Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-685-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global Impact of COVID-19 Restrictions on the Surface Concentrations of Nitrogen Dioxide and Ozone" *by* Christoph A. Keller et al.

Anonymous Referee #2

Received and published: 20 October 2020

In studying the effect of COVID-19 restrictions on air pollution, the meteorological variability complicates a direct comparison with pre-lockdown periods. The authors are well aware of this, and tackle this problem by comparing ground observations against model simulations based on a business-as-usual emission inventory. Local modelling biases (due to representation error, wrong emissions, meteo, or chemistry) are corrected for by a machine learning approach, trained in a pre-lockdown period (2018-2019). The paper is well written, and presents a sound and well-developed approach, hence I recommend its publications after addressing the following minor issues.

I agree with the major comment of the previous reviewer to provide more details about

Printer-friendly version

Discussion paper

the machine learning methodology and how the potential pitfall of autoregression of time series is dealt with.

For NO2, The machine learning approach appears to be surprisingly powerful to adjust a rather coarse chemical transport model (25 x 25 km2 resolution) to the local situation, given the strong gradients found in cities. Figure 2: it would be interesting to make a distinction between (rural) background stations and street stations. Is the bias correction method sufficiently strong to solve the representation error of the latter category?

Figure 4 shows underprediction of the uncorrected model for Milan and Taipei, overprediction for NYC, and alternating under- and overprediction for Wuhan. In my opinion, your analysis in 3.1 lacks some words about what we can learn from the modelling biases. Are representation errors dominating, or are we looking at e.g. wrong emission estimates?

Figure 5, just out of curiosity: is there a reason why so many observation sites in Romania measure significantly higher NO2 than expected by the BCM?

Figure A1: Showing results for more Chinese mega-cities would be instructive, especially given the strong local observation network in China.

Figures A1-A3: Sometimes strong NO2 reductions are already visible months before the official lockdown starts (e.g. Ljubljana, Vienna, Dublin, Boston, and Denver). Any explanation?

Figures A1-A3: the blue and red lobes in the pre-COVID period can be used to estimate the error in your methodology and put the results (e.g. in Table A1-A3) in better perspective.

Figures A1-A3: I am missing an indication of n, the number of observation sites used for each city.

Section 3.2: Personally, I find the results for O3 less striking, although I directly ad-

mit that an O3 analysis is more subtle and less straightforward than NO2. Figure 8a shows the flattening of diurnal cycle, which is used to explain the marginal effect of the measures on average O3 concentrations in Figure 6 and 7. I think it would be more interesting to see these figures for daily peak values of O3, instead of daily mean values.

Section 3.3, lines 247-252: I had to read this several times to understand, and I am still not sure if I do by now. First it is stated that NO2 concentrations do not change 1:1 with changing NOx emissions, but in the following sentence it is suggested that NO2 columns from OMI are used to scale underlying NOx emissions. Also, I can not deduce how the sensitivity study is set up exactly. Please rewrite.

Section 3.3: Your emission reduction results (e.g. Figure 9b) are potentially prone to sampling biases. According to Figure 5, the results for India are based on only 7 stations (!). Furthermore, as the ground-based monitoring stations are typically located in cities, the results reflect emission reductions within cities (such as traffic), but not necessarily emission reductions of other sectors such as industry or power plants. This should be addressed in a short discussion.

Conclusions: lines 305-313 describe an additional experiment about the effect of NOx emission reduction on surface ozone, which, according to my taste, should be shifted backward (e.g. in an additional section 3.4) before the conclusions start.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-685, 2020.

Printer-friendly version

Discussion paper