



EGUsphere, referee comment RC1
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Comment on egusphere-2022-92

Anonymous Referee #1

Referee comment on "Assessing the performance of various fire weather indices for wildfire occurrence in Northern Switzerland" by Daniel Steinfeld et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-92-RC1>, 2022

In short, this manuscript has too many problems/flaws to be accepted. This manuscript is characterized by:

(a) insufficient data and an inadequate methodology;

(b) have very little scientific significance, do not represent a substantial contribution to the understanding of natural hazards and their consequences, without new significant concepts, ideas, methods or data;

(c) be very confusing, with unclear, unnecessary sentences and even wrong statements;

To explain this assessment, please consider the following:

(i) the title does not adequately describe the study performed; on the one hand, the authors do more than evaluate fire meteorological indices (FWI), as they compare the FWIs among themselves, with meteorological variables and logistic models; on the other hand, they only do so in three specific locations in the Bern Canton; this is the only contribution of this study, but raises several questions;

(ii) One of these questions is how can it be said that a study is carried out for the entire

northern region of Switzerland (which is a subjective definition) when, in fact, a study is carried out in the canton of Bern, i.e., in only a sub-region of northern Switzerland?

(iii) Another question, and perhaps the most important, is how it can be said that a study is carried out for an entire region of northern Switzerland that the authors characterize as having a complex and diverse landscape, with mountainous and hilly terrain, using data from only 3 locations?

(iv) For the authors to be able to state that their results are valid for the northern region of Switzerland, or rather, for the 3 ecoregions of the northern region, they have to demonstrate that the information collected in only 3 meteorological stations (one weather station in each ecoregion) is sufficient to characterize the climate variability associated with the enormous landscape diversity (e.g., topography, vegetation), which the authors recognize characterize each of the regions;

(v) The decision to use data from only three stations is strange because the authors recognize the difficulty (or impossibility) of carrying out this study under these conditions (e.g., "...single weather station observations cannot capture the spatial heterogeneity within a complex topography...", line 141; "...weather stations are not necessarily located near the fire site and therefore don't have the same micro climate. This is especially true for the complex and diverse landscape of Switzerland with its steep topography and mixed forest types, and also due to local wind systems and localized convective precipitation during summers that may or may not hit a weather station. In addition, weather stations are located in open areas and may not present fuel moisture in forests", lines 366-370); the problem is not necessarily the use of only data from weather stations, which provide precious local meteorological information, but the small number of stations used in the study and the non-complementarity with other existing databases;

(vi) apparently, the authors confuse fundamental concepts; for example, is this study about fires or wildfires? The authors seem to consider that danger is equal to hazard or risk; the concepts are related, but they are not (definitely) the same; some examples are:

- "... Fire weather indices are widely used to understand and assess meteorological fire hazard"..., (Line 1)
- "... provide a measure of the daily fire danger, i.e., the risk of fire occurrence"..., (line 14);
- sometimes (lines 334-337) "a fire danger rating system consisting of multiple, seasonally and regionally varying indices could be implemented for real-time prediction using meteorological forecasts, which is also proposed by Reineking et al. (2010) for the southern Canton Ticino. This could improve the risk assessment ..."; or, the same approach can be used "to improve fire hazard prediction in the study area" (lines 390-392).

(vii) The authors opted for methodological approaches that they have to explain because they do not seem to be the most appropriate. The authors aim to “estimate”/relate the fire occurrences/number of fire days with FWIs and meteorological variables. However, as the authors know and can check (<https://cwfis.cfs.nrcan.gc.ca/background/summary/fwi>), the “The Canadian Forest Fire Weather Index (FWI) System consists of six components that account for the effects of fuel moisture and weather conditions on fire behaviour”; It is obvious that suitable weather and fuel humidity conditions for fire behaviour are also suitable for ignition, but the indices used by the authors were not developed to assess the impact of weather conditions and fuel humidity on the number of fires. The authors also recognize this, as they state “Because fire weather indices are based solely on meteorological information, they cannot provide perfect prediction of fire occurrence, but rather measure antecedent conditions (Andela et al., 2017). Furthermore, they are based on empirically derived correlations between weather and fire for specific climatic and vegetation conditions. This means that the transferability of the indices to other regions and under changing climatic conditions is limited” (lines 33-37); in addition, the authors state (lines 28-35, 193) and show (e.g., figure 2) that: (a) natural wildfires (caused by weather conditions/lightning strikes) are only a tiny fraction of all wildfires; (b) the vast majority of the wildfires are caused by humans; (c) “Fires with an unknown ignition source are probably human-caused”, and (d) (consequently) “Other non-meteorological factors that we have not considered here play a critical role in fire occurrence, particularly human activities to ignite but also to prevent fires”. This means that if the authors intend to use the FWIs as factors/parameters/predictors of the number of fires, must start to model the number of fires/fire days as a function of the FWIs; it is not sufficient to present “percentile score during fire days... percentile score during fire days” (Figure 3) or “Time series of yearly number of recorded fire days... extreme index days ... and extreme logit days” (Figure 4), because much unrelated time series present similar variability.

(viii) the authors state that “Because fire weather indices are based solely on meteorological information, they cannot provide perfect prediction of fire occurrence, but rather measure antecedent conditions (Andela et al., 2017). Furthermore, they are based on empirically derived correlations between weather and fire for specific climatic and vegetation conditions. This means that the transferability of the indices to other regions and under changing climatic conditions is limited (Weibel et al., 2010; Reineking et al., 2010; Krawchuk and Moritz, 2011), and their application outside the “area of origin” requires careful evaluation and adaptation (Weibel, 2009; Wotton et al., 2009; Padilla et al., 2011; de Jong et al., 2016; Bekar et al., 2020).” So, my obvious question is: did you adapt the FWIs to the characteristics of the study region before trying to assess their performance or relate them to the fire incidence? Can the author assess or compare FWIs without prior adaptation/calibration?

(ix) the authors often use “significant” and “significantly” to describe your results. What is the relationship between these adjectives and statistical significance? Which tests did you use to assess the statistical significance? What is the statistical significance of your results?

(x) Language is often confusing and incorrect. Some examples are the following:

- "However, in complex regions such as Switzerland with mountainous and hilly terrain, it is difficult to select an appropriate index.", (lines 1-2); "It is difficult to find an appropriate fire weather index, especially for regions with complex and diverse terrain", (line 41). Why is it so difficult? is it not enough to make a performance and comparative analysis? Where does the difficulty lie? The authors provide an answer to this question, but they did not carry out this study in the way they claim to be appropriate;
- "Fire weather indices are commonly used by fire management agencies to assess and predict weather conditions that are most conducive to wildfire" (lines 13-14); it is obvious that it is not the FWIs that can predict the weather, but the atmospheric models;
- "Because it is a linear model using only daily means as input"..., (line 134); if I understand correctly, one "input" is the "WeekRain" which is not a daily mean;
- The authors state (and repeat the idea) that "... statistical models trained on local fire statistics indirectly incorporate non-meteorological factors while still using only meteorological information as input"; as stated, this is not true. If this was true, why do you state that "Other non-meteorological factors that we have not considered here play a critical role in fire occurrence, particularly human activities to ignite but also to prevent fires. A first step in overcoming these limitations is to combine fire weather indices with non-meteorological factors such as forest composition, topography, and human activities"?
- The authors did not develop models of the fire days based on the FWI, meteorological variables or logit; however, in section 3.4, they use concepts of underestimation and overestimation!
- "how fire behaviour changes from spring to summer due to the soil layers", what does this mean? What properties of the soil layers?
- Lines 306-308, what situation? Summer, dry periods or convective precipitation?
- Lines 324-329: the used dataset includes foehn winds?
- The authors should avoid using subjective concepts, such as large, small, and slight, especially without providing some figures to clarify;

(xi) Although accepting the results presented as sufficient to justify a publication, the discussion of results is poor, especially considering the current knowledge on the subjects covered in the manuscript, which is not conveniently mentioned in the main text of the manuscript and portrayed in the reference list.

Given the serious problems existing at this stage of the study/manuscript, suggestions/comments on more specific issues and minor flaws will not be presented.