

Comment on acp-2020-1298

Anonymous Referee #2

Referee comment on "OMI-observed HCHO in Shanghai, China, during 2010–2019 and ozone sensitivity inferred by an improved HCHO/NO₂ ratio" by Danran Li et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1298-RC1>, 2021

This paper uses satellite HCHO/NO₂ to investigate the ozone sensitivity in Shanghai, China. While the linkage with surface observations is novel, there are several issues with the manuscript:

- The novelty of this paper is the inclusion of ground-based observations, but I think the ground-based measurements are underused in this study. Since a main part of this study is on HCHO, I don't see how the authors use ground-based measurements of HCHO to support satellite HCHO. Do you see similar temporal patterns from ground vs. space? This may also help understand the difference between column vs. surface HCHO.
- There seems to be some artificial strip patterns with HCHO (Figure 2), which looks like due to the influence of OMI swath changes. It is not clear how the authors process OMI HCHO data. The authors mentioned they re-grid the data to 0.01° × 0.01°, which is much finer than the resolution of OMI. No details are provided in terms of spatial downscaling. In general, spatial oversampling is used to process OMI data to achieve better resolution (e.g. Zhu et al., 2014). I suggest the authors consider following such procedure.
- It's not clear to me how the authors explore the impacts of anthropogenic emissions on HCHO. There seems to be several issues. First, the authors only consider the primary emissions of HCHO, but a lot of HCHO is produced secondarily from other VOCs like alkene. The HCHO yield should also vary with VOC species, and also meteorology. Second, as I pointed out earlier, the authors did not consider the role of biogenic emissions especially isoprene. Without secondary HCHO, there is little we can learn about the driven factors of HCHO from this paper.
- Recent literature report there is uncertainty with the regime threshold for HCHO/NO₂. The authors consider the uncertainty with diurnal cycle, but even at the overpass time, the regime threshold may also vary (Shroeder et al., 2017; Jin et al., 2020; Sourì et al., 2020). I suggest the authors be more cautionary about applying the thresholds to separate regimes. More validation analysis is needed to support their regime classification.
- As I commented previously, the correction for diurnal variation doesn't make sense to me. First, the authors did not consider the difference between column-based satellite HCHO/NO₂ vs. surface observed HCHO/NO₂. Given the variation of the boundary layer height, the relationship between surface and column HCHO/NO₂ should also vary with

time. Second, it's not clear to me why the authors use $\hat{\sigma}_{O_3}$ to weight FNR. If the authors are only interested in the time when ozone production is most efficient, wouldn't it be easier to look the 1-hour maximum ozone? Third, there is no evidence supports whether such changes actually improved the regime classification.

Minor Comments:

- Lines 116 to 120: Do you see similar seasonal cycle of HCHO from ground?
- Figure 1: Please define season here.
- Figure 1: I'd suggest include error bars to indicate spatial variation.
- Figure 3: Why did you choose to show seasonal cycle only? I think it will be more interesting if you can show the time series from 2010 to 2018, and see how HCHO is correlated with each factor. This may also help explain the inter-annual variability of HCHO.
- Figure 4: Need to include secondary HCHO from both anthropogenic and biogenic VOCs.
- Figure 7: How do you define urban vs. rural areas?
- Figure 8: It's unclear whether you're showing FNR and ozone for one site or three sites together? If one site, which site?

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Souri, A. H. et al. Revisiting the effectiveness of HCHO/NO₂ ratios for inferring ozone sensitivity to its precursors using high resolution airborne remote sensing observations in a high ozone episode during the KORUS-AQ campaign. *Atmospheric Environment* **224**, 117341–12 (2020).

Jin, X., Fiore, A., Boersma, K. F., Smedt, I. D. & Valin, L. Inferring changes in summertime surface ozone-NO_x-VOC chemistry over U.S. urban areas from two decades of satellite and ground-based observations. *Environ Sci Technol* (2020)
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