

Interactive
Comment

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

Anonymous Referee #2

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General comments: This paper contributes to further understanding the sources of high ozone levels in Houston. It helps put the issue into a different perspective by paying attention to what may be an important source of free radicals that is commonly overlooked.

Specific comments:

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 measure for the high residual obtained. (See Millet et al.(2008), Spatial distribution of isoprene emissions from North

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Interactive Discussion

Discussion Paper



America derived from formaldehyde column measurements by the OMI satellite sensor, *J. Geophys. Res.*, 113, D02307, doi:10.1029/2007JD008950 and Hudman et al.(2008), Biogenic versus anthropogenic sources of CO in the United States, *Geophys. Res. Lett.*, 35, L04801, doi:10.1029/2007GL032393.). Millet et al note that Houston is the only area for which an anthropogenic HCHO signal is detected, 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.

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 to underscore the importance and relevance of this work.

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.(Olaguer EP; Rappenglück B; Lefer B; Stutz J; Dibb J; Griffin R; Brune WH; Shauck M; Buhr M; Jeffries H; Vizuite W; Pinto JP. Deciphering the role of radical precursors during the Second Texas Air Quality Study) in the November issue of the *Journal of the Air and Waste Management Association*.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 9, 24193, 2009.

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