

## ***Interactive comment on “Exploring the sensitivity of soil carbon dynamics to climate change, fire disturbance and permafrost thaw in a black spruce ecosystem” by J. A. O’Donnell et al.***

**Anonymous Referee #2**

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General comments:

This manuscript addresses an important and relevant topic given the importance of northern soil carbon pools in the global carbon cycle. It was a well-written manuscript, with a clear and thorough introduction. The authors use the combination of modeling with fieldwork to explore future scenarios under various climate projections. My main concerns with this manuscript are in the field experimental design, and the assumptions that are made via this experimental design. In addition, I felt that there were too many figures, and a clearer idea of the importance of each figure would clean up the manuscript considerably.

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Specific comments follow: Pg8856 lines 16-18: Based on your abstract, it seems that the main objective was to test the sensitive of soil OC storage to a suite of individual climate factors and fire severity. Then here in your introduction you state that you are looking at the RELATIVE sensitivity of ADL and soil climate effects. Are you using changes in ADL as a surrogate for fire severity? As a reviewer, I am left feeling confused about what the true objective of this manuscript is based on the discrepancies in your abstract and then your introduction. I think you would be hard-pressed to attribute all differences in ADL to differences in fire severity. Therefore, is this paper interested in fire severity or ADL or both?

8860 section 2.3 This section of the manuscript is very weak and needs to be reworked extensively. Please add more detail to your study area. A map, lat/long, climate description. You give no indication of where the field measurements take place, and although you reference another manuscript (by the same authors), I shouldn't have to read that reference to know where your study area is and the basic climate, vegetation, soils of that area. In addition, there is no mention of how other potential driving variables in terms of soil carbon accumulation (and other soil properties such as organic horizon depth, soil thermal dynamics, active layer etc. etc.) were either accounted for or held constant. For instance topography, understory vegetation composition (which will be different in different aged stands), stand density, etc. This is a critical and weak aspect of this manuscript. 8863 Section 2.5 I found this section fairly confusing to follow. I would encourage the authors to reread this section from a non-modeling perspective and see if it can be re-written in more useful language. In particular, I would suggest spending more time walking the reader through figure 1. This seems like a very important section of the methods, yet I definitely struggled to understand what inputs were used where in the Fire-C model.

8867 Section 4.1 lines 16-17. Microbial decomposition? I am confused. I didn't think microbial decomposition was an input into either the GIPL or Fire-C Model. How can you positively identify this as a mechanism? I think you should state that based on

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your model runs, you are able to HYPOTHESIZE on two mechanisms that may contribute to organic carbon losses. Then state your results, and then show evidence that supports your hypotheses. I think it is safe to say that your modeling results suggest mechanisms but not that you were able to “identify”. To me, that implies experimental research looking at those mechanisms.

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Interactive comment on Biogeosciences Discuss., 7, 8853, 2010.

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