Response to Reviewers

Thank you for the reviewers' comments on our manuscript entitled "Impacts of urbanization on air quality and related health risks in a city with complex terrain" (egusphere-2022-486). These constructive comments are all valuable for revising and improving our manuscript. We study these comments carefully and have made correction as requested. Here are point-by-point responses (in blue color), and the changes are reflected in the revised manuscript (in red color). The line numbers in the authors' responses are obtained from the revised manuscript, in which all the revisions have been accepted.

Anonymous Referee #2:

General comments

Accelerated urbanization across the globe has resulted in considerable changes in land surface parameters and subsequently affect meteorological conditions and air pollutant levels. In this study, Zhan et al. reveal the changes in criteria air pollutants between 2015 and 2021 and probe the environmental consequence of rapid urbanization over a typical megacity situated in southwestern China, Chengdu. They also quantify the premature mortalities attributed to exposure to ozone and $PM_{2.5}$. This paper is well-written and presents results that would be interesting to the air quality modeling community from a practical perspective. I have several concerns that the authors should consider when revising the manuscript, as listed below. I recommend publication after the following comments are adequately addressed.

Response: We would like to thank the referee for the valuable and affirmative comments on our manuscript. We have carefully revised our manuscript based on the following comments.

Specific comments

1. The literature review could be better. The authors only provide an example of urbanization impacts focused on the YRD by Liao et al. (2015), while extensive studies have been focusing on identifying the effects of urbanization on the regional meteorological phenomena and air quality (including the Beijing-Tianjin-Hebei area and the Pearl River Delta). Furthermore, recent studies have widely acknowledged the critical role of urbanization in altering air quality in Chengdu [Wang

et al., 2021, 2022a]. Thus, these studies should be discussed and properly cited.

Response: Thanks for the constructive comment. We take your suggestion and deeply reorganize the introduction, in which we add a review of research on the effects of urbanization on regional meteorology and air quality in other regions, such as the Beijing-Tianjin-Hebei region, the Pearl River Delta region and the Sichuan Basin. Please see lines 62-73 in the revised manuscript for details. Thanks for the recommended literatures. We have learned a lot from these literatures, all the recommended literatures are cited in the revised manuscript.

2. Line 34-36: This sentence is a bit vague and I genuinely don't understand this sentence - please rephrase.

Response: Thanks for the constructive comment. The original sentence "This reminds us that the development of cities is also important for the urban air quality apart from the emissions reduction" is revised as "This reminds us that, in addition to regulating anthropogenic emissions, urban planning is also important for the urban air quality, especially for secondary pollutions like O_3." in the new manuscript. Please see lines 40-41 for details.

3. Figure 1: It seems that the shapefile used by the authors (NCL default shapefile) is wrong. Please check. Also, please clarify the source of SO_2 emission for subplot (b). Moreover, it would be valuable to provide the boundary of Chengdu city in Figure 1 for making it clear.

Response: Thanks for the constructive comment. We have revised the NCL default shapefile based on the map database provided by Dr. Yongjie Huang (https://github.com/huangynj/NCL-Chinamap). Thanks for the reminder and reviewer #1's **specific comment 5**. We remove the original Figure 1b since the role of SO_2 emission is never actually mentioned in the paper. Instead, we give the boundary of Chengdu and the distribution of monitoring sites in Chengdu. Please see the new Figure 1 in line 123.

4. Line 190: The criterion for $PM_{2.5}$ and MDA8 ozone is a bit vague. I believe that it is annual $PM_{2.5}$ concentrations less than 75μg/m^3 and MDA8 ozone less than 160μg/m^3. Please clarify the time period for these metrics.

Response: Thanks for the constructive comment. In China, the national ambient air quality standard for $PM_{2.5}$ is that daily average $PM_{2.5}$ concentrations cannot exceed 75 μg m^{-3}; the national ambient air quality standard for O_3 is that MDA8 O_3 concentrations cannot exceed 160 μg m^{-3}. The definition of $PM_{2.5}/O_3$ pollution is based on Chinese national standards. We have clarified these metrics in the revised manuscript. Please see lines 215-217 for details.

5. Line 193-196: The authors apply the annual mean MDA8 ozone concentrations for illustrating the variations of ozone across Chengdu over time. However, annual mean MDA8 ozone is not a meaningful metric as wintertime low MDA8 ozone would pull low ozone levels. In general, it is recommended for using the warm season (April-September) MDA8 ozone average (see Wang et al., (2022b)) or the 90th percentile of MDA8 ozone (which is based on Chinese NAAQS GB3095-2012).

Response: Thanks for the constructive comment. We agree that the annual mean MDA8 O_3 concentration is not a meaningful metric for the interannual variation of O_3. Since the annual metric for O_3 in China is the 90th percentile of MDA8 O_3 (GB3095-2012; HJ663-2013), we adopt the 90th percentile of MDA8 O_3 concentrations in the revised manuscript. Please see lines 221-225 for details.

6. It would be better to use "heat maps of (a) daily average $PM_{2.5}$ and (b) MDA8 O_3 concentrations" rather than "distribution" for the caption of Figure 2.

Response: Thanks for the constructive comment. The new caption of Figure 2 has been revised to "Figure 2. Heat map of (a) daily $PM_{2.5}$ and (b) MDA8 O_3 concentrations in Chengdu from 2015 to 2021." Please see lines 233-234 for details.

7. Line 219-220: "the premature mortalities due to O_3 fluctuate." This is an incomplete sentence. Please check.

Response: Thanks for the constructive comment. In the revised manuscript, this incomplete sentence is removed. Instead, we directly give the number of premature deaths attributable to O_3. Please see lines 247-250 for details.

8. Line 221: "annual average" might be "7-year average". Please check.

Response: Thanks for the constructive comment. Yes, it is "7-year annual average" instead of "annual average". In the revised manuscript, we have corrected this typo. Please see lines 22, 240-243, 250 and 492-493 for details.

9. Line 275-278: Is the WRF-Chem model performance comparable with prior studies over Chengdu (or Sichuan Basin)? It would be valuable to briefly compare the model performance with previous studies (Yang et al., 2021; Wu et al., 2022) for demonstrating the robustness of model results.

Response: Thanks for the constructive comment. In the previous version, we do not compare the WRF-Chem model performance with prior studies over Chengdu. Thanks for the recommended literatures. We add a brief comparison of our model results with those from prior studies in the revised manuscript, which further suggests that our simulations are reasonable. Please see lines 305-311 and lines 314-318 for details.

10. Line 369-370: This sentence is a bit vague and I genuinely don't understand this sentence - please rephrase.

Response: Thanks for the constructive comment. We apologize for this confusing sentence here. In the revised manuscript, a new sentence "Rising anthropogenic emissions of air pollutants and their precursors can significantly increase ambient air pollution." is adopted. Please see lines 423-425 for details.

11. Line 385-395: The authors attribute the ozone changes in Chengdu to the Ozone-NOx-VOCs regime but do not provide any details about the formation regime. A comprehensive discussion on the underlying mechanism of the VOCs-limited ozone regime in urban Chengdu is needed (Wang et al., 2022a).

Response: Thanks for the constructive comment. Yes, we attribute only a slight increase in O_3 concentrations from anthropogenic emissions to the non-linear sensitivity of O_3 and its precursor (VOCs and NO_x). Thanks to your literature and references therein, in the revised manuscript, we add a discussion on O_3 formation regime in Chengdu. From 2013 to 2020, metropolitan Chengdu

remains VOCs-limited regime, and the effect of reducing NO_x emissions may be partially offset by changes in VOCs. Please see lines 440-446 for details.

References

[1] Wang, H., et al. (2021). Impact of different urban canopy models on air quality simulation in Chengdu, southwestern China. Atmospheric Environment, 267, 118775. https://doi.org/10.1016/j.atmosenv.2021.118775

[2] Wang, H., et al. (2022a). Impact of Urbanization on Meteorology and Air Quality in Chengdu, a Basin City of Southwestern China. Frontiers in Ecology and Evolution, 10, 845801. https://doi.org/10.3389/fevo.2022.845801

[3] Wang, Y., et al. (2022b). Long-term trends of ozone and precursors from 2013 to 2020 in a megacity (Chengdu), China: Evidence of changing emissions and chemistry. Atmospheric Research, 106309. https://doi.org/10.1016/j.atmosres.2022.106309

[4] Wu, K., et al. (2022). Drivers of 2013–2020 ozone trends in the Sichuan Basin, China: Impacts of meteorology and precursor emission changes. Environmental Pollution, 300, 118914. https://doi.org/10.1016/j.envpol.2022.118914

Thanks for the recommended literatures. We have learned a lot from these literatures, all the recommended literatures are cited in the revised manuscript. Please see lines 719-721, 722-724, 729-732 and 735-738 of the references.