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

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Dear authors,

I believe the paper you submitted may be of interest for the community. It shows two examples where a modified Artificial Neural Network (ANN, hereafter) is applied to predict the landslide displacement as a function of previous rainfall and displacement records. This is done in a back propagation framework testing the performance of ANN on available historical data.

I would like to highlight that my expertise does not strictly include ANN, so my comments may be biased towards other methods or even missing something due to the obvious reasons. Nevertheless, I have appreciated the reading. Here, I will briefly

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Discussion paper

summarise my comments whereas you can find the original ones in the PDF attached (Commented_nhess-2017-253).

The paper is well written. The text can be smoothly read and I only have minor comments which are matter of style rather than critics (check the PDF). A similar comment goes for the figures. They are simple and effective and I have no negative comments.

The introduction is probably the section of the manuscript where I would advise more amendments. It is long and plenty of technical details on the history of the method. However, when it comes to describe the experiment you are proposing in your work, the text is cut short. I would suggest to give more credit to what you did, stating the research questions you want to address.

I also have a minor comment on the use of references in the introduction. When you generalise the interest of the community on landslides you suddenly add 27 citations in 5 lines. Is it really necessary? Some of them start with e.g., meaning the following references will just be examples among many others. As they are just example, you can surely cut down the extremely long list to a sensible number.

As I am not an expert in ANN I am not sure if and how multicollinear variables are typically handled for such method. However, I assume that cumulative precipitation calculated at different time windows may be mutually correlated. This may also be the case for dispacements. I have found an article which deals with such issue (check the PDF), but I would not ask to re-run the analyses. Instead you could either justify your choice or add a table (or figure) with Pearson's correlation coefficients for all the combinations in your input parameters. In this way, the reader can actually understand how the dataset you use is structured and what are the potential implications.

The method section is quite simple which is well suited for an applied audience. Reference are properly added in case of a more demanding readership concerned on the method.

The typical result and discussion section are merged together. I would have expected more efforts from an interpretative point of view.

Similar to the last comment, the conclusion section may be improved. What was your research question? Did you find what you were expecting? What strength and weakness can you see in your method? ANN was capable of back-projecting the dispacement in both cases (Macesnik and Ventnor Undercliff). Is there something in common between the two mass movements that can justify such impressive success? If one looks at Figures 5 or 12, the predictive performances (which you chose to depict rather then numerically express. This could also be added) are extremely close to the observed data. How can you interpret this? Is it a common result? Does it deviates from rainfall in other cases?

These are some of the questions I would try to answer or just the direction I would probably give to the conclusion section. As it is now, I believe it is quite plain.

This being said, I appreciated the reading. Kind regards, Luigi Lombardo

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-253/nhess-2017-253-RC1-supplement.pdf

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