Solid Earth Discuss., 2, C112–C119, 2010
www.solid-earth-discuss.net/2/C112/2010/

Solid Earth Discussions

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.

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Received and published: 24 October 2010

Dear Reviewer,

Thank you for the time you have devoted to reading this manuscript and for your helpful comments. We have taken all your remarks and suggestions into account when improving our manuscript.

Here are our answers to your 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 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.

We are aware that the surveying error does not allow us to carry out a quantitative analysis; however, the qualitative study, in which the extreme values of the hystograms of erosion and deposition points (points where the differences of height < -4cm and > +4cm, respectively) have been located, and where we can be certain (to a level of confidence > 84%) about the occurrence of erosion/deposition phenomena, allows us to evaluate whether the results of RUSLE obtained in the catchment are useful in identifying areas with the highest rates of erosion (or deposition).

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?

Not exactly - we only have ponctual observations, where an elevation change of 4 cm is equivalent at that point to 6 g.cm-2.year-1, which is a reasonable value if we consider the size of the rills (Fig. 1). We agree that it does not make sense to extrapolate these results in quantitative terms due to the limitations of the survey; however, we do have a fine sample formed by the extreme points, with which we can discuss the RUSLE predictions and describe their features.

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.

The correlation explains the degree of dependence between two variables. We have observed that the points with the highest risk of erosion were located where RUSLE estimations showed the largest erosion intervals. This can be checked from the statistics of Table 2 and the distribution histogram in Figure 5. What we wanted to do was to highlight that most of the extreme erosion points could be identified through the highest intervals from RUSLE calculations.

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.

Our objective was not to evaluate the different types of erosion; we used this threshold to evaluate the final soil losses. In fact, although the predominant phenomenon is rill erosion, rill and interrill erosion can, in fact, occur simultaneously.

Additionally, do you think regular spacing of measured elevation points combined with regularly spaced plants could introduce a systematic error?

We selected a cell size of 10 m to prevent systematic errors related to the spacing of 7 x 7 m between olive trees. The length chosen (10 m) allowed us to take the samples at different locations in the lanes.

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?

Our apologies. There is a mistake in both Table 1 and Table 2: the mean LS value corresponding to the year 2004-05 is equal to 0.24 (the other statistics and calculations are correct). We applied the same approaches and routines for the results from both surveys.

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?

We checked the adjustment of the erosivity quantiles with two type of functions: Gumbel and Pearson type III. The probability or frequency values used for the fits were calculated through the equations proposed by Weibull (W) and Greengorten (G). The comparison of the discordance value (D) and the statistic K, according to Kolgorov-Smirnoff's test, with a confidence level equal to 95%, determined whether the distribution of erosivity values-frequency was well-adjusted to Gumbel′s and/or Pearson type III functions (Table 4). Thus, if the D values are lower than K (0.349), as can be observed in Table 4, both functions would reproduce the erosivity-frequency relationships appropriately . When D<K, the best adjustment is determined by the lowest root square mean error (RMSE) and the highest correlation coefficient between observed and predicted erosivity values (R).

Specific comments: Check figure and equation numbering throughout the text.

Our apologies: the numbering of equations and figures has been corrected.

There is a lot of miss references.

Our apologies: we have now corrected the references.

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

Our apologies: the numbering of the equations has been corrected.

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.

The daily rainfall record for Setenil (5° 10 ′57″ W, 36° 51′ 51″ N; National Meteorological Institute, series 1950-1999) provided only 8 complete years of these 49 years. Thus, an additional analysis was carried out to check whether years with missing July and August records could be included, since for the summer season in this area, rainfall depth is usually very low. Our approach is based on the fact that events with rainfall depth below 10 mm are usually excluded from the calculation of annual erosivity. In this way, all available July and August rainfall data was analyzed. It was found that only one rainfall event greater than 10 mm occurred in 13.3% of the months of July in the data series. In August, two events with a rainfall depth of over 10 mm occurred in 6.9% of these years and at least one event over 10 mm occurred in 37.9% of the months of August. This analysis justified the inclusion of years with missing July and August records since the missing information was acceptable. In this way, we were able to use a 14-year record from the Setenil station.

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.

We agree with you, and so we have modified the sentence: “Figure 2 shows the distribution of erosion and deposition points”

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.

We have modified the text thus, in order to improve the phrasing: Amore et al. (2004) concluded that the USLE model was not sensitive to the size of the hillslope area and suggested that fine sub-divisions, even though better approximating the experimental conditions (plot or field areas) originally used to develop the models, was not necessarily needed for a better estimate of eroded soil.

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

Our apologies: we have modified the term “P” to “Pd” for daily precipitation.

Tables 1 and 2 repeat a lot of the same information. I suggest combining them into one. We consider that is more suitable to keep them separate.

Table 2. Shouldn't RUSLE estimated erosion and deposition points have opposite signs (erosion "-"). Here, we have followed the reviewer's suggestion.

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?

We have tried to describe the main features of erosion and deposition points through the comparison with the characteristics of the study area. High drainage area values were observed in the cells corresponding to the erosion points (mean drainage area = 0.07 ha) and the deposition points (mean drainage area = 0.09 ha), which justify the differences between the average values estimated in the catchment (mean drainage area = 0.02 ha).

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.

Yes, but we included it to clarify the comparison on the same scale of the saturated hydraulic conductivity (Ksat) histograms corresponding to the study area versus the erosion points (Fig. 4a) and to the study area versus the deposition points (Fig. 4c). We think this distribution makes it easier to compare them.

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

The histograms correspond to the distribution of RUSLE-values (rates of erosion) calculated for the study area (a), for the erosion points (b) and for the deposition points (c). We have clarified the diagram information in the caption for Figure 5 and at the end of Chapter 3.1.

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

ppm means 10-6. Both planimetric and altimetric precision are expressed as root square mean error (RSME), with a level of confidence of about 67%. We found 1 cm + 2 x 10-6 cm and 2 cm + 2 x 10-6 cm.

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

Our apologies: the numbering of the equations has been corrected.

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.

Our apologies: the numbering of the equations has been corrected.

Equation 1. Formatting issue with square root sign.

We have changed the sign.

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

Our apologies: this has been corrected.

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

We have taken your recommendation into consideration.

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

Our apologies: this has been corrected.

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Fig. 1. Fig 1. View of the hillslopes after storms, with details of rill-erosion and deposition (March, 2006)

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