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Discussions

Interactive comment on "Opinion paper: How to make our models more physically-based" *by* H. H. G. Savenije and M. Hrachowitz

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Hydrological models as living organisms.

1. General

We thank Prof. Zehe very much for his very detailed and thoughtful comments on our opinion paper. The exchange of ideas is very much appreciated and will hopefully lead to a wider discussion on how we should approach the challenge of modelling an ever-changing and evolving hydrological system. In one way or another the hydrological community will have to deal with the interactions and feedbacks between climate, ecosystem, hydrological system and society leading to gradually evolving hydrological properties and patterns, as opposed to applying static hydrological models, that are widely used even when studying the effect of climate change. Of course we are aware

of the tremendous efforts that are being made to this extent (such as LPJ, see: Sitch et al., 2003). Here, we only intended to provide a direction that is equally physical, but less complex as it focusses on the patterns that emerge at the macroscale.

It is hard to disagree with the reviewer's statement that the introduction of the ecosystem as an active agent in the hydrological system does not depend on the modelling philosophy. Whether the modeller uses a top-down (i.e. "conceptual") or a bottom-up (i.e. "physically based") approach does not matter, as long as the dynamics of an evolving and adapting system is accounted for in a meaningful and well tested way. We fully agree with this point of view and after re-reading our paper, we realized that it does indeed come across as a plea for "conceptual" models as preferred tools over "physically based" models. This was not our intention (please also see replies to Reviewer 1). Rather we wanted to make the point that <u>at the catchment scale</u> conceptual models are <u>not inferior</u> to physically based models and may in many cases even be more efficient or, in case of scarce data, even the only meaningful way to do this. This does not imply that 3-D "physically-based" models would not be able to <u>do</u> the job if sufficient observations to capture the natural heterogeneity in a meaningful way were available. As this is often not the case we think that such models are <u>not practical</u> in many regions and/or at larger catchment scales.

In the original manuscript we tried to emphasize and bring across the point that conceptual models can be very efficient at the catchment scale and can, if well parameterized and adequately tested, reflect the true physics emerging at that scale. The parameters, ideally observed at the modelling scale (e.g. catchment, landscape unit, grid cell), are then manifestations of the aggregated heterogeneity at the modelling scale. We do not question the importance of physically based models at laboratory, plot or at whichever scale where direct observations of the effective(!) parameters (or meaningful calibration, i.e. limited equifinality) are available at the spatial resolution of the model. We also state that models that do not incorporate pattern formation are not reflecting the true physics. Physically-based models that allow for that, as for example the ones cited by the reviewer, are of course not meant by this, as is also acknowledged, albeit without explicit references, by our statement "…recent developments that use conceptual formulations based on dual or multi-domain flow,…" (p.4,l.14-15). But, again we believe that at larger scales such models are less efficient to deal with, analyse and understand pattern formation. At such larger scales, emergent properties, such as linear reservoir recession, can be applied directly (just like at macroscopic scale we rather apply the gas law for the pressure and temperature of a gas than the interaction between individual molecules). In fact we call for appreciating the value of taking a step back, zooming out and exploiting the functional relationships emerging at the macroscale and thus for a "revaluation of conceptual models as physics-based representations of the hydrological system". We think that this is not the same as saying that physically-based models are incorrect or inappropriate. We only wrote that if physically-based models do not include patterns, which are essential components of the hydrological system, that they then miss essential physics.

In contrast to the reviewers impression, we did not make statements such as "conceptual models are the superior means to address these challenges, while so called physically-based models are rather useless" and we also did not mean to imply superiority of one over the other. We rather claimed that 1) taking a step back and zooming out to the macroscale, represented by conceptual models (if designed well) also reflects true physics at that scale, and 2) that they are very efficient to do so. In fact we fully agree with the reviewer's statement that "the truth might be somewhere in the middle". Both modelling approaches have a very valuable contribution to make in understanding hydrological processes at the catchment scale, and can "jointly contribute to the learning process as their strengths are complementary". At the smaller scale, physically-based models can do a lot to help us to understand why patterns emerge, and why infiltration, retention and drainage patterns that we observe in nature are indeed the most efficient ways to dissipate potential energy. At the larger scale, conceptual models can teach us which emergent patterns best describe system behaviour. Thus, we fully agree with Prof. Zehe that a dual strategy of top-down and

bottom-up approaches can help us to better understand how nature works. We will try to clarify and bring this out more prominently in the revised paper, so as to avoid the above misunderstandings.

We realise that by including the term "physically-based" in the title, we may have put the reviewers on the wrong footing. Therefore we plan to change the title in the revised version to "Hydrological models as living organisms", because this is the most important message of the paper. Our hydrological system is alive, including active living agents that manipulate their environment in a way that it is better suited for survival. As a result, our models should be alive as well, and reflect a living organism.

2. About the specific comments

P1. **The title is miss-leading:** Yes, we agree that the title is putting people on the wrong footing. Although the title does not imply or mean criticism on physically-based models, it apparently understood to be so, also by the first reviewer. Therefore we suggest a title that better conveys the essence of our opinion "Hydrological models as living organisms". All other things that reviewer remarks under P1 we agree upon.

P2. **The introducing statement should be precise and supported the literature:** Yes, we agree. We would do well to refer more explicitly to "physically-based models" that do represent features that arise from organization such as preferential-/macropore-flow and the related effects on "mixing" and solute transport. In addition, we will modify the language of doing "a poor job". We also agree that the different modelling approaches can learn form one another. Yet we remain convinced, that getting such models to reflect patterns that become apparent at catchment scale (such as the linear reservoir), remains a major challenge for detailed "physically-based" models, irrespective of the immense data demand that such models would have at that scale.

P3. **The scaling argument is not precise and a pseudo-argument:** We agree that Darcy-Richards models may provide complementary pieces of the puzzle, and that large-scale conceptual models can not be used to downscale to the plot or laboratory scale. There clearly is a scale jump between them and we can make advances in our understanding if we bring these two scales together.

P4. **What is wrong with empirical approaches:** We do agree that site-specific conceptual models have not much to offer. But this is precisely what the paper intends to address. It presents a general framework for deriving model structure from landscape features. Subsequently it provides a general (and global) method to derive the root zone storage capacity based on evolutionary reasoning. And finally it presents a way to derive the recession coefficient from storage depletion. The parameters thus obtained are of a general physical nature at the macroscale, which allows site specific models to be set-up and applied, but without the parameters being idiosyncratic. Finally, we do not contest the value and necessity of the conservation of mass, energy and momentum. Conceptual models do strictly conserve mass (within the limits of observational uncertainties). If well designed, calibrated (i.e. not only to stream flow but also to metrics that stronger reflect partitioning between runoff and evaporative fluxes, such as runoff coefficients) and tested they do also satisfy or at least not grossly violate the conservation energy. That leaves the transfer of momentum that is parameterized in relatively simple storage-flux relations and which leaves some room for improvement, in particular as the resistance terms aggregate all sorts of heterogeneity and organization which should be separated in a clean way (as also highlighted by the reviewer in P1). It is however not clear if the latter will significantly improve models beyond the academic interest of providing a theoretically satisfying description of the system. Rather, the simplified representation of the conservation of momentum is not necessarily a disadvantage. Also the Darcy and Richards equations are simple gradient-flux relation, not much different in set-up as the storage-flux relations of (non-)linear reservoirs. So also here, there is no fundamental difference. The difference lies in the scale of application and the laws that belong to these scales.

P5. **Representation of spatial organization and different runoff generation mechanisms:** It is of course true that physically-based models can represent spatial organi-

sation and diversity of runoff generation processes (as well), but this will go at the cost of enormous amounts of data. One has to know the characteristics of the subsurface in detail, which maybe one does in heavily instrumented experimental catchments such as the Panola (TrompâĂŘvan Meerveld McDonnell; 2006) or in artificial catchments, which are typically not available with current day observation technology (and may well never be) at the scale and resolution required. Again, we do not claim superiority, but we claim that conceptual models, from the perspective of the macroscale, are capable of reflecting the true physics at the right scale and are, moreover, efficient and practical tools to apply in such circumstances.

P6. **Representation of preferential flow:** We again fully agree, that preferential flow is the main challenge. However, it is not correct that conceptual models always assume perfect mixing. Over the last couple of years there was remarkable progress and a range of studies successfully implemented and tested formulations for incomplete mixing in conceptual models. It was shown that these formulations contributed to considerably improving the simultaneous reproduction of water and solute dynamics in a range of contrasting regions and catchments (e.g. Botter et al., 2011; Hrachowitz et al., 2013, 2015, 2016; van der Velde et al., 2014; Benettin et al., 2015; Harman, 2015).

P7. **Catchments as living entities:** Thank you for this suggestion. Indeed species such as earthworms and beavers function as ecosystem engineers, but so does the vegetation in engineering moisture infiltration, retention and drainage by 1) concentrating throughfall from the canopy towards preferential infiltration spots (e.g. Gerrits et al., 2010), 2) facilitating stem flow, 3) creating preferential flow along (former) root channels, 4) expanding the root zone storage, 5) providing organic material to the forest floor soil, and in doing so creating an attractive environment for mammals, insects, earthworms, fungi and microbes to improve the texture of the soil and by probably more that we have failed to mention. In brief, we agree that catchments are living entities, but rather than to talk of ecosystem engineers as individuals, we think that it is better to see the ecosystem as a conglomerate of mutually interacting agents that by coevolution create an environment in which the ecosystem survives - reminding of the definition of a meta-organism (or holobiont): "assemblages of different species that form ecological units" - even if the ecosystem changes its composition and the character of the individual agents in the process.

P8. **Dynamic models for dynamic geo-ecosystems:** Agreed.

P9. **Essential hydrological functions splitting, infiltration/recharge and drainage:** Yes, we fully agree and thank the reviewer for the interesting idea of arteries vs. veins and the reference of Zehe et al. (2013). This is right on the mark and we shall certainly include it in the revised manuscript.

P10. **Biota engineering their environment:** Agreed. This is an excellent and profound observation. We would, however, suggest to extend this idea a bit more. Surface tension is clearly one critical part of the story. Yet, we would argue that vegetation is as much a key control. What happens without vegetation? The water content below field capacity is only depleted by soil evaporation, which occurs at longer time scales than water extraction by transpiration. The storage deficits below field capacity therefore develop to a lesser extent than in the presence of plants. This in turn largely reduces, at least in not exceedingly arid regions, the non-linearity of the unsaturated zone: as much of it will remain much closer to field capacity throughout the year, not much storage is available and incoming water will largely only be routed to e.g. the groundwater with some time lag and only limited partitioning into gaseous and liquid phases.

P11. **Two water worlds:** We also do not think it is a mystery either – it is sometimes sold as such, though. It is how soil systems with, simply spoken, dual porosity work when the finer porous medium reaches saturation and no longer exercises a strong enough suction on the moisture in the larger pores, which in its bulk remains then on the outer limits or outside the Helmholtz-Stern double layer and is therefore subject to reduced flow resistance and can therefore move more freely and at higher velocities.

P12. **Dynamic root zone storage: Indeed both types of models have complementary value:** Again, we are not blaming any model types (nor the modellers for that matter), although this feeling apparently transpires in the reviews we get. We only intend to point out that conceptual models are also based on physical processes and that they are valuable at the appropriate scale.

P13. **A system evolves to greater efficiency:** We agree with the observation and that both types of models may add important pieces to the puzzle.

P14. **Darwinian thinking versus Newtonian thinking:** Thank you for your observation. We again want to stress that we fully respect and adhere to Newtonian theory. We also advocate the correct use of conservation laws. We only want to add to this. Darwin also did not contest Newton, he added a different dimension to Nature as we observe it. This is also what we do. And that does not mean that we extend the mechanism of biological evolution (by chromosomal replication) to catchment evolution. The process is completely different, although analogies can be easily drawn. We would argue that the climatic and geological predisposition of a catchment, i.e. the boundary conditions of the system, is in its original state the original "genome" of a catchment. Although, clearly catchments do not procreate in a biological sense, they keep on developing through the interplay of geological uplift, soil formation and erosion. This can be seen as a continuous circle of "life" in which the genome also evolves. In that sense, the genome is surely inherited after a new uplift event (posing for simplicity of the argument that this is a discrete and not a continuous process) and provides the basis for further evolution of the system. Similarly, can we distinguish genotypes and phenotypes? Well, is this not the objective of catchment comparison and classification efforts? All in all, it is an ecosystem evolving its capacity to cope with the environmental drivers and hazards, in a tendency towards optimality seeking a balance between productivity and survival. This is not curve fitting, but testing our model concepts against data.

P15. **Self-organisation causes simplicity:** Agreed, but also in the conceptual models there are multiple time scales: in interception, in transpiration, in infiltration, in groundwater replenishment, surface runoff, in groundwater drainage, in sub-surface flow on hillslopes, in channel flow in streams and rivers. It is not the idea that all these time scales are lumped; on the contrary. The trick is to identify the right processes in the landscape and to describe them by their appropriate storage-discharge relation (i.e. time scales). One can assume complete mixing, but also that is not necessarily a requirement of the concept (e.g. Hrachowitz et al., 2013).

P16. **FLEX-Topo really simple?**: We do not claim that the set-up is simple. It is definitely less simple than a single lumped model structure. Of course we can repeat the equations and their description in this paper, but it would bring it beyond the status of an opinion paper. Moreover, the equations and their relation are extensively discussed in the publications referred to in the paper by: Savenije (2010), Gao et al. (2014), Gharari et al. (2014) and Nijzink (2016).

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