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Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "Has fire policy decreased the return period of the largest wildfire events in France? A Bayesian assessment based on extreme value theory" *by* Guillaume Evin et al.

Anonymous Referee #1

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In the paper under evaluation, the authors compute return levels of wildfire area burned in southern France in two different periods and for three pyroclimatic regions (PCr-1, PCr-2 and PCr-3). The paper comprises five sections. After the introduction, the authors introduce in Section 2 the fire database and the statistical methods. Results and discussion are presented in Section 3 and 4, respectively. Finally, Section 5 is devoted to conclusions.

The overall impression of the paper is that, although the topic of research is interesting the paper is not publishable in the present form. Below are some comments:

Throughout the text "burned area" should read "area burned"

The authors should better explain what is the benefit of using a Bayesian framework within this context as compared with other competitor approaches. Such explanation will strengthen the paper.

p.2, l.30-31: the authors stress that "...our EVT framework is implemented in an explicit non-stationary context adapted to our case study". The authors should better explain, in a convincing way, to which "explicit non-stationary context" are they referring to. This is not clear neither from the introduction nor from section 3.

p.2, l.33-34: replace "...to assess uncertainties in return periods and related return levels." by "...to assess uncertainties in return levels."

p.2, l. 34: the sentence "This allows determining if the highlighted changes are actually significant." is not understandable as it is. Please elaborate.

p.4, l.3: replace "(or GEV)" by "(or GEV, in-short)".

p.4, l.3-5: the sentence "Grounding on this strong mathematical result, we assume that wildfire samples for each year are sufficiently large, so that annual maxima of BA can be considered to follow a GEV distribution." is not understandable as it is. Please elaborate.

p.4, l.5-9: the sentence "This has already been shown to be adequate...to cite a few of them" adds anything to the paper and should be removed from the text.

p.5, l.1: the authors should include in eq (1) the range of variation of x and theta.

p.5, l.3: the authors should also provide an explanation to the case xi=0.

p.5, l.3: remove "(the bound cannot be exceeded)" from the text.

p.6, l.7: the sentence "In this expression, we assume that the normalizing constant of the posterior is not known." should read "It is assumed that the normalizing constant of the posterior distribution is unknown.".

p.6, l.8-9: the sentence "The likelihood function...parameters theta" is not understandable as it is. Please elaborate.

p.6, l.13-14: the sentence "Indeed, raw BA are twodimensional data and have very skewed distributions, which often lead to extreme value distributions with an infinite variance" is not understandable as it is. Please elaborate.

p.6, l.24: the authors should better explain the reasoning for considering the joint prior distribution as being the product of the marginals; in other words, what is the justification for assuming that the GEV parameters are independent? Furthermore, in the joint prior expression, the term \Phi(\sigma) should read \Phi(\log\sigma).

p.7, l.18: remove "(for example the test of Kolmogorov- Smirnov)"

p.8, l.3: the authors should better justify the choice of the Bhattacharyya coefficient for measuring the distance between distribution functions. Furthermore, the authors should also highlight the advantages and limitations of such coefficient when compared with other competitors such that Malalanobis distance or the weighted L2-Wasserstein distance, among others.

Section 3: the authors should carry out the same analysis but considering several distance measures and to compare the results. This will strengthen the paper.

p. 6-7: the authors should clearly explain the steps for implementing their algorithm. In my opinion the authors fail in explaining their procedure in a way understandable to the reader. Furthermore, details on how their method is implemented in practice are largely omitted which difficult the procedure understanding. For example, nothing is said about the convergence of the MCMC method. Did the authors checked the convergence of the algorithm? Did the authors checked for the presence of metastability? The authors also should include a table displaying the acceptance rates for the GEV parameters within the MCMC algorithm.

p.8, l.20: the sentence "Posterior distributions of parameters..." should read "Posterior

distributions estimates for the extreme value parameters..."

p.8, l.20: replace "sigma" by "log(sigma)"

p.9, l.1: replace "...corresponding BA is ...." by "...corresponding BA estimate is ...."

p.9, l.2: replace "...predictive distribution of these..." by "...predictive distribution estimate of these..."

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