'259-15'268, 1991.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 5329, 2006.