Reply to Reviews of "Peculiar COVID-19 effects in the Greater Tokyo Area revealed by spatiotemporal variabilities of tropospheric gases and light-absorbing aerosols" by Damiani et al.

We included some additional information to the manuscript as requested by the Reviewer, but there are no relevant changes. Please note that below the Reviewer refers to line numbers of the revised tracked changes manuscript

We thank the Reviewer for reading our paper and providing constructive comments. We have incorporated the reviewer suggestions into the revised manuscript and provided point-by-point responses to each comment below.
In the following, we first report the referee comments (in black), then we provide our responses (in red).

The authors have addressed most of my comments and the manuscript is greatly improved. I believe the manuscript would benefit from further edits, and so I recommend this paper for publication after minor revisions.
R -> We thank the Reviewer for reviewing our manuscript once again. Below we addressed the additional comments.

Abstract – Total column is typically used to denote the total atmospheric column and includes the stratospheric component of species. I understand that the authors want to differentiate the total tropospheric column from the partial column and so need to carefully use these terms. One suggestion is to have all instances of 'total column' as 'total tropospheric column'. In the abstract and in the manuscript, the statement can be 'total and partial tropospheric column'.
R -> We agree. When possible, we now included the term "tropospheric".

L65 – Now that the authors mention about 'tropospheric ozone', it would be good to point out early in the manuscript why they do not use these in their analysis.
R -> We agree. Following your advice, we now stated early (in Section 2.1.3) that "Since OMI O3 only has some low sensitivity to the boundary layer while TROPOMI O3 is currently limited to tropical latitudes, we did not use satellite-based O3 datasets in this study."

L87-88 Should be 'Increases in surface O3'.
R -> OK! We changed the sentence as suggested.

L215-225 Would it be possible to comment on the minor differences between the NO2 product versions? Are the differences between the HCHO product versions also minor?
R -> The minor differences between TROPOMI NO2 versions 1.2.x. and 1.3.x affected only a small fraction of the observations. They were caused by improvements in the FRESCO-S algorithm devoted to retrieving cloud information. Indeed, since version 1.3.x., to avoid non-physical cloud fraction and pressure values, when the top of atmosphere reflectance is lower than expected, the surface albedo is reduced to match the top-of-atmosphere reflectance (Van Geffen et al., 2021). A further change in FRESCO is the treatment of very high cloud fractions (Van Geffen et al., 2021). Nevertheless, we excluded observations retrieved under these conditions. On the other hand, version 2.1.3 of the TROPOMI HCHO includes various improvements compared to the previous version 1.1.x., such as a new surface albedo retrieval algorithm, the adoption of new OCRA cloud-free maps, and the correction of some QF values over snow/ice regions (the latter did not affect the investigated area). More information can be found in the product read-me file and recent validation activities (De Smedt et al., 2021). We included this additional information in Section 2.1.2.

L235-240 Is it possible to calculate how much data you may lose and what would be the impact on the mean NO2 columns if you keep the same cloud threshold for both OMI and TROPOMI? I understand it maybe beyond the scope of the present study and so the authors can also reference to existing literature where the impact may have been assessed.
R -> Figure R1 shows the reduction in the number of OMI observations resulting from various cloud fraction (CF) thresholds (black) based on the long records of OMI NO2 data (2005-2020) recorded over Chiba station (e.g., for CF = 1, we used all data). Here, for sake of completeness, we also included the very uncertain observations recorded under high CF (i.e., CF > 0.5, shaded area). OMI observations reduce roughly linearly within the 0.3-0.9 CF range. Then, larger reductions occur for CF < 0.3. Taking a threshold of 0.3 (as in this study, dashed line) removes about 50% of the OMI observations while keeping the same threshold as that used for TROPOMI (i.e., 0.2) further reduces them by about 10%. Moreover, the overall tendency of the corresponding changes in the OMI NO2 column with the CF thresholds (red) shows that the NO2 column remains roughly constant for CF < 0.4. More information on the variation of OMI NO2 as a result of modifying screening criteria can be found in Compernolle et al. (2020). We now included some additional information in Section 2.1.3.

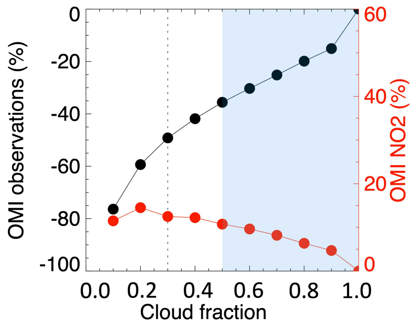

Fig. R1 - OMI NO2 data in 2005-2020 recorded over Chiba station showing changes in the number of OMI observations (black) and NO2 amount (red) as a function of various cloud fraction thresholds. Dashed line: OMI cloud fraction threshold used in this study (CF = 0.3).

Also, a brief comment on why OMI HCHO was not used in this work?
R -> In this study, we used OMI NO2 to evaluate the average weekly changes and, secondary, to confirm decreasing trends. Since biogenic emissions dominate HCHO, evident weekly cycles are not expected in OMI HCHO. Moreover, a recent study (De Smedt et al., 2021) has already shown the comparison between our full-time series of MAX-DOAS HCHO and OMI HCHO. So, we did not use OMI HCHO.

L580-585 This section can be strengthened. Is there a seasonality in the HCHO/NO2 weekly cycle? And a brief discussion is warranted if the observed differences are significant.
R -> Overall, due to the seasonal variations in NO2 and HCHO concentrations, the HCHO/NO2 ratio also shows significant seasonality, with a large ratio in summer compared to the other seasons (Irie et al., 2021). Nevertheless, the frequent cloudy conditions in the late-sprint to summer period and the limited temporal extension of the dataset prevent to evaluate seasonal differences in its weekly cycle. We included some additional information in Section 3.3.1.

L667-670 Why not perform a quick analysis to compare the NO2 from TROPOMI to that from GOME2 and strengthen this implicit assumption?
R -> Figure S5 (in the supplement), based on MAX-DOAS observations, already showed that changes observed around the satellite overpass (right column) were roughly representative of the daily changes (left column) at our location. As stated in the discussion, further evidence based on satellite observations would require geostationary observations. Comparing TROPOMI observations (with an early afternoon overpass) and GOME2 observations (with a mid-morning overpass) would hardly provide additional insights as the analysis, to be carried out over a region with a heterogeneous NO2 distribution, would be based on two different instruments characterized by very different spatial resolution (i.e., GOME 2: 40 km x 80 km vs. TROPOMI: 3.5 km x 5.6 km) and with a limited temporal resolution. We now explicitly mentioned Fig. S5 when mentioning this assumption.

Figure 1 Would it be better to also compute these changes for normalised mobility? We may be seeing a lower change in mobility for Japan because of a higher baseline, I presume.
R -> Please note that Google mobility data are already normalized to the baseline (Jan 3- Feb 6, 2020), and the baseline data is not provided. For the sake of clarity, we prefer keeping the original Figure 1.

New references

Compernolle, S. et al.: Validation of Aura-OMI QA4ECV NO2 climate data records with ground-based DOAS networks: the role of measurement and comparison uncertainties, Atmos. Chem. Phys., 20, 8017–8045, https://doi.org/10.5194/acp-20-801 7-2020, 2020.