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MS TITLE: Comprehensive evaluation of hydrological drought and the effects of large reservoir on drought resistance in the Hun River basin, NE China

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RESPONCES TO THE REVIEWERS' COMMENTS

We do appreciate all useful comments and suggestions on our manuscript.

The MS was thoroughly revised, against all revision comments from the editors and reviewers. We have taken this opportunity also to read through and tried to perfect the analysis details, discuss the results more comprehensively and pick up any minor grammar, wording or format problem and made corrections accordingly so that it strictly follows the Journal formatting requirements. Detailed corrections and revisions are listed below point by point. And, all the revisions have been addressed in the reply and highlighted in the manuscript with yellow background.

Reviewer # 2:

1. The description of the work in the abstract should be improved; in fact, while reading the abstract, the work appears to be only a case study application. On the contrary, I believe that the Authors are proposing a methodological framework to evaluate the impact of meteorological droughts in regulated river systems.

[Authors' response]: We gratefully appreciate for your valuable suggestion. As suggested, the description of the work in the abstract has been revised (Page 1 line 8 to 26).

2. (5) should be better explained, e.g. by adding a sketch. If this equation was previously used for the same scope, a reference should be added. I would also add a sketch describing depicting the general framework adopted here. A better description of the framework could help the reader understand the rationale behind the specific statistical tools implemented here.

[Authors' response]: We gratefully appreciate for your valuable suggestion. We have already added related references in our revised manuscript (Page 8 line 10 to 12). Meanwhile, we have added the schematic of determining the drought propagation threshold in Methodology section to make the presentation of methodology clearer (Page 9 line 2).

3. The CPD is used as condition to explain the occurrence of severity and duration of hydrological droughts; why using CPD and not meteorological drought characteristics estimated based on SPI (e.g. intensity, severity and duration of meteorological droughts)? This point is not very clear to me, and the same could be for a potential reader. Further, why using the 0.95 CPD quantile as a threshold for hydrological drought? Does it result for data analysis or from the hydrological response of the catchment?

[Authors' response]: We gratefully appreciate for your comment. As the discriminant standard, drought intensity and duration of meteorological droughts are relatively absolute and not convenient to monitor. Precipitation, the most basic hydrometeorological monitoring index, is easy to obtain and the results are intuitive and easy to apply. Therefor, the CPD is more suitable as condition to explain the occurrence of severity and duration of hydrological droughts.

[Authors' response]: The use of 0.95 CPD quantile as the threshold of hydrological drought was based on the relevant references and was the result of data analysis (Page 8 line 20 to 22). (https://doi.org/10.1016/j.scitotenv.2020.136502; https://doi.org/10.1016/j.jhydrol2020.125738)

4. What about drought frequency? It is mentioned at line 17 of page 9, yet not defined or investigated.

[Authors' response]: We gratefully appreciate for your comment. "Drought frequency" in this case refers to the number of droughts identified based on run theory, and we've changed the "drought frequency" to "drought events" in our revised manuscript (Page 12 line 5 to 6).

5. Section 4.2. What do you mean by "periodicity"? Do you mean frequency or probability?

[Authors' response]: We gratefully appreciate for your comment. The "periodicity" means the occurrence frequency of hydrological drought, and the periodicity was analyzed by calculating the return period in this study (Page 13 line 1 to 2).

6. At SY and XJWP stations, the correlation is very high for a large variety of SPI time scales; this makes the identification of PTMH values highly uncertain. This affects the conclusions drawn from figure 8; to overcome this issue I suggest to include in figure 8 the PTMH uncertainty bounds. This is the only major issue that should be addressed by the Authors.

[Authors' response]: We gratefully appreciate for your valuable suggestion. As suggested, the uncertainty of the correlation coefficient between the monthly SRI and the multi-time SPI has been calculated. The T_p was indicated by the month with strong correlation and low uncertainty (Page 15 line 9 to 13). Figures 8 and 9 have been replotted due to the changes of T_p values in some months (Page 16 line 4 and Page 18 line 1). Meanwhile, the conclusions related to T_p have also been revised.

7. *For clarity, in Figure 9 I suggest to use the same x-axis range for all D or S panels.*

[Authors' response]: We appreciate for your valuable comment. As suggested, we have used the same x-axis range for all D or S panels in Figure 9 (present Figure 11) in our revised manuscript (Page 19 line 2).

8. There are some typos in the text; I warmly suggest to revise the English language.

[Authors' response]: We are very sorry for the mistakes in this manuscript and inconvenience they caused in your reading. The manuscript has been thoroughly revised and edited by a native speaker, so we hope it can meet the journal's standard. Thanks so much for your useful comments.