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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.

Anonymous Referee #2

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It is an interesting work where the performance of the RUSLE model is evaluated at small scale and under Mediterranean conditions.

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.

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.

"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?

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?

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.

"...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?

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 causeeffect 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.

"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 MJmmha-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.

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).

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.

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

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Interactive Comment

In page 279, line13, what does “an old area” mean? Old in terms of what?

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

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.

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

I recommend to revise the English writing by a native speaker.

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