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Atmospheric Chemistry and Physics
Discussions

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 #2

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This manuscript investigates the changes in NO2, CO, and AOD over the Indian subcontinent while the COVID-19 lockdowns. I appreciate the effort that the authors have undertaken in the preparation of this manuscript. Unfortunately, this manuscript is too disjointed in its current form to recommend publication. The manuscript reads almost like a report without diving deeper into explaining why certain patterns are emerging. Further, some statements are misleading or not scientifically correct. Also, many figures are too blurry to be interpreted, so in some sense, this review is incomplete. Even upon revision, this type of analysis is better for a different journal.

Some specific recommendations for improvement are listed below:

Line 30 - Please include a citation for the specific # of cases

Line 33 - Missing "were" between "production suspended"

Line 36 - Capitalize "China" and please list the names of the three cities.

Line 38 - This entire paragraph is very disjointed and has some factual errors. Some sentences such as the first and ninth are superfluous. And there are some scientific inaccuracies as well. For example, CO is not a greenhouse gas (although some amount will be oxidized to CO2 eventually), ambient air quality monitors measure concentrations not emissions, and there are many sources of biogenic or semi-biogenic sources of aerosols, such as dust, sea salt, volcanic sulfates, secondary organic aerosols, and wildfires (e.g., Siberia), which often overwhelm the anthropogenic signal on a global scale.

Line 68 - The resolutions reported here are not quite correct. Please revise.

Line 74 - TROPOMI also measures NO2; it is unclear why that instrument is not used for the NO2 analysis as well.

Figure 2 - Too blurry to fully evaluate.

Line 102 - Modify "seasonal change" to "meteorological variability between years"

Line 103 - Mentioning the NO2 lifetime is here out of place. Please remove sentence.

Line 114 - Unclear why Spain is mentioned here?

Line 119 - RoC does better than a single year-to-year analysis, but does not account for emissions changes due to economic growth/decline, government policies, or years with anomalous weather patterns. The RoC reduces the effects of meteorology, but it brings up other errors, which are not mentioned. Please modify to state that emission changes due to economic or political factors are not included, and that meteorological factors are somewhat but not entirely removed. This is a very critical point, and must be addressed better throughout the manuscript.

Line 128 - Presumably “LD” is lockdown?

Line 135 - The CO analysis is confusing. When is TROPOMI used vs. when is MOPITT used? Is TROPOMI from 2020 compared to an average of MOPITT from 2015-2019? If so, this is not the scientifically correct manner to conduct this analysis. The two instruments have different spatial resolutions, are $\sim$20 years different in age, have different overpass times (morning vs. afternoon) and have different algorithms. This section needs to be re-thought. I would recommend using TROPOMI from 2020 and comparing to an average of TROPOMI CO from the prior two yers.

Figure 4 - Too blurry to fully evaluate.

Line 154 - It is confusing as to which AOD products are used here. Terra MODIS is 10:30 local time and Aqua MODIS at 13:30, so it’s confusing as to how/why Terra/Aqua is used for one analysis and Aqua only for another analysis. Please double check how the AOD products are being utilized here, and re-calculate as necessary, In particular it’s best-practice to not use Giovanni for a scientific paper, and instead download daily L3 files from NASA Earthdata (as has been done for the other products).

Line 156 - Should be Figure 5 not Figure 4

Line 166 - It appears there’s an increase in AOD between years between many regions? Am I interpreting Figure 5 correctly? If so, this is glossed over, and should be discussed more in-depth. In general, a much more lengthy discussion is needed here.

Figure 5 - Too blurry to fully evaluate.

Line 181 - Please see comment regarding RoC in response to Line 119. The same comment applies here.

Line 187 - Discussion of the aerosol indirect effect on precipitation is a bit tangential to the theme of the manuscript. I think it would be more appropriate to discuss that higher precipitation can lead to lower aerosol loading irrespective of emissions changes.

Table 2 - I appreciate this table, but it needs to be clarified. There should only be two significant figures when reporting the percentages. Also, it is unclear what the numbers in the brackets are referring to.

Figure 7 - This figure is confusing. It appears that NO2, CO, and AOD are all down before the lockdown period and then are also down after the lockdown period, but there appears to be no effort to account for the pre-lockdown drops in the post-lockdown drops. Therefore any conclusions based on the lockdown periods are misleading.

Figure 8 - This figure is hard to interpret. In particular, it is nearly impossible to line up the cities across the x-axis (time). This type of data is better for a supplemental table. I suggest removal in its current form.

Lastly, there is no discussion of uncertainty throughout the manuscript. This is particularly relevant with respect to quantifying the role of meteorology of the changing trace gas amounts, and in quantifying the role of economic/political impacts on emissions (when comparing 2020 to a 2015-2019 average). Also instrument error for all satellite instruments should be discussed.

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