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Climate of the Past Discussions

Interactive comment on "Glacial-interglacial vegetation dynamics in south eastern Africa depend on sea surface temperature variations in the west Indian Ocean" *by* L. M. Dupont et al.

Anonymous Referee #1

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General comments:

This paper presents an exciting and extremely and important dataset from vastly understudied part of the world. As such, and considering the quality of the data, there is no question in my mind that this will be a useful and well-cited work.

Having said that, I feel that the presented discussion (which is sometimes restrictive, tangential and a bit hard to follow) is quite limited, given the potential of the data. For me, this is acceptable as an introduction to the work, although I would have liked to see more discussion of vegetation dynamics, and the range of climatic scenarios that evidently have developed in the region. I look forward to both more focussed and more

extensive studies using on these data.

Specific comments/technical corrections:

Page 2262, line 20: may substitute “winter rainfall zone” for “winter rain area” to correspond with prevailing nomenclature.

Page 2263, lines 3-5: suggest changing as follows: “Others argue that most of South Africa remained under summer rain influence (Lee-Thorp and Beaumont, 1995; Partridge et al., 1999), even including the southern Cape (Bar-Matthews et al., 2010).” This distinguishes between the reasonably founded arguments of Lee-Thorp, Beaumont and Partridge et al. and the conclusions of Bar-Matthews et al, which are based on a record that stands in stark contrast to other regional records, and are inconsistent with all prior climatological and botanical work.

Page 2263, lines 6-9: change to “Not only are the latitudinal position, intensity, and influence of the westerly storm tracks - and with them the extent of the summer rainfall area - insufficiently clarified, but also the impact of local versus Northern Hemisphere insolation on the climate of South Africa is largely unknown.”

Page 2263, line 25: change ka BP to just ka, as the chronology is not derived from 14C ages. Page 2264, line 4-10: suggest leaving out this paragraph, and rather just state that terrestrial sites within or in the vicinity of the Limpopo-Maputo-Incomati drainage were used for comparison, and that the southern East African lakes will be considered in a more comprehensive review that is in prep. The issue is that considering the Intertropical Convergence Zone (ITCZ) as a line and using it for a boundary does not adequately characterise the nature of the South African monsoon region (Trenberth et al., 2000; Wang and Ding, 2008), and the study region’s climate cannot be disassociated from the influence of the ITCZ. It is understandable that the East African lakes be considered elsewhere, but not necessarily for the reason given.

Figure 1: Would be useful to show the watersheds for the drainages considered, and

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to direct the discussion of climate and vegetation specifically to these regions.

Section 3: A new vegetation reference was recently published (Mucina and Rutherford, 2006), and while not critical, may be useful to the authors in the future.

Page 2269, line 9: Could Tarchonathus not be distinguished from Artemisia? As they don't appear to exist in significant numbers this is not an important issue, but generally the two are distinctly identifiable.

Page 2271, line 4: "Drakensberg"

Page 2272, line 1: "might even" may not be the best wording here, as drought-prone Podocarpus would likely prefer valley environments in any case. Generally the 'over abundance' of Podocarpus is a function of its saccate morphology and wind dispersion (which the authors argue is of minimal importance at the site), but it should be considered that if the main transport vector is fluvial transport then there is likely to be a preponderance of taxa such as Podocarpus, which represent relatively humid environments, but are not necessarily indicating a spread of those taxa beyond hydrologically sheltered areas, and across the greater landscape.

Page 2272, line 8-9: While these taxa are prevalent in fynbos, they are not restricted to it, with all of them being found today in the drainage system considered in this paper. Mention of any 'link' may be better placed in a more complete discussion to avoid any potential confusion.

Page 2272, line 23: This paragraph needs some attention in terms of wording, but I would suggest cutting it instead. The statements are quite bold considering the complexity of the record. Podocarpus is in fact most prominent in MIS 5, and thus the description of Podocarpus forests expanding during interstadials (?) does not adequately explain the vegetation dynamics. Also, what evidence is used to infer the spatial distribution of where the various vegetation types grew? Needs more explanation.

Page 2273, line 11: Tarchonanthus is not restricted to dry savannah. While T. cam-

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phoratus does exist largely in the drier savannah east of Wonderkrater and Tswaing, T. trilobus and T. parvicapitulatus inhabit more humid regions to the east of the sites.

Page 2274, line 3: Big statement considering the distribution of sites, which generally come from moister upland regions. I would remove this.

Page 2274, line 6: Similarly, "over...southern Africa" greatly oversells the evidence.

Page 2274, line 6-16: I would remove this paragraph. It is not necessary, and the statements that: 1) cool upland vegetation provided a favourable environment for humans, and 2) and that modern behaviour was the result of especially favourable conditions are highly speculative.

Page 2274, line 19: What is a 'glacial climate'? There is great heterogeneity and variability evident in the record, and the implied lumping into glacial vs. Interglacial units does a very interesting record a disservice, I feel. Reword to something more along the lines of 'glacial-interglacial cycles have had a strong impact on regional climates'?

Page 2274, line 21-22: The temperature difference reported here is not incorrect, but the references are imprecise. Holmgren et al., 2003 do not calculate temperatures from the Cold Air Cave record. Better indicators of past temperature are from the Uitenhage and Stampriet Aquifers (Heaton et al., 1983; Stute and Talma, 1997; Stute and Talma, 1998).The Talma and Vogel (1992) record from Cango Cave is not entirely inconsistent with these data, but it does indicate as much as an 8°C difference between LGM and Holocene temperatures. Their calculations, however, have to be taken with a grain of salt as they do not reproduce some of the expected Holocene trends. Here and elsewhere change notation from 'K' to '°C'.

Page 2275, line 1: wording of this sentence is awkward. End sentence after 'periods'?

Page 2275, line 9: Perhaps in writing 'vegetation change' the authors are referring to the development of mountain scrubland in particular? Or are the authors actually saying there is less 'vegetation change' during periods of higher SSTs? I would think

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rather the former.

Page 2275, line 14-28: Indeed, relatively drier conditions would in no way be required for the EM2 taxa to develop. What is clear is that the mean temperature of the growing season is at or just beyond the limits for their growth. With a conservative temperature depression of 4°C, the area within the drainage basin that met the growing season temperature requirements of Ericaceae and Stoebe-type taxa would more than double. However, I see no implied paradox, at least not based on the taxa included in EM2. Ericaceae and Stoebe-type taxa thrive in regions with 400 – 1000 mm and 50 – 950 mm MAP respectively. Average precip within the basin presently is approximately 600 mm MAP, and thus it could get substantially drier and not inhibit the growth of the EM2 taxa (at least considered as MAP).

Page 2276, line 16: Would be worth citing that this supports the assertions of Partridge, Scholz and Trauth that precession only becomes a dominant factor during periods of high eccentricity.

Page 2276, line 19-24: This is a rather confusing section, perhaps because it relies on propositions by Laepple and Lohmann that are not adequately described here. What is a 'winter sensitive area'? Certainly the Limpopo basin is strongly influenced by seasonal winter aridity, but so too are areas far north of 20°S. What is the meaning - and thus the point - behind "...the southern African summer rain area south of ∼20°S correlates to Northern Hemisphere insolation using Southern Hemisphere forcing"? Is the correlation causal or coincidental? What "southern hemisphere forcing"? This all needs to be explained more clearly.

Page 2276, line 25 – page 2277, line 8: This paragraph should be reconsidered/rewritten. By what criteria do the authors distinguish between the 'summer rain area' and the 'southern African monsoon area'? Much work by Wang and co-authors has addressed this question and found no distinction in this area.

"...the strongest effect on temperature change is in summer resulting in an anti-

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correlation with Northern Hemisphere summer insolation." Effect of what? Over what time scales?

"Through the subtropical anticyclones the monsoon may influence the summer rain region." How may the anticyclones, which are opposing forces, be vectors for monsoonal influence?

"According to Tyson and Preston-Whyte (2000) depends the influence of the Indian monsoon on how much subsidence from the upper-level outflow in July enhances the subtropical anticyclones affecting the subcontinent's aridity in winter." What depends? Several words seem to be missing from this sentence.

Regarding the influence of the ITCZ, it should be considered as a line as the actual zone of convergence is much more extensive. Its position is the result of the complex interaction between the relative intensity of broader pressure cells and circulation systems. To view it (as is commonly done), as something of a wall of water masks its nature, and distorts its usefulness as element of analysis.

Page 2277, line 8: remove 'while'

Page 2277, line 27: "Southern Hemisphere summer insolation"? At which latitude? âĂČ References: Heaton, T. H. E., Talma, A. S., and Vogel, J. C. (1983). Origin and history of nitrate in confined groundwater in the western Kalahari. Journal of Hydrology 62, 243-262. Mucina, L., and Rutherford, M. C. (2006). The vegetation of South Africa, Lesotho and Swaziland. In "Strelitzia." South African National Biodiversity Institute, Pretoria. Stute, M., and Talma, A. S. (1997). Isotope techniques in the study of past and current environmental changes in the hydrosphere and the atmosphere. IAEA Vienna Symposium 1997. In "Isotopic techniques in the study of environmental change." pp. 307-318. International Atomic Energy Agency, Vienna. Stute, M., and Talma, A. S. (1998). Glacial temperatures and moisture transport regimes reconstructed from noble gas and d18O, Stampriet aquifer, Namibia. In "Isotope Techniques in the Study of Past and Current Environmental Changes in the Hydrosphere and the Atmosphere. IAEA

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Vienna Symposium 1997." pp. 307-328, Vienna. Talma, A. S., and Vogel, J. C. (1992). Late Quaternary paleotemperatures derived from a speleothem from Cango Caves, Cape Province, South Africa. Quaternary Research 37, 203-213. Trenberth, K. E., Stepaniak, D. P., and Caron, J. M. (2000). The Global Monsoon as Seen through the Divergent Atmospheric Circulation. Journal of Climate 13, 3969-3993. Wang, B., and Ding, Q. (2008). Global monsoon: dominant mode of annual variation in the tropics. Dynamics of Atmospheres and Oceans 44, 165-183.

Interactive comment on Clim. Past Discuss., 7, 2261, 2011.