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.

The authors made necessary revisions that address many of my concerns in the first round of review. I will outline several remaining comments below for the authors and the editor to consider, and I support the publication of the paper if they can be addressed.

1) Line 174: what observations are used to constrain NH3?

Thank the reviewer for the thoughtful comment. $PM_{2.5}$ observations are used to constrain NH_3. This sentence is revised as (Lines 174-177):

"The concentrations and emissions of $PM_{2.5}$ and $PM_{2.5}$ precursors (SO_2 and NO) that have observations are updated by the observed quantities, respectively. Besides, NH_3 concentrations and emissions are constrained by $PM_{2.5}$ observations, however, the VOC that are also $PM_{2.5}$ precursors are not updated due to the lack of direct and limited observations."

2) Section 5: My suggestion is this section can be cut or at least significantly shortened. The revised text reads to me that it is impossible to attribute how much of the derived PM2.5 emission changes between 2019 and 2020 are due to COVID. The absolute changes (Figure 10) are small. These changes are not strong enough relative to the changes in previous years (e.g., Table 1), so that COVID lockdown cannot be clearly attributed as a significant cause.

If the authors choose to keep this section in a shorter version, please discuss why the overall changes are so small relative to bottom-up studies (e.g., Zheng et al., 10.5194/essd-13-2895-2021, 2021). This might be true since COVID lockdown might increase residential emissions while reducing traffic emissions. But overall, the results provide little information towards a thorough explanation.

Thank the reviewer for the valuable and thoughtful comment. We agree with the reviewer that the changes between 2019 and 2020 are not strong enough relative to the changes in previous years for the whole year over the whole country (Figure 10, Table 1). However, although similar emission reductions and emission trends are obtained from the bottom-up technique (Zheng et al., 2021), the reduction amount and ratio from the bottom-up technique are larger than those estimated from DEPE (Figure 10 and Table 1). This is possibly due to significant reductions of $PM_{2.5}$ emission from the residential sector as in the bottom-up technique (Zheng et al., 2021), however, $PM_{2.5}$ emissions from the residential sector might not significantly changed around the COVID outbreak.

The abrupt changes of $PM_{2.5}$ emissions during the initial stage of COVID-19 in China provide a natural case study to validate the ability of the dynamic-based data assimilation method to obtain high temporal-resolution $PM_{2.5}$ emission estimates. Therefore, we discussed the assimilated results in detail in Section 5. We add this discussion in the text in Lines 334-336, 351-356.

3) Line 363-369: How would these factors affect PM2.5 emissions or the top-down inversion?

Thank the reviewer for the valuable comment. We delete this paragraph since it has less relation with the subject of the manuscript.

Response to Reviewer #3

We thank reviewer for his thoughtful comments and suggestions that have helped to significantly improve this manuscript.

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

Thanks for addressing my comments. The revised manuscript is overall well prepared. I have a couple of minor/technical comments.

1. Regarding the OSSE, did you run your model over the entire period and then substract the effect of boundary layer (or meteorology) over the entire period? Or, did you apply the result of the short-term period simulation (Jan 2016) to the entire period? If the former is the case, then it should be fine. But, the latter is the case, please specifically mention in your revised manuscript that the effect of meteorology is based on the short-term period simulation. This is because the effect of boundary layer or meteorology can be largely different depending on seasons or years.

Thank the reviewer for the thoughtful comment. The OSSE is performed from 0000 UTC 29 December to 0006 UTC 1 February 2016. The effect of boundary layer is roughly estimated based on the assimilation results from 1 to 31 January 2016. We agree with the reviewer that the influences of boundary layer could strongly vary with seasons or years and mentioned these in the revised versions in Lines 275, 285-286.

2. The caption of Fig. 6 is not very clear. I would recommend adding titles above Fig. 6a and 6b, and saying that Dec 2017 - Nov 2017, Dec 2018 - Nov 2017, respectively. Also, please revise the caption to clearly indicate "compared to what".

Thank the reviewer for the thoughtful comment. We have added titles above Fig. 6a and 6b. And we also revised the caption in Fig. 6.

3. Fig. 11. Description of (a) is missing.

Thank the reviewer for the valuable comment. We have revised the caption in Fig. 11.