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Reply on RC1

Pieternel F. Levelt et al.

Author 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-AC1, 2021

Summary RC1: 'Comment on acp-2021-534', Anonymous Referee #1, 08 Aug 2021

This paper presents the observed changes in the atmospheric column amounts of five trace gases (NO2, SO2, CO, HCHO and CHOCHO) detected by TROPOMI to investigate the reductions of anthropogenic emissions due to COVID-19 lockdown measures in 2020. It aims to provide guidance to data users on how to best interpret and analyze TROPOMI trace gas data not only for lockdown-driven emission changes but also for other event-driven changes. I would suggest small revisions before the publication

Response

We would like to thank the reviewer for the useful comments and suggestions. Based on our responses (see below) we believe that this input further improves the clarity of our paper.

Response to general comments:

- 1. The authors used different quality assurance values (qa_value) for different species. Are the definition of qa_value consistent among species? If not, a small summary is appreciated. Otherwise, it is very confusing for readers.

Response: As a general guideline, application of a qa_value greater than 0.5 ensures a basic level of quality assurance for all operational TROPOMI data products. However, some data products have provided additional qa_value thresholds relevant for certain scenarios as described in the respective TROPOMI Product Readme File (PRF) documents and as listed in Appendix A. For this paper, only NO2 differs, with the application of qa_value of greater than 0.75, and this is described in the second to last paragraph in Section 2.2 (lines 234-238). A qa_value of greater than 0.5 was applied the other operational TROPOMI data products used in this paper (CO and HCHO) as described in Sections 2.4 and 2.5, respectively.

To aid the reader we will update the text and listing in Appendix A to make it clear which qa_value threshold was applied to each of the operational TROPOMI data products. We will also make it clear in the text and Table 1 which data products are operational and which are prototypes.

- 2. The authors used different approaches to consider the contributions from natural sources and meteorology for different species. I would appreciate a table or graph to summary the approaches.

Response: We think that a table would be insufficient to clarify how the seasonal, meteorological, temperature-driven, and other effects have been accounted for in the analysis of the data products presented in this paper. These methods are currently described per species in Sections 2.2 through 2.6. We will however ensure in the revised manuscript that it is clear to the reader how the treatments differ per species as described in Section 2.

Response to specific comments:

- 1. Table 1. Are power plants primary sources of CO? Please confirm.

Response: To answer this question we first clarify what is meant by the word 'primary'. In the sense of primary or foremost, carbon monoxide (CO) emitted from power plants is not the largest source of CO. Based on the EDGAR v5.0 database which gives 2015 emissions per sector on a global basis, CO from the Power industry contributes just over 1%. The largest CO sources are transportation, residential cooking and heating, other industrial combustion (ex. Iron and steel production industry), and agricultural burning (which is incorporated in the broader label of biomass burning used in our paper).

In the sense of direct/primary emission, power generation is a primary source of CO as it is emitted directly from power plants.

As a result of this question, we will modify the table header label 'Primary emission sources' and update the order of appearance of the sources so that it is more apparent to the reader which sources are the largest. It should be noted, however, that based on analysis from the previous EDGAR emissions database (2010) the relative importance of largest sources of CO can vary per region and/or country (Janssens-Maenhout et al., 2015).

- 2. Line 187. "In future studies, the averaging kernels could be used for inversion modelling of emissions thus eliminating this error completely." It is not clear to me how the error can be eliminated completely. Please clarify.

Response: To better explain how the profile shape related error is eliminated, the sentence cited above is extended as follows,

"In future studies, the averaging kernels could be used for inversion modelling of emissions. As explained in Eskes and Boersma 2003, relative comparisons between the observations and the model used in the inverse modelling system, and therefore the resulting emissions, no longer depend on the retrieval a-priori profile shape when the kernel is applied to the model."

- 3. Line 204. Please clarify what are changes driven by mechanisms.

Response: This sentence is updated for improved clarity as follows,

"First we compare the concentrations in 2020 with those from the same period from earlier years and then carry out additional analysis to compare the lockdown-driven variability with seasonal and meteorological variability."

- 4. Line 230. Please add reference for the magnitude of 20-60%.

Response: We will add a reference to the NO2 ATBD in the revised manuscript to support this statement (van Geffen et al., 2021).

- 5. Line 265. Please add reference for the error.

Response: The reference Theys et al., 2021 will be added to this sentence in the revised manuscript.

- 6. Line 327. What is the term of novel applied to? The algorithm?

Response: the term 'novel' is applied to the temperature correction for HCHO.

We propose to simplify the sentence to: "this temperature correction is performed for each region and on the OMI and TROPOMI time series separately".

- 7. Line 338. Operational products instead of operations products.

Response: This sentence will be corrected in the revised manuscript.

- 8. Line 346. Please define "box-air mass factors" before use.

Response: To address this point we propose to modify the text and references as follows,

"Air mass factors are calculated following the formulation of Palmer et al. (2001), which combines altitude-dependent air mass factors (or Box-AMFs) with a priori glyoxal concentration profiles. The Box-AMFs represent the instrumental sensitivity to changes in concentration at any altitude and are precomputed using the radiative transfer model VLIDORT v2.7 (Spurr and Christi, 2019), while the a priori profiles are provided by the MAGRITTE chemistry-transport model (Müller et al., 2018, 2019)."

- 9. Figure 6 & 9. The data for June-Dec, 2020 is already available. Is there any specific reason to exclude them from the figures?

Response: At the time of manuscript preparation, the cutoff date was in part limited by the availability of the ERA5 surface temperature data needed for HCHO temperature correction. Now that this is no longer the case, the time series presented in these plots will be extended through end-November 2020. A data processing update carried out in early December 2020 affects the tropospheric column amounts and causes a discontinuity in the NO2 time series. This discontinuity will only be resolved once the operational reprocessed dataset is available (planned for the first half of 2022). As such, December 2020 will not be included.

- 10. Line 746. Is there any other evidences/reports from literatures to support the explanation for the enhanced CO?

Response: We have examined this issue and can, based on references, state generally that fires play a role in the observed, enhanced CO. However to more fully answer the question of why CO in 2020 is higher than 2019 is only possible by employing chemical transport model calculations and is beyond the scope of this work.

We propose to update the revised manuscript text and references accordingly. The sentences in lines 745-746 will be changed as follows

Original text: "Figure 7 shows that the CO amounts in southern India are higher in 2020

compared to 2019. The reason could be the accumulation of CO originating from elsewhere prior to the 746 lockdown period."

Proposed update: "Figure 7 shows that the CO amounts in southern India are higher in 2020 as compared to 2019. The enhanced CO values in 2019 and 2020 are detected above regions (e.g. Madhya Pradesh, Odisha, and Chhattisgarh) where seasonal forest fires commonly occur in April/May (Chandra and Kumar Bhardwaj, 2015, Srikanta et al. 2020). Thus, the enhancement of CO for the different years depends not only on the fire activity but also on how the meteorological situation prevents or permit the accumulation of CO in the atmosphere. To more fully address the reasons why CO is higher in 2020 than 2019, future studies could carry out calculations using a chemical transport model."

- 11. Figure D2. The font size is too small to read.

Response: This font size will be increased to improve readability in the revised manuscript.