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Comment on acp-2021-428

Anonymous Referee #2

Referee comment on "Assessment of strict autumn-winter emission controls on air quality in the Beijing-Tianjin-Hebei region" by Gongda Lu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-428-RC2, 2021

This manuscript evaluates the impact of emission reduction policies in China on changes in winter PM2.5 concentrations. In particular, the authors evaluate the emission reduction and PM2.5 concentration change in the BTH area during 2017AW using the chemical transport model and various observational data. I expect that such an attempt will greatly help policymakers assess the impact of policy implementation not only in China but also in many polluted regions. However, in order for this manuscript to be published on ACP, various issues must be resolved. Some detailed comments are below:

- Lines 27-28: The observed PM2.5 concentration was significantly reduced compared to the expected target values (15%). It seems better to emphasize this part in comparison with observations rather than models.

- Lines 30-31: It is difficult to easily determine the effect of the emission reduction policy and the inter-annual variability of the meteorological field, respectively. Among the PM2.5 concentration reductions derived from the model (20%), if the effect of the emission reduction policy is 8%, does the remaining 12% mean the effect of the meteorological field?

- Lines 65-76: If MEE implemented a strong emission reduction policy during 2017AW, is there any data on the amount of emission reduction estimated by MEE? If any, it should be compared and discussed with the emission reductions assumed in this study.

- Lines 89-90: Authors should address the biases of bottom-up emissions inventories in more detail in the introduction part.

- Lines 108 – 120: It is recommended to organize the station information in a table and mention only those that require detailed explanation.

- Line 168: Different from the values â□□â□□shown in Figure 3. Are the emission control and target areas different?

- Line 199: Is emission reduction necessary during the two-month spin-up period before the reduction policy is implemented? Emissions during the spin-up period should also be mentioned.

- Lines 211-213: The authors scaled up MEIC emissions from observations and models for the 2016 AW period. However, the authors do not specifically mention the criteria for increasing NO_x and CO emissions by 1.5 and 2.4 times, respectively (Line 203 does not provide such information). Although the authors uniformly increased NO_x and CO, do the differences in model and observation appear uniformly across the entire domain? Limitations on this should be mentioned. Moreover, although SO_2 concentrations would be underestimated in most regions, the increase in emissions was applied to only 7 model grids. Also, how is the number 2.1-6.8 times calculated?

- Lines 236, 244: The model underestimates despite the increase in emissions. The authors mention the positive bias of the monitoring network as one of the causes. Although there is a bias of two points compared to APHH in Fig. 2, it is not clear whether the value can represent the bias in the entire domain. Even SO_2 is inconsistent. In addition, APHH and both observation points are located in urban, so they are greatly affected by mobile sources. Therefore, we cannot be sure that the difference between APHH and the two points represents the bias of CNEMN and BJENM.

- Lines 285-298: Are 2017AW emissions scaled up in the same way as 2016AW? It is not clearly described in the manuscript.

- Line 289 and Lines 315-319: The authors scaled up emissions outside of BTH, but did not change emissions outside of the area shown in Figure 3. Is there any clear reason for that? Are MEIC emissions underestimated only in BTH and its vicinity but are assumed to be similar elsewhere? As the authors mentioned in the manuscript, $PM_{2.5}$ affects different regions through long-range transport, so the impact of fixed emissions outside the domain should be mentioned.

- Line 340: Since the influence of the meteorological field is also an important part of this study, it is recommended to present the changed PM2.5 concentration field as a 2-D map due to the interannual variability of the meteorological field. This will allow us to evaluate in more detail the impact of interannual variability in regional meteorological fields.

- Lines 346-348: The authors should highlight the significant differences between Zhang et al. (2021) and this study.

- Line 345: Zhang et al. (2019) evaluated the difference between the meteorological fields in December 2017 and December 2016. Therefore, the authors should also compare for the same period (December). This is because, even in winter, the meteorological field can have large fluctuations from month to month.

- Line 364: "PM2.5 in BTH decreased by 28% from 103 μg m-3 to 75 μg m-3 in the control period relative to the previous year". However, in the abstract, it is described as "PM2.5 in BTH in autumn-winter 2017/2018 relative to the previous year is 27%, declining from 103 to 75 μg m-3". The same value should be used for the same content.

- Lines 368-370: The authors used observations and models to evaluate emission reductions in the BTH region. However, there may be large errors in the scale-up assumed by the authors. Although there are difficulties to be derived only with limited data, the authors should evaluate the reliability of the presented numbers and make important comments about the effect of uncertainty.

- Line 371: How 8% is derived should be presented in more detail in Session 4.

- Line 737: Does "n" mean NMB?