

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
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Comment on nhess-2021-262

Anonymous Referee #2

Referee 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-RC2>, 2021

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 perform 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 context 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:

Context 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 al 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 is 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 introduction with a clear statement of the problem would certainly enhance understanding of the problem.

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.

Impact in real time simulations. It would be interesting to see what the application of

the model means in terms of fire emissions. I wonder if it could be possible to calculate the CO2 budget difference between the model and an assumption for persistence for example (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.

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 unusual.

I have few minor points:

Line 20; This increasing 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 changes in human practices.

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

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

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

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.

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

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

Line 145: fire risk -> fire danger

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

Line 229 FRE should be defined

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