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Interactive comment on "Proposal of a lumped hydrological model based on general equations of growth – application to five watersheds in the UK" *by* C. Prieto Sierra et al.

Anonymous Referee #2

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This paper tries to apply a concept originating from a rather different discipline, i.e. theory of growth of systems, to conceptual hydrological modelling. In itself, I surely laud the effort to "think outside the box", and to approach the problem of hydrological modelling from a new perspective. As any applied science, some cross-fertilization between disciplines can surely shed new light on old problems and as such stir innovative thinking.

However, in its current stage, the manuscript suffers from several major issues, which touch upon both the presentation and the essence of the modelling idea. Despite the generally good and clear language, I found the manuscript difficult to read, and the

train of thought hard to follow. I have tried to pinpoint several reasons for it:

1. The paper is very long and contains a lot of redundant information. The introduction and state of the art are far too ambitious and almost read like the introduction to a PhD thesis. Aspects such as the difference between conceptual and physically based models are known for decades, and are well described in several textbooks. At the same time, it is impossible to be comprehensive in this matter in the small space available. So, unless a very carefully condensation of the state of the art is made, such overviews have a tendency to become a slightly random in their content and superficial in their discussion. Instead, I would urge the authors to refocus the introduction, such that it directly zooms in on the aim of this paper: i.e. trying out a different conceptual approach to modelling the rainfall - runoff relation of catchments, based on theory developed for growing natural systems.

2. Related to 1, despite the rather long introduction and state of the art, surprisingly little argumentation is given as to why the authors think that growth equations may be applicable to the rainfall - runoff relation of catchments. After all, catchment hydrology behaves in a very different way than a typical growth processes. Growth tends to be governed by ecological laws and stable boundary conditions, while catchment hydrology of often determined by physical processes and rapidly changing boundary conditions. As such, the temporal variability and speed are also very different. While growth is often a slow process moving towards an equilibrium, catchment response is a quickly changing system that is continuously perturbed, and often exhibits threshold behaviour. Even if an equilibrium condition would exist, it is far from clear whether knowledge about this equilibrium would have any use in predicting the rainfall - runoff relation. It is therefore surprising that no case is made for the suitability of growth equations for hydrological simulation. In fact, some of the presented ideas make me think of recent approaches to use optimality theory in hydrological modelling (see e.g., Schymanski et al., 2008). While I am surely not an expert in this, the philosophy of this approach (and the concept of systems striving to reach some form of equilibrium)

seem compatible.

3. Yet, to partially answer my own question in (2), the ODE of what is called the "classical equation" (equation 6) is indeed very similar to the type of ODEs that are common in hydrology. Although the logical flow of section 3.2 is not entirely clear to me (why, for instance, should equation 7 result in equation 8?), the simplification of equation 6 results in various forms of linear and exponential reservoirs, which underpin the vast majority of conceptual hydrological models (e.g., the linear and exponential reservoirs)! The authors then extend the classic exponential store with the notion of the equilibrium discharge. While I have my doubts about the physical relevance of such a concept (see point 2), its definition in equation 14 essentially introduces some form of antecedent moisture conditions in their conceptual model. I see this as potentially the most interesting innovation of the paper, because few conceptual models deal with antecedent moisture conditions. However, in its current form, this aspect is lost in a large body of less relevant discussion.

4. The comparative test of the newly proposed model with IHACRES is weak. In my opinion, the main problem here is the performance measure. The value of the Nash Sutcliffe efficiency has been debated exhaustively in the current literature (see Schaefli and Gupta, 2007 as an entry point to the debate), but if one thing is clear then it is probably the difficulty of interpreting it as a performance measure, especially in the rather unusual way that it is combined with the bias (eq. 21). Indeed, the proposed model tends to perform slightly better in the comparison with IHACRES, but the difference tends to be rather small (Fig. 6). Would it not be much more informative to formulate some specific hypotheses of how the Verhulst model is expected to perform better than IHACRES, and then formulate specific experiments to test these hypotheses? Even if not, then the classic Klemes (1986) test in an uncertainty framework should be able to test both models for precision and accuracy, both of which are easy to grasp concepts. This, hopefully, would enable the authors to go beyond the claim that the model "yielded satisfactory results" (p9337, l19), which is hopelessly vague and unhelpful.

5. Lastly, the structure of the paper has to be improved. Not only does the introduction lack a clear case for the use of growth functions, but also the catchment description and the model testing set-up are described in the results section. These should be brought forward into a dedicated methods section.

To conclude, I found some potentially very interesting ideas in the manuscript, especially (1) the use of ecological principles to guide the formulation of a conceptual rainfall model model, and (2) the introduction of antecedent moisture conditions in such model. However, in its current state, these ideas are buried in a long and tedious manuscript that makes many assumptions and logical jumps that I found hard to follow. Therefore, I would strongly encourage the authors carefully rethink the storyline of the manuscript, strip down the content to the essence, and rebuild the story. Fundamental sections in this revision should be: - present a clear rationale for the use of growth function principles; - present a clear explanation of the assumptions behind their model formulation, and a discussion of its applicability (e.g., humid catchments); - present a clear strategy to test the performance of the model in view of the assumptions, principles, and inferred applicability.

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