Reply to the Anonymous Referee #1

We thank the reviewer for the constructive comments, which helped to improve our manuscript.

Below are our detailed point-by-point replies and suggested manuscript improvements (blue) for each comment (black).

In this manuscript, Xie et al. run an offline mineral dust emission model forced by a significant suite of climate model simulations that look at the broad stripes of climate change across the Phanerozoic. Based on the results of these simulations, it is argued that the principal driver of dust emission across the Phanerozoic is the degree of continentality, i.e., times when there are big continents across the subtropics. Greenhouse gas levels are of minimal import. The manuscript compares its results with the distribution of evaporite records and finds some agreement ("fair" used in the abstract may be overly generous), which is considered sufficient validation of the model. I will note that the results about the broad movements in Phanerozoic climate align well with the results of Hu et al. (2023, https://doi.org/10.1038/s41561-023-01288-y) on monsoon strength, possibly because the underlying model experiments use similar approaches.

We agree that the words have been used for describing model-data comparison is not appropriate, in the revised manuscript, we will consider using more appropriate language for summarising the model-data comparison. We will also highlight the consistency with the results of Hu et al (2023).

The work presented in this manuscript is a valuable contribution to our understanding of the geological record of dust by providing a sort of null hypothesis testable against that record: that dust emission (and the level of global deposition) are driven mainly by variability in hydroclimate (and factors directly related to hydroclimate such as vegetation cover). The experiments presented in the manuscript are suitable to provide this null hypothesis. The manuscript is clearly written and well-organised. However, the manuscript takes little account of research on the geological record of dust, either in the recent or deep past. The outcome is that evaporite distribution is used as a proxy for dust emission. However, this type of validation is circular. Evaporite deposition is, of course, more demonstrably controlled by hydroclimate and continentality. So, of course, a model that has a response strongly controlled by hydroclimate and continentality is going to match the distribution of evaporites quite well.

We agree that the geological dust record referenced in the original manuscript is not an adequate review in support of the proposition that "deep-time dust is poorly understood", in the revised manuscript, we will provide a more detailed review of the deep time geological record of dust.

We will also make it much clearer in the revised manuscript that we are not considering evaporites as a proxy for dust emissions. Instead, we are using evaporite distribution as a method for evaluating the hydrological cycle in the model. The idea of the validation is that because there are limited geological records that can directly represent dust emissions, we here carry out an indirect comparison to evaluate our simulations. The simulated evaporites from HadCM3 are compared to the evaporite records, and this model-data comparison is intended to evaluate the performance of HadCM3 in simulating the hydrological cycle. Because our analysis shows that the dust emissions are controlled by arid regions, this is an important, albeit indirect, evaluation of the model. We do not believe that there is any circularity here.

The alternate hypothesis that these simulations should allow to be tested once further work is done to characterise the geological record of dust through time is that dust emission through time is a balance between two, sometimes competing factors. First, dust is mostly produced by physical weathering in glacial margin environments rather than “just there” or forming from the collision of desert sands. Second, dust is most easily emitted in dry, but not too dry environments, where vegetation cover is minimal but crusting together of small particles by evaporite salts is also minimal or reduced by occasional re-wetting. The upshot of this hypothesis is that dust emission increases gradually in icehouse climates as sediment availability increases, peaks in the early part of greenhouse climates (or in states of higher global continentality) following an icehouse climate, and then wanes through the course of long eras of greenhouse climate, as the dust (technically silt-sized sediment) reaches long-term sediment sinks, like the ocean. Because glacial grinding is a contested process, it is therefore very helpful to have a modelling study that removes the sediment from the equation and looking at change across the Phanerozoic. But it would be best if the manuscript gave the reader better information about the alternate hypothesis and the observables in the geological record. I therefore recommend that this manuscript be published after revision to better review the deep time geological record of dust and consider the inconsistencies the modelling approach in the manuscript might have with even the relatively recent record of dust.

We agree that weathering related to glaciation could potentially play an important role in dust emissions through its impact on surface particle availability. However, testing the importance of this factor requires higher-resolution data of glacial movement and melting, as well as topography, of the Earth’s deep past, which is very limited in availability. In the revised manuscript, we will state clearly that we are not including some potentially relevant processes, including sediment availability, and summarise some of these processes. We will also highlight the uncertainties that this gives our results and highlight this as a topic for future work in the revised manuscript.

I will focus first on how the modelling in this manuscript looks in the light of geological evidence from the Holocene and Pleistocene, provide some useful background on ongoing work in reconstructing the deep time record of dust, and then provide some minor comments on the manuscript line by line. The model in this manuscript is one of atmospheric dust emission. By tuning to the AMIP models, the manuscript is strictly speaking only treating the dust sizes (0.1-10 µm) typically represented in the AMIP models on the grounds of their relevance to atmospheric radiation/clouds and presence in dust deposition records distal from emission (such as ice cores). See Mahowald et al. (2006 , https://doi.org/10.1029/2005JD006653) or Mahowald et al. (2014, https://doi.org/10.1016/j.aeolia.2013.09.002) for further discussion of these points.

We will make it clearer in the revised manuscript that the total dust emissions are representing the emission within a fixed size fraction, equivalent to the AMIP model size range to which the model is tuned.

However, the best proxies for the geographic distribution of dust emission are proximal deposits of dust, where particle sizes are bigger (and much of the mass is concentrated geologically). These are known as loesses and can be 10s or sometimes 100s of m thick. Loesses typically have particle sizes ~50 µm. Thus, if the approximate atmospheric dry deposition lifetime of 5-10 µm is 1.7 days (Mahowald et al., 2006), the lifetime of loess particles is ~ 1 hour. Remobilisation complicates the picture, and the nature of sedimentary geology is for sinks to be few at any one time. But the upshot is that the geological record of dust emission is strongly concentrated within the bounds of wherever the wind can blow dust in a few hours (~ 100 km). So when faced with a figure showing the world's loess deposition in recent sediments, any model that connects dust emission to hydroclimate etc. rather than sediment availability is faced with a problem. (See Fig. 2 of https://doi.org/10.1016/j.earscirev.2019.102947) for a good example. Loess is abundant in mid-latitude and some high-latitude areas across North America and Eurasia, South America and parts of Australasia that include New Zealand. And it is relative rare along the southern margin of the Sahara. The model presented in the manuscript might explain the rarity of Saharan loess for the most recent time slice. (But where are the Pliocene loesses in the Sahara contemporary with the Eurasian ones?) But North American loess, European loess, and New Zealand loesses are inexplicable. This observation is the motivation for the idea that dust emission is shaped by sediment availability, with sediment primarily being generated by physical weathering at glacial margins (see discussion in Mahowald et al., 2006 and Prospero et al., 2002, https://doi.org/10.1016/j.earscirev.2019.102947).

The original motivation for this work was to explore the climatic impact of changing dust emissions through time, in particular in terms of changes in marine biogeochemistry and iron fertilisation. The ultimate aim was to produce "maps" of dust deposition over oceans that could ultimately be input into marine biogeochemical models. Hence our focus on the smaller longer-lived particle sizes. We agree that this means that we are not simulating larger particles, which are more evident in the terrestrial geological record. In the revised manuscript we will explain the reasons for this focus, and also the implications of this in terms of model-data comparisons. In particular, we will highlight that by not explicitly including dust sources associated with glacial processes, we are unlikely to correctly simulate high latitude dust sources.

The manuscript certainly demonstrates limited knowledge of dust records through geological time. At line 49, it says, "similar orbital-driven dust (or aridity) records are also identified much further back in time, as early as the Late Cretaceous 50 (Niedermeyer et al., 2010; Vallé et al., 2017; Zhang et al., 2019)." In fact, the most widespread evidence for orbitally-driven dust deposition is from the Late Paleozoic. A good place to start would be a recent review by Soreghan et al. (https://doi.org/10.1144/SP535-2022-208) and probably too recent for the authors to see (https://doi.org/10.2110/jsr.2023.122). The work by Lynn Soreghan suggests loess deposition was widespread, even at low latitudes in late Carboniferous and early Permian Pangaea. Loess also seems widespread in much of the Permian (where we can see), not just in places Soreghan and her collaborators have looked but in Russia (Mouraviev et al., 2015, doi: https://doi.org/10.1134/S0031030115110064). Loess deposition certainly continues deeper into the long greenhouse interval (see Chan et al., 1999, https://doi.org/10.2110/jsr.69.477), but the Triassic peak in dust emission by the model seems inconsistent with the record, where the places we would expect to see loesses are mostly sandstones and evaporites.

Many thanks to the reviewer for providing useful reference on the paleo dust records, we will make use of these suggested reference in providing an improved review of dust records and comparison with our simulations.

For the inconsistency mentioned by the reviewer regarding the Triassic peak: The location (approximately 54°W, 17°N, rotated according to the GPlate model for the upper Triassic continent) where loess sediment was identified by Chan et al., 1999, is simulated to be an area where desert dust and evaporites could form (see plots for 218 Ma in both Figure A4 and Figure A6). We do not believe this indicates an inconsistency between our simulation and geological records. Firstly, the spatial resolution of our simulations is significantly different from the actual length of the cross-section where the loess is found. Therefore, for a grid box where the model predicts desert/evaporite, we cannot assume that the entire land area of that grid box is covered by desert/evaporite. Similarly, we cannot deny the possibility that loess may exist within that grid area. Secondly, both the sediment record and model simulation times are approximations, both around 213 Ma, with considerable uncertainty regarding the exact age. Therefore, we cannot conclude there is an inconsistency based on the fact that the absolute ages of the sediment record and model simulation do not precisely align.

I still do agree with the general thrust of the manuscript that the deep time record of dust is poorly understood, but this should not be done without emphasising the importance of loess as a proxy for dust emission, some more expanded references to work in this area, and more nuance when comparing the model results with evaporite distributions.

Minor Comments

Line 47: This is probably where sediment availability and glacial grinding should appear as a hypothesis. Additional possible references are: Reader et al. 1999 (https://doi.org/10.1029/1999JD900033) and Sugden et al. (2009, https://doi.org/10.1038/ngeo474)

Yes, we will add words about sediment availability and reference in the corresponding paragraph.

Line 100: Omitting the Zender et al. formulation is justifiable if you are clear that you are trying to help test between hydroclimate/meteorology and sediment supply hypotheses.

We'll make it clearer in the revised manuscript that our model simulations cannot test the sediment supply hypotheses .

Figure 1: The notation of units in the legend/colorbar is confusing. Normally, you don't superscript the exponential notation, so there's a clear distinction between e-09=10^-9 vs. e^-9

We'll correct and uniform the units in figures.

Line 339: Desert has become dessert

Corrected.

Fig. A4 Part 3: 213 Ma looks like a plotting error. Please check.

It was a wrong plot. We'll update it in the revised manuscript.