Response to the review comments

RC1: 'Comment on egusphere-2026-2127', I. Pérez, 08 May 2026 reply

This paper considers two improved databases that are used in a simulation model to investigate the particulate matter concentration. The study region considers the Iberian Peninsula and Northern Africa, although the analysis is focused on the southeast of the Iberian Peninsula. Five short simulation periods during the years 2022 and 2025were considered. Observations were obtained from 8 stations and the comparison between measured and modelled values was established with the correlation coefficient, MBE and RMSE. Since the paper is quite complete, only some minor changes should be introduced in the manuscript.

1. The authors should indicate the reason for the selection of such specific periods since there is a gap, from 2023 to 2024, which is not covered by this study. Moreover, a comment about the study representativeness would be acknowledged.

The selection of the simulation periods is primarily driven by the availability of completed model experiments and observational datasets associated with specific dust and PM10 episodes. Our first simulations for this work began in 2023, focusing on selected extreme PM10 events in the region. The study was later resumed in 2025, once a more complete and quality-controlled set of observational data from the monitoring network became available. As a result, the analysed periods are not continuous and correspond to well-defined case studies.

2. If selected periods correspond to pollution events, representative synoptic charts could be introduced to present the atmospheric circulation in such episodes.

Indeed, the synoptic conditions are important to properly contextualise the selected pollution episodes. Thank you for your comment. We will include in the Appendix a set of figures describing the synoptic evolution for each analysed period. These will include daily-mean fields of geopotential height at 500 hPa (Z500), temperature at 850 hPa (T850), and sea level pressure (SLP), covering the day of the PM10 peak and the surrounding days.

In addition, we will add a figure showing the corresponding daily mean PM_{10} concentrations during these periods to better illustrate the temporal evolution of each event.

3. The Pearson correlation coefficient may be affected by outliers. Fig. 11b presents some accused outliers in some simulations. A comment about the response of the correlation coefficient could be introduced, i.e., the authors could comment if successful or poor correlations may be considered spurious due to the outlier influence. Perhaps, supplementary graphs could show the outlier weight in the correlations.

We appreciate your comment about this. Indeed, Pearson's R can be sensitive to extreme values. However, in the context of this study, these "outliers" are not spurious measurements or anomalous points, but rather represent the extreme PM10 episodes that constitute the main focus of the analysis. The simulation periods were intentionally selected to evaluate the model performance during high-impact dust events, including both Saharan dust intrusions and local dust uplift episodes. Consequently, large PM10 concentrations and strong variability are expected features of the dataset. Excluding these values from the analysis would remove the most relevant part of the signal and would therefore not be appropriate for the objectives of this work.

This behavior can be clearly observed in Figs. 11 and F1 (in the new version), where many PM10 values are substantially above typical background concentrations. These high values are physically meaningful and correspond to observed and simulated dust events rather than statistical anomalies. We agree that Pearson correlation coefficients should be interpreted cautiously under highly episodic conditions. For this reason, the correlation analysis was complemented with additional statistical metrics such as RMSE and MBE, together with the inspection of temporal series and spatial PM10 distributions. This combined approach provides a more robust assessment of model performance and reduces the possibility of misleading interpretations based solely on correlation values.

To address your concerns about this, we will include an additional clarification in the revised manuscript discussing the limitations of Pearson correlation in event-based analyses characterized by extreme PM10 concentrations.

4. The authors could indicate the simulation that leads to the best results or, alternatively, the conditions to select one simulation type against the rest.

If your question is about which simulation episode should be selected, three of them (March 2022, April 2025, and November 2025) are associated with synoptic-scale situations characterised by low-pressure systems over the Atlantic that favour the advection of Saharan dust towards the IP. In particular, during the March 2022 event, the low-pressure system is located over northern Morocco, which enhances dust transport into the study region.

The remaining two cases (July 2022 and August 2025) correspond to more stable atmospheric conditions, with weak synoptic forcing and low wind speeds, which favour the accumulation of PM10. In the case of August 2025, the simulations indicate a local dust emission event over the southeastern Iberian Peninsula (as shown in the PM10 maps). These results are consistent with the observational data, which report a maximum concentration of 204.5 μg m^{-3} during this episode.

This particular case is therefore interpreted as a local dust uplift event, which is why it has been selected for a more detailed analysis. Specifically, it provides an opportunity to assess the ability of the newly developed SOILHD dataset to represent local-scale variability in dust emissions and its impact on PM_{10} concentrations.

RC2: 'Comment on egusphere-2026-2127', Anonymous Referee #2, 11 May 2026 reply

This manuscript introduces two new erodibility maps to be used for dust emission within WRF-Chem configured to use GOCART for aerosols, building upon prior work by using a finer spatial resolution and considering soil type. These new dust emission configurations were evaluated against a control scenario using observations of PM10 from SINQLAIR network in southeastern Spain. It was evident that a great deal of effort went into generating the dust source maps, completing the modeling simulations, and comparing the output to observations.

The manuscript itself still needs some work to better organize the content, explain to the reader what the figure is showing, and then tying everything together. There are figures barely mentioned in the text, and a good example of where the writing could be improved on this is Figure 11. The text jumps right in to say which configuration was best before introducing the timing of the event or discussing that all three simulations underestimate PM10 for the three days prior to maximum.

After looking at the Appendix, it turns out on the first day, a bias is present at all stations, irrelevant of the dust source. I would encourage the authors to take a step back and re-read the paper from start to finish with a fresh set of eyes to ultimately improve the readability and help the reader understand the bigger picture.

1. Line 44: The Ginoux 2001 reference is specific to dust in GOCART coupled with GEOS. A more appropriate reference for GOCART itself should be used here, in addition to a reference on the coupling of GOCART to WRF-Chem.

Chin et al., 2000 (https://doi.org/10.1029/2000JD900384) will be the new reference for the GOCART scheme, and in some occassion Ginoux et al. 2001 is also considered. However, we couldn't find a direct reference available for the implmentation of GOCART into the WRF-Chem model. We know for a bibliographic research and by other GOCART methodological papers that the implementation of GOCART into WRF-Chem occcurred during the year 2009. For example, LeGrand et al., 2019 (https://doi.org/10.5194/gmd-12-131-2019) says:

"… In 2009, GOCART aerosol physics, including algorithms for dust emissions, transport, dry deposition, and gravitational settling, were added to the Weather Research and Forecasting model with chemistry (WRF-Chem) framework".

But there is no a direct reference to the implementation. If you know about this reference, we would be pleased to include it, so please post it on a new comment.

2. Line 51: Is EROD an acronym that should be defined?

EROD is the default name for the soil erodibility variable used by WRF-Chem as an input for the GOCART dust emission scheme. A proper introduction to the name will be present in the new version of the manuscript.

3. Line 53: Where is IP? Can the study region be introduced further?

Thank you for the question. IP stands for Iberian Peninsula. The acronym is defined on line 28, and the spatial domain corresponds to domain d02 of our simulations (see Fig. 4). We will introduce this detail in a comment inside the new version of the manuscript.

4. Paragraph beginning on like 59: Another recent paper that can be cited is at https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2025JD045333?utm_source=researchgate.net&utm_medium=article/.

The reference and a comment will be included in the new version of the manuscript. Thank you for your suggestion.

5. Line 133-135: Friedl et al. 2002 is a relatively old reference for MODIS land surface characterization. What exactly was used to generate regions where does cannot be emitted? Can the surface type vary in time, for example from glaciers that retreat annually?

For the EROD-HR dataset, we employed the GMTED2010 topography dataset (7.5 arcsec resolution) together with the MODIS land-use dataset available within the default geographical data distributed with the WRF Preprocessing System (WPS), which contains 20 land-use categories at 30 arcsec resolution. In particular, the “barren or sparsely vegetated” category was used to define potentially erodible areas. Therefore, the reference to Friedl et al., 2002 is appropriate in this context.

We acknowledge that more recent and detailed land-cover datasets are currently available and may provide a more realistic representation of barren and erodible surfaces, including time-varying features such as glacier retreat or seasonal snow cover. However, the primary objective of the EROD-HR dataset was not to generate a fully realistic or dynamically varying erodibility database, but rather to isolate and evaluate the impact of increasing the spatial resolution while preserving a methodology conceptually consistent with the default WRF-Chem erodibility approach.

The development of a more physically realistic and soil-aware erodibility dataset was instead the purpose of the SOILHD dataset. This dataset was constructed from two independent high-resolution sources. First, the natural bare erodible soil fraction was derived from the LCZ-based dataset of Demuzere et al., 2022 (https://doi.org/10.5194/essd-14-3835-2022), which provides land-cover information at 100m spatial resolution while explicitly excluding urban areas and permanent snow/ice surfaces. This dataset was conservatively interpolated to a 1km grid to generate a fractional bare-soil coverage for each model cell. Subsequently, soil-type information was incorporated using the SoilGrids250m dataset by Hengl et al., 2017 (https://doi.org/10.1371/journal.pone.0169748), from which three different soil texture layers were implemented within the erodibility framework.

6. Figures 1 -3: Can a colorbar be added for sand, silt, and clay erodibility? Maybe consider to an appendix or supplemental doc as they are not discussed in the text.

Thank you for this suggestion. However, adding a conventional colorbar for sand, silt, and clay is not straightforward in this case, as the SOILHD erodibility maps (Figs. 1-3) are constructed from a ternary combination of the three soil fractions, where the final color at each grid point represents a unique mixture of sand, silt, and clay contributions rather than a single scalar field.

To address this and improve interpretability, we have revised Fig. 1 by including a ternary (triangular) diagram legend that explicitly represents the possible combinations of sand, silt, and clay fractions used in the SOILHD dataset. This allows the reader to directly relate the displayed colors to the underlying soil composition in a physically consistent manner.

In addition, we have expanded Appendix A to include the individual spatial distributions of each of the three soil components (sand, silt, and clay) as separate maps. These supplementary figures provide a more detailed view of the contribution of each texture class and complement the combined representation shown in the main figures.

7. Table 3: Are the mean and max temporal, spatial, or both?

They are both temporal and spatial (station-wise) means. A new caption has been added to Table 3 in the new version.

8. Equations 4-6: It is not necessary to include these equations as they are standard metrics.

The Figs. And their references alongside the text have been removed from the new version.

9. Figure 5: Can a reference box for the domains be added to at least one of the panels?

It is now added. Thanks for your suggestion.

10. Section 3.1: The organization of this section needs to be improved. The text jumps around from Figure 5 to 6, and then back to 5, with references to Figures 2 and 3 thrown in, then 7, and back to 5.

This section has been fully restructured, comments about Figs. 2 and 3 have been relocated to sec 2.2, and only some minor comments referencing to the latter were included in this section. Additionally Figures and Table 4 were commented ni order and more consistnetly.

11. Figure 8 is never referenced in the text. I would recommend taking this one out using a figure along the same lines for domain d03 showing the spatial map of PM10 but adding dots over the surface stations to get a visual representation of where and when the model performed well in section 3.2.

This figure has been added following your suggestions. The older figure has been moved to Appendix.

12. Section beginning on line 329: Can a rationale be given for the purpose of looking at the vertical profiles? Can anything in the vertical profiles be connected back to the erodibility or emissions difference in the previous figures?

This following comment has been added to the new version. Thank you for your suggestion. The motivation for analyzing the vertical structure is to assess whether the differences in surface erodibility translate not only into changes in near-surface concentrations, but also into modifications of the vertical redistribution of dust within the atmospheric column. In particular, changes in the erodibility field directly affect dust emission fluxes at the surface, which in turn influence the vertical mixing and uplift of particles through turbulent transport and boundary layer processes.

13. Section 3.2/Figure 10: The interannual and spatial variability of these results should be further discussed more coherently and elaborated upon.

Thank you for your suggestion. We have carefully revisited Fig. 10 and identified a coding issue in the post-processing routine used to compute the statistical metrics. After correcting this error, the resulting figure shows improved and more consistent performance across most periods and model configurations, with generally higher correlation values.

The corresponding subsection has been fully revised to incorporate the updated results. In addition to correcting the values, we have restructured the discussion to provide a more coherent interpretation of the interannual and spatial variability, explicitly linking model performance differences to the distinct meteorological and emission regimes represented in each simulation period. Furthermore, to improve the clarity of the multi-period comparison, a Taylor diagram summarizing the site-averaged statistics for each experiment and period has been added to the Appendix.

Finally, the conclussions have also been revised to improve clarity and match with the new results.

14. Figure 11: I am intrigued about the underestimate of PM10 from all dust emission configurations for August 23 through 25. Does this indicate that WFR-Chem could be underestimating something else that happens to be compensated?

Thanks for the comment, this is a very relevant point. The underestimation of PM10 between 23 and 25 August could be due to several factors, and at this stage we cannot attribute it to a single clear cause.

On the one hand, although a one-day spin-up period was used, it is possible that a longer time would be needed to fully equilibrate aerosol concentrations in the model under rapidly evolving conditions. This may affect the simulated background levels, particularly during the early phase of the episode. On the other hand, an important limitation of the present simulations is that only mineral dust emissions are included, while anthropogenic PM10 sources are not considered. This is likely to introduce a systematic negative bias, since observations show a persistent background level that never drops to zero (see Fig. 10 and F1 in the revised version). This background likely includes anthropogenic contributions that are not represented in the model, especially at inland stations with significant industrial activity such as LOR, where local anthropogenic emissions can substantially contribute to observed PM10 levels. The absence of these sources in the model could therefore partially explain the observed underestimation during this period,

Overall, this could explain part of the underestimation during those days and may also give the impression of some compensation effects when comparing the different emission configurations. In a future work we would like to address this by including anthropogenic emission inventories, as well as testing longer simulation periods and extended spin-up times to better capture background PM10 levels.

List of relevant changes made in the manuscript

Introduction and Background

- Added clarification regarding the selection of the five analyzed PM_{10} episodes and their representativeness.
- Expanded the description of the study domain and explicitly clarified the meaning of the acronym "IP" (Iberian Peninsula).
- Improved the introduction of the WRF-Chem erodibility variable (EROD).
- Updated the references used to describe the GOCART aerosol scheme, including the incorporation of Chin et al. (2000) hereafter.

- Added recent literature on dust-source characterization and erodibility mapping suggested by the reviewer, including additional discussion of previous erodibility datasets and their limitations.

Methodology

- Added a ternary legend to Figure 1.
- Expanded the description of the SOILHD dataset generation methodology.
- Updated description of Figures 2 and 3.
- An explanation of the choice of simulation periods has been added.
- A synoptic description of the periods of simulation has been added.
- Added a new appendix Figure containing synoptic charts for all simulation periods.
- Added a new appendix daily PM_{10} evolution figures associated with each event.
- Removed equations describing standard statistical metrics (R, RMSE, MBE) and their references in the text.
- Added discussion of the limitations of Pearson correlation coefficients under episodic extreme PM_{10} conditions.

Results

- Revised results concerning the number of erodible grid points over the RM.
- Added a domain reference box to Figure 5.
- Reordered figure discussion to follow the sequence in which figures appear.
- Added explicit motivation for analyzing vertical profiles.
- Relocated comments related to Figs. 2 and 3 from the Results section to the Methodology section.
- A new Figure 9 has been added, showing the average distribution of PM10 in the control experiment and its relative differences compared to the other experiments for the d03 domain (RM), along with red dots indicating the location of the SINQLAIR observation network. The previous Figure 9 has been moved to Appendix D.
- Added new discussion on Figure 9
- Completely restructured Section 3.2. Added new discussion and improved transitions between figures and tables, with a reorganized discussion to connect erodibility maps, emissions, and PM_{10} results with more cohesion.
- Corrected a coding error affecting the calculation of performance metrics in Figure 10. All statistical metrics were recomputed after correcting the error, and Figure 10 was updated. The interpretation of model performance across all periods was also revised.
- Expanded discussion of interannual and spatial variability.
- Added comment about missing anthropogenic PM_{10} emissions as a possible cause for underestimation of PM10 in the initial days.

Discussion and Conclusions

- Expanded discussion of synoptic controls on dust events.
- Added interpretation distinguishing Saharan dust transport episodes and local dust uplift events.
- Included discussion of the August 2025 local dust outbreak as a key case study for evaluating SOILHD.
- Revised conclusions to reflect the corrected statistical analysis.
- Updated conclusions to improve consistency with the revised results and discussions.
- Improved the overall readability, coherence, and scientific narrative of the manuscript.

Appendices

- Added a ternary legend Figure.
- Added new appendix figures showing SOILHD map together with:
  - Sand fraction map.
  - Silt fraction map.
  - Clay fraction map.
- Added synoptic charts and average PM10 maps for the peak days of every simulation period.
- Replaced the original Figure 8 with a new figure showing PM_{10} spatial distribution for CTRL and relative differences for the other experiments, along with the SINQLAIR observational monitoring station locations.
- Moved the previous Figure 8 to appendix D.
- Added a Taylor diagram summarizing model performance for all simulations and periods.

Tables and Captions

- Clarified in Table 3 that reported means and maxima correspond to spatially averaged station statistics and temporal averages.
- Improved several figure and table captions throughout the manuscript.
- Simplifications and Formatting
- Removed associated redundant explanatory text.