

**General comment:** I very much enjoyed much reading this opinion paper, as it elaborates on most of the key challenges we face in hydrological modelling and theory development today – particularly if we go for real (independent) predictions of hydrological dynamics (one could argue that we are rather able to predict the future than to “postdict” the past). More specifically I very much agree with the authors on the prominent role of organization and optimality for catchment functioning. These key system attributes arise indeed from feedbacks between biota and abiotic processes in a geo-ecosystem which emerge at the catchment scale (one might prefer “macroscale”) and static model configurations/parametrizations are indeed neither appropriate to deal with catchments as evolving systems nor to deal with emergent changes of system properties due to non-linear abiotic feedbacks as for instance soil cracking. My excitement (and agreement on “the what/ the object of desire”), does however not imply, that I agree with the authors main conclusions that models we usually categorize as “conceptual” are the superior means to address these challenges, while so-called “physically based” models are rather useless. My personal point of is exactly the opposite! This corroborates a) that the proposed OP paper is a valuable contribution, as it stimulates a controversial debate (not about the “what” but about the “how”) and b) the truth might be somewhere in the middle.

I highly recommend publishing this OP paper in HESS. Prior to this authors should consider the following comments that might help a) to improve the presentation of their without doubt very good ideas and b) to correct the somewhat biased appraisal of conceptual models against physically based models. In fact is not so much the type of model we use to explore the role of spatial organization and feedbacks, but whether the tool is suited with respect to processes, scale and underlying question. Both model types may jointly contribute to the learning process as their strengths are complementary as further elaborated below.

**Specific comments:**

- **P1 - the title is miss-leading.** The title is appears outdated and does not reflect the key message of the OP paper. The discussion whether models are “physically based” or “conceptual” is at best of scholastic value. All models are to a certain extent physically based, as they rely on the conservation laws of mass, momentum and energy. Any physical theory or model is, while being based on inference, yet an empirical fit to observables with the purpose to explain a class of phenomena as broad as possible with as less as possible equations and theorems (Popper, 1935). All physical models we use today are hence incomplete in the sense that there are phenomena which drop outside their range of validity. Hence, all our hydrological models do to a certain extent rely on conceptualizations of for instance of preferential flow, or root water uptake or root growth or for instance shallow turbulence or sediment transport capacity. The cardinal question is not whether a model is conceptual or physically based, but whether the conceptualizations to incorporate the key processes and feedbacks are based on solid grounds and on the true perception of what is limiting dynamics of interest. The art of doing proper physics (and modelling) is, due to my old high school teacher in physics, to come up with the right simplification/approximation which makes use of essential symmetries in the problem of interest and yet reflects the underlying controls in a physically consistent way. A key element of physical consistency (also in hydrology) is that any flux is the product of a driving by a potential gradient and a resistance term – as fluxes deplete their driving gradient and thereby perform work against the resistance. A macroscale resistance is however more than solely a material property: it

reflects both the spatial organization of for instance soil materials in the control volume as well as their textural compositions (Zehe et al. 2013). Conceptual models might hence still be physically consistent as long as fluxes are driven by gradients and perform work against resistance terms (Kleidon et al., 2013; Westhoff and Zehe, 2013; Westhoff et al., 2016). Explicit, separated treatment of both controlling factors is of particular importance when exploring the role of spatial organization in hydrological dynamics, as it is difficult to define “organization” without dealing with the terms “entropy”, “entropy production” and “entropy export”. Entropy production is however tightly linked to product of flow and the driving potential difference (Kleidon and Schymanski 2008).

- **P2) on page 1 - the introducing statement should be precise and supported the literature:** Such a sweeping and undifferentiated criticism of “physically based models” is not what I would expect from such well respected and distinguished colleagues. The authors are rather vague in precisely explaining what they mean with “... addressing in a meaningful and consistent way”. More importantly the authors do not cite a single recent study which uses physically based models to corroborate their statement/claim that “physically based models do a poor job”. It seems that the authors miss a large body of literature (surely not on purpose) which corroborates that physically based models may be either a) parameterized based on observables and reproduce catchment behavior (Loague and VanderKwaak, 2004; Ebel and Loague, 2006 & 2008, Zehe and Blöschl, 2004), b) calibrated using regularization techniques (Perez et al.; 2011), c) may reproduce preferential flow and tracer transport (Sander and Gerke, 2009; Wienhöfer and Zehe, 2014; Klaus and Zehe, 2010 & 2010) based on conceptualizing macropore flow also to explore their role of on catchment scale runoff production (Zehe and Blöschl, 2004; Loritz et al., 2016). Physically based model structure are indeed, as stated by the authors, subject to equifinality (as we solve and ill-posed problem). However, the set of acceptable model structures can be reduced by using independent data sources and constraining parameter sets, because these models rely on meaningful and measurable state variables and parameters (Ebel and Loague, 2006 & 2008; Klaus and Zehe, 2011). Please do not get me wrong – I am aware that physically based models suffer from many short comings (which reflect our incomplete understanding). And yet they provide many advantages and we may learn from their application, particularly also from their failures, if we honestly share and discuss deficiencies instead of hiding them by calibrating parameters. When criticizing either one or the other model paradigm (if we stick to this to me fruitless and outdated categorization), I think it is a matter of good practice to refer to recent studies and to criticize to the point, rather than “claiming” that models of this or the other type do a “poor job” or do not capture systems essentials in a “meaningful manner”. Last not least, their statement surrogates, that the other type of models (shall we call them Darwinian instead of conceptual?) do a much better job in capturing the outline essentials. This is rather a smart rhetorical trick, than is it justified by the examples provided in the paper.
- **P3) on page 1 - the scaling argument is not precise and a pseudo-argument.** The Darcy-Richards equation assumes that capillarity controlled diffusive fluxes dominate soil water dynamics and essentially relies on a local equilibrium assumption. The latter is crucial for defining a meaningful matric potential, which describes binding energy density of soil water to capillary forces in the pore space. The local equilibrium assumption is however violated at grid scales larger than 1m (Or et al. 2015; Vogel and Ippisch, 2008). Hence, Darcy Richards models need to be operated at a certain grid scale. It does not imply that these models

cannot be applied a large spatial extents (which does not imply computational overkill as it did 30 years ago). The good old argument that soil hydraulic functions from the point scale are not useful at larger scales is of course still valid and we can of course not average for instance the van Genuchten parameters as the retention curve based on the averaged parameter set does not characterize the average retention behavior. However, one may derive effective hydraulic functions based on a sample of retention curves using undisturbed soils cores from distributed locations by averaging the point pairs at given  $p_f$ -range and fit a curve to these averaged retention values. This implies to derive a curve characterizing the averaged retention behavior of the undistributed samples and has recently been shown to work not too badly when being used at the catchment scale (Loritz et al., 2016). Again, don't me wrong – I do not advocate modeling the entire of Europe at a 1 m scale using Darcy Richards models. But we can use them at scales up to 100 km<sup>2</sup> today (which implies computation times of order 1 day), and they might yield valuable and complementary pieces to the puzzle of how self-organization of environmental systems and their functioning are connected in a dynamic sense. This holds particularly for preferential flow as will be elaborated below. Last not least one could also argue that conceptual models are useless because they cannot properly be downscaled. Does this compromise the value of conceptual models when using them in comparative studies at scales of “organized simplicity” (Dooge, 1986). I think not!

- **P4) on page 3 – what is wrong with empirical approaches.** I agree that there is nothing wrong with empirical relations in fact empirical work and induction has been the god father of many scientific laws we use today; and empirical testing of concepts and theories forms the basis of natural sciences (Popper, 1935). However, empirical work should always lead to something which is generalizable, and not been mistaken to come up with site and case specific quick fixes to fit data. In this sense I think the FLEX-TOPO approach is a real step ahead in conceptual modelling, as it provides generalized model building blocks to reproduce the hydrological functioning of different and separable landscape entities (plateau, mid-slope and wetland). However, the approach it is not as simple as the authors claim it to be (as will be elaborated below). Also physically based models contain many empirical formulations and these are often poorly defined in the sense that there are many concurring formulations for the same process (nearly as much as conceptual models in the market): for instance the parametrization of a) soil hydraulic functions (there are many more models than the van Genuchten and Mualem- and Brooks and Corey models) or b) of the stomata resistance (according to Damour et al. (2010) there are more than 60 different approaches) or of c) transport capacity of sediments in open channel flow. So what is to be concluded from such diversity in empirical approaches, which all try to mimic the same thing? Maybe that we do incomplete experiments based on the wrong perception of what is limiting the process of interest or the dynamics of the system of interest? With respect to stomata resistances one could wonder whether it is not exclusively the opening and closing of stomata which is limiting photosynthesis, but turbulent exchange of CO<sub>2</sub> within the canopy – this would lead to different kind experiments to derive parametrizations for plant gas exchange. With respect to conceptual models one could argue that a lumped treatment of mass fluxes (instead of an explicit, separated accounting for driving gradients and resistances), surrogates that closing the mass balance is sufficient to reproduce hydrological dynamics. Personally, I think this is not enough - it is the triple of mass, energy and momentum balance that has at least to be closed when simulating hydrological dynamics for the right reasons.

Maybe equifinality is the price we have to pay for neglecting the latter two conservation laws in our models?

- **P5) on page 4 – representation of spatial organization and different runoff generation mechanisms:** The statement that physically based models cannot represent spatial organization and diversity of runoff generation processes is simply not correct. These models capture different runoff processes by means of fluxes across different boundaries: e.g. both sorts of surface runoff production (Bronstert and Plate, Zehe and Blöschl, 2004) by proper formulation of the upper boundary condition (for instance as Cauchy boundary condition), or of subsurface storm flow at the lateral boundary by means of seepage interface. These models may also represent different forms of lateral flow, for instance the fill and spill mechanism by incorporating bedrock topography and permeability (Hopp and McDonnell, 2009; Loritz et al., 2016) or pipe flow (Wienhöfer and Zehe, 2014).
- **P6) on page 4 – representation preferential flow:** The real challenge of preferential flow is that it implies a strong local disequilibrium and imperfect mixing between a fast fraction of soil water and the slower diffusive flow in finer fractions of the pore space. As outlined in a couple of excellent review articles (e.g. Šimůnek et al., 2003; Beven and Germann, 2013), up to now many concepts have been proposed to overcome the inability of the Darcy – Richards concept to cope with rapid, not-well mixed or even non capillary, preferential flow. These concepts range from early stochastic convection, dual porosity and permeability approaches assuming overlapping and exchanging continua, spatially explicit representation of macropores as vertically and laterally connected flow paths to non local formulations of the Richards equation. All these approaches have their advantages and drawbacks, but each of these approaches is capable to reproduce at least partly the essence of preferential flow – non-equilibrium flow and imperfect mixing. Conceptual models essentially assume perfect mixing in their reservoirs. It is hence not straight forward to understand how they can account for a process which is not well mixed per se. I think the authors should better justify this statement.
- **P7) Page 5 – Catchments as living entities:** I very much like the idea that biota exert dominant controls both on recent processes and on the past development of the catchment as a geo-ecosystem. I am, however, not sure what is meant with “meta organism” (I am aware of the term meta-population). One could also argue that the abiotic components and processes define the niche and the disturbance regimes for the population in an environmental system (rather than there is a niche for the entire landscape). The latter implies there might be a selection criterion against landscapes – stability in an ordinary sense is certainly too simple. The authors might find the term ecosystem engineers helpful in this argumentation (Schröder, 2006). These are species which create and stabilize their own niche as for instance beavers and earthworms.
- **P8) Page 5/6 – Dynamic models for dynamic geo-ecosystems:** I very much agree that steady model configurations are inappropriate to deal with processes and feedbacks in dynamic environments. This is also nicely discussed in Loritz (for the case of so-called physically based models) when modelling catchments with cracking soils. Other communities are way ahead in this respect, as they try to build their models around key feedbacks between biotic and abiotic processes. Tietjen et al. (2010) provides a nice example for a coupled model for soil water dynamics and concurring dynamics of shrub and grass vegetation and feedbacks between a deeper root system and higher infiltration rates into deeper soils compartments under shrubs. The latter provides them an advantage against grass vegetation. Even when

not going this way one may use for instance simple ecological temperature indices (Menzel et al., 2003) to a) determine the end and onset of the dormant period of vegetation (Loritz et al., 2016) or separate summer and winter runoff regimes in a much more meaningful manner than on simple definitions based on the Julian day (Seibert et al., 2016).

- **P9) Page 6 – essential hydrological functions splitting, infiltration/recharge and drainage:** I very much like the idea that a geo-ecosystems may develop towards a specific balance between recharge/ storage and drainage to support the underlying needs of biota in an optimum manner. One can even argue that is hydro-pedological setting which determines the “bottle necks” that either hampers the one or the other process from operating in an optimum manner. This implies that one can separate different types of preferential flow paths into wetting structures (arteries) and drainage structures (veins), which facilitate, depending on retention properties, relaxation back to local thermodynamic equilibrium and thus enhance entropy production in the system, by “bypassing the limiting bottleneck” for recharge or drainage (Zehe et al. 2013).
- **P 10) Page 8 - biota engineering their environment:** I agree that biotic actors play a key role in the partitioning process of water in the key zones by creating dynamic structures. However, the key control on soil water storage is capillary forces, which are essentially caused by a key property of the water (fluid): its high surface tension. Without capillarity and without the soil being a porous medium providing capillary pores where water is be stored against gravity, there would be no soil water storage at all and thus no field capacity. Infiltrating rainfall would drain into groundwater bodies, leaving an empty soil as the local equilibrium state - there would be no soil water dynamics at all, probably even no terrestrial vegetation and the water cycle would operate in a complete different manner without capillary forces.
- **P11) Page 8 – two water worlds:** I might be wrong but, the idea of two separate water worlds, one supplying runoff the other supplying transpiration, which is advocated in Brooks et al. (2010), is a somewhat straight forward interpretation of soil physics and the inherently low degrees of freedom water to mix across pores size fractions, than a real mystery (Zehe and Jackisch 2016).
- **P11) Page 8 – dynamic root zone storage:** I very much like the idea of a dynamic root zone. This also a particularly good example to underpin the added value of conceptual models as the right means to test such an idea in a large set of catchments in a comparative manner. I do, however, not see that this implies “unimportance to the un-saturated zone” in contrary it stresses its importance, as each soil water accounting scheme (including the beta store) implicitly accounts for capillary driven storage by accounting for field capacity (see reflection in absence of capillarity above) . As also physically based models may account for root growth and shrinkage the implementation of this, without doubt very insightful optimization idea, is at least not impossible and would for sure lead to interesting simulations. Complementary to that one might use the latter kind of models to test the idea, that plants minimize their energetic invest during root water uptake as recently suggested by Hildebrand et al. (2016). Testing this idea needs essentially an explicit treatment of capillary water potentials (hence physically based models). Overall, this corroborates that both types of models are of complementary value for understanding root water uptake and dynamic root zone storage in an optimality context. Blaming the one or the other model type does help in solving the questions we need to solve - we need to use the right tool at the right scale.

- P12) Page 8 – system evolve to a greater efficiency:** The insight/idea that environmental systems might be subject an optimization of either storage efficiency, recharge efficiency or drainage efficiency is very appealing. However, testing this idea not restricted is by the type of model we use, but it is restricted by the modeler’s creativity! If she/ he has this idea, she/he can either use the one or the other tool to test it. Conceptual models for optimization of root zone storage in a catchment inter-comparison or simple physically based models to test the idea of minimum energy expenditure of root water uptake. One may even use very complex physically based models to test for instance the idea that preferential flow paths reduce control volume resistances (as stated by the authors), and thereby accelerate flow against the driving potential differences and thus power within this flow. One may furthermore optimize the density of macropores, with respect to entropy production (Zehe et al. 2013) and test whether this model/system configuration, which splits rainfall into infiltration and surface runoff in manner which maximizes recharge efficiency, is suitable to predict runoff behavior in real catchment (works). Again, this corroborates that testing of organizing principles is not restricted to the use of any type of model. Both types of models may add important bits and pieces to the puzzle.
- P13) Page 9 /11 – Darwinian thinking versus Newtonian thinking:** It is quite fashionable to distinguish Darwinian and Newtonian approaches in hydrology. This is in fact very helpful when the terms are used in the way the authors do it – i.e. to stress that dynamic geoecosystems cannot be treated in a purely mechanistic manner and that one may learn from comparing diversity of many places (as Darwin learnt from diversity of species). The excitement decreases, however, when sticking to the old paradigm that runoff is “the one and only” and the system is “understood” after fitting the hydrograph. In fact most of the papers I know and we publish, justify their conclusions on a good fit of a model to “something”. This might be a rather weak foundation, as we all know that particularly at larger scales such a fit and the underlying model structure is constrained by for instance rainfall data (hence contaminated the underlying uncertainty and errors). Could it be that our success paradigm is too much biased from our engineering background and we spend too few papers on “explaining” and too many papers on “predicting/fitting” curves? Secondly, one needs to be careful not to overdo the analogy to Darwin’s work, particularly when using the term “co-evolution”. Is there a catchment Genome and do catchment inherit characteristics? Can we distinguish catchments genotypes and phenotypes? Is there competition among catchments/ecosystems and if so, what is the extension principle and how to define an optimum “fit”? Last not least, the authors might also be aware landscape ecologists have a total different understanding of co-evolutions, as they have. This is about the competition of for instance a parasite (cuckoo) and the host animal (titmouse) – the latter develops better and better strategies to detect the cuckoo egg (of course by trial and error and selection) the former better and better strategies to prevent that the egg is detected.
- P14) Page 12 – self-organization causes simplicity:** Due to my understanding of Dooge’s work self-organization causes either organized complexity at intermediate scales or organized simplicity at larger scales. In intermediate systems of organized complexity time scales of rapid subsurface flow in preferential pathways and in the river net are of the same order. Hence, one cannot treat the subsurface in a lumped, well mixed manner, as flow within the hillslopes exert first order control on timing of the catchment response behavior. At scales of organized simplicity time scales of rapid subsurface flow are much smaller than the

time scale of flood routing in the river network. Here we may treat the subsurface in a lumped manner, because the river controls timing of the catchment response behavior (yet no one would come to the idea to neglect the river and treat open channel flow in a lumped fashion).

- **P15) Figure 2 – is FLEX TOPO really simple?** Although I really appreciate the FLEX-TOPO approach as great advancement in conceptual modelling – I think it is neither intuitive nor based on a simple set of equations. The underlying scheme of connected reservoirs and splitters and convoluters is not an intuitive image of a hillslope. Neither a layman nor I would be able to identify the “plateau” or the “hillslope” from the scheme, when the titles were omitted. Similarly one cannot depict depth to bedrock or fast connected flow paths in the subsurface. I admit this scheme might be intuitive for the user group of FLEX TOPO, but it is not an intuitive picture of a catchment. A 2-d plot of the permeability field that represent a hillslope in a physically based model is on contrary a rather intuitive picture of a hillslope (compare Figure 3 in Loritz et al., 2016 <http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-307/hess-2016-307.pdf>). When using a proper color code also a layman can depict fast flow paths in the subsurface or the bedrock interface and its topography. She/he may judge relative differences flow velocities and identify upslope and downslope areas and guess where runoff will origin from. Moreover, I tried to right down the coupled equation set underlying the scheme in Figure 2, but I failed. In the underlying equation system is a set of coupled, non-linear ordinary differential equations, including chains of non-linear functions (where the output is the argument of the next function). I highly recommend that the author share this equation set with the readers, I doubt that it is at the end of the day simpler than the Darcy-Richards equation and the Diffusion Wave equation which represent subsurface flow and overland flow/channel flow in physically based models. The latter are of course solved for, but this can be easily written down in matrix form. The only thing that can be claimed to be simpler is that FEX topo might use a simple Euler forward time stepping scheme, while physically based models use more advanced numerical schemes.

In conclusion, I highly recommend publishing this OP paper in HESS, after the authors addressed these comments and I look forward to their (surely thoughtful) response.

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