The study of “Combined Lidar-Polarimeter Dust Retrievals using Spheroidal and Hexahedral Particle Shape Models” of Greema Regmi et al., includes important additions to the current literature of the remote sensing of desert dust. First, it includes the new development for the well-established GRASP retrieval, here incorporating irregular-hexahedral particles for modeling dust shapes. Second, it provides first results and first evaluation of synergistic lidar+polarimeter dust retrievals. And third, it includes a very important (and long due) evaluation of the spheroid model of Dubovik et al. (2006) regarding its limitations in the backscatter. The paper is thus very helpful for the remote sensing community and it should be published in Atmospheric Measurement Techniques journal. This should be done though after addressing the following revisions, which I consider major, thus I will need to review the manuscript again after it is revised by the authors.

<u>Main revisions:</u>

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.

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.

As a general point, I would like to draw the attention of the more senior authors of the paper to some mistakes that are understandable for an early scientist, but not for seniors. Reviewing a paper is a difficult and time-consuming task, and let’s not forget that is volunteering work. We should all respect the time and effort of the reviewer, and make sure that the manuscript submitted for review does not contain (at least) typos, grammatical and syntax errors, as well as mistakes on well-known facts (e.g. the fact that $-P_{12}/P_{11}$ at 180° is always zero for randomly-oriented particles).

Also please note that I didn’t go through the Appendices A and B, but I revised Appendix C.

Revisions:

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

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)).

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

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).”

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.

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.

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.

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

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.

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

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”.

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.

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.

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 depedance, 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.

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).

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).

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?

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”.

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.

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.

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).

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?

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.

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.

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.

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

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.

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

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!

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.

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.”

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.

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

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.

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.

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.

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.

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

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.”

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.

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.

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.

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.

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

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

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

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.

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.

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

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.

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

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.”

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.

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

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).

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.

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.

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

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.

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

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.”

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

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

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”.

<u>Minor revisions:</u>

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.”

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

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

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

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

Line 203-204: Correct $Kii(\boldsymbol{Q},\lambda,n,k,\epsilon,rk)$ to $Kii(\boldsymbol{\Theta},\lambda,n,k,\epsilon,rk)$, 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.

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,”

Lines 212: Replace “backscattering” with “backscatter”

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.”.

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,”

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

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

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

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

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

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

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

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.

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

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

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.

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

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

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”.

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