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Comment

Interactive comment on “Technical note: Dimensioning IRGA gas sampling system: laboratory and field experiments” by M. Aubinet et al.

M. Aubinet et al.

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Thank you very much for your comments, Thomas. Our replies are in the text below.

Comment : The aim of your paper is obviously a standardization of the eddy-covariance system (p. 10736, line 25ff). I disagree that such standardization is useful and I am happy that we have a good selection of gas analyzers and sonic anemometers. The choice of instrumentation depends very much on measuring height, canopy height, climate zone, wind roses, footprint etc., and you can find for all conditions an optimal instrumentation. Maybe ICOS found a standard instrumentation for a certain climate zone and measuring height, but for the reader it would be important to be informed of

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what is ICOS specific and what is of general importance.

Reply : We must specify that it has never been the aim of this paper to propose and discuss about standardization (this is rather the aim of ICOS protocols). Even if the experiment was made in the frame of the elaboration of these protocols, the paper's goal is quite different. By doing the experiment we indeed realize that, contrary to what we thought, the impact of filters on GSS frequency response was minor while those of rain cap was huge. We thought that this finding deserves publication and goes beyond any standardization procedure and potentially concerns all closed or enclosed gas analyzers used in eddy covariance systems. It's possible that some sentences in the introduction could have misled the reader. We changed them in order to clarify

Comment : Your paper has a relatively long theoretical part with many equations. I cannot see that the result part has any relationships to the theory. Of course, you have included Eq. 1 in Fig. 3 and another equation (which one?) in Fig. 4. But what are the settings? Eq. 5 does not have the typical form of a transfer function. Some more information is necessary here.

Reply : We agree with this comment and have already reworked and rewritten the theoretical part (notably, in response to comments by the other reviewers). Especially, the development of the Massman and Ibrom equation was not very useful, as tube attenuation appeared finally of very limited impact on this system. By testing all theoretical transfer functions (Moore + Massmann and Ibrom), we realized that the most limiting process in the lab (I mean when the IRGA works without filters and caps, with a 15 l min⁻¹ flow rate) is sensor line averaging. We thus introduced it in the theoretical part. It explained better the experimental relation found between fco and flow rate (see new figure 4 put in attachment to the reply to reviewer 1).

Comment : It is a great pleasure that a similar paper from NEON was published at the same time in the same journal (Metzger et al. AMTD-8-10983-2015). The NEON group obviously knows your research (reference to the conference presentation De

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Ligne et al., 2014). The ICOS group probably did not have information regarding the conference paper by Metzger et al. (2014). Both groups should make a reference to the other article (AMTD or better AMT with support by Copernicus). For the reader it is interesting and important to compare different laboratory and field experiments. There are two similar figures (ICOS Fig. 3 and NEON Fig. 3 and ICOS Fig. 4 and NEON Fig. 2). It would be great if both groups could communicate with each other and include in their figures a comparison with the findings of the other group, maybe for the Swagelok FW 2m filter.

Reply : We agree this is a nice opportunity. We knew of course that the LI-COR team redesigned their rain cap following our results (the De Ligne et al poster) and, in addition, Stefan Metzger participated to the elaboration of the ICOS protocols. However we learned they wrote a paper on the system only some days before submitting ours. That's why it is not cited in the former version. It seems clear that comparison and cross citation would improve both papers. We added citation to this paper in the discussion.

Small remarks: p. 10740, line 4: (GSMA) include "University of Reims" Reply : Done

p. 10742, line 23: Include the reference: Kollé, O. and Rebmann, C. (2007) Eddysoft - Documentation of a software package to acquire and process eddy covariance data. Max-Planck-Institute for Biogeochemistry, Jena, 88 p. Reply : Done

p. 10746, line 16/17: If the article is not yet published, give the journal etc. where it is "in press" Reply : The IRGA protocol has been accepted since about two years by the ICOS Monitoring System Assembly and should thus be considered as ready for publication (something like "in press"). However, it is planned to publish all ICOS protocols together in a book and all depends on the editors and on the protocol writers. I thus could not give more details at the moment. I hope this will be clarified for the final publication.

Figure 2: The relationship between the two x-axes is wrong (seconds and Hz) Reply : The frequency given in the figure refers to the concentration modulation frequency by

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the system, which is incremented by one Hertz at each step. It is without relation with the time axis. We add a sentence in the legend to clarify.

Perhaps the following paper is also a relevant reference for you: Moravek A., Trebs I., and Foken T. (2013) Effect of imprecise lag time and high-frequency attenuation on surface-atmosphere exchange fluxes determined with the relaxed eddy accumulation method. J Geophys Res: Atmosph. 118:10,210-10,224. Reply : Thank you !

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