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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of aerosol properties from ceilometer and photometer measurements: long-term evaluation with in-situ data and statistical analysis at Montsec (southern Pyrenees)" *by* Gloria Titos et al.

Anonymous Referee #3

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The paper is an interesting contribution to aerosol remote sensing as it highlights the potential of automated ceilometers being available in large numbers (networks operated by national weather services). The limitation in characterizing aerosols is caused by the low power and the single wavelength compared to advanced lidar systems. However, the very good spatial and temporal coverage is a big advantage (unattended continuous operation). To overcome the above mentioned limitation the joint exploitation with photometer measurements is proposed (by means of $GRASP_{pac}$) – a validation of this approach is provided in this manuscript. Moreover, long-term observations of

aerosol profiles (in terms of extinction and derived by the novel GRASP-approach) are presented.

The paper is clearly structured, well written and relevant. There are several promising applications: the benefit of successful retrievals of aerosol profiles (backscatter or extinction) with high temporal resolution (as described in this paper) could be enormous for model validation: up to now validation is mainly confined to surface values (PM10, PM2.5) or columnar values (AOD), e.g. in the framework of AQMEII. Consequently, I recommend publication in AMT. Only minor changes are suggested. Most of them are just details in wording – nevertheless a few clarifications to avoid possible misunderstandings are strongly recommended.

In the following page and lines are given in square brackets

- [2,23] "Aerosol Optical Depth". I would suggest to use lower case letters.
- [2,24] "range corrected signal (RCS) lidar values..." → "range corrected lidar signal (RCS)..."
- [2,29] "...the GRASP algorithm is a significant advance...". Maybe change "is" to "can be", as the (positive) result of this paper is not yet known when reading the introduction.
- [2,31] I would be cautious with the word "worldwide" (see also the abstract): There are quite different ceilometers in operation and it is not yet clear whether the conclusions of this paper (found for one CHM15k) can be transferred to other systems: Many ceilometers have a lower pulse energy (consequently a limited measurement range) than the CHM15k (e.g., CL31, CHM8k), are influenced by water vapor absorption (CL31, CL51, CHM8k) and dense networks do not exist in all parts of the world. It is not unlikely that $GRASP_{pac}$ can be applied to some other systems as well, but to my knowledge this has not yet been demonstrated.

This topic should briefly be discussed (maybe in the conclusions). There are certainly publications on these issues that might be useful. See also comment on [5,14].

- [3,1] I agree that the methodology presented in this paper can be a step forward, however, there are other options for quantitative aerosol remote sensing (including night time measurements). In the last years several publications have demonstrated that ceilometers can provide the particle backscatter coefficient (under certain atmospheric conditions, see e.g. Wiegner and Geiß, 2012). They use a different approach than Titos et al. by using calibrated ceilometer data and get an uncertainty in the order of 10% for the backscatter coefficient. How does this compare to $\text{GRASP}_{\text{pac}}$ (same order of magnitude?)? See also comment on [8,19].

- [4,2] Whenever "backscattering" is used in the paper make clear whether backscattering in the "lidar sense" is meant (i.e. scattering under 180°; in m^{-1} sr^{-1}) or "hemispheric backscattering" (in m^{-1}) as measured by the nephelometer. And explain how the comparison is made: From Section 4.1.1 I understand that it is assumed that scattering into the backward hemisphere is the same for all directions. Then, from the integral (scattering angles $90^\circ \leq \theta \leq 171^\circ$, extrapolated to scattering angles $90^\circ \leq \theta \leq 180^\circ$) the authors calculate a mean backscatter coefficient (which – under these assumptions – can also be applied to 180°) and compare this value with the $\text{GRASP}_{\text{pac}}$-retrieval. As scattering under 180° is typically larger than scattering under smaller angles, I would expect an overestimation by GRASP (see also discussion in [6,6ff]). That was indeed found. As this is one of the main topics of this paper, the authors should be very clear – this might induce an extension of the discussion.

- [4,5] What is "lpm"? Should be "l min^{-1}"?

- [4,32] "...using the manufacturer's overlap function." This seems to be for infor-

mation only, with no consequences for the retrieval as the vertical difference between the ceilometer site and the observatory is 760 m (correct? Or what does "downslope" mean? If the vertical difference is smaller, the overlap issue should be discussed in more detail. On the other hand a horizontal distance of 2.5 km is mentioned.). Or is there another reason for mentioning this? Please avoid confusion of the reader.

- [5,12] Please explain what "normalized" ceilometer RCS means?

- [5,14] "corresponds to the MSA altitude..." So RCS between 760 m and 7760 m (above sea level, distance from the ceilometer $<$ 7 km) are considered in the inversion? Here I would expect a comment on the measurement range of the CHM15k: data are available up to 15 km ([4,27]) but the range that can be exploited is smaller. In the framework of the CeiLinEx2015 campaign it was shown that in the free troposphere the CHM15k signals are quite noisy (the maximum range of Vaisala- and Campbell-ceilometers is even smaller; this may influence the "worldwide"-discussion from above as well). How does this affect the $\text{GRASP}_{\text{pac}}$-retrieval? Is the maximum range (7000 m) reduced, but keeping the 60 levels?

- [5,20] The authors mention that the retrieved r_{eff} is height-independent, but never use r_{eff} in the paper. So it is recommended to mention the volume concentration V instead (or in addition).

- [5,21] "backscattering": here "under 180°"?

- [5,22] Please add a short comment on the accuracy of the retrieved aerosol parameters that are used in Section 4 (see also comment [3,1]). This is an important/mandatory information.

- [5,26] "backscattering": here "hemispheric"?

- [5,30] Give an equation/definition of "comparison" and "relative difference" (see also [6,2] and figures): "GRASP minus in-situ" or "in-situ minus GRASP"? Divided by the "mean of in-situ"?
- [6,7] Is the angular range with respect to the backscatter configuration correct? It should be 90° instead of 10° here (cf. Müller et al., 2011a)?
- [6,12] "tends to overestimate all the studied...". Is this statement trivial?
- [6,22] "...there is a linear trend between scattering and extinction coefficients...": What does this mean? If scattering and extinction are the same, the single scattering albedo ω is $\omega = 1$. This is more or less the case under clean and turbid conditions according to Fig. 3a. If scattering and extinction show a linear dependence, the single scattering albedo is constant. The fact that in general large extinction coefficients correspond to large scattering coefficients is not surprising (ω is typically between say 0.8 and 1).
- [8,17] "Qualitatively speaking, the volume concentration...". I don't understand the reason for this sentence? The seasonal cycle of the volume size distribution is not shown in the paper. Is this sentence included because a similar behavior is plausible? Or because this is known from literature? Or is it an intrinsic feature of the GRASP-methodology?
- [8,19] A comment on the limitation to daytime: This is caused by the combination with the sun photometer. From the ceilometer's perspective the determination of backscatter coefficients can be provided during night time as well (likely even better) provided that a calibration is possible, see comment on [3,1]. The extinction coefficient can be estimated if the lidar ratio can be estimated (of course, subject to – maybe significant – errors). In Titos et al.'s paper the transformation from RCS to physical quantities is provided by using the photometer data

as constraint; this is conceptually superior (at the expense of the two measuring systems required).

- [8,23] above → larger than
- [10,1] Explain "center of mass"; e.g. by citing Mona et al. (2006) or alternatively (if you want to avoid another self-citation) Binietoglou et al. (2015).
- [10,4] Here and in Fig. 7, the center of mass is given with respect to sea level. The numbers are correct but may lead to misunderstandings as the reader would intuitively expect much lower values (main contribution is almost always from the mixing layer). Indeed, values of 1.0–1.5 km can be found from Fig. 7 when considering heights above ground. So I recommend to add the corresponding values in brackets, at least in one or two cases.
- [10,28] "Similar seasonal behavior...": This is not a result of this study, it is only a message from a paper by Pandolfi et al. (2014). This should be made clear.
- [11,12] Here, Illingworth et al. (2018) can be cited.
- [Fig. 5] delete "light" in the caption; also in Fig. 6.

Suggested references:

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