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Discussions

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.

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This paper has many similarities to the accompanying synthesis paper led by Christa Peters-Lidard. In a certain way, the two papers can be considered intellectual twins, being underpinned by the same Newtonian worldview, which has dominated hydrology for much of the last 40 years. Much of the discussion in the paper centers around the classic paper by Freeze and Harlan, which is the very exemplar of the Newtonian worldview. There is much that I like in the paper – I do agree that Freeze and Harlan provides a good (common) framework to address some of the current modeling challenges. Also, through the use of this common framework the paper remained internally consistent, and is a good vehicle to organize the many contributions of Eric Wood to

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Discussion paper

hydrologic modeling across scales.

My comments on this paper, and my challenge to the authors, is also going to be driven by the same intellectual argument that adopted in my commentary on the paper by Peters-Lidard et al. (2017). As in the previous case, I am going to pitch my comments at a philosophical level. I hope other, more detailed aspects of the paper get critiqued by other reviewers.

The words “physical realism” appears in the title. I want to discuss the meaning of “physical realism” with examples from personal experience. My true “hydrological” education (as opposed to education based on traditional reductionist theories, e.g., Richards equation) began when I developed (from scratch) my own continuous water balance model, called LASCAM, for the eucalyptus dominated, Mediterranean catchments of Western Australia, published as a 3-part paper in Hydrological Processes (Sivapalan et al., 1996a,b,c). This was back in the early 1990s. By today’s standards the model was not so remarkable – indeed, it has many similarities to the HBV model of Sten Bergstroem, even though I was not aware of HBV at the time. After a lot of (political) resistance during my time in Australia, the model is now (only after I left WA) widely used by government agencies and consultants in the State.

What was remarkable – at that time my ecohydrologic background knowledge was minimal – was that the ET component of the model was very “elaborate”: it accounted for at least 6 calibrated parameters to account for root water uptake from three separate compartments of the subsurface. I convinced myself that this was appropriate (i.e., physically realistic) and that the model performed well for the right reasons (e.g., through good comparisons to groundwater levels). However, my understanding of physical realism under the circumstances changed dramatically a few years later when one of my PhD students, Richard Silberstein, along with post-doc Neil Viney and several colleagues from CSIRO Australia did a well planned and executed field experiment in one of the jarrah forest catchments. By usual standards, it cost less than $30,000 in real cash, which was also remarkable. The key results are included in 2 papers published in Agricultural and Forest Meteorology (Silberstein et al., 2001, 2003).

To cut the long story short (details can be found in the published papers), the study was undertaken during two 2-week periods. One was at the end of a very wet winter (potential ET of the order of 4 mm/day), and the other at the end of very dry summer (potential ET of 11 mm/day), i.e., under very wet and dry atmospheric and soil moisture conditions. Yet, remarkably, measured actual total ET (we measured all the components of ET also) was about 2.5 mm/day (almost constant), every day, for each of the two 14-day periods. Remarkable, also because the results made a fool of me for having 6 parameters in LASCAM to capture ET (what would that mean for equifinality and predictive uncertainty?). Now I know the reason – the eucalyptus trees in Western Australia have the ability to send roots down to 30 meters or more to tap into groundwater, and so do not feel the stress that most conceptual models (which I adopted in LASCAM) think they do. This we confirmed through drilling down to the water table and through soil core analyses: there were roots at 30 meters, and when the water table dropped a few meters between winter and summer, the roots followed the water).

My point is that physical realism goes beyond the "physics" that we know and capture in our models, both conceptual and "physically-based". Physical realism must therefore include the fact that native trees, unlike human-engineered crops, adapt themselves to the environment, like the eucalyptus trees in Western Australia adapt through their deep-rooting strategies to cope with the Mediterranean climate (vast fluctuations of soil moisture storage and groundwater levels). Subsequent work by a later PhD student, Stan Schymanski, in northern Australia, near Darwin, which experiences a tropical climate (contrasting wet and dry periods), confirmed further the adaptation ability of native ecosystems, and that the traditional ET modeling techniques derived from crops do not work there either. However, the adaptation strategy here is different. Here trees (that tap into groundwater) transpire at a remarkably constant rate of 1 mm/day throughout the year. However, during the wet season an understory vegetation develops (i.e., grasses) that can transpire at as much as 3 mm/day while the soil is wet, and then die/senesce (ET = 0) during the dry season. Ecohydrologists among the authors of this paper can confirm that this story is indeed repeated everywhere around the world – the only difference is that you get different vegetation types and different adaptation strategies. This was brought home to me by a recent paper published by Berghuijs et al. (2014) who applied the same conceptual model applied to over 200 MOPEX catchments across the United States, and interpreted the resulting seasonal water balances in terms of the controls of climate, vegetation and soils.

Now, one must add that natural vegetation not only adapts to the prevailing climate and geology, as demonstrated by Berghuijs et al. (2014), but they also adapt/modify the environment around them. I make this argument briefly to connect to my comment on the previous paper by Peters-Lidard et al. (2017) in that notions of hydrologic similarity as well as model parameterizations must account for the co-evolution of climate, soils and vegetation. Berghuijs et al. tried to accommodate this similarity in a Darwinian sense. The adaptations of vegetation is part of this, and the modification of the environment (e.g., soils) by the vegetation is an extension of this co-evolution. Hubert Savenije and his group have been pushing this line of argument for some time, as part of their Flexi modeling framework. The paper(s) by Gao et al. bring out, again through conceptual modeling exercises (in the same spirit as Berghuijs et al.) that the root zone depth (or a bucket capacity in their models) can be seen as an outcome of the co-evolution with the climate (Gao et al., 2014; Savenije and Hrachowitz, 2017). This is indeed physical realism, more broadly defined than the traditional recourse to Newtonian models, where associated parameters are prescribed externally (and also differently in different places). The benefit of treating these parameters are co-evolutionary is that they are better suited to predictions under change.

I am sorry about this, but this is a long-winded prelude to my main comment on the paper. From the early days of my PhD I have been a great fan of Alan Freeze and his pioneering papers. I learned a lot from his papers about how to do science, how to use models to generate new insights, including creative numerical experiments with models. The paper by Freeze and Harlan has indeed provided the intellectual framework for much of the hydrological modeling we have undertaken in the last 40 years. It connected the strongly fluid mechanics based (mechanistic) paradigm of Eagleson (1970) to the geoscience thinking that followed, and blossomed with the advent of digital terrain models, fast computers and their visualization capability.

However, in view of the arguments that I have made above, we have to come to the realization that Freeze and Harlan has outlived its usefulness. We need a new generation of models combine aspects of the Newtonian worldview embedded in Freeze and Harlan with a Darwinian worldview that accommodates the fact (the true reality) that, through the actions of vegetation (and now humans), whole catchments are co-evolved, almost "living", things that adapt to the climate and geology, and adapt the environment around them.

To be fair, at the time Freeze and Harlan published their paper, the focus was on modeling of runoff generation processes (and there was much less attention paid to water balance per se). In the subsequent 40 years, starting with Eagleson (1978), we have come to realize that evapotranspiration is an important component of water balance, even to model runoff generation processes correctly, e.g., to accommodate the effects of antecedent conditions. This paper, by focusing on Freeze and Harlan, has totally ignored the most important hydrologic process globally, which is ET. So, in effect, the paper is more about the past than about the future.

By interpreting progress in process-based models of hydrology through the prism of Freeze and Harlan, this paper is completely missing many lessons learned from 40 years of modeling effort, and therefore providing a partial, and biased perspective to new entrants to the hydrologic modeling in the future. I don't know exactly what the right modeling approach should be (there is a lot of debate on this) – I am convinced that to be useful it has to combine elements of the Newtonian and Darwinian worldviews. In more modeling language it must mean a combination of the bottom-up (upward) and top-down (downward) approaches – benefiting from the strengths each of them bring while overcoming their weaknesses (Sivapalan, 2005). Given the co-evolutionary nature of catchment hydrology, we must abandon the false sense of superiority we give to so-called “physically based” models. Nature does not care what models we cook up to mimic it, nor what names we give them. Nature is nature regardless and physical realism must reflect what actually happens in nature, and not what transpires in the human mind about them, or as Klemês (1986) put it, “. . . the logic of hydrological processes cannot be deduced from algebra”.

Given the fact that the authors have decided to use Freeze and Harlan to frame this synthesis paper, and the paper is internally consistent, I don’t know how it can be turned around now to address my comments. I will leave it up to the authors: perhaps there can be a discussion of the limitations of Freeze and Harlan in respect of physical realism, and in respect of future modeling of hydrological processes, especially under change.

References cited

Eagleson, P. S. (1970). Dynamic Hydrology, McGraw-Hill, New York.

Eagleson, P. S. (1978). Climate, soil, and vegetation, 1. Introduction to water balance dynamics. Water Resources Research, 14, 705–712.

Klemês, V. (1986). Dilettantism in hydrology: transition or destiny? Water Resources Research, 22, 177S–188S.

Sivapalan, M., J. K. Ruprecht and N. R. Viney (1996). Catchment-scale water balance modeling to predict the effects of land use changes in forested catchments. 1. Small catchment water balance model. Hydrological Processes, Vol. 10, No. 3, pp. 393-411.

Silberstein, R. P., A. Held, T. J. Hatton, N. R. Viney, and M. Sivapalan (2001). Energy balance of a natural jarrah (Eucalyptus marginata) forest in Western Australia. Measurements in spring and summer. Agricultural and Forest Meteorology, Vol. 109, pp. 79-104.

Silberstein, R. P., M. Sivapalan, N. R. Viney, A. Held and T. J. Hatton (2003). Modelling the energy balance of a natural jarrah (Eucalyptus marginata) forest. Agricultural and Forest Meteorology, Vol. 115, No. 201–230.

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.

Schymanski, S. J., M. Sivapalan, M. L. Roderick, L. Hutley and J. Beringer (2009). An optimality-based model of the dynamic feedbacks between natural vegetation and the water balance. Water Resources Research, Vol. 45(1), W01412, doi: 10.1029/2008WR006841.

Berghuijs, W. R., M. Sivapalan, R. A. Woods and H. H. G. Savenije (2014). Patterns of similarity of seasonal water balance: A window into streamflow variability over a range of timescales. Water Resources Research, Vol. 50(7), pp. 5638–5661, doi:10.1002/2014WR015692.

Gao, H., M. Hrachowitz, S. J. Schymanski, F. Fenicia, N. Sriwongsitanon, and H. H. G. Savenije (2014), Climate controls how ecosystems size the root zone storage capacity at catchment scale, Geophys. Res. Lett., 41, 7916–7923, doi:10.1002/2014GL061668.

Savenije, H. H. G. and M. Hrachowitz (2017). HESS Opinions "Catchments as meta-organisms –a new blueprint for hydrological modelling". Hydrol. Earth Syst. Sci., 21, 1107–1116, doi:10.5194/hess-21-1107-2017.

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