Because the original manuscript is largely revised, please see the relevant changes made in the revised manuscript in the marked-up version.

Reply to Referee #1

We sincerely appreciate the supportive and thoughtful remarks from the anonymous reviewer. Pleases find our point-by-point response below.

The numbers of line, equation, figure, table and section in red refer to the original manuscript, whereas in blue refer to the revised manuscript. The references cited in the responses are listed at the end.

The manuscript presents an in-depth description how three scattering models perform for the retrieval of non-spherical mineral dust particles from remote sensing measurements. It is of great interest to find a good representation of the irregular shape of dust aerosol particles in optical models. The figures are well prepared and the text is written in a clear, but rather descriptive manner. However, I have major concerns whether the content fits to a publication in Atmospheric Chemistry and Physics. In the following, I will describe my concerns.

1. The title states "Retrieval of microphysical properties …" And you retrieve microphysical properties, but we don't know if they are correct. The manuscript merely describes the differences between two scattering models, namely the spheroid model and the irregular hexahedra (IH), and the calculations using spheres. The models are applied to synthetic data and to real world lidar measurements. However, the authors just report the retrieved microphysical properties without any validation. It remains unclear which of the non-spherical models retrieves more realistic values. The only validation briefly discussed is provided by an AERONET photometer retrieval which is as well based on the spheroidal model (stated very late in the text). Without a validation by independent observations, ideally with in situ measurements, I don't see that the paper fills in the scope of ACP and might be better submitted to another journal.

A1: We believe that the quality of aerosol microphysical properties retrieved by inversion of optical measurements depends on two factors: (1) the capability of the scattering model to reproduce the measurements; (2) the performance of the scattering model in the inversion procedure. The factor (2) can be evaluated through numeric simulations as presented in many previous studies on numeric inversion of remote sensing data (Dubovik and King, 2000; Li et al., 2019; Müller et al., 2019). At the same time, we realize that the lack of independent observations for a closure validation is the main limitation of this study. Therefore, in the revised manuscript, we carefully stated this limitation (L845-L848, L957-L958) and explained the motivations of the retrieval simulations (L496-L502, L515-L523). Furthermore, we

(1) complemented the comparison between the model-simulated optical properties with previous lidar measurements (Sect. 3.1, Sect. 6.1);

(2) added a discussion section (Sect. 6.2) where a comparison of the retrieval simulation with previous studies is presented;

(2) added a discussion section (Sect. 6.3) where a detailed comparison of the real case retrievals with previous in situ/laboratory dust size, CRI and SSA results is presented, and where a more critical discussion of the comparison with AERONET retrievals is presented.

2. From an atmospheric science point of view, it is even more problematic, that you move in a circle while calculating the optical properties with the IH model and then invert these optical data to microphysical properties using the same model (Section 4). It is no surprise that the 3+2+3 inversion with the IH model leads to the best agreement if the optical data were calculated with this model before. Looking at an irregular hexahedron and describe it with a spheroid will of course lead to some differences when it comes to shape-dependent properties. But the driving question is how do we best describe the real dust particles.

   The same circle appears in Section 5 where you retrieve the microphysical properties with the IH model and then calculate with the same model (and the other models) back the optical properties like lidar ratio and depolarization ratio. This is not a real comparison.

A2: As mentioned in the previous answer, inverting synthetic optical data (Sect. 4) enables to separate the retrieval accuracy influenced by the "inverse performance" of a scattering model (e.g., the sensitivity, ill-posedness, condition number etc. of the inverse system) from that governed by the capability of accurately reproducing real measurements (this part is studied in Sect. 3), because: (1) the "true state" of aerosol microphysics is a priori known; (2) no modelling error is introduced to the synthetic optical data. Indeed, better results are expected from the 3 + 2 + 3 inversion than the 3 + 2 inversion. However, we hope to quantify the improvement brought by the PLDR measurements since more and more state-of-the-art lidar systems are capable of performing spectral depolarization measurements which have been proved to be informative in aerosol typing. Compared to previous studies concerning the PLDR inversion (e.g., Müller et al., 2013; Tesche et al., 2019), we try to comprehensively understand the PLDR inversion performance under different scattering models and wide ranges of microphysical states by simulations. At the same time, we noticed that it indeed makes less sense to use the spheroidal model to invert optical properties generated by the IH model, and vise versa. Therefore, we removed the relevant section (Sect. 4.3 in the original manuscript).
Section 5 allows to directly compare the retrieval differences caused by the use of different scattering models by inverting the same real lidar observations and substantiate conclusions drawn in the simulations. The purpose of recalculating from the retrieved microphysical properties back to the measurements is to visualize the fitting error. Sometimes large fitting error can be found due to modelling error or ill-posedness. So the magnitude of the fitting error is considered as a measure of inversion quality (e.g., Dubovik et al., 2006; Fedarenka et al., 2016; Lopatin et al., 2021)

3. The settings used for the calculations appear arbitrary (although to a certain degree reasonable) and are not based on literature or sensitivity studies. The three size distributions used 4.1 are arbitrary and I don't see why they should fit to the observations. There are numerous measurements of dust particle size distributions in literature. Why don't you base your assumptions on them? Or at least explain why you choose certain settings. The same holds

for the 3+2+3 data set. What about 3+2+1 or 3+2+2? Have you tested your results with only one depolarization ratio as input as well? It seems arbitrary or at least not explained why you have taken 3 depolarization ratios.

A3: According to your comments, we made a major revision to Sects. 3, 4. First, the setting of aerosol microphysical properties is now based on a more comprehensive survey of previous in situ and laboratory measurements of dust aerosols (Sect. 2.2): the effective radius covers the range 0.1-5 µm, and the CRI is based on the results of Di Biagio et al. (2019). Second, the reason for choosing the 3+2+3 data set lies in the consideration that it is a configuration well implemented in many state-of-the-art lidar systems, for example LILAS, while in the revised manuscript we tested the results with different combinations of depolarization ratios as input, for both the IH and spheroidal models. Please read the Sect. 4 of the revised manuscript for more details.

4. The work is not properly set into the context of previous literature. The discussion section which is commonly used to place the new findings in the scientific context does not contain any citation. Over wide parts in Section 3, 4, 5 & 6 the manuscript just describes the findings and does not discuss them. Why do we see a certain behavior? – Sometimes it is shortly mentioned. What is new? What was not known before? And many findings were known before, e.g., the comparison between spheres and spheroids. This prior knowledge was not used or is not properly acknowledged by the authors.

A4: We have been fully aware of this shortcoming and added more discussions combining previous studies and comparisons with findings from previous literatures, especially for the discussion section in the revised manuscript (Sect. 6). Given sufficient previous discussions on the contrast between the spheres and spheroids (e.g., Veselovskii et al., 2010), we reduced the contents related to the discussions on the spherical model and kept a small part for illustrating our consistency with the previous findings. Please read the revised manuscript for more details.

Specific comments on the sections

1. General
    1. Please explain abbreviations at first instance, e.g., AERONET, SAMUM, VIS, NIR.
    2. I still have troubles with the naming of the models: Sphere model or spherical model, Spheroid model or spheroidal model?
    3. While reading, I sometimes got lost in the results and lost the track why you are doing it. Or in other words, the storyline behind is sometimes not clear. It would be recommended to have a short introduction at the beginning of each section.

A1.1: They have been explained at first instance in the revised manuscript.

A1.2: Both “spheroidal” and “spheroid” were ever used by previous literatures. For clarity, we exploited the terms: “spherical model”, “spheroidal model” and “IH model” throughout the revised manuscript.

A1.3: A short introduction at the beginning of each section has been added.

2. Sections 2.1 & 2.2
    1. BOREAL was described in Chang et al., 2022 and it is good to give a short recap of it. By why do you not mention that it is a Maximum Likelihood Estimation. I find this fact rather central. What are the advantages and disadvantages of this method?
    2. L137: Which studies? You certainly need to consider several previous studies.
    3. What are the limitations of the spheroids and the IH?
    4. Here and also in the introduction, you completely omit the approaches which use the discrete dipole approximation (DDA). Why? They provide some realistic particle shapes.
    5. L173-175 Please provide a formula how you converted the diameter.
    6. How comparable are the results of the two particle shape models? How do you ensure to use the same shape distribution? The results probably depend (strongly or not) on the assumption of the shape distribution. Do you choose a sphericity for the IH which matches the shape distribution assumed for the spheroid model?

A2.1: We added that BOREAL was based on the maximum likelihood estimation in the revised manuscript. Compared to other retrieval methods based on constrained linear inversion, main advantages of BOREAL include the convenient way of accounting for a priori constraints of different types and improvement of retrieval efficiency (please see L123-L126 in the revised manuscript). However, it cannot provide effective constraints as particles become larger, which leads to underestimates of particle size and is known as its main disadvantage. On the other hand, since the limitation of the maximum wavelength, it is a universal limitation for the inversion of lidar measurements, regardless of the algorithms. Detailed explanation can be found in L530-L539 of the revised manuscript.

A2.2: For example, Mishchenko et al. (2002) and Dubovik et al. (2006) found the spherical model cannot reproduce the flat angular variation of laboratory-measured dust phase function at side and backward scattering angles. However, since we adjusted the structure of Sect. 2 and for the sake of clarity, we removed this sentence in the revised manuscript.

A2.3: The main limitation of the spheroidal model is that the computational accuracy deteriorates at the backward direction due to the limitations of the used geometric optical method, which could be one of the reasons causing the discrepancy between the simulated backscattering properties and real lidar measurements (L279-L286). For the IH model, since it is a quite recent model, we have not found any studies that report the limitation on the used computational methods; a potential limitation could be that it does not account for the surface roughness, but we are not able to find any publications discussing this influence on dust-size irregular particles, either.

A2.4: We noticed some modellings of dust backscattering properties using the DDA and deformations of spheroids and ellipsoids to characterize dust shape, for example, the study of Gasteiger et al. (2011). However, unlike the spheroidal and IH model, we cannot find their published database that are applicable to lidar inversion. As a completement, we briefly introduced more shape models and scattering computation methods other than the spheroidal and IH models, and discussed their limitations for lidar inversion applications in the revised manuscript (L252-L266).

A2.5: A formular has been provided as Eq. (11) in the revised manuscript.

A2.6: To make the particle size comparable, we use the volume-equivalent radius as the size descriptor for both spheroidal and IH models. The axis ratio distribution for the spheroidal model is fixed to the retrieval of the laboratory measurement of Volten et al. (2001). The degree of sphericity for the IH model is set to 0.71. We cannot ensure "the same shape distribution" between the two models because they represent particle shape and the shape distribution in quite different ways. We ever tried to retrieve the degree of sphericity of IH particles but failed due to the underdetermination. So, we fixed this parameter to a value that was advised to characterize dust particles by many studies (L312-L314), and compare the IH model with the spheroidal model. We agree that the change of degree of sphericity will lead to changes in optical properties but it is out of the scope of this study. We pointed out this perspective in the conclusion section (L959-L961).

3. Section 2.4
    1. Section 2.4.1 needs to be updated. There are several studies conducted in the last 15 years concerning the size distribution of mineral dust and the contribution of fine and coarse mode dust and its changes during transport process. Furthermore, it remains unclear if the morphology is expected to change with source region or not. Overall this section is more a lose collection of facts and needs to be straighten. What do you want to tell me? And how are these findings linked to your own research? With this general literature section, you do not explicitly motivate the choice of your size distributions in Sect. 4.1.
    2. L233 What are the a priori constraints for m_R?

A3.1: The Sect. 2.4.1 has been updated with more recent studies on dust size distribution and its changes during transport process (transformed to Sect. 2.2.1 in the revised manuscript). Furthermore, we explicated the size and source independence of dust aspect ratio distribution by citing the results of Huang et al. (2020) (L193-L197); we summarized previous in situ results of dust coarse-mode distributions with respect to different sources, transport times and measurement methods (Table 1). Then the choice of size distributions for the modified simulations in Sects. 3, 4 is based on these studies.

A3.2: The a priori value and a priori standard deviation for m_R are 1.5 and 0.5, respectively, as indicated in L230 of the revised manuscript.

4. Section 3
    1. Please add subsections to make it easier for the reader.
    2. You rarely set your results into context of previous findings. Especially in this basic section, many findings were known before or observed in similar studies. Reading your manuscript evokes the impression you are the first ones to observe this behavior of the optical properties. You may reduce some of text and put a stronger focus on the comparison between spheroids and IH.
    3. Furthermore, the findings are not compared against observations from laboratory and field measurements. It remains unclear whether the reported results are found in reality or if they are "just" an output of different models.
    4. It would be good to show three typical size distributions for three ranges of r_eff which you are discussing. You may add it as an additional subplot in Fig. 3. The constraint of V_t =1 (L315-316) will then become more visible.
    5. L317-319 Why do we see this behavior?
    6. L325-328 Why do we see this behavior?
    7. Fig 4f: It seems that the blue line goes to negative depolarization ratios for large particles. Can it really be the case?
    8. Fig 4: You may add a dashed line at r_eff = 1.25µm to enhance the link to Fig. 5+6.

A4.1: Subsections have been added.

A4.2: Following your advices, we made major revision of Sect. 3. Specifically, we (1) deleted the contents describing already well-known non-spherical scattering properties but less relevant to this study (e.g., angular variations of the phase matrix) and focus on backscattering properties instead; (2) more concentrated on the behaviors which influence the settings of following simulations and the interpretation of the retrieval results, with proper citations; (3) added two subsections discussing the influence of m_I spectral dependence (Sect. 3.2) and the sensitivity of depolarization measurements (Sect. 3.3).

A4.3: We added contents about comparison of the model-simulated EAE, BAE, LR and PLDR with real lidar measurements (summarized in Table 2 of the revised manuscript). The choice of size distributions and CRIs is based on the literature survey in Sect. 2.2. We drew the conclusion that the results simulated with IH model are better in the ranges of real measurements than the spheroidal model (L439-L441).

A4.4: We adjusted the setting of size distribution in the section of retrieval simulation: we abandoned the “three typical size distributions” and tested a wider r_{eff} range (0.1-5 μm) for better representing dust particles.

A4.5: The main reason could be that these particles have similar averaged projected areas if they share the same volume-equivalent radius, since the bulk extinction coefficient scales with the averaged projected area. However, we noticed that this expression is not rigorous enough because this behavior only holds for particles apparently larger than the wavelength. In addition, by rescaling the figure, we found the extinction coefficients of non-spherical particles are in fact higher than that of spherical particles with the increase of r_{eff}. Thus, we revised the expression as L369-L372 and converted Fig. 4 to Fig. 3 in the revised manuscript.

A4.6: We attribute the inherent reason for the PLDR difference between spheroidal and IH particles to the contrast of particle shape. Compared to the spheroidal particles, the IH particles are more irregular with asymmetric surfaces that could cause more complex inner reflections and more significant change of the polarization state between the incident and backscattered light. However, as the size parameter becomes smaller than the geometric optics region, the PLDR decreases and so does the sensitivity to the surface deformation, as can be seen from Fig. 2 of Gasteiger et al. (2011) (L399-L403).

A4.7: This could be an artifact in the spheroidal model when calculating large particles due to the degradation of computational accuracy. To avoid this confusion, we limited the maximum r_{eff} to 5 μm and replotted the figure as Fig. 4 in the revised manuscript.

A4.8: Figures 5-8 have been removed in the revised manuscript for the purposes explained in A4.2.

5. Section 4.1 & Fig. 10
    1. The comparison of the IH to the results of Hu et al., ACP 2020, was already shown by Saito and Yang, GRL 2021. Where are the differences and similarities to that comparison?
    2. There are certainly more multiwavelength observations of mineral dust. Why have you chosen these ones and no other ones? Recently, I found some new results in Gebauer et al., ACP 2024.
    3. TD, FD and BD should be written in each figure because the abbreviations are not self-explaining (here and in the following figures).

4. Which dust size distribution would you expect to be present in the field observations? The reasons for the fitting or not are not properly discussed.
5. The text from line 430 onwards already belongs to Section 4.2 or to an own subsection, but it is not really linked to the content of Section 4.1.

A5.1: We imbedded Sect. 4.1 into Sect. 3.1 which allows for a wider comparison with real measurements. We abandoned the microphysical property setting in the original manuscript and moved to r_{eff} covering 0.1-5 μm and CRI from Di Biagio et al. (2019). In addition, we considered more measurements as listed in Table 2 of the revised manuscript. The comparisons can be found in L411-L432 of the revised manuscript. The difference from Saito and Yang (2021) is that apart from PLDR and LR, we also compared EAE and BAE, and we put this single comparison into the context of the comparisons with other measurements so as to acquire some extended perspectives.

A5.2: We added observations of mineral dust from Sahara (Freudenthaler et al., 2009), Central Asia (Hofer et al., 2020), USA (Burton et al., 2015), as well as the dust transported to the USA from Sahara (Burton et al., 2015) to enrich the real measurement set to compare. They are summarized in Table 2 of the revised manuscript. We also added the results in Gebauer et al., ACP (2024), their measurements at 1064 nm are of great interest in our study. We appreciate your suggestion.

A5.3: Figure 10 in the original manuscript has been removed in the revised manuscript.

A5.4: We expanded the range of r_{eff} to 0.1-5 μm instead of the three VSDs selected in the original manuscript. A general finding according to the new VSD setting is that the VSDs of both spheroidal and IH particles can better fit the measurements for r_{eff} (between 0.4-1 μm) smaller than most in situ results (r_{eff} > 1 μm, Table 1). For the r_{eff} consistent with the ranges of most in situ results (r_{eff} > 1 μm), the IH model provides overall better fitting to the measurements than the IH model (L421-L422, L424-L426, L429-L432). We also mentioned in the revised manuscript that the discrepancy between the simulations and measurements can result from the limitations of the exploited setting of microphysical properties (L799-L810)

A5.5: We kept "the text from line 430 onwards" in the beginning of Sect. 4 while moved other parts of Sect. 4.1 to Sect. 3.1 as mentioned in A5.1. In addition, we explained the motivation of this section in more details in the beginning of Sect. 4, and stress that this section cannot provide a direct comparison of the retrievals derived by the two non-spherical models (we realized Sect. 4.3 makes little sense thus deleted this whole section in the revised manuscript), please see L499-L505 and L518-L526.

6. Sections 4.2 & 4.3
    1. Now, you introduce some quantities with a *, probably to distinguish true and retrieved parameters. However, the ^ for retrieved

parameters as introduced before is not used. Please be more consistent and if necessary add a short description at first instance.

2. Overall, I don't really see the significance of these two sections (4.2 & 4.3). You use one model (here IH) to generate particles and this model of course retrieves the results with less uncertainties. What is the benefit? What do we learn from it?
3. Fig 11, 13, 14: Please find a better representation of the x-axis. The CRI pairs in the current version are not very intuitive. One idea would be to add a two dashed lines to separate the 3 m_R blocks. In that way the correlation with m_I would be easier to grasp.
4. L475 Why do you see this behavior?
5. L504 The formulation is not clear.

A6: We largely modified the microphysical properties in Sect. 4.2 to make them less arbitrary: the r_{eff} from 0.1 to 5 µm covering the possible range from fine to coarse mode dust; the S_{g} of 1.95 which is in the middle of the range of in situ measurements (Table 1); the m_{R} from 1.4 to 1.6 and $m_{\mathrm{I},355}$ from 0.001 to 0.009 covering the ranges of the laboratory measurements by Di Biagio et al. (2019). The updated simulations are then based on the modified microphysical properties and the main conclusions keep unchanged. Moreover, we tested the retrieval performance when different combinations of δ measurements are accounted for. Since Sect. 4.1 in the original manuscript has been removed, Sect. 4.2 becomes to Sect. 4.1 in the revised manuscript.

A6.1: To be more concise and avoid redundant naming and characters, "a*" is no longer used in the revised manuscript. Instead, we describe a microphysical property by specifying it is "retrieved" from the inversion or "true" as the defined value if necessary.

A6.2: Briefly, the significance of Sect. 4.2 (Sect. 4.1) is to quantify how retrieval results are influenced by the inversion procedure, a key factor apart from modelling error to determine the retrieval quality. The basic idea of the retrieval simulation is similar to those remote sensing inversion studies (e.g., Torres et al., 2017; Veselovskii et al., 2002; Xu and Wang, 2015) where only one forward model was used. Instead, we conducted for both spheroidal and IH model, and focus on quantifying the improvement brought by spectral PLDR measurements. A more detailed description of the motivation is presented in L499-L505 and L518-L526. The general conclusion from Sect. 4.2 (Sect. 4.1) is that the incorporation of spectral PLDR, especially at 1060 nm, to the inversion dataset brings large improvement in microphysical property retrieval for both spheroidal and IH models, although to different extent.
However, we realized that Sect. 4.3 makes less sense. We had aimed to quantify the retrieval difference between the spheroidal and IH models when inverting the same synthetic measurements. However, as you comment, when a set of synthetic measurements is generated with one scattering model, it is of course that inverting it with another scattering model leads to worse results because of the modeling differences. We stressed this point in the revised manuscript (L518-L522). Accordingly, we removed the whole

section and think inverting the same real measurements is a better way to compare the retrieval difference between different scattering models.

A6.3: Figures 11, 13, 14 have been removed because of the change of microphysical property setting and the unclear representation (i.e., "CRI pairs"). Instead, we made Figs. 7, 8 to visualize the variation of retrieval accuracy against the true r_{eff} for IH and spheroidal models, Figs. 11, 12 to visualize the variation of retrieval accuracy against the true m_R and m_I for IH and spheroidal models, and Figs. S1, S2 which are the same as Figs. 11, 12 expect for a smaller r_{eff}. We hope the new figures will be more intuitive.

A6.4: The inherent reason is still unclear at the moment. We did not find such behavior in the monomodal retrievals. Thus, it can be because the presence of the fine mode increases the inversion instability. But we can identify this less accurate retrieval by checking the fitting error in practice. In this regard, fitting error can be treated as an indicator of retrieval quality.

A6.5: We acknowledge this expression is not rigorous because we did not really calculate the correlation coefficients. We meant to say the feature that if the spheroidal model derives a higher V_t than the IH model, the it usually also derives a higher r_{eff}, a lower m_R and a lower m_I. We have modified this expression and moved it to the discussion (Sect. 6.2, L840-L842).

7. Sections 4.4 & 4.5
    1. Much of the information presented in Tab. 3+4 is not used in the manuscript.
    2. And again, I don't really see the value in it. It tells us, how far from the truth we get, when using a certain model or model configuration. If we create our data with IH, we get the best results with 3+2+3 IH. You show, how far we get from the simulated truth, if we describe it differently. But who knows how the mineral dust particles look like?

       All these comments underline the need of a clear motivation for your model comparison study. Because I think it is important to compare these scattering models and to point out the strengths and weaknesses. The focus of your study appears to me rather the comparison of these models than the retrieval of microphysical properties (as stated in the title). Because to do this retrieval you would need some more validation which retrieval fits best.
    3. L531: "the corresponding standard deviation of the noise distribution is a third of the maximum error" – Does it mean that you use a standard deviation of 3.3% for extinction and backscatter at 355 and 532?

4. I am a bit puzzled why you omitted the spheroids in this section. Please show the results for the spheroids as well or instead of the spheres.
5. L559-560 From the presented results of EAE and BAE you can not conclude on the limitations of the scattering models, because EAE and BAE seem to vary a lot with the assumed aerosol size distribution, which you state at the end of the sentence.
6. L561: What about only one depolarization ratio? Would you expect the same improvement or do you really need 3 depolarization ratios?
7. Section 4.5: Again, you don't link your findings to previous literature. What is new? What was studied already before? And I believe, if you do a careful literature search you'll find many conclusions going in the same or a similar direction. I am interested in your new conclusions on top of previous knowledge. Or at least to set your conclusions in the context of previous findings.

A7: We largely modified the contents of Sect. 4.4 (Sect. 4.2) since the setting of microphysical properties in Sect. 4.1 has changed. As a result, we modified Table 3 and moved it to Sect. 4.1 (as Table 4) in the revised manuscript, while we removed Table 4 in the original manuscript. Moreover, we replaced Fig. 15 with Figs. 13, 14 where the dispersions of the retrieved state parameters caused by introducing measurement noise are shown for different aerosol microphysical states, inversion measurement sets and scattering models. We drew more constructive conclusions on the influence of measurement noise when retrievals are derived with different scattering models from different measurement sets.

A7.1: Table 3 has been modified and moved to Sect. 4.1 (as Table 4) while Table 4 was deleted in the revised manuscript (see A7). A detailed discussion of Table 4 is presented in L603-L620.

A7.2: We agree that the "simulated truth" is never the "truth" because of the presence of modeling error. We also agree that the main limitation of this study is the lack of independent observations for the validation of retrieval results (L848-L849, L960-L961), and to this end, we extended the comparison with AERONET and added discussions about comparisons with previous in situ/laboratory measurements of dust microphysical properties, which are presented in Sect. 6.3 of the revised manuscript. However, the motivation of this simulation section (Sect. 4) is stated in **A6.2** and the point of Sect. 4.4 (Sect. 4.2) is to assess the influence of measurement noise since it is inevitable in a real measurement. The inversion of synthetic measurement set perturbed by random noise is a commonly used strategy in the development of retrieval methods (e.g., Müller et al., 2019; Torres et al., 2017; Veselovskii et al., 2002).

A7.3: Yes. To avoid any ambiguity, we presented the magnitudes of the maximum errors (L632) and standard deviations (L634) in the revised manuscript.

A7.4: The results related to the spheroidal model are now shown in Fig. 14 and are discussed together with those related to the IH model (L640-L651).

A7.5: According to the updated comparison results in the revised manuscript, the measurements of EAE and BAE can be reproduced by both models for r_{eff} between 0.1 and 5 µm, but compared to the spheroidal model, the IH model allows a r_{eff} range (r_{eff} > 1 µm) more consistent with the in situ results in Table 1. Thus, we modified this expression to L416-L422.

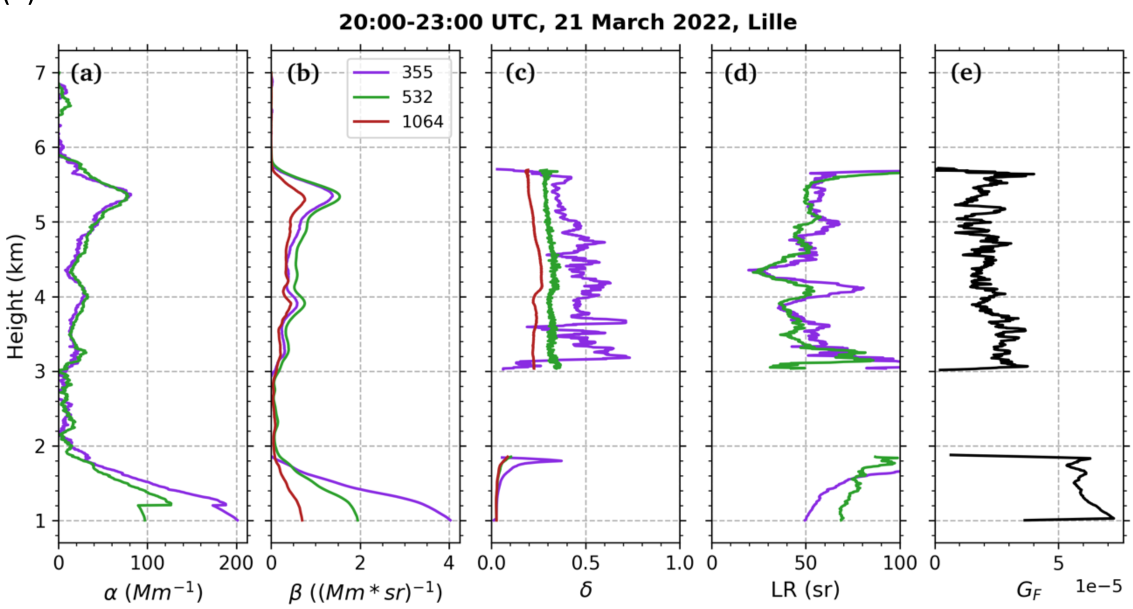
A7.6: We tested retrievals from measurements including different depolarization ratios, which is a main modification of this section (**A6**). Briefly, if only one depolarization ratio is included, δ_{1064} will bring more improvement than δ_{355} or δ_{532} due to the relatively large sensitivity to larger particles. However, including another δ_{355} or δ_{532} can further improve the retrieval of CRI and suppress the influence of measurement noise (L613-L615, L833-L835). So, the latest conclusion is that ($3\beta + 2\alpha + 2\delta$) with 1 δ at 1064 nm is the minimum configuration in reality.

A7.7: We have been fully aware of such insufficiency and more comprehensive discussions of the simulation results within the context of previous findings are presented in Sect. 6.1 and Sect. 6.2 of the revised manuscript.

8. Section 5.1
    1. L573: Do you really use the fluorescence measurements in the shown case studies to detect the dust layers?
    2. Fig 16: The data are not shown until 16 April 2019, 05:00 UTC as indicated in the figure caption.
    3. L594: What do these values tell me? Without an independent measurement or retrieval, I don't know which model fits best. m_R ranging between 1.45 and 1.68 it is a very wide range for mineral dust, the same holds for r_eff. You state, it was freshly emitted dust. However, a r_eff of 0.4-1.3 µm is much lower than your assumptions for fresh dust in Table 1.
    4. Please compare the retrieved CRI to the measurements of Di Biagio et al., 2019. What are their values for the Taklamakan and the Sahara? Are the retrieved CRI values reasonable?
    5. L605-610: The discussion is related to Fig 18 (whole layer) or Fig 19 (200 m layer)? Fig. 19 shows the size distribution and it so not so easy to get V_t and r_eff from it.
    6. Fig 19f: Please increase the scale for the depolarization ratio. The 3+2 spheroid results do not fit the measurements.
    7. L607-608: "Compared to the measurement error bars, all the scattering models are able to well fit the measurements." It is a very

dangerous statement, because it evokes the impression that everything fits. But this conclusion can not be drawn from your results. You use the optical data and invert them with a scattering model (IH) and then you apply the different scattering models to get to the optical data. And of course, this should fit somehow. But you are moving in a circle.

A8.1: As demonstrated by Veselovskii et al. (2022), dust particles have high δ_{532} and low fluorescence and they use the range $0.2 < \delta_{532} < 0.35$, $0.1*10^{-4} < G_F < 0.5*10^{-4}$ (G_F refers to the fluorescence capacity) to detect the presence of dust aerosols. We did not use the fluorescence measurements to identify dust aerosols for Case 1 because they are not available. However, the dust in Case 1 can be confirmed according to the study of Hu et al. (2020). The fluorescence measurements are available in Case 2. The values of G_F are within the range of dust, as shown in the following figure where the profile of G_F is plotted in panel (e):



The dust plume in Case 2 was further confirmed by PLDR, back trajectories and analysis of satellite images and synoptic conditions (L725-L739). To avoid the ambiguity, we deleted this sentence and did not show the fluorescence measurement in Case 2 in the revised manuscript.

A8.2: The data are shown until 16 April 2019, 00:00 UTC. The caption has been corrected.

A8.3: We found it is indeed a sketchy description that cannot provide helpful enough messages. To this end, we modified the interpretation of Fig. 18 (Fig. 17) in L1341-L1356, underscoring that the "variations of the inversions caused by different scattering models and input measurements are in line with our simulation results, indicating the results from ($3\beta + 2\alpha + 3\delta$) inversion are least affected by retrieval uncertainty" (L689-L697). A comparison of the retrievals with previous in situ dust size measurements is presented in Sect. 6.3 where the possible reasons for the systematic lower values of the retrieved r_{eff} are analyzed.

A8.4: The comparison of the retrieved CRI for Case 1 and Case 2 with the measurements of Di Biagio et al. (2019) is presented in Sect. 6.3 (L916-L922) of the revised manuscript.

A8.5: It is related to Fig. 19 (Fig. 18). The (V_t, r_{eff}) corresponding to each retrieved VSD is attached to Fig. 18a in the revised manuscript.

A8.6: Figure 19f (Fig. 18f) has been rescaled for easier reading. The (3+2, spheroid) result do not fit the measurements because the PLDR measurements are not inverted, indicating this configuration does not work well (L713-L715).

A8.7: With this sentence, we had hoped to state that the inverted measurements can be reproduced from the retrievals with an accuracy better than the measurement uncertainty, as an indicator of retrieval quality because we have seen large fitting errors can be the case in the simulation, as stated in L474-L475 of the original manuscript and in L590-L593 of the revised manuscript. However, we have realized such an expression can cause the ambiguity that "all measurements are well fitted". Thus, we modified the description in the revised manuscript (L710-L712).

9. Section 5.2
    1. Looking at Fig. 20, the dust layer seems quite homogeneous. Why do you limit your intensive optical properties to the tiny layer between 5.4 and 5.6 km height? What are the intensive optical properties for the whole layer (not shown)?
    2. The AERONET retrieval at 15:58 UTC, might it be affected by the feature at 9 km height (possibly an ice cloud) which is visible in Fig. 20?
    3. Why do you provide only the coarse mode r_eff from AERONET in Tab. 6? Especially, in case 2, the overall r_eff would be interesting to compare to your monomodal results. Then, it is not surprising that r_eff is higher for AERONET (L659). Please provide additionally r_eff for the whole AERONET size distribution.
    4. Table 5, case 1: Here report a layer height of 1 – 2.2 km, in Sect. 5.1 and Fig. 19, you use 2.0 - 2.2 km. Which layer do you use for the comparison in Tab. 6?
    5. To which layer height does the layer-averaged AERONET volume concentration correspond? Please add in L652 the layer height used for each case.
    6. I am bit puzzled what epsilon_fit represents in this section. Previously, you state that it quantifies how well the measurements are represented by the retrievals (L447). But here you have only the lidar measurements and then you do the retrieval. You don't have another independent quantity to compare your retrieval. Probably, you use the retrieved microphysical quantities and apply the same model to get the optical properties to report an epsilon_fit. However, it does not tell us, how well it fits in reality. Again, you're moving in a circle.

A9.1: We extended the retrievals to the layer 4.6-5.6 km, as shown in Fig. 22, and averaged the layer between 5-5.5 km for the visualization of VSD and SSA, as shown in Fig. 23. The intensive optical properties for the whole layer have been displayed (Fig. 20).

A9.2: We think it is less possible because we used the Level 1.5 retrieval product where the clouds should be well screened out according to Sinyuk et al. (2020) (L852-L853).

A9.3: The r_{eff} for the whole VSD retrieved by AERONET has been listed in Table 6. Indeed, the r_{eff} for the whole VSD in Case 2 is closer to the lidar retrievals due to the contribution of the fine mode.

A9.4: Table 5 is removed from the revised manuscript. The layer for Case 1 in Table 6 (Table 6 in the revised manuscript) is 1.5-2 km.

A9.5: The layer heights to which the AERONET columnar volume concentrations are averaged for each case have been specified in the caption of Table 6.

A9.6: The ϵ_{fit} in Table 6 represents the residual of the fitting to the measurements that take part in the inversion for both BOREAL and AEROENT retrievals. The definition of this parameter is expressed by Eq. (13) in the original manuscript, or Eq. (15) in the revised manuscript. To avoid the ambiguity, we removed this row in Table 6.

10. Sections 6 & 7
    1. At the beginning of discussion, you state it correctly, that BOREAL is able to reproduce the input measurements. But as long as we don't know the "microphysical truth", it is just a circle. You put some measurements in and get them out at the end. What do we learn from this? We know that the model works in the forward and backward direction.
    2. L695: "the $r$eff decreases to 11–12 µm due to the loss of sensitivity." What do you mean?
    3. L726-727: "All the retrievals fit the measurements well with a fitting error comparable with the measurement uncertainty." Again, I find dangerous to state it like this, because you do the retrieval on the retrieved quantities and then it is no surprise that it fits the original measurement. See also my previous comments.
    4. L728: If you consider the coarse mode r_eff from AERONET only. But to have a fair comparison, you should take r_eff from the whole AERONET size distribution.
    5. The data availability is not sufficient. Request to whom? Even better would be to publish the retrieval results separately as data set.

6. It is unusual to explicitly thank one of the co-authors in the acknowledgment section.

A10: The discussion section has been mostly rewritten by combining with previous findings as much as possible. In the revised manuscript, it consists of three subsections: Sect. 6.1 makes a further discussion of Sect. 3, aiming at demonstrating the capabilities of the IH and Spheroid models to mimic measured optical properties; Sect. 6.2 makes a further discussion of Sects 4 and 5, aiming at demonstrating the most preferable scattering model and measurement configuration for acquiring the best retrieval performance; Sect. 6.3 compares the real case retrievals in Sect. 5 with the corresponding AERONET retrievals and historical in situ measurements.

A10.1: We realized this paragraph makes less sense in terms of verifying the retrievals are capable of reproducing other optical properties that are not inverted. Thus, we deleted this paragraph. Comparisons of the real case retrievals with the corresponding AERONET retrievals and historical in situ measurements are presented in Sect. 6.3.

A10.2: It is a typo. We wrongly typed the values corresponding to “V_t” rather than “r_{eff}” in Table 6. We apologize for this.

A10.3: We are fully aware of the risk so this kind of statement no longer appears. Moreover, in order to be in line with the revised sections, especially the revised Sect. 6, the conclusion section is correspondingly modified.

A10.4: The comparison with AERONET concerning the r_{eff} for the whole columnar VSD is provided in Sect. 6.3 (L869-L873, Table 6) of the revised manuscript.

A10.5: The statement of data availability has been updated.

A10.6: The “acknowledgement” has been updated. The thank to the co-authors has been removed.

Technical corrections

- L185 reference appears twice.

It has been corrected.

- In Latex it is \AA ngstr\”o m exponent -> please use correct spelling

We spell it as “Angstrom” in the revised manuscript.

- L391 How does ...

The sentence has been deleted in the revised manuscript.

- Tab 3 caption: in parentheses ? The standard deviation is not given in parentheses. TD, FD, BD – please repeat the words in the table and not just the abbreviations.

The Tab 3 has been removed and we ensure that similar problems do not show in the revised manuscript.

- L579 Figure 16 shows the …

It has been corrected.

- L620 date is formatted differently here.

The format has been unified as: hh:mm UTC, dd Month yyyy.

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Reply to Referee #2

We sincerely appreciate the supportive and thoughtful remarks from the anonymous reviewer. Pleases find our point-by-point response below.

The numbers of line, equation, figure, table and section in red refer to the original manuscript, whereas in blue refer to the revised manuscript. The references cited in the responses are listed at the end.

This paper presents results of a sensitivity study on retrieving microphysical properties of mineral dust with three di erent light-scattering models, i.e, spherical particles (Lorenz-Mie theory), the spheroidal particle model (used e.g. by AERONET) and a novel method that uses particle shape of irregular hexahedral particles (referred to in this paper as Irregular–Hexahedral model (IH)).

Sensitivity studies include the use of 3 di erent types of dust particle size distributions which are described as fresh dust, transported dust, and bimodal dust (fine and coarse mode). Di erent refractive indices (real and imaginary parts) are tested, too.

In summary, this study adds significantly to work on improving the use of lidar data in the context of mineral dust. The part on simulations with synthetic data shows, as has been shown in many studies before, that the use of spherical particle geometry in inversion algorithms (e.g. BOREAL in the present study) leads to significant errors of the investigated microphysical particle properties. In the present case the BOREAL algorithm uses the theory of the 3 light- scattering models. The results show that in part significant improvements can be achieved with the IH model. In particular retrieval improvements can be achieved in regard to the imaginary part, which has not been seen from the use of the spheroidal particle model.

The manuscript can be accepted after some major modifications. For example, a wider literature review, including a summary of previous findings on the spheroidal model are needed. There are plenty of studies on that topic. Such an overview allows for putting the results obtained with IH into the proper context.

It also remains somewhat unclear why you constrained your simulations to a rather limited set of particle size distributions and refractive indices. Why were these real parts chosen? Why where these imaginary parts chosen? If it is meant to cover (broadly) the range of values that can be expected for mineral dust (you already provide some comments on this, including a figure; expand this part of the paper) then explain it in more detail in the manuscript.

You are using simulated optical data to test the performance of BOREAL for the light-scattering models. I.e. microphysical properties are retrieved and used for back calculating the input optical data. In general, this is a di icult approach because no independent data for validating your results are available. But I also understand from own work that this approach is commonly used in many data inversion studies for validation of results, simply for lack of technologies (in laboratory in-situ for example) that allow for generating data on optical and microphysical dust properties in an independent manner and repeatable manner. In view of this lack of available technology and methods for carrying out suitable quality assurance and validation- a field of research that is finally (slowly) evolving – I recommend you add a section in which you critically evaluate your retrieval results in view of 1) lack of independent validation data, 2) possible shortcomings on covering the whole range of dust optical and microphysical properties, and 3) the fact that A) you use the spheroidal and IH light-scattering models to generate simulation data, B) the optical data are used for data inversion, C) the results are compared to the same light-scattering methodology that has been used to generate the test data.

I also recommend a more critical evaluation of the IH model because I see that this model could solve a few issues on lidar observations of mineral dust and in particular the e orts on applying data inversion methods. Thus, more information on the pros and cons and limitations of that theory need to be provided.

Finally, the presentation of experimental data in the last major section of your paper shows lidar observations of mineral dust. These observations naturally are a ected by many uncertainties regarding what type of dust was present (for example backtrajectories are a nice tool but cannot verify the type of dust and thus PSDs and refractive indices at all; it is mainly about using such and other modelling tools for consistency checks of results). Therefore, please provide a more careful interpretation of your results. Consider the possibility of uncertainties that a ect the interpretation of the data inversion results in a much more careful manner. Provide a discussion and evaluation of the results in a more critical manner. I find it hard to believe that a layer of anthropogenic pollution can rest upon a mineral dust layer in such a clearly separated way (as you describe it – and you are simply referring to previous work as kind of "proof"). This is a very important point that needs to be addressed as your quality assurance and verification work rests upon these experimental data.

I also would like to see a bit of meteorological interpretation of how these dust layers were generated and subsequently transported. This analysis allows you a more careful interpretation of your results which currently are exclusively based on theoretical work (modelling and simulations). Please also add a few final statements in the discussion section on the implications of the results (obtained with the IH model) on possible radiative e ects of the dust plumes and how these e ects could di er from the impact of dust properties obtained with the spheroidal particle models.

Finally, regarding the use of AERONET results I'd like to see a more critical discussion and comparison of the results. On the one hand this comparison is suitable as AERONET provides a standardized, well tested set of data analysis tools and data products. On the other hand, however, I see this comparison as a weakness of your study because you compare column- integrated/column-averaged aerosol properties to heigh-resolved lidar observations. You provide too little information on how such a comparison allows for verifying your results (let alone validating your results). You only pick a few (a couple?) height layers from the lidar profiles. This is an insu icient test of the validity of your results if you want to stay with AERONET as a benchmark tool that could allow for testing the accuracy of your simulation and experimental data. AERONET uses the spheroidal model, and thus I find it hard to understand why it can serve as an anchor point (in the experimental section of the manuscript) for testing the IH model as well.

A: According to your recommendations, we made major modifications in the following aspects:

1. A wider literature review. Previous studies on dust microphysical properties, including size, shape and CRI as well as their dependence on source, aging and transport process are more sufficiently reviewed and presented in Sect. 2.2 of the revised manuscript. We conducted a more comprehensive literature research on different particle shape models and scattering computation methods, including both advantages and limitations to demonstrate the motivation of selecting the spherical, spheroidal and IH models in our study (Sect. 2.3). These previous studies then serve as the basis of the updated simulations (Sects. 3, 4) and the context of the updated discussion (Sect. 6).

2. Two new subsections subject to forward simulation. They aim to study (1) the influence of ignoring the m_I spectral dependency and using our spectral m_I treatment (Sect. 3.2); (2) the sensitivity of depolarization measurements to different microphysical properties for both spheroidal and IH models (Sect. 3.3), as supports to better interpret the retrievals from simulated data and real measurements.

3. A more extensive and literature-based simulation setting. We abandoned the "three typical size distributions" and adopted the following microphysical property settings for the

simulations based on the literature investigation: VSDs are monomodal with S_g = 1.95 (in around the middle of the range from literatures) and r_{eff} covering 0.1-5 µm, and CRIs are mainly based on the study of Di Biagio et al. (2019). We explained the reason for choosing these settings (e.g., L345-L348, L365-L366) and possible shortcomings on covering the whole range of dust microphysical properties (e.g., bimodal distributions were not considered, see L799-L810).

4. An extended and more comprehensive discussion section. We further evaluated our results by fully putting them in a context of previous literatures, drawing the conclusion that spectral depolarization measurements are crucial to improve the retrieval of dust aerosols and the IH model is preferable to be applied to lidar data inversion (Sects. 6.1, 6.2). With regard to data validation, we mentioned the lack of independent observations and made a more critical discussion and comparison with the AERONET retrieval by pointing out the differences in measurement time and space, the fact that the spheroidal model is used in AERONET; we made new comparisons with previous laboratory/in situ results and analyzed possible reasons leading to the observed discrepancy (Sect. 6.3).

5. A more careful verification of the observed aerosol types in real data application (Sect. 5). For Case 1 we rely on the aerosol characterization by Hu et al. (2020) because they provided closure analysis combining lidar measurements, satellite observations and meteorological conditions to identify the aerosol type (i.e., mineral dust freshly emitted from the Taklamakan desert) and interpret the vertical structure. That is, we briefly reviewed their analysis (L667-L668, L686-L688) rather conducted another turn of aerosol identification. For Case 2, however, we complemented an identification of the source and transport of the observed aerosols by analyzing satellite images (Dust-RGB/MSG), HYSPLIT back trajectories and meteorological data (NCEP/DOE Reanalysis II). As a result, we identified two stratified dust layers from different sources and undergoing different transport processes (L725-L739).

6. We made a brief and qualitative discussion on the influence of retrieval differences due to adopting different retrieval configurations (scattering model + inversion measurement set) on possible radiative effects at the end of the discussion section (L923-L936).

I also ask the authors to consider the following comments (more specific ones) and make respective modifications and improvements to their manuscript. I provide the line numbers and the sentences/text together with my recommendations.

Line 166, … for moderate size parameters …:

- please add the size parameters.

A: The exact values of the size parameter are not provided by Saito et al. (2021). They depend on the degree of sphericity and CRI (L301-L303).

Line 173 – 175, … we convert the scattering properties from functions of Dmax to functions of $r$vol via the effective volume of the IH particle ensemble which is provided by the model database ...:

- can you write down the equation for this conversion, please.

A: The equation for this conversion has been added (Eq. 12).

Line 180, … of 90, 100, 100 mJ at 355, 532, and 1064 nm …:

- The energy distribution is almost equal?

A: In the manuscript, the laser energy values (90 mJ, 100 mJ, 100 mJ) do not represent the direct output from the laser harmonic generators. Instead, they indicate the energy transmitted into the atmosphere after passing through optics designed to enhance polarization purity. The initial output from the laser includes a significant contribution from the 1064 nm wavelength. However, after polarization cleanup and optimization of the harmonic generator to maximize the 355 nm output, the energy levels across the three wavelengths (355 nm, 532 nm, and 1064 nm) become comparable.

Line 182, … at 387, 408 and 530 nm …:

- 2 for extinction and 1 for water vapor, I assume. But RH is not mentioned in the following sentence. Thus, what is 408 used for?

A: The 408-nm channel was used for water vapor and it was replaced by the fluorescent channel in 2020 (Veselovskii et al., 2020). We deleted this channel in the revised manuscript.

Line 196/197 … contain considerable giant particles (with diameters lager than 20 μm), which do not remain airborne for long due to their high settling rate.:

- add a reference, please, where more info on this settling of large particles (settling speed for example) can be found.

A: We rewrote Sect. 2.4 (Sect. 2.2 in the revised manuscript) to establish a tighter connection between previous literatures and this study, as well as to renew it with findings from more recent researches. A more detailed review on dust settling during transport processes with proper citations is given in L165-L175.

Line 204, … a shift towards smaller sizes and convergence into a more uniform size are expected due to …:

- Is there any literature on this topic that shows this shift. from experiments like SALTRACE in Barbados or more recent studies in the Caribbean Sea and the Western rim of north Africa? I am asking as I am not aware if Hu 2018/Arimoto 1997 show this settling mechanism.

A: We admit that the literature research is not sufficient for supporting the statement "… a shift toward smaller sizes…", which directly comes from the study of Reid et al. (2008), where they further referred to Prospero (1989): "For dust older than a week, certainly we expect a shift to smaller sizes. When modification does occur, it is reasonable to expect that the nonlinearities in dust scavenging mechanisms will aid in the convergence into a more uniform size". The study of Arimoto et al. (1997) observed the deposition of large particles but not the shift of the size distribution to smaller sizes: "The model-derived dry deposition velocities were at most weakly related to the MSD but more strongly correlated with the geometric standard deviations of the distributions, further evidence that the dry deposition mass flux of dust can be dominated by a relatively small number of large particles". The study of Hu (2018) showed a comparison of VSDs retrieved at source and during transport by AERONET (Figure 3.27 of that paper), where a shift of coarse-mode VSD to smaller sizes during the transport can be seen. However, the possible mechanism behind was not fully explained. More recent large campaigns like SALTRACE provide updated findings related to giant particle settling rather than the shift of the coarse-mode distribution. Further literature investigations found that the dust coarse-mode distribution can be reshaped by cloud processing, internal and external mixing and chemical reactions and thus presents large uncertainty, but no clear evidence of the “shift”. Given these aspects, we replace this statement with the findings from more recent studies (L165-L175).

Line 206/207, … Additionally, a fine mode of dust VSD was sometimes observed (d’Almeida and Schütz, 1983; Gomes et al., 1990).:

- this comment needs to be corroborated by more recent literature. It is known that measurements have often been compromised by instrument artifacts, particularly with respect to data presented in comparably historic literature.

A: We complement this point with more recent literatures in the revised manuscript. However, the presence of fine-mode dust has been a controversial topic. There are studies on both modelling and measurement for supporting their arguments (L159-L164).

Line 220, … components, this is not a major factor affecting dust CRI. ”:

- Why isn't it? Could one reason be that methods of inferring the average CRI are (highly?) inaccurate and/or immature themselves? Please spend a few more sentences on this if you agree. If you consider other reasons as (more) important than the one I mention, please mention them.

A: We realized that this expression is not enough exact. Our intention was to emphasize that the dust CRI has a more obvious wavelength than size dependence. As shown in Fig. 20 of the study of Kandler et al. (2009), the CRI of samples from SUMUM shows a relatively weak size-dependency compared to its wavelength-dependency (for particles between 1 and 10 µm in diameter). According to their method, the change of the component fraction with size is indeed the factor dominating the size dependency of the CRI, but we infer that the basic mineral components and their inherent CRIs used in the method dominate the wavelength dependency. In the revised manuscript, we compared the CRIs derived with different methods (i.e., Di Biagio et al., 2019; Kandler et al., 2011) and saw their contrasts in the size dependency (L211-L214).

Line 225, … wavelengths, we extrapolated or interpolated their published results.:

- please indicate in figure 1 by a different set of symbols which data points are the result of interpolation and extrapolation. At present this figure gives the impression that all data points have their origin in observational data.

- Please also explain for which mineralogical composition these data points have been inferred. At present your text implies that mineralogical composition is not relevant (in the sense of significance) which I doubt is the general case.

A: 1. All the data shown in Fig. 1 are derived by interpolation and extrapolation of the original results in Table 4 of their paper. Specifically, $m_{I,355}$ is from extrapolation using the original results at 370 and 470 nm; $m_{I,532}$ is from interpolation using the original results at 532 and 590 nm; $m_{I,1064}$ is from extrapolation using the original results at 880 and 950 nm. We made a clearer explanation in the revised manuscript (L218-L219).

2. We acknowledge our original expression "mineralogical composition is not relevant" to dust CRI is not proper and corrected it in the revised manuscript (please see the answer to "Line 220"). In contrast, Di Biagio et al. (2019) concluded the "sample-to-sample variability observed in this study is mostly related to the iron oxide and elemental iron content in dust" (L224-L225).

Line 226-228, ... Fig. 1, the relationship between the imaginary part at 355 nm (mI,355) and at 532 nm (mI,532) can be approximated by a linear function, whereas the imaginary part at 1064 nm (mI,1064) has a weak dependence on mI,355 with a value around 0.001.:

- I consider this plot and its interpretation a bit misleading.

- You basically write that mI_532 depends on mI_355 in a linear fashion? How can that be if the individual components in a dust grain (and the composition in a dust PSD) is dependent on wavelength? Do you have a sufficiently large set of data (aside from the publication by Di Biagio) that corroborates this comment?

- For what dust source in North Africa does this result hold true?

- Please explain in more detail why it seems reasonable that mI_1064 barely depends on mI_355? How does that compare to the result in a) in terms of what we can expect from the individual mineralogical components in a dust grain and a dust PSD?

A: 1, 2, 4. We believe Fig. 1 has no artifact because it is directly derived from interpolation and extrapolation of the Di Biagio results. As indicated in Sect. 4.4 and Fig. 9 of their study, the magnitudes of the imaginary part in the UV (370 nm) and visible (520 nm) are strongly correlated with the mass concentration of iron oxide than other components, which might indicate m_{355} and m_{532} are mostly dominated only by iron oxide whose CRI has a wavelength dependence like Fig. 1. On the other hand, they show that the imaginary part in the NIR (950 nm) presents weaker correlations with single components, thus, in contrast to the situation for m_{355} and m_{532}, we expect multiple components contribute to m_{1064}, which could explain the weaker correlation between m_{1064} and m_{355}. And since their measurements of SSA (CRI) and mineral components are independent of each other, we think their results are of relatively high confidence. We also added the averaged result of Kandler et al. (2011), and we found it is also in line with this relationship (Fig. 1). We explain this relationship in L220-L227 in the revised manuscript.

3. We modified Fig. 1 by indicating the source regions with different markers (Fig. 1), these messages are provided by Di Biagio et al. (2019). The dust in the Northern African region (labeled as NAF-S) was sampled by Di Biagio et al. (2019) in Tunisia, Morocco, Libya, Algeria and Mauritania, corresponding the 5 blue circle markers in Fig. 1. The added data (orange circle) is the average result of in situ measurements by Kandler et al. (2011) at the SAMUM ground station, Cape Verde.

Line 232, ... Consolidated by these laboratory measurements, we ...:

- what do you mean by this? Did you carry out laboratory measurements that add more info to Di Biagio's publication? Have these data/results been shown elsewhere?

A: What we were trying to say was that we modify the a priori constraints on CRI in BOREAL according to the laboratory measurements by Di Biagio et al. (2019) and the results in Fig. 1.

We did not carry out other laboratory measurements. We realized this expression is not proper and thus modify this paragraph. Please see L227-L228 of the revised manuscript.

Line 234-239, … 2022)). Then, $m_{I,532}$ is calculated from the relationship shown in Fig. 1, and $m_{I,1064}$ is fixed to 0.001. We believe taking account of the spectral dependence of the imaginary part of the CRI is essential in dust retrieval from lidar measurements because simulations suggest that ignoring it will lead to a retrieval error of 17-25% in V_t, as well as increases of retrieval uncertainty in other parameters (Veselovskii et al., 2010). …:

- yes, it is a good part of a sensitivity study.

- It would be better however if you showed the sensitivity (of the final microphysical parameters) in dependence of a variation of mI_532 versus mI_355 and mI_1064 versus mI_355.

A: As mentioned in the general answer (2), we added a subsection (Sect. 3.2) to further demonstrate the necessary of considering the spectral variation of the imaginary part by quantifying the optical difference caused by not accounting for this spectral variation; it also demonstrates the rationality of our strategy to treat the m_I spectral variation.

Line 247/248, … mixture. Therefore, we exclude mixture cases and only work with pure dust retrieval in this study.:

- If I look at the results section I am wondering about the case where anthropogenic pollution is sitting on top of a dust layer. Can it be excluded that no mixing of dust and this pollution occurs in the transition zone?

A: We agree that it is hard to exclude the possibility of mixing with anthropogenic pollution, especially during long-range transport. We assumed pure dust microphysical properties in the simulations, and we can identify relatively "purer" dust in real applications as much as possible by checking the optical properties (e.g., PLDR, LR or fluorescence signals), tracing back the transport pathways, analyzing the synoptic conditions around, and so on. We modified the expression to make it clearer (L247-L250). In addition, in the real case retrievals of this study (Sect. 5), we focus on the "pure" layer below 2 km for Case 1, and discuss the uncertainty due to the "non-purity" (possible cloud processing during the transport) for Case 2.

Line 265, … an acceptable …:

- It could be phrased into something that either shows that all other uncertainties are equally large or larger (which I assume they are) or you provide more justification why such a significant overestimation is "acceptable".

A: There is another well-adopted r_{eff} definition for non-spherical particles (Eq. (8) in the revised manuscript). The bias here refers to the calculating difference between Eq. (8) (Eq. (6) in the revised manuscript) and Eq. (8), which depends on the choice of the size descriptor. The volume-equivalent radius leads to a bias of 10-20% but it is lower than those when other descriptors, such as the area-equivalent radius and the maximum radius, are chosen. However, this bias won't affect the comparison of the r_{eff} from different retrievals and in situ measurements as long as it is calculated through the same definition. In this study, we ensure all the effective radii are calculated through Eq. (8) (Eq. (6)). We modified the original expression as that in L115-L119.

Line 268, … Although:

- please check the use of this word (although) in this sentence. It does not seem to make sense in view of the message of the sentence and likely can be removed.

A: We have improved this expression (L344-L345).

Line 272/273, … respectively. They are generated from a particle ensemble with: rv =1.5µm, ln$S$g = 0.6 (this leads to a $r$eff of 1.25 µm, a value for typical transported dust aerosols (Hu, 2018)), …:

- Do Hu et al. 2018 show a summary of literature values? Otherwise, it is not clear why these numbers can be considered typical.

Line 273/274, … mI = 0.0015 at 532 nm. The …:

- This value is at the minimum range (it actually is at the bottom) of values shown in figure 1. I therefore consider it contradictory to write "typical" in the previous sentence.

- Please explain why a simulation for such a low value can be representative of the rather wide range of imaginary parts shown in figure 1 and how you can extrapolate your results.

A: In Hu (2018), r_V = 1.5 µm is the median radius of the coarse mode retrieved by AERONET from the sunphotometer observation of a transported Saharan dust layer at Lille and there is no summary of literature values. Thus, we found this statement is not proper. As mentioned in the general answer (3), we rebuilt the simulation in the revised manuscript based on a wider VSD range (0.1 µm $\leqslant r_{eff} \leqslant$ 5 µm) in order to cover the properties of "typical" dust aerosols. In the revised manuscript, we focus on phase matrix elements more related to lidar measurements, i.e., P11 and P22, at the backward direction, as well as SSA. We visualize their variabilities for r_{eff} varying from 0.1 to 5 µm, m_R from 1.4 to 1.6, and $m_{I,532}$ from 0.001 to 0.007 in Fig. 2. The corresponding discuss can be found in L344-L357.

Line 295/296, … Figure 3 illustrates the variation of SSA with respect to the effective radius (reff) and the effective size parameter, $x$eff = 2πreff $/\lambda$, for …:

- Please explain more on the fact that the top-axis of this figure shows a size parameter of 100 which relates to a particle radius less than 9 micrometer.

- The bottom axis shows a maximum particle effective radius of 10 micrometer. What type of particle size distribution can realistically create such a particle effective radius and still fulfill the requirement of particle radii less than 10 micrometer for individual particles?

- I assume this (unclear?) relationship is largely driven by the fact that both particle size definitions are shown in the same plot? It thus might have profound impact on the interpretation and explanations of what is shown in this plot.

A: 1. According to the expression of the effective size parameter, x_{eff} = 100 at 532 nm means $r_{eff} = \lambda x_{eff}$ / (2π) = 0.532*100 / (2π) = 8.47 µm.

2. According the setting of the VSDs used to generate Fig. 3, the maximum radius of an individual particle is ~72 µm for r_{eff} = 10 µm, much larger than 10 µm. However, it is not necessary to prescribe the radius limit of 10 µm for a single particle in the forward simulation. But it is true that at such large r_{eff} the lidar measurements will lose most of the sensitivity. Thus, in the modified simulation we limit the maximum r_{eff} to 5 µm (Fig. 2).

3. We realized the unreasonable large particle size range (r_{eff} spans 3 orders of magnitude from 0.01 to 10 µm) causes the large variation of SSA and hides the relatively small variations driven by particle shape and CRI. Thus, in the revised manuscript we refined the r_{eff} range to 0.1-5 µm so that more details associated with realistic dust size range can be better identified (Fig. 2c, e).

Line 309/310/figure 3:

- are the orange curves underneath the green ones?
- Please change line thicknesses so that all colored curves become visible.

A: 1. Yes, because of the small sensitivity of SSA to particle shape, the curves representing the results simulated by different scattering models overlap with each other.

2. In fact, this illustrates that compared to the change of CRI and particle size, the change of particle shape has small influence on SSA. We modified the plot in the manuscript for a better visualization (Fig. 2c, e).

Line 312, … ln *S*g = …:

- I may have missed the explanation of the physical meaning of this parameter. It is the geometrical standard deviation, isn't it?

A: S_g is the geometric standard deviation of the particle volume size distribution (VSD). lnS_g represents the logarithm of the geometric standard deviation.

Line 330/331, figure 4:

- Please see my comment regarding size parameters (100), how this translates to particle size and how it compares to a seemingly larger effective radius?

A: Please see our answer to the comment.

Line 348, figure 5:

- can results for this specific example be generalized to a wider range of PSDs, and values of r_v and ln S_G? I think that is one major sticking point of this study.

A: In the revised manuscript, based on the new setting of the microphysical properties, we show α, β in Fig. 3a, and LR, δ in Fig. 4 against r_{eff} varying from 0.1 to 5 µm and CRI varying in the range indicated in Fig. 1, derived from the Di Biagio results. The behaviors of LR and δ at different wavelengths are provided. However, we did not check the variability against S_g since we fixed S_g to 1.95 throughout the updated simulation. The limitation of this setting is fully acknowledged (L800-L803). The analysis of the updated figures and be found in L366-L377 (for α and β) and L389-L407 (for LR and δ).

Line 360-362, … CRI. For the PLDR, however, the two types of non-spherical particles exhibit contrary spectral variations: a positive slope for spheroidal particles while a negative slope for IH particles, resulting in the largest PLDR difference in the UV.:

- this is certainly one of the key results, i.e. the different spectral slopes. Can this result be generalized to a wider range of PSDs, particularly with respect to reff and or geometrical standard deviation?

A: The updated simulation results for a wider r_{eff} range (as shown in Fig. 4) the spectral variation of PLDR for spheroidal and IH particles can change for different r_{eff} values. For example, at r_{eff} = 2 µm, the PLDR of IH particles reaches the maximum at 532 nm while that of spheroidal particles still monotonically increases with the wavelength.

Line 364, figure 7:

- where does this "kink" in the curves (blue, green) at around 600 nm come from? Is that an interpolation/extrapolation issue (e.g. mismatch).

A: We think it results from the imaginary part at this wavelength (590 nm) which is provided by Di Biagio et al. (2019), rather than derived by interpolation.

Line 377/figure 8:

- fig 8 b): the line styles represent the 3 different real parts for the three models used in this study? For example: solid, green (8b) refers to mR_sphere?

A: Yes. More precisely, the solid green line in Fig. 8b represents $\text{BAE}_{355\text{-}532}$ of spherical particles for a real part of 1.6. Please refer to the updated plots of EAE and BAE in the revised manuscript for a clearer visualization (Fig. 3b, c), as well as the corresponding analysis (L377-L380).

Line 391, … does …:

- … does …

A: It has been corrected in the revised manuscript.

Line 395-397, … The spectral dependence of the imaginary part is considered as described in Sect. 2.4.2. Hereinafter, unless explicitly stated, the imaginary part of CRI presented and discussed always refers to the monochromatic value at 355 nm, and …:

- It means that the values at 355 nm are given and the extrapolation method to the other wavelengths (as shown in section 2.44.2) can be used?

A: In the setting of microphysical properties for the retrieval simulation, we only vary $m_{\text{I},355}$ and $m_{\text{I},532}$ follows the linear relationship derived from Fig. 1 (Fig. 1), $m_{\text{I},1064}$ is fixed to 0.001. Correspondingly, in the retrieval, we only care the m_{I} retrieval at 355 nm, while $m_{\text{I},532}$ is calculated from the linear relationship derived from Fig. 1 (Fig. 1), and $m_{\text{I},1064}$ = 0.001. In the revised manuscript, we verified this treatment to take into account the m_I spectral variation at lidar wavelengths in Sect. 3.2, and we explained the notation in L465-L468.

Line 402/403, … Lognormal VSD (Eq. 6) … Transported dust (TD) rv = 1 µm, lnSg = 0.6, Vt = 1, reff = 0.84 µm …

- how does this reff value (it seems quite low) compare to experimental data, e.g. observed in the Caribbean (e.g. SALTRACE or AERONET)?

A: We realized the value reff = 0.84 µm is in general lower than AERONET retrievals and in situ measurements (like SALTRACE) of the coarse-mode dust. In the updated retrieval simulation, we tested for a r_{eff} range 0.1-5 µm to cover the size range (r_{eff} > 1 µm) closer to the in situ measurements of the coarse-mode dust (Table 1), as well as the range (r_{eff} < 1µm) that allows the models to reproduce the ranges of real lidar measurements (L411-L432, Table 2).

Line 409-411, … condition. In spite of that, all three scattering models can reproduce the ranges of spectral LR measurements for the TD type. For the FD and BD types, however, the Sphere model tends to underestimate LR at 532 and 1064 nm while the two nonspherical models are capable of well reproducing these values.:

- is that mainly driven by mean particle size?

A: We are afraid that it might be driven by many factors related to the change of the size distribution, for example the median radius and geometric standard deviation. When it comes to a bimodal distribution it could be more complex as we suspect it is also related to the width, position and fraction of the fine mode. We expect to figure it out in the future study.

Line 415/416, …measurements. The BAE comparison reveals that except for the TD type, all the scattering models tend to underestimate the BAE to different extent.:

- it means BAE values from the scattering models are lower?

A: Yes. From Fig. 10 (h-i) we can see the BAEs produced by the models are generally lower than the mean values of the measurements, although the IH model performs better. In the updated simulation, we conducted a more detailed comparison with more lidar measurements (L411-L432, Table 2, Figs. 3,4).

Line 418/419, … Such discrepancies suggest that there might be certain limitations in these scattering models that preclude them from reproducing the measured EAE and BAE, although …:

- it means that the "backscattering peak" at 180 degree cannot be accurately computed/simulated?

- Could it be an issue of the "statistical distribution" of the particles (random orientation)?

A: 1. With respect to the accuracy of the non-spherical models, it could be one of the potential reasons. However, there is few studies comparing the exact 180° laboratory measurements with the model simulations to verify this point. The spheroidal model does not account for the coherent backscattering effect for single scattering thus might fail to compute the backscattering peak of non-spherical particles. On the other hand, the IH model accounts for this effect and we can see it performs a little better. But we think it can also result from the inadequate setting of the microphysical properties (for example the limitation of the selected VSDs), which we pointed out in the revised manuscript (L799-L810).

2. As demonstrated by Mishchenko et al. (2002), the most significant outcome of the fixed orientation for non-spherical particles turns out to be the interference and resonance features of the scattering properties. In this regard, we think the "random orientation" is a physically reasonable assumption because it allows to reproduce the smooth structure of the scattering properties observed in laboratory or remote sensing measurements.

Line 424/figure 10:

PLDR:

- It seems that IH works better for large(r) particles (TD) and Spheroid model works better for smaller(er) particles (FD case).

- Is that something that can be tested for the BD case?

- Did you test various PSDs for TD and FD that would allow to check on this?

BAE and EAE in (i):

- could this result (larger simulated EAE compared to measured values and still rather good agreement of simulated BAE to measured BAE) reveal if IH works better for large particles and the Spheroid model works better for small particles?

A: PLDR: In the updated simulation and comparison with real lidar measurements, we expanded the VSD ranges but did not test more bimodal distributions. We found that indeed, the spheroidal model can better reproduce the measurements of lower PLDR values when r_{eff} varies in 0.4-1 µm, while the IH model can better reproduce the measurements of higher PLDR values as r_{eff} varies in a wider range. At the same time, we have to keep in mind that the updated settings are still much simplified.

BAE and EAE: As described in L416-L422 and Fig. 3b, c in the revised manuscript: the spheroidal model cannot reproduce the measured BAE when r_{eff} > 1 µm. And for other size ranges, the two models produce similar EAE and BAE. With this regard, we give a higher score to the IH model.

Line 432, … the retrieval derived with …:

- ... retrieval results derived ...

A: The expression does not appear in the revised manuscript since the whole subsection has been revised (integrated into Sect. 3.1).

Line 433/434, … Next, the (3β + 2α + 3δ) and (3β + 2α) of the created optical datasets are inverted into:

- This seems a somewhat challenging simulation strategy as the models likely cannot create accurate optical data in the first place.

- I understand that the (wrong input) optical data can be found from the retrieved microphysical results (i.e. the backcalculation).

- How can this possibility be verified on the basis of experimental data if no information on the microphysical properties (of these experimental cases) is available?

A: 1, 2. Our updated forward simulation demonstrates that most of the measured optical properties can be reproduced by the models after we tested more VSDs. Thus, as mentioned in the general response (3), we updated the retrieval simulation based on the same r_{eff} range in the updated forward simulation, so that the corresponding range of the synthetic measurements can cover most of the measured optical properties.

3. In the inversion of real measurements, we check the fitting error to see if the measurements can be reproduced by the retrieved microphysical properties (as we presented in Sect. 5). In the revised manuscript, a further comparison of the retrieval results from real measurements with the results provided by historic literatures is conducted (Sect. 6.3).

Line 449, … where n is the number of the measurements:

- what do you mean by number of measurements? Does it mean different wavelengths or different experimental data sets? Or number of simulation runs?

A: As explained in L508-L509, n is the number of inverted measurements in the retrieval. For example, if ($3\beta + 2\alpha + 3\delta$) data are inverted, then n is 8.

Line 455, … V_t and r_{eff} tend to be underestimated while m_R and m_I overestimated. Such …:

- Did Chang et al and Burton et al offer solutions do this phenomenon. Does any other literature on this observation of a compensation effect exist?

A: Burton et al. (2016) focused on the cross-talks happening to small particles (median radius smaller than 0.2 µm) while Chang et al. (2022) found similar issues to large particles. But both studies reveal the cross-talks are caused by the lose of measurement sensitivity, and recommend to add stronger a priori constraints to ameliorate this problem. For example, Burton et al. (2016) suggest using a cutoff radius to constrain the retrieved radius range; Chang et al. (2022) suggest the use of more accurate a priori constraints on CRI in the future study. At the moment, we attribute this issue to the inherent deficiency of BOREAL and give a more detailed explanation in the revised manuscript (L533-L540).

Line 465/466, … by either overestimating the imaginary part (for $m_R* = 1.4$) or underestimating the real part (for $m_R* > 1.4$).:

- An underestimation of the real part for mR = 1.4 to my opinion cannot be ruled out. The reason why it does not happen seems to be simply driven by the fact that lower real parts are not considered in the subsequent inversion. Can you please comment on this possibility.

A: From the original retrieval simulation results (i.e., Fig. 11), spherical particles indeed overestimate m_R when the true value is 1.4 for most of the times (but exceptions happen for the BD type). In the updated retrieval simulation, we found that the spherical model can largely underestimate m_R when the true value is 1.4 and r_{eff} is less than 0.6 µm, as shown by the dashed lines in the first column of the following figure (not shown in the revised manuscript):

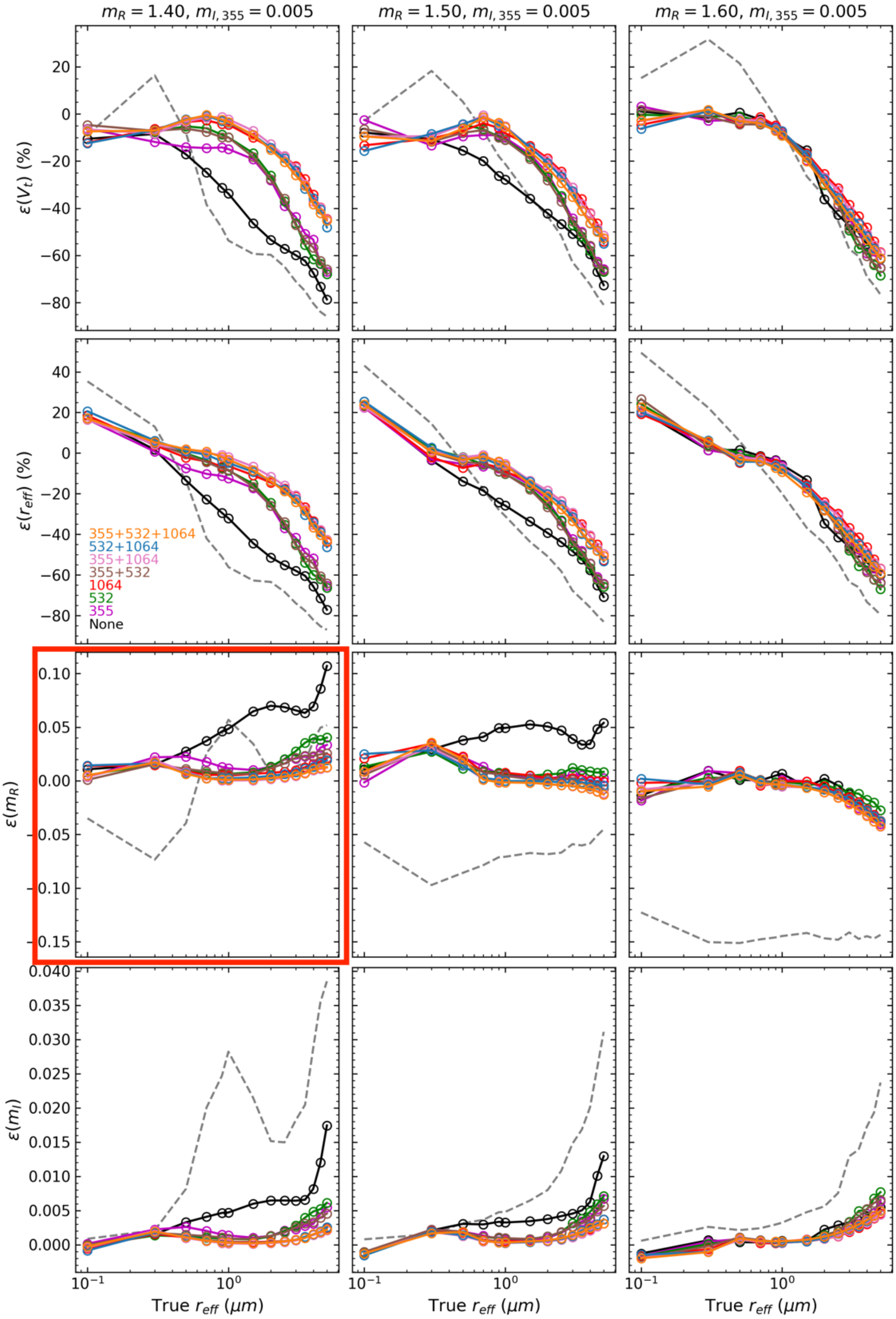

Figure. Same as Fig. 7 in the revised manuscript, but the results for different true values of m_R are shown. In addition, the results obtained by the spherical model are shown in dashed lines.

In the revised manuscript, we paid more attention to the non-spherical models and the comments related to the spherical model can be seen in L616-L620.

Line 477/figure 11:

- mI is overestimated in nearly all cases, but omega also stays in the region of overestimations.

- I consider this a highly interesting result as I would expect an underestimation of omega. Thus my question: what could be the reason for omega not obtaining lower values, given the overestimation of the imaginary part? Or do you show absolute errors only?

A: The omega is underestimated in fact. Here shown are the absolute values, as indicated by Eq. (12).

Line 504, ... for Vt and $r$eff, and for mR and mI, while it shows a negative correlation for Vt and...:

- You mention V_t twice, the first time in the context of a positive correlation and the second time in the context of a negative correlation. Can you please check this sentence once more?

A: We realized that this expression is quite confusing. We were trying to say that: the retrieval differences in V_t and in r_{eff} are positively correlated, namely when the spheroidal model derives a higher V_t than the IH model, it also derives a higher r_{eff}; similarly, the retrieval differences in m_R and in m_I are also positively correlated; whereas, the retrieval differences in V_t and in m_R are negatively correlated.

Line 521, ... turns ...:

- 'turn' instead of 'turns'

A: We realized this mistake. However, this subsection has been removed according to the comments of another reviewer.

Line 539/540, ... data. Furthermore, note that the long tail of positive 540 $\varepsilon(\varpi)$RMS occurring for the Sphere model corresponds to the long tail of positive $\varepsilon(m$I).

- please see my comment in the context of figure 11 (my note on line 477).

A: Please see the corresponding response to that comment.

Line 542/figure 15:

- that's a great set of results/presentation style!

A: We appreciate your affirmative! However, this subsection has been largely modified according to the updated settings for the retrieval simulation. Please see the updated one (Sect. 4.2).

Line 588/figure 16:

- I suggest you write a short sentence in the figure legend where you mention that this sudden increase of delta and LR at 2.9 to 3 km is driven by the strong gradients occurring when going from an aerosol layer to an aerosol-free layer. People not familiar with lidar data analysis might otherwise consider the strong increase as a dust feature.

A: The explanation has been added to the caption.

Line 591-593, … km. In particular, the decline of $r$eff above 2.2 km, retrieved from (3β + 2α + 3δ) measurements, supports the conclusion drawn by Hu et al. (2020) that a lifted fine-mode anthropogenic aerosol layer was above the well-mixed dust layer due to convection.:

- Without going into details of already published work (Hu et al., 2020): delta shows values around 0.25 in this anthropogenic-pollution layer (2.3 to 3 km). Doesn't this result indicate a mixture of anthropogenic pollution with dust?

A: Yes, it does. Thus, in the revised manuscript, we pointed out that the retrieval accuracy of the layer above 2.3 km cannot be guaranteed from the simulation results which are built on the pure dust assumption (L688-L689); and we focus on the layer between 1.5 and 2 km (Fig. 18).

Line 600, … between 2 and 2.2 km, showing …:

- The following text corroborates the results on mineral dust. Still, I am wondering why you picked a layer that is so close to the anthropogenic layer- thus maybe being affected by (minor) intrusions of anthropogenic particles from above. Wouldn't it be better to pick to height range that is more in the center of this well-mixed dust plume?

A: We repicked the layer between 1.5 and 2 km which is far from the anthropogenic layer and has evenly distributed optical properties in the revised manuscript (Fig. 18, L704).

Line 641-643, … Unlike in Case 1, AERONET derives a bimodal VSD with the coarse-mode $r$eff obviously larger than the BOREAL results. Moreover, compared to the BOREAL retrievals, the CRI from the AERONET retrieval is smaller and spectrally dependent for both real and imaginary parts.:

- It might be worthwhile pointing out that AERONET retrievals consider the whole column, thus representing an average set of data that is not considered in the case of the retrieved results (from lidar data) in this study.

- This is to my opinion a clearer statement on this topic than the sentence in lines 643- 645.

A: We agree with your suggestions and stressed the limitations in the comparison with the AERONET results in Sect. 6.3 (L853-L861). The modified expression of L643-L645 is in L869-L873.

Line 658-560, … loading. The volume concentrations derived with BOREAL and AERONET are in the same order, while the effective radii derived with BOREAL are smaller than the corresponding AERONET values by 30–50% regardless of the selection of the retrieval configuration …:

- Are these differences driven by the difference between column-integrated/column-averaged results and vertically resolved/layer-specific results?

A: Yes, we think so, especially for Case 2. Due to the columnar average effect, the coarse-mode part retrieved by AERONET can deviate from that of the dust layer.

table 6, … Col. AOD440 … 0.65 … 0.28:

- You could add the column-mean extinction coefficient, which might allow for a more detailed interpretation of the differences/agreements between results obtained from AERONET data and lidar data.

A: The contrast between the lidar-measured extinction coefficient and AERONET-column-mean extinction coefficient has been specified in L856-L858.

Line 696, … to 11–12 μm due …

- is it a typo? Shouldn't it read as 1.1- 1.2?

A: Yes, it is a typo. It should have been 0.47-0.48 μm (row for r_{eff} in Table 6).

line 702, … close to the AERONET-retrieved value in …:

- I'd like to repeat my question/comment regarding the challenge of comparing a column-integrating set of results to layer-specific data retrievals. Thus, a short note (in this spot of the paper) would be helpful for other readers of the paper.

A: The updated discussion section related to the comparison with AERONET results are provided in Sect. 6.3 (L852-L873).

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