Authors reply on comments of referee #2

Major comments:

The manuscript of Evin et al. attempts to demonstrate statistically that current fire policies in southern France had an effect on large fires burnt area with a return interval of 5 years but not on that of 50 years. They conclude that massive investments in aerial and ground forces are not sufficient to control large fires during extreme fire season (like the 2017 one) and that other strategies should be integrated (e.g. landscape management, self-protection) to leverage fire risk on the long-term. I appreciate the effort to demonstrate analytically a common believe (i.e. fire suppression policies are not sufficient) usually addressed with a qualitative approach or simple descriptive statistics. Although I agree with the general thesis that current fire policies are not yet able to manage large fire seasons like summer 2017 or the ongoing 2018 (and I fully support alternative strategies proposed in the manuscript), I'm not convinced that this experiment provides sufficient evidence that fire policies introduced in France since 1994 are inadequate to manage large fire seasons. Indeed, despite the statistical tests used in the study are quite sophisticated and applied correctly, I have a doubt about modelling fire return periods of 20 to 50 years with a time series of 21 years. Indeed, model uncertainty is very high (Table 4) and at the end of the results section authors state that this uncertainty limits the interpretation of the estimate. Consequently, the key point of the discussion and conclusion (that current fire policy implemented in France is not effective against large fires with a return interval of 50 years) is based on this single uncertain result. The statement at the beginning of the discussion, referring to the sole fire prone region PCr-1, i.e. ". . .the BA corresponding to a return period of 50 years has not significantly decreased" does not take into account limitations in the analyses. Consequently, the discussion that follows appears to force results interpretation toward a thesis (although, I repeat, it is a thesis that I fully support). In addition, it is not clear to me if differences in other fire regime drivers such as climate, fuel flammability and landscape connectivity where considered in the model when comparing 1973-1994 and 1995-2016 periods. Indeed, if you want to test the "fire policy" driver the model should account for the variability explained by other relevant drivers. Note that in Figure 2, after 1994 the sole fire peak in the number of fires>100 ha reaching a level similar to ones before 1994 corresponds to the 2003 fire season, i.e. the major climate anomaly hitting southern France during the period of analysis. Notably, one of the author in a previous similar paper (Curt and Frejaville 2017; DOI: 10.1111/risa.12855) stresses the increase in fire weather index, human pressure and fuel coverage in the second studied period.

The authors thank the referee for these comments which will help to improve this manuscript. Please find below our answers.

In your major comments you indicate that our central argument is that the new fire policy/strategy has only reduced the return period for large fires in one pyroregion, and that this result is somewhat 'over-interpreted'. In addition, it is true that uncertainty is central in this study and these results, in our opinion, provide interesting results and clear interpretations. As in any studies with a limited availability of data (as it is the case for all geophysical studies), results must always be interpreted in light of the corresponding uncertainties. In addition to visual assessment (see, e.g. Figure 5 of the current manuscript), the Bhattacharyya coefficient provides a quantitative assessment of the changes, taking into account the uncertainty in the estimates. This point will be included.

In addition, it is also suggested that other variables (climate, fuel flammability, and landscape connectivity) may be added to the results because they also control the area burned. In our opinion, additional analyses taking into account external drivers (other than the fire policy) are beyond the scope of this article, which aims at describing statistically the changes in extreme return levels of BA. Furthermore, these aspects have already been assessed in a previous study (Curt and Frejaville 2018). However, a discussion about the role of fire controls will be added. This will give weight to the results and reinforce our main argument which is that fire suppression policies are not sufficient, especially in pyroregions with high fire activity (PCr-1) and with increasing structural factors promoting fires.

Minor comments:

Comment R2 #2.1. Pg1, LN17, LN18 and throughout the text - Eliminate dots after “ha”

We eliminated the dots after “ha.” throughout the text.

Comment R2 #2.2. Pg1, LN16-LN20 – report initial and end period for fire statistics listed in this paragraph

We indicated the dates of the fire data (1973-2016); the recent period corresponds to 1994-2016

Comment R2 #2.3. Pg2, LN1 - Include here other relevant references, e.g. Moreira et al. 2011 (DOI: 10.1016/j.jenvman.2011.06.028), Fernandes et al. 2013 (DOI: 10.1890/120298)

We included the references proposed on fire policies issues.

Comment R2 #2.4. Pg2, LN3-LN5 – I believe here is missing a major driver of the burnt area in southern Europe, i.e. cultural and socio-economic aspects affecting landscape management (i.e., type of urbanization, agriculture and forestry, land control, use of fire, type of postfire management) which in turn contribute to determine fire likelihood and burnt area. Note that this is supported also by authors at Pg. 3, LN15

We added references on cultural and socio-economic aspects which promote fires and burned area in the Mediterranean.

Comment R2 #2.5. Pg2, LN9-LN11 – While I agree knowing the return period of large fires is useful to governmental agencies and reinsurance companies to evaluate the cost of future fires, I do not believe it is useful to the dimensioning of fire crews during an extreme fire event (this is something decided in real time once the ignition point, the fire weather, potential fire trajectories and values at risk are known). Rather, as the return period of a flood is useful to the dimensioning of infrastructures such as embankments of a river (a similarity used by authors at LN 6-7), the return period of a large fire in a valley is useful to the dimensioning of fuel management measures, e.g. how many fuelbreaks, where they must be located in the landscape, how much large they must be, which is the interval between fuel treatments to maintain fuelbreaks before large fires return, which in turn determine management costs and consequently the number of fuelbreaks I can maintain in a given period.

As you indicate, knowing the return period of large fires is not of such importance for preparing/dimensioning fire crews during an extreme fire event. Our sentence was a bit misleading: this is not important during a single event (especially if it is currently ongoing), but this is important - and now better taken into account by the civil security - each year and before or during the fire seasons. Firemen account for the daily fire danger and the tendency for the weeks to come in each region in order to dimension and pre-position the fire crews on the basis of the likelihood to have a large fire in a given region. This is why this sentence will be replaced by: “Concerning fires, this information can help each year to pre-determine the size of the fire crews and of fire tactical means such as airplanes and trucks in each region, in order to support ground forces if extreme fire events occur (Lahaye et al., 2014)”. We also agree that return periods of large fires can be important in order to dimension fuel management measures in a given region. It is known to be an efficient and sustainable measure for leveraging fire risk. However, as return periods of large fires are not yet calculated in France, fuel management is not currently done on this basis. We will add this comment to the text and to the discussion as a potential application of this study.

Comment R2 #2.6. Pg2, LN14 – later in reading the manuscript I assumed “return levels” the same as “return period”, but then I realized it was not the case. However, it is not clear which is the difference between the two. Please clarify here or in the method section.

The difference between “return levels” and “return period” will be clarified in the section “Materials and Methods” of the revised paper. Return periods correspond to the average time length (e.g. 20 years, 10 years) between two return levels (100 ha, 1000 ha).

Comment R2 #2.7. Pg2, LN15 - after “. . .dedicated studies are available” – Although later in the paragraph authors report several references in relation to methods used to calculate the fire return period, I suggest to insert here 2-3 references to previous studies calculating the large fire return period that author think are very relevant for fire management purposes

A reference on the fire return calculation for large fires in France (Hernandez et al. 2015) will be included.

Comment R2 #2.8. Pg2, LN20 – after “Extreme Value Theory” add “(EVT)”

“EVT” will be added after “Extreme Value Theory”.

Comment R2 #2.9. Pg2, LN29 – the fire policy change in 1994 in France appears here for the first time, but it is not clear in what the policy consists, and no references are provided. I would expect here, or later in the methods, a clear referring to the policy, and some quantitative data (i.e. indicators of changes in comparison to the previous policy, e.g. number of helicopters used during the fire season, annual area treated with prescribed burning) characterizing the policy. A table could be useful to synthetize information

Information on the main changes in fire policy will be incorporated. However, precise and quantitative data are often not available (e.g. the number of helicopters).

Comment R2 #2.10. Par 2.3 and 2.4 – Clearly state what μ,σ, ξ indicators means in terms of fire management μ, σ, ξ are parameters and are not directly linked to indicators of fire management. Return levels are more easily interpreted and the focus is put on return levels rather than the GEV parameters in the remainder of the manuscript.

Comment R2 #2.11. Pg8, LN24 – I do not see where the "parameter uncertainty" is reported. Include model uncertainty in figure 4?

The posterior distribution is a direct assessment of the parameter uncertainty. The dispersion of the posterior distribution indicates if the parameter uncertainty is large or not. This will be clarified in the revised manuscript. Note that model uncertainty (i.e. uncertainty related to the choice of the GEV distribution) is not assessed in this study.

Comment R2 #2.12. Pg12, LN2 – what is meant with "median return levels"? If 20 years, change "Table 4 reports the BA corresponding to high return periods (20 and 50 years)" in "Table 4 reports the BA corresponding to median and high return periods (20 and 50 years, respectively)"

As illustrated in Figure 5, for each return period (i.e. 20 years), we can provide the whole distribution of return levels. From this distribution, we could compute any quantile, (e.g. corresponding to probabilities 0.05, 0.1, 0.9, 0.95). In Table 4, as an indicator of the central tendency of the predictive distributions of return levels, we simply report the medians, i.e. the quantile 0.5. This will be clarified in the revised manuscript.

Comment R2 #2.13. Figure 1 – Large fires are defined as > 1000 ha, while in the text is > 100 ha. As regards the figure caption – after "pyroclimatic regions" include "(numbered circles)", or something in the legend clarifying what colored circles represent

Thank for this comment. Fires > 1000 ha should be indicated as "very large fires". We also agree that the definition of what the colored circles represent is missing. This will be corrected in the revised manuscript.

Comment R2 #2.14. Figure 3 – as the aim of the paper does not focus on statistical and methodological aspects I would move figure 3 to the supplementary material

In our opinion, it is important to show statistical and methodological aspects. Figure 3 is necessary as it shows the adequacy of the GEV distributions, which is not obvious for a non-statistician. Furthermore, this comment is in contradiction with comments R1 #1.17. and R1 #1.18. made by reviewer #1 about advanced statistical aspects (distance measures, converge of the MCMC algorithm).

Comment R2 #2.15. Table 2 – it is not clear how it is possible to model fire return intervals > 10 years with time series of 21 years (1973-1994 and 1995-2016)

The fire return levels are obtained from the fitted GEV distributions, using Eq. (3). The GEV model is used to extrapolate beyond the time period covered by the observations.

References

Curt, Thomas, and Thibaut Frejaville. 2018. "Wildfire Policy in Mediterranean France: How Far Is It Efficient and Sustainable?" *Risk Analysis: An Official Publication of the Society for Risk Analysis* 38 (3): 472–88. https://doi.org/10.1111/risa.12855.