**General Comments:**

The paper by Zhang et al. entitled “Impact of reduced anthropogenic emissions during COVID-19 on air quality in India” is on a very relevant and interesting topic which is to use the covid lockdown emission reductions for assessing impacts on air quality over India. Unfortunately, the analyses and interpretation are weak in several places (listed below). There are hardly any new trustworthy insights from this modelling study which have not been reported already by the authors in previous works on the same topic published recently (see Sharma, S., Zhang, M., Anshika, Gao, J., Zhang, H., and Kota, S. H.: Effect of restricted emissions during COVID-19 on air quality in India, Science of The Total Environment, 728, 138878, https://doi.org/10.1016/j.scitotenv.2020.138878, 2020). Instead there are even discrepancies from the earlier work based on interpretation of what appears to be the same measured dataset. While in the previous work (Sharma et al., 2020) it was reported that there was a 17% increase in ozone during COVID, in the present work it has been reported that a significant decrease in surface ozone (MDA8 values) occurred, without even clarifying what changed between the two studies except for additional modelling analyses in this study.

There are several major issues with the present submission which need to be addressed/clarified for meriting further publication in ACP.

**Validation of the model used in this work has not been done/described adequately:**

Authors use only 2 m level measurements of temperature and meteorological data and chemical data from 5 monitoring stations operated by the regulatory agency of India located within cities to compare their modelled output.

**Measured chemical data:** The authors present only daily averaged data in the plots (Figures 2 and 3). This would be fine but I could find no details of the original high resolution primary data (presumably available at temporal resolution of few minutes from the analyzers in the monitoring stations) to build confidence in the reader about the trustworthiness of the primary data and its quality assurance. If they could provide such high resolution data for the five stations (even for few days in both periods) for ozone , NO, NO2 , PM2.5 etc.. with gaps in measurements if any (after all there was a lockdown so maintenance could be difficult), and the calibration data of any of the analyzers, it would go a long way in instilling confidence in the highly averaged data. The reviewer looked up their previous study Sharma et al 2020 which has been cited for detailed description of the primary data and found that this reference did not contain these details and somewhat remarkably the Sharma et al. 2020 paper reported data until April 14$^{th}$ 2020 in that work, was submitted on April 16$^{th}$ , 2020 and accepted on April 19$^{th}$ , 2020. While this does not necessarily suggest that due diligence was not taken as given the nature of topic urgency to publish would have been a factor, the rapid turn-around time and lack of experimental details in the peer reviewed reference cited and which forms the basis of the daily averages does leave room for concern. So the authors should provide the original primary data as a time series for these 5 monitoring stations in the revised supplement alongwith details of calibration experiments and data quality control followed to allay such potential concerns about the primary measured dataset.

Also they should discuss whether data from 5 cities are adequate to make inferences about all of India with same degree of confidence which spans vast rural and countryside regions ? It might be advisable to better focus on the 5 cities alone for which they have the data and even there they should acknowledge how data from one monitoring station may be limited for representing air quality of the entire city. In fact a combination of monitoring station data and satellite data (agreed also can have issues but better than nothing) would be better.

**Validation of model using 2 m level measured meteorological data:**

For the kind of modelling investigation the authors are making namely, effect of emission changes on concentrations of pollutants use of only the 2 m level observations without comparison with satellite data, sonde data, mixing layer height data (see ERA5 products) seems to be a major shortcoming. Note that the changes in ventilation coefficient before and during lockdown and the changing season (Spring to Summer) can alone have big impacts on the concentrations.

**Changes in atmospheric chemistry of primary pollutant removal and formation of secondary pollutants:**

Currently the study tends to attribute all the observed concentration changes in pollutants primarily to the emission reductions. However it has been documented elegantly in the following paper: Kroll, J.H., Heald, C.L., Cappa, C.D. et al. The complex chemical effects of COVID-19 shutdowns on air quality. Nat. Chem. 12, 777–779 (2020). https://doi.org/10.1038/s41557-020-0535-z, that several other processes play a big role.

The purpose of using a model should be that these effects can be teased out through sensitivity experiments but unfortunately this has not been addressed in current version of the manuscript. For example the authors note that the temperature increased during the lockdown period. A key question is what effect the temperature change and the reduced emission of VOCs (no VOC measurements have been provided at all), NOx and CO would have on the removal rates of primary pollutants and formation of secondary pollutants.

Further have authors identified days when it rained in both pre covid lockdown and during lockdown periods which would cause strong biases for the comparisons.

**Existing inadequacies in VOC emission inventories and modelled ozone simulations over India:** While the authors are using pre lockdown and lockdown periods for comparison, it is a fact that of all emission inventories, VOC emissions are the most poorly constrained due to the absence of in-situ VOC data over many regions in India. A generic problem also seen is the tendency for overestimation of ozone by models over the Indian region. This suggests that the basic reactant mixture and chemistry are still inadequate for modelling ozone and secondary pollutant formation accurately over India. So how can one be sure that the changed chemical mixture between pre-lockdown and during lockdown are not skewed by these gaps in our basic undertanding? While it would be unfair to hold the authors to solve all these issues, one does expect that the limitations and existing issues are duly acknowledged in the work instead of making highly speculative and prescriptive measures for air quality mitigation based on such modelling results. Use of formaldehye for constraining VOC emissions where a large number of more reactive primary VOC emissions occur should also be discussed and clarified. Trusting the formaldehye from the model in absence of in-situ formaldehye measurements to compare with or even satellite or columnar measurements which have been reported from India is recommended.

Are benzene and toluene data available from the monitoring stations which could be included in the analyses? If so these should also be included in view of their health and SOA formation potential.

**Choice of scaling factors for emission reductions:** The authors make several assumptions and justification for the use of scaling factors for emissions which are valid (see Equations 1 and 2).

For example:

**Ammonia agricultural emissions:** Several satellite studies have indicated high ammonia emissions from agriculture and a recent by G.K. Singh, P. Rajeev, D. Paul, et al., Chemical characterization and stable nitrogen isotope composition of nitrogenous component of ambient aerosols, Science of the Total Environment, https://doi.org/10.1016/j.scitotenv.2020.143032 showed that agriculture activities and waste generation are major sources of ammonia. The assumption by the authors that the agricultural emissions do not change between pre-lockdown and during lockdown is not valid for large parts of the India in particular the Indo-Gangetic Plain because during the pre-lockdown dates farmers were still applying fertilizers to the wheat crops, whereas by last week of March this completely stops. So infact the ammonia and hence ammonium ion source from agriculture is likely stronger in pre-lockdown period and so cannot be treated as constant between both periods. As ammonia is such an important emission for PM2.5 too, this has large implications for the inferences currently drawn by the authors.

**Ozone production sensitivity indicator:** The use of HCHO/NO2 as based on Silman et al 1995 which the authors cite cannot be applied blindly because as noted by the original authors (Silman and He in their JGR paper in 2002) is suitable only for ambient ozone mixing ratios in the range of 80-200 ppb and then again for columns retrieved using satellite data. For ground based data, more robust proxies would be $H_2O_2$/ $HNO_3$ or even $O_3/NO_y$ . In the absence of measured VOC data presented by the authors to validate their model VOC data (note there are no measurements of HCHO presented), the authors should remove this discussion completely or present for each city site the high resolution $O_3$ Vs $NO_x$ data from daytime for pre and during lockdown periods.

In several instances, the grammar and language also need to be corrected. I recommend the authors to consider the above major concerns to revise and improve the manuscript.