Referee #1

Comment A1

The manuscript by Xu and coauthors presents new model developments within the IPSL Earth System Model framework, aimed at including vegetation dependence on desert dust emissions. Simulated fields are validated against observational datasets. The topic is of interest for the aerosol and climate modelling communities. Both the manuscript and the underlying scientific work are well crafted. This work is suitable publication after minor revisions, from my perspective.

Response

We sincerely thank the reviewer for the positive assessment and constructive suggestions. We have carefully addressed all comments in the revised version, with detailed point-by-point responses provided below.

Comment A2

Specific comments

67-68. Perhaps not many "original" studies such as, e.g. Marticorena and Bergametti (1995), but many "CMIP-class" models already include this effect, e.g. UKESM (Woodward et al., 2001: https://doi.org/10.1029/2000JD900795), CESM (Zender et al., 2003: https://doi.org/10.1029/2002JD002775), MPI (Stanelle et al., 2014: https://doi.org/10.1002/2014JD022062D) among others.

Response

Thanks for this insightful comment. The sentence has been revised in lines 68–71 to explicitly acknowledge these CMIP-class models:

"To date, only a limited number of process-oriented studies have explicitly investigated the effects of vegetation on dust emission as a primary focus. Nevertheless, such effects have already been incorporated in several Coupled Model Intercomparison Project (CMIP)-class models, such as UKESM (Woodward et al., 2001), CESM (Zender et al., 2003), and MPI (Stanelle et al., 2014), among others."

References:

Stanelle, T., Bey, I., Raddatz, T., Reick, C., and Tegen, I.: Anthropogenically induced changes in twentieth century mineral dust burden and the associated impact on radiative forcing, J. Geophys. Res. Atmos., 119, 13526–13546, https://doi.org/10.1029/2014JD022062, 2014.

Woodward, S.: Modeling the atmospheric life cycle and radiative impact of mineral dust in the Hadley Centre climate model, J. Geophys. Res. Atmos., 106, 18155–18166, https://doi.org/10.1029/2000JD900795, 2001.

Zender, C. S., Bian, H., and Newman, D.: Mineral Dust Entrainment and Deposition (DEAD) model: Description and 1990s dust climatology, J. Geophys. Res. Atmos., 108, 4416, https://doi.org/10.1029/2002JD002775, 2003.

Comment A3

155. Why only January? What about the Southern Hemisphere, for instance?

Response

The January temperature criterion was originally implemented for Northern Hemisphere sources to exclude frozen ground and ice-covered surfaces; thus, it does not fully capture Southern Hemisphere seasonality. We acknowledge this simplification and a month-by-month check will be implemented in future model versions.

The text has been revised in the Methods (lines 161–163) and Section 4 (lines 816–819):

In Methods:

"(1) Non-frozen or ice-free surface: Dust mobilization is restricted to surfaces not bound by ice or frost (Schulz et al., 2009). In this study, this is quantified by excluding regions where the mean January surface air temperature is below 0 °C, which mainly reflects Northern Hemisphere winter conditions and was originally optimized for major Northern Hemisphere sources;"

In Section 4:

"...the use of a January temperature threshold—originally designed for Northern Hemisphere sources—may not fully capture the seasonal freezing cycles in the Southern Hemisphere. While this simplification does not affect our assessment of vegetation-induced effects (due to the methodological consistency across all simulations), it contributes to regional biases that we aim to address in future versions through a full month-by-month temperature check."

References:

Schulz, M., Cozic, A., and Szopa, S.: LMDzT-INCA dust forecast model developments and associated validation efforts, IOP Conf. Ser. Earth Environ. Sci., 7, 012014, https://doi.org/10.1088/1755-1307/7/1/012014, 2009.

Comment A4

156. How is a "potential dust source area" defined?

Response

The "potential dust source area" is a predefined input mask in the model that constrains dust emission to regions where the climatological annual precipitation is less than 300 mm (Schulz et al., 2009).

We have revised the main text in lines 164–166 as: "(2) Precipitation-limited source areas: The grid cell must be located within a predefined potential dust source area, where climatological annual precipitation is less than 300 mm (Schulz et al., 2009), representing regions with limited annual moisture supply as potential dust emitters;"

References:

Schulz, M., Cozic, A., and Szopa, S.: LMDzT-INCA dust forecast model developments and associated validation efforts, IOP Conf. Ser. Earth Environ. Sci., 7, 012014, https://doi.org/10.1088/1755-1307/7/1/012014, 2009.

Comment A5

157. Which variable is a "surface wetness proxy"?

Response

This proxy represents the moisture balance (net water-equivalent depth, mm) in the uppermost soil layer.

We have clarified this in lines 167–170:

"(3) Completely dry surface: Dust emission is restricted to a completely dry surface, quantified by a surface wetness proxy (water-equivalent depth, mm) that represents the moisture balance in the uppermost soil layer. Emission is permitted only when this proxy falls below a value of 10^{-10}, which serves as a numerical threshold to enforce a dry surface condition and does not represent a physically meaningful soil moisture value."

Comment A6

191. Considering that fr >=1 by definition, there are three possible cases for the application of equation 4.4, from my understanding:

(1) u < ut < ut*fr , leading to no dust emissions

(2) u > ut*fr > ut , leading to dust emissions from both "pure" bare soil and bare soil between vegetation patches

(3) ut < u < ut*fr , resulting in positive emissions from "pure" bare soil but apparently negative emissions from bare soil between vegetation patches, which would pose some problems. Please clarify this aspect.

Response

We used the MAX function in the model code to ensure that each term in Eq. (4) only contributes to the total dust emission flux when its specific threshold (u_t and $u_t \times f_r$) is exceeded, thereby preventing unphysical negative values.

We have revised Equation (4) (line 208) to explicitly include the max operator:

$$F_{\mathrm{dust}} = (1 - A_v)\times\mathrm{MAX}[C\times u^2\times(u\text{-}u_t),0]+A_v\times\mathrm{MAX}\left[C\times u^2\times\left(u\text{-}f_r\times u_t\right),0\right]$$

We have also revised the corresponding text in lines 216–218 for clarity:

"Emissions from the first term of Eq. (4) that represents the contribution from surfaces without vegetation protection, occurs only when the default threshold velocity (u_t) is exceeded. Conversely, emissions from the second term accounting for the vegetation effect, are determined by a modified threshold velocity ($u_t \times f_r$), ..."

Comment A7

16 (Supplement). Regridded how? Via bilinear interpolation?

Response

The regridding was performed using bilinear interpolation. We have clarified this in the Spplement (lines 16–17): "…, using a bilinear interpolation approach."

Comment A8

252. Do you mean that deep convection follows the "Standard Physics" while turbulent mixing parameterizations follows the New Physics" scheme?

Response

We clarify that both the deep convection and turbulent mixing parameterizations follow the "New Physics" scheme (Boucher et al., 2020). The term "Standard Physics" was a remnant from a previous version and has been corrected.

The revised text (lines 289–290) now reads: "The deep convection and turbulent mixing parameterizations follow the "New Physics" scheme (Boucher et al., 2020), …"

Reference:

Boucher, O., Servonnat, J., Albright, A. L., Aumont, O., Balkanski, Y., Bastrikov, V., Bekki, S., Bonnet, R., Bony, S., Bopp, L., Braconnot, P., Brockmann, P., Cadule, P., Caubel, A., Cheruy, F., Codron, F., Cozic, A., Cugnet, D., D'Andrea, F., Davini, P., de Lavergne, C., Denvil, S., Deshayes, J., Devilliers, M., Ducharne, A., Dufresne, J.-L., Dupont, E., Éthé, C., Fairhead, L., Falletti, L., Flavoni, S., Foujols, M.-A., Gardoll, S., Gastineau, G., Ghattas, J., Grandpeix, J.-Y., Guenet, B., Guez, L. E., Guilyardi, E., Guimberteau, M., Hauglustaine, D., Hourdin, F., Idelkadi, A., Joussaume, S., Kageyama, M., Khodri, M., Krinner, G., Lebas, N., Levavasseur, G., Lévy, C., Li, L., Lott, F., Lurton, T., Luyssaert, S., Madec, G., Madeleine, J.-B., Maignan, F., Marchand, M., Marti, O., Mellul, L., Meurdesoif, Y., Mignot, J., Musat, I., Ottlé, C., Peylin, P., Planton, Y., Polcher, J., Rio, C., Rochetin, N., Rousset, C., Sepulchre, P., Sima, A., Swingedouw, D., Thiéblemont, R., Traore, A. K., Vancoppenolle, M., Vial, J., Vialard, J., Viovy, N., and Vuichard, N.: Presentation and evaluation of the IPSL-CM6A-LR climate model, J. Adv. Model. Earth Syst., 12, e2019MS002010, https://doi.org/10.1029/2019MS002010, 2020.

Comment A9

296. Was this calculation done online or rather calculated a posteriori based on monthly output?

Response

The simulated DAOD at 550 nm was calculated *a posteriori* (offline) based on the monthly outputs of dust load. We have revised the text in line 335–336: "The simulated DAOD at 550 nm was calculated *a posteriori* based on monthly model outputs using the following equation:"

Comment A10

329. Are those scaling factors applied to dust emissions? If so, in the case of the 4-mode configuration you would imply changing the overall size distribution at emission. Please comment on that.

Response

The scaling factors used to tune the DAOD were indeed applied *a posteriori* (offline) to the dust emissions. To reach the target global DAOD (Ridley et al., 2016), we applied the same scaling coefficient exclusively to Mode 2 and Mode 3, which collectively account for ~80% of the optical depth in the 4-mode configuration.

As noted, this modifies the relative mass contributions of the modes at emission (e.g., changing the 1, 4, 31, and 64% distribution (without scaling factors) to 1, 5, 36, and 59% (with scaling factors) for Modes 1–4, respectively). However, this adjustment only rescales their respective emission magnitudes and does not alter the intrinsic microphysical properties (e.g., MMD, σ) of individual modes.

We have added the following explanation in the text (lines 369–376): "… and *b* specifically for Mode 2 and Mode 3, while Mode 1 and Mode 4 were left unchanged in the 4-mode configuration. This strategy is based on the fact that Mode 2 and Mode 3 contribute most significantly to the global DAOD (together accounting for ~80 %), whereas Mode 1 and Mode 4 have a smaller contribution (Di Biagio et al., 2020). To ensure consistency throughout the dust cycle, these global scaling coefficients were systematically applied offline to the monthly model output (including dust emission, DAOD, surface PM concentration, and deposition) as part of an *a posteriori* calibration. Although applying such scaling coefficients to emission slightly modifies the relative mass contributions of different size modes within the 4-mode configuration, the intrinsic microphysical properties of each mode (e.g., MMD and σ) remain consistent with the original parameterization."

References:

Di Biagio, C., Balkanski, Y., Albani, S., Boucher, O., and Formenti, P.: Direct Radiative Effect by Mineral Dust Aerosols Constrained by New Microphysical and Spectral Optical Data, Geophys. Res. Lett., 47, e2019GL086186, https://doi.org/10.1029/2019GL086186, 2020.

Ridley, D. A., Heald, C. L., Kok, J. F., and Zhao, C.: An observationally constrained estimate of global dust aerosol optical depth, Atmos. Chem. Phys., 16, 15097–15117, https://doi.org/10.5194/acp-16-15097-2016, 2016.

Comment A11

332. How was the overall calibration process carried out in order to determine the optimal scaling coefficients (considering all three variables)?

Response

The calibration was conducted in a two-stage process (1) constraining the scaling coefficients so the global mean annual DAOD fell within the benchmark of 0.030 ± 0.005 (Ridley et al., 2016); and (2) identifying the optimal coefficient that yielded the best collective statistical metrics for DAOD, surface PM, and total deposition.

For instance, in the 4-mode configuration, a coefficient of 1.24 was selected over 1.44 because it yielded better collective metrics across all variables (Table S2), although both fell within the Ridley et al. (2016) range.

We have revised the text (lines 377–381) for clarity: "The calibration process prioritized maintaining the global mean annual DAOD within the target range of 0.030 ± 0.005, while simultaneously optimizing the model's overall statistical performance against independent observational datasets for DAOD, surface PM, and total deposition. Sensitivity analysis (Table S2) revealed that the optimal scaling coefficients are 0.74 for *a*, and 1.24 for *b*. Other coefficients produced a higher global mean annual DAOD and resulted in weaker correlation with observations in the 4-mode configuration (Table S2)."

Reference:

Ridley, D. A., Heald, C. L., Kok, J. F., and Zhao, C.: An observationally constrained estimate of global dust aerosol optical depth, Atmos. Chem. Phys., 16, 15097–15117, https://doi.org/10.5194/acp-16-15097-2016, 2016.

Comment A12

341. These processes apply to dust atmospheric dispersion in general, not specifically to PM10. This sentence seems out of place here.

Response

We agree with reviewer and have revised the text (lines 385–386) to more accurately reflect the factors governing surface concentrations:

"Surface dust PM concentrations are determined by the combined effects of vegetation-modulated emissions and subsequent atmospheric transport and deposition processes."

Comment A13

374. I do not understand this distinction, since I could not find any subsequent budget segregating e.g. global land vs oceanic dust deposition.

Response

The distinction was introduced to evaluate model performance across different deposition environments (Figs. S27–S28), as the physical processes and observational uncertainties vary between terrestrial and oceanic sites. We did not intend to imply a separate budget analysis.

We have revised the text (lines 434–435) to clarify this purpose: "To evaluate the model's performance across different deposition environments, we distinguished between observations from terrestrial and oceanic stations, where the corresponding grid cells were identified using a global land mask (NASA, 2025)."

Comment A14

491. It is not clear at which point those scaling factors were applied, i.e. online at the stage of dust emissions, or rather offline, on the monthly history files?

Response

The global scaling coefficients were applied offline to the monthly history files as an *a posteriori* adjustment.

We have explicitly clarified this offline approach in both the Methods (lines 372–374) and Results (lines 566–568).

In Methods:

"To ensure consistency throughout the dust cycle, these global scaling coefficients were systematically applied offline to the monthly model output (including dust emission, DAOD, surface PM concentration, and deposition) as part of an *a posteriori* calibration."

In Results:

"Through the *a posteriori* application of the global scaling coefficients (Sect. 2.3.3) to the monthly history files, ... All subsequent analyses were conducted using these offline-adjusted results."

Reference:

Ridley, D. A., Heald, C. L., Kok, J. F., and Zhao, C.: An observationally constrained estimate of global dust aerosol optical depth, Atmos. Chem. Phys., 16, 15097–15117, https://doi.org/10.5194/acp-16-15097-2016, 2016.

Referee #2

Comment B1

The manuscript titled "vegetation effects redistribute dust globally" by Xu et al. describes a model development work on including the wind drag partition effect due to the presence of vegetation on dust emission by coupling the atmospheric model LMDzORINCA with the land surface model ORCHIDEE. This would have an impact on changing global dust distributions in the model. The authors showed that including the vegetation effects mainly improved the dust simulations in LMDzORINCA by comparing the dust simulations against a suite of measurements and reanalysis data. The model configurations are overall carefully designed and the experiment is nice. I think that doing the regional tuning using the DustCOMM product is understandable but *ad hoc*, and probably to some extent reduce the impacts of the vegetation effects in the model–observation comparisons. I also have questions regarding how the drag partition effect is applied to the vegetated areas versus bare soils of the gridbox. I put my detailed science questions in the following, and I would suggest major revision.

Response

We sincerely thank the reviewer for the positive evaluation of our model development and the constructive feedback regarding the regional tuning and drag partition application. We have carefully addressed these points in the revised manuscript, as detailed in our point-by-point responses below.

Comment B2

Detailed comments:
Line 1: if this paper is mostly about the vegetation drag partition effect, you might want to add it into the title to make it more clear in terms of what is being studies in this paper.

Response

Thank you for this insightful suggestion. The title has been revised to: "Vegetation drag partition effects redistribute dust globally" (Line 1).

Comment B3

Line 63: Any difference between DOD and DAOD? You use DAOD for the rest of your manuscript.

Response

In this study, DOD and DAOD refer to the same quantity (dust aerosol optical depth). We have replaced "DOD" with "DAOD" in line 64 to ensure consistency throughout the manuscript: "Although tuning models against observed dust aerosol optical depth (DAOD) …"

Comment B4

Line 72: Why alternatively? What's the difference between this sentence and the last sentence? You cited the same papers.

Response

The term "alternatively" was used to distinguish between these two parameterization approaches: the first reduces surface shear stress, while the second increases the threshold friction velocity. Some studies (e.g., Okin, 2005; Foroutan et al., 2017) were cited in both cases as they discuss or implement both methods.

The text has been revised in lines 73–77 for clarity:

"Some models explicitly simulate the reduction of surface shear stress reaching erodible soils, typically as a function of vegetation-induced surface roughness length or roughness density (Shao et al., 1996; Okin, 2005; Foroutan et al., 2017; Klose et al., 2021). Other parameterizations represent this effect by enhancing the threshold friction velocity through a correction factor derived from vegetation-induced surface roughness (Okin, 2005; Foroutan et al., 2017; Wu et al., 2021)."

References:

Okin, G. S.: Dependence of wind erosion and dust emission on surface heterogeneity: Stochastic modeling, J. Geophys. Res. Atmos., 110, D11208, https://doi.org/10.1029/2004JD005288, 2005.

Foroutan, H., Young, J., Napelenok, S., Ran, L., Appel, K. W., Gilliam, R. C., and Pleim, J. E.: Development and evaluation of a physics-based windblown dust emission scheme implemented in the CMAQ modeling system, J. Adv. Model. Earth Syst., 9, 585–608, https://doi.org/10.1002/2016MS000823, 2017.

Comment B5

Lines 84: ESMs can choose to either do active simulations of vegetation and land-surface state, coupled with the atmosphere and the ocean, or use prescribed vegetation and land state. This is more like a choice to guarantee realistic vegetation and land in the simulations and save computational costs, rather than a drawback of losing two-way interactions.

Response

We agree with the reviewer that prescribing vegetation and land-surface states in ESMs is a common and effective choice to ensure realistic surface conditions and computational efficiency. We have revised the text (lines 87–90) to reflect this perspective and clarify that methodological variability often stems from the different proxies used:

"Some advanced models utilize prescribed land-surface states to ensure realistic vegetation representation and computational efficiency, with the differences in the proxies and parameterizations used to represent vegetation effects, particularly regarding the partitioning of aerodynamic drag and the representation of surface roughness density, introducing methodological variability across different modelling frameworks (Shinoda et al., 2011; Klose et al., 2021)."

Comment B6

Lines 85: You don’t need dynamic vegetation to see how vegetation impacts dust – prescribed vegetation works too. By using dynamic vegetation, you have the extra capacity of examining how dust and dynamic vegetation nonlinearly interact with each other, through feedback mechanisms such as radiation, clouds/rainfall, land albedo, nutrient cycles, etc. I don’t see you examine these links in the rest of the manuscript. You did not show evidence that your dynamic vegetation model is improving your dust simulations because those feedback mechanisms are important. So, I don’t see the why you criticize other model for not using a dynamic vegetation model.

Response

We agree that prescribed vegetation is a valid and widely used approach for investigating vegetation impacts on dust emission. We also acknowledge that the present study does not explore the complex feedbacks (e.g., radiation, clouds, or nutrient cycles) associated with fully interactive land–atmosphere coupling.

We have revised the text accordingly in lines 90–93 as follows:

“Despite this variability, the use of prescribed land-surface data represents a widely adopted and practical approach for investigating vegetation effects on dust emission. More complex configurations with interactive land–atmosphere coupling may further enable the investigation of feedback processes and facilitate future studies.”

Comment B7

Line 155: Why just January? Is this criterion not considered in other months? Or is this criterion determining dust emissions for that gridbox for the rest of the year?

Response

The January temperature criterion was originally implemented to exclude frozen surfaces for major Northern Hemisphere sources and was not applied month-by-month in the current version. A full seasonal temperature check will be implemented in future versions.

We have revised the Methods (lines 161–163) and added a discussion of this limitation in Section 4 (lines 816–819):

In Methods:

“(1) Non-frozen or ice-free surface: Dust mobilization is restricted to surfaces not bound by ice or frost (Schulz et al., 2009). In this study, this is quantified by excluding regions where the mean January surface air temperature is below 0 °C, which mainly reflects Northern Hemisphere winter conditions and was originally optimized for major Northern Hemisphere sources;”

In Section 4:

“... the use of a January temperature threshold—originally designed for Northern Hemisphere sources—may not fully capture the seasonal freezing cycles in the Southern Hemisphere. While this simplification does not affect our assessment of vegetation-induced effects (due to the methodological consistency across all simulations), it contributes to regional biases that we aim to address in future versions through a full month-by-month temperature check."

References:

Schulz, M., Cozic, A., and Szopa, S.: LMDzT-INCA dust forecast model developments and associated validation efforts, IOP Conf. Ser. Earth Environ. Sci., 7, 012014, https://doi.org/10.1088/1755-1307/7/1/012014, 2009.

Comment B8

Line 157: what proxy is this? Please clarify what the threshold of 10-10 mm is. is it soil moisture depth?

Response

This proxy represents the moisture balance (net water-equivalent depth, mm) between precipitation and evaporation in the uppermost soil layer.

In the model, the value of 10^{-10} is not a physically measurable depth, but a numerical cut-off used as a mathematical switch to ensure dust emission occurs only under a completely dry surface state.

We have revised the text in lines 167–170 for clarity: "(3) Completely dry surface: Dust emission is restricted to a completely dry surface, quantified by a surface wetness proxy (water-equivalent depth, mm) that represents the moisture balance in the uppermost soil layer. Emission is permitted only when this proxy falls below a value of 10^{-10}, which serves as a numerical threshold to enforce a dry surface condition and does not represent a physically meaningful soil moisture value."

Comment B9

Line 165–166: Can you describe in more detail how u_t is soil type-specific? Does LMDzORINCA assign one u_t value for each soil type?

Response

In LMDzORINCA, a baseline u_t is first assigned based on the correspondence between FAO soil classes and threshold values derived by Marticorena and Bergametti (1995) over the Sahara Desert (Claquin, 1999; Schulz et al., 2009). This baseline is then extrapolated globally using the FAO soil map. Finally, local u_t values are modified to account for surface properties such as topographic sheltering and soil mineralogy (e.g., iron oxides), following Claquin (1999) and Schulz et al. (2009).

We have revised the manuscript accordingly (lines 176–181) for clarity: "In this scheme, u_t is derived by establishing a spatial correspondence between the region-specific threshold velocities calculated by Marticorena and Bergametti (1995) and the FAO soil types database within the Saharan Desert (Claquin, 1999; Schulz et al., 2009). Based on this correspondence, the model assigns a baseline u_t to each soil type according to the global FAO soil distribution, and adjusts the local u_t values to account for surface characteristics, such as topographic slopes and iron oxide content, which increase u_t due to aerodynamic sheltering and soil crusting, respectively. Consequently, the threshold velocity in this default scheme depends on soil types and geological properties, and does not include vegetation effects."

References:

Claquin, M.-T.: Modelling the mineralogy and the radiative effects of desert dust (in French), Ph.D. thesis, Univ. Paris VI, Paris, France, 1999.

Marticorena, B. and Bergametti, G.: Modeling the atmospheric dust cycle: 1. Design of a soil-derived dust emission scheme, J. Geophys. Res.-Atmos., 100, 16415–16430, https://doi.org/10.1029/95JD00690, 1995.

Schulz, M., Cozic, A., and Szopa, S.: LMDzT-INCA dust forecast model developments and associated validation efforts, IOP Conf. Ser. Earth Environ. Sci., 7, 012014, https://doi.org/10.1088/1755-1307/7/1/012014, 2009.

Comment B10

Lines 183–184: You said in the introduction (e.g., line 78) that other models use random numbers without justification or sensitivity testing (e.g., LAI = 0.3), but you are here adopting a number that gives you the least drag partition effect from rocks and pebbles, which is kind of random to me too.

Response

We acknowledge that the choice of $\lambda_s = 0.002$ involves a simplified assumption. Our goal was to isolate the vegetative signal by adopting the lower bound of the reported range for λ_s (Marticorena et al., 2006; Foroutan et al., 2017), thereby minimizing interference from non-vegetative elements (e.g., rocks) for which a global dataset is currently insufficient. We agree that incorporating spatially and temporally varying λ_s (e.g., Leung et al., 2023) is a key direction for future work.

We have clarified this point in the revised manuscript (lines 198–200): "In this study, the lower bound of the reported range 0.002 was adopted to represent conditions with minimal non-vegetative solid obstacles, in order to minimize background interference, allowing us to focus on the role of vegetation in drag partitioning."

References:

Foroutan, H., Young, J., Napelenok, S., Ran, L., Appel, K. W., Gilliam, R. C., and Pleim, J. E.: Development and evaluation of a physics-based windblown dust emission scheme implemented in the CMAQ modeling system, J. Adv. Model. Earth Syst., 9, 585–608, https://doi.org/10.1002/2016MS000823, 2017.

Leung, D. M., Kok, J. F., Li, L., Okin, G. S., Prigent, C., Klose, M., Pérez García-Pando, C., Menut, L., Mahowald, N. M., Lawrence, D. M., and Chamecki, M.: A new process-based and scale-aware desert dust emission scheme for global climate models – Part I: Description and evaluation against inverse modeling emissions, Atmos. Chem. Phys., 23, 6487–6523, https://doi.org/10.5194/acp-23-6487-2023, 2023.

Marticorena, B., Kardous, M., Bergametti, G., Callot, Y., Chazette, P., Khatteli, H., Le Hégarat-Mascle, S., Maillé, M., Rajot, J.-L., Vidal-Madjar, D., and Zribi, M.: Surface and aerodynamic roughness in arid and semiarid areas and their relation to radar backscatter coefficient, J. Geophys. Res., 111, F03017, https://doi.org/10.1029/2006JF000462, 2006.

Comment B11

Lines 192–193: You assumed that the land that is not vegetation cover is bare soil. Is it true? Are urban cover, water bodies, and ice/snow considered? Please clarify in the main text.

Response

In our dust emission framework, land surface within potential source regions is simplified into bare soil and vegetated fractions. Anthropogenic types (urban) and non-emissive surfaces (water bodies and ice/snow) are excluded as they do not meet the physical criteria for natural mineral dust emission in this study.

We have clarified this in the text (lines 222–225): "Within potential source regions, the land surface is assumed to be composed of bare soil and vegetation. Anthropogenic land types, such as urban areas and irrigation-related water bodies, are excluded to maintain the focus on natural mineral dust emissions (Ginoux et al., 2012). Furthermore, surfaces covered by permanent ice, snow, or water are treated as non-emissive, consistent with the conditions for dust emission in the model (Sect. 2.1.2)."

References:

Ginoux, P., Prospero, J. M., Gill, T. E., Hsu, N. C., and Zhao, M.: Global-scale attribution of anthropogenic and natural dust sources and their emission rates based on MODIS Deep Blue aerosol products, Rev. Geophys., 50, RG3005, https://doi.org/10.1029/2012RG000388, 2012.

Comment B12

Lines 194–195: I got a bit confused here. λ_s comes from rocks and pebbles. What is the reasoning of applying f_r with rocks/pebbles impacts on the vegetated cover, while assuming no drag partition for the bare soil (you assumed $1 - A_v$ is bare soil)? Shouldn't bare soil have some rocks and pebbles? I am not sure if this is what Foroutan et al. (2017) did. If I understand correctly, Foroutan applied f_r to the grid-level threshold velocity and did not partition dust emissions from bare and vegetated covers.

Response

The application of λ_s and f_r follows a sub-grid dust emission framework that combines two limiting cases:

(i) Segregated case (bare soil regime), with complete spatial separation between vegetation and bare soil, thereby no vegetation-induced shielding of bare soil. While bare soil may contain non-vegetative roughness elements (e.g., rocks and pebbles), the default u_t is used without applying f_r, in order to remain consistent with the baseline parameterization and avoid potential double counting of non-vegetative roughness effects.

(ii) Interspersed case (vegetation-influenced regime), with uniform mixing where vegetation provides aerodynamic shielding. Here the correction factor f_r following Foroutan et al. (2017) is applied, accounting for both vegetative (λ_v) and non-vegetative (λ_s) roughness contributions. To avoid over-emphasizing the non-vegetative roughness in this study, we adopt the lower-bound value of λ_s.

This approach differs from Foroutan et al. (2017) by representing sub-grid heterogeneity through an area-weighted framework, rather than assuming the grid-scale homogeneity.

We have clarified these points in the revised manuscript (lines 209–219):

"This formulation can be interpreted as a first-order approximation between two limiting cases: (i) a segregated case, implying a complete spatial separation between vegetation and bare soil, thereby no vegetation-induced shielding of bare soil emissions, as expressed by the first term in Eq. (4); and (ii) an interspersed case, where the uniform distribution of vegetation reflects the effective protection from vegetation, as expressed by the second term in Eq. (4). Accordingly, total dust emissions at the grid scale are computed as an area-weighted combination of these two limiting cases, thereby representing intermediate conditions and capturing sub-grid surface heterogeneity without explicitly resolving their spatial organization of vegetation patterns (Deblauwe et al., 2008).

Emissions from the first term of Eq. (4) that represents the contribution from surfaces without vegetation protection, occurs only when the default threshold velocity (u_t) is exceeded. Conversely, emissions from the second term accounting for the vegetation effect, are determined by a modified threshold velocity ($u_t \times f_r$), where the correction factor ($f_r \geq 1$) represents the additional aerodynamic resistance induced by vegetation elements."

Reference:

Deblauwe, V., Barbier, N., Couteron, P., Lejeune, O., and Bogaert, J.: The global biogeography of semi-arid periodic vegetation patterns, Global Ecol. Biogeogr., 17, 715–723, https://doi.org/10.1111/j.1466-8238.2008.00413.x, 2008.

Foroutan, H., Young, J., Napelenok, S., Ran, L., Appel, K. W., Gilliam, R. C., and Pleim, J. E.: Development and evaluation of a physics-based windblown dust emission scheme implemented in the CMAQ modeling system, J. Adv. Model. Earth Syst., 9, 585–608, https://doi.org/10.1002/2016MS000823, 2017.

Comment B13

Line 197: Please explain further on how "dust emission is suppressed by both threshold increase and erodible surface reduction". I can see that f_r increases threshold velocity, but I am not sure how Eq. 4 limits the effective erodible surface if both A_v and $1–A_v$ are emitting dust.

Response

We agree that the term "erodible surface reduction" was inaccurate and have removed it.

In Eq. (4), dust emission is suppressed by the increase in threshold velocity ($u_t \times f_r$) in the vegetated fraction. When wind speed is between u_t and $u_t \times f_r$, only the bare soil fraction contributes to emission; when wind speed exceeds $u_t \times f_r$, both contributions are activated. There is no physical reduction in surface area.

We have revised the text (lines 219–221) to reflect this:

"Therefore, unlike the default scheme (Sect. 2.1.2), this revised approach (Eq. (4)) suppresses dust emission by increasing the threshold velocity for the vegetated fraction, providing a more physically consistent representation of sub-grid emission processes."

Comment B14

Line 211: Reader doesn't understand what revision 9010 is, please clarify in the main text. Is it a github pull request or what?

Response

We clarify that "Revision 9010" refers to the specific version number in the Subversion (SVN) repository of the ORCHIDEE land surface model.

We have clarified this in the main text (lines 238–239):

"… a dynamic density approach (revision 9010 in the ORCHIDEE Subversion (SVN) trunk) …".

Comment B15

Line 225, Eq. 5: Dust models typically just use the first term in the right-hand side for bare soil. Accounting for the gaps in the vegetated areas according to the densities are not super common. I wonder how important the last three terms are. Do they sum up being bigger than the first term? I guess it depends on how big the gaps are. It may be good to plot them out individually, in the supplement.

Response

We agree that pure bare soil (first term, Eq. 5) is the primary global source. However, bare soil gaps within grasslands (last three terms) provide a non-negligible and often overlooked contribution. Representing these gaps is an important aspect of our formulation.

Following the suggestion, we have added Fig. S3 to the Supplement to compare these terms. Globally, the area-weighted global mean fraction of pure bare soil fraction (23%) exceeds the combined grassland gap fraction (8%). However, the contribution from bare soil gap within grasslands becomes locally dominant in semi-arid regions (e.g., Western USA, Southern Africa, and India), as shown in the difference map (Fig. S3f).

We have incorporated this in the text (lines 261–265):

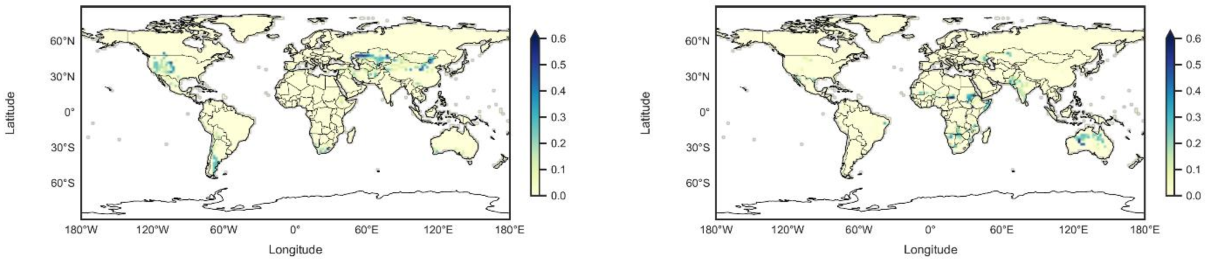
"The fraction of bare soil gaps within the grassland PFT is expressed as ($1-D$), representing the portion of the grassland area not covered by vegetation. This is shown for each grassland

(Fig. S3a–c) and their sum (Fig. S3d), and is compared to the fraction of pure bare soil (Fig. S3e). While pure bare soil dominates in hyper-arid regions such as North Africa, the bare soil gaps within grasslands become more prominent in (semi-)arid regions including the western USA, southern Africa, and India, where they can locally exceed the contribution of pure bare soil (Fig. S3f)."

We have put the new plot Fig. S3 in the supplement:

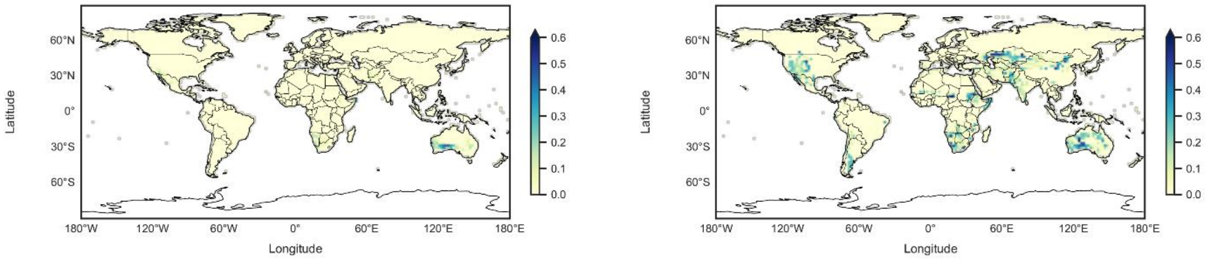
(a) Fraction of gaps within Temperate C_3 grassland

(b) Fraction of gaps within C_4 grassland



(c) Fraction of gaps within Tropical C_3 grassland

(d) Total fraction of all grassland gaps



(e) Fraction of pure bare soil

(f) Difference: (Total grassland gaps) minus (Pure bare soil)

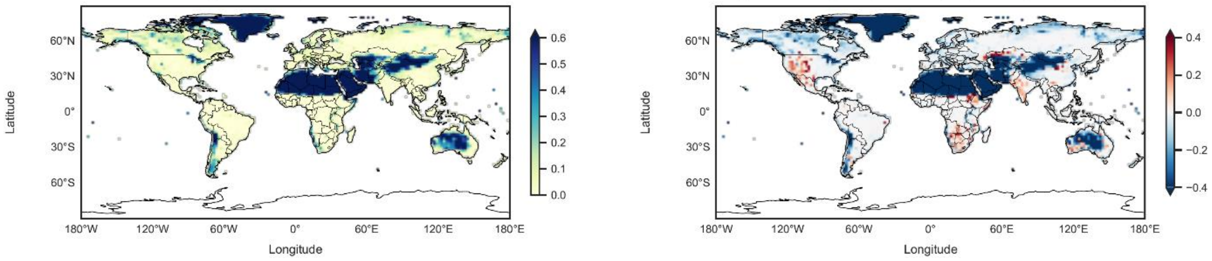


Figure S3. Global distribution of the bare soil fraction derived from ORCHIDEE averaged over 2004–2020. The fraction of bare soil gaps within (a) temperate C_3 grassland, (b) C_4 grassland, and (c) tropical C_3 grassland; (d) the total fraction of all bare soil in grassland gaps; (e) the fraction of pure bare soil; and (f) the difference between the total bare soil fraction in grassland gaps and the pure bare soil fraction.

Comment B16

Line 226: How did you spin up the vegetation state and the land surface to make sure the PFTs are close to reality in year 2008? Are there any comparison of the vegetation state against satellite products, from you or from previous papers?

Response

Regarding the spin-up and land surface state, the model is first spun up for 200 years to reach biogeochemical equilibrium, followed by a transient simulation for 2004–2020 driven by interannual meteorological forcing. This ensures that 2008 is part of a continuously evolving simulation rather than an independently prescribed state.

Regarding the evaluation, the vegetation representation (fractional vegetation cover) has been evaluated against the Copernicus FCOVER dataset for (semi-)arid regions in our previous work (Xu et al., 2026). The dynamic density approach for grasslands improves spatial correlation from 0.11 to 0.26 relative to the default fixed-density approach. Remaining differences reflect differences in definition between model-derived vegetation fraction and satellite-based greenness products.

We have added these clarifications to the text:

Lines 268–270: "In ORCHIDEE, building on the equilibrium with a 200-year spin-up, a continuous transient simulation was performed for 2004–2020 using interannual meteorological forcing, ensuring that the land surface state—including that of 2008—evolves consistently with climate variability."

Lines 239–242: "This updated approach has been shown to improve the spatial representation of fractional vegetation cover when evaluated against the Copernicus Land Monitoring Service FCOVER dataset (Copernicus Land Monitoring Service, 2020), compared to the default fixed density approach (Xu et al., 2026), with correlation coefficient increasing from 0.11 to 0.26."

Reference:

Copernicus Land Monitoring Service: Fraction of Green Vegetation Cover 2014-present (raster 300 m), global, 10-daily–version 1. Copernicus Land Monitoring Service [data set], https://doi.org/10.2909/09578c73-4f5d-4d2c-90ff-4e17fb7dbf69, 2020.

Xu, S., Luyssaert, S., Balkanski, Y., Ciais, P., Viovy, N., Wan, L., and Sciare, J.: Representing dynamic grassland density in the land surface model ORCHIDEE r9010, Geosci. Model Dev., 19, 1–25, https://doi.org/10.5194/gmd-19-1-2026, 2026.

Comment B17

Line 227: What defines the densities D? Are they prescribed as constants or dynamically simulated?

Response

Grassland density D is dynamically simulated by ORCHIDEE rather than prescribed as a constant. It is defined as the fractional area occupied by vegetation and varies according to plant physiological states, including reserve and labile carbon pools. The simulated monthly D fields are used as inputs for the F_{bare} calculation.

We have added the clarification in the main text (lines 258–260): "The grassland density (D) is dynamically simulated by ORCHIDEE, varying based on indicators such as reserve and labile carbon, reflecting vegetation response to resource availability. The monthly output of simulated D serves as a time-varying input for the F_{bare} calculation in Eq. (5)."

Comment B18

Line 234: After reading this part, I still don't understand why you apply f_r only to the A_v parts of the grid cell, as if you assume roughness density of rocks and pebbles are in the vegetated areas, whereas the bare soil area (1–Av) has completely bare and smooth surfaces. I still wonder if this is a realistic assumption.

Response

The formulation in Eq. (4) does not assume that the bare soil fraction ($1-A_v$) is smooth or devoid of roughness elements. Instead, it uses the model's baseline threshold velocity (u_t) as a reference state for the bare soil fraction, in order to ensure consistency with the original parameterization and avoid potential double counting of non-vegetative roughness effects.

The correction factor f_r is applied only to the vegetated fraction to isolate the additional aerodynamic resistance introduced by vegetation. The non-vegetative roughness density (λ_s) is prescribed using a lower-bound value in this study to avoid over-emphasizing non-vegetative roughness contributions.

Comment B19

Line 279: From your results, please comment on why you think DustCOMM rescaling is needed. That is, where do you think the regional dust bias comes from? Biases in meteorology and land-surface variables or other missing processes in your dust emission scheme?

Response

We use DustCOMM as an observationally constrained benchmark because globally gridded observations of dust emissions are not available. DustCOMM is widely used in the community as a reference dataset for evaluating regional dust emission patterns (Adebiyi et al., 2020; Kok et al., 2021b; Li et al., 2022; Leung et al., 2023).

Our untuned simulations show systematic regional biases relative to DustCOMM (Figs. S13, S15, S16), which motivate the application of regional scaling factors to correct baseline magnitude and spatial patterns. These biases likely arise from three main sources:

(i) simplified structural assumptions in dust source definition (e.g., January temperature and precipitation thresholds);

(ii) uncertainties in the driving meteorology, particularly surface wind speed representation;

(iii) simplified land-surface characteristics, including unresolved sub-grid roughness (e.g., rocks and pebbles).

The same rescaling factors are applied uniformly across all simulations, ensuring that differences among experiments arise from the physical parameterizations rather than tuning.

We have added a clarification in Sect. 2 (lines 321–325): "A same set of rescaling factors (Table S1) was applied to all simulations to ensure that any divergence between the control and vegetation-impact simulations was strictly attributable to the explicitly parameterized inhibitory effects of vegetation, rather than being masked or distorted by differential model tuning. These rescaling factors can be used to account for biases in regional dust emissions and to provide an observational constraint. A discussion of the potential sources of regional biases is provided in Sect. 4."

And in the Sect. 4 (lines 813–828):

"The inclusion of vegetation cannot fully address the biases inherent in the model, as our evaluation of emissions without the regional rescaling factors (Sect. 2.3.2) reveals systematic discrepancies relative to DustCOMM (Figs. S13, S15, and S16). These discrepancies likely arise from several structural limitations in the model. First, the constraints for defining dust source areas are simplified. For instance, the use of a January temperature threshold—originally designed for Northern Hemisphere sources—may not fully capture the seasonal freezing cycles in the Southern Hemisphere. While this simplification does not affect our assessment of vegetation-induced effects (due to the methodological consistency across all simulations), it contributes to regional biases that we aim to address in future versions through a full month-by-month temperature check. Furthermore, the use of a 300 mm annual precipitation threshold (Schulz et al., 2009) may lead to inherent underestimations in semi-arid regions where episodic dust events occur despite higher mean annual precipitation (Huang et al., 2014; Khusfi et al., 2020). Second, structural biases in the nudged meteorological fields, particularly in the regional representation of surface wind speed, likely contribute to these discrepancies. Finally, the lack of fine-scale surface information, along with simplifications in land-surface properties (e.g., representation of non-vegetative roughness, such as rocks and pebbles), may also contribute to these systematic biases. While the relative importance of these factors varies across regions, their combined effect leads to systematic biases in emission magnitude and spatial distribution. Regional rescaling is therefore applied to provide an observational constraint on simulated emissions. Addressing these biases remains an important direction for future model development."

References:

Adebiyi, A. A., Kok, J. F., Wang, Y., Ito, A., Ridley, D. A., Nabat, P., and Zhao, C.: Dust Constraints from joint Observational-Modelling-experiMental analysis (DustCOMM): comparison with measurements and model simulations, Atmos. Chem. Phys., 20, 829–863, https://doi.org/10.5194/acp-20-829-2020, 2020.

Huang, J., Wang, T., Wang, W., Li, Z., and Yan, H.: Climate effects of dust aerosols over East Asian arid and semiarid regions, J. Geophys. Res. Atmos., 119, 11398–11416, https://doi.org/10.1002/2014JD021796, 2014.

Khusfi, Z. E., Khosroshahi, M., Roustaei, F., and Mirakbari, M.: Spatial and seasonal variations of sand-dust events and their relation to atmospheric conditions and vegetation cover in semi-arid regions of central Iran, Geoderma, 365, 114225, https://doi.org/10.1016/j.geoderma.2020.114225, 2020.

Kok, J. F., Adebiyi, A. A., Albani, S., Balkanski, Y., Checa-Garcia, R., Chin, M., Colarco, P. R., Hamilton, D. S., Huang, Y., Ito, A., Klose, M., Li, L., Mahowald, N. M., Miller, R. L., Obiso, V., Pérez García-Pando, C., Rocha-Lima, A., and Wan, J. S.: Contribution of the world's main dust source regions to the global cycle of desert dust, Atmos. Chem. Phys., 21, 8169–8193, https://doi.org/10.5194/acp-21-8169-2021, 2021b.

Leung, D. M., Kok, J. F., Li, L., Okin, G. S., Prigent, C., Klose, M., Pérez García-Pando, C., Menut, L., Mahowald, N. M., Lawrence, D. M., and Chamecki, M.: A new process-based and scale-aware desert dust emission scheme for global climate models – Part I: Description and evaluation against inverse modeling emissions, Atmos. Chem. Phys., 23, 6487–6523, https://doi.org/10.5194/acp-23-6487-2023, 2023.

Li, L., Mahowald, N. M., Kok, J. F., Liu, X., Wu, M., Leung, D. M., Hamilton, D. S., Emmons, L. K., Huang, Y., Sexton, N., Meng, J., and Wan, J.: Importance of different parameterization changes for the updated dust cycle modeling in the Community Atmosphere Model (version 6.1), Geosci. Model Dev., 15, 8181–8219, https://doi.org/10.5194/gmd-15-8181-2022, 2022.

Schulz, M., Cozic, A., and Szopa, S.: LMDzT-INCA dust forecast model developments and associated validation efforts, IOP Conf. Ser. Earth Environ. Sci., 7, 012014, https://doi.org/10.1088/1755-1307/7/1/012014, 2009.

Comment B20

Lines 361: Please provide citations regarding which schemes the model uses for sedimentation, wet and dry depositions.

Response

As suggested, we have added the corresponding descriptions and citations for wet deposition, dry deposition, and sedimentation schemes in the revised manuscript (Lines 408–422). The relevant text is provided below for reference.

For the wet deposition (main text in lines 408–413):

“Wet deposition represents the removal of particles from the atmosphere through their incorporation into cloud droplets (in-cloud scavenging) or their removal by falling hydrometeors below the cloud base (below-cloud scavenging). The total wet deposition flux consists of two distinct mechanisms: convective scavenging following the schemes of Liu et al. (2001) and large-scale stratiform scavenging following the schemes of Giorgi and Chameides (1986) and Balkanski et al. (1993). The model sequentially updates the dust mass mixing ratio to reflect the removal during convective transport as well as both convective and stratiform precipitation.”

For the dry deposition (main text in lines 414–418):

“The dry deposition process is parameterized as a downward mass flux at the surface interface, representing the non-gravitational removal of particles onto land and ocean surfaces via turbulent diffusion, impaction, and interception. The dry deposition flux is calculated as the product of the dust mass concentration in the surface layer and its corresponding dry deposition velocity, which accounts for aerodynamic, quasi-laminar boundary layer and surface resistances (Hauglustaine et al., 2004).”

For the sedimentation deposition (main text in lines 419–422):

"Sedimentation refers to the gravitational settling of particles, governed by their terminal velocities determined by applying the Cunningham slip correction to the Standard Stokes velocity. To account for the settling of size-distributed dust modes, the model incorporates a Slinn correction factor (Slinn and Slinn,1980) as a function of the mass median diameter (MMD) and the geometric standard deviation (σ), as implemented in the LMDzORINCA framework (Hauglustaine et al., 2004)."

References:

Balkanski, Y. J., Jacob, D. J., Gardner, G. M., Graustein, W. C., and Turekian, K. K.: Transport and residence times of tropospheric aerosols inferred from a global three-dimensional simulation of ^{210}Pb, J. Geophys. Res. Atmos., 98, 20573–20586, https://doi.org/10.1029/93JD02456, 1993.

Giorgi, F. and Chameides, W. L.: Rainout lifetimes of highly soluble aerosols and gases as inferred from simulations with a general circulation model, J. Geophys. Res. Atmos., 91, 14367–14376, https://doi.org/10.1029/JD091iD13p14367, 1986.

Hauglustaine, D. A., et al. 2004. Interactive chemistry in the Laboratoire de Météorologie Dynamique general circulation model. Journal of Geophysical Research: Atmospheres, 109, D04314.

Liu, H., Jacob, D. J., Bey, I., and Yantosca, R. M.: Constraints from ^{210}Pb and ^{7}Be on wet deposition and transport in a global three-dimensional chemical tracer model driven by assimilated meteorological fields, J. Geophys. Res. Atmos., 106, 12109–12128, https://doi.org/10.1029/2000JD900839, 2001.

Slinn, S. A. and Slinn, W. G. N.: Predictions for particle deposition on natural waters, Atmos. Environ., 14, 1013–1016, https://doi.org/10.1016/0004-6981(80)90032-3, 1980.

Comment B21

Line 362: In particular, a big question is on how the model simulates deposition fluxes for the giant particles in mode 4. I don't think current dust models can simulate giant dust deposition and lifetime well. How do you make sure dust deposition and lifetime of mode 4 are reasonable? Have you done any evaluations in your current or previous papers? Please describe your deposition schemes in detail.

Response

We agree that simulating the lifetime of giant dust particles remains challenging in global models.

In our model, Mode 4 removal is dominated by gravitational sedimentation, parameterized via the Stokes–Cunningham formulation with a Slinn correction factor to account for the size distribution within each mode.

We evaluated simulated dust total deposition against a global dataset of 105 observational sites (Fig. 11). The results show better agreement in terrestrial regions near sources but underestimation over remote oceans (e.g., the Pacific; Figs. S27, S28). This pattern suggests that our model, like many current global models, tends to underestimate particle lifetime due to rapid gravitational settling for giant dust particles.

The size-dependent sedimentation treatment is described in the Methods (lines 420–422):

"To account for the settling of size-distributed dust modes, the model incorporates a Slinn correction factor (Slinn and Slinn,1980) as a function of the mass median diameter (MMD) and the geometric standard deviation (σ), as implemented in the LMDzORINCA framework (Hauglustaine et al., 2004)."

Further discussion is provided in Sect. 4 (lines 852–858):

"Finally, comparisons of simulated and observed dust surface concentrations and depositions highlight common deficiencies in global dust models regarding long-range transport efficiency of giant dust particles (Uno et al., 2009). In our simulations, the underestimation of dust deposition in remote downwind oceanic regions (e.g., the Pacific), contrasted with better agreement in near-source terrestrial regions (Fig. 11), suggests that the model likely underestimate particle lifetime due to overly rapid gravitational settling. Additional biases may also arise from under-resolved processes such as vertical lofting or uncertainties in regional source strength (Wu et al., 2020). These results underscore the necessity for improved representation of transport and removal processes to better capture the atmospheric lifetime and deposition of giant dust particles."

Comment B22

Line 468: Including vegetation effects itself changes the simulated regional dust variability. I wonder two things: 1) how would global dust look like had you not used DustCOMM to scale regional variability? 2) Does solely including vegetation makes your simulations agree better with the DustCOMM regional dust variability?

Response

We have analysed simulations without applying DustCOMM-derived rescaling factors (Figs. S13, S15).

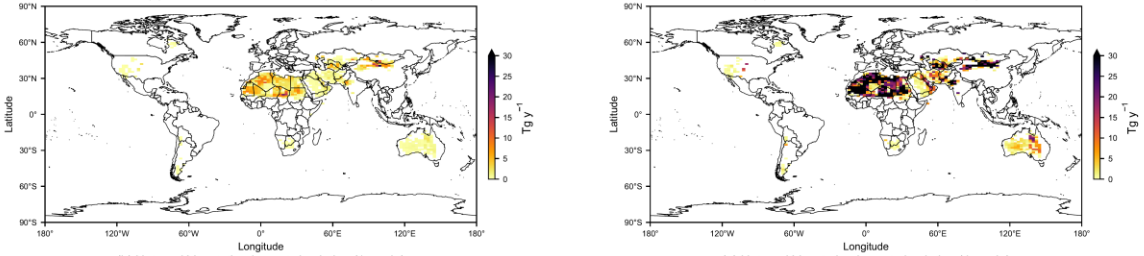
(1) Global dust emission without DustCOMM scaling:

Without DustCOMM scaling (Fig. S13), the spatial distribution remains broadly consistent with the tuned results, but emission magnitudes are lower in semi-arid regions (e.g., Central Asia, the Americas, Australia). Crucially, the relative reduction in emissions due to vegetation is identical in both tuned (Fig. 3b, Fig. S7d) and untuned cases (Fig. S13c, f) at the grid scale, as the same regional rescaling factors were applied to both simulations. This indicates that the quantified vegetation effect is independent of regional scaling and reflects a purely mechanistic response.

We have added the Fig. S13 in the supplement and revised the main text (lines 522–526): "The spatial patterns of global dust emissions without applying DustCOMM-derived rescaling factors (Fig. S13a–b, d–e) remain broadly consistent with those of the tuned simulations (Fig. 3a, Fig. S7a–c), although the untuned simulations show lower emission magnitudes in several semi-arid regions, including Central Asia, North and South America, southern Africa, and Australia. The relative reduction in local dust emission due to vegetation effects is identical between the untuned (Fig. S13c, f) and tuned cases (Fig. 3b) at the grid-cell level, as the same regional rescaling factors were applied across all simulations."

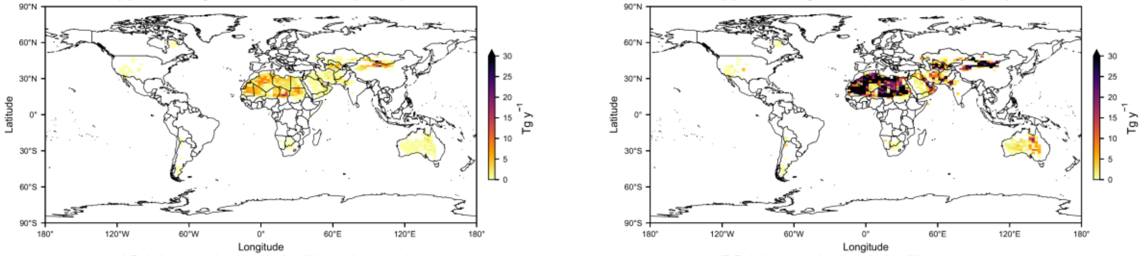
(a) Untuned Control simulation (1-mode)

(d) Untuned Control simulation (4-mode)



(b) Untuned Vegetation-impact simulation (1-mode)

(e) Untuned Vegetation-impact simulation (4-mode)



(c) Relative reduction in emission (Untuned, 1-mode)

(f) Relative reduction in emission (Untuned, 4-mode)

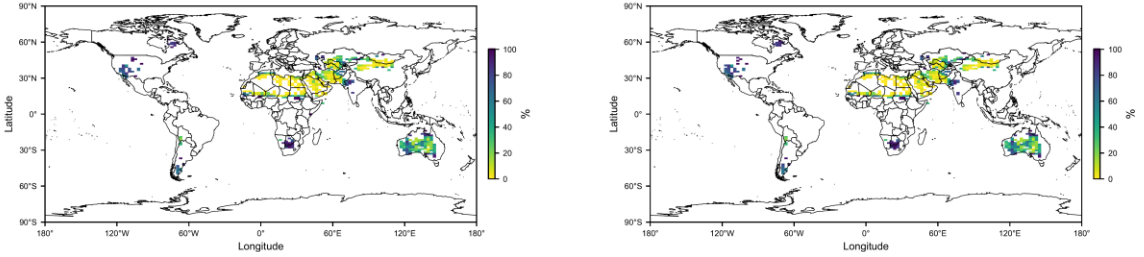


Figure S13. Simulated global mean annual dust emission fluxes (Tg y^{-1}) without regional rescaling. Panels (a–b) and (d–e) show the control and vegetation-impact simulations for the 1-mode and 4-mode configurations, respectively. Panels (c) and (f) display the corresponding relative reduction in emission (%) due to vegetation, calculated as (control–vegetation-impact)/control, in the 1-mode and 4-mode configurations, respectively.

(2) Impact of vegetation on comparison with DustCOMM (without tuning):

In the untuned simulations, including vegetation effects does not systematically improve agreement with DustCOMM (Fig. S15). While vegetation reduces emissions in regions where the control simulation tends to overestimate (e.g., the Sahel and East Asia), it further reduces emissions in regions where the model already underestimates (e.g., the Middle East and Central Asia).

This confirms that the remaining regional biases are primarily associated with structural limitations of the baseline model (e.g., source area definition) rather than the vegetation parameterization. Therefore, uniform DustCOMM-based scaling factors are applied to both simulations to provide a consistent observational benchmark.

We have added the Fig. S15 in the supplement and revised the main text (lines 542–546): "Furthermore, an evaluation of results without DustCOMM-derived rescaling factors (Fig. S15) reveals that the regional biases are inherent to the baseline model. In particular, the control simulation underestimates emissions relative to DustCOMM in several semi-arid regions, including the Middle East, Central Asia, and parts of the Americas and southern Africa. While vegetation modifies emission magnitudes and improves physical realism, it is not designed to resolve these underlying structural discrepancies. Improving source representation in semi-arid regions remains an important direction for future work.”

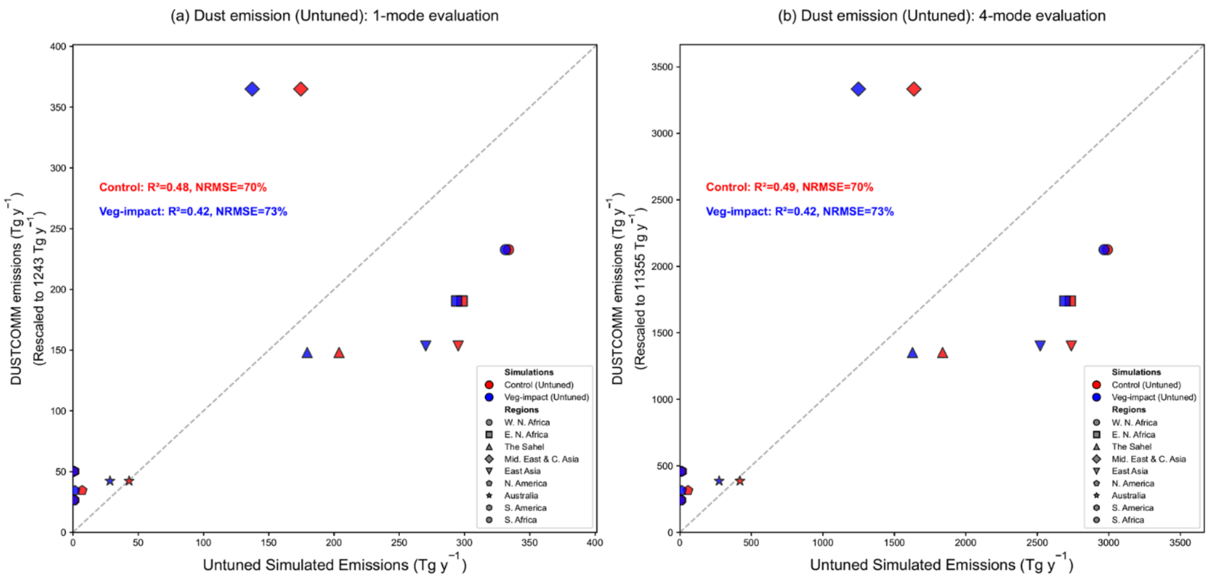


Figure S15. Regional evaluation of untuned simulated dust emissions against DustCOMM constraints (Kok et al., 2021b). Scatter plots for the (a) 1-mode and (b) 4-mode configurations compare emissions from the control (red) and vegetation-impact (blue) simulations without regional tuning. For direct comparison, DustCOMM emissions (y-axis) are rescaled to the global total of the untuned vegetation-impact simulation. The 1:1 line (dashed), R^2, and NRMSE are provided for reference.

Comment B23

Lines 471–472: I am a bit confused about this part: If you already applied DustCOMM-derived scaling factors to the control simulation, shouldn’t the red points in Fig. 4 be completely lying on the 1:1 line? Please clarify further.

Response

The regional scaling factors were derived based on the vegetation-impact simulation, which provides a more physically complete representation of the dust emission

To ensure a consistent comparison and avoid independent tuning, the same set of factors was then applied to the control simulation. Consequently, the deviations of the control simulation (red points in Fig. 4) from the 1:1 line explicitly reflect the regional biases arising from the absence of vegetation-induced aerodynamic effects.

We have revised the main text in lines 538–539 for clarity: “This is due to the application of rescaling factors derived specifically from the vegetation-impact simulation, thereby highlighting the regional biases introduced when vegetation effects are neglected.”

Comment B24

Line 484, Figure 4: Please show the regional dust variability from a control simulation without DustCOMM-derived regional scaling factors. Reader would want to know how much DustCOMM scaling factors have changed the regional variability of the control run.

Response

We have quantified the impact of DustCOMM-derived regional rescaling on the control simulation (4-mode configuration; Fig. S16).

The agreement with DustCOMM improves substantially after rescaling, with R^2 increasing from 0.49 (untuned) to 0.86 (tuned) (Fig. S16a).

The rescaling introduces the regional redistribution of emissions (Fig. S16b), with decreases of 21–76% in North Africa, the Sahel, and East Asia, and increases of 96–99% in North and South America and southern Africa. These adjustments reflect systematic regional biases in the baseline model.

We have added Fig. S16 in the supplement and added the text in lines 547–550: "Specifically, in the 4-mode control simulation (Fig. S16), compared to the untuned emissions, the rescaled results decreased emissions in North Africa, the Sahel, and East Asia by 21% to 76%, while substantially increasing emissions in North and South America and southern Africa by 96–99 % (Fig. S16b). Collectively, this calibration enhanced the control simulation's agreement with DustCOMM, with the R^2 increasing from 0.49 (untuned) to 0.86 (tuned) (Fig. S16a)."

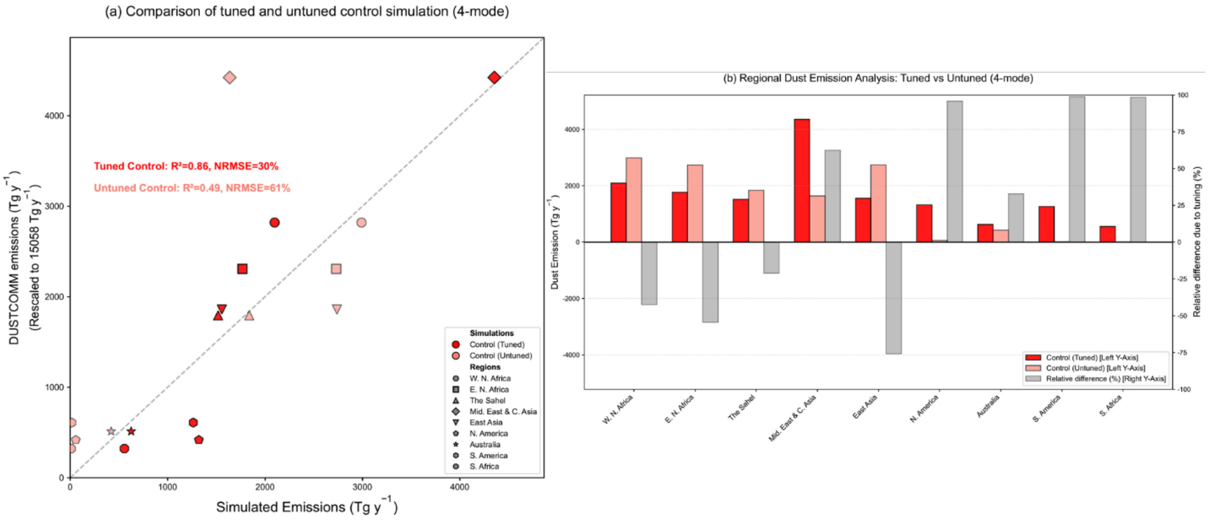


Figure S16. Comparison of tuned and untuned dust emission in the control simulation (4-mode configuration). (a) Regional evaluation of simulated dust emissions against DustCOMM constraints (Kok et al., 2021b) without (untuned; pink markers) and with (tuned; red markers) regional rescaling. The DustCOMM emissions were rescaled to the global total of the tuned control simulation for direct comparison. The 1:1 line (dashed), R^2, and NRMSE are provided for reference. (b) Regional dust emission magnitudes in tuned (red bars) and untuned (pink bars) control simulations (left Y-axis), and the corresponding relative difference due to tuning (grey bars; right Y-axis). The relative difference is calculated as (tuned−untuned)/tuned.

Comment B25

Ling 570, Figure 7: If you already corrected the regional dust emissions with DustCOMM-derived scaling factors, what is causing the current overall high biases in the DAOD simulations against the Kok et al. (2021) DAOD and AERONET–SDA AOT (Fig. 6)? Please comment in the main text. That is, I would like some elaborations on lines 556–560 on why these DAOD biases are present, given that you already applied the regional scaling factors on dust emissions.

Response

While DustCOMM-derived scaling constrains regional emission magnitudes, DAOD is a column-integrated quantity that depends not only on emissions but also on transport and removal processes.

Therefore, the remaining DAOD biases primarily arise from uncertainties in (i) large-scale circulation and convective activity controlling dust transport, (ii) vertical mixing and boundary layer structure affecting dust column loading, and (iii) meteorological fields influencing dust lifetime.

These structural limitations are independent of the emission rescaling and therefore cannot be corrected by tuning surface emissions alone.

We have expanded the discussion in the main text and Supplement (Sect. S2, lines 36–67) accordingly.

Main text in lines 635–642:

"Underestimations occurred in both major source regions such as Mali/Niger, Bodele/Sudan and southern Middle East, as well as downwind regions, including the African West Coast and mid-Atlantic primarily during JJA. These biases likely reflect deficiencies in simulating monsoon dynamics and associated convective processes, due to decoupling of dust–radiation interactions and unresolved sub-grid-scale haboob events (Pope et al., 2016; Balkanski et al., 2021; Bergametti et al., 2022). By contrast, the overestimations in the Kyzyl Kum and Gobi deserts during MAM and JJA might reflect insufficient long-range transport efficiency, leading to excessive near-source accumulation. Furthermore, the underestimation in the Taklamakan Desert suggests limitations in representing vertical lofting and atmospheric stability within topographically enclosed basins (Nan and Wang, 2018). A more detailed discussion of model–observation discrepancies is provided in Sect. S2."

References:

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Pope, R. J., Marsham, J. H., Knippertz, P., Brooks, M. E., and Roberts, A. J.: Identifying errors in dust models from data assimilation, Geophys. Res. Lett., 43, 9270–9279, https://doi.org/10.1002/2016GL070621, 2016.

Comment B26

Line 626, Figure 9: The differences in the model evaluation between the run with and without vegetation effects are overall modest. Why does vegetation effect have smaller impacts on PM10 than other dust variables? Please explain in the main text.

Response

We agree with the reviewer that vegetation effects have a smaller impact on PM_{10} than on other dust variables. This mainly reflects the spatial representativeness of the observational networks.

Most surface PM_{10} stations are located far from dust source regions, where vegetation-induced emission reductions are strongest. As a result, the vegetation signal is strongly attenuated during long-range transport and atmospheric mixing, leading to a weak response in surface concentrations.

In contrast, AERONET-SDA DAOD observations are more frequently located in or near dust source and transition regions, where vegetation-induced emission changes are more directly reflected in column-integrated aerosol loading.

Consistently, deposition observations—distributed across both land and ocean environments—show that vegetation-induced improvements are more pronounced over land than over ocean (Fig. S28), further supporting the spatial dependence of vegetation sensitivity across different dust-related variables.

We have added the explanation in the main text (lines 687–696) for clarity: "Accounting for vegetation effects led to a modest improvement in spatial correlation between the model and observations (Fig. 9), which is likely related to the spatial distribution of observational sites (Figs. 2b and 8a). Most surface PM concentration sites (Fig. 2b) are located in regions spatially distant from areas where vegetation-induced emission changes are most pronounced. As a result, vegetation-driven signals are strongly diluted during transport and mixing, leading to a weaker sensitivity in surface PM concentrations. In contrast, a larger proportion of AERONET-SDA (DAOD) sites (Fig. 2a) are situated in or near dust source and transition regions (e.g., the Thar Desert), where vegetation effects are more directly reflected in column-integrated dust loading. Consistently, dust deposition observations, which span both source and remote regions (Fig. 2c), show more pronounced vegetation-induced improvements over land than over ocean (Sect. 3.4), indicating that vegetation sensitivity is modulated by the spatial representativeness of observations."

Reference:

Mahowald, N. M., Engelstaedter, S., Luo, C., Sealy, A., Artaxo, P., Benitez-Nelson, C., Bonnet, S., Chen, Y., Chuang, P. Y., Cohen, D. D., Dulac, F., Herut, B., Johansen, A. M., Kubilay, N., Losno, R., Maenhaut, W., Paytan, A., Prospero, J. M., Shank, L. M., and Siefert, R. L.: Atmospheric Iron Deposition: Global Distribution, Variability, and Human Perturbations, Annu. Rev. Mar. Sci., 1, 245–278, https://doi.org/10.1146/annurev.marine.010908.163727, 2009.