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Hydrology and Earth System Sciences Discussions

Interactive comment on "A look at the links between drainage density and flood statistics" *by* B. Pallard et al.

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GENERAL COMMENTS

The study by Pallard et al. (2008) investigates the links between the drainage density of a river and selected flood statistics, i.e. the mean, standard deviation, coefficient of variation and coefficient of skewness of annual maximum series of peak flows. The addressed questions are indeed relevant in the scope of HESS, given the increasing attention that the hydrologic community is dedicating to the problem of prediction in ungauged basins. Recognizing the pattern of the relationships between drainage density and flood statistics, in fact, would allow one to estimate to what extent a river basin is prone to extreme floods on the basis of its drainage density. The analysis is performed

in three steps: 1) a numerical simulation through a spatially distributed rainfall-runoff model, 2) an analytical analysis of the links between the drainage density and flood statistics, 3) and then the case-studies. My main concerns are about the feasibility of some of the hypothesis behind the second step, that seem to lead to an erroneous representation of the infiltration process. More on this point is reported in the next section, together with a list of specific comments on other issues that are unclear to me. Technically the manuscript is well written and organised. I recommend the publication of the paper after revision.

MAJOR COMMENTS

From 2905-13 to 2905-20: Being myself a "minimalist"; modelling person I believe in this type of approach. My concern here is not on the type of approach but on the plausibility of the representation adopted for the net rainfall estimation. To describe the mean areal intensity of rainfall losses during the storm (rp), the authors adopt the Horton's equation (eq. 3). Given an event of duration d, with this model one obtains a reduction of the infiltration rate (given by the negative exponential) from the initial time to d. Here, in order to have a simplification of the integral form in (4), i.e. to work with a constant rainfall, the authors evaluate the losses rp at the time d and then subtract this value to the mean areal rainfall intensity rl. This however leads to unrealistic results. Let's consider, for example, two events with the same volume but different durations. In the real world we all know that higher runoff volumes derive from short and intense precipitation events. With the current formulation of the model what the authors obtain is exactly the opposite, since rp(d) decreases with increasing d, and this could eventually modify the mode of variation of CV with the drainage density. A possible solution to this problem could be to integrate the instantaneous infiltration losses evaluated with the Horton equation from t=0 to t=d and then to take the mean in order to preserve the assumption of constant precipitation.

From 2907-8 to 2907-17: I believe there is some problem with the units of the parameters values. In fact if I take the optimal value of drainage density (Dd=1) from figure 1

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and substitute it in equation (8), together wit the suggested values for A and M, I find tc=4 seconds. Line 2908-2: check also the units of the parameter epsilon: a value of 0.8 seconds sounds strange to me.

Line 2908-16-21: this concept is unclear.

From 2911-4 onwards (and figure 4): The effort of comparing the model results with the reality is indeed a surplus value for the study even if the empirical points in figure 4 show a significant scatter. I agree with the authors when they state that the moving average reported in the diagram do not contradict what was inferred through the previous investigations. However I believe that an indication of the significance of the linear regression (even if not significant) would be of help for the reader.

MINOR COMMENTS

Line 2902-17: it is written: "The links... is inspected". Please verify Line 2903-19: a parameter A0 is introduced that is not defined before

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