

Interactive comment on “Determining the susceptibility of soils materials to erosion by rain-impacted flows” by P. I. A. Kinnell

Anonymous Referee #1

Received and published: 29 February 2016

The author re-visited some old data sets and used a simple framework to interpret the controlled experimental results. The author argues that the soil erodibility, or soil susceptibility to erosion, is very much a function of the theoretical and modeling framework in which the notion of erodibility or susceptibility is introduced and formulated. The author demonstrates how the susceptibility of soils could be determined in situations where the impact of rain drops is moderated by the depth of overland flow.

Here are my major concerns with the manuscript:

1) In the introduction section, much is written about various values of soil erodibility, and how these values depended on the model adopted, and the erodibility value for the same soil may be quite different in relative terms. It is worth sharing the insight that while soil erodibility is a useful concept, its value is certainly a function of the

C1

theoretical and modeling framework, soil property, and the state that the soil is in. There is, however, very little in the introduction about what we know and we do not know about the moderating effect of water depth on the rainfall impact. A review of literature on rain-impacted flows is necessary to contextualise this short communication.

2) Comparing Eq.(1) and Eq.(2), the author basically assumes that the overland flow by itself is not able to erode, erosion (detachment) occurs entirely as a result of rainfall impact on ponded surfaces. Thus slope and flow rate, in the form of shear stress or stream power, do not feature in subsequent analysis. In many parts of world with serious soil erosion, the landscape is commonly rather steep and rugged. What is the impact of rainfall on soil detachment and on sediment-laden flows on steep slopes?

3) What are the objectives of the manuscript? Authors are required to make a clear statement in the introduction about the intent of the research and the objectives.

4) Without checking the author's publications in the early 1990s, and in the 2000s, it is fairly evident that most of the data and experiment results used for tables and diagrams have been published previously, especially Kinnell (1991, 1993a, 1993b, 2005a, 2005b, 2009). While there is nothing wrong to re-use and re-interpret previous experimental results, I would recommend that the author make absolutely clear what is the contribution that is new and original in the manuscript.

5) I guess that Table 3 was used to show the consistency in relative susceptibility. However, I could not find Eq. (10) and Eq. (11) in the text. Anyway, for controlled experiments, it won't be hard to have soil erodibilities in the right ranking order using two different modelling frameworks.

Apart from the issues outlined above, the paper is well written, with few typos and errors of omission.

Interactive comment on SOIL Discuss., doi:10.5194/soil-2016-5, 2016.

C2