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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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We would like to thank the editor for these very precise questions that give us the possibility to further clarify the main advances obtained by this work. We are surprised from some of the raised issues but we have to acknowledge that many (may be all) the questions raised by the editor are still due to lack of clarity of the manuscript. Thus, we assess the editor's comment in this response and attach a revised manuscript were we introduce some details previously missing and hopefully eliminate all possible confusions.

EC1a How does this paper differ from the previous paper by Iacobellis and Fiorentino (2000), which I am familiar with?

AC1a In the introduction the reader find that "*In the present work, we generalize the theoretical probability distribution proposed by Iacobellis and Fiorentino (2000) introducing a two component derived distribution where the role of runoff thresholds is emphasized.*"

Thus, the TCIF distribution introduced in this paper can be seen as a generalization of the IF distribution presented in Iacobellis and Fiorentino (2000). As well as the TCEV or the GEV distributions are generalizations of the Gumbel distribution, they are structurally well different. In fact, the entire section 3 is devoted to describe the new parts of the TCIF distribution. Nevertheless in order to further clarify this difference we introduced in the revised paper the equation (11) which represents the cdf of the IF distribution and the following sentence at the end of section 3: "*Notwithstanding the structural model complexity, the strong difference between the IF and the TCIF distribution is evident by comparing the respective cdf equations (11) and (21).*"

EC1b In both cases the derived flood frequency model was applied to a number of actual basins: Are the results you presented here any better than the previous results?

AC1b. Yes they are. Nevertheless we have to underline that 6 out of ten basins (5, 6, 7, 8, 9 and 10) were not considered in previous applications of the IF model (see section 4 of the revised manuscript). Only basins 3 (Sinni at Valsinni) and 4 (Basento at Gallipoli) were studied in (Iacobellis and Fiorentino WRR2000) while basins 1 (Celone at Ponte Foggia San Severo) and 2 (Venosa at Ponte Sant'Angelo) were considered in Fiorentino and Iacobellis WRR2001. It is well known that the concavity of the cdf curve in the gumbel probability plot represents the distribution skewness and all the case studies shown in figure 3 display a significant downward concavity. In particular for basins previously investigated: by comparing the probability plots shown in figure 3 for basins 3 and 4 with figures 2d and 2g in Iacobellis and Fiorentino (2000) it appears that the TCIF distribution performs a better fit to data with respect to the right tail (extreme events). The same behavior is observed for basins 1 and 2. This is not surprising because, as the reviewers already noticed, we introduced a distribution with

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more parameters (no one would be surprised to observe that a TCEV or a GEV distribution has a better fit when compared to a Gumbel distribution). Then, we decided not to introduce this comparison in the revised paper.

EC1c What have you learnt here that you did not know or appreciate before?

AC1c The main lesson learnt here with respect to our previous work: Fiorentino and Iacobellis (WRR2001) (commenting also on results of Iacobellis and Fiorentino, WRR2000) found that in basins climatically and geologically different, different runoff generation mechanisms may prevail and characterize the whole annual maximum flood distribution. Here we state that in some particular basins both mechanisms are present according to the season, the rainfall event and other factors. This is stated in first lines of section 3 "*Earlier applications of the IF model allowed to identify two different response types in basins of Southern Italy. In fact, depending on several factors including climate, geomorphology, soil- bedrock features, one of these mechanisms could prevail over the others. Nevertheless it is well known that, in general, different mechanisms may arise, in any basin, with different frequency and weight (e.g. Sivapalan et al. 1990). Thus, in particular basins, different mechanisms may coexist, being in turn responsible of peak runoff depending on the characteristics of the rainfall event and on the soil-bedrock antecedent conditions.*" We also conjecture that this double behaviour leads to high skewness of the annual maximum flood distribution. This important result, apart from some abstracts presented at previous EGU assemblies, was never presented in any journal paper. Important results regarding threshold effects are hereafter reported in AC2a.

ED2a If you look at the title, the main contribution here would appear to be the effect of thresholds? Or is it just the introduction of two different runoff mechanisms, which is the way I read the paper? Part of the reason for my impression is that the results do not reflect threshold effects, in the form of sharp break in the slope of the flood frequency curve. The results (to me) seem to be reflective of change of dominant runoff processes? This is consistent with the focus on skewness etc.

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AC2a We respectfully disagree with this editor's comment. In the revised paper we carefully distinguished between model's hypotheses and equations (section 3), results (section 5), and interpretation of results (in the conclusions). We perfectly agree that when looking at results their observation leads to focus on different mechanisms. This is certainly a very important feedback that one can catch from the interpretation of results. But if we look at the model's equations in section 3 and results in section 5, the key role is provided by the two thresholds introduced in equations (14) and (15), and their scaling behaviour which is supported by results shown in sections 5.2.1 and 5.2.2 (figures 2a and 2b). Other factors influencing the cdf are, the rainfall forcing, which is not process-dependent, and the distribution of contributing area which changes only in mean. Thus we agree that different runoff mechanisms are meant to deeply affect the flood generation process but they are analytically introduced and detected by means of the two thresholds. Following the editor's comment we were tempted to change the paper title in "Runoff Thresholds and Generation Mechanisms in Derived Flood Frequency Distributions" but we didn't, since we believe that the way to go for a better understanding of runoff generation mechanisms is well beyond this paper.

We also report in the conclusion section that

This interpretation of results is of particular interest within the ongoing research about thresholds dynamics in the hydrological processes. In particular, it is apparently contrasting with the hypotheses assumed by McGrath et al. (2005) which use a threshold rainfall intensity for infiltration excess and a threshold storage for saturation excess. Nevertheless one has to recognize that their runoff thresholds are used in a marked Poisson process with instantaneous rainfall events combined with a lumped water balance model while our runoff threshold are referred to rainfall events of critical duration equal to the lag-time τ_a *and they represent an infiltration rate averaged in space (over the contributing area a) and in time (over the lag-time* τ_a*). On the other hand we find good agreement with Sivapalan et al. (1990) and Sivapalan et al. (2005) that observed a change of dominant runoff processes from saturation excess to infiltration excess* *with increasing return period.*

ED2b What are the threshold on: my impression is that they are on intensity and depth (and they manifest differently at different return periods).

AC2b Yes they are. We regret that the manuscript was not clear with this respect and changed it as described here: we already introduced in section 2, that "*The scaling behaviour of f_a represents a significant signature of basin hydrological response. Fiorentino and Iacobellis (2001) also showed that a value of ε'= 0.5 means that runoff occurs when a storage capacity has been exceeded in the source area. [...] On the other hand, a value of ε'= 0 indicates the existence of a constant runoff threshold conventionally related to the average infiltration rate of the soil-bedrock system in saturated conditions.*" Then we also changed the description of threshold processes in section 5.2.1 "*- the L-type (frequent) response occurs when rainfall intensity exceeds a constant infiltration rate (1st threshold) in the source area a_L; - the H-type (rare) response occurs when rainfall depth exceeds a storage capacity (2nd threshold) in the source area a_H.*". In order to highlight this result we added also the following lines in section 5.2.1. "*In fact, we referred to the phenomenological interpretation provided by Fiorentino and Iacobellis (2001), which is here briefly resumed. In a simple scheme, if the runoff threshold corresponds to a constant storage depth W, the average infiltration rate in the critical duration τ_a is:*

$f_a = \frac{W}{\tau_a} \propto a^{-0.5}$. (27)

Thus, the storage threshold scales with a with a power law with exponent -0.5 as well as the lag-time a scales with a according to a power law with exponent 0.5 (Viparelli, 1963, Troutman and Karlinger, 1984, Iacobellis et al., 2002). Otherwise, if the runoff threshold corresponds to the gravitational infiltration rate c,

$f_a = \frac{c\tau_a}{\tau_a} = c$. (28)

Hence the rainfall intensity threshold is constant and the scaling exponent can be as-

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sumed equal to 0. Reasonably, equation (27) holds in areas where the expected antecedent soil moisture condition is dry while equation (28) holds in areas with wet antecedent soil moisture condition. For further details about this rationale the reader is kindly addressed to Fiorentino and Iacobellis (2001, section 5)."

We finally stated in the conclusions that

"*In fact, the scaling behaviour of the H-type runoff threshold corresponds to a storage threshold while the L-type runoff threshold corresponds to a constant infiltration rate.*"

EC3 I am surprised that the switch is from infiltration excess runoff at short return periods to saturation excess runoff at long return periods? Are you sure of this? This is different from my previous experience. If so, what is in the rainfall inputs that causes this?

AC3 Even in this case we regret to observe that the manuscript wasn't clear. Actually we say that the L-type is the "frequent" mechanisms, it is characterized by constant infiltration rate (intensity threshold) and could be interpreted as a saturation excess mechanism. On the other hand the H-type is "rare", it is characterized by a storage threshold (rainfall depth) and could be interpreted as an infiltration excess mechanism. In order to avoid confusion in the reader we systematically reported in the revised paper that the L-type process is "frequent" and the H-type is "rare".

EC Can you highlight differences between the previous (2000) work and this work, and thus highlight that the differences or improvements are a result of the improvement of the model, and especially the introduction of thresholds? Otherwise, this will become another "tuned" model.

AC Please find response to this comment in AC1 and 2.

EC The authors should think deeply about this, and make sure adequate explanations and discussion is included in the paper that separates the results here from previous results, and then attribute the differences to (1) change of dominant processes, (2) especially threshold effects. If the old model and this model both worked perfectly in comparison to observed data, then I will be concerned.

AC As already stated in EC1b, new results with respect to previous papers are introduced only for basins 1, 2, 3 and 4. Differences in the application, that were already discussed in the original manuscript, are now more clearly stated. Since we are now using a distribution with more parameters, it is not surprising that for all of them we obtain a better fit with respect to the right tail of the distribution. Also, if we look at dominant processes, in humid basins (3 and 4) it is still not surprising that they showed a good behaviour, mainly affected by saturation excess (L-type, frequent), when studied with a single component model. For better clarity about this we added the following lines in the conclusion section of the revised manuscript:

"*This is confirmed for humid basins 3 and 4 that were already studied by Fiorentino and Iacobellis (2001) and showed a saturation excess dominant mechanism when studied with a single component model.*"

It is less immediate, but still feasible, the interpretation of results provided for basins 1 and 2. A part of the conclusion section, which was already present in the manuscript after the first revision, is properly devoted to this purpose. Such results are confirmed by observations performed in many parts of Europe (e.g. Kirkby, HP1997, see also M. Kirkby's Dalton medal lecture in Wien , 2008) and can be considered important refinements of those obtained by Fiorentino and Iacobellis (2001).

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