Hydrol. Earth Syst. Sci. Discuss., 8, C2672–C2680, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C2672/2011/


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

**Y. Yokoo and M. Sivapalan**

yokoo@sss.fukushima-u.ac.jp

Received and published: 11 July 2011

Anonymous Referee #2 Received and published: 22 June 2011

<General comment>

"The paper investigates the climatic and landscape controls on the flow duration curve by analyzing the outputs of a rainfall-runoff model. Based on the paper results, the authors formulate a conceptual framework for the reconstruction of FDCs in ungauged basins. In my opinion, while the paper reasoning and results certainly provide some useful insights in the knowledge of the possible dependence of FDCs from climate,



soil behaviour, et cetera, some of the main conclusions are very weakly supported and also the faith in the capability of the proposed method for the reconstruction of FDCs in ungauged basins is not justified, since neither any comparison with real data is provided nor a sample application to generated data is shown. I believe that the paper weakness mainly relies in the need of better clarify the authors thinking. Also I suggest they should better focus on the consistency between model results and the paper conclusions and perspectives."

First of all, we thank the reviewer for the constructive comments on our manuscript. The reviewer highlights several communication issues and also misstatements regarding interpretation of the results, which we are happy to address in full: see below.

The paper was originally intended as an attempt to understand the process (climatic and landscape) on the FDC. In addition to results pertaining to this original question, we also found that the results presented insights that might point the way to interpret the FDCs as resulting from a 2-stage (fast and slow response) filtering of precipitation variability. It is on this basis that we came up on the conceptual framework that might help synthesize FDCs in ungauged basins, or extrapolate from gauged to ungauged basins.

In a sense, this is a proposal/hypothesis generated by the application of a theoretical model, but it remains to be tested. This is the biggest criticism of the reviewer, which is a reasonable one.

There are two reasons that we did not test this in an actual catchment: (1) the model we have used is a completely theoretical/conceptual one, which can be used to generate insights (as in the case of Reggiani et al., 2000, and Yokoo et al., 2008), but inadequate to apply to real catchments – it captures the basic processes, but is probably too simple to simulate an actual catchment with all of inherent heterogeneity/complexity, and we will be dealing with parameter estimation and uncertainty issues, which are beyond the scope of the study and even beyond the capability of the model. The usefulness of the model is to have the physical basis to separate fast runoff from slow runoff and from storage and evaporation processes, which are very valuable; (2) there is already ongoing work to apply the conceptual framework (and using models of appropriate complexity and process realism, and NOT the model used in this paper) to 200 MOPEX catchments across the United States to decompose the FDCs into their fast and slow components and on this basis to develop a classification system that captures the regional variations of the FDCs. This work is maturing and the results will be submitted to HESS in the next few months.

By the way, there is another paper in HESS(D) by Carrillo et al. (2011), where a more sophisticated model is applied in 12 of the MOPEX catchments, where we evaluate the model also in terms of the FDC.

In summary, we believe and hope that the understanding of the process controls on the FDC and the development of the conceptual framework that can help synthesize the FDC are sufficient for this paper to stand alone. As can be seen in the paper by Carrillo et al. (2011) application to real catchments requires effort at calibration and parameter estimation on top of a model that can simulate site specific processes (including snowmelt, leaf phenology etc in cold/humid regions). Therefore we feel that the application of the framework is beyond the scope of this paper.

<Specific comments>

“1. Page 3962, lines 3-4: the FDC, in general, is not a stochastic representation of "only" the within-year variability of runoff. Independently from the approach used, annual or total FDC, it represents both the inter-annual and the intra-annual variability (see for example Castellarin et al 2004).”

The reviewer is correct, in principle, although we would argue that it is the within-year variability that dominates the FDC. We would make the appropriate changes to the manuscript to clarify this.



“2. Page 3962, lines 11-12, the model representation includes two components of runoff: surface runoff and sub-surface runoff, not any reference is made to base (groundwater) flow which is usually considered an important contribution to low flows.”

The reviewer is correct in that deep groundwater flow is not included explicitly in the model used. However, this does not invalidate the main conclusion that the FDC can be separated into one for fast runoff and another for slow runoff: the baseflow would then be lumped with shallow subsurface flow.

“3. Page 3962, lines 12-13: the authors state that the SFDCs (Surface Flow Duration Curves) can be approximated with the use of a simple, non linear (threshold) filter. While this assertion seems feasible, the authors do not provide a proof in the paper. Since the transformation from the PFDC (precipitation flow duration curve) to the SFDC is a main part of their conceptual framework for the reconstruction of FDCs in ungauged basins, I suggest they could at least show (or refer to) a simple practical result of a filtered PFDC.”

If we understand it correctly, the reviewer requests us to build another conceptual model to demonstrate that SFDC could arise from a simple (nonlinear) filtering of the precipitation. This is the only way to address the reviewer’s request. First of all, this is precisely what will be presented in the next paper on the 200 MOPEX catchments. Secondly, we believe that a visual comparison of the PFDC and SFDC that is presented several times already gives a fair indication that this is indeed the case. We relied on this to make our case.

“4. Page 3965, lines 11-13 and section 2.1: the sensitivity on climate is tested, mainly, by varying the dryness index. It seems that not any variation is made about the rainfall process neither in terms of annual rainfall amount nor in terms of length of wet and dry periods. This issue should be at least raised and discussed.”

First of all, the results we presented are on the basis of several years of synthetic rainfall. So the effect of any inter-annual variability of rainfall is implicitly included in the results. We did explore the effects of event characteristics (duration and inter-storm periods etc), and found that these are much less than the effects of seasonality. This explains why we chose not to present the effects of event characteristics. We would be happy to mention in the methodology section.

"5. Some confusion is made about the model presentation. At page 3964, line 27, the authors state that they use a quasi-2-D model; at page 3965 they refer to the use of the REW (representative elementary watershed) used by Reggiani et al (2000). In the methodology section, at page 3966, line 3, and at page 3967, line 11, they clearly state that a lumped model is used in order to keep the model simple. In the model description the use of a quasi-2-D technique is not even mentioned and should be well clarified. This issue is not trivial, because one of the paper main conclusion is relative to the need of a quasi-2-D model for a correct representation of natural processes and for the correction of the right tail of the FDC in the conceptual framework for the reconstruction of FDC."

We insist that our model (as the model schematic describes) is a quasi-2D model, the same way as the VIC model and TOPMODEL (and even the Stanford Watershed Model, and in fact several models in current use) are quasi-2D models. Even while they remain lumped in the sense of the state variables, they are still able to define partial/dynamic saturation areas and also saturation excess runoff through appropriate closure relations or parameterization. So like TOPMODEL, they are able to mimic some 2D effects even while remaining lumped otherwise, which is why we call our model a quasi-2D model.

"6. Page 3972, line 9, the SSFDC in Figure 2 is a red curve, not a thin black."

Thank you. We will correct this.

"7. Page 3972, line 9, the authors state that the SFDC is "a slightly filtered version" of the PFDC, which preserves the intermittence of the original series. This concept is then reported in the paper conclusions, page 3978, lines 7-8, "the filtered product retains



[rainfall] intermittency". This conclusion is not fully supported by the results shown, since, at least, one could say that it is not always true. For example, one can see in figures 4c and 4d that the soil type deeply affects intermittency. Then, this concept should be better explained or refined or revised."

Perhaps we have not been precise in our statements. We were trying to highlight the differences between SFDC and SSFDC. The statement that SFDC is a slightly filtered version of the PFDC is still correct (when one considers the SSFDC in most cases except very arid climates) does not retain any of the rainfall intermittency. The degree of filtering evident in the SFDC depends on watershed characteristics, as shown in the model simulations. The intermittency in the SFDC is by no means equal to that of the PFDC, and certainly requires a model that captures the essential abstraction processes at the surface.

"8. Page 3972, and conclusions: the authors suggest the use of the regime curve (i.e. the mean within year variation of streamflows) for reproducing the SSFDCs (Sub-Surface Flow Duration Curves) and investigate confirmation in the results presented next. Nevertheless, the hypothesis is not confirmed in all cases relative to arid climate (R=1.5, Figures 3e and 3f, Figures 6c and 6d) and also in cases with shallow soil (Figure 6d). In practice one may conclude that the regime curve is not suitable for representing the SSFDC in all cases where an ephemeral streamflow regime (with consistent duration of zero flows) is observed. The authors could comply with this comment by showing more results relative to the case R=1.5."

We would agree with the reviewer. In fact we refer to this issue already, and the reviewer has noted this (see below). In any case we would reinforce the comment in the revised manuscript describing that estimation of SSFDC from a regime curve may not work in arid climates, where there is significant durations of zero flows.

"9. Page 3975, lines 7-13, the authors find a contradiction of their results, in terms of relationship between the slope of the FDC and the complex of soil porosity and hydraulic conductivity. The reasoning adduced for explaining this contradiction are frankly too generic and should be reinforced this being an important feature of FDC."

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.

"10. Page 3976, section 3.5: here the authors find that "there is a higher tendency to generate [...] zero flows under dry conditions, in particular years" i.e. when ET is a dominant flux. This observation is quite trivial, they also show that their water balance model drops the streamflow to zero whenever ET is greater than the outflows from the saturated zone. Also, they acknowledge that the regime curve "has less chance to go to zero". The logical conclusion should be, again, that the regime curve is not suitable for representing the SSFDC in ephemeral rivers, showing intermittency in particular years, because the regime curve simply averages between zero and non-zero flows, thus it can't go to zero. Nevertheless they get to the unexpected conclusion that "evaporation from saturated areas cannot be modelled using lumped formulation". Now, in my life of hydrologist I have seldom read that a lumped model is unable to describe the natural process variability, this is the first time that I read that a lumped model would be unable to catch the variability of the output of a lumped model (see point 5) ! I think that the authors should here carefully reformulate their thoughts and conclusions."

We have already addressed the first part of this comment (the feasibility of using the regime curve for reproducing the SSFDC) – see above.



We do not understand his last point about lumped models. It appears that the reviewer has got mixed up in his own words. The last sentence is still factually correct, i.e., a lumped model cannot allow evaporation from saturated areas, because there is facility to separate saturated and unsaturated areas. The fact that out model is quasi-2D means we were able to capture this. In any case, this is a minor point, not really important to the arguments, so we will delete it.

"11. Finally, at page 3979, the authors formulate their conceptual framework for the reconstruction of the FDC in ungauged basins. This comprises three components: the first one exploits the filtered PFDC. The second one is "a simple two component model of the vadose zone coupled to a shallow subsurface flow model, ...". What is this model? Is it the same model they used for the simulations? or is it another model which is not described apart from this sentence? What is the role here of the regime curve ? The authors should deeply revise the explanation of this point. The third one is a "2-D model to simulate the dynamics of the near-stream saturated areas". This one looks like the model they used for the simulations. So does the third component of the conceptual framework coincide with the application of the water balance model from Reggiani et al. (2000), or what?"

The reviewer has got confused here and we take the blame for this. What we present here is a conceptual framework that reflects a 2-stage partitioning of the FDC: (i) the first one is a filtering of PFDC into SFDC, (ii) a second slow filter that transforms the precipitation – fast runoff (which can be deemed as infiltration or wetting) and converts this into SSFDC, and (iii) a correction for the 2D effects, which we feel may be important in arid climates. There is NOT any reference to the continued use of the Reggiani REW model.

We hope to clarify this better in the revised manuscript.

"12. Page 3980, line 3, the authors mention, among the data needed for estimating the shape of the FDC in ungauged basins, the "monthly flow data". What is the role of monthly flow data, in the conceptual framework discussed at point 11? And how monthly flow data could suppose to be available for prediction in ungauged basins, i.e. basins without flow records? I think that the authors should provide at least a sample application of the proposed conceptual framework in order to clarify the above discussed issues."

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.