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Interactive comment on "The Relationship between the Anticyclonic Anomalies in Northeast Asia and Severe Haze in the Beijing–Tianjin–Hebei Region" *by* Wogu Zhong et al.

Anonymous Referee #1

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This paper examines the connection between the large-scale mid-tropospheric circulation over Northeast Asia and air quality in one of the most heavily populated parts of China. The analysis is generally well constructed; however, some aspects of the methodology are not sufficiently documented, some of the confidence levels appear to be overstated given the limitations of the data involved, and some of the interpretations need further clarification. I include a few suggestions along these lines below. The content is within the scope of ACP and a revised version of the paper could be a valuable contribution to research on this topic, helping to address some outstanding questions on how the large-scale circulation influences air quality in Beijing and surrounding areas. However, major revisions will likely be necessary for the paper to meet

that standard.

Major comments:

1, Why only December? This is not clearly explained in the text, and seems a strange choice given that only three years of data are used.

2, Details and/or citations for how the 'synoptic process mean' and 'synoptic process correlation coefficient' are calculated are missing from the paper. It is possible to infer the definition and application of a 'synoptic process' for PM2.5 from table 1 and figure 1, but this should be made more explicit to help readers put the results into the context of previous studies. It is less clear what a synoptic process means in the context of the AANA (tables 3 and 5). Does this comprise the same set of events as for PM2.5, or are these defined based on the intensity of the AANA instead?

3, The definition of synoptic processes for PM2.5 is potentially problematic, particularly with respect to interpretation of the level of confidence to assign to the results. Specifically, the authors should probably (1) define a minimum duration for a synoptic process and (2) allow for brief interruptions in a given synoptic process.

- Following the standard definition of 'synoptic' (see, e.g., Bluestein, 1992) and the composite evolution shown in figure 10, the minimum duration for (1) should probably be at least 12-24 hours (i.e., events should cover at least two reanalysis timesteps, and preferably 3-4).

- The allowance for brief interruptions would help to ensure mutual independence among the data points, given the persistence of meteorological conditions. A decent starting point would be to combine any two events of the same sign with less than 24 hours between them into a single point.

Note that applying these two criteria would effectively cut the sample size in half, which may call some of the statistical relationships into question even before considering

potential changes in the values of the correlation coefficients. Even without these adjustments, tables 4 and 5 appear to be overstating the confidence levels associated with variability in each year, most especially for PBLH.

4, Speaking of PBLH, the small correlations here may be in part due to the use of PBLH values from ERA-Interim, which are based on a Richardson number formulation that tends to underestimate PBLH and its spatiotemporal variability (e.g., von Engeln and Teixeira, 2013). Other work suggests that the tendency for ERA-Interim to underestimate PBLH may be less of an issue during winter over this part of China (Guo et al, 2016), but a close look at their results still suggests that there may be issues in capturing the day-to-day and diurnal variations that this study relies on. If the statistical relationships do not hold up, it might

5, The explanation for the relationship between vertical motion and the BL temperature inversion ("ascending motion inhibits invasion of cold air from the upper atmosphere ... propitious to the formation of thermal inversion layer in the lower level"; l.183-184) seems counterintuitive. One would expect mid-tropospheric subsidence and associated adiabatic warming to more effectively promote the development of an inversion layer at the BL top, as opposed to ascent. This might be reconciled by considering the north–south slope of isentropic surfaces in this mid-latitude region and how AANA-related variations in omega project onto the cross-isentropic component of the horizontal flow, as hinted by the authors around l.157-159 (concerning the role of horizontal advection in strengthening the temperature inversion). Perhaps composite analysis of the temperature budget at 925 hPa would help? Either way, this point requires further discussion and clarification.

6, Some of the secondary conclusions are not well supported; e.g., the statement that "severe haze had the tendency of becoming more persistent in recent years" (l.107-108) based on only three years' worth of December data.

7, The text is readable and understandable, but some word choices are not quite ap-

propriate and the text would benefit from editing for English. Please see technical comments below.

Specific and technical comments:

l.13: 'conductive' → 'conducive' (see also l.230)

l.22: this sentence implies that increased moisture is responsible for weakening turbulence – is this the intended meaning?

l.22: suggest 'were' → 'often'

l.29: 'the characteristics of' could be removed; also, the meaning of 'wide range' here is not clear – large spatial extent?

l.33: suggest 'for' → 'via'

l.35: 'increasing frequency' – does this statement still hold true after the winter of 2017-2018?

l.43: 'effect' → 'effects'

l.57: 'the weaker' → 'a weaker'

l.59: 'the' not needed before 'anticyclonic anomalies'

l.74: 'of' → 'from'

l.81: 'created' → 'applied'?

l.100-101: 'pollutions ... were' → 'pollution ... was'

l.113: 'negative patterns' – negative patterns in what variable?

l.118: 'cold air stayed inactive' – suggest something like 'cold air intrusions were suppressed'

l.121: what are meions?

l.121: 'in' → 'over'?

l.122: 'SLP in the Western Pacific was a positive anomaly' → 'SLP anomaly in the Western Pacific was positive'

l.123: 'southeaster' → 'southeasterly winds' (see also l.152, l.241)

l.126: 'activity' → 'incursions'?

l.135: 'mentioned' → 'aforementioned'

l.136: what is the intended meaning of 'marked' here? maybe change to something like 'a key circulation pattern influencing severe haze in the BTH region'?

l.145: 'from the horizontal direction' → 'in the horizontal dimension'

l.152: 'mountain' → 'mountains'

l.154: 'from the Western Pacific to the BTH region via Bohai Bay' might help to make the connection clearer for readers less familiar with the local geography

l.182: remove 'Actually' – would also be helpful here to make the connection between warm advection and humidity more explicit in the text, since the reference is to dry air intrusions rather than cold air intrusions

l.199: I am not sure 'upper troposphere' is the appropriate term to use here – perhaps 'free troposphere' or just 'higher levels' would work better? (see also l.244)

l.207-208: this sentence ('ascending motion in the lower level declined') appears to conflict with the conclusions in the previous paragraph ('the AANA generated ascending motion in its rear' and following sentences), as well as figure 8 which appears to show anomalous ascent extending basically all the way down to the surface – I think the intended meaning may be that the anomalous ascent is weak close to the surface relative to the anomalies in the lower and middle troposphere, but this is not communicated by the current text.

l.219: suggest replacing 'forward motion' with 'eastward propagation'

l.255-256: correlations with visibility are included in several tables, but not really discussed in the text – what in this work supports the contention here that PM2.5 concentrations better represent the characteristics of haze episodes than visibility? should remove or elaborate on this point

l.256: here it might be worth reiterating the connection between EAWM and AANA, since the latter is the focus of this work (e.g., something like '...stronger AANA, corresponding to a weaker EAWM...')

l.264-269: any speculations on why the statistical relationships were confined to the lower tropospheric components of the AANA in 2015? ENSO influence on the mid-tropospheric circulation perhaps?

l.282-283: here again the question: why were severe haze/non-haze events limited to December 2014–2016 here? acknowledging that the air quality measurement network is relatively recently deployed, are data unavailable for this region in other winter months, or for the most recent winter?

Table 2: even with $n = 38$, the correlation with PBLH does not reach the critical threshold for 99% confidence (0.41) – are sample sizes being counted differently?

Table 3: should clarify the definition of synoptic processes for AANA

Tables 4-5: confidence levels again appear to be overstated here, particularly for PBLH, again raising the question of how the number of degrees of freedom in these tests is specified

Fig 2: it is basically impossible to make out the contours for surface air temperature anomalies in (b) and (d) – suggest moving them to fig 3 or removing them entirely.

Fig 8: the PBLH anomalies are potentially misleading when plotted like this against the deeper circulation anomalies, especially without more information regarding the typical

location of the PBLH. could the anomalies over BTH specifically perhaps be moved to figure 7 (maybe using a linear scale in pressure rather than log-p to increase the vertical space near the surface), marking mean positions for the PBLH during haze / non-haze episodes as red / blue horizontal lines? this would also help to put the thermal advection in the context of the boundary layer depth, which may help in explaining the TIP changes relative to vertical motion changes.

Title: recommend removing 'the' before 'anticyclonic anomalies'

References

Bluestein, H. B.: Synoptic-Dynamic Meteorology in Midlatitudes. Vol. I: Principles of Kinematics and Dynamics. Oxford University Press, New York, 1992.

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