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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A stochastic model for drought risk analysis in The Netherlands" *by* Ferdinand L. M. Diermanse et al.

Anonymous Referee #1

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The paper 'A stochastic model for drought risk analysis in The Netherlands' attempts to provide a stochastic framework to generate time series of hydrological variables. In my opinion, the paper fails to deliver on its promises and requires substantial revisions before it can be considered for publication. Below some suggestions are made: hopefully they can help the authors re-draft the paper to make it more suitable for the journal and the hydrology research community more in general.

1. Synthetic hydrology has been around for a long time. The authors address a well-known problem in hydrology, namely the lack of hydrological time series which are long enough to provide robust estimates of water system performance and response. This issue has been addressed in hundreds of papers, starting from the seminal work of Matalas (1967) to more recent papers employing synthetic hydrology to inform water

supply vulnerability assessment under climate change (Nazemi and Wheater, 2014; Hao and Singh, 2016) and under changing drought (Borgomeo et al., 2014; Herman et al. 2016). A more through review of the synthetic hydrology literature is needed. This would help the authors better position their work in the vast literature on synthetic hydrology and would also help them better articulate the novel contribution of their work, which leads to the next comment.

2. Contribution. The methods section seems to be a copy-paste from a textbook on time series analysis in hydrology. Providing all the equations for the alternative AR models is not useful unless they are adapted to the case at hand. It is not clear to me how the authors integrate their temporal dependence modelling with the copula approach for the three gauging stations considered. Given the lack of discussion of previous literature and the description of the methods, it is impossible to see how this paper advances current methods. Figure 2 says 'transformation process' whilst in reality the authors are essentially showing how copulas work. How is this novel? In this sense, see also the relevant comment provided by Francesco Serinaldi on this paper.

3. Risk framework. Traditional hydrology and drought management are based on risk assessment informed by frequency analysis of observed hydrological data. The challenge right now is how to extend the methods that had been developed to deal with historical variability to deal with climate change. The paper does not address this issue, which is the main issue that researchers working on risk-based frameworks in hydrology are trying to address. The risk framework presented in Figure 1, although it may be valid to support Deltares' applied work, is not novel and should be revised in light of recent developments in risk-based frameworks for water management some of which are cited by the authors. Perhaps the authors should consider removing references to this risk framework and work more on positioning the work with respect to the synthetic hydrology literature, given the focus/results of the paper.

4. Approach/language is unscientific. In various parts of the paper the authors leave too much to imagination. For instance, in section 3.2, the authors should show the

results of the distribution fitting exercise instead of just saying that various tests were carried out. Similarly, for section 3.3., the authors should insert a table showing the results of the different tests. Line 10 on page 5 is another example of this unscientific language: unless the authors prove it, how can they say that it can be applied at different time scales?

Other comments: 1. Abstract: The first 10 lines of the abstract are not informative. The reader needs to wait until line 8 to see what the paper is about. The abstract should not give the reader an overview of the challenges facing decision-makers, rather it should tell her/him immediately what problem the paper tries to address (generating hydrological time series water management) and what's new about the study's approach. Also note that the authors mix the definition of water shortage with drought in the abstract. These are two different things, see for Van Loon and Van Lanen (2013) for some definitions.

2. Precipitation deficit seems to imply that the authors are looking at a precipitation anomaly, whilst in reality they are just examining evaporation-precipitation. Why not look at a precipitation anomaly instead, which is largely used to identify dry periods in rainfall time series?

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