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Hydrology and Earth System Sciences
Discussions

Interactive comment on "PERSiST: the precipitation, evapotranspiration and runoff simulator for solute transport" *by* M. N. Futter et al.

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Dear authors,

in principal I like the idea of developing integrated rainfall-runoff/solute transport models. This topic is quite timely and interesting. However, the three reviewers pointed out a number of critical issues in the manuscript that need to be addressed in detail before this manuscript could be considered for publication.

Complementary and/or in agreement with the reviewers' concerns I would like to point the authors to issues that I see as the most relevant:

(1) It is not clear what the actual working hypothesis of this paper is or what the authors

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are trying to convey. And how does solute transport play a role here as suggested in the title?

(2) In how far is PERSiST new, novel, innovative, different to or better than any other conceptual model approach? In principal, every conceptual model can be (and many actually were) applied in a (semi-)distributed way (e.g. Uhlenbrook et al., 2004; Lindstroem et al., 2010; Hellebrand et al., 2011).

(3) As also pointed out by the reviewers, neither proper credit is given to existing integrated rainfall-runoff/solute transport applications nor are these discussed in context with the presented method. It is important not only to refer to earlier models but also to highlight and discuss in detail the differences (advantages/disadvantages) of these previous models to PERSiST. In particular, the statement on P.8637, l.2-3 ("[...] they have not necessarily been well suited to simulating solute transport") is surprising as many previous models demonstrated good ability in reproducing well streamflow AND solute transport. Therefore, the manuscript needs to be put much more in context of earlier work and I would encourage the authors to consider at least the following references for detailed discussion:

Uhlenbrook and Sieber, 2005; Dunn et al., 2007; Shaw et al., 2008; Fenicia et al., 2010; Birkel et al., 2011; Arheimer et al., 2012; McMillan et al., 2012; Van der Velde et al., 2012; Hrachowitz et al., 2013a; Bertuzzo et al., 2013

(4) In the light of considerable progress over the past decade and as pointed out by the reviewers, the calibration strategy is too simplistic to adequately constrain a model with 108(!!) parameters (see for example the review given by Hrachowitz et al., 2013b). Using a certain calibration algorithm, you will surely enough find some mathematically feasible posterior distribution of feasible parameters. However, the parameterization will be subject to significant equifinalty. How do you make sure that your mathematically feasible paramterizations are also *realistic* parameterizations (cf. Beven, 2006; Gupta et al., 2008, Andreassian et al., 2012; Gharari et al., 2013). In line with the reviewers

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I would therefore also suggest to review your calibration strategy and consider more calibration objectives and/or criteria to constrain your model, thereby increasing the confidence in your models to represent reality in the best possible way.

I would encourage the authors to use the opportunity of the rapid delivery of reviewer comments to actually engage in direct discussion with the reviewers during the remaining Open Discussion Period on HESSD over the issues they raised, potentially clarifying many of the earlier concerns. In any case detailed responses to the reviewer comments need to be submitted before the end of the Discussion Period. Note that a revised version of the manuscript with detailed description of changes should NOT be uploaded at this point, but only after the Editor Decision is available after the end of the Discussion Period.

Looking forward to a fruitful discussion, Best regards, Markus Hrachowitz

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