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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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"Modelling catchments as living organisms"

We are very grateful to Remko Uijlenhoet for his very constructive and stimulating review. Here we present our replies to the issues raised and indicate how we will incorporate some of his suggestions in the revised paper.

Reviewer comment: The paper's topic and style are well-suited for a HESS Opinion paper. The style is relatively informal – actually I can vividly imagine the first author addressing an audience at a scientific conference (or a lecture hall full of students) with more or less the same words.

Author reply: We kept the language indeed quite informal to underline the subjective

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perspective of this Opinion Paper. We will, however, try to rephrase sentences that may come across as excessively colloquial as also requested by Reviewer 1.

Reviewer comment: It becomes clear from the Introduction that the paper deals with "catchment-scale hydrological" models. This is an important focus of the paper. Hence, I suggest this should be reflected in the paper's title.

Author reply: We agree and may change the title to "Modelling catchments as living organisms" (see also reply to reviewer 2)

Reviewer comment: The abstract does capture the reader's attention, but actually reads more like (part of) an introductory section. I propose to shorten (and thereby strengthen) it significantly.

Author reply: We agree. We will shorten the abstract in the revision to give it more focus

Reviewer comment: Introduction, first paragraph: I would have expected a reference to James ("Daisyworld") Lovelock's Gaia hypothesis. Perhaps the authors omitted it deliberately?

Author reply: The Gaia hypothesis is without doubt a very appealing and elegant attempt to characterize the world as a system of interdependent agents. Yet, there is little observational evidence that actually supports its main feature: that organisms not only stabilize but actually positively influence the system to improve the conditions for their own (or life's) development (e.g. Kirchner, 2002,2003; Ward, 2009). As far as we understand, Tyrell (2013) suggests to reject Lovelock's original strong definitions of Gaia, in favour of weaker definitions that replace homoestasis or improvement with co-evolution. In other words, this is a definition that states that different agents of the system do well adapt to the system and also influence the system through feedback, but without necessarily and systematically stabilizing or improving the system (which however can happen incidentally). As we think that what is happening in catchments

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rather reflects these latter, weaker definitions and not the original one, we chose not to refer to the Gia hypothesis here in order to avoid misunderstandings.

Reviewer comment: Section 1.1: In discussing drainage patterns, a reference to a book such as Rinaldo and Rodriguez-Iturbe's "Fractal river basins" would have been appropriate. Complexity in the subsurface was for instance addressed in Marc Bierkens' PhD thesis "Complex confining layers" and subsequent papers.

Author reply: Agreed, we will add and discuss these references.

Reviewer comment: Section 1.2: "This is Darwinian thinking, alien to the purely mechanistic, Newtonian philosophy on which much of our state-of-the-art modelling concepts are based." – there are of course (mechanistic) crop growth models (pioneered in the early 1970's by C.T de Wit and colleagues in Wageningen). Do the authors also consider such models to be "Newtonian" rather than "Darwinian"?

Author reply: That is of course a very good question. We think that this distinction can only be made with respect to the modelling domain. In other words, what was a model designed to do? If it does have the potential to reflect changes in its boundary conditions and the associated feedbacks, instead of operating with constant boundary conditions, then we would think that such a model operates in a Darwinian way.

Reviewer comment: Section 1.2: "Hydrological systems at all spatial scales, from the plot to the catchment scale, rather need to be understood as meta-organisms" – this again points towards Lovelock's work I think. In this section, the authors are critical about the "current generation models [...] mostly built on the foundations of time-invariant system boundary conditions". Although such models may indeed deprive us of developing a better understanding of what drives the change and thus of the systems' future trajectories", they may still serve a (practical) purpose, such as flood (or drought) forecasting. Of course, not all models (have to) serve the "higher" objective of advancing the science of hydrology. On the other hand, also "physically-based" models based on (coupled) partial differential equations may play a useful role in tackling

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specific questions in scientific hydrology.

Author reply: For the link to Lovelock's work please see reply above. For the rest we fully agree with the reviewer. Many of our models serve as useful tools for, in particular short term forecasting (see also replies to reviewer 1), and "physically-based" models are highly valuable for many applications (see also replies to reviewer 2). We will clarify this in the revised manuscript.

Reviewer comment: Section 2.1: The authors take the catchment as their spatial model domain. However, the horizontal and vertical extents of these domains may differ largely from one (sub)catchment to the next. How do the authors deal with the aspect of (spatial) scale in their modelling approach? How can model parameters identified at one particular scale (catchment size) be transferred to another (even if the general conditions of climate, soils and vegetation are comparable)? Another aspect the authors pay relatively little attention to is the human influence on catchment behaviour. Catchments, in particular in lowland areas, do not only (or necessarily) reflect the (natural) co-evolution of climate, landscape and vegetation. How is the modelling framework advocated by the authors able to deal with such human influences as polders, dams, irrigation, drainage, etc.?

Author reply: Transferability. This is indeed an unsolved problem. We think the basis for finding a solution needs to be catchment comparison studies and similarity frameworks. As one step into the direction of improving model transferability without the need for parameter regionalization, we recently published a paper (Gao et al., 2016, WRR) that illustrates the advantage of our approach: without the need for recalibration or statistical parameter regionalization, the transferability of a model based on landscape classes and water balance inferred Sumax, considerably improved compared to a standard 3-box conceptual model. Results from catchments that span 3 orders of magnitude in catchment area so far suggest that water balance derived estimates of Sumax are scale-invariant (Gao et al., 2014, GRL). Human influence. The way hydrology and society interact in a co-evolving way is part of the new 'science' of socio-

hydrology (Sivapalan et al. 2012, doi: 10.1002/hyp.8426. Of course socio-hydrological systems will also evolve towards self-organization, but the rate and proportion of human influence may very likely be too high for the relatively slow environmental processes to adapt and reach dynamic equilibrium. In practice, we think that human influence can only be incorporated if the actual disturbances are known and can be quantified. After such a model "reset" the system then may again be assumed to converge towards equilibrium (e.g. after deforestation; Nijzink et al., 2016, HESSD) until the next disturbance occurs.

Reviewer comment: Section 2.2: When the authors discuss the "co-evolution between climate, ecosystem, substrate and hydrological functioning", what aspects of climate do they have in mind – only precipitation and temperature (or solar energy), or also atmospheric composition (e.g. CO2)? The latter, although often disregarded in hydrological models, is known to affect plant transpiration. Regarding the "emergence of patterns", have the authors read Per Baks' "How nature works"? I am sure they have. The late Per Bak also claimed that the complex behaviour we often observe in physical (including living) systems does not necessarily require complex models to be mimicked.

Author reply:

Co-evolution. This is an excellent remark. We are in fact thinking of all of these effects, as they all influence hydrology and vice versa. Yet, we are a far way off being able to describe all the relevant processes and maybe more importantly their individual feedbacks in a way to implement them in meaningful models.

Emergence of patterns. Thank you for reminding us of Bak's work on self-organization that indeed describes in detail what we mean – this is spot on. We will include a suitable reference.

Reviewer comment: Section 3: Do the authors consider the VIC model, which they refer to (P.13, l.8), to be a physically-based or a conceptual model?

Author reply: Good question. We would define it as a physics-based conceptual model. Probably, as a community, we should move away from this duality physical-conceptual, because all models are to some extent physical and to some extent conceptual. There is a wide range of shades in the spectrum. Please see also the comments of reviewer 2 and our replies.

Reviewer comment: Section 4: “At the present level of technology there is still considerable uncertainty in the estimation of E, P and W time series” [determined from satellite information] – but don’t most satellite retrieval algorithms for E employ conceptualizations of the (eco)hydrological functioning of the land surface, which are not necessarily consistent with the modelling framework proposed by the authors? In other words, are the methods currently employed to estimate E from remotely sensed radiances fully physically based?

Author reply: We agree and there remains considerable work to be done. For the E-product we would only use those products that do not assume a hydrological model for soil moisture feedback. In the recent publication by Wang-Erlandsson et al. (doi:10.5194/hess-20-1459-2016), we made use of three products: the MODIS Reflectance Scaling EvapoTranspiration (CMRSET) at 0.05 degree (Guerschman et al., 2009), the Operational Simplified Surface Energy Balance (SSEBop) at 30" (Senay et al., 2013), and the MODIS evapotranspiration (MOD16) at 0.05 degree (Mu et al., 2011). In addition, a recent publication by Nutchanart Sriwongsitanon et al. (doi:10.5194/hess-20-3361-2016) indicated that the existing NDII product could be a powerful indicator for the root zone soil moisture storage. It is reasonable to assume that that the E, P, NDII and W products will become better in the future and that it may be a good idea to think of how these products can be used in future modelling applications.

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