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Climate of the Past Discussions

Interactive comment on "Marine sediment records as indicator for the changes in Holocene Saharan landscape: simulating the dust cycle" *by* S. Egerer et al.

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Response to Anonymous Referee 1

We would like to thank the referee for the detailed comments on the paper.

General comments

"The observational dataset used for this work is not the state-of-art. Recent work on

paleodust compilations has a better temporal resolution, and showed the importance of particle size distributions for the interpretation of deposition data. The analysis should be updated by comparing to this type of information that has become readily available. Potential model biases in the simulation of the physical climate may be relevant to the discussions, but they are not reported here. This aspect should be taken into account in the interpretation of the results."

We thank the referee for highlighting an additional study including an updated observational dataset. We refer to this issue in specific comment 8 more in detail. The role of potential model biases is referred to in specific comment 10.

Specific comments

1. *Page 5271, line 16-17: how would those affect dust emissions?*

We will replace this sentence by a more precise formulation: The increased solar radiation during the mid-Holocene led to a strengthening of the Westafrican monsoon, which weakened surface winds and led to reduced coastal upwelling intensity and sea surface temperature deviations. Dust emission occurs above a certain threshold wind velocity and thus less dust can be transported, when surface winds are reduced. Further, dust emission is very sensitive to near surface winds because of the nonlinear ($G \sim u^3$) relation. Thus, Adkins et al. (2006) linked changes in African aridity to changes in coastal upwelling intensity.

3. *Page 5272, line 3-5: With reference to this work, the study by Sudarchikova et al. (2015) uses a vegetation map for 6k with a lower LAI index over the Sahara, as shown in their work. On the other hand the study by Albani et al. (2015) accounts for vegetation changes as far as dust emission is concerned, but use PI vegetation as a base. In both cases, it is not conclusively possible to say that those models*

underestimate vegetation cover.

In the study of Sudarchikova et al. (2015), vegetation cover fraction appear to be similar for 0k and 6k, so it seems likely that vegetation cover is underestimated for the mid-Holocene. Due to the fact that there is no explicit analysis of North African vegetation cover, this will be rather formulated as a hypothesis. Also, the study does not explicitly focus on North Africa. Results of North African dust emission are exclusively presented in Sudarchikova (2012), which rely on the same analysis (though values differ slightly and are equal to values of the discussion paper) and support our hypothesis that they do not account for realistic vegetation cover in NA since Saharan dust emission at 6k is about 95 % of pre-industrial values.
In the study of Albani et al. (2015), modeled dust depositions in the North Atlantic are much higher than observations for the mid-Holocene. We will now rather express as a hypothesis that a possible cause could be the underestimation of vegetation cover, because the extent of vegetation cover is typically not fully captured by global climate models in North Africa for the mid-Holocene.

4. *Page 5272, line 10-11: global? Rather regional*

This is related to comment 3 and the fact that both studies (Albani et al. 2015, Sudarchikova et al. 2015) do not focus on North Africa, but examine global dust budgets. We agree that from regional observation data we can not draw conclusions on global values and will change the phrase accordingly.

5. *Page 5273, line 12-13: are the aerosol other than dust prescribed too to pre-industrial levels?*

The aerosol concentrations from natural sources are calculated interactively in the model. Additionally, emissions from anthropogenic sources are prescribed. In the analysis, we focus only on mineral dust.

6. *Page 5273, line 24-25: how do you account for that? Dry in which climate conditions?*

To clarify the statement, we will rather relate to "exposed paleolake beds", which serve only as preferential dust source if there is no water in the lake basins, thus under dry conditions. How they are handled in the model is described in Tegen et al. (2002): The surface material deposited in the paleolake basins is assumed to consist of silt-sized aggregates, which makes them a highly productive source of dust.

7. *Page 5277, line 9-16: what is the point of this segment?*

We will replace the reference for ODP Site 658C. Instead of the data of deMenocal et al. (2000), we will use data from Adkins et al. (2006), who accounts for sediment redistribution and gives thus more accurate results. Consequently, we will remove this segment in the revised manuscript.

8. *Page 5277, line 17-26: the study by Albani et al. (2015) cited by the authors provides an up- dated observational dataset with temporal (and size resolution) the study period. This also raises the question of the meaning of comparing model vs observed deposition rates over different size ranges. Please add a description of the model's particle size treatment and review the analysis on those bases. The comparison should be updated using those results.*

The study of Albani et al. (2015) will be mentioned to provide additional observational information. Since our study is related to fixed points in time (6k and 0k), the better temporal resolution does not lead to significant quantitative differences of deposition rates in marine cores GC37, GC49 and GC68 between Albani et al. (2015) and McGee et al. (2013) and thus we refer to the latter study. We will consider a comparison of particle size distribution between modeled and observed deposited dust in a revised version of the manuscript. The Model description (Section 2.1) will be updated by including a description of particle size treatment: Dust particles are grouped in 192 dust size classes with diameters ranging from 0.2 to 1300 μm. Vertical emission fluxes are integrated over all size classes and divided into aerosol modes for which log-normal distributions are prescribed: accumulation mode (mass mean radius (mmr)=0.37 μm, standard derivation σ=1.59 μm) and coarse mode (mass mean radius (mmr)=1.75 μm, standard derivation σ=2 μm). Emission into the super-coarse mode is neglected because of the short life time of particles.

We have plotted the size distribution of simulated atmospheric surface aerosol concentrations in the coarse mode (accounting for 98% of dust aerosols) for 0k and 6k at the positions of marine cores GC68, GC49 and GC37 (Fig. 1) and compared to observational data of dust size distribution in the cores from Albani et al. (2015). Note that in our model output it was not possible to separate the size distribution of dust from the one of all aerosols. However, most other aerosols exist primarly in the nucleation, aitken and accumulation mode with a smaller median diameter. Dust is the only representative of the insoluble coarse mode. In the soluble coarse mode, only sea salt particles exist with an approximately similar mass mixing ratio as mineral dust, the concentration of the remaining aerosols is much lower in comparison. Further, in our model output, we find a similar aerosol median diameter for soluble and insoluble particles. Thus, we assume that the aerosol size distribution obtained from our model results is in principle representative for the dust size distribution.

We notice quite similar dust particle concentrations for 0k and 6k in our model results. This is in agreement with observations and model results of Albani et al. (2015), who stated, that the temporal variability of the dust size distribution is very limited. The mean diameter for different times and cores is shown attached (Table 1). The mean diameter decreases slightly from north to south and is slightly higher for 6k compared

to 0k. The reason is probably, that at 6k and at higher latitudes, particles are removed from the atmosphere more quickly and thus coarser particles can be find close to the margin in the surface layer.

Compared to observations of Albani et al. (2015), the mean aerosol diameter is relatively small. Mahowald et al. (2014) pointed out that the atmospheric surface concentrations are in general finer than the ones deposited in marine cores because coarser particles are removed preferentially from the atmosphere whereas finer particles are transported further downwind to the Atlantic Ocean.

Our observed size distribution is in average higher than modeled size distribution of atmospheric surface concentrations along the Westafrican margin of Mahowald et al. (2014) (see Fig. 8k,l) but smaller than observed values (Mahowald et al. 2014); Fig. 8k.

Further, we have evaluated the particle size distribution over land, where the portion of sea salt is insignificant to ensure that particle size distributions are similar for dust and for all aerosols in the coarse mode. We compare the atmospheric surface concentration size distribution for a source region of dust (source), a region in the desert closer to the margin (sahara) and a region above marine sediment core GC68 (equal to Mahowald et al. (2014); Fig. 8 d,k and m). We see a slightly increasing median diameter downwind the dust source (Fig. 2). Nevertheless, we find no large variations in median diameter over land and over sea and hence this result once more confirms that the aerosol size distribution is approximately representative for the dust size distribution.

9. and 11. *Page 5278, line 21-25: to which region do these budgets refer to? Are they global, or rather limited to the study region depicted in one of the plots (and for instance the area is different in figures 6 and 7)? Regional budgets should be more appropriate, but the region should be clearly defined and indicated*

Indeed, the budgets refer to regional budgets, more precisely to those of North Africa as indicated in Fig. 6 and Table 6. This was not clearly mentioned in the text and will be added. The area of North Africa is defined at the beginning of Chapter (3.2) and underneath Fig.6 and Table 6 and 7 what we assume to be sufficient.

10. *Page 5278, line 23-28: how do the AO simulated conditions compare to observational evidence? Are there any biases in CMPIM5 runs with this model? This aspect is very relevant to the discussion of the relative importance of different conditions to changes in dust emissions. If biases are present in the simulations of the physical climate, their effects on this work and interpretations should be discussed*

We have plotted changes in simulated winds and precipitation between experiment $AO_{6k}LV_{0k}$ and $AO_{6k}LV_{6k}$ and the control run, respectively (see attached Fig. 3 and Fig. 4). In experiment $AO_{6k}LV_{0k}$, the amount of precipitation and the northward propagation of the Westafrican monsoon during summer is underestimated in comparison with paleoevidence (Bartlein 2011). This bias appears in most simulations of the PMIP intercomparison study (Braconnot et al. 2007). We found that in experiment $AO_{6k}LV_{6k}$, when additionally prescribing a more realistic land surface for 6k, precipitation is even overestimated in the southern Sahara and is in agreement with paleo data of Bartlein (2011) north of 20°N. Thus, our results confirm that a change in orbital forcing alone to 6k conditions is not sufficient to bring the extent of the monsoon and the amount of precipitation close to observational evidence and land surface-climate feedbacks play an important role. We will extend the Appendix by comparing precipitation and wind change plots for experiments $AO_{6k}LV_{6k}$ and $AO_{6k}LV_{0k}$ to the control and will add this issue in the discussion section.

Uncertainties in the simulated physical climate that arise from model biases for pre-industrial times are reported in (Giorgetta et al. 2013) for MPI-ESM (including ECHAM6 as atmospheric general circulation model) in the frame of CMIP5. They mentioned a dry bias in the tropics over land north of the equator. However, since

differences in precipitation between 6k and 0k are in agreement with paleoevidence, we assume this bias not to have a significant effect on our results. A more thorough discussion will be added in the revised manuscript.

13. *Page 5283, line 1-3: the un-mixing of riverine versus aeolian components, as well as focusing factor corrections thanks to the thorium profiling method are discussed for those sites in McGee et al. 2013. Please add the reference here.*
As we referred to a different study (compare 7.) we also remove this segment. The unmixing of riverine vs. eolian components will be added in the discussion part.

Regarding the more editorial comments (2, 12, 14, 15), we will implement all changes as suggested.