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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Is the near-spherical shape the "new black" for smoke?" *by* Anna Gialitaki et al.

Anonymous Referee #2

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The authors use an example of transported stratospheric smoke from the 2017 Canadian wildfire pyroCB to model the spectral dependence of depolarization and lidar ratio at 355, 532, and 1064 nm. Near-spherical, Chebyshev, and fractal shapes are used, but only the near-spherical shapes are able to match the results obtained from lidar measurements.

The subject is quite relevant and the results could be very significant for scientific community. However, there are some issues with the manuscript which must be clarified before it is suitable for publication. Examples are provided below:

Firstly, the title implies that the near spherical shape may be the "new black" for smoke. However, the case study focuses on a stratospheric smoke case. It follows to ask if this is only the "new black" for smoke in the stratosphere only or should we assume this

might apply to the troposphere also? From the example shown, my guess is no.

Page 3, Line 10: Is the spectral dependence really non-typical? The authors do a good job of convincing us that the high depolarization values are non-typical for smoke, but spectral dependence seems the opposite. In fact, decreasing depolarization with increasing wavelength is closer to typical from the references cited by the authors.

Page 3, Line 12: Something is missing from this sentence or perhaps the wording is intended to be: The starting point and main assumption of our investigation is that the particle near-spherical-shape can be highly depolarizing as shown in the work of Mishchenko and Hovenier (1995); Mishchenko et al. (2016); Bi et al. (2018) and Ishimoto et al. (2019).

Figure 2: The images seem to be incorrectly placed, given the aspect ratios. Oblate sphere should be flattened and prolate stretched

Page 6, Line 20 - 26 and Figure 6: The latitude limits in the text do not match those in the Figure and the red dashed lines do not match the section highlighted in Figure 4. But even more importantly, the corresponding browse images indicate a complex mixture of aerosol and ice from cirrus clouds which would explain the high depolarization ratios in these cases.

https://www-calipso.larc.nasa.gov/products/lidar/browse_images/show_detail.php?s=produc 10&browse_date=2017-08-15&orbit_time=17-55-33&page=3&granule_name=CAL_LID_L1-Standard-V4-10.2017-08-15T17-55-33ZD.hdf

https://www-calipso.larc.nasa.gov/products/lidar/browse_images/show_detail.php?s=produc 10&browse_date=2017-08-15&orbit_time=17-55-33&page=4&granule_name=CAL_LID_L1-Standard-V4-10.2017-08-15T17-55-33ZD.hdf

The images from Haarig et al 2018 are more convincing for the case the authors are making. Perhaps another CALIPSO image should be used if the authors would like to show satellite measurements.

Page 8, Line 12-13: The authors state: What is interesting here is that the retrieved sizes for near-spherical smoke particles are absent in the AERONET climatology product. However, the difference in using a mono-modal versus bi-modal distribution is something that should be explored or at least discussed more in the text. Are we seeing the result of an "effective" radius in a mon-modal distribution that presents a possible solution for the high depolarization and spectral dependence, when there could also be another solution obtained from a mixture in a bi-modal distribution?

Sections 4.2 and 4.3: Is this all that can be said for these solutions... we tried these shapes and they didn't work? It would seem that more could be gleaned from these failed attempts. For instance, these shapes show the correct trend in spectral dependence even though the absolute values/relative differences do not fit the measurements.

Page 9, Line 18: Can the authors offer an explanation for the low angstrom exponents other than coarse particles?

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