SUMMARY OF AUTHORS' REPLY TO REVIEWERS'COMMENTS

PAPER TITLE : Effect of climate change and variability on extreme rainfall intensity–frequency–duration relationships: a case study of Melbourne

AUTHORS : A.G. Yilmaz, I. Hossain and B.J.C. Perera

No.	Editor's Comment	Author's Reply	Author's Notes / Actions
1	This is a nice manuscript. To further improve it, please consider the both reviewers' suggestions. I am sure you can accomplish this (as you already have indicated in your reviewer's responses). I also recommend to include some more recent references, which tackle (the rather rare) investigations about changes in occurrence of rain storms (high intensity events), e.g.: Mueller & Pfister, J Hydrol, 2011: Increasing occurrence of high-intensity rainstorm events relevant for the generation of soil erosion in a temperate lowland region in Central Europe. Buerger et al., J Hydrometeorol, 2014: Towards Subdaily Rainfall Disaggregation via Clausius-Clapeyron.	Please see next column	Authors would like to thank Editor for his positive and constructive comments. Some recent papers on changes in occurrence of storm events with high intensity (including the ones recommended by Editor) were reviewed and added to the revised manuscript.

REVIEWER #1:

No.	Reviewer's Comments	Author's Reply	Author's Notes / Actions
1	General remarks: The topic of the manuscript is of high relevance for the scientific community. Effects of climate change on the "Intensity-Frequency-Duration" relationship of precipitation might have enormous effects on society, so the analyses of the historic development of this relationship can give us important insights. The manuscript is well written and structured. The chosen tools and techniques fits to the topics, but the description of some of the techniques should be improved.	Please see next column	Authors would like to thank Reviewer for his/her positive and constructive comments.
2	The outcomes are descripted in a proper way except the results of the deviance tests. The discussion of these results should be extended.	Please see next column	Please see response to the Comment 16.
3	The conclusions are quite short and should be extended.	Agreed	The conclusions were extended in the revised manuscript.
4	Minor remarks: (Trenberth et al., 2007) and IPCC (2007) refer to the same source.	Agreed	Trenberth et al. (2007) was replaced with IPCC (2007) in the revised manuscript.
5	(Hu,2013) (p. 6319, line 17) is not listed in the references.	Agreed	Hu (2013) was listed in the references in the revised manuscript.
6	"urban flash flood producing hourly rainfall intensities" is mentioned several times (p. 6312, line 22; p.6326, line 19; p.6329, line 9) but never defined.	Agreed	"Urban flash producing hourly rainfall intensities" was defined in the revised manuscript. This can be seen in the last paragraph of Section 4.4 (Stationary GPD Models).

7	It should be clarified which thresholds or return periods are relevant for " urban flash flood production" in Melbourne.	Please see the next column	Threshold values for extreme rainfalls causing urban flash floods were shown in Table 1 in the manuscript. These threshold values were decided using mean residual plots as recommended by several studies (please see Section 3.1 for details). The "urban" term was used in the manuscript, since data were obtained from a station (i.e. Melbourne Regional Office station) located in Melbourne metropolitan area. However, methods of this study (including threshold selection method) are equally applicable for the rural regions.
8	p.6315, line 16, 25 "La Lina" instead of La Niña.	Agreed	It was corrected in the revised manuscript.
9	p.6318, line 27+28 are redundant	Agreed	These lines were removed in the revised manuscript.
10	p. 6320, line 4+5. The test of data dependency must be done just once.	Agreed	These lines were removed in the revised manuscript, since the test of data dependency was explained earlier in the manuscript.
11	"Too high threshold selection decrease the bias, but increases the variance (...)" – this statement is clear concerning the variance, but not for the bias. Please include a short explanation.	Agreed	This part was re-written in a better way in the revised manuscript. This can be seen in Section 3.1. (Threshold Selection and Extreme Rainfall Data Set Construction) in paragraph 3 in the revised manuscript.
12	p.6321 line 3, (Formula (1)): in the description of the formula the terms "sigma" and " gamma " are used. Later in the text (p. 6322) the terms "scale" und "shape" parameter and in p. 6323 the shape parameter is named zeta instead of gamma. The notation should be harmonized.	Agreed	The notations for scale and shape parameters were corrected/harmonized in the revised manuscript.
13	p. 6322 Formula (2): Bracket is missing	Agreed	Bracket was added to the Equation 2 in the revised

			manuscript.
14	p. 6322, line 26: "(...) it is not realistic to attempt to estimate the scale parameter (...)" should be the "shape parameter"	Agreed	It was corrected in the revised manuscript.
15	p. 6323, line 6+7 "It should be noted that the exponential function has been adopted to introduce time dependency in the scale parameter to ensure the positivity of sigma.". There are several functions which never gets negative. So this is not an explanation why an exponential function is chosen.	Please see next column	Exponential function was used to ensure the positivity of scale parameter in this study; since it is recommended by some studies (e.g. Furrer et al. 2010). However, different functions can also be used for the same purpose. This explanation can be found under Section 3.4. (Non-stationary GPD (NSGPD) Models) in paragraph 3 in the revised manuscript.
16	p. 6323 line 16f – The explanation of the "deviance tests" is quite rudimentary – and also the discussion of the results of this test (p. 6325).	Please see next column	Explanation of the deviance statistic test was extended in the revised manuscript. Expanded explanation of the deviance test can be seen in the last paragraph of Section 3.4. (Non-stationary GPD (NSGPD) Models). In Section 4.3 (NSGPD Models), results of the deviance test was discussed for 3 hour storm duration as an example to show how deviance test is used to decide superiority or non-superiority of non-stationary models over stationary models. It was explained in this section that non-stationary models do not outperform stationary models for 3 hour storm duration. This is the case for all other storm durations (including the durations, in which extreme rainfall data showed statistically significant increasing trends) in both time periods (i.e. 1925-1966 and 1967-2010). With the extended explanation of deviation test in Section 3.4, it was felt by the authors that no further explanation/discussion of results of the deviance test is necessary.

17	p. 6323, line 23: "xˆ2_k distribution": - write it explicitly (chi square) or "chi", but not x , is misleading.	Agreed	“Chi square” term was used in the revised manuscript.
18	section 5 (conclusion): The results of section 4.5 (impact of IPO) is not mentioned in the conclusion.	Agreed	The results of Section 4.5 was mentioned in the conclusions in the revised manuscript. This can be seen in the second paragraph of the Section 5 (Conclusions).

REVIEWER #2:

No.	**Reviewer’s Comments**	**Author’s Reply**	**Author’s Notes / Actions**
1	The study by Yilmaz et al. deals with an important aspect of hydrological research which has received surprisingly little attention over the last decade: the nonlinear time series of extreme rainstorms, especially the ones with very short duration and the potential changes over time of intensity-frequency-duration relationships, - although the latest IPCC reports point out frequently that more extreme rainfalls will occur (and might have already occurred). The analysis of time series with very small sampling intervals (less than one hour) is however highly limited by data-availability. Yilmaz et al. have excellent data and their ideas, methods and way of approaching the questions is very well detailed, well structured and transferred	Please see next column	Authors would like to thank to Reviewer for his/her positive and constructive comments.

	in their article and besides some minor comments adequate for publishing.		
2	Minor comments: The conclusion as it is written now does not fit well to the introduction – maybe the introduction could already mention that this study is a 'demonstration' study and then the conclusion could point out how to proceed future research, e.g. how to get time series of such high resolution and long extent for other parts of Melbourne, Australia, and even how a global study could be perceived. Otherwise, the length of the conclusion is adequate.	Agreed	Conclusions section was expanded according to the Reviewer's advices in the revised manuscript. Limitations of this study and recommendations for future studies were explained in the conclusion section in the revised manuscript. This can be seen in the last paragraph of the Section 5 (Conclusions).
3	Maybe it could be pointed out somewhere why there would be a shift in extreme regime, if it would be climate-driven (e.g. changes in local evaporation due to higher temperature?).	Agreed	This was pointed out in the second paragraph of Section 4.6 (Climate Change and Extreme Rainfalls) in the revised manuscript.
4	The pictures need better resolution: they also look squashed.	Agreed	All figures were revised and resolution/clarity of figures was improved.
5	Yilmaz et al. deals with an important aspect of hydrological research which has received surprisingly little attention	Please see next column	Authors would like to thank to Reviewer for his/her positive comments.