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Comment on nhess-2022-90

Anonymous Referee #3

Referee comment on "A Scenario-based Case Study: AI to analyse casualties from landslides in Chittagong Metropolitan Area, Bangladesh" by Fahim Sufi et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-90-RC5, 2022

This work presents a framework for a tool where insights can be derived from data on past landslide events to inform response to future scenarios. While the premise is exciting and such a tool would be useful to inform decision making, I reject this manuscript for publication as I think the underlying methods should be reconsidered. Below, I explain why I have made my decision and provide suggestions for improvement.

My main reason for rejecting the manuscript is that the data inputs do not capture many important charactertistcs of landslides that may influence how impactful, in terms of casualties, a landslide may be. Some examples of these factors include, antecedent soil moisture[1,2] and slope material properties[3]. In addition, in this study, the only landslide types considered were slides, falls and topples, despite the fact that other landslide types (most notably, debris flows) do occur in Bangladesh. I understand that simple factors may have been chosen for ease of use and general applicability. However, inferring the impact of a future landslide with this tool would be problematic when landslide types, which are not reflected in the training data, are triggered. Debris flows, as previously mentioned, have resulted in many casualties yet their relationship with rainfall and antecedent soil mositure is complex [1,2]. Thus, if your tool is used to estimate impacts from a debris flow users may respond in an inappropriate manner as the tool has not been trained using debris flow data and thus, cannot capture the potential impacts of a landslide of that type.

There is also no assessment of the predictive performance of the tool. As the tool has currently been developed, the results would be misleading and likely improperly used by the anticipated end-user (e.g., emergency managers).

In addition to these general comments, the manuscript lacks clarity which ultimately impacts the reproducibility of the work. One example of this is the lack of explanation for the rainfall data used. It is not clear what rainfall corresponds to in the context of this study (e.g., average, maximum), how it has been measured (e.g., rain gauge, satellite) and at what scale (e.g., specific landslide, adminstrative boundary etc.). Even if the data

used is gathered from another source a summary of that data and how it was collected should still be mentioned in the text. While this is one example where clarity could be improved, there were many other instances where further clarification of data and methods used would be helpful.

To summarize, I think the topic of this work should be explored further; however, caution needs to be taken when developing a tool meant to inform those decision makers without technical expertise. In the next iteration of this work, the complex nature of landslide processes needs to be addressed somehow. The methods and data used need to be carefully documented and explained so that others can reproduce the work. The performace of the model should also be assessed and discussed. Limitations and uncertainty in the 'insights' being provided need to be clearly communicated to the end user.

[1] Baum, R. L., & Godt, J. W. (2010). Early warning of rainfall-induced shallow landslides and debris flows in the USA. Landslides, 7(3), 259-272.

[2] Wieczorek, G. F., & Glade, T. (2005). Climatic factors influencing occurrence of debris flows. In Debris-flow hazards and related phenomena (pp. 325-362). Springer, Berlin, Heidelberg.

[3] Medwedeff, W. G., Clark, M. K., Zekkos, D., & West, A. J. (2020). Characteristic landslide distributions: An investigation of landscape controls on landslide size. Earth and Planetary Science Letters, 539, 116203.