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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Quantifying the emission changes and associated air quality impacts during the COVID-19 pandemic in North China Plain: a response modeling study" *by* Jia Xing et al.

Anonymous Referee #1

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The manuscript titled "Quantifying the emission changes and associated air quality impacts during the COVID-19 pandemic in North China Plain: a response modeling study" by Xing et al. quantifies emission changes during the shutdown in spring 2020 caused by the Covid-19 restrictions in the north China plane. The emission changes in 2020 are estimated from observed concentrations using response-based inversion and are compared to conditions in 2019 and hypothetical conditions. The overall scientific question addresses an interesting and up-to-date issue which is relevant for air quality research and analysis.

The used response-based inversion model ("response model") includes a response

surface model ("RSM") developed in previous studies (e.g. Xing et al. 2018) which provides emission-concentration relations. Based on this, emissions of five pollutants (NOx, VOC, SO2, NH3, PM2.5) are corrected with respect to locally observed concentrations. The chosen approach appears to induce suitable corrections of emissions, however some points might need to be clarified/adopted:

1. Talking about emission-inversion, a more detailed description of existing top-down inversion methods is needed in the introduction which demonstrates the novelty of the presented method more clearly (ll. 52). Methods for emission optimization in the context of inverse modeling of parameters and chemical data assimilation should be noted and shortly discussed with respect to advantages and disadvantages of the new method.

2. What remains somehow unclear is the way how emissions are changed by the response model. Are emissions only corrected locally (i.e. the emissions at the location of the observations) or does the response model consider inverse non-local transport and transformation processes (i.e. correction of emissions at remote locations)? What is the temporal extension of the corrections? Maybe, this can be described more clearly in the manuscript. In case of non-local corrections, it would be interesting to show the spatial patterns of corrections induced by the inversion.

3. Concerning the evaluation with observations: In data assimilation, forecasts are usually evaluated by independent observations, which are not considered in the optimization procedure. This is the standard way to investigate the usefulness of applied corrections. The corrected forecasts should fit the observations used for correction for any consistent method by definition. Thus, an evaluation with those observations does not provide additional information in the methodical point of view.

Smaller comments:

- In the description of the correction of SO2-emissions: Are primary SO4 (C^s_{p-SO4}, ll. 118, Eq. (E4)) concentrations assumed to be correct? This might be worth mentioning in the description.
- Maybe related to point 2 (above): How does are the emissions from the bottom-up inventory of 2017 corrected by the response model? Is this also based on the observations used for correction later on? Or are the total annual emissions scaled by some correction values (i.e. constant correction of emissions keeping the annual and diurnal variations constant)?
- It might be worth explaining the estimation of the hypothetical no-shutdown emissions a bit more in detail (ll. 186). Are these estimated from temporally- and spatially averaged ratios between the periods?
- As far as I understand, Figure 4 shows averaged observations over the entire plain. Such spatially-averages should be used with caution as observations from single stations might be missing for some time (as noted in ll. 172). This hampers the interpretation of the temporal evolution of the plotted observations. Moreover, it needs to be made clear if the simulated concentrations shown in Fig.4 only refer to these stations, which provide used observations at each time. Drawing a continuous line might be misleading in case the simulated concentrations include different stations at different times.

Technical comments:

- The reference to Figure 2 in line 162 is not clear. It seams to be not connected to Fig. 2 of this manuscript. What is the content of the cited manuscript by "Xing et al, under review"?

- The x-axis of Figure 4 is not defined. Does this refer to the day of year?
- Fig. 4: The red line is hardly visible e.g. in Figure 4c. It might be useful to plot it as somehow thicker/ dashed line.
- Fig. 6: If I did not miss it, it would be interesting to include the concentrations resulting from changes in all emissions due to the shutdown (Period 2, incl. Shutdown-effects) in Figure 6 (maybe instead of plotting observations). This would make the overall changes in concentrations due to the shutdown more clear than comparing the simulated no-shutdown concentrations with observations. If this comparison was made for a specific purpose, maybe did not became clear to me in the text.
- Line 266: The response to O3 to NOx and VOC appears to be quite linear in the local regime as shown in Fig. 5. I would suggest to replace the formulation in ll. 266 by e.g. "opposite response" or "compensating effects".
- Line 285: To which decrease in NO3 concentrations is here refereed to? Is this the decrease in Period 2 compared to Period 1?
- Finally, three small technical suggestions (i) delete the "a" in line 280: "... result in strong NH3-rich conditions, ...", (ii) add an "a" in line 302: "..., we conducted a sensitivity analysis ..." and (iii) in line 340: e.g. "... was applied to the investigation of emission changes ..."

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