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Climate of the Past Discussions

Interactive comment on "ENSO and internal sea surface temperature variability in the tropical Indian Ocean since the Maunder Minimum" *by* Maike Leupold et al.

Anonymous Referee #2

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General comments This paper seeks to address the question whether or not more El Nino than La Nina events occurred in the Central Indian Ocean, that was found by the study of Roxy et al. 2014 for the western Indian Ocean. The Indian Ocean has been warming over the past century and the cause of this warming may be linked to warm El Niño events and/or a lack of La Nina cooling events. To address this question, the authors sampled and produced coral-based Sr/Ca-SST reconstructions with three sub-fossil corals and one modern coral cored alive in 1995. The three sub-fossil cores were dating with two U-Th each with errors of $\sim$1-2 years that are confirmed by the coral geochemistry. They then assess the coral records using a wide variety of data and spectral analyses methods to assess the records for the presence of ENSO events.

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Discussion paper

They report similar magnitudes of El Nino and La Nina events in their coral records for the three intervals of the Little ice age they investigated. This work has the potential to provide some insight into central Indian Ocean interannual variability for snapshot internals since 1675 CE during the Little Ice Age period observed in Europe; however, the manuscript presented here should be refined to make their research question and results clearer and more convincing.

Specific comments While this study is addressing an important question and producing valuable coral SST reconstructions for a location with few such records, this reviewer finds they do not address the research question posed for their study, were there more El Nino events than la Nina in a robust manner. They do look at magnitudes of these events but not the “asymmetry” they discuss in the introduction or that as suggested by Roxy et al. 2014. This should be a straight forward analysis to test this question but the authors use a wide variety of software programs and several data analysis methods to try and address this question that leads to confusion and as a whole, misses the point of their analysis. For example, they spend considerable time and present several figures with spectral analysis that look for periodicities/frequencies in their data. Since El Nino and La Nina are opposites phases of the ENSO variability or “periodicity” they are looking for, the spectral analysis tells you nothing about whether or not more El Ninos occurred than La Ninas. Spectral analysis is suggestive of periodicities similar to ENSO but is NOT conclusive evidence, see Hochman et al. 2019 (doi: 10.1175/jamc-d-18-0331.1) and Liu et al 2007 (doi: 10.1175/2007jtecho511.1). A large anomaly with the width of 2-7 years can be manifested as a significant 2-7 year periodicity in a spectrum leading to the misinterpretation of ENSO periodicity (try for yourself, do a FFT spectrum and wavelet spectrum of the volcanic explosivity index and compare). Furthermore, why do breakpoint detrending, removing monthly anomalies, etc. it is not necessary to answer your question. Additionally, using one-tie point per year to build the coral chronology introduces a large amount of uncertainty to your time series, especially in the monthly anomalies that could mask any real signal in time and frequency, see figure 12 of Williams et al. 2014 (http://dx.doi.org/10.1016/j.gca.2014.04.006), and

Table 5 in DeLong et al., 2014 (doi:10.1002/2013PA002524). If you are removing the annual cycle from your data, at least two tie points should be used, four is better otherwise your residuals will have a annual cycle still there that introduces spectral noise. There are a considerable number of other studies that look at ENSO variability to address similar questions. Why "reinvent" the data analysis approach? Just use the methods everyone else uses, band pass filter to remove low frequency variability (> 10 year) and trends and higher frequency annual cycle, see collective work of Kim Cobb's lab, (Cobb 2003, 2013, Sayani 2019, Grothe 2019 doi: 10.1029/2019GL083906; Chen et al., 2018 https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2018GL077619, Nurhati et al., 2011 DOI: 10.1175/2011JCLI3852.1) and McGregor 2010 (www.climpast.net/6/1/2010/) not to mention the excellent work by Hereid et al., 2013 (doi:10.1130/g33510.1 and doi: 10.1029/2012PA002352) and the new study published by Lawman et al. 2020 doi: 10.1029/2019PA003742 where they use ENSO variability via histograms and probability density functions to assess ENSO variability in the past that built upon the work of Emile-Geay et al 2016 where they used probability density to assess ENSO variability in a network of coral and mollusks reconstructions and climate models (DOI: 10.1038/NGEO2608). Furthermore, McGregor et al., used a Cluster Analysis to assess El Nino and La Nina amplitudes in fossil corals (DOI: 10.1038/NGEO1936) and they use wavelets to band pass filter their coral reconstructions in their 2011 paper (doi:10.1016/j.gca.2011.04.017). The PAST software you are using is capable of doing band pass filters. My second concern is the coral Sr/Ca records in the fossil corals that show large cold anomalies (up to 6°C?) in Figure 4. The labels in this figure are hard to read but a 4-6°C anomaly is not expected, even for a La Nina event. The anomaly in BoddamB (1856-1862) spans ∼6 years and would be manifest in a spectral analysis as a 6-7 periodicity. Look at the Wavelet spectrum for this coral, it will show you if this periodicity is center on this anomaly and this would be why you see 6-7 year peak in Figure 6b. Same could be said for Eagle 3 (1890-1894) and the three year peak. Please include the wavelet spectrum from each series in your paper (better than the spectrums you have and more convincing if not driven by

these anomalies). Back to the cold anomalies. Looking at the x-radiographs: B8 from (1856-1862) appears fuzzy, could this be dissolution or suboptimal alignment of the corallite to the slab surface (see DeLong 2013 doi:10.1016/j.palaeo.2012.08.019)? If you were to resample this time interval to the far right of that slab, is that cold anomaly still there? I would guess not. Coral E3 has the anomaly from 1890-1894 and this is over the core break in the x-ray image. Do these two paths overlap and how well do they agree with each other? The second path is very close the edge of the coral, could there be local diagenesis there? If you were to sample the second core piece just below the first path, is that cold anomaly still present? For Core Eagle5, the mean shift occurs as the sampling paths shifts from the top to bottom piece of the coral. If you sample a different path with optimal corallites, is this shift still there? All this shifts may be real but any large anomalies should be replicated to see if local diagenesis or suboptimal sampling produce the anomaly. I will note: if you use band-pass filters for your ENSO data analysis, these shifts are less meaningful, but you should make sure your coral Sr/Ca is reflecting the SST signal and not something introduced by sampling. Please include a figure of your raw coral Sr/Ca data with paths in depth in your supplemental materials. Additionally, mark where the XRD and SEM samples were removed from eh slab. It is possible to get pockets of diagenesis in small areas of the coral away from where you did the XRD, thin section, and SEM samples. See Quinn 2006 doi:10.1029/2005GL024972; Sayani 2011 doi:10.1016/j.gca.2011.08.026, Hendy 2007 doi:10.1029/2007PA001462).

The public comments have already questioned the use of the Maunder Minimum in the title and as a climate interval or temporal marker. The paper makes not connections to solar cycles and ENSO variance in the central Indian Ocean and the coral do not span the entire Maunder Minimum so why mention it in the title? I suggest the use of the Little Ice Age in its place, as the records presented are part of this interval and that term is accepted within the climate and paleoclimate literature.

The authors need to improve their review of coral Sr/Ca reconstructions in the Indian

Ocean. While it is true that there are not many records currently published from the region, there are more than the authors suggest, seven by my count. Line 38-39 if there are few coral Sr/Ca studies, why not list them all to be comprehensive and not just cite the authors own papers. I count 7 studies so is that really a few? Hennekam 2018 doi: 10.1002/2017PA003181 Zinke 2014 doi: 10.1038/ncomms4607 Zinke 2004 doi: 10.1016/j.epsl.2004.09.028 Zinke 2008 doi:10.1029/2008GL035634. The introduction section would also benefit from a more in-depth review of the literature on coral-based SST reconstructions of ENSO, both from the Indian Ocean perspective and also the Pacific Ocean. Lawman et al. (2020) in Paleoceanography and Paleoclimatology, McGregor et al. (2019) in Nature Geosciences, Grothe et al. (2019) in Geophysical Research Letters, and Tangri et al. (2018) in Paleoceanography and Paleoclimatology would all be useful for comparison, and have data available online. These and other ENSO reconstructions can be used for comparisons between basins back to the 1600s.

I question the authors' decision to count all positive SST anomalies in their coral records as El Niño events, despite the fact that they acknowledge the existence of warm IOD events occurring independently of ENSO (Section 2.2 Climate, lines 92-93). If the authors are comparing other ENSO records to this one, why not remove any positive anomaly events that are unconfirmed by other ENSO records as potential IOD events? Or, why not also compare their record with IOD records? Barring the complete removal of IOD-associated events from the record, I think it would be worthwhile for the authors to compare reconstructions with and without the positive SST anomalies that are not confirmed ENSO events to provide a more complete perspective on potential overestimation of El Niño frequency and strength. I also recommend that the authors review recent literature regarding the IOD, including the recently published Abram et al. (2020) Nature article reconstructing the IOD back to the 13th century AD.

In section 2.4, "ENSO Indices", the authors list the indices that they use for comparison with their coral records. However, they do not discuss whether these records are coher-

ent, or how they vary, over time. It also appears that they generated their own Niño3.4 anomaly record, which they call an index. From what I understand, the Niño3.4 index only extends back to 1870, using HadISST, not ERSST. While I applaud the authors for applying their own analysis to the data, it is unclear exactly how they calculated their anomaly record from the ERSST data, and as such they need to describe that process in more detail. Do not call the Wilson ENSO reconstruction Nino3.4, that name has already been taken, just call it Wilson ENSO.

Especially questionable is the application of the Quinn 1993 record (Ortlieb 2000 provides an updated version), which is subjective and based on written records, though I understand the authors are limited in the number of records that they can use due to the limited temporal scope of most ENSO records. I'm particularly confused as to why they did not compare some of their 19th century records to the extended multivariate ENSO index (MEI.ext), which spans 1871-2005 (Wolter and Timlin, 2011), or the more recent series of indices published by Sullivan et al. (2016) that include central, eastern, and mixed-type ENSO events back to 1854? Or any of the other ENSO reconstructions on the NOAA paleoclimate website, there are several to choose from (Cobb 2013, McGregor, 2010, Li 2011, Braganza 2009, Cook 2008, Gergis 2009).

At the very least, a comparison between the two main indices used (earlier than table 6/section 4.5) would greatly strengthen the authors' conclusions and help the reader understand their criteria surrounding the selection of El Niño events from these records for comparison. The authors cite Wilson et al. (2010), which analyzes the coherence between several ENSO reconstructions extending back to the 17th century, but do not address the paper's conclusion that inter-reconstruction coherence breaks down in the 19th century. Thus, using the Wilson et al. (2010) record to identify individual events in the late 17th – early 19th century seems questionable. Labeling this record Niño3.4 was also confusing, making it hard to differentiate between the Wilson record and the ERSST-based anomaly record from the Niño3.4 region.

This paper has a lot of potential, but needs extensive work. I commend the authors for

attempting an in-depth analysis of their data, but encourage them to consider alternative methods for analysis that would be both simpler to accomplish and ultimately more powerful in their application.

Technical corrections

Figure 6 The authors do not standardize their spectra in time, so that it becomes difficult to interpret the individual plots of Figure 6. Most of the plots are based on monthly resolved data with frequency as cycles/month, except for 6e which is based on annually resolved data and cycles/year and is thus shifted in frequency space.

In section 3.1 “Coral collection and preparation” more information about the x-ray system used and the settings applied in the generation of the x-radiographs would be helpful for replication or reproduction by later studies. Are these x-ray positive or negative images? It would also be useful to know how the coral collected from the derelict building arrived there – was it via human activity or storm or tsunami deposited? This is not necessary for publication, but could help guide the location and collection of other specimens.

In section 3.2 “Coral Sr/Ca analysis” was just one standard or known value used in the ICP analysis? Most labs use 2 or 3 (a gravimetric, a coral, and JCP international standard). The Schrag (1999) and de Villiers et al. (2002) methods bracket each sample for drift correction. which is typical for ICP-OES whereas every 5th sample is used for ICP-MS since that instrument does not drift as much. The exact analytical precision(s) $\pm$ 1sigma should be given with # of measurements and error bars of analytical precision on all graphs with coral Sr/Ca. It would also be good to see the raw Sr/Ca values plotted, not just anomalies. It is difficult to gauge the individual records from the anomaly plots alone.

In section 3.3 “Chronology” the authors suggest that they only use the minima of seasonal SST cycles as their chronological tie points, but their chronology would likely be more robust if they used at least 2 ties points (maxima and minima) for time assign-

ment.

In section 3.5 “Statistics”, it would be helpful to know which version of PAST (with citation) and MATLAB the authors used. I am confused as to why the authors chose to use the web application T-Test Calculator (web link needs to be given) rather than a t-test function in the other software listed or just use a t-table in a statistics textbook. Also, in general, the authors tend not to list the α, n, or other key statistical values for their data throughout the paper (except in some figures). All averages should be report with their standard deviations, and number of values, correlations should have p-value and n, and all errors as either 1 or 2 sigma, which are standard statistical practices.

In section 4.4, “ENSO Interannual SST variability”, the authors suggest that all of their coral records show statistically robust typical ENSO periodicities (3-8 years), but fail to address varying levels of statistical robustness. Their earliest composite record (E5, Figure 6a) for example has an ENSO periodicity that is only statistically significant at the α=0.1 level, but the authors do not discuss this in the text. Despite detrending before analysis, there is also evidence of roughly annual periodicities in both B8 (Figure 6b) and E3 (Figure 6c). Figures S8-10 and S11, supplementary analyses, are cited as confirming the power spectrum analysis results, but also bring out issues in the temporal continuity of these spectra and their directionality.

The Brönnimann et al. paper was published in 2007, not 2006 (this issue could be present in other references, and should be checked).

The GIM coral data seem to have been first published in Pfeiffer et al. 2009, not 2017.

All of the supplemental figures are mislabeled, and should be corrected. I recommend, in fact, that the entire Supplemental file be carefully reviewed and edited, as I noticed consistent issues in the labeling of materials and numerous typographical errors.

Ln 30: The opening sentence of the introduction reads a bit awkwardly, I would suggest rewording to something like “As the impacts of global climate change increase,

paleoclimate research is more important than ever". On the same line, I would remove the first word of the second sentence ("Especially") and simply begin the sentence with "The Indian Ocean. . .". Ln 32: should be "basin", not "basing". Ln 34: Remove "As" and begin the sentence with "Tropical corals". Ln 35: the sentence here continued from Ln 34 is somewhat awkwardly worded, and should be ended with "variability" not "variabilities". Ln 37-39: the sentence in this section repeats its point in the second half, I would delete the section after the first citation of Pfeiffer et al. 2006. Ln 40: change "are focusing on" to "focus on". Ln 41-42: change "lack of data in" to "lack of data from", remove "still" and "the" from the phrase "still limits the" and replace "the" with "our", and change "variabilities" to the singular. Ln 46: change "In fact, it was suggested" to "It is suggested". Ln 68: change "form" to "from". Also recommend moving the phrase "from October to April" from beginning to end of sentence. Ln 166: "Composite" should be "Composites". Ln 200 and 203: ranges in both of these lines contain values to three significant digits, while all others reported in paper are only to two. Ln 293: there is a period missing between "Indian Ocean" and "For". Ln 308: the end of the sentence here should read "Brönnimann et al. (2006) (Table 6)". Ln 313: should read "Indian" not "India" monsoon. Ln 337: remove the "events" before "non-La Niña", and make sure to correct the spelling of La Niña.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2020-22/cp-2020-22-RC2-supplement.pdf

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Discussion paper