

Interactive comment on “Integration of remote sensing, RUSLE and GIS to model potential soil loss and sediment yield (SY)” by H. Kamaludin et al.

Anonymous Referee #3

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General comment: This paper presents an application of the known RUSLE erosion factor framework for evaluation of soil loss at the catchment scale, with a case in Peninsular Malaysia. The structure of the paper and the text is generally clear and well organized. The figures and tables are clear.

However, I have some critical remarks on how the overall modeling process was conducted. In order to parameterize the five RUSLE factors, the authors just use some generic empirical equations, found in literature. They also estimate certain factors as the C (cover management factor) and P (erosion practices factor) using simple look-up tables. This is not the procedure as described for RUSLE (see Renard et al, 1993). By

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doing so, one just determines and decides- a priori - what the erosion level will be under certain soil, slope, land cover vegetation condition. We know that effects of slope and cover can be very variable, and that's why Rusle also prescribes specific (field) data inputs for estimating the erosion factors.

Limitations of the model The limitations for use of RUSLE (Renard et al, 1993) and its predecessor USLE (Wischmeier and Smith, 1978) were always clearly stated by the authors, documenting the models in the famous USDA Handbooks and scientific literature. Besides being designed for field-scale assessment, they also present clear methods (and boundaries) for use of the different (usually empirical regression) functions to determine the erosion sub-factors. RUSLE gives a gross annual soil loss estimate and issues on depositional processes and sediment delivery to the natural drainage system is not represented by the models.

I will go systematically through the parameterization of the model (by the authors).

R-factor (rainfall erosivity) Two equations Morgan (1975) and Roose (1975) are mentioned and used. Luckily, Morgan's equation was derived from the data of the Malaysian region, but Roose derived his equation from Western African rainfall data and conditions. We know that annual rainfall alone is a very poor predictor of erosivity. E.g. in the northern UK, we may have 2000 mm of annual P, but a rather low annual erosivity (long lasting low intensity rains), where in drier countries with 500 to 750 mm of annual rainfall may yield a much higher R (e.g. due to high intensity storms). In principle, and at least, the authors should show they validated the R-estimator for their region.

K-factor (soil erodibility) The authors give a table with values, which are realistic and the units are correct. However, at the bottom of this Table 1 (p.4586), we find for steepland $K=0.5$. Strange as in principle, the effect of slope steepness and length is evaluated with the LS-function and factor. Also the first value, when no information available, give $K=0$ sounds strange. I would at least give an overall average/median value then, being the first descriptor of a sample dataset (of K-values).

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LS factor (topographic) The used a grey literature reference for this factor (Bizuwerk et al, 2008). The equation here is from the old (outdated) USLE (1978). This equation has been evaluated (see Renard, 1993) as outdated, and was replaced by a multiple set of newer LS-functions in RUSLE. The authors should adapt this.

C and P factors As mentioned earlier, no attempt was made to seriously parameterize these factors, using field and remote sensing observations. A simple NDVI to C-factor look-up table is used. Many tables, all rather specific to an area or region and cropping systems and vegetation types can be found in literature. Why these values? The C-factor is derived basically from canopy cover estimates (from NDVI, but also LAI leaf area index can be used as proxy), in combination with surface soil cover (e.g. stoniness, residue. . .) and some other sub-factors. The procedure is documented in RUSLE, but we don't find this back in this paper. At least, the authors should show how they validated their NDVI value conversion to C-factors for their land uses and vegetation cover types.

SDR (sediment delivery ratio) and sediment yield The authors use a single SDR to catchment area bivariate regression function (eq. 8 p4575) for determining SDR and SY from soil loss (A). This has been documented long time ago, but recent evidence has indicated this bivariate model is over simplifying the situation far too much. Overall catchment land slope, drainage density, lithology and channel slope near the outlet determine largely sediment delivery from a catchment. In Figure 7 (p.4597), I'm surprised to see that a low SDR, leads to a high sediment yield and vice versa. I would think the opposite, if we assume that $SDR = SY/(A \text{ or gross erosion})$.

In conclusion, the paper is rather well presented, but contains many flaws especially on the RUSLE model parameterization and validation as well as on the SDR and SY estimation. These important issues need to be resolved first, before it is worth being published in HESS.

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