

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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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.

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 fast-growing 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.

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.

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.

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

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

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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)

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

Results: - Lines 205-207: Where can these data (statistics) be seen? - Line 231: Please check the text: '..more to than the ..' - Line 249-250: Please re-phrase this sentence. - Line 259: 'The lowest sandy subsoil layer...'. Please check this text.

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. Lines 318-319: In this experiment you had

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the opportunity to validate the statement about attributing the export of PyOM to mobile pyrogenic fractions directly produced during pyrolysis. Hadn't you?

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Technical Comments - Line 137: Typo in 'form'. - Line 459: Typo in 'form'. - Line 482: Typo in 'desobed'.

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