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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Modeling the Impact of COVID-19 on Air Quality in Southern California: Implications for Future Control Policies" *by* Zhe Jiang et al.

**Anonymous Referee #3**

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This manuscript investigated the air pollution during the COVID-19 lockdown period in Southern California. Using WRF-Chem modeling simulations, the authors found that PM2.5 concentrations decrease while O3 concentrations decrease/increase in rural/urban areas. This study suggests for Southern California control on primary PM2.5 emissions and balanced control on both NOx and VOCs emissions are needed to improve the air quality.

The text is concisely written and well documented. The topic is applicable for the Atmospheric Chemistry & Physics journal. However, the current manuscript misses detailed explanations and necessary analysis (please see the remarks below). First,



it is not clear how the emissions under COVID lockdown are projected. The authors listed several data sources and then 'scale' the 2012 CARB emissions, but did not show the details. The current manuscript also only listed the relative changes of each species by sector, but not the total change. Second, the modeling study completely ignore the non-linear O3-NOx-VOCs chemistry. The VOC-sensitive or NOx-sensitive regimes in southern CA could change under large emissions perturbations (i.e. 70% off in this case). More rigorous analysis is needed to support the conclusions. Third, the authors leave a lot of important information in the supplementary material. In my opinion, some of them should be moved to the main article.

In summary, the current manuscript shows important results but need further work. Major revisions as indicated in the comments and remarks below are needed before consideration of publication in ACP.

Detailed Remarks/Suggestions for Revision

Line 25: 'decrease' should be 'decreases'

Line 116: Table S2 is important, and needs to be moved into the main article. The author should also show the change of the total emissions because these sectors have different contributions. A figure could be added to the revised manuscript.

Line 117-120: How the authors estimate the emission changes from the fuel consumption? EPA uses the MOVES model to calculate the mobile emissions based on vehicle travel mileage, vehicle types, road types, and other factors. Which method is used here? Also in Table S2, the authors estimate the different reduction rates for onroad and off-road transportation. How it is computed?

Line 122-123: The assumption that the changes in power plant emissions are proportional to electricity demand in CA may ignore the impacts from interstate electricity transmission and the different responses from coal burning power plants, renewable energy sources such as wind and solar which might not change their outputs. EPA has the CEMS program which is monitoring the power plant emissions of CO2, NOx, and SO2, which are more reliable for the modeling study.

Line 134-137: I am concerned about the approach using the top-down NOx emissions here. Usually there are substantial differences between the top-down emission products and bottom-up emission inventories, so it is hard to replace only NOx in the bottom-up emissions with a top-down estimate. Second, I don't understand how the COVID NOx emissions are calculated. Figures S2 says 'NOx emission changes due to the COVID-19, which is quantified using the difference between the real-world NOx emissions and the emissions in a hypothetical scenario without considering the COVID-19'. So the real-world NOx emissions are from the top-down products while the scenario without considering the COVID-19 is from the CARB emissions in 2012-2018 extrapolated to 2020? If that is the case, the authors should be prove that the bottom-up CARB emissions and top-down emission estimates are consistent spatially and quantitatively. More explanation is needed here. Lastly, adjust the NOx emissions are very important to this modeling study, so Figure S2 should be moved to the main article.

Line 137-138: I am more confused. FigS2 did show changes before and after 03/19. But as Goldberg (2020 mentioned in the introduction, this change may be caused by the natural variability of NOx (NO2 observed by satellite) due to change of temperature. Second, I am curious how the anomaly is calculated. To remove the seasonality, usually multi-year climatology is calculated first then the anomaly can be estimated. After reading the manuscript, I don't think the authors use this method. Detailed explanation is needed here.

Line 173-174: What is the criteria to define rural and urban here?

Line 180: Figure S3 shows the spatial performance of WRF-Chem, which should be moved to the main article. The figure is too smart to read. Can the authors add a scatter plot to show the model performance? It looks like WRF-Chem overestimate the



PM2.5 and O3 in LA basin during the post-lockdown periods, so it is not surprising the emission reductions can improve the model performance.

Line 190: Why use the population-weighted concentrations here? As mentioned above, the population-weighted concentrations will have more weights on populous LA basin area, where the baseline model did not have good performance.

Line 198: Are the soil NO emissions taken into account in WRF-Chem? With different meteorology, the natural NO emissions can play a role here.

Line 201-201: As mentioned above, need to update Table S2 to show the contribution to the total emissions.

Line 205: Again, the population-weighted concentration changes are mainly determined by the populous areas such as LA basin. Can the authors also show changes in mean concentrations?

Line 208-214: Fig2 e-f, how to separate the meteorological impacts and emissions impacts? The differences between 'Base' and 'Lockdown' contains impacts from both factors.

Line 226: What are the reductions in the primary PM2.5 emissions for other PM2.5?

Line248: Again, Table S1 should be moved to the main article so the readers can figure out the differences among these sensitivity experiments. Also, why the authors select coefficient 0.3 for the last two experiments? Is it estimated from the future regulations in CA?

Line 251-252: This statement ignored the non-linear chemistry of ozone.

Line 266-267: Same concern here, it is dangerous to use the reduction from NOx0.3 and VOC0.3 runs to conclude that the VOCs reduction can offset the NOx reduction because the nonlinear ozone chemistry is ignored here. With change of NOx and VOCs, the ozone production efficiency will change as well. I doubt in these two runs, the ozone chemistry could shift into different regimes. Further analysis such as ozone isopleth diagram is needed here.

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