Response to Reviewer #2

We thank Tom Dijkstra for his very positive comments about our paper. The comments made by the Reviewer are in black, and our responses are in blue.

This is an excellent manuscript and it was a pleasure to read. I only have a few minor comments and a small number of suggestions for additional literature for the discussion section.

It forms a good step forward in the analysis of landslide hazard under deep uncertainty.

Specific comments:

P1:

L23 – 24: Dai et al. (2002)

Authors' reply: We will change the format of this reference in the revised manuscript.

P2:

L1: this is a very vague and generalised statement. Also, it is not just high intensity rainfall that causes more landslide events. Perhaps reword this slightly to show recognition that the process-response model is a bit more complex?

Authors' reply: In the revised manuscript we will rephrase this statement, reading instead:

"Similarly, another major factor affecting landslide occurrence is rainfall event intensity. Climate change is projected to increase the occurrence of extreme precipitation events in many regions worldwide, and therefore may also have important implications for the frequency of rainfall-triggered landslides."

L18: some good work on landslide inventories by e.g. Malamud, B.D., Turcotte, D.L., Guzzetti, F. and Reichenbach, P., 2004. Landslide inventories and their statistical properties. Earth Surface Processes and Landforms, 29(6), pp.687-711.

Authors' reply: We agree with the Reviewer that the paper by Malamud et al. is an interesting contribution to the literature. To note this reference for the reader we will add the following text on the revised manuscript:

"… (for discussion about landslide inventories, the reader is referred to Malamud et al., 2004)."

Reference: Malamud, B. D., Turcotte, D. L., Guzzetti, F., and Reichenbach, P.: Landslide inventories and their statistical properties, Earth Surf Proc Land, 29, 687-711, doi: 10.1002/esq.1064, 2004.

P6:

L21: if one week is sufficient to settle model I guess permeabilities are relatively high and critical hydrogeological processes are confined to a relatively thin near-surface zone? Further below (line 25) effects on gwt are introduced. Perhaps this requires a bit more information on typical slope geometry? Fig 2 gives some context, but if all cells are 1x1m then it seems that the model needs to cope with unsaturated flow through some 25 m of materials (top of slope) plus throughflow over distances > 100 m. Settling this in 1 week seems optimistic?

Authors' reply: Thank you for pointing this out. We agree that the explanation of the start-up period may not provide enough detail. On reflection we can see it oversimplifies the actual process/purpose of the initial part of the simulation. The aim of the 168 hours of zero rainfall is not so much to ensure steady state seepage or equilibrium, but to settle the distribution of the moisture content in the unsaturated zone cells. Often, in the smaller slopes, equilibrium is also achieved. For the larger slopes it is possible that an initially high water table may still persist after 168 hours, but this is not a disadvantage because such conditions may exist in reality.

To address this point, in the revised manuscript we will add a more detailed explanation, with paragraph 3 of Sect. 3.1 reading as follows:

"For this experiment the first 168 hours of rainfall forcing are set to an intensity of 0 mm.h^{-1} to allow the moisture distribution within the unsaturated zone to be established based on the suction-moisture curve. This is necessary because the initial moisture content in each unsaturated cell at time-step zero is a linearly interpolated value

between the initial surface suction and position of the estimated water table (where the pressure head is 0m). Then after the first iteration of the hydrological function in CHASM the Millington-Quirk equation is used to update the moisture content based on the suction-moisture curve. The implementation of a 168 hour start-up (zero rainfall) period should be sufficient to establish a representative moisture distribution and potentially steady-state seepage (equilibrium). However, if hydrological equilibrium is not attained, this is still an acceptable representation of the physical processes that may be observed in such slopes. For instance, it may replicate the high groundwater tables that often exist in our study area towards the end of a rainy season."

P9:

L16: it is interesting that the initial hydrological conditions are no longer significant. This appears to be a function of the locally relevant hydrogeological conditions (ie well draining soils)? I guess also that this is a consequence of the way in which the model is run - i.e. individual events at different ID. Once sequences of rainfall events are considered, perhaps the antecedent conditions would be of greater significance (dependent upon time between, and ID characteristics of successive events)?

Authors' reply: We thank the Reviewer for bringing up this important point. It was not our intention to suggest that the initial hydrological conditions are not a significant determinant of the occurrence of slope failure. Our initial classification tree (Fig. 5) shows that initial hydrological conditions do affect the likelihood of slope failure occurring. However, we also demonstrate that if we account for interdependencies between certain factors (e.g. effective cohesion and thickness of top soil), we can achieve the same level of prediction accuracy with a smaller tree. This is because of by combining the interacting factors (e.g. creating the new auxiliary variable "ratio between effective cohesion and thickness of the top soil") we can better capture triangular patterns like the one shown in Fig. 6a, and therefore we can achieve the same level of prediction accuracy of the original tree (Fig. 5) with a much simpler tree (Fig. 9). Nonetheless, if the classification tree in Fig. 9 were expanded, the initial water table depth would show up again and predictive accuracy would increase, albeit at the cost of increasing tree complexity.

We agree with the Reviewer that the impact of the initial hydrological conditions is a function of the local hydrogeology. Furthermore, as the Reviewer suggests, the impact of the initial hydrological conditions is closely related to the way in which our model is run, i.e. individual events at different ID with one week (168 hours) to settle the model. We opted not to consider the sequence of rainfall events, because it would not be straightforward to quantify the associated uncertainties using a single (or few) factor(s) that could be effectively included in the CART analysis. To acknowledge this limitation of our study, we will add the following text:

"In our study, we evaluate occurrence of slope failure for individual rainfall events with specified intensity-duration characteristics and variable initial depths to water table. As a result, a limitation of our analysis is that we do not consider explicitly the impacts of varying initial hydrological conditions that may result from sequences of rainfall events."

P11:

L15-16: you may be interested in the following publications for this discussion:

van Beek, L. P. H. (2003). Assessment of the influence of changes in land use and climate on landslide activity in a Mediterranean environment. thesis from Utrecht University dspace.library.uu.nl

Van Beek, L.P.H. and Van Asch, T.W., 2004. Regional assessment of the effects of land-use change on landslide hazard by means of physically based modelling. Natural Hazards, 31(1), pp.289-304.

Kuriakose, S.L., Van Beek, L.P.H. and Van Westen, C.J., 2009. Parameterizing a physically based shallow landslide model in a data poor region. Earth Surface Processes and Landforms, 34(6), pp.867-881.

van Asch, T.W., Van Beek, L.P.H. and Bogaard, T.A., 2009, June. The diversity in hydrological triggering systems of landslides. In Proceedings of The First Italian Workshop on Landslides (pp. 8-10).

Authors' reply: Thank you for your suggestions of these interesting studies. We chose to omit van Beek (2003), van Beek and van Asch (2004) and Kuriakose et al. (2009) from our discussion because these studies focus on the effect of land use change on landside hazard, which is beyond the scope of our study that assesses the effects of climate change on landslide occurrence and does not explicitly model the effects of vegetation on slope stability.

In the revised manuscript we will acknowledge the study by van Asch et al. (2009), which analyses the complex interactions between hydrological and stability responses of slopes. Of particular relevance to our study area are the shallow and deep slides discussed in this paper (rather than debris flows - which are not represented by CHASM and require a different type of model). Although we did not include an analysis of slip surface depths in this study, we are keen to explore this further since CHASM identifies the most likely slip surface location (and therefore depth) as part of the model outputs. We thank the Reviewer for confirming the importance of this aspect.

In the revised manuscript, we will add the following text:

"Extending this type of study to other slopes would allow further exploration of the complex interactions between soil depth, permeability, rainfall intensity-duration, antecedent rainfall and the resulting slide depth (see for example, Lumb, 1975; van Asch et al., 2009)."

References:

Lumb, P.: Slope failures in Hong Kong, Quarterly Journal of Engineering Geology and Hydrogeology, 8, 31-65, doi: 10.1144/gsl.qjeg.1975.008.01.02, 1975.

van Asch, T. W., Van Beek, L. P. H., and Bogaard, T. A.: The diversity in hydrological triggering systems of landslides. In Proceedings of The First Italian Workshop on Landslides, 2009.