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Dear Dr. Barnard,

Letter Report by P. K. Kaiser on the NWTRB Joint Panel Meeting on Seismic Issues in Las Vegas, February 24, 2003

1. Preamble

This letter report presents the review comments of a geotechnical consultant with expertise in rock mechanics and ground control, rock bursting and seismic loading of underground excavations. Hence, this report focuses primarily on questions and issues falling within this area of expertise. This consultant was previously involved in the Yucca Mountain Project as a member of a Drift Stability Panel and attended two workshops in Las Vegas in 1998/99. A panel report was submitted in May1999. Naturally, this report takes the findings of these previous studies into account when applicable.

For the NWTRB Joint Panel Meeting on Seismic Issues held on February 24, 2003, 28 documents[1] were received before the meeting and 12 printout packages of Powerpoint presentations were obtained at the meeting. Together with the 12 verbal presentations during the one-day Joint Panel Meeting, these documents form the basis for this report.

Four general questions were posed to the Board's consultants:

[1] Memo dated January 27, 2003: 23 Tabs; Memo dated February 5, 2003: 2 supplements; Memo dated February 5, 2003: 3 publications.

a) Are ground motions realistic and/or appropriate in light of intended use?

b) If not, what might be an alternative approach?

c) Are the approaches to seismic pre-closure and post-closure issues appropriate?

d) If not, what might be some alternative approaches?

The information provided before and at the February 2003 meeting was most helpful and has provided the Board's consultants with an excellent overview. A wealth of information was presented in a short time and it is understood that many of these documents are of preliminary nature and thus do not represent final conclusions or solutions. Consequently, the comments presented here deal with preliminary information and may loose their validity when new information becomes available or the adopted approaches are changed. Furthermore, it is evident that there is a huge amount of related information that was not accessible for this review. Hence, it was only possible to obtain a cursory insight into to the many aspects that have been or will be considered by the project team and this must be taken into account when reading this report. The following comments are based on a rather limited insight into an ongoing and complex planning, design and performance assessment process.

In addition to the four questions listed above, 12 specific questions were presented to the Board's consultants. Some of these questions cannot be answered fully because further information would be required or because they fall outside the area of expertise of the author of this report. Six of these questions are addressed later under respective headings.

2. Review comments

2.1 Are ground motions realistic and/or appropriate in light of intended use?

A tremendous effort is expended to develop pre- and post-closure seismic approaches. Overall, these approaches (e.g., as presented by M.B. Gross on General Post-closure Seismic Approach) seem to be sound and founded on established practice. The approach focuses on the right questions: How likely and how big will ground motions and fault displacements be? How much damage can be anticipated? What is the impact on the performance of the waste disposal system?

Unfortunately, relatively few concrete results, i.e. results demonstrating, for example, the impact on the engineered structures, were presented to evaluate whether this approach produces realistic and reliable results. As a matter of fact, the proposed approach has neither established what realistic results are nor how one would determine whether the results are realistic. From the discussions at the NWTRB Joint Panel Meeting on Seismic Issues, it would seem prudent to establish a procedure to identify results that are unrealistic. For lack of an accepted definition, the author of this report interprets the term "realistic and/or appropriate results" to mean "results that can be expected to be encountered and do not violate past measurements, observations, or current knowledge".

In other words, a rational approach has been proposed and adopted to produce realistic seismic analyses and design but it appears that no independent process has been put in place to determine whether this approach actually produces acceptable or realistic results, i.e., whether the results are actually possible. For example, if a probabilistic or an extrapolation approach produces an earthquake of a certain magnitude, an independent process should verify whether the geological structures in the vicinity of the predicted earthquake are actually able to produce such high ground motions (see also later discussion on ground motion capping and stress drop requirements). It is implied that a sound approach will produce realistic results. This is not necessarily the case. There may be components of a process that are not fully understood, inappropriately weighted, or in combination lead to unexpected results (e.g., when correlated parameters are treated as independent parameters). Hence, results of any process should be independently checked.

Furthermore, it is implied that conservatism at various stages of the proposed approach would produce conservative results. This is not necessarily the case or only if the adopted process does ensure that combined effects of conservative assumptions lead to conservative results (see also Section 2.2). Risking oversimplification, the following example is intended to illustrate how combining conservative assumptions may lead to unconservative results. By making independent conservative assumptions about the magnitude of the vertical and horizontal stress, i.e., by using for example the highest possible horizontal and vertical stress, the corresponding stress concentration around a drift would not be conservative, i.e. not produce the maximum possible stress. As a matter of fact, the combination of the highest vertical stress and the lowest horizontal stress would produce the most conservative stress concentration for the Yucca Mountain site. On the other hand, for conditions in the Canadian Shield, the combination of the highest horizontal stress and the lowest vertical stress would produce the most conservative stress concentration. While it is possible to clearly determine in this example what leads to the most conservative result (stress concentration near a drift), in complex processes with many interrelated factors, this is much more difficult. Hence, an independent approach to assess the validity of the result should be used to determine whether the results are realistic (as per above given definition).

Next, the question *"Are ground motions realistic and/or appropriate in light of intended use?"* is approached from two perspectives, from a cause for and an effect of ground motion perspective:

(1) Is the approach and are the assumptions about potential causes (earthquakes) leading to the proposed pre- and post-closure ground motions realistic?

(2) Are the proposed ground motions realistic from an engineering design perspective, i.e., for pre-closure, and from a performance assessment perspective, i.e., for post-closure?

The first issue is partially beyond this consultant's area of primary expertise. The earthquake specialists and seismologists on the NWTRB Joint Panel on Seismic Issues should address this issue. Nevertheless, the documents and presentations have identified several aspects that would suggest that the predicted ground motions are realistic for pre-closure but may not be realistic (too high) for post-closure performance assessment. This raises a series of questions that remain to be answered (listed below).

Whereas an impressive probabilistic seismic hazard analysis approach combined with an outstanding process of expert participation was utilized to obtain ground motion and fault displacement hazard results for pre-closure design and post-closure performance assessment, the resulting hazard probability distributions with mean and median spectral accelerations or peak ground velocities for post-closure of, for example, 14g and 5g or 7 and 3m/sec, respectively, at an annual probability of exceedence of 10^{-7} (at 10 Hz) seem to be very high, i.e., they seem to be much higher than one would anticipate from past measurements and physical observations from highly seismically active areas of the world. Based on the limited insight provided, it appears that the adopted process utilized valuable expert knowledge but then extrapolated far beyond the time-frame of applicability of this experience, i.e., far beyond the annual frequency levels constituting the base for the experience of the experts. These issues need to be properly addressed and resolved by expert earthquake specialists and seismologists.

The suggestion by Dr. Brune that the probabilistic seismic hazard analysis may not have handled uncertainty properly deserves immediate attention. His interpretation that systematic spatial variability may be resulting in an overestimate of randomness and, consequently, in larger uncertainties and larger ground motions at low probability should be investigated, addressed, and then integrated into the probabilistic seismic hazard analysis approach.

Furthermore, a number of issues are not sufficiently addressed in the documents received by the consultant or were not sufficiently explained during the presentations and the question period to form a well-informed opinion. The following issues should be considered and resolved. Why do the probability distributions not converge asymptotically toward a maximum acceleration? Isn't there a maximum possible earthquake for the given geological setting and thus a upper limit for physically possible ground motions? Does the extrapolation process take into account that the ground deforms plastically at large strains (during very large earthquakes) and limiting ground motions at an asymptotic threshold value? Or, does the adopted probabilistic approach properly account for system behavior changes during the transition from elastic or non-linear to plastic rockmass behavior? There seems to be little physical evidence in support the high, predicted ground accelerations (presentation by J.N. Brune). Has a realistic upper bound for ground motions been established?

In summary, this reviewer concludes from the evidence presented that whereas the proposed ground motions for pre-closure design seem reasonable, those for post-closure do not, i.e., they seem to violate some "past measurements, observations, or current knowledge". Even though, it appears that no one has yet provided a realistic upper bound for ground motions, further work to establish means to cap the maximum ground motion for post-closure performance assessment is highly recommended. This will instill confidence in the process and potentially have a significant impact on the costs of repository construction.

Sub-question: What can be learned from earthquake motions in mines that would help?

The second issue listed on the previous page, "are the proposed ground motions realistic from an engineering design perspective, i.e., for pre-closure, and from a performance assessment perspective, i.e., for post-closure?", can be addressed from a mining or civil engineering tunneling experience perspective. Most seismically active mines have installed seismic monitoring systems and some have installed strong motion sensors. The author is not aware of any case where low frequency ground motions generated by an earthquake or a mining-induced seismic event have generated ground motions, measured or inferred from rock ejection in tunnels or drifts, in excess of pga > 2g and pgv > 5m/s. Even these ground motion values are frequently questioned and considered unrealistically high. They are thought to be associated with localized ground motion magnification, directionality or geometric effects due to multiple mine openings.

Consequently, the proposed pre-closure ground motions are lower than maximum values measured in mines (due to rockburst or earthquakes) and the proposed post-closure ground motions are much higher than maximum values measured in mines. Therefore, a detailed analysis of earthquake (and rockburst) motions in mines might help to assess whether the pre-closure ground motions are realistic. Whether a detailed analysis of earthquake (and rockburst) motions in mines would assist in capping post-closure ground motions is questionable because of limiting time frames and event magnitudes. However, ground motion measurements from mines can be used to calibrate the numerical models used for drift stability analysis both for the adopted seismic pre-closure design and post-closure performance assessment approach.

Many mines today have full waveform systems with extensive, dense monitoring arrays. While the interpretation of seismic data in some mines is somewhat suspect, it can be shown that stress drops related to seismic events causing rockbursts (damage to underground excavations) rarely exceed 100 bar. The very high stress drops (>10,000 bar) required to match the projected post-closure ground motions therefore seem unrealistic and need to be justified.

On the other hand, total closures of mining drifts has been encountered in very strong rocks (>120 MPa) at relatively low peak ground velocities on the order of pgv = 1m/sec. Whether damage occurs depends on the static factor of safety or the stress level near the excavation before dynamic loading. The stress level is defined as the ratio of maximum stress near the excavation wall to the rockmass strength. Since it is relatively high at the Yucca mountain project (low rockmass strength relative in situ stress), it must be anticipated that relatively low peak ground velocities (possibly velocities exceeding 1 m/sec) can cause some damage to the planned drifts (as confirmed by drift stability analyses).

Again, ground motion measurements in mines and observed damage associated to these ground motions can be used to calibrate the adopted numerical models that form part of the adopted seismic pre-closure design and post-closure performance assessment approach.

Sub-question: What can be learned from ground motions related to nuclear testing?

Results and observation from the nuclear testing program could be used to calibrate the proposed pre-closure seismic design and post-closure performance assessment approach. Even though the type and intensity of ground motions (mostly p-wave), the frequency range, the boundary conditions, etc. are not identical, nuclear test results can be utilized to test the approach and to identify weaknesses in the approach. They can also be used to calibrate the adopted numerical models for drift stability assessment.

2.2 Are ground motions realistic and/or appropriate in light of intended use? If not, what might be an alternate approach?

As in all probabilistic approaches, it is essential that input parameter distributions and results based on models utilizing these parameter distributions be capped or truncated at limits defined by physically possible processes (e.g., compressive rock strength cannot be negative or greater the yield strength the strongest rock). It was not evident that the experts were asked to define limitations of their models or to define such physical limits. Hence, the expert consultation approach should be adopted to address these question and to resolve the outstanding issue of ground motion capping, i.e., to establish acceptable limits for individual model parameters and for results produced by these models.

Alternative approaches to establish realistic maximum ground motions, such as those presented by J.N. Brune and J.G. Anderson, should also be pursued and then integrated into the probabilistic seismic hazard analysis approach.

Role and limitations of conservatism

During the presentations, it was repeatedly pointed out that the recommended hazard probability distributions as well as the approach for seismic analysis were conservative and possibly extremely conservative. It was also stated that this conservatism should be acceptable as long as it was possible to arrive at safe solutions with these conservative assumptions.

This reviewer is concerned that over-conservatism or multiple layers of conservatism will lead to unrealistic results, as explained earlier, with two undesirable effects:

- If the conservatism stems from a lack of understanding, it tends to undermine the confidence in the process and proposed solutions, and
- Excessively conservative assumptions introduced new uncertainties because little or no engineering experience exists to verify the models for extreme events (e.g., how to calibrate numerical models to ground motions that have never been measured?).

In addition, as was pointed out by several questions, over-conservatism may lead to unpredictable and possibly unreasonably high costs, potentially, without a proportionally low level of risk. For a safe and cost-effective solution, credible and proven (numerical) methods must be used.

In the Drift Stability Panel report (1999), it was recommended to plan multiple lines of defense (as commonly adopted in dam construction). The concept of multiple lines of defense is a means to ensure that failure of one component does not lead to the failure of the system. The approaches presented, did not seem to include this concept in the repository plan. Introducing conservatism is not the same as planning multiple lines of defense. For example, if the drift stability analyses show (as presented) that drifts might collapse and rock would fill most of a tunnel at the location of failure, an appropriate line of defense would integrate an engineered, partial or full backfill as a protective element to enhance drip shield stability.

In summary, this reviewer is concerned that the adopted approach of using conservatism as a prevailing component of the process introduces uncertainties that do not lend confidence to the proposed approach.

2.3 Are the approaches to seismic pre-closure and post-closure issues appropriate?

As indicated above, this report focuses primarily on seismic pre-closure and post-closure issues of drift stability. Hence, comments in this section are exclusively related to drift stability.

Overall, the approach (as presented by M.P. Board on Seismic and thermal drift stability) utilizing computer generated rockmass models based on laboratory and field tests, ground motion time histories, and discontinuum codes (also recommended by the Drift Stability Review Panel) seem to be sound and founded on established practice. It was evident that significant progress was made in recent months in implementing a drift stability assessment process as envisaged by the Drift Stability Review Panel. Much effort is made to create numerical models that accurately and properly represent the two dominant rockmass types: non-lithophysal and lithophysal rocks. When combined with additional laboratory and field-testing, these models should be able to produce realistic estimates of rockfall sizes and volumes as well as impact forces.

However, several tools are being developed or modified for the specific rock units encountered at the site (e.g., 3DEC with discontinuous joints, and PCF for tuff). Whereas these codes are now widely used and are considered state-of-the-art numerical tools for rock mechanics, they are still, at least in part, under development. For example, ongoing improvements and adjustments are required to properly simulate brittle rock failure[2]. In other words, these codes have been tested on many examples (laboratory and field measurements) but they have not been fully calibrated for the failure processes encountered at the site. For example, these models do not automatically simulate tensile failure processes induced by the heterogeneity of the rock[3]. Whereas these codes represent the best available tools, they cannot yet be considered as tools of common engineering practice. In particular, they have not been calibrated to predict long-term behavior and have never been calibrated to predict rockmass behavior under extreme dynamic loading. Hence, it is essential that these models be extensively calibrated on conditions encountered at the repository (e.g., x-drift behaviour, large scale tests, heater experiments, etc.). Furthermore, they should be tested on nuclear test damage data and rockburst damage cases observed in mines with comparable rockmass conditions.

As discussed above, overly conservative assumptions may introduce new uncertainties. This is particularly true for seismic drift stability assessment in the post-closure stage. The extremely high ground motion assumptions imposed on the drifts lead to extensive unraveling processes that go far beyond what is typically encountered even in most burst-prone mines. Hence, it will be difficult to verify the numerical models because little or no experience exists with such high ground motions. Less conservative and more realistic ground motion assumptions would certainly assist in arriving at a credible drift performance assessment.

2.4 Are the approaches to seismic pre-closure and post-closure issues appropriate? If not, what might be some alternative approaches?

Recent studies of brittle failure near excavations in heterogeneous rocks have shown that conventional linear or curved failure criteria are not appropriate in simulating spalling-type failure processes near underground excavations (e.g., Cai et al. 1998, Martin et al. 1999; Kaiser et al. 1996, Kaiser et al. 2000; and Diederichs 2002). These studies show that strain-dependent strength models and failure criteria with convex upward failure criteria are required to properly

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[2] The fact that there is a distinct lack of test data in some of the rock units and for a scale that is relevant for stability assessment is in part responsible for these requirements to make ongoing adjustments.

[3] Whereas they can be used to simulated the behaviour of a given heterogeneity, say at a sample scale where the geometry of heterogeneity is known, they have not been fully tested to reliably predict the behaviour of a drift in a randomly heterogeneous rock mass. Note: an approach has been presented to predict block size in discontinuously jointed rock but this approach has not been widely tested. A similar approach to predict thermal spalling in a heterogeneous rockmass is required but has not been developed nor calibrated.

describe brittle failure processes in heterogeneous rocks near underground excavations. While the proposed approach and the adopted numerical models will produced meaningful results, it is highly recommended that models be adopted or develop that can properly simulate the strain-dependent tensile spalling mechanisms clearly observed at the test tunnels.

These new, sophisticated numerical models for predicting rock damage near excavations must be carefully tested and calibrated. As discussed above, nuclear tests and rockburst in mines provide an appropriate test platform.

Sub-question: Have the site conditions, including rock properties, been characterized properly?

The site conditions have been characterized by various means and to widely diverging degrees. While an extraordinary effort was made to determine seismic velocity profiles and to obtain shear moduli and damping ratios, it is noted that large parts of the repository site have not been investigated and anomalies between boreholes (at 700 ft) and tunnel measurements (at 1000 to 1150 ft) have not been properly explained. Shear moduli tests have only been executed in the small strain ranges (to 0.1%) and thus are not applicable for large events with large strain conditions (>1%).

Most disturbing is that very little information is available about the rock properties of the Lower Lithophysal Unit where most of the repository will be housed. The large scale slot compression tests and laboratory tests on 12" core will add some long overdue data and the proposed numerical simulation approach will go a long way to overcome part of this deficiency. However, it must be concluded that the Lower Lithophysal Unit is not properly characterized and that further testing will be required to establish its rockmass properties with sufficient accuracy for design and construction. Because the numerical models need to be calibrated for long-term behaviour (e.g., fatigue) and for spalling processes during thermal heating, particular focus is required on thermal and long-term properties as well on properties that assist in determining the brittle failure process of this heterogeneous rockmass (fracture toughness, variability in matrix properties, etc.)

It also seems that insufficient use has been made of the x-drift. Its performance must be accurately predicted (back-analysed) by numerical modelling.

Eventually, it will be necessary to select support based on a rockmass classification system. None of the systems in current use seems to be directly applicable and it may be necessary to develop a methodology that can be used during repository construction.

Sub-question: Are the models of drift stability (seismic and thermal) suitable and have they been used appropriately?

It is evident that significant progress has been made in model development (e.g., to simulate discontinuously jointed rocks as recommended by the Drift Stability Review Panel in 1999). These models (3DEC, PFC) are suitable for this investigation. Several areas requiring improvement have been mentioned above (brittle failure simulation, strain-dependent strength mobilization, and heterogeneous rock modelling).

Sub-question: Have the rockfall analysis … been appropriately modelled?

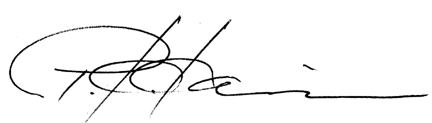
Again, the overall approach seems fine. Three minor deficiencies though have been identified: (1) the impact of failing support components (ribs) has not been simulated, (2) the effect of rock support (e.g., bolts) creating larger blocks has been ignored, and (3) the energy consumed in crushing and/or breaking blocks upon impact has been ignored. The relevance of these factors should be evaluated.

Sub-question: If the ground motion estimates remain the same, are there methods to mitigate potential problems in drift stability and repository operation and maintenance?

The implications of the predicted post-closure drift collapse in the Lower Lithophysal Unit does not seem to be reflected in the conceptual design utilizing a drip shield. If the openings are collapsing, the ravelling and failure process will continue until the rockmass bulking process fills the drifts. During this process, non-symmetric rock pressures will develop on the drip shield (in addition to the dynamic impact forces) and deform the shield. It is not evident that this mode of loading has been analysed. Considering the concept of multiple lines of defence, partial or complete back filling with engineered materials should be considered to control the ravelling process.

Overall, the presented information demonstrates that a highly sophisticated approach to repository planning is adopted. This report points at several aspects that may not have been fully addressed. It also concludes that the seismic ground motion assumptions, particularly for the post-closure performance assessment, do not seem to produce realistic results. Possible means to overcome some of the deficiencies identified in this report have been suggested where possible or questions that need to be answered have been posed.

Sincerely yours,



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