

Interactive comment on “Hydrodynamic characterization of past flash-flood events and their associated hazards from dendrogeomorphological evidence in Caldera de Taburiente National Park (Canary Islands, Spain)” by Julio Garrote et al.

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First, the authors would like to thank the anonymous reviewer for the suggestions and comments provided, which will help improve our work. Below we respond to his/her comments.

1- Since the main focus of the paper is on the use of dendrogeomorphological data sources, there is a lack of explanations in the methods section, how the dendrogeo-

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morphological data sources are obtained in detail. Later in this paper the uncertainties in the data become very important. Hence, the methods section should include more information on how wounds in trees (scars) are generated (only by currents or in combination with sediments); how the time-dependent development of wounds can be described; how wounds are defined as significant; do the data only result from visual inspections; what is the data quality etc..

We agree with the reviewer regarding the importance of using dendrogeomorphic data in our work. In this regard, the authors did not consider necessary the inclusion of a more detailed description of the characteristics of this data, since this information can be found in Genova et al. (2015), as we explicitly mentioned in the text. However, if the reviewer considers it necessary, the description of the data acquisition method, its characteristics, and the use of the information obtained will be expanded. In the case of the additional information requested by the reviewer regarding wood debarking, its genesis and its categorization, as well as its meaning and importance, there is abundant literature on this topic; for instance the article by Genova et al. (2015) and other collections recently published (Díez-Herrero et al, 2013; Benito & Díez-Herrero, 2015; Ballesteros-Canovas et al., 2015.). These works provide detailed explanations of search procedures, classification, interpretation and the quality of data derived from dendrogeomorphologic evidence. Nevertheless, if the reviewer and the editor believe that including more information on the above is appropriate, we are willing to add more detail to the manuscript regarding these methodological aspects.

2- A vital result of the paper is the large discrepancy in the model runs resulting in 50 m³/s and 1200 m³/s. There is a very extensive discussion on uncertainties in data and methods, which is honorable. From my point of view the discrepancy of the results is that large so talking about “uncertainties” is critical. Isn't it more a signal, that either data or methods are simply not suitable to answer the research question? Where is the border between uncertainty and infeasibility? Especially since both precipitation and dendrogeomorphological data include major uncertainties the conclusion that the

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obtained results could improve flood hazard and risk analysis is questionable.

Undoubtedly, conducting an analysis such as the one performed here can entail a significant degree of uncertainty. However, from the authors' point of view, the combination of data sources used reduces this uncertainty by attempting to relate indirect flash flood evidence to the theoretical clear water flood which could have caused them. The absence of flow data derived from flow gauges in the basin does severely limit calibration of any type of hydrological model. We are, therefore, aware of the possible sources of uncertainty regarding the results of the analysis, although the possibility of reducing these uncertainties is minimal. Consequently, doubts about the efficiency or applicability of studies such as the one presented here can be raised. However, from the authors' point of view, if these limitations invalidate the studies conducted in such basins we will be giving up a chance to investigate and attempt to understand how these small ungauged mountain basins work. This type of basins represents a very high percentage of the total, especially in developing countries, and from the point of view of flood risk and land use management, this lack of knowledge does not seem to be a good idea. Moreover, from the point of view of scientific research, real advances and innovations occur when researchers work on solving problems in basins with missing data and high uncertainties, but not when known methods are applied in basins with abundant and well known data, which would be mere repetitions of technical reports.

3- Minor comments: P5, L6: Include more information on dendrogeomorphological data and methods (see above).

As we mentioned above, a detailed description of the dendrogeomorphological data used in this work can be found in Genova et al., 2015, and we have thus omitted it. However, if the reviewers consider it necessary, we will include this information in the revised version and also mention some other papers such as Díez-Herrero et al. (2013); Benito and Díez-Herrero (2015); and Ballesteros-Cánovas et al. (2015).

4- Minor comments: P6, L10: The POT extreme value statistics is based on data lasting

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<24 hours and >24 hours. Please explain this more in detail. From my point of view one has to choose defined duration levels (e.g. 6 hours, 12 hours etc.) and perform the statistics for each series individually.

In this case it is possible that the authors have not clearly explained the work done. Statistical analysis to obtain quantiles of precipitation using the GP-POT distribution function was conducted on the daily (24 h) rainfall series. We will correct the wording to make this clearer.

5- Minor comments: P7, L24: Please explain more in detail how the v1997 topography was built.

To generate the v1997 topography, we combined two topographic surfaces: the current topography (v2009) and the 3D surface that adjusts the topographic position of the base of the trees when dendrogeomorphological information is available. The criterion used to combine both topographical surfaces was to conserve the data conservation with the highest elevation. Based on that criterion, the resulting combined surface retained the current morphology of the slopes outside the channel while, in the streambed and banks, the surface was obtained from the adjustment of the location of the dendrogeomorphological data.

6- Minor comments: P10, L11: The explanations given in sec. 4.3 can only hardly justify this large discrepancy found in the model results.

The authors believe that the combination of several of the factors stated in section 4.3 of the article can justify the discrepancies shown by the models. In the authors' opinion, the differences in precipitation between the weather station and the entire basin itself need to be considered because this would indicate that the precipitations could have been significantly greater, and perhaps more intense. It should also be considered, in the authors' view, that because of the characteristics of the basin and its high contribution of sediment and floating solid load (woody material) to the flood, the flow characteristics can vary significantly compared to a flood that is only composed of

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liquid flow. In this sense, with a smaller volume of liquid flow (which therefore requires a smaller volume of precipitation), the total volume (solid + liquid) of the avenue will be greater. Increasing the flow associated with the model that only considers precipitation and reducing the liquid volume of the flow associated with the model that takes into account the dendrogeomorphological data would reduce differences in the flow rates obtained by the two models considered.

7- Minor comments: Figure 8: Please add the original data (plotting positions) in the plot; the differences in the results seems a little bit strange. Please check also the whole calculation.

As suggested by the anonymous reviewer # 1, the statistical analysis performed and shown in the article will be reviewed in its entirety, to prevent any errors or inconsistencies in the present analysis.

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