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Comment on nhess-2021-218

Anonymous Referee #1

Referee comment on "Spatiotemporal evolution and meteorological triggering conditions of hydrological drought in the Hun River basin, NE China" by Shupeng Yue et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-218-RC1>, 2021

General comments

The authors present an analysis of drought characteristics in a Chinese river basin regulated by a large reservoir. They are concerned with drought characterization in terms of severity and duration, the delay between meteorological and hydrological drought and the role of a large reservoir in mitigating drought effects. Their work is based on statistical modelling of drought characteristics using the available data for their selected case study. Most of their work is based on already tested methods, so the originality is limited. The main contribution is the use of a Bayesian networks to model the drought propagation threshold. However, I am afraid the conclusions extracted from the discussion of results are only applicable to the specific case study under analysis.

The research topic is relevant for the readership of Natural Hazards and Earth System Sciences. However, the paper needs more elaboration before it can be finally published. The manuscript is poorly written, and many details require additional polish. It lacks a clear strategy for motivation of the work and the methodology is not entirely clear. Some figures need additional explanation and discussion.

The presentation of the work needs improvement. The background information provided in the introduction section is too general. It is a general discussion of droughts, with references to many papers, but it is not focused on the specific topics analysed in the paper or the methods that are applied. In its current drafting, it does not help the reader to understand the motivation of the work and identify where the authors go beyond the state of the art with their work. I suggest rewriting this section, stating the main research questions addressed in the work and referencing the authors that have tackled these questions before, summarizing their results and identifying the gaps to be filled by this work. The scope of the work presented in some of the references cited in the introduction (for instance, Wang et al, 2019; Wang et al., 2020) is very similar to that of this paper, and it should be discussed in greater detail.

I found the presentation of results a bit confusing. The methodology section presents the methods used, but it is not very helpful to understand the rationale for the work carried out. As it is, the paper is an exercise of fitting statistical models to data, but it lacks structure. The authors should clearly state their objectives at the beginning and devote the methodological section to present their strategy, indicating how they are going to tackle the research questions addressed in their work and why they chose the methods they are proposing. For instance, they analyse drought "resistance" without a proper definition of this term. I would have liked to see an orderly presentation of the objectives, the approach followed in their analyses, and the expected results, together with a clear definition of the metrics used to evaluate the performances of the models. It would have been very helpful to follow the presentation of results.

Apart from the lack of structure, the manuscript is not properly finished. There are too many grammar and typographical errors that indicate that the manuscript is still at an early stage of revision. For instance, the authors refer to the "tyson" polygon method (line 26), but I guess they mean "Thiessen". The authors should have checked their manuscript for such evident errors before submission. You may find below several of such errors, but the list is far from complete.

Specific comments

The methods section is missing SRI and SPI. They are well-known drought indices, but their application is not straightforward because it requires normalization of the probability distributions. What transformations were applied and how good were the results?

The definition of drought "severity" is not clear. On line 1 of page 5, it is stated that drought severity is the "run length", but run length is related to duration, not severity.

I have some trouble with the interpretation of the drought propagation time. It is stated that drought propagation time is the SPI time scale with the best correlation with the 1-month SRI. However, in the results section, the analyses are presented separately by months (figure 7). I interpret these results as the correlation coefficient obtained between n-month SPI and 1-month SRI for the values of each month in successive years. However, the values of the correlation coefficients change from month to month. If we focus on relatively high correlation coefficients (over 0.5), the band is larger in some months than in other. How can it be that the drought propagation time in June is 2-6 months, in July and August increases to 3-12 months and in September it drops to 2 months? Am I missing anything? I would expect some discussion of this in terms of droughts, physical processes, and reservoir operation, not only in terms of numerical results from the statistical analysis.

I am also troubled by the discussion of the propagation time. The sentence in line 5 of page 16 states: "Considering the lack of data before the construction of the DHF reservoir..." How can you make an analysis of the effect of a reservoir if there are no

control data without the reservoir? The authors are attributing the differences in the results obtained in the different stations to the reservoir. How can they be sure of this if there are no data prior to the construction of the reservoir? In fact, they even state that the reservoir "weakened the drought resistance upstream the DHF reservoir". I cannot imagine a way in which a reservoir may influence the threshold of rainfall deficit required to generate a drought of certain level in an upstream basin. This should be discussed. Could it be just variability in the data?

Finally, I was entirely lost with the presentation and discussion of the Bayesian network model to link cumulative precipitation deficit to hydrological droughts. What is the purpose of this analysis and why is a Bayesian model required? What is the utility of the conditional probability distributions shown in Figure 9? Are these distributions derived from the fitted models? What is their relationship with actual data and their usefulness? Please clarify all these questions.

Some of the figures need additional work or explanations. In Figure 1, I was not able to locate the stream gauge upstream of the reservoir (BKQ). The definition of severity is not clear in Figure 2. What are the bar charts that appear to the right of the plots in Figure 7?. Are these the PTMH?