EGUsphere, author comment AC2
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Reply on RC1

Rafaello Bergonse et al.

Author comment on "Differentiating fire regimes and their biophysical drivers in Central Portugal" by Rafaello Bergonse et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-342-AC2, 2022

We thank the reviewer for the overall positive feedback and the pertinent suggestions provided. We respond to each point raised in detail below.

1) Methods: Figure 1: Please add LULC information on this figure (as an additional panel).

R: A LULC map will be included in a different panel, as suggested.

2) Methods: Line 77-79, Please briefly explain how the High and Very High wildfire hazard classes were determined.

R: When building their wildfire hazard map for the whole of mainland Portugal, Oliveira et al. (2020) defined class breaks based on the configuration of the success-rate curve. This curve was obtained by plotting the fraction of the territory by decreasing hazard level vs. the fraction of total actual burned area. This information will be inserted into the manuscript to clarify this point.

3) Methods: Line 108-109, "Prior research developed for the study area indicated an association between fire regime parameters and particular biophysical conditions (Bergonse et al., 2022)." Please remove as it was already mentioned.

R: The referenced phrase will be removed from the manuscript.

4) Methods: How do you justify using RFAJ and TPJS was calculated from monthly rainfall data obtained from the Worldclim database (reference 1970-2000)? Was the data used for the same period? Why not using a drought indicator like the standardized precipitation and evapotranspiration index (SPEI)?

R: We used the Worldclim database because it is, to our knowledge, the only database available covering most of our study period with a suitable resolution; for further justification regarding the different time scopes of the variables within our dataset, please see point 2 of our response to comment 6, below.

We employed summer temperature and spring precipitation because both variables are related to what we intended to represent (fuel flammability and potential for vegetation growth and posterior fuel availability). Moreover, they are simple and straightforward to interpret and their seasonal values are used to restrict specific activities that may cause fires, being therefore linked to prevention measures applied in the country. For example, the use of fire practices by farmers is usually forbidden after spring, related to temperature and rainfall/humidity thresholds of the season.

The use of spring precipitation as a proxy of the potential growth of vegetation and therefore fuel availability later in the year was suggested by the results previously obtained by Oliveira et al. (2012), described in lines 118-120 of our manuscript. Moreover, in our previous article we have observed spring rainfall to have an important positive influence both over burned area and wildfire frequency (please see Bergonse et al. 2022, Table 4).

The relation between summer temperature and fuel flammability was also justified based on the published literature mentioned in line 124 of the manuscript. The pertinency of this variable was also confirmed by the results of our previous article, in which summer temperature was also shown to have a positive influence over burned area and wildfire frequency (please see Bergonse et al. 2022, Table 4).

We agree that drought indicators such as SPEI could be valuable to uncover other patterns regarding wildfire drivers. Reference to such indicators will be included in the Discussion section, together with appropriate references from the literature.

5) Methods: Lines 127-129: How do you aggregate the information from the different LULC maps from the different years? You only mention how you aggregate the classes not the different years of information. How do you cope with the Land use change? Please clarify.

Lines 144-146: Please change these lines above as they answer to my question

R: The mentioned lines will be repositioned as suggested.

6) Methods: Lines 161-164: MAJOR CAVEAT: databases used for NPP and climate variables.

Why don't you use NPP from MODIS which reaches present-day? Why do you rely on precipitation and temperature data which account for a period between 1970-2000 knowing that the last years have been record years in this area (Turco et al., 2019; sousa et al., 2019) and that drought conditions have been increasing (Vicente-Serrano et al., 2014)?

Ruffault et al. (2020), identified fire weather regimes objectively by dynamic k-means clustering based on the values of the weather and climate variables associated with each wildfire record, namely, temperature, relative humidity, wind speed, DMC and DC. Their results show that fire risk is higher when short-term meteorological extremes (warm and dry air, strong winds) combine with long-term summer drought, i.e. under the Hot drought, Heatwave and Wind-driven fire weather regimes. Therefore, wind is one of the drivers which is highly correlated and should not be discarded as also pointed by Vieira et al. (2020), nor the combination of factors. Moreover, the authors highlight that the frequency of heat-induced fire-weather is projected to increase by 14% by the end of the century (2071–2100) under the RCP4.5 scenario, and by 30% under the RCP8.5, suggesting that the frequency and extent of large wildfires will increase throughout the Mediterranean Basin. Thus, using more recent data which can account for the latest years is important.

R: We will respond to each of the raised issues separately.

- We did use NPP from MODIS. However, we used the previous version (version 06), which was the one available when the dataset for the article was assembled. This can be verified by following the link included in the manuscript (line 149): https://lpdaac.usgs.gov/products/mod17a3hgfv006/).

The newer version of the MODIS dataset (061) is more up-to-date, as it extends to the present day. However, regarding our study period (1970-2018), that is, the period used to characterize the fire regimes) the newer MODIS dataset includes only four more years (2015-2018), which are unlikely to alter the general, long-term tendencies in which this article is focused.

- We relied on climate (precipitation and temperature) data for the period between 1970 and 2000 because data for the remainder of the study period (2001-2018) was, and remains, to our knowledge, unavailable. The differences between the period used to characterize the fire regimes and the periods of available data for the climate variables and Net Primary Productivity have been acknowledged in the *Data Collection and Pre-Processing* section (lines 160-164). As we also mention in those lines, our approach to fire regime is a long-term approach, that is, our purpose is to define general tendencies over a relatively long time period. Although record years are extremely important to understand in different contexts, they are not so important in relation to our approach, as they would detract from the general tendencies of the fire regimes we wish to characterize.

We will include a subsection in the Results and Discussion section highlighting the differences in the temporal scope of the various variables used as limitation of this work. We will also acknowledge the potential importance of prevailing wind conditions as a fire regime driver, which was not considered in this article.

7) Some additional important references on the topic focusing on the Mediterranean or the Iberian Peninsula:

Turco M, et al. On the key role of droughts in the dynamics of summer fires in Mediterranean Europe. Sci. Rep. 2017;7:81.

Turco M, et al. Climate drivers of the 2017 devastating fires in Portugal. Sci. Rep. 2019;9:13886

Ruffault J, Moron V, Trigo RM, Curt T. Objective identification of multiple large fire climatologies: An application to a Mediterranean ecosystem. Environ. Res. Lett. 2016;11:075006

Ruffault J, Curt T, Moron V, Trigo RM, Mouillot F, Koutsias N, Pimont F, Martin-StPaul N, Barbero R, Dupuy JL, Russo A, Belhadj-Khedher C. Increased likelihood of heat-induced large wildfires in the Mediterranean Basin. Sci Rep. 2020 Aug 14;10(1):13790. doi: 10.1038/s41598-020-70069-z. PMID: 32796945; PMCID: PMC7427790.

Identifying large fire weather typologies in the Iberian Peninsula, M Rodrigues, RM Trigo, C Vega-García, A Cardil - Agricultural and Forest Meteorology, 2020

Vieira I., Russo A., Trigo R.M. (2020) Identifying Local-Scale Weather Forcing Conditions Favorable to Generating Iberia's Largest Fires . Forests 11(5), 547

Sousa P., Barriopedro D., Ramos A.M., García-Herrera R., Espirito-Santo F., Trigo R.M. (2019) Saharan air intrusions as a relevant mechanism for Iberian heatwaves: The record breaking events of August 2018 and June 2019. Weather and Climate Exetremes, 26, 100224, DOI: http://doi.org/10.1016/j.wace.2019.100224

Vicente-Serrano S. M., Lopez-Moreno Juan-I., Beguería S., Lorenzo-Lacruz J., Sanchez-Lorenzo A., García-Ruiz J. M., Azorin-Molina C., Morán-Tejeda E., Revuelto J., Trigo R., Coelho F., Espejo F. (2014) Evidence of increasing drought severity caused by temperature rise in southern Europe. Environmental Research Letters, doi:10.1088/1748-9326/9/4/044001

How do the usage of more recent databases influence the results as temperature is rising and weather temperature extremes are mounting in this area? And the influence of wind? These need to be tested and compared.

R: We thank the reviewer for the relevant and interesting studies suggested. We will definitely consider them when we revise the manuscript.

As we mentioned in the answer to the previous comment, we did not use a more recent climate database because we are not aware of the availability of one. We will gladly update our results if a more recent database is available.

Although prevailing wind conditions are definitely a potential fire regime driver, wind was not considered in this study. Our intention was to consider the influence of a set of potential drivers, but not to be exhaustive in this regard. The potential role of drivers left out of this study, such as wind conditions, will be considered in a subsection of the Results and Discussion section.

8) Methods: when using the CT model you are using a spatial and temporal varying information to assess which is the most important variables in each of the 3-4 clusters? Or the information is aggregated spatially and then related? These options would rely on not so recent meteorological characterization and might not reflect the actual influence of temperature. How do you account for that?

R: We are not sure we understand the question, but we will outline the essential points of the CT model. For each of the studied parishes (our units of analysis), our dataset includes as attributes the associated cluster (i.e. type of fire regime) and the values of the potential fire regime drivers. Although, as mentioned in previous comments and responses, the temporal scopes of the fire regime descriptors (used to generate the clusters) and the different potential drivers vary somewhat due to data availability limitations (the contrasts in temporal scope were shown in Table 1 and acknowledged in lines 160-164), it is assumed that all values are equally descriptive of general conditions throughout an equivalent long-term period. The information is therefore aggregated spatially (at the parish scale), and then related using the CT model. This CT model was built to assess the capacity of the different biophysical drivers to differentiate between fire regimes, that is, to correctly identify the fire regime each parish is associated to.

It is important to highlight that all biophysical drivers are intended to describe prevailing conditions, and do not take into consideration extremes. For example, we employed mean summer temperature to consider the effect of typical summer conditions, therefore disregarding the effect of exceptional, and thus relatively infrequent, years.

9) Discussion: Citing authors previous works on the same area and with similar approach is not a strong comparison. I would suggest the authors to look for similar results from other authors or different areas to support this point (e.g., lines 338, 345).

R: We will do as suggested.

10) Discussion: Lines 409-410: The authors say that "It is therefore possible that the potential effects of summer temperature in burned area are constrained by fuel availability". As the authors certainly know from the basics of the fire triangle or combustion triangle, which is a simple model for understanding the necessary ingredients for most fires, three elements are needed for a fire to ignite: heat, fuel, and an oxidizing agent (usually oxygen). A fire naturally occurs when the elements are present and combined in the right mixture. A fire can be prevented or extinguished by removing any one of the elements in the fire triangle. Therefore, we can have all the necessary weather and vegetation conditions but if we don't have ignitions, although the fire weather risk is high, the fire might not even start. Here the authors need to check for the presence of the conditions and not just suggest a possibility.

R: In the excerpt mentioned by the reviewer, we are indicating potential explanations for the fact that summer temperature, a well-known wildfire driver, was shown not to have a relevant role in our study area. Fuel availability is one possible explanation, as well as a lack of ignitions.

We will reformulate the phrase to read: "It is therefore possible that the potential effects of summer temperature in burned area are constrained by other factors, such as fuel availability or the inexistence of ignitions." Regarding the former, prior studies mention that the effect of temperature is mediated by the productivity of an area (Pausas e Fernández-Muñoz, 2012; Pausas & Ribeiro, 2013). When fuel is limited, the effect of the temperature is less expressive.

11) The authors lack to show the limitations of the data used and also other aspects which were not addressed in their study.

R: The limitation of the data used are already acknowledged in lines 160-164. We will address this issue together with the potential importance of other variables not considered in this study (such as wind conditions) in a future subsection of the Results and Discussion section.

12) The authors don't highlight how the conclusions on their current and previous work (Bergonse et al., 2022) are different.

Rafaello Bergonse, Sandra Oliveira, José Luís Zêzere, Francisco Moreira, Paulo Flores Ribeiro, Miguel Leal, José Manuel Lima e Santos, Biophysical controls over fire regime properties in Central Portugal, Science of The Total Environment, Volume 810, 2022, 152314, ISSN 0048-9697, https://doi.org/10.1016/j.scitotenv.2021.152314.

R: Both works share mostly the same dataset and approach, namely parishes as units of analysis, the same three fire regime descriptors, and a common set of potential biophysical drivers. However, they have different objectives, and therefore complementary conclusions. We therefore believe highlighting differences in the conclusions of both works would serve no useful purpose.

In the previous work, relations between each biophysical driver and each of the three fire regime descriptors were analysed using ordinal regression. A single fire regime was assumed for the whole study area, and the results suggested the existence of different fire regimes. This description of the previous work is given in lines 57-64 of the Introduction.

Contrarily, in this more recent work we use cluster analysis to distinguish different fire regimes, we characterize them, and then we use a classification tree model to assess the capacity of the biophysical factors to distinguish between the regimes. Due to the complementarity between both works, we draw on the relations found in the previous article between each fire regime descriptor and different biophysical factors to inform our discussion of the results of CT model (e.g. lines 345, 363, 367).

13) The usage of NPP and temperature seem to be mostly disregarded in the discussion. Justify the usage and discuss their importance.

R: The usage of NPP and temperature is justified, respectively, in lines 147, and lines 123-124 of the Data Collection and Pre-Processing section. Both variables are also considered in the Discussion section. NPP values are discussed in relation to the defined fire regimes (lines 361, 383) and in relation to the implication of these fire regimes to wildfire management (line 433). The importance of temperature is considered in lines 401-412 of the Discussion.

14) When applying for the classification tree model, the importance of each of the parameters is determined based on a linear or non-linear relation? How does that affect the identification of the most important factors?

R: Classification trees are produced by successive binary partitioning, or splitting, of the training data into a growing number of subsets (nodes). Each split is based on a binary condition, defined using the predictor variable (the splitter) that maximizes the homogeneity, or inversely, minimizes the impurity, of the two resulting nodes. In our case, this homogeneity was measured using the GINI criterion, which is based on squared probabilities of membership for each category of the dependent variable (i.e. each of the four fire regimes). GINI reaches its minimum (zero) when all cases in a node fall into a single fire regime.

Each split results in an improvement, which is calculated by comparing the homogeneity of the two resulting nodes with that of the original node. This improvement is attributed to the splitting variable. The importance of each variable for the overall classification procedure is based on the sum of the improvements in all nodes in which the variable appears as a splitter, weighted by the fraction of the training data in each node split (Steinberg, 2009).

We acknowledge that the criteria for quantifying the overall importance of each predictor variable is unclear in the manuscript. We will therefore integrate the above information into the Data Collection and Pre-Processing section, together with the additional reference below.

Steinberg, D. (2009). CART: Classification and Regression Trees. In X. Wu & V. Kumar (Eds.), *TheTop Ten Algorythms in Data Mining* (pp. 179–201). CRC Press.