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Comment on acp-2022-123

Anonymous Referee #1

Referee comment on "Long-term trend of ozone pollution in China during 2014–2020: distinct seasonal and spatial characteristics and ozone sensitivity" by Wenjie Wang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-123-RC1, 2022

Wang et al. analyze the surface ozone trends in China in 2014-2020, and use NOx and VOCs measurements in a box model to investigate the shift in ozone sensitivity regime in Beijing and Shanghai in this period. They find that ozone levels increased from 2014 to 2017, remained flat afterward, and decreased in 2020. They also find that summertime ozone sensitivity in Beijing and Shanghai has changed from a VOC-limited regime to a transition regime during 2014–2020. This study focuses on an important issue and is overall in high quality. The paper is well-written and easy to follow. I would recommend publication after the following comments being addressed.

- My judgement is that the highlight of this paper is using 2014-2020 VOCs and NOx measurements in Beijing and Shanghai in the box model to identify the change of ozone sensitivity. The ozone trend in China itself has been documented in a number of studies as recognized by the authors and is well understood. I would suggest highlighting the novelty and finding of shift in ozone sensitivity in the title and introduction, instead of ozone trends.
- Line 47: From Figure 5 it is clearly that ozone increases extend to 2019. I would suggest removing "reached a plateau after 2017".
- Line 95-97: In general, the authors can do better in catching up the more recent studies of ozone trends and ozone sensitivity in China. An example here, there are also studies pointing out the increases in tropospheric ozone in northern mid-latitudes extend to more recent years (e.g. 2017) than 2000 (Cooper et al., 2020; Gaudel et al., 2020).
- Line 102-106: It might be a bit biased to state "characterization of ozone trends in China remain sparse" and "not yet well understood how changes in precursor emissions influence the trend of ozone in China". In fact studies of spatiotemporal ozone trends in China have been a lot as shown in Table 2. Wang et al. (2021) has addressed "how changes in precursor emissions influence the trend of ozone" using satellite observations, and there are even more studies testing the response by using chemical

transport models (e.g. Chen et al., 2021) as mentioned in Section 3.3. The authors may want to soften the tune in the literature review and highlight the novelty of this study compared to the existing literatures.

- Line 130-137: Here urban and non-urban sites are distinguished by population density. It is a bit simple but fine. Nevertheless, I suggest the authors also refer to more comprehensive definition of urban/non-urban sites from the Tropospheric Assessment Report (Schultz et al., 2017) and a recent study by Gao et al. (2020), and see whether the urban/non-urban separation may influence the analyses.
- Line 148: Suggest citing Lefohn et al. (2018) for ozone metric information and implication.
- Line 168-171: The use of in-situ long-term VOCs data is much appreciated and makes the study stand out from existing literatures. However, the VOCs measurements, in particular their trends, should be presented in figure to support the study.
- Line 331-333. Do VOCs measurement in Beijing and Shanghai show decrease from 2019 to 2020? This is critical for understanding ozone decrease in 2020. A recent study by Yin et al. (2021) suggest the ozone decrease in 2020 is also partly attributed to decrease in VOCs emissions. I wonder whether the authors can prove or disprove such statement from their observations and box model analyses.

Reference

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