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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

Anonymous Referee #1

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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.

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 controleld 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 dimish 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.

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. p. 3963, line 26: studieS 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. p. 3964, lines 24-25: the issue of seasonality of precipitation on FDC has been already discussed by Botter at al., 2008] and can not be listed as a difference between the two approaches. p. 3964, last line: remove "of" 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. p. 3968, line 9: "resp"?? p.

3969, line 5-6: which is the difference between “area fraction of the saturated zone” and “saturated surface fraction”? 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? p. 3972, line 15: state more clearly what the regime curve is, and the temporal resultion used to calculate it in this case. p. 3972, line 22-24: not surprising, indeed. 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. Section 3.3: Smatkhin, 2001; Hughes et al., 2001 and Botter et al., 2009 already suggested similar results. 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. p. 3976, section 3.5: I suggest expanding this section, which is not as clear as the other sections of the paper p. 3979, line 1: I would say “climate/soil dominated”: the soil seems to be as important as the climate. p. 3979, lines 10-12: rephrase. p.3979, line 27: remove “that”. 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, SUBSURFACE RUNOFF” for the three lines, to better disclose the difference between the plots. 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). 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)

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