Line Broszka

Saki and Kostiuk (2025) studied the effects of two novel (CO_2-emission free) solid carbon materials from methane pyrolysis, SC_{plas} (plasmalytic pyrolysis) and SC_{cat} (catalytic pyrolysis), and reference materials (graphite and biochar) as soil amendment on soil biology, heavy metal immobilization and soil properties. Results across soil types (sandy and silty loam) and five polluted sites varied; SC_{plas} successfully immobilized some heavy metals at some polluted sites but had ecotoxicological effects and negatively affected nutrients cycles in both soil types and decreased water retention in sandy soils. SC_{cat} had little impact on soil biota, did not affect nutrient cycling in either soil type, but mobilized Cd at few sites. Conclusively, results suggested that site-specific physiochemical properties must be considered before applying the novel solid carbon materials as soil amendment.

The hypothesis and objectives are explicitly stated and methods thoroughly described, especially for experiment 1. The final discussion on how carbon materials interplay with soil hydraulic and chemical properties and organisms is strong. Lastly, the conclusion is clear. I find your paper interesting and of great relevance due to the novelty aspect of the solid carbon materials having the potential to be used as soil amendment whilst simultaneously being able to immobilize heavy metals, if found to be safe. Further, the materials might be able to act as carbon storage, a benefit only briefly touched upon in the manuscript.

However, I do have some major comments that I recommend being revised. This includes removing several figures as the large number of figures removes attention from important results, streamlining results shown in figures with text as there are multiple instances where results are misinterpreted or being oversold in the text, and lastly adding an in-depth point in the discussion on site-specific responses to heavy metal immobilization as e.g., it remains unclear why heavy metals were mobilized at some sites. Overall, these adjustments will increase clarity and scientific credibility and slightly affect the take-home messages. I perceive these adjustments necessary as the conclusions communicated in abstract are not consistent with the final conclusions.

Major concerns

1. **Too many figures**

The manuscript presents 11 figures with 26 graphs, many of which are not all central to addressing the hypothesis. This makes it hard for the reader to identify key results. I recommend focusing on figures that clearly present findings and are central to answering the hypothesis. Further, error bars need to be defined as either standard errors or standard deviations. Additionally, the use of different lower-case letters indicating statistical differences should be consistent across all figures as the interpretation seems to differ throughout your manuscript.

Response: We agree that the large number of figures can make it difficult to identify the key messages. To improve readability, we revised the Results text to more clearly guide the reader through the key findings and we revised the figure captions throughout to (i) explicitly define the error bars and (ii) standardize the interpretation of letter annotations across figures. We also re-checked post-hoc groupings and harmonized the letter notation so that treatments sharing at least one letter are not significantly different within a site.

Revision in manuscript: Figure captions and Results text revised; statistical lettering was checked and harmonized across figures.

For example, figures 2A and 2B (water retention curves) and 3A and 3B (hydraulic conductivity) are difficult to interpret visually and do not seem to greatly impact your conclusions. Consider leaving out these figures and presenting results on hydraulic conductivity in either Table 4 or a smaller table.

Response: We appreciate this suggestion. We retained the water retention and hydraulic conductivity results because they directly address the first hypothesis (effects on soil physical functioning). However, we revised the corresponding text to more explicitly link these figures to the main conclusions and to highlight the effect directions and their magnitude, rather than repeating values.

Revision in manuscript: Revised Discussion text in Sections 4.1.

Figures 5-7 (microbial biomass, EOC, N mineralization) present results as a function of incubation time. In the statistical analysis you did not include time as a factor in the ANOVA analysis. Since the study seems to focus on long-term effects of the amendments rather than when changes occur nor which mechanisms play a role, I suggest presenting cumulative results for EOC and N mineralization and net changes for microbial biomass between day 0 and 56, leaving out figures 5-7 and rather summarizing information in a table. Alternatively, current figures could be presented as time series rather than histograms to improve interpretation and comparison between treatments.

Response: we agree that a large number of panels can make it difficult to identify key messages. Therefore, we removed graphs 5 and 7 and summarized the information in a table. As for graph 6, we would like to keep it, since the materials we studied have a statistically significant impact on the EOC.

Revision in manuscript: The information previously shown in Figures 5–7 has been summarized in two new tables (Table 5 and Table 6), which present microbial biomass, EOC, and nitrogen species across sampling times and treatments. This allows clearer comparison between treatments and time points. **Table 5.** Microbial biomass (C_{mic}) and extractable organic C (EOC) during 56 days of incubation after addition of 40 t ha^{-1} solid amendment (mean and standard error, n = 4). Treatments were: C: control, SC_{cat}: solid carbon from catalytic pyrolysis, SC_{plas}: solid carbon plasma pyrolysis, BC: Biochar, and G: Graphite. Different lowercase letters represent a significant difference at $p < 0.05$ between treatments at a given sampling day (7, 14, 35, or 56).

	Treatments	Microbial biomass (µg g^{-1} soil dw)				Extractable organic C (EOC)			
		days				days			
		7	14	35	56	7	14	35	56
Sandy soil	Control	165.8±45.4^{a}	217.9±20.6^{ab}	174.1±18.7^{ab}	199.4±13.9^{a}	79.3±22.3^{a}	61.5±5.2^{a}	55.9±1.3^{ab}	59.5±6.0^{a}
	SC_{cat}	143.6±23.9^{a}	226.9±36.7^{ab}	213.9±35.6^{ab}	185.5±26.2^{a}	63.2±2.7^{a}	52.2±2.5^{b}	49.7±2.8^{bc}	57.0±5.2^{a}

	SC_{plas}	150.0±43.4^{a}	235.2±15.2^{a}	192.9±26.7^{a}	201.8±26.9^{a}	26.2±1.7^{b}	19.4±4.0^{c}	21.2±4.1^{d}	25.3±7.7^{b}
	Biochar	179.5±4.4^{a}	251.4±11.9^{a}	210.7±7.9^{a}	208.4±6.5^{a}	62.9±1.8^{a}	50.4±0.8^{b}	47.7±2.7^{c}	54.9±4.8^{a}
	Graphite	103.3±15.4^{a}	171.5±8.3^{b}	147.3±12.8^{b}	169.3±11.2^{a}	74.2±17.3^{a}	56.4±1.5^{ab}	57.2±4.8^{a}	58.9±5.7^{a}
Silty loam soil	Control	277.5±19.1^{a}	359.3±27.9^{a}	292.8±40.0^{a}	324.3±44.9^{a}	39.5±2.5^{ab}	36.1±1.5^{ab}	38.9±0.8^{a}	46.3±4.6^{a}
	SC_{cat}	268.3±31.5^{a}	312.9±33.5^{a}	252.3±55.1^{a}	292.6±30.5^{a}	31.6±2.9^{c}	30.8±1.5^{b}	32.4±2.7^{a}	32.1±1.1^{b}
	SC_{plas}	247.3±10.3^{a}	333.6±20.4^{a}	259.3±24.5^{a}	309.9±9.5^{a}	15.8±3.7^{d}	14.3±6.1^{c}	18.3±9.9^{b}	17.6±3.6^{c}
	Biochar	260.6±39.2^{a}	340.4±21.1^{a}	289.8±25.3^{a}	350.3±45.0^{a}	41.2±4.0^{a}	38.1±1.5^{a}	39.1±5.3^{a}	43.6±6.0^{a}
	Graphite	222.2±55.2^{a}	310.6±56.8^{a}	244.3±13.5^{a}	285.8±53.3^{a}	33.7±1.4^{bc}	33.1±2.0^{ab}	36.0±4.8^{a}	33.2±0.6^{b}

Table 6. Concentrations of ammonium and nitrate ions during 56 days of incubation after addition of 40 t ha^{-1} solid amendment (mean and standard error, n = 4). Treatments were: C: control, SC_{cat}: solid carbon from catalytic pyrolysis, SC_{plas}: solid carbon plasma pyrolysis, BC: Biochar, and G: Graphite. Different lowercase letters represent a significant difference at $p < 0.05$ between treatments at the four sampling days (7, 14, 35, 56).

	Treatments	**Concentrations of ammonium (μg g^{-1} soil dw)**				**Concentrations of nitrate (μg g^{-1} soil dw)**			
		days				days			
		7	14	35	56	7	14	35	56
Sandy soil	Control	0.6±0.98^{c}	0±0^{b}	0.8±0.7^{b}	7.5±6.6^{ab}	33.5±2.5^{a}	37.5±1.8^{a}	47.4±1.8^{a}	51.5±8.6^{a}
	SC_{cat}	1.7±1.8^{bc}	2.9±3.9^{ab}	1.6±1.9^{b}	5.2±5.7^{ab}	31.2±3.3^{ab}	37.7±2.7^{a}	47.6±4.7^{a}	53.2±6.1^{a}
	SC_{plas}	6.0±3.9^{ab}	8.6±4.5^{a}	12.7±6.5^{a}	15.3±4.0^{a}	24.7±1.6^{c}	27.7±0.8^{b}	31.5±1.9^{c}	32.2±0.2^{b}
	Biochar	0±0^{c}	0±0^{b}	0±0^{b}	0.1±0.2^{b}	26.8±3.8^{bc}	30.0±2.4^{b}	36.7±2.0^{bc}	47.1±2.8^{a}

	Graphite	6.5 ± 0.7^{a}	8.6 ± 2.2^{a}	15.3 ± 3.8^{a}	12.5 ± 6.6^{a}	27.8 ± 1.1^{abc}	31.5 ± 1.7^{b}	38.8 ± 3.9^{b}	49.8 ± 2.3^{a}
Silty loam soil	Control	3.7 ± 6.9^{a}	0 ± 0^{a}	0 ± 0^{b}	0.9 ± 1.1^{a}	17.3 ± 1.6^{b}	19.5 ± 0.6^{b}	24.8 ± 1.4^{b}	32.7 ± 0.9^{b}
	SC_{cat}	0 ± 0^{a}	0 ± 0^{a}	0 ± 0^{b}	0 ± 0^{a}	15.5 ± 0.6^{b}	16.8 ± 0.5^{c}	23.4 ± 0.4^{bc}	28.7 ± 1.5^{c}
	SC_{plas}	0 ± 0^{a}	0 ± 0^{a}	1.4 ± 0.9^{a}	2.7 ± 2.5^{a}	15.4 ± 0.7^{b}	16.5 ± 0.1^{c}	18.5 ± 1.7^{d}	21.5 ± 2.0^{d}
	Biochar	8.0 ± 9.0^{a}	0 ± 0^{a}	0 ± 0^{b}	0 ± 0^{a}	16.1 ± 0.6^{b}	15.7 ± 0.3^{c}	21.2 ± 1.2^{c}	25.4 ± 1.9^{c}
	Graphite	0.9 ± 1.7^{a}	0 ± 0^{a}	0 ± 0^{b}	0.03 ± 0.05^{a}	21.9 ± 0.6^{a}	23.9 ± 1.4^{a}	31.6 ± 0.8^{a}	37.3 ± 2.1^{a}

Across all figures, the letter annotation used to imply statistical differences is inconsistent. For example, in Figure 7C (nitrate ion concentration) SC_{cat} ('ab') is not significantly different (p. 14, l. 343) from the control ('a'),

Response: thank you for clarifying. We have checked all statistical letter designations in all figures and would like to note that different lowercase letters indicate significant differences between treatment options within each plot/soil, and that treatment options sharing at least one letter do not differ significantly (post-hoc test, $p < 0.05$).

Revision in manuscript: we checked all statistical letter designations in all figures.

the same applies for Figure 11A (plant available heavy metals) where SC_{plas} ('ab') is not significantly different (p. 17, l. 399) from control ('a'), whereas in Figure 10A (Cd concentrations) SC_{cat} 'b' is significantly different (p. 16, l. 378) than the control ('ab'). These are just a few of many contradicting examples. Please make it consistent between figures whether single letters are statistically different from letter combinations containing the same letter to avoid confusion. Also, ensure that you specify whether significant difference refers to control or other treatments.

Response: We agree and have made the statistical lettering consistent across figures. In all figure captions we now state explicitly that different lower-case letters indicate significant differences among treatments within each site/soil, and that treatments sharing at least one letter are not significantly different (post-hoc test, $p < 0.05$). This is now applied consistently, and the Results text refers to significant differences among treatments within each site.

Revision in manuscript: Updated captions for Figures 8 and 9; minor edits in Results text to align wording with the statistical output.

1. **Overselling results and conclusions inconsistent with figures**

There are several inconsistencies throughout your manuscript between results in figures and conclusions. Conclusions in some instances directly contradict each other. This is a major concern, as it influences the scientific credibility of your results. The inconsistencies between abstract, results, discussion and conclusions reduce clarity. In the end it heavily influences take-home messages and could potentially influence agricultural policies that rely on scientific results, if policymakers only read the abstract, thus misinterpreting the effectiveness of the novel solid carbon materials. I recommend rephrasing all examples below to accurately reflect data and align abstract, results and discussion to avoid contradictions.

An overarching issue in your manuscript is repeated claims that SC_{plas} "consistently" demonstrates "strong immobilization" (l. 28; 378; l. 397; l. 540; l. 618), however this is not reflected in figure 10 and 11. SC_{plas} is misrepresented as better and more effective on heavy metal immobilization than results show, thus overselling results. Figure 10 shows that SC_{plas} significantly reduced concentrations for Cd at two sites (although significance is unclear due to annotation as discussed in major comment above), for Cu at 3 sites, for Ni at two sites (at two sites below detection limit) and for Zn at no sites. SC_{plas} significantly increased Zn concentration at one site. Figure 11 showed no significance of SC_{plas} on Cd, significance for Cu at two sites (at two sites below detection limit), no significance for Ni (at two sites below detection limit) and neither for Zn. Altogether, these results do not convince me of the manuscript's repeated claims that SC_{plas} is consistent. I do agree that the effect is in some instances strong, however it seems to heavily depend on location and heavy metal.

Response: Thank you for pointing this out. We agree that our previous wording overstated the metal immobilisation effects of SC_{plas}. We re-checked Figures 8 and 9 and the associated statistical groupings and revised the manuscript to ensure that all statements are fully consistent with the results. Specifically, we removed the terms "consistently" and "strong" where these were not supported and now describe heavy-metal responses as metal- and site-specific. In the revised text, we report that SC_{plas} shows the clearest immobilisation effect for Cu (reduced concentrations in the soil-solution proxy at several sites and reduced plant-available Cu at specific sites), whereas Cd does not show consistent reductions across extraction pools. We also revised related sentences in the Results, Discussion, Conclusions, and Abstract accordingly.

Revision in manuscript: Revised Abstract; Results (Sections 3.2.1–3.2.2); Discussion (Sections 4.3); and Conclusions to remove unsupported claims of consistent/strong immobilisation and to align the interpretation with Figures 8–9.

Therefore, the manuscript should be adjusted in the following examples where the word "consistent" is used. The list below also includes examples where other results are misinterpreted:

- 1, l. 28 (abstract): "*In soils contaminated with heavy metals, SC_{plas} showed strong immobilization particularly for Cd and Cu, across several sites, outperforming the reference materials".* Figure 10 shows that (1) biochar performs better than SC_{plas} for Cd and Zn at two locations, (2) SC_{plas} performs better than biochar for Cu, (3) effects are quite similar for Ni (SC_{plas} more significant at one site, biochar more significant at the other). Graphite in no cases significantly increases concentration. Thus, I do not agree that biochar was outperformed by SC_{plas}, only for Cu this is true. Please rephrase this.

  Response: We agree. We revised the abstract to remove the claim that SC_{plas} outperformed the reference materials. The revised wording emphasizes metal- and site-specific responses and states that SC_{plas} showed the clearest immobilisation for Cu, whereas Cd effects were not consistent across extraction pools.

Revision in manuscript: Abstract revised to remove "outperforming the reference materials" and to report metal-specific effects.

1, l. 30 (abstract). You highlight a promise for SC_{plas} for site-specific soil improvement and only caution against hydrophobic effects, thus failing to warn against ecotoxicological effects seen in figures 8 and 9. Springtails showed avoidance of SC_{plas} in silty loam soil (Figure 9), whilst earthworms showed avoidance in sandy soils (Figure 8). Please add the ecotoxicological aspect to your promise for SC_{plas} to nuance it.

Response: Thank you for the clarification, we completely agree with you and have added the ecotoxicological aspect.

Revision in manuscript: The abstract has been revised to add an ecotoxicological aspect: "In addition, recorded ecotoxicological reactions - avoidance of sandy soil supplemented with SC_{plas} by earthworms and avoidance of treated loamy soil by springtails - require careful application."

1, l. 31 (abstract). You claim that SC_{cat} has more negative ecotoxicological effects than positive ones depending on the soil. This is not in line with figures 8 and 9 showing that SC_{cat} does not significantly influence avoidance behavior of earthworms and springtails in either of the soils. It is also not in line with conclusion in which you claim (P. 24, l. 625) that SC_{cat} is a safe soil amendment. Please adjust this incorrect claim.

Response: We agree that this statement was incorrect. Figures 6 and 7 show that SC_{cat} did not significantly influence avoidance behavior of earthworms or springtails in either soil, and our conclusions describe SC_{cat} as a comparatively inert amendment. We therefore removed the claim that SC_{cat} has more negative ecotoxicological effects than positive ones and revised the abstract to state that SC_{cat} showed generally minor effects on the biological endpoints assessed, indicating comparatively limited ecotoxicological effects under our test conditions.

Revision in manuscript: Abstract revised to remove the unsupported negative ecotoxicology claim for SC_{cat} and to align with Figures 6–7 and the conclusions. "In contrast, SC_{cat} was characterized by a generally neutral effect on soil biota and did not cause significant changes in the biological properties of the soil."

- 16, l. 367. You claim that springtail behavior in silty loam soil was only different for SC_{plas}. This directly contradicts results in figure 9B and sentence that follows, both showing that springtails also significantly avoided biochar and graphite. Please correct to include biochar and graphite.

Response: Thank you for clarifying. We stated that the behavior of springtails in silty loam soil differed only for SC_{plas}, meaning that in sandy soil they did not avoid soil with this additive, but in silty loam soil they began to avoid it. As for biochar and graphite, the behavior of springtails did not differ from the previous soil (avoiding both sandy soil and silty loam soil). However, for a better understanding of the text, we rephrased this sentence to make it clearer.

Revision in manuscript: we rephrased the sentence : "In the silty loam soil, unlike sandy soil, the behavior of *F. candida* was different only for SC_{plas}. 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)."

- 16, l. 378 (results): *"(…) with SC_{plas} consistently demonstrating a strong immobilizing effect"* (referring to figure 10). Remove 'consistently'.

  Response: We agree. The term "consistently" overstates the pattern shown in Fig. 8. We have removed "consistently" and revised the sentence to describe the response as metal- and site-specific, highlighting the effects that are supported by the figure and statistics.

  Revision in manuscript: Results (Section 3.2.1; Fig. 8 text) revised to remove "consistently" and to reflect site-/metal-specific effects.

- 16, l. 383 (results): *"SC_{plas} also reduced Ni concentrations (….), while SC_{cat} and other amendments had negligible effects (Fig. 10C)."* Figure 10C shows that biochar had a significant effect at both Braunschweig locations as well as SC_{plas}. Please adjust, so that "other amendments" are not all said to have negligible effects.

  Response: We agree. Figure 8C shows that Ni concentrations were reduced not only by SC_{plas} but also by biochar at the Braunschweig sites. We revised the text accordingly, so that "other amendments" are not described as negligible where significant effects are present.

  Revision in manuscript: Results (Section 3.2.1; Ni paragraph, Fig. 8C) revised to state that SC_{plas} and biochar reduced Ni at the Braunschweig sites, whereas SC_{cat} and graphite did not differ significantly from the control.

- 17, l. 397 (results): "*SC_{plas} consistently demonstrated a strong immobilizing effect*" (referring to figure 11). Remove 'consistently'.

  Response: We agree. We removed "consistently" and revised the wording to reflect that effects in Fig. 9 are metal- and site-specific (e.g., Cu reductions at specific sites), rather than uniform across metals and locations.

  Revision in manuscript: Results (Section 3.2.2; Fig. 9 text) revised to remove "consistently" and align the wording with the site-/metal-specific outcomes.

- 22, l. 540 (discussion): *"SC_{plas} consistently demonstrated a strong immobilizing effect across sites, particularly for Cd".* Remove 'consistently'.

  Response: We agree. We removed "consistently" and revised the discussion to avoid overstatement. In addition, we corrected the text to reflect that the clearest immobilisation effect of SC_{plas} is for Cu (and solution-phase Ni at the Braunschweig sites), while Cd does not show consistent reductions across extraction pools.

  Revision in manuscript: Discussion (Section 4.3 and related text) revised to remove "consistently" and to accurately reflect the metal- and site-specific patterns shown in Figs. 8–9.

- P 24, l. 618 (conclusion): *"Nevertheless, in soils contaminated with heavy metals, SC_{plas} consistently strongly immobilized heavy metals, particularly Cd, Cu and Ni."* Remove 'consistently'.

  Response: We agree. We removed "consistently" and revised the conclusion to avoid a broad claim of strong immobilisation across all metals. The revised conclusion now states that SC_{plas} shows the clearest immobilisation effect for Cu (and reduced solution-phase Ni in the Braunschweig soils), whereas Cd did not show consistent reductions across extraction pools.

Revision in manuscript: Conclusions revised to remove "consistently strongly immobilized" and to provide a metal-specific summary consistent with Figs. 8–9.

1. **Missing discussion on soil-specific responses to heavy metal immobilization**

Your results clearly show that the soil amendments SC_{plas} and SC_{cat} cannot be safely applied across all sites and soil types. You note that this variability underscores the importance of considering soil properties such as soil texture, pH and organic matter in amendment selection. Other than the examples of limited Zn mobility in Neckarwestheim (p. 23, l. 571) and reduced Cd mobility in Gundelsheim and Neckarwestheim (p. 23, l. 581), where you provide strong explanations related to pH and organic matter contents, other site-specific responses remain unexplained. Failing to discuss all variable site-responses to soil amendment leaves the reader without possible explanations, thus further complicating applicability of results. I suggest having a more comprehensive discussion of how site-specific properties potentially influenced amendment performance to improve applicability of your results.

For example, biochar has significant positive effects on Cd, Ni and Zn in Braunschweig 1 and 2, but not on remaining sites. SC_{plas} has significant (positive) effects on Cd, Ni and Cu at the same two sites. This indicates that Braunschweig sites might respond better to soil amendments than the remaining two sites, possibly due to differences in soil properties. Another example is that interestingly, in some cases heavy metal mobility is increased by soil amendments. This is only highlighted for SC_{cat} (l. 29), but not for the other soil amendments. In Gundelsheim biochar statistically significantly increased Cd levels (Figure 10A), graphite increased Cu levels (Figure 10B) and all amendments, especially SC_{plas}, significantly increased Zn levels compared to control (Figure 10C). These statistically significant observations were not addressed.

I recommend comparing site-specific pH, organic matter and texture (table 3) with the observed metal levels at each site for each soil amendment. Lastly, I recommend clarifying that the variability in your results cannot be explained with your current knowledge to acknowledge research gaps.

Response: Thank you for this important comment. We agree that the previous discussion did not sufficiently address the soil-specific responses in heavy metal immobilization and, in particular, did not adequately discuss cases where amendments increased extractable metal concentrations. We therefore merged the previous Sections 4.3 and 4.4 and substantially revised the heavy-metal discussion. The revised section now emphasizes that amendment effects were metal-, fraction-, and site-specific and discusses how soil properties such as pH, organic carbon content, texture, and initial metal concentration may have influenced amendment performance. We also added a paragraph comparing the stronger responses in the Braunschweig soils with the more limited responses in Gundelsheim and Neckarwestheim, and we now explicitly acknowledge that some site-specific responses cannot be fully explained with the available data because metal speciation, amendment-induced pH changes, dissolved organic matter composition, and competitive ion effects were not fully characterized.

Revision: The heavy-metal discussion was rewritten to include a more comprehensive interpretation of site-specific amendment responses, including both immobilization and possible mobilization effects, and to acknowledge remaining mechanistic uncertainties.

Minor arguments

1. **Title:** Indicates that the study deals with carbon storage, however the study only argues for increased carbon storage through microbial respiration of C which is not sufficient evidence. At least a measurement of total organic carbon would be needed to support this claim. I suggest changing your title to something like "Assessing the impact of solid carbon from methane cracking on soil hydraulic properties, soil biology, ecotoxicology and heavy metal mobility".

   Response: We thank the reviewer for this important point and agree that our study does not directly quantify long-term carbon storage (e.g., via changes in total organic carbon or long-term persistence), and that CO_2 fluxes alone are not sufficient to substantiate a "carbon storage" claim in the title. We therefore revised the title to avoid implying verified carbon sequestration and to better reflect the scope of the work, which focuses on soil hydraulic properties, soil biology/ecotoxicology, and heavy metal mobility/availability.

   Revision in manuscript: We changed the title to remove the explicit "increase carbon storage" claim and to emphasize the assessed endpoints (hydraulics, soil biology/ecotoxicology, and heavy metal mobility/availability).

   Missing background on methane pyrolysis: Readers of Biogeosciences might lack extensive chemical knowledge behind methane pyrolysis not enabling them to fully understand the chemistry of the novel solid carbon materials nor the effects on sorption of heavy metals or soil properties. I recommend elaborating on the materials section to point out differences between SC_{cat}, SC_{plas} and biochar (since you build your hypothesis on knowledge from biochar) and explain how the different physical and chemical properties (table 1) can possibly influence heavy metal sorption and microbial activity.

   Response: Thank you for this recommendation. We expanded the Materials section to better explain the differences among SC_{cat}, SC_{plas}, and biochar, including crystallinity, specific surface area, porosity, particle size, and surface chemistry. We also added text explaining how these properties may influence microbial activity and heavy metal sorption.

   Revision: The Materials section was expanded to provide background on methane-derived solid carbon materials and their potential effects on microbial activity and heavy metal sorption.

   "According to the characteristics of the studied materials presented in Table 1, biochar, which is characterized by an amorphous structure, high specific surface area, and developed porosity, as well as the presence of a significant number of functional groups (–COOH, –OH, –C=O), is a more biologically active matrix for microorganisms. The high specific surface area and porous structure provide more accessible sites for cell adhesion and colonization of microorganisms, as well as promote more efficient sorption of nutrients and metabolites. In addition, the larger particle size of biochar can promote the formation of stable biofilms, while solid carbons are characterized by finer dispersion, which, on the one hand, provides better contact with cells, but on the other hand, can cause physical stress to microorganisms.

The crystal structure of solid carbons indicates their relative chemical inertness. The specific surface area of these materials is an order of magnitude lower than that of biochar, which limits the number of potential sites for microorganism adhesion and reduces their sorption capacity for nutrients. The absence of mesopores in SC_{plas} indicates limited possibilities for the formation of microbial micro-niches, in particular with regard to the retention of water, substrates, and signalling molecules, as well as the absence of effective protection of microorganisms from physicochemical stress factors.

Thus, biochar is a more suitable material for long-term microbial colonization, while solid carbons are likely to be involved mainly in short-term interactions with microbial communities. At the same time, the presence of metallic iron in SC_{plas} may partially compensate for the low specific surface area of this material. Iron can serve as a cofactor for enzymes, catalyze redox reactions, and stimulate electron transport processes, which potentially distinguishes SC_{plas} from SC_{cat} and indicates the possibility of stimulating specific microbial processes. In contrast, SC_{cat}, given its combination of low specific surface area and lack of additional active components, is likely to exhibit the least biological activity."

2. **Methodology:** For the ecotoxicological tests on earthworms and springtails there are a few unclarities regarding which protocols were followed in each test.

- You reference Han et al (2021) when describing the earthworm avoidance test, thus implying having followed ISO 17512-1:2008 (2008) as this is the protocol followed by Han et al (2021) for their test on earthworms. You also refer to ISO (2008) for the avoidance rate threshold. Your experimental design is not consistent with ISO (2008) as the duration of 28 days breaks the standard of ISO (2008) being 2 days. Further, sample size, nor age and size of the earthworms are not consistent. Daily monitoring adds variability to the results and might pose stress to earthworms. This complicates the use of the avoidance rate threshold of 25 % stated in the protocol, as it might not be valid for a duration longer than recommended. It is unclear to me whether you have failed to meet the requirements for the correct protocol, used a modified version of the ISO protocol or an unknown protocol, which you fail to reference. I strongly recommend clarifying this, as it complicates cross-comparison of studies using avoidance tests and affects the credibility of your results. I also suggest referencing Han et al (2021) earlier when introducing the earthworm avoidance test.

  Response: We thank the reviewer for this important comment. The experiment was not conducted in accordance with ISO 17512-1:2008, and we acknowledge that the duration of the test and the experimental design differ significantly from the avoidance test specified in the ISO standard. To avoid confusion, we have revised the manuscript to clearly describe the experimental procedure used without reference to ISO 17512-1:2008. The earthworm avoidance test used in this study was a modified experimental approach developed for the purposes of this work. Han et al. (2021) is now cited only as the source for the calculation of the avoidance coefficient and the 25% avoidance threshold used to interpret the results.

- You explicitly state having conducted avoidance test with springtails in accordance with OECD guideline 232 (2009), a reproduction test used to test chemicals on collembolans (OECD, 2009), however, the experimental design is not consistent with this protocol. Rather, the design seems consistent with ISO 17512-2:2011 (2011), an avoidance test on collembolans, in terms of duration, replicates and number of springtails in each vessel. I suggest that you look into ISO (2011) and determine whether this is the protocol you used.

  Response: We thank the reviewer for this helpful clarification. We have carefully re-evaluated the applied methodology and agree that the experimental design corresponds to ISO 17512-2:2011 rather than OECD 232. The manuscript has therefore been corrected accordingly, and the methods section has been revised to accurately reflect the standardized avoidance test protocol used for *Folsomia candida*.

  Revision in manuscript: The avoidance test with springtails has been revised to state compliance with ISO 17512-2:2011. "The avoidance test with springtails was carried out in accordance with ISO 17512-2:2011 in plastic boxes (10.3 × 8.5 × 4.1 cm (height × length × depth). Boxes were filled with 100 g of control soil in one half and 100 g of test soil (1.11 g of solid carbon + 100 g of dry soil) in the other half without a barrier in between. Five replicates were prepared for each treatment. At the beginning of the test, 2 mg of dry yeast was homogeneously added to the soil surface in each test box, and then 20 springtails were added. The boxes were closed with lids ensuring gas exchange and incubated at 20 ± 1 °C. Two days later, the animals in each half of the boxes were counted manually under a microscope. Avoidance was calculated using the formula:

  (4)

  where AR is the avoidance rate (in %), Nc is the number of *F. candida* in the control part of the soil, N_t is the number of *F. candida* in the tested part of the soil, and No is the total number of *F. candida* in the box (20 specimens). The results were considered statistically insignificant if the avoidance rate did not exceed 25% (ISO 17512-2:2011, 2011)."

1. **Unclarities and conflicting statement regarding PAHs**

- 3, L. 96: You state that SC_{plas} is washed with dichloromethane to extract any residual PAHs, however later (p. 21, l. 501) you say that PAHs in SC_{plas} might have inhibited microbial respiration due to the toxic effects. So, was the PAH not completely removed after all? Please clarify whether this is the case.

  Response: We thank the reviewer for this important comment. We clarify that dichloromethane washing was applied to remove the majority of PAHs potentially formed during plasma-based methane conversion; however, it cannot be excluded that trace amounts of PAHs remained in the final SC_{plas} material. This residual presence may explain the discussion of possible toxic effects on microbial respiration.

  Revision in manuscript: The text has been revised as follows: "The SC_{plas} was washed with dichloromethane to remove most of the PAHs that could have formed during plasma formation with methane, but traces remained."

- 20, L. 481: Results for PAH levels in SC_{cat} are mentioned. These results do not appear in result section nor does the method for determination appear in methods section. Please make sure this is corrected.

  Response: We thank the reviewer for pointing this out. We have added the PAH analytical method to the Materials and Methods section. Phenanthrene and pyrene in SC_{cat} were determined in triplicate by GC-MS/MS after accelerated solvent extraction (ASE) and expressed as mg kg^{-1} dry material. Because these data characterize the carbon materials, the SC_{cat} PAH concentrations were added to Table 1, and the Discussion now refers explicitly to Table 1.

  Revision in manuscript: PAH method added to Materials and Methods; SC_{cat} PAH data added to Table 1; Discussion revised to refer to Table 1.

- 21, L. 501: Results for PAH levels in SC_{plas} are mentioned. These were also not mentioned earlier. Please make sure this is corrected.

  Response: We thank the reviewer for this comment. We have added the PAH analytical method to the Materials and Methods section. Phenanthrene and pyrene in SC_{plas} were determined in triplicate by GC-MS/MS after accelerated solvent extraction (ASE) and expressed as mg kg^{-1} dry material. The SC_{plas} PAH concentrations were added to Table 1, and the Discussion now refers explicitly to Table 1. We also revised the wording to present residual PAHs only as a possible contributing factor to the biological responses.

  Revision in manuscript: PAH method added to Materials and Methods; SC_{plas} PAH data added to Table 1; Discussion revised accordingly.

1. **Incorrect citation structure:** There are numerous examples of incorrect citations, where names appear inside brackets, although in these cases they should be outside, whilst only years should be inside brackets. This list is not guaranteed to be conclusive:

- 8, l. 218: *"(...) the study of (Ingwersen, 2001)".*

  Response: Corrected throughout the manuscript. Author names were moved outside parentheses where narrative citations were required, e.g., "Ingwersen (2001)".

- 19, l. 435: "*These findings align with (Bordoloi et al,. 2021).*"

  Response: Corrected. The sentence was revised to: "These findings align with Bordoloi et al. (2021)."

- 19, l. 436*: (Bordoloi et al., 2021) investigated how (…)".*

  Response: Corrected. The sentence was revised to: "Bordoloi et al. (2021) investigated how …"

- 20, l. 457: *"The results are consistent with those of (Blanco-Canqui, 2017)".*

  Response: Corrected. The sentence was revised to: "The results are consistent with those of Blanco-Canqui (2017)."

- 20, l. 471: "*According to (Jing et al., 2022; Yao et al., 2014)*"

Response: Corrected. The citation format and sentence structure were revised for clarity.

- 20, l. 484: *"In the experiment of (Van Zwieten et al., 2010)"*

  Response: Corrected. The phrase was revised to: "In the study by Van Zwieten et al. (2010), …"

- 20, l. 515: *"A study by (Wu et al, 2012) found…"*

  Response: Corrected. The citation was revised to: "A study by Wu et al. (2012) found …"

- 23, l. 566: *"(…) previous research by (Edah et al., 2020) and (Villagra-Mendoza and Horn, 2018)"*

  Response: Corrected. The citation was revised to: "previous research by Edeh et al. (2020) and Villagra-Mendoza and Horn (2018)."

- 23, l. 576: *"with (Edeh et al, 2020)"*

  Response: Corrected. The citation was revised to "consistent with Edeh et al. (2020)."

- 23, l. 590: *"(Wei et al, 2023) emphasized (…)"*

  Response: Corrected. We revised the sentence structure so that the citation now follows the correct format, with the author's name outside the parentheses: "Wei et al. (2023) emphasized …"

1. **Faulty citations.** You seem to cite things not explicitly mentioned in referenced papers.

- 9, l. 236: You cite Cao et al (2008) for ammonium-nitrate extractions yielding bio-available heavy metals, but the paper mentions only EDTA extractions, not ammonium-nitrate extractions. However, Meers et al., (2007) do mention ammonium-nitrate extractions. Perhaps you meant to cite this paper instead. Please check this.

  Response: Thank you for pointing this out. We checked the cited references and corrected the attribution. Cao et al. (2008) was retained for EDTA extraction, while Meers et al. (2007) was cited for ammonium-nitrate extraction.

  Revision: The citation for ammonium-nitrate extraction was changed from Cao et al. (2008) to Meers et al. (2007).

- 19, l. 421: *"A plausible explanation for this pattern is that SC_{plas} aggregates within the soil matrix, creating stone-like clusters that block pore connectivity and hinder water retention (Ajayi and Horn, 2016; Hardie et al, 2024)".* The way you cite implies that the studies examined SC_{plas}. Both studies were on biochar. The reference should be rewritten to include something like: *"(…) as suggested by several studies (Ajayi and Horn, 2016; Hardie et al, 2024) studying biochar. Their findings might also apply to SC_{plas} due to…".* When I investigate the articles I cannot find anything which backs up the claim that stone-like clusters decrease water retention. Please check this.

Response: Thank you for this comment. We agree that the previous wording could incorrectly imply that Ajayi and Horn (2016) and Hardie et al. (2014) investigated SC_{plas}. We revised the sentence to clarify that these studies examined biochar-amended soils. We also clarified that the "stone-like clusters" were our own visual observation during sample preparation, not a result reported in those studies. The proposed mechanism is now presented as a plausible but untested explanation.

Revision: The sentence was revised to distinguish our observation of SC_{plas} clustering from mechanisms reported for biochar-amended soils.

- 21, L. 521: "*In the silty loam soil, the toxic effect of SC_{plas} was likely to be neutralized by the high adsorption capacity of clay particles, which could bind PAHs and reduce their bioavailability (Yu et al., 2024)*" You point out that clay is likely the neutralizer of PAHs, however it seems off, when the article you are referencing concluded that SOM is more dominant than clay in adsorption of PAHs. Why not use SOM as a possible explanation? Yu et al (2024) do not directly claim that clay can neutralize toxic effect by binding PAHs and reduce bioavailability. Rather they claim that dissolved organic matter does this, whilst acknowledging that clay minerals and texture also play a role in the binding of PAHs. Be careful of the phrasing in the above example and please rephrase. Further, I suggest noting that you assume that your silty loam soil contains the common clay particles of high adsorption capacity e.g., kaolinite, smectite and illite, as you did not determine the adsorption capacity of your clay minerals.

  Response: Thank you for this valuable comment. We agree that the original wording did not sufficiently account for the relative roles of soil organic matter (SOM) and clay minerals in PAH sorption, and that the citation used was not fully appropriate. We have therefore replaced Yu et al. (2024) with Zhao et al. (2022), which more appropriately describes PAH adsorption by clay minerals. Considering the relatively low organic carbon contents in both soils and the comparatively high clay content (22%) in the silty loam soil, we consider clay minerals to be the most likely important sorbent controlling PAH bioavailability under the conditions of this study. However, SOM may still contribute to sorption processes, although its role is considered secondary in this case. We have revised the manuscript accordingly to reflect this interpretation and to avoid overstatement regarding the relative importance of individual soil components.

  Revision in manuscript: we have rephrased the text: "In the silty loam soil, the reduced toxicity of SCplas may be explained by decreased bioavailability of PAHs due to sorption processes, likely associated with clay minerals (22% clay content). Clay minerals are known to act as effective sorbents for polycyclic aromatic hydrocarbons (PAHs) in soils (Zhao et al., 2022). Soil organic matter (SOM) may also contribute to sorption, but due to the relatively low organic carbon content and the lack of mineralogical analysis, its role cannot be quantified in this study."

- 22, L. 547: "*Conversely, SC_{cat} may have introduced responsible organic components and has a lower surface area anyway, thus resulting in increased Cd solubility (Blanco-Canqui, 2017).*" Your referencing implies that Blanco-Canqui (2017) investigated SC_{cat}. They investigated biochar and do not mention cadmium, solubility nor surface areas in relation to solubility. I am unsure what exactly from the paper by Blanco-Canqui you aimed to reference. Please mention explicitly that Blanco-

Canqui studied biochar. I recommend rephrasing this sentence and leaving out "anyway".

Response: Thank you for pointing this out. We agree that the previous sentence was speculative and that the citation to Blanco-Canqui (2017) was not appropriate in this context. Since this part of the discussion was removed during revision, the problematic wording and citation are no longer included in the manuscript.

Revision: The sentence formerly at p. 22, l. 547 was deleted.

1. **Suggested extended use of your own references**

- Beesley et al (2010) could be used as a reference to compare similar results for biochar on Zn and Cd mobility and differing results for Cd.
- Ajayi (2016) could be useful to compare biochar effects on hydraulic conductivity and plant available water, as results are similar for the silty loam soils, although a different wood pyrolysis temperature was used.

Han et al (2021) could be used to discuss effects of pH on earthworm behavior and genotoxic effects of PAHs on earthworm DNA, in the part where you discuss earthworm response to PAHs (P. 15 l. 522)

Response: Thank you for your recommendation. We will use the information that PAHs may have a genotoxic effect on the DNA of earthworms for discussion.

Revision in manuscript: We used a reference to Han et al (2021) : "In addition, Han et al (2021) demonstrated the genotoxic effects of PAHs on the DNA of earthworms. (Han at al., 2021)."

Minor issues

1. 1, l. 31: I suggest not only cautioning against hydrophobic effects in sandy soils, but also ecotoxicological effects.

   Response: We agree. The abstract now mentions both hydrophobicity-related limitations and ecotoxicological responses.

   Revision in manuscript: Abstract revised.

2. 3, l. 72: Perhaps reference (TITAN Project) rather than inserting a link. The link should instead appear in the reference list.

   Response: We agree. We revised the wording and moved the project link to the reference list or appropriate project citation format.

   Revision in manuscript: Introduction/reference list revised.

3. 4, l. 106: You chose the application rate based on previous experiments with biochar. I suggest explaining why you chose the rate – did the results of cited studies present evidence why this application rate is optimal?

   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.

4. 9, l. 247: I do not think it is appropriate to say "their" sandy soils when referencing Streck and Richer (1997).

   Response: We agree and corrected the wording.

   Revision in manuscript: Text revised.

5. 11, Table 4: The porosity is the same for all treatments and the whole column could be removed instead explaining in table text that the porosity is the same.

   Response: We agree. Since porosity was calculated using the same assumed bulk density and particle density for all treatments, the repeated column was removed, and the common porosity value was stated in the table note.

   Revision in manuscript: Table 5 revised.

6. 10, l. 285: An 's' appears at the end of the line

   Response: Thank you. The typographical error was removed.

   Revision in manuscript: Typographical correction made.

7. 12, l. 316: You mention that SC_{cat} and SC_{plas} reduced CO_2 produced in silty loam soil. However, so did graphite and biochar. I think this should be included in the observation.

   Response: we agree that graphite and biochar also reduced CO_2, so we included this in the observation.

   Revision in manuscript: the text has been revised to include graphite and biochar, which also reduce CO_2 emissions: "In the silty loam soil, none of the amendments increased CO_2 production, but SC_{plas} and reference materials slightly reduced it (Fig. 4B)."

8. 12, l. 317: You refer to Fig S1A and B, which do not exist in the paper. Do you mean Fig 4?

   Response: Thank you for your clarification. No, we are not referring to Figure 4, but rather Figures S1A and B, which are included in the Supplement document.

9. 14, Figures 7A/7B: Missing a legend, since the total figure (7A-D) is cut in half.

   Response: Thank you for identifying this mismatch.

   Revision in manuscript: The figure legend for Figure 7 has been updated.

10. 15, l. 366: You have a space too much before "The avoidance"

    Response: Thank you for identifying this mismatch.

Revision in manuscript: We removed the space before the word "Avoidance".

11. 17, Figure 10: Missing figure text on letter annotations indicating statistical differences.

    Error bars are unclear on multiple bars or perhaps errors are so small, they are nearly invisible - if so, that should be clarified. Figure refers to “C: control”, which is unnecessary, as the legend labels it “Control” and not “C”.

    Response: We agree. We revised the caption to define error bars and statistical letters, clarified that very small error bars may be hidden by symbols or bars, and removed redundant “C: control” wording.

    Revision in manuscript: Figure 8 caption and legend revised.

12. 17, l. 403: You state that for Ni, SC_{plas}, biochar and graphite reduced concentrations below detection limits at Neckarwestheim. This is inconsistent with figure 11C, which shows a detected level for biochar.

    Response: We agree. We corrected the sentence so that it matches Figure 9C and does not state that biochar was below detection limit if a detected value is shown.

    Revision in manuscript: Results/discussion text revised.

13. 18, Figure 11: Refers to “C: control”, which is unnecessary, as the legend labels it “Control” and not “C”.

    Response: We agree and removed the redundant label explanation.

    Revision in manuscript: Figure 9 caption revised.

14. 19, l. 440: Unnecessary to start the sentence by “Other than in the sandy soil, (…)”

    Response: We agree and simplified the sentence.

    Revision in manuscript: Text revised.

15. 20, l. 490: Claiming that high crystallinity and low reaction surface of SC_{cat} likely ensure long-term stability as indicated by non-enhanced CO_2 production seems a bit of a stretch. More evidence is required to determine that SC_{Cat} possibly ensure long-term stability.

    Response: We agree that this statement is exaggerated, so we have removed it.

    Revision in manuscript: Removal of the statement high crystallinity and low reaction surface of SC_{cat} likely ensure long-term stability as indicated by non-enhanced CO_2 production seems a bit of a stretch.

16. 21, l. 491 and l. 493: Do not include the title of Table 1 in the bracket.

    Response: Thank you for identifying this mismatch. We have removed the table title.

    Revision in manuscript: Table title removed.

17. 22 l. 542: Do not say “other studies” when you only reference one single study.

    Response: We agree and changed the wording to singular.

Revision in manuscript: Text revised.

18. 22 l. 562: Do not use "studies" in plural when you only refer to one single study.

    Response: We agree and changed the wording to singular.

    Revision in manuscript: Text revised.

19. 23, l. 589: Once again you state that for Ni, SC_{plas}, biochar and graphite reduced concentrations below detection limits at Neckarwestheim. This is inconsistent with figure 11C, showing a detected level for biochar.

    Response: We agree. We corrected the sentence to match Figure 9C.

    Revision in manuscript: Discussion revised.