

Interactive comment on “Exploring spatial-temporal dynamics of fire regime features at mainland Spain” by Adrián Jiménez-Ruano et al.

Anonymous Referee #1

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In general, this is a clear, objective, well organized and written manuscript. My major concern is about the novelty of this study. Apparently, this topic is not new in Spain. The authors cited a significant number of studies on this theme. The motivation is obviously associated the well-known changes/trends in the fire incidence in Spain. It is even cited a study performed for the same regions and the same (EGIF) fire dataset although the study period is slightly different (1968-2010). The authors justify the paper with the inclusion of other fire regime features but I'm convinced if it is sufficient. Besides this problem, I only have a few number of general and a small number of specific questions, comments and/or suggestions. This study aims to detect break/change point and trend in time series of several fire regime features in Spain on different spatial scales/basis. The abstract is a good summary of the paper and the introduction comprehensively

C1

cover all important aspects to the study.

My general comments/suggestions/questions on the other sections of the manuscript and are the following: Study area Figure 1 do not helps to understand the location and size of the Eurosiberian and Mediterranean regions. The description of the type of climate is not very accurate, in the sense that any figure is presented or any study is cited. The AEMET/IM Iberian Climate Atlas, Peel et al. (2007), Kottek et al. (2006), among other could be cited and used. The three considered pyroregions present some similarities but also some differences to other studies not cited. For example, Sousa et al. (2015) and Trigo et al. (2016) identified different pyroregions. These similarities/differences should be discussed because they could have significant impacts on the obtained results.

Data The quality of the datasets is one of the most important aspects on this type of studies. The authors identified some completeness problems for small fires (burnt area < 1ha). The same type of problem was found for Portugal (Please see Pereira et al., 2011) whereby this study could be cited. Besides this aspect, what type of data quality analysis was performed on the fire dataset? Another important aspect is the size of the dataset. It is very important to know the size (number of fires) of the dataset as well as how many fires are in each group (NH, NS, N500, N500N N500S, NL, NH, etc.) as well as on each province/NUTS3 region. Please provide this information on the manuscript. Finally, since the authors do not provide the intra-annual distribution of any fire regime feature, it is not possible to understand the splitting of the annual data in to the summer (April-September) and winter (October-March). In fact, according to Sousa et al. (2015) and Trigo et al. (2016), it would make more sense another split (May-November and December-April). The authors should validate their options and discuss these aspects in the manuscript.

Methods The authors describes the characteristics of the used methods. However, it is also important to explain which other methods could have been applied for the same purpose and why these methods were selected. It is also important to explain why you

C2

limit the number of detect breaks to just 1.

Discussion This section need to be improved; sometimes, is just a repetition of the results presentation; others cases, studies with similar findings are cited; this is not the best/proper validation/interpretation of the results. For example, in line 38, the decreasing trend in MED region is justified with the study of Moreno et al. (2014) which suggested that “climate might have played a role in the change points”. However, the questions is the following: did Moreno or the authors detect any change in the climate? Even if those change occurred what is the impact on the fire regime features? The same happens, for example, in lines 384-385 and lines 395-396. In this later case, this means more or better/more efficient methods? How this improvement was assessed?

Reference list can be updated/enlarged.

Specific questions /comments/suggestions Line 260-261, should not be in the main text but part of the figure caption; Line 294, the caption of figure 4 is not clear; at the first reading only mention SS; Line 317, SD is not defined; Lines 409-410, average fire size is a very “dangerous” measure, especially due to data errors. This is recognized by the authors when removed small fires (burnt area <1 ha) from the analysis.

Tables & Figures

Tables and figure should be self-explanatory. Therefore, for example, explain/describe all acronyms, symbols, etc.

Table 3. Please explain how the thresholds (-0.43 and 0.43) were obtained to define “The most meaningful features”.

Figure 1. It is not clear if the named regions are the pyroregions; the “continuous” color scale is not a good option; it is virtually impossible for the common human eye to identify the associated value. This is also valid for figure 4 and figure 5. The presented CLC nomenclature is not the usual/official one. Please explain how was defined, i.e., which CLC classes are urban (eventually all the Artificial classes), grassland, shrubland, etc.

C3

The climate classification can be presented, for example, in panel b.

Figure 4. Caption is contradictory; first mention “Spatial distribution of significance level of SS values 1974-2013” and, in the end, “Provinces without symbols represent non-significant trends according MK”.

Figure 6. A “Table” and a Figure do not seems a good idea. What don’t you plot two figure, one for summer and other for winter and, in each case you only plot the “statistically significant” arrows?

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C5