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Interactive comment on "Synoptic drivers of co-occurring summertime ozone and $PM_{2.5}$ pollution in eastern China" *by* Lian Zong et al.

Anonymous Referee #3

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Review of acp-2020-596: Synoptic drivers of co-occurring summertime ozone and PM2.5 pollution in eastern China

The authors use the T-mode PCA to objectively classify the summertime synoptic weather pattern across East-Asia and the western Pacific Basin aiming to identify the mode(s) most favorable for compound pollution events across sub-regions in China, specifically for PM2.5 and O3. Many factors governing these events operating across an array of scales are explored. The PCA identified 4 synoptic regimes characterizing the seasonal set up of the 500 hPa WPSH from 2015-2018. An additional large-scale circulation is also at work here, the East-Asian monsoon, which is discussed in context to the WPSH. Additionally, the authors discuss the effects of precipitation frequency and boundary layer characteristics on regulating compound pollution events. Occur-

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Discussion paper

rences of pollution are based on Chinese governmental standards.

While this work has great upward potential to be a significant contribution to the community, many revisions are required before publication. Secs. 1-2 are written quite well and motivate the questions at hand. Beyond that however, I believe that more concrete connections can be and must be made between processes unfolding at different scales (synoptic down to the mesoscale) of motion that lead to Types 1 and 2 dominating the regulation of compound pollution events. For instance, connecting the modulations in the WPSH to changes in favorable PBL conditions and thermal stratification need to be made, in addition to changing precipitation amounts between the types. All of these processes dictate the amount of pollution in the atmosphere at any given time. The final sentence of Sec. 1 states that this manuscript will examine the SWPs responsible for co-occurring pollution events, but the synoptic scale processes have bearing on finer scale processes such as PBL characteristics that are critical for air quality (e.g. inversions associated with tropospheric sinking motion). The authors analyze changes in PBL height between the types and provide loose discussion of their implications for air quality, but further analysis is needed.

Major comments

1. The abstract needs to be shorted and be more specific. 2. How do the percentages of the PCs sum to 100%? Shouldn't there be other relevant synoptic patterns than just those 4, meaning that the leading 4 patterns account for most of the synoptic-scale pattern but not all? 3. The language used to describe the synoptic scale features needs to be presented in a manner consistent with meteorological standards (see Bluestein 1992). In its present form, it is very difficult to follow the discussion. Here is n example. On lines 226-227, the authors state "The westward extension and southward motion of the WPSH in Type 1, as shown in Fig. 4a, transports water vapor into the YRD region, and the prevailing southwesterly in the YRD region and westward flow from the north form a cyclonic convergence area, with high temperature and high humidity during the Meiyu season." The 850 hPa flow associated with each PC correlates highly with the

gradient in 500 hPa GH as rather expected, but what is meant by "southward motion of the WPSH?" Are the authors referring to the anticyclonic flow about the WPSH (i.e. northerlies to the west of the GH maximum)? Also, the sign of the relative vorticity should differ with height in the troposphere. For instance, should vorticity be negative in the lower troposphere (i.e. anticyclonic), it should be positive (i.e. cyclonic) in the upper troposphere (assuming a thermally direct circulation on a rotating sphere). Are the authors referring to the cyclonic shear vorticity anomaly apparent in the 850 hPa arrows around 120E/30N? The authors should use GH anomalies as reference points to describe the flow patterns, and they should make sure that it is clear which level in the atmosphere is being referenced in the text. More examples are given below. 4. Sec. 3.2: I feel as though the discussion of the PCs could be tied more explicitly to the vertical motion field. Obviously, the WPSH is characterized by mid-tropospheric downward vertical motion and doesn't need much justification. However, the strength of the sinking motion and its co-occurrence with low wind events is driven by the synoptic pattern and could be shown. I would suggest at least a supplemental figure showcasing how the vertical motion varies with PC, perhaps overlaid with the 10-m windspeed. This would set up the next section nicely, which returns to examining the spatial characteristics of PM2.5 and O3. 5. Diffusion of pollutants between the PBL and the free atmosphere is fundamentally related to the turbulent mixing and thermal stratification of the overlying atmosphere. While referenced here, I believe that this is an integral component of this work and must be explicitly addressed across the various subregions. How do the vertical profiles of temperature, moisture, and wind compare across the multiple PCs and subregions? How are these anomalies physically related to the different synoptic weather pattern differences between the PCs?

Other comments

1. Line 32: "Slight" should be "low" 2. Line 57: insert a "the" before "economy" 3. Line 72: Change "attached" to "caught" 4. Line 85: "US" should be "United States" 5. Line 105: The Miao et al. findings should be reproducible here but for a multitude of areas.

Cross-sections similar to their Figs. 6-7 would work, but regionally averaged vertical profiles would work as well. Vertical profiles of state variables (temperature, stability, vertical velocity, etc.) should be included in this manuscript as these quantities' vertical variation help to significantly modulate PBL and free atmosphere exchange of heat, moisture, pollution, etc. I would also suspect that summertime surface winds would be lower due to more infrequent midlatitude cyclone occurrences, so pollution "pooling" would be more frequent. 6. Lines 146-147: Subregions should be labeled in a figure to orient the reader. This can be done in panel (a) of Fig. 1. 7. Fig 1.: There is no "red box"? If there is, it is not clear 8. Figs. 2 and 3: Please change the color of "heavily polluted" regions to something other than turquoise. It can easily be misinterpreted as a "good" category 9. Line 200: How is "pollution day defined" for O3? By the thresholds laid out earlier (160 ug/m3 threshold)? Also, what constitutes "moderate" pollution? Same question applies for PM2.5. 10. Line 219-220: This low-level transport feature and its variation with PC is not shown in any figure but is referenced. I believe that at least one figure should show this feature since it is being discussed in forthcoming results 11. Line 226: How can you infer that water vapor is being transported into the YRD regions? The 850-hPa flow vectors are at best directed parallel to the YRD coastline. Otherwise they are directed offshore. Additionally, inferences about moisture transport should be made by wind/water vapor overlays or by integrated vapor transport/moisture convergence analysis (see Rahimi et al. 2018), which this figure does not have. 12. Line 227: Flow shifting from southwesterly to westerly with northward extent is anticyclonic, which we see in the figure. At the same time, we see a cyclonic sped shear maximum, so it is impossible without quantifying the vorticity explicitly to say if this is anticyclonic or cyclonic. Please remove "cyclonic." 13. Line 229: Is it the WPSH retreating or advancing? Its axis appears to shift north slightly... 14. Line 230: Consider deleting, "and the GH over northern China at 500 hPa is higher compared with Type 1 (Fig. 4b)." The change in the magnitudes of GH are not terribly important; it is the change in their gradients that regulates the winds in each PC. Line 231: The only Type 2 onshore flow (at 850 hPa) I see is around 120E by 30N, which lies directly west of the Type 2 GH maximum. This is an example of how you can use certain language relating flow properties to GH anomalies for specific PCs. Currently the authors state, "...southerly wind blowing from the ocean to the continent lies in front of the bottom of the high pressure,...", which is very unclear. More generally, I recommend the authors refrain from using "top" and "bottom" unless they are referring to the vertical axis (i.e. up and down). 15. Line 233: "and the rain belt moves northwards to the east of the YRD region." Are the authors referring to the belt as it compares to other PCs? If so, the different PCs may be compared to one another, but it is not guaranteed that any type will necessarily evolve from another type. Please clarify and reword throughout the text. 16. Lines 237-238: "...which implies that the rainy season in the YRD region ends in midsummer and the weather becomes hot and dry." How is this implied? 850-hPa flow is onshore beneath the western edge of the 500 hPa monomer of the WPSH. This linkage is not implicit and should be explained. Moreover, references made to shifts in precipitation need to be explicitly shown if they are going to be frequently referred in the text. 17. Lines 239-240: "continues to extend westwards and shift northwards," shifts westward and northward compared to PC3. Again, please indicate it's the synoptic pattern's position more explicitly. Something like, "In Fig. 4d, the monomer is located north and west of the feature in Fig. 4c". The word accordingly relates this sentence to the previous one, but it isn't clear what that linkage actually is. Also, please explain the linkage or remove the word "accordingly." 18. Line 241: Heat wave? How is PC4 related to a heat wave? Where is this shown in the figures or analyses? 19. Figs 3-4: How are levels of air quality defined? Are they arbitrary? If so, then a brief justification is required. If they are a community standard, then a source is needed. 20. Fig. 4 shows the PCs of the synoptic weather pattern and associated percentages of occurrence for the study period. 21. Fig. 5: 2017 is labeled twice. Should the second instance be 2018? 22. Line 263: Any hypothesis for why the lowest MDA8O3 occurs for PC3? Could it be related to the synoptic pattern of Fig. 4 and associated precipitation (not shown)? 23. Line 279: Delete "in the eastern region" 24. Fig. 8, Lin3 285: What constitutes "serious?" Perhaps it would be good to plot the pollution threshold values here for O3 and PM2.5. Plotting these curves (they would be straight lines) would help the reader to identify how bad (or good) the air quality actually is in terms of PM2.5 and O3. The authors discuss "over-standard" rates, so a threshold must have been used (plot it). I believe these values are 160 and 75 ug/m3 for O3 and PM2.5, respectively... 25. Line 286: For (2), over-standard rates are not plotted – concentrations are. Please clarify. If the authors are suggesting that O3 an PM2.5 concentrations are similar between PCs, then please reword. 26. Line 287: For (3), it looks like Type 4 is leading, not Type 1, for O3 concentrations from 0900-1500. Since this is when concentrations are largest, the "Type 1 > Type 4 > ..." may mischaracterize your argument. 27. Line 302: "Activities"? Do the authors mean "modulations"? This is unclear.. 28. Line 308: "Makes summer always hot and moist" grammar needs revisions 29. Line 316: "presents negative" should be followed by "(Fig. 9a)" to guide the reader. Also, why are Tmax anomalies negative under this PC? 30. Lines 312-321: Precipitation is integral to the lifecycle of PM2.5 and O3. The linkages between the precipitation anomalies and Fig. 4 should be explicitly discussed. I believe the authors attempt to do this in Sec. 3.2, but that discussion is more appropriate here. 31. Lines 328-331: This sentence is unclear and should be revised. Also, there is an instance here where the authors use an acronym in one part of the sentence but not the other. Please be consistent. Also, how do negative FLWD anomalies result in a deeper PBL? 32. Sec. 4.1, P3: I believe that stability should be discussed here in addition to a more detailed discussion of precipitation "anomalies" associated with each PC. Thermal stratification of the PBL will dictate the mixing depth of the PBL and thus regulate the pooling of these aerosol/pollution plumes. Looking at the correlation between Tmax, PF, FLWD, etc. is not enough. 33. Lines 346-349: Here is a wonderful chance to discuss what is special about PC4 on a synoptic level. Why is PC4 leading to the largest O3 events synoptically? Do these same conditions favor the co-occurrence of O3 and PM2.5? 34. What is the difference between the Yangtze River and the YRD? These should be labeled on a map for readers... 35. It seems as though there is a window of moisture availability that is large enough to allow hygroscopic growth of PM2.5 but is sufficiently small to allow for the important photochemical processes that regulate O3. It would seem to me that identifying this moisture window, as well as its sensitivity to PCs, would be a very significant contribution and I recommend that it be studied further to more precisely identify the PCs responsible for co-occurring O3/PM2.5 events. Identification of this moisture window can be based on an optimal relative humidity for compound pollution events too. This window can change by region and PC type. 36. Line 368: Strengthens compared to Type 1? Type 2's trough does not necessarily strengthen from the Type 1 pattern. Please reword. 37. Lines 357-387: The authors give the percentage of days with compound pollution for types 1 and 2. However, this does not elucidate which type is more efficient at producing compound pollution. The authors should include the percentages of compound pollution days for types 3 and 4. From the results here, I'd suspect that types 1 and 2 are the most efficient compound pollution setups, but this can be confirmed by including the percentages as for types 1 and 2. 38. Lines 396-397: These percentages need to be presented for Types 3 and 4 as higher percentages may indicate that PCs 3 and 4 are more efficient setups for co-occurring pollution events, even if the PCs occur less frequently. 39. Line 398: "line" should be "axis" 40. Line 400: Again, what is "Meiyu" season for non-local readers? 41. Lines 400-401: How do higher temperatures suppress O3 production? I would suspect that the higher relative humidifies are primarily responsible.... 42. Line 403: Is the low pressure trough at the surface or at 500 hPa? 43. Lines 402-404: Again, this "small amount of water vapor transport" suggests that there is a nominal vapor pressure deficit conducive to compound pollution events. In an environment of appropriate stability and PBL characteristics, compound pollution may be especially severe. 44. Lines 407-408: It appears that the WPSH only shifts north in your objective PC analysis, not southwards and eastwards...can the authors clarify? 45. Line 411: Why does water vapor lead to a sink of O3? Water vapor by itself cannot remove O3 from the atmosphere or prevent its formation. Are the authors referring to the supersaturation, dew point depression, etc.?

References

Bluestein (1992): Synoptic-Dynamic Meteorology in Midlatitudes: Principles of Kinematics and Dynamics, Vol. 1 1st Edition Rahimi et al. (2018): Exploring a VariableâĂŘResolution Approach for Simulating Regional Climate Over the Tibetan Plateau Using VRâĂŘCESM. J. Geophys. Res.

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