Referee report to the "Purely light-scattering aerosols could induce positive shortwave radiative forcing over high-albedo surfaces " manuscript by Longrui Zhang et al.

The manuscript discusses a quite unexpected effect of a positive radiative forcing caused by a stratospheric aerosol layer under certain conditions, especially at small solar zenith angles above surfaces with high Lambertian albedo. Although this effect was already reported by other authors, they explained it with the aerosol absorption while the presented manuscript proofs that the effect is observable also for non-absorbing aerosols. Authors suggest a simple model, which helps to understand the obtained results. Furthermore, authors compare the two-stream approximation, commonly used by climate models, with the results from the 16 stream radiative transfer modeling using DISORT and report biases associated with the usage of the two-stream approximation. Although the study is quite simple, its results can be interesting for the scientific community. After some improvements the manuscript can be accepted for the publications.

Detailed comments

- Line 76: Please remind the reader that Φ is the asymmetry factor.
- Section 2.1: Adequacy of the single-layer radiative transfer model with no background atmospheric molecular scattering or absorption needs to be discussed in more details.
- Section 2.1: In radiative transfer publications the number of streams is often refers to a hemisphere. This means that the full number of streams for both upward and downward directions is double. Please clarify, if 16 streams used by you refer to the full sphere or to a hemisphere.
- Line 103: Please precise how the instantaneous upward spectral irradiance was calculated in DISORT. Is it the radiance integrated over all directions or is it rather a flux, i.e. radiance weighted with cosine of the incidence angle?
- Section 2.2: Please discuss the adequacy of considering alumina particles spherical as required by the Mie calculations.
- Section 2.2: If the total number of streams is 32, the number of Legendre moments of 17 is too low.
- Section 2.3: Step size in the local time needs to be reported.
- Lines 160–163: Please discuss what is new/different in this study with respect to (Randels et al., 2013).
- Figure 2 caption: "Figure 2a, b and c" – I think you mean "Figure 2b, c and d", panel a is not described.

- Figure 2: It is unclear why ΔF is a function of the surface albedo. The integration over the latitude bin is already done and should consider the distribution of the surface albedo or was the integration done for set of assumed constant albedos?
- Section 4.4: The goal of this section needs to be explained. It is unclear to me why the authors present the dependencies of Φ on the solar zenith angle and surface albedo are presented. What is the potential scientific use of this information?
- Lines 314-315: Please explain the potential use of the parameter Φ for scientific studies.
- Lines 332-336: This text has no clear relation to the contents of the manuscript.

Technical corrections

- Line 77 : "independence to" $\longrightarrow$ "independence of"
- Line 92: The notation "#/cm^3" is quite weird, please use "particles/cm^3" instead
- Line 110 and throughout the text: To avoid confusion please put spaces around dashes and use long dashes, e.g. "input to the RTE-extinction coefficient" $\longrightarrow$ "input to the RTE – extinction coefficient"
- Section 4.2, title: the section title should be in bold face, "Radiation" $\longrightarrow$ "radiation"
- Figure 4, caption: "As ," $\longrightarrow$ "As,"