Reply on RC1

Volker Matthias et al.

Author comment on "The role of emission reductions and the meteorological situation for air quality improvements during the COVID-19 lockdown period in central Europe" by Volker Matthias et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-372-AC1, 2021

Point by point responses to the comments of Referee 3

We thank the referee for the valuable and very constructive comments that helped improving the manuscript considerably. All comments are repeated in this point-by-point answer of the authors. Reviewer comments are written in *Italics* while author's responses are given in **Bold**.

Comment Referee 3

This manuscript aims to assess the roles of emission reduction due to Covid-19 lockdown and meteorology in air quality during January-June 2020 in Central Europe. It first developed detailed emission inventories for this period based on the previous year's emission data and various activity data during the lockdown, and then examined meteorological conditions during the first half of 2020 in Central Europe and compared air quality data collected from 6 sites in the above period with those from previous years. Then a chemical transport model was used to simulate the air quality in 2020 and sensitivity runs were conducted to assess the impact of emission reductions during the lockdown and the effect of meteorology change to year 2016 and 2018. The key findings include a large reduction in NO2 concentration due to city lockdown, increase or decrease in O3, and smaller reduction in PM2.5 concentrations. It also demonstrated the importance meteorology in modulating the air quality.

The manuscript adds useful information to the large body of literatures on air quality during Covid-19 lockdown and the complex interplay of emission, meteorology and atmospheric chemistry. The methods are reasonable, though not particularly new. My main comment is that there are few discussions to convey new insights/findings of this study in comparison with numerous studies by other investigators on the issue.

See the answers to the follow-up comments below.

As shown in the introduction, there have been quite a lot of studies on the response of air quality in Europe (also see two more recent papers), what is new in the methodology adopted in the present work?

Compared to earlier more limited studies on the same topic, this investigation

aims at eliminating some of the restrictions seen there. In the following aspects, this study differs from previous approaches:

- A very detailed emission construction for bas case (noCOV) emissions in 2020 considering changes between 2016 and 2020 on sectoral national level.
- A very detailed construction of lockdown emissions, also compared to Dubois (see our response to another comment later in this document) and Guevara et al. (2020), who considered a shorter period.
- A comprehensive view on the effects of emission reductions and the meteorological situation in entire Central Europe. Similar studies are limited to one country or smaller regions (e.g. Velders et al. 2021) or shorter time periods (Menut et al., 2020).
- Simulations with other meteorological data from previous years but the same emission data revealing the impact that meteorological conditions can have.

And what are new findings of the analysis?

Our main new findings are the following:

- Lockdown emission reduction effects on ozone vary in time and place from concentration increases to decreases. In May and June, ozone increases can only be seen in cities and very limited areas with high NOx emissions
- NOx emission reductions during the lockdown resulted in decreased nitrate concentrations, but slightly increased sulphate concentrations because of more available ammonia.
- The meteorological situation in April 2020 was far more important for low PM concentrations in Northern Europe than lockdown emission reductions.
- Secondary pollutants like ozone and PM show larger variations caused by meteorological influences than by lockdown emission reductions in Europe.
- Observations only are not sufficient to say something about the lockdown effects on concentrations.

I am impressed by the careful development of the emission inventories for Central Europe for January-June 2020, how does it compare to another recent inventory (Doumbia et al. 2021)?

There are many similarities between the inventories and the methods that were used to derive Lockdown Adjustment Factors, e.g. the use of google mobility data for changes in road traffic. The data set presented by Doumbia et al. covers all continents which is why sometimes different data sets are combined or data sets are used that cover more regions than Europe. This study only uses data sets to derive LAFs that are available for almost all European countries. Some of them have higher temporal resolution than the data used by Doumbia (e.g. data for aviation and shipping). Doumbia et al. estimate higher emissions from the residential sector based on google residential data while we left this sector unchanged. The reasoning behind this that the heating demand is most likely not significantly modified when more people stay at home compared to the case when they go to work.

One main difference in the 2020 emission dada sets is that we extrapolated 2016 CAMS emissions for Europe to the year 2020 to derive the emissions for the noCOV case. Estimated total emission reductions for Europe from both models are quite close (25 -30% decrease in NOx in April.

We added a few sentences about the relation between the inventories in section 6.2.

Another comment is about the organization of this paper. It is currently very lengthy, and I think it can be shortened to highlight the novel parts of this study.

We reorganized the paper by moving subsection 6.1 upwards at the end of section 4 (see our answer to the next comment) and shortened it. We removed the data about the average concentration changes at the measurement stations in the first 6 months of the year 2020 because it does not add much information to what is described at the end of section 5.1. We moved the text about the differences between the meteorological situation in 2016 and 2018 to the appendix, because this is sufficiently described in subsection 5.2.

Although information about the modelled PM species was added, the main text of the paper (excluding appendix and references) is now shorter by 500 words.

I also suggest moving the model validation part presented in a later part to the earlier part before showing the model results.

We moved subsection 6.1 upwards at the end of section 4. It is now the new subsection 4.3 and the title was changed into "Comparison between model results and observations". Subsections in section 6 were modified accordingly and the numbering of the figures was adapted.

The discussion section (Section 6) is a really part of the general results, with no in-depth discussion such as a comparison with other researchers' work and significance/implications of the results, which should be included.

We moved subsection 6.1 to section 4 and present the comparison between model and observations there. We now put our results in perspective to previous studies, in particular for NO2 and O3. We also discuss interactions between inorganic PM components and uncertainties related to SOA modelling. We moved the part about differences between the meteorological situation in 2016 and 2018 to the appendix and thereby focused the discussion on the emission estimates and the modelled concentrations.

The conclusion section is rather a summary of the results and is also unnecessarily long. It should be condensed and highlight the key findings.

It was shortened at several places and now the key findings are more prominently presented. One sentence about lockdown effects on PM components was added.