Comment on “Why Do Modern Probabilistic Seismic-Hazard Analyses Often Lead to Increased Hazard Estimates” by Julian J. Bommer and Norman A. Abrahamson or “How not to treat uncertainties in PSHA”

Jens-Uwe Klügel
Kernkraftwerk Goesgen-Daeniken
4658 Daeniken
Switzerland
E-mail: jkluegel@kkg.ch (or jkluegel@msn.com)

Potential referees on the misrepresentation of my note to the editor related to the Baden-workshop (2004) are the other US-American participants in the Baden workshop (2004) as:

Dr. L. Mualchin, Prof. A. Hatheway, Dr. L. Reiter;

Referees on technical issues with knowledge of the PEGASOS-study:

Prof. G.F. Panza, Dr. P. Rizzo, Prof. V. Kossobokov;

Other referees are invited to come to NPP Goesgen for a more detailed familiarisation with the PEGASOS-study and the review results.

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Abstract: The paper discusses some heuristic biases with respect to the correct treatment of uncertainty in probabilistic seismic hazard analysis (PSHA) observed in the recent paper entitled "Why Do Modern Probabilistic Seismic-Hazard Analyses Often Lead to Increased Hazard Estimates" by Julian J. Bommer and Norman Abrahamson. It demonstrates that the distinction between aleatory variability and epistemic uncertainty in seismic hazard analysis is model dependent and represents a think model rather than an objective property of earthquake occurrence. It also shows that a mathematically correct treatment of uncertainty in modern probabilistic seismic hazard analysis (PSHA) requires consideration of all model parts (source, travel path of seismic waves, site conditions) with the associated random model parameters and their interdependency. Ground motion variability cannot be treated as an independent contributor to the overall uncertainty as suggested by Bommer & Abrahamson (2006). Such an approach leads to unrealistic results as confirmed by comparisons to empirical evidence. Furthermore, it addresses the fact that the methodology of traditional PSHA may lead to a violation of the energy conservation principle. Finally, a summary of some of the most problematic areas of current PSHA-methodology is given.

1. INTRODUCTION

In a recent paper entitled "Why Do Modern Probabilistic Seismic-Hazard Analyses Often Lead to Increased Hazard Estimates", Bommer and Abrahamson (2006) provided an interesting review of probabilistic seismic hazard analysis (PSHA) and expressed some ideas on how ground motion variability should be treated. Bommer and Abrahamson (2006) argued that "the main reason for the increases in the modern estimates of seismic hazard is that the ground motion variability in early application (and indeed formulations) of PSHA was not treated properly". In their paper they refer to recent studies and documents, which (at least with respect to the PEGASOS study, Zuidema (2006), Abrahamson et al (2004)) are not, or have not been, publicly available. Some recent discussions in "Engineering Geology" (Klügel, (2005a), Musson et al (2005), Wang (2005) and Klügel (2005b, 2005c)), which have revealed some problems using the approach supported by Bommer and Abrahamson (2006), are insufficiently appreciated in the discussed paper. I show that the distinction between aleatory variability and epistemic uncertainty in seismic hazard analysis is model dependent and represents a think model rather than an objective property of earthquake occurrence. A mathematically correct treatment of uncertainty in modern probabilistic seismic hazard analysis (PSHA) requires consideration of all model parts (source, travel path of seismic waves, site conditions) with the associated random model parameters and their interdependency. Ground motion variability cannot be treated as an independent contributor to the overall uncertainty as suggested by Bommer & Abrahamson (2006). Furthermore, I show that the PSHA-methodology as currently in use may lead to a violation of energy conservation principles and is therefore unable to provide a realistic seismic input for the design of critical infrastructures.

2. ON THE DISTINCTION BETWEEN ALEATORY VARIABILITY AND EPISTEMIC UNCERTAINTY

Before entering into a discussion about the distinction between *aleatory variability* and *epistemic uncertainty,* it is worth summarizing the definition of these terms. The definitions used in the PEGASOS-study (Zuidema, 2006, Abrahamson et al, 2004), which was sponsored by the Swiss nuclear power plant utilities, are as follows:

“Aleatory Variability – Uncertainty inherent in a non-deterministic (stochastic, random) phenomenon. Aleatory uncertainty is reflected by modelling the phenomenon in terms of a probabilistic model. In principle, aleatory uncertainty cannot be reduced by the accumulation of more data or additional information. Sometimes called randomness.”

“Epistemic Uncertainty – Uncertainty attributable to incomplete knowledge about a phenomenon which affects our ability to model it. Epistemic uncertainty is reflected in a range of viable models, multiple expert interpretations, and statistical uncertainty. In principle, epistemic uncertainty can be reduced by the accumulation of additional information”.

It is worth mentioning that from the perspective of modern risk analysis (Aven, 2003, 2005) such a strict distinction is artificial and misleading, because in general all our models and the results obtained as well as the conclusions, statements etc. are conditioned to the knowledge available. For instance, risk can be presented as a set of triplets (Klügel, 2005b, Klügel et al, 2006):

$$R = \left\langle H_i, P_i, C_i \right\rangle \qquad (1)$$

H_i represents the set of i events with possible adverse consequences. P_i represents the associated probabilities of these events and C_i represents the associated intolerable consequences on the object of investigation. People often forget (Aven, 2005) that the probabilities P_i used in equation (1) are a short note for the conditional probability:

$$P\left(H_i \middle| K\right)$$

where K is the knowledge or background information available for the analysis. This knowledge can change in the future, leading to new models and subsequently to different results of risk assessment. This means that in the end all uncertainties are epistemic. This discussion shows that “making the distinction between epistemic uncertainty and aleatory variability is not self evident, in fact the aleatory/epistemic quality is not an absolute attribute of uncertainty. Rather, it depends on the deterministic or stochastic representation that we make of a phenomenon. To the degree that the representation is compellingly deterministic or stochastic, so is the aleatory/epistemic character of the uncertainty” (Wen, et al, 2003). In other words, the separation between the epistemic and the random component (treated as aleatory) of uncertainty depends on the model. Because the definition of aleatory variability in the sense of an inherent property of earthquake occurrence is misleading, I suggest a more accurate distinction between the different components of uncertainty:

- epistemic uncertainty directly quantified in a probabilistic model (e.g. by propagating the uncertainties of input parameters through a “deterministic model” – for example an attenuation model), and
- a temporarily irreducible component (we are either not able to reduce this component of uncertainty by lack of knowledge or we don’t want to quantify this component directly to maintain our “deterministic model” simple) that we treat as random in our model.

Neglecting the model dependency of the separation between a directly quantifiable component of uncertainty (by uncertainty propagation through our “deterministic model” of the phenomenon) and the component that is treated as a random component, leads to a systematic error in the calculation process (Klügel, 2007). In the PEGASOS-study this model dependency was neglected. The paper by Bommer & Abrahamson (2006) highlights the heuristics why this could happen (N.A. Abrahamson was the team facilitator and integrator (TFI) of the subprojects 2 (Ground Motion Characteristics) and subproject 3 (Site Response Characteristics) and J.J. Bommer a key expert in subproject 2). It is important to note that the distinction between epistemic uncertainty (quantified via uncertainty propagation through a “deterministic model”) and randomness is not important for decision making as long as total uncertainty is captured correctly.

3. DISCUSSION ON THE MATHEMATICAL MODEL

Although Bommer & Abrahamson (2006) give an interesting review on the history of PSHA, they limit their mathematical discussion to ground motion characteristics (attenuation equation) and ground motion variability. Such a limited focus is misleading and insufficient to judge the validity of results of seismic hazard analysis. Modern PSHA, which is based on logic trees (SSHAC, 1997), attempts to address the uncertainties of all parts of a seismic hazard analysis beginning from source characteristics, modelling the characteristics of the travel path of seismic waves and including site effects. For this purpose a large amount of model parameters are introduced in the corresponding logic tree. These model parameters are treated as random (either by assigning different subjective weights to modelling alternatives by experts or /and treating the randomness by a corresponding statistical distribution for the parameter values). Therefore, a modern PSHA generally comprises a multivariate statistical problem. The aim of PSHA consists in the calculation of the probability (or frequency) of exceedance of specified ground motion levels at a specific site. The results of PSHA are typically represented as uniform hazard spectra, e.g. spectral accelerations for a specified probability of exceedance (SSHAC, 1997). As an intermediate step, the calculation procedure requires the estimation of the conditional probability of exceedance of a specified ground motion level . This conditional probability of exceedance of a specified value z (e.g. ground motion level) by a single component of the random variable $\left(X_1, X_2, ...X_i....X_n\right)$ in a multivariate statistical model is calculated as (Fisz, 1978):

$$F\left(x_n > z \middle| x_1, x_2, ...x_i, ...x_{n-1}\right) = 1 - \frac{\int_{-\infty}^{z} f\left(x_1, x_2, ...x_i....x_n\right) dx_n}{\int_{-\infty}^{\infty} f(x_1, x_2, ...x_i...x_n) dx_n} \qquad (2)$$

Here f denotes the probability density function of the n-dimensional random variable $\left(X_1, X_2, ...X_i....X_n\right)$. The X_i represent the random variables of our probabilistic model. The denominator represents the (n-1)-dimensional marginal distribution of the variables defining the conditions. Equation (2) illustrates that for the calculation of conditional probabilities the knowledge of the joint probability distribution of all random variables involved in the PSHA model is required. It is therefore incorrect to discuss the results of PSHA only in conjunction with specific ground motion models and the deviation of predictions derived from these models in comparison to the data. It is necessary to consider the dependency on the other random parameters of the PSHA model. The deviations between data and predicted ground motions may have been caused by some specific characteristics of the seismic source, which were not included as an explanatory variable into the ground motion model (model incompleteness, therefore an epistemic contributor to uncertainty). These source characteristic parameters and the associated uncertainty may have been included in another part of the logic tree. Therefore, their contribution to the ground motion variability (the standard deviation of the attenuation equation σ) must be eliminated in the PSHA model to avoid double counting of uncertainty in the logic tree model. This can be illustrated by the following example. The ground motion variability on magnitude and distance in the central and eastern United States was represented by EPRI (2003) as:

$$\sigma = \sqrt{\sigma_{source}^2 + \sigma_{path}^2 + \sigma_{\mathrm{mod}eling}^2} \qquad (3)$$

The uncertainty components σ_{source} and $\sigma_{\mathrm{mod}eling}$ are fully or at least partially reflected by the logic tree model (for large scale models like the PEGASOS-tree with 10^{15} retained branches it can be expected that they are included fully into the PSHA model); therefore, their contribution must be removed from the measured standard deviation before incorporation of the ground motion model into the logic tree. In case of a critical infrastructure, which has smaller dimensions than the characteristic dimensions of the seismic sources (or the surface of the fault rupture plane) even the contribution of σ_{path} must be adjusted because the spatial variability associated with different travel paths is significantly lower than in case of the development of a regional hazard map. The conclusion is that the ground motion variability observed by measurements and the variability to be modelled in a PSHA model are different items. In case of a complete logic tree model allowing to propagate the uncertainty associated with the different model parameters directly, the residual variability to be included in the PSHA model converges to zero ($\sigma \to 0$).

Similarly, it is discussed in Klügel (2007) that the ground motion variability observed in measurements and described by the residual term in attenuation equations mathematically represents an estimator of the total variability of the stochastic marked point process of ground motion observations at different locations triggered by the multivariate stochastic process of earthquake occurrence (in time). Therefore, in case of a validated attenuation model the total uncertainty modelled in a PSHA model should not exceed the total uncertainty of the (validated) ground motion equation. In dependence of the model the total uncertainty can only be distributed differently between the different parts of the PSHA model.

Let us discuss this point in more detail based on the mathematical model used by Bommer & Abrahamson (2006). They state:

"Ground-motion prediction equations can be expressed in the following form:

$$\log(Y) = f\left(magnitude, style-of\ \ faulting, dis\tan ce, site\right) + \varepsilon\sigma \quad \text{.....”} \ (4)$$

"where ε (*sic*) (epsilon) is the residual of a particular ground motion, measured as the difference relative to the median motion and expressed as a number of standard deviations" –. This text seems to contain a typographical error. What the authors probably mean is that

$$E = \varepsilon\sigma \qquad (5)$$

represents the residual of a particular ground motion. Nevertheless, the statement is quite revealing because in conventional mathematics (the mathematics used for science and engineering, but obviously not for seismology) the residual is defined as the difference between a measurement point to an estimate of the expected value according to the prediction model used. This definition can be found in any elementary text book on data analysis (Cook, 1982). The function *f* in (4) does not have the meaning of a median, but it is a regression mean and therefore an estimate of the expected value of the parameter $\log(Y)$:

$$f\left(magnitude, style-of\ \ faulting, dis\tan ce, site\right) = \tilde{E}\left(\log(Y)\right) \qquad (6)$$

A more accurate description in terms of stationary stochastic point processes would result (Klügel, 2007) in the definition that $f\left(magnitude, style-of\ \ faulting, dis\tan ce, site\right)$ represents an estimate of the mean function (a non-probabilistic function) of the stochastic marked point process of ground motion observations at different locations triggered by the multivariate stochastic process of earthquake occurrence (in time). This function delivers estimates of the expected value of $\log\left(Y\right)$ in dependence of source characteristics (e.g. magnitude M), style of faulting, distance and site characteristics (including topographical and directivity effects). All these definitions (even the incorrect one provided by Bommer & Abrahamson (2006)) show that the residuals R_i and therefore the "ground motion variability" depend on the model, because:

$$R_i = \log\left(Y_i\right) - f\left(magnitude, style-of\ \ faulting, dis\tan ce, site\right) \qquad (7)$$

The use of different models (or different regression shapes) for the function *f* leads to different values of the residuals, simply because the mathematical performance of these different models in comparison to the available data is different. Therefore, and contrary to the statements made by the authors, it comes as no "surprise" that some authors have been able to develop models with significantly reduced residuals. The reasons for this are manifold. The most simple explanations are a significantly improved classification of data (for example, separating hanging wall and footwall effects in case of normal or thrust faulting), the introduction of additional explanatory variables or even the development of "source to site" specific attenuation models. A better performance can certainly be achieved by using physical models that are able to capture multidimensional effects as well as interference and reflection of seismic waves (Panza et al, 2003). Such models are able to capture details of the travel path of seismic waves. It is also obvious from equation (7) that due to the dependency of the residuals of the ground motion equation on the model, the ground motion variability as observed is ***epistemic*** in the sense of the definitions provided in section 2. A final remark about the mathematical model: In equation (4) the erroneous interpretation of the function *f* as a median does not have many consequences, because for a large number of observations the probability distribution for ε can be approximated by a normal distribution with a zero mean and a standard deviation of σ. For a normal distribution the mean equals the median. The difference is of much greater importance with respect to expert judgment – the key element of the SSHAC procedures (SSHAC, 1997) for treating uncertainties. Confusing means and medians is one of the most widespread errors observed in expert elicitations (Kahneman et al, 1982). Assuming a consistent confusing of the mean with the median, the potential error in hazard estimates for such large logic tree models as in the PEGASOS study (the PEGASOS study was entirely based on expert judgement and experts' interpretation of existing information) can be as high as a factor of 50 (in terms of acceleration). In the PEGASOS project this effect was smoothed by providing an estimate for the upper bound ground motion level, thus assuring that the hazard cannot converge to infinity. In other studies (Yucca mountain project, see BECHTEL/SAIC (2004)) such an upper bound was initially not provided, which led to incredibly unrealistic results.

4. DISCUSSION OF PROOFS FOR THE EXISTENCE OF "ALEATORY GROUND MOTION VARIABILITY"

A key argument for the existence of "aleatory variability" in Bommer & Abrahamson (2006) is figure 1 presented on page 1968 of their paper. There the authors compare measured data from the Parkfield earthquake 2004 with different attenuation models and claim that the observed scatter represents aleatory variability, because the scatter cannot be explained by site conditions. The underlying assumption for this figure is that the models used for the comparison contain an exhaustive set of explanatory variables in the sense of an exhaustive deterministic model. This assumption is obviously not fulfilled. None of these equations

represents a multidimensional or a source specific model. None of them allows to model structural heterogeneities in the travel path of seismic waves. What do you expect from a model which is based on a simple one-dimensional (symmetric) amplitude decay approach if compared to spatially distributed (and therefore at least two-dimensional) measurements? Furthermore, the regression shape of most empirical attenuation equations is derived from the far field solution of seismic wave propagation in an elastic medium (Aki & Richardson, 2002). This approximation does not work in the near field and of course the associated scattering is expected to be large. The same applies for stochastic point source models, because in the near field a seismic source cannot be approximated by a point model. The finite dimensions of the source cannot be neglected. It is interesting to note that the authors are well aware of the model dependency of the "aleatory variability" of ground motion: "The large and apparently random (called aleatory, from the Latin *alea* meaning dice) variability in ground motion results from using very simple models for a very complex phenomenon." Unfortunately, Bommer & Abrahamson (2006) ignore this valuable observation in their analysis. My conclusion from this discussion is that it is more appropriate to call the discussed type of variability a (temporarily) irreducible epistemic uncertainty to make clear that the origin of this variability is epistemic and that it can be reduced in principle by further research. The SSHAC procedures (SSHAC, 1997, p.19) express a similar thought:
"It is evident, therefore, that the models used in the present analysis may be used only for a limited time depending on how sound their assumptions are". Future research may lead to the development of a new "model of the world".
Another example used as "proof" that the consideration of aleatory variability leads to an increase of the seismic hazard is a small sensitivity study performed as a part of the PEGASOS project. For the purpose of this study, the attenuation model of the original PSHA studies for the Swiss plants (a composite model derived from 3 "imported to Switzerland" attenuation equations) was used and the study was re-quantified assuming that the total

ground motion variability is “aleatory” and independent from the rest of the PSHA model. Indeed under this condition the calculated hazard using the FRISK88M® code increases. But is this proof that the obtained results are meaningful? Certainly not. The authors claim that I asserted “- without basis that the problem does not consist in the treatment of aleatory uncertainties but in the treatment of epistemic uncertainties”. This assertion raises serious questions about the scientific honesty of the authors, because my statement to the editor provided an explanation for this conclusion. The statement was (spell corrected): “The problem does not consist in the treatment of aleatory uncertainties, but in the treatment of epistemic uncertainties. The attenuation models used in the Basler & Hofmann study were taken from literature and derived from seismic recordings from many different areas of the world by statistical regression techniques. Accordingly, the laws used were not region specific as they would have been required to perform a site-specific analysis, because the region or site-to-site variability was not removed from the equations e.g. by calibrating them to regional conditions. As the benchmark checks performed as a part of the clients review demonstrated and as was confirmed by additional computations performed by PROSEIS under contract with Swissnuclear using FRISK88M® with the full input deck from subproject 1, this transfer of attenuation laws from other regions (or combining them statistically, declaring the actually calculated regression mean as being the median in further analysis) may change the results of a seismic hazard analysis by a factor of 2. This type of reducible (methodological uncertainty) was called – transfer uncertainty – in the presentation given by Dr. Klügel (*remark: at the Baden Workshop, 2004 quoted by Bommer & Abrahamson (2006)*). The key question of a site-specific hazard analysis consists in the use of regionally validated attenuation model – there the validation is performed on data. This task is not yet solved for Swiss conditions.”

It is obvious that here the discussion revolves around the question of how an imported attenuation model performs in the target area, and this is obviously a question of knowledge (an epistemic issue), because we were not able to compare the imported attenuation model with data from the region. The sensitivity study performed in PEGASOS *simply assumed* that the imported model was correct and could be regarded as a median of an aleatory uncertainty distribution. Therefore, the authors of the study (Abrahamson et al, 2004) assumed that the performance of this imported model is the same as for any other typical attenuation equation derived from regional data. There was and is no scientific basis for these assumptions. This is the reason why the sensitivity study performed is meaningless with respect to any judgment on the correctness of the PEGASOS results as well as for any other probabilistic seismic hazard study.

5. TREATMENT OF UNCERTAINTY AND ENERGY CONSERVATION

In their discussion Bommer & Abrahamson (2006) underline the importance of the consideration of extreme, rare time histories in seismic hazard analysis by providing numerous examples showing that the hazard results depend on the truncation level for ε and the value of the standard deviation in equation (4). Moreover, they claim that deterministic seismic hazard analysis (Krinitzsky, 2002) may be not conservative due to the limitation of the derived design spectrum to the regression mean +1 σ ($\varepsilon = 1$). This is certainly an important question, because it is undeniable that quite extreme time histories showing zero period accelerations above 2 g have been observed. But the observation of such time histories does not necessarily mean that they are important for engineering design. The reason for this is the existence of the energy conservation principle. Engineers know that energy cannot be created. Therefore, a low magnitude earthquake remains to be a low energy event despite the possible observation of time histories showing some spike acceleration values. One of the reasons for introducing the concept of magnitudes was to demonstrate to the public the different damaging effects of earthquakes with different energy contents. Customary engineering design methods are conditioned to the assumption that the seismic hazard input for their analysis contains only contributions from potentially damaging earthquakes.

Vanmarcke & Lai (1980) have shown that there is a trade-off between the observation of time histories containing spike acceleration values and the strong motion duration for the same content of energy measured by the Arias-Intensity (Arias, 1970). For the same Arias-Intensity the strong motion duration is nearly inversely proportional to the measured value of peak ground acceleration (see also the discussion in Klügel et al, 2006). The strong motion duration is a key factor affecting the potential damage of earthquakes especially for ductile failure modes. This can be compared with the stroke of a hammer against a stone wall. Such a stroke can lead to very high accelerations but may not lead to a damage of the stone wall due to the short impact time. The deterministic seismic hazard approach of using the mean + 1 σ spectral acceleration spectrum for design considers these effects. The selected design spectrum (regression mean of the attenuation equation +1σ) represents a statistically very rare spectrum. Using the model of a non-truncated lognormal distribution for the probability of exceedance of a specific ground motion level (spectral acceleration) it would require an uncertainty of $\sigma_{\ln} = 2$ (in natural logarithm scale) to ascertain that the mean seismic hazard of a PSHA exceeds this spectrum. For a comparison, the observed total uncertainty (within the meaning of equation (3)) in ground motion is in the range of $\sigma_{\ln} = 0.65 - 0.7$. In deterministic seismic hazard analysis this design spectrum is usually associated with an artificial time history with a strong motion duration corresponding to the magnitude of the design basis scenario earthquake(s) (additionally taking into account the distance between the critical seismic source and the site, and the site conditions) without considering the statistically observed reduction of strong motion duration mentioned above. This introduces another conservative element into the analysis. Of course more conservative assumptions are possible, but in terms of cost-benefit considerations, this is frequently not meaningful. It is also important to note that a contemporary deterministic seismic hazard analysis such as MCE (*M*aximum *C*redible *E*arthquake) does not make the claim to develop worst case design scenario earthquakes. The intention is to provide a robust design basis without exaggerating the importance of low energy events, as is a typical property of PSHA due to neglecting the energy conservation principle. Additionally, the authors claim that PSHA may lead to higher results than a deterministic analysis in dependence of the return period of seismic ground motion. This statement corresponds to the observed results of PSHA. But unfortunately, these observations are a result of the incorrect mathematical procedures applied in PSHA. First of all an increase of uncertainty with time is inconsistent with the modelling assumptions used in PSHA. In a recent round table discussion (discussing the PEGASOS-study), K. Coppersmith (Coppersmith, 2006), one of the key proponents of PSHA methodology and co-author of the SSHAC-methodology (SSHAC, 1997), ascertains that PSHA is based on an instantaneous seismo-tectonic model. On the other hand, PSHA (SSHAC model, SSHAC, 1997) is based on the assumption of earthquake occurrence as a stationary homogeneous Poisson process. Combining these two assumptions yields by logical inference that uncertainty cannot increase with time (at least not within the time period of the actual seismic cycle defining the minimal time period for the stability of seismo-tectonic conditions). The fact that contrary observations are made with respect to PSHA results is a consequence of the ergodic assumption (Anderson et al, 2000) replacing temporal characteristics of earthquake occurrence by spatial characteristics discarding the different physical meaning of these characteristics. Furthermore, it was established that the calculation of the conditional probability of exceedance of a specified ground motion level in the SSHAC model (Cornell-McGuire model, Cornell, 1968, McGuire, 1978) is incorrect (Klügel, 2007). The calculation procedure does not follow the mathematical laws of multivariate statistics (requiring the use of equation (2)). Furthermore, it is worth mentioning that the observation of rare time-histories is statistically compatible with a significantly lower mean hazard. This is the consequence of a well-known mathematical theorem called Markovs' inequality:

$$P\left[X \geq a\right] \leq \frac{E\left[X\right]}{a} \qquad (8)$$

Here X is a stochastic variable (e.g. a ground motion characteristic such as a spectral acceleration) so that $P(X<0)=0$ and $E[X]$ is the expected value of X, a is a possible parameter value of X . Let us assume that the hazard analysis resulted in an estimate for the mean hazard (an estimator for the expected value) in terms of peak ground acceleration (pga) of 0.2 g. Then the probability of occurrence of a rare spike acceleration of 10 g according to equation (8) is lower or equal to two percent. To my knowledge such high peak ground accelerations have not yet been registered. This example shows that the observation of rare time-histories, in general, is statistically compatible with a significantly lower mean hazard. The fact that the contrary is observed in PSHA results (Bommer & Abrahamson, 2006) - increase of the mean hazard as the result of increasing the truncation limits – is simply the result of incorrect mathematics utilised.

The current PSHA methodology (McGuire, 1978, SSHAC, 1997) find it difficult to comply with the energy conservation principle. The reason for this is that in the calculation process it is required to sum up the frequencies of all seismic sources contributing to the same acceleration level. This information is later used to construct the uniform hazard spectra or hazard curves by some reordering of the assembled calculation results. The problem here is that this summation is performed despite the fact that these frequencies from different sources correspond to different percentiles of the magnitude size distributions of the different sources and therefore to different energy levels. For example, an earthquake of magnitude 7 at a distance of 10 km to the site at a confidence level of –1 σ ($\varepsilon=-1$) can produce the same spectral acceleration as an earthquake of magnitude 5 at the same distance to the site at the confidence level +1σ . The contributions to the hazard (to the corresponding spectral acceleration) are combined although the energy content and therefore the damaging potential of the two earthquakes are very different. Considering the observations of Vanmarcke & Lai (1980) with respect to the relationship between peak ground acceleration and strong motion duration for a given energy level we must expect the strong motion duration of the magnitude

7 event to be longer than the average for the same magnitude, whereas the strong motion duration of the magnitude 5 event is shorter than the average for this magnitude. In dependence of the shape of the truncated Gutenberg-Richter law used for the magnitude-frequency-distribution, the frequency of the weaker event may be significantly higher than the corresponding frequency of the stronger event. Therefore, in PSHA we combine contributions from earthquakes with a completely different damaging potential into the resulting hazard curves. After the results are passed to engineers for design purposes they have to develop time histories reflecting the true energy content of the hazard spectrum. There is almost no chance of doing this correctly, because a uniform hazard spectrum represents the weighted sum of essentially an infinite number of contributing earthquakes (Wang, 2005). A later disaggregation of the uniform hazard spectrum into controlling events (in terms of magnitude and distance) does not help, because the disaggregation is performed based on acceleration (or probability) levels and in our example leads to the preferred selection of the weaker earthquakes, due to their larger frequency. Therefore, the scenario earthquakes developed from disaggregation may have too low of an energy content and the selected design scenarios may appear to be non-conservative. Indeed this effect was confirmed in a study by Chapman (Chapman, 1999). Therefore, we must consider the possibility of a (sometimes even non-conservative) violation of the energy conservation principle in applying PSHA results for the design of critical infrastructures.

6. COMPARISON WITH EMPIRICAL EVIDENCE

The key question with respect to the application of one or another seismic hazard analysis method consists in a measurement of performance of these methods by comparing them with observations (principle of empirical control). This question was completely ignored in the paper by Bommer & Abrahamson (2006). This is for good reason, because the performance of

PSHA according to the Cornell-McGuire (SSHAC) methodology is poor. A number of references can be found for this statement in literature.

Anderson and Brune (1999) performed a systematic comparison of the results of PSHA with “precariously” balanced rocks observed along the St. Andreas fault in the Mojave desert. They established systematic differences between the PSHA map of Siddhartan et al (1993) and the distribution of “precariously” balanced rocks. Stirling et al (2006) applied the theory of “precariously balanced rocks” to assess the validity of the New Zealand Probabilistic Seismic Hazard Map and found a deviation between the map and the PSHA results in activity rates corresponding to an overestimation of 1.3 σ. Viallet et al (2006) reported the outcome of a simple sanity check. They used recent PSHA results for France and combined them with a fragility analysis for industrial and dwelling structures to obtain an estimate for the expected number of deaths due to earthquakes over historical observation periods. They reported a significant overestimation (by order of magnitudes) of the calculated death toll to earthquakes in comparison to the historical facts. Klügel (2005a, 2005b) used a suite of simple sanity checks as part of a quality assurance acceptance procedure of the PEGASOS study results (Zuidema, 2006) and arrived at the conclusion that the results need to be rejected.

7. CONCLUDING REMARKS

The claim made by Bommer & Abrahamson (2006) that ground motion variability is of aleatory nature and represents an independent, inherent property of earthquakes has been proven to be invalid. The residuals of attenuation equations obviously depend on the model and therefore cannot be interpreted as an objective inherent property of earthquakes. The incorporation of “ground motion variability” as an independent from the model aleatory component would formally lead to an increase of the seismic hazard calculated by PSHA, but this is the result of an incorrect mathematical procedure violating some essential theorems of mathematical statistics. Examples and arguments given by Bommer & Abrahamson (2006) have proven to be incorrect. Based on some earlier discussions in *Engineering Geology (Vol. 82, 2005)* and the numerous findings of other authors (e.g. Anderson et al (2000), Wang (2005) and (2007), Viallet et al (2006) it is unavoidable to acknowledge that the traditional PSHA methods developed by Cornell (1968) and McGuire (1979) are approaching a fundamental crisis, because the deficiencies of these methods are becoming more and more apparent. The main problematic areas of this method include:

- the use of diverse ergodic assumptions (Anderson et al (2000), Klügel (2005), Wang (2006) leading to
    - mixing temporal and spatial characteristics of earthquakes,
    - an artificial separation of aleatory and epistemic uncertainties (due to the application of the de Finettis principle of exchangeability (Klügel 2005b)),
- the incorrect calculation of the probability of exceedance of a specified ground motion level violating theorems of multivariate mathematical statistics (Klügel, 2007),
- the neglecting of dependencies between random parameters of a PSHA model for example observed to a very large extent in the PEGASOS study (Klügel, 2005a, Klügel, 2005b, Klügel, 2005c, Wang 2006),
- the incorrect aggregation of expert opinions in the SSHAC procedures (SSHAC, 1997) by equal weights which assumes “infallibility of experts” in the sense that they are able to provide “biasfree” estimates (Klügel, 2005c),
- the problem of neglecting the dependency between the size distribution of earthquakes and the associated location distribution introduced by Cornell (1968), ignoring the geological dependency between these parameters (Klügel et al, 2006),
- the preferred use of the truncated Gutenberg-Richter-model (SSHAC, 1997) for the magnitude-frequency correlation, neglecting the dependency of the parameters of the equation on area and size of the objects of interest (Molchan et al, 1997) contributing to the problem of “b-line projections” (Krinitzsky, 1993),

- lack of time series analysis (Klügel et al, 2006),
- the preferred use of non-informative distributions for modelling random parameters in PSHA models (e.g. Gutenberg-Richter-equation, uniform spatial distributions for seismicity in areal sources),
- and, as was demonstrated in the present discussion, the violation of the energy conservation principles by combining the contributions of earthquakes with different energy contents into a uniform seismic hazard spectrum and hazard curves.

Against the background of these problems it must be concluded that the time has arrived to return to more meaningful approaches for seismic hazard analysis, as for example Molchan et al, 1997, Anderson et al, 2000, Krinitzsky, 2002, Klügel et al (2006) and to acknowledge the great progress made by deterministic analysis methods, which lead to transparent and reliable results.

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