

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

Anonymous Referee #3

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This paper presents a thorough and well-presented empirical analysis of storm rainfall and runoff across a number of highly urban basins. It is perhaps overly ambitious in brining so many facets together in one paper, leading to some difficulty for the reader to separate each of the analyses undertaken, but this is balanced by high quality analysis on a large number of high resolution flood events across 5 basins. The paper has 7 substantial conclusions, and each of them is based on a sound analysis of robust data. The language and presentation is overall good, and the paper is well presented.

Specific comments

The last two sentences of the abstract are unclear and unjustified – they can be improved easily.

C1

The role of soil moisture has not been considered in the paper – can the authors comment and justify on why this has not been considered in their analyses.

Figure 2 and the data– are the event data normal and if not have they been normalized before statistical comparison between events. Also – for rainfall, are the rainfall events in fact not independent – and does this not affect the validity of any comparison between catchments if indeed what is being compared is essentially the same rainfall events that pass over them all? Which sites are significantly different in the plot?

Figure 5 – what is the z axis scale on line 2 – 0-1%? I assume it means 0-100%. Also one of the plots then exceeds 100% in the graphic.

The general layout is difficult to follow as tables are referenced well before they are placed in the document – which can make the paper hard to follow – can this be improved in the final manuscript (e.g. Table 4).

Im confused with Tables 3 and 4 and how they are used in the conclusions – please address the following points. In table 3 you state associated p values are set out, but I see no actual reported p values, only asterix to indicate a p value that is significant, here at 5%. Next in table 4 the significant correlations are in bold, rather than asterix being used. In both it seems Spearman rank correlation and significance - see Table 3 where LLSugar has a 0.25* for Tlag-RWD(ih), while LHope has 0.26 - are not related. First in conclusion 4 its stated that dynamics of rainfall coverage are important drivers of rainfall variability – with spearman ranks values exceeding 0.8 for the five basins – from where is this data taken or reported in the paper – what table reports this? Next in conclusion 4 you note maximum rainfall coverage (storm core?) is significantly and positively correlated with peak flow for two of the five basins (smallest and largest), with values of 0.33, and not significant correlation in the others. Again I cannot seem to link this reporting to the results in text or table. The only 0.33 reported is for UBriar in table 4 and also referred to in the text.

I feel conclusion 7 is interesting and warrants further discussion or possible explanation

C2

– as urbanisation more than doubles in some catchments and the general consensus is more urbanisation equals more runoff and higher peak flows. This should also include some caveat regarding the fact storm water infrastructure was not included.

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