Title: The variations of VOCs based on the policy change of Omicron in traffic-hub city Zhengzhou

Authors: Bowen Zhang[1, 3], Dong Zhang[2, 3], Zhe Dong[2, 3], Xinshuai Song[1, 3], Ruiqin Zhang[1, 3], Xiao Li[1, 3,*]

Manuscript number: egusphere-2024-575

Dear Editor

Thank you very much for your time and effort in revising this paper. We apologize for the issues that were not addressed in the revised manuscript. The changes suggested in your current response were very clear, and we believe we have fully understood your suggestions and made the appropriate changes. Changes made in response to these comments are marked in yellow in the highlighted copy of the revised version.

The following is a point-by-point response to each comment.

On the influence of meteorology: the fundamental concern was that the impacts of meteorology on the analyzed pollution events have not been quantified, and therefore it cannot be ruled out that meteorology plays a major role (potentially as strong as or stronger than emissions changes) in the differences in pollution seen in the two analyzed events. The way this is currently addressed in the manuscript is by ignoring the meteorological

differences without giving valid reasoning: e.g. "However, the discrepancy in wind speed (0.3 m/s) between Case 1 and Case 2 is deemed to be of negligible magnitude during the observation periods, and the actual impact is inconsequential." How is 0.3 m/s determined to be "negligible" and its impacts "inconsequential"? As was highlighted in a previous round of reviewer and editor comments, this 0.3 m/s represents a 25% decrease, which could be responsible for a non-negligible 33% increase in pollution levels based on a simple box model framework. Required edits:

EITHER: quantify the potential effects of wind speed and other variables with, for example, a simple box model framework, and incorporate this into the analysis; in cases where this fundamentally changes the conclusions that can be drawn from the analysis -- for example, if the difference in wind speed is indeed sufficient to explain some appreciable portion of the difference in pollutant concentrations -- that should also be noted in the conclusions and abstract.

OR: remove unsupported statements such as the sentence quoted above about the magnitude of meteorological effects, and acknowledge -- here in the meteorology section, but also in the abstract and conclusions -- that meteorology may also play a role in differences between pollution events,

and the importance of that role was not determined here.

Response:

We are grateful for your perspicacious recommendations for amendments. After careful consideration, we have decided to remove the statements that were not supported by sufficient evidence. In addition, we have modified the meteorology section, as well as the abstract and conclusions, to recognize that meteorology may also play a role in differences between pollution events, but we have not identified its specific impact on differences between pollution events in this paper. We would like to quantify the potential effects of wind speed and other variables in further as your suggestions. The revisions are described below:

Lines 256-277: Previous studies have shown that meteorological factors such as low WS, high RH, and low precipitation are responsible for the increase in $PM_{2.5}$ pollution in Zhengzhou in winter (Duan et al., 2019). Our analysis of the correlation between different pollutants and meteorological conditions during the pollution period showed that $PM_{2.5}$, TVOCs and NO_x were positively correlated with RH (Fig. S3), which is consistent with the results of some previous studies (Wang et al., 2019). The comparisons of average concentrations of different periods between different periods are

presented in Tables 1 and 2. In this study, the WS on clean days (1.4 ± 0.8 m/s) was higher than in Case 1 (1.2 ± 0.9 m/s) and Case 2 (0.9 ± 0.7 m/s), while the RH was lower by 26.2% and 12.5% compared to Case 1 and Case 2, respectively. These findings indicate that high RH and low WS influencing the occurrence of pollution during the observation period, which should be further studied in further.WS, Temp and RH conditions during infection and recovery periods were generally similar, and meteorology may also have played a role in the differences between pollution events, but its specific influence was not determined here. The average concentration of $PM_{2.5}$ during the recovery period was 1.6 times the value during the infection period. Furthermore, the concentrations of other pollutants including SO_2, NO_2, CO, and O_3 all showed a similar trend between infection and recovery periods. The TVOC concentration during the recovery period was 1.2 times the value during the infection period, showing an obvious increase trend after resuming production. Decreased trends of air pollutants were found in other studies before and after the outbreak of the novel coronavirus (COVID-19) in early 2020 (Qi et al., 2021; Wang et al., 2021).

On the VOC ratio analysis: the added lines (334-339) do not clarify this analysis. Instead they further obfuscate it, since not *two* sentences in

close proximity both state that pentane is coming from a combination of LPG emissions and fuel evaporation, without acknowledging that coal combustion and vehicle exhaust may also contribute. Further, the X/E ratio looks suspiciously uniform -- was it really perfectly on the 2.01 : 1 line for almost all observations? Or are the observations clustered in the lower-left of graph 3d (near the origin) more scattered? If so, it would be more helpful to zoom in there.

Required edits:

EITHER: remove the paragraphs about the VOC ratios, as this analysis does not seem central to your conclusions (which are largely based on the PMF analysis instead).

OR: explicitly acknowledge in each of the paragraphs about a different pair of species that the observed ratio does not rule out linear combinations of other sources. In particular, in the paragraph about isopentane/n-pentane, it should be noted that the observed ratio does not rule out contribution from coal combustion and vehicle exhaust, and in the paragraph about isobutane/n-butane, it should be noted that the observed ratio, while consistent with LPG usage, could also be achieved by a combination of

vehicular and natural gas emissions.

Response: We regret any inconvenience caused by the remaining issues with the revisions. In accordance with your recommendation, we have explicitly acknowledged in each paragraph on different species that the observed ratios do not preclude the possibility of linear combinations from other sources. In addition, zooming in on the lower left part of Figure 3d still shows that almost all observations are very close to the 2.01:1 horizontal line. We have added the zoomed-in section to Figure 3d.

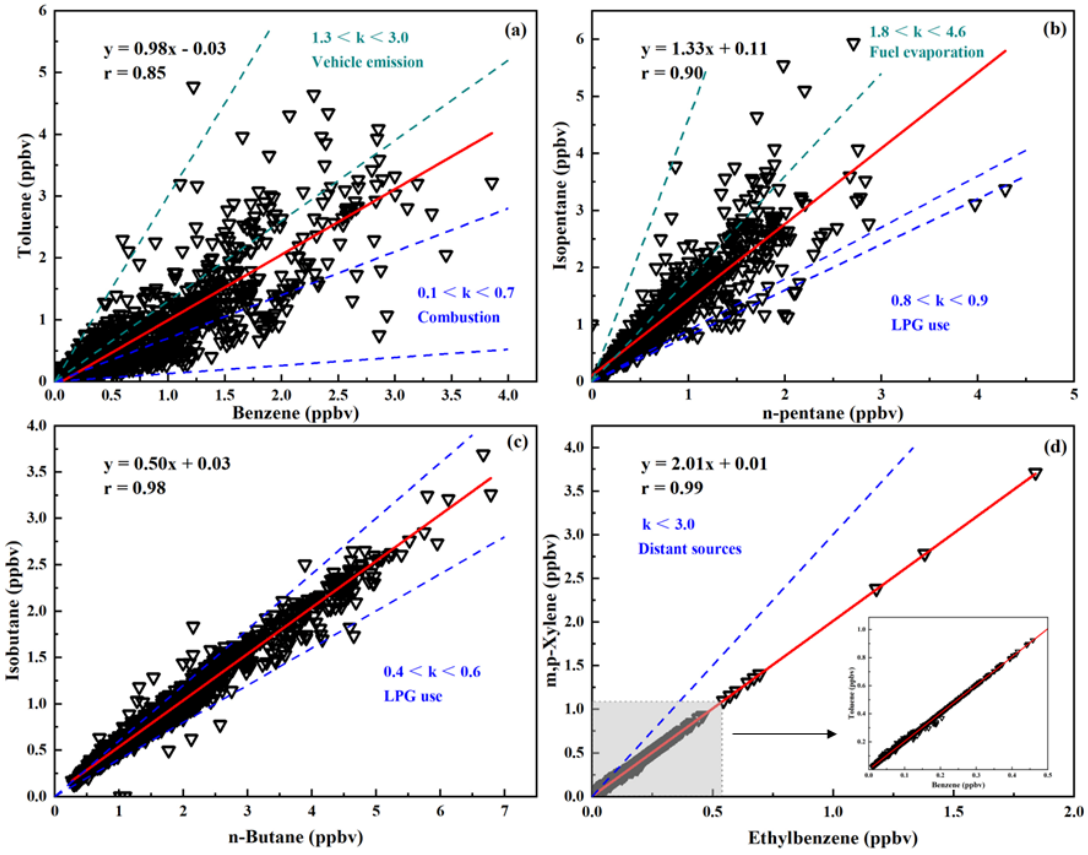

Fig. 3. Correlation analysis between specific VOC species.