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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.

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We address the remarks, suggestions, and comments of anonymous reviewer #1 point for point. The reviewer's text is preceeded by » and followed by «. Our answer starts with A:

»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.«

A: The paper is meant as an introduction to the palynology of the marine site. As promised in the 'Introduction' a paper about the wider, continental, context has been submitted. More papers, e.g. in connection with stable isotope data are planned. Because of the complications of long pollen transport ways to the marine site and the relatively low taxonomic determination, we are cautious in our interpretation. Yet, the material is well suitable for studying regional glacial-interglacial vegetation changes and we tried to do just that in this manuscript. We don't agree that it is not focussed.

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

A: OK

»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)."«

A: OK (thank you for suggesting the elegant phrasing)

»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."«

A: OK

»Page 2263, line 25: change ka BP to just ka, as the chronology is not derived from 14C ages.«

A: OK

»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.«

A: We shall delete the first 'offensive' sentence of the paragraph (we come back to the issue answering the comments about the discussion).

»Figure 1: Would be useful to show the watersheds for the drainages considered, and to direct the discussion of climate and vegetation specifically to these regions.«

A: The upper panel of the figure would get extremely busy if we would put water sheds in. Moreover, the large watershed of the Limpopo would distort the view of the pollen source area. The number of grains declines exponentially with the distance of travel. Although theoretically pollen could come all the way down the river, this is negligable compared to the amounts that are more from the vicinity. The lower panel showing the region in more detail does denote the main river courses.

»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.«

A: Thank you; meanwhile I got my hands on a copy.

»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.«

A: You are right that it is a weakness of my analysis. However, as you say, it is not important in this case.

»Page 2271, line 4: "Drakensberg"«

A: OK

»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.«

A: True. Even more so, because saccate pollen is not only dispersed by wind but by water as well. Still, an increase in Podocarpus pollen abundance should tell us something of an increase in forest distribution. We'll change the sentence into "Types of humid mountain forests probably were more common – possibly also at lower altitudes – during MIS 9, MIS 7, and the later part of MIS 5."

»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.«

A: We'll change the text into "fynbos-like vegetation (as found in high-altitude cool, wet parts of the Grassland Biome; Mucina and Rutherford, 2006)"

»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.«

A: It was meant to be a summary of the above. As such we'll delete it but we'll relocate some aspects to the discussion of the separate endmembers.

»Page 2273, line 11: Tarchonanthus is not restricted to dry savannah. While T. camphoratus 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.«

A: We just cited the interpretation of Scott (1999). We'll add "probably"

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

A: We'll change the text into "cool upland vegetation types, in the terminology of Scott (1999), might have dominated the moister uplands in southern African during glacial periods."

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

A: We do not agree. The amount of published sites indicating the spread of cool upland vegetation types during the last glacial maximum is remarkable. Our record suggests that this has been the case also during older glacial periods.

»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.«

A: OK

»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'?«

A: OK

»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.«

A: This is very confusing. Holmgren et al. (2003) give ranges of temperature change between glacial and interglacial. They write on Page 2316: "However, it is possible to use the mean values of d18O ratios of carbonate and groundwater for the Holocene and Late Pleistocene to obtain an estimate of the temperature difference between the two periods. Given that the Holocene mean values for carbonate (corrected to calcite values) and water are –3.86% and –4%, respectively, and those for the Late Pleistocene are –3.21% and –4.8%, mean temperatures of 16.3°C and 10.6°C can be derived for the Holocene and Late Pleistocene. The difference of 5.7°C compares well with previous estimates of 5–6°C obtained from analyses of dissolved gases from fossil aquifers in South Africa and Namibia (Heaton et al., 1986; Stute and Talma, 1998)."

However, Heaton et al. (1983) write on Page 254: "The interpretation of possible temporal variations in dN is therefore hindered by our uncertainty as to the precise temperature during infiltration. It can only be stated that there is no evidence for variations larger than ± 2‰ for the d15N-value of the nitrate produced in this area during the past 40,000yr. If the surface temperatures 20,000 to 40,000B.P. were 5-10°C colder than at present, then the data suggest variations were generally as little as ± 1‰"

But Heaton et al. (1986) do give temperatures. Is Stute and Talma (1997) not an abstract?

We'll change our reference into "(Heaton et al. 1986, Stute & Talma 1998, Holmgren

et al. 2003)"

»Here and elsewhere change notation from 'K' to '°C'.«

A: OK

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

A: We'll change into "Other studies report limited increase of C4 grasses in South Africa related to colder and drier periods during MIS 4-2"

»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 rather the former.«

A: Yes, text will be corrected.

»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).«

A: We'll pick up your suggestion as follows: "The plants contributing to the EM2-signal are not specifically adapted to aridity but to cooler conditions, while Ericaceae and Stoebe-type also grow in much drier regions than the present-day South African eastern Escarpment. The spread of mountainous vegetation indicates lower air temper-

atures during the glacial on the one hand, while on the other hand the correlation with lower SST suggests a relation between lower precipitation and glacial vegetation. Hence, we infer that lower temperatures combined with moderately less rainfall might have been the driver of the considerable extension of the mountain vegetation in eastern South Africa during glacials."

»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.«

A: Yes, we'll add "which is a feature of tropical climates (Trauth et al. 2003, Clement et al. 2004, Scholz et al. 2011)". Partridge et al. tweaked their data too much to settle the assertion.

»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.«

A: We'll expand the text and describe the Laeple & Lohmann-model as follows: "Their study uses the regional seasonal variation to model glacial-interglacial temperature variability relying on the modern relationship between local insolation and temperature throughout the year. As the seasonal sensitivity to local insolation differs from region to region, Laepple & Lohmann (2009) distinguish between different temperature response regimes. So is the region of southern Africa south of ∼20°S characterised by a summer precipitation maximum leading to evaporative cooling of the surface temperature, which acts as a negative feedback with regard to temperature as function of local insolation.

Such a region has a higher temperature sensitivity in winter than in summer and is, therefore, called a winter sensitive area. The local response in a winter sensitive area at the Southern Hemisphere correlates to Northern Hemisphere insolation although driven by local insolation (Laepple & Lohmann 2009)."

»Page 2276, line 25 – page 2277, line 8: This paragraph should be reconsidered/rewritten.«

A: We'll do that as follows "The precessional component is rather weak in the pollen record. This might be the expression of the region being at the southern limit of the tropics. Of the monsoonal characteristics it receives seasonal tropical rainfall but does not experience the seasonal change in wind direction (Leroux 1983, Wang & Ding 2008). According to Trenberth et al. (2000) the monsoon is explained by a vertical atmospheric structure of divergence in the upper troposphere and convergence in the lower troposphere. South-eastern Africa between 20°S and 30°S lies just south of the southernmost position of that atmospheric structure. The dominance of the glacial-interglacial variability in the record suggests that the monsoon did not have a strong impact during most of the past 300 ka except for periods when eccentricity was strong and precession variability large."

»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.«

A: The main point of the paragraph is to draw a line between the region under discussion (south-eastern Africa between 20° and 30°S) and central Africa (between 20°S and the equator). The variability seen in our pollen record is only partly influenced by the precessional variabilty of monsoonal climate and dominated by glacial-interglacial variability. Although the region clearly has rain during summer, it is rather at the limit of the monsoon for the following reasons.

a) Wang & Ding (2008) use two indexes to define the global monsoon area. Southern

Africa (south of 20°S) only fulfills one criterium, that of the monsoon precipitation index (seasonal tropical rainfall). The other criterium, the monsoon westerly index capturing the seasonal change in wind direction, is not fulfilled.

b) Trenberth et al. (2000) explain the global monsoon by its vertical structure in the atmosphere characterised by divergence in the upper troposphere and convergence in the lower troposphere. Their assessment for Africa shows the southernmost position of the vertical structure in January at 20°S.

c) Leroux (1983) describes the climate domain south of 15°S as trade dominated.

d) Lau et al. (J. Meteorological Society Japan, 85A, 403, 2007) does not recognise southern Africa as one of the six major global monsoon systems of West Africa, South and East Asia, South and North America, and Australia.

e) Laepple & Lohmann (2009) also distinguish between southern Africa and central Africa. They call both regions monsoonal, but their empirical transfer function between daily insolation and temperature differs between the regions. The transfer function for southern Africa is winter sensitive (see above) while for central Africa the model cannot well explain the seasonal cycle.

»"...the strongest effect on temperature change is in summer resulting in an anti-correlation with Northern Hemisphere summer insolation." Effect of what? Over what time scales?«

A: This sentence also refers to the empirical transfer model of Laeple & Lohmann (2009). Unfortunately, an error occurred; "anti-correlation" should have been "correlation". In other words, for central Africa the relation between insolation and temperature is complex but mainly in phase with Northern Hemisphere summer insolation, whereas in southern Africa the relation is in phase with Southern Hemisphere summer insolation. Thus, the correlation between insolation and temperature is opposite between central and southern Africa.

»“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.«

A: This refers to Tyson & Preston-Whyte (2000) who state on page 174-175 at the end of Chapter 11: "What is relevant for southern Africa is the involvement of Indian monsoon air in the ITCZ over Africa in the southern-hemisphere summer (. . .) and the extent to which subsidence from upper-level outflow from the Indian monsoon in July may play a role in enhancing the subtropical anticyclones affecting the subcontinent and its dryness in winter."

»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.«

A: Yes, the ITCZ over land is complex, because the flow discontinuity at the surface is mostly poleward from the zone with maximum convergence, cumulus convection, coudiness and precipitation. However, it is not envisioned that any of the structure migrated over the study region.

»Page 2277, line 8: remove ‘while’«

A: The whole paragraph is rewritten.

»Page 2277, line 27: “Southern Hemisphere summer insolation”? At which latitude?«

A: 30°S (we’ll add it in the text)

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