Review of Li, et al., ACP-2024-367

This paper presents HONO and associated measurements from a ground site in Beijing during the months of June to October, 2021. There is an attempt to assess non-traditional sources of HONO (sources aside from OH + NO) using co-measured NOx and particle loading. There is a serious flaw in the paper that cannot be overcome by revision or further analysis (see below). The full paper as submitted here should be rejected for publication. However, the authors should consider publishing the HONO data and associated summary of measurements from Beijing (there have been many) as an ACP Measurement Report. I have the following General and Specific comments.

General comments.

The major flaw in this paper is there is only an NOy measurement at the site, which we know to measure not just NOx but also PAN, HNO_3, particle, and alkyl nitrates (NOz compounds). The authors try to argue that the impact of NOz compounds is minor. We know that this is not true, especially for the mid-day period when NO_2 is below 10 ppbv, and there is obvious O_3 production (see for example Zhang et al., 2015, Zhang et al., 2023 (which shares some authors with this paper). Under these conditions in particular, equating NO_2 with NOz will result is errors of factors of 2 -3 at least. All of the interpretation that the authors try to do with this data is fatally flawed.

We are given essentially no details about the measurement site and are given only references to describe the HONO measurement. So, we have no idea if the method has interferences, from other N compounds aside from NO_2. We have no idea what materials that might from or store HONO (soil, asphalt) surround the site. At least a brief description of these is necessary.

The authors basically prescribe nighttime OH, and broadly parameterize daytime OH. We know that there is substantial variability in OH, so these shortcuts will mask much of the chemical dependencies that the authors are trying to uncover in their analyses.

In several places the authors try to use slight differences in R^2 to say something about what factors might be more responsible for HONO. In some cases these R^2 values are below 0.1, which means neither factor was significant. Such an analysis doesn't tell us anything.

Specific comments

The abstract is too long and has too much detail.
Line 20. What are the units of P_{emis}?
Line 29. The solubility of HONO is very much pH dependent. Below its pKa (3.28) it is not very soluble.
Line 55. The authors have missed VandenBoer, et al., 2015.
Line 70. This sentence is meaningless.
Line 89. The authors did not tell us what the impact of their measured HONO on O_3 production was.
Line 113. “was” should be “were”

Line 119. OH is going to vary a lot, with dependencies that are very much at the heart of what the authors are trying to get at. This specification is not very useful.
Line 141-142. What were the averaging times for these?
Line 158. Don't know what a "brief combing" is.
Line 194-195. This isn't what Figure 4a shows.
Line 216. Emission factor of what?
Line 225. Too many significant figures, only 3 at most are justified.
Line 241. What are the units?
Line 268. Too many significant figures, only 2 (3 for the last number) at most are justified.
Line 280. There is no explanation and justification for this equation.
Line 294. You haven't described the observation location.
Line 3456. The averaging here (monthly) is so broad as to make this statement meaningless.
Line 373. See previous comment about Henry's coeff.
Line 425-435. This could all go in the SI, there is nothing new here.
Line 450-459. This could all go in the Conclusions.

References

VandenBoer, et al., Nat. Geosci., 8, 55-60, 2015.
Zhang, G., et al., Atmos Environ., 103, 289-296, 2015.
Zhang, H., et al., Sci. Total Environ., 905, 166852, 2023.