Reply to the 1st anonymous referee's comment on

Krätschmer, S., van der Does, M., Lamy, F., Lohmann, G., Völker, C., and Werner, M.: *Simulating glacial dust changes in the Southern Hemisphere using ECHAM6.3-HAM2.3*, Clim. Past Discuss. [preprint], https://doi.org/10.5194/cp-2021-73, in review, 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.

Dear referee,

thank you very much for reviewing our manuscript and the helpful suggestions you provided. Please find below our replies to your comments.

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).

We will rephrase the sentence as follows:

"During transport, the dust particles directly influence Earth's radiation budget by scattering and absorbing short- and longwave radiation depending on particle size and mineralogical composition (Dufresne et al., 2002; Balkanski et al., 2007), which in turn affects the atmospheric stability by altering the vertical temperature profile and relative humidity (Boucher, 2015)."

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

We will rephrase the sentence as follows:

"We compare our present-day simulations to results obtained in the scope of the global dust model intercomparison in AeroCom phase I in order to […]"

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.

The regional correction factors are a natural consequence of the parameterization of a sub-grid process on a mm scale in a model running with a typical resolution of 100 km and is essentially a mean to compensate for the lack of required information for an exact calculation of the considered process. A precise explanation can be found in Tegen et al. (2019):

> "In previous versions, a global correction factor of 0.86 was applied on the threshold friction velocity to account for the inhomogeneity of the factors influencing dust emissions (e.g., surface

wind) across the rather coarse model grid boxes. In ECHAM6.3 the surface orography is not taken into account for the aerodynamic surface roughness, in contrast to earlier versions. The subsequent changes in surface wind distributions over dust source areas require additional regional correction factors. For each relevant region that contains dust sources the correction factors are chosen such that the emissions agree with the values by Huneeus et al. (2011). These regional correction factors can be modified via the model namelist."

We will include some of the information above on the correction factors as well as the according reference in our revised manuscript.

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.

This sentence has been used to emphasize that the temporal change in the interaction (more precisely, the heat exchange) between ocean and atmosphere can be neglected due to the much higher inertia of the ocean surface, i.e. $\tau_{Oc} \gg \tau_{At}$. Since taking into account the spatio-temporal development of SIC and SST would imply coupling a complete ocean/sea-ice model to our current setup, this approximation saves a significant amount of computational resources. Instead, a constant external forcing file of SSTs representative for the considered time period is prescribed.

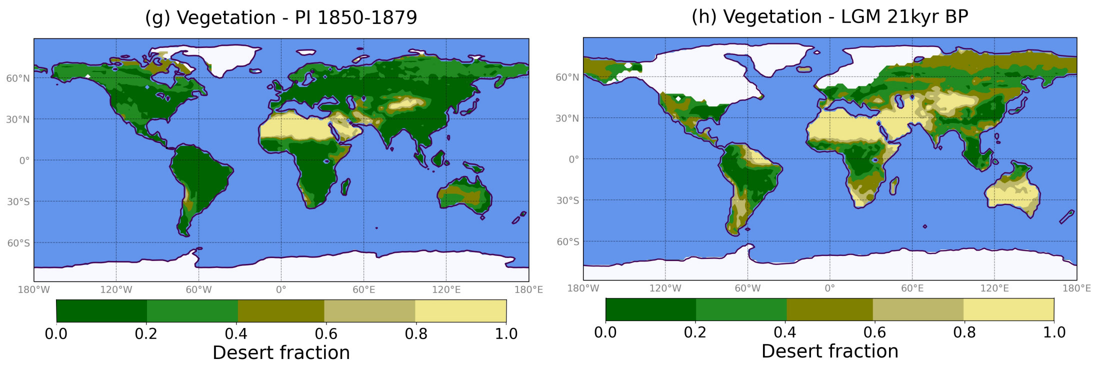
213-214: This statement is essentially based on a set of global metrics compared to Huneeus et al. (2011). It is true that the dust scheme is described in more detail Stanelle et al. 2014, and there validated against a wide set of observations of other features of interest for the representation of the 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.

Since it turned out that the model version already came with a set of tuning factors matching the results found in Huneeus et al. (2011) for present-day conditions, we did no further tuning. The tuning factors were only changed in the scope of the provenance studies. Stanelle et al. (2014) only shows the emission flux for present-day (PD) and the anomaly PD-historic, while we show the emission flux for pre-industrial (historic) conditions and the LGM, which makes a direct comparison rather difficult. However, comparing the PD plot of Stanelle et al. (2014) to our PI plot, we can still recognize the typical dust source areas and emission patterns, in particular in the Southern Hemisphere.

Concerning changes in absolute values for dust emission etc., it needs to be emphasized that Stanelle et al. (2014) used the model version ECHAM6.1.0-HAM2.1-MOZ0.8 for their simulations, while we used ECHAM6.3.02-HAM2.3-MOZ1.0 for our study. Besides the changes in the mineral dust emission scheme already addressed above, further changes in the model include in particular modified aerosol-cloud interactions (Tegen et al., 2019). Due to the full coupling of HAM2.3 to ECHAM6.3, all those changes have eventually an effect on regional, and thus global, dust emissions. Since the aim of our study is completely different from Stanelle et al. (2014), a thorough comparison between results obtained with the outdated model setup from 2014 and our new setup is beyond the scope of our study.

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.

Thank you for your suggestion, the dust emissions are indeed affected by the dynamic vegetation model. Please find in the following two maps showing the simulated deserted fraction of each grid box for PI and LGM as an addition to Figure 2.



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.

Thank you for the hint! The correct reference is:

K.E. Kohfeld, R.M. Graham, A.M. de Boer, L.C. Sime, E.W. Wolff, C. Le Quéré, L. Bopp: Southern Hemisphere westerly wind changes during the Last Glacial Maximum: paleo-data synthesis, Quaternary Science Reviews, Volume 68, 2013.

No additional data were included. We will correct the reference in our revised manuscript.

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).

Although data from "[…] marine sites that have been flagged because they are located within zones of thick nepheloid layers and ice-rafted detritus, which can contaminate aeolian signals […]" had already been excluded from the dataset we use for comparison (Kohfeld et al., 2013), we agree that the reconstructed detrital flux estimates based on changes in ^{232}Th might still contain non-aeolian contributions from glacier erosion and riverine input, which are not considered in our model. Additionally, it should be taken into account that we compare (simulated) aeolian dust deposition fluxes onto the ocean surface to marine sediment data, i.e. also any horizontal dust transport processes in the ocean during sedimentation are not considered. We will point this uncertainty out in the discussion section in our revised manuscript.

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.

We agree! Albani et al. (2012) found the dominance of South American dust only because they tuned the dust emissions in their simulations "for each macro-area […] a posteriori by applying a factor yielding the best fit between the simulated and observed LGM and current deposition rates […]" and is thus not suitable to be used as a reference indicating contradicting model results compared to our simulations. We will adjust our argumentation accordingly in our revised manuscript.

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).

We will point out more clearly in our revised manuscript that our model results are not intended to question the geochemical data regarding the provenance of dust found in Antarctic ice cores.

Additionally, we will include in the discussion section that the reconstructed dust fluxes used in our study for comparison with our simulation results (DIRTMAP, Kohfeld et al. (2013)), which are based on the assumption of relatively constant proportions of ^{232}Th in continental lithogenic materials, might be overestimated by 30–40 % in regions receiving fine-grained dust from Patagonia and Australia (McGee et al., 2015). The study of Trudgill et al. (2020) supports our finding of Australia being the predominant dust source during the LGM for dust deposited in the SW Pacific, however, they also suggest based on their grain-size analysis of sediment cores from the Tasman Sea that these might contain non-aeolian contributions, more precisely fluvial sediments from New Zealand, which are not considered by our model and might partly explain the discrepancy between our model results and the observational data.

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?

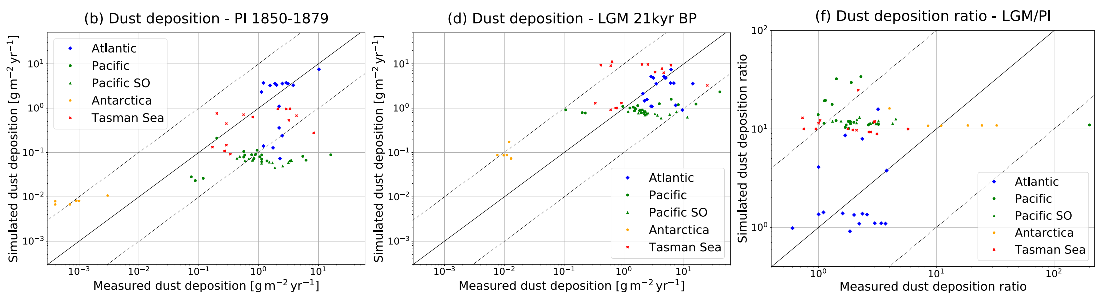
Ice core data from Greenland (Steffensen, 1997) and Antarctica (e.g. Delmonte et al., 2004) indicate the dust deposition of varying particle size distributions during glacials compared to interglacials. Since we also find a change in dust particle size during the LGM compared to PI (in particular over Antarctica), this formulation has been chosen to point out to the reader that although one might expect that the model exhibits this change in particle size for physical reasons and thus might yield a possible explanation for the according observational data, it is caused for a different reason. Considering the confusion this formulation has apparently caused, we will rephrase this sentence accordingly.

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

We agree! It was not mentioned at an earlier point in the text. However, this were the findings of Stanelle et al. (2014) using an older version of the model, so it can be considered very likely that the same findings can be attributed to the same causes. We will include the according reference in our revised manuscript.

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

Thank you for this suggestion, please find below the accordingly adjusted scatterplots.



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.

We agree! We will include the considerations about the potential influence of the prescribed sea surface temperatures on our simulation results already in the discussion in section 3.2.

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

Since we do not bring up new aspects to the discussion in our final paragraph *4. Conclusions*, in particular after moving the considerations about the potential influence of the prescribed sea surface temperatures on our simulation results already up to discussion section 3.2, we tend to not give any references in the conclusion section at all because those relevant for our paper are already mentioned in the discussion section.

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