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Climate of the Past Discussions

Interactive comment on "Madagascar corals reveal Pacific multidecadal modulation of rainfall since 1708" *by* C. A. Grove et al.

Anonymous Referee #1

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Overall quality: This is a badly written and confusing paper. I do not think the authors provide sufficient evidence to support their inferences regarding multidecadal modulation of rainfall in the western Indian Ocean. Before embarking on the various analyses, the authors must first demonstrate clearly that the various tracers measured in the four corals: a) share common signals on inter-annual timescales, and b) that these shared signals have a climatic or environmental interpretation. For example on Line 83, the authors refer to Grove et al (2010) demonstrating a significant correlation between one of the coral records (MAS1 G/B) and "regional" rainfall. Grove et al (2010) do report a significant correlation of this coral G/B record and rainfall at one station (i.e. not regional) – the correlation of annual values is 0.30 – suggesting an extremely low amount of common variance (<10%) between the coral and environmental record. Where is the evidence to support the authors' climatic/environmental interpretation of

the other coral tracers? I think there are sufficient high quality climatic data for the western Indian Ocean over the past 40-50 years (e.g. SST, gridded rainfall products) for the authors to undertake calibration and verification exercises and thus demonstrate that the corals are indeed recording climate.

As the focus of the paper is interannual to decadal time scales, it would also be extremely helpful for the reader to see all original annual time series referred to in the text plotted on the same time axis, ideally as anomalies from a common time period mean (e.g. 1950-2000) and with a suitably weighted decadal time scale filter (this could usefully go in Supplementary Materials). This would allow the reader to visually assess the levels of agreement between the different coral and environmental tracers and should, importantly, be supplemented by a table of correlation coefficients and their significance levels

The authors made measurements of the different tracers in four different corals though these are at various points in the paper presented for single corals (e.g. Figure 5) or a 3-core composite (e.g. Figure C1) – this is very confusing.

The authors also use various different temporal filters with no clear supporting rationale e.g. a) 50-70 year (Figure 2) – this also seems a rather long filter length for the interdecadal timescales of the PDO; b) 10-year running means (Figures 5 & 6) – also note that running means are not a good method for filtering data as they can introduce spurious periodicities (e.g. Mitchell et al 1966 WMO Technical Note No. 79) – weighted filters are more appropriate; c) 120 month low pass filter (Figure 5); d) 360-month low pass filter (Figure 6); and e) "13 point smoothing" (Figures C1 & C2). Why so many different filters – either explain why each one was applied or simplify and consistently use just a couple which emphasise decadal variability.

In summary, I do not consider that this paper is suitable for publication without both significant rewriting and also, most importantly, additional statistical analyses that demonstrate that the coral tracers analysed contain a significant proportion of climate/environmental variance on interannual and decadal timescales.

Specific comments:

Line 1: This is as far as I can see the only mention of "North America" in the paper – why does it appear in the first line of the Abstract? The PDO also influences rainfall elsewhere than Australia and North America.

Line 7: "massive" rather than "gigantic" is a more correct description.

Line 7: does "four" refer to the 4 corals used or the measured climate/environmental tracers?

Lines 9-10: Is this really the "first evidence" for PDO influencing Indian Ocean rainfall? What about findings of Deser et al (2004), Reason & Rouault (2002) – both of which cited by authors?

Lines 24-26: Is this relationship between Indian Ocean SSTs and Sahel rainfall relevant to the present study which is focussed on eastern Africa and Madagascar?

Line 36: What are "temperature troughs"?

Line 39: Does this paper really provide insights into "Indian Ocean rainfall"? How does rainfall variability in Madagascar relate to Indian Ocean region rainfall variations – the wider regional significance of rainfall reconstructions obtained from Madagascar corals could, for example, be assessed using available rainfall data sets (e.g. Smith et al 2008 J Geophys Res 113, doi: 10.1029/2008JD009851); a similar assessment of the regional significance of SST reconstructions from Madagascar corals would also be useful.

Line 42: Does Lough (2007) really provide a record of "changing land-ocean interactions"? These rainfall/river flow reconstructions have also now been superseded by Lough (2011 Paleoceanography 26 doi:10.1029/2010PA002050).

Lines 42-44: The authors do not provide 300 years of soil erosion from 4 corals – only

one coral extends back to 1708.

Line 45: Spell out PDO as first mention in main body of text.

Lines 52-53: There are several other published reconstructions of the PDO (e.g. Biondi et al 2001 J Climate doi:10.1175/1520-0442; MacDonald & Case 2005 Geophys Res Lett doi: 10.1029/2005GL022478; Shen et al 2006 Geophys Res Lett doi: 10.1029/2005GL024804; Linsley et al 2008 Palaeocean doi:10.1029/2007PA001539 – these should also be mentioned and also that they do not seem to agree prior to the 20th century calibration period; why did the authors choose to use the D'Arrigo & Wilson (2006) reconstruction?

Line 56: What does "thought to exceed anomalies associated with ENSO" mean?

Lines 58-60: Not really clear why Australian rainfall is introduced here; but if kept should also cite various papers that provide evidence that the phase of the PDO modulates the strength of ENSO teleconnection patterns and associated rainfall anomalies over eastern Australia (e.g. Power et al 1999 Clim Dyn doi: 10.1007/s003820050284; Verdon et al 2004 Water Resources Res doi: 10.1029/2004WR003234; Meinke et al 2005 J Clim doi:10.1175/JCLI-3263.1).

Line 61: It would be very helpful for the reader to conclude with a brief outline of the questions addressed in this study, i.e. where the rest of the paper is going.

Lines 67-69: How were the corals dated? From density bands or geochemical tracers?

Line 72: "fourth" not "third" coral.

Line 75: How was growth rate measured?

Lines 76 & 77: Why not use term "annual growth bands" as is common in the literature rather than "laminae"?

Line 77: Surely the fact the cores were sliced has to come before describing the X-ray prints?

Lines 81-82: Lough et al (2002) do not attribute appearance of luminescent lines to humic acids, just that they are a good proxy for freshwater flood plumes.

Line 86: "starts" rather than "ends".

Line 90-91: MAS1 (p3) is said to start in 1904 not 1906? Previous paragraph states MAS3 starts in 1930 not 1935!

Line 91: Sr/Ca used as "indicators of SST" – where is the statistical demonstration that the Sr/Ca ratios in these corals are capturing a significant amount of SST variance?

Line 92: No "reconstruction" of "suspended sediment runoff" is provided in this paper! The authors simply present the Ba/Ca time series and infer that these are proxies for suspended sediment.

Lines 95-96: Abram et al (2003) interpret elevated Mn ratios in their corals to a phytoplankton bloom not "an indicator of ash fallout from slash and burn deforestation". Similarly Lewis et al (2007) do not attribute Mn ratios in their corals as "an indicator of ash fallout from slash and burn deforestation" but to initial erosion of topsoil associated with land clearing.

Line 98: What does "a high level of accuracy" mean?

Line 99: The Materials and Methods section should also provide a description and appropriate references for the instrumental climate and paleoclimate data used in this study; It should also include a description of the statistical "Methods" used to analyse the various data sets.

Lines 100-112: Unclear what this contributes to the paper; also, if included, what about SST variations rather than air temperatures – I do not see how the latter relate to coral records.

Line 115: The "Results and Discussion" section should start with a clear demonstration that a) the different coral records share common variance, and b) that the coral records

capture climate/environmental variability; Thought should also be given (assuming the 4 different corals are providing similar records of interannual variability related to climate) to defining a composite index which should then be consistently used in the rest of the paper.

Lines 116-117: The authors measured G/B ratios in the corals and inferred they are proxies for humic acids.

Lines 116-128: Where is the demonstration that the MASB (or any of the other coral G/B series) are reliable rainfall proxies? This is absolutely essential before proceeding to subsequent analyses. What about the other reconstructions of PDO – why was this one chosen?

Line 127: "positive rainfall anomalies" – where?

Lines 129-134: Already said (though incompletely) in Materials and Methods.

Lines 134-144: Almost impossible to follow, especially in absence of clear climate: coral relationships.

Lines 148- 151: Incorrect references – see earlier comment.

Lines 151 onwards: I am getting totally confused and find it almost impossible to determine what the authors did and why. They need to clearly separate their new results and then follow with a discussion of their interpretation of these with reference to the relevant literature. There seems to be a continual "tweaking" of time series to different frequencies and reference to individual coral records rather than a set of consistent (composite) coral series.

Line 206: "first evidence"? No demonstration that corals are recording "southwest Indian Ocean rainfall" – see several earlier comments.

Lines 224-225: What does "without using an agent" mean? What is "CIO SST"?

Lines 246-247: Maybe they are unrelated? How is this relevant to the study? With what confidence can it be said that the Mn concentrations are due to "slash and burn deforestation" in this region?

Lines 249-250: Linear trends rather than "linear equations"; also see earlier comments regarding use of 10-year running means; why was linear trend analyses not undertaken on original annually-averaged data?

Lines 251-252: References to Lewis et al (2007) and Abram et al (2003) incorrect as do not relate to "ash fallout".

Lines 263-292: Unclear why these analyses were undertaken and the authors several times invoke different unsupported arguments to account for when their results do not agree.

Lines 493-505: Figure 2 – This is a correlation map so it shows the pattern of SSTs associated with both phases of the PDO; why was the 5% significance level not used? (NB terminology is usually 95% confidence level or 5% significance level);

Lines 518-533: Figures 5 and 6 – as indicated earlier unweighted running means (here 10-year running means) are not an optimal method of presenting time series data as they can introduce spurious periodicities; "Note that multi-decadal oscillations.........high coherence with SST" – why not convince the reader by providing some statistical analyses to back up this statement?

Line 550: How was the "composite" series formed? Why is a "13 point smoothing" used? Is this a running mean, cf earlier comments.

Lines 565-570 Figure D1: Given the authors have used a global rainfall data set, why have they not shown that the coral G/B records (which are inferred to be rainfall proxy) are correlated with rainfall (see several earlier comments).

Supplementary Material: Explanation of the composite coral G/B series provided – but explain what "normalising" was undertaken (e.g. with respect to mean and/or SD)?

Interactive comment on Clim. Past Discuss., 8, 787, 2012.