Review of Ren et al.

Major comments:

I am concerned that the authors suggest the need to shift from a focus on NO_x reduction to VOC reduction. Once NO_x reductions cease to result in increased rates of ozone production due to the non-linear response of ozone to NO_x, ozone levels will again start to decrease. Thus continued NO_x reductions will eventually reduce ozone even without reducing VOCs. Certainly there are plenty of reasons to reduce VOCs, but continued NO_x reductions will eventually reduce ozone and efforts should not move away from reducing NO_x. The authors could consider calculating at what point that might start to occur. This paper, Wang et al., 2022 - https://doi.org/10.5194/acp-22-8935-2022 – used box modeling to show that cities transitioned from VOC-limited to a transition regime, and concluded that China needs concurrent reductions in both VOC and NO_x reductions. The authors need to address the findings of this paper, and differentiate their analysis, which is similar in assessing the long-term trends in ozone.

The authors do not provide a convincing argument that the increase in satellite HCHO is due to a lack of emissions reductions. According to Zheng et al., 2018a referenced by the authors, the VOC composition has changed significantly with reductions in residential and transportation VOCs but increases in industrial and solvent VOCs. The authors need to discuss the impact of the change in VOC composition, particularly any shift in yield of HCHO from the change in VOC mixture and the reduction in NOx. Also, the authors do not address whether biogenic VOCs have any impact on the VOC budget. In addition, ozone is co-produced with formaldehyde in high-NO_x conditions, and thus as VOC oxidation increases as NO_x levels go down, it is possible that increased HCHO production and even a lengthening of the HCHO lifetime buffers the signal from any VOC emissions reductions.

I do not believe that the analysis presented by the authors is convincing and novel in its current form. I would suggest that the authors improve their analysis and resubmit at a later time. The manuscript could also benefit from an English language edit.

Minor comments:

Line 54 - Schroeder et al., 2017 (http://dx.doi.org/10.1002/2017JD026781) showed that there is a large range of ambiguity in CH2O/NO2, and that inhomogeneities in vertical mixing degrade the ability of column HCHO/NO2 to classify surface ozone sensitivity. They also show that ozone sensitivity on exceedance days may be different than non-exceedance days. The authors need to address these points.
Line 59 – According to Zheng et al., 2018a, the change in VOC source categories is large. Does this have an impact on HCHO?
Line 80 – I am unable to access this link - (http://106.37.208.233:20035/).
Line 81 – Can you explain what you mean by 'unified to the reference state'?
Line 83 – Do you mean the daily average?
Line 98 – Do you mean 'chemical transport model'?
Line 105 – Why not just use OMI through 2021?
Line 110 – Can you just clarify whether OMI and TROPOMI use the same NO2 a priori?
Figure 1a – Just show the ozone season, May-July. Clearly from panel b) there is a strong seasonality, and we are more interested in increases in ozone during that time that annual changes. On panel b, show a trend just through May-July vs. Dec-Feb.
Figure 3 – Why is this data now only April – September? Be consistent with your time periods. Panel b) – Is this just 13-14 local time?
Line 191-192 – I do not understand the reference to Pusede and Cohen, 2012. Nowhere in that paper do they show ozone as a function of HCHO/NO_2.
Given that Wang et al., 2022 show different trends in Beijing, Shanghai, and Guangzhou, the authors should take the approach of Jon and Holloway (2020) and separate their analysis by city (i.e. Fig. 1b).
Could the authors please explain why they averaged the data into half month increments.
Line 195 – I think it would be more useful to show Figure S2 in place of Figure 3. I am not sure how it is useful to average all the sites together when there are strong regional differences.
Line 201 – You say "This varied response is also reflected in different thresholds for other photochemical indicators … or patterns of O3 isopleths…". Please be specific about what these other studies say and how it fits with your results, right now this sentence is too speculative to be useful.

Line 202 – You should have the information to actually discuss changes in temperature and radiation across these regions and be a little more specific about whether the could impact HCHO/NO2.
Line 207 – Give the previously reported values.
Line 216 – Given that you have shown strong regional differences, I do not think it makes sense to show Fig. S3 at all, and move forward only with regionally-specific analysis.
Figure S6 – It is very confusing to have the timeseries split between the supplement and main text. I don't see why putting Fig S6 into Figure 5 and 6 would be problematic.
Figure 5b and 6b – Why are you showing annual trends when your ozone analysis is April – September? Stick to analyzing the same time period throughout.
Line 251 – I don't understand the statement "Based on the non-linear relationship of O_3−NOx−VOC, the changes in ozone precursors is the direct factor affecting ozone level." Are you trying to argue that other factors like meteorology or changes in PM are not the cause? If so, this statement is not convincing as there has been no discussion of these other factors.
Figure S7 – Show the same thing for HCHO.
Figure 6b – Are those trends really statistically significant? Also you need to look at just April-September.
Line 269 – Could it not also be an indicator that as urban ozone titration decreases, and OH increases due to the reduced NOx level, that oxidation of VOCs increases?
Line 271 – The authors have not discussed the importance of biogenic isoprene to the VOC budget.
Line 273 – Show us a temperature timeseries.
Line 311 – The authors are assuming that HCHO columns directly relate to VOC emissions, while changes in chemistry due to NOx reductions could have a large impact (see my above comment). Thus I do not think you can state that the increase in HCHO is due to increased VOC emissions without further evidence. The authors need to explain how the "vigorous development of stall businesses" could cause this increase.
Line 325 – The authors haven't discussed PM2.5 at all to support this statement.
Figure 8 – I don't understand the purpose of this figure.