

This study by Jiang et al. investigated the formation, chemical composition, and optical properties of secondary organic aerosol (SOA) formed from ozonolysis and OH radical oxidation of indole, one of the important nitrogen-containing heterocyclic VOCs. With the presence of NO<sub>2</sub>, indole SOA formation potential decreased by a factor of two, but the light absorbing potential per mass was higher by a factor of 5. Using mass spectrometric techniques, the authors showed that the presence of NO<sub>2</sub> shifted the indole SOA formation chemical pathways, in which the formation of 3-nitroindole was significantly enhanced, hence affecting the overall SOA chemical composition and optical properties. This is an interesting study and valuable to the community of Atmospheric Chemistry and Physics. Overall, the authors appear to have addressed the reviewers' comments but there are some additional concerns associated with their responses.

#### Specific comments

- Figure S10: While the thermogram of C<sub>8</sub>H<sub>6</sub>O<sub>2</sub>N<sub>2</sub> indeed looks a single peak, other compounds (i.e., C<sub>8</sub>H<sub>7</sub>O<sub>4</sub>N, C<sub>8</sub>H<sub>7</sub>O<sub>3</sub>N, and C<sub>8</sub>H<sub>5</sub>O<sub>3</sub>N) show much broader peaks, which are characteristics of multi-modal peaks. At least from calibrations, a single compound shows a very similar peak width (Lopez-Hilfiker et al., 2014; Stark et al., 2017), meaning that wider peaks indicate the presence of multiple peaks next to each other. This may be due to the presence of isomers with different volatilities or due to the effect of thermal decomposition. Therefore, I suggest rephrasing the sentence "... detected by FIGAERO-CIMS exhibit only one peak in the thermogram and no substantial fragmentation." 2-D thermogram (Wang and Ruiz, 2018; Takeuchi et al., 2022) may be a better way to illustrate the absence/presence of thermal decomposition in a holistic manner.
- Line 166ff: I agree with the authors that at this current moment, it is very difficult to do calibrations of all the detected organic compounds and it is not unreasonable to assume uniform sensitivity. However, I believe it is still important to note here that I-CIMS sensitivity could easily vary by a few order of magnitude reported in literature (Aljawhary et al., 2013; Lee et al., 2014), and therefore the reported fractions do not speak to the actual abundance. One idea is to explicitly say XXX% of the sum "signals" so it is obvious that percentage does not correspond to mass fraction.
- Line 200: I suggest rephrasing this sentence. Reaction of gas-phase RO<sub>2</sub> with NO<sub>2</sub> produces peroxy nitrate (RO<sub>2</sub>NO<sub>2</sub>), which is thermally in equilibrium with RO<sub>2</sub> and NO<sub>2</sub> (Orlando and Tyndall, 2012). Production of fragments is typically from reaction of RO<sub>2</sub> with NO, RO<sub>2</sub>, and NO<sub>3</sub> that generate alkoxy radical (RO), which may undergo fragmentation pathways.
- Section 3.4 and Figure 5: Do you know how much indole reacted with ozone vs. OH vs. NO<sub>3</sub> radical? From Figure S2, almost all indole appears to have reacted away before the addition of TME (source of OH radical), and I wonder if that means NO<sub>3</sub> radical oxidation was the major oxidation pathway of indole in AS-NO<sub>2</sub> experiment, as opposed to ozonolysis.
- Table 1: I suggest adding [O<sub>3</sub>] as well because it is one important parameter of experimental condition. Also, the SOA mass concentration should be added next to the SOA yield because the SOA yield is a function of the SOA mass concentration and cannot be simply compared without it.
- SI Text 3 (SOA density): I suggest that the authors consider revising this paragraph incorporating their response to the comment 8 by the reviewer 1 to be complete.

## References

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