

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 #2

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Many papers have recently appeared in the literature, including papers on India, documenting reductions linked to COVID-19 measures. A large fraction of those papers made use of TROPOMI and OMI observations. Therefore I asked myself the question: what is new, and what have I learned? The authors write: "While recent studies have utilised either only satellite observations or only surface observations, this study goes further by adopting an integrated approach by combining both measurement types" Indeed, the paper contains interesting plots showing how surface and satellite analyses agree well, and document the relative reductions during LDN for the regions and cities in India. What I found also interesting is the analysis of the land use dependence and impact of changes in fire activity. The paper by Biswal et al. is a well written, easy

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to read and clear paper, with good English. The paper contains an extended set of relevant references.

Because of the above I am in favour of publishing the paper after my comments have been taken into account.

Weather variability normally has a large impact on the BAU/LDN ratios. and can easily cause differences of the order of 20% in local differences between years. It is a bit of a pity that this aspect has not been explored by the authors. The paper is based on observations only, while models would need to be introduced to compensate for weather variability. Nevertheless, it would be good if the authors could provide a rough uncertainty estimate for the BAU/LDN relative changes due to the neglect of weather variability.

Substantial differences are found between the OMI and TROPOMI products: can those be understood? Newer satellite datasets are available for OMI. Why have those not been used?

It would be interesting to include also an analysis of the reductions of the major coal power plants, similar to the reductions observed for individual cities.

Due to transport of pollution, background values will be largely caused by downstream NO₂ from the cities and power plants. This is not discussed well enough, and influences the land-use dependent analysis.

Detailed comments:

Intro, page 3/4: when referring to “decreases” I would always like to know with respect to what: is it compared to climatology, to concentrations before the lockdown, or in comparison to 2019? I suggest this information is provided in most cases (now it is given for only some references).

I110: "Moreover, as both instruments use similar retrieval schemes, their NO₂ measurements should be comparable with a suitable degree of confidence?" I understand

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that there are also major differences between these retrievals. These could be highlighted more.

I147: The resolution of TROPOMI in 2020 is 3.5 x 5.5 km

I150: TROPOMI has a (2x) better resolution than 10x10 km. Why was this resolution chosen? 5x5 would be more appropriate.

sec 2.1.1, 2.1.2: I miss papers discussing the validation of the OMI and TROPOMI products.

sec 2.1.1, 2.1.2: The authors suggest that the retrieval approaches are comparable. However, there are also major differences. Compared to NASA v3 there are two newer retrieval products available: NASA v4 and the QA4ECV products. Have the authors considered using those?

Sec. 2.1.3: Is there a classification of these stations available (e.g. traffic, urban background etc.)? Please comment. Can stations be compared, and is a correlation with the satellite data meaningful?

I188: "Details of the pollution mapping". Do you mean "population mapping"?

Sec. 3.1. If I look at the satellite data it seems there is some impact of the fires, but the fire signal seems to be rather modest compared to the cities/power plants. For the fire anomalies I expect the differences to be even smaller. Why is it so important to exclude fire-influenced locations?

I222: "we have considered the grids with zero fire anomaly to assess the changes in NO₂ during the lockdown." Plumes from the fires may be transported over long distances, so blocking fire pixels does not mean the fire signals have been removed.

I258: "Background VCD_{trop}". How is it determined if a pixel is "background"?

I265: "biomass burning activities" From figure 1 it is not so clear if there was an increase or decrease in fire activity in the West. Could you comment on this?

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Sec 3.4: "The change observed over different land use" This title is a bit unclear, please reformulate

Sec 3.4.1: Land use changes occur over short distances. In contrast, the transport of (elevated) NO₂ can happen over hundreds of kilometres. Therefore, the emissions from urban areas or coal plants will enhance concentrations over crop/forest grid cells, which blurs the interpretation. Also, differences in spatial resolution between OMI and TROPOMI may have similar effects. As such, there is a bit of a danger to over-interpret the results of e.g. Fig. 4.

Sec. 3.4.2: I would suggest not to repeat numbers from the table inside the text. Better to highlight a few.

I339: "The actual reduction in VCD_{trop} NO₂ is greater for the larger urban area". But what is the reason? Is it related to different reductions in those cities or to differences in the relative contribution of the background not caused by emissions inside the city?

Sec 3.5, fig. 6: Without knowledge of the station classification or location relative to the sources these plots are not so meaningful. I would suggest to remove them, or put them in the supplement. Combining road-side and urban background stations in the plots will strongly reduce correlations.

Fig. 7: Ahmedabad is a clear outlier. Can the difference between surface and satellite observations be understood, for instance with information on the locations of the surface sites?

I 411: "adding a constant". This is not a logical thing to do in case of log-log plots, because the impact will be very non-linear. But the vertical scale in 8c is linear? Please re-write or remove the explanation.

I 415: It is not easy for me to judge from the figure panels if the relation follows a power law. And what does a factor 2.0 for OMI tell me?

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