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Climate of the Past Discussions

Interactive comment on "Abrupt shifts of the Sahara-Sahel boundary during Heinrich Stadials" *by* J. A. Collins et al.

Anonymous Referee #2

Received and published: 20 February 2013

This paper presents an exercise to analyse the movement of the Sahel-Sahara boundary during last 60 ka based on quantifying the amount of dust in a latitudinal transect of marine cores. Although the exercise is well resolved and statistically sound, I consider this study lacks the effort to fully understand other mechanisms that are influencing the sedimentation of particles at the study sites. In addition, the authors do not attempt to go further on analysing the composition of dust in marine cores and they rely on the XRF elemental data without exploring the mineralogy or even the isotopic composition (Sr and Nd), both probably more sensitive analyses to track the Saharan dust particles in marine sediments that are available for other marine cores. Nevertheless, in spite of these concerns that are further developed below, I believe that the paper merits publication in Climate of the Past, once the following remarks are considered.

(1) Novelty of the study. Last paragraph of the introduction justify why this is an interesting study and what the novelty of the presented results is. To me, the originality or the novelty of this study is not well explained (neither in the introduction nor later in the manuscript). The authors should better justify the reasons to carry out this work in the context of many other publications that have reconstructed dust variability during last glacial cycle on marine sediments from NW Africa. Some of those previous studies from NW African margin sediments (Jullien et al. 2007; Mulitza et al. 2008; Tjallingii et al. 2008; Itambi et al. 2009; Zarriess et al. 2011) and, previously, from the Western Mediterranean (Moreno et al. 2002) already recognized an increase of dust input during HE, due to an increase of aridity in Africa, similar conclusions reached by this new study. Besides that, all the data used in this study were already presented in other papers where XRF core scanner data were also employed to discriminate the eolian fraction in marine sediments (eg. GeoB9526-5, Zarriess et al. 2011). Thus, since similar conclusions were already attained by previous studies and almost all the presented data were already published (including the data to construct their model, see S1 in Supplementary material), I recommend the authors to be more specific in the introduction when stating the novelty of this study. Particularly, they should highlight the importance and novelty of the statistical methodology followed to construct a curve of dust amount for every marine core that is later transformed in a curve of SSB position.

(2) End-member modelling. To me, the end-member unmixing model lacks the adequate number of samples, especially for river material since it is constrained by just one river without specifying if the 9 selected samples represent a special section of the river. Since Senegal river is 1790 km long, I suspect the particles in suspension would probably have different geochemical composition from the headwaters to the mouth). Those data come from a relatively “old” paper (Gac and Kane, 1986). Dust end-member is not fully constrained either. It is said (Page 122, Line 23) that dust end-member, compared to river end-member, is enriched in Si and K. Checking carefully both tables in Supplementary material (soil and aerosol data versus river sediment data) it is clear to me that the amount of K is really similar in both end-members (3.1

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+/ 0.2 vs 3.5 +/ 1.4) making uncertain the differentiation of dust and riverine material just based on elemental geochemistry. I would suggest including in this end-member modelling exercise other parameters such as grain-size, mineralogy or Nd isotopes that would be more sensitive to the origin of the material. At least, the reasons to not consider those indicators should be explained in the manuscript. My last concern regarding this topic is related to my first comment on the novelty of this study. I would ask the authors to clarify what is new in their end-member modelling respect to the model presented in the Nature paper by Mulitza et al. (2010). If I am right, authors of this study used the same samples, same data, and applied same methodology (see supplementary material in Mulitza et al. (2010). Thus, it should not be presented in the Methods section (Page 123) but just citing the source.

(3) Controls on past sediment composition. Discussion on factors that control sediment composition is well written, concise and clear but particularly one of the factors may be further explained. I think that N-S advection by bottom currents may be more important than authors state in the manuscript (Page 126, Line 10). Sediment reworking, partitioning and winnowing depends on local factors, it is true, but it is important to consider that all four studied cores are located under the same oceanographic setting, so under the same “local” factors. A bathymetric map and more information about surface and deep currents would be necessary to really discard the influence of those “local” processes on the final results.

(4) Mechanisms causing equatorward shift of dunes during HE. This study shows a connection among the SSB position and HE, indicating an increase in aridity during those cold episodes. Although this idea is not new, it is in some way “quantified” and “mapped” in this work thus being of interest for the community. About the mechanisms, I am missing the classical idea of Rea (1994) that argued the necessity of arid climate (but not hyper-arid climate) to have the ideal conditions to generate and mobilize dust particles since some humidity is necessary for the weathering and dust particles generation. I am not sure if such a climate is coherent with dune formation or with Heinrich

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Events and may be further developed in the paper. Finally, some other studies that also marked an increase in dust input during HE but in the Mediterranean (eg. Si/Al ratio in Moreno et al. 2002) should be cited and maybe included in the final figure (Fig. 3).

Specific comments

Abstract, line 14: SSB is not explained before (it should appear in parenthesis in line 11)

Page 121, Line 14: "Continental pollen records. . .." It should be changed by "pollen records in marine cores" since both references are palynological studies on NW African margin marine cores.

Page 123, Bloemsma et al., 2012 and Mulitza et al., 2010 are not included in the reference list

Page 123, Line 24. "by using an end-member unmixing analysis using".

Page 124, Line 16. It would be recommendable to plot dust % in Fig. 2 of Supplement to really see the comparison with Al/Si ratios.

Page 127, Lines 1 and 2. I wonder if there are SEM images or any other indication (mineralogy, Sr and Nd isotopes, etc) that can really confirm that the material delivered during HE is wind-blown dust and not transported by turbidity currents.

Page 127, Line 8. Discard sea level as an important factor requires further arguments. Why the influence of sea level is not seen in the records? Please, explain.

Page 129, Line 16. At the end of this assertion a "classical" reference would be recommended, such as Pye (1989)

Page 130, Line 10. Why "our data suggest a gradual response to insolation forcing"? Please, explain and refer only to the Holocene. . . during HE is not exactly "gradual".

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Page 130, Lines 26-28. The three-phase evolution of HE is not clear. Please, remove. Fig. 1c I suggest marking a line with the present-day SSB position to see the interception with the regression line at 43% of dust.

Fig. 2. Add “core 1, core 2, core 3 and core 4” in the figure and caption since those records are referred with numbers in the text more than with their name (GeoB. . ..).

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Interactive comment on Clim. Past Discuss., 9, 119, 2013.