Comments from Anonymous Referee #1

Overall comments:

This study evaluated how variation in SOC enhancements across the soil profile could contribute to alleviating drought stress on maize at a Swiss site using an agro-hydrologic model where key soil hydraulic parameters were modified based on highly validated pedo-transfer functions. The authors also tested the effects of these SOC changes under future climate conditions using downscaled climate projections. Overall, the authors found that moderate increases in SOC down to 65cm depth could provide important drought adaptation benefits in the early summer, but with diminishing returns after these thresholds.

This was a very interesting and thoughtfully designed study that adds value to the literature on SOC-water-plant-climate interactions in a few key ways. First, the authors examine the sensitivity of feedbacks of enhanced SOC (taken as given) on plant transpiration and interactions with drought conditions – an interaction that is still highly uncertain and not always a chief topic of study in hugely expanding literature on SOC and climate change. Second, the authors tackled an important question dealing explicitly with varying over depth of SOC additions – also important as emphasis increases on building SOC (particularly for climate change mitigation). Where SOC is enhanced in the soil column matters both biogeochemically and biogeophysically. Third, the study looks at these interactions and the efficacy of building SOC under current and future climate conditions – a critical consideration that should be systematically investigated with additional model runs both at the site-level and regionally (notwithstanding limitations on ascertaining SHPs at larger spatial scales). These responses obtained were explained decently, and seem to be consistent with emerging findings across the literature the authors' cited that while SOC can provide benefits in times of drought, these benefits can be limited and are regionally heterogenous.

There are a few clarifications and smaller points that I raise below for the authors to consider. I consider these to be minor revisions, however, and I'm happy to support publication of the manuscript after these are addressed.

> Many thanks for the thorough review of our manuscript and the constructive criticism that helped a lot to improve the manuscript.

Minor comments:

Introduction:

- Lines 58-63: there is discussion of soil structure here and I understand that one way SOC in part impacts the soil water retention is through changes in porosity, and structural characteristics. However, the PTFs seem to only consider soil texture, and this does also play a leading role in soil water storage and flows – an overview of the role of texture and SOC influences does not really appear in your intro despite being the basis for your PTFs, and so I wonder if a few quick sentences should be added here. I'll note that structure is rarely explicitly represented in regional, gridded models.

> Thanks for pointing this out. To clarify the point for the reader and to provide more insight, we revised a part of the introduction section as follows, lines 61-64:
>
> "Soil texture also strongly affects how soil hydraulic properties respond to organic amendments, as shown by a meta-analysis from Edeh et al. (2020), who reported decreased hydraulic conductivity of sandy soils and increased the hydraulic conductivity of clayey soils after biochar additions."

- Lines 77-92: McDermid et al 2022 also leveraged a set of PTFs for SHP calculations that included SOC, which they varied over for global simulations and found SOC declines reduced soil moisture. However, to your interesting point, they found decreases in conductivity with SOC declines, which as you highlight is textbook but opposite of what you found. I see you offer an explanation in your Discussion – and I've added a comment on this below.

> The comment and positive co-thinking about these observations are much appreciated. We think the SOC-K_s (saturated hydraulic conductivity) and SOC-soil moisture relations should not be directly linked. The former is a link between two properties, while the latter is a complex relationship as a result of various soil properties, climatic patterns and very importantly, time.
>
> Here we note that the negative relationship between SOC and K_s is not a result of our work, or of a single PTF. We are using a broadly accepted PTF that is _one of several_ broader scale studies that has found this relationship between these two properties. In our discussion we tried to find the logical link between our findings and what this PTF inherently reflects about the SOC- K_s relationship.
>
> The benefit (or lack of) of having SOC in terms of extra stored soil moisture that we present is a complex product of model simulations, in which the above relationship is just one small (albeit likely influential) element that influences the soil's ability to allow water redistribution (a transport phenomenon).

Methods:

- I don't see suction (h) as predicted with your PTFs in Table 3? Is this changing in response to SOC too?

Changes to the soil’s texture, structure, SOC, etc. are expected to influence the water retention and hydraulic conductivity characteristics. Table 3 shows the parameters of eq. 2, which are used to represent soil water content and conductivity as a function of the suction h. One can always invert the equation and have h as function of water content and therefore K as a function of h, as it is presented in Obiero et al. (2013). However, in predicting and expressing these relationships one of the axes has to be fixed, while the other is expressed as its function. Given that those relationships are represented in the model, and trusting that the model’s embedded processes are correct, any influence of SOC on the temporal patterns of h over time are modelled and can be evaluated if desirable. We have offered a brief discussion about such observations, lines 583-588.

“Alternative modelling approaches considering dynamic changes in soil hydraulic properties could also be applied in the future to investigate the influence of soil structural dynamics on the adaptation benefits of SOC accumulation (e.g. based on Meurer et al. (2020b), Meurer et al. (2020a)), as to our understanding, current models do not facilitate the representation of soil as a temporally variable medium.”

- Likewise, there can be important interactions between bulk density and SOC. However, I realize these are difficult to disentangle. Are you actually accounting for these feedbacks here? And if not, how would you expect these feedbacks to modify your results, if at all?

Although it is possible to use bulk density (BD) as input to the euptfv2, we decided not to include BD on our simulations to avoid the magnified effects of a second PTF. BD is known to be affected by SOC, but we do not have direct information on how BD would develop in our scenarios. According to the literature, there is a quasi-linear relationship between SOC and BD, where higher values of SOC lead to lower BD (Dexter et al., 2005; Keller & Håkansson, 2010; Kätterer et al. 2006). By not using a PTF that includes BD as a direct input, an effect of the BD-SOC relationship is inherently included in the developed PTF relationships, it is just not separated from the effect of texture or SOC. It would be possible to demonstrate this phenomenon on synthetic data, for instance. Therefore, we argue that by considering changes in SOC and leaving out BD as a factor that is directly accounted for, an effect of BD is implicitly accounted for in our work, even if in a somewhat uncertain and database-dependent way.

- I would provide a few sentences on the formulation and bit more motivation for using Tred(dry). I think this is a fine metric for analysis but not everyone may be familiar with it and there are many ways of evaluating the impacts of water limitation on crops/plants. Another way to look at this too would be to pull out of the 990 simulations those drought (low precipitation and/or high VPD) years and composite those responses compared to "average" years (excluding the most impacted years).

  Transpiration reduction due to dryness (Tred(dry)) is provided as a stress indicator directly by SWAP, and as such is documented. It quantifies the amount of transpiration that was prevented due to the fact that the crop water uptake was limited by lack of water. It can thus be seen as a simple and direct crop indicator for drought. We choose $\mathrm{Tred}_{\mathrm{dry}}$ because we wanted to look at direct plant responses to drought events as function of the soil water conditions. We added more explanations about how $\mathrm{Tred}_{\mathrm{dry}}$ is calculated at lines 157-171, as follows:

"Water stress was evaluated according to the reduction function by Feddes (1978), with the optimal root water uptake in the h ranges of -325.0 cm (h_{3H}) or -600 cm (h_{3L}) to -30 cm (h_2), oxygen stress linearly increasing for h higher than -15 cm (h_1) and drought stress linearly increasing for h smaller than -8000 cm (h_4). The crop growth module considers that the actual transpiration can be reduced by drought (too dry), $\alpha_d(z)$, lack of oxygen (too wet), $\alpha_o(z)$, or too saline conditions (physiological drought), $\alpha_s(z)$, which factors are known to reduce crop growth. The actual root water flux, $S_a(z)$ (d^{-1}), is then a function of all considered stresses:

$$S_a(z) = \alpha_d(z)\alpha_o(z)\alpha_s(z)S_p(z) \qquad [1]$$

where $S_p(z)$ is the potential root water extraction rate at a certain depth. The actual transpiration, T_a (cm d^{-1}), is calculated by integrating the root water flux over the root zone:

$$T_a = \int_{-D_{root}}^{0} S_a(z)\partial z \qquad [2]$$

where D_{root} is the root layer thickness (cm).

In our simulations, we did not consider stresses caused by saline conditions and focused on the drought-induced transpiration reduction (Tred_{dry}) as an indicator of drought stress during the cropping period.”

Results:

- So my most major comment is on Figure 5 – I think the presence of multiple lines of the same color, one showing the absolute interannual Tred and the other the time-varying offset, is a bit confusing. I understand that the same color links the SOC scenario, but it takes a couple of looks to get it all straight. The authors could leave this figure as is, but I think the figure would benefit from some slight re-working. E.g. maybe using a slightly different color for the offset lines (e.g. bright yellow vs duller yellow or dashed yellow) or maybe plotting separately just one number for the long-term mean Tred across the SOC scenarios (since there does not appear to be huge variability in this) and then leaving the interannual offset trend lines as they are. I also think the “offset” terminology doesn’t quite capture the value of the measure here – when I think offset I’m thinking displacement of some sort. Maybe just the word “change” or “delta” would suffice or maybe even the Tred “gain” or “benefit”

Indeed, the fact that the lines had the same color and shape doesn’t help in understanding this (already) quite complex figure. We changed the shape of $q_{0.05}$, $q_{0.50}$ and $q_{0.95}$ to dotted, in order to highlight the offset lines. We think that is important to keep all %SOC values, to highlight that there is no much extra gain in adding 4% SOC instead of 2%, in most cases.

We use the term offset in the sense of “compensation”, “counterbalance”, as it represents the gain on drought resistance between the current SOC and the addition of any other amounts. We changed the term for “average transpiration gain” (ATG) with SOC increase, as defined now in lines 319-321.

Discussion:

- Lines 408-421: Per my comments on the Intro, I appreciate your explanation for the reduced conductivity with SOC gains – very interesting. You provide a possible mechanism, but it’s not completely clear how this maps onto your results by way of what processes are included in your model. Would these interactions – the tortuosity of the conductive pathways – be what mediates this response in your model specifically?

Yes, the connectivity and tortuosity of the conductive pathways has a role in determining how easily the water can permeate within the soil profile. The most tortuous and not continuous the pores are, the less water will be able to permeate in the soil matrix and more energy will be necessary from the plant to extract it. This is translated into the hydraulic conductivity, which is one of the major mediators of the soil water dynamics in the model, playing a major role in the Richards equation (eq.1). What are input to the model are the saturated hydraulic conductivity (K_s) value and the unsaturated hydraulic conductivity curve that is scaled to K_s as its matching point, and described by a parameter describing its slope. Our argument about their impact is that in case the root zone is dried by climate- and root-water-uptake driven drying, and there is no precipitation in a given period, the soil with lower conductivity at the same level of suction will have reduced capability to replenish the root zone with water from below the root zone. This in turn can potentially lead to enhanced drought effects, which is what we believe we see in our simulations. Of course another effect of the addition of SOC in this soil is the increase in water retention, as showed in Figure 4, meaning that the root zone can have a greater capacity to store water, and not be in need to rely on water transport from below. In the simulations, these two changes take effect in combination, and the simulation results reflect the outcome of these opposing effects on plant water availability.

- Maybe a quick word on how your experiments be impacted with dynamic vegetation (prognostic LAI, since it appears your LAI was prescribed?)?

Our simulations considered a prescribed LAI, assuming that farmers will maintain the current growing period also under climate change, by choosing varieties with higher thermal requirements. At the discussion we pointed out this important point to be considered, lines 558-572:

“In this study, we focused on transpiration reduction, which is likely to imply biomass reduction, but may not necessarily imply yield reduction – depending on the timing of water stress. Other studies have investigated impacts of CC on yields for grain maize in Switzerland (Holzkämper, 2020; Holzkämper et al., 2015a) and it was found that yield trends differ depending on the choice of varieties assumed to be planted. In our study here, we focus on drought impacts on crop transpiration alone. Subsequent yield formation will be affected by crop transpiration, but also by various other drivers (e.g. temperature & radiation limitations, timing of stresses, heat stress). In order to obtain a clearer view on the impacts of SOC increases on crop transpiration, we elected not to consider the multitude of such interactive effects in the presented study. In future work, it will be interesting to explore possibilities to further increase the benefits of SOC additions by combining that strategy with other adaptations of crop and soil management (e.g. earlier maturing varieties, cover cropping, mulching of soil to reduce evaporation). In this context, it will be advisable to also account for a dynamic development of phenology and thus leaf area index to account for possible interactions between crop growth and soil moisture conditions.”

- Lines 466-469: Given your explanation here on the early season match between available water and available water capacity, I was wondering if and how increasing winter (or early spring) precipitation factor into the increasing offset of Tred shown in Figure 5 (and from Figure 6, it would seem this is driven by early season)?

  Yes, in line with the response given to the comment on L408-421, we argue that the extra available water that is stored during the wet period before the crop is cultivated is usable during the early season, and would therefore postpone and/or diminish the intensity of the stress for a couple of days in the early cropped period, as shown in Figure 6.

- Lines 486-490: May also summarized in Powlson (see refs below)

  The idea in this study was to keep the management alternatives more generic, without referring to particular management changes. We assumed that SOC increases can be achieved. A suite of different measures could help to get there, but we believe that is a subject this study should not address. And indeed, if no-till provides only benefits in the topsoil, then the hydrological effects are not likely to be significant. See revised lines 507-514, where we consider tillage effects in soil hydraulic properties:

“A meta-analysis on effects of tillage on SOC (Krauss et al., 2022) has shown that it is not uncommon that depletion in SOC of a subsoil layer co-occurs with increased SOC levels in the topsoil. We tested this with the particular soil and PTF used in our study, and found that the hydrological effects of reducing SOC at the depth of 25-32 cm were almost identical to the scenario in which the same amount of SOC increase in the depth of 0-25 cm was simulated but without subsoil SOC depletion (**Error! Reference source not found.**). We emphasize that, from the point of view of water availability to plants with deep roots, management strategies should aim at increasing SOC content deeper than only in the topsoil.”

References:

Dexter, A. R., Czyż, E. A., Birkás, M., Diaz-Pereira, E., Dumitru, E., Enache, R., Fleige, H., Horn, R., Rajkaj, K., Rosa, D. d. l., & Simota, C. (2005). SIDASS project: Part 3. The optimum and the range of water content for tillage – further developments. *Soil and Tillage Research*, *82*(1), 29-37. https://doi.org/https://doi.org/10.1016/j.still.2005.01.005

Kätterer, T., Andrén, O., & Jansson, P.-E. (2006). Pedotransfer functions for estimating plant available water and bulk density in Swedish agricultural soils. *Acta Agriculturae Scandinavica Section B-Soil and Plant Science*, *56*(4), 263-276. https://doi.org/https://doi.org/10.1080/09064710500310170

Keller, T., & Håkansson, I. (2010). Estimation of reference bulk density from soil particle size distribution and soil organic matter content. *Geoderma*, *154*(3), 398-406. https://doi.org/https://doi.org/10.1016/j.geoderma.2009.11.013

McDermid, S.S., E. Weng, M. Puma, B. Cook, T. Hengl, J. Sanderman, G.J.M. De Lannoy, and I. Aleinov, 2022: Soil carbon losses reduce soil moisture in global climate model simulations. *Earth Interact.*, 26, no. 1, 195-208, doi:10.1175/EI-D-22-0003.1.

J. Obiero, L. Gumbe, C. Omuto, M. Hassan and J. Agullo, "Development of Pedotransfer Functions for Saturated Hydraulic Conductivity," *Open Journal of Modern Hydrology*, Vol. 3 No. 3, 2013, pp. 154-164. doi: 10.4236/ojmh.2013.33019.

Powlson, D. et al (2014) Limited potential of no-till agriculture for climate change mitigation, Nature Climate Change volume 4, pages 678–683 https://www.nature.com/articles/nclimate2292

Comments from Anonymous Referee #2

This very interesting modelling study presented by Maria Eliza Turek and co-workers examines the potential benefits of SOC addition to Swiss maize cultivation on crop transpiration.

The manuscript itself it nicely written and well-structured – the ideas behind the study are reasonable and well explained and the proceeding during the modelling approach is described in a clear way. The outcomes of the study show that there is a (small) benefit of SOC addition on water retention, and that incorporation into depths below 65 cm do not lead to additional gains for the presented Swiss sites.

However, I am a bit doubtful about the scenarios considered in this study. I understand that the addition of SOC in the topsoil is the main focus and certainly what will happen under adapted management practices. The subsoil so far is still kind of a debate and a lot of research is happening at the moment to better evaluate the importance and future of subsoil SOC under future agricultural practices and climate change. Some studies, however, already point out that an SOC enrichment in the topsoil is likely to go along with a (slight) depletion of SOC in the subsoil (and directly below the enrichment layer, respectively). However, this certainly is dependent on the agricultural measure and most studies (and meta-analyses) of recent years have focussed on tillage (e.g. Krauss et al. 2022: *https://doi.org/10.1016/j.still.2021.105262*; Meurer et al. 2018: *https://doi.org/10.1016/j.earscirev.2017.12.015*). Nevertheless, a recent study from Germany (Skadell et al. 2023: *https://doi.org/10.1016/j.agee.2023.108619*) showed that, averaged over a variety of management practices, SOC stocks still increase in the upper and lower subsoil. Still, the variation across the soil profile was very different between different management practices. If I understand the modelling correctly, the management was kept the same during the simulations, while the SOC content in the soil increases. I actually think that the presented scenarios are kept too simple and too optimistic. The authors should consider to extend them towards a potential "subsoil depletion" scenario.

> We appreciate very much the careful revision of our manuscript and the insightful suggestions that helped us to improve our work.

Some minor comments:

Figure 1: I assume that it is minimum and maximum temperature that is shown in the top panel?

> Correct, we added this information at the caption (lines 191-192).

- 249 – 261: do I understand correctly that no calibration and "only" validation was performed?

  Yes, we used the model without a calibration of the soil hydraulic parameters, because we wanted to use directly the results from the PTF's and create reproducible scenarios.

- 272 – 277: I am not sure if "reduction factors" is a good expression here. My first thought was that the authors assume a reduction in SOC below the enrichment layer (which has also been shown in some studies, see studies above), but from what I understand from Table 4, so was simply the increase reduced. What about the increase of topsoil SOC at the expense of subsoil SOC (see my comment above)?

  We have considered this interesting point of reducing SOC at the subsoil as a consequence of adding SOC to the topsoil and performed a simulation where SOC was increased until 0-25 cm and a reduction of 0.1% was applied at the layer of 25-32 cm. We considered a scenario, where the soil had an 4% addition in the topsoil, while 0.1*4% were reduced in the layer below. The very small initial SOC (Table 2) did not allow much flexibility on the SOC reductions. For this particular soil and PTF, the results on transpiration did not differ notably from the scenario where only an addition of SOC was considered as shown in Figure 1 below. Therefore we elected not to present this as a separate scenario, but we added this point and reference to the targeted model run to the discussion, see lines 507-514:

"A meta-analysis on effects of tillage on SOC (Krauss et al., 2022) has shown that it is not uncommon that depletion in SOC of a subsoil layer co-occurs with increased SOC levels in the topsoil. We tested this with the particular soil and PTF used in our study, and found that the hydrological effects of reducing SOC at the depth of 25-32 cm were almost identical to the scenario in which the same amount of SOC increase in the depth of 0-25 cm was simulated but without subsoil SOC depletion (**Error! Reference source not found.**). We emphasize that, from the point of view of water availability to plants with deep roots, management strategies should aim at increasing SOC content deeper than only in the topsoil."

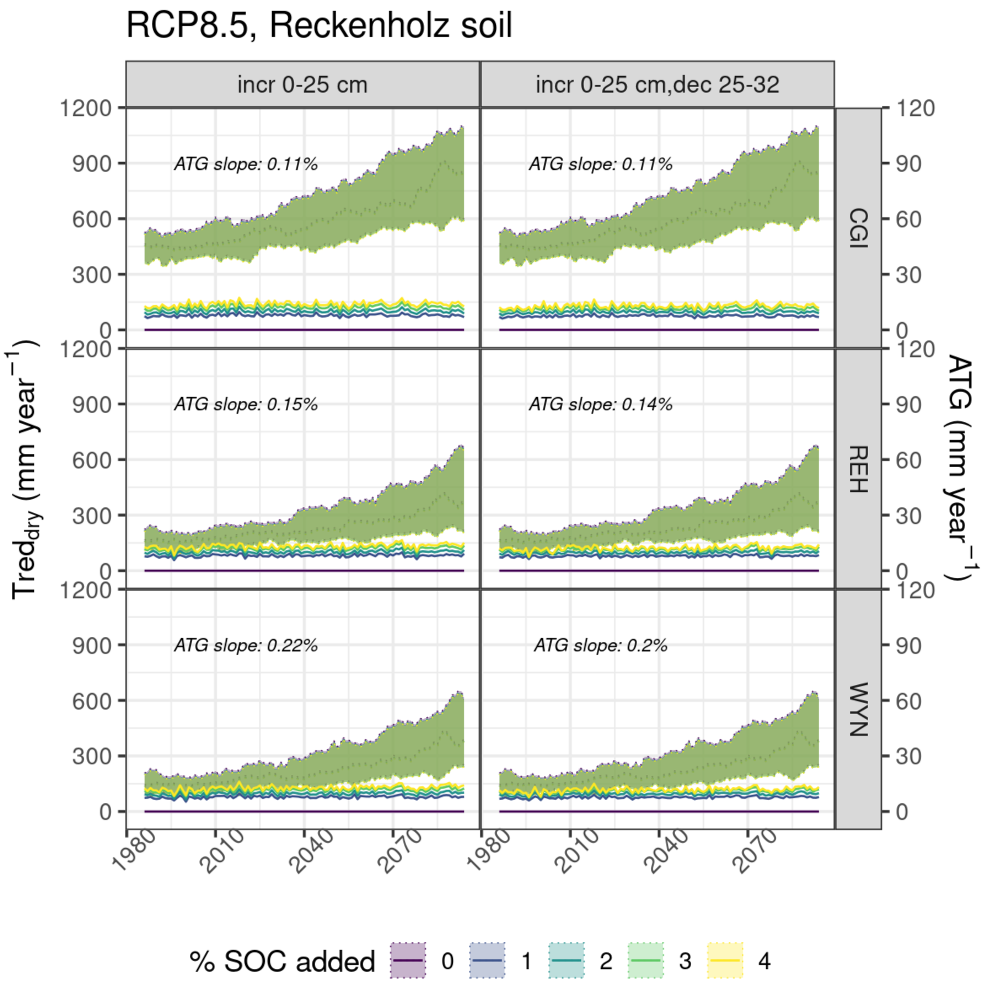

Figure 1: Transpiration reduction due to drought stress (Tred$_{dry}$) (left axis) for actual and future climate conditions considering different levels of SOC increase (left panel) and increase/decrease (right panel) in in the soil at different effective soil depths. Climate projections considering RCP8.5 and averaged for every 10 years. Shaded area refers to the values between (dotted) quantiles $q_{0.05}$ and $q_{0.95}$ of the climate projections. The slope refers to the average transpiration gain, ATG, (right axis; interpretable as average seasonal gain in transpiration with SOC increase) between 0 and 4% addition of SOC. *ATG slope* refers to the slope of the ATG line between 0 and 4% SOC addition.

- 337 ff: these results are interesting. However, does this mean that the authors assumed unlimited access to (ground-)water? What was the reduction if yield given the changes in precipitation (and certainly temperature) patterns?

  Groundwater availability was not considered in our simulations. The lower boundary condition at the soil profile was set as free drainage, which means a soil profile with deep groundwater levels. The bottom flux is driven only by gravity flow and the head pressure gradient equals zero. We added this information at lines 305-307:

"The bottom boundary condition was set as free drainage, representing a soil profile with deep groundwater levels."

About the yield, we added to the discussion at lines 558-567:

"In this study, we focused on transpiration reduction, which is likely to imply biomass reduction, but may not necessarily imply yield reduction – depending on the timing of water stress. Other studies have investigated impacts of CC on yields for grain maize in Switzerland (Holzkämper, 2020; Holzkämper et al., 2015a) and it was found that yield trends differ depending on the choice of varieties assumed to be planted. In our study here, we focus on drought impacts on crop transpiration alone. Subsequent yield formation will be affected by crop transpiration, but also by various other drivers (e.g. temperature & radiation limitations, timing of stresses, heat stress). In order to obtain a clearer view on the impacts of SOC increases on crop transpiration, we elected not to consider the multitude of such interactive effects in the presented study."

- 368 – 370: this is not clear to me. From what I understood did the authors rather assume a constant SOC level in the soil, but not a constant SOC addition – the latter would lead to a build-up over time. Or was the SOC level adapted annually within the model?

In this study, we consider generic scenarios of SOC increase without referring to particular management changes to achieve such increases. We assume that management has been successful to increase SOC from the beginning of the simulation period and SOC remains stable over the simulation period. The purpose of choosing these generic scenarios is to investigate which changes can be most beneficial for crop transpiration. We then discuss in section 6.2, which management options may be suitable to increase SOC. We clarified this in the M&M section, lines 282-284:

"We assumed that management improvements have led to increased SOC from the beginning of the simulation period and that SOC remained stable over the simulation period, thereby testing different scenarios of successful carbon sequestration."

- 475 – 477: yes, that´s an important point. How was this handled in the model? Did the roots reach a maximum depth/length?

  The model considers a maximum rooting depth that is allowed by the soil profile (135 cm) but the roots reach a maximum depth depending on the cultivar (100 cm).