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Discussions

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Reply on RC2

Tomàs Artés et al.

Author comment on "Wildfire–atmosphere interaction index for extreme-fire behaviour" by Tomàs Artés et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-122-AC2, 2021

We appreciate the work done by the reviewer and the provided concerns. We would like to remark that due to the previous comments of Dr. M. Pinto we modified the manuscript and another index as CHaines and changed the neural network improving the accuracy of the classification. That modifications are detailed in the answer to Dr. M. Pinto's comment.

Note: **In bold anonymous reviewer**

An index designed to identify atmospheric instability that promotes or facilitates extreme fire behaviour (EFBI) is introduced. Indices constructed from surface-based parameters often fail to discriminate between extreme and non-extreme fire behaviour. Decades ago, atmospheric stability was proposed as a missing ingredient in such indices. The EFBI provides a measure of atmospheric stability designed to fill this gap, and be used in conjunction with existing surface-based indices to provide a more comprehensive assessment of fire danger.

Results are presented that suggest using the EFBI in conjunction with a surface-based index (the Canadian Fire Weather Index, FWI) does improve the predictive performance compared to only using the FWI.

Case studies are included that illustrate the EFBI for specific extreme fire-behaviour events, including pyrocumulonimbus (pyroCb) cases.

I have a number of major concerns with this paper:

1. It was not sufficiently polished for submission. (It could have benefitted from an internal review before submission.)

We assume that most of the polishing are detailed in the points of concern expressed by the reviewer.

2. It lacks references to, and comparison with, similar published indices and concepts.

The proposed literature by the reviewer is very valuable and will be included in the article allowing the reader the better understanding of the parameters used in the EBFI.

3. It lacks explanation of the ideas, theory, or reasoning that underlies the EFBI.

The concepts required to compute EFBI are mentioned and described in the article. Ideas and reasoning of the proposal are implicit in the formulas and described in paragraph in line 90 and 155. References to other articles describing or using some of the components included in the EFBI are included in the article, allowing the reader to have a deeper understanding of the underlying factors affecting the behavior of the EFBI in addition to examples like the description that starts in line 174. The overall aim of the paper is to evaluate the quantifiable amount of information that EFBI provides in relation to convection trends in fires on a global scale using reanalysis weather data.

4. The EFBI is used and promoted as an index for identifying extreme fire behaviour (defined by fire spread rate), but appears to be designed to identify a small subset of those events – fires that produce deep moist convection.

EFBI evaluates the convection trend and potential conditional change around the fire using ERA5 reanalysis. It is true that fires driven by convection may be only a subset of all extreme fires, but the EFBI provides the mechanisms to identify these critical events in advance, providing essential information to firefighting services and enhancing the potential response to those fires. Currently, there are neither open and *global* datasets of fire events classified by type nor exhaustive data from vertical soundings or radar analysis of fire columns that would allow the classification of fire events and all their transitions. Although the FWI can contribute to discriminating fires according to their spread, our results show that EFBI provides an additional contribution to this discrimination on the trend of convection of the atmosphere (also caused by moisture at surface) and conditional instability.

5. The performance of the EFBI is not convincing.

In the original manuscript the NN classification of large events reached 65,46% using EFBI and 58% with FWI only. After the comments of a first reviewer, we changed only the NN reaching a 78,37% with a 1.85% of standard error (included in the answer to M.Pinto not in the manuscript read by the reviewer) and added C-Haines in the comparison. We acknowledge that EFBI does not explain all extreme events, and this fact is included in our manuscript. However, in our analysis, we show which percentage of fire events (among the selected by the described automatic method) is explained by the EFBI. Most of PyroCb papers commented by reviewer focus on single events in which the authors explain a single observed element of the fire. In our analysis, we use the EFBI without any prior knowledge of the fires and it still provides satisfactory results, which corroborates that the ideas exposed by the reviewer are still valid and agree the results hereby presented. The accuracy achieved with the EFBI in classifying fire events at the global scale (78.37%) increases the accuracy obtained with FWI and C-Haines (63.42%), showing that the EFBI provides relevant information in the characterization of fires at that scale. In addition to the analysis at the global scale, in our work, we present 3 case studies in Pedrogao (Portugal), Bolivia and Australia, which is more than the cases studied in most of the references suggested by the reviewer.

The manuscript will require substantial revision to address these concerns. In anticipation of a major rewrite, I decided not to provide a list of minor points to be addressed.

Major concerns expanded:

6. The paper contains incomplete equations with undefined terms (the EFBI equation is difficult to understand and possibly contains a sign error), and insufficient information in figure captions. A number of terms and concepts are used loosely, which make comprehension difficult for the reader. For example, the term "convective fire" is used almost synonymously with extreme fires. Given all fires produce heat and convection, the term "convective fire" needs to be clearly defined. Also, it is not clear if it refers to fires that produce deep convection columns, or specifically fires that produce moist convection (i.e., pyroCu or pyroCb). Discussions often include references to the very broad topic of "wildfire-atmosphere interactions". For clarity the reader needs to know which specific interactions are being referred to.

We thank the reviewer for noting the typo in the EFBI formulas. The typo was in the formula stating the condition CIN <= 0. CIN is negative when there is inhibition, opposite to CAPE. The manuscript was modified stating CIN >= 0 in eq.4, meaning that no convective inhibition exists. The EFBI is based on already well known meteorological concepts such as CAPE and CIN. EFBI estimates the temperature difference at surface that may cause a null or positive CIN. All the required formulae to compute the EFBI are presented in the article. Regarding the term *convective fire,* it refers to a fire which is driven by convection, as opposed to, for instance, a wind driven fire. As the reviewer mentions, fire produces convection, however fire spread is not always driven by convection as a main factor. We refer to convective a fire when it is driven mostly by convection, not the wind, topography, fuel types, fuel conditions, etc. As suggested by the reviewer, the term "convective fire" has been defined and the definition included in section. We agree with the lack of precision in some of the text in the manuscript. We have further explained the term "wildfire-atmosphere interactions" where it was present in the text.

7. There are two main topics that need to be referenced, plus a paper that the authors might wish to consider for comparison: (i) In the manuscript the EFBI is claimed to be similar to the Haines indices (Lines 73-75), but a description of these indices (and the often-used modified Haines index, C-Haines) is lacking. Most readers familiar with the Haines indices will not immediately see the similarities. Indeed, the Haines indices were designed to assess atmospheric dryness and absolute stability, whereas the EFBI assesses conditional instability. The Haines indices were developed because existing stability indices used for thunderstorm forecasting were known to be invalid for extreme fires. Given this historical progression, the EFBI design might appear to readers to be regressing. It follows that a comprehensive justification is required to demonstrate that the EFBI is a better discriminator of extreme fire behaviour than the Haines indices, as is implied in the manuscript. (ii) Recent works that develop indices and techniques for identifying and predicting pyroCb (a specific subset of extreme fire events) should also be acknowledged and compared with the EFBI. The EFBI has much in common with Potter's (2005) FireCAPE concept, Lareau and Clement's (2016) use of the Convective Condensation Level, Tory et al.'s (2018) pyroCAPE concept, the ideas that underpin Tory and Kepert's (2021) Pyrocumulonimbus Firepower Threshold (PFT), and Leach and Gibson's (2021) pyrocumulus prediction model. Section 5 of Tory et al. (2018) discusses and compares some of these concepts and ideas. (iii) The authors might like to compare their FWI/EFBI analysis of extreme fire events, with a paper by Di Virgilio et al. (2019) that performed a similar comparison using the McArthur Forest Fire Danger Index and C-Haines to analyse pyroCb events.

The authors thank the reviewer for the relevant points mentioned in this comment and the number of references provided in it. As regards the overall message in the comment, we would like to note that the EFBI does not exclusively looks into conditioned stability as the EFBI also shows the buoyancy trend. In 1988, Donald Haines provided an extremely useful and pragmatic index to estimate the potential dry and unstable air. Currently, some very severe wildfires had high C-Haines values, although sometimes that only happens when using specific pressure levels different from the ones considered in high, medium, and low versions of C-Haines. In a previous answer to the valuable comment of M. Pinto, we replied by including a figure that depicts that fact in the fire event of Pedrogao (Portugal), where C-Haines index values depict a low sensitivity in all three versions compared with the EBFI. Nowadays we have resources to compute buoyancy trends using all the levels. However, we still lack a database of fire events classified by fire type on that basis, while we only know some fire characteristics such as fire spread. Therefore, we proposed an index that can be efficiently computed on a global scale and exploits the known information about fires. In our results, we state that the EFBI provides valuable information and contributes to enhance the accuracy performance of the classification at global scale despite there is uncertainty in the datasets used due to resolution gaps and cloud coverage affecting GlobFire (based on MCD64A1). Finally, as we described in the paper and also in point number 5 above, there is an enhanced performance of the EFBI when compared to the C-Haines and FWI together or only with the FWI.

The scope of the paper was not detecting pyroCb and therefore the datasets used in the article do not include pyroCb or pyroCu detection. The comparison of the EFBI with other indices, in addition to the C-Haines, which is already considered and included in the new version of the manuscript, is out of the scope of our work. We would like to note that in most of the papers suggested by the reviewer there is not only a lack of comparison with other important works like Haines, but also, the analysis are done at national scale with very few study cases. An exception to this was the work of Di Virgilio et al. (2019), who used two different indexes and a database which covered the cases within the state of Victoria (Australia) and analyzed 196 cases to discriminate two classes of fires of which 40 were pyroCb fires and 166 were considered as standard wildfires. We found all those papers essential to describe the state of the art of the fire and atmosphere research and cited them in the revised version of the article.

8. The EFBI is introduced without sufficient explanation. The paper does not describe the underlying theory behind the index, or why the index takes this specific form, or how it compares with similar indices and concepts.

The article mixes several research fields and explains the tasks done in each with plain text for a multidisciplinary journal for Natural Hazards and Earth Sciences. In data and methods section we included the following statement: "The proposed EFBI determines the amount of increase in temperature degrees at the surface required to cause a null CIN and quantifies the change in the convective trend (addition of CAPE and CIN), allowing the prediction of fast fire spread due to convection. In cases in which the atmosphere is already unstable, CIN is equal to or greater than 0 being $\Delta T=0$, the values assigned to the index are the full integration of CAPE+CIN." followed by the formulas used to compute it and its explanation. There is also an example in section 3 with a skew-T diagram. The article does not provide any new meteorological concept but uses known concepts applied for fire danger and evaluates the results with two global datasets to see the use feasibility for fire danger under GWIS project. Therefore the work proposes a first version of the index at global scale which is comparable around the planet and ready to use with global datasets. The information provided by EFBI has been used in fire events in Europe and California during 2020 and 2021.

9. The type of convective instability being targeted is not mentioned. The EFBI

looks like it is targeting moist convection, but it is used to identify any fire that spreads rapidly. Can the decision to apply a moist-instability index to dry events be justified? In general, hot, dry and windy conditions favour extreme fire behaviour. Large values of the Haines indices correlate well with deep well-mixed layers, which have neutral stability on average. Moist plume growth, on the other hand, requires a much more specific set of conditions and consideration of the atmosphere above the mixed-layer.

The article mentions already that extreme fire behavior is not just caused by convection (line 276). The aim of the work is not modelling the fire plume, but a first approach including convection trend evaluating the usefulness of the ERA5 and GlobFire at global scale. EFBI assesses convection trend using the vertical integral of the buoyancy and how it would change with an artificial increase of the temperature at surface (defined in section 2). We agree with the reviewer that the work does not justify the decision to apply EFBI for fires not driven by convection. However, results show a considerable increase in accuracy in the classification that when using CHaines+FWI or only FWI. It would be a major improvement to use a global database with a good accuracy of classification between spread types for each moment of the fire. The study of detailed moist plume growth, transition between wind driven fires and convective and viceversa would require field measurements like doppler radar or lidar.

10. The manuscript doesn't present compelling evidence that the EFBI has significant value as an extreme-fire predictor. The scatter plot in Fig. 8 shows that large EFBI occurs more often with slow spread rates than fast spread rates. 7 shows EFBI is elevated throughout almost the entire fire period, but the extreme fire behaviour was present only on the first day. Fig. 10 shows poor correlation between EFBI and the burn rates – which is acknowledged by the authors. Fig. 12 confirms southeastern Australia was very unstable during the last few days of 2019 – a result that was also well predicted by the C-Haines and PFT indices.

The EFBI shown in the results a considerable amount of improvement to discern between fires that spread less than 1000ha and more than 10000ha in one day. It has been compared with FWI and with FWI combined with C-Haines using a machine learning approach using two main datasets and a cross-validation. The accuracy performance of the EFBI and the variability in the cross validation is included in the manuscript and in the answer provided to M. Pinto. In addition, there are more factors involved in extreme fire events, fuel availability, vertical wind profile field measurements, etc. Figure 7 shows a considerable peak of the EFBI. We conclude that EFBI could be a necessary but not sufficient condition as happens with many other fire danger indices. Figure 7 shows fire spread speed below 1 km/h only when EFBI is below 200. Figure 10 is using GlobFire for the fire spread speed (one perimeter per day with uncertainty) and is using forecast data.

The fire in Robore burnt for more than two months and was visible when looking at the entire Earth. Robore fire case transitioned from wind driven to convection driven and vice versa several times. The last case study shows how the daily maximum of the EFBI behaves spatially during the fire events in south-eastern Australia. As the reviewer mentioned, EBFI alone is not enough to detect sufficient premises for a pyroCb. However, EFBI, vertical wind profile and the rest of the fire context can be used to assess periods of extreme fire behavior. For real firefighting planning, EFBI has been used in Dixie fire with a time horizon of 10 days ahead meanwhile Tory's PFT is 24 hours ahead. Both useful, but different goals and application. EFBI leads to a planning measure for extreme fire spread at long term always keeping in mind the limitations and uncertainties.

In summary, the manuscript is rough in presentation, is missing important background information, introduces a concept without sufficient explanation and justification, and the results as presented do not convince me that the EFBI has more to offer than existing indices and extreme-fire prediction methods. I encourage the authors to consider the ideas presented in the referenced papers to see if the EFBI can be adapted to better identify extreme fire conditions, or to develop arguments that demonstrate EFBI superiority over these other indices.

The aim of the article is not to show superiority or validate other works but evaluate the usefulness of the proposal on a global scale using open data. The data used in this work has limitations, uncertainties and there is a lot of future work as stated in the paper. We could apply only FWI to detect extreme fire behavior, but we decided to quantify and evaluate the performance of a feasible method to assess convective trend at global scale. The work is a first and pragmatic approach to use a global fire event database and ERA5 weather reanalysis trying to be distinguish only two fire types by daily speed. Therefore, the article is more focused on analyzing an approach at global scale with the available data instead of creating, analyzing causes of pyroCb or comparing with previous proposals in the literature. Finally, several of the key and exceptionally good works provided by the reviewer do not evaluate performance on a global scale, validate Haines or show superiority over all previous indexes. Each work is focused on different goals and provides extremely valuable contributions. Given the distribution of fire events by speed, where slow fires are still the most common ones, it would be extremely useful to provide useful open data to countries which are starting to have these extreme fire events that are still considered as a rare phenomenon.