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Hydrology and Earth System Sciences Discussions

Interactive comment on "Spatial rainfall variability and runoff response during an extreme event in a semi-arid catchment in the South Pare Mountains, Tanzania." *by* M. L. Mul et al.

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GENERAL COMMENTS

The manuscript by Mul et al. (2008) describes the analysis of the hydrological processes resulted in a flood event occurred in a semi-arid catchment in Northern Tanzania. The study was performed by using a unique data set specifically gathered in a previously ungauged river basin. The topic of spatial and temporal rainfall variability and runoff response is relevant and perfectly matches the scopes of HESS. In my opinion, the description of the study might be improved by including: i) description

of the original contributions of the paper, ii) clarification of the assumptions made in rainfall data analysis and flood peak estimation, iii) justification of these assumptions given their important effect on the results, iv) discussion about the uncertainty related to this study. Technically, the submitted manuscript is well written; nevertheless some diagrams should be improved. I recommend the publication of the manuscript after revision. The next section reports a list of specific comments that should be addressed.

SPECIFIC COMMENTS

From 2660-2: The current version of the manuscript does not illustrate adequately the original contributions of the study. I believe that the main reason that justifies the publication of the paper is the new data set, characterised by a high spatial and temporal resolution, specifically gathered in a previously ungauged semi-arid basin in Northern Tanzania.

From 2662-22 to 2663-6: The utility of this analysis should be better explained. I think that the analysis would like to show the magnitude of the March 2006 flood event. Nevertheless, it is difficult to draw some conclusions by analysing Table 1 as the seasonal return periods vary from 2 to 195 years. Then, it is well known that the return period of the rainfall may be extremely different from the return period of the runoff. This is particularly true for this study where a daily rainfall is taken into account while the concentration time of the catchments under study is equal to around 1-2 hours. Furthermore, the manuscript states that a limitation of this particular analysis is that the altitude of the Same station is different from the other stations. In my opinion it should be stated that the evaluation of these return periods is affected by other several questionable assumptions (e.g. use of Same station data to evaluate the return period of daily rainfall in different raingauges stations; a priori assumption of the Gumbel distribution; inaccurate approach to evaluate the seasonal return period). In order to have an indication about the magnitude of the March 2006 flood event I would like to suggest the use of the envelope curve (e.g. Castellarin et al., 2007). The curve can be easily constructed by using flood data of a homogeneous macro-area (e.g. Brandimarte et al.,

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2008; Hicks et al., 2005). Probably, this might be done by using the World Catalogue of Maximum Observed Floods (International Association of Hydrological Sciences, 2003). It would be interesting to see where the estimated peak discharge is positioned inside the macro-regional envelope curve.

Lines 2663-14 and 2667-23: The manuscript states: 1) "the high spatial variability, related to the high intensity rainfall observed is not uncommon to result in flash floods"; 2) "having a large part of the rainfall contributing to slow processes is not uncommon for flash floods". I have got a question: do the authors consider the March 2006 event a flash flood? If yes, they should better explain such discordances, if not, they should elucidate the utility of the sentences 1) and 2).

From 2665-6: This sentence should be reformulated. At the moment it seems that the so-called gradually varied flow is the most general equation in river hydraulics. It should be stated that the gradually varied flow is a strongly approximated solution of the general Navier Stokes equations.

From 2665-8: In my opinion, this paragraph should include the assumptions of the gradually varied flow.

From 2666-10 to 2666-15: I believe that, in view of the several assumptions made (e.g. gradually varied flow, uniform and a priori Manning value, cross sections as a trapezium), the fact that "the flood mark points do not fully agree with the backwater curve"; is not only due to the fact that "the flood marks not necessarily mark the highest water level". In my opinion, the uncertainty related to the model is of the same order of magnitude as the uncertainty related to the flood marks. If the flood marks were less accurate than the extremely simplified model used herein, I would not use such data at all.

From 2667-7 to 2667-8: Given that the weir was destroyed during the peak discharge (as stated in 2663-22), it is not clear how the recession limb of the hydrograph was reconstructed.

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From 2667-12 to 2667-14: The manuscript states that only the 9% of the rainfall contributed to runoff. Probably this low percentage may be a useful warning about the reliability of the peak flow and hydrograph estimation. Furthermore, it is not completely true that the runoff volume is not significantly affected by the uncertain estimation of the peak discharge; especially taking into consideration that the recession limb of the hydrograph was (probably, see above) completely estimated as well.

From 2667-17 to 19: This sentence need to be clarified. The increase of the base flow from 15 l s-1 to 75 l s-1 should be commented in view of the fact that the season preceding the flood was extremely dry (as stated in 2661-4).

From 2671-9 to 2671-15: The results are deterministically presented, although the analysis was performed by using extremely simplified models (e.g. the backwater computation). I think that the manuscript should include some more serious discussion about the assumptions made and the uncertainties related to the study.

From 2671-27 to 2671-29: This sentence should be reformulated. It seems that the only conclusion of the study is that "by using the assumption of uniform spatial distribution of rainfall, the predicted runoff can easily be over- or underestimated". I believe that this statement was, in principle, already known. If the sentence is related to the test site under study, a more serious discussion about this issue is essential.

TECHNICAL CORRECTIONS

Figure 1: The diagram should be improved. Please increase the size of the left panel and make clear the location of the rivers and the boundary of the sub-catchments described in the manuscript (e.g. Ndunduve sub-catchment, Pangani river, etc).

Equation (3): Is n = 0.05 as for the previous analysis? Please specify.

Figure 5: The manuscript refers to this Figure using "Fig. 5a" and "Fig. 5b", but the figure caption states Fig. 5 [...](left) [...] (right).

Caption of Figure 5: "critial depth"

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