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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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

Anonymous Referee #4

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—Overall merit—

The subject matter of the paper is clearly relevant for publication in HESS, dealing with issues relevant to flood prediction in ungauged and poorly gauged basins. The paper takes existing work in relation to derived flood frequency, and uses the concept of two runoff mechanisms associated with the exceedence/non-exceedence of a threshold to explain high skewness in the tail of observed flood frequency distributions, and then goes on to assess the ability of a derived two-component model to better capture/explain this skew for selected catchments in Italy.

While the methodology used is suitably novel, I recommend against publication of this paper in it's current form due to (i) incompatibility between the process description and the model conceptualisation, (ii) results insufficiently strong to support the conclusions drawn, and (iii) general problems with the readability of the manuscript. I will detail these criticisms in the section to follow.

My recommendation is for significant revision.

—Specific Comments—

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.

In more detail:

- 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.

- Even with this different terminology, from the description given in the paper, the underlying mechanism is saturation excess for both cases; for the "frequent" type, antecedent conditions in the riparian zone are close to saturation, and will immediately generate runoff once significant rainfall (i.e. enough to overcome other losses) commences. In comparison, for the "rare" type, the near-stream zone is contributing as before, with progressively larger areas contributing as soil further away from the stream "fills up"; this is a continuous - rather than a threshold - process. The "frequent/humid" and "rare/arid" event responses described in this paper merely represent the two end-member cases,

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but they are manifestations of the same physical process (i.e. saturation excess generated by some partial area of the catchment, with that portion increasing as rain duration/intensity increases for given antecedent conditions).

- Related to this, the authors describe the "rare" event responses as corresponding to the situation where the basin- wide storage threshold is exceeded (as distinct from the "frequent" event responses, where it is not). I see no such threshold, but instead a continuum. It is nowhere established in the paper that "rare" events actually correspond to the entire catchment being saturated; simply that the partial contributing area is larger for a "rare" event than for a "frequent" event.

- 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/non-exceedence of a threshold; to the best of my understanding, this impression is false.

Additionally, the results presented (primarily in Figure 5) do not unambiguously support statements made in the text (eg. "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 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). It isn't clear to me from my readings of the manuscript exactly how these parameter 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.

Finally, I will admit I had some trouble following some of the text in this paper. This may be partly due to my lack of familiarity with some of the methods and referenced used (unfortunately, I do not have access to registration-only journals at present, so cannot easily check the background of the work; the Iacobellis and Fiorentino, 2000, model that this work is based upon, for example), but it is also due to general readability. This runs the risk of undermining the technical merit of the work, so I would recommend extensive review of the grammar by a native English speaker to overcome this. Some important (to my mind) statements and assumptions are not directly attributed to a reference. Sometimes a reference is given in preceding or subsequent text, but it is unclear whether these references refer to individual sentences or whole paragraphs; the text should be rewritten in a way to make this clear.

—Technical Comments and Corrections—

Abstract:

- Grammatical problems with second sentence; not sure of meaning.

Section 2:

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- 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.

- (p908, lines 1-4) The explanation given here is unclear to me. To my understanding, the characteristic lag/response time tau_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?

- (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=tau_a) seems to me to be a fundamental assumption of the derivations to follow, so it needs to be adequately justified.

- (p909, line 22-24) I think a citation for the assertion that e'=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.

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

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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.

Section 5:

- (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".

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

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