Interactive comment on "Predictive power of a shallow landslide model in a high resolution landscape: dissecting the effects of forest roads" *by* D. Penna et al.

Anonymous Referee #2

Received and published: 10 September 2013

This paper presents an interesting case study from a 2.6 km2 area in NE Sicily where a large number of shallow landslides have been observed in recent years. Penna et al. use an existing 2d landslide susceptibility model (Borga et al., 2002) to investigate its ability to predict observed landslide release areas and to assess the role of forest roads in that context. This is certainly a topic that fits into the scope of this journal. Let me just start to say that I have general concerns about the meaningfulness of the used model. I know that combining a simple soil hydrological model with an infitite slope failure model (based on a high-resolution DTM) to locate landslide areas has been done for approximately two decades, and such models do have some capability

to identify potential landslide areas since near-surface lateral water flow and shallow landslides are strongly related to topography. On the other hand, it is also clear that 2-m to 20-m landscape elements are far from representing infinite slopes. Also a landscape with uniform soil depth and soil properties (such as porosity or hydraulic conductivity) is a bad representation of reality. Nevertheless, such models have been widely compared with observed landslides, and there is some general knowledge about the ability of such models to predict landslide areas. This knowledge is well-summarized in the introduction of this paper.

So what's the added value of this work? It highlights the sensitivity of the underlying DTM and confirms that coarser resolution has a smoothing effect on terrain attributes, which particularly matters for the unstable areas (not so much for the generally stable areas). On the other hand, it makes us aware of man-made terrain modifications, such as forest roads, which can largely increase the susceptibility of the terrain to landslides. And it shows that such roads must be represented in the DTM. The lessons learned from this work are partly intuitive, but it makes sense to properly demonstrate them using a concrete case study.

So, all in all, there's not much to say about this manuscript. It is concise and intelligible, it properly describes the methods and the case study, and it contains a few messages (in particular for potential users of such models). I only have a few minor comments and questions:

- Page 9766, line 2: "interested" should be "intersected"

- Page 9766, line 22: what is c? I assume it is lateral flow. If so, please add "c" to the sentence above.

- Page 9766, line 25: independent OF time and OF the local wetness

- Page 9767, line 5: should r be r(0) as in the equation?

- Page 9768, line 16: explain the abbreviation GEV

- Pages 9771 and 9772: the used landslide inventory includes both the 2009 event and previous events; according to picture Fig. 3 a) the terrain was "modified" by previous scars; this leads me to the question: from what date is the LiDAR derived DTM that was used in the model? Is it appropriate to use one and the same DTM for modeling landslides that occurred over several years, being aware that topography has changed by the landslides?

- Page 9768, line 12: the term "unconditionally unstable" bothers me; how is it possible that landscape elements are "unstable even when the soil is dry"? According to Table 3, such areas constitute between 6 and 13 % of the total area (depending on DTM resolution). This just cannot be realistic (and brings me back to my initial concern of using an infinite slope failure model for landscape elements of a few meter).

- Page 9778, line 8: what is EI?

- Page 9782, line 2: according to the reference list the paper of Burton and Bathurst was published in 1988; but in the text on page 9763 it is 1998. What's correct?