Comment on acp-2022-428
Anonymous Referee #1

Referee comment on "Quantifying the drivers of surface ozone anomalies in the urban areas over the Qinghai-Tibet Plateau" by Hao Yin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-428-RC1, 2022

This paper investigate the mechanism of short-term surface ozone anomalies in the urban areas over the Qinghai-Tibet Plateau. The topic and presentation are fine in general. A revision according the following comments should be provided.

The description for producing meteorological normalized concentration in Section 3.3 is quite vague. For example, (1) how the final result be sensitive to the four-week period window? If it is chosen to reflect the seasonal variability, is it really considered to be superior/useful than traditional deseasonalization methods? (2) p6, l17, "This selection process was repeated 1000 times to generate a final input dataset." How is the final input being generated exactly? By using sample mean, median, or anything else? Since the figure results presented in this study are not as variable as I expected from the main text, I am not quite convinced that a random resampling method would lead to such smooth results. A time series plot of original data for each station is desired.

A significant portion of this study is devoted to the discussion of ozone extreme values. To provide a more systematic discussion, an to facilitate better communication, I suggest the authors should quantitatively work on the percentile variation instead (e.g. the 5th and 95th), as suggested by following references:


Minor suggestions:

p4, l6-7, the data quality control procedures should be briefly stated.

p6, l1-2, this part seems to come from nowhere.

p8, l4, Yin et al. (2017)

p10,l25, what does it mean for "seasonal cycles of surface ozone anomalies"? Should the anomaly is deseasonalized already in Eq (5)? If it refers to remaining seasonality variation, can it imply that the methodology in Eq (5) is not appropriate?