Response to Reviewer #2 comments by Leupold et al.

We are grateful for the feedback provided by an anonymous reviewer. The reviewer raises fourteen (RC2 –1 to RC2 – 14) specific comments, which are addressed in detail below. Additionally, technical corrections are provided by the reviewer, which are addressed below, as well. Furthermore, additional comments to individual points of the manuscript are provided in an annotated pdf. In the following, we will repeat the reviewer's statements (in bold font) and our reply to it. Below the responses to these specific comments, we respond to the technical corrections and to the additional comments on the manuscript except for cases where e.g. typos are highlighted.

General Remarks to the comments of Reviewer #2:

Reviewer 2 has problems with understanding our concept of ENSO asymmetry, which **does not refer to the question whether there are more El Ninos than La Ninas**, but their **magnitude in terms of SST anomalies** in the Indian Ocean. A quote from the abstract of our original manuscript (Line 17-19): 'El Niño events have occurred more frequently during recent decades **and** it has been suggested that **an asymmetric ENSO teleconnection (warming during El Niño events is larger than cooling during La Niña events)** caused the pronounced warming of the western Indian Ocean.' We agree that our manuscript requires an unambiguous definition of ENSO asymmetry in the central Indian Ocean and we will provide this in the introduction of a revised version of our manuscript.
We do not aim to reconstruct ENSO frequency with the central Indian Ocean corals, as done in numerous studies with corals from the tropical Pacific (cited as examples by Reviewer 2), where ENSO dominates and causes large SST anomalies that can be unequivocally identified in coral proxy data. ENSO (and IOD)-induced SST anomalies are small in the Indian Ocean (~0.5-0.7°C, see Figure 2 of our manuscript and Roxy et al., 2014) relative to the background variability (~0.3-0.4°C in the peak ENSO season from December-February; see Figure below) and their identification requires a reference record of past ENSO events. We are aware of the excellent coral reconstructions from the tropical Pacific that record past ENSO events that would be ideal for this purpose. However, to date all these reconstructions are restricted to certain time windows and do not cover the entire time intervals of our central Indian Ocean corals. We therefore used the classical list of ENSO events from Quinn alongside with the updated list of Brönnimann et al., 2007 (https://doi.org/10.1007/s00382-006-0175-z), who evaluated and synthesized a number of ENSO reconstructions (ERSST NINO3 by Smith and Reynolds, 2004 (https://doi.org/10.1175/1520-0442(2004)017<2466:IEROS>2.0.CO;2); Mann NINO3 by Mann et al., 2000 (https://doi.org/10.1175/1087-3562(2000)004<0001:GTPIPC>2.3.CO;2); Cook/D'Arrigo NINO3 by Cook, 2000 (https://www.ncdc.noaa.gov/paleo-search/study/6250) and D'Arrigo et al., 2005 (https://doi.org/10.1029/2004GL022055); Stahle SOI by Stahle et al., 1998 (https://doi.org/10.1175/1520-0477(1998)079<2137:EDROTS>2.0.CO;2); Quinn and Neil extreme El Niño events by Quinn and Neal, 1992 (Quinn, W., & Neal, V., 1992: The historical record of El Niño events. Climate Since AD 1500: 623–48. R. Bradley and P. Jones.)). So, our interpretation does not rely on an outdated version of ENSO events. Rather, by including the original list of Quinn, we aim to evaluate the sensitivity of our analysis to different ENSO reconstructions.

The excellent coral IOD reconstruction of Abram et al. (2020) was published after the submission of our manuscript. However, Abram et al. (2020) demonstrate the tight coupling between the IOD and ENSO during the past millennium, lending confidence to our approach.

Anonymous Reviewer #2

Specific comments:

RC2 - 1
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.

The term 'ENSO asymmetry' is based on the conceptual work of Burgers and Stevenson ('The Normality of ENSO', 1999, GRL, vol 8) and An and Fin ('Nonlinearity and Asymmetry of ENSO', 2004, Journal of Climate, https://doi.org/10.1175/1520-0442(2004)017<2399:NAAOE>2.0.CO;2). ENSO asymmetry refers to the fact that El Niño events are often stronger than La Niña events, as seen in the tropical Pacific. This does not always apply to teleconnected sites. For example, Brönninam et al. (2007) find that 'the responses to El Niño and La Niña are close to symmetric' in Europe during winter and spring.

In the abstract of our original manuscript, we state that: 'El Niño events have occurred more frequently during recent decades **and** it has been suggested that **an asymmetric ENSO teleconnection (warming during El Niño events is larger than cooling during La Niña events)** caused the pronounced warming of the western Indian Ocean.' (Line 17-19)

Based on the comments of reviewer 2, we assume that he believes we aim to address the question **whether or not more El Niños occurred than La Niñas. This is not the point addressed in our manuscript.** We do not want to focus on the frequency of past EN/LN events or to address the question whether or not more El Niño than La Niña events occurred in the central Indian Ocean. The question we address is: do El Niño events warm the Indian Ocean more than La Niña events cool it. However, we agree that the main aim of our study should be expressed more clearly. We will define what is meant by ENSO asymmetry and explain why we investigate it in the introduction of a revised version of our manuscript. We will also shorten the sections on the time series analysis, as these were only used to show that ENSO periodicity is observed in the coral records and may distract the reader from our main results.

RC2 - 2

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).

We used different approaches to test if ENSO frequencies are present in the coral time series: Power Spectrum, Singular Spectrum and Wavelet Coherence Analysis. While the power spectra do not provide conclusive evidence of ENSO, the Wavelet Coherence Analysis does: it shows that there is a positive correlation between an ENSO index (we used the Nino3.4 from Wilson et al. 2010; see discussion further below) and the coral time series at interannual periodicities. However, we realize that this analysis is actually more important than the Power Spectra, and we decided to exchange the figure 6 with figure S11 (wavelet coherence analysis).

RC2 - 3

Furthermore, why do breakpoint detrending, removing monthly anomalies, etc. it is not necessary to answer your question.

For detrending we used published methods by Mudelsee (2000; https://doi.org/10.1016/s0098-3004(99)00141-7) and Mudelsee 2009; https://doi.org/10.1140/epjst/e2009-01089-3). Detrending was necessary to compile the composite records. We then also used this detrended data for the power spectrum analysis. Removing monthly anomalies is a standard procedure to investigate interannual variability.

RC2 - 4

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 ad-dress similar questions.

We developed the age model following the pioneering work of Charles et al., 1997 (https://science.sciencemag.org/content/277/5328/925), who has proposed to use the month of August as one single anchor point in any given year at the Seychelles, a site located slightly further west than Chagos with a similar monsoon-dominated SST seasonality. Charles et al. have demonstrated that with their approach, monthly anomalies can be computed from coral proxy data, and that these monthly anomalies can be correlated (calibrated, in fact) with instrumental SST anomalies. See Charles et al., 1997, Figure 2 B.

Due to the strong cooling of the western and central Indian Ocean following the onset of the Indian summer monsoon in boreal summer, which is seasonally phase-locked, this age model is very precise (the non-cumulative age model error is +/-1 month in any given year). Each additional anchor point would introduce an additional error which, in the Indian Ocean, tends to be larger during the other seasons of the year.

We note that other studies recommended by reviewer 2 as examples also rely on one anchor point per year, e.g. McGregor et al., 2013 (https://doi.org/10.1038/ngeo1936); Hennekam et al., 2018 (https://doi.org/10.1002/2017PA003181).

The approach proposed by the Reviewer (using more anchor points in any given year) would only be applicable at sites that have large-amplitude, sinusoidal seasonal cycles, where age model errors become a problem in the transitional seasons in fall and spring due to the rapid change in SST during a short time period. In fact, the examples cited by the reviewer are from sites with large seasonality, in particular the paper of Williams et al. 2014 (http://dx.doi.org/10.1016/j.gca.2014.04.006), that focuses on red algae from high northern latitudes.

To validate our approach, error estimates based on the standard error where shown in the composites for each mean monthly value.

RC2 - 5

Why "reinvent" the data analysis approach? Just use the methods everyone else uses, band pass filter to remove low frequency variability (> 10year) 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; Chenet 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.clim-past.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.

We are aware of the excellent work of Kim Cobb's lab and the many other excellent coral-based ENSO reconstructions from the tropical Pacific. For reconstructing ENSO frequency, we agree that the approaches mentioned above are appropriate. However, as mentioned above, reconstructing ENSO frequencies is not our aim and we therefore did not apply any of these methods. Instead we show with our wavelet coherence analysis results that there is a positive correlation between an ENSO index (we used the Nino3.4 from Wilson et al. 2010 (https://doi.org/10.1002/jqs.1297); regarding this index see further below).

RC2 - 6

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 fora La Nina event. The anomaly in Boddam B (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).

This anomalously cold peak in Boddam 8 is a relative extreme peak in a phase of generally colder sea surface temperatures. Such decadal variability in the Indian Ocean was already describe in Cole et al., 2000 (https://doi.org/10.1126/science.287.5453), Pfeiffer et al., 2006 and 2009 (https://doi.org/10.1130/g23162a.1; https://doi.org/10.1007/s00531-008-0326-z) and Charles et al., 1997 (https://science.sciencemag.org/content/277/5328/925), and the typical periodicity is 9-13 years. We therefore interpreted this anomaly as one cold event in a decadal cooler phase (note: a 6 year cold interval would not result in a 6-7 year, but in a ~12 year periodicity, as the cold interval would only represent one half of a warm-cold cycle). The extreme peak lasts < 1 year and the absolute value of this cold event is smaller than 4-6°C: the difference between the cold anomaly event and the minimum peak the year before or the year after this cold peak, respectively, is only 2.6-3.1°C. Large short-term cool events are possible as Chagos lies in a region where open ocean upwelling occurs (see Leupold et al., 2019; https://doi.org/10.1029/2018GC007796).

We agree, however, that this figure is too small to be read properly. We will therefore add a larger version of this figure to the revised version of the manuscript.

As mentioned above, we will exchange our power spectrum analysis plots with the wavelet coherence analysis plots (Figure S11). The wavelet coherence analysis was performed with the Nino3.4 from Wilson et al., 2010 (https://doi.org/10.1002/jqs.1297) and shows that there are EN/LN signals recorded in our corals from the central Indian Ocean.

RC2 - 7

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 X-ray of Boddam 8 shows traces of saw-cuttings, as the original slab cut in the field was a bit thin for further cutting. We will mention this in the Figure caption of a revised manuscript. This has nothing to do with the preservation of the sample. However, the orientation of the corallites can be seen clearly on the X-ray image. They are always parallel to the slab surface. In fact, the coral shows very even annual growth bands, so irregular growth patterns are not a problem.

All sampling paths were selected so that we get a continuous record for each coral sample. This includes also both sampling paths on coral slab E3. For this sample, there is an overlap of 10 mm, which means 10 subsamples, for each sampling path, i.e. there is around one year of overlap.

Regarding possible effects of diageneses: our diagenesis screening revealed that diagenetic modifications to the Sr/Ca record are neglectable. The combination of optical and scanning microscopy and XRD measurements is a well-established method for detecting diagenetic alterations in carbonates by Smodej et al., 2015 (https://doi.org/10.1002/2015GC006009), which has already been applied in several studies, e.g. Deik et al., 2019 (https://doi.org/10.1002/dep2.64), Hallenberger et al., 2019 (https://doi.org/10.1038/s41598-019-54981-7), Pfeiffer et al., 2019 (https://doi.org/10.1029/2019PA003770), Utami & Cahyarini, 2017 (http://journals.itb.ac.id/index.php/jets/article/view/2270), Zinke et al., 2016 (https://doi.org/10.5194/bg-13-5827-2016). The method of Smodej et al. (2015) can identify localized areas of diagenetic calcite and we can therefore also assess potential heterogeneities in preservation. Note that the samples for SEM and XRD measurements were taken from the edges of the coral slab where we expect the 'worst' preservation. From there, also the sub-samples used for U/Th measurements were taken. U/Th is even more sensible to diagenesis than coral Sr/Ca, but our U/Th data shows consistent results with small age errors. We can therefore conclude that even the edges of the coral slabs do not show significant amounts of diagenetic alterations.

We can include a figure of our raw Sr/Ca data in a revised vision. We will also provide better indications where XRD and SEM samples were taken from the coral slabs.

Please note that decadal-multidecadal temperature variability is common in the tropical Indian Ocean and has been described in numerous studies, e.g. Charles et al., 1997 (https://science.sciencemag.org/content/277/5328/925); Cole et al., 2000 (https://doi.org/10.1126/science.287.5453); Pfeiffer et al., 2009 (https://doi.org/10.1007/s00531-008-0326-z); Hennekam et al., 2018 (https://doi.org/10.1002/2017PA003181). The anomalies seen in our Sr/Ca data are in the range of observed temperature variability at Chagos (Pfeiffer et al., 2009) and there is no reason to suspect that they are artefacts from sampling or diagenesis.

RC2 - 8

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.

We agree and as already mentioned in our reply to this public comment, we used the term Maunder Minimum in a misleading/incorrect way. We will adjust it in the text and also change the title of the manuscript. However, we will probably use "…since 1675" instead of "Little Ice Age" as suggested by the reviewer, so that everyone is aware of the exact time interval we are focusing on.

RC2 - 9

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.

With "few" we meant relative to studies using d18O to reconstruct environmental parameter or relative to studies conducted in the western or eastern Indian Ocean. However, we will go through this paragraph and add above mentioned literature in a revised version.

RC2 - 10

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 the1600s.

We will include a more in-depth review of the literature on coral-based SST reconstructions of ENSO in the introduction of the revised version.

RC2 - 11

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.

The study by Abram et al. (2020) is indeed a very important one. It states that there are extreme IOD events that occurred independently of ENSO, but that there is also that "a persistent, tight coupling existed between the variability of the IOD and the El Niño/Southern Oscillation during the last millennium." This supports our approach.

In fact, as it can be seen in Table 6 in the manuscript, all positive anomaly events found in the coral records can be explained with El Niño events listed in either Quinn 1993 or Brönnimann et al., 2007 (https://doi.org/10.1007/s00382-006-0175-z). We just wanted to point out, that there is the possibility, that such events can overlap with IOD events or can even occur independently. Abram et al. (2020) named three extreme IOD events (2019, 1961, 1675) that occurred independently of ENSO. However, both in 1675 and 1961 no positive anomaly events can be found in our records.

Furthermore, the main focus of our study was to study the ENSO teleconnection between Indian Ocean and Pacific Ocean. Unfortunately, the coral time windows of Abram et al. only partly overlap with our data. In a revised version we can mention the IOD events that do not coincide with ENSO events as documented in Abram et al., 2020.

RC2 - 12

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 coherent, or how they vary, over time. It also appears that they generated their own Niño3.4anomaly 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.

Regarding the introduction of indices we used we agree that it might cause confusion as we used two indices named Niño3.4. However, we did not generated our own index. We will rename the Wilson Nino3.4 index so that there will not be any confusion and explain in more detail which index we used to show what.

RC2 - 13

Especially questionable is the application of the Quinn 1993 record (Ortlieb 2000 pro-vides 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).

The goal for future coral paleoclimate studies should be to compile a consistent coral data product which overlaps with the entire time period that is studied. However, up to this point there does not exist such continuous index which overlapped with our records.

We are aware that the Quinn record is based on written records. However, we did not only rely on the Quinn record from 1993. We **compared** Quinn 1993 with the list of ENSO events compiled in Brönnimann et al., 2007 (https://doi.org/10.1007/s00382-006-0175-z). We believe that this gives some indication of the sensitivity of our results with respect to different ENSO reconstructions.

Both records cover all our coral time windows, including our 17th century coral record. We wanted to use as few indices as possible, and the same indices for all coral time windows shown in our study, for consistency. Brönnimann et al. (2007) combined several reconstructed ENSO indices (ERSST NINO3 by Smith and Reynolds, 2004 (https://doi.org/10.1175/1520-0442(2004)017<2466:IEROS>2.0.CO;2); Mann NINO3 by Mann et al., 2000 (https://doi.org/10.1175/1087-3562(2000)004<0001:GTPIPC>2.3.CO;2); Cook/D'Arrigo NINO3 by Cook, 2000 (https://www.ncdc.noaa.gov/paleo-search/study/6250) and D'Arrigo et al., 2005 (https://doi.org/10.1029/2004GL022055); Stahle SOI by Stahle et al., 1998 (https://doi.org/10.1175/1520-0477(1998)079<2137:EDROTS>2.0.CO;2), climate field reconstructions and early instrumental data and also assessed the data for consistency.

RC2 - 14

At the very least, a comparison between the two main indices used (earlier than table6/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 the19th 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 Niño3.4was 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.

In section 2.4 of the initial submitted manuscript we already introduce all indices we used for this study. However, we agree that we can explain more in detail which index we used for what so that it does not lead to any confusion. For example, we did not use the index by Wilson et al. (2010) for identifying single ENSO events, as we are aware of the papers conclusion that inter-reconstruction coherence breaks down in the19th century. This is in fact the reason why we decided to use the lists of events from Quinn (1993) and Brönnimann et al., (2007). However, for Wavelet Coherence analysis, we need time series data, and for this purpose, we used the Wilson et al. (2010) record. In a revised version of the manuscript, we will place more emphasis on explaining and comparing the ENSO indices. We will shorten the manuscript by omitting unnecessary discussions on spectral analysis, and table 6/ section 4.5 will be made more central.

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.
We did not standardize these plots because they do not have to be compared with each other. Every sub-figure is there to show ENSO periodicities and each resolution is mentioned in the figure caption. However, we decided to exchange this figure with the figure showing the Wavelet Coherence Analysis anyway (see above).

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.
We will provide the additional information about the x-ray system in the revised version of the manuscript.
The derelict buildings were indeed built by humans living on the islands. However, it is not known whether they found their material as boulders on the beach or if they quarried them on the island to get their building material. There are no written records from Chagos from this time. As there can be found hundreds of boulders at the beaches of Chagos nowadays (see Figure S1a) it is likely that the Chagossians first used material they found at the beaches close to their Colony to build their buildings. This is also suggested by the shape of some of the corals found in the walls.

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)±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 total, 5 different standards were used, including the international standards JC-p-1 and JC-t-1. They were measured before and after the entire measurement sequence. We will explain this in more detail in the methods section in a revised version. Furthermore, we will add error bars on all coral Sr/Ca graphs.
During method development for Sr/Ca analysis we started off with standard-sample-standard bracketing as in Schrag et al. (1999) but found that inserting 6 samples did not compromise our results at all. (We re-measure every 12^{th} coral sample at the end of each measurement run, and we find no evidence of drift problems). A similar strategy is also used in isotope geochemistry. The resulting uncertainty of 0.8 permil (1SD) in our data is speaking for itself and is much better than all ICP-MS data we know of. The very general statement of reviewer 2 that ICP-OES instruments drift more than ICP-MS instruments is not valid, at least for our instrument.

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 assignment.
See our comment below reviewer comment RC2 - 4.

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 at-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.

We agree, that these information should be added and we will do it in a revised version.

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 (Figure6b) 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.

Annual periodicities are still visible, because only the long-term trend was subtracted and not the annual cycle. However, as mentioned above, we will exchange this figure with figure S11 and show the wavelet coherence analysis plots instead.

Additional Comments in Manuscript PDF (initial text of the manuscript with line numbers and in italic, reviewer comments in bold with our answer below each comment):

L 37: "There are only some studies including Sr/Ca measurements for SST reconstructions (e.g. Pfeiffer et al., 2006),…"

Comment 2: if there are few, why not list them 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.2004.09.028Zinke 2008 doi:10.1029/2008GL035634.Zinke 2016 doi: 10.5194/bg-13-5827-2016 Bryan 2016 doi: 10.5194/bg-13-5827-2016Abram 2020 https://doi.org/10.1038/s41586-020-2084-4

"Few" was meant relative to studies using d18O to reconstruct environmental parameter. But we will go through this paragraph and add above mentioned literature in a revised version.

L 40: "…and/or are sampled at only bimonthly (Zinke et al., 2004; Zinke et al., 2008) or annual resolution (Zinke et al., 2014; Zinke et al, 2015)"

Comment 3: Bimonthly meaning every two months or sampled twice per month?
Regardless, bimonthly is probably fine for resolving the seasonal cycle, just as well as, monthly. So what is the point you are trying to make here?

Bimonthly means in this case every two months. For resolving the seasonal cycle, it is of course better to have 12 values per year than 6 values.

L 46: "…demonstrating an existing stable SST-ENSO teleconnection between the Pacific Ocean and Indian Ocean…"

Comment 5: How do you know this is stable? The premise of your paper is to assess if it weakens or exists in the past 200 years. Delete "stable"

With "stable" we meant "stationary". We see that we did not explain it very well. We will define what we mean with "stationary".

L 49: "This asymmetric ENSO teleconnection has been suggested to contribute to the overall
50 warming of the tropical Indian Ocean."

Comment 6: The use of "asymmetrical" was confusing from the abstract to here. At first I thought you were referring to a spatial asymmetry but you mean a temporal or different response to La Nina-El Nino events. Scientist talk a lot about the ENSO spatial pattern so this is easy misinterpretation. Why not use a better term? Yes, Roxy 2014 use the "asymmetry' term but their paper is confusing as well. Help the reader out and explain better what is meant by asymmetrical ENSO teleconnection between Indian and Pacific oceans.

We agree, that we have to explain better what we mean with "asymmetrical ENSO teleconnection". We will do this in a revised version.

L 53: "…the core top (1950-1995) was shown to record SST variability at Chagos on grid-SST scale (Pfeiffer et al., 2009)."

Comment 7: Explain what grid-SST scales are? Do you mean a particular gridded SST data product(s)?

Pfeiffer et al. used ERSST version 2, but that data was consistent with other SST products such as HadISST.

L 55: "We identify past warm and cold events in each record and use these events to compile composites to evaluate the symmetry of positive and negative ENSO-driven SST anomaly events in the tropical Indian Ocean."

Comment 8: By Symmetry you mean the magnitude of the La nina nad El nino events are the same or not. Why not just say you are looking at magnitude differeneces?

As mentioned above, we will explain what "ENSO asymmetry" means in a revised version. The concept is widely used in conceptual papers on ENSO and ENSO teleconnections

Comment 9: Roxy 2014 Fig 5 shows Western Indian Ocean has most of this warmer El nino events, not the central Indian ocean.

We agree, that the western Indian Ocean is most affected by warming as shown in Roxy et al., 2014 using HadISST data. However, this warming trend is still visible in the Seychelles-Chagos-Thermocline-Ridge region, which also suffers from a lack of observations on historical timescales (see Pfeiffer et al., 2017; https://doi.org/10.1038/s41598-017-14352-6). Furthermore, Roxy et al. used HadISST data, OI SST data shows a much stronger response in the equatorial Indian Ocean.

L 101: "Both anomaly records are not significantly different (t-value = 0.34; p-value = 0.37)."

Comment 12: 2011 la nina has different magnitudes.

We are not sure what the reviewer wants to point at with this comment.

L 154: "…measured in 2017 in the HISPEC laboratory of the Department of Geosciences, NTU, following techniques described in Shen et al. (2012). These age determinations are consistent with our Sr/Ca chronologies."

Comment 14: **what about dating uncertainties? were U-th a single annual band? how and where were these taken, please mark x-ray images to dating samples.**

We marked where the U/Th samples were taken on the X-ray images (Figure S2). The age model was developed in the following way: For each coral sample: 1st age dated by U/Th (in 2016) → from this age band the years were counted on the x-ray images (and combined with raw Sr/Ca) → upper or lower most counted year was compared with 2nd age dated by U/Th (in a second measurement run in 2017). As every second age that was dated with the second U/Th measurement fitted to the age model developed using the x-rays and raw Sr/Ca data, dating uncertainties due to sampling for U/Th measurements are neglectable.

L 625: Figure 6

Comment 23: **Do all in years, not months! make log log plots. This is confusing since you do not have units on the frequency. looks like a-d are in months and not years. Put units on all graphs. time interval for ERSST.**

See our comment below the first technical comment by the reviewer regarding Figure 6.

L S109: Figure S11

Comment 24: **How do you do this? Nino 3.4 is modern SST.**

We agree, that we used the term Nino3.4 in a way that led to confusions. In this case, wavelet coherence analysis was performed for each coral Sr/Ca record with the Nino3.4 index by Wilson et al., 2010. This index extends beyond the instrumental period, until 1607. We will change the name of this index so that it will not lead to any confusion anymore.