SOIL Discuss.,
https://doi.org/10.5194/soil-2019-88-AC1, 2020

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

Benjamin Andrieux et al.

benjamin.andrieux@uqat.ca

Received and published: 28 February 2020

Response to RC1

We wish to thank 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 #1 below.

General comments In this study, the authors were evaluating post ïňĄre 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 pro-

portion of C resistant to acid hydrolysis. Through the manuscript (already in Abstract) there are problems with abbreviations, one can ïňĄnd through the text carbon and C, bioradiactive C and CBioR, carbon dioxide and CO2, etc. If you have started to use abbreviations, please be constant through entire text.

We acknowledge that there are some problems with abbreviations. We will solve all the abbreviation problems in the manuscript to be constant throughout the manuscript, by doing the following modifications: L. 16, 18, 20, 26, 33, 36, 40, 379, 418, the word "carbon" will be abbreviated with "C" but it will remain not abbreviated in the titles. L. 18, 20, "Mn" and "Al" will be changed for full words "manganese" and "aluminum", respectively. L. 63, "CBioR" will be replaced by "bioreactive C stock". L. 65, because they are used for the first time in the manuscript (abstract excluded) we will add the significance for "Mn" and "Al" abbreviations. L. 73-75 will be replaced by "[...] 1) soil CBioR increases as forest stands get older, leading to a buildup of soil bioreactive C stock under the cold conditions of the boreal forest; 2) alternatively, if the bioreactive soil C stock reaches a new equilibrium because of rapid turnover, the proportion of bioreactive C stock should decline as total soil C stock increases with TSF". L. 144, 152: according to the Soil journal manuscript preparation guidelines and because it is not ambiguous, we will use "CO2" instead of "Carbon dioxide". L. 209, we will remove "manganese (Mn)" to keep only "Mn". L. 262, "CBioR" will be replaced by "C lability". L. 294, "CBioR" will be replaced by "bioreactive C". L. 342, we will replace "exchangeable aluminum" by "exchangeable Al". L. 425, "time since fire" will be replaced by "TSF".

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 reservoirs through ïňĄre chronosequences.

According to Anonymous Referee #1, we have not fully introduced the usefulness of better understanding soil C bioreactivty in the section 1 Introduction. Several improvements will be done, as follow: L. 39: "[...] on soil heterotrophic respiration. Furthermore, the bioreactive C fraction of soil organic matter is cycled on time scales relevant to global warming. Thus, quantifying the size of the bioreactive soil C reservoir and understanding the controlling factors of soil C bioreactivity (CBioR) is key to inform models of C cycle in face of and to better anticipate global warming". L. 47: "[...] from the decomposition of soil organic matter by clay surfaces (Six et al., 2002). At steady state and when the accumulation of physico-chemically stabilized soil C occurs, soil C can only accumulate as non-protected C (Castellano et al., 2015) that is more prone to decompose quickly". L. 55: "[...] a relative measure of soil C lability (Laganière et al., 2015; Xu et al., 1997). Assessing the sizes of resistant and bioreactive soil C fractions through a fire chronosequence would help modelers to enhance the current and future C balance of landscape prone to wildfires".

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.

The hypotheses at the end of the introduction are general hypotheses, whereas the hypotheses included in the second section 2 Material and methods include detailed hypotheses of the third general hypothesis. To clarify this point, we will modify the text as follows: L. 72-77 will be replaced by (also including the modifications mentioned previously) "From there, our general hypotheses are that once site factors such as overstory composition, surficial deposits and soil drainage are accounted for, as they were in the present study: 1) soil CBioR increases as forest stands get older, leading to a buildup of soil bioreactive C stock under the cold conditions of the boreal forest; 2) alternatively, if the bioreactive soil C stock reaches a new equilibrium because of rapid turnover, the proportion of bioreactive C stock should decline as total soil C stock increases with TSF; and 3) soil CBioR is primarily controlled by TSF and moss dominance in the O layer (FH horizon), and by soil physico-chemistry in the mineral soil (see 2.3 for detailed hypotheses)." L. 181 will begin with "According to our third general hypothesis and to address the complex interplay among climatic and non climatic factors, [...]".

Study design needs some improvements (see my detailed comments), as currently

it is difïňĄcult 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).

All the details about the field work can be found in the section 2.1 Site selection, sampling design and field work. We have explained on L. 95-98 that for each plot we used 20 microplots to identify the dominant moss types, at the same place where we measured the thickness of the FH horizon. According to Anonymous Referee #1, a summary of the field work would help the readers, so we will add a diagram of the sampling plot as supplementary materials (as Fig. S1).

I also can't understand why the samples were incubated with so high temperature (+26â ËĞD ËĞC), and only with one temperature. Usually, during the incubation, one attempts to mimic the ïňĄeld conditions (use temperatures similar to real soil temperature). And due to that I'm really concerned that are the cumulative respiration calculations actually valid.

We thank Anonymous Referee #1 for this comment but there is a misunderstanding here as our goal was not to mimic real soil temperature as observed in the field. Our goal with the incubation was to assess the accessible soil C for microbial decomposition that is the functional soil C reservoir contributing to greenhouse gas emission. In our study, we chose to incubate soil samples at 26°C according to the literature (temperatures between > 0°C and 35°C are the most commonly used). Low C mineralization rates were observed for boreal forest soils incubated under 15°C (see Paré et al. 2011 ; Laganière et al. 2015). So 26°C is a trade-off to maximize CO_2 production by the microcosms while performing measurements in the optimum temperature range for the microbial breakdown of soil organic matter and this has been verified globally (Carey et al. 2016). We were mostly interested in the soil C bioreactivity, that is why we have incubated soil samples with only one temperature. Usually, several temperatures for soil incubation are used to assess the Q10 value, i.e., a relative measure of temperature sensitivity of C mineralization (Laganière et al. 2015). Due to the high number of soil samples implying a tedious logistic for the incubation experiment, we did not use several temperatures to assess the Q10 index.

Results and Discussion section could beneïňĄt also from info dealing with O layer thickness changes through time since ïňĄre. It would be good also to present the Cslow and Cfast values for different soil horizons.

The topic of O layer thickness and soil organic matter accumulation was studied previously and details about some O layer properties (as O layer thickness, bulk density and C concentration) can be found in a previous study (Andrieux et al., 2018). We acknowledge that presenting Cslow and Cfast values for the different soil layers would help the readers to have a full picture of post-fire changes occurring with soil depth. Based on Anonymous Referee #1 comments, we will include more details about the O layer thickness in Table 1, for several post-fire age-classes. Moreover, we will add: 1) a figure in the supplementary material (soil C quality by pool and for each soil layer as a function of time since fire); 2) the accumulation rate for each of the soil layers in Table 2 and; 3) we will also enhance the sections 3.1 Post-fire soil C pool size and 4.1 Post-fire soil C quality, as follows: L. 301, integrating a new paragraph: "These general trends are mostly influenced by the size of the Cslow and Cfast pools of the O layer, being 5 times and 2.4 times larger than the top 35 cm of the mineral soil one (i.e., sum of the two mineral soil layers), respectively (Table 2; Fig.Sx). Cslow and Cfast decrease with soil layers from the surface soil horizon (O layer) to the deeper mineral soil, both in absolute size and proportion (Table 2 ; Fig. Sx). Consistently with the whole data set, the proportion of Cslow and Cfast do not vary quantitatively with TSF for all the soil layers analyzed separately (Table 2). The size of the Cslow pool increases linearly with TSF in the O layer only (R^2 = 0.09, p = 0.01), not in mineral soil layers (p > 0.07 for both mineral soil layers). The size of the Cfast pool increases linearly with TSF in the O layer (R^2 = 0.12, p = 0.003) and in the top 15 cm of the mineral soil (R^2 = 0.05, p < 0.05), not in the deepest mineral soil layer from 15 to 35 cm (p > 0.21)." L. 356: "[...], such as cold temperatures under a thickening O layer developed with TSF (Table

1), could have slowed down labile C degradation and allow its accumulation (Kane et al., 2005). Our results also emphasize that changes in the size of the soil functional reservoirs with TSF are stratified within the soil profile. This pattern is consistent with the fact that fire impact on soil C stock is limited to surface soil horizons (Andrieux et al., 2018)".

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 ïňĄnd 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 ïňĄre) through time since ïňĄre.

More details about soil C pool sizes will be added separately for the FH horizon and the mineral soil layers (please, refer to our previous response).

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.

All the abbreviation problems will be solved (please, see our previous response).

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.

All the abbreviation problems will be solved (please, see our previous response).

P2 L76: If you have started to use "FH horizon" (actually we are missing explanation for that), why to jump her into "O layer"? It occurs also later in text. Material and Methods

P3 L93: DeïňĄnition/explanation for FH horizon is needed. You are also using "O layer" in text, that is maybe more understandable for the reder

We acknowledge that using "O layer" instead of "FH horizon" will be more understandable for the reader, so we will replace "FH horizon" with "O layer" through the text (L. 20, 26, 76, 97, 98, 101, 114, 115, 118, 128, 133, 145, 150, 187, 199, 212, 241, 253, 259, 277, 284, 285, 293, 302, 304, 306, 311, 320, 322, 326, 356, 359, 361, 362, 363, 365, 370, 372, 374, 375, 384, 392, 401, 410, 442) and in tables, figure and figure captions. Moreover, the hypotheses named "FH1" and "FH2" will be renamed "O1" and "O2" (L. 193, 197, 199, 212, 213, 214, 219, 220, 228, 303, 305, 308, 319).

P3 L96-98: These 400 cm2 microplots for moss biomass measurements, how many of them per sample plot?

We used 20 microplots per plot, as mentioned L. 96 (please, see also our previous response).

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.

All the mineral soil samples (0-15 cm, 15-35 cm and first 15 cm of the B horizon) were prepared following the same standard protocol. We will add this precision as follows: L. 115-117 (including previous corrections mentioned above) "O layer samples were sieved through a 6-mm mesh before being oven-dried (60°C), whereas mineral soil samples (either top 15 cm of the mineral soil, mineral soil from 15 to 35 cm or top 15 cm of the B horizon) were dried by air and passed through a 2-mm sieve".

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?

Pyrophosphate extractable Fe and Al analyses were done for the top 15 cm B hori-

zon samples only. We will clarify this point as follows: L. 119-121 : "Finely ground sub-samples (< 0.5 mm) were used for C concentration, pyrophosphate extractable Fe and Al (top 15 cm B horizon only) and acid hydrolysis analyses." B horizon pyrophosphate extractable Fe and Al are used to determine if a soil is classified as a podzol or not and to define to which sub-group it belongs to in the Canadian System of Soil Classification. Moreover, the characteristics of the B horizon mirrors the soil processes of podzolization (Shaetzl and Anderson, 2005). In our study, we used the pyrophosphate extractable Fe and Al (i.e., metal oxides) of the B horizon as a proxy for the podzolisation status of the soil. Also, we will add a short description of the soil horizons and redirect the reader to the diagram of the plot inventory and soil sampling were we will add a photo of a soil profile (see our previous response), as follows: L. 84: "The soils that develop under this cool and humid climate with acidic litter inputs typically belong to the Podzolic order (Table 1). In boreal forests, podzolic soils often have a thick organic surface horizon (O layer), an eluviated A horizon (Ae) from where leached materials accumulate in the illuviated B horizon (Fig. Sx)".

P4 L137-140: Incubation temperature +26â ËĞD ËĞC? Why so high temperature? The chosen incubation temperature is not representing anyhow the conditions (soil temperature) in the ïňĄeld. Usually during the incubation the temperature is chosen to be similar to the ïňĄeld 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 ïňĄnd from the ïňĄeld, the outcomes could be unexpected.

Our aim was not to assess the decomposition rate of soil organic matter that could be observed in situ. The goal of our experiments was to assess the accessible soil C for microbial decomposition, that is the functional soil C reservoir contributing to greenhouse gas emission. For this purpose, long-term lab incubation at high temperature have most often been used (Paul et al., 2006). We used a consistent method for all our soil samples, so we are confident to compare our results among all the studied sites, including the bioreactivity of soil C that is a relative measure of soil C lability. A global analysis also revealed that soil heterotrophic respiration was optimal around this temperature (Carey et al. 2016). But we do agree with Anonymous Referee #1 that our results do not allow extrapolations of C mineralization rate that could be observed in the field. Please, see also our previous response.

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.

Overall, final CO2 measurements were done 4h and 24h after sealing the jars for O layer samples and mineral soil samples, respectively, because O layer CO2 production rate was greater. Some exceptions have been made. Nevertheless, this sample taking time after closure is meaningless because all CO2 production rate were standardized on a daily (24 h) basis.

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.

Rescaling the respiration rate on a 24 h basis is inherent to the soil incubation method (Paré et al., 2006) and allow the direct comparison among samples. The temperature remained constant through the course of the experiments (not only during measuring periods), so this parameter did not have affected our estimations. Please, see also our previous responses.

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 ïňĄnd from the manuscript the O layer or FH horizon thickness changes through TSF.

Here, we assume that Anonymous Referee #1 comments on L. 293-301. As mentioned above, we will add more details about soil C pool sizes separately for the O layer and

the mineral soil layers (please, refer to our previous response).

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 ïňĄre and through succession? There have been some studies from Northern-America lately, dealing with forest ïňĄres and soil organic matter quality in permafrost soils. It would be interesting actually to compare the ïňĄndings.

As shown in the subtitle 4.2.1 Soil carbon bioreactivity in the FH horizon, here we deal with the O layer only. Moreover, the sentence L. 364-365 we clarified the sentence as mentioned above: “[...] the high proportion of acid-insoluble C of O layer samples (73 $\pm$ 5%, data not shown)”. We acknowledge that it was not clear that 73% was the average value for all the O layer samples. We will clarify as follows: L. 364-365: “This is reflected in the high proportion of acid-insoluble C of the O layer samples (among all the O layer samples, mean $\pm$ sd = 73 $\pm$ 5% ; Table 1)”. Here, we will refer the reader to Table 1 in which we will give the proportion of acid-insoluble soil C sorted in several classes of time since fire.

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.

According to Anonymous Referee #1 previous comment, values of acid-insoluble C by proportion will be included in the manuscript. Please, see our previous response.

P11 L256-260: To long and confusing sentence, consider rephrasing.

There is a mismatch between P. 11 and L. 256-260. L. 256-260 explains the terms of the equations (4) and (5). We are sorry not to be able to respond to this comment.

There is also a lot of talk considering Figure S2 from supplementary material. If this ïňĄgure is so important, why to include it into the supplementary material?

Fig. S1 (there is only one figure in supplements) represents all the CO2 measurements that we have done during the course of the incubation experiment. We have referred only twice to this figure in the manuscript (L. 146 and 158). We wanted to show that the shapes of the curves are consistent with the ones often observed with the incubation method of soil samples. However, because our paper does not focus on methodological issues of soil incubation, we would prefer to keep Fig. S1 in supplements.

Conclusions Conclusion should be short summary of your work and ïňĄndings. Currently there are many other studies (with references) included into summary, that actualy should belong into the discussion section

According to Anonymous Referee #1, we acknowledge that the current conclusion section contains information that should be included in the discussion section. Therefore, this section will be renamed 4.2.3 Implication for C cycling and research needs.

Figures Like mentioned earlier, it would be good to see the "Cslow" and "Cfast" also for different soil horizons through TSF (in separate ïňĄgure)

A separate figure will be added. Please, see our previous response.

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 ïňĄre or at the end of succession and chronosequence). It would be much more informative to give these values through chronosequences (starting close to ïňĄre and then with certain interval after the ïňĄre)

The minimum, maximum, mean and standard deviation of values included in Table 1 are informative because the readers can have a look at the variability of both response and explanatory variables used in the subsequent analyses. According to Anonymous Referee #1, giving these values through the chronosequence is relevant, so we will add this information for all post-fire age-classes. Please, see also our previous responses.

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