Responses to short comment #2

We thank Robert Reinecke for his interest in our manuscript and for his comments! In the following, we reply to all comments one by one. Comments by Robert Reinecke are in blue.

I very much enjoyed reading your submitted manuscript. However, I would like add two notes on this interesting discussion:

Firstly, I was missing from the manuscript that we not only use models to distill that one perfect equation but continuously use them to further our system understanding. As you noted quite precisely natural systems are very complex and modeling, for example a watershed, requires to take into account many variables and simulate a large system. Because of that, and also because these systems almost always contain human interactions which additionally make everything more complex, building a model is never a finished process that ends with one equation that best describes the system. Often it is a first "educated guess" that is then used as a foundation to understand the system further e.g. by using sensitivity analysis. It would be great if this is a little more reflected in this paper. I would also like to mention Wagener, T., McIntyre, N., Lees, M.J., Wheater, H.S. and Gupta, H.V. (2003), Towards reduced uncertainty in conceptual rainfall runoff modelling: dynamic identifiability analysis. Hydrol. Process., 17: 455-476. doi:10.1002/hyp.1135 as a possible citation.

Agreed. Model building, and the scientific endeavor in general is mostly incremental rather than an one-off process with a final absolute outcome. Moreover, we are also aware that “in the context of system investigation, models can also be seen as laboratories, designed and deployed by humans to investigate to a given extent and under given conditions aspects of a presumptive phenomenological reality” (quote from: Perdigão, 2017). In that sense, we agree with Robert Reinecke that what we suggest in the manuscript is a tool "which can be used together to guide model analysis and optimization in a pareto trade-off manner" (see p 13 line 326-327), and we will add to a revised version of the manuscript a phrase that his happens in the general setting of incremental learning.

A second discussion point I would like to raise is that in line 308 you clearly state that you are maintaining a information-theoretic point of view, which is good and clearly sets the scope of the discussion; nevertheless, I think an important point is missing: the skill of a researcher to implement a model well enough. Let's say, for the sake of the argument, that our perceptual model (a term coined by Keith Beven) of reality is almost perfect and with our modeling approach, whatever technique we apply (bucket, neural network ...), we would theoretically reach a high level of model performance. But because implementing models is a hugely difficult task, amplified by the lack of computer science and computer engineering background in the natural sciences (Hutton, Christopher, et al. "Most computational hydrology is not reproducible, so is it really science?." Water Resources Research 52.10 (2016): 7548-7555.), we may reach a very high computational complexity but possibly also a low model performance. I think this discussion should be reflected in your paper. It doesn't make your approach less applicable but highlights that looking only at this metric is not enough to guide the community to better research!

We fully agree with Robert Reinecke that a perfect conceptual model is just a necessary, but not a sufficient precondition for perfect predictions, and that the actual coding of a model contains endless opportunities to mess things up. Along the lines of Robert Reinecke's comment, we can state that poor coding will increase model computational complexity (e.g. think of redundant loops that can be replaced by a one-go matrix computation, or overly fine-grained time-stepping), and wrong coding will reduce model performance (or increase information loss). We suggest adding a phrase to the summary and conclusion at the place mentioned in the previous comment, stating that bit-by-bit is also a tool to promote better (less poor and less wrong) coding, together with a reference to the Hutton et al. (2016) paper.

Small notes on the abstract: 16: "length of the model" it is explained later in the manuscript but very misleading here. I was thinking of lines of code or runtime when reading it first

Agreed. We suggest adding the following explanation:

"The basic dimensions of computer model parsimony are descriptive complexity, i.e. the size ~~length~~ of the model itself, which can be measured by the disk space it occupies, and computational complexity, i.e. the model's effort to provide output.

29: "low performance" unclear if it refers to computational performance or model fit to observations or expected system behavior.

Agreed. As "performance" already appears in line 14, we suggest explaining it there:

"Measuring performance and parsimony for computer models is therefore a key theoretical and practical challenge for 21st century science. "Performance" here refers to a model's ability to reduce predictive uncertainty about an object of interest. The basic dimensions …"

Yours sincerely,

Uwe Ehret, on behalf of all co-authors

References

Perdigão, R.A.P. (2017). Fluid dynamical systems: From quantum gravitation to thermodynamic cosmology. M-DSC Monograph. (Hard copies for order at www.fluiddynamicalsystems.com).