Response to Anonymous Reviewer #1 and Anonymous Reviewer #2

The text in italics refers to the literal reviewers' comments. The text in blue is the literal text edit from the revised manuscript followed by the updated line numbers.

Reviewer 1: *The manuscript by Hoffmeister et al. offers interesting new insights into the water dynamics of an agroforestry system. The study is methodologically very well implemented. The manuscript is complete, well structured and easy to read.*
I have few general comments:

- *The outcome of the study (the most important results) could be better elaborated. This could be implemented in the discussion (content headings for subchapters, summarizing opening or closing sentences) as well as it should be more strongly reflected in the abstract and conclusions.*
- *In my opinion, the abstract and conclusions would benefit from a stronger revision (see specific comments).*
- *The methods and results sections sometimes seem somewhat elaborate. The authors could shorten them by omitting some of the less relevant content, but above all by using shorter wording and sentence structure.*

The specific suggestions for improvement can be found directly in the pdf document.
I wish the authors much success and enjoyment in fine-tuning their manuscript!
Reply: Thank you very much for the positive and encouraging general feedback towards the manuscript. We enjoyed reading and discussing your constructive suggestions and interesting questions. Small text changes such as commas are not addressed specifically in the following but will are considered in the final version of the manuscript. We believe that the answers to the above-mentioned general comments are included in the following pages, where we address all specific comments.

L. 15: *The abstract needs more work. The starting motivation sentence is to weak and a conclusional last sentence is missing.*
Additionally, methods are completely missing. What was measured in situ, how was it evaluated? Which modeling tools were used?
Most of the presented results are very descriptive. Either they are not important enough to mention them in the abstract, or they lack interpretation. You could rethink: what are the really important findings from your study (and why)? Formulation tasks/aims with strong verbs (instead of explore, analyse) could additionally help to that (making more clear what the study really is about).
Reply: We agree that the abstract needed to provide more information about the methods and the synoptic analysis and revised it accordingly, especially regarding the follow-up comments.

L. 15: *"may" sounds somewhat weak and vague combined with "under forcasted scenarios". Maybe you can be more precise here, or is it at least "likely"?*
Reply: Thank you. We agree that this sounded very vague and we reformulated the sentence: "The Western Cape in South Africa is a water scarce region which will likely receive less rainfall and higher air temperatures under projected climate change scenarios." (L. 14)

L. 20: *Describing you study, use either tasks or aims listing the elements. Mixing it up is irritating.*
Reply: Thank you! We considered this and reformulated our objectives to avoid mixing elements and to sharpen the focus: "Our objective is to explore whether the AFS positively impacts the water balance

by combining measurement campaigns to characterise the spatial variability of various key system properties with continuous monitoring." (L. 19)

L. 23: *Here, I find it hard to imagine what exactly you analysed (or how). The step from rwu/event characteristics to infiltration felt quite large to me reading the sentence. Maybe you can link the elements better (prepare the reader better that this will be about infiltration) giving more details about the methods or rephrasing the sentence (changing the order, starting with infiltration).*

Reply: We modified the paragraph in the abstract accordingly: "The campaigns encompassed extensive soil sampling to determine soil characteristics (nutrient concentrations, hydraulic conductivity, texture, water retention) in the laboratory as well as terrestrial laser scans of the field site, especially of the windbreaks. The continuous measurements covered meteorological, soil water content and soil water potential observations over a six-month period (in summer). These were applied to understand soil water dynamics during rainstorms and dry spells, including root water uptake as well as soil water storage." (L. 22)

L. 31: *In the last sentence, you repeat what you already said earlier, and you refer to hypothesis we not yet know about. It would be nice to have a conclusion sentence referring to the content of you study (and not if the study worked in general).*

Reply: We agree that the conclusions were formulated somewhat weak and added the following sentences to the abstract: "We could demonstrate positive effects of the windbreak on the water balance and dynamics in the blackberry field site, even though questions remain as to the extent of these benefits and how they compared to disadvantageous aspects brought about by the presence of the trees (e.g. increased water usage). Irrigation did, in fact, shift the AFS from a water-limited to an energy-limited system." (L. 35)

L. 34: *Flexibility and adaptation measures of what?*
The sentence is quite long and complicated as a first sentence. I don't think "challenging" is necessary, the change is the challenge, the word elongates the sentence but does not add information. Maybe the rest of the sentence can be streamlined a bit, too.

Reply: Thank you, we rephrased and streamlined the sentence: "In a changing world, agricultural flexibility and adaptation measures are required to uphold and enhance global living standards, while protecting and restoring ecosystems, as well as to ensure agricultural productivity amid more frequent water shortages, particularly in the global south (Douville et al., 2021)." (L. 40)

L. 38: *This sentence is a bit irritating to me, as the listed elements overlap (enhance resilience and productivity is a benefit) or are unclear (which perspectives? which multiple benefits?). I think it could be rephrased to become more logical.*
(Adding a colon after the sentence, or something around "as will be described in the following", could also help.)

Reply: We rephrased the sentence to make it more logical and to improve the readability: "AFS describe the combination of woody perennial species with crops and/or livestock components. It has the potential to deliver multiple benefits and offer new perspectives for existing agricultural systems including their greater resilience and productivity (Sheppard et al., 2020a)" (L. 43)

L. 97: *I'm still missing the methods.*

Reply: We added a sentence to include the synoptical analyses to the mentioned methods and hope that this addresses the reviewer's point: "By merging the different methods we could infer process such as infiltration through the combination of nutrient analyses with soil water dynamics during rain events, or by reflecting on the energy budget through shade-cast simulations and evapotranspiration estimates." (L. 114)

L. 156: *the? each?*
Reply: The three samples come from one profile at three different depths. We modified the sentence to make this clearer: "Additionally, we took three undisturbed soil samples in 250 ml cylinders from a selected soil profile pit, one at the surface and one each at depths of 0.3 and 0.5 m during the field campaign in September 2019 to determine soil hydraulic properties and some additional variables." (L. 176)

L. 158: *The order does not make sense here (taking the undisturbed samples, measuring textur, Ksat measurement)*
Reply: Indeed, that is correct. Thank you for noticing this. We listed the measurements in no specific order. We agree, it makes sense to list them in the order as carried out and adapted it in the revised manuscript: "Soil hydraulic conductivity of the undisturbed samples was measured with the Ksat apparatus (UMS GmbH, Munich). Soil water retention characteristics on drying samples were measured on the same samples in the HYPROP device (UMS GmbH, München, Germany). A small fraction of the sample (about 10 g) was then transferred to the WP4C potentiometer (Decagon Devices Inc., Pullman, WA, USA) and subsequent weighing, further drying and measuring contributed further reference points to the water retention curve. Soil texture was determined through wet sieving of ground soil and smaller fractions were again separated with the sedimentation method after Köhn (ISO 11277:2002)." (L. 178)

L. 160: *I think there is no need to go into detail of how Ksat works. I consider it a standard method, everybody who does not know and is interested can look it up. This applies also to Hyprop and WP4C including the drying and weighing. The methods description should be as concise as possible.*
Reply: Thank you for the reminder of the audience addressed in HESS. We removed detailed descriptions of the methods mentioned.

L. 180: *Maybe this makes better sense further down?*
Reply: Yes, we agree. Thank you for picking it up. We moved it into the respective data analysis section (2.3.3).

L. 223: *As above, I think this detail can be spared.*
Reply: Thank you. We shortened the whole paragraph about root water uptake: "Daily root water uptake (RWU) is derived after Jackisch et al. (2020) including a nocturnal correction from stepwise diurnal changes in soil water content between two consecutive days assuming that RWU is the decrease in soil water content between two subsequent nights. If the hourly soil water content time series of a sensor did not show a stepwise decrease, RWU could not be calculated for that sensor on that day. The water limitation factor fw (e.g. Ghausi et al., 2023) was calculated as the ratio between actual and potential transpiration with the assumption that AET is represented by RWU." (L. 233)

L. 293: *On average, events had a low intensity of ... and a duration of ...*
Reply: We adapted the sentence accordingly: "On average, the events had a low intensity of 1.6 mm h^{-1} and a duration of 11 h 41 min." (L. 298)

L. 311: *I can't make sense of the three given here and the locations in the table.*
Reply: Thank you for pointing this out. After considering both reviews, we decide to move the table entirely to the appendix (table A2). We modified figure 3 to include the most important parameters in the main manuscript. For the appendix, we adapted the caption of the table to clarify the

abbreviations: "Abbreviations are: WB P = profile, WB = windbreak, BB = blackberries, E = east, M = middle, W = west, FC = field capacity, PWP = permanent wilting point, PAW = Plant-available water."

L. 359: *I feel this paragraph can be shortened by rephrasing*

Reply: Thank you. We acknowledged the comment and rephrased the paragraph to make it more concise: "The daily root water uptake (fig. 5) calculation was not successful on many days leading to missing values for 48 % of the observations (one value per sensor per day) at the windbreak and 56 % at the blackberry location. Missing days were spread over the entire measurement period, with only four days of RWU estimates available from all eight sensors. At the windbreak, gaps occurred most frequently in the topsoil (20-40 cm) whereas at the blackberry location it occurred more often for the sensors located at deeper depths (40-60 cm, 60-80 cm). On days without missing values (21 days at the windbreak, 14 days at the blackberry location), 44 % of the estimated RWU primarily occurred within 20-40 cm, followed by 28 % in the top 0-20 cm at the windbreak. In the blackberries 70 % was abstracted from the top 0-20 cm. Note that, on the four days with complete sensor data, the RWU was consistently greater at the blackberry compared to the windbreak location (12 Oct.: 0.56 mm d^{-1} < 0.82 mm d^{-1}, 13 Oct.: 0.72 mm d^{-1} < 0.85 mm d^{-1}, 18 Oct. 0.66 mm d^{-1} < 1.22 mm d^{-1}, 21 Nov.: 0.4 mm d^{-1} < 1.17 mm d^{-1})." (L. 359)

L. 383: *?*

Reply: It is a common procedure to define the end of a rainfall event if it has not rained for six hours straight. We updated the text to improve the wording (strongest): "The most precipitation during one event accumulated a total of 118 mm of rainfall within a 21 hour period and occurred on 25 October 2019.". (L. 382)

L. 438: *You could give the precipitation/time (e.g. monthly average) of you study period in comparison to the long-term yearly precipitation/time, such that one has and understanding what your study is representative for. It is interesting to know how much winter and summer differ on the study site, and how typical your study period summer was.*

Reply: That was a great idea, thank you. For the duration of our measurement period (mid-September to mid-March), we summed the monthly averages of a 30-year time series and added this rainfall information to the discussion: mid-September to mid-March: 206.5 mm (summer), rest of the year: 580.5 mm (winter), total: 787 mm. In the summer season, approximately half of the precipitation of the winter season occured: "Total precipitation was 245 mm (30-year average for the same period is 206.5 mm) and partially covered the annual average of 787 mm for the region (Meadows, 2015; Veste et al., 2020; Climate-Data.org, 2024)." (L. 424)

L. 440: *Maybe name the monthly sum, for better comparison?*

Reply: That was a good point, especially after considering the previous comment. We added the monthly sum to the text.

L. 455: *This transition to soil water retention is a bit surprising.*

Reply: This might be a misunderstanding due to a poorly chosen word on our part. We were not referring to soil water retention in this paragraph but to soil water storage following rain events, i. e. how much is the vol. water content increasing after a rain event and thus indicating how much water is stored in the soil. We changed the wording to avoid this misunderstanding: "Overall, we observed a higher proportion of rainwater stored in the soil at the blackberry location in contrast to the windbreak location, where on average 63 % and 54 % of the rainfall reached the soil column, respectively." (L. 451)

L. 461: *is it "such as" (an example) or "in other words"? Also, it would be helpful to keep the same order when opening the paralell (surface, infiltration, subsurface). But I actually think you just need one half of the sentence, it is redundant.*
Reply: Thank you for pointing this out. We removed half of the sentence to avoid redundancy.

L. 463: *What about surface/crust effects? Maybe you can give an estimation how representative topsoil values are for the soil surface from your visual experience.*
Reply: Thanks for mentioning this interesting point. It is a valid question as we have regular irrigation and high clay and silt contents in the topsoil under semi-arid conditions. From our visual experience of the observation plot, we did not notice any physical soil crusts and no apparent biocrusts. The reason for this might be the frequent walking on the work paths preventing physical incrustation, while a high cover of accompanying plants leaves little room for biocrusts. However, we did not expect spatial homogeneity in the topsoil Ksat values. There is a substantial difference between the berry rows themselves (rather porous, lightly packed soil, flat) and the space between the rows (compacted, rock fragments, steeper parts), that were frequently used for working on the plants. In the six topsoil samples from March 2022, Ksat was on average 302.3 mm h^{-1} (± 191.3 mm hr^{-1}), thus showing that the sample from 2019 is in a similar order of magnitude. However, the data from the samples demonstrated inhomogeneity between the windbreak and berry sample location, where Ksat was on average 163.2 mm h^{-1} and 441.4 mm h^{-1}, respectively.
Furthermore, the maximum precipitation intensity was quite an outstanding value. The average precipitation rate (> 0) of the whole measurement period was 7.1 mm h^{-1}, confirming what was already observed in the manuscript with Ksat exceeding max. precipitation intensity. We added this information to the main text of the manuscript: "Observed Ksat values (302.3 mm h^{-1} ± 191.3 mm h^{-1}) varied in the range of silty soils. Great heterogeneity of topsoil Ksat is expected due to the difference between the soil in the berry rows (lightly packed soil, flattened) and in-between the rows (compacted, rock fragments, steeper parts) and was confirmed by a nearly threefold average at the blackberry location compared to the windbreak. Ksat values exceeded maximum precipitation intensities (max. 82.6 mm h^{-1}) at both locations, providing favorable conditions for water infiltration into the soil." (L. 462)

L. 477: *Can you remind your readers about your texture, if it is easily transported by water, and how much of a textural change you would expect - such, is it still possible that the hypothesis is true, because differences could be minimal, or do you think it becomes unlikely because of the missing textural differences? As transport by surface runoff would take place mainly during extreme events, the kinetic energy is high.*
Reply: The texture of the topsoil was classified as silty clay loam with clay fractions up to 40 %. We agree that surface runoff would mainly occur during extreme events with high kinetic energy.
As mentioned before, we do have events occurring with high precipitation intensities (max. observed 82.6 mm h^{-1}), which definitely have the potential to produce a splash erosion even in cohesive soils which become then erodible. We addressed this suggestion in the revised manuscript by including the content of this comment and reply: "We observed very high precipitation intensities (max. observed 82.6 mm h-1), which probably produced surface runoff with high kinetic energy, and therefore, had the potential to produce splash or sheet erosion even in cohesive soils. Possibly, the windbreak may not be apparent in the soil water content changes but downslope erosion of fine soil could explain the unexpected observed lower Ksat values near the windbreak, which is underpinned by larger bulk density and lower porosity at the windbreak. We did not find considerable texture differences between the two locations, but fine particles could be masked through the formation of aggregates (Jackisch et al., 2017). Carbon addition may also increase and stabilise aggregates in fine-grained soils." (L. 490)

L. 483: *or heterogeneous preferential flow out of the system*

Reply: We consider redistribution to include preferential flow and, naturally, this may not only occur in the lateral but also in the vertical direction. We added this detail to the text: "In general, for most events, the cumulative soil water storage at both locations did not align with the recorded precipitation amount, supporting the occurrence of lateral redistribution at the soil surface or subsurface. In the case of lateral subsurface flow, i.e. soil water redistribution, water moved horizontally instead of percolating downwards when reaching a less permeable soil layer. This was evidenced by a substantially decreasing Ksat with depth (at 0.5 m Ksat = 3.2 mm h^{-1}) and might benefit the windbreak. The often-observed delayed responses of soil water content changes after the onset of a precipitation event can be an indicator for both infiltration after surface runoff and lateral redistribution. Additionally, the simultaneous reaction of the deeper sensors with the shallower ones is evidence for preferential flow through e.g. macropore input (fifth event in table 1, bottom middle panel in fig 2)." (L. 479)

L. 488: *thus, not allowing for an absolute, but a relative (between locations) interpretation of the results?*

Reply: Yes, this is correct. The absolute values might be biased by the irrigation. As the irrigation influence should be the same at both locations, relative comparison between the two locations is still possible. We added half a sentence to clarify this: "The influence of the irrigation on these estimations is unclear, although it should be consistent at both locations, thus, allowing relative interlocal comparison. Nevertheless, we are cautious about the achieved RWU estimates at this site due to missing data." (L. 500)

L. 510: *I would be careful here, to my knowledge, water can be limiting before, I mean this is the permanent wiliting point - the point of no return (for sunflowers). I have seen matric potentials much before PWP beeing considered as the point where transpiration is reduced in literature.*

Reply: Thank you for referring to the critical context of the permanent wilting point (PWP). Yes, it is true that the permanent wilting point is only based on sunflowers and that water limitation of other plants might be reached much earlier. The PWP mainly describes the soil water status at which water flux are depleted and the water's mobility is strongly limited, but as mentioned water fluxes already slow down earlier than that. We reformulated the text so that it is rather in the frame of "before reaching complete water limitation" at the permanent wilting point: "This is confirmed by the matric potential sensors, which show the plant does not reach the PWP (fig. 2), i.e. the point at which water fluxes are nearly immobile. Water becomes a limiting resource for many plants already at lower absolute matric potential values." (L. 521)

L. 532: *It sounds like a framing to me - not considering overall effects, but only benefits. An alternative would be to give a heading that already contains an outcome (Windbreaks induce benefits...). You could do this througout the discussion (giving the main outcome of the subsection in the heading), I always find it very helpful as a reader.*

Reply: Thank you. We liked this suggestion and renamed the subsections of the discussion. This should also address your general comment referring to the stronger highlighting of the main results of our study.

L. 536: *These are too many informations within one sentence (ideally, 1-2). Divide into two sentences?*

Reply: Thank you for this useful comment. We removed some information and shortened the sentence to facilitate the information flow. "Carbon addition may increase and stabilize aggregates in fine-grained soils." (L. 496). The remaining information was moved to the discussion (L. 556).

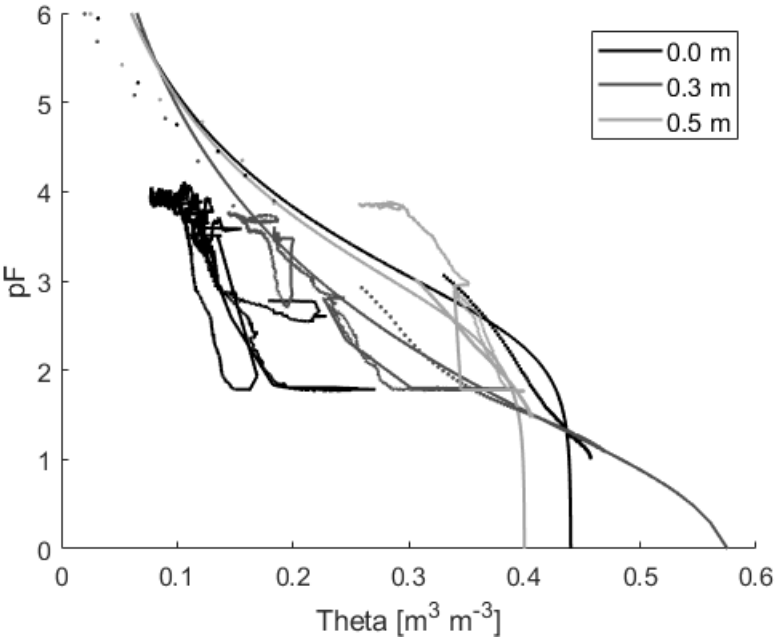
L. 545: *are the findings only at this location, or did you measure only here? it should be clear from the sentence.*

Reply: Thank you for pointing this out. We modified the sentence to clarify the measurement location: "Both the volumetric water content (at both locations) and matric potential (measurements at the windbreak only) observations consistently show that the topsoil is drier than the soil at greater depths (fig. 2)." (L. 564)

L. 562: *...but matric potential sensors are more delayed. how big is the gap between the two sensors/the difference between lab and field retention curves?*

Reply: This is also an interesting point. We did not observe any substantial delay in the response of the sensors to the rain events (manuscript, fig. 2). Additionally, our water content sensors cover a rather big measurement volume, which might take some time to react but therefore also more representative. We also question the assumption of a general delay of matric potential sensors, as potential changes travel as waves at higher speeds (celerity) than soil water fluxes, which are controlled by filter velocities.

In the following figure, you can see the big difference between field (dots) and lab (lines) retention curves. Reasons for this are manifold; on the one hand, we can observe hysteresis effects. On the other hand, we need an equilibrium between the two measures, which does not happen so quickly during/after a rain event also the sensors might not be close enough to each other to be able to produce reliable retention curves.



L. 568: *choose which one is important here (September - March vs yearly) and use it for precipitation only as well as for the combination of the two*

Reply: Yes, we agree, one information is enough. We modified the text accordingly. Thank you!

L. 569: *where is enery lost?*

Reply: Thank you for this question. We realized that our formulation was probably misleading. We were not referring to potential energy but to incoming solar radiation energy. The paragraph about water consumption estimations is removed in the revised manuscript.

L. 579: *repetition of "potential"*
Reply: Thank you for pointing this out. We rephrased the sentence to avoid the repetition: "An additional potential not discussed in much detail in this study is the carbon sequestration of the windbreaks in the landscape." (L. 590)

L. 580: *use unit symbols. and: per metre of what?*
Reply: Thank you. The unit must have slipped our attention. The number is referring to meter of windbreak. We add this detail to the text.

L. 591: *windbreak themself or windbreaks themselves*
Reply: Thank you. We corrected this mistake in the text.

L. 592: *in what way? we want qualitative findings here, if there is an impact, in which direction is it?*
Reply: (see below, answered together with the next comment)

L. 593: *again*
Reply: Thank you. Yes, we understand and agree that this was missing in the. We rephrased the conclusions and included some qualitative statements into the revised manuscript.

L. 596: *I think the sentences I crossed out are unnecessary and they seem to not fit to or represent your work. If you want to make a kind of political statement, hide it more in the following sentences or make it shorter and put it at the end - would be my suggestion.*
Reply: Thanks for pointing this out. We removed the sentence from the conclusions.

Reviewer 2: *Hoffmeister et al. have provided a manuscript on the Hydrological and Pedological effects of combining Italian Alder and Blackberries in an Agroforestry Windbreak System in South Africa. A relevant and interesting topic.*

This describes a challenging topic, and I must compliment the authors on the wide range of techniques used and the quantity of work that has gone into all the field, laboratory and modelling work. Such studies are very challenging, yet you have used some contemporary and modern techniques. Having said that, I found it possibly a bit too diverse with some gaps that left me wondering what the main take-away results of the manuscript are and suggest further thought on what the useful impacts of this study are.

I'm concerned about RWU study which leads to your AET. You seem to assume if there is a decrease in soil water, that the decrease is RWU, but you don't know what the contribution of irrigation might be? I'm also concerned about the interception storage of 0.664 mm of the Alder trees which may significantly influence effective rainfall. With an LAI of almost 6, I would expect an interception of about 3 to 4 mm. The PET results in some places are in the region of 23 mm, which is very unlikely, if not impossible. The evaporative fraction results also look very low and you claim this means it is a water limited environment and then later that it is energy limited. There was no calibration of the volumetric soil water probes which may need more than just an offset, meaning that the daily changes in VWC may also not be accurate. I understand the challenges of equipment costs but two pits is quite lean for a conclusive result on differences between the two areas in terms of soil water.

Would it be worth including a graph of solar irradiance and PET? Do you have measurements of LAI over time?

I have made 133 comments in the attached. Some of them are small editorial or grammar suggestions while others will be more challenging and I have included a number of additional concerns as comments. Apologies for any misinterpretation of the results. I hope my comments make sense and are able to contribute to an improved manuscript in the future.

Reply: Thank you very much for this detailed and constructive feedback. We appreciate the time and effort spent for improving the manuscript. Minor suggestions such as rewording, editorial suggestions, commas are not addressed specifically in the following.
We will address all raised concerns in the previous paragraph in more detail in the following specific replies. Thank you very much specifically for pointing out the challenges regarding our interception estimation, which we appreciated very much, and for pointing out that our PET estimation was erroneous. We corrected that in the revised manuscript. Measurements of LAI are unfortunately not available.

L. 15: *It would be useful to include the measurement period and number of seasons measured in the Abstract.*
Reply: Thank you for pointing this out. That was an important detail missing and we added it to the revised manuscript's abstract: "The continuous measurements covered meteorological, soil water content and soil water potential observations over a six-month period (in summer)." (L. 24)
L. 24: *This is the only place in the document where the word macro-pore appears. Theories mentioned in the abstract should appear in the body of the document.*
Reply: Thank you for pointing this out. We discovered inconsistencies in the text regarding the spelling and wording and used "macropore", "macro-pore" and "preferential flow" more or less synonymously. We harmonized this in the manuscript.

L. 85: *define dryland please. Do you mean 'without irrigation'? Or do you mean dry areas.*
Reply: We are not discussing irrigation but areas experiencing water scarcity because precipitation is balanced or even surpassed by evapotranspiration from surfaces and vegetation. Drylands, as defined by the United Nations Environment Program (2005), with an aridity index of less than 0.65, include the Western Cape according to the World Atlas of Desertification (2018). We believe this commonly used terminology and therefore refrained from adding a definition to the revised manuscript.

- Millennium Ecosystem Assessment, 2005. Drylands Systems". Chapter 22 in: Ecosystems and Human Wellbeing: Current State and Trends, Volume 1. Island Press
- Cherlet, M., Hutchinson, C., Reynolds, J., Hill, J., Sommer, S., von Maltitz, G. (Eds.), World Atlas of Desertification, Publication Office of the European Union, Luxembourg, 2018

L. 99: *I think the negative effects of windbreaks needs to be included in the Introduction which has focussed only on the positive at present it seems? Provide a balanced review of windbreaks.*
Reply: We added a paragraph on challenges associated with windbreaks to the introduction: "In a comprehensive review on US windbreaks, Smith et al. (2021) found that the main drivers leading to windbreak removal are the poor conditions of the trees, the age of vegetation, conflicts with irrigation and machinery, and competition with crops. The first two points highlight the importance of proper windbreak maintenance, intrinsically coupled with additional time and labour. The latter two points demonstrate how important it is to design windbreaks appropriately, so that resource competition between tree and crop can be limited by e.g. suitable spacing and choice of species in combination. Within this concert however, the windbreak's effect on the local water balance remains a critical research challenge." (L. 70)

- Smith, M. M., Bentrup, G., Kellerman, T., MacFarland, K., Straight, R., and Ameyaw, Lord: Windbreaks in the United States: A systematic review of producer-reported benefits, challenges, management activities and drivers of adoption, https://doi.org/10.1016/j.agsy.2020.103032, 2021.

L. 113: *I would specify that the green is the extent of the windbreak canopy and the grey is the blackberry canopy. Specify at what date since the canopy sizes change with pruning etc.*
Reply: We added a sentence to specify which is which canopy. However, it is only a sketch to illustrate where the measurements were taken and not to scale with exact dimensions of the canopies. We pointed that out in the figure caption of the revised manuscript: "Figure 1. Left: Sketch of sampling design and location of the alder-blackberry AFS near Stellenbosch, South Africa. For illustrative purposes, the alder canopy is shown in green and the blackberry rows in grey shading. The triangles show the location of the soil water sensors for the monitoring, each point signifying four soil water content sensors and three matric potential at the point near the windbreak." L. (129)

L. 181: *soil water. I would avoid using he word moisture and use water if you can since moisture could have sources other than water. Therefore, soil water probes.*
Reply: To our knowledge are the terms "soil moisture" and "soil water content" used synonymously in the hydrology community. Nevertheless, we replaced soil moisture with soil water content in the revised manuscript to stay consistent within the manuscript.

L. 200: *water on the sensor affecting the transmission of the ultrsonic electromagnetic reflection.*
Reply: Thank you! We included that suggestion into the revised manuscript. "This error also occurred in some cases in the morning, likely attributable to water on the sensor affecting the transmission of the ultrasonic electromagnetic reflection." (L. 211)

L. 207: *I'm assuming you worked out that this was equivalent to the irrigated amount of 2.7 L per plant with some interception loss?*
It is unfortunate that you don't have irrigated volumes (unless I have missed them?)
Reply: Yes, that is correct. However, we did not consider any interception loss in the calculation as the drip irrigation system was installed directly at the soil surface. We agree that it would have been extremely helpful to have exact data on irrigated volumes. This data was not available.

L. 223: *What did you do about rainfall and irrigation cancelling out decreases in VWC? Perhaps you can briefly include that the model includes the water inputs. Did you apply canopy rainfall interception? This will be significant under the WB. I know you estimate interception based on LAI but is this included in the RWU calculation?*
I'm also concerned that these results for WB and BB of RWU are from a single pit at each location. Profile soil water content across a field can be quite variable. Of course there are limitations to how much equipment can be installed.
Reply: We understand two main concerns in this comment, which we would like to address separately:

1) RWU calculation and influence of interception/rainfall/irrigation

Just as a brief summary: The root water uptake estimates were derived solely from the soil water content measurements, which were in total eight time series captured with the TDR sensors (two locations, each equipped with sensors at four different depths). Precipitation and percolation of water into the soil influence the soil water balance. Therefore, the RWU model cannot be applied to days with rainfall and considering interception is not relevant. The influence of irrigation on the RWU estimates is in our case more difficult to assess. The lack of irrigation data or precise time windows were irrigation occurred does not allow removing those days from the estimated RWU time series and, therefore, the absolute amounts of root water uptake are probably underestimated. However, the drip irrigation system influences both measurement locations similarly; hence, comparison of relative changes between them is possible anyways.

2) Soil water content across a field, representativeness

We agree that soil water content is strongly heterogeneous. However, several studies (e.g. Brocca et al., 2007, 2009, Mälicke et al. , 2020) show that soil water content observations are rank stable, i.e. wettest locations remain the wettest and driest locations remain the driest, even after rainfall events or dry spells. Considering this means, that great care needs to be taken when comparing absolute values but dynamics and relative differences should be fairly representative.

- Brocca, L., Morbidelli, R., Melone, F., and Moramarco, T.: Soil moisture spatial variability in experimental areas of central Italy, J. Hydrol., 333, 356–373, https://doi.org/10.1016/j.jhydrol.2006.09.004, 2007.
- Brocca, L., Melone, F., Moramarco, T., and Morbidelli, R.: Soil moisture temporal stability over experimental areas in Central Italy, Geoderma, 148, 364–374, https://doi.org/10.1016/j.geoderma.2008.11.004, 2009.
- Mälicke, M., Hassler, S. K., Blume, T., Weiler, M., and Zehe, E.: Soil moisture: Variable in space but redundant in time, Hydrol. Earth Syst. Sci., 24, 2633–2653, https://doi.org/10.5194/hess-24-2633-2020, 2020.

L. 226: *consider using other terminology. Evaporative fraction is usually LE/(Rn-G) in the literature. What you are describing is more of a crop factor?*
Reply: Thank you very much for pointing this out. You are absolutely right and we mixed up the terms. AET/PET is known as the crop coefficient K_c or the water limitation factor f_w (Ghausi, 2023). We will use the latter as it appears more suitable to our case and will adapt the revised manuscript accordingly.

- Ghausi, S. A., Tian, Y., Zehe, E., and Kleidon, A.: Radiative controls by clouds and thermodynamics shape surface temperatures and turbulent fluxes over land, Proc. Natl. Acad. Sci., 120, https://doi.org/10.1073/pnas.2220400120, 2023.

L. 231: *canopy?*
Reply: Yes, canopy characteristics were measured for individual trees and also for the whole windbreak as a collective. However, the LiDAR also allows for the measurement of the whole tree – we therefore leave the heading as it was.

L. 231: *from the LiDAR?*
Reply: The sentence was updated to include the origin of the point cloud data – this was indeed from from the LiDAR scans. We will adapt the sentence in the manuscript: "The point clouds derived from the TLS campaign were processed to obtain structural tree data, foliage data, and windbreak characteristics." (L. 243)

L. 248: *How was this calculated? I'm assuming it comes from the LiDAR? Couldn't find anything in the materials and methods on it?*
Reply: We added the following explanation to the revised manuscript: "The leaf creation algorithm by Bohn Reckziegel et al. (2022) was used to estimate foliage by restricting leaf classes to "small", "medium" and "large" categories with corrected ratios according to leaf sizes for *Alnus sp.* (San-Miguel-Ayanz et al., 2016). The leaf spacing definition was varied from 2.0 to 3.0 cm to estimate total leaf area on a tree basis and leaf dry mass assuming the specific leaf mass of black alder (*Alnus glutinosa* (L.) Gaertn.) of 13.3 ± 0.3 m^2 kg^{-1} (Johansson, 1999)." (L. 258)

L. 251: *This is essentially a constant fraction of rainfall regardless of the rainfall event depth, which is not an ideal way to handle interception. Commonly a problem in water-balance studies though.*
Reply: Interception is mainly influenced by two components. The first component relates to vegetation type, encompassing characteristics such as density, age, and height, which can be quantified by the leaf area index (LAI). The second component pertains to precipitation, which varies in intensity, duration, and frequency. Our understanding of interception conceptualizes it as a reservoir that accumulates with each rain event until reaching full capacity, after which water begins to drain through trunk drainage or throughfall. Consequently, storm frequency exerts a greater influence on annual interception than the total amount of rainfall during storms. Conceptual models use this approach to estimate interception and interception loss (e.g. Rutter et al., 1971, Gash et al. (1995). Therefore, our approach is deemed to be a reasonable assumption including LAI and storm frequency (in the yearly sum) and given the data at hand.

- Rutter, A. J., Kershaw, K. A., Robins, P. C., and Morton, A. J.: A predictive model of rainfall interception in forests, 1. Derivation of the model from observations in a plantation of Corsican pine, Agric. Meteorol., 9, 367–384, https://doi.org/10.1016/0002-1571(71)90034-3, 1971.
- Gash, J. H. C., Lloyd, C. R., and Lachaud, G.: Estimating sparse forest rainfall interception with an analytical model, J. Hydrol., 170, 79–86, https://doi.org/10.1016/0022-1694(95)02697-N, 1995.

L. 257: *do you mean 'remaining'?*
Reply: Yes, thank you! We corrected this in the revised manuscript.

L.258: *why?*
Reply: We agree, the reasoning behind this step was not presented. We therefore added the following: "…interval of 10 min, this was applied in order to simulate the shade cast specific to the windbreak in its defined position." (L. 275)

L. 258: *10?*
Reply: No, minute is correct. The Sonbesie weather station has a 60 second data update rate: http://weather.sun.ac.za/api/Measurement%20description%20-%20Sonbesie%20CR1000.txt, nevertheless we will update this in the text to "60 second" to avoid any confusion.

L. 265: *I'm not convinced that you can provide an estimate of WUE of a windbreak, based on the WU of a completely different plant? I know you use the word 'rough' but is it valid. Do they have similar, size, LAI and rooting depth perhaps?*

Reply: Since no specific information is available for our alder species we must infer information from similar species, it was our intention that this frames the results presented. We admit that the estimations were quite vague. It was a humble attempt to compare the water savings due to wind shading effects with the losses by tree water consumption. Future work should empirically measure the study species in detail and assess water use efficiencies. We removed the section in the revised manuscript.

Fig.2:

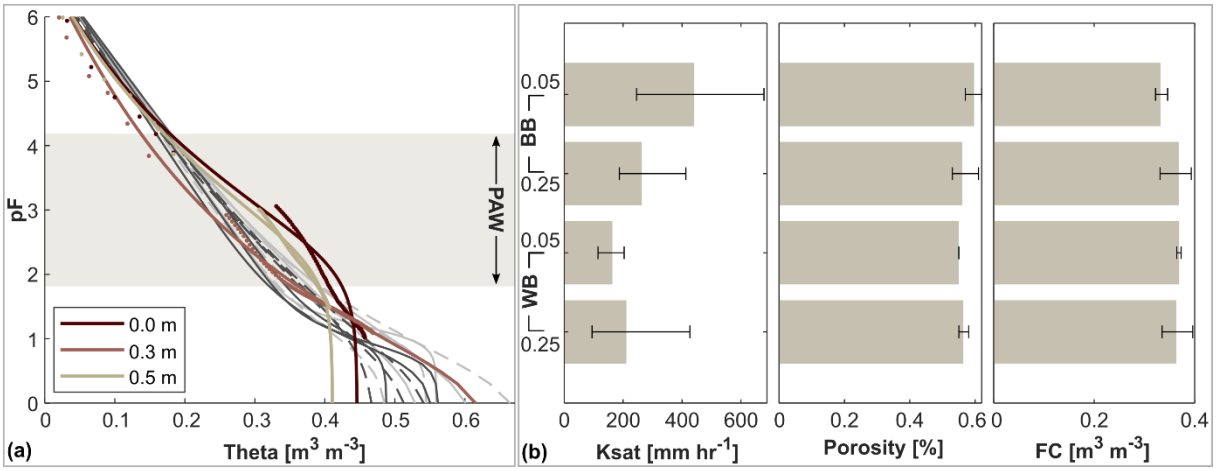
- *I'm assuming the P values in the graph are hourly? I suggest including them as daily values so that they match up to the values reported in the paragraph above.*
- *Use the same tick marks and labels on the y-axis of both WC graphs. Currently the scale is slightly different to difficult to compare.*
- *In the materials and methods, the 3rd probe is described as being at 40 cm?*

Reply: Thank you for paying close attention to the figure and your suggestions.

- We understand your point, but we prefer keeping the values as hourly data so that they match with the other plots, especially with the soil water content time series, so that one can observe dynamic evolution of both on the same time scale.
- Good point. We adapted the figure axes.
- Thanks. We corrected it.

L. 317: *Restructure so that this follows the paragraph above.*

Reply: Thank you for pointing at this paragraph. We realized that the table and associated text might not have been the ideal way to present the results. We added a figure to illustrate the most striking values from the table and remove the table from the main text. We will also rephrase the paragraphs in the relevant sections (3.2.1 and 4.1.2, 4.2.1) to explain the observed measurements better:



"Figure 3. Various soil characteristics of the undisturbed soil samples. The left panel (a) shows soil water characteristic curves (vol. water content theta vs. soil suction pF) of undisturbed soil profile samples taken at different depths at the monitoring location within the windbreak rooting influence, adjacent to the soil water content sensors. The values (dots) were taken during the drying process of the sample under laboratory conditions and parameterized with the PDI model by Peters (2014) (lines). Lines in grey represent additional undisturbed soil samples (darker shade are the upper samples, dashed line represents the WB samples). The shaded boxes illustrates the area of the PAW (plant-available water storage). The right panel (b) displays averages (bars) and ranges (lines) of different

properties of the additional soil samples from March 2022: Soil hydraulic conductivity (Ksat), porosity and water content at field capacity (FC).” (L. 328)

L. 349: *Please revise grammar of this sentence. I'm not sure what you are trying to say here? There the VWC fluctuates with depth and between measurement sites?*
Reply: Thank you. Yes, that was indeed our message. We adapted the sentence in the revised manuscript: “The water content differed with depth and between the two measurement locations. At both locations (blackberries and windbreak), the upper soil water content was consistently lower than that at greater depths.” (L. 349)

L. 359: *Why?*
My guess is that these were days when irrigation or rainfall increased VWC?
Reply: It is correct that on those days, we observed either an increase or a constant soil water content in the time series. The step-shaped decrease during the run of a day is mandatory though as it is the basis for the RWU estimation. The reasons for the lack thereof can be numerous such as precipitation events, irrigation or water redistribution and it was not clearly distinguishable what the reason was for each specific day that RWU could not be calculated.

L. 369: *These values are very low for an irrigated crop during a dry Cape summer? How does this compare to the PET?*
Reply: The RWU calculation based on soil water content changes worked only for very few days. Therefore, we most likely underestimate the RWU substantially. The water limitation factor, or previously evaporative fraction, is precisely the ratio between RWU and PET and had mean value of 11.3 %, indicating that the transpiration was strongly water limited or that we underestimated RWU considerably.

L. 389: *Do you mean the soil water INCREASE was greater than the rainfall?*
Reply: Yes, indeed. Thank you for pointing this out! We modified this.

L. 402: *This seems very low for such a high LAI. I would have expected values of up to 2 or 3 mm. In the commercial forestry industry it is typically 3 or 4 mm or 7 - 35% of the rainfall.*
Reply: Thank you! The interception in windbreaks is likely to be lower than that in closed-canopy forests as branches all the way down the canopy (in windbreaks) are exposed to wind effects and movement, thereby shedding some additional water from the canopy. The lower branches in closed-canopy forests experience less movement and can therefore hold water until on the canopy until it is evaporated from the canopy. We also indicated in the discussion that the total interception may be an underestimate as it did not take rainfall events less than 2 mm into account. We assume the expected values of 7-35 % of rainfall refer to annual canopy interception, hence cannot be directly compared to ours, which is only based on half a year and especially the dry season. The reference period thus greatly affects the number of storms being considered. In the literature it is difficult to find interception estimates either for our Alder species or for windbreaks, but we added interception values from literature to the discussion for deciduous trees to give a range of possible interception (e.g. 22 % for temperate deciduous broadleaf forests (Dingman, 2015)): “The literature gives interception values of, for example, 22 % of yearly rainfall for temperate deciduous broadleaf forests, which would lead in our case to 53.9 mm for the measurement period or 173 mm for a whole year (Dingman, 2015).” (L. 444).

- Dingman, S. L.: Physical Hydrology, Third Edition, Waveland Pr Inc, Long Grove, Illinois, 643 pp., 2015.

L. 409: *previously you say you were going to use the annual water use of a nearby vineyard?*
Reply: We used the annual evapotranspiration of a nearby vineyard, which was estimated by Veste et al. (2020). We changed the wording to make this explicit.

- Veste, M., Littmann, T., Kunneke, A., Toit, B. du, and Seifert, T.: Windbreaks as part of climate-smart landscapes reduce evapotranspiration in vineyards, Western Cape Province, South Africa, Plant, Soil Environ., 66, 119–127, https://doi.org/10.17221/616/2019-PSE, 2020.

L. 412: *I would like to see this as a depth of water evaporated in mm from the area covered by the tree canopies which you will easily calculate? This allows a realistic comparison against PET and rainfall in mm.*
Reply: According to your comment L. 265, we decided to remove this part.

Fig. 6:
This top part of the image is too small?
I thought the WB was about 40 m long so would expect the shaded area to be longer?
Reply: The shadow model was calculated based on 29 trees clipped from the total windbreak length. Trees at the edge were omitted from the analysis due to their distinct growth forms. We removed the figure from the manuscript due to its minor significance to the overall manuscript, as we only extract one value from it to estimate the insolation effect. We preferred to incorporate this information into the text and place greater emphasis on the remaining figures, which will also be revised to include more content.

L. 441: *I'm not clear on how you calculated RWU based on changes in soil profile water content without knowing what the irrigation input was?*
Reply: We calculated RWU for every day that was possible, i.e. every day where a decrease in soil water content between two nights was observable. The irrigation lead to an underestimation of RWU, because it either reduced or completely removed the decrease. Even if we had precise data on irrigation, we would not be able to know how much of the irrigation input would precisely reach the soil within our control volume of the sensor. We would still have to calculate the RWU in the same matter, but in a following step, we could then have compared the differences in RWU on days with and without irrigation.
L. 443: *Not always. It depends on the size of the event relative to the interception.*
Reply: We value your feedback on our interception estimates very much. We elevated this point and previously discussed points in the revised manuscript in the section on interception (4.1.1).

L. 448: *This assumption still concerns me a bit. Its not directly related. If interception capacity is 2 mm and the event is 4 mm then half the rainfall is intercepted. If the rainfall event is 8 mm then a quarter of the rainfall event is intercepted. Therefore not directly proportional.*
Reply: Thank you. We included this point together with previous ones in the revised manuscript. We hope that the previous replies consider all concerns regarding the interception sufficiently.

L. 450: *Agreed. The relationship would be valuable but you don't provide it in your study?*
Reply: Yes, we agree. Unfortunately, we do not have the data to provide this relationship for our study area.

L. 457*: Is could be interception but it could also be many other things like differences in water use and soil properties, macropores. I think it is a bit of a leap to attribute the difference in soil water between two sites to purely interception when they are under different landcovers.*
Reply: Yes, that is correct. We rephrased it to mention also other possible sources for the discrepancies: "This can potentially be attributed to interception differences between the two locations. The difference between the two locations is 26.5 mm for the entire period, which aligns with the interception amount of 40 mm per year stipulated in the literature for alder species (Muthuri et al., 2004). However, the alternated wind field due to the obstacle in the flow path, might also cause a change in the precipitation pattern at and around the windbreak. Häckel (1999) state an increase of up to 15 % in precipitation behind the obstacle (until up to 10-times plant height) and a reduction of 10 % directly at the windbreak." (L. 453)

L. 468: *How are you comparing a soil storage with a precipitation intensity. The units are different?*
Reply: Yes, we agree. We removed intensity as we were only talking about accumulating precipitation amounts. Thank you!

L. 470: *explain how please.*
Reply: In response to the comment on L.317, we made substantial changes to the section that this comment addresses. We believe that the issue of this comment is covered during the rephrasing process.

L: 485*: perhaps rephrase or clarify. Increase in soil moisture to get RWU doesn't sound right?*
Reply: We rephrased the sentence. We meant soil water content decreases during the course of a day: "Root water uptake calculations did not work for approximately 50 % of the data points due to the absence of an decrease in the soil water content time series during the day, which results in a typical step-shape curve that is the necessary for RWU estimation (Jackisch et al., 2020)." (L. 498)

L. 489: *I couldn't find in the materials and methods whether the Alder trees were also drip irrigated and at the same frequency or not? In this last sentance you indicate that they are?*
Reply: There is no irrigation in the row of the alder trees, i.e. between the trees or directly adjacent. However, the first row of blackberries at a distance of less than 2 m is already irrigated and we assume the alder roots to tap there, too.

L. 501: *I agree. Although from the 20-40 down to 60 to 80 cm, there was a decreasing trend in RWU which doesn't fit with a theory of increasing RWU at depth? Would the roots have gone through the Bw1 rocky layer? What is the depth of the soil profile?*
Reply: Yes, we agree that the observed pattern does not fit with our theory. Unfortunately, we cannot differentiate if we just did not capture the RWU or if it did not occur at those depths. Reasons for this discrepancy could be that the roots bypass the depths, where the sensors are located, to tap water from deeper depths or grow rather horizontally. At the foot of the hillslope was a slightly eroded wall next to some alder trees, allowing to look at a transect into the soil. It seemed like some alder trees were growing strong roots that would reach below the soil profile depth of approximately one meter, extending into the bedrock.

L. 502: *Why do you say, 'in addition'? Why can't the RWU be using for the windbreak? Your previous point was that there may be deep roots.*
Reply: As mentioned in the paragraph, we have only very little data on RWU and are not sure on the influence of irrigation on them. Potentially, we were also not measuring deeply enough, therefore we are very careful with the interpretation of the RWU data. We removed "in addition" from the text.

L. 504: *refere to previous comment on 'available energy'.*
Reply: If this addressing the issue of AET/PET, we made adaptions in the revised manuscript to rename the fraction to “water limitation factor”.

L. 507: *Did the irrigation not take care of the water-limitation?*
Reply: Thank you for your question. Yes, the irrigation mitigated the water limitation, especially for the blackberries. We wanted to outline possible explanations for the differences in RWU fractions at the two locations, especially because the pattern is opposing the expectation of more RWU near the windbreak. Most likely both is causing this: 1) We could not capture enough RWU due to missing diurnal decreases in the time series, and, 2) the sensors are either not in the spot where most RWU occurs (directly adjacent to roots) or the roots tap water from greater depths than the sensors measured. We removed the sentence on water limitation.

L. 512: *Please confirm this with a graph of PET as this doesn't make sense to me from your descriptions. Your evaporative fraction is AET/PET which was 9 and 12%. here you say AET is closer to PET. Firstly it doesn't seem very close and secondly, closer than what? What is your comparison here? Irrigated vs unirrigated. If this is the case you need evaporative fraction for both scenarios.*
Reply: We meant to say that due to irrigation, the water limitation was averted and this was shown by the Budyko aridity index (3.7 without irrigation and 0.7 with irrigation). Restricted water supply drastically influences and limits the evapotranspiration. Therefore, if water is not a limited resource, the actual evapotranspiration is greater (than if water was limited) and more similar to maximum potential evapotranspiration. The now-called “water limitation factor f_w” and previously wrongly termed “evaporative fraction” was estimated for the two different locations, but is not addressed in this paragraph about the Budyko aridity index.
However, as the reviewer suggested one can look at the water limitation factor for both scenarios: At the berry location K_c was 23.0 % for the period without irrigation and 11.6 % for the irrigated period, and at the windbreak, K_c was 12.4 % without irrigation and 9.5 % with irrigation. The fraction of AET of PET is greater at the berries than at the windbreak, independently if looking at the whole observation period or if dividing into the periods with and without irrigation. The difference between irrigated and not irrigated period is greater at the berry location than at the windbreak. However, it is also based on only one single estimation of RWU for the not-irrigated period. Therefore, no conclusions should be drawn from this assessment.
L. 515: *Impossible. Too high.*
Reply: Thank you very much for noticing this. We definitely agree. We redid our calculation, ended up at 10.8 mm PET for the entire day and corrected this in the revised manuscript.

L. 517: *These values in mm/day are too high. Normally shaded areas would be avoided so I'm not sure of the value in these results although they would provide a guide for farmers are how much of a space to leave if almost full sunlight is required.*
Reply: Following the previous comment, we recalculated the PET values and will adjust the estimations in the manuscript. The fraction of 54 % remains the same, but the reduced PET is 5.8 mm d^{-1}. We agree that if full sunlight is a requirement, then this estimation might not be as relevant. However, this example’s aim is to illustrate the effect of sun shading on the water balance in the soil, which might also affect crops at a somewhat greater distance to the shading object (here: windbreak).

L. 523: *This reduction is only in the vicinity of the WB and not the entire crop.*
Reply: The influence of wind reduction is a multiple of the height of the obstacle (here: windbreak). On the object’s leeside a 50 % reduction may occur up to a distance of a tenfold of the obstacle height (in our case: ca. 100 m) or differently formulated maximum zonal effects may extend five times the

height of the windbreak downwind (Campi et al.,2009, McNaughton, 1988). The normal conditions are reached at a thirtyfold of the height. Hence, in terms of wind speed our entire crop field is in close vicinity to the windbreak.

- Campi, P., Palumbo, A. D., and Mastrorilli, M.: Effects of tree windbreak on microclimate and wheat productivity in a Mediterranean environment, Eur. J. Agron., 30, 220–227, https://doi.org/10.1016/j.eja.2008.10.004, 2009.

L. 525: *There measurements are based on a change in reference evaporation with an empirical crop factor*

Reply: Yes, that is correct. Veste et al. (2020) used a standard method, the FAO combined Penman-Monteith approach, to determine reference evapotranspiration, which requires reference values for resistances, and a season-dependent crop coefficient to estimate crop-specific evapotranspiration. However, their reduction of wind speed is a measurement conducted in the field.

L. 527: *integral assessment of what?*

Reply: We rephrased the sentence to avoid doubling of "assessment" and to clarify the message: "In a nearby vineyard, Veste et al. (2020) measured a 20 % reduction in wind speed and evapotranspiration due to tree shelterbelts. For the sake of completeness, it should be noted that sunlight is essential for the growth of the blackberry crop, and excessive shading may adversely affect growth, and thus, the yield of the field, hence, a detailed assessment of shading effects is crucial. Shading is predominately a factor of height, volume and porosity of windbreak crowns, other structures in the landscape, aspect and slope." (L. 537)

L. 530: *This should be accounted for in planting and when pruning the WB.*

Reply: We totally agree. If sun light is crucial for the crop, the shading effects in general but also specifically on slopes need to be considered.

L. 544: *rainfall interception possibly, which I think could be higher than your estimates but does depend on the size of the rainfall events.*

Reply: Yes, interception is one possible explanation together with changes in precipitation patterns around windbreaks and different soil storage capacities.

L. 549: *Solar radiation not shown so we will have to take your word for it?*

Reply: The topsoil matric potential observations exhibited a diurnal pattern, which intensified in drier soil conditions. Given that diurnal patterns essentially arise from solar radiation, we deemed it unnecessary to include a figure, particularly as it was solely meant as a description of the signal evolution. To prevent any ambiguity, we adjusted the sentence in the revised manuscript: "The former can be seen in the observations of the matric potential, which exhibited pronounced daily fluctuations." (L. 567)

L. 562: *Agreed. The manufacturer will specify in the manual that calibration is required.*

Reply: Yes, but even calibrated sensors are susceptible to offset errors. As mentioned in our reply to the comment of l. 223, studies show that vol. water content sensors remain stable in their order. Since we are mostly interested in the relative dynamics, it is not so critical for us to know the precise absolute values. In addition, in-field or soil-specific calibration is not easily undertaken.

L. 564: *This is an unconfirmed QSM model estimate and comes from the section on windbreak properties rather than the sections on AET or PET?*
Reply: The WUE was approximated based on an equation for biomass growth found in the literature (Gholz et al., 1979) and estimated windbreak properties from the terrestrial laser scans at the field site. This paragraph was removed in the revised manuscript.

- Gholz, H. L., Grier, C. C., Campbell, A. G., and Brown, A. T.: Equations for estimating biomass and leaf area of plants in the Pacific Northwest - research paper 41, Corvallis, Oregon, 1–38 pp., 1979.

L. 567: *Please explain. 742 mm + 1402 mm is more than 1902 mm?*
Reply: The sum of 1402 mm comprises both irrigation and precipitation for our measurement period from mid-September to mid-March. To get an idea of the yearly water input, only the remaining precipitation from mid-March to mid-September (500 mm) is added to the 1402 mm (irrigation already included), a period during which no irrigation occurred, and adds up to 1902 mm.

L. 592: *Did you establish that soil evaporation was a significant component of the ET?*
Reply: Thanks for pointing this out. We changed the word to evapotranspiration, as this is what we talked about throughout the manuscript.

L. 592: *how did you show this?*
Reply: Thank you for bringing up this matter. We revised the conclusions as suggested by reviewer 1 and accordingly rephrased the sentence. There was a slight oversight on our part; our intention was to use infiltration and water redistribution as illustrations of processes impacted by the windbreak.

L. 594: *possible occurrence - you didn't prove this.*
Reply: Correct. We adapted this suggestion into the revised manuscript.

L. 595: *I don't remember any references to monoculture carbon sequestration?*
Reply: We added a reference to the revised manuscript to address this comment. Thank you: "Sheppard et al. (2024) for example, showed that a poplar windbreak in South Africa of similar dimensions could store nearly 200 tonnes of CO2 equivalent per km of windbreak in the aboveground portion alone." (L. 594)

- Sheppard, J. P., Larysch, E., Cuaranhua, C. J., Schindler, Z., du Toit, B., Malherbe, G. F., Kunneke, A., Morhart, C., Bohn Reckziegel, R., Seifert, T., and Kahle, H.-P.: Assessment of biomass and carbon storage of a Populus simonii windbreak located in the Western Cape Province, South Africa, Agrofor. Syst., https://doi.org/10.1007/s10457-023-00940-1, 2024.

L. 599: *Did you measure surface runoff?*
Reply: No, we do not have any observations of surface runoff.

L. 602: *What are the open questions?*
Reply: As mentioned before, we rephrased the conclusions and included future outlooks and thus open questions.

Appendix: *You can't have an Appendix that is not referred to in the text of the document. It needs to support some statement.*
Reply: Thanks! The reference must have disappeared during the last steps of manuscript preparation. We added a reference to the appendix figure in the revised manuscript's main text, e. g. "…see fig. A1 for remaining events." (L. 291).