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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Prediction of rainfall induced landslide movements by artificial neural networks" *by* Janko Logar et al.

Anonymous Referee #3

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The proposed manuscript presents an application of neural networks for predicting the displacement of two landslides, one with maximum displacement rate of a few meters per day and one with a fraction of millimiter per day, using observed rainfall as the only predictor. The issue is relevant because it applies to real-time monitoring of active landslides, with obvious impact on lives and infrastructure, and it is well within the Journal's aims and scope.

My impression is that the manuscript is written in a clear and concise way, though (as already noted by one of the Reviewers) the introduction contains many references to other works that are only related to this one because of the use of neural networks but no other links or overlap are outlined, and it looks like there exists little context this paper fits within. After the long description of the NN applied to landslides bibliography,

the Authors get away with the statement "Simple multi-layered feed-forward artificial NN were employed": does this NN variant fit within one or more the quoted references? If not, why did you chose this particular variant? I believe that this is part of a bigger problem which, in my opinion, is the following.

The Authors state that "the aim of the paper is to test the ability of artificial neural networks to make reliable short term predictions of rainfall induced landslide movements based on normally available data: rainfall and measured displacements". The content of the paper is devoted to explain the method used to achieve the goal (which is convincing, in my opinion), the available data for the two mentioned case studies, and the outcome of training/validating NN for such case studies. For the non-expert reader, though, it is impossible to grasp the relevance of such outcome since: a) no "absolute" metric is applied to the results, as also noted by another Reviewer. The Authors state in the Abstract that NN "can predict landslide movements with sufficient accuracy", but it isn't clear according to what metric or standard, or if the accuracy reached in predictions is enough to take countermeasures in real time, for example. b) there is no comparison whatsoever with other methods, predictions, or estimates. The Authors state that the monitoring of active landslides is relevant for the threat they pose to the population and infrastructures (one of the case studies is an "urban landslide"), thus some other method should exist to predict landslide evolution. Is it possible to correlate the landslide movements to rainfall measurements in a statistically sound way, for example? If nothing exists, then it should be pointed out; if something exists (maybe an empirical method or practice), the authors should attempt a comparison to that. As a matter of fact, the proposed bibliography consist of 33 references, if I am not mistaking, out of which 4 are devoted to NN in general terms, 20 to NN applied to landslides, 5 to the two case studies, and only 4 to landslide methods in general. Thus, I ask myself if no alternative method exist in the literature that can be quoted and that this study can be compared with.

Another point I am concerned about is the assumption that rainfall is the only variable

that one can take into account for predicting landslide movement. This is intuitively the case, if one compares Fig. 3 to Fig. 4 and Fig. 9 to Fig. 12: it is clear that both landslides mostly move when rainfall intensity is larger. A closer inspection, though, reveals that displacements are different in magnitude, in the different sections. Is it possible to explain these differences using NNs? Figure 5 shows "ANN predictions of the rate of horizontal movements": is this for one section, or what? (By the way, figures' captions are not really informative). In paragraph 2.1, "selection of input parameters", it is stated that temperature, evotranspiration, landslide area, depth (though found to be constant in the present case studies) and slope may also play a role in landslide movement. Then, only rainfall and previous movements were "selected", and that looks arbitrary. All in all, it wouldn't be much difficult to introduce additional inputs in the NN machinery and check if they have a sizable effect on the output, and I believe it would be worth doing, if data exists. Both landslides are reported to exist since a few decades, and for the landslide in the UK an "extensive network" of instruments was installed, so maybe data exists and other kind of methods were introduced before to try and predict the landslides behaviour; if not, again, the novelty of this study should be highlighted. Can the Authors at least comment on these issues?

In conclusion, I believe that if the Authors could provide solid evidence that the proposed method perform better of other existing ones (if any), to what degree, and that NN can rule out the use of additional observed quantities other than rainfall, this could make the method much more robust and the paper much more conclusive about the ability of NN to predict landslide movement, for the two case studies. Instead, in the Conclusion(s) section, the Authors state that "NN is an alternative method to the mathematical models which are mostly used for the assessment of the landslide behaviour", but it is not clear what are the models and what they would predict, in the same circumstances described in the manuscript. Also, the final statement "training data should include obvious and/or theoretically justified input sets" is a bit obscure to me, for the reasons I have described above about input data.

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