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# Comment on acp-2021-608

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Referee comment on "First triple-wavelength lidar observations of depolarization and extinction-to-backscatter ratios of Saharan dust" by Moritz Haarig et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-608-RC3, 2021

" **First triple-wavelength lidar observations of depolarization and. extinction-to-backscatter ratios of Saharan dust**" by Haarig et al.

This paper presents the first triple-wavelength lidar observations of depolarization together with extinction-to-backscatter ratios of Saharan dust and also compares the results with aerosol properties derived from ground-based photometers. The paper proposes an advanced lidar measurement techniques. It is well written and suitable for the journal. However, I found that some clarifications in the approach description are needed before publication.

Main comments:

- The paper emphasizes the fact that the triple-wavelength lidar observations of depolarization together with extinction-to-backscatter ratios of Saharan dust were done for the first time. However, for scientists as myself, who is not specialist in lidar instrumentation, it is not fully clear the importance of this new advanced technique. I would encourage authors to emphasize the achievements. For example, as a reader I have following questions:

- What is the real advantage of the new technique? I could see triple-wavelength lidar observations of depolarization together with extinction-to-backscatter ratios of Saharan dust in other papers, for example, in Muller et al., (2012)?
- Is there any important differences (comparing with previous studies, e.g. Muller et al., 2012) were revealed? If yes, then what are those differences? May be, the new technique mainly confirms the previous results? If so, this should be stated.
- If I understood correctly, previously the measurements at 1064 were not simultaneous, but possible with few minutes of delay? If that is really a case, how critical is to have really simultaneous data, specifically thinking that more than 3 hours are required to get some meaningful data is necessary.
- Why in the figures 1 and 3 the different lidar characteristics are provided in different

altitude ranges? These parameters can't be retrieved simultaneously even with this advance technique? If yes, this shows some remaining serious limitation of the measurements. Also, the spectral dependencies of depolarization ratios, lidar ratios and other properties are different and often contradicting to each other especially at low and high altitudes.

- In my opinion, there are several weaknesses in the methodology of comparisons of lidar data with AERONET in the present version of the manuscript. They need to be addressed and corrected:

- First of all, the AERONET information content is significantly different from that of lidar:
  - AERONET is measuring aerosol properties in total column while lidar in specific altitudes and doesn't observe not negligible aerosol layer near ground;
  - Lidar measures signal in back scattering, while AERONET radiances are measured mostly in forward hemisphere with maximum scattering angle of ~ 140-150 degrees. The properties provided by the AERONET retrieval at 180 degrees are not directly constrained by the measurements.
- The above differences should be considered and discussed seriously in the AERONET and lidar comparisons:
- AOD and AE from AERONET are nearly directly measured by photometers and very reliable. It would be good if integrated extinction values and their spectral dependence compared AERONET AOD and AE, before comparing more complex retrieval products.
- The reported aerosol lidar ratios and depolarizations are not fully direct measurements and rely on some caveats and assumptions in processing. It would be nice if those assumptions and their accuracy are discussed. For example, lidar community tends to consider desert dust properties spectrally independent while, in reality, it is not true. Even AE of extinction for dust is rarely zero, and it can be notably positive or negative.
- In the considered observations, the observed aerosol seems to be not a pure dust in the entire vertical column. How the analysis assure that lidar characteristics derived at specific altitudes are for the same dust as seen in total column by AERONET?
- . The authors write: "The drop (I ASSUME "increase") of the imaginary part of the refractive index at 440 nm compared to 675 nm is too strong in the AERONET inversion procedure, resulting in too high lidar ratios at 440 nm. In-situ studies could not confirm the spectral slope of the imaginary part used in AERONET inversions as discussed by MuÌ□ller et al. (2012)." First, all AERONET "retrieves" imaginary part, not "uses" as the authors imply. Therefore, the spectral dependence is induced by the necessity of measurement fit. In these regards, I think the sensitivity to the imaginary part is likely higher in AERONET data than in lidar ones. Also, the obtained spectral slope tends to agree with the majority of other analyses of the dust (Shettle and Fenn 1979; WMO1983, Patterson et al. 1977; Sokolik et al. 1993; Koepke et al. 1997; Sokolik and Toon 1999, and there are many more recent data). In contrast, as I mentioned above the lidar community tends to assume no spectral dependence for desert dust properties. In these regards, I would suggest the authors to elaborate this aspect and if there is full confidence in the results to put a clear statement suggesting to use neutral or less pronounced spectral dependence for imaginary part of the dust. Some more extensive literature review of this subject is also desirable.

- One of the weakest points of the paper is comparison with so-called GRASP retrievals. First of all, I would like to emphasize that GRASP and AERONET retrievals have the same origin (Dubovik and King, 2000 and Dubovik et al. 2006). Indeed, Giles et al. (2019) was reporting only updates in quality assurance of the AERONET results but not the change of the retrieval, Sinuyk et al. (2021) outlined few new elements in the retrieval but emphasized that overall the retrieval methodology remains the same as in Dubovik and King, 2000 and Dubovik et al. (2006). Therefore for the same data GRASP and AERONET retrievals should not show any significant differences. Certainly, GRASP is more flexible and can easily use the different (more complete observations than a 4 wavelengths) and this is why GRASP was used in the paper, I suppose… However, what was the RATIONAL for comparing lidar results with results of inversion of AOD data only (i.e. no angular sky radiances) as done by GRASP-AOD approach by Torres et al. (2017)? It is evident, that AOD HAS practically NO SENSITIVITY to aerosol properties at 180 degrees!!! Torres et al. (2017) clearly stated that GRASP-AOD strongly relies on the a priori assumptions about complex refractive index and particle sphericity. It is interesting that the authors see the better agreement of GRASP lidar ratios with the lidar data. This probably shows nice potential of GRASP-AOD retrieval once the constraints are correctly assumed by Torres et al. (2017). However, overall, I do not see much justifications of comparing AOD only retrievals with lidar results!
- From my viewpoint, the comparisons of GRASP results with the lidar ones would be much more convincing if GRASP would be used for inverting AEONET AOD and radiances at more wavelengths (even if at some new channels only AOD data would be added). Moreover, I would think that the best approach would be to use GRASP for simultaneous inversion of the lidar data and AERONET dada as illustrated by Lopatin et al. (2013) and especially Lopatin et al. (2021). Such approach would be the most appropriate to reveal the strength and shortcoming of GRASP aerosol model, since would show if the model can adequately reproduce all (lidar and photometer) data or not.
- The authors emphasize serious limitations of Dubovik et al. (2006) spheroid model to reproduce the lidar measurements. Based on my personal experience I would probably agree that Dubovik et al. (2006) model would have difficulties to reproduce non-monotonic spectral dependence of depolarization ratio, as well as, the highest values of the depolarization ratio. However, I do not think that the comparisons of lidar data with AERONET and GRASP shown in the paper can be considered as a solid justification for such a statement. Once again, I would suggest trying the joint inversion of lidar and photometer data by GRASP as demonstrated by Lopatin et al. (2013, 2021). By the way, Lopatin et al. (2021) shown that using direct lidar measurements, e.g. volume depolarization instead of aerosol depolarization ratio results in better comparisons between photometric and lidar results.

Minor comment:

The following sentence seems to be awkward and incorrect: "Ln.33 The particle depolarization ratio sensitively depends on the form of the backscattering particles, whereas the dust lidar ratio is a function of particle shape”.

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