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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 #2

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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.

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).

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.

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.

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.

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.

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

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.

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.

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.

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.

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?

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.

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