

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

E. R. Vivoni et al.

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Responses to Reviewer 1: M. Sivapalan

1. The reviewer comments: "However, in the context of Figure 5, I am concerned about the index of hydrologic similarity that authors use, as presented in Eq. 8. This index uses A_c (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. 5. As far as I know from Sivapalan et al. (1987), the local water table is governed by the ratio $\ln(a/\tan b)$ and not by $\ln(A_c/\tan b)$. 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."

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We are confident in our use of the modified index $\ln(Ac/\tan b)$ for several reasons. (1) The index is used only to classify basin locations, not in model calculations. As a result, its use has no impact on the observed scale dependence. In fact, its use is limited to showing where (in space) changes occur in the water table position and how different runoff mechanisms are distributed. (2) The index is relatively insensitive to the use of either Ac (upslope area) or $a = Ac/w$, where w is the contour width, since $Ac \gg w$, at the scales of this model. For grid-based models, w is a constant and equal to the grid cell length. In this model, w is variable in each Voronoi element, but is typically in the small range of 10 to 100 meters. The range of Ac is quite large, from 900 m² to 800 km². Action: We have added the following sentence in our description of the index: "As a result, this index is used simply here to classify a diverse set of basin locations according to hydrological behavior."

2. The reviewer comments: "Considering the novelty of these results (related to the scaling of the runoff coefficient), the discussion of the results reported in Figure 13 is not sufficient. I was not able to fully understand the reasons for the reported scaling behavior...It is therefore important that the authors expand on their descriptions of the results and explain them, either based on good hydrological intuition and the 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."

We agree with the reviewer that Figure 13 is a novel result and this it should be emphasized and discussed more thoroughly. We do caution over-emphasis since this result is only obtained for the two storm types (drizzle and thunderstorm) and for a limited number of subbasins. Action: We have revised the paragraph discussing the results to include several new interpretations and an expanded discussion. We have included the following sentence: "This suggests that larger basins are less sensitive to the initial water table position for low intensity events." We have revised the following sentence: "Consistent with this, the dry condition, where $FG + FP$ are small, has a runoff ratio which does not vary with A (runoff ratio approximately 0.04), indicating that the impact

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of the initial condition has effectively been reduced across all basin scales." We have added a transition sentence: "In contrast, the thunderstorm event exhibits opposing runoff ratio scaling behavior." We have introduced a new sentence describing the results and second sentence of interpretation: "Note that a maximum value in runoff ratio is observed at areas ranging from $A = 20$ to 60 km^2 , depending on the initial wetness. The peak runoff ratio identifies basins with surface properties that promote high runoff (e.g., lower forest fraction)." We modify an existing sentence to explain the scale variations for large basins in the thunderstorm event: "Interestingly, a slight decrease in runoff ratio occurs for the largest basins, which results from lower runoff production as more permeable regions are sampled. Since this occurs for all wetness conditions in the thunderstorm event, it is likely due to the fraction of forested areas rather than initial wetness." Overall, we believe that the results are now explained in sufficient detail for the purposes of this study. Further work is needed to fully characterize the scaling behavior of the runoff ratio.

3. The reviewer comments: "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."

As in most modeling studies, we acknowledge our results are conditioned on the characteristics and limitations of the numerical model as well as the model application to the study region. Action: We have introduced a new paragraph in the discussion addressing this point. Within this, the following sentences are most pertinent: "While our study is focused on a single basin, the watershed exhibits similar hydrologic behavior to other regions in the Great Plains [Garbretch et al., 2004]. As a result, basin response characteristics identified in this study may be applicable to similar settings in the broader region. It is important to consider, however, that the behavior in other basins with different climate and surface characteristics may vary from our study results." We also

indicate how generalization can be achieved in future work: "Testing the robustness of our results in alternative settings or with other model structures would be a fruitful avenue that may lead to generalizable conclusions on the role played by runoff mechanisms on basin response nonlinearity and scale dependence."

4. The reviewer comments: "The authors may want to comment on the overall utility of this work and the ensuing results for the development of a general theory. What is the ultimate use of these results for predicting behavior, e.g. in ungauged basins somewhere else?"

We agree with the reviewer in this respect. Action: We include a new paragraph that comments on the general utility of this work and the testable hypothesis arising from this study. "Insights from the modeling experiments reveal that the basin flood response is related to the runoff mechanisms excited as storm properties interact with particular catchment locations and their wetness state. Identification of the intimate link between runoff response characteristics and the underlying mechanisms provides a process-based explanation for nonlinear responses in gauged and ungauged basins. An important result emerging from our modeling exercise is that large changes in basin response occur when the dominant mechanism transitions between surface and sub-surface runoff. If runoff partitioning can be properly captured in numerical models, there is the possibility of reproducing observed nonlinear responses across a range of real watersheds. Distributed modeling results also provide a physical explanation for the scale-dependence of runoff generation in complex basins. A testable hypothesis arising from our experiments is that the scale-dependence of the runoff ratio exhibits different regimes which vary according to the underlying mechanisms. Our results indicate that spatial heterogeneities in landscape and initial wetness interact with storm forcing to produce runoff generation patterns that exhibit variations with aggregation scale. Capturing surface-subsurface dynamics in numerical models of gauged and ungauged basins may allow understanding of process controls on runoff scale-dependence."

5. The reviewer comments: "In spite of the fact that the authors used a well calibrated

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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?"

We agree that the results must be considered as model generated. While we do have observations in the basin, these are quite limited to quantify the nonlinearity and scale-dependence in the runoff response and its relation to runoff production mechanisms, thus the utility of the numerical model as a tool for scientific inquiry (e.g. virtual laboratory). Observations consist of >50 years of streamflow at the outlet (~800 km²) and at Dutch Mills (~100 km²). Peacheater Creek has less than 10 years of record. Two major limitations are the rainfall data availability and quality (no long term gauges in the basin, NEXRAD over short period), and the lack of groundwater records to assess antecedent wetness conditions. As a result, we cannot effectively use the observations to infer the scaling behavior in hydrograph characteristics, groundwater table position and runoff ratio. We do agree that this would a fruitful avenue of future research using the limited data set in a novel manner. Action: We have used some of the observations in the record (Figure 2) to motivate that the basin under study does exhibit nonlinear response that varies among basins with different scale, in support of our model results. We have not, however, carried out a rigorous analysis as the data sets are limited in spatiotemporal extent. We state the limitations in the observed records in section 3.2 as: "These limited observations, however, do not allow rigorous study of the runoff non-linearity and scale-dependence as a function of storm and initial wetness conditions." We have also stated in the introduction that our analysis is based on a model: "Our analysis is based on the physical processes represented in the model and how these interact to generate runoff".

6. The reviewer comments: "One of the major differences between observations from real-world catchments and this model is that real storms have spatial and temporal

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variability whereas the model has so far assumed spatial and temporal uniformity. 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."

We agree with the reviewer comment concerning the limitations of the present study caused by use of a spatially-uniform rainfall series. Departures between observations and model results may be related to this. We are currently working on this topic through use of synthetic precipitation forcing fields created under controlled conditions. Action: We have added the following sentence to reinforce our discussion section: "The spatial and temporal variability of the precipitation forcing is expected to resonate with the runoff production mechanisms and the distribution of travel times to influence basin response and its scaling behavior, a topic of current investigation." and modified the subsequent sentence to account for this discussion: "Ultimately, the distributed hydrologic model can be used as an interpretive tool to assess the surface-subsurface processes that control runoff production resulting from a range of possible forcing conditions."

7. The reviewer comments: "The paper is quite dense, with many figures and tables, and toward 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."

Action: We have expanded the discussion section considerably, adding two new paragraphs and refining the existing text to address this comment, as well as others provided by the two reviewers. We have not expanded the discussion too much, just sufficient to describe the general implications of our work. By design, the manuscript remains concise, but complete, in its results and figures, as we feel this style allows the reader to see the 'whole' and its interacting parts, without getting too lost in the details of a particular analysis.

Responses to Reviewer 3: Anonymous

1. The reviewer comments: "I suspect that some readers might question whether the analysis of the model behavior actually tells us any more than the model equations themselves, i.e. the nonlinearities that are described and explained are inherent in the model structure. I think this aspect can be addressed by minimizing some of the discussion of the more intuitive behavior, putting greater emphasis on the more novel outcomes, and presenting a more general discussion at the end of the paper regarding implications of the results for other studies."

We agree that our study is reliant on a particular model structure. Nevertheless, we have identified the underlying causes for nonlinearity and scale-dependence in this construct are related to the surface-subsurface partitioning. Action: We have made explicit statements in the introduction: "Our analysis is based on the physical processes represented in the model and how these interact to generate runoff" and the discussion section on the dependence on the model structure: "Despite this, the metrics introduced here to assess the mechanistic causes of catchment response can be useful tools for detailed investigations in other basins or with different numerical models. Testing the robustness of our results in alternative settings or with other model structures would be a fruitful avenue that may lead to generalizable conclusions on the role played by runoff mechanisms on basin response nonlinearity and scale dependence." We have also presented a discussion of the implications to other studies (see response to comment 7, reviewer 2). Finally, we added greater emphasis to the more novel results related to Figure 13 (as suggested by reviewer 1). For the sake of completeness, we did not minimize discussions in other sections. We did, however, place greater emphasis on the results in Fig. 13 in the introduction: "An important question is whether the catchment runoff ratio exhibits scale-dependence and if this is linked to the runoff mechanisms excited at particular basin scales."

2. The reviewer comments: "Results are presented for three different catchment scales in Figures 2 and 6. The results are difficult to compare across the scales because they

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are presented in terms of the total discharge. If the data were converted to specific discharge, they would be much clearer. The same goes for the discharge figures quoted on p993, line 8."

The reviewer suggestion is a good one. However, our intent with Figures 2 and 6 is to illustrate the variability in discharge among the different sized basins in absolute terms, not in relative terms. As a result, we are not keen on presenting the specific discharge in the figures. Quantitative comparisons are presented in other supporting material (such as Fig. 12, 13 and 14) across the various basin scales. Action: To help compare across basins, we have added the specific discharge (qs) for the observed events in Table 1 (which refer to the time series in Fig. 2). We have not included specific discharges for the model response (Fig. 6) as catchment area and qp are compared in Figure 12.

3. The reviewer comments: "The rainfall data for each of the three sub-catchments for each storm event cited in Table 1 would be useful, rather than just runoff ratios."

Action: We have added a note to the Table 1 caption that indicates the basin-averaged rainfall depth at the scale of the Baron Fork as: "Basin-averaged rainfall depths in the Baron Fork are 34.05 mm, 35.11 mm and 60.51 mm for events 1, 2, and 3, respectively." As noted in comment 2, we added the specific discharge for each event to Table 1, which complements the runoff ratio.

4. The reviewer comments: "I am unconvinced about the generality of some of the scale effects that are discussed. For example, p998, line 14-17, many upland areas have plateaux regions on the hill tops where groundwater drainage may be very slow. Conversely, agricultural activities in many lowlands have resulted in extensive sub-surface drainage networks, which tend to lower the water table. This would lead to the opposite effect from that presented, in terms of the basin water table."

The results of the scale effects on the basin water table are limited to the 15 sub-basins sampled in this study. We have emphasized in the work that this is a limited set. As

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a result, it is possible to find small basins with poor drainage due to low slopes, as pointed out by the reviewer. The example provided with respect to the lowlands is not applicable in our study as we have ignored subsurface tile or drainage systems in agricultural areas. Action: In reference to the scale-dependence of initial groundwater table position, we have been more explicit about the limited set of subbasins by modifying: "Note the deeper mean Nwt for smaller basins at all initial wetness states, an indication of rapid groundwater drainage in upland areas primarily composed of steep hillslopes for the selected subbasins." and "Since lowland regions have less effective drainage in natural settings, inclusion of these areas in the basin average reduces mean Nwt." We believe this is an interesting result that requires further verification by sampling more subbasins.

5. The reviewer comments: "Similarly, the discussion of the scale influences in section 4.4 appears to relate it, at least in part, to land use and the presence of more forestry in small catchments. This is unlikely to be particularly transferable to areas with different land use patterns. If this is the case, then perhaps it would be of more value to relate the processes to land use or soil type. Additionally, it is clear that in this example the land use and soils were classified together, and therefore it is difficult to disentangle the influences of the soil properties from those of the land use."

We acknowledge that the discussion of this scale effect required clarification. Action: We have modified the discussion in section 4.4 to explain Figure 13. The revisions tie the scale influences to the amount of forest cover as this an appropriate surrogate in our application for soil permeability (see Table 2). Our discussion explicitly states: "The peak runoff ratio identifies basins with surface properties that promote higher runoff (e.g., lower forest fractions). Interestingly, a slight decrease in runoff ratio occurs for the largest basins, which results from lower runoff production as more permeable regions are sampled." This landscape property (high soil permeability in forests) is transferable to other regions. We consider this as a vegetation property rather than a land-use characteristics as these are natural deciduous and evergreen forests. We have also

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indicated in the text that the vegetation and soils were classified together by adding the following in section 3.3: "For this study, as in Ivanov et al. [2004b], the spatial variability of soils and vegetation are overlapping and correspond to forest, grassland and urban classifications." As a result, the reviewer correctly states the difficulty to separate the influence of soil and land use properties in this application.

6. The reviewer comments: "How much of the interpretation carried out is a function of the model structure used for the analysis? How might this have changed if, for example, the model included runoff from perched water tables or a by-pass flow mechanism? The model limitations need to be discussed."

We agree that our results are conditioned on the model structure and that alternative constructs may lead to different results. Nevertheless, we present a set of tools to identify the effect of runoff mechanisms on basin nonlinearity and scale dependence that can be used to test other model structures. Our model captures a variety of runoff production mechanisms including runoff from perched water tables, but not the by-pass mechanism (although this can be mimicked through the use the anisotropy ratio). Action: We have added a new paragraph that highlights the model dependence. The following sentences are most pertinent: "Despite this, the metrics introduced here to assess the mechanistic causes of catchment response can be useful tools for detailed investigations in other basins or with different numerical models. Testing the robustness of our results in alternative settings or with other model structures would be a fruitful avenue that may lead to generalizable conclusions on the role played by runoff mechanisms on basin response nonlinearity and scale dependence." In addition, we have included the following sentences in the introduction to the model description to indicate the model limitations: "In the following, we present a brief discussion of the model physics, emphasizing those components most relevant to understanding the coupled surface-subsurface basin response. The reader is referred to Ivanov et al. [2004a] for additional details and discussions of model limitations." Also see our discussion to comment 3 from reviewer 1.

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7. The reviewer comments: "A more general discussion of the implications of the experimental findings, beyond these particular catchments, would be useful. What are the more general messages and how will we use this understanding."

Action: We have added two paragraphs to the discussion section to address the implications of our study and how these findings provide testable hypotheses that can be explored in other regions. The reader is referred to the revised discussion and to our response to reviewer 1, comment 4.

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