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.

REPORT 1 (Anonymous referee #1 Submitted on 23 Oct 2024)

General comments

In the last round of revision, the introduction, results, and discussion were all greatly improved in the revision. The manuscript now reads easily and makes a great contribution to the characterization of Mediterranean soil respiration. I have only very minor and technical revisions to suggest.

R: We appreciate the constructive review from Reviewer 1

L.9: add a comma after "ecosystems"

R: Done

L.10: remove comma after "temporal"

R: Done

L.32: Rs should be subscripted

R: Done

L.121: remove the word "manual", not accurate

R: Done

L.155: Some discussion of the slope=0.73 is warranted

R: We have added a brief discussion on this (Now Lines 155-159). “The differences in magnitudes between the two instruments could be due to a the time lag of between 5 and 30 minutes in that comparison or due to the two different optical techniques. Such discrepancies are found even between same IRGAs model (Kutikoff et al., 2021). Nevertheless, only relative values alley/UnderTree of 7810 campaigns were used in this study”

In addition, we have reviewed the calculations, and we have observed that there is an outlier. The most correct value now is 0.78.

L.200: Equation 1 can be removed, formula is not necessary to describe subtraction

R: Done

L.241: remove "in addition"

R: Done

L.284: change "higher" to "lower", according to the results in figure 5 and line 285

R: Done

L.297-305: P values should be reported for the linear fits in figure 7b,c,d, and the significance of the relationships should be discussed.

R: Done. We have added “when SWC < 15% (pvalue < 0.01; r = 0.902), whereas, under the tree, the increase was when SWC < 20% and less intense (pvalue = 0.06; r = 0.456). The Reco (ecosystem respiration) values obtained via modeling appear not to respond to rainfall pulses (Fig. 7d), whether the soil was previously dry (pvalue = 0.78; r = -0.072) or not (pvalue = 0.59; r = 0.119)” now in lines 305-308

In general, lines 489 to 491 and lines 508-516 discuss the relationships of figure 7

L.332: change "upscale" to "upscaled"

R: Done

Figure 9: there is an errant blue bar to the bottom right of the figure

R: Fixed.

L.441: change "known, variations in" to "known that variations in"

R: Done

L.444: remove "as well"

R: Done

L.454: change word "compartments" to "locations"

R: Done

L.535: change "humidity" to "soil moisture"

R: Done

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.

REPORT 2 (Anonymous referee #3 Submitted on 27 Oct 2024)

General comments

The authors have made minor revisions to the manuscript to address reviewers' comments. It will be a nice contribution to the literature on spatial heterogeneity of soil respiration. The comparisons between soil and ecosystem respiration are also useful. It's not surprising that the model used for estimating ecosystem respiration from the eddy covariance data does not reflect the pulses of CO2 emissions following rain events. Clearly, eddy flux measurements in semiarid ecosystems with sporadic rainfall can be challenging, and continuous soil measurements such as undertaken here provide important insights into mechanisms of CO2 losses. The writing could still be improved for grammar and to reduce the word count throughout. Some details related to the figures (mainly captions) could be improved. More details below.

R: We appreciate the constructive comment from Reviewer 2

L.32, poor sentence structure, delete "Main" and … "it"

R: Done

L. 154-155, Does this slope imply that the analyzers differed? Or is it because the 7810 was used in canopy and 8100 was used in alley? Also - does this refer to the table 1? Or is the 73% a coincidence? Please make that clear.

R: The 73% of the table is a coincidence. Moreover, we have reviewed the calculations, and we have observed that there is an outlier. The correct value is now 0.78. It must be admitted that this is still a low value, but it may be due to several factors. The most important is that there may be a time lag of up to half an hour between the two measurements of both methods, which may considerably affect this comparison. On the other hand, the two instruments use different optical techniques: The Li8100 uses NDIR (Non-dispersive Infrared) while the Li7810 uses OF-CEAS (Optical Feedback – Cavity Enhanced Absorption Spectroscopy). Also, the Li8100 computed the fluxes sometimes by exponential regressions and other times by linear regressions (see section 2.2.1 Continuous Measurements) while the 7810 always used exponential regressions. However, the absolute value of the 7810 campaigns was not used, but relative values were used to quantify the gradients as well as to perform upscaling.

We have added the next sentences (Lines 155-159) that briefly references these issues: "The differences in magnitudes between the two instruments could be due to a the time lag of between 5 and 30 minutes in that comparison or due to the two different optical techniques. Such discrepancies are found even between same IRGAs model (Kutikoff et

al., 2021). Nevertheless, only relative values alley/UnderTree of 7810 campaigns were used in this study"

L. 229, Fig 2. Nice plots. Presumably the horizontal lines (at the notch) indicate the median and the black dots represent the mean? Please indicate in the caption.

R: Done. We have added this information in the figure description.

L. 274, Fig. 4. Is each panel actually one tree, with paired under-tree and alley chambers? It's a bit confusing, please consider redoing the labels of the panels, and/or explain it in the caption better. So the bars would represent the quantiles for a week of half-hourly data for each chamber? Also it would be nice if the axis scales for Rs were not so compressed.

R: Thanks. You're right. Each panel is about a tree-alley pair. The labels are confusing. We've deleted the labels "chamber1", "chamber2" and "chamber3" and rewritten the description of the figure to better fit reality as "Daily variability of Rs in the three pairs of alley/Under-Tree chambers in one week of data in July (a,b,c) and one week of data in April (d,e,f). The bars represent box plots of 30-min data during the week". Hopefully it's better understood now. (lines 278-279)

L. 276, is this spatial- temporal variability a feature of the chambers? or is it related to spatial variation among the trees or locations in the alleys? Consider relabelling Figure 4, and clarifying the text in this section.

R: Done. We have added the next sentence "Spatial variation in Rs among six chambers and the three tree/alley chamber pairs is shown by the inter-chamber variability in Figure 4." in line 269-270

L. 329, Fig. 8, I prefer the term "longitudinal" gradients to "distance" gradients as above. Please be consistent with the wording.

R: Fixed

L. 346, Please rephrase to: The under-tree area was 27% of the flux tower footprint, which implies...

R: We replace the sentence for "The soil surface classified as "under trees" represents 27% of the olive grove, which indicates that in winter half of the soil respiration of the olive grove originates in a soil that covers a quarter of the total surface of the land." Now in lines 349-351

L. 367-368, delete this sentence, it is not needed. Plus it is an exaggeration at least half of the data were gapfilled (modelled). And since you do not do a direct comparison to weekly or monthly measurements, you can't directly compare.

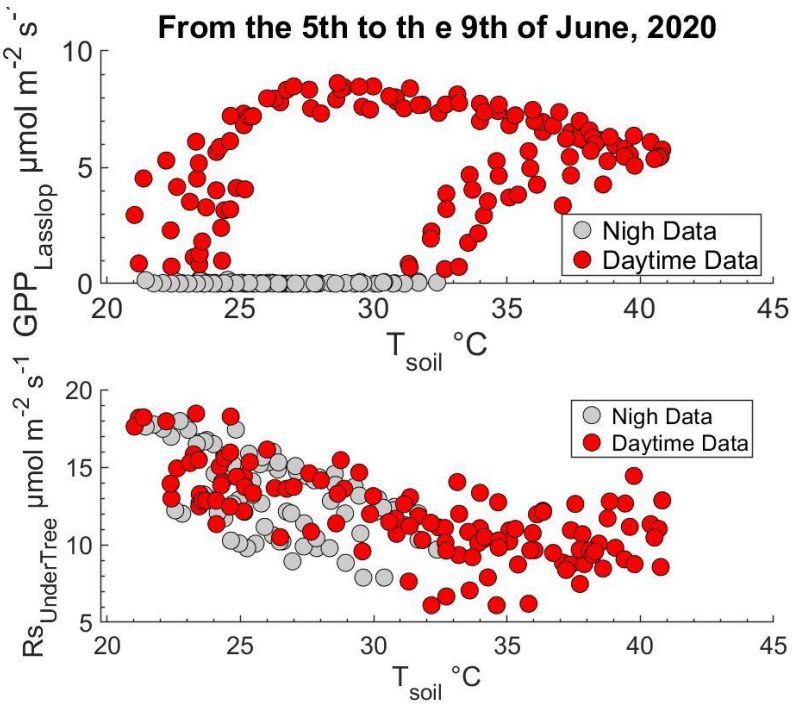
R: Done

L. 400, change “rhizosphere” to “under-tree rooting zone”

R: Done

L 431-448, I am still skeptical that the Q10 values <1 can be explained by biology (reduced photosynthates due to stomatal closure). Rather, the source of the soil respiration could be from deeper roots, and the Ts measurements are from higher up. This would lead to a time lag between respiration and temperature measurements that could be the cause of the apparent low Q10s. However, the authors have the data to test the mechanism: is GPP reduced on a similar time scale to the Q10<1? Circumstantial evidence but it would lend some credence to their conjecture.

R: It is possible that there may be a time lag between deep respiration and surface temperature, however this should also exist in the winter months, where we have Q10>1. As you say, we do indeed have data to support this discussion. Below we have plotted 4 summer days of GPP (day time based or Lasslop) relationship with soil temperature (top) and also under tree soil respiration with soil temperature (bottom). As we can see GPP also decreases with temperature in summer, when we also have a Q10 < 1.



We have therefore added the following sentence to lines 461-464 of the discussion. “Here, we have observed that GPP values also decrease with soil temperature in the summer months (data not shown). However, we also observed negative Ts and Rs ratios at night so this mechanism could have a delay of hours or there may be additional mechanisms that we cannot elucidate.”

L. 510, what is meant by "iii) the birch effect is less noticeable when observing the computation of Rz (autotroph + heterotroph)"? Please rephrase. Also be consistent with "Birch".

R: Done. We have rephrased: Now the sentence is the next (lines 514-515) "This process is more difficult to observe when soil respiration is largely autotrophic as the Birch effect primarily involves heterotrophic respiration (Birch, 1964)"

L. 530-532, This statement could be reworded into a recommendation for future research: "variation of Rs during this period is decoupled from changes in soil temperature and may be directly related to stomatal closure with high VPD and transport of newly produced photosynthates." In this example it seems like a fairly straightforward analysis to evaluate the relationship between GPP and VPD and potentially canopy conductance, to see if there is really an effect of reduced photosynthates that also reduces soil respiration. Perhaps the authors have time to do a simple test, which would be a very nice addition to the paper, highlighting the complementarity of the Rs and eddy flux measurements.

R: Thank you for your comment. This is indeed a mistake, as we conclude a causality that has not been developed in the article. We have not measured the flux of photosynthates, nor have we performed an analysis of the relationships with VPD or stomatal closure. However, we have done a brief analysis with GPP. We reformulate it based on the contribution we made previously. The sentence would be as follows (lines 537-539):

"Although the process needs to be studied in more depth, it could be related to a reduce in newly produced photosynthates given a high VPD since GPP also decreased with soil temperature in summer"