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Comment on nhess-2021-174

Anonymous Referee #2

Referee comment on "Environmental Factors Affecting Wildfire Burned Area In South-Eastern France, 1970–2019" by Christos Bountzouklis et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-174-RC2, 2021

n this manuscript, the authors investigate whether (and if yes, how) topography and vegetation types affect the probability of burning in three administrative regions located in south-eastern France over the period from 1970 to 2019. Their main objective is to determine whether the proportion of fires that burn a certain land cover type or topographical configuration (disproportionally to their availability) has changed over time as a consequence of the implementation of a new fire policy that took place in France in the early 1990s and showed high efficiency at reducing burnt areas and fire numbers. For this purpose, they analyze a long-term georeferenced fire-shape dataset (which in my knowledge has never been analyzed but by Ganteaume and Barbero 2018, doi.org/10.5194/nhess-19-1055-2019) and compared this dataset with LULC data over time. The main results show the increasing importance of south-facing slope over time and higher burned area in Sclerophyllous vegetation than in other vegetation types.

While the subject of this manuscript is of interest to the fire community and the data gathered sounds relevant to answer the general hypotheses outlined here above, I have a number of broad concerns and comments that should be carefully addressed before I feel comfortable recommending this paper for publication. My opinion is that the manuscript suffers from serious flaws, including (i) inappropriate analyses of the data and over-interpretation of the results, (ii) some doubts regarding the homogeneity of the wildfire database that might lead to some bias in the interpretation of the results, (iii) the need to better use the literature to introduce and discuss the hypothesis and results of this work. To put it clearly, I think that the issues raised in this paper and data gathered to answer them are promising but it is most likely that addressing these concerns would require an important amount of work, including new analyses and a great amount of rewriting. I detail below these three main concerns as well as other comments I noted while reading this manuscript. As it is my belief that this manuscript would require important rewriting, I did not feel it would be relevant to provide, at this stage, a list of very detailed comments.

(i) Perhaps, my main concern regards the analyses whose results are shown in Fig, 8,9,10, 11, 12, and 13) and that do not seem appropriate for the objectives raised in this paper. Indeed, despite an overall agreement of your results with the literature, there is no guarantee that the conclusions drawn from your current analyses are not affected by other drivers of fire spread, such as fuel connectivity, weather conditions, and many other

possible interactions including those that might occur between your studied factors. The presentation of results is very convoluted (see for instance the section from L261 to L279 that is very difficult to follow) and no framework currently rigorously demonstrates that LULC influence is significant from a statistical point of view. In light of these remarks, it seems essential to rethink the analytical framework to ensure that these three points are correctly addressed by developing a more appropriate analytical framework that takes into account (or at least minimizes) these interactions and presents the results in a more relevant manner. One interesting possibility would be to determine whether the observed values of fire selectivity are significantly different from those that would be observed if fires were to occur at random locations in the landscape. I would advise you to look at the methodologies shown in Moreno et al. (2011) and Barros et al. (2014) that seem relevant because they assess fire selectivity through a null-based model that is independent of spatial relationships or any bias of fire data. Note that other approaches might also be relevant. Besides I found that the approach developed in Fig 7 and the conclusions drawn from these analyses were highly descriptive and speculative (see L224-L255). No analyses on the spatial pattern are provided, simply a description of the maps. These analyses should therefore also be improved to strengthen the results and their interpretation.

Barros, A. M., & Pereira, J. M. (2014). Wildfire selectivity for land cover type: does size matter?. PloS one, 9(1), e84760.

Moreno, J. M., Viedma, O., Zavala, G., & Luna, B. (2011). Landscape variables influencing forest fires in central Spain. International Journal of Wildland Fire, 20(5), 678-689.

(ii) I was surprised by the results in Fig 6 showing no trend in fire numbers, as it differs from the decreasing trend generally reported for this area see for instance Fig. 2 in Curt and Fréjaville, 2018). A possible explanation for such a discrepancy is that your database could be biased by a non-constant reporting of fires over time, which in turn might affect your results and conclusions. What I suspect is that a higher proportion of fires are now reported compared to what it was in the early 19070's especially for the smallest fires. To reduce the potential biases induced by this inconsistency, one solution would be to compare your dataset against the french promethee fire database for fires > 1ha (that is currently considered to be a relevant and robust fire size threshold to study fire ignitions in France, Pimont et al. 2021) in order to determine the fire size threshold above which you consider your database is not affected by an evolution of detection /reporting over time. Furthermore, I think that this manuscript would really benefit from including, somehow, a size factor in the analytical framework. Indeed, it would be relevant to test whether under severe fire weather, fires are expected to become larger and less dependent on land cover, which is generally reported in southern Europe.

(iii) I found that references to previous studies were not enough detailed, sometimes vague, or even missing and my opinion is that this manuscript could be greatly improved on this matter. As mentioned in one of my previous comments, several important papers, whose vast majority address fire regimes over the Iberian Peninsula, investigate the impact of LULC on fires but were not cited in this manuscript, including for instance Carmo et al. (2011), Bajocco et al. (2008), Moreno et al. (2011), Nunes et al. (2005), Koutsias et al. (2012) among many others. Furthermore, like any other area, the French Mediterranean region has its own peculiar context and history regarding fire regimes, whose description and analyses could also be improved. For instance, in France, the papers from Fréjaville and Curt (2016), Ruffault and Mouillot (2015), Ruffault et al. (2016), and Evin et al. (2018) studied the consequences of the introduction of this new fire policy on different metrics. Some of their results and discussions might provide relevant results for your discussion and the building of your hypotheses. There are a few papers also that have explored the drivers of spatial fire weather and or fire hazard in Southern France that might help, including the works from Ruffault *et al*. (2017) and Pimont *et al.* (2021). Note that this is by no means a list of papers that need to be cited

but rather some references that the authors you might find useful to improve the quality of your manuscript.

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