

# ***Interactive comment on “Modelling and assessment of urban flood hazards based on rainfall intensity-duration-frequency curves reformation” by Reza Ghazavi et al.***

## **Anonymous Referee #1**

Received and published: 13 November 2016

The title of the paper indicates a very interesting piece of work. However, I have difficulties following the line of work. Below I list the most important of my concerns.

The authors use the word flooding many times throughout the study but never actually assess flooding, neither theoretically, nor in the case study. This is probably because they use a model for sub-surface piped sewer systems using 1D simulations without capabilities of simulating flooding. They may use the tool for modelling on-ground runoff in channels? Please justify your modelling approach, either by explaining how you have adapted/used the software or by shifting to one of the many 1D2D tool available. Please also choose a suitable title.

According to the authors one of the key objectives is to update and improve previous

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work on IDF-relationships for the region. However, in the end I cannot derive if new data were used or not – and what the findings were.

You claim that the model is calibrated and performs well. Your watershed is 39 km<sup>2</sup> and your peak discharge is less than 0.1 m<sup>3</sup>/s in the largest of the three events you have measured. These are very small events that are not representative for the flows you are trying to model. Hence the model is NOT calibrated.

You compare two methods to estimate design storms. I miss a discussion about which method you prefer and why.

There is a very non-linear response between peak precipitation and peak runoff that are not justified based on the manuscript. You have a high degree of urbanization and hence the response should be more linear. Also, there are some things that makes me wonder if the results are realistic at all. The non-linearity could be the result of low degree of non-permeable surfaces in spite of the high degree of urbanization and hence a response from the pervious surfaces. However, if this is the case then the time of concentration should be higher than 40 minutes from a watershed of the size studied. In conclusion it is absolutely impossible to replicate the study even if all data were presented because critical information is missing.

The authors claim that they do the study because they wish to study the impacts of further urbanization and climate change impacts and cite the works by e.g. Willems and Semadeni-Davies. However, the paper contains no attempt to make projections into the future, nor how to manage current deficiencies (if any). By the way, if you wish a more recent and white publication for the work by Willems you can cite the open source publication where Willems is also author (Arnbjerg-Nielsen et al 2013).

It is very difficult to follow the line of thought several places because of poor language. Not even the first sentence in the abstract is proper English.

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P2, L63: You cannot use the rational method to determine the flooded urban area directly. The title of the work by Asgari et al also suggests that the statement does not reflect the content of what is cited.

P5, L127: Please present the Sherman equation the first time it is mentioned or give a reference. If you assume it known the first time there is no need to write it out on page 8.

P6, L163: Since you have DEM data available it would be possible to extend the analysis to cover flooding.

P7, L201: Correct citation is to Butler and Davies.

P11, Table 3: The numbers in columns three to six does not have the unit (min). Table should be rearranged.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-304, 2016.

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