**Associate Editor:** We have now received the reviews from both reviewers of this manuscript. These are annotated below. Both reviewers were quite positive and provided detailed and seemingly useful reviews. Understanding the spatial patterns of burn severity is interesting and very important in western forests, and would hold significant interest for the readers of the journal. It is also quite well written and has clear conceptual value. I agree with the majority of the reviewers' comments, and the paper is likely to be suitable for publication in Landscape Ecology if the authors are willing to complete relatively substantial revisions.

We thank both reviewers and the Associate Editor for overall positive comment. We also appreciate the time spent on the review and attention to detail therein.

The first reviewer provided very helpful and detailed comments. In particular, I find that the readers of the journal would first think of classic landscape configuration metrics when they read the paper, and perhaps wonder what the methodology of this paper provides that 30-year-old metrics don't already provide. Bill Romme's classic work in Yellowstone first used these metrics to describe the landscape mosaic, and the freeware FRAGSTATS makes the calculation of those metrics very simple. This is not to say that the authors should abandon the foundation of their paper; instead, I agree with Reviewer 1 that a better discussion and analysis is needed to suggest that the new metric has utility over and above what one could garner from FRAGSTATS. Also, it is not clear this work might be useful for a historical landscape (i.e., one that results from a mosaic of fires over several centuries), and the paper should therefore be re-cast as one with an emphasis on recent fires.

There are a couple good suggestions here. In response to the first one (comparisons to existing landscape configuration metrics) we compared our stand-replacing decay coefficient (SDC) to two metrics of patch complexity commonly used in the FRAGSTATS software package to. Specifically, we calculated the area-weighted mean shape index (AWMSI) and the area-weighted mean patch fractal dimension (AWMPFD). We chose these two metrics because they provide related information on patch characteristics while remaining fairly insensitive to the spatial grain or extent of the landscape. We found a correlation between SDC and AWMSI (on a log scale), but not between SDC and AWMPFD. Both of these relationships are shown in a new figure in the supplemental material (Figure S2). Despite the overall correlation, the relationship between SDC and AWMSI is less consistent for more simply-shaped (lower AWMSI) patches. Based on this we argue in the text: “although there is some overlap between SDC and existing patch complexity metrics, SDC appears to better differentiate ecologically relevant patterns of fire severity as they relate to tree regeneration following stand-replacing fire.” This is all explained in a new paragraph in the “Applications of this approach” section (now the second paragraph of the section).

In response to the second suggestion (how relevant is our approach to historical landscapes) we reframed Introduction by removing language related to assessing departure from historical conditions and uncertainty in these assessments. We retained some language on characterizing historical fire regimes because we felt it was relevant for laying out the purpose and need of this work.

Reviewer also presents a good point about the impact of scale on landscape metrics, including references, that the authors should address.

This is an important consideration, but we do not believe that the SDC vs. fire size and SDC vs. proportion stand-replacing relationships are indicative of scale dependence in the SDC metric. We added the following sentences in the first paragraph of the “Applications…” section:

“It is possible to interpret this inverse relationship between fire size/percent stand-replacing and SDC as simple scale dependence in the SDC metric (Wu et al. 2002). However, the fact that both of these variables tend to be positively associated with stand-replacing patch size (Miller et al. 2009; Harvey et al. 2016) suggests that SDC is capturing a real phenomenon (distance to edge) that is affected by the scale of stand-replacing effects and is not an artifact of scale dependence.”

The second reviewer was also positive about the paper, and was more concerned about terminology and more minor points, suggesting clarifications about how fire terminology (such as "stand-replacing", and "mixed-severity", as well as "intensity" vs. "severity") is defined throughout the paper. The authors should carefully consider their use and clarity of terminology throughout. The reviewer also suggested a acknowledgement of serotiny in lodgepole pine in their discussion of seed dispersal, which I also find to be an important omission that the authors should address.

In the Introduction and in the “Scale and “percent stand-replacing” section we added language to distinguish between “mixed-severity fires” and “mixed-severity fire regimes”, as suggested by Reviewer #2. This included revising the definition of “mixed-severity fires”.

Regarding the point on serotiny in lodgepole pine, we added a sentence explaining that the effect of patch size on tree regeneration may not be evident in forests with high levels of serotiny. This new sentences is at the end of the second paragraph of the “Scale and percent stand-replacing” section.

Should the authors choose to revise the manuscript, they should include detailed and thorough responses to each comment made by both reviewers, as the level and adequacy of these responses will largely determine whether the authors have been able to address those concerns well enough for publication. Particular emphasis in revision and the author's responses should be placed on the comparison to classical landscape metrics, which is the most intensive revision of those suggested.

Done.

**Reviewer 1**: This concise and well-written short research communication outlines a new approach for quantifying the spatial pattern of fire severity impacts in burned areas. The authors appropriately test the approach on both artificial landscapes and on two actual wildfires that differ fundamentally in configuration of burn severity patches. Whether or not the new metric will ever see wide utility, the paper has conceptual value for pointing out the importance of spatial heterogeneity in burn severity and using this information to better understand and anticipate post-fire response.

We appreciate the overall positive comments, as well as the effort spent on this review. Incorporating the suggested changes and addressing the concerns identified undoubtedly improved our manuscript.

The paper could perhaps be improved, or made of greater interest to a broader readership of this journal, if it compared the method developed here with other methods that have already been developed by landscape ecologists for quantifying landscape configuration (i.e. the large set of existing landscape metrics, including metrics of shape complexity, fractal dimension, contagion, aggregation, lacunarity, etc.). What additional information is provided compared to simpler metrics, or general landscape metrics already in common use?

This is a very good point. As stated previously, we compared our stand-replacing decay coefficient (SDC) to two metrics of patch complexity commonly used in the FRAGSTATS software package to. Specifically, we calculated the area-weighted mean shape index (AWMSI) and the area-weighted mean patch fractal dimension (AWMPFD). We chose these two metrics because they provide related information on patch characteristics while remaining fairly insensitive to the spatial grain or extent of the landscape. We found a correlation between SDC and AWMSI (on a log scale), but not between SDC and AWMPFD. Both of these relationships are shown in a new figure in the supplemental material (Figure S2). Despite the overall correlation, the relationship between SDC and AWMSI is less consistent for more simply-shaped (lower AWMSI) patches. Based on this we argue in the text: “although there is some overlap between SDC and existing patch complexity metrics, SDC appears to better differentiate ecologically relevant patterns of fire severity as they relate to tree regeneration following stand-replacing fire.” This is all explained in a new paragraph in the “Applications of this approach” section (now the second paragraph of the section).

Along these same lines, simply comparing some summary statistics of the patch size distributions among the two wildfires (Lines 127 - 130) appears to do a good job of describing how they are different. Why is the SDC metric needed? The authors could go into greater depth as to how their approach sheds new light on the spatial structure of high-severity burn patches within a wildfire, compared to other approaches.

Another good point. Based on this we added the following justification:

“we sought to develop a more robust method for characterizing spatial distributions of stand-replacing patch area. Our intent was to derive a quantitative measure of these distributions that did not rely on binning data in to patch size classes (Figure 1) or distance-to-patch edge classes (Figure S1), to allow for robust comparisons between individual fires or sets of fires.” This was added to the beginning of first paragraph of the “Alternate characterization of fire effects” section.

Additionally, we already had the following sentence:

“The concept of “core patch area” is one approach that can address this. However, core patch area is a binary classification that depends on a single distance threshold.” This appears later in the same paragraph.

In the Introduction, the authors seek to place their metric in a broader context of comparing contemporary with historical fire regimes. However, the context provided seems not to fit the new metric, which relies on wall-to-wall mapping of burn severity such as is not generally available for presettlement fires. It would seem that the approach presented here cannot readily be applied to fire history reconstructions that rely mainly on dendro methods, and that relate to time periods for which remote sensing products are unavailable. So if the new approach can only be applied to relatively recent fires, is it appropriate to place the paper in this (historical fire regime) context? I suggest instead placing the paper in the context of understanding and predicting ecological responses to contemporary fires, where the configuration of high-severity patches is of great importance for reasons mentioned in the manuscript.

We agree with the points here. In response, we reframed Introduction by removing language related to assessing departure from historical conditions and uncertainty in these assessments. We retained some language on characterizing historical fire regimes because we felt it was relevant for laying out the purpose and need of this work.

There is interesting discussion of the importance of scale dependence in the distribution of patch severities (Lines 83 - 101; Lines 213 - 227; Figure 5). However, this scale dependence has the potential to confound interpretations of the SDC, because its value can vary consistently with fire extent (as the authors demonstrate). Thus fires of different sizes may not be comparable. The landscape ecology literature is replete with papers discussing scale dependence of landscape metrics and some reference can be made to those. Examples:

Restating our response to the AE’s comments:

This is an important consideration, but we do not believe that the SDC vs. fire size and SDC vs. proportion stand-replacing relationships are indicative of scale dependence in the SDC metric. The fact that larger fires tend to have smaller SDC values is reflective of a real process, namely that larger fires have more area that can be farther from the edge of a stand-replacing patch. Thus the fact that SDC varies with fire size and percent severity is a strength of the metric – it is unlikely that small fires would have very low SDC values. However because the correlations in Figure 5 are imperfect, SDC can distinguish among similarly-sized fires that have larger or more round patches stand-replacing patches and those that have smaller or more irregular-shaped patches, which is the greatest strength of this metric. We added the following sentences in the first paragraph of the “Applications…” section:

“It is possible to interpret this inverse relationship between fire size/percent stand-replacing and SDC as simple scale dependence in the SDC metric (Wu et al. 2002). However, the fact that both of these variables tend to be positively associated with stand-replacing patch size (Miller et al. 2009; Harvey et al. 2016) suggests that SDC is capturing a real phenomenon (distance to edge) that is affected by the scale of stand-replacing effects and is not an artifact of scale dependence.”

Wu, J., 2004. Effects of changing scale on landscape pattern analysis: scaling relations. Landscape ecology, 19(2), pp.125-138.

Wu, J., Shen, W., Sun, W. and Tueller, P.T., 2002. Empirical patterns of the effects of changing scale on landscape metrics. Landscape Ecology, 17(8), pp.761-782.

Shen, W., Darrel Jenerette, G., Wu, J. and H Gardner, R., 2004. Evaluating empirical scaling relations of pattern metrics with simulated landscapes. Ecography, 27(4), pp.459-469.

We incorporated just one of these, Wu et al. (2002), to maintain the Short Communications length requirements.

This may be a minor point, but the methods and existing sources of burn severity mapping have well-documented limitations that the authors should briefly refer the reader to. How "real" are these patterns identified by simple normalized ratios of spectral reflectance band-widths? Surely there are some caveats that are worth bringing up, briefly.

We added the following sentence to the end of the third paragraph of the “Applications…” section.

Some more specific comments and edits:

Line 24: "fire effects" seems too broad a term relative to burn severity, which is what is actually mapped.

We replaced with “stand-replacing patches”.

Lines 28-29: The authors make a convincing case that their approach distinguishes among different spatial configurations, but does it do so in more useful ways than other, existing and widely used descriptors of spatial configuration (e.g. indices of contagion, aggregation, fractal dimension, the various Fragstats metrics, etc.)?

We added this to the sentence: “and does so uniquely from commonly used descriptors of spatial configuration”.

Line 148: not clear what "at patch sizes" refers to - grammatical error?

We fixed this, thanks.

Line 154: It is not immediately clear why the SDC approach is more "process-based" than (for example) a simple quantification of the patch size distributions in different severity classes.

We added text early in this paragraph to provide rationale for our approach.

**Reviewer 2**: The authors provide a rather novel approach for assessing spatial properties of fire severity and fire effects, and have done a nice job justifying the need for such a new approach. Collectively, the authors are all highly-regarded in their respective fields, and I appreciate the obvious synergy that emerged from this collaboration. I have only a few minor comments, mostly for clarification, to suggest.

We appreciate the positive comments. It’s very nice to get this type of response.

General Comments:

Better define what is meant by "dry conifer forests".

We removed mention of “dry” forest and changed to “conifer-dominated” forests. We did this to have broader applicability. And, since we added the caveat about serotiny (as suggested), we feel these concepts are applicable more broadly.

Seems to be a difference in mixed-severity "fires" and mixed-severity "fire regimes"; please reconcile. Crown fires are typically stand-replacing, but mixed-severity fires only kill 20-70% of canopy, according to the definition provided by authors.

Good point. In the Introduction and in the “Scale and “percent stand-replacing” section we added language to distinguish between “mixed-severity fires” and “mixed-severity fire regimes”. This included revising the definition of “mixed-severity fires”.

Line 77 - the idea of stand-replacing fires is first mentioned, although does not fit into the definition of mixed-severity fires, as provided by the authors.

As stated, we revised this definition.

Line 86 - I understand where this is going, but not sure the "small" spatial scale example is the best one. For example, many patches of burned area are quite small, yet can be entirely stand-replacing. I like the tree example, but not sure the point is being made as best as can be here. Consider evaluating this.

This point is somewhat related to the two previous points. Based on all three of these comments, we revised this portion of the text quite a bit to clarify these issues.

Line 95 - Should the authors consider the occurrence of serotiny in lodgepole pine as part of this discussion of seed dispersal? I would suggest they should.

Regarding the point on serotiny in lodgepole pine, we added a sentence explaining that the effect of patch size on tree regeneration may not be evident in forests with high levels of serotiny. This new sentences is at the end of the second paragraph of the Scale and “percent stand-replacing” section.

Line 200 - How were the observed proportions of stand-replacing areas calculated, then compared to predicted? Unclear.

We added added a sentence to more clearly describe the SDC estimation process (L181-183).

Line 209 - Why are large burned patches expected to burn at high intensity in subsequent fires? What is the ecological reasoning for this?

We added clarification to this sentence. This is the last sentence in the “Alternate characterization of fire effects” section.