nature portfolio

Peer Review File

Plant biodiversity limits carbon recapture after wildfire in warming boreal forests

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

9th Jan 24

Dear Mr Eckdahl,

Your manuscript titled "Plant biodiversity limits carbon recapture after wildfire in warming boreal forests" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

Should additional work allow you to address these criticisms and meet our editorial thresholds as outlined below, then we would be happy to look at a substantially revised manuscript.

In the following editorial thresholds, we ask that you:

*** Provide a solid discussion of your findings in the context of relevant literature, reconsider (tone down) the generalisation of the results / accuracy of predictions in view of data / methods used and discuss limitations

*** Provide a clear definition of concepts / terms used and revise all terms that were used inappropriately

*** Clearly describe and fully justify all aspects of your methodology to guarantee the reproducibility of the study and allow a sound interpretation and discussion of the results

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Erika Buscardo, PhD
Editorial Board Member
Communications Earth & Environment
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Alienor Lavergne, PhD
Associate Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements (Download the link to your computer as a PDF.)

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to disciplinespecific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I quite enjoyed reading this paper and I believe it presents important results that will be of interest to a broad readership. The question of how plant-soil feedbacks influence forest responses to changing fire and climate in the circum boreal forest is an important issue in understanding they had atmosphere feedback with climate change. This paper takes a fairly deep dive into relating two components of biodiversity that are rarely well linked in studies of forest resilience to fire: microbial soil communities and vascular plant communities. I believe the statistical analysis were appropriate to the data and the results of those analyses provided robust and interesting results. Methods descriptions were clear and contained an appropriate level of detail. My main issue with the paper is that the inferences drawn sometimes exceeded what seemed appropriate for the sampling design (details below). I also found the language difficult to understand at times and feel the paper would benefit from breaking down some of the complex sentences into smaller pieces. A conceptual model could be a useful way to help present the complex interactions that are discussed in the text. My specific comments are below. My review version did not have line numbers; hopefully you can follow my generalized references to the ms.

1. Abstract, sentence 4: This sentence in the abstract is long and complex, making it difficult to understand. The use of the verb "to find" implies an experimental design where a particular pattern was deliberately sought out. Is that the intended meaning?
2. P.2 Text, main section: Be careful of the use of "alternative" (meaning 2 options) vs. alternate (meaning multiple options).
3. “Though limited propagule…” I'm not sure this is a complete sentence.
4. P.2 General text: I find some of the wording and construction of sentences to be difficult to follow. I'd recommend some additional editing for readability that could improve clarity (esp. for North American readers).
5. P.2 Last paragraph: As in the introduction, I would favour laying out the objectives of the study more broadly than is currently being done. In particular, rather than specifying that the focus was on conditions that impair recovery capacity, why not emphasize more generally the relationship between recovery capacity and fire severity? This alternate phrasing leaves open the possibility that the researchers would also have reported any positive effects if those were found in the data.
6. P. 3 Results & Disc, Paragr. 1: If the burn caracteristics are being framed in the context of fire

severity, it would be helpful to have a summary of the estimates of burn depth along with ResOL.
7. P.3 The header for section 2.1 does not make sense to me. Please rephrase.
8. P.3: “There was virtually no conifer regeneration…” This result was very surprising to me, and I felt I needed more context to understand it. If this is highlighted as a significant part of the paper, why were not drivers of conifer recruitment analysed specifically?
9. Section 2.1, Paragr. 2: I wasn’t able to follow the logical justification for interpreting causal processes mediated by fungal effects on plant recruitment. Should there be citations to other work here? Further explanation of the hypothesized mechanisms would be helpful.
10. Section 2.2, Paragr. 1: “Hence, under the weakened ability…” Please explain this statement further so that a general audience can understand.
11. P4, bottom section: I think that the impact of this paper would be substantially increased if it included a conceptual figure that illustrated the fairly complex set of feedbacks being described here. Although this study lies within my broad area of interest and expertise, I found it difficult to quickly grasp the full set of interactions being described in the text. An illustration would be a big help. Maybe I’m just tired, but then, many of the readers may be in a similar state of mind. Please help us out.
12. Section 2.4: I think this concluding statement about C recapture needs to be tempered by the reality of the data being examined. After all, we are only looking at a snapshot of vegetation recovery from 2 years after fire! In the case of species established from seed, many individuals may be quite small at 2 years but with the capacity for substantial growth and C uptake over ensuing years. A slight change in language would improve the credibility of the claims of the paper.
13. Conclusions: The statement, "wide range of burn conditions," left me confused in my first read through the ms because the methods specifically state that sampling was preferentially done in the more severely burned parts of a burn. This aspect of the sampling design necessarily restricts inference to burn conditions that are at the severe end of the spectrum. Please modify so that the domain of inference is clear.
14. Conclusions: "...virtually no coniferous seedling reestablishment" - please see my notes regarding hesitation in how appropriate this inference is from the sample design. At the least, I am uncomfortable with this result taking such a prominent place in the conclusions.
15. Concl: “ Yet, it appears…” I like this sentence, but it needs further detail to summarize what those feedbacks are. Additional feedbacks are described with some detail in the following sentence; a similar level of description would help the reader better understand what the first set of feedbacks are referred to.
16. Section 5.6: Was any effort made to record presence of species in the larger plot that were not sampled in the biomass sampling quadrats? With a relatively small sample area, I can imagine that there was a good chance of species being missed.
17. 5.6: I'd like to see the methods description specifically address the assessment of conifer seedling regeneration. Firstly, what was the potential for very small seedlings to be missed in the biomass harvest and sampling process? Were any seedlings noted, and if so, how big were they? Thirdly, what is the time frame of seedling recruitment for Pinus sylvestris after fire, and is 2 years long enough to provide an adequate assessment of regeneration? Finally, It seems likely that the small sample area (9600 cm2, just under 1 sq. m total) would have been insufficient to detect conifer seedlings at low density. Without some stronger justification for the adequacy of the sample design, I am hesitant to ascribe much to observations of very limited conifer regeneration.
18. Random observation: Typo in Ref 11: Should be "Hollingsworth"

I hope these comments are helpful and I look forward to reading a published version of this paper in the future, Jill Johnstone

Reviewer #2 (Remarks to the Author):

GENERAL COMMENTS

In order to characterize vegetation regrowth and post-fire carbon recovery across the Fennoscandian boreal forest, this study evaluated tree survival and mortality, new plant establishment, soil organic layer depth, and soil microbial biomass and nutrient properties two years after fire in 49 plots that spanned mean annual temperature (MAT) gradient of about 7C. The plots were conifer-dominated forests before the fire, and the study sought to evaluate the likelihood and underlying mechanisms of transitions from conifer- to broadleaf-dominated forest following fire. High-severity fire was predicted to foster transition to broadleaf tree species, largely due to substantial reduction of the soil organic layer. Low-severity fire was predicted to sustain conifer dominance, largely due to retention of soil organic matter and associated limits on establishment of broadleaf tree species. Moderate-severity fire was predicted to remove enough of the soil organic layer to limit conifer forest recovery while not consuming enough of the soil organic layer to favor establishment of broadleaf trees. Results largely supported these hypotheses with the exception that even the low-severity plots had little or no new conifer establishment by two years after fire.

Overall, this study focuses on an important topic that has potentially to substantially alter the carbon-cycle feedbacks of boreal forests to the global climate system. The findings seemed reasonably well supported by the analyses. The emphasis on the importance of long-term carbon emissions from post-fire soil decomposition rather than focusing only on emissions during the fire itself seems like an important contribution of this study. However, I found several issues that could be substantially improved before the manuscript is ready for publication.

First, the title did not seem appropriate for the objectives of findings of the study. The title emphasizes biodiversity as the factor that limits carbon recovery, but the plant-soil feedbacks seemed to play a more important role than biodiversity itself. The Conclusion section focused little on biodiversity and more on plant-soil feedbacks. The strength of the plant-soil feedbacks seemed to be influenced more by the depth of the remaining organic soil layer, and to some degree, by the resprouting ericoid shrubs, but not by the overall species diversity. Most of the analyses focus on plant function groups rather than actual species divdersity.

Second, I found much of the writing difficult to follow, and I found myself having to re-read several paragraphs two or three times to understand them. Several terms needed to be defined more explicitly, and there were several places where broad terminology was used when more specific terms would have been more appropriate. In general, it seems like the writing may be suitable for people who use the same terminology and in the same context as the authors, but there were many cases where it would be better to make the writing more accessible to a broader audience. For example, in the second paragraph under Section 1, the phrase, “damaging regenerative structure,” was used when it would have been better to spell out what you meant by regenerative structure. Similarly, the third paragraph uses the phrase, “residual ecosystem structure,” instead of more explicitly explaining what you meant. Also, it seemed like several concepts needed to be explained in more detail for a broader audience. For example, the end of Section 1 refers to MAT-enhanced shift in soil communities toward saprotropic bacteria without explaining the implications of such a shift for biodiversity or carbon sequestration. I provide other examples under my specific comments, below.

Third, given the different types of data analyzed in this study, I would prefer to see the Results and Discussion each as separate sections rather than a combined Results and Discussion section. As written, a relatively large portion of the text in this section seemed to focus on interpreting results that had not been presented yet. For example, the first paragraph under Section 2.1 provided results of this study similar to what would be expected in a Results section of a paper. However, the second and third paragraphs of Section 2.1 made interpretations beyond the results that had been presented in the previous paragraph, and they did not cite any specific results from this study or cite any other literature. This type of interpretation would be better in a Discussion section after all of the results had been presented. Similarly, the last paragraphs of Sections 2.3 and 2.4 did not cite specific results of this study and cited few if any other articles to place the findings in the context of the broader body of literature.

Fourth, the interpretation that this study found a “weakened early recovery capacity of boreal conifer forests” (as stated in the first sentence of the Conclusion) seemed premature given that the sampling was only conducted in the second-year post-fire. This interpretation could be better supported if data were provided on seed production or weather/climate during the first two years after fire. This study cited one study that found high seedling densities in boreal Eurasia (Kharuk et al. 2021 – cited at the end of the first paragraph of Section 2.1), but it was not clear of that study only used data from the first two years after fire. In most parts of the world, the window available for tree regeneration after fire lasts for at least several years, if not for a few decades. A short window is typically only found when very dense vegetation, such as resprouting shrubs or shrubs that establish from a dormant seedbank, rapidly form a dense cover. Also, it was not clear how the seedlings were sampled in this study. According to Section 5.6, all vegetation was clipped within six 40 x 40-cm quadrats and then weighed by functional group. However, it was not clear if this was where the tree seedlings were also counted. This seems like a small sample area per plot for quantifying tree seedling density.

Finally, although this study focused on vegetation recovery and associated carbon-cycle feedbacks to the climate system, it would have been useful to also discuss albedo feedbacks to the climate system. In a region where snow covers the ground for a large portion of the year, the albedo feedbacks of conifer persistence or a shift from evergreen conifer do deciduous broadleaf trees may have a similar, if not stronger influence on the climate system than the carbon-cycle feedbacks.

SPECIFIC COMMENTS

At the end of the first paragraph of Section 1, does the growth of broadleaf trees compensate for the loss of above- and belowground carbon previously stored in the conifer forest, and if so, could you provide a rough time frame before the site transitions from a source to a net sink of carbon?

The second paragraph of Section 1 refers to “limited propagule input,” but this study was only focused at the plot level and did not evaluate broader seed sources. Particularly when focusing on intermediate fire severity, local conifer seed sources will persist, and given the long-distance seed dispersal of broadleaf species like birch, it is not clear that seed sources of broadleaf trees will be limiting.

In the first paragraph under Section 2.1, you mention that there was virtually no conifer regeneration on the forest floor (< 0.1% of total recovering biomass) and then compare this to the conifer seedling density of > 1000,000 seedlings/ha found across boreal Eurasia. It would be useful to also report the results for the present study in seedlings/ha. The study was conducted in the second growing season after fire, and a conifer seedling in its second year of growth is likely to have little biomass. I would expect the resprouting shrubs and some of the fast-growing herbs to have much higher total biomass by the second growing season than conifer seedlings, but a small number of conifer seedlings present in the second year after fire could still provide potential for abundant carbon sequestration as the conifers grow larger over the first couple of decades after fire.

I could not follow what was meant by the “window of time” in the third paragraph under Section 2.1. Was this a window of time in relation to time since fire or a window of time in relation to the current degree of climatic warming, or something else?

In the second paragraph of sections 2.2, when you refer to “overstory seedling growth,” does this mean the growth of tree seedlings, or the growth of seedlings of species eventually capable of becoming overstory trees?

At the end of the second paragraph under Section 2.2, it would be useful to clarify what you mean by “deborealization,” which is referred to in parentheses as the “shift towards intensified nutrient cycling supportive of temperate forest structure and function.” Is this more succinctly described as a shift from conifer- to broadleaf-dominated forest, with associated increases in rates of nutrient cycling and reductions in the depth of the soil organic layer? In a related comment, it would be useful to clarify what you mean by “temperate forest structure and function?” I suspect you are referring to something specific meaning here, but temperate forest structure and function could encompass a very broad range of meanings.

The end of the last paragraph of Section 2.3 refers to “regional species pools,” but this study was focused at the plot level and did not address broader-scale species pools.

When you refer to inhibiting “novel” species establishment, do you mean broadleaf tree species. If so, it would be useful to say this directly. If not, it would be important to clarify what you mean and whether this refers to any species that was not already present or only species that have certain functions within the ecosystem.

In the last paragraph of the Conclusion, you refer to post-fire vulnerability of boreal C stores expected to be “greatest in regions with high concentrations of fire-resisting vegetation, such as boreal Eurasia, with successional dynamics thus not well-adapted to recovery after higher burn severity.” However, this contradicts that statement you made earlier (Section 2.1) about vigorous conifer recruitment having been observed to be common (> 100,000 seedlings/ha) across boreal Eurasia.

In the last paragraph of the Conclusion, you refer to regions with high concentrations of “fire-resisting vegetation,” such as Eurasia, and you mention that the successional dynamics are not well-adapted to recover after higher burn severity. This seems to contradict the example I referred to in the previous where you previously referred to another study (Kharuk et al. 2021) that found > 100,000 seedlings/ha across boreal Eurasia. It would be useful to clarify what you mean by “fire-resisting vegetation," and why you interpret this vegetation as not being well adapted to recover after high-severity fire.

Section 5.4 is titled "PLFA Analysis," but PLFA was never defined in the text.

Author response to *Plant biodiversity limits carbon recapture after wildfire in warming boreal forests*

Reviewer text is left in **black**. Author responses are given in **blue.** Manuscript revisions are given in **red**. Please see separate documents for a revised manuscript and tracked changes.

Thank you to both the editors and reviewers for your time investment in this manuscript. We have thought through both the general and specific feedback and responded to each point below. In addition, we have revised the manuscript to improve overall readability including: (a) organization of information, including order of introduction, (b) breaking up of dense sentences into simpler ones, (c) replacing unclear vocabulary with more direct or explicitly defined terminology, as well as (d) better clarifying and framing the study aims and scope. Please see the two additionally provided documents for a revised manuscript and tracked changes.

Reviewer #1 (Remarks to the Author):

I quite enjoyed reading this paper and I believe it presents important results that will be of interest to a broad readership. The question of how plant-soil feedbacks influence forest responses to changing fire and climate in the circum boreal forest is an important issue in understanding they had atmosphere feedback with climate change. This paper takes a fairly deep dive into relating two components of biodiversity that are rarely well linked in studies of forest resilience to fire: microbial soil communities and vascular plant communities. I believe the statistical analysis were appropriate to the data and the results of those analyses provided robust and interesting results. Methods descriptions were clear and contained an appropriate level of detail. My main issue with the paper is that the inferences drawn sometimes exceeded what seemed appropriate for the sampling design (details below). I also found the language difficult to understand at times and feel the paper would benefit from breaking down some of the complex sentences into smaller pieces. A conceptual model could be a useful way to help present the complex interactions that are discussed in the text. My specific comments are below. My review version did not have line numbers; hopefully you can follow my generalized references to the ms.

Dear Professor Johnstone, thanks very much for your positive feedback. It is very helpful, especially with your diverse experience with boreal forests. We hope our addressing of your concerns has improved the manuscript in a satisfactory way.

We have revised the paper where relevant to make the inferences drawn more clearly justified by the data collected and sampling design (see detailed responses below). Further, we have made edits throughout the manuscript to break up dense sentences, improving readability (see responses below, and the revised manuscript copy).

We have also added a conceptual diagram which we believe helps to summarize the complex interactions described in the text. Figure 2 discusses the complex environmental filtering that occurs during community assembly, visualizing several of the sampled variables that come into play during this process.

Our main goal was to show a robust relationship between biodiversity and early plant regrowth. The underlying processes that build this relationship are indeed complex, and we aimed to provide several mechanisms for its establishment. Though our mechanistic study is not exhaustive and we now place greater emphasis throughout the revised text on the need to further study boreal disturbance dynamics in order to better predict future carbon cycling.

1. Abstract, sentence 4: This sentence in the abstract is long and complex, making it difficult to understand. The use of the verb "to find" implies an experimental design where a particular pattern was deliberately sought out. Is that the intended meaning?

We broke the sentence in two. We clarify that the wildfires were naturally occurring and that surveying them revealed a large range of fire severities

LN31: This study surveyed 49 separate, naturally-occurring wildfires occurring across the near-entire climatic range of boreal Fennoscandia. A large range of fire severities was found to limited early conifer recovery while preserving biotically-derived material that restricted growth of alternate broadleaf overstory forms.

2. P.2 Text, main section: Be careful of the use of "alternative" (meaning 2 options) vs. alternate (meaning multiple options).

We have replaced ``alternative'' with ``alternate''.

3. “Though limited propagule…” I'm not sure this is a complete sentence.

``Though..'' was replaced with ``Although, …''

4. P.2 General text: I find some of the wording and construction of sentences to be difficult to follow. I'd recommend some additional editing for readability that could improve clarity (esp. for North American readers).

In addition to specific addressing of the reviewers' comments we have made edits throughout the manuscript to break up dense sentences, improving readability. Please see the revised manuscript copy.

We are uncertain what changes might benefit North American readers. The main author is native to the West U.S./Canada/Alaska and has tried to frame some of the points better for perspectives developed from readers focused on those forest types.

5. P.2 Last paragraph: As in the introduction, I would favour laying out the objectives of the study more broadly than is currently being done. In particular, rather than specifying that the focus was on conditions that impair recovery capacity, why not emphasize more generally the relationship between recovery capacity and fire severity? This alternate phrasing leaves open the possibility that the researchers would also have reported any positive effects if those were found in the data.

The study aims are rephrased more broadly:

LN90: The aim was to investigate patterns of ecosystem reorganization after burning to determine how C recapture is controlled by the interactions of plant regrowth with residual ecosystem vegetation, microbial communities, soil structure and climate.

Because there is previous evidence of thresholds of organic layer thickness on plant growth (especially broadleaf trees) in the region (LN231, revised manuscript), we did not have reason to expect the increases in forest warmth to be fully utilized by regrowing vegetation. Due to recent advances in understanding the role of plant-soil feedbacks in establishing plant species diversity gradients (LN52 revised manuscript), our original hypothesis was that growth would be better constrained by biodiversity than climate and/or fire severity.

6. P. 3 Results & Disc, Paragr. 1: If the burn characteristics are being framed in the context of fire severity, it would be helpful to have a summary of the estimates of burn depth along with ResOL.

Agreed, this would be useful information. It was originally not included because it was an estimate, not a measurement, with some negative values present due to random error. We include it now with the mean and standard deviation.

LN107: Across the sampled plots, a broad range of residual organic layer thickness (ResOL, 2.5 to 17.1 cm, mean 7.0 cm), surviving conifer basal area (LiveConifer 0.0 to 47.5 m2 ha-1, mean 14.4m2 ha-1) and estimated depth of organic layer burn (BurnDepth, mean 4.6 cm, standard deviation 4.3 cm) were observed

7. P.3 The header for section 2.1 does not make sense to me. Please rephrase.

The header was changed to the following:

2.1 Conifer-induced barriers to plant regrowth diversity can be reduced by wildfire burning

8. P.3: “There was virtually no conifer regeneration…” This result was very surprising to me, and I felt I needed more context to understand it. If this is highlighted as a significant part of the paper, why were not drivers of conifer recruitment analysed specifically?

It was also surprising to us, as our initial intention when we conceptualized the study was to analyze the differences between drivers of broadleaf vs. conifer recruitment. The limited biomass contribution of the conifer seedlings that we did sample (< 0.1% of sampled biomass) were included in the Bray-Curtis dissimilarities that were used in db-RDA. The resulting tri-plots provided no information on the drivers of conifer sprouting (conifer vector tip near 0 in Figure 3, lower panel).

We therefore focused our discussion on the drivers of early broadleaf growth under the conditions where conifer recruitment is minimal.

The plant community regrowth patterns were compared in detail (e.g., db-RDA and regression) to the density of the surviving mature conifers (LiveConifer).

9. Section 2.1, Paragr. 2: I wasn't able to follow the logical justification for interpreting causal processes mediated by fungal effects on plant recruitment. Should there be citations to other work here? Further explanation of the hypothesized mechanisms would be helpful.

We agree that this was not explained in enough detail and have completely revised this section to include more data and references. Please see the revised manuscript for this section revision.

Specifically, we note that fungal PLFA markers dropped along gradients of MAT due to burning, along with increases in gram-positive bacteria markers. Unfortunately, PLFA markers do not differentiate ectomycorrhizal fungi from the broader fungal community. Though reduced conifer density increased regrowth biodiversity (Figure 4a) and greater GP:GN ratios (negatively covarying with fungi) encouraged broadleaf tree growth (Figure 3).

In the text, we now better emphasize the speculative nature of the meditation while adding several references regarding ectomycorrhizal sensitivity to burning. For example:

LN149: However, a residual influence of surviving trees was detected in their ability to limit the species richness of regrowing plant communities. This limitation was potentially mediated by the replacement of conifer-supporting fungi with more opportunistic microbes capable of driving soil turnover conducive to a greater diversity of plant establishment.

10. Section 2.2, Paragr. 1: "Hence, under the weakened ability…" Please explain this statement further so that a general audience can understand.

The preceding (Section 2.1) was rewritten to better describe how the surviving overstory conifers did not demonstrate an ability to limit nitrogen availability. That is, the variable iN was not constrained by LiveConifer as it was by RhizErc. The line in Section 2.2 is now replaced:

LN172: Under the limited ability of surviving conifer overstory to control nutrient mobilization, the proportion of rhizomatously resprouting ericoid shrubs in the sampled regrowing vegetation

(RhizErc) was negatively related to soil inorganic N availability (iN) and the nutrient-mobilizing GP:GN in db-RDA.

11. P4, bottom section: I think that the impact of this paper would be substantially increased if it included a conceptual figure that illustrated the fairly complex set of feedbacks being described here. Although this study lies within my broad area of interest and expertise, I found it difficult to quickly grasp the full set of interactions being described in the text. An illustration would be a big help. Maybe I'm just tired, but then, many of the readers may be in a similar state of mind. Please help us out.

Thanks for this tip, it is agreed that a figure would help. We added a conceptual diagram as Figure 3. In order for boreal forests to replace primary productive capabilities damaged by fire, it must recover previous vegetation or shift to arrangements with novel species. Individual plants must pass through several environmental filters in order to establish. It is proposed that overly strict filters during community assembly can limit growth rate under a given mean annual temperature. In the revised manuscript we now try to better encapsulate the complexities of plant community assembly by describing them as supporting explanations for the emerging relationship of biodiversity restraint on plant regrowth.

12. Section 2.4: I think this concluding statement about C recapture needs to be tempered by the reality of the data being examined. After all, we are only looking at a snapshot of vegetation recovery from 2 years after fire! In the case of species established from seed, many individuals may be quite small at 2 years but with the capacity for substantial growth and C uptake over ensuing years. A slight change in language would improve the credibility of the claims of the paper.

Agreed, the results should not be extended to make any substantial claims about the maximal C storage abilities of the forest later in time. Our intention there was only to state that biodiversity can affect C recapture in general. The sentence was rephrased:

LN268: The current study provides a direct demonstration of the influence of plant biodiversity, as modulated by natural environmental interaction, on the ability of boreal forests to recapture C in the years immediately following wildfire.

It is true that lower levels of biomass can grow substantially in later years. Though delays in recovery can extend the period over which C recapture begins, limiting the time-integrated C storage of the forest over a disturbance cycle. Current biodiversity theory holds that initial species richness during community assembly tends to increase peak biomass of the system. Though these trends lack support in real forest ecosystems. We add an additional explanation to the end of the section to highlight the potential importance of early ecosystem recovery while maintaining the need for further research:

LN271: Mesocosm growth experiments generally show a positive relationship between initial species richness and peak system biomass. Yet species richness in natural climax systems relates to its biomass variably, implying a filtering effect during community assembly that benefits from expanded selection within initial pools of species function (Hagan, 2021). This theory aligns with the current study's observation of diversity constraints on the accrual of biomass in the sampled burn scars. When paired with a warmth-enhanced pace of decomposition, this effect has the potential to both delay the transition of forests to states of net C recapture and establish recovery trajectories limiting its maximal accrual. Much more research is needed to connect early forest recovery patterns to longer term growth trajectories, especially under conditions of adaptive mismatch between climate and extant vegetation (Reich et al. 2022). However, extending understanding of biodiversity from small-scale experimentation and observation in climax systems to the greater spatiotemporal scales of forest disturbance cycles is important for better determining the C storage capacity of the boreal region in the coming decades of climate change.

13. Conclusions: The statement, "wide range of burn conditions," left me confused in my first read through the ms because the methods specifically state that sampling was preferentially done in the more severely burned parts of a burn. This aspect of the sampling design necessarily restricts inference to burn conditions that are at the severe end of the spectrum. Please modify so that the domain of inference is clear.

The intention of selecting study sites within the more severely burnt sections of each fire (as indicated by NBR), was to ensure we did not sample in sections where the fire was only beginning to develop, or was dying off. Because each of the 49 separate fires had different burn dynamics and distributions of field load, the maximal severity achieved within each fire was highly variable across those 49 plots. We have rephrased using the word “burn severity” which we defined explicitly earlier in the text (reductions in conifer biomass and soil organic material).

LN290: …under a wide range of burn severity…

14. Conclusions: "...virtually no coniferous seedling reestablishment" - please see my notes regarding hesitation in how appropriate this inference is from the sample design. At the least, I am uncomfortable with this result taking such a prominent place in the conclusions.

It was also a surprising result to us. So much so that we took extra care during study design to ensure its accuracy. Please see response to comment 16 below for additional detail on our methodology. In summary, we performed additional plot-wide surveys to corroborate what was measured in the sampling quadrats.

15. Concl: “ Yet, it appears…” I like this sentence, but it needs further detail to summarize what those feedbacks are. Additional feedbacks are described with some detail in the following sentence; a similar level of description would help the reader better understand what the first set of feedbacks are referred to.

This is a good point. We assume that it is due to mycorrhizal relationships, but our measurements cannot support this in detail. We have revised this section.

LN294: Yet these conifers did manage to place restrictions on overall plant diversity. Inhibitive plant-soil feedbacks emerged through the resprouting of ericoid shrubs, which suppressed both nutrient-mobilizing saprotrophic bacterial communities and overall N availability.

16. Section 5.6: Was any effort made to record presence of species in the larger plot that were not sampled in the biomass sampling quadrats? With a relatively small sample area, I can imagine that there was a good chance of species being missed.

We agree that the 6 sampling quadrats represent only a small area of each of the 49 20x20m^2 wildfire plots. For random sampling to be most effective at representing the larger plot area, a significant sampled area must be covered, otherwise strongly non-representative samples are possible. However, we carefully considered this during study design and data analysis as follows:

1. The biomass and species richness data are both taken from within the quadrat areas. While the sampled area may have missed some species that are present in the wider forest plot, the species richness to biomass growth relationship is a direct measurement taken within the same space. That is, the species richness within the sampled area was related to the plant growth within that area.
    a. While the small sampled area may produce outlying high or low strength of relationship between species richness and biomass relative to what might be measured in the entire 20x20m^2 plot, this random error is mitigated by replication (49 plots). Should we have taken a greater sampled area per plot it may have increased the already high Pearson's r value (0.699) of this relationship by suppressing outlying sample points, but is unlikely to have a strong effect on the trendline (Figure 4c).
2. The covariates that drove the differentiation of plant species and functional group regrowth (Figure 2), on the other hand, were taken from varied points across the 20x20m^2 forest plot and averaged. This increased the spatial scale of the analysis from the quadrat to the stand level. In this case, a low sampled area might indeed miss plant species occurring in the plot that would have responded to the sampled environmental filters. However, this effect of missing species occurs also naturally over larger areas (i.e. 10s of meters or more) due to the stochasticity of plant community assembly. That is, higher sampled area per forest plot may still miss information regarding species that would have otherwise been affected by the site conditions simply because they were not present by chance (e.g. not dispersed from the immediate or surrounding area, sprouted after instead of before a similarly competitive species (i.e. priority effects)). Our study aimed to investigate regional patterns in plant regrowth and therefore chose to spread our sampled area across the landscape rather than increase it in individual forests (car space for samples and person-hours are both limited). This

approach may have, in fact, given more information about individual species' response to environmental conditions by capturing their behavior over a greater variety of plant community structures (whereas high sample area in only a few forest plots may have missed their presence entirely). db-RDA is specifically designed for gaps in species response (i.e. species missing from sample points over environmental gradients). We therefore believe our methods to have been robustly informative for determining the most important drivers of plant community assembly both at the plot and regional level.

3. To assess heterogeneity of species regrowth and its influence on sampling methodology we performed presence-absence surveys in 16 sub-areas of each of the 49 plots. We found the survey to match satisfactorily with the biomass samples. That is, only few species were missing, with these species not being abundant in the forest plot. However, this validation was not quantified and was therefore not included in the manuscript. But we now mention this briefly in a revised version.
4. During the floristics survey we found no conifer seedlings to reach above the regrowing understory on the forest floor. Conifer seedlings were only found within the quadrat cuttings (clipped at the interface with the pyrogenic char layer below). These seedlings were no more than 5cm in height each, and together amounted to less than 0.1% of sampled biomass across all plots combined.

Therefore, we believe the sampling methodology to be adequate for our choice of analyses and results interpretation. We have added a paragraph to the end of the ``Floristics Survey'' subsection, to make these points clearer.

LN756: Random sampling of the relatively small areas covered by the quadrats had the potential to miss the measurement of plant species present in each of the larger 20x20m2 plots. However, during postfire plant community assembly information about species that can contribute to possible community assemblages in a given burn scar can also be missing due to stochastic processes (e.g. absent from surrounding species pools or succumbing to priority effects), thereby requiring regionally dispersed replication for improved statistical constraint on the drivers that influence development of plant community structure. The current study design chose to trade small sampled area per plot for spatially-dispersed, high replication across broad environmental gradients in order to reach this goal. As an added precaution, biomass to species richness relationships were derived from the same sampled area (within the quadrats), forming a directly measured relationship between these two properties. Furthermore, during methodological development, and at each of the 49 field sites, the 20x20m2 plots were divided into 16 parts where a presence-absence plant survey was performed to assess plot heterogeneity. No alarming deviations in species occurrence relative to that sampled in quadrat cuttings were detected. Notably, across all plots, no prominent conifer seedlings were observed, which aligned with conifer biomass observations being restricted to only seven, less than 5 cm tall seedlings found within the biomass cuttings. Therefore, the sampling methodology was determined to be well-suited for regional study of variation in plant community response to wildfire and the implications for regional C cycling.

17. 5.6: I'd like to see the methods description specifically address the assessment of conifer seedling regeneration. Firstly, what was the potential for very small seedlings to be missed in the biomass harvest and sampling process? Were any seedlings noted, and if so, how big were they? Thirdly, what is the time frame of seedling recruitment for Pinus sylvestris after fire, and is 2 years long enough to provide an adequate assessment of regeneration? Finally, It seems likely that the small sample area (9600 cm2, just under 1 sq. m total) would have been insufficient to detect conifer seedlings at low density. Without some stronger justification for the adequacy of the sample design, I am hesitant to ascribe much to observations of very limited conifer regeneration.

We have addressed these issues in detail in our response to point 16, which we summarize here, and provide some additional comments.

During the field campaign we performed a presence-absence survey across each of the 49 plots in order to determine the representativeness of our small quadrat sampling area. We determined the sampling error to be acceptable (few plant species tended to be missing from the samples, each contributing low amounts to the plot coverage). This validation of the sampling method was not quantified and therefore not included in the original manuscript, but we do now mention it in the revised version. During the plot-wide survey we did not find any prominent conifer seedlings among the regrowing vegetation. Conifer seedlings were only found within the quadrat cuttings (cut at the interface with the pyrogenic char layer below. No moss layers remained in the burnt plots.), all of which were below 5 cm in height. Because we cut the vegetation so low, leaving the char surface bare, it is unlikely we missed any in the cuttings.

Adding the 6 sampled areas over the 49 plots, the 294 quadrat cuttings amount to about 47 square meters. While the methodology may have some error in the conifer contribution to sampled biomass in each plot, we believe the replication over the plots to be adequate for determining there to be negligible conifer contribution to regional plant recovery at the 2 years post fire.

A goal of our manuscript was to determine the mechanisms by which early postfire net carbon emissions can be increased by changing climate. We do not assert that later conifer recovery will not occur, but that there is limited conifer influence during an early window by which alternate vegetation and associated microbes can establish. From the trends in our data, and from comparison to other literature throughout the manuscript, it appears as if climate warming on the current landscape may increase soil decomposition while at the same presenting inhibitions on plant growth. We do state that early broadleaf encroachment and microbial community shifts may inhibit future conifer recovery, but do not claim that this is strongly supported by our data.

Unfortunately, there is little published data on typical *Pinus sylvestris* seedling densities in boreal forests at specific intervals after wildfire. But we do know the species recruits continuously, especially under canopy openings, and is therefore capable of recovery even after the 2 year period.

We have attempted to clarify our research aims and scope throughout the manuscript. Especially through the addition of the conceptual Figure 3.

18. Random observation: Typo in Ref 11: Should be "Hollingsworth"

Thanks, and nice catch! It is now fixed.

I hope these comments are helpful and I look forward to reading a published version of this paper in the future,
Jill Johnstone

Thanks again, for your helpful tips. Thanks especially for your openness with the tips regarding the variety of readers and reader-energy levels. We agree that the text can be better written for a broader audience and hope that the changes will make for an improved contribution to the field.

Reviewer #2 (Remarks to the Author):

GENERAL COMMENTS

In order to characterize vegetation regrowth and post-fire carbon recovery across the Fennoscandian boreal forest, this study evaluated tree survival and mortality, new plant establishment, soil organic layer depth, and soil microbial biomass and nutrient properties two years after fire in 49 plots that spanned mean annual temperature (MAT) gradient of about 7C. The plots were conifer-dominated forests before the fire, and the study sought to evaluate the likelihood and underlying mechanisms of transitions from conifer- to broadleaf-dominated forest following fire. High-severity fire was predicted to foster transition to broadleaf tree species, largely due to substantial reduction of the soil organic layer. Low-severity fire was predicted to sustain conifer dominance, largely due to retention of soil organic matter and associated limits on establishment of broadleaf tree species. Moderate-severity fire was predicted to remove enough of the soil organic layer to limit conifer forest recovery while not consuming enough of the soil organic layer to favor establishment of broadleaf trees. Results largely supported these hypotheses with the exception that even the low-severity plots had little or no new conifer establishment by two years after fire.

Overall, this study focuses on an important topic that has potentially to substantially alter the carbon-cycle feedbacks of boreal forests to the global climate system. The findings seemed reasonably well supported by the analyses. The emphasis on the importance of long-term carbon emissions from post-fire soil decomposition rather than focusing only on emissions during the fire itself seems like an important contribution of this study. However, I found several issues that could be substantially improved before the manuscript is ready for publication.

Thank you for your careful assessment of our manuscript. We appreciate the time taken to provide the detailed feedback and we hope to address your concerns satisfactorily.

First, the title did not seem appropriate for the objectives of findings of the study. The title emphasizes biodiversity as the factor that limits carbon recovery, but the plant-soil feedbacks seemed to play a more important role than biodiversity itself. The Conclusion section focused little on biodiversity and more on plant-soil feedbacks. The strength of the plant-soil feedbacks seemed to be influenced more by the depth of the remaining organic soil layer, and to some degree, by the resprouting ericoid shrubs, but not by the overall species diversity. Most of the analyses focus on plant function groups rather than actual species divdersity.

True, diversity can't really do anything by itself. Diversity is a property of a biological community that tends to correlate with ecosystem functions as well as processes that assembled that community**. Because the environmental filters we analyzed interact with each other and affect the plant communities in different ways under different conditions that are difficult or impossible to exhaustively measure, we sought a higher level metric by which to more simply judge their ability to acquire carbon.

Species richness was the strongest determinant of plant regrowth across the sampled variables from all 49 plots as demonstrated in the strong multiple regression ($R2 = 0.615$) shown in Figure 4a and simple regression ($r = 0.699$) shown in Figure 4c (revised manuscript). We consider this to be our main result and titled the manuscript accordingly.

We aimed to provide several mechanisms for this relationship in the data analysis and discussion, though we agree this may disorient the reader if not organized carefully. We now include a conceptual diagram (Figure 2, revised manuscript), to demonstrate some of the factors that might influence the composition of the ecosystem and how these factors can interact to control the levels of species richness found in the plots.

Note that there are several aspects of overall biodiversity including taxonomic and functional diversity. We find taxonomic diversity (i.e. species richness) to relate strongly to carbon accumulation, while the sampled environmental variates appear to more strongly explain functional diversity. This means there may be some redundancy in species function. Though not enough information is available about each species to exhaustively describe why these two aspects of diversity don't behave exactly the same in the data analysis.

We have now rewritten the conclusion to reference the concept of diversity and how its impact is built by the underlying ecosystem feedbacks.

**Integrating community assembly and biodiversity to better understand ecosystem function: the Community Assembly and the Functioning of Ecosystems (CAFE) approach
Bannar-Martin et al 2017

Second, I found much of the writing difficult to follow, and I found myself having to re-read several paragraphs two or three times to understand them. Several terms needed to be defined more explicitly, and there were several places where broad terminology was used when more specific terms would have been more appropriate. In general, it seems like the writing may be suitable for people who use the same terminology and in the same context as the authors, but there were many cases where it would be better to make the writing more accessible to a broader audience.

We agree that much more care can be taken to explicitly define terms in order to reach a broad audience. Thank you for your specific recommendations, which we address in this document individually below. Please see the revised manuscript for overall improvements in readability.

For example, in the second paragraph under Section 1, the phrase, “damaging regenerative structure,” was used when it would have been better to spell out what you meant by regenerative structure.

This has been revised to the following:

LN76: Therefore, intermediate ranges of fire severity in warming boreal forests can both restrict recovery of occupant overstory species by reducing their reproductive capabilities while failing to completely remove the plant-soil feedbacks that augment biodiversity constraints on overall vegetation recovery.

Similarly, the third paragraph uses the phrase, “residual ecosystem structure,” instead of more explicitly explaining what you meant

This has been revised:

LN90: The aim was to investigate patterns of ecosystem reorganization after burning to determine how C recapture is controlled by the interactions of plant regrowth with residual ecosystem vegetation, microbial communities, soil structure and climate.

Also, it seemed like several concepts needed to be explained in more detail for a broader audience. For example, the end of Section 1 refers to MAT-enhanced shift in soil communities toward saprotropic bacteria without explaining the implications of such a shift for biodiversity or carbon sequestration. I provide other examples under my specific comments, below.

We have added a conceptual diagram (Figure 2) to help with explanation and interpretation of the results. Specifically, there we described how removal of nutrient-controlling ericoid mycorrhizas can allow greater soil turnover, promoting decomposition and more nutrient demanding plant communities.

Third, given the different types of data analyzed in this study, I would prefer to see the Results and Discussion each as separate sections rather than a combined Results and Discussion section. As written, a relatively large portion of the text in this section seemed to focus on interpreting results that had not been presented yet.

Thank you for this advice. We agree that the information can be better organized across sections. The presentation of the data is now arranged to better support discussion, such that a fundamental re-arranging of text into results discussion is not necessary in our opinion. As such, we have chosen to retain merged results and discussion similar to publications we see in the journal, as well as meet the strict length limitations. However, if there is consensus among the reviewers and editor that separate results and discussion are still needed we would certainly consider this additional step.

For example, the first paragraph under Section 2.1 provided results of this study similar to what would be expected in a Results section of a paper. However, the second and third paragraphs of Section 2.1 made interpretations beyond the results that had been presented in the previous paragraph, and they did not cite any specific results from this study or cite any other literature. This type of interpretation would be better in a Discussion section after all of the results had been presented. Similarly, the last paragraphs of Sections 2.3 and 2.4 did not cite specific results of this study and cited few if any other articles to place the findings in the context of the broader body of literature.

Sections 2.1, 2.3 and 2.4 are now substantially rewritten to present the necessary results before discussion, as well as placing the results in the broader body of literature.

Specifically, we compare the results to poor conifer regeneration in other sites globally in section 2.1 as well as place the results of 2.4 in the context of biodiversity-ecosystem function (BEF) theory.

Fourth, the interpretation that this study found a “weakened early recovery capacity of boreal conifer forests” (as stated in the first sentence of the Conclusion) seemed premature given that the sampling was only conducted in the second-year post-fire. This interpretation could be better supported if data were provided on seed production or weather/climate during the first two years after fire. This study cited one study that found high seedling densities in boreal Eurasia (Kharuk et al. 2021 – cited at the end of the first paragraph of Section 2.1), but it was not clear of that study only used data from the first two years after fire. In most parts of the world, the window available for tree regeneration after fire lasts for at least several years, if not for a few decades. A short window is typically only found when very dense vegetation, such as resprouting shrubs or shrubs that establish from a dormant seedbank, rapidly form a dense cover. Also, it was not clear how the seedlings were sampled in this study. According to Section 5.6, all vegetation was clipped within six 40 x 40-cm quadrats and then weighed by functional group. However, it was not clear if this was where the tree seedlings were also counted. This seems like a small sample area per plot for quantifying tree seedling density.

We agree that observations over extended time periods would be more informative of long-term conifer recovery. However, the aim of our study was to uncover mechanisms by which climate change can provide for increases of net emissions of carbon from boreal forest ecosystems during the early stages of postfire recovery. Unfortunately, there are limited observations of the typical timing of conifer seedling regeneration in boreal Eurasia and the relationship of that timing to long-term success. The Kharuk et al. 2021 reference produced its numbers for Scots pine based on observations in ``the first years after fires'', not providing a more specific time period. By using this reference we intended to show that Scots pine is capable of vigorous regeneration in boreal Eurasia early post fire (i.e., first two years), but in our plots, did not. Therefore, we do not put forward any strong conclusions from our data about longer term conifer recovery and its impact on carbon storage over more extended time periods. We simply state that a weak recovery had occurred by 2 years postfire. We did, however, suggest that the influence of rapidly resprouting ericoid shrub cover on microbial communities and nutrient cycling may impact longer term patterns of tree establishment, as you also mention.

LN138: Furthermore, across all sampled plots, there was virtually no conifer regeneration on the forest floor (< 0.1 % of total recovering biomass), despite vigorous *Pinus sylvestris* recruitment previously observed during the first years after wildfire in boreal Eurasia (e.g., > 100,000 seedlings ha-1 (Kharuk et al 2021), two orders of magnitude greater than the current study's estimate of about 1500 seedlings ha-1, all below the understory canopy).

LN158: Prolonged study of the recovery capacity of boreal conifers is crucial to better determine their contributions to the forest C cycle at the decadal scale.

Please see response to Reviewer #1 Point 16 for detailed explanation regarding the sampling methodology. We briefly summarize in the following:

1. The small sampling area is compensated for by high sample replication across the 49 plots, giving a regionally dispersed perspective on plant regeneration.
2. In each of the 49 plots, a species survey was performed across its entire area to validate the representativeness of the quadrat samples. This validation was not quantified, but now mentioned in the revised manuscript.
3. No conifer seedlings were found to be prominent among the recovering vegetation during the plot-wide surveys. They were only found mixed within the cuttings within the quadrats, with each seedling being less than 5 cm in height.

Therefore, we believe that our study provides evidence for a weak early recovery of conifers. Conifer recovery may proceed in the coming years, but by that time, an amount of carbon will have been released during decomposition and some competing vegetation established.

Finally, although this study focused on vegetation recovery and associated carbon-cycle feedbacks to the climate system, it would have been useful to also discuss albedo feedbacks to the climate system. In a region where snow covers the ground for a large portion of the year, the albedo feedbacks of conifer persistence or a shift from evergreen conifer do deciduous broadleaf trees may have a similar, if not stronger influence on the climate system than the carbon-cycle feedbacks.

Thanks for this interesting suggestion. It is true that shifting forest cover might adjust albedo to compensate for any carbon emissions in terms of radiative forcing. High-latitude ecosystems have very low insolation during the winter months, but have much higher insolation during the summer months when the surface might be darker due to fire damage. Changes in aerosol concentrations, humidity, cloud cover, etc. might also have an effect on radiative forcing. We do not have any data to quantitatively support any of these effects in detailed discussion. However, we do add this important point in the text.

LN311: Land cover shifts associated with this wildfire driven reorganization of the elements have the potential to further determine atmospheric energy balance by adjusting surface reflectivity.

SPECIFIC COMMENTS

At the end of the first paragraph of Section 1, does the growth of broadleaf trees compensate for the loss of above- and belowground carbon previously stored in the conifer forest, and if so, could you provide a rough time frame before the site transitions from a source to a net sink of carbon?

Yes, within the cited study the transition to deciduous dominance results in a net carbon sink in the ecosystem over the disturbance cycle. That is, carbon stored in the previous boreal conifers and soil was more than replaced by total deciduous forest carbon before repeated wildfire. This was rephrased, with added temporal information, with the following text amendment:

LN61: Vigorous recruitment of broadleaf trees after conifer forest wildfire can rapidly sequester C, reaching a greater total amount than that held in previous conifer forest structure, providing a net sink of greenhouse gasses from the atmosphere over the disturbance cycle.

The references mentioned do not provide information on when the forest returns to a net sink. Time-sensitive postfire emissions monitoring is rare, though in the revised manuscript we do include a reference showing that burnt Alaskan forests do not return to a C-sink until 13 years after fire**. Currently, a research group is monitoring burn scar fluxes from the 2018 Swedish fires, having found net emissions of carbon to persist for more than 4 years, surpassing those of initial combustion (this data is still being processed, and not currently available).

** Carbon dioxide balance in early-successional forests after forest fires in interior Alaska
Ueyama et al 2019

The second paragraph of Section 1 refers to “limited propagule input,” but this study was only focused at the plot level and did not evaluate broader seed sources. Particularly when focusing on intermediate fire severity, local conifer seed sources will persist, and given the long-distance seed dispersal of broadleaf species like birch, it is not clear that seed sources of broadleaf trees will be limiting.

That paragraph was in the introduction, in order to give background on establishment limitations of plants. One of the references (Pichon et al. 2023) cited in that sentence shows how novel species seed addition is required to use increased nutrient and warmth to maximize biomass accumulation. The other (Day et al. 2023) demonstrates how adequate experimental broadleaf seed addition can partly overcome establishment restrictions placed by thick organic layers in boreal forests. Given this, broadleaf establishment in burnt forests with residual organic soil layers is likely to be limited by the amount of broadleaf propagules dispersed into the area.

The added conceptual diagram (Figure 2) better places these ideas in the context of our study. There we explicitly acknowledge that dispersal was not measured directly in the study. Though, it is argued that dispersal limitation will interact with site conditions (that is, higher propagule inputs can act to partly overcome hindrances such as organic layer depth), and these effects will emerge as biodiversity constraints on plant growth.

This will be especially true for species that are currently not common in a given area, but can grow there. For example, we found species like *Sorbus aucuparia* to establish in cooler, nutrient poor regions. Because this species was so rare across all plots, we suggest that it is currently not well dispersed in northern regions. However, this is not directly proven.

In the first paragraph under Section 2.1, you mention that there was virtually no conifer regeneration on the forest floor (< 0.1% of total recovering biomass) and then compare this to the conifer seedling density of > 1000,000 seedlings/ha found across boreal Eurasia. It would be useful to also report the results for the present study in seedlings/ha. The study was conducted in the second growing season after fire, and a conifer seedling in its second year of growth is likely to have little biomass. I would expect the resprouting shrubs and some of the fast-growing herbs to have much higher total biomass by the second growing season than conifer seedlings, but a small number of conifer seedlings present in the second year after fire could still provide potential for abundant carbon sequestration as the conifers grow larger over the first couple of decades after fire.

We agree with this possibility. 7 seedlings over the 0.0047 hectare sampled area in our plots equates to about 1500 seedlings/ha, two orders of magnitude lower than the cited observations. We now report these calculations in the revised manuscript.

LN138: Furthermore, across all sampled plots, there was virtually no conifer regeneration on the forest floor (< 0.1 % of total recovering biomass), despite vigorous *Pinus sylvestris* recruitment previously observed during the first years after wildfire in boreal Eurasia (e.g., > 100,000 seedlings ha-1 (Kharuk et al 2021), two orders of magnitude greater than the current study's estimate of about 1500 seedlings ha-1, all below the understory canopy).

I could not follow what was meant by the "window of time" in the third paragraph under Section 2.1. Was this a window of time in relation to time since fire or a window of time in relation to the current degree of climatic warming, or something else?

It was meant that under the current climatic conditions there is a window of time after wildfire burning where conifers have less control on ecosystem function.

LN160: However, the current study reveals an important window of time following wildfire burning, under current climatic conditions, where coniferous control can be breached…

In the second paragraph of sections 2.2, when you refer to "overstory seedling growth," does this mean the growth of tree seedlings, or the growth of seedlings of species eventually capable of becoming overstory trees?

We mean these have been observed to inhibit growth tree seedlings in general.

LN185: …and can even suppress tree seedling growth.

At the end of the second paragraph under Section 2.2, it would be useful to clarify what you mean by "deborealization," which is referred to in parentheses as the "shift towards intensified nutrient cycling supportive of temperate forest structure and function." Is this more succinctly described as a shift from conifer- to broadleaf-dominated forest, with associated increases in rates of nutrient cycling and reductions in the depth of the soil organic layer? In a related comment, it would be useful to clarify what you mean by "temperate forest structure and function?" I suspect you are referring to something specific meaning here, but temperate forest structure and function could encompass a very broad range of meanings.

Indeed, the terms can encompass many things. Our meaning is well captured in your suggestion and so we have incorporated it into the text.

LN190: …deborealization (i.e., shifts from conifer- to broadleaf-dominated forest, with associated increases in rates of nutrient cycling and reduced soil organic layer depth).

The end of the last paragraph of Section 2.3 refers to "regional species pools," but this study was focused at the plot level and did not address broader-scale species pools.

This would be true if the study was performed only in a single plot, where only the local (burn scar and immediate surroundings) species pool would apply. However, we examined 49 plots over a larger region. This means that regional variation in species pools will be included in the trends in the data analysis, if only implicitly. We revise for greater clarity:

LN135: Under these conditions, the ability of burnt forests to recapture C after fire appears to be determined by multiple, interacting controls derived from postfire soil conditions and regional variation in species pools under the influence of climate.

When you refer to inhibiting "novel" species establishment, do you mean broadleaf tree species. If so, it would be useful to say this directly. If not, it would be important to clarify what you mean and whether this refers to any species that was not already present or only species that have certain functions within the ecosystem.

Our species richness measurements include all sampled plant species and does not discriminate by function. We have clarified where the term ``novel species'' better refers to broadleaf trees.

LN201: High-severity wildfire that removes ericaceous rhizomes along with organic layer material may reduce restrictions on novel species establishment, especially broadleaf trees.

And made sure to add early in the introduction section that our interests included all novel plant species, including those destined for the understory.

LN67: However, more is to be known regarding the mechanisms and extent by which biotically-derived material left behind after fire can continue to resist alternate growth of both overstory and understory vegetation.

In the last paragraph of the Conclusion, you refer to post-fire vulnerability of boreal C stores expected to be "greatest in regions with high concentrations of fire-resisting vegetation, such as boreal Eurasia, with successional dynamics thus not well-adapted to recovery after higher burn severity." However, this contradicts that statement you made earlier (Section 2.1) about vigorous conifer recruitment having been observed to be common (> 100,000 seedlings/ha) across boreal Eurasia.

In the last paragraph of the Conclusion, you refer to regions with high concentrations of "fire-resisting vegetation," such as Eurasia, and you mention that the successional dynamics are not well-adapted to recover after higher burn severity. This seems to contradict the example I referred to in the previous where you previously referred to another study (Kharuk et al. 2021) that found > 100,000 seedlings/ha across boreal Eurasia. It would be useful to clarify what you mean by "fire-resisting vegetation," and why you interpret this vegetation as not being well adapted to recover after high-severity fire.

Attentive catch, thanks for spotting that. What we mean is that large fire-resisting vegetation areas such as found in boreal Eurasia tend not to have successional dynamics as well implemented as areas of North America where near-complete combustion and broadleaf stand growth is more common (thereby offering a variety of seral stages and stand types in the landscape to provide a diversity of plant propagules to suit a variety of burn scar conditions). While the example of vigorous recruitment shows how Eurasian forests *can* recover well, we propose that changes in the extremity of fire severity in areas that do not have the proper vegetation recovery dynamics implemented, are vulnerable to poor carbon recovery.

LN314: Extended postfire vulnerability of boreal C stores is expected to be greatest in regions with high concentrations of fire-resisting vegetation, such as boreal Eurasia, with successional dynamics that may not be well-adapted for providing plant function capable of quick recovery under changing extremities of burn conditions.

Section 5.4 is titled "PLFA Analysis," but PLFA was never defined in the text.

The section title is now spelled out. Within the text, the abbreviation is defined at its initial occurrence.

Thank you again for your careful attention and thoughtful suggestions. We believe the revised manuscript is greatly strengthened by your contributions and hope your concerns have been well addressed.

23rd Feb 24

Dear Dr Eckdahl,

Your manuscript titled "Plant biodiversity limits carbon recapture after wildfire in warming boreal forests" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Alienor Lavergne, PhD
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I find that the revised version of the ms has reasonably addressed the comments and criticisms I posed from the earlier version. The addition of a conceptual diagram (Fig. 2) is helpful in laying out the hypothesized interactions driving plant and microbial responses across the climate gradient. One small source of confusion with this new figure is the interpretation of the vertical brown bars (presumably representing tree trunks), which have a diagonal line with two tones of brown that may represent different C pools, but this is not clearly indicated.

I did notice some minor wording errors in the revised text, but I will leave those to be corrected in collaboration with journal typesetting process. Otherwise I believe the manuscript warrants publication in its current form.

Reviewer #2 (Remarks to the Author):

The manuscript is improved over the previous draft. The new conceptual diagram helps to illustrate the interrelated variables addressed in this study, and some of the writing was clarified. However, there were still several places where I felt that further clarification of the writing or analyses was needed, and some of the results could have been interpreted more conservatively. I refer to several specific examples with line numbers referring to the draft without track changes.

Lines 33-34: The sentence states, “A large range of fire severities was found to limit early conifer recovery,” and this wording assigns a causal relationship between fire severity and conifer recovery, which I do not believe is supported by the results. For example, the limitation on conifer recovery be driven more by soil properties than surviving trees and a loss of seed sources for conifers is a function of the extent of high-severity patches rather than a function of fire severity itself. It would be more appropriate to state the “early conifer recovery was limited across a broad range of fire severities.” This would be an accurate statement of the results without overstating a causal relationship.

Lines 40, 100, and 249: The term “biodiversity deficit” is used in at least three places in the manuscript. When calling something a deficit, it should be defined as a deficit relative to some baseline or expected value. It was never clear what baseline was used to distinguish a biodiversity deficit. Is there a baseline plant biodiversity that boreal forests are expected to have at the scale of a small sample plot? If not, it would be better to refer to low biodiversity rather than a deficit.

Lines 57-60: This is a dense sentence, and some of it does not seem to be well supported. To clarify the writing, first I would change “The extent of overstory dominance shifts in forests after fire,” to “the extent of shifts in overstory dominance after fire.” Then, I do not believe the “is controlled by” at the end of the sentence is fully supported. It would be better to change “which in turn is controlled by their landscape abundance in response to changing climate” to “which in part is controlled by their landscape abundance in response to changing climate.” Alternatively, “controlled by” could be replaced with “influenced by.”

Lines 72-73: This sentence refers to limited propagule inputs to burn scars due to slowly migrating landscape vegetation cover. At least initially, limited propagule inputs to burn scars is driven primarily by interactions between fire severity (the proportion of pre-fire vegetation that persists following fire) and patch size. Over the long term (decades or longer), propagule inputs may be driven by slowly migrating landscape vegetation cover.

Lines 77-79: When stating that “intermediate ranges of fire severity in warming boreal forests can both restrict recovery of occupant overstory species by reducing their reproductive capabilities,” it seems to assume that there is not tree survival following intermediate ranges of fire severity? I would expect local seed sources to persist following intermediate fire severity, and seedling establishment could potentially be more successful following the temporary reduction of competing vegetation. Also, it is not clear why the sentence refers to “warming boreal forests.” Is there something in the sentence that applies to warming forests but would not apply without warming?

Line 88: This sentence could be simplified from “This study surveyed the assembly patterns of the plant reestablishment stage” to “we sampled plant establishment within two years following fire.”

Line 133: It seems that “are determined by climate” is overstating the role of climate, or at least it goes beyond what is supported here. It would be better to say “influenced by” or “related to” climate.

Line 139: The statement that there was “virtually no conifer regeneration on the forest floor” contradicts the finding that there were about 1,500 conifer seedlings per hectare (lines 142-143). A seedlings density of 1,500 per hectare within the second growing season after fire would typically be considered a high density. In the Methods (lines 772), the text states that only seven conifer seedlings were found in the sample plots. I did the calculations to see how seven seedlings total could blow up to 1,500/ha, and with six 40 x 40-cm quadrats sampled per plot in 49 plots, seven seedlings would blow up to just under 1,500 seedlings. However, this is a very small sample area (just under 1 m2 in each of the 49 plots), and I am concerned about whether meaningful seedling densities can be drawn from such a small sample area. The biomass of 1-2-year-old seedlings will be very low, especially when compared to other species that can resprout from an existing root system, so I would expect conifers to make up a minimal portion of the biomass, even if seedlings were abundant. However, based on the small area sampled and the short time since fire, I would substantially reduce the emphasis on the small number of conifer seedlings found in this study. For example, lines 291-292 also refer to "virtually no coniferous seedling reestablishment at 2 years post fire," in contrast to the 1,500 seedlings/ha reported earlier (lines 142-143).

Lines 150-151: The text refers to the ability of surviving trees to "limit the species richness of regrowing plant communities." This implies a causal relationship that I do not believe is well supported by the results. It would be better to refer to a trend of species richness decreasing as the abundance of surviving trees increased.

Lines 254-257: It is not clear why correlations (Pearson's r) are presented here. The first sentence under Section 5.7.2 Regression and model selection (line 791) refers to simple regression being performed with significance reported by a p-value and correlation with Pearson's r. Calculating the correlation between two variables is not the same as a regression, even in the case of a simple linear regression where squaring Pearson's r would be equal to the R2. There are different underlying assumptions, particularly, that correlation analysis does not view one variable as the explanatory variable and the other as the response. The text on lines 253 and 257 uses the phrases, "explained" or "explained by" and cites the correlation value. It is not appropriate to interpret that one variable explains variance in another variable based on the results of a correlation analysis.

Lines 273 and 284: I have rarely seen the term "climax" in ecological studies published within the last few decades. I understand the intended meaning here, but it would be better to use another term, like "late-successional communities."

Figure 4. The caption states that "significant correlations ($p < 0.05$) between predictor variables are also labeled. I do not see labels of significant correlations on a figure. I do see R2 values, but this is not an indication of significance. It is not clear what the R2 values represent or why an R2 would be calculated for a subset of variables included in a regression.

Author response to *Plant biodiversity limits carbon recapture after wildfire in warming boreal forests*

Reviewer text is left in **black**. Author responses are given in **blue.** Manuscript revisions are given in **red**. Please see separate documents for a revised manuscript and tracked changes.

Reviewer #1 (Remarks to the Author):

I find that the revised version of the ms has reasonably addressed the comments and criticisms I posed from the earlier version. The addition of a conceptual diagram (Fig. 2) is helpful in laying out the hypothesized interactions driving plant and microbial responses across the climate gradient. One small source of confusion with this new figure is the interpretation of the vertical brown bars (presumably representing tree trunks), which have a diagonal line with two tones of brown that may represent different C pools, but this is not clearly indicated.

I did notice some minor wording errors in the revised text, but I will leave those to be corrected in collaboration with journal typesetting process. Otherwise I believe the manuscript warrants publication in its current form.

Thank you again for your contribution to the development of the manuscript. Your insight has been appreciated. We have adjusted Figure 2 to reduce confusion regarding the tree trunks.

Reviewer #2 (Remarks to the Author):

The manuscript is improved over the previous draft. The new conceptual diagram helps to illustrate the interrelated variables addressed in this study, and some of the writing was clarified. However, there were still several places where I felt that further clarification of the writing or analyses was needed, and some of the results could have been interpreted more conservatively. I refer to several specific examples with line numbers referring to the draft without track changes.

We appreciate your time and effort in further reviewing the manuscript and providing helpful commentary. We hope that we have addressed the additional issues satisfactorily with a new version.

Lines 33-34: The sentence states, “A large range of fire severities was found to limit early conifer recovery,” and this wording assigns a causal relationship between fire severity and conifer recovery, which I do not believe is supported by the results. For example, the limitation on conifer recovery be driven more by soil properties than surviving trees and a loss of seed sources for conifers is a function of the extent of high-severity patches rather than a function of fire severity itself. It would be more appropriate to state the “early conifer recovery was limited across a broad range of fire severities.” This would be an accurate statement of the results without overstating a causal relationship.

We agree this is not the most appropriate phrasing and have adjusted this sentence.

LN036: Early conifer recovery was limited across a broad range of fire severities. Although, preserved biotically-derived material restricted growth of alternate vegetation, such as broadleaf overstory

Lines 40, 100, and 249: The term “biodiversity deficit” is used in at least three places in the manuscript. When calling something a deficit, it should be defined as a deficit relative to some baseline or expected value. It was never clear what baseline was used to distinguish a biodiversity deficit. Is there a baseline plant biodiversity that boreal forests are expected to have at the scale of a small sample plot? If not, it would be better to refer to low biodiversity rather than a deficit.

By deficit, we meant an amount that served to constrain biomass accumulation more than any of the environmental variables, e.g. MAT. We replace the wording throughout the manuscript.

LN041: These results suggest that large portions of the boreal region are currently at risk of extending postfire periods of net emissions of C to the atmosphere under limitations in plant biodiversity generated by wildfire and a changing climate.

LN099: It was anticipated that these complex interactions can emerge as low levels of plant biodiversity that constrain the ability of boreal ecosystems to recapture C under increased warmth, likely establishing longer-term forest recovery trajectories that limit landscape C storage over the coming decades of climatic change.

LN249: As a consequence, the utilization of resources made available by wildfire burning may be bounded by restricted plant diversity under longer and warmer growing seasons associated with increased MAT.

Lines 57-60: This is a dense sentence, and some of it does not seem to be well supported. To clarify the writing, first I would change “The extent of overstory dominance shifts in forests after fire,” to “the extent of shifts in overstory dominance after fire.” Then, I do not believe the “is controlled by” at the end of the sentence is fully supported. It would be better to change “which in turn is controlled by their landscape abundance in response to changing climate” to “which in part is controlled by their landscape abundance in response to changing climate.” Alternatively, “controlled by” could be replaced with “influenced by.”

Thanks for these suggestions. We have rephrased accordingly.

LN058: The extent of shifts in overstory dominance after fire then appears to be largely determined by the amount of on- and off-site provisioning of broadleaf generative material, such as seed and budding sources, which in turn is influenced by their landscape abundance in response to ongoing climatic change.

Lines 72-73: This sentence refers to limited propagule inputs to burn scars due to slowly migrating landscape vegetation cover. At least initially, limited propagule inputs to burn scars is driven primarily by interactions between fire severity (the proportion of pre-fire vegetation that persists following fire) and patch size. Over the long term (decades or longer), propagule inputs may be driven by slowly migrating landscape vegetation cover.

Very true that there are several factors influencing overall propagule input. Though, our intention here was to discuss mechanisms by which novel species can establish and have adjusted accordingly.

LN072: Although, limited propagule inputs of <u>novel species</u> to burn scars due to slowly-migrating landscape vegetation cover, and their interaction with remaining plant-soil feedbacks (such as thick organic soil layers and ecto- and ericoid mycorrhizas), could limit the effect of enhanced growing conditions on C recapture in the early postfire environment

Lines 77-79: When stating that "intermediate ranges of fire severity in warming boreal forests can both restrict recovery of occupant overstory species by reducing their reproductive capabilities," it seems to assume that there is not tree survival following intermediate ranges of fire severity? I would expect local seed sources to persist following intermediate fire severity, and seedling establishment could potentially be more successful following the temporary reduction of competing vegetation. Also, it is not clear why the sentence refers to "warming boreal forests." Is there something in the sentence that applies to warming forests but would not apply without warming?

We were referring to the information in the sentences leading up to the statement regarding mounting observations of poor conifer recovery in warming forests as well as knowledge of plant-soil feedbacks that restrict novel growth. We rephrase to make this more clear.

LN076: In light of these combined challenges to regeneration under climate change, it proposed that intermediate ranges of fire severity in warming boreal forests can both restrict recovery of occupant overstory species by reducing their reproductive capabilities while failing to completely remove the plant-soil feedbacks capable of strengthening biodiversity constraints on overall vegetation recovery.

Line 88: This sentence could be simplified from "This study surveyed the assembly patterns of the plant reestablishment stage" to "we sampled plant establishment within two years following fire."

We rephrase:

LN089: This study surveyed the plant reestablishment stage within 2 years following fire across 49 separate conifer forest wildfires…

Line 133: It seems that “are determined by climate” is overstating the role of climate, or at least it goes beyond what is supported here. It would be better to say “influenced by” or “related to” climate.

We rephrase:

LN132: This means that wildfires can replace components of fungal communities with nutrient-mobilizing saprotrophic bacteria in proportions that are related to climate.

Line 139: The statement that there was “virtually no conifer regeneration on the forest floor” contradicts the finding that there were about 1,500 conifer seedlings per hectare (lines 142-143). A seedlings density of 1,500 per hectare within the second growing season after fire would typically be considered a high density. In the Methods (lines 772), the text states that only seven conifer seedlings were found in the sample plots. I did the calculations to see how seven seedlings total could blow up to 1,500/ha, and with six 40 x 40-cm quadrats sampled per plot in 49 plots, seven seedlings would blow up to just under 1,500 seedlings. However, this is a very small sample area (just under 1 m2 in each of the 49 plots), and I am concerned about whether meaningful seedling densities can be drawn from such a small sample area. The biomass of 1-2-year-old seedlings will be very low, especially when compared to other species that can resprout from an existing root system, so I would expect conifers to make up a minimal portion of the biomass, even if seedlings were abundant. However, based on the small area sampled and the short time since fire, I would substantially reduce the emphasis on the small number of conifer seedlings found in this study. For example, lines 291-292 also refer to “virtually no coniferous seedling reestablishment at 2 years post fire,” in contrast to the 1,500 seedlings/ha reported earlier (lines 142-143).

Over 49 plots each with 6 (one plot had 7) 40x40cm quadrats would mean a cut sample area of 47.2 square meters. We also visually surveyed each plot as a whole (49 *20x20m = 1.96 ha), finding no conifer seedlings to have breached the understory canopy. While a larger size may have been more accurate, we believe it is enough to determine the seedling recovery to be relatively small compared to the cited study where seedlings were two orders of magnitude higher (Kharuk et al 2021, >100,000 seedlings per ha). Furthermore, we found broadleaf tree biomass to vary substantially, contributing large portions to plant biomass in warmer regions (Figure 1c), and respond strongly to the sampled environmental variables. Conifer seedlings always contributed a small amount to total biomass and did not respond to their environment statistically. Therefore, we believe there is good evidence to support that sprouting conifers have relatively weak recovery and ecosystem influence compared to other plant types at this stage. Though, it is true that they may gain greater prominence at later stages of natural regeneration. We have rephrased to be more careful with these topics.

LN135: Under these conditions, LiveConifer failed to provide any restrictions on the mobilization of inorganic N to the soil solution (iN, $p = 0.757$) and did not significantly contribute to the differentiation of plant species or functional groups regrowing below the forest canopy in db-RDA (Figure 3). Furthermore, across all sampled plots, there was relatively little conifer regeneration on the forest floor (< 0.1% of total recovering biomass), despite more vigorous Pinus sylvestris recruitment previously observed during the first years after wildfire in boreal Eurasia (e.g., > 100,000 seedlings ha^{-1} [24], two orders of magnitude higher than the current study's estimate of about 1,500 seedlings ha^{-1} sampled over 47.2 m^2 of biomass cuttings). No seedlings were found to reach above the understory, as visually surveyed across the 1.96 ha of the 49 plots. Seedlings below the understory are expected to have reduced survival rates due to competition with forest floor plants, including for light [25, 26].

LN292: Across the near-entire climatic extent of boreal Fennoscandia, a relatively weak early recovery of boreal conifers was observed under a wide range of burn severity, resulting in less than 0.1% coniferous biomass contribution to resprouting vegetation at 2 years post fire

Lines 150-151: The text refers to the ability of surviving trees to “limit the species richness of regrowing plant communities.” This implies a causal relationship that I do not believe is well supported by the results. It would be better to refer to a trend of species richness decreasing as the abundance of surviving trees increased.

We agree that this point should be made more cautiously.

LN151: However, a residual influence of surviving trees was detected in their relationship to reduced species richness of regrowing plant communities.

Lines 254-257: It is not clear why correlations (Pearson's r) are presented here. The first sentence under Section 5.7.2 Regression and model selection (line 791) refers to simple regression being performed with significance reported by a p-value and correlation with Pearson's r. Calculating the correlation between two variables is not the same as a regression, even in the case of a simple linear regression where squaring Pearson's r would be equal to the R^2. There are different underlying assumptions, particularly, that correlation analysis does not view one variable as the explanatory variable and the other as the response. The text on lines 253 and 257 uses the phrases, “explained” or “explained by” and cites the correlation value. It is not appropriate to interpret that one variable explains variance in another variable based on the results of a correlation analysis.

We understand the different assumptions and intents of correlation and regression analysis, though had used the summary metrics imprecisely while utilizing the equivalences you mentioned. Throughout the revised manuscript we have now properly distinguished between the correlation and regression approaches.

Lines 273 and 284: I have rarely seen the term “climax” in ecological studies published within the last few decades. I understand the intended meaning here, but it would be better to use another term, like “late-successional communities.”

We have adjusted the phrasing.

LN275: Yet species richness in natural late-successional communities relates to its biomass variably.

LN285: However, extending understanding of biodiversity from small-scale experimentation and observation in mature systems to the greater spatiotemporal scales of forest disturbance cycles is important for better determining the C storage capacity of the boreal region in the coming decades of climate change.

Figure 4. The caption states that “significant correlations ($p < 0.05$) between predictor variables are also labeled. I do not see labels of significant correlations on a figure. I do see R2 values, but this is not an indication of significance. It is not clear what the R2 values represent or why an R2 would be calculated for a subset of variables included in a regression.

The results are put together in a path analysis. The predictors MAT and MicConc relate significantly and this is labeled by the standardized regression coefficient and colored arrow. This means that part of the influence of MAT on species richness is statistically mediated by MicConc. The caption was rephrased to make this clear.

Fig. 4 (a) Two multiple regression results explaining species richness and plant growth were visualized with incoming arrows labeled, colored and sized according to standardized regressions coefficients of predictor variables. All significant regressions ($p < 0.05$) between predictor variables are also presented accordingly. Simple regressions of biomass regrowth on MAT (b, R2 = 0.353) and species richness (c, R2 = 0.489) were plotted. Heteroscedasticity of the MAT regression was reduced by shifting plot plant growth values across the species richness axis. A random number between ±0.2 was added to the species richness values during chart generation to improve legibility.

Thank you again for these comments, they were very attentive and did a lot to help refine the points of the manuscript.