

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

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I have followed the discussion and the authors' response to the four reviewers. In essence the reviewers have focused on the technical aspects of the work and the presentation. In this connection, the authors have responded to these comments satisfactorily.

However, I have broader questions about the aims of the work, the methodology and the conclusions arising from this study.

1. How does this paper differ from the previous paper by Iacobellis and Fiorentino (2000), which I am familiar with? In both cases the derived flood frequency model

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was applied to a number of actual basins: Are the results you presented here any better than the previous results? What have you learnt here that you did not know or appreciate before?

2. 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.

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

3. 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?

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.

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.

I look forward to having a discussion with the authors about these points and to seeing a revised version of the manuscript.

Cheers Siva

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