

Interactive comment on “Eddy covariance for quantifying trace gas fluxes from soils” by W. Eugster and L. Merbold

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Overall: No doubt, this is certainly a very well written and referenced manuscript, which gives a handy and easily read introduction to the eddy covariance techniques. In terms of the measuring technique and application of EC, it contains a lot of useful and relevant consideration on how and where this could be useful for scientists outside the strictly micrometeorological community. The ms contains the basics of transport in the inertial sub layer including the most relevant mathematical parameterization explained in plain language, which can be understood also by scientists not working directly with atmospheric transport as their main topic.

When I am a bit hesitant still, it is due to some of the same consideration as MM has

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stated in his review namely, is there a need for a paper of this type ? Despite the positive aspects I've mentioned above I do not see that the ms adds to any specific part of our knowledge about EC and as such the ms does not contain any really new findings or results. One may argue that if a paper on EC technique for soil scientists is needed, why not then one for oceanographers or microbiologists ? or to bring it a bit further, a simple introduction to lab analysis of soil samples for micrometeorologists? As such the ms is not a typical contribution to a scientific journal and should maybe not be judged by the same standards. From my perspective this is more of an editorial decision, and if the verdict is, that a ms like the present one is useful as an introduction, which can then be studied more closely in some of the excellent text books on the subject, I have no objections.

Despite that I find, that the ms contains the essentials about eddy covariance measurements I miss a bit more on the constraints. I would find it useful to have a quantitative evaluation of, where the technique is applicable, or in other words; under which conditions would you expect to obtain useful results? Especially when considering bare soils with little organic content (p.543 L. 1), fluxes are often very small and measurements would to a relatively large degree be affected by uncertainties and instrumental noise. Also e.g. uptake of CH₄ in soils is not very likely to be detectable by standard EC techniques and can in my opinion not necessarily be considered superior to chambers. Some quantification on how to determine sensitivity limits in this context I would consider useful information in a paper like the present one. Also, I think it could be appropriate to clearly point out, that though EC measurements are continuous, variations in the measured variables may reflect heterogeneity in the measured surface or changes in the footprint, rather than temporal variations in the observed parameters.

Specific: I have very little here and see no important flaws in the manuscript.

Paragraph 2.2.2 : could a drawing be helpful to support the text here? P552 L1-5: it is unclear to me what is meant here, consider rephrasing. P553L25- Some discussion of the problem of obtaining good quality data during night might be appropriate here.

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Paragraph 4.2.1., Though fascinating, EC techniques does not strike me as the best choice for manipulation experiments, which is why I find this part a bit lengthy. You could consider to shorten it or focus more on land-use or management techniques.

P559L21: Do we need the considerations on aircrafts here?

P561L8-15: likewise, isotopic EC flux measurements are certainly not for newcomers.

Interactive comment on SOIL Discuss., 1, 541, 2014.