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Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Evident $PM_{2.5}$ Drops in the East of China due to the COVID-19 Quarantines in February" *by* Zhicong Yin et al.

Anonymous Referee #1

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The authors simulate the decline in PM2.5 concentration that resulted from emissions reductions during the COVID-19 pandemic using GEOS-chem. They use 1985 and 2010 emissions to simulate the 2015-20 period. They obtain reasonably good correlations between simulated and observed daily mean PM2.5 and show that COVID-19 led to a significant decline.

The study is interesting, in the sense that knowing how much PM2.5 declined due to COVID-19 after other factors are accounted for is useful, and well-timed. The physical and chemical processes responsible for PM2.5 concentrations during COVID are discussed to some extent.

In response to my comments during the access review, the authors added two new subfigures elucidating the role of meteorology in generating PM2.5, and they added a

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literature review of chemical mechanisms for the formation of the remaining pollution. These additions are valuable, but in my opinion further major revisions are still needed before the paper can be published, as follows:

1. Abstract and introduction.

The abstract and introduction should be refocused towards atmospheric processes. While atmospheric processes are discussed (lines 30-37 and 42-49), for Atmospheric Chemistry and Physics they should be the main topic of the introduction. The main topic of the introduction is currently Chinese air quality and COVID, but the paper is about the disentangling effects of meteorology from the effects of the COVID lockdown, and so there needs to be more detail on meteorology in China. This is done very well in the introduction to Yin and Zhang (2020); perhaps some more detail specifically on how 2020 meteorology differs from the climatology would distinguish the two studies? You say that variations in the surface wind, boundary layer height and moisture conditions affect air quality, which is not wrong, but specifically what do they typically do in China, when, and where? The literature review also lacks detail; care should be taken to point out explicitly how this paper differs from the large number of other works on the topic. I appreciate this is difficult because of the very large number of very recent publications, but it is definitely possible to do more here.

2. Data description

What technology is usually used to meaure PM2.5 for this dataset? When I tried the URL it didn't work. Please reference the dataset more thoroughly.

3. Model description

This section needs a description of how the model represents aerosol microphysics. The model evaluation presented at the end of this section deserves considerably more detailed study in its own section – what are the biases in the model and how might they affect the subsequent analysis? Unless you can reference other studies evaluating an

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identical model configuration?

4. Method to quantify influence of quarantine

Running GEOS-chem for two different emissions scenarios seems like a good idea, and it's good to see that the changes due to meteorology are consistent between years.

However, did you consider the physical justification for a linear decomposition? If we consider, crudely, the Chinese airshed as a simple chemical reactor in steady state, then the linear decomposition would not be obviously appropriate (though it may be a reasonable approximation) since the steady-state concentration is the product of the emissions and the loss lifetime.

Line 99 (minor comment) – I don't fully understand the "the PM2.5 percentage due to changing meteorology". Do you mean "the change in the percentage of PM2.5 due to changing meteorology" here and later in the paragraph?

Line 107 – "the change in emissions resulted in a linear change in air pollution". I don't think this is the message of the very nice Cai et al paper that you cite here. In fact, it is well established that emissions changes often do not lead to linear changes in air pollution, even though I do accept, from the evidence you present, that this is case in China from around 2013 to 2019. The most obvious reason is the sulfate-nitrate-ammonia thermodynamics discussed by Cai et al. Naively, reducing sulfate emissions should reduce concentrations linearly, but reducing nitrate and/or ammonium emissions may not change concentrations at all, or may result in very large decreases in concentrations, depending on the regime (whether saturated by, or limited by, ammonia, for example). Similarly, reducing primary emissions may lead to more new particle formation, as discussed by others, and more secondary aerosol formation, which would also mean the decrease in number concentration is likely sub-linear. Line 197 of the manuscript points this out explicitly. New particle formation wouldn't directly affect changes in mass concentration, but it could have important indirect effects through the size dependence of aerosol dry and wet deposition rates.

So while decreases in concentration may be linear with emissions in specific cases, and does seem to be true in China, this will not be true in general, and should be clarified. Also linearity in previous years, e.g. from 2013 to 2017, does not imply linearity in subsequent years. The linear extrapolation method used therefore brings with it a large uncertainty which should be studied in detail.

5. Results

Line 146: the description is good but some more introductory detail and referencing would be useful. For example, what is the East Asia deep trough? Please supply reference, e.g. Song et al, J. Climate 2016.

Line 149: This is potentially a useful result, but what is the importance of the hygroscopic growth? Its importance surely depends on whether the PM2.5 measurements are of dry or of hydrated particles. If dry particles are measured, hydration might still be important if it affects deposition rates. So what is the difference in humidity and what difference to the size of typical particles would that lead to?

Can you calculate approximate ventilation rates for the boundary layer in the different meteorological conditions, or otherwise increase the level of quantitative detail in lines 140-150, which are currently very qualitative? Can this be used to back up the conclusions about PM2.5? For example, the regression of PM2.5 against "BLH, wind speed, SAT and humidity" done in Yin and Zhang (2020) looks like a nice technique to understand the relationship of air pollution and meteorology, could you do the same thing here for 2020 data? Or at least provide similar numerical detail for what is the BL height and how it varies in the years studied? Is there a role for sea surface temperature here also?

Line 160-165 can you estimate, with quantitative justification, uncertainty ranges for these numbers?

Line 169 – the impacts of COVID-19 quarantines on air quality was weaker south of

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30N. This is an interesting conclusion. Could it be related to meteorological differences? Is this consistent with the later statement that in north China, secondary aerosol concentrations increase when primary aerosols decrease? Is that true in south China?

Line 176 what are the reasons for the regional differences?

6. Conclusions

Line 227-240 It is valuable to point out these shortcomings and qualifications for your study. Can you take this further by estimating uncertainties as I suggest above, and speculate what the effect of the interactions between emissions and meteorology would be?

What are the implications of the study for the practice of atmospheric chemistry and physics, beyond those of Yin and Zhang (2020)? Please spell these out in the conclusion.

Figure 1: what is the significance of the red color on the left side of subfigure a)?

Figure 3: state that these figures show simulated data. What is responsible for the increases on the far left of Figure 3c?

Figure 4 please label color bars with units

Detailed language editing is needed throughout the paper.

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