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Atmospheric
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Interactive comment on "A disjunct eddy accumulation system for the measurement of BVOC fluxes: instrument characterizations and field deployment" *by* G. D. Edwards et al.

Anonymous Referee #1

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The paper describes a development and deployment of a disjunct eddy accumulation device for VOC flux measurement. While the method has been described before it has not been widely used and thus the publication of the description of the system is justified. The paper also gives some preliminary results on BVOC fluxes at UMBS field station.

The paper is well written. I have concern on a couple of issues on the theory of the DEA, which are of fundamental nature. These must be addressed before the paper can be accepted for publication in AMT. In addition I have few minor comments also listed below.

Major comments

Equations (7) and (9) are incorrect. In the first term on the right-hand-side of the Eq. (7) should be average of absolute values of vertical wind speed, not the absolute value of the average (see Turnipseed et al., 2009, Eq. (16)). I hope this is just a typo and a correct equation is used for flux calculations as these formulation lead to very different results. There is the same error in the first and second terms on the right-hand-side of Eq. (9). However, Equation (2) is correct in this sense.

The authors use a dead-band similar to the one used in most REA systems in their DEA system. While this can be justified as the authors have done, the equations derived for the disjunct (true) eddy accumulation (Eqs. (2) and (7)) are not strictly valid for this sampling strategy. Instead an empirical correction factor, similar to that of beta-coefficient of REA, should be applied to account for systematic error due to the use of deadband. As the authors have simulated the effect of the deadband on fluxes (page 2719, lines 6-16), the correction should be easy to apply. This correction seems to be smaller, i.e. coefficient closer to unity, than that for the REA as EA weights more eddies with higher vertical speeds.

Page 2716, 25-27: "...w_{min} is too conservative...collection of of disjunct samples that are not truly representative of the turbulent flux". I disagree here. In order to the equations used in the (true) eddy accumulation the samples need to be collected in a random manner as related to turbulence. If we systematically reject samples based on either the value of vertical wind speed or trace gas concentration this can lead to systematical error in the flux. In the case of this work the rejection of samples with low vertical wind speed leads to systematic overestimation of the flux.

Page 2718, lines 18-20: "As no such empirical correction coefficients are used in the calculation of the DEA flux, we assume no correction factor is needed". This is actually a wrong assumption due to the differences in the true DEA sampling strategy and the sampling strategy applied here, as explained already above.

Page 2719, lines 16-17: "The 0.66σ threshold used in CABINEX saw a difference of 8.6%. As shown previously, this difference is small compared to the total flux uncertainty". This comparison missleading as the authors compare systematic bias to the random uncertainty.

Minor comments

Page 2706, line 24 – page 2707, line 3. "As a large part of flux...appropriate integral timescale". This citation to the paper by seminal Lenschow et al., (1994) is very often copied to the introductory parts of the manuscripts utilizing disjunct eddy methods. However, it is only half-truth. With the many applications of DEA and DEC the integral timescale is actually shorter than the sample interval. In these cases the random error depends on sample number (Rinne and Ammann, 2012) and with sufficiently large sample number we can reach the same accuracy.

Page 2707, lines 3-5: "...disjunct eddy accumulation...is a direct flux measurement..." The authors are correct in that the (true) eddy accumulation is direct flux measurement. However, the approach taken here, which employs dead-band, is not strictly direct flux measurement, as the derivation of the EA equation does not apply to it.

Page 2712, lines 7-8: "...ozonolysis lifetimes for isoprene are several order of magnitude smaller than for OH reactions..." "smaller" should probably read "longer".

Page 2720, lines 11-12: "2.09 ($\pm$ 0.19)" and "1.96 ($\pm$ 0.43)". The notation used by the authors has too many significant digits, as the last decimal is well below the uncertainty. More proper notations would be "2.1 ($\pm$ 0.2)" and "2.0 ($\pm$ 0.4).

Section 7: How was the value of I_s determined?

Page 2724. Lines 15-19: "...there is quite close correlation between the variations in the temperature diurnal profiles and the corresponding monoterpene concentrations... ...This lends to confidence that the DEA method was able to capture the changes in the driving forces for ambient monoterpene emission". I am not sure I understand how

the fact that the measured concentrations correlate with temperature lends confidence to the measured fluxes.

Page 1726, line 9: "...BVOC production rates..." should read "...BVOC emission rates..." as Eq. (14) does not refer to monoterpene production, but implicitly to emission from storages.

Figures (7) and (9): While the model produces a diurnal cycle of monoterpene concentration with maximum at night, the measured concentrations may even suggest afternoon maximum. The previous type of diurnal cycle is commonly observed at sites where temperature driven emission from storage pools of e.g. conifers dominate (Hakola et al., 2000). The latter on the other hand has been observed at sites where temperature and light driven de novo emission dominate (Zimmermann et al., 1988; Rinne et al., 2002). Thus this small discrepancy in the model and observations is interesting.

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