

### **Comments on Reviewer #1 (R1):**

***“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”.***

We agree with the R1; the time resolution for the Palestina record does indeed not allow for robust interpretations at sub decadal timescales. The manuscript has been changed accordingly from its original version and now discusses SASM multidecadal variability based on  $\delta^{18}\text{O}$  reconstructions in South America only. In addition, spatial precipitation patterns during the MCA and LIA, and their relation with the Atlantic Multidecadal Oscillation are evaluated and discussed along the main manuscript.

***“... 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 r. I strongly suggest adding sub-section headings to break up the manuscript into organized parts, especially in the “Results and discussion” section”.***

We appreciate the suggestion of adding sub-section headings to break up the manuscript into better-organized parts. We think this comment was helpful in order to strengthen the clarity of the story. In the same vein, we also included as sub-sections some results that were not included in the original manuscript. We also asked for editing for English usage and grammar from a US colleague.

***“....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”.***

We agree with the referee comment. As outlined above, we now choose a conservative estimate and refrain from interpreting signals at frequencies below 16 years (which is twice the lowest resolution of the record). The discussion and interpretation of the records and their statistical analyses were modified by instead focusing on the decadal and multidecadal variability.

***“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”.***

Significant spatial and temporal variability related to the MCA/ LIA climate signals is indeed observed in the South American records. Hence we agree completely with this comment and concur that this aspect was not correctly discussed in the initial manuscript. In order to assess the regional significance of our record, a detailed comparison between the Palestina record and others records available was made. Such comparisons allow us to highlight differences in timing and structure of these events over different regions of South America, resulting in new information on SASM behavior, in particular during the MCA. Mechanism involved are discussed at multidecadal timescales and mostly related to the Atlantic Multidecadal Oscillation which appears to have a stronger impact on precipitation in this area and time period than Pacific (multi-)decadal modes.

***“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”.***

The entire paragraph was rewritten in order to clarify our ideas about spatial variability and physical interpretations as documented in the existing records.

***“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)”.***

The aim of our paper is to assess decadal- to multidecadal modes of variability and how they have affected SASM intensity over the past millennium. Given the significant present-day influence of the PDO on South American climate (see Garreaud et al., 2009, for a detailed review), we deem it necessary to initially consider all modes of variability that are known to influence the region, including the PDO. Our results do, however, indeed confirm that the Atlantic Multidecadal Oscillation appears to have more strongly modulated SASM behaviour over the past 1600 years. In this sense, the influence of the PDO is not discussed in great detail in the main text.

***“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 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?”.***

We agree. The whole paragraph has been modified emphasizing the integrated signal of fractionation processes and moisture recycling from the moisture source region to the

site of precipitation. Additionally, a better description of the seasonal distribution and contribution of precipitation to the cave system has been developed to clarify this aspect.

***“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 analyse the full length of the samples (or at least do not present the full data here)”.***

Monitoring isotope  $\delta^{18}\text{O}$  values of different waters (drip, rainfall, river) forming the cave system were added to the manuscript in order to clarify the basis for our interpretation of these records. Indeed, processes involved in the calcite precipitation, which could bias climatic influences of the Palestina speleothems record were ruled out. R1 further asks why we did report the total speleothem length if it was not analysed completely. We agree with the reviewer that this aspect was confusing and now clarify in the manuscript that only 78 mm of the PAL4 record (total length ~17 cm) and 39 mm of the PAL3 record (total length ~10 cm) were analysed. This was a pragmatic choice given limited time and resources as we were particularly interested in developing a high-resolution record for the most recent 1600 years.

***“A more robust “Hendy test” is to assess whether  $\delta^{18}\text{O}$  is constant and whether  $\delta^{13}\text{C}$  and  $\delta^{18}\text{O}$  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”.***

Further tests to rule out kinetic fractionation were not carried out due to the difficulty of extracting samples from exactly the same layer. As pointed out by the reviewer, even though the speleothems have different sampling resolutions and age models, the two oxygen isotope records are nearly identical both in terms of the mean values and the magnitude of change through the overlapping ~900 years interval. This replication in the  $\delta^{18}\text{O}$  records indicates that speleothem calcite is not significantly impacted by kinetic isotope effects since we would expect that such effects would be sample specific (Hendy, 1971). Indeed, the observed similarities provide evidence that a common factor governs the isotopic signal in the two speleothem records, primarily related to water infiltration reaching the cave environment and associated with the precipitation in the SASM.

***“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 Cascazunga 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  $\delta^{18}O$ ".*

Discrepancies between records are shown in the main manuscript. In addition calculated dates for each time series are shown in order to highlight mismatches between records referred to chronological uncertainty. Differences between Palestina and Cascayunga cave records are most apparent during the time interval 1100 - 1420 A.D., a period which is not constrained by any age in the Cascayunga record but for which five calculated ages exist in the Palestina record. Hence we believe that it is worth mentioning this issue in the main manuscript and discuss that some peaks in the Cascayunga record appear to correspond to similar peaks in the much better constrained Palestina record. In addition, between 1520 and 2000 A.D., the Cascayunga age model is constrained by linear interpolation of one date (age) assuming a constant growth rate until the present. This period is constrained by 6 ages in the Palestina record, which adds more confidence to the interpretation and allows defining a better timing of climatic events in this part of the Amazon basin

*"I suggest that the 10 yr cycle is not significant and all discussion of this should be removed from the manuscript (see earlier comments)".*

All discussions involving frequencies below 20 years were removed from the manuscript.

*"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 subheadings). 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 section 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".*

More subheadings and a complete reorganization of the text have been made in order to structure and to simplify our manuscript, hopefully providing greater clarity of our interpretations and the regional reconstructions. High-frequency Pacific Ocean influences such as El Niño and La Niña are no longer discussed, as we focus now solely on decadal and multi-decadal variability with an emphasis on the role of the Atlantic Ocean.

*"I suggest the authors focus the paper primarily on the LIA/MCA trends in their record and how these fit into the broader regional context 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".*

We agree with the reviewer. We now focus our discussion on how the AMO affects rainfall patterns in tropical South America over the last millennium and how the oceanic signal is transmitted to the continental interior through shifts in the position of the ITCZ, resulting in changes in SASM intensity, ultimately recorded in  $\delta^{18}\text{O}$  departures in South American proxies. We have applied statistical tests to highlight differences in the isotopic signal from proxies across tropical South America during positive and negative AMO phases, leading to characteristic spatial footprints of the SASM during each phase of the AMO. As already outlined above, frequencies higher than 20 years are no longer discussed.

**Technical Corrections:**

English language issues have been resolved, as we have included a US colleague in the group of authors.

**Figures:**

We have changed the font size of all the figures and added additional figures to the manuscript.

Figure 4c was removed from the manuscript.

Figure S4 has been included in the main text and is being discussed as suggested by R1, since this is indeed a relevant result of this study.

### **Comments on Reviewer #2 (R2):**

1. We agree with the reviewer that the quantitative aspect of our paper was not as strong as it could have been. This has been improved substantially. However, cross-correlation between records, while desirable, is not easy to achieve in most cases as it requires a thorough understanding of the sampling resolution, chronological uncertainties etc., of each individual record and access to all original data. We have therefore refrained from calculating metrics such as correlation coefficients between most records, as it would introduce additional artefacts and uncertain interpolations for most proxies considered. We prefer to work with the individual original reconstructions as published by the various authors. The exception to this rule is the Bahia record (DV2), as we were provided with both original and annually interpolated data by the first author of that record.

We agree with the comment regarding the cross wavelet analysis in Fig S6. This figure has been moved into the main manuscript and is now discussed there in detail.

2. We have added several new analyses to the manuscript, including an analysis of the influence of the AMO on isotopic proxies over the past millennium, as well as a spatial analysis of how differences in  $\delta^{18}\text{O}$  vary across the continent between MCA and LIA. Other results discussed include the relation between two remote regions in South America, such as the Bahia record in north – eastern Brazil and the Palestina record in the upper Amazon Basin in the eastern Andes, which suggests that precipitation in both regions are to some extent related to the same mechanism during the MCA. Finally we believe that there is great value in replication of climate signals with our new record, as it confirms the spatial representativeness of previous analyses and also shows that features such as the MCA double peak are real climate events and not related to proxy-specific noise in an individual record.
3. The isotopic data presented for the period between 900 and 1150 A.D. was generated by linear interpolation of three U/Th dates in the PAL4 speleothem and corroborated by one more date in the PAL3 speleothem. We believe this is a strong argument to confirm this is not an artefact of dating or sample resolution as the large quantity of analyses adds confidence to the reality of these variations in  $\delta^{18}\text{O}$  values during this period. On the other hand, larger isotope variations during this time interval, when compared to the whole record could be suspicious, as suggested by R2. During the MCA, however, variations in the same sense and structure are observed and shared with other records in the northern region of South America. These shared features across proxies, in our opinion, provide strong evidence for regional changes in climate driven by the same large-scale mechanism during this time interval.

4. We agree with the reviewer. Additional sub-headings have been introduced and the text was completely reorganized. We believe that the main message of the paper is now much clearer and that the restructuring helps guide the reader to the main points of the manuscript.
5. This is a valid point indeed. In tropical South America, the definition of the MCA and LIA events is quite variable between studies and proxies considered, and discrepancies are observed in relation to the intensity and temporal definition of these events (e.g. Rabatel et al., 2008; Vuille et al., 2012). Indeed, this variability does not seem to be related simply to chronological errors of individual records but may also show a latitudinal change of the climatic perturbation and its isotopic footprint in and out of this region along the Andes. Here we have defined both periods in relation to the Palestina record as this is our main research focus. This is consistent with the approach taken in previous studies where these periods were also defined based on the respective records analysed. But we are well aware that onset and duration of MCA and LIA can vary considerable depending on location and variable considered. Nonetheless the median of reconstructions document mostly warm conditions from about 950 to about 1250 A.D. (MCA) and colder conditions from about 1450 – 1850 A.D. (LIA) (IPCC, 2014). Here we defined the MCA as the period 900 - 1100 A.D. and the LIA as the time during 1350 – 1820 A.D., which is almost inside the median conditions reported. However, we discuss times of maxima and minima, discrepancies and similarities between Palestina and other Andean and non-Andean records in the manuscript.
6. A US colleague revised the main manuscript before re-submission.

#### **Other Points, details on major points**

1. We agree with this statement. We have employed additional statistical analyses in order to be more quantitative rather than qualitative in relation to forced SASM variability and how it is affected by the AMO on multidecadal timescales.
2. We have changed the sentence.
3. We agree with the comment by R2, but as outlined in the first major point above such quantitative comparison using cross-correlation are not really feasible. Still, we have added additional analyses to better document relationships between proxies in a more quantitative way.
4. We agree. We have re-organized the discussion focusing on the main results. We also made sure not to contradict our own arguments. We believe that our discussion of the mechanisms governing the spatial footprint of the SASM over major key periods is now much more stringent and consistent and follows a physically plausible scenario.

5. Again we agree with this comment. The main manuscript is now focusing almost entirely on the Atlantic influence at multidecadal timescales. The discussion of the Pacific influence is significantly shortened and included only to clarify the main results of this work.
6. We have changed this sentence in order to clarify that the 65 years frequency observed in the Palestina  $\delta^{18}\text{O}$  record, related to the AMO, is most dominant during the MCA.
7. The east – west comparison highlights the common signal during medieval times and opposite conditions during LIA, which is evidenced by the  $\delta^{18}\text{O}$  records in each region. This switch from in-phase to anti-phase behavior suggests that this mechanism is indeed not stable on centennial time scales as suggested by R2. We ascribe this flip to a strong dependency of this mechanism to changing boundary conditions such as Pacific and Atlantic SST. Clearly this aspect requires more study to disentangle the possible mechanisms, which could explain the partial decoupling of signals in the SASM region.
8. We believe that the closer agreement in  $\delta^{18}\text{O}$  time series with the Pumacocha rather than the Cascayunga record is based on the strong and regionally coherent signal of the isotopes in the Andean region. It is important to keep in mind that the Cascayunga record does not present nearly as strong a chronological control as the Palestina and Pumacocha records. This aspect is elaborated on in more detail in the main text along with suggestions for better constraining the isotope record of this cave.

### **Minor points**

1. Appropriate references have been added to the first sentence.
2. The citation has been changed to Mann et al., (2002): Mann, M. E.: Little Ice Age, Encyclopedia of Global Environmental Change (ISBN 0-471-97796-9), V1, pp 504 – 509, 2002.
3. We have added the following references concerning the southern Amazon Basin: “Warm SSTA in the tropical North Atlantic region have been associated with drought conditions over the southern (e.g. Yoon and Zeng, 2010; Ronchail et al., 2002; Marengo et al., 2008), and western Amazon Basin (Espinoza et al., 2011; Lavado et al., 2012) “.
4. The Thompson reference is now in the manuscript: Thompson, L. G., Mosley-Thompson, E., Davis, M. E., Zagorodnov, V. S., Howat, I. M., Mikhalev, V. N. and Lin, P.-N.: Annually resolved ice core records of tropical climate variability over the past ~1800 years., Science, 340(6135), 945–50, doi:10.1126/science.1234210, 2013.
5. The wavelet plot is based on the composite record of the PAL3 and PAL4 speleothems, now called Palestina record.



## Comments on Marie Pierre Ledru “SC”.

1. We appreciate this comment, but the aim of this work was to evaluate the isotopic signal and the potential of this proxy for reconstructing regional patterns of SASM variability in Andean and non-Andean regions in relation to some oceanic modes at multidecadal timescales. Hence this paper focuses on  $\delta^{18}\text{O}$ , which is a common high-resolution proxy used to reconstruct SASM variability in lake cores, ice cores and speleothems. The other records suggested by M.P. Ledru (pollen, vegetation, dendroecology) are not easily comparable to  $\delta^{18}\text{O}$ , as they do not integrate moisture pathways and rainout mechanisms upstream in the same way as isotopic proxies do.
2. There is indeed significant spatial and temporal variability related to onset and duration of the MCA and LIA observed in the South American proxy records. We therefore performed a detailed comparison between the Palestina record and other  $\delta^{18}\text{O}$  records available. These comparisons allow us to highlight differences in timing and structure of these events over different regions of South America, and to discuss this new information of SASM behavior especially for the MCA.
3. We are really sorry about this but when the first manuscript was submitted; we had no knowledge of the study published by Ledru et al., 2013. We appreciate being pointed toward this reference. It has helped us improve our discussion in the revised manuscript.

*“....a weak SASM activity and the important result of a strong Atlantic influence during the MCA” and “a stronger SASM activity during the LIA”, although this similarity is never mentioned*”. We actually referred to these conclusions in the light of previous studies based on  $\delta^{18}\text{O}$  records, such as Bird et al. (2011a) and Vuille et al. (2012), which are referred to in the main manuscript.

*“Indeed, apart from the MCA and the LIA, two major events were pointed out in Ledru et al: a dry episod between 1250 and 1550 AD and, during the last century”*. This comparison is now being discussed in the revised manuscript. These events are probably not related to changes in SASM activity, since the equatorial paramo region displays a damped seasonality and the SASM signal could be biased by equatorial rainfall during the other seasons.

On the other hand, we provide a higher resolution record located to the south, which reveals that seasonality is an important factor to explain SASM variability. Our interpretations are supported by statistical analyses which bring new insights related to the footprints of the AMO in SASM precipitation.