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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Application of logistic regression to simulate the influence of rainfall genesis on storm overflow operation: a probabilistic approach" *by* Bartosz Szeląg et al.

Anonymous Referee #1

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Comments on "Application of logistic regression to simulate the influence of rainfall genesis on storm overflow operation: a probabilistic approach" submitted by SzelÄĚg et al.

General comments: In their manuscript the authors present a comprehensive study on predicting combined sewer overflow (CSO) events through employing a probabilistic model. This model utilizes a generator of rainfall per year, a synthetic precipitation generator (yielding rainfall characteristics for each event), and a logistic regression (logit) model that predicts the number of CSO events, whereby two configurations are considered in the logit model: (i) rainfall height and duration as independent variables;

and (ii) average rainfall intensity as the only independent variable. In the model chain, a sub-division into three different types according to the synoptic situation is applied: convective, frontal, or convergence zone.

Predicting the number of CSO events utilizing stochastic approaches is an interesting approach worth to explore. However, it is hard to judge whether the approach presented by the authors is suitable to pursue this goal. I have to admit that I am not an expert in stochastic modelling which is why I focus on some more general aspects of the work. Even though the paper presents a lot of thorough analyses which are timely and well suited for the publication in Hydrology and Earth System Sciences, it lacks sufficient comprehensibility and an independent validation. Moreover, the authors claim – even though stating that further analyses are still required – that the approach is universal and transferrable. I have some doubts as to whether this statement is valid, given the results shown in the manuscript which are only supported by one single case study. I think it's OK to have only one case study but I would suggest rephrasing universal by a more conservative term.

However, I believe that there is a lot of potential to improve this article. Maybe some of my concerns might arise from my limited understanding of the topic. Even if my comments seem too critical, my intention is to improve the article.

My major concerns pertain to:

Wording: a lot of terms not common in the literature are used throughout the manuscript, e.g., receiver should be replaced by receiving waters. What do you mean by episode? Is it an event or a period or the event duration? Please be more specific! Movement of air could be replaced by advection. The term forecast is not appropriate in this context. I would replace this term by prediction. There is a lot of confusion regarding the usage of overflow and discharge. You do not predict discharge, only the number of overflow events.

Independent validation: Even though the logit model seems to perform very well, it

does not become clear how well the model works in my opinion. Is there any chance to add some comparisons with observations in the Figure 9 (at least averages as vertical lines)? Is it possible to perform a split sample test in order to analyze if the model chain provides reliable results for an independent period of time not involved into parameter estimation?

Transferability of the stochastic approach: as already mentioned, for me it remains unclear how the method could be transferred to other urban catchments. You argue that the catchment detention is physically meaningful. I agree in principle but I could imagine that a lot of other catchment characteristics might be relevant too. For instance, what is the impact of the network structure on the results? Even though the detention is tangible, its empirical deviation (Eq. 5 and 6) is rather empirical. Is there any chance to quantify this value with simplified hydrological calculations? This would support your argumentation regarding the transferability and the practical relevance. It might be worth to discuss the added value when compared to long-term simulation using hydrological (and hydrodynamic) models.

I would suggest revising the manuscript. Some things are too detailed (Section 3) or too general (e.g., the discussion on rainfall models that are not used here in Sect. 4.3; I would expect some methodology rather than an introduction to this topic). Sect. 5.2 should be sub-divided in order to increase readability.

Specific comments:

P2L5: National guidelines?

P3L18: I would suggest adding some more details on the catchment area (e.g., dry weather flow).

Figure 4: When I read the paper for the first time, my first idea was that you compare genesis of rainfall vs. not distinguishing the genesis as shown in Figure 4. In Figure 9, however, you mainly compare the average number of events vs. a modelled number of

events (including the comparison genesis vs. generalization of events?).

P5L9: The discussion on an “increase in the roughness of the substrate” is awkward in my opinion and not correct. Is this discussion really needed here? I would suggest rephrasing this section in a way that makes the explanations more concise, given the topic of the paper.

P17L4p: This statement remains unclear to me. Please be more precise! Why do you abandon this approach? I found this explanation confusing. I thought that M was modelled or even assumed to be constant? How is this related to your argumentation? P18L31: I don’t think that there is any exact model. Please rephrase.

P19L10pp.: I do not understand how the numbers in the text are related to Figure 9. What means N (which has never been defined before)? Is it the simulation mentioned in P8L1p? Maybe it’s worth to provide a table that summarizes the symbols used throughout the manuscript?

Technical comments:

P2L19: Do you mean Thornsal and Willams (2008)?

P3L2: Consider dropping knowledge.

P11L3: What is IC? Iman-Conover?

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