After reading the comment by Dr. Prosdocimi, I would like to share my opinion on a key point of the present manuscript. Since I had the chance to review the paper by Read and Vogel (2015b), which is still under review as far as I know, I can say that I had the same doubts about the equality $p0=h(t)$. In particular, in that paper the Authors introduce this equality as a definition, whereas the identity $p0=h$ is a special case holding true only for iid data (exponential inter-arrival times), whose extension to independent and "non-identically" distributed data seems to be not so straightforward. Referring to that paper (but this comment holds also for the present manuscript), my arguments on this point are as follows.

We appreciate your thoughtful comments on this paper as well as the one referred to above (Read and Vogel, 2015b) which is still under review. As discussed earlier in these comments, the authors agree that the language in this paper should be revised to reflect $h(t) = p_t$ as an assumption to verify, rather than a definition. Please refer to comments to Reviewer 2 for our approach in justifying this assumption for the cases described in this work.

Given a known or estimated time dependent model for flow intensity X (peaks over thresholds or annual maxima), $FX(x;\theta(t))$, whereθ is a generic parameter vector, the Authors attempt to deduce the distribution of the waiting times, $FT(t)$, for the next exceedance of a given value $X = x0$, exploiting the hazard function to link $FX(x;\theta(t))$ and $FT(t)$. Now, the hazard function is defined as

$$h(t) = \frac{f_T(t)}{1-F_T(t)} = -\frac{\frac{dS_T(t)}{dt}}{S_T(t)} \quad (1)$$

where fT is the probability density function of the waiting time, FT is the corresponding cumulative distribution function, and $ST = 1-FT$ is the survival function, which is also known as reliability, and gives the probability that the system experiences no failure within $(0, t]$.
From the above definition it follows that the cumulative hazard function is

$$H(t) = \int_0^t h(s)ds = \int_0^t \frac{dS_T(s)}{S_T(s)} ds = -\ln(S_T(t)) \quad (2)$$

For iid (independent and identically distributed data), regardless of the form of $FX(x;\theta)$, it is known that the waiting times of exeedances over a given quantile (high) threshold $x0$ are memoryless and follows an exponential distribution with rate parameter $\lambda=p0$, where $p0= SX(x0;\theta)$ is the probability of exceedance corresponding to $x0$. Under exponential arrivals, the hazard function is constant,

$$h(t) = \frac{f_T(t)}{1-F_T(t)} = -\frac{\lambda \exp(-\lambda t)}{\exp(-\lambda t)} = \lambda \quad (3)$$

From this very specific result ($h(t) = \lambda = p0 = SX(x0;\theta)$), which holds true under iid conditions), the Authors deduce that it still holds true in the form $h(t) = pt = SX(x0;\theta(t))$ under independent and non-identically distributed conditions. This is actually the core of the paper. i.e. establishing the advocated link between $FX(x;\theta(t))$ (or $SX(x;\theta(t))$) and $FT(t)$ (or $ST(t)$). However, this is not a definition as stated in Read and Vogel (2015b), but an assumption that requires to be verified analytically or numerically, or both. It cannot be a definition because the hazard function is already defined as in Eq. (1), and a different definition would imply that the relationship between

$h(t)$ and $ST(t)$ in Eq. 3-above is no longer necessarily true, thus preventing its application. In more detail, if the Authors' assumption is true, in order Eq. 3-above to be applicable, it should be

$$S_T(t) = \exp\left(-\int_0^t h(s)ds\right) = \exp\left(-\int_0^t S_x(x_o;\theta(s))ds\right) \quad (4)$$

However, I cannot see any reason why *SX* and *ST* should be linked by this relationship, which holds true only if $h(t)$ is defined as in Eq. 1 so that the integral in Eq. 2 holds true.
To summarize, I think that the key point in order to make results convincing is to show the validity of Eq. 3 above, or similarly, the identity (or approximate identity) of the assumed theoretical hazard functions and the actual hazard function resulting from simulations, under non iid conditions for whatever model $FX(x;\theta(t))$. If this hypothesis is not verified, all the framework provides no advantages, as it would require simulations from $FX(x0;\theta(t))$ to obtain quantities such as $FT(t)$; however, this procedure does not need time-to-failure analysis and hazard related concepts.

Again, we refer the reviewer to our response to reviewer 2 above. We do not prove the relationship in Eqn. (4) for all $F_x(x;\theta(t))$, which as you point out would be something of a 'golden ticket' to apply the HFA to nonstationary natural hazards. Rather we present a case for the widely applied GP2 model, for which the results can be used across many types of natural hazards as discussed in the paper. We selected a trend model which has also been shown to provide a reasonable approximation to series of annual maximum floods by several others, recognizing that this is only one of many possible forms. Thus our paper is not exhaustive in terms of applying HFA to nonstationary natural hazards, instead it is only intended to serve as an initial study, worthy of future research which involves experiments with similar models of nonstationary hazards. We acknowledge that the literature provides other approaches to characterizing the nonstationary behavior of natural hazards, however, as our revised manuscript stresses, most of that research involves nonstationary models of the natural hazard process *X*, with little attention given to the probabilistic behavior of the return period *T* associated with a design event chosen to protect against such nonstationary hazard processes.