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Discussions

Interactive comment on "Hazard function theory for nonstationary natural hazards" *by* L. K. Read and R. M. Vogel

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I only recently saw this paper and immediately set out to read it as it discusses a topic which I find interesting and that I have been thinking about recently. The topic of the paper is indeed interesting, and it is a positive signal to see different approaches to the description of how non-stationarity might affect the risk connected to natural hazards. I have some comments to the authors, and I am hurrying to post this while I still can.

Firstly, I was wandering if the authors were aware of a nice paper from Villarrini et al. (2013), which also uses tools from Survival analysis to investigate non-stationarities in POT data. Their modelling framework is quite different, but I found the idea of using the Cox proportional hazard models quite nice, as it gives a simple regression model.

The advantage of the approach of Villarini et al (2013) is that one can include variables different from time in the model, although this comes to the cost of undermining the possibility of doing useful prediction for the future as the risk would depend on the unknown values of the model covariates.

Further it is not very clear to me, why one can say that $h(t) = p_0$ 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.

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, peaks-over-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?

Lastly I spotted the following smaller issues:

Page 6893 Line 19: the term C_x has not been introduced at this point, I would move the sentence to the point in which C_x is introduced.

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

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

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

References

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

Prosdocimi, I., T. R. Kjeldsen, and J. D. Miller (2015), Detection and attribution of urbanization effect on flood extremes using nonstationary flood-frequency models, Water Resour. Res., 51, doi:10.1002/2015WR017065.

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

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