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Climate of the Past
Discussions

CPD
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Interactive Comment

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 2

We would like to thank the referee for comments on the paper and suggestions of re-arrangement. Technical corrections will be implemented as proposed by referee 2.

1. **Model**
a) The authors should give more details on the nature of the dust emission process that

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is modeled, and on the specific dust sources that the model uses (these issues are only briefly mentioned in section 2.1). For example, does the model assumes preferential geomorphic sources or uses some grain size criteria to determine from where the dust will be emitted? Does the model assume that sandblasting is the major/only active dust emission process?

The description of the dust emission process will be extended following Tegen et al. (2002) (see section 2.1): The role of exposed paleolake beds as preferential sources of dust under dry conditions is accounted for in the model. 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 Tegen et al. (2002). Dust particles are emitted from preferential and potential source regions if specific criteria are fulfilled (e.g. the wind velocity has to exceed a threshold, the soil is not covered by snow, the upper soil layer has to be dry).

Dust particles are grouped in 192 dust size classes with diameters ranging from 0.2 to 1300 μm. After exceeding a threshold friction wind velocity, that is specific for each size class and depends on soil moisture and texture, dust fluxes increase nonlinearly as a function of wind velocity. The explicit formulation of the calculation of horizontal fluxes is following Marticorena and Bergametti (1995). The mechanism considered in the scheme is saltation bombardment. The ratio between vertical and horizontal emission fluxes is prescribed for different soil types based on empirical measurements and depends on particle size distribution and surface properties (Marticorena et al. 1997). Soil types are clay, silt, medium/fine sand and coarse sand (Tegen et al. 2002). Emission into the super-coarse mode is neglected because of the short life time of particles. Turbulent dust emission (e.g. in form of dust devils) is not considered in the model due to its coarse resolution.

b) The model assumes a uniform vegetation fraction at 6k over large areas in the Sahara (Fig. 1, upper right panel). Obviously, this is not realistic, as the Sahara is *composed of different geologic, geomorphologic and soil units. Although it is clear that the authors don't have enough information to produce a more detailed map, they should discuss the potential effect of variable vegetation fraction on their results. Using a uniform vegetation cover might partly explain why their modeled dust fluxes differ from calculated ones.*

We agree that the assumption of uniform vegetation cover is unrealistic and its effect on dust emission is a source of uncertainty. As correctly stated by the referee, we do not have more detailed information to produce a more realistic map. A more in-depth discussion about the simplified uniform vegetation cover will be added in the discussion section: Although dust emission is nonlinearly dependent on vegetation cover, this is a valid simplification in the model as long as vegetation cover is not varying too much. The homogeneity of prescribed vegetation surely effects the pattern of dust emission. However, prescribed preferential dust sources account for highly productive areas and thus a somewhat heterogeneous pattern. However, the total amount of emitted dust and the corresponding deposited amount of dust in the North Atlantic is not significantly effected by the exact vegetation distribution. A more diverse vegetation cover could, however, influence near surface winds and thereby change dust emissions since dust is emitted only above a certain critical wind velocity and dust emission is very sensitive to changes in near surface winds because of the nonlinear dependence ($G \sim u^3$).

2. **Validation dataset**

The authors should describe in more detail the validation dataset that they are using (Table 4). How the fluxes where calculated? Are all fluxes comparable (i.e., carbonate was dissolved or not?) For example, McGee et al. (2013) used grain size end member modeling to separate different sediment populations and calculated the dust flux only on the identified eolian end member. Was this procedure done for all other data

The description of the validation dataset will be extended: The terrigenous fraction of the sediments was calculated by subtracting the carbonate, opal and organic carbon percentages from the total flux following Wefer and Fischer (1993) for all sites. The studies of deMenocal et al. (2000) and Adkins et al. (2006) both investigate fluxes at core ODP Site 658C, but the latter study accounts for sediment redistribution via ^{230}Th normalization similar to McGee et al. (2013). Additionally, McGee et al. (2013) apply grain size endmember modeling to separate eolian and hemipelagic fluxes. Therefore, 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). Thus we ensure that data are comparable and represent only terrigenous vertical fluxes.

3. **Discussion**

In the current paper the discussion is relatively limited, and simply repeat sentences that appear in the results. Thus, the section "discussion and conclusions" seems more like a conclusions section. On the other hand, some important discussion sentences are scattered in the results section. I suggest writing a separate discussion section by gathering the discussion sentences that appear in the results and discuss in detail the findings of the study. For example, what are the reasons that the modeling approach estimated a 0k:6k ratio of 2-3, about a half of the ratio calculated by McGee et al. (2013)?; what are the possible errors of this examination (see comment above) and how these might effected the results?; The issue of the north-south trend in dust fluxes; implications of the findings.

The discussion will be largely expanded and parts of the results section will be moved to the discussion (e.g. north-south gradients in dust deposition and related wind patterns, page 5283, line 5; implications of the factor separation, page 5283, line 7; type of deposition and meteorological conditions, former page 5279, line 11). Additional issues that will arise in the updated discussion section are: model biases and uncertainties; connection of dust emission and transport to the physical climate, changes in precipitation and surface winds; uncertainties arising form simplifications of reconstructed land surface area.

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4. **Re-arrangement**

Part of the results (e.g., 5277-5278) consists of methodology and discussion issues. Try to keep only the results in this section; move other parts to their relevant sections. 5280, Section 3.3: the first part of this section belongs to the method section and not to the results.

The first part of section 3.3 will be moved to the methods (section 2.4). In this context, section 2 is renamed from "Experimental design" to the more general "Methodology". To avoid too much repetition, the discussion part will not be conceived as a separate section, but discussion issues will be inserted in the "Discussion and conclusion" section.

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