

Interactive comment on “Boreal forest soil chemistry drives soil organic carbon bioreactivity along a 314-year fire chronosequence” by Benjamin Andrieux et al.

Anonymous Referee #1

Received and published: 20 December 2019

General comments In this study, the authors were evaluating post fire carbon stock changes in functional reservoirs (bioreactive and recalcitrant) using the proportion of C mineralized in CO₂ by microbes in a long-term lab incubation, as well as the proportion of C resistant to acid hydrolysis. Through the manuscript (already in Abstract) there are problems with abbreviations, one can find through the text carbon and C, bioradiative C and CBioR, carbon dioxide and CO₂, etc. If you have started to use abbreviations, please be constant through entire text.

Introduction is informative, but I would expect more talk on the topic, why the C bioreactivity is important, and what does it mean if we have the changes in C bioreactivity

C1

reservoirs through fire chronosequences. The hypothesis at the end of the introduction are OK, but when the other set of hypothesis are presented in Material and methods section, this creates some confusion. Study design needs some improvements (see my detailed comments), as currently it is difficult to understand how many microplots (for moss biomass measurements) were established per sample plot (maybe a scheme describing the measurements from sample plot would be useful as supplementary material). I also can't understand why the samples were incubated with so high temperature (+26°C), and only with one temperature. Usually, during the incubation, one attempts to mimic the field conditions (use temperatures similar to real soil temperature). And due to that I'm really concerned that are the cumulative respiration calculations actually valid. Results and Discussion section could benefit also from info dealing with O layer thickness changes through time since fire. It would be good also to present the Cslow and Cfast values for different soil horizons. Right now there are two separate paragraphs in discussion dealing with soil carbon bioreactivity (separately for FH horizon and mineral soil), but in results section one can't find the values for these two horizons separately (see my detailed comments under Results and Discussion), instead authors are presenting the combined values (Fig. 3). And this brings us to another problem – authors are stating (in discussion) that 73% of the C in FH horizon is acid-insoluble. However, is not shown in results section, nor discussed in discussion section, that are there differences in recalcitrance of the soil in FH horizon (this is the part of the soil that is most affected by fire) through time since fire.

Below are my detailed comments on the manuscript: Abstract P1 L16: Here and later in the text, if you started to use abbreviations “carbon (C)”, please be constant through the text.

Introduction P1 L33: Here and later in the text, if you started to use abbreviations “carbon (C)”, please be constant through the text. Change “carbon-cycle” to “C-cycle”. Later in the text also change “carbon-quality”, carbon balance”, etc. P2 L76: If you have started to use “FH horizon” (actually we are missing explanation for that), why to

C2

jump her into "O layer"? It occurs also later in text.

Material and Methods P3 L93: Definition/explanation for FH horizon is needed. You are also using "O layer" in text, that is maybe more understandable for the reader P3 L96-98: These 400 cm² microplots for moss biomass measurements, how many of them per sample plot? P3 L114-117: What about mineral soil from 15-35 cm depth? I can understand that you were missing that sample from one plot, but here the soil preparation of samples from that depth is not described at all. P3 L120: What do you mean with "B-horizon" here? You haven't been describing the soil horizons. Are these now samples from mineral soil from depth 0-15 cm, or 16-35 cm? P4 L137-140: Incubation temperature +26°C? Why so high temperature? The chosen incubation temperature is not representing anyhow the conditions (soil temperature) in the field. Usually during the incubation the temperature is chosen to be similar to the field conditions, and also different temperatures are used. Why in this study the samples were incubated with only one temperature? The respiration rates increase rapidly with higher temperatures, and if using much higher temperatures (soil temperatures) than one can find from the field, the outcomes could be unexpected. P4 L148-151: It would be good to know how the CO₂ measurement times (sample taking times after closure) changed due to soil layer and progress of the experiment. P5 L157-165: As the incubation was done with only one (really high) temperature, I think the standardization to 24 hour period and the cumulative C mineralization calculations can be really biased, as the temperature is not taken into account.

Results and Discussion P8 L193-301: Would it be possible to see the "Cslow" and "Cfast" also for different soil horizons through TSF? I also can't find from the manuscript the O layer or FH horizon thickness changes through TSF. P10 L364-365: Is the talk now about completely insoluble C, or this actually includes also acid- soluble C in FH horizon? Is this 73% now some kind of average for entire 314 year chronosequence? What are the values close to the fire and through succession? There have been some studies from Northern-America lately, dealing with forest fires and soil organic matter

C3

quality in permafrost soils. It would be interesting actually to compare the findings. P11 L391: It would be interesting to see the values of insoluble/acid soluble C (%) for mineral soil in this section (as was presented in previous section for FH horizon) and this for entire chronosequence. P11 L256-260: To long and confusing sentence, consider rephrasing. There is also a lot of talk considering Figure S2 from supplementary material. If this figure is so important, why to include it into the supplementary material? Conclusions Conclusion should be short summary of your work and findings. Currently there are many other studies (with references) included into summary, that actually should belong into the discussion section

Figures Like mentioned earlier, it would be good to see the "Cslow" and "Cfast" also for different soil horizons through TSF (in separate figure) Tables Table 1. Min and Max of FH depth, soil thickness, pH Bulk density particle size is not giving much to the readers as it is not known on what side of the succession these values are (close to the fire or at the end of succession and chronosequence). It would be much more informative to give these values through chronosequences (starting close to fire and then with certain interval after the fire)

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2019-88>, 2019.

C4