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Atmospheric
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Discussions

Interactive comment on “Measurements of HNO_3 and N_2O_5 using Ion drift – Chemical Ionization Mass Spectrometry during the MCMA – 2006 Campaign” *by* J. Zheng et al.

Anonymous Referee #2

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This paper describes measurements of HNO3 and N2O5 (perhaps?) in Mexico City during an air quality study. The results indicate that gas phase levels of HNO3 are largely controlled by partitioning with particulate ammonium nitrate in this environment with the particulate dominating in the morning. I think this result is solid. This is in accord with earlier work. This observation also agrees with another paper Hennigan et al., which is also in open discussion in ACPD (Atmos. Chem. Phys. Discuss., 8, 4811-4829, 2008). The paper is generally well written but could use some editing. I think the paper is worthy of publication in ACP but I would like to see the following points addressed.

1) I am surprised at the low levels of HNO3 observed in the campaign. It would be interesting to compare HNO3 (or total nitrate) production rates during the day from NO2 reacting with OH to observations. This would give some insight into lifetimes and be a check on the consistency of the data. 2) The comparison between the ID-CIMS and the ICMS really doesn’t look that great. If you were to remove the four highest points on the graph (which largely control the correlation coefficient) above 1.2 ppbv there doesn’t seem to be much correlation. This should be addressed as the data seems to be scattered and there is little evidence for a bias in one direction. Is this due to detection limit issues? By the way detection limits are stated for the ID-CIMS but I am not sure how they are derived this should be fixed. 3) A lot of time is spent on how the ID-CIMS sensitivity can be calculated. However, as stated there are a lot of parameters used each with its own level of uncertainty. The comparison of the calibration to this calculation indicates that this can be off by more than a factor of two (for HNO3) and 50% for the N2O5. So I am not there is that much utility to these calculations as you can probably guess the expected sensitivity reasonably well with some knowledge of the ion molecule rate constant and an estimate of the reaction time. For this reason, I question the inclusion of this lengthy section of the paper (much of sections 2.1 and 2.2). You could cut it out and describe the calculations and nothing would be lost. If this section is not cut some justification for its inclusion should be added. For example, a comparison of the calibration and calculations and a discussion of why they are different. However, I suspect all you are finding out is that HNO3 is easily lost in the instrument. 4) My final comment is the inclusion of the N2O5 results. I think a better case needs to be made that the observed signal is indeed N2O5. I- could react with many things in the Mexico City atmosphere such as PANs to give some NO3- signal. This section should be pulled out or calculations using the N2O5 equilibrium constant and observations of NO2 and ozone to see if these levels are at least feasible in this warm environment.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4877, 2008.