No.	The comments	Our responses
1	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.	We appreciate your valuable comments and suggestions. We have incorporated them in the revision as documented below.
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 the cited reference actually says. Not acceptable.	Thanks for this kind remind. Yes, changes were made based on the specific comments in the revision as documented below.
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	Thanks for this good suggestion. The study area is a mountainous area, full of excessively steep slopes, and the maximum relative altitude difference is 2300m. Slopes more than 45° account for about 15% of the study area. Approximately 2% of the slopes are steeper than 60°. Slopes along the riverbanks are approximately vertical. For slopes more than 60°, the normal force on slopes from potential sliding blocks is almost zero, so these slopes

	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.	are statically unstable no matter how large the shear strength of rocks is. The Newmark's sliding block model may be inappropriate for this case. Actually, falls and slides had already occurred on part of some slopes before the earthquake. We modified some rock parameters and corrected the steepest slopes steeper than 60°, as detailed below. We have checked the application of the equations second time, and it is no problem of the computing procedure.
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.	This is a good comment. Yes, we ran a conventional Newmark analysis and added the results to the fourth part in the revision, and then the area under the curve method was used to draw comparisons between both analyses, see Line 347-360.
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.	Thanks for this kind remind. Yes, changes were made in the revision as suggested, see Line 41, 46, 53, 115, 118, 131, 156, 200, 201, 216, 252, 300, 316, 341, 530, 561, 569 in the revision.
5	The English expression needs a great deal of work.	Thanks for this kind remind. Yes, we have double checked the manuscript and corrected the language errors with the help of a native speaker.
Specific comments		
1	Lines 44-50: This material, including the references, is quite clearly taken from Jibson (2011) and should be so referenced.	Thanks for this kind remind. Yes, changes were made in the revision as suggested, see Line 49-50 in the revision.
2	Line 56: Just saying "inexact reasoning" leaves the reader with a certain lack of confidence. Perhaps a brief explanation of what such a	Thanks for this good suggestion. Yes, we explain the reason of using inexact reasoning and add a brief definition of "inexact reasoning" based

	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.	on Shortliffe and Buchanan (1975) in the revision, see Line 68-73.
3	Line 69: Replace "erect" with "vertical."	This is a good comment. Yes, change was made in the revision as suggested, see Line 86 in the revision.
4	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?	Thanks for this good suggestion. The landslide inventory is carried out by visual interpretation method through comparison between pre-earthquake satellite images from Google Earth and 0.2m-high-resolution post-earthquake aerial images. The database is not available to others. Changes were made in the revision as suggested, see Line 92-96 in the revision.
5	Lines 91-98: This text is pretty much verbatim from Jibson et al. (1998, 2000). Direct quotes really need to be referenced.	Thanks for this kind remind. Yes, changes were made in the revision as suggested, see Line 115-118 in the revision.
6	Line 102: What is "the shallow of slopes"? Not at all clear what that phrase means.	This is a good comment. Yes, changes were made in the revision, see Line 123.
7	Line 103: "Jibson" is misspelled.	Thanks for this kind remind. Yes, change was made in the revision as suggested, see Line 123 in the revision.
8	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.	Thanks for this good suggestion. Yes, changes were made in the revision, see Line 126-127.
9	Line 112: Should this be "vertical" rather than "horizontal"? It is the vertical, not the horizontal, shaking that is ignored in Newmark's analysis.	This is a good comment. Yes, the expression is a little misleading, and we have revised the expression in the revision, see Line 115. We want to explain here that FS has no relationship with input accelerations, no matter horizontal, vertical or inclined accelerations.

10	Line 121: What is meant by "basic friction angle"? Please briefly define. Does it refer to intact rock? Joints? Or what?	This is a good comment. Basic friction angle is the angle of frictional sliding resistance between rock joints, which can be obtained from residual shear tests on natural joints (Barton, 1973). We have added a brief definition in the revision, see Line 143-144.
11	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 plagiarism. All it takes is simple referencing.	Thank you very much for this kind remind. Yes, change was made in the revision as suggested, see Line 156-157 in the revision.
12	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.	Thanks for this good suggestion. There exist numerous high mountains and deep valleys with steep slopes in the study area. Actually, falls and slides had already occurred on local area of some slopes before the earthquake. For some steepest slopes (usually more than 60°), the shear resistance between the block and the sliding surface does not work anymore in Newmark's sliding block model. No block can stay on that steep sliding surface, and the calculated FS will be nearly zero in this case. Actually, we think that the sliding block model is inappropriate in this case, the unstable blocks are already failed, and the further sliding will occur along a failure plane inside the slope, the angle (α) of the inclination of the failure plane will be complementary of an angle of $45°-\frac{\phi_b}{2}$. According to the rock parameters, $45°-\frac{\phi_b}{2}$ is around 30° in the study area. Therefore, for simplicity, we assigned an angle (α) of 60° to those slopes more than 60° to avoid a too low FS from Newmark analysis. After the correction, static factors of safety range from 0.40 to 181.29. We also run a

		conventional Newmark analysis, and static factors of safety range from 0.54 to 283.35. The minimum FS values have little differences. Changes were made in the revision, see Line 168-176. For another reason, it is difficult for a statically stable slope to fail under an earthquake. Earthquakes usually make statically unstable slopes or slopes on the boundary fail. For this reason, it is important to truthfully characterize the shear strengths of slopes. Shear strengths assigned to the geologic units were from results of hundreds of shear tests from the references. We assigned the original shear strengths to the geologic units other than increasing strengths to make statically unstable cells stable as Jibson et al. (1998, 200) did, which will change the statically stable level of the whole area, especially the slopes on the boundary at first. In addition, we considered size effect of the potential slide surface, this would yield lower F_S, which, in turn, yield higher displacement. However, the actual inventory of landslides was used to calibrate the predicted displacements, and the confidence levels indicated by certainty factors fit well of the spatial distribution of coseismic landslides as shown in the hazard map (Fig. 16), see Line 336-346.
13	Lines 161-168: This discussion is clearly related to the approach of Jibson et al. (1998, 2000), which should be referenced here.	Thanks for this kind remind. Yes, changes were made in the revision as suggested, see Line 200-201 in the revision.
14	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	Thanks for this good suggestion. Yes, some rock parameters were corrected in the revision, see Line 640-641. We have double check the equations and ran the analysis again. The main cause for the low FS values is those steep slopes, and we assigned an

	and equations used. Go back to see if it was done right.	angle (α) of 60° to those slopes more than 60° to avoid a too low FS in the revision, see Line 168-176.
15	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.	Thanks for this kind remind. Yes, changes were made in the revision, see Line 216-218.
16	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.	This is a good comment. Yes, changes were made in the revision as suggested, see Line 237-238 in the revision.
17	Line 204: Replace "scalar" with "vector."	This is a good comment. Yes, change was made in the revision as suggested, see Line 240 in the revision.
18	Lines 218-219: This statement is completely incorrect and the 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.	Thanks for this kind remind. Yes, changes were made in the revision, see Line 252-254.
19	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.	Thanks for this good suggestion. Yes, changes were made in the revision, see Line 261, 262, 263, 319. The words "belief" and "disbelief" are original description of the certainty factor model, but we think the referee's expressions are better in this case.
20	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	This is a good comment. According to Jibson et al. (1998, 2000), the predicted displacements do not correspond directly to real slope movements in the field. We think that different cases have different displacement ranges, and it is

	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.	meaningless to compare the displacements with other cases because different ground motion intensity and empirical regression are used. However, we can analyze the sliding types based on the distribution of displacements within a certain case. 60cm may be a large displacement in some case, but an average level in this case. For the new method shown in the manuscript, the displacements range from 0 to 123cm, while for a conventional Newmark analysis, the displacements range from 0 to 121cm, almost in same range.
21	Line 276: Define "proportion of landslide area." Not clear what this means.	Thanks for this good suggestion. Yes, changes were made in the revision, see Line 313.
22	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.	This is a good comment. Yes, the description here may mislead readers. We want to explain the changing tendency of the proportion of landslide area with the increase of CF. According to Jibson et al. (1998, 2000), if a proportion of failed slopes (the proportion of landslide area) gets close to 1.0, it is incredible. Changes were made in the revision, see Line 318-320.
23	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.	This is a good comment. However, we have different opinions. Firstly, for the widely distributed steep slopes, the shear strengths do not work anymore in Newmark's sliding block model. Therefore, we assigned an angle (α) that the complementary of $45°-\frac{\phi_b}{2}$ to those slopes more than 60° to avoid a too low FS from Newmark analysis in the revision, see Line 168-176. Secondly, it is difficult for a statically stable slope to fail under an earthquake, earthquakes usually make statically unstable slopes or slopes on the boundary fail. For this reason, it is important to truthfully characterize the

		shear strengths of slopes. We may yield a higher level of FS if we increase strengths as Jibson et al. (1998, 200) did. But this will change the statically stable level of the whole area, especially the slopes on the boundary at first. In addition, according to Jibson (2011), FS from a limit-equilibrium analysis shows a slope to be either stable or unstable, but the likelihood of failure cannot be judged. Therefore, it is not necessary to keep all cells stable through changing strengths. As we considered size effect of the potential slide surface, this would yield lower F_S, which, in turn, yield higher displacement. However, the actual inventory of landslides was used to calibrate the predicted displacements, and the confidence levels indicated by certainty factors fit well of the spatial distribution of coseismic landslides as shown in the hazard map (Fig. 16), see Line 336-346. Finally, even for a same study area, different displacements will be yielded because different ground motion intensity and empirical regression are used. The predicted displacements do not correspond directly to real slope movements in the field (Jibson et al., 1998, 2000). Therefore, we chose the certainty factor model (CFM) to explore the relationship between the landslide occurrences and the predicted displacements, and the coseismic landslide hazard is expressed by certainty factors not the displacements.
24	Line 470, figure 1: What are the colors on the map? Needs a color bar with values of whatever is being shown.	Thanks for this good suggestion. Yes, changes were made in the revision, see Line 556.
25	Line 478, figure 3: Change "shadow" to "shallow."	Thanks for this kind remind. Yes, change was made in the revision as suggested, see Line 565 in the revision.

26	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!	Thanks for this kind remind. Yes, changes were made in the revision as suggested, see Line 530, 568-569 in the revision.
27	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.	Thanks for this good suggestion. Yes, we modified some strength values and corrected the steepest slopes steeper than 60° in the revision, see Line 640-641, 168-176. After the correction, static factors of safety range from 0.40 to 181.29, see Line 195 in the revision.
28	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.	Thanks for this good suggestion. Yes, change was made in the revision, see Line 560.
29	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	Thanks for this good suggestion. Yes, change was made in the revision, see Line 610. As we discussed above, different cases have different displacement ranges, and it is meaningless to compare the displacements with other cases because different ground motion intensity and

	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.	empirical regression are used. For the new method shown in the manuscript, the displacements range from 0 to 123cm, we should have a different standard of displacement for judging the high landslide probability. On the other hand, the ground motion intensity plays a crucial role in the magnitude of displacements. If we apply a maximum PGA of 0.25g, the maximum displacement will drop to about 46cm, as shown in another case study we have.
30	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."	Thanks for this good suggestion. Yes, changes were made in the revision, see Line 612-614.
31	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.	This is a good comment. Yes, the proportion has been up to 0.14 after the correction. The text is to explain the changing tendency of the proportion of landslide area with the increase of CF in the curve. Changes were made in the revision, see Line 318-320, 620.
Finally, we deeply appreciate the time devoted by the reviewer to the review process. Your constructive comments are invaluable to the improvement of our manuscript.		