

Clim. Past Discuss., referee comment RC1
<https://doi.org/10.5194/cp-2021-73-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on cp-2021-73

Anonymous Referee #1

Referee comment on "Simulating glacial dust changes in the Southern Hemisphere using ECHAM6.3-HAM2.3" by Stephan Krätschmer et al., *Clim. Past Discuss.*,
<https://doi.org/10.5194/cp-2021-73-RC1>, 2021

The authors present a manuscript describing changes in the dust cycle between pre-industrial and LGM climate conditions simulated by a state of the art atmospheric and aerosol model, where surface boundary conditions rely on a dynamic vegetation model that influences surface properties linked to the generation of dust emissions, also in different climates. This study is indeed a welcome contribution to the dust and paleoclimate research field, in particular providing elements to discuss how the dust cycle is influenced by climate conditions.

The manuscript is generally well written and figures are clear, the design of the study is well conceived, and the methodology and results are overall nicely described. However, I found some aspects that should be clarified and/or improved; in particular I would recommend that the authors provide a clearer and enhanced description of specific aspect of the methodology and of the comparisons with observations.

Specific comments

30-33: More precisely, dust scatters and absorbs both SW and LW radiation, although scattering prevails in SW (still, the single scattering albedo of dust is not equal to 1, e.g. Balkanski et al., 2007) and absorption in the LW (although scattering may be important too, e.g. Dufresne et al., 2002).

63-65: "We compare present-day simulation results to model results ...". Please rephrase.

165-169: It's not very clear to me what these regional correction factors are exactly, and how they are applied to the present study, to maximize the match with which observations and how, or what are they values. Please clarify the procedure in more detail.

176: "Since our simulation periods are comparably short"... compared to what? I do not understand this passage. I gather you use an atmosphere only model coupled to land surface scheme and consider prescribed SST for the ocean surface. Okay, so how does this sentence fit into that? Please rephrase.

213-214: This statement is essentially based on a set of global metrics compared to Huneus et al. (2011). It is true that the dust scheme is described in more detail Stanelle et al. 2014, and there validated against a wide set of observations of other features of interest for the representation of the dust cycle; however I would expect to see some comparison here too, with the current version of the ECHAM model setup, also because it appears that some tuning was done, and I found no reference to another paper describing it. The spatial patterns of dust emissions indeed appear to show some difference with Stanelle et al. 2014, also concerning the Southern Hemisphere. Please add some more information in this respect or an appropriate reference if that exists already.

261: Among the model factors affecting dust emissions surely there is also the vegetation cover, here simulated thanks to a dynamic vegetation model. I would suggest adding a panel showing a map of the vegetation fraction, or anyway a vegetation-related variable that closely resembles the way vegetation affects dust emissions in the model.

283: The observational data used for figure 3 do not appear to correspond to the original DIRTMAP dataset (i.e. Figure 8 in Kohfeld and Harrison, 2001). Please make sure that you add a reference corresponding to the actual version of the dataset you used, and specify whether additional data were included.

283-315: Several data points in the Southern Ocean appear to be south of the Polar front, which should raise a flag about non-aeolian contributions to the terrigenous fraction of the sediment, and therefore the opportunity to use these data for a robust estimation of dust mass accumulation rates (e.g. Kohfeld and Harrison, 2001).

352-356: There is a substantial difference in the experimental design of Albani et al. (2012 and 2014) and this work; here it appears that the amount and proportions of dust from different sources result only from the model itself (and indirectly the regional tuning on dust emissions made on present day conditions, apparently), whereas the cited work explicitly used regional tuning also for the LGM, in a data-assimilation fashion, in order to obtain a match on dust amounts, LGM/interglacial ratio, as well as source mix based on geochemical fingerprinting on Antarctic ice core samples (e.g. Delmonte et al., 2010). In other words, one could say that the CAM3 results that you mention indicate a dominance of South American dust because ice core data suggest just that, of course under the

assumption that simulated transport and deposition can be considered reasonable.

352-368: Based on my previous comment, I would recommend that a more thorough discussion is carried out considering also the available data on dust provenance. It is indeed very important that you explain your results based on the modeled processes, as you did, but I believe that they should also be put more in perspective by comparing them to observational evidence, also for this particular aspect (which by the way you mention later on while discussing the matter of size, and you also acknowledge in the conclusions).

412-414: Is there a variability on size distributions at the stage of dust emissions in your model formulation? I don't think so, so I'm a bit confused, why would you expect that?

472-474: Where does this come from? This aspect is not shown or discussed anywhere in the text.

478-479: I would suggest adding two lines bracketing the +/- 1 order of magnitude in the scatterplots of Figure 3, for a clearer reading.

500-504: I would recommend that these considerations on the chosen boundary conditions are also reported in the methods and/or results sections, as appropriate.

466-504: I would suggest enriching a bit the conclusion section with references to the literature, where appropriate.

References

Balkanski, Y., Schulz, M., Claquin, T., & Guibert, S. (2007). Reevaluation of mineral aerosol radiative forcings suggests a better agreement with satellite and AERONET data. *Atmospheric Chemistry and Physics*, 7, 81–95.
<https://doi.org/10.5194/acp-7-81-2007>

Dufresne, J. L., Gautier, C., Ricchiazzi, P., & Fouquart, Y. (2002). Longwave scattering effects of mineral aerosols. *Journal of the Atmospheric Sciences*, 59, 1959–1966.
[https://doi.org/10.1175/1520-0469\(2002\)059<1959:LSEOMA>2.0.CO;2](https://doi.org/10.1175/1520-0469(2002)059<1959:LSEOMA>2.0.CO;2)

Delmonte, B., Andersson, P., Schöberg, H., Hansson, M., Petit, J. R., Delmas, R., Gaiero,

D. M., Maggi, V., and Frezzotti, M.: Geographic provenance of aeolian dust in East Antarctica during Pleistocene glaciations: preliminary results from Talos Dome and comparison with East Antarctic and new Andean ice core data, *Quat. Sci. Rev.*, 29, 256–264, doi:10.1016/j.quascirev.2009.05.010, 2010