

Interactive comment on “Formaldehyde and its relation to CO, PAN, and SO₂ in the Houston-Galveston airshed” by B. Rappenglück et al.

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Reply to Referee #2

We appreciate the important comments made by the reviewer and we hope that our manuscript has improved.

1) The statement at the beginning of 3.2 is not correct. CO is not produced only by combustion. There is a large background source of HCHO and CO from the oxidation of methane and isoprene that could be accounting in large measures for their high residual obtained [See Millet et al. (2008) and Hudman et al (2008)]. Millet et al note that Houston is the only area for which an anthropogenic HCHO signal is detected,

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but even here it is a minor enhancement above that from the isoprene background. Their resolution is high enough so that they can distinguish urban vs forested areas. Hudman et al note that the oxidation of isoprene is a larger source of CO than that from tailpipes in summer, the time of TexAQS. Again, notable enhancements of CO were not observed over Houston. Overestimates of anthropogenic CO emissions also apply to Houston. Including this source would add some perspective to the results and could help account for the residuals obtained in the regressions.

Answer: The first paragraph of section 3.2 has been rewritten as follows:

“In the natural atmosphere secondary formation from oxidation of methane and isoprene largely controls the background CO concentration. Hudman et al. (2008) found that CO from biogenic sources exceeds the contributions originating from anthropogenic sources during summer times over the eastern United States. Primary sources for CO are combustion processes. Apart from biomass burning, major combustion processes are associated with anthropogenic activities concentrated in urbanized areas. As shown in aircraft studies by Herndon et al. (2007) the HCHO/CO ratio may vary by a magnitude depending on daytime and sampling locations. According to Herndon et al. this may either be due to direct emission sources which have a different fraction of concomitant CO (or do not have CO at all) or secondary production of HCHO during the daytime. The latter has been verified by satellite studies over a wide range for the south-eastern part of the United States (Millet et al., 2008). Since CO is directly being emitted from combustion, CO has previously been used in urban studies to evaluate the traffic exhaust related HCHO emissions (Anderson et al., 1996; Possanzini et al., 1996; Friedfeld et al., 2002; Rappenglück et al., 2005; Garcia et al., 2006). Dynamometer studies showed that the emission ratio of HCHO/CO is typically 0.001- 0.002 ppbv/ppbv for gasoline engine passenger cars, but can be 10x higher for diesel cars depending on driving conditions (Schmitz et al., 1999).”

2) Although mentioned briefly in the Introduction, I think some additional detail regarding the quantitative importance of HCHO for ozone formation is needed. This will serve

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to underscore the importance and relevance of this work.

Answer: We added the following text to the Introduction: “The importance of assessing potential primary HCHO has recently been underscored by Olaguer et al. (2009). Olaguer et al. reported a sensitivity study which assumed that 1% of the flow from 13 flares in the HSC area was HCHO that survived the combustion process. According to the results of this study peak ozone would increase about 30 ppbv at some surface monitoring sites.”

3) During TexAQS-II, a number of long-path DOAS measurements were also made. I think the paper would benefit by pointing out these measurements and though not necessary any correspondence between those and the measurements reported here. In this regard, the authors might want to consider the paper by Olaguer et al. in the November issue of the Journal of the Air and Waste Management Association.

Answer: The Olaguer et al reference has been added to section 3.3.

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