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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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The paper presents a methodology to estimate parameters of a rainfall runoff model by conditioning them using prior information on the baseflow index for different soil classifications. The authors use a Bayesian approach to constrain the model parameters. The paper is well written and generally I find their approach interesting. I have some comments to make which I believe the authors need to address.

We thank the reviewer for the comments and suggestions. The paper has been significantly revised to include a new period of simulation, and a new extended discussion, plus various minor improvements.

Detailed comments 1) My major concern about the implemented methodology is with the approach the authors follow to quantify the impact of afforestaion. The only information used to estimate the posterior distribution of the parameters is that afforestation leads to an increase in baseflow and the likelihood function they implemented is a binary function which retains from the prior parameter distribution all those parameter sets that lead to an increase in the base flow index. The resulting posterior distribution is therefore an arbitrary distribution that is not conditioned with any quantitative information about the extent of reduction in the base flow index. Making inference based on such a distribution does not have a sound basis and I feel that the authors should be circumspect about their analysis and conclusions.

First of all, currently we can't in general quantify the base flow change under forest – the only consistent information that we found in the literature (Wheater et al., 2008; Brown et al., 2005) is that the BFI increases after afforestation. We think that this is a limited but valuable piece of information that might constraint the parameter space. We argue that the resulting distribution (as discribed in the paper) is not an arbitrary one and differs from the prior distribution (i.e. the one without BFI increase condition). To illustrate the point we propose to consider the example in the supplement.

We are circumspect about the results, noting that more information needs to be developed and included to reduce uncertainty and to extend to various practical applications.

The issue of development of the method for wider applicability is now covered more extensively in the discussion section.

2) The authors have indicated that some of the parameters are not identifiable using the prior information they used. They have stated in their conclusions that the model is not sensitive to these parameters over their particular case study period while admitting that it could become sensitive for other periods. Wouldn't this limit the applicability of the model for prediction?

It should not limit the model applicability for the predictions, since if a model is more sensitive to some parameters that are not sufficiently restricted, then the prediction bounds are only to expand.

The issue of development of the method for reduced uncertainty/wider applicability is now covered extensively in the discussion section.

Why not try to estimate these parameters using additional prior information and validate the model over a period that covers all possible spectrum of the flow regime in the catchment?

It is a good idea to introduce other sources of information, as we recommend in the paper, but this would considerably complicate the theoretical basis..

Also, defining suitable sources of information is a challenge, as previous regionalisation studies have been with daily or monthly data, and one or two with hourly data, whereas at this small catchment scale, 15-minute data are appropriate. And we would prefer avoiding a direct connection between physical characteristics and conceptual model parameters (where much of the existing information lies) for the theoretical reasons covered in the paper. Nevertheless, we are currently working on this significant problem.

The issues of introducing additional prior information are now discussed in more detail in the discussion section.

The PDM model has a weakness by itself when applied to dry periods, since it has overly simple ET calculation, and assumes that the effective precipitation separation between slow and fast stores ratio is fixed (that might not work for dry times). For the completeness, we set up the model for the summer, 2007, and the results are shown in new Figure 7.

3) Page 1915, line 11: What is Ndef? Does it stand for the conventional Nash Sutcliffe measure?

Thank you for pointing this out. Yes, it does mean the traditional NS coefficient, but

since the notation is not used afterwards, we changed the sentence to '...and the predictions are evaluated using the traditional and proposed Nash-Sutcliffe efficiency coefficients'.

Please also note the Supplement to this comment.

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