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Biogeosciences
Discussions

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

Anonymous Referee #3

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In this study Roger and co-authors use an uncoupled landsurface model (CLM) and a coupled land-atmosphere model (CLM+CESM) to quantify the biophysical effects of forest succession following fire disturbance. The science underlying the study appears sound and the detailed account of vegetation succession is original and therefore warrants publications. The manuscript is well crafted, comprehensive and embedded in the relevant literature.

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 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 loosing readers.

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

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

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

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

P12096, l 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.

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

P12096, l 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?

P12097, l 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.

P12097, l 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.

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

P12098, l 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?

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

P 12100, l 27: '... succession was halved...' add ('half deciduous tree'). Use the terminology that was introduced in the Methods section.

P12101, l 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.

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

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

P12104, l 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.

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

P12105, l 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?

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

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

P12105, l 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?

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

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

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

P12105, l 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.

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

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.

P12107, l 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.

P12107, l 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.

P12110, l 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.

Interactive comment on Biogeosciences Discuss., 9, 12087, 2012.