

***Interactive comment on “Optimization of a gas sampling system for measuring eddy-covariance fluxes of H<sub>2</sub>O and CO<sub>2</sub>” by S. Metzger et al.***

**Anonymous Referee #4**

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General comment In the last years enclosed path analysers became a valid alternative to open-and closed path systems for eddy covariance (EC) measurements. The comparison with open path analysers showed that the air sampling system has a large impact in calculated fluxes. It is therefore relevant and appreciable that a focus is given to this previously neglected part of the EC measurement system. The paper analyses the effect of the different parts composing the sampling system, including rain cap, tube and filter. It presents results from laboratory and field testing. Writing is fluent and the objectives are within the scope of AMT. In the current version of the paper there are some formal aspects to be improved. Findings should be better referenced to previously published peer-reviewed papers, and not to somewhat confusing ‘NEON

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requirements' or presentations to conferences, like 'De Ligne et al., 2014; Metzger et al., 2014'. Also, all the experimental conditions should be clearly defined, otherwise it is better to avoid to report the findings, as in case of observations dating back to 2008. A separate discussion section would be helpful in order to present and discuss the results from other experiments. Specific comments Title: I recommend adding '...with enclosed path analyser'. Abstract. I must say that I'm not fully convinced by the finding that 4 Watts applied to the sampling system are recommendable for all circumstances. Possibly, this indication should be limited to the studied experimental conditions. In fact, temperature requirements to reach 60% air relative humidity change according to the distance from dew-point, which depends on relative humidity and temperature itself. Energy needed possibly also changes with wind speed and consequent thermal dissipation of the sampling system. I add that an evaluation of the impact of heating close to the sonic is missing in literature to my knowledge, and it is possible that energy budget and Bowen ratio estimates become biased. These issues should be at least mentioned. P10988: 'p=101.325 kPa'. Was the pressure controlled? At the elevation of Lincoln, NE, USA, the air pressure should be around 97 kPa. P10989: Please avoid to present the results coming from undefined experiments. P10990 L3-4: Probably, considering that you are talking about power, you should define the f50% as the frequency value at which your signal has a loss of -6dB. P10991: 'H2O concentration'. I believe that the term 'concentration' should be reserved to volumetric values, so possibly it should be better 'H2O mole density' here. P10993: Please check equations 7-10. P10993-10994. Results obtained with CFD should be better presented and discussed or otherwise omitted. P10995: Please introduce the acronym 'SDM'. P10997: 'the package...is available upon request': I recommend to report what is freely available only. P10999: Reported values of R2 and p-values are quite unusual. P11010: What is a 'virtually improved system'? P11001: 'ICOS conducted...'. Please report what you found in your defined experiments here (Results), and place in the introduction or discussion (to be placed separately) other observations. Sometimes placing all together is functional, but here it seems confusing. P11012: '...a 10% increase...',

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with respect to 70 cm, so 77 cm has 1/3 decrease in frequency response, and a 10% decrease has no effect? I'm not convinced by these thresholds, 4W, 70 cm, since such thresholds are unusual in physics. Figure 4: I would expect points in the graph, not lines. Minor notes on writing P10986: 'exceed several 10%' is unclear. P11001: Particular->particulate (?).

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

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