Dear reviewers and editor:

We would like to thank you for taking the time to review our manuscript and suggest such valuable information in order to improve its quality. Even minor suggestions helped in the progress of the text's value.

A set of minor changes and typos has been corrected throughout the document. Following this introduction, you may find a detailed answer to Reviewer #1's and Reviewer #2's comments. Changes due to Reviewer #1's comments are indicated in purple in the new version, while changes due to Reviewer #2's comments are indicated in green. Changes due to comments from both reviewers are marked in red.

Reviewer #1:

Summary of the Paper

The paper titled "Disentangling the Drivers of Soil CO2 Ventilation in a Mediterranean Dryland using In Situ and Remote Sensing Techniques" by Jesús Abril-Gago et al. investigates the main atmospheric drivers controlling soil CO2 and radon (Rn) dynamics in a Mediterranean shrubland in southern Spain. The study identifies 10 periods where dilution and enriching ventilation periods can be seen.

We appreciate Reviewer #1 efforts to review the manuscript. Now we will proceed answering every comment and suggestion.

General comments

The authors provide a comprehensive view of the ventilation phenomena. The methodology is detailed and involves sensible statistical analysis. The identification of surface atmospheric pressure as a key driver is a significant contribution to understanding soil-atmosphere interactions in drylands.

The paper does not discuss the flux contribution of the phenomena to the overall surface flux. Doing so, would strengthen the discussion and connect to the authors introduction: "CO2 fluxes obtained with the EC technique have been traditionally associated with biological processes […] while abiotic geochemical and mechanical processes […] were traditionally neglected".

The paper is well-structured with clear sections on methodology, results, and discussion. The use of figures and tables support the findings effectively. The writing is clear and the arguments are well-presented.

The authors appreciate Reviewer #1's comments. We also appreciate the issue raised regarding the quantification and contribution of the CO_2 fluxes due to the ventilation mechanism. For this reason, we have addressed it and included a discussion of this topic in Section 4.4 of the manuscript. The reply and details of such topic are given in the reply to the next comment, since it focuses on this specific topic.

Technical comments

As the paper does not discuss the flux quantification for the phenomena, it is hard to grasp how important is it and if the enriching and diluting ventilations compensate each other over the time. This point should be further discussed.

We especially appreciate Reviewer #1 for this comment, since it evidences that an interesting discussion was missing from the manuscript.

We have now calculated the CO_2 fluxes for the different periods considered: enriching ventilation, diluting ventilation, precipitation pulses, and background (the rest of the dataset). The corresponding average (± standard deviation) CO_2 fluxes for these periods are present in the table below.

	Enriching	Diluting	Precipitation	Background
F^{CO2} (µmol m^{-2} s^{-1})	0.05 ± 0.11	0.30 ± 0.13	0.62 ± 0.32	0.11 ± 0.10
% of cases	4.1%	3.5%	19.3%	73.1%

Thus, significant CO_2 emissions are confirmed to happen during precipitation and after, as considered in the manuscript. Additionally, significant CO_2 emissions were recorded during the diluting ventilation, suggesting that the large CO_2 concentrations stored in the soil are ejected into the atmosphere and recorded by the eddy covariance tower. On the other hand, during the enriching ventilation, very low CO_2 fluxes were recorded, suggesting that underground accumulation of CO_2 may be happening. However, the CO_2 emitted during the diluting phase overpasses the CO_2 captured during the enriching phase. Finally, the average background CO_2 emission (non-ventilation, non-precipitation) suggests that the ecosystem is a natural source of CO_2, at least during the period considered in this study.

This discussion has been added to Section 4.4 as:

> […] boundary layer. Consistently, CO_2 fluxes remain relatively low during this phase (0.05 ± 0.11 µmol m^{-2} s^{-1}), approximately half of those observed under non-ventilation conditions, supporting the interpretation that CO_2 is being temporarily stored within the soil.
>
> […] CO_2 and Rn into the soil pores, compressing air within the soil and displacing CO_2-rich soil air to the atmosphere producing a significant CO_2 flux (0.30 ± 0.13 µmol m^{-2} s^{-1}) up to three times that observed under non-ventilation conditions.

The authors have identified surface atmospheric pressure as a key driver for the ventilation phenomena. It would add to the paper to have an annex figure which identifies periods which shifts in surface atmospheric pressure and maybe other conditions (strong turbulence) are in place but ventilation is not possible (high water content). It would be a counter example for figure 1.

The authors appreciate this suggestion by Reviewer #1. We believe this could be an interesting phenomenon to observe, since it could confirm the low soil humidity condition as necessary for soil ventilation. However, due to the relatively limited dataset, we could not observe such a case. The only period not fulfilling the soil humidity condition coincided with a period of almost

constant atmospheric pressure and low friction velocity. Nevertheless, the authors will consider this suggestion and take it into account on upcoming campaigns, aiming to sample during a more humid period (e.g. spring). Additionally, this hypothesis could be explored at other stations, equipped with similar instrumentation within international observation networks such as ICOS and ACTRIS.

Overall Assessment

Overall, the paper is clear and brings contributions to the field of soil-atmosphere interactions in drylands. The methodology is well-explained, and the findings are supported by the observations. The flux quantification not a goal for the study is a minor limitations that should be mentioned in the discussion and could be addressed in future research.

We appreciate Reviewer #1 comments and dedication to review the manuscript. We hope that the changes introduced please Reviewer #1 concern, especially regarding the lack of discussion about the quantification of the fluxes caused by the phenomena investigated.