

Dear reviewers,

We would like to express our gratitude once again for your comments and suggestions that have further improved the manuscript.

Below are our responses to the second iteration of reviews. Reviewers' comments are in black italic font and our answers are in blue bold font and are placed right below each comment.

RC1 (Anonymous Referee #1)

Dear authors:

You have answered correctly the questions raised in the peer-review process; and consequently, the manuscript has improved. Nevertheless, there are some minor issues that must be accomplished before publication:

There are several typos and English language missuses in the paper that need to be revised. For example, vegetation types are in upper letters; and the description of the equation in line 290 must be consistent (for example, I or i (for location)); among others.

Thank you for bringing up these issues. The text has been read and corrected. All categorical variables were given capital letters in order to be consistent.

RC2 (Anonymous Referee #2)

It is my second review of this manuscript. I would like to thank the authors for their relevant and detailed responses to my last comments. The modifications made by the authors improved the quality and readability of the manuscript. However, the manuscript requires a significant rewriting before I feel comfortable recommending it for publication. I detail below the points that I would like to be addressed by the authors.

- The introduction of a selectivity index (Jacob's index) undoubtedly strengthened the manuscript results compared to the previous version. But as applied in the present study, this approach also has two main drawbacks that should be either solved (by refining analyses) or (at the very least) discussed in a dedicated section. First, a working hypothesis of the authors is that the study is "relatively small", and they assume that "the available area to burn is defined as the total forested area in the region" (L255). I disagree with this assumption because it implies that all other fire drivers (fire weather, vegetation structure for a given class, land use) should be considered constant. I was under the impression that this is not the case in southern France, where climate and land use are spatially heterogeneous and characterized by steep gradients. It also contradicts the author's conclusion regarding the effect of coniferous type on BA (L583-584). The largest fires account for much of BA and generally occur under severe fire weather (under which bottom factors play a weaker role than in milder fire weather). I, therefore, expect the effect of land-cover to be underestimated. Both biases (and others not discussed here) should be deeply addressed in a dedicated section. I encourage the authors to add a section in the discussion.

Thank you for your detailed reasoning behind this point. Even though there is no universal rule defining the extent of the available area for a fire to burn, we decided to recalculate Jacob's index in a manner similar to Barros and Pereira, 2014 and Nunes et al., 2005, thus making the results comparable to those of similar studies. For each fire the available area to burn equals to twice the amount of area burnt.

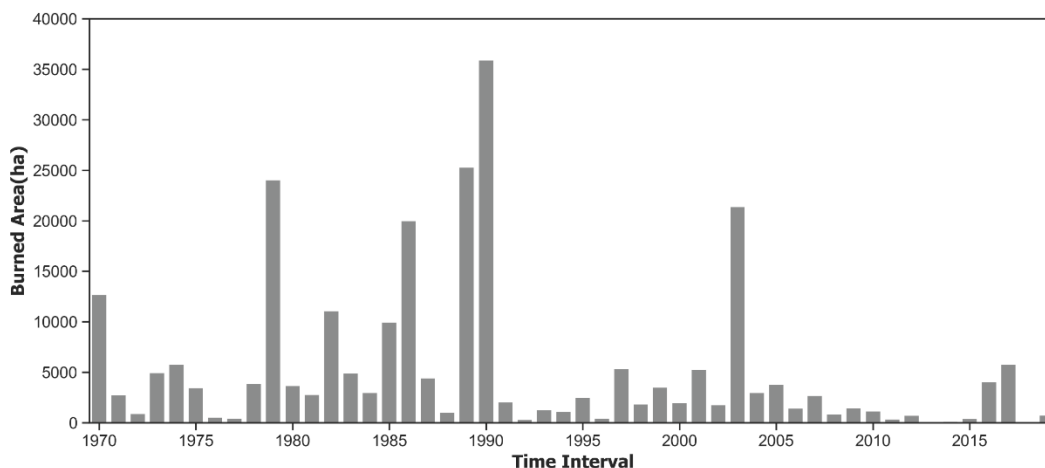
a buffer zone around each fire perimeter that equals

- Despite the efforts made by the authors to strengthen the analyses of the spatial trends in fire activity in their study area, I remain unconvinced by the relevancy of this analysis (Figs 8 and 9). I can even recognize individual fires in these maps and I do not see how it could represent any general trends given the stochasticity in fire shapes. Perhaps, decreasing the spatial scale would decrease the impact of fire stochasticity. But, even then, I very much doubt that it could bring useful information. My advice here is to remove these results from the manuscript.

We thank you for your suggestion on this point. With the introduction of the Contextual Mann Kendal method in the revised version of the paper, the specific figures were meant to give an overview of the burned areas between the two periods and not as a method to analyze the spatial trends, since there is much more solid and robust method for that matter. However, by your comment, we realize that this is not clear especially since these figures are in the "Spatio-temporal analysis" section. We decided to keep the figures but move them to a different section ("Fire History").

- The number of fires recorded in the author's database is inconsistent with that of the national database (L142-143). Reporting these results without a detailed explanation of these differences might create some confusion among the readers not perfectly aware of the French fire context. A straightforward solution would be to remove fire numbers from the manuscript and reformat Fig 7.

We agree with your concerns, it is indeed simpler to remove the number of fires from the figure to avoid potential confusion with fire numbers in the Promethee database. The annual number of fires has been removed from the figure:



- There is a general tendency for the authors to overinterpret their results and focus their discussion on issues not directly addressed by their analytical framework nor related to their findings. There are numerous examples throughout the manuscript and I did not make the effort to note them all here. Perhaps, the conclusion of the manuscript is symptomatic of this tendency of overinterpretation of the results and discussing issues that are not directly related to the results shown in the present study. For instance, the authors said, that in "Bouches de

Rhone [...] BA patches are limited in size by fuel continuity, despite recurrent strong Mistral winds" while none of this driver has been explicitly investigated here. The following sentences are of the same type. I therefore strongly encourage the authors to carefully revise their manuscript and avoid, as much as possible, the overinterpretation of their results. In the case of the conclusion, that would mean to consider only the role of land-cover type and topography on BA in southern France.

We thank you for communicating the specific view. We have simplified the discussion and revised the text to avoid the problem of overinterpretation as you suggest.

References

Barros, A. M. G., & Pereira, J. M. C. (2014). Wildfire selectivity for land cover type: Does size matter? PLoS ONE, 9(1). <https://doi.org/10.1371/journal.pone.0084760>

Nunes, M. C. S., Vasconcelos, M. J., Pereira, J. M. C., Dasgupta, N., Alldredge, R. J. and Rego, F. C.: Land Cover Type and Fire in Portugal: Do Fires Burn Land Cover Selectively?, Landsc. Ecol., 20(6), 661–673, doi:10.1007/s10980-005-0070-8, 2005.