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Sandia National Laboratories

Albuquerque, New Mexico 87185

December 12, 1983

Ms. M. J. Wise
High-Level Waste Licensing
Management Branch
Division of Waste Management
U.S. Nuclear Regulatory Commission
7915 Eastern Avenue
Silver Spring, MD 20910

WM Record File A-1158

WM Project 10,11,16
Docket No.
PDR
LPDR B,N,S

Distribution:
M WISE
(Return to WM, 623-SS)

Dear Ms. Wise:

Enclosed please find a trip report that summarizes the activities during my attendance at recent meetings in Seattle, Washington (November 28, 1983) and Richland, Washington (November 29 - December 2, 1983). The meeting in Seattle was a pre-workshop meeting between NRC and its consultants. The meeting in Richland was a BWIP/NRC Workshop on Underground Test Plan.

Also enclosed are revised, written comments based on SNLA reviews of the BWIP Exploratory Shaft Test Plan Document (draft, SD-BWI-TP-007). Preliminary comments were sent to Dr. Nataraja prior to the workshop meetings. Mr. Mark Board (SAI) and I reviewed the document from somewhat different perspectives as reflected in our comments. Mr. Board has a background in in-situ testing, whereas my review emphasized the various analytical aspects of the geomechanics and thermomechanics issues.

If you have any questions or comments, please contact me at FTS 844-6268.

Sincerely,

Krishan Wahi

Krishan K. Wahi
Waste Management Systems
Division 6431

KKW:6413:jth

Encl: Trip Report

Copy to:

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Internal Report
Sandia National Laboratories

December 1983

Trip Report: A. NRC/Consultants Pre-Workshop Meeting: held November 28, 1983, at Seattle, Washington.
B. BWIP/NRC Workshop on Underground Test Plan: held November 29 - December 2, 1983, at Richland, Washington.

The following is a two-part summary report that describes the activities of two recent meetings with NRC. The first meeting was a pre-workshop meeting between the NRC and its consultants, and was held on November 28, 1983, in Seattle, Washington. The second meeting was a BWIP/NRC Workshop on Underground Test Plan, and was held November 29, through December 2, 1983, in Richland, Washington.

A. NRC/Consultants Pre-Workshop Meeting in Seattle.

This meeting was held to discuss and debate the preliminary comments resulting from a review of the BWIP draft Exploratory Shaft Test Plan (ESTP) document. Several NRC staff members and consultants had reviewed the ESTP prior to this meeting.

A major objective of this meeting was to integrate the various comments and discard redundant points. Clarifications were given by each reviewer on his or her comments. Agreement was sought on the statement of key questions that were to be posed later to the BWIP staff. After a morning session, the attendees were divided into two groups: 1. Geomechanics and, 2. Geology/Hydrology.

The Geomechanics group finalized a set of ten general concerns raised in the draft SCA document. Each participant in this group was asked to select two or three specific comments from his own review that were the most significant. These questions or comments would be brought up during the workshop, if time permitted.

The technical discussions during this meeting were very useful in clearing up some of the confusion and certain misinterpretations of the contents of the ESTP document. As a result, some comments were eliminated even prior to the workshop.

B. BWIP/NRC Workshop on Underground Test Plan.

Introductory remarks were made by D. Squires (DOE), P. Saget (DOE), and R. Wright (NRC) at the opening of the workshop on November 29. The remainder of the morning was devoted to presentations by DOE/BWIP personnel to give an overview of the Exploratory Shaft Test Plan (ESTP). The speakers included H. Dietz on Programmatic, T. Wintczak on Geology, R. Gephart on Hydrology, W. McCabe on Geomechanics, and R. Bielefeld on Constructibility overview. Copies of all the vu-graphs shown were handed out as notes. The possibility of a second exploratory shaft was mentioned by H. Dietz. He also compared the test schedules associated with one versus two shafts. The ESTP document did not contain any reference to a second shaft.

The NRC's preliminary comments in the areas of geology, hydrology, and geophysics were discussed during the afternoon session on November 29.

On November 30, the entire day was devoted to the discussion of geomechanics comments. As planned, the ten general draft SCA concerns were raised one-by-one by R. Nataraja (NRC). BWIP's response was sought on NRC's perception of whether these concerns had been appropriately addressed in the ESTP document. The BWIP/DOE staff seemed to be in general agreement with NRC's assessment. (The meeting minutes reflect this impression). Suggestions were made by NRC representatives on how to modify the ESTP. Individual geomechanics tests were discussed next. Each member of the NRC's geomechanics team was assigned to discuss two to three tests in his area of expertise. Many important concerns were raised on these specific tests. Due to time constraints, not all the questions regarding any particular test could be raised. One area that was not discussed (as it relates to testing) was that of numerical models. Future dialogue is desired to bring out performance assessment/modeling concerns since the test data will be used to validate, develop or calibrate many of the models.

The discussions of the first two days raised additional questions for the NRC hydrology team. As a result, they requested further discussion on the specific tests for hydrologic characterization. This discussion took place during the morning session of December 1. Some confusion existed among the BWIP staff as to whether they wanted to "take credit" for the barrier provided by the host rock (namely, Cohassett). NRC suggested that BWIP develop a defensible rationale for the proposed hydrology tests in the Cohassett, whether they wanted to "take credit" or not.

The afternoon session on December 1 was used to prepare lists of agreements, disagreements, open items, etc., and each side (BWIP and NRC) displayed its list(s). All members of the NRC team participated in preparing the lists and contributed in their respective field of expertise.

The Friday session was between the managements of NRC and DOE/BWIP. Meeting minutes were written, finalized, and signed in this session.

REVIEW COMMENTS

BY

K. K. WAHI

GENERAL COMMENTS AND RECOMMENDATIONS

- The test plan is very comprehensive and appears to address a majority of the issues in sufficient depth.

- A great deal of the review material (from this reviewer's viewpoint) is missing. Specifically, no information whatsoever is given on the numerical models or codes to be utilized, developed, or modified. An opening statement is, however, included in both volumes that promises the addition of a discussion on performance assessment models to these documents.

- It is unclear whether the media will be modelled as isotropic or transversely isotropic. Contradictory statements are made throughout the document regarding the number of elastic moduli that would be measured or derived. The differences (namely, two versus five moduli) are not trivial, either from a testing or a modeling point-of-view.

- Failure mechanisms are not addressed except in a very general way.

- The possibility (or the degree) of coupling of interactive phenomena is largely ignored. Vague statements that favor uncoupled models are made here and there. It is our belief that the close relationships among thermal, mechanical, geochemical, and hydrologic processes in a basalt repository environment warrant the following couplings as a minimum:

  Thermal-Mechanical
  Thermal-Hydrological
  Thermal-Geochemical

- It is not clear whether model development, model calibration, or model validation is expected from any given test. It would seem that most, if not all, of the models that are expected to be used are already developed but need validation (or calibration).

- No permeability measurements are included in the test plan. Effect of temperature on various hydrological, thermal, and mechanical properties must also be investigated, the current test plans do not address temperature dependence of properties.

- The high horizontal stresses can be expected to cause many problems during ES activities. The shaft liner should be monitored for strains and displacements throughout the testing period. It is not clear whether the variation of stress ratio as a function of depth will be characterized.

- Often an issue is raised that brings up certain questions. Instead of anticipating the questions and answering them in the same paragraph or sub-section, an answer is hidden away many pages later. Some reorganization of the document could be useful in that sense.

- It appears that no liner will be installed in the canister heater hole. Is this also true of the conceptual repository design? If so, how is the retrievability affected?

- No test plans are mentioned that would characterize the backing material or any other component of the waste package. Are such tests outside the scope of in-situ testing?

- The test results may show that the horizontal emplacement scheme is not feasible. It is recommended that certain tests be conducted regardless of what emplacement scheme is to be eventually utilized. Specifically, a vertical canister hole should be drilled in one of the drifts to compare with the horizontal hole response. Likewise, a modest program of room widening and pillar convergence monitoring should be included in the test plan. Data from such tests would be very useful in further justifying critical decisions.

- Experience of past and ongoing rock mechanics and heater experiments should be utilized. The ES Test Plan does not refer to experiments such as Project Salt Valult, Stripa, Asse, and WIPP. Even NSTF at BWIP is not mentioned in any significant way.

SPECIFIC COMMENTS
VOLUME I

PAGE #

p.1-6	Some geochemistry should be included under the "Exploratory Shaft" investigations. A fairly comprehensive discussion is, in fact, included in Volume II concerning tracer tests. What is the status of NSTF? When will that activity end?
p.1-7	A reference is needed regarding the "preliminary design criteria" for the repository Seal System.
p.1-8	It is stated that a starter hole (100 ft) has been sunk and the big hole drill rig has been assembled on its support pad. The decision to blind drill appears to be prematurely firm.
p.3-21	In Table 3-1, we recommend that another objective (I-7) be added: Repository Horizon. Confirm the suitability and superiority of the preferred horizon (Cohassett) or select an alternate horizon.
p.4-3	In Table 4-1, vertical hydraulic conductivity should be included among the needed data. Appropriate measurements should be planned during testing.
p.4-17	Geomechanics Characterization sub-objective (e) should encompass joint behavior in the context of model development and validation.
p.5-7	In paragraph 1, reference is made to "geoelectric layers below the basalt." Please clarify.

p.5-23 Does the distance of 10km in the third criterion apply from the nearest edge of the repository?

p.5-33 It should be possible to make some judgement on the geochemical nature of the fracture-filling material and how it is different from that of other horizons. The differences can strongly impact the long-term performance (e.g., radionuclide retardation).

p.5-39 What is considered a "rather long" test period?

p.5-46 It appears that the borehole for the heater test will not be lined. Is this also true of the conceptual design? If so, what assurance exists for retrievability?

p.5-63 The estimated maximum number of sensors functioning at a given time should be 3962, not 2962.

SPECIFIC COMMENTS
VOLUME II

PAGE #

p.2-21 Discontinuity data analysis is shown to include calculations of average aperture and spacing. How meaningful are such values, given that the models will not include fracture flow?

p.2-30 It is stated that estimation of the locations of intraflow structure will require thin section analysis. However, it is not mentioned whether such an analysis is planned.

p.2-43 The expected value of dense interior thickness is 45m; the predicted range is 35-45m. Shouldn't the expected value be intermediate rather than extreme?

p.2-44 Possibility of additional boreholes is mentioned. What would that do to the isolation capability of the site?

p.2-46 A specific geologic condition (thin interior of candidate horizon) is mentioned as requiring a contingency plan. Is this the only circumstance under which a contingency is required or is it just an example?

p.3-3 Table 3-1 shows no permeability measurements or tests. This appears to be a major oversight.

p.3-9 What numerical models are envisioned to evaluate the impact of data from ES experimentation of the waste isolation potential of basalt?

p.3-11 Item 1 points out the constraints on the type and number of tests that can be performed in (or from) the ES. How can they effectively evaluate horizons other than the Cohassett? Perhaps a different method of shaft construction would alleviate these problems.

p.3-19 Analytical solutions are referenced to perform transient and steady-state calculations. What about using numerical models that can simulate more realistic initial and boundary conditions?

p.3-27 Have any of the models that utilize a REV-like concept been exercised under changing stress and temperature fields? Does such a capability exist even in principle?

p.3-69 What is the relative magnitude of oscillations in hydraulic conductivity as progressively larger volumes of rock are tested? Are there data from the BWIP site that show this behavior?

p.3-70 Potential errors in numerical models could also occur from neglecting or oversimplifying coupled phenomena.

p.3-84 It will be very difficult to quantify the changes in "equivalent porosity" due to changing stress and temperatures. This is particularly important because none of the planned testing activity addresses any hydrologic measurements as a function of temperature and stress.

p.3-101 Last paragraph. It is assumed that a thick entablature unit will be present at all test locations. This is unlikely to happen.

p.3-136 What is the basis for assuming an effective porosity of 0.001 to 0.0001?

p.4-8 Paragraph 3 of 4.1.3.2.2. It is not clear what is meant here. The wording appears to be backwards. What do they intend to do if the test site is not "representative of the proposed repository location?" Changing the test site does not alter the geologic characteristics of the overall site!

p.4-12 With respect to artificial support (rock bolts), are the numerical models capable of modeling such support systems? Or, is it not planned to model the support?

p.4-18 Back computing of rock mass deformation moduli will not be easy if significant anisotropy exists.

The paragraph on Block Tests seems to say that the instrumentation system at NSTF was (is) unnecessarily complex. Are there other similar lessons to be learned from the NSTF experience?

p.4-20 Paragraph 2, here (and elsewhere) no detail is given on the nature and sophistication of the models to be used. Will these models be linear elastic, elastic-plastic, elastic-fracture etc?

p.4-21 The term "strength" can mean many different things. Do they mean yield strength, compressive strength, tensile strength, shear strength......? Definitions are needed for strength, failure, stability, etc.

p.4-23 Last paragraph, considering the discing problems experienced at the BWIP site, it will be difficult to obtain "relatively large" core samples. Further, laboratory measured joint properties are bound to differ substantially from those in situ. What correlation techniques will be used?

p.4-24 Last paragraph. The alleged relationship between new fractures and high-temperature creep is not clear.

p.4-27 What is meant by "overstressing?" What is an iterative stress redistribution approach?

Last paragraph. On one hand, the spacing between rock mechanics drift 1 and the mine-by drift is chosen to "minimize interference", on the other hand instrumentation is to be installed from drift 1 "into the influence zone of the future mine-by drift." Is there a contradiction here?

p.4-28 Line 5, "----shown in Section 4.3.4.9.1----" should read, "----shown in Section 4.3.9.4.1----". Same correction applies to line 7 on p.4-30.

p.4-33 The canister scale test makes no mention of the back-fill or other components of the waste package.

Under 4.1.3.9.1, the development of canister and room-scale models is a stated purpose of the test. One hopes that the models would already be developed and the experimental data would help validate the model.

p.4-34 Over what distance (or rock volume) are the enchanced gradients envisioned?

p.4-37 Section 4.1.3.10.4. Only thermal-mechanical coupling is explicitly mentioned. Do they not envision thermal-hydrologic coupling in the repository design?

p.4-59 Last paragraph, lines 3 and 4 make no sense.

p.4-63 If thick shotcrete application is required, what are the implications on the actual repository construction? Is it feasible on such a large scale?

p.4-101 The values of thermally induced stresses in Table 4-5 are presumably for the canister-scale test. What are the values for the conceptual repository design with the long horizontal hole with several canisters?

p.4-127 The cross-hole seismic test data will yield dynamic moduli which tend to be substantially higher than the static values. Also, will there be an attempt to measure five moduli appropriate for a transversly isotropic material?

p.4-129 The equations for E and assume an isotropic medium. Please clearly state the assumptions regarding isotropy.

p.4-139 First paragraph suggests that the medium will be treated as anisotropic.

p.4-161 Last paragraph. The assumption of no pillars is valid only if horizontal emplacement with very long distance between drifts is the design. The possibility of a vertical emplacement scheme has not been ruled out. Why not design some pillar and room widening tests in case the ES data are more favorable towards a vertical emplacement design?

p.4-170 Under stability monitoring, what type of rock mass failure criterion is anticipated?

p.4-172 Under rock property monitoring, will there be any attempt to qualitatively or quantitatively define joint dilation?

p.4-173 How strong is the dependence of deformation modulus of basalt on confining stress? Is there any specific evidence of that dependence for the Cohassett flow?

With respect to agreements between predicted and measured response, are any benchmarking activities planned? Both WIPP and ONWI have conducted benchmarking studies in the past.

p.4-188 The anticipated deformation of the crown and sidewall of the canister hole are not given in Table 4-5 as claimed.

p.4-200, 201 Under hydrologic properties section, no measurements of potential temperature dependence are planned.

p.4-204 Hydraulic conductivity and thermal expansion data are not given in Table 4-5 as the paragraph claims.

p.4-205 The expected increase in sonic velocities is likely to be compensated in part due to a decrease in the intact-rock modulus with increasing temperature.

p.4-228 Fracture-free intervals that are amenable to hydraulic fracturing may not exist given that tens of fracture per meter are common.

p.5-8 The last paragraph under 5.2.2.2 is confusing in that a concern is expressed with respect to the contact between the liner and the shaft wall. The original hole will be bigger than 6 feet and the annulus between the shaft wall and the liner will be filled with cement/grout. Can't see why contact between the liner and the shaft wall would be difficult to achieve.

p.5-15 What is the resolution of the various devices proposed to measure the shape and diameter of the shaft?

Comments on the Canister Hole Drilling Test

- It is stated on p. 4-184 that, "This orientation is expected to eliminate the need for support in the hole." However, the minimum principal horizontal stress is still 40% higher than the overburden. This means a differential stress will still exist that will tend to destabilize the hole. Whereas we agree that the need for support will not be as great as for the less favorable orientation, it is by no means eliminated.

- Deformation modulus versus radial distance data are planned to be obtained from cross-hole seismic tests. This does not appear to be a very realistic expectation, particularly in light of the last line of paragraph 4.3.10.8.

- A concern is expressed on p. 4-190 about gravity induced cave-in of a horizontal hole. The differential stress due to a 3:2 stress ratio ($_{H1}$: $_{H2}$) in the vicinity of a 30" diameter vertical hole should be of equal or higher concern with regard to cave-ins.

- As a contingency, why is the alternative of a liner to stabilize a hole not given a consideration?

Comments on the Canister Hole Heater Test

- Given that the rate of heating is designed to result in a canister hole wall temperature of 500°C, some very interesting fluid flow patterns should occur. Many attempts have been made to analytically predict existence of convection cells, two phase flow and such. Why doesn't the test plan include some qualitative observation of such phenomena?

- The planned measurements of hydraulic conductivity before and after heating should yield important information on the effects of thermal cycling. It is puzzling why no such measurements are envisioned <u>during</u> heating to determine the temperature dependence of hydraulic conductivity as well as other properties.

- Reference is made to the use of a high-temperature borehole television camera. Does such equipment function well (or at all) at such high temperatures as 400 or 500°C? Has the camera been used at NSTF?

- The temperature dependence of deformation modulus will be a complex combination of softening due to heating of the intact rock and stiffening of rock mass due to joint closure resulting from thermal expansion of the rock.

- A 250% temperature change may grossly overestimate joint alteration.

REVIEW COMMENTS

BY

M. P. BOARD

SNLA REVIEW of BWIP'S DRAFT EXPLORATORY SHAFT
TEST PLAN - REVIEW SET B

Major Comments on Hanford Test Plan

1. The test plan is well written and organized. It is reasonably easy to follow and, at least at the outset, attempts to relate needs to individual tests. The plan is, however, voluminous. One almost needs a shorter version to simply plow through the material.

   The basic testing program here seems quite standard and involves very little which has not previously been performed at this or other sites. This is not bad, but it appears as though little improvement has been made in testing technique after several years of experience.

2. The start of the test plan should contain a series of flow sheets which show the various stages of information gathering leading to repository qualification and design. Various sections of the flow sheet will be expanded, leading to the necessity for the various tests here. The present method of presentation of test needs is poor, and it is difficult to see where in the total program the data is required, and what particular problem the testing will solve and by what method. The rock mechanics portion attempted to do this, and it consequently improved my understanding.

3. There was little, if any, discussion of how the present program interacts with the past seven years of testing at the Hanford Site. Nothing was discussed about what was learned and applied from the near surface test facility. The question must be asked - "How was this data/experience used in designing or specifying these tests?"

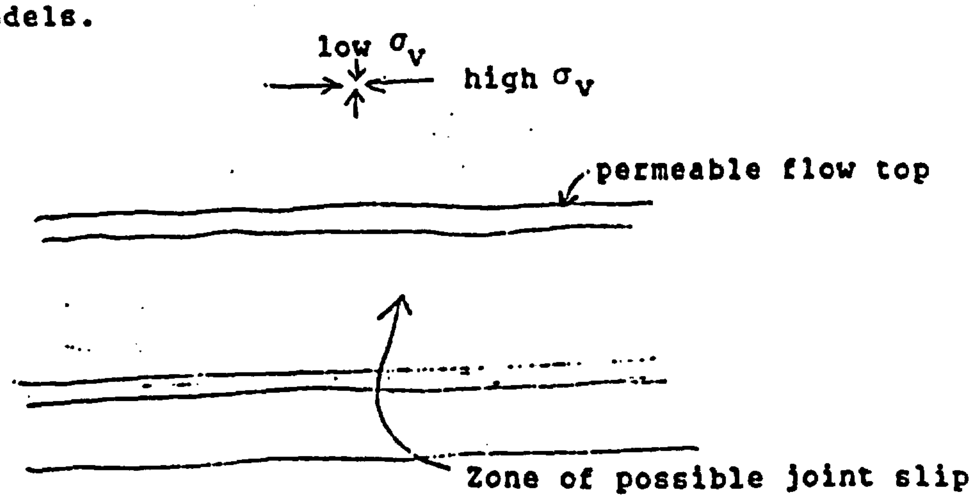
4. In the Volume 1 site selection discussion, little mention is given to relationship of the geotechnical features of the principal borehole to the other Hanford Site holes. I am particularly curious how geophysical, hydrological, etc., data compares to holes away from the RRL. This is aimed at the continuity of the Cohassett flow and its variability. For over five years, the Umtanum flow was prime, and was suddenly dropped based on the principal borehole information - is this true?

5. The stress magnitudes (σ_{Hmax} = 9,000 psi) and ratios σ_H/σ_v up to 2.75 are very disturbing. These stresses, added to the brittle nature of the basalt could mean relatively bad ground problems. This is already evidenced by discing and borehole stability

problems on site. The shaft constructability section basically ignores this potentially severe problem, which could result in significant spalling and possible rock fall in the shaft as it is being sunk.

6. Compounding the problem in #5, there was no program given for monitoring the structural stability of the shaft and shaft liner. Surely, this is warranted based on the stress state and heavily jointed nature of the material.

7. The hydrology program does not address the problem of excavation -- induced changes in permeability. As the repository is advanced, the vertical and horizontal stresses are redistributed, resulting in a highly deviatoric state over the repository. This can result in slippage (shearing) along fractures above the plane, which can conceivably move upward as the repository is advanced. When the hydrologic integrity of the repository is based on confinement to a very thin (40m ±) layer, this action of shearing in the "relaxed" zone could be very significant and some attempt should be made to examine its effects for inclusion into models.



Note that the above problem is particularly important in the basalt program since the horizontal stresses are so high. Perhaps this problem is examined elsewhere in the program, but it would seem that the first underground excavation is a good place to make these measurements.

8. There was no discussion in the hydrology section on how field testing can help to define permeability of fractures under temperature and stress fields. The flow here will be primarily fracture flow. Several other field tests have shown order of magnitude changes in permeability as functions of stress and

temperature. These must be accounted for in fully coupled transport models; field data are required. Why is there little apparent interaction between the hydrology and geomechanics programs?

Geomechanics

9. Continual and obvious reference is made to possible spalling and rock bursting as though they were standard features in an excavation at 3,500 ft. They are not. It brings out a question as to: (1) is the basin tectonically stable? and (2) will these high stresses adversely affect drift stability when thermal loadings are added to the mechanical stresses?

10. General comments on the geomechanics section. An attempt was made to orient the reader toward why these tests were needed, and how the data would be used. This is appreciated. The great number of tests suggested here seem unwarranted for the ultimate use of the data. Some of these tests, in my opinion, will only end up proving well known theories, such as the stress distribution around a hole, without adding greatly useful data that can help in the overall program. For example, there are seven separate tests or techniques given which can potentially measure the deformation modulus. Of these seven techniques, several are repeated many times to explore variability. This is a great number of tests to measure what should be a simple quantity to examine. Another example:

    Plate Bearing Tests, six total (three vertical, three horizontal) -- all to measure the deformation modulus and possible fracture creep.

    Flatjack or Rocha Slot, eight total, measure same as above.

    Borehole Jack -- measure same as above on small scale.

    I would suggest they re-think their need to measure basic in situ properties such as:

    1. The stress-strain behavior of bulk rock, thus calculating the deformation modulus. If transverse isotropy is truly important, design a flatjack test which will monitor deformation and load in mutually perpendicular directions. Or, do two plate bearing tests.

2. Determine shear strength and shear stress-displacement and normal stress-displacement properties of fractures for inclusion in models.
3. Thermal expansion coefficient and how it changes as a function of temperature.

A few well-planned tests such as the plate bearing and triaxial block test will provide this information without the tremendous expense and time of all those suggested in the plan.

11. The measurement of stress along the periphery of the openings seems unwarranted. What will be gained?

12. The mine-by experiment was a plus. I feel it is well conceived and could have beneficial results. I was particularly pleased at the discussion of attempting to model the situation with numerical methods in an attempt to predict the results. I feel it would be very nice to try and predict this with three model types:

    1. Discrete element
    2. Finite difference and/or finite element
    3. Boundary element

    with varying degrees of detail in the constitutive models. Examine which code predicts results closest. In this test should be added a borehole from one drift to the next which would be used for borescope observation of fracturing and for a drift-to-drift convergence control measurement.

13. The terms "failure" and "strength" must be defined, as they mean different things to different people. The term "overstressed" has no meaning.

14. The canister-scale heater test is justified by the necessity of measuring thermal conductivity. Of all in situ properties, this is the one we <u>can</u> accurately predict. Instead, they should be emphasizing thermal expansion, which can be a highly nonlinear function of temperature, and is not well known.

15. The triaxial block test should incorporate fracture permeability measurements as functions of confining stress and temperature. This is the ideal "coupling" between the hydrology and rock mechanics programs. This data can be used in fully coupled discrete element or difference codes for transport prediction.

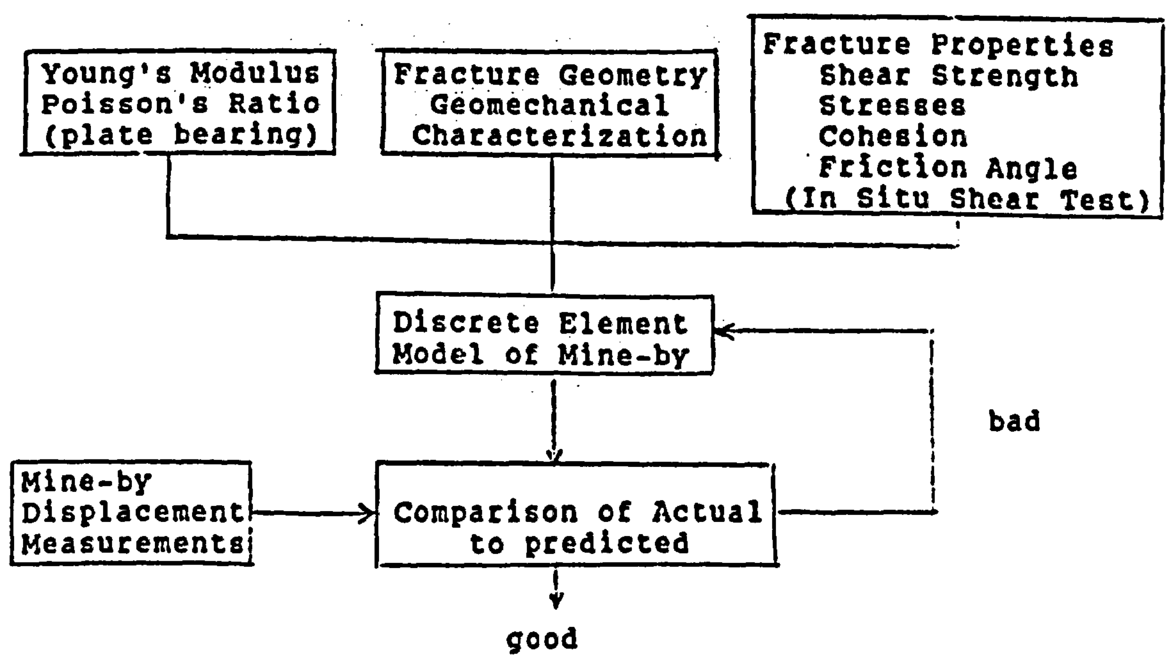
16. The acoustic emission experiments were a welcome addition.

17. The proposed use of several different techniques for stress measurement is well thought out and should be included.

18. The people should be aware that all of the instrumentation in the breakout area will likely get blown away due to the blast shock. A better idea is removable displacement tools such as portable deflectometers; though less accurate, they will not be destroyed.

19. Too bad the room-scale heated test was not discussed in more detail. I am cautious here, as small instabilities such as a rock falling from the back could be interpreted as "failure," which could seriously damage the entire program. This experiment must be well thought out.

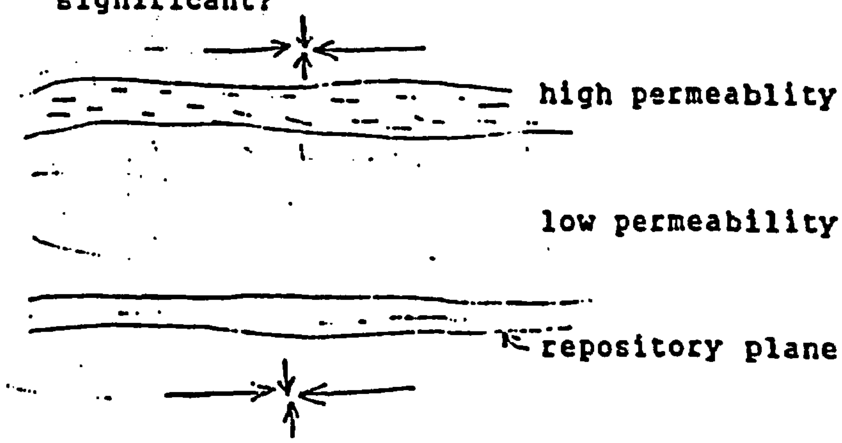
20. The terms "failure criteria" or "strength criteria" were often mentioned, but the testing discussed gave no concrete methods as to how in situ measurements would lead to a general failure criterion. Instead, an approach should be taken in which in situ measurements of the shear properties of fractures should be made, followed by inclusion in a general failure law (e.g., Mohr-Coulomb) which would further be used to predict the mine-by experiment:



Specific Comments on Volume I

PAGE/PARAGRAPH	COMMENT
3-2	None of the sub-objectives mentions the need to monitor shaft and grout seal mechanical stability with time. With heavily jointed rock under obviously high horizontal stresses, sliding along fractures and shaft creep can be seen. This could affect shaft integrity over long time periods, and should be included.
3-3 Hydrology	Nowhere does it discuss how the properties measured in situ at shaft relate to risk assessment models performed over the past several years based on surface boreholes. i.e., can the same models be used, but merely new data replaced?
3-4 Geomechanics	Are the thermo-mechanical tests simply confirmatory (of near-surface results), or are they intended to be used to develop site-specific constitutive models?
4-3 Rock Material Properties	Porosity (connected, nonconnected) shear strength-fractures? Since when is in situ stress a rock material property? No geophysical logs in primary borehole?
4-3 Geology Data	Intra-flow structures? Thickness and geologic description -- porosity, brecciation, etc.?
4-3 Discontinuities	Infilling material. How can aperture be obtained from nonfilled fractures?
4-4 Discontinuity Properties	Rock mass classification. Does moisture content refer to infilling materials?
Tectonic Features	Why are width, offset, etc. referred to as tectonic features? Isn't this simply a euphemism for faulting?
4-5/pp1	Shouldn't a geophysical survey be made in the principal borehole simply as a means of confirming the surveys from other holes as well as obtaining a signature of the flow in question to make sure it is recognizable in other holes? i.e., one

PAGE/PARAGRAPH	COMMENT
	cannot always tell one flow from the next merely by physical inspection, but must rely on the geophysical signature.
4-5	Poor statement of justification. Should simply state that actual site-specific measurements of fracture properties are necessary for modeling and design. Quality of data has nothing to do with it. Instead of tectonic features it should be straightforward and say faulting, etc. This is confusing as folding is also tectonic. Please define tectonic feature, i.e., is a blowout structure considered tectonic? Is high horizontal stress considered a tectonic feature, etc.?
4-6/last	Shouldn't the ability to drill long holes and thus sample continuity and variability of formation over large areal extent be important?
4-10	No planned tests are given to measure effective porosity, dispersivity and hydrochemistry. Hydrochemical type is confusing.
4-12	Nowhere is there a mention of an attempt to study or quantify enhanced permeability due to excavation and the readjustment of stresses around the repository. Could this not be highly significant?



highly deviatoric stresses cause slippage on joints at various orientations

PAGE/PARAGRAPH	COMMENT
	possibly enhancing ground-water flow to the interflow zones? How will this be modeled -- what data must be gathered to substantiate? Is this not a data need?
4-14	Again, the justification can simply be made that data on rock mass properties and excavation must be site specific.
4-14/last paragraph	Hopefully, the initial design will be sufficiently conservative that personnel danger is not of great concern.
4-16	The testing <u>does not</u> prove that the excavation does not adversely affect isolation capability over the operational life. There is <u>no</u> mention of hydrologic isolation.
4-16	If one has a large flatjack test, why a plate bearing test -- don't they yield the same information?
4-16	If the only thermo-mechanical test data needs are thermal conductivity, one might as well forget the heater test, as it has been seen that this is the easiest of properties to predict.
4-18	There is no mention of the very great reasons for need of in situ stress data -- the evidence of core discing indicates unusually high stresses, and possibly an active tectonic region?
4-19	If we know the in situ stresses, the approximate excavation-induced stresses are calculable and known theoretically. The measurement, in my opinion, is not necessary.
4-20	What is "overstressing"?
4-21	Comment on why simply applying levels of uncertainty from near-surface or laboratory measurements is not appropriate.
	Please define "failure:" does it mean violent, as in rockburst? Does it mean

PAGE/PARAGRAPH	COMMENT
	slippage along fractures -- nonviolent? Is it controllable?
4-22	Of all parameters which have been easily determined in situ, and which compare most favorably with lab measurements, is the thermal conductivity. There is really no justification in additional measurements in situ. Instead, the measurement of thermally induced displacements has been a far more thorny problem (e.g., at Stripa). There is justification in determining the in situ thermal expansion coefficient as a function of temperature, but not thermal conductivity.
4-22	Constructability -- Does not mention the possible need for use of various blasting techniques should one induce too much damage.
4-24	I am most concerned about an early indication of two things during shaft construction: • The water inflow and pressure of the formations drilled. • The stability of the shaft walls. The stability of the shafts is to be measured via "geophysical techniques." How is this possible? The only indication I think is reasonable is direct physical observation. Finally, the shaft liner and grout must be monitored for strain over time to insure we understand its stability.
4-25	How will grout implaced quality be determined?
4-26	How will oil in drilling affect the hydrologic measurements?
4-27	Why is hydraulic inflow not considered a constructability criteria?
5-2/table	Does hydrology have nothing to do with constructability?
5-2	Shaft liner sealing has no data for hydrology or geomechanics.

PAGE/PARAGRAPH	COMMENT
5-23	What about tapped aquifers above the repository? What about areal continuity as seen from correlation to other holes in the Pasco Basin? It's as though all other criteria based on years of testing are eliminated for these three criteria.
5-27	Horizontal stress is of great concern; note that for Cohassett Flow, the ratio $\frac{\sigma_H}{\sigma_V} \approx 2.75$, or $\sigma_H \approx 9{,}100$ psi. This is extremely high for this depth and of concern for stability. The Rock Mass Rating Q = 8 to 20 with such high stresses further indicates possible heavy support.
5-26 & 5-27	Nowhere does it give the <u>areal continuity</u> of the flow as a criterion.
5-43	I question the usefulness of borehole jacking and crosshole seismics to examine variability. The best technique, in my estimation, is straight geological observation and mapping.
5-44	What is the essential difference between the plate bearing and flatjack tests? They both measure the same thing.
5-46/Underground Geo-Mechanical testing	Why measure stress on the wall of the opening? Won't displacements tell you the plastic deformation and stress redistribution?
5-50/Shaft Construction	The blind boring technique virtually eliminates the ability to see the rock as the shaft is being sunk. Only a limited knowledge is gained of the effectiveness of the grouting operation. I am particularly concerned about shaft stability and how the dilation of the rock surrounding the shaft will affect the permeability, thus rendering the liner and grout seal ineffective. There is currently highly significant incidence of core discing at the site.

PAGE/PARAGRAPH	COMMENT
	The shaft (although not directly analogous) will be a large borehole discing test. The larger scale of the shaft will result in greater probability of block motion into the shaft. There is no testing given to monitor the stability of the shaft over time. No displacement or liner strain. This is a bad deficiency.
6-7	They may well have mild bursting in the advancing face.
	There is no good basis given for determination of stability of openings.
6-9	Predicted range of thickness of dense interior is 35-45mm (1.3 - 1.8 in.)?
6-10	How can one measure hydraulic conductivity to 10^{-15} m/sec in situ?
6-15	We all assume that values in the predicted range mean <u>acceptable</u> from the standpoint of repository performance.
8-3	Rockburst not included in hazards.

Specific Comments on Volume II

PAGE #	COMMENT
1-2	Table shows no data on shaft or opening construction or hydrologic characteristics.
2-4	Please define "tectonic features."
2-5	Will percussion holes used for permeability work not create problems with cuttings in fractures?
2-10	Pressure?
2-30	Thought should be given to performing some detailed line surveys as described in Call (17th US Symp. Rock Mech.) to obtain greater detail on jointing to supplement the grosser maps.
3-6	Define what "cluster test" is.
General Hydrology	The tests defined here all deal with the bulk rock mass permeability. With a rock such as basalt, it is obvious that flow will be confined to fractures or interflow zones. Is it not important to examine the fracture conductivity as a function of temperature and stress in the in situ environment? This can be accomplished in the flatjack testing. More detail must be given in the test plan on the types of numerical models which will be used for prediction of g.w. flow. If ubiquitous jointing is assumed and an equivalent porous media model is used, one must demonstrate how the permeabilities measured here will change as a function of temperature and stress. This point can be very significant (as shown by Hardin, et al., 22nd US Rock Symp.) and should not be ignored. In other words, to address these two problems, one must make some very basic measurements on the features themselves (joints) and extrapolate to larger size.
3-35	Why did you assume an isotropic stress field in figure 3-9?
4-4	A good flow chart is needed to describe testing methodology.

PAGE #	COMMENT
4-7 Fig. 4-2	Why does the large flatjack test influence less volume than the borehole jack?
4-11	I do not see the utility in the deflectometers at the shaft breakout. All this expense, and what will it show? Very little I suspect, particularly since blast damage will likely occur. Let's face it -- this excavation is basically unlike any other deep excavation, and the continual reference to this being an "unprecedented case" is ludicrous. The stability of the breakout is as easily measured by installing closure pins or extensometers in the excavation after it is begun.
4-13	Intelligent method of excavation monitoring.
4-15	The measurement of stress along sidewalls using flatjack cancellation pressures will show little, if anything, in my opinion. The test plan indicates no significant data to be gained.
4-18	I seriously question the need for two triaxial block tests. If the rock is indeed transversely isotropic, one axis of the test can be oriented along one axis of symmetry, and another parallel to the second symmetry axis.
4-19	Six plate bearing and eight flatjack tests are excessive.
4-20	See little benefit in flatjack tests to complement the block test <u>and</u> plate bearing tests.
4-21	The cross-hole seismics are good to examine variability, but difficult to correlate to <u>static</u> modulus. Interpretation must be made by experienced people.
4-22	The term "overstressing" has no meaning. Also, the term "strength" must be defined.
4-23	Again, one needs a definition of "strength." Is it a total loss of load bearing capacity, joint slippage, what?
4-24	I like the idea of trying the block modeling to the joint testing for in situ comparison.

PAGE #	COMMENT
4-27	What model is to be used to predict the room scale test? A block model or continuum approach?
4-34	Inability to maintain a stable hole should invalidate the site!
4-35	What is meant by non-linear constitutive models and why is this suggested? Be more specific as to why linear thermoelasticity is insufficient.
4-42	Isn't the best criterion for stability that the displacement of the opening comes to equilibrium?
4-43	Isn't the use of deflectometers in the breakout a bit extravagant and somewhat meaningless? What will it show?
4-54 & 4-55	Again, a very great number of references made to borehole or drift spalling. This does not sound like a stable repository environment!
4-59	Examination of closure and support loading data will aid in constitutive model development.
4-76	I agree with several techniques for stress measurement.
4-127	Note: Seismics give dynamic moduli which are highly dependent on the local stress state. I have found these data very difficult to interpret.
4-130	Rather than a tape recorder, a digital oscilloscope is preferable which can store waveforms on floppy disks.
4-149	Yet another modulus of deformation test? This one requires some additional development of the diamond chain saw.
Mine-by	Mine-by seems like a reasonable test. How about the addition of a borescope hole from drift-to-drift which could double as an absolute drift-to-drift convergence measurement?

PAGE #	COMMENT
4-172	Mine-by -- Can permeability changes be determined in this rib pillar as well as pore pressure measurements to give an idea of the excavation-induced effects?
4-186	The canister hole drilling should rely more on visual observation that all of the remote instruments which will simply show the displacements about a circular hole in a biaxial stress field.
4-200	The heater test should examine more closely the response of backfill and liner to heat and water.