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Interactive comment on "Local and synoptic meteorological influences on daily variability of summertime surface ozone in eastern China" *by* Han Han et al.

Anonymous Referee #2

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The authors conducted an extensive analysis of local and synoptic meteorological influences on daily variability in summertime surface ozone in eastern China for the time period of 2013 – 2018. They derived a multiple linear regression (MLR) equation for each grid within the eastern China domain to capture the linear relationships of daily average ozone concentrations as a function of 10 local meteorological variables and 2 synoptic factors, the latter derived using the singular value decomposition (SVD) method. Not to be pedantic, it is an overstatement to call the MLR equation a model. They further examined synoptic weather patterns (SWPs) over eastern China using a self-organizing map (SOM) clustering technique. The MLR and SWPs provides a rich source of information but the authors were short of making a connection between the

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Discussion paper

two. One interesting point from MLR was, as local meteorological variables, relative humidity in the central and southern parts of eastern China and temperature in the BTH region showing the largest influence on surface ozone concentrations. The study would have been more in-depth should the authors have endeavored to understand the mechanism(s) driving that. Would it be possible to use their SWPs to further understand that point? The authors did use their derived MLR to validate the calculated surface ozone concentrations under the 6 SWPs, but they only showed visual comparisons between the predicted and observed values. It'd make a stronger case if they could show some quantitative comparison.

Most of Section 6 "Discussion and conclusions" repeated the results prior to it with the last paragraph suggesting the potential significance of the study. There was not really much discussion but repetition. I suggest that the section be shortened and changed to "Summary".

Figure 14 is missing from the manuscript.

In the huge body of published work on surface ozone as a pollutant, the majority has used ppbv as units for ozone, and indeed in study of atmospheric trace gases mixing ratios have been used conventionally. The authors' use of mass units was a bit peculiar. I suggest that they provide unit conversion upon the first appearance of the mass units if they insist upon using them.

Some specific comments:

1. Line 36: The first sentence covered both human and vegetation health but the reference cited, Yue et al. (2017), was on vegetation. 2. Lines 78 – 80: Shen et al. (2017a) is not the first and only reference for such a well-established point. There is a huge wealth of research on this point dating back to decades ago. This seems to be a fairly common problem nowadays, that for an extensively, long studied topic, only most recent few studies would be cited whereas a long list of monumental studies leading up to the recent works tend to be left out. In my opinion, we need to do due diligence

to cite the references where credit is due. 3. Line 167: What was “daily surface ozone” meant? Daily average or daily maximum 8-hr average ozone concentrations? Also, the acronym for the latter would be DM8(H)A; it’s curious why the authors used “MD8A” instead. 4. Lines 298 – 299: Not clear where this came from. 5. Line 301 – 302: Was “higher meridional wind” enough to bring in “clean and humid marine air to the south” regardless of wind direction? 6. Lines 305-306: How did the authors know that “the impacts of relative humidity on surface ozone are mainly through the chemical processes”? 7. Line 321: why did R2=0.38 qualify to be “strong”? 8. Lines 388: why is precipitation included in the indexes?

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