Review of SE-2013-63.

Title: Short-term spatio-temporal spring grassland fire effects on soil colour, organic matter and water repellency in Lithuania

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First of all we would like to say thank you very much for your review. We are deeply and sincerely appreciative of your deep and objective review of our manuscript. Your comments were important to help clarify several aspects and increased substantially the manuscript quality.

The manuscript falls within the scope of SE and the research carried out is of good quality. However, I have three main concerns:

The quality of English grammar and style must be deeply revised by a native English speaker
A native speaker has helped us revise the English in this version of the manuscript.
The paper was reviewed by an English native speaker.

I am not in agreement with the statistical approach carried out. I think that nonparametrical tests should give more solid results. Variable transformations to fit the normal distribution are useful when most variables are normally distributed. But I am not sure this is useful when most (in this case all) variables are not normally distributed. Especially, I have serious concerns with the analysis of color data.
Thank you very much for your comments. In this version of manuscript we used non parametric methods. In relation to the soil color data, previous studies used the same method (Scharenbroch et al., 2012)[1].

I am not convinced by the explanations about changes in soil organic matter content and soil water repellency as a consequence of leaching of organic substances (under the environmental conditions of the study area). May be right, but more solid support is necessary.
Thank you very much for this comment. We fully agree with this and we rearranged this section according to your suggestion.

[1] Reference in the paper

Detailed comments

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Line 2 — You must add 1-2 lines stating why your research is necessary.
Remove "first".
Added and Deleted

Line 5 — Were soil samples collected regular or randomly in the 400 m^2 plot? What depth?
Requested information added

Lines 6-9 — You need to say that you collected 250 soil samples before talking about "the 250 samples".
Perhaps you mean that you assessed soil water repellency in fine earth (< 2 mm) and aggregate sieve fractions 2-1, 1-0.5, 0.5-0.25 and < 0.25 mm) from 250 soil samples.
Added and substituted

Line 9 — You do not mention "WDPT" again, so remove the abbreviation.
Deleted

Line 10 — At what depth colour changes were observed? If only at the surface, you cannot talk about incorporation of ash/charcoal.
Thank you very much for your question. We analyzed the top soil color (0-5 cm). The ash and charcoal produced after the fire are responsible for soil darkening. To avoid misunderstandings, we can delete "into soil profile". The ash and charcoal with the time are mixed and can be incorporated in the first top soil centimetres.

Line 13 — Substitute "Soil water repellency (SWR)" with "SWR".
Substituted

Line 13… — I suggest using "more severe SWR" than "higher SWR".
Changed

Line 15 — Substitute "aggregate fractions" with "aggregate sieve fractions".
Substitute "the SWR" with "SWR".
Substitute

Line 16 — Until 5 months after fire or during the first 5 months after fire? Not the same.
It was substituted by "during the first…"

Lines 16-17 — Move "fractions" after "finer".
Added

Line 17 — Reading only the abstract I find serious problems in this statement. Decreased severity of SWR cannot be attributed to leachability of organic compounds only because of the reasons you mention. If after a burn, I think that the composition of organic substances volatilized during burning and now coating aggregates must be homogeneous and probably there are not differences in leachability. Many other reasons may exist.
We rearranged this section

Lines 19-21 — Please, re-write this sentence.
Rewritted.

Lines 21-23 — Of course. Any consideration about other factors (fire severity, temperature or previous wettability).
Thank you very much for your comment. In this work we did not assess other potential causes. The fire severity was low, the temperature of the fire and previous wettability was not assessed since. We did not study a controlled fire. We add some more explanations. The increase of SWR may be related to the incorporation of hydrophobic ash.

Line 23 — Substitute "repellent" with "hydrophobic" (water-repellent, in any case).

Substituted by "hydrophobic"

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Line 8 If very recent, climate change effects on fire regimes and role in boreal ecosystems are difficult to outline. Delete "recent". May be you mean recent land-use changes and fire suppression policies, but not "recent climate change". Re-write, for example: "However, climate change, recent land-use changes and fire suppression policies may have important implications on the fire regime, fire severity and the role of fire in boreal environments".

Changed

Lines 10-13 I have serious concerns about boreal grassland decline in absence of fire. Is the distribution of boreal grassland and forest conditioned only by fire or are other agents playing their own role? I have deeply read Bond et al. (2005) and have not found a statement to strongly support this, except in a wide sense or for tropical forests and some other ecosystems. In fact, they wrote that fire- dependent ecosystems such as grasslands or savannas are of much greater extent in tropics and southern hemisphere than the temperate and boreal

areas. And more:

> "*The third major fire-prone biome, boreal forests, are often dominated by fire-adapted trees with serotinous cones that release seeds only after crown fires (Johnson, 1992; Keeley & Zedler, 1998). However, by our measure of fire dependence, the dominance of the gymnosperm tree growth form does not depend on burning according to the simulations. If fire dependence were measured by changes in species composition, rather than broad functional type, large areas of boreal forest (and other ecosystems?) might be considered 'fire-dependent'*".

It is nonsense to suppose the regression of boreal grasslands in absence of fire, when boreal forests are strongly fire-dependent. In addition, Schindler and Lee (2010, DOI: 10.1016/j.biocon.2010.04.003) suggested that increased fire risk is expected in boreal ecosystems under climate warming. So, I suggest deleting this lines.

We delete it

Line 15 Re-write: "on soil properties from boreal grassland ecosystems (Pereira et al., 2013a, c)".

Re-wrote

Line 16 Move this statement to the beginning of the paragraph.

Moved

Line 17 Remove "the" before "studies"
Re-write: "grassland soils"

Removed and Re-written

Line 25 The effect of fire on moisture is strong but ephemeral. I suggest substituting with "water holding capacity" or "water balance".
You repeat "organic matter" and "organic matter consumption"; "ash" and "ash nutrient input" (what is the first ash for?)

We substituted "moisture" by "water balance". We removed "organic matter consumption". Ash erosion and nutrient input are considered indirect impacts of fire on soil properties.

Line 27 I am not in full agreement with this statement. Read jumping the parentheses: "After fire, the degree of direct and indirect impacts on soils […] have consequences for the complex spatio-temporal nutrient distribution and availability for plant recovery". Plant recovery may be difficult, easy or even improved depending on the type of plants and strategies. I suggest re-writing this sentence and the following (in the next page).
Check "have" (has?).

Thank you for your comment. We rewrite "After the fire, the degree of direct and indirect impacts on soils […] have consequences in the spatio-temporal nutrient distribution."

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Lines 5-9 Delete "Fire can change soil colour." And re-write: In fires of high severity, temperatures increase soil redness, especially at temperatures of 300–500 ^{o}C (Terefe et al., 2008) or above 600 ^{o}C (Ketterings and Bigham, 2000; Ulery and Graham, 1993), which is attributed to the destruction of the organic matter and increase in iron oxides such as hematite (Terefe et al., 2005). In contrast, low severity fires…".

Thank you for the comment, we deleted the sentence and rearranged the sentence according to your suggestion.

Line 10 Also in the soil surface and the surface of coarse particles.

Added

Lines 11-14 It seems that all the knowledge on burned soils is about colour. Can you re-write the sentence?

We rearranged

Lines 14 … The conexion between the sentence starting with "In addition…" and the first part of the paragraph is not clear. What were Eckmeier et al. studying?

We put the sentence in the next paragraph and we re-wrote "studies have been carried out about fire effects on soil colour in comparison to unburned plots". Eckmeier et al. studied the effects soil lightness of colour. We added in the text

Lines 20-21 Why? What do you want to use burned soil colour for? As an index of what?

We aim to study the temporal effects of fire on soil value. We want to know the degree of the impacts and if this low severity fire and if had implications in SOM and SWR. To our knowledge, no previous studies were carried out about temporal effects of fire in soil value, and represent and advance to the current knowledge.

Line 28 Not all SOM, but the major part. A small part remains as black carbon.

We rephrase it.

Lines 28-29 If all SOM is volatilized, it does not decrease considerably. It disappears.

We deleted this part.

Line 29 Only topography? What about rainfall? If rainfall is nor present or is too weak, SOM will not be eroded.

Also, in this case, "SOM" is not very accurate. I suggest "organic residues in the soil surface".

We added rainfall. We maintain SOM, because the study of Novara et al. 2011, was about SOM.

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Line 3 "It is"…

Changed

Lines 3-5 This is not completely exact: depending on fire severity, burning can induce soil water repellency in previously wettable soils, or enhance it in some cases. But fires with severity high enough to induce SWR in wettable soils may have no effects when soil is water repellent previously to burn (see Doerr et al., 1998, for example).

We describe this idea in the lines 11-15 (Old paper version)

Lines 8-10 Here and through the text. When you cite several references together, sometimes you have ordered them by date, in other cases alphabetically. Please, use homogenous criteria.
According to Solid Earth Guidelines references are ordered first alphabetically then chronologically. We followed this principle.

Line 24 Property loss? What does this mean? Soil properties?
We mean, houses, human goods etc.. We changed to "goods loss".

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Lines 3… The fire? I suggest: "In April 15th 2011, an area of XX ha near Vilnius (Lithuania) was affected by a wildfire. The burned area is located at coordinates 54°42'N and 25°08'E and elevation is 158 m.a.s.l.".
Add the area: the difference between 20 and 25 ha is a considerable surface.
We changed according your suggestion. In relation to the area, was the information given by the responsible authorities.

Line 5 "Fire severity was…".
Changed

Lines 6-7 "Three days after fire"…
I suggest avoiding the use of "we".
The first suggestion it is equal in the text. We changed "we", here and elsewhere in the paper

Line 8 How many points? The number varies if you consider the sampling points are in the center of each cell of the grid or you include the borders.
We show this information in the lines 14-15 (old paper version). The sample points considered the grid borders

Line 9 "… immediately after burning and 2, 5, 7 and 9 months later".
Changed

Lines 10-13 This information should be moved to the objectives. Perhaps some rewording is required.
Changed

Line 12 You mean the effects of low severity fires.
Yes

Line 20 What do these references support?
These references support the fact that soil color (in this new version the value) is currently analyzed with the Munsell color chart.

Line 21 Substitute "Soil organic matter" with "SOM content".
Changed

Line 23 "to remove".
Changed

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Line 7 I suggest Doerr (1998), better than Mataix-Solera et al. (2013) for this.
Changed

Lines 10-13 Substitute "Water Drop Penetration Time" with "WDPT".
Doerr (1998) considered 3600-18000 and >18000 s classes. If you did not recorded WDPTs longer than 18000 s, tell us. If you only considered >3600 s as the final class, tell us (what is not "according to Doerr (1998)").
Changed
We did not record SWR higher >3600 s. The maximum was 2777.1 s. Thank you very much. We will add the classes that miss from Dekker and Ritsema

Line 15 What data?
You have just said that "data were considered normal and homogeneous at a p > 0.05" and then "Original data did not follow the normal distribution and heteroscedasticity". Re-write.
Thank you for your comment. When we write "data were considered normal and homogeneous at a p > 0.05" we mean that it is a general requirement for the data be considered normal. We changed to " In this study original data did not follow the normal distribution and heteroscedasticity"

Line 17-18 Ok. Soil chroma was considered normally distributed after transformation.
But is this really valid or is it an artifact? Correlation does not imply causation. It is used for description, but I do not think it can be used as just another soil variable.
I am absolutely not in agreement with treating soil chroma as a continuous variable. Chroma is not uniform for every hue at every value, and perhaps useful in this case if all observed colors were the same hue and value.
Thank you for your comment. You allowed us to correct an important question. We want to refer to Soil Munsell "value", not Chroma.
It was used also to calculate the Pearson correlation coefficient with the other variables (used SQRT transformed data). In this work the Hue of the samples was 10YR, so the value was uniform. Previous studies treated Soil munsell value and chroma statistically (Scharenbroch et al., 2013). Our methodology is not new.

Line 19 Squared root transformation of what? SWR? Or is it WDPT?
WDPT data. We changed. In this new paper version we used non-parametric analysis of data

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Line 9 Chroma has not relation with darkness.
Here and through the text (Figure 1, for example). What do you mean with "Munsell chroma value"? I think you mean chroma. Avoid using "chroma value", as "value" is another Munsell parameter.
Thank you very much for your comment. You are correct. Munsell Value is related to the soil darkness/lightness. As mentioned before we analyzed "soil value"

Lines 11-12 I am not in agreement. If you have not found statistically significant differences among average chromas from different months (all are "A"), you cannot say that it decreased with time.
Deleted

Line 5 Separate this section in one for colour and another for SOM content.
Done

Lines 12-14 Only one case and one-unit chroma variation? Again: isn't this an artifact?
The unit of Soil value in the graph is an average of all the samples in the "unburned" (25 samples) and "burned plot" (25 samples) for each sampling period. The statistical treatment of Munsell value and Munsell chroma, was already used in previous works (Scharenbroch et al., 2013). We think that no.

Line 15 Substitute "SOM" with "SOM content".
Done

Line 16 Substitute "Soil organic matter" with "SOM content".
Done

Lines 16 … What are the differences? Please, provide mean ± SD values.
In this new version of the paper the graphics were done using ± SD values.

Lines 19-21 What trends? There is no trend in the control plot (all groups are "A"). In the burned plot, SOM content first increases(B immediately after burning and 2 months later) and no further changes.
We Deleted the sentence

Line 23 Substitute “The SWR” with “SWR (measured as the squared root of WDPT)”.
In this new paper version we used non-parametric methods

Lines 23-25 and first lines of next page. Difference between treatments in the burned plot? What treatments were applied in the burned plot? Do you mean between dates? This is confusing. I think you are talking about different treatments, but you mean control and burned sites, which are not treatments. Keeping unburned is not a treatment you applied (neither burning, I hope so).
Here and through the text, try to revise this.
Thank you very much for your comment. We changed “treatments” by “plots”

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Line 4 “Sieve fractions”.
Done

Lines 6-7 You have not defined what “high SWR” is. You have defined WDPT classes (and later, you have used squared root of WDPT).
Try to be more accurate. I think you write SWR in many cases, but you mean WDPT (or squared root of WDPT).
We will avoid using “high SWR”. In this new paper version we used non-parametric tests.

Lines 5-8 Specify sieve fractions at each case. Sometimes you use only “finer fractions” and the reader does not know what fractions you mean. Do it as in lines 7 or 10, or simply cite the sieve fractions.
Done. We have corrected according the examples of line 7 and 10.

Line 12 Delete one of the repeated “afters”.

Done

Lines 13-14 “Observed in the SWR finer fraction”?
We changed

Line 17 Substitute “studied period” with “experimental period” (“studied” is repeated in the same sentence).
Done

Line 24 Not in “g”.
Thank you very much. In the finer fraction (<0.25 mm**), the major % was classified as “low”. We corrected it.**

Line 25 After or immediately after fire? “After” means all your experimental period.
In this new paper version we named Immediately after the Fire (IAF)

Lines 26-27 Not especially in the finer fraction. Why especially? It occurs in the two finer fractions. Can you say the composite samples were especially wettable?

We rearranged the sentence. Thank you very much.

Line 28 You have not defined “wettable”.

We defined

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Line 3 Substitute “significant at a p<0.05” with “significant (p<0.05)”.
Changed

Lines 11-12 “Especially” repeated in a short time.
We deleted the first “Especially”

Lines 2-8 Substitute “correlation between XX vs. YY” with “correlation between XX and YY”.
What are the coefficients? Tell us, even if they are in tables.

We changed. The correlation coefficient results are too many. We think that maybe will increase the confusion to the reader. The table can make a better resume.

Line 6 Repeated “the correlations”. Re-word.
We rearranged

Line 13 Substitute “as in the present one” with “as in our experiment”.
Done

Line 18 Soil colour or only chroma?
We changed is soil munsell colour value

Line 20 How can rainfall dissolve black ash (organic residues not completely burned) so reducing soil darkness with time? Leaching only affects soluble minerals. In any case, soil darkness/brightness is measured by Munsell value, not Munsell chroma.
Thank you very much for your comment. The rainfall cannot dissolve black ash. We rearranged the sentence. The incorporation of burned material into soil deeper profiles can reduce the their darkness.
As we mention before we analyzed the Soil munsell “value”.

Lines 21-24 But you have not determined soil nutrients. So, this (true) statement is not necessary here.
We deleted

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Lines 1-2 Black colour does not increase temperature, sun radiation does. Re-write “decreases albedo facilitating the temperature rise during day hours”.
We rearranged

Line 3 Remove the comma before “and”.
How does the black ash cover change microbial activity and diversity?
Removed and answered in the new paper version.

Line 5 Can you provide any data on the rainfall amount after fire? At least, explain if it was a rainy period.
We added data about the rainfall

Line 7 Is this area the same study area in Pereira et al. (2013a)? In this case, you mean vegetation cover. If you only write “vegetation”, many other factors may be implicit (diversity, species composition, vegetation structure…).
Thank you, we changed.

Line 10 Vegetation cover.
Done

Line 11 In Boreal grasslands, effects on biodiversity may vary with fire recurrence. Your assumption is valid only for low recurrence of fire.
Thank you very much for your comment. To avoid misunderstandings, we deleted the assumption of “effects on biodiversity, since to our knowledge, no studies were carried out about fire recurrence in boreal grasslands

Lines 15, 17 and 20 SOM content.
Changed

Pages 2130 and 2131

According to the measured acidity (pH 7.2, Table 1), I have serious concerns about if organic matter may be leached. Leaching only affects soluble organic substances, which lose solubility when pH increases. Leaching of organic compounds is negligible at pH 7.2. This is a key point of your work and needs much more explanations. Especially when leaching seems to affect more strongly certain sieve size fractions.

Are you sure that leaching is the key here? It is a flat area, so there is no erosion risk. But changes in nutrient concentrations and increased root activity may affect (or be correlated with) microbial activity. What do you think? If soils were wettable before burning, I should assume that hydrophobicity is concentrated in volatilized and later condensed organic substances now covering the surface of aggregates.

In my opinion, microbial activity may destroy hydrophobic substances coating aggregates in the burned soil with time, and the intensity of this effect may be conditioned by the size of aggregates, as relations S/V vary exponentially with aggregate size.

Thank you for your valuable comments. We have rearranged this section in the new paper version and add some explanations about SOM leaching, microbiological activity influence in SOM mineralization, root development and invertebrates impact on SWR reduction.

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Line 6 Re-write: “that, in water-repellent soils, the finer fraction…”
Done

Line 10 Delete the “>” signe.
Deleted

Line 15 “Measurement periods” do not exist in your experimental design. Substitute “measurement periods” with “sampling dates”.
Done

Line 25 Consider previous comments and remove or support this statement.
Done

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Line 4 Only sand-sized aggregates? I suggest writing that fire may decrease the average aggregate size or decrease the proportion of sand-sized aggregates.
Chanbed

Line 8 “Particle size”.
Changed

Lines 8-9 You are putting together particles and aggregates. The processes you are citing here are very different.
Particle size may change after high temperatures by melting. In contrast, changes in aggregate stability increases the proportion of finer aggregates (as a consequence of fragmentation of coarser aggregates).
In any case, discussion here about changes of particle and aggregate size is speculation, as you have not studied it.
The important issue here: how do these processes (if present in your experiment) affected the properties you have studied?
We rearranged this part, according your suggestions. Thank you for the valuable comment

Line 16 SOM content.
Changed

Lines 16-19 I'm not convinced.
However, I think that a non-parametric statistical approach should have been more interesting.
Have you determined WDPT in dark and white ash?
Thank you for the comment. The fire did not have direct effects on soil (observed by the similar Soil moisture, observed in the work of Pereira et al., 2012b). Thus we assume that the increase of SWR can be attributed to hydrophobic ash.
The WDPT was determined in black ash, as mentioned in the work of Pereira et al. (2013b).

Line 21-22 This citation concerns unburned soils.
We mentioned in burned and unburned soil

Line 26 Better use “water repellent” for soil and “hydrophobic” for substances.
Changed

Table 1	The first row is separated by a line. Remove it. You have analyzed 25 samples. Can you provide ± SD? "Silt loam" according to…? USDA? I suggest moving the notes at the foot to the head of table. **The first row line was placed by Solid Earth Editing. The rest is done.**
Table 2	In the main text, you make no difference between WDPT classes more than wettable, low, strong and severe WR, with no reference to other classes (6-10 s, for example). I suggest grouping WDPT classes in wettable (< 6 s), low (6-60 s), strong (61-600 s) and sever WR (601-3600 s). **Changed**
Figures 1-3	A suggestion: colour figures increase citations. Don't ask me why, but it occurres. **Changed**
Figure 1	After fire or immediately after fire? If no significant differences are observed among chromas from different dates in control plots after ANOVA, post-hoc has no sense: remove all the "A"s in the control series. As in a previous comment: chroma or value? **Immediately after the fire (IAF). We removed the letters. It is "value"**
Figure 2	After fire or immediately after fire? If no significant differences are observed among SOM% from different dates in burned plots, remove all the "A"s in the burned series. Also, delete the "a" for dates 5, 7 and 9 months clusters. **Changed**
Figure 3	After fire or immediately after fire? Remove "a"s in 5, 7 and 9 months clusters. **Changed**
Figure 4	Is it separated because of formatting the SED manuscript? Please, check that figure has been provided complete. **The figure was separated because did not fit totally in one page**