

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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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 of limitations in

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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.

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.

(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.?)

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

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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?

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 look 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.

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.

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.

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

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red boxes, but there are three red boxes at three locations.

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”

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