This manuscript discussed the application of several machine learning models in landslide susceptibility analysis. However, I don’t think it is worth being published in a high quality journal like NHESS. Here you can find my concerns:

(1) My biggest concern is from the novelty of this study. What is the new thing of it? A quick Google search showed that too many similar researches have been published. Most of them are characterized by the key words like “machine learning”, “landslide (or other hazards) susceptibility”. And the most important objective of such studies is to compare the ability of different models. But in my opinion, it doesn’t make sense when you compare too many models. They are just regular exercises on this topic.

(2) The structure of the MS is confusing. It is not using a widely accepted template for paper: Introduction—Methods—Study area—Results—Discussion—conclusion.

(3) It seems that a real Discussion section is missing.

(4) You selected 25 factors as input data of the model, but why are these factors not others? I mean all these factors are from literature and experience, aren’t they? How do you justify they are necessary, and the factors not selected by you are not necessary?

(5) In Abstract and Conclusion, quantitative results are really few.