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Discussions

# *Interactive comment on* "Toward seamless hydrologic predictions across scales" *by* Luis Samaniego et al.

**Anonymous Referee #1**

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Samaniego et al. propose MPR to be a practical and robust method that provides consistent (seamless) parameter and flux fields across scales owing to the inconsistent and unrealistic parameter fields for land surface geophysical properties in many existing land surface and large-scale hydrological models. Although this study is properly motivated, I am having a hard time to understand what are the new advances from this manuscript comparing to Samaniego et al., WRR 2011 and Mizukami et al., 2017, particularly given that Mizukami et al. is submitted to WRR and perhaps under review.

Mizukami, N., Clark, M., Newman, A., Wood, A., Gutmann, E., Nijssen, B., Samaniego, L., and Rakovec, O.: Towards seamless large domain parameter estimation for hydrologic models, Water Resources Research, submitted., 2017

Another reason for my trouble of identifying new advances may be that lots of previous



concepts and methods (REA, REW, HRU etc.) are touched but in a rather scattered manner, i.e., without a coherent synthesis, thus making it difficult to follow the authors' logic chain to lead to the new contributions from this study. By briefly glancing through Samaniego et al., WRR 2011 I was guessing that perhaps in this study the major contribution is to introduce MPR as a robust parameter estimation approach for land surface and/or large-scale hydrological models, which in my mind are not really the same as those watershed-scale or highly-distributed hydrological models. For example, the application of MPR to PCR-GLOBWB has been largely illustrated in this manuscript. However, I am then confused again realizing there is another manuscript (Mizukami et al.) where MPR has also been applied to PCR-GLOBWB.

I therefore strongly encourage the authors clearly articulate the major advancements in this study. That said, I have a few specific comments as below.

1. L2, Page 2. "must made" –> "must be made" 2. L6, P10. It is not a good practice to jump from Fig. 2 to Fig. 7 (whilst Fig. 3-6 not introduced yet) 3. L6-8, P13. I don't think the argument so far can support this conclusion. Given the numerous processes controling the propagation from soil porosity to evapotranspiration and the fact these processes are very often presented & parameterized in different models with varying levels of complexity (i.e., model structure uncertainty), I could not really make sense out of this conclusion from my own experience (in both watershed modeling and land surface modeling) either. 4. L9-11, P13. As a modeler I could not agree with this conclusion either. A good parameter estimation method should never alter the true value of a parameter with very clear physical meaning, such as soil porosityA parameter, no matter at what resolution(s). Rather, the so-called predictive uncertainties mentioned here should be used a signature to diagnose whether the model itself is sufficiently robust, not the other way around. Otherwise, we are playing with the parameters to get the right anwer for the wrong reasons. 5. L26-27, P15. Why is this well-accepted fact (among modelers at least) being used as a hypothesis? 6. L10-11, P16. Don't follow the logic. According to L6-7, the majority-based approach in Noah-MP is giving 2.3%

HIGHER mean porosity than MPR. Why now the porosity field estimated by Noah-MP tends to have lower water holding capacity calues? 7. L19-21, P16. Does not read well. How could "dynamic(s)" be enhanced or constained? 8. L3-4, P17. Not so apparent to me. It appears to me PCR-GLOBWB does not perform bad either. But this may be due to the difficulty to link the flux-matching test with the spatial patterns here.

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