

Dear Editor and Reviewers:

I quite appreciate your favorable consideration and the reviewer's insightful comments concerning our manuscript entitled "Assessment of liquefaction-induced hazards using Bayesian networks based on standard penetration test data" (No. nhess-2017-80). Those comments are all valuable and very helpful for revising and improving our manuscript, as well as the important guiding significance to our research. Now, we have made correction exactly according to the reviewer's comments, which we hope to meet with approval.

Once again, thank you very much for your comments and suggestions. The main corrections in the paper and response to the reviewer's comments are as follows:

Yours sincerely,

Xiaowei TANG , Xu BAI, Jilei HU, and Jiangnan QIU

Report #1:

Suggestion for revision:

1. Page 2, line 12, Liquefaction empirical models, please try to include research by (Goh 1994; Zhang and Goh 2013, Pal 2006; 13 Toprak et al. 1999, Zhang et al. 2015, Zhang and Goh 2016) based on historical data: Zhang WG, Goh ATC. 2013. Multivariate adaptive regression splines for analysis of geotechnical engineering systems. *Computers and Geotechnics* 48: 82-95. Zhang WG, Goh ATC, Zhang YM, Chen YM, Xiao Y. 2015. Assessment of soil liquefaction based on capacity energy concept and multivariate adaptive regression splines. *Engineering Geology* 188: 29-37. Zhang WG, Goh ATC. 2016. Multivariate adaptive regression splines and neural network models for prediction of pile drivability. *Geoscience Frontiers*. 7: 45-52.

Response: It is very grateful to your suggestion. These proposed papers by the reviewer are very valuable and helpful to our manuscript. The authors had studied them carefully and added them to Line 12-13, Page 2 in Introduction and Line 20-25, Page 23 in References.

2. Page 11, Furthermore, there are no effective simplified methods for estimating ground cracks and sand boils, and simplified methods for calculating lateral spreading

(Bartlett and Youd 1995, Wang and Rahman 1999, Goh et al. 2014) require the free face ratio or ground slope, which were not included in the data collected for this study. Please check Goh et al. 2014, the reviewer think it is Goh and Zhang 2014.

Response: Thanks for your careful inspection. The authors indeed made a mistake of the author's ranking of the reference (Goh and Zhang 2014). The authors revised it as shown on Line 19, Page 11.

3. About the use of liquefaction potential index LPI, other researchers have used the similar term-seismic liquefaction potential, the authors are encouraged to include MARS_LR approach: Zhang WG, Goh ATC*. 2016. Evaluating seismic liquefaction potential using multivariate adaptive regression splines and logistic regression. *Geomechanics and Engineering*, 10(3): 269-284.

Response: The authors studied the proposed paper and found that Zhang and Goh (2016) solved the liquefaction classification problem using the Multivariate Adaptive Regression Splines (MARS) approach based on Logistic Regression, rather than a calculation of liquefaction potential index (Iwasaki et al. 1982). In addition, the liquefaction potential index (LPI) was taken as an input variable for assessing liquefaction-induced hazards.

4. Line 21, Page 15, discussion part, it should be noted that the Goh and Zhang (2014) method for estimating lateral spreading is the multivariate adaptive regression splines (MARS) method instead of the multiple linear regression (MLR).

Response: The authors revised the mistake according to your suggestion as shown in Line 21, Page 15. Thanks a lot for your kind suggestion again.

5. As is well known, the ANN is a black-box method, that is, quite difficult to interpret the built model (maybe mainly through the weights, bias values, as well as the transfer functions). The authors should also explain the interpretability of the developed BN model and compare it with the MARS method and the empirical equations.

Response: In the Discussion, the authors explain the interpretability of the developed BN model and compare it with the MARS method from Line 24-26, Page 16 to Line 1-4, Page 17: Both the BN model and the MARS model are probability models which can possess interpretability in mathematics, unlike the ANN method with "black-box"

technology. They can easily develop comprehensive models that take into consideration all the independent variables with highly nonlinear. However, The MARS model reflects the functional relationship between the output parameter and the independent variables, and its equation form should be known at first before constructing the model. Additionally, the MARS model can only predict a single output (e.g. liquefaction potential or lateral spreading) at one time, whereas the BN model can reflect causalities or logical relationships among all the variables in graphically without any mathematical expression, and it also can predict several outputs (e.g. liquefaction potential, settlement, and lateral spreading, etc.) simultaneously and can proceed construction model and prediction under condition of missing values using the EM algorithm. It is worth to note that the main difference between the two models is that the BN model can skillfully combine with the prior knowledge and evidence (e.g. liquefaction data) by Bayes' formula that can improve the prediction accuracy of the BN model, but the prediction of the MARS model only depends on collected data.

6. As a data-driven method, what is the main limitation of the proposed BN model?

Response: The limitations of the BN method are that it needs a mass of data when constructing a BN model to guarantee a certain accuracy, if relative small amount of data are collected, it easily results in a non-robust BN model structure; and the causality or the logical relationship between two variables in a BN model obtained only by the data-driven algorithm is sometimes acceptable in mathematics, but not true in physics. The authors added the content from Line 5 to Line 9, Page 17.

Report #2:

Suggestion for revision:

I would only like to advise the authors to remove the word “risk” from the title since this is misleading as the study does not deal with risk assessment (e.g., “Assessment of liquefaction-induced hazards using Bayesian networks based on standard penetration test data”)

Response: The authors quite appreciate your suggestion for the revision of the misleading statement in the title. The revised statement of the title is shown in Line 1, Page 1.

References

Iwasaki T., Tokida K., Tatsuoka F., et al. Microzonation for soil liquefaction potential using simplified methods. Proc. 3rd International Earthquake Microzonation Conference, Seattle, 1319-1330, 1982.