

Interactive comment on “Extreme weather exposure identification for road networks – a comparative assessment of statistical methods” by Matthias Schlögl and Gregor Laaha

Matthias Schlögl and Gregor Laaha

matthias.schloegl@ait.ac.at

Received and published: 3 March 2017

1 Response to Reviewer 1

We would like to thank the referee for the very positive evaluation of our manuscript and the provided feedback. Please find our responses below, with referee comments in italics, and authors' responses in standard format.

[Printer-friendly version](#)

[Discussion paper](#)



1.1 Specific Comments

1. *page 2, line 30: Regarding works on extreme temperatures modelling, the authors may wish to consult (...).*

Comparative studies on extreme temperature modelling are rare. Grotjahn et al. (2016) argue in favor of the POT approach for the application on large scale meteorological patterns, but the comparison is based on literature review rather than data-based analyses. We will add this information to the MS. Hasan et al. (2012), Caroni et al. (2016) and Kharin et al. (2007) appear less relevant, as they only apply a single approach (AMS) and did not compare the results to the alternative approach.

2. *page 7, line 14: The authors might wish to discuss why they haven't considered using distribution fitting statistical tests such as the Kolmogorov-Smirnov and/or the Anderson-Darling, for the assessment of the performance of the parameter estimation methods.*

Distribution-fitting tests are primarily useful for gaining an appreciation whether a lack of fit is statistically significant, or rather an effect of sampling uncertainty, but they have little discriminative power to identify the “true” or “best” distribution to use (e.g. Stedinger, 1993). Hence, they do not provide a straightforward measure for comparing goodness-of-fit across AMS and PDS approaches. We will add a note to the text.

3. *page 7, line 21: adding a reference to Makkonen I., 2006, Plotting Positions in Extreme Value Analysis, J Applied Meteorology and Climatology, 45, 334–340 might be helpful to the less informed reader.*

We will add this reference.

4. *page 8, line 2: The selection of the base value for the conditional performance measures, namely T , to be 10 years should be better justified and supported by*

relevant references (i.e. international or national technical ordinances or standards, best practice documentation etc).

Although different return periods, ranging from 2 – 100 years (and more) have been used in engineering and storm water management and no common standard about recommended return periods seems to exist, return periods of at least 5 – 10 years are often considered as a lower threshold in storm infrastructure design (e.g. GRCA, 2014; EPA, 2014). Hence, such a level appears well suited to separate expected occurrence (i.e., non-extremes) from extreme events. We will add this information to section 2.5.

5. *page 8, line 13: Since “synoptic” has a reserved meaning in meteorology you might wish to replace it with “combined plotting” or any other suitable term throughout the manuscript.*

We will follow this suggestion and replace “synoptic” by “combined plotting”.

6. *page 8, lines 16–17: Please rephrase/simplify the first sentence of section 3.1.*

We will rephrase this sentence.

7. *page 13, line 7: From this point on the reader has to remember that GP refers to PDS and GEV refers to AMS. For the sake of clarity, it might be advisable to replace “GP” with “GP/PDS” and “GEV” with “GEV/AMS” in the remainder of the text.*

We will replace “AMS” and “GP” with “AMS/GEV” and “PDS/GP” in sections 4 and 5 as proposed.

8. *page 13, lines 8–9: “(indicated by negative deviations)” only 2 out of 4 diagrams in Fig 5 show negative values at low return periods.*

Thanks for pointing this out; we will modify the text accordingly.

9. *page 13, line 9: “this behaviour changes in the opposite for higher returns periods” – If I am reading fig. 5 correctly, this is actually true only for precipitation.*

[Printer-friendly version](#)

[Discussion paper](#)



This would also have an effect on the text of the Discussion (p15, l13) and Conclusions (p20, l23) sections.

We will rephrase the respective sections accordingly.

10. *page 13, line 12: “(ie underestimation of negative magnitude)” is not very clear – consider rephrasing as “(ie more negative values)”*

We will rephrase the section containing this sentence.

11. *page 19, line 32: “as well as the number of breaks set within this range” I am not quite certain about the meaning of this phrase. Could you please clarify?*

Both methods employ a sequence of thresholds that are generated based on a specified range and resolution of values. We will revise the entire paragraph (also with reference to the comment of Reviewer #2) to improve readability.

1.2 Technical Comments

All technical comments have been implemented as proposed by the reviewer.

1.3 References

- GRCA: Technical and engineering guidelines for stormwater management submissions, Ganaraska Region Conservation Authority, Port Hope, Ontario, available at: http://www.grca.on.ca/Guidelines_for_swm_submissions-_FINAL.pdf (Accessed 24 February 2017), 2014.
- EPA: Addressing green infrastructure design challenges in the Pittsburgh region – Abundant and frequent rainfall, United States Environmental Protection Agency, Pittsburgh, Pennsylvania, available at: <http://www.3riverswetweather.org/sites/default/files/Rainfall%20white%20paper.pdf> (Accessed 24 February 2017), 2014.

Printer-friendly version

Discussion paper



- Stedinger, J. R., Vogel, R. M. and Foufoula-Georgiou, E.: Frequency analysis of extreme events, Chapter 18 in Handbook of Hydrology, edited by DR Maidment, McGraw-Hill., 1993.

2 Response to Reviewer 2

We also would like to thank Dan Rosbjerg for his useful feedback. Our response is given below, with referee comments in italics, and our responses in standard format.

1. *Only one specific method for selecting an appropriate threshold for POT events has been applied. This choice might be crucial for the results and the conclusions, and it is not verified that the choice is optimal, although it is argued that some graphical criteria have been fulfilled. Another choice might lead to somewhat different conclusions.*

We agree that different thresholds might lead to somewhat different conclusions. However, appropriate threshold choice is one of the most discussed issues related to the threshold excess approach (e.g. Scarrot and MacDonald, 2012). We have discussed this issue in the discussion section “Secondly, threshold selection in the threshold excess method is a legitimate subject for debate...”

In this study, 100 time series (i.e. 25 stations with 4 meteorological indicators each) have been analyzed. In order to be able to perform a standardized and reproducible threshold selection for this large number of time series, we decided to use some sort of supervised automated threshold selection method.

In this respect we have tested several automated threshold selection methods, to be precise ATSM (automated threshold selection method) by Thompson et al. (2009) and MTM (multiple threshold selection method) by Deidda (2010). However, both methods yielded dissatisfying and inconsistent thresholds. Threshold values of similarly distributed time series obtained by ATSM varied considerably,

[Printer-friendly version](#)

[Discussion paper](#)



and parameter estimates of MTM were depending on range and resolution of the thresholds considered. While certain patterns of convergence were found based on sensitivity analysis, we argue that these procedures somehow replace the threshold selection problem with that of selecting an appropriate range and an appropriate number of breaks (c.f. response #11 to reviewer 1).

Having tested several options, the square-root-rule criterion by Ferreira et al. (2003) – which has been used in various other studies as well (Scarrot and MacDonald, 2012) – has been employed. The results have been double-checked by means of diagnostic plots for threshold selection (mean residual life plot, parameter stability plots), which are the sole basis for threshold selection in many other studies (c.f. Coles, 2001; Della-Marta et al., 2009; Scarrott and MacDonald, 2012). Results show that the thresholds derived in this way provide reasonable results (c.f. also Figure 8). We will revise and enhance the discussion section accordingly (also, with ref. to Reviewer 1) in order to clarify the issues pointed out by the reviewer.

- The assessments are based on conditional root-mean-square deviation and conditional mean absolute error as metrics. With the condition applied ($T > 10$ yr) the number of observations available for calculation of the metrics is drastically reduced. For example, if the AMS sample is covering 30 yr, only the three largest observations are applicable for calculating the metrics; in a 50 yr sample only the five largest observations can be used. Taking into account that the variance of the order statistics is strongly increasing towards the upper end of the ordered sample, it is evident that the metrics become highly uncertain.*

Note that we are not solely analyzing conditional errors for the desired extremes ($T > 10$ yr), but also the overall G.O.F. We think this specific assessment of the desired extremes is important, as the overall G.O.F is mainly representing the non-extreme part which is usually of little relevance. However, we agree that the proposed metrics are of limited robustness, especially if time series are short and

[Printer-friendly version](#)[Discussion paper](#)

the condition is selected for high return periods.

Depending on the length of the time series available, the value for x should be chosen accordingly. In our study, most of our time series date back to the period between the world wars, or even further back as early as 1895. Choosing $T > 10$ yr seems feasible in these cases.

We will add this point to the discussion.

3. *For assessment of empirical probabilities in the ordered sample the Weibull plotting position has been selected. While the choice of plotting position formula in many cases is of minor importance, it might be influential in the present case with overly weight on the upper order statistics. If F indicates a chosen probability distribution, and y_m is the m 'th order statistic in a sample of size N , then the Weibull plotting position stems from the fact that $E[F(y_m)] = m/(N + 1)$. However, with M denoting the median operator, we have $F^{-1}(m/(N + 1)) < M[y_m] < E[y_m]$. Thus $F^{-1}(m/(N + 1))$ is relatively close to the modal value of y_m (where this exists), but far from being unbiased. A more balanced and consistent choice of plotting position would be the median plotting position as, independently of F , we have $M[F(y_m)] = F(M[y_m]) \approx (m - 0.3)/(N + 0.4)$.*

The choice of a plotting position (PP) was mainly important when distribution parameters were estimated graphically from a probability paper. In this paper, according to common standard, parameters are estimated using analytical equations (e.g. based on L-moments method or maximum likelihood method) which do not depend on the choice of plotting positions. The choice of a specific plotting position is therefore of minor relevance for our paper.

Only the conditional performance measure depends, to some extent, on the chosen PP, as it uses a quantile estimate related to $T = 10$ years to select extreme values. To assess the sensitivity of the measure to the choice of the PP, we computed G.O.F. results with two alternative measures (i.e. based on Beard (median) and Gringorton plotting position) for the GEV case. We found that differences in

[Printer-friendly version](#)

[Discussion paper](#)



CRMSE₁₀ and CMAE₁₀ values are only minor. As far as Weibull PP and median PP are concerned, mean absolute deviations in CRMSE₁₀ values are around 0.09 – 0.17 °C for the different temperature indices and 1.15 mm for precipitation. Summary statistics of absolute CRMSE₁₀ deviations between Weibull PP and median PP (including parameter estimation based on both MLE and LMOM for each of the four meteorological indicators) are presented below:

```
> sapply(delta_abs_crmse, summary)
      dt      precip      tmax      tmin
Min.  0.008714  0.08059  0.0005396  0.02376
1st Qu. 0.060140  1.12000  0.0513100  0.13130
Median  0.084670  1.69500  0.0863700  0.15180
Mean    0.093220  1.68000  0.0960000  0.16620
3rd Qu. 0.100400  2.15100  0.1263000  0.17920
Max.    0.218600  3.69600  0.2328000  0.35560
```

```
> sapply(delta_abs_cmae, summary)
      dt      precip      tmax      tmin
Min.  0.00146  0.01179  0.002527  0.0009262
1st Qu. 0.03299  0.53900  0.033770  0.0968800
Median  0.06176  1.16600  0.060990  0.1352000
Mean    0.07185  1.22500  0.074560  0.1432000
3rd Qu. 0.07904  1.67300  0.086700  0.1693000
Max.    0.20000  2.96400  0.218500  0.3472000
```

When using median PP instead of Weibull PP, results in terms of best fitting estimation method change in 16% of all cases. However, in these cases, effects on results in terms of return level estimates are only minor, since – except for one case – changes occurred in cases where both estimation methods yield very similar parameter estimates. Results of the differences in return levels based on

[Printer-friendly version](#)[Discussion paper](#)

Beard and Weibull PP are presented below:

```
# return levels w/ Beard PP - return levels w/ Weibull PP
```

```
> delta_rl
```

```
$dt
```

	r110	r120	r150	r1100
1	-0.09	-0.15	-0.21	-0.26
2	-0.03	-0.02	0.01	0.03
3	0.02	-0.01	-0.07	-0.16
4	-0.04	-0.04	-0.03	-0.01

```
$precip
```

	r110	r120	r150	r1100
1	-0.44	-2.12	-5.97	-10.47
2	-0.45	-0.73	-1.16	-1.52
3	0.09	-0.39	-1.46	-2.64
4	-0.04	-0.05	-0.14	-0.26
5	-0.67	-0.80	-0.91	-0.95

```
$tmax
```

	r110	r120	r150	r1100
1	-0.12	-0.21	-0.35	-0.46

```
$tmin
```

	r110	r120	r150	r1100
1	0.22	0.50	0.92	1.23
2	-0.07	-0.04	0.05	0.15
3	0.34	0.73	1.25	1.61
4	0.33	0.51	0.76	0.94
5	0.13	0.57	1.26	1.85

Additional plots showing a graphical comparison between Weibull, Beard and Gringorten PP for all four parameters and for all 25 stations each are attached in the supplement.

In addition, our decision to use Weibull PP is based on the fact that we wanted to find a common ground regarding PP. Weibull PP is the one most commonly used in extreme value analysis and has been applied in most reference works in this area (e.g. Coles, 2001). We basically followed the argumentation of Lasse Makkonen (2005, 2008, 2013), who argues that the Weibull plotting position is the most suitable plotting position, independent of the underlying distribution $f(x)$. For further information, see the following publications and the cited references (also including opposing views) therein:

- Makkonen, L. (2005): Plotting Positions in Extreme Value Analysis. *Journal of Applied Meteorology and Climatology*, 45: 334–340. doi:10.1175/JAM2349.1. Available at: <http://journals.ametsoc.org/doi/pdf/10.1175/JAM2349.1>.
 - Makkonen, L. (2008): Bringing Closure to the Plotting Position Controversy. *Communications in Statistics – Theory and Methods*, 37: 460–467. doi:10.1080/03610920701653094.
 - Makkonen, L.; Pajari, M. & Tikanmäki M. (2013): Closure to “Problems in the extreme value analysis” (*Struct. Safety* 2008:30:405–419). *Structural Safety*, 40: 65–67. doi:10.1016/j.strusafe.2012.09.007.
4. *There is a basic difference between calculation of the return periods in AMS and POT, which is important for $T < 10$ yr. For example, a 2 yr POT event corresponds to approximately a 2.54 yr AMS event, and a 5 yr POT event to a 5.52 AMS event. It is not evident how the difference between POT and AMS*

Printer-friendly version

Discussion paper



return periods has been handled.

Thanks for pointing this out; it is perfectly right that AMS return periods (T) and POT return periods (T_*) are not the same, they are rather related in the form of

$$\frac{1}{T} = 1 - e^{-\frac{1}{T_*}}$$

This inequality was considered by converting AMS return periods to PDS return periods (in order to avoid underestimating the probability of occurrence). However, it turned out that this has not been done in Fig. 5. We have clarified this issue in the methodology section and made corrections to Fig. 5. References to Langbein (1949), Rosbjerg (1977) and Madsen et al. (1997) have been added.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-373/nhess-2016-373-AC1-supplement.zip>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-373, 2016.

[Printer-friendly version](#)

[Discussion paper](#)

