Thank you very much for your valuable review of this manuscript. It helped to improve its quality.

General comments: The paper deals with actual problem of fire impact to soil organic matter and soil water repellency. The paper brings interesting information on duration and post-fire elimination of soil water repellency in association with measurements of soil colour and organic matter content. Thus the paper is aimed to existing gap of knowledge on the post-fire evolution of soil properties and processes in boreal environment. However some aspect should be improved or explained before to be accepted publication.
Thank you very much

Page 2121, Line 5 – 9: Sampling must be characterized clearly (sampling times, sampling depths, number of samples at each plot).
We added

Line 7 and elsewhere in the text: The term "composite sample" is not appropriate. It is common soil It is common soil sample where soil was passed through a 2-mm-sieve.
We changed "composite sample" to "fine earth"

Line 24: Probably you mean "aggregate size"
Yes. We changed

Page 2122: The literature review presented on this page and partly also on the page 2123 can be shortened because many citations are too general and vague, without direct relationship to the subject of the paper.
We shortened it

Give information on the relationship between SOM and Munsell colour properties (hue, value, chroma) as well as on the methods of soil colour measurement used in the reviewed literature.
We added this information

Lines 22 – 29: Give more detailed information on the present knowledge of changes in SOM associated with fire and heat impact (e.g. Atanassova and Doerr, 2011). Especially changes in SOM which are related to the increase of SWR (volatilization, condensation, structural changes, conformation changes, etc.).
We added

Page 2124, Lines 3-17: As you stated, it is well known that fire can induce soil water repellency. However, post-fire changes in SWR are not well understood. In the literature review you should emphasize just this aspect. Doerr et al. (2009) stated that more detailed studies are needed to determine (i) the duration of fire-induced SWR in different vegetation types and (ii) the relative roles of physical, chemical, and biological factors in breaking down post-fire SWR. Your paper looks to be focused just on this gap of knowledge! Some papers have been published also on the soil moisture threshold for the loss of SWR. Existing information should be mentioned also in your review (e.g. Doerr and Thomas, 2000; MacDonald and Huffman, 2004).

Thank you very much for this input. It was important to raise the relevance of this contribution

Line 26-27: The aim of the paper should be closely related to the gap of knowledge identified in the literature review.
We changed

Page 2125, Lines 18-21: Procedure of the color determination must be described in detail.
We described
In the cited paper (Ketterings and Biigham, 2000) the Munsell color charts were not used.

We deleted

Line 26 and elsewhere in the text: The term "composite sample" should be reserved for the samples prepared by mixing of soil samples from a number of discrete subsamples (from different plots). This is not the case of your samples.
We changed "composite sample" to "fine earth"

Page 2126, Line 7: Substitute reference "Mataix-Solera et al. (2013)" with Doerr et al. (2005). (Doerr et al., 2005: Effects of heating and post-heating equilibration times on soil water repellency. Australian Journal of Soil Research, 43, 261-267).
We added

Line 22: I am not sure if ANOVA test is the best choice for WDPT values. Even better can be use of some non-parametric test. In such case, WDPT values need not be transformed.
In this new version of the paper, the data were analyzed using non-parametric methods

Page 2127, Line 9: The formulation "Soil colour was significantly darker (lower Munsell chroma value)" is unfortunate. You have evaluated Munsell chroma only. But both, Munsell value and Munsell chroma have been reported to decrease with soil heating (in literature).
Thank you very much. We corrected in this new version of the manuscript. We analyzed the Munsell value, not the Munsell chroma. We changed this throughout the manuscript.

If Munsell hue is the same in all samples, Munsell value is a measure of soil darkness (a smaller value is darker than a larger value) while Munsell chroma can be considered as a measure of soil colour (a smaller value is less colourful while a darker value is more colourful). If possible you can evaluate both parameters in this paper.
We agree with your suggestion, however, in this study, we analyzed only soil value

Pages 2129 – 2130: I am not sure if the transport of hydrophobic substances and charred material into deeper horizons can be so dominant process, especially in the silt loam soil. Intuitively, a leaching of water-soluble nutrients (ions) is a different process compared to translocation of insoluble hydrophobic substances and charred materials. Parts of discussion and references which are not related to your results could be removed (e.g. nutrient availability and leaching, effects on biodiversity). The significant changes of WDPT values occur mainly in first 5 months after the fire. But visually, the changes of Munsell chroma and SOM content (in the burned soil) in first 5 months after the fire (Figs. 1 and 2) do not support your statement about a crucial role of ash leaching for the WDPT decrease.
Thank you very much for your comment. We agree with it. The transport of hydrophobic substances is different from the leaching of ions. In this new paper version we removed the parts not related to our results. We also provided a deeper discussion about the changes of Soil Value and SOM and the implications on SWR decrease.

Page 2131, Line 13 - 16: Why do you think that impact of fire on soil (surface) was minimal? What was the soil moisture? Is it possible to distinguish the SWR induced by heat and condensed hydrophobic substances from the SWR caused by admixture of ash?
We hypothesized that the the heat produced by the fire was minimal because the soil moisture was the very similar to that analysed in a previous report. We did not add the results of soil moisture to this study because we do not have data from 7 and 9 months after the fire. We added the soil moisture data from the immediate period after the fire in the discussions. In this study, we could not distinguish between the SWR induced by the heat and by the ash admixture.

Page 2132, Lines 25 – 26: There is no reason for the conclusion about slower leaching of hydrophobic substances from the fine fractions. The fine fractions were the most hydrophobic immediately after the fire

when the SWR was not affected by leaching. Thus, it is normal that in this fraction WR will persist for a longer time than in the fractions with much lower initial WDPT values.

Thank you very much for your comment. It is true that the leaching is not different according to the different soil aggregate size. However, according to previous studies, the microbiological activity is higher in macro compared to micro aggregates. In the new manuscript version we provided a discussion about this process.

Page 2133, Lines 28 – 30: How nutrients leached from ash and soil may contribute to the rapid reduction of SWR?
We deleted this part.

Pages 2131 – 2133: At least it is necessary to take into account other processes possibly involved in the WDPT decrease during post-fire period. Water repellency induced by burning may be eliminated by a series of wetting and drying cycles (Doerr et al., 2009).
Thank you for your suggestion. We added this concept to the manuscript.

SWR elimination is often associated with soil moisture exceeding a "critical soil moisture threshold" (demarcating wettable and water repellent states). In addition, Doerr and Thomas (2000) showed that after wetting, SWR is not necessarily re-established when soil become dry again. Decrease of SWR can be related to the changes in the spatial organization of amphiphilic molecules (Horne and McIntosh, 2000; Huras and Schaumann, 2006; Roy and McGill, 2000).
Thank you very much for this suggestion. We added this to our discussion.

The fast breakdown of SWR can be also attributed to greater biological activity and to restoration of vegetation.
We added this information in the new version of the manuscript.

Table 1: The soil type "Eutric podzoluvisol" is not included in the WRB (2006) system. Your soil probably belongs to Albeluvisols.
Changed