

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

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### **Response to Referee #1**

1. *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*

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*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 hydro-logic models, Water Resources Research, submitted., 2017*

Thank you for the comment. We are sorry for not making clear enough the differences between Mizukami et al. (under review) (hereafter [MCN+2017]) and this manuscript. We will make sure that this is clearly explained in the revised manuscript.

[MCN+2017] is aiming at the development of “a model agnostic MPR system called MPR-flex and then applied MPR-flex to the Variable Infiltration Capacity (VIC) model to produce hydrologic simulations over the contiguous USA (CONUS)”. In [MCN+2017] no attempt has been made to verify the flux-matching condition of ET obtained with VIC using the MPR-flex parameterization across scales.

In this manuscript (hereafter [SKT+2017]) we attempt to describe the progress towards seamless parameterizations in land surface or hydrological models. We present a short description of what has been made (the literature on the topic is extensive) and provide a simple example to visualize how many of the existing models are estimating a fundamental parameter such as soil porosity. We postulate, based on our own experience, a way forward that uses MPR, provide a “Protocol for evaluation of model parameterization” (which is not publish before), implement it to PCR-GLOBWB (also new and unpublished) and carry out a series of experiments (based on the spirit of the E. Wood’s recommendation) to demonstrate how to spot faulty parameterizations (also not publish before). We also compare the effects of the parameterization on three models (mHM, WaterGAP, and PCR-GLOBWB) as part of these experiments (all using the same forcings and underlying data). It should be clearly noted that **any of these ba-**

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**sic components are part of [MCN+2017].** In the revised manuscript, we will make clearer the scope of both papers.

2. *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.*

We will introduce a new table to synthesize the main developments related to concepts and methods (REA, REW, HRU etc.) used to obtain distributed parameter fields.

3. *... 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.*

The hydrological process implemented in a land surface model (LSM) can be similar to those of a hydrological model(HM). We agree that LSMs and HMs are not the same because they aim at different purposes, and that the former ones tend to be much more complex than the later ones. The parameterization of soil parameters (e.g., soil porosity), however, can be based on the same principles of soil physics, and is often found in a large number of LSM/HM as shown in this manuscript.

The reviewer's confusion may have been originated by weak formulations in p.9

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I.2 or in p.18 I.16. We will clarify these sentences in the revised manuscript. MPR has been applied to PCR-GLOBWB **only** in this manuscript up to now. The MPR-flex development presented in [MCN+2017] is applied **only** to VIC up to now.

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

Thank you for the recommendations. We will clarify them in the revised manuscript.

5. L2, Page 2. "must made" - "must be made"

Done

6. L6, P10. It is not a good practice to jump from Fig. 2 to Fig. 7 (whilst Fig. 3-6 not introduced yet)

Will be amended in the revised manuscript.

7. L6-8, P13. I don't think the argument so far can support this conclusion. Given the numerous processes controlling 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.

We are not claiming that MMS is better or worse than MPR. We are only comparing the values obtained by MMS w.r.t. those estimated by MPR and estimate the differences. For sure we do not now at this scale which values are more close to reality, the only fact we know is that the MPR estimates used in two HMs are good

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enough to close the water balance in relatively well in over 300 basins over Pan-EU as shown in Rakovec et al. 2016. <http://doi.org/10.1175/jhm-d-15-0054.1>. We will clarify this formulation in the revised manuscript.

8. 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 porosity. A 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 answer for the wrong reasons.

We do not agree with the reviewer in this point. An effective parameter at 5 km resolution or coarser is an “effective” parameter representing the heterogeneity of the underlying land surface and hence cannot be observed or measured directly but can only be estimated. Because of this fact, the effective values of porosity cannot compared directly with field samples. Binley, A., Elgy, J., & Beven, K. (1989). [doi:10.1029/2008WR007695](https://doi.org/10.1029/2008WR007695) and many other publications from Beven and Blöschl, Wood etc. make this fact very clear. If a model is applied at point scale (at most meters) then a parameter estimation method would lead to parameter values that can be obtained in laboratory.

9. L26-27, P15. Why is this well-accepted fact (among modelers at least) being used as a hypothesis?

This “obvious fact” is used as a hypothesis to demonstrate that the Noah-MP model using the same soil map as mHM can lead to very different latent heat estimates although the forcings of both models are similar. We will reformulate the sentence and improve the text in the revised manuscript.

10. 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

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now the porosity field estimated by Noah-MP tends to have lower water holding capacity values?

Thank for pointing out this inconsistency. L6-7 refers to the mean over whole Pan-EU. L10-11 refers to a analysis in Germany whose results are reported in Fig. 6. We will clarify the text in the revised manuscript.

11. L19-21, P16. Does not read well. How could “dynamic(s)” be enhanced or constrained?

This text will be improved in the revised manuscript. We use the term “enhancing” to indicate changes in long-term mean and variance of soil moisture that occur by reducing/increasing the maximum water holding capacity (porosity times depth). We will make this point clear in the revised manuscript.

12. 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.

If the parameters for both models are estimated based on streamflow only, then the model performance as reported in Table 2 tend to be comparable. ET estimates, however, differ greatly as shown in Fig.7 . In this case, both models in this experiment use collocated grids so that a cell at a coarser scale (30 arc min) have exactly the same number of underlying cells at finer resolutions (5 arc min), everywhere and for all models. Consequently, flux matching of ET made on two different resolutions is not a problem, and what is reported here is not an artifact of matching “spatial patterns”.