The author has taken some efforts to improve the manuscript. However, I still have more concerns about the manuscript and the authors' responses.

1. To be honest, I am not clear about the significance of this study. If we want to present the decreased human activities in the COVID-19 pandemic using pollutant concentrations, an analysis of observed NO_2, SO_2, and $PM_{2.5}$ is enough. Why we need the simulated NO_2, SO_2, and $PM_{2.5}$ using the modified emissions that are adjusted by observed NO_2, SO_2?

2. About the title. The main purpose of this work is to infer the changes in human activities in the COVID-19 pandemic, which can be directly reflected by changes in emissions over China. Please note that the change in emissions is more important than changes in PM2.5 concentrations in the context of the manuscript. For example, most sentences in the summary section are about the changes in emissions other than PM2.5 concentrations. Hence, please revise the title of the manuscript to reflect the changes in emissions.

3. I do understand meteorology should be excluded when retrieving emissions out of measured concentrations. The authors should clarify why "meteorological influences were reduced by combining surface data with output from a three-dimensional chemistry model to calculate estimated emissions" in the manuscript.

4. I believe several days smooth is important for the method. Otherwise, the adjusted emissions will vary very sharply. The explanation of the seven-day smoothing process is not convincing. As I know, there is a long period of LNY holidays every year in China, which should robustly impact the anthropogenic emissions and pollutant concentrations near the LNY-period. Hence, there may be no clear weekly variations in China in the period. At least, the authors should compute the significance of the weekly variations in that period to support the validity of the seven-day smoothing process.

5. Fig. S7-9 just provides a spatial estimation of the model performance. It is not very important for the study. Following Fig 4, the authors should provide time-series estimations in every grid-cell (or sites) near LNY-period. For example, the spatial distribution of temporal correlation coefficients or temporal RMSEs is needed.

6. The Authors repeated the equations in detail on their method. But respectfully I do not very agree with this explanation. The major flaw is that the β is set to be a fixed coefficient (i.e., linear relationship) by default for any model simulations. I will illustrate that through four aspects.

1) In the response, the authors use the equation (page 6)

$$\frac{E_{adj}}{E_{mod}} = \beta \cdot \frac{C_{obs}}{C_{mod}} \qquad (A)$$

which are applied to the real world or to a model (E_{adj1} and C_{adj1}) (the first sentence of page 7) and derive the relationships, $E_{adj}\!:C_{obs} = E_{adj1}\!:C_{adj1} = E_{adj2}\!:C_{adj2}$ (the second sentence on page 7). Please note that the relationships stand just when β is unchanged for *adj*, *adj1,* and *adj2* according to eq. (*A*). But, at least in *adj1* and *adj2* simulations, the authors use $\beta = 1$ and $\beta \neq 1$ respectively. On the contrary, if authors believed such different β setting in *adj1* and *adj2* simulations are both reasonable, the relationships $E_{adj}\!:C_{obs} = E_{adj1}\!:C_{adj1} = E_{adj2}\!:C_{adj2}$ cannot stand.

2) In experiment *adj1*, the authors chose arbitrary $\beta = 1$ for the simulation. As a result, the adjusted emission E_{adj1} and simulated C_{adj1} are arbitrary. C_{adj1} is not equal to C_{obs}, and E_{adj1} is not the emissions corresponding to C_{obs}. In this case, why the eq. (*A*) still stands for *adj1*?

3) This method implies that the value of β is unchanged no matter what β they chose in *adj1* (here the authors chose arbitrary $\beta = 1$). I am afraid β would change when choosing a very large (10 as an example) or very small (0.1 as an example) β in *adj1* because the large scaling in emission will cause non-linear responses to pollutant concentrations. Please show the readers that the spatial distribution of β is unchanged when using different β (for example, 0.1, 1 and 10) in *adj1*.

4) Again, linear change in emissions does not cause a linear change in concentrations, considering many non-linear impacts of chemical reaction, deposition processes and meteorology. Hence, simulations with different emission amounts should have a different relationship between emission and concentrations. In another word, for *adj1* (without regard to the point (2))

$$\frac{E_{adj1}}{E_{base}} = \beta_1 \cdot \frac{C_{adj1}}{C_{base}}$$

and for *adj2*

$$\frac{E_{adj2}}{E_{base}} = \beta_2 \cdot \frac{C_{adj2}}{C_{base}}$$

Apparently, the manuscript implied $\beta_1 = \beta_2$ without any explanation. Hence, please show the readers why the β derived from *adj1* can be directly applied to *adj2*.

7. Once reducing the emissions in SO_2 and NOx (two critical precursors for $PM_{2.5}$ considering the abundant NH_3 over China) in the model according to observed NO_2 and SO_2, the $PM_{2.5}$ concentrations generally approaches to observations. Hence, it is not surprised to get good NO_2, SO_2 and $PM_{2.5}$ simulations. From this point, $PM_{2.5}$ is not “totally independent”. Please add some discussion for the validation.