

## ***Interactive comment on “An assessment of the impact of a nation-wide lockdown on air pollution – a remote sensing perspective over India” by Mahesh Pathakoti et al.***

### **Anonymous Referee #1**

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This paper examines NO<sub>2</sub> and CO concentrations and aerosol optical depth over India as observed by satellite remote sensing instruments before and during COVID-19-related lockdown restrictions in the country. The authors conclude that changes in emissions due to lockdown restrictions are responsible for the observed changes in air quality. While this is an interesting and timely topic of discussion, the analysis performed here does not adequately support this conclusion.

The main factor missing in this analysis is an appropriate accounting for non-emissions-related variability in pollutant concentrations. NO<sub>2</sub> concentrations from the 2020 lockdown period were 18% lower than the 5-year mean. This relatively small difference

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could easily be explained by meteorological effects, especially since the changes are observed over a short period. From the analysis performed here, it is not possible to say that these decreases are driven by emissions alone. No statistical analysis is performed to indicate that the decrease is significantly lower than what could be expected due to interannual variability. In comparing regions and cities, every observed decrease in pollutant concentration is attributed to emissions changes, while pollutant increases are generally ignored in the discussion. There is no discussion on what may cause the regional differences.

Additionally, the CO analysis consists of comparing TROPOMI observations during the lockdown to the climatology as observed by MOPITT without an evaluation of biases between the instruments. Without this information, there can be no confidence that any decrease seen during lockdown is due to emissions changes and not an artifact of instrument biases. I would not recommend publication of this paper in its current form. However, publication could be warranted if a consistent CO product is used before and after lockdown, a stronger statistical case is made for the significance of the observed changes, and a more thorough discussion of drivers of variability (interannual and regional) that includes impacts of meteorology is included.

Specific Comments: Line 52: CO is not generally considered to be a greenhouse gas itself, although it does contribute to the radiative budget by producing ozone and affecting methane and carbon dioxide oxidation.

Line 57-61: This sentence does not make grammatical sense. Please state which instruments are measuring which pollutant more clearly.

Section 2: Resolution information is incorrect or inconsistent. The resolution of OMI level 3 data is 0.25°x0.25°, not 25km. MODIS level 3 AOD is 1°x1° not 100km. TROPOMI CO is listed as 10 km in the table and 7 km on Line 71. The resolutions of the level 2 CO observation pixels are given, but the data must have been gridded somehow – what resolution (spatial, temporal) was the data gridded to? Was it con-

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sistent between MOPITT and TROPOMI? Also, TROPOMI provides high quality NO<sub>2</sub> data with finer spatial resolution than OMI – why was OMI chosen for NO<sub>2</sub>?

Line 80: Some information about the restrictions would be helpful for readers. What was different between phase 1 and phase 2? This is important context in understanding the rest of the results.

Line 88: The authors should take care throughout the paper to distinguish between emissions and concentrations. Emissions were affected by lockdown restrictions, but the data used here represent pollutant concentrations. This mistake is also on Lines 105, 147, 204, 205, 217, 220, 248, 255, 257, and the Table 3 caption.

Figure 1: I find this figure very hard to follow. Could the data processing methodology not be more succinctly described in text?

Figure 2: The text in this figure is very blurry. It is hard to understand what is being shown. The same is true for Figures 4 and 5.

Lines 102: How can a difference be caused by seasonal changes when you are comparing the same time period from two years? More likely this is due to meteorological effects (e.g. winds, cloud cover, etc.).

Line 105: I don't understand what is meant by "detect build up and removal of NO<sub>2</sub> during the lockdown period". Maybe this is because the figure is blurry. Do the authors mean that the concentrations increase and decrease during the period? If that is the case, and there is no corresponding change to the lockdown measures, then that's additional evidence of non-emissions factors playing a role.

Line 114: It's not clear why Spain is being mentioned here.

Section 3.1.1: I don't think the analysis here and in Figure 3 show that the NO<sub>2</sub> changes are "obviously due" to emissions changes. For one, the lockdown started March 22, and 2020 concentrations are lower than 2019 starting March 7, and lower than 2015-2019 for the whole time period. In Figure 3c, 2020 concentrations in Phase-

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2 are higher than in 2017 – essentially, it doesn't look like the concentrations in 2020 in Phase-1 or Phase-2 are outside of what looks like natural interannual variability. A more rigorous statistical approach is needed to demonstrate whether the difference is significant.

Line 137: "there is a decrease in CO levels" – the mean values cited show an increase from 2019 to 2020.

Section 3.2: As mentioned, pre-lockdown CO is from the MOPITT observations and the lockdown values are from TROPOMI. Without evaluating the biases between the instruments, or at least citing research that performs such evaluations, results from this section are not trustworthy. The small changes between 2019 and 2020 could certainly be explained by instrument biases.

Line 139: Assuming that there is no bias between MOPITT and TROPOMI, it's not clear to me why smaller changes in CO compared to NO<sub>2</sub> is due to lifetime. If it's an issue of lifetime, how does that explain that in the north and south regions 2020 is higher pre-lockdown, lower in phase 1, and higher again in phase-2? Figure 4d, which is based on TROPOMI alone, seems to show that CO can change quite a bit from week-to-week.

Line 159: Again, I don't see how differences between the same time period in 2019 and 2020 can be due to seasonal differences.

Line 164: "a significant lowering of aerosols. . . is observed over the Indian region during the lockdown" – what about the large increase in the central and eastern regions during Phase 1? Also, the word "significant" usually implies statistical significance, and such statistics were not performed here.

Line 182-184 "A RoC of 5%...during this period" is unclear to me.

Figure 5: Why are the magnitude and distribution of AOD differences in Phase-1 so different than Phase-2, and so different than pre-lockdown? A description of what restrictions were in place would help here.

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Table 2 is unclear to me. The text says that colors indicate increases and decreases, so then what do the numbers represent? They do not seem to correspond to each other, although maybe I'm just missing something.

Figure 7: The colors are a bit misleading. For one, darker blue usually indicates a large decrease, and here it means a large increase which may confuse readers. Secondly, the color representing no change (i.e. RoC=0) changes slightly in each row.

Section 3.4: This section could use more discussion. There is quite a bit of difference between regions –some have all pollutants decreasing, some have increases in one pollutant and decreases in another. It would be useful to see at least some speculation about what is causing the regional differences.

Line 257: “Thus, exhaustive lock down. . .led to improvement in the air quality and short term climatic effects over India” overstates the results of the paper. The analysis here shows that some pollutants may have seen decreases in concentration but does not at this point demonstrate that these changes were caused by emissions changes during lockdown and not, for example, meteorology. Secondly, the short term climatic effects are largely speculative and not directly supported by any results here.

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