Replies to the comments from anonymous referee #2

We would like to start the reply by thanking the anonymous referee #2 for their constructive, enthusiastic and insightful review. Their detailed suggestions and feedback have significantly enhanced the quality of the manuscript. All the comments and insight are very much appreciated. We have copied the comments into this document; the comments are in Times New Roman blue font while our answers are in Calibri black font. Line numbers refer to the version of the manuscript without track changes.

GENERAL COMMENTS:

This manuscript presents a scheme for obtaining mineral dust aerosol particle size distribution at emission (discriminated by mineral species) from given mineral-specific precursor surface sediment particle size distributions in the COSMO5.05-MUSCAT atmospheric model, and validates this scheme against aerosol observations of size-resolved mineralogy and elemental concentrations.

I congratulate the authors for a very in-depth analysis that clarifies many of the difficulties in successfully representing size-segregated dust aerosol mineralogy in models. The problem is very nicely and precisely described in the introductory section. I particularly liked the dual validation against both aerosol mineralogy and elemental concentration observations, and the insights this allowed on the difficulties in representing iron in dust aerosols.

You will see I have quite a number of recommendations for improving the manuscript. While many, none of my observations are substantial, with maybe one exception (see my recommendation below to eliminate MONARC vs. COSMO-MUSCAT comparison). Also, in order to speak to the community of dust researchers that do not work on models, but rather on experimental techniques, I would really like the authors to consider sharing their opinions in the conclusions section on surface sediment sampling and sieving methods to best serve dust modelers (see my comment below).

Thank you so much for your positive feedback. We have followed your general suggestion regarding the elimination of the MONARC vs. COSMO-MUSCAT comparison.

SPECIFIC COMMENTS:

(lines 1-2) I suggest eliminating the first sentence of the abstract, as it describes one of multiple observational campaigns used to validate the modeling work, and instead the next phrase introduces the topic more generally. The JATAC 2022 campaign is mentioned later in the abstract, so by eliminating the first sentence of the abstract a reference to the JATAC campaign is not lost.

We have rephrased this line as to better reflect the content of the manuscript.

(line 28) Replace "oxide bearing minerals" by "oxide-bearing minerals".

We have changed this and all the rest of bearing minerals.

(line 48) The silt-sand grain size boundary is usually set at 62.5 mm (e.g., Blott and Pye, doi:1111/j.1365-3091.2012.01335.x). Why is 50 mm used here? Is that the definition used in one of the the soil mineral atlas? Please, specify.

This size division is used as it is the one used in the GMINER dataset. A clarification has been added in line 49.

(line 72) Specify the type of measurements of this North African compilation.

It has been clarified that this refers to in-situ aerosol samples in line 75.

(lines 74-75) The phrase "JATAC offers mineral- and elemental-specific measurements" is confusing. Measurements or what? Do you mean measurements of elemental concentrations and of mineralogical composition? Are these size-resolved? Please, clarify.

This line has changed to better reflect the nature of measurements, as size-resolved mineralogical and elemental mass concentrations from in-situ aerosol samples (line 78).

(line 103) By "anthropogenic aerosols" do you mean biomass burning aerosols from man-made fires? Please, specify, as "anthropogenic aerosols" could also be understood as aerosols emitted from industrial activities.

We did mean biomass burning aerosols. Nonetheless, all this section has been removed as per suggestion of anonymous referee #1.

(line 144) For clarity, specify the range of possible values of f_{eff}. Is it a scalar from 0 to 1? Also, please clarify if $U_{ts}^* > U_t^*$ or if $U_{ts}^* < U_t^*$.

Indeed, f_{eff} can have values between 0 to 1, and more descriptive sentences have been added to clarify the interactions between the defined surface friction velocities (lines 121-144).

(lines 146-148) How is U_{ts}^* derived from COSMO's 10-m wind speed? Can you provide an equation or at least expand a bit the explanation in words?

The explanation on how U^* is derived from COSMO’s 10-m wind speed has been added in Equation 3 (lines 134-136).

(line 155) You used "D_p" for particle diameter previously, so I would replace "D_{pi}" by "D_p" here.

The structure of these sentences has changed but we aimed at maintaining consistency with the variables throughout (lines 148-158).

(line 157) Replace "Because threshold" for "Because the threshold".

Done (line 146).

(line 158) Add "mass" in "the soil particle mass size distribution (PSD)".

Done (line 147).

(lines 206-207) What is the thickness of the uppermost ERA5-Land soil layer? This is an important parameter to mention here.

We have added the 7 cm thickness layer detail in line 356.

(lines 216-217) This sentence on phosphorous seems arbitrary here, as this is not relevant for the topic of this manuscript. Phosphorous is not described in terms of results, neither are the measurements including phosphorous. I recommend eliminating this sentence.

We agree and have consequently removed that sentence.

(lines 222) Here it is the first time that it is mentioned what minerals are part of the "phyllosilicates". However, the term "phyllosilicates" appears twice before (including in the abstract). Instead of clarifying it here, do it in the abstract and upon first appearance in the main text.

Thank you for pointing out this inconsistency, we have now defined the term the first time it appears.

(line 241) Figure 1 should be Figure 2, and viceversa, based on the first mention of each figure in the text.

We have changed the order of the figures accordingly.

(lines 251-253 and caption of Figure 1) Where does the measurement of the total mass PSD come from? It doesn't come from GMINER, right, as GMINER only compiles PSD's within the clay-silt size ranges? Is it instead coming from a separately collected soil sample? If so, add info on the sample. Please, clarify in the text and in the caption.

The total mass PSD comes from the SoilGrids database. This has been added to Figure's 2 caption.

(caption to Figure 1) Replace "Gómez Maqueo Anaya, Sofía (2025)" by "Gómez Maqueo Anaya et al. (2025)".

The reference has been formatted accordingly.

(lines 275-276) This phrase is confusing. Do you mean that Ds corresponds to what parameter specifically of the wet-sieved soil PSD? It is confusing because the wet-sieved soil PSD is a curve that describes how the mass is distributed across particle sizes, while Ds is a scalar. Please, be more explicit on how Ds relates to the wet-sieved soil PSD.

We agree that the previous phrasing was ambiguous. We have revised the text (lines 208-216) to clarify that D_s does not correspond to a single scalar parameter of the wet-sieved PSD, but rather that the wet-sieved PSD represents the population of 'indivisible' primary particles (D_s) that are released during fragmentation. In this context, D_s marks the scale where the fragmentation theory transitions from describing the breakdown of aggregates to describing the release of primary, cohesive minerals.

(line 314) What does this *M* represent? Mathematically, it looks like k is a natural number that goes from 1 to the number of minerals. I would either describe this, or directly eliminate the use of *M* here (just leave "the mass fraction of each mineral *k* in...").

We have changed the notation accordingly (line 253).

(Figure 2) I highly recommend to add the percentage of each fraction of the pie charts, for more direct visual identification of these fractions.

We have applied your suggested modifications to the figure.

(equation 9 and 12) I recommend replacing "k e M" by "k = 1" in the bottom of the sum symbol, and "8" at the top of the sum symbol.

Done (line 257, 268, 278 and 280).

(line 324) Replace "The combined mineral mass" by "The combined soil mineral mass".

Done (line 264).

(line 327) Replace "total mineral mass" by "total soil mineral mass".

Done (line 267).

(line 329) Replace "formulation, mineral" by "formulation, soil mineral".

Done (line 269).

(line 333) With respect to "in the clay and silt categories": but based on Table 1, the aerosol bins are five rather than just two. Clarify.

We have clarified that this size categories refer to those of the SMA, and not the model's size categories (line 273).

(line 341) With respect to "size classes": are these the five bins described in Table 1? Clarify.

Please refer to previous answer.

(line 345) By "fully dispersed" soil, do you mean the wet-sieved soil? Is a wet-sieved soil fully dispersed? Or by "fully dispersed" soil you mean a conceptual (not achievable methodologically) PSD of a hypothetic fully dispersed soil?

By "fully dispersed" soil, we refer to the theoretical state in which all soil aggregates are completely dissociated into their primary, indivisible mineral grains. This state represents the physical baseline for the BFT.

(line 348) Replace "present in the silt-sized range" by "present in the soil in the silt-sized range".

Done (line 290).

(line 349) Not clear. If what is emitted as aerosol are these clay-sized fragments, they would be emitted as clay-sized aerosols (not silt-sized). Shouldn't it be "fragments of silt-sized particles...". Please, clarify.

We have replaced "fragments of clay-sized particles" with "primary clay minerals emitted within silt-sized aggregates" (Line 291). This clarifies that we are not suggesting clay particles break into even smaller pieces, but rather that primary clay minerals (as defined in the GMINER database) are often emitted as components of larger silt-sized aggregates during the physical emission process.

(line 386) Change sub-title to "Compilation of North Africa aerosol mineralogical measurements".

Done (line 485).

(line 388) "in-situ aerosol mineralogical measurements".

Done (line 487).

(lines 389-391) It is not clear to me why for this first model-observation comparison you choose to select time windows from short- and long-term monitoring programs that specifically fall within the DUSTRISK 2022 campaign period. Why wasn't a different simulation run for the full length of each of the monitoring programs in the compilation? One reason may be that each simulation is too computationally expensive, and so it is preferable to, as you did, run one single simulation with a given simulation time period, and then sub-sample data from the campaigns to coincide with this period. Are the simulations really that expensive to warrant this? If so, why was the time window of the DUSTRISK 2022 campaign selected? Wouldn't it be preferable to choose a period that is as long as possible while still falling within as many of these campaign as possible? In any case, please explain in the text the motivation for these choices in methodology.

The selection of the DUSTRISK 2022 simulations was done to allow for a direct, controlled comparison between mineralogy schemes, which results are previously published, by keeping all other variables constant. While we acknowledge the potential value of a more extensive simulation period, the current scope was limited by the significant temporal gaps within the dataset and available computational resources. We have added this justification in line 489.

(line 394) Does this mean including summer and non-summer months? But weren't winter months filtered for?

We have clarified in the text that only measurements representative of Northern Hemispheric winter months were retained (line 496).

(line 396) Cite the Zenodo DOI.

Done (line 529).

(lines 439-443) What about potential contamination from local dust emissions (from the island)? Can you say something about this possibility? Were local soils sampled and analyzed to evaluate this possibility?

We have revised the text (lines 417-425) to address the potential for local contamination. The inflow site, situated on the coast of Praia, is strategically positioned to face the prevailing northeast trade winds, ensuring that air masses are intercepted before they pass over the city or the island's interior. While we acknowledge that minor urban anthropogenic influences may persist, the upwind orientation effectively minimizes the contribution of local island soils to the mineralogical signal, making it the most suitable site for validating long-range Saharan dust transport.
Furthermore, studies on the neighboring São Vicente Island have documented the dominance of strong northeast trade winds (Carpenter et al., 2010; Fomba et al., 2014; 2025), a meteorological regime shared by Santiago Island. Consequently, the coastal inflow site is considered largely free from local dust resuspension, primarily representing long-range transport from the African continent. Conversely, while local sources may have contributed to the dust budget at the outflow site this was not specifically evaluated. However, previous research suggests that local dust typically accounts for less than 5% of the total mass in this region (Cardoso et al., 2018).

(lines 471-472) Why is 38 mg m^{-3} chosen as a threshold? Also, please explain how dust is quantified (at least the small version, citing Souza et al. (2025) for the details).

The 38 µg/m³ threshold is based on long-term time series analysis of baseline concentrations at the CVAO, during which the visibility is strongly impaired and PM mass concentrations are often above 80 µg/m³. PM concentrations above this threshold are often dominated concentrations of crustal elements such as Al, Fe, Ti, nns-Ca (non-sea salt Ca). The mineral dust or crustal matter contribution is quantified by applying established chemical stoichiometric relationship of these elements in the soil . As given in Deabji et al., (2021), Souza et al., (2025), the quantification follows $MD = 1.16 \times ((1.90 \times Al) + (23.3 \times Ca) + (2.09 \times Fe) + (1.67 \times Ti))$. Text updated in lines 445-453.

(lines 538-556 and Figure 4) It is not clear why the MONARCH vs. COSMO-MUSCAT comparison is done. I believe these are different model architectures, and the soil mineralogy compilations used are also different, so it is not clear if a comparison here can provide useful insights (many potential contributions from different processes to model output differences). Also, the authors do not come back to discussing this comparison later in the manuscript. I suggest eliminating this analysis (including eliminating panels a and b from Figure 4).

As the reviewer correctly points out, this model has different architectures and size resolutions. The analysis has been removed from the manuscript even though the COSMO-MUSCAT derived mineral mass PSD after the suggested redistribution for the same soil type remains as a figure in the Appendix.

(lines 557-558) Do you mean that a "modified"-approach simulation was conducted for the time period coinciding with the DUSTRISK 2022 campaign? Please, clarify.

Yes, precisely. For clarity we have added Table 2 with the simulations performed and their differences.

(Figure 4) I suggest eliminating panels a and b (see my previous comment). Also, One single legend with minerals is needed. Add y-axis label. The caption needs to specify the location with respect to the source of the aerosol output. Is it right after emission?

We have removed the panels and added the required information into the figure caption.

(line 562) Please, add a couple more sentences explaining the differences between the two simulations.

Table 2 and its subsequent presentation contain more information regarding the differences between the two simulations which consist basically on a difference in the treatment of the soil mineral descriptions as given by GMINER. As well as modifications in lines 597-600.

(lines 562-563) This is confusing. Do you mean that compared to observations, the modified model predicts higher (lower) mass fractions for illite and kaolinite (quartz)? If this is the case, this is actually true for illite, but not for kaolinite and quartz. Or instead, are you comparing models? If so, why do you write "bulk measurements" (measurements = observations).

This has been rephrased (now line 601), many thanks for pointing this issue out. We mean that because the modified scheme redistributes mineral across modeled size bins at different rates, it ultimately yields higher mass fractions for phyllosilicates and lower mass fractions for quartz.

(lines 564-565) But where are the simulation output and observations shown for the clay-size fraction? Isn't Figure 5 the "bulk" quantity (i.e., mass fractions integrated with respect to particle size)?

We have now explicitly added a paragraph explaining how the sizes are reconciled between the observations and the measurements between lines 534 and 541.

(lines 572-573) What do you mean by size limitations? Be explicit.

We have deleted this sentence.

(line 576) "Feldspar shows little change in model-observation correlation between the two modeling approaches".

The wording has changed on line 616.

(line 577) "in the "modified" scheme with respect to the original scheme".

The wording has changed on line 621.

(lines 596-597) Cannot point measurements falling within a model grid cell be averaged for better comparisons against model output?

Yes, while that can be done, there are no measurement points that fall within the same grid cell at the same time of the measurement. We have refined the discussion for that results section on line 639 to better emphasize these challenges regarding the scale mismatch between point measurements and 28 km grid-cell averages.

(caption in Figure 6) Eliminate "are" in "The error bars are represent".

Done.

(lines 610-615) Prior to the second sentence in this paragraph, I suggest stating that both model approaches cannot represent the variability in observed variability in elemental concentrations. In fact, this is relevant to this full section. This model underrepresentation is quite marked.

We agree with this suggestion and consequently have added a paragraph before the elemental concentrations' evaluation on lines 733-739 on the subsection 5.2.

(line 611) Replace "silicate" by "silicon".

Done in this instance and following similar instances, e.g., line 741.

(lines 613-615) Isn't there a blank measurement of silicon concentration?

Indeed, blank measurements were conducted for both filters and TXRF sample carriers. However, due to inherent variability in filter density and surface reflectance of the carriers, blank values were inconsistent across samples. This made reproducible blank subtraction impossible, and consequently Si concentrations could not be quantified with sufficient certainty. Given these uncertainties, we determined that Si could not provide reliable or meaningful information for the model-measurement comparison. We have therefore removed Si from the comparison and added an explanation in line 441.

(line 617) Call Figure 6 at the end of the first sentence of this paragraph.

This paragraph has disappeared on the current version of the manuscript.

(lines 617-618) Where is this shown (what figure)?

This is now shown on Table 7.

(lines 618-619) Where is this shown (what figure)?

Shown on Table 7 as well.

(line 621) Call Figure 6 at the end of the first sentence of this paragraph.

Done (line 831).

(lines 621-623) I would lower the emphasis here, that is, in line 622, I would eliminate "sharply" and replace "reasonable" with "comparatively better".

We agree and have changed it correspondingly (line 836).

(line 625) Again, I would replace "adequate" with "comparatively better" to be more objective.

This sentence disappears under the current version of the manuscript.

(line 629) Call Figure 6 after "in both schemes".

Done (line 844).

(line 630) Call Figure 5.

Done (line 846).

(line 634) "clay-sized particles".

The wording has changed found between lines 850-857.

(Figure 6 and Figure 7) Can these figures be combined into a single figure and still be readable? If so, I recommend doing so.

We did, thank you for the suggestion, now Fig. 11.

(line 639) Call Figure 7 at the end of the first sentence of this paragraph.

Done (line 866).

(lines 665) In order to avoid confusion between the use of clay for size and for elements (clay = phyllosilicates), I recommend replacing "clay minerals" and "silt minerals" here for the actual minerals.

We changed the nomenclature on this and the following instances, e.g., line 646.

(line 672) By how much does the "original" model underestimate phyllosilicates?

This is now better exemplified by Table 5, which is reporting mean biases between observed and simulated mineral mass content.

(line 677) Replace "underestimates" by "underestimating".

This has changed since the mineralogy content reported is from an updated classification scheme. Nevertheless, we have the changed the wording from “underestimates” to “underestimating” when appropriate.

(caption of Figure 9) I recommend not using the term "silt minerals". Rather, specify the minerals.

We agreed and have changed it accordingly, see Fig.7.

(lines 704 and 708) Replace "silicone" by "silicon".

Done.

(line 736) I do not see that dramatic improvement for BIN01.

This is correct and the changes are now better quantified by the added mean biases on Table 6.

(line 739) I do not see that increased underestimation for BIN26.

This is true as well, and we have changed the wording for the potassium comparison paragraph found between lines 785-794.

(line 749) Call Figure 12 after ""original" and "modified" approaches".

Now Table 6 is called in line 796.

(Figure 12) Why aren't the original model values included for Mg for BIN09 and BIN26, and for S for BIN01?

The original scheme produces no values for Mg and S in these size fractions. This is because illite, the only Mg-bearing simulated mineral, is restricted to BIN01 and BIN03 therefore there are no Mg values for the coarser size bins. Similarly, S values are absent in BIN01 because gypsum, the only S-bearing simulated mineral, has no description for that size category in the original scheme.

(line 756) "comparison (Figure 12) mirroring".

The wording has changed in line 804.

(line 787) "compilation of aerosol measurements".

Done (line 906).

(line 794) "Validation with in-situ elemental concentration-based, mineral-like measurements".

Done (line 905).

(line 818) Why are these two mineral phases particularly ill-represented in SMAs? For gypsum, is it due to its high water solubility and the fact that water sieving is usually used? And for calcite? Can you make a recommendation in this section on how to measure soil PSD? And what about soil sampling methods? (in both cases, for the objective of improving dust representation in models).

Indeed, it is related to with the water solubility and the wet-sieving methodology. We have added sentences to this aspect (line 944).

With regards to what would be needed from a soil's PSD characterization with the objective of improving the model, we have emphasized in the text that a more complete database is needed, specifically regarding missing mineral phases and their clay-silt-sand fractioning (line 969). Based on our experience and finding from related projects (e.g., the FRAGMENT project; González-Romero et al., (2023)), soil's PSD measurements face the primary challenge of dispersion, as any technique used inevitably alters the sample. The closest mimic of the physical process of dust emission through the saltation and sandblasting processes involves the lab-based dust generators (e.g., GAMEL) coupled with rapid measurement techniques like APS or OPCs; this provides a more realistic PSD than traditional static wet-sieving or sedimentation.

Whenever possible, in-situ PSD measurements during actual emission events would be ideal. Giving the complexity and inherent trade-offs of the soil PSD measurement methodology, we feel that adding a brief recommendation in the manuscript might oversimplify a nuanced technical issue.

References:

Cardoso, J., Almeida, S. M., Nunes, T., Almeida-Silva, M., Cerqueira, M., Alves, C., Rocha, F., Chaves, P., Reis, M., Salvador, P., Artiñano, B., and Pio, C.: Source apportionment of atmospheric aerosol in a marine dusty environment by ionic/composition mass balance (IMB), Atmospheric Chemistry and Physics, 18, 13 215–13 230, https://doi.org/10.5194/acp-18-13215-2018, 2018.

Carpenter, L.J., Fleming, Z.L., Read, K.A. *et al.* Seasonal characteristics of tropical marine boundary layer air measured at the Cape Verde Atmospheric Observatory. *J Atmos Chem* 67, 87–140, https://doi.org/10.1007/s10874-011-9206-1, 2010.

Deabji, N., Fomba, K. W., El Hajjaji, S., Mellouki, A., Poulain, L., Zeppenfeld, S., and Herrmann, H.: First insights into northern Africa high-altitude background aerosol chemical composition and source influences, Atmospheric Chemistry and Physics, 21, 18 147–18 174, https://doi.org/10.5194/acp-21-18147-2021, 2021.

Fomba, K. W., Müller, K., van Pinxteren, D., Poulain, L., van Pinxteren, M., and Herrmann, H.: Long-term chemical characterization of tropical and marine aerosols at the Cape Verde Atmospheric Observatory (CVAO) from 2007 to 2011, Atmos. Chem. Phys., 14, 8883–8904, https://doi.org/10.5194/acp-14-8883-2014, 2014.

Fomba, K. W., Quaye, D. T., Achterberg, E. P., Marais, E., and Herrmann, H.: Saharan and Namib Dust Phosphorus Fluxes in the North and South Atlantic Regions, ACS Earth and Space Chemistry, 9, 1465–1478, https://doi.org/10.1021/acsearthspacechem.4c00411, 2025.

González-Romero, A., González-Flórez, C., Panta, A., Yus-Díez, J., Reche, C., Córdoba, P., Moreno, N., Alastuey, A., Kandler, K., Klose, M., Baldo, C., Clark, R. N., Shi, Z., Querol, X., and Pérez García-Pando, C.: Variability in sediment particle size, mineralogy, and Fe mode of occurrence across dust-source inland drainage basins: the case of the lower Drâa Valley, Morocco, Atmos. Chem. Phys., 23, 15815–15834, https://doi.org/10.5194/acp-23-15815-2023, 2023.

Souza, E. J. D. S., Fomba, K. W., Anaya, S. G. M., Schepanski, K., Freire, S. M., Materić, D., Reemtsma, T., and Herrmann, H.: Particle-bound mercury in Saharan dust-loaded particulate matter in Cabo Verde, Journal of Hazardous Materials, 487, 137 053, https://doi.org/10.1016/j.jhazmat.2024.137053, 2025.