

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

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Response to RC2

We wish to thank the reviewers for dedicating their time to review our manuscript and for their comments that are very helpful to improve our manuscript. We address all the comments raised by Anonymous Referee #2 below.

In this manuscript, the authors use 1-year soil incubations to analyze how “bioreactive” and “recalcitrant” soil C pools vary over a 350 year fire chronosequence. They use a linear regressions and confirmatory path analysis to test hypothesized cause and-effect

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relationships between the soil pools and other soil chemistry variables. In general, the manuscript is informative, well-supported, and easy to read.

We thank Anonymous Referee #2 for her/his positive comments.

General comments: 1. I would like to see the correlation between incubation-derived estimate of C_{slow} and the acid-insoluble residue as a figure, since the latter is often used as a proxy for the former without direct comparison. In this manuscript, it's not clear which estimate of bioreactive/recalcitrant C is used in the models and in the figures.

In our study, we did not use incubation results to derive estimates of C_{slow} . C_{slow} was estimated as the fraction that is acid-insoluble. Nevertheless, incubated-derived C_{slow} can be calculated as the residuals of incubated-derived C_{fast} subtracted to total C. So, the correlation between C_{BioR} and acid-insoluble residues would inform if acid hydrolysis and incubation of soil samples methods give consistent results to estimate soil C cycling. We will add a figure with these correlations in supplements and a short paragraph in the discussion section. We agree in part with Anonymous Referee #2 about clarifying which estimate of bioreactive or recalcitrant C is used in the models. Indeed, the response variable at the center of the direct acyclic graph in Fig.2, Fig.4 and Fig.5 is C_{BioR} . C_{BioR} has been defined in the section 1 Introduction “as the proportion of C mineralized in CO_2 by microbes at constant temperature and constant water content over a long period of times as a relative measure of soil C lability (Laganière et al., 2015; Xu et al., 1997)” (L. 54-55). Moreover, we explain in the section 2.4.2 Soil C quality and bioreactivity (L. 252-264) that C_{fast} and C_{slow} are functional reservoir sizes (so stocks) calculated from the incubation experiment and from the acid hydrolysis experiment, respectively, and that we have analyzed these response variables as a function of TSF (referring to Fig.3 in which legend and caption use the same abbreviations, see also L. 266-267). However, we will bring more clarity in the text, as follows: L. 51-52 (inputs): “As part of this study, we characterized the acid-insoluble and bioreactive soil organic C pools (C_{slow} and C_{fast} , respectively, expressed as stocks) that

accumulate following fire.” L. 252: “First, we wanted to estimate variation in the size of the bioreactive or recalcitrant soil C pools (Cfast and Cslow, respectively, expressed as stocks) with TSF.”

2. Why is soil texture hypothesized to influence moss dominance in the causal models?

We acknowledge that the explanation is lacking. We will add a short sentence, as follows: L. 220: “[. . .] their influence on moss dominance. Indeed, we expected that Sphagnum spp. would dominate over feathermosses under wetter conditions induced by greater precipitation, finer-texture soils holding more water, or both, because of their greater dependence to high soil water content.”

3. Moss community composition (Sphagnum vs feather moss) is included, but is there any difference in moss abundance that could be included in the model? Even non-sphagnum mosses have distinct biochemistry and low decomposition rates compared to vascular plants.

In this study, we included an index of moss dominance only based on a presence/absence survey (section 2.4.1 Index of moss dominance, L. 240-250). Neither moss community composition nor moss abundance was sampled for this study.

4. The conclusion contains lots of new analysis not included elsewhere in the manuscript and would be better presented as an additional discussion section

According to both Anonymous Referee #1 and Anonymous Referee #2, we will transfer the section 5 Conclusion to the discussion section. Please, see also our response to Anonymous Referee #1.

5. Discussion of temperature generates some confusion about what is actually being measured. Since the incubation temperature (26 °C) is much warmer than the MAT of the study site, the bioavailability assays correspond to “potentially available” C more than a realistic estimate of in situ soil respiration. This is a reasonable choice but leads to some confusion in the introduction and discussion: -The value of ana-

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lyzing the recalcitrant SOC fraction is justified in regards to the C-quality temperature hypothesis, since “recalcitrant” C should respond more to warming. But, since the incubation temperature is 26C, the “recalcitrant” C that is actually measured is SOC that is preserved even when temperature is increased to unrealistically high levels. -Lines 378-383 seems to suggest that climate does not drive SOM decay rates or transfer between SOC pools, which is not supported by the study. -The connection between these results and the temperature sensitivity of soil respiration (lines 385-390) is also unclear. This section could be improved by discussing the relationship between hypothesis FH1 and the C-quality temperature hypothesis, and the implications of the results for the validity of the CQTH.

- In our study, the “recalcitrant” soil C was assessed with acid-hydrolysis of soil samples. This method assumes that the non-hydrolysable C fraction is not accessible for microbial degradation (L. 52-53; L. 167-168). Nevertheless, this point of view has practical implications for modeling purposes (Paul et al., 2006) but is controversial (Kleber, 2010; Lehman and Kleber, 2015). That’s why we make a strong case on CBioR and not on “recalcitrant” C. Indeed, in our study CBioR correspond to “potentially available” C (i.e., relative measure of C lability under standard conditions), our data has to remain in the context of lab incubation, and cannot be extrapolated to in situ soil respiration. All our samples have been processed equally, so the results of soil incubation (CBioR) are comparable among our soil samples and highlight some of the processes driving the potential C loss from boreal soils through microbial respiration. Moreover, we have indicated that the “recalcitrant” C can be processed by microbes synthesizing enzymes involving Mn (L. 363-371). - To avoid confusions, we will add “also” into L. 382, as follows: L. 382: “[. . .] such as exchangeable Mn concentrations and pH, might also be used to modulate soil C dynamics in such models, [. . .]” - Because we have not incubated our soil samples at several temperatures, we cannot assess the temperature sensitivity of soil respiration (Q10, please see our response to Anonymous Referee #1). So, we do not feel comfortable discussing the CQTH hypothesis. Nevertheless, we acknowledge that L. 385-390 is unclear, so we will modify it as follows: L. 385-387:

“Furthermore, when synthesizing data of in situ experimental warming, Carey et al. (2016) found no change in soil respiration rate for warmed compared to control plots at the global scale, whereas changes were found to be significant for the boreal biome.”

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