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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 numerous and helpful comments. We will change text to address specific comments and believe it will result in a clearer manuscript. Proposed changes are outlined below.

General Comments.

The writing style is wordy and could easily be shortened without affecting the content. A more concise style is likely to increase the readership. Although the discussion (p12107, l24 to p12109, l10) addresses the caveats of this study, it would be good to make a reference to the major caveats in the Methods section. While reading the manuscript, I was at unease with the results and discussion parts which appeared as

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overselling to me because at that point in the manuscript, it was not clear that the authors realized these caveats. After the caveats were brought up (almost the end of the manuscript), I had to re-read the results and discussion to get convinced that these sections were balanced after all. Listing the caveats earlier in the manuscript will prevent losing readers.

REPLY: We appreciate where the reviewer provided specific comments on shortening the writing style, and have addressed those below. We will also attempt to make the discussion section more palatable by including subsection headers. Regarding model caveats, we will include text in the methods that addresses the major study caveats which were not quantitatively addressed. We will also add text in several places that cautions readers by highlighting uncertainties in our approach, and hope the resulting manuscript does not appear to over-sell readers on its results.

Specific Comments

P12089, I 28: 'Despite these low albedo's ...' Rephrase or explain how roughness length affects net radiation.

REPLY: To be more clear, this text will be changed to "Turbulent exchange is decreased because of lower surface roughness, causing an increase in ground temperature and emission of longwave radiation. Thus, despite more energy absorption from low summer albedos, net radiation and heat fluxes are suppressed (Chambers et al., 2005; Liu and Randerson, 2008)".

P12091, I 2-5: These lines justify your experiment but also indicate the x4 scenario is unlikely. Comment when present the scenario's.

REPLY: This is a good point, as BAx2 is often implied as the main scenario of interest. We will add the following text in the Methods when our burn area scenarios are introduced: "While a transition from BAx1 to BAx2 is the most likely scenario given recent projections, BAx0 and BAx4 are included to better constrain fire-climate relationships

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and examine regional feedbacks."

P12092, I 9: It is surprising that the boreal biome follows administrative borders. Explain or use a better mask.

REPLY: Because our study uses long-term fire inventory data, which were only available in Alaska and Canada, we necessarily neglected grid-cells in the continental U.S that may otherwise be classified as boreal. However, these gridcells are few in number, occurring in the northern stretches of the Cascade and Rocky Mountain ranges, and would not be expected to contribute substantially to continental climate changes within our experimental design. The reviewer's point is well-taken, however, and will be addressed with similar text as above in our discussion of caveats.

P12095, I 19: There are many models in this study. Specify which model was run on an annual time step.

REPLY: We will clarify this text by saying "This simple fire model..."

P12096, I 9: 'This version of the CLM incorporates...' Do you mean assimilates or do you mean remote sensing products are used as a driver? In case of the latter comment that this approach can not be used for prognostic simulations of LAI.

REPLY: Indeed, remote sensing products are used as a driver for this version of the CLM. We will add text here stating that LAI is therefore prescribed for our simulations.

P12096, I 12: The text documents the problem but does not justify the solution., Justify why you multiply by 0.05.

REPLY: The CLM has an outstanding issue, particularly at high latitudes, of over-estimating evaporation over bare soil. Because we prescribe freshly burnt surfaces to be the non-vegetated PFT, we inherit this problem. It is in fact exacerbated in our simulations because post-fire landscapes contain varying amounts of soil and forest floor organic matter which adds resistance to evaporation. Our solution in this context was simple, but effectively added evaporative resistance to freshly burnt surfaces. A

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dramatic scalar was needed in part because, as one limits soil evaporation, the model wants to evaporate more because VPD increases, creating a negative feedback. Because our succession curves were static, this fix merely resulted in more realistic trajectories of post-fire latent heat fluxes. This scalar was only applied to freshly burnt surfaces, which covered about 1% of our domain, and thus had a negligible overall effect. We will add the following text to better highlight why this change was necessary: "The bias was likely exacerbated in these simulations due to the presence of organic matter in freshly burnt surfaces, which is assumed to be absent in the CLM's non-vegetated PFT."

P12096, I 25: 'We therefore extracted year 40 for our analysis' What was done with year 40? Was it the restart file for scenario-based simulations (thus a kind of spin-up)? Something else?

REPLY: These offline CLM simulations were run for 40 years so that model state variables would reach equilibrium on an annual basis (driven by annually repeating climate forcing). In most cases, 10-20 years was long enough, but to be safe, we ran all scenarios for 40 years. Because there is no inter-annual variability in these simulations, the last year was extracted to be representative of the model state.

P12097, I 16: '...only 80 yr because of its stronger forcing and climate responses' This is not a justification why you ran this scenario for only 80yr. Even a scenario with a stronger response can be run for 120 yr. Unless you encountered numerical instabilities or impossible climate change. If this is the case, report as this in itself is an interesting result.

REPLY: Ultimately, limited computing resources led us to run this scenario for only 80 years. BAx4 was chosen for an abbreviated run time because of its strong climate response signals. This enabled us to detect relatively high signal-to-noise ratios after 80 years, which was not necessarily the case with other scenarios. We will add text addressing these points: "Simulations ran for 120 years after branching with the excep-

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tion of BAx4, which proceeded for only 80 years due to limited computing resources and this scenario's stronger forcing and climate responses."

P12097, I 17-26: List the references only once, either in this paragraph or in the caption of figure 3. This is one example where the manuscript could be shortened without any loss in content.

REPLY: This is a helpful suggestion, and we will abbreviate this section accordingly.

P12098, I 22: 'our analysis focused' mention which analysis i.e. our sensitivity analysis.

REPLY: We will clarify that this refers to our sensitivity analyses

P12098, I 26-27. Rephrase. Do you mean that it was forced at 2 or 4 times the burn area but that in reality you expect that after a fire peak, the fire probability would start to decrease again because the landscape now contains a lot of young forests which are less likely to burn than old forest?

REPLY: This is correct. To better clarify, we will change the text to, "In our main simulations, burn area was assumed to increase spatially in direct proportion to historical distributions. This assumption may be problematic for frequently burning grid cells because younger stands are less likely to burn. Additionally, historical data are limited to 50 years, and the spatial pattern of fires may change in the future."

P12099, I 11: 'Despite being validated in several ways' I found a section where the successional pathways are parameterized but I must have overlooked the independent validation.

REPLY: Succession was indirectly validated by two main analyses described in section 2.4: comparison of site-level post-fire energy flux trajectories and domain-wide vegetation distributions (driven by our historical fire patterns and succession curves). It should be noted that these validations are indeed indirect, and we will add text to highlight this.

P 12100, I 27: '... succession was halved...' add ('half deciduous tree'). Use the

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terminology that was introduced in the Methods section.

REPLY: Will add.

P12101, I 25: 'outgoing longwave was higher due to shorter roughness lengths' I can think of a relationship i.e. more canopy coverage, more longwave scattering within the canopy hence less outgoing longwave radiation. Also, more canopy coverage will come with a higher roughness length. So I expect there is a correlation between roughness length and outgoing longwave radiation but the way it is now written it is presented as a causal relationship whereas outgoing longwave radiation is determined by its emissivity rather than roughness length. Rephrase or explain the relationship between roughness length and outgoing longwave radiation.

REPLY: The reviewer makes a good point that, theoretically, more canopy cover could increase longwave scattering and, everything else equal, decrease longwave emission. However, this typically does not happen for two reasons. Increased scattering, by itself, will ultimately result in similar outgoing longwave emission. This is because any scattering directed towards the ground must either be scattered back or absorbed. Absorbed photons will increase surface temperatures and hence longwave emission. The dominant effect, however, results from the increased roughness due to more canopy cover (after a point, more canopy cover will have a negligible effect on roughness and may even decrease it, but this does not apply to boreal forests). This roughness increases turbulent exchange, cooling the surface and thus decreasing longwave emission due to the Stefan-Boltzmann Law.

To make the relationships clearer in the text, we will change the wording to: " Anomalies during summer months were comparatively smaller (Fig 5) and mainly controlled by differences in energy partitioning and turbulent exchange. With increased burning, domain-wide albedos were slightly elevated. Shorter roughness lengths decreased turbulent energy exchange between the biosphere and atmosphere, which tended to heat the ground surface. Consequently, although less solar energy was absorbed,

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outgoing longwave radiation increased. Despite this decrease in net radiation, latent heat was still elevated (+1.6% and +3.6% for BA_x2 and BA_x4, respectively, from June - August) because deciduous trees partition more available energy into transpiration during the growing season."

P 12102, I 5-9: Rephrase making use of 'local cooling' and making clear that whether this has a global impact depends on remote feedbacks ...

REPLY: This sentence was not initially focused on global impacts, but rather on regional (continental) responses, and the fact that these are determined both by local forcings and remote feedbacks. To clarify, we will change this sentence to: "However, continental responses depend on both local forcings and remote feedbacks involving the atmosphere, ocean, and sea ice."

P12104, I 5-6: use 'sensitivity test' as introduced in the methods sections rather than 'altered boundary conditions'.

REPLY: We will change the text to use this term

P12104, I 20-22: the use of 'driving datasets' hints at uncoupled model set-up. Is this correct? Also, I don't understand the variance analysis. Please, explain the underlying principle (not only the method) in the text.

REPLY: We agree. The terminology used in this section was confusing and our theoretical approach to uncertainty analysis was not well described. This section will be re-written to better describe our approach and its limitations, ie. uncertainties not addressed quantitatively. We propose the following text:

"Sources of uncertainty, bias, and fallible assumptions in a study such as this are numerous. In an effort to evaluate their impacts, we quantified two major sources of uncertainty: that associated with the construction of vegetation distributions, and that from the large variability in high latitude climate and consequent low signal-to-noise ratios produced by our surface perturbations. For the former, we conducted four sen-

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sitivity analyses that modified varying components of the BAx1 and BAx2 simulations (Table 5). . .”

“The CESM coupled climate model displays a considerable amount of high-latitude inter-annual and inter-decadal variability because of long-term oscillations in atmosphere-ocean dynamics (Jahn et al., 2012; Landrum et al., 2012). This is true even with the slab ocean model (Subin et al., 2012), and is especially prominent in winter: we found the standard error of linearly de-trended surface temperature for land north of 45°N in CESM for BAx1 to be 2.37 °C for December - February vs. 1.97°C in Qian et al. (2006) reanalysis data. Because of limited computing resources, we were able to run our equilibrium climate simulations for a maximum of 120 years each, with the exception of BAx4, which ran for 80 years. This resulted in somewhat low signal-to-noise ratios for many climate variables. To quantify the uncertainty in our relationship of domain-wide temperature vs. burn area originating from these limited run times, we performed 1×10^6 Monte Carlo simulations of the estimated slope from BAx0 - BAx2. For each seasonal period, a population of $\Delta(\text{temperature})$ was created from the annual values of seasonally-averaged temperature. Simple linear regressions were fit to three points chosen from the normally distributed populations of $\Delta(\text{temperature})$ in BAx0, BAx1, BAx2 (Fig 8). In this way we were able to use 360 simulation years from three experiments to estimate the mean and variance of modeled temperature change associated with a doubling of burning. These two major sources of uncertainty for surface temperature effects, ie. vegetation distributions and climate model signal-to-noise ratios, were pooled to provide an overall estimate for our approach. We calculate uncertainties of 41%, 37%, and 28% for the slopes of temperature vs. burn area during annual, winter - spring, and February - April time periods (Table 4). There are, however, other uncertainties due to fundamental assumptions and potential model biases that we were unable to address directly (section 4.3).”

P12105, I 1: 'Due to computing resources ...' should this be due to limited computing resources?

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REPLY: Will be changed.

P12105, I 5-7: Explain the underlying principle. It appears as if you use spatial heterogeneity as a substitute for model and driver uncertainty. Is this correct?

REPLY: Not quite. However, we will re-write the section to be clearer in both the theory and approach. See above for the proposed changes.

P12105, I 8: 'Two major sources...' list them

REPLY: This will be changed (see above).

P12105, I 11: Use subtitles to structure the discussion i.e. uncoupled, coupled, caveats, etc.

REPLY: We appreciate this suggestion, and will use headers to arrange the discussion into four subsections: (1) background and comparisons to previous work, (2) summary of results and implications, (3) caveats, and (4) future directions.

P12105, I 12: Is the climate-vegetation system more sensitive to changes in the high latitude than in for example the tropic, the semi-arid regions or the Mediterranean regions?

REPLY: This is a very important and difficult question to answer, and depends greatly on the forcing applied to each system and response metrics of interest. We did not mean to imply that the climate-vegetation system in high latitudes is the most sensitive compared to other biomes. However, it does typically experience large responses to climate forcings because of characteristics relating to the atmosphere (shallow atmosphere, low water vapor content) and vegetation (biome distributions depending on thermal thresholds, large albedo responses, etc.), and strong regional feedbacks (sea ice and land ice and snow).

P12105, I 15: '...species composition.' The simulations deal with changes in PFT's which is aggregates three taxonomic groups including species.

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REPLY: We will change 'species' to 'plant functional'

P12105, I 17: '...amplify...' Given that '...perturbations...' is not specified it should read '...amplify or dampen the responses'.

REPLY: Because the text is highlighting phenomena that result in positive feedbacks to climate changes, the responses will be amplified in either direction. In other words, perturbations of both warming and cooling will be amplified because of boreal vegetation-climate characteristics. Negative feedbacks are needed to dampen responses to forcings.

P12105, I 19-e.f. a summary figure visualizing previous responses would make the discussion easier to digest.

REPLY: We respectfully choose not to summarize these prior study results in a figure. The studies mentioned used different experimental approaches, spatial and temporal domains, and report varying result metrics.

P12105, I 19-e.f. Distinguish between model and data studies. It is interesting to compare models to models but that fact that they agree does not tell us anything about the physical system. The similarities between your results and those from Euskirchen could simply be the results of both studies relying on Sellers-scheme to calculate albedo. Given the spatial domain of your studies most of the relevant literature will come from other model studies.

REPLY: This is a valid point. The reviewer is also correct that we are really only able to compare our results to other modeling studies due to the large spatial and temporal domain. We would also argue that we compared components of our study to a substantial fraction of the available data from field studies in our validation sections. This is not brought up in the discussion. However, to clarify this, we will add text stressing that these studies are model-based in places of these comparison paragraphs.

P12106, I 3 '... uses a data-driven approach...' I would not call this a model driven

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approach rather than a data driven approach. Some data were used to parametrize, validate and drive the models but none of the results relies on the analyses of field observations.

REPLY: This is a fair point. We will change the text to say, “. . .and uses a data-informed modeling approach to. . .”

P12107, I 6: ‘...context dependent.’ Specify the context. Give an example of a context where the albedo effect will dominate and give an example of a context where the ET effect will dominate.

REPLY: The previous sentences in this paragraph describe how the ET effect is dominant in the context of arctic bare ground being replaced by deciduous trees (Swann et al. 2010), and how albedo dominates over ET in our study (context being increased boreal forest fires).

P12107, I 17-23: These results are not entirely justified but the model set-up. This section appears as overselling especially because the modelling caveats are only dealt with after presenting this result.

REPLY: This is a very good point, and we will offer two revisions to address this. Firstly, to avoid over-selling our results before the discussion on study caveats, we will add a paragraph to section 2.5 (Methods: Sensitivity analyses) that points out some of the major uncertainties that we did not directly address:

“The above sensitivity analyses are directed at quantifying uncertainty in surface forcings caused by altered vegetation distributions from varying fire prescriptions. We also used a Monte Carlo approach to quantify uncertainty arising from low signal-to-noise ratios in our climate simulations (section 3.5). There are, however, numerous other sources of uncertainty and error in our approach that we were unable to address. Prominent among these include the assumption of static forest distributions and succession patterns, biases in our driving vegetation and fire data, and biases in the

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CLM land and CESM climate models. A more detailed discussion of model caveats is provided in section 4.3.”

Secondly, we will begin this specific paragraph on negative feedbacks to climate change with the following: “Although our modeling experiments are conducted at equilibrium, they may offer insight into past and future feedbacks to climate change in boreal North America because of the similar operating timescales for forest re-growth and climate changes (several decades).”

P12110, I 21: ‘We also include no representation of time scale...’. Rephrase as the current wording is too vague for such an important statement. I understood as that you did not simulate a temporal evolution i.e. 2000 to 2120 but rather than studied the effect of increased fire if the environmental conditions of 2000 plus a change in burnt area would persist for 120 years without any other additional changes in for example atmospheric CO2 concentration, temperature due to GHG-emissions, etc.

REPLY: We will clarify and expand on this point mentioned by the reviewer with the following text: “We also include no representation of time scale in our experiments, but instead simulate climate responses to a land surface at equilibrium. Although landscape changes are implicitly assumed to take place over one to two centuries, numerous decades were needed to detect robust climate signals. This diminishes the practical applicability of our results and comparisons to other dynamic phenomena, such as climate change.”

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