Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-318-AC5, 2018

Natural Hazards and Earth System Sciences
Open Access
Discussions

Interactive comment on "Assessment of Forest Fire Rating Systems in Typical Mediterranean Forest, Crete, Greece" *by* Mohamed Elhag and Slivena Boteva

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Received and published: 19 June 2018

Re: Reviwer#4 Dear editors, authors and reviewers, the paper examines the applicability of the Canadian FFDRS and US KBDI as indicators of fire potential in Mediterranean forests, a useful goal given the fire prone nature of many Mediterranean vegetation communities. Some work is required on the paper, however, prior to publication in my view. The authors state (in lines 286-288 and 363) that the FWI is applicable as an assessment of fire risk in Crete. The study period covers only two fire seasons, however, one of which the authors note (lines 337-338) was a year of exceptional drought. I think that a longer period of study would be required to definitively assert the value of

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Discussion paper

the FWI. An appropriate conclusion, I suggest, would be that the FWI shows promise and that a longer analysis is justified. To further support the author's case, it would be useful, as reviewer 3 notes, for readers to be able to see the extent of fire activity during the two fire seasons and understand how typical that activity was through the authors providing details of average fire numbers and area burnt. Acknowledged and edited to be "FWI shows promise" in the conclusion section. Regarding methodology, two fire seasons are enough to reach such conclusion. This is not the first work to endorse such results with two fire seasons only. Other scholarly work like Hély et al. 2001 and Dimitrakopoulos et al. 2011 reported the same in two seasons.

It isn't clear to me that the authors acknowledge that the KBDI was developed as simply an indicator of long-term dryness, and not specifically a fire danger index. KBDI doesn't, for example, include any dependence on wind or relative humidity which, depending on the type of fuels, affect fire behavior to a greater or lesser extent, and which the FWI does incorporate. KBDI is widely considered as a dryness index rather than fire danger index in many works of literature such as Srinivasan et al. 1998; Svoboda et al. 2002; Heim and Richard 2002 and Garcia-Prats et al. 2015. Therefore, authors considered KBDI as a dryness index.

The only discussion that relates to this point is in lines 192-194, implicitly, where constant values of weather parameters are assumed - an assumption which, incidentally is not justified in the text - and in line 377, where the authors report that KBDI is not adequate for indicating daily fire danger. Related to this point is the assertion (lines 87-88) that forest fire activity is dependent mainly on short-term weather. It is true that short-term weather is important, of course, but in many forests antecedent conditions are also important for sufficient drying to have occurred to permit fuels to burn. The reason that indices such as KBDI were developed was to quantify this long-term drying. The assertion needs to be substantially qualified or removed. It was not totally unjustified, Dimitrakopoulos et al. 2011 support the short-term weather. The reference is now incorporated in the text and the assertion is removed.

I note reviewer #3's reference to an earlier paper, covering similar but not identical material, and agree that it would be very worthwhile, indeed important, to cite that paper. Acknowledged and cited.

Finally, substantial additional editing is required, I suggest, prior to publication. I offer some examples, but these are not by any means comprehensive: Lines 187-191 are a word-for word repeat of lines 98-102, including the mis-spelling of "cover"; line 235 refers to "initial Spread Index unit equation". Acknowledged and redundant text is omitted.

The discussion here is about the KBDI, not the ISI (unless I've completely misunderstood the derivation, in which case greater clarity of argument is indicated); In section 3.1.4, it is not clear to me what is a result and what is being reported from the existing literature; Line 384 should read "... it is shown to be inappropriate to predict the needle moisture content..."; Lines 387-390 do not constitute a sentence. The discussion is about the KBDI, but it doesn't mean that ISI is ignored. Estimation of the DC is an ancillary procedure but worth to mention to give the readers the full picture of implemented methodology. Lines 384 is edited and lines 387-390 are removed.

In summary, I concur with reviewer #3 that substantial reworking of the manuscript is required prior to publication. Kind regards,

We thank reviewer #4 of his/her comments and his/her insights to enrich the current work but we, unfortunately, won't be able to do any extra work regarding the fire seasons and data collection. As you may have noticed, this is a completely independent work with no finical support from any kind or even a postgraduate program. Fieldwork and data collection was the worst part of this article especially when you take into consideration the data inconsistencies and discrepancies in Greek authorities. If review #3 and reviewer #4 insists on extra work, then authors kindly ask the reviewers to secure a source of fund to go on with extra work. We appreciate reviewer concerns, but we can also fulfill their concerns with our own personal money. Two fire seasons are enough to reach the current conclusions and as we mentioned before this is not the first work to be based on two fire seasons only.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-318/nhess-2017-318-AC5-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-318, 2017.