Overall response

We thank the reviewers for their in-depth commentary on our manuscript. We have made substantial changes to the manuscript as will be noted throughout our response. We have worked on improving the clarity of the language used and deepening the discussion of the results throughout the manuscript. Additionally, we have changed some of the figures used in the paper, including featuring more results from the observational campaigns with which to compare the model simulations to.

Below we provide a detailed response to the comments from each reviewer (shown in blue text).

Review 1

Ratcliffe et al discuss one of the recent (and yet unexplained) results in atmospheric dust science, "the mysterious long-range transport of giant mineral dust particles" (van der Does et al, 2018). We know from observations that coarse and giant dust particles are transported long distance, and we know from theory and models that this cannot be fully understood or explained. To address this problem, the authors use the HadGEM-3-GA7.1 model in a free running configuration, and explore how it represents the transatlantic transport of dust. In doing this, they modify the intensity of some processes in the model, such as the sedimentation rate, vertical mixing, scavenging or the absorption of solar radiation, in order to find in which conditions the model can sustain the long-range transport. The only significant modelled process able to modify the dust vertical distribution is sedimentation: a drastic reduction of this process (bringing its magnitude to one fifth of the control magnitude, i.e. a reduction by 80%) can significantly alter the transport of coarse dust and keep it sustained for significant transport distances. More precisely, the required reduction is 50% at the source, 50-80% at the Canaries and Capo Verde, and 80-95% at the Caribbean. Given that sedimentation is a well understood process, the authors say, in fact this means that there must be an additional force keeping the particles aloft, which has not yet been identified. Thus study moreover examines the role of the other processes on the vertical distribution of dust.

The research presented is interesting, significant, and relevant, and the authors have clearly invested considerable effort in their use of the HadGEM model. The study has the potential to merit publication; however, the current manuscript does not yet meet the standards of scientific clarity, structure, and analytical depth required for acceptance. I therefore recommend **major revision**. In the next round, I expect a **substantially rewritten mansucript**, not just incremental improvements. I would like the authors to make a major effort to elevate the scientific presentation, organization, and critical reflection on their results. With such a comprehensive revision the paper will be suitable for consideration.

Here are a few comments and suggestions:

1) One of the main results seems to be that a sedimentation reduction of 80% is needed to enable the long-range transport of coarse dust (see abstract, see lines 340, 528, and other places in the paper). However a more articulate result is given at line 301 with different reductions at different locations. I believe that the more articulate message would be more powerful, given that there is no overarching goal of coming with a simple number working everywhere to tune the model, but rather a desire to understand and quantify the model's shortcomings.

We have updated the manuscript to specify the range of reductions at different locations. For example, in the abstract, this has been changed to "*Reductions in sedimentation velocity of 50-*

95%..."(Line 7). And in Section 3.2, we have updated this to say (line 459): "*These results show that a sedimentation velocity reduction of at least 50-80% at the Sahara, Canaries and Cape Verde, and 80-95% at the Caribbean is needed...* "

2) It is unclear why only the 3xSW model run is compared to REControl whereas all other runs are compared to a Control without radiative transfer turned on. This being a clear (probably strategic) choice in the design of the research, it merits an explanation. It is only when I arrived at line 479 that I may have guessed (without being sure) the reasoning behind such choice: is it because you want to separate the variations in the meteorology from the strict dust processes? If this is a reason, then it should be not only stated in section 2, but an explanation of the pros and cons of this choice should be given.

When radiative effects of dust in the model are turned on, dust-radiation interactions are able to alter atmospheric heating rates, and therefore winds and meteorology, which in turn can impact dust emissions and dust transport. Therefore, when changes to dust transport processes are introduced (e.g. sedimentation, convective mixing, etc.), dust-radiation interactions are switched off in order to explicitly be able to attribute dust changes to the changes introduced. If dust-radiation interactions were switched on, this would not be so clear cut. However, when making absorption changes to the dust properties, it is necessary to enable dust-radiation interactions to enable the model to respond to these changes. This is why the 3XSW experiment is compared to REControl.

Additional explanation has been added in the methodology (Section 2.2) for clarification (line 292): "*When interacting with radiation, dust is able to influence radiative heating, and therefore regional meteorology, which subsequently impacts dust emissions, atmospheric dust concentration and transport. Therefore, most of the experiments are run with RE off in order to ensure identical emissions and meteorology and so that the impact of the sensitivity test is the only changing feature and can be directly assessed. The exception is the SW absorption sensitivity test, which requires radiative interactions to be switched on in order to allow the changes in dust absorption to affect radiation fields. This introduces the potential for different evolutions of atmospheric dust to impact circulation differently between REControl and 3xSW. This is discussed further in Section 2.1.2.*"

3) We need to remind that the authors' investigation is about the modelled dust in HadGEM3 and the processes represented in that model. Before results can be generalised to other models, one need to explain how similar or different such other models are. And in any case it would be good to have a language throughout the paper that clearly highlights that results are not for the "real" dust out there in the troposophere, but for the modelled dust which is a possible representation of it. It may be good to mention early on that given that each process investigated has a size-dependency (which is not altered in this study), the results obtained are valid for this specific modelling of each process (emissions, sedimentation, transport, mixing, radiation, etc.) which may be different than the processes in another model or in the natural world.

In the Discussion (Section 4), we have included the following to clarify, for at least sedimentation, that the results of this study are somewhat limited to the model used here, but corroborate results from other models (line 697): "*Our results are limited to the HadGEM3-GA7.1 model, so whilst we are not able to validate these results across models, which may use different atmospheric mixing and wet deposition schemes, we are able to see great similarity in the results of the reduced sedimentation experiments with Drakaki et al. (2022) and Meng et al. (2022).*"

We have added additional statements in the text to clarify which processes have a size dependence, and clarified that these are not altered, including the inclusion of the equation for Rb and St in Section 2.1.4 which are used to show the size dependence of turbulent mixing in the model.

4) At lines 528-533 the authors discuss how their results for HadGEM3 compare to those of Drakaki and Meng. At line 537 they state that the order of magnitude reductions are remarkably similar. I think it would be important here to (1) name the models used in those papers, (2) explain if those models use similar or different sedimentation schemes (and any other significative difference or similarity), and (3) discuss the significance of the similarity of results in light of the differences between models.

The names of the models are now included, along with further information about the differences/similarities between the three models described in the manuscript (line 686): "*Drakaki et al. (2022) and Meng et al. (2022) both employ an adaptation to Stokes law which is more representative for particles with an effective diameter greater than 10 μm. They use a drag coefficient suggested by Clift and Gauvin (1971), which is also used by Mallios et al. (2020) for representing prolate spheroids. With increasing size, the divergence between Stokes and the drag coefficient of Clift and Gauvin (1971) grows. Meng et al. (2022) use a Stokes correction factor which exceeds 10% for particles with d>45 μm (as described in Zender et al. (2003))*."

5) Improve the overall quality of the language. Although I am not a native speaker, some expressions come across as overly colloquial. In addition, the current phrasing at times seems to rely on the reader to infer or "guess" the authors' intended meaning, which should be avoided. E.g. guess some symbols (e.g. d in line 1 of the abstract and line 4 of the introduction: certainly it means diameter, but certainly this has to be stated). Guess some timescales (e.g. "relatively recently" on line 26: can the authors indicate a clear time range?). Guess some symbols as detailed in the comments below regarding the methodology section. Guess where is the land in figure 5 (line 356). There are many things in the paper which are unclear to a reader. Normally, a reviewer should not be required to identify so many instances where clarity is lacking. The expectation is that authors submit a well-written manuscript so that reviewers can focus on evaluating the science rather than the writing. In its current form, a full scientific assessment is not possible; a substantially clearer and better-written paper is required before the science can be properly reviewed.

We have improved the quality and clarity of the language used in the manuscript. Below are some of the specific examples mentioned by the reviewer that we have dealt with, though there are more throughout the text which we will not list here.

- "d > 2.5" at line 1 has been changed to "*diameter > 2.5 μm*" in the abstract and d is defined in the introduction (line 23): "(*diameter (d) < 2.5 μm*)"
- "relatively recently" has been updated to "*the last 20 years*" (line 36)
- Figures 7,8, and 9 (used to be 5, 6, and 7) now include a vertical dotted line to represent the west coast of Africa.

6) Concerning the figures, I'd suggest to bring figure 9 to section 2.1.3 where CM and TM are explained (with also some extra text to explain those concepts).

While we appreciate the reviewer's suggestion, we have opted not to move Figure 9 earlier in the text as the second and third panels are showing simplified results and conclusions from the simulations to aide in the final discussion of the results, and would complicate the article unnecessarily by introducing results too early. We have instead moved Figure 1, which shows a schematic of the processes mentioned (CM, TM, scavenging, etc) earlier in the text (into Section 2.1.3) to aide in the introduction and explanation of these processes. Added detail in the text, e.g. at line 211: "*In the analysed region in this study, TM is typically most active below ~1 km above surface level, and CM impacts on dust are most noticeable in the SABL and SAL as shown in Figure 1.*"

To bring figure S2 into the main paper given that it is mentioned often in the text: perhaps it could be incorporated into figure 4.

What used to be Figure S2 is now included in the main text as Figure 5. We kept Figure 4 (now Figure 6) and Figure 5 separated, as Figure 5 shows the total mass concentration from the experiments and Figure 6 shows only the mass from size bin 6.

Figures 5, 6 and 7 are of a similar type and the authors could consider combining them into one figure with several sub-panels.

Now refers to Figures 7, 8, and 9: We have decided to keep these figures separated as we feel that keeping them in the separate sections of the papers is helpful for the evolution of each individual process adjusted in the results section.

In figure 8 I am not sure if the difference of differences (panel c) is more at risk of creating a confusion rather than making things clearer: can the description perhaps be made without showing this panel?

This figure has been removed and replaced with the vertical profile of total mean dust concentration.

7) Also, consider well the figures in the supplement: are they really needed (an in that case would they be better in the main paper?) or are they not so important (and in that case they could be omitted all together? My personal feeling is that S1 could go into the main paper, perhaps included in figure 3; S2 as already said could go into the main paper. S3 and S4 are not so useful in my opinion and could be removed all together. S5 shows that emissions are stable and perhaps this could be simply mentioned in the text (without a figure). S6 is a nice figure highlighting a change in the vertical distribution only at the Caribbean and perhaps it could be kept and also added to figure 4. Finally S7 illustrates the atmospheric warming due to 3xSW, which is a nice point discussed in the paper, therefore it should also be included in the paper. Now, if you follow all my suggestions the paper will have some figures with many figure panels and you may want to go into detail and checking if a selection can be operated among those panels.

We have added, removed and changed some of the figures.

- Figure S1 has been moved to the main manuscript in Section 2.1.5 Tuning (now Figure 2). This figure was kept separate from Figure 4 (old Figure 3) as this already contains a lot of information, and the information on Figure 2 is best suited in the methodology.
- Figure S2 (now Figure 5) has been moved to the main text and now contains the observational profiles. This was kept separate from Figure 4 (now Figure 6), as one shows total mass concentration profiles, and the other shows only size bin 6 (20-63.2 um) dust mass.
- Figures S3, S4, and S5 have been removed.
- Figure S6 has replaced Figure 8 in the 3xSW experiment section (Section 3.6 Figure 10).

- Figure S7 (now Figure S1) has remained in the Supplementary Material as we feel that we can purely describe the quantitative values in the paper. As Reviewer 2 suggests: "Fig. S7 may not be necessary as a figure, since the values it displays are reported in the text".

8) The Conclusions section currently reads largely as a summary of the results. I would encourage the authors to expand this section with a more critical discussion of the implications of the findings and to place the work more explicitly within the broader context of existing research. In particular, it would be helpful to highlight the novelty of the contribution, discuss any limitations, and outline how the results advance or refine current understanding. Clarifying these aspects would make the conclusions more impactful and informative for the reader.

We have included the points suggested by the reviewer in the Conclusions section, where they have not already been covered in the Discussion section. Main findings of the paper are briefly summarized in the Conclusion, as the reviewer recommends, 'make the conclusions more impactful and informative'. We have also added a paragraph on modelling using size bins/modes.

For example, in the discussion, we now state at line 726 "*This study is the first time, to our knowledge, that all the processes in a model impacting the coarse particle lifetime have been tested with previous studies only focusing on the gravitational settling rate.*"

9) The fact that coarse and giant dust has been missed for a long time is also due to the past use of investigative methods unable to reveal it: instruments unable to reveal them and models unable to represent them. This was due in part to the fact that theory predicted these particles not to be there, therefore not justifying the additional effort. This has to be clearer in the wording of the paper. E.g. the expressions "were not frequently measured" (line 27) and "coarse particles are often disregarded" (line 43) do not convey this message and seem to imply an overall unjustified neglection of the coarse mode, whereas there has been a historical evolution of investigative methods and of our thinking. Please improve the language explaining why they were not measured (line 27) and why they were disregarded (line 43).

We have added the following in the first paragraph of the introduction (line 30): "*Coarse particles have been assumed to have relatively short, or even negligible, atmospheric lifetimes based on their larger diameter, and hence mass and calculated Stokes setting velocity. Therefore, due to sampling constraints due to instrument inlets and pipework (Rosenberg et al., 2014; Ryder et al., 2019), as well as the assumed short lifetime, coarse particles were not typically measured in observational campaigns).*"

10) Line 31: add references to Adebiyi and Kok (2020) and van der Does (2016); moreover, put papers in order of publication (oldest to newest).

We have added a citation to van der Does et al (2016) at line 38. As this sentence is referencing the observational results from field campaigns, not model underestimations or comparisons, the Adebiyi and Kok (2020) does not fit to cite here.

The citations have now been ordered alphabetically, as preferred by ACP.

11) Lines 45-47: the effect of constraining models to AOD observations is discussed, amongst others, in the conclusions of O'Sullivan et al (2020). I suggest to mention these considerations here.

We have added the following statement in the introduction (line 57): "*If a model experiences such imbalances in microphysical processes, the tuning towards observed AOD alone may result in the model having to compensate for these imbalances by altering the intensity of aerosol sources and sinks (O'Sullivan et al. (2020)).*"

12) Lines 49 and 58: there is a large difference between a 40% underestimation (line 49) and 11-fold underestimation (line 58). Such a difference in evaluation should be explained to the reader.

We have added further detail to this paragraph to clarify why these stark differences are reported. We have shifted the sentence pertaining to the Ryder paper earlier in the paragraph nearer to the mention of model dust size cut off (line 53): "*Ryder et al. (2019) found that often at least 40% of the dust mass lofted at the Sahara was greater than 20 um in diameter, suggesting models with size cut-off at 10-20 um will strongly underestimate lofted dust mass*."

We have also altered the description of the Ratcliffe et al (2024) results to be more comparable to others, using percentage differences instead of factor differences (line 70): "*Specifically, they found that between 2-3.7 km altitude at the Sahara, HadGEM3-GA7.1 underestimated dust mass contribution of 20-63.2 µm by ~40%, corroborating the value suggested by Ryder et al. (2019). The model underestimate increased with transport to 5-25% at Cape Verde (an underestimate of up to a factor of 240), and upon transport to the Caribbean, there was a negligible mass of 20-63.2 µm particles remaining in the model*."

13) The tuning parameters indicated at lines 193-198 all affect the dust at the time of emission, and leave the dust transport unaltered. Perhaps this should be clearly stated. Moreover, it would be useful to explain the effect of each tuning parameter on the emitted dust mass and the airborne PSD. Last but not least, to which observation was the tuning tailored? Fennec (line 195) was localised at some of the dust source regions (not all of them), whereas at line 198 tuning has been generalised to a much wider region (the Sahara and Atlantic).

We have included figure S1 from the supplementary material in the main text (new figure 2) and clarified the reviewer's points in the text.

We confirm that the tuning factors do not contribute to transport processes, and have added this to the text. We have included more details about the equations that the tuning factors are used in (Section 2.1.5). The tuning factor, k1, has the most impact on the emitted PSD. We include the following (line 263): "*Reductions in k1 result in reductions in the emitted fine fraction of dust; this is a consequence of the size dependence of $U*t$ which is a roughly U-shaped curve with a minimum at ~50 µm diameter (Bagnold, 1941). Thus, lower $U*$ predominantly impacts the finer particles which have a higher threshold friction velocity to overcome.*"

Additionally, we give more details on the observations used for tuning (line 275): "*The tuning is based on observations of near-surface dust concentrations (e.g., Prospero et al., 2010), AOD (from AERONET (AErosol RObotic NETwork; Holben et al., 2001) and MODIS (Levy et al., 2013)), and Fennec airborne in-situ mean dust size distributions over Mauritania and Northern Mali (Ryder et al., 2013b)*."

While we acknowledge that the Fennec results are not representative of all emissions sources in the Sahara, in this context we assume that they are generally representative enough for this study, and provide a rare observation of coarse dust PSD at low altitudes over the remote Sahara. We have added the following information in the manuscript (line 280): "*For this study, we assume that the Fennec data is representative of Saharan dust emissions. The tuning carried out for this study is* *based purely on the Sahara and trans-Atlantic dust transport, considering no other sources across the globe and is thus only suitable for the purposes of this study*."

14) Line 219: my understanding from what the authors wrote here is that for 0.05S-0.5S, the fraction 0.05-0.5 indicates a reduction in the Stokes velocity, not of the dust mass sedimented as the abstract and earlier paragraphs had suggested. Please clarify nicely this from the early stages of the paper.

This notation indicates a fractional decrease in the Stokes velocity in the sedimentation experiments. I.e. 0.05S indicates the Stokes velocity is multiplied by 0.05, or a reduction of 95%. This is also clearly defined in Table 2. The text in section 2.2 describes the reduction of the Stokes velocity, Vs.

The theoretical definition of aerosol sedimentation is widely accepted to describe the effect of gravity on downwards motion of aerosol particles between model levels, rather than deposition mass, which refers to the aerosol mass deposited to the Earth's surface *(Seinfeld and Pandis, 2016)*. To clarify this, the introduction now includes the following (line 130): "*In models, gravitational settling of dust is typically governed by the Stokes velocity, which controls the downwards transport of aerosol mass between model layers throughout the atmosphere as a result of gravity (Seinfeld and Pandis, 2016). This is distinct from the process of deposition, which defines the mass of aerosol removed from the atmosphere and deposited to the Earth's surface.*"

We have also clarified this by referring to it occasionally as 'sedimentation velocity'. Line 300: "…*we aim to test reductions in the sedimentation velocity (also simply sedimentation)*."

15) Throughout the manuscript, the authors use the term "coarser dust". In atmospheric science, the standard terminology is "coarse dust" (and, where relevant, "giant dust"). Using the comparative form of the adjective implies an unnecessary contrast (e.g., coarser than fine particles), which is not needed given that the authors already define the category by a diameter threshold (d > 2.5 um). I recommend replacing "coarser" with the neutral and commonly used term "coarse". The same comment applies to the use of "finer". Additionally, please ensure that each dust category is explicitly defined by a clear diameter range, e.g. when you use the word "fine" the first time in the paper indicate that you mean d < 2.5 um.

We have altered the wording used throughout the manuscript and include the following information at line 28: "*Henceforth, coarse, super-coarse, and giant size ranges will be referred to collectively as coarse particles for simplicity, though where appropriate, the individual size ranges will be specified*."

Additionally added at line 23, "*Fine (diameter (d) < 2.5 μm)*…"

16) There are many symbols and concepts in the methodology section that require an explanation. What are G and F for example? mass fluxes?

G and F are mass fluxes: G is the horizontal mass flux, and F is the vertical. Added additional information in the introduction of these variables (line 163): *"The horizontal (G) and vertical (F) dust mass fluxes (both in kg m−1 s−1) are derived using the method of Marticorena and Bergametti (1995). G is calculated in each of the nine logarithmically-spaced size bins (i) between 0.0632 and 2000 μm diameter"* why is there a horizontal dust flux that is different than the usual advection term in the atmosphere (concentration x velocity)? and where are the other quantities in equation 1 extracted from to apply the equation?

The horizontal mass flux (G, equation 1) is separate to atmospheric advection. The horizontal mass flux G is solely used to determine F, the vertical dust mass flux – that is dust emitted from the surface into the atmosphere, from where it can be transported and subject to advection via meteorology. The following has been added to section 2.1.1 (line 171), "*Although the horizontal surface dust flux (G) is calculated for 9 size bins, the vertical dust emission flux (F) is only calculated for size bins 1-6 (up to 63.2 μm diameter; Table 1) which are emitted into the atmosphere to undergo further transport and mixing. G is solely used to determine the vertical dust mass flux (F) for dust emission, and is not connected to transport in the atmosphere. Wind-related parameters in Equation 1 are extracted from model meteorology; bare soil fraction is extracted from model grid cell surface type*."

the authors also mention a constant (C) and a tunable parameter (D): give the quantities used.

Values of tunable parameters are now included in Section 2.1.5. Tunable parameter 'D' in Eq. 1 is now referred to as 'E' in the manuscript, and as 'C' is unchanged, we have used the fixed numerical value in the equation instead of 'C'.

It is also unclear how dust is redeposited to the surface (line 134) as I suppose that this is not yet the atmospheric sedimentation that the authors test, nor what happens to the dust that is emitted with d > 63 um.

We have removed the sentence, "Some of the particles lofted are redeposited to the surface within the same time step and never make it into the model atmosphere" to avoid confusion.

No dust is emitted d>63.2 um. The largest 3 size bins (i.e. where d>63.2 um, bins 7-9) used in the calculation of G (equation 1) are only subsequently required for calculation of F in equation 2 – i.e. horizontal surface movement of dust in size bins 7-9 impacts emissions in size bins 1-6. We have reworded the first mention of F and G for clarity (line 173), "*G is solely used to determine the vertical dust mass flux (F) for dust emission, and is not connected to transport in the atmosphere*"

17) I have similar questions for equation 2: where is the soil clay fraction taken from?

Added additional clarity after Eq. 2 (line 181): "*where Fc is the soil clay fraction based on Nachtergaele et al. (2008) which is constrained to a maximum of 0.1 based on Gillette (1979) and Alfaro and Gomes (2001), as described in Woodward et al. (2022)*."

and what is the reasoning behind the scaling factor (sum of G, i=1-9 / sum of G, i=1-6)?

Added additional clarity just before Eq. 2 (line 178): "*The total vertical mass flux is related to the total horizontal flux across all nine size bins according to the method of Marticorena and Bergametti (1995). The vertical dust mass flux (F) for i=1 to 6 follows the size distribution of the horizontal flux (Woodward et al., 2022)*."

18) Line 143: please give the prescribed values for absorption, scattering and asymmetry parameter. It is unclear how this works (I would assume absorption and scattering to vary with the dust amount and the particle size at least, not to be fixed and prescribed).

The total scattering and absorption coefficients (i.e. in m-1) do vary, since the scattering and absorption efficiencies given in table 3 have a dependency on the mass of dust in each size bin via the untis of m2 kg−1. This clarification has been added to section 2.1.2. In Section 2.1.2 'Dust radiative interactions' we now point to Table 3, which now also includes the values for mass scattering efficiency and phase function (line 193): "*The values for the mass scattering efficiency, mass absorption efficiency, single scattering albedo (SSA), and phase function are shown in Table 3 in Section 2.2*."

19) Line 151: please explain the conceptual difference between CM and TM. It may be beneficial to show figure 9 at this stage of the paper, to aid in explaining it.

As above in response to 6): While we appreciate the reviewer's suggestion, we have decided not to move Figure 9 (now Figure 11) earlier in the text as the second and third panels are showing a representation of results and conclusions described in the final discussion of the results. We have instead moved Figure 1 earlier in the text (into Section 2.1.3) to aide in the introduction of these processes. We have added some more details in Section 2.1.3 to help differentiate between CM and TM in the model (line 203): "*The convection scheme in HadGEM3-GA7.1 represents sub-grid-scale transport of heat, moisture and momentum.... The parameterised convection is responsible for both upwards and downwards movements of dust throughout the whole model atmosphere... In the analysed region in this study, TM is typically most active below ~1 km above surface level, and CM impacts on dust are most noticeable in the SABL and SAL. More details on the turbulence and convection schemes are in Walters et al. (2019)*."

20) Line 161: please clarify to the reader that impaction scavenging refers to the effect of cloud drops. Please clarify also if the dry deposition mentioned in the following line is due to TM + sedimentation together. Please clarify if the sedimentation velocity at line 164 is a vertical velocity pointing downward (like I would think).

Section 2.1.4 edited for clarity in the following ways:

- Added to clarify that the wet deposition occurs by precipitation (line 238): "*Wet deposition of dust can occur through impaction scavenging (IS)* <u>*by precipitation*</u> *below-clouds...*"
- We have added the following to clarify that turbulent mixing and sedimentation are both active near the surface for dry deposition of dust (line 216): "*Above the boundary layer, dust is subject to gravitational sedimentation at the Stokes velocity. Within the atmospheric boundary layer, gravitational settling is combined with turbulent mixing, according to Equation 3 (Woodward, 2001). Dust can experience dry deposition from the two lowest model levels in each time step through sedimentation and TM*."
- We also state now that the sedimentation we refer to is the Stokes velocity which has units in m s-1, which is stated in the text (lines 216 and 222): "*...gravitational sedimentation at the Stokes velocity.... Stokes deposition velocity (in m s−1)...*"

21) Line 180 sees some symbols which may be confused with previously used symbols. R could be confused with R_A and R_B of equatin 3, and C could be confused with C of equation 1 and C_c of equation 4. Moreover, here C is a dust concentration, therefore how does it relate to M_i in equation 1? I suggest to use consistent and unequivocal symbols throughout your paper, which may mean changing some symbols used in the references that you cite.

Some of the symbols have been changed to improve clarity:

- R in Eq.5 has been changed to P to avoid confusion.
- C in Eq.1 has been changed to its fixed numerical value (C=2.61).

22) In table 1, my understanding is that the representative diameter is the geometric mean of the bin edges. I suggest to clarify this in the caption.

Clarified in caption: "... *representative diameter (Drep) also known as the geometric mean diameter...* "

23) In section 2.2 the authors mention 8 simulations (line 209) and an additional one (line 210). For better clarity, I suggest to include the control runs in the simulation counts and thus there would be 9 and 2. The whole text in the next few lines would have to be adapted to this change.

Text has been altered (line 288): "*Including the Control, nine simulations have been performed in an RE off setup with changes to transport and deposition mechanisms (experiments listed in Table 2). Two simulations were run with RE on, REControl and one in which the SW absorption of dust has been tripled*"

24) Line 276: the authors refer to bin numbers but the figure does not show them. Please refer to diameter, or alternatively add the bin numbers in the figure, so as to better guide the reader.

Dashed lines representing the bounds of the size bins have been added to Figure 4 (old Figure 3), as well as numbers across the top of the first panel relating to the bin numbers as described in Table 1.

25) Lines 317-318: the terms MCA lands here in an unclear way for two reasons. First, the construction of the sentence make it sound like a property of the Control ("Control with MCA"); please construct the sentence better. Secondly, it is my understanding that the MCA in this article and in the reference cited (that I have not checked) is the mass median altitude (normally I would expect the centroid to be the centre of mass, not the median). Please consider adapting the term.

This sentence has been reworded for improved clarity and we have changed the acronym to 'mass median altitude' (MMA) at line 358: "...*all RE off experiments, and the dust mass median altitude (MMA) of the model experiments over the depth of the model atmosphere: the altitude at which 50% of the mass is below and 50% is above*"

26) in figure 4a only 4 lines are shown and in 4d there are only 3 (there should be 5 of them). In the caption the sentence about normalisation to bin 6 is unclear, and the sentence on showing only > 0.001 feels unnecessary given that there are no gaps in the data.

Now refers to Figure 6: The two missing lines on panel d are missing because they did not exceed the very low threshold mass concentration of 0.001 ug/m3. We have added additional clarification to the figure caption to avoid confusion. "*Model mass concentrations less than 0.001 ug m-3 are not shown, and consequently the Control and 0.5S experiments are not included on panel d.*"

27) Line 345: increase in mass concentration: an increase compared to what? is it a comparison between model runs? between the SABL and the SAL of a same model run? please guide the reader into looking at the same part of the figure that you are looking at.

The text has been clarified to specify that it has increased relative to the Control simulation. Line 466: "*an increase in mass concentration within the SABL and SAL* <u>*relative to the Control*</u>*... This increase within the SAL* <u>*in the NoCM experiment*</u>*...*"

28) Line 357: I cannot find the decrease above the SABL when looking at the figure. Please guide the reader's eyes.

We have defined the relevant longitudes to make this clearer (line 481): "*In size bins 5 and 6 (6.32-20 and 20-63.2 µm) over land (~17-0◦W), there is an increase in mass concentration in NoCM up to 4-5 km altitude, with a decrease in dust transported above these altitude*."

29) Line 376: I suppose the authors meant Westerly (not Easterly) transport.

Changed to "*westwards transport*"

30) Line 503: "due to the mostly positive values": I think that here you want to say that the positive numbers in figure 8c reveal that there is less loss of particles; however the way you wrote sounds like these positive numbers are the CAUSE (not the method for revealing) this effect. Please make the wording clear.

The figure that this sentence was describing has since been removed from the manuscript.

31) Lines 504-506: 1% and 5% sound negligible to me compared to what we would like to obtain. Are we sure that it is worth mentioning these very small numbers, that are probably smaller that their uncertainty?

As above.

32) Lines 513-517: the increase of the air temperatures is really interesting and perhaps could be better explained. At line 513, do you refer to the surface temperature or to the temperature at a specific altitude? At line 514, the term "central SAL" is a bit unclear; as you refer to the 3-4 km altitude why not say "at 3-4 km altitude, i.e. at the centre of the dust vertical distribution in the SAL"? Moreover the sentence at the end of the page about the temperature inversion could be better explained and expanded.

We have clarified this section by referring to the specific altitudes and altitude ranges (line 644): "*In the 3xSW experiment, there was an increase in air temperature of up to +1 K at the Sahara above 3 km altitude (Figure S1 in Supplementary Material). In the east Atlantic, the SAL experiences greater heating in the 3xSW experiment, with a maximum increase of +0.9 K at ~4 km altitude. At the Caribbean, air temperature was greater in the 3xSW experiment between 1.5-5 km altitude, with a peak of +0.7 K at 3.5 km altitude. These changes in air temperature are likely a direct result of the increased dust SW absorption influencing atmospheric heating rates*."

Review 2

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.

With regards to the mentioned findings, these have now been added to the discussion and have had more attention placed on them.

- Old lines 301 and 310: Refers to the more specific percentage changes of sedimentation required at different locations – throughout the text, w now use these variable changes instead of simplifying. E.g. In the abstract: "*Reductions in sedimentation velocity of 50-95%… with the required reduction increasing with transport distance*."
- Old line 321: refers to decreasing sedimentation increasing altitude of transported dust. We have now included observations on the associated figure (Figure 4), which further highlights the benefits of the raised transport altitude. We also include the following in the Section 4 discussion (line 675): "*The reductions to sedimentation also raise the altitude of the dust plume in the SAL, bringing it better into agreement with observations*." And in the Conclusions (line 801): "*Reducing sedimentation results in coarse particles being transported further and at higher altitudes*."
- Old line 373: refers to limited impact on bins 5-6 in CM and TM experiments due to strong sedimentation which dominates. In section 5, we have included (line 799): "*other tested processes were found to have minimal impact on coarse particle transport, but did reveal insights on the vertical mixing and fine particle transport.*" As well as further discussion in Section 4 (lines 725-754).
- Old line 451: Refers to increased size bin 5 particle concentrations in the NoTM experiment. This has been added to the discussion (line 751): "*Removing TM from the model did result in some additional size bin 5 transport towards the Caribbean in the MBL as they are not mixed down to the surface. This does not directly explain the presence of coarse particles in the SAL*

measured by the AER-D and SALTRACE campaigns, but this may suggest that TM could be acting too strongly in mixing dust down towards the surface."

- Old line 469: Refers to the impact of impaction scavenging on fine particles. This has been mentioned now in the abstract "*Impaction scavenging removal increases the mass concentration of fine dust.*"

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?

We have now added a discussion to Section 4 which discusses the potential for combining processes in future work (lines 775-787): "*Whereas in this study we only tested one process at a time, there is scope for future work to test the sensitivity of coarse particle transport to a combination of these processes. Additional testing could include combining the effects of reduced sedimentation with altered TM and CM in the SAL, or with increased SW absorption, which showed evidence of improved coarse particle vertical distribution. Since all the sensitivity experiments tested here showed some impacts on dust transport which were size-dependent, it would be useful for further work to investigate combined sensitivity tests. For example, a moderate reduction to sedimentation could be implemented (such as 0.5S) and combined with other dust transport adjustments. This would result in increased retention of dust in size bins 5-6, which should then enable the other altered processes to impact the coarse dust PSD evolution. This could then allow, for example, IS to increase dust VSD across the full size range, 2xCM to increase coarse dust within and above the SABL at more significant mass concentrations, or 3xSW to have a more significant effect due to the more absorbing nature of coarse dust. Additionally, testing the reduction of sedimentation of only the two coarsest size bins could be beneficial. Further theoretical and observational work to better understand how reduced sedimentation due to other processes, such as electrical charging or particle asphericity, may impact different dust size ranges, would be beneficial here in informing further experiments.*"

• 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.

We have significantly extended the discussion in Section 4, as partly shown above. We have also added discussion on the potential impact of these processes on coarse particle lifetime and the uncertainty that still exists (lines 709-724): "*Mechanisms which are not represented in GCMs, such as electrical charging, particle asphericity, may be responsible for the counteraction of coarse particle sedimentation in the real-world, however, it is still not fully understood how much of an impact these mechanisms play. The results of this work re-emphasize the importance of improving our understanding of these processes, since reductions to sedimentation were the only changes resulting in the necessary retention of coarse dust. Huang et al. (2020) estimate the impact of* *asphericity could increase coarse particle lifetime by ~20%, though this assumes that the particles are randomly oriented. In a scenario where the particles are horizontally oriented in the atmosphere they would experience increased drag, and subsequently increased lifetime (Mallios et al., 2020). However, at deposition buoys across the Atlantic, van der Does et al. (2018) found particles over 200 µm diameter were individual spherical quartz particles. In this case, this would suggest alternative mechanisms had resulted in the long-range transport, for example, electrical charging, which may transport particles at a greater altitude (Toth III et al., 2020) and over greater distances (Méndez Harper et al., 2022). Electrical charge may be retained for longer on rounder, more spherical particles, rather than aspherical ones which are more likely to experience charge accumulation and discharge in the narrower tips (which Griffiths and Latham (1974) found experimentally in ice crystals). Additionally, quartz particles are found experimentally to experience greater charging than clay minerals (Harrison et al., 2016, and references therein). It is hypothesised that charging of aspherical particles could result in orientation of the particles with the longest axis positioned vertically (Ulanowski et al., 2007). It is not fully understood how the effects of asphericity and electrical charging interact with each other to alter coarse particle lifetime*."

• 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.

In the model experiments methods section (2.2), we now explicitly refer to 'self-lofting' when discussing the 3xSW experiment. This now ties the model experiment more clearly with the self-lofting section in the introduction (line 326): "*…sensitivity to size resolved transport through <u>SW heating-induced self-lofting</u> in the model. Line 616: "Additionally, we have repeated the sentiments from the introduction in the results Sensitivity to Tripled SW Absorption Section 3.6: " As suggested in the introduction (Section 1), the SW absorption of dust heats the atmosphere causing positive buoyancy, which can lead to upwards transport of aerosol, a process known as self-lofting, which could enhance coarse particle transport (Boers et al., 2010; Colarco et al., 2014; Johnson and Haywood, 2023; Khaykin et al., 2022).*"

We have furthered our discussion of the 3xSW experiment in Section 4 (lines 650-664): "*In this experiment, absorption was specifically tripled to elicit a strong response, however, results show a relatively limited response of the dust. Colarco et al. (2014) tested the impact of different radiative properties of dust in a climate model. They found that their 'most absorbing' representation of dust radiative properties brought their climate model (NASA GEOS-5; Goddard Earth Observing System Model, Version 5) into better agreement with observations, by increasing dust transport altitude, westwards dust transport, and atmospheric lifetime. Between their control experiment and with the 'most absorbing' experiments, there was less difference in the dust radiative properties than between our RE-Control and 3xSW, yet the response of the changed dust properties appeared greater in their experiments than in our own. They attribute their findings to an enhanced longwave (LW) effect driven by including the representation of coarse particles (represented up to 10 µm diameter).*"

• 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.

We have moved the normalized vertical profiles from the Supplementary Material to the main article (new Figure 5), and also added profiles from the observational measurement campaigns referenced in the paper as an observational constraint in the four regions. Additionally, we now include observational profiles in Figure 6 (showing the vertical profiles of the sedimentation experiments).

• 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.

We have deepened our discussion throughout the manuscript, for exmaple, we include the following in discussion of the NoCM experiment in Section 3.3 (line 477): "*…the size bin 1 particles interact in a different way with the CM processes to the other size bins, the reasons for which are not clear. <u>We hypothesize that this could be a result of size bin 1 being particularly susceptible to impaction scavenging or Brownian diffusion once it has preferentially accumulated in the lower SAL.</u>* "

We have also included some extra detail about the impaction scavenging experiment (line 608): "s*ince IS is proportional to the dust mass concentration per size bin, IS removal will be weaker for the largest two size bins which contain less mass in the model Control experiment. This also means IS will be strongest in the fine range, and we hypothesise that this is why IS is so important at the fine size range*."

• 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.

We have added more detail to the introduction of the CM parameterization (line 203): "*The convection scheme in HadGEM3-GA7.1 represents sub-grid-scale transport of heat, moisture and momentum. The parameterised convection is responsible for both upwards and downwards movements of dust throughout the whole model atmosphere (Lock et al., 2000), and is based on the mass flux scheme of Gregory and Rowntree (1990). Downdrafts and convective momentum transport are also included (Gregory and Allen, 1991). The interaction of CM with dust is not size-dependent in the model… CM impacts on dust are most noticeable in the SABL and SAL. More details on the turbulence and convection schemes are in Walters et al. (2019).*"

In our sedimentation experiments, we reduce the Stokes velocity of the particles. We have made this clearer in the manuscript now. In Section 2.1.4, we make it clear that sedimentation impacts the dust throughout the atmosphere before explaining that turbulent mixing processes combine with sedimentation in the boundary layer (line 216): "*Above the boundary layer, dust is subject to gravitational sedimentation which is equal to the Stokes velocity. Within the atmospheric boundary layer, gravitational settling is combined with turbulent mixing, according to Equation 3 (Woodward, 2001).*"

We now include more details about the other resistances impacting particle deposition, including the equation for Rb (the surface layer resistance): Equations 4 and 5.

We also clarify in Section 2.2 (model Experiments) that our sedimentation experiments only alter the Stokes velocity (line 303): "*To vary the sedimentation, the Stokes velocity, Vs (appearing in Equation 3 for calculating the dry deposition velocity), is altered in the UM and JULES code.*"

• 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.

We have now included a paragraph in the Discussion (Section 4) which briefly discusses the results of a separate experiment that we carried out whereby we tested how the coarse dust would respond to injection at altitude at the Sahara. We have also included an extra figure in the supplement to compliment this discussion. We found that although it did increase the lifetime of the duts in the atmosphere, it was not by a great enough amount to result in the long-range transport we see in the observations. Lines 761-773: "*The higher dust particles are transported vertically upwards through the SABL, the longer their atmospheric lifetime is expected to be. Firstly there is a larger altitude range to overcome in order for deposition to occur, and secondly dust is more likely to transported to parts of the atmosphere with stronger horizontal wind speeds (typically the African easterly jet, peaking at around 700 hPa, with wind speeds up to 25 m s−1 (Chouza et al., 2016)), enabling faster westwards transport. This is part of the hypothesis behind our CM experiments; increased CM at the Sahara could transport dust particles higher. However, if there is something inherent in the model which inhibits this initial vertical mixing, the long-range transport of coarse particles will still be limited. An additional experiment (not shown here) tested the importance of particle injection height at the Sahara by injecting the model dust directly at 5 km altitude above the Sahara. We found that the time taken for 50% of size bin 6 dust (20-63.2 μm) to be deposited increased from ~3 hours to ~12 hours when the dust was raised (Figure S3 in the Supplementary Material). Transport time from the west coast of Africa to Barbados is expected to take 5 days or within the range of hours (Chouza et al., 2016). Therefore we conclude that sedimentation for super-coarse dust still dominates the transport and deposition processes in the model, even when dust is artificially injected high up in the SABL.*"

• 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.

Figure S2 (old) has now been moved to the main text (new Figure 5), with the addition of the observed vertical profiles. We have added additional discussion about this figure in the main 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.

We have now added the following (line 28): "*Henceforth, coarse, super-coarse, and giant size ranges will be referred to collectively as coarse particles for simplicity, though where appropriate, the individual size ranges will be specified*." And changed all instances of the use of 'coarser' in the manuscript. Originally we were using 'coarser' to refer to all three size ranges as 'coarse' by itself is defined as 2.5<d<10 um.

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).

This section of the paragraph has been altered following the reviewer's suggestion: "*Coarse particles have been assumed to have relatively short, or even negligible, atmospheric lifetimes based on their larger diameter, and hence mass and calculated Stokes setting velocity. Therefore, due to sampling constraints due to instrument inlets and pipework (Rosenberg et al., 2014; Ryder et al., 2019), as well as the assumed short lifetime, coarse particles were not typically measured in observational campaigns. In theory, a spherical dust particle of d = 30 μm at 5 km altitude in the Saharan atmospheric boundary layer (SABL) should be deposited to the surface in ~20 hours (Ryder et al., 2013a). Some observational measurements recorded long-range transport of coarse particles over hundreds or thousands of kilometres further than expected (e.g. Betzer et al., 1988), but it was not until the last 20 years that new research became focused on measuring these particles and understanding the quantity and size distribution of coarse particles in the atmosphere (Ryder et al., 2013b, a, 2018, 2019; van der Does et al., 2016, 2018; Varga et al., 2021; Weinzierl et al., 2009, 2017).*"

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

Clarification added (underlined) at line 85: "*If the particles were oriented horizontally <u>(with its longest axis parallel to the surface)</u> in the atmosphere…*"

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.

In the methods, when introducing the reduced sedimentation experiments, we now justify our reasoning for the experiment (line 301): "*Alterations to the sedimentation in this study are hypothesised to act as a proxy to processes or mechanisms in the real-world which reduce the settling velocity of the particles, such as asphericity and electrical charging*."

Additionally, based on comments by both reviewers, we have added a paragraph in Section 4 (Discussion), discussing further the potential impact of processes such as asphericity and electrical charging (Lines 708-724).

Section 2.1: What is the modelled output frequency?

The configuration used here has a calculation time step every 20 minutes. For our analysis, we use data output once per month that is averaged across the whole month.

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

Vegetation cover is calculated in the land surface model (JULES) and is incorporated in the emissions equation of dust in the form of B, the grid cell's bare soil fraction, in Equation 1.

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

Values of tunable parameters are now included in Section 2.1.5 Tuning. Tunable parameter 'D' is now referred to as 'E' in the manuscript, and as 'C' is unchanged, we have used the fixed value in the equation instead (C=2.61).

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

We have expanded section 2.1.4 to include the equations for calculating Rb and the stokes number (St), as well as giving more detail on the stokes number and stokes velocity (line 227): "*The Stokes number (St) is a dimensionless characterization of the behaviour of a particle in a fluid flow and represents the inertial effects, whereas the Stokes deposition velocity (VS ; used in calculating St) is the terminal velocity at which the particle settles within a fluid*."

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

Underlined text has been added (line 235): *"Cc has a dependency on Drep <u>and accounts for slip flow and can impact the deposition rate of fine particles below 1 μm diameter by up to 10%."</u>*

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

As the value of 'C' in Eq.1 is unchanged, we have changed equation 1 to show the fixed numerical value instead (C=2.61), so that C is now only used to represent dust concentration in Eq.5.

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.

There is a tuning factor to correct for the effects of spatial and temporal averaging, and obtain the soil moisture near the surface. This tuning factor, k2, is described in Section 2.1.5. In our experiments, it remains unchanged from the value used in the CMIP6 configuration. Though we have expanded our explanation of both k1 and k2 in Section 2.1.5 to include the equations in which they appear and impact the friction velocity and threshold friction velocity.

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.

Figure S1 has been moved to the main manuscript in Section 2.1.5 Tuning. Additionally, we include the values of the adjusted tuning variables.

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".

We have included the following to explain why we tune the emissions to give a better coarse dust representation (line 248): "*Ratcliffe et al. (2024) showed that CMIP6 configuration of HadGEM3-GA7.1 significantly underestimates the mass loading of coarse dust from emission and this is exacerbated with long-range transport. Thus, to better understand the causes of this growing underestimation, we have tuned the model's dust emissions to start with a better estimation of the real-world dust size distribution based on observations from the Fennec airborne field campaign.*"

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

This information has been moved to Section 2.1.

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

Now refers to Figure 4:

- In the figure caption, it now states: "*Where the Control simulation profile is less visible, it generally closely follows the NoTM experiment in all panels.*"
- Figure caption now also contains a pointer to the locations depicted in figure 3 (used to be figure 2): "The locations of the field campaigns and model box regions are shown in Figure 3."

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

We kept Figure 4 (now Figure 6) and Figure S2 (now Figure 5 in the main text) separated, as Figure 5 shows the total mass concentration from the experiments and Figure 6 shows only the mass from size bin 6.

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

Done.

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.

We have added the following statement to try to tackle this (line 478): "*We hypothesize that this could be a result of size bin 1 being particularly susceptible to impaction scavenging or Brownian diffusion once it has preferentially accumulated in the lower SAL.*"

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?

Now refers to Figure 7: We think this is because the dust is not mixed out of and away from the SABL as it might normally be, thus remaining within the SABL, increasing concentrations, especially at lower altitudes within the SABL. This has been included in a more in-depth discussion Figure 7 (line 468): " *Without CM the dust is mixed less evenly through the depth of the SABL and SAL, nor is it mixed away - either downwards from the SAL base towards the MBL or upwards from the SABL and SAL into the free atmosphere. This allows dust to build-up within the SABL and SAL, especially in the lower SABL and SAL, in the NoCM experiment, depleting the concentration of dust in the MBL and free atmosphere*"

Line 484: "*These results are somewhat counter-intuitive as we might expect that lack of CM would result in weaker transport of coarse dust mass upwards within the SABL, though the model results in Figure 7 show increased size bin 5 and 6 dust mass over the SABL in the absence of CM. The largest increases in mass for size bins 5 and 6 are closest to the surface, although we do see mass increases all the way up to the top of the SABL to 4-5 km altitude*."

Line 376: Size bin 5 experienced prolonged westerly transport.

Typically, in meteorology, air transported from the east is described as an easterly, hence why we used easterly in the text. However, we acknowledge this has caused confusion here and have instead opted for "*prolonged westwards transport*" instead (line 500).

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

This was originally referring to a percentage difference plot, where this statement was true. We have now altered the statement to be correct for the current plot.

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.

Figures S3 and S4 have been removed from the Supplement.

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.

We have amended this statement (line 532): *"...mixing the dust beyond the SAL top, whereas in reality, dust transport is typically capped by a strong inversion..."*

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.

Titles on the figures have been altered for clarity.

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.

This figure has been removed.

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.

We have made significant changes in Section 3.6 (Sensitivity to Tripled SW Absorption) which are explained over the following comments. We have now included Figure S6 from the supplement (new Figure 10) which shows the total dust mass profiles for the REControl and 3xSW experiments.

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.

This paragraph has now been removed.

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.

We have removed the figure previously in this section to improve clarity. We have replaced it with Figure S6 from the supplement which shows the lofting in total dust mass (new Figure 10). As suggested, we have removed Figure S7 from the supplement and instead only discuss the values 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.

We have deepened the level of discussion in Section 4 now to include further discussions, including the above-mentioned point (line 693): *"…despite differences in CM and TM parameterisations between the models substantial changes to the sedimentation are required. … Our results are limited to the HadGEM3-GA7.1 model, so whilst we are not able to validate these results across models, which may use different atmospheric mixing and wet deposition schemes, we are able to see great similarity in the results of the reduced sedimentation experiments with Drakaki et al. (2022) and Meng et al. (2022) across different models"*

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.

We now refer to the ranges of sedimentation reduction required at different locations rather than just citing ~80%. E.g., Line 801:"… *a reduction of 50-80% reconciles the modelled dust size distribution with observations at the Sahara, but in order to match observations at the Caribbean, a reduction of 80-95% was required. Sedimentation is a well-understood process, hence, we theorise an* *overarching issue exists in our understanding of the processes acting on coarse mineral dust particles in the atmosphere which is counteracting their sedimentation by ~50-95%"*

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.

We have greatly developed our discussion of the potential impact of compounded results in Section 4. Rather than extending the conclusions too much, we have opted to keep this in Section 4.

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 AerosolParticles, 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.