Atmos. Meas. Tech. Discuss., 8, C3746–C3751, 2015
www.atmos-meas-tech-discuss.net/8/C3746/2015/

Atmospheric Measurement Techniques Discussions
Open Access

Interactive comment on "Optimization of a gas sampling system for measuring eddy-covariance fluxes of H₂O and CO₂" *by* S. Metzger et al.

S. Sargent

ssargent@campbellsci.com

Received and published: 6 November 2015

General comments

The paper seems generally too long and a bit disorganized. I offer three general suggestions to improve overall readability:

1) There are several sections that intermingle introduction, methods, results and summary. In many cases this leads to redundancy, but in others, specific information is hard to find because it is not in the expected section.

2) Discussion of preliminary testing and analysis of prototypes going back to 2008 adds

little insight, and dilutes the more important evaluation of the design elements tested in this study. CFD is mentioned a few times, without providing details or results. This provides little useful support for the conclusions of this paper, and could be omitted. Discussion of the laboratory water ingress tests could also be omitted or significantly edited.

3) The "NEON requirements under evaluation" cited throughout the paper are presented in the supplemental material, with no explanation of their origin. If these requirements have been reviewed/approved/published, an appropriate publication should be cited. If the publication of this paper is intended to constitute peer review of these requirements, then this should be clearly stated. A brief review of these requirements shows that some are not strictly possible (4.1626), are redundant (4.2006), or simply restate the manufacturer's recommendations (4.1618). In general the detailed nature of many of these requirements seem to simply describe the hardware that has already been selected, rather than to guide the infrastructure design based on scientific goals. Where the requirements have not been met, they have been dismissed (page 11012, lines 8 through 23). The readability of this paper could be improved by removing these references to NEON requirements.

Laboratory Frequency Response Testing

The laboratory frequency response testing suffers from several errors. It is not clear to what extent these errors are mistakes in the presentation of the material in the paper, nonstandard usage of common terms, or actual errors in the analysis.

The first error, on page 10991 line 15 states: "…the Fourier Transform of the actual time series' time derivative is the system transfer function (Truax, 1999)." This is not correct. The Fourier transform of the derivative of a step function does give the transfer function, but this is not true for a square wave. A text on signals, communication, or control theory, or a math text that introduces the Fourier transform might be a more appropriate reference.

I am confused by the usage of the terms “transfer function” and “power spectra” in this paper. The transfer function is defined as a ratio of input to output, normally meaning amplitude spectra, not power spectra. This is correctly introduced on page 10989, line 26, but the term “power spectra” is then used incorrectly in page 10990 line 2 (and throughout the paper). It is not clear whether the transfer functions presented in this paper are given as ratios of amplitude spectra or power spectra. This should be corrected throughout, to conform to the normal convention. If the intent is to use the term “transfer function” to mean a ratio of power spectra, this nonstandard usage should be clearly emphasized.

A related issue is the half-power frequency, also known as the cutoff frequency, corner frequency, or bandwidth. This is normally defined as the frequency where the transfer function falls to the square root of $\frac{1}{2}$ (0.707), not 50%. It is not clear whether the half-power points presented in this paper are correct or not. It appears that the transfer functions given in fig 6 may be the (correct) ratio of amplitude spectra, but that the “half-power frequencies” given in figure 7 appear to be the (incorrect) 0.50 point on these transfer functions. I encourage the authors to conform to the conventional usage of the half-power frequency.

The equation given for the transfer function (equation 6) is the square of the function normally used for a transfer function. It would, of course, be correct if the “transfer function” is based on power spectra, but this would be nonstandard usage of the term (and should be highlighted). A more helpful reference for this transfer function might be Moore (1986), (see his equation 2), or any text that introduces first-order, linear time-invariant (LTI) systems.

Another serious mistake involves equation 7 through 10, and begins at page 10993 line 5: “Resistor-capacitor theory…” These equations are not correct, and all of the analysis based on these equations is invalid. An example of a more appropriate reference is (see equation 9):

W.J. Massmanm, A simple method for estimating frequency response corrections for eddy covariance systems, Ag For Met 104 (2000), pp 185-198

Equations 7 through 10 are used throughout this paper. The negative frequencies shown in figure 4 are the most obvious examples of incorrect analysis, but all results and discussion based on these equations is incorrect.

Page 1103 line 26: “The frequency response of the combination of all elements in an enclosed IRGA EC system is shown in Fig. 2…” This statement is misleading, because the results in figure 2 do not include the IRGA itself. This should be clarified.

I thank the authors for providing raw data in the supplement. The lab test CO2 fast data set is well labelled and organized. I’ve processed these files as a step-response test using these simple steps:

1) Extract 8 s (400 samples) centered on the first rising edge

2) Calculate the derivative

3) Calculate the amplitude spectrum

4) Divide the spectrum by the first point (at 0 Hz, representing the mean of the time series)

This data set seems to have very good signal-to-noise ratios, with no evidence of offset or drift over time. This makes any filtering, averaging, detrending, or apodization unnecessary. I’ve calculated the transfer functions for individual GSS components by dividing the individual transfer functions by that of the IRGA with no GSS (similar to your equation 4, but with no additional normalization required). I’ve compared these transfer functions to simple mixing-volume models, with reasonable results:

1) The effect of the rain caps match a first-order LTI model (ideal mixing volume) with the time constant approximately equal to the physical residence time.

2) The combined effect of the tube and filter follows Massman’s equation 9.

3) The combined effect of the rain caps with the tube and filter also follows Massman's equation 9.

Unfortunately, on page 10991, lines 9 through 14, the authors dismiss this (correct) step-function approach. I would encourage the authors to revisit the analysis of the lab frequency response data, and I am happy to provide more details of my analysis if desired.

Laboratory measurements of pressure drop

This test is interesting and useful. A minor suggestion: it is very difficult to distinguish all of the curves in figure 3. It would be helpful to see a table of pressure drop at some example flow.

Field Frequency Response tests

I applaud the authors for investigating water frequency response as a function of tube heating and relative humidity, etc. This is very difficult to characterize, but very important.

The paragraph beginning on page 11007 line 22 states that "…aliasing…was not observed." The bandwidth setting indicates the LI-7500 was configured to respond to frequencies above 5 Hz, and an inspection of figure 6 makes it appear there must have been some signal all the way to 5 Hz (and presumably, beyond). This would seem to guarantee there was aliasing in the LI-7500. Perhaps this could be clarified.

This paragraph also briefly addresses the problem that the LI-7500 is exposed to significant air temperature fluctuations that are partially damped by the LI-7200 GSS. Dismissing this problem by relating it to turbulent flux seems irrelevant. The LI-7500 H2O spectrum was used to normalize the LI-7200 H2O spectrum to derive a transfer function. If the two analyzers measure different signals, this transfer function will be affected.

A related issue is the statement in the caption for figure 6, indicating only data with

sensible heat flux > 50 were used. It would seem more appropriate to screen data to use time periods with low sensible heat flux and/or high latent heat flux. Perhaps the authors could comment on this, and the body of the paper would seem a more appropriate place than the figure caption.

Page 11007 line 11 compares the system transfer function (with f50% incorrectly defined) to the laboratory result which characterized the GSS only (the IRGA frequency response was normalized out). This is not a valid comparison.

The paragraph beginning on page 11008 line 18 discusses the CO2 frequency response, but there seem to be no results given for CO2. Perhaps the authors could comment on this.

Summary and Conclusions

Page 11011, line 20: the paper states: "To our knowledge, this is the first study to show that large rain caps can limit overall system frequency response." I don't understand this claim, given the fact that in eight places this paper cites De Ligne, et al. (2014) which clearly states this conclusion.

Also, now that these researchers have published their results in AMTD, a reference should be added:

Technical note: Dimensioning IRGA gas sampling system: laboratory and field Experiments M. Aubinet, et al. Atmos. Meas. Tech. Discuss., 8, 10735–10754, 2015 www.atmos-meas-tech-discuss.net/8/10735/2015/ doi:10.5194/amtd-8-10735-2015

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 10983, 2015.