

# ***Interactive comment on “The evolution of process-based hydrologic models: Historical challenges and the collective quest for physical realism” by Martyn P. Clark et al.***

## **Anonymous Referee #1**

Received and published: 22 February 2017

Manuscript ID: hess-2016-693

The evolution of process-based hydrologic models: Historical challenges and the collective quest for physical realism

Clark et al.

Summary — The manuscript is one in a series of discussion papers by the lead author over the last few years and was prompted by the Symposium in Honor of Eric Wood: Observations and Modeling across Scales held June 2-3, 2016 in Princeton, NJ. The authors use three questions posed by Freeze and Harlan [1969] to examine progress in process-based hydrologic modeling over the last fifty years and to define

[Printer-friendly version](#)

[Discussion paper](#)



outstanding research challenges. The manuscript is generally well-written, but is more narrow in focus than its title and introduction suggest. The specific answers to the three questions are not based on general surveys of the field, but are provided through the lens of the symposium topics. This is probably a good thing, since it constrains the length of the manuscript, but it requires some rewriting or additional wording to clearly define how the questions from Freeze and Harlan are evaluated in the rest of the manuscript.

Comments ——— 1. For example, the first question discusses whether physically-based mathematical descriptions of hydrologic processes are available and whether the relationships between the component phenomena are well enough understood. This question is addressed in section 2 (model structure) by focusing on scaling relationships and the representation of local processes in regional models. While an important aspect of model representation and one that continues to challenge the community, it is not the sole challenge to the question posed by Freeze and Harlan (but an obvious angle given the topic of the symposium). The manuscript would be improved by a better connection between the question from Freeze and Harlan and the discussion of scaling. Explain why scaling is the main topic that is being discussed and how it relates or ranks compared to other aspects of question one. Following that, the discussion of scaling in section 2 can remain largely unchanged.

2. Same for model parameters (section 3). The problem is not only that spatial information is not always of sufficient resolution and quality or that the information does not exist, but also that some parameters are not directly observable at the scale of the application (it's not even always clear that the equations we use at certain spatial scales are necessarily the right ones). The challenge (I.229 and following) is not only to make the best use of the information that we have, but there may also be an opportunity to change our physical descriptions to make better use of the available information at a particular scale. Section 3 focuses mostly on parameter upscaling and does not discuss the use of new data sources, the use of inverse methods, etc. I am not advocating

[Printer-friendly version](#)

[Discussion paper](#)



to discuss all topics because the manuscript will lose focus, but it would be good to motivate better why the authors focus on this particular aspect of question 2 from Freeze and Harlan.

3. The section on model execution (section 4) requires some discussion of the purpose or end goal of our simulations. One could argue that we run at higher resolutions and finer time steps simply because we can and because we lack the scaling relationships that allow us to be more "economical" with computing resources. The statement (l.320-321) that "[...] more complex models may not have as much physical realism as computationally frugal alternatives" raises the question 'why are we doing them?'. And the final paragraph of section 4.2 leaves me again wondering what the end goal is of the model simulations.

4. l.389-l.399: It would be nice to call out some of the specific advances that have been made in response to the questions from Freeze and Harlan.

Minor comments ————— \* l.101-102: "both sets of solutions can occur in the same model". Do you mean for the same process? This is not entirely clear and an example would be helpful.

\* l.103-104: "are readily shared among different modeling groups". I don't quite understand what "shared" means in this context (even given the example).

\* l.129: "summarizes recent in developing". Word missing after "recent"

\* l.141: "Another class of methods is" Suggested change "Another class of methods consists of"

\* l.143: "described earlier" Where, I cannot find the earlier reference.

\* l.157: "However, if I and D are comparable in scale, this becomes problematic." Explain why.

\* l.284: "for upper 50 m". Suggested change "for the upper 50 m"

\* I.289: "on the other hand". Missing a "on the one hand"

\* I.291: "guide the interaction" should read "guide to the interaction"

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-693, 2017.

[Printer-friendly version](#)

[Discussion paper](#)

