Ms. Ref. No.: AGRFORMET-D-23-01404  
Title: Fuel constraints not fire weather conditions limit fire behavior in reburned boreal forests  
Agricultural and Forest Meteorology  
  
Reviewers' comments:  
  
Note from handling editor: Dear authors, we have received comments back from both reviewers and both suggest minor revisions. I agree and request you to revise the manuscript according to these suggestions. However, regarding the suggestion of reviewer 1 on 10-100 times simulation, I understand WFDS cannot be used in ensemble runs and computationally expensive as it is for single runs. However, convergence of the simulations is still an important issue, so please consider adding a section showing the results are reproducible, if possible by adding and averaging one or two more simulations.

We appreciate the comment from the handling editor that WFDS is indeed, not used for ensemble runs. WFDS is a deterministic model, and thus will intrinsically converge across replicated model runs, but we also agree with the reviewer’s comment that it is important to demonstrate that our results are not sensitive to varying the initial conditions of the model, namely tree placement. We made two main additions to this end: first, we have added text in the manuscript that describes the context of WFDS more clearly (Lines 121-122, 223-226 in the version without tracked changes). Secondly, to demonstrate convergence of the simulations, we have run 5 follow-up simulations with random distribution of fuels for the once-burned high fuel high weather scenario. In none of our test cases did we see fire spread across the 200 m area of interest with relatively minor differences in rate of spread. There were some differences in maximum distance spread however. These results indicate that the broad trends in fire spread were consistent regardless of tree placement, implying our results in this study would be insensitive to additional runs (full results in a table below). We are however, currently collecting drone data on boreal forest structure which would allow us to explore the role of explicit tree placement in fire behavior models in this system in the future.

|  |  |  |  |
| --- | --- | --- | --- |
|  | **Spread [YES/NO]** | **Max Dist. Spread [m]** | **Rate of Spread [m/s]** |
| **Oneburn 0** | *no* | 142 | 1.17 |
| **Oneburn 1** | *no* | 136 | 1.16 |
| **Oneburn 2** | *no* | 96 | 1.19 |
| **Oneburn 3** | *no* | 72 | 1.25 |
| **Oneburn 4** | *no* | 142 | 1.17 |

Reviewer #1: The paper 'Fuel constraints not fire weather conditions limit fire behavior in reburned boreal forests' by Hayes et al. touches upon an interesting question.  
Deciduous forests are indeed thought to constrain fire spread in Alaskan boreal forest, yet we don't know have these limitations will be altered by changing fuels and fire weather.  
The paper is well written and generally easy follow.

Here are a few comments which I hope that the authors will consider in improving their manuscript:  
- Need to account for smoldering fires: the paper is very much focused on aboveground fuels. This is understandable as aboveground fuels sustain fire spread. Yet, smoldering combustion is an important characteristic of boreal fires, and I feel that the authors should provide this context in their manuscript.

We appreciate the reviewer’s comments, and agree that smoldering combustion is an important characteristic of fire behavior in boreal forests. Unfortunately, modeling approaches to simulating smoldering and fire spread simultaneously are still extremely limited. Very few fire behavior models can accomplish both at the same time, at a scale relevant to both types of combustion. We’ve added lines 221-223 in the manuscript (the version without tracked changes) describing this limitation more explicitly, and added lines 375-385 in the discussion mentioning the value of future work that focuses either on smoldering combustion or the transition between smoldering and spreading.

- Limited number of simulations: the current paper's conclusion is drawn from very few simulations. I would invite the authors to generate 10 to 100 times more simulations with randomly drawn fuels. This would lead to a more robust analysis.

WFDS is a deterministic model, and thus will intrinsically converge across replicated model runs – there is very little stochasticity in this particular program. We agree with the reviewer that varying the location of fuels could produce a more robust analysis but were limited by computation power (each simulation takes two weeks). To this end: first, we have added text in the manuscript that describes the context of WFDS more clearly (Lines 121-123). Secondly, to demonstrate our results are not sensitive to initial fuel placement, we ran 5 additional simulations of the once-burned high fuel / extreme weather scenario, varying initial tree placement randomly. The resulting rates of spread are in the table below: while rate of spread differs marginally across simulation, the broad trend in fire spread were consistent, implying our results in this study would be insensitive to additional runs. We are however, currently collecting drone data on boreal forest structure which would allow us to explore the role of explicit tree placement in fire behavior models in this system in the future.

- What about twice burned forests: the paper focuses on differences in fire behavior in forests that burned once or thrice. What about forests that burned twice? Can this be more explicitly included in the paper?

Due to the limitations in computer power mentioned above, in this work we focused on forests that burned once or thrice, since they had the largest difference in fuel characteristics. We have added text that more clearly describes the exclusion of twice-burned forests (Line 237-240), but agree that including twice-burned forests in future work would be an interesting way to explore the gradient of fuel connectivity and abundance and their impact on fire behavior.

L39 - Please be more specific when calling out species names

L171 Fig. 1 does not seem to be referred to in the text.

L174 Please define at first use

All added/changed, good catches, thanks!

L186 – Where?

DOI added.

Figure 2 caption

* In full in caption
* Please explain color use in caption

Figure 3 caption

How were outlier defined?

* Please explain legend class 1-1000 hours fuels in caption for non-specialists

Figure 6

* Could use same color scale for panels a and b to further highlight the difference between the 2 scenarios

Figure 7

* Figure is too small to be read. Please re-arrange.

Figure S1

How is an outlier defined?

* Please be more specific about species names.

All added/changed, thanks!

Reviewer #2: The paper presented provides an interesting simulation modeling experiment parameterized using field data representing fuel load and structure in Alaskan forests, transferred to the fire-physics model WFDS. The manuscript is well written and presents interesting results that are relevant to important and ongoing conversations in fire management and science. I have a few relatively minor questions and suggestions related mainly to making sure that the methods are clear to the reader, and that the simulation scenarios can be completely understood.

First, it is a bit distracting that the number of repeated burns being examined changes so often throughout. For example, In Figure 3 we see data from 0, 1, 2, and 3 times burned sites. Tables 1 & 2 are limited to 1 and 3 times burned, and then Figure 5 shows data for 1, 2, and 3 times burned, but not 0. Similarly, it is not reported why only the once-burned and thrice-burned landscapes were selected for modeling, over the twice-burned landscape (or the inclusion of a mature forest control simulation). Presumably this is simply due to limitations related to computation time, but it would be nice to report that and justify to the reader why this choice was made. As it is, it seems a bit confusing or unnecessary to report the fuel conditions in twice-burned landscapes in multiple places (e.g., Figures 3 and 5), but then not discuss them very much or use them in model parameterization. The fuel conditions in twice-burned sites are also different from the once- and twice-burned landscapes in both cases, which might suggest it would have a different fire behavior than was observed in either of these scenarios, so the reader is left wondering why it was excluded. At a minimum, I think a statement justifying and explaining the selection of these two scenarios is needed.

We agree with the reviewer that switching from presenting once, twice- and thrice-burned site characteristics to presenting only once-burned and thrice-burned model scenarios is confusing for the reader. Indeed, the limitations were computing time – individual runs for WFDS take two weeks each, and for this study we prioritized comparing between landscapes with the largest difference in fuel characteristics. We’ve added a statement justifying and explaining the focus on once- vs thrice-burned landscapes in Lines 234-2373 (in the draft without tracked changes) in the methods and added language in the discussion that mentions this limitation and the potential for future work that incorporates a broader gradient in fuel characteristics, like those found in twice-burned landscapes (Lines 385-403 and 416-418).

Additionally, the model outputs could be better understood with more detail reported about the parameterization. Specifically, the model run time or stopping point (if a built-in triggered model stop by fire cessation) needs to be reported.

We have added a statement that says we ran all simulations for 1000 seconds as requested to the manuscript on lines XX accordingly. We’ve also added a longer description of WFDS characteristics in Line XX and XX.

I also wondered why the authors limited surface fuel moisture (10%) to be the same in both the moderate and extreme weather scenarios. It seems reasonable to expect that dead fuel moisture would also vary to some extent alongside live fuel moisture. Historical datasets report litter moisture contents ranging from 1 to 1000% (e.g., Wotton and Beverley, 2007), so 10% is quite low, which makes sense for these scenarios, but there is also substantial variability in that range. Given the importance of the litter cover in the thrice-burned landscape (60%) holding this measure stable between all 8 scenarios this is presumably quite important the outcomes of fire spread observed.

While the reviewer is correct that litter moisture contents can occur across a much greater range than the 10% used here, fire spread occurs within a narrower range of dead fuel moistures. The maximum fine dead fuel moisture for fire spread is typically between 20-30% (Anderson 1982, Scott and Burgan 2005). The 10% value we choose to use is similar to reports from experimental crown fires reported by Stocks et al 2004 and Drury 2019. As our intent was to model moderate to extreme fire conditions the relatively low value seems reasonable. We’ve added text in lines XX suggest that future work on the role of surface fuel moisture, and burning conditions, is needed (Lines XX).

Finally, although the modeled fires appear to have halted, it may be worth adding some more discussion of the simulated fire behavior where they did spread, in terms of things such as spread rates and suppression capability. The authors emphasize that the fires ceased within a short distance of the transition zone, which is very important, but nonetheless, the observed behavior in the areas that did burn (including in the example that burned continuously throughout the domain) is actually quite extreme if the modeled rate of spread (ROS) is accurate. Beginning with an ROS between 1.5 - 2 m/s and dropping to a sustained/equilibrium ROS of 1.1 m/s (66 m/min) indicates a fire that is capable of spread over 1 km within the next 30 minutes, and is burning at an intensity (kW/m) that is largely too great for any direct attack if attempting fire suppression. Those are some scary rates of spread, if you are a fire manager, particularly if you're relying on old burns as firebreaks around communities. How realistic are they? How often are these weather conditions likely to occur in a year?

We agree with the review that our modeled rate of spread are quite fast and we appreciate the question regarding the accuracy of the model results. The key point of our results is that the fire was spreading at a relatively fast rate of spread, especially as the fire approaches the burned forest, but didn’t continue through the burned areas. It is not appropriate to extrapolate the fire behavior within this zone to larger areas or over longer time periods as the review has suggested for fires that ceased to spread. For the one case we had where the fire did spread, the rate was in line with the higher end of experimental rates of spread reported by Stocks et al (2004), which might be expected given the larger wind speeds used in these simulations and a likely propensity for WFDS to slightly overpredict (Hoffman et al. 2016). Ultimately we did not significantly add any new discussion on the fire behavior results as the goal of this study was primarily to understand mechanisms, rather than interpret potential management situations of a fire spreading under some set of environmental conditions. The question regarding how accurate are the model results is a very good one and also one that is difficult to address concisely. While there have been a number of model evaluation studies for WFDS (see lines xx-yy for a partial list) there are no known tests in the ecosystems under study in this manuscript. We have added a section on lines xx-xx that highlights the need for experimental data to not only improve understanding but support model validation.   
  
Line 5: Abstract - Drier vs. Dryer

Corrected, thanks!

Lines 64 - 65: Perhaps missing a description of the rest of the gradient. Seems somewhat odd to mention all, but only describe 3-fire landscape conditions in detail.

Line 134: How many 1x1 plots?  
Line 134: How many 200 m^2 tree plots?

Added more detail to better communicate the number / distribution of plots and subplots. See Lines 181-182.

Line 143: First mention of sampling cubes. Elaborate on what they are used for and how they were distributed.

Clarified language, see Lines 194-196.

Line 174: Does surface fuel moisture not vary with daily weather (10% in both scenarios)? Given the importance of surface fuels, particularly in the thrice-burned landscape I wonder if this affected the results. I recognize that 10% is definitely on the low end for surface moisture content, so it is probably a good representation of a pretty extreme moisture, or at least a moderate one, I just wondered why this was the only variable held constant between the two weather scenarios.

Line 178: How do these conditions (97 & 109% vs. 77 and 89%) compare to the NFMD data? Are they also 90th percentile conditions? How were moderate vs. extreme weather thresholds selected and how do the compare to typical fuel moisture?

Lines 180-181: Why not unburned, and twice burned landscapes? Seems odd to bother reporting their fuel loads (and doing fuel sampling in them) if not going to model, although I do understand the unburned fuel measures were used in the runup zone. Perhaps at least an explanation of why this choice (1- and 3-x burned only) is needed?

We’ve added more explanation of this choice in lines XX-XX. We would also consider moving the results of fuel characteristics in twice-burned landscapes to a supplement if including them in the main text appears to be too misleading to readers.

Lines 191 - 199: Please report the simulation duration/run time. It is implied that the fires extinguished but without knowing the simulation duration it's possible to misinterpret that the simulation simply stopped. How was the model run stopped? Did the fires need to fully stop spreading? Could they have sustained a slow creep to the far end of the simulation domain if given more time?

Figure 2. What do the colors mean?

In the graphic, cylinders (which represent conifers) are green and cones (which represent deciduous species) are green. We’ve added detail to the caption accordingly.

Line 220. Typo in what is meant to be 'downed', I believe.

Corrected, thanks!

Figure 5. Could be interesting to include 'unburned' or mature forest/reference plots here.

Figure 7. The labels (a & b) are not actually on the panels.

Added, thanks!

Line 274/ Figure 9: These are very, very fast rates of spread! Even the scenarios that stopped spreading, the lowest ROS is about 45 m/minute. Assuming equilibrium spread, that indicates an expected run of over a km within the next 30 minutes of spread. Depending on the fuel, that is presumably between an intensity class 4 or 5 fire, so something like 4000 - 10,000 kW/m, making direct attack impossible and having flame lengths 3 - > 6 m. It's a bit surprising that they extinguished, but not impossible given adequate gaps in the fuel.

Figure 11. Difficult to see the size of the points, due to overplotting. Perhaps they should be transparent?

Line 283: The plots included don't consider canopy fuel loads except in the scenarios selected for simulation, and therefore the mature forest is at a serious disadvantage in reporting this lower fuel load. Although the statement is presumably being limited to 'reburned' forests, Figure 3 is also titled 'reburn history' and includes the 0-burned sites (which do follow the trend reported, I realize). Maybe some rephrasing for clarity (limit to fine and large downed fuel?), or could report the total fuel load at the unburned/0-burned sites. These values were presumably calculated, in order to parameterize the runup zone, but are not reported in Table 1 or 2.  
  
Terminology throughout: I think quartile is generally only used for quarters (e.g., 25% of the data falls in the first quartile, 50% below the second etc.). Perhaps the term should be quantile or percentile, when not referring to these values.

Good catch, changed (Lines XX), thanks!

It is a bit inconsistent in the manuscript between whether the most severe weather scenario is referred to as "high weather" (figures) vs. "extreme weather" (text). Although the high scenario is often defined as "extreme" in the figure captions, it might make sense to just use a consistent term throughout, but this is optional change based on author preference.

We agree, we’ve switched to using “extreme” weather in both the figures and the text.