Please see below the response to the reviewer regarding their concerns. We also included a PDF version of the revised manuscript with changes highlighted in yellow.

REVIEWER 1

General comments

This manuscript presents valuable measurements of soil respiration in a semi-arid agricultural setting. In general, the work highlights many of the challenges in measuring and interpreting semi-arid soil respiration. The authors demonstrate a strong grasp of the literature relevant to semi-arid olive plantations, and the potential competing factors influencing soil respiration. Unfortunately, the organization of the introduction and discussion does a disservice to the data presented. The topics introduced range widely, with the four stated objectives (lines 92-96) ranging widely from presenting specific data, to interpreting driving mechanisms, to comparing eddy covariance models. The manuscript feels caught between exploring soil mechanisms on one hand, and upscaling to the ecosystem level on the other. The comparison between chamber measurements and eddy covariance, while in principle a relevant topic, is underdeveloped, even to the point of distraction from the main contributions of the manuscript. Conversely, the interesting scientific contributions on the driving mechanisms of soil respiration are understated in the scientific arc of the manuscript. These should be further developed with analysis, potentially leading to much more satisfying conclusions on the role of heterotrophic vs rhizosphere respiration, and also the interplay between moisture and temperature. Additionally, I would be particularly interested in further discussion of mechanisms by which the tree itself drives factors that influence Rs. I suggest the acceptance of this manuscript with minor revisions to improve the scientific organization and to further the analysis and interpretation of potentially enlightening data.

R: We appreciate the thorough and constructive review from Reviewer 1

We have moved two figures to supplementary material to facilitate inspection of the main results. Also, we have organized the objectives by reducing them to three; and we have generated subsections in the discussion to make the topics discussed in the manuscript clearer. Now the objectives are:

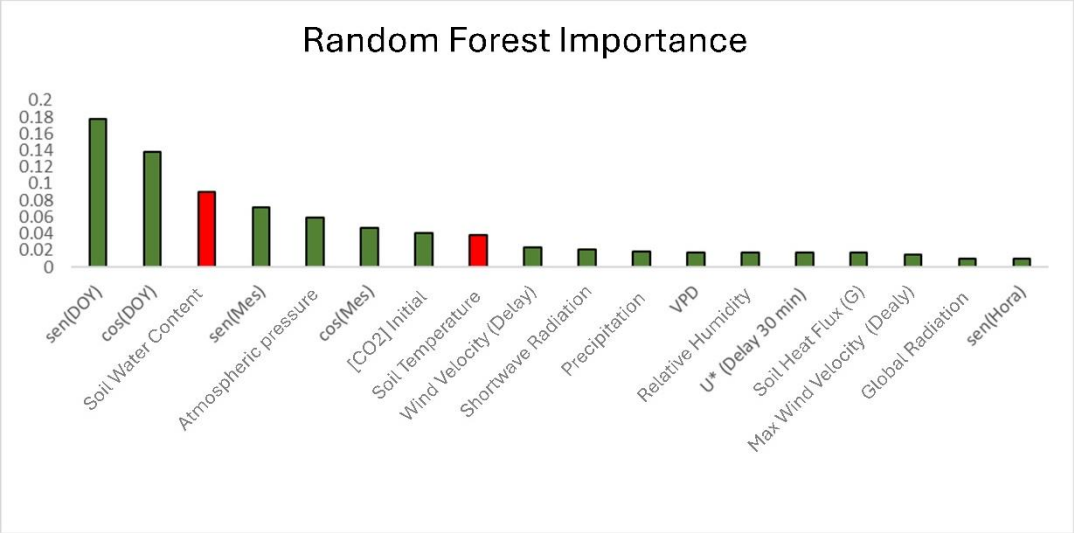
> "to i) determine the temporal and spatial variability of R_s in an olive grove; ii) analyze the main environmental drivers in R_s and its temporal and spatial dependence, including rain pulse events; and iii) assess modeled ecosystem respiration (R_{eco}) using data from an eddy covariance tower comparing it with upscaled ecosystem Rs" (now lines 98-102).

And the discussion section now has the following subsections:

> 4.1 Spatial differences; 4.2 Eddy covariance models comparative; 4.3 R_s drivers; 4.4 Rain Pulse Events

Regarding the relevance of the topic of the drivers of R_s we fully agree with the reviewer. However, we believe that this topic warrants in-depth discussion in a separate manuscript. The main topic of this manuscript is to study the temporal and spatial

variability of Rs. An extension of the analysis related to drivers of Rs may distract from the primary focus and would make the manuscript very long, losing the possibility to address the issue about the upscaling. Therefore, we are working on deeper analysis in another manuscript in which we delve into the multitude of drivers involved for soil respiration and potential models based of its main drivers. Below, there is an example figure illustrating the importance of different drivers when applying machine learning techniques to better understand the key drivers of this agroecosystem. Here, we can see how atmospheric pressure, or wind speed (5th and 9th place) can be important, in addition to the soil temperature and humidity (in red).



Regarding the request of the reviewer about the possibility to include new analysis that potentially lead to more satisfying conclusions on the role of heterotrophic vs rhizosphere respiration and also the interplay between moisture and temperature, we have added a new result (Figure 5 inset) on the interdependence of temperature and SWC, which shows the relationship of parameter Q_{10} with temperature and with SWC, and we have added some text to describe this new figure 5 (now lines 286-293)

Finally, we have modified the introduction and discussion enhancing data interpretation about why the tree itself drives the factors that influence R_s.

Specific comments

● 19-20: The mention of EC model performance feels out of place in the abstract, especially since the daytime and nighttime models are not otherwise mentioned in the abstract. This is emblematic of the rest of the manuscript, where the EC contribution is potentially interesting, but not developed sufficiently to address scientific questions.

R: We agree. Since it is not mentioned before in the abstract we have removed this sentence.

● 37-50: The discussion of mediterranean climate, climate change, and drivers of Rs is hard to digest all at once. I suggest introducing Rs drivers, the mediterranean climate, and potential climate change as separate topics.

R: Point taken. We have reorganized the main ideas and separated the topics (now lines 37 - 42). We have briefly introduced the main R_s drivers and then developed the behaviors of these drivers in the mediterranean climate. At the end we deleted the

climate change allusion removing the “Mediterranean regions are at high risk of being impacted by climate change (Cramer et al., 2018).”

● 64: The Birch effect is introduced in name, but not in terms of mechanisms relevant to the paragraph - further discussion of mechanisms, such as the Birch effect, would greatly increase the applicability of this research.

R: Thanks for the suggestion. We have added the following sentence: “This mechanism can contribute to 5% of total annual respiration in semi-arid regions (Delgado-Balbuena et al., 2023) but has not been continuously explored in olive grove soils.”(Now Lines 69-70)

● 69 and elsewhere: the power of semi-continuous measurements vs measurement campaigns is apparently true, but not sufficiently developed here. The discussion of maximum variability in the discussion section is not necessarily convincing. It would be nice to see some analysis or at least discussion on how continuous measurements improved either the understanding of mechanisms or the upscaling efforts.

R: We appreciate the reviewer's comment. The course of the manuscript reveals and develops the benefits of having applied continuous measurements.

On the one hand, the statistical power is necessarily greater due to a greater number of replications (around 18,000 values per chamber per year compared to the typical 12 or 24 measurements per year), which means that statistics such as the mean are much more rigorous, having a number of samples similar to that of the eddy covariance technique.

On the other hand, continuous measurements have made it possible to carry out annual balances, minimizing the amount of data filled in by extrapolations resulting from regressions. We emphasized the advantages of continuous measures by reducing levels of uncertainty, offering the possibility of more rigorous upscaling as well as more easily detecting events during the night or unfavorable weather days.

Following the referee's suggestions, we have added the following sentences “A similar number of samples as the eddy covariance technique (~18,000 values per chamber per year) is much larger compared to the weekly or monthly measurement of typical studies, meaning that statistics such as the mean are much more rigorous. “(lines 367-369) ; “The use of automatic chambers made it possible to carry out annual balances of R_s” (line 408) and “Continuous measurements allowed us to study the contribution of each location to the total R_s” (line 391) to highlight the power of continuous measurements.

● 92-94: These aims feel both too wide and too overlapping. I think the entire manuscript could be strengthened significantly by focusing these aims, and adjusting the introduction and discussion accordingly.

R: We agree. As we mentioned before, we have rewritten the objectives joining i) and ii) and the creation of subsections in the discussion has been adjusted to these objectives (now lines 98-102). In addition, we have narrowed the discussion and tried to adjust it more to the introduction

● Rain pulse events: Section 2.5, 3.1, and 3.4. The role of rain-pulse events becomes one of the main themes of the paper, but it is relatively unmentioned in the introduction. The concept

should be better introduced in the introduction, which would also strengthen the discussion and interpretation of mechanisms. Particularly, I would be interested to read the author's discussion on the role of mechanical porespace displacement vs increased respiration. I also thought the discussion of types of rain events was excessive, e.g. Figure 4 and lines 248-257.

R: We appreciate the reviewer's comment. We have introduced this concept better in introduction adding "Additionally, rain-pulse events are frequent in semi-arid regions during dry season periods like Mediterranean climate have and may alter the annual carbon balance. The Birch effect (Birch, 1964) explains how carbon dioxide emissions increase by a high rate of rapid mineralization after the soil is rewetted due to a rain pulse event. This mechanism can contribute to 5% of total annual respiration in semi-arid regions (Delgado-Balbuena et al., 2023), can reduce significantly the annual net carbon gain (Jarvis et al., 2007) and has not been continuously explored in olive grove soils"" (now lines 66-71).

We have included a subsection in the discussion about "4.4 rain pulse events" and we strength the interpretation of its mechanism with more discussion and literature about the gas displacement (now lines 482-486)

Finally, we have followed the recommendation of the reviewer about reducing the information of types of precipitation. We have moved Figure 4 to the supplementary material and deleted part of the content of previous 248-257 (now reduced to lines 261-264).

● **Section 3.3: Q10 variability is discussed using only the Ts and SWC data from 5cm depth. Hysteresis is mentioned, but further discussion is warranted on the role of time lags for temperature and water to propagate through soil. This is relevant also for the discussion of rain pulses.**

R: We appreciate the comment and have added in the discussion the dependence of Q_{10} with depth. (now lines 437-441)

We also added more discussion about this topic in lines 477-479: "Furthermore, we identified hysteresis behavior only in summer in alleys, which could indicate diurnal changes of SWC close to some critical value for the R_s/T_s relationship. However, finding the hysteresis pattern also with SWC indicates that there is another factor in addition to temperature involved in R_s"

● **Tree-alley dichotomy: The data very nicely examines the gradient of temperature, moisture, and Rs going between the tree and the alley. Especially given how nice the data appears, this seems like it would be a great opportunity to investigate the mechanistic role of the tree on Rs and its drivers. I was also wondering about why it was necessary to identify a tree-alley dichotomy, when it appears so nicely represented as a continuous transition.**

R: Thanks for the appreciation. In the presentation of the results, we made it clear that there is a continuous gradient between the alley and the tree (Line 151-152)

We recognize we did not use the word "dichotomy" properly and we have changed the language from "dichotomy" to "distribution" (Line 394)

Finally, yes, we agree that it is a great opportunity to continue researching the distribution of roots, the microbiota or the autotrophic/heterotrophic respiration relationship along that gradient. We are currently working on it.

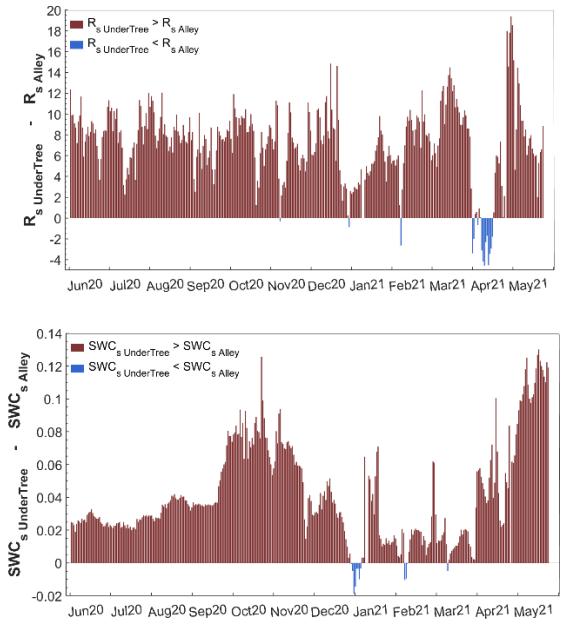
● Figure 5 and Discussion 430-447: The discussion of VPD and plant-Rs relationships is interesting, but not necessarily convincingly supported by the data shown in figure 5. This would feel better supported if the role of root exudates were introduced as a potential mechanism in the introduction.

R: We appreciate the reviewer's comment. Now, we mention the role of root exudates as a potential mechanism in the introduction (now lines 59-60). Also, we have included mention of the role of exudates in the discussion to give more coherence to this part (lines 384-386 and lines 456-459).

● Figure 6, and elsewhere: Why are Rs and SWC reported as ratios between the two locations, while temperature is the simple difference? This was also bothering me elsewhere in the manuscript, where ratios may not have been appropriate, as in comparing Rs spatial variability when all the emissions are quite low.

R: We appreciate the reviewer's comment. The reason why the temperature was different has to do with the kelvin units, which implied fractions of 0.01.

We still think that the ratios can provide more accurate information about the total contribution to the R_s or SWC of each location compared to the other since with the differences it is more difficult to visualize these relative data as can be seen below.



However, and at the suggestion of the reviewer nº2, we have decided to move this section to the supplementary (now Figure S3) material section as it may distract from the main results.

● 391-409: This section particularly feels underdeveloped. It is apparent that neither model is perfectly representing the ecosystem, but the mechanisms are unclear. I feel it is premature to suggest using the flawed models, perhaps it would be better to identify this as an area requiring further research on modeling mechanisms.

R: Good point. We have rewritten these sentences being more cautious and thus not discriminating against any modeling and encouraging more in-depth research and in other experimental sites on this issue, now lines 424-427.

● 468-470: The discussion of why EC did not pick up the Birch effect is quite interesting, but it is difficult to think why EC would not be real-time. Perhaps this is also best left as an area for future research.

R: The EC technique captures the rainfall pulses when we observe NEE. However, it is the models that fail and are not capable of detecting these pulses. For example, including ΔSWC in them would improve their predictive ability for the birch effect. Now we have modified the text to clarify: "At the ecosystem scale, we observed slight pulse signals with a lag of several days; therefore, the NEE models based exclusively on radiation or temperature may not be the most accurate for real-time characterization of this phenomenon. However, incorporating soil water content into these models would significantly enhance their predictive ability for the Birch effect. " (Now lines 501-504) The descriptive nature of the study means that each of the themes has not been deeply analyzed. Among them, this one you mention could be a line in which to delve deeper. Thanks for the appreciation.

Technical corrections

Generally here are some paragraph-level organizational comments:

● Lines 20-21, those are two relatively big and unrelated findings that don't fit well together in one sentence.

R: Fixed it. We separated it in two sentences.

● 31: Not clear what is set of carbon fluxes is being referenced with 'second largest'

R: We added "Globally" at the beginning of the sentence.

● 239: 'y' should be translated to 'and'

R: Corrected.

● 267: Not clear what trend "the trend in Rs" is referring to

R: We added "daily" before "trend".

● 280 and elsewhere: "compartments" may not be the right word to refer to different chamber settings.

R: We have replaced "compartments" with "locations".

● 405: It is expected that lasslop et al. (2010) would have greater…

R: We have fixed the grammatical mistake. "would" was added before "have".

● 486: Rs_alley should be subscripted

R: Done.

REVIEWER 2

This article presents one year of soil respiration data from an olive grove with the goal of quantifying spatial, temporal, moisture-driven, and microclimatic variation therein. They upscale their data accordingly and compare it to two different eddy covariance techniques for estimating Reco. This is a solid contribution to the semi-arid carbon cycling literature with a thorough and appropriate treatment of the topic. However, in general, the text leans towards redundancy and could lose substantial length without losing content. This was especially noticeable in the Introduction in the paragraphs beginning L60, L69, and L86 as well as the Discussion, which would benefit from subsections to organize ideas.

R: Thanks for the comment. We have worked to reduce the content of the introduction. We have also restructured the overall manuscript by reducing the number of graphs from results section and have added the following subsections to the discussion:4.1 Spatial differences; 4.2 Eddy covariance models comparative; 4.3 Rs drivers; 4.4 Rain Pulse Events for a better organization of the ideas.

L15: azimuthal vs angular vs directional are used throughout the text to refer to this idea, choose one and stick with it

R: Thanks for the appreciation. We have unified the language by choosing “angular”.

L33: Worth noting here that Rs is composed of both heterotrophic and autotrophic components

R: We appreciate the reviewer's comments. We presented these concepts with the following sentence “Mainly, R_s it is composed of heterotrophic and autotrophic respiration” now in line 32

L55: Fragment of a sentence

R: We moved the cite to the end of the sentence.

L100: “Arbequina” & “Cortijo Guadiana”, not sure why these are in quotes?

R: We removed the quotes.

L103: Köppen classification: Csa, is a clearer order for presenting these terms

R: Done

L123: Here the criteria for determining the best fit should be specified

R: Done. We added the sentence “ (lower residuals values)” after “best fit” now in line 128

L126 -126: More information on when gaps occurred would be helpful to the reader, perhaps in the supplement?

R: Thanks for the suggestions. We have added a figure (Fig S1) in the supplementary material that indicates the percentage of gaps in the data each month of measurement

due to instrumentation malfunction (gray) and after applying filters (black) in a) transparent chambers and b) opaque chambers.

L129: I suggest the authors break this into a section on continuous measurements and another on discrete campaigns

R: Done

L132-133: "the spontaneous seedlings…" this has already been stated in the paragraph above

R: We removed the sentence.

L139: Missing "a"

R: Done.

L144: This is a little confusing, but readily becomes clear when the values in the supplement are seen. As this table is compact, I suggest including this (or the values in line) in the main text

R: Thanks for the comment. We have moved the table of supplementary materials to the methods, now in L 151-156

L228: Fig 3a is referenced, not 3c

R: Fixed it

L253-255: Error terms should be included here and the associated figure when possible

R: Due to the suggestions of other reviewers, we decided to remove this section.

L264: "…, not being an isolated case" this is awkward phrasing. Suggest "During July, one chamber consistently showed no diurnal variability." If consistent is too strong, "frequently" could also be used.

R: We changed "not being an isolated case" to "During July, one alley chamber consistently showed no diurnal variability."

L298: "the greatest [increases] in Rs rates" – but also, it is clear the highest magnitude (ie., the greatest) Rs values are at the lowest IEP alleys, these need to be noted.

R: Thanks for the comment. We replaced "increases" with "greatest". On the other hand, we added "However, the four highest magnitude Rs values were found in the lowest IEP and PPT in the alleys but were excluded from the regression as they were statistically classified as anomalies"" now in the lines 297-299.

L319: "began to be found concerning the chamber" this too is awkward, "[relative to] the chamber closest to the tree" is clearer

R: We changed "began to be found concerning the chamber" for "were found relative to the chamber closest to the tree"

L325: Link between variable SWC and punctuality of irrigation is not obvious. Perhaps variation in SWC being driven by irrigation is what is meant?

R: Yes, we really mean that the high variability in SWC was driven by irrigation and prevented the detection of significant differences in soil water content. We have changed the text accordingly (now lines 325-326)

Figure 10: Difficult color scheme for distinguishing the different cardinal directions

R: Thanks for the appreciation. However, we believe that it is not necessary to change it since the color scheme is supported by the N, W, S, E nomenclature, which is found in both subplots (figure 8)

L345: 'in magnitude' can be removed. Sentence starting "However, although…" this seems contradictory? If they are similar in magnitude, how can they be inverse?

R: We removed "in magnitude" and we changed "similar" to "closer"

Figure 11: Not sure the right-angle connections are effective here. Lines connecting points should also be the same color as the points to improve readability

R: We have chosen the right-angle on purpose precisely to be able to visually compare the vertical axis more quickly between different methodologies and to more effectively measure the "jump" in CO_2 emission due to the pulse. On the other hand, we have followed your recommendation and increased readability by making the colors of the points and lines the same color.

L362: It seems highly likely that the differences are largely due to older trees having much larger root systems, this deserves stronger wording than "could be partially explained by"

R: We have added "which mean bigger and larger root systems" now Lines 376-377

L369: This explanation is confusingly stated and misses the most likely driver - that there is almost certainly higher %SOC under the tree canopy, so even when moisture conditions are suitable for microbial activity in both microsites, Rh is greater under canopy (see L44 of the intro)

R: We added "meaning higher soil organic carbon under the tree canopy", now in lines 385-386

L386: [distance correction] is clearer than "correction distance"

R: Fixed it

L387/8: arboreal individuals -> trees

R: Fixed it

L510/1: Rs = heterotrophic and autotrophic respiration, perhaps aboveground respiration and Rs together is meant (ie., Reco from the eddy covariance)

R: Thanks for the appreciation. It was a mistake. We changed the sentence. Now is corrected in lines 535-536 "when autotrophic aboveground respiration and heterotrophic soil respiration was observed together with eddy covariance"

COMMUNITY COMMENTS 1

This detailed assessment of the spatial and temporal variability in soil respiration in an irrigated olive orchard in southern Spain provides insights into the biotic and abiotic controls over this important component of the carbon cycle. Continuous fluxes were measured under trees and in the alleyways between trees for one year, and more detailed spatial measurements were taken on a campaign basis to evaluate the effects of distance from the tree trunks and directional position around the trees. The research is important because semi-arid and Mediterranean systems are underrepresented in ecosystem research but are potentially more sensitive than other systems to changes in climate (warming, drying) and land management (such as irrigation, herbicide). Moreover, the analysis of spatial variations due to the spacing of the trees allowed for detailed assessment of the contribution of vegetation versus bare ground to the fluxes, and these were scaled up for comparison to ecosystem respiration measured at a nearby eddy covariance tower. A substantial number of analyses were conducted with the large dataset and were presented clearly in the figures. The main findings of the paper were that the respiration fluxes from under the trees was about three times higher than from the alleyways, on average per m2, whereas the total contribution from under the trees to soil respiration was 39% scaled to the whole area. Unfortunately, but not unsurprisingly, the scaled-up soil respiration was substantially higher than the ecosystem respiration measured from the flux tower. The temperature sensitivity estimated as Q10 on a weekly basis from the soil respiration fluxes was higher in the alleyways than under the trees during the hotter and drier summer period but not during the wetter cool season.

R: Thanks for the comment.

The paper could benefit from considering a few questions and suggestions. The organization of the main findings could be streamlined a bit and the authors should consider whether all the figures are really necessary to support the take-home messages. My main concern is that the role of soil water content (SWC) and its regulation of microbial carbon substrate availability could be investigated in a bit more detail.

R: We appreciate the suggestions. We have reduced the number of figures by moving them to the supplementary material and restructured the introduction/discussion to make the main messages clearer.

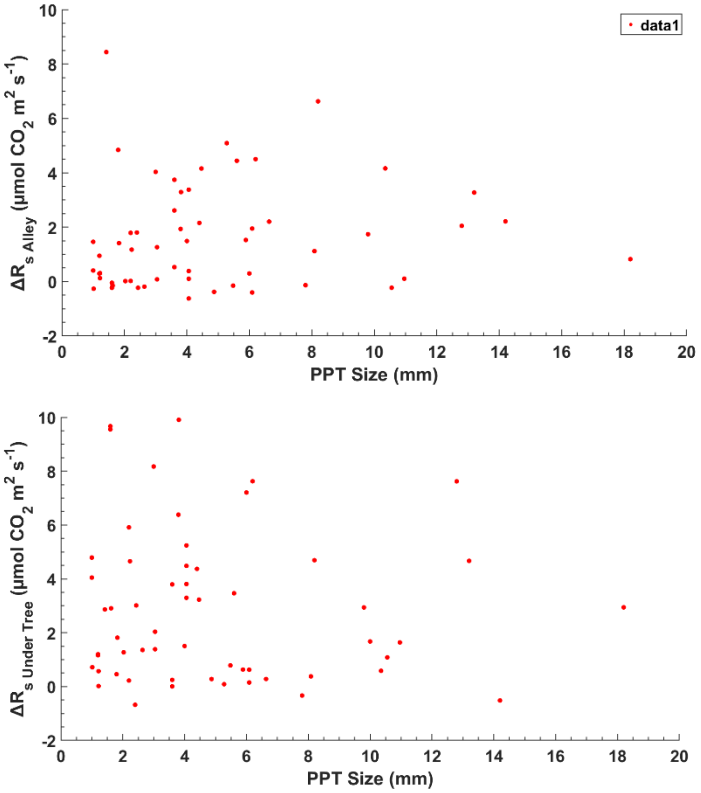
Regarding the role of soil water content and its regulation of the microbial carbon substrate, we have modified the manuscript according to the comment of the reviewer about SWC that are mentioned bellow. However, we think that we do not have enough data to analyze this issue deeply since, unfortunately, we do not have monitoring of the SOC or the microbial community during the sampling year. Furthermore, they are not treated as the main themes of the study although it is true that they are mentioned transversally. Therefore, we have reorganized the discussion and introduction to reduce the weight of the SWC topic since they are poorly detailed and can overshadow the main topic, which is the temporal and spatial variability of soil respiration.

1) It was not clear how the irrigations in summer affected SWC and Rs under the trees. Are the irrigations shown in Fig 3? If so they are hard to see. Consider changing the monthly indicator tics on the x-axis to point downwards instead of upwards.

R: Thanks so much for the appreciation. We update the figure. Now the irrigation period is indicated on the graph, and we eliminate the marks on the x axis.

2) I would expect that infrequent, large precipitation events would have a disproportionate influence over the Rs. Did you look at this? Can you do an analysis of delta Rs for the different PPT event sizes in the same way as in Fig 5a? Or plot delta-Rs versus delta-SWC by bin. You did some plots of delta-Rs versus event size in Figure 9 for a couple example weeks; why not for the full dataset? Maybe the effect of inter-event period is more important than the event size in regulating the delta-Rs across the full dataset, in which case it would be good to make that more clear.

R: Thanks for the appreciation. The size of the events was included in figure 8 (now figure 6) for the set of events. This graph shows how the effect of the period between events predominates more than the intensity of the event, at least in the alleys. In fact, we have already tried to visualize the relationship between PPT size and ΔR_s and it does not seem to have much influence.



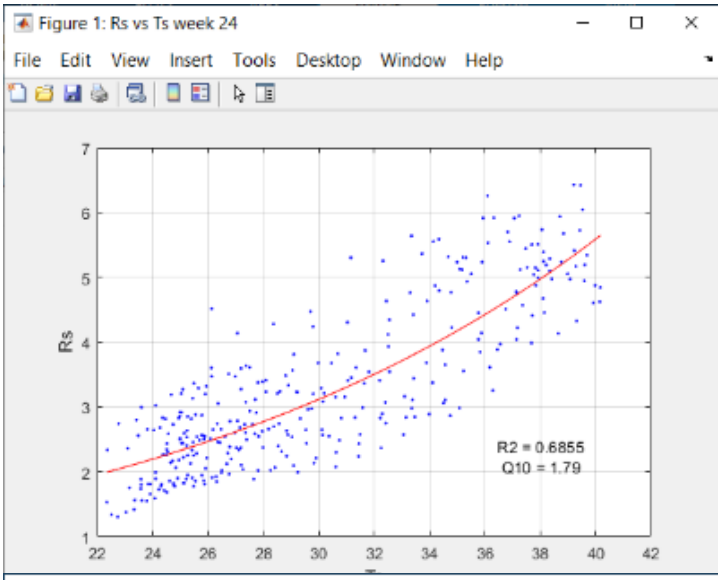
On the other hand, the relationship ΔSWC vs ΔR_s was explored, but we decided to include it transversally in Figures 9 (now figure 7) categorically in "dry" periods and "wet" episodes.

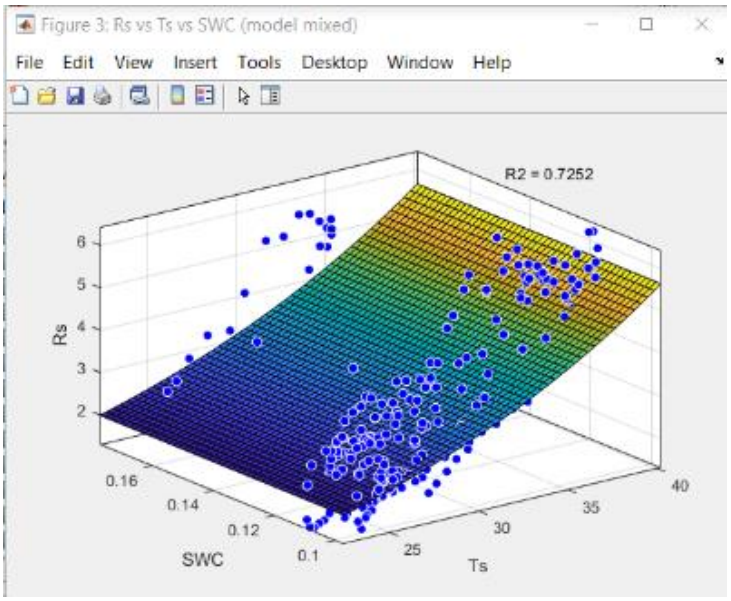
3) I don't get much out of Fig 6, consider moving it to supplement unless it is critical to one of your main findings.

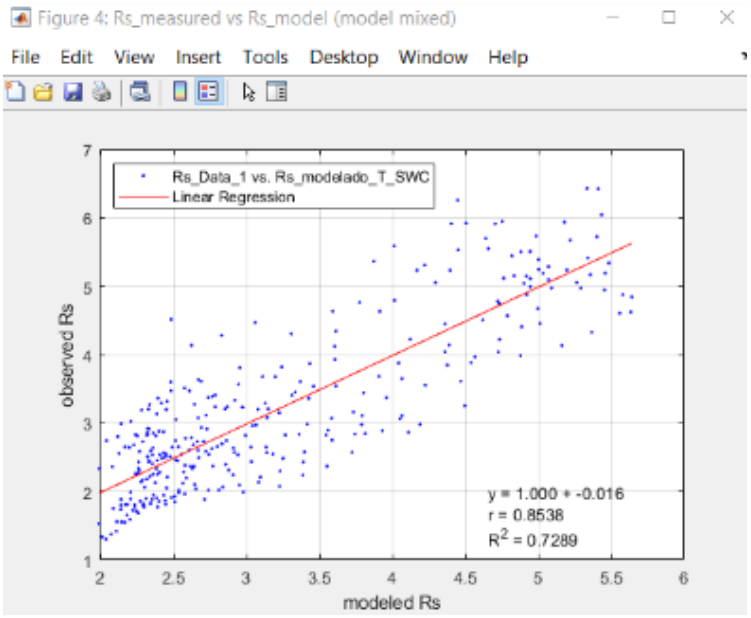
R: Thanks for the suggestion. After your observation and those of other reviewers we have realized that its presence in the main results is inconvenient, so we moved it to supplementary material (now figure S3)

4) Why did you not consider the effect of SWC on temperature sensitivity? There is a large literature on this and it seems like a missed opportunity not to incorporate an alternative analysis that would allow it.

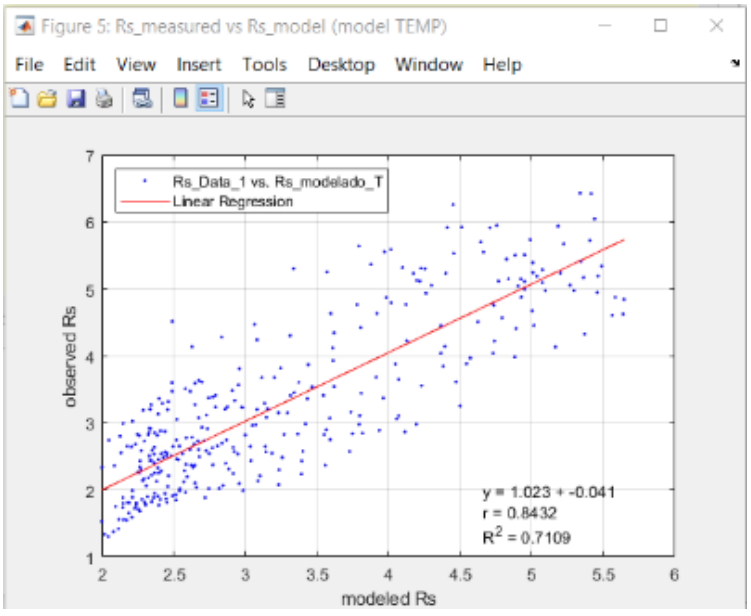
R: Actually, we already tried to include the effect of SWC for the Q_{10} calculation and the results were very similar with and without this inclusion. The following screenshot shows a random week for which we have calculated Q_{10}. Specifically, the relationship between Rs and soil temperature is shown (figure 1) and its modeling (figure 5) using $R_s = R_{ref} Q_{10}^{(T_s-10)/10}$ (model TEMP) and as well as the relationship between R_s and SWC and Temp (figure 3) and its modeling (figure 4) through $R_s = R_{ref} Q_{10}^{(T_s-10)/10} \beta^{(SWC_{OPT}-SWC)^2}$ (model mixed). As can be seen in the values of the regressions of modeled vs. actual values, including or not including the effect of SWC hardly changed the modeled values. However, we think this is an artifact of the q10 calculation time scale as we see how changes in SWC values do affect R_s (see rainfall pulses) but these occur on an hourly scale and on this scale, we run the risk of not having enough values to have a good representation to explore R_s as a function of temperature. Therefore, we finally decided to stay with only the effect of temperature and explore the role of SWC in future studies.







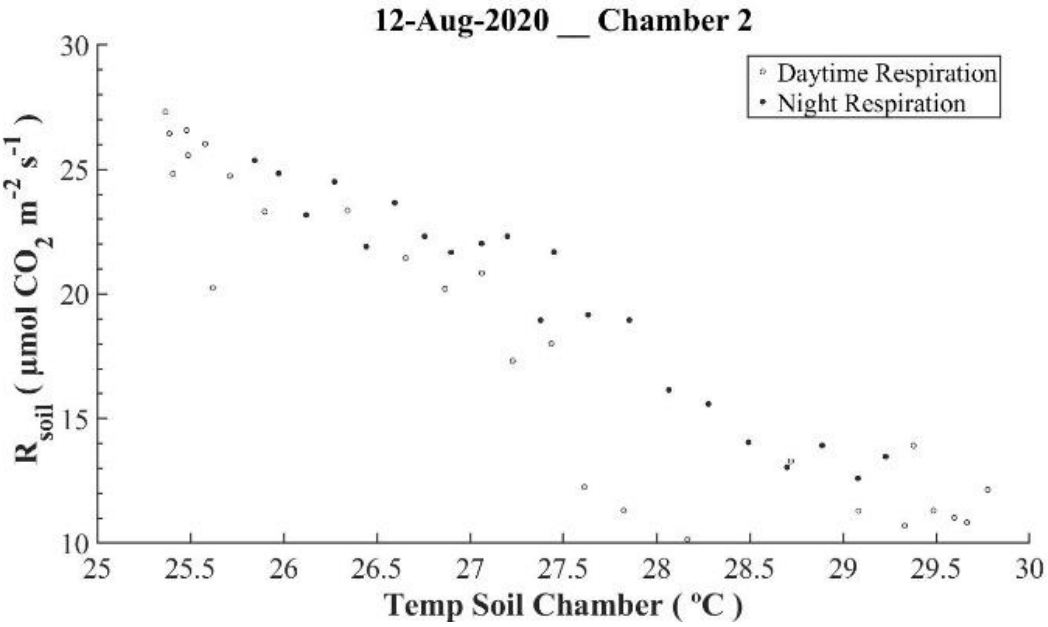
$$R_s = (R_{ref} \cdot Q_{10}^{(T_s-10)/10}) \cdot \beta^{(SWC_{OPT}-SWC)^2}$$



$$R_s = (R_{ref} \cdot Q_{10}^{(T_s-10)/10})$$

5) Related to the point above, the apparent Q10 values <1 are not biologically meaningful, so there must be an artefact. Why would Rs increase with decreasing temperature, only under the trees? Perhaps this is the result of the night-time irrigations stimulating Rs when the temperatures are lowest? Maybe the results would be different if you considered only the midday soil temperature and Rs, or filter the data for SWC such as the bins in Fig 9 (why were different SWC thresholds used for alleyways and under trees?).

Although we have not measured photosynthesis, we think that, like Tang et al. (2005) and Makita et al (2018) , a q10<1 is linked to a reduction in metabolic activity directly related to the closure of stomata at high temperatures. It is known that photosynthesis and Rsoil are related (Högberg et al., 2001). However, we think that the reduction in photosynthesis is not the only factor since, as you have suggested, we already did an exploratory analysis of how the Rs/TEMP ratio changed at night and day. As shown in the figure below (chamber 2 is a chamber under the tree that was irrigated at night) Rsoil also decreases on "warm" nights and this is a very interesting topic to discuss, but which we believe comes from the main idea of the manuscript.



Nevertheless, the idea of separating daytime and nighttime Q_{10} calculations seems very good to us, and we have taken note of it for the future exploration of this phenomenon.

Högberg, P., Nordgren, A., Buchmann, N., Taylor, A. F. S., Ekblad, A., Högberg, M. N., Nyberg, G., Ottosson-Löfvenius, M., & Read, D. J. (2001). Large-scale forest girdling shows that current photosynthesis drives soil respiration. *Nature*, *411*(6839). https://doi.org/10.1038/35081058

Tang, J., Baldocchi, D. D., & Xu, L. (2005). Tree photosynthesis modulates soil respiration on a diurnal time scale. Global Change Biology, 11(8). https://doi.org/10.1111/j.1365-2486.2005.00978.x

Makita, N., Kosugi, Y., Sakabe, A., Kanazawa, A., Ohkubo, S., & Tani, M. (2018). Seasonal and diurnal patterns of soil respiration in an evergreen coniferous forest: Evidence from six years of observation with automatic chambers. *PLoS ONE*, *13*(2). https://doi.org/10.1371/journal.pone.0192622

We have added these observations to the discussion, now on line 456-459

6) Is there any data available on soil, root or microbial carbon stocks under the trees and in the alleyways? If so these could be used to improve the discussion of the biotic regulation of the fluxes via rhizosphere processes. I can understand if the authors prefer not to speculate too much, if insufficient data is available.

R: Thanks for the observation. No, we don't have it, so we can only base the discussion of it on assumptions and the literature. In the future we will have this very complementary and valuable data. We recognize that the lack of carbon stock information is a weak point of our study.

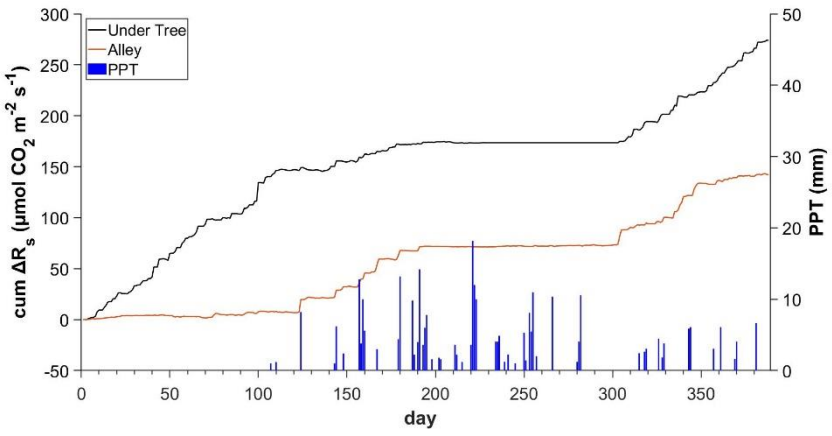
A secondary important consideration is that the Reco partitioning using the standard daytime and nightime methods does not capture these important CO2 pulse responses to precipitation events.

R: Thanks for the comment. Indeed, it is something on which we have focused the discussion since it seems very important to us to endorse the use of chambers to characterize these processes that are so common in semi-arid climates.

We have added the following sentence "The conventional daytime and nighttime methods for Reco partitioning via Eddy covariance data modeling appear not to respond to rainfall pulses" on lines 492-493 to emphasize this point.

7) What percentage of annual Rs is released during those pulses for the alleyways and under trees, and scaled to the ecosystem? I believe this could be calculated from the accumulated delta-Rs. Is this approximately similar to the magnitude of difference between scaled-Rs and Reco?

R: Great point. We have made the graphs of the accumulated delta R_s and by matching the rain pulse events, when there was an increase in R_s (> 2.5 with respect to the median of the period) we have managed to more precisely estimate the contribution of the rain pulses, being close to 15% (after weighting and applying the correction factors for scalar ecosystem) and we have now added the next "The rain pulse events implied an accumulated flux of 310 g C m^{-2} in the allyes and 110 g C m^{-2} under the tree" now in lines 304-305 in results section.



However, our analysis was done on a daily scale, and pulses can last from an hour to tens of hours. Therefore, close estimation is difficult. However, in the discussion we have

added a few words about it: “which implied that up to 18% of CO_2 emissions occurred on days with pulses of rain which implied that 15% of $R_{s,eco}$ emissions came from rain pulses” (now in the lines 496-498). Although our analysis was carried out on a daily scale and the duration of the pulse is usually less than 24 hours, around 3-4 hours, so the estimated contribution will be probably less of this 15% of the total.

8) I think it’s good that you have called the eddy flux data “modelled” Reco but it still could be viewed as evidence against the whole eddy covariance method. In the abstract and the text it would be helpful to make it really clear that the partitioning method is the issue, not necessarily the data.

R: Thanks for the comment. The Eddy covariance technique already has a multitude of corrections that have been addressed by experts for decades. Here we did not want to go into the basis of the technique but rather the “simplistic” models for semi-arid climates that are applied to EC data. We have rewritten part of the abstract (now line 13-14) and the discussion (now lines 416-419) by making it clear that the problem with the Eddy data comes from applying the partition methods and not from the measurements.

9) If there is time to do additional analyses consider using a neural network partitioning estimates of Reco, or perhaps using only actual nighttime quality-controlled data for both Rs and Reco for a more direct comparison. But that might be beyond the scope of the current work.

R: Your comment is very useful. We agree that it would provide a more effective comparison of the two methods. However, as you said it is outside the scope of this study. In fact, we are already working on a study applying machine learning techniques to better understand all the drivers of this agroecosystem and improve predictions, both at the chamber and ecosystem scales (Eddy covariance).

Specific comments:

Section 2.5 and throughout. Please clarify the language related to “rain pulse events” or “pulse events” because it is somewhat confusing. Instead consider calling them “CO2 pulses” in response to “precipitation events”.

R: We think here, the idea is that “pulse” can be interpreted as a pulse of water, or as a pulse of CO2.

In fact, although it is used often in the literature, we think the word “pulse” might be inappropriate in this context. A pulse is rhythmic. Maybe “outburst” is a better word. So, a rain event causes an outburst of soil respiration.

We agree that the terminology is unfortunate, but it is now so widespread that it makes little sense to call it something else. For that, we adapt to widespread practices and we created homogenous language always referring to it as “rain pulse events”.