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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lidar and in situ observations of continental and Saharan aerosol: closure analysis of particles optical and physical properties" *by* G. P. Gobbi et al.

Y. Balkanski (Editor)

balkansk@lsce.saclay.cea.fr

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The following comments have been delivered by Anonymous Referee #1 and have been forwarded to the authors, but due to communication delays they could not be published earlier on the ACPD website:

General comments

The paper is of rather poor scientific quality. Without major revisions it must be rejected. The paper is based on simple one-wavelength backscatter lidar data. Nevertheless, the authors claim (and this not for the first time) that it is not only possible to derive the particle extinction coefficient but also surface area, volume concentration etc. from this

rather limited lidar information. This is by no means possible, and I am sure the authors know it since this is the clear message of the literature on lidar inversion methods published during the past five years.

Even in the case of five or more backscatter coefficients (at five or more wavelengths) it is not possible to retrieve any trustworthy information on microphysical properties. I am really frustated about the fact that the authors (Gobbi, Barnaba) already published a large number of papers in which they state again and again that it is possible to estimate extinction and microphysical properties from single-wavelength lidar data. The authors permanently ignore all the state-of-the-art papers regarding the ill-posed problems associated with lidar data inversion, e.g., published in Appl. Optics. So, most of the lidar results presented here are just speculation. This has nothing to do with solid, careful, quantitative measurements.

Specific comments

page 447, line 19: What do you mean by ..... to invert remote sensing retrievals....?

447,27: Concerning space lidars, the authors may not know that ESA will launch an HSRL that simultaneously measures backscatter and extinction coefficients of particles. 449: A detailed description of the methodology is needed! The following questions need to be answered in addition: How are the different parameters obtained? How come the input parameters into play? How large are the uncertainties of the estimated products, 50, 100, 200 percent? Why is a model needed when nothing is known about the atmosphere that is remotely sensed? Explain the model that delivers the lidar ratio guess.

450,13: What is the physical background for the claimed simple relationship between backscatter coefficient and lidar ratio? The backscatter coefficient and the extinction/backscatter ratio depend on size distribution, chemical composition, shape, and particle number concentration in very different ways.

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450, third paragraph: Too short, more explanations, more details are needed, give proper uncertainty numbers for all retrieved products.

As mentioned above, it is simply not possible to retrieve volume and surface concentration from simple one-wavelength lidar data in the case of tropospheric particles. Tropospheric aerosols are often a mixture of marine, dust and anthropogenic particles. Even in the case of the stratosphere (where size distribution and composition are well known) the errors are of the order of 50 percent. So, again, give a proper list of uncertainties for all of the retrieved parameters. The errors are certainly much larger than 100 percent.

451,15: Please give suggestions concerning the shape of dust particles. What shape did Mishchenko use? Is that a realistic shape? Give uncertainty numbers regarding the impact of particle shape.

451,22-25: Without clear statements concerning the uncertainties, all the figures are useless. The reader must have a fair chance to make his own conclusions about the quality of the results shown in Fig.1.

452,25: I am not convinced that the Saharan dust depolarization ratio is larger than 30 percent. I have never seen any paper (except papers of Gobbi et al.) with depolarization ratios larger than 25 percent. This includes Saharan dust as well as Asian dust. Consequently I have my doubts about the quality of the lidar receiver unit (detector channel configuration). The influence of ice clouds may have partly led to the high depolarization values, too.

453,3: ...behavior is similar....because of the oversimplifying model...is my answer.

453, 6: Again, please give clear, quantitative error statements.

455,11-20: I can not follow. Give a clear description of the different steps done to obtain Fig.2. Sorry, I do not see agreement at all! To my opinion, Fig.2 already tells us that simple one-wavelength lidar results are rather uncertain.

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Another story: Humidity correction of in situ data! What do we learn if the applied humidity correction procedure changes the data by a factor of 2-6 (cf. Fig2a and 2b). As a conclusion, because of the high uncertainties in the lidar data and the high uncertainties introduced by the humidity correction, the comparison (closure) is useless. The rather large scatter in the results underlines this.

456,10: I do not find Fig.2c.

456,15: I expect that the lidar ratio was approximately height-independent. So, the good correlation of backscatter with extinction is caused by the assumption on the lidar ratio profile.

457, I stop here with detailed statements. Figs.3 and 4 are based on all the questionable, speculative assumptions made before.

459, last paragraph of 4.3: Be careful with general statements based on these rather uncertain results. What is eastern European aerosols (what are the source regions)? Why is western European aerosol more absorbing? My own observations show the opposite.

461: Again, all the statements on page 461 are useless, are just speculative. The uncertainties in the results are too large.

463,464 (conclusions): I am not willing to accept the paper and the conclusions in the present form. The authors have to be realistic. Sentences like (464,17) ' On the whole, the analysis here provides a good in situ validation of the single wavelength lidar estimates of aerosol extinction, surface area and volume' sounds like a bitter joke.

Fig4: Lidar ratio should be shown, seems to be too small.

Without substantial revisions along my comments the paper must be rejected. The conclusions must be completely rewritten.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 445, 2003.

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Interactive Comment