

Interactive comment on “COVID-19 lockdown induced changes in NO₂ levels across India observed by multi-satellite and surface observations” by Akash Biswal et al.

Anonymous Referee #1

Received and published: 10 December 2020

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.

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. 2. Section 3.5. I would suggest

C1

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1023>, 2020.

C2