EGUsphere, referee comment RC1
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Comment on egusphere-2022-1440

Anonymous Referee #1

Referee comment on "Elucidating ozone and $PM_{2.5}$ pollution in the Fenwei Plain reveals the co-benefits of controlling precursor gas emissions in winter haze" by Chunshui Lin et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1440-RC1, 2023

General comments:

The paper titled "Elucidating ozone and $PM_{2.5}$ pollution in Fenwei Plain reveals the co-benefits of controlling precursor gas emissions in winter haze" by Lin et al. evaluates the status of ozone and PM2.5 pollution in a typical megacity of the Fenwei Plain, one of the most polluted regions in China, which reported a general trend of increasing secondary pollution (ozone and SOA) in winter haze, and the causes of this trend and the possible measures in controlling the complex pollution by O3 and PM2.5 were further studies and discussed. With this, the authors claimed that the co-benefits of reducing NOx and VOCs simultaneously in reducing ozone and SOA, that would be also suitable for other polluted regions of China suffering ozone and PM2.5 currently. The manuscript was well written and presented clearly. Therefore I recommend the publication of Lin et al. work after some issues were properly revised and improved.

Specific and technical comments:

- Method, more details in the calibration of PTR-MS should be provided. In addition, What kinds of VOCs species were used in the standard mixture? Please list the VOCs species that calculated from the kinetic rate constant, and the uncertainty on the calculated VOCs should be discussed.
- Line 109-112, it is better to provide more details for NR-PM2.5 monitored by an AMS which usually measured NR-PM1. I note that a novel PM2.5 was firstly equipped with AMS for the winter campaign in 2014 (Elser et al., 2014), It is unclear for the other winter campaigns.
- Line 121-122, why the reduction in NO_2 for the observation sites was not used? Which would be more precisely than the satellite image.
- Line 144-146, please list the VOC/VOCs information that used as input data for box model. I note that HCHO was not used to constrain the model, how about the other OVOCs? Considering the OVOCs was also from secondary formation. In addition, I am concerns on the model performance in the ozone simulations, as the majority of

alkanes was unavailable in the model if only the VOC/VOCs measured by the PTR-MS. As least, the authors should provided more details in the performance of the box model and the analysis in the uncertainty.

- Line 200-202, I do not agree that the secondary formation could be the major source of formaldehyde, as the measured and modelled formaldehyde showed different diurnal pattern. The similar level may suggest large uncertainty in the modelled formaldehyde.
- Line 245-247, the significant reduction in primary fossil fuel OA (77%) from 2012-2014 to 2019-2021 could be expected, due to the implementation of the clean air act in 2013. The more magnitude of reduction in cooking OA (84%) is interesting, more evidence should provided and discussed here.