RC1: 'Comment on nhess-2021-173', Anonymous Referee #1, 04 Aug 2021

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Anonymous Referee #1: The paper "Spatial variability in the relation between fire weather and burned area: patterns and drivers in Portugal" aims at analysing large (>100 ha) summer forest fires in mainland Portugal. Although the dataset is sound and of potential interest, I found several flaws in the methods and results interpretations. The use of statistics is not always appropriate, expecially the use of OA, UA and K (they are used for classification accuracy against a reference.

Answer: We want to start by thanking the reviewer; first, for the work and time spent on reviewing our manuscript, as we recognize the difficulties that the editors have in finding good reviewers; second because your comments and suggestions were very helpful in clarifying and strengthening the manuscript.

The reviewer states that the objective of this work was to "analyze large (>100 ha) summer forest fires in mainland Portugal". We comprehend that the objectives were not clear and clarified the three objectives of this study in lines 2-4 (Abstract) and lines 95-99 (Introduction) were to 1) assess if the DSR90p threshold is adequate to identify the bulk of burned area (BA) for mainland Portugal; 2) identify and characterize regional variations of the DSRp threshold that justify the bulk of BA, and; 3) analyze if vegetation cover can explain the spatial variability of the DSRp threshold.

We do not agree with the reviewer when he states that the use of OA, UA and K were not appropriate. Effectively, these statistics were used for classification accuracy against a reference as the reviewer proposes. This was explained in the section 2.4, together with a detailed explanation of the accuracy metrics and the citations of previous articles that defined and used the same statistics in similar conditions/situations. However, we agree that this reference may be insufficient and provide a better explanation. Therefore, we changed the manuscript accordingly, in lines 216-232. Additionally, we improved the explanation and justification of these results, including more references, in lines 361-367.

We appreciate the reviewer's acknowledgement of the quality of the datasets. On one hand, the wildfire and land use data used in this manuscript are Portuguese official datasets, produced and provided by Portuguese national authorities. These datasets were used in many other studies, by a large number of authors for a wide variety of purposes (Bergonse *et al*., 2021, Tarín-Carrasco *et al*., 2021). On the other hand, the ERA5 is recognized as the best or one of the best global atmospheric reanalysis datasets (Huai et al., 2021, Muñoz-Sabater *et al*., 2021, Urban *et al*., 2021) and used worldwide (Chinita *et al*, 2021, Sianturi *et al*., 2020). Therefore, it is one of the most used meteorological datasets in the world. We added this information in the manuscript, in lines 121-127.

Citations included in the manuscript:

Bergonse, R., Oliveira, S., Gonçalves, A., Nunes, S., DaCamara, C. and Zêzere, J.L.: (2021) Predicting burnt areas during the summer season in Portugal by combining wildfire susceptibility and spring meteorological conditions, Geomatics, Natural Hazards and Risk, 12:1, 1039-1057, DOI: 10.1080/19475705.2021.1909664, 2021.

Chinita, M.J., Richardson, M., Teixeira, J. and Miranda, P.M.A.: Global mean frequency increases of daily and sub-daily heavy precipitation in ERA5. Environ. Res. Lett., 16, 074045, https://doi.org/10.1088/1748-9326/ac0caa, 2021.

Huai, B., Wang, J., Sun, W., Wang, Y. and Zhang, W.: Evaluation of the near-surface climate of the recent global atmospheric reanalysis for Qilian Mountains, Qinghai-Tibet Plateau, Atmospheric Research, 250, https://doi.org/10.1016/j.atmosres.2020.105401, 2021.

Muñoz-Sabater, J., Dutra, E., Agustí-Panareda, A., Albergel, C., Arduini, G., Balsamo, G., Boussetta, S., Choulga, M., Harrigan, S., Hersbach, H., Martens, B., Miralles, D. G., Piles, M., Rodríguez-Fernández, N. J., Zsoter, E., Buontempo, C., and Thépaut, J.-N.: ERA5-Land: a state-of-the-art global reanalysis dataset for land applications, Earth Syst. Sci. Data, 13, 4349–4383, https://doi.org/10.5194/essd-13-4349-2021, 2021.

Sianturi, Y., Marjuki and Sartika, K.: Evaluation of ERA5 and MERRA2 reanalyses to estimate solar irradiance using ground observations over Indonesia region. AIP Conference Proceedings 2223, 020002, https://doi.org/10.1063/5.0000854, 2020.

Tarín-Carrasco, P., Augusto, S., Palacios-Peña, L., Ratola, N. and Jiménez-Guerrero P.: Impact of large wildfires on PM10 levels and human mortality in Portugal. Nat. Hazards Earth Syst. Sci., 21, 2867–2880, https://doi.org/10.5194/nhess-21-2867-2021, 2021.

Urban, A., Di Napoli, C., Cloke, H. L., Kyselý, J., Pappenberger, F., Sera, F., Schneider, R., Vicedo-Cabrera, A.M., Acquaotta, F., Ragettli, M.S., Íñiguez, C., Tobias, A., Indermitte, E., Orru, H., Jaakkola, J.J.K., Niilo, R.I. R., Pascal, M., Huber, V., Schneider, A., de' Donato, F., Michelozzi, P., Gasparrini, A.: Evaluation of the ERA5 reanalysis-based Universal Thermal Climate Index on mortality data in Europe, Environmental Research, Volume 198, 111227, ISSN 0013-9351, https://doi.org/10.1016/j.envres.2021.111227, 2021.

Anonymous Referee #1: Furthermore no analyses on the spatial pattern are provided, simply a description of the maps. The relationships between LULC and weather and fire occurence need to be deepen analysed. The authors need to clarify and explain better the reason why they divided in cluster the dataset, the significant differences between groups,.... Other multivariate analysis methods can be adopted considering more variables (e.g. geographical gradients, inhabitants,...) in order to obtain a strong explanatory analysis and then building relationships or models to be tested.

Answer: We do not agree with the first phrase in this comment of the reviewer. First, we have some difficulty understanding it, because the reviewer does not specify what spatial pattern he refers to. Second, any spatial pattern that the reviewer may refer to are (the maps of) the results obtained with the selected methods that, obviously have to be described and discussed (in the adequate sections), but they do not necessarily have to be subject to additional analysis with another methodology, because they are obvious and easy to interpret. Third, the spatial pattern of the DSRp was deeply analyzed using cluster analysis, with an explanation of the dendrogram based on the correlation coefficient between FTBA and DSRp in each of the municipalities (Figure 7); and, additionally, showing and explaining the differences of the curve FTBA vs DSRp, in each cluster (Figure 9). We also validated the relationship between the LULC and the DSRp (and, consequently, with the clusters) using the contingency tables and statistical tests. We consider that this analysis was sufficient to explain and justify the results of this manuscript. However, we understand that the explanation was not sufficient and we will present additional justifications in the following lines and also in the manuscript.

It is important to underline that this study is not about the relationship between LULC and weather and fire occurrence. In summary, this study is about the relationship between extreme fire weather and high/extreme burnt area which slightly change spatially due to LULC. The relationship between fire weather and the fire incidence has been analyzed by many researchers for several years, as mentioned in the manuscript (please, see lines 17-27, 380-385). The role of LULC in the incidence of fire has also been analyzed and some references on this subject are cited in the manuscript. The authors of this manuscript are also authors of articles published on these relationships, but many more references could be provided. However, in this study, we intended to carry out a deeper analysis between fire weather and the incidence of fire and ended up discovering that this relationship depends on LULC, but does not depend on any of the other more important factors of the fire incidence, as described in lines 246-259, and also motivated us for the analysis described in sections 3.2, 3.3 and 3.4.

It should be noted that, while LULC, topography, population statistics, etc. are structural (essentially fixed or stationary) wildfire hazard factors, the meteorological conditions are conjunctural (essentially variable or dynamic) wildfire hazard factors. Despite a few space-time analyses (e.g., Orozco et al., 2012; Pereira et al. 2015, Parente et al., 2016), usually, and for obvious reasons, the influence of these two types of factors on the fire incidence is studied separately.

However, it was precisely as a result of an in-depth analysis of the relationship between extreme fire weather (specifically DSRp) and fire incidence (specifically the burnt area) that it was possible to conclude that LULC - a structural factor - influences the impacts of meteorological conditions - a conjectural factor of fire risk. As far as we know, this is the first study that identifies and establishes that the relationship between fire weather and fire incidence depends on LULC, for Portugal.

It is also important to underline that, to establish this relationship, we used objective methods and adequate statistics that ensure the robustness and statistical significance of the results. The description of the study carried out also includes the chronology of the performed analysis. In a previous study/paper (Calheiros et al., 2020), the relationship between fire weather and fire incidence was analyzed in-depth for the entire Iberian Peninsula. Among other results, we found that the DSR90p is a good indicator of extreme fire weather and well related to the burnt area in the Iberian Peninsula, as mentioned in lines 47-49. In this study, we started by verifying whether the relationship between DSRp and BA found, in general terms, for the Iberian Peninsula, was also verified in Mainland Portugal, at municipality level, and what is the spatial variability of the extreme value of DSRp above which most of the burned area is registered (objective 1 and 2 of this manuscript). To assess the spatial variability, we based our analysis on the 278 municipalities of mainland Portugal. To objectively interpret the obtained spatial patterns (Figure 5), we complemented and deepened the analysis with the use of clustering algorithms, to classify the municipalities into statistically different groups in terms of the relationship between FTBA and DSRp. Based on our knowledge and experience, we tested all the most likely factors that could help to explain the obtained results. The emerging patterns showed that all of those most likely factors, such as topography, altitude (Figure 1); slope (please see Figure 5 of Parente and Pereira (2016)); population density (please see Figure 2 of Pereira et al.(2011) or Figure 2 of Parente and Pereira (2016)); rural and urban area type (please see Figure 3 of Pereira et al. (2011)); road density/distance to the nearest road (please see Figure 2a of Parente et al.(2018)) and climate type (please see Figure 1a of Parente et al.(2016)) were not able to explain the obtained spatial patterns. The only factor with a similar spatial pattern was the LULC, which is the reason why we decide to explore this possibility more deeply, with contingency tables and several accuracy metrics to assess the influence of the type of vegetation cover on the relationship between DSRp and TBA (as described in lines 209-232). Therefore, we agree that the explanation was not clear in the text, so we changed the manuscript accordingly, regarding this discussion/subject in lines 430-456 (Discussion section).

One of the objectives of this manuscript was to identify and characterize regional variations of the DSRp threshold that justifies the bulk of BA. The analysis was performed on the spatial basis of the municipalities. The high number (278) of these administrative regions difficults the interpretation of the results and the objective and statistically significant assessment of differences between the results obtained for different municipalities. The cluster analysis was performed to execute and simplify this task, but also to identify the major macro scale patterns. We agree with the reviewer that this procedure was not clear and added a phrase to clarify and explain better the reason why we decide to use cluster analysis in subchapter 2.3, in lines 179-181. We want to underline that the significant differences between groups were clarified and explained along with the dendrogram, in Figure 7, the spatial distribution of the clusters, in Figure 8, as well as in the text, especially in subchapter 3.3.

Furthermore, we improved the explanation of the dendrogram and clustering procedure as follows:

The following notation describes the linkages (the distance between two clusters) used in the *complete* clustering method (MathWorks, Inc.):

- Cluster *r* is formed from clusters *p* and *q*.
- n_r is the number of objects in cluster *r*.
- x_{ri} is the *i*th object in cluster *r*.
- *Complete linkage*, also called the *farthest neighbour*, uses the largest distance between objects in the two clusters (Eq.1).

$$d(r,s) = max\left(dist\left(x_{ri}, x_{sj}\right)\right), i \in (1, \dots, n_r), j \in (1, \dots, n_s) \quad (1)$$

A distance metric is a function that defines a distance between two observations. The Matlab function *pdist* used in this study, that computes the pairwise distance between pairs of observations, supports various distance metrics, namely: Euclidean distance, standardized Euclidean distance, Mahalanobis distance, city block distance, Minkowski distance, Chebychev distance, cosine distance, correlation distance, Hamming distance, Jaccard distance, and Spearman distance. We used the correlation distance in this article:

Given an *m*-by-*n* data matrix X, which is treated as *m* (1-by-*n*) row vectors x_1, x_2, ..., x_m, the correlation distance between the vector x_s and x_t are defined as in Eq.2:

$$d_{st} = 1 - \frac{(x_s - \overline{x_s})(x_t - \overline{x_t})'}{\sqrt{(x_s - \overline{x_s})(x_s - \overline{x_s})'}\sqrt{(x_t - \overline{x_t})(x_t - \overline{x_t})'}}, \quad (2)$$

where $\overline{x_s}$ is described in Eq.3:

$$\overline{x_s} = \frac{1}{n}\sum_j x_{sj} \; and \; \overline{x_t} = \frac{1}{n}\sum_j x_{tj}. \quad (3)$$

The selected (1-r2) threshold was 0.35, meaning that the coefficient of determination in the municipalities within the same cluster is higher than 0.65. This value was selected after the analysis of the dendrogram and results from the balance between the correlation between municipalities and the total number of clusters. For example, on one hand, if we have chosen 5 clusters, the correspondent correlation between municipalities within the same cluster will be larger than 0.5, a value that we considered too low for this analysis. On the other hand, for a higher correlation, for example, 0.75, which corresponds to 1-r2=0.25, the number of clusters will be much higher, increasing the difficulty of interpreting the maps and dendrogram. The explanation of the dendrogram and clustering procedure is provided in the new version of the manuscript (in lines 182-204).

Finally, as appointed in manuscript (lines 41-44), we want to highling that: “Cluster analysis for the Iberian Peninsula has identified several regions with similar fire regimes, using several variables related to fire, as intra-annual pattern of burnt area (Trigo et al., 2016; Calheiros et al., 2020; Calheiros et al., 2021), fire activity and weather risk (Jimenez-Ruano et al., 2018), large fire-weather typologies (Rodrigues et al., 2020) or burnt area tendency (Silva et al., 2019)”.

Citations included in a new version of the manuscript:

Orozco, C. V., Tonini, M., Conedera, M., & Kanveski, M. (2012). Cluster recognition in spatial-temporal sequences: the case of forest fires. *Geoinformatica*, 16(4), 653-673.

(The other citations were already listed in the manuscript)

Anonymous Referee #1: The discussion section is mainly a list of municipalities in the different groups or sentences related to parameters not considered in the study. The first part is belonging to methods.

Answer: We believe that the previous version of "Discussion" fulfils what is expected in this section of an article, namely justifying, validating and interpreting options (data and methodology) and results, based on the findings of previous studies. In the submitted version of the manuscript, we started by discussing the methodological options (that we moved to the Methods, as suggested by the reviewer, now in lines 157-166). Then we discussed the characteristics (including the limitations) of the LULC dataset and the potential impacts on our study (now in Methods, lines 167-176). Then, we discussed the obtained results, presenting the justification, interpretation and validation of the findings, in line with previous studies (now in lines 380-429). As the study was carried out using municipalities as a spatial unit, we assumed that it is expected that there will be references to some municipalities. However, these references to the municipalities only occur in 4 of the previous 68 lines of the discussion, which represents less than 6% of the total number of lines of the discussion. Nevertheless, we made changes to the discussion section (also deleted the previous references to municipalities) and add paragraphs regarding other issues appointed by the reviewer. The new paragraphs of the discussion are in lines 430-456.

We agree with the reviewer that the first part of the discussion belongs to the Methods section and, therefore, we changed the manuscript accordingly, in lines 157-176.

Anonymous Referee #1: The conclusion is redundant, repeating results and discussion elements.

Answer: We agree that the text has repeated ideas, results and conclusions. Consequently, we changed this section according to the suggestion of the reviewer, now in lines 458-479.

RC2: 'Comment on nhess-2021-173', Anonymous Referee #2, 28 Oct 2021

Citation: https://doi.org/10.5194/nhess-2021-173-RC2

Anonymous Referee #2: The manuscript from Calheiros et al is very difficult to read. It reads as a series of bullet points linked together and the authors did not even bother at breaking the text into paragraphs or making any effort to increase readability. This is not a result of the authors not being native English speakers (I believe paragraphs also exist in Portuguese) but rather denotes a major lack of attention to detail.

Answer: First of all, we want to thank the reviewer for his pertinent commentaries that contribute to improving this manuscript. We deeply regret the absence of paragraphs. The manuscript was written with paragraph breaks. We suspect that paragraphs were deleted by the software used for the submission of the manuscript. We corrected it in the new version of the manuscript.

Regarding this subject, we also changed the Introduction. It is important to refer that this modification did not change the subject matter, only changed the way of writing. The Introduction is now in lines 14-99.

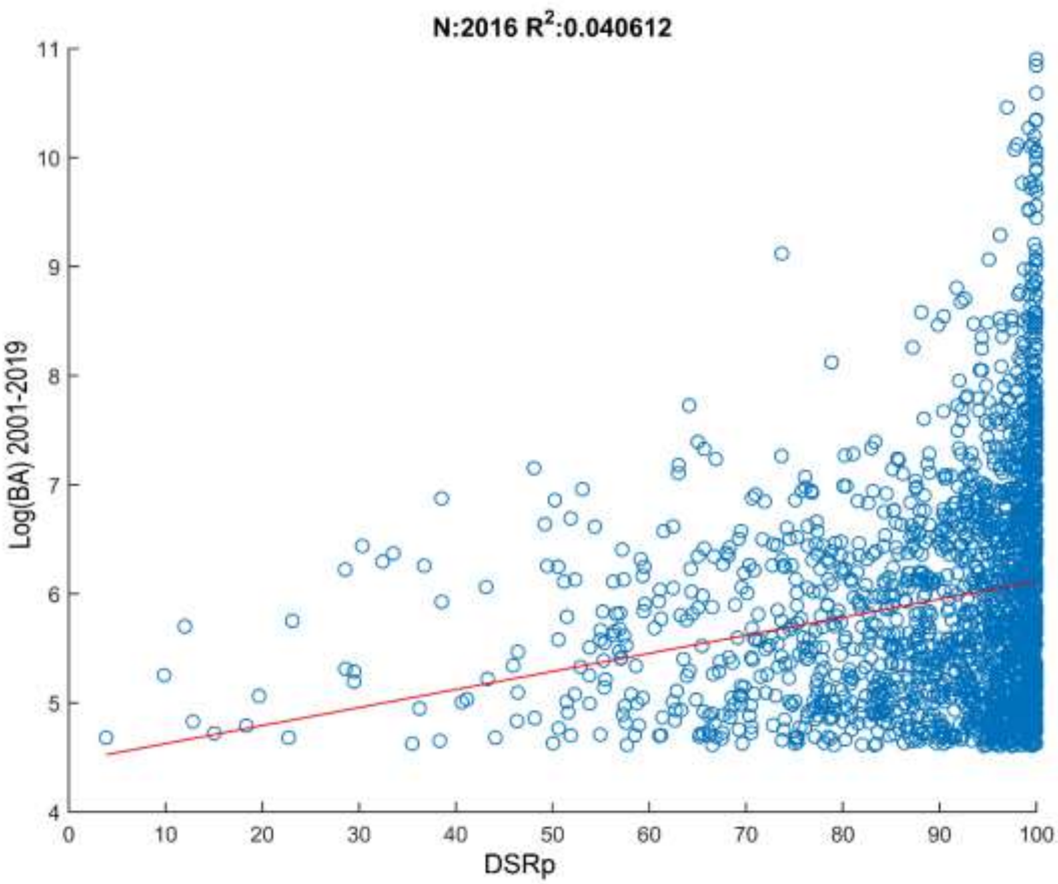
Anonymous Referee #2: The authors examine thresholds in burnt area associated with DSRp and how they differ across Portugal. The way they present the data is somewhat misleading: they make us believe that DSR has a very high correlation with burnt area. These types of correlations have been described before and they result from the ordering of values (from small to large). If that order is removed and simple scatter plot of burnt area DSR is presented, that relationship usually breaks, or is much weaker. I would thus encourage the authors to be more careful when using these types of analyses.

Answer: We disagree with the reviewer but we believe that the manuscript does not explain clearly this subject, so we change it accordingly. In particular, we agree that the DSR vs BA scatter plot does not reveal a simple robust relationship between these two variables. Please see the figure below (also added to the manuscript – Figure 2), where the logarithm of the burnt areas - Log(BA) - is plotted against the percentiles of DSR. This is due to several reasons (e.g., ignition source, firefighting activities, geographical/landscape features, fire barriers, limitations of the Fire Weather Index System to represent the role of fire weather drivers, humidity of live fuel moisture and the convective influence in fire behaviour, etc.) but, in essence, the most important one is that the wildfire activity does not only depend on the weather. This means that: (i) wildfires can occur in days with relatively low values of DSR; (ii) small wildfires can occur in days of high DSR, due to rapid fire-suppression activities or other constraints (especially fuel). However, it is well known that extreme wildfires only occur in days of extreme fire weather (Fernandes et al., 2016). These facts are validated by our results, revealing that only 6% of the Total Burnt Area (TBA) occurs with DSRp<80 and 12% of TBA is registered in wildfires with DSRp<90. These reasons explain all the main features of the figure below, namely: small wildfires are registered in days with almost all values of DSR, although the much small number of wildfires in the lower left quarter of the plot area, and the huge number of events near the right vertical axis, especially for DSR>DSR90p. In effect, DSR seems to act as an upper limit to the maximum burnt area. It is precisely the relationship between the burnt area and the DSR in this “region” of the plot that is investigated in this study. This is clearly explained in the manuscript and illustrated in Figure 3 and Figure 4. We added this clarification in the manuscript, in lines 369-379.

Furthermore, we'd like to add that cumulative statistics are commonly used, including in wildfire science. See, for example, Cumming, S. G. (2001). A parametric model of the fire-size distribution. Canadian Journal of Forest Research, 31(8), 1297-1303.

Jiang, Y., & Zhuang, Q. (2011). Extreme value analysis of wildfires in Canadian boreal forest ecosystems. Canadian journal of forest research, 41(9), 1836-1851.

Kanevski, M., & Pereira, M. G. (2017). Local fractality: The case of forest fires in Portugal. Physica A: Statistical Mechanics and its Applications, 479, 400-410.



Anonymous Referee #2: I'm not familiar with the clustering techniques used by the authors, and I will not comment on those. I will just point out that the results are rather shocking because pretty much all clusters are distributed across all Portugal, but it is well known that fires in N PT differ substantially from S PT (the authors actually state this in their introduction as well).

Answer: A better explanation of the purpose of cluster analysis and the aim of its application in this study can help understand the results.

We want to clarify the three objectives of this study (as stated in lines 2-4 of the Abstract and lines 95-99 in the Introduction) were to “1) assess if the DSR90p threshold is adequate to identify the bulk of burned area (BA) for mainland Portugal; 2) identify and characterize regional variations of the DSRp threshold that justify the bulk of BA, and; 3) analyze if vegetation cover can explain the spatial variability of the DSRp threshold “. If we had performed a cluster analysis on the number of fires or burnt areas, the results would be clusters in regions where the incidence of fire is higher, ie in the central-north and extreme south region (Algarve) as the reviewer suggests. Results of this type of study can be consulted, for example, in:

Pereira, M. G., Caramelo, L., Orozco, C. V., Costa, R., & Tonini, M. (2015). Space-time clustering analysis performance of an aggregated dataset: The case of wildfires in Portugal. Environmental Modelling & Software, 72, 239-249.

Parente, J., Pereira, M. G., & Tonini, M. (2016). Space-time clustering analysis of wildfires: The influence of dataset characteristics, fire prevention policy decisions, weather and climate. Science of the total environment, 559, 151-165.

Kanevski, M., & Pereira, M. G. (2017). Local fractality: The case of forest fires in Portugal. Physica A: Statistical Mechanics and its Applications, 479, 400-410.

Some of these papers are already cited in the manuscript.

However, as explained in the manuscript in lines 6-7 and now in lines 178-181, the cluster analysis was motivated by the “spatial distribution of DSRp80TBA and DSRp90TBA” (lines 258-259), using a methodology described in section 2.3 (now in lines 179-205), “based on the DSRp vs FTBA curves aimed to find groups of municipalities with similar fire-weather relation” (lines 412-413) i.e., to group the municipalities that present similar relationship between DSRp and TBA and help to explain “some important differences appear among DSRp thresholds that explain 90 and 80% of the TBA” (lines 460-461). The results were extensively described in section 3.3 (lines 290-328) which allow us to easily understand the purpose of having performed this analysis. In summary, the cluster analysis “revealed that municipalities where large wildfires occur in high DSRp present higher BA in forests and are located in coastal areas. In contrast, clusters with lower DSRp present greater BA in shrublands and are situated in eastern regions.” (lines 10-12).

Regarding the pertinent reviewer comment, we improved the explanation of the cluster analysis, in lines 182-204.