

[Interactive  
Comment](#)

# ***Interactive comment on “Approaches for delineating landslide hazard areas using receiver operating characteristic in an advanced calibrating precision soil erosion model” by P. T. Ghazvinei et al.***

## **Anonymous Referee #2**

Received and published: 8 February 2016

I guess that the proposed experiment consists in verifying if the RUSLE variables can play the role of predictors in a landslide susceptibility model. As such, I could have followed this approach and see if modeling and validation would have confirmed this. But, in this case, they should clearly state the point and more deeply geomorphologically discuss the link between soil erosion and landslides in the study area. They should have discussed how and where soil erosion can cause landslides. I’m sure at the foot of the slopes soil erosion is responsible for a decrease in the steepness, unless we move into the fluvial scarps, where the down-cutting can trigger landslides. At

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



the same time, soil erosion in the topside of the slopes remove terrains unloading the potential failure surface. However, there's no sign of discussion on this.

Although I'm not a mother tongue reviewer, the English is really weak! It is hard to follow the sense of a lot of sentences. The number of unclear sentence is so high that the reader has to feel the sense of the research. Besides, unfortunately I found a number of scientific flaws.

The geomorphological analysis of the landslides is almost absent. We have to accept that Authors consider the movement typology as a marginal aspect of their experiment. They never mention the landslide typology. Do they consider soil erosion as capable to cause rock falls! Besides, there's a mix of causative and triggering factors in their treatment. There's a systematic misconception and/or confusion of basic points such as Risk, Hazard and Susceptibility. The interaction between rainfall and landslide activation is interpreted in terms of liquefaction (!), without saying a word about neutral pressure and effective stresses.

The model building relies on a very simple method (frequency ratio) without facing the problem of diagnostic areas or seed cells optimal selection. The RUSLE estimates of soil loss is then reclassified into five classes, but the Authors did not give any criteria for setting the threshold values separating the classes. The validation procedure is really weak! One ROC plot! We don't know if the extraction of stable pixels strongly control the results, nor if Authors replicated the validation tests in a n-folds scheme? At the same time, a so strange shape for the ROC curve would deserve some discussion. Finally, even in the case of a correctly written and properly structured manuscript, I have to say that the experiment design is not of great relevance and that the adopted model building and validation strategies (frequency ratio and one ROC curve!) configure a too simplistic approach to the issue. The stochastic modeling of geomorphological phenomena is nowadays a much more hard science! I tried to annotate the manuscript (see the attached file) but I couldn't do it systematically.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/3/C3189/2016/nhessd-3-C3189-2016-supplement.pdf>

---

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 6321, 2015.

**NHESD**

3, C3189–C3191, 2016

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C3191

