

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

Anonymous Referee #1

Received and published: 12 December 2016

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 par-

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ticularly having material from the Gouritz River catchment for “ground-truthing” in a source to sink approach. I am also okay with the conceptual 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? 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. 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? In fact the TOC sample at 125 cm just plots outside the uncertainty level given by the Bayesian 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’. 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

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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.

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't even measured on the same samples the TOC was dated. Why was there no radiocarbon dating conducted on foraminifera from the same material?

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't see the evidence for that conclusion neither for the claim, that the SST's in Mossel Bay were warm if there is no TEX data for that core depth presented. Moreover, what does the average line for deposit mean for the interval shown in Fig. 5? 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.

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.

Urquhart, 2014: Africa. In: Climate Change 2014: Impacts, Adaptation, and Vulnerability. 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.

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-Roumazielles, 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)

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”.

Page 5 line 21: Not sure how this description of the bathymetry fits into this part of the oceanography. Would suggest shifting that.

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

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the way to do it? One approach could be following the below: “Prediction of Geochemical 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. Bloemsa, R. Tjallingii, D. Heslop, U. Röhl and Ian W. Croudace.”

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.

Page 9 line 27: To be statically significant one have to at least count 150 specimens per sample not only 20.

Page 12: By which evidence sedimentological etc. was a paleosoil and a flood deposit distinguished? Only by different dD values? That should be better described and presented in the text. Page 12 line 10: why would rainfall in the highlands automatically lead to flood events? Page 14 line 15: If that is stated then values should be given as well. As the age model was derived from a Bayesian approach one can give an uncertainty value here for the age model. Page 14 line 17: The recent review paper by 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> Should be included in that part of the manuscript.

Also Woodborne, S., G. Hall, I. Robertson, A. Patrut, M. Rouault, N. J. Loader, and M. Hofmeyr (2015), A 1000-Year Carbon Isotope Rainfall Proxy Record from South African Baobab Trees (*Adansonia digitata* 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.

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?

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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

Fig. 5 Could do with more labels on the Y-Axis i.e. at least 500 year tick labels between tick marks. Page 12 line 12: twice 'mainly used here! Rephrase grammar is wrong in that part of the sentence! Page 12 line 26: Formatting issues and missing space.

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 (~4880-1150 cal yr BP).where is the rest of the data?

Where are the figure captions of the supplement? And what are the dots in SF1? The calibration samples or the subsampling for the organic geochemistry?

[Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-100](https://doi.org/10.5194/cp-2016-100), 2016.