

I was pleased to review the revision of this manuscript. In most cases, I feel that the authors responded to my suggestions and answered my comments. I also understand now why the authors did not include analysis of the data including the chronosequence. The points below are where I think additional information is still required.

Line 79-80: Post fire soil moisture is more complex than you've described in the introduction. I don't feel that referencing one paper is sufficient. Most of what I've seen in the literature suggests that soil moisture is typically higher at burned sites than at unburned sites as a result of reduced evapotranspiration.

Line 80-83: The addition of this sentence does not address my previous comment "A relatively large amount of organic layer remains after the fires at all of the site (minimum 20 cm). I think it's important to note this somewhere in the paper, as this minimizes post-fire changes (less active layer thickening and ground temperature increase) than if, for example, less than 5 cm remains." It's not about the vegetation regeneration, rather it's about the antecedent organic layer thickness and what remains after the fire. More context is needed.

Line 94-102: This doesn't fully address my comment. My concern is that you describe permafrost and post-fire impacts very generally throughout the introduction and rest of the manuscript, without providing context that your sites are in a particular type of environment. It affects your results and conclusions. You can't make broad conclusions for all types of environments based on a certain subset of sites. I think more context needs to be provided in the introduction to put your sites in context.

Line 217: You provided more information in your response to my original comment and it would be helpful to add some to the text in this section. For your reference here is what I'm referring to from the original review:

*Line 201-203: Why were these thresholds chosen?*

*Response: This is the common method of international fire severity division, and it is also a standard means of division. According to the Cocke et al., (2005) and Roy et al., (2006), the dNBR optimality values for these average changes are 0.241 for grass and 0.57 for shrub. Therefore, these values are selected as threshold values through the classification of fire severity by vegetation burn status and the comparison with dNBR (Key and Benson, 2006; Escuin et al., 2008).*

Line 292: COVID-19 epidemic should be "pandemic".

Line 459: The addition of this text does not address my original comment fully. The original comment was "Here you say SMC is decreasing, but you only have one measurement in time. How can you say it is decreasing? You haven't described the chronosequence at all in the results, so I don't think it's fair to conclude this." I realize now why you did not describe the chronosequence (editor's request), but it still makes understanding the conclusion difficult for the reader.