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Climate of the Past Discussions

Interactive comment on "Thirty thousand years of vegetation development and climate change in Angola (Ocean Drilling Program Site 1078)" *by* L. M. Dupont et al.

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GENERAL Firstly, we want to thank the reviewers who both gave very consistent and helpful comments. We essentially picked up the recommendations, on which I comment below. We also want to thank F. Chalié for correcting many typing errors.

CONCERNING THE AGE MODEL (Editor and F. Chalié)) Because the chronology is important and the problem with varying reservoir ages acute, we rather extensively discussed the age model. Most recent model results indicate varying of subtropical reservoir ages between 300 and 500 years during the past 27 ka (Franck et al., 2008, CPD 4: 81-110). We do not have any specific estimates of reservoir ages from the area under discussion, which hampers our interpretation. We, therefore, have been very

careful in our interpretation and refrained, for instance, from inferences about leads and lags in relation to global environmental change. The described phenomena and our interpretation are robust enough to cope with the possibility of different reservoir ages that would change the age model. Luckily, the study area is mostly outside the region of strong upwelling, which constrains the fluctuations in the reservoir ages. This is mentioned in the second paragraph of Section 4. Only around H1, the possibility arises of strongly enhanced reservoir ages as the result of increased upwelling. This is mentioned in the discussion and will be stressed in the new version.

CONCERNING POLLEN TRANSPORT (Referee #3 and F. Chalié) It is true that we do not extensively discuss the different modes of pollen transport from the continent to the marine site, but we have made specific remarks opening the discussion in Section 6. Of course it is an important topic. However, recently three papers have been published that explicitly cover this topic discussing marine surface sediment pollen distribution and the results of a transect of sediment cores along the west coast of southern and central Africa (Dupont & Wyputta 2003, Rommerskirchen et al. 2003, Dupont et al. 2007). The first two papers focus mainly on wind transport and the latter approaches the effects of aeolian, fluvial, and ocean transport. The results of the published papers are far too comprehensive to discuss in our CPD paper again. We consider roughly the Angola area as the potential source area for the marine site, which is a considerable region and a generous estimate. Theoretically pollen from an even wider area of Africa could find their way to the site, but such long-distance "travellers" would be swamped by the regional production. The different contributions of wind and rivers to the pollen transport remains difficult to quantify. In our view, the difference in transport agent is specifically relevant during Deglaciation, when in our model the SE trade winds increased and consequently Podocarpus pollen dominate the assemblage. We use the divergences in total pollen versus Podocarpus concentrations resulting in high Podocarpus percentages as an indication for the predominance of wind transport (second paragraph of Section 6.2). Unfortunately, we cannot blindly take the Podocarpus pollen percentages as a measure for wind transport, because we have no independent

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data on the distribution of Podocarpus trees on land.

CONCERNING POOR SAMPLES (Referee #3 and F. Chalié) The poor samples accumulate mainly during the glacial part of the diagram, where pollen concentrations are generally lower, and shortly before 4 ka and the end of PZ6. As F. Chalié mentions, these samples neither have apparent low diversity nor are they dominated by one pollen type. The poor samples also fit well within the pollen zone in which they are classified. In our revision, we will specify in the third paragraph of Section 3 which samples are concerned and add the suggestion of F. Chalié.

HUMAN IMPACT (Referee #3 and F. Chalié) The idea of human impact is attractive. In particular, because the climatic interpretation of the environmental changes during the past 2-4 ka remains unsatisfactory. On the other hand, a scenario relying on human impact must assume very strong influences based on hardly any evidence. Yet, we appreciate the comments.

FIGURE 7 (Referee #3 and F. Chalié) We will replace Figure 7 to take up the suggestions.

COMMENT 3 of Referee #3. Pollen concentration is given in Figure 3.

OTHER SPECIFIC COMMENTS OF F. CHALIE. The mollusc fragments on which most radiocarbon ages have been measured were recognisable enough and in good condition to render re-crystallisation unlikely. We hypothesize shell burrowing, because many living molluscs do burrow. We will insert a remark about the condition on which the estimate of low sedimentation rates for the period 27-22 BP is based. We will follow the recommendation of a more delimited location map and also give information about the elevation of the mountains. Changes in the overlap between fluvial and aeolian source areas through time are feasible but difficult to quantify. See also above. The elemental iron record (Mulitza unpublished results) gives additional indication of increased fluvial input during Deglaciation and early Holocene. Unfortunately this evidence is not yet published or quotable. The "inconsistency" of the Podocarpus pollen

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records from several marine sites cannot simply be reduced to the effects of shore distance or water depth. If anything, we would expect higher and not lower relative amounts of the well-dispersed Podocarpus pollen grains in the distal core. Moreover, another core situated further along the trajectory of the trade winds, does show a relative Podocarpus maximum (as mentioned in the discussion, first paragraph of Section 6.1). I welcome the suggestion to pinn down a more detailed story of Podocarpus in west equatorial/tropical Africa by sifting through the existing marine and terrestrial evidence. Maybe we could write such a paper together? We will follow the suggestion to change "the younger part" to "the upper part". We will also follow the recommendation to include latitudes of cited sites. These sites will also be plotted in a figure. We suggest that the Rhamnaceae pollen grains might represent Ziziphus species because the grains we counted were rather small. Yes, I am well aware of the work of Sowunmi concerning the significance of the oil palm in West Africa – an also of the attacks to her hypothesis by Maley and others. Sowunmi's hypothesis certainly has merit. The occurrence of Elaeis guineensis in the marine sediments off Angola, however, already started around 4 ka. This is rather early to assume (large scale) human activities in the region. A more important reason for us to be cautious with inferences is the lack of corroborating terrestrial evidence, or lack of any terrestrial evidence for that matter. To be frank, we cannot distinguish between pollen from different Hymenocardia species. Each indication for a change in fire regime is rather weak on its own, but put together we think they make a reasonable case.

I don't comment here on the numerous technical comments and corrections other than that we are grateful for the work you’ve done and will gladly follow the recommendations bar four exceptions. 1) We will complete the sentence about "Lake Malawi shows an increase of productivity during the Younger Dryas period" with "interpreted as the effect of strong northerly winds over Lake Malawi connected to a southern position of the ITCZ". 2) I have been informed that "Mbo" means "lake". Thus either "Barombi Mbo" or "Lake Barombi". See the original map of Brenac. 3) The format of reference abbreviations is something that the editor decides, not we. 4) It is technically

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impossible to use empty boxes in Figure 2 because of their small sizes in the drawing.

Interactive comment on Clim. Past Discuss., 4, 111, 2008.