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

Anonymous Referee #1

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In this paper, the authors claim that the current models are not physically based unless the model contains the characteristics of "an active organising agent", which is the ecosystem. They therefore rely on the Darwinian theory which states that systems try to optimize themselves such that their potential for survival increases. This is indeed true for all living organisms in an ecosystem, which keep on evolving due to the interaction between both the organisms themselves as well as between the organisms and the surrounding abiotic environment, where latter may be changing, albeit through climate change or human impact as land use change or use of pesticides, ... However, models do not work this way. Models are human inventions trying to describe nature in a formal language (dictated by mathematics), but remain inevitably with shortcomings as nature, due to its stochastic nature, cannot be captured in a few formulas. Increasing the knowledge on processes in the model makes the model more complicated and

often leads to numerous parameters that cannot be quantified through observations and require calibration. Furthermore, some equations used are extrapolated from the lab-scale to a model-scale of several meters to sometimes tens of kilometres, for which one could doubt whether the same type of equation remains valid if pixel-averaged values are used, yet, many papers can be found in literature where these equations (e.g. the Darcy equation) seem to behave well as the model outcomes seem to match the observations quite well. Taking an equation developed at the lab-scale to the grid-scale could be considered as a way of conceptualisation. Therefore, its parameters should therefore be interpreted with great care, even though the model is called 'physically-based'.

The fact ecosystems try to optimize themselves, in a Darwinian way, is, according to me, not a reason to state the models should be wrong. It is true that, if the system is changing rapidly, then the model's parameters may have to be changed in order to mimic the ecosystem's behaviour, but that's merely a calibration exercise that should be performed as soon as the model predictions (mean, variance, ... or extreme behaviour) start to deviate from those of the observations. Such exercise should be done, irrespective of whether one uses a physically-based model, a conceptual model (which is advocated in the paper) or a purely empirical model. Any modeller should be aware that modelling a non-stationary system with a stationary system will result in a failure of the model if its parameters do not get updated (given that all hydrological processes are represented in the model). Conceptualizing physical properties, as presented in section 3 of this paper, is not solving this issue. The parameters of the individual stores need to be updated when the ecosystem changes. The paper does not at all address this, but presents the conceptual model as a solution for modelling non-stationary data.

In case the ecosystem is more or less in equilibrium, and thus stationary, then the problem of all types of models (physically-based, conceptual and empirical) becomes one of calibration, where e.g. root zone storage can be different depending on the plant species (several models take this into account: e.g. in CLM, only the moisture in the

top layer is available to small vegetation, while for other vegetation types, the top two or top three soil layers (reaching more than a meter depth) are available. Other models (e.g. SWAP) take into account a rooting depth, root depth distribution or even root growth. In theory, one should be able to make these data pixel specific. Making them temporally variable is theoretically possible as well, but both (theoretical) options make the modelling exercise infeasible. Not taking this into account leads to (marginally?) larger uncertainties in the model predictions.

The plea made in the paper of taking ecosystem evolution into account in the models is a correct one, the solution provided, and the advocacy of a 'Darwinian approach to hydrology' is not convincing.

While reading through the paper, I was very surprized, and disappointed, by: 1. The lengthy abstract including references. The abstract looks like a part of the introduction, and does not summarize what the paper is about.

2. The language that is used. It is very informal, very often purely spoken language ("But there is more to it" (page 5, line 7); "No need to try and describe the sub-surface partitioning zone, . . ." page 10, lines 13-14; "This is good news for prediction in ungauged basins" (page 14, line 18). "And what is wrong with empirical formulations" (page 3, line 17), ". . ., but what many "physically based" models do not see" (page 9, line 13) – true models do not see, but one shouldn't phrase it this way –, . . .

3. Sometimes, the text read as if it was the written down version of a presentation, where such statements are made, where statements may be made too bluntly, just to make a point. But, to me, such language cannot be used in a paper. Some examples where the authors make very blunt statements: (a) "hydrological models do an astonishingly poor job in . . ." (page 2, line 14). If so, then please prove this by demonstrating that no model ever could make predictions that were better than XXX (being an RMSE, NSE, . . .), where the XXX is a poor result. (b) "This inevitably results in models begin inadequate representations of real-world systems, haunted by frequently unrea-

sonable model and/or parameter uncertainties and thus unreliable predictions" (page 2, lines 16-18). What does this mean? That the majority of our model is completely off? That we cannot rely any of our model results as a tool in water management? I believe we definitely can make use of these models, and many warning systems are adequate because they rely on model results.

4. The fact that the authors try to convince the reader, but, at least for me, completely fail in trying to explain what is wrong with our models? Going through Darwinian theory, which the authors try to apply it to hydrological models, is not at all convincing. The arguments that are brought are very poor (to me, blood running in veins is not an example that is similar to the water flowing in a landscape, I don't see the similarity, and that's only one example). The fact that self-organization exists is true, but many models account for this, furthermore, some of these self-organizing processes are at time scales that are much larger than the average length of time series that are modelled (>100 year for the processes, to 10-50 year for time series, so, in the modelled time frame, the changes to the landscape may be very small, other impacts (such as land use change) will be much larger!).

5. In section 3, the authors try to explain why a conceptual model should and can do the job. However, this contains nothing novel, it is merely a description of a conceptualisation of physical processes as is commonly done in hydrology.

6. Stating that the fact that the baseflow demonstrates an exponential decay, and that groundwater is "organised" to flow to the river (page 13, lines 22-23), is, to me, not at all a fundamental question in hydrology. In fact, I cannot cite papers where this issue is being brought up as being a fundamental question. It is merely caused by the boundary condition that a river poses to the groundwater body. Stating that it may have to be attributed to the theory of maximum entropy production seems inappropriate if not wrong. In fact, any organized system has a low entropy. Maximizing entropy should not lead to increasing organization of flow lines. . .

7. Section 4 deals with the practical consequences. Actually, whatever is in this section has, to me, nothing to do with "Darwinian theory" but is merely

(1) a listing of some advantages of a conceptualization of hydrological processes.

(2) a demonstration of the usefulness of DEM info. The fact that DEMs can be used for discerning between different hydrological processes is well known (actually one of the most famous hydrologic models, i.e. the Topmodel, is based on this). So, again, nothing new is stated here.

(3) an alternative way of estimating a model parameter (in this case the root zone storage capacity). The fact that plant root depth depends on the climatological condition, is known as well. Many models indeed approximate the root zone depth using a constant value, and this is wrong, but, this is a simplification (amongst the many others) of the model. (Actually, all conceptual models are highly simplifying physics). Trying to estimate the root zone storage capacity from E and P, can be done as is stated, and this value can be used in a model, but, due to all simplifications, one might end up with a sub-optimal model if this value is used... Furthermore, what is the link between Darwinian theory and the calculation of root zone storage if the latter value is not "continuously" changing (as Darwinian theory considers systems to evolve to an optimum)? How should conceptual models take this into account? As far as I understand the paper, only once, E and P time series are used to fix the root zone storage capacity (not to evolve in time).

(4) an alternative way for estimating the recession constant of the groundwater reservoir by using GRACE data. But one could argue that for very small catchments, the recession found by GRACE data is not to the same as is found for the recession curves in the hydrographs of the catchment due to scaling differences. Furthermore, the idea of using GRACE for estimating the recession parameter of a conceptual model is not verified. It is only bluntly stated that the recession of Sg obeys the same (?) exponential function as the recession of the drainage network (page 17, lines 12-14). Such

statement at least requires a demonstration that the same (!) exponential function (or at least recession constant in this function) is indeed found.

(5) a plea for using remote sensing data as additional source of data. But what is "Darwinian" about this?

Each of the subsections in section 4 is not convincingly showing new insights that support the idea of a Darwinian approach to hydrological modelling.

To a certain extent, I can fully agree with the authors that fully fledged physically-based models are so complex to work with as they have too many, non-observable parameters and therefore too many parameters to calibrate that they do a poor job, while working with simple conceptual models that simplify the physical processes e.g. in a set of reservoirs, can provide much better results (at least if the model is used for predictions but not for trying to get detailed insight in the system: e.g. particle tracking cannot be done, but may be necessary if one wishes to figure out the source of a pollution...). This is actually the main message of the paper, but by making use of theories that are not suited for it, in order to try to make this point. Furthermore, the applicability of these theories on hydrology is not demonstrated with specific examples, while many statements are made bluntly, mainly because too often, spoken language is used instead of a formal written language.

Given the above comments, this paper seems the result of a wild and out-of-the-box thinking, without any proof or demonstration of its potential, and furthermore, the language used is inappropriate for a paper, I believe it cannot be accepted in this shape, but needs more work, making it a state-of-the-art scientific text (without the spoken language), including proofs of the statements that are made and making sure that the final sections (3 and 4) support the theories being argued in the first two sections. Finally, referencing to a poster, which is included as appendix, should not be done. Rather, the material in the poster, if relevant, should be included in the paper.