

Interactive comment on “Characteristics and controlling factors of the drought runoff coefficient” by Rei Itsukushima

Anne Van Loon (Referee)

a.f.vanloon@bham.ac.uk

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The paper by Itsukushima aims to quantify and model the relationship between drought conditions and controlling factors based on geology, land use, and topography. This would have been an interesting topic, but the research does not do what is promised. The analysis used average annual discharge divided by average annual precipitation, which clearly is not the same as drought. Average annual Q/P includes both high- and low-flow periods and the annual timescale is too long for many droughts. This is unfortunately not the only misunderstanding in the paper. Terms are mixed up, a whole body of literature is missed, important factors are left out of the analysis, and the work does not lead to new insights. This paper cannot be accepted for publication in HESS. I explain my rejection below and give more detailed comments in the attached file.

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Throughout the paper there is confusion between annual discharge, low flow and drought, both in the literature review and in the analysis. The paper analyses average annual discharge, but talks about drought and low flows. The literature about drought cited in the Introduction is not relevant for the current work on runoff coefficients. Contrary to the claim of the author that this is the first attempt to study the relationship between runoff coefficient and controlling factors, there are already many papers doing this. For example, Berger & Entekhabi (2001), Laaha & Blöschl (2006), Merz & Blöschl (2009), Carey et al. (2010), Sawicz et al. (2011), Ali et al. (2012) and papers by the same and other authors. The probabilistic approach of looking at different return period is maybe new, but the calculation, interpretation and discussion of what this means is unsatisfactory. It is unclear how the runoff coefficient is calculated for each occurrence probability? Have you just divided the 400yr discharge by the 400yr precipitation? Why? What does this mean?

Other methodological flaws include: using different time period of data for precipitation and discharge to compute the runoff coefficient, as the meteorological input might be completely different between these periods, and not taking any climate-related factors into account, as the differences identified in the paper seem (at least partly) to be a function of ET and snow, which are related to latitude and altitude. The analysis should be completely redone and extended. The framework of Wagener et al. (2007) could be a useful guidance.

Finally, the discussion section just mentions a random selection of papers, without thorough synthesis of what the results of this study mean and how they compare to the large body of existing literature on this topic. The discussion and conclusion also contain a lot of misunderstandings and misinterpretations.

References:

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Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-330/hess-2019-330-RC2-supplement.pdf>

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, <https://doi.org/10.5194/hess-2019-330>, 2019.