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Atmospheric Measurement Techniques Discussions

Interactive comment on "Lag time determination in DEC measurements with PTR-MS" *by* R. Taipale et al.

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Review for

Manuscript amt-2009-105 Lag time determination in DEC measurements with PTR-MS
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This MS deals with an important problem of (D)EC measurements, namely the proper determination of the lag time between the 3D wind measurements and the concentration measurements, usually delayed by several seconds due to the inlet line of the detector system. Normally, this problem is tackled by calculating the covariance function of vertical wind and concentration, but this function is often lacking a 'clear signal', especially when dealing with low fluxes, noisy data or for data with generally low signal-to-noise ratio.

The authors present a study where several different techniques (visual interpretation, 2 different constant lag times, maximum of covariance function, new averaging technique) to determine the lag time have been compared using simulation data (taken from a high-frequency data set with added noise) and ‘real-world’ measurements of different compounds from a DEC system. The analysis and statistical methods applied seem to be adequate and the text is well structured and presented with clear figures. The MS generally reads well, and the number of relevant references cited to discuss the background and results is adequate.

In my opinion, this MS is well suited for AMT, and adds important results to the variety of people working with (D)EC-flux measurements. Thus, it should be accepted for AMT with minor corrections.

However, I would like the authors to discuss/comment the following issues more detailed:

- p413, L14pp; why do the authors return to the ‘raw’ covariance after applying the AVG procedure? In theory, the covariance function should show a sharp peak, and the AVG approach tries to remove statistical errors contained in the ‘raw’ flux data. However, if one (after determining the ‘real’ lag time through the AVG approach and removing statistical noise) applies the ‘raw’ (un-averaged) covariance to calculate the final flux, this still includes statistical errors. Wouldn’t it be good to apply the covariance found by the AVG approach to finally determine the flux? Can the authors comment on this?

- P413, L14pp.; why have the authors chosen a 5 second averaging for the AVG approach? Have you tested other averages as well (e.g. 3s, 7s, . . .), were the results comparable to the findings reported in this paper? Or was 5 seconds just an arbitrary guess?

- P413, L20pp; could you please add some more info to the VIS approach? How was the manual assessment done, why is it different from the MAX approach, have some lag times been excluded (e.g. due to 'spikes')?

- Sec. 2.2; the noise added to the EC data was normally distributed, and from this data set (H2O_noise), a DEC sub-sample has been taken to do compare the different lag-time approaches. Was the noise finally added to the disjunct sub-samples distributed normally as well, or might the sub-sample have added a bias to the results?

- Methanol and monoterpene (m137) compounds chosen from the measured data have been used to present the performances of the different approaches for lag-time determination in this study. Both compounds show a relatively good signals at pine forests. As the AVG approach seems to perform well for low signal to noisy flux data, I wonder if the authors have applied their approach for other compounds as well (with even lower expected flux signal compared to the detection limit of the measuring system)? I would expect the AVG approach to even perform better than the others under 'more difficult' conditions.

- How do the different approaches perform in different weather conditions, i.e. may the results obtained lead to different conclusions in different weather conditions? Is there a diurnal pattern of the AVG/MAX/... performance? Maybe it would be worth giving (as a table?) some statistical results like in Fig. 5 for sunny/covered, warm/cold, ... days as well if the approaches tested show different behaviour under different conditions.

- Fig. 3; what about adding a 'bad example' to the figure, a data file where the covariance function is not as well pronounced as in Fig. 3? This could show the robustness of the AVG approach (I guess it will perform well then).

- Fig. 4; would it make sense to add Figs like Fig. 4 for m33 and m137 as well (without REF then)? Maybe as one stacked Fig? This could add more info, as one could see the performance of the different approaches for different compounds and different situations (see comment raised before). I would also prefer to have temperature and PAR added to that Fig. as well to get an impression of the ambient conditions during the experiment.

- What about adding a figure with the frequency distribution of the lag-times found with the different approaches? Do they show the same (diurnal, see TYP approach) pattern and a maximum at the lag-time of the CAL approach? Are the lag-times found with the different approaches completely different (for a specific file), or do they differ only slightly in lag-time and the different flux results are caused by the statistical noise of the covariance function? Could the authors comment on this?

- I personally don't like the abbreviation CAL for the 'calculated' approach as I think it might be confusing and could be mixed up with 'calibrated' – but that might be my personal problem. . . What about DET for 'determined' instead?

- Language issues: I think the MS is written generally well, but I also think some expressions might need a revision (e.g. conundrum, fared better. . ., manoeuvre, . . .).

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