

**Comments on “Influence of biomass burning plumes on HONO chemistry in eastern China”  
by Nie et al.**

This manuscript presents two-month measurements of HONO at a suburban site in the western Yangtze River delta, eastern China. The authors showed the influence of biomass burning plumes on the HONO concentrations and carefully discussed the potential mechanisms and implications. The interaction of atmospheric oxidants with aerosols is an important aspect of atmospheric sciences and the subject fits well the scope of ACP. Overall, I found this work interesting and promising. My only concern is that the increase of the HONO concentration or HONO/NO<sub>x</sub> ratio doesn't necessarily mean an enhanced production. It can also be explained by reduced photolysis in the presence of thick plumes. I would recommend publication if the authors could address this problem and other issues as listed below.

**General comments**

The authors have discussed two kinds of effects of biomass burning plumes on the measured HONO concentrations: direct HONO emissions and secondary HONO formation from heterogeneous reactions. However, the aerosols plumes can attenuate the solar radiation leading to reduced photolysis and vertical mixing. This may also cause an accumulation of ground level HONO and elevated HONO/NO<sub>x</sub>. Due to fast photolysis, the daytime budget of HONO is quite different from that at the night time as reflected by their distinct HONO/NO<sub>x</sub> ratios (Qin et al., 2009; Sörgel et al., 2011). I am wondering if the day/night differences of HONO/NO<sub>x</sub> also exist in the present study. I would suggest the authors to include the solar radiation as another dimension in their analysis.

**Specific comments**

Page 7860 line 14: “A mixed plume of BB and anthropogenic fossil fuel (FF) emissions was observed on 10 June with even higher HONO concentrations and HONO/NO<sub>2</sub> ratios.”

Is it because higher PM concentrations reduced solar radiation and the photolytic sinks

Page 7861 line 9: “Among these sources, heterogeneous processes are commonly accepted as the dominant, yet least understood, pathway to produce HONO.” If it is the least understood pathway, it shouldn't be accepted as the dominant source. I would suggest the authors to change this formulation.

Page 7863 line 15: the stripping solution of WRD determines its sampling efficiency and potential artifacts. The authors should justify the use of H<sub>2</sub>O<sub>2</sub> stripping solution. Genfa et al. (2003) could be a nice reference for this purpose, in which intercomparison was carried out to demonstrate the performance of H<sub>2</sub>O<sub>2</sub> solution.

Page 7863 line 19: *“The residence time of sampling air is actually very short in the sampling tubes (about 4.5 s) and WRD (about 0.2 s), the artifact caused by the NO<sub>2</sub> conversion on the surface of the sampling tube and WRD solution is therefore small (Spindler et al., 2003).”* If the short residence time is sufficient for complete absorption of HONO, it could be long enough for artifact production as well. However, I don't think artifacts should be a problem here. The use of acidic stripping solution has to a large extent avoided the formation of artifact NO<sub>2</sub>. In addition, H<sub>2</sub>O<sub>2</sub> can rapidly oxidize HSO<sub>3</sub><sup>-</sup> and further inhibit the artifact reactions.

Page 7863 line 26: it is better to mention that the NO<sub>2</sub> measurements are subject to artifacts due to the use of the Molybdenum converter. This is important when comparing the present results (HONO/NO<sub>2</sub>) with other artifact-free measurement (e.g. DOAS).

Page 7864 line 19: can you also show the diurnal variation of HONO/NO<sub>2</sub>, HONO/NO<sub>x</sub> during BB and non-BB periods. Also I would suggest marking the BB and non-BB periods in Fig. 1.

Page 7864 line 21: *“The samples with potassium concentrations higher than 2 μg m<sup>-3</sup> and the ratio of potassium to PM<sub>2.5</sub> larger than 0.02 were defined as BB samples, the remaining ones being categorized as non-BB samples.”* Can the authors explain why 2 μg m<sup>-3</sup> and potassium to PM<sub>2.5</sub> ratio of 0.02 were chosen as threshold values? Are these numbers critical for the conclusion? What the figure will look like if you plot hourly “HONO/NO<sub>2</sub>/particle surface areas” against “K<sup>+</sup>/PM<sub>2.5</sub>”?

Page 7865 line 16: *“At least several hours were therefore needed before the BB emissions get to our measurement site, so the contribution of direct emissions to the observed HONO can be considered negligible.”* Negligible means the life time of HONO is much shorter than the transportation time. Could you estimate the life time of HONO in the BB plumes? The radiation might be largely reduced in the presence of BB plume and the life time might be longer than expected.

Page 7865 line 23: “no difference in the ground surface between the BB and non-BB periods during the campaign, the elevated HONO concentrations observed during BB episodes are expected to be due to aerosol-related heterogeneous processes.” and Page 7866 line 1: “Given that there was practically no difference ... to aerosol-related heterogeneous processes.”  
As aforementioned, aerosol plumes could reduce the solar radiation and photolysis of HONO resulting in elevated HONO concentrations, which might also be able to explain the observations.

Page 7866 line 8: “In Fig. 7, we selected the samples ... The results showed a significantly larger surface area concentration for BB aerosols compared with non-BB aerosols.”

In Fig. 7, is the difference in surface areas concentrations caused by the density difference or size distribution difference (between PM1 and PM2.5)?

Page 7866 line 19: “The values of this ratio were 40% higher during the BB period than that during non-BB period (Fig. 8), indicating a higher NO<sub>2</sub> conversion efficiency of BB aerosols.”  
The same concern as in my general comments.

Page 7867 Section 3.3: During the mix plume periods, the RH approached 90%. For the same PM<sub>2.5</sub> (dry mass concentration), higher RH results in higher ambient PM concentration, thicker aerosol optical depth, and less radiation. Will it help to explain the higher HONO/NO<sub>2</sub>? I would also suggest including HONO/NO<sub>2</sub> ratios into Fig. 11.

Page 7876 Figure 1: a comma is missing between NO<sub>2</sub> and PM<sub>2.5</sub>.

### Technical corrections

Page 7860 line 9: “was not associated ‘with’ potassium”

Page 7860 line 11: “principle” or “principal”?

Page 7861 line 22: do you mean “gaps”?

Page 7860 line 18: “to HONO formation”, changed to “to the HONO formation”

Page 7860 line 19: “suggests an important role of BB in atmospheric oxidation capacity”.

Here” role in ... capacity” doesn't sound good, you can say “suggests an important role of BB in atmospheric chemistry”

### Reference

Genfa, Z., Slanina, S., Brad Boring, C., Jongejan, P. A. C., and Dasgupta, P. K.: Continuous wet denuder measurements of atmospheric nitric and nitrous acids during the 1999 Atlanta Supersite, ATMOSPHERIC ENVIRONMENT, 37, 1351-1364, [http://dx.doi.org/10.1016/S1352-2310\(02\)01011-7](http://dx.doi.org/10.1016/S1352-2310(02)01011-7), 2003.

Qin, M., Xie, P. H., Su, H., Gu, J. W., Peng, F. M., Li, S. W., Zeng, L. M., Liu, J. G., Liu, W. Q., and Zhang, Y. H.: An observational study of the HONO-NO<sub>2</sub> coupling at an urban site in Guangzhou City, South China, *Atmospheric Environment*, 43, 5731-5742, 10.1016/j.atmosenv.2009.08.017, 2009.

Sörgel, M., Trebs, I., Serafimovich, A., Moravek, A., Held, A., and Zetzsch, C.: Simultaneous HONO measurements in and above a forest canopy: influence of turbulent exchange on mixing ratio differences, *Atmos. Chem. Phys.*, 11, 841-855, 10.5194/acp-11-841-2011, 2011.