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Discussions

Interactive comment on "The role of storm dynamics and scale in controlling urban flood response" *by* Marie-claire ten Veldhuis et al.

Anonymous Referee #2

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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.

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. 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.

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.

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.

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.

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.

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.

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.

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.

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 ($\sim$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.

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