COMMENTS FOR THE AUTHOR:

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.

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.

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

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.

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.

Sincerely,

Daniel M. Kashian

Wayne State University

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.

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?

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.

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.

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:

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.

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.

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.

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.)?

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

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.

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.

General Comments:

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

\* 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.

\* 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.

\* 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.

\* 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.

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

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