

REV 2

General comments

This study examines effects of climate and forest composition on soil organic carbon stocks in humid boreal forests of Canada. It uses elevation and latitude to create a climate gradient of forests spanning 4°C, dominated by balsam fir or black spruce trees. The authors found an effect of climate on carbon cycling (inputs and outputs) but no effect of climate on overall total soil organic carbon stocks (C stocks of the organic layer and top 40cm of the mineral soil), which is a result that is supported by some studies but not by others.

This study is important and the paper would be of interest to readers of Biogeosciences because of the large stocks of SOC that exist in boreal forests that we know are vulnerable to the rapid warming already occurring in northern ecosystems, however the mechanisms behind these C losses are not well understood and result in large uncertainties in modelling efforts. Furthermore, empirical measurements are needed to verify laboratory incubation results because the dominating controls determined in isolation in the laboratory are often difficult to observe in an intact system. This study is a strong contribution, therefore my criticisms are intended to strengthen the manuscript and provide “food for thought” for the authors.

The two larger scientific concerns I have are: 1) the metric used to evaluate the effects of climate is degree-days, and while there are instances where that is made explicitly clear it needs to be consistent throughout the manuscript. Climate is more than temperature, and climate change involves changes to precipitation as well as temperature. The authors nicely point out that the results of this study are applicable to “cold, humid” climates, however only the temperature component of climate change is tested, despite a 600+ mm range in precipitation across all the sites. If it is not possible to test MAP, I would like to see some info on soil moisture included at the very least;

We thank the reviewer for the insightful and constructive comments!

1-We have addressed this concern in length in the response to reviewer 1. Please see comment (1) in reply to reviewer 1.

and 2) Lability is a tricky concept that is measured in many different ways. This makes it difficult to compare between studies and interpret meaning. I challenge the use of mineralization as a measure of lability, especially in this study where lability is used as a potential explanation for Q10 variability (which is also respiration/temperature based). I don't necessarily think this part of the study should be removed but the caveats of the incubation as an indicator of lability should be discussed explicitly and critically. Also, Schmidt et al., 2011 suggests that even recalcitrant OM can be decomposed under the right environmental conditions, how do you know that labile OM is exclusively being mineralized in your incubations?

2-This is an interesting point! No, we do not know if some of the evolved C comes from recalcitrant forms and it may well be possible. We will change labile to bioavailable C as in Andrieux et al. (2020), the rationale being that we do not have information on the chemical nature of the organic material but rather on the potential for microbes to degrade it under standard conditions.

For the most part, this is a well prepared and presented manuscript. The figures and tables included are all useful, however some of them are blurry and difficult to read (Figures 3 and 4 in particular). There are several sentences in the text that require rewording, or reorganization. I've pointed out a few below. Some work is needed to make your hypotheses in the introduction clearer.

3-We will prepare figures with a better resolution and we will also move the concepts of organic matter reactivity in the text prior to stating the hypotheses.

Specific comments

Abstract

Line 12 "climate change is [a] matter of concern"

accepted

Line 19 "climate (cumulative degree days >5degreesC)", write like this throughout OR "climate (DD)" once DD is defined. Also, should mention somewhere in the manuscript why DD was chosen instead of MAT to represent climate

accepted

Line 25 change "spruce ones" to "spruce forests"

accepted

Line 28 "contrary to common soil organic matter stabilization hypotheses". My intuitive thought is that greater cycling would result in increased losses and decreases in stocks, or is the assumption that labile portions get respired and the recalcitrant C is left behind and stabilised by minerals?

We will change the term cycling by inputs

Line 31 "apply to the context of this study: cold and wet environment". I appreciate that this statement was included, however not much has been done to address the "wet" part of that statement. Precipitation is variable (MAP: 954 - 1631 mm) and not tested, and no soil moisture data has been shown

We will add a metric of aridity and present results of the relationship between soil respiration and soil moisture, which was not significant. More details in response to Reviewer 1 (1)

Intro

Line 36, "Boreal forests should also experience the most intense warming" could be changed to "are experiencing the most intense warming"

accepted

Line 56, The sentence that starts with “However, because both C fluxes to and from the soil are accelerated by temperature...” has great points but the sentence took a while to process as written.

I suggest: “...the net effect of increased temperature on soil C accumulation will vary if the rates of input and output fluxes are differentially affected by temperature” or something like that

Accepted: much clearer!

Line 80 -84 This comment about wildfires, although important and relevant, is out of place here as your hypotheses have nothing to do with assessing the effects of wildfire on SOC stocks. Consider moving wildfire to the general climate change/ boreal section at the beginning of the intro if you want to keep it. This paragraph should have more info about litter quality differences between the two forest types and the effect on SOM, for instance.

Accepted

Hypothesis 1

Line 85: warmer sites accumulate more carbon? Is this reasonable given the greater driving hypothesis that climate warming = losses of SOC to the atmosphere? Can both be true? I think the mineral-associated OM and MEMS framework should be the part of the explanation that is missing and should be described in more detail before getting to the hypotheses here. Also isn't litter of higher quality (lower C:N, more labile) more easily decomposed and respired?

The framework developed by Cotrufo et al. (2013) was used as a cornerstone to define this hypothesis (more stable C) with greater C inputs. We elaborate in the discussion about the outcomes. We agree to move the description of the MEMS framework in a section that proceeds the hypotheses.

Line 90: can you clarify this point? I think I know what you mean, and I think it's related to my question above, but it needs to be clearer. I like that the Andrieux, 2020 reference is included but I shouldn't need to go to that paper to understand the sentence. Is the point that the total (O.L. + mineral-associated to 40cm depth) carbon stock is important to capture? As opposed to studies that evaluate only O.L. stocks or only mineral C stocks. Can the Andrieux, 2020 paper be introduced in the main body of the intro before we get to the hypotheses? That might set things up better

Accepted: We will introduce the concept sooner in the text and this should make the hypotheses leaner. Andrieux et al. (2020) found that about 10% of the whole soil total organic C from the O horizon to 40cm down the mineral soil could be qualified as fast C.

Hypothesis 3

Line 93: this is the hypothesis that I'm having trouble with. Is it fair to use mineralized losses (C and N mineralization) as the measure of labile carbon and nitrogen content, and then to use that data as an explanation for Q10 variability which is also respiration and temperature based? Shouldn't an independent measure of lability be considered? For instance, a chemical measure of lability? How do you know for sure that what is mineralized in the incubations is labile?

Materials and Methods

Line 115, do you have any quantitative measure of “closed-canopy”? This is brought up again in the discussion and I don’t follow the logic with regard to bryophyte distribution.

We don’t but open canopy boreal stands are easy to avoid and have an abundant understorey of bryophytes that may change the studied processes. Bryophytes, especially Sphagnum species and also lichen can change the soil microclimate and the decomposition process greatly (Pace et al. 2018 <https://doi.org/10.1016/j.foreco.2018.02.020>; Pace et al. 2020: <https://doi.org/10.1007/s11104-020-04587-0>) . We wanted to avoid these situations.

Table 1, Please change annual precipitation to MAP

Accepted

Line 198, include simple description of the coefficients b1 and b2

Yes, we will: b1 is RS10 and b2 =ln(Q10)/10 while $Q_{10}=e^{10*b2}$; an error has slipped into the paper and Eq.3 is of no use and will be deleted.

Line 259, “depending on rates” why is this dependent on rates. Do rates reach zero? Please explain in the section.

We will add an explanation: we adapted the periods during which the lid was closed prior to CO2 measurements to get concentrations that were within the range of calibration of the IRGA.

Line 262, was the nitrate and ammonium flushed to simulate field flushing of these species? Was this done monthly and why? Yes, this is done to maintain the soil humid and to flush the accumulation of metabolic products that may interfere with the decomposition process.

Line 265, how can you assume that what was mineralized was labile? Doesn’t the Schmidt et al., 2011 reference suggest that even recalcitrant OM can be mineralized under the right environmental conditions? Couldn’t recalcitrant OM be decomposed at 22C?

Incubation is an empirical method where we measure what the microbes are able to process under standard conditions. We will change labile to bioreactive. We will add in this section a clarification/definition of what we call labile C and N; which is not, as the reviewer rightly points out, a chemical definition.

Results

Line 279, instead of “this variability could not be attributed to a single factor” write, “this variability could not be attributed to species, DDS or their interaction (Table 2)”

accepted

Line 280, the sand comment seems out of place as soil texture is not mentioned anywhere else in the paper and was not tested.

Yes, we can remove the sentence. Coarse textured soils lead to little mineral-organic interactions and OM stabilization. But we did not explore these relationships and our design is poorly suited for this.

Line 282, use humus layer or organic layer but not both.

We will make sure that we have consistency in the terminology

Line 284, I appreciate that the OL and mineral C proportions are shown here, but no need to say that 33% is close to 25%. If the proportions are not significantly different between forest types then you should say that instead.

We agree!

Line 285, use DDs instead of climate in the results so that it is clear what is being used as a metric for climate.

Table 2: is Total C the sum of carbon in OL, 0-40cm, and coarse woody debris? This should be clear in the caption.

Line 302/309, stick with degree-days instead of climate, the two are used interchangeably in this paragraph and the next

Figure 2 is blurry

All of the above will be fixed!

Line 340, do you think differences in Q10 would be observed under a larger range in MAT (>4C)?

We don't know but we will make the data available for use in a larger gradient.

Line 344, replace "ones" with "soils"

Figure 4 is hard to read, blurry and small

This will be fixed

Discussion

Line 369, remove "in"

Accepted

Line 385, is there a relationship between MAT and MAP? Yes for fir only; See first comment to reviewer 1.

Line 386, it would be great to include the soil moisture data

Accepted

Line 387, “[Furthermore], the size of the SOM stock is not only controlled by climate or NPP, [but is also] strongly influenced by soil types....”

Accepted

Line 397, including MAP

Accepted

Line 403, add reference for needle statement

Added and figure adjusted

Line 419, is this because black spruce sites are already generally wetter than balsam fir?

Good point! I am not sure that they speculated on this but we can mention it as a supposition.

Line 437, replace “congruent results, that is to say” with “the”

Accepted

Line 466, this would be easier to interpret if there was more info on “closed-canopy”

We will add a sentence on the role of bryophytes on organic matter cycling.

Line 470, are you using “active” synonymously with labile? If so, just use labile for consistency

Yes, we will use a consistent terminology. We will use the term available as explained above. (available to microbes)

Line 475, “to maintain” should be “to the maintenance”

Line 477, this is first time we are seeing MAOM, please write it out in full

Line 483, this is the first time we are seeing POM, please write it out in full

Line 476 – 489, There are several points being made in this section with no clear connection. It is difficult to understand the connection between MAOM, DOC and POM and how it relates to your results. I would start this as a new paragraph and refine

We agree with the comments above; We will re-write, the last section as it is confusing. We will indicate that: *Stabilized MAOM reservoirs may reach a saturation point, especially in non-recently disturbed soils (Lavallée et al. 2020). Boreal regions show the highest concentrations of dissolved organic carbon (DOC) in the surface soil globally (Langeveld et al. 2020), indicating that the capacity of these soils to immobilized DOC as water percolates through the soil column is limited. Cotrufo et al. (2021) suggested that under cold and wet conditions, it is not MAOM but poorly stabilized particulate organic matter (POM) that dominates the dynamics of SOC cycling. If indeed MAOM reservoirs have reach saturation, and POM dominates the SOC cycling, our results suggest that warming, while accelerating SOC cycling does not lead to changes in the stocks of either POM or MAOM stocks. However, more research is needed to determine how the different fractions of SOM are impacted by changes both in aridity and in temperature and to identify climatic thresholds from which SOC stocks become vulnerable.*

Conclusion

Line 492, replace “active” with “labile” for consistency

See above, we will use bioreactive or reactive as in Andrieux et al. (2020)

Line 501, change “with changes in climate conditions” to “with projected changes to temperature” or something like that to tie it back to the climate change projections for the area

Accepted

Line 501- 503, I appreciate this final recommendation. Could expand it to include “ these results indicate that climate change effects on SOM storage and dynamics need to be studied both within and among forest ecosystem types [in order to do what?]. How will continuing to do "within and among" studies help solve the problem? Please state explicitly. I think that would make for a more impactful ending!

Yes; we will complete the sentence... in order to separate the direct effects of climate change from that of vegetation change.