



EGUsphere, referee comment RC3
<https://doi.org/10.5194/egusphere-2022-342-RC3>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on egusphere-2022-342

Anonymous Referee #3

Referee comment on "Differentiating fire regimes and their biophysical drivers in Central Portugal" by Raffaello Bergonse et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-342-RC3>, 2022

This manuscript aims to characterize fire regimes in Central Portugal and investigate the degree to which the differences between regimes are influenced by a set of biophysical drivers. The authors used civil parishes as units of analysis and cumulative percentage of parish area burned, Gini concentration index of burned area over time, and area-weighted total number of wildfires over a reference period of 44 years (1975-2018). The authors used cluster analysis to aggregate parishes into groups with similar fire regime and a classification tree model to assess the capacity of a set of potential biophysical drivers to discriminate between the different parish groups. The methods used seem to be suitable and the manuscript is nicely written. However, I have some criticisms, including some changes concerning the datasets used and the novelty of the present work, that should be addressed before considering the paper for publication in Natural Hazards and Earth System Sciences journal.

Major suggestions/comments:

Novelty of the work:

The results are fairly described and the discussion focused in a reduced number of previous publications, including two previous works of the same authors that exhibits strong similarities with the present work. The novelty (and need) of the present results of is not clearly addressed. The baseline of the present work in terms of data and methods is very similar to the previous two works. The data used is the same and the statistical approaches are slightly different, but very related with the previous ones. The main results are the same: the role played by LULC, slope and spring rainfall in fire behavior.

The present paper adds the classification in 4 FR for central Portugal. However, the FR

classification is closely dependent of the data used. This leads to my following comment.

Datasets:

FR regime classification is strongly dependent of the historical data over the region. Therefore, the used of climate data that does not describe the last two decades, when we are facing a change in fire paradigm over Europe, with the occurrence of the so-called *megafires*, highlights the fragilities of the FR classification.

Besides the 'old' meteorological datasets, the higher fire intensity or severity of the observed fire behavior trends was not included in the FR classification. The authors used burned area, however the burned area inside a civil parish may not be a good indication of fire intensity; other parameters (available through remote sensing datasets) should be included.

The inclusion of the suggested datasets, considering the aim of the present work, will strongly improve the quality of the results, highlighting its novelty.

Slope:

"Topography was expressed by slope (80th percentile, in degrees), which can be expected to promote flame propagation". Why using the 80th percentile and not 90th or 75th. Did you make a sensitivity analysis for this choice? Did the authors include elevation information? Why?

Rainfall:

"RFAJ was calculated from monthly rainfall data obtained from the Worldclim database (1970-2000)". The authors present an assessment for 44 years (1975-2018) and one of the crucial datasets used is only characterizing half of the period. The data used is not representative of the fire regimes in Portugal, namely considering the fire behavior in the XXI century. Please use an alternative database for precipitation data that characterizes the entire period evaluated, e.g. ERA5 (1979 to present), or alternatively change the period of analysis to 1975-2000.

..."in the form of raster maps of approximately 30 seconds (about 1 km resolution), which were resampled to a 25 m pixel". How was done the resampling? Which co-variables were used to do resampling? And, why to do the downscale if the data is further aggregated at

parish level?

Temperature:

The data used is not representative of the fire regimes in Portugal, namely considering the fire behavior in the XXI century. Please use an alternative database for temperature data that characterize the entire period evaluated, e.g. ERA5 (1979 to present), or alternative change the period of analysis to 1975-2000.

Net Primary Productivity (NPP):

Please consider to use the most recent version of the NPP product (2000-Present). Alternatively, consider to use the Climate Data Record of NDVI (annual mean or sum) available since 1981 to present (<https://www.ncei.noaa.gov/access/metadata/landing-page/bin/iso?id=gov.noaa.ncdc:C01558>).

Lines152-160

This paragraph seems to be out of order. Please consider to reorganize the paragraph (before NPP paragraph).

Lines161-165

The different periods considered for the different datasets could have a strong impact on the results, as the spatial patterns for precipitation and temperature in the last 30 years of the XX century may have strong differences in compared with NPP in the first 20 years of the XXI century.

With the aim to have a fire regime description that really reflects the recent vegetation, climate and fire behavior trends, I strongly suggest to include: a) Temp and Precip from ERA5 from 1979-Present; b) NDVI from 1981-Present. Therefore, the period of analysis would be 1981-Present (41 years).

Lines 360-361 “*This is confirmed by FR3’s low Net Productivity Ratio, ..., which is indicative of a relatively reduced forest cover.*” Please provide a reference or provide the analysis that allow this statement (or remove the sentence)

4.2.3 FR2: Please provide a better characterization of this FR. As it is, seems that this fire regime is not a separate fire regime and may indicate that the classification in 4 fire regimes is not the adequate.

Lines 401-408: The less clear relation with summer temperature is probably related with less adequate database used, that does not reflect the temperature changes in the last two decades. Please check the impact of use of the suggested dataset for meteorological parameters.

Lines 409-412: Is the statement supported by the NPP results of this work? Please explain.

Minor

Lines 90-95: changed format.