

Interactive comment on “Risk assessment of meteorological drought in China under RCP scenarios from 2016 to 2050” by Kuo Li and Jie Pan

Anonymous Referee #1

Received and published: 12 January 2017

This paper addresses a relevant scientific question within the scope of NHESS. The questions tackled are interesting and deserve to be studied, however the article has many structural problems. I will mention the most glaring ones, but there are more.

The first problem is that it is not possible to know if the methods used by the authors were developed by themselves or not. The authors do not sufficiently nor correctly cite the literature in order to put their work into context. When they use new methods, if they use new methods at all, they do not explain what is new, why it is new and how did they develop the method. They do not justify the choices made. That is enough to reject the paper.

Furthermore, the methodology is weak and it is not developed enough. The authors

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make use of a single climate simulation, but they do not discuss how this limitation impacts the uncertainty of their results. The simulations is downscaled to a higher resolution, but they do not discuss how this adds uncertainty to the study. We do not know if the model used is able to simulate droughts in the present. They should validate the capacity of the model to reproduce drought patterns in China. Then they develop an integrated index of drought hazard, but their choices seem quite arbitrary. They don't justify the weights used, they do not explain why they use SPI-12 and not another scale, they do not even sufficiently explain how they calculate the SPI (I guess they use a gamma distribution, but they do not explain this). Afterwards they estimate the vulnerability. They use a lot of data in order to estimate vulnerability, which is good, but they do not explain where the data comes from, how do they determine which factors are related to exposure, sensitivity or adaptive capacity. They do not justify if the factors are independent enough from each other. They do not test the robustness of the index (how does it change if we change a the factors?). They do not explain how they normalize the data, etc. They do not discuss the quality of the statistical county data used. The list is, unfortunately, long.

The figures do not have enough quality and there are not enough figures. The box plot figures are not well explained. The vertical axis is labeled “Percentiles”, when it does not show percentiles. The data shown is dimensionless and thus the axis should not be labeled. Maybe “Percentiles” should be in the title of the plot. The box plots show the percentiles, but the authors do not explain what the whiskers and the limits of the box mean (which percentiles do they represent?). The maps in Figure 4 show the risk in the hazard in the future, but the authors say they show trends. These are not trends, these are maps of the hazard in a future time frame. The authors should also show the values of the hazard in the present climate and maps showing the differences. Same happens for the risk.

Writing an article in English is not easy when your mother tongue is very different to English. I have the same problem and it is often painful. But sometimes it is not clear

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if the authors have difficulties with the language, which they have, or the underlying scientific concepts. The author's should send the article to a native English speaker in order to correct the grammar and the vocabulary.

My point of view is that the article should be rejected. However, I encourage the authors to continue their work and re-submit the article in the future when their work is mature enough. The topic of the article is interesting and I am sure that studies on the future drought risk in China are necessary. Furthermore, the statistical data the authors use in order to calculate the vulnerability is very interesting. Unfortunately they submitted their paper too early.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-257,
2016.