

GENERAL COMMENTS

This is a promising investigation with interesting results that advance our understanding of dust transport mechanisms and their effects on modulating particle size distributions. This investigation provides particular understanding in bridging the gap between observations and physical parametrizations with regard to the transport of giant and coarse dust particles.

Ratcliffe et al. present a well-conceived set of sensitivity modelling exercises assessing the impact of several physical mechanisms on the deposition of coarse dust particles. The results are significant and relevant, and the investigation is clearly worthy of publication. To further strengthen the manuscript and ensure the findings are presented with the clarity they deserve, I recommend addressing several points before acceptance. I therefore suggest major revisions. My comments are as follows:

- The manuscript contains several nuanced and noteworthy results beyond the ~80% reduction in sedimentation highlighted in the abstract, discussion and conclusions (e.g., lines 301, 310, 321, 373, 451, 469). I would encourage the authors to draw more attention to these findings, as they collectively paint a richer picture of the compounded effects at play and cloud bring the community closer to reconciling observations with physical parametrizations. For instance, while not all of the sensitivity experiments directly improve the modelled coarse fraction relative to measurements, some of the changes yield improvements in the fine fraction, a result that is itself interesting and worth highlighting explicitly.
In light of this, I am missing a discussion that explores what a combination of these effects might look like. Although no single physically sound parameter adjustment produces the magnitude of change needed to fully match observations, a discussion of the compounded effects of multiple mechanism could provide insightful. What does the combined sensitivity look like, and which combination of effects yields the best overall agreement to the volume particle size distributions observed?
- The manuscript introduces several physical parameters, such as asphericity, topography and electric charging, that are not thoroughly revisited or in the discussion. It would greatly benefit the reader if the authors could offer at least a qualitative assessment on the expected influence of these parameters on coarse particle transport, even if a full sensitivity analysis is beyond the scope of this work. Similarly, a brief discussion of which combination of these parameters might be expected to yield the greatest improvements in the coarse fraction would be a valuable addition and would help contextualize the results within the broader modeling landscape.
- The physical reasoning motivating the sensitivity test on increased dust absorption in the shortwave (SW) and its expected connection to the retention of coarse particles would benefit from a more explicit explanation. As currently presented, the reasoning why a self-lofting dust plume would retain a greater proportion of coarse particles is not sufficiently developed. Additionally, the reported 1% increment is modest, and given the degree of variability inherent in free-running meteorological simulations, it is difficult to assess whether this difference is physically meaningful. If the proposed

mechanism relies on the assumption that a warmer, vertically elevated dust plume generates stronger updrafts capable of lofting a greater number of coarse particles, this should be explicitly stated and supported with evidence.

- Throughout the manuscript, the vertical mass concentration distribution is frequently referenced as a key diagnostic. To better contextualize these results, it would be very helpful to include an observational reference against which the modelled vertical structure can be evaluated. For instance, lidar-derived vertical profiles from the SALTRACE campaign (e.g., Haarig et al., 2019) could serve as a useful benchmark. Including such a comparison would allow the reader to assess whether the changes introduced by the various sensitivity experiments bring the modelled vertical structure closer to or further from observed conditions.
- More broadly, the manuscript would benefit from a deeper discussion of the physical reasoning behind the results. While the sensitivity experiments are well-designed and the results are clearly presented, the interpretation of why certain parameter changes produce the responses they do is often underdeveloped. For example, the finding that the absence of impaction scavenging leads to improvements in the fine fraction warrants a more thorough explanation.
- A few additional technical details would help the reader fully evaluate the experimental design and interpret the results. First, there is no description of the model's parametrization for convective mixing. Second, regarding the sedimentation experiments, while the authors vary the Stokes deposition velocity, is it not the only parameter affecting the dry deposition velocity calculation. A discussion of the role of the other resistances involved in the deposition equation and how they are expected to interact with the coarse fraction would make the overall discussion and interpretation of the results clearer.
- Finally, it is worth considering whether the underrepresentation of coarse particles in the model may stem, at least in part, from insufficient injection height at emission, if particles are not lofted high enough into the atmosphere, they may never reach the layers where drag forces would be sufficient to sustain them aloft. This idea is partially explored through the *2xCM* experiment, but the results and their implications are not discussed in enough detail to fully evaluate this hypothesis.
- As a final technical note, given the frequency with which Fig.S2 is referenced throughout the manuscript, it would be worth considering moving this figure into the main body of text.

SPECIFIC COMMENTS:

Line 1: The use of the term “coarser” warrants some clarification. Since coarse dust is generally understood to refer to particles above 2.5 or 3 micrometers, and given that a working definition is also presented in Line 21, it would be helpful if the authors could clarify their intended use of the adjective.

Line 26: The framing of this sentence would benefit from some additional context. Instead of leading with a time-based framing, consider opening from a theoretical perspective, noting that these large particles are not predicted to exist in the atmosphere, yet have been observed in measurements since 1988 (Betzer et al., 1988).

Line 66: The meaning of “oriented horizontally” is not entirely clear in this context. I would appreciate some clarification.

Line 106: While here the various processes governing the transport and deposition are outlined, asphericity does not appear to be revisited beyond this point. Given that particle shape can have a meaningful influence it would be worth including at least a brief discussion with regards to this parameter.

Section 2.1: What is the modelled output frequency?

Section 2.1.1: How is vegetation cover accounted for within this emission scheme?

Line 132: Could a brief description regarding the C and D coefficients be added?

Line 171: It would be helpful to clarify the relationship between R_B and the Stokes number and between the stokes number and the Stokes deposition velocity.

Line 175: A brief description of the relationship between the Cunningham slip factor and particle diameter would be a helpful addition.

Line 181: Could another coefficient be used to represent dust concentration? C is already used before.

Line 191: It is understood that the model employs the Fecan et al. (1998) parametrization for soil moisture, and it would therefore be helpful if the authors could clarify whether an additional tuning step is applied on top of this parametrization. If this is the case, a brief description of what it entails would be interesting.

Line 195: It would be worth considering moving Fig. S1 into the main body of the manuscript. If the authors agree, it would also be beneficial to include a brief discussion of the differences between this study’s configuration and the CMIP6 one, particularly with respect to the fine fraction.

Line 202: Some additional clarification would be helpful here. It would be useful to confirm whether the purpose of retaining the tuning factor is to increase the proportion of coarse particles in the atmosphere relative to what the untuned scheme would produce, and if so, whether this is indeed what is meant by the model being “affected by a fine bias at emission”.

Line 211: I would move this information to Section 2.1. where the model configuration is introduced.

Figure 3: It would improve readability if distinct line styles were used to differentiate the observational measurements from the model runs, as the current presentation makes it difficult to distinguish between the two at a glance. Furthermore, the *Control*, *noTM*, and *2xCM* are difficult to identify in the figure, if this is because the *0.5S* run produces very similar results, it would be worth to explicitly note this.

It would also be helpful to include a reminder in the figure caption that the locations are depicted in Fig.2

Figure 4: It could be merged with Fig.S2.

Line 330: The acronym MBL appears here for the first time, please introduce the full term.

Line 354: It would strengthen the discussion if the authors could offer some physical reason as to why finer particles appear to interact differently with the CM processes.

Line 358: Since different results were expected, i.e., that the lack of *CM* would result in weaker transport of coarse dust mass upwards within the SABL, could the authors more explicitly state why we see the results of Fig. 5?

Line 376: Size bin 5 experienced prolonged westerly transport.

Line 378: I do not see the concentration accumulation for all size bins.

Line 392: It is worth considering whether Figs. S3 and S4 are sufficiently informative or if they add new information to the manuscript to warrant inclusion.

Line 402: The phrasing of this sentence may inadvertently imply that the other sensitivity simulations are intended to be realistic representations of atmospheric conditions, which could be misleading.

Figs.5,6,7: The readability of these figures could be improved by changing the titles to express the differences in mathematical form, e.g., *NoTM – Control*.

Line 482: The supplementary figure referenced here may not be necessary, as a clear and concise statement of the result in the main text would be sufficient to convey this finding to the reader without the need for additional visual support.

Section 3.6: Interpreting the results presented in this section would be considerably aided by the inclusion of the modelled average vertical dust mass concentration profile, as without this reference it is difficult for the reader to contextualize the findings in relation to the typical vertical position of the SAL.

Line 503: The phrase “due to mostly positive values” does not constitute a physical explanation and it is a restatement of the result rather than an interpretation of it, which I would like to read.

Paragraph starting at line 509: It would be worth considering moving Fig. S6 into the main body of text. Conversely, Fig. S7 may not be necessary as a figure, since the values it displays are reported in the text.

Line 536: This would be a good place to reflect on the broader implications of the sensitivity results with respect to sedimentation. Specifically, it would be worth noting that despite the differences introduced through each models' *CM* parametrization and the inclusion of topographic effects, neither appears to substantially change the strong reduction needed in sedimentation to bring the model closer to agreement with observations.

Line 576: It is worth noting that the findings of this study extend beyond this single conclusion. As demonstrated in Section 3.1, the magnitude of the sedimentation reduction required to improve model performance varies between locations.

Line 585: Given the knowledge gained through these sensitivity runs and the wealth of relevant literature available, this section would be a natural place to attempt a synthesis of the compounded effects, which would further strengthen the manuscript's conclusions.

References:

Betzer, P. R., Carder, K. L., Duce, R. A., Merrill, J. T., Tindale, N. W., Uematsu, M., Costello, D. K., Young, R. W., Feely, R. A., Breland, J. A., Bernstein, R. E., and Greco, A. M.: Long-Range Transport of Giant Mineral Aerosol-Particles, *Nature*, 336, 568–571, <https://doi.org/10.1038/336568a0>, 1988.

Fécan, F., Marticorena, B. & Bergametti, G. Parametrization of the increase of the aeolian erosion threshold wind friction velocity due to soil moisture for arid and semi-arid areas. *Annales Geophysicae* 17, 149–157, <https://doi.org/10.1007/s00585-999-0149-7>, 1998.

Haarig, M., Walser, A., Ansmann, A., Dollner, M., Althausen, D., Sauer, D., Farrell, D., and Weinzierl, B.: Profiles of cloud condensation nuclei, dust mass concentration, and ice-nucleating-particle-relevant aerosol properties in the Saharan Air Layer over Barbados from polarization lidar and airborne in situ measurements, *Atmos. Chem. Phys.*, 19, 13773–13788, <https://doi.org/10.5194/acp-19-13773-2019>, 2019.