

Reply to Referee #2

We sincerely thank the Referee for his/her thorough and insightful evaluation of our manuscript. We greatly appreciate the constructive comments and the positive feedback on the scientific value and clarity of our work. The points raised have been extremely helpful in identifying areas for improvement and clarification. We have carefully addressed each of the comments below and made the necessary revisions to strengthen the manuscript. We are confident that the changes have significantly improved the quality and readability of the paper. Below, we provide a detailed, point-by-point response (in red) to the Referee's comments (in black).

Referee's response

The authors have addressed my major concerns with respect to the previous submissions adequately and implemented an updated methodology. Overall, these revisions resulted in a manuscript that differs significantly from the original submission and reflects a fundamentally new and physically consistent implementation of the method. I really appreciate the efforts of the authors to provide a good overview of the changes due to the new methodology but also give detailed insights. That helped a lot. Thanks.

As a consequence, I think the manuscript is now almost ready to be published and I have only some few minor corrections/questions. Furthermore, I acknowledge very much the efforts from the authors making the created data set publicly available even though the LIVAS data set itself is available only upon request.

We sincerely thank the Referee for the continued, thorough, and constructive evaluation of our manuscript throughout the review process. We truly appreciate the positive assessment of the revised methodology and the recognition that the manuscript now reflects a physically consistent and substantially improved implementation compared to the original submission. We are also grateful for the Referee's acknowledgment of our efforts to clearly document the methodological changes and to make the derived data set publicly available within the existing data policy constraints of LIVAS.

We are pleased to see that the manuscript is now considered almost ready for publication. Below, we address the remaining minor comment and question in a point-by-point manner.

General comments

The current methodology seems logical and consistent. However, I wonder how the reference height z_0 is determined or set. I think this is not stated in the manuscript. Please do so.

Below find some comments, please note that all line numbers refer to the track changed version-888-ATC2.pdf

27. The wording "physically meaningful" in the abstract is a bit confusing. What do you mean by that? Can you explain in more detail or rephrase the sentence?

We thank the Referee for this helpful remark. By "physically meaningful," we intended to refer to the known underlying dependence of dust lidar ratio on the mineralogical composition of dust particles. However, as the Referee correctly points out, our study does not explicitly investigate the chemical or mineralogical composition, and therefore the term may indeed be misleading in this context. To avoid confusion, we have removed this expression and revised the sentence in the abstract accordingly.

310: what are uniform extinction profiles? uniform wrt what? Please describe more clearly.

We thank the Referee for this helpful comment. The Referee is right that the term "uniform" can be misleading in this context. What we intended to describe were extinction profiles that do not exhibit cut-offs introduced by the filtering procedure. In some cases, the averaging of profiles can result in missing (NaN) layers due to the quality filters applied in the LIVAS dataset (following Tackett et al., 2018). If such profiles are retained, the resulting mean profiles may be biased low because part of the extinction layer is effectively removed.

To avoid this potential underestimation, we exclude profiles that contain these filtered layers. Since the term "uniform" was not sufficiently precise, we have removed it and clarified the wording in the manuscript.

311: I also don't understand the wording "without any vertical cut off". What do you mean with that? Please rephrase and describe more exactly.

We thank the Referee for pointing out that this wording was unclear. By this, we refer to extinction profiles that contain vertically missing layers introduced by the filtering procedure applied along the CALIOP overpass. In some cases, the LIVAS quality screening (following Tackett et al., 2018) flags one or more altitude bins and assigns them NaN values within an otherwise valid extinction profile.

If such profiles are included in the calculation of mean extinction profiles, the absence of these layers can lead to an artificial reduction of the vertically integrated extinction and, consequently, to an underestimation of the derived optical depth. To avoid this issue, we exclude profiles containing such vertically missing layers. We have now rephrased the corresponding text in the manuscript to describe this more precisely and avoid ambiguous wording.

To clarify the previous two comments, we have revised the manuscript text as follows: "(ii) there should be no disturbance in the mean profiles due to filtering processes (i.e., extinction profiles without missing vertical layers caused by filtering along the overpass)."

We hope that the revised wording now clarifies the previous two points and avoids any potential confusion.

329: Either "dust (A)OD" or "DOD" but not "dust DOD".

We thank the Referee for pointing this out. We agree and have corrected the wording accordingly in the revised manuscript.

385: ... "the estimation of the uncertainties of the random uncertainty" what do you mean with that?

We thank the Referee for this comment. The wording was indeed unclear. We intended to refer to the estimation of the random uncertainty. The sentence has been corrected accordingly in the revised manuscript.

398: geometric AIRMASS factor-> airmass is missing

We thank the Referee for noting this. Corrected accordingly.

433 ff: How do you determine the reference height used in your equation? I think it is nowhere stated.

We thank the Referee for raising this point. The reference height in our retrieval is defined as the maximum altitude available from the CALIOP altitude, i.e., approximately 30 km. Above this, the atmosphere is assumed to be essentially aerosol-free, so the signal is dominated by the molecules' effect, which makes this level suitable as a reference height. We have now clarified this explicitly in the revised manuscript.

455: Please describe which synthetic profiles you have used and where do they come from.

Section 3.3.1. Out of curiosity, how long is the calculation time with your method?

We thank the Referee for this comment.

The synthetic profiles have been generated with a python code that generates random profile structures. The code assumes up to three Gaussian layers with variable amplitude and width (limits can be provided by the user). The profiles are forced to reach 0 at the last height bin. The first layer contributes always partially because its peak is adjusted so that it is located close to the start from the profile to avoid having very small values close to the ground which would be unrealistic. A random structure of configurable amplitude and frequency (e.g. 100 m frequency) is added to simulate fine aerosol layer structures that can be usually seen in the atmosphere. Seeds are being used so that the results of the algorithm are still random but repeatable. This is achieved by seeding the random generator functions with specific seed numbers. The profiles are scaled so that their vertical integral is 1. For the construction of the aerosol backscatter and extinction profiles we generate a number of profiles that are used as a basis for constructing the extinction profile. We scale each of those profiles with the desired AOD (ranging from 0.1 to 1). For each profile and each AOD class, we divide with a constant lidar ratio (ranging from 20 to 68) to generate the corresponding backscatter profile. Our final aerosol backscatter/extinction profile dataset is 3-dimensional (profile, AOD, lidar ratio dimensions), with the extinction profiles being always the same for a given lidar ratio class.

Regarding computation time, the minimization of Y is computationally inexpensive. The retrieval for a single dust case requires on the order of seconds on a standard desktop computer, making the method suitable for large datasets.

Eq. 10, what does the index k stand for?

We thank the Referee for noticing this omission. The definition of the index k has now been added to the manuscript, where we clarify that it refers to the individual case within the i -th grid cell.

557: References given in brackets: are you sure these references cover all Middle East dust or at least describe the differences between the major dust sources? If not, the statement is a bit misleading. I guess many of the above-mentioned references have not proposed to use lower dust lidar ratios over the Middle East explicitly. Please formulate more precise.

We thank the Referee for this important remark. The Referee is correct that the cited studies do not explicitly propose the use of lower lidar ratios for the Middle East. Rather, they report lower lidar ratio values for dust layers associated with sources in this region. In addition, some of the references concern North African dust rather than exclusively Middle Eastern sources.

To avoid any potential misinterpretation, we have revised the text to make this distinction clearer. The sentence now reads:

“While the outcome over North Africa is in good agreement with previous studies based on ground-based lidar observations, a contrasting pattern is observed over the Middle East, with earlier studies reporting lower dust LR values.”

We hope this revised wording clarifies the point and avoids confusion.

625: “higher values” with respect to what?

We thank the Referee for this comment and apologize for the lack of clarity. In this context, “higher values” refers to the magnitude of the retrieved dust lidar ratios compared across the different desert regions analyzed in our study.

Specifically, our results show that lower dust LR values are generally observed over the Middle East, whereas comparatively higher values are found over North Africa. For example, over the Arabian Peninsula the adjusted dust LR values range from about 38–45 sr in the central and southern regions to about 46–54 sr toward the northern Arabian Peninsula and Mesopotamia, with most values remaining at or below the default CALIPSO dust LR of 44 sr.

715: “dust LR hotspots restricted mainly over the major dust sources.” This statement is a bit confusing. LR is an intensive particle property. Thus, why are high LR hot spots restricted to major dust sources? Please explain/discuss a bit there.

We thank the Referee for this thoughtful comment. We agree that the lidar ratio is an intensive optical property and not directly determined by source strength alone. Our interpretation is that the spatial patterns we observe may be related to differences in dust mineralogical composition among source regions, which can influence particle absorptivity and thus the resulting lidar ratio.

At the same time, we acknowledge that our study does not explicitly analyze mineralogical composition, and therefore this explanation remains a plausible interpretation rather than a demonstrated conclusion. However, as noted in line 1070 of the manuscript, a dedicated analysis combining our retrievals with independent mineralogical information (e.g., EMIT-based products) is planned as a next step to further investigate this aspect.

731 and other locations in the text: It is worth mentioning that when discussing the LR you are referring to 532 nm only. The references you have given in this paragraph for example give LR values at multiple wavelengths.

We thank the Referee for this important remark. The Referee is correct that our analysis refers specifically to lidar ratios at 532 nm, and we agree that this should be stated explicitly to avoid ambiguity. In the revised manuscript, we have therefore added a lot of times “at 532 nm” in the relevant parts of the text wherever lidar ratios are discussed.

The Referee is also right that not all cited studies report values at 532 nm, as some provide lidar ratios at other wavelengths (e.g., 355 nm). When referring to the literature, our intention was either to highlight differences between dust source regions or to provide a broader literature context on reported dust lidar ratio values. These source-related differences are generally observed across wavelengths (e.g., as summarized in Floutsi et al., 2023; Table 1).

942: “In elastic lidars (such as CALIOP), for the derivation of the aerosol extinction and backscatter coefficients it is required a factor known as the lidar ratio.” This sentence is hard to understand, maybe reformulate? -> e.g. For the retrieval of the particle bsc. And ex. Coeff with elastic lidars (as CALIOP), the lidar ratio is needed as a-priori input.

We thank the Referee for this helpful suggestion. We agree that the original wording was not sufficiently clear. Following the Referee’s recommendation, we have reformulated the sentence to improve clarity. The revised text now

reads:
“For the retrieval of the particle backscatter and extinction coefficients with elastic lidars (such as CALIOP), the lidar ratio is required as a-priori input.”

970: Maybe discuss pollution contribution here already briefly. Even though you have a strict definition of dust cases, slight pollution cannot be ruled out I guess....

We thank the Referee for this valuable suggestion. The Referee is right that, even with a strict dust-case definition, a minor contribution from pollution or other aerosol types cannot be fully excluded. This aspect is now clarified in the manuscript.

Specifically, we note that elevated uncertainties over parts of the Middle East and Central Asia may reflect the convergence and mixing of dust plumes from different source regions, involving mineralogically distinct dust types, as well as potential aerosol-type misclassification in CALIPSO retrievals. To clarify this point, we have revised the text where misclassification is first introduced to read: “Additionally, such misclassification may contribute to the observed spread in LR. For example, this may include pollution over the Middle East or misclassified smoke..”

972: “... the total LR uncertainties remain relatively low (typically ~2–5 sr)...” please state to what LR you are referring to, i.e. its not the uncertainty of the fixed CALIOP LR but your adapted one.

We thank the Referee for pointing out this lack of clarity and apologize for the confusion. The uncertainties indeed refer to our retrieved (adapted) dust lidar ratios, not to the fixed CALIOP LR. To make this clearer, we have revised the text. The sentence now reads:
“In addition to the dust LR maps, the estimated uncertainty fields further reinforce the robustness of the retrieved regional patterns.”

This revised wording makes explicit that the uncertainties correspond to our retrieved dust LR fields.

1030: two synergies -> two synergistic methods

We thank the Referee for pointing this out. The wording has been corrected to “two synergistic methods” in the revised manuscript.

1066: direct radiative effect -> of what? Please state directly

We thank the Referee for this comment. We clarify that the term refers to dust-induced direct radiative effects. The text has been revised accordingly to state this explicitly.

1067: “updated CALIPSO dust profiles” -> “the updated CALIPSO dust profiles”

We thank the Referee for this suggestion. The text has been revised accordingly.

1070: You may refer here not only to Ansmann and Müller, 2005 but also newer research in this topic, e.g. <https://doi.org/10.1029/2025JD043829> and <https://doi.org/10.5194/acp-25-9737-2025>

We thank the Referee for this valuable suggestion. We agree and have added the proposed recent references to complement Ansmann and Müller (2005) and to provide a more up-to-date perspective on this topic.

1075: You may also add here the 532nm HSRL in space (<https://doi.org/10.5194/amt-17-1879-2024>)

We thank the Referee for this helpful suggestion. We have added the recommended reference and revised the text accordingly. The sentence now reads:

“A future assessment and validation of our findings will be possible through comparison with vertically resolved lidar ratio observations from the ATLID lidar onboard the EarthCARE satellite (Illingworth et al., 2015; Wandinger et al., 2023; Wehr et al., 2023), as well as with 532 nm high-spectral-resolution lidar (HSRL) measurements from the ACDL instrument onboard the DQ-1 satellite, launched in April 2022 (Dai et al., 2024).”

1090: The AERONET acknowledgement is not needed anymore?

We thank the Referee for this comment. The AERONET acknowledgment has been removed because AERONET data are no longer used in the revised version of the study. Specifically, the AERONET comparison figure included in the original manuscript was omitted. With the updated methodology, the MIDAS (POLDER-3/GRASP) DOD and the recalculated CALIOP DOD (using the revised LR and iterative solution of the Klett equation) are consistent, so a comparison with AERONET would essentially reflect a MIDAS (POLDER-3/GRASP)–AERONET evaluation driven by CALIOP sampling.

Moreover, such evaluations have already been extensively performed in previous studies. As noted in the manuscript, MIDAS and POLDER-3/GRASP AOD products have demonstrated strong performance in comparisons against AERONET observations (Dubovik et al., 2011; Li et al., 2019; Gkikas et al., 2021; Zhang et al., 2021).

1093: Please make clear that the provided data is the result of your work. Would it be possible to provide the updated dust-only profiles also?

We thank the Referee for this important comment. We have now clarified in the revised manuscript that the provided dataset reflects the outcomes of our methodological developments and analysis.

In accordance with the data providers' policy, access to the LIVAS dataset is currently granted upon request, as stated in the Data Availability section of the manuscript, where the relevant contact information is provided. We also note that the dust lidar ratio dataset produced in this work is publicly available, and when combined with the LIVAS backscatter profiles, interested users can perform the corresponding conversions to dust-extinction profiles independently.

To make clear that the provided data is the result of our study we adjusted as:

“The annual and seasonal gridded dust lidar ratio (LR) dataset produced through the CALIOP–POLDER-3/GRASP and CALIOP–MIDAS synergies over North Africa and the Middle East within the framework of this study is publicly available at <https://doi.org/10.5281/zenodo.18016524> (Moustaka et al., 2025).”

Comments to the Supplement:

S2.1: Please explain HERA, please give the reference for Young et al. 2008.

We thank the Referee for pointing this out. We apologize for the lack of clarity. HERA refers to the Hybrid Extinction Retrieval Algorithm, which also utilizes an optimized iterative retrieval for isolated aerosol layers and is described in detail by Young et al. (2008). We have now spelled out the acronym and added the appropriate reference in the revised manuscript. “...

The scope of this paper is to determine the lidar ratio of pure dust cases over dessert regions.” which paper? the statement is ambiguous. I guess you mean your own one? and please change dessert to desert.

We thank the Referee for pointing this out. We agree that the wording was ambiguous. We have revised the sentence to refer explicitly to the scope of our study and corrected the typo (“dessert” → “desert”) in the revised Supplement.

“..isolated elevated layers, a forward Klett inversion is necessary” – it's basically not necessary, but done this way...

We thank the Referee for this helpful remark. We agree that “necessary” was too strong in this context. Our intention was to indicate that a forward Klett inversion is typically applied in this configuration rather than being strictly required. We have revised the wording accordingly in the Supplement. The text now reads: “...with the exception of isolated elevated layers, a forward Klett inversion can be applied.”

Below EQ. S2.1: “Here z is the range from the lidar, ζ is a surrogate variable used for range in the nested integral, z_0 is an aerosol free range...” how is z_0 determined -> please explain

We thank the Referee for this comment. As explained in our response above, z_0 corresponds to the highest CALIOP altitude (~30 km), where the atmosphere is assumed aerosol-free. This has now been clarified in the manuscript.

Below EQ. S2.4: “transmission from the laser beam emission point up to the reference range z_0 .” -> formulation not correct. Could be also “down to the reference height” when thinking of space-borne lidars.

We thank the Referee for pointing this out. We agree that the wording “up to” may be ambiguous in the context of spaceborne lidars. We have therefore revised the text to read “transmission from the laser beam emission point to the reference range z_0 ” to ensure neutral and physically consistent phrasing.

“We will replace the left part of Eq. (S2.10) in Eq. (S2.11).” -> “We will insert Eq. (S2.10) in Eq. (S2.11).” ?

We thank the Referee for pointing this out. We have corrected the wording to “We will insert Eq. (S2.10) into Eq. (S2.11).”

References

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