My thanks to Referee # 2 for comments, and also to Referee #1 as well as Francesco Serinaldi for independent comment.

Responses to Referee # 1 have already been posted, but a further comment is added here with respect to the final Referee #1 point raised:

If Y has a Weibull distribution then it is not guaranteed that g^{-1} is defined on the whole of the range of Y* which always runs up to + ∞.*

In response, the definition of the transformation *g(X)* should be extended to read:

"Define the transformation $Y_i = g(X_i)$ where $g(X)$ is a positive strictly monotonic decreasing function of X. Further, let the inverse function $g^{-1}(X)$ be defined over the same range as X."

This definition is also incorporated in response to Francesco Serinaldi's comment on the need for clear definition of *g(X)*.

Responses to Referee # 2 follow below.

I wonder why the Author did choose HESS and why He does not try to demonstrate a bit more why hydrologists or earth system scientists should be interested in His results. I think F. Serinaldi is right when he says that "proposing 'new' statistical results in hydrologic journals is not a good strategy to guarantee the quality of the scientific production, as there is a very good chance that professional statisticians with expertise in EVT do not comment on the paper".

HESS was chosen because the paper is intended for hydrological application and HESS is a well-known hydrological journal with an open review process. The latter is important because I felt it would be helpful in this instance to have an open discussion as part of the review.

I have made response previously with respect to the Serinaldi comment on "new" statistical methods. I could perhaps add that if I felt that I really had contributed some new aspect of extreme value theory then of course submission to a specialist journal such as *Extremes* would have been appropriate. However, no doubt Reviewer # 1 can confirm that the paper involves no new theory worthy of such high-level consideration. Indeed, the basic idea is just a generalisation of the concept " for positive-valued data you can estimate the probability of getting a larger value by estimating the probability of getting a smaller value of the reciprocal". The paper nonetheless does have something useful to offer for practical application in that for sufficiently large sample sizes it is pointed out that the Weibull distribution (as asymptotic distribution of smallest extremes) can be used as an alternative to the GEV as a single expression for estimating exceedance probabilities for positive-valued variables, and with no less extreme value justification. This is conditional on existence of extreme value limit distributions in any given instance (as noted by Referee #1), but these conditions apply as much to distributions of maxima as to distributions of minima. Because hydrologists will probably prefer working in terms of the original measurements rather than transformed data, I have suggested transforming the limit Weibull distribution to create new distributions for application to maxima, presented in the original scales. However, while perhaps useful for hydrological data representation, such transformations cannot be proposed as a statistical advance.

I would have expected to get some insight on how to use these new distributions in hydrology from a paper in HESS.

My thanks for the suggestion. Yes - the paper would benefit from some additional comment in this regard. In fact, the advantage of the proposed approach is that is that H distribution, and the many other possible distributions of maxima in the class as defined, are all derived from transformations of Weibull distributions with non-negative values of the location parameter. Therefore, no new formal estimation methods are required for practical application beyond those already developed for 3-parameter Weibull distributions.

Informal Weibull estimation by curve fitting could also be used along the lines suggested by Bardsley (1989. This text would be added to a revised paper together with an example application.

Personally I like the idea of non-negative extreme value distributions for the cases in which Monte Carlo simulations are needed and one does not want to generate negative flood peaks, for example.

I didn't have simulation in mind but it would be nice if the distributions found some application for simulation purposes in this way.

However I do not think that having an upper bound is in general a good idea in hydrology (see e.g. Papalexiou and Koutsoyiannis, 2006).

Following the cited reference, this referee comment is probably more with respect to the practicalities of defining and estimating an upper bound, particularly in the context of probable maximum precipitation. The limits of physical processes ensure that environmental variables must indeed have upper bounds, poorly defined though they may be. We can always specify some impossibly large magnitude to make the point. For example, a 10,000-metre daily rainfall anywhere on Earth does not have a vanishingly small exceedance probability, because the exceedance probability is exactly zero. The same applies for a 1,000-metre daily rainfall.

So somewhere below 1,000 metres of rain there must be a universal upper bound to daily rainfall amounts. To be sure, this bound is so nebulous that it is best expressed as a probability distribution. However, that distribution itself must be bounded above and does not extend to infinity. There is nothing new about extreme value distributions bounded above – as is the case with the Type 3 extreme value distribution of largest extremes. Having apparent Type 1 or Type 2 distributions which are mathematically bounded above is in fact consistent with the reality of upper bounds for environmental variables. That is, having data well-matched by Type 1 or Type 2 extreme value distributions is not evidence to negate the existence of upper bounds.

The important practical point to make, however, is that any estimated value of an upper bound in such situations will be so poorly defined by the data as to be meaningless and will have little effect on estimated exceedance probabilities over the data range and for some distance beyond the largest value. Put another way, it would take a very large number of simulations for the upper bound to have any evident effect in distribution (b) in Figure 1.

With the provisos mentioned, incorporating upper bounds in hydrological parameterisations is more a good idea than a bad idea.

I would suggest the Author either to submit His work to a statistical journal or to extend it in a way to make it useful for the readership of HESS, which would imply to demonstrate the applicability of the method in hydrology, for example by showing that the proposed distribution is more appropriate that the standard ones in a particular case study.

As noted, the paper would not be acceptable as a contribution to a statistical journal. For demonstrating usefulness for HESS readers, probably the best way would be to use some data simulated from a rescaled version of the present Figure 2 distribution – which is not well described by any GEV.

The abstract, which includes definitions and one equation, is very unusual for hydrology journals. Besides, Section 2 is a repetition of the abstract. I would suggest to shorten it.

Yes – the abstract should be reduced and made less explicitly mathematical. I also need to add "Technical Note: " in the title.

Figure 2: I would suggest to plot all three distributions of Figure 1 also here

Yes – that would be better for completeness, my thanks for noting it.