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

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REVIEWER 1: My major concern is about the novelty of this study. Apparently, this topic is not new in Spain.

AUTHORS: First of all, we would like to thank the reviewer for his/her comments and suggestions about the manuscript. We have tried to amend and address all pointed issues.

Regarding the novelty of our work, indeed this topic is not new in the context of Spain and we were very aware of that. However, we believe that our work goes a step further in providing insights and analyse dynamics in fire regime features. Specifically we have (i) extended the analysis to other fire regime features in our change-point analysis; (ii)

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we also apply traditional trend analysis to these other features; (iii) assessing not only the sign of trends, but its magnitude, which has not yet been addressed; (iv) at different scales; (v) this work would also allow progress in the fire regimes zoning; (vi) finally, our most novel contribution is exploring the relationships and association among trends in fire features using Principal Components in an effort to provide a more synthetic interpretation as well. We also provide a summary map of the main trends detected which allows to outline homogeneous zones of temporal dynamics at province level. To our knowledge such kind of analysis and cartographic outputs has not yet been done.

REVIEWER 1:(Study area) Figure 1 do not helps to understand the location and size of the Eurosiberian and Mediterranean regions.

AUTHORS: We have removed the bioregions layer in the second map of Fig. 1, because we finally considered that this information is not truly necessary for the study area description.

REVIEWER 1: 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.

AUTHORS: We have improved the climate types' description and added the AEMET/IM Iberian Climate Atlas reference.

REVIEWER 1: 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.

AUTHORS: We added these two references and briefly discussed the similarities/differences between their zoning and our pyroregions. In any case, we are using the 'official' regions provided by Spanish authorities because they are fully adapted to the way in which fire events are reported and spatialized.

REVIEWER 1:(Fire 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?

AUTHORS: We have included this reference in Fire data section (Pereira et al. 2011). Regarding to the data quality analysis, fire data comes from EGIF database, which generally guarantees high quality. However, we have found several grid coded errors that were corrected, and also discarded few records whose location was reported outside Spain limits. Apart from that, no other quality analysis has been done.

REVIEWER 1: 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.

AUTHORS: This is a very good suggestion. Indeed we should have already provided this. We have added a table with this information.

REVIEWER 1: Finally, since the authors do not provide the intra-annual distribution of any fire regime feature, it 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.

AUTHORS: We appreciate your suggestion; however, we have done the seasonal split mostly according to the fire danger seasons established by the Autonomous Communities legislations. Moreover, the seasonal partition proposed by others authors does not match the intra-annual distribution of some fire features such as natural fires.

We have run some tests exploring the May-November and December-April seasons and the results do not change significantly so they are not sensitive to those differences in the cut-off months. On the other hand, we plan to incorporate climate information in further developments and applications of our proposal (for instance. for fire regime zoning) in which we believe our seasonal split fits best.

We have included part of this justification in the corresponding section and the different tests addressed in the Discussion.

REVIEWER 1:(Methods) - The authors describe 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 limit the number of detect breaks to just 1.

AUTHORS: We have selected the most commonly employed methods in the literature and specifically to fire data for trend analysis. However, precisely because we weren't sure of the performance of Pettit and didn't find any work comparing Pettit to other methods, we explored other possibilities for the change point analysis, to determine if there is any variation depending on the method and also be able to report a 'consensus' result rather than a single one. We didn't limit change point detection to 1. It is true that Pettit and AMOC methods only are able to detect 1 point, but PELT reports more than one. The thing is that most of the time there is only 1 point detected, although in those cases where more than one was detected we reported them (Table 1).

REVIEWER 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 are 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?

AUTHORS: The reviewer raises an interesting concern. Regarding to line 38, the work by Moreno et al.2014 detects climate influence in upward changes in all fire regimes, regions and vegetative season. Also in some downward change points of the Mediterranean and Northwest. However, the authors explicitly do not find or mention a real climate change beyond its influence on fire metrics.

Pausas and Keeley, 2009 review the importance of fire has waxed and waned in association with changes in climate and paleo atmospheric conditions.

Pausas 2004 reports a significantly relation between burned area variability and summer rainfall.

Pausas and Fernández-Muñoz concluded that the fire regimen change in Valencia cannot be explained by gradual climate change observed.

Turco et al. 2014 assessed the impact of climate changes incorporating regional climate models, which captures quite well the observed trends. However, they admitted the complex impact of climate change in burnt area, because of the triple relationship (climate-fuel-fire). They estimated an increase in fire frequency and a stable o slight decrease in burnt area in hotter scenarios.

Moriondo et al. 2006 found an increase in fire risk in two future scenarios for the entire Mediterranean region. Specially, fire features such as increase in number of seasons with fire risk, increase in the number and length of extreme events contribute in a great extent.

Salis et al. 2014 did no address the climate influence in wildfire regime, but the weather relation.

Venäläinen et al. 2014 concluded that weather and climate are the major factor controlling fires, but not the only ones. In addition, fires were only related to current-year climate variables.

REVIEWER 1: 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?

AUTHORS: Regarding to lines 384-385 we share with Moreno et al. the fact that in the NW region human factors play an important role in terms of fire activity and fire trends. On the other hand, in lines 395-396, the first reference refers only by the introduction of new fire policy (fire suppression and prevention practises). The improved was assessed by means of a statistical framework based on spatially explicit daily fire occurrence data, the corresponding weather variables and the associated fuel moisture derived from a process-based model. The second reference investigated the role of fire suppression strategies in synergy with climate change on the resulting fire regimes in Catalonia, Spain. They addressed this issue with a spatially-explicit fire-succession model at the landscape level to test if the use of different firefighting opportunities related to observed reductions in fire spread and sizes.

REVIEWER 1:(Specific comments) - Line 260-261, should not be in the main text but part of the figure caption.

AUTHORS: We have moved this line to both figure captions (Figures 2 and 3)

REVIEWER 1: Line 294, the caption of figure 4 is not clear; at the first reading only mention SS.

AUTHORS: We have rephrased the sentence of this line to be more explanatory.

REVIEWER 1: Line 317, SD is not defined.

AUTHORS: We have defined SD as “standard deviation”.

REVIEWER 1: 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.

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AUTHORS: We have replaced the “average fire size” by “total burnt area” in this line.

REVIEWER 1:(Tables & Figures) - Tables and figure should be self-explanatory. Therefore, for example, explain/describe all acronyms, symbols, etc.

AUTHORS: We have incorporated the full name of all acronyms in the captions, or a section reference regarding to fire features description.

REVIEWER 1: Table 3. Please explain how the thresholds (-0.43 and 0.43) were obtained to define “The most meaningful features”.

AUTHORS: This threshold was stablished based on the actual values we retrieved from PCA-Varimax. There is no rule-of-thumb when it comes to determine a correlation threshold. We now realise that reporting a cut-off value of 0.43 it’s rather awkward. In fact, the actual value is 0.4 but, again, this is based on the two most correlated featured in each component.

REVIEWER 1: 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.

AUTHORS: In Figure 1, we have included in the caption a pyroregions description while we have removed the elevation colour variable. In Figures 4 and 5 we have changed the continuous colour scale to a discrete colour scale for the variables mapped.

REVIEWER 1: 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.

AUTHORS: The CLC is a generalization or summary of all the land cover categories. We added how we defined them and which specific sub-category is within each one. The regrouping was as follows. Urban: all the artificial surfaces; Grassland: only pastures and natural grasslands; Shrubland: only moors and heathland, sclerophyllous vegetation and transitional woodland-shrub; Water bodies: all wetlands and water bodies; Cropland: all agricultural areas (except pastures); Forest: only broad-leaved, coniferous and mixed forest; Barren: all open spaces with little or no vegetation.

REVIEWER 1: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”.

AUTHORS: We appreciate this observation, we mean that we have finally selected the significance Sen’s slope values according to the Mann-Kendall test, because the Sen’s Slope doesn’t report significance. Thus, we discard the provinces with non-significant trend according this last test. We have rephrased the sentence so as not to be confused.

REVIEWER 1: 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?

AUTHORS: We appreciate your suggestion, but we believe that adding another map here can saturate this figure to the detriment of the effort to summarize the main trends. On the other hand, we have previously divided both seasons between the components 1 and 2 in Figure 5. Finally, it is important to note that the table which accompanies Figure 6 is actually its legend. We have explored and tried different versions of this figure and in the end this was the better way to show and summarise our findings.

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