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Hydrology and Earth System Sciences
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Discussions

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.

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RC C3967: 'Requested Review', Anonymous Referee #1, 11 Aug 2013 Printer-friendly Version Supplement

Comment 1: This paper presents the application of a fairly general model of complex systems to the problem of simulating streamflow. The paper itself is poorly written and does not motivate the study in a meaningful way, however the ideas are new as far as I know. The authors draw some interesting parallels between parameters of the growth model and certain conceptual hydrologic theories, but in the end the model

is only applicable to a limited set of conditions (humid environments), and then only some of the time (regimes of rainfall excess). The authors promise to address limited applicability in the future, but at this point I am left wondering why I should look forward to these developments.

Answer 1: After both referee's reviews, it is clear that we have failed to present the ideas in a clear and meaningful way. However, we are also glad that both referees have found interesting and new ideas that encourage us to improve the manuscript following their guidelines. We are going to rewrite the paper completely removing redundant information, rethinking the storyline of the manuscript and emphasizing the interesting ideas buried in the actual manuscript. Regarding your concern about the limited applicability of the model, we have to point out that so far, we limit the set of conditions to humid environments because we have only applied the method to this type of watersheds, which is the type of watershed we usually work with in the North of Spain and UK. Our intention is to present this model and prove its good performance on humid environments and leave possible extensions to other watershed types for future research. We will clarify this issue in the future revised version of the paper.

Comment 2: The demonstration of the proposed model shows that it may be more resistant to overfitting than IHACRES, however the demonstration is not large enough to draw general conclusions and there is no analysis of significance. The analysis of parameter interaction is exceptionally poor and the analysis of parameter identifiability is only marginally better. The authors do not motivate their model as filling any particular lack in ability of current methods, and note that in its current form it is not suitable for general application. I do not see any science here, where is the question that needs answered by the proposed solution?

Answer 2: The main objective of the paper is two fold: i) to present an approach of the problem of hydrological modelling using a new perspective from a rather different discipline, i.e. theory of growth of systems, and ii) to show its performance on humid watersheds. So, even though we do not fill any particular lack with respect to current methods, in our opinion this is also science, especially considering that we achieve better or equal results than IHACRES using less parameters. We will also clarify this matter in the future revised version of the paper.

Comment 3: I recommend major revisions because I don't see any errors in the math or the logic, and the idea is novel and may have potential (as discussed by the authors in their conclusion) in the future. But I hesitate to offer to review this paper again because the authors have failed to spark in me any interest in their subject or results. However, if four major improvements were made, it could be worth taking another look: 1. Motivate the model as (i) filling a gap in our understanding or ability and (ii) by explicitly illustrating a conceptual link between watershed behavior and all parts of the model.

2. Show convincingly that the model has some advantage, perhaps due to resistance to overfitting during calibration. This will require a larger set of test experiments, and a more robust investigation of parameter interaction and identifiability.

3. Address the issue that it is only applicable to watersheds in humid climates.

4. Reorganize the manuscript so that it presents a logical flow of ideas.

Answer 3: We would like to thank the referee for the opportunity to revise the manuscript. In our opinion, we could motivate the model to make it interesting for the audience of the journal, show its potential to watersheds in humid climates, its future applicability to other type of watersheds and improve the logical flow of ideas.

Comment 4: Basically, the argument made in the paper for this model is unconvincing. The authors should use the introduction to motivate us to conceptualize watersheds as being isomorphic with growth models. They do a good job of drawing connections between certain model parameters and hydrologic phenomena but the motivation for the model itself is somewhere between completely missing and severely under-developed.

Answer 4: The reasons why growth models are able to reproduce reasonably well the

behaviour of many complex systems at an aggregate level are essentially of an empirical nature. However, it is true that the isomorphism between a watershed and a general system must be clarified and explained in more detail in the paper. We argue that river discharge behaves as a growth variable, in the same way as, for example, the total population of bacteria in a nutrient-limited environment. A watershed is "fed" by the rainfall and, at an aggregate level, the total discharge at the outlet acts a surrogate (with a certain delay) of the level of saturation of the catchment, which governs the amount of rainfall that turns into discharge (effective rain). It can be conjectured that the "S-shaped" reflects the balance of two opposing forces. On one hand, a wetter watershed increases the proportion of effective rainfall and reduces the travel time to the outlet, potentially due to the fact that new runoff flow paths grow geometrically as they interact among them. However, this process is limited by physical and climatic variables: an obvious limit is that discharge cannot exceed the precipitation minus the evapo-transpiration, which is equivalent to a time-dependent capacity in dynamic systems jargon. These and other homologies, with new references linked to the proposed model, will be addressed in the new version of the paper.

Comment 5: The introduction (both section 1 and section 2.1 – I cannot tell any distinction between material presented in these two sections) meanders through a history of hydrologic modelling, touching on many diverse topics that are unrelated to the contribution of this study. For example: the model does not help with the problem of ungauged basins, it does not provide insight on the philosophy of an "optimal" parameter set (nor does it allow us to bypass this issue), and no multi-criteria objective functions are used in the application study. All of these topics apparently lead into the motivating sentence at the end of paragraph 15 on page 9312: "The problem of the . . . determination of the correct structure of a model is one of the major challenges in hydrology." This apparently means, to the authors, that we should address the problem by proposing a plethora of new conceptualizations. My question is; "what gap in our understanding does this model fill?" The authors note this on page 9315 paragraph 5, but fail to outline their "modeller's objectives".

Answer 5: We will make the introduction and state of the art sections less “meandering” and more focused. It is expected that this model can eventually help with the problem of ungauged basins, since its high parsimony make it potentially more suitable than other existing models for regionalization, and less prone to equifinality. We do think that new conceptual models, based on a somehow different set of assumptions, can help think out of the box (literally, since most lumped models are based on boxes or reservoirs). We will better clarify the “modeller’s objectives”: to present and efficient, compact and useful model to practice hydrology in a bounded set of environments.

Comment 6: In the introduction, just tell us what you are going to do and why, and dispense with the history of hydrologic modelling and description of issues which are not related to your methods or results. The real meat of what we want from an introduction has been replaced by trivia.

Answer 6: As expressed in answer 5, we will suppress all irrelevant information, focusing on the original content of the paper.

Comment 7: Section 2.2 is a history of the mathematical method with no relevance to a hydrologic application. Your contribution – which is the application of an existing method to a different problem, requires a motivation for this application, not a detailed history of the method in diverse fields. As far as I can tell, the only sentence with any relevance to the present study in section 2.2 is the last one about Chow’s use of the s-hydrograph – the resemblance that is referred to, however, is not explained in any way. If you were to motivate the use of this model by some science question then add some of this history in the paper - that would be ok. But history does not substitute for a motivation or science question.

Answer 7: We will focus on the history of growth models which are more relevant to the findings in the paper, trying to explain the reasons why there is an isomorphism between the discharge at the outlet of a watershed and other state variables used in growth models. We will delve into the similarities in the processes and interactions tak-

ing place in different complex systems where growth equations have been applied, and hydrological systems. In particular, there are clear and explicit similarities between the proposed models and a family of delay differential equations used in biological models for single species dynamics: Hutchison’s equation, Nicholson’s model, etc (see, for a general review, Delay Differential Equations and Applications, Arino et al., Springer, Berlin, 2006).

Comment 8: Section 3 is the first time we actually get to material that is at all relevant to the paper. In this section, which claims to describe the homology I am looking for, the only justification for the model is that growth models describe time-varying behavior, and streamflow is a time-varying quantity. There is no further justification for the “unbounded function and bounded one”. Nor for the “limiting factor” except to note, without reference, that “it is consistent with the observation that a watershed will tend to generate a constant discharge.” First of all, what observation? Second, all you are saying is that watersheds are dynamic systems with a steady state. So again, I argue that almost any dynamic systems model will be able to represent time-varying quantities and steady-state behavior. What do growth models give us that we don’t already have?

Answer 8: The concept of equilibrium discharge, linked to an equilibrium runoff coefficient, is one of the pillars of the proposed model. It is true that the word “observation” should be changed to “assumption” that a constant and long enough rainfall, under stable evapo-transpiration conditions, will induce a constant discharge. We agree, as the reviewer says, that “watersheds are dynamic systems with a steady state”, but we go beyond that, proposing a specific model structure that is suitable for hydrological systems. Growth models, linked with an expression for the dynamic equilibrium discharge, are new and convenient to characterize the behaviour of, at least, some types of watersheds.

Comment 9: Further, I do not see how the argument of steady state leads to equation (3) – there is no difference between P and PET in that equation, as discussed in the leading text. The multiplication between Q and Qeq is not motivated other than to state blankly that this is how growth models are.

Answer 9: Equation (3) simply states that discharge variations come from the balance of two opposing forces: a birth factor (f1) and a decay factor (f2). The former is only governed by the size of the population (Q), while the latter implies a capacity factor depending on exogenous variables, i.e. the rainfall and the evapotranspiration; the shape of the capacity function is described latter. However, we agree that the justification of the proposed expression of Qeq needs more elaboration, which will be included in the revised version of the paper.

Comment 10: Now, the motivation for the Qeq parameter expression in (4) is good! There is hydrologically-relevant explanation as to where this comes from. I would like a reference for this c_eq idea though, listed in the sentence above equation (4).

Answer 10: The notion of an equilibrium runoff coefficient is inherent to many classic methods in an event-based hydrological framework, as the curve number (CN) approach to evaluate net rainfall or the rational method. For instance, the parameter CN reflects both the evolution of the runoff coefficient for different rainfall depths, and its maximum attainable value (Chow, 1988). However, we are not aware of previous expressions for a dynamic Ceq, used in a continuous modelling framework. We will include references about the Ceq idea listed in the sentence above equation (4).

Comment 11: In section 3.2.1 the authors are still trying to draw a connection between the growth model (and its classical quantities like carrying capacity and population) with hydrology. But again, no explicit isomorphism is explained. What is system saturation here, and is it different than hydrological saturation discussed in the previous section? (I know the answer, but it is up to you to lay this out for the reader.) The point is, by section 3.2, we should already be speaking in terms of hydrology considering section 3.1 was supposed to be all about isomorphism (or homology). Paragraph 15 is all redundant material.

Answer 11: As already state, we will elaborate further on the isomorphism issue, and paragraph 15 will be suppressed. All references to the model structure outside the watershed realm will be extracted to section 3.1, leaving for section 3.2 strictly the description of the new hydrological model.

Comment 12: Related to organization of the experimental sections, First of all, section 4 is titled incorrectly; it is not results. It is a set of experiments including methods and results. It would be a lot easier to understand and to absorb the implications of your experiments if you were to lay out the methods first to give us some idea of why we are going to run this particular collection of experiments, and what we expect to learn. The type of disorganization found in this section is why we typically use a methods section and a results section – sometimes it is ok to not take this conventional route, but this was not one of those cases. From the introduction paragraph in this section I have no idea where we are going to go, or why.

Answer 12: We will add a new “Method” section, explaining our aims and ways of attaining them.

Comment 13: For example, why are the experiments not homogeneous in their presentation? The introduction paragraph of section 4 only tells us that we will investigate 4 basins, so if you are going to treat the four basins differently, the reader needs to know how and why before we get started.

Answer 13: All basins (5) are treated with the same approach and methods. The first two basins are analyzed in more detail, i.e. using several subseries, due to the fact that this is the procedure followed by the benchmarking cases. Basically, we have mimicked the type of analysis presented by the referred papers, using Ihacres.

Comment 14: From the very beginning of the paper tell us explicitly that the model you develop is only applicable to humid climates, and in the abstract tell us that the model is only capable of reproducing behavior related to excess saturation. These are very severe limitations. I understand that you want to present an “approach” but from a practical perspective this approach is not yet very promising or very general, so it is necessary to stress that from the beginning. If it were me, I would wait to publish until I had a viable model. This is most definitely still just an idea with little to no practical value and I am personally of the opinion that it does not warrant a publication at this point. I know that you disagree, but my question is why don't you finish building your model and resubmit once it's viable – or at least a complete idea – or at least explains something that we didn't already know?.

Answer 14: As pointed out in previous answers, we have only applied the model to watersheds in humid climates because that is the type of watershed we usually work with in the North of Spain. This does not mean that it is only valid for this type of watersheds and it is a subject for further research. Besides, we disagree on the judgment that this model has no practical value or that it is not viable at its present state. It is indeed fully viable and finished as presented in the paper; the ideas are complete, although they probably deserve a better elaboration. As a practical tool, the model is a competitive and very convenient option for lumped hydrological modelling. Furthermore, it introduces a few concepts not previously found in the hydrological literature. We will clarify this issue in the future revision of the paper.

Comment 15: Page 9328, related to "however . . ." and "strictly speaking . . .": I agree with the authors that Figure 3 tells us very little about parameter interaction. A better illustration of parameter interdependence would be an OF response surface – actually three of them, one for each parameter pair. In addition to response surfaces, we need to see joint parameter distributions, not just "optimal" parameters. A simple study showing different "optimal" parameters over multiple calibration periods does not really address the issue of parameter identifiability.

Answer 15: This paper focuses on the presentation of a new conceptual model and an analysis of its performance in five watersheds previously studied. A comprehensive analysis of parameter identifiability is out of scope and only some preliminary results on this topic are supplied. Figure 3 will be taken out from the new version, since it

belongs to a topic which is intentionally not addressed in this work.

Comment 16: Page 9313 paragraph 15, first line: Darcy was before the mid-twentieth century.

Answer 16: We will clarify the temporal reference, it refers to numerical models.

Comment 17: Page 9315 paragraph 25: Bayesian methods do not, in general, make use of the concept of a Pareto optimum. There may be certain techniques founded on Bayesian principles that do, but these would be the exception, not the rule. What exactly is the class of techniques you are talking about here – Bayesian or multiobjective? And why is it relevant to your paper? In addition, please note the relevant page(s) in your book reference (Beven 2012).

Answer 17: The Pareto optimum refers to multi-objective methods, not to Bayesian. We agree that the reference to Bayesian methods is not relevant to the paper, but the multiobjective approach is linked to the objective function we have chosen to calibrate the model. We will add the exact references to Beven's book.

Comment 18: Page 9316, first line and on: Related to the words "this type" - what non-statistical approach have you been talking about? Further, all Bayesian methods – by definition – allow for the consideration of epistemic uncertainty. Further, there is no such thing as an "epistemic nature of a residual", a residual is a measure of distance between a two estimates, and may be a result of the fact that there is uncertainty in either estimate. The uncertainty may be epistemic in nature, but the residual is simply a measure of distance.

Answer 18: It should say Bayesian instead of non-statistic. The "epistemic nature of a residual" will be replaced by Âńthe epistemic nature of the errorÂż

Comment 19: Page 9317 paragraph 15: "Savageau (1980) . . ." This sentence seems promising but really gives the reader no information. After reading it I know nothing about Savageau's argument other than that the behavior of complex systems arises due to the interaction of system processes and components – which is an obvious truism. What does Savageau do with the growth model and why should I care? Further, why do I care about single variable vs. multi-variable growth models – what does it have to do with modeling hydrology?

Answer 19: Savageu draws on system theory to deduce a general expression for growth models, including the logistic one. According to him, the macro-scale evolution of complex system can usually be described by the interaction of a few governing variables (from the whole set of state variables that are needed to fully describe it); the choice of such leading variables make simple growth models an appropriate tool in many cases. We will further elaborate on Savageu's approach and its relation with the proposed model; this is related to the aforementioned fact the justification of growth equations as hydrological tools is poorly substantiated in the paper. The same can be said about multi-variable growth models: they give clues about general growth models as a potential set of structures valid for hydrological purposes. The paper mentions that the logistic model is the simplest structure of this type that can be applied in the hydrological realm, but does not explicitly show how other growth equations, including multi-variable, can be also used. In the reviewed version, we will explain in more detail what we exactly mean by a general framework of growth models valid for hydrological systems, and give examples (for instance, the hydrological version of Savageau's equation).

Comment 20: Further, about "what all applications have in common is gradual variation . . . over time governed by a limiting factor." So you are saying that you choose a growth function simply because it simulates smoothed time-varying behavior with a limiting factor. So do an uncountable number of other models – these are basic quality of most dynamic systems models. Regarding the limiting factor: what does that have to do with hydrology and why do I care?

Answer 20: We choose growth functions because we claim that they are able to reproduce the aggregate behaviour of hydrological systems. Other dynamic system mod-

els, outside the framework of growth models, can also work and many of them are described in the literature. The existence of a dynamic limiting factor is the key assumption that allows extending growth equations to watersheds. The paper intends to convey, seemingly not as clearly as we had wished, that the existence of an equilibrium discharge, given by a function of precipitation and evapo-transpiration, gives us the chance to think of a watershed as a growing and decaying "organism".

Comment 21: Further, what is an equation of evolution and what does it have to do with the subject of the paper?

Answer 21: Following the argument from the previous comment, an equation of evolution of such "organism" (the watershed) is equivalent to a fully operational hydrological model.

Comment 22: 9319 paragraph 20: "especific"

Answer 22: Thanks for pointing out this typo, it will be corrected in the revised version of the paper.

Comment 23: Page 9321: It would be good to give a short explanation why (8) is the desired behavior of a watershed with Qeq=0. Since this is a cornerstone in the development of the model, it would be good to let the reader intuit the need for this form rather than just refer them to a reference.

Answer 23: Equation 8 is the general expression for a non-linear reservoir with no inflow, also known as the Horton-Izzard equation. Different values for B can be obtained based on different assumptions: B=1/2 for orifice flow (i.e, assuming the watershed is like a barrel with a hole at the bottom); B=1 reflects a Hortonian flow (basin as pure aquifer). In the revised paper we will provide an explanation of the desired behaviour for the evolution of a drying watershed, which can be justified drawing on several physical arguments.

Comment 24: Page 9321 paragraph 5: of course particular cases occur if you choose particular values of certain parameters. I think you mean that certain special cases of (8) have been named and studied. Furthermore, I think some equation numbers are wrong here and in the next paragraph. Also, for (7) to take the form of (8) when Qeq=0 and r defined by (9), it is necessary to use the convention that 0/0=1; this should be stated. .

Answer 24: We thank the reviewer to pinpoint an error in the text references to equations 8, 9, 10 in paragraphs 5 and 10 of page 9321. It is also true that 0/0=1 must hold in the expression of Qeq in order to yield equation 8. Bearing in mind the general expression for Qeq=P*ceq, it is straightforward that Lim P->0 (Qeq/Qeq)=1. This will be stated in the revised version of the paper.

Comment 25: Page 9321, paragraph 10: Seems that you mean equilibrium discharge instead of maximum obtainable discharge. What if we are in the falling limb of the hydrograph? This is the first time "maximal obtainable discharge" has been discussed and I do not see this concept in any of the equations.

Answer 25: Maximum obtainable discharge is equivalent to equilibrium discharge, and is used as a synonym. The latter expression is preferred, and has been generally used in the paper, since it does not denote that the actual discharge is always lower than the equilibrium discharge, which can be zero during dry spells. The level of precipitation and ET defines saturation or a load capacity for the watershed at each moment, and the systems tries to adapt to it, either "growing" or "decaying"

Comment 26: Page 9322: Move the "Eq 4" reference to follow the "As has already been discussed" clause. More importantly. Now we are back to the definition of the equilibrium rate. There is no logical flow in this article – we jump back and forth between pieces of the model. It should be structured to:

(1) Lay out an argument for using this model including explaining isomorphism (or homology). (2) Define the model. (3) Define the parameters and their conceptualizations.

Answer 26: We thank the reviewer again for pointing at structure and presentation jumps and repetition. We will follow his/her advice on the new flow of presentation of the concepts.

Comment 27: Please avoid jumping back and forth between concepts unless absolutely necessary. I see no reason why this Qeq related to c_eq concept needs to be discussed in technical detail in two separate sections of the paper.

Answer 27: We admit that inconsistence and will work thoroughly on it in the new version.

Comment 28: Furthermore, at this point (Page 9322 paragraph 10) we have no explanation as to why we would want to extend the concept of the Budyko functions. This goes back to pg 9313 – you said there also that we would extend these concepts but not why (I see that your goal is to make this parameter local in time, but this is not explained). Specifically, the distinction between "mean conditions" and instantaneous conditions needs to be made explicit. Again, I recommend putting all this c_eq material in one single discussion and laying it out succinctly from beginning to end.

Answer 28: True, the recommendation is welcome and it will be done as indicated by the reviewer.

Comment 29: Page 9324, last sentence of paragraph 8: "have little sensitivity to" or "be relatively insensitive to".

Answer 29: Thanks for the comment; we will follow your suggestion in the revised version of the paper.

Comment 30: Page 9324, paragraph 15: Is this the same \tau as in equation (17)? This delay parameter should also be dependent on topography, size of the watershed, vegetation, soil types/depths, and other characteristics of the vadose zone.

Answer 30: No, it is another parameter, different from the memory time span used in the equilibrium discharge function. This \tau is a delay parameter, accounting for the travel time of the net rainfall to the outlet of the watershed. It is expected to depend on the river length, steepness and soil properties (i.e. as a time-to-peak in a linear-system approach). We will clarify this issue in the revised version of the paper.

Comment 31: Page 9325, paragraph 5: How is the logistic equation used to integrate anything? Why are times notated as subscripts here but as functional inputs elsewhere?

Answer 31: What we mean, and this will be clarified in the revised version, is that we use the exact solution of the logistic equation as a finite-difference expression to solve numerically the proposed model, assuming that the precipitation remains constant within each time step (zero-order hold approximation in control theory jargon). Note that we use subscripts only for the numerical finite-difference solution of the proposed equation.

Comment 32: Page 9325 paragraph 20: . . .the mathematical "nature" . . . Furthermore, I am not sure that I agree with the argument in this paragraph – or at least I don't see it. My interpretation is that this is not an SDDL because (1) it is a deterministic equation (not stochastic), and (2) Qeq is dependent on P, not Q, so the increment of the process does not depend on past values of the process. Related to (1) you have not considered uncertainty at all in formulating the model – only given lip service in the introduction. Please do not introduce and suggest treatment of a model that you do not develop or apply. Related to (2) At most you might consider it an SDE (if it really was stochastic), but not an SDDL.

Answer 32: As long as P(t) is conceived as a noise, the proposed logistic model becomes a classic SDE (even with analytical solution for a Gaussian P) , although in this particular paper we have not explored the implications of adopting such approach. In the same way, if the constant tau is considered as a function of present discharge, implying that a watershed "flows" faster when there is more water in it, the SDE becomes a delay differential equation, also with a stochastic term. We assume tau=constant,

and so the delay treatment becomes trivial, but it is not if tau=F(Q). For daily data in catchment of this size (100-1000 km2), there is not enough discretization to identify the law that governs the variation of tau with Q; however, if we had hourly discharges and precipitation, we could analyze that.

Comment 33: Page 9326: So even though there is all that talk of multi-objective and application-specific optimization in the introduction all we do is fit a weighted residuals-based objective function. Notice that the "constraints" you mention are just the parametric form of the model, so this optimization is no different than any single-objective residuals-based optimization of any conceptual hydrologic model. The constraints are simply due to the rather involved numerical method for solving the master differential equation, they are not actually related to other modeling objectives. You have simply parameterized a model with three parameters and optimized those three parameters. Also, that is a very long description of Matlab's fmincon function does that lengthy description really tell us anything we need to know to understand your contribution or understand or reproduce the results of your experiment?

Answer 33: Regarding the constraints we mention, we refer to upper and lower bounds associated with parameters. We agree with the referee that we are just optimizing a single objective function dependent on three parameters, but as many other conceptual hydrological models, it leads to a non-convex problem with multiple local optima, that is why we use the combination of random initial solutions within the bounds and nonlinear mathematical programming solvers such as fmincon under Matlab. We also agree that this information does not help to understand the model or reproduce the results of the experiments, and that is why we will rewrite and reduce this part clarifying that any solver for global optimization can be used to get the optimal parameters of the model.

Comment 34: Page 9327 – Reference to Table 1: Caption in the table: this table appear to use three word for the same idea: intervals, sub-periods, and sub-series. Is there a difference between these three things? Please choose a single vocabulary and stick with it. It is ok, in the text, to draw connections with Littlewood's vocabulary, but don't do this in the table caption. Or if you feel you absolutely must draw this connection in the table caption, then the Littlewood reference (year) is necessary in the caption. You state in the text that the table uses the “same nomenclature as Littlewood.” I am confused. Also conflicting vocabulary in the text near this area.

Answer 34: We appreciate the message and will clarify this issue in the revised version of the paper.

Comment 35: In the table: What is the “#X”? What is the “#1-8”? What is “Mean #1-#8” and why is it below “#X”? and what is “#1-6” and why are the dates in parentheses? You explain the last one later in the text, but this information should be in the caption. Also, what is a “calibrated sub-period”? Tell us exactly what you did – don’t make us guess.

Answer 35: We will include this information in the caption.

Comment 36: Page 9328, “Fig 1 shows . . .”: median and quartiles of what? You still have not explained exactly what you did and at this point I have to learn from the figure caption that you mean and quartiles of “optimal” parameters in each of the 8 sub-periods. Also, on here, it would be helpful to mark the parameter values that result from using all of the data for calibration (last line of Table 1).

Answer 36: We will try to make this clearer in the next version.

Comment 37: Page 9328, related to Figure 2: You have chosen to illustrate a portion of the best-performing sub-period. Might is also be nice to see scatterplots – or some other explicitly comparative plot to get an idea of where, in the hydrograph, the errors in model estimates are large and small? The CDF plots are nice, but they say nothing about timing. Also, what is an “adjusted interval”?

Answer 37: We will include scatterplots to show the goodness of fit. Adjusted intervals = sub-periods = sub-series (see comment 34).

Comment 38: Page 9328 “that showed a linear correlation”: Why would you only con-

sider linear parameter interdependence – this is basically the type of parameter interdependence that it not interesting from an optimization perspective. Figure 4 and Table 2 are essentially redundant. My suggestion is that only the figure is necessary because the table is too dense to be meaningful. Just a suggestion.

Answer 38: The linear relationship is a result that has been detected, but there are not enough independent data to talk about parameter inter-dependence. We think this remark on the parameters correlation is not necessary in the context of the paper and blurs the key findings. We will follow the reviewer’s suggestion and remove table 2.

Comment 39: Page 9329 un-numbered section: Previously the authors have made references to Littlewood, but only discussed their methods in that context – not their results. While reading the previous section I often wondered why they don’t compare their results with Littlewood – here I see that they do, but I have had to read through a mess of material to get to the heart of the matter. Here is the first explicit description of Littlewoods methods – after a whole section that drew implicit analogies. The authors seem to be using “transfer function” (or TF) to mean a model, which is fine but why the redundant vocabulary?

Answer 39: Transfer functions are a sub-model within IHACRES, the one that routes net precipitation to the basin outlet. We will clarify it. It is also true that there is some confusion on the procedure used to evaluate the new model: most of the effort is put on the comparison of the IHACRES results, but there are also some other tests of quality not related to that. We will IHACRES a clear path of model evaluation, including both independent tests and benchmarking with IHACRES.

Comment 40: Page 9329 relate to Fig 6: what is a VERHULST 3param? Why is this name first appearing in this figure caption and legend? How many parameters does IHACRES have and how were they calibrated?

Answer 40: Verhulst 3param is another name for new logistic model (this will be changed in the next version, to be consistent). IHACRES has 5-6 parameters and several procedures have been used to calibrate them, depending on the application case. We will briefly indicate them in the new version of the paper.

Comment 41: In the text there are three different descriptions of the NS – as "Nash-Sutcliffe", as "the values of variance explained by the model" (Page 9327), and "coefficient of determination" (Page 9329). Perhaps it would be good to have one sentence that explains how you want us to interpret your evaluation statistic and then use consisted vocabulary to refer to that statistic. This is just an aesthetic issue unless I am wrong in interpreting the: coefficient of determination" as equivalent to NS (it is interpreted this way when the regression for the COD is slope 1, bias 0). Or are you now using a new evaluation stat – so that the 10th and 11th bars of Figure 6 are not comparable to all the others and Fig 6 has an incorrect caption?

Answer 41: Nash-Sutcliffe coefficient is equivalent to variance explained and coefficient of determination. These expressions are used as surrogates of quality of fit in the paper, and the inconsistence is a matter of vocabulary: all tests have been made using the same objective function.

Comment 42: I have no idea what experiments produced bars 10 and 11 of Fig 6. This is a completely separate issue from the fact that I don't know what evaluation statistic these bars plot (NS or a more general definition of coefficient of determination). It would be nice to split Fig 6 up into separate charts for calibration-period results and evaluation-period results. Really, the evaluation results are of primary interest and they are kind of lost amongst all the other data in this figure.

Answer 42: The meaning of bars 10, 11 in figure 6 will be explained in the next version of the paper (it follows Littlewood's paper). All tests, as already said, use the same NS measurement error. Figure 6 is oriented to compare results with IHACRES, and due to this we do not distinguish calibration an validation periods. The NS values obtained for calibration and validation are shown with detail for the Teifi river in table 2 and figure 4. For the rest of the rivers, the partial results have been omitted for the sake of brevity,

but all basins follow a cal-val procedure.

Comment 43: Page 9329, you refer to the "delay parameter" of IHACRES, but have previously told us nothing about any parameter of this model. This entire description of Littlewood's experiment is lost on me because you have provided no background on the IHACRES necessary to put this discussion in context. .

Answer 43: Once again, it has been tacitly assumed that the reader is familiar with IHACRES structure and parameters, which does not necessarily have to be the case. The IHACRES model includes a delay parameter which is not equivalent to /tau in the logistic model. This will be clarified in the revised version of the paper.

Comment 44: Page 9332: Using "data" from 1986 to 1989. What does "always starting and ending in a hydrologic year" mean? With respect to all experiments: what initial state value was used?

Answer 44: In UK, and also in Spain, it is customary to consider hydrological years starting on the 1st October (approximately the beginning of the rainy season). The initial value is taken as the measured initial value. This will be explained in the new version of the paper.

Comment 45: Page 9334: I am very confused by section 4.4.1. First of all, we are still in section 4, so please don say "as discussed in section 4..." Why is there a bold section header interrupting a single, continuous train of thought (i.e. about optimizing \tao)? I imagine that the rest of my confusion comes from the redundant use of notation \tao.

Answer 45: It should say "...as discussed in section 3.2.2". Probably the reviewer's understandable confusion stems from two facts that can be promptly corrected:

1- Title of section 4.4 should be: Analysis of some variants to the basic model. Probably, it deserves a section on its own (section 5) 2- There is no redundancy in the notation of \tao, there is a misunderstanding that should be avoided in the new version. \tao is independent and has nothing to do with the memory parameter \lambda in the Qeq function. It is true that both have time units, but \tao is in the order of hours and \lambda is about one month. In the basic model, we fix \lambda, and in section 4.4.1 we test the effect of treating it as a free parameter.

Comment 46: Page 9335, Section 4.4.3: now we age getting back to model development equations in the "Results" section.

Answer 46: Since we are testing some variants to the initial model, we check the influence of using a different equation (but with the same fundamentals) for the equilibrium discharge. We are not developing the model again; we are trying to evaluate the effect of changing some of its "pieces" within a stable and clear framework. This will be better explained in the revised version.

Comment 47: Page 9336, paragraph 10, last line is missing a word. Similarly, the first sentence of paragraph 15 has several grammatical errors.

Answer 47: Thanks for pointing out these typos. We will correct them in the revised version of the paper.

Comment 48: Page 9336, paragraph 20: where do you get the conclusion that the model is "well-conditioned for optimization?" You have not tested at all for equifinality – in no way do your experiments address this issue.

Answer 48: Although that conclusion might not be scientifically or formally proved, it is an evidence for us after fiddling repeatedly with the model. Figure 1 is yet another not conclusive but informative support of that judgement.

Comment 49: How is delaying a rainfall equivalent to interpolating to a lower timestep? It seems like the method you use for delay may require a smoothed (interpolated) rainfall time-series or may not. Furthermore, I still fail to see why this constitutes a delay equation. Also, why should we want a model that delays rainfall rather than explicitly recognizes transit time? The latter is a more intuitive conceptualization.

Answer 49: Any delay lower than the data timestep amounts to an assumption on how

rainfall occurred within the timestep, i.e. and interpolation. If we let \tau be a real number, we have to adopt a certain hypothesis on how the rainfall is distributed over the length of the data interval (one day in this case). It is a delay equation because the term Qeq is transformed into Q in a non-syncronous way. Such delay behaves as a characteristic time of the watershed and is related to the maximum of the cross-correlation function between P and dQ/dt; another possible definition of a characteristic time of a watershed is the cross-correlation between P and Q (this latter is roughly equivalent to the time-to-peak in a linear-system framework. The expression "transit time" is bound to a mechanistic metaphor (time=transit length / mean velocity) not used in the proposed framework.

Comment 50: Page 9338, paragraph 20: This topic is "the" object . . .

Answer 50: We have followed your suggestion.

Comment 51: Page 9338 Last paragraph: proved is a really strong word. What do you mean by an "infinitesimal scale"? At an infinitesimal scale we have quantum mechanics, not growth laws. This is the reason that conceptual models like growth models are – with 100% certainty – not "correct" models of the system and can never be "proved". These types of models are, and always will be, convenient conceptualizations – useful but never exact, never unarguable, and absolutely never to be taken as fact.

Answer 51: We refer to "infinitesimal" in the time-domain (dt->0), not in the space domain (quantum mechanics). We will make this sentence clearer: if it was proved that the proposed model equation still holds when dt->0, we can more easily investigate the dependence of model parameter on the temporal level of aggregation. This will probably help in regionalization tasks, where one of the main concerns is the variation of parameters with the time scale.

RC C4781: 'Review', Anonymous Referee #2, 10 Sep 2013 Printer-friendly Version

Comment 1: 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.

Answer 1: We thank the referee for the positive comment.

Comment 2: 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 pin point several reasons for it.

Answer 2: After both referees' reports, we agree that we have failed to present the ideas in a clear and meaningful way. However, we are also glad that both referees have found interesting and new ideas that encourage us to improve the manuscript following your guidelines. We are going to rewrite the paper completely removing redundant information, rethinking the storyline of the manuscript and emphasizing the interesting ideas buried in the actual manuscript.

Comment 3: 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.

Answer 3: We agree on this view, and according to it we will remake the initial sections with a more focused approach.

Comment 4: 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.

Answer 4: The reasons why growth models are able to reproduce reasonably well the behaviour of many complex systems at an aggregate level are essentially of an empirical nature. However, it is true that the isomorphism between a watershed and a general system must be clarified and explained in more detail in the paper. We argue that river discharge behaves as a growth variable, in the same way as, for example, the total population of bacteria in a nutrient-limited environment. A watershed is “fed” by the rainfall and, at an aggregate level, the total discharge at the outlet acts a surrogate (with a certain delay) of the level of saturation of the catchment, which governs the amount of rainfall that turns into discharge (effective rain). It can be conjectured that the “S-shaped” reflects the balance of two opposing forces. On one hand, a wetter watershed increases the proportion of effective rainfall and reduces the travel time to the outlet, potentially due to the fact that new runoff flow paths grow geometrically as they interact among them. However, this process is limited by physical and climatic variables: an obvious limit is that discharge cannot exceed the precipitation minus the evapo-transpiration, which is equivalent to a time-dependent capacity in dynamic systems jargon. These and other homologies, with new references linked to the proposed model, will be addressed in the new version of the paper.

Comment 5: 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.

Answer 5: As the reviewer points out, the logistic equation, in its basic version, is equivalent to a standard box-model with a non-linear (exponential) reservoir. Our contribution, apart from an alternative framework to justify the equation, is the inclusion of a delay factor and an equilibrium discharge function, including antecedent moisture. These two elements, we argue, make the logistic equation a stand-alone expression to predict discharges in a certain type of watersheds, while standard reservoir models would operate in two stages: calculation of the net rainfall (possibly using a non-linear

reservoir) and routing. There are clear similarities between the proposed models and a family of delay differential equations used in biological models for single species dynamics: Hutchison’s equation, Nicholson’s model, etc (see, for a general review, Delay Differential Equations and Applications, Arino et al., Springer, Berlin, 2006). We agree that we must make clarify and explain more thoroughly the rationale behind the equilibrium discharge, which we think is a promising concept. Besides, we also recognize that its relationship with a dynamic extension of the classic Budyko curves is not properly presented.

Comment 6: 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.

Answer 6: The approach to model testing used in the paper has been intentionally simple and is based on two tests:

1. Application of an objective function based on a linear combination of mean square error and bias, as performance measure. Our intention with this choice is to balance extreme and mean discharges in the evaluation of the goodness of fit.

2. Make a comparison with a popular lumped model (IHACRES). In this relative evaluation procedure, we stick to the same parameters used by the IHACRES's authors in the cited papers, which are the NS coefficients.

We still support the idea that the NS and the bias are the two single most meaningful indicators of performance for a hydrological model, only beaten by expert visual inspection; this is even truer if we consider some the most common applications of lumped models: estimation of extreme discharges and quantification of water resources in a watershed. Many sophisticated testing frameworks try to mimic expert judgment, but none of them has replaced simple functions of residuals, and the educated eye. In the revised version of the paper, we intend to separate different residual-distance measures (NS, NS-log, bias) instead of clustering them in a single objective function, and include some new graphs (for instance, scatter plots of observed vs. measured discharges) to facilitate visual inspection of the data.

Comment 7: 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.

Answer 7: We will improve the structure of the document, following the reviewer's advice. We will place a data and model testing set-up section (methods) before the results.

Comment 8: 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, 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. Fundamen-

tal 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.

We thank the reviewer's comments once again and will follow his/her suggestions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 9309, 2013.