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Hydrology and Earth System Sciences Discussions

Interactive comment on "Conditioning rainfall-runoff model parameters for ungauged catchments and land management impacts analysis" *by* N. Bulygina et al.

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General comments: Conditioning of parameter space by suitable and easily available data sets derivable from other sources than discharge data is a very important task within the PUB framework. This paper uses Baseflow index from HOST classes to condition parameters of a simple (1+5 parameter) hydrological model. In order to map changes in land-use (afforestation) and land-management (soil degradation through over-grazing) additional conditions are introduced. Also the idea to add a variance term to the Nash-Sutcliffe coefficient is innovative.

We thank the referee for reviewing our paper, and acknowledging the considered task

importance and the proposed coefficient innovation. The paper has been significantly revised to include a new period of simulation, and a new extended discussion, plus various minor improvements detailed below.

In Detail: It becomes not entirely clear how the simulated BFI is achieved: On page 1917:15 you write “the model is run to estimate corresponding BFI for. . ..” What is the model input? I think, to calculate the BFI properly you need at least one year of simulation. . ..! Please state more precisely!

The following was added: ’...the model is run for 2 years (2006-2007 WY) to estimate corresponding BFI for each parameter set’.

Unfortunately, the model validation has been done based on a very short time series of only 3 months (January to March). Why did you not extend this to a longer period of (at least) one year? The conditioning on BFI should be validated for both, the wetter and the dryer periods of the year!

The reason for 3 month performance estimation period is twofold. The first one is the good quality data availability – as it is stated in the paper: ’The period used for the modelling demonstration is 1st January 2007 to 1st April 2007, where the best quality and the most complete data exist. ’ And the second reason is that PDM conceptual model is not designed to predict dry period behaviour, because ET and excess precipitation are represented in a very simple way. Despite these restrictions the model was run for the summer period (July-August, 2007) – see new Figure 7. And the following was added to the paper: ” Predictions for a summer period (end of June 2007 to beginning of August 2007) are shown in Figure 7. While the good performance is maintained in general, the model significantly over-estimates stormflow following the rainfall events during the relatively dry periods in the middle of July. Difficulty in simulating wetting-up periods is typical of this PDM model and similar conceptual models. In particular, the simple evapotranspiration calculation and the inability of the model to maintain percolation while turning off stormflow generation (i.e. the assumption of constant ïĄą) are

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thought to be the main causes of this error. Both Figure 6 and Figure 7 show that the model is least successful at Gauge 9 in terms of explaining the base flow observations. This may be because the influence of the lake in sustaining low flows is more complex than represented in model; or may be due to low flow gauging errors."

Page 1914:20: You define NS statistic, but you name it only NS. Use NSstat or similar instead.

The probabilistic version of Nash-Sutcliffe statistics was named NSprob.

Page 1915:11: What does NSdef mean? Please explain.

The reference to NSdef was deleted and the following text was put instead: 'the traditional and proposed above Nash-Sutcliffe efficiency coefficients.'

Page 1918:10-13: Drop this sentence.

The sentence was dropped.

Page 1919:24: You write "particulary, perhaps. . .", please drop one.

We think, those two words together express better what we what want to say.

Page 1919:25: please explain kf ("fast flow residence time")

The explanation was added: 'seeking some more information about the model parameter values particularly, perhaps, about the fast flow residence time inverse kf'.

Page 1920:3: Why you expect changes in peak flow arrival time?

The peak flow arrival time might change due to changes in the slow flow residence time and fast-slow flow effective precipitation partitioning coefficient, as well as due to changes in the interception loss.

While celerity is fixed, your model may not be able to predict this?

Thank you for raising this point. We assume that the channel celerity parameter won't

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change significantly under different land use, but we didn't put it into the paper. The following was added to the case study description section: 'In this work, we keep the celerity parameter unchanged for both current day and land use change scenarios, assuming that stream network hydraulics won't differ significantly under the scenarios'. The issue of estimating parameters of a channel hydraulic model is now covered more fully in the discussion.

While reduction of input (through higher interception storage and higher transpiration rate) explains the slightly slower response for the afforestation scenario, in the case of soil degradation conditioning seems not to select those parameters reflecting faster runoff response (even so we would expect flashier and faster runoff response)?

Soil type –land use combinations for Pontbren catchment are not distinguished by peak arrival times on the modelling time scale (15 minutes) for current day and soil degradation scenarios. This is noted in the paper.

Page 1931/1934: Fig 1. / Fig. 4 should be combined

Figure 1 shows model changes due to interception loss changes. This may be added to any conceptual model. Meanwhile, Figure 4 shows the conceptual model (PDM) used in the case study – one from many other possible models. Following this logic we prefer to present the figures separately.

Page 1925: Table 1: What is the source of your data (reference)?

There is a reference: 'The second land management change considered is increasing stocking density, leading to soil structural degradation. Following the approach of Hollis [Packman et al., 2004], degraded soil is assigned an appropriate analogue HOST class to represent the change (Table 1)' in the Method section. The reference was added to the Table 1 heading.

How the analogue classes are created? E.g. class 18: the switch to class 20 leads to higher variance (0.207) but the same BFI (0.52).

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The approach is empirical, and is based on the following (as written in the text): 'The rationale for the proposed changes is that soil structural degradation, in the form of topsoil and upper subsoil compaction and seasonal 'capping' and sealing of soil surfaces, causes a reduction in the effective soil storage, which in turn results in increased surface runoff. Therefore, the general principle is that soil structural degradation affects the soil storage/wetness component of the HOST classification, but does not alter the hydro-geological component'. We realize the method limitation, and we say about it in the conclusions: 'The approach for land use change analysis is largely theoretical (i.e. HOST class change and interception loss modelling) and built mainly on literature review, and therefore requires testing on experimental data. This may be done using paired catchment or manipulation plot experimental data [Brown et al., 2005; Marshall et al. 2009], and will be one of our future work directions'. The BFI variance for the analogue classes is partially reduced due to the requirement that the degraded condition BFI is no lower than the original (not degraded) BFI.

Page 1936: Fig. 6: very small, should be enlarged, change unit for flow (SI unit is m3/s, not "cumecs")

The figure was enlarged, and the unit notation was changed.

Page 1938: Fig. 8: the quality should be better (higher resolution), change unit for flow (not "cumecs")

The quality was improved, and the unit notation was changed.

Conclusion: The paper is well written. The argumentation is clearly structured. The figures are well selected. The very short validation period makes the final results very uncertain. It must be clarified what data the simulated BFI is based on. The paper needs minor revision.

The summer prediction evaluation was added (Figure 7), the simulated BFI origins are clarified.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1907, 2009.