Dear Dr. Vico (handling editor) and Dr. Rammig (co-editor-in-chef)

Thank you very much for your handling and support for the evaluation process. Also, thank you for email and request of major revision of the manuscript. To check the changes of MS, please kindly see the version with track changes and clean/final version. We responded to point to point comment and question from the handling editor. **The line numbers in the below responses are referred to the track changes version of the MS.**

On behalf of the authors

Thuy Nguyen

Major comments

I see you have addressed most of the other comments received. The reviewers who had originally assessed your work were unavailable for a second review and we could not secure an additional opinion, despite many trials. I am thus providing extensive comments on some of the remaining issues

Thank you very much for your great effort to handle the submission as well as your evaluable and constructive comments to improve the MS.

How was the irrigation amount and timing chosen? I understand this was discussed in a previous publication, but it is such an important point for these results that it needs to be explained around L162.

We added here the text to explain the irrigation as line **157 to 164**. Now as: "The irrigation systems [T-Tape 520-20-500 drip lines (Wurzelwasser GbR, Müzenberg, Germany)] were installed parallel to the crop rows with 0.3 m intervals. A nearby weather station (approx. 100 m from the experiment) recorded every 10 minutes weather variables (global radiation, temperature, relative humidity, precipitation, and wind speed). In addition, the precipitation amount was manually collected by a plastic rain gauge next to each rhizotrone facility. The Penman-Monteith equation was employed to estimate reference evapotranspiration. Daily crop evapotranspiration was calculated based on the single crop coefficient and the reference evapotranspiration (Allen et al., 1998). Irrigation amounts were estimated as the weekly sum of the calculated crop evapotranspiration."

Reference:

Allen, R.G., Pereira, L.S., Raes, D., Smith, M., 1998. FAO Irrigation and Drainage Paper – Crop Evapotranspiration. FAO, Italy.

More importantly, I see missing a proper discussion of the meaning of the two water treatments for the plant water status. Reading the discussion, it seems the assumption is made that rainfed=water stress and irrigated=well watered, but this seems unjustified (would that mean that in any year all rainfed crops in the region are water stressed? Are conditions such that irrigation is widely used?). I thus see the need to start the discussion with one on indicators of plant water status in the experiment. Of particular relevance are the consequences on plant water status of using the same irrigation strategy in 2017 and 2018. Moreover, the fact that the irrigated treatment was not replicated should appear as a caveat also in the discussion.

Thank you for this comment. We added one paragraph to discuss about this from **line 641-666**. As now:

“This study investigated soil-water-plant relations, more specifically the interactions of the root and shoot growth processes and water fluxes under variations of soil water status and atmospheric demands. To the best of our knowledge, the comprehensive data collected from soil to root, plant, and atmosphere under field conditions in this work was unique. However, we acknowledged the lack of treatment replicates which was due to the complex and expensive construction of the rhizotrone facilities. We also acknowledged the small size of plots that did not allow the extensive destructive sampling (i.e. leaf area, biomass, or determination of leaf water potential etc.). Each rhizotrone site originally contained the irrigated, rainfed, and rain-out sheltered plots (Nguyen et al., 2022a; Cai et al., 2016). The overall aim of the experiments was to investigate the root and shoot responses and gas fluxes (CO_2 and H_2O) of wheat and maize to the variations of soil water and soil hydraulics. Note that the studies did not intend to investigate the impacts of similar irrigation strategies on plant water status among seasons (i.e. in 2017 and 2018) because the irrigation practices were less common in the regions. The collapse of manual rain-out shelters due to strong wind after the 2016 growing season resulted in only two water treatments (rainfed and irrigated). Based on experiences from the previous seasons (wheat), we argued that such combinations of two water treatments and two soil types, to some extent, could still create a wide range of soil water conditions for the maize trial. For instance, the “rainfed” treatment at the stony soil in the upper rhizotrone (F1P2) could lead to severe water stress compared to other treatments, especially in the summertime when the atmospheric evaporative demand is high. In fact, mild water stress was observed at the F1P2 around mid -June in 2017. In 2018, the sites were slightly modified to induce more severe water stress (Nguyen et al., 2022a). One rainfed plot with the stony soil had late sowing while one rainfed plot with the silty soil had the higher sowing density (data not shown in the study). Unprecedented weather (extremely hot and dry) in 2018 resulted in severe drought stress at the rainfed plots with the stony soil. To compare the effects of soil types and water treatments on crop, we presented here only data from two plots (rainfed and irrigated) for two soil types. In spite of the experimental limitations, the relative differences among the treatments, soil types, and seasons as well as measured dates were clearly illustrated which ultimately supported the overall aim of our study.”

I also suggest that the discussion is extended to present some implications of this work. They are not just mechanistic model parameterization! Note that adding these important points of discussion is not in contrast to some of the comments received before. These are important and very related points, whereas the reviewers had suggested to remove somewhat tangential (albeit interesting) material.

Thanks for this comments. We extended the discussion via adding one paragraph to elaborate some implications of our work. As now in line **667-686**:

“The simultaneous measurements of atmospheric conditions, leaf water potential, and transpiration rates, coupled with measurements of root, stem and whole soil-plant hydraulic conductance, root architecture (root length), and soil water potential distribution illustrated the complex responses of the shoot and root growth and hydraulic conductance vulnerability to soil water availability. The different responses of crop processes to soil hydraulics and climatic conditions suggest further field investigations for other soil types, growing seasons, and water regimes. Future studies considering the effects of progressive soil drying or irrigation strategies on plant water status and crop growth at field conditions will be necessary. This is very relevant for those crop-growing regions that require irrigation. Our results show that the leaf water potential threshold can vary within the same genotype depending on soil types, climatic conditions, and water management. Large variability of minimum leaf water potential has been reported for maize genotypes under greenhouse conditions (Welcker et al., 2011). Field studies are required concerning the stomatal functions, water relations, hydraulic vulnerability traits, and root: shoot responses, especially of different maize cultivars in responding to drought stress. This will suggest implications for selecting agronomic cultivars and traits under changing climates. Results from this study show that soil-crop models should focus not only on simulating stomatal regulations to capture the response to drought stress, but also require adequate representations of root and leaf growth and adjustments. The soil hydraulics strongly influenced soil water availability and crop growths. Regional applications of soil-crop models for simulating gas fluxes and crop growth processes and for estimating irrigation amounts must account for the environmental heterogeneity within the spatial simulation unit whereas the soil heterogeneity is the key variable."

The conclusion section contains still some discussion points. For the best readability, the conclusions should be streamlined to be just that – conclusions – and any discussion point should be moved to the appropriate place in the discussion section.

Many thanks for your suggestion. The conclusion was streamlined. The text of discussion (line **694-698**) was put in the discussion section 4.3 (now as to line **607 and 620**). The text from **708 to 727** was moved to the discussion, section 4.1 (now as in line **464-484**). The line **732 to 734** related to implication to soil-crop model was moved to the new discussion (about the implication of the study) (see above response, line **667-686**).

Minor comments:

L63 an not as

This was corrected.

L94: something is amiss in this sentence

The sentence was corrected.

L199 and elsewhere: emerge is a verb; you can use emergence or, alternatively, emergence

This word has been corrected.

L308: what does "almost similar" mean? Be specific, possibly using statistical indicators (confidence intervals and the like)

We revised the text for this paragraph with more specific number and variables, as now in line **323-327**:

"Predawn and midday leaf water potential were around -0.4 MPa and -1.6 MPa for all plots, respectively. Leaf transpiration rate was around 3.1 millimole m^{-2} s^{-1} for all water treatments and soil types at 12 AM. This indicated the recovery of plant after watering at the rainfed plot with stony soil (F1P2).

L413: remove considerably

This was removed as suggested.

L470: climatic, non climate

This was corrected.

Note also some additional editorial comments provided as private note.

Additional private note (visible to authors and reviewers only):

Please note that there is still some substantial (self) plagiarism, which should be avoided. Please revise the manuscript to avoid that, or else we will be forced to reject that.

Thank you again for this suggestion. We substantially revised text to avoid the self-plagiarism, especially in the materials and methods and other small parts of the main text. More specifically in those lines:

Line 63

157-174 for water application and irrigation

Line 188-190: soil water measurement

Line 216-220 for leaf gas measurement

Line 222-230: sap flow measurements

Line 577

On a side note, will you make your data publicly available? Or will the data be available upon request? If you plan any of these, do specify where the data will be or that the data can be requested. Your decision on this matter will not influence the editorial decision.

We added here to data sources and citations for the below and aboveground data, respectively that could be publicly accessed. Two relevant publications (below and aboveground data) were added in the reference lists.

Lärm, L., F.M. Bauer, N. Hermes, J. van der Kruk, H. Vereecken, J. Vanderborght, Nguyen TH et al. 2023. Multi-year belowground data of minirhizotron facilities in Selhausen. Scientific Data 10(1): 1–1

Nguyen, TH, G. Lopez, S.J. Seidel, L. Lärm, F.M. Bauer, et al. 2024b. Multi-year aboveground data of minirhizotron facilities in Selhausen. Scientific Data 11(1): 1–11.

Polina Shvedko
Notification to the authors:
It seems that a table is included as Supplementary material 10. If it is so, it should be re-labelled as Table S1 and the references in the manuscript text should be adjusted accordingly. In this case figures of the supplement must be numbered consecutively: Figure S1, Figure S2, etc.

Many thanks for this guidance. The Figures and Table in the Supplementary material were updated as suggested. The names of those Figures and Table in the main text were also changed.