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Discussions

Interactive comment on "Using cellular automata to simulate wildfire propagation and to assist in fire prevention and fighting" *by* Joana G. Freire and Carlos C. DaCamara

Anonymous Referee #2

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General comments

The manuscript is an interesting exercise whereby the authors try to replicate the fire growth patterns of a large wildfire in Portugal. I find some problems regarding the structure of the work:

- Section 2 is essentially methods, so I suggest moving it to the methods section.

- Sections 4.2 and 4.3 are a mix of results and discussion. Reorganizing it and renaming it "Discussion" would be an improvement and it could be the place to discuss the shortcomings indicated below.

- The Conclusion is too long. I suggest to rename it “Summary and Conclusion”.

The ms. suffers from lack of explanation/rational for a number of decisions made regarding modeling, which seem too arbitrary. There is some confusion regarding the concept of probability, which in this ms. is (at least in part) akin to fire spread rate. Also, better discussion is needed of the strengths and weaknesses of the approach, especially given that I was not impressed with its performance and the tuning procedures seem to apply only to this fire in particular.

Title: Replace “fire prevention and fighting” by “fire management, more encompassing and elegant.

Specific comments

P2, L17-20. This sentence is far from accurate. Current fire spread models and tools used operationally are deterministic but are empirical in nature, e.g. FARSITE. Thus the usage of deterministic models that “attempt a physics-based description of fires, fuel and atmosphere as multiphase continua prescribing mass, momentum and energy conservation, which typically leads to systems of coupled partial differential equations” is restricted to the realm of research, as they are impossible to use in real time and their outputs are erratic. In fact, the physical mechanisms of fire spread remain largely unknown.

P2, L24. Again, empirical models are deterministic. “fill a gap” suggests they are somewhat half-way between empirical and physics-based models, which is not true. They are simply of a different kind.

P3, L3. I would better describe terrain as “undulated” rather than “steep”.

P3, L11. Better use “fire proneness” instead of “wildfire propensity”.

P3, L12. This statement is too strong. Antecedent years rainfall influences subsequent fire activity in fuel-limited systems, which is not really the case of Portugal.

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P4, L1. “Consequently” indicates that the previous information leads to this conclusion. However, fire danger as per the FWI is determined by a combination of atmospheric influences and fuel dryness, being independent of fuel accumulation. The previous sentence refers drought and the typical weather conditions in the area, but not the prevailing weather during or before the fire, namely the dominant effect of wind speed. Revise (also, “consequently” should not initiate a paragraph).

P4, L10. “protection”, not “salvation”.

P4, L14. Replace “copious . . . loading” by “heavy fuel loads”.

P4, L34. “shrubland”, not “shrubs”. Also, better use farmland or agriculture than cultivated, as the latter is ambiguous (it can denote planted forests).

P5, Table 1 and subsequent: you need to state what “probability” refers to. Fire spread?

P7, L10. From what follows, probabilities p express relative rate of spread rather than the probability of fire spread, e.g. according to a rule commonly used, rate of spread of the headfire perimeter is one order of magnitude faster than the backfire rate of spread, but their likelihood of spread is the same. Please make this more explicit.

P7, L22. This in fact is the effect of slope, not the effect of elevation.

P8, L8. You need to explain the rational for this rule and where does it come from. Physically it does not make sense, unless the increase in flame size due to wind would extend to new cells, which is impossible to happen.

P8, L15. The first two paragraphs in Results, and part of the 3rd, belong in Methods.

P8, L16-19. If I understood correctly, this parameterization and the obtained time step depends on this wildfire rate of spread, right? Consequently, it cannot be applied to fires with different rate of spread.

P9, L2. Also, it depends of the changes in fuel types, or is this already accounted for in more effective fire fighting? (use fighting or suppression, not combat - replace here

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and elsewhere in the ms.).

P9, L15. Why 0.2? A threshold of p=0.5 is usually assumed for go/no-go events. Again, this initial paragraph belongs to Methods.

P11, L6. Methods.

P12. "Since fire containment was mainly due to actions by firemen along the perimeter". You don't know if this was the case (also in Fig. 9 legend) . . . in may instances it should be due to changes in fuel type, topographic effects, or the presence of linear interruptions such as roads.

P13, L22. Still, 35% implies a high degree of underestimation in fire growth.

P13, L25. I don't see how this connection can be made, because probability of burning in this model is independent of fire suppression operations, and I don't think you know how fire suppression operations were carried out and where the resources were placed.

P13, L26. Why is this rule non-local? It suggests it is somewhat universal but no rational or basis was given for the rule.

P13, L27. I don't think there's evidence for this "very good" performance. Also, the added value for fire management is not proven. Why is this model preferable to fire growth simulators that are becoming more and more used operationally? Indicate advantages and disadvantages (preferably not here but previously in the Discussion).

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