

## ***Interactive comment on “Quantitative Risk Assessment of Vehicles Hit by Landslides: A Case Study” by Meng Lu et al.***

**Anonymous Referee #1**

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The paper illustrates a methodology for Quantitative Risk Assessment of Vehicles Hit by Landslides in a Kennedy roadway in Hong Kong. It must preliminarily say that not novelty methods at all are consider to the fundamental topic within which the case-study proposed by authors evidently falls. The proposed manuscript needs to reach a differential with respect the previous work in this topic. As the manuscript is, it seems like a pragmatic solution (description of an engineering solution) to a case study that still lacks explanation and detail on some questions regarding the geotechnical conditions of the study site.

However, I think that a good contribution of your research can be to support establishing new guidelines for highways design for purposes of roadway safety in terms of landslide risk reduction hitting vehicles & persons. For this, the methodology must be

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more detailed looking for include some uncertainties involve in the process providing innovative or novelty assessment processes or methods. The authors should consider that include solutions to the assumptions and uncertainties involve in the processes, omitted in other research can be the innovative level required for a relevant paper.

There are clear probabilistic methods, but there are many uncertainties and assumptions that are not clear to the reader. This is because much of the data used for evaluations comes from secondary data obtained from other sources, which are assumed to be true and are not discussed by the authors.

The conclusions look more like a summary of the work. Additionally, the authors state that “The suggested method can also be potentially used to analyse the highway landslide risk in other regions”, but if are not clearly established some conditions of applicability in Hong Kong, how do you expect that this method could be used in other regions?

As mentioned above, part of the data is obtained from secondary sources. Hence, it is not possible to reproduce its acquisition process, even more so when some of these processes are poorly explained. Regarding those results that are obtained or calculated by the authors, if it is possible to reproduce them in part.

Some recommendation for authors: I should suggest to include the specific site and region of the case study in the title (see attached document). Abstract must be revised once all modification have been made. Some Figures must be re-designed for a relevant scientist paper publication. Methods must include an innovative formulation proposed by the authors, maybe the key of this could lies in the incorporation of those aspect omitted in other studies. Moreover, the limitations of the proposed model should be more explicit in the main text and and discussion of them may be incorporate. A figure containing a graphical workflow is convenient. The authors are suggested to read and take into account more high-quality papers about this particular case.

It is therefore opinion of this reviewer that the paper could be accepted after major

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revisions and modification in the relevant parts of the study.

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2020-11/nhess-2020-11-RC1-supplement.pdf>

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2020-11>, 2020.

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