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Interactive comment on “High latitude cooling associated with landscape changes from North American boreal forest fires” by B. M. Rogers et al.

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We thank the reviewer for the provided comments and address them individually below.

Specific Comments

(1) Page 12088, line 14: I was really confused by the reference to both winter-spring and February-April in this sentence, as it seemed redundant. My suggestion is to keep just report the temperature change for one or the other.

REPLY: For clarification, we will modify this sentence to include only the changes during the Dec – May period, and add a sentence explaining that maximum changes occur during Feb – Apr.

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(2) Page 12088, line 25: I was surprised to see this reporting that northern high latitudes have been warming “five times” faster than the global/Northern Hemisphere mean. Most of the recent reporting of this issue is that northern high latitudes are warming at twice the global/Northern Hemisphere mean over long time periods (multiple decades). You might want to check your source for the “five times” figure to see if it is restricted to a short time period or to a particular season.

REPLY: We thank the reviewer for pointing this out. Estimates on the rate of temperature change in the arctic compared to the globe depend on time period and region. The estimate of a five-times greater rate in the Arctic was based off comparisons of land temperature changes in the North American arctic zone from 19504 - 2003, using estimates from the Arctic Climate Impact Assessment [2004], and global rates from the IPCC [2007]. However, to make this claim broader and applicable to the arctic as a whole, as it sounds in the text, we will modify this statement to say “...have been warming approximately twice as fast as the global mean during the last century (IPCC, 2007)”.

(3) Page 12094, line 10: Here you indicate that the probabilities were derived from data over 60 years, yet on previous page you indicate that the Canada fire data base was for 1961-2010. I see from information in Figure 2 that you mention that the probabilities were calculated also on information from the Alaska fire data base, but there is no information in the paper on the temporal span of that data base. I think you just need to clarify in the text that the sixty years corresponds to the temporal span of the Alaska fire data base.

REPLY: This is a good point: that we used 50 years of data from the Alaskan and Canadian databases for our fire return intervals (FRIs), but 60 years of data for constructing our burn probability curve. The difference is subtle. Technically the Alaska Large Fire Database contains data back to 1940, and the Canadian National Fire Database back to 1917. However, data before approximately 1960 are considered unreliable for some applications due to omissions in both databases. To address this, we only used data

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after 1960 for our FRIs. To build our probabilities, however, it was helpful to include data back to 1950 because it allowed to assess fire probabilities in stands older than 50 years, which was relatively high. This introduces the potential for a small bias in the probability of fires in stands younger than 10 years, as fires that burned in the early 1950s are less likely to have a second fire in the database during initial post-fire years due to omissions. However, many data scars after 1960 will have mostly corrected for this. In any case, to clarify this point, we will add the sentence: “Because data coverage becomes increasingly sparse and many forests approach maturity at this time, we assumed burn probability to remain constant after 60 years”.

(4) Definition of the winter, spring, summer, and fall in the paper: You use these seasonal designations very explicitly in Tables, but nowhere in the manuscript do you define them. Is winter DJF, spring MAM, summer JJA, and fall SON? Please define these seasonal terms in the methods.

REPLY: To clarify this, and our frequent reference to the ‘domain’, we will add the following paragraph in the methods section:

“Impact analyses focused on mean anomalies to climate variables over the North American boreal forest domain (section 2.2.1), although surrounding areas were assessed for feedbacks and broader effects. Unless stated, summer, fall, winter, and spring refer to JJA, SON, DJF, and MAM time periods, respectively.”

(5) Page 12102, line 12: Note that “late winter” does not appear as an entry to Table 3, just winter.

REPLY: Will change sentence to: “Winter and spring temperatures were notably affected by albedo changes (Table 3).”

(6) Page 12101, line 27: “Bax2” should be BAx2”.

REPLY: Will correct (pg. 12102, line 27)

(7) Page 12105, line 9: Note that “29%” reported on this line is reported as “28%” in

Table 4.

REPLY: Will correct

(8) Page 12107, lines 28-29: Change “permafrost melting” to “permafrost thawing”.

REPLY: Will correct

(9) Finally, the Discussion section is very comprehensive, but it also seemed rather long. I don't have any specific suggestions for shortening it, but if another reviewer provides suggestions for how to make it shorter and more snappy, I think that would improve readability of the manuscript.

REPLY: At this stage, we respectfully choose to leave the discussion as is. We believe the topics discussed are relevant and important for the science addressed in this study. If another reviewer expresses similar concerns about its length, or provides specific suggestions, we will take these into account and attempt to shorten the section.

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BGD

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