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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Censored rainfall modelling for estimation of fine–scale extreme" *by* David Cross et al.

Anonymous Referee #1

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General comments

The aim of this paper is to improve the ability of mechanistic rainfall models extremes to reproduce rainfall extremes. This is achieved by fitting these models to the amounts by which rainfall totals exceed a certain threshold level, or *censor*. Totals below this level are censored: their value is taken to be zero when the models are fitted. Applying a threshold to data is a standard approach in extreme value modelling. It's use here is a novel and interesting idea. If modelling rainfall extremes is the primary goal then using the censor to reduce the influence of small totals is sensible.

Of course, a key issue is the choice of censor: a low censor may not achieve the desired objective but as the censor is raised the precision of estimation reduces and,

in the current context, may exacerbate the parameter identifiability problems to which the fitting of these models are prone. This is analogous to the choice of threshold in an extreme value analysis and therefore it is unsurprising that in Figure 13 they consider informing this choice using a graphical approach that is common in extreme value modelling. It may be productive to explore other methods proposed in the extreme value threshold selection literature.

Overall I am positive about this paper. My main criticism is that better reproduction of rainfall extremes is achieved by tuning various things: censor; model parameterisation; fitting properties; perhaps even the model itself, in order to achieve this objective. There is further work to be done to provide methodology to make these choices.

Specific comments

page 11, line 288. This isn't quite correct. These weights are not optimal. However, in practice they are close to being optimal and are easier to estimate than the weights that are optimal. On that note: how are these weights estimated?

page 12, lines 305-308. This isn't quite correct. The sampling distribution of the GMM estimators is approximated by a MVN distribution, i.e. there is an approximation involved and the result is for the rule that is used to calculate the estimates, rather than the estimates themselves. Line 306: Hessian of what? Lines 307-308. This sentence isn't clear. Presumably the point is that the calculation of confidence intervals can fail in some cases. Perhaps it would be sufficient to reverse the ordering of the sentence to make the causation clearer.

page 13, line 345. If you are interested in the 1000 year return level why not simulate 100 realisations of 1000 years duration?

pages 14-16, Figures 4-6. There seems to be a slight upward curvature in the lines

based on the 10000 year simulation. In the context of an extreme value analysis this is consistent with the shape parameter discussed on page 4 (line 111) being positive. This might be worth a brief comment. Is there any work that examines how the extreme value properties of this type of model relate to the model parameters and therefore provide a link to the theoretical basis that underpins extrapolation from extreme value models?

page 17, lines 385-386. I disagree. I suppose that it depends what you mean by "poorly identified". However, it is to be expected that as the censor is increased uncertainty about model parameters increases. If we think that we need a larger censor, because otherwise there is systematic underestimation of extreme rainfall totals, then we need to accept higher levels of parameter uncertainty.

page 18, Section 6.2. Is this level of tinkering with the choice of censor justified? Having a different censor for different levels of data aggregation feels like cherry-picking. Also, in the previous section an argument was made against a censor of 0.6mm for Atherstone but now it is being used.

page 19, Figure 8 caption (and elsewhere). "optimal censors" seems like a bold claim given the difficultly of choosing the censor.

page 28, line 516. The independence criterion *isn't* a requirement in extreme value modelling. See Fawcett and Walshaw (2012) Estimating return levels from serially dependent extremes. Environmetrics 23(3), 272-283.

Technical corrections

page 3, line 81. At this point, or perhaps even in the abstract, it is worth explaining briefly the nature of the censoring. At the moment we need to wait until page 7 for this.

page 4, line 118. "behavioural parameterizations". Given that you use this term later it

would be worth explaining (somewhere) what this means in the context of the current paper.

page 6, line 160. "Lower variability" may be better than "less variance".

page 10, line 262. Presumably the reason for the missing data in 1974-75 was political rather than environmental. It might be worth noting that the fact that the data are missing is not expected to be informative about rainfall totals.

page 12, line 313. "ldots extreme values continued to be underestimated . . . " might be better.

pages 14-16, Figures 4-6. The plots would be clearer if the scale on the lower horizontal axis was return level in years. The AEP on the upper horizontal axis would then be unnecessary. The scale of the Gumbel reduced variate adds no information in itself. These plots are quite crowded and

page 17, line 358. I'm not sure that I would use "confidence intervals" here. Perhaps "simulation bands"? ... and say explicitly what this means, i.e. how the lines in the plot are calculated.

page 17, line 379. I'm not sure what this sentence means. Are we supposed to be looking at Figure 7 for evidence of this?

pages 20-21, Sections 6.2.1, 6.2.2 and 6.2.3. I don't see the point of including these sections. Section 6.2.1 shows exactly what we expect: by excluding properties that are difficult to reproduce we are able to reproduce well the properties that are not excluded. The comparison in Section 6.2.2 is unsatisfactory because we cannot compare like with like, owing to the truncation of the data but not the model. Section 6.2.3 just shows that there are clear local minima in the objective function but we can't expect to search too far in the search for confidence limits.

page 27, Figure 14. I don't think that these figures add much to the statistics concerning the proportions of totals lying below the censors, with the possible exception of the

visualisation of the resolution of the Atherstone data.

page 28, line 514. The rule to try to create independent peaks needs to be given earlier: before the concept appears in Table 3.

pages 28-30. Do we need both “Further discussion” and “Conclusions”?

page 31, line 591. Is the first inequality sign the wrong way round?

page 32, Figure A.2. Below and to the right of the plot is says that $1/L$ has an exponential distribution, which, according to the description of the models on page 8, isn't true.

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