"A Disjunct Eddy Accumulation System for the Measurement of BVOC Fluxes: Instrument Characterizations and Field Deployment" by G.D. Edwards *et al*.

Response to Reviewer 1

We thank the reviewer for their feedback and insightful comments that we feel have helped us to strengthen the paper.

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 typos in these equations were corrected.

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 dead-band. As the authors have simulated the effect of the dead-band 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.

We agree, in part, with the concerns that the reviewer has made. To our knowledge, there is no previous report of the use of a dead band in DEA measurements and DEA itself is still as flux measurement technique that is utilized infrequently. Therefore, we feel it is necessary to derive both the equations of (true) DEA measurement theory first in order to then ascertain the effect off application of the dead band sampling method to these equations. The correction factor, as similar to the β constant used in REA, was indeed calculated for the dead-band DEA sampling system described here. As noted by the reviewer, this was fully described in the text and determined to be *c.a.* 8% for the dead-band used during this field campaign. All data were corrected for by inclusion of this factor.

Page 2716, 25-27: "...wmin 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.*

We agree that this is indeed one of the consequences to using the dead-band approach. As reviewer 2 describes elsewhere, the issue facing the operator is that, as Δc increases, does this trade off lead to significant improvement in the precision of the concentration analysis? It would appear that in this field deployment of the DEA instrument, the trade off did not yield significant benefits. Owing to the issues with mass flow control we described in the text it combined with the fact it was our first opportunity to test the DEA sampler in the field, a very wide bandwidth was chosen which, as described above leads to an overestimation of flux by ~8% which was corrected for in the data analysis.

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.

Our intent was to show that no additional corrections beyond the ~8% error in flux was needed. We recognize the text is not clear in this and have altered the narrative to clarify this issue.

Page 2719, lines 16-17: "The 0.66_ 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 misleading as the authors compare systematic bias to the random uncertainty.

Again, this statement could be misleading and we have altered the text to clarify this issue

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.

The text was clarified to reflect the reviewers concern, with appropriate citation of the work of Rinne and Ammann.

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.

The text was clarified to reflect the reviewers concern with the inclusion of the sentence "true eddy accumulation" to differentiate previous DEA measurements from those reported here.

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

The suggested change was made to the text.

Page 2720, lines 11-12: "2.09 (_ 0.19)" and "1.96 (_ 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 (_ 0.2)" and "2.0 (_ 0.4).

A change to more appropriate sig figs was made in the revised manuscript.

Section 7: How was the value of Is determined?

Literature data regarding the value of I_s were used in this study (70 μg C g^{-1} h^{-1}). The text has been updated to show the value used in this work and appropriate citation included in the revised manuscript.

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.

This confusing wording was removed in the revised manuscript

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.

The suggested change was made to the text.

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.

We agree with the reviewer as to this interesting observation. However, we feel that given the absence of literature data or supporting measurements as to speciated monoterpene concentrations at this site, any firm conclusions as to the nature of this afternoon "maximum" is difficult to determine.