

General comments

With great interest I read the manuscript “Amending solid carbon from methane cracking to arable soils: A sustainable approach to increase carbon storage and heavy metal immobilization?” by Saki, Kostiuk et al. In light of climate change mitigation, there is a great need for both alternative energy sources and soil amendments that could sequester more carbon. The approach described in this manuscript could contribute to both, with solid carbon materials derived as a by-product from hydrogen production. The manuscript focuses on the use of this solid carbon as a soil amendment and how this would affect soil physical properties, ecotoxicology, and heavy metal availability.

While the research is undoubtedly interesting and, and the experiments generally seem sound, the manuscript itself could have been better written and appears sloppy / messy in some places. It lacks context for the audience, and for example information about the upscaling potential of this method. The discussion too closely follows the structure of the results without going into possible relevant mechanisms and the bigger picture. Methods are described that do not appear in the result section (EDTA extractions) or important information is lacking (e.g. which earthworm species), and basic changes in soil properties following amendments (e.g. pH) are not described. The number of figures is on the high side; perhaps some could be moved to an appendix. Recommendations for further research are not provided at all. I suggest a major revision of the manuscript to tackle these issues.

Response: We thank the reviewer for the careful evaluation and constructive assessment of the manuscript. We agree that the manuscript required substantial revision to improve clarity, context, and interpretation. We therefore revised the manuscript throughout to improve readability and consistency, expanded the Introduction to better introduce methane-derived solid carbon materials and their comparison with biochar and graphite, and clarified the rationale for testing these materials as soil amendments.

In the Discussion, we reduced repetition and moved away from a figure-by-figure interpretation. In particular, the previous Sections 4.3 and 4.4 were merged into one section discussing heavy metal concentrations in soil solution and operationally defined plant availability together. The revised section now provides a broader mechanistic interpretation, including ion-specific adsorption, surface complexation, precipitation, redox reactions, pH-mediated effects, and site-specific soil properties. We also added a new section on general applicability and future research needs, addressing the advantages and limitations of SC_{cat} and SC_{plas} relative to biochar and graphite, practical aspects such as application rate and field-scale use, and key knowledge gaps for future studies.

We further revised the Methods and Results to improve consistency, including clarification of extraction methods, amendment application rate, earthworm and springtail test details, and the presentation of figures and tables. Where appropriate, figures were reduced or moved into tables/supplementary material to improve readability. We also revised the conclusions to avoid overstatement and to emphasize that the effects of both novel solid carbon materials are site-, material-, and purpose-specific.

Revision: The Introduction, Methods, Discussion, figures/tables, and Conclusions were substantially revised to improve context, clarity, mechanistic interpretation, and practical applicability.

Detailed comments

Title: The title might not be too appealing to attract the right audience. What about something like: “The potential of a novel sustainable soil amendment for increasing carbon storage and heavy metal immobilization”?

Response: Thank you for the suggestion. We revised the title to make it clearer and more appealing to the target audience while avoiding overstatement of carbon storage or heavy metal immobilization effects.

Revision: The title was revised to: “Assessing solid carbon from methane cracking as a soil amendment: effects on soil hydraulic properties, soil biology and heavy metal mobility.”

L.20 (and later in methods): the application dose of 40 tons/ha is on the high side when compared with biochar, where often 10 or 20 tons/ha is used in research because in practice doses are even lower. In the methods, the choice for the dose needs to be better justified.

Response: Thank you for this comment. We agree that 40 t ha⁻¹ is relatively high compared with many practical biochar application rates, however, this is still within the range of biochar application rates in many studies (e.g. see the meta-analyses of Ye et al., 2020). We therefore added a clearer justification in the Methods. The rate was selected as a high-dose scenario to ensure detectable changes in soil hydraulic properties and contaminant mobility within the short- to medium-term laboratory experiments. We also clarified that this dose should be interpreted as a screening dose rather than as a general recommendation for field application.

Revision: The Methods section was revised to justify the 40 t ha⁻¹ application rate and to clarify its experimental purpose.

L. 30-32: from the above description of the results, I would conclude differently, e.g. because you write that SC_{cat} had almost no effect on the studied biological properties.

Response: Thank you for pointing out this inconsistency. We agree that the previous final statement did not follow logically from the preceding results, particularly because SC_{cat} showed almost no effect on the biological endpoints. We revised the abstract to align the interpretation with the results. The revised text now states that SC_{cat} showed minor effects on the assessed endpoints and comparatively limited ecotoxicological effects under the tested conditions, but also limited benefits as a soil amendment. We also revised the interpretation of SC_{plas} to emphasize metal- and site-specific effects and to avoid overstatement.

Revision: The abstract was revised to provide a more balanced and internally consistent conclusion on SC_{cat} and SC_{plas}. “The solid carbon from catalytic pyrolysis (SC_{cat}) had no significant effect on the biological properties studied and showed no significant influence on plant-available water, although it moderately increased hydraulic conductivity in the sandy soil. ”

L. 48-50: here I feel context is lacking. The idea to use these materials as a soil amendment must come from somewhere. What chemical/physical properties do they have that suggests they can be added to soils? Later on (L.53-54) you write that the materials are expected to behave as biochar, but why? And similarly, why is graphite also chosen as a reference material? (L. 78) In other words, the materials need to be introduced much better and the context better provided. The audience (the soil science community) needs to know what these novel materials are and how they compare to things they already know (such as biochar).

Response: Thank you for this comment. We agree that the introduction needed more context for the soil science audience. We revised the Introduction to better introduce methane-derived solid carbon materials and explain which material properties may make them relevant as soil amendments, including stability, particle size, surface area, porosity, hydrophobicity, and surface reactivity. We also clarified why biochar and graphite were selected as reference materials: biochar as a well-studied carbon-based soil amendment and graphite as a more crystalline and comparatively inert carbon reference.

Revision: The Introduction was expanded to explain how SC_{cat} and SC_{plas} compare conceptually with biochar and graphite and why their effects need to be tested rather than assumed.

L. 51-52: does this statement refer to natural SOC or soil carbon in general? Not all C is equal.

Response: Corrected. We revised the sentence to clarify that the statement refers specifically to soil organic carbon, not to soil carbon in general. We also changed the wording from soil fertility to soil quality and functioning to better match the scope of the study.

Revision: The sentence now reads: "Soil quality and functioning are often positively related to soil organic carbon content (Giandon, 2015; Lal, 2004)."

L. 60-61: why does biochar reduce GHG emissions? Some more mechanistic explanations would be valuable.

Response: Thank you for this suggestion. We added a mechanistic explanation of how biochar may reduce greenhouse gas emissions. The revised text now explains that biochar can contribute to carbon sequestration through the long-term stabilization of recalcitrant aromatic carbon and may reduce greenhouse gas emissions by influencing N_2O , CH_4 , and CO_2 production through changes in nitrification, denitrification, soil redox conditions, labile carbon availability, and microbial respiration.

Revision: The Introduction was revised to include mechanistic explanations for the effects of biochar on greenhouse gas emissions.

L. 86-97: most readers will not know much about these methods. For example, which of the two is more common or scalable? Some context would be better.

The production methods by plasma pyrolysis and catalytic pyrolysis using iron ore as catalyst are pursuing pilot plant demonstrations by the companies Monolith(USA) and Hazer (AUS), respectively (ref attached). While the two processes produce (decarbonized) H_2 at low carbon intensity as major product, they also produce solid carbon as co-product in stoichiometric amount. The produced carbon of this study are made by plasma pyrolysis and catalytic pyrolysis using Microwave technologies. Plasma technology achieves higher temperature than catalytic pyrolysis. As a result the produced carbon has different crystalline and porous structures which may find different applications.

L. 96: the mention of PAH is somewhat out of the blue here. Only later in the discussion it becomes clear why this is relevant and apparently common for such materials. I think the common possible risks of the solid materials should already be mentioned in the introduction.

Response: Thank you for this comment. We agree that the relevance of PAHs should be introduced earlier. We added a short explanation in the Introduction that PAHs can form during high-temperature carbonization processes and may condense on the surface of solid carbon materials. We also mention that similar PAH-related risks have been reported for improperly produced or insufficiently treated biochars, which explains why PAHs are relevant for assessing the safety of these materials as soil amendments.

Revision in manuscript: The Introduction was revised to briefly introduce PAHs as a potential risk associated with solid carbon materials and to explain why PAH residues are relevant for soil organisms and microbial processes.

L. 99: the pyrolysis T of the biochar is on the high side compared to the average pyrolysis T. How would that have influenced the properties of the biochar and your results?

Response: Thank you for this important comment. We selected a biochar produced at 850°C because our experimental materials obtained by catalytic pyrolysis and plasma processing of biogas consist predominantly of highly carbonized carbon structures. Therefore, a high-temperature biochar provided a more appropriate reference material than low-temperature biochars. We have added a discussion to the manuscript explaining that biochar produced at 850°C is characterized by a higher degree of carbonization and aromaticity, greater stability against microbial degradation, and a lower content of labile organic compounds and oxygen-containing functional groups. These properties make it more comparable to our carbon-rich test materials and more relevant for evaluating their potential for long-term carbon sequestration. However, the lower abundance of surface functional groups may also reduce some chemically mediated interactions compared with biochars produced at lower pyrolysis temperatures.

Revision: “This temperature was intentionally selected because the investigated carbon materials obtained from catalytic pyrolysis and plasma processing of biogas are composed predominantly of highly carbonized carbon structures.”

Table 1: how were these characteristics determined? E.g. the iron content and SSA

Structure : X-Ray diffraction

Specific surface area , mesoporosity : BET and BJH methods using N₂ adsorption isotherm at 77K, respectively

Particle size : Scanning Electron Microscopy

Metallic Iron content : Thermal Gravimetry Analysis

Table 2: hoe were the C_{org} and N_t contents determined from the LOI described on L. 120-121?

Response: Thank you for pointing this out. We clarified that C_{org} and N_t were not determined from LOI. Total carbon and total nitrogen were measured by dry combustion, and C_{org} was calculated as the difference between total carbon and inorganic carbon.

Revision: The Methods section was revised to specify how C_{org} and N_t were determined.

L. 170: which endogeic species were introduced? That is key information to include.

Response: We thank the reviewer for this important comment. We agree that the identity of the introduced endogeic species is key information and should be explicitly stated. Therefore, we have revised the manuscript to specify the earthworm species used in the experiment. For the sandy soil, the introduced endogeic species was *Aporrectodea caliginosa*, whereas for the silty loam soil, *Aporrectodea icterica* was used. These were the endgeic earthworms that occurred most frequently in the respective soil types. This information has been added to the Materials and Methods section.

Revision in manuscript: The following sentence has been added to the Materials and Methods section: “For the ecotoxicological study, we used juvenile endogeic earthworms (175-357 mg living biomass) that were collected from the sites where the soils were taken from (*Aporrectodea caliginosa*- the sandy soil, *Aporrectodea icterica*- the silty loam soil).”

L. 193: how was earthworm activity monitored?

Response: We thank the reviewer for this comment. We have clarified the procedure used to monitor earthworm activity in the manuscript. Earthworm activity was assessed by daily monitoring of the earthworms' location within the microcosms. The position of each individual was marked on

the outside surface of the microcosm using a marker, allowing us to track changes in its spatial distribution and activity over time.

Revision in manuscript: The following text has been added to the Materials and Methods section: “Earthworm activity and spatial distribution were monitored daily throughout the 28-day exposure period. The position of each individual (i.e., in the control or amended soil compartment) was recorded once per day. In addition, the external position of each earthworm within the transparent vessels was marked on the outer surface of the vessel to ensure consistent tracking of its location over time.”

L. 229: how was the Cd content determined? And why is only Cd shown and not the contents of the other metals from Figure 10?

Response: Thank you for this comment. We clarified in the table footnote how Cd content was determined and why only Cd is shown in the site-characterization table. The Braunschweig soils originate from the raster cells described by Ingwersen (2001), where Cd contamination was the main basis for site characterization. Therefore, Cd was included in Table 3 as the primary contamination indicator used to describe and select the sites. Cu, Ni, and Zn were not included in Table 3 to keep the site-characterization table concise, because these metals were not used for site selection but were analysed as experimental response variables and are presented in the Results section.

Revision: A footnote was added to Table 3 to clarify the source and method of Cd determination and the reason for presenting Cd in the site-characterization table.

L. 234-240: were the amendments mixed into the dried soil and then immediately extracted in the various extractions? How long was the equilibration phase with 0.0025 M CaCl₂ and was this before or after addition of the amendments? This is unclear.

Response: Thank you for pointing out this lack of clarity. We revised the Methods section to clarify the sequence of amendment addition and extraction. The amendments were first mixed into the air-dried soils, and no separate pre-incubation was performed before extraction. The CaCl₂ solution was added after amendment addition, and the 24 h shaking period represented the equilibration/extraction phase.

Revision: The CaCl₂ extraction method was revised to clarify when the amendments were added and how long the CaCl₂ equilibration/extraction phase lasted.

L. 397: I don't agree that SC_{plas} consistently demonstrated a strong immobilizing effect. For Cd, perhaps the element of most strong focus, this was not shown.

Discussion: I found the discussion in general too long and too strongly tied to the figures presented in the results. It would be better to discuss each material per category of investigation (soil physical properties, ecotoxicology, and heavy metal availability), rather than per characteristic within each of these categories. This would already shorten the discussion substantially because it avoids some repetitions (e.g. when discussing the separate heavy metals). This means including more generic statements like those in L. 489-494. Then, a final section discussing the applicability in general would really strongly benefit the manuscript. This shortly summarizes the pros and cons of both novel materials relative to the references, talks about practical factors like upscaling, and makes recommendations for future research.

Response: Thank you for this comment. We agree that the previous wording overstated the effect of SC_{plas}, especially because Cd immobilization was not consistently observed. We removed this statement and revised the text to state that SC_{plas} showed the clearest immobilization effect for Cu at

selected sites, whereas Cd, Ni, and Zn responses were site-, metal-, and fraction-dependent. We also merged the previous Sections 4.3 and 4.4 to reduce repetition and to discuss heavy metal concentrations in soil solution and plant availability of heavy metal together in a more general and mechanistic way. In addition, we added a new section on general applicability and future research needs, summarizing the advantages and limitations of SC_{cat} and SC_{plas} relative to biochar and graphite, practical considerations such as application rate and field-scale use, and key knowledge gaps for future studies.

Revision: Sections 4.3 and 4.4 were merged and rewritten to avoid overstatement, reduce duplication, and strengthen the material- and site-specific interpretation. A new “General applicability and future research needs” section was added to synthesize the practical implications of the study.

Section 4.2: this section lacks entirely a discussion on the possible effect of the native SOC (e.g. statement in L.462-463). If any effect (e.g. on soil respiration) is shown, it could come directly from the amendment, or indirectly because decomposition of the native SOC is primed, or the native SOC is stabilised via bonding with the amendment. Or both effects are prevalent but cancel each other out in the net effect. The minimum that would be required is discussing how the pH changed after amendment, as pH strongly links with microbial activity and could be a key driver for the observed changes in EOC, respiration and microbial biomass following amendment (and also for heavy metal availability). But the authors can also discuss more mechanistically how the materials could affect the native SOC. A nice starting point (for biochar) would be Han et al Geoderma 2020, see e.g. their Figure 5 (doi:10.1016/j.geoderma.2020.114184).

Response: Importantly, in contrast to many biochar-based studies (e.g. Han et al., 2020, Geoderma, doi:10.1016/j.geoderma.2020.114184), the solid carbon materials made by methane pyrolysis used in our experiment did not induce measurable changes in soil pH. We therefore consider pH-driven shifts in microbial activity and nutrient availability to be unlikely drivers of the observed effects in our system.

L. 479-482, L.501-503: the role and relevance of PAHs should already be introduced much earlier in the manuscript. I got confused that PAHs were detected even though SC_{plas} was washed with DCM.

Response: We agree that the role and relevance of PAHs should be introduced earlier in the manuscript to provide better context for the interpretation of the results. We also acknowledge that the detection of PAHs in SC_{plas} may be unexpected given that the material was washed with DCM. To address this concern, we have included information on PAH content and the characteristics of the studied solid carbon materials in Table 1 (Characteristics of the studied solid carbon materials). This addition provides the necessary background information at an earlier stage of the manuscript and improves the clarity of the study design and results interpretation.

Revision in manuscript: PAH concentrations have been added to Table 1 (“Characteristics of the studied solid carbon materials”).

| | Solid carbon from catalytic pyrolysis (SC_{cat}) | Solid carbon from plasma pyrolysis (SC_{plas}) | Biochar | Graphite |
|-------------------|---|--|-------------------------------|----------|
| Starting material | methane | methane | Straw (<i>Miscanthu</i>) | graphite |

| Structure | s x <i>giganteus</i>) | | | |
|--|---------------------------|--------------|-------------------------|-------------------------|
| | crystallized | crystallized | amorphous | crystallized |
| Specific surface area, (m ² g ⁻¹) | 5.9 | 58.8 | 346.4 | 17.4 |
| Mesoporosity | 2< pore diameter <50 nm | n.d. | 2< pore diameter <50 nm | 2< pore diameter <50 nm |
| Particles size | < 200 μm | <200 μm | 125 and 1000 μm | < 50 μm |
| Metallic iron | 1.7 wt.% | n.d. | n.d. | n.d. |
| Phenanthrene, (mg kg ⁻¹) | 0.09 ± 0.02 | 2.5 ± 0.05 | n.d. | 0.1 ± 0.02 |
| Pyrene, (mg kg ⁻¹) | 0.01 ± 0.002 | 1.7 ± 0.2 | n.d. | 0.02 ± 0.004 |
| C _{org} (%) | 98 | 100 | 77.44 | |
| N _t (%) | n.d. | n.d. | 0.59 | |
| C/N(%) | n.d. | n.d. | 131 | |

L. 523-524: earthworm biomass results are not presented in the manuscript, but are referred to here. Perhaps they can be added to an appendix if they are deemed key.

Response: Earthworm biomass was monitored during the experiment; however, these data were not included in the main text of the manuscript because they were not a primary endpoint of the study and were not directly related to the main research objectives. In addition, considering the overall length of the manuscript, we aimed to focus on the parameters most relevant to the study questions. However, to avoid confusion, we have revised the text accordingly and added the reference to the earthworm biomass results.

Revision in manuscript: A table presenting earthworm biomass data has been added to the Supplementary Information.

Section 4.3: I suggest to merge this section with 4.4, as it contains quite some duplication, especially when making the section more general and mechanistic. I also found this section quite weak content-wise. The section talks a lot about how enhanced CEC drives the observed patterns, but heavy metals

mostly bind via ion-specific adsorption and many more mechanisms can play a role (see. e.g., Figure 2 and associated text in Joseph et al. GCB Bioenergy 2021, doi:10.1111/gcbb.12885). Also, the section talk a lot about functional group diversity without giving any details about this, which is not very compelling. If functional group diversity is mentioned, what is known about it from the reference materials can be discussed from the literature, and ideally it should be investigated for the novel materials. The same applies to the porosity for SC_{plas} (porosity is not determined for SC_{plas} in Table 1, but changes in it are given as an explanation in L.543). Finally, like with section 4.2, the section could be much more strongly linked to changes in pH induced by the amendments.

Response: Thank you for this detailed suggestion. We merged Sections 4.3 and 4.4 to reduce duplication and revised the section to provide a broader mechanistic interpretation of the heavy metal results. Following Joseph et al. (2021), we no longer explain the patterns mainly by CEC, but instead discuss multiple possible mechanisms, including ion-specific adsorption, surface complexation, ligand exchange, precipitation, redox reactions, pH-mediated changes in metal solubility, and interactions with mineral and organic soil constituents. We also removed unsupported statements about functional group diversity and porosity where these properties were not directly measured for SC_{plas} and SC_{cat} .

Revision: Sections 4.3 and 4.4 were merged and rewritten to emphasize metal-, fraction-, and site-specific responses and to present the proposed mechanisms as plausible explanations rather than confirmed processes.

Minor and technical comments

L.15: why the quotation marks for “plasmalytic”?

plasma-based approaches

Response: Corrected. We removed the quotation marks and replaced “plasmalytic” with “plasma-based” to avoid ambiguity and improve readability.

Revision: The sentence now reads: “Methane pyrolysis, including catalytic and plasma-based approaches, has attracted attention...”

L.18-19: the last part of the aim doesn’t read well, please rephrase

Response: Corrected. We rephrased the aim sentence to improve readability and clarify that the study assessed soil hydraulic properties, heavy metal mobility, and potential ecotoxicological effects on soil organisms.

Revision: The sentence was revised to: “This study investigated the potential of solid carbon materials from catalytic pyrolysis and plasmalysis, alongside biochar and graphite as reference materials, to modify soil hydraulic properties and heavy metal mobility, and to assess potential ecotoxicological effects on soil organisms.”

L.27: “had almost no effect” --> what does this mean? Did it or did it not have a statistically significant effect? Please rephrase.

Response: Corrected. We replaced the vague phrase “had almost no effect” with a statistically clearer statement. The revised text now specifies that SC_{cat} showed no significant effect on the biological endpoints assessed.

Revision: The abstract was revised to: “ SC_{cat} showed no significant effect on the biological endpoints assessed and had no significant influence on plant-available water, although it moderately increased hydraulic conductivity in the sandy soil.”

L.39: “negative emissions technologies” --> the term Carbon Dioxide Removal (CDR) technologies is nowadays more common. Please rephrase.

Response: Corrected. We replaced “negative emissions technologies” with the more commonly used term “carbon dioxide removal (CDR) technologies.”

Revision: The sentence was revised to: “Carbon dioxide removal (CDR) technologies, such as bioenergy with carbon capture and storage (BECCS), have emerged as key strategies to achieve these targets.”

L 61: “both soil nitrogen (N) dynamics”--> why both? I think this word can be removed.

Response: Corrected. We removed “both” because it was unnecessary in this sentence.

Revision: The sentence was revised to “It can also influence soil nitrogen (N) dynamics.”

L. 81-82: Information seems an odd word choice here, very non-specific. What about guidelines, or a framework?

Response: Corrected. We replaced the non-specific word “information” with a more precise formulation and revised the sentence to emphasize that the findings provide guidance/framework for soil-specific assessment.

Revision: The sentence was revised to: “Our findings provide a framework for evaluating whether, and under which soil-specific conditions, solid carbon from plasma and catalytic pyrolysis can be considered as an amendment to agricultural soils.”

L. 86: not relevant to mention the project name here.

Response: Corrected. We removed the project name from the Methods section because it was not relevant to the synthesis description.

Revision: The project name (“TITAN project”) has been removed from the text.

L. 148: here 40 microcosms are mentioned, combining both soils, but later on in the methods (L.191) a total number per soil type is given. This is inconsistent.

Response: We thank the reviewer for this comment. We would like to clarify that the numbers refer to two different experimental setups and therefore are not inconsistent. The statement on L.148 (40 microcosms) refers to the experiment used to assess soil respiration, microbial biomass, extractable organic carbon (EOC), and nitrogen mineralization across both soil types. In contrast, the description on L.191 refers to a separate earthworm experiment conducted in planar vessels, which included a total of 28 units.

L. 148: I assume the 115 g is only for the sandy soil? And for the silty loam it has to be $4 \times 31.25 = 125$ g?

Response: We thank the reviewer for this careful observation. We agree that the originally reported value (115 g) was correct for the sandy soil only, but it did not accurately reflect the corresponding amount for the silty loam soil. As correctly pointed out by the reviewer, the appropriate value for the silty loam soil is 125 g (4×31.25 g). We have revised the manuscript accordingly to correct this inconsistency and to ensure clarity and accuracy of the reported soil amounts.

Revision in manuscript: The text has been revised as follows: “Each microcosm contained either 115 g of fresh sandy soil or 125 g of fresh silty loam soil, corresponding to 100 g of dry matter (DM) in each case.”

L. 153-156: do I understand correctly from L.151-152 that the number of vials and thus the amount of soil declines over time in the soil respiration measurements? If yes, would be good to explicitly mention that

Response: The reviewer is correct that the number of vials decreased over time. However, the amount of soil used for each soil respiration measurement remained constant, since each measurement was performed using soil from a single vial. We have clarified this point in the Methods section to prevent potential confusion.

Revision in manuscript: The following clarification has been added to the Methods section: “Accordingly, the number of vials remaining per microcosm decreased over time. The amount of soil used for each measurement remained constant because each vial contained the same quantity of soil.”

L. 216: what is the area associated with the 200 g Cd input per year?

Response: Thank you for pointing this out. The value refers to an area-based Cd load, not to the total Cd input for the whole irrigation area. We corrected the sentence to specify the unit as $\text{g ha}^{-1} \text{yr}^{-1}$.

Revision: The sentence was revised to: “Initial heavy metal assessments in the early 1980s indicated Cd inputs of up to approximately $200 \text{ g ha}^{-1} \text{yr}^{-1}$ in the wastewater irrigation area.”

L. 224: areas --> area

Response: Checked. I have changed it accordingly.

L. 283-284: this statement conflicts with L. 280-281.

Response: We revised the paragraph to remove the apparent conflict between the general statement about the water retention curves and the treatment-specific PAW results.

Revision: The Results section was revised to align the text with Fig. 2 and the statistical differences shown in Table 4. “The addition of SC_{plas} visibly altered the shape of the water retention curves in both the sandy (Fig. 2A) and silty loam soil (Fig. 2B), whereas the other amendments showed only minor deviations from the control curve. The effect of SC_{plas} was soil-specific. In the sandy soil, SC_{plas} reduced the volumetric water content between pF 0 and pF 3 and decreased the plant available water (PAW), whereas in the silty loam soil, SC_{plas} increased both quantities (Table 4). Similar to SC_{plas} , graphite significantly increased plant available water in the silty loam soil. In contrast, SC_{cat} and the biochar did not change the amount of plant available water compared to the control in the silty loam soil.”

Table 4: the footnote can be included in the table header for clarity

Response: Corrected. We moved the definitions of SC_{cat} and SC_{plas} into the Table 4 caption for clarity.

L. 293: “with values sharing the same letter not being significantly different..”

Response: Corrected. We revised the wording to: “values sharing the same letter are not significantly different.”

L. 293: “post-hoc Tukey test”.

Response: Corrected. We specified the statistical test as “ANOVA followed by Tukey’s post-hoc test.”

L. 311-313: this is a weirdly phrased statement. Rather than using the word 'pronounced', it would make more sense to directly describe the positive / negative effect.

Response: We agree with the reviewer, so we have removed this sentence and now begin the paragraph directly with a description of the observed effects in each soil type.

Revision in manuscript: We have removed the first sentence from this paragraph.

Figure 4 (and elsewhere): I find 'test material' an odd description. What about 'solid amendment' instead?

Response: We thank the reviewer for this suggestion. We agree that the term "solid amendment" is more appropriate and clearer than "test material". Therefore, we have revised Figure 4 and the corresponding text throughout the manuscript accordingly.

Revision in manuscript: The term "test material" has been replaced with "solid amendment" in all figure and table captions, including Figure 4.

L. 347: SC_{cat} mentioned twice

Response: We thank the reviewer for pointing this out. We confirm that SC_{cat} was inadvertently mentioned twice in this sentence. We have corrected this duplication in the revised manuscript.

Revision in manuscript: The sentence has been corrected as follows: "In silty loam soil, only the addition of graphite increased the content of nitrate significantly in comparison to the control, while the other materials (SC_{cat}, SC_{plas}, biochar) decreased nitrate contents starting from day 14 on (Table 6)."

L. 356: endogenous --> endogeic

Response: We thank the reviewer for this comment. The term has been corrected to "endogeic" in the revised manuscript.

Revision in manuscript: The term has been corrected in the text as follows: "In terms of avoidance test with earthworms, all specimens survived the 28 days of incubation in control and amended soils. In the sandy soil, the avoidance test showed a significant avoidance of endogeic earthworms against soils amended with SC_{plas} and graphite, but not against soils amended with SC_{cat} or biochar (Fig. 5A)."

L. 368: I do not find 'largely' the right word here. The avoidance is only slightly higher than the statistically significant limit of 25%

Response: We thank the reviewer for this comment. We agree that the term "largely" was not appropriate in this context. Therefore, we have revised the sentence to more accurately reflect that the avoidance was only slightly above the statistically significant threshold of 25%.

Revision in manuscript: The sentence has been revised as follows: "Springtails showed avoidance of soils amended not only with biochar and graphite, but also with SC_{plas}, whereas no significant avoidance was observed for SC_{cat} (Fig. 6B)."

L. 397: "also varied by metal and site" (because repetition from L. 377)

Response: Corrected. We rephrased the second sentence to avoid repeating "varied by metal and site."

Revision: The sentence was revised to: "A similar site- and metal-dependent pattern was observed for plant-available heavy metal concentrations, as determined by ammonium nitrate extraction."

L.377 the sentence was revised to: “The effects of soil amendments on heavy metal concentrations in 0.0025 M CaCl₂ solution were site- and metal-specific (Fig. 10).”

L. 471-474: the use of brackets makes this sentence confusing.

Response: Thank you for your comment. We agree that the numerous parenthetical phrases made the sentence difficult to read. The sentence has been revised to improve clarity by incorporating the information previously provided in parentheses into the main text.

Revision: “According to Jing et al. (2022) and Yao et al. (2014), the presence of clay particles on pyrogenic carbon surfaces can increase adsorption capacity by approximately fivefold. This enhancement is mainly attributed to ion exchange associated with clay particles and electrostatic attraction associated with biochar. A decrease in nitrate ion concentration in the silty loam soil was observed after 14 days. Although the temporal pattern of the biochar effect on nitrate varied with soil texture, biochar generally decreased nitrate concentrations. This finding is consistent with previous studies showing that biochar can reduce inorganic nitrogen leaching through adsorption (Liu et al., 2018; Major et al., 2012; Sun et al., 2017; Yao et al., 2012).”

Section 4.3: no need to give the abbreviations of the metals again.

Response: Corrected. We removed repeated definitions of the metal abbreviations in Section 4.3 where these abbreviations had already been introduced earlier. The section now uses the metal names without redefining the abbreviations.

Revision: “For zinc (Zn)” and similar repeated abbreviation definitions were revised to “For zinc,” “For copper,” “For nickel,” and “For cadmium.”

L. 625-627: this repeats L. 620-624 and could be removed.

Response: Corrected. We removed the repetitive final paragraph and revised the conclusion to avoid restating the same interpretation twice.

Revision: The conclusion was shortened and restructured to provide one concise final synthesis of the material-specific benefits and limitations of SC_{cat} and SC_{plas}.

Citation: <https://doi.org/10.5194/egusphere-2025-3866-RC2>