

RC2: ['Review of acp-2021-534'](#), Anonymous Referee #2, 20 Aug 2021 [reply](#)

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.

General points

- 1) 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.

Response: We agree that Section 3 can be shortened with most of the discussion removed or moved to the Appendix, however we believe it should not be completely removed for several reasons. We want to give the reader context for the regions described in Sections 4 & 5. Secondly section 3, illustrates the consistent view TROPOMI offers from the global scale down to city-level (to our knowledge the multi-city panel figure 2 is a unique figure and of value to the reader). Lastly, section 3 also highlights our methodology: that is, first we analyzed the observed spatial and temporal patterns in TROPOMI NO₂ data which further lead to the identification of regions of interest for the other four trace gases we present. Instead of discussion style, this shortened context-setting section will serve as a compact review of work that has been already published.

- 2) 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.

Response: While the text stating the aims of our paper can be re-evaluated and modified as needed, we disagree that these statements should be removed completely. User guidance is critical for an ever-widening community of data users, especially for under-represented regions and user types. Beyond experienced

scientific users information needs to be made available so that (TROPOMI) data are properly utilized, analyzed, and interpreted. We maintain that proper user guidance is essential to ensure scientific integrity in an age of open data.

- 3) 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.

Response: Appearances of term ‘concentration’ in general discussion portions of the text will not be modified. We have, however, checked for all appearance of the term ‘concentration(s)’ when in direct reference to TROPOMI column measurements and for clarity these appearances as listed below will be replaced with the term ‘column amount’ where appropriate.

- Line 18-19: We report clear COVID-19-related decreases in NO₂ concentrations on all continents.”
- Line 59, 64: Indirectly referring to TROPOMI observed concentrations
- Lines 99-100: “Using the spectral radiance measurements from TROPOMI, atmospheric concentrations of different gases are retrieved as well as cloud and aerosol properties.”
- Line 201-202: “First we compare the concentrations in 2020 with those from the same period...”
- Line 324-325: “For this paper, we correct the HCHO concentrations for this meteorological impact prior to using the data in the analyses.”
- Line 378-379: “ A strong reduction in the NO₂ tropospheric concentration of about 40% is observed over Santiago during this period”
- Line 474: “Figure 3 and Figure 4 illustrate the tropospheric concentration of NO₂ over Europe,...”
- Line 541-542: “This is clearly illustrated in the upper panel of Figure 5 showing CHOCHO concentrations,”
- Line 568: Caption Figure 6, (correct to ‘median tropospheric column amount)
- Line 616-617: “The small delay between the initial decrease in NO₂ concentration and the observed decreases in the other trace gas signals”
- Line 631-632: “NO₂ and SO₂ the concentrations are clearly lower across the country in 2020 as compared to 2019. Although less prominent, concentrations of CO, HCHO, and CHOCHO appear to be lower in April 2020”
- Line 638: Caption Figure 7
- Line 647, 649: References to Fig 7 and Fig 8a with “NO₂ column concentrations”
- Line 665-666: Caption Figure 8
- Line 722-723: “HCHO column concentrations”
- Line 766-769: TROPOMI observed “NO₂ concentrations”
- Line 792: “CHOCHO concentrations”
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We also propose to modify two sentences (lines 189-192) as follows, “TROPOMI observes atmospheric concentrations of trace gases integrated over a vertical column, which is not the same as a direct measurement of the (near-surface) emission. The amount of a given trace gas integrated over a vertical column at a certain location depends not only on emission and deposition, but also on atmospheric transport and (photo)chemical reactions.”

- 4) 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.

Response: The units in the revised manuscript for all figures and when mentioned in the text will be in the TROPOMI standard base unit of mol/m², making note that for figure and color bar label readability both millimol/m² and micromol/m² are sometimes used. Additional, supporting information and explanation of the TROPOMI data file units can also be provided in Appendix A.

- 5) 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.

Response: We have evaluated some of the ‘claim’ statements and will modify several in the revised manuscript. We, however tend to disagree on the point of societal relevance as TROPOMI data is increasingly utilized to address a variety of societally relevant issues.

Specific points

- 1) 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.
Response: will be reworded for clarity in revised manuscript, Suggestion: spectral bands selected to measure the absorption by a large number of trace atmospheric constituents as well by clouds and aerosols.
- 2) Line 112 – 120: I would suggest removing this paragraph
Response: These sentences were added to briefly illustrate and explain why we go beyond qualitative comparison. We do agree to streamline text as needed using comparable language throughout for specific mentions of methodologies employed (per species) to make our comparisons more quantitative.
- 3) 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
Response: will be updated in revised manuscript
- 4) Line 203 – 204: Sentence appears to be incomplete
Response: will be reworded in revised manuscript
- 5) Line 338: operations => operational
Response: will be corrected in revised manuscript
- 6) Line 532: sentence appears to be incomplete.

Response: Yes, can be reworded for revised manuscript; Suggestion: City-scale impacts of lockdown on NO₂ tropospheric column amounts for Wuhan and Beijing *are presented_* in Sect. 3.

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

Response: Given the different lifetimes of species, meteorological differences, among other reasons described in Section 2, a simple difference plot will include many more features of trace gas variability than just the reduction of emissions due to COVID-19 lockdown measures, and is likely to be more confusing to the reader and may add little value.

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

Response: This will be corrected, and as described in our response to Reviewer #1, given the data availability, all time series will be extended through Nov 2020.

9) 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.

Response: this will be clarified in the revised manuscript

10) 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.

Response: this will be clarified in the revised manuscript, however recent literature (for which we will add an explicit citation) supports the stated lifetime.

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

Response: See response regarding same comment for item 7) regarding Figure 5.

12) Line 794: Sentence appears to be incomplete

Response: will be updated in revised manuscript. Suggestion, “For HCHO, after correcting for the effect of seasonal and temperature variations, we observe a coincident 40% *reduction_*.”

13) 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.

Response: will be updated as needed for clarity

14) 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.

Response: will be updated as needed for clarity