

Misztal et al (2014) uses a novel approach to estimate fluxes at the 2-5km scale from aircraft data. The innovation, and applicability of this approach certainly make this appropriate material, however, I cannot recommend it for publication as is, but it should be published after major revisions from the authors.

As I understand it, the main improvement this paper makes is the application of the wavelet transform method to retrieve isoprene fluxes at the 2km level, and thereby achieve excellent spatial coverage, that at first glance appear to match modeled surface isoprene fluxes well, although inter-comparisons between observationally-derived flux estimates are somewhat inconclusive. This is a broadly applicable technique, which could be useful for validation or comparison with model results. Thus, it represents a significant improvement to current methods. This reviewer appreciated the extensive discussion of uncertainties, as well as the experimental technique.

There are two main challenges that face the authors. The first is one of focus and information. The title of this paper is 'Airborne Flux Measurements of Biogenic Volatile Organic Compounds over California'. The figures do not suggest that this paper is primarily about BVOC fluxes. Figure one shows the flight tracks, as expected. Figure 2-4 show validation data for the flux calculation. Figure five shows concentration tracks of four VOCs, only two of which get any mention in the paper, and only figure six presents actual flux results, which only involve isoprene, and these results are semi-quantitative. Thus it is confusing because the only VOC flux mentioned significantly in the paper is isoprene, yet three full page maps are devoted to showing track values of VOCs which are not significantly referenced in the paper, only one BVOC flux measurement is discussed, and only in a semi-quantitative manner.

Thus, my main recommendation to the authors is that they decide what they want the paper to be about, and reframe the title and introduction, or add some results. Summarily: is the paper predominately about the method applied, the results from that method, or a general overview of BVOC and VOC fluxes and concentrations measured from airborne sources? If the former, then the validation statistics need to be presented (intercomparisons between surface and aircraft isoprene measurements and fluxes) more clearly. If the middle, then only two figures with maps are presented, and little effort is spent to summarize the results. If the last option, then more information about the other VOCs should be provided. In my opinion, in any case, the results are a bit thin, and this is the second challenge that faces the authors.

The second challenge that the authors face is the difficulty of accurately distilling the aircraft data into something that can be understood. As it stands now, the authors only make generic statements (ie flux averages and standard deviations), and show qualitative maps. This is a gap which this paper needs to address. If the authors wish to preserve model intercomparison for another paper, they could at least combine data from the tracks to show isoprene flux

measurements, discuss differences in fluxes when tracks overlapped between different days, and over different locations. Instead of saying things like ‘low background in the central valley’, state what these values are.

More specific comments:

Page 6: line 27: Remove this sentence as the referred sections have been removed.

Page 9: How long are the flight segments? You mention 100km, but also 20-200km. It would be useful if the authors could report the mean and standard deviation of used lengths in trajectory calculations in the supplementary information.

Page 13: In the discussion of wavelets, the authors present the general theory, and then immediately jump to a discussion of the wavelets vs FFT method. It would help immensely to add a paragraph explaining exactly how the wavelet is used in this case to derive the fluxes.

In the first iteration of the response to reviewers, the authors state: “We now state more clearly that for the wavelet fluxes we actually integrate long segments (e.g. 100 km) and based on wavelet decomposition, we reconstruct the time domain for the wavelet co-spectra to yield time series of discrete coherent structures which are subsequently aggregated to 2-km surface fluxes.” I cannot find where this is done, until I read this comment, I thought that the authors were in fact calculating directly 2km fluxes. This statement could be placed into the text almost verbatim, and would make things very clear.

Page 14:

For determination of linear flux divergence: Karl et al. (2013) only reported flux divergence values for three tracks. What is done on on the other days? Do you use an average?

Page 15: Why is the error contribution of flux divergence for a reactive scalar like isoprene, lower than the flux divergence of a passive scalar like CO₂? This is confusing since because the Damkohler number is of order 1, and so the chemical timescale is on the order of the boundary layer timescale.

You state that the survey flight specific random error is 5%. What is used to obtain this number? If I estimate using using equation 3 from Karl et al, 2013, (citing Lenschow 1994), assuming a flight level of 400m agl (referenced in the abstract), a boundary layer height between 1000 and 2000 m (typical levels as noted by reviewers in comment 19 of reviewer 1), and a sample length of 100km (noted as the average by the authors), errors range between 14 to 17%. . If I'm misunderstanding the equation used to compute the random error, please make more clear what you mean by this value.

Line 30: For the flux divergence, you state that it varies by a factor of two. Is this based on the three OH estimates that you have from Karl 2013, or something else? Could you present evidence that the OH levels are similar on other days? The estimated noontime OH levels range from 78 ppq to 1.4ppt, a difference much greater than a factor of two.

Page 16:

Line 6: If fluxes are only evaluated over 5km to reduce the error, those are the values that should be shown in Figure 6.

Page 19:

Line 21: Should figure 5 be referenced here? If so, why not zoom figure 5 so that only the part discussed in the text is shown.

Line 24: If these other VOCs are not to be referenced in the paper, remove the other VOCs from Figure 5.

Page 20 Line 18: Since methanol is not mentioned further in the paper, why not remove this sentence?

Line 25 state what the central valley is, and what the background flux level is.

Page 21: It is curious to me that the uncertainty in the instrument aircraft fluxes are equal to the REA uncertainty. How was the REA uncertainty derived? How were those measurements made? I suggest this information be added to the supplement. As noted by the authors, it is unfortunate that there are not more data points to analyze, because it is interesting that one of the two data points is exactly identical to the REA measurement.

Summary: I wish to reiterate: I think that this uses a novel approach and derives some interesting results. However, the way the paper is framed, compared with the contents of the results section, seem to be at odds. I came away from the paper without a real understanding of what the BVOC fluxes look like in California.

Karl, T., Misztal, P. K., Jonsson, H. H., Shertz, S., Goldstein, A. H., and Guenther, A. B. (2013): Airborne Flux Measurements of BVOCs above Californian Oak Forests: Experimental Investigation of Surface and Entrainment Fluxes, OH Densities, and Damkohler Numbers, *J. Atmos Sci*, doi 10.1175/JAS-D-13-054.1.

Lenschow, D. H., J. Mann, and L. Kristensen (1994): How long is long enough when measuring fluxes and other turbulence statistics?, *J. Atmos. and Oceanic Tech.* doi: 10.1175/1520-0426(1994)011<0661:HLILEW>2.0.CO;2.