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Comment on nhess-2022-52

Anonymous Referee #2

Referee comment on "Modelling ignition probability for human- and lightning-caused wildfires in Victoria, Australia" by Annalie Dorph et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-52-RC2, 2022

This study models ignition probability in the state of Victoria, Australia, distinguishing between human and natural fires. It is in generally well written, clear, and I see no issues in the methodology. However, some modelling choices in terms of the independent variables chosen are debatable (see specific comments below). The authors select a number of putative independent variables that are expected to influence such likelihoods, but in practical terms the effects of some are unconvincing, as judged from the partial dependence plots. In order words, the models could be more parsimonious. Overall, I felt the Introduction and Discussion sections could be made stronger than currently they are; just a suggestion that if implemented would increase the relevance of the work.

Specific comments

L28. Not sure about this distinction. Fire size is a feature of spatial pattern. So, what’s spatial pattern in the context of the sentence?

L49. I don’t think weeks qualify as very short period of time … The shift across dead fuel moisture thresholds is very often on a daily scale as fires cease to spread at night and resume the next morning.

L64-66. Check for better phrasing.

Table 1: add “relative” before “humidity” in the FFDI row.

L79. Why was live fuel moisture included as a predictor? All fires start on the dead component of the fuel complex and lightning-caused fires in particularly are highly dependent on the forest floor moisture content. While live m.c. definitely should play a role in fire spread such role has never been quantified or even demonstrated outside the lab. Also, having a model with live m.c. as a predictor can do more harm than good, given the spatial mismatch (in terms of scale) between actual live m.c. and estimated live m.c. and how uncertain remotely-sensed estimates of live m.c. are.

I also see that FFDI is not influent beyond a very low value, which is probably an outcome of having both FFDI and dead m.c. in the model – correlation between the two is expected to be strong. Or maybe not, because Nolan's m.c. model (if I recall well) is for 10-hr fuels, not really the fine fuels that drive fire ignition and spread. In this respect, and also having in mind management applications, why did the authors employ Nolan's model in lieu of the m.c. models used by fire management agencies? Both depend of the same variables, i.e. RH and temp.

L271. Like in the case of FFDI/dead m.c. I see potential confusion/redundancy with the simultaneous use of soil moisture and the FFDI, as the FFDI includes a drought component that should be correlated with the upper soil moisture.

L310. What is the plausible explanation for the effect of increased lightning ignition likelihood with higher annual rainfall? Higher NPP and thus higher fuel accumulation?