

Interactive comment on “Impacts of short-term mitigation measures on PM_{2.5} and radiative effects: a case study from a regional background site near Beijing, China” by Qiyuan Wang et al.

Anonymous Referee #2

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General comments

This manuscript attempts to examine the impacts of emission reduction on PM_{2.5} and radiative effects (surface DRF as the authors defined) using field measurements and WRF-Chem simulations at a regional background station in the Beijing-Tianjin-Hebei region in China. The impacts are examined by comparing the changes in observation-derived speciated PM_{2.5} concentrations and DRF during and after an emission-controlled period. The paper is reasonably written and results are reasonably presented, and it can be accept for publishing with revisions that address the following issues.

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A major weakness in the study is that, as the main objective is to investigate the impacts of emission reduction measure on PM_{2.5} and DRF (emphasized in the title and abstract), this paper has a major flaw in separating the effects of emission reduction and meteorological conditions. Although the authors make an effort to make comparisons between the during- and-post-control periods under stable meteorological conditions, the determination of the “stable” conditions is quite rough, and it is not clear how similar the meteorological conditions are for the days selected for the comparison (even under stable conditions, the degree of the stability would significantly affect air quality). To separate these two factors, I would suggest the authors to do a more thorough analysis of the meteorological conditions, or ideally, based on the information they have and/or can obtain, construct an emission reduction scenario for the NCCPC control period and conduct additional WRF-Chem simulations and analysis.

Another issue is about the source apportionment in Section 3.2 using PMF. The authors assign the third source factor to secondary inorganic aerosols (SIA). This is not appropriate, since SIA is not an emission source, and it may have contributions from other sources they identify, such as coal combustion, mobile, industry, and biomass burning, i.e., SIA is not independent to other four identified anthropogenic emissions sources.

Specific comments

1. Page 8, line 13. It is better to show the regression results, and specify the values of a and b used.
2. Page 9, lines 52-54. Small changes in sulfates may also be attributed to small changes in SO₂ emissions during the campaign.
3. Figures 4 and 5. Copy the source legend from Fig 5 to Fig 4.
4. Page 10, lines 64 -72. First, as pointed earlier, the approach to determine the “stable conditions” is rough. Second, the samples (3 days and 2 days) for the stable conditions

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are too small, which would make the comparison statistically no meaningful. A better analysis is needed to separate the impacts of emission reduction and meteorological conditions.

5. Page 15, lines 42-43. It is surprising that with an averaged surface concentration of 6.0ug/m³, EC imposes the largest cooling effects in surface DRF during the non-control period and several factors higher than that of OM, while the light extinction by OM is much higher than by EC. An explanation would be helpful.

6. Fig 8 seems too small and a little bit complicated, which make it difficult to the reader to understand the effects of meteorological conditions on air quality in the BTH area. In addition, the location of the Xianghe site should be specified in the figure. Similar figure for October 12-23 might also be needed when you do analysis in decomposing the influences of the emission reduction and meteorology (especially for the five “stable” days).

Technical

The language need to be polished. The authors need go through the manuscript carefully and make edits. Following are just a few pickups.

Page 2 line 35, page 15 line 46, page 17 line 82: change “would” to “should”?

Page 2 line 44, change “experienced” to “experiencing”?

Page 3 line 78, change “low-voltage” to “low-pressure”.

Page 14 line 88, “genesis”?

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