Biogeosciences Discussions

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Comment on bg-2021-178

Anonymous Referee #2

Referee comment on "Climatic variation drives loss and restructuring of carbon and nitrogen in boreal forest wildfire" by Johan A. Eckdahl et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-178-RC2, 2021

GENERAL COMMENTS

This study assesses the effect of wildfires on the C & N stocks in boreal forests of Sweden. It uses a "space substitution for time" approach, where the authors choose "control" unburnt areas adjacent to the burnt sites and consider those to be comparable to the burnt sites before the fire. It is fully understandable to use this type of approach as, most of the times, it is the only one available when studying the impacts of wildfires. Having said that, this approach has its limitations because, many times, those "comparable" sites are not exactly the same to the burnt sites, and, actually, many times that is the reason why they did not get burn in the same place. The authors, therefore, need to: 1) demonstrate this is not the case in the selected sites (e.g. maybe using remote sensing indexes before the fire to check if they were comparable), 2) acknowledge and discuss this limitation in the manuscript.

For example, the authors state in L293: "TEM differences between paired burnt and control plots were observed to increase both with control and burnt TEM levels however not along gradients of MAT ". Unless I am misunderstanding this, it sounds like there were already differences in soil moisture between the burnt and the unburnt sites before the fire, therefore, they would have had different fire behaviours and impacts in the hypothetical case where all had been burn.

Along these same lines, more than referring to the observed differences as "losses" (which imply measurements before and after fire), it would be probably more accurate to talk simply about "differences". Also considering that the sampling was done already 1 year after the fire (what is a pretty short time for wildfire investigation for sure but you may be seeing indirect effects already).

Another key issue that I am missing is why there is no information about the short-term climate variables in the discussion. Theses short-term variables are key drivers of fire behaviour and therefore impacts on C&N stocks.

I believe these issues need to be resolved before a decision upon acceptance of the

manuscript can be made. In additions, I have other specific comments below and in the pdf attached.

SPECIFIC COMMENTS

ABSTRACT: some rephrasing will improve readability, please see my detailed comments in the pdf attached.

INTRODUCTION:

-L30: please add more updated references.

- L45: please describe a bit more the differences between Eurasian and North American fuels and fire regimes here, as it is key to understand the implications of this study's findings.

-L47: the explanation about fuel chemistry also needs expanding.

- L50: please update these references, there are several reviews on the topic less than 15-20 years old.

- L55: this is only true for some types of emissions calculations and, right now, the most commonly used models using this type of approach (e.g. GFED) are changing. Please update this part and do provide references less than 17 years older, as this is a topic that is rapidly changing.

- L66-70: this paragraph needs references.

- Please explain somewhere how the fire 2018 was one of the two (with 2014) most extreme over the recent years, and that it was associated to drought, as this is important context for this study.

- Hypothesis 3 is very long and complex, not enough information has been given in the introduction to understand it completely. Please elaborate this part and consider dividing it in two.

MATERIALS AND METHODS:

- L88: please explain a bit more how the fire scars were detected (beside "remote sensing data").

- L99: Sentinel-2 during the time of the fire? Were all fires long enough for them to be captured with sentinel infra-red? What if the main fire front had already passed? This does not sound like the best option for choosing the best plot.

- L148: one year after the fire many of the needles, specially those affected by fire (i.e. brown or black) would have fallen already. This needs to be acknowledged as a limitation of this method.

- L156: how the representativeness of the whole understory is proved using 4 patches per plot? How were these patches chosen in areas with a very heterogeneous understory distribution? Please explain.

- For the methods used no references are given, are these new methods? If so, how do you ensure the representativeness of your measurements?

- L166: “visual estimates for percentage volume of needles, broad leaves, woody material, moss and lichen were multiplied by total weight to form CCVs”: does not this assume densities of these different materials are similar, what it is obviously not true?

RESULTS

- L208: “transferred large amounts of C and N from lower soil layers to the highly nitrogenous surface layer of char.” This was not a “transfer” but a conversion. Please rephrase.

- In several places the authors talk about C and N “lost” from the different soil layers and now “found” in the char layer. This assumes that all char is coming from soil, what may be the case in some places but not in others where the understory and canopy led to charred inputs to the ground (as discussed in L315). Even if these inputs may not be too substantial in some places they can be in others. Please rephrase.

- Also, when talking about “increases” of C/N in the mineral soil layers: did any of the fires really affect the mineral soils? If not, then a direct effect is not possible and the variability observed may be due to differences between control and burnt sites. Please clarify and discuss.

DISCUSSION

- L313: “highly nitrogenous char layer”, this is not correct because, as explained several times in the Results Section, the char layer has a lower N content than the unburnt soil components.

- L314: “The char layer was likely largely produced by fire interacting with the understory and moss/litter layer”: what data are supporting this statement? There was a substantial difference between the duff layer in the burnt vs unburnt sites too, so it is expected fire did burn through this layer considerably too (as explained in the second half of the sentence). So the current wording is misleading, please rephrase.

- L324: please add data to support the statement of “low level of overstory damage”… for example, the charring heigh or the fraction of the canopy scorched/burnt.

- L340-345: the comparison of the char layer with the charcoal in Hart and Luckai is not appropriate. Those authors selected individual pieces of charcoal within the soil, with a high proportion very probably coming from wood and therefore displaying very specific characteristics. The char layer in this study is a mix of charred organic materials with inorganic charred materials (also called “wildland fire ash”, see Bodi et al. 2014). In addition, there will be also some unburnt components even after cleaning the samples (as the mix is not easy to separate, specially one year after fire). A better comparison would be the charred layer in Santin et al. Geoderma 264 (2016) 71–80, and other similar studies.

L346-350: along the same lines than previous comment, it may be that the inorganic N is adsorbed to char but, also, that the inorganic components of the “ash” layer are still present in your “char layer”.

- Section 4.2: why the immediate weather parameters are not discussed here (e.g. SPEI)? These will be the ones more closely related to fire characteristics and therefore differences between burnt and unburnt plots.

- L365: “ignition probability” does not relate to fuel but to ignition opportunities such as human causes (e.g. accidental) and lighting. please rephrase.

- L367: What info/references support this statement? This does not necessarily has to be the case, actually, very dense forest plots may have a higher moisture and, therefore, may be less susceptible to burn (as explained in Section 4.3 actually).

- L370: I don´t understand this statement, was TEM not the same in the control plots than in their burnt counterparts?

FIGURES

It would be very useful to have a figure with pictures showing burnt and unburnt sites.

TECHNICAL COMMENTS

Please see attached pdf with minor suggestions directly on the manuscript.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-178/bg-2021-178-RC2-supplement.pdf