

Interactive comment on “Fire-regime variability impacts forest carbon dynamics for centuries to millennia” by Tara W. Hudiburg et al.

Anonymous Referee #3

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General comments

This paper examines patterns of forest ecosystem carbon dynamics in response to long-term past fire regime at the watershed scale. As noted by the authors, knowledge on this topic is scarce and the modeling exercises presented in the paper bring important new evidences that fire history left persistent legacies on ecosystem carbon trajectories on the centennial to millennial time scales, questioning the usual basal assumptions of ecosystem models. Globally, the text is clearly written, the scientific context and knowledge gaps are clearly exposed as the problematic and the general hypothesis. Also, the questions addressed here are very pertinent. That said, I advise the authors to follow previous comments and advises from SC1, RC1 and RC2. Moreover, a more deeper review of fire ecology with respect to carbon cycling could: i) help to better understand the choice of DayCent for this study; ii) bring a more critical inter-

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pretation/discussion of the processes you mentioned (line 99-100) linked in DayCent model and improve the interpretation and discussion of the results. I also noted several improvement possibilities (see also Technical corrections): 1/ Structure: Mixing results and discussion is sometimes confusing (especially for section 3.4). Because section 3.1 to 3.3 are not full discussions but rather descriptions and comparisons between your model estimates with values of other studies, it should not will be difficult to separate results and discussion. For example, discussion could contain a section on the limits, a section with the implications for projecting future ecosystem states and another for research development needs. 2/ Hypotheses: Based on Kelly et al. (2016), the general hypothesis assuming forest carbon budget modeling would be different between equilibrium runs and paleo-informed runs is explicit. Nevertheless, the alternative hypotheses that you mentioned (line 103) and results that were “expected” (line 301) are not explicitly described. You could add these hypotheses in the introduction. 3/ Model parametrization: According to SC1, DayCent is quite well described. Unfortunately, I was not able to access the model input and parametrization file. While is it clear that you informed the model with paleo-fire reconstruction from Dunette et al. (2014), it is less clear what you do with the vegetation data. You wrote that you “pair a paleoecological record of vegetation and wildfire activity” (line 98) and that DayCent requires input of vegetation cover (line 145), but no information is provided on vegetation in section 2.3. It would be important to get more details.

Specific comments

1. Does the paper address relevant scientific questions within the scope of BG? Yes. The paper deals with many fields within the scope of BG.
2. Does the paper present novel concepts, ideas, tools, or data? Yes. New data from modeling exercise based on previous works are presented.
3. Are substantial conclusions reached? Yes.
4. Are the scientific methods and assumptions valid and clearly outlined? Yes.

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5. Are the results sufficient to support the interpretations and conclusions? Yes.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.
8. Does the title clearly reflect the contents of the paper? Yes.
9. Does the abstract provide a concise and complete summary? Yes.
10. Is the overall presentation well structured and clear? Yes, but could be improved (see General comments).
11. Is the language fluent and precise? Yes.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, but see SC1 comments for [date] CE.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes. Values for equilibrium scenario should appear in Figure 3 or equilibrium scenario should be removed in lines 301-305. As the Chickaree Lake watershed is the object of this study, some characteristics such as the watershed size and topography (slope characteristics) could be mentioned. Moreover, you defined 8 partial paleo-informed scenarios but only 4 are represented in Figure 1. To facilitate the reading, I suggest to represent all partial paleo-informed scenarios in Figure 1 or you can specify that you show only 4 on the 8 scenarios in the figure caption.
14. Are the number and quality of references appropriate? Yes.
15. Is the amount and quality of supplementary material appropriate? NA

Technical corrections

Line 48: should read "greater than simulated under an equilibrium and climate warming

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scenarios"? Line 71: NECB appears for the first time here but is defined at lines 162-163. Line 103: the "alternative hypotheses" are not clearly exposed and should appear here. Line 112-114: should be in the Discussion or Conclusion section. Line 117: same comment as SC1 Line 125: should read "Dunette et al. (2014)" Line 125-127: the sample resolution of the core results from the chronology based on 14C dates. I suggest to reorder the sentence. Line 129: should read "Dunette et al. (2014)" Line 160: autotrophic respiration is accounting in NPP yet. Line 163: how fire emissions are calculated in the model? Line 234: what is STATSGO? Line 252: should read "Figure 2" instead of "Figure 1". Line 275: should read "Kelly et al. (2016)". Line 275: should read "Together, this work and ours". Line 280: it is not clear what the equilibrium scenario is doing here. Line 286: can you justify the threshold of 1 Mg C ha⁻¹? Line 296: should read "stand-replacing". Line 303: "lower" compared with equilibrium or paleo-informed scenario? Line 301: "As expected" refers to a hypothesis? I think you should present this hypothesis in the introduction. Line 301-305: you mention the equilibrium scenario in your comparison and refer to the Figure 3, but values for the equilibrium scenario don't appear in this figure.

Finally, I recognize the great potential of this paper and the important gap it helps to fill in the carbon cycling-related fire history knowledge. I am happy to see that such research is unfolding and I advise the authors to consider previous comments to improve their manuscript.

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