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Interactive comment on "Modeling of wildfire occurrence by using climate data and effect of temperature increments" *by* Amir Hossein Sadat Razavi et al.

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This paper aims at assessing the effect of temperature increase (driven by climate change) on wildfire occurrences in the "temperate" and "boreal" forests of the United States. Unfortunately, the manuscript is poorly written, the methodology is not correctly applied, the results are not correctly obtained, the discussion is mostly absent, and the conclusions are mostly incorrect. I deeply regret but I have no choice but to reject this paper.

I will briefly mention the most severe problems that have led me to reject the paper:

1. The Introduction is vague and does not address the two key points: 1) why study wildfire occurrence in "temperate" and "boreal" forests of the United States and 2) what is the novelty of such study? And why restricting the study to fires with "natural causes", and exclude fires with "man made origin"? Do the authors consider that meteorological and vegetation factors have a distinct influence depending on the origin of the fires?

2. The methods and data section does not explain how were the "temperate" and "boreal" forests defined, and there is no mention to the spatial scale of the analysis. The authors also have a naive view of the classification of forests (they just say that forests are classified based on their "distance to the equator"). And why did the authors restrict to fire occurrences larger than 1 kmˆ2? The list of selected input variables to the ANN model is a potpourri of short-term and long-term meteorological and hydrological variables mixed with soil and vegetation parameters. It is hard to understand why "total pressure" (also referred to by the authors as "absolute pressure") is a relevant local variable for fire, and why the zonal and meridional components of wind are separately used (instead of e.g. combining the two as wind speed). The authors claim that the inputs are independent but no evidence is given to support their claim. The authors do not explain how "large, destructive, uncontrolled quick (rapid spread), self induced, unplanned and unwanted wildfire cases" were selected, and why did they use these characteristics. The authors do not explain how non-fire occurrences were selected. The authors do not perform any analysis regarding the redundancy of the variables and of the added value of each one to the ANN model performance.

3. The Results section suffers from lack of clarity and has serious mistakes. Why is the formula (data-average in previous 7 days)/(max-min) used to "normalize" the data? And max and min refer to what? And why interpreting an output of the ANN model of greater than 0.8 as "warning attention should be sent to responsible organization", of between 0.5 and 0.8 as "should be monitored in the next few days", and of lower than 0.5 as "no attention is necessary"? Regression outputs in Figs 2 and 4 are meaningless; this type of regression basically involves two points and therefore the high values of the corre-

lation is an artifact! (see e.g. https://en.wikipedia.org/wiki/Anscombe%27s_quartet). Regarding Figs 3 and 5, it is not correct to merge the histograms of training, validation, and testing. Anyway, the analysis performed is not discussed, nor is the model performance assessed. The sensitivity analysis is also incorrect; first of all, it makes no sense to add the same temperature increase in space and time; and it makes no sense changing temperature alone and keeping all the other parameters unchanged. Finally, in Figs 6 and 7, it makes no sense to superimpose a regression line on a response that is clearly non-linear. And do the authors consider that a ANN model can be reliably applied outside the space of parameters where it was trained?

4. The Conclusions are mostly incorrect. Results of the paper do not show that "weather forecast is useful to detect fire hazard in the next 7 days"; the proposed ANN model (using e.g. the Keetch-Byram Drought index and NDVI) can be hardly viewed as direct (or even an indirect) product of weather forecasts. The importance of temperature is not demonstrated by the similarity of the curves in Figs 6 and 7 (similar in what sense? just because of their sigmoid-type?) And what is the evidence provided by the authors that "with low values of temperature, other input variables (which ones???) have a lot more contributory role"? And the same could be said of the subsequent sentences regarding high values of temperature. Finally, the authors state that "there is a positive linear relationship between wildfire occurrence and temperature increase"; well, if the response is the one shown in Figs 6 and 7, it is certainly non-linear!

I sincerely hope that the above remarks, although harsh, will help the authors in reformulating this study on a much sounder basis.

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