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Discussions

Interactive comment on "Have trends changed over time? A study of UK peak flow data and sensitivity to observation period" *by* Adam Griffin et al.

Anonymous Referee #1

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The paper "Have trends changed over time? A study of UK peak flow data and sensitivity to observation period" by Griffin, Vesuviano and Stewart presents an investigation of changes in the parameter estimates of the GLO distribution through time for British network of near natural catchments.

The paper introduces some interesting approaches to quantify and visualise changes trough time. It reads well, is well organised and has suitable tables, figures and references. I feel there is maybe not a very clear focus in the results presented by the authors: the study is interesting and well executed, but there isn't a final clear concept that emerges as the final take home message in the paper other than "it's compli-

cated" (which is a good take home message but one that was already known before this paper). From a more technical point of view I think the authors lack a discussion of uncertainties in the estimation (see more on this below) and of the possible correlation/interaction between the parameter estimates, either for the stationary of non-stationary case. As they also state in the end of the paper the final estimates obtained for the design events of interest depend on the estimated values of all parameters, so that even if the shape parameter is estimated to be closer to 0 (rather than negative) the final estimates of the 50-year are still larger, because of the changes in the location and scale parameter. This can be difficult to understand and accommodate, and I think it would deserve a larger exploration and discussion in the paper.

The other point I think the authors need to reflect on is the choice of link functions used to model the distribution parameters: I believe that linear trends might not be the most suitable ones for this application (again see more on this below).

Some other specific comments

Page 3 - line 4: an initial version *of the benchmark network* (to clarify it is not the NRFA the authors are talking about).

Page 3 - line 9: from the writing I understand the data used is the instantaneous peak flow data - not the daily. Maybe this could be specified more clearly since the proportion of daily data missing is mentioned above.

Page 3 - line 13-14: I imagine this is because this is the area of the country with the most urbanisation, but this could be spelled out for those not familiar with British geography.

Page 3 - line 26: this is a very good point, often overlooked in practice. In the FEH estimation procedure though xi and QMED are constrained to be the same I recall - but I gather the authors do not attempt to do that in this paper.

In Figure 3 in the extended window there seems to be some correlation between the functional shapes of the scale and the shape parameters. From experience of fitting extreme value distributions to at-site data I know that especially when large events are added to the analysis dramatic changes in the shape parameter are sometimes also connected to fairly large reductions in the scale parameter: this makes sense as some of the variation in the data is now explained by a higher skewness instead of a large variability. I wonder if the authors could comment on this and if they have noticed a similar phenomenon in their moving averages.

Section 3.2.1/Figure 3 - since you use the greek letters to discuss the values of the parameters I would add them to the plots so it is easier for the reader to connect the text and the figure. Alternatively you could use the words location. scale, shape in the text.

Page 8 - line 7: would it be the case that opposite signs could be seen for the 2-years and 5-years events as in the case study presented in Figure 3?

Section 3.3.1: are the three linear models fitted separately or is this one unique linear model fitted to all the AMAX (in which case I am impressed things converge with no problems). Also, maximum likelihood is used in the estimation changing the estimation procedure, maybe using L-moments for trends as in Jones (2013) could have been relevant in this context. It is a bit odd that two estimation approaches are used to find trends, ML could have been easily employed to do the moving averages as well (probably leading to very similar results). On the other hand using ML for the moving average would have possibly allowed the estimation of some form of uncertainty, to assess whether the apparent shifts in the parameter values are not contained within the sampling variability. In general uncertainty/variability in the estimation is not mentioned at all in the paper, while it could well be that the changes in the point estimated identified by the authors are swamped by the variability of the estimation.

Page 8: line 22. The authors discuss some issues connected to the fact that the linear

form imposed to the shape parameter means one should be careful when extrapolating outside the time range used in the regression. Note that this is also technically true for the scale parameter as well, which should be positive. Later in the paper the authors point out that that the linear form used for the shape also makes it impossible for them to calculate some of the percentage changes. I would imagine that using some form of truncated logistic regression or some other link function in the model (see the mgcv::gevlss function in R) would make fix some of these problems? I understand this would require the complete reworking of the findings - but it would seem the reasonable thing to do.

Page 9 - line 2: what is $P_Q(s)$? I see it is defined later - maybe this paragraph could be rewritten to make this clearer

Page 10, line 4: why is 0.02 an extreme negative trend? (I mean if you miss a -, and I am not clear if 0.02 would be linked to some specific large change in the design event).

Page 10, figure 5, right panel: red and green are the definition of things colour blind people can not distinguish, maybe use purple and yellow?

Page 11 - line 16: "which is quite different" in what sense? Maybe useful to give the range of the values (i.e. what is the maximum of it) or to comment more on what you mean by quite different. I also think this has something to do with the fact the location and scale parameters are also estimated to span quite different values in the non-stationary model than in the stationary model. Finally as mentioned before: is this difference significant?

Page 14 - section 3.5: I am not entirely clear on what is being described here. Why does the assumption that the non-stationary parameters are valid for more than 50 years only hold for 66 stations? Are these stations with more than 50 years or stations for which the $\kappa(t)$ function stays within the required bounds? Do I understand correctly that you are using L=50 and applying the formulae shown in Section 3.3.2.

References

D. A. Jones; On an extension of the L-moment approach to modelling distributions which include trend. Hydrology Research 1 August 2013; 44 (4): 571–582. doi:

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