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Interactive comment on "Airborne measurement of OH reactivity during INTEX-B" *by* J. Mao et al.

Anonymous Referee #2

Received and published: 21 September 2008

Review of Airborne measurement of OH Reactivity during INTEX-B

The authors should be congratulated on this important contribution and complex technical achievement; namely the successful completion of OH reactivity measurements on an aircraft platform for the first time. The paper is well written and includes new insights into our understanding of photochemical processing. In particular, the paper shows vertical profiles of atmospheric reactivity in remote regions and compares them to two alternative calculated values. A further interesting finding is that HCHO is a good indicator of missing; or unaccounted for OH reactivity. The following comments should, however, be addressed before publication in ACP.

1) Little information has been provided about the spatial distribution of the reactivity measured. A map with superimposed flight tracks colored for the OH reactivity measurements would help the readers to form a better impression of the latitudinal/longitudinal OH reactivity dependence. From Figs 4 and 5, plots for the boundary layer (0-2km) and for the upper troposphere (9-12km) would seem appropriate.

2) Line 11; Page 14220: Can the authors clarify the fate of the H atom produced during photolysis of water vapor? The H atom concentration that exits the wand presumably combines with oxygen in ambient air and forms HO2. Is the ratio of OH/HO2 really 1:1 ?

3) An important consideration. Are the reactivity measurements presented inherently a lower limit because of potential reformation of OH due to certain alkyl peroxy radicals (e.g. acetyl peroxy + HO2 reactions) and alkenes in ambient air (O3 + alkenes) or can this be completely ruled out?

4) Line 17, Page 14222: The authors mention that knowledge of the OH concentrations is not necessary for the reactivity measurement but add that these concentrations are measured anyway. The typical OH and HO2 operating concentrations within the set up should be therefore briefly mentioned.

5) Line 28; Page 14222: It is stated that a stainless steel flexible line was used for sampling. Did the authors do tests with different inlet materials such as Teflon tubing and find the stainless steel tubing more suitable? For example has it been tested that the stainless steel tubing does not result in significant loss for compounds such as peroxides or acids in ambient air which would also be contributors to the OH reactivity?

6) Line 7; Page 14224: Is it really the case that the isoprene standard used by the authors has an uncertainty of only 0.8%..i.e. 1.029 ś 0.009 ppmV (Apel- Riemer Environmental, Inc.)?

7) Line 18; Page 14224: The authors make the point that NO values measured during their study was less than 1 ppbv and so no corrections are necessary. Since a new instrumental technique is being presented it would be helpful to be more specific here. At what range of NO can the instrument operate reliably. For this it will be necessary

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to arbitrarily set an acceptable inaccuracy due to NO of say 20%. The reader is of course interested to know whether the instrument in this form be used to investigate lofted pollution plumes, regions affected by lightning NOx etc.

8) Line 28; Page 14226: It is commendable that the authors have determined the uncertainty of their measurements in a rigorous manner using more than one approach for wall loss determination. Under clean air conditions the OH reactivity is circa 2 s-1 and uncertainty about 0.7 s-1, which implies that at such values the uncertainty is \sim 40% while at values near 6 s-1 it is circa 20 %. This should be made clear in the abstract.

9) The strong temperature rise in the inlet must surely impact the concentrations of PAN and perhaps other thermally sensitive species giving rise to different compositions between the ambient and in the measurement region in the instrument. To what extent does this affect the reactivity determination? PAN itself does not react very fast with OH but its decomosition products may. PAN also appears not to have been included in the sum of individual reactivities.

11) Based on the observation that the measurements do not match the two calculated values the authors could add to their conclusions the statement that future assessments of atmospheric reactivity should be made by measurement rather than either of the calculated methods performed here.

12) p. 14218, line 24. HO2 is a major precursor of tropospheric ozone. The caveat, in the presence of NOx, should be added since at low NOx its self reaction is a radical termination step.

13) P14218, line 25, largely determined, should be replaced, since light, H2O and ozone are considered to be the key factors. Perhaps instead ,OH is dependent on atmospheric constituents emitted by biogenic and anthropogenic processes, could be used.

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