

## ***Interactive comment on “Size of wildfires in the Euro-Mediterranean region: observations and theoretical analysis” by C. Hernandez et al.***

### **Anonymous Referee #1**

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article

Review of Manuscript submitted to Natural Hazards and Earth System Sciences

Title: **Size of wildfires in the Euro-Mediterranean region: observations and theoretical analysis**

Authors: C. Hernandez, P. Drobinski, S. Turquety, and J.-L. Dupuy

### **Reviewer’s recommendation: minor corrections**

This manuscript examines the relationship between burnt area and wind speed

C274

over the Mediterranean region and Eastern Europe. The paper presents a quite extensive piece of work and deserves publication in NHESS. However, I think some improvements (minor revision) could be implemented before publication.

### **Some general comments**

1. My main concern is about statements concerning the robustness of the results, especially in view of the uncertainties in observations and in the complex relationship between fires and other factors (e.g. environmental and socio-economic). Somehow, I feel that this uncertainty is not sufficiently assessed and acknowledged, which results in perhaps overly confident statements. The results might be more convincing if the uncertainties of the data and methods used were deeper analysed, or at least better discussed. Here is a list with some suggestions:
  - (a) It would be appropriate to better clarify whether this manuscript focuses on the analysis of the link between wind and fires, or in the discussion/evaluation of the fire model PCA. In addition, an example of how this model performs could increase confidence in the results.
  - (b) Showing and discussing the similarities and/or differences between the MODIS and EFFIS datasets, where available, could increase confidence in the data used.
  - (c) The complex weather-fire relationship need to be better discussed, especially considering potential confounding factors (e.g. changes in fire fighting or ignitions over the years). In particular, fires have a large seasonality (generally there are more fires in summer in Mediterranean regions) and the fire drivers may change during the year. An analysis aggregating the data for season could shed light on this issue.

C275

- (d) MED and EAST are very different, also in terms of surface. Have you tried to analyse sub-areas of the MED domain?
2. The authors do not give proper credit to relevant previous works that have analysed the weather and/or climate relationship with fires in similar regions (see the section references for several examples).
  3. The results are often discussed in reference to “heatwaves and droughts”, although the authors do not analyse droughts. I suggest removing the reference to drought or, in addition to wind and temperature, also analysing some drought indices.

### Some specific comments

1. ABSTRACT: I think that the abstract is not easy to understand for a wide and diversified audience. If possible, please rewrite, or briefly explain, the “cellular automation model” and the “percolation threshold”.
2. TITLE. I think that the title does not reflect the main contents of the paper.
3. P 1204 L 25. I do not understand why there is a reference to boreal areas.
4. P 1205 L 4. Please use more recent references (e.g. San-Miguel-Ayanz et al. 2013, Ganteaume et al. 2013).
5. P 1205 L 5-11. This part is not clear. I think that the year 2003 was one of the most severe in Portugal in terms of fire activity. I do not know if it was also severe in other European countries (e.g. is 800'000 ha above the average in Mediterranean Europe?) and the cited reference is still not available. The fire drivers may also need deeper discussion. Please re-write and include published references.

C276

6. P 1205 L 12- P1206 L4. The discussion on the fire drivers could be improved by including citations on studies in Europe (see “Reference” for some suggestions). Specifically, please include some references for the past evolution of forest fires in Mediterranean regions (see e.g. Koutsias et al 2013; Turco et al 2013b; Fréjaville and Curt, 2015), for the climate/weather-fire relationship (see e.g. Pausas 2004; Thonicke et al 2010; Moreira et al 2011 for a review; Pausas and Paula 2012; Bedia et al 2013; San-Miguel-Ayanz et al 2013; Turco et al. 2013b; Lecina-Diaz et al. 2014; Loepfe et al 2014) and climate change impacts (see e.g. Batllori et al 2013; Turco et al 2014).
7. You could change Silva et al. (2010) with Ganteaume et al. (2013).
8. P 1206 L12. Why has the influence of precipitation not been analysed?
9. P 1206 L5-24. The objective of this study is not outlined clearly. In addition, the authors do not indicate clearly their contribution with reference to previous and/or related work.
10. P 1207 L 19. How do you compare the MODIS and meteorological data, since they are at different resolutions?
11. P 1208 L 9-11. Repetition. I think this sentence can be deleted.
12. P 1209 L 6. I think that a figure with this information could be added in the revised version of the manuscript.
13. P 1209 L1. Are there similar results in other regions/studies?

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C277

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C278

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C279