Response to comments by referee 1

We would like to thank you for your comments and helpful suggestions. We revised our manuscript according to these comments and suggestions.

General comments:
Visibility was used as a proxy for pollution, why not directly PM concentrations ?
The paper is occasionally very hard/heavy to read: page 28264 is a perfect example of the general problem : whole page is filled with lists of numbers and extremely long sentences without any clear structuring, so at the end the reader is definitely losing the whole idea of the text, and it is very hard to see what all those.
Responses:
Thank you for your kindly comments and suggestions.
In order to characterize the degree of the air pollution in Beijing, PM concentration was a good indicator. However, haze is defined by the visibility in China as shown in Table 2 in the revised manuscript (CMA, 2010), and it was remarkably negative correlated with PM concentration (Yang et al., 2015). Considering the occasionally missing data of the PM, visibility was used as an index to classify the air pollution degree.
We are sorry for the bad presentation in our manuscript. In the revised version of our manuscript, several long paragraphs were separated into some small parts or rephrased concisely. Afterwards, a native speaker revised our manuscript to make the language better understanding for the reader.

Specific comments:
Question 1
(25254):
While with most other numbers (like mixing height with 10 cm accuracy?) there are obviously too many decimals given in the paper - with this for some strange reason far to few; 1km accuracy is not normally very acceptable with a station location?
Response 1
Thank you for your suggestions. We have revised the significant digits thoroughly according to your suggestions.

Question 2
(28255):
We defined all of the meteorological sounding profiles as a convection state when they exhibited negative lapse rates for the virtual potential temperature within 200 m and bulk Richardson number within 100 m, and the other profiles were defined as a stable state."
Would it not be useful to consider/define also neutral cases? (See also comment below)
Question 3
• (28258):

"Therefore, the near-neutral atmospheric stratification that occurs when a cold air mass passes through is the main cause for the serious underestimation in the observation results by the ceilometer."

This seems to be a logical conclusion: however, it remains still unclear, was it really verified that these situations were truly neutral? The paper only states that these situations were corresponding to "conditions with low relative humidity and large wind speed, with winds mostly from the north" – but no direct proof on the "neutrality" is given?

Response 2 and 3

Thank you for your suggestions. The neutral condition seldom emerged in the actual environment. However, the mixing layer is near-neutral during strong winds and cloudy day. In our analyses, the overestimations are emerged during strong winds crossing, not all the near-neutral conditions. Otherwise, in the process of the analyses, we found it was very difficult to define the neutral condition because of the influence of the urban canopy, which affected the lapse rate of the virtual potential temperature. Therefore, we used two meteorological conditions as same as the former studies (Eresmaa et al., 2006; Münkel et al., 2007). Although we just used two meteorological conditions, we showed the evidence of the bulk Richardson number is approximately 0 for the near-neutral condition (Fig. S1).

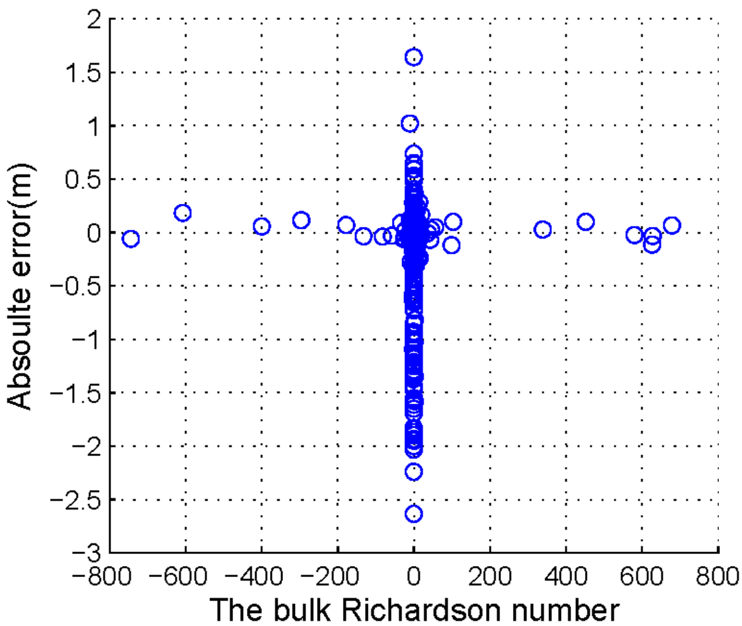

Figure S1 Relationship between the bulk Richardson number and the absolute error of the measured MLH.

Question 4

(28259):

"After determining the reasons for the underestimations and overestimations in the ceilometer data, the results with large errors according to certain principles must be eliminated."

Yes, that is an easy option – much better one would be to correct the mixing height

algorithm so, that it would give better estimates in these conditions- was this approach doomed to be impossible?
Response 4
Thank you for your suggestions. Interpreting data from aerosol lidars is often not straightforward, because the detected aerosol layers are not always the result of ongoing vertical mixing, but may originate from advective transport or past accumulation processes (Russell et al., 1974; Coulter, 1979; Baxter, 1991; Batchvarova et al., 1999). Therefore, improving the algorithm cannot solve the underestimations and overestimations of the ceilometer observations, and the only option to correct the MLH is to eliminate the data with large AE. After determining the reasons for the underestimations and overestimations of the ceilometer results, the elimination is much easier to implement.

Question 5
• (28259):
"The elimination results are good, and this method replaces the time consuming method of filtering the data manually, which is of great practical value for future measurements of MLH with ceilometers. For overestimations, we used the date of dust occurrence based on the sand–dust weather almanac to eliminate the time periods of dust crossing when the ratio of PM2.5 and PM10 suddenly decreases.
Now, it would be extremely important to document what was exactly done here- was the PM-ratio used at all, or just some undocumented "sand-dust weather almanac"??
This method itself seems to be one of the very useful/new things developed, but unfortunately it remains very unclear how this could be utilized in e.g. other locations?
Response 5
Thank you for your suggestion. The sand-dust weather almanac is like a yearbook of sand-dust, which is compiled by the China Meteorological Administration. In order to make this paragraph clearer, we added some description in the methodology section and rewrote this paragraph as follows.
The methodology section:
To identify the sand-dust crossing, the ratio of $PM_{2.5}$ and PM_{10} was used as an index. If there was no sand-dust crossing, the ratio of $PM_{2.5}$ to PM_{10} might almost exceed 50% (Liu et al., 2014). A sudden decrease in the ratio to 30 % or lower and PM_{10} concentration higher than 500 μg m^{-3} usually indicate a sand-dust crossing. The ground observations of $PM_{2.5}$ and PM_{10} during the same period were made by the ambient particulate monitor (RP1400a, Thermo Fisher Scientific, USA). The data were acquired at a time resolution of 5 min and processed with a resolution of 60 min. A detailed description is provided by Liu et al. (2014).
Section 3.1:
For underestimations, the meteorological data were used to eliminate the periods when cold air passed with a sudden change in temperature and WS. For overestimations, we referred to the sand-dust weather almanac to identify the

sand-dust days (CMA, 2012, 2013, 2014, 2015). Using the principal described in Sect.2.1.1, the exact times of sand-dust starting and ending were determined as the times at which the ratio of $PM_{2.5}$ to PM_{10} suddenly decreased or increased, respectively. Finally, the data obtained during the sand-dust periods were eliminated.

Question 6
(28259):
"First, the effectiveness of the data must be verified after performing the elimination by the aforementioned method. The results of the evaluation indicate that the effectiveness of the data in different seasons is significantly negatively correlated with wind speed and significantly positively correlated with relative humidity" "Effectiveness" probably not the best possible label for availability of useful MLH data. The Figure 7 indicates that the above statements may be true, but the text "explaining" the reasons for over– and underestimation fails to give a clear explanation why this is to be expected: the text should be restructured so that the reasons behind Fig.7 would come more clear.
Response 6
Thank you for your suggestion. We have revised the "effectiveness" to "availability". In addition, we also have rephrased this paragraph to make the explanation clearer as follows.
To provide a detailed description of variations in the MLH, we selected continuous measured MLH and meteorological data over a 3-year period (from December 2009 to November 2012). First, the availability was verified after the MLH elimination. The results of the evaluation indicate that the availability in different seasons is significantly negatively correlated with WS and positively correlated with RH (Fig. 7a). For spring and winter seasons with large WS and low RH, the availability is low, whereas for summer and autumn seasons with small WS and high RH, the availability is high. In particular, the availability is lowest in January at 63.5% and highest in June at 95.0%. The successful retrieval of MLH over the 3-year period is approximately 80%, much higher than in a previous study (Munoz and Undurraga, 2010).

Question 7
• (28261):
"To avoid the influence of data elimination on the study, we analysed the relationship between daily changes of the mixing layer and the sensible heat flux and found that the average MLH from 12:00 to 17:00 LT and the sensible heat flux were well correlated (Fig. 8) and had a correlation coeffcient of 0.65, which characterizes the dominant role of radiation in the variations" ? Not really clear what the beginning of the sentence says : "avoid" ? ! radiation is NOT a synonym for sensible heat flux – this should be clear in the text +This would be a perfect place for on equation /reference to some well-known formulas connecting sensible heat flux & MH ? any references to well-known MH formulas!

Response 7

Thank you for your suggestions. We rewrote this paragraph and added the relationship between the sensible heat and MLH. The revised paragraph is as follows.

To gain a better understanding of the reasons for the MLH variations, we use the daily mean instead of the monthly mean to do the analysis. As the simple framework in which we can analyse the MLH variations in Beijing, we consider the thermodynamic model of the mixing layer growth (Stull, 1988), as follows:

$$\frac{\partial z_i}{\partial t} = \frac{\overline{w\prime\theta_s'} - \overline{w\prime\theta_{z_i}'}}{\gamma z_i} \quad (6)$$

where z_i is the MLH (m), t is the time (s), θ_s is the virtual potential temperature near the ground (K), θ_z is the virtual potential temperature in the top of the mixing layer (K), and γ is the lapse rate of the virtual potential temperature (K m^{-1}). Suppose the heat from the ground is the only way to warm the mixing layer and the heat flux at height z_i is zero, then the MLH is related to $\overline{w'\theta_s'}$. Considering that Q_H is defined as equation (1), MLH is correspondingly related to Q_H. Therefore, the relationship between daily changes in the Q_H at 280 m and MLH was analysed. The results showed that the average Q_H and MLH from 12:00 to 17:00 LT were well correlated, with a correlation coefficient of 0.65. Because net radiation (Q*) should be balanced by the Q_H, Q_E, and soil heat flux (Q_G) given as follows (Stull, 1988):

$$Q^* = Q_H + Q_{HE} + Q_G \quad (7)$$

the strong correlation between the Q_H and MLH proves the dominant role of radiation in the variation of MLH (Fig.~8). This proves the dominant role of radiation in variation of MLH (Fig. 8).

Question 8

(28262):

“Two components are closely related to turbulent energy: the heat flux caused by radiation and the momentum flux generated by wind shear” Why not simply state/show how (equation?) how these are EXACTLY related ?

Response 8

Thank you very much. We have added the equation in the revised manuscript.

Question 9

(28263):

“In summary, the mountainous wind in summer causes the mixing layer to gradually decline at night, which suppresses the development of the mixing layer before noon, and the prevalence of plain winds after noon causes the mixing layer to increase rapidly. Therefore, this regional circulation leads to the concave-down variation in the fast development stage of the mixing layer in summer compared to the spring. <- Ref Figure 9. Figure 9 does NOT really show significant differences between MLH spring vs summer? So the long discussion & strong statements seem to be not justified? Or is there a real reason why even those small differences are so significant/important?

Response 9

Thank you for your suggestions. I am very sorry for the misunderstanding because of language. In order to make this section clearer, we have rephrased this section and added some new analyses. The daily MLH range is 728, 828, 562 and 407 for spring, summer, autumn, and winter, respectively. It exhibits significant differences between spring and summer. If we do the T test for this two seasons, we find the differences in the growth rates are significant ($P<0.05$). Therefore, we revised this section as follows.

Question 10
(Fig10):
fraction velocity ->friction velocity
Response 10
Thank you for your suggestion. We have fixed it.

Question 11
• (28265):
Equation 1/the only equation in the paper.
If you present an equation you should explain all the terms, not just virtual(?) potential temperature Describing those all would probably soon also reveal that differentiating gravitational constant is not generally a good idea (<-error in at least in the buoyancy production and depletion term + dividing rho should be rho0..in the 3rd term ?)
Rersponse 11
Thank you for your suggestions. We have added the explanations of the other parameters in the equation and revised the equation as follows.
In order to verify these results, we examined the TKE budget equation. If we presume a horizontal average and neglect the advection of wind, then the forecast equation of the TKE can be written as follows (Garratt, 1992):

$$\frac{\partial \bar{e}}{\partial t} = -\overline{u'w'}\frac{\partial \bar{u}}{\partial z} + \frac{g}{\overline{\theta_v}}\overline{w'\theta_v'} - \frac{\partial(\overline{w'e})}{\partial z} - \frac{1}{\rho}\frac{\partial(\overline{w'p'})}{\partial z} - \varepsilon$$

where θv is the virtual potential temperature, g is the acceleration of gravity (m s^{-2}), ρ is the air density (kg m^{-3}), u is the horizontal velocity (m s^{-1}), w is the vertical velocity (m s^{-1}), p is the air pressure (Pa), z is the height (m), e is the TKE (m^2 s^{-2}), ε is the dissipation term of TKE (m^2 s^{-3}).
Question 12
• (28265):
"The turbulent transportation term does not generate or destroy the TKE, and it just moves the TKE from one position to another position or redistributes the TKE. This term remains constant at zero in the entire mixing layer" Well, the first sentence reasonably correct, but don't quite get the meaning of the second one? Sounds like you claim the second term to be zero everywhere?
Response 12
I am sorry for the misunderstanding. "This term remains constant at zero in the entire

mixing layer" have been revised to "the integral of this term in the mixing layer remains constant at zero."

Question 13
(Conclusions):
More general on the conclusion "The presented results on the atmospheric mixing layer and its thermal dynamic structure under different degrees of pollution provide a scientific basis for improving the meteorological and atmospheric chemistry models and the forecasting and warning of atmospheric pollution."
My first impression is that "scientific bases" is not the correct term here-maybe more like "useful empirical information " or something similar: the paper does not really present any new parameterizations or models , not probably even directly supporting that.
Response 13
Thank you for your suggestion. We have fixed "scientific basis" to "useful empirical information".

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