

Interactive comment on “Production of N₂O₅ and CINO₂ in summer in urban Beijing, China” by Wei Zhou et al.

Anonymous Referee #1

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Review of “Production of N₂O₅ and CINO₂ in summer in urban Beijing, China” by Zhou et al. (acp-2018-349)

This manuscript presents measurements of ambient N₂O₅ and CINO₂ in urban Beijing using chemical ionization mass spectrometry and derivatization of the uptake coefficient of N₂O₅ and the yield of CINO₂. The data set are certainly of interest to the atmospheric chemistry community. On the other hand, major issues like instrument calibration, size of the data set, and presentation of the results, etc. stopped this reviewer from recommending publication of this manuscript in its present form in Atmospheric Chemistry and Physics. The authors are suggested to address the following concerns before a further consideration can be given.

Main issues

1. The authors are suggested to be consistent in the presentation of their results. Take the abstract for example, $\tau(\text{N}_2\text{O}_5)$ -1 has been used whereas $\tau(\text{N}_2\text{O}_5)$ is given in Table 1; The exact values for $\tau(\text{N}_2\text{O}_5)$ -1 in the abstract is different from the values in the main text (Page 15 Line 15); Scientific notation has been used with $\tau(\text{N}_2\text{O}_5)$ -1 but not with direct N_2O_5 loss rates (0.00044-0.0034 s⁻¹); Finally, the contribution of heterogeneous uptake of N_2O_5 (7-33%) cannot be derived from the above-mentioned numbers. These certainly hurts the readability of this manuscript.

2. (Page 5 Line 20), I don't agree with the expression that BBCEAS was deployed for inter-comparison of N_2O_5 . The IAP-CIMS was not calibrated at all. To me, BBCEAS provided a calibration reference for the IAP-CIMS. Also, as stated by the authors, BBCEAS measures the sum of N_2O_5 and NO_3 . How did they determine NO_3 and subtract the values of NO_3 subsequently? Please elaborate.

3. (Page 6 Line 1-2), the campaign is quite short, which could be still fine, but the authors are suggested to be more conservative with their findings. (Page 15 Line 13-15), expand the discussion in the time needed for the steady state assumption, and justify whether this requirement was met in the current study. (Page 17 Line 23-24), explain and justify why these three particular time periods are selected.

4. (Page 6 Line 12-21), what were total ion counts of the reagent ions for the IAP-CIMS? Given the high affinity of I- with multiple species in the urban air, was reagent ion depletion observed during the campaign? Was the zero point regularly measured with the IAP-CIMS during the campaign? What were the detection limits and sensitivity of the IAP-CIMS for this particular method? While sensitivity of IAP-CIMS might be derived from comparison with other instruments, how to determine the detection limits? How would this affect the lower points in the measurements?

5. (Page 8-9), a lot of description was given for the calibration of UoM-CIMS but the key is that the IAP-CIMS was not. I still think that it might be OK with the current reference method. But, do consider the uncertainty caused by the assumptions during the entire

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process. I would like to see that the authors add a new session to evaluate the potential impact on their general conclusions (say, the relative importance of different pathways) due to this uncertainty (e.g., 10% or 20% uncertainties in the calibration factors).

6. (Page 9 Line 13), elaborate “this calibration was scaled to those in the field...”
7. (Page 11 Line 7-9), do the authors mean that ambient particles were dried and then measured with the SMPS? Where did the hygroscopic growth factor come from?
8. (Page 11 Line 16), why is $\tau(\text{N}_2\text{O}_5)$ -1 defined as the ratio of $p(\text{NO}_3)$, instead of $P(\text{N}_2\text{O}_5)$, to the N_2O_5 mixing ratio?
9. (Page 13 Line 13-14), If this is true, why didn't we see high CINO_2 ?
10. (Figure 2), if the steady state assumption was met, are we able to derive conc. Of NO_3 at least for two hours per day?
11. (Page 15 Line 7), how was Cl_2 measured? Was Cl_2 calibrated?
12. (Figure 4), I would like to see Figure S6 instead of Figure 4 here. The data points are quite scattered and hence the attempt to use a single linear regression for all the data points just does not make sense.
13. Check the references thoroughly. For example, Brown et al. 2003a in the main text whereas Brown, S.S., ...2013a in the reference list.
14. (Table 1), add the range or standard deviation in addition to the average values.
15. (Table 2 and the corresponding main text), there are limited number of data points so that statistically we can't draw any conclusion for sure, e.g., the effects of RH (page 18 Line 16-17).

Minor issues

16. (Page 2 Line 2), “on the following day”?
17. (Page 4 Line 14-17), do we really want to name this methodology as I-CIMS?



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comment

Personally I prefer iodine adduct CIMS. Also, the authors are suggested to put more effective numbers with m/z values since it is ToF-CIMS after all (Page 7 Line 10-15). Finally, do we really know where the electron/charge is attached? (Page 7 Line 10-15)

18. (Page 6 Line 13), drawn “into” the sampling room?
19. (Page 7 Line 3), so it is CH₃I in N₂?
20. (Page 12 Line 4), do we want to add “nighttime formation”?
21. (Page 12 Line 21), are those reported numbers averages of 1-min average, or 5-min average, or 30-min average?
22. (Page 13, Line 21-), units for quite many numbers are missing.
23. (Page 14 Line 16-18), a good correlation between NO and black carbon does not necessarily mean NO is the most scavenger for N₂O₅.
24. (Page 19 Line 2), also include indirect N₂O₅ loss via titration by NO.
25. (Figure 3c & 3d), repeat the figure caption “the data were binned according . . .” in the main text to help the readers understand how these two plots are derived.

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