Hydrol. Earth Syst. Sci. Discuss., 9, C1781–C1788, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C1781/2012/

Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "A generic method for hydrological drought identification across different climate regions" *by* M. H. J. van Huijgevoort et al.

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Received and published: 26 May 2012

We thank Anonymous Referee 2 for commenting on the manuscript. In general, Referee 2 mentions that the manuscript does not introduce new scientific merits. However, to the authors' knowledge, drought analysis on the basis of runoff in transition zones explicitly dealing with zero runoff periods and combining these with runoff periods, has not been done before. Therefore, we believe that the manuscript does contribute new insights and can improve identification of drought events at global scale, for example through large-scale models for current and future climates, which is relevant to explore

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future drought across the globe or continents (see Introduction, below).

Introduction
First of all, we would like to mention that the paper aims to present a new methodology rather than an application (i.e. our examples of drought in selected river basins derived from observed flow and global drought from land surface models). The drought analyses for the river basins and at global scale (Section 4) are just meant to illustrate the methodology. The paper is certainly not a validation study of land surface models, and hence a check against observations is not included. In the next phase, the proposed methodology will be the base for exploration if large-scale models (e.g. global hydrological models, land surface models) are able to capture drought at the global, continental, or major river basin scale, as a follow-up of studies focusing on soil moisture (e.g. Sheffield and Wood, 2007), or river flow (e.g. Gudmundsson et al., 2012; Prudhomme et al., 2011). These studies lack a single metric for hydrological drought (i.e. drought in runoff) that can be applied everywhere on the globe, which is a prerequisite for a proper model intercomparison. The methodology is primarily meant for natural conditions (e.g. no storage dams) and is not supposed to provide a drought impact assessment at a detailed scale, which requires a more context-specific approach (e.g. drying out of water holes, stakeholders affected). We will revise the objective of the paper to make this message more clear.

We will answer to the points raised by Referee 2 in more detail in the following lines.

1. *The authors claim to provide a single metric, but I interpret the approach as simply using a second metric where the first one fails. I would think that is different; it does not make the two indices consistent or make for a 'generic' overall approach.*

By combining both methods, more information is gained than by simply applying both methods separately. With the combined method, droughts identified in periods with runoff can continue in following periods without runoff and are then identified as belonging to one single drought event with the associated longer duration rather than applying the two methods separately. We will add a line in the Introduction to make this more clear:
Line 12 page 2037: The generic drought identification method combines the threshold level method and the consecutive dry period method and allows a single drought event to continue in periods with and without runoff. In this manner, new information is gained compared to applying both methods separately.
An important part of how the methods are combined is the scaling procedure. This was also mentioned by Referee 1 as unclear in the manuscript. We propose to extend step 6 in the methodology (Page 2044) to the text given in the answer to Referee 1 (point 4). By extending this part, the benefits from using the combined method instead of two metrics separately and the reasons why we consider this a generic method will be more clear.

2. *Ephemeral river systems will obviously be more resilient to lack of flow than would be perennial river systems. That is not to say that there are no hydrological drought impacts in such systems; they can be related to the drying out of water holes, floodplain storage dams or tanks and the lowering of floodplain aquifer groundwater tables, for example. It seems reasonable to assume that the time since last flow has some value in predicting such impacts I suppose. The approach proposed here might work for monsoonal rivers, where the river falls dry in most years for variable durations of time. However it is not clear to me if this approach works if there are typically several years in between flow events. As a secondary related comment, I strongly suspect that 10 or even 32 years*

(Table 2) will be too short to establish a reasonable baseline for such systems.
Since the method uses percentiles derived from the time series of runoff, drought events are determined in a relative way from these values. There is no difference in the method when applied to monsoonal rivers or rivers that have much longer dry periods. In both river types, the method will work the same, only the dry periods that are longer than in normal situations will be considered as a drought. Considering the comment on the length of the time series, we agree with the referee that longer time series are always better for identification of drought events. Unfortunately, these longer time series are not always available. The 10 year period was used for the Irrawaddy river, which has a very regular flow regime. This indicates that the time series is representative for this particular river. In this paper, we have only used the observed discharge series to illustrate the drought identification method and the length of the time series is less important for that purpose (see Introduction, above). In the revised manuscript we will make a remark on the length of the time series.
Line 25 page 2038: Although for drought analysis long time series are needed, in this paper some shorter discharge series, which were considered to be representative for the different climates, were used because these are only meant for illustration.

3. *The index proposed here seems to be intended to be used in combination with spatial hydrological models. I am not familiar with the models used here, but am familiar with the generally poor hydrological performance of land surface schemes in arid regions, particularly in predicting no flow conditions (in the absence of river evaporative losses they tend to predict continuing infinitesimally small flows, for example). This may be attributable to the representation (or lack thereof) of the large river losses that typically occur in arid regions. (As an aside, it is also not always straightforward to identify 'no flow' conditions in measured streamflow*

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records). This leads me to doubt that there is value in attempting to provide hydrological drought information in arid regions on the basis of such models. Some evidence that the model(s) used have skill in reproducing observed no-flow durations may alleviate this concern.

The referee expresses concerns about the use of land surface models in drought analysis with the proposed method. We are aware of the fact that these models have their limitations in capturing all relevant hydrological processes. By using a threshold for the minimum flow, the infinitesimally small flows (and unrealistic flows) in the models were set to zero, we have used a threshold of around 0.08 mm/day.

The aim of this manuscript is to describe and present the combined method for drought analysis (see Introduction, above). The time series from the land surface models are only used as an illustration of the method. The purpose of the newly-developed drought metric is to explore in a next phase if these models capture large-scale drought. For large scale hydrological analyses, output data of these models are being used extensively (e.g. Andreadis et al., 2005; Dirmeyer et al., 2006; Sheffield et al., 2009; Corzo Perez et al., 2011; Haddeland et al., 2011; Prudhomme et al., 2011; Stahl et al., 2011, 2012). We will expand the part about LSMs in the Discussion with some of these concerns.

We support the referee's point that reliable measurements of low river flows are not straightforward either (e.g. Rees et al., 2004), which is very relevant to consider for model validation, and will add a remark about this in the Discussion.

Line 15 page 2050: Large-scale hydrological analyses extensively use output of large-scale models (e.g. Andreadis et al., 2005; Dirmeyer et al., 2006; Sheffield et al., 2009; Corzo Perez et al., 2011; Haddeland et al., 2011; Prudhomme et al., 2011; Stahl et al., 2011, 2012). As large-scale models have difficulties capturing all hydrological processes, infinitesimal runoff values that may occur in model results, can be set to zero using a minimum threshold, depending on the purpose of the study. Since in this study, model

results are only used as illustration of the method, they have not been validated against observations has been done. Further drought analyses with large-scale models might require additional validation, for which limitations in measuring very low flows (e.g. Rees et al., 2004) should be taken into account.
To accommodate the concerns of the Referee about the ability of the models to reproduce no-flow situations we have included an example of a time series from the multi-model ensemble median compared to observed discharge (Fig. 1). The runoff is taken from the grid cell in which the gauging station of the Ashburton river is located. This time series from the ensemble median shows that the models can reproduce no-flows periods and that the variability of the observed discharge is in this case comparable to the variability in runoff. It is impossible to directly compare the values of these time series, since runoff from a grid cell is clearly a different hydrological variable than discharge (e.g. Stahl et al., 2012). However, regimes are similar.

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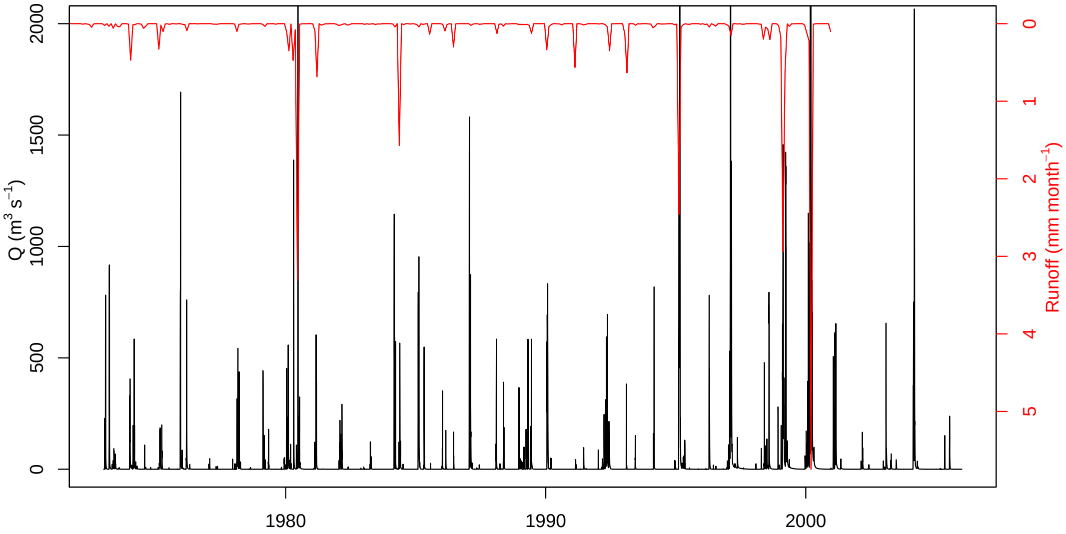

Fig. 1. Example of time series from the ensemble median and observed discharge for the Ashburton River.