Review of Arup Report

“Seismic Risk Study: Earthquake Scenario-Based Risk Assessment”

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Scope

This document presents a brief review of the report by Arup (REP/229746/SR001, Draft Rev. A.09, issued 26 November 2013) on scenario-based seismic risk assessments for the Groningen area. The review was carried out by the authors of this report between Wednesday 4th and Friday 6th December.

The review begins with a general assessment of the report, and then addresses each section (Sections and Appendices in the order in which they appear in the report) with more specific comments, many of which serve to illustrate the basis for the general conclusions. A list of references cited in this review is also included at the end of the document.

In view of the limited period of time in which this review has been conducted, the focus has been placed firmly on major technical issues and not on any minor details of presentation. However, in some cases ambiguities in the technical narrations and use of terminology are noted because these may have implications beyond this specific report.

General Assessment

The report covers the development of a database for exposure in the Groningen field area, focused primarily on buildings, and on the characterisation of their seismic resistance through the development of fragility curves for different categories into which the exposed building stock is classified. The study then identifies a number of scenario earthquakes (of magnitudes M_w 3.6, 4, 4.5 and 5) and estimates the resulting levels of building damage and consequent casualties in terms of deaths and numbers of injured. The study includes some sensitivity analyses to explore the influence of varying different factors in the risk model, such as the ground-motion exceedance level and the fragility curves adopted.

The work that Arup have conducted on developing and characterizing the exposure database for the Groningen area is first class. This element of the work is very commendable and constitutes an extremely valuable contribution from the Arup team. The exposure database and the building classification scheme that has been applied to it provide an excellent basis for conducting seismic risk analyses to quantify the threat presented by potential future induced earthquakes in the Groningen field.

Uncertainties in the technical data, such as the fragility curves, are significant. The results from the technical assessment of the hazard and the risk need to be understood with this in mind. The results of this risk assessment is not in line with that seen for other earthquakes of comparable magnitude, as noted in the report. In particular the estimates for casualties seems to be high in comparison to the impact of other similar magnitude earthquakes and suggest conservativeness in these results.

A number of technical issues need to be addressed. Most importantly, the conclusion that the three sets of damage and casualty estimates obtained using three different sets of fragility curves can be taken as "*providing a good indication of the possible levels of damage and numbers of casualties that could occur in future earthquakes in the Groningen region*" will need to be further substantiated. Firstly, while there is clearly very considerable epistemic uncertainty in the risk assessments, estimates of the uncertainty ranges should be inferred from viable models rather than simply a collection of available models. In this regard, fragility functions adopted from studies based entirely on different building typologies and the damage experienced in moderate-to-large magnitude earthquakes, without any adjustment for short-duration motions, are unlikely to represent genuine epistemic uncertainty. Secondly, many vitally important aspects of epistemic uncertainty have not been considered, which therefore undermines the claim that the results might be taken to represent the range of possible outcomes due to potential future earthquakes in the Groningen field. One of the key uncertainties not explored in this study is that associated with the choice of the most appropriate ground-motion prediction model, both in terms of medians and standard deviations. Thirdly, none of the calculations presented in the Executive Summary represent realistic simulations in terms of the treatment of variability in the ground-motion fields due to these scenario earthquakes.

Executive Summary

All results in the Executive Summary make use of median or 84th percentile ground motions, yet reference is repeatedly made to the damage and casualties that are "expected". We strongly believe that mean results should be presented in this scenario risk assessment, in order to provide the "expected" values. Otherwise, stronger justification is needed for the presentation of median (or 84th percentile) results based on full correlation of ground motions. Furthermore, it should be made clear in this Executive Summary that the damage and loss results have a 50% (or 16%) probability of being exceeded (should fully correlated ground motions occur), but are not the results that would occur, on average, should the event be repeated many times.

It is not clear whether the results for the casualties that are presented in this Executive Summary are for a day- or night-time scenario.

In the closing sentence on p.7 there is an important statement regarding the more pessimistic loss estimates obtained in the Arup calculations: "*These higher building damage and casualty estimates are possible but appear to be higher than observed levels of damage and casualties from tectonic earthquakes of similar magnitude elsewhere in the world*". The higher estimates of damage and deaths correspond to fragility curves for the more severe damage states (DS4, DS5) that are unproven for the region—and indeed for which no compelling arguments are offered—and the entirely unrealistic scenario of an earthquake producing shaking at the 1-in-6 probability level at all locations. Exactly how this is deemed to be "possible" is not explained. Moreover, it is noted also that the statement to the effect that those estimates of damage and numbers of casualties appear to be inconsistent with available field data is not followed up or discussed in any way. Not a single physical argument is put forward for why such devastating outcomes (that exceed anything observed following comparable tectonic earthquakes around the world) could be expected in Groningen. We believe this is an indication that the loss estimates are conservative. This is further compounded by the lower impact due to the expected shorter duration of the small-magnitude induced earthquakes compared to larger tectonic earthquakes from which the empirical fragility functions have been calibrated.

Chapter 1: Introduction

The equation presented in Section 1.2 is not the one applied in the risk analyses presented, as the hazard is not modelled as the "*rate of exceeding different measures of earthquake ground motion*", but is either modelled as conditioned on both a given magnitude and percentile of ground motion (50 or 84) or just conditioned on a given magnitude.

Chapter 2: Seismic Hazard

The use of loss estimations based on a single earthquake scenario and median ground-motion levels at all sites is justified on the basis of the same approach having been used by Chen *et al*. (2013) for studies in California. The Chen *et al*. (2013) study makes use of the HAZUS approach and includes a simple flat statement that they calculate median motions for their scenario ground motions, for which no justification is given (although this is standard procedure in the HAZUS approach). Bommer & Crowley (2006) explored this issue and concluded that it is justified since the ground-motion variability is embedded in the HAZUS vulnerability curves. Since the HAZUS methodology is not being applied for the damage calculations in the Arup study (an element of the HAZUS approach is only adopted for casualty estimates) the invocation of the Chen *et al*. (2013) study is not a robust justification for their approach. Moreover, the Chen *et al.* (2013) study then goes on to produce state-wide loss estimations calculated using hazard curves obtained from PSHA calculations at multiple locations, an approach that has been shown to give incorrect results for a distributed portfolio of exposure (Crowley & Bommer, 2006).

On p.16 the authors state that they do not use the version of the Akkar *et al*. (2013) GMPE with the modification at small magnitudes proposed by Bommer (2013) since they are considering scenarios with magnitudes M_w 3.6, 4, 4.5 and 5. Since the modifications to the PGA equation affect predictions for earthquakes of less than M_w 4.2, the explanation given is questionable. The M_w 3.6 scenario is clearly chosen to replicate the 2012 Huizinge event and the modified PGA equation was calibrated to the ground-motion recordings from that event, hence it is difficult to understand why the modified equation was not used for that case. The original equation, when compared to that modified using the Groningen field recordings, leads to overestimations of PGA by a factor of 2 at the epicentre, increasing with distance to a factor 4 just beyond 20 km, for the M_w 3.6 scenario earthquake. For the M_w 4.0 event, the overestimation is less severe, at about 1.3 at the epicentre and above 1.5 at 20 km. Clearly, these overestimated accelerations will have resulted in overestimations of the resulting damage and human consequences in these scenarios.

Another point we note in passing here concerns the equation at the top of p.17 to define the mean of normal distribution. The equation is mathematically correct but if one is to use only a single value of ground motion this might indeed be considered more meaningful and useful than the median (although it will not yield the same mean loss estimate as obtained from sampling the full distribution of ground-motion variability). See comments on Section 6 regarding the use of this value.

The discussion of the spatial correlation of ground motion near the top of p.17 is confused, and needs to clarify that the proximity or separation of locations (which may, for example, all be at the same distance from the source) affects their respective value of ε_{Φ} rather than the "*ground motion level*". Similarly, the statement at the beginning of the second paragraph that "*for a given earthquake, the ground motion inter-event variability (τ) is the same*", is meaningless: it should instead have been stated that for a given earthquake, ε_{τ} is the same.

There are some serious shortcomings in the discussion of the influence of the surface geology on ground motion and hazard levels (Section 2.5). At the foot of p.20, there is a statement that "*weak soils can significantly reduce or amplify earthquake ground motions depending on the amplitude and characteristics of the incoming ground motion*." Although very weak soils can limit ground motions by failing under very strong shaking (or through liquefaction in the case of saturated cohesionless deposits), it is the stiffness rather than the strength of the soils that influences the nature of the surface motion. Site response analyses require characterisation of the soil stiffness and not the soil strength.

Then, at the top of p.21, the authors state that "*the Dost et al. (2004) GMPE is based on ground motion records measured directly on the local ground conditions*", something that the analyses we have conducted—and shared with Arup—clearly demonstrated not to be the case. Although that equation was developed from recordings on soil sites in the northern Netherlands, it is well established that despite the widespread use of the time-averaged shear-wave velocity over the uppermost 30 m at the site, V_{s30} (which stems from boreholes often being limited to 30 m rather than any geophysical rationale), the ground motion depends on the stratigraphy over several 10s or 100s of metres, or even kilometres, at the site. The presence of the basal anhydrite layer in the Groningen field (whereas it is below the reservoir in the Roswinkel field from where the Dost *et al.* data were obtained) has been put forward as a viable explanation for the remarkable over-prediction of Groningen PGA data by the Dost *et al.* (2004) equation.

At the end of the second paragraph on p.21 there is a passing comment about work by TNO indicating that the local soils can amplify ground motions by factors of between 2 and 4. Does this refer to PGA? At what frequencies do these factors apply? Given the soft nature of these soils and the likelihood of nonlinear response, for what levels of motion do these amplifications apply? Most importantly, what are these amplifications relative to? In other words, a factor of 2 to 4 higher surface motions on the soil sites than motions at what locations or horizons? The statement as it currently stands is meaningless.

The value of the comment on anecdotal observations regarding site response at the start of the third paragraph on p.21 is not clear.

Chapter 3: Building Exposure

As noted earlier in this review report, and also in previous meetings and review exercises, we believe the exposure work to be of very high quality, and not only commend Arup for it, but also strongly recommend for Arup to continue its good activities on this front. The comments below are thus of a relatively minor nature.

The occupancy modelling would probably warrant a few sentences in this Chapter, given its importance for casualty modelling. We understand that HAZUS casualty model requires the population in each usage category during the day and during the night to be estimated. Did the population data from Bridgis require further elaboration to get these figures? Although the work on the building database—documented briefly herein because it is covered in other Arup reports—is outstanding, it is difficult to assess the models for occupancy because it is limited. Given the fact that the report presents estimates of injuries and deaths this is an important omission.

Chapter 4: Building Vulnerability

For the calibration of fragility functions, it is noted that the USGS ShakeMaps for the Roermond earthquake are based only on the ground-motion prediction equation. We actually understand that the macroseismic intensity values from Haak *et al*. (1994) were used as observations, and transformed to PGA values[1] in the ShakeMap using the equation by Wald *et al*. (1999).

The choice of the latter equation for converting the intensity measures of existing fragility functions is not explained, and the arguments defending the mathematically incorrect procedure of inverting the Wald *et al*. (1999) equation to obtain PGA from intensities are not tenable, not least because the variability in the relationship must also be propagated, as for PSI. If the authors need to convert from MMI to PGA, they could have made use of the more recent—and, by virtue of a large amount of small-magnitude data, potentially more applicable—relationship by Worden *et al*. (2012), which was derived in such a way as to be usable in either direction (*i.e*., to obtain MMI from PGA or PGA from MMI).

The conversion of PSI to PGA, although done correctly from a mathematical perspective, is based on the equation of Spence *et al*. (1992), which was used because it is the only such correlation that is available. The relationship is based on a very small dataset, which could have led to an underestimated standard deviation, and its applicability here is not discussed at any point. Given the availability of fragility curves derived in terms of PGA, and the questionable applicability of the curves adopted by Arup (given that they are based on different building types and calibrated to much larger magnitude earthquakes), this additional level of uncertainty could have been avoided.

We assume that the same fragility functions presented in Section 4.8 are used for all building use types? We would suggest that a clarifying comment on this is made, as it could be necessary to differentiate the vulnerability of public buildings, schools etc. in future studies.

[1] http://earthquake.usgs.gov/earthquakes/shakemap/atlas/shake/199204130120/download/stationlist.xml

Are the unreinforced masonry fragility functions (Section 4.8.1) for in-plane failure (or perhaps they cover both in-plane and out-of-plane as they are based on observed damage data)? This could be important to understand when comparing the results of analytical models with these empirical functions.

The fragility functions of the RC buildings (as presented in Figure 22 and Table 7) lead to more fragile buildings (for DS3, 4 and 5) than the 1920-1960 and post-1960's unreinforced masonry buildings. Whilst poorly designed and constructed RC moment frame buildings can behave very badly under seismic action, it is stated in the report "*most RC buildings in the Groningen area are expected to be shear wall buildings*". We also understand (from Table A.2) that the majority of these buildings are 1-3 storeys (and hence of a similar height to the unreinforced masonry) and were constructed after the 1980's. We would thus have expected these recently constructed, stiff, low-rise RC wall buildings to have a fragility that is at least lower than that of the older unreinforced masonry buildings. We would also have expected there to be a distinction between the fragility functions for the low rise (1-3 storey) and the mid-rise (≥ 4-storey) RC buildings. Although the percentage of these buildings within the exposure model might be small, we wonder if they may accommodate a not insignificant proportion of the population, especially during the day, and thus they might have an appreciable effect on the day-time losses.

The report contains the following phrase (p.35): "*These UK fragility functions for reinforced concrete buildings were developed in a consistent format with the Coburn and Spence (2002) fragility functions for unreinforced masonry*". We wonder if it would not have been better to take the reinforced concrete functions from Coburn and Spence (2002), for better consistency between the results of different building types? Indeed, it is earlier stated by the authors that "*It is therefore preferred to calibrate 'sets' of 'families' of fragility functions with available functions that cover the full range of adopted building typologies (i.e. a set of functions developed by the same authors using the same dataset), which can be validated based on the masonry data alone, and trust that the reinforced concrete buildings (and those of other materials) will be well-represented*." In the end, however, the fragility functions for reinforced concrete and steel buildings were not taken from Coburn and Spence (2002).

It is noted that the results of the dynamic analyses in the Structural Upgrading study are not inconsistent with the results of Figure 24; we believe the citation should be to Figure 25?

Chapter 5: Risk Calculation

The HAZUS methodology for population distribution and casualty estimation has been used in this report. We believe it is necessary to comment in this section on the other population distribution and casualty models available, and the potentially significant sensitivity of the results to this part of the analysis. In particular, the reasons for selecting the HAZUS approach for casualty estimation—beyond availability—should be discussed, given the potential impact of presenting casualty estimates that can be expected to cause very significant alarm.

Chapter 6: Risk Assessment Calculation Results

The fact that the time of day does not affect the casualty results is quite surprising to us, given that a larger proportion of people are outside during the day. We were comparing, for example, Table 27 and 28 on page 62, and we noticed slightly higher SL=4 outcomes for day-time than night-time. We wonder what is giving rise to these similar figures? Is the day-time residential population very small, and is there an increase of the total population of the region during the day (that is then found predominantly within non-residential buildings, such as commercial buildings and schools)?

One question we have on this section is why the loss estimates obtained using the mean ground-motion level (which, unsurprisingly, lie between the losses estimated for the 50^{th} and 84^{th} percentiles of motion) are shown since they are not really discussed or used at any point.

At the end of Section 6.3.3 the casualty estimates based on 84^{th} percentile ground motions are presented. The authors note that they are conservative, but that "*as noted previously, it is recommended that these higher casualty estimates are taken into consideration*". What do the authors mean here?

We are not sure what is the objective of Section 6.4. We do not understand why the median number of buildings has been considered as the metric for presenting the results of the scenario assessment. One reason could be to show (indirectly) the influence of the ground motion variability on the distribution of losses, but in this case we would propose that the mean and standard deviation is presented, rather than the median. In Section 6.4.1 there is a reference to "*the mean number of damaged buildings*", but we believe this should be the "median number of damaged buildings".

Another comment on Section 6.4 concerns the results shown in Figure 42. The text above indicates that the simulations of the uncorrelated motions included samples of the variability up to 3 sigmas and above, whereas no indication is given regarding the ranges of epsilon values covered by the simulations for the fully correlated case. This makes is rather difficult to interpret these results.

The modelling of the ground-motion field for a scenario earthquake by randomly sampling from the intra-event variability (with or without spatial correlation) is actually the correct approach for risk calculations with a distributed exposure, and certainly more coherent than using median or 84th percentile motions at all locations. We are not clear why Arup have explored such calculations but then made no use of them in their final conclusions.

The last section of this chapter refers to other casualty models. Some brief comments on the difference between these models and the one applied by the authors (from HAZUS) would be useful, given that they were not included in the sensitivity study. The motivation for choosing the HAZUS approach in preference to any of these other approaches is worthy of documentation.

The overview of small-magnitude damaging earthquakes in Section 6.6 is a useful idea but the limited level of information provided here reduces the value of this material. Nonetheless, as noted earlier, Arup do make the observation that their loss calculations do not seem to be reconcilable with any of these case histories, which is pertinent and relevant, but then this does not prompt them to question the validity or credibility of their model. A calibration or comparison with the case histories in the overview of small-magnitude damaging earthquakes would have strengthened the credibility of the overall analysis.

Chapter 7: Conclusions and Recommendations

Subsection 7.2.1 is given the promising title of *Uncertainty Reduction by Research and Development*. The key point that it fails to make is that the necessary first step is to develop a comprehensive model for what the epistemic uncertainties actually are and to quantify them through analyses and appropriate experts judgements (rather than assembling a risk estimation calculation from readily available models), although it is recognised that such information may be available in other documents not available to the reviewers. When this has been done, the next stage is to conduct detailed and systematic sensitivity analyses (see, for example, Crowley *et al*., 2005) to identify the absolute and relative influence exerted by the various uncertainties, in order to establish where it is worthwhile expending effort towards reducing uncertainties. To propose a list of activities—some of which are potentially very expensive and time-consuming—without such a framework is not appropriate.

Appendix B: Building Vulnerability

There is an error in Equation (3), Section B5, as the “sigma” on the left-hand side of the equation should be squared.

In Section B6.1 a reference is made to Table A.4 which is not included in the report.

Does Figure B.4 come from Haak *et al.* (1994)?

Appendix C: Arup Ground Motion Duration Study

One general observation that can be made from the outset is that we find it questionable to calibrate the simplified SDOF analytical model (aimed at representing the actual hysteretic behaviour of masonry buildings analysed as SDOF structures) on the basis of output from a complex Finite Elements analysis (of the ‘Villa’ structure) that, as the authors themselves point out, does not adequately consider several aspects of masonry response. This may have some influence on the very minor influence of duration that the authors end up finding, which does not match what has more generally been found in studies of the seismic performance of masonry (*e.g*., Bommer *et al*., 2004).

The finding of a limited influence for duration may also be influenced by the choice of ground-motion records used in the analyses, and in particular the accelerogram suite representing the “short-duration motions”. Although it is a seemingly convincing argument for using the magnitude of M_w 4.7 from the PGV disaggregation rather than the M_w 4.2 from the PGA disaggregation, for calibration of curves expressed in terms of PGA, the smaller magnitude would have been more correct by virtue of consistency. Even accepting this step, they then search for records from earthquakes in the range from M_w 4.5 to 5.5, which is hardly centred on the chosen value (and hence the unsurprising result that the durations of the chosen records were skewed towards larger values). We are told that the 30 records with the shortest were then selected but these still remain biased high with comparison to their target (Figure C.1).

In Section C3.5 it is noted that DS5 was based on displacement at the level where “*a large reduction in capacity occurred*”. How to the authors define “large”? Although Arup consider a maximum displacement criterion for DS5, it is defined according to the reduction in capacity, and thus is consistent with the approach of Pinho and Crowley (2013). However, in order to appreciate how similar the two assumptions might or might not be, it would be necessary to know the reduction in capacity assumed by Arup for the definition of DS5.

The fragility functions (based on long duration records) presented in Figure C.9 lead to a median collapse capacity of over 2.5g which is twice the median collapse capacity of the functions used by Arup for unreinforced masonry (post-1960) buildings in the seismic risk study. We are comparing with the post-1960 masonry structures as we understand that the “Villa” model is closer to this category; if this assumption is wrong, the differences between the collapse capacities would be even higher. We wonder if similar conclusions on the reduced impact of short duration records would be obtained with models that produce fragility functions that are closer to those used in the study. Finally, we would have expected the study to have been carried out on the pre-1920s structures which are the most predominant structure and which are more likely to have stiffness and strength degrading behaviour.

Appendix D: Detailed Results of the Risk Assessment Study

We find some of the results presented in D11 rather confusing and difficult to interpret, and as mentioned before we believe interpretation of the mean damage/loss and standard deviation would be easier. We would have expected this section to also discuss and show that the standard deviation of the damage/loss is lower (and underestimated) when uncorrelated ground motions are used, and is much higher (and overestimated) when fully correlated ground motions are used.

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