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Interactive comment on "Hydroclimate variability of the South American Monsoon System during the last 1600 yr inferred from speleothem isotope records of the north-eastern Andes foothills in Peru" *by* J. Apaéstegui et al.

Anonymous Referee #2

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This paper presents a new speleothem record from northeastern Peru covering the last 1600 years with high temporal resolution, thereby providing an important new paleoclimate dataset for this data-sparse region. While this is a very welcome contribution to regional to large-scale paleoclimatology, I do not find the accompanying climatic analyses, discussion and conclusion particularly well designed and revealing. The way the manuscript is currently presented, I do not see much novelty that could not have been known based on the already existing records from the region. I therefore think this manuscript could and should be substantially improved by major revisions.

Major Points

1. The paper presents a number of comparisons between paleoclimate records from the region and also reconstructions of large-scale climate indices. The authors often claim to identify close relationships between records, however these are always only based on visual comparison and no statistical analyses or calculations to actually quantify these relations are provide. To demonstrate relationship a minimum of quantitative comparisons like correlations should be used. A starting point could be the cross-wavelet analysis in Fig. S6, which is not even mentioned in the main text. In the current form, the main conclusions are not sufficiently founded in my opinion.

2. In contrast, the main conclusions are mainly based on findings and interpretations from previous work. It is not very clear, where the data and analyses from this work actually contribute to significant new knowledge. This could also be improved by streamlining the text, see point 4 below.

3. Although I am not an expert in cave records and their sampling and dating, the variance structure of the record in the medieval times between ca. AD 900 and 1150 (compared to the rest of the record) appears highly suspicious to me. Can the authors clearly rule out that this is not an artifact of the dating or sampling of the record, or even related to dating resolution? (My lack of knowledge in this field does not allow me to assess this). Generally, how strongly can the spectral interpretations be biased by dating procedures and resolution? Given that the spectral analyses of the record are a very important basis of the conclusions of the study, I think this needs to be more elaborated on to make sure there are no false conclusions.

4. I find the results and discussion section very difficult to read. There is no clear structure and continuity and large fractions are more a literature review than a discussion of the actual results of this study. I think this should be completely re-organized in order to guide the reader through the main points of the results.

5. I have some problems with the very fixed temporal definitions of the MCA and LIA.

Many studies have shown that these periods may not have been globally consistent anomalous periods, with large regional differences in the timing of extremes (if at all existing), see e.g. (PAGES-2k-Consortium, 2013). Importantly, many of the South American records presented here do not fit in the fixed definitions used by the authors (MCA=900-1250; LIA=1400-1850); and the periods highlighted in the Figures do actually not match this definition. In contrast, there are clear differences in the timing of maxima and minima among the records, which should be elaborated on in my opinion. I suggest a bit more careful use of these terms. This could also help avoiding some of the "eyeballing" mentioned in my first point above.

6. While I am obviously not a native English speaking person either, I think the language of the manuscript needs improvement in many places (with clear language and grammar/punctuation errors even in the abstract, e.g. lines 8 and 13). For a better understanding of the text, I strongly suggest correction by a native or very good English speaking person before resubmission.

Other Points, details on major points

1. Abstract, last sentence: I think this statement can only be made if the relation between the proxy records and Atlantic/Pacific variability is assessed in a quantitative way.

2. P. 541, l. 15ff: In my opinion, an agreement in the range of absolute d18O values between proxy records is not a confirmation of a common climate signal. This must first be related to a climatic data set. I know that this is hardly possible due to the lack of data, so at least replace "confirming" by "suggesting".

3. P. 542 l.10: The "striking relationship" is only based on visual comparison. I agree that the low frequency variations of the two records look similar but such a strong statement should be backed by some numbers.

4. P.542, l.24f. I disagree with this statement and "coherence" is again used based on

visual comparison alone. The authors even contradict to this statement further down in the paragraph were they describe the clear differences between the records during the "LIA".

5. I think the argumentation that the SASM was strongly influenced by Atlantic variability (based on time series and spectral comparison) in medieval times is truly interesting and should be more focused on in the discussion. In contrast the Pacific influence is only assessed by (often contradicting) description of existing literature. There is no real conclusion taken from the Pacific records in Figure 4c resulting in vague statements. By strengthening and streamlining the discussion and providing some quantification, the authors may be able to make the statement of strong Atlantic influence during "MCA", which I find the strongest and most important conclusion of the paper, more convincing to the reader. In contrast, the part about the Pacific influence could be significantly shortened, as it does not lead to clear conclusions.

6. P.549, l. 2-3: I disagree with this statement, as the $\sim$65 year cycle is only dominant in the medieval period and (much weaker) towards present.

7. P.549. l. 11ff: I would be careful about the east-west antiphase, because at some places, the authors also highlight the common signal during medieval times, making a stable antiphase relationship on centennial time-scales unlikely.

8. I would find it interesting to know why the Palestina record has a much closer match to the Pumacocha record than to the nearby cave record from Cascayunga. And the fact that the Cascayunga record actually shows a maximum in the early 16th century, when the Palestine record has very low values. If there are any possible interpretations for these (mis)matches, the authors should provide them.

Minor Points

I will not comment on all language errors etc. at this stage and only mention some other minor things that appear important to me.

1. The first sentence in the introduction needs some references in my opinion.

2. P. 536, l. 3: Rabatel et al. (2008) is a study for South America so does not fit into the context of this sentence. Replace by a more appropriate citation from the NH.

3. P. 538, l 21: Please provide a reference for the Southern Amazon basin.

4. P.544, l. 8: The Thompson et al. references are not in the bibliography.

5. Figures 4d and 5d. Please clarify which of the Palestina records the wavelet plot is based on.

6. Figure S3: In my pdf, the plot is empty. . .

References

PAGES-2k-Consortium (2013). Continental-scale temperature variability during the past two millennia. Nature Geoscience 6: 339-346.

Interactive comment on Clim. Past Discuss., 10, 533, 2014.