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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "SAWA experiment – properties of mineral dust aerosol as seen by synergic lidar and sun-photometer measurements" *by* A. E. Kardas et al.

Anonymous Referee #1

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General comments

The authors describe the analysis of lidar/photometer data taken during a desert dust event over Poland. They state that they propose a (new) method to retrieve basic information on mineral dust aerosol particles. But they do not show any (robust) error analysis that would support the robustness of their technique and indicate that the parameters estimated are obtained with sufficient accuracy.

Their reference list is incomplete. They do not provide an overview of papers concerning Saharan dust observations over Europe (with lidar, Sun photometers etc.). This is necessary. Furthermore there are already several attemps to combine satellite and

/or Sun photometer and lidar observations to retrieve Saharan dust properties, even microphysical properties (e.g., Mueller et al., EARLINET-AERONET dust observations, JGR, 2003). Dubovik et al. (JGR, 2006) discusses the possibilty how to use depolarization ratio and lidar ratio to get an idea about microphysical properties. All these attemps are not referenced.

The quality of their observational results is generally low, appropriate statements concerning uncertainties in the basic optical properties are missing. These optical properties are however then used to estimate the microphysical properties. Thus the results at all highly speculative.

Because of the very low quality of the paper, I have to recomment to reject the paper.

Specific comments:

page 12156, 15: Explanation of the abreviation SAWA is not given. What does it mean: Saharan Aerosol at WArsaw?

page 12157, 9-12: NAAPS forecasts are not enough to identify the source region of the dust. Hysplit backward trajectories must be computed (based on reanalysis data). I do not believe that the dust is partly advected from the Arabian peninsula.

page 12157, 13-17: Microptops photometers have an uncertainty of the order of 0.05-0.1, and thus the uncertainty is large when dust optical depths are of the order of 0.2, as is the case here. That must be stated in a paper.

page 12157, 18-28: From the telescope information I estimate that the overlap is not complete for heights below 800-1000m. How is that considered in the comparison of the photometer optical depth (height integrated extinction coefficient) and the lidar-derived integrated Klett extinction profile. I assume the error introduced by uncertainties in the overlap correction is very large.

page 12158, 1-9: Signals are collected by means of a 12-bit transient recorder. This certainly causes problems to detect cross polarized signals in the Rayleigh atmosphere

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(upper free troposphere). Good signal quality in the Rayleigh atmosphere is however required to perform an accurate calibration of the depolarization ratio. What is the uncertainty of the depolarization ratio calibration? What do you assume for the Rayleigh–atmosphere–related depolarization? Do you consider the receiver optical properties (efficiencies of the parallel channel for cross and parallel polarized light, efficiencies of the cross channel for cross and parallel polarized light) in these calibrations? The calibration uncertainties can be very large (50% and more).

page 12159: It is not necessary to describe the 25-35-year-old Klett method once more. There are several tens of papers available. Even ten are available concerning the combination of lidar and photometer observations to obtain more reliable Klett solutions. Nevertheless, even when using highly accurate photometer observations, the lidar-derived extinction profile is always highly uncertain, because of the range variability of the lidar ratio. Especially in the case of a two aerosol layer system, as described in this paper. Angstrom exponents derived from these extinction values are thus extremly uncertain. The same is then true for the further retrieval products (estimates of the microphysical dust properties).

page 12160, 10: Again, the overlap strongly affects Eq.7, how is the overlap corrected for?

page 12160, 14–16: There are many desert dust lidar ratio papers (GRL, Appl. Opt, JGR, ACPD, Mattis, Balis, deTomasi, Immler, etc.). Why not use a realistic start value of about 50˜sr instead of 1˜sr (!!!). References to previuos lidar ratio observations in dust are welcome.

page 12160, 16: In the case of a two-layer system you are 'lost'. How did you solve the problem without knowing anything regarding the lidar ratios in the urban haze layer (bottom layer, affected by dust) and the dust layer (lofted layer, that is obviously affected by urban haze)???

page 12161-12162: The description of the T-matrix methodology is insufficient. The

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information content of Figure 2 and Figure 3 is insufficient to evaluate the trustworthiness of the retrieved dust parameters. The interpretation of the results shown in Figure 10 (section 3.3) is insufficient. I am missing a detailed sensitivity study. Their conclusion cannot be drawn on the basis of a few simulation studies, as there are too many ambiguities regarding the link of optical to microphysical properties of desert dust (complex refractive index, size distribution, shape of particles). In that context I am missing all literature relevant to the interpretation of the simulations studies, see e.g., Kalashnikova et al., JGR, 110, D18S14, 2005 (there is a wealth of important literature in their reference list), Dubovik et al., JGR, 111, D11208, 2006 (there as well the authors may find useful literrature in the reference list), see also Kalashnikova and Sokolik, GRL, 29, 1398, 2002; Koren et al., JGR 106, 18047-18054; Kalashnikova and Sokolik, J. Quant. Spectrosc. Radiat. Transfer, 87, pg. 137-166, 2004. The authors remain very unclear about how they arrived at the results shown in figure 2, and they show too few examples. There is no way to evaluate the relevance of their results in the context of existing studies (see my suggestions on useful literature), nor the possibility to judge on the trustworthiness of their simulation results. Nothing is said about the structure of the look-up table (2nd paragraph on pg. 12161). What aspect ratios, refractive indices, mode widths of the particle size distributions were used for generating the look-up table? Did the authors consider any distribution of aspect ratios (I cannot find that from figure 2)? The authors only mention one single value of the complex refractive index. What is the reason for picking only that single parameter? That value is insufficient, and it does not take account of the ambiguities that are caused by the non-spherical shape of dust particle. What maximum size parameter was used in the T-matrix calculations? Did the authors consider instabilities of the T-matrix algorithm? It is a well known fact that the code becomes instable for large size parameters. The existing experimental data set is simply insufficient to draw such conclusions as mean size and aspect ratios.

page 12163, 1-11: Once more: How did you calibrate the depolarization ratio (see Alvarez, JAOTech 2006). A more appropriate citation for the depolarization technique is Sassen (BAMS 1991, or Academic Press 2000, or Springer book 2005). Eq.(11) describes the total (Rayleigh plus particle) depolarization ratio, how did you get the dust depolarization ratio? The dust depol. value is significantly larger than the total depolarization ratio (for 532nm backscatter values corresponding to a dust optical depth of 0.2). The total depol. ratio is much lower because Rayleigh backscattering causes almost zero depolarization.

page 12164: Forecasts (NAAPS model) are not convincing. Please calculate backward trajectories as mentioned above (HYSPLIT can be used via Internet).

page 12164-12165: As mentioned above, all results are rather uncertain, an error analysis is not given, the new approach (microphysical properties) is highly speculative, the applied methods (optical properties) are already published. The Angstroem exponents calculated from lidar signals considering 1064nm are extremely uncertain. Klett solutions for 1064nm are always highly uncertain as a consequence of the rather weak 1064 nm Rayleigh backscattering in the clean free troposphere (in the signal calibration height).

Furthermore, the authors seem to mix total depolarization ratio with dust depolariztaion ratio. They use the total depolarization ratio in the estimtion of dust microphysical properties together with the rather uncertain Angstroem exponents.

Because no new aspects of photometer/lidar remote sensing of dust is presented and no attempt is made to seriously discuss all the error sources and the overall errors in the optical and microphysical properties, I have no choice: I recommend to reject the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12155, 2006.