Responses to Reviewer #2 on "Does Nonstationarity in Rainfall Requires Nonstationary Intensity-Duration-Frequency Curves? By Poulomi Ganguli and Paulin Coulibaly

We thank Referee #2 for reviewing our manuscript and providing constructive feedback. Our responses are embedded within the comments (in BLACK) in BLUE.

Reviewer #2

Comment 1. The manuscript could do with a good proof read and rewrite. There are lots of little mistakes which makes the paper very difficult to read. I was constantly stopped in my train of reading by small errors or references to figures/tables which weren't explained. The supplementary material is 66 pages and has 37 Tables. This is huge and difficult to come to terms with – I couldn't follow it all. As I don't believe a specific structure is required can I recommend the following? Group the supplementary text and figures and tables into sections. That way you will have separate sections to refer to in the main text. You can then go sequentially through the text. S1 is the infilling, S2 is the autocorrelation method and results, S3 non-stationarity test method and results, S4 GEV fitting. I may have got the headings incorrect but I hope what I mean is clear. Then with the results you can just reference a section for detailed results and focus on discussing the figures in the main text. Trying to interpret 37 tables (some split into two) – almost all which are referenced in the main text - it is like trying to read a thesis.

Response: This is indeed a good point and we have revised the supplementary section and reorganized the material into various sections as suggested. We discussed corresponding results in the form of tables and figures under each subsection making it more coherent and easier to read. Also, we have moved some of the Tables (for example, Table S1) from supplements to main manuscript reducing the length of the Supplements to 57 pages with 30 tables all together.

Comment 2. Moving Table S1 to the main text, and maybe removing Figure S1 altogether will make the manuscript more standalone and easier to read. This manuscript is a bit short on doing justice to some of the previous work done in this area.

Response: Here we partially agree with the reviewer's comment. We have moved Table S1 to the main text. However, we have retained the Figure S1 in the Supplement since the figure provides a conceptual representation of changes in probability density functions of extremes in a nonstationary environment. We feel the figure will help readers in understanding how the nonstationarity may lead to changes in the distribution of extremes, which can potentially lead to the changes in the frequency of extremes.

Comment 3. Page 2 – Line 21: This is the only line discussing previous work to do with non-stationary IDFs. I think this work deserves more attention given that the focus of this manuscript is non-stationary IDFs. My recommendation is as follows:

In Page 1 – Line 23: "In a warming climate . . ." I would be a bit more careful here and expand this. I would cite Lenderink and van Meijgaard (2008) and Wasko and Sharma (2015) as papers that link temperature increases to intensifying rainfall. Most of the papers cited at the end of this sentence deal with temporal precipitation trends (and not necessarily links to temperature). It is important to make that distinction.

The reason I make the above point is the covariate used for non-stationarity is important. The authors don't raise this till the second last of their manuscript citing Mondal and Mujumdar (2015). This needs to come up in the introduction to put this manuscript novelty in context. There are more papers in this space. For example Agilan and Umamahesh (2017) and Ali and Mishra (2017) who argue for temperature to be used as a covariate (and not necessarily time). Indeed Wasko and Sharma (2017) show that temperature is a good covariate when predicting future rainfall. Other work by Agilan and Umamahesh may also be relevant and should be discussed. Finally, I am pretty sure at least one of the Yilmaz papers suggests not much evidence (if any) for using non-stationarity so in the introduction this is not cited correctly (though I note in the discussion it is). To summarise – the literature review needs to be expanded on the above point.

Response: Agreed. We expanded the literature review section in the revision. We add following sentences in the revision:

"For sub-hourly and up to six-hourly extreme precipitation, increases at or above the C-C rate have been found in the Netherlands (Lenderink and van Meijgaard, 2008; Lenderink et al., 2017), Switzerland (Ban et al., 2014), Germany (Berg et al., 2013), the UK (Blenkinsop et al., 2015), the Mediterranean (Drobinski et al., 2016), most of Australia (Wasko and Sharma, 2015, 2017), North America (Shaw et al., 2011) and China (Miao et al., 2016), while in India (Ali and Mishra, 2017) and northern Australia (Hardwick Jones et al., 2010) negative rates have been observed. The extent of urbanization also contributes to extreme regional precipitation through urban heat island effect and aerosol concentration (Dixon and Mote, 2003; Mölders and Olson, 2004; Nihongi et al. 2007; Mohsen and Gough, 2012; Wang et al., 2015). Agilan and Umamahesh (2017) incorporated six physical processes, namely, time, urbanization, local temperature changes, annual global temperature anomaly (as an indicator of global warming), El Niño-Southern Oscillation (ENSO) and Indian Ocean Dipole (IOD) as covariates in the nonstationary GEV models for analyzing extreme precipitation in the city of Hyderabad, India. Their analysis indicated that the local processes, urbanization and local temperature changes are the best covariates for short-duration rainfall, whereas global processes, such as, global warming, ENSO cycle and IOD are the best covariates for the long duration rainfall. In their study, however, time was never qualified as the best covariate for modeling local scale extreme rainfall intensity. Singh et al. (2016) performed nonstationary frequency analysis of Indian Summer Monsoon Rainfall extreme (ISMR; defined as cumulative rainfall over continental India during 1 June to 30 September) and found evidence of significant nonstationarity in ISMR extremes in urbanizing/developing-urban areas (transitioning from rural to urban), as compared to completely urbanized or rural areas.

However, their analysis was performed at a spatial resolution of 1° using gridded daily precipitation data obtained from Indian Meteorological Department (IMD). Ali and Mishra (2017) showed that a strong (higher than C-C rate) positive relationship exists between 3-hourly and daily rainfall extremes and dew point, and tropospheric temperature (T850; or the temperature in the upper troposphere at 850 hPa) over 23 urban locations in India. The latter two were subsequently used as covariates for nonstationary design storm estimates. The results indicated an increase in rainfall maxima at a majority of locations assuming nonstationary conditions over stationary atmospheric conditions. In contrast, in another studies, over Melbourne and Victoria, in Australia, Yilmaz et al. (2014; 2017) found superiority of stationary models over nonstationary models. Yilmaz et al. (2014; 2017), considered both nonstationarity in time and large scale climate oscillations affecting Australian rainfall in their analyses. However, most of these previous studies have analyzed changes in expected point estimates of nonstationary versus stationary Design Storm Intensity (hereafter referred as DSI), but have not reported the statistical significance of the difference between two methods of estimates". To our best knowledge, no thorough comparison of stationary vs. nonstationary methods for deriving IDF statistics has been conducted in Southern Ontario.

Comment 4. Another problem I have is with the paragraph on Page 3 that starts with "secondly" – I don't think any of the research questions actually address the "secondly" point. Reading page 7 it seems you adopt the GEV and don't necessarily test this is a better fit than other distribution. This is fine – but the way this paragraph sets up the reader for something else. Either omit the "secondly" paragraph altogether or add another point to the bottom of Page 3 saying you use a GEV and the reason for doing so.

Response: This is indeed a good point. Agreed! We have re-organized this section and moved limitations of GEV in subsection 3.3 (lines 15 – 22) in section 3. The choice of the GEV was based on a previous study where various distribution functions were compared in the study area (Switzman et al. 2017).

Comment 5. You introduce the EC data without context – so I had no idea why it was there until I got to page 11.

Response: We appreciate the reviewer's point. We have introduced few sentences in the introduction section (page 4, line 20-23) to highlight the rationale behind the inclusion of EC data. We argue that:

"… so far very few studies have reported the difference between the updated versus EC generated IDF, taking into account nonstationarity in design consideration. Simonovic and Peck (2009) compared updated versus EC IDF for the city of London, Ontario and reported EC IDF curves shows a difference of the order of around 20%. However, their analysis was based on the stationarity assumption of precipitation extremes." Similarly, Coulibaly et al.

2015 have compared EC-IDF with stationary GEV based IDF across southern Ontario, no nonstationary methods were investigated.

Comment 6. Top of Page 11 reads like a discussion and seems squished between the presentation of results in Figure 5 and 6. You could consider a separate discussion section and reordering of the text.

Response: Agreed. We have moved this part of the text to Discussion and Conclusion section.

Other comments:

Page 2 – Line 16: If you are to introduce an abbreviation (TBRG) it helps to capitalise the first letter in each word before the abbreviation. This happens at several points in the text – I won't comment on the other occurrences.

Response: Agreed. We have capitalized the first letter in each word before the abbreviation for TBRG and other words in the revised version of the manuscript.

Page 2 – Line 22: "The nonstationary behaviour. . ." I think I would expand this sentence to just state what places/regions the citations have studied. Reason being – in the abstract and following sentences you are referring only to Canada – so when I get to this point I am not sure if you are being Canada specific or not. Maybe this should be the start of a new paragraph and expanded a bit.

Response: Agreed. As suggested we have expanded this sentence to include list of regions where the citations have studied in Page 4, lines 6 – 13. We also started this in a new paragraph as suggested. We have added following sentences in the revision:

"The nonstationary behavior of rainfall extremes is already being reflected in the increase in frequency or magnitude of such events, resulting in a shift of its distribution [Figure SPM 0.3 in Intergovernmental Panel on Climate Change Special Report on Extremes, IPCC SREX Report: Field, 2012; Fig S1: IPCC AR5 working Group Report, (Stocker et al., 2013)]. For instance, seasonal and annual extreme precipitation events in north-central and eastern US in 2013 (Knutson et al., 2014); extreme rainfall events in the Golden Bay region in New Zeeland (Dean et al., 2013); increase in precipitation rate in northern Europe (Yiou and Cattiaux, 2013), successive winter storm events in southern England in 2013/2014 leading to severe winter floods (Schaller et al., 2016), are primarily attributable to intrinsic natural variability and partly to anthropogenic influences."

Also, in Page 2, lines 1-6, we list the places where increase/decrease in extreme precipitation is linked to C-C scaling. We have added following sentences:

"For sub-hourly and up to six-hourly extreme precipitation, increases at or above the C-C rate have been found in the Netherlands (Lenderink and van Meijgaard, 2008; Lenderink et al.,

2017), Switzerland (Ban et al., 2014), Germany (Berg et al., 2013), the UK (Blenkinsop et al., 2015), the Mediterranean (Drobinski et al., 2016), most of Australia (Wasko and Sharma, 2015, 2017), North America (Shaw et al., 2011) and China (Miao et al., 2016), while in India (Ali and Mishra, 2017) and northern Australia (Hardwick Jones et al., 2010) negative rates have been observed."

Page 2 – Line 26: What result? This sentence doesn't make sense – maybe some expansion of the sentences here would help.

Response: Agreed. We have revised this sentence as:

"The asymmetric changes in the distribution of extremes owing to climate change have been subsequently validated for winter temperature extremes over the northern hemisphere (Kodra and Ganguly, 2014), and regional short duration precipitation extremes in India and Australia (Mondal and Mujumdar, 2015; Westra and Sisson, 2011)".

Page 3 – Line 7: Replace "secondly" with "The second drawback of IDF curves is". You have written too much to have just the word "secondly" here. Stylistically, I don't think "first", "second" etc need to be in italics. Particularly at the bottom at Page 3 – if you are that keen on this maybe a bullet point list would be better?

Response: We have revised this section in the current version of the manuscript.

Page 4 - Line 1: Remove "secondly".
Response: Agreed and incorporated as suggested.

Page 5 – Line 6: The reference to Table S1 doesn't belong here. I also believe Table S1 belongs in the main text.

Response: Agreed. Table S1 is moved to the main manuscript.

Figure 1 – Are the record lengths for daily or sub-daily? I don't think the caption says which.

Response: Agreed and we have revised the caption accordingly. This includes hourly, sub-hourly and daily record, which we together termed as short-duration Annual Maxima Precipitation (AMP) record.

Page 5 – Line 26 – "Imputation" isn't the correct word I don't think. Infilling maybe?

Response: Agreed and incorporated as suggested.

Page 6 – Line 21 – Stylistically, why don't you just say "Tables S2-S4"? I do feel if you composed the supplementary material in sections you could say section S1 and be done with it.

Response: Agreed and incorporated.

Page 6 – Line 24 – "Figure 2 shows . . ." You are repeating a previous a sentence Section 3.2 – Is the KPSS test in Figure 2?

Response: Agreed. We have included KPSS test in the flowchart.

Page 8 – Line 4 – who else makes this assumption that only the location and scale parameter vary? I know other authors make this assumption so this assumption needs to be put in context of the other work done in this area.

Response: Agreed. We have included list of references that assumes location and scale parameter(s) vary. We have added the following sentences in page 10, line 21 in the revised manuscript:

"For nonstationary model, the shape parameter is assumed as constant throughout. Here it should be noted that for modeling temporal changes in ζ requires long-term observations, which are often not available in practice (Cheng et al., 2014). Hence, following previous studies (Cannon, 2010; Cheng et al., 2014; El Adlouni et al., 2007; Gu et al., 2017) we incorporated time-varying covariates into GEV location (GEV$_t$-I), and both in location and scale parameters (GEV$_t$-II) respectively, to describe trends as a function of time".

Page 8 – Line 18 – So I went to the supplementary material as the text recommends and I saw four models fitted for each duration but I wasn't sure which model was which. Could this section in the main text be rewritten (maybe use some sort of list?) to say what models were fitted and clearly state their abbreviation

Response: This section has been revised. Further, as suggested the abbreviations of models are included in page 10, line 22 and in the footnote of Table 4.

Page 8 – Line 26: I disagree. Skewness of a distribution does not indicate a temporal trend. This a good example of a vague sentence with a Figure in brackets (in this case Figure 3) but no mention of what I am meant to get out of looking Figure 3 in reference to this sentence. This happens throughout the text.

Response: Agreed. We have revised the sentence as follows:

"The skewness is a measure of the asymmetry in the AMP distribution. Positive values of skewness indicate that data are skewed to the right."

Further, we have removed such inconsistencies in the revised version of the manuscript.

Figure 3 – your caption says hourly and sub-hourly. The headings in the captions go up to daily. You say you did statistical tests at 5 and 10% but don't say which final significance is presented in the plot. A legend wouldn't go astray . . .

Response: We have revised the Figure 3 caption as suggested.

Figure 4 – is there a particular time used for the non-stationary plots?

Response: This comment was not clear to us. Nevertheless, we have revised the Figure 4 caption to avoid any ambiguity. We have revised our figure caption as:

"DSI estimates of median (horizontal line within the box plot) and 95% credible intervals for 100-year return periods of stationary versus nonstationary models (a - i). The boxplots indicate the uncertainty in estimated DSI using Bayesian inference."

Page 10 – Maybe I missed this somewhere but what is the "z-statistic"? Is this the statistical test for the difference between two means?

Response: The reviewer is correct. The z-statistic is the test score for the difference between two means. We clarify this procedure in the Supplementary section of the revised manuscript.

Figure 6 – Should this have a negative scale too? Are there some sites which decrease?

Response: The reviewer is correct. We have added color map for negative scale too in the revised manuscript.

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