

Interactive comment on “300-years of hydrological records and societal responses to droughts and floods on the Pacific coast of Central America” by Alvaro Guevara-Murua et al.

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General comments

Many thanks for the opportunity to review this manuscript for Climate of the Past Discussions. The paper concerns the reconstruction of hydrological variability over a 300 year period for Guatemala, Central America, using documentary evidence in the form of city and municipal council records beginning in 1640. Given its focus on climate variability in the last 400 years, it is likely to be of considerable interest to the readership of CPD. The authors also make a compelling climatic argument for the results to be more widely representative of conditions along the western coast of Central America during the study period. The scientific approach adopted, and the methods applied, are en-

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tirely appropriate for the nature of the study and indicate rigour. The archival sources used for the study appear to be very rich in detail and have been explored exhaustively. The paper is extremely well written - I would go so far as to say that it is a pleasure to read - and the authors should be commended for this. It is well structured and very well organised (although note my comments below), and suitably illustrated with appropriate tables and figures. The results are discussed in good detail, and make reference to a wide but appropriate range of the previous literature. Overall, I have no reservations in seeing the paper published following the revisions recommended below.

Major point

My main concern with the manuscript is in relation to the results as they are introduced in section 4.1. It is fairly standard within historical climatology publications to provide the reader with an indication of the confidence with which each year in a reconstruction is classified. This could, for example, be in the form of a graphic showing the number of individual quotes used for each year of the reconstruction. If this approach were used, some recognition would, of course, be needed in the text to reflect that a single detailed quote describing climatic variability over a region might provide much more relevant climatic information than 10 less detailed quotes. If this type of graphic is not appropriate for this study, then, following Kelso and Vogel (2007), each year could instead be given a 'confidence rating' (from 1-3) to indicate the confidence with which the authors regard the classification for that year. Given the quality and quantity of material within the documentary sources, I would imagine that most years would be given a high confidence rating. However, lower confidence ratings could be used to highlight some of the concerns for individual years that the authors highlight within later sections of the manuscript (e.g. in the final sentences of section 4.2).

Kelso C, Vogel C (2007) The climate of Namaqualand in the nineteenth century. *Climatic Change* 83:357-380.

Minor points

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1. The opening paragraph of section 3.1 discusses factors which could lead to crop loss or shortages. Given my experience of reading equivalent historical source materials for southern Africa, I am slightly surprised that there is no mention of either pestilence or conflict as having potential impact on harvest yields. If these factors are irrelevant for Central America, it might be useful to include some explanation as to why this is the case. This may need no more than an additional sentence.

2. To assist the reader, the opening paragraph of section 3.4 could make greater cross reference into table 1.

3. When I first read section 4.1 of the script, I was slightly surprised that no qualitative descriptions of, for example, the wettest and driest years within the reconstruction period were included. A few illustrative quotes, for example, would add real "colour" for the reader to an otherwise fairly brief and "dry" section. I then read on and discovered that descriptions of individual events were embedded within section 5.1 under the heading of "Discussion". I appreciate that some of the quotes are used to highlight discrepancies or difficulties in classifying individual years (see my major point above). However, the authors might consider a little reorganisation to move some of the descriptive material from section 5.1 into section 4.1.

4. The second paragraph of section 5.3 notes that the recent observations that warm ENSO conditions are associated with lower precipitation might be a non-stationary relationship. This is not the first time that this sort of discrepancy has been identified within historical climatology studies. The authors should refer to Adamson and Nash (2014) or Ashcroft et al. (2016) as entry points into this literature.

Adamson GCD, Nash DJ (2014) Documentary reconstruction of monsoon rainfall variability over western India, 1781-1860. *Climate Dynamics* 42:749-769.

Ashcroft, L., Gergis, J. and Karoly, D.J., 2016. Long-term stationarity of El Niño–Southern Oscillation teleconnections in southeastern Australia. *Climate Dynamics*, 46(9): 2991–3006.

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Interactive comment on *Clim. Past Discuss.*, doi:10.5194/cp-2017-30, 2017.

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