

## ***Interactive comment on “Dynamic Processes Dominating Ozone Variability in Warm Seasons of 2014–2018 over the Yangtze River Delta Region, China” by Da Gao et al.***

**Anonymous Referee #1**

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General comments: In this manuscript, the authors focus on the inter-annual variations of warm seasons (April–September) ozone over YRD, China from 2014 to 2018. The relations between the inter-annual ozone and synoptic-scale circulations and the associated meteorological controlling factors were revealed. The authors highlight five dominant synoptic weather patterns (SWPs) in the warm seasons in YRD using the t-mode principal component analysis and reconstructed the inter-annual O<sub>3</sub> variation based on SWPs frequency and intensities. The analysis is mostly sound, especially on inter-annual ozone variations impact by SWPs induced meteorological factors. But some analysis need deeper explanation in physical or photochemical principals, and some conclusions need more robust supports. My specific suggestions and comments

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are as follow. Specific comments: 1. In the title and content of the paper, I feel the “Dynamic Processes” could not give a direct and effective cognition to reader. I suggest “Ozone Variability in Warm Seasons of 2014–2018 over the Yangtze River Delta, China induced by synoptic patterns” or similar titles should be better. 2. In abstract line 37-41 and also in the context, the 2 sentences may conflict. I am not sure “the strengthening of the ridge and trough in the westerlies” is conflict with “the weakening of the continental high under SWP2” and “the southern low pressure weakening and WPSH weakening under SWP4, and the north China anticyclone weakening under SWP5”. In comparison with the similar previous studies (Han, et al., 2020; and Gao et al., 2020), this paper is not clear in spatial distribution of pressure and lack of clear pictures in synoptics. 3. In figure 1, “43.40” need mention in the context. 4. In the EOF analysis, the spatial distributions of EOF1 are generally negative and time series of EOF1 presents a decreasing trend. Actually, the O<sub>3</sub> generally increase all over YRD in recent years. So, I suggest multiply -1 with spatial distributions of EOF1 and time series of EOF1 make the statement easy to follow. 5. The authors reveal RH is the key factor dominating inter-annual variations of ozone, and indicate that its unclear in the relations between RH and ozone in previous study. I suggest the authors gives a further clear explanation of the RH effects on ozone. RH could related to the cloud cover (solar radiation), stable of air in BL and so on. In figure 3b, sunshine duration (may related to cloud cover?) is not important in ozone inter-annual variation, and opposite to RH, which may implicate that stable of air (accumulation of air pollutants) is important? 6. In line 402, “the cloud cover hard to form” should be “the cloud hard to form.” 7. What is the unit in figure 4 of W (vertical velocity), m/s or Pa/s? 8. In line 427-428, the sentence “At 500 hPa . . .” should indicate the area of downward motion. 9. What is SR in fig. 4 etc.? SD? 10. From figure 4-8, a summary table with values of meteorological factors in 5 SWPs could be better than sub-figure (e) for comparisons. 11. In section 3.4, I wonder why do you reconstruct the EOF1 time series? It could be more valuable to reconstruct the inter-annual variations of ozone concentration based on SWPs frequencies and intensities. And What’s SWPIIs? Technical corrections: 1. In the caption

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of figure 2, are they “orange dash line”? Pink? 2. There are several typos that need careful checking. For example, “meddle” in line 313; “wins” in line 468; “SR” in line 336 could be SD.

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