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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Geotechnical stability analysis, fragility of structures and velocity of movement to assess landslides vulnerability" *by* O. Cuanalo et al.

Anonymous Referee #2

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The paper tackles an interesting topic on the estimation of landslide vulnerability with interesting new data on slopes and social settings in Mexico rarely presented in the landslide literature. The authors improved somehow the problems of the previous submission in terms of stucture of the manuscript. Definitions of the landslide terms (hazard, vulnerability, risk, etc) are also introduced as a sub-part of Section 1 (introduction), but it cannot be considered as a state-of-the-art discussion. To proof the approach of their method (severity index, safety factor, expected damage degree), the authors should investigate the pros and cons of the currently available vulnerability methods proposed in the landslide litterature, discuss the problems of data access, and the

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problem of spatial scale of analysis. As it is presented, I do not see any novelty in the approach proposed by the authors. For instance, the calculation of the exposure level can be assimilated to a landslide susceptibility mapping (e.g. location of slope with a height > 10 m, threshold in safey factor), so hy not using this term? Further, no information on the method used (and the water level introduced in the analysis) for the safety factor calculation are given, dramatically limitating the repeatability of the calculation. For the expected damage degree, no clear informaiton is given on the method used to establish the fragility curves; do the authors used a database of damage in relation to characteristics of the landslides? Then the database should be presented and the statistical relationships should be discussed. Again, with the information given, there is no possibility to reproduce the analysis thus limiting the outcome of the work. The Discussion Section is weak, and is basically only a synthesis of the introduction section. I encourage the authors to provide a detailed analysis of their data. The description of the rainffall/landslide relationships is also partly out of the topic of the manuscript, and I suggest the authors either to cancel it. Table 4, Figure Table 5, Table 6, Figure 1, Figure 3 and Figure 4 are also not necessary because they are very scholars - the authors can refer to textbooks instead to these figures/tables. Finally, the English language is still relatively poor and must absolutely be checked by a native English speaker. Because of these drawbacks, the manuscript is not acceptable for publication in NHESS.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 5689, 2014.