

Nat. Hazards Earth Syst. Sci. Discuss., author comment AC2  
<https://doi.org/10.5194/nhess-2021-285-AC2>, 2022  
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## Reply on RC1

Fátima Arrogante-Funes et al.

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Author 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-AC2>, 2022

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Dear referee,

Thank you for your time on our manuscript and thoughtful comments, as well as for highlighting the weaknesses of this version. We take their recommendations very seriously and revise the manuscript accordingly. Your input is positive for us and by following your suggestions we will be able to strengthen the wording, methodology and discussion.

We hope that we have given the necessary answers to the suggestions and addressed all your doubts so that it is suitable for publication.

We have provided a detailed response to their comments below. Your comments are in bold and our responses in normal font.

## REPLY

### 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.**

First of all, we would like to thank the reviewer for his/her review of the manuscript, constructive comments and suggestions to improve it. We have carefully considered

his/her comments, especially for his/her effort to strengthen the methodology, discussion and clarity of the manuscript.

## **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.**

First, thanks for the appreciation, we are going to work more on understanding this part of the study. RUSLE has not been directly applied as a potential indicator of post-fire soil erosion. The tables that the FAO considers appropriate to apply to the RUSLE have been applied to it in order to obtain the potential RUSLE for other phenomena FAO/UNEP/UNESCO. (1979) such as runoff, flooding, etc. Based on the bibliography, we have implemented this methodology in our study as other authors have already done (Chuvieco et al., 2014, Chuvieco et al., 2010).

On the other hand, in relation to the specific case of Africa, there are many parts of the world (Mediterranean countries, north America, China) where the occurrence of fire is favoured by human beings (Ortega et al., 2012; Tao et al., 2013; Mildrexler et al., 2009; Scipioni & Tagliaferri, 2009). As cropland increases, the agricultural-forest interface increases, consequently increasing the probability of ignition of forest land. It cannot be forgotten that in the occurrence of fire we not only have natural causes but also human ones.

In addition, the objective of this work is not focused on occurrence since this is included in danger/hazard and not in vulnerability that encompasses the potential damage that a system may suffer from different external agents (IPCC, 2007) (lines 54-58). In this risk section, exposure in terms of population and assets, ecosystems, among others, is not taken into account either (as you point out in the suggestion that we use the United Nations International Strategy for Disaster Reduction (UNISDR) in lower sections of this post). Perhaps these comments that you suggest in relation to the probability of occurrence of fire in these certain areas of Africa would be of greater interest for works with the objective of evaluating risk or danger, but not for one that has vulnerability as its objective.

Chuvieco, E., Aguado, I., Yebra, M., Nieto, H., Salas, J., Martín, M. P., Vilar, L., Martínez, J., Martín, S., Ibarra, P., de la Riva, J., Baeza, J., Rodríguez, F., Molina, J. R., Herrera, M. A., & Zamora, R. (2010). Development of a framework for fire risk assessment using remote sensing and geographic information system technologies. *Ecological Modelling*, 221(1), 46–58. <https://doi.org/10.1016/j.ecolmodel.2008.11.017>

Chuvieco, E., Martínez, S., Román, M. V., Hantson, S., & Pettinari, M. L. (2014). Integration of ecological and socio-economic factors to assess global vulnerability to wildfire. *Global Ecology and Biogeography*, 23(2), 245– 258. <https://doi.org/10.1111/geb.12095>

FAO/UNEP/UNESCO. (1979). A provisional methodology for soil degradation assessment. Food and 692 Agricultural Organization of the United Nations, Rome.

Mildrexler, D. J., Zhao, M., & Running, S. W. (2009). Testing a MODIS global disturbance index across North America. *Remote Sensing of Environment*, 113(10), 2103-2117.

Ortega, M., Saura, S., González-Ávila, S., Gómez-Sanz, V., & Elena-Rosselló, R. (2012). Landscape vulnerability to wildfires at the forest-agriculture interface: half-century patterns in Spain assessed through the SISPARES monitoring framework. *Agroforestry systems*, 85(3), 331-349.

Scipioni, A., & Tagliaferri, F. (2009, April). Integrated services to support detection, prevention and planning of the agricultural-forest-rural land against fires. In EGU General Assembly Conference Abstracts (p. 10252).

Tao, J., Zhang, Y., Yuan, X., Wang, J., & Zhang, X. (2013). Analysis of forest fires in Northeast China from 2003 to 2011. *International Journal of Remote Sensing*, 34(22), 8235-8251.

**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 is produces some starge 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?**

In this work, with the aim of estimating the ecological vulnerability to fires on a global

scale in the PFRD section, AF (adaptation of the ecosystem to fire) and PSE (potential soil erosion due to the fire) were taken into account. According to some experts (Duro et al., 2007; Nagendra & Rocchini, 2008), the NPP is not a good environment proxy (I understand that you are referring to this) since high values of NPP do not imply that the vegetal cover that is settling after a fire belongs to the previous vegetal formation. The first stages after the occurrence of fires are related to herbaceous patches. And then, if there is land available to settle down between the different conditions of light and water, the woody plants would begin to proliferate. In addition, the times, the trend, the area, among others, determine the way in which this influences the NPP. If this work were to focus on a local/regional scale, the characterization of this NPP as a recovery proxy measured based on time series of fire perimeters could be possible through different techniques, as proposed by Viana-Soto et al. 2020 and Viana-Soto et al. (2022). But, for a study on a smaller work scale, such as the global scale of this work, it would be far from representing reality since calibrating said variable for the whole world is a challenge. That is why its inclusion was ruled out for this first approximation of ecological vulnerability to fire. Likewise, in the first stages of this work, tests were carried out and the inclusion of the NPP was assessed. In addition, as reflected in the extensive bibliography, other works were reviewed in which this variable was not included in the AF part (adaptation to fire) (Turner et al., 2003; Duguay et al., 2012; Aretano et al. 2015). Subsequently, as we have explained now, we proceeded to withdraw it.

As for what the PFRD measures, the potential deterioration suffered by the soil as a result of a fire through the RUSLE and the tables provided by the FAO in terms of recovery (PSE), applicable to the RUSLE, previously used by other authors for the phenomenon of fire. In other words, first the time it would take for the soil to recover a good state is potentially measured. In addition, it also takes into account how the ecosystem is in relation to fire (AF), which provides a potential measure of the regeneration capacity of ecosystems or the potential response of ecosystems to fire. With this, as in other works (Chuvienco et al., 2014), the potential AF could be estimated.

On the other hand, thank you very much for providing information from different parts of the world such as Africa or South America. This will be very useful in the discussion section. The zones that you indicate (Africa, South America, Boreal Zones) are very specific parts of the extensive ecoregion to which they belong. It is very interesting to know in more detail these possible inconsistencies in order to explain the limitations of this model. But certainly, we consider that it is very important not to forget that the objective of this article is on a global scale and that is why the information provided is much more general than what could be expected from a local/regional scale in which the detail of those areas that you mention would have a greater differentiation. In addition, the input variables in some cases come from vector maps with a single data per ecoregion, as is the case of Shlisky et al., (2017). This map is generated through the transfer of knowledge from different works over the years on the ecoregion. And, based on this, hence the justification for our spatial unit to be the ecoregions. This is the information that exists on a global scale and consequently also limits the results, generalizing the different zones that make up the ecoregion.

Thank you very much for your appreciation, it will substantially enrich the discussion of the results in the next version of the manuscript.

Aretano, R., Semeraro, T., Petrosillo, I., De Marco, A., Pasimeni, M. R., & Zurlini, G. (2015). Mapping ecological vulnerability to fire for effective conservation management of natural protected areas. *Ecological Modelling*, 295, 163–175.  
<https://doi.org/10.1016/j.ecolmodel.2014.09.017>

Chuvieco, E., Martínez, S., Román, M. V., Hantson, S., & Pettinari, M. L. (2014). Integration of ecological and socio-economic factors to assess global vulnerability to wildfire. *Global Ecology and Biogeography*, 23(2), 245– 258. <https://doi.org/10.1111/geb.12095>

Duguay, B., Alloza, J. A., Baeza, M. J., De La Riva, J., Echeverría, M., Ibarra, P., Llovet, J., Cabello, F. P., Rovira, 680 P., & Vallejo, R. V. (2012). Modelling the ecological vulnerability to forest fires in mediterranean ecosystems 681 using geographic information technologies. *Environmental Management*, 50(6), 1012–1026. 682 <https://doi.org/10.1007/s00267-012-9933-3>

Duro, D. C., Coops, N. C., Wulder, M. A., & Han, T. (2007). Development of a large area biodiversity monitoring system driven by remote sensing. *Progress in Physical Geography*, 31(3), 235–260. <https://doi.org/10.1177/0309133307079054>

Nagendra, H., & Rocchini, D. (2008). High resolution satellite imagery for tropical biodiversity studies: The devil is in the detail. *Biodiversity and Conservation*, 17(14), 3431–3442. <https://doi.org/10.1007/s10531-008-9479-0>

Shlisky, A., Waugh, J., Gonzalez, P., Gonzalez, M., Manta, M., Santoso, H., Alvarado, E., Ainuddin, A., Rodríguez-trejo, D. A., Swaty, R., Schmidt, D., Kaufmann, M., Myers, R., Alencar, A., Kearns, F., Johnson, D., Smith, J., & Zollner, D. (2007). Fire, ecosystems and people : threats and strategies for global biodiversity conservation. The Nature Conservancy Global Fire Initiative Technical Report, 17. [http://mrcc.isws.illinois.edu/living\\_wx/wildfires/fire\\_ecosystems\\_and\\_people.pdf](http://mrcc.isws.illinois.edu/living_wx/wildfires/fire_ecosystems_and_people.pdf)

Turner, B. L., Kasperson, R. E., Matsone, P. A., McCarthy, J. J., Corell, R. W., Christensene, L., Eckley, N., Kasperson, J. X., Luers, A., Martello, M. L., Polsky, C., Pulsipher, A., & Schiller, A. (2003). A framework for vulnerability analysis in sustainability science. *Proceedings of the National Academy of Sciences of the United States of America*, 100(14), 8074–8079. <https://doi.org/10.1073/pnas.1231335100>

Viana-Soto, A., Aguado, I., Salas, J., & García, M. (2020). Identifying post-fire recovery trajectories and driving factors using landsat time series in fire-prone mediterranean pine forests. *Remote Sensing*, 12(9), 1499.

Viana-Soto, A., García, M., Aguado, I., & Salas, J. (2022). Assessing post-fire forest structure recovery by combining LiDAR data and Landsat time series in Mediterranean pine forests. *International Journal of Applied Earth Observation and Geoinformation*, 108, 102754.

**A second issue arises with the calculation of PFRD for the northernmost regions of Canada (High Arctic Tundra ecoregion). Why is PRFD 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?**

Of the 11 input variables that this study has, when in an ecoregion there is no representativeness due to the fact that some of them present for that NoData zone, information not available, incorrect information or perhaps, it does not have a vegetation cover consistent enough to able to host a fire, the ecoregion is removed. The reasons for this were, firstly, because the objective of the work is in tune with fire and that is why any

ecoregion that does not contain sufficient plant cover to house said phenomenon lacks interest for the objective of this work.

Second, if we have the ecological vulnerability to fires calculated differently in each ecoregion based on different variables, the work loses the objective of being global since the ecoregions could never be comparable to each other and this would mean that they could not be estimated. vulnerability categories for the whole world, for this reason the ecoregions that cannot host these 11 starting variables were left out.

Certainly, one of the challenges of working on a global scale is the lack of precision in the data, the poverty of the data and sometimes the unreliability of the data in certain parts of the world. However, understanding these limitations, it is possible to generate approximate models that allow us to understand in general terms what is happening in the world without setting demographic limits or any other partition of a human nature.

Based on this appreciation, we are assessing the possibility of being able to filter the ecoregions based on the occurrence of fires in order to only take into account those where the incidence is clearly real or in a greater quantity. Thus, as you tell us, probably some Tundra ecoregions could be removed. Depending on the result we will assess its inclusion.

Your comments are very beneficial for this work, to explain the limitations of the model as well as to validate and improve the discussion. I greatly appreciate the effort to find these weaknesses in order to strengthen these aspects in the next version.

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.**

Thanks for the suggestions. The multicriteria analysis for a local/regional scale is interesting and enriching since it is easier to find a panel of experts representative of the territory. But in contrast, for a global scale it would not be relevant given the difficulty of finding representatives of the entire territory of the Earth. This would result in a biased study based on the territories of which it was or was not representative (Borrero & Henao, 2017; Hämäläinen & Alaja, 2008). For this reason, it was decided to use the cross-tabulation integration methodology that tries to be as objective as possible, also used in spatial studies at global scale (Chuvieco et al., 2014) or at local/regional scale (Arrogante-Funes et al., 2020; Martínez-Vega et al., 2007; Isabel et al., 2003)

Based on this first study and initiation on the global scale of ecological vulnerability to fires, we have detected the limitations of using classic heuristic methods and that is why we are developing improvements using Auto ML models and Fuzzy algorithms in order to avoid bias. that these methods cause (Bruzón et al. 2021). Certainly, these new works arise from this first exploration and that is why implementing something different would be a new work and would not fulfill its idea.

In relation to justifying the intervals, given the disparity of the sample due to having such an extremely large study area, it was decided to divide it according to quantiles. This decision will be justified based on bibliography in which this method is used, through works such as Pereira et al., (2020), Xing & Ree, (2017), among others.

Bruzón, A. G., Arrogante-Funes, P., Arrogante-Funes, F., Martín-González, F., Novillo, C. J., Fernández, R. R., ... & Ramos-Bernal, R. N. (2021). Landslide susceptibility assessment using an AutoML framework. *International journal of environmental research and public health*, 18(20), 10971.

Pereira, H. M., Rosa, I. M., Martins, I. S., Kim, H., Leadley, P., Popp, A., ... & Alkemade, R. (2020). Global trends in biodiversity and ecosystem services from 1900 to 2050. *bioRxiv* (Preprint).

Xing, Y., & Ree, R. H. (2017). Uplift-driven diversification in the Hengduan Mountains, a temperate biodiversity hotspot. *Proceedings of the National Academy of Sciences*, 114(17), E3444-E3451.

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.**

Thanks for finding the weakness of the manuscript. It will be done for the next version of the manuscript. We are going to develop a EFAST analysis in order to know which variable has the greatest effect on the result (Saltelli et al., 1999). Several works from different sectors of the knowledge use this method to measure the global sensitivity of the output (Ciffroy, 2020; Gómez-Delgado & Tarantola, 2006; Xing et al., 2017; Wei & Hua, 2014).

Saltelli, A., Tarantola, S., & Chan, K. S. (1999). A quantitative model-independent method for global sensitivity analysis of model output. *Technometrics*, 41(1), 39-56.

Ciffroy, P. (2020). A comprehensive probabilistic approach for integrating and separating natural variability and parametric uncertainty in the prediction of distribution coefficient of radionuclides in rivers. *Journal of Environmental Radioactivity*, 225, 106371.

Gómez-Delgado, M., & Tarantola, S. (2006). GLOBAL sensitivity analysis, GIS and multi-criteria evaluation for a sustainable planning of a hazardous waste disposal site in Spain. *International Journal of Geographical Information Science*, 20(4), 449-466.

Xing, H., Xiang, S., Xu, X., Feng, H., Yang, G., & Chen, Z. (2017). Global sensitivity analysis of AquaCrop crop model parameters based on EFAST method. *Scientia Agricultura Sinica*, 50(1), 64-76.

Wei, H., & Hua, Y. (2014). EFAST Method for Global Sensitivity Analysis of Remote Sensing Model's Parameters. *Remote sensing technology and application*, 28(5), 836-843.

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**Borrelli, P., Robinson, D. A., Fleischer, L. R., Lugato, E., Ballabio, C., Alewell, C., ... & Panagos, P. (2017). An assessment of the global impact of 21st century land use change on soil erosion. *Nature Communications*, 8(1), 1-13.**

**Grégoire, J. M., Eva, H. D., Belward, A. S., Palumbo, I., Simonetti, D., & Brink, A. (2012).**

**Effect of land-cover change on Africa's burnt area. *International Journal of Wildland Fire*, 22(2), 107-120.**

**Andela, N., & Van Der Werf, G. R. (2014). Recent trends in African fires driven by cropland expansion and El Niño to La Niña transition. *Nature Climate Change*, 4(9), 791-795.**

**El Gibari, S., Gómez, T., & Ruiz, F. (2019). Building composite indicators using multicriteria methods: a review. *Journal of Business Economics*, 89(1), 1-24.**

**Bockstaller, C., & Girardin, P. (2003). How to validate environmental indicators.**

***Agricultural Systems*, 76(2), 639-653.**

**Moriarty, P. E., Hodgson, E. E., Froehlich, H. E., Hennessey, S. M., Marshall, K. N., Oken, K. L., ... & Stawitz, C. C. (2018). The need for validation of ecological indices. *Ecological Indicators*, 84, 546-552.**

**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.**

**Tate, E. (2012). Social vulnerability indices: a comparative assessment using uncertainty and sensitivity analysis. *Natural Hazards*, 63(2), 325-347.**

## 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.**

It will be done for the next version of the manuscript.

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

Thank for the appreciation. It will be done in the next version of the manuscript. Based on the various previous studies that used this method, we are going to justify the use of qualitative cross tabulation with it, such as the works by Arrogante-Funes et al., (2020), Chuvieco et al., (2014), Martínez-Vega et al., (2007), Isabel et al., (2003), among others.

On the other hand, the multicriteria analysis for a local/regional scale is interesting and enriching since it is easier to find a panel of experts representative of the territory. But in contrast, for a global scale it would not be relevant given the difficulty of finding representatives of the entire territory of the Earth. This would result in a biased study based on the territories of which it was or was not representative (Borrero & Henao, 2017; Hämäläinen & Alaja, 2008). For this reason, it was decided to use the cross-tabulation integration methodology that tries to be as objective as possible, also used in the previous spatial studies (previous paragraph).

Arrogante-Funes, P., Bruzón, A. G., Arrogante-Funes, F., Ramos-Bernal, R. N., & Vázquez-Jiménez, R. (2021). Integration of vulnerability and hazard factors for landslide risk assessment. *International journal of environmental research and public health*, 18(22), 11987.

Borrero, S., & Henao, F. (2017). Can managers be really objective? Bias in multicriteria decision analysis. *Academy of Strategic Management Journal*, 16(1), 244-259.

Chuvieco, E., Martínez, S., Román, M. V., Hantson, S., & Pettinari, M. L. (2014). Integration of ecological and socio-economic factors to assess global vulnerability to wildfire. *Global Ecology and Biogeography*, 23(2), 245-258.

Hämäläinen, R. P., & Alaja, S. (2008). The threat of weighting biases in environmental decision analysis. *Ecological Economics*, 68(1-2), 556-569.

Isabel, M. P. M., Calcerrada, R. R., & Vega, J. M. (2003). Valoración del paisaje en la zona de especial protección de aves carrizales y sotos de Aranjuez (Comunidad de Madrid). *Geofocus: Revista Internacional de Ciencia y Tecnología de la Información Geográfica*, (3), 2.

Martínez Vega, J., Romero Calcerrada, R., & Echavarría Daspet, P. (2007). Valoración paisajística y ecológica de la Comunidad de Madrid: su integración en un índice sintético de riesgo de incendios forestales.

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

Thanks for the appreciation. It will be done in the next version of the manuscript.

**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?**

The term forest outside of a local/regional scale has more than 200 definitions according to the FAO (UNEP,2020). As Chazdon et al., (2016) collected in his study, each of these definitions, depending on the sector of knowledge from which it is spoken, refers to industrial plantations, natural plantations without silvicultural treatments, only trees, trees and any woody formation, other forms of life such as animals, forests dependent on human communities... The controversy of the term Forest continues today.

On the other hand, what seems to be agreement is what "is not a forest", places such as urban areas, farmland, water, ice... And precisely these areas have not been considered in our study. Ecoregions belonging to areas of ice, bare ground and rock were removed (lines 125-127). Through the variable Burnable Area we are left with the areas (or pixels) where there is vegetation cover (lines 129-138). In conclusion, and given the uncertainty of the term, we could speak of Forest Fires in our work.

It is for this reason that in the treatment of the forest area we have tried to be as neutral as possible in order to accommodate the more than 200 existing definitions throughout the world, since the scale of work is global and it is necessary that, this work is as relevant as possible to any part of the world.

In any case, and following your recommendation, the document will be rigorously revised. In the document, as can be seen in its title, the term Wildfire appears, it is proposed to replace by Forest Fire, if you agree with it.

Chazdon, R. L., Brancalion, P. H., Laestadius, L., Bennett-Curry, A., Buckingham, K., Kumar, C., ... & Wilson, S. J. (2016). When is a forest a forest? Forest concepts and definitions in the era of forest and landscape restoration. *Ambio*, 45(5), 538-550.

United Nations Environment Programme (2020). The State of the World's Forests 2020: Forests, Biodiversity and People. <https://wedocs.unep.org/20.500.11822/32472>.

**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.**

Thanks for the recommendation. It is possible that the introduction was modified based on your proposal for the next version of the manuscript. Certainly, for the following works we are already following this scheme.

**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.”**

Thanks for the suggestion and for the clarification. It will be modified in the next version. Certainly, we refer to exposure to all the elements of the territory that can be damaged, they would be the so-called potential losses.

**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.**

Thank you for the recommendation and the effort to make the manuscript as clear as possible. We will rigorously review the text and proceed to make the pertinent changes.

**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.**

In the introduction, in order to simplify the extensive bibliographic review, it was decided to summarize with what you point out. The digital vegetation model used was ORCHIDEE. For the next version of the document will be added.

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

The term exceptionality refers to the Representativeness Criteria which described in lines 139-147 of this manuscript: “...In this way, each biome is guaranteed to have at least one priority ecoregion, so ensuring, for example, that the ecoregions in the savannah forest biome can also be classified, in addition to the more important moist tropical forests, which would otherwise dominate the list of values due to their high rates of species richness and endemic species...”.

Distinction will be replaced by distinctiveness. So, distinctiveness is related to Biological Distinctiveness Index which described in lines 153-211 of this manuscript: “... is more than just biodiversity at the species level, in that it also covers the diversity of ecological functions and the processes that support structural biodiversity (Ricketts et al., 1999). Specifically, this study is based on taxonomic rarity, species richness, functional diversity, and habitats with a unique evolution...”.

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

Thank you for the recommendation and the effort to make the manuscript as clear as possible. We will rigorously review the text and proceed to make the changes.

**Line 127: The value 14 did not change.**

Perhaps, a correct rephrasing for the next version of the manuscript should be: "In this way, the final number of ecoregions was 660, having representation of all terrestrial biomes."

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

It will be done for the next version, thank for the suggestion.

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

Sorry for the confusion and thanks for your appreciation. It will be done by the next version of the manuscript.

**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.**

Yes, they are the same things. It will be done by the next version of the manuscript.

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

For the next version, the index concept of the text could be divided into an index for the upper levels and an indicator for the lower levels as you suggest. Therefore, this recommendation would be made.

**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.**

Thanks for finding the weakness of the manuscript. We will rigorously revise to make the document much more robust.

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

It will be done for the next version of the manuscript.

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

Yes, they are the same. Previously in this post, we are going to work on this terminology.

**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 crosstabulation. 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.**

It will be done by the next version of the manuscript.

The phrase “**in which the most valuable component was prioritized**” means that when two categories coincide, they rise to the next level.

All the levels and the different categories of the variables are in the main document in the tables:

- Biological Distinction (distinctiveness) Index: Lines 210-211
- Conservation Status Index: Lines 263-266
- Ecological Index: Lines 272-273
- Adaptation to the Vegetation to Fire: Lines: Lines 298-299
- Soil Erosion Potential: Lines 312-313
- Post Fire Vegetation Regeneration Delay: Lines 325-326
- Ecological Vulnerability to Wildfire Index: Line 334

Regarding the sensitivity analysis, the one proposed is the EFAST method previously cited in this post (Stalli, et al. 1999).

Saltelli, A., Tarantola, S., & Chan, K. S. (1999). A quantitative model-independent method for global sensitivity analysis of model output. *Technometrics*, 41(1), 39-56.

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

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

Thanks for the clarification of both cases. It will be changed for the next version of the manuscript.

**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.**

Thank you very much for providing information from different parts of the world such as Africa or South America. This will be very useful in the discussion section. The zones that you indicate (Africa, South America, Boreal Zones) are very specific parts of the extensive ecoregion to which they belong. It is very interesting to know in more detail these possible inconsistencies in order to explain the limitations of this model. But certainly, we consider that it is very important not to forget that the objective of this article is on a global scale and that is why the information provided is much more general than what could be expected from a local/regional scale in which the detail of those areas that you mention would have a greater differentiation. In addition, the input variables in some cases come from vector maps with a single data per ecoregion, as is the case of Shlisky et al., (2017) or the tables of biodiversity provided by World Wildlife Fund, (2006), among others.

Shlisky, A., Waugh, J., Gonzalez, P., Gonzalez, M., Manta, M., Santoso, H., Alvarado, E., Ainuddin, A., Rodríguez-trejo, D. A., Swaty, R., Schmidt, D., Kaufmann, M., Myers, R., Alencar, A., Kearns, F., Johnson, D., Smith, J., & Zollner, D. (2007). Fire, ecosystems and people : threats and strategies for global biodiversity conservation. The Nature Conservancy Global Fire Initiative Technical Report, 17.  
[http://mrcc.isws.illinois.edu/living\\_wx/wildfires/fire\\_ecosystems\\_and\\_people.pdf](http://mrcc.isws.illinois.edu/living_wx/wildfires/fire_ecosystems_and_people.pdf)

World Wildlife Fund. (2006). WildFinder: Online database of species distributions.

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

Yes, they are the same. It will be done by the next version of the manuscript.

**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.**

We are going to revise rigorously this part in order to provide to the reader a better understanding of it. We chose a table to show our results because a number is an exactly result whereas the graphic in which the reader has to find an approximated value. We find this visualization of the result more interesting for the police makers, among others.

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

Again, sorry for the confusing of the different terminology. And yes, they are the same. It will be done by the next version of the manuscript.

**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 avoiding 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.**

Quoted earlier in this post, the concept of forest is such a controversial topic all over the world. Depending on the source, the countries, the knowledge sector, among others, there are more than 200 definitions of forest (UNEP, 2020). If we only consider a number of trees and a percentage of cover for them, we may only focus on plantation trees. It is not representative of each part of the world. So what is the appropriate forest concept for this global scale? Perhaps, the adequate concept of forest is based on the concept of "non-forest" in which there is more agreement: urban areas, crop land, water, ice... Thus, being aware for each part of the world, for the global scale, forest would imply different woody plants and not only trees, in certain extensions (Chazdon et al., 2016). Because, the objective of the global scale is to have a previous panorama that shows what happens in general terms around the world. The global scale allows us to focus on different local/regional scales in which we can more rigorously define the variables and concepts according to the study area to provide more precise data. According to that, the term forest fire could be used since the pertinent processes were carried out, as stated in lines 125-127 and 129-138.

In any case, as you suggest, the terms forest fire will be replaced by Wildfire as written in the title of the manuscript.

Chazdon, R. L., Brancalion, P. H., Laestadius, L., Bennett-Curry, A., Buckingham, K., Kumar, C., ... & Wilson, S. J. (2016). When is a forest a forest? Forest concepts and definitions in the era of forest and landscape restoration. *Ambio*, 45(5), 538-550.

United Nations Environment Programme (2020). The State of the World's Forests 2020: Forests, Biodiversity and People. <https://wedocs.unep.org/20.500.11822/32472>.

**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.**

Thanks for your appreciation. It will be done by the next version of the manuscript.

**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.**

Thanks for finding the weakness of the manuscript. It will be done for the next version of the manuscript. We are going to develop a EFAST analysis in order to know which variable has the greatest effect on the result (Satelli et al., 1999). Several works from different sectors of the knowledge use this method to measure the global sensitivity of the output (Ciffroy, 2020; Gómez-Delgado & Tarantola, 2006; Xing et al., 2017; Wei & Hua, 2014).

Saltelli, A., Tarantola, S., & Chan, K. S. (1999). A quantitative model-independent method for global sensitivity analysis of model output. *Technometrics*, 41(1), 39-56.

Ciffroy, P. (2020). A comprehensive probabilistic approach for integrating and separating natural variability and parametric uncertainty in the prediction of distribution coefficient of radionuclides in rivers. *Journal of Environmental Radioactivity*, 225, 106371.

Gómez-Delgado, M., & Tarantola, S. (2006). GLOBAL sensitivity analysis, GIS and multi-criteria evaluation for a sustainable planning of a hazardous waste disposal site in Spain. *International Journal of Geographical Information Science*, 20(4), 449-466.

Xing, H., Xiang, S., Xu, X., Feng, H., Yang, G., & Chen, Z. (2017). Global sensitivity analysis of AquaCrop crop model parameters based on EFAST method. *Scientia Agricultura Sinica*, 50(1), 64-76.

Wei, H., & Hua, Y. (2014). EFAST Method for Global Sensitivity Analysis of Remote Sensing Model's Parameters. *Remote sensing technology and application*, 28(5), 836-843.