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Hydrology and Earth System Sciences Discussions

Interactive comment on "Interrill erosion, runoff and sediment size distribution as affected by slope steepness and antecedent moisture content" *by* M. B. Defersha et al.

Anonymous Referee #3

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The authors conducted pan experiments under a rainfall simulator to assess the interaction among multiple variables on splash detachment and interill sediment yield. These variables included soil type, slope, and antecedent wetness conditions. Runoff was also evaluated; runoff was considered both in terms of the influence of the above variables but also as a variable that in itself can influence erosion. Based on the abstract, erosion models that included both rainfall intensity and runoff provided a better fit than erosion models that only considered rainfall intensity. Additionally, there were a number of interactive effects dependent on the specific combinations of variables. The experiments appear to be useful in light of the limited information on erosion processes in the region.

However, there are two key areas that require further explanation in the text:

1. As a primary concern, it seems quite important to suitably answer why runoff decreases so dramatically at higher slopes. There is a particular need to separate whether this decreased runoff is a real outcome of steeper slopes or – more importantly – to demonstrate that it is not some artifact of the experimental set-up that may call into question other conclusions. Reviewer #2 correctly states that changing slopes would decrease the amount of rainfall capture, but I think the impact is overstated. For a 45° slope, capture would be only 71% of a flat surface, but for a 45% slope (0.45 unit rise for 1 unit run), I calculate that capture would be 91% of a flat surface. Declines in runoff between 25% and 45% slopes are more than this 9% change in most cases. Especially in the pre-wetted trials, there would be minimal soil water storage available, so runoff rates should remain nearly the same across trials with varying slopes unless there is some subsoil flow (I see soils are supported on 90 mm of gravel in the trays and there is mention of a drainage outlet).

If there is sizable subsoil flow in the gravel, it is important to clarify that the trays are not necessarily representative of actual field conditions and that the correlation between slope and runoff generation is only relevant to the experimental set-up. Furthermore, if there is subsoil flow in the experiment, given that runoff may be a dominant variable in sediment yield, it would be important to indicate that the relationship between sediment yield and runoff may be transferable to the field scale but that sediment yield and slope may not be (given the complicating interaction between slope and runoff in the experimental set-up). Overall, I think the authors should be hesitant to make any general conclusions regarding slope in the paper if it does appear slope has an impact on runoff that would not scale to field conditions.

2. There needs to be more explanation of what data was fitted to the models and how the models were fitted (minimizing least squares error?). Presumably, in fitting

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a model such as Model I, sediment yield would be measured over each 15-minute storm interval and plotted against the corresponding intensity during that storm, but this needs to be explicitly stated. It would be ideal to also provide a figure of the yield vs intensity data and the best-fit lines for the different models for several different sets of factors (e.g. Soil Type A, 9% slope, prewetted; Soil Type A, 9% slope, dry; etc.). Also, it seems that the authors should be more careful in considering which model is most suitable. For all models, the R2 is often very high with only several isolated exceptions that tend to fall under Model I or II (e.g. prewetted, Soil C, 45% slope – R2= 0.60). It would be enlightening to consider why these isolated failures in certain models occur instead of simply saying Model V always gives consistently good R2 values. Particularly since the models do not use the exact same input data (some use intensity only; some use intensity and discharge; some use intensity, discharge, and slope) there is the possibility that Models III, IV, and V simply have some ability to compensate for possible errors in the expected intensity by also being weighted by another factor.

If anything, the authors should consider whether there is really enough evidence to demonstrate that any model is better or if in fact when fitted, all models appear to be suitable in most cases.

Finally, as noted by the other reviewers, the paper does need further grammatical editing and could also benefit from additional conciseness to make the manuscript more readable. The other reviewers have already provided numerous editorial suggestions, and I will not attempt to replicate their sizable efforts.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6447, 2010.