Authors' responses to Referee #1. Reviewer's comments are in black text and authors' responses are in blue text.

Anonymous Referee #1

The authors investigate the NO2 changes over India during COVID-19 lock-down period using both satellite and in-situ measurements. The authors investigated the differences between rural and urban areas. The contributions from natural sources are also considered. The manuscript is easy to follow and the primary conclusions are sound. I recommend publication after revisions.

We thank you for recommending the publication of the article and appreciate the comments on the novelty of the study. Your constructive comments have helped us to improve the manuscript further. In the revised manuscript, the analysis has been updated using 5 km × 5 km TROPOMI VCDtrop NO2. We have also used ERA-5 data to investigate the changes in meteorological parameters (temperature, wind speed and boundary layer height) between BAU and lockdown. Section 3.8 linking the Google mobility change with NO2 change has been added. The limitations of the study have also been discussed in Section 3.9.

General comments:

1. Section 3.1. The authors considered the grids with zero fire anomaly to assess the changes in NO2 during the lockdown. How about the grid cells surrounding big fires? I would suggest remove those grids from final analysis as well, since their NO2 patterns are very likely driven by fires.

By considering the grids with zero fire anomaly, we excluded almost all the grids which have recorded fire activity during the analysis period. Upon further investigation, we find that a fire grid was surrounded by nearby fire grids in most of the cases (Figure 1 shown below) because the fires are likely to happen in fire-prone areas. Therefore, we mostly excluded the nearby grids covering big fires in our analysis. However, as the fire-plumes can be transported to long distances (longer than the nearby grids), the impact of long-range transport of forest fire plumes cannot be ignored over the areas with no fire activity. In the revised manuscript, we have included it as one of the limitations of our study in Section 3.9.

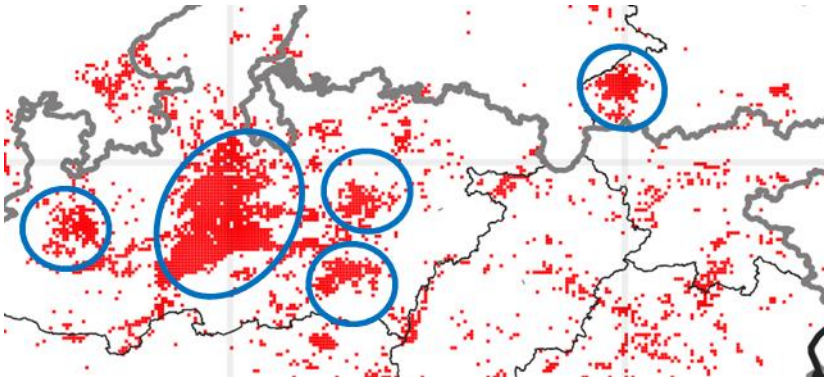

Figure 1. Zoomed map of fire activity over Central India. Blue circles indicate the clusters of fire activity

2. Section 3.5. I would suggest more investigation on the comparison between satellite and ground measurements. I'm not surprised by the low correlation between those two datasets in Figure 6. However, I don't see the reason why the correlation is even smaller during lockdown

period. Please clarify this in the text. In addition, which datasets can represent the local NOx emission changes better? Does the difference indicate the uncertainty of one dataset? I suggest addressing those questions when performing the analysis

The comparison between satellite data and ground-based measurements has its limitations and it is reasonable to raise a suspicion of the low correlation. The low correlation (0.45) between OMI and surface NO2 was also reported in Ghude et al. (2011). While they used data from a single site, our study includes multiple sites representing the satellite instruments' ability to capture the spatial heterogeneity. One of the reasons for the lower correlation can be the choice of surface station. Generally, urban background sites are preferred for this kind of analysis. However, the surface NO2 monitoring station type classification is not available for the CPCB sites; therefore, sites used in our analysis could be potentially impacted by traffic emissions resulting in lower correlation. Moreover, in-situ measurements are more sensitive to the local emission sources, whereas remotely sensed measurements provide values averaged over space as well as time. Therefore, in-situ measurements have larger variability than remotely sensed observations resulting in a low correlation.

Our analysis suggests that OMI and TROPOMI are sensitive to the emission changes at the surface because of positive correlation between the changes observed by space-based observations (OMI and TROPOMI) and the surface measurements. We find that TROPOMI has better sensitivity to changes than OMI because of a higher correlation.

Further, the reason for lower correlation during the lockdown can be linked to the lower NO2 levels (i.e., lower signals), resulting in a lower signal-to-noise ratio, therefore having larger uncertainty. We have updated this in the manuscript (now in Section 3.6)

3. Section 3.6. The authors remove grid cells with fire counts and power plants. How about other industrial plants? Will the grids with industrial plants bias the correlation between NO2 and population density?

India's industrial locations are often part of the urban agglomerates scattered around the city and are part of urban emissions. Therefore, we did not remove the industrial locations. To check for the bias, we have calculated the correlation between NO2 and population density after removing the data from industrial location and did not find a large difference in the correlation.

4. Conclusion. “The reduction observed over the urban areas was linked with reduced traffic emissions due to travel restrictions for COVID containment.” I would suggest a comparison with mobility data to support this conclusion.

Thank you for the suggestion. In order to link the observed reduction in NO2 levels with the traffic emissions over the urban areas, we analyzed the Google mobility percentage reduction for three mobility categories: transit stations, workplace and residential, along with daily percentage change observed by OMI, TROPOMI and CPCB across urban India from 1st March 2020 to 31st May 2020. We find that the percentage reduction observed by satellites and surface monitoring are consistent with each other and follow the same trend of the Google mobility reductions. The comparison is discussed in Section 3.8.

5. Simultaneous meteorology conditions. The authors mentioned that meteorology conditions constant during recent years by citing some references. In this way, the natural emissions are

not the driver of the emission changes. Since this is the foundation of the whole analysis, I recommend a sub-session to clarify this point.

We use ERA-5 data to investigate the changes in meteorological parameters (temperature, wind speed and boundary layer height) between BAU and lockdown and analyze the differences in probability density functions. The meteorological changes have been discussed in a new Section 3.1.

Specific comments:

1. Page 2, line 47. I suggest using the term of large to replace larger in the phase of larger localised emissions.

Thank you for the suggestion. The necessary change has been made in the text.

2. Page 2, line 59. The description of "spatio-temporal similarity with ground-based measurements" is confusing. Do the authors indicate the satellite and ground measurements share the similar spatial and temporal resolution?

Sorry for the confusing statement. We have modified the statement to "Spatio-temporal coincidence with ground-based measurements".

3. Figure 4. I suggest adding a map to show the definition of the domain of Central, NWest, IGP and so on. It will be easier for readers to follow.

A map of India along with different regions (shown in different colors) along with in-situ measurement locations is shown in SUP Fig. 1.