Overall comments:

1. This paper adopts the complex network method [*Fan et al.*, 2017] to find the seasonal relation between the global sea surface temperature anomalies (SSTA) with the rainfall in southwest China (SWC). But comparing the governing equations used in this study to *Fan et al.* (2017), I feel some modification is done without explanation in the manuscript. In addition, similar (even more advanced) approach and extended application have been presented in quite some studies, e.g. [*Liess et al.*, 2014; *Lu et al.*, 2016]. The authors chose a small region and used a coarse resolution (2.5 X 2.5), which might not work for this approach and the research questions they intended to answer. My understanding is the such complex network needs a vast amount of data to feed, otherwise, the results learnt might be biased. I would suggest the study to extend to a large region to fully utilize that method. Also the authors might want to consider extend their literature reviews on the complex network.

2. The authors chose NHESS to publish their paper; however, I find the scope of the study does not fit well with the journal unless the authors improve their writing to emphasize that. Simply speaking, rainfall in SWC does not necessarily indicate hazards, unless it is extremely – dry or wet.

3. The manuscript needs a substantial improvement on the language. The manuscript is very hard to follow and understand, many sentences are ambiguous. Figures and legends are unclear and misleading. The description of the study approach is incomplete and an in-depth discussion from both statistical and physical perspectives is missing in the manuscript.

Major and specific problems that must be addressed before reconsideration are attached below:

Major problems must be addressed:

1. Introduction: The 2$^{nd}$ and 3$^{rd}$ paragraphs in the introduction are literature review on rainfall in SWC and applications of complex network methods respectively, however, both of them just list several related studies without a logical construction. It is quite difficult for readers to follow up, and it does not help leading to the specific research questions of this study. Furthermore, at the end of this paragraph (i.e., Lines 29-31), authors claim that "most of studies discussed the rainfall of the SWC only for single season", therefore this study would like to explore the relation in different seasons. This is not true. There are many studies done by both Chinese scholars and overseas scholars on different seasons, even if different studies might have focus on one or two seasons. So I recommend the authors to remove this claim. I strongly suggest the authors to revise the literature review, most importantly to include appropriate literature for the scientific part (research gaps etc.) and for the method they mainly adopt (complex network).

2. Line 60: As this study discusses the relation between rainfall and SSTA for four seasons independently, why the authors still need removal of the seasonal cycle of rainfall and SSTA data? Also, in fact, I am not clear how the authors did the removal. Please clarify.

3. Line 75-80: Why should we separate the positive and negative degree? Since the authors only mention that they are different characteristics and display the corresponding regions for positive and negative degrees, but the explanation for the underlying mechanisms of these two degrees is lacking. For instance, in Line 100-110, the authors state that most of the clusters locating in the tropics are reasonable because of the important role of Hayley circulation in moisture transportation. But how does Hayley circulation involve in both positive and negative degrees? The authors need to provide clear explanation otherwise it is difficult for readers to follow. And how do the positive and negative degrees contribute to the rainfall in SWC?

4. Lines 82-88: I suggest the authors to clearly state methodology in details in Section 2.2 instead of in the result section. In addition, the authors should pay attention to address the following questions in their revised manuscript: (1) how to obtain the shuffled data? (2) this sentence, "no correlation between the shuffled time series" (Lines 85-86), is quite confusing. Does it mean "no significant autocorrelation for one time series" or "no significant correlation between two different shuffled time series"? (3) it is also unclear why this threshold (i.e., 0.11) is appropriate. At least some sensitive test should be provided to see the effect of choosing 0.11 or close values. It seems that values around 0.1 are all reasonable guess based on Figure 1. Also, I suggest to reorganize the result section into several subsections in line with revised methodology part, in order to have a clearer structure.

5. Line 88-90: the authors try to verify the significance of the correlation through comparing the PDFs for the real data and shuffled data. However, the difference is not that distinct, with the maximum correlation of the real data only reaching 0.2 compared to 0.1 for the shuffled data. Besides, how is the data being shuffled? Just randomize the whole original time series? Or as done in Fan et al. (2017), the time series is shuffled only in year level and the time ordering within a year is unchanged.

6. Figure 2: The maps are too small for the reader to interpret. The same problem is with Figure 3, Figure 6 - 8 (a, c) and Figure 9 (a). In addition, the caption of Figure 2 is not consistent with color bar.

7. L99 – 100: Why is the regional size in spring greater than other seasons? I suggest some explanation should be provided. In fact there are many places that the authors only present the observation from figures without extended discussion or

explanation. It is very important to provide insights rather than just purely stating patterns that can be seen from the figures.

8. Figure 4 and 5: The legend of color bar is missing. The descriptions of these figures are quite confusing (Lines 110 - 119). For instance, $C_3$ is the grid with the largest value in the Figure 4 (c), then why it is chosen there? As in the manuscript, $C_1$ to $C_4$ are selected based on the largest in-degree value (C1 is consistent with Figure 4, but I am not sure about C4). The same problem is also found in Figure 5 (d) – inconsistent with the manuscript. Authors should clarify their statement carefully.

9. Similar to Comment # 5: L113 – 115: What are the relationships between the identified nodes spatial patterns and the inhomogeneous spatial distribution of rainfall in SWC? The authors could elaborate more about the spatial distribution of rainfall in SWC.

10. L115 – 118: What are the possible mechanisms that induces the changes of the spatial distributions of identified nodes with seasons? E.g., the joint-effects of terrain and important SSTA nodes.

11. L118 – 119: The sentence may be inappropriate, please rewrite it. Since one node of SSTA may positively and negatively correlate with different nodes in SWC.

12. Lines 121-125: A significant test for correlations much be done, as the absolute values of correlation in Figure 6 (b, d) is only around 0.1. With all these very weak correlation values, I cannot be convinced by the statement such as "a high daily SSTA in East Equatorial Pacific is probably observed … in SWC". The same problem is also found in the discussion for different nodes (Lines 135-144). And the color bars of Figure 6, 7, 8 (a, c) and Figure 9 (a) are incomplete.

13. Figure 6 a&c: I think there are better ways to select critical SSTa regions, rather than just simply comparing Figure 2 and Figure 3. I think the authors could utilize more advanced method (e.g. in [*Kawale, 2013; Lu et al., 2016*])

14. L141 – 144: Can the authors provide the links between nodes C2, C3 and the SSTA nodes for both MAM and JJA. Because both C2 and C3 are important nodes in spring and summer based on figures 4 and 5. The authors should explain more if the SSTA nodes affecting C2 are different with the nodes affecting C3 in spring or summer.

15. Conclusion part: This section is only a brief summary of the study. The authors are expected to provide an in-depth discussion from the physical perspective, like potential mechanism, instead of just listing some related results from previous studies. I do not see any contribution from this study from reading the conclusion part.

16. When I read the abstract (Lines 9-10), I got interested in the study because the authors claimed that "the time-lag of the teleconnection links … prediction of rainfall in SWC". After I read this manuscript, I do not see how this study could achieve this, the authors should provide related analysis or discussion to support how this study can improve the rainfall prediction.

Some minor issues:

1. L128: I suggest removing ", which has been closed to the limit of the time lag" unless the authors can evaluate the significance of it.

**Suggested References:**

Fan, J., Meng, J., Ashkenazy, Y., Havlin, S., & Schellnhuber, H. J. (2017). Network analysis reveals strongly localized impacts of El Niño. Proceedings of the National Academy of Sciences of the United States of America, 114(29), 7543–7548. https://doi.org/10.1073/pnas.1701214114

Kawale, J. (2013). Mining Relationships in Spatio-temporal Datasets. PhD dissertation. University of Minnesota Twin Cities, Minneapolis, Minnesota, USA.

Liess, S., A. Kumar, P. K. Snyder, J. Kawale, K. Steinhaeuser, F. H. M. Semazzi, A. R. Ganguly, N. F. Samatova, and V. Kumar (2014), Different Modes of Variability over the Tasman Sea: Implications for Regional Climate, *J. Clim.*, *27*(22), 8466–8486, doi:10.1175/JCLI-D-13-00713.1.

Lu, M., U. Lall, J. Kawale, S. Liess, and V. Kumar (2016), Exploring the Predictability of 30-Day Extreme Precipitation Occurrence Using a Global SST–SLP Correlation Network, *J. Clim.*, *29*(3), 1013–1029, doi:10.1175/JCLI-D-14-00452.1.