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Discussions

Interactive comment on "Reconstructing the Salgar 2015 Flash Flood Using Radar Retrievals and a Conceptual Modeling Framework: A Basis for a Better Flood Generating Mechanisms Discrimination" *by* Nicolás Velásquez et al.

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The proposed manuscript presents and analysis an interesting and relatively well documented extreme flash flood occurred in May 2015 in Columbia that killed about 100 people an induced severe damages. A large part of the manuscript is devoted to the implementation of several numerical models (a distributed rainfall-runoff model, a sediment transport model and a slope stability model) used to reproduce some observed patterns of the event and to propose some interpretation on the dominant flood generating mechanisms during this specific flood event. The manuscript is suited to

HESS, potentially interesting but deserves some major improvements before it can be published. The major concern lies in the proposed approach consisting in pretending providing explanation on dominant hydrological processes based on a rainfall-runoff model that is essentially calibrated or validated based on a single downstream peak discharge estimate. This is really unreasonable. Distributed rainfall-runoff models may be implemented to reproduce such events and their performances may be evaluated, but it must be clearly explained how the values of their numerous parameters and state variables (especially soil moisture and possible groundwater levels) are fixed. This is partly done in the manuscript, but not sufficiently. The manuscript must clearly define which parameter values are determined a priori, based on which observed data and which parameter values are possibly adjusted to fit the model. Moreover, even if distributed, hydrological models especially when implemented at large scale, can not account for the complexity of rainfall-runoff processes related to small scale variability an preferential flows. The models may reproduce the dynamics of the rainfall-runoff response, but are far from perfect. The interpretations of the authors based on this model implementation exercise are not consistent with the real accuracy of the models. The modelling exercise is interesting but first the authors should try to find additional information to support their analyses (a single downstream discharge value is not sufficient, see detailed comments for suggestions) and remain prudent in their conclusions.

Detailed comments: P1L15: the virtual tracer experiment separates the simulated "runoff" and "subsurface flow " contributions in the model (i.e. fast and delayed contributions), but real-world tracer experiments could provide very different partitioning as illustrated by numerous past geochemical hydrograph separation studies. Simulated processes can not be simply considered as representing effective processes on the considered watershed. This is a much to simplistic point of view on hydrological processes. P4L103: difficulting does not exist. P8L216: I do not know if it is possible to really say that some watershed are geomorphomogically prone to flash floods. At least, several studies (Marchi et al., 2010 ; Smith et al, 2018) do not show clear relations between geomorphological settings and magnitude of extreme peak discharges. P10L254: the selected velocities are relatively high, especially for average cross-sectional velocities (see Lumbroso et al., 2012). The provided estimates may be a little high. Are their some films that could help reduce the estimation uncertainties and provide some ideas of possible peak velocities. According to figure 15, the flood extent has been mapped, and probably flood marks identified, along a large part of the main stream. Since the second event has been produced mainly in the upstream part of the watershed, it would have been interesting to base the analysis on some other peak discharge estimates along the main stream and its tributaries. The ability of the proposed model to reproduce the spatial distribution of the flood peaks on the watershed could have then been tested. P10L270: the same paragraph is repeated twice. Figure 5 : impossible to read. The legends must be increased. The five compartments described in the text must be clearly identified. P13-17: this part could be placed for most of it in an appendix. Moreover, all the variables used in each equation must be clearly defined, which is not always the case and makes it difficult to follow the explanations. The variable names are changing from one equation to the other as for instance A, A2, A3 between eq. 13-17. If it is the same variable, use the same symbol. P18: The radar rainfall rates must be quality checked. The area is relatively far from the radar and mountainous, two settings that could introduce uncertainties and errors. Are their some available raingauge measurements on the affected watershed or on nearby areas ? How do the radar-based rainfall estimates compare with corresponding raingauge measurements ? The two considered rainfall events are spatially heterogeneous unlike what is stated further in te manuscript. The upper part of the watershed is almost not affected by the first event. It would be essential to distinguish this upper part in the rest of the analysis since the average simulated soil moisture and and runoff components may hide a significant spatial variability. Conclusions drawn on the importance of the first event for the saturation of the soils could be largely nuanced by a more detailed spatial analysis. P18L486: What about the first flood event. Is their any possibility - based on local information - to have an idea of the possible value of the first peak discharge. It would be interesting to know if the simulated discharge - 160

m3/s corresponding almost to full-bank flow according to the estimates on page 10, has been observed or no. This would give one more reference point for the evaluation of the implemented rainfall-runoff model. P19L494: It is essential for the second event, according to the spatial rainfall heterogeneities, to provide some distributed simulation results : what part of the flood volume has been produced on the 15% upstream part of the watershed ? Is the contribution form the intermediate watershed significant ? P21L530: It would be essential to provide some information on the real timing of the floods that could for sure been provided, at least approximately, by eyewitnesses. Discussion on simulated timings, that may be wrong is of little interest. P21L539: the sentence "Event 1 does not trigger a flash flood event" is not supported by the facts and probably excessive. It did certainly not produce significant overflows and damages, but may have produced a significant flood events (an estimated discharge for this first event is clearly missing in the manuscript). If so, according to the duration of the event, the flood can also be considered as a flash flood. Part 4: the simulation part, and the interpretation of the results is not uninteresting. But the spatial variability must be shown and commented as suggested before and a clear difference must be made between these simulation results and the real-world. What is presented is the outcome of a numerical model, with the selected parameter and initial state values: some other choices could have provided equally good results if compared to the only available estimated peak discharge but very different flow separation between the various simulated components. Again, the choice of the various parameter values must be clearly justified. Some sensitivity analyses of the results and partitions to these values would also be welcome to strengthened the analyses: are the conclusions always the same if the values are varied over a reasonable range ? I ave doubts. P22L562: Why should the soils be wet upstream since this area has not strongly been affected by the first rainfall event ? P24L590: The spatial agreement is not a real surprise since the landslide model has been calibrated and according to the spatial distribution of rainfall.. P24L600: Can the concentration of landslides in the first part of the rainfall event be confirmed in any way (eyewitnesses). By the way, this is a surprising result. In general, the landslide density has a general tendency to be related to the rainfall amounts and the progressive increase of soil saturation in mountainous areas (sign that infiltration dominates the hydrological processes throughout intense storm events). The obtained results would imply that even a very short-lived intense rainfall would have produced landslides in the area. I have many doubts that this is realistic. P25L619: The notions of order are not presented (probably not Strahler order according to the value). Please explain. Figure 15: this figure shows that the post-event survey database is much richer that what is used and presented in the manuscript. Intermediate values of discharges could have been estimated for instance. The comparison between simulated flow depths and observed flood extents is far from perfect. Can this be attributed to the Digital terrain model ? A critical analysis of the digital terrain model could be provided in the manuscript (comparison between observed and extracted cross sections for example). P27L647: there is no information about flood 1, the authors can not speak about evidence of remarkable behavioral difference. We do not even know if a flood of significant magnitude occurred... The rainfall can not be described as spatially quasi-homogeneous. What do the author mean with "return flow" and "20 groups" L654: the authors can not state that the second convective core results mainly in surface runoff. First, the most affected area has hardly been saturated by the first event (spatially detailed result will probably show it) and in anyway, that is a simulation result and not necessarily reality. P28: The conclusions have to be deeply rewritten to introduce nuance and prudence according to all previous comments.

Lumbroso D., Gaume, E,. 2012. Reducing the uncertainty in indirect estimates of extreme flash flood discharges, Journal of Hydrology, doi:10.1016/j.jhydrol.2011.08.048 Marchi, L., Borga, M., Preciso, E., and E. Gaume, 2010: Characterization of selected extreme flash floods in Europe and implications for flood risk management. Journal of Hydrology, 394 (1-2), 118-133. doi:10.1016/j.jhydrol.2010.07.017. Smith J. et al., 2018. Strange Floods: The Upper Tail of Flood Peaks in the United States. WRR, doi:10.1029/2018WR022539

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