

Atmos. Chem. Phys. Discuss., referee comment RC1  
<https://doi.org/10.5194/acp-2021-428-RC1>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.



## 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 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  $\text{NO}_x$ ,  $\text{SO}_2$ , 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  $\text{NO}_2$  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  $\sim 3$  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  $\text{NO}_x$ , 2.4 for  $\text{CO}$ , and 2.1-6.8 for  $\text{SO}_2$  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.,  $\text{NO}_2$ ) 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  $\text{NO}_x$ , the authors need to explain how to treat the relation between observed  $\text{NO}_2$  and the  $\text{NO}_x$  emissions (usually emitted as  $\text{NO}$ ).
- Lines 219 -222: It is well known that  $\text{CO}$  is a final product of NMVOC oxidations in many textbooks. So, it is not easy to agree that modeled  $\text{CO}$  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  $\text{CO}$  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.,  $\text{HNO}_3$  and PANs) in the  $\text{NO}_2$  chemiluminescence detection instrument equipped with a molybdenum converter, which converts  $\text{NO}_2$  to  $\text{NO}$ . Here, the molybdenum converter also oxidizes  $\text{NO}_2$  ( $\approx \text{HNO}_3 + \text{PANs}$ ) to  $\text{NO}$  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  $\text{NO}_2$ . Considering this issue, the correlation between the simulated and observed  $\text{NO}_2$  would be better. In other words, the data points of  $\text{NO}_2$  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  $\text{SO}_2$  emission, the  $\text{SO}_2$  concentrations were still significantly under-predicted. The under-predicted  $\text{SO}_2$  concentrations can influence  $\text{SO}_2$  and  $\text{PM}_{2.5}$  in the BTH areas via the atmospheric chemical and physical processes (e.g., secondary aerosol formation and the transport to the BTH) because  $\text{SO}_2$  has  $\sim 5$  days lifetime. Accordingly, the estimation of the emission changes for the AW2017 simulation is probably hampered by low simulated  $\text{SO}_2$ . 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  $\text{SO}_2$  and sulfate (as the authors mentioned). However, as shown in Figs. 4 and 5, the modeled  $\text{NO}_2$  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<sup>th</sup> 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 “ $\text{NH}_3$  emission varied greatly from city to city from HS1617 (AW2016 in this study) to HS1718 (AW2017). In some cities,  $\text{NH}_3$  emissions were largely reduced, such as in Beijing (6.4%), Taiyuan (33%), and Zhengzhou (19.6%), while the  $\text{NH}_3$  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  $\text{SO}_2$  and  $\text{NO}_2$  emissions have a decreasing trend while atmospheric  $\text{NH}_3$  experienced a significant increasing trend (Xia et al., 2016; Ge et al., 2019). If  $\text{NH}_3$  emissions increase in your simulation for the AW2017 case, what change would be expected in the concentration of  $\text{PM}_{2.5}$ ?

## 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

Dunlea, E. J., Herndon, S. C., Nelson, D. D., Volkamer, R. M., San Martini, F., Sheehy, P. M., Zahniser, M. S., Shorter, J. H., Wormhoudt, J. C., Lamb, B. K., Allwine, E. J., Gaffney, J. S., Marley, N. A., Grutter, M., Marquez, C., Blanco, S., Cardenas, B., Retama, A., Ramos Villegas, C. R., Kolb, C. E., Molina, L. T., and Molina, M. J.: Evaluation of nitrogen dioxide chemiluminescence monitors in a polluted urban environment, *Atmos. Chem. Phys.*, 7, 2691–2704, <https://doi.org/10.5194/acp-7-2691-2007>, 2007.

Ge, B., Xu, X., et al.: Role of ammonia on the feedback between AWC and inorganic aerosol formation during heavy pollution in the North China Plain, *Earth and Space Science*, 6, 1675-1693, 2019.

Xia, Y. M., Zhao, Y., & Nielsen, C. P.: Benefits of China's efforts in gaseous pollutant control indicated by the bottom-up emissions and satellite observations 2000–2014. *Atmos. Environ.*, 136, 43–53, 2016.

Winer, A. M., Peters, J. W., Smith, J. P., Pitts Jr., J. N.: Response of commercial chemiluminescence NO-NO<sub>2</sub> analyzers to other nitrogen containing compounds, *Environ. Sci. Technol.*, 8, 1118-1121, 1974.