Response to reviewer #2

1. I'm always nervous about claiming novelty in the abstract of a manuscript, since an author needs to be very sure that that is the case. You claim that this is the first reported one step synergistic retrieval of desert dust by a polarimeter and HSRL. That may be the case, but there is another manuscript with some of the same coauthors that was published a few months ago:

   Litvinov, P., Chen, C., Dubovik, O., Zhai, S., Matar, C., Li, C., Lopatin, A., Fuertes, D., Lapyonok, T., Bindreiter, L., Dornacher, M., Lehner, A., Dandocsi, A., Gasbarra, D., and Retscher, C.: Synergetic retrieval from multi-mission spaceborne measurements for enhanced aerosol and surface characterization, Atmos. Meas. Tech., 18, 7679–7716, https://doi.org/10.5194/amt-18-7679-2025, 2025.

   Perhaps the uniqueness of this manuscript is the comparison to an HSRL style lidar. In any case this manuscript was finalized just as you submitted this one, so it is understandable that it was not mentioned. That should be fixed in review though, and I think a few sentences would be of value about how this work differs with / complements that of Litvinov et al 2025 and the SYREMIS approach described in that paper.

   We thank the reviewer for the suggested literature. Litvinov et al., 2025 provides an interesting synergistic retrieval utilizing GRASP. We have added mention of this work to the manuscript (see line 126). However, there has not been any conventional (or HSRL) lidar and polarimeter joint retrieval of dust to the best of our knowledge, making this a key novelty of the study.

1. My main problem with this work is that it represents only two scenes of dust observed by a lidar and polarimeter so it is difficult to know if the conclusions are widely applicable. I look forward to confirmation by future analysis from other campaigns. I think the uniqueness of case 2 should be mentioned in the abstract/conclusions, since I understood it to be atypical as described in section 4.2.2.

   We completely agree that a broader dataset would strengthen the generalizability of these findings. As noted in the text, after an exhaustive search of available NASA campaigns over the ocean, these two cases were the only ones that had overlapping, cloud-free HSRL-2 and RSP dust measurements. We have now highlighted the unique and potentially atypical nature of Case 2 in both the Abstract and the Conclusions.

2. Here's an aside. If you or other readers of this review are looking for example airborne datasets, I recommend looking at:

Groß, S., Ewald, F., Stevens, B., Wirth, M., Dekoutsidis, G., Ehrlich, A., Kouklaki, D., Kruger, K., Rosenburg, S., Volkmer, L., von Bismark, J., Hirsch, L., Luebke, A. E., Marinou, E., Mayer, B., Pinol Sole, M., Wendisch, M., Windmiller, J., Amiridis, V., Koopman, R., Kubota, T., and Rapp, M.: Persistent EarthCARE underflight studies of the ITCZ and organized convection (PERCUSION): contribution to EarthCARE validation, Atmospheric Measurement Techniques, 19(11), 3933--3959 , https://doi.org/10.5194/amt-19-3933-2026, 2026.

For a list of older campaigns funded by NASA see table 5.1 in: Da Silva, A. M., Maring, H., Seidel, F., Behrenfeld, M., Ferrare, R., and Mace, G.: Aerosol, Cloud, Ecosystems (ACE) Final Study Report, National Aeronautics and Space Administration, https://ntrs.nasa.gov/citations/20205007337, 2020.

The challenge will be finding field campaigns that have both polarimeter and lidar and observations of dust.

Alternatively, an investigator could look at combined EarthCARE and PACE datasets. Those spacecraft have orbits such that they overlap once a day. See https://github.com/seanremy/pace-earthcare-matchups for a tool to find coincident observations. Thus PACE HARP2 or SPEXone polarimeter data could be combined with EarthCARE ATLID observations, the challenge being finding dust since orbit path overlaps tend to occur at about 40˚S.

> We spent time during the study searching for co-located cases between HARP2 and EarthCARE. As the reviewer correctly noted, their orbits primarily overlap in the Southern Hemisphere at approximately 40°S. While there are occasional dust events originating from the Patagonia region, our methodology specifically requires dust over the ocean, and we were unable to find any relevant cases suitable for our analysis. Furthermore, at the time of this study, both instruments were very newly deployed and still undergoing improvements for aerosol retrievals. For our analysis, it is critical that measurement uncertainties

are low enough to confidently distinguish the differences between the two shape models; using early data risked misinterpreting calibration artifacts as particle shape variations.

1. An interesting aspect of TAMUdust2020database is that it uses different methods for forward/side scattering than backscattering. How is transition between the two managed, and does it matter in the multiple scattering sense domain?

   In the TAMUdust2020 database, the transition between physical/improved geometric optics methods and the invariant-imbedding T-matrix method is managed via smooth interpolation in the size parameter domain to prevent artificial discontinuities. Because GRASP relies on pre-computed kernel matrices integrated over continuous size distributions, any residual single-particle transition noise is heavily damped during bulk integration and multiple scattering radiative transfer, resulting in stable simulations.

2. I think your parameterization scheme is useful for those planning polarimeter + lidar retrieval algorithms, where three aerosol modes are used with a spherical and a non-spherical coarse mode. However, you used the HSRL dust mixing ratio product to constrain retrievals. Do the same data used to make that product get used in your test retrieval? If so, how are you ensuring there is no circular logic? What is it about the dust mixing ratio product that contains information not already in use during a retrieval?

   While the backscatter and depolarization data are used to derive the HSRL dust mixing ratio, using it as a first guess does not introduce circular logic. GRASP treats this strictly as a soft constraint/initial guess for the vertical profile shape. The forward model must still reconcile the full spectral suite of RSP and HSRL measurements. The mixing ratio product provides a physically guided geometric starting point, ensuring the algorithm converges on the true global minimum rather than getting trapped in a local minimum.

3. Frankly, I was a little disappointed to see HSRL as the only lidar configuration. Can you speculate on the results if you had included a lidar that does not have sensitivity to extinction, ie a non-HSRL type lidar?

   We thank the reviewer for this point. Without independent extinction constraints, a conventional (non-HSRL) backscatter lidar retrieval depends entirely on the retrieved or assumed lidar ratio. Because retrieval accuracy for elastic systems depends heavily on specific forward-modeling and inversion assumptions, any quantitative performance estimate would be highly speculative. Therefore, we prefer not to include speculative estimates in the manuscript, but we have added a brief note clarifying our focus on HSRL.

Now for some more specific comments

- Lines 53-55: please cite PACE, HARP2, SPEXone and AOS mission publications, if they exist. Here's some that I know:

Werdell, P. J., Behrenfeld, M. J., Bontempi, P. S., Boss, E., Cairns, B., Davis, G. T., Franz, B. A., Gliese, U. B., Gorman, E. T., Hasekamp, O., Knobelspiesse, K. D., Mannino, A., Martins, J. V., McClain, C. R., Meister, G., and Remer, L. A.: The Plankton, Aerosol, Cloud, Ocean Ecosystem Mission: Status, Science, Advances, B. Am. Meteorol. Soc., 100(9), 1775-1794 , https://doi.org/10.1175/BAMS-D-18-0056.1, 2019.

Werdell, P. J., Franz, B., Poulin, C., Allen, J., Cairns, B., Caplan, S., Cetinić, I., Craig, S., Gao, M., Hasekamp, O., Ibrahim, A., Knobelspiesse, K., Mannino, A., Martins, J. V., McKinna, L., Meister, G., Patt, F., Proctor, C., Rajapakshe, C., Ramos, I. S., Rietjens, J., Sayer, A., and Sirk, E.: Life after launch: a snapshot of the first six months of NASA's plankton, aerosol, cloud, ocean ecosystem (PACE) mission. in: Sensors, Systems, and Next-Generation Satellites XXVIII 131920E) SPIE., 2024.

Werdell, P. J., Cairns, B., Caplan, S. A., Cetinić, I., Foley, S. R., Franz, B. A., Gao, M., Fasnacht, Z. T., Huemmrich, K. F., Ibrahim, A., Knobelspiesse, K. D., Mannino, A., McKinna, L. I. W., Poulin, C., Rajapakshe, C., and Sayer, A. M.: Advancing Earth System Science With the NASA Plankton, Aerosol, Cloud, Ocean Ecosystem (PACE) Satellite Mission, Global Change Biology, 32(4), e70869 , https://doi.org/https://doi.org/10.1111/gcb.70869, 2026.

McBride, B. A., Martins, J. V., Puthukuddy, A., Xu, X., Borda, R. F., Barbosa, H. M. J., Hasekamp, O., and Remerh, L. A.: The Hyper-Angular Rainbow Polarimeter-2 (HARP2): a wide FOV polarimetric imager for high-resolution spatial and angular characterization of aerosol and cloud microphysics. in: 70th International Astronautical Congress (IAC) , 2019.

Hasekamp, O. P., Fu, G., Rusli, S. P., Wu, L., Noia, A. D., aan de Brugh, J., Landgraf, J., Smit, J. M., Rietjens, J., and van Amerongen, A.: Aerosol measurements by SPEXone on the NASA PACE mission: expected retrieval capabilities, J. Quant. Spectrosc. Ra., 227, 170 - 184 , https://doi.org/https://doi.org/10.1016/j.jqsrt.2019.02.006, 2019.

Fu, G., Rietjens, J., Laasner, R., van der Schaaf, L., van Hees, R., Yuan, Z., van Diedenhoven, B., Hannadige, N., Landgraf, J., Smit, M., Knobelspiesse, K., Cairns, B., Gao, M., Franz, B., Werdell, J., and Hasekamp, O.: Aerosol Retrievals From SPEXone on the NASA PACE Mission: First Results and Validation, Geophysical Research Letters, 52(4), e2024GL113525 , https://doi.org/https://doi.org/10.1029/2024GL113525, 2025.

Vane, D., Moran, V., Piepmeier, J., Braun, S., Kirschbaum, D., Trepte, C., and Ivanco, M.: The Atmosphere Observing System (AOS): A core component of NASA's Earth System Observatory (ESO). in: 2022 IEEE Aerospace Conference (AERO) 1-7) , 2022.

Also you should note that AOS is cancelled.

We thank the reviewer for pointing this out. We added the citations for HARP2, SPEXone and AOS as suggested. We changed the sentence to future NASA Missions instead of AOS.

- Line 63: please cite supporting literature for statement that HSRL can distinguish aerosol types.

We added the citations for the statement that HSRL can distinguish aerosol types as suggested.

- Lines 76-77: please cite ORACLES campaign publications, like this one:

Redemann, J., Wood, R., Zuidema, P., Doherty, S. J., Luna, B., LeBlanc, S. E., Diamond, M. S., Shinozuka, Y., Chang, I. Y., Ueyama, R., Pfister, L., Ryoo, J.-M., Dobracki, A. N., da Silva, A. M., Longo, K. M., Kacenelenbogen, M. S., Flynn, C. J., Pistone, K., Knox, N. M., Piketh, S. J., Haywood, J. M., Formenti, P., Mallet, M., Stier, P., Ackerman, A. S., Bauer, S. E., Fridlind, A. M., Carmichael, G. R., Saide, P. E., Ferrada, G. A., Howell, S. G., Freitag, S., Cairns, B., Holben, B. N., Knobelspiesse, K. D., Tanelli, S., L'Ecuyer, T. S., Dzambo, A. M., Sy, O. O., McFarquhar, G. M., Poellot, M. R., Gupta, S., O'Brien, J. R., Nenes, A., Kacarab, M., Wong, J. P. S., Small-Griswold, J. D., Thornhill, K. L., Noone, D., Podolske, J. R., Schmidt, K. S., Pilewskie, P., Chen, H., Cochrane, S. P., Sedlacek, A. J., Lang, T. J., Stith, E., Segal-Rozenhaimer, M., Ferrare, R. A., Burton, S. P., Hostetler, C. A., Diner, D. J., Seidel, F. C., Platnick, S. E., Myers, J. S., Meyer, K. G., Spangenberg, D. A., Maring, H., and Gao, L.: An overview of the ORACLES (ObseRvations of Aerosols above CLouds and their intEractionS) project: aerosol--cloud--radiation interactions in the southeast Atlantic basin, Atmospheric Chemistry and Physics, 21(3), 1507--1563 , https://doi.org/10.5194/acp-21-1507-2021, 2021.

Thank you for pointing this out. We now added the citation as suggested.

- Line 106: you cite a paper by Wang et al 2024 about SEM images of particle shape. I worry that assessing shape that way doesn't necessarily correspond to shape as it is relative to polarimeter/lidar observations.

The reviewer is right about the fact That SEM images of the particles doesn't that way doesn't necessarily correspond to shape as it is relative to polarimeter/lidar observations. SEM images are often 2D projections that lack the complete 3D morphological and orientation information to which lidars and multi-angle polarimeters are sensitive. We included the Wang et al. (2024) citation just to highlight the fact natural dust is almost always highly irregular, rather than trying to draw a direct line between SEM visuals and the exact shape metrics our instruments retrieve.

- Lines 187-190: I had understood that the Dubovik spheroid model uses a distribution of aspect ratios. Is that something you could consider too?

The reviewer is correct that the Dubovik spheroid model relies on a distribution of aspect ratios to represent dust. TAMUdust2020 database also accounts for a distribution of aspect ratios and shapes. TAMUdust2020 database defines 20 distinct irregular hexahedral particles. Each of these is generated by randomly tilting the six faces of a regular hexahedron, resulting in particles that have aspect ratios ranging between 1.14 and 4.02. The database then uses various ensembles with different weightings (number mixing ratios) of these 20 particles to simulate the bulk optical properties.

- Line 213: reduced accuracy for what?

Thank you for pointing this out. We apologize for the lack of clarity here. What we meant was that the simulated optical properties become less precise for sphericity values below 0.69 because they rely on an extrapolation of the aspect ratio and sphericity relationship (Saito and Yang, 2021). We revised the sentence as "..they also note that the single scattering properties calculated for $\psi \leq 0.695$ have reduced accuracy due to extrapolation."

- Line 216 / Table 1: please identify size parameter units

We changed "Size parameter" to "Size parameter (Unitless)" in the table.

- Line 232 (eqn 4) why isn't this defined as C = 2Rv / D ?

The reviewer is right that mathematically, it simplifies perfectly to C = 2R_v / D. However, we intentionally chose to keep it in its expanded form in Equation 4 because we wanted to explicitly show the relationship between the sizes of the two shapes and how they can be converted into each other through a shape factor.

Section 2.3 generally: I'm still not entirely convinced that this effort to harmonize the meaning of the size parameter is worthwhile. Ultimately, it will always be an apples to oranges comparison. What matters is how well a model fits the data and if the retrieved parameters are reasonable, even if the treatment of shape in that model varies.

We agree that comparing these two shape models is an "apples-to-oranges" comparison. We included it because community questions frequently arise regarding how these models relate, and we felt it was necessary to show the extent to which they can be directly connected. Crucially, retrieved microphysical metrics (such as median radius) can vary by over 20% depending on the definition used. Quantifying this sensitivity is critical for accurately interpreting and reporting retrieved parameters.

- Line 287: please define what makes RSP measurements 'high quality'

Thank you for pointing this out. We originally meant to highlight the lower uncertainty, as well as the cloud-free conditions and the 110 viewing angles. However, we agree that the original phrasing was unnecessary. We have changed the sentence to: 'RSP made cloud-free measurements at a total of 110 viewing angles under a solar zenith.."

- Line 299: spell out acronyms and cite relevant papers for AirHARP, AirHARP2 and AirMSPI2

Thank you pointing this out. We now spelled out acronyms and cited relevant papers as suggested.

- Line 309: I know enough about RSP to know that it has SWIR channels that were probably inoperable for transit flights because cooling wasn't available. This should be noted somewhere and discussed. Presumably those SWIR channels contain unique information about the coarse size mode.

We agree that including shortwave infrared (SWIR) 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. With new instruments 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.

- Line 316: You used fixed uncertainties of 3% and 0.002 for I and DoLP respectively for RSP. However, there is a more detailed uncertainty model for that instrument. It probably wouldn't change your results, but for your awareness check appendix B of:

Knobelspiesse, K., Tan, Q., Bruegge, C., Cairns, B., Chowdhary, J., van Diedenhoven, B., Diner, D., Ferrare, R., van Harten, G., Jovanovic, V., Ottaviani, M., Redemann, J., Seidel, F., and Sinclair, K.: Intercomparison of airborne multi-angle polarimeter observations from the Polarimeter Definition Experiment, Appl. Optics, 58(3), 650--669 , https://doi.org/10.1364/AO.58.000650, 2019.

We thank the reviewer for bringing this detailed uncertainty model to our attention. We agree that utilizing the comprehensive model described in Knobelspiesse et al. (2019) would be more rigorous. We face a similar situation with the HSRL measurements, where the actual uncertainties are dynamically higher at the top of the profile and below the boundary layer than the fixed values we assumed. However, integrating these dynamically varying uncertainties would make the current retrieval problem extremely complex. While we recognize how important accurate error characterization is, we had to rely on fixed uncertainties for this iteration. We agree this is a valid limitation of our study. We have updated the "Additionally, note that this study relied on fixed measurement uncertainties for both the RSP and HSRL data to maintain computational tractability. Instrument uncertainties can vary dynamically, and detailed uncertainty models exist for instruments such as the RSP (Knobelspiesse et al., 2019). Future work should seek to incorporate these dynamic uncertainties to further refine the accuracy of the retrievals. Future studies addressing these effects will lead to more accurate dust retrievals but remain beyond the scope of this study.

- Line 320: are these details about GRASP and RSP geometry really necessary?

We thank the reviewer for this question. In our experience, aligning and matching instrument viewing geometries between different sensors (such as RSP) and algorithm coordinate frames (such as GRASP) is often non-trivial, time-consuming, and prone to misinterpretation. We intentionally included these explicit geometric details to ensure full transparency and reproducibility, providing a clear reference for researchers trying to combine these specific datasets in future studies.

- Line 359-359: The section about RSP does not speculate on the sources of uncertainty like is done for HSRL here. I think the HSRL sources of uncertainty is not necessary if not also describing the same for RSP.

Thank you for pointing this out. We revised the text to remove the details in the HSRL uncertainty.

- Line 357: that GRASP cannot account for systematic uncertainty is a major shortcoming of that algorithm, and the assessments of uncertainty for both RSP and HSRL2 are for combined systematic and random uncertainty. So in this assessment random uncertainties are overestimated and systematic are underestimated. Can you speculate on the impact of this on retrievals and subsequent manuscript conclusions?

  We would prefer not to speculate as there has been very little work on the impact of off diagonal elements of the error covariance matrix on retrieval products. We haven't conducted experiments to quantify this effect.

- Line 379: how about dust shape? Is this uniformly distributed vertically? I recall seeing presentations that it may not be, at least the Saharan plume.

  For each aerosol mode GRASP retrieves the spherical fraction, but otherwise we assume that the dust shape is uniformly distributed vertically.

- Table 2: I'm confused by the caption text "decreasing wavelength starting at 0.355 to 0.555 and increasing from 0.67 to 1.064." Why wouldn't you just present them in wavelength order?

  We apologize for the lack of clarity. Upon further review, we realized the sentence was redundant because the values of k are already listed in the table. We have removed the sentence from the revised manuscript to improve clarity.

- Table 2: Define what is meant by 'concentration'

  We apologize for lack of clarity. We changed to 'concentration' to 'column volume concertation'.

- Table 2: 'sphere fraction'. Is that by volume, optical depth, etc?

  It is sphere fraction by volume. Thank you for pointing this out. We now changed 'sphere fraction' to 'sphere fraction by volume' in the Table 2.

- Line 389: define a lagrange multiplier.

  We added the definition of the lagrange multiplier as suggested.

- Line 437: this classification scheme – was it applied for all test scenarios, including RSP only?

We apologize for the lack of clarity here. This was applied only when the Lidar measurements were used, i.e., in HSRL-only and HSRL+RSP. We added this to the manuscript.

- Line 476: “Exact same” is redundant colloquial term. Simplify to ‘same’

Thank you for pointing this out. We changed this as suggested.

- Line 487: I think you mean ‘increasing wavelength’

We apologize for the typo here. You are right that it should be increasing. We changed it in the manuscript.

- Figure 2: Define ‘hex_rv_eff’ etc in the labels and identify somewhere units of wavelength (which here are in microns but elsewhere in the text are in nm)

Thank you for pointing this out. We added the unit of hex_rv_eff’ in the caption for the clarification.

- Line 578: Do you mean a chi squared error metric is minimized? Or maybe defined what the weighting is for the RMSE minimization.

In GRASP, the optimization process minimizes a weighted multi-term objective function rather than an unweighted root-mean-square error (RMSE). The weighting factor for the RMSE minimization in GRASP is defined as the inverse variance of the measurement uncertainty.

Figures 7 and 8: I find the increase in retrieved refractive index for both case 1 and 2 for the longest and shortest wavelengths to indicate potentially non physical behavior. Could this be an artifact of the smoothness constraints or some other aspect of GRASP?

We thank the reviewer for this comment. The increase in the refractive index at 355 nm is consistent with the known physical behavior of mineral dust, which absorbs strongly in this region due to its mineralogy. However, we agree that the increase at 1064 nm is likely a retrieval artifact arising from multiple factors. We observe that this artifact is more pronounced for the spheroidal model than for the hexahedral model.

Additionally, the HSRL data incorporated at 355 nm and 1064 nm provide limited sensitivity to particle refractive indices. Using a smoothing order of 2 for the imaginary refractive index (k) may have introduced a GRASP overfitting artifact, similar to polynomial boundary oscillations. In the absence of coincident in-situ measurements for direct validation, we have revised the text to emphasize that our focus is on assessing the relative performance of the hexahedral versus spheroidal shape models, rather than claiming high absolute accuracy for individual refractive index retrievals.

Line 649/650. This statement about not using spheroid models if using lidar depolarization is important and should be repeated more strongly in abstract/conclusions

We have updated both the Abstract and Conclusions to explicitly and strongly emphasize that spheroid models should not be used when incorporating lidar depolarization measurements