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Climate of the Past
Discussions

Interactive comment on "Linking catchment hydrology and ocean circulation in Late Holocene southernmost Africa" *by* Annette Hahn et al.

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Rebuttal Anonymous Referee #1

Thank you to the reviewer for the helpful comments on this manuscript! We have tried to include the helpful comments of the reviewer in the modified manuscript (for the moment only the modified figures could be attached). This study represent a multi-proxy approach based on a marine sediment core GeoB18308-1, located on the South Coast of South Africa, offshore the Gouritz River, Southern Cape. The study reconstructs approximately the last 4 ka and additionally presents samples inland within the catchment areas of the Gouritz River itself. The authors interpret their data as demonstrating humid conditions in the Gouritz River catchment during the Medieval Climate Anomaly with lower, but highly variable sea surface temperatures in the Mossel Bay area. On

the contrary they claim that the Little Ice Age was characterized by relatively warm sea surface temperatures in Mossel Bay and arid climatic conditions favorable to torrential flood events sourced in the Gouritz headlands. I am generally excited about this work as it shows new data from an area missing detailed marine/terrestrial records. I think it is a very detailed and solid approach particularly having material from the Gouritz River catchment for "ground-truthing" in a source to sink approach. I am also okay with the conceptional model explaining the atmospheric circulation system which is based on A.L. Cohen and P.D. Tyson 1995. I am in favour of publication of this record, however would like the authors to respond/check some aspects of the paper which I describe below. My main two concerns are the construction of the age model and the interpretation of the data in the LIA. Age Model First of all, if two labs are used, Poznan and Beta lab in this case, it should be shown that the results are consistent between labs. Has a comparison on an aliquot sample been done which shows that both labs come to the same conclusion? Unfortunately, no comparative study of the 2 C14 labs has been done and we have no remaining funds to do so. However, both labs have assured us that they follow stringent procedures to ensure the quality and reproducibility of their results. Table 1 shows all material dated but only Figure 4 caption reveals what was used for the age model thereafter. It should be clarified in table 1, which of the core depths were not part of the age model. As I understand depths 123 cm, 285 cm and the reworked package at 26- 66 cm depth were excluded from the age model. we have added this information also to Table 1 Hence the levels taken into account are 16.5 cm, 69 cm, 125 cm etc. Core depth 69 cm, TOC measured, gives a calibrated age (median) according to table 1 of 1294 cal. Age BP. The level used thereafter is 125 cm, TOC measured, gives a median of 598 cal. Age BP. Also the two levels below are significantly younger than core depth 69 cm dated. Why were these samples part of the age model and not excluded although they could be equally reworked material? Sorry there is a mistake in the caption of Figure 4: the reworked package is at 26-69 cm depth and (not 66cm)! The 1294 cal. Age BP age at core depth 69 cm, was removed from the age model as part of the redeposited package. In fact the TOC sample at 125 cm just plots outside the uncertainty level given by the Baysian age model. Normally this software should give a probability estimate stating how likely this date is part of the age model or not. I somehow also see a mismatch between the table 1 data and Fig. 4. For example the plot shows two TOC point just below the blue shaded area 'reworked package, erosional contact 0 . I believe that this refers to the sample at 60 and 69 cm core depth according to table 1. However, the author writes that samples between 26-66 cm were excluded. So the sample at 60 cm should not be in there. Moreover, 490 cm core depth has a median age of 4720 cal. Age yr BP which is not even part of the axis in Fig. 4. And there are more examples C2 were the cal. age from table 1 does not fit the cal. age on the axis of Fig. 4. If the author could clarify this mismatch and the core depths used and revise. We thank the reviewer for pointing out these errors! There were errors in Tab 1 and Fig. 4 that were responsible for this mismatch. They have been corrected. It is not clear to me why one would calibrate with an SHcal and then with the marine 13 in the core intervals below despite high BIT index in that interval? Moreover, the BIT index wasn 0 t even measured on the same samples the TOC was dated. We understand that this is an unusual approach, it is unfortunately a consequence of working at the marine-terrestrial interface since the TOC in such a nearshore depositional area is bound to be a mix of terrestrial and marine material. This we had to take into account when calibrating our C14 dates. However, we have no way of determining the exact percentage of marine our terrestrial material in each dated TOC sample - our choice of a calibration curve has to be based on interpretations of the available data. Compound specific dating was unfortunately also outside our budget. The solution we offer is therefore relying on the(what we think) best interpretation of the available data: using our various indirect parameters (XRF data, sediment color) and a direct indicator of soil input (the BIT index) we have identified sediment intervals that are marine and other that are fluvial deposits and chosen the calibration curves accordingly. We believe that resulting age model is reliable enough for the scope of this paper, but we are open to suggestions that will help us improve our age-depth estimations! Why was there no radiocarbon dating conducted on foraminifera from the same material? Unfortunately,

dating foraminifera ages are not available here, this has financial reasons, but also because plantic foraminifer do not live at these shallow depths and benthic species are prone to recording the 14C signal of old bottom water masses. Interpretation of the LIA interval: I am not sure the data during LIA supports the claim made for that interval. The author states that there is missing age control in that period due to re-depositional events. So no data is shown. Instead the author concludes that based on redeposited material which can be characterized as reworked soil, the time frame of the LIA must have had torrential rains and flashfloods on the background of an arid climate. I can 0 t see the evidence for that conclusion We agree with the review that we draw this conclusion very swiftly – it draws on ideas from the catchment sample analysis 5.1.1. that we neglected to refer to in the discussion of the LIA climate (5.2.3). We hope that the revised version of 5.2.3. (below) presents the evidence for an LIA Arid climate with flashfloods more clear: Continuous sedimentation at the core site was interrupted at $\sim$650 cal yr BP. One or more erosive event deposits are inferred from the sedimentology (erosive contact at $\sim$60-65 cm; fine sand with intercalated organic layers and lumps from $\sim$30-60 cm) and an age reversal indicated by the 2 radiocarbon dates in this interval ($\sim$1,466 cal yr BP at a depth of 31 cm $\sim$640 cal yr BP at a depth of 60 cm and; Fig. 4; Table 1). Due to the discontinuous nature of the deposition we are only able to curtail the timeframe of deposition to having taken place between $\sim$650 cal yr BP (the youngest age in the event deposit) and a post – bomb date $\sim$13.5cm above the redeposited. From the redeposited sediment package 3 samples have been analyzed organic geochemistry (see Table 3). The average values over the possible timeframe of deposition is plotted in Fig.5. The high BIT-index ($\sim$0.7) indicates that the redeposited package can be characterized as reworked soil material. The averaged δDC31 signature of $\sim$-135‰ is comparable to that of Gouritz river paleoflood deposits described in section 5.1.1. We therefore suggest that the origin of the event-deposited material in core GeoB18308-1 is similar to the origin of these terrestrial paleoflood deposits. Our catchment study (Leaf wax δDC31 of paleoflood and soil deposits-see 5.1.1) indicates that paleoflood deposits are primarily induced by an increase in high latitude precipitation i.e. precipitation in the upper parts of the Gouritz catchment. The shift in δ13CC31 towards slightly more depleted values in the event deposited material in core GeoB18308-1 (average in the redeposited unit: $\sim$-28‰) furthermore indicates that the n-C31 alkanes contained in the event deposit were produced by plants under less water stress (c.f. Collister et al., 1994, Ehleringer and Cooper, 1988) than those deposited before $\sim$650cal yr BP. We therefore infer an increase in upper catchment rainfall inducing floods for the time period of the event deposit(s) ($\sim$300-650 cal yr BP). This roughly falls into the timeframe of the so-called "Little Ice Age (LIA)" recorded as humid throughout the South African WRZ (Meadows et al., 1996; Benito et al., 2011; Stager et al., 2011; Weldeab et al., 2013) due to a northward shift of the SHW (Tyson and Preston-Whyte, 2000; Chase and Meadows, 2007). In the uppermost Gouritz catchment (Seweweekspoort site) a major SHW sourced rainfall regime has been documented (Chase et al. 2015). Desmet and Cowling (1999) indicate that despite the general SRZ regime in the Gouritz catchment, the SHW supply additional rainfall in extreme events. We suggest that an increase of these extreme SHW-sourced rainfall events produced large floods during the LIA ($\sim$300-650 cal yr BP). . ..

neither for the claim, that the SST 0 s in Mossel Bay were warm if there is no TEX data for that core depth presented. The reviewer is correct; we were not able to calculate SSTs for the LIA timeframe due to the confounding influence of the very high soil content in this interval. The warmer SSTs in the LIA is merly something we suggest as a consequence of applying the Cohen and Tyson model to our findings. We see how this is misleading and have reformulated the abstract accordingly and we have removed the following sentence from the conclusion: In contrast; a weakened, more northerly SIA (e.g. during LIA conditions) has the opposite effect: the weaker Agulhas current is less liable for upwelling and the more frequent SHW advect warm surface water plumes onto the Agulhas bank in analogy to the modern day winter situation Moreover, what does the average line for deposit mean for the interval shown in Fig. 5? The average line for deposit represents the averages of all the measurements made in the redeposited sediments. To make this clearer we have modified Fig. 5 and the caption

accordingly.The presentation of the LIA climatic / oceanographic data is difficult due to the lack of age-control in this part of the core. We have however collected data from the redeposited interval that we believe represents the LIA climatic / oceanographic conditions. They can however not be plotted against time in figure 5 since these are not continuous, but event deposits. WE therefore opted for presenting averages of the measured data points in the event deposits. Obviously we have done a bad job in presenting this data. This leads the reviewer to enquire what the average line for deposit means. We hope the modified fig. 5 is easier to read. The SST conclusion in this paper is with odds of Zinke et al., 2014 (Zinke, J., B. R. Loveday, C. J. C. Reason, W. C. Dullo, and D. Kroon (2014), Madagascar corals track sea surface temperature variability in the Agulhas Current core region over the past 334 years, Sci. Rep., 4. doi: 10.1038/srep04393) who show that Agulhas Current SSTs cooled through the Little Ice Age. How can these opposing findings be explained? Moreover, I think there should be more evidence for that claim presented. Thank you for this reference. As stated above we have no SST data for the LIA timeframe so unfortunately we have no basis for a discussion. 2) Specific questions/issues: Page 2 line 4: I feel that a more African specific chapter of the IPCC report should be cited here rather than Metz or Kirtman et al : "Niang, I., O.C. Ruppel, M.A. Abdrabo, A. Essel, C. Lennard, J. Padgham, and P. C3 Urquhart, 2014: Africa. In: Climate Change 2014: Impacts, Adaptation, and Vul- nerability. Part B: Regional Aspects. Contribution of Working Group II to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Barros, V.R., C.B. Field, D.J. Dokken, M.D. Mastrandrea, K.J. Mach, T.E. Bilir, M. Chatterjee, K.L. Ebi, Y.O. Estrada, R.C. Genova, B. Girma, E.S. Kissel, A.N. Levy, S. MacCracken, P.R. Mastrandrea, and L.L. White (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 1199-1265. Done Page 2 line 20: There are more recent studies by now showing insolation driven responds of Southern Africa climate and should be cited here: (Daniau, A.-L., M. F. Sánchez Goñi, P. Martinez, D. H. Urrego, V. Bout-Roumazeilles, S. Desprat, and J. R. Marlon (2013), Orbital-scale climate forcing of grassland burning in southern

Africa, Proceedings of the National Academy of Sciences, 110(13), 5069-5073. doi: 10.1073/pnas.1214292110); (Simon, M. H., M. Ziegler, J. Bosmans, S. Barker, C. J. C. Reason, and I. R. Hall (2015), Eastern South African hydroclimate over the past 270,000 years, Scientific Reports, 5, 18153. doi: 10.1038/srep18153) We have added Daniau et al. 2013 and Simon et al. 2015 Page 3 Line 1: Biastoch et al., 2009a does not show that strong SHW reduce leakage into the SA and should not be cited here in this respect. This study only shows what effect shifting the SHW to Leakage strength has. It does not evaluate what a change in the strength of the SHW does to leakage variability. In this respect the citation of Durgadoo et al., 2013 in the line below is wrong as in this paper the authors show that an equatorward shift in westerlies increases leakage and not like written in this paper page 3 line 4:" a weakening of the Agulhas Current and the leakage of warm water due to northward displacement of the SHW". Both references were removed Page 5 line 21: Not sure how this description of the bathymetry fits into this part of the oceanography. Would suggest shifting that. The descriptions of the bathymetry have been shifted to be included in the section 3.1. Page 6 line 29: Is that a valid common method to calibrate XFR scans? I would rather think that taking sub-samples and analyzing for bulk major and trace elements would be the way to do it? One approach could be following the below: "Prediction of Geochemi- cal Composition from XRF Core Scanner Data: A New Multivariate Approach Including Automatic Selection of Calibration Samples and Quantification of Uncertainties By G. J. Weltje, M. R. Bloemsma, R. Tjallingii, D. Heslop, U. Röhl and Ian W. Croudace." Yes we did take sub-samples and analyzed them for bulk major and trace elements, it is not expressed clearly in our methods section, but we have added that 28 dried and ground subsamples were analyzed for bulk major and trace elements for calibration purposes. Page 8 line 19: Why and how was the original method modified? Does the modification have advantages compared to Hopmans protocol? If so that should be stated there. This can be answered in the methods section (expert co-authours: Gesine Mollenhauer and Enno Schefuss). Page 9 line 27: To be statically significant one have to at least count 150 specimens per sample

not only 20. This can be answered in the methods section (expert co-authours: Peter Frenzel and Stephanie Meschner). Page 12: By which evidence sedimentological etc. was a paleosoil and a flood de- posit distinguished? Only by different dD values? That should be better described and presented in the text. Soil horizons and flood deposits were also distinguished by their sedimentology in the field. We have added the following information to 5.1.1.:"Catchment samples were all taken at lowland locations, however some were identified as soil samples from horizons of darker, finer material while others were identified as flood deposits by their lighter, coarser facies (Fig. 6)." Page 12 line 10: why would rainfall in the highlands auto- matically lead to flood events? We do not infer that this is automatically so in every catchment, but it seems to be the case in the Gouritz catchment; the layers that we have identified sedimentologically as flood deposits contain organic material that was synthesized under different conditions than the plant material contained in the soils. The deuterium values of the flood deposits are depleted – this gives an indication of their origin as rainfall becomes deuterium depleted with origin. We hope to have made this chain of thought clearer in the text. Page 14 line 15: If that is stated then values should be given as well. As the age model was derived from a Baysian approach one can give an uncertainty value here for the age model. Added: (+/-2Æą: 835-1100cal yr BP) Page 14 line 17: The recent review paper by Should be included in that part of the manuscript. Also Nash, D. J., G. De Cort, B. M. Chase, D. Verschuren, S. E. Nicholson, T. M. Shanahan, A. Asrat, A.-M. Lézine, and S. W. Grab (2016), African hydroclimatic variability during the last 2000 years, Quaternary Science Reviews, 154, 1-22. doi: http://dx.doi.org/10.1016/j.quascirev.2016.10.012 Woodborne, S., G. Hall, I. Robert- son, A. Patrut, M. Rouault, N. J. Loader, and M. Hofmeyr (2015), A 1000-Year Carbon Isotope Rainfall Proxy Record from South African Baobab Trees (<italic>Adansonia digitata</italic> L.), PLoS ONE, 10(5), e0124202. doi: 10.1371/journal.pone.0124202 should be added here as their record also shows the wettest period was c. AD 1075 in the Medieval Warm Period. Nash et al., 2016; Woodborne et al., 2015 included Fig. 5: For comparative purposes other regional paleoenvironmental records are plotted. How was secured that there are no age model offsets between this study and the other records? We see the concern of the reviewer – no age model is prefect, but each record is based on a relatively reliable (published) independent age depth model. We do not see how we can improve this. 3) Technical corrections Fig. 2: page 29 line 5: legend says Carr et al., 2014 in the figures in the map it says Carr et al, 2015 Done Fig. 5 Could do with more labels on the Y-Axis i.e. at least 500 year tick labels between tick marks. Done Page 12 line 12: twice ‘mainly used here! Rephrase grammar is wrong in that part of the sentence! Done Page 12 line 26: Formatting issues and missing space. Done Page 13 line 21. Fig. 5 shows the main record only till 4 ka according to the axis however the text states: ” The oldest part of the 18308-1 paleorecord (âĹij 4880-1150 cal yr BP) Has been corrected to 4058 cal yr BP ...... where is the rest of the data? Where are the figure captions of the supplement? I have added these to the bottom of the paper And what are the dots in SF1? The calibration samples or the subsampling for the organic geochemistry? No, the discrete measurements (in mg/kg) for calibrating the XRF scans are plotted as squares. âĂČ

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/cp-2016-100/cp-2016-100-AC1-supplement.pdf

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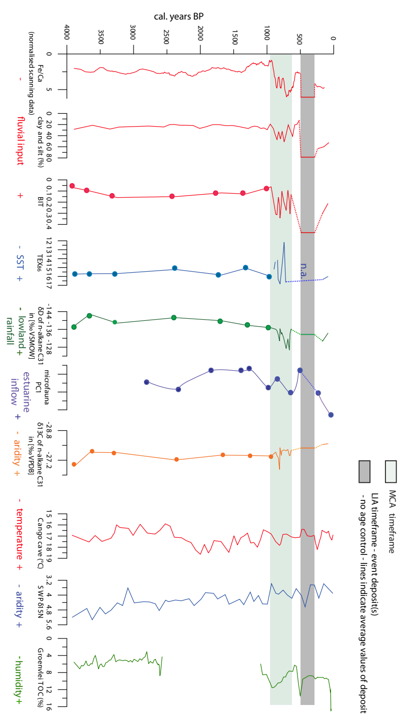

Fig. 1.

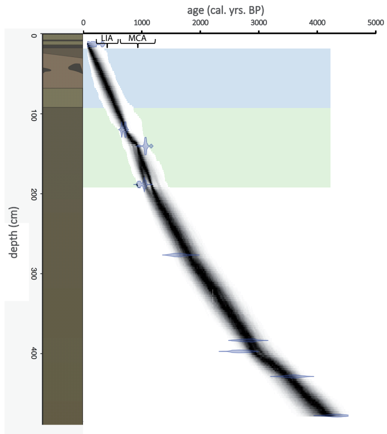
age (cal. yrs. BP)
0
1000
2000
3000
4000
5000
LIA
MCA
depth (cm)
0
100
200
300
400

Fig. 2.