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Atmospheric
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Interactive comment on "Biogenic volatile organic compound emissions during BEARPEX 2009 measured by eddy covariance and flux-gradient similarity methods" *by* J.-H. Park et al.

Anonymous Referee #2

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This MS describes and analyses measurements of a set of BVOC above a Ponderosa pine forest in the US. The data set includes BVOC ecosystem exchange measured directly by (disjunct) eddy covariance techniques, and a larger data set of BVOCs which had been sampled with a concentration gradient system. The flux-gradient similarity approach (K-theory) was used to estimate fluxes from the gradient system, and the results were compared against the directly measured EC-fluxes and discussed with results obtained at the same site within the last decade. In my opinion, this is an interesting MS which could contribute to a better understanding of BVOC emission characteristics and feedback mechanisms in atmospheric chemistry. However, I also think that this MS still has some weak points and thus should be improved before it can be published in ACP.

-I don't understand the argument not to use CO2 and H2O or H (sensible heat) fluxes to determine the eddy diffusivity K which then are used to estimate fluxes of other compounds (like BVOCs) from a concentration gradient above the canopy. The authors state that CO2, H2O or H sources and sinks are much different from those of BVOC, and that it would be much better to use BVOCs instead to obtain K. However, this is not completely true, as there are different sources and sinks for the variety of BVOCs as well. For example, the authors themselves discuss that e.g. sesquiterpenes (SQT) are very reactive and interact on short timescales within the canopy, while they did not found this for other compounds (e.g. monoterpenes, MT). Furthermore, they themselves mention different production processes (growth for methanol, temperature controlled emission from pools for MT, temperature and light controlled processes for m/z 69) and speculate about different sources (e.g. emission from soils for methanol). It rather would be interesting to see, if K-values obtained from CO2, H2O or H-fluxes would lead to higher variability compared to Kuniv averaged from Km33, Km69 or/and Km81.

-I don't know which additional data from that specific site would be available, but instead (or, in addition) using the (disjunct) EC-fluxes to invert their Eq 4 and to resolve for K, they could also use K=0.4 z u* with u* from the sonic (e.g. Foken, Stull). And as I guess that the sonic data was recorded continuously (even during the time of the profile measurements) this could avoid the problem of the time delay between fluxes and profiles.

-Another potential problem of the K-theory is that it might have problems in convective conditions, i.e. during noon when the fluxes are highest. Furthermore, in my opinion you probably should not use the level at 9.5m (L3) but L2 at 13.6m instead as L3 is close above the canopy (8.8m from Fig 1) and you most likely don't have a well developed turbulence there (but patterns of large scale eddies penetrating the canopy layer). This would not work well with the K-theory. However, the BVOC gradients get

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smaller as well and might approach the measurement uncertainties. . .

-The site description (p25085) mentions a clear day-night pattern in wind direction, which might be a circulation system causing advective flows. Can you comment on this? Furthermore, you mention a new tower was placed north of the old one, but only 10m away. You state that the wind direction during nights is predominantly from 30-60°, and during day from 220-280°, so there is a risk that the towers might influence the measurements. Did you check this?

-You should briefly mention the measurement levels for the meteorological data (p25085).

-Instrument setup: the mismatch of the sampling times for EC-fluxes and K-theory might cause some problems, especially during times when conditions change rapidly, e.g. during morning and evening hours. Could that be the outliers in Fig 8?

-Was the BVOC inlet heated to avoid condensation in the inlet line?

-Section 2.2.2; calibration: Your calibration routines (p25087) remain unclear to me. I assume the first mentioned calibration (twice daily) is a two point calibration (zero and one concentration but several masses), the second one performed irregularly during instrument service is a 5 (6)-point calibration but only for d-limonene? Could you clarify this?

-P25089, L9: did you apply any filtering to remove data with too low turbulence, e.g. based on u*?

-P25091, L4pp: can you comment on the storage term calculations? Methanol and MT were emitted during night as well under low turbulence, m69 is light-dependent and should be emitted when conditions start getting turbulent. Would you have an example of a reactive compound that should be emitted under low-light conditions, but show a weak storage?

-P25092, L27: from fig 4b, I'd say that this is a rather pronounced vertical gradient for m69 and you present high fluxes for m69 as well, so this statement is unclear to me.

-P25095, L15: was Kuniv even calculated when not all 3 K-values could be calculated, i.e. when Km69 was missing? How many K-values did you use for Kuniv at least?

-P25095, L24pp: I suppose you try to avoid comparing K with K taken from the inverted equation. Either you for example use another way to get K (see comment above), or you take only Km33 and Km81 but leave Km69 for validation as anyway only 51% of Km69 were available. However, m69 is light-dependent, m33 and m81 are not.

-P25097, L1pp; and fig3: are the mentioned fluxes EC-data, or data calculated by the K-theory?

-I actually like the discussion with ‘older’ data and analyses from the same site (back to 1999). There are not many sites where this is possible!

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25081, 2012.