Biogeosciences Discuss., 7, C4753–C4756, 2011
www.biogeosciences-discuss.net/7/C4753/2011/

Biogeosciences
Discussions

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 #1

Received and published: 12 January 2011

*** General comments ***

This manuscript explores an interesting and important topic, given the carbon density of high-latitude ecosystems and their uncertain vulnerability to climate change. The authors report the results of coupling field data, from an Alaska boreal chronosequence, with a soil thermal model and a long-term mass-balance model, all to explore soil carbon dynamics under changing climate. This is an innovative study design given the radically different models being used.

There are some serious problems, however:

The ms is well written, but methods are frequently unclear. I applaud the inclusion of

Figure 1, but the text needs better clarity and more details in many spots. The figures are general unimaginative and sometime obfuscate. Model-data comparisons should be rethought. For all these, see comments below.

More seriously, the study's goals don't seem well matched with its design and tools used. The authors themselves seem unclear about their aims, or utility of their results, given the fuzzy description of their various goals ("refine," "inform"). Too often, the authors claim insight into soil processes or mechanisms that simply can't be supported by the experimental design and models used (a situation not helped a lack of clarity about observed versus modeled results).

For example, the Fire-C model is (I believe; the authors give almost no details) an extremely simple, mass-balance model, designed for explorations and sensitivity analyses of soil carbon over millenia (as done by Harden et al. 2000). Its utility in exploring decadal- to century dynamics seems questionable, and, whatever its other strengths, I don't see any insight it can provide into ecosystem- or pedon-scale mechanisms. Finally, then, the authors are simply reporting the behavior of this particular model, operating on a timescale for which it wasn't designed.

In summary this is an unusual approach to an important problem, but one I think fatally flawed by the choice of models, tools, and field data limitations.

*** Specific comments ***

1. P. 8856, l. 24-26: so the study's 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.

2. P. 8858, l. 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. This should include basic model approach (mass-balance?), assumptions, implementation (Excel?), etc.

3. P. 8860, l. 10: I recognize the cited studies are available, but a bit more detail

here would be useful. In particular, is this chronosequence replicated? If not, how did authors attempt to control for non-temporal factors? Also give name ("Hess Creek") and lat/long.

4. P. 8862, l. 1-5: given this, does it make sense to include absolute numbers ("5.3 kgC/m2" etc) in the abstract?

5. P. 8863, l. 1-10: this text should be moved to discussion. Note that sentence seems to imply Fire-C is an ecosystem model, which I believe is incorrect.

6. P. 8867, l. 16: "through our modeling analyses, we were able to identify two mechanisms underlying OC losses." 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 unreplicated (I think) chronosequence. I'm unclear as to what new scientific insights can be, or are, gained from this.

7. P. 8868, l. 27: same as comment above. At the very least 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 ("dominant mechanism"); these sentences are simply reporting the behavior of a mass-balance model, and it's impossible to draw inferences about shifts in OC mechanisms.

*** Technical corrections ***

8. P. 8854, line 7: clarify this is soil moisture.

9. P. 8854, lines 24-26: this final sentence doesn't say much; consider removing.

10. P. 8857, l. 3-8: these sentences should be in intro, not methods.

11. P. 8858, l. 14: "inform"? A more specific verb would be useful.

12. P. 8859, l. 8-25: this is quite confusing. Authors are calculating an inherent k, so shouldn't it appear on left-hand side of equation? (Ditto for Eq. 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.

13. P. 8860, l. 4: does this refer to §2.4? Title doesn't match.

14. P. 8863, l. 21-22: abstract says 450 years. Be consistent.

15. P. 8864, l. 15: "marginally good" …but nonsignificant!

16. Table 2: what do error terms refer to? Clarify.

17. Table 4: give equation.

18. Figure 2: clarify that black line isn't "climate" but "weather," i.e., what authors observed over study period.

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

20. Figure 5: isn't this simply duplicating data shown in Table 4? If so, probably remove. If not, show data, not simply lines; define OHT in caption. 21. Figure 8: caption refers to "green line" but figure is not colour.

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