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Comment on acp-2022-565

Anonymous Referee #1

Referee comment on "Measurement report: The 4-year variability and influence of the Winter Olympics and other special events on air quality in urban Beijing during wintertime" by Yishuo Guo et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-565-RC1>, 2022

This study focuses on relating air pollutants concentrations in wintertime in Beijing to meteorological conditions and to temporary modifications in the anthropogenic emissions caused by "special events" like the Chinese New Year days, the COVID lockdown period or the 2021 Olympic games. The aim of this study is to infer information useful to guide future air pollution mitigation actions in Beijing based on an analysis of the effects of the restrictions during these special events. In this reviewer's opinion, the real value of this study is in contributing to understanding the atmospheric processes driving air pollution in this area of the world as a function of both meteorological factors and changes in the emissions. Besides, I consider comments like "the concentrations of PM_{2.5} [...] showed similar year-to-year variabilities, decreasing from 2019 to 2021 and then increasing in 2022" (Abstract, lines 25 - 27) somewhat misleading, because the year-to-year variability is interpreted as part of long-term trend triggered by the changes in the emissions, while here just four years are contrasted and the effects of meteo anomalies is big. In some parts of the paper, the simple Authors' judgement not the data is used to explain phenomena, like the rise in concentrations in 2022 shown as an effect of the resumed social activities. I encourage the Authors to first present the observed changes in atmospheric composition as a consequence of the variability in the meteorology, then show the effects of restrictions net of the variability caused by the changing meteo conditions. Although all these effects are analysed in detail in the main part of the text, the Abstract and the conclusions fail in presenting a fair description of such complexity.

The presentation of the results is well organized and supported by clear figures. It remains, however, a bit qualitative. The inter-period variability of the single aerosol and gas parameters is well monitored in the text, but it is often unclear whether when two variables share qualitatively the same pattern they also do quantitatively (see e.g., CO and NO₂ in Fig. 3 vs total OOMs in Fig. 4). Maybe, by normalizing the data, the magnitude of the changes will show up more clearly.

Finally, the presentation of air ions and condensable vapors monitoring clearly provides new information on the processes governing SOA and PM_{2.5} formation in and out periods of restrictions. Such results should be better emphasized in the Abstract.

Specific comments:

Section 3.1.1. This section does not discuss the difference in wind direction of year 2022 with respect to the previous years (Fig. 1). In addition, does the higher RH and lower UVB in 2020 and 2022 with respect to 2019 and 2021 (Fig. 2) has something to do with an anomaly in cloudiness/fog?

Line 165: clarify that the "year-to-year variability" refers to the 1st – 22nd Jan period.

Section 3.1.2. The year-to-year variations in trace gas levels are interpreted only in terms of variations in the emissions (decline, "rebound", "recovery" etc) and completely neglect the meteo anomalies discussed in the previous section; why? Clearly the drop in concentrations of year 2021 must be influenced by the windy conditions favouring pollutant dispersion. In this reviewer's opinion, the drop of the mean NO_x and CO concentrations from 2019 to 2020-2022 can be attributed to changes in the emissions only in light of the different meteo conditions: in 2019, the winds were less frequently flowing from polluted sectors and still concentrations of NO_x and CO were higher than in 2020 and in 2021, hence this can footprint an actual change in traffic emissions.

Lines 187-188 "The variations of CO, NO and NO₂ were generally opposite to that of the BLH, suggesting that atmospheric diffusion capacity could play some role". It is well established that the concentrations of primary pollutants is affected by the BLH, so why using the conditional tense?

Lines 191-192 "This is likely associated with the recovery of social activities during the Post COVID-19 Period". This interpretation is actually what the paper is supposed to demonstrate, while here this statement is provided unsupported of meaningful data.

Lines 194-195 "Consequently, our results show that Beijing was successful in the reduction of SO₂, while some challenges still exist in the restriction of NO". The paper does not discuss the air quality targets in China, therefore this statement remains somewhat subjective and out of focus.

Section 3.1.3. The sharp year-to-year change in OOMs concentration, their progressive increase in oxygen and nitrogen stoichiometric ratios and especially the gradual decrease with respect to PM_{2.5} (Fig. S7) deserves further discussion. Apparently such changes can

be useful to unmask the nature of SOA formation in this environment. Since PM_{2.5} levels can be thought as an overlap between a background contribution and a locally-formed fraction, the decrease in OOM/PM_{2.5} with time seems to be related to a sharp decrease in the local sources: such sources are apparently characterized by OOMs rich of carbon.

Line 313 "Meteorological conditions were not substantially different among different special events, with a few notable exceptions" this statement seems unsupported by the data shown in Fig. 12 and 13 where several variations can be found.

Section 3.2.2. The discussion in this section is sometimes difficult to follow. It would need a sum-up too. The current conclusions "As for the CNY period, its restriction effect on NO_x was comparable with Olympics, but not for SO₂ due to fireworks. Overall, our results suggest that the restrictions during Olympics were the most effective in controlling primary gaseous pollutants." are ambiguous: can this "restriction effect" be identified based on atmospheric composition data are just derived from general information about the emissions (e.g. the fireworks)? The simple conclusion about "the restrictions during Olympics were the most effective in controlling primary gaseous pollutants" is somewhat too little in summarizing the information provided by Fig. 14 and S10.

Lines 370 – 371 "OOM concentration also showed a strong association with the pollution level (Fig. S11).". This is clear from Fig. S11, but why in the period of no restriction the same dependence looks so much variable between different observation periods (Fig. S7)?

Section 4 "Summary and conclusions" not fully acknowledges the complexity of the results presented in the previous sections. The information provided here is often incomplete: for instance, line 453 "The control measures during COVID and Olympics were effective in reducing NO_x and SO₂" makes no mention to the effect that the COVID period restrictions had no effect on CO instead. I suggest to expand the summary making a more systematic survey of the changes observed in the single special events (COVID, Olympics and CNY) with respect to the reference, highlighting what can be explained by meteo anomalies, what can be actually attributed to changes in the emissions and what is yet to be explained. The results about NPF events, air ions, SA and OOM should be summarised in a dedicated paragraph, because these are unconventional parameters providing originality to the present study.