

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2022-110

Anonymous Referee #1

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Referee comment on "Peculiar COVID-19 effects in the Greater Tokyo Area revealed by spatiotemporal variabilities of tropospheric gases and light-absorbing aerosols" by Alessandro Damiani et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-110-RC2>, 2022

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The authors focus on the spatiotemporal variability of gases and light-absorbing aerosols in the Greater Tokyo Area during the COVID19 lockdown and the resulting changes in mobility. In general, I find this manuscript to be of interest for publication and appropriate for Atmospheric Chemistry and Physics. The manuscript is well written and would benefit from some additional details in the methods and discussion sections. Consequently, I can only recommend this paper for publication after major revisions.

### Major comments

The content of the manuscript needs to be backed by more references. I have mentioned some instances under minor comments.

The authors also need to provide an overview of other studies which have investigated the impact of COVID19 lockdown. The authors should also compare their findings with existing literature (Cooper et al., 2022, Miyazaki et al., 2021).

Line 128 There may have been a version change in TROPOMI products in this time period. If yes, the authors should briefly mention this and discuss how did they go about it. Also, what do the authors mean by 'interpolation' here? Do they mean 'oversampling'? How have the uncertainties been considered? Are the TROPOMI columns shown in Figure 3 and referred to in line 235 error-weighted averages?

Line 240-245 I would recommend the authors to also perform a sensitivity check if they considered median and error-weighted mean (if not already) of the TROPOMI HCHO columns and see if the interpretation of the results changes.

Can the authors use existing literature to comment on the relative contribution of biogenic and anthropogenic sources to HCHO and interpret the current findings of no apparent changes in TROPOMI HCHO?

Figure 5 It is difficult to read and interpret sub-figure (a) because of the image resolution. Also, the figure caption needs to describe that the NO<sub>2</sub> mean is shown in grey. Can the authors consider an alternative way to present the data in this figure? For example, have the data as a table (supplementary material) and only show the top 10 or 20 cities in a figure (main text). The contours in sub-figure (c) are also complex to interpret as the color legend is only for population density. Please describe in the caption how the contours should be interpreted.

Minor comments

Lines 24-34 The introductory paragraph lacks references. More references for health effects of NO<sub>2</sub> such as Achakulwisut et al. (2019), and for NO<sub>2</sub> trends using satellite data such as Vohra et al. (2022).

Lines 35-40 Have the emissions of ozone precursors significantly decreased worldwide? I do not suspect the same in Asia and Africa. The authors should mention whether this refers to any city or a larger region and if both VOCs and NO<sub>x</sub> emissions are decreasing? It would be a good idea for authors to add details of the studies cited.

Line 91 Reference for HCHO as a proxy for VOCs.

Line 112 Which year are the MAX-DOAS measurements from? Refer to line 185.

Line 113 Any reference to support this?

Line 133 Any reference to support this? Why is the cloud fraction criteria different from OMI? Is it possible to assess the impact on the results if this cloud fraction threshold were to be changed to 0.3?

Line 177 OMI overpass time earlier is 13:40 LT and here it is 1:30pm. The authors should

use consistent values and formats.

Line 212-216 Confusing. Please rephrase.

Line 240 Is there an increasing trend in CH<sub>4</sub> which could be playing a role here?

Line 249-250 There needs to be some discussion around what the value of this ratio is. What is the transition regime you have been considering given this varies with both space and time (Duncan et al., 2010)? Add references too.

Line 275 Were the CAMS measurements for 2020 not available at the time of analysis? If they are available now, they should be included in the study.

Line 289 Earlier in line 145, the OMI data record is mentioned as 2005-2019.

Line 290-291 How many cities have been removed because of Friday being a rest day? The authors should list them either in the main text or in the supplementary for completeness. Also, if a lot of cities have been removed, the authors can consider the weekdays to be from Monday to Thursday and compare with Sunday for the weekend effect.

Line 305 What are the “two contour lines”?

Line 412 How does this look compared to findings from Miyazaki et al. (2021)?

Figure 3 The color scale for the last column should be reversed. Warm colors should indicate positive values and cool colors negative.

Figure 4 Caption text “Results are shown as percentage changes with respect to the 2013–2019 average (left and central panels) and 2019 (right panel).” is confusing. Please rephrase.

Figure 6 OMI total columns are referred to in the figure but tropospheric columns in the caption. Please correct as needed.

For data products (OMI/TROPOMI/MAX-DOAS, etc), please include URL stating when they were last accessed or point to references if data not publicly available, so that potential users can use these.

## References

Achakulwisut et al., doi: 10.1016/S2542-5196(19)30046-4, 2019.

Cooper et al., doi:10.1038/s41586-021-04229-0, 2022.

Duncan et al., doi: 10.1016/j.atmosenv.2010.03.010, 2010.

Miyazaki et al., doi: 10.1126/sciadv.abf7460, 2021.

Vohra et al., doi:10.1126/sciadv.abm4435, 2022.