Replies to the comments from anonymous referee#1

We sincerely appreciate the reviewer's deep theoretical review and thorough analysis. We acknowledge the mistake in connecting non-sphericity/high irregularity directly to high depolarization, rather than simply stating it indicates the presence of non-spherical particles. We have corrected this phrasing across the entire manuscript. That said, we want to reassure the reviewer that our results did not rely on this assumption. All retrieved values and conclusions stem directly from our theoretical simulations and numerical retrieval outputs. Thank you again for pointing this out and helping us improve the theoretical precision of the manuscript.

1. The authors draw various conclusions regarding the shape characteristics of the dust particles, making frequent use of the false casual relation: "higher particle linear depolarization ratio (PLDR) is due to high-irregular particles". Firstly, PLDR is not directly related to irregularity, for example, particles that are nearly-spherical present quite high PLDR values (Bi et al., 2018). Moreover, there is no rigorous proof in the literature that the simple, unrealistic, shapes for dust particles used in this study (i.e., spheroids and irregular hexahedrals) can reproduce the full suite of the real dust particle scattering properties (especially the backscatter) after assigning to them real-dust-particle aspect ratios: a) for spheroids the real-dust-particle aspect ratios do not reproduce the backscatter measurements (as shown also in this study), and b) for irregular-hexahedrals, when we use real-dust-particle aspect ratios the spectral dependence of e.g. the PLDR is not reproduced (Saito and Yang, 2021). Thus, there is no proven correspondence of the aspect ratio of the simple shapes used herein, to the aspect ratio of real dust particles, and the authors should avoid making such assumptions. For this, a more rigorous proof would be necessary, by e.g. comparing the retrieved properties with collocated in-situ observations, but as the authors state in the manuscript, these observations were not available for the case studies analyzed. Revise the whole text accordingly.

We acknowledge this important criticism. We agree that the non-zero depolarization indicates the presence of non-spherical aerosols, but magnitude of depolarization does not reciprocate with the non-sphericity. However, high depolarization ration for irregular shapes and near-spherical shapes are caused by a different mechanism (electromagnetic interactions on complex particle boundaries vs interference of electromagnetic waves). "Higher particle linear depolarization ratio (PLDR) is due to high-irregular particles" is generally true with exceptions of near-spherical shapes. To our knowledge we do not know any other simple shapes that provides high DPR. Nevertheless, for clarity, we have systematically revised the text so that we no longer refer particle as "highly irregular" based solely on high depolarization. We changed the terms 'highly irregular particles' to dust particles with high depolarization ratio.

2. The evaluation of the spheroid model is done using the scattering database of Dubovik et al. (2006) that is not optimum for the backscatter of large particles (as dust), since for these particles it uses IGOM. The authors highlight this in the conclusion section, but this should be also included in the abstract, so as for the reader to understand that part of the limitations in the backscatter discussed for the spheroids may be partly due to decreased accuracy of the backscatter calculations with IGOM.

We thank the reviewer for raising this important point, which prompted us to examine the question in considerably more depth. We agree that the Dubovik et al. (2006) database computes large-particle (size parameter ≳ 30–60) optical properties with the geometric-optics-integral-equation method (IGOM; Yang and Liou, 1996), and that IGOM has documented accuracy limitations at scattering angles beyond 175°, noted in Dubovik et al. (2006) itself. In particular, IGOM neglects the coherent backscattering enhancement (CBE) arising from constructive interference of reciprocal ray paths (Saito and Yang, 2023; Saito et al., 2026), which rigorous computations show amplifies P_{11}(180°) by a factor between 1 and 2 (Zhou and Yang, 2015; Zhou et al., 2025; Ding et al., 2016). In response, we have expanded the discussion of these limitations in the revised manuscript [4.2.2; this addition also addresses the reviewer's related comments at Lines 207 and 568–570], including a new paragraph analyzing whether this numerical deficiency could account for the spheroid model's poorer performance in our retrievals.

However, after examining the sign of the documented IGOM biases, we respectfully argue that this limitation should not be presented in the abstract as a qualification of our conclusions regarding the spheroid model, because three independent lines of evidence indicate that the IGOM deficiency cannot be the dominant cause of the spheroid shortcomings we observe and in fact acts in the opposite direction:

a) **Backscatter intensity:** Neglect of CBE biases modeled backscatter low (and hence modeled lidar ratio high, by factors of ~1.3–2; Huang et al., 2023). In our retrievals, the spheroid forward model errs in the opposite sense: it overestimates backscatter and underestimates extinction within the core of the dust layer. A backscatter-deficient forward model would not be expected to converge to a state that overshoots the measured backscatter.

b) **Depolarization**: CBE amplifies the co-polarized backscattered return at least as strongly as the cross-polarized return (amplification factor exactly 2 versus ≤ 2 for smooth particles; Zhou, 2018; see also the analogous polarization asymmetry in exact weak-localization theory, Mishchenko, 1992). Neglecting CBE would therefore, if anything, bias modeled δ high whereas the spheroid model's primary deficiency in our study is δ that is too low.

c) **Shape deficiency established with exact numerics**: The inability of spheroids to reproduce observed dust depolarization is documented in studies that used numerically exact methods and therefore contain no geometric-optics error at all (DDA: Gasteiger et al., 2011; invariant-imbedding T-matrix: Lin et al., 2018).

Taken together, these considerations imply that correcting the IGOM deficiency would degrade, rather than improve, the spheroid model's agreement with the observations (i.e., the missing backscatter enhancement partially masks the shape model's deficiencies). Recently, an analytical solver of the scattering properties of spheroids has been developed, and the future work should include the validation of IGOM-computed scattering properties of spheroids with the counterparts based on the analytical approach (Ding and Yang, 2023).

Revisions:

Line 3: Change “a conventional spheroid model” to “the conventional spheroid model of Dubovik et al. (2006)”

Changed as suggested.

Lines 13-14: Change “physically plausible estimates” to “plausible estimates”, unless you prove its values and the spectral dependence is physical plausible (by e.g. evaluating it against climatological values of the dust refractive index (see comments for section 4.3.2 below)).

Changed as suggested.

Line 20: Change “physically realistic retrieval” to “plausible retrieval”, same comment as above.

Changed as suggested.

Lines 35-37: Dust mainly scatters SW light, whereas in the LW it scatters and absorbs by comparable amounts, as shown in Fountoulakis et al. (2024) (see Fig. 5 and relevant discussion therein). Correct lines 35-37 accordingly: “dust particles contribute to ... the longwave (Tegen and Lacis, 1996; Mahowald et al., 2014; Song et al., 2022).”

We thank the reviewer for pointing us to Fountoulakis et al. (2024), which we agree is a valuable reference here. However, we would respectfully distinguish between two separate statements: the relative magnitudes of the scattering and absorption coefficients, and the sign of the resulting radiative effect. This is borne out by the DRE calculations shown in Figs. 13 (BOA) and 14 (TOA) of the same paper where the LW DRE is positive in every configuration examined. Their conclusions state explicitly that the LW DRE at TOA should not be considered negligible, and that over dark surfaces the LW warming can dominate the SW cooling at low SZA, yielding a net positive total DRE. We therefore do not think the cited work supports removing the LW warming statement.

That said, the comment prompted us to recognize that our original phrasing was imprecise in two respects: it did not make clear that the LW effect is a net one arising from both scattering and absorption, and it stated the SW cooling without qualification, whereas the SW DRE at TOA is potentially positive over bright surfaces (e.g., Fig. 14 of Fountoulakis et al.). We have revised the text to read:

"over dark surfaces dust particles contribute to radiative cooling in the shortwave, while contributing to net warming effects in the longwave over all surface types (Tegen and Lacis, 1996; Mahowald et al., 2014; Song et al., 2022; Fountoulakis et al. 2024)."

Lines 42-44: “The geophysical properties of dust ... challenging.” Airborne dust is often externally and/or internally mixed with other aerosol types. Thus, when mention the retrieved “geophysical properties of dust” in the atmosphere, the dust mixing state should also be taken into account. See e.g. the work of Shrestha et al. (2026) and the relevant references therein and provide a relevant review here.

We agree that transported mineral dust is frequently mixed internally and/or externally with other aerosols (such as sea salt or anthropogenic particles) during transport. The list of limitations we presented is not exhaustive; factors such as surface roughness and complex mixing states are also key model assumptions. While adding surface roughness to the irregular hexahedral models improve the lidar backscattering simulations, an incorporation of the surface roughened irregular hexahedral models to the retrieval system is left for future work. We have added them as limitation of the shape models to the conclusion section.

See last paragraph in the conclusion: Some of the limitations of both shape models studied here are chemically homogeneous composition, neglects particle surface roughness as well as the internal and external mixing of dust with other aerosols (such as marine sea salt or smoke). For cases like in this study, the long-range transported dust can get internally/externally mixed with the sea salt particles during the transport that can dynamically shift the hygroscopicity and linear depolarization ratio of the column, and introduce errors into the inversion algorithm Shrestha et al., 2026. Future studies addressing these effects will lead to more accurate dust retrievals but remain beyond the scope of this study.

Lines 58-61: "leading to more accurate retrievals ... Dubovik et al., 2019)" It may also lead to retrieving the coarse mode size distribution, if observations at longer wavelengths are available. Explain here that you consider the capabilities of current polarimeters in space (if this is what you mean), with limited spectral range, and discuss the capabilities of e.g RSP or future space sensors as 3MI.

We thank the reviewer for highlighting the importance of an extended spectral range, particularly at longer wavelengths. The selection of spectral channels in this study was intentionally aligned with the polarimetric configuration of NASA's PACE mission. As noted by Zheng et al. (2024), super-coarse particles d>10 microns exhibit weak radiative signatures in the visible to near-infrared (VIS-NIR) spectrum for AERONET-based retrievals, while remaining highly sensitive in the thermal infrared (TIR). RSP doesn't take observation in the Thermal infrared. Nevertheless, evaluating how super-coarse particles influence polarimetric signals across available VIS-SWIR channels is a crucial area of research. Given that MetOp-SG's 3MI mission is now operational, we plan to incorporate its extended spectral channels into our retrieval framework in future work.

Lines 63-65: "They also provide precise measurements ... McLean et al., 2021)." Lidar observations (usually up to 1064nm) provide limited information for the coarse mode of dust particles, especially for the larger sizes. Figure 1 and Tables 3 and 4 in McLean et al. (2021) show this. Moreover, larger dust particles (larger effective radii than the ones shown in McLean et al. (2021) sensitivity study) should be also included in the discussion here, see e.g. Ryder et al. (2019). In addition, the chemical composition (especially the imaginary part of the refractive index) is not well-retrieved, as shown in e.g. McLean et al. (2021). Revise accordingly.

Thank for the reference. We added "However, studies have shown that standalone multi-wavelength lidars (up to 1064 nm) face limitations when retrieving coarse-mode microphysical properties. McLean et al. (2021) showed high coarse-mode retrieval errors compared to the fine mode, leaving key parameters like absorption, size, and variance largely unresolved, particularly for large dust sizes. Therefore, while MAPs and lidars each offer impressive capabilities for capturing aerosol properties, each instruments have their own limitations."

Lines 65-66: "While MAPs and lidars each offer impressive capabilities for capturing aerosol properties," Revise accordingly to previous comments.

Revised as suggested

Lines 165-179: Provide the limitations of TAMUdust2020 in reproducing the backscatter of dust particles, since this is (partly) what you use it for in this study. The statement "Many studies have used this shape model for different dust types and instruments and found it to be effective." is too vague and it does not provide the reader the necessary information for evaluating its usage for different lidar applications. See for example the difficulty in reproducing the spectral dependence of backscatter coefficient and PLDR as shown in Saito and Yang (2021) and in Chang et al. (2025). Provide here a full review of limitations.

We thank the reviewer for this feedback and have rewritten Lines 165–179 to show limitations of the TAMUdust2020 database. We added "While none of the current existing dust optical property models produce accurate spectral dependence of the depolarization ratios at three lidar wavelengths, particularly at 355 nm (e.g., Gasteiger et al., 2011; Saito and Yang, 2021; Kong et al., 2022), the irregular hexahedral models of the TAMUdust2020 database significantly outperform spheroid models. Saito and Yang (2021) demonstrated that spectral lidar and depolarization ratios are overall consistent with those of dust plumes measured by HSRL or Raman lidars, except for the overestimation of depolarization ratio at 355 nm. Furthermore, Chang et al. (2025) observed a slight underestimation of backscatter at 355 nm alongside an overestimation of coarse-mode bulk depolarization at longer wavelengths. These discrepancies may stem primarily from assuming idealized, smooth-surfaced geometries that neglect microscopic surface roughness, edge rounding, and internal cavity reflections that strongly govern 180-degree backscatter physics (Saito and Yang, 2021). Oppermann et al. (2026) demonstrated an improvement in lidar backscattering simulations by incorporating the surface roughness effects into irregular hexahedral models, and the future work includes testing of the Oppermann et al. model for the present work."

"Line 207: Provide a brief discussion on the limitations of the Geometric-Optics-Integral- Equation approach in reproducing the backscatter of light.

We agree that noting the limitations of the IGOM approach is valuable for readers evaluating backscattering calculations. To maintain a concise paper length while providing this information, we have added the following brief statement in the text:

"…and the IGOM approach (Yang and Liou, 1996). For the largest size parameters where IGOM is used, the neglect of coherent backscattering interference causes it to underestimate the backscattering phase function by up to a factor of two (Zhou and Yang, 2015; Ding et al., 2016;Saito et al., 2026), a limitation we return to in Sect. 4.2.2."

Lines 249-262: The term "effective radius" is widely used to describe the size distribution of an ensemble of particles, not the size of a single particle (see e.g. Hansen and Travis (1974)). I see that the work of Saito and Yang (2022) uses this term to describe the size of a single particle, based on the definition of Grenfell and Warren (1999). This is very confusing for the reader, but since it is a term that is already used in the literature, it is valid to use it here as well. In order to avoid confusion though, include here a brief description of the distinction to the widely-used term. Moreover, change "effective radius" throughout the manuscript to "single-particle effective radius".

Thank you for pointing this out. We agree that it can be confusing for the reader. There are places where we are referring the effective radius as the effective radius of the size distribution like in the figure 2. Otherwise, we changed the "effective radius" to "single-particle effective radius" as suggested.

Lines 261-262: “However, ... size distribution.” Discuss more about the “uncertainty inherent in the underlying assumption of a common size distribution”, and why you “anticipate that the sensitivity to this choice is secondary”. No quantification is needed, but more discussion, since this is too vague of a statement.

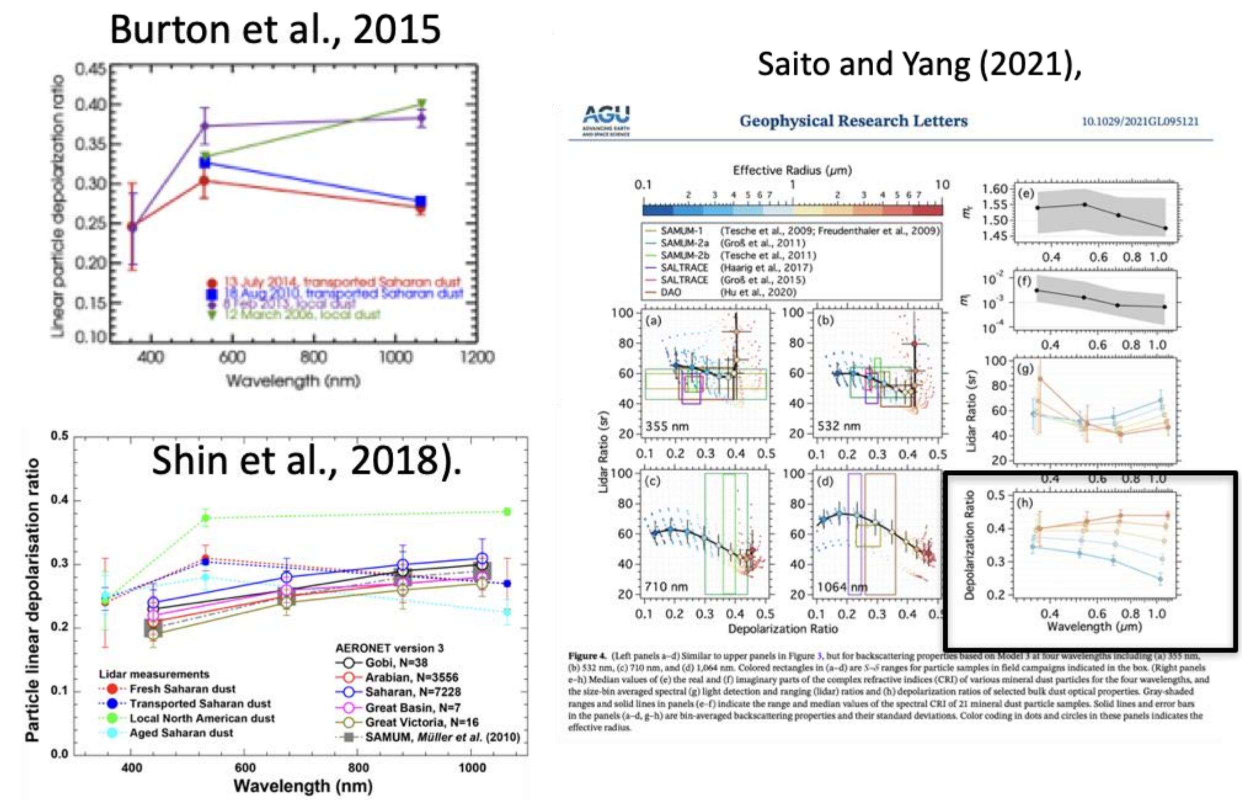
We thank the reviewer for asking for more detail on this point. While assuming a shared size distribution across both shape models adds a small uncertainty, its impact on our results is secondary. As shown in Figure 2, the single-scattering phase matrix elements (P11 and -P12/P11) for both the spheroid and hexahedral models are very similar across the scattering angle range covered by our satellite views. Because both shape models are applied over the same size range, the differences in our retrievals are driven mainly by the shape models themselves rather than small variations in particle size weighting. We have updated Lines 261–262 to make this reasoning clear to readers.

Lines 280-282: “At the same level, the total depolarization...non-spherical particles.” The high PLDR values do not necessarily indicate “extremely non-spherical particles”. E.g., Bi et al. (2018) presented the high PLDR values for near-spherical particles, and Saito and Yang (2021) showed that PLDR does not really change for different sphericities of irregular hexahedral particles.

We changed “extremely non-spherical particles” to non-spherical particles.

Lines 282-284: “This spectral shape of δ, ... Di Biagio et al., 2019).” Dust mixtures with spherical particles, as reported in Sugimoto and Lee (2006) is plausible. Particles with “high aspect ratios” do not cause this spectral dependence, as reported in Saito and Yang (2021): “However, δ shows little sensitivity to particle shape...”. I cannot find anything about the “coarse and absorptive particles” in Di Biagio et al. (2019) (or in the previous two references), can you explain what you mean here? Rephrase accordingly.

We sincerely thank the reviewer for identifying these errors. We agree that attributing the spectral behavior of δ to high aspect ratios was incorrect, and that Di Biagio et al. (2019) was improperly cited in this context. We have rewritten Lines 282–284 to clarify that particle aspect ratio alone does not drive this spectral trend (Saito and Yang, 2021). Instead, the size and the spectral variability in δ for coarse mineral dust is heavily modulated by strong spectral absorption (e.g., iron oxide content at shorter wavelengths), which dampens internal field interactions and reduces backscatter depolarization in the large-particle limit (See figure 6 and 8 in La Luna et al., 2026). Also, there are other older papers that show that this depolarization ratio for locally emitted dust. The manuscript text has been revised accordingly.



Lines 295-296: Change “These δ values ...in shape.” to “These δ values are lower than those observed in Case 1, suggesting either the presence of other "non-dust" aerosols in the layer or that the dust particles are potentially smaller or have different shape properties than the particles in Case 1.” (see main revisions point #1 above).

Changed as suggested

Line 304: Change “...and yield physically plausible retrieved parameters” to “and yield physically plausible retrieved parameters regarding the size and RI of particles” (see main revisions point #1 above).

Changed as suggested

Lines 309-310: “at five wavelengths: 0.410, 0.469, 0.555, 0.670, 0.863 μm” Why the longer wavelengths of RSP (that we expect to be essential for dust), i.e. at 1590, 1880 and 2250 nm (Knobelspiesse et al., 2011.), were not used? Weren’t they operational?

We agree that including longer wavelengths improve coarse mode dust retrievals. However, the selection of spectral channels in this study was intentionally aligned with the primary polarimeters of the NASA PACE mission. However, we fully recognize the value of incorporating longer wavelengths to constrain larger aerosol particles and surface boundaries and will do so in our future studies.

Lines 344-345: "...whereas that at 0.355 µm contains information on the imaginary part of the refractive index." Change it to "... whereas that at 0.355 µm contains information on the imaginary part of the refractive index at 355nm.", since Chang et al. (2025) note in Sect. 3.3 of their work: "Also note that the mi here refers to mi_355, of which δ_1064 is independent".

Changed as suggested.

Lines 370-372: "While dust ...(Adebiyi et al., 2023)." I think that you are confused, Adebiyi et al. (2023) show in their Fig. 7d that dust with sizes r > 15µm contribute less than 1% to globally-averaged total dust extinction at solar wavelengths. This contribution has to do (mailnly) with the globally-averaged coarse-dust-particles load, and not only with the extinction of the coarse dust particles. The contribution of coarse dust to the total extinction is better shown in Fig. 8 of Ryder et al. (2019), where you can see that the contribution of the coarser particles may be up to 40% (for dust above desert). The fact that the retrieval that you use considers dust particles with radii<15µm, should be stated here as a deficiency, when coarser particles are present. Change accordingly.

We thank the reviewer for identifying this misinterpretation. We agree with your assessment regarding Adebiyi et al. (2023) and appreciate the reference to Ryder et al. (2019). The coarse mode particles in Ryder et al. (2019) is defined as particle diameter>10 µm (radii > 5 µm). From Fig 8 Ryder et al. (2019), 40% of the contribution is from the particles with radii > 5 µm. For transported Saharan dust (e.g., the AER-D SAL curve in Ryder et al., 2019), the contribution of particles radii>15 µm (D >30 µm) is extremely low. Since our study focuses specifically on transported dust, assuming an upper radius bound of r = 15 µm (D = 30 µm) captures nearly all of the optically relevant particle spectrum for this regime.

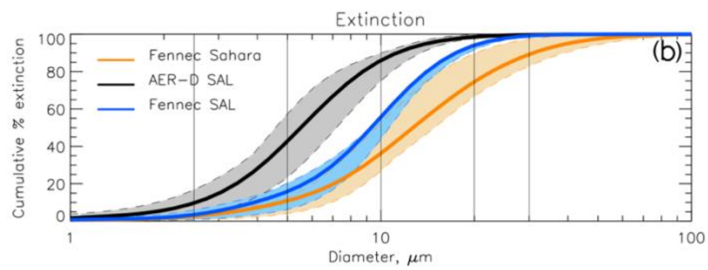


Figure 1: Figure 8 from Ryder et al. (2019)

We've revised the text as "While dust with sizes r>15micron have been observed with in situ measurements, the contribution of these particles to the total extinction is very low, particularly in the case of transported dust event Ryder et al. (2019)"

Lines 378-380: "This assumption ... Cattrall et al., 2003)." Dust layers from different sources (with different microphysical properties) may be present in the column. For example, the back-trajectories shown in Fig. 1 show dust coming from different sources. This fact limits the applicability of your assumption. Discuss more about this here.

We thank the reviewer for pointing this out. The back-trajectories in Figure 1 indicate that the dominant air masses at different altitudes originate from Northern Mali, Mauritania, and Algeria. Dust from this specific Saharan corridor has been shown to exhibit a relatively homogenous microphysical and optical profile, characterized by similar clay ratios and iron oxide-driven absorption in the shortwave spectrum (Di Biagio et al., 2019; Formenti et al., 2011). While the dust layer at 1 km in Case 1 was transported from southern Mali—a region known to have different dust properties, the AOD contribution of this specific layer is small compared to the total column AOD. Therefore, while we acknowledge that this assumption is a fundamental algorithmic limitation of our GRASP configuration, the assumption of vertical uniformity of intensives within a given mode is reasonable and necessary approximation for this retrieval. It is also an assumption common to almost all prior aerosol retrieval algorithms.

Lines 387-388 and Table 2: "The upper and lower bound of k ...in Di Biagio et al. (2019)" The maximum values of the imaginary part of the refractive index is low compared to the values reported in the literature, especially for 355nm, e.g. from OPAC database. Justify the selection of the RIs from only the work of Di Biagio et al. (2019).

We chose to base our search space boundaries on Di Biagio et al. (2019) because it represents one of the most comprehensive, modern, and geographically resolved laboratory studies of pure mineral dust based on coincide measurements of scattering and absorption properties of dust particles in an aerosol chamber. Their work explicitly isolated the optical properties of dust from 19 global regions and directly linked the imaginary refractive index to iron oxide content. The upper bounds we selected (e.g., k = 0.008 at ~355 nm) safely encompass the highest absorption values expected for pure Saharan dust, which aligns not only with Di Biagio et al. (2019) but also with findings from modern field campaigns (e.g., SAMUM, FENNEC) and AERONET climatology (e.g., Dubovik et al., 2002). However, we performed retrievals by varying the upper bounds and during the study none of our retrievals seemed to land anywhere near the upper bound.

Lines 445-446: "The remaining ...marine mode below." Fine aerosols (i.e. fine marine particles) are also found in the marine boundary layer (MBL). Assigning the fine mode as another aerosol type above the MBL will result in retrieving fine mode properties with characteristics of marine particles and "other aerosols". How do you address this issue?

We thank the reviewer for raising this important point regarding fine-mode aerosol distributions within the Marine Boundary Layer (MBL). We agree that fine marine particles exist within the MBL. In our inversion setup, assigning fine mode types relative to the MBL height acts merely as a weak *a priori* constraint (governed by small Lagrange multipliers) to guide convergence, rather than a strict hard boundary or physical mask. Because these constraints are deliberately loose, the inversion framework retains sufficient flexibility to retrieve fine-mode particles throughout the MBL whenever the observed radiance and polarization signals dictate it. As shown in our retrieved vertical profiles, the model successfully retrieves fine-mode aerosol concentrations within the lower boundary layer despite this initial *a priori* setup. We have updated the manuscript to clarify the soft nature of this constraint and prevent any confusion regarding hard aerosol classification.

Lines 459-463: "In this section, ... using MAPs and/or lidars." The observations that you use in the MAP and lidar retrieval include also the P_22 element of the phase matrix. Include a discussion (and related plots) for this element in this section.

We thank the reviewer for this point and would like to clarify how P22 is handled in our study. We do not use the P22 phase matrix element directly as an independent, angularly-resolved input in our retrieval algorithm. Instead, the non-spherical depolarizing effects associated with P22 are captured through the linear depolarization ratio in our lidar retrievals. We show the simulated DP for both shape models in the Appendix. If the reviewer is referring to the angular dependence of phase matrix elements, our forward modeling discussion focuses specifically on P11 and -P12/P11, as these are the primary terms governing first-order single scattering in multi-angle polarimetric observations

Lines 484-485: “At backscattering angles, ... definitions.” At the backscatter –P12/P11 is always zero for randomly-oriented particles (irrespectively of shape, size, RI). This is the reason no differences are found. Correct this mistake.

I can see how the previous phrasing was misleading. By "backscattering angles," we were referring to scattering angles greater than 170° (excluding exactly 180°). To clarify this, we have revised the text to state that P11 varied by up to 20% instead of using the term "backscattering angles”.

Line 523: “This is likely ...higher depolarization ratios.” Higher PLDR do not necessarily indicate “highly irregular” dust. See similar comments above and main revisions point #1, and revise accordingly.

Revised accordingly.

Lines 520-521: “The GRASP retrievals ... the observations.” There is excellent agreement, but not along the whole extend of the profile. Revise accordingly.

You are right that there are certain regions in the profile that doesn’t exactly overlap with the observation. We changed the sentence to “demonstrate excellent agreement with the observations throughout most of the profile”

Lines 536-546: “This spectral dependence ... longer atmospheric residence times.” a) You should avoid calling this spectral dependence as “non-typical” or “extreme” for dust, when PLDR observations at 1064nm are very few worldwide to draw such a broad conclusion. b) The retrieved size distributions (SDs) do not indicate “very coarse particles” (Fig. 7 and 8). Moreover, Saito and Yang (2021) show that the PLDR of 40% does not indicate “very coarse particles”. You should plot the retrieved SDs (all SDs in Fig. 7 and 8) along with the climatological values for transported dust and dust above the sources (e.g. see “transported” and “desert” size distributions in Fig. 9 of Ryder et al. (2019)). The SDs you retrieve seem smaller than the minimum typical “transported” dust in the area you study, and not even close to the size distributions measured above sources (Sahara). c) As mentioned above and in main revisions point #1, revise the statement “the depolarization ratio at 1.064 μm is exceptionally high (> 38%), suggesting the presence of highly irregular particles”. d) According to b) and c), the assumptions you present about the “very coarse, flat particles” in Lines 542-546 do not hold. Revise a)-d) accordingly.

a) Thank you for pointing this out. We’ve removed the phases “non-typical” and “extreme” and rephased as “Similar values have mainly been reported for local emissions (Burton et al., 2015, Shin et al., 2018). Additionally, La Luna at al., 2026 showed that this spectral nature of dust indicates the presence of coarse mode particles. The persistence of these larger non-spherical particles may be facilitated by higher average drag coefficients compared to volume-equivalent spheres (Bagheri and Bonadonna, 2016) potentially extending their atmospheric residence times.”
b) We revised the manuscript to not link high depolarization to presence of highly irregular particles to just presence of nonspherical particles throughout the text
c) We agree with the reviewer that comparison with the climatological would significantly improve the quality of the paper. However, size distributions from Saharan dust events is high variable and driven by differences in transport lengths and atmospheric residence times (Formenti and Di Biagio, 2026). Hence, comparing our retrievals to climatological averages without knowing the actual particle sizes for these specific events could biases. Furthermore, because this study

analyzes two specific dust cases without *in-situ* size distribution measurements for direct ground-truth validation, performing a formal closure study is not feasible with the current dataset. Hence, we have intentionally kept the manuscript focused as a comparative study rather than a validation against broad climatological baselines.

Lines 542-546: “It raises the possibility ... longer atmospheric residence times.” As discussed in the previous comment, this should be revised.

Revised accordingly.

Lines 557-568: “This exploration- Chang et al., 2025).” This analysis (presented also in Appendix C) regarding the limitations of spheroids in reproducing the PLDR spectral dependence is very useful!

Thank you.

Lines 568-570: “In contrast ... enhanced backscatter.” The irregular hexahedrals are not a “more realistic representation of irregular particles”, they only reproduce better the observations. Avoid using such logical jumps. Highlight here that the backscatter calculations for the larger spheroids have also limitations due to the use of IGOM (see main revisions point #2), but these limitations are not due to the shape of the particles. For example, the “enhanced backscatter” should have been also taken into account for the spheroids backscattering calculations. Delete this whole phrase.

Changed the phase to “In contrast, the hexahedral shape model, with a lower sphericity (~0.7), reproduced the observations better.”

Additionally, we have added the following paragraph just below the passage referenced by the reviewer addressing their main revisions point #2:

“As noted in Sect. 2.2, the spheroid kernels rely on IGOM for size parameters beyond the T-matrix convergence limit (≈ 30–60, depending on aspect ratio and refractive index), so that essentially all coarse-mode backscatter in the spheroid model derives from IGOM at the wavelengths considered here. IGOM neglects the coherent backscattering enhancement (CBE) produced by constructive interference of reciprocal ray paths (Saito and Yang, 2023; Saito et al., 2026), which rigorous computations show amplifies the backscattering phase function by a factor between 1 and 2, leading IGOM-based models to underestimate backscatter and overestimate lidar ratios by comparable factors (Zhou and Yang, 2015; Ding et al., 2016; Zhou, 2018; Huang et al., 2023). In principle, part of the spheroid model's difficulty in reproducing the lidar observations could therefore be numerical rather than physical in origin. However, the signs of the documented IGOM biases are inconsistent with the misfits found here: our spheroid retrievals overestimate backscatter and underestimate extinction within the dust layer, the opposite of the backscatter-deficient signature expected from neglected CBE, and because CBE amplifies the co-polarized return at least as strongly as the cross-polarized return for smooth particles (Zhou, 2018), its neglect would bias modeled δ high, whereas the spheroid model's principal deficiency is δ that is too low. Consistent with this interpretation, IGOM-based ellipsoidal optics have been shown to reproduce observed dust depolarization ratios well despite underestimating backscatter intensity by a factor of ~2 (Huang et al., 2023), and spheroid ensembles computed with numerically exact methods likewise fail to reproduce observed dust depolarization (Gasteiger et al., 2011; Lin et al., 2018). We therefore attribute the spheroid model's shortcomings primarily to the shape model itself and note that, because the missing CBE partially offsets the shape-related backscatter deficiencies (e.g., Chang et al., 2025), the spheroid model would likely compare less favorably to the observations if its geometric-optics component were corrected."

Line 571: "...while also retrieving physically consistent size and refractive index values." For the refractive index the values are not that physically-consistent, if you compare them with climatological values for dust refractive index (see also comments for section 4.3.2). Change to "..while also retrieving plausible size and refractive index values."

Changed as suggested

Lines 605-606: "Across the two shape models ...the dust mode." Figures 7 and 8 do not really show that they present smaller variations. Delete this statement.

Deleted as suggested

Line 609: "The dust mode -marine modes." Change "... far exceeding the fine and marine modes." to "... exceeding the fine and marine modes.".

Changed as suggested

Lines 609-610: "In contrast...shape-related variability" Due to their lower concentrations (less signal) their retrieval should be more unstable. The reason of the relative stable retrievals is "the fine mode is ...shape-related variability". Revise accordingly.

Changed to "In contrast, the fine and marine modes' results remain relatively stable, likely because fine mode is intrinsically less sensitive to particle shape, and the marine mode is forced to be spherical."

Lines 615-616: "Moreover, ... stronger depolarization." You have shown morphology-related biases under stronger depolarization (Appendix C), but you haven't proven (e.g. by showing a case with low/medium AOD and high depolarization) that there is any AOD dependence on the morphology-related biases. Rephrase.

Thank you for pointing this out. We revised the text too to "..indicating that morphology-related biases become more pronounced under stronger depolarization."

Lines 627-629: "Given the high sensitivity ... measurements." The RSP wavelengths you used are not very sensitive to the large dust particles. Delete this statement, and rephrase the rest of the paragraph accordingly.

Changed to "Given the ability of polarimeters to retrieve particle size more accurately than Lidars,"

Lines 629-630: "However... properties" Since the hexahedral retrieval does not present these high inconsistencies, then the assumption about the modes is not the reason. The reason is the inability of the spheroids to reproduce (high) PLDR. Rephrase.

Thank you for pointing it out. We rephased as suggested.

Lines 638-642: "When spheroids ... dust particles (See C)" This is a very helpful result of this study!

Thank you.

Line 646: "...may be significantly underestimated if an unrealistic particle shape is assumed." The irregular-hexahedrals are also unrealistic shapes for dust, and still they provide more physically-consistent results for the size of the particles. Moreover, your study does not present results from other unrealistic shapes for dust, thus you cannot generalize. Delete or change this phrase in "...may be significantly underestimated if a spheroidal particle shape is assumed."

Changed as suggested.

Lines 649-650: "Therefore, we do not recommend using the Dubovik et al. (2006) spheroid shape model for dust retrievals that incorporate δ observations" This conclusion should be also included in the abstract of the paper.

Added to the abstract.

Lines 654-655: "The hexahedral model ...across RSP and HSRL-2" Include here that the "hexahedral model" is still a model, and the results should be evaluated against in-situ data.

We changed the line to "Although in situ measurements are currently unavailable to validate whether the hexahedral shape model is the optimal descriptor for dust, the two cases analyzed here indicate that it nevertheless outperforms the spheroidal model…"

Lines 658-660: "For both fine mode ... SSA." No, there is sensitivity there as well. Delete this statement and rephrase the rest of the paragraph.

We revised the paragraph as : "For both fine-mode and marine-mode aerosols, retrievals from all setups show that both modes are non-absorbing (SSA ~1). Additionally, we see that the differences in retrieved n, k, and SSA between the two shape models are smaller compared to the differences between the RSP-only, HSRL-only, and RSP+HSRL retrievals. This might be because of their lower concentrations, or because fine-mode aerosols are typically less influenced by shape assumptions while marine-mode aerosols are explicitly constrained to spherical shapes in the retrieval. We see that the difference between shape models in the combined retrieval is larger compared to the single-sensor retrievals. However, all of these differences are much smaller compared to the differences we observe in the dust mode."

Figures 7 and 8: Regarding the refractive index results: You should also include here a comparison with the climatological values (maybe as a shaded area defined by the min/max values provided in the literature, see for example Fig. 3 in Ryder et al. (2019)), so the reader understands the discussion about "realistic" and "unrealistic" values for the RIs.

We thank the reviewer for the suggestion to compare our retrievals against climatological data, which provides valuable context for the study. However, adding these data directly to the figure would significantly overcrowd the plot and compromise its readability. Instead, we have expanded Section 4.3.2 to include a detailed comparison with climatological (Dubovik et al., 2002; Di Biagio et al., 2019).: 'Although the values retrieved using the hexahedral shape model are generally closer to climatological estimates (Dubovik et al., 2002; Di Biagio et al., 2019). than those from the spheroidal model, they do not fall strictly within the climatological range at certain wavelengths. Nevertheless, it is essential to consider the inherent limitations of climatological datasets. Specifically, climatological imaginary refractive indices typically rely on Mie theory—which assumes spherical particle geometry—while the real part is frequently assumed to be spectrally constant. Therefore, while climatological data serve as a valuable benchmark, minor deviations from these values do not necessarily imply inaccuracies in our retrievals.

Lines 672-674: “This suggests that... refractive indices.” This is also a very helpful result from this study!

Thank you.

Line 676: “...suggesting fewer highly non-spherical particles,” change to “...suggesting fewer non-spherical particles,” (see also previous comments).

Changed as suggested.

Line 678: “highly irregular dust particles” change to “irregular dust particles” (see previous comments).

Changed as suggested.

Lines 680-682: “In contrast, ... signature.” This is not true if you compare the results with climatological values of the refractive index. Do the comparison, as indicated above, and re- write the discussion regarding the refractive index results using the hexahedral model.

We did the comparison as suggested.

Line 692: “..."measurement-derived" AOD (from direct integration)...” Plot these values in the corresponding plots in Fig. 7 and 8, for direct comparison with the retrieved values.

We appreciate the reviewer's suggestion. However, because the figures were already quite crowded, we have reported these specific numerical values directly within the main text in line 714 as “The column-integrated extinction from HSRL-2 measurements is 0.723 and 0.726 for Case~1, and 0.40 and 0.401 for Case~2 at 0.355 and 0.532 micron, respectively”

Lines 730-731: “However, ... boundary layer.” No, they are not “slight”, some of them are quite large. Change accordingly.

Changed to “However, the profiles of fine and marine profiles are vastly different for each instrument setup, particularly below the boundary layer.”

Lines 734-736: “These discrepancies ... variation.” This does not seem to be the reason. Why not assign it to the (prominent) peak at ~2km? It seems that there is not much information for differentiating fine and coarse particles along the vertical. Rephrase accordingly.

Changed to “This might be because RSP alone has limited vertical sensitivity and the strong overlying dust layer further attenuates the signal reaching the boundary layer, increasing uncertainty in the profiles below the boundary layer.”

Lines 738-739: “Overall, ... a uniformly distributed fine mode,” No, not all setups retrieve a “uniformly distributed fine mode”. Change accordingly.

Changed to: “Overall, all three setups retrieved a fine mode, the dominant dust mode, and the marine mode below the boundary layer height, indicating that GRASP is not introducing unrealistic compensations for the differences in assumed aerosol shapes in the retrieved profiles.”

Lines 740-741: “indicating ... profiles.” This happens for the same instrument setup, not for different ones. Change to “indicating that for the same instrument setup, GRASP does not introduce unrealistic compensations for the differences in assumed aerosol shapes in the retrieved profiles.”

Changed as suggested.

Figure 9: Change the label of x-axis in “extinction coefficient” and provide the corresponding wavelength. Include 1 more plot for each Case, showing a comparison of the lidar-measured extinction coefficient profile and the retrieved (total) extinction coefficient profile.

The vertical profiles display the same extinction values as those in Figures 5 and 6. We have updated the figure captions to include the relevant wavelength information. Specifically, Figure 5 presents the lidar-measured extinction coefficient and retrieved total extinction for Case 1, while Figure 6 shows these values for Case 2.

Line 801: “This is a ...shapes.” Include this information in the abstract of the manuscript as well.

In light of the discussion above regarding the reviewer’s main point #2, presenting the IGOM limitation in the abstract as a possible explanation for the spheroid shortcomings would likely misrepresent the balance of evidence. Instead, we have added the following sentence to the abstract noting that the documented sign of the IGOM backscatter biases is inconsistent with the spheroid misfits we observe:

“Although the spheroid database relies on geometric-optics calculations with known accuracy limitations at the backscattering angle, the documented sign of these errors is inconsistent with the spheroid model's misfits, indicating a limitation of the spheroid shape model itself.”

Additionally, we have replaced the second half of this paragraph with the following:

“As discussed in Sect. 4.2.2, however, the documented sign of the IGOM backscatter errors is opposite to the spheroid misfits we observe, indicating that the shape model itself, rather than the computational method, is the dominant limitation and that the spheroid model would likely compare less favorably still if its geometric-optics component were corrected.”

Line 804: “refractive indices (n, k), and SSA.” I’m not that sure they are very realistic. We will revisit this, after comparing with climatological values for the refractive index of dust (see comment above).

We thank the reviewer for this constructive suggestion. Incorporating comparisons with climatological data provides valuable context for our study, and we have added references to the available climatological datasets in the revised manuscript. “low as 1.45 (the lowest allowed in the search space) across all HSRL-2 wavelengths (0.355, 0.532, and 1.064 microns). This value is lower than reported climatological range of 1.48 - 1.55 (Dubovik et al., 2002; Di Biagio et al., 2019). This also results in k values (e.g., 0.0010, 0.0009, and 0.003) that correspond to relatively weak dust absorption, falling near the lower end of values documented in the literature (Dubovik et al., 2002; Di Biagio et al., 2019).”However, it is essential to note the inherent limitations of these datasets. Specifically, climatological imaginary refractive indices typically rely on Mie scattering theory which assumes spherical particles and the real part of the refractive index is frequently assumed to be constant. Therefore, while climatological data serve as a useful benchmark, deviations from these values do not necessarily indicate inaccuracies in our retrievals.

Lines 811-813: “In contrast... combined data.” This implies that the retrieval is successful. Re- evaluate this after comparing the retrieved refractive indices with climatological values of dust refractive index, and highlight the necessity of the evaluation of the retrieval with in-situ observations.

Same reasons as the comment above

Line 816: “...RSP’s sensitivity to fine mode size” As stated in comment above, RSP employs also longer wavelengths, why aren’t they used? Emphasize here that the sensitivity to (mainly) fine mode size is due to the wavelengths used in this study.

We agree that including shortwave infrared channels can provide valuable information regarding coarse-mode dust properties. However, the primary focus of this study at the time was on the PACE mission, hence we didn’t include the longer wavelengths. However, with new polarimeters like 3MI now in orbit, it will be highly valuable to investigate the impact of these longer wavelengths. We plan to explore this in future work by comparing the respective retrievals with and without the SWIR channels.

Thank for pointing this out. We rephased the line to “The joint retrievals combined the complementary strengths of each instrument using I and DoLP from RSP at 0.410, 0.469,0.555, 0.670, 0.863 µm alongside vertical extinction, backscatter, and depolarization ratio from HSRL-2 at 0.355, 0.532, and 1.064 µm, drawing on RSP’s wavelength-dependent sensitivity to fine-mode size and refractive indices and HSRL-2’s sensitivity to vertically resolved aerosol concentration and type.”

Lines 818-819: “... yielded more ...(i.e., lower SSA)” Re-evaluate this as discussed in previous comment(s).

Same reasons as previously discussed.

Lines 827-828: “The hexahedral shape model ... to spheroids.” Include a comment for the necessity of external validation of the results with in-situ data.

Thank you for your comment. We added the line “In conclusion, our findings demonstrate that the spheroid model of Dubovik et al. (2006) fails to produce physically meaningful properties for high-depolarization cases whereas the irregular hexahedral model (sphericity of 0.71) provides physically better retrievals, though these results ultimately require validation with future in situ measurements.”

Lines 828-830: “During ... properties.” You haven’t discussed this in the results section of the manuscript. Provide more details or delete.

We agree with the reviewer and have removed this statement from the revised manuscript. Upon review, we realized this conclusion was based on preliminary retrieval runs conducted before final model optimization. Fully describing those intermediate test configurations would require extensive additional text without contributing to the core focus of the paper.

Lines 830-831: “Overall, ... retrievals” Rephrase to some similar to: “spheroids cannot provide physical meaningful retrievals for high PLDR cases. Irregular hexahedrals provide more plausible retrievals, which need to be evaluated with in situ data.”

Changed as suggested. We replaced the text to “Spheroids cannot provide physical meaningful retrievals for high PLDR cases. Irregular hexahedrals provide more plausible retrievals than spheroids, and comprehensive evaluation with collocated in situ data is left for future work.”

Lines 832-838: Include in this paragraph the problems introduced by not considering the fine mode of marine particles.

Thank you for the suggestion. We added “However, without all three modes, the algorithm could not adequately fit the data. Without the marine mode, GRASP was unable to accurately reproduce the vertical profiles below the boundary layer and without the fine mode, it struggled to fit the radiometric intensity at the shorter wavelengths. Hence, the fine and marine mode retrievals were sensitive to the observational constraints provided to the retrieval.”

Line 983: “...fail in accurately in capturing larger, highly nonspherical particle properties...” revise according to main revisions point #1 and similar revisions above.

Revised the text as ‘’..capturing nonspherical dust particle properties, instead compensating by returning unrealistically low size and real refractive indices’’

Lines 993-995: “The inability - dust particles.” Does this mean that for another sphericity the PLDR observations would be reproduced? Discuss this. Change “high irregular dust particles” to “dust particles”.

We did not perform any tests on the other sphericity values. Changed “high irregular dust particles” to “dust particles”.

Minor revisions:

In some parts of the document the phrasing and syntax is not optimum. Although I include the main issues here, it is not possible to do it for all. Go through the whole text and revise accordingly.

Line 51: Replace “in the aerosol microphysical model as used in the retrieval algorithm.” with “in the aerosol microphysical model used in the retrieval algorithm.”

Changed as suggested

Lines 101-102: “(Lindqvist et al., 2014; ... Lin et al., 2018)” Include also the work of Gasteiger et al. (2011).

Changed as suggested

Lines 105-107: “However, Wang et al. (2024) ... exhibited irregular shapes.” Mention here that Wang et al. (2024) showed only Asian dust samples.

Changed as suggested. See line: ..analyzed the Scanning Electron Microscopy (SEM) images of various Asian dust and concluded that, out of the 449 atmospheric dust particles analyzed, only three were spherical and the rest exhibited irregular shapes.

Lines 187-188”: Replace “while higher sphericity values indicate a more compact shapes” with “while higher sphericity values indicate more compact shapes”.

Changed as suggested.

Line 190: The description of GRASP algorithm should be included in a different section, i.e. “2.3 GRASP algorithm”.

Thank you for the suggestion. We added a subsection as “Hexahedral Kernel in GRASP Algorithm”

It is better to use “GRASP” instead of “the GRASP”, change this throughout the document. Lines 190-200: These lines need change in phrasing and syntax.
Line 200: Change paragraph

Changed as suggested.

Line 203-204: Correct Kii(Q,λ,n,k,ε,rk) to Kii(Θ,λ,n,k,ε,rk), and “for ranges of extinction cross section (Q), size (rk), refractive indices (n + ik) and axis ratios(ε) shown in table 1” to “for ranges of size (rk), refractive indices (n + ik) and axis ratios(ε) shown in table 1”. Check if the above are typos, and not errors in your calculations.

Thank you for pointing this typo out. We changed the ‘Q’ to ‘Θ’

Lines 209-210: Replace “To evaluate ...GRASP algorithm,” with “To evaluate the aerosol retrievals from GRASP using other dust morphologies, we developed new hexahedral scattering kernels for the GRASP algorithm,”

Changed as suggested.

Lines 212: Replace “backscattering” with “backscatter”

Changed as suggested.

Lines 245-247: “We note that λ ... within the kernel files.” Equation 6 provides the conversion between the volume-equivalent size parameter and the maximum-dimension size parameter (originally used in TAMUdust2020). What this has to do with the invariance principle? Replace with “The structure of the GRASP kernels requires a reference wavelength to pin each size parameter to a specific radius within the kernel files.”.

Changed as suggested.

Lines 251-252: Change “However, ... lidar observations,” to “However, they find that the preferred metric for size equivalence is less obvious for angular dependent parameters like the phase matrix elements. Espinosa et al. (2022) shows that, in the context of polarimeter and lidar observations,”

Changed as suggested.

Line 271: “Total AOD of > 0.3 ” what is the wavelength?

Thank you for pointing this out. Changed to 0.3 at 532 nm.

Line 288: Change “DoLP cover scattering angles ranging from 162.3◦ – 75.3◦” to “DoLP cover scattering angles ranging from 75.3◦-162.3◦”.

Changed as suggested.

Line 381: Change “than that for sea and fine model” to “than that for sea and fine mode aerosols”.

Changed as suggested.

Table 2 (caption): “The variables had very weak a priori constraints of 0.06 pushing the retrieval toward the initial value.” Do you mean the opposite?

Thank you for pointing this out. Changed to: “The variables had very weak a priori constraints of 0.06”

Figure 2: Use the same y-axis range for the plots that show the differences (the plots in the middle and right).

A primary objective of this figure is to evaluate the differences in P11 and -P12/P11 between hexahedral and spheroidal particles when using effective radius versus volume-equivalent radius. While fixing the y-axis range across all panels would produce a cleaner visual presentation, maintaining independent y-axis ranges is essential for our analysis, as it allows us to discern subtle wavelength-dependent variations that would otherwise be obscured.

Figure 3: Correct the title of the plot, it should say “Case 1” not “Case 2”.

Thank you for pointing this out. We corrected the title of the plot.

Line 578: Change “and hexagonal retrievals” to “and irregular-hexahedral retrievals”.

Changed as suggested.

Lines 589-590: “The results were consistent, which increases confidence in the generalizability of our findings.” This does not prove much about the physical consistency of the results. Highlight this here.

We would like to clarify the importance of these tests and its implications. Utilizing the limited datasets we have of combined Lidar+MAP data of dusty scenes, we compare results from neighboring pixels to our test case that also likely contain dust. Retrievals from both shape models and difference that we see between the shape models were consistent and smooth from the main results reported in the manuscript. This improves confidence in the results as we can replicate findings across the range of a dust plume with slightly varying optical properties. While testing across different dust cases would have been ideal, this test suggests that this methodology of Lidar+MAP retrieval could be applied to future measurements beyond that of our single pixel test case.

Line 756: Replace “clean dust” with “dust-only”

Changed as suggested.

Line 757: “under clean conditions” What do you mean by “clean conditions”?

We meant in the absence of other aerosols. We removed the phase “under clean conditions” for clarity

Lines 765-766: “Since HSRL ...observations.” Make a new plot showing the comparison of the vertical retrievals of LR, with the measured LR at 532nm.

Although plotting the lidar ratio profiles would be interesting, we feel it would present redundant information since the individual extinction and backscatter profiles are already provided. To keep the manuscript concise, we chose not to include them.

Line 825: Replace “...using the spheroidal model” to “...using the spheroidal model of Dubovik et al. (2006)”

Changed as suggested.

Lines 968-969: “The bulk depolarization ratio ...size range.” Provide the formulas.

We added the formula for the bulk depolarization ratio as suggested.

Lines 984-985: Change “Interestingly, ... in Case 1.” to “This bias was not observed in Case 2, where the observed δ values were approximately 10% lower than in Case 1”.

Changed as suggested.

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