

Interactive comment on “Multiple technical observations of the atmospheric boundary layer structure of a red warning haze episode in Beijing” by Yu Shi et al.

Anonymous Referee #2

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Within this study, the authors discuss a weeklong period when there was extremely poor air quality in Beijing in December of 2016. The authors use data from a 325-m tower, a backscatter lidar, 12-hourly radiosondes, and a wind profiling radar to analyze the atmospheric boundary-layer structure during this time frame. Boundary-layer heights are determined from the lidar, wind profiler, and radiosonde, which are inter-compared and used to evaluate how the height changes depending on the air quality. The authors also find that the TKE and u_* are inversely correlated with PM_{2.5} concentrations.

Unfortunately, there are numerous major concerns with this study which are listed be-

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low. Furthermore, there is little if any new material in this paper. The authors are using well-established techniques to determine the PBL height, and some are improperly applied. One key finding is that the aerosols hygroscopically grew as water vapor increased, but this has been documented in many previous studies (e.g., Svenningsson et al 1992, Chuang 2003, Pan et al 2009). Additional key findings are that the PBL heights were lower during polluted days and that turbulent activity is inhibited on these days, which have also been thoroughly documented (e.g., Schäfer et al 2006, Quon et al. 2012, Petäjä et al 2016) The material in the paper may appear novel since the authors do not relate their results and findings to previous studies in their paper. Given the numerous concerns with the manuscript listed below and the aforementioned limited novelty of the study, I unfortunately cannot recommend this paper be acceptable for AMT.

Major comments:

a) In Sect. 3.3, the authors use the maximum gradient in the potential temperature to identify the PBLH in radiosonde data. This method is only appropriate in the convective PBL (as in the cited Hennemuth and Lammert, 2006 study). Based on the profiles shown in Fig. 4, the PBL was stable and this method is inappropriate to determine the PBLH. Seidel et al (2010) further discusses how the PBLH should be determined in a SBL and the problems of using the maximum gradient in potential temperature in these conditions. It's also important that the authors clarify what they mean by the PBL height. Is the layer that is currently being mixed as it interacts with the surface the desired layer? This is often very shallow in the SBL. Alternatively, do the authors mean the height of the residual layer (from the previous day's CBL)? If the later is the case, then the potential temperature method may be appropriate. However, it must be noted that this height (top of residual layer) is not where pollutants are actively mixed, which is the desired quantity for air pollution studies. See also Keller et al (2011) for a method in which the SBL height can be calculated from a temperature profile.

b) In Sect. 3.4, the authors identify the PBLH as the maximum in the wind speed. While

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this method has been used before (as the authors note in the cited studies), it is only appropriate for identifying the PBLH when a low-level jet is present, which there is not during much of this study. Instead, the authors should use a more appropriate method for determining the PBLH from a WPR, such as Cohn and Angevine (2000) or Bianco et al (2002). It is important to consider that WPR are often currently unable to measure the PBL height in the stable boundary layer due to the PBL being shallow (below the minimum range gate) and there is rarely a refractive index maximum signature. If a different more appropriate method is not used, then the WPR PBL height measurements should be removed from this study.

c) This study could benefit from a discussion of the synoptic and mesoscale meteorology. Based on Fig. 2, it looks like there were cold fronts (strong NW winds) on both 12/15 and 12/22 which advected pollutants away and resulted in air quality. Between the fronts, the PM_{2.5} slowly increased as the air was stagnant (weak and variable winds in between) and pollutants emitted locally likely slowly built up. The wind direction also seems to be cyclical every day, perhaps this is in response to a local mountain-valley circulation around Beijing? This warrants more investigation. It appears that these factors, namely the stagnant air, dominate over the PBL processes in resulting in the poor air quality.

d) While the authors provide numerous references early in the manuscript, there is little-to-no discussion of how the key findings and results compare to previous studies. For example, how does this haze episode compare to other poor air quality case studies in Beijing and other areas, considering both meteorology and boundary-layer processes? The authors should relate the hygroscopic growth to that observed in other studies (there are many). There have also been many other studies comparing lidar PBL heights with those from radiosonde and/or wind profilers, mainly focused on the convective boundary-layer where the top of the boundary-layer is clearly defined. These studies should be related to as well.

Minor comments:

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a) P. 1 line 13: 'Turbulent activities were greatly inhibited during haze pollution'. This must be rephrased, as the evidence in the paper does not support this statement. It is unclear if the haze suppresses turbulence, or if weak turbulence results in poorer air quality.

b) P. 2, line 8: Please define all the acronyms, such as for URBAN, MIAGE, and SURF (similarly to how COST was defined and spelled out).

c) Figure 1: Add a reference scale for distance on the plot. Currently, this map is not very informative in itself. It would be beneficial to add other details to the map relevant to the study (i.e., other important locations, elevation map, any significant pollutant sources, etc) if possible.

d) P. 3 line 14: It is stated there are 15 platforms on the tower, but only 14 levels are subsequently listed. This inconsistency should be rectified.

e) P. 4 line 16: List out the six air pollutants measured.

f) Table 1: What is the difference between 'heavy polluted' and 'serious polluted'? Describe in the text these categories (and all the other ones). It is unclear which category is worse. These are later defined in Table 2 at the end of the manuscript, but these definitions should be moved to the discussion of table 1.

g) P. 5, line 3: Here, the authors claim that the lack of diurnal variation of temperature and RH is due to heavy pollution. This is plausible, but another reason is more likely. The high RH and low visibility, secondarily combined with the low wind speed, all indicate that fog or low stratus is present. The fog or stratus itself would greatly reduce insolation and daytime heating. This would effect would likely dominate over aerosol effects in reducing solar radiation reaching the surface. The discussion here needs to be modified accordingly. It would also be useful to include a plot of cloud cover somewhere.

h) P. 5 lines 6-7: RH cannot be used to quantify the total amount of water vapor in the

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air, as RH is highly-dependent on the temperature. The increase in moisture discussed here may simply be due to a lower temperature, not an increase in water vapor content. Please use mixing ratio or another conserved quantity.

i) Sect 3.2: Rename this section “Lidar observed boundary layer heights”, as the WPR and radiosonde and not discussed in it. These paragraph needs to be rewritten and better organized. Start by explaining why the extinction profile can be used to identify the ABL height, then lead into three different methods that can be used that will yield different estimates, but also each have their own limitations. Describe each method separately, as currently the description of all three is spread throughout the entire section.

j) P. 7, line 10: What is the dilation of the Haar wavelet? The determined ABL height has been shown to be sensitive to this in many previous studies.

k) Figures 3 and 4: Why is extension near the surface so small? I would expect the extinction profile to be nearly uniform (most of the time, except in the presence of clouds) throughout the entire PBL. This leads me to believe that the overlap correction for the lidar is not correctly determined and/or applied. Also, in calculating the extinction coefficient, is attenuation considered? Also, please state if time here (and throughout the manuscript) is local time or GMT.

l) P. 8 line 8: What is meant by “transformation zone”? This is not a commonly used term. m) P. 8 line 10: This reasoning is insufficient to discount the lidar observations, especially given following concerns with the PBLH determined from both the radiosonde and wind profiler. If the extinction is plotted on a logarithmic scale (which is more appropriate given the large dynamic range that it can vary), a real gradient associated with the PBLH determined by the lidar would likely be more apparent. Hints of this gradient are apparent on the plot now during these clean periods at around 250 m on 12/15 and 12/22.

n) P. 8, line 23: The fact that the PBL was statically stable is unsurprising given the

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timing of the radiosonde profiles at 08:00 and 20:00 local time, which are respectively about 30 min after sunrise and 3 hours after sunset during the experimental period. Thus, a nocturnal inversion would barely erode by 08:00 (if at all, depending on the energy balance as insolation is small) and a nocturnal inversion would have formed by 20:00. Due to the timing, these profiles are not representative of conditions during the day when pollutants are actively mixed. I suspect that if the profiles were during the midday (noon local time or early afternoon) the profiles would indicate instability and mixing. This must be discussed at the very least.

o) P. 8, line 28-29: Based on this description (and the profiles), it seems as though the top of the stable boundary layer where mixing is occurring is at 100 m (not 700 m), while the top of the decoupled residual layer is at 700 m.

p) P.9, line 5: Why would an easterly wind advect water vapor? This is unclear and not supported by the data. A map of the water vapor field at the time would be needed to support this.

q) P. 9 line 6: The authors should provide a reference here for hygroscopic growth, as it has been detailed in numerous prior studies.

r) P. 9 line 8: The profile still appears stable in panel c) as the inversion is not completely eroded yet with potential temperature increasing from the surface upwards, thus the ground inversion is still apparent (although weaker) than in panels a) and b).

s) P. 9 line 11: Why is H_{θ} at 604m? By eye, the maximum in the gradient of the potential temperature (and humidity) is near 350 m.

t) Figure 4: The tick marks on the right-most y-axis in panel c) are not aligned with all the other tick marks, which is confusing and deceptive. Also, why do all the profiles start at around 50 m? Is this height above ground level or height above sea level?

u) P. 9 line 13: Rephrase the sentence “At the inversion layer, it was easier to appear the larger value of the potential temperature gradient”, as its meaning is unclear.

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- v) P. 9 line 16: Again, please provide evidence to support this statement. Perhaps in Fig. 4 a few panels could be added (and a supporting discussion in the text) that show profiles where there is no inversion layer apparent (or it is weak).
- w) P. 11 line 4: The equation for u_*' is incorrect. The $u'w'$ and $v'w'$ quantities should both be squared. Make sure it is calculated correctly in Fig. 6, b as well.
- x) Fig. 7: Please make the colorbar for the wind direction (in leftmost panel) circular. With the current colorscale, wind direction at 359 deg is red and 1 deg is blue, even though there's little difference in the wind direction. It would also be beneficial to provide plots from daytime conditions when the PBL is well-mixed during the pollution episode, as both of the selected time periods are near midnight.
- y) Table 2 (and its discussion): There appears to be little difference between slightly, moderately, heavily, and seriously polluted conditions. There is no clear trend in the listed variables with increasing pollution. I suspect that the calculated small differences are not statistically different between categories, except compared with 'good' air quality. This is unsurprising, given the meteorology remains roughly constant during all periods between times when the air quality is 'good' when pollutants slowly build up. This should be discussed.
- z) P. 15 line 6: Again, there is no evidence in the manuscript that north-easterly winds brought pollutants and water vapor to Beijing. Based on Fig. 2, it looks like this first increase in pollution was the first night when winds were light, thus pollutants likely emitted locally were not dispersed and built up quickly in a shallow layer near the surface.
- aa) P. 15 line 13: Rephrase the sentence 'The turbulent fluxes ... radiation from the ground', as its meaning is unclear.
- bb) P. 15 line 15: How is turbulence suppressed when the sensible heat flux is positive? Normally a negative heat flux with cooling near the surface will strengthen the stability

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limiting turbulence, while a positive heat flux weakens stability.

Editorial corrections:

- a) The manuscript needs heavy editorial corrections throughout.
- b) P. 3, line 12: 49 m above sea level is listed twice in this sentence. Remove one instance.
- c) P. 3, line 13: Why is 'August 1979' here?

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