We thank the reviewers for their helpful comments in working to make this manuscript of publishable quality. We have responded (in blue text) to the comments provided by both reviewers (in black text) in the following document.

Reviewer 1

I thank the authors for their careful revision and for addressing my previous comments. The manuscript has improved significantly, and I recommend acceptance subject to minor revisions to address the following points:

1. It is not clear why the vertical profiles from the observations are not compared with the results from REControl and 3xSW. Given that this comparison would directly test the model's performance. and these profiles are already shown somewhere else in the manuscript, I would encourage the authors to include them or explcitly state why the comparison is omitted.

We have now updated Figure 10 so that it contains the observed profiles. Because the total mass concentrations vary significantly both between the aircraft campaigns and the observations and model data, we now show the data on a normalised axis. This disparity between the observations and model total mass concentration is discussed more in Ratcliffe et al. (2024).

2. The description of how the observed vertical profiles were derived is currently too brief. Please expand the methods to clarify the processing steps involved. As a result, the normalised total mass profiles in Fig.5 appear a bit "out of the blue". It would help readers to have a clearer description of how these are retrieved.

We have implemented a slightly more in-depth introduction to the aircraft campaign data in Section '2.3 Observations' from line 345 onwards: "To evaluate the model's vertically-resolved trans-Atlantic transport of mineral dust, we use in-situ observations of the full dust size distribution from three airborne field campaigns between the western Sahara and Caribbean… These profiles are used in this study to assess the vertical distribution of dust in the model simulations. All observed concentrations are at ambient conditions."

Reviewer 2

The authors have substantially improved the paper and followed the reviewer comments. The research presented is sound and significant. I have only a few

comments for improvement which could qualify as MINOR REVISION with no need to be checked again by the reviewers.

MAIN SUGGESTION:
Could the authors add an additional simulation with an added new size-dependent lofting term, tuned to match the observations, and give the magnitude of the lofting term per size bin? This could then tie to the discussion at lines 708-723.

Unfortunately, it is not possible for us to run any further simulations, and so we are not able to accommodate this suggestion at this stage.

COMMENTS CONCERNING THE GIANT PARTICLES:
The statement at lines 34-38 nicely explains the history of the neglection of coarse, supercoarse and giant particles. At lines 51-53 we read a similar statement for the historical neglection of such particles in numerical models. It would be more powerful to bring these two statements side by side and with the same underlying reason for the neglection which is their expected quick sedimentation.

We have now reworked this section, moving some sentences around to make the discussion of coarse particle neglect more cohesive.

Line 33 onwards: “Therefore, due to the assumed short lifetime, and sampling constraints associated with instrument inlets and pipework (Rosenberg et al., 2014; Ryder et al., 2019), coarse particles were not typically measured in observational campaigns. They are also often disregarded in global climate models (GCMs) for this same reason and the additional computational expense, resulting in most models not representing particles greater than 10 or 20 μm in diameter (Gliß et al., 2021; Zhao et al., 2022).”

At line 28 we read that "coarse, super-coarse, and giant size ranges will be referred to collectively as coarse particles". It is however to be noted that due to the model internal representation, giant particles are ignored in the simulations. In this form the statement is therefore misleading. Similar comment applies to line 339.

We have altered our statement at line 28 slightly by saying “particles with d > 2.5 μm” rather than the named size ranges. We have also clarified later in the paper that when we begin comparisons with the model, only particles up to 63.2 μm are considered, and thus our definition of ‘coarse particles’ is now limited to 2.5<d<63.2 μm, which does include a small rage of giant mode particles (d>62 μm).

Line 28: “Henceforth, particles with d > 2.5 μm (i.e. coarse, super-coarse, and giant particles) will be referred to collectively as coarse particles for simplicity, though where appropriate, the individual size ranges will be specified.”

Equation 2: please specify in the text that the ratio of sums in this equation redistributes the emitted dust mass into the model six internal bins

We have now added the italicised text to clarify this.

Line 177: “The vertical dust mass flux (F) for i=1,6 follows the size distribution of the horizontal flux (Woodward et al., 2022), *whereby the ratio of the sums (Σi=1,9 and Σi=1,6) redistributes the emitted mass into the six transported model bins.*”

Lines 249-253: it is unclear how the giant particles up to 300 um that were found in the Fennec campaign are accounted for given that the model only accounts for the coarse and super-coarse modes. Are the larger particles from the airborne campaign ignored, or is their mass redistributed into the model size bins?

In our study, we only compare the size ranges that are represented in the model (0.0632-63.2 μm) so that we can have a direct comparison between the model and observations.

In the caption of Figure 2, we have now added to say that the Fennec data is used “in the same model size bins (between 0.0632-63.2 μm)”.

Additionally, in Section ‘2.3 Observations’, we clarify that the campaign measurements have been limited to only analyse the modelled size range. Line 350: “Together the campaigns provide the full dust size distribution over 111 vertical profiles up to 8km altitude. Only measurements within the bounds of the model size bins are analysed for this study ($0.0632 < d < 63.2$ μm).”

Added in Section ‘2.1.5 Model Tuning’ at line 259: “Only particles observed within the model size range are used for comparison here (0.0632-63.2 μm).”

OTHER SUGGESTIONS:
Lines 72-73: "The model underestimate increased with transport to 5-25% at Cape Verde (an underestimate of up to a factor of 240)". This is unclear: is the dust mass at Cape Verde 5-25% or 1/240? The two numbers are very different! Also the word "increased" might be confusing given that it reduced from 40% to a smaller quantity.

Thank you for pointing out this sentence. I appreciate that it is confusing and that it was written while directly observing Figure 4 from Ratcliffe et al (2024). So as not to end up explaining the specific numbers in too much depth from this other paper, we have simplified our statement so that it only mentions the Sahara and Caribbean and is more qualitative.

Line 69 onwards: “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 diameter

particles to total mass by ~40%, consistent with the value suggested by Ryder et al. (2019). The model underestimate increased with transport as the coarse particles were more swiftly deposited in the model than the observations. Upon transport to the Caribbean, there was a negligible mass of 20-63.2 µm particles remaining in the model but observations confirmed the presence of these particles as they still made up ~10% of the total dust mass observed."

Lines 99-101: it may be worth reminding that the lofting of charged particles is expected to be the result of their interaction with the Earth's electric field.

Added phrase (in italics) at line 89: "… travel higher in the atmosphere with longer lifetime (Méndez Harper et al., 2022) *as a result of their interaction with the Earth's electric field*, and have been shown to…"

Line 94: this is the first occurrence of the acronym SAL, and it should be spelled out here (not at subsequent line 96.

Fixed.

Line 95: it is unclear what the authors indicate as "mixed layer". I suppose that this should be changed into "SAL"

Changed to "SAL".

Line 113: why is self-lofting a distinguished process than convective mixing due to solar heating of the atmosphere? I would have thought it to be a special case of CM.

We agree that there is some overlap, however, we have retained this as a separate point as we are specifically interested in the impact of the radiative heating of dust caused by the dust's optical properties as opposed to larger-scale convective mixing processes.

Line 173: specify which fixed SSTs have been used

The model uses observed SSTs (sea surface temperatures). Thus, we have updated the manuscript to include this detail (line 151): "The model uses fixed *observed* sea surface temperatures (SSTs) but is otherwise free-running"

There is an inconsistency at the bottom of page 8 and the beginning of page 9, where TM is described to be active in the BL and CM in the SABL: the SABL is a type of BL, isn't it? Maybe the authors meant TM for the MBL and CM for the SABL? Similarly, it seems to me that at line 249, the BL could be replaced with the MBL.

What we refer to as the model boundary layer, is the layer that the model's boundary layer scheme has diagnosed for the purpose of applying relevant turbulent mixing

processes driven by surface fluxes of heat and momentum. Over the Sahara the model boundary layer could be ~1.25km altitude, with the TM max height at 1.8km. However, surface heating can lead to dry convective mixing extending through a much deeper layer up to ~6km (referred to as the SABL). In this case, the model boundary layer height serves more of an identifier for where the character of mixing and application of sub-grid model physics processes change.

We have added the following in 'Section 2.1.3 Atmospheric mixing of dust' from line 210: "The top of the model BL is typically ~1.2 km altitude at the Sahara and between 0.5-0.7 km altitude over the ocean. The model-defined BL is not the same as (e.g.) the SABL, which is much deeper layer extending up to ~6 km latitude as a result of surface-heating-induced dry convective mixing at the Sahara (Cuesta et al., 2009). In this case, the model BL height identifies where the character of mixing and application of sub-grid model physics processes change."

Equation 3: I would add perhaps "in the MBL" or "in the BL" next to this equation and add a second equation V_D = V_S for the case of the SAL (and SABL?).

We have brought forwards the definition of Vs to clarify the difference and feel that the sentence following this, explains the difference between the Vd equations in and out of the model BL. Line 221: "Above the model BL and the turbulent mixing maximum height, dust is subject to gravitational sedimentation at the Stokes velocity (Vs ; in m s−1). Within and just above the model BL, gravitational settling is combined with turbulent mixing, according to Equation 3 (Woodward, 2001)."

Line 285: state the magnitude of the underestimation.

Unfortunately, it is not clear what the reviewer is referring to here. I think there is some mismatch between the referenced line numbers.

Lines 284-286 in the previous manuscript read as follows: "In order to test the sensitivity of coarse dust transport in the model, we have removed or scaled the impact of various relevant dust transport processes. Figure 1 shows the relevant processes in the model atmosphere impacting mineral dust transport and deposition."

Line 312: "these tuning factors" refer to the three of them (E, k1, k2). I suggest to be explicit about this or the reader may think that here the reference is only to k1 and k2.

Changed to "The tuning factors"

Table 3: the phase function is usually an angular distribution of scattering properties, not a single parameter. Maybe this is the asymmetry parameter? Please clarify.

Fixed the column title and caption to say “Asymmetry parameter”.

Line 351: are "20 June averages" the average for June for 20 years (wich ones)? or the average for a single date, the 20th June? please clarify.

As in Section ‘2.1 Model Configuration’, this is the monthly mean from 20 June months between 1995-2014. We have now clarified these details in Section ‘2.3 Observations’ at line 359: “We use 20 monthly June averages (1995-2014) from the model in order to create a model average representative of a climatology”

Lines 367-368: I would add "figure 5" in this sentence to guide the reader given that the text jumps between figures 4 and 5.

Added more explicit pointers to the relevant figures:

Line 369: “First, we compare the Control with the observations: the Figure 4 VSD shows that…”

Line 375: “In terms of the vertical distribution of dust, Figure 5 shows that…”

Line 729: what defines a comparison as "robust" or not?

In this case, we believe the comparisons are less robust due to the increased number of changed variables between the simulations (i.e., the meteorology, and dust emissions).