

## ***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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This paper examines the question of whether abrupt mid-Holocene dust flux increases recorded in NW African margin marine sediment cores reflect similarly abrupt changes in vegetation and precipitation in dust source areas. It also examines which parts of North Africa are represented by these dust flux records. The study centers around time slice experiments spanning 8 ka through the preindustrial that employ dynamic vegetation and interactive dust.

I find this study to be a valuable contribution to our understanding of North African climate changes over the Holocene and of how to interpret dust deposition records

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in this region. There are a few areas where I think the study could be made more complete and where the writing could be clearer, which I list below, but overall I find this to be worthy of publication after revision.

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.

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  $\mu\text{m}$  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  $<10 \mu\text{m}$  grains, so it was necessary to compare against fluxes of  $<10 \mu\text{m}$  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.

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.

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 precipi-

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tation. 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.

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°E (east-west) by 11°E (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.

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

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could be stated more clearly and emphasized in the text.

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.

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.

-David McGee

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