The revised manuscript by Chenxi Xu et al. has been improved relative to the initial submission. However, the manuscript still contains conclusions not supported by the analyses. In general, we suggest the authors focus on presenting only the analyses that persuasively demonstrate their proposed mechanisms of variability. If this cannot be done, we suggest they soften the conclusions that cannot be robustly substantiated. The following review contains three major concerns should be addressed before publication, as well as a short list of smaller issues. Based on the work required to make the suggested changes, we recommend an additional round of major revisions.

Major Concerns

(1) The authors' treatment of uncertainty has improved from the initial version (e.g., the inclusion of 95% CIs and the age measurement uncertainties in Fig. 11), but the majority of the conclusions in this paper rest upon signals that have not been demonstrated to be more than noise. Specific examples are below:

  a. What is the uncertainty on the difference presented in Figure 10b? These records seem to have been generated by subtracting one proxy from another without propagating the error. Therefore, it's impossible to determine if there's any trend in these data, or if one reconstruction is different from another. The authors need to demonstrate this conclusively in order to validate their mechanism.
  b. We find the relationship in Figure 11 difficult to interpret due to substantial uncertainties in the age model of the stalagmite oxygen record. Time errors in the speleothem record are presented, and are on the same order as the timescale of the signal of interest (10-30 year uncertainty is ~10% of the length of the record, and on the same order of magnitude as the timescale of interest of the analysis). Thus, we expect the analysis comparing multidecadal signals in the tree ring stack to those in the speleothem record to be very sensitive to the uncertainty in the speleothem record timescales. For this analysis to be convincing, the authors need to address the relationship between signal and error.
  c. Could the authors explain in more detail why error estimates are not available for the Hulma record? It is not clear why they are not available simply because these records were generated by a pooling method.

(2) We are not yet convinced that these 5 tree ring records should be stacked. Presumably we'd want to stack these records to reduce local noise associated with a coherent regional signal. For the following two reasons, we are not convinced that the 5 sites experience a coherent regional climatic signal. First, the authors' response to our initial review indicated that low correlation between two sites was expected because they are far apart – this would seem to undercut the argument for placing them into the same stack, as the climatic drivers operating on these two sites are likely to be different. Second, Fig. 6 shows that only 3 of the 5 tree ring sites fall in the region where the H5 d18O variation has a significant correlation with precipitation amount variation suggesting that there's not a coherent, single regional signal across this region. We are

concerned that by stacking these 5 sites (as opposed to possibly the three or four most westerly sites), the authors may be averaging signals from two separate hydroclimatic regions. We strongly suggest that the motivation for stacking be clarified and strengthened.

(3) The spectral analysis method description and presentation has been substantially improved. The revised power spectrum exhibits a clear, significant signal at periods of ~4 and ~5 years. However, we are not convinced of that the centennial-scale peak, which is reported as corresponding to a ~133-year cycle, is a signal of a centennial-scale cycle as opposed to a secular trend. Part of our concern derives from the mismatch between the timescale of the cycle and the location of the signal peak in Fig. 7. The peak of the ~133-year cycle should be between 0.01 and 0.005 cycles/year. Instead the peak of the signal occurs at a value below 0.005 cycles/year. Because we cannot be confident that centennial scale variability is preserved in H5, the discussion of centennial variability as a preservation of the ISM signal is poorly substantiated. Therefore, we recommend that the authors refocus their paper to interpreting their record with respect to ENSO primarily. We feel this suggestions is robust for the following reasons:
   a. There is significant, robust spectral power at ~4 and ~5 years, which is consistent with ENSO timescales.
   b. Spatial patterns shown of the correlation between SST and d18O looks like ENSO variability, with the strongest impact in the eastern and central tropical Pacific (Fig. 8).

<u>Minor Comments</u>

(1) Links for Ganesh, JG, and Manali datasets to the NOAA Paleoclimatology Archive do not work. Please update so we can replicate analysis.
(2) Organization could still use improvement. For example, many methods are described in the results and discussion section. We need these in the methods section so they can be fully evaluated. For example:
   a. Pooled method (there must be a way to assess certainty!)
   b. Bandpass filter for decadal and multidecadal trends
(3) Figure 10: add legend for different colored shaded area.
(4) Figure 7: label y axis “**Log** power” and x axis with units (cycle/year)

This comment was prepared following a SPATIAL laboratory group discussion. Rich Fiorella, Annie Putman, and Chao Ma compiled this short comment with additional input from Gabe Bowen, Zhongyin Cai, and Yusuf Jameel.