

## ***Interactive comment on “Masked millennial-scale climate variations in South West Africa during the last glaciation” by I. Hessler et al.***

### **Anonymous Referee #2**

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The presented manuscript addresses links between the high (northern) latitude climate change and the vegetation in the South African tropics and subtropics. This study adds to a number of existing publications of the authors on closely related subjects. In the new study, the authors extended an existing pollen record from ODP Site 1078 into glacial MIS3 and compared the vegetation changes to results from an Earth System Model of Intermediate Complexity (EMIC). Overall, the manuscript is well written and addresses an interesting question that would be generally well placed in *Climate of the Past*. However, there are several major issues concerning the originality and significance of the study. In my review, I will concentrate on the proxy part of the manuscript.

General comments:

A) Although new pollen data are presented in the manuscript, it remains unclear what

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is really original concerning the climate processes that govern the last glacial African tropics. The muting of D-O and Heinrich signals in hydrological proxy records of equatorial Africa is not new but has been previously documented and discussed. For example, in the catchment of the Gulf of Guinea, glacial riverine runoff did not fluctuate in concert with Greenland interstadials and stadials (Weldeab et al., 2007, *Science* 316, 1303-1307). These authors argued that the impact of short-term northern hemisphere climate variability was dampened by glacial oceanic boundary conditions of the eastern South Atlantic. In the revised version, the authors should account for the relevant published information on this subject. They may also state what is new and original in their study and how it differentiates from the existing previous work of the authors.

B) The authors discuss their findings mainly in terms of northern hemisphere climate variability and its impact on the African tropics. The manuscript would profit from a more detailed discussion of the potential links of their study area to (high-latitude) southern hemisphere climate and ocean variability. For example, surface ocean hydrology (and potential dampening of northern-hemisphere climate signals) in the eastern South Atlantic Ocean should be influenced by the Agulhas leakage that again is linked to the Southern Westerlies (Biaostoch et al., 2009, *Nature*, 462, 495-498; Beal et al., 2011, 472, 429-436). Various studies have shown that the variability of the Agulhas Current system has a strong impact on large-scale ocean temperature and climate at various time-scales (Biaostoch et al., 2008, *Nature*, 456, 489-492; Bard & Rickaby, 2009, *Nature*, 460, 380-383).

C) The pollen record of ODP Site 1078 integrates over a vast catchment area, including riverine (Balombo River) and eolian (e.g., Namibian desert) sources. It appears questionable how a single record can be representative for the climate variability covering several vegetation zones. Shouldn't it be more likely that regional differences in precipitation and vegetation response to climate change result in attenuation or even elimination of signals in a single pollen record integrating over the various local vegetation response?

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D) How significant are the pollen data in terms of statistics? The authors counted approximately 300 pollen grains most of which belong to grasses. Ecologically important vegetation groups are only represented by low relative pollen percentages, e.g. “tropical forest” (max. 2%), “Miombo woodland” (max. 3 %), and mountain vegetation (max. 8.5 %). Are these vegetation units and their temporal changes represented with sufficient statistical significance? In order to account for changes of rare pollen grains, a much higher number should be counted.

E) The climate variability of tropical systems commonly contains a strong precessional or semi-precessional component depending on the exact position of the study site. Precessional variability is also reflected in the intensity of the African monsoonal circulation and associated precipitation patterns of the African rain belt. The authors should test for the presence of variability in the precession and semi-precession bands and should discuss why signals are present or absent. This issue could be well included in chapter 5.4.2 Monsoon circulation.

F) Potential taphonomic biases are not addressed sufficiently. The authors should therefore address the potential influence of differential preservation of pollen grains. The pollen record may have also been biased by different sedimentation rates and grains size composition at the study site and changes in oxygen content of the intermediate water masses.

More specific comments:

**Abstract** The first three sentences (lines 2-9) of the Abstract contain introductory information and should be erased. Instead, some more specific information on the “mechanisms that partly cancel out each other” should be given.

**Chapter 3.1 Site description and age model:** The authors should provide more specific information on which planktonic foraminiferal taxa and molluscs have been used for AMS dating since life style and habitats of these organisms may cause significant biases of the dating results.

Chaper 5.4.1 Atmospheric CO<sub>2</sub> concentration: This chapter addresses a more general issue that appears not really relevant for the subject of the paper and should therefore be erased in the revised version.

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Interactive comment on Clim. Past Discuss., 7, 3511, 2011.

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