Our replies to all comments are shown in blue and the original referee's comments are shown in black.

**RC2**: 'Comment on egusphere-2025-3542', Anonymous Referee #2, 23 Nov 2025

This paper deals with the assessment of soil erosion rates and the drivers of soil loss in Mediterranean olive groves. The paper is a review paper, gathering heterogenous data on studies of soil erosion/runoff rates in Mediterranean olive groves, considering separately different measurement methodologies, and distinguishing management practices. It is easy to read and straightforward.

It is a very useful approach, as there was so far no review giving consistent values of soil erosion rates in such environments. A very interesting focus is made on results differences between erosion/runoff rates regarding the various measurement methodologies and corresponding contrasting spatial and temporal scales. The major role of soil managmeent practices on erosion mitigation is evidenced. I have no major issue with this paper and recommend its publication providing a few minor to moderate edits are made.

We appreciate the reviewer's validation of our "scale-dependency" approach.

Line 35 : provide a figure of the total area of olive coltivated in the Mediterranean basin.

We will add a new figure displaying the distribution of olive groves in the EU Mediterranean basin (obtained from CORINE land cover)

Lines 52 to 62 : I would shorten this paragraph : too much emphasis is put on soil trunctation results, while many more methods are discussed after in the paper. I would rather highlight the large differences in erosion rates by mentioning two constrasting results. The detailed data about ST methodology is anyway presented in Table 2.

The purpose of this paragraph is to highlight the studies which has reported highest or most alarming soil loss rates, and most of them correspond to ST studies. We will try to clarify this in the text.

Lines 63-65 : Please mention that most if not all the methods mentioned are as well widely used for soil erosion studies for many other agricultural systems.

We will add this clarification to the text.

Lines 75-76: I would invert the terms 'large-scale' and 'small-scale' in the text : 'large-scale' for a local study, vs. 'small scale' for a lesser detailed study.

We refer to spatial scale of the area of study rather than degree of study detail (like in Figure 2). We will clarify this in the text

Lines 88-100: Please provide some references supporting your assumptions.

We will add references

Lines 134-138: the two sentences are repeting the issue with the normality of the residues distribution, this could be simplified and clarified.

We will simplify and clarify that sentence, combining the conditions (non-normality and low R2) into a single, cohesive statement.

Lines 153-157: I would put this paragraph in the methods section.

We will move this paragraph to the methods section

Line 160, Table 2 : For runoff plots, the erosion rate unit should be either t/ha/year or t/ha. Line 160, Table 2 : I would for the runoff plots method separate/sort the studies at the event/yearly scale for clarity purpose.

We will separate them and use t/ha/year for yearly studies, however we will use t/ha/mm for event scale studies. We adopt this normalization for event-based data because raw soil loss (t/ha) varies heavily with rainfall depth, making direct comparisons between studies uninformative.

Line 215, Table 3 : I do not understand how you calculated the average erosion rate for each method, I cannot find the same values through calculation with Table 2. Could you add a sentence or two in the methods section describing briefly how you calculated the average erosion rate for each method?

The averages in Table 3 were calculated as the arithmetic mean of the reported values from the individual studies listed in Table 2. We will add a sentence in the Methods section clarifying that "Average rates presented in Table 3 are calculated as the arithmetic mean of all independent observations collected for each methodology."

Lines 177-185 : when we look at the values of soil erosion for ST studies, there is a large difference between FRN and tree-mound measurements. How could this be explained?

These discrepancies are attributable to the sources of uncertainty outlined in lines 224-228, in particular the interpretation of soil compaction as soil loss in ST studies can explain, at least partially, the higher ST values when compared to FRN. We will revise the text to explicitly clarify how these methodological limitations explain the observed differences.

Lines 186-193 : I was wondering if the FG studies mentioned are only measuring suspended (0.45-63µm) sediments, as it is the case for many studies, or if the bedload is accounted for as well. While this should not change the explanations about sediment redeposition within the catchment, I wonder if this could not represent a non negligible part of the sediments that are transferred through the streams in Mediterranean landscapes.

The reviewer is correct that the FG studies cited in our review (e.g., Taguas et al., 2013; Gómez et al., 2014) primarily rely on automatic water samplers and turbidity probes at the catchment outlet. Consequently, the reported values represent suspended sediment yield. While bedload transport certainly occurs in these Mediterranean ephemeral streams, particularly during high-intensity events, it is technically challenging to monitor continuously and is often not captured by standard gauging setups. We agree that this likely represents an underestimation of the total sediment export, although suspended sediment is generally considered the dominant fraction in these specific fine-textured olive catchments. We will modify the text in the to explicitly state that while these values represent the majority of the export in fine-textured soils, they likely underestimate the total sediment load transferred through the stream network.

Lines 245-251: how was the reduction in soil loss/runoff calculated? This % could be added as a new column in Table 2.

The reduction percentages were derived by averaging the specific reduction values only from studies that provided paired data (i.e., direct side-by-side comparisons of CP vs. No-CP under identical conditions).

While we appreciate the suggestion to visualize this in Table 2, we have opted not to add a specific column for "% reduction." Table 2 is already quite extensive and adding another column would compromise the table's readability and fit. Furthermore, we already explicitly show value for No-CP and CP that give the reader an idea of this reduction. Moreover, not all studies listed in Table 2 provide

paired data (direct comparisons between CP and No-CP under identical conditions) that would allow for a valid line-by-line percentage calculation.

Lines 254-257 : some repetition in the two sentences, please simplify to clarify.

We will remove the last sentence

Lines 259-263 : it could rather be in the methods section.

We will move it to the methods section

Line 269, Table 4 : was the vegetation cover information provided in each study or how did the authors quantify this information ?

It was provided in some studies. We will clarify this in the text

Line 269, Table 4 : the authors do present some combinations of factors for Multiple OLS regression. Are the other combinations (e.g. clay + veg cover) not statistically significant?

We evaluated all possible combinations but we only showed the ones with statistically significant results. We will clarify this in the text

Lines 272-280, 284-285, 347-349 : since the methods section deals with the issue of log-normal transformation, I wonder if the detailed procedure is necessary to present here. Should we not directly consider the results using only log transformation?

We believe maintaining this distinction and mentioning this in the text is scientifically relevant, as it highlights that certain factors exert a linear influence on runoff or erosion, while others exhibit a non-linear relationship. However, we will try to avoid repeating details already mentioned in the Methods.

Lines 283-296, 328-338, 360-361 : it is not straightforward to present here the results of Multiple OLS regression as the title of the section is 'slope' or 'vegetation' or 'rainfall intensity'. Perhaps a specific section dealing with Multiple OLS regression would clarify the text.

We agree that the structure was confusing. We will try to create dedicated a subsection titled "3.3.X Combined drivers of erosion" to discuss the Multiple OLS results (e.g., Slope + Vegetation), separating them from the single-variable analysis or several subsections for each combination.

Line 323, Figure 3 : I do not understand the units for soil loss, as the variable was log-transformed?

The statistical test was performed on log-transformed data to satisfy assumptions, but for the figure, we plotted the original data (linear scale) and fitted an exponential curve (which corresponds to a linear relationship in log-space) to make it interpretable for the reader. We will clarify in the figure caption: "In Figure 3a, the regression model was fitted to log-transformed data, but data is plotted on a linear scale with the resulting exponential curve for interpretability and for consistency with Figure 3b"

Lines 418-422: I do not understand why these sentences are written here in the text. I think they could be removed or relocated elsewhere more appropriate in the text.

We agree, we will remove this paragraph.

Lines 426-434 : I do not agree with the equation that is presented, due to timescale issues. Indeed, the soil truncation method integrates multi-decadal erosion, providing a fairly robust average of combined water and tillage erosion rate. Runoff plots are to the best implemented over a few years. This latter method does not alllow to have a robust average of multi-decadal soil water erosion rate (considering the elusive occurrence of higly erosive events).

We agree with this valid critique. Comparing multi-decadal ST data with short-term RP data carries a significant temporal mismatch, primarily because short-term plots often miss the extreme, high-magnitude events that ST captures. However, we present this as a conceptual comparison. We will explicitly state that the discrepancy between ST and RP reflects the combined contribution of tillage erosion and the extreme events missed by short-term monitoring. However, we have also added a note that this comparison becomes quantitatively more robust in specific scenarios where timescales align—for instance, when mound measurements are taken on younger trees (e.g., <20 years) that match the duration of long-term runoff studies.

Lines 490-525 : there are ten key-takeaways, all relevant. But this is quite a lot to remember. Would there be a possibility to merge some takeaways (for example 2-3-4) to help the reader to have a clearer view of these messages?

We agree. We will merge the suggested takeaways into one.

Minor edits :

Line 83, Figure 2 : why is '137Cs studies ?' mentioned ?

Line 85 : 'spatial and temporal scaleS', 'land measurement methods', 'olivE grove'

Line 115 : remove '(microplot, plot, catchment)' to avoid repetition with lines 118-119.

Line 219 : separate as a new paragraph.

Line 219 : 'For RP studies, RP corresponds to an artificial setup where bounded plots have restricted flow interactions that can lead to...'

Line 288 : 'combined effectS'

Line 309 : 'larger areas than for RS studies'

Line 379 : 'significantly lowerS'

We will do these minor edits