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Discussions

# *Interactive comment on* "Risk for large-scale fires in boreal forests of Finland under changing climate" *by* I. Lehtonen et al.

**Anonymous Referee #1**

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Manuscript (MS)

Journal: NHESS Title: Risk for large-scale fires in boreal forests of Finland under changing climate Author(s): I. Lehtonen et al. MS No.: nhess-3-4753-4795, 2015 MS Type: Research Article

Referee Report

General comments to the author

There are several reasons why this article is not ready to be accepted. 1. The article is not very innovative. There are several studies about the impacts of climate change on the fire regime, using (different types of) models to simulate burnt area of the number



of fires, based on fire weather data or indices, computed with future projections of meteorological data, simulated by one or several GCM/RCM. Some of these studies were performed for Finland in spite of, fortunately, this country being only marginally affected by wildfires. In the introduction, the authors seem to justify this paper with the assessment of the socioeconomic and ecological impacts (missing in the previous studies performed for Finland) but this pioneering aspect (not included in the objectives) was not considered in this study. The authors need to justify this paper, addressing relevant scientific and/or technical questions within the scope of NHESS and presenting new data and/or novel concepts, ideas, tools, methods or results;

2. This paper is unnecessarily confusing in fundamental aspects, namely the data (both fire and climate) and methodology sections. First, the authors need weather data (air temperature and air relative humidity, wind and accumulated precipitation) at the same location and time (12:00) to compute all the components of the Canadian Fire weather system as well as their spatial/temporal means (FWI, DSR, MSR, SSR, etc.). For the sake of dataset's homogeneity, the authors should have selected just one meteorological dataset which can provide all the necessary data. Such data sets exist. For example, the ERA Interim dataset comprises all the required data. However, the authors selected two different databases: the ERA Interim for wind speed and the Finnish Meteorological Institute (FMI) observed dataset for air temperature and relative humidity, wind and accumulated precipitation. This decision has the additional drawback of the FMI dataset being composed by daily mean values of those variables which required the estimation of afternoon values, a procedure that unnecessary increases the data errors. It seems a little strange that FMI dataset comprises mean values but not the value observed at noon as well as values of air temperature, relative humidity and precipitation but not wind speed. Second, the study period is 1996 – 2014 (please replace minus sign by en-dash whenever defining time periods) however, the dataset was enriched with 9 extra fires, 7 of them before 1971 (almost a climatological period of 30-years before the study period). What is the need to include this small number of large fires? Why did the Finnish fire dataset include a Swedish fire? These fires

increased the fire models performance or the confidence/statistical significance of the findings? How? Third, the authors decided to use high spatial resolution climate data. So, why use GCM instead of RCM simulations? This is not very important but, please explain why to choose $0.1° \times 0.2°$ grid and not $0.1° \times 0.1°$. The use of GCM outputs required the downscaling of this data. The authors decided by a non-physical approach and wrote that they followed the methodology of Aalto et al. (2013). However, the title of this paper is “Spatial interpolation of monthly climate data for Finland: comparing the performance of kriging and generalized additive models”. In fact, Aalto et al. tested the following methods: kriging with external drift (KED), generalized additive models (GAM), and GAM combined with residual kriging (GK). So, please, explain which method was, in fact, used in this paper. In addition, it is important to explain why another method was used to downscale wind speed and which were the “other variables” (page 7, line 1) used in this procedure?

3. The confidence on the findings of this study is clearly affected by several factors. The most important of them are: (i) the short study period; (ii) the very small number of fire events; (iii) the development and robustness of the fire models (eq. 3 and eq. 4) based on a not very clear relationship with the fire indices. Eighteen years of data means just 18 data points to fit which could lead to erroneous conclusions, especially with so few number of large fires in Finland. In fact, 112 large fires (i.e., fire with $10 \leq BA < 200$ ha), in the 1996 – 2014 means 6 large fires/year which is simply too low. In addition, the authors do not present a histogram of the large fires allowing the reader to understand how many of these fires are close to 10 or to 200 ha. Finally, I’m particularly concerned with the fire models’ development and evaluation. Who intends to simulate a process with climatic variables has to describe the climate of the study area and unequivocally demonstrate the dependence of this process with these variables. The climate description at monthly scale is of fundamental importance to clearly understand the impacts of climate and climate change on fire activity in Finland and to understand the selection of DSR and MSR as predictors. For example, Fig 1 present the projected changes in the period of 7 months (April – October) which is a too long period and raised several



questions/doubts. For example, the projected increase in precipitation and air temperature is identical in all months (between April and October) or is due to an increase in just a few months, eventually out of the fire season? The predictor selection process must be clarified. How were the predictors DSR and MSR selected? Did the authors test other potential predictors? It is quite intriguing that the selection of DSR and MSR as the predictors in an exponential model when, as recognized by the authors, “most of large forest fires are still ignited with relative low DSR”. How was the DSR threshold of 15 determined? Did you test other thresholds? Which were the results? As the authors know, R is not the only or the best indicator of the quality of a model and not even an R equal to one ensures good performance of a model. It is important for the reader to know if the assumptions were tested. Differences between obtained values of rp and rs were not discussed/interpreted/validated. As the authors also know, Pearson’s correlation (rp) is a statistical measure of the strength of a linear relationship while Spearman’s correlation coefficient (rs) is a statistical measure of the strength of a monotonic relationship. Thus, it can be very dangerous to use rp to assess the strength of an exponential relationship. On the other hand, there are authors claiming that an rs equal to 0.39 reveals a weak association while an rs equal to 0.58 reveals a moderate correlation. This implies that the fire models (number of large forest fires and burned area) presented in this manuscript may not be as strong as the authors claim and, eventually, they should be used very carefully under current conditions and its use discouraged out of calibration range (future climate scenarios), specially without a previous proper assessment (e.g., with cross validation). The authors seems to be satisfied with R2 values of 0.67 and 0.81. However, these results mean that the models are only able to explain, respectively, 45 and 65% of the variance of number of large forest fires and burned area which must be addressed by the authors. Climate change and its impacts are a matter of assessing the existence, magnitude and significance of changes between current and projected statistical distribution. Therefore it is of fundamental importance to assess the statistical significance of all the results obtained in this study, besides of the correlation coefficients. The values of the mean bias and RMSE may, or not, increase the confidence in these models as it much depends on other factors such as: the mean burnt area of the 112 large fires used in this study (which is unknown for the reader) and on the size of the time series. In this sense, what were the periods of calibration and validation of the models (please see the caption of Table 4)?

4. The manuscript is not always well written. There are several unnecessary repetitions (e.g., Caption of Table 4) and citations. The authors use and abuse of relative and subjective concepts (e.g., strong, majority, most, a few) without clear/objective definition which is strongly discouraged. There are several sentences as well as table captions beginning with numerals which, in my opinion, should be avoided. The fire related concepts are not the ones usually used within the fire community. For example, in the title, the authors claim they are studying fire risk when, in fact, they are studying fire danger as recognized in page 4, lines 24 – 26. Why use fire source instead of fire cause? I suggest the use of the Glossary of Wildland Fire Terminology.

Some more specific comments

While the problems identified above are not corrected it does not seem appropriate to discuss detailed issues. I include just a few comments/questions to illustrate/underline some of the subjects discussed before.

1. Page 4, lines 24 – 26: "Hence, there exists a clear need to update the fire danger assessment by using several models instead of the usually applied multi-model mean approach." An explanation on how the changes presented in the manuscript were computed is advised. Please note the caption of Figure 1 "Dots indicate the multi-model mean change…";

2. Page 6, lines 2 – 3: Information about the size and location of the Åland Islands in relation to mainland Finland should be provided for non-Finnish readers; it is also important (Page 12, lines 2 – 7) to include the latitude and longitude in Figure 3 and 7;



3. Page 6, lines 6 – 8: "Those fires which exact coordinates were not reported were located in the middle of the municipalities where the fires reportedly had occurred". Does this mean that, in the case where the spatial coordinates of the fire were missing, it was assumed to be equal to the coordinates of the centroids of the municipality where the fire had initiated?

4. Page 6, lines 8 – 9: Which "other types of wildland fires" besides forest fires were excluded? What is your definition of forest fire? Please clarify it.

5. Page 8, lines 10 – 18 and Figure 1: Why present projected changes in April-October daily maximum temperature, mean relative humidity and mean wind speed? It will be much more informative to present the projected changes of air temperature, relative humidity and wind at noon because these are the variables used to compute the Canadian fire weather indices. Please define "fairly", "robust" and "coherent" and explain why projections for relative humidity are "fairly robust" and projection for other variables are not. Please explain why projections of wind speed are not coherent?

6. Page 11, lines 16 – 18 and Table 3: The coefficients of eq. 4 were defined/used (please see the manuscript and table caption) or estimated? In the second case, please explain how;

7. Page 11, lines 16 – 18 and Table 3: The number of decimal places of coefficients a and b seems to be exaggerated. How many decimal places of the coefficients a and b are significant figures?

8. Page 12, lines 22 – 25: What do you mean by "rather similar"? With such a small number of large fires at national level is it advisable to consider sub-regions? How many sub regions were considered and how many large fires occur in each sub-region?

9. Page 14, lines 3: The value of 1.5% written in the manuscript, is not shown in Table 5. I believe it results from the sum of 0.7 with 0.8 which corresponds to fires with BA >=10 ha. This should be clearly explained in the manuscript;

10. Page 15, lines 8 – 9: I believe this sentence should be changed because it is not in accordance to the sentences in the following lines;

Technical corrections

At this stage, it is not opportune to suggest any technical correction.

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