Answers to Referee 1

RC1: Table 1 - runoff coefficient and storage coefficient: the ',' needs to be replaced by '.' i.e. 0.2 instead of 0,2 etc.

AC1: Agree. Changes will be done.

RC2: Figure 2 - Event 2, please check if the correct version of this Figure is included? Has the radar location moved in relation to the catchment, or was the black square in the figure accidentally moved somehow? (i.e.,event 2, meridional distance between ca 2 and 5 km, instead of between ca -4 and -7 km for events 1 and 3). Please check and either correct the Figure or explain the Figure in more detail.

AC2: the authors thank the reviewer for highlighting this detail, the figure without explanation is not clear enough. The author will add in the capture of the final version the following explanation:

"Event 1, 3 and 4 were detected in South-Western quadrant of the radar coverage, while Event 2 was detected in North-Western quadrant. "

RC3: Page 15, sentence on lines 8 - 10 (Results...water depths) needs revising as it is currently not clear.

AC3: The authors apologise for the typo in this sentence. It will be corrected as follows:

"*Results presented in the boxplots show that normalised runoff peaks are more strongly affected by changing spatial resolution of rainfall inputs compared to normalised maximum water depths."*

RC4: Page 20 line 20: should be 0.05 instead of 0,05m

AC4: Agree, corrected.

RC5: Please double check references list, i.e. page 24 lines 17, 21 & page 25 lines 5, 11, 14 & 17 - all 'A' seems to have been replaced by 'a'

AC5: Agree, corrected.

RC6: In the appendix, please check for consistency: is the 'runoff factor' or 'c', the same as the 'runoff coefficient'? And please define 'h'.

AC6: Agree. Runoff factor and runoff coefficient are the same thing: "runoff factor" has been chose to define "c". "h" is not a parameter, but it is a unit, and stands for "hour". Being a universal codification, as well as mm, the authors believe its definition is not necessary in the text.

Answers to Referee 2

General comments:

RC1: Some comments of the reviewers or the editor are said to be mentioned in the answer of the authors but are not really addressed in the new manuscript (ex: EC3 - the potential uncertainty of models are not really discussed in the text, neither climate change in the introduction; EC5 – the answer to this is not found in the text). This is quite annoying and should be changed in the revised manuscript (reading again the comments on the first version on the manuscript)

AC1:

1-climate change: the editor mentioned his agreement with Reviewer#2 that expressed his concern about the fact that it was not proved so far that climate change was "leading to a potential increase in extremes". We agreed with both reviewer#2 comment and editor warning. In the last submitted version we thus modified the first sentence of the abstract from:

"Cities are increasingly vulnerable to floods generated by intense rainfall, because of their high degree of imperviousness, implementation of infrastructures, and changes in precipitation patterns due to climate change."

To:

"Cities are increasingly vulnerable to floods generated by intense rainfall, because of urbanization of flood prone areas and ongoing urban densification".

The authors believe that both Reviewer#2 and Editors comments were taken into account in the last submitted version.

2-model uncertainty: referee#3 first addressed the uncertainty of the model, warning that "*The use of a semi-lumped model (which even does not include green areas) seems less appropriate when the systems response to fine scale spatio-temporal rainfall is to be assessed."* In order to take into account that, the revised version of the manuscript included the sentence (pag. 5 lines 16 to 20):

"A hydrodynamic urban drainage model has been built for the catchment area using Sobek-urban software (Deltares, 2014). Although fully distributed models best describe the effect of rainfall variability on a catchment, the use of a highly detailed semi-distributed model with runoff areas of approximately the same size or smaller than the highest rainfall input resolution of 100mx100m, is a close alternative."

Uncertainty of hydrological models itself was then addressed by referee #4 (RC4), about the sensitivity of the model to its parameters (in particular runoff factor and storage coefficient). The authors acknowledged that uncertainty, although a study on the sensitivity to those parameters was out of the scope of this manuscript (AC4 to referee #4).

Also referee #2 highlighted the fact that "*The confidence usually given to models based on high resolution data is false, as accuracy is not necessarily improved by higher precision" (RC1).* The authors stressed out that the purpose of this paper was not to test the performance of the model, since it cannot be validated with observations, but to test its sensitivity to rainfall resolution (AC1).

The authors believe to have properly addressed this topic in the discussion of the first revision process. However, the authors also feel the need to explicitly mention the uncertainty issue in the final version of the manuscript. To this aim, the following sentence will be included in the introduction of the manuscript (p 4, lines 15-21):

"This study aims at analysing the sensitivity of this urban hydrodynamic model to changes in rainfall spatial and temporal resolution. The study's focus is on model uncertainty related to rainfall input; model performance is not tested here, since storms were virtually applied to the catchment, which did not allow a proper model validation based on water level and flow observations. However, model geometry was strictly checked and model parameters were estimated based on literature values and experts opinion, so that the model is considered to be a reliable representation of local pluvial response."

And the following one in the conclusion (p.22, lines 23-26)

To give a more robust meaning to these sampling numbers, more storm events should be analysed and more catchments should be tested to confirm the findings of this study. Additionally, model sensitivity to rainfall input resolution should be analysed in relation to other sources of uncertainty, such as those related to model structure and model parameter estimation. This requires installation of a polarimetric radar in the city, which is planned for the near future. This will enable model validation according to locally observed rainfall and sewer observations and analysis of different aspects of model uncertainty under different rainfall resolution scenarios.

RC2- I still believe it would be relevant to include one hydrograph for at least one event with flow simulated for various resolutions. It would complement well the discussion based on relative values (because as mentioned in the discussion, the highest relative deviations are found for the event 4 which is generating the less runoff).

AC2: The authors agree that it would be interesting to analyse hydrographs for a deeper understanding of model sensitivity to the various rainfall inputs. The model however has a large number of nodes (approximately 3000) with different behaviours and it would be misleading to present just one hydrograph as it if were representative for the behaviour in such a wide range of nodes. Even selecting a node at or the near the catchment outlet is not an option, since flow directions may change during events due to the influence of a pumping station. Therefore we prefer to leave analysis of hydrographs to a separate study where this can be done in detail to single out differences between events, subcatchments and nodes within subcatchments.

RC3- Few words on the model validation should be added, to justify the fact that it is legitimate to use it for such a study.

AC3: Please refer to the sentence proposed in AC1.

Detailed comments:

1) Introduction

RC4: p.4 l.17-21: given that there is a precise list of questions, we would expect a similar presentation in the conclusion to answer them. l.18 “urban drainage model” is too general in the question, maybe you should use “highly detailed semi-distributed model”.

AC4: p.4 l.17-21:

The authors agree with the reviewer that it is good practice to refer back to questions in the introduction, even if these were too general to be fully answered in this single study, and would like to add the following text to the Conclusion (p. 22 lines 16-20):

The findings of this study helped to provide initial insights into how small-scale precipitation variability affects hydrological response and to what extent an urban drainage model can properly describe such a response. The outcomes showed that critical thresholds are to be expected in terms of the relationship between rainfall resolution and model scales. This study points out that scale relationships are relevant in determining model output sensitivities.

l.18. Agreed and corrected accordingly.

2) Presentation of the case study

RC5: p.5 l.19-20 : a map of the catchment with all the runoff areas (the location of the study area within the Rotterdam region could also be indicated in this figure) could be helpful. More quantitative information is given anyway after with the “model lengths” section.

AC5: The authors agree with the suggestion and will provide a map of runoff areas as well as an image of the location of the catchment.

RC6: 2.2 rainfall data : I am not sure, but I do not think Rotterdam is within the range of the CESAR radar so you should explain how you selected the portion of studied rainfall event and where you implemented it over the catchment (i.e. where and why did you apply the heaviest rainfall).

AC6: Indeed the CESAR radar does not cover Rotterdam area, this will be explicitly mentioned in the introduction (p.4 lines 17-19), where the sentence at AC1:

model performance is not tested here, since storms were virtually applied to the catchment, which did not allow a proper model validation based on water level and flow observations.

3) Method

RC7:p.8 l.11 : “spatially aggregated”, how was it done? Simply the mean over various pixels or the aggregation is done on radar data before implementing a Z-R relationship. I think it should be explained.

AC7: The authors agree, sentence p.8 l.11-13 will be completed as follows (p8 lines 15-16):

“Rainfall data were spatially aggregated by averaging rainfall pixels from the original resolution (30 m near the radar, 100m elsewhere) to 500m, 1000m, and 2000m.”

RC8: Eq. 1: is it done on rainfall rate or radar reflectivity (if not the Z is a bit confusing and should be modified).

AC8: Z is indeed confusing, since Z in this case is rainfall. Z will be substituted with R.

RC9:Figure 4 : indicate what represent the axis

AC9: Axis title will be added to the figure

RC10:p.8 l.27 : “the magnitude of the distance”; this expression is not clear. Please change.

AC10: It will be replaced with “value of the lag distance”.

RC10BIS: Dimensionless parameters: include systematically the equation to help the reader

AC10BIS: Agree, equations will be mentioned in brackets where the text refers to the dimensionless parameter.

RC11:p.11 l.15-16 : use “length” rather than “resolution” because the parameters were introduced as lengths.

AC11: Agree. This will be changed accordingly.

RC12: 3.2.4 : l.28 : “which corresponds to the inverse of a sewer density”. I do not understand this, given definition of the sewer density in 3.1.3. How two quantities with the same physical dimension (a length) can be inverse? Why defining the sewer length LS before and not using it?

AC12: We thank the reviewer for pointing this out; the use of the terms is indeed confusing here. We will adjust the text by using sewer length to represent sewer density, as it was defined in 3.1.3. The text in 3.2.4 l28 will be removed.

4) Results and discussion

RC12:p.13 l.7-17 : comments and physical interpretation that would ease the reading should also be added, only stating the figures is not enough.

AC12: this first part is meant to be an introduction to results and discussion, which will be detailed in each following subchapter. This is why we just summarise main results for scale lengths and dimensionless numbers and leave interpretation to the following paragraphs. The authors thus believe that comments and physical interpretation in this general introductive paragraph are not necessary.

RC13: p.13 l.22-23 : it would help the reader if you just said that it was obtained for various rainfall resolution and that no hydrological modelling are used. Figure 5 : the “vs.” are reversed in the caption and not in the manuscript (other figure captions should also be checked). The legend says mean over the pixels, and the text over the catchment please clarify. From my understanding it is not “standard deviation of normalized rainfall volume” but “normalized standard deviations of rainfall volumes” (as said after by the way). Authors should be much more careful with the words; such imprecise vocabulary make it much harder to understand, and this was already mentioned in the previous reviews !!

AC13:

- p.13 l.22-23 : Agreed, the following sentence will be added:

“This result was obtained only analysing various rainfall resolution and no hydrological modelling was used.”

- *The authors apologise for the error of reversed “vs”and will correct it.*
- Agreed, changed into “over the catchment” in both legend and text.
- The standard deviation was computed over normalised rainfall volumes, which in turn have been computed through Eq.2. So the correct statement is “standard deviation of normalized rainfall volume”, which will be used in the whole text.

RC14:p.13 l.28-30 : as said in my previous review (and this has not been addressed), the decrease is due to the artificial transfer of rainfall outside the boundary of the catchment. It could be the other way if the heaviest portion of the storm was outside. So I am not sure that the tendency is really relevant, but the order of magnitude are and this should be stressed. By the way this effect is explained at the sub-catchment scale in sections 4.1.4 and 4.1.5, so it should also be discussed here as well. I think is should also be changed in the abstract.

AC14: As it is stated in lines 28-30, the decrease is due to the artificial transfer of rainfall outside the boundary of the catchment. That was also a finding in Ogden and Julien (1994) analysis, and indeed it depends on the position of the storm with respect to the catchment. The study focuses on that, i.e. how much rainfall values deviate from a reference not only with coarsening the resolution, but also in relation to catchment size. In that sense, results show that deviations are bigger when rainfall resolution approaches catchment size. This finding agrees with Ogden and Julien findings, they analyse 50 storms, in different positions and orientation. The authors believe that the result is not biased by a decentralised position of the storm with respect to the catchment, as the storm is dynamically moving over the catchment, so the storm core will always be at the catchment boundary at some point in its trajectory.

In 4.1.4 and 4.1.5 the following sentence will be added:

This effect is also due to ‘catchment smearing’ addressed in 4.1.1.

And the definition of ‘catchment smearing’ will be mentioned in 4.1.1.

We prefer not to change the abstract since it does not refer to these particular results.

RC15: p.14 l.15-16 : “a more pronounced internal spatial structure” what does it mean and how did you obtained that (there is only a 50m differences in storm decorrelation length). The expression is also used after in the section and the meaning should be explained there as well.

AC15: “a more pronounced internal spatial structure” will be substituted by “a steeper spatial gradient”, which is the meaning the sentence intended to have.
The text will be changed to read as follows (p.14 lines 15-18):

“Storm cells in event 2 are characterised by steeper spatial gradients in rainfall intensities compared to event 1 and as a result maximum rainfall intensity values are more strongly affected by changes in rainfall resolution.”

RC16:p.14 l.21 : a clear reduction “of” the median

AC16: This will be corrected

RC17:p.15 l.9 : check sentence because not clear

AC17: Same comment was posed by Referee#1, the sentence will be changed as follows:

Results presented in the boxplots show that normalised runoff peaks are more strongly affected by changing spatial resolution of rainfall inputs compared to normalised maximum water depths

RC18: 4.1.3 : given that the results are used in the discussion of the previous sub-section (which uses the storm structure), I would suggest to move it before. Figure 8: include the event number in the figure (the “going clockwise” of the caption is not “user friendly” for the reader)

AC18: Anisotropic semi-variogram results are used in the following sub-section, where rainfall sampling number is used for analysis. Thus the authors believe that 4.1.3 is in the right place. Sub-section 4.1.2 does use storm structure, but not related to storm de-correlation length.

Event numbers will be added to the caption of figure 8

RC19: 4.1.4 : the “in-sewer” in the title is a bit confusing because it is also the case in section 4.1.2

AC19: The authors agree with the reviewer that this is confusing and will add “in-sewer” to the title of 4.1.2 as well.

RC20: p.17 l.14-15 : “deviations ... increasing” is not clear. Are you mentioning a monotonic trend in the curves ?

AC20: “Monotonic trend” cannot be used since the curves are not fully monotonic in some cases. Instead, “deviation growth” is used. However, since it is not clear, ”grow” will be changed into “increase”.

RC21:p.17 l.28 : it is not clear to me how the threshold of 0.5 was obtained (I already mentioned this issue in my previous review and this was not addressed).

AC21: 0.5 is not a proper threshold but the authors think it is worth to be mentioned, in reference to the earlier study of Ogden and Julien (1994), who have identified this value as being critical for storm smearing. In order to give the reader this information, this sentence will be added (p.17 lines 28-31):

“Results suggest that for most subcatchments, storm smearing occurs for L_R/L_D ratio above 0.5. Figure 9 shows that while storm smearing already affects water depths at values of L_R/L_D below 0.5, the effect becomes a lot stronger for values between 0.5 and 2”

RC22: 4.1.5 : again from Figure 10, the threshold of 20 mentioned in the text seems rather arbitrary and should be justified (not obvious from the rather steady trends Fig. 10) or simply not used.

AC22: the value of 20 was used in the text as a reference to clarify the observed effect, it is not meant to indicate a threshold, which would indeed be rather arbitrary. As it is clearly stated in the text, “Deviations from 100m simulation results remain between 0.9 and 1.1 for L_R/ L_{RA} <20, while higher deviations up to almost 50% occur for L_R/ L_{RA} >20”. The author preferred to choose a value to give a first numerical insight on what “runoff sampling number” means, being aware and having made previously aware the reader about all the limitations that this study has.

RC23: p.18 l.17 : 900m of pipes per 100m2 seems rather high, is not it 100m*100m

AC23: Yes indeed, the authors apologise for the typo, it will be corrected into 100m*100m.