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Climate of the Past Discussions

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 David McGee for his constructive comments and helpful suggestions.

1. What does modeled dust deposition look like downwind in the Bahamas or in the central tropical/ subtropical Atlantic? It would be useful to compare against the records of Williams et al. 2016 and Middleton et al. 2018 as well, especially given that dust deposition in the Bahamas should reflect summer deposition rather than winter/spring deposition, and all the distal sites presumably reflect a broader range of sites than the NW African margin sites.

In my doctoral thesis (Egerer, S. (2018). Linking marine dust records to Saharan landscape evolution during the Holocene: a theoretical study. Phd Thesis, Hamburg: Universität Hamburg. doi:10.17617/2.2552057), I additionally compared to the Bahama and central/tropical Atlantic core sites. However, in this study, we decided to focus on the dust cores close to the margin. The temporal resolution of the core sites further away from the margin is much coarser and thus uncertainty is much higher. Also, we would like to focus more on the abrupt change at the core sites close to the margin and would thus like to restrict this study to these sites.

2. Section 2.2/Section 3.1: For the comparison with coretop dust fluxes, what grain sizes are being transported in the model? Grains at these sites are quite coarse (10-40 μm modal grain sizes for sediment inferred to be windblown dust), so if the model is getting the right answer for the right reason at ODP658 and GC68, it should be depositing quite coarse dust at these sites. Albani et al. simulated only <10 μm grains, so it was necessary to compare against fluxes of dust at these sites rather than the total dust fluxes reported by McGee et al. Related to this point, I'm surprised that the coastal dune fields in Mauritania/Western Sahara (e.g., Lancaster et al., Geology 2002) aren't dust sources in the model, as I've always thought that the coarse grains deposited at these sites must have a quite local source.

The grain size distribution in this study is quite similar to Egerer et al. (2016), which peaks around 6-7 μm. Already in Egerer et al. (2016), we discussed possible reasons and consequences of the discrepancy between the grain sizes in the simulation and observed particle sizes. Despite the mismatch, we think that it is valid to compare both, because we think the fluxes <10 μm are proportional to the total amount of deposited dust. We are afraid our global circulation model with a grid cell length of about 200km is not suitable to resolve and reflect dune field activity. As was discussed in our manuscript, the dust source areas in the model are in conflict with satellite observations (Schepanski et al., 2009) and thus might not reflect dust source areas realistically.

3. There should be a more detailed and complete summary of modern observations

of the seasonality and flux of dust in the region – for example, see R.F. Anderson et al., Phil. Trans. A 2016, especially citations 33-35 for studies of dust deposition in the eastern tropical North Atlantic and at Cape Verde. The work of Skonieczny et al. Is also useful for documenting modern transport from dust sources in the NW Sahara.

We will involve these studies in the chapter 'Changes in the seasonal cycle of dust emission and atmospheric circulation'. Here it is mentioned, that maximum surface dust concentrations at the Cape Verde Islands are found in boreal winter (DJF), whereas in our simulations dust emissions peak later in early spring (FMA).

4. The discussion of wind changes should be expanded and clarified (admittedly, winds are a bit of a fixation for me.) First, winds are only shown for the two timeslices (Figure 10) rather than plotted as a timeseries as is done for the dust, vegetation and precipitation. It would be fairly easy to plot January-through-April northeasterly wind strength over the some portion of the NW Sahara to see whether similarly abrupt changes occur in winds between 6-4 ka. Second, it should be noted in the text that the large changes that the authors find in FMA northeasterly winds are consistent with the changes in upwelling inferred from SST and biogenic flux records along the NW African margin (Adkins et al., 2006; Bradtmiller et al., 2016; Romero et al., 2008) (note that these changes in upwelling proxies are as abrupt as the changes in dust fluxes at these cores). Third, on page 18 lines 14-20, the authors first state that the FMA wind changes are potentially as important as the vegetation and precipitation changes, then end the paragraph by saying that "changes in atmospheric circulation due to a shift of the monsoon system are of minor importance concerning the rapid shift in North Atlantic dust deposition." Aren't the winter/spring wind changes a part of the monsoon system? Or does this second statement just focus on the summer monsoon? If so, that should be clearly stated.

We will add a time series of the 10m wind strength over NW Africa during early spring (FMA) (Fig. 1). (Note: The shape is in principal equal to Jan-April but we chose FMA for consistency with Fig.11). We thank you for this suggestion. Indeed, there is a clear

rapid rise of the wind strength between 6 and 4 ka BP in the western Sahara in line with the increase in dust emission. We will clearly state the relation between changes in wind strength and dust emission in the text. Furthermore, the wind strength is linked to vegetation through the roughness length. The decrease in vegetation cover is in line with an acceleration of surface winds. We will also underline the consistency between the FMA northeasterly winds and changes in upwelling inferred from SST and biogenic flux records not only in strength but also in speed. Yes, indeed we refer here to the summer monsoon and will change this sentence accordingly.

5. I agree with the final point of the paper, that dust fluxes at NW African margin sites reflect conditions in specific NW African dust source areas, and they are not representative of the whole of North Africa. That said, Figure 7 suggests that the dust changes are at least representative of an area 17°(east-west) by 11° (north-south) – quite a large part of NW Africa. So I think this statement should be qualified – the dust records are representative of a large area, just not as large as has sometimes been implied or stated.

We will specify the statement accordingly in the discussion.

6. I think the paper's other main point is that, at least in this model, the rapid increases in dust deposition recorded in these sites require rapid decreases in precipitation and vegetation density (and perhaps rapid increases in winter/spring winds) in NW Africa – the rapid dust changes cannot be attributed to thresholds inherent to dust emission superimposed on gradual changes in climate and vegetation. The study thus suggests that mid-Holocene drying of NW Africa proceeded much more rapidly than the decline in insolation would suggest, correct? If I have this correct, I think this second point could be stated more clearly and emphasized in the text.

Your argument is correct and we will emphasize the stated point more clearly in the discussion.

Other comments:

Page/line: P1/L6: Here and in page 5/line 4, “Therefore” is used improperly. It should be used to mean “Because of this”, but in both places it is used in place of “In order to do this”. Please change to “To do this” or equivalent.

We agree and will change the phrases accordingly.

P8/L5: “At the more northern cores GC37 and GC49 the change in dust deposition is rather moderate.” If this statement is intended to mean that the magnitude of the dust deposition change is smaller at GC37 and GC49, I disagree: see McGee et al. 2013 Figure 5, which shows that relative increases in dust fluxes are similar at GC37, 49, and 68 (the relative change is smaller at ODP658, presumably because this record is bulk terrigenous flux and so overestimates fluxes during the AHP.) If this statement is intended to mean that the rate of change is slower at GC37 and GC49, I also disagree: McGee et al. 2013 demonstrates that the smoother changes recorded at GC37 and GC49 could just be due to bioturbation (greater smoothing of the record due to lower sedimentation rates), not a slower rate of change in dust deposition.

We intended to say that changes in dust flux are not as sharp as for the more southern core sites. From McGee et al. it is not clear, whether there is indeed a slower rate of change in dust deposition or if a smoothing due to bioturbation is indeed the cause of the slower change rate.

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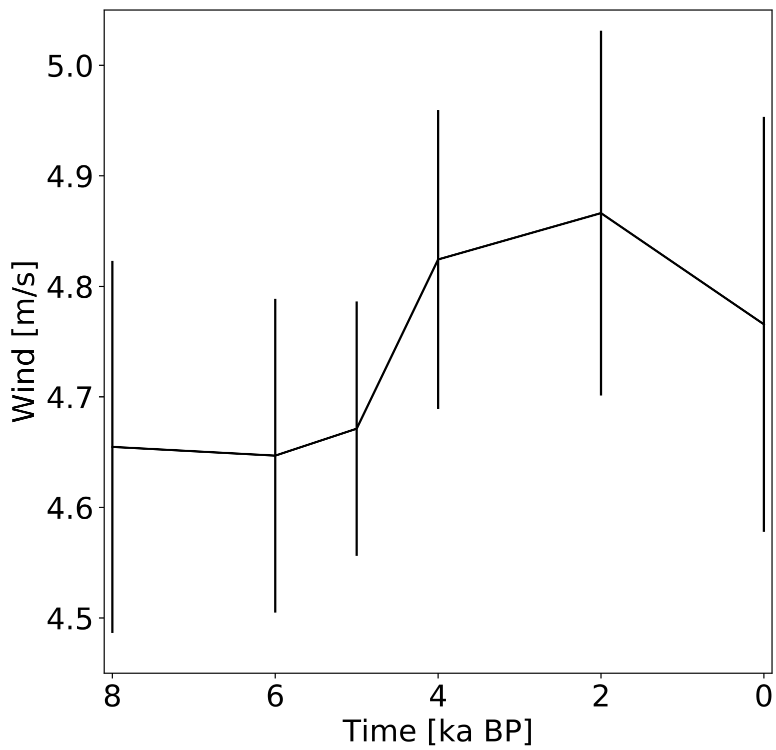

Fig. 1. Time series of 10m wind strength over NW Africa during early spring (FMA).