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Atmospheric Measurement Techniques Discussions

Interactive comment on "Field intercomparison of two optical analyzers for CH₄ eddy covariance flux measurements" *by* B. Tuzson et al.

Anonymous Referee #4

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The manuscript is in the scope of the AMT, it deals with important issue, and it is very timely as more and more laser spectrometers are being used in flux measurements. I find the instrument comparison reported in the paper well conducted and it mostly well written. There are, however, a few chapters that need clarification and a few that would benefit from better analysis.

How sensitive are results of the comparison of measured fluxes to the fumigation to the footprint model applied? This should be at least discussed in the paper.

Page 2969, line 24: How is the precision here defined? Is it based on 95 % confidence interval assuming normally distributed noise, i.e. 1.96 times the standard deviation of one second time resolution measurements?

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Page 2971, line 18: What does calibration factor mean here? Is it a correction factor for concentration given by analyzer (i.e. multiplicator, with unit of ppm/ppm)?

Page 2971, line 19: At what response time the reproducibility measurements were conducted. I believe precision and reproducibility are the same thing, so the result here should correspond to results from previous sub-chapter.

Page 2971, lines 25-26: "The following paragraphs are dealing with considerations related to water vapour that are not directly covered by the WPL theory, such as the effect of water vapour on the spectral line shape". Hovewer, on the very beginning of the next page (page 2972, lines 2-6) "This is, because the laser spectrometers measure the mixing ratio of CH4 to total pressure (Pt), including also the vapour partial pressure (Pw=[H2O]Pt), . . ." Isn't the dilution caused by the water vapor (and temperature) exactly the point of the WPL terms. I found this part a bit confusing.

Page 2975, line 1: ". . .density flux correction equation. . ." I am not sure if the WPL terms should be called "correction" as they really are terms in the flux equation when it is derived physically correctly, not making some simplifications needed to arrive to the form containing only the term I in the Eq. (6).

Page 2976, lines 1-2: Does the small downward flux (-5 nmol m-2 s-1), seen after dilution correction, differ significantly from zero.

Page 2977, lines 15-19: Discussion on temperature sensitivity of measured concentrations. Authors should mention/discuss whether this small sensitivity has any effect on measured fluxes.

Chapter 3.1.5. I found this chapter a bit short and weak. The authors could apply a transfer function of e.g. Horst (1997) and find the response time of that function by fitting the observed spectra to the ideal one. The authors could also discuss how large is the high-frequency flux loss derived using the observed response-times.

Page 2979, lines 9-15: Why did the authors decide to use different high-frequency

AMTD

3, C1382–C1385, 2010

correction methods for the two analyzers?

Page 2980, lines 14-15: “As soon as the wind direction is constant, the scatter of the measured and footprint weighted fluxes is getting very small”. It would make sense to filter the data so that only the half-hourly periods with rather constant wind speed are considered in further analysis. Have the authors considered this?

Page 2980, lines 16-19. Is the difference between the fluxes measured with two systems statistically significant? Now it is stated that it is “well below the uncertainty that one would expect for eddy covariance flux measurements”. The authors should use filtering suggested in the comment above to analyze the significance of the difference.

Conclusions: The conclusions are very vague when it comes to actual instrument comparison. The authors could at least list the pro’s and con’s of the two different analyzers is they do not wish to give an explicit recommendation.

Minor comments

Page 2963, lines 4-5: “typically tens of m2”. This is an understatement for many flux measurement sites. The measurement height can easily be 10 meters or more above displacement height in forested sites. I would say rather “typically from tens of m3 to thousands of m2”.

Page 2964, 14: “particle density”. I would say “number density”.

Page 2965 lines 5 and 11, page 2977, line 4; “SNR” the term appears three times in the entire paper. Thus I would not abbreviate it by write signal-to-noise ratio. This would not significantly lengthen the paper but would make it easier to read.

Page 2974, line 27: “..gives us a solid evidence. . .” I would write “. . .gives us solid evidence. . .” However, I am not a native English speaker, so I may be wrong.

Page 2978, line 14: “(Fig. 6)” It seems that this reference to Figure 6 proceeds the first reference to Figure 5. Please check.

AMTD

3, C1382–C1385, 2010

Page 2979, line 18: "...correction reduction..." I would write "...reduction of correction..." Figure 5a: Please indicate by which analyzer this concentration time series was measured. Figure 7: Please explain how the boxplot should be interpreted.

References

Horst, T.W., 1997: A simple formula for attenuation of eddy covariance fluxes measured with first-order-response scalar sensors. Boundary Layer Meteorology, 82, 219-233.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 2961, 2010.