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Hydrology and Earth System Sciences Discussions

Interactive comment on "Evaluation of drought propagation in an ensemble mean of large-scale hydrological models" *by* A. F. Van Loon et al.

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We want to thank the anonymous referee for his/her positive response to our manuscript and his/her valuable comments. Please find below our response to the suggestions.

1 Suggestions

1.1 Point 1

Thank you for your compliment.

1.2 Point 2

We selected meteorological and hydrological droughts with the variable threshold level method (e.g. Hisdal *et al.*, 2004). This method involves three steps, i.e.:

1. the construction of a duration curve based on monthly values of the hydrometeorological variable (i.e. precipitation, soil moisture, groundwater storage, subsurface runoff, total runoff),
2. the selection of the 80th percentile of this curve for each month as variable threshold of that variable, and
3. the calculation of the drought characteristics, duration and severity, by comparing the time series of the hydrometeorological variable with the variable threshold.

The difference with the standardized precipitation index (SPI) and standardized runoff index (SRI) is that SPI and SRI fit a statistical distribution through the duration curves calculated in step 1 before selecting a percentile from the curve (Lloyd-Hughes & Saunders, 2002; Shukla & Wood, 2008). The difference between the 80th percentile taken from the raw duration curves (used by the threshold level method) and the 80th percentile taken from the fitted curves (used by SPI and SRI) is expected to be limited because of two reasons. First, we used a very long data set to create the duration curves, namely 38 years, leading to many data points in the curve and probably little

HESSD
9, C3940–C3950, 2012

Interactive Comment

difference between fitted and raw curves. Second, we did not choose an extreme percentile. In the tails, fitted curves deviate more from the raw curves, but for the 80th percentile differences are expected to be small. Additionally, when using SPI and SRI a priori knowledge is needed on the most appropriate statistical function to fit the data, leading to additional subjective choices, which can be avoided when using the variable threshold level method. This is explained earlier by Van Huijgevoort *et al.* (2012). They state that in case long time series are available, calculating percentiles from the raw duration curves is expected to lead to more robust results than using the fitted curves. The research of Wanders *et al.* (2010) confirms this statement. They found comparable meteorological drought characteristics using SPI and the variable threshold level method in different climate zones around the world (5–8% difference in number of droughts and 3–4% difference in duration of droughts).

There are many other ways to calculate droughts using a kind of threshold approach, e.g. Regional Deficiency Index (RDI; Stahl, 2001; Hannaford *et al.*, 2011), fixed threshold level method (Hisdal *et al.*, 2004), Cumulative Precipitation Anomaly (CPA), Soil Moisture Deficit Index (SMDI) (e.g. Wanders *et al.*, 2010). These approaches do not differ significantly from each other, but the numbers for the drought characteristics for a specific hydrometeorological variable will differ in some cases. For example, Peters *et al.* (2006) and Tallaksen *et al.* (2009) use a fixed threshold in the Pang catchment (UK) instead of a variable threshold. They came to similar conclusions on propagation (e.g. lag, lengthening) as in the current study.

Regarding our specific research, we expect that using a different method for calculating drought characteristics would slightly change the exact numbers in Table 4 and 5, but general conclusions regarding drought propagation would not change. To assure this, we stayed close to the original time series of the hydrometeorological variables and focused on the longer and more severe droughts, so the impact of the choice of method used would be negligible.

We agree with the reviewer that it is good to mention this reasoning in our manuscript.

1.3 Point 3

We agree with the reviewer that the role of baseflow is very important in hydrological drought development. In our research, baseflow is studied using the variable Qsub, subsurface discharge. We will mention this in our manuscript.

1.4 Point 4

The large-scale models used in this research have not been calibrated, because they run on global scale (Haddeland *et al.*, 2011). Only WaterGAP, has applied a correction factor on cell runoff to match observed river discharge in a few large basins, and evapotranspiration is adjusted accordingly (e.g. Alcamo *et al.*, 2003; Hunger & Döll, 2008). Hence, calibration statistics can not be provided.

Validation of the individual models against observations for low flows has previously been done by Gudmundsson *et al.* (2012) and Stahl *et al.* (2012) for Europe. They found that the ensemble mean is by far the best to reproduce observed low streamflow, as was also found previously by various other studies for average and high flow conditions (see references in manuscript). In this research, we do not want to repeat those exercises of comparing individual models to observations. We want to take a step further, by going from studying only streamflow to studying the propagation of drought through the hydrological cycle and from general low flow statistics to characteristics of individual drought events and their temporal distribution.

We agree with the reviewer that if there would be one model that performs best globally, in all climate zones and in both fast and slowly responding catchments, we should certainly use only that model. The experience so far is, however, that the overall best model does not exist (e.g. Haddeland *et al.*, 2011; Stahl *et al.*, 2012). Some models are, for example, very good in modelling temperate regions, but bad in cold climates, others are good in cold climates, but very bad in tropical regions. If we would be studying (a

HESSD
9, C3940–C3950, 2012

number of) individual catchments or regions, we could chose the best model for (each of) the catchment(s) or region(s). However, large-scale models are mainly used at global scale, where such a choice can not be made, and this study aims to test these large-scale applications.

1.5 Point 5

In Europe (e.g. EU, 2007), a distinct difference is made between low groundwater levels and low streamflow due to man-made activities (called water scarcity) and climate variability (drought). In this study we follow this concept. Evaluating drought propagation in catchments affected by man-made activities can be done by using an observation-modelling framework. We describe this approach in a paper, which is now under review at Water Resources Research (Van Loon & Van Lanen, under review). In short, this approach is based on the naturalisation of disturbed time series (using a hydrological model) and comparison of the naturalised with the original (disturbed) time series, both on the raw time series and on drought events and propagation. In the current manuscript, we focus mainly on natural headwater catchments that are not influenced by anthropogenic effects. Exception is the Upper-Guadiana catchment. It was hard to find an undisturbed groundwater-dominated catchment in a semi-arid climate. For the Upper-Guadiana catchment, we could therefore only compare our results with knowledge obtained from observations before major man-made activities and from the naturalised situation modelled with a catchment-scale rainfall-runoff model.

We will add a few lines on this issue to the manuscript.

1.6 Point 6

We will provide a flow chart to demonstrate propagation of drought and refer to similar illustrations (e.g. Changnon Jr, 1987; Tallaksen & Van Lanen, 2004; Sheffield & Wood,

HESSD
9, C3940–C3950, 2012

2011).

1.7 Point 7

Thank you for your suggestion. We will revise the discussion section and make clear where we discuss our own findings and include references only at appropriate places.

1.8 Point 8

Indeed, the range of individual models varies drastically, especially for subsurface runoff (baseflow). Large-scale models are used more-and-more in hydrology and drought research, mostly for applications on global scale (e.g. Andreadis *et al.*, 2005; Sheffield & Wood, 2007, 2011; Dai, 2011; Corzo Perez *et al.*, 2011) for which the use of only observations or catchment-scale models is impossible. Therefore, assessments like ours are needed to pinpoint the strengths and weaknesses of the large-scale models. We used the ensemble mean, instead of the model for which simulations were best, because of the reasons mentioned at Point 4, i.e. there is no overall "best" model, models are used on global scale and should be tested for their merits on global scale, and the ensemble mean of a number of models performs better than individual models, both on general hydrological processes and on drought.

1.9 Point 9

When considering the types of drought as distinguished by Wilhite & Glantz (1985), Tallaksen & Van Lanen (2004), and Mishra & Singh (2010), we can conclude from our research that simulation of hydrological droughts is far more uncertain than simulation of meteorological and soil moisture droughts. This is related to uncertainty in the simulation of drought propagation, e.g. inadequate simulation of storage in large-scale models.

When considering the hydrological drought typology of Van Loon & Van Lanen (2012), we can conclude that the simulation of *classical rainfall deficit drought* is most certain (because it is mostly determined by precipitation control), snow-related types and *composite drought* are most uncertain (because mostly determined by temperature control and catchment control, respectively). This is related to the inadequate simulation of snow processes (and again storage) in large-scale models.

This is mentioned in the paper, but we will try to explain it better.

1.10 Point 10

The use of average catchment precipitation instead of grid cell precipitation will not lead to different results in the drought analysis. There are two reasons for that. First, the differences between observed catchment precipitation and grid cell precipitation for the studied case study areas were small, as is demonstrated by Van Huijgevoort *et al.* (2010, 2011). Second, meteorological droughts have a large spatial extent and frequently cover a large region, as is demonstrated by Peters *et al.* (2006) and Tallaksen *et al.* (2009), so there is little chance of missing a meteorological drought event by using a slightly different spatial coverage.

The use of simulated streamflow at the outlet gauging station instead of grid cell runoff has been tested for the Upper-Guadiana case study area. Upper-Guadiana is the only studied area that is large enough to encompass more than one grid cell. When studying simulated routed streamflow instead of grid cell runoff in the Upper-Guadiana, we found that the lag between meteorological drought and hydrological drought increased slightly, but that the shape of the time series did not change at all. Our results regarding the lack of attenuation and multi-year droughts are also valid when using streamflow at the outlet gauging station. This is mentioned in the manuscript, but based on the

HESSD

9, C3940–C3950, 2012

Interactive Comment

comments of the reviewer we will extend the explanation.

A spatial cross-correlation of droughts in different grid cells can not be performed because we studied only one grid cell in most case study areas. A cross-correlation analysis between different types of drought (meteorological and hydrological) in these case study areas has been undertaken previously, but has proven to be very difficult. To our knowledge, the best effort is elaborated in the recent paper of Wong *et al.* (2012). They provide an up-to-date literature review about statistical interrelations and found that copulas have some potential to link a hydrological drought to preceding meteorological drought(s). We do not plan to perform a similar analysis in this manuscript.

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HESSD

9, C3940–C3950, 2012

Interactive Comment

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HESSD
9, C3940–C3950, 2012

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