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# Reviewer Comments on acp-2021-479

Anonymous Referee #1

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Referee comment on "The effects of the COVID-19 lockdowns on the composition of the troposphere as seen by In-service Aircraft for a Global Observing System (IAGOS) at Frankfurt" by Hannah Clark et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-479-RC1, 2021

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Review of acp-2021-479 "The Effects of the COVID-19 Lockdowns on the Composition of the Troposphere as Seen by IAGOS"

Hannah Clark, Yasmine Bennouna, Maria Tsivlidou, Pawel Wolff, Bastien Sauvage, Brice Barret, Eric Le Flochmoën, Romain Blot, Damien Boulanger, Jean-Marc Cousin, Philippe Nédélec, Andreas Petzold, and Valérie Thouret

**Summary and General Comments:**

The authors report IAGOS measurements from Frankfurt airport of ozone (data since 1994) and CO (data since 2001) during the MAM 2020 COVID lockdowns. In addition, IASI-SOFRID CO satellite data, and ECMWF (boundary layer heights, FLEXPART trajectories) are used in supporting analyses. In general, the authors show increases in March and May 2020 surface layer ozone, little change in March and May 2020 free-tropospheric ozone, and decreases in MAM surface layer CO.

This analysis is a worthwhile and appreciated effort to quantify the effects of COVID emissions changes on the trace gases ozone and CO – there are few published studies that use in-situ profile measurements during the spring 2020 period that aim to accomplish this.

My main concern with this manuscript is that it is difficult to tell how robust, particularly for the surface layer ozone increases, the 2020 anomalies truly are. There are drastically different periods used to calculate the 2020 anomalies for ozone (1994-2019) and CO (2016-2019). The reasoning behind using the 4-year baseline for CO is the decreasing

trend since measurements began in 2001. This makes sense. However, there is also a clear increasing trend in surface layer ozone since 1994 (Figure 3b). One assumes that this is at least partly the result of decreasing titration of ozone by NO from long-term NOx emissions reductions. If using a 2016-2019 baseline period to calculate surface layer ozone anomalies, the March 2020 positive anomalies may disappear entirely, and the May anomalies will likely be reduced substantially. 1994-2002 appear to have a strong influence on the 2020 positive ozone anomalies. The results are also shown for months with approximately half of the typical number of profiles. There are no statistics presented on confidence intervals/p-values to confirm the significance of the results and whether they fall outside of expected recent interannual variability.

The results presented here underscore the difficulty of quantifying COVID-related air pollution changes from a single location. The vast majority of published studies of surface and satellite data use dozens to hundreds of locations to bolster their results. It is why Steinbrecht et al. (2020) required dozens of ozonesonde stations to show a convincing decrease in NH free-tropospheric ozone from sonde measurements.

Minor Comment: Were NOx measurements also available on these flights (Berkes et al., 2018; https://amt.copernicus.org/articles/11/3737/2018/amt-11-3737-2018.pdf)? Even if only recent years are available, you could calculate Ox = ozone + NO2. If Ox is about the same in 2020 as past years, and NO2 or NOx is lower, that would further support the argument of reduced NO titration leading to increased ozone in 2020. At the very least, the profile data should be combined with nearby surface NOx data to confirm the NO titration argument, rather than leave it to speculation.

To summarize, I suggest the following analyses in addition to other topics raised in the line-by-line comments:

- Re-assess the ozone results with the same baseline period as CO of 2016-2019.
- Produce a more robust statistical analysis indicating the significance of observed ozone and CO anomalies. This is important because only one location is being analyzed, and there is a lot of noise, interannual variability, and underlying long-term ozone trends in the data.
- Attempt to incorporate nearby surface NOx/NO2 measurements to confirm the reduction in NO titration of surface layer ozone (or IAGOS NOx if available)
- Integrate boundary layer CO to account for changes in boundary layer height that potentially reduce the surface layer CO mixing ratios in 2020.

**Recommendation:**

This paper could be considered for publication in AMT if the authors present more compelling evidence that the IAGOS ozone and CO data collected in MAM 2020 were directly influenced by COVID-related emissions changes, and not simply a result of

interannual variability, long-term trends in ozone (surface layer increases) and CO (decreases), and meteorological factors (e.g. boundary layer heights). I recommend Major Revisions that include an assessment of the statistical significance of the results.

**Specific/Technical and Line-by-Line Comments:**

Line 26: Cite Liu, F. et al. (2020) paper for China TROPOMI NO2 decreases: https://advances.sciencemag.org/content/6/28/eabc2992

Line 27: Cite Duncan et al. (2016) paper, which describes the relationship between economic downturn and NOx emissions/OMI NO2 satellite measurements: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JD024121

Line 26 and/or Line 197: Cite Goldberg et al. (2020) paper, which controls for meteorological variability when examining COVID-related TROPOMI NO2 decreases, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL089269

Line 79: Change "balloon and sonde measurements" to "balloon-borne ozonesonde measurements"

Line 110: Stylistic comment, suggest to remove "life"

Line 120: Small typographical error "1° horizontal"

Line 125: Please define GFAS acronym

Line 127: "anthropogenic *sources*"

Line 144: How many profiles in total are averaged into the MAM 1994-2019 ozone climatology? Similarly, if there were no profiles in April 2020, how many of the 84 profiles were from March and May 2020? It might be more proper to indicate March/May rather than MAM in the text and figures. (Also see General Comment about the chosen baseline period for ozone).

Line 181: change reservoir to emissions

Line 189: "in the amount of NO as evidenced by the TROPOMI satellite measurements of NO2"

Line 198: I don't understand what is meant by "but that the photochemical effects from NOx were dominant." Is this just referring to reduced titration of ozone from NO? Please clarify.

Figures 4 and 5: Is it correct that there are 7 nighttime profiles and 13 daytime profiles in May 2020? How robust is the result of a 41% increase in nighttime surface layer ozone from 7 profiles?

Line 232: change "seen by balloons and sondes" to "observed by ozonesondes"

Line 235-236: change "balloon and sonde" to "ozonesonde"

Line 236: Suggest to add: "For example, there was no notable decrease in free-tropospheric ozone in the sparsely-sampled Southern Hemisphere."

Line 245: Change "inflexion" to "inflection"

Figure 8: (Similar to comment for Line 144) Please indicate how many CO profiles are available for MAM 2016-2019

Line 265: Change "biased low" to "anomalously low"

Line 270: Now I see that there are CO profiles for April 2020, so it would be helpful to indicate if the ozone instrument was inoperable in April 2020 (or whatever the cause is)

Paragraph near Line 280: Is all of this discussion necessary? Isn't the boundary layer height simply calculated at the grid point closest to the Frankfurt airport, where all of the surface layer data are collected? The ECMWF output is a fairly coarse 1° resolution, so I would assume this is the case. Please correct me if I am wrong.

Line 284: Why not be consistent in definitions of day and night as for ozone (10:00-18:59 UTC and 00:00-09:00/19:00-23:59 UTC)?

Lines 289-290: Given the relatively long lifetime of CO, a simple check on whether decreased CO concentrations near the surface are a result of dilution in a deeper boundary layer would be to integrate the boundary layer CO content. This will simplify discussion and may lead to more convincing results.

Line 302: Why are you also excluding fire sources of CO? What does that have to do with lockdown decreases in emissions?

Line 304: Please indicate in the text that the trajectories terminate at Frankfurt in the surface layer (>950 hPa).

Figures 12 and 13: Please number the regions on the map and legends so it is easier to identify the source regions. It's a bit difficult to distinguish some of the colors.

Lines 336-337: Including information from MACC-City fire source CO should help confirm this hypothesis.

Line 363: The sign in front of 2 ppbv and 1% should be negative, and actually -1.8 ppbv to be consistent with the previous text (Line 315).