PSHA Validated by Quasi Observational Means

R. M. W. Musson
British Geological Survey

INTRODUCTION

It might seem odd to be writing about confirmation of the validity of probabilistic seismic hazard assessment (PSHA) in 2011, given that the method has been successfully applied in countless studies worldwide over the last 40 years. However, the fact that papers still occasionally find their way into print attacking the method as mathematically invalid seems to indicate that there is still some requirement, if small, to demonstrate the soundness of the method. A number of mathematical arguments have been advanced over the last few years purporting to show mathematical or logical flaws in the standard PSHA methodology that invalidate the results. A comprehensive summary of these objections can be found in Klügel (2008).

It is not the purpose of this short paper to enumerate these claims or analyze them in detail. Rather, the intention is to point out that the results of a PSHA study using the Cornell-McGuire method can be duplicated by a completely different route, and without any but the simplest mathematics, using a quasi-observational approach. The fact that two completely different approaches to computing the answer to the question "What is the probability of ground motion *Y* at a given site?" yield the same answer suggests the validity of both methods. If the basis of conventional PSHA were flawed, the results could not agree with the results of another study that shared none of the same procedures.

It seems that many seismic hazard practitioners are unaware of this "refutation of refutations"; hence the present paper may be timely. It used to be received opinion that computing hazard in different ways necessarily led to different results (*e.g.*, Makropoulos 1993), which would certainly raise questions as to which results were "right." The statement "based on these assumptions, the annual probability of a PGA of 0.2 g at this site is 0.001" purports to be a factual statement, and therefore it should be possible to determine its truth or falsity.

APPROACHES TO PSHA

In a die-based experiment, the statement "the probability of rolling a two on this die is 0.166667" (*i.e.*, this die is fair) could be tested by making a suitable number of rolls and observing directly the number of twos that are rolled as a fraction of the total number. In seismology, a similar solution could be applied to answering the question "What is the probability that tomorrow there will be an earthquake larger than 6 *Mw* somewhere in the world?" It would be sufficient to collate the data for the past 1,000 days and observe on how many days an earthquake above 6 *Mw* was recorded. To answer the question "What is the annual probability of 0.2 g PGA at my site?" is more intractable, as the data are insufficient, and there may actually be no past observations of the target condition.

The test of a statement about earthquake ground motion would be to make observations over a long enough number of years (say, 100,000) and count the number of times an acceleration over 0.2 g is recorded. Or even better (but even more impractical), observe the same year 100,000 times in independent parallel universes. This would give a very classical frequentist estimate of the probability, based on simple counting.

While one cannot in actuality venture into multiple parallel universes, one can very easily do the next best thing, which is to simulate what would happen if one could. One can think of a PSHA study as consisting of two parts: a model (represented by a PSHA input file) and a process (represented by a seismic hazard program). The model is essentially a conceptualization of the seismic process expressed in numerical form, describing 1) where earthquakes occur, 2) how often they occur, both in terms of inter-event time and magnitude-frequency, and 3) what effects they have. With these three elements, one describes everything that determines the statistical properties of the seismic effects that will occur at a given site in the future. This is, therefore, all that one needs to simulate that future. One does not know precisely what will happen in the future, one only knows the aggregate properties of the seismicity and the ground motion propagation. Therefore simulations need to be stochastic. For each simulation, the probability density functions for earthquake occurrence in the model are randomly sampled to produce one possible outcome compatible with the model.

If the lifetime of a structure is 50 years, there are advantages in simulating 50 years at a time, but really, it is only the total number of simulated years that matters. The procedure is as shown in Figure 1. The earliest documentation of this procedure I can find (with some variation) is in Sólnes (1997). It has been used regularly for hazard studies by the British Geological Survey (BGS) since the mid-1990s (Musson 1998, 2000).

To generate a synthetic catalog from the model, proceed one year at a time and one source at a time. Assuming a Poisson model, one obtains the probability that in the first year of the catalog, the source will produce zero, one, two, etc., events. It is not even necessary to use a Poisson model if one believes some other model fits better. For each simulated earthquake in each catalog, one assigns a possible ground motion at site, calculated from a relevant ground motion model, based on the magnitude and distance (and fault type, etc.) of the simulated event. Since

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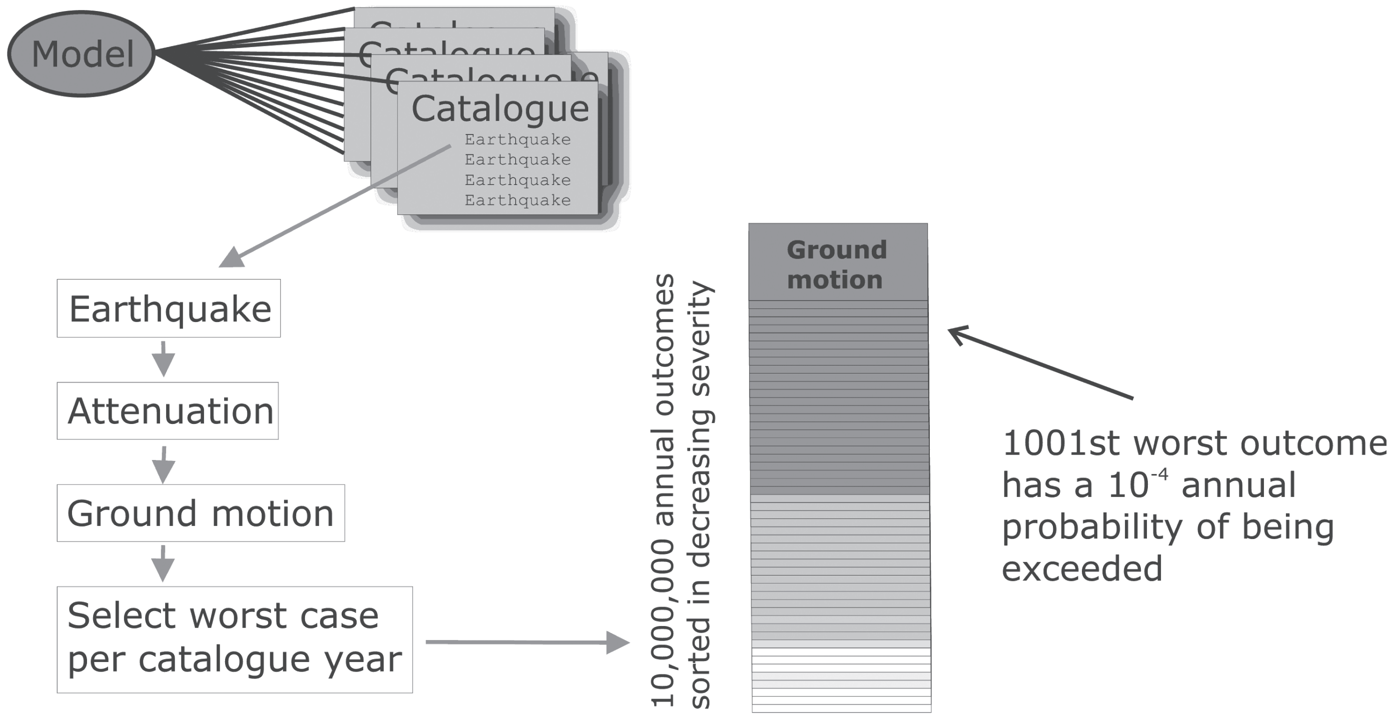


▲ **Figure 1.** Diagrammatic representation of the Monte Carlo simulation process.

it is evident that the same magnitude at the same distance does not always give the same ground motion, variability is introduced by generating a random value of epsilon (the number of standard deviations from the median) and assigning a final ground motion for this event based on the "scattered" value, where the degree of scatter, or aleatory variability, is defined by the sigma value of the ground motion model in the usual way.

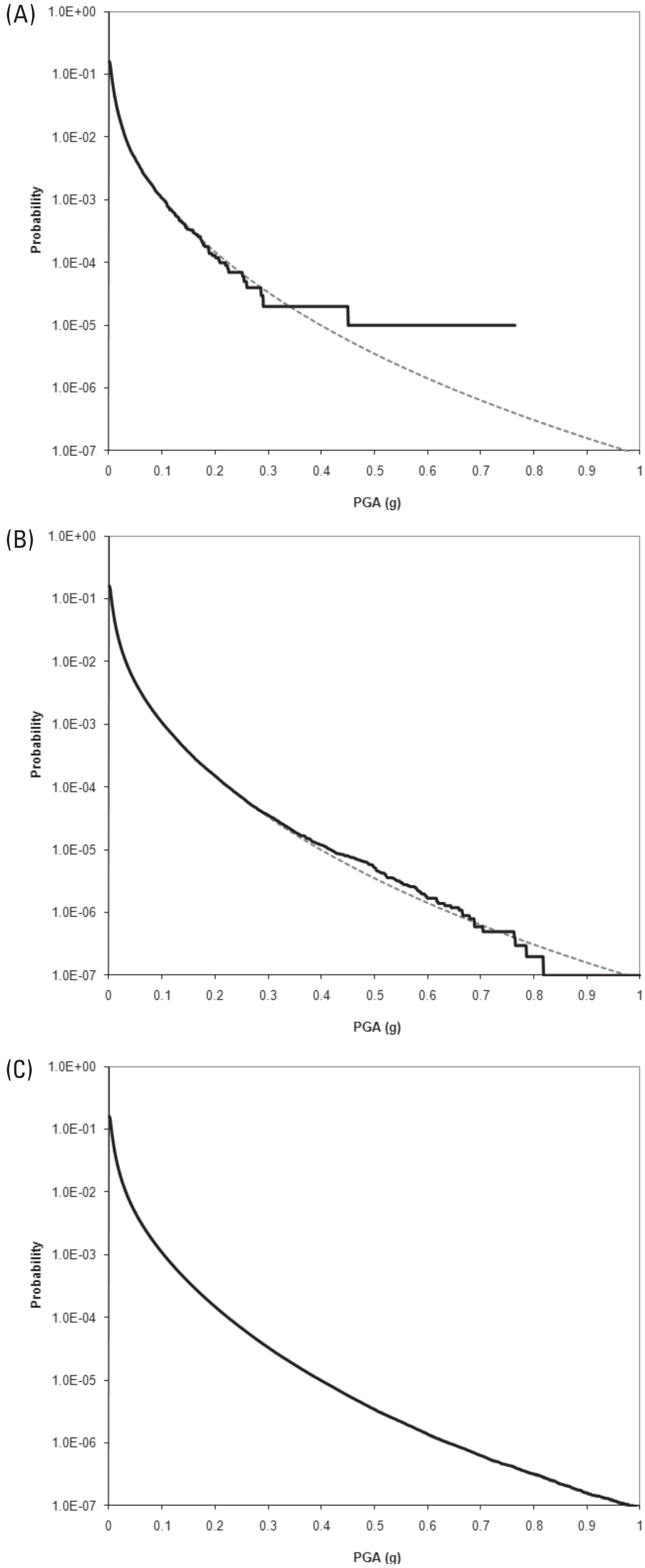
One 50-year catalog, with ground motions assessed at site for each event, represents one possible outcome of the seismicity around the site in the next 50 years that is compatible with 1) what is known about the properties of the regional seismicity and 2) what is known about the relationship between ground motion, magnitude, and distance. Obviously, the content of this single catalog owes much to chance, and reality may be quite different. But when one repeats the process a very large number of times, say 200,000 times, the result is 10,000,000 years' worth of pseudo-observational data, from which computing the probability of any result is as simple as counting.

In fact, this is as close to a purely frequentist approach to probabilistic hazard as one can get, as the simulated observations form a collective in the manner of von Mises (1957). In the study of probability one can observe a continuum between frequentism (probability is a counting exercise of things that have happened) and subjectivism (probability is an expression of belief that something may happen). From an extreme frequentist viewpoint, to assign a value to the probability of an earthquake occurring next year is meaningless, because there is no collective of "next year" repeated sufficiently often from which to count the results. A probability estimate from historical data is insufficient, unless all years are the same in every respect, which clearly they are not. But estimates from historical data are in practice rather useful, so the extreme frequentist position is really rather sterile (Musson forthcoming and references therein).

In the simplest case, epistemic uncertainty is discounted and simulations are based on a single set of assumptions. Since this is not very realistic, epistemic uncertainty can be introduced via a suitable probability density function. For example, in the simplest case, one could compute (according to some system) a maximum magnitude value for a specific seismic source. It is to be expected that the actual value lies near this estimated value rather than exactly on it. So the maximum magnitude might be described in the model as 6.8 ± 0.2, and in each simulated catalog, a value will be drawn randomly from this adopted probability function to govern earthquakes occurring on that source. In practice, it is usually simpler to discretize this as a logic tree, with a value drawn from the tree according to the weights of the branches in each simulation, but the process is conceptually the same.

However, the way a logic tree is treated is totally different in conventional PSHA and the Monte Carlo simulation approach. In conventional PSHA (Coppersmith and Youngs 1986), hazard is computed for each branch, given a number of hazard values equal to the number of branches. The final expected value is the weighted mean. In the Monte Carlo simulation approach, the individual branches are not evaluated in their entirety; all branches are sampled randomly, and a single hazard calculation made at the end. The total mass of simulated observations reflects all the possible outcomes that are implicit in the complete structure of the logic tree.

The two approaches, however, despite being entirely different and sharing no common procedures, give exactly the



▲ **Figure 2.** Reproducing a Cornell-McGuire study (dashed lines) with a Monte Carlo simulation study: (A) 100,000 years of "data," (B) 10,000,000 years of "data," (C) 1,000,000,000 years of "data."

same results for the same input models. It is impossible for both methods to include the same error when they operate in entirely different ways, and therefore they can only give the same results if 1) they include different errors that somehow consistently have exactly the same effect, or 2) they are both right. The first of these is not credible, which leaves the second. Thus, for instance, the argument in Wang *et al.* (2003) that PSHA results are invalid because the method depends on simultaneous rupture on different sources is clearly invalid. One can simply look at the simulated catalogs and find that they are just like natural earthquake catalogs. The hazard results can be seen to be derived from quite natural seismic processes, in that (at least in a well-constructed model) there is no detectable difference between the actual historical catalog and the set of synthetic catalogs used in the hazard calculation (Musson and Winter, 2011); provided, of course, that the same completeness constraints are applied. If Wang *et al.* (2003) were correct, it would not be possible for the simulation method to duplicate Cornell-McGuire results without simultaneous ruptures on different sources.

A specific example is shown in Figure 2. This relates to a model originally constructed for assessing hazard to a nuclear site in southern England (Seismic Hazard Working Party 1987). The original study employed a logic tree, seven source zones, and one fault source, and used a program called PRISK (originally based on EQRISK) to perform the calculations. The source model was reconstructed in the format used by the BGS in-house software M3C, which uses the Monte Carlo simulation approach. As one increases the number of years of "observation," the hazard curve in Figure 2 gradually converges with that calculated in the original study until the agreement is perfect. This agreement is achieved despite the fact that the calculation methods have nothing in common. Therefore, they validate one another.

METHOD AND MODELS

What the original study and its reconstruction have in common is not the method, but the model. Therefore any deficiencies in the results shown in Figure 2 have to be attributed to the model and not the method. It seems to be a common tactic in anti-PSHA literature to pick on some particular study, criticize results that depend on the model, and imply that the method is at fault. Castaños and Lomnitz (2010) is a recent example, the point of criticism being the very high accelerations at 10^{-8} annual probability calculated by the Yucca Mountain study (Stepp *et al.* 2001), as if it were foreordained that any PSHA study for Yucca Mountain would obtain the same result irrespective of the decisions taken by the modelers.

Thus, a common point of attack is the use of the Gutenberg-Richter (G-R) magnitude frequency distribution to describe earthquake occurrence, and more specifically the *b* value (see, among others, Krinitzsky 1993 and Klügel 2008). The reason why so many PSHA studies use this distribution to describe the rate at which earthquakes of different magnitudes occur is that it usually gives a good account of what is actually observed in the earthquake record. But PSHA does not depend on it. PSHA only requires some description of earthquake rates for different magnitude ranges. If any better representation of earthquake occurrence than G-R can be found, it can be used instead. Thus, Musson and Sargeant (2007) found that the seismicity of southern and central England could not be modeled adequately using a straight G-R model and used instead a composite model that demonstrably gives an adequate fit. The comparison of simulated catalogs with the historical catalog is a good test of the realism of any PSHA model; it should be demonstrable that the historical catalog lies within the population of possible catalogs that can result from the model (Musson 2006; Musson and Winter 2011). One can also mention the venerable PSHA program SEISRISK III (Bender and Perkins 1987), which simply calls on the user to enter the annual frequency of earthquakes in a number of bands. Whether these follow a G-R relationship or not is entirely left to the analyst. Wu *et al*. (1995) examined a range of hybrid models including characteristic events as well as time- and slip-predictable models, all of which can be used as alternatives to the more familiar Poisson/Gutenberg-Richter without needing major changes to PSHA software.

The same is true with the modeling of aleatory variability, another frequent target in criticisms of PSHA. It is clear that the same magnitude-distance combination does not always produce the same ground motion. Therefore, one cannot accurately model the hazard without including this. So much is self-evident. The question is how to do this. The commonest current practice, of using unbounded log-normal variability, described by a sigma value obtained by regression in a ground motion study using a large data set, could perhaps be improved upon. It is demonstrated by Atkinson (2006) that the true variability in ground motion due to path effects (which is what is really represented in the hazard calculation) is less than the sigma computed from a large data set involving many different paths. (In other words, the ergodic assumption does not hold.) However, reducing the sigma value arbitrarily loses some of the epistemic uncertainty (Strasser *et al*. 2009), since the sigma value from a large ground motion study actually compounds some epistemic uncertainty (variability between different paths) with true irreducible aleatory variability (due to slip distribution, path refractions, etc.). One does not know the characteristics of the actual paths relevant to the hazard at site, and these paths may not have the precise properties implied by the median of the ground motion model.

So present practice is not ideal, but it is better than anything else that has been proposed so far, which is why it continues to be used. The important thing, from the perspective of the present paper, is that aleatory variability is handled as a property of the model, not the method. If a better practice is discovered, models will start making use of it, but it will still be PSHA. And, of course, variations in practice already do occur from model to model and study to study. Changing the type of seismicity model or ground motion model does not make probabilistic seismic hazard any less probabilistic.

CONCLUSIONS

Attempts to prove that the Cornell-McGuire PSHA method is invalid founder on the demonstrable fact that the same results can be obtained by a completely different route; a route, indeed, which is so simple as to reduce PSHA to counting outcomes in simulations, in an approximation to illustrations of drawing balls from urns in classical probability texts. Since both methods cannot be at fault in such a way as to produce the same erroneous results, it follows that the results are not erroneous, but mutually confirmatory. Further, examining the mass of simulated earthquake data can be used to demonstrate that the method does not imply unrealistic behavior of seismicity, and also that the individual model is realistic.

Other criticisms of PSHA turn out to be criticisms of models rather than of the method. Often these seem to be criticisms of individual studies, from which inferences are drawn that all studies share the same flaws, which does not follow. PSHA provides a very robust method of studying earthquake hazard, capable of great flexibility in approaching the different problems that the hazard analyst is frequently faced with.

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British Geological Survey
West Mains Road
Edinburgh EH9 3LA UK
rmwm@bgs.ac.uk