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Interactive comment on “Airborne flux measurements of biogenic volatile organic compounds over California” by P. K. Misztal et al.

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Overall Evaluation:

While I have rated the manuscript "accept subject to minor revisions," I do have some important concerns that I think should be addressed before the manuscript is approved for publication. I do think that this paper merits eventual publication, as the techniques and analyses presented here are a significant step forward in the science and demonstrate an approach to quantifying emissions of trace reactive species over horizontally heterogeneous surfaces.

Major Comments:

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Using a single representative vertical flux divergence to extrapolate all fluxes at flight level down to the surface to estimate surface emission has several assumptions that should be discussed in more detail. The vertical flux divergence is dependent on the rate of isoprene oxidation (which depends on OH concentration and other oxidants), the time rate of change of isoprene concentration, and differential (with height) horizontal advection of isoprene. Since the flights were conducted with a variety of different conditions and locations, one would expect different isoprene lifetimes and horizontal advection scenarios. These effects need to be taken into account or at least evaluated for their impact to be assessed in order to estimate the accuracy of the extrapolation to the surface. Furthermore, I don't see any estimate in 2.7 of the random error contributed by the flight segment length. Thus, the error analysis in 2.7 seems to me to be too optimistic, at least without further justification. This has relevance later on in section 3.2, where it is not clear how much of the variations and differences between e.g. tower and aircraft are due to random errors and how much to real variability.

In several places, it is claimed that the CWT method can be used to obtain 1 to 2 km spatial resolution in fluxes. This is misleading. True, you can get fluxes to this resolution, but with very large random error. This needs to be considered further and the random error as a function of sample length needs to be discussed and quantified. Among other references, you might take a look at "Errors in Airborne Flux Measurements," by Mann and Lenschow, 1994, J. Geophys. Res., 99, 14519-14526.

Other Comments:

abstract, l. 6: spell out PTR-MS

abstract, l. 13: the statement, " Vertical flux divergence of isoprene is expected due to its relatively short lifetime. . ." is misleading. Vertical flux divergence is expected for almost all atmospheric species. In conserved species it is a reflection of time changes in the mean concentration. More accurately, you should say that there is a major contribution to vertical flux divergence of isoprene due to its relatively short atmospheric

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lifetime.

abstract, l. 15: I can't make sense of a vertical flux divergence expressed as a percent. The units of species vertical flux divergence should be concentration/time. Do you mean that the this is the percent difference between flight-level flux and surface flux?

p.7968, l. 15: spell out BEIGIS. Also earlier MEGAN and BEIS

p. 7969, l. 9: "Stacked," instead of "Vertical."

p. 7975, l. 8: ...capable of eddy flux measurements...

p. 7975, l. 10: What is meant by area "ratio of about 2?" Does this mean that the diameter changes from 2.047 in. to about 2.89 in.? Same comment applies later to the area ratio of about 5. Also, I think that you should use metric units for these dimensions.

p. 7975, l. 16: "unaffected" is a bit too strong. Perhaps something like "minimized" would be more appropriate.

p. 7976, l. 7: "is a fast sensor which" is redundant.

p. 7976, l. 13: I'm not clear what 10 Hz separated by a relatively longer gap of 2 Hz means in this context. Can you elaborate more on what this means? Do you really mean 0.1 s dwell time and 2 samples/s?

p. 7977, l. 8: "sawtooth" rather than "tooth"

p. 7977, l. 14: So, the sensitivity is 10 pptv/17 s, where 17 s is the averaging time?

p. 7977, l. 17: I'm a bit confused by the comparison between absolute sensitivity and normalized sensitivity. Do they really have the same units?

p. 7978, l. 28: ...measurements of concentration profiles in the mixed layer overlying the surface layer of the daytime convective boundary layer... I suggest using CBL instead of MBL, as "mixed boundary layer" is not accepted terminology.

p. 7979, l. 1: This top-down bottom-up method applies only to a conserved species

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not VOCs in general. You should say "a conserved species" not a VOC.

p. 7979, l. 25: ...on all eight research flights, and MVK+...

p. 7980, l. 3: 150-300 m is very deep for an assumed surface layer. What is this based on? Certainly not on 10% of the PBL depth, nor on applicability of surface-layer parameterizations.

p. 7980, l. 13: ...should be maximized. (Actually more accurately, if you sample significantly more than 1/integral scale, increasing the sampling rate won't gain you anything.)

p. 7980, l. 18: It's not clear to me what you mean by "total cycle length." Do you mean the sample rate for each species is 1.25 to 2 samples/s?

p. 7980, l. 25-26: Do you really mean non-stationarities or do you mean horizontal heterogeneity? In l. 24 "affected by diurnal effects" is not usually a problem for aircraft measurements of a single flight segment.

p.7982, l. 9: ...an integrated straight stretch... What is meant by integrated? What is integrated? Do you mean continuous or contiguous? Why does it have to be straight?

p. 7982, l. 16: See earlier comment about random error in flux measurements as function of sample length.

p. 7982, l. 29: ...controlled by its relatively... .."racetracks" at multiple levels... (profiles is redundant)

p. 7983, l. 1: again, flux divergence has units of concentration/time, not percent.

p. 7983, l. 3: A storage term of 2-5% of what? The chemical reaction term? This seems very small to me in view of the diurnal variability of the isoprene concentration in the PBL. Was the buoyancy flux also extrapolated down to the surface similar to the isoprene flux when used to estimate w^* ?

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p. 7983, Eq. (4): Where did you get this equation come from? It isn't in Horst and Weil (1992), who considered only the surface layer not the mixed layer. It seems that the authors have the wrong citation; this equation is presented in Weil and Horst, 1992: Footprint estimates for atmospheric flux measurements in the convective boundary layer. A chapter in Precipitation Scavenging and Atmospheric Surface Exchange, Vol 2, S. E. Schwartz and W. G. N. Slinn, Coords, pp. 717-728, Hemisphere Publishing Corporation, Washington, 1172 pp.

p. 7983, l. 24: "superposition?" or "summation"?

p.7984, l.12: do you mean "minimal roll angle"? Constant roll angle could imply a constant rate turn.

p. 7984, l. 22: Standard usage for cross-spectrum is the real and imaginary parts of the Fourier transform, and co-spectrum for the real part, which you are using here.

p. 7986, l. 12: By "measurement footprint" it seems that you are no longer talking about the flux footprint that you were discussing earlier, but the concentration footprint, since the flux footprint does not extend hundreds of km upwind. This needs to be pointed out.

p. 7990, l. 25: ...for a setting where...

Table 1: You might also include w^* or the surface virtual temperature flux in the table, so the reader can e.g. estimate the convective turnover time. Your estimate of 10 minutes (p. 7979, l. 9) means a large surface virtual temperature (i.e. buoyancy) flux.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 7965, 2014.

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