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 under-represented 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.

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.

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.
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.
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.
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.
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?).

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.

A secondary important consideration is that the Reco partitioning using the standard daytime and nighttime methods does not capture these important CO_2 pulse responses to precipitation events.

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?
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.
9) If there is time to do additional analyses consider using a neural network partitioning method that includes soil moisture (and perhaps VPD), not just temperature, in the eddy flux 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.

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 "CO_2 pulses" in response to "precipitation events".