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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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"The paper nicely presents and discusses the results of a series of numerical simulations of a lumped hydrologic model, aimed at investigating the major climatic and landscape controls on the FDC of a river basin. The final goal is the development of a conceptual framework to predict/reconstruct the behavior of FDC in ungauged sites. I found the paper clear, well written and well organized. The topic is appealing and surely relevant to water resources management and PUB. I thus suggest the paper to

be published after minor revisions. In what follows I provide some suggestions (mainly related to the presentation of the material), that the authors may want to consider before submitting the final version of the manuscript. "

Thank you for your supportive comments. Your suggestions below will surely contribute to improving our paper and we would follow most of your suggestions in the revised manuscript.

Most of the reviewer's comments are on the presentation, and we will follow these carefully and revise the manuscript accordingly, as much as possible.

<General technical comment>

"The declared objectives of the paper are twofold: use rainfall runoff simulation to investigate the dependence of FDC on the relevant physical processes; and use these results to derive a conceptual framework for the application to ungauged catchments. With respect to the first goal, I have the impression that some of the author' conclusions may be, as they are stated, somewhat trivial or simply a riformulation of concepts already known from the literature. For instance, it is not surprising that surface runoff impacts the durations of the highest stream flows, while the ordinary flows are mainly controlled by subsurface transport processes. Also, it is already known that the slope of the plateau in the FDC can be related to the type of subsurface response (slow groundwater vs quick subsurface runoff) dominating the catchment hydrologic response [e.g., Smakhtin, 2001; Castellarin et al., 2004; Vogel and Fennessey, 1995 just to cite a few of the papers already included in the refs of this paper]. This is not to diminish the value of the results shown in this paper, but only to encourage the authors to better discuss their findings in relation to what is already known. A per the second point, I think that the potential of the approach for the prediction to ungauged sites should be made a little bit more explicit using specific examples."

As you pointed out, we may have missed to cite some key references for results that are already in the literature. Following your suggestion we would make these citations

in each case, where appropriate.

On your second point, which is also suggested by other reviewers as well, we admit the necessity for introducing examples for discussing the potential of FDC reconstruction in ungauged sites. However, the main point of this paper is to demonstrate the feasibility of a simple conceptual model based on separation of total runoff into two fast and slow components, and then reconstruct them with the use of a 2-stage partitioning model.

The application of this conceptual framework to actual catchments is a work in progress (as mentioned in the submitted manuscript); we have indeed analyzed the FDCs for 200 catchments in the USA, and partitioned these in terms of the FDCs for fast and slow runoff components, and with the use of a 2-stage partitioning model have followed how precipitation variability cascades through the system. It is premature and beyond the scope of this paper to present these results here. The model we propose to use is not the one used in this study but one that actually includes cold region processes that are important for many parts of the USA.

<Detailed comments>

"p. 3962, line 4: I do not think FDC per se includes only the within year variability of flows. For instance long term FDC may include also the itner-annual variability. "

You are right and we will remove “within-year” from the sentence in the revised manuscript; however we argue most of what is captured is within-year variability.

"p. 3963, line 26: studieS "

Agreed.

"p. 3964, lines 4-18: I think the authors should distinguish between the papers describing the theory and the application of the stochastic dynamic model to which they refer here (maybe the ref list should be expanded with this respect). Note that in the series of WRR works of the authors cited here there is explicit reference also to the FDC and not only to the probability distribution of the flows. "

This is a minor distinction – the main difference is the way that the variability is presented: either as a form of the cumulative distribution or as a pdf. To incorporate your suggestion, we would try to distinguish the papers on “theory” and those on “application of the stochastic dynamic model. We will make this clear in the revised manuscript.

"p. 3964, lines 24-25: the issue of seasonality of precipitation on FDC has been already discussed by Botter at al., 2008] and cannot be listed as a difference between the two approaches."

We disagree with the reviewer. We went back and read the Botter et al. paper. The seasonal variations (ie month to month) of precipitations do not appear to have been considered – if it is, then there is no mention of how they rainfall event characteristics vary month to month. More importantly, the relative seasonal variations of precipitation and potential evaporation (either in phase or out of phase) are not considered either, and these impact significantly on the flow duration curve.

"p. 3964, last line: remove “of” "

We would remove “of” in the revised manuscript.

"p. 3965: lines 23-25: I do not think this is a shortcoming of tha paper. However, you should say something about the ability of the model used in reproducing observed streamflows. "

We will add to mention the model’s reproducibility of observed streamflow in the revised manuscript, as our results show similar tendency found in the literature.

"p. 3968, line 9: “resp”?? "

Agreed.

"p. 3969, line 5-6: which is the difference between “area fraction of the saturated zone” and “saturated surface fraction”? "

The “saturated surface fraction” is as described, a state variable. On the other hand,

“area fraction of the saturated zone” is equal to area fraction of water table which is assumed to be constant at 1.0. We will describe this better in the revised manuscript.

"p. 3971, line 8-10: why most of the simulated years (10) are disregarded? Do the effect of the initial conditions last for 10 years? How can you quantify this effect? On the other hand, are you sure that three years are enough to properly capture the intra-seasonal variability of rainfall-runoff processes? "

We think the questions you raised are important and difficult ones, regarding the effects of initial condition. But we also think that it is out of the main focus of this paper. We took a long enough spin-up period as long as 10 years.

On our use of three years long model output for our discussions, we believe it would be enough. It is because we are not discussing the variability at time scales less than monthly. We introduced seasonal amplitude to the intensities of precipitation and potential evapotranspiration both to be equal to their mean values and these would appear even in the three year long model output. We would add explanations similar to the above in the revised manuscript.

"p. 3972, line 15: state more clearly what the regime curve is, and the temporal resultion used to calculate it in this case."

We meant the regime curve as temporal variation of mean monthly flow within the year; we will present this definition in the revised manuscript.

"p. 3972, line 22-24: not surprising, indeed. "

We would briefly describe the results, already known in the literature, including citation to the key references in the revised manuscript.

"p. 3974, first lines: you should discuss more explicitly the implications of the departure between the FDC and the regime curve in arid climates within your framework. Also, consider that this difference may be simply related to the higher coeff. of variations of the flows in arid climates. "

Thank you for your suggestion. We would revisit Figure 3 for more explicit discussions. Also, we would examine how the coefficient of variation of the flows in arid climates affects this difference. We would describe for these points in the revised manuscript.

"Section 3.3: Smatkhin, 2001; Hughes et al., 2001 and Botter et al., 2009 already suggested similar results."

We would introduce their results in the main text of the revised manuscript.

"p. 3976, lines 13-15: again this seems to be similar to what explicitly suggested by equations of the stochastic model mentioned in the introduction."

Following your suggestion, we would cite the key references to the effects of evapotranspiration under arid climate in the revised manuscript.

"p. 3976, section 3.5: I suggest expanding this section, which is not as clear as the other sections of the paper "

Following your suggestion, we would expand this section to add more explanations for better clarity of this section in the revised manuscript.

"p. 3979, line 1: I would say “climate/soil dominated”: the soil seems to be as important as the climate. "

We would say “climate/soil dominated” rather than “climate dominated” in the revised manuscript.

"p. 3979, lines 10-12: rephrase."

We would rephrase this sentence as follows in the revised manuscript. “Previous work has explored the process controls on recharge (Struthers et al., 2006; McGrath et al., 2007; Harman et al., 2011), and on shallow subsurface flow in hillslopes (Harman et al., 2009).”

"p.3979, line 27: remove “that”. "

Agreed.

"Figures 3 to 6: I suggest including titles in the Figure plots: “IN-PHASE seasonality” and “OUT-OF-PHASE seasonality” for the two columns, and “TOTAL, SURFACE, SUB-SURFACE RUNOFF” for the three lines, to better disclose the difference between the plots."

Agreed; we will follow your suggestion in the revised manuscript.

"Figure 6, caption. I guess you should mention that default value of R is 0.5 when the soil depth is allowed to vary (as in Figures 6c and 6d)."

Definitely.

"Figure 7 and 8: do you have in mind semilog or a natural plots here? The choice impacts the position of the inflection points, and hence the graphycal extension of the ranges associated to the three sub-parts (and their relative importance). If you are thinking about semilog plots, maybe log(Q/Q_m) would be a better title fof the y-axis (units are immaterial and should be removed)"

We intended to draw a FDC with semi-log axes vertically for these two figures, and we will modify these figures with axes titles in the revised manuscript.

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