

Interactive comment on “Contrasting large fire regimes in the French Mediterranean” by Anne Ganteaume and Renaud Barbero

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Anonymous Referee #1 Received and published: 2 November 2018 The authors present a study that investigates and compares some of the characteristics of large fire regimes in two neighboring administrative regions of French Mediterranean Area. Based on a long-term (from late 1950’s!) georeferenced local dataset of large fires (> 100 ha), they explore various options to quantify some spatial and temporal metrics and draw conclusions about the similarities and differences in LF regime between those two regions and their underlying drivers. Such issues related to the spatial temporal

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characteristics of LF regimes are of potential interest for the fire science community. Examine (or reexamine) some of these questions in Mediterranean France in the light of new elements brought in by a detailed dataset is therefore an attractive prospect. In general, this is a well-documented and well-written manuscript with clear language. However, I had a hard time understanding the main objectives of this paper, or what were the author’s purpose, which prevent from evaluating the added value of their study regarding the understanding of fire regime in this area. In its current form, the general impression is that this manuscript is a suite of more or less relevant and unrelated analyses that do not form or follow any guiding thread. There are many interesting ideas in this manuscript, as well as the use of an extensive and valuable fire dataset, but they are rather disconnected from each other and the general feeling is that the authors do not take full advantage of the potential of their dataset. I have attempted to summarize in the four points below my main concerns that should be addressed for considering publication. (i) A lack of clear scientific hypotheses or research questions, (ii) improper interpretation and discussion of the outcomes of the analyses, (iii), questionable author’s choices regarding the methodology and analyses and (iv) a lack of description and of validation process of their dataset. I detail below these four main concerns.

Answer: We thank the reviewer for this thorough review and the suggestions that helped to improve the quality of the manuscript. We provide below a point-by-point response. We hope the research questions, objectives and the added value of the manuscript are now better stated.

#1 The research questions addressed in this study are not clearly stated. According to the title and some parts of the introduction (e.g. L52-56) the main goal of the authors is to investigate the “fire regime” of two different regions but that remains a very broad and undefined notion. Thus, and while the discussion is well written and very informative, a proper and clear scientific question is missing (see L98-104), which provides from clear conclusions of the article (see my point #2 below) and from a rationale choice of analyses (see my point #3 below). My opinion is that the authors should deeply revise

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their working hypotheses and focus their analyses around a few well-defined questions regarding fire regime characteristics. Answer: We agree with the reviewer. This part has been revised accordingly

#2 The presentation of study's results appears mainly descriptive in some places, disconnected from the analyses in others, and a discussion of broader hypotheses, processes or wider implications is missing (probably because a clear research question is missing, see point #1 above). By way of example, the description of the main results and conclusions in the abstract (L13-23) is highly representative of the whole manuscript: the first sentences (L13-17) are very descriptive and no explanations are given by the authors about the consequence of these findings, the following ones (L18-21) are interesting but concerns two specific figures and by no means linked to the previous sentences or analyses and. The last sentence (L21-23) is very disconnected from the rest of the manuscript since none of the analyses presented by the authors specifically deal with the year 2003. More similar remarks could be made for the rest of the manuscript. Answer: The manuscript has been thoroughly revised according to these comments.

#3 Following points #1 and #2 above, some of the author's choices and rationale regarding data analyses are questionable. For instance, I could not understand what the information was brought by Table 2 and its description (L193-L201). It seems a less clear (and unnecessarily complex) representation of Figure 5. Answer: We have removed the unnecessary table and better commented fig. 5.

Figures 4, 6 and 7 are arguably the most interesting part, but unfortunately these are too superficially described and discussed. For instance, one might wonder about the impact of landscape transformation when studying such a long time period in figure 4 or about the meaning of the results from Figure 7 that is only very little discussed (L350-351). By contrast, Figure 5 is intensively discussed (L323-336) but does not bring much more information than previous studies on this topic. Answer: This has been revised accordingly.

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Finally, the authors mention several interesting ideas regarding for instance the study of fire shapes, but no specific analyses are made on this point, leaving their conclusions highly speculative (L308-315). Answer: We had some points regarding the fire shapes but without being lengthy as this is beyond the scope of the paper.

#4. I agree the authors that long-term georeferenced fire dataset used in this study is one major and significant novelty compared to previous studies. Yet this database is only too superficially described (L132-134) and no details are given about how data were collected and reported. Answer: We tried to better describe the database which was not an easy task as it is a governmental database and it is difficult to obtain a very detailed information!

Besides and while I understand the difficulty for a proper and full validation process of the author's fire dataset, I think that the study would really gain from a comparison of your dataset with other fire statistics products (such as Landsat) for fire shapes validation. Answer: We are not sure to understand this comment. Approximate fire contours that concerned older fires (i.e., before 1990) had been corrected using aerial photos and Landsat satellite images (i.e. delineation of unburned areas and fire boundaries adjustment were performed). We provide this information in the material and methods section. If it is the comparison of all the fire shapes with Landsat images that is required, we think it is beyond the scope of this paper.

Also, I was quite surprised by the number of fires (L136, N=1227 fires > 1 ha) that appear to be very low. For the period from 1973 to 2016 alone, the French official fire database (PROMETHEE, available at www.promethee.com) reports N=4561 fires > 1ha for the same two regions. Answer: Yes this is a good point raised by the reviewer. This number is indeed lower than in the Prométhée database (918 on the same period as that of Prométhée which began in 1973) as mainly fires >10 ha are preferentially targeted in the DDTM database. However, the total burned area did not differ that much: 219 878.8 ha in Prométhée and 215 163.6 ha in the DDTM dataset (as it is mainly driven by large fires, $LF \geq 100$ ha) and regarding large fires, the DDTM database

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recorded a total of 237 LF which is very close of the 233 LF recorded in Prométhée. Same result regarding the burned area: 203 481.5 ha in the DDTM and 189 922.7ha in Prométhée. In conclusion, for both databases, the area burned by large fires represented between 88 and 92% of the total area which was the thing that matters most as the topic of this paper is related to large fires Moreover, other previous papers based on the Prométhée database gave the same conclusions.

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