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Natural Hazards and Earth System Sciences
Discussions

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 2: This temporal analysis of fire regimes features in Spain may be a very valuable addition to the fire science field, as it considers traits of fire regime characterization not contemplated before, beyond the usual number of fires and burned area, from a temporal perspective. There are many previous studies on how climate, topography, vegetation, and land use influence fire regimes, characterized by number of fires or fire frequency, severity/intensity, size of burned area or pattern. As there is abundant previous work on fire regimes characterization, the factor that set this analysis aside and merits publication is the application of change and trend detection procedures to fire features of special interest in Spain (i.e. large fires over 500 ha), and the PCA-Varimax Rotation applied to summarize trends. Procedures, though, may be applied

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elsewhere at different spatial and temporal scales. However, the authors state that their temporal analysis aims "to refine and improve the spatial outline of fire regimes" and has an "ultimate goal of characterizing fire regimes". How it is proposed that their temporal variability in fire regime features is considered when defining fire regimes? (line 50). It is unclear how they propose this to be done, or how their stratifications in space (three regions, provinces NUT3 level) and time (two fire seasons in winter-spring and summer, line 140) correspond to fire regime stratifications in Spain by other authors like Moreno & Chuvieco 2012 (four regimes), or official Spanish reports (that need citation (line 120). Other partitions of the territory were possible, and these pyroregions need better justification and definition.

AUTHORS: First at all, we would like to thank the reviewer for his/her useful comments and suggestions about the manuscript. We really appreciate the positive evaluation about our work. Indeed, there are many works devoted to this subject, and thus it is not easy to bring some novelty. We are particularly grateful for appreciating the novelty of our proposal. Regarding to the application of the procedures at different spatial and temporal scales, we would like to bring some light here. In fact, we are currently working in a new fire regime zoning in which we are including trend magnitude as a key parameter because we believe that a complete fire regime characterization should account for at least the dynamics of the main fire features. This work provides enough evidence of changes in fire features; therefore, we can infer that fire regime zones may not be the same in 1974 than in 2013, something that is assumed in current works, for instance Moreno & Chuvieco 2012. Bringing this up here was not possible since we have limited space. However, the way in which we propose this to be done is, for example, by using trend outputs as another input of the cluster or zoning algorithm. This also would involve downscale the spatial reference unit to a finer one (10x10 grid). As the reviewer has pointed out, replicating this analysis to other temporal or spatial scales would be easy. Regarding the regions of analysis, we have used these three regions (Northwest, Hinterland and Mediterranean) because we want to know the overall behaviour of trends. We coincide with the reviewer in that it might not be the most appropriate partition, since their mean values or dynamics are not homogenous. For this reason we have lowered the scale to the NUTS3. In this sense, this second stratification has been chosen because we tried to increase the degree of detail in the trends description within each region. In any case, note that those regions are used in other studies that we took as reference to stablish comparisons; an all official statistics in Spain are referred to them.

REVIEWER 2: Some descriptive statistics of the fire database in the 2.2 Fire data section would probably help to justify the spatial and temporal stratification used.

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

REVIEWER 2: Lines after 185 explain three algorithms for change point detection. Why settings were determined to find at least one, but no more than two breakpoints in PELT, and one (Q=1) in BinSeg? This makes sense for comparison purposes with AMOC and Pettitt, but is there not a risk to miss other significant changes?

AUTHORS: We didn't limit change point detection to 1. It is true that AMOC and Pettit methods only are able to detect 1 point, but PELT generally 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). However, we would definitively prioritize the most coincident change point as the most likely or strongest one among all methods.

REVIEWER 2: The authors refer to CCAA in Spain the international readers will not be familiar with, i.e. Andalusia, Galicia or Asturias, not in Figure 1. Labels seem to be missing. What is the black line crossing the land cover map?

AUTHORS: We agree that it will be more useful to include place-names of provinces (NUTS3) and CCAA (NUTS2), for this reason we have finally included a politic map of Spain in the new version of Figure 1. On the other hand, the black line crossing the land cover map represented the limit between both biogeographic regions (Eurosiberian and Mediterranean). However, we finally removed it because we believe that land cover alone describes best our study area.

REVIEWER 2: Regarding Figures 4 and 5, Sen's slope values are hard to distinguish.

AUTHORS: We really appreciate this observation, thus in Figures 4 and 5 we have changed the continuous colour scale to a discrete colour scale for the variables mapped.

REVIEWER 2: Why is the level for correlation in table 3 set to 0.43? Please explain.

AUTHORS: This threshold was established 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.

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