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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement report: Distinct Emissions and Volatility Distribution of Intermediate Volatility Organic Compounds from on-road Chinese Gasoline Vehicle: Implication of High Secondary Organic Aerosol Formation Potential" *by* Rongzhi Tang et al.

Anonymous Referee #2

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General comments

The manuscript presents novel data regarding IVOC emission factors for a gasoline/E10 Chinese vehicle, that meets China V standard. Methods are sound, the language is cogent and very easy to follow. The presentation of the results is very clear and the main findings are thoroughly discussed and compared to previous literature, considering differences and similarities with US-based data. As the paper entails im-

portant implications for both the scientific community and policymakers, I recommend final publication after minor revisions. The following comments are mostly aimed to improve the readability, interpretability and usefulness of the study for future work.

Specific comments

1. To facilitate the use of your new data in modeling studies using the Volatility Basis Set (VBS) scheme, I would recommend to present the volatility distribution data also in terms of saturation concentration bins, in a similar way to Zhao et al. [2016] (Figure 4). Also, it would be convenient if you can report a Table, maybe in the SI, reporting the mass fraction distribution of organics for each saturation concentration bin (e.g. Table S5 in Zhao et al. [2016]). These values are usually a key input for the VBS schemes in state-of-the-art numerical models. In addition to this, I would suggest to report the median IVOC-to-THC ratio in the abstract as well, as that is key information for modelers.

2. In the "atmospheric implications" section, I would suggest to at least mention the possible limitations of the study, and maybe possible future directions. One example could be the fact that only one vehicle was tested (China V), and different values might be obtained for different vehicles (even vehicles that meet the China V emission standard), implying that the total uncertainty associated with the estimated emission factors might be larger. Also, when discussing why your estimate of total IVOC emissions in China is conservative (lines 476-480), can you report what is the current percentage of vehicles that meet the China V standard in the Chinese car fleet? This would help the reader understanding the extent of the implications of the assumption made in estimating that the total IVOC emissions in China are 30 Gg.

3. In Section 3.3, you mention several times that recent Chinese regulations failed in controlling PM emissions (and IVOC emissions as well), whereas they were effective for NOx and THC, according to your data. Can you expand on that? Which regulations did they implement? Why do you think they were ineffective for PM and IVOCs but

effective for NOx and THC? Maybe some additional references might help – Expanding the discussion on this point can be useful to guide policymaking.

4. Some claims in the introduction can be better substantiated by referencing previous literature. E.g. lines 58-59 "A large discrepancy remains between modeled and measured SOA. One possible reason is missing SOA precursors." Two recent modeling works that discussed these two points are Giani et al. [2019] in Europe and [Huang et al., 2020] in China, and I suggest to add a citation to strengthen your claims. In the introduction, I would also stress the point that understanding and characterizing IVOC emissions, as well as their volatility distributions, is crucial for improving numerical models that aim to predict OA.

5. I am a little skeptical about the parametrization presented in Section 3.5, which seems somewhat arbitrary. Does the logarithmic curve have some sort of physical insight or is it based only on the shape of the calculated curve? Why not using something like k-exp(. . .) as in the actual model used to derive that curve (Equation in Section 3.4), also because you're claiming that after 24h SOA/POA is approximately constant? The other concern that I have is that there are a lot of parameters to be estimated (9 in high-NOx conditions), which might cause overfitting to your data, thus losing generalizability. Is it a specific reason why you're using so many parameters? Is there a way of having a simpler parametrizations with similar fit performance? If so, a simpler model (i.e. with less parameters) should be preferred. I would suggest that at least you should better justify your choices for the proposed parametrization in Section 3.5. I believe that Section 3.5 can be largely improved, either by better substantiating your choices or performing some further calculations (that might exceed the scope of the paper, though).

6. What are the dots in Figure 5? Please explain in the caption. (I'm assuming is the SOA/POA ratio to be read on the right scale?)

References

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