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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.***

**B. M. Rogers et al.**

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We thank Dr. Amiro for the helpful comments and address them individually below.

### **General Comments**

My major comment is that it is not clear if the models have included greenhouse gas forcing of climate that results from the increase in forests fires (combustion sources and changes to the forest carbon flux when the age structure is changed). Although it would seem that this must be included, it is not presented transparently, and I would expect a clear categorization of forcing caused by surface energy balance changes compared to greenhouse gas emissions.

REPLY: Indeed, we did not simulate any biogeochemical (greenhouse gas) forcings

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from fires. We attempted to note this explicitly throughout the manuscript. The abstract, discussion, and conclusions all point to the need for further work to include greenhouse gasses. We also attempted to stress throughout that climate responses were due to changes in surface energy budgets. However, to make this clearer, we will add clarifying text in the introduction, methods, and discussion.

In the final paragraph of the introduction, we will add "Here, we extend these analyses of boreal forest fire biophysical impacts to the continental scale...". In the methods, we will add, "Thus, these scenarios varied only in their surface biophysics due to altered North American boreal forest compositions". In the discussion, we will change "...a near-linear response of boreal North America winter and spring surface temperatures to vegetation changes..." to "... a near-linear response of boreal North America winter and spring surface temperatures to surface energy budget changes"

### Specific Comments

Pg 12089. Line 20. The definition of mature forests at several hundred years old is confusing in the boreal context. For many parts of the boreal forest, "mature forests" may only be about 70 years old. The authors are really referring to some specific plant communities with a certain species mix. I think they really mean something closer to "very old-growth" boreal forest, not "mature".

REPLY: This is a good point. In fact we do mean 'mature' in the sense that climax tree species (mainly spruce) are dominant, and that canopy structure and forest floor organic layers are sufficiently well-developed. As you point out, this may happen within 70 or so years in some forests. To avoid confusion, we will simply delete "(ie. several hundred years old)" from this sentence.

Pg 12091, line 6. At odds with this, the last decade showed no increase across Canada, although Alaska has an increase in area burned. For a current paper, this should be noted; Canadian data can be found at the CIFFC (Canadian International Forest Fire Centre) website.

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REPLY: This is a relevant point. As pointed out, while burned area in Alaska has shown a marked spike during the 2000s, burn area in Canada has decreased relative to the 1980s and 1990s. Together, however, there has been a steadily increasing trend since the 1960s, the time period assessed in this study (see Fig R1 using data from the Alaskan Large Fire Database and Canadian National Fire Database). We will change the mentioned text to state, "Indeed, climate change-mediated intensifications of fire regimes are already being observed (Gillett et al., 2004). Since the mid-20th century, burn area has been increasing..."

Pg 12094 line 7. It would help to explain more how this burn probability was determined. It is not clear how much of this is based on the FRI pixel classification, and how much from the studies that are referenced. For example, I recall that Lavoie estimated that there was decreased probability of burning in jack pine stands for about the first 15 years following fire; then equal probability thereafter. Fig 2 is a significant result of the current paper and an explicit description of the method is needed.

REPLY: We agree that this section is terse, and will explain how we arrived at the burn probabilities in greater detail by replacing the previous text with the following: "Polygons were rasterized to 0.005° (approximately 500 m), and post-fire stand ages were tracked for all burned pixels. Burn probabilities for each stand age were calculated based on the annual re-burning of pixels, and subsequently aggregated for the entire time period. A linear regression was fit to the annual probabilities for years 1 – 60 after a fire. Because data coverage becomes increasingly sparse and many forests approach maturity at this time, we assumed burn probability to remain constant after 60 years (Fig 2). While this approach cannot fully capture the age dependence of fire susceptibility, it qualitatively represents the expected pattern across the continent. We also tested the sensitivity of our domain-wide results to this function (section 2.5)."

Pg 12094, line 13. The term "deterministic" doesn't fit well here. I think the authors are trying to say that it is difficult to predict, but it is still deterministic.

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REPLY: Will change sentence to " Post-fire succession, even in America's boreal forests of low species diversity and high fire severity, varies substantially across sites depending on environmental and disturbance characteristics (Bonan and Shugart, 1989)."

Pg 12097, line 11. The model runs appear to have biomass burning as one of the parameters. But it is not clear to me that the carbon dioxide releases through boreal fire combustion, or the changes in carbon sink by a changed forest, are included. Perhaps I missed this, but does the model only simulate change to energy balance, without including changes to the greenhouse gas balance? If all aspects of the fire effect (greenhouse gas budget, energy balance) are included, the paper needs to break these out clearly. Randerson's paper seemed to do this in a transparent way. On Page 12019, there is a discussion of the "biogeochemical" effect in the literature, but no apparent conclusion from the present study. If greenhouse gases are not included, enhanced discussion is needed.

REPLY: As addressed in the first comment, we will attempt to make it clearer that only land surface biophysics are being altered in this study. This particular sentence is potentially confusing because of the 'biomass burning' reference. We intended to indicate that everything besides vegetation composition was held constant for our climate simulations, which includes aerosol emissions from anthropogenic, volcanic, and biomass burning sources (from GFED2). In this way, we kept the fire aerosols and carbon pools constant at year 2000 levels, but altered the vegetation composition and hence surface energy budgets. We will also change the wording of this paragraph to more clearly highlight this point:

"CESM was spun-up with repeating year 2000 forcings (solar radiation, atmospheric CO2 concentration, and aerosols from volcanic, anthropogenic, and biomass burning sources) for 35 years using land cover from our historical (BAx1) vegetation scenario. Four branch runs were then initiated with land surfaces corresponding to BAx0, BAx1, BAx2, and BAx4. Thus, these scenarios varied only in their surface biophysics due

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to altered North American boreal forest compositions. Simulations ran for 120 years after branching with the exception of BAx4, which ran for only 80 years because of its stronger forcing and climate responses.”

Pg 12107, line 20. When temperature changes are given in degrees C, the fire-cooling effect would be better to express in degrees C also, instead of percent.

REPLY: We agree on this comment, and will provide feedback strengths in terms of degrees C.

Table 1. The numbers appear to be percentages, but this is not stated.

REPLY: Will be corrected.

Figure 1. The classification scheme appears to include forest areas in British Columbia that are not normally classed as boreal. Many of these are montane or temperate rainforest. The temperate rainforest especially has a very different fire regime from the boreal region. Successional trajectories are different from the boreal ones shown in subsequent figures. Also, this area of complex topography poses challenges for classification and modelling at the 2-degree grid scale. If these areas are to be included, some discussion is needed, but it would be preferable to only include boreal areas, which have been clearly defend through ecological classification.

REPLY: This is a very good point. Our classification scheme was ultimately derived from monthly temperature data (Lawrence and Chase, 2007), which wound up including gridcells throughout British Columbia. Because these forests burn much less frequently, our remote sensing-based succession trajectories included very few data points from this region, and, as you state, are not very representative for British Columbia. However, because this area burns infrequently, it also contributed very little to energy budget anomalies in our altered burn area scenarios (Fig R2). To address this, and other uncertainties involved in our domain classification, we will add the following text to the uncertainties section in the discussion: "Our domain classification is

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also imperfect, undoubtedly including omissions and commissions in boreal forest 500-m pixels, and containing some forests not traditionally identified as boreal, such as those in British Columbia. These areas, however, tend to burn infrequently and contribute little to continental energy budget anomalies under altered burn area scenarios".

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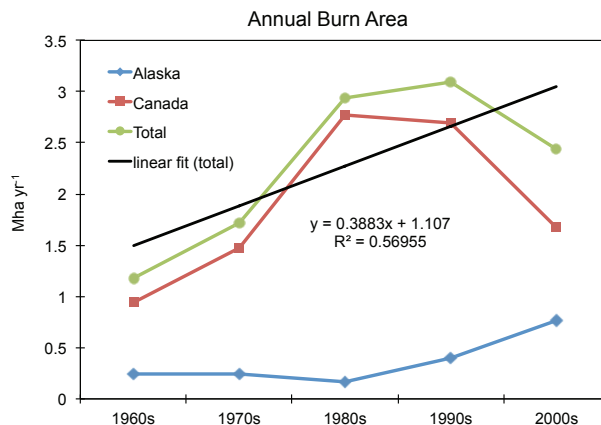
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Fig R1. Annual burn area by decade for Alaska, Canada, and combined. A linear trendline was fit for the combined time series. Data was derived from the Alaskan Large Fire Database and the Canadian National Fire Database.

**Fig. 1.** Figure R1.

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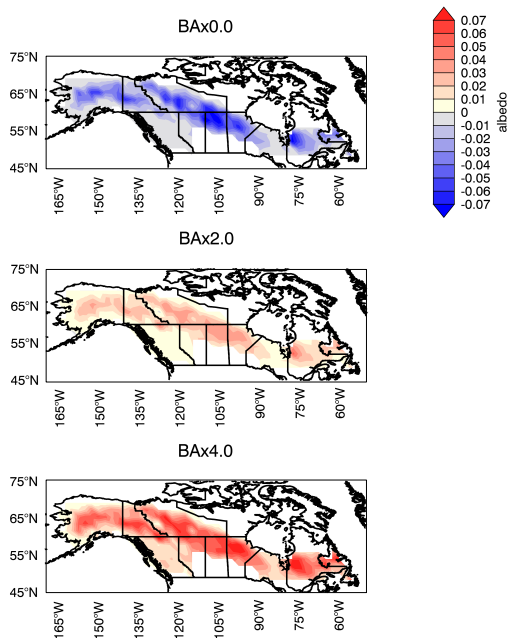


Figure R2: Changes to annual albedo in BAx0, BAx2, and BAx4.

Fig. 2. Figure R2.