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Discussions

Interactive comment on "Probabilistic landslide ensemble prediction systems: Lessons to be learned from hydrology" *by* Ekrem Canli et al.

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Dear referee #1, dear editor. First of all, we would like to apologize that our replies to your comments are somewhat late. There have been changes of positions and countries of residence among the authors. In order to facilitate a more timely revision we have now invited a new author to the team to support the revision and finalization of the manuscript. We also want to thank you for your thoughtful comments on our manuscript. For our revision, we intend to closely follow your and the other reviewers' recommendations. Please find our replies to your specific comments below. Kind regards, The authors

Anonymous Referee #1 General comments: The paper reviews recent developments

in applying ensemble prediction systems to probabilistic hydrologic forecasting and uses a case study to demonstrate how ensemble prediction might be applied to landslide forecasts. The paper is well written and could be accepted with minor revisions. The discussion and conclusions would benefit by (1) adding brief remarks about additional sources of uncertainty from DEM data, especially given the sensitivity of the infinite slope model to slope angle; (2) commenting about treatment of large-scale heterogeneity in regional scale model ensembles; (3) clarifying how validating landslide models using extreme events degrades model or forecast accuracy. These points are amplified in my detailed comments below. I have also noted several minor editorial corrections.

REPLY: Thank you. We will discuss the respective topics (DEM uncertainties, spatial heterogeneity, and extreme event validation) in our revised manuscript.

Specific comments: Page 7, lines 5-7, Greco and Pagano (2017) seem to indicate in their Figure 2, that warning needs to start during the latter part (triggering rainfall) of Phase 1. Common sense indicates that warning or at least issuing an advisory that slides are likely with additional rainfall during this stage is prudent. Waiting until stage II is probably too late.

REPLY:We agree that warnings or advisory information during the later stages of a landslide triggering rainfall event might already be too late to initiate effective counter measures (e.g. evacuations). In our manuscript, we therefore formulated the sentence to indicate that "warnings should generally be issued during indications of stage (II)" (i.e. the onset of a potentially triggering rainfall event). In any case, the provision of warnings and even landslide outlooks is a highly sensitive issue which must be carried out by an authority with the legal and political responsibility and within the existing legal framework.

Page 13, line 1, it is probably worth mentioning either here or somewhere that for the sake of simplicity, you varied only three of the most sensitive model parameters, cohesion, friction, and soil depth, but in an operational landslide forecasting system varying additional sensitive parameters would be prudent.

REPLY: We will add this information as suggested.

Page 15, line 19, consider inserting “or a property zone map” between “soil depth map” and “is provided”

REPLY: The integration of a property zone map would not influence the failure probability map but would indeed affect the risk as well as the decision on a warning.

Page 16, line 14, What is meant by “unpicking?” separating?

REPLY: We meant “unpicking” in the sense of untangling. Specifically in this sentence we want to say that the differences between model predictions matter and that it should be the goal to explore the reasons for the differences. For our revision, we intend to use the word “exploring” instead of “unpicking” to avoid misunderstandings.

Page 17, lines 23-33, Although “large scale” and “small scale” are used correctly in previous sections of the paper, they are used incorrectly here. Large scale maps and models are detailed (see https://en.wiktionary.org/wiki/large-scale). Similarly, small scale maps and models are generalized (cover large area with little detail). It would be clearer to use “local scale” and “regional scale” or similar terms.

REPLY: Yes, we were not consistent with the usage of large and small scales. In our revised manuscript, we will correct this and preferably use terms which are less prone to be confused (e.g. large area, regional scale).

Page 17, lines 25-34, occurrence of landslides on more gentle slopes that have low susceptibility according to the model ensemble (Fig. 5, Page 14, lines 28-29,) as well as the modeling artifact mentioned on page 14, lines 9-10 suggest that even using an ensemble, the modeler needs to account for certain inhomogeneity, including infrastructure, such as retaining walls, as well as broad deterministic differences imposed by geology, etc. Given the high sensitivity of the infinite slope model to slope, why are

effects of uncertainty in the DEM on model results still mostly unexplored? Shouldn't DEM uncertainty be an area for further research?

REPLY: Of course, DEMs are not perfect and necessarily represent a simplified topography. We think that since the advent of high resolution LIDAR DEMs, there is not much that researchers and practitioners could complain about in relation to DEM quality. But we agree that the modeler must take into account errors and artifacts present in the DEM (e.g. in our case the retaining wall which has an unrealistically high probability of failure). For a real-time application of our landslide forecasting model, such areas would need to be excluded. In any case, we intend to also discuss DEM related uncertainties in our revised manuscript.

Page 18, lines 3-5, See Gioia et al. (2015) for a case study of using literature values to parameterize such a model.

REPLY: Thanks for the hint.

Page 18, lines 21-29, The WMO (2012) argument makes sense for meteorological and hydrological models because they are calibrated to variables that can be measured continuously (temperature, humidity, precipitation, streamflow, and so on). Please clarify how the argument applies to landslides when the models are calibrated to landslide events, which as the authors point out, are rare. Thus, in this case, the statistical distributions are trained to the extreme events. The only common, almost daily events to which landslide models could be compared for validation are the absence of landslides. While it's true that most recent publications about rainfall thresholds for landslides have included some non-landslide inducing precipitation in developing or validating thresholds; it seems uncommon to continuously evaluate those thresholds against daily absence of landslides. This is an important point, because, if I understand correctly, you are arguing that the way that process-based landslide susceptibility models are being calibrated or validated (by comparison with past events) is biasing them in a way that decreases forecast accuracy. If so, and a new or different approach is needed for validating models, what do you suggest?

REPLY: On the regional scale, landslides are very much binary events; they either happen (1) or not (0). This is a major difference to floods where continuous measurements (e.g. of streamflow) can be carried out. Continuous measurements for landslides are very much linked to the local scale where monitoring systems for progressively moving landslides are being used to measure e.g. displacement, soil moisture and rainfall. However, the results of such local scale monitoring systems can not easily be used for regional scale forecasts of landslides. Thus, landslides can be considered extreme events when they are analyzed on the regional scale. Understandably, model calibration with a limited number of extreme events is challenging and we argue that it is therefore questionable whether such forecasting models can be expected to be able to precisely predict future extreme events. Including rainfall events that did not trigger landslides is a useful strategy for validating model outputs; however, it is also not entirely straightforward as landslides often take place undetected or unreported. It is beyond the scope of the paper to propose new approaches for the calibration and validation of regional landslide forecasting models. It was rather our intention to draw intention to this issue and to provide basis for discussion.

Focusing on the absence of landslides as a criterion can be problematic at the regional scale, as landslides (e.g. in forests) may remain undetected. Page 18, line 27-29, The problem here is not just a matter of lowering model sensitivity. In many cases, available data are not adequate to create a very sensitive model using any procedure. For example, in the rolling hill terrain of the Esino River Basin, Gioia et al. (2015) found there was no well-defined relationship between topographic variables and landslides, making it difficult to attain high model sensitivity. For other areas, such as the Colorado Front Range (Alvioli and Baum, 2016), model sensitivity was impaired by the quality of available DEMs, such as those derived from legacy photogrammetrically mapped topographic contour data, as well as other data uncertainties. Mergili et al. (2014b) similarly seem to have experienced difficulties obtaining high sensitivity even when

using a 3-D method and ranges of depth, cohesion and friction. While I appreciate main the point of your paper that accounting for uncertainty through the use of model ensembles will improve forecasts, it seems clear that continued research is needed on other fronts as well to overcome some of the challenges in making accurate forecasts.

REPLY: Thank you for your comment. We will describe the problem (i.e. the inability to create a sensitive model due to data) in our revised manuscript.

Page 19, lines 8-9, What do you mean here by averaging performed by the infinite slope stability model? Don't 3-D models such as the model of Xie et al. (2003, 2004, 2006), r.slope.stability (Mergili et al. 2014a), and Scoops3d (Reid et al. 2015) perform a sort of averaging over the neighborhood of each point in the search grid? By considering a group of neighboring cells in each trial failure, the 3-D models effectively average out effects of the ground surface irregularities at the same time as they account for effects of the finite extent and lateral boundary effects of realistically shaped trial landslides.

REPLY: Yes, thank you for this comment. Indeed, this paragraph was formulated in a misleading way. In the revised manuscript we will rephrase it as follows: "The most commonly applied modeling approach relies on the infinite slope stability model which reduces the landslide geometry to a slope-parallel layer of infinite length and width. This leads to very pronounced patterns of the factor of safety, whereas modeling approaches that introduce more complex landslide geometries produce smoother results since the effects of neighboring pixels are averaged out. Whether complex approaches such as r.slope.stability (Mergili et al. 2014a), Scoops3d (Reid et al. 2015) or approaches based on slip circles or ellipsoids (Xie et al. (2003, 2004, 2006) are able to outperform the infinite slope stability model depends on the settings, notably the landslide geometries. In theory, the infinite slope stability model is suitable for shallow landslides with length-to-depth rations above 18-20 (Griffiths et al., 2011; Milledge et al., 2012)."

Griffiths, D. V., Huang, J., and de Wolfe, G. F.: Numerical and analytical observations on long and infinite slopes, Int. J. Numer. Anal. Met., 35, 569–585, 2011 Milledge, D., Griffiths, V., Lane, S., and Warburton, J.: Limits on the validity of infinite length assumptions for modelling shallow landslides, Earth Surf. Proc. Land., 37, 1158–1166, 2012.

Technical corrections: Page 2, line 29, change “explicitly introduces in into the model” to “explicitly introduces it into the model” Page 6, line 24, capitalize “it” Page 8, line 22, change “provide” to “provides” Page 8, line 31, change “if” at the end of the line to “it” Page 13, line 13, change “a proof on concept” to “a proof of concept” Page 15, line 16, change “parameter” to “parameters” Page 16, line 22, delete comma after “data assimilation applications in both” Page 18, line 16, change “calibration if capable” to “calibration is capable” Page 18, line 27-28, change “had to be” to “must be” Page 18, line 30, change “a nonsuperior state over” to “an inferior state below” Figure 4, add outline of area shown in Figure 5.

REPLY: All suggested technical corrections will be integrated into the revised manuscript.

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F. and Fellin, W.: Spatially distributed three-dimensional slope stability modelling in a raster GIS, Geomorphology, 206, 178–195, doi:10.1016/j.geomorph.2013.10.008, 2014b. Reid ME, Christian SB, Brien DL, Henderson ST (2015) Scoops3DâĔŸAĔĞ Tsoftware to analyze three-dimensional slope stability throughout a digital landscape. Virginia: U.S. Geological Survey Techniques and Methods 14-A1, 218 p., https://dx.doi.org/10.3133/tm14A1 WMO: Guidelines on Ensemble Prediction Systems and Forecasting, World Meteorological Organization, WMO-No. 1091, Geneva. Available at: http://www.wmo.int/pages/prog/www/Documents/1091_en.pdf, last access: 30 November 2017, 23 pp., 2012. Xie, M., Esaki, T., Zhou, G., and Mitani, Y.: Three-dimensional stability evaluation of landslides and a sliding process simulation using a new geographic information systems component, Environ. Geol., 43, 503–512, 2003. Xie, M., Esaki, T., and Zhou, G.: GIS-based Probabilistic Mapping of Landslide Hazard Using a Three-Dimensional Deterministic Model, Nat. Hazards, 33, 265–282, 2004. Xie, M., Esaki, T., Qiu, C., and Wang, C.: Geographical information system-based computational implementation and application of spatial three-dimensional slope stability analysis, Comput. Geotech., 33, 260–274, 2006.

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