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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 #1

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To the editor,

Crosta and colleagues present an interesting new dataset in a climatically important region. The authors' approach to grouping diatoms by productivity regime is appropriate and helpful for interpreting the data. The manuscript would benefit from minor proofreading to correct typos and several irregular English usages, but in general the text reads reasonably well. However, I believe that the authors need to expand on their analysis, specifically to make a stronger case that precipitation is the strongest influence on diatom/opal accumulation. This is a major flaw in the paper, and should be addressed in the final version.

The data show a peak in total diatom AR during the LGM, and again during the AHP. Freshwater diatoms show this same pattern, and the authors cite this positive relation-

ship as evidence that diatom productivity is primarily controlled by DSi input via river discharge. If these were the only data available, this would be a reasonable conclusion. However, the data also show that upwelling diatoms peaked during the LGM and AHP, and that windblown diatoms peaked during the LGM, H1 and Younger Dryas. The magnitude of the upwelling peak during the AHP appears from Figures 3 and 4 to account for nearly half of total diatom AR- during the LGM, this number is probably closer to 60%. The authors' assertion at the beginning of section 5.2 that the LGM was a period of increased precipitation is contrary to most other regional and North Atlantic records of the LGM, which suggest a much drier climate (as would be expected from lower SSTs during that time). It is also not consistent with the paleo-salinity data cited by the authors, which points to increased salinity (decreased precipitation) during the LGM. However, the peak in upwelling and wind blown diatoms during the LGM could easily be explained by increased upwelling due to dry, windy conditions. In short, the authors need to expand their analysis to reach an interpretation consistent with the many different lines of evidence provided by this rich dataset.

The other major shortcoming of this paper is a lack of attention to some of the millennial scale events. The authors comment briefly on H1 and the Younger Dryas, but completely ignore the 8.2 ka event (which is possibly the event they identify at 8.5 ka). This record has the temporal resolution to provide some insight into the spatial extent and effects of these events, and it is a shame not to see this explored. I was surprised not to see some comparison to the records of deMenocal (QSR; 2000) or Adkins (Paleoceanography; 2006), the latter of which shows conclusive evidence of the 8.2 ka event. Since ODP site 658 is far enough from the continent to be unaffected by riverine input, this might also provide a point of comparison for upwelling vs. precipitation changes.

Technical comments: Abstract, line 16: "could" should be removed Introduction, line 21 p. 2447: this sentence makes an important point for the paper, but is confusing to the reader. Re-word to clarify. Stratigraphy: "datings" should be "dates", and "rubber"

should be “ruber” Section 3.3: at the end of this section, authors stat that “Lake diatoms can not be directly transported to the marine environment.” This is confusing, since presumably these are the same diatoms being blown into the ocean. This should be clarified. Section 5.2, line 26, pg. 2460: What are the dating uncertainties of this study? Of the comparison studies? Are they sufficient to resolve lead/lag relationships? If the authors wish to make this point, they need to be more explicit about the uncertainties involved.

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