Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-89-AC4, 2017

Hydrology and Earth System Sciences
Open Access
Discussions

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

Luis Samaniego et al.

luis.samaniego@ufz.de

Received and published: 1 May 2017

1. *The main points of the paper are: (i) state-of-the-art LSMs and HMs do not have consistent and realistic parameter fields for land surface geophysical properties, and as a result do not satisfy a flux- matching condition (ii) the MPR technique can be used as a generic parameter estimation technique to greatly reduce these limitations (iii) a specific case of this improvement is demonstrated using the PCR-GLOBWB model. In my view the innovation is in the recognition of the problem across multiple models, the wider breadth of application of MPR, and the protocol needed to achieve this. To some extent the purpose of the manuscript is to demonstrate the very significant consequences of different parameter estimation approaches in large-scale LSMs/HMs, and to show the advantages of using MPR. In my view this is a relevant objective for scientific publishing, in relation to*

relatively new techniques such as MPR, because such examples provide specific examples to which the hydrological modelling community can more easily relate (as opposed to reading about the MPR technique in the abstract, or in relation to its application to a specific model). The main uncertainty for me is the extent to which this material is also contained in the submitted manuscript by Mizukami et al, as that manuscript is cited in relation to many of the main points made here. I leave this point for the Editor to consider.

Thank you for the valuable comments and recommendations.

We described in detail the extend of Mizukami et al. (under review) (hereafter [MCN+2017]) and this manuscript in the Response to Referee #1 and Referee #2.

[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:

(a) Attempt to describe the progress towards seamless parameterizations in land surface(LSM) or hydrological models(HM). We present a short description of what has been made (the literature on the topic is quite extensive) and provide a simple example to visualize how existing LSMs/HMs are estimating a fundamental parameter such as soil porosity (not found in literature),

(b) Propose, based on our own experience, a way forward that uses MPR and systematizes its application by providing a "Protocol for evaluation of model parameterization" (This has not been publish before),

(c) Implement this protocol to PCR-GLOBWB (also new piece of work and unpublished),

(d) 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), and

(e) 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 underlaying data)

It should be clearly noted that **none of these key elements belong to Mizukami et al. (under review) (hereafter [MCN+2017])**.

Specific comments

1. *Title: "Toward seamless hydrologic predictions across scales" This might be interpreted by readers as referring to seamless predictions across temporal scales, i.e. the linking of nowcasting with NWP. Perhaps "Toward seamless hydrologic predictions across spatial scales"?*

   Thank you for the good suggestion. Done.

2. *P2 L2 "trade-offs that must be made to reach a final objective" missing word*

   Done.

3. *P2 L9 "numerical weather prediction, land surface schemes, and hydrologic models" It would help to provide a reference or some text to enable readers to distinguish among these three terms. Many would know two of these terms, but far fewer could reliably distinguish all three.*

   References will be provided in the revised manuscript.

4. *P2 L29 "In this case, one states that a physical process is parameterized." It would be helpful to introduce the concept of sub-grid phenomena here, to distinguish between phenomena which are resolved by a given grid resolution, and those that are parameterised. Otherwise, the concept of parameterisation and references to "the missing (complex) processes" remains rather vague. The missing processes should all be sub-grid – anything else that is missing is simply a missing process.*

   Thank for the recommendation. The concept of sub-grid phenomena that are not modeled will be introduced in the the revised manuscript.

5. *5. P3 L1 "Parameterizations in land surface models have increased in their complexity during the past decades, but the procedures to estimate constants for the parameterizations have not changed much." Has anything changed as grid sizes got smaller? Did any processes become resolved that were formerly parameterized?*

   By comparing versions of land surface models, for example, multi-processes (parameterizations) have been introduced, e.g., in Noah-MP. Phenological processes and radiative transfer schemes have become extremely detailed in the new versions of Noah-MP and other LSMs. Runoff generation mechanisms, on the other hand, have not changed much in most LSMs/HMs. We will make a list of model improvements as grid sizes got smaller in the revised manuscript.

6. *P3 L7 "The reasons for the lack of progress in creating scale-invariant parameterizations are manifold." At this stage you have not established that scale-invariant parameterizations are either desirable or feasible (also relevant to P4 L24). From this point on in the paper it seems that the parameterization problem can be solved by scale-invariant parameterizations, but that there are no other credible paths being explored. I would like to see some mention in the Introduction of non-MPR approaches to parameter estimation which are also taking a serious approach to the problem. Alternative methods are unlikely to satisfy the flux- matching criteria, but they might be partly competitive, e.g. (i) other spatial scaling attributes (e.g. sidestepping the scaling problem by assuming scale-independent distribution functions), (ii) strong links to mapped geophysical attributes (e.g. regularisation), (iii) strong links to observed functional responses of hydrological systems (e.g. Yadav et al (Advances in Water Resources 30 (2007) 1756–1774)).*

   Good point. We will mention these alternative paths ways to be explored and we will discuss their main advantages and disadvantages in the revised manuscript. We consider, however, that it will be out the scope of this manuscript to test them.

7. *P4 L19 "The numerical constants can be specified with a great level of precision, but the physical constants and parameters cannot be because they must be treated as random variables (Nearing et al., 2016)" I don't know the Nearing et al paper in detail, but I am surprised to hear that something termed a "physical constant" really requires treatment as a random variable. Surely if it is well enough defined to earn the moniker physical constant, then it can be determined experimentally to relatively high accuracy for practical purposes? Are the authors suggesting we should treat g as a random variable in hydrology because it is determined by measurement, which is subject to error? On the other hand, I accept that parameters may usefully be described as random variables.*

   Depends on the accuracy and precision with which we know a physical "con-

   stant". Its description can be done by a density function having a know mean and quite small standard deviation. For example, we know the value of the standard acceleration due to gravity with high accuracy (no bias) and precision(very small stdev). In this case and for practical purposes of parameter estimation, we could treat it as a constant. This is not necessarily the case for other physical constants such as the thermal conductivity of a given soil type. In this case with need a transfer-function of infer it based on soil texture fields and other predictors. We will clarify this statements in the revised manuscript.

8. *P4 I would like to see the term "seamless" defined in the introduction (the abstract provides this, but not the introduction), and particularly an argument made for why seamlessness is (in principle and/or in practice) a desirable attribute.*

   Good point. We will define it in the introduction of the revised manuscript to avoid miss interpretations. See definition in the Response to Referee #2, point 10.

9. *P9 The paragraph starting on L3 seems misplaced. The rest of the section is a description of MPR, whereas this paragraph is an assessment against criteria.*

   This paragraph will be relocated or rewritten in the revised manuscript.

10. *P9 L3 "Currently, MPR is the only method that consistently and simultaneously addresses the scale, nonlinearity and over-parameterization issues" If scale, nonlinearity and over- parameterization issues are the key criteria for assessment, then I would expect them to all be mentioned in the introduction; however, only scale really features in the introduction.*

    Good point. These issues were introduced in other publications related to MPR (e.g., Samaniego et al. 2010b). They will be introduced in the introduction of the revised manuscript.

11. *P9 L26 This whole paragraph (slightly rewritten) might sit well in the introduction if there was some material there on regularization procedures.*

We will use it in the introduction of the revised manuscript.

12. *P9 L33 "Consequently, greater care should be taken in their selection." It is unclear what "greater" refers to. Are regularization functions being imposed without care? In which cases?*

    If a regularization function is poorly chosen, or lack important predictors, the resulting parameter value might be badly estimated and its posterior distribution could be poorly estimated. For example, the Cosby et al. 1984 PTF is a very simple one (used in SCA-SMA) that relates porosity to sand content only. The application of this regularization function will under/over predict porosity in soils having low sand and high clay/loam fractions. We will mention this example to make clear our point in the revised manuscript.

13. *P11 L19 "Kling-Gupta efficiency (KGE) of the compromise solution > 0.6" Some justification is needed for any threshold on KGE, as it is much easier to do well in some environments than others.*

    This part of the protocol remains still subjective. It depends of on many factors such as the input forcings and quality of the land-surface properties. It is difficult to give a justification, but we will try to make it more objective recommendation in the revised manuscript.

14. *P12 L3 "minimize the occurrence of discontinuities and ease the transferability of model parameters across scales and locations" These criteria for success should both have been outlined much earlier in the paper, either in the Introduction or at the end of the review.*

    Good point. We will revise the introduction to mention them.

15. *P12 L17 "which constitute the basis for the EDgE project" Needs a reference to the project, or delete if not relevant.*

    We will add the reference to http://edge.climate.copernicus.eu

16. *P18 L28 "MPR ... is feasible to implement in existing LSM/HMs whose goal should be seamless parameter fields across scales." The authors need to add an additional clause to this sentence (based on material from earlier in the paper) so it is clear WHY seamless parameter fields across scales are essential.*

    Good point. We will reformulate this section in the revised manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-89, 2017.