Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-695-RC2, 2017


Hydrology and Earth System Sciences Discussions
Open Access



# *Interactive comment on* "Scaling, Similarity, and the Fourth Paradigm for Hydrology" *by* Christa D. Peters-Lidard et al.

**M. Sivapalan (Referee)**

sivapala@illinois.edu

Received and published: 8 March 2017

This synthesis paper serves an important purpose. It not only helps to celebrate the legacy of Eric Wood (who has made valuable contributions to the topics of scaling and similarity), but also provides a long term (40 years) perspective about where we have come from, what we have learned and where we might go in the future in terms of theory development.

I liked much of what saw in the paper, I am sure there are critical comments that can be made on some details of the history of progress that the authors have provided. I hope these are picked up by other reviewers. In view of the historical nature of the article and a purported new vision offered by the authors, to be effective (and be different) I chose to focus only on the big picture, and decided to keep my comments at a philosophical



Printer-friendly version

Discussion paper

level. My comments are not necessarily a criticism of the paper, but provide a broader perspective that, if the authors agree and choose to adopt, might bring about a more satisfactory closure to the paper. I am sorry that I use several of my own papers to buttress these arguments, and believe the same opinions are held by many others too.

The main argument behind the paper is that even while we have made a lot of progress in the last 40 years on issues of scale and similarity, progress towards a universal theory of hydrology, has slowed down, and the current paradigm is unlikely to lead to further advances. The argument then is that we need a new paradigm (which the authors call the Fourth Paradigm). The fourth paradigm is supposed to be somewhat related to learning from data (and lots of it). The key statement in the paper in this context is this one:

"Fundamentally, these approaches conform to the third paradigm, in the sense that they take as given a set of conservation equations that govern behavior at the fundamental (patch, tile, grid, hillslope, or REW) scale. Testing both the scaling and closure assumptions as hypotheses using data would move hydrology towards the fourth paradigm."

This is confirmation to me that the authors continue to approach the problem within the constraints of the Newtonian framework or worldview, now supplemented by approaches fashionably borrowed from the information sciences currently in vogue. This may advance computational hydrology (I am sure it will), but I am afraid that it will not advance theory development, which was ostensibly the primary focus of the paper.

From a medical doctor analogy, I am in agreement with the authors about the nature of the disease (the theory challenge), their diagnosis, and even the direction from where a cure might come from (data/information based learning). To my mind, much of what appears after the presentation of this viewpoint is a lot of hand waving, and does not convince me that it will lead to theory development of the kind they are hoping for. This gap in their logic or unfinished business is surprising, given that the nature of the cure has been evident for some time.

Of course, in the era of the “big data”, one can understand the thinking that big data will be the panacea to solve all of our ills. I am sure there will be lots and lots of action (including lots of hits and misses), to keep lots of people busy (a veritable cottage industry dealing with lots and lots of noisy statistics and uncertainty analysis). Real progress will be limited unless the focus on data-based learning is guided by some kind of overarching vision or theoretical framework. This is currently lacking in the paper – what I see is a blind faith that lots and lots of data will somehow bring about breakthroughs that we otherwise have not managed to obtain so far. It could, but only under certain conditions. In my mind, it is not data that produces theoretical breakthroughs, but the kinds of questions that you ask of the data (Sivapalan, 2009). The authors themselves cite Beven and Kirkby (1979) – TOPMODEL theory did not come from data mining, but from somebody sitting down, observing things and letting the imagination go wild. The same think can be said of Budyko (1974). The solution here is not more information theory, but more process hydrology, and plain hard science.

The disappointment for me is that a theoretical framework (one I can mention confidently, others may also exist) to guide this kind of data analysis (i.e., the Fourth Paradigm) already exists. It started becoming articulated a decade ago (Sivapalan, 2005; McDonnell et al., 2007) and has gathered momentum since then, and has found expression as the Darwinian Approach in several papers (Thompson et al., 2011; Harman and Troch, 2014). A prelude to the kind of big-data based Newtonian-Darwinian synthesis that is relevant to this paper has already appeared in the PUB Synthesis Book (Blöschl et al., 2013). In fact, the PUB book carried out a synthesis of catchment scale predictions organized across scales, places and processes. The notion of scale and similarity was the foundation for the extrapolation across places found in the PUB Book.

Chapter 2 of the PUB Book carefully presented the theoretical basis for the synthesis, which was the notion that catchments are co-evolved complex systems. This means that one does not look at catchment as a physical object that provides the boundary conditions for the balance equations for water movement (as one does in a Newtonian approach, which is traditional), but as co-evolved "living" systems, with a long history of co-evolution. Patterns of landscape properties and processes are just a snapshot of a something that has been co-evolving, and one looks at the similarity, differences and scaling that one observes at a moment in time or at a point or area in space arise from multiple trajectories of the same co-evolutionary (land forming and life sustaining) processes, underpinned by the same organizing (if not well known yet) organizing principles. Chapter 12 of the PUB Book presented the outcomes of the synthesis, and discussed how work along these lines can lead to accumulation of knowledge, which is a prelude to new theories. There is much more that can be done along these lines, with new data that is coming on line, as the authors say.

My point is that the Fourth Paradigm will not be a new paradigm unless backed up a broader Earth science perspective, such as this co-evolutionary view. By the way, this is the same worldview that is behind the highly successful Critical Zone Observatories in the United States and also in Europe. So what I am saying is not a biased perspective to impose my own views, but is a widely held perspective in the Earth science communities. Of course in the era of big data and hyper-resolution modeling, one is tempted to believe more in the power of satellites and subsurface geophysics and the power of computers (and of techniques like data assimilation) to generate results that are satisfactory enough for predictions.

But if one seriously believes that improvements in theory will be needed for predictions, or can in the long term lead to better predictions (predictions for the right reasons), as I am sure the authors do believe, then there is no alternative but to seriously consider the new co-evolutionary worldview to generate new kinds of questions with which to interrogate the patterns that one finds in the data, test hypotheses about the underlying causes, and use a multitude of tests of hypotheses to move towards general theories. In the absence of such a vision, the combination of traditional Newtonian paradigm and the big data, in my opinion, is a massive exercise in model calibration, parameter regionalization and data assimilation, that will keep a lot of people busy, but will not advance fundamental theory.

Big data can indeed help us generate new patterns (at a range of time and space scales) that trigger curiosity and imagination, and will lead to many more examples of simple theories such as the TOPMODEL theory. Indeed the availability of data from thousands of catchments around the world, such as MOPEX and other datasets, is already generating new non-Newtonian understanding through the mechanism of comparative hydrology, as the paper by Berghuijs et al. (2014) illustrates (for example).

In conclusion I enjoyed reading the paper, and indeed agree with the authors on what they are proposing, but believe that they should go to the next (and final) step and frame the problem from a co-evolutionary perspective. The co-evolutionary view is also very critical to frame the new prediction problems in a changing world in the new Anthropocene era (Sivapalan and Blöschl, 2015). They should present avenues, in the style of the natural history approach adopted by Charles Darwin (as described in Harman and Troch, 2016 and Thompson et al., 2012) to generate hypotheses from the data and methods (experiments, numerical simulations etc.) to test these hypotheses to develop new theories.

Of course, this not anything new or unique to hydrology: it is indeed the scientific method, and for this reason I draw inspiration from Jacob Bronowski, and point to a quote from his TV series and book (Bronowski, 1956, p. 23) of the same name "The Ascent of Man":

"All science is the search for unity in hidden likenesses...The progress of science is the discovery at each step of a new order which gives unity to what had long seemed unlike... For order does not display itself of itself; if it can be said to be there at all, it is not there for the mere looking... order must be discovered and, in a deep sense, it must be created. What we see, as we see it, is mere disorder."

References Cited

Sivapalan, M. (2005). Pattern, Process and Function: Elements of a New Unified Hydrologic Theory at the Catchment Scale. Contribution to: Encyclopaedia of Hydrologic Sciences, M. G. Anderson (Managing Editor), Chapter 13 (Vol. 1, Part 1), pp. 193-219, John Wiley & Sons.

McDonnell, J. J., M. Sivapalan, K. Vaché, S. Dunn, G. Grant, R. Haggerty, C. Hinz, R. P. Hooper, J. W. Kirchner, M. L. Roderick, J. Selker, and M. Weiler (2007). Moving beyond heterogeneity and process complexity: A new vision for watershed hydrology. Water Resources Research, Vol. 43, W07301, doi: 10.1029/2006WR005467.

Thompson, S. E., C. J. Harman, R. A. Schumer, J. S. Wilson, N. B. Basu, P. D. Brooks, S. D. Donner, M. A. Hassan, A. I. Packman, P. S. C. Rao, P. A. Troch and M. Sivapalan (2011). Patterns, puzzles and people: Implementing hydrologic synthesis. Hydrological Processes, Vol. 25, No. 20, pp. 3256–3266, doi: 10.1002/hyp.8234.

Blöschl, G., M. Sivapalan, T. Wagener, A. Viglione and H. H. G. Savenije, Editors (2013). RUNOFF PREDICTIONS IN UNGAUGED BASINS: A SYNTHESIS ACROSS PROCESSES, PLACES AND SCALES. Cambridge University Press, 500p.

Harman, C. and P. A. Troch (2014). What makes Darwinian hydrology "Darwinian"? Asking a different kind of question about landscapes. Hydrol. Earth Syst. Sci., 18, 417-433, 2014

Sivapalan, M. and G. Blöschl (2015). Time scale interactions and the coevolution of humans and water. Water Resources Research, 51(9), 6988–7022 doi:10.1002/2015WR017896.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-695, 2017.