

Interactive comment on “Only small changes in soil organic carbon and charcoal concentrations found one year after experimental slash-and-burn in a temperate deciduous forest” by E. Eckmeier et al.

Anonymous Referee #1

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General comments

I suggest deleting the section on soil color and limit the scope of this paper to the effect of slash-and-burn on soil organic carbon (your title). I have never really understood why the authors wanted to know if there was a correlation between soil color and charcoal soil concentration (application ?). I was wondering how your fire fit (season, severity, etc) with past slash-and-burn. You only have one plot in one type of forest. In your introduction, you specified that burning had a high spatial and temporal variability. It would be interesting if the authors could discuss this issue.

Specific comments

P 596 line 12. “..were incorporated into the soil matrix by soil animals.” Your study did not test that. It should be only mention as possible explanation in the discussion
P 597 line 4. Why used Westerling et al. 2006 a reference. This reference is for western US and unless I am wrong, deciduous forests was not part of their results.
P 597 lines 20-26. I would delete this paragraph and concentrate on the effect of fire on charcoal production.
P 598 line 14. Delete question III.
P 598 site description. More information on the understory composition and the presence or absence of organic soil (and thickness) should be provided.
P 599 site description. Weather condition on the day of burning should be given. And how these conditions (e.g. burning in October) represent (or compare) past slash-and-burns.
P 599 lines 4 and 15. I have concerns with the statistics and the sampling design. 20 soil samplings in a 30x30 plot or 11x8 plot (even smaller) are not true replicate. Unless the authors did a spatial analysis for this site, there is only one replicate (the 20 should be bulked) for this site. And why 20 and not 40 and what about the distribution of these spoil samples; it should be explain.
P 599 lines 15-16. Number of replicates do not correspond to number of replicates in table 2. Explain please.
P600 lines 5-9. I would delete. See general comment.
P 600 lines 18-21. See also comment above, but more information should be provided especially on how you have treated your replicates.
P 600 lines 23-26. I don't understand. Fire is known to reduced soil bulk density. The authors have soil BD for each soil samples. Therefore, they should be able to compare carbon stocks.
P 600 Results, information on fire severity should be provided.
P 601 line 3. P-value should be in table 2 ?
P 601 results. No mention of C, N and CN ratio. Why ?
P 602 line 24. P-value in table 2 ?
P 603 lines 7-25. I would eliminate this section.
P 604 lines 23-24. Nothing new.
P 604 Conclusion. You should discuss the limitation of your study.

References: unless I have missed it, I think Zackrisson et al. is not cited in the manuscript.

Figures and tables. I am not sure if table 2 and figure 1 are both necessary. Maybe

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keep table 2 and add p-values. I don't think figure 2 is necessary. It should be eliminated and figure 3 (see general and specific comments) as well. Table 2. Why in table 2 "n" don't equal 20 ?

It should be submitted as a technical note only.

Interactive comment on Biogeosciences Discuss., 4, 595, 2007.

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