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Discussions

Interactive comment on "Towards identification of critical rainfall thresholds for urban pluvial flooding prediction based on crowdsourced flood observations" *by* Christian Bouwens et al.

Anonymous Referee #2

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The prediction of urban flooding (location of occurrence and intensity) is an increasingly relevant topic in the field of urban water management. The aspect is interesting not only against the background of a possibly growing risk of urban flooding due to increasing urbanization on the one hand side and a less rapid underground infrastructure development that has difficulties in keeping up with the aboveground urban growth on the other hand. The fact of potentially changing characteristics of extreme rainfall due to a changing climate further contributes to this significance. The present manuscript discusses i) the prediction of urban flooding solely based on detailed rainfall information and ii) the influence of few catchment characteristics (imperviousness, elevation difference) through correlation analyses for a specific case study in the Netherlands.

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*Main points:

1. Setting and boundary conditions of the study are very case specific (flat catchment, specific drainage infrastructure and operational regime (OP), rare data availability (OP data, flood reports)). Hence, transfer of findings to other cases - without having carried out similar analyses for other systems - would be, at least questionable. This clearly lowers the scientific significance of the work (reproducibility). Despite the fact that various interesting aspects of general relevance are discussed (use of citizen-reported flood incidents as ground truth data for urban flooding, changepoint analysis), identified (non-)correlations as well as rainfall thresholds – which relate to the key research questions in the paper - are exclusively valid for the particular case Rotterdam. I am wondering why this clearly limiting aspect has not been discussed in the manuscript. I strongly encourage the authors to address this aspect adequately, e.g. by clearly labeling results and findings as case specific (title, abstract, conclusions) and discussing the relevance of findings for other systems.

2. The title prominently suggests research on a currently popular topic: the collection and evaluation of crowdsourced data to extract meaningful information. The main text then reveals that 'crowdsourced data' are here understood as structured recordings on flood incidents reported from the public (!?) which the researchers "obtained" from an existing database (!?) – cf. page 3, line 39 -41. It is not entirely clear to me if the term "crowdsourced" refers to the fact that different people, i.e. the crowd reported the incidents or that the recordings are received from various different sources but are then formalized and archived. In a way this issue is somewhat peculiar since in previous publications the same data set had been named "citizen-reported flood incidents"... Irrespective of the fact if data used in this study can be referred to as "crowdsourced data" the novelty aspect (in the current version of the manuscript) is marginal. Hence I suggest reconsidering the title formulation or a thorough revision of the paper shifting the focus to the use of crowdsourced flood reports as such, e.g. discussing the quality of this source of information.

3. Using rainfall threshold values (which do not account for the spatial structure, moving patterns of storms) to warn for area-specific flooding incidents is a bit far-fetched. From my point of view, the presented results are not convincing enough to allow lumping changing factors (such as downstream drainage system behavior, operational regime, spatial rainfall variability) which eventually influence the degree of flooding and the location at which flooding occurs into a single rainfall threshold value that predicts flooding in a particular urban subcatchment. I suggest using a physically-based dual-drainage model to systematically partition the influence of these aspects and to so put conclusions on a more solid basis.

4. The spatial correlation analysis between interval-specific rainfall depths and reported surface flooding observations leaves me a bit puzzled. Beside the fact that identified correlations are at the very low end in terms of occurrence and significance, the following points are at least debatable:

i) As this particular part of the analysis is based on only six events I am asking how representativeness is ensured. I do not fully understand why the number of reports must be greater than 40 to allow an event to be included in the analysis. No justification is given on what the selection of this threshold is based on, nor a sensitivity analysis is conducted to show how results alter in case more events (with less reports) are selected. In any case it should be questioned to what extend six events provide enough input for a spatial correlation analysis to come to a meaningful conclusion.

ii) Disentangling dependencies: the weak but still existing correlation between population and report density suggests an inherent dependence between the two variables: the more people live in an area, the more reports can be submitted. Hence it should be discussed to what these variables can be independent at all! Depending on the way how flood reports are submitted, normalizing over population density may be a first step to research this aspect. Technically different, but similar with regard to the dependency aspect, the statement "... imperviousness was confirmed to be an appropriate parameter to predict urban pluvial flooding" (p. 17, line 2-3) is somewhat trivial,

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i.e. misleading since i) yes, sealed surfaces produce higher surface runoff and ii) solely considered, the degree of imperviousness does allow a prediction of flooding potential – it must be considered in context with other factors. A multivariate analysis approach is recommended.

iii) Considering drainage network capacity constraints: the spatial analysis somehow ignores the fact that urban flooding can substantially be influenced by hydraulic behavior of the actual drainage network (e.g. hydraulic capacity constraints further downstream in the network may lead to manhole overflows). In other words: here flooding is expected to occur right where the rain cell is present, suggesting that the main cause for surface flooding is the pure amount of rainfall at the spot maybe combined with a limited capacity of street inlets. It remains an open issue to what extend the found spatial correlation is influenced/biased through this aspect (still, it is outlined in the outlook for further research – p. 17, line 37) and if this could be a reason indeed for the decreasing correlation when increasing the spatial resolution of data (finer grid). Moreover, this aspect is very likely to become more relevant when researching systems with higher terrain elevation variability, i.e. elevation difference in the catchment.

*Some minor points (not complete):

- The paper's layout is corrupt at many points, it seems that the manuscript had been submitted in a rush.

- Scatter plots, especially Fig. 2, 3, 5, are difficult to read. In particular outliers are difficult to spot.

- The discussion of particular events in the text referring to Fig. 4, 5 (p. 10, line 10, 22) is useless unless it is indicated in the graph and has a particular meaning.

- The treatment of outliers in the OP data is occasionally fuzzy (p. 9, line 5ff; p. 10, line 10-11) and sometimes arbitrary (p. 15, line 4-6). The authors should revise the analysis to avoid the impression that leaving out particular data was done to let results

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look a bit better.

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