Interactive comments on “The role of storm dynamics and scale in controlling urban flood response”

Reviewer 2:

The authors carried out data-driven assessment of the relationship between rainfall variability and streamflow response at catchment outlets for 5 urban catchments in the Charlotte, NC, area. This area has a relatively dense network of stream gauges and high-quality historical data to allow such a study. Though spatial variability of rainfall and land cover is reflected via fractional coverage, radar rainfall estimates and impervious cover, the study is largely about catchment scale response. Though mentioned in the title, this study has little to do with storm dynamics. The authors describe various analyses, largely statistical in nature, carried out using the above data along with the NEXRAD-based rainfall estimates. They arrive at 7 specific conclusions.
I have a number of major issues, including a few pertaining specifically to methodology, as elaborated below.
AR: We infer from the reviewer’s comments that he/she expected a very different manuscript when accepting to review. We understand the disappointment and see that it has led to misunderstanding in the interpretation of the manuscript. We will adjust our phrasing for those instances that seem to have caused the confusion: “storm dynamics and scale” in the title will be replaced by “storm position, movement and scale. Similarly, “rainfall spatial distribution” in the abstract will be replaced by “storm position, movement and scale” to be more explicit about what spatial aspects of rainfall were studied and what we mean by storm dynamics.

Major comments
1. Methodology
In my view, the authors’ data-driven, largely statistical, analysis could benefit greatly from drawing from the vast literature on modeling studies as well as from applying simple modeling approaches. While I appreciate the motivation for the data-driven approach, I find that the authors are left to connect the dots based almost exclusively on somewhat tenuous observations from noisy data points and a very small number of publications by the same group.
AR: We thank the reviewer for his appreciation of the data-driven approach. He/she is right in that deriving conclusions from field observations is challenging, given the complex nature of the processes involved. The opportunity to study urban flood response based on such long records of combined radar-rainfall and flow observations is unprecedented. We want to emphasise that the datasets are of high quality, hence, what we see represented in the data is not noisiness, but complexity of the underlying processes. Statistical analyses allow us to identify critical parameters for describing flood response, without the need of making any pre-assumptions as in an empirical modelling approach.

My visual examination of the figures in the manuscript suggests that, while various statistical analyses and testing were carried out, the results are overall less than convincing. Calculating correlation to highly nonlinear data, for example, is not appropriate.
AR: We are fully aware of the non-linearity of the processes we’re studying; to deal with this, we have used Spearman rank correlations (not Pearson, which assumes linearity) in our analyses.

In my opinion, deriving empirical unit hydrograph for each catchment at least for a sizable number of single-pulsed events will shed light to the results very significantly. As far as I can tell, the authors have the data to do this. Solving this inverse problem is tricky but doable, given that the authors have high-resolution rainfall and streamflow data. Such analysis would also be entirely in line with the data-driven approach.

AR: Unit hydrograph and similar empirical models make vastly simplifying assumptions, including spatially homogeneous rainfall and fixed rainfall-catchment response relationships. Our analyses aim to identify if and under what conditions such assumptions are realistic. In fact, they show that for the majority of storms, storm characteristics and catchment response are far too complex to be modelled through these simplified relationships. Consequently, all the models could show us is poor fits for most of the storms; this, the data can tell us more straightforwardly.

1.1 Use of radar data (or lack thereof)

In my view, the authors over-rely on the RWD analysis which is basically a proxy for excess rainfall (or runoff depth)-weighted travel time to the outlet. Because it does not account for spatially-varying velocities, attenuation effects, storage effects, nonlinear effects and integration effects, I do not think it is very amenable to quantitative analysis other than perhaps using as an index to infer the general location of the precipitation core relative to the outlet. If that is the case, I strongly think that the authors are better off examining the radar rainfall data directly. They will show with great certainty where the heavy rainfall was and in which direction the storm was moving, etc. Similarly, I find the exposition on storm vs. catchment scale to be a rather roundabout way to deal with the issue. It would be quite straightforward to characterize the size of the heavy rain cores directly from the radar rainfall data.

AR: the reviewer's interpretation of the rainfall-weighted flow distance (RWD) analysis is partly correct: it represents rainfall-weighted distance (along the flowpath network), which equates to travel time only if mean flow velocities along the network are the same across the catchment and across events. The latter is an assumption often made in simplified, empirical hydrologic models. Instead, we analyse the integrated effect of varying flow velocities, attenuation and storage along the flowpath network on hydrological response. This tells us to what extent the position of the storm relative to the catchment determines flow peak and lag time. If we were to analyse rainfall data directly, as the reviewer suggests, we would lose the relation with the catchment characteristics that we aim to analyse.

1.2 Stormwater infrastructure

The authors acknowledge its existence, including dams, but it is completely unclear what they are and what impact they may have. Because the size of the storms that the authors are dealing with is small (the largest several events per year), one would expect potentially significant impact by the storm drain network. The impact by the dams and other detention structures would potentially be greater. Little of this, however, is explained or justified.

AR: The only quantitative information available to us about stormwater infrastructure in the Charlotte watershed is the number of dams, which is low for all 5 catchments (0, 1, 0, 5 and 8 for the smallest to largest catchment). Additionally, a study has been recently published by Bell et al (2016), that includes some additional information for 3 of the 5 catchments we studied. Based on this, they computed the percentage area of mitigated area by detention structures: 5.5, 5.8 and 3.2 % for Little Hope, Upper Briar and Upper Little Sugar, respectively. These numbers show that the impact of detention structures on hydrological response is likely to be very small.
The reviewer is right that this information is relevant and we will add this reference and related information to the manuscript in section 3.4.

1.3 Flowpath analysis

It is completely unclear how this was done. Is this meant to capture channel flows only or both channel and hillslope flows? In their lag time analysis, how did they account to spatially varying roughness/velocity? The nature of this analysis has large implications in interpretation of the results.

AR: The methodology of rainfall-weighted flow distance analysis has been used in multiple previous, well-cited publications by our group as well as by other groups (Smith et al (2002); Smith et al (2005); (Zoccatelli et al. (2011); Nikolopoulos et al. (2014); Emmanuel et al (2015)). It represents the position of a storm relative to the flowpath network and is used to analyse how storm position and movement influence hydrological response (flow peak and lag time).
Regarding the reviewer's question about lag time analysis: we derive lag times directly from the data, as explained in section 2.2.1. Hence, there is no need to make assumptions about flow velocities as one would do in an empirical hydrological model.

2 General lack of clarity and specificity
I find the manuscript a very difficult and frustrating read due to loose notations and very liberal use of certain expressions. I illustrate this using a couple of examples below.
Hydrologic response – I am not sure exactly what the authors mean by this expression which is used numerous times throughout the manuscript. In this work, the authors deal with streamflow response at the catchment and subcatchment outlets only. Urban flooding is a concern not only along the main channels, for whose response the outlet flow is a reasonable descriptor, but also in all upstream areas. I was led to believe by the title that this study deals with the role of spatiotemporal variability of rainfall on urban flooding across scale but it is largely about catchment- and subcatchment-wide response to rainfall.
AR: we realise that the use of the term "rainfall spatial distribution" in the abstract may have been misleading and will replace this by "storm position, movement and scale", as indicated in our reply above. To our knowledge, the term hydrological response is commonly used to describe aspects of rainfall-response in hydrological systems, including peak flow, lag time, runoff ratio etc. The term flood response, then, is used refer to hydrological response to intense events, in the upper tail of the rainfall frequency distribution. To clarify this point, we will rephrase the text in the abstract and introduction, where we outline the context and purpose of our study, to "hydrological response at the (sub)catchment"

Variability - The authors introduce many different types of variability in the manuscript: spatial variability, temporal variability, catchment variability, flow variability, peak flow variability, lag time variability, variability in runoff ratio expressed in terms of CV, climatological variability and possibly more. Many of these expressions are, however, rather loosely defined or undefined. For example, by "climatological variability", I believe the authors mean event-to-event variability. Also, fractional coverage is part of spatial variability of rainfall. If the authors mean inner variability, i.e., variability of positive rainfall by "variability of rainfall", they should indicate as such. If CV is used to measure variability, the authors should clearly state of what quantity, if not the complete mathematical expression. Again, the numerous loose descriptions, definitions and notations (see below) make reading this manuscript rather frustrating in that one has to guess at what the authors may actually mean.
AR: we have used the term variability predominantly to refer to spatial variability of rainfall and to variability in frequency distributions, in terms of coefficient of variation or inter-quantile range (for distributions of values for peak flow, lag time and runoff ratio. This terminology is commonly used in the literature and we did not expect it to be a cause of confusion. We found a few instances where the terms storm scale and storm position are better suited than rainfall variability; we will adjust the text accordingly.

3 Inconsistent and missing notations
There are many places where the notations are missing, inconsistent, if not incorrect, or confusing. For example, on page 9, r and r(t,x) are never defined. If they mean the

same, this is an abuse of notation as the former is a variable and the latter is a function. Also, the usual notation would be r(x,t), not r(t,x). Neither is DRw(t) defined. I do not see how D(t) is a random variable that takes values from 0 to 1. According to Eqs.(8) and (9), if there is excess runoff at time t, D(t) should be zero (assuming r(t,x) denotes rainfall at time t and flow path x). And yet, in Fig 5, RWD seems to be positive even when r(t,x) is zero.

AR: Indeed, a definition of r(t,x) , used in equation 9, is missing. Thanks for spotting the omission, we will correct this. DRw(t) should be D(t), this will be corrected. D(t) is a distance value normalised by maximum flow distance and varies from 0 at the outlet to 1 at the maximum flow distance, i.e. at the headwaters of the catchment, as explained in section 2.2.2. Since RWD is distance multiplied by weighted rainfall it is indeed zero when rainfall is zero. In figure 5, RWD is above zero only when rainfall intensity is above zero (it may be very low, but not zero).

4 Significance

There are 7 specific conclusions the authors draw from this work which are stated in the Summary and Conclusions Section as well as in the abstract. In my view, most of them are already well known and established. I suspect that most practicing hydrologists and water resources engineers, particularly in urban areas, would find them largely a restatement of what they already know and practice.

For the last “unexpected” conclusion, the authors state “We find that urbanisation plays a minor role in explaining variability in peak flow and lag time in the five basins in Little Sugar Creek.” It is not completely clear what is meant by “variability of peak flow and lag time” but, assuming the authors meant event-to-event variability, the above is explained by the following two observations. The first is that these are small catchments (_111.1 km2) and hence, when there is heavy rainfall, it is very likely rain over most or all of the catchment area. This greatly reduces the likelihood of impervious areas amplifying event-to-event variability in runoff generation as they will almost always generate runoff. The second is that, unlike pervious areas, impervious areas will run off essentially all rainfall. As such, there is little event-to-event variability to be expected over impervious areas in small catchments.

AR: We believe the conclusions are not quite as obvious as the reviewer suggests. A few examples to illustrate this:

- in conclusion 2: “Lowest peak flow variability is found for the most urbanised basin”. In many previous studies it has been assumed that urbanisation leads to higher peak flow variability.
- in conclusion 5 and 6: the position and movement direction of a storm play a minor role in explaining variability in hydrological response compared to rainfall volume and peak intensity. This is contrary to previous studies, where storm position and movement have been found to influence flow peak and lag time, often based on very small data samples or theoretical modelling studies (see e.g. Ogden et al., 1995; Seo et al., 2012; Ruiz-Villanueva et al., 2012). It is important to recognise that this large set of field data challenges previous findings.
- last conclusion: contrary to what the reviewer states, our data show (figure 3) that the scale of a large fraction of the storms is (much) smaller than basin scale, especially for the larger basins (>10km2). Hence, one would expect variability in flow response for storms that are spatially concentrated over urban regions versus those that are concentrated over non-urban regions. Our data do not confirm this and we give possible explanations why the field data are probably showing a strongly smoothed signal.

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