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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydrologic system complexity and nonlinear dynamic concepts for a catchment classification framework" *by* B. Sivakumar and V. P. Singh

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We thank M. Sivapalan for his Short Comment. We are, however, disappointed with his comments. The only point M. Sivapalan makes, through his long-winded Short Comment, is that our proposal needs to be tested. Since this point is already made by all three referees, M. Sivapalan adds nothing through his Short Comment for improving our work and manuscript.

As we have discussed in detail in our "General Response to Guest Editor Comment (EC), Referees' Comments (RCs), and Short Comment (SC)," we have substantially

revised our manuscript in light of the various comments and concerns raised by the RCs, SC, and EC. As discussed therein, we have now focused on the essential first step in the classification proposal (i.e. identification of complexity), with analysis of streamflow data from a large network of 117 gaging stations in the western United States. We have also incorporated all the relevant review comments (both technical and presentation), including removing certain sections/significant portions of the text. The comments by M. Sivapalan are basically to test the overall proposal we presented earlier. We will investigate the remaining tasks (further verification of Step 1, and also implementation of Steps 2 and 3) in great detail in the future. Here, we offer specific responses to the Short Comment by M. Sivapalan.

Short Comment – M. Sivapalan (SC – C2426): The other three reviewers have been rather polite and measured even while quietly raising their concerns with this paper. I have enormous respect for the authors' work, their substantial contributions and reputation, but in this instance I am afraid that I have to be honest and express serious concerns in my personal capacity – I have serious objections to the publication of this paper in its present form and recommend rejection.

Author Response: We are puzzled, and indeed troubled, by these statements. Our belief/expectation was/is that discussions and debates in our research would be polite, despite the differences in our views and disagreements in our opinions, and particularly so when they occur in a public forum (such as the HESS Discussion forum). Therefore, the statement by M. Sivapalan that "The other three reviewers have been rather polite and measured even while quietly raising their concerns with this paper" is not only puzzling but also troubling. In our opinion, the three referees have only done what was/is expected of them (and what is normally expected from referees in general); i.e. reviewing our manuscript and offering their comments politely.

We appreciate the praising statement by M. Sivapalan on our work and our substantial contributions to the field of hydrology. However, we do not believe that such contributions and reputation, if any, should have any relevance on the assessment of the

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quality of our manuscript for publication. In this regard, we are very pleased that all the other referees assess our manuscript purely based on its quality and merits and offer their critical and constructive comments for improvement, regardless of our reputation. We are also concerned that M. Sivapalan may be making some comments that are inappropriate for his role in this instance (i.e. someone writing a Short Comment on a manuscript): while he says that he is commenting in his 'personal capacity,' he still offers his opinions on the nature of the reviews provided by the other reviewers (polite or not), which is normally the role of the Editor.

Short Comment – M. Sivapalan (SC – C2426): The paper argues that hydrologic system complexity is an appropriate basis for a catchment classification framework and nonlinear dynamic concepts constitute a suitable methodology. Even while I am not in agreement with this (I have not seen any results to convince me), I would have been prepared to give the authors an opportunity to demonstrate their method.

Author Response: Sections 3 to 5 in the previous version explained the role of (hydrologic) system complexity as an appropriate basis for a catchment classification framework and the role of nonlinear dynamic concepts as a suitable methodology, including two examples for demonstration. In Section 3, in addition to defining system complexity, we discussed how complexity in hydrologic systems can occur/be viewed, including the roles of process, scale, and purpose of study. In Section 4, we highlighted the role of nonlinearity in hydrologic systems and discuss (just) two basic nonlinear dynamics-based methods for study of system complexity (in the specific context of data variability): (1) phase space reconstruction – to identify complexity of system evolution, at least qualitatively; and (2) correlation dimension – to determine dimensionality, a useful quantitative indicator of dominant number of variables influencing system dynamics. In Section 5, we demonstrated the utility of (these two) nonlinear dynamic methods for identification of hydrologic system complexity, through their application to two streamflow time series. Further demonstration on this can be found in Sivakumar et al. (2007), where phase space reconstruction method is applied to a host of hydro-

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logic time series (with due consideration to geographical locations, scales, processes) for identification of the extent of complexity of hydrologic systems (more specifically, time series). While demonstration on still more hydrologic time series for identifying complexity is certainly desirable (which has been done in some of the publications by B. Sivakumar), the fundamental utility of nonlinear concepts for system complexity identification and classification is clear.

As we clearly mentioned in our manuscript, there are different definitions of 'complexity' (and also 'nonlinearity'), and we consider a specific case of complexity, which is defined in terms of data variability, and more specifically 'dimensionality.' It must be noted, however, that data observed within a hydrologic system (e.g. streamflow) are essentially the outcome of, and a reliable representation of, the mechanisms/dynamics that occur within that system. Consequently, data variability can represent the system complexity, at least in some useful manner. This is why we call it as an "inverse approach;" that is, going in an inverse way from data to system.

In view of these observations, we are clearly surprised by the following statement by M. Sivapalan: "Even while I am not in agreement with this (I have not seen any results to convince me), I would have been prepared to give the authors an opportunity to demonstrate their method." We can only suggest either/both of the following reasons for such a statement: (1) M. Sivapalan is not particularly aware of our works on nonlinear dynamics and chaos theories, or, at the least, he has not studied them in detail; and (2) M. Sivapalan may be interpreting 'complexity' in a different way from what we did/do in our study.

Short Comment – M. Sivapalan (SC – C2426): The authors say they offer a three-step procedure for a classification framework: (1) detection of possible patterns and determination of complexity levels of hydrologic systems; (2) classification of hydrologic systems into groups and sub-groups based on patterns and complexity; and (3) verification of the classification framework through establishing relationships between the data patterns/complexity and the catchment/process properties. Remarkably, that

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is where the paper ends: a proposal. The authors themselves say that their previous work, Sivakumar et al. (2007), has already used complexity as a way to classify catchments. If it is already in the literature why do we need another paper on the same idea? What is new? Is it this proposal? If so, this is not at all satisfactory to me.

Author Response: In essence, Yes, it is this proposal that is new. We indeed recognize(d) that our classification proposal needs to be implemented on real hydrologic systems for evaluation of its practical applicability and effectiveness. However, we would also like to point out that the germination of an original idea (in this case, the classification proposal), strongly supported by scientific reasoning (in this case, system complexity as a basis and nonlinearity as a suitable methodology), is clearly a fundamental advancement in science. Once the original idea having scientific merit is in the public domain (through publication), its applicability and effectiveness can be tested by ANYONE interested, and it is not a requirement that it must always be tested first by the proposers. This was indeed our view when we submitted our manuscript, with a new classification framework proposal, supported by our reasoning for considering complexity as a basis and nonlinear dynamic concepts as a suitable methodology (and our view remains the same, at least philosophically). While we certainly considered the full implementation of the proposal and have been working towards it since, we must also emphasize that it is a lengthy and enormously challenging task and needs to be carried out in multiple stages (each requiring careful interpretation), perhaps requiring a series of publications. In view of these, we believe that one reasonable way forward in the implementation of our classification proposal is to carefully test each step of the proposal as extensively as possible, before moving to the next. This is the approach we have adopted here in the revision, and focused on the essential first step, i.e. identification of complexity.

In regards to the requirements for publication, while we again agree that demonstration of a proposal is desirable, we would also like to point out that there are numerous examples in the hydrologic literature (and scientific literature in general) for publishing

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articles based on germination of original ideas and proposals alone, without their testing/demonstration on actual hydrologic systems. Indeed, M. Sivapalan himself is an author of at least a few of such publications in leading journals, such as the following:

1. Wagener, T., Sivapalan, M., Troch, P. A., and Woods, R. A.: Catchment classification and hydrologic similarity, Geog. Compass, 1(4), 901–931, 2007.

2. Wagener, T., Sivapalan, M., Troch, P. A., McGlynn, B. L., Harman, C. J., Gupta, H. V., Kumar, P., Rao, P. S. C., Basu, N. B., and Wilson, J. S.: The future of hydrology: an evolving science for a changing world, Water Resour. Res., 46, W05301, doi:10.1029/2009WR008906, 2010.

These publications, essentially with ideas and proposals without any demonstration on actual catchments, are as long as or even longer than our manuscript. All these articles also contain (in fact, formed by) a significant amount of information (taken) from previously published articles by M. Sivapalan and/or his co-authors and other researchers, so as to essentially help the readers to better appreciate and understand the research progress and proposed ideas. It is obvious that these articles were considered suitable and appropriate for publication in leading journals by M. Sivapalan, his co-authors and/or the editors who handled them (not only for the proposals but also for the background information), and rightly so in our opinion, as they offer the readers a better means to understand such works.

In view of these, we are disappointed that M. Sivapalan does not appreciate our reasons for providing the necessary background information, especially considering that nonlinear dynamic and chaos concepts are still new to a significant majority of the hydrologic community (we say this based on our interactions with researchers around the world). In fact, NONE of the manuscripts submitted to/published in the current HESS Special Issue on “Catchment classification and PUB” (including the ones co-authored by M. Sivapalan) have cited the study by Sivakumar et al. (2007), despite the fact that it is one of the very few recent studies on catchment classification before the current

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Special Issue on “Catchment classification and PUB.” We can only interpret that the authors of such papers, including M. Sivapalan, consider the study by Sivakumar et al. (2007) as largely irrelevant to their studies or they simply ignore it. All the more reason for inclusion of necessary background information in our manuscript.

Nevertheless, in addressing the review comments, we have now revised our manuscript, with demonstration of the first step of our proposal (identification of complexity) on 117 streamflow time series. We will conduct detailed investigations of the second and third steps, as appropriate, and report the results in our future publications. We have also significantly modified/shortened our manuscript, especially on the review part. We hope the readers would look into the literature in detail to appreciate our work.

Short Comment – M. Sivapalan (SC – C2426): So what else is there in this rather long manuscript? (i) a rehash of ideas recycled from previous papers presented as review, which I have seen several times before. (ii) another brief historical reivew of classification efforts, to add to the many that are alerady in the literature. (iii) then there is the whole litany of ideas and definitions of complexity, deterministic chaos etc that are drawn from the work of Sivakumar – but which is already well established in the literature in volumes.

Author Response: Our Response above has already addressed this.

Short Comment – M. Sivapalan (SC – C2426): And then follows the proposal that I mentioned above. To be publishable the proposal must be put to the test: the authors should apply their method to a collection of actual catchments along the lines of their proposal, come back with results to demonstrate that the approach is capable of accomplishing the classification they promise, that the classification has hydrological meaning, that it can be used to separate catchments and their behavior into meaningful classes, and that this will lead to harmonization of models and model types.

The paper will have no basis for publication without such concrete results. I will eagerly await publication of such results. It does not matter if the proposal ends up being a

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success or failure. What matters is that work is actually done along the lines of the proposal, and we get to see and assess the outcomes. Such a process by itself could be useful to the community.

Author Response: As mentioned above, we indeed recognize(d) that our classification proposal needs to be implemented on real hydrologic systems for evaluation of its practical applicability and effectiveness. However, we would like to emphasize again that implemetation of our full proposal is a lengthy and enormously challenging task and needs to be carried out in multiple stages (each requiring careful interpretation), perhaps requiring a series of publications. In view of these, we believe that one reasonable way forward in the implementation of our classification proposal is to carefully test each step of the proposal as extensively as possible, before moving to the next. This is the approach we have adopted here in the revision, and focused on the essential first step, i.e. identification of complexity. We have applied the correlation dimension method to 117 streamflow time series and identified four reasonably distinct groups. We will verify these results further, and also carry out the subsequent steps in the future.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4427, 2011.

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