Thank you for the opportunity to submit a revised draft of the manuscript “Effects of short-interval disturbances continue to accumulate, overwhelming variability in local resilience”. We greatly appreciate the time and suggestions provided by the reviewers and have addressed all comments. Concerns raised by the reviewers’ seemed to primarily focus on the framing and statistical design. We’ve made several broad-scale revisions to address those concerns: we’ve added in explicit text of research questions, in order to clarify the framing and objectives of the manuscript. Following R1’s suggestions in particular, we have revised the statistical design to better addresses the questions being asked by the study while avoiding conflicting assumptions about the variables used. We’ve adjusted figures to better demonstrate the results. Finally, we’ve made the data and code used in our analysis available in a public repository. We feel the revisions solidify the narrative of the study to better reflect the insights of the data.

Please see below, in blue, a point-by-point response to the reviewer’s comments and concerns. All page numbers refer to the revised manuscript file with tracked changes.

**Reviewers’ comments to the authors**

**Reviewer 1**

How changing disturbance regimes will alter postfire succession is of keen interest in forested ecosystems worldwide. The authors of this study investigated the role of compound disturbances (sequential fires) in boreal black spruce forests, where well-established theory and multiple empirical studies supporting a potential transition from conifer to deciduous forest with short-interval fires. The mechanisms are well known. Here, the authors propose to extend understanding by examining how post-fire succession differs with the number of fires (1 to 3) that occurred in about 70 yrs and landscape position (upland vs. lowland). The second objective as evaluating the relative importance of variation in fire frequency and fire intensity. The topics are timely and important, but the authors overpromise and underdeliver. The authors seem not to have developed a narrative that reflects new insights from these data. The data do show (i) the (expected) shift from conifers to deciduous species with reburns, (ii) that it takes one more fire in the lowlands than uplands (3 vs 2); and (iii) that which deciduous species came in after fire differs between uplands and lowlands, which likely matches different environmental tolerance, or filtering. However, the study does not (and cannot based on its design) test for an over-riding effect of disturbance frequency vs. disturbance intensity (actually, they only work with severity) on postfire succession, so that “theoretical” context is oversold. There are inconsistencies in the analyses that are difficult to understand, and much of the discussion does not follow from the actual results of the study. The paper would need to be completely revised to address conceptual framing, presentation of results, and interpretation of what the data actually say. Ultimately that is a new manuscript that shifts its focus and conclusion.  
  
GENERAL COMMENTS  
  
Resilience and other terminology (e.g., intensity, severity) are not clearly defined or used consistently, and how resilience is assessed quantitatively (what counts as resilient or not?) And of what to what?  The study does not rigorously test disturbance theory, and is oversold to the detriment of highlighting the interesting and new contributions. Unfortunately, this field study cannot test the supposition that fire frequency can overwhelm the effects of intensity–perhaps the authors might suggest this possibility in the Discussion as a potential new direction to be tested, but it has not been demonstrated. The title states that the paper indicates that disturbances overwhelm variability in local resilience – but how is local resilience quantitatively defined, how is its variability quantified, and how do you know when that variability has been “overwhelmed”?

We appreciate the reviewer’s point. Resilience as a term is fraught, especially when used in papers without taking the time to provide a definition. We have added sentences defining the term ‘resilience’ as used in this study in lines L32-36. We also more carefully define the term “resilience mechanisms” (L34-36).

We also appreciate the reviewer’s point that the initial framing of the study implied that local resiliency would be quantitatively defined, when here we test local resilience qualitatively through the difference between sites.   
  
Care is needed throughout in the use of disturbance intensity (energy release) vs. disturbance severity (effect on the ecosystem). These terms appear to be confused, and while the authors focus on disturbance intensive, the study only addresses burn severity.

We agree with the reviewer’s point and have taken care to use each term intentionally. We’ve replaced “intensity” with “severity” throughout the manuscript where appropriate.   
  
In the introduction, the authors say that there has been little empirical research on the ongoing effects of short-interval events. This is probably true for the system in which this study was conducted, but even for fire, there is a lot of work on short-interval fires, e.g., throughout the southwestern US, in dry conifer forests, in the long leaf pine systems of the southeastern US, Rocky Mountains, etc. Grasslands were mentioned previously by the authors as well, and many papers address effects of fire frequency in grasslands (e.g., all the work from Konza Prairie, for example). It’s important to define how you are defining short intervals (in the context of the system) and realize that in many systems, those short-interval fires are fundamental to system functioning. Whether “short” is a departure or not from historical disturbance regimes varies. The introduction emphasizes variation in disturbance intensity in sequential disturbances, but methods (L138-139) indicate only high-severity, stand-replacing disturbances. The introduction should be revised to eliminate variable disturbance intensities.

We appreciate the reviewer’s point and agree that referring to short-interval fires without specifying system-specific context is misleading. We have added a more explicit distinction between addressing short interval fires more generally (L91-92) and defining short-interval fires specifically in the context of the boreal (L92-94, L96-98).   
  
Except for the plots that had not burned in at least 70 years, all other plots were sampled 15-16 years postfire. That had to be inferred from the site selection, so it might be helpful to state it directly in the methods.

We agree with the reviewer and have added a sentence accordingly (L162).  
  
Some issues regarding the statistical analysis questions should be addressed. Covariance in model predictors, assurances of normal data (or correct distributional models), model selection procedures, and ecological interpretation of effect sizes and directions should be clarified and expanded. Goodness of fit metrics should also be reported (even the “best” model may show a good or poor fit to the data).

We agree with the reviewer’s comment that inadequate information was presented regarding the statistical analysis. We’ve altered the statistical design of the study, and added text and tables to more specifically communicate both statistical analysis decisions and context of results. Specifically, we’ve added sentences in the methods section describing model section procedures (L204), we’ve made explicit our decision to choose certain distributional models (L195-199) and we present goodness of fit metrics in a table in the supplement (Table S5).

Use of a random effect for topographic position for the initial linear models should be justified (why not account for it as a fixed effect, since it was already hypothesized, and then you could compare effect sizes)?

Based off of these revisions and other feedback received, we’ve changed the statistical design of this study and dropped the use of random effects. We appreciate the reviewer’s suggestion that we use topographic position as a fixed effect and have incorporated it into our updated model structure.

The spatial autocorrelation that was detected may still not be accounted for correctly (testing the residuals would be wise, and if there is still spatial autocorrelation, adjusting for it in your models). Spatial autocorrelation will lead to bias in results in which a non-significant effect appears so (so the null hypothesis will be rejected incorrectly).

In revising the statistical design for this paper, an error was caught in how density and basal area were scaled up to hectare estimates. Correcting for that error lead to different spatial autocorrelation results, where no significant spatial autocorrelation occurred between plots. Even so, overdispersion did exist in our models, which we accounted for using negative binomial and gamma distributions.   
  
The Results could be reorganized to match the hypotheses, have fewer subheadings, and state actual results rather than “there are significant differences.” Results appear to be reported sometimes for trees > breast height only (Fig 3 and 4?), sometimes for trees and seedlings separately (Table S4), and sometimes for trees and seedlings combined (Fig 6?). It is unclear when each grouping is reported and, more importantly, why.

We appreciate the reviewers suggestion, and have made the following revisions: we’ve reorganized the result section into three sections that match the research questions (patterns of post-fire regeneration, post-fire soil characteristics, and the interactive effects of fire and topographic position on regeneration), removed excess subheadings and focused on stating actual results. We’ve dropped the distinction between trees and seedlings (especially in Table S4) and clarified with a sentence in the methods section that the results reported are for trees and seedlings combined (L173-174). We think these revisions make the results section much stronger, and appreciate the feedback.

Later on in the paper, discussion of resprouting will be important, because resprouting deciduous species may be more likely to have reached the “tree” threshold at 15 years since fire, potentially resulting in an apparent transition to deciduous dominance when conifers could still emerge as dominant over time based on seedling abundance.

We agree that resprouting is an important discussion. We’ve chosen not to focus on asexual vs sexual reproduction explicitly in this manuscript in order to keep the focus at a broad level, but intend to dive more fully into that subject in a subsequent manuscript. We’ve added a sentence in the discussion that identifies the absence of resprouting in this particular manuscript as a limitation (L334-336).   
  
In the Discussion, the lack of context, placing this study within the published literature addressing similar questions and exploring the mechanisms underpinning the observed responses, was very surprising. There appear to be only two references cited in the entire Discussion, whereas many, many studies have addressed related questions in this or similar ecosystems. This is a serious shortcoming.

We agree and have added the suggested content in several places throughout the discussion (i.e., L283, L291-301)  
  
It would be more impactful if the authors did not start Discussion with a recap of the Introduction. Rather, start with the important inferences drawn from your study, then proceed to interpret the results in the context of your questions or hypotheses. All text from L267-274 can be eliminated, as it is redundant. The sentence on L274-276 is a much better topic sentence with which to lead. From here to the end of that paragraph nicely summarizes why your study is important.

We agree with the reviewer’s suggestion and have eliminated the respective text accordingly to make the discussion both more concise and more specific.   
  
A number of points raised in the Discussion are not supported by the results. E.g., L283-284 – Figure 4 does not support that conifers declined later in the lowland plots – they are essentially zero after one fire in both. The same is true for BA. How are these statements supported when the data indicate otherwise? This point is repeated again on L311-312, again without support in the data.

We’ve revised figure 4, 5 and 6 to better depict our data, and hope these revisions make our results more clear. [does this need more?]  
  
Caution is warranted for concern about “never returned to prefire levels” because sampling was done only 15-16 years after fire! Of course, that is way too soon to expect recovery to levels of forests that are pushing 100 yrs old. The relevant points here are whether there are differences among the burn histories themselves. The forest that did not burn recently is a reference for mature forest, not 15-yr old forests. Please make sure the study is interpreted in context of the time-since-fire.

We agree and have eliminated the sentence in the discussion that referred to organic layers in the context of pre-fire levels.   
  
Overall, there is potential in these data, but there is a mismatch between how the paper is framed, what the data say, and what is concluded.  
  
SPECIFIC DETAILS  
  
L14, as for paper birch, please give Latin name for black spruce at first mention. Make sure abstract is completely self-contained, but then give Latin names again at first use for each species in the main text.  
L19, extra word (“of”)  
L20, either between topographic positions, or with topographic position  
L21, spp. should not be italicized

Thank you for pointing these errors out! We have made the appropriate corrections on L14, L19, L20 and L21 respectively.

L24-25, the abstract concludes that frequency can overwhelm intensity of disturbance but does not provide context nor say how was this tested. As noted previously, the study cannot demonstrate this.

We agree with the reviewer that the sentence in question in the abstract was misleading. We have revised the final two sentences of the abstract to more specifically reflect the conclusions and impact of the study by focusing on the impact of local site conditions, rather than intensity of disturbances (see L21-25 for specific wording).   
  
L28, 41-42, and more. Ecosystem function and functioning is used extensively in reference to resilience, but this study measures structure only and not function. Recommend focusing instead only on structure when introducing resilience, except perhaps to discuss how ecosystem function is related to structure.

We agree with the reviewer’s recommendation. Accordingly, throughout the manuscript, we have revised the language used to refer to broadscale changes to ecosystems to focus primarily on ecosystem structure.

L29-30, “grasslands vs. forests” doesn’t make sense in context of this sentence. Both can be maintained by disturbances.

We appreciate the observation – our point was to provide an example of ecosystems maintained by disturbances, as said by the reviewer. We’ve changed “vs” to “or” (L30).   
  
L 59-60, wording is a bit confusing. Authors state they will look “at 1-3 events in short succession”, but one event in short succession is not possible. Suggest rewording.

Agreed. We’ve reworded the phrase to “1, 2 or 3 events occurring in short-interval” (L63).   
  
L 60-61, authors switch from disturbance intensity to severity, seemingly using them interchangeably. Suggest using severity throughout, since this is what is assessed.

We agree with the reviewer’s suggestion and have revised the manuscript to use the term “severity” throughout when describing disturbance impacts.   
  
L67, careful here, because trends in fire over only 10 years is very short, and trends are varying a lot by region. Plenty of studies are showing increased fire with climate warming. It is also unclear how forest loss is relevant to this study. Suggest omitting that point.

We agree with the reviewer’s point – we had included that text to help build the context of the paper for an Ecology audience, but feel it is not needed, and have eliminated that sentence.   
  
L68, fires do kill trees (when they are high-severity), but they do not necessarily result in a loss of forest. Forest is only lost if forest regeneration fails.

Fair point. We’ve removed that point, as addressed in the comment above.   
  
L82, please be more specific about your definition of short interval. Clarification appears later, but there is a gap between 30-year definition, 50-years in literature, and actual fire return intervals in this study. Please clarify and provide citation or ecological justification.

As mentioned above, we’ve taken care to add a general definition of short-interval fires, as well as a definition specific to the boreal. We appreciate the reviewer’s point that different systems rely on different numeric definitions and have added a sentence mentioning that point specifically within the text (L91-94).

L85-86, are these the best references? I might expect studies that specifically study the role of dispersal distance versus post-fire establishment. Do McCaughey and Burns & Honkala assess this, or do they only state that species differ in their dispersal distances?

We have now added additional references. Overall, though, these are the standardized references for species-specific dispersal with specific distances for each. Establishment at long dispersal distances in burn areas can be less effective than potential dispersal if conditions are harsh, but that’s not what we’re addressing here, which is potential.

L90-91, if “research suggests 50 years or more are required for full aerial seedbank regeneration”, it seems a more appropriate definition for short-interval fire would be <50 years.

Agreed. We’d fallen into the trap of defining short-interval fires based on the intervals between fires in our study but have changed the definition based on the mechanisms present in the system (L97-98).   
  
L93, here is an example of mis-use of fire intensity. Consumption of soil organic matter is important in these systems, but this is a measure of burn severity, not intensity. They are correlated, but should not be used synonymously.

That is fair, the writing was a bit too informal. Changed to severity.  
  
L100-105, hypothesis that short-interval fires will operate this way has already been demonstrated by Johnstone et al., as cited here, and the mechanisms are known. What here will be new, rather than confirmatory? The next paragraph goes into this – so by “emerging community”, you mean the deciduous species? Please clarify. It would help to have your questions and hypotheses all at the end of the introduction.

That is true, this hypothesis has been supported by Johnstone’s work; however, there are several valuable and new things here. First, it never hurts to confirm in new systems. Second, we are working in reburned deciduous stands as well, which have never been investigated. Third, we’re contrasting across landscape settings, which is also essentially new.   
  
L119-131, no questions or objectives stated here, and it is hard to understand hypotheses without questions. Also, instead of just “continued change”, is there a more explicit expected result?

We expected continued forest loss, but as mentioned above, this is the first time people have investigated 3 short interval burns in succession. So explicit expectations are hard to come by.  
  
L121, ‘in’ between frequency and boreal. Lowercase ‘interior’.

Thank you for pointing this out, we’ve made the appropriate change.   
  
L132, 149-158, suggest starting methods section with description of study area, including climatic and other differences between upland and lowland sites. Include typical differences in fire severity or post-fire seedbed conditions. Authors note that upland v. lowland are climatically similar, but are those differences in precipitation (Table S2) negligible?

We’ve moved those paragraphs around accordingly. The point was investigating black spruce dominated landscapes, we assume that climate (averaged over the time since last fire) is not playing a role in the differences in regeneration between the sites. We also focus on the trends across fires in each location individually, so variance on the base rate between sites is less significant than the similarities in the trends between sites.  
  
L140., based on Table S1, final burn occurred in 2003 and 2006 as well. So final burn in 2003-2006?

Thank you for catching – we’ve changed the text accordingly (L153).

Line 142, a single fire can be “short-interval”?

We agree that that wording was confusing: we’ve changed to “recent (<50 years)” (L155).   
  
L150-151, so plots could be 50 m away within the same fire? That seems close for those systems.

We agree with the reviewer that 50 m is close for this system. We went back and checked the actual distance between plots and only 2 plots are within 70 meters of one another. The rest are several kilometers apart on average. We’ve changed the language to reflect this (L142-143).

L171, was relative proportion done by density or basal area? Did you consider using an Importance Value, which ranges from 0-2 and sums relative density and relative BA? That is commonly used in forest ecology and could be a useful integrated measure in your study.

Relative proportion was calculated based on the number of stems of a species present divided by the number of stems of all species present. We’ve added a sentence to clarify that (L177). We have not planned on using an Importance Value in this particular manuscript, though we agree with the reviewer that they can be a useful measure.   
  
L173, well, tree density and BA are both components of stand structure, among other features. I would call each of these three responses components of stand structure.

Agreed. We’ve changed the text to better reflect our particular motivations for using each metric within this manuscript (L179-181).  
  
L187, were data tested for normality prior to analysis? Were any transforms required? Density data are notoriously non-normal, and often require a square-root or log transform when used in linear models. Alternatively, a different data distribution could be assumed in the model. Please report whether data met model assumptions.

We agree with the reviewer’s suggestion to report our decisions behind distributions more explicitly. We’ve added text to explain how we selected the data distribution we used in our models in L196-200. In addition, we altered our study design to use generalized linear models. Therefore, we use negative binomial regression to model density data, which the reviewer correctly points out can be notoriously overdispersed.   
  
L190, is this correct use of random effects? There is a hypothesized ecological effect of upload/lowland, so it shouldn’t be a random effect. It seems that the authors should compare effect sizes of fires and landscape position.

We’ve dropped the random effect approach, and instead compare the effect sizes of fires and landscape position directly, as suggested. This is an excellent suggestion; it makes the results much more straightforward.

L193, not sure what you mean by hierarchically tested. This use of random effect (to account for site) makes sense here, however, as you do not hypothesize any meaningful variation based on the fire alone.

This text has been eliminated through revisions to our analysis. [is this a cop-out?]  
  
L195, did you test for covariance among predictors?

Covariance was detected among topographic position and variables like elevation and solar radiation, since a majority of variation in those topographic variables occurred between sites. We’ve dropped both variables from our statistical approach and explained our decision in a sentence on L204-205.  
  
L203, unclear how model selection was actually performed. What do you mean by hierarchically test? Did you retain only the top model was on AIC? What about equally supported models?

We’ve added a sentence in the methods section to clarify how model selection was performed (L204).

L204, suggest reporting R version, rather than R Studio version.

We’ve incorporated the reviewer’s suggestion (L209).  
  
L205, please report standard errors instead of standard deviations, because your sample sizes vary among groups.

We’ve incorporated this suggestion throughout the manuscript and we’ve added a sentence indicating accordingly in the methods section (L210).   
  
L207-208, good to see this considered, but how to address it might need further exploration. With separation distances of only 50 m, data are likely to be spatially autocorrelated. How does a random effect for topographic position account for autocorrelation among plots? If there is spatial correlation among lowland plots, then the random effect will not alleviate this issue. Have you tested the model residuals? You may need to adjust your models to account for the autocorrelation.

As mentioned above, an error was caught in the data while revising the analysis that eliminated significant autocorrelation between plots.   
  
L213-214, instead of just saying “significantly higher”, please avoid using “significantly” (because if you have already told us what alpha level you are using, we know that any difference is statistically significant), and say something about the magnitude of the difference . In other words, emphasize the ecological dimensions. In Figure 3, it looks like conifer BA is nearly double in uplands vs. lowlands, and densities are maybe 80% greater. Deciduous species were largely absent from the lowlands.

We appreciate the reviewer’s point, and have revised how we reported our results to focus more on the ecological dimensions, rather than the significance.   
  
L216-217, and elsewhere. Please use the SE instead of the SD because n varies. With a SE, the reader can easily double it to estimate a 95% CI.

We appreciate the reviewer’s suggestion and have incorporated it throughput the manuscript accordingly (see comment above).   
  
L218, Figure 4 does not show a decline in tree density overall; rather, it shows the density of conifers and deciduous stems with 1, 2 or 3 fires.

We agree with the reviewer’s observation and have altered the sentence accordingly (L221-222). We’ve also moved the reference to Figure 4 to the end of the next sentence, where it is more appropriate.   
  
L219, “pattern of pairwise differences” is awkward, and that phrase is not even needed. Begin, “Density of deciduous species increased to about xx stems/ha after two sequential fires in upland plots and after three successive fires in lowland plots (Figure 4). Note that from Figure 4, it also appears that density is essentially zero for conifers in all burned plots (it’s not possible to tell what that line indicates – but it seems that conifers are not recovering in any plots.

We appreciate the recommendation and have eliminated the offending phrase. We’ve also altered Figure 4 to be more readable by dropping the Dunn’s multiple pairwise comparison test, correcting a scaling issue with the data and plotting in more condensed graphs.

L225, well of course it can’t recover to prefire conditions in only 15 years, please remove this point. The key in your paper is the difference, and postfire years 15-16, among plots that had 1, 2 or 3 fires.  Also, please tell us the units…how much BA? How much different? Also, Figure 6 appears to be cited ahead of Figure 5.

We’ve removed the point about pre-fire levels, and have revised how we report the results by specifying the difference (in factors) between reburn history. Thank you for pointing out the error in figure citations! We’ve corrected accordingly.   
  
L228, “presence” should probably be “proportion”

We’ve taken that subheading out to condense and simplify the results section.   
  
L229-235, these species-level results are interesting, as this is where you are finding some intriguing results that address the interaction between the fire frequency and the abiotic template (landscape position). The community is shifting to deciduous dominance when at least 2 fires take out the conifers, but then the species that come in depend on their suitability for different environmental conditions. However, it is also unclear whether this is trees, or seedlings, or trees + seedlings. Once-burned plots still appear to be dominated by black spruce based on Fig 6 and when considering seedlings in Table S4. Is “canopies” being used to refer to trees only rather than all stems? I would suggest consistently grouping trees + seedlings together throughout this study, unless there is a particular reason to break them out and examine them separately.

We appreciate the reviewer’s suggestion, and have incorporated it throughout the manuscript. The difference between tree and seedling regeneration within this study is interesting, but we felt it did not contribute specifically to the narrative of this particular manuscript. As mentioned, we’ve added a sentence in the methods section to indicate accordingly (L173-174).  
  
L233-235, L267-74, L281-283 are examples of redundancies that do not need to be restated.

We agree and have removed those sentences accordingly.   
  
L255-256, but there really isn’t conifer BA to work with (see Figure 5), so why would you attempt to explain it? Deciduous BA maybe…

We agree with the reviewer’s point that conifer basal area in this study was too low to model effectively. We’ve removed that portion of the analysis accordingly, and added a sentence explaining our decision in L199.

L269-271, not clear how resilience was assessed for either the original or for the new forest communities recovering after a single reburn. Also the use of resilient and resistant is an issue (terms were never clearly defined), and there is no demonstrated depth of understanding/testing of resilience concepts.

We agree with the reviewer’s concerns about the use of the terms resilience/resilient. As mentioned, we’ve added a definition early in the introduction (L32-36). One challenge in this study is that we evaluate supposed resilience more qualitatively (via the differences between the two sites) than quantitively. [Sure this needs more, not sure where else to go with it]  
  
L286, be careful – you have not demonstrated any kind of critical threshold in this study. I would avoid that term, or test for it rigorously. Suggest omitting.

We’ve omitted that point.   
  
L299-300, authors note that tree density and basal area were consistently higher in upland versus lowland but it’s not clear where and how this was assessed other than in unburned plots. This is difficult for the reader to assess with the information, figures, and tables presented in this paper.

We’ve revised the figures and information presented in the paper in an effort to make our results more clear. [Same concern in responding to this one as I mentioned above]  
  
L300-301, were “pre-fire levels” measured in burned plots, or should this refer instead to unburned reference stands? Is there an expectation that density and basal area should return to pre-fire levels in 15 years?  
  
L308, “tree occupation”, implying presence/absence, did not seem explicitly assessed in this paper.

Presence of species was evaluated in Table S4, and we agree with the reviewer that the use of “tree occupation” obscures the work done. We’ve changed the verbiage to “presence” here and elsewhere to be more cohesive.   
  
L309-311, it is not clear what information supports this statement about earlier black spruce decline in uplands.  
  
L316-335, in this paragraph specifically but throughout the Discussion, readers would expect to see a lot more literature cited to place the authors’ findings in the context of current understanding. What is already known versus what is a new finding of this study?

We appreciate the reviewer’s suggestion. We’ve tried to address it in two ways: we’ve added more references in sentences that required them to begin with (i.e., L283), and we’ve added text in the discussion that more explicitly places the results of this manuscript in context with existing work (L290-306).   
  
L339, high frequency fire?

Thank you for catching this! We’ve corrected the typo (L365).   
  
Literature cited needs to be reviewed for typos, order, inconsistent citation style, etc. It appears not to have been carefully proofread.

We’ve proofread and corrected for typos.

L515. Typo, should be m2/ha for basal area.

Typo corrected.

Table 1 and 2 captions: Ha should not be capitalized.

Typo corrected.

Table 2, but how good were the models? “Best fitting” doesn’t give information on how well they explain the data. Please provide R2 values or other goodness of fit metric.

We’ve included AIC values, root mean square error values and null and residual deviance in Table S5.   
  
Table 3, consider removing insignificant terms from the table, as they are not interpreted and have no compelling ecological explanation. In caption, add superscript for m2.

This table was eliminated through revisions to statistical design.   
  
Figure 3, should be m2/ha for basal area.

Typo corrected.

Figures 4-5, a number was mis-cited in the text. Note that these could be combined into a single figure, as they are variations on measures of stand structure.

Good catch! Typo corrected.

Figure 6 – These plots should technically not be line graphs, but rather bar graphs, because you are testing categorical differences. Error bars should not be displayed by using the standard deviation, but instead should be using standard error. If you use two SE, you can say that these are 95% CIs. It is also unclear how the proportions were calculated (see above). It would be useful to include numbers somewhere for what the totals are, as well. One challenge is that 2 of 4 trees is 50% dominance, whereas 1000 or 4000 trees is only 25%, but these are very very different conditions. The actual values were not presented (or perhaps were missed in the SI).

We appreciate the reviewer’s suggestions to improve the figure. We’ve changed the figure to a bar graph of presence of species between the two sites. We’ve included a phrase explaining how we calculated proportion both in the methods section (L177) and in the caption of the figure (L560-561). The data for Figure 6 was presented in Supplemental Table 4, we apologize if that was unclear. We’ve added a sentence into the figure caption with the location of the actual values (L564).

Figure 7, measuring soil consumption 15-16 years after fire seems questionable. I suggest deleting that variable from the manuscript completely. It does not vary, which is not surprising. Depth of the organic layer and percent cover of exposed mineral soil are sufficient, and much more appropriate to report at this time postfire.

We agree with the reviewer’s suggestion. We’ve moved the variable to the supplement section.

Table S2, why not use metric units here? Need to report units also for temp (is this degrees F?).

We’ve changed accordingly.

Table S4, recommend highlighting which species are conifer versus deciduous. Also, recommend spelling out genus, especially because there are multiple “P”s that aren’t all the same.

We’ve made the changes accordingly.   
  
Table S5, what do “n1” and “n2” mean here? Why isn’t “n1” for upland conifer density 2 vs 3 the same as “n2” for upland conifer density 1 vs 2?  
Table S5, upland conifer basal area, 1 vs 2 has a p-value of 0.17 (not significant) – but in the table it is asserted that this is significant at p < 0.05.

Table S5 has been updated with different values (model selection parameters) after revising the statistical design of the manuscript.

Table S6, please define abbreviations for terms. In general, remember that all tables and figures must be completely self-contained, so that a reader can interpret them without reference to the main text.

Table S6 was removed during revisions to the analysis.   
  
  
**Reviewer 2**  
  
Comments to the Author  
I know of no other study that shows what happens to tree species as you get three quick fires in a row; and we have only a handful of studies for two in a row. Further, they show that each successive fire is about equally devastating for black spruce. This should certainly be published.

We appreciate the reviewer’s support!

Below I carp about details regarding processes and species’ traits; in particular there is not enough detail. I understand however the dilemma of the authors: this journal wants to see broad arguments that transcend a given biome and the particular traits of a few species. And so they wrote it that way.

We thank you for these detailed comments regarding species-specific traits. The reason many were omitted or only briefly discussed was exactly as said: to stay at a high functional, generalizable level per the style of the journal. Given that this is now being submitted to Ecosphere, we’ve now included a more explicit discussion of species-specific traits, particularly in the introduction section where we now address dispersal distances of species like aspen and black spruce more specifically (I.E., L79-80).

At no point do they try to show how the probability of a burn declines as you entertain 1, 2 or n successive burns within t years. Why not use the negative exponential? It has only the single parameter, mean return time.  This is precisely the sort of general approach that would make the editor happier.

With these data, we cannot discuss the probability of burning. Separate work on probability is in progress but has been halted by COVID.   
  
Line 49. One of those empty statements ecologists love.

That’s fair. We’ve rephrased to make the paragraph more concise and more specific (L50-51).

Line 80. No. it replaces within 5 years; the cones are empty by that point (the Greene et al paper—which they do not cite-- about seed abscission of black spruce and jack pine)

We appreciate the reviewer’s correction and have corrected the text accordingly by adding “5-10 years” in L72. We’ve also added the Greene et al. 2013 citation in this context (L72-73).

Line 85. It is not said explicitly but implied that black spruce has relatively poor dispersal capacity. Compared to what? It has the lowest recorded terminal velocity of any conifer. Yes aspen and possibly paper birch might travel father due to even lower terminal velocities, but certainly not jack pine or white spruce. They should be adding text about asexual reproduction here.

We appreciate the reviewer’s point that dispersal distances of the species in this study were not stated outright. We’ve added species-specific dispersal distances into the text (L79-80). We agree with the reviewer’s point here and below that asexual reproduction is an important factor in this system and this study, but do not feel it fits within the scope of this manuscript. To acknowledge that, we’ve added a sentence in the discussion addressing it as a limitation in this particular manuscript (L334-335). We hope to investigate asexual reproduction more explicitly in future work.

Line 90. Clearly this depends on growth rate which in turn will depend on local site conditions, altitude, latitude etc. so say: this 30 year limit is for our area

We agree. We’ve added language to clarify that our definition of short-interval fires is specific to the region investigated in this manuscript (“within this region of the boreal”, L96-97).

Line 95 and the entire paragraph. They miss the point about the organic layer. All moss layers make dreadful seedbeds for small-seeded species except for Sphagnum because it has the quasi-vascular structure that permits the surface to remain at least somewhat wet during a long rainless interval. Thus it is a barely acceptable suitable seedbed for any small seeded species, not just black spruce. That we do not find birch and aspen on it tells us more about their requirements for subsequent growth vs-a-vis such a nutrient poor substrate than about germination requirements. Likewise, all three species will have their best germination rates on mineral soil.

As for organic layers being completely consumed by smoldering combustion. . . these must be upland sites where indeed organic layers as great as 30 cm can be pruned down to mineral soil. They can consult their own Figure 7. Even after three successive fires the percentage of lowland mineral soil is still trivial. And the absolute amount of organic material remaining after three fires is still too great for aspen germination (check out Hesketh et al, which they do not cite)

Summing up, black spruce occurs on both site types (boggy Sphagnum; upland feathermoss); the distinction is important.

[Not sure how to respond to this section, starting with “Line 95”]  
  
--a major lack here is a serious discussion of the asexual reproduction of birch and aspen. This is what is allowing them to deal so well with successive fires. By contrast, the brief discussion of the recruitment strategy of these two species focused solely on their recruitment by seed. (By contrast, the asexual layering of black spruce is useless for responding to a fire. . .)

Thank you for pointing this out. Although we agree that this is an important discussion, we felt it was beyond the scope of this particular manuscript and would be better addressed in a separate analysis taking species traits more strongly into consideration than we have done here. As mentioned above, we’ve added text in the discussion that points this out as a limitation of this particular study (L334-335).

Figure 6. the decline remarkably linear. Whether upland or bog, each successive fire reduces black spruce proportion by about 35%.

[Does this require a response?]

--at some point they need to talk about age to first cone production and not just age to producing a large aerial seedbank. If black spruce can have just a little bit of recruitment it could potentially infill as the Sphagnum aggrades—if they can go awhile without fire.

[Not sure how to respond to this either]  
  
--The problem with choosing black spruce:  
It is now clear that black spruce (unlike aspen, birch, or pine) is doing badly after fire much of the time everywhere in the North American boreal. I am part of a paper just submitted by about 25 authors, all of whom have contributed their post-fire data sets on black spruce regeneration. (I can send them a copy of it if they want, but many of these data sets have already been published. . .). It will not undercut their argument to say that the dramatic declines they have seen in their study are accelerating a decline that is general (but far less dramatic).

We agree with the reviewer’s point and would appreciate a copy of the paper, if available.