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Interactive comment on "Reproducing an extreme flood with uncertain post-event information" *by* Diana Fuentes-Andino et al.

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Note to the editor and authors: As part of an introductory course to the Master programme Earth & Environment at Wageningen University, students get the assignment to review a scientific paper. Since several years, students have been reviewing papers that are in open online discussion for HESS, and they have been asked to submit their reports to the discussion in order to help the review process. While these reports are written as official reviews, they were not requested for by the editor, and we leave it up to the editor and authors to use these reports to their advantage. While several students were asked to review the same paper, this was not done to provide the authors with much extra work. We hope that these reports will positively contribute to the scientific discussion and to the quality of papers published in HESS. This report was supervised by dr. Ryan Teuling.

In 1998, the Hurricane Mitch flooded the capital of Honduras with a return time of 500 years. This flood damaged 40% of the city's capital stock and one thousand casualties were reported (angel et al., 2004; JICA, 2002). Due to the power of floods, discharge and water level gauging equipment get destroyed. The discharge and water level of floods are needed for prevention and mitigation of floods. This high societal relevance initiated the authors to improve the knowledge of flood hydraulics. They state that post-event data have been used to simulate the flood hydraulics (Horrit et al., 2010; JICA, 2002), the GLUE framework has been used to account for uncertainty in models (Aronica et al., 1998; Brandimarte and Di Baldassarre, 2012; Pappenberger et al., 2005a, 2007) and that rainfall-runoff models have been coupled with hydraulic models (montanari et al., 2009). In this study the LISFLOOD-FP model was used to model the dynamics of the water level along the river channel and floodplain. This model scheme was specifically designed to predict flood inundation, and not flood routing (Bates, 2000). This model has extensively been validated in rural areas, but not in urban areas (Horrit, 2010). The use of a inundation-specific scheme can be justified, however this is not done in the paper, because the flood inundation was of greater importance in this report, having a compounded weight of 0.7 over the compounded weight of 0.3 for flood routing in the model evaluation. The use of this model over a full 2D dynamic model is justified by the fast computation time, which is favorable for the uncertainty analysis since the model will be run many times. For the uncertainty in the input hydrographs of the LISFLOOD model, 100 representative hydrographs were made. This was done with a GLUE analysis of the combination of TOPMODEL and Muskingum-Cunge-Todini. The choice of TOPMODEL is justified but the Muskingum-Cunge-Todini is not. However, due to the lack in cross section data in the subcatchments, it is logical not to use a more complex routing model (Todini, 2007). The simulations were concluded to be trustworthy because the simulated discharge, times of peaks and 90% of the high-water levels were within the uncertainty bounds of the evaluation data. In the introduction, the author states to combine a RRM, a hydraulic model and post-event data within an uncertainty analysis framework to prove that reasonable estimation of an extreme flood is possible when hydrometric data are lacking. To my opinion, this aim is not ambitious enough. Previous work already have simulated this 1998 flood (ENEE, 1999; JICA, 2002) and the flood with a 50 year-return time design discharge in the same area (Mastin, M.C., 2002). This state-of-the-art is not included in the introduction, which I think should be the case. For the evaluation of the model, uncertainty bounds of the observed peak discharge, time of peak and water levels are introduced. Only the 50% of the observed peak discharge and not the 2.5 hours and 1.8 meter uncertainty bound of the time of peak and water levels are argued. Also it is not clear in the method how the extreme flood estimation is evaluated to be 'reasonable' or 'trustworthy'. To me it seems that after obtaining the results that 90% of all water level observations fall within their chosen uncertainty bound, they subjectively classified it as reasonable, trustworthy and realistic. Using the by the authors chosen uncertainty bounds, the aim of this paper was already reached by the MIKE11 simulations, the 1D unsteady flow hydraulic model from JICA, fourteen years before the start of this study (Figure 10 of the paper). Due to the unclear assumption of the uncertainty bound, the unambitious aim, and especially the fact that this aim was already reached by others, I advise to refuse the paper.

Major Arguments:

1. The aim to show that the 1998 flood could be reproduced is found in the abstract, the introduction and the conclusion. To me, the purpose of this study is not clear from this aim. In the discussion, the purpose is described: Bonnifait et al. (2009) reproduced an extreme flood event, but not within an uncertain analysis. In this study the uncertainties in model parameters, rainfall input and evaluation data have been accounted for. Not having this aim clear makes it hard to follow the storyline and what the actual message is that is meant to convey since there is no clear structure. In the AGU Fall Meeting Abstracts, the HEC-RAS model was compared with the Lisflood-fp model. This comparison resulted in better prediction with behavioural parameter sets and the obtained uncertain flood extension will be useful for decision makers (Fuentes Andino,

D. C., et al. 2012). Then in the EGU General Assembly Conference Abstracts in 2013, it is stated that the results of Lisflood will be evaluation with the GLUE analysis, but that the challenge is the how to evaluate the results when there are uncertainties in the model parameters and evaluation data. I think that both the aim of mapping the flood extension better and the aim of evaluating the Lisflood model were tried to be combined in this research, without this being possible. The results of the parameter sensitivity are analyzed (Figure 5-9), without this contributing to the aim and hypotheses. And the Lisflood model is compared with the JICA simulation (Fig 10), without really paying attention to the comparison. P14, l22 states in one line that the water levels of Lisflood encompasses the observations better than JICA, without ever stating that JICA produced water levels as well. This work could be a significant contribution to the society, if the uncertainty analysis produces improved flood-inundation maps compared to previous work, as stated in the abstract of Fuentes Andino in 2012. I suggest the aim to be: to prove that the flood water level and flood extension are more reliably modelled including than excluding an uncertainty analysis.

2. In the abstract of Fuentes Andino 2013 it was stated that the model evaluation is the challenge in this method. However, I do not see the argumentation of the choice of the fuzzy values included in this paper. In the model evaluation, the fuzzy set values were justified for peak discharge by selecting a value between the minimum and maximum uncertainty from literature of a mountainous area. However, the fuzzy values for the time of the peak and the water level are not justified. P14 L7 'The prediction of high–water marks was quite acceptable with average degrees of belief for the criteria ðİŚŚ4−102 varying from 0.46 to 0.75 for behavioral simulations even when the criterion was relaxed.' Is 0.46 really quite acceptable? The a and b fuzzy set values are 0.5 and 1.8, meaning that with the degree of belief of 0.46, the model simulates 1,2 meter more or less than the observations. This sounds like a huge difference to me.

Minor Arguments:

1. P6,L21. 'Since discharge hydrographs were not measured. . . Chiquito River, Grande

River, Guacerique River, Salada Creek and Las Lomas Creek sub–catchments (points 1, 2, 5, 8 and 9 in Fig. 1 and 2 and Table1).' Why is this done? Because the hydrographs were not measured or to propagate the uncertainty of the input hydrographs in the GLUE analysis of the Lisflood model?

2. Header 3.2 states 'Rainfall-runoff modelling within an uncertainty analysis', while in fact already a combination of the TOPMODEL rainfall-runoff and the Muskingum-Cunge-Todini hydraulic model is implemented. I suggest: Calculating representative hydrographs for the subcatchments.

3. P12, L13: Here it is stated that the propagation of water level uncertainty is more evident at highly dense urban areas, referring to Figure 11. In Figure 11 I see a likelihood of inundation map. If the urban area is more likely to inundate, does this mean that there is more uncertainty here?

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