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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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We thank the referee for the review and the good suggestions.

The referee had two important questions about the averaging method. We improved the method description in Sect. 2.4 and included more detailed answers in this response letter. The referee also gave good suggestions for improving the manuscript. We considered many of them already when drafting the paper. We would have gladly presented some of the suggested figures (etc.) if they had served as explicit illustrations of our results. Unfortunately, even after reconsideration during the revision, they proved to be rather obscure or inconclusive and had to be left out (please see the answers below).

We noticed a small mistake in the calculated lag times for the IRGA and PTR-MS measurements in Sect. 2.4. They were revised from 2.7 and 7.0 s to 2.9 and 7.1 s to take account of the tubing inside the instruments. We also revised the error definition in Sect. 3.2 to clarify the error analysis. The effects of these corrections were fairly marginal but enhanced the consistency of the paper. We modified the text and figures accordingly.

Response to the specific comments

P413, L14: Right, the AVG approach was designed to facilitate automated lag time determination by reducing the noise in a covariance function. If this function has a sharp peak, the difference between the original and averaged value at the indicated lag time can be substantial. The difference can be practically negligible if the peak is less ideal. Averaging always reduces noise but may also remove part of the real signal. To avoid this problem, we found it safer to use the original covariance function for the final flux value. However, this does not mean that AVG is incapable of eliminating random flux errors. As indicated by the DEC simulation, AVG did not yield a statistically significant error while MAX did. We also made tests to see whether our results would have changed if the final flux value had been derived from the averaged function. There was no significant effect on the median fluxes of H_2O_{noise}, M33, M37, and M137. Moreover, the error in H_2O_{noise} remained insignificant. Without additional evidence, we prefer to rely on the original covariance function when determining the flux value as this method seems more immune to possible losses of real flux signals. We added information on this issue to Sect. 2.4: "This ensured that no part of the real flux signal was eliminated due to the averaging."

P413, L14: We inserted the basis for the five-second averaging window in Sect. 2.4: "The width of the averaging window was estimated visually using the EC measurements of H_2O and the simulated DEC data of H_2O_{noise}. The chosen window was deemed wide enough to allow a sufficient noise reduction but also narrow enough to prevent a considerable shift in the covariance function maximum." A reminder of the

need to check the optimum window for each application was added to the conclusions. We tested the effect of the averaging window on the H_2O_{noise}, M33, M37, and M137 fluxes by changing the window from 3 to 7 s at intervals of 1 s. The median fluxes were not affected, and more importantly, the error in H_2O_{noise} was never statistically significant.

P413, L20: We elaborated on VIS in Sect. 2.4: "The VIS method was based on manual assessment using the following guidelines. The lag time was determined visually from a figure showing a covariance function for the lag time range -180 to 180 s. This first view gave a general idea of the function patterns and noise. It was zoomed in on the lag time window used in MAX to look for a positive or negative peak with a minimum height of about two times the noise and a minimum base width of about 2–3 s. The lag time was derived from around the middle of the peak, i.e., not necessarily from the maximum. If an unambiguous resolution was impossible, only the flux uncertainty was calculated."

Sect. 2.2: Exactly, normally distributed random numbers were added to the original H_2O time series. We could not think of a theoretical reason why the distribution of the added noise in the disjunct time series had not been normal as well. In answer to our speculation, we used the Lilliefors test (e.g. Conover, W. J.: Practical Nonparametric Statistics, Wiley, 1980) to see whether the disjunct sampling affected the distribution. The tests showed that the hypothesis of normally distributed noise cannot be rejected at the 95% confidence level. Hence this issue appears an unlikely source of error.

Other compounds than methanol and monoterpenes: In the absence of a direct reference to the actual DEC measurements with PTR-MS, the focus of the paper was the DEC simulation with the H_2O data. We chose methanol and monoterpenes as the examples due to their general importance in PTR-MS measurements at pine forests and elsewhere. Although these compounds are usually relatively easy to measure, there were moments when their flux signals were near the detection limit. This probably made these two examples more representative than first thought. We naturally hope

that AVG will be scrutinized in later studies using several other compounds. This hint on future work was inserted in the conclusions.

Performance in different weather conditions: Although surely relevant, we did not study the effect of weather conditions on the method performance as we wanted the results to have statistical significance. Classification according to weather and micrometeorological conditions was not possible due to the amount of data. We added a brief reference to this issue to the conclusions.

Fig. 3: Since the figure is intended as a simple visual aid for the reader to see the differences between the lag time methods, we would like to refrain from adding a "bad example". We think that the DEC simulation results offer a more statistical way to estimate how robust or erroneous the AVG approach is.

Fig. 4: Temperature and PAR time series were added to Fig. 4. We also tried including the lag times of M33, M37, and M137, but the figure became too ambiguous. That is why we settled for this short statement in the original manuscript (P414, L18–19): "Also M33, M37, and M137 exhibited quite similar behaviour (not shown)."

Lag times: We inserted more information in Sect. 3.1: "While typically larger than the constant lag times, the medians determined with MAX, AVG, and VIS never differed significantly at the 95% confidence level. In the case of H_2O_{noise}, they were also in agreement with the median lag time of H_2O." In most cases, also the median fluxes were concordant with each other (P415, L12–16; P416, L19–22) and the momentary variations were mostly related to the noise in the covariance function. With respect to the lag time behaviour, we believe that only two conclusions should be drawn from our results. First, CAL and TYP are inadvisable options (P414, L19–21). Second, the lag time should be determined individually for each compound (P415, L1–2). Presenting lag time frequency distributions was one of our first ideas which soon turned out quite unfruitful. The distributions contained little information, usually only the general pattern was barely distinguishable.

Abbreviation CAL: We changed CAL for CALC. It is hopefully less confusing.

Language issues: We replaced the most unusual expressions with more conventional ones. Please see Response to Referee #1.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 405, 2010.