

Interactive comment on “Wildland fire potential outlooks for Portugal using meteorological indices of fire danger” by Sílvia A. Nunes et al.

Anonymous Referee #1

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The entitled paper “Wildland fire potential outlooks for Portugal using meteorological indices of fire danger” elaborates and compares models using one or two meteorological factors derived from a meteorological daily fire danger index (DSR). The aim of the paper fits with NHESS journal scope and objectives, presenting a useful tool for fire prevention issues. Methods are clearly outlined, I suggest the authors minor changes related to the order of the model description in the methods section (see section specific comments below). The results support the main hypothesis, which lead to reasonable conclusions. Data and methods are proper described, as well as mathematical methods and test applied. In my opinion it might be reproducible for other case studies. The title and abstract reflect the work done and the obtained results. The abstract is understable for a diversified audience. Figures and tables have a proper

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size and resolution. Regarding some of the captions of the figures please see the section technical corrections below. The authors provide the previous related works following them and contributing with some specific pre fire season information. The number of references is adequate and can be found. Generally speaking the paper is well structured and with a proper length.

Specific comments Data and methods: - Why only July and August as fire season? As you mentioned in the results section in the year 2017 the highest BA values occurred out of this period - Even though there is the reference to the explanation about the Portuguese Rural Fire database, what is the reference unit of the BA? Perimeters? Total BA at municipality or parish level?

Results: - In the data and methods section you explain the models considered as 1) null model, 2) a nested model with one covariate and 3) a nested model with two covariates. Then, in the results section, the model with two covariates is coming first (3.2) than the one with one covariate (3.3). As 3.3 section is explained/justified of what happened in the previous section, maybe you could consider to change the order in the methods section. - In page 13 you mention that the threshold of 0.7 is lowered to 0.66. Why 0.66?

Discussion and conclusions: Are there any future developments projected of these models incorporating any other kind of covariates, not only meteorological derived? Is the performance of those models being influenced of any possible missing covariates?

Technical corrections Page 1, line 25: of 2017 Page 2, line 18: System (EFFIS) Page 3, line 26: July 1st, August 31st Page 3, line 28: April 1st Page 4, line 1: 1990-2018 is Page 5, line 7: Lilliefors test Page 7, line 20: please revise the sentence Page 8, line 4: performance of Page 9, figure 4: please add a), b) c) if it is according to the journal graphic rules Page 12, figure 6: the caption should be self explicative, no need to go to check a previous figure. Please rewrite Page 14, figure 9: same comment as before You use "worth noting" many times in the text.

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