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Interactive comment on "Rapid increase in simulated North Atlantic dust deposition due to fast change of northwest African landscape during the Holocene" *by* Sabine Egerer et al.

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We thank the reviewer for constructive comments and helpful suggestions.

2 (19-20). What about Egerer et al. 2017?

It makes sense to include Egerer et al. 2017 if we talk about the Holocene instead of the mid-Holocene period since Egerer et al. 2017 focus on the dynamics of the whole Holocene period. We will change the sentence accordingly.

3 (3-6). Is dust having any feedback on climate in your simulations?

Yes, indeed the impact of dust on the solar radiation budget through scattering and

absorption and the alteration of the cloud structure is implement in the model (for details see Stier et al. 2005). We will highlight this explicitly in the model description. For the analysis, the feedback of dust on climate can nevertheless not be quantified. To do so, additional sensitivity experiments would be necessary (which we already mentioned in the discussion).

4 (29-31). You mention initial and boundary conditions. How long have you run your dust experiments from there?

If we understood the question correctly, the answer is given in 4 (15-17): To achieve an equilibrium for the vegetation distribution for each time slice, we first run 30 years with an accelerated vegetation dynamics followed by 570 years with vegetation dynamics at normal speed. We take the last 200 years for evaluation. To avoid confusion, we will shift this paragraph to the above mentioned passage.

5 (5). Since we cannot see the manuscript in preparation, please spend a couple of words on it.

Recently, the manuscript was published in Climate of the past and all details can be found in there. We will add the citation (Dallmeyer, A., Claussen, M., and Brovkin, V.: Harmonizing plant functional type distributions for evaluating Earth System Models, Clim. Past Discuss., https://doi.org/10.5194/cp-2018-41, in review, 2018).

Figure 5. The model results (absolute values, North-South gradient) are similar to Egerer et al. 2017, but quite different from Egerer et al. 2016. Could you briefly explain what changed? This could be useful in your discussion concerning the apparent overestimation of deposition fluxes in correspondence of the more northern cores.

The progressive development and installation of new hardware and software at the Max Planck Institute for Meteorology demanded the use of different model versions for the different studies. Older model versions were used in Egerer et al. (2016) (echam6.1-ham2.1) and Egerer et al. (2017) (echam6.1-ham2.2). In the new model version (echam6.3-ham2.3), the definition of the roughness length is no longer depending on the orography, but on the leaf area index of the vegetation. Due to this changes, a regional modeling factor was introduced in the new model version. So on the one hand the differences arise from different tuning of the dust module. On the other hand, note that the prescription of the vegetation distribution is quite different in Egerer et al. (2016) and Egerer et al. (2017). And both differ from the vegetation distribution which we get in this study with dynamic vegetation. Since dust emission is highly depending on the vegetation distribution, we think that in terms of absolute values it is inappropriate to compare the three studies.

Also, somewhere in the text please discuss a bit more the data, e.g. what are the assumptions in terms of isolating the dust flux, what size ranges you are comparing, etc.

The dust flux was calculated as the difference between the total flux and the carbonate, opal and organic carbon flux. In McGee et al. (2013) and Adkins et al. (2006), the 230Th normalization method was used to determine dust fluxes. Also, McGee et al. (2013) use endmember modelling to separate eolian and hemipelagic fluxes. Thereby, the coarse endmembers (approximately between 8 μm and 30 μm in size) are assumed to characterize eolian dust. We will add this information to the 'Comparison with marine sediments' section. Model description: The size distribution of the emitted particles is prescribed via log-normal functions of a coarse (mass mean radius (mmr) = 1.75 μm, standard derivation σ = 2 μm) and an accumulation mode (mass mean radius (mmr) = 0.37 μm, standard derivation σ = 1.59 μm). We will add mmr and σ in the model description. The problem of differing size ratios between simulated and observed fluxes was already discussed in Egerer et al. (2016) and in earlier studies (e.g. Mahowald et al. (2014). Thus, we will refer to this studies in the manuscript.

10 (10-15). Could you calculate dust emission budgets for the two sub-regions and see the absolute and relative changes to support your discussion?

In Fig. 1, the dust emission budgets for the western Sahara (as in Fig. 7 in the manuscript) and for the area around the Bodele Depression (12°N-18°N, 14°E-21°E) are plotted. This supports the argument, that emissions from the Bodele Depression are rarely significant for changes between 6 and 4 ka BP and occur mainly after 4 ka BP. We will add Fig. 1 to the manuscript and thank the reviewer for the suggestion.

10 (15-20). These explanations all refer to the real world I believe, but what about in your simulations? 10 (15-20). In general I do not think you really show that one source is dominant for the cores in your model simulations. You may argue explicitly that it seems reasonable to assume so. In this paragraph it seems that model and observations are mixed up, whereas I think you should separate clearly what you can say for each of them, and later discuss if you see convergences and/or differences.

In the simulations, it is unfortunately not possible to track dust particles from source to sink since the tracers for dust are interacting with surrounding particles and can not be extracted. To support the argumentation, we will include Fig. 1 to the section. We think that the discussion (also involving 'real world believes') is necessary to be conducted in this section because the choice of the dust emission area that is responsible for dust deposition at the margin and the next chapter is based on that discussion. We will nevertheless separate the arguments arising from our simulations and previous observations more clearly.

10 (19-20). The two potential sources have different composition; but which one does match the chemical composition of dust in those cores you are looking at?

Unfortunately, in the paper which we referred to, this information is not given. We were pointing to the approach that if the chemical composition is significantly different in the two source areas, it must be clearly differentiable in the sink area. Since this is a vague argument, it might be better to remove this sentence from the manuscript.

Figure 8. I wonder if you can highlight somehow in the figure the boxes you discuss in the text.

Yes, this seems like a good idea. We will mark the boxes of strong changes in dust emission and whether they coincide with a strong change in lake area, vegetation cover or both with different colors.

19 (13). Please add a concluding paragraph that concisely describes your results with a short summary.

To summarize our findings we will give a short conclusion where we answer the questions that we posed in the introduction.

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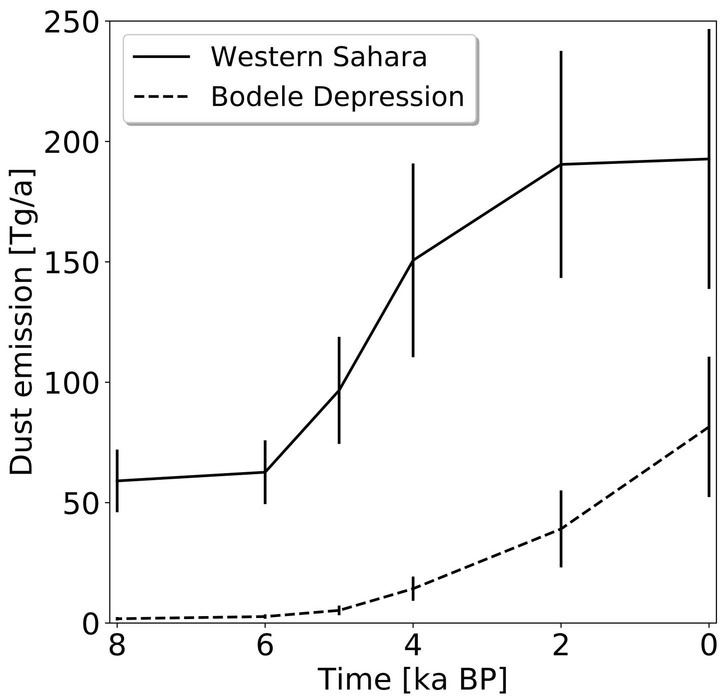

Fig. 1. Simulated dust emission from the western Sahara and from an area including the Bodele Depression.