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Comment on nhess-2021-285

Anonymous Referee #1

Referee comment on "Global assessment and mapping of ecological vulnerability to wildfires" by Fátima Arrogante-Funes et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-285-RC1>, 2022

GENERAL COMMENTS

The paper addresses an important and timely topic, with a global scale analysis. It relies on a large set of pre-existing global maps of 11 variables, or indicators, deemed relevant for the purpose of assessing ecological vulnerability to wildfires. These indicators are organized in a four-level hierarchy, with ecological vulnerability at the apex.

I think the paper has problems in three domains: i) indicators/variables chosen and indices derived from them; ii) indicator/variable aggregation procedure; iii) validation and uncertainty/sensitivity analysis.

SPECIFIC COMMENTS

i)

Explanation of the meaning and justification for the choice of first-level is clear for those involved in the calculation of the second-level Biological "distinction" (BD) and Conservation "state" (CS) indices, which are well-grounded on relevant concepts from fire ecology, landscape ecology and conservation ecology. However, Potential Soil Erosion (PSE) and Adaptation to Fire (AF) indices, at the same hierarchical level, are questionable, and problems become apparent in the higher-level index resulting from their integration, Post-Fire Regeneration Delay (PFRD).

Regarding PSE, the authors of the RUSLE map used in the analysis (Borrelli et al., 2017) state that it does not consider the short-term effects of fire, and clarify that it deals only with land cover / land use change (Pg. 10, Methods – soil erosion modelling). The authors of the present paper need to acknowledge this and discuss how it may affect use of the RUSLE map for their assessment of ecological vulnerability. In addition, Borrelli et al. (2017) state that "...potential overall increase in global soil erosion driven by cropland expansion. The greatest increases are predicted to occur in Sub-Saharan Africa, South America, and Southeast Asia...". However, Grégoire et al. (2012) and Andela et al. (2014) showed that cropland expansion in Africa, especially in the Northern Hemisphere, was responsible for substantial reductions in area burned. Taking this into account, and considering the importance of Africa in the global fire scene, does it make sense to include this indicator in the assessment of vulnerability to fire when it is associated with land use changes that strongly reduce fire incidence? An increase in erosion is expected in association with a land use change that reduces fire incidence. Similar processes may occur wherever land use intensification leads to a decrease in fire incidence.

It is harder to comment on AF, because the authors are quite confused, here. They

misunderstood Shlisky et al. (2007) and state that fire regimes may be "fire-dependent", "sensitive", and "independent", when these labels apply not to fire regimes, but to ecosystems, or ecoregions (more on this in Detailed comments, below). The authors need to sort out these issues.

PSE and AF are integrated to form PFRD, which is described as "...an indicator of the difficulties faced by the environment when recovering naturally from fire." This index produces some strange results. How can its values be high in Zambia, NE Angola, parts of the Sudanian savannas of NH Africa, and the Llanos of Colombia/Venezuela, where Net Primary Productivity (NPP) and fire frequency are very high, but low in boreal forests, where NPP is much lower and fire return intervals much longer? What is it really measuring? Would the calculation of PFRD not benefit from incorporating NPP? In what sense can it be said that these tropical savanna areas less fire-resilient than e.g. boreal forests? A second issue arises with the calculation of PFRD for the northernmost regions of Canada (High Arctic Tundra ecoregion). Why is PFRD Very High there, while the region just to the south of it (Middle Arctic Tundra) is considered "Without fuel / No data"? How do you justify that, since it implies a reversal of the expected latitudinal gradient in vegetation abundance and fire incidence? If you had no fuel, or no data to perform the calculation in the Middle Arctic Tundra, how can you do it for the High Arctic Tundra?

ii)

The authors aggregate their variables/indicators into first-level indices, and then aggregate these indices up the hierarchy using cross-tabulation. This is approach, which is simple to implement, has drawbacks. It requires variable discretization, which wastes information, and requires hard to justify, implicit decisions on the numbers of classes, and on the positions of the thresholds between classes. It ignores issues of compensation between indicators and implicitly weights all indicators equally. At a minimum, the authors need to justify these implicit decisions, but it would be preferable to aggregate the variables/indicators using one of several available multicriteria methods, namely those revised by El Gibari et al. (2019) for the specific purpose of building composite indicators, or indices.

iii)

Validation of composite indices often is problematic, because they deal with unmeasurable criteria, or are not meant to predict an effective impact but to estimate a risk or a potential effect (Bockstaller and Girardin, 2003, Moriarty et al., 2018). This is the case for the present paper, and the authors acknowledge it in lines 558-560. However, that does not imply the issue can be ignored, or postponed for future research, as the authors propose to do. Given the constraints on empirical validation of the proposed index, it becomes especially important to focus on conceptual, or design validation (Bockstaller and Girardin, 2003), i.e. assessment of the scientific quality of the construction or design of the index, and on sensitivity / uncertainty analysis of the implications of decisions made while constructing the index (Saisana et al., 2005; Tate, 2012), Therefore, I urge the authors to strengthen their defense of the scientific quality of the index, both in terms of the variables chosen and the way they are aggregated. They also need to perform a sensitivity analysis of the key implications of the decisions implicit in variable discretization and in the chain of cross-tabulations implemented. This is essential to demonstrate the validity and reliability of the index and to facilitate its proper use.

References

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cropland expansion and El Niño to La Niña transition. *Nature Climate Change*, 4(9), 791-795.

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Saisana, M., Saltelli, A., & Tarantola, S. (2005). Uncertainty and sensitivity analysis techniques as tools for the quality assessment of composite indicators. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 168(2), 307-323.

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CORRECTIONS (AND A FEW MORE SPECIFIC COMMENTS)

Line 13: "biological distinction" does not sound right in English. I believe "biological distinctiveness" is preferable. Please correct throughout the text.

Lines 15-16: why did you choose to combine the various indicators using qualitative cross-tabulation? This option needs a justification, because there are alternatives, e.g. multicriteria evaluation.

Line 37: forest "masses" is not used in English. Please replace with "stands", or "patches".

Line 41: Are you really talking about fires in forests, only? Or are you using the term in a broader (and inappropriate) sense of vegetation fires?

Lines 47-50: In terms of natural hazards terminology, I recommend using the United Nations International Strategy for Disaster Reduction (UNISDR) terminology on disaster risk reduction (2009). It considers that risk assessment involves the combination of hazard, exposure, and vulnerability, according to the definitions proposed in that glossary.

Line 62-64: Exposure is not a part of vulnerability and is defined somewhat differently from the way you use it. I quote from UNISDR, 2009: "Exposure - People, property, systems, or other elements present in hazard zones that are thereby subject to potential losses. Comment: Measures of exposure can include the number of people or types of assets in an area. These can be combined with the specific vulnerability of the exposed elements to any particular hazard to estimate the quantitative risks associated with that hazard in the area of interest."

Line 63: I believe that the standard use in the specialized literature is to define "index" as the result of aggregating two or more "indicators". This terminology has the advantage of distinguishing different levels of the analysis hierarchy. You use "index" for all levels, which occasionally is confusing. Please consider adopting the distinction indicator/index in the text.

Lines 80-81: This explanation of how adaptation to fire was estimated is too vague, please elaborate, including identification of the dynamic global vegetation model used in the analysis. Otherwise, it is not possible to evaluate the adequacy of this indicator.

Line 85: is "exceptionality" what was previously called "distinction"? Please clarify and use consistent terminology, avoiding "distinction".

Lines 114-116: Terminology: "distinction", "index". Also "state", where it should read "status".

Line 127: The value 14 did not change.

Line 136-137: Already mentioned, can be deleted.

Line 195: "Monotonic", not "monotonous".

Line 258: Where do the weights come from, how were they obtained? Table 3 shows "Maximum scores", not weights. Are they the same thing? If so, please consistently use a single term to refer to the concept.

Line 267: You don't integrate the index, you integrate lower level indicators, to create the index.

Line 280: This section is confusing. It is not fire regimes that are "fire-dependent", "sensitive", and "independent", it is the structure and function of ecosystems, or ecoregions. Notice that a fire-independent fire regime would be a nonsensical concept. See Shlisky et al. (2007), pg. 5: "Ecosystems can be classified in terms of their relationship to fire regime characteristics such as fuels, flammability, ignitions, and fire spread conditions within a given ecosystem." The authors need to improve their understanding of the concepts they are using here.

Line 281: "Maps", not "cartographies".

Line 314: Is a "factor" the same as an "indicator"? Please use the technical terminology consistently.

Line 330: I don't understand the meaning of "in which the most valuable component was prioritized". Please clarify. The procedure described here is a cross-tabulation of a cross-tabulation. It is pertinent to question the sensitivity of your results to the decisions implicit in the methodology, namely number of levels and placement of thresholds, especially when accumulating the results of successive cross-tabulations. I realize that to the paper is essentially normative, in the sense that it prescribes a procedure to assess an index that is not directly measurable, and for which empirical validation may not be feasible, as the authors acknowledge. However, this should not exempt the authors from performing a sensitivity and uncertainty analysis of the implications of decisions on the discretization of quantitative variables, and on the procedures used to weight and integrate them.

Lines 346-347: Florida and Thailand are not located in temperate zones of the globe.

Line 363: ...neither is the Yucatan Peninsula.

Line 366: It is surprising to see Zambia and NE Angola mapped with a very high Post-fire Regeneration Delay, especially considering how often they burn. Please clarify this apparent inconsistency.

Line 375: Are "potential ecological damages" what is called Ecological Value before and after this point? Please clarify and use the terminology consistently.

Line 393: Section 3.3.2.: This section reports too many numbers in text format. Just stress the key points of Table 10 in the text and use charts and graphs to summarize the rest, if necessary.

Line 395: Are "Ecological Indices" yet another name to "Ecological Value"? Please clarify and keep the terminology consistent.

Line 396: Your analysis is static. How can it be influenced by fire trends? What do you mean? Also, several of the vulnerable areas are not Forests, e.g. Tundra and Mangroves. Are you using "forest fire" to refer to all vegetation fires, regardless of the type of ecosystem where they occur? Please avoid doing this and use the more generic expression "vegetation fires" when not strictly referring to fires in forests. The same applies to the text in lines 401-403.

Line 446: Table 10 – what is the criterion to order the biomes in the Table?

Lines 457-458: "...to a lesser extent, by the way we combined the factors in the different indices." How can you know that this is a less important factor if you did not perform an uncertainty / sensitivity analysis? Please clarify.

Line 557: "intuitive" does not sound like a good term, since it is the opposite of objective and rational. I suggest you replace the term by "easily understood", or something similar.

Lines 579-580: The unfeasibility of empirical validation of your index against a set of objective, measurable data makes it especially important to perform a sensitivity / uncertainty analysis, so that potential users understand the strengths and weaknesses of the index, and use it properly. It cannot be postponed to a subsequent paper and should

be included in this one.