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Interactive comment

Interactive comment on "Thermal alteration of soil organic matter properties: a systematic study to infer response of Sierra Nevada climosequence soils to forest fires" *by* S. N. Araya et al.

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The referee comments are followed by our responses. We have indicated location of texts in the manuscript by a combination of page number and line number.

General Comments

The present work describes research performed to investigate the impact of fire temperature on the organic matter composition of soils from a climosequence derived from the Sierra Nevada (USA). It was intended to infer the implications of changing climate on topsoil SOM properties that might experience changing fire regime. This subject is of course of interesting for the readers of SOIL and deserves a proper treatment. In

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Discussion paper

order to reach their objectives, the authors conduct a systematic study to examine the changes of topsoil organic matter from the selected soils occurring during their heating under laboratory conditions at increasing temperatures. Those experiments show that major changes occur at temperatures > 250°C and that those changes are expressed mainly in a loss of C and N and the formation of aromatic C. This approach has also been used in many other studies which came to the same results, although they used other materials. Taking this into account, the study is not really a novel idea or concept, but confirms already existing data. The authors try to put their results into the context of climatic change. Well, the climate change may change the C contents in soil, the quality of the litter and the fire frequency, but the chemistry occurring during combustion will not be altered. The climate change will also not have a considerable direct impact on fire intensity, since the latter is mostly determined by the available fuel and its moisture (OK, if there is more fuel due melting of permafrost soils, yes there is an indirect impact of climate on fire intensity. If the climate turns drier there are only more fires if the fuel density does not change etc.). I think, it is difficult to perform the relationship which was done by the others which turns some of the conclusions into opinions which can be obtained without conducting the present work. I think also, that it is difficult to conclude that climatic change lead to more fires with temperatures above 250°C. If the right fuel is present, the temperatures of a forest fire can easily increase to 1000°C and more - even without climatic change. Of course not much charcoal will accumulate under those conditions. The produced accumulated charcoal, on the other hand, is the material which enters the soil and will change the chemical composition of its SOM. This impact will be higher than the transformation of SOM in the mineral phase itself, since the low thermal conductivity of the latter prevents easy increase of soil temperatures and thus major heat-induced alterations of its SOM. Considering further that mineral soils were analyzed, I strongly suggest to include a part explaining the temperature profile of mineral soils during fire in the introduction. Summarizing my concerns together, it is not clear to me, how the used experimental design can add to a better understanding of the impact of climate change on the transformation of topsoil

SOM due to fire. I think if the authors present their data as a relationship between soil properties and their alterations due to the impact of fire and the content of SOM (without trying to include climate change), the paper would still be of interest but less speculative.

Author response: We appreciate the reviewer's comments and summary. We have revised the introduction section overall for clarity and to address comments from both reviewers. Specifically, we have added a brief explanation of vertical temperature profile during fires in the introduction (P4, L 9-14).

Specific comments

P4, L12: sugars cannot be aromatic. I suggest, . . .higher aromaticity due to transformation of carbohydrates and lipids (actually, the latter may rather transform into smaller units)

Author response: We have re-written the statement as suggested. (P4, L 20).

Mention which Sierra Nevada was studied (USA).

Author response: We have added California to clarify the location. (P5, L 23).

The whole description of the study site is very extensive which is not necessary to be able to follow the approach. It should be considerably shortened.

Author response: We understand the reviewer's concern, but we believe shortening this section would make the methods, site info and justification to brief for audiences that are not familiar with the study area. We have left it as is for now, and would be happy to revisit this discussion if the reviewer and editor think it is a must.

I missed the information if the litter layer was included in the sampling or if only mineral soils was used. This question is of importance since the litter represents the fuel.

Author response: Only the mineral soil was used in the study. We have added a sentence in the methods to explicitly state that that O horizon was not used (P7, L17).

Why was oxygen supply limited? In natural above-ground fires, the fire commonly stops under oxygen depletion. In addition, it has to be considered that pyrolysis allows to go to higher temperatures before complete combustion occurs.

Author response: We have edited the statement regarding oxygen supply during heating, the intended meaning was that oxygen supply was not limited. The statement in section 2.2 is meant to express that oxygen was not limited since volume of furnace was relatively large (P8, L1).

P8, L4: At the used temperatures no alteration of the mineral phase is expected. In addition, dry soils were used, thus higher moisture due to colder climate is not probed. Therefore, the only differences in soil properties which are relevant for the effect of fire temperature is the quality and the quantity of organic matter which can be related to climate regime but may also be altered by other environmental conditions (Whereas the quantity of C has been considered for the discussion of the data, the quality was largely neglected). Thus, in my opinion, it is difficult to relate unbiasedly climate regime to impact of fire on SOM.

Author response: While mineral transformation is generally associated with extremely high temperatures, we have observed some mineralogical changes in the soils (which the results were reported Araya et al. 2016, SOIL, doi: 10.5194/soil-2-351-2016). In addition, the properties of the mineral phase, including the physical and chemical properties, might affect the response of SOM to heating. Change in the quality of SOM was explored by FTIR and C and N isotopic composition.

Page 10: With respect to the concomitant loss of C and N, see also early work of (Knicker et al., 1996; Almendros et al., 2003)

Author response:

Page 11: The reviewer is correct; these are important works. We did in fact cite works of these authors in this paper. But, it is important to considered with stable isotopic data interpretation that different chemical compound classes don't exhibit the same resistance against heat. For example, carbohydrates transform into furans at temperatures at which lignin polymers are only cleaved and only some side chains are released as CO2 or water (Sharma et al., 2001; Sharma et al., 2004). Since different biomolecules have different stable isotopic ratios, this behavior can result in the shift of the ratios for the whole sample. We have now added these useful citations to the discussion section as they are very useful to support the arguments for why we see the pattern of C and N loss with fire.

Author response: We agree that the different sensitivity of organic compounds to heating might also explain the shift in isotopic composition. we have now elaborated on this idea in Discussion (P15, L13 – 15, P16, L7 in the first version of the manuscript).

Page 13: I suggest to include FTIR spectra of the unburnt soils which would allow to relate the initial quality of the SOM (most likely affected by the climate and vegetation cover) to changes induced by the fire.

Author response: We had included the unburnt soil but it was not described in the caption of Figure 5. We have added description to the caption of Figure 5 to clarify.

P14, L6: The authors are certainly not discussing the physico-chemical properties of a soil, but the physical (pore size etc.) and chemical (pH, chemical composition etc.) properties.

Author response: We have changed the wording as physical and chemical properties. We have also edited the paragraph to clarify the meaning (P14, L18,20, and P18, L6).

P14, L8: In the reference "Knicker et al." it is stated that fire can lead to increase or decrease of organic C in the soil, depending upon the intensity of the fire and the amount of produced char derived from the vegetation cover and litter layer.

Author response: The reference Knicker et al (2005) was misplaced, it has now been moved to the following statement which where top soil OM increase after fire has been reported (P 14, L 24).

Page 15, line 4: Other studies showed that a considerable amount of N is transferred into the so called Black N (Knicker, 2010; de la Rosa and Knicker, 2011), which would much better explain the decrease of the C:N ratio with temperature (This does not mean that ammonium is not formed, but not all N turns into inorganic forms..)

Author response: We have included black N as a possible explanation (P15, L19-21)

Page 17, line 5: The reference represents a review and does not show studies in which the degradation of lignin and hemicellulose starts at 130°C. However, studies of the same author showing complete removal of carbohydrate at 350°C are (Almendros et al., 1994; Knicker et al., 1996; Knicker et al., 2008).

Author response: The reference is indeed a review, we have updated it with the reference for the original research (Chandler et al. 1983).

As mentioned above, I have problems with the part Climate Change implications. For example, the statement of line 16 to 23 on page 18 is very general and has only very few relation to the results of the data. Actually, if the rain-snow transition zone is moving higher, considerable losses will occur due to increased SOM degradation. If less SOM is present, this may give less fuel to the fire (only a mind game...)

Author response: We have edited this section for clarity and to address the concerns of the reviewer.

Page 19: Fires in the range of 250 to 450°C (topsoil temperature) are in the range of low to medium severity fires. The respective mineral topsoil is expected to have temperatures which are still below 200°C. High severity fires are hotter than 510°C and are recognized by the fact that mostly ash remains on the soil.

Author response: We understand the reviewer's point here. But, this particular statement is referring to systems experiencing low-severity fires experiencing medium to high severity fire in the future. The text in this section reads: "Our findings of important changes in soil physical and chemical properties occurring between 250-450 °C are important for recognizing that critical transformations of topsoil SOM are likely to occur when, as a result of climate change, systems that are adapted to low severity fires experience medium to high severity fires."

We appreciate the thoughtful comments from the reviewer. Thank you.

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