

Review of revised version

Mixing layer height and its implications for air pollution over Beijing, China

by Tang et al.

The paper has been improved considerably: it is much better structured and more precise than before. The "acp-2015-691-author_response-version2.pdf" demonstrates that nothing remains as it had been. The point-to-point response gives further useful insight, so only very few questions remain to be clarified.

So, in spite of the improvements, I recommend to address the points listed below to make the paper more convincing. The first part of the paper dealing with the retrieval of the MLH is certainly easier to optimize; more critical is the second part, when MLH is linked to other parameters. This is due to the fact that there are several parameters (wind field [local and regional], radiation, aerosol chemistry including hygroscopic growth, and more) interacting with the ML at local and regional scales. Consequently, the discussion must often remain qualitative as it was not possible to measure all parameters at all relevant sites and to investigate what is the reason for what. With respect to the first part the authors assume that the MLH determined by BL-VIEW is correct under all meteorological conditions (which might not be true – I don't suggest this, but it could be). As the complete discussion of the second part is however based on this assumption, the first part must be very clear.

As a consequence I recommend to explicitly mention in the conclusions that more studies are required to check whether the conclusions of this paper are generally valid, and to include short comments (see below) whenever a statement/conclusion is uncertain or the validity might be limited.

I feel that all figures/tables except Fig. S1 are worthwhile to be included into the main manuscript.

After consideration of these comments, the paper can be published.

Two main points (in the point-to-point response useful information is provided but the issues are not fully solved).

1. The MLH-error

- Page 9: "In this study, we used the gradient method...". This sounds as if the authors have developed an own retrieval. This is however not the case: they have used BL-VIEW provided by Vaisala. This should be clearly stated here. If there are different

versions of this software or if there are different parameters that can be changed by the user, the selected configuration shall be mentioned here. The reason is, that this selection might influence the retrieved MLH and thus, that the complete discussion might depend on this selection. So whenever the authors come to a certain (critical) conclusion I recommend to mention that this conclusion is based on certain assumptions (e.g., that the MLH is accurate within 100 m). As an example: in case that the ceilometer fails to retrieve the MLH (too low aerosol backscatter) or if the "wrong" aerosol layer is assumed to be the MLH, then the calculated correlations between MLH and other parameters (second part of the paper) could be erroneous. But I agree that it is beyond the scope of this paper to review MLH-retrievals by ceilometers.

- Page 10: It still remains unclear what the "absolute error (AE) of the measured MLH" is. The reader can understand it either in the sense of "accuracy of BL-VIEW" or as "the difference between $MLH_{sonde} = MLH_{reference}$ and MLH_{ceilo} ". Both errors exist, but the authors only understand it according to the second option. Maybe one sentence can be added to avoid any misunderstanding.

By the way: I would avoid the acronym AE, first, because it is normally used for Angström exponent, and second, because it is not necessary.

- Page 11: "Once the aerosol concentration becomes uniform in the vertical direction, the ceilometer cannot calculate the MLH through sudden changes in the attenuated backscatter profiles, resulting in serious underestimations." I don't understand this: If no MLH is retrieved from the ceilometer, why is this an underestimate? Is the MLH set to zero? This would not make sense. Please explain (maybe when BL-VIEW features are discussed in section 2.2.1).
- Page 13 (and many similar cases): "at 739 ± 137 m": it must be explained what the second number means and how it is calculated. Is it the error of the quantity, is it its temporal variability or something else.

The expression "consistent" (twice on page 13) is qualitative only; a more precise description would be welcome.

2. Potential over-interpretation

- Fig. 9a; Page 15: In the point-to-point response it is stated that the gradual decrease of the MLH in summer is really significant

(in a statistical sense). However, a problem remains, when the trend is smaller than the uncertainty of the MLH-retrieval itself. This might lead to an over-interpretation of the data. I am not saying that the observed trend is not real, but one has to be cautious. This point should become clear when the above mentioned amendments has been made, or a brief remark might be added.

- Page 20: The paragraph starting with "In our data, scatters under poor..." is still sort of difficult to understand (what is scatters?), is partly relying on assumptions/speculations and consequently can be omitted or drastically shortened. If the data on the chemical composition are not measured or available, they should not be used in the discussion. Otherwise this might lead to another over-interpretation, in particular as the uncertainties of the MLH are not well characterized in this paper (see previous comments). This make the paper "vulnerable" and rather weakens than strengthens the benefit. So I recommend to skip everything that cannot be proven by real facts/measurements.

Having this in mind the whole section 3.4.2 should be checked for potential optimizations.

- Page 21: "the critical threshold for the MLH...". I still don't understand what is meant and how it has been derived. I see from the new table, that at relative humidities larger than 80% the correlation between MLH and visibility suddenly becomes very high, but I don't see how the "critical thresholds" are derived. Please explain in more detail and in a conclusive way.

Minor and technical points

- Fig. 1: I am not sure if this is really important but I feel that the (city limits) of Beijing should be marked, in particular as it is referenced on page 16.
- Page 5: "in the morning (00:00UTC) and at night (12:00UTC),..." Here, local time should be given (it is however given later in the manuscript). This is also a not-mandatory change.
- Eq. 3: Symbol u_* is not explained here
- Page 8: "10 km from the station of the Institute of Atmospheric Physics". Suggestion: replace with "10 km from the BJT-site".
- Page 9 (Section 2.2.1): I am not sure if it is worthwhile to mention that the authors are aware of the water absorption issue (see point-to-point response).

- Fig.2: It is still hard to find to which dot a number (PM-ratio) belongs (see my first review and point-to-point response). The meaning of the numbers should be mentioned in the text.
- Fig.5: It is still unclear how the authors retrieve 1600 m, the derivative of the backscatter profile rather suggests 1500 m. Add a vertical line where you assume the MLH.
- Fig. S2 (and more): Dots are much too large; it is impossible to infer MLHs from them. And include grids to facilitate the reading (see above)! This is also true for Fig. 7b, though it is only possible for the left axis (height). I appreciate that many figures have been improved in this respect.
- Eq. 7: It is only used in Section 3.4.1. So maybe it should be moved to that part of the paper (not mandatory).
- Fig. 8: Add the symbol Q_H in the figure caption.
- Page 14: Second line below Eq. 6: should be Θ_{z_i} or (to be fully consistent) Θ_{v,z_i} .
- Page 15, first line: "The daily MLH range is 728": range is meant as "diurnal amplitude"?
- Page 17: "(Tab. 2 and Fig. 10)". It should be mentioned that it is the same information but the figure has a better resolution with respect to visibility.
- Page 17: "during light haze pollution the RH significantly increased to 63.1 %". Must be slight haze!
- Page 17: End of paragraph starting with "The measured values under light..." can be drastically shortened (refer to the table).
- Page 18. "As shown in Fig. 10d, ..." Should be "as shown in Tab.2" and can be drastically shortened.
- Page 19: In the text a poor correlation of 0.08 is mentioned whereas in the figure a correlation coefficient of 0.72 is shown. The difference is explained later but here it is misleading.
- Page 19: the "ventilation coefficient". Is this quantity introduced by the authors or it is a common parameter (if so, give a citation).
- Page 19: "However, although the atmospheric...": This sentence is hard to understand. Please rephrase.