It is appreciated that the authors took great efforts to elucidate the complex relationship between WPSH and air pollution in China during summer, which is interesting. However, the manuscript was not well organized (synoptic characteristics and their influences on pollution are better given in a section), and most analyses were superficial without observational evidence. The manuscript needs better organization and careful English editing.

1. Only four types were identified, which tends to oversimplify the complex synoptic situations during summer in eastern China. By contrast, six types were identified by Han et al. (2020) to understand the influence of synoptic weather on the summertime O3 pollution in eastern China; nine types were classified by Ye et al. (2016) for the aerosol pollution in the North China Plain. The region selected for the classification should be consistent with the studied region (eastern China in Fig. 1). Why use the region shown by the black squares in Fig. 4? And present it in a larger domain in Fig. 4? The classified region cannot resolve the processes needed? The selection of the classified region can significantly influence the classification results.

2. The classification results are odd. For example, in July of 2016, it quickly turned form Type 2 to Type 4, and then became Type 1 during a few days (Fig. 5). The WPSH cannot jump like that (e.g., from Type 2 to Type 4 in 24 hours). Besides, almost the whole June of 2016 was identified as Type 1 (“South BTH-North YRD O3-PM2.5 compound pollution”), while the South BTH and North YRD often

experience clean and pollution situations during a few days (https://www.aqistudy.cn/historydata/daydata.php?city=%E5%8D%97%E4%BA%AC&month=201606). The synoptic pattern of Type 1 cannot explain the formation and evolution of pollution in South BTH and North YRD. The variations of pollution level may be primarily controlled by other synoptic weather systems at 900/850-hPa, rather than the 500-hPa WPSH. The same problems also existed in other Types/regions. The classification results and their relationships with pollution are suspicious and unreliable.

3. Physically, the synoptic weather patterns influence the pollutions via PBL structure (e.g., large-scale subsidence), regional-scale transport of pollutants (e.g. PM2.5 and VOCs), and occurrence of precipitations. All these physical processes underlying were not well analyzed. The authors cannot just use simple correlation analysis to explain the mechanisms, which makes the conclusion unreliable and inconvincible. At least, typical pollution episodes of each synoptic weather type should be analyzed in-deep with observational evidence to validate the hypothesis given.

4. "The BLH was calculated according to the method given by Guo et al. (2016, 2019), and the FLWD [frequency of light wind (< 2 m s−1) days], precipitation frequency (PF), and MDA8 O3 were also counted." The detailed information about BLH should be directly given. At present, only a few cities have afternoon soundings during summer. Only 08:00 and 20:00 LT soundings were used to calculate the BLH, which

is inappropriate for this study. How to calculate the FLWD and PF? Why only choose these specific parameters? Are they significantly correlated to the pollution levels in all the studied regions (e.g., BTH, YRD, GZP)? How about the precipitation intensity and amount, and its lasting time? How about the wind directions and wind shear, and associated transport of pollutants (PM2.5 and VOCs)?

5. In the Results and Discussion sections, the “rain belt”, “Meiyu”, “rain band”, “heavy precipitation” were simply analyzed with very few observational evidence. Since this study focuses on the movement/evolution of WPSH in summer (rainy season), more in-deep analysis on the links between precipitation and pollutions should be given with observational evidence and typical episodes.

6. Section 4.2 “Effects of NO2 on O3”. The best proxy of photochemical reactivity is the ozone potential efficiency (OPE) but not the ratio of O3 to NO2. High photochemical reactivity probably appeared with high O3 and NO2 concentration but reasonably with a low O3/NO2 ratio. It is not accurate to take the O3/NO2 ratio as the judging criteria.

Specific comments:

Fig. 5, two “2017”?

“BTH, YRD, PRD, Guanzhong Plain (GZP), Northeast Megalopolis (NEM) regions”,

the locations of these studied regions should be clearly described in manuscript and presented in the Figure.

Line 165-166, “More detailed information about the T-PCA method can be found in Miao et al. (2017).” The detailed information of the method should be directly given.

Some literatures were not properly cited. Please carefully check the citations of the whole manuscript. Some are given below. “In general, PM2.5 pollution is featured with obvious diurnal and seasonal changes. Due to the influence of atmospheric diffusion conditions such as precipitation and wind speed (WS), it tends to be enhanced in the morning and evening, lower at noon, and higher in winter and lower in summer.” Dose the author mean that the diurnal variations of precipitation and wind speed modulate the pollution level? It is odd. How about the emission and PBL?

“Summer O3 pollution has gradually been prominent, replacing PM2.5 as the primary pollutant in the air…” Is it true? At present, PM2.5 still is the dominant pollutant in China.

“Miao et al. (2015) suggested that strong northwesterly synoptic winds, low BLH (boundary layer height), high RH and stable atmosphere are more prone to aerosol pollution in the BTH region during wintertime …” The strong northwesterly winds would favor the dispersion of pollutants in winter.

“Shi et al. (2020) studied the spatial distribution of O3-8h (O3 8-hour moving average) and PM2.5, and their sensitivity of meteorological parameters; pronounced positive (negative) correlation between temperature (BLH and absolute humidity) and O3-8h was found, but the relation between WS and O3-8h was spatially different; for PM2.5, it was negatively (positively) correlated with temperature, WS and BLH (absolute humidity).” It is another inappropriate citation. Please carefully read the previous study (Shi et al., 2020) and properly introduce it.

“Recently, Han et al. (2020) revealed that meteorological factors can explain ~46% of the daily variability in summertime surface O3, while synoptic factors contribute to ~37% of the overall meteorological effects on the daily variability of surface O3 in eastern China.” More detailed information on Han et al. (2020) should be presented since it is quite similar to this study, such as its studied period, method, and classification results?

Line 95-97, “The abovementioned indicates that the variation of meteorological factors, which are mainly driven by the evolution of different weather circulation situations, play a non-negligible role in air pollution. Therefore, classification of air pollution according to the meteorological circulation has become particularly important …” The abovementioned literatures cannot support this statement.

Line 101-102, “In recent years, it has become possible to objectively classify atmospheric circulation conditions using weather data such as GH, sea level pressure…” It is not true. The objective classification method has been used since the 1990s.

Line 104-105, “the objective approach has been widely used in air pollution research (Beck & Philipp, 2010)…” Beck and Philipp (2010) didn’t study the pollution issues.

Line 115-118, “Many studies have suggested that PM2.5 and O3 pollution are mainly related to the East Asian summer monsoon (EASM) and western Pacific subtropical high (WPSH) (Li et al., 2018a; Xie et al., 2017; Yin et al., 2019; Zhao et al., 2010).” More detailed information about these previous studies can be given, their studied periods, temporal scale and spatial scale. Seasonal variation? Inter-annual variation? The paper of Xie et al. (2017) has been withdrawn, please check (https://acp.copernicus.org/preprints/acp-2017-500/).

Line 129-131, “the compound O3-PM2.5 pollution-related meteorological conditions, should be complex and likely to be associated with certain weather types”. This statement is not well supported.

Han et al. (2020) https://doi.org/10.5194/acp-20-203-2020

Ye et al. (2016) http://dx.doi.org/10.1016/j.atmosenv.2015.06.011