Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2019-350-RC1, 2019

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Choice of a wildfire risk system for eucalyptus plantation: a case study for FWI, FMA^{+} and horus systems in Brazil" *by* Fernando Coelho Eugenio et al.

Anonymous Referee #1

Received and published: 20 December 2019

General comments The subject of the article is topical, as there is currently a high scientific interest on the relationship between fire weather indices and the occurrence and spread of forest fires locally, so that local vulnerability is taken into account. However, the authors have not been able to compose a coherent, explanatory and attractive text. Besides the grammar mistakes, which are innumerable, words and phrases, related to the essence of the work, are missing. Some specific comments For example, in Abstract, the phrase "In this sense, the present study aims at selecting the wildfire risk models FWI, FMA and RIF-Database for the Eucalyptus plantations" confuses the reader since it is not clear why the aim is to select them, and what are they selected for; The reader can only guess the objectives of this article in the context of the ab-

stract. Therefore, we assume that the models are chosen among others to test their performance in predicting wildfire occurrence in Eucalyptus forests. However something like this is nowhere written. E.g. we read in Abstract phrases like the following: '. . .the database was comprised (!) the period. . . , with 10,447 occurrences (of what?)' 'The validation and choice of the results (What results, about what?). . .. 'We observed in days with wildfire, a greater sensitivity (sensitivity related to what?) of the FMA model,. . ." 'Additionally, considering the skill score value, the FWI model presented the best results for subzone 1.' (subzones based on which criteria?? this information does not help us understand the importance of the results). Unfortunately, the whole article is riddled with such expression/cohesion problems. Even in the Conclusions there is absolutely no clarification on the purpose of the article, and no reference to what the authors searched for. Reference to the results is superficial, and information is incomplete. Finally, even though the methodology is appropriate, there are unclear points, such as: 1. the reason behind the cut of the study area into subzones and the criteria for doing so, 2. which fire data are used for the analysis? only the fire occurrence? are the other data mentioned, e.g. the area burned, used in any way? If not, why are they mentioned? 3. The comparison of 3 'models' or fire weather indices that are based on totally different concept on the risk classification is challenging, how do the authors ensure that results are comparable? Especially taking into account that the authors conclude to the need for calibration of FWI risk limit values. Considering all the above, I have no choice but to reject the paper.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2019-350, 2019.