Authors' response to reviewer #2

We thank reviewer # 2 for carefully reading our manuscript and the provision of many useful comments and detailed suggestions. We have considered all comments. They gave as useful hints where improvements of the paper were necessary to better understand our methodology and conclusions. Below all points raised by the reviewer are repeated; our comments are added in italics.

The changes (revised version vs. AMTD-paper) are highlighted as displayed by latexdiff ("diff.pdf", maybe renamed when uploaded as a supplement). For the sake of clarity only small changes are explicitly mentioned in our point by point replies, otherwise we refer to the corresponding parts of "diff.pdf" (in blue). Note, that some of our responses interact with comments of the other reviewers, so sometimes it is difficult to refer a change to one specific reviewer's comment.

Point by point replies

General comment

[...] Overall, the paper is well written and easy to follow, but however needs some more critical discussion on certain points.

In my point of view, using just one ceilometers/location might not be sufficient to answer the question given in the title. It is clear, that it is difficult to extent the study to other locations at that stage, but however, this aspect should be discussed more in detail. As highlighted by reviewer 1, I share the opinion that a day/night comparison might be interesting.

For meteorological conditions being a main driver of turbulent mixing it might be interesting to include some meteorological observations characterizing the measurement location and selected study period. With a observation height of about 5 m it might be interesting, which amount of the measured concentration is originated from the actual location and which amount is advected from neighboring areas or "removed" by vertical mixing. Here again a night/day difference would be interesting. How does this aspect influence on the analysis at one selected point?

→ *The first concern of reviewer #2 is the limited number of ceilometers and the missing discussion of day-night differences. This was also one*

of the major criticisms of the first reviewer (please see our reply to reviewer #1 as well).

We agree with reviewer #2 that more ceilometers would have been beneficial for the study. It was however not possible to set up several ceilometers and/or to use mobile systems. Up to now ceilometers do not belong to the standard equipment of urban air quality networks, maybe this will change in future. So we had to rely on additional resources (in the framework of a campaign) with the inherent limitations (e.g. temporal availability).

Nevertheless we believe that BAERLIN2014 provided very valuable scientific results even if there was only one ceilometer available. We were able to demonstrate how differences in MLH-retrievals play a role for calculating correlations between MLH and air quality parameters. By addressing standard retrievals (the proprietary software of the ceilometer manufacturer) and air quality measurements from an official monitoring network we think that the conclusions are relevant. Based on our research, open questions could be identified one of which being the need for investigations of the variability of the mixing layer over a large municipality. So we hope that in future the wishes of the reviewer (and ours) to have more ceilometers and at least one full annual cycle of the MLH can be fulfilled, and that our paper will be a motivation for setting up the corresponding infrastructure (see also our replies to the detailed comments of reviewer #2 below).

As described also in the reply to reviewer #1 our conclusions on the large spatial variability of correlations between MLH and PM_{10} are confirmed if we restrict our discussion to the stations nearby (less than 6.4 km from the ceilometer site). Over this small spatial domain the representativeness of a single MLH retrieval is very likely. Our discussion on the correlations between MLH and NO_x-concentrations also remain valid when focussing only on the vicinity of the ceilometer site.

As a consequence we have added a new paragraph (see pages 23–24 of diff.pdf) and more references (see page 9 of diff.pdf). Following the suggestions of reviewers #1 and #2 we have extended Sect. 5.1 by discussing day-night differences and the influence of the wind field (see comment of reviewer #3) (see pages 24–26 of diff.pdf). In addition a short comment on differences between working days and weekends has been added.

A detailed discussion of the influence of the local sources to measure-

ments in 5 m height is beyond the scope of this paper (see the clarification of our objectives in the introduction: page 3, line 9 ff of diff.pdf): such small scale investigations require much better temporal and spatial resolution of the measurements (and associated models). For example, station #42 used for the BAERLIN2014 project as reference is being classified as urban background station. This determines major pollution sources such as major streets to be not within the direct surrounding area (>100 m) and includes usually residential areas. Therefore minor sources like smokers, restaurants, barbecue, and household sources determine the moderate emissions in the vicinity. A moped or car passing the station for a short period of time is not detectable in an averaging period of one hour. Note, that the altitude of the ceilometer (5 m above ground) is not relevant for the determination of the MLH (see also comment on "p.8 line 1" below).

Specific comments

- p.2 line 3-4: Is there evidence in your study? Otherwise put this sentence in the introduction or conclusion.

  → *From our point of view this message is important. That was the reason why it was included in the abstract. Note, that we have written "seems to be unrealistic ... a city like Berlin". It is not meant as a statement that is valid for all metropolitan areas worldwide at any time (for this we indeed do not have evidence); e.g., for cities surrounded by (high) mountain ridges or extreme pollution episodes the situation might be different. The sentence should be understood as a "warning" not to over-interpret correlations between MLH and concentrations of pollutants. To make this clearer we have modified the sentence in the following way: "seems to be unrealistic ... a city like Berlin (flat terrain)", and we have extended the introduction to better explain the scope of the paper (page 3 of diff.pdf).*

- p.2 line 19: measurements, data instead of techniques

  → *Improved*

- p.2, line 27: box models

→ *Corrected*

- p.3, line 16: COBOLT: add one sentence highlighting novelty, functionality

  → *We have added a much more detailed description of COBOLT according to the suggestion of reviewer #1, see pages 11-14 of diff.pdf.*

- p.3, line 19: aim to instead of may

  → *We don't want to change this. The reason is that our study aims to show the influence of the retrieval on the derived MLH and the heterogeneity of the concentrations and thus may help the user to draw conclusions. We don't aim to show a link between air quality and MLH because there are more variables than just the MLH that control pollutant concentrations (see several comments of all reviewers and the statements in our manuscript). However, we have substituted "assess" by "interpret" (page 4, top, of diff.pdf).*

- p.4, line 1: specify "active remote sensing networks" (e.g:...)

  → *We have added (e.g. the above mentioned ceilometer networks); page 4 line 19 of diff.pdf. This refers to the introduction where we have added (e.g. almost 100 instruments by the German Weather Service) (see page 3 of diff.pdf).*

- p.5, line 15: chemical processes? What about Ozone? Where does it come from – downward mixing, secondary formation?

  → *In this section of the paper (introduction) we give an overview over previous publications that are relevant for our study. In the Schäfer et al. (2012) paper ozone was not considered, thus, it is not mentioned here. However, later in our paper we discuss this issue (Sect. 5.2): downward mixing, destruction of ozone by sometimes high NO_x concentrations, production of ozone when NO_x levels are low because of the notable amount of green spaces (parks, forests and leisure areas), or ozone formation by photochemistry (page 27, lines 17 ff, of diff.pdf)*

- p.5 line 28-31: This is not part of your analysis and could be moved to the conclusion.

→ *Thanks for this remark: we agree and delete this part, as these ideas have already been included in the conclusions (so it was sort of a duplication).*

- p.6, line 9: specify "secondary material"

  → *We changed "secondary material" to "secondary aerosol compounds", see page 6, line 30 of diff.pdf*

- p.6, line 14: hourly measurement

  → *Corrected*

- p.8, line 1: how representative is the measurement location in 5 m height for near surface pm10 concentration? How does this impact the representativeness for the MLH measurements for this area?

  → *The ceilometer was installed 5 m above the ground. For the determination of the MLH a change of the altitude of the ceilometer in the range of a few meters is not relevant. Concentrations are measured at the BLUME stations approximately 3.5 m above ground. These values are expected to differ from measurements directly at the curbsite (see Bonn et al., (2016)). The latter might show much higher temporal fluctuations (e.g., passage of a car). Such microscale effects are not considered when correlations with the MLH are investigated. To resolve these problems certainly models at the building-resolving scale help. Moreover, during the transport from e.g. major traffic sites to the reference location strong vertical gradients will be smoothed (see also reply to "General Comment"). We have now briefly touched the topic of "scales" in the conclusions (page 31 of diff.pdf).*

- p.16 Figure 5: legend has to be added

  → *Done*

- p.17 line 2 ff: this chapter defines the scope of the study and in my opinion appears to late in the manuscript which results in a misbalance between introduction/methods and results. The first part until 5.1 is more an introduction to a new topic than a presentation of results. I might be helpful to include some of these aspects in the introduction

(without changing the whole manuscript). Line 2: Ozone and NOx also measured at BLUME?

→ *Thanks for this suggestion. Indeed this paragraph does not fit here very well. We have completely rephrased and re-arranged this paragraph. We removed text that was not relevant for our study. We moved the modified text to section 3 (new caption: "The BLUME network and the BAERLIN2014 campaign"). Now, all information related to the underlying data sets are combined in one section. We have also explicitly mentioned the distance of the BLUME-stations to the ceilometer as this is an essential point in view of the correlations discussed later (end of Sect. 3, page 9 of diff.pdf).*

Ozone and NO_x are also measured by the BLUME network. This becomes clearer after moving text from Sect. 5 (introductory remarks) to Sect. 3, see above (from page 21 of the AMTD-version to page 7 of diff.pdf).

Moreover, we introduced a new paragraph to the introduction to make the scope of our study more clear (see also reviewer #1; page 3 of diff.pdf).

- p.18 line 8: it is unclear on which basis the median was calculated. 67 measurements each hour at every station?

  → *Yes, this is true for the concentration measurements at the BLUME-stations: the temporal resolution is one hour, and the whole measurement period of 67 days (i.e., when co-incident PM_{10} and MLH measurements were available) is considered. With respect to the MLH we rely on all available 10-minutes retrievals (up to six, depending on the MLH-retrieval) of the corresponding hour, for all 67 days. So, up to 402 MLH-values are considered for the MLH-median. An new paragraph has been added to the end of Sect. 4.4 (pages 19–20 of diff.pdf).*

- p.18, line 16-20: can you proof your assumptions by adding meteorological observation here? Is there a secondary circulation generated by the Urban Heat Island? Please specify the term "meteorological interpretation".

  → *We are aware that we missed to clearly outline the scope of the*

paper as necessary. We have corrected this now by adding a new paragraph to the introduction (page 3 lines 10 ff of diff.pdf).

In this context the term "meteorological interpretation" should be understood as the interpretation of processes that control the development of the mixing layer and surface concentration – and their interaction. A thorough discussion of the meteorological reasons and atmospheric chemistry responsible for the observed distribution of pollutants was however not the goal of the study. Nevertheless we have included several comments to point at reasons for poor or unexpected correlations.

Finally we want to emphasize that we present diurnal cycles of MLH and concentrations averaged over 67 days. The analysis of the interactions between meteorological fields (e.g. wind), atmospheric chemistry and emissions should rather be carried out with a high temporal resolution. This analysis would certainly benefit from a "complete" set of observations. Such a data set is however unrealistic. Thus, tentative answers may be found by numerical models. But models do not necessarily display proof of understanding or concepts but provide a further tool and support understanding. Anyway, such investigations are far beyond the scope of this paper (see a short comment in the conclusions page 31 of diff.pdf).

- p.20, line 31f: see comment above

  → *See previous reply*

- p.21 general: here you mention briefly the problem of point measurements. This aspect could be further discussed. It is interesting if there is a mismatch between the timing of MLH and air quality observation. Does a low MLH mean a high concentration at the same time? What is the order of the processes? Where do meteorological conditions come into play?

  → *The answer to this question is closely related to the previous replies. It is not unlikely that a temporal delay between MLH and concentrations might occur, however, this delay is influenced by e.g. the wind field (upwind/downwind, low/high wind speed) or specific characteristics of the traffic (emissions). In case of secondary produced constituents it depends on the concentration*

of the precursors and the solar irradiance. These influences are certainly time dependent, so it is hardly possible to detect them when long temporal averages are considered as in our study. We have briefly discussed the influence of the wind speed at the end of Sect. 5.1 by adding several paragraphs and Table 3 (pages 24–26 of diff.pdf).

- Chapter 6: It is not the extended mixing layer itself which is the initial precursor of dilution of pollutants near the ground. Several processes interact which each other which might as well lead to an extension of the mixing layer height.

  → *We agree with the reviewer. We have mentioned the complex distribution of pollutants several times in the paper. Our goal was to compare this complexity with different schemes to determine the MLH from ceilometer data.*