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Interactive comment

Interactive comment on "The impact of elevation and flow dynamics on hydrological drought and wet spell characteristics in semi-arid southeast Arizona" *by* Mengtian Lu et al.

Anonymous Referee #2

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The authors deal with a modification on the definition of hydrological drought that is supposed to be more robust for ephemeral rivers with prolonged zero-flow periods. While I understand the reasons behind the need of such modification of the van Huijigevoort et al. method, I find the message not well communicated and the proposed solution not fully on point. Both the tackled issue and the proposed solution need, in my opinion, to be better described, maybe with the help of a visual representation of a real study case (for a specific event/year) or even an artificial case (that highlight the key drawback of the current method) before showing the performance on all the available data. At the current state, it is difficult to gasp how the suggested modification actually works, since the full description is based only on text, and the examples in Fig. 4 is

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Discussion paper

really hard to read. Also, since the method is needed for zero-flow rivers, the authors should focus only on such stations rather than all the available stations. If I understand correctly, the authors state that the method of van Huijigevoort et al. may "break" a drought event in two events if an event start during the wet period and continue during the dry one, but this should be highlighted in a figure that show one of such case and how the method solve the issue. My understanding is that the author define a cdf of number of (antecedent) dry days that is different for each day of the year, rather than the same for all the day based on the total length of the zero-flow. This seems a solution to avoid "breaks" for events that started during the wet period, but can lead to difficulties for events starting at the beginning of a dry period. If my understanding is correct, I suggest to the authors to consider, first of all, if their goal is to produce an indicator that can be update in near real time (while the event is developing) or that defines the drought on past data. In the second case, better solutions can be found than the one proposed. Since a "true" definition of drought/wet spell start and length is not available (for obvious reasons), the authors need to clearly highlight that their outcomes are at least more reasonable that the one obtained with the previous method and not just as arbitrary. A second point of contention for me is the need for a better explanation on the reasoning behind this kind of definition of hydrological drought and (especially) wet spells in zero-flow rivers. As the authors stated, they are looking at an issue that arise for specific rivers, with zero-flow during most of the year and flow only during monsoon, but it is not clear what is the goal of having hydrological drought (or wet spells) defined for such rivers in such a way. While the classic analysis of dry spells (length of periods of zero-flow) or wet spells (length of period with positive flow) in such rivers is relevant, I do not understand, for instance, the reasoning to define a day as part of a "wet spell" even if the flow is zero (e.g., first half of 2007 in Fig. 6a). Finally, the title of the paper is ambiguous, since the focus in on a redefinition of drought events in zero-flow rivers but the title seems to imply that the effects of elevation and flow dynamics in semi-arid rivers will be discussed. Follow some minor comments: P2L30. Why wet spells on rivers are defined based on precipitation here? P2L39. This is not true for all the cases. There are plenty of evidences that over some regions less extreme drought are expected (e.g., northern Europe). P2L50. What is the relevance for water managers in rivers with zero-flow? Are those rivers under any water managing? P4L88. Why the river is here identified as perennial, whereas is defined ephemeral in the rest of the text (see e.g., P3L74) or just partially perennial (P4L103). Please be consistent. P4L120. You should focus only on the NB rivers, and eventually show that your method works for the other rivers too (if this is the case). P6L142. This should read as: "The TLM has a problem for locations with zero flow (in a specific period) for a considerable amount of years". Please reword. P7L181. It is really difficult to extrapolate how the two methods work from fig. 4b. If 2003-2004 is a good example year, please make a specific figure that highlight the key differences between the two methods. P8. Sensitivity Analysis. This section does not seem well thought-out in my opinion. The range of values adopted in this analysis need to be better supported by some reasoning (e.g., pooling up to 180 days? moving windows of 5 years?). P9L265. This description of the behavior of drought is a consequence of your definition of the events rather a fact. You need "independent" evidences on the behavior of drought to support that your reconstruction is more adherent to the reality than the one obtained with the previous method. P10L290-300. This analysis on the long-term variations is out of topic and not well supported by formal trend tests and analyses. P11. Fig. 8. This figure is rather confusion, and, in my opinion, not the best way to convey the key finding that the authors want to show here. P12. Section 4.3 is this on only one river station or all the stations combined? This is not clear. P13L374. If a backward moving window is used (rather than a most common centered one) this need to be clarified and justified in the methodology.

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