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Hydrology and Earth System Sciences Discussions

Interactive comment on "On the role of the runoff coefficient in the mapping of rainfall to flood return periods" *by* A. Viglione et al.

A. Viglione et al.

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We would like to thank A. Castellarin for his positive review and his useful comments which are addressed in the following (line numbers refer to the original manuscript).

1) RANDOMNESS OF THE RUNOFF COEFFICIENT

My only concern is relative to the degree of randomness that is present in the simple scheme adopted by the authors. The variability of event runoff coefficients for a given basin reflects the physical characteristics of the catchment and also its climate (see e.g., Gottschalk and Weingartner, 1998). The Authors point out that this variability should describe the antecedent soil moisture state of the catchment. Nevertheless, modelling the randomness of rainfall and runoff coefficient independently of one another, as done in section 4.1, could lead to inaccurate results. For instance, a mean or

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low value of rc could be adopted in the MC simulations for the rainfall-runoff modelling of a rainstorm that follows closely in time other significant rainfall events of the synthetic series. This necessarily impacts the simulated flow peak, and could have also implications on the resulting annual flood series. The utilization of a conditional probability for runoff coefficients is clearly out of the scope of this study. But the Authors could improve the discussion of this point.

In the present work, we do not generate a synthetic rainfall 'series', but a number of independent storms. This is one difference with the work of Sivapalan et al. (2005), which also models the interstorm period. For example, when we model 40 events in a year, this does not mean that it was raining only 40 times, but that we consider only 40 rainfall events as significant, i.e., as storms. In section 4.1, the assumption of "rc independent of the event storm" means that rc is caused by the antecedent rainfall, which is not modelled directly, and not by the storm event itself (i.e., rc embedds the antecedent conditions in a single value).

To better explain this point, we added the following text at the beginning of Appendix A: "We use a simplified version of the rainfall and rainfall-runoff models presented in Sivapalan et al. 2005. The main simplifications are that we do not consider seasonality and do not generate a continuous series of synthetic rainfall but a number of independent storms."

We also changed "flood producing storms" to "event storms" in the title of section 4.1 because we use the wording "flood producing storm" for the maximum annual flood only.

2) EVENT RAINFALL OR ANTECEDENT RAINFALL?

The threshold behaviour of runoff coefficients (see section 4.2) is analysed in the study with respect to event rainfall volume. Merz and Blöschl (2009) show that antecedent soil moisture conditions control runoff coefficients to a higher degree than event rainfall. The Authors decided to model the antecedent conditions by means of rc random *variability (see previous point). The antecedent soil moisture conditions could also be roughly approximated by the 5 (or 10, or 30) day antecedent rainfall depth (see e.g. Merz and Blöschl, 2009). A threshold behaviour could have been modelled with respect to the antecedent rainfall volume from the syntethic series. Even though I doubt that this modelling strategy would have changed the main outcomes of this study significantly, a discussion of this point would enrich the presentation of the results.*

The answer to point 1 above is applicable also to this remark.

Section 4.2 analyse the case of dependence of the event runoff coefficient from the event storm. In section 4.2.1 (2 possible rc) this dependence is complete, i.e., there is no antecedent condition involved. In section 4.2.2 (continuous distribution of rc) the antecedent condition is accounted in the randomness of rc (see answer to point 1).

3) WHAT RUNOFF COEFFICIENT GIVES A 1:1 CORRESPONDENCE OF TP AND TQ?

In my opinion, the way in which the manuscript is structured and, in particular, the current organization of subsections 4.1.3 and 4.2.3 may be misleading, in the sense that it probably stresses a relatively marginal problem. Is it really important to know what runoff coefficient results in a 1:1 correspondence of TP and TQ? Viglione and Blöschl (2009) and this manuscript clearly point out that the main hypothesis of the design storm procedure is an oversimplifying assumption, which is scarcely applicable even for a "simplified world". From an engineering viewpoint, I believe that the step forward is the identification of the TP that should be used in a design storm approach to obtain the desired TQ for an average (or median) rc value. In the best case scenario, a few empirical rc values are available for the basin of interest, corresponding to observed peak flows (or annual floods). In this case the estimation of the mean or median of the rc parent distribution (or the distribution of the flood producing runoff coefficients) is viable, whereas the 10% or 90% quantile could only be guessed.

This is a very good suggestion. Actually fig. 5 answers to the question of which TP

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should be used in the design storm method to obtain the desired TQ using the median rc (black lines). We changed the title of sections 4.1.3 and 4.2.3 from "What runoff coefficients give a 1:1 correspondence of TP and TQ" to "Choice of the runoff coefficient in the design storm method". In Sec. 4.1.3 we added the text: "In this section, we examine what is the result of the design storm method when choosing different runoff coefficients. In particular we comment on the result of the design storm method when choosing the commonly used median value of rc showing that generally, in our simplified world, this does not give the correspondence TQ=TP." and also, referring to Fig. 5: "Reading the graphs, the black line provides the storm return period TP that should be considered to obtain a flood return period TQ when using the median flood producing runoff coefficient in the three systems. In the dry system, one should use a value of TP close to 1000 years to obtain TQ=100 years and the ratio TQ/TP changes a lot depending on the desired TQ (i.e., TP should be chosen smaller than TQ for TQ < 10 years). In the wet case, instead, one should always choose TP > TQ, e.g. TP$\approx$300 years to have TQ=100 years."

For the systems with threshold effect in runoff generation we added a figure (the new Fig. 11). The following text has been added in Sec. 4.2.3: "The coloured lines of Fig. 11 show the mapping corresponding to the critical storm duration tr* (i.e., the result of the design storm method) when different rc are selected for the three systems analysed in Fig.9. The black line refers to the median flood producing runoff coefficient. In all three cases, using the median runoff coefficients produces flood return periods that are very different from the rainfall return periods. Comparing Fig.11 with Fig.5, one sees that the ratio TQ/TP strongly depends on the desired TQ when the threshold effect is present. This would be expected because of the different percentage of under-threshold and over-threshold events for different values of TQ (see Fig.9, Panels a, c, d), i.e., different mechanisms dominate for different flood magnitudes. The graphs can be used to select TP so that the design storm method results in a flood with the desired return period TQ. If considering the median flood producing runoff coefficient, with a value TP=1000 years one would obtain TQ$\approx$70 years in the system with high

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threshold, while one would obtain TQ≈20 years only in the system with low threshold volume. This is a clear example of how wrong can be the assumption TQ=TP of the design storm method when the design runoff coefficient is not correctly selected."

Regarding the first part of the comment, we think that the evaluation of the optimal runoff coefficient that gives the 1:1 mapping is not a marginal problem because it corresponds to the general assumptions when applying the design-storm-procedure. In the artificial world analysed in the paper this assumption can be checked and we do it. We also added a section entitled "Biases in the design storm method when assuming TQ=TP and the median rc" to address the practical question about the consequences of the application of the design storm method assuming TQ=TP and choosing the median rc as design runoff coefficient (see response to J. Skøien).

4) DISTRIBUTION OF RUNOFF COEFFICIENTS, p. 637, 638, Fig. 3

I would suggest to refer to CVc (instead of standard deviation or variance) throughout the text (equations 2 and 3, and examples on p. 638). This would be consistent with the information reported on Fig. 3.

We added CVc.

5) Please check "practise" on p. 638

Changed to "practice"

6) Line 5 on p. 639, the median is expressed by a unclear notation, which should be explained. Moreover, is the notation really useful?

Actually not, we removed it.

7) From line 5 on p. 645 - "If a continuous deterministic relationship (...) (not shown here) (...)";. I think that showing some of these results would enrich the presentation.

The graphs of the mapping TQ-TP would be exactly as in Viglione and Bloeschl (2009). For space reasons we prefer not to repeat the figures here.

8) Fig. 2, panel e) - the ratio for the central case is 2, as reported, or 1?

It is 2. The case with ratio 1 is given in panel a).

9) Fig. 4, panel b) - the envelope line does not seem to describe the results of the Monte Carlo simulation as effectively as in panels d) and f), and it is not an envelope. Please check its correctness.

The envelope curve correspond to TQ=100 years (exactly), while the Monte-Carlo points correspond to TQ between 50 and 200 years (see line 14 page 634). That is the reason why one can have points above the analytical curve (see e.g. fig. 2 panel b).

10) Caption of Fig. 8 - please include "(TQ = 100 years)" after "slices" as in caption of Fig. 9.

Included in captions of fig. 8.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 627, 2009.