Dear authors,
The three reviewers agree in the potential interest of the research shown in your manuscript entitled “Fresh and degraded maize shoot and root residues temporarily change soil hydraulic properties” for publication in SOIL, although the indicate clearly the need to address several limitations in the manuscript in its current form
The major of these limitations can be summarized in:
1- The need to address the major limitations of the study more thoroughly in the Discussion section, like the lack of truly “fresh” measurements, the inclusion of only one soil type, the robustness, the absence of direct measurements supporting several of the proposed mechanistic interpretations, and the limitations involved in extrapolating results obtained under controlled laboratory conditions to field applications,
2- Clarifying the differences between the current manuscripts and another entitled “Integrating the Multi-Step Flux Method for full-range soil hydraulic characterization: from saturation to oven dryness”, currently under review for this same journal.
In the answer to these comments, the authors seem to be able to address most of these concerns in a revised version of the manuscript, although some of the still not properly answered. Among these comments that remain not fully answered still the need to properly clarifying the differences between the current manuscripts and the one “Integrating the Multi-Step Flux Method for full-range soil hydraulic characterization: from range soil hydraulic characterization: from saturation to oven dryness” justifying why they should be published separately.
For these reasons my recommendation to the editor is that this manuscript should be returned to the authors requesting major modifications addressing all the issues raised by the reviewers, not forgetting justifying the need of having two different manuscripts.
Thanks for considering SOIL for submitting your interesting research.

Answer:

We thank the editor to address the open questions regarding the two manuscripts currently under review in SOIL and we would like to explain that the two manuscripts have fundamentally different objectives and therefore warrant separate publication.

The manuscript “Integrating the Multi-Step Flux Method for Full-Range Soil Hydraulic Characterization: From Saturation to Oven Dryness” is a technical note that focuses on methodological development. Its primary contribution is the integration of the Multi-Step Flux (MSF) approach with commonly used measurements of saturated hydraulic conductivity and evaporation-based methods, enabling a more complete characterization of soil hydraulic properties, particularly in the near-saturated range. This methodological advancement fills an important gap in the determination of unsaturated hydraulic conductivity close to saturation and provides valuable information for investigating the effects of soil structure on hydraulic properties. While similar MSF concepts have previously been developed by other groups (e.g., Weller et al., 2011, 2012), our contribution lies in integrating these measurements within a unified workflow applied to the same sample.

In contrast, the present manuscript addresses a completely different scientific question, namely the effects of crop residues on soil hydraulic properties and the feasibility of determining hydraulic properties of crop residues themselves. Measuring hydraulic properties of pure crop residue materials represents a methodological challenge in its own right and is central to the objectives of this study. The focus is therefore on understanding

residue-induced changes in hydraulic behaviour rather than on advancing measurement methodology.

We also note that the Multi-Step Flux method was not applied in the present experiment because it had not yet been fully developed for this purpose at the time the measurements were conducted. In particular, the specialized sample containers developed for the 100% crop residue treatments were not yet compatible with the MSF setup. Furthermore, the experimental design required the timely measurement of 24 samples before incubation and 19 samples after incubation. Given that only a single MSF station was available and that a single MSF measurement required approximately one day per sample, it would not have been feasible to complete the measurements within the necessary timeframe. This was especially critical because biodegradation of the crop residues was itself a major process under investigation and imposed strict temporal constraints on the measurements. was not the subject of this study, it is therefore unclear to us how and why these two manuscripts, which differ significantly from one another, should be linked or even combined.

Nevertheless, we fully agree that information on hydraulic conductivity in the near-saturated range would have strengthened the study and provided additional insight into the observed effects. We have therefore expanded the discussion to acknowledge this limitation more explicitly and to highlight the potential value of future MSF measurements. Specifically, a new section, 4.3 Study Limitations and Extrapolation to Field Conditions, has been added, where we discuss the absence of near-saturated hydraulic conductivity data as a limitation and outline opportunities for future research using the MSF approach. In this section we also address the inclusion of only one soil type, the robustness, the absence of direct measurements supporting several of the proposed mechanistic interpretations, and the limitations involved in extrapolating results obtained under controlled laboratory conditions to field applications as requested.

The lines given below correspond to the changes shown in the manuscript with tracked changes.

Referee #1:

This laboratory study evaluates the effect on soil hydraulic parameters (SHP) after mulching and incorporation of maize crop residues in a loam soil.

The authors presented a robust protocol for determining soil hydraulic characteristic curves of packed soil–residue samples with different concentrations and pre- and post-incubation treatments, using a combination of HYPROP and WCR4 devices.

As noted by the authors, the effects are mainly observed at low matric suction. I would therefore question why unsaturated flow was not measured directly using tension devices in the near-saturation range (e.g., using a KSAT device), instead of relying on an indirect method based on fitting procedures, which reduces K_s to merely a fitting parameter.

I understand that this is a posteriori analysis, although it is not far from the authors' initial hypothesis and is not entirely unexpected, given that the same authors adopted this approach in a parallel manuscript currently under review for this same journal, "Integrating the Multi-Step Flux Method for full-range soil hydraulic characterization: from saturation to oven dryness."

I therefore suggest that the authors consider discussing the potential integration of such direct measurements within their protocol, as well as clarifying the differences between the two manuscripts, with particular reference to the potential advantages for future research of integrating the present protocol with a Multi-Step approach.

A further consideration for the authors: although I have not reviewed the other paper in detail, the authors might wish to consider positioning the two manuscripts as companion papers, one being more methodological and the present one an application. I leave this suggestion for their consideration.

Overall, I appreciated the authors' clarity and the linearity of their presentation of the results, and I believe the manuscript could be of interest to SOIL's readers.

Answer:

We thank the reviewer for the positive assessment of our work and for the constructive suggestions regarding the determination of saturated and near-saturated hydraulic conductivity.

We fully agree that direct measurements of saturated and near-saturated hydraulic conductivity would have provided valuable additional information, particularly because the strongest effects of crop residues on soil hydraulic properties were observed at low matric suctions. For this reason, we have expanded the Discussion and added a dedicated section (4.3 Study Limitations and Extrapolation to Field Conditions; Lines 474 – 512) to explicitly address this limitation and to highlight opportunities for future research.

There are several reasons why direct measurements of near-saturated hydraulic conductivity were not included in the present study which are explained in detail in the following.

First, the study design required that measurements were conducted within a relatively short time frame after harvest. Prolonged measurement periods and extended phases of complete saturation (anaerobic conditions) could have altered residue decomposition and microbial activity, thereby affecting the properties that were the focus of the investigation. Therefore, we tried to avoid biological feedback effects caused by significant changes in the micro-environmental conditions within the sample.

Second, the Multi-Step Flux (MSF) approach mentioned by the reviewer was not yet fully established in our laboratory when the experiments for this study were conducted. In particular, the method had not yet been adapted for the custom sample containers developed for the 100% crop residue treatments. Furthermore, the experimental design required measurements of 24 samples before incubation and 19 samples after incubation. At that time, only a single MSF station was available, and one MSF measurement required approximately one day per sample. Consequently, it would not have been feasible to characterize all samples within the required timeframe while preserving the temporal consistency that was essential for studying residue biodegradation. We therefore focused on obtaining robust water retention and unsaturated hydraulic conductivity data using the HYPROP-WP4C workflow, which itself required methodological adaptations to characterize samples consisting entirely of crop residues.

Third, the primary objective of this manuscript differs fundamentally from that of our companion manuscript entitled "Integrating the Multi-Step Flux Method for Full-Range Soil

Hydraulic Characterization: From Saturation to Oven Dryness." The latter is a methodological contribution that focuses on extending hydraulic characterization into the near-saturated domain by integrating the MSF approach with established methods. In contrast, the present manuscript addresses a distinct scientific question: how maize shoot and root residues, their incorporation (mulching versus incorporation), and their decomposition affect soil hydraulic properties, and whether hydraulic properties of crop residues themselves can be determined reliably. Thus, while the two studies are complementary, they have different objectives, experimental approaches, and scientific contributions.

We agree with the reviewer that future studies would benefit from combining the protocol presented here with direct measurements of saturated and near-saturated hydraulic conductivity when adapting the protocol to avoid long periods of anaerobic conditions. Such measurements would improve the characterization of flow processes close to saturation and could provide additional insights into residue-induced changes in soil pore structure. We have incorporated this perspective into the revised Discussion and explicitly identify the integration of Multi-Step Flux measurements as a promising avenue for future research.

Referee #2:

This manuscript investigates the effects of fresh and degraded maize shoot and root residues on soil hydraulic properties (soil water retention and hydraulic conductivity) using controlled laboratory experiments. The topic is relevant to soil physics, residue management, and agroecosystem water dynamics. The study provides valuable experimental data on the temporal evolution of hydraulic properties following crop residue incorporation and decomposition. The manuscript presents valuable and potentially publishable findings. However, several methodological limitations, particularly regarding statistical robustness, interpretation of hydraulic conductivity results, decomposition during measurements, and extrapolation to field conditions, require further clarification and discussion.

1. The study relies on a relatively small number of replicates (mostly n = 3), and in several cases the number of replicates decreases after incubation due to biomass loss. The authors acknowledge this limitation, yet strong conclusions are drawn regarding differences between treatments and decomposition effects. Confidence intervals are reported for fitted parameters, but no statistical comparison is provided for the hydraulic curves themselves. A more rigorous uncertainty analysis and statistical evaluation of treatment effects on hydraulic parameters is needed.

   Answer: Preliminary experiments demonstrated a high reproducibility of hydraulic measurements on repacked samples when a strictly standardized packing protocol was followed, as described in the manuscript and as shown by the control samples (Lines 120 -126, Fig. 2 and confidence intervals of fitting parameters presented in Table 2, Fig. S2 in the new supplementary material). Based on these findings, we considered three replicates per treatment sufficient for the objectives of this study. Note, that for the 100% CR samples, we had more replicates since higher uncertainty was expected (roots = 4 and shoot = 6 replicates).

   The selected experimental design allowed us to investigate a wide range of shoot and root residue concentrations simultaneously while ensuring identical plant

material quality and environmental conditions across treatments. This required a substantial number of HYPROP devices, climate-controlled laboratory space, incubation capacity, and additional samples for carbon and nitrogen analyses.

Nevertheless, we agree that the limited number of replicates contributes to uncertainty in the estimation of soil hydraulic parameters, particularly for treatments where the number of valid replicates decreased after incubation. We therefore acknowledge that the statistical power for detecting treatment effects is limited.

In response to this comment, we performed a bootstrap analysis (See lines 168 -176) for all tested treatments to derive the 95% confidence intervals of each fitting parameter plus to present the uncertainty of the fit as confidence bands for 100% CR in Figure 4 and for CR-soil mixtures in Fig. S2 in the new supplementary material. We now present the 95% confidence intervals for each fitting parameter, the RMSE for water content and conductivity to evaluate the overall performance of the fit (Table 2 and Table 3). For the CR-soil mixture, we prefer to present the raw measurement points in the manuscript rather than the fitted curves because it highlights the consistency of the packing, measurements and the process due to biodegradation of CR. We also address the reviewer concerns in the discussion (Lines 420 - 440 and new section 4.3).

2. One important methodological concern is that residue decomposition already occurred during the first HYPROP measurements, as acknowledged by the authors. Consequently, the "fresh residue" treatment was not truly fresh throughout the measurement period. This complicates the interpretation of treatment effects and weakens the distinction between pre-incubation and post-incubation conditions. The implications of this limitation should be discussed more explicitly, preferably in a dedicated subsection of the Discussion.

   Answer: We agree and acknowledge that some decomposition occurred during the HYPROP measurements of the pre-incubation treatments as shown by the presented carbon and nitrogen measurements. Consequently, the term "fresh residue" should not be interpreted as completely undecomposed residue throughout the measurement period. Rather, it refers to residue that was analysed immediately after sample preparation and prior to the dedicated incubation treatment.

   The intended contrast of our experimental design was therefore between pre-incubation and post-incubation conditions, rather than between completely undecomposed and strongly decomposed residues. While biological decomposition likely commenced during the hydraulic measurements, this period was short compared to the incubation and thus still represents the earliest stage of decomposition investigated in this study.

   We acknowledge that decomposition is a continuous process and that the distinction between treatments should therefore be interpreted as differences in the degree of decomposition rather than as discrete states. Since we distinguished between fresh, pre-incubation, and post-incubation treatments in the carbon and nitrogen analyses, we will revisit the terminology used throughout the manuscript to ensure consistency and clarity. For the soil hydraulic measurements, however, only pre-incubation and post-incubation conditions were investigated, and this

distinction is already consistently reflected in the figures and tables. We address this concern now by presenting a carbon decay model fitted to all treatments (Lines 188 -199) which shows the continuous and fast decay depending on time and temperature during the experimental duration. We further clarify this point in the revised manuscript in the abstract (lines 22 – 30) and expand the discussion of the implications of decomposition during the hydraulic measurements as a study limitation (see section 4.3).

The study was conducted using a single loam soil under controlled laboratory conditions. Since soil texture strongly influences residue-soil interactions and hydraulic behavior, the conclusions should be restricted to the investigated soil type. The manuscript occasionally presents findings in a generalized manner that may not be applicable to sandy or clayey soils.

Answer: The study focuses on the influence of crop residues on soil hydraulic properties and on the properties of the crop residues themselves. Since the effects observed occurred primarily in the range of low pF values and corresponded to newly formed mesopores and macropores, this suggests that the influence of crop residues on the soil's hydraulic properties depends mainly on the crop residues themselves and less on soil texture. Nevertheless, we can only discuss this in relation to our soil texture and based on existing literature. We will therefore take great care to present our results with a focus on loamy soils.

3. The discussion concerning unsaturated hydraulic conductivity remains somewhat speculative. The authors themselves note that conductivity measurements were limited to specific pF ranges and that fitting parameters showed substantial uncertainty. Given these limitations, conclusions regarding conductivity changes should be presented more cautiously. Additional discussion of the reliability of the conductivity estimates is required.

   Answer: We thank the reviewer for this important comment and agree that the interpretation of the unsaturated hydraulic conductivity results should be made with appropriate caution.

   In particular, the estimates for the 100% crop residue treatments are associated with considerable uncertainty because hydraulic conductivity measurements were available only over a limited matric potential range, resulting in greater uncertainty in the fitted hydraulic conductivity functions and parameters as now presented by the bootstrapping approach in Fig. 4. We discussed internally whether these results should be included at all. However, given the lack of comparable data on the hydraulic properties of pure crop residue materials in the literature, we considered it valuable to present these results while clearly highlighting their limitations and uncertainty. In the revised manuscript, we rephrase the discussing accordingly (see lines 424 – 440) and focus mainly on the measured potential range.

4. The manuscript attributes many observed changes to modifications in pore architecture, residue morphology, and soil-residue interfaces. However, no direct measurements of soil structure (e.g., X-ray CT imaging, pore-size distribution analysis, microscopy) were conducted. As a result, several mechanistic explanations remain hypothetical. This limitation should be emphasized more clearly.

Answer: We thank the reviewer for this comment. Yes, unfortunately, we did not have access to X-ray CT at the time, which would have allowed us to conduct explicit analyses of the effect of crop residues on soil structure. However, in the new version of the manuscript, we now present and discuss the equivalent pore size distribution derived by the water retention curve. This was an excellent idea for discussing changes in pore size classes by the mixing and biodegradation of crop residues. See Figure S3 in the supplementary material as well as lines 174 – 176, lines 240-244, and section 4.1 in the discussion.

5. The experiments were conducted under constant temperature and humidity conditions that do not represent field environments where wetting-drying cycles, rainfall, biological activity, and root growth occur simultaneously. A dedicated discussion is needed regarding how these laboratory findings can be extrapolated to field conditions and what limitations exist for practical agricultural applications.

   Answer: As mentioned in a previous response to Reviewer I, we now explicitly address the study's limitations in the new version of the manuscript. The conditions of the experiment are significantly less dynamic than in field trials; nevertheless, we also included a wetting/drying cycle as a result of the experimental setup (pre-incubation/incubation/post-incubation), two climatic conditions (HYPROP/incubation), and rooting should also not play a role in the post-harvest phase in the field. But of course, this laboratory experiment primarily focused on investigating the mechanism and provide a data base for modelling concepts which were not available so far.

Referee #3:

Leuther et al. present the results of a controlled incubation experiment investigating how the incorporation and degradation of maize shoot and root residues affect soil water retention and unsaturated hydraulic conductivity. The study addresses a relevant question and provides potentially valuable experimental data on the temporal dynamics of soil hydraulic properties following crop residue addition. The manuscript is generally clearly written and structured, and I agree that the work could be of interest to readers of SOIL.

Generally, I agree with the comments of Reviewer 2, particularly regarding the need to address the major limitations of the study more thoroughly in the Discussion. These include the lack of truly "fresh" measurements because decomposition already occurred during the initial HYPROP measurements, the inclusion of only one soil type, the absence of direct measurements supporting several of the proposed mechanistic interpretations, and the limitations involved in extrapolating results obtained under controlled laboratory conditions to field applications.

However, I have a couple additional concerns:

1. I do not understand the basis for the repeated-measures ANOVA used to compare fresh, pre-incubation, and post-incubation C and N concentrations. A repeated-measures analysis requires observations from the same independent experimental units across time. However, the Methods appear to indicate that the C and N analyses involved initial subsamples of fresh material, separate dummy samples at the pre-incubation stage, and final experimental samples at the post-incubation stage. Please clarify which experimental units were repeatedly measured or paired across the three time points. If the observations at the different time points came from different samples, a repeated-measures ANOVA would not seem appropriate. The statement that normality and

homogeneity of variance were assumed “due to the low number of technical replicates” should also be reconsidered. Low replication makes these assumptions difficult to evaluate, but it does not provide a statistical basis for assuming that they are met. Please also clarify what constitutes a “technical replicate” and whether technical replicates were treated as independent observations in the statistical analyses.

Answer: We can see the concerns of the reviewer about the statistical evaluation and we appreciate this important observation. The observations at the three sampling stages did originate from the same pool (fresh), but the experimental units for pre-incubation and post-incubation were different and therefore do not satisfy the assumptions of a repeated-measures ANOVA. In addition, we acknowledge the concerns about the statement regarding the assumptions of normality and homogeneity of variance. The low number of technical replicates does not justify assuming that these assumptions are met. The reported replicates were technical replicates derived from pooled samples and therefore do not represent independent biological observations. Consequently, formal statistical comparisons of C and N concentrations will not be performed in the revised manuscript. We have removed the repeated-measures ANOVA from the revised manuscript (section 2.6) and focus on the descriptive presentation of the results in section 3.3.

In order to discuss and illustrate carbon turnover dynamics, we now provide an exponential decay model assuming a stable background soil carbon pool corresponding to the carbon concentration measured in the control treatment for the CR soil mixtures and a relative more stable pool in 100 % CR samples taken from literature (Lines 191 – 201 and Figure 5f). The model provides temporal trends of C and an estimate of the turnover of plant-derived residues in our experimental setup while considering day and temperature during the experiment. The derived C decomposition rates are very similar to data presented in the literature, for example a study by “Elias et al 2024: Microbial and mineral interactions decouple litter quality from soil organic matter. Nature Communications” where the authors investigated respiration of fresh litter and the interactions between litter quality and soil mineralogy.

2. The basis and interpretation of the value of 1.45 vol.% per g C kg^{-1} reported in the Abstract require clarification. As I understand it, this value appears to be the arithmetic mean of four separate regression slopes from Figure 6 (1.5 and 1.5 at pF 0 and 1.8 pre-incubation, and 1.5 and 1.3 post-incubation). If so, this averaged value is not itself a directly estimated relationship and combines regressions from different matric potentials and experimental stages. Please explain how this value was derived and clarify the rationale for presenting it as a single quantitative effect of C concentration on water retention.

   Answer: The 1.45 vol.% per g C kg^{-1} referred to the previous sentences, which discussed water content at saturation and field capacity. The value was presented here as the mean slope of both regressions for the experiments conducted after incubation. You are correct that this was not entirely clear and we rephrased the section and present now the individual slopes (see lines 34 - 38).

Minor comments:

1. The reported soil texture fractions sum to 90% (33% sand + 48% silt + 9% clay), rather than 100%. Please check these values.

Answer: Corrected. The soil texture consists of 33% sand + 48% silt + 19% clay (Line 108).

2. The caption of Table 2 states that 95% confidence intervals are shown in parentheses, but these do not appear to be presented in the table. Please check the table or caption.
Answer: Thank you very much for your thoughtful reading of the manuscript. We now present the confidence intervals of the fitting parameter and the RMSE of water content and k (See Table 2 and Table 3, Fig. S2 in the new supplementary material).

3. The fresh root N concentration is reported in the Results as 0.7 g kg^{-1}, whereas Table 4 reports 7.1 g kg^{-1}.
Answer: Corrected (Line 294). As reported in Table 4 and Figure 5 the initial N concentration was 7.1 g kg^{-1}.