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Interactive comment on "Historical droughts in the Qing dynasty (1644–1911) of China and the role of human interventions" *by* Kuan-Hui Elaine Lin et al.

Anonymous Referee #1

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This is an important topic in the research on past climate change. The topic also fits the journal. It cannot be accepted for publication, because there are lots of problems must be solved seriously.

First, the manuscript selected the drought and reconstruction and its impact on human society. So far, there are lots of studies in this direction. May I know the innovative points of this manuscript, in comparison with these existing findings? The authors did not make it clear.

Second, there is a big gap between their aim and their methods. In the manuscript, "our objective is to make every drought and associated variables as literally clear and operationally independent as possible." How the authors could evaluate the objective of "clear and operationally independent"? This object has not been discussed in the

later sections. Did authors achieve the aim? Please clarify.

Third, the authors are very proud of REACHES. I also read the paper introducing REACHES as shown in the reference of the manuscript. In fact, the database is from Compendium of Chinese Meteorological Records of the Last 3,000 Years (Zhang, 2013). This book is the basis for whole research and makes REACHES scientific and trustable. The authors should not over-emphasize the importance and innovation of REACHES.

Fourth, as mentioned by the authors, "To comprehensively compare and analyze drought and associated data series from the REACHES with other socioeconomic variables from independent data sources, several archival and index data were also collected for analysis." May I know the similar or different features in the records of these documents? The authors did not discuss enough to compare these sources.

Fifth, there are different categories of drought recorded in the historical documents. Why there are different records in the documents? Please clearly explain the reasons to have these different records in historical documents. Then, the readers will know rationale of these categorizations and see the importance of this research.

Sixth, I am not convinced by the Kernel method. It is common to have the missing data in the documents among different regions. If using the number of records, the results will be disturbed by the data availability condition. The results are thus not reliable at all.

Seventh, in Page 11, there are some linkages according to the one record, "the events would be decomposed and then displayed in a way that drought linked with rainfall, drought linked with frost, drought linked with rice price, rainfall linked with rice price, frost linked with rice price and so on to further calculate their pairwise coefficients." It is not persuasive to have such findings by only one record. In fact, the authors should revise the whole manuscript to review their findings. Please avoid similar problems.

Eighth, please check the language. There are some typos. Such as, it should be Guang Ling but not Quang Ling in Page 11.

Ninth, the language is not clear and concise enough. There are many redundant sentences in the manuscript, such as Page 2 "Studying past drought and humidity has been a long practiced subject in historical climatology and paleoclimatology". If the authors still want to keep these redundant sentences, why not add the references?

In terms of reference, the authors have many judgements without the proofs from their articles. For example, Page 3 "Yet, tree ring reconstruction usually suffers from growing seasonality of trees and blurred interpretation of isotopes." This is not your findings from the manuscript. There are many similar examples in the manuscript to show the authors are not careful enough to conduct the research and claim their findings.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-115, 2019.