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Climate of the Past Discussions

Interactive comment on "Climatically-controlled siliceous productivity in the eastern Gulf of Guinea during the last 40 000 yr" *by* X. Crosta et al.

Anonymous Referee #2

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This paper presents the first diatom and phytolith record from the eastern Gulf of Guinea, a key area for understanding the West African monsoon variability. The record (40 ka) covers the last glacial period including the LGM, the last deglaciation and the entire Holocene period, and thus register environmental changes under very different boundary conditions. Fluxes of biogenic silica and of different diatoms groups to the sediments are estimated and interpreted in terms of changes in river discharges bringing dissolved silica to the sea (thus in monsoonal precipitation inland), and in wind regime.

As a whole, it is an interesting paper. It complements previous studies from the same core (GeoB4905-4) and from a neighbouring core (MD07-2707) which have provided information on changes in freshwater inputs to the marine realm. Nevertheless, the

interpretations/conclusions are not discussed thoroughly enough to be fully convincing. In particular, I am not sure that high total diatom AR and freshwater diatom AR reflect wet conditions and enhanced upwelling during both the AHP and the LGM (despite a short dry interval around 20 ka). While it is well established that the early-mid Holocene and the Bölling-Alleröd periods have been wetter than today, generally wet LGM conditions conflict with most terrestrial and marine records from the region, including those from the same site. Possibly, alternative or nuanced nterpretations could be proposed to reconcile this nice diatom dataset with other records. The paper could certainly benefit from more detailed comparison with other data, discussion on similarities/ differences and on their potential causes.

A careful reading of the manuscript is needed to correct typing mistakes (e.g., *G. rubber* for *G. ruber*, Livingston for Livingstone. . . .), re-write some unclear or uncorrect sentences, and correct or add some literature references for previous works used in this paper.

I address below more specific questions, remarks and suggestions that should or could be taken into account in the final version.

In the introduction (Section 1), it is postulated that " There is . . . a direct connection between DSi stocks and diatom production in surface waters and occurrence in sediments of the Gulf of Guinea and precipitation regimes over western Equatorial Africa ". This has to be demonstrated through the paper. I suggest to summarize here the major paleoclimatic-paleoceanographic results from previous studies in the eastern Gulf of Guinea (only cited here) which have also be conducted for reconstructing regional inland climate and linkages with higher latitude climate. Comparison with the Southern Ocean is fine, but the close relationships between the investigated area and the North Atlantic could also be pointed out.

In Section 3 (Material and methods), the age model is based on ^{14}C ages previously published by other authors. Although references are given in the Fig. 2 caption, the

data source should be clearly indicated in text (same remark for opal content in the sediments).

I am not confortable with the ecological diatom group identification (Table 1). It is not so easy to separate windblown and river-transported diatoms in the region. *Cyclotella* spp. are here combined to " Meroplanktonic diatoms ". Are all encountered *Cyclotella* marine or brackish taxa ? This is important because freshwater *Cyclotella* (especially *C. ocellata , C. stelligera)* are abundant in eolian dust derived from the Sahara-Sahel (see e.g., Gasse et al., QR, 1989). Same remark for *Cocconeis* spp. or *Achnanthes* spp. (brackish or freshwater taxa?), although these taxa are much less frequent in sediment traps and surface sediments of the eastern Atlantic. It is true that *Aulacoseira granulata* is windblown over long distances over the ocean, but it also thrives in African rivers (95-80 % of the diatom assemblages in the Niger and Congo Rivers). Does the Guinea Current transport diatom valves from the Niger delta along the coast ? Did the authors observe the modern diatom flora of the Sanaga River and local eaolian dust samples? Nothing is said about potential selective dissolution, which is often an important problem in the interpretation of diatom records. Are all the diatom groups similarly preserved ?

Discussion (Section 5)

In the introduction (p. 4, l. 7), the absence of upwelling in the eastern Gulf of Guinea is outlined to reinforce the hypothesis of changes in local river discharges as the major controlling factor on siliceous productivity. Later on (Fig. 4), it is surprising to see that " upwelling diatoms " prevail in a large part of the record, and changes in upwelling conditions are regarded as crucial on datom production. Increased upwelling DAR during both the early-mid Holocene and the LGM might have different causes, but this is not clearly explained. One can suppose that, " during warm stages, stronger river discharges might have yielded to upwelling of nutrient rich sub-surface waters ", while, during the LGM, the ITCZ migrated southward reinforced the impact of dry NE trade winds in the region.

I also wonder if the LGM sea level drop might have favored the transport or reworking of benthic and freshwater diatoms previously sedimented close to the coast toward deeper slopes. The discussion on relatively high river discharge during the LGM should be reinforced. It would be interesting to add in Fig. 5 the Fe/Ca record (52 ka) from core GeoB4905-4 (Adegbie et al, Palaeo-3, 2003) which suggests very low river discharge from $\sim$28 to 16 ka. If the authors maintain their conclusion of rather humid LGM conditions, some data might be used in the discussion : (i) the SSS curve (plotted in Fig. 5C) shows some minor positive shifts ; (ii) pollen data from the montane Lake Barombi Mbo, western Cameroon, suggest only slightly drier LGM conditions (possibly due to the persistence of fog or stratiform cloud cover at this specific site ; Maley & Brenac, 1998) ; (iii) A high–resolution climate simulation of (Precipitation minus Evaporation) at the LGM compared to present, from the atmospheric-GCM NCAR CCM3 (Kim et al., Climate Dynamics, 2007) suggests a small patch of increased effective moisture close to the Sanaga basin. . .

The resolution of the record is relatively high. Therefore, abrupt events (Heinrich, YD, Holocene dry spells) could be better considered. Several authors have showed the N Atlantic forcing of droughts in West Africa. The comparison with the remote Lake Abhé is interesting, but why not use the records from the much closer Lake Bosumtwi (e.g., Talbot and Johannessen, EPSL, 1992 ; Peck et al., Palaeo3, 2004 ; Shanahan et al., Palaeo3, 2006. . .).

To sum-up, in its present state, the manuscript raises a series of unsolved questions which could be more clearly addressed. I am however convinced that it may become a very good paper after substantial revisions.

Interactive comment on Clim. Past Discuss., 7, 2445, 2011.