We note our responses are indented and in blue.

We sincerely appreciate Reviewer 1's remarks, which have significantly helped us improve the quality of our article. Our responses can be found below.

1. General comments:

The study by Giraud et al. (2025) presents a novel modelling framework that enables to simulate plant growth and related C and water flow within the plant, in interaction with a soil model simulating the interactions between C inputs from roots and soil microbial dynamics. Using this new and exciting framework, the authors have investigated in silico the effect of drought on soil C dynamics in the rhizosphere of a young hypothetical cereal plant, taking into account the effect of dry spells on root C inputs and on their dynamics in the soil. Considering this impressive piece of work, authors have already done a good job in synthetizing the main features of the coupled models and of their simulation outputs, although the manuscript remains quite dense and demanding for a reader not familiar with the original models.

We will reorganise the article, add explanatory graphics and shorten the plant-related sections to make the work clearer to the readers, using the recommendation of reviewer 1. We explain our intended adaptations in response to the detailed reviewer's comments below.

Besides the detailed comments below, here are my main remarks:

1. The present work is built on two main models described in Giraud et al. (2023) and Sircan et al. (2025), which have been further improved and coupled here. Given that both previous articles have already tackled some of the issues described here (e.g. effects of water stress on plant growth and exudation, C dynamics in the rhizosphere), I would recommend the authors to try to focus more on the benefit of the coupling when describing the simulation results or discussing them, i.e. to focus on the effect of drought on root-derived C and their effects on SOC dynamics in the rhizosphere, while reducing the length of other, less important parts of the manuscript.

We will shorten the results and discussion section with respect to the plant part. However, in line with some of the detailed comments of the reviewer, we will keep details on the description of the plant processes to point out relevant differences to the previous paper. In the discussion, as recommended as well by reviewer 2, section 4.3 will be expanded.

2. Only few details are given in the main manuscript regarding the soil model, although some are indicated in Appendix. I think it is especially important to mention in the Material & Methods which soil processes have been taken into account or not in the model (not necessarily the equations), and how soil water content and soil temperature may affect microbial activity, so that the reader is able to better interpret the simulation results. In particular, I understood that the dynamics of the bulk soil organic matter is not represented, but only the dynamics of dissolved C. In this regard, the discussion on rhizospheric priming effect is interesting, but may be limited if all the "native" soil organic matter is not included in the model but only its labile part.

We have added figures 1, 2, and 3 for this purpose, but we will update these figures to clearly list the processes that are represented. We will underline how water

content may affect the microbial activity in more detail, as the link between water and microbes is one of the main advancements of the soil model. Currently, the effect of the soil temperature is not implemented.

The discussion in section 4.3 about the priming effect will be expanded to include this additional limitation of the represented carbon.

3. The study focuses on the effect of drought, but here and there a change in temperature is mentioned during the drought period, without information about this in the Material & Method. If temperature changed over time or among the scenarios, this should be clearly stated; if not, this should also appear explicitly in the M&M.

In the current version of the model, the temperature affects only the plant processes and not the soils (as shown in Figure 3). We will add a table to show the weather conditions during the baseline and drought conditions.

4. I keep struggling to understand the link between macroscale voxels and microscale domains of root segments or outside root segments… As it seems to be an important feature of this work and could be inspirational for other similar modelling frameworks, I would recommend authors to illustrate this a bit more clearly in Fig. 1, using realistic volume dimensions and showing which volumes/segments are included within which types of voxels.

We will update figure 1 for a better illustration of the coupling framework.

Given the number of variables investigated, crossing three scenarios of drought with three sets of soil parameters makes it quite challenging to get a clear picture of the main results in the end. An idea to clarify things for the reader could be to formulate some hypotheses about the effects of drought on root-derived C inputs to the soil and their dynamics in the rhizosphere, and to use the simulation results to validate or invalidate them.

We will add compact hypotheses to the introduction, to help guide the discussion section. For instance:

1. Droughts will lead to a lower microbial activity in the rhizosphere and to a lower release of plant carbon
2. Consequently, we will observe a slower soil carbon dynamic, which can lead to both an increase or decrease of soil carbon storage.

2. Detailed comments:

Title: “carbon stabilization by plant” is a bit confusing. Also, authors have only investigated the dynamics of root-derived C (not aerial residues). Maybe something more neutral: In silico analysis of carbon and water dynamics in the rhizosphere under drought conditions?

We thank the reviewer for this suggestion, we will change our title to the one proposed by the reviewer.

Abstract:

L8 (and throughout the text): I am not familiar with the use of “biokinetic” in this context; I would suggest only using “kinetic”

> We will update the term.

L11-13: The reader may also wonder whether the drought experienced in the soil affected soil microbial activity, independently on the indirect effect of drought on C input by roots

> We will evaluate the results in the bulk soil, far away from the roots. The drought had however a limited effect on the microbial activity as we do not represent the effect of temperature on the microbial activity and we only had a small decrease in water content far away from the roots.

Introduction:

L18: “and as an intra- and inter-domain signalling carrier” is not very clear to me, please explain/rephrase

> We will update the sentence to: “and as a variable connecting domains and processes […]”.

L37: “Plants can also exert direct feedback effects on themselves (e.g., aboveground-belowground feedback)” : this could be rephrased and simplified

> We will rephrase to: “Plant also have inner feedback loops, such as aboveground-belowground interactions, which affect their fitness”.

L41: What about nutrient concentration gradients in the rhizosphere?

> We will rephrase to: “Plant-soil exchanges lead to the creation of the rhizosphere, an area of soil around the root with characteristics distinctive from the bulk soil, such as rhizodeposition, microbial communities, soil structure, water and nutrient content”.

L76-79: This study extends the work of Giraud et al (2023) that focused on water and C flow within the plant. It would therefore be useful to state a bit more explicitly that this new study focuses on the root-derived C dynamics in the rhizosphere, taking into account the previous modelling framework of Giraud et al. for integrating the retroaction between water flows and C inputs in the rhizosphere, and coupling it to another model of rhizospheric soil C dynamics. Also, stating here at the end of the Introduction some hypotheses linked to water spells that will be verified with this new modeling framework would help the reader to focus a bit more on the originality of this work.

> We will change lines 76-79 to:
> “This study aimed to set up and implement a fully integrated plant-soil-microbiome model that accounts for root-microbiome interactions via the release of C and uptake of water from the rhizosphere by the roots. We present an extended version of the FSPM CPlantBox […]. The two models were coupled using an updated version of the multiscale setup of Mai et al. (2019) to obtain higher accuracy and stability. The framework was used to conduct an in-depth analysis of the root-derived C dynamics in the rhizosphere under drought conditions against baseline scenarios”.

For the hypotheses, see the “general comments” section.

M&M:

L100: Meunier et al. (2017). Rephrase “and the coupled the stomatal”.

We will rephrase to “CPlantBox simulates the plant water flow using the analytical solution of Meunier et al., (2017), and the resulting stomatal regulation.”

L123-124: This could be mentioned earlier on in the Introduction.

We will add this simplified description of the coupling in the introduction at line 88.

L126-137 and Fig. 1 & 2: After reading this several times, I am still not sure that I really understood the spatial scheme. There are macroscale soil voxels that may or not contain microscale root segments. However, both appear with the same size on Fig. 1. The terms macroscale and microscale are perhaps misleading? And what is then the perirhizal zone described as microscale in Fig. 2: one full voxel containing at least one root segment? Perhaps this could be clarified in Fig. 1, e.g. if showing a spatial description of the 3D root segments and the voxelization around it with realistic dimensions, and explaining where materials are actually exchanged and where specific reactions may - or not - take place.

We thank the reviewer for the precise description of what is unclear in the figure. We will update it to illustrate the coupling approach in more detail. Among other changes, we will make our graphic more similar to Figure 1 of Mai et al (2019) to point out the exchange of information.

We will aim to make the graphics self-explanatory in response to the reviewer’s questions: in our setup, all soil voxels are of same size, whether or not they contain a root. The perirhizal zone is described as microscale because the logarithmic scaling of the cells allows us to have very small cells around the root surface. We have therefore a much higher resolution than on the macroscale.

To avoid confusion, we will rename the macroscale model as “3D soil model” and the microscale models as the “1D soil models”.

L175: I don’t understand what $\Omega \setminus \partial\Omega$ means

$\Omega \setminus \partial\Omega$ means that the equation is valid for the whole domain except for its boundaries. We will add an explanation at first occurrence.

L219: For simplicity

We will rephrase to ‘For simplicity, the implementation of Eq. (6) for the 3D soil and 1D soil are given respectively in appendices B1 and B2.’

L225: It would be useful to remind to the reader what ξ is, even if it was introduced a few equations ago

We will explain the meaning of that variable below equation (12) as well.

L329-332: If a new calibration was introduced to better reproduce expected trends, this should be emphasized more in the discussion, e.g. if authors choose to discuss the validity of hypotheses from their simulation results.

> We will expand the discussion around L645. The calibration allowed us to have the starch storage as the regulatory variables, then exudation, while growth and maintenance were maintained.

L347: Again, an illustration of all these different scales in Fig. 1 would really help the reader to understand more quickly how these scales are interconnected

> We will add an illustration of those scales in Fig. 1.

L373: I don't think that oligotrophic and copiotrophic have been defined anywhere. For a reader not familiar with soil microbial ecology, it would be useful to define these terms, and explain what are the expected behavior of these two microbial pools.

> We will add an explanation of the oligotrophs and copiotrophs in the description of the TraiRhizo model (section 2.1.1).

L380: It would be worth adding to which class these tresholds correspond to Poeplau and Don, i.e. 0.65 (threshold between degraded soils and moderate soil quality), 0.83 (moderate/good soil quality) and 1.16 (good/very good soil quality). It would also be important to state that the pedotransfer function used here was developed for German soils. One may also wonder whether this study from Poeplau and Don at field scale across Germany is really relevant to identify hotspots of C in the rhizosphere, this could lead to additional comments in the Discussion.

> We will add the interpretations of the classes and underline that they are used to exemplify the hotspot analysis. Indeed, these thresholds were developed for a different scale. Therefore, they might not be relevant for our smaller scale model. For this reason, we did not consider it in the original manuscript.

Results:

L405: "an increasing concentration" compared to what or according to what?

> We will rephrase to: 'The simulation highlights a concentration of dissolved low molecular […] that increase with time in response to root exudation in the upper part of the root system, near its centre'.

L408-432: For brevity, I would remove this part. If these simulations results have already been presented and explained in Giraud et al. (2023), shouldn't the focus be here rather on the exudation and mucilage secretion in response to drought, starting at L433?

> Because of the re-calibration of the plant model, the results are different to the ones obtained in Giraud et al. (2023). Consequently, we found it important to explain these new results, as mentioned by the reviewer about lines L645-647. We will add this remark at the beginning of the section.

L449-450: Shouldn't this sentence be mentioned earlier in the paragraph, before showing the actual results for each soil parametrization?

We will move the sentence ‘We found an effect of the biokinetic parameterisation on exudation and mucilage release.’ to line 434: ‘Both the plant water balance and the kinetic parametrisation affected the exudation rate and mucilage release’.

L456-458: I am not sure this is a very important simulation result to emphasize, given that the variation of this maximum exudation rate per cm2 among the scenarios is quite small.

We will replace the description of the maximum exudation rate per scenario and give only the overall maximum, to be compared against experimental results in the discussion.

L464-465: I don’t understand. If there are fewer roots, why is there a higher maximum C concentration?

We will rephrase to: ‘That second peak is especially strong for earlyDry and lateDry due to lower root growth, i.e., We had root tips in that area of soil over a longer period, leading to higher concentrations’.

Fig. 8: I would suggest to increase the size of the figures and to add a title to each sub-graph with the name of the variable detailed in the caption

We will increase the figure size and separate it in several figures if needed, as well as adding the caption.

L492: I still struggle to understand the meaning of a variable perirhizal truncated volume among the treatements... Please try to better explain why it is important and which biological or physical information it actually brings.

Using the truncated values enabled to compare the concentration of the different C pools across the 1D models (by giving them the same outer radius).The complementary cumulative volume distribution allowed us to observe the concentration profiles across the perirhizal zones. We could thus underline that we obtained results at this smaller scale that differed from those in section 3.3. We will put this section (as well as section 2.4.3 and the description of the truncated 1D models) in the appendices to clarify and shorten the article. The difference between the scales can still be observed by using the sections 3.3 and 3.7.

L499 and after: Please give the full meaning of each concentration when used in the main test, and not only its symbol, as reading and understanding this part is quite challenging…

We will replace all the symbols of concentration with the full names.

L519: “the negative effects of the low soil θ on microbial activation” - but this relationship was not introduced in the Material & Method. It’s really necessary to better explain how the SOC model works and how it depends on soil water content and soil temperature.

We will update figures 1-3 to better illustrate the different interactions. The effect of water potential on the microbial community was relegated to the appendices (like all soil reactions) to put the main focus of the article on the interactions between the models.

L531: “the relative volumes of the SOC hotspots” - I continue to be lost…

We will add an explanation on the meaning and relevance of the volume-normalised hotspot volume, using the work of Landl et al (2021) at the end of section 2.4.5 .

Discussion:

L616-617: “the virtual plant’s starch pool can be interpreted as representing both actual starch reserves and newly synthesised wall material”. I am not sure that I fully understand the conceptual difference between biosynthetic growth and expansion. Is the second type of growth independent on C? I thought that root growth was explicitly included here in the C balance. If so, why would a part of the root growth be included in another term with starch?

Biosynthetic growth corresponds to the creation of new cell wall, and depends on the direct supply of carbon (Hillty, 2021, Verbanˇciˇc et al., 2018). The expansion growth corresponds to the dilatation of those walls, driven by the cell’s turgor pressure. Therefore, expansion growth depends on the supply of C but does not need to be concurrent to C supply. For this simulation, the virtual plant’s starch pool, which varied according to the C supply, can be interpreted as representing both actual starch reserves and newly synthesised wall material, thereby enabling expansion growth to occur at night.

However, as this section will be strongly shortened, we will give a simpler description of the outputs of the plant models, compared against those of Giraud et al. (2023).

L645-647: Now I understand why authors developed the description of such results at L408-432. The present statement could therefore be made earlier, at L408.

See our response to the comment of L408.

L679: a ratio of dormant oligotrophs and what other variable?

We will update the sentence to ‘a high ratio of dormant-to-total oligotrophs’.

L707: “and organic C-dependent soil hydraulic parameters” - I guess that the benefit of having simulated specifically mucilage secretion in this study would be linked to this s feedback on plant water uptake. Maybe it is worth explaining this?

We will add a longer discussion on the effect of organic C on the soil hydraulic parameters as part of an outlook on the future development of the model. The effect of organic C on soil hydraulic parameters will indeed be added to the setup in a following study.

L709-719: I find this paragraph very interesting. However I am surprised that authors do not link this heterotrophic respiration by soil microorganisms to root respiration simulated during the dry spells, as the modelling framework enables to do this, which is rather unique. Looking at how the ratio between the two sources of CO2 evolves over time and how the resulting total basal respiration evolves over drought may reveal interesting features.

We thank the reviewer for pointing to this very relevant link. We will add a figure of the proportional respiration dynamics and include this interesting aspect in section 3.5 and in the discussion after line 719.

L735-736: “The lateDry scenario led to the lowest plant growth but to a higher SOC hotspot volume, indicating a more resource-efficient root system exudation.” Why would an increase in the number of rhizospheric hotspots be considered more resource efficient? Do authors suggest that SOC hotpsots are associated to a better feedback for the plant (e.g. in terms of water or nutrient uptake)? Please explain.

Following our usage of the SOC classes of Poeplau and Don (2023), higher SOC concentrations are linked to healthier soil. However, as their classes were created for a higher scale, we will be more careful in describing the possible effects of these larger SOC hotspot volumes.

L870-871: This definition of microbial pools is really needed in the main text.

We will add a description in section 2.1.1.

3. Typos

L23: A comma is missing after “exudation)”

L289: Remove bracket

L540: cycling

L564: “can cause a the”

L598: for further optimizing

We thank the reviewer for taking the time to point out the spelling errors. These typos will be corrected and the text carefully checked for other errors.