With black text from the Reviewer1, and with *red coloring authors comments.*

Review of “Microphysical and Compositional Differences Between Saharan and Middle Eastern Dust Revealed by UAS Observations” by Kezoudi et al.

General comments

The manuscript describes ground-based remote sensing and airborne in situ measurements of dust particles above Cyprus. The manuscript reaches sound conclusions. However, the overall presentation of the results needs improvement. In its current form, the paper reads more like a measurement report than a scientific article. To my disappointment, there is limited integration of the remote-sensing and in situ observations; based on the abstract, I expected more blending of these complementary approaches. They both keep their supportive but distinct lines. The manuscript deserves publication, but major revision is necessary. Please find below my comments and suggestions for improving the manuscript.

We sincerely thank the reviewer for the careful evaluation of our manuscript and for the constructive comments. We appreciate your recognition that the study reaches sound conclusions and deserves publication.

We acknowledge your concern regarding the limited integration of the remote-sensing and in-situ observations. Our intention was not to strongly emphasize a tight blending of these approaches, but rather to present them as complementary measurements addressing the same dust events. To avoid creating misleading expectations, we have revised the abstract to better reflect the actual scope and focus of the study. The sentence “The campaign took place between 18/10/2021 and 18/11/2021 with continuous ground-based remote-sensing measurements, complementing 36 UAS flights.” is now rephrased to “The campaign was conducted from 18 October to 18 November 2021 and comprised 36 UAS flights.” (Lines 6-7).

We thank you again for your valuable suggestions, which have helped us improve the clarity and presentation of the manuscript.

There are several issues with indentation/formatting and inconsistent terminology (e.g., aerosol, dust, particles) throughout the manuscript.

We have carefully revised the manuscript to ensure consistent terminology regarding "aerosols," "dust," and "particles," while also correcting all indentation and formatting issues to maintain a uniform presentation.

The ordering of references in the text appears somewhat inconsistent. In some cases, references seem to be sorted by relevance, while in others they appear alphabetical and/or chronological. I suggest that the authors choose one consistent approach and apply it throughout the manuscript.

Thank you for the comment. Now the references are ordered in chronological order. See example at lines 43-44: "(Mona et al., 2012; Sugimoto and Zhongwei, 2014; Mamouri and Ansmann, 2015; Toledano et al., 2019)".

Specific comments

Abstact, there are acronyms that are used for the first time and are not expanded (GPAC, POPS, UCASS).

Thank you for the comment. They are now expanded in the abstract too.

Page 3, starting line 77, What OPCs were used (manufacturer and model)? The same applies to COBALDs. Similarly, for the remote-sensing instrumentation: lidars, ceilometers, and sun photometers come in different models with different features. Were the same instrument sets used at all ground stations (CAO-AMX, CAO-Nicosia, CARO-LIM NF)?

Thank you for this suggestion. The manuscript has been updated to include the specific manufacturers, numbers, and technical specifications for the instruments whose data are directly utilized in this study. This includes the OPCs, impactors, and relevant remote-sensing instrumentation (lidars, ceilometers, and sun photometers), supported by appropriate literature citations. See updated Sections 2.1 and 2.4.

Page 4, line 83, CAMS is an acronym, please expand.

Thank you. It is now expanded as Copernicus Atmosphere Monitoring Service (CAMS) in Line 296.

Page 4, line 85, HYSPLIT is an acronym, please expand and use appropriate reference (e.g. Stein et al., 2015).

Thank you. It is now expanded and citation added in Lines 299-300.

Page 4, line 85, authors use North Sahara (expanded) but later line 87 they use acronym ME, later they use NA (North Africa) not North Sahara on line 94.

We thank the reviewer for the comment. We have updated the revised version to use the acronym NA instead of 'North Sahara' throughout the paper where applicable.

Page 5, Table 1, there is a mixture of terms altitude (i.e. above sea level) and height (i.e. above ground) it is very confusing especially in remote sensing field. In the table, the term "Africa" is used for HYSPLIT source, should it be "North Africa" or the HYSPLIT analysis source is nonspecific?

i) *All altitudes are expressed in kilometers ASL; this effectively clarifies the measurements presented (clarified in the legend). Also, to avoid confusion, we have renamed the "BL height" to "BL top" in Table 1.*

ii) Corrected to NA.

Page 5, line 96, the sentence starting with "Both UAS belong..." sounds akward, did you mean something like: "Both UAS are fixed-wing aircraft, primarily made of foam with plywood reinforcements."?

Thank you for the feedback. It is now corrected to what suggested by the reviewer (Lines 116-117).

Page 5, line 99, "Relative Humidity" should be lower case, unless you are introducing a variable (Relative Humidity (RH)).

It is corrected to lower case (Line 123).

Page 5, line 96, The paragraph as a whole feels superficial for a scientific paper. Even though a reference is provided (Kezoudi et al., 2021a), it would still be helpful to include key details such as aircraft weight and wingspan, sensor manufacturers, operating ranges, and (ideally) calibration procedures and uncertainties.

Details added in this paragraph where the UAS are described (Lines 117-120) as following: "The I-SOAR has a wingspan of 2.5m, a Maximum Take-Off Weight (MTOW) of 6.5kg, can carry a payload of up to 2kg, and is capable of flying for up to 110 minutes. The Skywalker-2015 features a 1.83m wingspan, a MTOW of 4.2kg and is capable of carrying a payload of up to 1.5kg, with a flight endurance of 90 minutes.".

In the revised paper, sensor manufacturers, operating ranges, calibration procedures and uncertainties are discussed in Sections (2.4.1 and 2.4.2).

Page 6, Section 2.3 The authors omitted a description of the sampling strategy. Did they use data from both ascent and descent? Were the flights helical or linear? Was remote sensing used to plan the flight strategy, and if so, how?

Thank you for the comment. The flight strategy is now described in the text in Lines 125-131: "The flight pattern consisted of linear vertical profiles, with the aircraft climbing and descending along segments of 1.5–2 km at rates of 2–3 m/s. Flight duration typically ranged from 50 to 80 minutes, primarily constrained by battery capacity and weather conditions. The UAS missions aimed to obtain high-resolution vertical profiles through coordinated ascents and descents between waypoints, with representative profiles derived by averaging data from both phases. The flight strategy was guided by high-resolution, near-real-time lidar observations (Section 2.1.1), which were used to identify the presence and vertical extent of dust layers before and during operations, enabling optimized flight timing and coordination, and defining the altitudes at which the impactors were deployed to ensure effective in situ sampling of the dust layers."

Page 6, line 106, uncomplete reference (POPS; et al., 2016)

Thank you, reference is now updated in line 136.

Page 6, line 110, the flow rate unit is wrong, probably should be cubic centimeters per second, about 0.18 l/min.

Thank you. Corrected to cm^3/s (Line 141).

The sentence: "POPS is able to accurately measure ..." sounds very optimistic, the size range 0.1 to 3.4 um too, please check Pilz et al., 2022 and Pohorsky et al., 2024 for details on actual calibration of POPS.

Sentence changed to: "POPS measures the particle number size distributions in an optical size range of 0.13–3 μm at 1Hz resolution." (Line 143)

Page 6, line 116, double parentheses after reference. *Corrected*

Page 6, line 117, the sentence beginning "Computational Fluid Dynamics (CFD)..." in the context of Girdwood et al. (2022) is misleading. No CFD simulation was conducted for the underwing setup, but only for the Talon top-nose setup. It would be better to base the argument on the angle of attack (AoA): if the AoA in the underwing UCASS setup remains within the recommended range, then it should be acceptable.

Corrected to: "Computational Fluid Dynamics (CFD) simulations confirm that integrating the UCASS beneath the wings enables its airflow measurements to align with those obtained from the UAS nose-mounted pitot tube for an angle-of-attack range of ±10° (Girdwood et al., 2022)". (Lines 150-152)

Page 6, line 130, (i) when combining the size distributions from both instruments (POPS and UCASS), it is unclear what equivalent diameters were used. POPS was calibrated with PSL spheres, whereas UCASS was calibrated with dust particles of different refractive index. Was the POPS size range recalculated to dust-equivalent diameters using the same refractive index as UCASS? If yes, how? Please clarify. (ii) Also, in Figure 1b the size distributions were merged; please describe the merging procedure. Were the overlapping bins and counts simply averaged, or did the authors use a more sophisticated method?

(i) We thank the reviewer for raising this point. The POPS size range was not recalculated to dust-equivalent diameters when combining it with UCASS measurements. As clarified in the revised manuscript (Section 2.4.2: OPC calibration and associated uncertainties) and Lines 206-215, we explicitly assessed the impact of refractive index differences between the PSL-based POPS calibration and dust conditions using Mie scattering calculations at the POPS operating wavelength. Assuming a dust-representative refractive index (m = 1.52 + 0.002i), the resulting diameter adjustment is approximately 4%. In absolute terms, this corresponds to shifts of <0.05 μm for particle diameters below 1.22 μm and up to ~0.1 μm for larger particles. These differences are small relative to the POPS size bin widths (>10%) and therefore do not significantly affect the combined size distributions. For this reason, no correction was applied. Given the minor magnitude of this adjustment and its negligible impact on the derived size distributions and integrated quantities, a full recalculation of the POPS size bins to dust-equivalent diameters was not applied."

We have now clarified this point explicitly in the revised manuscript to ensure transparency and consistency in the combined use of POPS and UCASS data.

(ii) This is mentioned in Lines 164-167 with the below text: "To achieve a complete particle size distribution for both POPS and UCASS, the size bins of the two instruments were combined. For the size range between 0.1 and 2.3 μm, POPS data were solely utilized in the analysis. For particle sizes exceeding 2.3 μm, UCASS data were employed. This cutoff diameter was chosen based on prior research that indicated possible artifacts of size around 2.2μm of ambient measurements within this size range when using UCASS (Kezoudi, 2020).".

Page 7, line 161, the sentence "In principle, there is no upper cut-off…", followed by "However, the upper limit is mainly limited…", is confusing. Is there an upper limit or not? Please clarify.

Clarified and corrected to: "In principle, there is no strict upper cut-off diameter for dust collection on the adhesive substrate. In practice, however, the effective limit is set by the sticking efficiency, which decreases for larger particles due to their higher inertia and tendency to rebound." (Lines 246-248)

Page 9, line 171, The authors describe using multiple-substrate GPaC systems for sampling multiple atmospheric layers. From the current text, it is unclear how the second layer is identified during flight in order to deploy the second substrate. Additionally, there is a general discussion of sampling duration; could the authors provide basic statistics on successful samples (16 out of 22)?

Thank you, relevant text was added (Lines 246-249) to clarify this topic: "During flight, the identification of distinct atmospheric layers and the timing of the opening and closing of the two impactors were guided by real-time lidar observations, which allowed for the detection of dust layers, and were further supported by the vertical structure obtained from UAS profiling flights conducted earlier on the same day, when available.".

Regarding the sampling duration of each sample is given in Table 2; additional text is added in Lines 459-460 using basic statistics as requested: "The average sampling duration of the successful samples was 238±147 seconds.".

Page 11, Section 2.4, I suggest moving/merging this section into Section 2.1. This would address my comments regarding Section 2.1 and improve clarity. The same applies to Section 2.5.

Thank you for this suggestion. The specified sections have been moved accordingly, resulting in a more cohesive and streamlined presentation of the text.

Page 12, Figure 4, it is unclear how the authors arrived at these specific end points. On which estimates are the end points based?

Figure 1 caption is now clarified as follows: "HYSPLIT backward trajectories used for the sensitivity analysis for two dust events on 31/10/2021 (13:00 UTC, arriving at 2.5 km ASL) and 15/11/2021 (13:00 UTC, arriving at 2.0 and 2.5 km ASL) over Orounda. Multiple *trajectories were initiated from the same endpoint using small spatial offsets in the meteorological grid to assess variability. These cases are representative examples of the dust events analyzed in this study."*

Page 13, line 254, please be specific which Lidar you are talking about on the first occasion you mention it. This comment holds for any other instrument thought the whole text, please be specific.

It is mentioned in the following sentence (Line 321): "This is derived from the height-resolved observations of the Range Corrected Signal (RCS) in the green channel and volume depolarization ratio recorded by the CE376 Cimel aerosol lidar".

This is addressed for all the instruments mentioned in the text.

Page 16, starting line 287, the following paragraphs mix the terms "height" and "altitude." Furthermore, Figure 9 uses the unusual combination "height ASL." Typically, "altitude" is referenced to sea level, and "height" is referenced to ground level. This is not necessarily incorrect, but it is confusing—especially when the terms are used inconsistently throughout the text.

Thank you for noting this. Now the term "altitude" is used throughout the paper.

Page 16, line 298, above this line authors use kilometers, here they changed to meters, please be consistent.

Corrected to km (Line 403).

Page 17, line 300, there is no Table 2.1, probably typo.

Thank you. Corrected to Table 1 (Line 405).

Page 17, lines 309-311, the authors use the terms "coarse aerosol" and "coarse dust particles." Does this reflect different coarse-mode composition for NA and ME? "Coarse dust particles" is a specific aerosol type, whereas "coarse aerosol" may include dust, sea salt, pollen, etc.

To ensure scientific accuracy and consistency, we have revised the manuscript to distinguish between size-based and composition-based descriptions.

Specifically, we now use the term "coarse-mode particles" when referring to the general size distribution or the physical distinction between fine and coarse modes (e.g. Lines 408-409: "A clear separation between fine and coarse particles is evident around 0.7μm"). The term "dust particles" is reserved strictly for instances where the chemical or optical properties specifically identify the mineral dust component of the aerosol (e.g. Lines 503-504: "This study demonstrates the effectiveness of a novel, cost-efficient methodology for quantitative characterization of airborne dust particles using a sensor package that integrates OPCs and impactors deployed on UAS.")

Page 19, Figure 11, the lognormal fits appear to be far off (up to 5 orders of magnitude for the ME data) on the right-hand side of the size distribution. Is there an explanation for this?

Were the fits applied to all data points, or only to averaged distributions? Could the authors include uncertainty estimates (e.g., error bars) for the averaged data points?
We thank the reviewer for the comment. The lognormal fits were consistently applied to the averaged size distribution data; however, in response to the reviewer's suggestion, we have now refined the fitting procedure. By identifying and removing specific outliers that previously skewed the right-hand tail of the distribution, the revised fits provide a significantly more accurate representation of the measured data. Error bars are added in the revised figure. The updated fits and error bars are now shown in Figure 13.

If I understand correctly, the authors used averaged number/volume distributions (Figures 10, 11, and 13) for the OPC/GPaC flights, whereas Figure 9 shows clearly stratified layers (in mass concentration). On what basis do the authors assume that layers (e.g., at 0.5 km and 2 km ASL) are of the same type? This also seems at odds with Section 4, where the authors claim that the method "enables high-resolution vertical profiling." Why is such resolution needed if the analysis averages over the whole dust layer?
The averaged number and volume size distributions correspond to layers that were first identified from the vertical OPC profiles. The altitude ranges used for averaging were therefore based on the stratified structures visible in these profiles. We assume the layers are of the same type based on a synergistic identification and validation approach. While the layers appeared at different altitudes (e.g., 0.5 km and 2 km ASL), they both exhibited volume depolarization ratios > 0.1, which consistently indicates the presence of non-spherical mineral dust. This lidar-based classification was further corroborated by in-situ chemical and morphological analysis of the impactor samples collected at both altitudes, confirming their shared mineralogical characteristics (Lines 129–132).
Regarding the altitude range selected for opening and closing the impactor, this was determined using real-time lidar quicklook data and, when available, profiles from flights conducted within the previous hour (please see Lines 129-131). This allowed us to identify the vertical boundaries of the dust layer and target the appropriate sampling altitudes. The high vertical resolution of the measurements is therefore essential for detecting these layers and defining the altitude intervals used for the subsequent averaging and analysis.
Page 22, Figure 13, could the authors add standard deviations (as error bars) to the figure?
Thank you for the suggestion. Standard deviations are now added. Figure 13 is now Figure 15 in the updated manuscript.
Page 22, line 377, the authors introduced the terms "Cyprus cases" and "Cyprus samples", first I had impression they were trying to build an argument that collected NA and ME samples are very distinct from "Cyprus samples" and the NA and ME samples are not contaminated from local sources (Cyprus samples). I was wrong. Please, try to be

consistent if you introduce certain terms through the manuscript and keep using the same terms.

Thank you for highlighting this potential point of confusion. We appreciate the opportunity to clarify that these labels denote collection sites rather than geochemical origins. In accordance with your suggestion, we have revised the text to ensure consistent terminology and improved clarity for the reader. Text (Lines 483-492) corrected to: "The NA and ME samples collected over Cyprus can be differentiated from the other source regions in the Ca-Al-Mg and K-Ca-Fe ternary plots. In the Ca–Al–Mg ternary diagram, dust of ME origin observed over Cyprus exhibits a distinct shift toward the Ca-rich vertex, whereas NA samples align more closely with previously reported Saharan measurements (Kandler et al. 2020), where the same analytical procedure was applied. This pattern likely reflects intrinsic differences in the mineralogical composition and soil structure of the respective source regions. A similar trend is observed in the K–Fe–Ca ternary diagram, where Ca enrichment again serves as a distinguishing feature between the sources; however, the Saharan samples collected during the campaign exhibit relatively higher Fe and lower K contents compared to previously reported Saharan dust. Overall, both NA and ME samples collected over Cyprus show slightly lower inter-particle variability than those from other regions, as indicated by the reduced extent of the statistical confidence envelope."

References

- Pilz, C., Düsing, S., Wehner, B., Müller, T., Siebert, H., Voigtländer, J., and Lonardi, M.: CAMP: an instrumented platform for balloon-borne aerosol particle studies in the lower atmosphere, Atmos. Meas. Tech., 15, 6889–6905, https://doi.org/10.5194/amt-15-6889-2022, 2022.
- Pohorsky, R., Baccarini, A., Tolu, J., Winkel, L. H. E., and Schmale, J.: Modular Multiplatform Compatible Air Measurement System (MoMuCAMS): a new modular platform for boundary layer aerosol and trace gas vertical measurements in extreme environments, Atmos. Meas. Tech., 17, 731–754, https://doi.org/10.5194/amt-17-731-2024, 2024.
- Stein, A.F., Draxler, R.R., Rolph, G.D., Stunder, B.J.B., Cohen, M.D., & Ngan, F. (2015). NOAA's HYSPLIT atmospheric transport and dispersion modeling system. Bulletin of the American Meteorological Society, 96(12), 2059-2077

We thank the reviewer for the suggested citations; these have been studied and integrated into the updated text (Lines 143-144, 288) and reference list.