

Interactive comment on “Impacts of Changing Hydrology on Ravine Growth: Experimental Results” by Stephanie S. Day et al.

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Manuscript Title: Impacts of changing hydrology on ravine growth: experimental results

Manuscript Authors: Stephanie S. Day, Karen B. Gran, and Chris Paola

Summary: The manuscript shares details of an experimental study of ravine growth using a saturated substrate and regulated overland flow water volumes. Based on the experimental design, the authors make a case for channel width defined through hydraulic geometry. Sediment transport is modeled in the usual case, with the exception of varying slope, which the authors explain accounts for the linear nature of sediment flux.

C1

Review: There are several aspects of the study that should be reported that are absent. While the study is very intriguing, an overall lack of experimental methods limits the utility of the results and impact the vitality of the interpretations.

First, simply listing that you used a topographic scanner provides limited information on the applicability of DEMs obtained during overland flow. Perhaps more applicable to the “mud” samples, how exactly was the bed position determined for slope calculations during overland flow? If the entire sample had a sheet flow, then how were elevations determined beneath the flow? Was flow stopped so topography could be determined? Also, please clarify the gridding choice of 2 x 2.5 cm.

Second, there should be more information concerning the soils and their preparation. “Mud” is extremely generic. And, did anyone look at the bulk density of these samples? Water content and bulk density have an impact on erodibility and, therefore, an impact on the outcome of the experiment. Please be explicit concerning the sample, especially how the sample was prepared. Also, what was the initial slope of the samples?

Third, records of this type are not unique, as many have reported similar findings (not referenced). And, why allow slope to vary in your sediment transport model but not in your widening model? Also, comparisons made between rainfall derived erosion and overland flow (only) erosion are not comparable. Why then use Istanbuluoglu for comparisons? Volume is the only comparable term in the two studies, i.e. overland flow volume (current work) and storm volume (past).

As an opinion for the work, I believe that the experimenters neglected important issues of the study in this report. I do not think that the interpretations are incorrect but are skewed to the experiment. The interpretations are eloquent and the overall presentation is very well positioned. However, I regret that, without pertinent information concerning the systems employed and material used, the manuscript should be sent back to the authors for major revision.

I will be happy to review another version.

C2

