Hydrol. Earth Syst. Sci. Discuss., 6, S271–S276, 2009
www.hydrol-earth-syst-sci-discuss.net/6/S271/2009/

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.

e. gaume (Referee)

gaume@lcpc.fr

Received and published: 10 March 2009

General comments

The manuscript builds on the so-called design storm procedure and analysis in particular "the correspondence of the rainfall return period and the flood return period" (first sentence of the abstract). I feel divided about it. The presented work is of good scientific quality, well presented, with an attempt to propose some guidelines to support the analyzes (theoretical upper limits). But some initial choices lead to relatively complex results and some important conclusions could be better emphasized. Both aspects are developed in my detailed comments. To summarize my opinion, concerning the initial methodological choices, the manuscript does not refer to the well established result on which the also well-known rational formula is based: i.e. the peak discharge of a

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

watershed is highly correlated to the maximum average rainfall intensity over a duration close to its time of concentration when the runoff coefficient is constant. On such a subject, this can not be ignored. At least, alternative analyzes should be included, relating the flood peak discharge return period Tq, to the return period of the rainfall event T'p according to its maximum average intensity over the time of concentration of the watershed (1,8 to 2 times the response time in the case of the linear reservoir). I am quite sure that figure 1.a will be much clearer (1 to 1 relation with a slight dispersion around the first diagonal due to the shape of the event – its duration in the present case, and a larger dispersion due to the dispersion of runoff rates). Concerning the conclusions of the manuscript, the fact that the described design storm approach is inappropriate, especially for "dry" catchments should be underlined. In this case, the return period of the flood peak discharge is strongly determined by the return period of the runoff coefficient which is chosen arbitrarily. This is an important conclusion for practitioners. Likewise, the general tendency of the runoff coefficient to grow as the return period of the flood peak discharge grows is also an interesting result that is consistent with other publications (see detailed comments). It deserves some more discussion. Is it in particular linked to the specific stochastic rainfall and rainfall-runoff model or is it probably a general figure for rainfall-runoff processes? Two final comments to conclude. The way the authors compute the return period of the design storms is unfamiliar to me (see detailed comments). They should demonstrate that it corresponds to common engineering practices. I have some doubts on this. A more rigorous definition and use of the term return period would be welcome (see detailed comment). As a conclusion, this manuscript presents and interesting and useful work but would be highly improved, and especially gain in clarity, if the suggested modifications and additional analyzes were realized.

Detailed comments

About the design storm procedure and the choice of the variables defining the rain event return periods:

HESSD

6, S271–S276, 2009

The design storm procedure, initially developed in urban hydrology to my knowledge, is a non-satisfactory last-resort solution used sometimes in rural hydrology by consultants, when limited streamflow data is available or extrapolation is needed. As illustrated in the paper, this approach, if applied as proposed in the manuscript and not using the well-known rational method (see my last comments), is inappropriate as its basic assumption (i.e. equality of flood and rainfall return period) does not hold. This is never really acknowledged by the authors. Considering this, is it necessary to develop on this method, and in a way promote it, in a high quality scientific journal? Moreover, and this is my major criticism, the problem as defined in the manuscript is ill-posed! Return periods (i.e. probability of exceedence or distribution function values) can not strictly speaking be attributed to rainfall events and floods but to some of their characteristics: peak discharges, average intensity over a given duration (IDF curves)... Misnomers as flood or rainfall return periods should not be reproduced in scientific papers which role is to clarify problems and not to add to the general confusion. The intention of the authors to stick to what is presented as a common engineering practice - I also have some comments on this (see conclusion of these comments)- leads to non completely rigorous choices from a statistical point of view and finally very complex results and analyzes. Let me explain this. What is studied in the paper is the link between the return period of the flood peak discharge, which is a consistent and well defined statistical variable, and the return period of the average intensity of the rain event over its duration which can be computed of course but which is inconsistent and an ill-defined statistical variable. First, the intensity i of the rain event over its duration is not the same variable as the one used to construct the IDF curves: i.e maximum average intensity of the event over a pre-selected aggregation time. IDF curves can eventually be used to compute some value Tp linked to i, but it should not be called return period since the occurrences of i in the observed or simulated data sets will not be consistent with Tp. Moreover, the considered duration changes from one event to the other: the various Tps are computed using different IDF curves which is also not consistent. Tp is something like a statistical UFO "Unidentified Flawed Object" with complex links to the IDF curves even in the simplistic case considered in the manuscript (rainfall events rectangular pulses, see appendix of the paper). It would be very useful for the readers to discuss this somewhere. Finally, the authors argue that the approach presented corresponds to common practice. I totally disagree with this: the most common practice is the so-called good-old "rational method", very similar to what is presented except on one major point: the considered intensity is the maximum average rainfall intensity of the event over the time of concentration of the watershed!!! In this last case: no problem. The considered time window is constant and it is the same statistical variable as the one used in the IDF curves. The rainfall-runoff model used in the manuscript is based on a linear reservoir which has an infinite time of concentration. To apply the rational formula, the critical storm duration should be considered in this case: something like 1.8 the response time of the reservoir.

About the possible generalization of the obtained results:

It is praiseworthy to try to give some orientations about the possible effects of the variability of the runoff coefficient between and within storm events, but probably hopeless, as these conclusions will be tightly linked to the model used to derive the peak discharges or to the watershed. What is almost certain is that the runoff coefficient to be considered in the rational formula has a general tendency to grow with the return period a does generally not exceed 1: see discussion on this issue in a recently published paper in HESS (Gaume, 2006). The rest is very open.

Comments while reading the text:

P628, Line 1: "rainfall return period" and "flood return period" remove these vague notions and be more precise on what exactly are the studied statistical variables.

P628, Line24: "typically performed using the design storm". It is typical in urban hydrology where runoff coefficients can be considered as constant, but not when they vary in rural catchments. If it is a common practice in some countries, should it be considered as judicious? Apparently no, according to the manuscript. Please describe somewhere

HESSD

6, S271–S276, 2009

Interactive Comment

the design storm procedure and especially which characteristics of the storms are considered to select this design storm: certainly not its average intensity as it is the case in the paper.

P629, Line 1: The return period of a storm does not exist strictly speaking. Please be more precise. Lines 1-9: equality between return periods hold if the right characteristic of the rainfall event is considered and if the runoff coefficient is constant. This has been often verified and is at the origin of the rational formula.

P632, L1-2: No, the storm duration has no relation with the aggregation level!!! This corresponds to a wrong application of the design storm method based on IDF curves! Errors, if they exist, should not be propagated. Same remark for lines 17-21.

P633, L5: provide a precise definition of the response time of a linear reservoir, if necessary, recalling the equation of the reservoir.

P633, L9-12: This choice does neither seem consistent with the design storm procedure, nor with the IDF curves (see general comments).

P634, L11: This Tr* seems to be the characteristic time that should be used to computed the return period of the event according to what I think is the design storm procedure.

P638, L7: deltac and sigmac have not been defined.

P638, L7-24: the explanations are very complex. What comes out is that in the dry system, the return period of the rare events is controlled by the return period of the runoff coefficient rather than the return period of the rain event intensity. This is typically a case where the design storm procedure will be inefficient.

P639, L15-20: The back-computation of runoff coefficients is a very interesting approach.

P640, L1-5: The fact that r1:1 is greater than the median value is very interesting, but

HESSD

6, S271–S276, 2009

Interactive Comment

is it still the case if the return periods are correctly computed?

P640, L20: Part 4.2.1 is mostly descriptive and the results obtained quite complex. Various phenomena are interplaying which are difficult to separate on figure 8. In particular, the rainfall event volume is not independent on its duration which may explain the two peaks in figures 8.b, d, f. It is not independent on the rainfall intensity neither which may explain the shape of figures 8 a, c, e. By the way, do the lines correspond to analytical results in figure 8.

cited : Gaume E., 2006. On the asymptotic behavior of flood peak distributions. Hydrology and Earth System Sciences, 10(2),233-243.

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

HESSD

6, S271–S276, 2009

Interactive Comment