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Review of acp-2021-534

Anonymous Referee #2

Referee comment on "Air quality impacts of COVID-19 lockdown measures detected from space using high spatial resolution observations of multiple trace gases from Sentinel-5P/TROPOMI" by Pieternel F. Levelt et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-534-RC2, 2021

In this study, results from more than 2 years of TROPOMI measurements of several tropospheric trace gases are reported and evaluated for possible impacts of the measures taken to reduce the spread of the Coronavirus in spring 2020. The approach taken is to compare data from 2020 to those from 2019 and, in the case of HCHO and CHOCHO, also to an OMI based climatology. In addition to the well-known reductions in NO2, decreases of different magnitudes are also found for SO2, HCHO, CHOCHO and CO over China and India.

The strong point of this manuscript is the combination of data on five trace gases, all measured from the same platform which tell a consistent story. These new data and comparisons are interesting and very relevant, are within the scope of ACP and should be published.

The discussion of the reductions in NO2 columns on the other hand is neither new nor particularly interesting and I strongly suggest removing section 3, which displays data, which has already been shown in very similar ways in various previous publications. I simply do not see the benefit of repeating them here. Several published studies have gone beyond a simple comparison of data from 2020 and 2019 and have attempted corrections for meteorology, long-term trends and sampling, all of which are only discussed qualitatively here.

General points

In the abstract and introduction, it is stated that this manuscript aims to "provide guidance on how to best interpret TROPOMI trace gas retrievals and to highlight how TROPOMI trace gas data can be used to understand event-based impacts on air quality

from regional to city-scales around the globe". I do not think that the manuscript is doing this, and am not convinced that an ACP paper would be the right place to provide such guidance. I would suggest removing these statements throughout the paper and focusing on the science question.

In the manuscript, the term "concentration" is often used where columns are meant. I think that these two quantities are not the same and would suggest that the authors search the manuscript for the term and replace it wherever they discuss columns. Also, the tropospheric columns retrieved from TROPOMI are not "averaged concentrations", nor "column concentrations", nor "column averaged amounts" but integrated concentrations. Please correct.

The strength of this paper is the combination of results from several trace gases in a consistent way. I, therefore, would suggest that also the units used for the different columns are consistent between molecules and between different figures and tables. I do not see any advantage in using different units for NO2 than for HCHO, CHOCHO and CO other than that these are the units provided in the operational product. Please make consistent.

In some places, the article reads like a TROPOMI advertisement. I do not think this is necessary – the great TROPOMI data and figures presented speak for themselves and I do not see the need to highlight the "societal relevance of the TROPOMI mission" in a scientific paper.

Specific points

Line 98: "absorption regions for clouds" – this sounds a bit odd. I assume that the absorption bands of O2 are referred to which are used for cloud products.

Line 112 – 120: I would suggest removing this paragraph

Table 1: CHOCHO: Not sure, why Lerot 2010 is cited here and why the comment on precursors made for HCHO is not repeated for CHOCHO

Line 203 – 204: Sentence appears to be incomplete

Line 338: operations => operational

Line 532: sentence appears to be incomplete.

Figure 5: I would suggest adding difference plots as a third line.

Figure 6: Something is not right with the glyoxal columns – they extend into the future!

Figure 6 and elsewhere: The region over which the data is averaged is called Northern China. However, this region is not particularly far to the North of China but rather in the central East of the country.

Line 617: I am not convinced by the explanation given for the delay in reductions in VOC and SO2 compared to NO2. To my knowledge, the difference in lifetimes is of the order of hours and secondary HCHO formation is a matter of hours or maybe days but not weeks.

Figure 7: I would suggest adding difference plots as a third line.

Line 794: Sentence appears to be incomplete

Figure C2: While I am convinced that the COBRA SO2 product is better than the DOAS SO2-product, this figure does not prove that. The figure mainly shows the difference in absolute values of the two products.

Figure D2: I did not know what an "ombrothermic diagram" is before checking so maybe other readers would also benefit from an explanation here. I also think that the caption is not correct as the two columns are for different regions and not for temperature and precipitation as stated.