Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-150-RC3, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The promotion effect of nitrous acid on aerosol formation in wintertime Beijing: possible contribution of traffic-related emission" *by* Yongchun Liu et al.

Anonymous Referee #3

Received and published: 9 April 2020

The title of this paper is very intriguing that (1) wintertime HONO promotes aerosol formation and (2) >50% of observed HONO is traffic related in Beijing. After reviewing this paper, I think it will be a grave mistake if the editor decides to publish this paper with these two conclusions in any form. The conclusions are pure speculations. I find no evidence to support either of the two claims in this paper.

The discussion for conclusion (1) is in section 3.2. One of the many mistakes in this section is that the authors do not understand that the largest source of OH is from the reaction of HO2+NO. Even when OH production from HONO photolysis is larger than from O3 photolysis, the effect on OH is much smaller than the photolysis rate

comparison. Line 301-304 is based on another paper; the data in this paper do not either support or dispute that oxidation by OH promotes aerosol formation. Figure 2D is used at the observation evidence supporting conclusion (1). There are many reasons that HONO/CO correlates with OA/CO. For example, CO is primary in winter in Beijing. If HONO and OA variations are from secondary sources, there will be high correlations as shown. Line 318 states "... it was reasonable to mainly ascribe the increase of OA concentration to local secondary formation initiated by OH radical from HONO photolysis." It is a pure speculation. The observation data in this paper do not support this statement. It is the same with Line 328. The vague statement cannot be supported by the data in this paper. Line 332 is again a speculation. Ammonia is mostly neutralized by sulfate in Beijing. Line 338-400 is another speculative and ambiguous statement. Line 345-345 cites other people's work but is not supported by the data in this work.

Conclusion (2) is based on some calculation that was not described in the paper. Line 376-378 states that the mean emission factor is 1.17% with a lower limit of 0.18% and an upper limit of 1.8%. (Why is the mean so close to the upper limit and 6.6 times larger than the lower limit?) The mean value is similar to previous studies and is not the reason for conclusion (2). Line 381 gives a vehicle HONO emission rate of 0.085 to 0.34 ppbv/h. The unit implies some volume was used in the calculation. No discussion was given on what volume was used and how it varied in a day. Another important factor not considered in this study is the outflow of vehicle HONO and NOx by advection at night. It is the largest sink at night but is not included in the budget discussion. The nighttime source of NO2 from the ground is 38 times less than vehicle emissions. However, no other paper I know of found that HONO concentrations at night cannot be explained mostly by a ground source. It led me to conclude that the vehicle HONO emission source in this paper is overestimated by 10-100 times. The authors should look at previous modeling papers that included vehicle HONO emissions. What they found is that the effect of vehicle HONO emissions is small.

In summary, I think that the calculation and reasoning of this paper are either incorrect or ambiguous. It does seem likely that any revision can correct the flaws in the two main conclusions. I suggest that the authors scratch the conclusions and redo the analysis of their observation data.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-150, 2020.