

Interactive comment on “Ozone production, nitrogen oxides, and radical budgets in Mexico City: observations from Pico de Tres Padres” by E. C. Wood et al.

Anonymous Referee #1

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General Comments

The paper by Wood et al discusses data from a field campaign held in the Mexico City basin in 2006. Such data are potentially interesting, owing to the high concentrations of atmospheric pollutants commonly observed in this area. The authors aim to study ozone production under such conditions. The paper is generally well written.

General problems are understanding what the results are representative of, given they focus mainly on one hour of one day. Also, there are a large number of assumptions made, which I'm not always sure are well justified.

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Specific Comments

1. The main issue I have with this paper is what conclusions can be drawn from 1-2 days of data? Even then, much of the focus is within 1 hour. The title of the paper is somewhat misleading. The reader might expect a more complete assessment of ozone production in the Mexico City basin. I would suggest it is altered to reflect the fact that it is really a case study for a very short period of time with particular conditions. 2. On a related note, how applicable are the results? The authors note that O₃ production reached 50 ppb/h at one point, which is indeed a high value. However, nowhere do they discuss how frequently such high values occur, and whether they think they are applicable across the whole of the Mexico City basin. 3. On page 15741, line 25, the authors mention that ozone values are frequently 'unsafe' in the MCB. This sentence would be more informative if they told us what the unsafe values were and how unsafe is defined in this context. 4. The other main issue with the paper is the large number of assumptions made. The authors have done their best to justify the assumptions, but the reader is still left wondering about the conclusions drawn, given the small amount of data and the number of assumptions made. For instance, where does the value of 40 ppt of HO₂ come from given the authors calculate the HO₂+RO₂ sum to be 95 ppt? 5. The value for O₃ deposition of 0.4 cm/s needs to be justified by at least a reference (page 15754, line 2). 6. In section 3.3.3, the authors state that significant partitioning of the organic nitrates to aerosol would decrease their OH estimate - would this be a linear relationship and how would the overall results be affected? 7. At the bottom of page 15762, top of page 15763, it transpires that alkene measurements are not available for this period. Past studies have shown that alkene-ozone reactions play a crucial role in OH production in urban areas (including some of the papers quoted by the authors as well as the BERLIOZ study). Do the uncertainty limits quoted here really reflect the real situation? 8. Much of the results section is quite hard to digest. The reader has to read through and remember many assumptions to follow the calculations through. There is a lot of information here and the paper seems to move rapidly from calculating one 'indicator' to the next. I wonder if it may be better to present fewer of the calculated

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parameters here, but to stick to those that are based on firmer assumptions?

Technical issues

1. Page 15744, line 2. New paragraph after (R2 and R3).

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15739, 2008.

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