Atmos. Chem. Phys. Discuss., 11, C12715–C12717, 2011
www.atmos-chem-phys-discuss.net/11/C12715/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Direct measurement of ozone production rates in Houston in 2009 and comparison with two estimation methods" *by* M. Cazorla et al.

Anonymous Referee #1

Received and published: 5 December 2011

The paper describes field measurements of a recently developed instrument (MOPS: Penn State Measurement of Ozone Production Sensor) used in a recent study in Houston, USA to derive ozone production rates in ambient air. The results of the ozone production rates of the measurements are compared with "calculated" production rates derived from simultaneous measurements of concentrations of NO and hydroxyl- and hydroperoxyradicales and "modeled" values derived from numerical simulations of the model RACM2. The concept and the results are very interesting. It is not completely clear to me whether the results are unexpected, but I have the feeling that more experiences with the method are required to get a better handling of the uncertainty determination. In my view the paper merits publication if the following comments are

carefully considered. 1. Abstract: I am not sure whether I agree with the last sentence: “This difference indicates possible missing radical sources . . .”; I find the combination “indicates and possible” not convincing and I have reservations with the interpretation (see next comment) and therefore I suggest to delete this sentence 2. I think LIF measurements allow to quantify simultaneously OH and HO2 and I suggest to produce plots equivalent to Fig. 3a (and 3b) of Hofzumahaus et al. (2008): I find it only adequate to stress the results of the Pearl River Delta study in the context of this study if the measured OH concentrations very strongly deviate from the modeled OH ones 3. Introduction, first paragraphs: Lowering Ambient Air Quality Standards: I my view the effects of elevated ozone on human health and ecosystems should be in the center of such a debate rather than how often the standards are violated 4. Page 27524, line 20, Equation (2): First term on the right side, related to HNO3 production: I don’t believe that the term “ozone loss” is appropriate because no ozone is destructed when NO2 reacts with OH to form HNO3: I think you mean that further (net) ozone production by NOx is prevented by production of HNO3. I think P(RONO2) includes PAN formation which is again no “ozone loss”. 5. Page 27524, Equation (2) and (3): Is ozone loss by dry deposition (not) considered in your study ? 6. Page 27525, line 2: I think the study of Hofzumahaus et al., (2008) refers to a very peculiar receptor site in China and the difference between modeling and measurements is very large at this site (compare comment 2) and therefore I suggest to be careful to stress this case (too much) in the context of this study 7. Page 27527, line 25: I find the term “chemical loss” problematic in the context of this study (comp. comment 4) 8. Page 27529, line 6, Fig 1: I recommend to use calendar months and days instead if “day of the year” for labeling of time 9. I am little worried that the correlation coefficients (R2) between “measured” and “calculated” P(O3) (see Fig. 3, b) values are that low without any visible tendency (R2: 0.34). Do both measurements measure the same quantity ? Are there obvious problems with the measurements used for the determination P(O3) ? Obviously the correlation between “modeled” vs. “measured” values are substantially better (though the plot shows a systematic deviation). 10. Fig. 5: is the plot looking (much) different

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when using NOx instead of NO ? Possibly Fig. 5 shows the basic relation of P(O3) vs NOx as expected from a radical chain reaction system implying that this kinetic system seems to be applicable to the measurements 11. Page, 27537: I think the last sentence is premature ("MOPS measurements of SHARP challenge the understanding of photochemistry") – unless the authors present more material concerning strong disagreement between OH measurements and photochemical model

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27521, 2011.

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Interactive Comment