

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

### **Anonymous Referee #4**

Received and published: 17 June 2018

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 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

C1

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. 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 behaviour to a greater or lesser extent, and which the FWI does incorporate. 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. 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. 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". 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. In summary, I concur with reviewer #3 that substantial reworking of the manuscript is required prior to publication. Kind regards,

C2

