

Third Response to Reviews

## **On the Consistency of Scale Among Experiments, Theory, and Simulation**

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### **1 General**

We respond to the comments beneath the comments made. The authors' response is shown as **AU: red**.

### **2 Editor Decision**

Editor Decision: Publish subject to revisions (further review by Editor and Referees) (10 Jan 2017) by Prof. Remko Uijlenhoet

Comments to the Author:

Dear authors,

Thank you very much for submitting your replies to the issues raised by the reviewers and for submitting the revised version of your paper.

As you can see, reviewer #1 still feels the paper will probably not easily fit into the scope of the other papers to be published as part of the Special Issue of HESS honoring Prof. Eric Wood. The main point of this reviewer is that the spatial scale at which you deal with the topic of scale consistency among experiments, theory and simulation (namely that of soil as a porous medium) is likely going to be quite disparate from the spatial scales at which Prof. Wood and many of his colleagues have been and are working (namely that of landscapes, catchments, river basins, and continents). The reviewer is therefore not convinced that your work is going to provide new perspectives for hydrologists trying to model the world at such larger scales. That may be true, but at the same time the reviewer does not seem to have major concerns with the technical details of your work. Therefore, I am inclined to leave the discussion about the value of your work from a broader hydrological perspective to the readers of the pages of HESS and other scientific journals rather than to the reviewers of your manuscript alone.

**AU: We are of the opinion that Reviewer #1 has a particular area of hydrology that is of his/her interest. This area seems to be more**

applied than the subject of our manuscript. This difference in emphasis does not mean that we must conform our paper to the interests of the reviewer. Nor does it mean that the reviewer must conform his/her interests to those of the authors. The issues of importance should be related to the technical aspects of the paper. This reviewer, or any reviewer, is handicapped when the topic of the paper does not fall into his/her area of expertise. From our perspective, the technical statements of the reviewer have been dealt with appropriately. Hopefully, this has added some clarity to the work.

Reviewer #2 also seems to have no major issues with the technical content of your revised manuscript. This reviewer still finds the way in which you present your work, including your reference to Prof. Wood's work, not appropriate for a regular paper that is submitted to become part of a special issue. Although I am sympathetic with this reviewer's perspective, I feel that the presentation style is merely a matter of taste.

**AU: The reviewer has not updated his/her comments, but we have received your clarification from this reviewer. We agree that there is a stylistic difference between what we have done and what the reviewer suggests. We prefer the style we have chosen and find it satisfactory. We have made a few changes to try to accommodate the reviewer while maintaining our preferences for our paper.**

In conclusion, I recommend to accept your manuscript subject to minor revisions. As far as I am concerned, the paper will not be sent out for review again. I would appreciate it very much if you could give it one last try to accommodate some of the issues raised by the referees. Thank you very much in advance. I look forward to handling the revised version of your manuscript.

**AU: This has been a most tedious review process. We appreciate the efforts of those who looked at this paper to improve the manuscript. It can be difficult for reviewers when the vision of authors is different from theirs. Nonetheless, the reviewers have given us an opportunity to consider a different perspective and make sure that our work is presented as we would like. We are grateful for having had that opportunity.**

Best regards,

Remko Uijlenhoet

### 3 Report 1

Anonymous during peer-review: Yes

Anonymous in acknowledgements of published article: Yes

Recommendation to the Editor

1) Scientific Significance Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)? Fair

2) Scientific Quality

Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Good

3) Presentation Quality

Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)? Good

For final publication, the manuscript should be rejected

Please note that this rating only refers to this version of the manuscript!

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Not sufficient new material to warrant publication.

Limited relevance for hydrology.

*3.1 Comments embedded in text of first revised manuscript with highlighted text in quotes.*

No significant changes were made. I read the replies and cannot escape the impression that in the communication between the authors and this reviewer both sides appear to have some difficulties to grasp what the other side means to convey. I have made progress there and gained a better understanding, but the rather tort response from the authors shows that this is not mutual. As a case in point, my reference to MODFLOW was an attempt to try to get one of my points across a bit differently, and certainly not an invitation to build a TCAT version of it. A full paragraph discussing the limitations of MODFLOW would not have been necessary.

**AU: We have done our best to address the reviewer's comments as they were presented and as we understand them. The two subjec-**

tive comments remaining for this reviewer are that the manuscript does not contain sufficient new material to warrant publication and that the work presented is not sufficiently relevant to field-scale hydrology—perhaps these two items are related in the view of the reviewer.

Resolution of these issues depends upon one’s vision of the role and importance of fundamental research in an applied field. Put another way, do we accept current model formulations and do our best with them in light of difficult application issues, or do we attempt to identify ways in which current models can better represent the systems that we wish to approximate? Our view is that we need to do both of these things, while the reviewer evidently believes improving current models is either unimportant or a hopeless task. This is an interesting philosophical point. We feel sure that both the authors and the reviewer are interested in improving our ability to describe complex hydrologic systems. The avenue toward the most important and rapid advancements is often not clear. We favor a climate of mutual intellectual respect in which the ideas of all working toward the resolution of societal problems are encouraged to contribute, their work reviewed for scientific accuracy, and the impact to the field revealed in time by the community and those that build upon the work.

We believe that our view point is not unique in regard to the potential value of the small-scale approaches taken in this work. To support this claim, we performed a Google search on pore scale modeling of porous media, and this search returned about 870,000 results. Special issues dedicated to pore scale modeling have occurred routinely in the hydrologic literature, and special sessions on this topic convened at virtually every hydrologic venue with which we are familiar. Our conclusion is that others have found some worth in the sorts of approaches taken in this work, and all scientifically valid avenues toward potential advancements are worthy of consideration.

However, to address this point, we have added a paragraph at the end of the objectives to explain that this work addresses a focused fundamental issue using experimental and computational approaches at a scale that is much smaller than field-scale hydrologic problems and noting that this work is ideally a stepping stone toward improved understanding and models of hydrologic systems.

I have some background in the area the authors are covering, but after providing extensive detailed comments to help the authors identify parts of the

paper that are hard to follow or otherwise would benefit from modification am being informed that my limitations undermine my ability to understand the contribution of the paper. I am rather pessimistic in fearing that the vast majority of the HESS readership will be burdened by similar if not larger limitations.

**AU: We do not share the reviewer's pessimistic opinion of the abilities of the HESS readership.**

To see the contribution of the paper it helps to be aware that most hydrological problems manifest themselves and need solutions at the megascale. Hydrology is moving in the direction of modeling megascale problems with macroscale models through the use of brutal computing power for running models with a very large number of nodes and sophisticated parameter optimization techniques. In the revision the authors do a better job of highlighting the limitations of these models, but most hydrologists are aware of that, although usually not with the thermodynamical rigour that I ascribe to the authors.

At the megascale, hydrologists face massive issues with heterogeneity, spatial and temporal dynamics in porous medium properties caused by such factors as swelling and shrinkage, bioturbation, tillage, land use changes, and what not. The scientifically elegant and physically imperative approach advocated in this work is not generally considered the biggest fish to fry.

For the work to make a significant contribution, some expansion is still required, as per my earlier suggestions and those put forward by the authors.

**AU: These last three paragraphs boil down the areas of contention between the reviewer and the authors. We do not disagree at all with the reviewer's statements concerning the need for megascale solutions of hydrologic problems, of the need to harness computing power, of the importance of heterogeneity, swelling, bioturbation, tillage, land use changes, and subsidence. We admire the work of many individuals who address these problems in a timely manner as solution of these problems is crucial for the welfare of society. Applied hydrologic research is extremely important and worthwhile!**

**At the same time, we adopt the perspective that using traditional theoretical descriptions of hydrologic problems in an era where computing power is exploding, techniques for investigating sub-pore-scale processes are developing, and where the ability to manage and manipulate data is unprecedented overlooks an important element of hydrologic studies. Giants of the past did the best they could in describing problem physics in a way that would allow these descriptions to be used to simulate or describe system behavior. Few**

would claim that this work has been overwhelmingly successful. We believe that by ensuring that the fundamental physical description of hydrologic problems takes advantage of new methods of model support, better megascale models can and will result. We know that the work in the present paper does not immediately “plug in” to megascale models and reduce the difficulty that megascale modelers are currently wrestling with. We do claim that this systematic approach to dealing with changes of scale can be a boon to the ultimate development of megascale models that bury some elements of the system physics in fitting parameters. If one is in a hurry to get a megascale result, any result, then this work is not a significant contribution as the reviewer suggests. If, on the other hand, one values fundamental work that has the potential to support more elegant and complete field-scale models that capture operative mechanisms, this work is a contribution whose significance will be determined in time as this next generation of models develops. For example, the use of hysteresis and irreducible saturation in current models is an artifact of the failure to account for small scale physics. We now have ways to formulate the physics better based on fundamental considerations. Work is needed to include these advancements in production codes. We also maintain that the consistent method of scale change employed here in dealing with microscale and macroscale variables is a method that can be employed in subsequent transfers of scale to the megascale while ensuring consistent definition of variables. This is in contrast to some models currently being advocated that do not transfer scales carefully and thus do not provide a means of relating measurements one might make in the field to information developed in a laboratory or in a theoretical model.

#### 4 Report 2

Anonymous during peer-review: Yes

Anonymous in acknowledgements of published article: Yes

Recommendation to the Editor

1) Scientific Significance Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)? Good

2) Scientific Quality

Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work,

including appropriate references)? Good

3) Presentation Quality Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)? Fair

For final publication, the manuscript should be accepted subject to minor revisions

Please note that this rating only refers to this version of the manuscript!

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

**AU: The editor has noted that this reviewer has no major issues with the technical content of the revised manuscript. The reviewer did not provide an updated set of comments, but included comments we previously considered and answered as best we could. We have added yet another round of notes and comments trying to allay the discomfort that this reviewer has with our chosen method of presentation.**

**The editor did provide some additional guidance through email regarding this reviewer's comments, which centered on relation of this work to Professor Wood's work and evidently on the objectives. In response to these comments, we have deleted reference to Professor Wood from the abstract. Also note that we revised and shortened the objectives in the last version; and we are confident that these objectives are succinct, accurate, and met in this manuscript.**

**This reviewer seems unhappy with the listing of equations that define variables. These different definitions are important and identify precisely what we are measuring, comparing, and manipulating. The word "pressure" is used ubiquitously in studies of porous media. However, the actual meaning of the word in relation to data measured or modeled is, at best, glossed over. Our work requires and utilizes clear and careful definitions. Thus we believe the equations are a very important part of this work. We have added the second paragraph in the theory section which explains why we think it is necessary to clearly define larger scale variables in terms of microscale precursors in equations (1) – (19).**

**We have gone through the remainder of the paper, as well, but have**

made no other significant changes.