Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-496-RC2, 2016

Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #2

Received and published: 30 October 2016

The paper under consideration pertains to the extremely important issue of modeling floods in cases where the data is scarce or not available at all. Authors' ideas are illustrated by the flood event in Tegucigalpa triggered by Hurricane Mitch. Authors use the post event data collected two and even three years after the event. They propose an interesting modeling framework and discussion of the results. The nature of discussion obviously raises a lot of questions. One can easily think of a variety of other models that could be used and might lead to different conclusions. The material to verify the results is simply not solid enough but this is what the paper is about.

I read the paper with great interest as it touches upon the problem that hydrologists often have to deal with but to be honest, I am quite disappointed with the way the material was presented. It is not easy to follow the reasoning of the authors, not mention that one has to read this paper alongside with other publications to have the clear picture of

the subject. Please find below some remarks that in my opinion could make the paper more useful to the readers:

1. Authors do not mention various important sources of uncertainty such as insufficient knowledge of the analysed phenomena as well as the errors introduced by the models used in the calculations, which always simplify the described processes. Authors treat the models as black boxes, not even mentioning what assumptions and principles they use. Although TOPMODEL, MCT routing and LISFLOOD-FP are relatively well-known, in the paper I would prefer to have some basic information on the equations used, their dimensionality, model parameters etc. After all they present debatable simplifications and there exists a bunch of other models using different sets of equations, often treated as better representations of reality. One crucial issue in this respect is the way how hydraulic roughness is introduced in the model, since it creates one of the most important sources of uncertainty. Please refer to the in-depth discussion of such issues in Warmink and Booij (2015), Uncertainty analysis in river modelling, Rivers-Physical, Fluvial and Environmental Processes, GeoPlanet: Earth and Planetary Sciences, Springer, 255-277

2. Authors stick to the GLUE method as the framework for the uncertainty analysis without even mentioning that it is a quickly developing area and other methods could be successfully used in this context, to mention Markov Chain Monte Carlo algorithms, sequential Monte Carlo samplers, or likelihood-free algorithms such as Approximate Bayesian computations (ABC) as the implementation methods. A short discussion on the choice of GLUE method could be important in this respect.

3. Authors have a tendency to make the reader look for the information that could be easily provided in the paper. Some examples from pages 8 and 9:

When mentioning Kuiper statistic test, it would be useful to mention why this test was chosen. What makes it better than other much more popular statistic tests? Are we dealing with cycling variations?

“A stopping criteria as in Pappenberger et al. (2005b)” – do you mean one criterion or a few? Please provide or describe this criterion – the idea behind it.

What is K-means flat algorithm? Maybe an explanation that it refers to K-nearest neighbor classifiers (if it is the case) could give some more information to more advanced readers

In this point I am trying to convince authors to make the paper more self-contained, otherwise only the readers familiar with most of the tools used in the paper will benefit from reading it.

4. In the discussion part authors do not explain sufficiently why various disagreements between the model and observational results occur. I believe that a lot of them can be attributed to weak representations of the topography and roughness, which in such a complex catchment most likely cannot be represented by one parameter.

My overall opinion is that the paper is absolutely worth publishing, but the authors should attend to the above remarks in their revised manuscript. I am sure the paper will find a lot of readers, because it introduces the original methodology, discusses an interesting flood case study and obviously could be used to other data-limited events.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-496, 2016.