Hydrol. Earth Syst. Sci. Discuss., 12, C6697–C6699, 2016
www.hydrol-earth-syst-sci-discuss.net/12/C6697/2016/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Mapping dominant runoff processes: an evaluation of different approaches using similarity measures and synthetic runoff simulations" *by* M. Antonetti et al.

S. Gharari (Referee)

shervangharari@yahoo.com

Received and published: 11 February 2016

The papers tries to evaluate different existing models for dominant runoff process (DRP) and compare it with an expert driven map which have been already existed. This DRPs then are used for comparison in a synthetic case study. I believe the study is important and relevant and deserved to be published.

Let me start from the part which I don't really understand. The synthetic case study itself. To my point of view the design of synthetic case study in many cases, which this case is one of them, is not that easy. The synthetic case study means that you have a known solution for which you seek to achieve via a model or strategy and it is

a isolated test which cannot say much about the real system behavior. The question is that in this specific case how the authors are making sure that their synthetic data is truly reproducible by the three different models (this is the fairness of the comparison) and not biased to one of the existing models. I guess this is almost impossible to show, because we don't know what the true mapping is. Another point, the perception of DRP are also time scale dependent, meaning that if you generate a longer synthetic case study bringing in the evaporation, transpiration and other processes which happen in a longer period of time you may end up having different conclusion.

For me what is more interesting than the synthetic case study is to see how our available expert knowledge is transferable to different DRPs established by different automated mapping models and how it affects the final outcome, in this case discharge, given the real data. I elaborate my point; imagine that using GH11 the authors can make the model which contains three DRPs and as they did they find a way to introduce the expert knowledge to the model by setting some constraints, the same can be done for the other models using similar strategy but of course different set of constraints as they are more detailed (based on land use and geology). These models should provide ensembles which then can be used to compare the different models. Anyway we cannot find which mapping model is more realistic based on expert knowledge but what we can do is to see how those mapping together with our expert knowledge in the model can reproduce the output which then can be used as a proxy of how close we are to the output and how sufficient is the mapping complexity to hold all of our knowledge about the processes. This way the different models can be compared. Having said that I do not agree with statement as such: "We therefore recommend not only using expert knowledge for model building and constraining but also trying to obtain spatially distributed landscape classifications that are as realistic as possible." in my point of view we can never have the confidence to say which one is more realistic that the other one as long as our understanding is biased. However we can say which mapping is sufficient for the specific purpose. That is the entire point I wanted to hint at.

“Once a model structure and its parameters have been identified for each landscape in a gauged catchment, they can be transferred to an ungauged catchment where the landscapes have similar hydrological behaviour.” not that accurate statement in my point of view. Still some caution is needed.

“As the results of this study suggest, the use of expert knowledge should not be limited only to the phases of landscape classification or model building and constraining, but should play a crucial role in each phase of the whole modeling process.” please clarify this. What do the authors mean by this sentence? Can you make such a conclusion from your study?

“However, the adaptation of these classifications to the characteristics of our study sites was beyond the scope of this study” but the authors already did? Right?

I would like to see figure 12 with its distribution for GH11 simulations. Are most of the simulations closer to upper or lower limit?

Overall the paper is interesting and I believe it can be published after major revision specially for the set up of the synthetic case study and its related conclusions.

With kind regards

Shervan Gharari

PS. I should thank Dr. M. Shafiei who helped in reviewing this paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 13257, 2015.