Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC3
https://doi.org/10.5194/acp-2022-87-RC3, 2022

Comment on acp-2022-87

Anonymous Referee #3

Referee comment on "Secondary $PM_{2.5}$ decreases significantly less than NO_2 emission reductions during COVID lockdown in Germany" by Vigneshkumar Balamurugan et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-87-RC3, 2022

The authors present measurements from ten metropolitan areas in Germany to evaluate the impact of lockdown restrictions on air pollutant concentrations. They use the GEOS-Chem (GC) chemical transport model to simulate the pollutant concentrations for 2020 and 2019 and derive the percent changes during the lockdowns to find that although NO2 reductions were evident PM concentrations did not drastically change. Furthermore, they discuss the impacts of the NOx reductions on radical and ozone concentrations as well as PM2.5 formation and the role of NH3 emissions on PM pollution. This paper is interesting and fits well within the scope of ACP after the following comments are answered.

Main comments:

My main concern is on the assumption that the VOC emissions did not change during the lockdowns based on a limited number of published studies that only account for a small fraction of the VOCs. Given that VOCs can originate from multiple sources that vary by season and meteorology I consider that there is limited confidence in this assumption. Furthermore, VOCs will be responsible for SOA in the model and can account for a significant part of the PM mass. I consider that a sensitivity analysis of the model to VOC changes would be a more honest approach and valuable addition to this study. The response of SOA to these changes and their relative influence compared to NH3 emissions, especially during PM pollution days, would indicate whether VOCs are also an essential source of PM pollution in future scenarios.

Given that this work is based on the WRF model it would be great to see a more detailed evaluation of the model for the different gas- and particle-phase components. Evaluation of the model at high and low concentration periods from previous years and how accurately they are predicted would be of value and give some context on the uncertainty of this approach. Evaluation of the chemical composition derived by the model to ambient observations would also be important. Are there any chemically speciated measurements in Germany during this period that the authors could compare their model to? If not, has this been done in the past and what was the agreement of the model to the observations?

Other comments:

Line 51: First time that VOCs are introduced

Line 225: Which VOCs? How much of the reactivity do they represent?

Line 235: What are the expected VOC emissions during the winter in Europe?

Line 290-294: OA formation and specifically SOA could also be affected by changes in VOC emissions both of biogenic and anthropogenic nature. Further discussion here would be of value.

Line 335-337: I find this statement a stretch given the number of other sources of PM pollution.

Line 339-348: Some statistics on how many days were the "simultaneous" or "independent" would be great here not only for one region but for all regions in Germany.

Figure 6: I find this figure hard to follow and the messages are not clear to me. It would be great if the timeseries panels fit the whole page and the "simultaneous" or "independent" periods are highlighted by the background color of the graphs. Adding the temperature and RH timeseries would be great too. The authors can also include the NH3 measurements in a different panel and the background colors could guide the reader's eye's to evaluate whether there is a good or bad agreement between PM, NH3, RH, and temperature increases. Furthermore, it would be great to see a graph that highlights what happens in different regions of Germany and some more statistics on these trends to evaluate their importance.