

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

Anonymous Referee #2

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I read with the interest the manuscript by Nunes et al. , the authors use a rather simple and sound methodology to forecast the intensity of the fire season in Portuga. the main idea is that a large part of the variability in summer burnt areas in Portugal could be explained (and therefore predicted) by the prefire season spring conditions. There are two raisons for that. First, fire activity is largely driven by fuel moisture conditions, which are in turn dependent on meteorological spring conditions. Second, it has been shown that dry spring conditions increase the likelihood of heat waves during summer, which are known to be associated with large fire occurrence in Portugal. The performance and degree of predictability of their modeling framework is assessed by considering whether the model can discriminate weak and severe years and at which date can this model provide robust predictions. The authors conclude that, given the relatively good

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performance (severe or weak years can be predicted up to one month in advance), this approach can be of interest for the fire community.

This is an interesting work that can have operational applications for fire management/suppression in Portugal and also stimulate the application and developments of some similar approaches in other regions. The method and result sections are clear and well written, with some interesting analyses (such as the evaluation of type A and B decisions for instance). I have a series of recommendation to improve this paper even more.

Overall, I found the discussion was rather descriptive, mostly rewording the main results and methods of the study, without bringing much information and developments. The main focus on theoretical/fundamental aspects related to the fire-weather relationships while this is not the objective of the paper. One could expect that the discussion would bring much information about the added value of the work comparing to other methods (does that really bring much information than seasonal forecasts ?), its potential use for direct operational applications, the potential improvements (other indices, why is the year 1998 not correctly classified?)...

I was also surprised by the choice of your indices for the prefire season (Dpfs). I don't see the rationale for using the cumulated DSR as an index for "vegetation stress". First the DSR includes many other information that is not related to vegetation dryness (such as wind speed). Second, as the DSR already depends on its previous values, I don't understand why you should use its cumulative value. The way I see it, a daily index that is recognized to be a proxy of vegetation dryness (e.g. the Drought code of the FWI among others) would be more appropriated here. Also, and If your objective was to obtain the best performance I was also wondering why you did not compare several indices.

Finally, I think that much emphasis is placed on the "diagnostic model of BA", i.e. using meteorological information during the pre-fire and the fire seasons while this model

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does not bring much added value to the field.

Other comments

P2, L25-28: That part is interesting and should be developed. You should also provide some citations that shows the links between spring drought and the likelihood of summer heatwaves. Not sure the references are appropriate

P3, L15: could you provide more details on how does the DSR differs from FWI.

P4 : L8-14: That would be clearer if that part was moved before indices descriptions

P4 : L16, is $\psi(d)$ computed according to the mean and standard deviation of day d or over the entire population (including all days)

P7, L7, why starting from May 26 only. That would be interesting to start earlier to see also when does that information becomes relevant.

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