Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., author comment AC2
https://doi.org/10.5194/nhess-2021-262-AC2, 2021

Reply on RC2

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Author comment on "Forecasting the regional fire radiative power for regularly ignited vegetation fires" by Tero M. Partanen and Mikhail Sofiev, Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-262-AC2, 2021

We would like to thank the referee for the review and thoughtful comments. In response to the comments, we have answered the questions raised in them and propose to make the following changes in the revised manuscript, as provided below.

The referee's comments and responses to them:

The aim of the paper “Forecasting the regional fire radiative power for regularly ignited vegetation fires” is to develop a predictive model for fire radiative power (FRP) based on climatic curve of FRP and weather parameters. The model is trained and evaluated against SEVIRI/MSG observations. It seems to performs reasonably well in the south-central African savannah but not so well in irregularly ignited fire regions.

This paper is sufficiently clear in its methods and results, but the motivation and contest of work could be explained better. Also I have few concerns regarding the predictant used for the fire prediction model and its potential application in real time.

My main concerns are:
Contest of work. I suppose the real aim is to develop a method to forecast fire emissions for air quality applications in a similar way of what done in Di Giuseppe et all 2017 and 2018 for GFAS. This is important as in absence of a fire predictive model, fire emissions are usually kept constant during the forecast integration. Another application could be to estimate a FRP to FRE conversion based on a realistic fire emissions diurnal cycle instead then a constant or flat one. This ia also important as it could improve the conversion between FRE and dry-matter. If these are the main aims, as I think they are, they should be clearly state in the introduction which instead drift between fire danger, problems in identifying or predicting an ignitions and climate change. A more structured introducton with a clear statemnt of the problem would certainly enhance understanding of the problem.

Response: The introduction will be revised to better explain the problems of FRP forecasting and the aims of this paper.

Predictors for the FRP model. The FRP predictive model is only based on weather parameter as integrated into fire danger metrics. By the author own discussion (line 120)

FRP is directly related to fuel amount that is notoriously not included in fire danger formulations. This means that you could have an ignition with very little fuel available but severe weather conditions and your method would not be able to pick up on this. I understand that real time monitoring of fuel amount is not easy to obtain but I wonder if at least the inclusion of some vegetation parameter in the form of LAI, NDVI would make sense. At least a discussion of this issue should be added to the paper.

Response: The occurrence of fires in each area in all its complexity is unique to each area. However, a common feature of well-predictable fires considered in the paper is that they are set regularly as a part of agriculture / forestry practices, i.e. the fire processes and vegetation state are controlled. It greatly reduces the variability of the fuel load, which is not an easily observable quantity. As a result, it was easier to include it indirectly by calibrating the model with the past fire strength than predict from indirect indices, such as LAI.

Impact in real time simulations. It would be interesting to see what the application of the model means in terms of fire emsions. I wonder if it could be possible to calculate the CO2 budget difference between the model and an assumption for persistence for exemple (i.e. FRP of today equal to yesterday). This could give an idea of the difference in atmospheric composition budget that the use of this method could bring.

Response: A comparison with the persistence forecasting is indeed a natural demonstration of the model skills. However, in our case it is not applicable: the model is made for predicting autonomously, without any fire information for the prediction period. Please note that our application year is 2018 whereas the training year is 2010. One cannot formulate any reasonable persistence algorithm for such forecasting time scale.

Split between training and testing dataset. For what I understand the mean FRP curve was derived for the 2010 year and the verification is conducted for the same year. Testing should be performed on a dataset that hasn't been used for training. I would like the author to comment on this as this is quite anusual.

Response: The training data are not predicted. The FRP curves in Fig. 1 are not predictions but represent the training set, a full 2010 (i.e. they are fit to 2010 data). The evaluation time period is 2018, Figs. 2-4.

I have few minor points:

Line 20; This incrising fire activity.... I am not sure this is the case as climate change can induce different human behaviours which might offset the increase in fire activities. We have already seen a reduction in burned areas due to achanges in human practices.

Response: This is the result in the paper by Pechony and Shindell (2010) referred to in the previous sentence (line 19). It will be made clearer that the result is related to the paper mentioned in the previous sentence.

Line 33 please check the use of parenthesis in citation when should be in line citations

Response: They will be corrected.

Line 46 "Unfortunately..", please revisit this sentence as it is not clear

Response: The text will be modified to be more accurate in the revised manuscript.

Line 49 "...of fire occurrence " you mean ignition ?

Response: The fire occurrence meant both the fire ignition and evolution in time. Corrected.

Line 50 and afterwards. Please be aware that FWI and the like are not fire risk indices. They provide a measure of hazard and not risk. A better definition is fire danger indices.

Response: These will be corrected.

Line 103 I disagree that fire extension and spread cannot be measured or predicted. There are fire behaviours models that do this with very good results

Response: The sentence referres to wildfires that have not yet occurred and also implied regional scales. The sentence will be clarified.

Line 118 and afterwards. The connection with the fire emissions I belive is the main scope of this work and this should be clarified. How this work would allow that estimation to be more accurate ?

Response: This part of the text will be expanded to include a more detailed discussion of the improved emission production accuracy provided by the method.

Line 145: fire risk -> fire danger

Response: This will be corrected.

Line 150: "or a best fit line", you mean a climatological estimation

Response: It means a regression line.

Line 229 FRE should be defined

Response: FRE is defined in line 175.

Figure 1. Left panels. There is clearly an annual cycle in the fire index chosen but a linear fit is used. Thus, December for exemple will have very unreasonable values. Can you comment on this ?

Response: The value of the meteo component, which is a part of the product of the three components of the FRP function Eq. (1) (the first component of the right hand side of the equation), becomes irrelevant outside fire season. The reason for this is that the FRP reference component (the middle component) is zero (or small) outside fire season (Fig. 1 right panels), which makes the entire FRP function zero.