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Climate of the Past Discussions

# *Interactive comment on* "Decreasing Indian summer monsoon in northern Indian sub-continent during the last 180 years: evidence from five tree cellulose oxygen isotope chronologies" *by* Chenxi Xu et al.

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This comment was prepared through a group discussion of the SPATIAL laboratory at the University of Utah.

Overview: Xu et al. provide a new stacked record of tree ring cellulose oxygen isotopes from five locations along the southern Himalaya, spanning a time range of 1743-2008 CE. They find significant correlations with regional climate indices of precipitation and Indian monsoon strength over the instrumental record, and from this, infer that their stacked record can be used to reconstruct the strength of the Indian monsoon prior to



the industrial record. From this, they draw two potentially exciting conclusions from their analysis: (a) high-frequency ENSO variability (e.g., periods of 2.4-5 years in their figure 7) may be recorded in the stacked tree ring dataset and (b) low-frequency centennial scale variability (e.g., periods of 160-350 years in their figure 7) may reflect long-term variability in monsoon strength, which they support by an analysis of long-term changes in the land-sea temperature contrast derived from proxy records. However, the authors do not provide information on uncertainty and error, and therefore, it is difficult to assess the robustness of their conclusions. Our view is that this data merits publication, but that considerable revisions need to be made to help clarify their analyses and support their conclusions. Therefore, we recommend acceptance pending major revisions described below.

Major comments: (1) Error and uncertainty are not adequately explored or explained. We provide several examples of analyses in the paper that would benefit from a more thorough treatment of error and uncertainty propagation:

No uncertainty is given on the individual chronologies provided (e.g., the uncertainty from combining individual trees at a location to estimate the average delta18O record at that location), nor is it propagated to the averaged delta18O chronology of the stacked record in figure 4.

The authors make several comparisons between their stacked tree ring record and other proxy indicators of ENSO (Fig. 9), stalagmite oxygen isotopes (Fig 10), and Indian Ocean SSTs and Tibetan Plateau temperatures (Fig 11). However, they do not consider either the potential errors in proxy reconstructed values (e.g., the error in reconstructed SSTs), nor potential errors in the age model used to assign a date to those proxy values. As a result, it is difficult to assess how robust the signals they derive from comparisons between proxy records are, and how they compare to the variability. For example, in figure 11, it is not clear that the reconstructed land-sea temperature anomaly is a substantial, robust, or significant deviation from zero if estimates of uncertainty are absent.

(2) The rationale for why the authors think that their stacked record reflects regional changes in the monsoon is absent – the signals from each location appear coherent, but it is not shown that they are actually coherent. There's a wide range in correlation coefficients between sites in Table 2, where the lowest correlation coefficients suggest that sampling at Manali only explains ∼5% of the variance observed at Bhutan. Thus, while we find the possibility that these sites record a regional-scale signal to be exciting, the rationale for combining all of these datasets for a regional interpretation should be explained in more detail.

Additionally, the authors hint that the relationship between sites may not be stationary (pg 5, L18-21), as they note decadal-scale changes are often not observed coherently through their stacked record. The analysis would benefit from exploring potential reasons for why this might be the case – are there other potential explanations than variations in the ISM?

(3) The authors draw conclusions that may not be supported by their time series analysis methods. A section describing the spectral analysis methods, software, etc. that were used should be added to the methods section so that their results could be replicated by other researchers. Additionally, it is not clear how the authors determined the significance levels plotted in Figure 7 – this should be explained. A few additional comments/questions regarding the time series analysis are provided below:

The conclusion that their record captures centennial-scale variability requires more justification considering their record is only 350 years long. They claim significant spectral power at 160 and 350 year intervals (Fig 7) – though the 350 year peak is the secular trend in their 350 year dataset, and the 160 year cycle may also not be significant – more details about how significance levels are determined should be provided.

It was not clear why a 31-year moving correlation was used in figure 9 – could you expand on this choice?

(4) Writing is imprecise and organization needs improvement. We have provided a few



of the more pressing examples to help guide revisions:

- The methods section requires substantial additions. First, the time series analysis methods used need to be described in an additional subsection, and in enough detail that other researchers could recreate the analysis. Second, many of the paragraphs in the results start with a description of how an analysis was done – these should be moved to the methods section.

- The introduction brings up several relevant factors about the ISM without relating them directly to this study. This section would be more impactful if it were better focused on what is known about how these factors influence the ISM rather than just providing a list. This addition would help clarify how your results improve our understanding of the ISM.

- The discussion section analyzing why there may be a weakening monsoonal circulation over last few hundred years requires a more in-depth analysis. The presented tree ring records cannot answer this question, and the question diverges from the main focus of the paper. The land-sea contrast mechanism described is potentially interesting, but the authors need to be more descriptive about: (a) how well do we know there has been a change in the land-sea temperature contrast? (b) are there other potential explanations, given the long list of factors influencing the ISM the authors list in the introduction?

Following the SPATIAL laboratory group discussion, Rich Fiorella compiled this short comment based on input from Gabe Bowen, Rose Smith, Annie Putman, Crystal Tulley-Cordova, Chao Ma, Zhongyin Cai, Yusuf Jameel, Brenden Fischer-Femal, and Sagarika Banerjee.

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