Interactive comment on “Improving predictive power of physically based rainfall-induced shallow landslide models: a probabilistic approach” by S. Raia et al.

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The paper illustrates a new version of the TRIGRS code, devoted to the assessment of rainfall-induced shallow landslide susceptibility, which adopts a stochastic approach to consider parameters to be sampled randomly from a given probability distribution.

Authors are inspired by the well known uncertainty in assigning precise values to geotechnical and hydrological properties of slope materials, especially when landslide susceptibility is assessed over wide areas.

The results related to two test-sites confirm the expected improvement from the original deterministic to the new probabilistic model.
Accordingly, the paper substantially contributes to the landslide susceptibility assessment, allowing (also thanks to the appendices) a reasonable reproducibility of its modelling procedure. It gives appropriate credit to previous experiences, being based upon a vast list of references. The paper is well organized and supported by several figures and tables, useful to highlight important aspects; although English is not my mother-tongue, it appears well written.

However, it is opinion of this reviewer that a couple of aspects deserve a comment:

- The abstract begins with a rather “strong” sentence: “distributed models to forecast the spatial and temporal occurrence of rainfall-induced shallow landslides are deterministic”. Such a statement seems rather ungenerous towards a family of distributed models which, since at least 1990’s, adopt a probabilistic approach. This is the case of LISA (Hammond et al., 1992), which, followed by other codes (e.g. SINMAP – Pack et al., 1998; PISA – Haneberg et al., 2004) perform spatially distributed probabilistic landslide hazard analyses.

References

- TRIGRS-P is tested in two study areas, which appear to be well known to authors, who have already published some papers dealing with Mukilteo and Frontignano areas. However, with regards to some key-aspects such as the hydrogeological setting of both areas, only generic statements are given. For example, in the case of Frontignano, the water table is set to a fraction of the depth to the failure plane, but no evidence is pro-
vided of the real depth of the water table, which is notoriously a fundamental parameter for a model which allows computing in both saturated and unsaturated conditions.

As regards technical aspects, throughout the text there are some words in parentheses (highlighted in the manuscript) which probably should be included in the text or eliminated. Moreover, the words “Fig.” and “Eq.” should be written in extenso.

In conclusion, the opinion of the reviewer is that the paper can be fully accepted after minor revisions.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/6/C2259/2013/gmdd-6-C2259-2013-supplement.pdf

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