Response to Anonymous Referee #2 on the manuscript “Observations of the spectral dependence of particle depolarization ratio of aerosols using NASA Langley airborne High Spectral Resolution Lidar”.

Thank you for your supportive and helpful comments. Responses to the specific comments can be found below. Reviewer comments are in blue and author responses are in black.

The discussion paper of Burton et al. presents very interesting measurements of the linear depolarization ratio at three wavelengths for cases with long-range transported Saharan aerosols, with locally-generated desert aerosols, and with transported smoke aerosols. The measured data is discussed in context of existing literature. The paper is well-structured and overall well-written. It is a valuable contribution and should be published in ACP after some minor corrections.

1) For someone starting to read this paper, it remains unclear for a long time which depolarization ratio (linear or circular?) is meant. Please explicitly write "linear depolarization ratio" in the title, the abstact, the main text (at least once per section), and the figure captions.

We will revise the manuscript to say “linear particle depolarization ratio” in the title, abstract, and body text.

2) The abstract (p24753 l18) says "... is inferred to be ...": In my view, "coated soot aggregates" are one possible explanation for the smoke measurements, but there are certainly other types of soot-like particles that would explain these measurements. Thus, I suggest to write "... can be explained ..." or something similar.

We have no objection to making the suggested change in the abstract. As a side note, we would be interested to learn more specifics about the other explanations the reviewer has in mind, perhaps in another comment in this discussion forum. This manuscript is part of a learning process for us and we are very open to learning more about possible explanations for depolarization in smoke measurements.

3) Eq. 2: The definition of beta_parallel and beta_perpendicular is unclear. The text calls them "signal", but beta usually is the backscatter coefficient.

Agreed. As Gimmestad (2008) points out, adding parallel and perpendicular subscripts for the backscatter coefficient is imprecise since that notation mixes an atmospheric parameter (backscatter) with instrument-specific factors. In the manuscript, we were really just looking for a way for readers to see in a glance which of two frequently-used ratios we use. However, Equation 1 does this in a more correct way. We will simply delete the middle portion of the double-equality in Equation 2.

4) Fig. 4 at about 150km distance on track and 4km altitude: The linear depolarization ratio increases from <0.1 at 355nm to _0.2 at 532nm and _0.25 at 1064nm. As this wavelength dependence is quite uncommon, I wonder if these numbers are real aerosol properties or just a measurement artefact. Can you comment on this?

It’s somewhat difficult to see on the curtain plots, but the local maximum of the depolarization at that location corresponds to a local minimum in the aerosol backscatter coefficient (conversely, the peak in backscattering is at an altitude just below the quoted linear depolarization ratios and this backscatter peak corresponds to a local minimum in depolarization values). The total scattering ratio (R in the manuscript) at 532 nm is about 1.4 at that spot. The estimated systematic error (at 532 nm) is therefore approximately 17% relative error. Taking these into account, I would say that there is non-zero depolarization at 532 nm and 1064 nm and that the values at these wavelengths exceed the 355 nm value (indicating perhaps some coarse-mode dust aerosol) but that the relatively low backscatter in that

thin layer precludes drawing a conclusion about whether the spectral dependence definitely increases between 532 nm and 1064 nm or not.

5) "pure dust", "pure Saharan dust" in several places of the paper: Though these terms have been used in the literature, they are, strictly speaking, wrong. Dust particles are solid particles, but desert aerosols usually contain also a non-negligible number of small spherical particles that are no dust particles. This was shown in measurements during field campaigns, for example SAMUM. The "desert mixture" of the aerosol database OPAC also contains small spherical particles. They are very important for the depolarization ratio at 355nm and thus the spectral dependence of the depolarization ratio. The spectral dependence of the depolarization ratio presented in Sect. 3 shows that such non-dust particles were present in both measured "dust" cases. Thus, I suggest to write "pure desert aerosol", "pure Saharan aerosol", "dust-containing aerosol", or something similar, but not "pure dust".
We will make that change in the manuscript.

6) More generally, I like to encourage the authors to replace "dust", where appropriate, by "desert aerosol", "Saharan aerosol", "dust-containing aerosol", "dust-dominated aerosol", etc., throughout the paper, to take into account that the measured aerosols contain also non-dust particles as discussed in comment 5. I admit that this is not always considered in the literature, nonetheless the suggested naming would be more precise.
We will support the effort to introduce more precise descriptions of dust-dominated aerosol by making this change in the manuscript as well.

7) p24767 l16ff.: Does the incomplete geometrical overlap not increase the uncertainties of the depolarization measurements?
The depolarization measurement is made as a ratio of two channels, which are affected the same way by the incomplete geometrical overlap, so therefore there is no increase in uncertainty, except through a decrease in signal strength. That is, since the amount of light reaching the detectors for both channels is reduced, there can be an increase in the amount of noise relative to the signal; however, for the smoke plume featured here, the amount of scattering was quite high and loss of signal is not a concern.

8) p24770 l24 and Fig. 15: Please mention how the size of the soot aggregates is defined? Volume-equivalent, maximum dimension, or?
Volume-equivalent particle radius. We will put this description in the caption to Fig 15 and the main text.

I find the technical details of the system and the error analysis well-described. However, since I am not so familiar with all the effects that can happen in the optics and the electronics of such an advanced lidar system, I hope that other reviewers are more familiar with this topic.

Technical corrections:
A) p24756 l8: "6-km" –> "6 km" Corrected in the revision
B) p24762 l18: "The particle depolarization spectral dependence..." –> "The spectral dependence of the particle depolarization ..." Changed in the revision
C) Fig. 3 caption: "Aerosol backscatter and extinction curtains ..." –> "Curtains of aerosol backscatter and extinction coefficients ..." Changed in the revision
D) Fig. 6 and 12 caption: "backscatter" –> "backscatter coefficient" Corrected in the revision