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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.

E. V. Taguas et al.

evtaguas@uco.es

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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:

Specific comments: The objectives, especially objective (1) are not clearly exposed. It is not clear for the reader whether the natural spatial variability of soil erosion rate

will be estimated using (R)USLE as a tool for that or the performance of this model will be instead evaluated using GPS measurements. In addition, the argumentative line of the introduction is a little confusing and then it somewhat fails to successfully lead to the reader to the objectives. In short, the objectives are not clearly deduced from the introduction.

Our original introduction presented the following structure: 1) Importance of soil erosion studies in Spain, and in particular, under olive orchard land-use. 2) Previous studies of soil losses in olive orchard land-use on the plot scale and new areas of interest derived from the microcatchment scale. 3) The presentation of USLE (or its development, RUSLE) as the commonest tool in Spain for predicting the risk of potential erosion (in spite of the disadvantages, such as the fact that it was designed for use on the plot scale). 4) The use of GPS surveys for evaluating the morphological changes of the topography as well as erosion/deposition dynamics. 5) The interest of long term studies of soil losses for supporting environmental decisions.

We agree that the relationship of this sequence of ideas with the first objective -where we had also mentioned the use of other experimental measurements (rainfall, runoff and sediment at the catchment outlet) for quantifying the sediment delivery ratio in the catchment and to value the results of the application of RUSLE- could be considered inappropriate. As a result, we have modified the organisation of the introduction and objective 1:

“Therefore, the overall objectives of this study were: (1) to examine the spatial variation of the RUSLE predictions in an olive orchard microcatchment using topographical GPS measurements; (2) to evaluate the long-term soil losses in the catchment after the model performance was examined by exploring a simple method based on the quantile estimation of erosion.”

As the authors pointed out in the Introduction, the GPS is a useful tool to monitor noticeable changes in the terrain as it is the case for gully erosion and similar. However

-and if I understand well- the uncertainty of the GPS height measurements is around 4 cm. Then how can be possible to detect changes of few millimeters in the soil surface level by GPS as it is the case in the present study where very small soil loss rates (e.g. 1.5 Tn/ha) are reported.

We have evaluated the limitations of the equipment we used (Leyca 1200 GPS system) whose altimetric precision is equal to 2 cm + 2.10-6 cm, expressed as the root square mean error (RSME). The theory of error distribution assigns a probability of 67% to the RMSE, which means that the altimetric error is less than 2 cm in 67 of 100 measurements. In some studies, these type of considerations are not taken into account, but this restriction is important when studying a fine sample formed by points, where you are confident enough (extreme values of the histogram) to discuss the RUSLE predictions and describe their features.

"The RUSLE values at points where soil loss and deposition are evident processes – elevation differences of <-4.0 cm in the case of erosion and elevation differences of >+4.0 cm in the case of deposition – were checked to evaluate the model results" (page. 284). It could be argued to what extent sedimentation areas are more 'evident' than erosion ones (especially interrill erosion areas) since sediment normally are deposited in a relative (much) more reduced area than that the sediments come from (i.e. erosion or sediment source areas). In short, are the erosion areas to some extent underestimated?

Our analysis is not suitable for evaluating the size of the erosion and deposition areas, since we simply set a threshold of 4 cm through which we identified a fine sample formed by the points, giving us a high enough level of confidence to compare them with the RUSLE predictions.

What is the uncertainty or error introduce in the dataset as a consequence of using AnnAGNPS output ? Is it is really necessary to replace missing data with those provided by AnnAGNPS?

We used the sediment loads measured in the catchment together with the mean value of erosion estimated by RUSLE to calculate a rough value for the sediment delivery ratio. Since the period of measurements was interrupted for 83 days, the AnnAGNPS model (Bingner and Theurer, 2003) was calibrated using 22 events with a root square mean error equal to 87.2 kg/ha (Taguas et al., 2009). It was necessary to complete the data series since substantial events were not recorded.

The first paragraph of section 2.3.2 where the determination of the erosivity index is treated is much confusing. By the way, although the proposed Ed index showed a good correlation with the RUSLE-EI, why don't use directly this EI index since this is indeed the proper erosivity index defined and proposed by the RUSLE, the model that, precisely, the authors are dealing with.

The reason we used Domínguez-Romero's EI for Cadiz instead of RUSLE' EI is because the rainfall measurement in the catchment area did not start until April 2005 and the only available daily rainfall data, from the nearest meteorological station in Olvera (5° 15 ′31″ W, 36° 55′ 59″ N, DGAP – Junta de Andalucía), were complete and available for the study period (September 2004 and September 2005).

We have revised this paragraph as follows: Erosivity for the period September 2004-September 2006, corresponding to the analysis interval of the topography, was calculated using the relationships estimated by Domínguez-Romero et al. (2007) for daily erosivity (Ed) in Cadiz (Eq. 3). Since the rainfall measurement in the catchment area did not start until April 2005, the only available daily rainfall data, from the nearest meteorological station in Olvera (5° 15 ′31″ W, 36° 55′ 59″ N, DGAP – Junta de Andalucía), were complete for the whole study period. The rainfall from the catchment and the meteorological station in Olvera were well-correlated (r = 0.86).

"...deposition points were concentrated next to the outlet: where LS-factor is higher" (pag. 286, line 23). Is it not strange that deposition occurs under topographic conditions that normally favour erosion rather than sedimentation?

LS-factor responds to both hillslope length and slope, so very great lengths combined with low slope values (and/or high saturated hydraulic conductivity values) can favour deposition. Other topographical factors, such as curvature (profile/plan), could also be involved.

The authors pointed out that deposition points were situated in areas with higher values of saturated hydraulic conductivity. It is very difficult to see the connection or the cause -effect relationship between deposition and saturated hydraulic conductivity. Maybe the higher local hydraulic conductivity is not a cause but and consequence of the deposition. I mean, as a consequence of the deposition of coarse material, the hydraulic conductivity values of the top soil -as determined in the lab using small cylinders- are larger than the corresponding values out of the deposition area.

We agree with you. However, we took the measurements of saturated hydraulic conductivity during July and August 2004, before both GPS surveys (September 2004 and September 2005). So, these higher conductivity values should be considered as a "cause". Anyway, we consider that is acceptable because in areas with larger rates of infiltration, the flow was potentially lower or slower and the deposition of sediments was favoured.

"Fleskens and Stroosnijder (2007) remarked that the low frequency of intense rainfall events determines annual erosion. However [sic], in Andalusia, the mean annual rainfall values vary from 200 to 2000mm (CMA, 2009) and mean annual erosivity varies from less than 50 to 10 000 MJ.mm.ha-1 h-1 (CMA, 2009). Therefore, the use of average climatic values for analyzing soil erosion is debatable" (pag. 289, line 20) Why "However", I cannot see the contradiction between Fleskens and Stroosnijder's claim with that of the authors. On the contrary, I understand that Fleskens and Stroosnijder state the same idea: just few events (low frequency) control erosion rate and then erosion rate would be not properly reflected through average values.

Originally, we used the word "However" in order to highlight the importance of the spa-

tial and temporal annual variability of erosivity, since in areas under the influence of an Atlantic rainfall regime, many events throughout the year make a significant contribution to annual soil losses (as happens in this catchment, for example - see Taguas et al., 2009). Taguas et al. (2010) determined the same features evaluated by Fleskens and Stroosnijder (2007) in another olive orchard catchment in Cordoba (Spain); nevertheless, after 5 years of measurements, we found that the shape of the histograms of erosive events also showed high inter-annual variability.

We recognize that our statement was not suitable, since we mixed the attributes of the events with the accumulated values of rainfall (annual rainfall depth and annual erosivity). Therefore, we have removed the word "However" and modified the text as follows:

"On a event scale, some studies performed in Spain showed that a few events of low frequency controlled the annual soil losses (Fleskens and Stroosnijder, 2007; Taguas et al., 2010); while Taguas et al. (2009) found that a large number of erosive events during the year made different degrees of contribution to the accumulated annual sediment loads. In Andalusia, the mean annual rainfall values vary from 200 to 2000mm (CMA, 2009) and mean annual erosivity varies from less than 50 to 10 000 MJ.mm.ha-1 h-1 (CMA, 2009). Therefore, the use of average climatic values for analyzing soil erosion is debatable".

Technical corrections: In equation (5) what does P stand for? In any case, it is not recommend using the same symbol for different parameters: P is already the support practice factor symbol in eq. (2).

Our apologies: we have modified the term "P" to "Pd" , to mean "daily precipitation".

The reference to equations and figures must be updated along the text. For example, in page 286, line 12 the authors may refer to eq. (5) instead of eq. (3); there are a lot of this type of error along the whole text. Similarly, the layout and the figures caption must be revised. For example, there is not and 'above' and 'below' in Fig.3 since both subfigures are located side by side. Even more in the text Fig. 3 is cited as Fig.3a and b; a and b should no be used as acronyms of above and below.

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

In page 278, line 11, I guess the authors means “avoid” instead of “maintain”.

Our apologies: this has been corrected.

In page 279, line13, what does “an old area” mean? Old in terms of what? Our apologies: the highest zone (1.4 ha) is an area currently planted with young olive trees, which was previously used for growing cereals - as can be seen in Fig. 1

In page 289, line 12 the authors may say “discontinuity” instead of “discontinuation”.

This has been corrected.

In page 280, line 4, the accuracy of altimetric precision is given in ppm instead of cm, mm or so on, please clarify this point.

Both planimetric and altimetric precision (expressed as root square mean error), have been corrected: 1 cm + 2 x 10-6 cm and 2 cm + 2 x 10-6 cm.

In page 288, line 24 please delete either “given” or “due to”. This has been corrected.

I recommend to revise the English writing by a native speaker. We have taken your recommendation into consideration.

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