Clim. Past Discuss., 10, C328–C331, 2014
www.clim-past-discuss.net/10/C328/2014/

Climate of the Past Discussions

Interactive comment on "Nonlinear regime shifts in Holocene Asian monsoon variability: potential impacts on cultural change and migratory patterns" *by* J. F. Donges et al.

Anonymous Referee #2

Received and published: 29 April 2014

Donges and collaborators describe the use of recurrence networks to analyse Holocene oxygen isotope time series from ten caves in order to detect periods when the Asian Monsoon experienced significant changes. The authors combine the use of this relatively new way of analysing paleoclimate data with a consideration of the effect of chronological uncertainties on the robustness of the results. They identify several major climate shifts in the Asian Monsoon domain, which roughly coincide with the millennial Bond events and rapid climate change events widely documented elsewhere. Furthermore, they claim to identify a previously unreported period of significant monsoon regularity at ∼7.3 ka. Finally, solar forcing is suggested as a major forcing for these regime shifts and links between climate variability and societal change in Asia

are made.

Understanding the past dynamics of the Asian Monsoon is clearly a very important and current topic and one which will be of interest to Climate of the Past readers. The methods used by the authors are appropriate, but many of the statements in the paper are insufficiently supported by the analysis or figures presented. Their main findings (regime shifts in the monsoon, links with solar variability, Bond events and societal changes) have been widely documented before. The effects of dating errors on trend analyses have also been previously considered (e.g. Mudelsee et al., 2012, Climate of the Past, doi:10.5194/cp-8-1637-2012), although not to the same extent as here. The combination between recurrence network analysis and COPRA age modelling highlighted in this study is new and could warrant consideration for publication in this journal provided the authors can convincingly address the issues identified below.

1. Most, if not all, time series analysis methods can introduce/identify random events/periods as being significant. It is therefore often useful to combine two or more time series methods to increase the confidence that the periods/events identified are not an artefact of one method or another. Can you make a comparison of your RN results with other ways of identifying regime shifts, such as those published by Sergei Rodionov (GRL 2004, doi:10.1029/2004GL019448) or Manfred Mudelsee (Computer & Geosciences 2000, doi:10.1016/S0098-3004(99)00141-7)?

2. In the caption of Fig. 9 you specify that periods of significant climate change are marked with dark blue (9B) and dark green (9C). However, in sections 4.3.1 and 4.3.2 you identify periods with "unusual" L and T values which don't seem to agree with the dark blue or dark green areas in Fig. 9. For instance, you mention (in section 4.3.1) 8.5-7.9 ka as being a significant epoch identified in your analysis, but the only dark blue area around this age in Fig 9B is at ∼8.5-8.4 ka. What is the relationship between the significant areas identified in Fig. 9 and the significant epochs mentioned in 4.3.1 and 4.3.2? A similar question can be asked for Fig. 10 and the discussion related to it (4.4).

3. In your analysis of the spatial extent of regime shifts (section 4.3.1) at 8.5-7.9 and 5.7-5 ka, Liang Luar has low L values whereas most other records have high L values. Qunf cave has high values at ∼8-7.9 ka, but low values at ∼5.7 ka. However, you suggest that AISM and EASM branches at these times are affected by the same processes. Can you speculate on a mechanism which would explain this this type of response? You also say that " Maxima of L between 5.7 and 5.0 ka BP are found in all records with the exception of Heshang. . .", a statement which is not strictly correct because over this interval the Liang Luar and Qunf values are close to minimum rather than maximum, and Hoti Cave is not represented.

4. At the end of section 4.3.1 a clear link is made between the solar variability as expressed by Steinhilber et al. (2012) and the RN analysis results. You mention that most of the unusually high L values coincide with or are temporally close to strong negative solar anomalies. Steinhilber et al. reconstruction is characterised by relatively high frequency changes of TSI and therefore it would be very useful to specify what is your TSI threshold below which you would identify a "strong negative anomaly of solar irradiation" and how temporally close your transitions need to be to these solar anomalies in order to consider them linked. Otherwise, most points along the time series continuum can be considered as being "temporally close" to one of the "strong" negative solar anomalies. Furthermore, the solar-monsoon link suggested in this section seems to be in contradiction with this statement from section 4.3: "However, comparison with a recent reconstruction of Holocene total solar irradiance (TSI) (Steinhilber et al., 2012) fails to provide convincing evidence for such cycles in the TSI data (Fig. 9D)." Similarly, in the Abstract and Conclusions you clearly link the Bond events with your regime shifts, but in section 4.3.1 you say "We note that only in one case (B2), these periods coincide with the timing of high-latitude Bond events. . .". Figures 9 and 10 also don't make a convincing case of a link between Bond events and your RN analysis. Please note that I'm not disputing the link between Bond events or solar variability and the Asian monsoon as documented in other studies, but rather argue that your findings as presented do not convincingly support these teleconnections.

5. The link between the RN analysis and the discussion related to the effects on human societies is rather convoluted and the length of section 5 should be greatly reduced and focused only on what this study brings new to the debate. It would be useful to include a figure in which known periods of societal change are plotted together with the results of the RN analysis and focus the discussion around this figure. The Bond events (section 3.1) and RCC episodes (3.2) have also been discussed extensively in numerous publications and these sections should be significantly shortened.

6. In the Conclusions section (and the abstract) I'm unclear on what basis the authors defined the periods of significant regime shifts. For instance the 8.5-8.0 ka period specified in these sections is defined as 8.5-7.9 in section 4.3.1, 8.5 and 8.0-7.9 in section 4.3.2, and is not identified at all in section 4.4. A similar case can be made for the other periods presented in Conclusions and Abstract.

7. The last paragraph in the Conclusions seems unnecessary. More and higher resolution data are always needed for a deeper understanding of the issues investigated as is the integration of several different proxies to provide a more complete picture.

8. The divisions of the horizontal scales for the figures need to be the same across the manuscript. In some figures the tick marks are every 0.5 ka, while in others every 1 ka. To make the figures more readable I suggest labelling ticks every 1 ka, with secondary tick marks every 0.5 ka.

9. Figures 9 and 10: mark periods with unusual L and T values on the Steinhilber plot.

10. Unless a clearer and more robust link with societal change is made, the words "Potential impacts on cultural change and migratory patterns" should be removed from the title.

Interactive comment on Clim. Past Discuss., 10, 895, 2014.