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Hydrology and Earth System Sciences Discussions

Interactive comment on "Influences on flood frequency distributions in Irish river catchments" *by* S. Ahilan et al.

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Dear Reviewer,

The authors would like to thank you for your detailed comments regarding manuscript HESS-2011-71 entitled "Influences on flood frequency distributions in Irish river catchments" for publication in the HESS Journal.

All comments, both general and specific together with technical corrections have been considered and addressed as follows:

Reviewer 1 General Comments

In the "result" section, the place given to previous work is far too important: the authors very shortly describe their own results, and lengthily discuss previous findings. This is very confusing for the reader, since it becomes difficult to know what are the new insights brought by this paper compared to previous work. I have to say that after reading this section, I had the feeling that most conclusions were motivated by results obtained in previous work or by expectations, rather than by the results presented in this particular study. Consequently, I would advise to split this "result" section in two parts: a "result" section solely focusing on the analyses carried out by the authors, and a "discussion" section linking those analyses with previous work, and proposing physical explanations to the obtained results.

The structure of the revised manuscript has been amended to include separate Results and Discussion sections.

The description of methods could be very significantly shortened: the authors are presenting in full detail standard statistical techniques that could easily be found in textbooks.

The discussion of the methods has been significantly shortened in the revised manuscript.

A discussion on the problem of model choice is missing. In particular, the authors seem to take for granted that the flood distribution is a GEV, and that the issue is to choose its type. The fact that type II EV distributions might be preferable is motivated by the fact that distinct hydrogeological or hydraulics processes are activated for small or large floods. But to me, this rather suggests that the flood distribution might be a mixture of several distributions, which can also explain the convexity of empirical quantile curves for some stations. Conversely, it is perfectly possible to observe type II or III EV distributions in the absence of any significant influence of hydrogeology / floodplains etc. I'm not suggesting including other distributions to this study, but at least some discussion would be of value.

A significant body of work has recently been completed that investigated suitable distributions for AM data in Irish catchments. This was undertaken as part of the Irish National Flood Studies Update (FSU) that utilised the additional years (almost 40) of hydrometric data that has become available since the publication of the Flood Studies Report (FSR, 1975) that provided guidance on flood estimation in Ireland and the UK. This work of the FSU reaffirmed the recommendation of the FSR that GEV-type 1 are best suited to Irish AM data. On this basis, the starting point of this study was that GEV-type 1 distributions could be assumed and concluded that GEV distributions could be assumed and other distributions were therefore not investigated.

Similarly, a discussion on uncertainties is missing. The whole study is based on point-estimates of parameters, but the purpose of flood frequency analysis is not only to provide a "best estimate" of the quantile curve, but also to quantify related uncertainties. In particular, the uncertainty in the shape parameter is in general very large, so that it could correspond to any of the three EV types. In particular, when a type I distribution cannot be rejected, it doesn't mean that the distribution is of type I, and there is therefore a risk in using predictions based on a type I distribution just because one could not reject this hypothesis. Consequently, rather than just choosing one EV type, maybe one should rather quantify this uncertainty and provide predictions that account for it? These issues should be discussed in my opinion.

This comment has also been referred to in the 'specific comments' and has been addressed through Monte Carlo tests for the revised manuscript. This is explained in the author responses to the specific comments.

Reviewer 1 Specific Comments

RC1: Would it be possible to recall, from the very beginning of the paper, the correspondence Type I –Gumbel, Type II – Frechet-like (or fat tail) and Type III – Weibull-like (or light tail). I'm personally always getting confused between the three types.

AC1: To develop appropriate probabilistic models and assess the risks caused by ex-

treme floods, engineers frequently use the Generalised Extreme Value (GEV) distribution. The GEV distribution is a flexible three parameter model that encompasses the type I - Gumbel, type II – Frechet and type III – Weibull extreme value distribution. The values of the shape parameter determines the extreme value type I, II and III distributions. Specifically, the three cases $k = 0$, $k > 0$ and $k < 0$ correspond to the Gumbel, Frechet, and Weibull distributions. The revised manuscript now includes a statement to this effect.

RC2 p. 3307 line 8-11: Please provide a reference for this statement. I have to say I'm not keen of such general statements, unless accompanied by more details (e.g. for which distribution? For which target level of precision/ accuracy? Etc.).

AC2: The most commonly used procedure for estimating a design flood in Ireland today is to follow the methods set out in the Flood Studies Report (FSR) (NERC, 1975). The FSR recommended that the return period of the estimated flow in a single site analysis should not exceed the length in years of the available flow record by more than a factor of two (at least 50 yr of data should be used to estimate the 100-year flood). This recommendation applies to a GEV type I distribution. The authors are in agreement with the reviewer in that sample size is a critical parameter in the standard error of the estimated design flow and that this error decreases with increasing sample size. However, sample properties are also significant parameters in computed standard errors and consequently, sample size alone is not a sufficient parameter for us to comment on the accuracy/ precision obtainable for a flow record of given length.

RC3 page 3307, line 17-20: these points are not really convincing: All three types of GEV have closed-form expressions for the pdf, the cdf and useful moments – they just depend on an extra parameter for types II/III.

AC3 Line 17 – 20 of p 3307 have been amended in the revised manuscript to correct this point.

RC4 p. 3309 line 5-10: There is a kind of logical fallacy here: FAI are used to explain the type of GEV distribution, yet to compute a FAI, a flood quantile is required, which requires... choosing a type of GEV! Please comment on this and explain why this may or may not be problematic.

AC4 The authors acknowledge that the development of FAIs is based on flood quantiles based on a regional flood frequency analysis that assumes a GEV distribution. The main source of error in the FAI will therefore be associated with errors in the flood quantile (QT) arising from the inherent assumption of regional homogeneity in the analyses. Given the usual shape of rating curves in Irish catchments, water levels are not particularly sensitive to changing flow quantiles and therefore, the FAI is not a sensitive index in this regard.

In any case, and since submission of the original manuscript and in the course of the additional work that was necessary to fully address the review comments, the use of the descriptor that defines the alluvium area in the catchment (ALLUV), was considered to better reflect the extent of floodplain inundation for the purposes of the study. This therefore, as opposed to FAIs is now being used.

RC5 p. 3310 line 14-25: would it be possible to include a few illustrative hydrographs (with corresponding rainfall) from typical and contrasted catchments?

AC5 Attempts were made to obtain illustrative hydrographs with corresponding rainfall. However, at the time of writing, these were unavailable.

RC6 p. 3313 line 17-21: this sentence already appears earlier in the paper, please delete or reword.

AC6 This sentence has been removed in the revised manuscript.

RC7 Section 3.1. this can be shortened – such summary statistics will be well-known to the reader interested in this paper.

AC7 Section 3.1 has been shortened in the revised manuscript.

RC8 p. 3315 line 4: the term “statistical power” is a bit ambiguous since it has a very precise meaning in the context of statistical testing (probability of correctly rejecting the null hypothesis), yet only approach 3 is a test. Please reword.

AC8 The term “statistical power” has been changed to “statistical competence” in the revised manuscript.

RC9 Section 3.2.1: this can be significantly shortened (virtually removed), all those formula & definitions are standard and can be found in textbooks.

AC9 Section 3.2.1 has been significantly shortened in the revised manuscript to exclude, as suggested, formulae and standard definitions found in standard texts.

RC10 P 3316 line 16-17: I would disagree, it might also be due to the fact that the sample size is too small to see a type II/III behavior.

AC10 The authors agree with the review comment and the wording in the revised text has been amended to make the relevant statement less definitive.

RC11 p. 3317 line 20-23: a bit unclear (what is meant by “a distribution occupying a greater proportion of the measured data”?).

AC11 The wording of this statement has been changed to: “The distribution that by visual inspection, matches closely to the area of scatter of measured data in the moment or L-moment ratio diagram is expected to be a suitable candidate distribution to model the measured data.”

RC12 p. 3317 line 25 and 27: even in the absence of bias (in the statistical sense), there would still be sampling uncertainty and therefore scatter in the observed moment ratios. Please reword.

AC12 Line 25 and 27 of p 3317 have been reworded as requested.

RC13 p. 3318 line 2: Wouldn’t it be more logical to use the same number of stations and the same sample sizes than in the analyzed dataset, given the objective behind this Monte-Carlo simulation?

AC13 As suggested by the reviewer, the Monte Carlo simulation was updated to match the 172 stations that were analysed in this study. These revised results are in Figure 5. It was also suggested that the Monte Carlo simulation should reflect the sample size of the analysed data set. However, the sample size of the analysed data set varied from 7 to 69 years and therefore it is not practical to undertake Monte Carlo simulations for each sample. On this basis, a sample size corresponding to 50 yr flow records was adopted.

However, in the additional Monte Carlo tests (undertaken to address the review comments), each realisation used a set of 143 stations with the length of the generated samples for each being the same as the length of the data available for the station.

RC14 p. 3318 line 7: it could also be from any other (non-GEV) distribution!

AC14 From the Flood Studies Report (NERC, 1975) and the Irish Flood Studies Update, the GEV distribution was determined to be the most suitable distribution for Irish annual maxima data series. Consequently, distributions other than those in the GEV family were not considered in this study.

RC15 p. 3319 line 1-3: this statement is a bit too strong – a negative estimated value of k is not sufficient to exclude a type III distribution, due to possible estimation errors.

AC15 This statement has been made less strong and the role of errors in parameter estimation for identifying particular distributions has been noted.

RC16 P 3319 line 8-16: can be shortened, even removed.

AC16 P 3319 line 8-16 contains information that will be known to readers of this paper and has been removed in the revised manuscript.

RC17 p. 3320 line 1-4: this sentence already appears earlier, it can be deleted.

AC17 P. 3320 line 1-4 has been deleted in the revised manuscript.

RC18 p. 3320 line 16: How can you compute a BFI in an ungauged catchment? I guess this variable has been mapped or regionalized? Please specify.

AC18 BFI is a mapped index for ungauged catchments that is based on soil characteristics. This clarification has been included in the revised manuscript.

RC19 P 3322 line 15-30: can be shortened, even removed.

AC19 P 3322 line 15-30 has been significantly shortened in the revised manuscript.

RC20 p. 3324 line 4-7: this distinction between OPW and EPA gauges is not really interesting for an international readership.

AC20 No reference to either OPW or EPA gauges is made in the revised manuscript.

RC21 p. 3324 line 8-10: these numbers should be related to the error level of the test – if all distributions were of type I, would it be surprising to observe 11 decisions "type II" out of 143 stations, given the error level? This point should be at least mentioned and discussed. Specific methods also exist to handle this issue, see e.g. "field significance evaluation" [Livezey and Chen, 1983; Douglas et al., 2000] and "false detection rate" [Benjamini and Hochberg, 1995; Wilks, 2006; Renard et al., 2008]. The authors might wish to implement such methods.

RC21 The significance of the number of GEV distributions was statistically investigated by Monte Carlo testing in light of this review comment. This testing showed the number of GEV type II distributions (8 in total out of 143 stations) to be not statistically significant. The number of GEV type 3 distributions (44 in total) was however, significant. For a two-tailed test with a 95% confidence limit 4.3 % of the tests could be expected to be identified as GEV type II by chance and 1.9% could be expected to be identified as GEV type III by chance.

Regarding the GEV type II distributions, separate Monte Carlo tests were undertaken to determine how likely it was to get a result with 8 stations clustered together if their locations were random. In each test, 8 gauges were randomly placed at locations in a

2-dimensional lattice with 140 nodes, corresponding closely to the number of gauging stations investigated. The percentage of simulations in which the random placement produced a cluster of all eight stations in any configuration or location was calculated as less than 0.1 %. The hypothesis that the spatial location of GEV type II stations is random was rejected at the 95% confidence level and the spatial clustering of 8 stations is extremely unlikely to have occurred by chance. So although the number of GEV type II distributions is not significant, their spatial pattern is. In contrast, the number of GEV type 3 distributions is, by itself, significant, regardless of their spatial distribution. In both tests, the results do not change appreciably if the number of simulations is increased so 100,000 simulations produced stable results.

The revised manuscript includes details and results of this Monte Carlo testing.

RC22 From p. 3324 line 11 to page 3326 line 5: can be significantly shortened, virtually removed, this is mostly textbook material.

AC22 Text and equations from p. 3324 line 11 to p. 3326 line 5 in the revised manuscript have been removed.

RC23 p. 3326 lines 6-11: some words are a bit too strong in the context of statistical testing, e.g. "valid" (there will be wrong decisions due to the error level!), "underestimated/overestimated" (the truth is unknown!). Please reword.

AC23 The revised manuscript has been reworded as suggested.

RC24 p. 3326 line 19-24. I find this clustering quite subjective and not really convincing based on the sole information given in the maps – there are green dots spread out pretty much everywhere in the maps. Unless the authors use information/knowledge that do not appear on the maps, but this should be further explained in this case.

AC24 The AC 21 aside, the authors agree with the reviewer comments regarding the subjectivity of the 'clusters' of GEV type III distributions presented in the original manuscript. In hindsight, the presentation of these distributions as distinct clusters

may not have been the most appropriate and does not add anything to the paper. One objective of the paper focusses on exploring the role of hydraulic influences that may be associated with GEV distributions. In this regard, the clustering of the GEV type 3 distributions is of secondary importance as the paper correlates these distributions to catchment characteristics that are conducive to floodplain inundation.

The same however cannot be said for the GEV type II distributions. These do appear as a single cluster and it was this clustering that allowed us to develop the hypothesis that the karst terrain is of influence in this distinct cluster.

The wording the revised manuscript has been amended to reflect the looser 'clustering' of the type III distributions.

RC25 p. 3327 line 2: It's quite difficult to see any FAI details in figure 7c, I'm not sure this GIS-based representation is well suited to this variable. As a consequence, it is difficult for the reader to be convinced by the impact of FAI from the information provided by this sole figure.

AC25 In completing the additional analyses for addressing reviewer comments and preparing this revised manuscript, the authors used the catchment descriptor, ALLUV , as an indicator for the extent of floodplain inundation upstream of the gauging stations that were investigated. A relatively stronger relationship between GEV type III distributions and ALLUV (the area over which alluvium material deposits on floodplains) compared to the observed between GEV type III3 distributions and FAIs (as used in the original manuscript) was observed in the revised analysis.

Therefore, and for simplicity, all references to FAIs have been removed in the revised manuscript.

RC26 p. 3327 line 3-14: should be moved to a discussion section – those are not results from this particular study.

AC26 The content of lines 3 – 14 of p 3327 is consistent with information contained in the ‘Background’ section of this paper. These lines have been deleted in Section 4.3 of the revised manuscript and the Background section (Section 2.2) has been amended slightly to include and additional sentence relevant to the impacts of floodplain attenuation in producing shifts from GEV type I to GEV type III distributions.

RC27 p. 3327 lines 15-16: As previously, this clustering is not convincing: why are two nearby sites excluded from the cluster? How about the three sites on the east coast, where there is apparently no karst? This needs to be justified and discussed in far more depth.

AC27 In addition to the issues of clustering already addressed in AC21 and AC24, the further statistical analysis undertaken for the revised manuscript revealed that two of the GEV type II distributions on the east coast are more appropriately represented by a type 1 distribution. A single GEV type II distribution still exists along the east coast and there is no clear underlying reason why this should be in the context of the available data that was analysed. This suggests that an additional influence not investigated is important at this single location.

RC28 From p. 3327 line 17 to p. 2238 line 6: as previously, this should be moved to a discussion section – those are not results from this particular study.

AC28 The text from line 17 on p 3327 to line 6 of 2238 has been significantly shortened and made more specific to the results reported in this paper. The pertinent points in this text had been included in Section 2.1 of the manuscript.

RC29 p. 3328 line 13: “meaningless” is a rather strong word: I don’t think estimating a distribution is meaningless in karst regions, but for sure it might require more care! What is disputable is rather to blindly use a given distribution on the sole ground that it works quite well elsewhere!

AC29 The text has been revised to reflect this comment. The word ‘meaningless’ has been removed and the task of estimating distributions for karst regions is described as

‘more challenging’.

RC30 p. 3329 line 2: maybe something more “extreme” than an annual rainfall might be a better explanatory variable?

AC30 The authors are in agreement that rainfall frequency, as opposed to average annual rainfall totals, would be a more suitable variable for correlating to GEV shape parameters in the context of the study presented. However, this data is not readily available for Irish catchments and therefore, average annual rainfall, while inconclusive for determining frequency, is still used in the revised manuscript. Furthermore, given that Ireland experiences persistently wet conditions throughout the year, correlations between average annual rainfalls and shape factors may be expected to reflect similar trends that would be observed between rainfall intensities and shape factors of GEV distributions.

RC31 p. 3329 line 6: How can the influence of rainfall frequency be deduced from figure 8? The lack of relationship with annual rainfall is not conclusive of the role of frequency. It might be more convincing to define a variable describing the rainfall frequency, and evaluate whether it is indeed linked with the shape parameter.

AC31 This is addressed in AC30 above.

RC32 p. 3329 line 16: I’m sorry but I can’t see this increase in Figure 8d.

AC32 Given the review comments, and in particular, the suggestion that two-sample testing be undertaken to assess the statistical significance of the explanatory variables in GEV type II and III distributions that were investigated in Fig. 8 (original manuscript), it was felt that Fig. 8, in light of the results of the two-sample testing, does not add significantly to the revised manuscript and has been removed.

RC33 p. 3329 line 19-22: those are others’ results and should rather be placed in a discussion section.

AC33 This has been addressed in the revised manuscript.

RC34 From p. 3329 line 28 to page 3330 line 9: same comment.

AC34 This section of text has been included in the discussion of the revised manuscript.

RC35 Page 3330 line 14: this is a different issue – in a climate change context, what is questionable is essentially to assume that the flood distribution is stationary (irrespective of the chosen distribution!).

AC35 The results presented in this study are based on the assumption that Irish AM data series are independent and identically distributed (assumption of stationarity). While the assumption of stationarity in Irish AM data series at specific locations, has been questioned, important national research undertaken as part of the Irish Flood Studies update, indicates that these trends are not widely observed and traditional methods of hydrological flood frequency analyses as used in this research, are applicable.

The text in the revised manuscript has been amended so that this point is more clearly made.

RC36 From p. 3330 line 15 to p. 3331 line 7: This discussion on CC does not really belong to a "result" section. Moreover, I think some statements are a bit speculative – for instance, p. 3331 line 1-5: what the authors describe here is a trend, and detecting a trend is not sufficient to demonstrate the impact of CC (see the distinction between detection and attribution made by the IPCC in the assessment reports).

AC36 In light of the revisions made in addressing RC32 and given that the paper is not focused on climate change signals in Irish hydrological data, line 15 to line 7 on pp 3330 and 3331 have been deleted from the revised manuscript.

RC37 p. 3332 line 5-8: this sounds a bit speculative to me.

AC37 This statement is not intended to be speculative. Rather, the reasoning for GEV type distributions being observed in areas of karst terrain is hydrologically plausible considering the finite nature of storage provided by karst.

RC38 The reference list could be broadened in my opinion: in particular, the topic of identifying the type of distribution of some extreme hydrologic data has a quite long history, which doesn't appear in this list. See e.g. El Adlouni at al. [2008] and references therein.

AC38 The main text and reference list in the revised manuscript have been amended and broadened respectively to include references associated with the topic of identifying the type of distribution of some extreme hydrologic data.

RC39 Tables A1: might possibly be removed.

AC39 Table A1 has been removed in the revised manuscript.

RC40 Tables B1, B2 and B3: Those tables are not useful for an international readership, I doubt many readers will go through those numbers. I think they should be removed. If some of this information is important in the authors' opinion, it should rather be presented as a figure.

AC40 Tables B1 to B3 have been removed from the revised manuscript.

RC41 Figure 1: this representation is not adequate, the percentages could just directly be given in the text, or alternatively, the authors should rather show the whole distribution of record lengths.

AC41 The contents of Figure 1 have been included in the main text of the revised manuscript as requested.

RC42 Figure 2: This figure is incomplete and not very useful in the present state: please include a scale, a legend, increase text size, etc.

AC42 Figure 2 has been removed in the revised manuscript.

RC43 Fig 3: please increase font size. Moreover, why not using the catchment area in the x-axis, as in left column?

AC43 Font sizes for Fig. 3 in the attached manuscript have been increased. Catchment area was used for the x-axis parameter when it was felt that a relationship between a parameter and this area existed. Skewness is sensitive to sample size and for this reason is plotted against sample size. The right column of plots in Figure 3 are included to give a general overview of the Irish AM data behaviour.

RC44 Fig 5: I'm not very familiar with moment diagrams, but I don't understand why the simulated EV1 data are not scattered around the green square representing the EV1 distribution. Please explain.

AC44 An accepted way of exploring the suitability of different probability distributions utilises dimensionless moments and moment ratio diagrams. Both ordinary moments and L-moments are shown in Fig. 5. The simulated GEV type I data are not scattered around the green square in Fig. 5(a) because of the sample size (n = 50 based on average record lengths at the sites investigated) and the number of simulations used in this study. Wiltshire (1986) examined the statistical power of this test by Monte Carlo methods using GEV type I distributed parent distributions. It was found that the power of the test increased with increasing record length (n) and an increasing number of sites. Issues with using ordinary moments have been presented in the paper and to overcome these limitations that result in data not being scattered around the green square in Fig. 5(a) as mentioned in the review comment, L-moments are used in Fig. 5(b). The simulated data on the L-moment ratio diagram shows considerable scatter around the theoretical GEV type I distribution as would be expected, and the data presented therefore highlights the need to use L-moments as opposed to ordinary moments for such studies. The inclusion of the ordinary moment ratio diagram is consistent with methods in which the statistical power is progressively increasing.

RC45 Fig 8: I think such figures are far easier to read than the previous GIS-based representation, although I understand the latter is of interest for a more detailed investigation (but a zoom is needed, which is not feasible in a article format!). Consequently, I'd encourage the authors to use this type of presentation for their results. In particular:

âĂć Indices could be computed to summarize the variables presented in the GIS (e.g. percentage of lake area, relative FAI area, etc.) and shape parameters could be plotted against these indices. âĂć I think the authors should also use sample size as a covariate, just to check that it does not spuriously influence the shape parameter values, due to sampling variability. âĂć Two-sample tests could be used to assess whether the explanatory variables have distinct distributions depending on the EVII/EVIII decision (i.e. create 2 samples (EVII stations) vs. (EVIII stations) and check whether e.g. Urban area have distinct distributions on these 2 samples). âĂć A more "continuous" version of the 2-sample test above would be to perform a multiple regression analysis based on the reduced variate Z in equation 8 to assess the impact of the various explanatory variables.

AC45 The authors are in agreement that that the GIS-based representations at the resolution which are possible in a manuscript of this type are somewhat limited. Figure 7 therefore, has been amended in the revised manuscript to show only the spatial distribution of the GEV type I, II and III distributions across the country. However, given the significance of the type 2 distributions in the western region that is underlain by karst, with a thin glacial over mantle, this figure in the revised manuscript also includes a 'zoomed snapshot' of this region.

âĂć Furthermore, indices for karst (similar to those that were plotted in GIS representations in the original manuscript), should they have been available or determined, would also be of limited use given the complexity of karst hydrology.

âĂć The influence of sample size on shape parameter values was investigated using Monte Carlo tests. This work is has been explained in AC21.

âĂć Independent two-sample t-tests were used to determine the significance levels of mean values of explanatory catchment properties on the choice of GEV type II/ III distributions and type I/ III distributions. Results of this testing indicated that channel slope, the area of floodplain alluvium deposits and the area of land under pasture within a catchment are significant properties in choosing a GEV type III distribution (confidence intervals between 80% and 95%). Chi-square contingency tests also indicated that pasture and stream slope were significant (confidence intervals $\sim$ 90%). In linking strong floodplain behavior to type 3 distributions as this paper does, these results are important. Regarding GEV type II distributions, rainfall was a significant property and in the context of the hydrological behaviour of karst terrain, high rainfall can promote type II distributions. This again is important. No links with other catchment properties were identified in this two-sample testing.

The following technical corrections were identified by Reviewer 1 p. 3318 line 20: “year” missing

p. 3320 line 14: what does FSU mean?

p. 3321 line 2: figure 3a,c,e

p. 2232 line 10-11: “concave upward” and “convex downward” sound like pleonasms, and are actually inverted! (concave means downward, convex upward).

Figure 6: Maybe a log-log scale would be better to zoom in the lower part of the curve? Moreover, the colors used for EV1 and EV3 are difficult to distinguish.

These were all addressed in the revised manuscript.

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