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Reply on RC2

Annalie Dorph et al.

Author 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-AC2, 2022

We thank the reviewer for their comments. We have addressed each of their comments and proposed solutions or clarifications were necessary. Below we outline the changes that can be made in response to each comment (reviewer comments are italicized, response in plain text).

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?

Yes, fire size in a component which can define elements of spatial pattern. However, spatial pattern is more generally considered to be the arrangement of different patches in space and the relationships among them geographically (often considered as spatial composition and configuration). In this context, ignitions and fuels can determine where fires start and spread, affecting spatial patterns fires create over time.

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.

This is an error in phrasing on our part. The sentence being referred to in text currently reads: "Forest fuel moisture content can shift across these thresholds in very short periods of time (e.g. within weeks, …)". Instead based on the work it is referring to, the sentence should say "within a week".

L64-66. Check for better phrasing.

The reviewer is referring to the sentence: "Specifically, we ask are the key drivers of both human-caused and lightning-caused fire ignitions consistent with the global patterns previously reported? We extend previous research of both human and lightning-caused ignitions and ask to what extent does fuel moisture influence ignition probability"

We can re-phrase the sentence as follows:

"Specifically we ask, whether key drivers of human- and lightning-caused fire ignitions are consistent with previously reported global patterns. In addition, we extend this previous research to determine to what extent fuel moisture influences ignition probability."

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

We can amend this as suggested.

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.

Live Fuel Moisture was included as it has been linked to fire behaviour and extent (Nolan et al., 2016, 2018), therefore we might expect ignitions to be influenced by live fuel moisture. The spatial mismatch is an issue for live or dead fuel moisture when using the remotely sensed data. Of course, there are fine scale variations in each of these, however when predicting landscape fire risk at a daily scale these values appear to be useful. We therefore wish to retain the analysis as it is.

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.

All variables which were included in the models were tested for correlations and any combination of variables with a Pearson correlation coefficient above 0.7 was excluded from the model. Further, the threshold effect of FFDI above 20 is a result supported by other published research (Clarke et al., 2019).

The question of which model to use is potentially irrelevant for this research and we argue we should not focus on existing models used by management agencies. Instead, we should be using the most up to date representations of values to ensure we are producing the most accurate models of processes.

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.

It is possible the reviewers' previous understanding of the relationship between soil moisture and drought factor has influenced their interpretation of how this relationship is represented in our data. As mentioned above, all predictor variables were tested for correlations. When Pearson correlation coefficient was > 0.7 for pairs of variables, one of them was removed. For lightning ignitions, FFDI and soil moisture had a -0.4 correlation, well below the threshold used to eliminate variables. The root zone soil moisture variable that was used measures the percentage of available water content in the top 1 of the soil profile. While FFDI does include a component of soil moisture (drought factor), the inclusion of relative humidity, air temperature and wind velocity alters this measure enough that the variables are not correlated.

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?

In this landscape, rainfall and elevation are highly correlated. Therefore, we used rainfall here as a surrogate from topographic variation as areas of higher annual rainfall correspond to higher altitude and more topographically complex landscapes. Mean annual rainfall is known to influence fuel load and accumulation (Thomas et al., 2014) and this is likely to be the main cause. However, due to correlations between these variables (as highlighted by the reviewer) we could not include all these measures and mean annual rainfall as a surrogate was deemed appropriate.

References

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