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.

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Responses to General Comments

We appreciate Referee 1's review of our manuscript, and we found most of the comments to be very helpful and constructive. The referee highlights a number of important points, and we value theopportunity to address these comments and provide clarification for the editors and scientific community at large. In the revised manuscript, we have added considerable text to further clarify our primary objectives (responses 1 and 11), model descriptions (response 2), study sites (response 3), and findings (response 7).

The reviewer has expressed concerns regarding our experimental approach for addressing the study's goals. In particular, the reviewer questions the application of the Fire-C model to address near-term effects (next 100 years) of climate and disturbance on soil carbon storage. In response #6, we detail our rationale and justification for using the coupled Fire-C and GIPL models to evaluate the sensitivity of soil C dynamics. It is important to note that both models have been calibrated in prior publications (Romanovsky&Osterkamp 2000; Nicolsky et al. 2009; O'Donnell et al. 2010) and the GIPL model was specifically calibrated in the present study to our *in situ* measurements of soil temperature. Furthermore, while the Fire-C model was originally used to evaluate change in soil C over several millennia, the model operates on annual time-steps, and thus, is also appropriate for decadal- to century-scale changes in soil C dynamics. Given these prior findings and current calibration efforts, we were confident in conducting sensitivity analyses to evaluate the response of soil C to a suite of different climatic and disturbance scenarios.

Responses to Specific Comments

- Page 8856, Lines 24-26: So the goal is to "refine our understanding" of soil OC dynamics. This is pretty weak tea. . . and it's not clear to me that even this is accomplished.

We modified our overarching objective to state that "we aim to quantify the importance of individual climatic and disturbance factors governing soil OC dynamics in the boreal region." We feel this more accurately describes the goals of this paper.

- Page 8858, Lines 2-5: This is much too brief. Authors need to describe the model in much more detail, given that its results are a central focus here.

We added text to this section to expand upon our modeling approach. For instance, we now identify the model as using a "mass-balance approach." We also note that

Microsoft Excel was used to run all modeling scenarios. We also point out that this model does not consider the dissolved carbon losses from soil. Finally, we already had provided citations for several peer-reviewed, published journal articles that used some version of this model. Furthermore, we provide detailed descriptions of the current modifications to the model in Sections 2.2.1, 2.2.2, and 2.2.3.

- Page 8860, Line 10 – I recognize the cited studies are available, but a bit more detail here would be helpful. In particular, is the chronosequence replicated? If not, how did authors attempt to control for non-temporal factors? Also give name ("Hess Creek") and lat/long.

We agree with the reviewer's suggestion. Chronosequences in this paper were not replicated, although soil profiles were replicated within the chronosequence - for example Several stand ages were replicated (Unburned Mature, 2003 Burn) for soil characterization and soil C stocks, as reported in O'Donnell et al. (2010). We added text to the methods section to describe how we controlled for non-temporal factors (e.g. aspect, vegetation, elevation, and climate). We have now named the chronosequence as "Hess Creek" and report latitude and longitude coordinates.

- Page 8862. Lines 1-5: Given this, does it make sense to include absolute numbers in the abstract?

The reviewer brings up a good point. However, we contend that by reporting the values in the abstract, it provides the reader with a better sense of the magnitude of each effect on soil OC stocks. To indicate that C loss amounts are approximate, we modified the abstract text to state that C loss are "relative" and we added an approximate symbol ("$\sim$") before each value.

<center>C5019</center>

- Page 8863, Lines 1-10: This text should be moved to the discussion. Note that sentence seems to imply Fire-C is an ecosystem model, which I believe is incorrect.

In response, we moved this text to the discussion section. We also changed the term "ecosystem models" to "biogeochemical models"

- Page 8867, Line 16: This is overstating the case and crystallizes the problems I have with this study: no OC losses were observed, so the authors, finally, are reporting how their model (originally designed for much longer time periods) reacts to input changes based on a few measurements in an un-replicated (I think) chronosequence. I'm unclear as to what new scientific insights can be, or are gained from this.

The reviewer brings up a number of points, which deserve attention and some clarification.

First, the reviewer has expressed concern about the use of the term"Fire-C model", which was originally developed by Harden et al. (2000) to evaluate soil C accumulation over several thousand years. In particular, the reviewer suggests that the model is only appropriate for much longer time-scales than the current study. However, the Fire-C operates on annual time-steps (see O'Donnell et al. 2010). Input parameters (i.e. NPP) are in units of g C m^{-2} y^{-1}, and reflect average NPP over a fire cycle. Given these facts, we contend that this model is appropriate for evaluation of change in soil C, not only on millennial time-scale, but also on century time-scales.

Second, the reviewer states that "no OC losses were observed." While it is unclear, perhaps the reviewer is suggesting that no OC losses were observed *"in the field."* However, our modeling simulations show clear soil C losses in response to various climatic and disturbance factors (Figures 8 and 9). We are confident in our findings,

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given our efforts to verify and calibrate the models used in this study. In O'Donnell et al. (2010), we conducted a model verification, comparing simulated C stocks in the Fire-C model to measured C stocks at the Hess Creek chronosequence. In the present study, we calibrated the GIPL model using field measurements of soil temperature (Figure 4), soil moisture (Supplemental Figure 3), and snow depth (Figure 3), and laboratory measurements of soil thermal conductivity (Table 3).

Third, the reviewer states that the authors "are reporting how their model reacts to input changes based on a few measurements." Given the model verification and calibration approaches discussed above, we felt confident to conduct simulations to test the sensitivity of soil C to a suite of different climatic and disturbance factors (Table 1). Sensitivity analyses are a common tool used by modelers to evaluate the magnitude and direction of a dependent variable to a driving variable or a changing parameter. A number of recent studies in Alaska's boreal region (Zhuang et al. 2002; Carrasco et al. 2006; Fan et al. 2008; Yi et al. 2009) have conducted similar sensitivity analyses as in this study. For this particular study, we contend that our findings are useful for evaluating the importance of both individual and combined climate-disturbance factors on soil C dynamics in the boreal region of Alaska.

- Page 8868, Line 27: At the very least the authors need to say "We also showed that modeled soil OC stocks were sensitive..." making it clear exactly what they've shown. Same comment applies to line 16-17 on next page. These sentences are simply reporting the behavior of a mass-balance model and it's impossible to draw inferences about shifts in mechanisms.

We changed the text on page 8868 to "Our sensitivity analyses also showed that soil OC stocks..." On page 8869, we state that "this finding is likely due to...", which suggests that there is some uncertainty regarding the "dominant mechanism."

Response to Technical Corrections

- Page 8854, Line 7: Clarify this is soil moisture.

We modified the text here to state "fire-soil moisture interactions."

- Page 8854, Lines 24-26: This final sentence doesn't say much, consider removing.

We omitted this sentence from the abstract.

- Page 8857, Lines 3-8: These sentences should be in intro, not methods.

We omitted the first sentence on Lines 3-4 from the manuscript.

- Page 8858, Line 14: "inform"? A more specific verb would be useful.

We changed the text here to state "these relationships were then used as a control on soil OC dynamics in the Fire-C model..."

- Page 8859, Lines 8-25: This is confusing. Authors are calculating an inherent k, so shouldn't it appear on the left-hand side of equation. (Ditto for equation 2). What depth soil temperature was used. What is exact definition of function f? And please be notationally consistent: no "x" for multiplication if not used elsewhere.

We modified equations (1) and (2) so that the variables we were solving for were on the left-hand side of the equation. We also added text to the Methods section to describe the soil depths we used for prescribing soil temperature in the model. We also added text to better define the function, $f(\theta)$. Finally, we omitted the use of "x" in the equations.

- Page 8860, Line 4: Does this refer to Section 2.4? Title doesn't match.

We modified the text in parentheses to state "see Section 2.4 *Modeling soil temperature dynamics* section for details."

- Page 8863, Lines 21-22: Abstract says 450 years. Be consistent.

We changed the text in the abstract to 500 years.

- Page 8864, Line 15: "marginally good" but non-significant!

We changed the text in the Results section to state "marginally good, but non-significant"

- Table 2: What do error terms refer to? Clarify.

We added a footnote to Table 2, stating "Note: Values represent mean $\pm$ one standard error."

- Table 4: Give equation.

We changed the caption for Table 4 so that it now includes the exponential equation.

- Figure 2: Clarify that black line isn't climate but weather (i.e. what authors observed over study period).

We omitted "climate" from the legend for Figure 2.

- Figure 4: These model-data comparison would be much more informative if plotted as observed versus modeled, perhaps coloured by day of year, with a 1:1 line shown.

While we appreciate the reviewer's suggestion, we prefer this graphical approach for two reasons. First, by plotting modeled and observed data versus time, it is easier detect seasonal anomalies, particularly during freeze-up in fall and soil thawing in spring. Second, this graphical approach is the convention in permafrost literature (see Romanovsky&Osterkamp 2000; Nicolsky et al. 2009).

- Figure 5: Isn't this duplicating data shown in Table 4? If so, probably remove. If not, show data, not simply lines. Define define OHT in caption.

We now define OHT in the caption of Figure 5.

Table 4 summarizes the parameters and statistics for the exponential equations depicted in both Figure 5 and 7. We contend that Figures 5 and 7 are an important component of the results, helping to illustrate the impact of different factors on active layer depth across a range of organic horizon thicknesses. We originally plotted the data points, as suggested by the reviewer, but each figure quickly became too cluttered with information. We believe that the current iteration of Figure 5 and 7 are the most effective way to portray these results.

- Figure 8: Caption refers to "green line" but figure is not in color.

We omitted the word "green" so now the legend simply refers to a "dashed line."

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