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Interactive comment on "On the low frequency component of the ENSO-Indian Monsoon relationship; a paired proxy perspective" *by* M. Berkelhammer et al.

Anonymous Referee #1

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Berkelhammer et al. analyze an ENSO reconstruction (Li et al.) and a speleothem d18O record (Sinha et al., 2011), a proxy for the Indian Summer monsoon (ISM), to investigate both high and low frequency relationships between the ISM and ENSO. They find a coherent relationship in the high frequency domain wherein El Nino events are associated with reduced ISM rainfall (a relationship observed in the instrumental data) and a coherent relationship in the low frequency domain wherein persistent El Nino-like conditions are associated with enhanced ISM rainfall. I appreciate that the authors fully sampled the uncertainty in their data (including time uncertainty) when investigating the coherence and phase, and find the results of the study very interesting.

My primary critique of the manuscript as it stands now is their interpretations of what

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climatic factors might result in the multidecadal relationship they observe, which impress me as overly speculative. The authors begin their interpretation of this variability (on page 3113) by stating that because the relationship is the opposite of that expected for the interannual band, "the shared power cannot be through a direct causal relationship". I'm not sure I follow why this must be case; couldn't there be, for example, low frequency changes in Pacific climate (e.g., the PDO) that interact with the ISM in a different way from that typical of the interannual band? Furthermore, the authors suggest that North Atlantic variability may play a role, as the ENSO reconstruction they use is derived from the NADA. However, I don't find this argument particularly convincing or justifiable. The mechanism they propose – an atmospheric bridge that results in both enhanced ENSO and enhanced ISM in response to cool North Atlantic conditions – seems very speculative to me and in terms of the ISM, is in disagreement with the paleoclimate record, which shows, during the last glacial period as well as potentially the Holocene (e.g., Gupta et al., 2003, Nature) that North Atlantic coolings are associated with a reduction in ISM intensity.

In addition, the authors reject any influence of the Indian Ocean on multidecadal power on the ISM, because their ENSO proxy is ultimately from North America. I'm not sure I follow this logic; as they mention, the Indian Ocean exerts considerable control on the ISM, and once again, it is very possible that low-frequency variability in the IO also affects at least the western Pacific region, and therefore potentially ENSO. Ultimately, I do not think that there is justification for such an outright statement as "we reject this mechanism" (re: multidecadal IO influence).

In sum, I urge the authors to revisit their dynamical interpretations of the multidecadal power and also refrain from making definitive statements; ultimately, their study is based on the analysis on one reconstruction and only one proxy of the ISM, which is not enough data to conclusively state what is driving the low frequency relationship they observe. There is no rule dictating that low frequency climatology must act in the same manner as interannual climatology, therefore I think that there are probably sev-

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eral possibilities that could explain the relationship. Again, the first thing that comes to mind is the PDO, which I urge the authors to investigate.

I have a couple other comments as well:

1) I recognize that the Cobb coral data are not continuous, and that would preclude a low-frequency analysis, but it would be useful if the authors could perform their high-frequency coherence and phase analysis with these data vs. the ISM proxy. This would provide a useful comparison to the NADA-based analysis.

2) How were the proxy ENSO and speleothem data filtered? There is no description of the type of filter applied, which I assume is a bandpass filter (?) In Figure 3, the speleothem data is shown with a running average filter, but I assume that this was the not the method used to isolate the low frequency component for the correlation and phase analysis, as moving averages impart a doppler effect on the data. I recommend showing the data in Figure 3 with lowpass filters applied to both, or with the bandpass filters used in the analysis.

Interactive comment on Clim. Past Discuss., 9, 3103, 2013.