Reply to the interactive comment by referee #2

We thank the anonymous referee for her/his comments, which helped to clarify a number of points and improve the revised version of the paper. The reviewer's comments are quoted above the author's responses and are marked in italic.

General Comments:
"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"
We thank you the reviewer for recognizing and highlighting the main findings of our work.

Specific comments:
- *Page 9766, line 2: "interested" should be "intersected"*
We actually meant "interested" but changed it to "characterized".

- *Page 9766, line 22: what is c? I assume it is lateral flow. If so, please add "c" to the sentence above.*
Done. We revised the text to explain the meaning of 'c'. We thank the Reviewer for pointing this to us.

- *Page 9766, line 25: independent OF time and OF the local wetness*
Yes, we changed "from" into "of"

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

The error is due to a pdf conversion problem. We will make sure it will appear right in the final published version.

- *Page 9768, line 16: explain the abbreviation GEV*
Done!

- *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?*
The topography of the area was affected by the 2009 and previous events. However, these disturbances induced by the events were of smaller impact with respect to anthropogenic disturbances such as the road networks. This prompted us to consider the LIDAR DTM obtained after the event as an appropriate choice.

- *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).*
We agree with the Reviewer that our statement that "...this prediction as an indication for likely failing sites in future storms rather than areas proved stable during previous storms" requires a more thorough explanation, as follows.
Accordingly with the Reviewer, two main types of error can be identified by comparing observed landslides and areas characterised as unconditionally unstable or stable by the model: 1) a site is characterized by the model as unconditionally unstable or with a low value of critical rainfall, but no scars were observed on it; 2) a site is predicted as unconditionally stable or characterised by a high value of critical rainfall, but slope failures were mapped on it (see also Casadei et al., 2003). The first type of error indicates that the model over-represents areas potentially subject to shallow landsliding. Such error typically arises because of incorrect model assumptions, i.e. the action of processes external to the model framework and local conditions specific to the study catchment. The most common situation in our case is the assumption of a given soil depth exceeding the actual one. However, it should be observed also that type 1 error (low value of return time and no landslide observed) can result from concealing of areas failed at some time in the past. For instance, our observations in experimental basins (Tarolli et al., 2011) indicate that small, shallow debris flow scars rapidly heal and are difficult to detect after as few as three-four years. These findings suggest that unfailed slopes characterised by the model as unconditionally unstable or with low critical rainfall should be interpreted as likely sites of failure in the future rather than areas presumed to be tested and proved stable during previous storms.
Casadei, M., Dietrich, W. E., and Miller, N. L., 2003. Testing a model for predicting the timing and location of shallow landslide initiation in soil-mantled landscapes, Earth Surf. Processes Landf., 28, 925–950.

- *Page 9778, line 8: what is EI?*
EI is the Efficiency Index described in Section 4.3.

- *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?*

1998 is correct. We corrected the reference in the reference list. We thank the Reviewer for pointing this to us.