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Atmospheric Chemistry and Physics Discussions

Interactive comment on "COVID-19 lockdowns highlight a risk of increasing ozone pollution in European urban areas" *by* Stuart K. Grange et al.

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Grange et al. utilized time-series random forest models to analyze the changes of NO2 and O2 concentrations caused by the COVID-19 lockdowns across European countries. This work has important findings from the natural experiment of atmospheric pollution that most urban areas in Europe is in the VOC-limited scheme of O3 formation (e.g., at least in Spring). Therefore, only mitigating traffic NOx emissions might bring in unwanted increase of urban O3. Overall, the manuscript is well organized, and the data analysis is solid and consistent.

Line 29: I suggest add the explanation of the evaluation metric of Google mobility; e.g., the search frequency of points of interest, or the visit frequency (or duration spent) at

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points of interest?

Line 36: Please reconsider the wording “near-minimum”. I suppose commercial, transportation and recreation activities would be drastically declined, and the impact on essential industrial sectors would be less substantial.

Line 57: Please describe the distance between traffic sites and urban-BG sites in the selected urban areas. I wonder whether these traffic sites in various European countries would be deployed based on a unified, clear principle (e.g., distance to road curb, daily traffic volume)? Or, consider to enhance the statement around Line 70.

Line 65: Please briefly describe how to match air quality and weather sites in this study. Line 104: It is not clear, in Figure A1, whether the distribution of R2 represents the interval of R2 (minimum to maximum) for each site-specific RF model? In addition to R2, other validation metrics like normalized mean error can be used to evaluate the average discrepancy between modelled and observed results. And, I am surprised that both NO2 and O3 share good model validation results but Ox has lower R2. What are the possible reasons and implications?

Line 109: what is the percentage of underestimation.

Line 147: What is the possible cause (from the perspective of atmospheric chemistry or model validation performance) of comparable O3 concentrations in the late period of this analysis to the business-as-usual levels, while NO2 concentrations still indicated some degree of NOx emission reduction?

Line 170: I consider the less correlated relationship between lockdown date and O3 surge possible is because O3 is a more regional pollutant than NO2 (high contribution from regional transport). I wonder how about analyzing the maximum daily average 8-hr instead of all O3 observations?

Line 185: Is there any supporting mobility data to verify the actual change of mobility activities in Germany and Switzerland vs. in France and Italy?

Line 205: Please consider to add the increase of maximum daily average 8-hr ozone concentrations.

Line 210: The authors has strong assumptions that the future reduction pace of NO2 would follow that in the past decade, and the O3 increase would greatly relate to the change of traffic emissions. I am not very confident with these assumptions. In particular, O3 pollution is a regional issue, and is relevant to emission controls not only for NOx but also for VOCs (e.g., deeper mitigation of NOx might lead to O3 reduction). Similar concern for the statement in the abstract (e.g., the predicted situation in 2028)

Line 265: and biogenic VOCs emissions.

Figure A3. What are the measurement methods and data reliability of VOC concentrations?

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