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Interactive Comment

Interactive comment on "Aerosol microphysical retrievals from Precision Filter Radiometer direct solar radiation measurements and comparison with AERONET" *by* S. Kazadzis et al.

Anonymous Referee #1

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The paper is on the retrieval of aerosol properties: from spectral AODs the effective radius (r_{eff}) and the volume concentration (V_c) are derived. The advantage of the LE-approach (linear estimation) is that it is based on spectral AOD measurements only, i.e., no principal plane and/or almucantar measurements are required. The authors applied the LE-method to coincident and co-located measurements of the PFR- and the Cimel-radiometers. The results are inter-compared, and compared to results of the routine AERONET inversion scheme. The differences between the three types of evaluation are discussed with respect to the solar zenith angle and the AOD. The agreement varies between good and acceptable depending on the aerosol-parameter.

The paper is clearly structured and well written. However, the scientific reasoning must be extended and must be more convincing. A few suggestions of desirable/mandatory amendments follow. If the missing arguments are provided, the paper can be published.

The scientific concept of the LE-retrieval obviously is, that AOD is somehow related to V_c (this is plausible, as a higher AOD normally is associated with larger V_c) and that the spectral dependence of the AOD is somehow related to r_{eff} (this is also plausible, the Angström exponent is well known). So, in other words, the products of the retrieval are the same as conventional products (AOD and Angström), the only difference is that this information is expressed by two other variables. As a conclusion it is mandatory to explain the advantages of the "r_{eff} + V_c"-concept over the "AOD + Angström" approach. Modelers certainly would prefer r_{eff} and V_c, but if the errors are too large, the supposedly benefit disappears. In this context, the error analysis in Sect. 3.3 is quite useful, but should be extended (the variability of aerosol distributions is large! and includes more parameters than the authors have varied). Is an error of 60% acceptable for modelers? Can the operator identify whether a case I (small, acceptable errors) or case II (large, unacceptable errors) situation had occurred?

The paper's message is that a "simple approach" can provide useful aerosol information. Whether the retrievals are correct or not and which one is the best, is not answered as no independent data are available (i.e., no validation is possible), the paper only points out, how large the differences between the three retrievals are. Additional to the assessment of the accuracy of the microphysical parameters (as mentiened above), probably a review of previous papers will help. I assume that there are validation experiments for the routine AERONET-retrieval available. Then, comparisons with these results can serve as a benchmark. As more optical data are included in the AERONET-retrieval, it can be expected that these results are in principle more accurate.

It is shown, that the agreement of the AODs of the two radiometers is excellent. As a

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consequence, it is expected that the retrieval of r_{eff} and V_c gives similar results: "same input" plus "same retrieval" should provide the "same result". From this point of view the differences between the two LE-results are surprising, and only the differences to the AERONET-inversion are relevant. Therefore, the authors must give more details of the data, e.g., it is not clear how many spectral AODs (at what wavelengths) are involved in the PFR-LE and the Cimel-LE. This might explain the differences. Another reason can be the large sensitivity to even small measurement errors as briefly mentioned in Section 4. Anyway, this issue must be clarified.

The measurement period was dominated by a complex meteorological situation when two quite different aerosol types were mixed. As a consequence, it is certainly difficult to assume a reasonable refractive index for the atmospheric column. Though the authors state that the relationship between AOD and microphysics is not very sensitive to the refractive index, this issue should be discussed in some detail. So not only the dependence on the solar zenith angle and the AOD should be highlighted but also the dependence on the "degree of mixing" (even during the dust event it was not pure dust?). By the way: I do not understand the sentence "despite the presence of a strong dust event" in the abstract: why is the accuracy of the AOD influenced by the dust event?

The retrieved r_{eff} is always somewhere between the two modes shown in Fig. 2. This is expectable, but the question comes up how valuable this information is: r_{eff} neither describes the coarse mode nor the fine mode, and the uncertainty of r_{eff} might be large (see above).

It would be nice to have another measurement example when only one aerosol type was present. I don't know for which period coincident measurements are available. Maybe no further data are available.

The number of references should be increased: a few examples of sun photometer measurements (of dust events) and their inversion should be cited and commented.

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A few further comments in brief (partly already implicitly mentioned above):

The basic idea (in terms of radiative transfer) of the LE should be briefly reviewed, summarized and compared to the fundamentals of the routine inversion scheme. This is essential to understand the limitations and the potential of the LE.

The instruments should be described in more detail in particular the spectral channels (see comment above). The comments in the introduction are not sufficient and in Section 2 the relevant information is also missing.

Most figures must be optimized: larger labels, check description of colors in Fig. 6, Fig. 11 has the wrong number, ”log” should be “ln” in Fig. 2, and the y-axis scale should be changed in Fig. 6 (starting at 0.15 or so).

The abstract can be shortened; don’t give too many details.

Page 102, line 17: it’s an ill-posed problem for Cimel as well.

Page 108, line 15: don’t describe wavelength-differences in percent; it does not make sense.

Page 106, line 1: why is the upper limit of the inversion windows 10 μm and not 15 μm as in case of Cimel?

Page 109, line 12: “The best-fit line...” This sentence should be re-phrased; it is not clear, why the LE is mentioned here.

Page 113: the equation of log-normal size distribution may be given, even it is well known. I would rather appreciate more equations describing the LE-formalism.

Finally: the conclusions shall be reviewed: certainly a few duplications with the abstract can be avoided.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 99, 2014.