



Interactive comment on “A new disjunct eddy-covariance system for BVOC flux measurements – validation on CO₂ and H₂O fluxes” by R. Baghi et al.

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We thank the editor for the constructive comments which helped to improve the paper. The comments have been addressed in a point-by-point manner, as detailed below. The original Editor's Comments are reproduced in Italics. The Author Responses are under the headings of **AR**.

1) *In Sections 4.5 and 4.6 (and also in the Abstract) the different flux methods (EC, DEC, SDEC) are only compared in terms of correlation. This is not sufficient! The correlation coefficient is not a measure for quantitative (1:1) agreement. It is only a mea-*

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sure for linear dependence between two quantities (allowing biases and slopes/ratios different from 1). Please provide more specific information on the quantitative agreement between the methods, e.g. slopes and offsets of a linear regression analysis (with corresponding uncertainties) or similar statistical analysis.

AR: This information will be added to the manuscript.

2) P4158,L22: *This statement is not in full agreement with the corresponding statement in the conclusions "...of the same order but rather lower than the values reported in the literature..."*

AR: Appropriate modifications will be done to the abstract and in section 5.2.

3) P4173,L8-9: *I do not understand here why the underestimation of the DEC latent heat flux can be "attributed to a different response of the two analysers to H2O fluctuations". DEC analysers do not need the same fast time response as EC analysers. Please explain this issue in more detail.*

AR: We are here discussing about a difference in the calibration of the two analysers and not about their response time. For example if the calibration of one analyser has drifted during the experiment, or if the two analysers have different response to parameters like temperature, etc., this could have an impact on the estimated flux and partially explain the discrepancy observed between the two methods on latent heat fluxes. Also, as discussed with referee 2, a dampening effect on water vapor can occur and affect the measurements. Precisions will be added to the manuscript in section 4.6.

4) P4173,L10-12 *I do not understand the explanation why the two systems are not inter-calibrated for the period of the field campaign. This would allow to distinguish between (i) simple differences in calibration and (ii) problems in the performance of the DEC/DES system.*

AR: The two systems were calibrated separately and not inter calibrated to stay as

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close as possible to real field conditions. In this way the measurements were independent from each other. Furthermore, a direct comparison of concentration measurements with the two analysers showed that it was not possible to determine a unique linear coefficient between them probably because they were affected by external environmental parameters (temperature, etc.).

5) P4177,L7: *This is usually called a "standard emission rate"! The formulation "reduced... emission rate" is misleading here. Please use a clear and consistent denomination for isoprene fluxes and emission rates that are normalised to standard conditions throughout the manuscript!*

AR: The Editor is right. This will be modified in the revised version of the manuscript.

AR: Language and formulation suggestions have been taken into account.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 4157, 2012.

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