Review of 'Temporally-resolved sectoral and regional contributions to air pollution in Beijing: Informing short-term emission controls'

General comments

This paper analyses the impact of various short-term emission controls on $PM_{2.5}$ concentrations in Beijing. Various aspects are analysed in multiple model experiments, including the timing of the emission control, the area of emissions control, the emissions sector that is controlled, and interactions between different controls. This is a strong paper containing a great deal of valuable analysis, and valuable insights into air pollution episode control policies. However, a more detailed description of the methods is needed to be able to understand whether the results have been interpreted correctly.

Specific comments

- In this paper you refer to the evaluation of your model setup in a previous paper. However, I think it is necessary to include the results of evaluation of these simulations against measurements within this paper. In some cases, in Ansari et al. (2019), the model showed biases for key pollutants. While this by no means invalidates the results of this study, the reader of this paper should be made aware of the biases in the model (and their direction), possible reasons for these issues and how they could affect the interpretation of your results. It would be particularly useful to know whether the model estimated the magnitude of the episodes correctly, which you could show by adding measurement data to Figure 1.
- On page 3, the sentence beginning on line 18 details the two phases of APEC emissions controls. Please cite the source of your information on the controls here.
- There is no mention of whether there was any spinup for the model runs. If there was no spinup, the PM concentration reductions achieved by each day of emissions cuts may be unrealistic. If the baseline run covers the 14-day period (should be specified), and there is no spinup for the 5 day runs, then emissions reductions may be overestimated. For example, in 'Run No. 10' the first day would be expected to have lower PM concentrations compared with baseline day 10 anyway due to it having a 'cold start.' Please specify whether spinups were performed, if 5 day runs were initialised with fields from the baseline, or whether they were cold starts.
- If I have interpreted it correctly, to make Figure 3 you calculate the difference between the $PM_{2.5}$ concentrations in the baseline run, and in the runs with a day of reduced emissions. So the height of the stacked line shows the sum of the reductions in PM made by implementing the control in each individual run. However, since the graph appears to be a time series of PM, the first impression on seeing this figure is that you portioned the total PM by the day on which it was emitted. However, due to the non-linearity of the relationship between PM (and its precursors) emission volume and PM concentrations, and due to the lingering and transport of PM, the concentration reductions sum do not account for the total PM. I.e. on the 24th, the sum of reductions is around 120 ug/m3, whereas Figure 1 shows the daily mean was over 350 ug/m3. I suggest the figure should be adjusted so it is clearer that it represents the concentration reduction as a result of emissions reductions for each day. You could do this changing the y axis to 'concentration reduction,' which would flip the graph horizontally. Another issue is that Figure 3 suggest that these would be the reductions achieved by a combination of emission reductions on those days (while the simulations are actually separate so will not simulate any synergistic effects). A better way to represent this, while making your experimental design clearer, could be to us e a format similar to Figure S1, with each emission reduction shown separately.
- It would be helpful to define how you calculate the integrated contribution so that the unit of '$\mu g\ m^{-3}h$' (should it be '$\mu g\ m^{-3}h^{-1}$'?) can be understood.

- Multiple source apportionment studies suggest that agriculture and biomass burning are major contributors to $PM_{2.5}$-caused mortality in China, with similar contributions to transport and power generation sectors. It should be specified whether additional run that estimates the background contribution includes these sectors, and whether their emissions are included in the model at all.
- It would be useful to include a breakdown of emission volumes and spatial pattern by sector so that the concentrations reductions can be put in context. For example, controls on the power sector seem to produce a relatively small PM decrease, but is this due to this sector comprising a relatively small proportion of emissions, the composition of species emitted from power sector, or the spatial distribution of emissions?
- Please could you comment on the sources of the 'background' levels of $PM_{2.5}$ (i.e. natural + outside domain). This is especially relevant for understanding the implications of the sentence on P15L10, where you report that even with no emissions, $PM_{2.5}$ concentrations would be 79 ug m^{-3} under these weather conditions.

Technical Corrections

P1L1 – 'to air pollution' rather than 'to air quality' sounds better in this sentence and is consistent with the title.

For your date axes on Figures – sometimes they look a bit crowded (such as figure 7). I suggest changing the axis title to 'day in October' and removing the '/10' from the tick labels.

Figure 11 - missing subplot labels. Perhaps the subplots in this figure could be merged into one large figure which would make it easier to visually compare the runs.