Reviewer 1

General Assessment

This cross-institutional study presents, according to the authors, the first combination of UAV-mounted free-stream impactor measurements and a regional mineralogical dust model (METAL-WRF) to characterize the elemental and mineralogical composition of Saharan dust during atmospheric transport at multiple altitudes. The observational dataset: 14 analyzed cases, approximately 26 000 individual particles characterized by SEM-EDX, supported by AERONET retrievals, HYSPLIT and FLEXPART-WRF trajectory analyses, and satellite imagery represents a significant field effort. The scientific questions are well-motivated, and the multi-platform observational strategy is commendable. The topic is of clear relevance to dust radiative forcing, ocean biogeochemistry, and cloud microphysics. However, the manuscript requires substantial revision before it can be accepted. Several methodological limitations that directly affect the discussion results are not adequately addressed or quantified. The narrative is at times unfocused, attempting to advance too many sub-arguments simultaneously without a single clear thread connecting them.

Some concerns are discussed and the specific comments are listed below with clear reference to lines in the manuscript.

Comments by section

Abstract

Reviewer comment:

The abstract reads as somewhat disconnected, listing results without a clear narrative arc. The authors should ensure the abstract communicates: (1) the specific gap being addressed; (2) what was done; (3) the key quantitative findings; and (4) the principal conclusion and its implications. As currently written, the transition between the mineralogical results and the model comparison feels abrupt.

Author response:

We thank the reviewer for this helpful comment. We have revised the abstract to improve its overall structure and coherence. In particular, we have aimed to more clearly present the scientific gap it addresses, the methodology, the key findings and the main conclusions.

Introduction

Reviewer comment:

Lines 33–34: The term 'distinct functions' is imprecise. Replace with 'distinct roles in atmospheric processes' and be specific about which properties are relevant in the context of this paper.

Author response:

We thank the reviewer for this comment. We have replaced the expression "distinct functions" with "distinct roles in atmospheric processes" and revised the paragraph to identify the elemental and mineralogical composition of desert dust as the main focus of this study, while highlighting how these properties can help constrain the representation of dust composition in atmospheric models.

Reviewer comment:

Lines 36–37: The use of 'milling' as a dust production process is ambiguous and potentially misleading in the context of aeolian dust. 'Milling' might refer to an industrial or laboratory process. If the cited studies used laboratory-milled mineral samples, this must be stated explicitly. Particles analyzed in the laboratory can have different surface properties from naturally produced aeolian dust, and generalizing those findings to atmospheric dust without acknowledging this distinction can be misleading.

Author response:

We thank the reviewer for this comment. Upon re-examining Gustafsson et al. (2005), we confirmed that the study investigated the water-uptake properties of commercially supplied mineral samples under laboratory conditions, without explicitly reporting milling as part of the sample preparation. We therefore removed this term and revised the sentence to emphasize the influence of mineral-specific chemical composition, surface properties, and particle morphology on interactions with atmospheric water.

Reviewer comment:

Lines 46–48: This is an opportunity to expand on the regional and local characteristics that drive differences in dust properties, since source-region variability is precisely what this manuscript addresses. Other regional and global modelling efforts, and other soil mineralogy atlases beyond those currently cited, should be acknowledged here. Other soil map atlases should be mentioned in this context.

Author response:

We thank the reviewer for this comment. The Introduction has been substantially expanded to discuss how regional differences in geology, soil characteristics, and weathering conditions, together with local emission processes and atmospheric transport, influence the mineralogical

composition of desert dust. Furthermore, additional regional and global mineral-resolved modeling efforts have been included, along with a broader discussion of soil mineralogy datasets.

Reviewer comment:

Line 72-75: The citation list supporting the statement that model validation 'has been primarily done using ground-level in-situ measurements of mineralogical composition' could be expanded. The authors should cite Nickovic et al. (2013), Ilic et al. (2022), Mifka et al. (2022), and Gonçalves Ageitos et al. (2023), which represent directly relevant regional and global modelling efforts addressing dust mineralogy.

Author response:

We thank the reviewer for these additional references. The original statement has been revised to include the suggested studies.

Reviewer comment:

Lines 74–78: The claim of being the 'first time worldwide' that such a combination of measurements and models has been attempted is likely correct but should be reframed to emphasize the scientific value of the findings. What insights does this first-of-kind combination provide that were not previously possible? The novelty claim is more compelling when tied to its scientific consequences.

Author response:

We thank the reviewer for this comment. The original statement has been removed, and the revised Introduction focuses on the value of combining in-situ measurements of transported dust composition with mineral-resolved model simulations.

Reviewer comment:

Missing references: The introduction would benefit from citing the Barreto et al. African dust campaigns and the Journet et al. work on dust mineralogy, as well as the EMIT imaging spectrometer programme (which the authors mention in the conclusions as future work but do not introduce in the introduction where it would provide context for why satellite-based mineralogy constraints are needed).

Author response:

We thank the reviewer for highlighting these relevant studies. The Introduction has been expanded to provide a broader overview of previous investigations of African desert dust and to include the soil mineralogy dataset developed by Journet et al. (2014). Furthermore, a discussion of the EMIT imaging spectrometer has been added, highlighting its potential to provide improved

constraints on source-region mineralogy. Regarding Barreto et al. (2022a, b), we acknowledge the importance of these studies in characterizing the vertical structure of the Saharan Air Layer and the optical and microphysical properties of atmospheric aerosols. However, as these studies do not specifically investigate the mineralogical or elemental composition of desert dust, we have chosen to focus the corresponding discussion on studies more directly related to the compositional aspects addressed in this manuscript.

Free-stream impactor analysis

Reviewer comment:

Line 123: 'Particle areas' is ambiguous. This likely refers to the projected cross-sectional areas of particles on the substrate, not to particle surface area. Clearer phrasing is needed to avoid confusion.

Author response:
We thank the reviewer for pointing this out this. We have revised the wording to “projected cross-sectional areas of particles on the substrate” to clarify the meaning.

Reviewer comment:

Line 127: 'Lower Z than oxygen' is informal lab shorthand and should not appear in a journal manuscript. Write, for example, 'elements with atomic number lower than that of oxygen' and define Z on first use.

Author response:

We thank the reviewer for this suggestion. We have replaced the informal expression “lower Z than oxygen” with “elements with atomic numbers lower than that of oxygen” in the revised manuscript.

Reviewer comment:

Line 131: The stoichiometric assumption that iron is present exclusively as Fe^{3+} (Fe_2O_3) is introduced without justification or reference. In freshly emitted Saharan dust, a non-negligible fraction of iron occurs as Fe^{2+} in silicate mineral matrices (illite, biotite, amphiboles). The sensitivity of the derived elemental mass fractions to this oxidation state assumption should be noted, or a justification provided for why Fe^{3+} is the appropriate assumption for transported dust.

Author response:

We thank the reviewer for this important comment and agree that the original assumption regarding the oxidation state of Fe required further justification. We acknowledge that Fe can occur in both Fe^{2+} and Fe^{3+} oxidation states in mineral dust, including as structural Fe in

aluminosilicate minerals. However, previous studies have shown that Fe^{3+} is generally the dominant oxidation state in transported African dust. In particular, Deboudt et al. (2012) reported an average Fe^{3+} contribution of approximately 77% in Fe-bearing aluminosilicate matrices and 90% in iron-oxide inclusions. Furthermore, Formenti et al. (2014) identified hematite and goethite, both containing Fe^{3+}, as the principal iron-oxide minerals in West African dust.

To address the reviewer's concern, we have revised the assumption used for the stoichiometric conversion of Fe. Instead of considering Fe exclusively as Fe^{3+} (Fe_2O_3), we now adopt a mixed oxidation-state representation, assuming 80% Fe^{3+} as Fe_2O_3 and 20% Fe^{2+} as FeO. This provides a more realistic representation of the Fe oxidation states reported for transported African dust while accounting for the presence of ferrous iron.

To further assess the impact of this assumption on the derived elemental mass fractions, we performed a sensitivity analysis comparing an FeO-only representation with the adopted mixed oxidation-state representation (80% Fe^{3+} as Fe_2O_3 and 20% Fe^{2+} as FeO). The resulting Fe mass fractions were 5.36% and 5.31%, respectively, corresponding to a relative difference of less than 1%. This indicates that the derived Fe mass fractions are only weakly sensitive to the assumed iron oxidation state. We have also included this finding in the discussion of uncertainties in the Conclusions section of the revised manuscript.

Revised manuscript:

Chemical elements present in atmospheric dust particles typically occur in the form of oxides. To estimate the mass fraction of each element in the sample, the detected elements were converted to oxide-equivalent masses using assumed stoichiometric compositions. For Fe, a mixed oxidation-state representation was adopted, assuming 80% Fe^{3+} (Fe_2O_3 equivalent) and 20% Fe^{2+} (FeO equivalent), consistent with the predominance of ferric iron reported in transported African dust (Deboudt et al., 2012; Formenti et al., 2014). The mass of each element was then calculated by multiplying the estimated particle mass by the ratio of its elemental mass contribution to the total reconstructed chemical mass, including the associated oxygen. The total particle mass is estimated from its cross section visible in the microscope (Kandler et al., 2018), yielding a volume and an average density for the different mineral groups of 2.65 g cm^{-3} (Kandler et al., 2007), consistent with the value reported for dust by Hess et al., 1998.

Reviewer comment:

Line 134: What are the implications of using a common average density of a mineral group in this method? The goal is to understand the composition of a particle and some minerals, namely hematite and goethite have larger density than for example quartz. This should be clarified or justified in the manuscript.

Author response:

We thank the reviewer for this comment. We acknowledge that individual dust minerals have different densities, with Fe oxides and hydroxides, such as hematite and goethite, exhibiting higher densities than common silicate minerals. However, since Fe oxides and hydroxides generally represent a minor fraction of the mineralogical composition in our samples, their influence on the overall mass estimates is expected to be limited. We have clarified this assumption in the revised manuscript.

Reviewer comment:

Lines 140: What are the classification uncertainties of the method and how do they affect the discussion?

Author response:

We thank the reviewer for this comment. To quantify the classification uncertainty associated with the assumed representative mineral compositions, we performed a sensitivity analysis in which alternative elemental compositions were considered for kaolinite (2 alternatives), illite/muscovite (3), smectite (3), and feldspar (2), while the representative compositions of the remaining mineral groups were kept fixed. The full-factorial combination of these alternatives resulted in 36 mineral-composition configurations. Each configuration was applied using the same classification procedure and, to ensure comparability among the sensitivity runs, the analysis was subsequently performed using a common particle population.

For each configuration, the mean mineral volume fractions were calculated for the 14 selected samples. The resulting distribution of the 36 mean volume fractions for each mineral group was summarized using percentile ranges. The central estimates of the mineral volume fractions remained relatively close to those obtained from the original classification, but the sensitivity analysis revealed substantial uncertainty in the partitioning among the clay-mineral groups, particularly illite/muscovite, kaolinite and smectite, with feldspar showing the next largest variability. In contrast, quartz, calcite and Fe-oxide/Fe-hydroxide remained essentially unchanged, while gypsum showed only limited variability. The detailed methodology and results are provided in Sect. S1 and Table S2 of the Supplement.

Importantly, the sensitivity analysis did not alter the main interpretation of the individual source-attribution cases. The samples identified as having the strongest Fe-oxide/Fe-hydroxide and calcite/gypsum enhancements remained consistently distinct across the sensitivity ensemble. This was expected to some extent because the representative compositions of these mineral groups were kept fixed in the sensitivity analysis, although their classified contributions could still change indirectly as the reference compositions of the other mineral groups were varied.

We have therefore added the sensitivity analysis to quantify the uncertainty associated with the assumed reference compositions, updated the reported mineral volume fractions accordingly, and provided the detailed methodology and results in Sect. S1 of the Supplement.

Changes made in the manuscript:

A new Sect. S1 has been added to the Supplement describing the classification-sensitivity methodology, the alternative representative mineral compositions, the common particle-population approach, and the resulting percentile ranges.

A brief description of the sensitivity analysis has been added to Sect. 2.2.2 of the manuscript, and the reported mineral volume fractions have been updated in the Abstract and Sect. 3.2 to reflect the 36-configuration sensitivity analysis.

The discussion of the selected source-attribution cases has also been revised to explicitly account for their robustness across the mineral-composition sensitivity ensemble.

In addition, the elemental-composition comparison shown in Fig. 11, 12 and A2 (previously Fig. 9,10 and A4) has been updated following the revised mineral-to-element conversion and the use of the common particle population. These changes resulted in small corresponding changes in the measured and modeled elemental percentages.

Model description

Reviewer comment:

The model description does not specify an upper particle size threshold corresponding to the 25 µm threshold applied in the observational analysis. For a meaningful model–measurement comparison, this should be clarified.

Author response:

Thank you for this comment. The upper particle diameter limit in METAL-WRF is 20 µm. Following the reviewer's suggestion, we revised the observational analysis to apply the same 20 µm threshold when comparing elemental mass fractions derived from SEM–EDX measurements with model simulations, ensuring consistency in the particle size ranges considered.

For the separate mineralogical composition analysis, the 25 µm threshold was retained to include a broader range of the observed particles, as this analysis does not involve a direct comparison with the model.

Reviewer comment:

FLEXPART-WRF is introduced as a separate modelling framework from METAL-WRF, running at a slightly different horizontal resolution (20 km vs. 22.5 km). The rationale for this separate

configuration should be explained. Was it technically possible to run FLEXPART as a module within METAL-WRF?

Author response:

We thank the reviewer for this comment. The FLEXPART-WRF simulations were driven by the operational forecasting system used during the ASKOS campaign, whereas the METAL-WRF simulations were performed retrospectively after the campaign. Although it is technically possible to run FLEXPART as a module within METAL-WRF, implementing such a configuration was beyond the scope of the present study.

The difference in horizontal resolution (20 km for FLEXPART-WRF and 22.5 km for METAL-WRF) is relatively small, and we do not expect it to result in significant differences in the meteorological fields relevant to this study.

Reviewer comment:

Line 168: The appendix table referenced in the text as Table A1 is labeled Table A2 in the Appendix. This must be corrected throughout.

Author response:

Thank you for pointing this out. The table numbering and corresponding references have been corrected throughout the revised manuscript.

Reviewer comment:

The manuscript does not discuss the uncertainty introduced by the choice of soil mineralogy atlas used in METAL-WRF. Given that the implementation of EMIT atlas is planned as future work, can discussion of the currently available soil mineralogy data sets (Claquin/Nickovic, Journet and EMIT) and how they relate to possible active source area during the campaign can be included in the discussion of the results. Furthermore, it could contribute to the Fe\Al ratio discussion.

Author response:

We thank the reviewer for this comment. The modeling uncertainty cannot be directly assessed in the present study, as the current version of METAL-WRF relies exclusively on the GMINER30 and FERRUM30 databases for soil mineralogy. We are currently developing a new version of METAL-WRF that also incorporates the EMIT database. However, a direct comparison between the two model versions will be the subject of a forthcoming study. However, this is an interesting comment, and therefore we present here some preliminary comparisons between the different soil mineralogy data sets.

Following the reviewer's suggestion, we added a comparison of the soil mineralogy represented by Claquin et al. (1999), including the revisions introduced by Nickovic et al. (2012), hereafter referred to as C1999-SMA, with that of Journet et al. (2014), hereafter referred to as J2014-SMA, and the EMIT Level-3 mineral mass fraction product (Brodrick et al., 2025). This comparison was performed to provide an estimate of the uncertainty associated with the prescribed source mineralogy. The analysis was further expanded to include the corresponding Fe/Al ratio maps. This additional analysis is now presented in Section 2 of the Supplement.

The Fe/Al ratios derived from these soil mineralogy maps differ from those calculated by METAL-WRF. In the revised manuscript, we have also clarified that the better agreement between modeled and measured Fe mass fractions, compared with the underestimation of Si, Al, and Mg, may be partly attributed to differences in how these elements are represented in METAL-WRF. The model derives its mineralogical composition from the GMINER30 database, which does not explicitly represent certain mineral groups, such as vermiculite, chlorite, and mica, that can contribute to the Si, Al, and Mg content of desert dust (see Table 3 in Menut et al., 2020). Their omission may therefore contribute to the underestimation of these elemental fractions. In contrast, the Fe mass fraction is derived from the FERRUM30 database (Nickovic et al., 2012, 2013), which accounts for iron incorporated into aluminosilicate minerals, as well as iron associated with oxides and hydroxides, including hematite and goethite. This broader representation of Fe-bearing mineral phases may partly explain the better agreement between modeled and measured Fe mass fractions.

The corresponding discussion has been added to the Results section of the revised manuscript.

Finally, K and Mg have also been included in the revised plots for completeness.

Reviewer comment:

Backward transport analysis The FLEXPART-WRF emission sensitivity fields indicate regions where surface emissions would most likely have contributed to the observed aerosol population, but they do not confirm that emissions actually occurred there at the relevant time. The satellite provides qualitative support but is itself subject to retrieval uncertainties and detection thresholds. The convergence between trajectory sensitivity and satellite dust signal is largely qualitative and the paper presents it as more confirmatory than the methodology warrants. The authors should temper the language around source attribution.

Author response:

We thank the reviewer for this comment. We agree that the agreement between the trajectory sensitivity analysis and the satellite dust observations provides supportive rather than definitive evidence for source attribution. We have therefore revised the manuscript to temper the corresponding statements.

Reviewer comment:

Lines 177–178: The sentence introducing the satellite constraint is ambiguous. The authors should restructure: one sentence stating the motivation and type of observational constraint being introduced, followed by a second sentence identifying the specific satellite products used. The current version buries the key information inside a long clause.

Author response:

We thank the reviewer for this suggestion. The text has been revised to clearly separate the motivation for using satellite observations from the description of the specific satellite product employed.

Atmospheric conditions

Reviewer comment:

The subsection title 'Atmospheric Conditions over the analyzed period' implies a discussion of meteorological conditions, but the section begins immediately with the number of selected data points. There is no characterization of the synoptic conditions during the campaign, no assessment of whether June 2022 was a typical or anomalous dust month relative to regional climatology, and no classification of the sampled cases by synoptic regime, trajectory cluster, or aerosol load category. Without this context the reader cannot judge whether the 14 cases are representative of the broader transport regime or constitute a potentially unrepresentative sample.

Author response:

We thank the reviewer for this valuable comment and agree that the original section did not provide sufficient context regarding the atmospheric conditions during the campaign and the representativeness of the selected cases. To address this concern, we have expanded the analysis to include: (1) a characterization of the synoptic conditions during June 2022 and a classification of the 14 sampled cases according to the prevailing synoptic regimes; (2) a comparison of the June 2022 atmospheric circulation with the 1991–2020 climatology; and (3) an assessment of the June 2022 dust load relative to the regional dust climatology.

These analyses are now presented in the newly added Section S3 of the Supplement. We have also revised Section 3.1 of the main manuscript to summarize the key findings and added two columns to Table 1 to provide further information on the atmospheric conditions associated with each case.

Regarding the suggested trajectory cluster analysis, the limited number of sampled cases (14) would not allow for a sufficiently robust statistical classification. We therefore opted for a case-by-case trajectory analysis, as presented in Figure 3 of the revised manuscript.

Lidar measurements

Reviewer comment:

The campaign included continuous lidar observations providing vertically resolved aerosol information, which is used in the present manuscript solely for cloud screening purposes (Table 2). No comparison between the modelled vertical structure of the dust layer and the lidar-derived aerosol profiles is presented. Given that METAL-WRF provides dust concentrations at multiple vertical levels and that the in-situ measurements are point samples at specific altitudes within the SAL, the absence of a vertical model evaluation represents a significant gap.

Author response:

We thank the reviewer for this comment. To answer this concern we have compared all available matching METAL-WRF and POLIPHON-derived dust profiles between 0.5 and 6 km a.s.l. The POLIPHON-derived dust mass concentrations were vertically averaged within atmospheric layers defined by the METAL-WRF vertical grid and compared with the corresponding modeled concentrations. Overall, METAL-WRF exhibits a positive concentration bias (thickness-weighted normalized mean bias of +59%; root mean square error of 132 μg m^{-3}). The modeled dust layer is also displaced downward on average, with its mass-weighted centroid approximately 0.42 km below the POLIPHON-derived centroid.

However, such a detailed comparison between the modeled SAL structure and the current model configuration is not in the scope of this study. This study aims mostly to investigate the relative elemental mass fractions of key elements as detected at the UAV impactors and described in the model. A more accurate analysis of the SAL requires a more advanced modeling configuration. The current model simulations use a horizontal grid spacing of 22.5 × 22.5 km and 32 vertical levels. This configuration is relatively coarse for explicitly representing non-hydrostatic motions. Also, additional vertical levels and finer vertical resolution within the Saharan Air Layer should be used to improve the representation of dust gradients and layer placement. We plan to conduct such an assessment for selected case studies in a future work, using finer model configuration and testing relevant parameterizations.

Reviewer comment:

Line 188: 'Consistent dataset' is undefined. What criteria determine consistency here? This should be stated explicitly.

Author response:

We thank the reviewer for this comment. For a few flights, some information regarding the sampling time or altitude was missing. By "consistent dataset," we meant a dataset for which all the necessary information was available. However, since only a few flights were affected, we have decided to omit this phrase from the revised manuscript.

Reviewer comment:

Table 2: The relevance of the lidar-based cloud screening column is not explained in the text. Why does cloud presence matter for the interpretation of the impactor samples? This should be clarified, either in the table caption or the surrounding text.

Author response:

We thank the reviewer for this comment. Upon reconsideration, we concluded that the lidar-based cloud screening information was not essential for the interpretation of the impactor samples in the present analysis. Therefore, we have removed this column from Table 1 (previously Table 2) in the revised manuscript to avoid potential confusion.

Reviewer comment:

Lines 200–213: The discussion of synoptic and local weather and aerosol conditions appears suddenly and without connection to Table 2, which is not referred to in this passage. The subsection needs restructuring so that the meteorological context, the case selection criteria, and the aerosol characterization flow logically.

Author response:

We thank the reviewer for this comment. The subsection has been revised and restructured to improve the presentation of the synoptic and local meteorological conditions and to provide a clearer connection between the meteorological context, case selection, and aerosol characterization.

Reviewer comment:

Line 205: 'Dust-related objectives' is referenced before they have been formally stated. Either state them here or refer to where they are defined.

Author response:

We thank the reviewer for pointing this out. The term “dust-related objectives” was intended as a general reference rather than to specific objectives defined elsewhere in the manuscript. To avoid confusion, we have removed this phrase from the revised manuscript.

Reviewer comment:

Line 211: 'Diverse range of aerosol scenarios' is vague. These scenarios should be classified as suggested above so that the reader understands the variability captured by the dataset.

Author response:

We thank the reviewer for this comment. The revised manuscript provides a clearer description of the aerosol conditions represented by the selected cases.

In-situ mineralogical composition

Reviewer comment:

Line 227 paragraph: It should be stated explicitly that the reported average mineralogical fractions are average across all 14 cases shown in Figure 4. This is currently assumed but not stated.

Author response:

We thank the reviewer for this comment. The text has been revised to explicitly clarify that the reported average mineralogical fractions were calculated across all 14 cases presented in Figure 6 (previously Figure 4).

Specifically, for each of the 36 mineral-composition scenarios, we first calculated the mean volume fraction of each mineral group across the 14 cases. We then calculated the median and the 5th–95th percentile range (P5–P95) of these 36 mean values to account for the sensitivity of the inferred mineralogical composition to the different assumptions regarding the elemental composition of clay minerals.

This calculation procedure has been clarified in the revised manuscript.

Reviewer comment:

Lines 235–236: The connection between air masses remaining above 2 km altitude and the dominance of fine clay minerals is not demonstrated. The statement needs a more explicit mechanistic link.

Author response:

We thank the reviewer for this comment. Since our analysis did not specifically investigate the relationship between particle size and mineralogical composition, we agree that the proposed connection is not sufficiently supported by our results. Therefore, we have removed this sentence from the revised manuscript to avoid an unsupported interpretation.

Reviewer comment:

Lines 237–240: The selection of cases for detailed analysis is framed as 'slightly more distinct' without quantitative justification. The authors should state an explicit thresholds that motivated the case selection. The order in which cases are then analyzed should also be explained upfront. The section should be reorganized into clearly labeled subsections either by the case characteristics dependent on the thresholds or chronologically to improve readability.

Author response:

We thank the reviewer for this comment. We agree that the previous description of the selected cases was qualitative and did not sufficiently justify their selection. We have therefore revised the text to provide a more specific description and clearer justification for the selection of these cases: “These four cases were selected based on the mineral groups showing the most pronounced differences among the 14 cases. Specifically, for Fe-oxide/Fe-hydroxide, calcite, and gypsum, the second-highest volume fraction was at least 50% greater than the third-highest, indicating a clear separation of the two highest-ranked cases from the remaining samples. Such a separation was not observed for the other mineral groups.”

The revised section now introduces this selection criterion before the detailed case analysis. The selected cases are subsequently discussed in clearly labeled subsections organized according to their distinct mineralogical characteristics and the associated transport pathways. This structure allows cases with similar mineralogical signatures and source-transport behavior to be discussed together and facilitates comparison between the Fe-oxide/Fe-hydroxide-enhanced cases and the calcite/gypsum-enhanced cases.

Reviewer comment:

Figures 5–8: Individual figures span 2–3 pages and the layout has significant design problems: panels are not matched in width or height, some panels overlap bounding boxes, and there is excessive white margin around individual panels. The authors should substantially reduce the number of panels in each figure and move supporting panels to supplementary material, retaining in the main text only those panels that are directly discussed. The figure design should be standardized across all panels as much as possible.

Author response:

We thank the reviewer for this comment. Figures 7–11 (previously Figures 5-8) have been substantially revised and simplified. Several panels presenting similar or redundant information have been removed, retaining only those directly relevant to the discussion. In particular, the Atmospheric Infrared Sounder (AIRS) Dust Score product, derived from measurements aboard NASA's Aqua satellite, has been entirely removed from the revised manuscript. The figure layouts

have also been improved to ensure greater consistency in panel dimensions, alignment, and spacing.

Total columnar mass comparison

Reviewer comment:

Figure 9: A scatter plot is not the most informative way to demonstrate that the model leads or lags the observations in time. A timeseries comparison with multiple model lead/lag lines in the style commonly used in numerical weather prediction verification would more directly illustrate this behavior and allow the reader to assess the temporal offset. AERONET observational uncertainties should be included in whichever plot type is chosen.

Author response:

We thank the reviewer for this valuable suggestion. Figure 11 (previously Figure 9) has been revised to present a time-series comparison between AERONET observations and METAL-WRF simulations at different temporal offsets, allowing a clearer assessment of potential model leads and lags. The AERONET observational uncertainties have also been included in the revised figure.

Reviewer comment:

The averaging window of ±3 hours is later referred to as a 6 hour window which is correct but slightly confusing. Perhaps choose the preferred way of referring to it.

Author response:

We thank the reviewer for this comment. In the revised manuscript, the ±3-hour window is no longer used to estimate model uncertainty, as this has been replaced by the sensitivity interval derived from the 36 mineral-composition configurations. The ±3-hour window is now considered only in Figure 11 (previously Figure 9) to investigate potential temporal offsets between METAL-WRF simulations and AERONET observations.

Elemental mass fractions

Reviewer comment:

Title and terminology: 'Elemental mass percentages' could be changed to 'elemental mass fractions' throughout, as this is the more common term in the literature. The authors may retain percentages as units but the suggested quantity name should be fractions.

Author response:

We thank the reviewer. It was corrected.

Reviewer comment:

Lines 346–347 and Figure 10: The method used to evaluate model uncertainties is not stated in the text. It can be implied that it is the ±3 hour window but this must be clearly stated.

Author response:

We thank the reviewer for this comment. In the previous version, the model uncertainty was estimated from the variability within the ±3 h time window from the in-situ measurements. In the revised analysis, the model uncertainty is replaced by the sensitivity interval derived from the 36 mineral-composition configurations and is expressed using the 5th–95th percentile range. This is now explicitly described at the beginning of Sect. 3.3.2.

Reviewer comment:

Line 363: 'Comparable' to what, specifically? The reference point is not clear. State the comparison explicitly.

Author response:

We thank the reviewer for pointing this out. The wording was unclear. The intention was only to indicate a similar order of magnitude, not directly comparable elemental percentages. However, Solomos et al. (2023) reported absolute elemental concentrations, whereas the comparison in this section is based on elemental mass fractions. To avoid a misleading comparison between different quantities, we have removed this sentence from the revised manuscript.

Reviewer comment:

Lines 365–371: The context paragraph is weak. What the current and previous studies may have in common is dust from the SAL, with trajectory analyses suggesting emission from similar West African source regions. This should be made explicit. The acknowledged differences in sampling altitude and location are correct but the discussion does not help the reader understand whether the marginal differences in reported elemental fractions are scientifically meaningful and in what way.

Author response:

We thank the reviewer for this comment. We agree that the previous paragraph did not provide sufficient context to support a meaningful comparison between the studies. In particular, differences in sampling location, altitude, period, and reported quantities made it difficult to assess whether the relatively small differences in elemental composition were scientifically significant. Rather than retaining a comparison that could be misleading, we have removed this paragraph from the revised manuscript.

Reviewer comment:

Lines 372–373: The selection of Fe and Ca for case-by-case comparison cannot be justified solely by good model–measurement agreement. The text should explain the scientific motivation for each element and then note that the model agreement for these elements makes the comparison meaningful.

Author response:

We thank the reviewer for this comment. We agree that the selection of Fe and Ca should be based first on their scientific relevance rather than on the level of model–measurement agreement. We have therefore revised the text to clarify the motivation for selecting these two elements.

Fe and Ca were selected because they represent different mineralogical components of North African dust and show clear source-related variability. Fe is associated mainly with Fe-bearing minerals and Fe oxides, whereas Ca is strongly linked to carbonate-rich material, particularly calcite, as well as gypsum. Differences in the relative abundance of these components have been used to distinguish between North African dust source regions; for example, Scheuvens et al. (2013) identified the (Ca + Mg)/Fe ratio and carbonate content among the useful markers for source discrimination.

This is particularly relevant for the present study because the selected cases in the previous section showed contrasting enhancements in Fe-oxide/Fe-hydroxide and calcite/gypsum.

Reviewer comment:

Lines 381–383: It is not clear why Figure 11 and Figure A3 are presented as separate figures apparently showing the same data (scatter plots of in-situ vs. model elemental fractions), one without and one with regression lines. Either consolidate into a single figure or provide a clear rationale for the separation. The paragraph should also begin by explaining what analysis was performed and why, before presenting the results.

Author response:

We thank the reviewer for this comment. We agree that presenting Figure 13 (previously Figure 11) and Figure A3 separately was unnecessary, as both figures showed the same model–measurement comparison. In the revised manuscript, Figure A3 has therefore been removed and the analysis is now presented only in Figure 13, which includes the regression lines and corresponding statistical information.

We have also revised the beginning of the corresponding section to clarify the methodology used for both the campaign-averaged and case-by-case comparisons. For each individual case, the

measured elemental mass fractions are compared with the corresponding METAL-WRF estimates. For the model, the median across the 36 mineral-composition configurations is used as the central estimate, while the 5th–95th percentile range represents the variability across these configurations. Measurement uncertainties are estimated using a bootstrap approach.

Conclusions

Reviewer comment:

Lines 416–417: 'No statistically correlation is found' is a grammatical error.

Author response:

Thank you for pointing this out. It was corrected.

Reviewer comment:

Lines 417-418: The inference that METAL-WRF is 'better suited for climatological-scale applications' while the WRF model suite was developed and used extensively for NWP is an interesting statement. Do the results clearly indicate this? Alternative explanations, including soil atlas uncertainty, meteorology are mentioned later in the section after this conclusion is made.

Author response:

We thank the reviewer for this comment. We agree that the agreement in campaign-averaged elemental mass fractions does not necessarily imply that METAL-WRF is better suited for climatological-scale applications. Therefore, this statement has been removed from the revised manuscript. The Conclusions have also been revised to discuss potential sources of uncertainty, including those related to soil mineralogy.

Reviewer comment:

Lines 423–427: The attribution of elemental underestimations to ‘limited set of mineral species’ or ‘missing minerals’. Can this be expanded in accordance to previously suggested expansion of the discussion to other soil mineralogy atlases not used in this study?

Author response:

We thank the reviewer for this comment. The discussion has been expanded to include other soil mineralogy atlases.

Reviewer comment:

Lines 431-435: The observational uncertainty sources such as the collection efficiency is listed as one factor among many, whereas it potentially affects the results significantly.

The conclusions should reflect, if possible to evaluate this, the significance of uncertainty sources rather than listing them uniformly.

Author response:

We thank the reviewer for this comment. The Conclusions have been revised to discuss the uncertainties associated with the assumed elemental composition of clay minerals and the oxidation state of Fe, which were quantitatively assessed in this study. We acknowledge that particle collection efficiency may represent an important source of observational uncertainty. However, its quantitative assessment was beyond the scope of the present work. Consequently, collection efficiency is only acknowledged as a potential uncertainty source.

Reviewer 2

This study presents the elemental and mineralogical composition of Saharan dust above Cabo Verde based on in situ observations conducted by UAVs and compared against a regional mineralogical dust model (METAL-WRF). Additionally to the laboratory analysis and particle classification, backwards trajectories were performed to identify source regions and satellite observations were used to independently verify active dust emissions and the model-derived columnar mass was validated against AERONET. The study is of great interest to the desert dust community and adds important new insights to the ASKOS campaign. Therefore, I recommend publication to AMT, after the authors address the following comments.

Reviewer comment:

Abstract: The motivation behind the study is not clear. The authors should work on strengthening the motivation and study objectives and clearly present them.

Author response:

We thank the reviewer for this helpful comment. We have revised the abstract to improve its overall structure and coherence. In particular, we have aimed to more clearly present the scientific gap it addresses, the methodology, the key findings and the main conclusions.

Reviewer comment:

Lines 18- 19 in relation to line 28: Please be more specific when referring to the study period. The term “summer 2022” is rather vague and implies that the reader is familiar with the ASKOS observational periods, which is not always the case. This becomes more evident later on in line 28 when the authors refer to “20-day period”.

Author response:

We thank the reviewer for this comment. In the revised abstract, the general expression “summer 2022” has been replaced with “June 2022” to specify the observational period more clearly.

Reviewer comment:

Line 21: How were the chemical signatures presented here determined? Please briefly clarify.

Author response:

We thank the reviewer for this comment. The abstract has been revised to briefly clarify how the mineralogical composition was determined. Specifically, we have clarified that the mineralogical composition was inferred through a sensitivity analysis after the revision.

Reviewer comment:

Line 28: “case-to-case variability”- it is not clear yet if the study is based on long-term observations or solely on case studies. Such information should appear in the abstract.

Author response:

We thank the reviewer for this comment. We have revised the abstract to explicitly clarify that the analysis is based on 14 sampling cases collected during the ASKOS campaign in June 2022, rather than on long-term continuous observations. The term “case-to-case variability” therefore refers to differences among these 14 individual sampling cases.

Reviewer comment:

Lines 62-64: Several citations are missing here, especially with respect to the Saharan Mineral Dust Experiment (SAMUM) project. Please consider citing the following two studies.

- Kandler, K., Schütz, L., Jäckel, S., Lieke, K., Emmel, C., Müller-Ebert, D., … Weinbruch, S. (2011). Ground-based off-line aerosol measurements at Praia, Cape Verde, during the Saharan Mineral Dust Experiment: microphysical properties and mineralogy. Tellus B: Chemical and Physical Meteorology, 63(4), 459–474. https://doi.org/10.1111/j.1600-0889.2011.00546.x
- Kandler, K., Lieke, K., Benker, N., Emmel, C., Küpper, M., Müller-Ebert, D., … Weinbruch, S. (2011). Electron microscopy of particles collected at Praia, Cape Verde, during the Saharan Mineral Dust Experiment: particle chemistry, shape, mixing state and complex refractive index. Tellus B: Chemical and Physical Meteorology, 63(4), 475–496. https://doi.org/10.1111/j.1600-0889.2011.00550.x

And in case authors are interested on an overview paper: Ansmann, A., Petzold, A., Kandler, K., Tegen, I., Wendisch, M., Müller, D., Weinzierl, B., Müller, T. and Heintzenberg, J. (2011) 'Saharan Mineral Dust Experiments SAMUM–1 and SAMUM–2: what have we learned?', Tellus B: Chemical and Physical Meteorology, 63(4), p. 403-429. Available at: https://doi.org/10.1111/j.1600-0889.2011.00555.x.

Author response:

We thank the reviewer for suggesting these references. Both Kandler et al. (2011) studies have been included in the revised Introduction to acknowledge previous investigations of the microphysical, chemical, and mineralogical properties of Saharan dust collected during the SAMUM campaign in Cabo Verde. Regarding the Ansmann et al. (2011) reference, although this study provides valuable information on the SAMUM campaigns, we have chosen to retain the more specific studies by Kandler et al. (2011a, b), which directly address the mineralogical and chemical composition of the collected dust particles and are therefore more closely related to the focus of this manuscript.

Reviewer comment:

Lines 65-67: It is worth mentioning the intensive campaign periods as well as the period that the UAVs were operated (June 2022).

Author response:

We thank the reviewer for this suggestion. The Introduction has been revised to provide a broader overview of the scientific importance of dust mineralogy and its relevance to various atmospheric processes, as well as modelling efforts. We consider that a detailed description of the intensive campaign periods would interrupt the flow of this discussion. Nevertheless, the revised manuscript specifies that the UAV measurements analyzed in this study were collected during June 2022 and provides detailed information on the individual sampling cases. Furthermore, references are provided for readers interested in a more comprehensive description of the ASKOS campaign.

Reviewer comment:

Lines 130-131: Both statements need supporting references.

Author response:

We thank the reviewer for this comment. The text has been revised for clarity, and supporting references have been added (Liu et al., 2022; Deboudt et al., 2012; Formenti et al., 2014).

Reviewer comment:

Line 165: What do the authors mean by the term “case”. Still, it is not clear to the reader. Does it correspond to the cases where in situ data were available from the UAV flights?

Author response:

We thank the reviewer for this comment. Yes, the term “case” refers to an individual in situ dust sample collected using a UAV-mounted impactor during the ASKOS campaign. Each sample was collected over approximately 5 minutes. For each case, the METAL-WRF output closest in time to the sampling period was selected for the model–measurement comparison. In total, 14 cases were considered in this study. This definition has been clarified in the revised manuscript in the section 2.2.1.

Reviewer comment:

Lines 208-2013: Authors should at least consider the study of Gebauer et al., 2026 (https://acp.copernicus.org/articles/26/3439/2026/), where the annual cycle of aerosol optical and geometrical properties based on multiwavelength-Raman-polarization lidar measurements in Mindelo are discussed in detail. How are the MBL and SAL altitudes defined in the study that is in preparation? Additionally, authors should also consider discussing first the atmospheric conditions and afterwards the backtrajectory results in combination with the sampling altitudes provided in Table 2.

Author response:

We thank the reviewer for this suggestion and for bringing the study by Gebauer et al. (2026) to our attention. During the revision, the original passage to which this reference was relevant was removed as part of the restructuring of the manuscript. Furthermore, the discussion of synoptic and local meteorological conditions has been expanded and reorganized to precede the backward-trajectory analysis, providing a clearer connection between the atmospheric conditions, transport pathways, and sampling information presented in Table 1.

Reviewer comment:

Line 225: Why there are 12 cases present in Figure 4, but 14 in Table 2? If I am not mistaken, the two last cases are missing, correct?

Author response:

We thank the reviewer for the comment. Yes, the last two cases were omitted in the original manuscript. They have now been included, and the revised figure presents all 14 cases.

Reviewer comment:

Figures 5-8: The style of the figures needs to be revised. Please try to confine all information to at least a single-page figure. Some subfigures could be even combined, e.g., Fig. 5a and 5h or 5i.

Author response:

We thank the reviewer for this comment. Figures 7–11 (previously Figures 5-8) have been substantially revised and simplified. Several panels presenting similar or redundant information have been removed, retaining only those directly relevant to the discussion. In particular, the Atmospheric Infrared Sounder (AIRS) Dust Score product, derived from measurements aboard NASA's Aqua satellite, has been entirely removed from the revised manuscript. The figure layouts have also been improved to ensure greater consistency in panel dimensions, alignment, and spacing.

Reviewer comment:

Sect.3.2: The section needs some restructuring to convey the main messages to the reader sufficiently. I suggest to include a general introduction and discussion of Fig. 4 together with the backtrajectory results and previous studies as already partially done, followed by the introduction of the more distinct cases. Authors could split the case studies discussed in detail into subsections and include all discussion for the case of June 15 for instance into one subsection to avoid repetitions. The statements in lines 316-318 are important and need to be highlighted. Additional information on the vertical homogeneity derived from ground-based lidar would further assist the authors to strengthen those statements.

Author response:

We thank the reviewer for this helpful comment. We have restructured the section to clarify the progression of the analysis and better highlight its main messages. The revised section now begins with a general discussion of Figure 6 (previously Figure 4) and relevant findings from previous studies. This is followed by the introduction of the four cases selected for detailed source-attribution analysis based on their distinct mineralogical signatures, using the quantitative criterion described in the revised manuscript.

“These four cases were selected based on the mineral groups showing the most pronounced differences among the 14 cases. Specifically, for Fe-oxide/Fe-hydroxide, calcite, and gypsum, the second-highest volume fraction was at least 50% greater than the third-highest, indicating a clear separation of the two highest-ranked cases from the remaining samples. Such a separation was not observed for the other mineral groups.”

The revised section now introduces this selection criterion before the detailed case analysis. The selected cases are subsequently discussed in clearly labeled subsections organized according to their distinct mineralogical characteristics and the associated transport pathways. This structure

allows cases with similar mineralogical signatures and source-transport behavior to be discussed together and facilitates comparison between the Fe-oxide/Fe-hydroxide-enhanced cases and the calcite/gypsum-enhanced cases.

An overview of the backward trajectories is provided in the preceding section of the revised manuscript. Therefore, this section presents only the corresponding trajectory plots for the four cases examined in detail.

Regarding your comment “The statements in lines 316-318 are important and need to be highlighted. Additional information on the vertical homogeneity derived from ground-based lidar would further assist the authors to strengthen those statements.”:
we agree that ground-based lidar observations can provide useful information on the vertical structure and homogeneity of the dust layers. However, this assessment is primarily based on intensive optical properties and therefore does not provide the same level of compositional detail as the SEM/EDX analysis presented here. A more comprehensive investigation combining the lidar-derived vertical structure with the detailed particle-scale mineralogical information is currently being developed as part of a separate follow-up study. To avoid duplicating that ongoing analysis and extending the scope of the present manuscript, we have therefore refrained from including a detailed lidar-based vertical-homogeneity assessment here.

Reviewer comment:

Line 339-340: What does this practically mean for the model? The authors should elaborate more on that.

Author response:

We thank the reviewer for this comment. Figure 11 (previously Figure 9) and the accompanying discussion have been revised to better explain the implications for model performance. The results indicate that METAL-WRF generally reproduces the magnitude and temporal evolution of the observed dust load, with most simulated values falling within the AERONET uncertainty range. However, differences in the timing of dust events are observed between the model and measurements. Although the highest overall correlation is obtained for a −3 h model offset ($R = 0.91$), the temporal differences vary among individual cases. This suggests that the model does not exhibit a single systematic timing bias and that applying a uniform temporal correction would not necessarily improve agreement for all cases. This interpretation has been clarified in the revised manuscript.

Reviewer comment:

Lines 358-359: After this statement I would expect some explanation/ hypothesis regarding the discrepancies observed for Si, Al and S. This comes instead later, after a literature overview that could be skipped or placed somewhere prior to that discussion.

Author response:

We thank the reviewer for this comment. We have organized the discussion to present the complete model–measurement comparison, including the campaign-averaged results, case-by-case comparisons, and temporal variability, before discussing the possible explanations for the observed discrepancies in Si, Al, and S. We believe that this structure allows the discrepancies to be interpreted in the context of the overall model performance, providing a more comprehensive discussion of the findings.

Reviewer comment:

Line 396: Do other models also underestimate Al?

Author response:

We thank the reviewer for this question. Underestimation of Al has also been reported for other mineral-resolving dust models. For example, Gómez Maqueo Anaya et al. (2026) reported an underestimation of Al by COSMO5.05-MUSCAT when compared with elemental observations from the JATAC 2022 campaign. The authors suggested that this discrepancy may partly arise from the representation of Al-bearing minerals, including the omission of certain mineral phases such as chlorite. These findings are consistent with our hypothesis that the underestimation of Al by METAL-WRF may partly reflect limitations in the representation of soil mineralogy. However, Al biases are not necessarily systematic across different models and observational datasets.

Reviewer comment:

Lines 431-437: I would expect such a discussion already when presenting the results.

Author response:

We thank the reviewer for this comment. The discussion of uncertainties from both the observational and modeling perspectives has been expanded in the revised Conclusions. We have retained this discussion in the Conclusions to provide an integrated assessment of the potential uncertainty sources after presenting the complete set of results. Furthermore, some of these analyses, particularly the comparison of soil mineralogy datasets, remain preliminary. Therefore, we have chosen to discuss them cautiously in the Conclusions rather than present them as established findings in the Results section.

Technical corrections:

Reviewer comment:

Abstract: According to the author guidelines (https://www.atmospheric-measurement-techniques.net/submission.html) all abbreviations should include their respective explanations.

Additionally, as stated in the author guidelines, “latin phrases should not be hyphenated (e.g., "in situ", not "in-situ")”. Please revise your manuscript according to the guidelines.

Author response:

Thank you, it was corrected.

Reviewer comment:

Line 100: UAVs have been already abbreviated (line 67). Please remove the explanation.

Author response:

Thank you, it was corrected.

Reviewer comment:

Line 123: Please add “.. to determine the particle morphology…”

Author response:

Thank you, it was added.

Reviewer comment:

Line 127: Please explain the abbreviation for atomic number “Z”.

Author response:

Thank you, it was rephrased.

Reviewer comment:

Lines 152-153: Please remove the parenthesis from the citations as they are part of your main sentence.

Author response:

Thank you, it was corrected.

Reviewer comment:

Line 162: I believe the authors mean Table A1. There are two tables listed as Table A1 in the appendix.

Author response:

Thank you, it was corrected.

Reviewer comment:

Line 234: Remove “of”.

Author response:

Thank you, it was corrected.

Reviewer comment:

Line 268: Add “the” to “... were the strongest”.

Author response:

Thank you, it was added.

Reviewer comment:

Line 389: Add “an” to “... reveals an element-dependent..”.

Author response:

Thank you, it was added.