

# ***Interactive comment on “Ambient Nitro-Aromatic Compounds – Biomass Burning versus Secondary Formation in rural China” by Christian Mark Garcia Salvador et al.***

## **Anonymous Referee #1**

Received and published: 9 November 2020

### General:

This paper provides measurements of nitro-aromatic compounds in rural China using FIGAERO-TOF-CIMS and provides insight into the formation mechanisms of different compounds. This paper is well-written and a nice addition on this particular topic. I recommend publication after consideration of my comments.

### Major Comments:

1. I suggest that the authors perhaps bring up occurrence of high sulfate haze events in China during winter, which has received a lot of attention, as additional motivation for better understanding more of these surprising secondary processes that dominate

PM in China during winter. I think this will broaden the readership of their work.

2. I suggest that the authors provide more information regarding the calibrations in the SI. Calibrating this instrument for NACs is challenging and the atmospheric community would benefit from a detailed description of how the authors performed their calibrations.

Specific Comments:

Introduction:

1. Page 3, Line 28 has a few grammatical issues. Remove “causes” and also “to” in between “results” and “strong”
2. Page 4, Line 16: please also cite [Lee et al., 2014]

Methods:

1. Section 2.1: I'd be curious to see HYSPLITs and also know if any information is available regarding boundary layer heights—both of which I think will help interpret the data. As the authors correctly point out, the AtChem model does not factor in meteorology and adding this information will help augment the chemical interpretation of their work.
2. Page 5, Line 15: is this a WHO limit or a national limit for PM<sub>2.5</sub>? Please clarify.
3. Page 6, line 3, please rename to Filter Inlet for Gases and Aerosols (FIGAERO) coupled to a chemical ionization time-of-flight mass spectrometer (TOF-CIMS).
4. Page 7, were any reference gases added to monitor drift in signal? Were any gas-phase zeros performed?
5. Page 7, Line 21: some of these compounds are light sensitive, were they used fresh for calibration?

Results:

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1. Figure 2: I suggest pointing out the levoglucosan peak in the spectra and other markers used to indicate BB events.
2. Page 10, Line 9: your diurnal peaks in particle phase NACs are similar to those from residual wood burning seen in Gaston et al 2016.
3. Page 17, Lines 20-22, I'd be curious to know if the secondary production of NACs was also correlated with OOA from the AMS measurements.
4. Figure 8, any explanation for why the model shows poor agreement from 12/27 until 12/31?

#### References:

Lee, B. H., F. D. Lopez-Hilfiker, C. Mohr, T. C. Kurten, D. Worsnop, and J. A. Thornton (2014), An iodide-adduct high-resolution time-of-flight chemical-ionization mass spectrometer: Application to atmospheric inorganic and organic compounds, *Environ. Sci. Tech.*, 48, 6309-6317.

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