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Interactive comment

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

Anonymous Referee #1

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Review of "Is the near-spherical shape the new black for smoke?" by A. Gialitaki et al., 2020

The manuscript topic fits well within the journal scope is providing new insights on biomass burning aerosol layers. Nevertheless, it needs major revisions before being ready for publication.

Major comments:

1) A substantiated and consolidated verification of the measurement quality and the potential role of systematic errors affecting the measurements is a preliminary paramount step when such high PLDR values are measured. This is particularly true for stratospheric aerosols as calibration of aerosol depolarization measurements of strato-

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spheric particles is quite difficult and cannot rely on molecular calibration approach.

2)The fact that such high PLDR values were reproduced using T-matrix simulations, assuming near-spherical shapes, for biomass burning is not itself a verification of the fact that observed particles were indeed transported stratospheric smoke plumes. More information on possible particle composition, and its possible organic origin, should be inferred from other optical measurements (multi-wavelength particle extinction and backscatter measurements).

3) Because of 2) the proposed approach is rater weak. It is not possible to generalize statements just from a single case study. Moreover it seems a sort of ill-posed problem and the minimum in Eq. 8 might be relative, i.e. what happens if instead a mono-modal distribution a bimodal is chosen? or a gamma instead of normal distribution? Probably Eq. 8 will provide independently a solution.

4) As stated by Sassen and Khvorostyanov, smoke can directly act as ice nuclei before liquid clouds form (https://iopscience.iop.org/article/10.1088/1748-9326/3/2/025006). This fact can partially explain the higher PLDR (considering a process in progress). This aspect, very likely is not mentioned in the manuscript and can be the reason of PLDR increase.

5) The simulations themselves are not original as in fact similar simulations were performed in the past by Bi et al. 2018, Mishchenko et al. 2016; Ishimoto et al., 2019, as the authors explicitly admit. What is different with respect to those manuscript?

6) The title (i found it funny) might be misinterpreted and considered inappropriate

Specific comments are available in the attached manuscript

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2020-22/acp-2020-22-RC3-supplement.pdf

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