Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-220-RC1, 2017


Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Searching for the optimal drought index and time scale combination to detect drought: a case study from the lower Jinsha River Basin, China" *by* Javier Fluixá-Sanmartín et al.

**Anonymous Referee #1**

Received and published: 25 May 2017

Summary: This manuscript presents an analysis of meteorological drought metrics over the lower Jinsha River Basin in China. They use precipitation data from 29 meteorological stations and calculate various formulations of the Standardized Precipitation Index (SPI), the Rainfall Anomaly Index (RAI), the Percent of Normal precipitation (PM) and Deciles (DEC). These indices are then evaluated spatially and in the context of their intensity using what the authors call the Overall Drought Extension (ODE) and the Overall Drought Intensity (ODI). Characterizations of these metrics are then compared to historical documentation of droughts over the lower Jinsha Basin from 1960-2014 to assess their efficacy in characterizing historical drought events. The authors make



various conclusions about which of the indices and their spatial characterizations best represent the historical data.

General Remarks: This is generally a well written paper (it nevertheless could benefit from some English writing improvements and attention to typos throughout) that seeks to evaluate how best to characterize meteorological droughts over the lower Jinsha Basin. It should be published after some major revisions regarding clarity and content, which I outline as general comments below.

1. The authors make clear that their assessment is specific to meteorological drought and therefore focus exclusively on precipitation. This is fine as far as it goes, but they make several statements about temperature and river discharge assessments in the context of droughts that are too critical and not entirely accurate. Moreover, they point out that temperature/ET plays an important role in droughts within their study region (e.g. Pg. 3, Lns. 27-29). While they note as a caveat in their conclusions that ET has not been considered and may explain some of the deficiencies in their assessments, it is too little and to late in my opinion. The authors need to take on this obvious criticism of their study more directly and provide more guidance on how it might impact their results, if not try to quantify the impact of ET in an assessment metric. They also should not be so dismissive of the vast amount of work that has shown integrated drought metrics like modeled soil moisture, PDSI, SPEI, etc. to work as a suitable measure of drought (they mentioned the US Drought Monitor, but fail to note it is based on PDSI!). For instance, their paragraph starting on Pg. 2, Ln. 31 is far too dismissive of integrated metrics and reads like a poor justification for why they focus only on precipitation. If they only have reliable precipitation data over their study region that is fine, but a focus on precipitation alone in this case should not be falsely justified by an attempt to dismiss integrated metrics. This aspect of the paper needs to be modified throughout.

2. I am not convinced that the metrics proposed by the authors are new. They claim that the ODE and ODI are newly developed metrics and tout their development at

multiple points within the manuscript. The ODE is just a form of drought area index and is no more than a measure of the total area of their study region in drought. A similar criticism can be made of the ODI. I therefore have no criticism of the application of these methods, just that they should not be touted as newly developed metrics or metrics of particular novelty that somehow add to the importance of their study.

3. The authors present quantitative metrics for comparing drought conditions based on their metrics and the historical records of droughts in the region. What is not clear, however, is how they actually translate the historical data into quantitative measures that can be compared to the drought metrics. In other words, they define skill scores in terms of hits, misses, etc., but what actually constitutes a hit or a miss? Is it just timing? Are magnitudes considered? It is also not clear why the authors consider the historical accounts a reliable benchmark, relative to the more quantitative measure of droughts that they develop in their study. I do not think enough emphasis is placed on skill scores that are impacted by inaccuracies in the historical records (in terms of how well they characterize the timing, severity and spatial extent of droughts) relative to what the authors construct from the network of precipitation records.

Specific Comments:

Pg. 10, Ln. 17: While not essential, the authors might consider using two consecutive positive or negative years to end or start a drought. There are definitely periods in Figure 2 that identify droughts as separated by a single year of positive SPI values or very short droughts that represent just single-year excursions. If more persistent and widespread droughts are the interest, a 2-yr criterion for beginning and ending droughts might help.

Pg. 15, Ln. 6: The authors optimize the characteristics of their drought metrics based on skill assessments over the full historical interval. This is akin to calibrating the forecast model and then performing in-sample skill assessments. A more rigorous assessment would be to optimize over a specific period and then assess the skill in an



out-of-sample period. This could be done using block hold out periods or leave half out assessments. As it stands, however, the authors optimize over the same period that they assess the skill of their metrics. This is particularly relevant when considering the authors' methods for future drought assessments. Their in-sample skill assessment is very likely to exaggerate the efficacy of their metrics for future droughts.

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