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Discussions

Interactive comment on "Assessing the impact of rainfall seasonality anomalies on catchment-scale water balance components" *by* Paolo Nasta et al.

Anonymous Referee #2

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The paper deals with the assessment of water balance components (i.e. water yield, evapotranspiration, groundwater recharge, etc) and relative deficit in case of climatic anomalies related to seasonality in a Mediterranean basin. This is done by parameterizing a rainfall generator model according to two different schematic representation of seasonality (called "static" and "dynamic"), and using synthetic rainfall series as input to the SWAT hydrological model.

While shifts and changes in seasonal patterns have been addressed by many researchers as key factors in analyzing the hydrological impact of climatic fluctuations, the consequent issue of how these phenomena may impact the regulation of artificial reservoirs, designed for annual or multiyear storage purpose, deserves attention.

The paper is in general well sounded and relevant although it could be improved in my

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Discussion paper

opinion, accounting for the following suggestions:

1. The paper is compound by two main issues: the first one is referred to the analysis of the climatic forcing and the parameterization of the rainfall model; the second one is related to the use of SWAT model to obtain different components of water balance. A stronger emphasis is given to the first one, which is also performed by comparing different methods, while the second one is much less discussed. Also, the overall paper goal could be better assessed and the methodology more detailed in the introduction. To make an example, the sentence "The goal of the study is to characterize the rainfall seasonality and its anomalies by using two approaches." (line 81) is in my opinion somehow misleading with respect to the overall paper objectives and developments.

2. Dealing with issue #1, i.e. seasonality assessment, in the introduction the PCI and SI methods are indicated as most popular approaches. Nevertheless, the authors do not use them but rather prefer an SPI based analysis and the procedure proposed by Feng et al (2013). A better acknowledgement could be provided about the reasons of such choices, and the comparisons between the performances of different methods.

3. AT line 184 the authors state that they "assumed that the duration of the wet season follows a normal distribution…". While I do not doubt that such hypothesis may be a feasible one, I would expect some kind of validation or testing of it through observed data.

4. The stochastic Poisson point process with exponential distribution of pulses that is finally used for rainfall generations, I believe could be referenced to classical papers like Rodriguez-Iturbe, I. et al (Journal of Geophysical Research, 1987) and /or Eagleson (WRR, 1972), may be also of interest a more recent application by Veneziano and Iacobellis (WRR, 2002) on Italian datasets, among many others. The use of seasonal parameterization on a stochastic rainfall generator is also a matter of interest.

5. I believe that also conclusions should be reinforced. First by better depicting which practical use the methodology could be exploited for and, second, by deepening the

discussion about the characterization of rainfall seasonality and its anomalies, according to different approaches, which was mentioned as a goal of the study.

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