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Discussions

Interactive comment on "On the Relationship Between Flood and Contributing Area" *by* Christopher Spence and Samson Girma Mengistu

Anonymous Referee #1

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I have completed my review of the paper "On the relationship between flood and contributing area" by C. Spence and S.G. Mengistu, submitted to HESS. This paper attempts to investigate the relationship between streamflow (Q) and contributing area (A_c) using a mix of modelling and very limited data. The goals of the paper, as indicated by the authors are (1) "to test the hypothesis that the relationship between a catchment's floods and contributing area is a power function" and (2) to compare contributing area and flood frequency distributions. While the context and motivation for the work is sound and very well expressed (the introduction was a very enjoyable read), the execution of the methodology and the interpretation of results were plagued by a number of critical problems. I have three primary concerns about this paper which I believe make it unpublishable, even with major revisions. These are reflected in my

major comments #1, 2, and 3 below.

Major Comments (1) One of the critical issues in this paper is that the authors used a model which implicitly assumes a functional form between contributing area and discharge from the landscape. The PDMROF model used here, based upon the Probability Distributed model of Moore (1986), generates runoff as the water applied to the percentage of water stores which are filled to capacity, effectively an approximation of saturation excess on a heterogeneous landscape. While the Q-A relation individual flood events will be modified by the available runoff and be somewhat sensitive to the shape of the pareto distribution and the current soil moisture deficit, for a given time step, the volume released will be roughly linearly proportional to the percentage of filled water stores, as is clear from figure 5 – the PDMROF results fall within a b of 0.89 and 1.12: a linear model (b=1) is a very reasonable fit to the data. Since they are using the percentage of filled water stores as a proxy for contributing area, and not reporting whether or not they are using mean hourly flow/flow at the end of the time step or which version of contributing area (start of time step, mean, or end of time step), the scatter in this modeled data around the 1:1 line could easily be a temporal discretization artefact.

If the storage distribution is treated as a pareto (i.e., power law) distribution with a scale parameter (i.e., minimum storage) of 0, this will necessarily lead to an outflow-contributing area relationship which will be something very close to a simple power law function. This makes the hypothesis test (that the catchment flood and contributing area can be fit with a power function) straightforward, as the modeled results will certainly echo this – *this assumption is built into the model used to test the hypothesis*, which is the key issue. One cannot use a model to test a hypothesis which is itself built into the model.

Because of the built-in assumptions of PDMROF, I am doubtful that one can use a model such as the one used here (MESH) to test the primary hypothesis of this paper that the relationship between a catchment's floods and contributing area is a power function. A more appropriate approach would be to use a model which simulates all of the connections and overflow thresholds in the flow network while making no assumptions about the contributing area/outflow relationship.

(2) The estimated scaling relationship to generate the Q_tr-A_c relationship of equation 8 is generated using 2 data points and untested on verification data, yet the authors report coefficients to 4 significant digits. While it is clear the authors recognize many of the significant issues with using this equation (they catalogue many of them), I don't believe there is any justification to include it in the paper at all. The relationship is purely hypothetical and not supported through testing or comparison to additional data. The authors cite a list of reasons (in this case, "uncertainties") which describe why the expression is flawed, with insufficient evidence to supports its use.

(3) This model is of insufficient quality (as indicated in figure 3) to support the stated hypotheses. "Cleaning this data" with bias/error corrections does not fix the problem.

(4) The comparison of the model results with the (literally one data point) Agriculture Canada Ae delineations is weak. The authors acknowledge this: "The observed contributing area differences might be mainly due to the approaches employed to generate them". I agree with this statement. However, they move forward with this comparison as if there were more to it, introducing the bias and error corrections (which were not explained with sufficient detail to understand or replicate) to weakly assess model uncertainty.

Minor Points (given my recommendation, a non-comprehensive list) (1) The discussion of the history of MESH on pg 8 could be relegated to a simple citation of Pietroniro et al. (2007)

(2) Contrasting the GRU approach and PDMROF on page 9 and suggesting that PDMROF is an improvement over the GRU approach makes little sense; one is a discretization approach, one is a means of representing the flow-storage relation (i.e., the hydrologic process description). PDMROF could easily be applied in a GRU context, and actually is within the model used in this study. Rather, PDMROF is a replacement for

the runoff representation of WATROF.

(3) pg 11 ln 18- grid resolution of 10km or 10kmˆ2? The former implies only ~20 grid cells were used.

(4) pg 12- no need to define NSE in such detail.

(5) pg 12 – which Ostrich calibration algorithm was used?

(6) eqn 2 – this test statistic should be cited.

(7) pg 14 – this entire discussion of probabilities and plotting position could be relegated to a citation.

(8) pg 15 – a mean annual flood error of 100%

(9) It would be useful (for clarity) to split figure 3 into an Area distribution plot and a flood frequency distribution plot.

(10) The concepts in the nutrient management discussion seem only peripherally linked to the main objectives of the paper.

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