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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "On the spatial variability of the regional aerosol distribution as determined from ceilometers" *by* Matthias Wiegner et al.

Anonymous Referee #3

Received and published: 22 July 2020

The manuscript by Wiegner et al. has the objective to assess the value of using the Mixing Layer Height (MLH) and the Integrated Backscatter (IB) in MLH vertical region, both derived from a network of laser ceilometer operating at a regional scale, to quantify the spatial variability of the aerosol distribution.

The authors present an extensive analysis mainly based on the correlation among spatial differences of the MLH and IB values for two data clusters collected in the two metropolitan areas of Munich and Berlin, using a sufficiently long dataset (2 years approximately depending on the applied data filtering options discussed in the manuscript). For both the clusters, an empirical approach to filter out potential outliers, due to extremes or supposed unphysical values, is also applied and meteorological conditions (i.e. cloudiness and cloud types) are taken into account into the data analy-

sis.

The authors conclude the manuscript asserting that MLH is a more homogenous variable than IB at a regional scale. Discussion on the data homogeneity is based on the spatial variability of both the parameters and on the related uncertainties (also for different retrieval schemes).

The authors' conclusions are a bit vague because they should provide recommendation on the usage of one variable or another with respect to specific applications and this cannot be done without considering the total uncertainty budget for each variable.

I must admit that the manuscript title and the spirit of the manuscript, which seems to investigate in parallel MLH and IB with aim to show which is the best variable to use for assessing the aerosol spatial variability, creates in the reader the expectation to have a final recommendation on which is the best variable to use for investigating the aerosol spatial distribution. My opinion is that different applications needs different input variables. It is undoubted that MLH is a fundamental variable to provide as input to air quality models, for example. Nevertheless, needs of satellite data or forecast models may be different.

I think that the paper could be a bit restructured to become an assessment of what a ceilometer network can or cannot do for the study of the aerosol spatial variability. May be the authors meant to write the paper in this way but an external reader may have a different perception.

Some of the considered aspects are not new for example the uncertainty affecting the retrieval of the IB due to the low signal-to-noise of the ceilometer and to the instability of the calibration constant over the time. These have been already discussed in literature. Similarly, the ceilometer accuracy in capturing the aerosol geometrical properties, such as the MLH, in opposition to the larger uncertainties of retrievals of the aerosol optical properties is already known from the existing literature.

Therefore, my main point for the authors is to adjust the manuscript, in particulate the abstract, the introduction and the summary and conclusions following a different and more concrete style, always referring to the total uncertainty budget and stressing that different variables may be useful for different applications.

A second point is that the authors must also motivate why they use the IB only instead of profiling measurements for their data analysis. To my opinion, the investigation carried out for the IB must be extended to the backscattering profile or to the attenuated backscatter which although not fully quantitative aerosol optical property, is often used for the satellite validation (e.g. CALIPSO data) and also recently used in forecast models and atmospheric reanalysis.

I report below other general and specific comment (line-by-line) afterwards:

I report below other general concerns and specific comment (line-by-line) afterwards:

1. Several times in the manuscript the uncertainty of the MLH and IB values is discussed, and this is a great aspect. Nevertheless, the estimation of the uncertainty for the MLH is mainly related to the algorithmic uncertainty: it seems that in the applied algorithm there other uncertainty contribution which are neglected or not properly considered. For example is the algorithm working on the 15 m raw resolution data? Is an uncertainty of half of the raw resolution, at minimum, used in the MLH estimation? At page 7, lines 8-10, it is written that ".... For this purpose the MLH is smoothed in time ....". Is this smoothing considered in the uncertainty quantification? Which kind of smoothing is applied and it is one of the main reason why the MLH is very homogenous? I think the authors must clarify these aspects.

2. Outliers are considered and treated in the data analysis, very often just using empirical criteria sometimes not motivate by references or specific justifications. Similarly the use of the linear regression method is used without showing any probability density function for the differences of the MLH and IB values. Scatter plots (very small) are used to graphically investigated the results. I think the authors must report the pdfs

instead or along with scatter plots. The pdfs can help to understand where the outliers are and to check the robustness of linear regressions. Otherwise robust linear regression methods may be used.

3. The authors investigated the considered dataset both merging night and daytime data and separating them. My impression is that the investigation of the nocturnal boundary layer is a bit risky considering that fact that the ceilometer is often not sensitive to the static boundary layer height (e.g. when MLH < 220 m) and considering the potential increase in the false positive errors related to an improper gradient attribution in the covered vertical range. The authors are asked to comment on this aspect.

4. The calculation of the ceilometer calibration constant CL reveals an instability over the time, which has been also already pointed out in literature; the authors estimated the calibration instability using a standards deviation (about 14 % considering data from all the stations). The plot shown in Figure 11 clearly shows a quite scattered data distribution: I think the use of standard deviation can underestimate the uncertainty and in general it is not clear how the standard deviation is estimated over the 24 months period. The authors should clarify and improve the robustness of the uncertainty estimation (e.g. showing the pdf and using, if needed, the IQ-range). In addition I think that the use of the same ceilometer type at different measurement sites cannot provided unbiased results although it can reduce the uncertainties related to the presented study: the big difference in the value of CL among the different sites, along with a dependence on seasonal meteorological conditions, is one point against the authors' hypothesis. I ask the authors to reconsider this concept in the different part of the manuscript where it is mentioned.

5. There is a lack of a more general discussion on the reproducibility of the presented approach if applied to other commercial instruments.

Specific comments:

Abstract Line 1: please put "only" at the end of the sentence.

Line 2: "at" instead of "for".

Page 1, line 16: please consider also air quality applications.

Page 1, line 23: put "For example, this.." at the beginning of the sentence.

Page 2, line 3: CALIPSO is an acronym.

Page 2, line 5: rephrase as "... to the topic of the aerosol spatial variability."

Page 2, line 16: "have" instead of "has".

Page 2, line 20: put "in the framework of air quality study" between commas.

Page 3, line 7: ".....we exploit data from (put the number) ceilometers..."

Page 3, line 10-11: rephrase with an appropriate language, please use the concepts of co-location, sampling or representativeness uncertainty.

Page 5: Table 5 could be enriched with all the information discussed at pages 3,5, and 6 to describe the measurement sites, also in order to shorten the text.

Page 7, lines 1-7, the provided description is not fully clear, please improve it.

Page 7, line 12: put "Although" before "such conditions".

Page 8, line 30: put "however" at the beginning of the sentence.

Page 9, line 16-17: the sentence is not clear, please clarify.

Page 9, line 22: some more explanation on the nature or on the reasons for the filtered outliers would be interesting although they are representative of a small fraction of the dataset.

Page 11, line 15: is "mixing Layer Height" the right way to define the nocturnal boundary layer? Please check this carefully.

Page 15, line 7: please show the pdf at the Lindeberg site to demonstrate that the MLH

distribution is narrower than for other sites.

Page 15, line 9: It appears that discussion on the presence of outliers for the M cluster is less extensive than for B cluster.

Page 18, line 1: comparison with literature paper are often reported in the authors' manuscript but it is never mentioned which are the MLH algorithm applied. Please describe them shortly to make more meaningful the comparison with literature results.

Page 22, line 2: "publication in literature".

Page 22, line 8: change the article "a" with "an".

Page 22, line 15: explain better the impact which the incomplete overlap region below 220 m, considered for this study, may have on a comparison with AOD from a use photometer.

Page 23, line 31: check the article ""an".

Page 24, line 12-13: please quantify the number of removed cases.

Page 24, line 25: please check again this sentence.

Page 25, line 3: not sure what's an "aka" constant is.

Page 25, line 3-4: add a reference from existing literature at the end of this sentence.

Page 26, line 1: is the data distribution normal? Please provided more details.

Page 28, liens 17-22: This part seems not to be well integrated into the section. Please check it.

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