

Interactive comment on “Application of runoff coefficient and rainfall-intensity-ratio to analyze the relationship between storm patterns and flood responses” by N. W. Kim et al.

Anonymous Referee #1

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This manuscript attempts to develop a better understanding of the link between storm and catchment runoff pattern at different scales based on analysis of runoff coefficients and rainfall-intensity ratios. I do acknowledge the good intention of the authors: in principle, the objective of the paper is of some relevance and a well-designed experiment analysing the suggested factors could potentially reveal interesting aspects of catchment functioning. I however do have some serious concerns about the methods in this paper, which do not seem to have been developed really to a sufficient degree, and which may potentially cause a misleading interpretation of the results. I therefore think that the paper cannot be published in its current form and needs a considerable re-think and major re-design of the experiment to become a meaningful scientific publication.

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My main concern is the model and the way it is used in this analysis. I do understand that model predictions may serve as supporting information in an experiment where insufficient observations are available. As significant scaling relationships are to be expected only above a certain catchment area (as noted by the authors and also supported by several references they provide), I am surprised that the authors chose to focus their work on a wide range of relatively small sub-catchments (<100km²). Clearly, at such smaller scales, observations are indeed in most cases unavailable. The first question arising for me was therefore, why did the authors not turn to larger river basins, for which observations in a sufficient number of sub-catchments (which can be relatively large, say >100km²) are more readily available?? In times of increasing awareness for open data, such observations are quite accessible, and in many cases freely available for download.

Let us keep the data issue aside for a moment and let us assume that we actually need modelled data for an analysis as the suggested one. A meaningful interpretation of modelled variables critically hinges on the model and the way it is used. In other words, to avoid misinterpretations, it is indispensable that the model and its parameters provide a suitable representation of real-world processes. This, however, can only be assumed if the model, at the very least, allows for the dominant processes occurring in nature and if the model is rigorously tested. I am very doubtful that the model predictions in this paper reflect real hydrological dynamics for two reasons. Firstly, the model used, although referred to as a “physically-based” model (obviously for applying kinematic wave, green-ampt and darcy), seems to omit highly relevant processes: neither in this paper, nor in the Choi et al. (2015) paper cited by the authors and which appears to introduce the model used here, any mention is made of evaporation and transpiration. It must therefore be assumed that these processes are not accounted for. How complete, and thus how trustworthy, is a hydrological model that does *not* account for evaporative fluxes, that are in many environments larger than drainage?? Besides the representation of the relevant processes, another major problem with the application of the model in this paper is the way the parameters were selected. The

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model was calibrated using merely one single objective function and post-calibration only evaluated using two further objective functions. In spite of these low number of model constraints, no convincing model performance was achieved (although claimed otherwise by the authors). It is quite stretch to assert that a model is working when it is only calibrated to individual events with only one objective function (NSE), which then for some cases does not even exceed a value of 0.75. more importantly, the results shown in figure 3 illustrate that the model does not at all work for the socheon gauging station, where it exhibits particularly poor skill for 8 out of 20 events. How can such a model be assumed to be a valid tool for predicting flows in ungauged locations?? In addition, why was the model calibrated on the individual events and not on the entire period of observations. this would have increased the confidence in the model results at least a bit.

Other comments: (1) It does, throughout the manuscript, not become clear what the actual research question is. Which scientific hypotheses do the authors want to test? This needs to be made explicit in the introduction section

(2) The spatial data extension method is sold as a new method. This is quite an exaggeration in my opinion. What is done is that the model parameters obtained from calibration of a catchment are transferred to sub-catchments of the calibration catchment. There is nothing particularly novel in that, as also illustrated by the references given in the paper. in addition, it remains absolutely unclear how the parameters that are inferred from "geophysical catchment characteristics" were determined and varied between the catchments. This needs to be specified and justified in detail.

(3) P.5,l.94-101: this better fits into the methods section

(4) P.5,l.102-1-4: this is irrelevant and can be condensed.

(5) P.6,l.122; fig.1: please add the 27 rain gauges in the map

(6) P.6,l.123: why were only 20 events chosen? How were events defined? How were

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different antecedent wetness conditions accounted for?

(7) P.6,l.139ff: why was the model not calibrated and evaluated according to more objective functions (e.g. NSE of logQ) and catchment signatures (e.g. can it reproduce the observed flow duration curves, peak distributions, limb densities, autocorrelation functions, . . .)?

(8) P.8,l.174: which information does the metric rmod give us? In how far is it different to the correlation coefficient?

(9) P.9,l.202: vertical percolation *only* under saturated conditions? That assumption lacks any physical evidence, really. If it was true, how can then a soil ever reach field capacity?

(10) P.9,l.205: initial conditions are just that. They are not model parameters! Why are values that can be readily and robustly estimated from topographic data, such as the slopes of the land surface and the channel used as free calibration parameters here?? Why are, on the other hand, values that cannot be observed at the scale of interest (here 200x200m), such as the effective soil porosity or hydraulic conductivity set to fixed values?? How were these values determined?? What is the justification??

(11) P.11.249ff: why was the model, if already not calibrated to the entire data series, not calibrated to all events simultaneously?

(12) P.12,l.280: in hydrology we *cannot* verify anything, in particular when the model itself is not very realistic. The best we can do is to "test" our models.

(13) P.13,l.325: I am far from being convinced that using a fixed values for TC allows for a meaningful analysis. At the very least, please justify why equation 9 is deemed to be suitable.

(14) P.14,l.335: it is true that C partly reflects mean rainfall intensity. But it also, and maybe more importantly, reflects antecedent wetness conditions, which are not at all considered here. why?

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(15) P.15,l.362: why is this ratio introduced here? what is the idea? What is the justification?

(16) P.16,l.387: for a meaningful interpretation of the results, model uncertainty has to be reported here and incorporated in the subsequent analysis (how does it propagate through the analysis? How does it affect the interpretation?)

(17) P.17,l.413ff: fine, but so what does this tell us? What can we learn (assuming for a moment that the model is a plausible representations of reality and results are meaningful)??

(18) P.17,l.432: “dependent” and “independent” is not very clear. Please rephrase

(19) P.18,l.435;fig.5: please use same scales on figure axis to allow a better comparison

(20) P.18,l.441-442: how do you know that this is due to more retention and infiltration?? This is a sweeping and speculative generalization. What about the effect of antecedent wetness?

(21) P.21,l.534ff: for a meaningful interpretation, the results need to be separated into classes of different antecedent wetness.

(22) P.23,l.575ff: so what does that mean? What do we learn from that?

(23) P.24,l.612-613: no, it is not out of the scope of this study. If this study wants to make a meaningful contribution, a robust model uncertainty analysis is critically required to avoid misinterpretation of the results.

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