Review of the 2nd version of the paper : Reconstructing the Salgar 2015 Flash Flood Using Radar Retrievals and a Conceptual Modeling Framework: A Basis for a Better Flood Generating Mechanisms Discrimination. Nicolás Velásquez et al.

General comments about the revision:

I appreciate the revision of the manuscript, that make the paper to be more concise and better structured. Specially the rewriting of the introduction, the study site and data description and part of the methodology appears in a clearer way. I would however still suggest several rewording of the section 3.2 and 3.3. The theoretical background of the latter one must be clarified to be maintained in the manuscript, either by referring to the related literature, or by explaining the hypotheses behind the floodplain sub-model.

Finally concerning the methodology, I have one main doubt about the actual connection between the hydrological model set up and the landslide model one. The strength of the overall paper methodology, is to use the soil storage dynamic simulated by the hydrological model to deduce landslide occurrence. However, it does not appear clear any more if the soil storage set up (Z) is the same for both simulations. This assumption has to be clearly clarified.

The results and the discussion appear in a clearer way as well in that second version, specially when describing the results of the hydrological model, explaining the different flow processes during the two distinct events. I think that the results could even be better inserted within the current literature, showing standardize figures or adjusting the discussion. My comments below support this point.

The discussion about the limitation of the landslide model might be more more detailed. I think, the authors should be able to settle on the reasons of the result limitations: does it come from the lack of spatial information about soil and land cover properties, or from too strong assumptions of the landslide model? Ruling on the reasons of the limitation would bring a direct outlook of the study.

Finally, I found really interesting that the soil storage capacity available before flood event impacted not only the flood magnitude but also the response time of the catchment. From my point of view, it could be appear as one of the main incomes of the paper.

I. Comments on the methodology's description:

I.1. Description of the shallow landslides sub-model :

From my point of view, the description of the sub-model could be clearer expressed. It doesn't emphasize the crucial definition of the model. The stability state of the cells, which is presented on the first part of the section, depends on the stability criteria which is presented later. I suggest here a draft, but please feel free to adapt it :

"The shallow landslides sub-model coupled to the hydrological model is proposed by Aristizabal et al. 2016. The stability of each cell is calculated through the assessment of the different stresses applied to the soil. The stability of the soil decreases with the pore water pressure (Graham, 1984). The slope failure occurs when the saturated soil thickness above the slip surface (Z_w, here related to the gravitationnal storage S_3, eq. 9) is greater than a critical saturated depth (Z_c), which depends on the soil cohesion (C), the hillslope (beta), […] (eq. 10).

Eq 9 : Zw = S_3 / (W_c – W_{fc}); with W_c and W_{fc} the soil saturation depth and the field capacity respectively, S_3 the gravitationnal storage.

Eq 10 : […]

According to that soil stability definition, the topography and the soil properties, cells of the catchments are classified into 3 groups : i) the unconditionally stable cells for which the maximal value of Z_w (i.e. Z) is smaller than Z_c; ii) the conditionally stable cells, for which the stability will depends on the saturated soil thickness (Z_w), and iii) the unconditionally unstable cells, for which their properties lead to unstable conditions for any value of Z_w. Shallow landslides are calculated at each time step of the hydrological simulation, on the second cell class, where the soil stability depends of the storm event."

In addition, I suggest to summarize the specific parameters to the landslide sub-model in a table as done for the hydrological model. I suggest as well to indicate the references used to set up the parameter values.

Finally, the lines 563 – 566, page 19, correspond to the landslide sub-model description; they should be inserted in that section.

I.2. Description of the flood plain sub-model :

I still have some difficulties to understand the theory behind the calculation of the flood plain. I'm ok with the assessment of the water depth (eq. 11), the friction velocity (eq. 12) and the sediment concentration (eq. 13). Then, I have some trouble to follow the method. Could you please define what is a constitutive coefficient (r) and add a reference for the eq. 14 ?

The main trouble I had, is that you're going into sediment fluxes assessment to calculate flood plain area. What the gain of that method compared to a direct assessment of the flood plain area through the simulated discharge, the simulated velocity and the DEM ?

II. Comments on the results and the discussion:

- page 15, line 451 – 453: "The simulation shows that Event 1 generates a hydrograph with a peak flow of Qmax = 160 m3s−1 . It is important to note that during precipitation Event 1 there were no damage nor flooding reports by local authorities." Can you link the simulated peak flow value to a flooding / no flooding status ? I mean, could we consider the simulated peak flow as consistent with the fact that they were no flooding reports, or is that assessment to high ?

- Page 16, line 483. : "Although some of the surface speed values used in the analysis are unrealistically low". It would be better to directly limit the sensitivity analysis over a range of realistic speed values. The assessment of such a range might be done choosing a range of realistic roughness coefficient (Manning, Strickler, ...), and using the relation between runoff speed (v), and water level (h) under cinematic wave hypotheses ($v = S^{1/2}n^{-1}h^{2/3}$), with S the slope, and n the Manning coefficient. As you mentioned somewhere in the manuscript, this range of observed speed values finally has to be rescaled to be adapted to the model resolution: the transit time along the water paths must be maintain although the DEM data processing can have modified the simulated water path lengths (L_{num}). In other terms, the ratio L_{obs} / v_{obs} = L_{num} / v_{num} must be kept when rescaling the surface speed range.

- Page 16, comments on Figure 10: To describe the sub-catchment I would suggest to indicate as well the surface area [km^2] in addition to the percentage of the basin. I also think it could be really interesting to describe the flow peaks in terms of $m^3.s^{-1}.km^2$ which can be compared to flash flood features found in the literature (for example, see figure 6, Page 8 in Gaume et al, 2016).

- Page 17, comments on Figure 12: a) The transit times of the events 1 and 2 are really interesting, because the catchment response is slightly shorter in event 2, while the rainfall storm was located really in the upstream and remote part of the catchment. In contrast the first event was located closer to the outlet, and therefore we might have expected faster response. It's mean that the storage capacity before the event has not only an impact on the magnitude of the flood but also on it timing. The result is not straight forward, and could be mentioned here or in the discussion. b) It's also really interesting to see that the runoff and the subsurface flow start at the same in the event 2, while in the first one, we can see a delay of around 2 hours between the start of those two processes.

- Page 18, line 528-530: "In event 2, the convective rainfall and the runoff show a similar evolution, denoting a strong influence of the convective portion (figure 12b)". From my point of view, it rather means that the stream network (as there are mainly runoff) does not temporize, convert and attenuate the rainfall input signal.

- Page 18, Figure 13: I'm still not convinced by the interest of this figure, but it remains your paper.

- Page 19, line 578, about the landslide model set up. Do you mean that you have two different calibrations of Z (i.e. S_3) for the landslide model and for the hydrological model? If so, that makes the connection between both model simulations inconsistency...

- Page 19, about the landslide results: First, I would suggest no to insist so much to the difference between the observed and the simulated number of unstable cells. The way to observe landslide are quite qualitative, as – I assumed – it was certainly based on color differentiation between aerial views. The successive soil transport to the landslides and the soil spread through the runoff might have lead to detect unstable cells where there were only sediment charged overflow occurring over it. Second, I would suggest to rather focus on the spatial distribution. I slightly disagree with 'considerably well spatial distribution representation' . I agree that the upstream landslide were really well detected. However the false positive detection in the south part of the catchment should be discussed. Where are the limitations of the landslide model? Is it the fact that the same cohesion or other soil parameter are choose uniform over the catchment ? In that case, different land cover, soil textures, at the good positive and the false positive cells could support this hypothesis. Or is that the landslide model it self that is to simplified ? → improvement taking rainfall intensities, …

- page 20, lines 609 – 612: It would be interesting to add the proportion of the river length for the different orders.

- Page 21, lines 654 – 661: I would suggest to refer to the importance of the rainfall spatial distribution in connection / interaction with the soil storage capacity ones (Zocatelli et al, 2010, and to Douinot et al, 2016). Those are exactly the two main differences between the two events.

- Page 22, lines 671 – 674. The description of the different order are interesting but I don't see the link with the results present in section 4. Could you specify how did you deduce those assessments?

III. ***Technical comments:***

- Page 4, line 121 – 129, when introducing the 3 models. Similarly to the short description of the hydrological model done between the lines 123 – 126, I suggest to keep the short description of both sub-model initially given in the first version of the manuscript :

"The shallow landslide sub-model follows the formulation described in (Aristizábal et al., 2016). The hydraulic sub-model corresponds to a low-cost 1D model (hereafter referred to as HydroFlash) that [assumes infinite sediment supply and] estimates the cross-sectional filled area at all time step."

- Page 4, line 123; page 5, line 137, page 13, line 390: the sub-model simulated flood plain inundation is alternatively called : "hydraulic sub-model", "inundation sub-model", "flash flood submodel", and "HydroFlash". For a sake of consistency, and clarity, I suggest to choose only one of those terms and use it everywhere in the manuscript when calling that sub-model.

- Page 5, line 161: please put bracket around "HAND"

- Page 6, line 190 – 192: By soil properties map, I was thinking to spatial distribution of the soil properties, as used to define the hydrological model. It would give an overview of the spatial distribution of the soil classes. I'm still thinking it is of interest. If you don't want to add another map, I would suggest just to adapt the color scale of the slope map to the soil classification. In that way, the reader could guess the spatial distribution and the proportion of each soil class. Another option, might be to add a column in table 1, with those proportions.

- Page 7, line 196 – 198 : " Unfortunately […]." I would suggest to remove this sentence for a sake of conciseness.

- Page 7, line 211 – 212: the range of value of the stream discharge [185 – 222] $m^3.s^{-1}$ does not correspond to the range of value of the velocities [5 – 7] $m.s^{-1}$. A maximum discharge of 259 $m^3.s^{-1}$ is expected. Please change either the velocity of the discharge stream range.

- Page 7, line 213: the sentence "The timing of peak flow is also [an] important information" can be removed.

- Page 7, line 216 – 222: Please refer to the figure 16. Some details of how the contrast between both images are calculated would be welcomed.

- Page 8, line 230 – 236 : the optimal distance for radar rainfall observation is described twice. Please remove "optimum" line 233 in "optimal optimum" and the sentence line 236 : "The results of the radar QPE methodology indicate that the rainfall estimation works well within a radius of 120 km."

- Page 8, line 256 – 258 : the sentence "Chocho is the [...]" can be removed.

- Page 10, line 297 : add a space between "Figure" and 6.

- Page 12, table 3: are the capillarity and the gravitational storage really in mm? Or rather in cm?

- Page 12, table 3: write "capillarity" instead of "capilary".

- Page 12, line 358 : write "10 parameters" instead of "ten parameters".

- Page 12, line 363 – 365 : please specify here the objective of the calibration. Here a suggestion: "The model simulation is calibrated to reach a base flow of 3 $m^3.s^{-1}$. The calibration consists in scaling each physical parameter by a constant value in the entire basin (Francés et al. (2007b)). Table 3 includes the mean value for all the parameters used in the model and the scalar value adjusted during the model calibration."

- Page 13, line 367 : the reference date is missing for "Aristizabal et al. ..."

- Page 13, equation 6, 7, 8, 9, 10 : In the equation, the index "i" refering to the cell "i" of the catchment is specified in the left side of the equation but not in the right one. Please either specify the "i" index to any or no one of the parameter with cell dependent value.

- Page 15, equation 16: the index "j" should be added to the parameter Fd,i which depends on it.

- Page 15, line 449 – 451 : please remove the sentence "The model simulation is set up [...]". It refers to the method and therefore should be inserted in the section 3.

- Page 16, line 470: the observed timing could be indicated in brackets, as a remember.

- Page 16, line 482 : "particularly in the low end values", do you mean the discharge recession?

- Page 17, line 496 – 500 : those lines correspond to the method description and should be inserted in section 3.2.

- Page 17, line 516: The first sentence "It is well known [...]" is not relevant and could be remove for a sake of conciseness.

- Figure 2: unit of the slopes is [-] and not [$mm.^{-1}$]

- Figure 5, plot a) y-axis: please confirm the rainfall unit : [mm / 5 min] ?

- Within all the manuscript : choose between "sub-model" or "submodel" and maintain the same spelling.

References:

Eric Gaume, Marco Borga, Maria Carmen Llassat, Said Maouche, Michel Lang, et al.. Mediterranean extreme floods and flash floods . *The Mediterranean Region under Climate Change. A Scientific Update*, IRD Editions, pp.133-144, 2016, Coll. Synthèses, 978-2-7099-2219-7.

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D. Zoccatelli, M. Borga, A. Viglione, G. B. Chirico, and G. Bl ̈oschl. Spatial moments of catchment rainfall: rainfall spatial organisation, basin morphology, and flood response, 2011, HESS, doi:10.5194/hess-15-3767-2011