

Interactive comment on “Stochastic or statistic? Comparing flow duration curve models in ungauged basins and changing climates” by M. F. Müller and S. E. Thompson

Anonymous Referee #1

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The paper: “Stochastic or statistic? Comparing flow duration curve model sin ungauged basins and changing climate” by Muller and Thompson presents the results of a detailed and extensive comparison between a purely statistic method for the estimation of FDC in sparsely gauged areas and a process based model. The comparison is done for a set of case studies in Nepal under current climate conditions, with a view on the implications of changing rainfall regimes in terms of prediction accuracy. The paper is very well written, the topic is timely and the results are very interesting. Therefore, I have to congratulate the authors for the work, which certainly required a lot of efforts (e.g. leave-one-out techniques, Monte Carlo analysis, etc). While I recommend the

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paper for publication in HESS, I would also like to raise a few comments/questions that could possibly help the authors during the revision of their paper.

General COMMENTS

Presentation - I really enjoyed reading the manuscript and I found the results really interesting. As far as I know this is the first rigorous benchmark of the FDC model developed by Botter et al in 2007 (and later extended by Muller et al. 2014) versus statistical techniques. However, even though I'm quite familiar with the topic, I found the text and the Figures quite dense. In fact, it took me some time to read the paper and understand all the details shown in the multi-panel Figures (24 plots, overall). I think the main reason for that is the huge amount of analyses performed and discussed in this paper – so I'm not sure there is a solution for the “high density” of the text. The subheading is certainly helpful to guide the reader across the different sections of the paper. Nevertheless, I'm wondering if the authors could try to make some more efforts to improve the readability of the manuscript somehow (e.g. adding more text to comment on the figures in the main paper, reducing the number of figures in the main paper and further clarifying some of the methods).

Stochastic model and parameter estimate – maybe this is a very trivial comment, but: the results the authors are discussing in the paper derive from the coupling between i) the physically based modeling of FDC of Muller et al., 2014 AND ii) a set of models used for the estimate of the model parameters. TO some extent, it is not known the amount of error in the prediction introduced by the model itself, and the amount of error introduced because of the particular choice in the procedures used for the model parameter estimate (e.g. compared to alternative methods). I would like to see the authors better stress this point while commenting their results (see detailed comment below). Clearly, testing several alternative methods to predict FDC parameters in the process based approach would be unfeasible here because of the huge computational efforts required. However, I feel like the performance of the process based method could be further improved by testing more sophisticated procedures for the parameter

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estimate (in particular, I'm concerned with the estimate of λ_P – see below).

Specific COMMENTS

Title: I'm wondering if “changing rainfall” would be more appropriate than “changing climate” provided that ET is constant and the only change considered in the paper is about rainfall regimes;

P9766, line 15: I'm not sure I would agree with this statement (but I can be wrong). Isn't the source of error a combination of violation of assumptions and errors in the estimate of model parameters (Figure 5c)?

P9769, line 12-13: are you referring here to the fact that the linear version of the model has a single timescale in the hydrologic response? In this case, I think the nonlinear version of the model proved to be flexible enough to model also catchment where the hydrologic response is more complex and include fast near-surface processes or deep groundwater;

P. 6769, lines 21-23: does the analysis presented in the quoted paper consider the same type of stochastic physically based model used in this paper? As far as I can tell, this is the first assessment of this type of stochastic model vs statistical methods; maybe it would be worth to clarify this;

P. 9772, lines 9-10. At this stage one may wonder why the recession coefficient a is not necessary (this becomes clear later)

P. 9772-9773: why not using the same procedure to estimate the mean flow both in the stochastic and in the statistic models? λ is a critical parameter of the stochastic approach, and its estimate requires quite some care, so I'm wondering which is the error introduced by the use of reasonable values of ET and SSC instead of more accurate methods.

P. 9774, line 12-16. It would be useful to mention the size distribution of the study catchments in the text. In fact, the size of the catchment might be an issue in relation

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with some of the model assumptions (uniformity of rain and soil). Looking at Table 1, it seems that the authors are using the model in a range of areas that exceeds the maximum size where the model has been previously tested (a few thousands of km²).

P. 9775. I have some concerns about the procedure used to calculate λ_P at the catchment scale. Essentially, if two point Poisson processes are independent, then the frequencies are additive. If the events are synchronous, the frequency of such events in the aggregated processes is preserved. Thus, in large areas like those handled in the paper, even though two sites are featured by the same rainfall frequency the aggregated process could be in principle featured by a quite different (and larger) frequency (whenever a fraction of the events in the two sites are not synchronous). This way, larger frequencies (and smaller intensities) would be associated to larger catchments, because of the larger probability that only a fraction of the catchment is mobilized. I presume the same may be true for T_d . Based on my previous experience, this could significantly improve the performance of the stochastic approach (see lines 16-18 at P. 9785);

P 9775: first equation: I suspect a “(-1)” is missing on the l.h.s. The rationale of averaging interarrivals instead of frequencies should be provided.

P 9775, line 13: I think you need to remove “on any given day”. You are interpolating the annual rain depths among stations here;

P 9778, eq (3). I suspect a “-“ sign is missing after “1”

P9780, section 2.3.2: I didn't find this section particularly clear, maybe the authors could expand a bit the text to better discuss the estimation procedure under changing climate.

P9780, lines 19-23: since in the current climate the recession params are estimated using eq (1), where Q_0 is indirectly dependent on the (current) rainfall regime, I'm wondering if changing rainfall parameters but keeping recession parameters as they

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are does not create a sort of “paradox” (Q0 should be different ina changing climate, thereby impacting the recession params).

P9782, lines 18-22: again, I’m wondering if this is related to the specific procedure used to estimate k, or rather it is a shortcoming of the method per se;

P9784, lines 3-5: “the estimation method for recession parameters makes the process-based approach more sensitive..”?

P. 9786, line 2: I would say “The estimation procedure of the params of the process-based model assumes..”

P. 9787, lines 6-9: again, I’m wondering to what extent this statement applies to the process based model, or to the combination of process-based model and the parameter estimation procedure;

P. 9789, line 12: the wording “non-seasonal” here could be misinterpreted (I know what you mean);

P. 9789, line 15: awkward sentence?;

P. 9789, line 26: the use of sporadic instead of erratic is intentional? Maybe “Erratic” could be preferred just to be consistent with the literature?;

P. 9790, lines 1-11: I like the idea of using the concept of resilience to assess the robustness of prediction. As a footnote, I’m a bit concerned about translating the resilience of streamflow distributions (as in Botter et al., 2013) to resilience of FDC using the NSC. While the general idea is the same, the numerical results can be different. As an example, two completely disjoint pdfs produces the same regime Instability RI (according to the notation used in Botter et al., 2013), but different values of NSC ad defined in equation (3) of this paper (depending on where the means of the two distributions are located). If you compute the resilience through differences in FCD, the changes of the flow pdf that involve the smallest discharges are likely to be weighted more, because the same frequency differences (in terms of pdf) are accounted for in a

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larger number of instances (i.e., for many values of Q). The issue becomes even more nuanced because of the log in equation (1), which smooth the difference in the largest discharges. Moreover, the compensation effects related to increasing lambda and decreasing alpha are particularly tricky (the resilience is the change in pdf divided by the active forcing). This is not a criticism to the proposed approach, but just a small note to highlight the fact that previous results in terms of streamflow pdf resilience could not be straightforwardly translated to variability of NSC and FDC.

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