Reply to the reviewers:

Reviewer 1. General Comment:

This manuscript addresses one of the most relevant topics in the study of desert dust, which is the need to better characterise coarse and super-coarse particles in the atmospheric load. Using data collected during two campaigns (the 2021 Cyprus Fall Campaign and the 2022 ASKOS campaign in Cape Verde), the authors aim to demonstrate that ground based remote sensing instruments, such as the AERONET sun-photometers, have limitations in their products when it comes to particles larger than 15 μm, leading to an underestimation in the number of these particles. They also show the relevance of parameters such as the shape of these particles in simulating their properties. To this end, the authors focus on comparing the volume-extinction ratio obtained from their observations with the value that can be determined from AERONET products or simulation algorithms. The observed values are based on extinction profiles obtained from lidar measurements and volume concentrations from particle counters installed on UAVs.

The topics covered in this study are highly relevant and fall within the scope of this journal. The structure is clear and logical, and the language is formal and easy to understand. The methods and selection data criteria used are described in detail, and the estimated uncertainties are well justified. The figures and tables are presented clearly and are relevant for illustrating the results. The research is original, as few campaigns involving UAVs have been carried out, and the results are compared with the literature. In addition, the article includes an adequate number of references and the dataset used is provided. The title is appropriate and clearly represents the objective of the study. The abstract accurately describes the research conducted and the relevance of the results obtained. The conclusion summarises the methodology, discussion and reinforces the relevance of the results. Therefore, I strongly recommend the publication of this article after some minor revisions are addressed.

We sincerely thank the reviewer for the time and effort dedicated to evaluating our manuscript, for his or her appreciation of our work, and for the positive and constructive feedback. We have carefully addressed all the revisions suggested and we believe that the revised version of the manuscript has been substantially improved as a result of the reviewer’s comments.

Reviewer 1. Specific Comments:

RC1.1: In Figure 1, four values of the sphericity are indicated, but only three colour intensities can been distinguished.

Thank you for pointing this out. Now the plot has 4 lines. See the new Figure 1 in the revised manuscript.

RC1.2: Introduction: Do you have any information about the error in the retrieved parameters from AERONET (AOD, AE, volume size distribution and refractive index)? What data level is used in the analysis?

We thank the reviewer for this comment. Level 1.5 AERONET data were used in the analysis (as we have now specified in section 4.2.2 of the revised manuscript). The estimated accuracy of the AERONET AOD product inferred from the literature is ±0.02 (Holben et al. 1998), while the uncertainties in the retrieval products (e.g., volume size distribution) depend on the aerosol load (Sinyuk et al. 2020), being up to 35% for radii up to 7.0μm while uncertainties rise up to 100% for larger sizes (Dubovik et al., 2000, 2002). This clarification has been added into Section 4.2.2 of the revised manuscript (Lines 405-412).

RC1.3: Sections 3.3.1. and 3.3.2: It might be better to indicate the name of the instrument in the section title and use the abbreviation from there on (e.g. 3.3.1. Portable Optical Particle Spectrometer (POPS).

Thank you for the suggestion, the section names were changed to the suggested ones. See section titles for 3.3.1 and 3.3.2 in the revised manuscript.

RC1.4: Section 3.3.3: Is the sensitivity of the impactors known?

We thank the reviewer for this question, which allows us to add more information on this, however we refer to the paper cited below for more information. The following text has been added in the newly revised paper to answer this question (see lines 294-298290):

Kezoudi et al. (Kezoudi, M., Papetta, A., Kandler, K., Ryder, C. L., Leonidou, A., Keleshis, C., Stopford, C., Thornberry, T., Mamouri, R.-E., Sciare, J., and Marenco, F.: Microphysical and Compositional Differences Between Saharan and Middle Eastern Dust Revealed by UAS Observations, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2025-5234, 2025.) discusses the collection efficiency of the impactors in comparison with UCASS, and finds that it is near 1 in the size range 4-14 um in diameter, falling down to much less than that (~ 10%) for smaller particles, and decreasing progressively to a similar low efficiency for particles > 20 um. Given that the impactors are not used here to determine the PSD, but rather to determine intensive properties such as the refractive index and the particle shape, we deem this uneven collection efficiency as acceptable.

RC1.5: It might be better to group sections 3.4 and 3.5. under a new subsection 3.4. focused on simulation algorithms.

Thank you for the suggestion. Sections 3.4 and 3.5 have now been combined under a new subsection titled "Simulation Algorithms." (section 3.4 of the revised manuscript, with two subsections).

RC1.6: Line 276: What lidar wavelength is used?

Thank you for the comment. The lidar wavelength (532 nm) has now been added to the text at line 313 of the revised manuscript.

RC1.7: Line 277: Unfinished sentence.

Thank you for pointing this out. We removed this.

RC1.8: Section 3.5, line 295: Why is that specific configuration selected?

Thank you for the comment, which help us add more information on the selection of the configuration. This specific configuration was selected so that dust emissions are treated as active tracers within WRF-Chem, allowing them to interact dynamically with atmospheric processes (advection, convection, boundary-layer mixing, and deposition). The model also includes dust–radiation interactions, enabling feedbacks with meteorological fields. Regarding the dust emission scaling constant (C), we used C = 0.4 $\mu g\ s^2\ m^{-5}$, following the evaluation of Zhao et al. (2010), who showed that this value yielded realistic AODs for the Sahel region. Given the comparable dust source characteristics in the EMME region, and following Georgiou et al. (2022), this value was adopted in our configuration. The text has been revised to include these clarifications (Lines 334-338 of the revised manuscript).

RC1.9: Table 1: please indicate the meaning of the acronyms (VLDR, AOD NIC// AMX, OSCM) in the table caption.

Thank you for the suggestion. The acronyms (VLDR, AOD, NIC//AMX, and OSCM) are now defined in the caption of Table 1.

RC1.10: Section 4.2.1, line 331. What AOD value is chosen as the constraint?
We thank the reviewer for the comment. The AOD value used as the constraint was shown in Table 1, but for clarity the reference to the table has now been added also to the text, Line 387 in the revised version.

RC1.11: Figure 2: Why are not UCASS and POPS landings indicated?
We thank the reviewer for pointing this out. We have modified Figure 2 to show the flight time of UCASS and POPS. Also, the landing times of both UCASS and POPS are now indicated in Figure 5.

RC1.12: Figure 4a: The curve representing the mean values is difficult to distinguish. I suggest plotting the other curves with dashed lines.

We thank the reviewer for the suggestion. To improve the current version of the plot we increased the thickness of the mean line.

RC1.13: Figure 5a: Why are there red points if the RH limit is not exceeded?
Thank you for the question. All red points in the four figure panels are where the RH limit is indeed exceeded, but perhaps the reviewer has been confused by the red dots on the temperature profile curve?

RC1.14: Section 4.3.3.: How are the data points fitted in the intervals where measurements are taken from both instruments?
We thank the reviewer for the question. In the intervals where measurements from both instruments are available, the log-normal functions are fitted to all the data points within those intervals (Line 530-531 of the revised manuscript).

RC1.15: Figure 10: legend, please include WRF-**Chem**

Thank you. WRF-Chem has been added to the figure legend.

Some typos:

Thank you for spotting these. Please see detail below.

RC1.16: Line 1: "relay", maybe relate? Ok (Line 1 of the revised manuscript)

RC1.17: Lines 6-7: "observations" is repeated Ok (Lines 6-7 of the revised manuscript)

RC1.18: Line 61: perhaps include a parenthesis for the references? Ok (Line 66 of the revised manuscript)

RC1.19: Line 92: two uses of e.g. Ok (Line 99 of the revised manuscript)

RC1.20: Line 97: satellite "images"? This is correct as it is, because we here mean a type of instrument (imager). See e.g. https://ntrs.nasa.gov/citations/20150008538

RC1.21: Line 102: "outlines" Ok (Line 110 of the revised manuscript)

RC1.22: Line 149: Eq. 17 from section 4.3.3. Eq 18 from section 4.3.3 (Line 156 of the revised manuscript)

RC1.23: Line 208: "Raman" Ok (Line 216 of the revised manuscript)

RC1.24: Line 254: u (m/s) Ok (Line 282 of the revised manuscript)

RC1.25: Line 262: Scanning Electron Microscope (SEM) Ok (Line 288 of the revised manuscript)

RC1.26: Line 274: aspect ratio (AR) Ok (Line 311 of the revised manuscript)

RC1.27: Line 286: radius between 0.1 to... Ok (Line 323 of the revised manuscript)

RC1.28: Line 288: Cyprus Institute (CYI): the CYI acronym is defined before in the Fall Campaign section 3.1.1 (see line 181 of the revised manuscript).

RC1.29: Line 407: Refractive index (RI): Ok (Line 503 of the revised manuscript)

RC1.30: Line 502: theoretical: Ok (Line 628 of the revised manuscript)

RC1.31: Figure 11: first line of caption, please remove one in from "in in Table 1" Ok

Reviewer 2. General comment:

The manuscript presents the investigation of the volume-to-extinction ratio of dust. In general, this is a very interesting and important topic, however, the manuscript comes short in a proper discussion and explanation of some of its findings and conclusions. Furthermore, to my opinion, the authors are often very imprecise with their explanations/descriptions. Therefore, I suggest major revisions of the manuscript before publication.

In the manuscript, two cases are discussed, and the derived volume-to-extinction ratio is compared to former studies. It would be very helpful and valuable to include a detailed characterization of the two cases. To my opinion, the two cases are not directly comparable to former studies of the volume-to-extinction ratio.

The authors should include a better / more detailed discussion about in-situ measurements, especially for the Cape Verde case showing very large values.

The structure of the manuscript is not quite straight forward.

We thank the reviewer for taking the time to carefully read our manuscript, for finding the topic of the volume-to-extinction ratio scientifically relevant, and for the constructive feedback provided.
We agree that a clearer characterization of the two selected cases is important, and we have addressed this in response to RC2.20 and RC2.36. As also noted in RC2.40, the cases considered here, (i) a mixed dust event and (ii) a dust case with the presence of giant particles, are not isolated cases, and similar events have been reported previously in the literature. However, this study is the first to use such cases for deriving the volume-to-extinction ratio, illustrating the methodological challenges and limitations of approaches based on AERONET or aerosol models, particularly when we are in the presence of particles of a size beyond the investigative range of those methods.

We have thoroughly revised the paper following the general and specific comments provided by the reviewer and this has allowed us to substantially improve the clarity and structure of the manuscript.

Reviewer 2. Specific comments:

RC2.1: Line 2: The authors refer to once as volume-to-extinction ratio and once as parameter. It should be made clear that the same is meant.

Thank you for pointing this out we keep the term “volume-to-extinction ratio" and we have removed all instances of the word “parameter” when referring to this quantity.

RC2.2: Line 3: Does the volume really depend on the composition? How?
Thank you for the comment. In the abstract at lines 2-3 we mentioned that the volume-to-extinction ratio depends on composition (through the refractive index which affects

the cross-section and thus the extinction). We did not write that the volume depends on composition. We hope that this is clearer.

RC2.3: Line 3: Is combining airborne in-situ and ground-based remote sensing really a novel, synergistic approach? There have been many more studies before doing this.

Thanks to the reviewer, maybe the combination of airborne in situ and remote sensing are not novel, but we need to say that there aren't so many studies out there. For instance, reviewer 1 suggested that "The research is original, as few campaigns involving UAVs have been carried out, and the results are compared with the literature." Moreover, the term novel is used here specifically for the exploitation of this combination of experimental methodologies for the observation of the volume-to-extinction ratio of dust. We are not aware of other studies using the method introduced here, but if there are we would be grateful if the reviewer could provide the references. We removed the word 'novel' anyway to avoid any confusion.

RC2.4: Line 8: 'reveal significant variability…' – Isn't this depending on the different cases which show differences in the measured properties and rather refer to different mixtures which are reflected by the variability than to a large variability for the same composition.

Thank you for the comment. As explained with equation (11), the volume-to-extinction ratio for particles much larger than the wavelength mainly depends on geometrical factors (the size and the shape), and the full treatment (equation 9) also includes the scattering efficiency which in turns depends also on the refractive index. Our results show that the volume-to-extinction ratio is affected, in the cases considered, by the contrast between pure dust and dust mixed with other aerosols. It is also affected within a same event by the variation of the geometrical properties of the particles with altitude. To clarify this point, we have revised the sentence at Lines 7-9 of the revised manuscript.

RC2.5: Line 33: 'dust can act as both CCN and INP' – Dust itself (talking about the coarse mode in the mixture as linked to the previous sentence) shows no hygroscopicity and is thus a rather inefficient CCN, except if you refer to dust as a mixture including also the fine mode contribution. But that should be made clearer, especially as you are talking about the coarse mode in the previous part of the section. Give references for your statements.

We thank the reviewer for raising this point, which has led us to expand our literature review. Although freshly emitted dust is largely insoluble and therefore has limited hygroscopic properties, it has been shown that the uptake of soluble substances on the dust particles (internal mixture) can enhance its CCN efficiency (Kumar et al., 2009a;

Nenes et al., 2014, p.2). Based on Kohler theory, the larger the soluble particle the lower the supersaturation needed to activated as CCN (Andreae et al., 2005). Thus, even in lower concentrations, coarser dust particles coated with soluble material, can act more efficiently as CCNs (be activated at lower supersaturations) compare to the finer ones.

The cited studies show that the CCN activity of dust is not limited to fine dust particles. Even though larger particles may be in lower concentration in the atmosphere, however their larger size increase its efficiency as CCNs.

We have revised the relevant text in the document accordingly, including relevant references (Lines 35-38 of the revised manuscript).

RC2.6: Line 34: which models? Give references.
Thank you for the comment. We have now specified what is meant with atmospheric models (Lines 39-40 of revised manuscript).

RC2.7: Lines 39f: Which various environmental and economic applications? Please give references.

Thank you for the comment. The environmental and economic applications mentioned refer to climate projections, air quality assessment, and aviation safety, already mentioned in the text. For clarity we added respective references for each of the mentioned applications (See lines 44-46 of the revised manuscript).

RC2.8: Line 51: Why is the density important for the volume?

Thank you for the comment. The density is not required to derive the particle volume itself, but it becomes necessary when converting between mass and optical quantities (see equation 4). To clarify this, we have revised the text (Lines 55-56 of the revised manuscript).

RC2.9: Line 56: What is meant by traditional in-situ measurements? To my opinion airborne in-situ measurements are already well-established and provided valuable data also on this topic.

Thank you for the helpful comment. In this context, our intention was to refer to ground-based in-situ measurements. To clarify this, we have revised the text in Lines 61-62 of the revised manuscript by replacing “traditional” with “ground-based”.

RC2.10: Line 70: To derive the vertical profile, what is the required sample time at one altitude? Please answer in the methodology section.

We thank the reviewer for this question that prompted into better explaining the observations taken for our campaigns. We have chosen for our study a vertical sampling resolution of 10 m, thus downsampling our UAV observations obtained at 1 Hz and with a ~1 m/s ascent rate. This information has now been added to the methodology section, at lines 426-429 of the revised manuscript.

RC2.11: Lines 75ff: It has to be mentioned that in-situ measurements have to measure the full size-range for improvements. For that, it is also of importance to give a better description of the in-situ measurements in the methodology section.

We thank the reviewer for this valuable comment. We agree that improvements in retrieval methods should, whenever possible, be achieved with in-situ observations covering the full particle size range, or alternatively take into account any significant part of the size spectrum that may be missing. Accordingly, we have updated the paragraph in the Introduction (see line 80-82 of the updated manuscript). The methodology section (Sections 3.3.1 and 3.3.2) includes a detailed description of the measurement ranges of the optical particle counters used in this study.

RC2.12: Line 175f: Did really ESA organize the campaign?

Thank you for the comment. Indeed, the Joint Aeolus Tropical Atlantic Campaign (JATAC) was organized by ESA and NASA (see Marinou et al., 2023), while the ASKOS campaign was the ground-based component led by the National Observatory of Athens (NOA), where CYI contributed with airborne observations. To clarify this, we have revised the text (Lines 187-189 of the revised manuscript).

RC2.13: Section 3.2.1 CIMEL: What is the power of the lidar? And is an upper altitude of 30 km really achievable with a cimel lidar? Please give a reference for the system.

Thank you for the comment. The CE376 system uses a green laser with a pulse energy of approximately 6 µJ and a near-infrared laser with approximately 4 µJ. As mentioned at lines 208-209 of the revised manuscript (line 195 of the preprint), the typical maximum detection altitude is around 10 km during daytime and up to 18 km during nighttime. The upper range of 30 km refers to the recording range, not to the effective detection altitude. A detailed description of the system is provided in Papetta et al. (2024), and additional technical information is available on the manufacturer’s website (https://www.cimel.fr/solutions/ce376/). The paragraph has been revised accordingly for clarity.

RC2.14: Section 3.2.2 PollyXT: Please give information about the lidar instrument that are relevant for this study and describe the performance of the system.

We have added the additional info as requested by the reviewer, and we've added additional references which provide good description of the instrument. The relevant information about the lidar, for this paper, is included, and more details can be found in the cited references (Lines 228-232).

RC2.15: Section 3.3.1 POPS: Please provide information on the measurement principle. How to derive the PSD and the volume? What is the effect of drying the aerosol on the comparability with studies of ambient conditions?

The POPS (Printed Optical Particle Spectrometer) is an optical particle counter widely used in the aerosol research community (see e.g. the references cited in the paper). The instrument determines particle size based on the intensity of light scattered by individual particles as they pass through a focused laser beam. The PSD is derived from the number of particles detected in each size bin during a specified period of time (1 second). The particle volume can easily be calculated from the diameter (Section 4.3.1). Please note that we have now replaced the wording optical particle counter to the more generic term optical particle spectrometer.

An inlet allows the air into the optical chamber, with a pump controlling the flow. In these conditions (pumping through an inlet) the temperature of the air is altered and therefore also the relative humidity: therefore it is not possible to use this instrument to sample ambient conditions, and it is preferable to dry the air so that the aerosols are in a known state of hydration. Drying the air also protects the chamber from flooding when flying in humid atmospheres. This explanation has been added in lines 261-264 of the revised manuscript.

RC2.16: Section 3.3.2 UCASS: Please provide information on the measurement principle. Again, how is the PSD and the volume derived?

Like for the POPS, the UCASS determines particle size based on the intensity of light scattered by individual particles as they pass through a focused laser beam, and counts those particles within a number of pre-determined size bins. Differently than for POPS, UCASS is an open-path instrument and the scattering experiment is performed in the atmosphere in ambient conditions. Using an open path instrument is essential when observing the larger particles, because inlet systems tend to lose those particles before they reach the instrument. This information has been added in lines 272-274 in Section 3.3.2.

RC2.17: Section 3.3.3 Impactors: Please provide information on the measurement principle and what is measured.

We have added additional information in Section 3.3.3 describing the measurement principle and analysis of the impactor samples, citing also references for further details on the methodology and associated uncertainties.

RC2.18: For all in-situ measurements, please include a better description of the uncertainties of the in-situ measurements.

Thank you for the comment. Section 4.3.2 provides the uncertainties which are related with the OPS observations. For the impactors cited references in Section 3.3.3 have detailed discussion on the methodology and associated uncertainties of the impactors. In addition, based on community feedback, we have expanded the discussion in Section 4.3.2 on UCASS, including the consistency of extinction retrievals between UCASS and the Raman lidar, as well as the ACTRIS calibration performed on the UCASS units used in this study to confirm the accuracy of the UCASS size-bin settings (Lines 500-502 and 509-514).

RC2.19: Line 282: Why is there trust in the model about this information, when there is little trust in the models in general?

In our study we use the WRF-Chem model to compare the volume-to-extinction ratio that it derives from its internal microphysical representation of dust to our observations of this quantity. Models, such as this one, are frequently used in the atmospheric sciences, and it is not true that “there is little trust in the models in general”. In fact, models often fill the gap between observations and permit addressing atmospheric features and processes with a more global perspective. Models generally benefit from comparisons with observations, because the latter can trigger awareness of the modelling limitations, and can further trigger model improvements. Moreover, our knowledge can only be enhanced by the comparison of dust in the atmosphere as represented with different methodologies, giving us different perspectives, rather than by ignoring some tools selectively. We therefore disagree with the reviewer, who suggests that “trust” should be the reason why we choose to use the model or not for this comparison.

RC2.20: Case selection: More information on the different cases would be helpful, especially for the Cape Verde case with very large reff.

Thank you for the comment. The selected cases were chosen based on three criteria as mentioned in the manuscript (Lines 349-353 of the revised manuscript): (i) availability of multi-instrument observations (UAV-OPS, lidar, and sun-photometer), which are required to apply the proposed methodology; (ii) cloud-free conditions, ensuring reliable lidar extinction retrievals and robust AERONET size distribution products; and (iii) dust presence (e.g., AOD > 0.2). The main characteristics of the selected cases are summarized in Table 1. We believe this table provides a detailed overview of the main parameters for each case (incl. AOD, depolarization, Angstrom etc.). To further support the discussion, we have added HYSPLIT back-trajectory analyses for the selected cases in the Appendix. Regarding the large reff, previous studies have reported mean effective radii of approximately 2–10 µm in the Saharan Air Layer (SAL) and in regions close to the dust sources over the Sahara (Ryder et al., 2018; 2019). This information has been added to the revised manuscript (Lines 363–368 of the revised manuscript).

RC2.21: Line 326: What is the criteria for uniform?

Thank you for spotting this. We incorrectly used the term “uniform” referred to the vertical structure of the dust layer. We have clarified this by rephrasing the sentence to ‘vertically homogeneous’ at Line 381 of the revised manuscript

RC2.22: Line 328: The VLDR follows the concentration of the aerosol and can thus be misleading. Why do you not use the PLDR for a clearer characterization and classification?

Thank you for the comment. We agree that the particle linear depolarization ratio (PLDR) would provide a clearer characterization of particle type and shape, and therefore we updated Fig.3 to show both VLDR and PLDR. However for Figure 2, we prefer to use VLDR as it is less noisy and moreover as the reviewer points it gives also a feel of the dust quantity in the air. Please note that in this study the linear depolarization ratio is used only for illustrating the vertical and temporal structure of the observed aerosol layers. The VLDR/PLDR values are not used in any of the quantitative analyses or calculations presented in this work.

RC2.23: Line 332: The lidar ratio of 35 sr rather indicates that this is not a pure dust case.

Thank you for this comment. Previous studies conducted in the region have shown that values around 35 sr are typical for Middle Eastern dust (Mamouri et al., 2013; Mamouri and Ansmann, 2016; Teri et al.,2025). We agree with the reviewer that the Middle Eastern dust may not necessary be “pure”, and this is a good reason to characterize it. We have clarified this point in the revised manuscript Lines 390-393.

RC2.24: Line 334: ‘addressing this uncertainty is beyond the scope of the present study.’ – To my understanding, addressing this uncertainty is essential when the property is used to retrieve the ratio that is claimed to be the main finding of this study! Please give at least an estimate and range for the uncertainty.

Thanks to the reviewer for pointing out this very important point. We fully agree that the uncertainty on the lidar ratio (LR) and on the retrieved extinction coefficient should be quantified, as it directly affects the retrieval of the volume-to-extinction ratio. We have now revised the manuscript and have added an estimate of these uncertainties in lines 393-396, as well as in Figure 10.

RC2.25: Line 335f: Please see my prior comment that the VLDR follows the concentration.

Thank you, as mentioned also in RC2.22, Figure 3 was updated to show also PLDR.

RC2.26: Line 340: What is a sky error?

We thank the reviewer for this question. The “sky error” refers to the discrepancy between the measured and model-fitted sky radiances. This quality check is also described in Dubovik et al. 2002. This text has been added in the revised manuscript Lines 406-408.

RC2.27: Line 345: What does balanced contribution mean? A higher contribution of the fine mode does indicate that in this case you don’t have ‘pure’ dust, right?

Thank you for the comment. We understand that the term balanced contribution may be confusing, and we have also considered the reviewer’s observation regarding the fine-mode contribution as an indication of non-pure dust. To improve clarity, the sentence has been rephrased in Lines 414-417 of the revised manuscript.

RC2.28: Figure 4: The main point you want to highlight with this figure should be mentioned in the text.

Thank you for your comment. With this figure we want to (1) show the VSD that can be achieved with AERONET; (2) illustrate the little variability of this VSD during the day in the Cyprus case (unfortunately not possible for the Cape Verde case due to insufficient observations) and (3) highlight that the size-distributions for these two cases differ substantially. This description is added in the text Lines 412-414 of the revised manuscript.

RC2.29: Figure 5: Same here. Please describe what you want to show with this figure and don't leave it to the reader to draw their own conclusions.

Thank you. Figure 5 highlights the atmospheric conditions (temperature and RH) encountered during the flights and illustrates how the atmospheric quality criteria are applied and help us identify interesting layers like the marine boundary layer top and the SAL layer. We revised the text to include this description in Lines 436-445.

RC2.30: Equ. 14: Maybe I have missed it, but where do you get the size information from?

We thank the reviewer for this question which permits to explain this better. Both POPS and UCASS provide size-resolved particle counts (sections 3.3.1-3.3.2) and the computation of the particle size-distribution is given in lines 424-428, explaining how the particle concentration (N_{c_i}) at a given altitude z and each size bin of radius r_i is calculated using eq. 12 This provides the size-resolved information used in Equation 14.

RC2.31: Line 383: There have been more studies on the in-situ as well as on the lidar side dealing with that kind of topic. Please give more balanced references of former studies.

Thank you for the comment. Indeed, apart from SAVEX-D several campaigns have demonstrated the presence of coarse particles, including the Fennec campaign (2011) (Ryder et al. 2013), SAMUM1-2 (Weinzierl et al 2009, 2011), SALTRACE (Weinzierl et al. 2017) and the AER-D campaign (2015) (Ryder et al. 2018). We have added these references in the revised manuscript, Lines 472-475.

RC2.32: Figure 7: Again, please describe the findings. And, it is quite clear that the two cases differ, looking at the information that has been provided so far.

Thank you. The purpose of this figure is (1) to assist the description of the methodology which uses height resolved particle size distributions to calculate the volume-to-extinction ratio, (2) to assist the reader in understanding how we combine two OPS for retrieving the full size distribution (as described in the text triangles are the UCASS observations and circles the POPS observations); and (3) to show how the PSD varies with altitude in the two cases presented as discussed at lines 469-474. The text has been revised for clarity (Lines 464-468).

RC2.33: Figure 8: Is the main information of this figure the log-fit? Please specify how the fitting parameters have been derived.

Thank you for the comment. Yes, with Figure 8 we want to demonstrate the procedure of calculating the log-normal fit by combining the data from both OPSs. In addition it assists the description of the methodology for the best fit.. The log-fit was performed using a non-linear least-squares approach on the particle number distribution values as a function of radius. The fitting routine minimizes the squared residuals between the observations and the modelled function (Equation 17). The presented shaded are presents the lognormal fit error are estimated from the square root of the diagonal elements of the covariance matrix returned by the fit, whilst the error bars are the standard deviation in the selected altitude range of the number and volume size distribution at each size bin. The resulting lognormal distributions are used later to derive column-averaged PSDs from OPSs and as input for the scattering calculations using MOPSMAP. We have expanded the discussion around this figure to include this information (see lines 531 – 540)

RC2.34: Line 444: There might be also large uncertainties connected to the in-situ measurements. A clear discussion on the uncertainties associated with the in-situ measurements should be included.

Indeed, there are uncertainties associated with the in-situ observations as mentioned in Line 563 of the revised manuscript (447 of the initial manuscript) and as explained in section 4.3.2. Following the reviewer's suggestion, we have now expanded this section with a discussion on how the uncertainties on both methods can influence the comparison between AERONET and the UAV-OPS observations (Lines 552-568). To assist this discussion we have also added the uncertainty envelope of AERONET.

RC2.35: Figure 9b: I do not understand how this volume size distribution is derived from the information given in Figure 7. Please provide more explanation.

Thank you for the question. The reviewer is probably looking at the y-axis scale that gives a maximum of around ~5 x 10^2 um3/cm3 in figure 7d and around 10^0 um3/um2 in figure 9b, and asks how these can be the same data. The column-averaged PSDs in Fig. 9 are derived from the height-resolved PSDs in Fig. 7 using Eq. 18, by vertically integrating the volume concentration between z_min and z_max (as stated in Lines 512–515 of the manuscript). This vertical integration introduces a scaling factor proportional to the thickness of the sampled layer (z_max − z_min ≈ 5 km ≈ 5x10^9 um) and accounts for the change in units (um3/cm3 in figure 7d and um3/um2 in figure 9b). Therefore the scaling factor in the case of 9b would be 5x10^9 um x 10^(-12) um^(-3)= ~ 5 x 10^(-3) cm^3/um^2.

RC2.36: Line 460: To my understanding, more small particles lead to less volume. This would explain the findings for the case. However, this is not a value for 'pure' dust.

Thank you for your comment. The relationship between effective radius is given by equation 9, therefore the reviewer is right in saying that more small particles lead to a smaller volume-to-extinction ratio. We agree that the 15 November 2021 case most likely does not represent a pure dust event, due to lower LR, lower VLDR, important contribution of fine mode particles and high Angstrom exponent. It is well-known that Middle Eastern dust is often mixed with fine-mode non-dust aerosols (Mamouri et al. 2013, 2016, Bimenyimana et al. 2025, Kezoudi et al. 2025). Throughout the manuscript, we have clarified that this case probably involved a dust mixture (Sections: 3.1.1 Fall Campaign, 4.1 Case Selection, 4.2.2 AERONET, 4.2.1 Lidar Observations). On the other hand such mixtures are frequent and when we speak of dust, often we may be referring to a mixture. Regarding Line 460, we've updated this sentence (see Lines 582-583 of the revised manuscript).

RC2.37: Figure 10: The presentation of the findings in this Figure makes it hard or impossible to see what is described in the text. Again, more and detailed information about that case would be very helpful as it is so different to all the other cases. Please also provide an estimate of the uncertainties of the in-situ measurements.

Thank you for the comment. Figure 10 is described at lines 579-627. Please can the reviewer explain better what is unclear? Regarding the uncertainties of the remote sensing and in-situ measurements used for the derivation of the zeta ratio these are shown as error bars in Figure 10.

RC2.38: Section 5.2: For my understanding, it would be much easier to follow if this section would be moved to the general description of the volume-to-extinction ratio.

Thank you for your comment. We removed the separated section and integrated this part to the general description of the volume-to-extinction ratio (Lines 621-627 of the new manuscript). In addition, Lines 542–546 (of the previous manuscript) have been moved to Section 3.4.2 (Lines 339-345 of the new manuscript) to improve the flow of the Results section.

RC2.39: Line 543: How is the total dust mass concentration derived?

Thank you for your comment. The total dust mass concentration at any grid point is obtained by summing the mass contributions from all dust size bins in the model domain. As mentioned in the text, the GOCART dust scheme is used to compute dust emissions from the surface, which are subsequently transported through advection, convection, and turbulent diffusion, and removed from the atmosphere via physical processes such as gravitational settling and wet deposition. As a detailed discussion of WRF-Chem processes is beyond the scope of this paper, we refer the reader to the cited references in Section 3.5 for further information.

RC2.40: Conclusion: It should contain a proper discussion also on the uncertainties of the in-situ measurements. In general, I find it difficult to put such a significance on the findings presented in this study, as the number of cases is not large, and as the largest differences to former studies were found for cases that are, to my opinion, rather not pure dust cases, or where further information would be needed to fully understand what is going on (including a proper error estimation).

We thank the reviewer for this valuable comment. As noted in our previous responses, we provide a description of the in-situ measurement uncertainties in Section 4.3.2, whereas also the cited references provide a detailed error estimation for each of the in-situ instruments used. While these uncertainties may influence the exact magnitude of super-coarse and giant particle concentrations, their presence cannot be neglected, given the consistent observation of larger particles throughout the ASKOS campaign (see also CC1.6).

We acknowledge that the study includes a limited number of cases (as mentioned in Lines 739-743).However, the selected cases are meaningful for illustrating the methodological challenges and limits associated with retrieving the dust volume-to-extinction ratio in the presence of giant particles, because AERONET and many atmospheric models typically assume an upper size cut-off or simplified size distribution. In addition, these cases allow us to highlight situations in which in-situ airborne measurements provide complementary information that may not be fully captured by current remote-sensing approaches.

For the selected cases similar features of Middle Eastern dust have been reported in previous studies. For instance, mixed dust cases with lower lidar ratios have also been observed by Mamouri et al. (2013, 2016), while other in-situ studies (Bimenyimana et al., 2025, Christodoulou et al., 2023) have shown that dust arriving in Cyprus from the Middle East can carry significant amounts of anthropogenic pollutants. On the other hand, as mentioned in the text, the East Atlantic case is consistent with earlier observations of giant dust particles in that region (e.g.Ryder et al., 2013, 2018; Kudo et al., 2021; Weinzierl et al., 2009, 2011, 2017)

In response to this comment, we have now expanded the conclusions section to discuss the interpretation of these results under the effect of the in-situ uncertainties and the limited number of selected cases (Lines 737-750).

Community Comment from Albert Ansmann. Introductory paragraph:

From the papers of Ryder et al. (2018) and Weinzierl et al. (BAMS 2017) we know already that large mineral dust particles with diameters larger than 30 micrometer can survive over several days of long range transport before they are removed by sedimentation processes.

On the other hand, we know also that the AERONET inversion scheme, used to analyze multiwavelength AOD and sky radiance data, to retrieve microphysical properties (including volume concentration) considers particle diameters up to 30 micrometer only so that volume-to-extinction ratios derived from AERONET observations may be wrong in the special situations, e.g. when analyzing lidar observations of optical dust properties close to the dust source region.

The question is now how relevant is this effect and what can we learn from the study presented in this manuscript, suggesting that a systematic underestimations of dust mass concentrations occurs when using the volume-to-extinction ratios derived from AERONET observations?

We are sincerely grateful to Albert Ansmann for providing input in order to help us improve this manuscript and better explain some points on which there seems to be some doubt. Yes: you have nailed a limitation of AERONET in the above sentences: the inversion scheme not only limits the upper particle size (this choice is explained by the reduced sensitivity of the AERONET wavelengths for the larger particles), but it also forces the volume size-distribution to decrease to zero towards that upper limit, and this forcing to zero may in fact “hide” the existence of the giant particles. Now the question is: how relevant is the existence of the giant dust mode? Is it happening sporadically, as we seem to understand from Albert Ansmann’s comment, or is it ubiquitous, as suggested by the title of a paper by Ryder et al (2019)?

It is difficult to give a definite answer on this point, due to the global AERONET network not being able to disentangle the picture, and because the evidence from campaigns is sporadic. However, there have been more and more observations suggesting that the previously underestimated existence of the giant mode occurs relatively frequently, at least in the source regions (Ryder et al, 2015) and in the Saharan Air Layer that traverses the Atlantic. These particles have been revealed by a variety of methods, including ground-based samples (Prospero et al, 1970), submarine sediment traps (van der Does et al, 2016), atmospheric samplers mounted on buoys (van der Does et al, 2018) and several recent airborne campaigns such as those cited by Albert. The modelling community has, for example, started to realise that previous assumptions on the dust particle size had led to the creation of models underestimating the coarse dust by design (Adebiyi and Kok, 2020). This discussion has been also added in the conclusions lines 691-695.

We can of course only report the observations that we have collected and we do not wish to generalise our results, but our suggestion is also that we should be open to accept the existence of such giant particles, when experimental evidence becomes available. It is by cumulation of such evidence over time that we will be able to answer the question of whether such giant particles are sporadic or more widespread. Our paper is not aimed at answering this question, but in the article we report having observed the giant particles during the ASKOS campaign, and this has a repercussion on the volume-to-extinction ratio for the specific case. The UAV data presented here cannot alone answer the more general question, but we wish also to remind that we had evidence of such large particles over Cape Verde also during the AER-D experiment in summer 2015 (Marenco et al, 2018; Ryder et al 2018).

A component of AER-D was the Sunphotometer Airborne Validation Experiment in Dust (SAVEX-D). Two research flights were performed nearby two types of ground-based sun photometers, a PREDE/SKYNET and a CIMEL/AERONET (Marenco et al, 2018; Estellés et al., 2018 ; Nakajima et al, 2020; Kudo et al, 2021). Similarly to the ASKOS case presented in our paper, the SAVEX-D experiment revealed the existence over Cape Verde of giant particles that had gone undetected by AERONET (Meritxell Garcia-Suñer, Víctor Estellés, Franco Marenco, Claire Ryder, Gaurav Kumar, Masahiro Momoi, Monica Campanelli, Akiko Higurashi, Hitoshi Irie, Debbie O'Sullivan, Jennifer Brooke, and Joelle Buxmann, Validation of AERONET and SKYNET columnar aerosol volume distributions with airborne integrated in-situ measurements in dust conditions, article in preparation.). Those observations had been obtained with a similar methodology (airborne in-situ particle observations, on-board the Facility for Airborne Atmospheric Measurements, FAAM) but with a large manned airplane and a completely different set of instruments. This should at least warn us about the fact that the undetection of such giant particles by AERONET happens sufficiently frequently to be a possibly credible observation, not to be easily dismissed.

My review here is motivated by a number of not well-discussed points and partly not well-explained aspects. I miss for example a more critical discussion of the airborne (UAV) measurements by the authors, especially regarding the limits of the applied methods to obtain the volume size distribution of dust particles when there are giant dust particles with diameters above 30-40 micrometer in the air.

Thanks for prompting us a thorough verification of our experimental methods, as this can only help us reinforce the paper. Although the UAV in-situ measurements are subject to their own uncertainties (as all observational methodologies), they provide valuable information, as also mentioned in RC2.40. We now discuss on the methodological limitations and uncertainties in the revised manuscript (Section 4.3.2 Lines 509-514, Conclusions Lines 737-743).

We address the reviewer's general and specific comments in detail below.

General Comments:

GCC1: Page 3, Line 70: The authors write: UAVs can provide high vertical-resolution profiles of aerosol properties for fine, coarse, and giant particles. My question: How well can particles with diameters of 20,30, and 40 micrometer be counted with the UCASS? A bias in counting, i.e, wrong counting of the few large to giant particles has a huge effect on the derived dust volume concentration and can dominate the volume concentration retrieval completely.

In the paper of Kezoudi et al. (ACP, 2021), a strong bias is shown (in Figure 9) between the retrieved dust extinction coefficient (from the UAV in situ observations of the size distribution) and the directly measured dust extinction coefficient (from Raman lidar) in pronounced dust layers. The extinction values from UCASS were systematically too large pointing to too large counts (regarding large to giant particles), in most case by a factor of 2. And the volume concentration derived from such wrong counts or particle numbers (or extinction coefficients) would be 'wrong' by a factor of 2 as well, I conclude.

Please, expand the discussion on this problem!

Thank you for the comment. We agree that miscounting of large particles can influence the derived volume concentration and consequently the ζ ratio. In the paper by Kezoudi et al. (2021), the UCASS derived extinction coefficient was larger by about a factor of two compared to Raman lidar, but this does not necessary mean that there is miscounting or a bias: the difference can be caused by sampling in a slightly different location, with more dust, and Figure 1 of that paper shows that the atmospheric scene was quite inhomogeneous. However, for the case for 24th June 2022, in Cabo Verde, the UCASS-derived extinction coefficients were consistent with those retrieved from the PollyXT lidar, providing additional confidence in the UAV-based PSDs for this case (see figure below). This observation of the reviewer was added to Section 4.3.2, Lines 509-514.

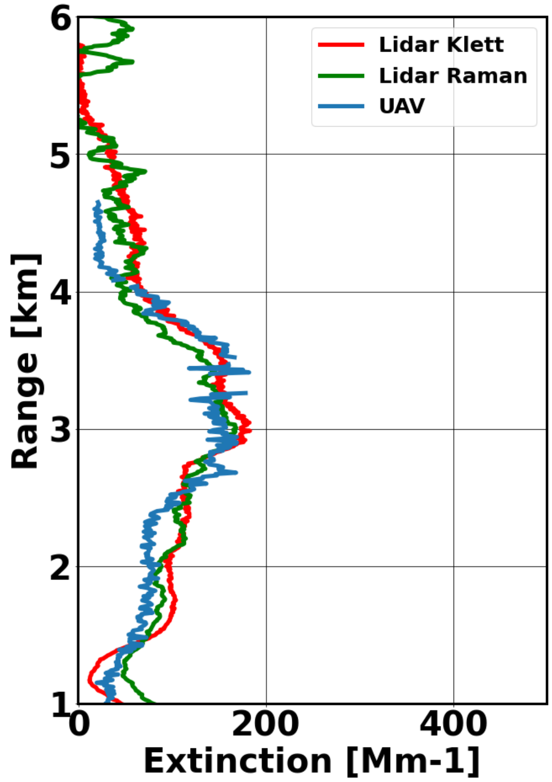

Figure 1 - Lidar extinction profiles (Klett, red line and Raman, green line) vs UCASS derived extinction profile (blue line) for the case 24th June 2022

GCC2: During the ASKOS campaign a strong dust outbreak was studied (Cabo Verde, 24 June 2022). The Angstroem exponent was around 0.15. However, to my opinion, the detailed multiwavelength lidar observations (they are not presented in this paper!) do not support the presence of very large to giant dust particles in the observed dust layers, as derived from the UCASS observations!

I checked the Polly lidar data base. The lidar measured particle linear depolarization ratios (PLDRs) of 23-25% (355nm), close to 28-30% (532 nm), and 22-24% (1064nm) on 24 June 2022 over Cabo Verde. These values are in good agreement with typical PLDR values after long-range dust transport to Barbados (4000 km west of Africa. Haarig et al., ACP. 2017), or even to central Europe after fast long-range transport (two days from Sahara to Leipzig, Germany) as reported by Haarig et al. (ACP, 2021). The drop of the depolarization ratios from 30% at 532 nm to 22-24% at 1064nm does, to my opinion, not support the presence of large to giant dust particles, i.e., of particles with diameters larger than 10 micrometer. Burton et al. (ACP, 2015) found exceptionally high PLDRs of 30% (355nm) , 40% (532nm) , and 40% (1064nm) in just ONE unique dust plume (in a dust source region) with freshly emitted giant dust particles. Hu et al. (2022) measured 32% (355nm), 37% (532nm), 32% (1064nm) close to the Taklamakan desert. Even here, close to the Taklamakan source region, the very large and giant particles seem to fall out quickly.

To my opinion, the PLDR spectrum measured with lidar over Cabo Verde on 24 June 2022 was quite normal for aged dust plumes, pointing to dust particles at all smaller than 5 micrometer (in radius). These lidar observations are in agreement with the accompanying AERONET photometer observations (AODs, size distribution) at Cabo Verde, pointing to a dust effective radius around 2-3 micrometer. More details to AERONET observations are given below.

Apologies to disagree with the comment here, but PLDR is not usually considered a measure of particle size, whereas it is known that the Angstrom exponent is typically near-zero when the particles are much larger than the wavelength of light, as predicted by geometric optics (scattering cross-section independent of wavelength). Usually PLDR is associated with particle shape, and not size.

The particles with diameters larger than 10 µm (radius) represent only a fraction of the total number concentration in this case <0.1%, accounting for roughly up to 10% of the total volume and ~1% of the total extinction. Therefore these large particles will have a limited impact on optical properties. Also, the PLDR is not expected to be significantly affected, since it is primarily sensitive to particle shape rather than number concentration or optical cross-section.

GCC3:My rough computations indicate that particles with diameters of 20, 30, and 40 micrometer fall about 500m, 1500 m, and 3000 m per day, respectively, at tropospheric conditions below 4 km height. How many days did the dust travel from the source regions to Cabo Verde arriving on 24 June 2022 according to HYSPLIT backward trajectories? Probably more than three days? I miss trajectory studies and travel times in the manuscript.

Thanks to the reviewer for the comment. HYSPLIT backtrajectories have been added to the Appendix of the manuscript following also comment R2.20. You are correct that for the 24 June 2022 event, the trajectories indicate air masses originating from Mali and Mauritania, traveling for approximately 2-3 days before reaching Cabo Verde. For the 15 November 2021 event, the air masses originated from the Middle East, passing over Iraq, Saudi Arabia, Jordan, and Israel before reaching Cyprus after 3-4 days of transport. These results have been added to the main text (Lines 371-374), and the trajectory plots are shown in Appendix A. It is also true that our current understanding of the particle transport processes is that these particles should fall (we trust your calculations), whereas the experimental evidence is that they stay suspended in the atmosphere much longer than one would expect. This discrepancy between observations and theory is discussed for example in van der Does (2018) and in Adebiyi and Kok (2018). Like the cited authors, we are not able to give a final and proven explanation on why the particles stay suspended in the atmosphere for such a long time.

In conclusion, the message of the paper is clear. The study is worthwhile to be published! We have to be careful when converting dust optical properties into dust mass concentrations based on AERONET derived mass-to-extinction conversion factors! But how relevant is this effect? How often do such conditions occur? This important question remains open!

Thanks very much for your appreciation of our work. And yes, we will need more systematic observations of the volume-to-extinction ratio, which is the property that controls the conversion of dust optical properties into dust mass concentrations. We believe that the reliance on AERONET may not be sufficient for this, and/or that AERONET retrievals could in the future be improved to remove the constrain of the size-distribution to zero at the upper boundary of the size interval. As to your questions (how relevant is this effect? How often do such conditions occur?) please see our reply to the introductory paragraph of your comment.

CC1. Specific comments:

CC1.1: Page 5, In Eq. (7), the effective radius is 3V/4A! Maybe I missed the point! To my opinion, the effective radius is defined as 3V/A.

Thank you for the comment. In our formulation, the effective radius is expressed as the definition used in Schumann et al. (2011, Eq. 5). In this definition A is the projected area. For spheres we have S=4A where S is the surface area, and in this case we would have 3V/S: maybe this is what you meant?

CC1.2: Page 9 241-242: POPS can provide reliable vertical profiles of the particle size distribution. Please provide uncertainty numbers already in this section!

We thank the reviewer for this comment. We chose to present all instrument-related uncertainties, including those of the POPS measurements, in a dedicated section (Section 4.3.2). We have added a sentence in Section 3.3 (Line 252-253) to refer the reader to Section 4.3.2.

CC1.3: Page 9, 249-250: UCASS airborne particle size distributions were in close agreement with other reference OPCs? Please provide clear uncertainty values already in this section?

We thank the reviewer for this comment. The UCASS units used in this study was calibrated at the University of Hertfordshire and has been previously compared with other reference optical particle spectrometers, including the CAPS, as described in Sections 3.3.2 and 4.3.2. In addition, a comparison between UCASS-derived and lidar-retrieved extinction coefficients (Figure 1, GCC1) showed good agreement, providing an additional level of confidence in the measurements. We chose to present all instrument-related uncertainties, including those of the UCASS measurements, in a dedicated section (Section 4.3.2). We have added a sentence in Section 3.3 (Line 252-253) to refer the reader to Section 4.3.2.

CC1.4: Page 12, Table 1: Angstroem exponents of 0.76-1.17, i.e., clearly larger than 0.6, indicate a quite undefined mixture of dust and anthropogenic pollution. And the selected Cyprus case of 15 November showed Angstroem exponents around 1. At such conditions, pollution dominates the optical properties of the aerosol. What can we learn from UAV observations at these complicated conditions ... with respect to the dust volume-to-extinction relationship?

Thank you for this comment. As also discussed in response to RC2.36, we have clarified throughout the manuscript (Lines 392-393, 416-417, 582-584, 712-714) that the 15 November 2021 Cyprus case does not represent a pure dust event, but rather a dust-pollution mixture, as indicated by the relatively high Ångström exponent and fine-mode contribution (Lines 416-417).

The case provides valuable insight into the behavior of the dust volume-to-extinction ratio (ζ) under mixed aerosol conditions. Such mixed dust conditions are frequently observed during Middle Eastern dust outbreaks, and the derived ζ values are therefore relevant for estimating dust contribution during such events. In addition, the UAV-based and AERONET-derived PSDs and ζ values showed good agreement, demonstrating that AERONET retrievals remain reliable even under fine dust and mixed dust conditions.

CC1.5: Page 13, Figure 3: One needs to show the PLDR instead of the VDR to get a good impression on the dust fraction. The Cyprus profiles tell us that the full layer from the surface to 2.5 km was well mixed and consists of dust and pollution aerosol, but the dust fraction remains unresolved when showing VDR.

Thank you for the comment. We agree, and as per RC2.22 we have added the PLDR profiles in Figure 3 in the revised manuscript.

CC1.6: Page 13, Figure 4(a): The Cyprus volume size distribution shows a pronounced fine mode (even in the volume size distribution). Thus, pollution dominates the optical properties. Figure 4(b) on the other hand (Cabo Verde, 24 June 2022) shows a typical dust size distribution with a typical 'left wing' of dust fine mode particles. The maximum of the coarse mode is at 2 micrometer, for me a reasonable value, and in full consistency with the observed Polly PLDR spectrum. AERONET observations in Tamanrasset (directly in the dust source region) often show maximum values around 5 micrometer. But here (24 June, Cabo Verde) such a maximum is not found, and thus giant particles were obviously absent according to the AERONET observations but derived from the in situ measurements. So, please expand the discussion on this point, especially regarding the limits of the UCASS observations expressed in terms of counting uncertainties for large to very large and giant particles.

Thank you for this comment. It must also be noted that the observations in this paper capturing very large and giant particles are not isolated. In addition to previous literature (see response to this community comment's introductory paragraph, where we mention amongst other the SAVEX-D results where a similar comparison of airborne in-situ and AERONET was made), similar evidence was found consistently across all UAV flights during the ASKOS campaign (preliminary analysis), suggesting the presence of super-coarse particles (as discussed in our answer to the introductory paragraph of this review).

We acknowledge that the detection of very large particles with UCASS is subject to uncertainties (all observations do have uncertainties) and we discuss and quantify them in section 4.3.2. Moreover, as noted in our response to GCC1, the extinction coefficients derived from UCASS showed good agreement with the Raman-lidar-retrieved extinction for the 24 June 2022 case, which provides additional confidence in these observations.

CC1.7: Page 20, Figure 9: As a reader, I have just to accept (or believe) the strong deviation between the size distribution derived from AERONET observations and derived from in situ observations. I have no option to check how trustworthy the in situ observations are. It would be desirable (in future) to model the dust optical properties (especially the backscatter coefficients and depolarization ratios at 355, 532, and 1064nm) with sophisticated optical models that are able to properly consider the dust size and shape characteristics. But such models are not available yet.

Thanks to the reviewer for the comment and suggestion. Readers are encouraged to interpret the results by taking the uncertainties into account (see section 4.3.2).

We agree with the reviewer on the importance of developing advanced optical models that can represent realistic dust particle sizes and shapes. Progress in this direction will require advanced three-dimensional shape observations and heavy computational power, to better constrain such models. We have added the suggestion of the reviewer in the conclusions (Lines 730-731).

CC1.8: Page 21: Figure 10(b) is then not a surprize … But is that the truth?

Thank you for the comment. Indeed, Figure 10 is a result of the high contribution from the coarse mode in the PSD and presence of giant particles. As mentioned before, this is not the first time giant particles have been observed in the Eastern Atlantic. However, when the observation of the giant particles is collocated with AERONET observations, we expose a methodological limit of the latter: hence we understand the surprise. This is however our result. As mentioned previously, this was not an isolated case but it was observed throughout the flights we performed during the campaign, and has been moreover reported by previous literature. For these reasons, we do not find reasons to reject the result.