The following are the main changes implemented in response to the referee comments, along with their corresponding source/line numbers:

- Further details of the bibliometric analysis were added (lines 131-147)
- A new paragraph discusses methodologies not applied in the reviewed studies, sediment fingerprinting and radiometric dating, but valuable for future research, while noting possible limitations. (lines 497-512)
- Further information on the statistical analysis (lines 182-196)
- New figures added:
    - Figure 1: Distribution of olive orchards in the European Mediterranean basin.
    - Figure 4: Geographical distribution of the reviewed studies
- Improved tables:
    - Table 2: added new columns (vegetation cover and spatial and temporal scale)
    - Table 3: No-CP and CP values are now included.
- Section "3.3 Statistical analysis of erosion drivers" has been rearranged and now is divided into two subsections:
    - 3.3.1 Ordinary Least Squares (OLS) regression
    - 3.3.2 Multiple linear regression (MLR)

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

Referee #1

The manuscript "Soil erosion in Mediterranean olive groves: a review" by Andres Peñuela et al. present the results of a review study including several studies conducted across last decades about the estimation of the entity of soil erosion processes that occur in olive groves in some Mediterranean countries. Many studies report estimation made by direct measurements, some using erosion models. The topic addressed by the manuscript is relevant in the context of soil science applied to agriculture, since soil erosion is known as one of the main soil degradation processes affecting olive groves (and other permanent crops) in the Mediterranean area. Many studies have been conducted in last decades, but the absence of a common framework for soil erosion assessment has led to heterogeneous results, as this work highlights. In this framework, the review collects results obtained from tens of studies with different evaluation approaches, with the aim to identify the extent of soil erosion in this agroecosystem and the factors influencing it, taking into account of different scales (temporal and spatial) of the problem. Data collection was performed in order to obtain a homogeneous dataset to be analysed accordingly to methods usually adopted for similar studies. Considering the broad issue, the results of the data collection and analysis are presented in a concise form, in general complete but that can be improved, and discussion help the reader to understand a clear picture on the topic. In my opinion the work is well structured and written, even if a revision of English can be beneficial. The abstract provides a concise and complete summary. Some specific comments on other sections follow:

We thank the reviewer for their positive assessment and helpful suggestions regarding the statistical presentation and data organization.

- introduction: this section provides an overview on the research about soil erosion in Mediterranean olive groves, specifically on its quantification with different methods and over different temporal and spatial scales; whereas for direct measurement methods, they were briefly introduced with some references, example of models used for soil erosion estimation are not cited in this section, thus I suggest to add some references to most used models, beyond RUSLE. In fact, as a review, some readers could not be aware of models for this purpose. See some other details in the attached pdf.

We added references to other models commonly used in the introduction to provide a broader context of the modelling landscape (lines 107-109).

- Methods: data collection: vegetation cover is not mentioned among collected variables, but it is used in the analysis. Please add it. Statistical analysis: I suggest to add here details about test performed to check assumptions (now they are named in results)

We updated the table 2 section to explicitly list "vegetation cover (%)" as a collected variable. We added further details about the diagnostic tests in the Methods section (lines 181-189 and 192-195).

- Results: Table 2: I suggest to add the variable vegetation cover and indicate the plot's dimension. Section 3.2: since they are mentioned, I suggest add in the Table 3 results for CP and No-CP.

Table 2: We added columns for vegetation cover range and plot size range where available.

Table 3: We added "No-CP" (No Conservation Practices) and "CP" (Conservation Practices).

Section 3.3: I suggest not to indicate results for model that are not definitive, thus only discuss log-transformed model's results id assumptions are not satisfied.

We agree, we have removed the detailed statistics for the non-significant OLS models in the results section.

Section 3.3.2: no information is given about variable vegetation cover in the presented dataset: is it vegetation cover only between tree's rows or also under trees? What method is used to measure it?

We have now clarified in the Methods section that "vegetation cover" refers to the percentage of ground covered by herbaceous vegetation or cover crops in the inter-row areas. (line 151-152)

Section 3.4: since this section does not present results, I suggest to consider it as discussion.

We clarify that Section 3 is explicitly titled 'Results and Discussion'. Therefore, subsection 3.4 ('Final thoughts and future challenges') is intentionally positioned here to serve as the synthesis component of this combined section

Please see minor comments in the attached pdf, also in conclusion sections.

In my opinion the requested changes are of minor significance with respect to the relevance of the manuscript, even if I suggest the authors to consider my suggestions. Finally, I suggest to accept the paper with request of minor revisions.

Referee #2

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 management 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 thank the reviewer for their positive assessment and constructive comments.

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

We added a new figure (Figure 1) 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 truncation results, while many more methods are discussed after in the paper. I would rather highlight the large differences in erosion rates by mentioning two contrasting 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 have further clarified this in the text (lines 64-69).

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 added this clarification to the text (lines 76-78)

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 further clarified this in the text (line 88-89)

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

We added references (lines 113-114)

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

We removed the first sentence.

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

We moved this paragraph to the methods section (lines 161-165)

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 clarified this in the Table 2 caption. We also separated the event scale studies and the yearly scale ones.

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 added a clarifying sentence (lines 225-226)

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 partly attributable to the sources of uncertainty such as the interpretation of soil compaction as soil loss in ST studies and hence, the overestimation of soil loss rates. We also identified an additional source of uncertainty in tree mound measurement studies, the lack of methodological consensus regarding the identification of the original soil surface. We revised the text to explicitly clarify how these limitations may partially explain the observed differences (lines 290-302).

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. 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 clarified this in the text (lines 254-257).

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). We clarified this in the text (lines 316-317)

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 removed the last sentence

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

We moved it to the methods section (lines 177-181)

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 showed this in Table 2

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 clarified this in the text (lines 399-400)

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 avoided 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 rearranged section 3.3 to create dedicated subsections: 3.3.1 Ordinary Least Squares (OLS) regression and 3.3.2. Multiple linear regression (MLR)

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 clarified this 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 380-382)

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 removed 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 now 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 526-530).

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 merged the suggested takeaways into one (lines 594-599).

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 did these minor edits

Editor

EC1: 'Comment on egusphere-2025-3542', Olivier Evrard, 24 Nov 2025

This review manuscript provides a synthesis on data published regarding soil erosion in Mediterranean olive groves. Overall, the text is rather concise, well written and of interest to the international research community.

We thank the Editor for these suggestions to improve the paper's reach and context.

In my opinion, the database compiled by the authors should be made accessible in open access (I apologize if it is; then it means that I missed the link).

We uploaded the dataset on Zenodo, we have included the DOI in Data availability

The manuscript would also gain from further contextualization (i.e., about what is considered as 'Mediterranean' whether the climate or the region is investigated; about the surface covered by olive groves in the area under investigation; about the implementation of conservation techniques under olive groves – e.g. are there agricultural statistics available?)

We agree that "Mediterranean" can be ambiguous. We define the study scope as regions with a Mediterranean climate (Köppen Csa/Csb), primarily focusing on the Mediterranean basin where >95% of olive production occurs (lines 132-133).

We agree that providing data on the adoption rates of conservation practices adds crucial context. While harmonized statistics for the entire Mediterranean basin are scarce, national data from major producers like Spain and Italy indicate that adoption remains low despite the known benefits. We added a sentence in the Introduction citing recent agricultural statistics (from the Spanish Ministry of Agriculture). We explicitly state that while conservation agriculture is promoted, over 50% of the olive growing area in Spain does not have a vegetation cover (lines 44-49).

It would also gain from a general bibliometric analysis: what was searched for in the literature? Using which tools and relying on which queries? General information about the study locations (a map would be useful), the journals in which data was published, etc. would also be useful.

We added a map showing the geographical distribution of the reviewed studies (figure 4).

We added a new paragraph explaining the bibliometric analysis (lines 131-147).

Detailed comments

LL.53-60: could these erosion rates be converted into equivalent soil depth loss by erosion to further evaluate whether they are meaningful?

We have opted to retain mass-based units rather than converting to depth (mm) to avoid misinterpretation. Mediterranean erosion is often driven by concentrated flow (rills/gullies), making an "average depth" physically misleading. Furthermore, conversion introduces uncertainty regarding bulk density, and retaining mass units ensures direct comparability with the European tolerable soil loss thresholds cited in the manuscript.

LL.92-100 this text is interesting but does it rely on any reference?

We added the following references (lines 113-114):

de Vente, J., Poesen, J., 2005. Predicting soil erosion and sediment yield at the basin scale: Scale issues and semi-quantitative models. Earth-Science Reviews 71, 95-125. https://doi.org/10.1016/j.earscirev.2005.02.002

Boix-Fayos, C., Martínez-Mena, M., Arnau-Rosalén, E., Calvo-Cases, A., Castillo, V., Albaladejo, J., 2006. Measuring soil erosion by field plots: Understanding the sources of variation. Earth-Science Reviews 78, 267-285 https://doi.org/10.1016/j.earscirev.2006.05.005

L.145 a map of all study sites would be helpful (maybe all regions of Spain are not equally covered?) – a general bibliometric evaluation would be useful as well.

We added a map showing the geographical distribution of the reviewed studies (figure 4).

We added a new paragraph explaining the bibliometric analysis (lines 131-147).

Table 2; is it all data available from the literature or did you make a selection? If so, based on which criteria?

The dataset derives from a systematic review of 1,385 records retrieved from Scopus and CAB Abstracts. Selection was not arbitrary but followed a rigorous screening protocol based on some key criteria such as: studies reporting specific erosion rates rather than qualitative descriptions and primary field or modelling research conducted in Mediterranean olive orchards.

As mentioned above we added an explanation to the revised manuscript.

L.176 about ‘tolerable soil loss’: maybe this should be further discussed in your manuscript (in the final thoughts?) given you are making strong suggestions to go further towards sustainability.

We agree that the concept of tolerable soil loss is the necessary quantitative benchmark for defining "sustainability." Without this reference point, terms like "high" or "severe" erosion remain subjective.

We highlighted that even the "non-alarmist" average gross soil loss rates measured in runoff plots (5.5 t/ha/y) exceed the upper limit of tolerable loss by nearly 400% (lines 554-556). This quantitative comparison provides the necessary justification for our strong recommendations: conservation practices are not merely an optional improvement but a fundamental requirement to prevent irreversible soil depletion.

L.195… is there always a validation in the modelling studies compiled? If not, what proportion of studies do include a validation? Are there studies simulating scenarios before having conducted such a validation beforehand?

We added a paragraph on the % of modelling studies that performed calibration or validation (lines 204-214)

Table 3: I would add the number of studies related to each method in the table; I guess some general information about the spatial scale/temporal scale under investigation should also be calculated and added?

We added additional information about spatial scale and temporal scale in Table 3.

L.230 ‘heighten uncertainty’: please rephrase

We rephrased it (lines 300-302)

L.245… the associated data would be super helpful

We uploaded the data into Zenodo as mentioned above

Section 3.3.2 and beyond about the ‘vegetation cover’: what is considered by the authors (the main trees, their canopy, understory vegetation, both the trees and understory vegetation?)

Percentage of ground covered by herbaceous vegetation or cover crops in the inter-row areas. We clarified this in the text (lines 152-153)

L.308 is ‘compelling’ the right term to use here?

We rephrased the sentence: The results from runoff plot (RP) studies provide further evidence of this protective effect (lines 360).

L.349 I guess that this sentence is unclear and should be rephrased.

We simplified this paragraph (lines 388-390)

L.354 is ‘dislodges’ the right term here?

We will rephrase this sentence: Higher intensity rainfall has significantly more kinetic energy, resulting in the detachment of a greater volume of soil particles, a process known as splash erosion. (391-393)

LL.360-361 this sentence is somewhat disconnected from the main text.

We removed this sentence

L.372 low in organic matter >> depleted in organic matter?

We replaced it

L.388 I would avoid the use of terms such as “seemingly”

We removed it

L.401 I guess that these studies do not miss these events, they integrate and cumulate all of them, which is different.

This sentence is confusing. We wanted to explain that gully erosion is not captured by ST. We corrected it (468-469).

L.408 ‘before it can exit the catchment’ >> before it is exported from the catchment?

We corrected it

L.416 “models, when properly calibrated…” >> do you have this important information from your database?

We included information about the % of studies where models where calibrated (lines 204-214)

L.416 another technique that may connect all the scales would be the use of sediment fingerprinting; is it something that you think may be useful/encouraged?

Thank you for the suggestion. Sediment fingerprinting could indeed offer a potential way to connect processes across different scales and to identify sediment sources within a catchment. However, sediment fingerprinting primarily yields estimates of relative sediment source contributions rather than absolute soil loss rates; when combined with radiometric dating of depositional archives, it has the potential to link erosional processes across different spatial and temporal scales.

Our main concern is its suitability for long-term assessments in landscapes affected by active gully erosion, particularly in Mediterranean regions where sediment deposits in water bodies are often limited due to the dry climate. Although footslopes or sediment fans might serve as alternative sediment archives, they are susceptible to re-erosion by gullies or tillage.

Moreover, when fallout radionuclides (FRNs) are used, gully erosion can introduce subsoil material with little or no FRN activity. The mixing of FRN-poor subsoil with FRN-rich surface soil in depositional areas may lead to misleading interpretations of soil loss rates, sediment sources and transport processes.

We have added a new discussion paragraph on this matter (497-512)

LL.426-433 I don’t really understand this suggestion of study ‘in areas close to the catchment limit’

We mean upslope areas, where contributing areas are small and deposition minimal. We clarified this in the text. (516-519)

L.440 is data available on olive yields to support this statement?

We added a reference to support the statement (536-537).

Infante-Amate, J., González de Molina, M., Vanwalleghem, T., Soto Fernández, D., Gómez, J.A., 2013. Erosion in the Mediterranean: The Case of Olive Groves in the South of Spain (1752–2000). Environmental History 18 (2), 360–385. https://doi.org/10.1093/envhis/emt001

LL.460-485 instead of incentives only, we may also think about another political approach, which is the one of ‘cross-compliance’: if farmers do not implement good practices, they are not eligible to public support. This section could also benefit from relying more on data from your compilation.

This is a very relevant policy instrument, especially in the EU context (CAP). We added mentioned the "cross-compliance mechanisms" as a critical lever for enforcing minimum soil cover requirements. (577-583)

Conclusions

L.487 ‘diverse body of research’: maybe this could be further contextualized in the text

We rephrased the sentence: This literature review has synthesized a broad spectrum of research spanning from plot-scale field experiments to catchment-level monitoring and long-term soil truncation estimates. (589-591)

L.504 I do not think that this statement on connectivity is supported by data provided/compiled in this research, is it?

We agree, we removed this from the conclusions

L.513 see my comment above on which is considered as ‘vegetation cover’

We explained this, as mentioned above

LL.526-530 this is interesting but it reads as an advocacy, do you think that it is appropriate?

We have softened the language in the "Final Thoughts" section. Instead of "The path... is clear... must be driven by," we now write, "Current evidence suggests that shifting towards permanent ground cover is a viable strategy for sustainability," focusing on the scientific conclusions rather than prescriptive advocacy.