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Reply on RC2

Jason R. Schroeder et al.

Author comment on "Changing ozone sensitivity in the South Coast Air Basin during the COVID-19 period" by Jason R. Schroeder et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-178-AC2, 2022

Thank you for taking the time to review our draft manuscript. The authors appreciate the reviewer's suggestions and will address each of them.

The reviewer notes that the version of WRF indicated in the draft is ten years old. The authors thank the reviewer for catching this detail - we made a mistake when preparing the draft, and the version of WRF that was used is WRFv4.2.1 (far less old). We will make this correction in the text. The authors also appreciate the reviewer's suggestion for adding a meteorological evaluation, and we have prepared a Supplemental Information document that includes a table of meteorological evaluation.

The authors also thank the reviewer for suggesting inclusion of transportation-related activity data in a Supplemental Information document. We have included these data in graphical format in the Supplemental document for interested readers.

The reviewer also points out areas where our paper seemingly does not align with recent work by Parker et al (2022). Parker et al (2022) is cited in the draft manuscript reviewed by this reviewer, however, the authors acknowledge that we can do more to further compare/contrast the approaches used in these two papers. This will be included in the Discussion section. The differences in results are believed due to subtle differences in spatial scale: our results are presented as basin-averages over the whole SoCAB, while the final sentence in Parker's abstract (pointed out by the reviewer) explores results on sub-basin scales. Figure 10 in Parker et al shows their map of base case O3 minus COVID-adjusted O3 (both modeleed). Spatially, the majority of the study area in their figure shows a decrease in O3, and a small region in LA county shows an increase in O3. On average over the SoCAB, our results appear to be in good agreement. We will make additions to the Discussion section to address these points. The reviewer also brings up Parker's conclusion that parts of the SoCAB were still VOC-sensitive. It should be noted that Parker's basis for this conclusion is outcome-based (i.e. did O3 drop in response to NOx reductions?) rather than process-based (i.e. did the indicators for O3 sensitivity change?). It should be noted that, at the chemical process level, there are numerous scenarios where O3 chemistry may "flip" from VOC-sensitive to NOx-sensitive while still producing an increase in O3 due to non-linearities in chemistry alone, (especially when

dealing with airmasses that are near the chemical transition point!) Therefore, Parker's observation that O3 increased in some areas while NOx emissions dropped is not a solid indicator of the underlying chemical regime (*especially* given that the SoCAB is near the chemical transition point!). Our paper presents observation-based evidence that the underlying chemical regime indeed flipped - though we do note in our Discussion section that this may not yield even results over the entire air basin, and that while the basin as a whole is expected to see O3 improvements as NOx is decreased, select areas may see O3 increases in the coming years.

We hope that these additions are satisfactory for the reviewer! Thank you again for taking the time to review our paper.