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Interactive comment on "Complex Networks Reveal Teleconnections between the Global SST and Rainfall in Southwest China" *by* Panjie Qiao et al.

Anonymous Referee #1

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General: The general idea of the paper seems relevant and interesting to me. Complex networks are a reasonable choice to study teleconnections in climate systems. Their use in climate science has increased in the last decade, but I am not aware of an application to this region. Furthermore, two-parameter networks are still a rather novel method in this field. However, there have already been several studies linking SST and rainfall in China using different approaches (Zhou et al. 2010, Wu et al. 2012). Furthermore, the chosen region is rather small and its special importance (if there is any) was not made clear to me. I do not see a reason speaking against extending the study area to China as a whole.

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Discussion paper

The authors mention both floods and droughts as hazards that could be better understood based on this work. However, they do not mention droughts after the introduction. Their inclusion into the abstract is therefore misleading. They are also not showing if the correlations they find actually influence the extreme rainfalls that produce floods or whether the effect is only present for low magnitude precipitation. I am therefore not certain, whether this paper (in its current state) fits the scope of a natural hazard journal.

The used data might not be fit to answer all of the questions the authors pose. They mention the complex topography of the study area, but it is unlikely that a 2.5x2.5° grid is sufficient to fully represent this complexity. The MSWEP precipitation dataset (Beck et al. 2016) with a resolution of up to 0.1° and the same temporal range could be better suited for this task. The data is not always described to the necessary extent. It is unclear whether rainfall or rainfall anomalies are studied.

The methods are not fully described in at least two cases. First, the removal of the seasonal cycle is mentioned, but not explained in details. Second, the splitting of the time series into seasons is not completely clear. Does this lead to one time series for each season? How do these look like: 3 months data – 9 months gap – months data - . . ., or a gapless series of the 3 months.

Pearson Correlation is possibly not fit for the data. When explained, the methods are presented in a way that is understandable to a scientific audience. The potential of the complex networks is not fully exploited. Additional network parameters (e.g. betweenness, clustering) could provide further insights and could support the interpretations that the authors make.

The English language of the manuscript is often poor. There are several (> 50) cases of missing words, typos, grammatical mistakes and poor wording. In some cases, this leads to poor understandability. In contrast, the mathematical formulae are well written and described.

The title of the paper is misleading to some extent, as most of the grid cells that have a substantial degree lie fully or partly outside of China.

The contents of the figures are well chosen. They do however need visual improvement to maximize information gain and understandability. Especially the color maps need improvements. Most figures could be larger in size, as they are hard to interpret in the current form.

The introduction seems too long and repetitive at times. The discussion of the results could be more thorough. Apart from that, the overall length of the paper seems fitting.

Specific:

I have the suspicion that parts of the presented correlation could be caused by common seasonality in the compared parameters. This is supported by the fact that some of the timelag-correlation plots show a minimum and a maximum that are offset by $\sim$180 days (Fig. 6b and 8d). The relationship mentioned in lines 118-119 hints at this as well. Due to this I would appreciate a larger maximum timelag ($\pm$365 days) as well as example plots and statistics for rainfall and SSTA.

Instead of using shuffling for the definition of the threshold, I would suggest a classic 95th percentile significance test combined with a multiple testing correction (e.g. Benjamini-Hochberg). Furthermore, Spearman Rank Correlation is a more fitting measure, as the data is likely non-linear.

Uncertainty bounds should be stated with each of the derived timelags, as these are likely up to $\pm$40 days in some cases (e.g. Fig. 7d).

Technical:

I will not spellcheck the whole manuscript. A very frequent mistake is the lack of “the” in front of words that require it (e.g. lines 6, 23, 25) or its unnecessary presence in other cases (e.g. lines 16, 28). Verb tenses (e.g. lines 4, 19) and prepositions are two other major problems. I advise the authors to make use of professional spell-checking.

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Discussion paper

The color bars of Fig. 2 and 3 should scale linearly. A higher contrast between the different colors would enhance interpretability. Fig. 4 and 5 could need an overview map, of where in the world this is.

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