Author's responses

Interactive comment on "Vertical mobility of pyrogenic organic matter in soils: A column experiment" by Marcus Schiedung et al.

Anonymous Referee #1

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You find our final author responses written in blue.

General Comments
This manuscript provides information on the vertical mobility of pyrogenic organic matter in soils of contrasting nature. The study uses a column experiment where isotopically labelled char gets added to soils collected at different depths. The movement of both added PyOM and native soil is then traced through the soil column. The study is very relevant, well designed and impeccably executed. The MS is well written and provides a pleasant reading. It has been a few years since I did not enjoy reading a first submission as much as I have with this one. I have little doubt that this work will prove quite useful for researchers studying PyOM dynamics and ecosystem carbon cycling.

We are grateful and appreciate the positive feedbacks on our submitted manuscript.

There are just a few aspects where I would appreciate that the authors provide further critical discussion to make this contribution an even more useful one. These are as follows:
The PyOM used in this study derives from ryegrass. I can understand why such fastgrowing precursor biomass was used to produce PyOM in this labelled experiment. However, it is unavoidable to think that the resultant pyrogenic material will be of a highly contrasting physico-chemical nature compared to those derived from woody vegetation. Therefore, these distinct characteristics may greatly affect the mobility of the various PyOM produced. As such, I recommend the authors to include a paragraph in the discussion showcasing the potential limitations and applicability of the results obtained in this study. The results obtained here may be directly applicable in agronomic studies using grass-derived biochar. However, the mobility of PyOM in charcoal generated during wildfires affecting woody vegetation might be different from that observed in grass-derived PyOM.

We agree with the reviewer that this is an important aspect, which needs to be included to indicate the limitations of this experimental set-up. The ryegrass in our experiment is mainly used because it is much easier to produce with high label than wood. A lower label however, would constrain the tracing of the mobilized PyOM because of its quite low mobile fraction.
In the present manuscript we already mentioned this limitation in line 302-303 by stating: "It needs to be noticed that we only included one type of PyOM (ryegrass derived and produced at 450°C) which constrains general assumptions". However, we will follow the suggestion and highlight this important limitation again in the discussion and include it further in the conclusion. In general, grassland systems represent a major source of fire derived organic matter globally, so the use of grass material is still legitimate.

The existence of fluctuating levels of moisture in the soil is just natural. Please briefly include an statement about how soil drying and wetting events may cause the mobility of PyOM potentially diverge from your observed results obtained under saturated conditions.

We agree that unsaturated flow conditions are highly relevant for the mobility of PyOM under field conditions. Based on the findings from our experimental set-up, we cannot provide any statement how the mobility would be influenced under unsaturated conditions.
We found a quick interaction of the PyOM with the mineral phase even under saturated conditions in our experiment. Unsaturated conditions and unsaturated water flow could potentially increase the contact time of PyOM and the mineral phase and thus influencing the interaction with each other but it could also increase the preferential flow and thus increase the downward movement with less interaction with the mineral phase. This would also highly depend on the texture and aggregation of the soil. Therefore, this requires more research and we stated in line 392-393 of the current manuscript: "This quick mineral interaction will control the long-term stability of PyOM in soils and requires more research under unsaturated and field conditions."
We also discussed that our experimental approach has limitations that prevent a direct transfer to field conditions due to the continuous saturated flow conditions in our experiment and in line 408-409 of the current manuscript, we state: "In addition, we did not include unsaturated flow conditions which would resemble the mobility and retention under field conditions".

I appreciate the addition of fresh PyOM in the subsoil to get deeper mechanistic understanding of the dynamics of PyOM in the soil. However, besides high erosion rates and subsequent deposition, it is just hard to envisage this happening in a real setting. Not that this is a problem, you might just want to make a brief mention of it.

We agree with the reviewer that the input of fresh PyOM in subsoils maybe not be common under field conditions. We stated this also in the beginning of our introduction in line 34-36 of the current manuscript: "Mass transport of PyOM mainly occurs during the first rain event after a fire, resulting in a translocation and re-deposition within the landscape and eventually in a PyOM burial at depositional sites (Abney et al., 2019; Cotrufo et al., 2016; Rumpel et al., 2015)." This burial may happen before the PyOM is significantly oxidized on its surfaces.
We further address this, when we discuss the dynamic of mobilized PyOM in section 4.2. by stating in line 323-323: "This may further cause an underestimation of the vertical PyOM transport from depositional landscape positions after redistribution following an initial lateral mass transport (Rumpel et al., 2015)." We will highlight again that depositional sites could represents location were PyOM may have entered subsoil regions and add a sentence in the discussion.

I am very satisfied with the methodology employed, as well as the results, tables, figures and derived conclusions. I congratulate the authors.

We highly appreciate the reviewers positive feedbacks.

Specific Comments
Introduction: This is a short but well accomplished introduction.

- Lines 55-57: The authors state that 'the chemical composition, physical properties and the particle size control the mobility in soils and the interactions of PyOM with the soil mineral phase which further depend on other soil properties'. While this is true, it is important to also consider the preferential transport of fine PyOM derived from grass biomass reported elsewhere (e.g. Saiz et al, 2018). This is an important aspect considering the fine, and most likely, light nature of the PyOM used in this study, which undoubtedly will greatly affect its initial mobility after formation. (Reference: Saiz et al. 2018. Preferential Production and Transport of Grass-Derived Pyrogenic Carbon in NE-Australian Savanna Ecosystems. Frontiers in Earth Science 5, 115. doi:10.3389/feart.2017.00115)

We appreciate that the reviewer provided further literature to highlight this important aspect. We also agree that this needs to be clearly mentioned and we will improve the mentioned section. Saiz's reference will be added to the manuscript.

Materials and Methods:
- Line 82: The values presented in Table 1 appear to have been produced by you. If that was the case, state the methodology used to obtain them.

All these values were measured by us. The methods, which were used for the bulk soil measurements, are given in section 2.4. To clarify this, we will refer to this section in the caption of table 1 and change to: "Table 1: Soil texture, total organic carbon (TOC), δ13C, pH, electrical conductivity (EC), oxalate extractable Fe(o) and Al(o) and density fractions (free particulate organic matter, fPOM and mineral associated organic matter, MAOM) for the topsoil (0-10 cm depth) and subsoil (40-60 cm) of the loamy and sandy soil (± 1 SE). See section 2.4 for the used methods."

- Line 95-on: If possible, please provide more information about the PyOM produced (i.e. O/C, H/C, etc.). This will make your work more inter-comparable with other studies.

We are currently accessing the O/C and H/C ratios by additional CNHS O analysis and we will include these parameters in Table 2.

- Lines 98-103: These lines describe how PyOM was produced and, the oxidation treatment that some of those samples underwent. Please try to re-phrase these sentences as I got quite confused with the two oxidation instances that the text makes reference to.

We agree that the current description may not be clear and we will rephrase to the following: "Artificially altered PyOM was produced by chemical and heat accelerated oxidation presented by Cross and Sohi (2013); in brief, 1 g of C was oxidized with 0.1 mol of H_2O_2 at 80°C for two days. The samples were gently shaken five to seven times a day to ensure a homogeneous reaction."

- Line 99: Table 2 shows what it seems to be a large variability between batches that have been treated in similar way. The authors may want to include some comment about it. But most importantly, if I understand well, the sandy soil gets added PyOM which is up to 10% higher in its C content compared to the PyOM that gets added to the loamy soil. Would this discrepancy not create an artifact in the behaviour of PyOM in both soils? Please critically discuss this aspect.

We noticed that a variability between the produced ryegrass batches exist. Such variation is hard to explain since the growing conditions were controlled and similar for all three batches. However, it is possible that the uptake of, for example, silica differs slightly between the batches which would result in variations of the C content. However, all the recovery calculations (total C and 13C) are based on the actual values presented in Table 2 and thus the variability is taken into account.
We agree that this needs to be mentioned in the manuscript and we will include in section 2.2 the following: "It needs to be noticed that the three batches of ryegrass PyOM varied in C content and $\delta^{13}C$. To include this variation, all further calculations are based on the individual values (Table 2)."

- Line 143: Please state the nominal mesh of the glass fibre filter used.

Will be changed to: "…filtered using a glass fiber filter (<0.7 µm)."

Results:
- Lines 205-207: Where can these data (statistics) be seen?

The data can be seen in figure 2. We choose the same scaling between the topsoils and subsoils as well as between the sandy and loamy soil to allow a direct comparison of the percolated PyOM. We will refer to Fig 2 again.

- Line 231: Please check the text: '..more to than the ..'

We will correct this sentence to: "Thus, PyOM addition significantly increased the nSOC leaching for the sandy topsoil the control without PyOM addition, from which 1.7 ± 0.1 % of the initial nSOC were leached ($p<0.01$)"

- Line 249-250: Please re-phrase this sentence.

We will rephrase the sentence to: "After the percolation, 89-96 % of the fresh and oxidized PyOM-C remained at its initial location in the PyOM layer in both soils."

- Line 259: 'The lowest sandy subsoil layer. . .'. Please check this text.

We will specify and rephrase to: "In 4.6-7.0 cm depth below the PyOM layer, the recoveries of oxidized ($p<0.05$) and fresh PyOM-C ($p<0.01$) were significantly higher in the sandy subsoils compared to the topsoil.

Discussion:
- Line 314-315: 'The first flush contributed to the highest export of PyOM from the soil columns and the mobilized amounts decreased with the percolation for all soils'. This sentence is at the beginning of a discussion section. You need to contextualize the 'flush' term a bit better.

We specified the first flush as the first 1,000 l m^{-2} in the result section. We will specify it here again by adding: "The first flush, leached form the column with 1,000 l m^{-2}, …"

-Lines 318-319: In this experiment you had the opportunity to validate the statement about attributing the export of PyOM to mobile pyrogenic fractions directly produced during pyrolysis. Hadn't you?

The production of large quantities of highly labelled PyOM is very challenging. Therefore, we do not have enough material left to determine water extractable fractions with a sufficient certainty.

Technical Comments
- Line 137: Typo in 'form'.

We will correct and carefully check the manuscript again for similar typos

- Line 459: Typo in 'form'.

Same as above

- Line 482: Typo in 'desobed'.

Same as above