

# ***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 #1**

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### General comments

The authors present an interesting new speleothem d18O record from Palestina Cave, located in the eastern Andes region of Peru, which spans the last 1600 yrs at sub-decadal resolution. The record is based on two overlapping U-Th dated stalagmites with very robust age models, thus providing a key dataset to investigate decadal to centennial scale hydroclimate variability in western Amazonia. The authors interpret the d18O signal as reflecting the strength of the South American summer monsoon (SASM), which is consistent with previous studies of precipitation isotope systematics

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and proxy records from this region. The record is compared with additional speleothem, lake, and ice core records from nearby locations and with two more remote speleothem records: the Bahia Cave record from Northeastern South America (Novello et al., 2012) and the Cristal Cave record from Southeastern South America (Taylor et al., 2010).

The authors focus primarily on the paleoclimate expression of the Medieval Climate Anomaly (MCA) and the Little Ice Age (LIA) in these three regions. They utilize the MCA/LIA trends, spatial patterns, and the results of spectral and wavelet analysis of these records to assess the mechanisms for decadal to multi-decadal SASM variability. An important result of this work is that it provides further support for a weakened SASM during the MCA and a strengthened SASM during the LIA. Furthermore, their results suggest that the Atlantic Multi-decadal Oscillation (AMO) modulates the SASM, especially during dry periods such as the MCA. These results also show increased decadal scale SASM variability during the LIA, which the authors attribute to the North Atlantic Oscillation (NAO) or the Interdecadal Pacific Oscillation (IPO).

This data presented is robust though their conclusions are not always well supported by the data. For instance, I am not convinced that 8-10 year cycles can be robustly identified in a record with an average resolution of 5 years. This has implications for a substantial portion of the text and conclusions. Please see below for details. Furthermore, it took several careful readings of this work before I could piece together the story, as it is quite poorly organized, needs additional development of the introduction, discussion, and conclusions, and would greatly benefit from editing for English usage and grammar. I strongly suggest adding sub-section headings to break up the manuscript into organized parts, especially in the “Results and discussion” section. Overall, though, I think this is a nice record and would be suitable for publication following major revision to address the following issues.

Specific comments

Abstract: already I received a red flag when reading that the record has ~5 yr time

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resolution and yet they identify a significant 8 year cycle. At best, the nyquist frequency is the highest frequency signal that can be identified. Given that the resolution of the record varies between 2 and 8 years, a conservative choice would be to reject any cycles shorter than 16 years (twice the lowest resolution of the record). Even so, this would be 2 data points per cycle, so personally I would consider any cycles shorter than ~20-30 years suspect.

Page 535, Line 21: I would in fact argue that MCA/LIA climate signals are not “globally” synchronous. In fact, significant spatial and temporal variability in the expression of these events is seen globally and the Southern Hemisphere expression, in particular is still poorly constrained (see recent IPCC AR5 report, for example and references). I suggest adding more references and discussion of the complexity of the LIA/MCA climate patterns and also, perhaps, some discussion of the proposed mechanisms of climate variability during these periods. A key result of this record is that it fills in more details about the expression of the LIA and MCA in South America, so it is worth devoting a bit of time to this in the manuscript.

Page 536, Lines 18-24: This sentence confused me somewhat. Please try to clarify this whole paragraph: describe how the existing records are interpreted and what the spatial patterns suggest thus far.

Page 538, Lines 24-27: The mechanism through which PDO is thought to influence SASM strength needs to be clarified here. Furthermore, there is much debate about whether the PDO is a true climate mode or just the low-frequency expression of ENSO. If the PDO/IPO is to be kept in this manuscript, the PDO and the nature of the link with SASM need to be more clearly described and referenced (see for example, Shakun et al., 2009; GRL).

Page 539, Lines 2-13: The traditional “amount effect” is not likely to explain the isotopic variability of precipitation in this region (see, e.g. Vuille et al, 2012, Clim.Past) In particular, the water isotopes likely reflect an integrated signal of fractionation processes

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and moisture recycling from the moisture source region to the site of precipitation. This needs to be more clearly explained utilizing appropriate references. Finally, the last line of this paragraph is too vague – exactly how significant is winter precipitation?

Page 539-540: Materials and methods: Is there no drip water isotope data or other monitoring data from the cave that can be reported here? Also, I am curious why the authors report the speleothem length as 10 and 17 cm, yet they clearly did not analyze the full length of the samples (or at least do not present the full data here).

Page 541, Lines 3-6: A more robust “Hendy test” is to assess whether d18O is constant and whether d13C and d18O are correlated along a single growth lamina. If this was completed, the authors should report the results here. In either case, the authors could point out the good replication between the two stalagmites as evidence for quasi-equilibrium calcite precipitation.

Page 542, Lines 11- 20: The authors refer to some discrepancies between their record and other records (Pumacocha, I think) which are not shown – if the authors mention this discrepancy and attribute them to age model uncertainties, they should show the data in the Supplementary information. Furthermore, they also distribute some differences between the Cascayunga speleothem record and their record to chronology issues – I do not find this convincing as visual “wobble-matching” within age model uncertainty in no way helps to resolve these discrepancies. Nevertheless, the very striking agreement shown in Figure 3 between their record and the Pumacocha record, especially, is quite convincing that this is a robust record of regional precipitation d18O.

Page 543, Line 12: I suggest that the 10 yr cycle is not significant and all discussion of this should be removed from the manuscript (see earlier comments).

Page 544: From here on out, I find this whole discussion section somewhat confusing and in desperate need of organization (and also deserving of one or more sub-headings). The authors need to clarify the spatial rainfall patterns expected during La Nina, El Nino, and positive/negative AMO/ITCZ migration (either here or earlier in sec-

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tion 1 or 2) and then clearly lay out the evidence for the LIA and the MCA from the three regions and from the time series/wavelet analyses.

Page 547: I suggest the authors focus the paper primarily on the LIA/MCA trends in their record and how these fit into the broader regional contest of the SASM (as they already do), and then focus primarily on the AMO signal which seems the more robust result. As mentioned previously, the shorter frequencies are more suspect given the resolution of the record. As such, I suggest that the paragraphs on this page be removed or substantially shortened.

Technical corrections

There are too many English language issues with the manuscript for me to point out each one, so I suggest having a native English speaker edit the work before re-submitting.

Page 536, line 9: Here and elsewhere, avoid use of the word “nowadays”. This is very informal and a more appropriate statement might be that “LIA rainfall increased by 20% compared to the 20th century (or “compared to modern”)

Page 538, Line 2: Do you mean that 63% of annual rainfall occurs in November AND March or November THROUGH March?

Page 539, Line 26: rather than use the imprecise phrase “lies around”, I suggest you say “averages” or, better yet, “the mean growth rate of PAL3 is 0.049 mm/yr”.

Page 541, Line 4: in parentheses, give the values as “PAL4  $r^2 =$  ” rather than just “PAL4 =”

Figures – In general, the font sizes of the axis labels and titles are too small to read clearly.

Figure 1: It is hard to see the proxy record locations with this color scheme – please try to either change the color of the location symbols and labels or change the color scale

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for precipitation.

Figure 4c: These tropical pacific records are not well explained in the text. Furthermore, if the focus of the paper is kept on the Atlantic, this section of the plot may not be necessary.

Supplementary info, Figure S4: I found myself referring to this figure repeatedly while reading the manuscript. Perhaps it should be included in the main text?

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

**CPD**

10, C112–C117, 2014

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