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Interactive comment on "Spatial and temporal variability of Terminal Classic Period droughts from multiple proxy records on the Yucatan Peninsula, Mexico" *by* Stephanie C. Hunter et al.

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Comments: General comments: Thank you for this opportunity to review the manuscript "Spatial and temporal variability of Terminal Classic Period droughts from multiple proxy records on the Yucatan Peninsula, Mexico", by Stephanie Hunter, Diana Allen, and Karen Kohfeld. The manuscript seeks to: 1) objectively and systematically identify drought events in a number of Yucatan proxy indicators and determine to what extent these correspond to the Terminal Classic Period (TCP); 2) identify spatial and temporal differences among these records, and 3) assess potential driving mechanisms of drought events. Some of the manuscript's positive points are its discussion

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of limitations in the data and the application of an apparently objective set of criteria to identify hydroclimatic changes in the proxy records. Overall I think this is an important paper with relevance for multiple fields (paleoclimatology/archaeology) and that it is publishable in Climate of the Past following revisions, and I find the manuscript takes into account (or can be improved so that it does) the criteria/aspects that are outlined under the review criteria on the Climate of the Past website.

Response: First, we would like to thank M. Peros for taking the time to review our manuscript, and for his insightful comments on our work. He has also helped us find some small typos that we missed, so we are very grateful for his attention to detail!

Comments: Specific comments: Fundamentally, the manuscript relies on a comparison of presumed droughts based on the proxy data and its comparison to the TCP. But, unless I missed it, we are left to take the timing of the TCP at face value as 800-100 A.D. What is this date range based on? There are a number of citations in the first sentence (Lines 33-36) but these citations are essentially the proxy data that are used in this paper. In much the same way as the manuscript has a good discussion of the meaning and limitations of the proxy data, I think it would benefit from a short discussion of the actual TCP from an archaeological point of view. What archaeological data are used? What limitations are there in that data? I am not an expert on the archaeology of the region, but my understanding is that the "collapse" – or the period of time this transition occurred - was time transgressive (i.e., occurred at different times at different places). While it might not be in the scope of this paper to attempt to plot those vertical orange bars at different times based on location, acknowledging the nuance in the timing of the TCP that the proxy records are being compared to would be useful, in my opinion.

Response: We noticed some differences across the literature in the time period chosen to represent the TCP; for example, Medina-Elizalde et al., (2010) takes the TCP to be the period from 800-950 C.E., while Turner and Sabloff (2012) says the TCP is from 800-1000 C.E., while Acuna-Soto et al. (2005) states the TCP is from 750-950 C.E. So while there are differences, we chose 800-1000 C.E. to be encompassing of the time

periods most commonly chosen to represent the TCP; however, we agree that adding a discussion of these differences and the archaeological evidence used to date the TCP would be an excellent addition to our manuscript, and can add this to the next version.

Comments: (And on a related note, is the TCP 800-1000 A.D. , or 850-1000 A.D.?) The caption and I think orange bars in Fig. 6 place it at 850-1000 A.D. whereas it is 800-100 AD elsewhere. And should dates be reported in C.E. and not A.D.?)

Response: Thank you for pointing this out- as noted above, there are some discrepancies in what the timing of the TCP is taken to be, but we have tried to be consistent throughout our manuscript. However, we made a mistake with Figure 6; the TCP should be labeled as 800-1000 A.D., and we will extend the shaded orange area to cover this time period as well (this does not affect the results of the analysis).

In addition, your comment about C.E. vs A.D. lead us to look into this more, as we were not aware of the connotations behind the two notations and believed it was simply a matter of preference (and noticed both used in the literature). We found the article published at https://www.thoughtco.com/when-to-use-ad-or-ce-116687 to be particularly interesting, and in the spirit of promoting inclusivity will be changing all the notation within our manuscript to C.E.

Comments: The use of changepoint analysis is interesting and a useful approach I think. I can see how it would be useful to identify changes in mean state (as in Fig 3a) but I wonder about its utility for assessing variance (Fig 3b). And my concern here is that within each timeseries (unless it is the tree ring data which I assume is annual), the temporal spacing (or timing) of adjacent proxy measurements will vary based on the initial sampling resolution and sedimentation/growth rates. I could see this being less of an issue for determining mean state, as I said, but I wonder to what extent this affects the variance measures. In this technique, does the data need to be evenly spaced, and if it is not, what kind of effect does this have on the results?

Response: We agree that in our analysis, the mean changepoints seem to be more

useful than then changes in variance. There were no consistent patterns observed in the variance changepoints across site locations or across the records we used; in fact, most of the records did show that there was a change in variance during or near the TCP, and it was rather the changes in the mean that helped to identify spatial patterns across proxy records. We wanted to include the variance changepoint analysis on the basis that, if more extreme droughts were observed during the TCP, perhaps this would show as a change in the variance at this time period. However, the results of the variance changepoint analysis was inconclusive in this regard. We think it would be useful to include a note about this in our discussion section in the next revision of our manuscript. In regards to your question about the spacing of points in the changepoint analysis, the concept of time is not necessarily taken into account; the r package (changepoint) reads in a list of ordered points that only have an index value associated with them. In the case of the proxy records, for those with lower than annual resolution, each point may represent anywhere from 2 years to a few decades (sample resolution is listed in our Supplementary Information, Table S1). Therefore, while it is possible to still conduct the variance analysis, the changepoints identified don't reflect a single year- they are more of a change in variance between the time periods represented by each measurement. This could be part of the reason why we were unable to identify a consistent pattern in the variance changepoints (that the low sample resolution was not able to capture rapid changes in variance that may have signified the TCP), or it's possible that similar changes in variance were observed all over the Yucatan Peninsula, and that it is not necessarily a useful criteria for identifying local droughts. These points will be added to our manuscript in our discussion of uncertainty regarding the changepoint analysis.

Comments: A large portion of the discussion is devoted to the question of whether the droughts were caused by ITCZ migration, and to do this the authors looks for corresponding changes in reconstructions of ENSO, PDO, and AMO. But to answer this question, would it not be better just to compare the data to a reconstruction of ITCZ position, such as one published by Lechleitner et al., (2017) (and cited on page 14), or

possibly the Ti record from the Cariaco Basin (Haug et al.?) This would seem to be a more direct way to address the question.

Response: This is a good question, and we have two answers to this. It would be very interesting to do a changepoint analysis on the Lechleitner et al. (2017) record of ITCZ variability to compare to our records. F. Lechleitner has been kind enough to share the z-scores from that paper with us, and we would like to add this record to our manuscript along with the ENSO, PDO, and AMO records. As for the Cariaco Basin titanium record (Haug et al. 2001; 2003), while its original interpretation was that it reflected changes in the ITCZ movement, and therefore could be used as an analog for climate on the Yucatan Peninsula, it has been shown that there are distinct differences in the climate signals in the Cariaco Basin record and other Yucatan Peninsula proxies (see Medina-Elizalde et al., 2010), and so we thought it might not be the best record to compare to.

Comments: And I think the analysis of the Mann et al., 2009 reconstructions was a good approach. It is interesting though, because individual proxy records of some of these climate modes show results that seem to differ from the Mann et al., 2009 reconstruction. For example, the Laguna Pallcacocha, Ecuador data (Moy et al., 2002) seems to show positive (warm) phase ENSO between about 800-1100 AD, which would be consistent with southward displacement of ITCZ and drought on the Yucatan. Interestingly, I think (but I could be wrong) that the Mann et al., 2009 reconstruction is based in part on this dataset, but the point is that there is reliable proxy data (from individual sites) that records different activity than the large-scale reconstructions.

Response: This is interesting, and in fact this seems to support our conclusion that ITCZ displacement played a role in the TCP droughts, but that local factors may also have contributed. It is our understanding that the Mann et al. (2009) reconstructions are regional averages of temperature anomalies calculated from global gridded temperature anomalies, which were reconstructed using a global climate proxy network. Therefore, the Laguna Pallcacocha record (Moy et al., 2002) was a part of the Mann et

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al. (2009) reconstructions, but as the averages are weighted, it seems to have played more of a role at different time periods in the reconstructions (see Figure 2 & Figure S6, Mann et al., 2009). So, it is likely that the numerous proxy records that went into making the ENSO reconstruction do have some local differences from the large-scale reconstruction. Looking more closely at the Laguna Pallcacocha record (Moy et al., 2002), the red intensity of the sediment appears to be a proxy for the frequency of warm ENSO events, but not necessarily the magnitude of ENSO events. However, Moy et al. (2002) does note that peak ENSO variability occurs at about 750 C.E. (just before the TCP), and begins to decline after that- perhaps that is related to the change in mean ENSO state observed around 900 C.E. in our analysis? It is perhaps beyond the scope of this paper to look more into that particular proxy record (as we chose to focus only on Yucatan Peninsula proxy records), but it does seems to support our theory of local effects may have contributed to the TCP droughts, rather than just large scale circulation patterns.

Comments: I understand why the charts in Fig 3a and b are plotted by "Index value", which I think is basically the number of the sample starting from the earliest one, but why are the reconstructions in Fig 6 plotted by age? I assume the Mann et al., 2009 reconstructions are annual (I haven't checked recently) but it seems inconsistent.

Response: You are correct that the reason for this difference is that the reconstructions all have annual resolution, and so 1 index value = 1 year (as opposed to the proxy records, where 1 index value may represent numerous years). If this is confusing, we can easily change the axes back to index values.

Comments: Finally, there are improvements that could be made to the figures/tables captions to make the manuscript easier to understand. For example, the caption for Figure 5 says that two locations had records that meet all 4 criteria, and that these are highlighted by red boxes, but there are three red boxes at three locations.

Response: Thank you for catching this typo- the caption should read "The three loca-

tions of the Yucatan Peninsula which had records meeting all four inclusion criteria for drought (highlighted in red boxes)".

Comments: Technical corrections: Line 115: "A couple of" is too casual – please reword. Line 722: There is something unclear about this figure caption... does the mean (top) need to be mentioned when there is already a caption for it? Line 778: Should this be a table and not a figure? Line 794: Typo "at for each".

Response: Line 115: "A couple of hypotheses..." will be changed to "Two hypotheses...". Line 722- We think you meant Line 772: You are correct, we used to have this as one figure but when we posted online, it became two. The first sentence on Line 772 was a relic of that; the first line of the caption will be changed to say "Example results graphs from the changepoint analysis (variance). Line 778: While this looks like a Table, it has been uploaded as a figure due to the graphics added. We will leave this up to the editors of Climate of the Past as to whether they prefer this be described as a table or a figure. Line 794: The typo will be corrected to "for each".

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