The author analyzed the long-term trends of $PM_{2.5}$ chemical components and their drivers using numerical models. Some issues still need to be addressed although the author have made great efforts according to previous comments. I suggest major revision for the manuscript prior to be finally published in ACP.

1. In the introduction, the author spent lots of length to describe the air pollutants control measures for $SO_2$, $NO_x$ and $NH_3$ during different stages in the past decade. Since the aims of this manuscript is to evaluate the changes of SIA as responses to stringent measures, more results of SIA variations should be cited and summarized. Indeed, extensive researches have reported on this topic. Also, the author should compare the trends in $PM_{2.5}$ and components in this paper with previous studies.
2. Considering that the author divided 2000-2019 into three periods, e.g., period I (2000-2012), period II (2013-2016) and period III (2017-2019), annual trend used in the analysis might not be appropriate. Annual trend often refers to year-to-year variations. Measurements during three periods covered different seasons (winter, summer etc.) and sites (urban, suburban or rural), these actually influenced the conclusions because it's well known that more polluted air quality frequently occurred during wintertime in urban site. The author used $PM_{2.5}$ at a long-term monitoring site during 2012-2020 to verify the decreasing trend summarized from meta-analysis. I supposed that this evidence could only support that the decreasing trend was reliable. The quantitative results, e.g., decreased by 8.2% from period I to period III, was still to be evaluated. It is a bit confused that the author collected publications covering four-season measurements to summary the trends from period I to period II, however, only January was chosen to do simulations. The author explained that severe haze pollution often occurred in January. The effectiveness of precursors controlling measures could be season-dependent. That's another uncertainty for this study.
3. The author attributed all the variations of $PM_{2.5}$ and chemical components to changes of gaseous precursors resulting from control measures. The reasons were somewhat pale and inadequate. In fact, the responses of SIA to precursors were complex and sometimes non-linear. Previous studies have concluded that enhanced atmospheric oxidation capacity, faster deposition of total inorganic nitrate and the changes of atmospheric circulation could be possible drivers. That's likely why $PM_{2.5}$ showed no significant trend from period I to period II despite control measures were implemented since 2013. Please cite more relevant publications, and then rephrase and expand the related explanations throughout the study. In current revised manuscript, the results and discussion were flat.
4. For model simulations, the author fixed meteorology in 2020 to exclude the impacts of meteorology. Thus, the CMAQ simulation before and during the COVID-lockdown didn't represent the actual results during these periods. In Figure S6, the author compared the CMAQ results with ground observations for $PM_{2.5}$, $SO_2$ and $NO_2$. This is not reasonable. The author should firstly do simulations using real

meteorology to evaluate the performance of CMAQ model, and then do controlled experiments using fix meteorology. Also for Figure S3-S5, the author only assessed the simulation results in January 2010 with observations. Indeed, they should do these year by year using real modelling results.

5. The author compared the model results between 50% reductions in $NH_3$ emissions and 50% reductions in acid gases, concluding that reducing acid gases is more effective. Did the author do sensitivity cases with reductions of 50% $NH_3$ and 50% acid gases, which might be more close to the facts. Another issue is quantifying how much the precursors should be decreased to fulfill air quality targets.

6. In Figure 4, all measurements were averaged to derive the two pie charts. The author only filtered the data for meta-analysis using the measurements at sites that include both $PM_{2.5}$ and SIA, we noticed that many of these re-filtered measurements didn't include mental species ($Na^+$, $Mg^{2+}$, $Ca^{2+}$, $F^-$), which called "Other", accounting for 36.8-37.4% during non-haze and haze days. The inconsistency among measurements used for averaging species caused large uncertainties to the conclusions. Studies simultaneously measured all species could be more reliable and scientific, at less for the pie charts.

7. The author concluded that increased SIA formation is the major driving factor for haze pollution, which was obviously true consistent with previous studies. Due to the limitations of collecting datasets from publications instead of long-term filed measurements, the contribution of SIA slightly increased from 36% during non-haze days to 40% during haze days. The concentrations of SIA and other $PM_{2.5}$ components synchronously increased from non-haze to haze days. Thus, it is not appropriate and convincing to draw this conclusion solely based on this study.

8. The first reviewer mentioned that the results in Figure 2a and b,c,d crossed several pages, and the interruption makes it hard to read. In the response, the author only added more detail figure caption to Figure 2. Indeed, the reviewer suggested to recombine the figures, rephrase the sentences or rearrange the paragraphs, making them more coherent in the context.

9. In Figure 7, S3, S4 and S7, the south China Sea were missed in maps. This is really less rigorous.

10. The author added more citations in the revised manuscript, which were not shown in the Reference.