

## ***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.***

**C. Yue et al.**

chaoyuejoy@gmail.com

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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

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

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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 CO<sub>2</sub> 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.

[General Response] We would like to thank the reviewer for the thoughtful comments and suggestions. In line to the reviewer's comments, the revised manuscript has been shortened and some structural change has been made. To allow to easily track the essential modifications in the revised manuscript, we keep the newly inserted text as the color of "red". As the page and line numbers in the original and revised manuscripts are different due the structural change of the manuscript, we try our best to help the reviewer quickly locate the modifications by indicating the section and paragraph numbers in the revised 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 appro-

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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.

[Response] The revised manuscript has been restructured to include 7 figures and 7 tables. We moved part of the results to the Supplementary Material to make the manuscript more concise and focused on the objectives as listed in the introduction section. Many details in the method section are shortened and summarized, with reference being made to the Supplementary Material, while keeping in mind that readers should be provided with enough general information to be able to understand the manuscript. Details potentially interesting for readers are provided in Supplementary Material. All the sections in Supplementary Material are properly referred to in the main text.

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'être* 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 CO<sub>2</sub> 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.

[Response] Following reviewer's suggestion, the introduction has been re-organized and expanded to be more focused on the scientific questions that could be addressed

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by the model developments presented in this manuscript. Please refer to Paragraph 2, 3 and 4 in the Sect. 1 in the revised manuscript.

3. The presentation of results in the Discussion section: When I read through section 2.4.4 I fully expected the CO<sub>2</sub> 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.

[Response] The results of CO<sub>2</sub> and climate variability analysis are now presented as section 3.5 and included as part of the results section.

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 CO<sub>2</sub>FIXCLIMVAR and CO<sub>2</sub>FIX-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 CO<sub>2</sub>FIXCLIMVAR and CO<sub>2</sub>FIX-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.

[Response] We use the same correction ratio as we regard this ratio reflects the intrinsic structural model error and thus is forcing-independent. Besides, there are no corresponding observation data available for this purpose. We add the following text in section 2.4.4 to make it clear: "the same site-specific GPP correction ratio is used .... This was done for two reasons. First, this site-specific ratio is considered to reflect the model internal structural error that could not be resolved by parameterization and is therefore independent of forcing factors. Second, there are no corresponding CO<sub>2</sub>FIX scenario observation data available to derive the site-specific ratios. "

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5. Discussion in the context of previous work on the issue of CO<sub>2</sub> 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 CO<sub>2</sub>, 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 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.

[Response] Following the reviewer's suggestion, the discussions have been fully expanded to include findings of previous works, and are presented in section 4.4

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.

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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).

[Response] We agree that discussions on this important aspect of fire and permafrost dynamics are missing in the original manuscript. The soil organic layer, soil temperature and permafrost dynamics as related with fire disturbances are now discussed in section 4.4 Paragraph 6 in the revised manuscript. As the version of ORCHIDEE used in present study does not include the postfire change of soil organic layer thickness and its thermal and hydrological role during cycles of fire disturbance, the model fails to reproduce the seasonal amplitude of the soil temperature change for different periods after fire disturbance. This is discussed in section 4.4 and presented in Figure S7 in Supplementary Material. Given this model deficiency, it does not make enough sense to provide similar information on soil temperature as other variables in original Table 7 (now Table 5 in the revised manuscript).

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

[Response] Change has been made as suggested.

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

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[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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).

[Response] Due to the difference in terminology, it seems that organic soil horizons (or organic soil layer) is used more in boreal studies. Here ground fuel has the same meaning as the organic soil (or forest floor as also explained in footnote b of Table S1) as moss is not explicitly simulated. We modified the sentence as below (Sect. 2.3.2) in the main text: " In fires of boreal North America, carbon emissions come mainly from ground fuel (or organic soil horizons) .... " and further added the following sentences in the Supplementary Material section 1: "As ORCHIDEE-FM-BF does not simulate a profile of litter through the different vertical horizons that occur in the boreal organic soil, the total amount of aboveground litter is considered as the ground fuel."

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

[Response] We agree with the reviewer’s comments. As the model does not simulate explicitly different vertical horizons that occur in boreal organic soil but simulate the

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aboveground litter as three pools with different turnover rates, so the amount of total aboveground litter is considered as organic soil layer (or organic soil horizons). And it’s this part that will be burned in the model. To make it clear, we added the following sentences in the Supplementary Material Section 1: "As ORCHIDEE-FM-BF does not simulate a profile of litter through the different vertical horizons that occur in the boreal organic soil, the total amount of aboveground litter is considered as the ground fuel."

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

[Response] Change has been made as suggested.

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

[Response] The subject of the sentence is "The parameterization of fire.... and the fraction of ....", so the use of "are" is reasonable. However, "fraction" has been modified into "fractions".

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

[Response] We apologize for this typing error, now it’s been corrected.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

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depth of mineral soil.

[Response] Thanks for the reviewer's comments. The major objective of the present manuscript is to calibrate the model against several carbon flux and carbon stock variables during the postfire forest regrowth, i.e., their temporal change during the forest succession. As argued in the manuscript, the mineral soil carbon does not change significantly during the forest successional time span. So the idea is only to qualitatively evaluate soil carbon pool simulation with the overall error to the carbon fluxes being quantified, but not to have a targeted evaluation focusing on the mineral soil carbon dynamics. So based on this consideration, we consider that this qualitative evaluation without the soil depth information is sufficient. For this reason, the section of mineral soil carbon evaluation has been moved now to Supplementary Material to reduce the size of the manuscript, and the second paragraph of the section 5 in Supplementary Material is added for better clarification. We hope this could help to address the reviewer's concern.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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.

[Response] We agree with reviewer's comments. Two modifications have been made

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in the revised manuscript to better address this issue. The following sentence has been inserted into section 2.4.2: "The dynamic vegetation mechanisms in ORCHIDEE-FM-BF do not allow the realistic representation of this species shift at the intermediate forest stage on a single simulation pixel, thus each site is prescribed to be fully covered by the boreal needleleaf evergreen forest PFT". And the following sentence has been inserted into section 3.4.1 for brief discussion: "The underestimation in the intermediate-aged forest is partly because the whole simulation pixel is prescribed to be fully covered by the boreal needleleaf trees, and this precludes the occurrence of broadleaf trees which often dominate the early succession stage and have higher LAI and productivity. "

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

[Response] Change has been made as suggested.

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".

[Response] The simulated fire carbon emissions are comparable or within the range reported by other synthesis and local studies at Canadian sites, however are underestimated at Alaskan sites. A big part of the under-estimation can be explained by the under-estimation of forest floor carbon. We have now included this analysis in the section 4.2 (the discussion for fire carbon emissions simulation) in the revised manuscript. Meanwhile, taking into account the comments of the other anonymous referee, we restructured the section 4.2 to make it more clear.

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

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[Response] Change has been made as suggested.

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 al. (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.

[Response] The fire fuel consumption in Amiro et al. (2001) is derived by using the Canadian Fire Behavior Prediction (FBP) system and many data are from experimental fires which cannot fully represent the real case big fires. We update the discussion by citing a more recent synthesis study (French et al., 2011). Also, in the original manuscript, some more recent and local studies are already cited (Randerson et al., 2006; Kasischke & Hoy, 2012). Given the constraint in the length, we deemed the revised discussion (section 4.2 in revised manuscript) is sufficient, so finally we dropped the Amiro et al. (2001) as the a reference.

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.

[Response] We think this section is necessary as it briefly discussed the difficulty in model calibration to reduce errors that arise from the forcing data. However, following the reviewer's suggestion, this section has been shortened to 9 lines. Please refer to the section 4.1 of the revised manuscript.

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

[Response] Change has been made as suggested.

31. Page 7329-7330: Move the reporting of results in section 4.3 to the Results section

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and just let this section deal with discussion aspects of those results.

[Response] The result have been moved to section 3.5 of the revised manuscript and now this section handles only the discussion aspects of the results (section 4.4 in revised manuscript).

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

[Response] Change has been made as suggested.

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

[Response] Yi et al. (2010) has been added as citation.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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

[Response] Change has been made as suggested.

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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”.

[Response] Change has been made as suggested.

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

[Response] We agree that the cohort approach has been pioneered by Balshi et al., 2007 and been extended in other following studies as pointed out by the reviewer. The difference between our approach and Balshi et al. (2007) is that, as we simulate explicitly the forest stand structure and the self-thinning process as the forests grow older, it could represent the postfire forest dynamics in a more realistic way. It also opens the possibility to simulate the surface fires in which only shrubs and small trees are killed (by selectively removing the small trees in the forest stand and allow them to regenerate), which are prevalent fire type in Russian boreal forests. However, we agree that it's not appropriate to describe this approach as "novel". Correspondingly, we restructure the section 5 of the revised manuscript to make it more clear and more relevant with the objectives as listed in the introduction section.

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

[Response] Change has been made as suggested.

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