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Interactive comment on "Suitability of quantum cascade laser spectrometry for CH_4 and N_2O eddy covariance measurements" *by* P. S. Kroon et al.

Anonymous Referee #4

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The manuscript presents a promising new system for simultaneous measurements of CH4 and N2O fluxes by eddy covariance method. However, the structure of the manuscript is sometimes confusing. For example, the sample flow rates are first discussed on page 1142, lines 28-29, and then again on Page 1145, lines 5-6. Also, the effect of humidity fluctuations on fluxes is first discussed on Page 1151, lines 4-25, and then again on Page 1152, lines 17-23. I also have several comments on the both content and style, as listed below.

Page 1139, lines 13-15: Actually more important than the sampling frequency, is analyzer response time. If the instrument response time due to e.g. mixing in the measurement cell would be many seconds it does not help to sample the output signal at 10 Hz. On the contrary, one can sample the output at a much lower rate that 10 Hz

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(~0.1-1 Hz) as long as each measurement represents concentration during very short time period (<1 s) (see e.g. Dabbert et al., 1993). This distinction between sampling rate and response time is very important and must be cleared out.

Page 1142: A schematic of the measurement system would help the reader to follow the description.

Page 1143, lines 15-16: Equation (1): Storage change term goes to zero for stationary conditions.

Page 1145, lines 10-13: Here a figure showing a comparison of CH4 and N2O, and temperature spectra could show the reader the effect better. This analysis would also show empirical evidence on the high frequency damping.

Page 1145, lines 19-23: The rise in the very high frequency end of the heat flux ogive looks suspicious. As both ogives are normalized by the respective flux they both approach unity in the high frequency end, and possible high frequency damping may not be obvious. An other method, proposed by Ammann et al., 2006, is to fit the ogives to yield the same form in lower frequencies.

Page 1146, line 1: What kind of vertical concentration profile was assumed in the calculation of storage change term?

Page 1147, lines 14-21: Isn't sigma variance of the signal rather than stability?

Page 1148, lines 14-17: The instrumental drift in the timescales longer than the running mean filter timescale do not affect the flux as this effect is filtered with low pass filter. This could have an effect when using linear detrending and especially block averaging. The drift can also cause underestimation to the fluxes.

Pages 1148-1149, lines 24-4: The description of the derivation of the effect of low pass filtering to the fluxes is weak and should be rewritten on a more explicit way. Currently it is not possible to follow the reasoning. Also it is not clear what the Figure 2 represents. This should be written explicitly in the text and the figure caption should be modified.

Now the captions of Figures 1 and 2 are nearly identical, even though the figures are not. Are ogives presented in Figure 2 just ogives with integration started from high frequencies?

Page 1149, lines 20-21: Why is the low-high calibration factor the best way to estimate the NEE. I don't see any reasoning in the paragraph that justifies this statement.

Page 1150, lines 1-3: The zero offset does not affect fluxes as the average is subtracted to yield c'. That is unless the zero offset affects the gain in the calibration.

Page 1150, lines 9-13: Was any u* filtering to remove fluxes measured during insufficient turbulent mixing utilized?

Page 1150-1151, lines 15-3: Indeed, the negative fluxes might be due to the random uncertainty. Before stating that these fluxes are interesting, one should estimate the uncertainty of each single flux value using the Wienhold method. Generalizing the flux uncertainty calculated only for one single half an hour period does not prove your point. Especially the uncertainty for N2O, presented in line 24 flux seems extremely low. Also if the data is not filtered using u* criteria you check if these negative fluxes occurred during weak turbulence.

While most covariance functions shown in Figure 4 look reasonable the one in lower right panel is suspicious. I guess lag time in this case is taken to be around three seconds? I so, why should the lag time be much different from that in other panels? Is that due to variations in flow rate? Are the lag times for CH2 and N2O the same?

Page 1151, lines 10-21: Why Webb correction for latent heat flux was not conducted if the latent heat flux data was available?

Page 1152, lines 17-23: One could assume that the high frequency loss of water vapor variations is in the same range than for CH4 and N2O and therefore not very large. Thus a best estimate for the correction could be calculated and should be applied.

Page 1153, lines 5-6: Here again is the requirement of instrument response time con-

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fused with sampling frequency.

Table 2: What does V˜80 mV and V˜180 mV mean? This should be stated in the Table caption.

Some general comments:

SI units should be used, and thus Torr should be converted to Pa.

Several misspelled English words, such as: Oktober, should be October. Week values, should be weekly values.

Minor comments:

Page 1138, line 19: These global warming potentials depend on the time horizon as both CH4 and N2O are chemically destroyed in the atmosphere. Therefore it should be mentioned that these are GWPs for 100 time horizon.

Page 1140, lines 3-4: "(Coord. N 52deg01'15" E 4deg01'17")" I believe correct expression would be "(52deg01'15"N 4deg01'17")". Same applies to Table 1.

Page 1140, line 9: I am not sure if polder if an English word. Maybe it should be shortly described, as many readers may not be familiar with this expression.

Page 1144, lines 7-15: The method described here acts as low pass filter removing the effect of low frequency eddies on the flux. Other commonly used averaging methods, block averaging and linear detrending, should maybe be also mentioned.

Page 1143, lines 19-20: This kind of evaluation of raw data would be very impractical for long term flux measurements. In this kind of measurements spikes are usually removed automatically. Maybe this should be mentioned in the manuscript.

Page 1151, lines 4-9: "non-performed Webb-correction" is not very elegant expression. I would rather write something like "Negative fluxes could be caused by density fluctuations due to temperature and humidity variations".

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Page 1152, line 8: Two values are given but not specified. I guess the first one is CH_4 and the second N_2O.

Page 1153, line 14: "A first indication..." Does not sound like good English to me.

Dabberdt, W. F., Lenschow, D. H., Horst, T. W., Zimmerman, P. R., Oncley, S. P. & Delany, A. C., 1993: Atmosphere-surface exchange measurements. Science, 260, 1472-1481.

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