

Interactive comment on “Development of bridge failure model and fragility curves for infrastructure overturning and deck sliding due to lahars” by Joaquín Dagá et al.

Anonymous Referee #2

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Review of "Development of bridge failure model and fragility curves for infrastructure overturning and deck sliding due to lahars" by Dagá et al.

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Summary

This manuscript focuses on the development of fragility curves for bridges affected by lahars. The main damage mechanisms considered are pier/abutment overturning and deck sliding. Development of the fragility curves follows a process of developing a conceptual model and limit state functions, incorporating analytical models of scour and pressure and finally, Monte-Carlo simulation. These curves (for bridges without (C1) and with (C2) piers) are then parametrised and tested against bridge failure data.

While the developed fragility curves seem conceptually acceptable, there is little justification of critical assumptions, the validation is statistically unsound and manuscript is difficult to follow and understand.

My main concern is that the statistics used in section 6, which underpin the conclusion, do not support the authors assertions. The Z test used in this manuscript only provides

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evidence that the null hypothesis (analytical curves match empirical data) cannot be rejected. It does not demonstrate the 'truth' of the null hypothesis. I refer the authors to the ASA statement on Statistical Significance and *P*-Values (Wasserstein and Lazar, 2016) and urge them to consider other, more suitable, approaches to validation of their fragility curves. Crucially, the number of samples and source of the empirical population is not described. I am unclear on the utility of extending data beyond 100% destruction (3.75 m/3.5 m) in both datasets as the probability of a Type I error (falsely rejecting the null hypothesis) with the authors data is 0. More concerning is the insufficient number of samples, from the abstract I believe the number of bridges in the empirical sample is 14, and am unsure on how the 'analytical' sample was taken. With such small samples, the probability of a Type II error (falsely accepting the null hypothesis) is very high. As the authors did not provide enough details on their statistical test, I am unable to calculate this, but am confident it is too high to make any meaningful conclusions.

For these reasons, I believe the manuscript needs extensive modification before it can be considered for publication. For this to be acceptable for publication, the authors need to fully justify their bridge failure model and support this model with critical analysis using their empirical data. As a suggestion, given the low number of empirical samples, it may be better to evaluate the performance of the model against their individual examples - this may provide a level of qualitative support for the model. To assist the authors, I have outlined my main concerns in the following sections.

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Main issues

Language, structure and figures

As the other reviewer states, the grammar makes it difficult to follow the logic of this manuscript. English proof-reading is needed to ensure the minor issues of tense, pronouns and adjectives are addressed and do not confuse the reader. The use of 'on the other hand' (Page 3, line 9; Page 4, line 5; Page 8, line 2; Page 10, line 5; Page 16, line 11 and more) also causes a lot of confusion. Figures 3 - 5 need to be modified (thinner line weights, different symbols, patterned lines) to ensure the graphics are easily readable in greyscale. Figure 1 is well drawn and designed, although definition of Q , q_{min} , q_{max} is needed.

Introduction

Page 2, line 5: "This implies less exposure and therefore, vulnerability ...". This is wrong, risk is generally considered as a function of the hazard, exposure and vulnerability. In this example, the exposure and vulnerability are the same but the hazard is lower - resulting in lower risk. One could also argue that exposure is lowered, but this will not lower the vulnerability.

Page 2, paragraph 3: "From available literature..." not much literature has been explicitly surveyed here - only examples of risk management software. The Wilson (2014) review is quite extensive, but the manuscript would benefit from a broader review of available literature on bridge fragility functions.

Proposed failure model for infrastructure overturning and deck sliding and experimental design

The conceptual model for bridge failure discussed and seems reasonable, but there has been no critical analysis or justification of assumptions used. In particular:

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- Page 5, line 1: The foundation has no piles. Is this justified by bridge designs (especially in your study area)? It may be a valid assumption, but the authors need to justify this with data (i.e. in the bridges used in subsequent sections, did any have piles?).
- Page 6, line 15: So you are not explicitly modelling the effect of scour on the resisting moment? Destabilisation from erosion (mentioned on Page 7, lines 1-7) would surely have a large role on changing the location or size of the moment. How is this accounted for?
- Page 7, line 25: The estimation of velocity Mannings formula is based on the assumptions of a one-dimensional, steady state flow, which is unlikely around bridges. Also, how was the effect of rheology on the flow accounted for? The velocity (and height) will depend on the rheology of the flow, this should probably be accounted for in the Monte-Carlo simulations.
- Page 7, line 27-29: How valid is a 'clear fluid' scour model for lahars? Is this model used? The grammar is unclear on page 8 (On the other hand), but if it isn't used - why is it mentioned in such detail?
- Page 8, how is the bending moment calculated? Where is the impact force located? Debris tend to collect on the surface of the flow, increasing the moment - the magnitude of this effect may be important (particularly for deck sliding).

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In Table 1, the variables of γ_{Gravel} , D_{Gravel} , h_{imp} , e_{Super} are not mentioned in the manuscript. How are they used in the Monte-Carlo simulations?

Validation

I refer the authors to my initial comments on the manuscript.

In equations 19 and 20, the important parameters n_a , n_e , x_a , x_e are not defined or fully explained. Although n_e might be assumed to be 14, what is the value of n_a ?

On page 15, line 15 and on: At these p/Z-values, the null hypothesis is not rejected. However, this does not establish that empirical and analytical proportions are the same due to the low sample size. The significance has not been fully tested, as you have not established the statistical power of the samples.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2017-330>, 2017.

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