

Interactive comment on “Comparison of two closed-path cavity based spectrometers for measuring air-water CO₂ and CH₄ fluxes by eddy covariance” by Mingxi Yang et al.

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Received and published: 21 September 2016

General Comments

In “Comparison of two closed-path cavity based spectrometers for measuring air-water CO₂ and CH₄ fluxes by eddy covariance” the authors compare the performance two commercially available gas analyzers (Picarro G2311-f and LGR FGGA) used to measuring CH₄ and CO₂ fluxes. As a research article on the intercomparison of gas measurement instruments this manuscript falls squarely into the scope of AMT. As stated in the article, instrument performance over terrestrial sites does not necessarily apply to ocean studies as the fluxes of CH₄ and CO₂ over the ocean are much smaller, justifying a study looking at air-sea fluxes. The findings will be of interest to anyone at-

tempting to make such field measurements, specifically for deciding which instrument is best suited to their needs. Paper provides a thorough description of the Picarro and LGR in action, highlighting certain technical characteristics of which future users should be aware (e.g., lack of line broadening correction applied internally on LGR, positive offset in dry CO₂ mixing ratio in LGR, etc.).

The findings are scientifically sound, following the necessary instrumental and processing techniques that have been developed over the past couple decades. The stationary platform avoids need to correct for motion. Intervals were filtered for non-stationarity. Estimates for high frequency flux loss were performed and magnitudes compare well with previous studies. Flux quality is verified by showing the mean cospectra for CO₂ and CH₄ fluxes against the momentum flux cospectra. Drying the airstream to at least one of the systems (as was done here) was important to rule out spurious CO₂/CH₄ flux due to water vapor. The paper was thorough, using different methods to estimate performance when possible (e.g., theoretical and empirical estimates for flux detection limits, filter function and ogive estimates for high-frequency loss, etc.).

With regard to the main thrust of the paper (comparing the performance of measuring flux from the two instruments) the results support the conclusions. The interpretations on the effectiveness of the numerical corrections for water vapor are not as strongly supported by the data. The paper should acknowledge the limitations of the dataset for specifically assessing the true impact of water vapor. An in-depth assessment of the effectiveness of the numerical corrections was not required for this paper, and could be the subject of another study. Some other minor issues/comments are discussed below.

Specific Comments

Line 101 – Inner diameter is more useful if anyone wanted to run the numbers (e.g., Reynolds number, expected flow rates, etc.).

Line 107 – It's unfortunate that the flushing time is slower than 10Hz. It would be good to know how much improvement the LGR might see at faster flow rate.

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Line 127 – Not clear how you know that the LGR only applied dilution correction (and not line broadening) internally.

Line 177 – I don't understand how Figure 3 shows that "cross-sensitivities between CO₂ and CH₄ vs H₂O fluxes are well accounted for by Equations 1 and 2." It seems to show only the impact of the corrections, not real information about how water vapor is influencing the flux measurements. The absence of "additional non-linearity" just means that the numerical corrections are generally linear (i.e., coefficients b and d are effectively zero). To determine the impact of water vapor would require having simultaneous flux measurements using identical instruments, with one dried and one undried. In Figure 2 it's clear that the difference between the LGR numerically corrected fluxes and LGR ambient fluxes is small compared to the difference between LGR and Picarro fluxes. Because the difference between the instruments is greater than the numerical correction for H₂O, it is difficult to assess the true impact of water vapor on the measurements. Presumably the impact of water vapor on the measured fluxes is not large due to the attenuation of the water vapor fluctuations in the long tube line (as stated in lines 147, 211) – the larger lag time in H₂O should cause any real correlation between fluctuations in H₂O and CO₂/CH₄ to become uncorrelated. But I would be wary of concluding that the numerical corrections are functioning properly when there is no actual way to verify with the dataset.

Line 198 – Same general comment as above – no clear proof that water vapor is not a factor.

Line 218 – Interesting that the scatter in hourly CO₂ flux from LGR was only 50% higher than Picarro, while Fig. 7 shows order of magnitude greater variance for LGR compared to Picarro. Any idea why this is? Is this just due to averaging?

Line 286 – While not large, tubing can cause some high frequency attenuation of CO₂ fluxes (Goulden et al. 1997, Ibrom et al. 2007). With an 18m tube it may not be insignificant. Of course, for the comparison of the two instruments it doesn't really

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matter, since both will be measuring the same air.

Line 289 – It's not clear to me why reducing the flow rate will show response time. Reducing the flow of the calibration gas should not change the mixing ratio of the gas in the chamber. How does this work?

Line 305 – Does the Picarro (undried) show the same high frequency loss in the spectra as Picarro (dried)? If so, the high frequency loss may be attributable to the long tube line, not just the nafion.

Based on the shorter lag time here than for the maximum covariance lag used in the CO₂/CH₄ calculations it appears that the time constants were found using a short bit of tube from the cal gas tank - through the nafion - then through the gas analyzer? Future measurement campaigns from this site would benefit from inlet testing (timed release of calibration gas in front of the inlet tube). Time constants for the whole system could be obtained, and used to estimate an overall high frequency flux loss. Also lag times could be measured directly, and be used to verify the maximum covariance lags.

Technical Corrections

Line 55 – is rigorously and quantitatively confirmed was rigorously and quantitatively confirmed

Line 108 – “LPM (at atmospheric pressure)” same as “SLPM”, which was the notation used in the previous paragraph. Why not stay consistent?

Line 187 – Stationarity Stationarity

Line 218 – In this one sentence you report CH₄ first, then CO₂. Every other time it's CO₂ followed by CH₄ (even the next sentence). I would keep it consistent.

Line 234 – Switched from mmol m⁻² d⁻¹ to mmole m⁻² d⁻¹ for this section. Keep consistent.

Line 273 – (Kort et al. (2012) (Kort et al. 2012)

Line 351 – Unnecessary “)” at end of CD10N equation.

Line 358 – Water vapor correction based on Schotanus et al. (1983)?

References

Schotanus, P., F. T. M. Nieuwstadt, and H. A. R. de Bruin, 1983: Temperature Measurement with a Sonic Anemometer and its Application to Heat and Moisture Fluxes. *Boundary- Layer Meteorol.*, 26, 81–93.

Goulden, M.L., B.C. Daube, S.-M. Fan, D.J. Sutton, A. Bazzaz, J.W. Munger, S.C. Wofsy, 1997: Physiological responses of a Black Spruce forest to weather. *J. Geophys. Res.*, 102, 28,987–128,996.

Ibrom, A., E. Dellwik, H. Flyvbjerg, N. O. Jensen, and K. Pilegaard, 2007: Strong low-pass filtering effects on water vapour flux measurements with closed-path eddy correlation systems. *Agric. For. Meteorol.*, 147, 140–156.

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