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Natural Hazards and Earth System Sciences
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Discussions

# *Interactive comment on* "Evaluating Simplified Methods for Liquefaction Assessment for Loss Estimation" *by* Indranil Kongar et al.

**Indranil Kongar et al.**

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Reply to comments from referee 1 (by comment number), including the additional comment posted later

Reviewer #1: The authors test various methods for the assessment of liquefaction using data collected following two recent earthquakes in New Zealand. The study is within the scope of Natural Hazards and Earth System Sciences, it is generally well written, testing these methods using a large database of observations is a valuable exercise and the analysis appears to be carefully performed. Therefore, I recommend that this paper is accepted for publication but only after the following editorial changes are made

We thank the reviewer for his confirmation of the value of the research and recommen-



dation for publication. Our responses to specific comments are included below.

1. Abstract, first sentence: This sentence is grammatically incorrect. In addition, it is probably too long to be easy understandable.

We shall amend to correct the grammar and to make it easier to understand

2. Abstract and throughout: "methods" or "procedures" are what is being talked about here. Therefore, these words should be used rather than "methodologies", which are the principles that guide research practices.

We shall replace "methodologies" with the word "method"

3. Abstract and throughout: The word "data" is plural and hence the sentences should read "although these data may not" and "the input data are publicly".

We shall amend the text to ensure that the word "data" is used as a plural

4. P. 2, l. 8: This should probably read "both future risk assessments and post-event rapid response analyses".

We shall amend accordingly

5. P. 2, l. 9: Should it not be "liquefaction effects and physical damage" rather than "liquefaction risk and physical damage"?

We shall amend accordingly

6. P. 5, l. 12: USGS are not the only group to publish assessments of the ground motion following earthquakes so this organisation should only be given as an example.

We shall add a note to make clear that the USGS is only one possible source of post-earthquake ground motion data

7. P. 5, l. 13 (and elsewhere): Because of the high epistemic uncertainties in ground-motion prediction it is generally considered best practice to use a logic tree comprised of a set of ground-motion models rather than a single ground motion prediction equa-

tion. Hence I suggest slightly modifying this sentence.

We shall modify this sentence to reflect the reviewer's comment

8. P. 5, l. 15: I do not understand the comment "Although the use of Vs negates the requirement for ground investigation" because to assess Vs requires measurements on site, although they could be non-invasive (e.g. based on ambient noise approaches) as well as invasive (from boreholes). Vs30 could be estimated from geology or topographical slope, for example, but these would be uncertain and ideally should not be considered for site-specific analyses (e.g. Lemoine et al., Bulletin of the Seismological Society of America, 102, 2585-2599 2012).

We thank the reviewer for highlighting this mistake. The original comment "Although the use of Vs negates the requirement for ground investigation" applies only to the Christchurch case study for which Vs data already exist. However, more generally the reviewer is correct to point out that Vs assessment does require ground investigation. In this instance, the intention is to talk about general approaches rather than a specific case study, and so we shall amend the text accordingly.

9. P. 5, l. 17: I would change "extrapolate" to "estimate" or "interpolate" as extrapolation should be avoided.

We shall amend the text as per the reviewer's suggestion

10. P. 5, ll. 22-25: As noted in my comment 8 Vs30 from topographic slope (as provided by the USGS Global Vs30 Map Server) is uncertain because of the weak correlation between these variables. This should be commented on as a weakness of this approach.

We shall add a comment to highlight this weakness

11. P. 5, ll. 30: Boore et al. (Bulletin of the Seismological Society of America, 101, 3046-3059, 2011) update the relationships of Boore (2004) and other authors have proposed relationship for other parts of the world since 2004. I recommend making a



comment that such relationships ideally should be regionally calibrated. Some checking that the equations of Boore (2004) are appropriate for New Zealand would be useful.

We acknowledge that ideally relationships should be regionally calibrated. However, the rationale of the paper is to test simplified methods to be used for loss estimation, principally by insurers. In this case, part of the definition of a 'simplified' method is that the relationships that underpin it already exist in the literature and no new model development is required. It should be noted that the relationships proposed by Boore et al. (2011) are not updates of the relationships of Boore (2004) – rather they are alternative relationships specific to Japan, while the original Boore (2004) relationships are specific to California. Nevertheless, we are happy to investigate the appropriateness of the Boore (2004) relationship to Christchurch, albeit on a small sample dataset of Vs profiles (13 sites), and will add this work to the revised manuscript.

12. P. 5, Equations 8 and 9: It is not statistically correct to invert equations based on standard regression analysis (it would be acceptable if orthogonal regression had been used). I recommend adding a note that this inversion could be a source of uncertainty. Ideally a set of equations predicting Vs0-10 from Vs30 and Vs10-20 from Vs30 should have been derived based on regression in the correct direction.

We shall add a note reflecting the reviewer's comment

13. P. 7, ll. 5-6: There seems to be a problem with the phrase "and for the other zones are given".

We shall amend the text to make this sentence clearer

14. Section 2.3: Is it not circular to test this model on data from the Christchurch 2011 earthquake as data from this earthquake was used to develop it? I recommend adding a comment on this.

We acknowledge that there is some circularity in this test and comment will be added to that effect. However, it is worth noting that the datasets used to develop the model and test the model have not come from the same source and so may not be identical. Furthermore the original model has been calibrated to optimise estimation of the areal extent of liquefaction whereas in the case study exercise, site specific predictions are tested

15. P. 8, l. 23: “compatring” should be “comparing” and “eartqhuake” should be “earthquake”. Please spell check before manuscript submission.

We shall amend these errors and run a spell check

16. P. 9, l. 3: What is the source of the moment magnitude of 6.2 for the 2011 earthquake? Both the USGS and Global CMT give Mw 6.1 for this event. Perhaps it is GeoNet. This should be stated.

The source is GeoNet and this will be stated

17. Figure 2: What is the source of these contour maps?

The source is the New Zealand Geotechnical Database and this will be stated in the caption

18. P. 9, ll. 24-25: Are the results of SPT after the ground has liquefied appropriate to assess whether the ground is liquefiable? I would have thought that SPT values would be changed by liquefaction.

Firstly we should note that there is a mistake here since the LPI values have been obtained from CPT, not SPT, data. Historically it has been thought that after liquefaction occurs, soils densify and increase their resistance to future liquefaction. However, Lees et al. (Soil Dynamics and Earthquake Engineering, 79, 304-314, 2015) conducted an analysis comparing CPT-based strength profiles and subsequent liquefaction susceptibility at sites in Christchurch both before and after the February 2011 earthquake, and concluded that no significant strengthening occurred and that the liquefaction risk in Christchurch after the earthquake remained the same as it was beforehand. The



study by Orense et al. (Geotech Eng J SEAGS & AGSSEA, 43(2), 8-17, 2012) came to similar conclusions and therefore our view is that the post-earthquake CPT data is appropriate for assessing liquefaction susceptibility

19. P. 10, ll. 1-5: Are Vs profiles at only 13 points sufficient to estimate Vs profiles for the entire region? This may be appropriate if there are no changes in geology or topography across the city but it sounds too few for accurate results. A brief discussion on the uncertainties with this approach would be useful here. It would be useful to check the robustness of the interpolated profiles by removing one or more of the 13 profiles and comparing the results.

We acknowledge that modelling Vs profiles for an entire city based on 13 samples has major limitations and the resulting estimations therefore carry significant uncertainty. We shall add a discussion of this as suggested. However we are not sure there is much value in carrying out the robustness check as suggested by the reviewer. Due to the aforementioned limitations, the check will almost certainly show that the interpolated profiles are not robust. However, testing the limitations/weaknesses of input data is not the point of this study. The purpose of this study is to investigate how a non-academic loss estimation analyst can use realistic and publicly available data and existing models to estimate liquefaction occurrence. A sample of 13 Vs profiles is a realistic dataset and the analysis in this paper has shown that despite its limitations, the resulting LPI model corresponding to it performs better than most other simplified models. There is little that an analyst can do if an input dataset is weak and nothing better is available – they can either make use of what they have or not conduct the analysis at all. It is not clear what value there is to be gained simply by proving that the input dataset is weak.

20. Section 4: It could help readability to split this section up into subsections for each of the tests.

We agree that Section 4 would benefit from being split into sub-sections

21. Section 4: Why are the Zhu et al. (2015) models performing poorly when the

Christchurch data was used in their development? There is a little discussion of this on pp. 14-15 but more discussion could be useful.

There are two possible reasons for this. One is that the model development and test datasets have come from different sources and so may not be identical. The second is that the Zhu et al. model has been optimised for quantification of the areal extent of liquefaction, whereas in the case study, site specific predictions are being tested

22. P. 5, l. 28: There is a problem with the grammar in the phrase "in both models though that the observed rates that are".

We shall amend the text to make this clearer

23. P. 17, l. 19, "tectonic uplift": Could it not also be "tectonic subsidence"? What about just saying "tectonic movements"?

We shall amend the text accordingly

24. P. 18, ll. 26-27: There is something missing from the sentence "To calculate duration, there are 19 strong-motion accelerograph stations in Christchurch that record ground motions at 0.02s intervals" as the stations are not there just to calculate duration.

We shall amend this sentence to make it clearer that duration can be measured from stations but this is not their primary purpose.

25. P. 19, l. 18: It could be useful to say that even though the methods based on LPI are the best approaches tested that they still do not predict very well.

We acknowledge this is a fair conclusion and will add a comment to this effect

26. Figure 1: More information could be added to this map, e.g. the faults that ruptured in these earthquakes, the locations of the strong-motion stations used to estimate the durations, the locations of the 13 Vs profiles and the main areas of liquefaction (Figure 3). Currently, this map is not that useful and could be removed or combined with Figure



2 and/or Figure 3.

We shall add the suggested information to Figure 1

27. Table 4: It would be useful to combine this with Table 5.

Table 4 relates to section 3 (Model test application) and Table 5 relates to section 4 (Results). Furthermore, Table 4 is referenced in the text much earlier than Table 5. We do not believe it would be appropriate to combine these tables

28. Table 6: This could be added as an additional two lines to Table 5.

We agree that these two tables can be combined

29. Table 7: Is there not space to include these results in Table 5?

It is important to stress the difference between Tables 5 and 7. The results in Table 5 are based on specified fixed thresholds, whereas the results in Table 7 are based on optimised thresholds. Therefore combining the two tables would necessitate the addition of two columns to identify the relevant threshold in each case. Therefore it may be possible to combine the tables if it can be published in landscape format - in portrait the combined table would appear very cluttered.

30. Table 9: Could these numbers be added to Table 3 after conversion to SI units (e.g. cm)?

We shall add this to Table 3

31. Tables 10 and 11: Give the units of the values reported here. Metres?

Yes, this is metres and the tables will be updated accordingly

32. In addition to my comments yesterday, it would also significantly improve this manuscript to include some maps showing the areas predicted to liquefy by each of the methods. That would help understanding of the differences between the results rather than relying on statistical tests.

We are happy to add the type of maps suggested by the reviewer, however due to the number of models tested in this paper, we believe it is appropriate to only include a selection of maps for the best performing models (e.g. the two best models or similar) in the revised manuscript

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