Dr. Prosdocimi, thank you for the comments on our paper. We are in fact aware of Villarini et al., (2013) and now include a citation to this work in our revised manuscript, along with other references which employ such regression methods. As you point out, this approach is different from that proposed here as we do not consider co-variates in the nonstationary model presented in this work. However, there is no reason why one could not include covariates in the regression model we include for modeling the nonstationarity in the parameters of the generalized Pareto model. We also see great value in using proportional hazard (Cox type) models in this context and hope that others will pursue these models in future work, thus we have included a citation to this work and a discussion of the use of such models as an alternative to our own approach.

Further it is not very clear to me, why one can say that h(t) = p0 and that for the stationary case the hazard failure rate is constant (page 6891 - line 26). I guess it is because we assume that for each year there is a constant probability of exceeding a design event and that the process is memoryless, but it is not clearly stated. I feel like many details of Section 2 are not supported by a clear motivation, while they constitute the foundation for the results of the paper. It could be that more details are given in the
Read and Vogel (2015b) paper, but I don't have access to it. Since the approach taken by the authors is quite different from the traditional survival analysis approach of modelling the time to failure/reliability I think it is worth a bit more discussion. Also, since time is the explanatory variable in the regression, the trend directly impacts the hazard function, but one could actually compute the non-stationary hazard function under a different regression model with some more relevant covariate, and all the functions would **need to be recomputed.**

The authors agree with this comment, raised by other reviewers as well, regarding the clarity of our assumption h(t) = p_t. For the stationary case, h(t) = p_o we can prove such an assumption by using hazard function equations (Eqns. 1-3 in the paper) and writing the following:

$$h(t) = \frac{f_T(t)}{1-F_T(t)} = -\frac{\lambda \exp(-\lambda t)}{\exp(-\lambda t)} = \lambda$$

This result is widely understood in hydrology, and forms the basis of the theory behind the probabilistic properties of the return period for the stationary case. This implies a constant hazard rate that reflects the constant exceedance probability *p_o*, associated with the design event *x_o*, so that $h(t) = \lambda = 1 - F_X(X_o, \theta) = p_o$**. The nonstationary case is more complicated, and as you'll note in the response to Reviewer 2, we have verified our assumption in two different and independent ways.**

It is true that the selection of a trend model will impact the resulting hazard function, but we do not know to what extent. We also evaluated a linear trend model in the scale parameter as well and came upon a similar finding with regards to the time to failure distribution and impact on reliability. It would be interesting for others to test the sensitivity to the trend model to understand how this impacts the distribution itself and the risk and reliability metrics. Clearly future research is needed along these lines.

Finally I have a more conceptual point, which is not a critique of this paper, but it is something that has been puzzling me for a while across several papers on which the authors might have

something interesting to say from their experience. It is not surprising that, with increasing trends in the magnitude of floods, design events will be exceeded more frequently than the time they were designed for (end of page 6900).
If magnitudes are getting bigger the tail of the natural hazard distribution must be getting thicker, and hence the "new" probability attached to a specific design event will inevitably be smaller. When the point process representation of extremes is employed one can find a direct relationship between the distribution of annual maxima, peaksover- threshold and the number of events recorded in one year. This is very convenient for making inferences in the non-stationary case, as pointed out in Katz et al (2002) and in a recent paper of ours Prosdocimi et al. (2015). The authors propose a very interesting tool to frame this dependency in an elegant and useful way - I just wished to point out that one of the straightforward consequences of increasing magnitudes must be the increase of the actual exceedance probability connected to specific design events.
Page 6899 Line 13: the chosen models investigate cases for which one of the effects of the non-stationarity seems to be a shift from negative shape parameter to positive, and hence to upper-bounded frequency curves. Is this realistic?

Thank you for these insights. Surely it is possible that under severe nonstationary conditions, the very shape of extreme value distributions will be quite complex and possibly dynamic. Whether or not such results as an evolution in the shape parameter from negative to positive are realistic or not, will only be fully understood after more emphasis on the type of analyses we have performed, are pursued. One could easily investigate the impact of various forms of nonstationarity on the shape parameter, on the upper tail behavior, and it is likely that the shape parameter will become positive, thus indicating distributions which exhibit upper bounds, rather than lower bounds. It is indeed compelling to think that a watershed which currently exhibits only a lower bound on flood exceedances, could someday exhibit an upper bound. Thanks again for this very interesting insight.

Lastly I spotted the following smaller issues:
Page 6893 Line 19: the term Cx has not been introduced at this point, I would move the sentence to the point in which Cx is introduced.

The authors agree and have made this change.

Page 6896 Line 15: the intermediate formula shows the results for the exponential case (not the GP2 described in Eq. (9)) and a _ suddenly appears.

Sorry but we could not see the place in the paper which this comment refers to, as the exponential trend model discussed on p. 6896 is shown in Eq. 10 and the stationary GP2 model presented in Eqn. 9 (but not referred to in this section).

Page 6897 Line 18: should be h(t), I think

The authors have revised our notation according to reviewer comments and made this change.

The Lee et al. reference of pg. 6890 line 21 is missing in the reference list

Thank you for pointing this out. It has been added.

References

Katz, R., M. Parlange, and P. Naveau (2002), Statistics of extremes in hydrology, Adv. Water Resour., 25, 1287û1304.

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Villarini, G., Smith, J. A., Vitolo, R, and Stephenson D.B. (2013). On the temporal clustering of US floods and its relationship to climate teleconnection patterns. Int. J. Climatol. 33: 629û640 DOI: 10.1002/joc.3458