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Hydrology and Earth System Sciences Discussions

Interactive comment on "Controls on runoff generation and scale-dependence in a distributed hydrologic model" *by* E. R. Vivoni et al.

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Review of "Controls on runoff generation and scale dependence in a distributed hydrologic model" by ER Vivoni, D. Entekhabi, RL Bras and VY Ivanov

This paper focuses on event storm response, and explores how the characteristics of storm response, e.g., runoff coefficient, runoff peak and time peak etc. depend on both storm properties as well antecedent conditions, and how they scale with increasing catchment area.

The work presented is an extension of considerable work carried out over the past decade, much of which is cited in the paper (Robinson et al., 1994; Robinson and Sivapalan, 1997; Menabde and Sivapalan, 2001; Sivapalan et al., 2002). Most of the previous work has been done using somewhat idealized/simplified situations, but nevertheless have yielded considerable insights into the storm response behavior and their scale dependence. The main advance, in this respect, in this paper is the use of a much more sophisticated, distributed model of the runoff generation component, tRIBS, which can simulate a full suite of runoff generation mechanisms. Whereas some of the previous work adopted an explicit treatment of river network structure, and tried to link the estimated scaling behavior to river network structure (Menabde and Sivapalan, 2001; Gupta and Waymire, 1998), in this paper the focus is on the landscape (hillslope response) and the effect of the resulting patterns of runoff generation on the scaling behavior and the nonlinearity of the runoff transformation.

I generally very much like this paper. It does yield considerable insights into the nonlinearity of the runoff generation mechanisms, including how these depend on the storm properties (ie intensity and duration), and also on the antecedent conditions. Some of the results are merely confirming what was already known from previous work. On the other hand some of the results are new, and are revealed for the first time only because such a comprehensive runoff generation model was not available before.

However, I do have some concerns and questions about this work, especially about the discussion of some of the results.

1. The dependence of mean water table depth presented in Figure 5: this is consistent with the view that, due to differences in drainability, larger basins are, on average, wetter (shallow water tables) than smaller basins (deeper water tables). This is intuitively OK if one understands that smaller, headwater basins tend to be steeper than larger basins.

However, in the context of Figure 5, I am concerned about the index of hydrologic similarity the authors use, as presented in Eq. 8. This index uses Ac (upslope area) and not a (upslope area per unit width). I want to be convinced by the authors that the use of this index does not contribute to the scale dependence presented in Eq.

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5. As I know from Sivapalan et al. (1987), the local water table depth is governed by the ratio ln(a/tan), not by ln(Ac/tan). The theory is very clear about this. I repeat, I want to be convinced that there has been no mistake in their model that arises from inappropriate or inadvertent use of this index.

2. Most of the results presented in this paper are interesting, but as would be expected based on my knowledge of the previous work. However, the dependence of storm runoff coefficient on catchment area that is reported here is indeed new. They have been talked about, but without much evidence from field experiments or even numerical simulation models. The only exception to this is the work by Goodrich et al. (1997), although the nonlinearity reported in that paper was due to the arid nature of the catchment studied.

Considering the novelty of these results, the discussion of the results reported in Figure 13 is insufficient. I was not able to fully understand the reasons for the reported scaling behavior. I can only understand that on average larger catchments (being flatter) are less draining and therefore would be expected to be wetter. But the reported results for the drizzle and the thunderstorm are somewhat counter-intuitive.

It is therefore important that the authors expand on their description of the results and explain them, either based on good hydrological intuition and recourse to the model theory, or insert (again) the explanatory results they previously withheld and build up the explanation using more insightful figures. This is extremely important.

3. While the results are interesting and some are possibly general and universally applicable, some of the detailed results, surely, are only applicable to the particular region from where these catchments came from. One needs to be careful about claiming generality from a model that was calibrated or tailor made for a specific set of catchments in a particular locality.

Noting that storm properties and the antecedent conditions are independently prescribed and are relevant to this region only, it is very difficult to generalize the results

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to other places in other climates. If this is indeed the case, what is the ultimate use of these results for predicting the behavior (including nonlinearity and scale dependence) in ungauged basins somewhere else? The authors may want to comment on the overall utility of this work and the ensuing results for the development of a general theory.

4. In spite of the fact that the authors used a well calibrated model, the results must be considered as model generated. The authors have access to the measured data in these catchments. Did some of the nonlinearity and scaling behavior, as generated by the model, show up in the observed data also? Have the authors done the analysis to confirm that what is being reported through model predictions are also exhibited by the observed data?

5. One major difference between observations from real-world catchments and this model is that real storms have spatial and temporal variability, whereas the model has so far assumed spatial and temporal uniformity. These spatial and temporal variabilities will tend to “resonate” with the variability of travel pathways and travel times (fast surface runoff, slow subsurface flow etc.), and this resonance will certainly manifest itself in the scaling behavior of both runoff coefficient and flood peak etc. This has been reported, for a simple case, by Robinson and Sivapalan (1997). Therefore, any departures in the real world from the model predicted behavior may be partially explained in terms of this phenomenon. The authors may want to make a comment on this, and indicate how they hope to overcome this problem in future work.

6. The paper is quite dense, with many figures and tables, and towards the end I feel that the authors are rushing through the results and do not stop to explain these results and their implications or manifestations sufficiently well to the discerning reader. I think the discussion needs to be expanded somewhat. Not more long but sharper.

In conclusion, the authors have covered considerable ground in this paper compared to much of the previous work - connecting spatially distributed runoff generation processes, through the river network, and covering catchment areas ranging from 0.8 to 800 km2.

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