Hydrol. Earth Syst. Sci. Discuss., 8, C2681–C2694, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C2681/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Towards reconstruction of the flow duration curve: development of a conceptual framework with a physical basis" *by* Y. Yokoo and M. Sivapalan

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Received and published: 11 July 2011

Anonymous Referee #3 Received and published: 22 June 2011

"1) Overview

This study presents, in a hypothetical setting, an investigation of the climatic and landscape controls on the shape of the flow duration curve (FDC) with the use of a stochastic rainfall generator and a lumped physically-based rainfall runoff model. The sensitivity of the simulated FDC shape to a variety of combinations of climatic inputs (i.e. precipitation, potential evaporation and their within year phase) and landscape proper-

ties (i.e. soil type and depth) were investigated through changing corresponding model parameters. The results indicated that the FDC can be disaggregated into two components: surface runoff and subsurface runoff. Moreover the authors argue that the surface runoff can be approximated with the use of a non-linear filter on the precipitation duration curve, whereas subsurface runoff can be approximated with the use of a linear filter on the FDC constructed from the regime curve. A correction to the lower tail of the FDC is also suggested to capture the effects of high evapotranspiration at low flows. The authors conclude that the FDC can be reconstructed for the ungauged basins with the use of the above three-step conceptual framework.

2) General Comments:

This study is a purely theoretical study that aims to identify the dominant controls on the shape of the flow duration curve (FDC). The authors try to utilize the understanding gained from hypothetical simulation experiments to derive FDCs for ungauged basins. I think one of the main drawbacks of the study is that it is purely theoretical and based on a limited number of experiments with a lumped (quasi 2-D) rainfall runoff model. Although in the discussion section the authors mention that they will investigate the proposed framework in some 200 watersheds in the U.S., I still think that the proposed framework should be demonstrated with observed data in one or more real watershed systems in the current manuscript before it could be accepted for publication. I think the link between the regime curve and the intermediate segment of the FDC is a unique contribution of this manuscript. For that reason, the description of the regime curve should be clarified and the link between the regime curve and the intermediate segment of the FDC should be more rigorously discussed. Some points of discussions are provided in the main comments section. I think the manuscript will eventually make a good contribution in the subject area of prediction in ungauged basins and the topic is suitable and interesting for the HESS readership. However, the manuscript should be revised based on the comments provided in this section and sections listed below before it could be accepted for publication."

Thank you for your constructive comments to our manuscript. Also, we appreciate very much for your careful review of our manuscript in detail, and the many useful suggestions.

A common refrain of all three reviewers is that the paper does not present a demonstration of the proof of concept (of the conceptual framework we propose) in actual catchments. In our response to Reviewers #1 and #2 (already submitted) we argued that this was beyond the scope of this paper for three reasons: (1) Application of the current REW model to actual catchments is not a straightforward exercise. It will require inclusion of landscape heterogeneity and process complexity that is present in actual catchments, and will require calibration as well. Such analyses are already reported in another paper in HESS(D) by Carrillo et al. (2011) where a much more sophisticated and complete model is applied to 12 of the MOPEX catchments. (2) There is a separate study currently underway where the conceptual framework presented here is to used to separate the components of the FDCs, explore regional patterns, and explore the underlying process controls. The results of this study will be submitted to HESS(D) in the next few months. (3) We felt that exploring the physical controls of the FDCs in a generic sense using a simple physically based model, and extracting the conceptual framework would be sufficient for this paper.

However, due to the fact that this is raised by all three reviewers, we have decided to include examples of the separation of the FDCs into the components from three selected MOPEX catchments, but without the inclusion of more advanced modeling results. This is certainly feasible, and will hopefully allay the concerns of the reviewers. These example results from the future work will show the relationship between the PDC and the SFDC and the regime curve and the SSFDC. We believe this will be sufficient for the purposes of this paper. Results of further modeling studies and the classification that results from that work will be presented in subsequent papers.

Main Comments:

"1) The authors need to explain clearly that they are focusing on "runoff" rather than "streamflow" which is the main variable used for constructing FDCs. When streamflow is used to construct FDCs watershed size and routing parameters strongly influence the shape of the FDC. A discussion on the runoff vs. streamflow use in constructing FDCs should be provided. Make it clear that runoff is used in this manuscript."

The reviewer is right. We would not claim we are predicting FDCs for "streamflow" but only "runoff", as per the reviewer's definitions. Our model does not include river routing, the effects of which can be significant for fast runoff, especially in large basins. It may not affect slow runoff that much. We will clarify that our model does not include river routing and the result pertain to runoff only.

"2) Hydrologists would like to see a hydrograph, or in this case, runoff time series to understand the behavior of the watersheds. Please include at least one time-series graph showing rainfall, surface and subsurface runoff components before transforming into FDC. I suggest providing this for Figure 2."

We do not see the point of the reviewer – we wonder why hydrologists can't interpret FDCs directly. In fact the beauty of the FDC is that it is a compact description of within-year runoff variability.

We did consider including one hydrograph in response to the reviewer, but how does showing just one example help interpret all the other cases. Surely we cannot produce hydrographs for every one of the many cases we have studied.

We decided to not accede to this suggestion – however, we stand by in readiness to include it if the reviewer and editor insist on it.

"3) Page 3975, Line 4-13: I think the authors need to provide a more process based explanation to this contradiction based on the model structure. Why do authors think that sand provided steeper slope compared to silt considering the processes embedded in the model? The explanations given in Ward and Robinson (1990) are more related to flashy vs. delayed response of watersheds based on the soil hydraulic conductivity values. It appears that macropores and preferential flow is blamed without evidence."

Reviewer # 2 had a similar concern and this is how we responded to it.

We too are concerned that our results apparently contradict some results presented in the literature. This is a contradiction between the predictions of a completely theoretical model that uses accepted laws of hydrology (e.g., Darcy's law) and observations in actual catchments that we have not modeled or studied directly. We can only highlight these differences, speculate about the possibilities or reasons, and hope further modeling or field studies can clarify the reasons for the differences. Nature is much more complicated than is represented in our model, and we acknowledge that. The goal of the paper to present trends, which can serve as hypotheses to be tested in future studies. This is precisely what we state in the paper.

Macropores and preferential flow are not just blamed – we are only speculating as to the potential causes, hoping that this will trigger others with more knowledge of these catchments can come up with better explanations.

"4) Page 3976, Line 11: It is not clear why shallow soils should have smaller water holding capacity? To my knowledge water holding capacity is a function of soil type and composition and is independent of soil depth (it may get slightly lower with depth due to compaction). Do you mean "limited moisture supply"? Please clarify in text."

We meant "water holding capacity" as the "product of porosity (which depend on the soil type and composition) and soil depth". We would clearly define this in the main text of the revised manuscript, and perhaps used the term "storage capacity" rather than "water holding capacity" which in soil science means something related to soil type and not soil depth.

"5) Page 3978, Lines 4-18: None of these points has been mentioned during the simulation results presented in the previous sections. This paragraph seems to be out-of-

context. I suggest removing this paragraph."

We disagree with this comment. Figure 6 was presented to better explore why the lower tails of the FDCs drop at their end. Without this figure, we think we cannot discuss the reason for the dips of FDCs.

We will explain better in the revised manuscript why we present this figure, i.e., for an explanation we calculate subsurface runoff as "outflow minus ET from saturated surface" in Section 2.2.1.

"6) Page 3979, Line 4: I think this is an over-statement. In soils with high hydraulic conductivity, soil depth (shallow vs. deep) will be a significant factor in surface runoff generation due to saturation excess mechanism."

The reviewer is only partially correct. We return to this point again later in answer to another of the reviewer's query.

Even though it makes intuitive sense that shallow soils leads to a flashier response, the reality is that in this case the fast runoff happens due to runoff from partial saturation areas formed by the rise of the water table, and therefore these two processes are inter-connected.

The reality is that the effect of soil depth is partially modified by the magnitude of the saturated hydraulic conductivity. Even in shallow soils, if the Ks is large, then water will drain fast by subsurface flow, and water table will not rise sufficiently to cause saturation excess runoff. This is well understood in studies going back to Freeze (1975, 1981), and is explored in more detail in the original REW model application of Reggiani et al. (2000), and also in Yokoo et al. (2008).

For the benefit of the readers and reviewers we will clarify this better in the revised manuscript.

"7) It appears to me that the link between the regime curve and mid-segment of the FDC will hold for basins with base flow dominated response, where the flashy behavior of the flow is smoothed out. In flashy watersheds, dominated by the surface flow, this link may no longer hold. In the manuscript, sensitivity to climate factors and soil type utilized 8m deep soil which will provide more sustained flows. I wonder how the results will change if 6m deep soil was used? Moreover, there will possibly be problems in snow dominated basins."

The reviewer is indeed correct on both counts. We would counter that does not invalidate the point of the paper, that there exists two parts to the behavior, a fast one and a slow one. Some catchment have them both in significant ways, others may be more dominated by one or the other.

If we change the soil depth, depending on the Ks value it will change the relative dominance of fast and slow runoff components. This was explored in Yokoo et al. (2008) in respect of the regime curve, and in Reggiani et al. (2000) in respect of the annual runoff partitioning. We decided not to include all cases, only those sufficient to bring out the main points of the paper.

Our model did not include snow and snowmelt, and leaf phenology, both of which will impact the slow response and the seasonality of flows. We believe that the conceptual framework built on slow and fast runoff components will still be valid, except we need appropriate models to handle snowmelt and leaf phenology. In fact our application of the conceptual framework presented here to 200 MOPEX catchments does include these two aspects.

"8) Page 3979, Lines 20-29: Please provide more details on how this framework will be applied in ungauged basins. Should we estimate the parameters of these models from similar gauged basins and apply these models in ungauged basins? I think these procedures should be explained more explicitly. Moreover, they should be demonstrated with observations in one or more watersheds."

The approach is not as straightforward as the reviewer suggests.

The first step is to study empirically the FDCs in a large number of catchments, including the components PDC, SFDC and SSFDC using some kind of baseflow separation algorithm.

Next is a process of organizing these catchments into classes of common or similar behavior. This could be aided by the focus on the individual components: PDC, SFDC and SSFDC (and the connection to the regime curve)

Third, we will build models of appropriate complexity and process realism (snowmelt when needed, leaf phenology changes when needed etc.), and using these models to explore the climate and landscape controls on the FDC and the components. These are three components that we are presently working on and they build on the conceptual framework proposed in this paper as the working hypothesis. We believe that once all three pieces are in place these can be the basis for extrapolation from these gauged basins to ungauged basins.

In other words, predictions in ungauged basins will be based on (1) a classification of catchment behavior (with respect to FDCs) based on known catchment characteristics and past observations of catchment response, and (2) a model of appropriate complexity and process realism, which also falls out of the classification scheme.

Unlike what the reviewer thinks or misunderstands, it does not involve any application of the present REW model with choice of appropriate regionalized parameters. It is too much to expect that this is possible at this stage.

Minor Comments:

"Page 3962, Line 6: "PFDC" Not clear why "F" is used in "Precipitation duration curve". I suggest changing "PFDC" with "PDC"."

We would be happy to change "PFDC" to "PDC".

"Page 3962, Line 14: The description of the regime curve is not clear. The term ensemble is generally associated with multiple runs of a model. Also time step is not clear.

Since there are 12 points on Figure 2-5, does this mean regime curve is long-term mean flow value for each calendar month? Describe the regime curve more clearly. Because we do not see the regime curve itself but its FDC, it is really difficult to understand what it really means with the current descriptions provided in the manuscript."

The regime curve is normally seen as the mean monthly variation of flow. For most catchments we are dealing with, at the monthly scale, the routing is a minor issue and the differences between runoff and streamflow are minor. We admit that in this case the model only predicts "runoff" as the reviewer calls it.

However, in this paper, in order to convert to daily FDCs, we construct the regime curve at the daily scale, which means it reflects mean (between year or ensemble mean) daily flows, and in this case we are ignoring the effects of river routing. The reviewer is right and we will clarify this in the revised manuscript.

This is repeated in answer to another of the reviewer's query.

"Page 3963, Line 2: "temporal streamflow variability" This sentence is rather confusing because FDC does not contain any temporal information, but contains frequency distribution of flow within a specific "time period". I suggest replacing this sentence with a more common definition of FDC provided in many references cited in the manuscript. One can also construct the FDC for any time averaging scale such as hourly, daily, weekly, monthly etc. Therefore the next sentence needs to be modified to reflect this information."

Agree, will make the correction. We will rewrite these two sentences following your suggestion in the revised manuscript.

"Page 3963, Line 26: Replace "studied" with "studies""

Agree, will make the correction.

"Page 3964, Line 13: As far as I remember, Yilmaz et al. (2008) utilized not only one parameter but various parameters of the SAC-SMA in their sensitivity analyses.

Moreover, they also provided understanding on the shape of the flow duration curve with three segments, which is very similar to Figure 7 in the current manuscript – See Paragraph 37 and Figure 5a in Yilmaz et al. (2008) manuscript. Please improve the citation as necessary."

Thank you for pointing this out, as it reinforces the findings of our paper. We will indeed highlight more the findings of the Yilmaz et al. (2008) paper, which we did not fully appreciate before.

"Page 3965, Line 13: Again "regime curve" definition is not clear. The terms "ensemble mean" and "regime curve" is not explained in the cited reference (Yokoo et al., 2008). Moreover, is regime curve constructed using "runoff" as described here, or "using streamflow" as described in page 3972, Line 15?. Runoff and streamflow should not be confused: the latter includes the flow routing by the channel network."

The regime curve is normally seen as the mean monthly variation of flow. For most catchments we are dealing with, at the monthly scale, the routing is a minor issue and the differences between runoff and streamflow are minor. We admit that in this case the model only predicts "runoff" as the reviewer calls it.

However, in this paper, in order to convert to daily FDCs, we construct the regime curve at the daily scale, which means it reflects mean (between year or ensemble mean) daily flows, and in this case we are ignoring the effects of river routing. The reviewer is right and we will clarify this in the revised manuscript.

"Page 3968, Eqn 2: Replace "resisntance" with "resistance"."

Agree, will make the correction.

"Page 3968, Eqn 3: Replace "seepagefaces" with "seepage faces"."

Agree, will make the correction.

"Page 3970, Line 15: "zr-zs m for ys" seems to be a typo."

Agree, will make the correction.

“Page 3971, Line 21: “three different soil types”. Section 3.3. provides results for only 2 soil types. Explanation needed in Section 3.”

Agree, will make the correction.

“Figure 1: Correct the mismatch between “OF” in the figure and “FO” in the caption.”

Agree, will make the correction.

“Figure 1: According to Table 1, the term “Z” is the depth of soil layer. Hence “Z” in the figure should extend to the top of the “zs” but not the datum.”

You are right. We would correct Fig.1 to follow your suggestion.

“Figure 2: In y-axis replace Flux(m/d)” with “Flux(mm/d)””

We would replace as suggested.

“Figure 2:Caption: Replace “within year daily variation” with ““within year daily flow variation””

We will replace as suggested.

“Figure 2: Please explain how exceedance probabilities were calculated for each of the FDC component (i.e. SFDC, SSFDC, PFDC and regime curve). Does figure 2 mean that in 3 year time period, around 32% of the time surface runoff was observed whereas 100% of the time subsurface runoff was observed? Please explain in text. “

You are right. We will make the correction.

“Page 3972, Line 9: Replace “SSFDC, thin black curve” with “SSFDC, thin red curve” ”

We will replace.

“Page 3973, Line 8: Replace “gradient assumed” with “gradient” ”

Agree, will make the correction.

“Page 3973, Line 12: Need to explain “in-phase” and “out-of-phase” cases in this description for figures 3a and b.”

We would explain it in this paragraph of the revised manuscript.

“Figure 3: “In” and “Op” terms in labels should be explained.”

We would explain in the caption of Fig.3.

“Page 3973, Line 5: “semi-arid”. Other descriptions for R=1.5 states “arid”. Make this description the same throughout the text.”

Agree, will make the correction.

“Page 3973, Line 24: I suggest replacing “predicted” with “simulated” as there is no prediction of a future event in this experiment. “

Agree, will make the correction.

“Page 3974, Line 12: “silt” or “silty loam”? Correct this confusion throughout the manuscript.”

“Silty loam” is correct and we would use this consistently now the revised manuscript.

“Page 3975, Line 25-26: I think an important discussion is missing here. Shallow soil provides more flashy response (steeper FDC slope) whereas deeper soil provides more “sustained” flows due to increased moisture supply. Figure 5b shows that shallow soil simply dries out easily due to limited moisture content. More discussion along these lines should be digested into the text.”

We could expand the discussion. However, the reviewer is not completely correct here. Even though it makes intuitive sense that shallow soils leads to a flashier response, the reality is that in this case the fast runoff happens due to runoff from partial saturation areas formed by the rise of the water table, and therefore these two processes are inter-connected. Nevertheless we will explain this better in the revised manuscript.

“Page 3976, Line 1: “Fig. 5b and d” do you mean “Fig. 5c and d”?? “

Agree, will make the correction.

“Page 3976, Line 16: Correct the English usage. Perhaps “reason lower tail” should be replaced with “reason for lower tail” “

Yes. We would change to “reason for lower tail of the FDCs”.

“Page 3976, Line 25: Replace “zero” with “zero.)” “

Agree, will make the correction.

“Figure 6a: y-axis: Replace “Elux” with “Flux””

Agree, will make the correction.

“Figure 6: x-axis: Replace “Exceedence” with “Exceedance”.”

Agree, will make the correction.

“Page 3978, Line 29: Replace “variability” with “variability (PFDC)” ”

We will replace to “variability (PDC), as we are suggested to use PDC rather than PFDC.

“Page 3979, Line 16: Replace “only captured” with “only be captured””

Agree, will make the correction.

“Page 3979, Line 15: “water holding capacity” is this the correct term here? Or do you really want to emphasize “limited moisture content of the soil” which could be due to “shallow soil” or soil hydraulic properties.”

The reviewer’s point is that “water holding capacity” is a soil physics term, whereas what we mean is “storage capacity” which combines the depth of soil and the porosity

of the soil. We will replace water holding capacity with “storage capacity”.

“Figure 4b: y-axis: Replace “FLux” with “Flux””

Agree, will make the correction.

“Page 3980, Line 3: If you have monthly flow data then please explain what you mean by “ungauged” basin.”

This issue was raised by Reviewer #2 as well. This is how we responded.

There is a poor choice of words in the offending sentence and we will rephrase that. Essentially we were highlighting the main results that the FDC curve can be conceptualized as a combination of two major components: a fast runoff component and a slow runoff component. The fast runoff component reflects the precipitation directly bar a certain level of nonlinear (threshold) filtering. The slow runoff component (noting the caveats discussed in the paper, especially in arid regions) is reflected in the regime curve. This provides avenues for extrapolation from gauged to ungauged places, through the use of a 2-stage partitioning model of complexity appropriate to the region.

Firstly we will rewrite the last paragraph to reflect this. Secondly, this claim will be properly tested in the work that is currently in progress in 200 MOPEX catchments across the United States. Hopefully the reviewer will still remain engaged and help review that paper when it appears in HESS(D).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3961, 2011.