Response to Anonymous Reviewer #2

The text in italics refers to the literal reviewers' comments. The text in blue is the literal text edit suggestions for the revised manuscript.

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 but will be visible in the tracked changes version of the revised manuscript.
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 will correct 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 is an important detail missing and we will add it to the revised manuscript's abstract.

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 will harmonize 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 will therefore refrain 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 will add 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 challenges leading to a 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, which is associated with additional time and costs. The latter two points demonstrate how important it is to design windbreaks appropriately, so that competition with crops over resources can be limited by e.g. suitable spacing and choice of species in combination.”

- 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 will add a sentence to specific 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 will point that out in the figure caption of the revised manuscript.

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 will replace 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: Thanks. We will include that suggestion into the revised manuscript.

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 is 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 written.

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 terrestrial LiDAR scan campaign were processed to obtain structural tree data, foliage data, and windbreak characteristics".

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 will add 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. 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 will correct this in the revised manuscript.

L.258: *why?*

Reply: We agree, the reasoning behind this step was not presented. We therefore, will add the following: "…interval of 10 minutes, this was applied in order to simulate the shade cast specific to the windbreak in its defined position."

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 will remove 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 will adapt the figure axes.
- Thanks. We will correct 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 be the ideal way to present the results. We will add 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.

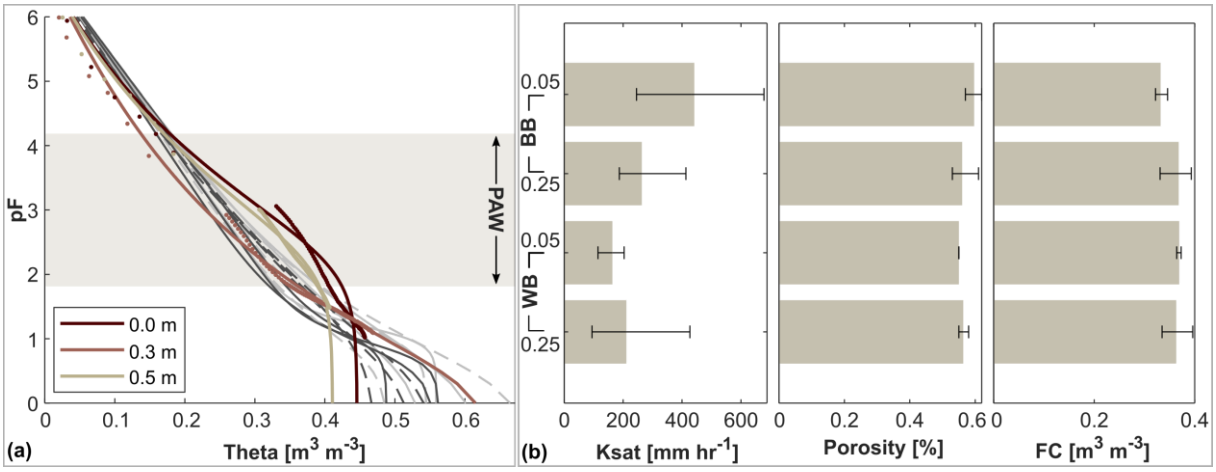

Figure 3. Various soil characteristics of the undisturbed soil samples. The left panel (a) shows soil water characteristic curves (vol. water content theta vs. soil suction pF) of undisturbed soil profile samples taken at different depths at the monitoring location within the windbreak rooting influence, 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. 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 will adapt the sentence in the manuscript: "The water content differs with depth and between the two measurement locations. At both locations (blackberries and windbreak), the upper soil water content sensors consistently measured a lower water content than the sensors at greater depths."

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 is strongly water limited or that we underestimate 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!

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 suggest to add interception values from literature for deciduous trees to give a range of possible interception (e.g. 22 % for temperate deciduous broadleaf forests (Dingman, 2015)).

- 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 will change 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 during the revisions.

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 propose remove the figure from the revised manuscript due to its minor significance to the overall manuscript, as we only extract one value from it to estimate the insolation effect. We prefer 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 leads to an underestimation of RWU, because it either reduces or completely removes 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 will also elevate this point in the revised manuscript.

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: We value your feedback on our interception estimates. We will include this point together with previous ones in the revised manuscript.

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 will rephrase it to mention also other possible sources for the discrepancies.

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

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

L: 485*: perhaps rephrase or clarify. Increase in soil moisture to get RWU doesn't sound right?*
Reply: We will rephrase the sentence. We meant soil water content decreases during the course of a day.

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 patters 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 are 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 will remove the term “in addition” from the text.

L. 504: *refere to previous comment on 'available energy'.*
Reply: If this addressing the issue of AET/PET, we acknowledge the feedback and will make 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 will remove 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 suggests 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 will correct 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 will rephrase the sentence to avoid doubling of "assessment" and to clarify the message.

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 will adjust the sentence in the revised manuscript.

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 will be 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 will change 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 will revise the conclusions as suggested by reviewer 1 and accordingly rephrase 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 will adapt this suggestion into the revised manuscript.

L. 595: *I don't remember any references to monoculture carbon sequestration?*
Reply: We will add 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."

- 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 will rephrase the conclusions and will include 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: We agree. The reference must have disappeared during the last steps of manuscript preparation. We will refer to the appendix figure in the revised manuscript.