

Interactive comment on “Spatial and temporal evaluation of erosion with RUSLE: a case study in an olive orchard microcatchment in Spain” by E. V. Taguas et al.

Anonymous Referee #1

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The manuscript describes a case study to validate RUSLE model in a Mediterranean catchment using ground survey. The subject matter is of great interest to readers of the SE. However, I believe that this manuscript has problems with proper methodology and needs a substantial revisions and additional review.

General comments:

One of the biggest concern with the paper is that the accuracy of the GPS measurements do not allow for a proper comparison with RUSLE estimated data. It was reported that an elevation difference of 4.0 cm can be measured with 80% confidence. Annual erosion as estimated by RUSLE was 1.5 and 3.2 t/ha/yr for the period 2004–5

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and 2005–6 respectively. This translates into elevation change of 0.1 and 0.2 mm. Even maximum calculated sediment yield of 22.1 t/ha for some cells is a 1.5 mm change in depth. This is well within surveying error. Quantitative survey results were not reported. Fig. 3 shows points of “deposition” and “erosion” and I don’t understand how this was compared to RUSLE estimates. If a point shows “deposition” does it mean that an elevation change of 4 cm (600 t/ha/yr) was recorded? How does it compare to a value of 1.5 t/ha/yr calculated for this cell by RUSLE? In the abstract and elsewhere in the manuscript it is stated that erosion points (surveyed) located in certain areas correlate very closely with RUSLE predictions. I don’t see how such a conclusion could have been reached.

Page 288, line 23. I can imagine a 4 cm threshold being sufficient to detect larger rills, but it is too large for 1 year of splash or sheet erosion. Additionally, do you think regular spacing of measured elevation points combined with regularly spaced plants could introduce a systematic error?

Why did LS factor varied so much between 2004-5 (LS=0.32) and 2005-6 (LS=0.17) as reported in Table 1? I realize it was calculated using consecutive surveys, however, an elevation change of few millimeters or centimeters could not have resulted in 50% LS decrease! Was the same routine used to calculate both?

The authors fit various distribution functions for annual erosivities and report accompanying statistics (Section 2.5.2 and Table 4), but fail to interpret the results or draw clear conclusions. Statistics D, K, or W varied, but what does it all mean? What point did you try to convey to the reader?

Specific comments:

Check figure and equation numbering throughout the text. There is a lot of miss references.

Page 282, line 12. Equation 3 to which you refer defines slope length factor, not EI.

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Page 283, line 2. Shouldn't this be 49-year record instead of 14 (1950-1999)? The whole paragraph is confusing and it is not clear what method was used to fill in the missing rainfall data.

Page 286, line 14. "Figure 2 shows the distribution of erosion and deposition" – no, it shows location of control points. In fact, survey measured soil loss is not reported.

Page 288, line 16. "Amore at al. (2004) also concluded that different experimental conditions (plot or field areas), which were originally used to develop models such as WEPP and USLE, were suitable for estimating the eroded soils." – this is poorly phrased and does not convey the conclusion of the paper cited.

Equation 5. What is P? Precipitation? You've already used P to denote support practice in Eq. 2.

Tables 1 and 2 repeat a lot of the same information. I suggest combining them into one.

Table 2. Shouldn't RUSLE estimated erosion and deposition points have opposite signs (erosion "-").

Table 3. Mean drainage area for the entire study area is 0.0 ha, erosion points 0.1 ha and deposition points 0.1 ha. What is the meaning of this?

Figure 4 and accompanying explanation in the text is confusing. The diagram in the lower left corner is redundant, it is the same as one in the upper left corner.

Figure 5c. This diagram displays soil accumulation. Either call it deposition rates or use erosion rates with negative values.

Technical corrections:

Page 280, line 4. Survey precision reported as "1 cm +/- 2ppm". Linear error measured in ppm?

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Page 284, line 3. Reference to Eq. 3 as rainfall erosivity equation. Should be Eq. 5?

Page 284, line 21. Should this be Equation 7 instead of 5? Check equations throughout the text, there seem to be a lot of miss references.

Equation 1. Formatting issue with square root sign.

Table 3. In the second column there should be "Dv" instead of "Dt".

Labels and text on the figures are too small. If this is printed it will be impossible to see.

Figure 4. Both first and second rows of diagrams labeled as "a".

Interactive comment on Solid Earth Discuss., 2, 275, 2010.

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