

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1  
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## Comment on nhess-2022-140

Anonymous Referee #1

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Referee comment on "Seasonal fire danger forecasts for supporting fire prevention management in an eastern Mediterranean environment: the case of Attica, Greece" by Anna Karali et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-140-RC1>, 2022

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This work aims at providing high resolution probabilistic seasonal FWI forecasts for Attica (Greece) and verifying these forecasts using probabilistic verification measures for skill assessment (ROC skill score and reliability diagrams). To accomplish that, the authors compute FWI and its components for the fire season MJJAS using the dynamic forecasting system SEAS5 with lead times of 0 and 1 month (issued in May and April, respectively). The manuscript describes innovative research with potential operationality in a country that has been recently affected by large wildfires. The main issue with the manuscript is the almost non-existent discussion. Indeed, the majority of the problems found in the manuscript can be solved with the disentanglement of the section "Results and Discussion" into two sections. Furthermore, the paper has many typos and sentences that are not well written. I urge the authors to carefully re-read everything. Thus, since some of the revisions may take some time to tackle, I suggest major revisions.

### Comments:

Line 96: You say that "FWI represents the frontal fire intensity...". I do not agree with this sentence. Frontal fire intensity is the energy output rate per unit length of fire front. FWI is not that. Can you please elaborate on what you intended to say? Also, I think the explanation of the FWI components is very loose, especially because you introduce FFMC, DMC, and DC explaining what they are but never defining what the abbreviations mean. Indeed, lines 88 – 91 are not reader-friendly. Moreover, in line 92 when you introduce ISI and BUI for the first time you should put in parenthesis "(Initial Spread Index)" and "(Build-up Index)". You only do this on the second appearance.

Line 113: Indeed, FWI is to be computed at local time noon. I searched for Greek time, and in summer there is a difference of three hours to 12UTC, i.e., you are computing FWI at 15 local time. As I understand SEAS5 sub-daily values are available in a 6-hour window, which means you had to choose between using values at 9 or 15 local time. It is of paramount importance that you explain this here. Also, your discussion section must take this issue in consideration.

Results and Discussion: Sections 3.1 and 3.2 are mainly a description of the results with a somewhat poor discussion. I recommend the authors to disentangle this section into two separate sections (3 Results; keeping 3.1 and 3.2; and 4 Discussion; 5 Conclusions). Moreover, the authors show a total of 9 figures in the results (and 1 in the methods), which seems too much for a 2-page section (results and discussion). I believe that with a stronger discussion section the 9 figures can better fit the amount of text. However, I recommend the authors to verify if all the figures are essential to the main body of the manuscript (if not, send some of them to supplementary material). For example, Figure 1 is not essential. Above I recommended the authors to design a scheme, which may also be present in supplementary material (for readers who are new to the field understand the methodology of forecasts and lead-times).

Lines 280 – 285: It is crucial to understand that FWI is a fire risk index, which only gives a picture of how susceptible a region is to burn. Without an ignition and vegetation prone to burn there will be no fire. It is interesting to see that it is possible to relate FWI forecasts for MJJAS fire season issued in April and May with the number of fires, but I do not think this is a key issue. However, doing such an analysis, why didn't the authors choose Fire Radiative Power (FRP), which could give an aggregated vision of the fire intensity in the region for the period of study? Wouldn't it be a more interesting variable than the number of fires?

Figures 3 – 4: More discussion on these figures is needed. The authors need to develop on why fire season wind speed is better forecasted in March than in April. Why does precipitation show no skill? Also, it is very hard to relate the colours to values. I recommend you use one colour in steps of 0.2 and try to use more contrasting tones of reds and blues (this applies to Figures 2 – 6).

Line 27: "as regards" to "regarding".

Line 84: When defining FWI you should add the Van Wagner reference.

Line 116: replace "temperature" by "temperatures".

Line 117: Why May – September as fire season? Why do you include May and ignore October? Is this common for Greece?

Line 118: Why do you stop at 1-month lead time? Wouldn't it be interesting to see the predictive skill of forecasts starting in March? I believe 6-month lead times (7 months prior to target) are available, which means that for September there are forecasts initialized in March. Does it have to do with the spin-up period? Moreover, please try to better explain what a spin-up period is.

Lines 117 – 123: When explaining forecast lead-times and methods a figure with a scheme is usually very useful for the reader to understand the methodology. Can you include one?

Line 135: "while in a second step of bias correction is applied". Please improve language.

Figure 1: More adequate as supplementary material.

Figures 8 – 9: The meaning of the size of circles should be written in the caption.

Figure 10: "Annual number of fires (NOF) in Attica per year...". Please, remove "per year".

Conclusions: Too much repetition of results.