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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Near real time processing of ceilometer network data: characterizing an extraordinary dust outbreak over the Iberian Peninsula" *by* Alberto Cazorla et al.

Anonymous Referee #1

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The manuscript by Cazorla et al. is suited for publication in acp after some revisions. It shows a method how a network of ceilometers could be calibrated in an unattended and automatic way and the paper demonstrated the usefulness of such a network in a case study of desert dust over the Iberian Peninsula.

Major critical comments

I am a bit confused on the main objective of this manuscript. It tackles several issues and by doing so it creates confusion. First, the papers deals with near real time data processing, however it misses an evaluation how good the near real time results are in comparison to results which are obtained not in near real time. Second, the paper

describes an automatic calibration method for ceilometers. It highlights that the calibration works best for low AOD and then it is applied for a case of desert dust where AOD is not low. Third it describes quite lengthy the meteorological situation of the dust episode, describes the episode as it was observed by ceilometers but then it lacks the description of other measurements during this episode. I am confused what the main objective of this study is. The authors could re-formulate parts of the manuscript focusing on one or two main objectives.

The authors state that the desert dust event was unusal. In what sense? Why it was unusual? Please add some comments.

* P4 line 15: I doubt that the telescope field-of-view is 1.8mrad. Please verify and provide the correct value. 1.8mrad is the FoV of so-called x-ceilometers manufactured by Jenoptik, now Lufft.

* CL and CL* are not really constant. They change over time. There was a discussion within E-Profile and ToProf about the vocabulary. I suggest using calibration value instead of calibration constant.

* I dont understand why the authors focus on CL which is systematically biased by ignoring the two-way transmission of particles when their method includes the use of sun photometer measurements in order to estimate the correct lidar ratio. I would understand it in case that there are no co-located sun photometer measurements which is not the case for ICENET. The description of the method should be adapted. Either there are information about AOD and hence CL* could be derived or AOD is not available.

* p7 lines 3-11: it is missing a description about the treatment of lower 250 or 300m where the overlap correction is very large and often unreliable.

* the authors highlight that the best calibration values are obtained for low AOD. But then they derive calibration values during a desert dust episode with high AOD. This

is very strange for me. For me it would makes more sense deriving calibration values before and after the desert dust event at times with low AOD and interpolate them for the desert dust measurements. This would even enable comparisons for ceilometer and lidar during night-time.

* The ceilometers were measuring 5 days. The calibration was only performed on 11 half-hour intervals. It would be good if the method descrption includes a few sentences how the calibration values for each individual profile were derived. Example: Figure 7 shows that there is a calibration value for each profile.

* the unit of center of mass is missing. It should be m or km (e.g. p8 lines 23/24, but also on other parts of the manuscript Minor critical comments

* I found 5 times “Iberian Ceilometer Network (ICENET)”. It is sufficient to explain the abbreviation one time

* P4 line 17: the authors wrote “the overlap is 90% complete at 555 m agl”. The manufacturer provides an individual overlap function for each single ceilometer. I strongly doubt that all 5 ceilometers have the same overlap. I suggest providing a range.

* P4 line 35 “Raman-shifted channels” this formulation is a bit sloppy. The channels are not Raman-shifted. Re-frase!

* p5 line 11-12: please add the emitted laser energy P0 in the details of the calibration value.

* p5 line 23: the rcs signal is also normalized to the number of laser shots which vary from profile-to-profile

* p5 line 23/24: “and also, the overlap function of the instrument is factory determined.” Although it is correct that the overlap function is determined by the manufacturer, I guess the authors wanted to point out that the overlap correction is already included in the rcs.

* p5 line 28: “a region in the ceilometer profile”. Here is missing the depth of the region. On page 7 the authors wrote about 990m. It should be mentioned already here.

* p6 line 9: “low aerosol load, where the transmittance due to particles is close to 1”. Please provide an AOD value what you consider as low. For instance the transmittance is 0.9 for an AOD of 0.1. Is this considered low? Or it is still too high?

* p6 line 25 ff "continously calculating“ Do you really mean continously or you mean repeatedly? And there are missing some details. I doubt that CL is calculated for every single profile. Do you use a temporal average for improving signal-to-noise ratio?

* p7 line 3/4 “In this study 30-min profiles are used for comparison with lidar profiles.” Is the calculation of the calibration value based on 30min too or a longer average is used?

* p7 line 17: “has to be close to the CL” what is close? Maybe the author could write something like “has to be close to CI within x%”

* p11 line 21: “total attenuated backscatter” what is the difference between attenuated backscatter and total attenuated backscatter?

* table 5: it is obvious when the site, the ceilometer and the sun photometer have the same geographical coordinates, that ceilometer and sun photometer are co-located. Hence it is not needed to write: co-located

* Figure caption 1: Raman with capital R

* Figure 2: middle image: unit of center of mass difference is missing. it’s m or km?

* Figure caption 3: Monte Carlo instead of Montecarlo

* Figure 7: it’s attenuated backscatter or total attenuated backscatter?

* Figure 9: I suggest adding the sun photometer values in the top panel. As the lidar ratio was derived, it is straight forward to derive the integrated backscatter from sun

photometer AOD

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