Dear editor,

Thank you for handling our paper. We are grateful that the two referees are helpful to improve our work. We provide a point-by-point response to the reviews and revised the manuscript accordingly.

With the recently available near real-time data, we are now estimating emissions of twelve months in 2020 instead of only eight months in the last version of our manuscript. We have checked and updated our emission estimations and used an atmospheric chemical transport model to evaluate our emission results, showing good simulation performance, which suggests that our emission estimation method and results reproduce the response of China's anthropogenic emissions to the COVID-19 pandemic. We have updated the manuscript with all of the new values and new figures. The marked-up manuscript version shows what we have revised.

Thank you for your consideration of this manuscript.

Sincerely,
Bo Zheng on behalf of all co-authors

Referee #1

The authors develop a simple method based on the most recent statistical data for estimating the anthropogenic emissions of air pollutants in China during the period from January to August in 2020. They report for the first time the changes in air pollutants emissions caused by the COVID-19 lockdowns in China using a bottom-up approach. Additionally, the relative changes in monthly emissions from 2019 to 2020 are compared with the satellite and ground-based observations. The emission datasets developed in this study provide essential and important information for the analysis of the COVID-19 pandemic in China.

Consequently, the contents of this manuscript and datasets developed in this study are suitable for "Earth System Science Data". However, there are some points which should be analyzed and clarified. The reviewer recommends the acceptance of this manuscript after minor revisions.

Response:

We appreciate the referee's positive and constructive comments. Our point-by-point responses are given as follows.

(Major comments) (1) Lines 100-112: To what extent can the simple method developed in this study reproduces the changes of emissions in the past years? For example, by comparing with the MEIC in the emission changes from 2018 to 2019, it might be possible to validate the method and estimate its uncertainties. Such analysis should be added for identifying the application of the method to other cases and other regions.

Response:

We thank the reviewer's suggestions and agree that the validation of our method will identify the application possibilities in other cases. Our method relied on the monthly statistical data to track emissions, which have already been used by the MEIC model to reconstruct the monthly variation of emissions in the past years. The only difference is that MEIC has more constraints on energy consumption and emission factors in the past years than in the COVID period. To evaluate our method and the uncertainties based on an independent method and dataset, we discussed with the editor and decided to run an air quality model driven by our estimated emissions to simulate the interannual changes in air pollutant concentrations and evaluate the simulation results against surface observations. The comparison results reveal a broad consistency, suggesting that our emission estimates can reproduce air pollution changes well. The discrepancies and implications for uncertainties have also been discussed in the revised manuscript.

(2) Figure 4: Some differences among six pollutants are found in the industrial emissions. The values for CO and NMVOCs are small positive in January and May to August while the values for SO_2, NO_x, and BC are negative in the same period and the value for $PM_{2.5}$ is positive in January only. These differences should be discussed.

Response:

The emissions of air pollutants tended to be lower in 2020 than in 2019 due to the reduced industrial activities during the lockdown. However, the activities of part of the industrial sources before and after the lockdown were larger in 2020 than in 2019, which drove up emissions of specific air pollutants. For example, the productions of iron, steel, and non-ferrous metals during January and February were 3.1%, 3.1%, and 2.2%, respectively, higher in 2020 than those in 2019, which have generated higher emissions of CO and $PM_{2.5}$ in January 2020. The productions of iron and steel from May to August in 2020 were 2.4–9.1% higher than the corresponding months in 2019, leading to higher CO emissions in 2020. The productions of crude oil and petrochemical products such as ethylene during January and February were 3.7% and 5.6% higher in 2020 than those in 2019, which explains the higher NMVOCs emissions in Jan 2020. From May to August, the productions of crude oil and the total volume of crude oil refineries process were 0.6–12.4% higher in 2020 than in 2019, which caused more NMVOCs emissions. These monthly changes in industrial activities have been shown in Table S2, which have also been clarified in the main text of the revised manuscript.

(3) Figure 5: In January, there are big differences between emissions and observations for SO_2 and NO_x while their differences for CO and $PM_{2.5}$ are smaller. The authors should discuss the reasons more carefully. The regional background largely affected the observed CO as the authors pointed out in lines 227-229. However, it is surprising that the differences between emissions and observations are relatively small in Figure 5(c). Further discussion is needed.

Response:

To account for the impact of regional background, we run the air quality model WRF-CMAQ to simulate surface concentrations of air pollutants and compared the changes in modeled concentrations to surface observations in Figure 5 in the revised manuscript.

The comparison suggests that the model simulations driven by our estimated emissions reproduced the changes in surface observations well. The results also reveal some discrepancies between simulations and observations, probably caused by uncertainties in emissions and modeling, which have been discussed in the revised manuscript.

(Minor comments) (1) Lines 118-120: Is there no observation data of NMVOC or NMHC concentration in China?

Response:

Not yet, the surface measurement network in China does not report NMVOC or NMHC.

(2) Line 128: For NMVOC (and NO_x), the emission declining from January to August seems to be not found in Figure 1.

Response:

This sentence has been rewritten as follows.

"China's emissions of SO_2, CO, $PM_{2.5}$, and BC in 2019 reveal an evident seasonal variation with emissions declining from January to August…"

(3) Line 210: Is "surface emissions" correct?

Response:

We have changed "surface emissions" to "anthropogenic emissions" in the revised manuscript.

(4) Lines 226-229: It looks like small differences between emissions and observations in Figure 5d. If the effects of regional background are large, the differences may be more increasing.

Response:

Please refer to our response to the major comment (3).

(5) Figure 1: It's better that the monthly emissions are decomposed into source sectors like Figure 4.

Response:

Done.

Referee #2

Zheng et al. (2020) developed a bottom-up approach to estimate anthropogenic emissions over mainland China during and after covid-19 lockdown. The results suggest the reduced anthropogenic emissions due to covid-19 lockdown are mainly from industry and transportation sectors. Despite all the merits of this approach mentioned in the manuscript, the emission estimates need thoroughly evaluated to better support the conclusion. Therefore, the reviewer recommends a major revision before accepted for publication.

Response:

We thank the referee for his/her effort to improve our manuscript. We provide point-by-point responses to the comments as follows.

General comments:

In this work, changes in the emission are evaluated against changes in surface observations and satellite retrievals. However, the changes in the surface concentrations do not necessarily reflect the similar changes in the emission. There are many processes/factors that could affect surface concentrations. This kind of evaluation does not provide much information on the uncertainty of the emission estimates. As mentioned in the manuscript, meteorology plays a significant role on surface concentrations, which is not considered in this work. A better way to evaluate the emission estimates would be comparing surface concentrations from an emission-driven model simulation with surface observations. It would be interesting to see a combination of a top-down approach (via observational constraints) and a bottom-up approach (used in this work) to better assess the emission estimates and to add more value to this work.

Response:

We agree with the reviewer that it is a better way to evaluate our emission estimates through a model simulation. In the revised manuscript, we have run the air quality model WRF-CMAQ driven by our estimated emissions for 2019 and 2020, simulated the interannual changes in surface concentrations of air pollutants, and compared the modeling results with surface observations in Fig. 5. The comparison results suggest that the model simulations driven by our estimated emissions reproduced the changes in surface observations well. The text in the discussions has been revised accordingly.

Following the reviewer's suggestions integrating top-down and bottom-up approaches, we have collected the top-down estimated air pollutant emissions from previous literature, which are broadly consistent with our bottom-up estimates. The consistency between top-down and bottom-up results proves the reliability of the emission results.

Specific comments:

Page 3, line 75-76, are the emissions from cooking included in residential sector?

Response:

Yes, the emissions from cooking stoves have been included in the residential sector.

Page 3, line 89-94, does EF2019/EF2018 have monthly variability? Or should EFm2019/EFm2018 be used?

Response:

Only the power sector in the MEIC model has monthly variability in EF2019/EF2018, mainly caused by the improvement of air pollution control efficiencies (e.g., via installing new devices). Since the change in emission factors may not occur in the same month of different years, we use the ratio of annual average emission factors to represent the continuous improvement in the air pollution control of power plants. For the other emission sources, we do not have monthly variability in the EF2019/EF2018.

Page 4, line 106-108, could you explain "assumption of no change" to "predict the 2019-to-2020 change"? Just curious, do you have estimates in cooking sources? Should be higher in 2020 than 2019?

Response:

Yes, we have estimated the emissions from cooking sources, the activities of which have been estimated based on population and energy consumed for cooking per person. The food demand per person and the associated energy use for cooking are relatively constant for two consecutive years. Therefore, we assume that both of these two factors remain unchanged in 2019 and 2020, resulting in an assumption of no change to predict the 2019-to-2020 change in cooking activities and emissions from cooking sources.

Page 5, line 141-142, is it possible to separate the impacts from Chinese New Year and COVID lockdown? As mentioned in the manuscript, one happened in Feb 2019 and one in Jan 2020. It would be interesting to see the impacts from COVID lockdown only.

Response:

The COVID lockdown overlapped with the Chinese New Year in 2020. For example, the lockdown measures in Wuhan started two days before the Chinese New Year and lasted for two months covering the entire holiday of Chinese New Year, making it difficult to separate the impacts from the holiday and COVID lockdown on emissions.

Page 6, line 178, do you have estimates for aviation emissions?

Response:

No, the aviation emissions are not included in our estimates.

Figure 4, any explanations on higher industrial sources for CO, NMVOCs, and $PM_{2.5}$, in Jan 2020 than Jan 2019?

Response:

The higher industrial source emissions of CO, NMVOCs, and $PM_{2.5}$ are due to the larger industrial activities in Jan 2020 (especially before the lockdown) than in Jan 2019. The productions of iron, steel, and non-ferrous metals during January and February are 3.1%, 3.1%, and 2.2% higher in 2020 than those in 2019, which are the major driver of higher emissions of CO and $PM_{2.5}$. The productions of crude oil and petrochemical products such as ethylene during January and February are 3.7% and 5.6% higher in 2020 than those in 2019, which explains the higher NMVOCs emissions in Jan 2020.

Figure 5, see general comments.

Response:

Please refer to our responses to the general comments.