

## Response to Reviewer #1

Thank the reviewer for the insightful and detailed comments and suggestions, which helped to significantly improve the manuscript.

The reviewer's comments are shown in *blue italics* with the author responses in black.

*This study assimilates hourly air quality observations to provide detailed primary PM2.5 emissions inventory across China. Some insights about inter-annual, monthly and diurnal variation of the derived emissions, as well as impacts from COVID-19 lockdown are discussed. While the scope of this study is definitely within the scope of ACP, I have a serious reservation of publishing the manuscript at the present form. The main issue is about inadequate/insufficient discussion of the results and missing information of fundamentals which I will outline later. I am happy to re-review the revised manuscript, and will support the publication of this paper, if the following concerns can be adequately addressed.*

*Major points:*

*1) Assimilation approach regarding secondary PM2.5: it is hard for me to understand (Page 6-7) how "the impact of the secondary PM2.5 is ignored" during the assimilation. Secondary aerosol makes significant contributions (>50%) to observed PM2.5. This fact almost applies to all the hourly PM2.5 observations. Observations that are very remote and during less photochemically reactive periods may be less constituted by secondary aerosols, but I think these records make minor contributions to the air quality records this paper used which distribute mostly over eastern China. WRF-Chem simulation of PM2.5 includes important secondary species, which is fundamental to successfully capture PM2.5 spatiotemporal variations in China. So please further clarify:*

*What emission species are exactly optimized during the assimilation? Do you only constrain PM2.5 emissions and let PM2.5 precursors (SO<sub>2</sub>, NH<sub>3</sub>, NO<sub>x</sub>, VOC) to stay the same as the a priori? If so, how uncertain are the constrained PM2.5 emissions, if the a priori precursor emissions are incorrect and they make significant contributions to the observed PM2.5?*

*Lines 142 suggests that "hourly observed ... PM10, PM2.5, SO<sub>2</sub>, NO<sub>2</sub>, O<sub>3</sub>, and CO" are assimilated. So maybe these concentrations are used to also constrain the PM2.5*

*precursor emissions at the same time? If so, you should also briefly present the results of optimized precursor emissions, and how they affect the constraints on PM<sub>2.5</sub> emissions.*

*Overall, I do not understand how successful constraints on PM<sub>2.5</sub> emissions can be achieved with "ignoring secondary aerosol".*

Thank the reviewer for the valuable comments and suggestions. We agree with the reviewer that secondary aerosol is important to observed PM<sub>2.5</sub> and PM<sub>2.5</sub> precursors (SO<sub>2</sub>, NH<sub>3</sub>, NO<sub>x</sub>, VOC) play crucial roles in the formation of secondary aerosol. The WRF-Chem model has good performances for simulating the spatiotemporal variations of PM<sub>2.5</sub> over China, due to the model advances, especially with the improved description of the secondary formation mechanisms of aerosols. We did not clearly describe the assimilation process related with the secondary formation. As the reviewer commented, the aerosol secondary formation is captured by the WRF-Chem model, while the PM<sub>2.5</sub>, O<sub>3</sub>, NH<sub>3</sub>, and PM<sub>2.5</sub> precursors that have observations (SO<sub>2</sub> and NO), are updated by the observed quantities, respectively, but the VOC that are also PM<sub>2.5</sub> precursors are not updated due to the lack of direct and limited observations.

The detailed updated chemical variables and emission species follow Peng et al. (2018). Six conventional air pollutant observations (i.e., PM<sub>10</sub>, PM<sub>2.5</sub>, SO<sub>2</sub>, NO<sub>2</sub>, O<sub>3</sub>, and CO) are obtained from the Ministry of Ecology and Environment of China. The concentrations and emissions related to the six observations are constrained by the observed quantities. PM<sub>2.5</sub> observations are used to constrain the mass concentrations of P<sub>2.5</sub> (the fine unspciated aerosol contributions), S (sulfate), OC<sub>1</sub> (hydrophobic organic carbon), OC<sub>2</sub> (hydrophilic organic carbon), BC<sub>1</sub> (hydrophobic black carbon), BC<sub>2</sub> (hydrophilic black carbon), D<sub>1</sub> (dusts with effective radii of 0.5 μm), D<sub>2</sub> (dusts with effective radii of 1.4 μm), S<sub>1</sub> (sea salts with effective radii of 0.3 μm) and S<sub>2</sub> (sea salts with effective radii of 1.0 μm). Besides, the emissions of the unspciated primary sources of PM<sub>2.5</sub>, sulfate, nitrate and NH<sub>3</sub> are also updated. PM<sub>10-2.5</sub> observations (the differences between the PM<sub>10</sub> observations and the PM<sub>2.5</sub> observations) are used to constrain the mass concentrations of P<sub>10</sub> (the coarse-mode unspciated aerosol contributions), D<sub>3</sub> (dusts with effective radii of 2.4 μm), D<sub>4</sub> (dusts with effective radii of 4.5 μm), D<sub>5</sub>, S<sub>3</sub> (sea salts with effective radii of 3.25 μm), S<sub>4</sub> and the emission of PM<sub>10</sub>. SO<sub>2</sub> observations are used to constrain the SO<sub>2</sub> concentrations and emissions.

NO<sub>2</sub> observations are used to update the NO and NO<sub>2</sub> concentrations and NO emissions. CO observations are used to constrain the CO concentrations and emissions, while O<sub>3</sub> observations are used to constrain the O<sub>3</sub> concentrations.

To clarify the details of the assimilation process and the treatment for the secondary formation, the text in Lines 171-177, Page 7, is added. The details of the updated chemical and emission species follow Peng et al. (2018), which is not include in the text.

“Since the secondary formation process can be captured by the WRF-Chem model, the impact of the secondary PM<sub>2.5</sub> is indirectly considered. The detailed updated state variables with the according observations follow Peng et al. (2018). The concentrations and emissions of PM<sub>2.5</sub>, NH<sub>3</sub>, and PM<sub>2.5</sub> precursors that have observations (SO<sub>2</sub> and NO), are updated by the observed quantities, respectively, but the VOC that are also PM<sub>2.5</sub> precursors are not updated due to the lack of direct and limited observations.”

*2) A missing piece of information is showing the improvement in model simulation/prediction after the assimilation? How is the agreement of simulated PM<sub>2.5</sub> vs. observations improved after the assimilation? If you also constrained precursor emissions, comparison vs. observed air quality species other than PM<sub>2.5</sub> should also be provided.*

*Another suggestion is to compare your results vs. the updated MEIC that has extended to more recent years (not just 2016). This discussion is especially necessary considering that MEIC contains detailed bottom-up information. Differences of the derived inter-annual and inter-month variations of emissions vs. MEIC will be indicative where and when MEIC might be unrepresentative and why.*

Thank the reviewer for the insightful suggestions. In this study, the hourly forecasts of PM<sub>2.5</sub> concentration from the cycling assimilation experiment matched the independent observed quantities well, as shown by Figure 1. This indicates that PM<sub>2.5</sub> simulations are improved by the assimilation. Moreover, our previous work demonstrated that simulations forced by the posterior emissions could produce smaller biases and errors for PM<sub>2.5</sub>, SO<sub>2</sub>, and NO<sub>2</sub> than those with a priori emissions, when independent verifications against the observed quantities (Peng et al., 2020). This discussion is added in the text in Lines 139-141, Page 6.

The manuscript is updated by extending the comparison to MEIC from 2016 to 2020. For years 2016-2020, the PM<sub>2.5</sub> emissions from MEIC are 8.10, 7.60, 6.70, 6.38 and 6.04 Tg, respectively. The PM<sub>2.5</sub> emissions from DEPE are 8.17, 7.91, 7.53, 7.13 and 6.89 Tg, respectively. However, MEIC focuses on the contributions of the anthropogenic activities, but does not consider the contributions of biomass burning emissions. Meanwhile, the DEPE has the posterior PM<sub>2.5</sub> emissions updated by the EnKS with both contributions from the anthropogenic and biomass burning emissions. The mean annual PM<sub>2.5</sub> emission from biomass burning in China (2003~2017) is 0.51 Tg (Yin et al., 2019). Thus to fairly compare with the MEIC, we subtract the contributions of biomass burning emissions from the dynamics-based estimates of PM<sub>2.5</sub> emissions, which gives posterior PM<sub>2.5</sub> emissions of 7.66, 7.40, 7.02, 6.62 and 6.38 Tg for years 2016-2020, respectively. The text and figures are all updated by the extended comparisons with the MEIC from years 2016-2020.

Yin, L., Du, P., Zhang, M., Liu, M., Xu, T., and Song, Y.: Estimation of emissions from biomass burning in China (2003–2017) based on MODIS fire radiative energy data, *Biogeosciences*, 16, 1629–1640, <https://doi.org/10.5194/bg-16-1629-2019>, 2019.

*3) Section 5: This section attributes the difference of 2020 emissions vs. the previous years to the COVID-19 lockdown. However, the 2020 emission vs. 2019 is not entirely stronger than the difference between other neighboring years (e.g., Figure 3 and Table 1). So how much of the 2020-2019 emission difference can also be contributed by continuous environmental policies (as discussed in Section 3)? Overall, Figure 11 does not provide continuous signal of lockdown either, as some provinces show temporary increases at certain phases. The authors discuss New Year fireworks. But how can they only occur in certain provinces (and not occurring during the first several days of New Year)? Overall, the attribution of 2020-2019 emission difference to COVID-19 lockdown and the relevant discussions about temporal changes of these differences are weak.*

Thank the reviewer for the valuable comment.

We agree with the reviewer that the 2020 emission vs. 2019 is not entirely stronger than the differences between other neighboring years. As shown by Figure 3, the lockdown impact is mainly shown in February 2020. The emissions at the following months of February 2020 are similar to those of 2019, due to the epidemic prevention and control

policies enforced by the China government. This discussion is added in the text in Lines 338-340, Page 12.

The current EnKS, as a top-down technique, is unable to provide dynamics-based PM<sub>2.5</sub> emission estimates for different sectors and contributions from different policies, although the bottom-up technique has the potential to untangle the different contributions from different policies and quantify the different impacts on different sectors. One future work is to integrate the top-down technique with the bottom-up one, by which the emission estimates for different sectors and policies could be quantified. This discussion is added in the section of discussion in Lines 380-385, Page 14.

We agree with the reviewer that it is hard to see continuous and consistent signal of lockdown for the whole China, as shown by the original Figure 11 (new Figure 12). It is very likely that the inhomogeneous spatial variations of PM<sub>2.5</sub> emissions possibly relate with different traditions and policy enforcements for different provinces. As the third reviewer pointed out, the increased PM<sub>2.5</sub> emissions for BTH and SCR are not resulted from the firework, but possibly led by the long national vacation of spring holiday of year 2019 (Ji et al., 2018). This discussion is added in the text in Lines 356-358, 360-361, Page 13.

*Specific comments:*

*1) Abstract: key quantitative results should be presented. The current form of abstract is too qualitative and less informative. Line 24-27 reads redundant and irrelevant, and is suggested to be replaced with a more concise sentence stating the significance of these results.*

Thank the reviewer for the valuable comment. The abstract is modified to emphasize the findings and significance of the study. The updated abstract is:

“Timely, continuous, and dynamics-based estimates of PM<sub>2.5</sub> emissions with a high temporal resolution can be objectively and optimally obtained by assimilating observed surface PM<sub>2.5</sub> concentrations using flow-dependent error statistics. The annual dynamics-based estimates of PM<sub>2.5</sub> emission averaged over mainland China for years 2016-2020 without biomass burning emissions are 7.66, 7.40, 7.02, 6.62 and 6.38 Tg, respectively, which are very closed to the values of MEIC. Annual PM<sub>2.5</sub> emissions in China have consistently decreased of approximately 3% to 5% from 2017 to 2020.

Significant PM<sub>2.5</sub> emission reductions occurred frequently in regions with large PM<sub>2.5</sub> emissions. COVID-19 could cause a significant reduction of PM<sub>2.5</sub> emissions in the north China plain and northeast of China in 2020. The magnitudes of PM<sub>2.5</sub> emissions were greater in the winter than in the summer. PM<sub>2.5</sub> emissions show an obvious diurnal variation that varies significantly with the season and urban population. Compared to the diurnal variations of PM<sub>2.5</sub> emission fractions estimated based on diurnal variation profiles from US and EU, the estimated PM<sub>2.5</sub> emission fractions are 1.25% larger during the evening, the morning peak is 0.57% smaller in winter and 1.05% larger in summer, and the evening peak is 0.83% smaller. Improved representations of PM<sub>2.5</sub> emissions across time scales can benefit emission inventory, regulation policy and emission trading schemes, particularly for especially for high temporal resolution air quality forecasting and policy response to severe haze pollutions or rare human events with significant socioeconomic impacts.”

*2) Why are observations before 2016 not used in the assimilation?*

The experimental period is from 2016 to 2020. We did not start the experiment earlier than 2016, due to the lack of concentration observations.

*3) Line 154: what is the spatial autocorrelation before and after the selection of stations?*

The spatial autocorrelation of the thinning of observations is close to the original observations. We have added this sentence in Line 155-156, Page 6.

*4) Line 196: What are the "weather effects" referring to?*

Thank the reviewer for pointing out this sentence. We realized that PM<sub>2.5</sub> emission is not related to the weather conditions, liking PM<sub>2.5</sub> concentrations. So we have moved this sentence in Line 220, Page 8.

*5) Figure 3b: the winter seasons show a sharp change from increases in 2017 to decreases in 2018. Is it related to the coal ban for residential heating since the 2017-2018 winter?*

The winter seasons demonstrate a sharp change from increases in 2017 to decreases in 2018. It is related to the coal ban for residential heating since the 2017-2018 winter. In 2017, spatial distributions of the changes of PM<sub>2.5</sub> emissions in December compared to November show obvious increases in most China. However, the changes in 2018 show

significant decreases in areas of Beijing, Tianjin, Hebei, Shanxi, Henan and Anhui provinces. This discussion is added in the text at 236-242, Page 9:

“The monthly DEPE also demonstrates the effectiveness of strict implementations of emission reduction policies in China, such as the coal ban for residential heating since the 2017-2018 winter. There was a sharp change of PM<sub>2.5</sub> emission, from increase in 2017 to decrease in 2018. As shown by Figure 6, spatial distributions of the changes of PM<sub>2.5</sub> emissions in December compared to November in 2017 show obvious increases in most China. However, the changes in 2018 show significant decreases in areas of Beijing, Tianjin, Hebei, Shanxi, Henan and Anhui provinces due to the implementation of the coal ban.”

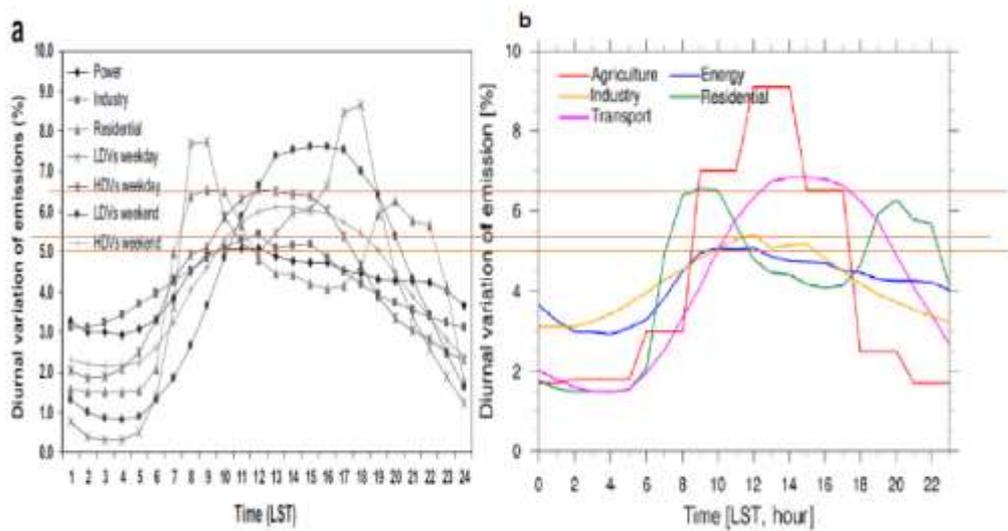
*6) Line 215-216: As I understand, the centralized heating system in North China has a fixed date of turning-on and turning-off during each heating season. So a sudden drop of emissions from March to April looks reasonable to me. Do you suggest that the turning-off date is variable in different places to smooth-out the differences, or residential heating does not contribute that much to the total emissions variations between these two months?*

We agree with the reviewer that the centralized heating system in North China has a fixed date of turning-on and turning-off during each heating season. Therefore, a sudden raise of emissions from October to November and a sudden drop of emissions from March to April is reasonable. But the turning-on and turning-off date are variable in different places, which help to smooth-out the differences. This discussion is added in the text at 248-252, Page 9:

“The centralized heating system in North China has a fixed date of turning-on and turning-off during each heating season. Therefore, a sudden raise of emissions from October to November and a sudden drop of emissions from March to April are shown. But the turning-on and turning-off date are variable in different regions, which imposes a smoothing impact on the emissions.”

*7) Section 4: some recent bottom-up developments have more details about diurnal emission variations (e.g., Du et al., 10.5194/acp-20-2839-2020, 2020, Figure 1). Discussion about comparison of your results vs. these recent diurnal profiles can be insightful.*

Thank the reviewer for the thoughtful suggestion. As shown in Figure R1, the diurnal variation profiles for the power, industry, residential in Du et al. (2020) are the same as in Wang et al. (2010). However, the diurnal variation for transportation in Wang et al. (2010) are divided into four subcategories: light duty vehicles emissions in weekday, light duty vehicles emissions in weekend, heavy duty vehicles in weekday and heavy duty vehicles in weekend, which is more carefully than those in Du et al. (2020). So we did not compare the diurnal variation in Du et al. (2020), but we added this reference in Line 316, Page 12.



**Figure R1.** Diurnal variations of emissions from the individual sectors from (a) Wang et al., 2010; (b) Du et al., 2020.

8) Line 310: missing words here.

Thank the reviewer for pointing out the mistake. We have revised the text in Line 362, Page 13.