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Comment on acp-2021-428

Anonymous Referee #1

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-RC1, 2021

The manuscript evaluated emission controls on air quality in the BTH region with observation products and CTM simulations. The topic is of great importance to environmental policymakers. However, several issues should be addressed properly in the manuscript for publication in ACP.

General comments for the modification

- The objectives and motivations do not seem clear in the manuscript (e.g., Estimating the emissions reduction for mitigation measures, reproducing the AW2017 case, or determining contributions of parameters to the air quality). The authors had better make your objectives and motivations articulate and explicit in the manuscript. The manuscript is also lacking in the implication of this work to report to the readers or scientific community. Accordingly, the results should be are congruent with the objectives and the implication of the study.
- Second, validation is crucial for evaluating the effects of emission controls on air quality. The manuscript did not discuss the validation of NO_x, SO_2, CO precursors for the AW2017 simulation, although there are some comparisons of particulate matters. Thus, the authors need to compare the simulated gaseous species with observations (e.g., in-situ ground or satellite data).
- Lastly, the authors had better reorganize the manuscript to strengthen the methodology (i.e., Adding a method section).

Specific comments for the modification

- Lines 142-144: The manuscript did not mention what was utilized for NO_2 observation during the APHH campaign. Was it different with the Chemiluminescence detection system? If both measurements are not based on the same principle, the differences can be caused by instrumental sensitivity (as the authors mentioned). However, the different local sources at both network sites are also an important issue that cannot be ignored. There is ~3 km distance between them. The authors need to discuss it.

- Lines 210-213: It is an important part of the methodology. The authors used scale factors of 1.5 for NO_x, 2.4 for CO, and 2.1-6.8 for SO_2 to conduct the CTM simulation for the AW2016 case. Were the spatially same factor applied? Also, no matter which (ground or satellite) observation data is used for the emission estimations, there are two crucial issues of i) nonlinearity between emissions and concentration of a species (e.g., NO_2) and ii) transfer between adjacent grid cells in the calculation. The authors need to clarify how the scale factors are derived (i.e., procedure). Furthermore, in particular, for the scale factor of NO_x, the authors need to explain how to treat the relation between observed NO_2 and the NO_x emissions (usually emitted as NO).
- Lines 219 -222: It is well known that CO is a final product of NMVOC oxidations in many textbooks. So, it is not easy to agree that modeled CO is relatively unaffected by NMVOC emissions. The authors need to explain some reasons in the manuscript in terms of the lifetime of NMVOCs and their chemical evolution during long-range transport. The enhanced levels of CO would occur in other remote areas other than BTH regions through long-range transport.
- Lines 231-235: I think there is a more important reason for the inconsistency. That is interference (e.g., HNO_3 and PANs) in the NO_2 chemiluminescence detection instrument equipped with a molybdenum converter, which converts NO_2 to NO. Here, the molybdenum converter also oxides NO_z (≈ HNO_3 + PANs) to NO under typically operational temperature 300 – 350 °C (refer to Winer et al., 1974 and Dunlea et al., 2007). Dunlea et al. reported the interference in the chemiluminescence detection accounting for up to 50% of ambient NO_2 Considering this issue, the correlation between the simulated and observed NO_2 would be better. In other words, the data points of NO_2 in Fig. 4 would shift to the left, and the intercept would decrease. The authors had better discuss and/or reanalyze it.
- Lines 242 -243 & Figure 4: Although the scale factors of 2-7 were applied to grid cells somewhere (which was not specified in the manuscript, but probably around Shanxi province) in the MEIC SO_2 emission, the SO_2 concentrations were still significantly under-predicted. The under-predicted SO_2 concentrations can influence SO_2 and PM_5 in the BTH areas via the atmospheric chemical and physical processes (e.g., secondary aerosol formation and the transport to the BTH) because SO_2 has ~ 5 days lifetime. Accordingly, the estimation of the emission changes for the AW2017 simulation is probably hampered by low simulated SO_2. The authors had better discuss how to treat this issue in your estimate. Also, the authors need to present the results for the AW2017 case, similar to Fig. 4.
- Lines 270-272 and Fig. 5: It is not easy to agree that the errors in the boundary layer dynamics are related to the overestimation of nitrate alone. The issue should also apply to sulfate and others. Therefore, the errors in the boundary layer would not be the main reason for the overestimation. It is reasonable to discuss the overestimation of nitrate in terms of understanding like a relationship between SO_2 and sulfate (as the authors mentioned). However, as shown in Figs. 4 and 5, the modeled NO_2 concentration (a precursor of nitrate) is underestimated while nitrate is overestimated. It is a logical contradiction. Thus, the authors need to re-examine the overestimation of nitrate, considering the 4^{th} comment pointed out by this reviewer.
- Lines 285 – 298: The authors need to discuss a clear description of how to estimate the emissions fluxes for AW2017. It is also required to explain how to treat the nonlinearity between emissions and concentration in the estimation.
- Lines 330-340: Zhang et al. (2010) mentioned "NH_3 emission varied greatly from city to city from HS1617 (AW2016 in this study) to HS1718 (AW2017). In some cities, NH_3 emissions were largely reduced, such as in Beijing (6.4%), Taiyuan (33%), and Zhengzhou (19.6%), while the NH_3 emissions showed increases in some other cities, such as Tianjin (5.0%), Shijiazhuang (0.2%) and Jinan (35.2%)". These variations are

not marginal. Also, some studies reported that the SO_2 and NO_2 emissions have a decreasing trend while atmospheric NH_3 experienced a significant increasing trend (Xia et al., 2016; Ge et al., 2019). If NH_3 emissions increase in your simulation for the AW2017 case, what change would be expected in the concentration of $PM_{2.5}$?

Minor comments for the modification

- 1: Provide information on the number of data in Figure 1.
- Line 142: "<10%". Clarify it, as for example, 0-10%, ~10%, or ~%.
- The authors mentioned several grid points, for example, "seven grid squares" (Lines 213), "2 grid points" (Line 242), "13 grids" (Line 305), and "14 model grids" (Line 318). Clarify or leave out because readers cannot find out such information in the manuscript.

References

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