Dear Editors,

We thank both anonymous reviewers for their comments and constructive criticism. We provide below our answers to all comments of the referees. For convenience, the referee comments are given in *italics*. Please contact me if you need any further information.

Best regards,
Jonathan Donges (on behalf of all co-authors)

Referee 1 (Clim. Past Discuss., 10, C232–C234, 2014)

> *The manuscript by Donges et al. attempts to detect episodes with pronounced changes in Asian monsoon dynamics during the Holocene by using a recurrence network analysis, a newly developed technique for time series analysis, on the Holocene speleothem records in Asian monsoon regions. The authors identified several epochs with abrupt regime shifts in Asian monsoon variability (8.5-8.0, 7.3 5.7-5.4, 4.1-3.6 and 2.8-2.2 ka BP), which align with the high-latitude Bond events and other episodes of Holocene rapid climate change. The co-occurence of these epochs with pronounced minima and strong variability in solar activity confirmed the previously proposed solar forcing of these abrupt changes in Asian monsoon climate. The authors also discuss the linkage of changes in monsoon climate variability and major culture changes. As a whole, the paper is good and interesting, although the linkages of abrupt shifts in Asian monsoon with the Bond events and RCC periods, and the relationship between culture changes and monsoon climate variations, have been already discussed by other authors. I believe, however, that it could be gain in quality and clarity, if the following comments and suggestions are considered.*

We thank the referee for this positive judgement. We would like to stress that the central and novel contribution of the paper is a nonlinear time series analysis of Asian monsoon paleoclimate records explicitly considering dating uncertainties that integrates information from several records that have not been statistically considered in conjunction before. The second major contribution of our paper is a thorough review of previously suggested linkages between climatic variations in the Asian monsoon and cultural changes in the region. This review sets the basis for discussing our results in light of earlier work.

> *Usually, nonlinear means that one variable is not directly proportional to another variable. So I was wondering what's the input variable (forcing) while we view the monsoon climate as the output variable (response). What's the nonlinear regime shifts in monsoon variability? Correspondingly, what's the linear regime shifts in monsoon variability? How to define and distinguish them? My understanding is that the nonlinear regime shift here means the abrupt changes in monsoon climate, i.e., from one state switch to another state within a relatively short time period. So I suggest the authors change the title as " Abrupt shifts in Holocene . . .."*

In the title and throughout the paper, we use the term "nonlinear" to indicate properties of a time series describing a process such as the Asian monsoon intensity that can be described and quantified by methods of nonlinear time series analysis such as the recurrence network analysis applied in the paper. Changes in these properties, e.g., the recurrence network transitivity T describing the dynamical regularity of a time series (segment), indicate a nonlinear change or bifurcation occurring in the process underlying the time series. For example, a change from a regular state (e.g., steady or periodic variations, high T) to an irregular one (e.g., chaotic or stochastic variations, low T) is described as a nonlinear regime shift, while a change in linear time series characteristics such as mean or standard deviation would be described as a linear shift (Donner et al. 2010, Zou et al. 2010). This is the sense in which we use the term "Nonlinear regime shifts ..." in the title. We would like to keep this wording in the title and the manuscript, because we believe that it is more precise than "Abrupt shifts ..." which may also refer to sudden linear shifts. We clarify our use of the term "Nonlinear regime shift" in the revised version of the paper.

> *Page 909 line 16."High T values indicate epochs with regularly varying climate . . .. or time intervals with stationary or slowly changing climate, while low T values imply epochs with more erratic (i.e. less predictable) climate fluctuations. In contrast, large L values highlight time intervals including rapid shifts between different climatic regimes,*

while low L values point to a more stationary climate during the corresponding epoch." It seems to me that the climate variability in the epoch with high T values may similar to the epoch with low L values. In figure 6, it is apparent that both T and L are large during the period of 7.0-8.0 ka BP for COPRA sequences and both T and L are large during the period of 3.8-4.4 ka BP for raw and COPRA series. There are also some periods with the similar pattern in figure 7 and 8 for other record. So, how to interpret these contradictions.

Please note that conceptually, *T* and *L* are independent quantities. They may be correlated for some segments of a time series, but they do not need to be. The reason is the following: *T* measures the regularity of a time series (segment) as is described on page 909, line 16. In contrast, *L* can be interpreted to measure whether there are changes between different dynamic regimes within the considered time series segment. Large deviations of *L* from the baseline state (light blue horizontal bars in Fig. 7) indicate the presence of regime shifts within the considered segment. Values of *L* that are consistent with the baseline state point to a more stationary climate in the sense that there are no pronounced regime shifts within the considered segment. Hence, large values of *T* and *L* may well coincide for certain time series segments, as correctly observed by the referee.

For example, consider the time period 7.0-8.0 ka BP for the Dongge DA record (Fig. 6), where both *T* and *L* are large and significantly outside the baseline state for the COPRA ensemble, as was already pointed out by the referee. In this case, this coincidence can be interpreted as follows: Large *T* indicates that monsoonal variability was predominantly regular during this epoch (e.g., note the smaller noise level in the time series as compared to the data for the younger Holocene). In turn, large *L* indicates the presence of, albeit regularly occurring, regime shifts, e.g., between climatic states of different amplitude levels as they are evident in the time series data between 7.0 and 8.0 ka BP. In other words, the co-occurrence of large T and L is evidence against the possibility of an irregular switching back and forth between different climatic regimes.

A corresponding more detailed explanation will be given in the revised paper in Section 2.3.2.

The authors use the Figure 11 to show the relationship of regional monsoonal regime shifts relative to cultural change and migratory events in Arabia, India, South-East Asia and China during the Holocene. However, I can's see there are close linkages between the abrupt establishments or terminations of human cultures and climate regime shifts indicated by the RN analysis. The authors may need to indicate clearly how these two things were connected.

We thank the referee for pointing this out and agree that the discussion of Fig. 11 in Section 5 needs to be more focussed and structured. The text has been revised accordingly.

In chapter 5 "Effects on human societies", the authors obviously have a comprehensive understanding of the culture changes and migrations, but I don't think the manuscript in its present form makes the best use of their considerable expertise. Instead connecting all the changes in culture with climate events (e.g. page 926 line 21-28), they need to concentrate on the epochs identify with RN analysis, as many papers have discussed the influence of climate change on the human societies.

As stated above, the text in Section 5 is being restructured in the revised version of the paper to emphasize the two main contributions of this section: (i) An extensive review of the existing literature on potential cultural impacts of monsoonal changes in Asia during the Holocene and (ii) a discussion of the novel results obtained in this paper in the light of this review.

The authors state that they observe a epoch with significant variation in monsoon variability around 7.3 ka BP, which has been rarely reported so far. Actually, in the paper by Wang et al., (2005, Science), this event has been reported and highlighted in figure 1.

We thank the referee for pointing us to this paper. We include and refer to the findings by Wang et al. in our revised manuscript and change the abstract accordingly.

other minor changes
page 903 line 14, 'China' should be 'East China'
page 937 line 14, During the period of 3.0-2.7 ka BP, the Tarim Basin should be dominated by the westerlies, but not monsoon climate. So it's unsuitable to discuss the

culture change in this region with monsoon climate variations. Also in line 13, 'the Tarim basin in western China/Tibet', the Tibet should be deleted.

We also included these minor comments in the revised paper.

Referee 2 (Clim. Past Discuss., 10, C328–C331, 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.

We thank the referee for this evaluation and point to our response to the first comment of referee 1 above, where we restate the main novel contributions of our paper.

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)?

In our paper, we use significance testing on several levels to increase the confidence in the results and minimize the probability of discussing random events/periods. On the level of single time series/paleoclimate records, we use a statistical null model to identify events/periods of unusual climatic change (see, e.g., the results given in Figs. 7,8). In the next step, this information coming from the 10 records under study is combined and, using another null model, only those events/periods that are very unlikely to co-occur by chance are kept and discussed as significant episodes of monsoonal change on a continental scale (see Fig. 9 for results). Additionally, the effects of dating uncertainties are explicitly considered throughout the analysis pipeline using a Monte Carlo approach (Fig. 10 for results).

We agree with the referee that comparison to regime shifts detected by alternative methods is desirable. However, such a detailed comparison is out of the scope of this paper and might even be misleading, because the two suggested papers propose methods for detecting linear regime shifts in the mean, while we focus on identifying nonlinear regime shifts (see also our response to referee 1 on this matter). We would like to add that some of the authors are currently working on a review paper on comparing different methods of nonlinear time series analysis in the context of applications to paleoclimatic data that will be submitted soon to Quaternary Science Reviews.

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).*

In fact, Fig. 9 summarizes results obtained from all 10 records using the methodology described in Section 4.2. In the discussion in Sections 4.3.1 and 4.3.2 we refer to these results, but also to certain local signatures of nonlinear regime shifts based on the analysis of single time series that are presented in Figs. 7 and 8. The same holds for the discussion of results obtained for COPRA ensembles, where Fig. 10 summarizes results obtained from all 10 records, while the figures in the supplementary information (Figs. S2, S3) show results obtained for individual records. We revised the text in the respective sections to clarify where we refer to continental-scale and regional/local events, respectively.

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?

The referee is right in pointing out that our statement on maxima in *L* is misleading. In the revised paper, we refer to “large deviations” from the baseline state indicated by the null model (base line: light blue bars in Fig. 7) instead of “maxima”, because this is in fact the relevant feature indicating the presence of regime shifts in the considered time series segments (see above).

However, the pattern observed by the reviewer (i.e. the different L dynamics at tropical Liang-Luar Cave relative to northern ISM and EASM sites) is indeed very interesting and we thank the reviewer for this finding. Interestingly, both periods (8.5-7.9 and 5.7-5.0 ka BP) show high T values at Liang-Luar, reflecting “quiet” or “less erratic” climatic behavior. This coincidence might hint to climatic disturbance in the higher latitudes (see, e.g., the regime shift at Dongge cave indicated by large *L* between 8.5-7.9 ka BP which goes along with baseline values of *T* that do not point to an exceptionally regular climate variability there), while climatic conditions in tropical southern latitudes remained rather stable. This finding hints to a northern origin of the disturbing mechanism, thus corroborating our suggestion of a connection between high-latitude Bond events and monsoonal climate. The influence of Bond events seems to diminish from Europe towards tropical south Asia. It is thus possible that the connection is realized via a modulating of atmospheric circulation related to the Siberian High.

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.

We thank the referee for pointing out this imprecise statement and have corrected it in the revised paper.

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.

In the revised manuscript, we investigate the possible linkage between solar variability and RN analysis results in more detail and attempt to quantify it. To illustrate this, we overlay the TSI data by Steinhilber et al. with a low-pass filtered version in Figures 9 and 10. The corresponding statements in the paper have been revised accordingly.

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)."

We thank the referee for pointing out this apparent inconsistency. However, in Section 4.3 we intended to state that the Steinhilber record does not show an apparent cyclicity while n_L in Fig. 9 visually appears to be consistent with an underlying approx. 1500 year cyclicity. Clearly, a quantitative analysis is needed to support this observation. The corresponding statements has been revised according to the results of the more detailed investigation proposed above.

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.

Throughout the manuscript, we mainly use Bond events and RCC episodes as a temporal reference and benchmark for the detected nonlinear regime shifts in Asian monsoon variability. We thank the referee for again pointing out this inconsistency in our paper. In the revised version, we make more careful statements on potential coincidences between these different types of climatic events.

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.

We agree with the referee that some parts of Sections 3 and 5 can be shortened, specifically those on Bond events and RCC episodes, and revised the paper accordingly. However, we think that the extensive review of the literature on Holocene Asian monsoon changes and their potential impacts on human societies given in Section 5 is appropriate and useful for the readers of Climate of the Past, since to the best of our knowledge, such a recent review on this specific region (Asia) and timeframe (Holocene) is not available so far. As argued in our response to referee 1, we make an effort to restructure and clarify Section 5 to disentangle the review from our own results. For a figure summarizing known periods of societal change and the results of RN analysis we point the referee to Fig. 11 (page 975) which is already present in the current published version of our discussion paper.

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.

We agree with the referee and revised the paper thoroughly to consolidate these inconsistencies.

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.

We think that our paper makes a convincing case that the desirable data features are highly important for advancing the rigorous statistical analysis of paleoclimate variability in the future. Hence, we would like to keep the last paragraph of the Conclusions section, but agree to shorten it in the revised version of our paper.

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.

The figures have been revised accordingly.

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

We thank the referee for this helpful suggestion and changed Figs. 9 and 10 accordingly.

> *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.*

As argued above, a major contribution of our paper is providing an extensive review of the literature on potential influences of monsoonal variations on human societies in Asia during the Holocene and putting our own results into this context. This contribution is reflected in the second part of the title. Therefore, we would like to keep the title unchanged.

Additional references

Donner, R. V., Zou, Y., Donges, J. F., Marwan, N., & Kurths, J. (2010). Recurrence networks—A novel paradigm for nonlinear time series analysis. *New Journal of Physics*, **12**(3), 033025.

Zou, Y., Donner, R. V., Donges, J. F., Marwan, N., & Kurths, J. (2010). Identifying complex periodic windows in continuous-time dynamical systems using recurrence-based methods. *Chaos: An Interdisciplinary Journal of Nonlinear Science*, **20**(4), 043130.