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Natural Hazards and Earth System Sciences
Discussions



# *Interactive comment on* "An improved method of Newmark analysis for mapping hazards of coseismic landslides" *by* Mingdong Zang et al.

**Anonymous Referee #1**

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The subject of this paper is interesting and potentially relevant to journal readers. Characterizing the bulk shear strength of jointed rock masses is a difficult problem and one that deserves attention. Doing so and using the results to improve Newmark analyses in such materials would be a significant contribution. The paper, however, falls short of accomplishing its aim for several reasons, as detailed below. It will need major revision before it could be considered for publication.

GENERAL COMMENTS:

1. Referencing in the paper is inadequate and, in places, comes dangerously close to plagiarism (see below). Whole paragraphs have been lifted almost verbatim from other papers without referencing. Some key references are missing. In some places (described below), referenced statements are made that are the exact opposite of what



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the cited reference actually says. Not acceptable.

2. The models in the paper are running far too hot: they yield unrealistically high hazard levels. The problem can be traced back to the factor of safety (FS) calculation, which obviously yields values that are far too low. That, in turn, is probably due either to shear strengths being too low or to a mistake in the application of the governing equations. A very large proportion of the map area (perhaps one-third) is statically unstable, which is simply not realistic and shows that either the model or the input data are flawed. And everything thereafter is tainted.

3. We don't really know if what the paper does is an improvement over the standard way of applying Newmark analysis. The paper needs to run a conventional Newmark analysis and then compare the results to the "improved" method to see if it is actually an improvement. Without a comparison, it is not possible to evaluate whether this yields better results.

4. Wherever Jibson et al. (1998) is referenced, it should be changed to Jibson et al. (1998, 2000). The 2000 paper is an updated version of the 1998 paper and contains much of the same information.

5. The English expression needs a great deal of work.

SPECIFIC COMMENTS:

Lines 44-50: This material, including the references, is quite clearly taken from Jibson (2011) and should be so referenced.

Line 56: Just saying "inexact reasoning" leaves the reader with a certain lack of confidence. Perhaps a brief explanation of what such a method entails would be appropriate. As stated, it is unclear if "inexact reasoning" is the name of the type of method or a description of one. It makes a difference in how this is perceived.

Line 69: Replace "erect" with "vertical."

Lines 75-76: Is this a published inventory? If so, reference it. If not, the method of inventorying and other parameters must be described here. Is the inventory available to others?

Lines 91-98: This text is pretty much verbatim from Jibson et al. (1998, 2000). Direct quotes really need to be referenced.

Line 102: What is "the shallow of slopes"? Not at all clear what that phrase means.

Line 103: "Jibson" is misspelled.

Line 105: The term "thrown landslides" is not really an accepted term in the international landslide community. The proposed mechanism is not proven or provable and should be considered hypothetical.

Line 112: Should this be "vertical" rather than "horizontal"? It is the vertical, not the horizontal, shaking that is ignored in Newmark's analysis.

Line 121: What is meant by "basic friction angle"? Please briefly define. Does it refer to intact rock? Joints? Or what?

Lines 130-133: Again, this is nearly a direct quote from Jibson (2011) and Jibson et al. (1998, 2000). It is important to avoid the appearance of plagiarismâĂŤall it takes is simple referencing.

Line 161: If there are slopes where FS=0.09, they are moving. Maybe a slope with FS=0.9 could be considered on the boundary, but at FS=0.09, failure is a virtual certainty if the slope has been correctly characterized. If these slopes are not, in fact, moving before the earthquake, it means that the approach for assigning FS is flawed, at least in part. It might mean that the strength characterization of the material in that area is significantly wrong. Or some other parameter is off. Or the equations are being applied incorrectly. But a FS that low is simply wrong: the slope would be moving. Something is very wrong in how this was done.

Lines 161-168: This discussion is clearly related to the approach of Jibson et al. (1998, 2000), which should be referenced here.

Lines 172-173: Eliminating flatter slopes is perfectly fine, as is setting a minimum FS=1.01. But the fact remains that the raw FS calculations yielded absurdly low FS values, which should cause revaluation of the input parameters and equations used. Go back to see if it was done right.

Lines 183-184: This is stated exactly backward. A higher critical acceleration would relate to lower susceptibility; a lower critical acceleration corresponds to higher susceptibility. It takes less acceleration to trigger movement; therefore, it is more susceptible to failure.

Lines 201-202: Add Bray and Travasarou (2007) to this list. Also, Jibson (2007) superseded both Jibson (1993) and Jibson et al. (1998) with respect to empirical regressions. Cite the most recent work.

Line 204: Replace “scalar” with “vector.”

Lines 218-219: This statement is completely incorrectâĂŤthe exact opposite of what Jibson et al. (1998, 2000) said. They clearly demonstrated (see Jibson et al., 2000, fig. 14) that larger predicted displacements do correspond to greater incidence of slope failures.

Line 227: The words “belief” and “disbelief” suggest that this is a matter of faith rather than science. Perhaps “total confidence” and “total lack of confidence” would work better.

Lines 261-269: This is a mischaracterization of what Jibson et al. (1998, 2000) found. They showed that most shallow, brittle failures occur at model displacements of less than 15 cm. A displacement of 60 cm is very large and would be more likely to correspond to larger, deeper slides. This again suggests that the model is running too hot and is over-predicting displacement and thus hazard.

Line 276: Define “proportion of landslide area.” Not clear what this means.

Line 280: Actually, figure 16 shows the proportion of landslide area approaching 0.06, not even close to 1.0. The proportion is definitely increasing, but it remains quite low.

Lines 297-300: The model results strongly suggest that the shear strengths used were too low, which yielded unrealistically low values of FS, which, in turn, yielded displacements that are far too high.

Line 470, figure 1: What are the colors on the map? Needs a color bar with values of whatever is being shown.

Line 478, figure 3: Change “shadow” to “shallow.”

Lines 480-483: The drawing might have originally been adapted from Wilson and Keefer (1983), but the adapting was done by Jibson et al. (1998, 2000), where this came from, as was the language of the caption. Referencing!

Lines 508-510, figure 10: This figure clearly shows that a very large proportion of the map has FS<1 and was therefore set to FS=1.01. When Jibson et al. (1998, 2000) did this, it was for a few dozen cells out of a million. In the case shown in figure 10, it appears that perhaps one-third the model had FS<1. That means the FS model is seriously flawed. Either the strength values are way off or the equations are wrong. But a model that is this statically unstable is simply not correct and needs adjustment. This undermines the basis for all following analyses and conclusions.

Lines 511-513, figure 11: Same problem. This map shows absurdly low critical accelerations. Not only would landslides be happening before the earthquake, any significant seismic shaking would have triggered tens or hundreds of thousands of slides if the critical accelerations were actually this low. And that didn’t happen. This model is running way too hot.

Lines 518-520, figure 13: Same problem again. This map predicts large areas with very large displacements. If this were accurate, the landsliding in the Ludian earthquake would have been widespread and catastrophic. Jibson et al. (1998, 2000) found that modeled displacements of >10 cm related to high landslide probabilities. That would include almost all of this map. Even accounting for differences in calibration between their model and this one, figure 13 is simply not realistic. The model needs to be dialed back.

Lines 521-524, figure 14: It is very difficult to see the landslides in this figure for visual verification. Can they be rendered in black to be more visible? It needs to be made clear that the numbers being shown are not estimates of the probability of landsliding (you can't have a negative probability), but rather confidence levels. In fact, the term "probability" should be removed from the caption and the text. This is not probability, it is, in the words of the paper, a confidence level based on "inexact reasoning."

Lines 530-532, figure 16: The vertical axis is cut off at a very low proportion of 0.06. Why? Can the results be portrayed up to a higher proportion of landslide area? 0.2? 0.5? 1.0? The text refers to 1.0, but the graph goes nowhere near that value.

REFERENCES

Bray, J.D., and Travasarou, T., 2007, Simplified procedure for estimating earthquake-induced deviatoric slope displacements: Journal of Geotechnical and Geoenvironmental Engineering, v. 133, no. 4, https://doi.org/10.1061/(ASCE)1090-0241(2007)133:4(381).

Jibson, R.W., 2007, Regression models for estimating coseismic landslide displacement: Engineering Geology, v. 91, p. 209-218, https://doi.org/10.1016/j.enggeo.2007.01.013.

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