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Interactive Comment

Interactive comment on "Effects of runoff thresholds on flood frequency distributions" *by* A. Gioia et al.

A. Gioia et al.

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Response to Reviewer 4

RC: In concept, the two-component methodology used is novel, but the physical explanation upon which it is based is fundamentally flawed in my opinion. Put simply, from my reading of the paper, runoff is only generated by a saturation excess mechanism, there is no threshold distinguishing "arid/rare" flood responses and "humid/frequent" responses, and therefore a two-component method seems to be incompatible with the runoff behaviour described by the paper. In my opinion, the description provided in the paper only supports a single-component model.

AC: We accounted for this criticism modifying the structure of the paper in order to better address the methodology and results. The two thresholds are estimated following two different methodologies and obtaining as a result, in both cases, that the first one represents an infiltration rate over a small portion of the basin, while the second is a storage threshold over a larger area. This is a significant result of the analysis and is deduced from the scaling behaviour of the thresholds with area.

RC: I'm not sure I'm convinced by some of the terminology used, such as the reference to "arid" and "humid" response types, when both response types are acknowledged by the authors to occur in most catchments - just at vastly different frequencies. It would seem to me to be simpler to refer to a "low contributing area/frequent" response type, and a "higher (variable) contributing area/rare" response type.

AC: We accepted this suggestion and revised the paper accordingly. The two mechanisms are defined as: theL-type (frequent) response and H-type (rare) response type.

RC: The use of a two-component model may be convenient and beneficial from the perspective of trying to improve statistical fit, particularly for the tail of the flood frequency distribution, and the authors may be able to justify the methodology on this basis. However, the current manuscript gives the impression that the two components represent two distinct runoff generation mechanisms distinguished by the exceedence/nonexceedence of a threshold; to the best of my understanding, this impression is false.

AC: We have to point out that, as in our response to the first comment by this reviewer, actually the difference between the two thresholds is one of the main results of this paper. A better fit with respect to other distributions is not one of our goals as we specify in the following point.

RC: Additionally, the results presented (primarily in Figure 5) do not unambiguously support statements made in the text (e.g., "Skewness of the observed distributions is always captured by the TCIF model") and in any case do not unambiguously demonstrate improved fit to data over the TCEV model it is compared against. While in most

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cases there is a visual improvement for rare events (i.e. the tail), performance is visually poorer at lower return periods for many catchments (no statistics are presented, and so the reader has to rely only upon a visual inspection of Figure 5). Either the authors have failed to include supplementary/supporting data that more clearly demonstrates improved performance in all cases, or else they are glossing over these deficiencies. Put simply, the results currently presented in the paper are insufficiently conclusive to support the idea that improved understanding has been demonstrated by this research. Even if the authors can demonstrate an unambiguous improvement in fit provided by the TCIF model compared to the TCEV model, I don't understand the methodology sufficiently to be sure that this relates to a genuine improved fit based on improved representation of flood generation processes relative to the TCEV (and/or better utilisation of existing climate/landscape data), or whether it simply relates to the increased degree of freedom in the TCIF model (5+ parameters) compared to the TCEV (4 parameters).

AC: In order to account for this comment we deeply revised the paper. In particular, a new procedure for parameter estimation was introduced, described in section 5.2.2. Then, the role of the TCEV parameters estimation was presented as a preliminary estimation of parameters $Lambda_L$ and Λ_H and also as a second, independent, proof of the scaling behaviour of the two thresholds. By means of a numerical procedure, the statistical fit of the TCIF distribution was improved. This is demonstrated by the visual comparison in figure 3 and also by the descriptive statistics in table 5 of the new paper. Nevertheless, it is important to state that we did not aimed to demonstrate that the TCIF distribution may perform better than the TCEV in terms of descriptive capacity. It would be unfeasible and unreasonable to compare results obtained by means of a four-parameters and a 15-parameters derived distribution. In order to avoid this confusion we eliminated the TCEV curves from figure 3, we only left the TCEV descriptive statistics in table 5 but, again, with the only purpose to show that the descriptive statistics are at least as good as those obtained from a TCEV distribution.

RC: It isn't clear to me from my readings of the manuscript exactly how these parame-

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ter values are obtained: by statistical fitting (calibration) against existing flood records, or by some independent evaluation (i.e. regionalisation methods or from data maps)? If the former, then the improvement may be due simply to the additional parameter(s). I had a hard time following the sections dealing with the evaluation of parameter values in each catchment, so the authors may be able to easily address this concern. Regardless, I think it is important that this issue is clarified in the text.

AC: We also accepted this criticism and revised the paper reorganizing the entire section devoted to parameters estimation. Now it is clearly stated which parameters were estimated independently from discharge measures and which one were estimated by statistical fitting against existing flood records.

RC: Abstract. Grammatical problems with second sentence; not sure of meaning.

AC: This sentence, and the entire abstract were rephrased.

RC: Section2: General comment: The main purposes of this section appear to me to be (1) establish the theory/equations behind the single component model, (2) demonstrate through review of literature the different behaviour associated with the two different flood generation "components" ("ordinary" and "rare" floods, with different process controls and behaviours), and hence the limitations of a single-component model. I find this story a bit hard to follow - possibly because there is too much detail? Maybe with more focus, this section can be more to the point, and easier to read.

AC: This section was slightly shortened removing some unnecessary comments and trying to keep more focused on the paper's objectives.

RC: (p908, lines 1-4) The explanation given here is unclear to me. To my understanding, the characteristic lag/response time τ_a (for a given contributing area, a) is a conceptual parameter which is defined as being equivalent to the minimum rainfall duration associated with the maximum flood peak (for the same contributing area, a) - by definition, these two timescales cannot be anything other than equal. I don't understand how the Fiorentino et al. (1987) paper elaborates on this, and finds only that these values are "close to" one another; and since the paper is a difficult paper to obtain, maybe it would be beneficial to explain it's relevance in more detail here. Or maybe this entire sentence is unnecessary in this paper?

AC: The lag-time is defined as the lag of direct runoff centroid to effective rainfall centroid while the critical rainfall duration is the one that maximizes the flood peak. Notwithstanding many conceptual models accepts the hypothesis that they coincide, in principles they are different quantities. Fiorentino et al (1987) observed on real data that the basin lag-time is close to the critical rainfall duration.

RC: (p908, line 6) Need to give a reference for the value 0.7. The assumption that the runoff peak is a fixed 0.7 times the net rainfall intensity (from the period t=0 to t=τ_a) seems to me to be a fundamental assumption of the derivations to follow, so it needs to be adequately justified.

AC: Also this assumption was made following Fiorentino et al (1987), now this is specified in the revised paper.

RC:(p909, line 22-24) I think a citation for the assertion that ε'=0.5 implies that runoff occurs only when the soil storage capacity has been filled needs to be given. If the whole of this paragraph relates to the findings of Fiorentino and Iacobellis (2001), then it must be more explicitly worded to this effect.

AC: We accepted this criticism and rephrased this sentence.

RC: Section 3: If the "scientific contribution" of this paper relates to the development and application of a two-component probabilistic model for estimating flood frequency, it would seem important to distinguish the approach from previous two-mechanism derived flood frequency studies (eg. Sivapalan et al., 1990, as referenced in the introduction). The authors should be very explicit as to the "new contribution" stemming from their work.

AC: This approach is completely different with respect to Sivapalan et al. (1990). The only common point lies in the hypothesis that two different mechanisms occurs in runoff generation. We respectfully believe that this approach is absolutely novel in the frame of derived distributions and this is clearly stated in the revised paper.

RC : (p917, line 24). Spelling mistake "...rather *than* to sample variability". - (p918, line 2). Suggest rewording: "...is more likely to be reduced by the advent..." rather than "knocked down".

AC: Both sentences were moved in the conclusions section and rephrased.

References

Fiorentino, M., Rossi, F., Villani, P.: Effect of the basin geomorphoclimatic characteristics on the mean annual flood reduction curve, Proc. of the 18th Annual Conference on Modeling and Simulation, Pittsburgh, part5, 1777-1784, 1987.

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Sivapalan, M., Wood, E. F., and Beven, K. J.: On hydrologic similarity, 3, A dimensionless flood frequency model using a generalized geomorphologic unit hydrograph and partial area runoff generation, Water Resour. Res., 26(1), 43-58, 1990.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 903, 2008.