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Geoscientific Model Development Discussions

Interactive comment on "A Hydrological Emulator for Global Applications" *by* Yaling Liu et al.

Anonymous Referee #1

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The manuscript by Liu et al. addresses the interesting issue of model complexity needed for global hydrological simulations. They present a new simulation tool based on the existing *abcd* model, and show that their simulations show a fair performance when compared with simulations from the VIC model. While I am generally supportive of work aimed at finding optimum model complexities, I feel the current study will need additional work to further show and quantify the benefits of the current code. At the moment, the main message seems to be that a low-dimensional model can produce positive correlations at the monthly timescale with another model, and that the runtime of the simple model is shorter. Both findings are not particularly new, and, in my view, they are not enough to merit publication. The suggested benefits of a simpler model (the possibility of focussing on uncertainty and spatial heterogeneity) might be true, but none of this is actually shown in the paper and no model or code is presented that takes full advantage of these suggested benefits. I believe the authors should present

more work in this direction before the manuscript can be accepted for publication in GMD.

My main concerns are the following:

- The motivation for choosing the *abcd* model is poor. Many simple models exist, and no objective criteria were used to select this particular model. The authors could have started with a simpler version, and adding components/complexity until a pre-defined threshold performance was reached. This would have made the selection less arbitrary. How does the modelled runoff for instance compare to a baseline "model" which is simply the monthly P−PET? The choice for the *abcd* model should be motivated better, but preferably a more systematic approach should be taken.
- The notion that simple models can do a good job in describing the output of more complex models is not new. In particular, Gab Abramovic has written numerous papers on this topic. This work should be considered and used in the interpretation/motivation.
- The motivation for the study is weak. In the current work, the authors only show a single application of their model (at grid and basin scales) and argue this is a good alternative to more complex models. But why not use the output of these complex models directly if the main goal is a best assessment of monthly average predictions of water balance partitioning? Such (multi-model) output is readily available at the global scale and does not require the running of even a simple model. Of course a simple model can be used for sophisticated uncertainty assessment (important advantage), but the authors did not yet do any work in this direction. This should be part of a revised version.
- The choice for the VIC model is poorly motivated. While I agree that some studies have shown that VIC produces positive NSE scores against observations,

many of these studies evaluated their results at very course time resolutions at which nearly any model would show a good performance (in particular because at monthly timescales the seasonal cycle dominates, which is easy to reproduce). The VIC model will generally not work well when evaluated at hourly or daily timesteps, even when calibrated. Related to this point is the issue of temporal resolution. It can be questioned whether nonlinear processes such as snow accumulation and melt can be modelled at a monthly timestep and at course spatial scales (see Melsen et al., *Hydrol. Earth Syst. Sci.* doi:10.5194/hess-20-1069-2016). In order to show that this is indeed possible, the authors should show that their model is able to outperform a baseline model consisting of, for instance, a mean seasonal cycle (as in Schaefli & Gupta, *Hydrol. Process.* 21, 2075–2080).

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