Hydrology and Earth System Sciences Discussions

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Comment on hess-2021-120

Anonymous Referee #1

Referee comment on "Critical transitions in the hydrological system: early-warning signals and network analysis" by Xueli Yang et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-120-RC1, 2021

This paper applies the autocorrelation process and a harvested population model as well as network analysis for the early warning signals of transition in the hydrological system at the global and CONUS city scale.

Overall, the paper is well-written and certainly contains many novel ideas potentially helpful for the borader community.

I see the paper will be strengthened by

- Line 50: important role
- Line 219 – 222: discussion about the relative magnitudes of AR1 and s.d. I agree that visually figures 1b and d are quite distinct, but the authors may want to provide more information about whether any objective measures of high and low exist or any reference state exists in AR1 and s.d. in gauging the state of the system regarding how far away from the tipping point.
- In addition, it appears unclear to the reviewer whether Figure 1 is an example toy problem to illustrate the concepts or related to the main finding.
- 2.3: the beginning few sentences seem to be repetitive of part of the introduction – therefore, may be better to combine with the introduction or shorten it.
- Line 253-254: single-plural mismatch
- "The year of critical transition was determined based on the abrupt change of slopes in each cumulative time series. We then divided each original precipitation (or PET) time series into two (quasi)stationary parts using this critical transition year" – maybe good to provide in the appendix, since this is quite important. Also, may want to give more explanation for what you mean by the two parts being quasi-stationary.

- Was the critical transition year 1994 identified a priori from the method described in point 6?
- Line 326 – 332: how is the claim supported? Maybe an additional figure or if not important may choose not to mention. The reviewer is confused.
- For city-scale analysis, is each transition year for each city identified using the same method as that for global scale?
- Why is CONUS PET not analyzed? Is it due to a lack of data?
- Figure 4a, is it possible to indicate region number 1-9 corresponding to the adjacency matrix in a? This will facilitate the readers to connect the meaning of b to the spatial pattern of the network in a.
- The trend of AR1 within the moving window prior to transition year: in figures showing this metric, the non-monotonic trend can make it less useful as an indicator.
- Another question the reviewer is wondering about: I understand the paper's network analysis focuses on the network topology structure prior to transition, but will the network structure end up being different after the transition? i.e. will the enhanced network connectivity stay or gradually 'relax' towards some 'climatological equilibrium state'?
- The phrase potentially catastrophic transition may be less emphasized: the mean precipitation anomalies (for CONUS cities) are analyzed. If another variable like the maximum precipitation or number of days exceeding historical summer mean (just arbitrary examples of more catastrophic flavor), the reviewers will be more convinced.
- The reviewer thinks that making more efforts to connect the global scale to the city scale will make the paper more coherent. For example, results in Fig. 3 are partially tied to global climates. Figure 5 and Figure 2 also seem to have some connections. In the introduction, the motivation for city-scale analysis may allude to some of these findings. E.g. city-scale responses are embedded in global hydrologic cycle changes but form systemic coherent structures/patterns – highly appealing to system-based network analysis.