

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 #1

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It is not clear the novelty/contribution of the work. The methods are not particularly innovative, nor the research questions. The paper reads somewhat like a consultancy report. While it makes a good case study, I am not sure if it is suitable as a scientific publication.

Does the novelty/contribution lie in the use of crowdsourced data? If so, the paper is not written as if it is. If the novelty is indeed in the use of crowdsourced data, then the paper should focus on the crowdsourced data and make more of a discussion/examination of the use of the data to make a strong case of its associated difficulties/advantages. As it is, the introduction and lit review are only in a general sense with no focus on crowd-

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sourcing. And only about 1/3 of the methods and results are based on crowdsourced data.

Also, I am not sure if the historical complaint reports used can be called “crowd-sourced”. The reports are in the order of 10-100 per day (as inferred by Fig. 2). However, in today’s context, crowdsourcing commonly refers to sourcing from a large pool of people using the Internet, smartphones, surveillance cameras etc. to obtain observations in the hundreds to thousands to millions. Thus, it may be misleading to describe the observations referred to in this paper as crowdsourced.

Other concerns:

There could be a temporal resolution mismatch between the flood complaint reports used and a storm event. That is, in a storm event, it may not be possible to determine which reports coincide with the peak rainfall. Thus, there are some uncertainties in the “crowdsourced” observations that are nontrivial and may affect the validity of the methods/results.

The paper found a strong correlation between surface imperviousness and the number of flooding reports and concluded that “there is some explanatory power behind degree of imperviousness as an urban pluvial flooding parameter”. However, the correlation is likely a spurious correlation due to the correlation between population density and the number of flooding reports, and population density and imperviousness. Thus, this conclusion of the paper is not well-justified. It may be advisable for the authors to use other methods (e.g. multivariate linear regression) to exclude the effects of population density and arrive at a more justifiable conclusion.

Other minor points: 1. Page 4, Line 7, km² instead of km² should be used. 2. Page 5, Line 10, I guess the area of the green roof park should be 40,000 m² instead of 40.000 m². 3. Page 6, Line 25, equation (1) should be appeared before equation (2). 4. The authors should also remove duplicated flooding reports in their temporal and spatial correlation analysis. 5. Instead of using the rainfall intensity at the center of the

study area for the temporal correlation analysis, I would suggest the authors to use the aerial average rainfall intensity of the whole study area. This is to take into account the rainfall spatial variability, which could be rather high at the scale of the study area. 6. What is the unit of dt , mm or mm/h? It seems to be mm/h according to the definition in equation (1), but is shown to be mm in Figures 2, 3, and 4. The authors should also provide more clarified explanations for the variables in equation (1).

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