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Comment on acp-2021-608

Anonymous Referee #1

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-RC1, 2021

General comments

The authors present a novel technique for measuring the 1064 extinction product from rotational Raman scattering in combination with 1064 depolarization ratios and compare their retrievals with sunphotometer measurements. This is an important study for the lidar community as these 1064nm products are still far from being a standard. It will reduce the uncertainties of the aerosol optical properties in the infrared spectrum and provide additional information on the spectral dependence that can be crucial for the aerosol typing and the microphysical inversions. The paper is well organized and written in a clear and easy to follow way. However, certain additions and clarifications are required before publication, mainly concerning the rotational Raman technique itself and the comparisons with the sunphotometer measurements.

Specific comments

-- Concerning the technique for measuring the 1064nm extinction coefficient (e.g. Lines 66-71)

Two interference filters (IFF) with a bandwidth of 9nm are placed one after the other before the infrared Raman channel in order to suppress the light from the elastic scattering. Even though the transmission information at 1064nm from the manufacturer seems sufficient for this, factors such as the temperature and/or the incidence angle of the collected light on the IFF could shift the transmission spectrum of the IFF allowing cross-talks from the elastic line. Have the authors checked that this is not the case experimentally? This can be checked by placing a third typical IFF centered at 1064nm in the rotational Raman channel and see if any significant amount of light is detected.

In addition, the Raman spectrum is affected by the air temperature. Is the bandwidth of the IFF sufficient to eliminate any temperature effects in the profiles? What are the uncertainties? Even if such issues are reported in a previous study, a reference must be added along with a few lines explaining the main findings

Temperature effects can be even more pronounced in the near range (below the full overlap region) where the angle of incidence of the collected light in the IFF is still expected to change with range, especially for biaxial systems. This translates to a shift of the central wavelength of the IFF which in turn could result to temperature dependencies, especially if the IFF central wavelength shifts to a more temperature sensitive region. In the near range this could create overlap-like effects to the signal. Values of the extinction profiles at 1064nm in figures 3 and 4 are not provided in the near range, probably due to overlap issues. Could temperature issues be the reason why the 1064nm extinction profile in figure 3 starts at 2km while the 355nm and 532nm profiles start at 1km? Please add a description explaining whether such issues are present. If they are, please provide some information on how they have been dealt with.

-- Concerning the comparisons with the AERONET inversions

The lidar and sunphotometer measurements are not simultaneous. They have a time difference that is larger than 11 hours (around 0 UCT for the lidar -middle time -, and 11:35 UTC for the first sunphotometer measurement). The atmosphere could have changed significantly in the meantime. The authors use only the inversion on 23rd of July in their analysis. The retrievals on 22 of February are also available. They should be used to indicate the degree of atmospheric change in Figure 4, Figure 5, and in the discussion. Looking at the AERONET size distributions on 22 and 23 it seems that there are substantial changes. Furthermore, the inversion on 3rd of March is also available. Having provided the degree of atmospheric change in a ~11hour interval from the 22/02 case, the authors could add the 2 inversions on 03/03 in the analysis since the time difference is not far greater (~14 hours).

In addition, an AOD and Angstrom exponent comparison part or section should be included in the manuscript prior to the inversion comparisons in the Discussion as these AERONET products are far more accurate. Extinction and backscatter-related Angstrom profiles are also missing from figures 3 and 4. Please include them as well.

Moreover, it should be stressed in the Discussion section that differences with the AERONET retrievals, especially in the lidar ratio, are expected also due to the overlap function of the lidar system. For figure 1 and 3 it is obvious that the extinction profiles are available only above 1km and the 1064nm extinction profile above 2km! This is certainly not negligible, and could affect the comparison, even for the intensive properties since the aerosol type tends to be different inside the PBL. As mentioned above, a comparison with the AERONET AOD and Angstrom values will help quantify the degree of the uncertainties in the overlap region.

All the above points must be addressed in order to draw any firm conclusions from the comparisons with the columnar aerosol properties.

-- Concerning the 1064 depolarization ratio profiles

The 1064nm PLDR profiles have different vertical structure than the 355nm and 532nm profiles which in general show similar patterns. This is especially visible in figure 1 where PLDR maxima and minima are reproduced in both 355nm and 532nm profiles but not in the 1064nm profile. Can the authors explain this behavior? Is this a matter of atmospheric variability since the 1064nm depol. measurements last only for the first 20 minutes. In this case, and serving mainly as a test, would the profiles be more consistent if the first 20 minutes are used so that all thee depolarization ratios can be measured at the same time?

Technical Comments

-- Line 111-112: Have the two instruments been intercompared? Please provided references.

-- Line 114, Lines 144-145: Here it is not clear what the reference value is and how the cirrus clouds facilitate the selection of a better value, especially for people outside of the lidar community. The authors should provide a clearer description and provide references.

-- Figure 1, 3, and the tables: How were the mean lidar ratio and Angstrom values calculated? From averaging the lidar ratio profile or by averaging the extensive products (backscatter, extinction) first and then dividing them? If not yet applied, the authors should switch to the later as this leads to less noise.

-- Figure 4 and 5 on the right: Please add also the time of the lidar measurement in the figures