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Discussions

Interactive comment on "Simulating boreal forest carbon dynamics after stand-replacing fire disturbance: insights from a global process-based vegetation model" *by* C. Yue et al.

Anonymous Referee #3

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Overall Evaluation

This manuscript represents a useful exploration of issues that may need to be addressed in simulating carbon dynamics associated with fire disturbance in boreal forests. The issue of carbon dynamics associated with stand demography has been largely ignored by most large-scale ecosystem models, and it is refreshing to see a large-scale modeling group take on this issue. The strengths of the study are that it (1) makes use of eddy covariance and other biogeochemical/biophysical data from three chronosequences in North America, (2) model validation/uncertainty is evaluated comprehensively across a diversity of ecosystem-level structural characteristics using several criteria, and (3) the impact of model uncertainty at the site level is explored at

the regional level. The weaknesses of the manuscript, in my opinion, are that (1) it is very long, (2) the presentation for the motivation for the study in the Introduction is too model centric, (3) there should not be presentation of new results in the Discussion section, (4) there may be a flaw in logic with respect to comparison of simulations in Figure 13, (5) there is an inadequate discussion of the sensitivity results associated with increases in atmospheric CO2 and changes in climate with respect to previous boreal forest research evaluating these issues, and (6) the apparent lack of appreciation of the importance of post-fire soil warming and thaw on decomposition dynamics. However, in my opinion, these are all addressable weaknesses. See below for my comments on these issues and other issues I came across during my review of the manuscript.

Specific Comments

1. The Length Issue: I really appreciated having all the detail about the changes in the model and on the model validation, but 35 pages of text, 9 tables, and 13 figures just seemed to be too much. I think the paper could be tightened up a lot and a reduction down to around 25 pages of text, 5 or 6 tables, and 8 or 9 figures would be more appropriate to the effective number of take home messages from this study. I think it would be useful to think about how much of the methods could be included in supplementary material and how many tables and figures could go into supplementary material. However, I urge the authors not to just offload table and figures into supplementary material and try to cite them in the main text. If you want a reader to look at a table/figure to understand a "key" point, then that table or figure needs to be in the main body of the manuscript.

2. The presentation concerning the motivation of the study in the Introduction: In general, I don't find it very interesting for model development to be the raison d'etre of the study. The model is a tool to answer questions that cannot be answered without the model, and so it is better, in my opinion, to have the questions be the focus of the study. The study has some interesting questions about the importance of CO2 and

climate variability in the response of the model. There are also some side issues that the model evaluates including the importance of representing snag dynamics and the use of coarse- vs. high-resolution climate data. My preference is to see an Introduction written without ever referring to a model by name and not introducing and justifying the model being used until the Methods section.

3. The presentation of results in the Discussion section: When I read through section 2.4.4 I fully expected the CO2 and climate variability analysis to be the major point of the results section. It was very strange to see it reported in the Discussion section. I urge the authors to structure the paper so that the results of this analysis are presented in the Results section, where they belong in my opinion.

4. Possible Flaw in Logic in Figure 13: Maybe I misunderstood something, but I felt that the model should be calibrated separately for GPP with respect to the CO2FIX-CLIMVAR and CO2FIX-CLIMFIX simulations. I realize from the methods in section 2.4.4 that you used the same GPP correction ratio that was used in GPPCAL-CMCD, but in my opinion this wouldn't necessarily result in GPP optimization for the CO2FIX-CLIMVAR and CO2FIX-CLIMFIX simulations. I think to really have these simulations comparable to the GPPCAL-CMCD simulation, you need to optimize GPP for these two simulations. I'd be very interested to see the statistics reported for Table 6 for these two simulations in which GPP was corrected as was done in the manuscript vs. in which GPP was optimized in these two simulations.

5. Discussion in the context of previous work on the issue of CO2 and climate sensitivity: In the presentation and discussion of Figure 13 in section 4.3, there was not much comparison to previous boreal forest analyses of these issues (just two references in the last sentence about drought and temperature). The effects of changes in CO2, climate, and fire regime have been evaluated in several modeling studies, for example in Balshi et al. (2007, 2009) and Hayes et al. (2011) (which are cited in other parts of the manuscript) and in Yuan et al. (2012; Assessment of historical boreal forest carbon dynamics in the Yukon River Basin: Relative roles of climate warming and

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fire regime changes. Ecological Applications 22:2091-2109) (which is not cited in the manuscript). A richer discussion is needed with respect to these sensitivity analyses that were conducted in this study.

6. The importance of post-fire warming and thaw on decomposition dynamics. It doesn't appear to me that the combustion of ground-layer carbon affects the simulation of soil thermal dynamics by SECHIBA, and that there is no post-fire warming and thaw effects on decomposition in the model. That is okay in my opinion at this stage of ORCHIDEE development, but it is important to recognize this shortcoming of the model with respect to the discussion of next steps as it is the focus of much research (some of which involves co-authors on your manuscript). A key issue to ultimately evaluate in future versions of ORCHIDEE_FM_BF is whether the consideration of the post-fire warming/thaw issue influences carbon dynamics in comparison to this version of the model. In general, consideration of this issue will likely increase the sensitivity to climate variability in a warming climate. This issue has been treated Yi et al. (2009, 2010) and Yuan et al. (2012). You might also look at Jafarov et al. (2013). Note that if I'm mistaken on this connection between soil carbon and soil thermal dynamics, then it would be good to have a soil temperature variable included in Table 7. Yi, S., A.D. McGuire, J. Harden, E. Kasischke, K. Manies, L. Hinzman, A. Liljedahl, J. Randerson, H. Liu, V. Romanovsky, S. Marchenko, and Y Kim. 2009. Interactions between soil thermal and hydrological dynamics in the response of Alaska ecosystems to fire disturbance. Journal of Geophysical Research – Biogeosciences 114, G02015, 20 pages, doi:10.1029/2008JG000841. Yi, S., A.D. McGuire, E.S Kasischke, J. Harden, K. Manies, M. Mack, and M. Turetsky. 2010. A dynamic organic soil biogeochemical model for simulating the effects of wildfire on soil environmental and carbon dynamics of black spruce forests. Journal of Geophysical Research – Biogeosciences 115, G04015, 15 pages, doi:10.1029/2010JG001302. Jafarov et al. (2013. The effects of fire on the thermal stability of permafrost in lowland and upland black spruce forests of interior Alaska in a changing climate. Environmental Research Letters 8, 11 pages, doi:10.1088/1748-9326/8/3/035030).

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7. Page 7302, lines 17-18: "The fire cause snag pool" is awkward wording. Perhaps change to "A snag pool associated with fire disturbance".

8. Page 7304, line 10: Perhaps change "Discontinuous permafrost layer were observed" to "Permafrost occurs".

9. Page 7305, lines 11-12: Change "among stem and coarse root" to "among stems and coarse roots".

10. Page 7306, line 15: Change "in case of clearcut" to "in the case of clearcut".

11. Page 7307, lines 8-9: Change "To make . . . observation," to "To promote agreement between the simulated and field-based estimates of productivity,".

12. Page 7307, lines 13-16: Note that there is no mention of organic horizons as a "ground fuel" in this sentence. Most of the combustion is from organic horizons (see Turetsky et al. 2011).

13. Page 7308, lines 1-2: Note that litter pools in black spruce forests are pretty small compared to organic soil horizons.

14. Page 7308, line 16: Change "with rather a small amount" to "the amount is small".

15. Page 7308, line 19: Change "pools are summarized" to "pools is summarized"; note that "fraction" is the subject of the sentence.

16. Page 7311, line 4: Change "sties" to "sites".

17. Page 7311, line 9: Change "this data" to "these data".

18. Page 7312, line 12: Change "south Alaska" to "south central Alaska".

19. Page 7312, line 15: Change "necessarily occurred" to "necessarily occurs".

20. Page 7316, lines 22-25: It is important to identify the depth of mineral soil be considered in both the model and observations to make this a meaningful comparison between simulated and observed soil carbon. There is nothing in Figure 4 about the

depth of mineral soil.

21. Page 7318, line 17: Change "When doing above," to "The".

22. Page 7318, line 20: Change "model output" to "model outputs".

23. Page 7321, line 9: Change "HCDD" to "HHCD".

24. Page 7322, lines 1-3: Could the LAI underestimate be associated with the fact that the model is simulating stand-replacement instead of successional trajectories that might include deciduous seral stages with higher LAI. If so, this successional issue is something you might want to pick up on the Discussion.

25. Page 7326, line 21: Change "that that" to "that".

26. Page 7327, paragraph 2, lines 10-18: I'm surprised that underestimate of forest floor carbon (see Table 7) was not mentioned as a possible reason for the underestimate of fire carbon emissions in the Alaska sites. Can the relative biases in the simulation of forest floor carbon at each of the sites explain the under- vs. overestimation of fire carbon emissions? Note that on line 13 of this paragraph, that "froests" should be "forests".

27. Page 7327, line 23: Change "contributes" to contribute".

28. Page 7328, lines 1 and 2: Note that Amiro et al. (2001) is pretty old, and there has been a lot of work done since then. Also, if I'm not mistaken, I think the estimates in Amiro et la. (2001) are largely from experimental burns in Canada, which are conducted under fuel moisture conditions that are generally wetter than would be experienced during a real wildfire in the boreal forest. Drier fuels usually mean greater burn severity and higher fire emissions, so estimates of fire emissions based on experimental burns are generally biased low.

29. Pages 7328-7329: Is section 4.2 really necessary? It seems to me that all the points made in this section were pretty obvious in the Results sections. Certainly, this

section could be boiled down to a single paragraph of 10 or so lines.

30. Page 7328, line 25: Change "supposed" to "assumed"?

31. Page 7329-7330: Move the reporting of results in section 4.3 to the Results section and just let this section deal with discussion aspects of those results.

32. Page 7333, line 1: Change "reason" to "effects"?

33. Page 7333, line 14: I think it would be appropriate to cite Yi et al. (2010) here as well.

34. Page 7334, line 1-2" change "fluxes ... scale" to "fluxes at national or regional scales".

35. Page 7334, line 14: Change "processbased" to "process-based".

36. Page 7334, line 23: Change "in North American boreal forest" to "in the North American boreal forest".

37. Page 7335, line 1: Change "possible to evaluate model" to "possible the evaluation of model".

38. Page 7335, line 2: Change "which allows" to "to allow".

39. Page 7335, lines 3-7: I think you need to mention several other things here: permafrost dynamics, soil organic horizon dynamics (sensu Yi et al. 2010 and underestimate of forest floor carbon), post-fire decomposition dynamics, and succession dynamics (the underestimate of LAI). Note that "permafrost layer" should be "permafrost dynamics" in the sentence. Also change "the model is found generally being able" to "the model is generally able".

40. Page 7335, lines 7-10: Why is the cohort approach implemented in this study a novel approach given that the approach of tracking boreal forest cohorts in process-based models was pioneered by Balshi et al. 2007 and has been extended in a number

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of regional analyses since then (Balshi et al. 2009, Hayes et al. 2011, and Yuan et al. 2012)?

41. Page 7335, line 10: Change "And this will help" to "This progress will help".