

Review of the manuscript by Fiorella et al. "Detection and variability of combustion-derived vapor in an urban basin"

### **General Remarks:**

The manuscript presents follow-up work and an extension of continuous observations of co-located measurements of atmospheric CO<sub>2</sub> mole fraction and deuterium excess in atmospheric water vapor to investigate the impact of combustion-derived water vapor (CDV) at a monitoring station in the Salt Lake City basin in Utah, USA, during winter. The particularly low deuterium excess values of CDV significantly influence the isotopic signature of atmospheric water vapor during inversion situations at cold temperatures when atmospheric humidity is generally low and during stable weather conditions when combustion-derived emissions (CO<sub>2</sub> and H<sub>2</sub>O) accumulate in the atmospheric boundary layer. The authors estimate, in a Keeling-style mixing model approach, the range of CDV d-excess values, which turns out to be large. They claim that these results could be used to constrain contributions of combustion to urban humidity and meteorology (Abstract), or possibly verify CO<sub>2</sub> emissions amounts and/or emissions reductions (Conclusions).

From their four-year observations, the authors convincingly show that the isotopic signature of atmospheric water vapor can be significantly modified by CDV during winter, but I am not convinced that there is a realistic chance to use the observed relation between high CO<sub>2</sub> and low d-excess in atmospheric water vapor in a quantitative way. As discussed by the authors, the variability of combustion material and its large range of H<sub>2</sub>O/CO<sub>2</sub> stoichiometry when burned to CO<sub>2</sub> and H<sub>2</sub>O as well as potential isotope effects during production and emission strongly modify d-excess of CDV. Furthermore, not all CO<sub>2</sub> emissions during winter can be solely associated with combustion processes, but some CO<sub>2</sub> emissions may also originate from biogenic sources that are not associated with net H<sub>2</sub>O emissions. Therefore, the constraints on urban humidity and CO<sub>2</sub> emissions mentioned in the Abstract and Conclusions, to my understanding are not justified. A sensitivity study including a thorough uncertainty analysis would be required to support these optimistic statements.

In view of the weaknesses of the "tracer" CDV d-excess, I think the manuscript is too detailed. It has too many figures showing similar, mainly semi-quantitative, features that make the manuscript unnecessarily lengthy. For example, I am not sure that all three case studies (described in Figures 7, 8, 9) need to be presented and discussed in detail. Figure 7 would be sufficient to convince the reader that the processes introduced before really take place and are visible in the observations. In addition, Figures 4, 5 and 6 give somewhat redundant information, with Figure 5, to me, being the most convincing. Figure 4 more or less summarizes what is visible in detail in the time series shown in Figure 3, and Figure 6 somehow "hides" the large variability in the Keeling plots, that are expected because the signature of CDV is not well defined and variable in time. I am missing the error analysis that quantifies the ranges of d-excess and emission factors stated.

Technical: (1) There are many abbreviations used in the manuscript (VHD, PCAP, WBB, SDM, ...), which are new for the reader. It would very much help to spell them out again if they had not been used for a while. (2) Please note that CO<sub>2</sub> concentrations are also calibrated as micromole per mole (or ppm), but not ppmv (Fig. 7, 8, 9).

### **Specific Remarks:**

Introduction, first sentence: please give reference.

Page 2 line 5: "produce";

line 16: "from"

Page 3 Eq. (1): why sum up to 2200 m?

line 22: how long was the tubing and was it heated (e.g. to avoid condensation effects)?

line 25: give reference to script.

Page 4 line 2: how often was calibrated? measurements uncertainties?

line 3: "meteorological";

line 27: is the time shift between ASB and WBB taken into account in the pre-2014 data?

line 28: better spell out CDV in the title.

Page 6 Fig. 1: the yellow line is not well visible;

line 1: is the total  $\Delta\text{CO}_2$  from combustion processes, i.e. no flux from biosphere?

line 2: subscripts "obs"

Page 7 Fig. 2 and line 6: In the figure (mixing heights) ground level starts at 0 m while in the text total heights in m a.s.l. are reported; this is confusing

Page 9 line 6: is the correlation really "strong" and does this Figure provide new information compared to Fig. 3?

lines 10-14: in Fig. 5,  $q_d$  is plotted vs.  $q$ , the text explanations are thus unclear.

Page 12 Figure 6: would like to see single events here to better judge on the significance of the correlation (see general comment concerning the significance of the Keeling approach to estimate end members)

Page 13: please give times as local station time or in UTC; panels in Figure 7 (and 8, 9) seem to have been mixed up and do not correspond to the text. As  $d$ -excess is shown only in relation with moisture (and not vs. time), it is difficult to see the temporal correlations between  $\text{CO}_2$  and  $d$ . Perhaps add a seventh panel.

Pages 15-18: please explain why it is important to discuss these two case studies.

Page 19 line 7: what is MST?

Page 20 Fig. 10: uncertainties hardly visible

Page 21 lines 6-8: May be WBB is generally not well located on the topographic bench; what is SBI?

Page 22 lines 2 and 26-27: this seems to me far from realistic – please justify and make an uncertainty estimate (see general comments)