Hydrology and
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Discussions

EGU

Interactive comment on "Does Nonstationarity in Rainfall Requires Nonstationary Intensity-Duration-Frequency Curves?" *by* Poulomi Ganguli and Paulin Coulibaly

Anonymous Referee #2

Received and published: 25 July 2017

The novelty of this manuscript is applying the existing methods proposed by Cheng and AghaKouchak (2014) and Cheng et al (2014) for non-stationary IDFs to Canadian gauge rainfall. The conclusion that statistical trends in rainfall do not necessarily require the use of non-stationary IDFs is a good one. Unfortunately the impact of the results is lost in the relatively poor structure of the manuscript.

General comments:

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.

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.

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.

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.

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

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.

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.

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.

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

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?

Page 4 - Line 1: Remove "secondly".

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

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

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

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.

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

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.

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?

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.

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 . . .

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

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

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

References:

Lenderink, G., and E. van Meijgaard (2008), Increase in hourly precipitation extremes beyond expectations from temperature changes, Nat. Geosci., 1(8), 511–514, doi:10.1038/ngeo262.

Wasko, C., and A. Sharma (2015), Steeper temporal distribution of rain intensity at higher temperatures within Australian storms, Nat. Geosci., 8(7), 527–529, doi:10.1038/ngeo2456.

Agilan, V., and N. V Umamahesh (2017), What are the best covariates for developing non-stationary rainfall Intensity-Duration-Frequency relationship?, Adv. Water Resour., 101, 11–22, doi:10.1016/j.advwatres.2016.12.016.

Ali, H., and V. Mishra (2017), Contrasting response of rainfall extremes to increase in surface air and dewpoint temperatures at urban locations in India, Sci. Rep., 7(1), 1228, doi:10.1038/s41598-017-01306-1.

Wasko, C., and A. Sharma (2017), Continuous rainfall generation for a warmer

climate using observed temperature sensitivities, J. Hydrol., 544, 575–590, doi:10.1016/j.jhydrol.2016.12.002.