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Discussions

Interactive comment on "Streamflow drought: implication of drought definitions and its application for drought forecasting" *by* Samuel J. Sutanto et al.

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General Comments

The authors performed an intercomparison of three different streamflow drought indicators, with the goal to highlight the differences in the drought characteristics associated to each index and to detail the implication on drought forecast. I found the overall goal of the study meaningful, given the confusion that still arise among scientists and operational users on the topic, but I also found the paper and its structure generally out of focus. The key message of the paper "...developers of DEWS and end-users should clearly agree among themselves upon a sharp definition on which type of streamflow drought is required to be forecasted for a specific application." is in my opinion, even if

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Discussion paper

relevant, better suited for a short communication or letter paper rather then a research paper.

The research results that should support this conclusion as reported in this paper are somewhat lacking in both clarity and rigorousness.

The main drawback of the analysis is the fact that the authors uses three drought indicators that rely on quite different input data and basis hypotheses to conclude that they provide a different picture of drought. This result is quite obvious after an attentive read, given the background premises: - daily data for threshold methods vs. monthly data for SSI. - 90th percentile for threshold methods vs. median for SSI (SSI=0). - Event-based approach for threshold vs. single monthly value for SSI All these discrepancies in the drought definition make the intercomparison a mere exercise, and its outcomes are hard to translate into actual general considerations.

An additional drawback is the general lack of details on the implementation of the three approaches, which severely limits the possibility for the readers to extrapolate meaningful information from the research outputs.

Finally, the analysis on the implications on drought forecast, which should be the main focus of the paper according to the title, is very limited in scope, and it needs to be significantly expanded in order to keep it as the focus of the paper.

Specific Comments

Introduction

The authors should better highlight how different definitions of streamflow drought in DEWS exists also for two reasons: 1) different users have different needs that can be accommodate by different indicators (e.g. river navigation may be affected more by FT droughts that VT droughts), 2) different available input data lead to different definitions (e.g. threshold methods may not be suitable for monthly data, and daily data may not be available in near-real time).

Data and Methods

The description of the different drought indices need to be more explicit. How the drought events are defined for each index? How is the onset computed? Severity? Duration? Any event definition in the SSI? Etc... Also, more consistency on the adopted thresholds need to be enforced (why SSI=0 is used as threshold when 90th percentile is used for VT and FT?). It is also worth to mention that a VT method based on the same LISFLOOD data is currently operationally implemented as part of EDO (https://edo.jrc.ec.europa.eu/).

Results and discussion

There is a clear unbalance between the historical analysis and the forecast. Give the title of the paper, I would aspect much more emphasis on the latter.

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