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NEVADA NUCLEAR WASTE STORAGE INVESTIGATIONS

RADIONUCLIDE MIGRATION IN TUFF AND GRANITE PEER REVIEW DOCUMENTATION

APRIL 1981

UNITED STATES
DEPARTMENT OF ENERGY
NEVADA OPERATIONS OFFICE
LAS VEGAS, NEVADA

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UNITED STATES
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LAS VEGAS, NEVADA

CONTENTS

INTRODUCTION

This document is a compilation of review comments on the tuff and granite radionuclide migration investigations. These investigations are being managed by the Nevada Nuclear Waste Storage Investigations (NNWSI). The principal NNWSI participants conducting the investigations in tuff are the Los Alamos National Laboratory (formerly the Los Alamos Scientific Laboratory), Sandia National Laboratories, and Argonne National Laboratory, and for granite, the Lawrence Livermore National Laboratory. These investigations will determine radionuclide migration/retardation in tuffaceous and granitic rocks in situ.

The NNWSI are a part of the National Waste Terminal Storage (NWTS) Program of the U.S. Department of Energy (DOE). The NNWSI were formally organized in 1977 and are being managed by the Waste Management Project Office of the DOE's Nevada Operations Office. The NNWSI are in the process of developing or improving the technology for high-level nuclear waste handling, containment, and isolation, and determining whether potentially suitable rock units on or adjoining the Nevada Test Site (NTS) are technically acceptable for a licensed, permanent nuclear waste repository.

The review comments compiled in this document are the result of a peer review meeting conducted on August 18-19, 1980, at the DOE/NV building in Las Vegas, Nevada. The list of peer reviewers and the agenda for the review precede the review comments. The correspondence transmitting the reviews are presented in chronological order. The later review commentaries are those of the NNWSI Technical Project Officers representing the Los Alamos National Laboratory and the Lawrence Livermore National Laboratory. The final summary review commentary is that of the NNWSI Technical Overview Contractor, Sandia National Laboratories.

The draft program plans for the radionuclide migration investigations of tuff and granite were provided to the peer reviewers in advance of the meeting. These draft doçuments described the proposed accomplishment plan and objectives of the investigations. While these investigative tasks will be subject to internal technical reviews and quality assurance programs, the Office of Nuclear Waste Isolation (ONWI) and NNWSI Project Management decided that their importance warranted an external assessment of the sufficiency and quality of their program plans and expert recommendations for the inclusion or direction of activities. Programmatic decisions with regard to the review comments and recommendations as well as the NNWSI responses are embodied in current Project activities as outlined in the Nuclide Migration Field Experiments, Program Plan (LA-8487-MS) and Program Plan: Field Radionuclide Migration Studies in Climax Granite (UCID-18838), and in the FY 1981 NNWSI Project Plan and FY 1982 Forecast.

Peer reviewers representing appropriate fields of expertise were invited to attend the review sessions. Nationally known as well as prominent state and local scientists were selected to participate in the peer review process. At the meeting, the NNWSI Technical Project Officers, Principal Investigators, and technical staff members involved made detailed presentations and answered questions about their investigative activities and findings. The presentations were concluded with question and answer sessions. The second day of the peer review meeting was an open discussion between the peer review panel, technical NNWSI Project participants, and ONWI representatives. The peer review panel then met and summarized their overall assessment and recommendations which they orally presented to the ONWI representatives and NNWSI Project Management.

NEVADA NUCLEAR WASTE STORAGE INVESTIGATIONS

RADIONUCLIDE MIGRATION

PEER REVIEW PANEL

AUGUST 18, and 19, 1980

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Mackay School of Mines
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Environment Canada
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AGENDA

NEVADA NUCLEAR WASTE STORAGE INVESTIGATIONS

PEER REVIEW

FOR

RADIONUCLIDE MIGRATION

AUGUST 18, 1980

8:30 a.m.	INTRODUCTION	NV/ONWI
	LLNL GRANITIC PROGRAM	
8:45 a.m.	INTRODUCTION	DANA ISHERWOOD
	OBJECTIVES	
	SITE SELECTION	
9:30 a.m.	INITIAL FLOW TESTS	WILLARD MURRY
	MODELING	
	LABORATORY FLOW TESTS	
10:00 a.m.	BREAK	
10:15 a.m.	WATER COLLECTION AND CHARACTERIZATION	DAVID COLES
	SELECTION OF TRACERS	
	LABORATORY SUPPORT STUDIES	
11:00 a.m.	INJECTION - COLLECTION DESIGN	DANA ISHERWOOD
	POST-TEST SAMPLING AND ANALYSIS	
	SUMMARY	
11:45 a.m.	LUNCH	
	LASL, SNL, AND ANL PROGRAM IN TUFF	
1:00 P.M.	INTRODUCTION	BRUCE ERDAL
1:30 P.M.	FIELD OPERATIONS AND DESIGN	KEITH JOHNSTONE
2:15 p.m.	BREAK	
2:30 p.m.	LABORATORY STUDIES	ARNOLD FRIEDMAN
	FLOW-PATH ANALYSIS	
3:15 p.m.	EXPERIMENT ANALYSIS	BRUCE ERDAL
	SUMMARY	
4:00 p.m.	ADJOURN	

COMMENTS ON TESTS

GRANITE

1. Why wouldn't block test in laboratory be a valuable addition?

   a. as an aid in designing in situ test

   b. as supplemental method of achieving objecti~~ons~~ves ~~1 + 4~~

2. We suggest that in conjunction with block test, a post-test evaluation procedure of coring (pilot hole to inject plasticizer) to investigate sorption sites is needed before application in field. How many cores/m^2 are needed?

3. We suggest that that actinide migration should ~~be~~ be included in this program rather than subsequent programs.

TUFF

1. Applaud idea of appropriate block tests for actinides as a way of better defining the conditions to be studied in situ. Emphasize necessity of pointing towards in situ test.

2. Concerned about effect of diffusion into porous medium.

3. Concerned about how to extrapolate from 1 - 1.5 m flow field in single fracture to next appropriate scale, for more complex geometrics and test configurations. (Test configuration is unrealistic for larger scales).

Assessment of radionuclide migration is critical to DOE's waste management program, and we believe these two programs with their different approaches in different rock types will provide an important contribution to the understanding of the parameters controlling migration. A careful coordination between programs will be very beneficial.

RNM Peer reviewers summary comments.

8/19/80

12/16/80

2630 Trentham Way
Reno, Nevada 89509
October 10, 1980

WM PO

Mr. Robert M. Nelson, Jr.
Project Manager
Nevada Nuclear Waste Storage Investigations
Department of Energy
Nevada Operations Office
P. O. Box 14100
Las Vegas, NV 89114

Subject: Radionuclide Migration in Fractures Study

Dear Mr. Nelson:

As you know, I became incapacitated as the result of an auto accident shortly after the peer review meeting, so this response is rather late.

The radionuclide migration in fractures programs are well thought out, and I concur with the joint statement of the peer review group. There are several additional comments I can make with respect to flow fields in fractures, sorption/retardation and ground water chemistry.

Without intimate knowledge of the flow field that will be generated during these studies, the data collected will be difficult to interpret in a meaningful way. This is not so much the case with the tuff fracture study, since the fracture surface will ultimately be exposed by mining, and a method has been developed that shows promise of determining the flow distribution within the fracture from the deposition of selected radionuclides on its surface. By contrast, the ability to gain direct access to the fracture surface does not exist for the radionuclide migration in fractured granite study. The flow field model presented by LLNL is probably not applicable. First of all, the theoretical flow field is not equivalent to a source and a sink in a uniform flow field. The edge of the flow field in unsaturated granite is a free surface, not a boundary between two saturated regions. The requirement along the free surface is that the head in the flow field adjacent to the free surface must be equal to the elevation head. Secondly, the theoretical flow field, even if properly developed, will only apply to the aperture between two smooth parallel plates. The real fracture aperture will vary from place to place, and discharge along a fracture is proportional to the cube of the aperture, so that small variations in aperture are significant. It appears that for the granite case we will never know with any certainty what the flow field looks like. One might point out, however, that in measuring sorption/retardation in granular materials, it is also impossible to define the flow field in detail. One is measuring effects that are hoped to be representative of similar rocks elsewhere. In a practical sense, it would be virtually impossible to characterize fracture surfaces in detail along a long flow path. I assume, however, that obtaining fundamental knowledge is part of the radionuclide migration in fractures experiment and therefore knowledge of the flow field is required.

To my recollection, little or no time was spent in the consideration of the velocity of groundwater in the flow field and the effect of the rate of movement of groundwater on the apparent distribution coefficient. About ten years ago I investigated the problem of radionuclide migration in fractures with respect to andesite and breccia from Amchitka. The results of this study were published last year (Journal of Hydrology, 43(1979) 415-425). Recently (Water Resources Research, Vol. 16, No. 4) Grizak and Pickens discuss both the theoretical and experimental aspects of matrix diffusion during solute transport. Except for rather high groundwater velocities there would probably be a greater time related sorption or matrix diffusion problem with tuff than granite. Higher groundwater velocities relative to normal groundwater velocities are expected for the experiment. Therefore one would expect that values obtained for retardation of radionuclides during the experiment might be lower than experienced during actual groundwater transport.

Effort is being made to collect representative groundwater samples for each of the experiments. Although I certainly concur with this part of the programs, I also have some misgivings concerning how the water chemistry data will be used. A large number of water samples have been collected over the last three or four years by the Water Resources Center, D.R.I., of both matrix and fracture waters from the tunnels of Ranier Mesa. The water chemistry varied considerably between the matrix water and the fracture water. However, waters from the fractures were not identical either. I think this variation in water chemistry is probably a peculiarity of the unsaturated zone. What water chemistry does one use for the experiment? The water chemistry of the granite hasn't been studied to the same extent as the water chemistry of the tuff, but I suspect that the water chemistry of the granite is much better behaved and probably won't present a serious problem.

I hope that you will find these comments useful. I do not intend them to reflect in any way a negative attitude toward the two projects. These are the kind of field experiments that must be carried out for us to learn anything about fracture flow and radionuclide transport along fractures.

Sincerely yours,

Paul R. Fenske

Paul R. Fenske
Consulting Hydrogeologist

PRF:sp

cc: Bruce Erdal
Dana Isherwood

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Berkeley, California 94720
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Earth Sciences Division

10/21/80

September 5, 1980

Mr. Robert M. Nelson, Jr.
Project Manager
Nevada Nuclear Waste Storage Investigations
Nevada Operations Office
P. O. Box 14100
Las Vegas, Nevada 89114

Dear Mr. Nelson:

The following are some specific comments that are in addition to those made August 18-19, 1980, by the Peer Review Panel concerning the in situ experiments on radionuclide migration to be conducted at the Nevada Test Site.

1. One of the difficult field problems will be to maintain a saturated fracture in either the granite or the tuff so that flow paths between inlet and outlet holes remain relatively constant. Ideally one would like to have a complete recapture of injected water, but more likely one will have to settle for a reasonable percentage return of the injected water. Initial trial injections may reveal those pressure conditions that produce an optimal return of fluid, and I presume this will be part of the field test plan for both granite and tuff.

2. Most natural fractures have variations in aperture across their rough surfaces, so the location of those boreholes that are drilled normal to the fracture surface may encounter tight zones (minimum aperture) where the fracture flow is minimal or absent. Trial injection and withdrawal tests will be necessary to determine the nature of the local flow fields in the vicinity of such access holes.

3. Since the drill waters will invade the intended flow areas of the fractures, keeping an accurate inventory of all drill water should provide the first indication of how leaky (or tight) the test fracture is. A non-sorbing tracer in the drill water might result in a known amount of tracer being left behind in the fracture that you could look for in the subsequent flow tests. For example, if only one-tenth of the tracer in the drill water that was left in the fracture can be recovered, and subsequent flow tests reveal difficulties in establishing a desirable flow field, you may have a method of deciding how to choose a satisfactory field situation before a great deal of time and effort have been expended.

EAD

AMD

4. In the case of the granite experiments, the injection of helium as the first fluid to be used may cause problems. This non-wetting gas will tend to fill all the largest pore spaces (maximum apertures) in the fracture. When water is subsequently injected, some of these large pore spaces may remain blocked with helium such that water bypasses them and you will not have as complete a flow field as you would like. Since air may be present in the fracture even now, this problem may already exist, but I would think injecting helium could only aggravate the situation.

5. The problem of recovering samples of the fracture surface for laboratory inspection is one of the very important elements in both granite and tuff. There was some discussion in Las Vegas in the case of granite of injecting grout to protect the fracture surface with its sorbed species. Perhaps this should also be considered for the experiment in tuff unless some preliminary work can show that the planned excavation work will not adversely affect the fracture surface. Obviously, laboratory tests to select a fluid with the right viscosity, wetting characteristics, setting time, inertness to the radionuclides, etc. will be important in deciding whether this approach can be used effectively.

6. Another difficult problem in the case of granite is to know where to locate post mortem cores to obtain samples of the fracture surface. Although this is not planned for the experiment in tuff, perhaps consideration should be given to such drilling to be able to compare the core method with the excavation method of exhuming a fracture surface in tuff. In the event a suitable grout can be found for either (or both) experiments, one would have another indication of the flow paths between inlet and outlet holes that should show exactly where the action was. Cores of the fracture surface containing no grout, because flow bypassed the area, would be very meaningful in deciding how many boreholes are needed to sample the flow adequately. Should cores ever be taken outside the intended flow field, presence of grout would be one clue to directions that unrecovered fluids took.

7. A possibility that the geophysical specialists should be asked to consider is whether the flow field can be mapped indirectly. If the injection fluid can be tagged (in the geophysical sense), an array of permanent stations along the tunnel walls might enable you to "map" the flow field in the fracture during the experiment. If such a scheme can be devised, it could provide important information in interpreting results.

8. Bob Galbraith at LBL was in direct charge of the installation of 90 packers in boreholes used to isolate fractures in the ventilation test at Stripa. You may wish to call him (415/486-6031) to discuss the problems he encountered in setting these packers in granite.

9. Finally, I want to reiterate the comment made by the Peer Review Panel that these two programs with their different approaches in different rock types will provide an important contribution to the problems of understanding the parameters controlling radionuclide migration.

Very truly yours,

Paul A. Witherspoon

Paul A. Witherspoon
Division Head
Earth Sciences Division

cc: D. Isherwood, LLL
B. Erdal, LASL

Atomic Energy of Canada Limited
Nuclear Fuel Waste Management
Whiteshell Nuclear Research Establishment

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AGB-80-480

1980 August 25

R.M.Nelson, Jnr.,
Project Manager
Nevada Nuclear Waste Storage Investigations
Department of Energy
Nevada Operations Office
P.O.Box 14100
LAS VEGAS. Nevada 89114
U.S.A.

Dear Bob,

I am enclosing my own personal reports on the two in situ migration proposals recently peer reviewed at Las Vegas. While I support the concensus summary previously presented, you will note that my report diverges from it somewhat in emphasis. My background is mainly in chemistry and geochemistry and thus my detailed comments are mainly restricted to these aspects of the proposals.

If you or anybody else needs any points clarified, please call me.

Yours sincerely,

Peter Sargent

F.P.Sargent,
Applied Geoscience Branch

FPS/jb

Attach.

cc: Dana Isherwood - LLL
Bruce Erdal - LASL

EAD

PEER REVIEW OF

"FIELD RADIONUCLIDE MIGRATION STUDIES IN (a) CLIMAX GRANITE AND (b) TUFF"

BY

F. P. SARGENT

Whiteshell Nuclear Research Establishment
Pinawa, Manitoba ROE 1LO

COMMENTS ON BOTH PROPOSALS

The two distinct proposals from LLL and LASL/ANL/SNL were well prepared and are an important step forward in the DOE Waste Management Program and the Assessment of Geologic Disposal.

The approaches in the two proposals and two geologic media are sufficiently different to support funding of both. However, it is important that these differences not be seen as discrepancies or inconsistencies. Therefore, I suggest that DOE be prepared to identify and justify the different approaches. For example, the present proposals differ in the following

-: choice of preferred fracture orientation, vertical vs. horizontal

-: choice of source of groundwater

-: actinides vs. 'stand-ins' for actinides

-: emphasis on lab scale vs. field

-: charging of operating costs

-: inclusion of inflation cost escalator

Both proposals make motherhood statements re learning from each other's experience and the free exchange of information. However, there is no formal mechanism proposed! I suggest this be the responsibility of the principle investigators and the DOE/NTS program coordinators.

Comments on LLL Proposal (Granite)

General Comments

1. The proposal was well set out - apart from lack of details of operating budget.
2. Both the title and the details of the proposal emphasize the field work.
3. I wondered if the degree of optimism expressed re development of equipment not available commercially, was justified.
4. The authors have purposely kept the chemistry (migrating species) as simple as possible feeling that the geology and hydrogeology are potentially complex.
   I support this reasoning but the project must eventually lead to the use of actinides.

Specific Recommendations

1. The rationale for omitting the actinides must be highlighted.
2. The possible extension of this project to include actinides must be part of the program plan.
3. Iodine as iodide should be included as a tracer.
4. Use of crushed material for sorption experiments be avoided.
5. The authors be encouraged to perform some block tests similar to those in the Tuff proposal.

Comments on LASL/ANL/SNL Proposal (Tuff)

General Comments

1. Chemistry and laboratory work well set out but no breakdown of costs per project.

2. Field work often vague and not defined. Appreciate the problems but I would like to see more specifics in the description of field work. Perhaps this will come in the Engineering Plan.

3. Need a detailed operating budget.

4. Admire the use of laboratory block tests but must be assured that these will lead to field tests. The assurance could be provided by means of a detailed operating budget.

Specific and Technical Comments and Recommendations

1. Need to investigate the nature of porosity in the Tuff.

2. Determine the effects of diffusion into rock pores on nuclide migration in fracture.

3. Determine effects of pressure on varying the extent of diffusion into bulk rock.

4. Concerned about establishment of rock water equilibrium. I suggest more attention be paid to this.

5. The use of crushed material for sorption studies be avoided.

United States Department of the Interior
GEOLOGICAL SURVEY
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DENVER FEDERAL CENTER
DENVER, COLORADO 80225

10/21/80
EAD August 27, 1980

Mr. Robert Nelson
U.S. Department of Energy
Nevada Operations Office
P.O. Box 14100
Las Vegas, Nevada 89114

Dear Mr. Nelson:

As per request, here are my written comments on the Climax Stock Granite and the Tuff *in situ* radionuclide migration proposals.

Both proposals were well prepared and reflected the thought and effort that went into their preparation. If their objectives are attained, they will provide much-needed information under more-or-less actual field conditions. The approaches are different, and therefore I will address them individually.

Tuff Studies (LASL, SNL, ANL)

This is the more comprehensive and expensive of the two proposals, and not surprisingly, will yield the most information if successful. There are pitfalls, however, that could seriously impair the value of this program. The authors recognize the potential difficulties and propose a number of alternatives. In fact, so many options are discussed that one referee referred to the Proposal as a "shopping list." This is a bit harsh, but I wonder whether the manpower and budget allocatins are sufficient to support an elaborate and complex methodology development program in addition to the actual *in situ* experiments. Along this same line of thought, in a program as ambitious as this, it is entirely possible that no satisfactory method will be found to accomplish crucial aspects of the program. If this occurs, are the review processes adequate to recognize this at an early stage so that these approaches--or the entire program, in an extreme case--can be abandoned before time and money are expended unnecessarily?

Control of ground water conditions (pH, Eh, temperature) is necessary if valid results are to be obtained. Inability to do so is just one example of an experimental difficulty that must be overcome if the experiment is to be a success.

Climax Granite Studies (LLL)

This proposal, although more modest, is perhaps more realistic in that it has more limited objectives, relies more on existing technology, and is more amenable to future scale-ups. While I appreciate the desire of the authors to keep the program simple and inexpensive, there are two omissions that I feel should be corrected:

1) Laboratory studies using granite blocks should be included, both as a means of perfecting techniques to be used in the in situ study, and also to provide a basis for comparing laboratory and field data. Removing blocks of granite is admittedly more difficult than removing tuff blocks, but techniques were developed in Sweden for doing this.

2) It is highly important that actinides--expecially neptunium and plutonium--be included even at the expense of increasing the scope and budget of the study. Cerium is not an acceptable stand-in for these elements, which are among the more hazardous constituents of radioactive waste over the long term. Failure to include them in the current study would delay obtaining necessary migration data until the latter part of this decade. Given the urgency of establishing waste disposal site criteria by the middle of the decade, I do not feel such a delay can be justified. Granted the inclusion of actinides will increase the experimental difficulties and require additional safety controls, but this will be somewhat mitigated by the use of laboratory block studies to perfect techniques, as suggested above. Moreover, the use of short-half-life isotopes of these elements will greatly reduce the hazard. In any case, the valuable data so obtained will more than justify the additional time, effort, and expense.

Even though these two proposals take different approaches and are not designed to qualify either granite or tuff as a waste disposal medium, it is naive to assume that this will not happen. Time is short, and the limited field data will be utilized for all it is worth. This being the case, it is important that the data from the two studies be comparable. I feel that the above suggestions will produce this comparability while at the same time maintaining the essential integrity of the two proposals.

It was a pleasure to serve on the Peer Review Committee, and I wish both programs the utmost success.

Very truly yours,

J M Cleveland

J. M. Cleveland

Department of Geology

The Florida State University
Tallahassee, Florida 32306

10/21/80 copy to [illegible]

9/11/80 copies to [illegible]

August 25, 1980

Robert M. Nelson, Jr.
Project Manager
Nevada Nuclear Waste Storage Investigations
D.O.E., Nevada Operations Office
P. O. Box 14100
Las Vegas, Nevada 89114

Dear Dr. Nelson:

Attached are my comments on the two radionuclide migration proposals that were the subject of our peer review of 8/18-19.

The emphasis of these remarks is on what I perceive as research strategy questions. This is because there is not much to criticize with respect to the technical plans and the scientific competence of either team, and also I understand that there will be later peer reviews devoted to the more detailed aspects of the experiments.

I would be pleased to take part in such reviews; this one was a very educational experience.

Sincerely yours,

J. K. Osmond
Professor

EAD

Review Perspective

There appear to be important quasi-scientific aspects to these projects... related to engineering and administrative missions ... which this reviewer is unqualified to evaluate. My perspective is strictly that of a nuclear geochemist, and recommendations made here should be judged accordingly.

Initial attempts to review these two projects separately resulted in considerable duplication of discussion; the two recommendations I regard as most important apply to both projects. These two points of discussion are therefore presented first, in a somewhat philosophical vein, followed by what I consider to be minor specific criticisms of each project separately.

Questions of Strategy: Both Projects

Based on the reading of the two proposals and listening to oral presentations by several researchers, there is an apparent inconsistency concerning the optimum hydraulic parameters of the field experiments: fissure size, desired flow rates, water residence times, importance of gravitational effects, etc. It may very well be that in situ, the investigators will have to take what they can find, but it ought to be recognized by the field workers that a fissure that permits gravitationally driven flow times of only a few hours is far too conductive for the successful achievemnet of the radioisotope retardation objectives. And the project leaders should recognize the possibility that achieving these objectives may require the use of fissures demanding high pressure, and run times of many months.

My second point relates to the first, and to several other questions concerning the scientific research objectives. These two research projects are attempting to fill a gap in the spectrum of understanding of the phenomenon of migration of radionuclides in rock fissures. One boundary of this gap is represented by laboratory research, primarily of the 'batch' type, where surface area, mineral composition, and water chemistry are the critical parameters. The other boundary is represented by field studies involving bore holes and water sampling, using either injected or naturally occurring radionuclides, in which the critical research parameters are geologic and hydraulic. At neither boundary are the levels of understanding of radioelement migration and adsorption very satisfactory, and certainly not good enough to extrapolate with confidence into the domain of research staked out by these proposals.

The investigators involved recognize this and are attempting to fill the gap by attacking both flanks... (a) laboratory study of cracked rock (blocks or cores), and (b) injection into well defined fissures underground. But both groups appear to regard the one as only a necessary step toward the successful achievement of the other.

I personally think that a more productive scientific effort will result if the core and block, bench-type, studies are pursued as a parallel effort; one in which the research parameters, flow rate, rock stress, flow field dimensions, fissure orientation, etc., can be controlled more readily, and where alternative techniques of water injection and removal, and isotope tracing and recovery, can be developed.

Difficult-to-study phenomena usually require repeated experimental runs of a sort more easily conducted in the lab. Even if they have to be on a smaller scale (a point not conceded here), such experiments will probably come closer to achieving the stated scientific objectives than will the major efforts involved in one or two <u>in situ</u> runs, especially considering the gamble involved in finding and instrumenting the right sized fracture, as mentioned above.

I do not regard this as an argument for major restructure of either proposal. I suspect that the investigators attach more importance to their laboratory studies than their porposals suggest; or, if not now, that they will ultimately do so as they pursue promising and cost-effective research leads.

This is a recommendation that both groups:

1. emphasize, right from the beginning, the importance of bench type research in achieving project objectives,

2. pursue laboratory studies vigorously even after underground experiments are begun, and

3. avoid premature committments of time, money, and personnel to the in situ phases at a level that will hobble laboratory work.

The Granite Proposal (LLNL)

The objectives and research design seem well conceived. I agree that it would be nice to include actinide studies, but only if it could be done without diluting the effort described in this proposal. In addition to the many other hydraulic questions needing answer s, this experiment suffers from an uncertainty regarding the history of the fractures to be used, i.e., whether or not they have been saturated. With regard to the observed natural seeps, analysis of the naturally occurring nuclides, e.g., uranium, radium, might lend clues to the natural flow rates.

Although small core experiments are included in the proposal, there does not appear to be much interest in larger core or block type experiments. This may indicate the investigators belief that such bench type research is too difficult. More review of the possibilities is warranted. What would seem to be pertinent references are not cited, including several mentioned in Witherspoon, P. A., D. J. Watkins, N. G. W. Cook, M. Hood, and J. E. Gale (1979) Laboratory investigations on the hydraulic and thermomechanical properties of fractured crystalline rocks. In Proceedings of the National Waste Terminal Storage Program Information Meeting, Columbus, Ohio, Oct. 30-Nov. 1, 1979. ONWI-62, pp. 19-25.

The Tuff Proposal (LASL, SNL, ANL)

Both the 'cold' and the 'hot' injection experiments seem rather ambitious, unless bench type pilot studies are first completed successfully.

Both the written proposal and the oral presentation, however, give cause for much confidence; considerable expertise and resources are available.

An early answer to the question of matrix versus fissure permeability in tuff is needed; also whether bedding plane fractures are representative of the overall fracture system with respect to permeability.

Plans for a post-mortum examination of flow fields were imaginatively designed. It is hoped that a more detailed discussion of the specific radionuclides chosen for study will be presented to a later "hot experiment' peer group.

Environment Canada / Environnement Canada
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NELSON

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August 28, 1980

EAD

R.M. Nelson, Jr.,
U.S. Dept. of Energy,
Nevada Operations Office,
P.O. Box 14100,
Las Vegas, Nevada 89114
U.S.A.

Dear Dr. Nelson:

RE: PEER REVIEW MEETING RE: FIELD TESTS FOR RADIONUCLIDE MIGRATION IN GRANITE AND TUFF

Thank you for the opportunity to participate in the peer review of these two projects. I found both projects to be extremely well documented at this early stage and commend both groups for their efforts in that direction. I understand that the peer reviewers are meant to submit individual letters as well as presenting our consensus viewpoints at the Tuesday PM meeting. Since most of my concerns were addressed at that meeting I will only summarize those which are of particular interest to me, as well as some other minor points, in the paragraphs below.

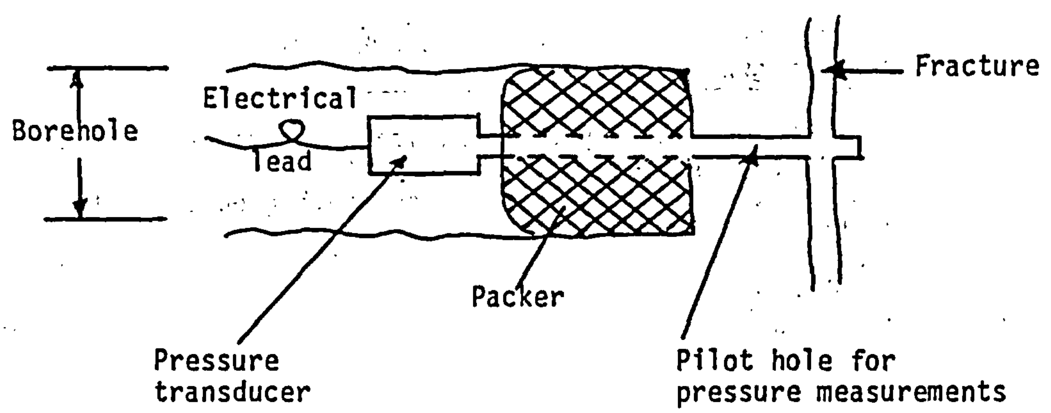
CLIMAX GRANITE

1) The starting point of a vertical and potentially unsaturated fracture seems the most critical problem. If the fracture were saturated I would be much more comfortable trying to impose a recharging-discharging pair of boreholes on the existing flow field. If the fracture is unsaturated and the analogy is drawn between the gravity field and a saturated areal flow field I am not sure that an injection-withdrawal system can be established with much degree of certainty. As a consequence of this concern I would suggest two things:

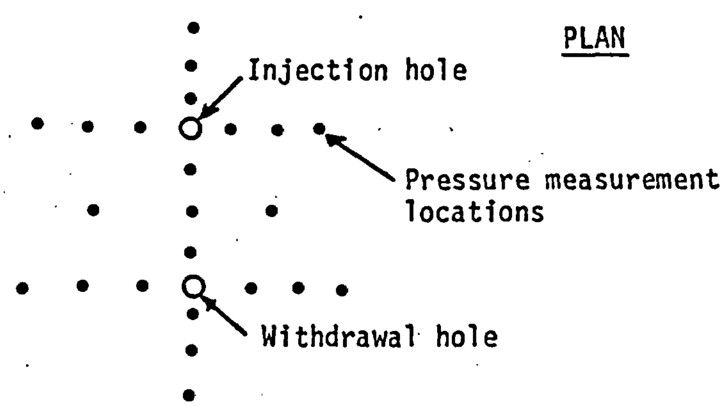
a) Attempts be made to determine if the fracture is indeed unsaturated. So far, unsaturation is simply inferred. This could be done by locking in on the fracture with a fluid filled straddle-packer with a pressure transducer measuring the straddled interval. If the fracture is saturated a significant pressure build-up should occur. If the fracture is unsaturated the pressure should remain at shut-in values and the fluid will eventually drain into the fracture. (I would perhaps repeat this several times to eliminate any artificial desaturation in the borehole vicinity).

b) Every attempt be made to measure and verify the flow field which is expected between boreholes. This could be done with small access holes (1/4") for pressure measurements extended into the fracture beyond the end of ordinary NX holes. (the NX holes being advanced to as much as a few inches from the fracture and the pressure pilot-hole then advanced the remainder of the distance).

CROSS SECTION



PLAN



2) Since the minimum volume of fluid between two straddle packers is about 0.4 L/ft., I would suggest that mixing capability exist in both the injection and withdrawal straddle intervals. Even with mixing in these intervals and minimal dead space in the withdrawal line, there is a dilution component which will likely need to be assigned to equipment-related effects. This component is recognized by the investigators and will be minimized in the laboratory study. The question remains as to how the residual will be accounted for in the analysis of the tracer breakthrough data. I cannot visualize other than incorporating it into the dispersive term of the solute transport equation, however it may well mask the longitudinal dispersion in the fracture itself.

3) A pulse imput at this scale (1-5 m) will likely provide sufficient information, however consideration might be given to other input conditions (i.e. constant concentration). Pulse inputs in actual field scale (10-1000 m) experiments often provide ambiguous results, due to the peak spreading and excessive tailing. Constant source inputs tend to offer more tracer to look for at higher concentrations. In either case I think one of the most important aspects of any tracer test is to conduct a tracer mass balance in order to ensure that the transport parameters measured are applicable to the entire system under study. Tracer which is 'lost' simply suggests that all the parameters affecting solute transport in a particular system have not been accounted for.

4) see 4 below.

Non Welded Tuff

1) My main concern with this experiment is one which the investigators appear well aware of, that is matrix diffusion, or diffusion of nonreactive and reactive solute from the fracture into the adjacent porous matrix (porosity of 20-40%). I would suggest not only quantifying the actual matrix porosity (mercury porosimetry or ambient and pressure saturation) but also diffusion coefficient measurements for each of the tracers. Free water diffusion coefficients will likely be insufficient, as it is the tortuosity of the porous matrix which has a considerable effect on the magnitude of the effective porous media diffusion coefficient. Reactive diffusion coefficients will also likely be necessary for the reactive species. I believe the porous matrix will have an overwhelming affect on the entire experiment and a good deal of effort should be expended to quantify the effects of the matrix on the breakthrough curves and the distribution of the solutes after injection.

2) I have the same concern with the Tuff experiment as with the granite regarding verification of the flow field by pressure measurements (see 1(b) above).

3) Comments in 3 above may also be applicable.

4) The potential volumes of natural (or synthetic in the case of the granite) water required are conceivably quite large. Calculations show that at realistic injection pressures (1-50 psi) the likely injection rates for a fracture between 10 and 100 µm are between 10 and 100 mL/min.

Also, the water transit time in the fracture between injection and withdrawal points is theoretically of the order of minutes to a few tens of hours. (However matrix diffusion of even non-reactive solutes may slow this down considerably).

General

A concern I have regarding both projects is their applicability at the larger field scale (10's to 1000's of metres) where tracer tests will be done probably with boreholes only and perhaps relying solely on breakthrough curve data. (The tests to be conducted underground are essentially laboratory scale under field conditions). I would simply suggest that the investigators keep the large scale in mind when they are designing the test details in order that equipment, methods and analytical techniques might be transferred to the applied field scale as well.

Thank you again for the opportunity to participate in these reviews.

Yours sincerely,

John Pickens

for G.E. Grisak

cc: D.J. Isherwood
B.R. Erdal

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9/16/80 Copies to Edd Gilbertson? Kunich

10/21/80 Copy to Frank

WMPO

September 10, 1980

Mr. Robert Nelson
Nevada Operations Office
U.S. Department of Energy
Post Office Box 14100
Las Vegas, Nevada 89114

Dear Mr. Nelson:

In accordance with your charge to the peer review panel members, I submit herewith my comments on the radionuclide migration test program plans of Lawrence Livermore Laboratory as well as the Los Alamos, Sandia, Argonne National Laboratories plan. Both programs to develop field techniques for radionuclide migration have good prospects of producing important results. They are programs long over-due. Since both have the intention of developing field techniques, I will attempt to confine my comments to constructive criticisms. I will further restrict myself to aspects of transport through fractures as a hydraulic phenomenon, since my experience is more in that direction than in the chemical aspects.

One area of concern expressed by several panel members is that of the state of saturation of the fractured rocks surrounding the tunnels. The supposition that the water table lies many hundreds of feet below the test horizon at both the Climax Stock as well as Ranier Mesa is consistent with other observations at the Nevada Test site. It is also possible that in any given locality such as these, perched water table conditions above the regional one may exist. At the Climax Stock, fractures which are open only down to a certain depth may contain nearly static water and therefore constitute a limited perched water table condition, togther with fractures connected to this body. In the vicinity at similar levels may be other nearly static water bodies with different heads. That is the classical view expressed by Tolman (1930), and many followers. However, in my experience with construction-engineering hydrology and mining hydrology, the apparently disparate hydraulic heads observed in nearby drill holes is usually a consequence of transient conditions, rather than static. Differences of water levels simply mean that you haven't waited long enough for equilibrium to be established between adjacent pervious fractures via the more numerous but much tighter intervening fractures. In the undisturbed environment, water table coincidence is likely to be established in time. Now, the Climax Stock has been disturbed by mining and drainage to the sub-surface workings. Consequently, different heads may have developed in different portions of the plumbing system which take time to respond to the new boundary conditions. Immediately above and around a tunnel, there develops a drainage situation wherein the head has to fall to the elevation of the crown of the tunnel, or even the spring line. Unsaturated conditions can conceivably develop above the crown, although no one has ever studied the saturation-desaturation phenomenon, nor the associated, more obscure properties of relative permeability in a system partially saturated with air and partially with water. Because there is an entire heirarchy of fracture apertures, orientations, and interconnections, the geometry of the drained conduit system has a strong influence on the distribution of saturation. If any fractures desaturate during drainage, it must be the largest-aperture individuals that are intersected by the tunnel or drill holes. Below the spring line, desaturation would be impossible, except if the water table has indeed been lowered in a regional sense to a level far below the tunnel workings. Since the water table at the Climax Stock site has not been located, we cannot know whether or not there is a likelihood of unsaturated conditions below the tunnel, but we know that there is a possibility above the tunnel.

The history of water table decline during the last 8-10,000 years at the Nevada test site has been suggested by decreasing hydraulic potential with depth below the water table and may be used to anticipate what we might find at the Climax Stock. It would be germain to ask what is the water table elevation in the alluvium at the north end of Yucca Flats, which is a lower bound on the elevation of the water table in the contiguous Climax Stock. I would expect the hydraulic gradient and flow in the Climax Stock fracture system to be towards Yucca Flats. At the five hundred meter depth of the candidate workings, a single desaturation history would be implied. Utilizing distributions of fracture orientations, positions and apertures, it would be entirely possible to develop a statistical model of the fractured medium, upon which a simple drainage-desaturation process might be imposed, wholley analogous to the saturation-desaturation investigations conducted in the 1950's by Irving Fatt on tubular analogues of inter-granular porous media. The model would be instructive of what we should expect at the Climax workings.

Peripheral to a tunnel at atmospheric pressure below the water table, hydraulic gradients and flows are directed towards the tunnel in all fractures which are discharging to the tunnel. All fractures saturated to the face flow by reason of evaporation at the intersection, but those fractures which spill and therefore seep into the tunnel and down the tunnel walls have larger flow components and therefore larger gradients. As observed, only a few fractures actually discharge. It can be shown that capallarity is the controlling element, imposing the requirement that the largest aperture be the first to discharge, then smaller ones as the gradient increases. As example, in spring-time when the water table may rise because of recharge, more seeps appear. Elsewhere on the tunnel wall, because a fracture above may be seeping, capallarity is destroyed at the intersection of lesser fractures, which also therefore discharge. The invasion of air into a fracture is a phenomenon which I have published a brief note on, which also is restricted to the largest of fractures. It is possible for a maximum-aperture fracture to be unsaturated above the crown but discharging at the floor level, while in between, say at spring line, it may be saturated to the face but static.

I mention these details as reason to believe that the hydraulic potential distribution in a variety of fractures of different orientations and apertures around the tunnel may be quite different from one individual to another. Some of the complexity may exist if the water table is well below the tunnel, for then the idealized assumption of purely vadose (pressure = 0) flow in the fractures around the tunnel is likely to be disturbed by the capillarity, and the discharging/nondischarging character of individuals. If all fractures were of the same fine aperture at their point of intersection with the tunnel, one would expect the same capillary difference across the interface, and all fractures to be saturated to the tunnel wall. Then the only flow would be to make up for the evaporative losses. Evidence that this does happen, even in the tightest of rocks, is the common observation of salts deposited from groundwater, by the dust which adheres to moist walls, as well as the moisture transported out of the tunnel by the ventilation system. If I remember rightly, these circumstances are probably to be found also at the Climax Stock. Neglecting such radial components due to evaporation, the gradients remote from the wall in a system of intersecting fractures can be well approximated by taking the projections of a verticle unit hydraulic gradient directed downwards, i.e., pressure is everywhere zero. However, one cannot expect that to be the case if there are appreciable gradients towards the tunnel, as in the case of seeping conditions. The same statement can be made if there is a variety of fractures of different apertures, also located above the water table. Allowance must be made for the fact that in the unsaturated case, the tunnel acts much as a solid inclusion, and therefore some gradients must develop to divert water around the tunnel. To accelerate the flow around the spring line, steeper gradients than normal must develop, and at crown and invert, lower gradients.

If the Climax test site were demonstrated to be one of fully saturated conditions above the water table, a reasonable estimate of the boundary conditions might be made. Since it is unknown whether saturation exists or not, or even the elevation of the tunnel with respect to the water table, it seems encumbent upon the investigators to make a further effort to determine what is the original potential field around the tunnel, before perturbing it with any injection-withdrawal experiment. To establish that it is saturated, I suggest that a detailed survey of accessible fractures be made. If precipitated salts appear at the exposure, it must be water bearing and saturated. If not, it is either tightly mineralized or air-filled. If it is water-filled, a wick or sponge should easily destroy the capillarity and produce some water. If it is air-filled, it may be easy to inject air into a portion of that fracture and have the air reexit adjacent to the probe. Conversely, if it is saturated it will have no permeability to air. It has also occurred to me that a microporous tip might be inserted into a fracture to act as a tensiometer, so that the negative fluid pressure due to the capillarity at the fracture intersection could be measured. I don't know of anyone who has done any of these things, thus I expect that the innovative people at Lawrence Livermore Laboratory will be able to develop an approach to this solution.

At the tuff site in G Tunnel, Ranier Mesa, a different situation has been described: the rock is probably saturated, and is weeping water from all faces, and all fractures. Nevertheless, the water table is reportedly deep, and the entire situation may be vadose. Varying discharge rates of different fractures, in excess of evaporation, likewise imply a variety of gradients in fractures communicating to the tunnel. These conditions hardly sound like vadose circulation, but rather a perched water table situation, where there is a local groundwater level higher than the tunnel workings. In neither the Climax nor Tunnel G situation can it be safely assumed that the existing hydraulic gradient is vertically downward with the value of unity, i.e., that gravity acts independently. I submit that at the tuff site, it is just as important to investigate the potential distribution in the candidate fractures before testing is undertaken. At the very least, when two or more holes have been drilled into the candidate fracture, the fractures should be packed off and hydraulic potential carefully measured at those points of intersection.

In the case of flow through saturated fractures, we have long made the unjustified assumption that a parallel-plate analog pertains, whereas there is evidence that the aperture varies from place to place sufficiently to cause ribbon-like concentrations of flow. This non-ideality obviously is of concern to those trying to model arrival times and break-thru curves to be interpreted from the tests. If the candidate fracture is a large one, that is about 100 microns, it might be unsaturated, either from natural causes, or from the disturbance produced by the tunnel, or by the intentional injection of air during testing. If it is partially saturated, the conduit available for air-flow would be restricted by the water, and vice-versa for water flow. Alternating the fluid medium is not advisable, because it may take many pore volumes of flushing to replace one phase with the other. I think that saturation needs to be proven, and that saturation should be maintained. Otherwise there is no hope of ascertaining where the saturation exists and consequently no hope of defining the flow field. If you can't prove saturation, move the test to a site where you can, for instance by proving its position well below the water table throughout time.

That brings me to the subject of water sampling. If indeed it is possible to establish whether or not an intersecting fracture is water-bearing by applying a wick to its exposure, then it follows that it should be possible to induce seepage to a point of desired collection. With the device that Livermore intends to place against the back to collect a water sample, the space between the collector and the wall might be filled with an inert plastic foam so as to destroy the capillarity and induce seepage. Then, the discharge from the funnel may be carried down a tube to the level of the floor of the drift where it will discharge into a Peclet bottle, displacing argon to maintain purity. All air would be excluded by capillarity in the sponge and no purging with argon at the roof level would be required. The head corresponding to the height of the back of the tunnel above the floor would be applied as a suction to the sponge, thereby enhancing the seepage. In that way, it might be possible to collect sufficient water from the most copious Climax Stock seeps that manufactured artificial water might be unnecessary for the experiment. The same might be said of the water supply needs for the experiment in G Tunnel.

As to fracture orientation for the experiment, I am of the opinion that the best candidates are vertical fractures. I believe it necessary, in any event, to predetermine the hydraulic gradient before perturbing it with an injection-withdrawal scheme. It is not particularly helpful to have to assume a vertical unit gradient. Although that is avoided by selecting an horizontal fracture, it is doubtful in my mind that such a bedding-plane parting is actually a hydraulic feature a tunnel diameter or more away. You may have to satisfy yourselves of that by appropriate water-injection testing. To do so would probably require angle holes to intersect the fracture well behind the face. I have pointed out at the meeting the need to conduct the experiment behind the compression zone, at least two diameters away from the tunnel wall. The radius of minimum permeability corresponding to the highest stress may act partially as a no-flow boundary to the conduit at lesser stress deeper in the rock. In any event, it would minimize the hazard of tracers appearing at the tunnel walls. Experiments should not be conducted in a region of stress gradient, as that would ordinarily imply an aperture gradient as well. Enhanced inhomogeniety would be a consequence of linear elastic behavior at the fracture, and discharge proportional to the cube of aperture.

Further considerations of the choice of fracture orientation are worth mentioning. Most of the water-bearing fractures that I have observed in sedimentary rocks are of tectonic origin, rather than stratigraphic. Only in the case of bedded limestones adjacent to quarries have I seen evidence that lateral shear has permitted beds to move along a bedding plane to produce an opening. Glacial scour or excavation can provide the necessary relief of stress. Perhaps the same may happen in the periphery of the tunnel. In none of those cases can one expect to find a bedding plane opening persisting deep into the formation away from the excavation face. Conversely, we know that the steep fractures and faults are extensive features, some of which daylight at the surface and which are clearly water-bearing. When bedding plane surfaces are water-bearing, I have sometimes found them to be so only adjacent to verticle fractures which provide the water. Those horizontal openings may have existence only near the tunnel. Thus water flows horizontally by capillarity away from the vertical fracture. Thus, in scaling up a migration experiment closer to prototype conditions, it becomes increasingly likely that the only conduits that have meaning are the steep tectonic joints and faults, not the bedding planes. Furthermore, there's likely to be a mineralogical difference between the coatings and exposed rocks at a bedding plane surface, as opposed to a fracture. I am mindful of the fact that the single-fracture migration experiments being planned are only an intermediate step towards the design and conduction of large scale prototype migration tests. These will, of necessity, involve a variety of fractures of all orientations and apertures constituting an imperfectly interconnected system of conduits.

A distinct problem will be the evaluation of the hydrodynamic dispersion in the intersecting system of conduits, quite apart from the chemical processes that take place on individuals. Digital modeling of the mixing process that I undertook some years ago convinced me that a fracture network of diverse apertures has tremendous mixing capabilities, providing a great deal of dilution, smearing of the contaminant front, and corresponding decrease in concentrations of transported nuclides. This was done in absence of any assumptions or data concerning the microscopic behavior of individual species transported in the fractures. Thus the large scale transport evaluation will be greatly facilitated by the results expected from the Climax tests in granite as well as the G Tunnel tests in tuff.

Another implication of the variability of fracture apertures from place to place on a single individual is that you cannot expect to find reproduceability of results from one test spot to another. Indeed, the characterization of even a single fracture as a dispersive conduit would require many tests on that plane. The same is even more true in assessing the transportive-dispersive-retardive character of a system of intersecting fractures of different apertures. What you obtain from a single test is only an example of the possibilities. In the true far-field, with path lengths measured in hundreds of fracture spacings, then the dispersion takes on the form of an ellipsoidal cloud moving from a point source of contaminant. It is unlikely that field tests of a sufficient scale will ever be conducted to describe this phenomenon. We can look forward to testing small pieces of the entire flow field. I see no possibility of avoiding the extrapolation from the known to the unknown, approaching prototype scales. An intimate knowledge of the detailed processes is therefore going to be necessary. What happens in a single fracture is thus just the first step towards reality.

I take pleasure in the exposure to this research you are conducting, and hope I may be priviledged to be involved with the sequel.

Yours very truly,

David T. Snow

/gm

August 22, 1980

Mr. Robert M. Nelson, Jr., Project Manager
Nevada Nuclear Waste Storage Investigations
Department of Energy, Nevada Operations Office
P. O. Box 14100, Las Vegas, NV 89114

Dear Mr. Nelson:

In addition to the comments presented at the meeting as a collective report for the entire peer review committee, the following comments are also of my personal concern. They involve somewhat more specific and technical details.

Comments on Nuclide Migration Field Experiments (Tuff)

1) The program is an ambitious one involving three national laboratories; each laboratory contributes its field of strength to the main areas of responsibility in the program. The success of the program will depend, to a great extent, on how well the mutual collaboration among the three laboratories proceeds.

2) The proposed laboratory task appears to be loaded and time may run out before a guide to field test be obtained. It is recommended that priority should be set for the procedures and tracers in the laboratory migration experiments. For example, in water flow path test, select one or two most promising tracers, such as ^{3}H and/or ^{222}Rn and then conduct detailed, even repeated test, rather than using too many tracers with ambiguous results. In conducting radionuclide migration test, rather than attempting to use too many nuclides (which are also costly to produce), it is more efficient to concentrate on certain representative nuclides; ^{90}Sr may be chosen for cationic waste (^{137}Cs appears to be more sorptive), ^{129}I for halogon group, ^{99}Tc for multivalent elements, and ^{239}Pu for multivalent and actinide elements.

3) As retardation of nuclides is due to sorption phenomena, principally ion exchange, the exact mineralogical nature of the fracture surface must be studied in detail before and after experiment to understand the mechanism of ion exchange processes.

4) Although Eh of groundwater is hard to control and is important in migration of nuclides capable of existing in variable oxidation states, for Pu study, groundwater with high Eh may be used to advantage, as high Eh water can cause migration of Pu in the form of PuO_2^{2+} or PuO_4^{2-}. Ordinary groundwater is rather reducing and this may fix Pu to a stable, immobile form as

PuO_2, as indicated in Oklo natural reactor site.

5) Injection of a mixture of representative radionuclides in appropriate proportions should be attempted in the migration study. This simulates the natural condition more closely; the observed results may help understand such processes as common-ion effect, interaction, etc.

6) The investigators are lucky to access to so large a variety of analytical instruments. Thus, for characterizing any specific nuclide they are able to select the most optimum instrument.

7) If thermodynamic data are expected to be obtained through the laboratory study (as has been indicated), temperature should be controlled and specified in each experiment.

Comments on Field Radionuclide Migration Studies in Climax Granite

1) Although chemically similar to non-welded tuff, granite shows strong textural contrast to tuff. This physical difference may result in some sort of difference in migration pattern and thus increases our understanding of parameters controlling nuclide migration.

2) Livermore Laboratory appears to have great strength in mechanical design for experimental work, but has less access to various means for producing and characterizing radionuclides than Los Alamos group. In this regard, mutual cooperation and exchange of information between the two programs are not only imperative but also beneficial.

3) Natural fractures are very irregular. A fluid passage may be pinched out within the attempted length for flow path study. Drilling of at least four bore holes around the injection hole for groundwater collection provides better understanding of not only actual flow pattern but also the nature of the fracture surface in that particular geologic setting.

4) Same comments on choice of radionuclides in conducting migration test for tuff are also applicable here.

It is a great pleasure to serve on this peer review panel. I appreciate very much having this opportunity and look forward to such opportunity in the future.

Sincerely yours,

Liang-chi Hsu
Professor of Geology and Geochemist

ch/Lh

CC:
D. J. Isherwood, LLNL, Livermore, CA
B. R. Erdal, LASL, Los Alamos, NM

ACTION WMPO
INFO
R. F.
AMA
AM: & C ✓
AMD

September 5, 1980

Mr. Robert N. Nelson
Department of Energy
Nevada Operations Office
P.O. Box 14100
Las Vegas, Nevada 89144

Dear Bob:

Enclosed are written technical, editorial, and programmatic comments on the two field nuclide migration program plans. The comments are a combination of comments from both R. J. Serne and F. H. Dove, representing the ONWI funded programs WRIT and AEGIS, respectively. These comments are offered in addition to verbal comments voiced during the August 18 and 19 meeting in Las Vegas, and written concensus comments by the ONWI programmatic reviewers to be prepared by Dr. Bill Ubbes of ONWI.

Very truly yours,

Jeff

R. J. Serne, Staff Scientist
WRIT Program Manager
Water and Land Resources Department

F. H. Dove Harvey
AEGIS Project Manager
Water and Land Resources Department

RJS:dk

bcc: Jeff O. Neff
Bill Ubbes
Bruce Erdal
Keith Johnstone
Arnold Friedman
Dana Isherwood
Jim Duguid
John Kircher

TECHNICAL COMMENTS

I. TYPE OF TRACER INJECTION

After listening to other technical reviewers and later consulting persons within PNL, it appears that the most useful tracer injection would utilize elements dissolved in the ground waters being used at concentrations below simple oxide or hydroxide solubility limits. The type of injection should be a step input with a duration equal to that of the suspected water (or nonsorbing element) travel time through the fracture. Attempts should be made not to grossly exceed this injection period or not to significantly shorten the injection period. An estimate of the dilution factor expected for each nuclide can be calculated and then only tracers with a high enough solubility and sensitivity should be used. Recall that because of added dispersion due to a longer residence time the dilution of a sorbing species will be larger than for the water tracer, ^{3}H, itself. Also, the hydraulic perturbation in the fracture flow field due to tracer injection should be minimized, particularly in the fractured tuff experiment.

II. SCALING FROM BATCH LAB Kd's TO FIELD Ka's

Both programs need to discuss how laboratory experiments on crushed rock will be compared with intact rock. The main concern is in determining the effective surface area corrections between crushed material and intact fracture rock surfaces. The comparison of batch to column to in situ sorption must be performed via surface area normalization.

III. DISSOLVED ORGANICS IN ACTUAL GROUNDWATERS

To abate criticism on ignoring the organic question we suggest both programs measure the total organic carbon in the actual seep waters and offer to provide ONWI (and Dr. Jeff Means at BCL) with several gallons to 10 gallons of actual seep water. Dr. Means is under contract with ONWI to provide detailed analyses of orgainics in ground waters and to assess their significance. The last time we checked, Dr. Means was begging for real water samples to perform such work.

IV. MINIMUM RESIDENCE TIME OF NUCLIDES IN IN SITU FRACTURE

If migration of radionuclides, in the field fracture experiment, is assumed to be controlled by linear sorption equilbirium and linear sorption/desorption kinetics, then, assuming dispersion is negligible, radionuclide migration is described by the classical equations of chromatography. From the equations it can be shown that a radionuclide peak at a fracture inlet will at first travel with the carrrier velocity, if sorption kinetics are not infinitely fast, and eventually forms a retarded peak traveling at a modified velocity, reduced from the carrier velocity according to the linear soprtion equilibrium coefficient. The transition from one behavior to the other depends on the number of theoretical mass transfer units, n, passed by the carrier fluid, where:

$n = (aK_a/E)\ (kx/v)$

a = surface area/volume of fracture, cm^{-1}

K_a = distribution coefficient based on surface area, cm

E = void fraction in fracture

k = sorption kinetic constant, sec^{-1}

x = distance from inlet, cm

v = carrier velocity, cm/sec

Note that $K_a k$ is the desorption kinetic constant and aK_a is the same as the often used K_d.

Values of $n = 25$ are required before the reformed and retarded peak begins to take its expected position (within 5%) according to the sorption coefficient. This then becomes the criterion for whether kinetics are important, given the above typical assumptions, since once $n > 25$ the peak assumes its "equilibrium" position. From the definition of n, it's the carrier travel time, x/v, which determines if kinetics are important; the velocity alone should not be used as a measure. Furthermore, for $n < 25$, the peak location relative to the carrier "front" (e.g., the location of a peak of a non-sorbing tracer) is not determined by the sorption coefficient alone, and the peak location seems to correspond to a smaller than actual sorption ceofficient. Thus laboratory column experiments utilizing small lengths (x) and faster than realistic water velocities ($\dot{v}$) may force n to be smaller than 25 which would elevate kinetics to an important role. Again extrapolation to probable long term safety assessment scenarios leads to the conclusion that reaction kinetics observable in short term laboratory experiments are fast enough to be modeled by equilibirium concepts. This is, if kinetics are important to modeling real scenarios the rates would be so slow normal laboratory experiments could not detect the slow changes.

For the experiments in the field, both programs should make certain that the residence time for water is long enough to allow the formation of chromatographic peaks.

The LASL-SNL-ANL program has words to the effect that such provisions are being taken. Their derivation based on five sorption half lives may in fact be the same as that based on theoretical plates. In the engineering test plan they should show whether this is true. The PNL derivation for residence time for a fracture would be:

$$t_{res} = \frac{X}{v} = \frac{mE}{k\ aKa}$$

where for fracture flow

$$n \geq 25 \text{ and } E=1 \text{ and } a = 2/\text{aperture width}$$

$$t_{res} \geq \frac{12.5 \text{ aperture width}}{k\ Ka}$$

The kinetic constant k can be determined from a plot of the batch experimental data of the nuclide remaining in solution versus time. Assuming linear kinetics one should see an expontential relationship. Plotting the logarithm of nuclide remaining in solution versus time would give a slope equal to k.

The LLNL engineering test plan should also make these calculations and then make sure they do not exceed a velocity which violates this relationship.

V. ADDITION OF TRACERS

LLNL Plan (p. 34). We strongly recommend that I be added as a tracer. Perhaps ^{129}I, ^{125}I, or ^{131}I would be feasible. Iodine-129 can be counted on an intrinsic Gediode albeit the long half life and this low sensitivity.

LASL-ANL-SNL Cold Test (p. 21). Some very low sorbers like I, Br, MoO_4 should be added to the list because those listed may be sorbed and never give an effluent breakthrough curve.

Since the tracers may be added at rather high concentrations, LLNL expecially should consider whether linear isotherms for sorption are still valid especially near the injection point. Both programs should measure isotherms in the lab at equivalent surface areas and residence times as expected in the field.

VI. SENSITIVITY ANALYSIS

We feel that both hydrologic and mass transport calculations can be performed prior to the Engineering Test Plan reviews which could scope many of the uncertainties and areas of concern voiced during the 2 days of review. WIPAP, WRIT and AEGIS contributions in FY-1981 and early FY-1982 might be to use existing geochemical models, hydrologic models, and mass tranport models with field input data (values measured and possible ranges for parameters not known) supplied by the two projects. Several types of sensitivity analyses have been alluded in the other comments: Comment V--Residence Time, Comment I--Type of Tracer Injection. Other analyses of the experimental hydrology could be performed to help scope the probable results one will find after conducting the in situ experiments. Some simple mass transport calculations could be performed to elicit probable dilution factors and residence times of nuclides. Some geochemical calculations on existing computer codes could be performed to check thermodynamic equilibrium states for the groundwater.

EDITORIAL COMMENTS

I. AUGUST 1, 1980 VERSION OF FIELD RADIONUCLIDE MIGRATION STUDIES IN CLIMAX GRANITE

Page 5: groundwater is spelled as two words here but as one word in the rest of the plan.

Page 8, 25 and 45: Mispelling of aperture.

Page 10: Predic-tions is a typo.

Page 12: First line: characteristics are partially controlled.

Page 13: See technical discussion III dealing with surface areas.

Page 14: First line: Climax Stock.

Page 20: The authors dwell on the concept of equilibrium conditions when in reality water/rock in the seeps may not be in chemical (thermodynamic) equilibrium. The authors should instead try to prepare a water with the same geochemical composition as emanates from the crack once steady state flow is obtained in the nuclide tracer test.

Page 23: If it takes a few weeks or a month to fill the collector with seep water how will you allow for daily or frequent standardization of pH, Eh and specific ion electrodes (They do not remain in "calibration" for a month.)

The use of As species to calculate an Eh needs to be expanded to be useful. The Fe^{2+}/Fe^{3+} and S^{2-}/SO_4^{2-} couples or others also should be measured to check whether the system Eh is truly at equilibrium or whether mixed potentials exist such that the Pt electrode is really a mixed signal with little value for thermodynamic comparisons.

Page 24: Technicon Autoanalyzer

Page 27: Figure 7 is the aperture of the plexiglas system variable or fixed If fixed what is the aperture

Page 33: End of second paragraph: Water will be prefiltered to <u>remove</u> suspended solids >0.1 <u>μm</u>.

Page 35: <u>Possibly</u> as TcO_2 as WRIT has not conclusively proven the precipitate is TcO_2. Also LLNL should make calculations to see whether ^{99}Tc and Ce can be added to water in high enough concentrations to be measured after dilution. Technetium-99 has a very low specific activity and $Ce(OH)_3$ may limit it's use (see Technical point No. 1).

Page 36: What will be the source of ^{226}Ra as to our knowledge it is not currently available on the commercial market.

Page 40: Subtask 3.3 criteria No. 5. Why is it mandatory that the collector be capable of continous monitoring Although convenient it doesn't seem to be mandatory for technical reasons.

Page 41: Testing materials for "inertness" to nuclide sorption is a good idea.

Page 42: Point 2 RC should be Tc. Also by following ^{3}H to figure out a dilution factor one could underestimate the dilution of other nuclides because the breakthrough curve for the nuclides will be more disperse thus the peak concentrations will be lower than the ^{3}H.

Page 43: It is unlikely that nuclides will be present on the rocks at concentrations large enough to be detectable by electron microprobe. The statement about Tc precipitation and the resultant distribution of rocks implies that sorption distributes a nuclide selectively along the fracture while precipitation distributes a nuclide evenly along fracture. This may not be true, precipitation could knock all isotope out at a particular spot also.

Page 45: Instead of refering to MacLean et al.. the WRIT preferred methodology as presented in Relyea et al. 1980 "Methods for Determining Radionuclide Retardation Factors: Status Report" PNL-3349 should be used. Also how will surface area be measured to obtain Ka?

Page 50: Step 1 has a 25.2 week time yet shows only 6 weeks on chart. Step 36, 25 weeks of lab studies seems like a significant under estimation of time needed.

Page 54: The date of 9/30/80 for completion of the initial flow test is unrealistic.

Page 55: Laboratory support studies talked about here are confusing where do they show up in Figure 13? Figure 13 shows all lab work is over in FY-1981. Don't you mean work on retreived cores just to see where nuclides actually sorbed occurs in FY-1983? The write up reads like more lab sorption work is occurring.

Page 57: The technical quality is the responsibility of line management. Either LLNL defines line managers different than PNL or this is an odd relationship. At PNL line managers figure out who sits at what desk, they figure out salaries and sign time cards. They don't make too many technical decision, project managers do.

II. <u>AUGUST 1, 1980 VERSION OF LASL-SNL-ANL NUCLIDE MIGRATION FIELD EXPERIMENTS</u>

<u>Abstract Principal Objectives No. 2</u>. It may be more appropriate to state at depth experiments under closely <u>monitored</u> conditions.

Page 17: Third paragraph spelling aperture.

Page 21: The list of cold tracers is biased to medium to good sorbers and may not give any breakthrough. Program should include some mobile tracers like I, Br, MoO_4, etc.

Page 29: Smart and Laidlow typo.

Page 31: Second paragraph, "No problems in regard to the LASL capability to conduct the tracer investigation" is not a sentence.

Page 33: The spike injection should be step input with a duration equal to the water travel time. Core and block experiments should be controlled to have the same residence time as expected in situ <u>not</u> just have the same velocity unless pathlengths are identical.

Page 34: The proper reference to "AA" is atomic absorption spectroscopy.

Page 36: If autoradiography is the detection technique than the bromide and iodide tracers would have to be, <u>radio</u>tracers. Also where is the proof that β emitters can be detected from solid surfaces by autoradiography?

Page 38: Eh and pH probes makes more sense than Eh and pH gauges.

Page 39: Chemical interactions objective 2: The actual sucessful completion of mathematical descriptions of the chemical interactions is an extremely bold proclamation. Possibly a little "reality" (sic hedging) should be used.

Page 40: Third paragraph you talk about "the immobile phase" when all you mean is the crushed rock. Immobile phase has different meanings to different people. Soil physicists talk about mobile and immobile water phases and thus these words have technical jargon connotations which will make your generic usage confusing.

Page 41: Second paragraph, concentration in the "mobile phase" again gets confusing with technical jargon why not call it injected water? Last line, b<u>e</u>havior.

Page 45: Of primary concern is the necessity for control of the water and nuclides <u>residence times</u> (not flow rate!).

Page 49: More usage of immobile phase (rock) and mobile phase (water). Why not just use rock and water?

Page 50: Last line, Sec. X?

Page 62: Table V, pH and Eh probes not gauges.

Page 67: Reference 47, Glueckau<u>f</u>.

PROGRAMMATIC COMMENTS

1. Both these programs have requested support from WIPAP and WRIT. We at PNL want to honor this request. To facilitate the request there must be a formal allocation of manpower and resources from WRIT and AEGIS. As both WRIT and AEGIS are in the final negotiations on their FY-1981 program plan, it will be possible to include these commitments. As both programs had not budgeted for the commitments in the first draft, two options are possible: 1) ONWI could increase the budgets of both programs, or 2) ONWI and WRIT/AEGIS could agree to change scopes and drop some proposed activities. The WRIT and AEGIS project managers plan to facilitate such decisions with ONWI in September 1980.

2. Specific areas of interplay between WRIT and AEGIS and the two field nuclide migration programs include:

   a. Hydrologic Modeling of fractures, usage of existing hydrological and mass transport codes, pretest sensitivity analysis, pretest parameter selection (to assure all necessary model input variables will be measured).

   b. Detailed geochemical modeling of laboratory and in situ tests, determination of thermodynamic equilibrium conditions for rock/groundwater interactions, prediction of predominate aqueous species for macro and trace constituents.

   c. Experimental design and methods for laboratory sorption experiments.

3. WRIT staff suggest that experimental details on laboratory experiments dealing with element retardation be pre-screened by WRIT prior to commencement of significant work for the following reasons:

   a. Although the laboratories involved are all familar with WRIT methodology and in general agreement on the basic actions needed to design useful sorption experiments, the details and choice of parameters to measure, to control, optimum ways to execute such control and values to chose (such as flow rate) in the past have led to disagreement and controversy. The net result has been the production of some incomplete and marginal quality data.

b. Such an active role would allow WRIT staff to actively pursue the role of liason to the AEGIS-SCEPTER modelers. WRIT staff have experience and are quite active in the area of "translating" laboratory data into accurate and useable model input data. WRIT staff frequently help the modelers interpret input and output for and from the models to assure proper results are produced and that the GIGO syndrome is lessened.

4. PNL suggests that the following programs and individuals receive copies of the quarterly reports and topical reports in draft form as soon as they are transmitted to DOE and ONWI. Such a request is to facilitate timely and continual exchange of technical information and progress status.

WRIT

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Battelle
505 King Avenue
Columbus, Ohio 43201
Telephone (614) 424-6424
Telex 24-5454

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September 15, 1980

TO: Distribution

Attached for your review is the Summary of Radionuclide Migration Studies Programmatic Review Session held at the Nevada Operations Office on August 18-19, 1980.

If you have any questions or comments, please call me.

Sincerely,

R. A. Robinson

for W. F. Ubbes
Project Manager
Field Testing

WFU:dlm

Enclosure

Distribution:

- R. J. Serne, PNL
- F. H. Dove, PNL
- S. Pahwa, Intera
- G. Kilp, WAESD
- D. Isherwood, LLL
- B. Erdal, LASL
- J. O. Neff, RL/C
- R. Nelson, NVO

SUMMARY OF RADIONUCLIDE MIGRATION STUDIES
PROGRAMMATIC REVIEW SESSION

DOE-NVO, 8/18-19, 1980

ATTENDING:

J. Serne, BPNL/WRIT
H. Dove, BPNL/AEGIS
S. Pahwa, INTERA/SCEPTER
G. Kilp, WAESD
W. Ubbes, ONWI

1.0 GENERAL COMMENTS

1.1 Programmatic Direction

1.1.1 There appears to be a general loss of programmatic direction as to the interrelationship between the RNM program and the WRIT and WIPAP programs. ONWI needs to better define interfaces between the modeling, lab, and field efforts.

1.1.2 The program plans do not adequately assess the amount of lab effort required to support these programs. Are LLL and LASL counting on WRIT to supply this?

1.1.3 Relationship of the in-house modeling efforts to WIPAP need to be better defined. What is the scope of the in-house modeling efforts?

1.2 Techniques

1.2.1 The diversity of techniques between the two programs is very good. LLL appears to be concentrating on <u>when</u> and <u>how much</u> of the tracer comes out at the end of the flow path, while LASL is concentrating on <u>where</u> it goes in the fracture. <u>Both</u> pieces of information may be necessary. Exhumation of the entire fracture (in tuff) to map field lines is a very good idea (though not possible in granite).

1.2.2 "Pulse" <u>versus</u> "slug" tracer injection: Perhaps <u>both</u> methods should be used; injection characteristics (mixing, etc.) will determine whether or not it is even possible to inject a tracer pulse (spike). Slug injection should last as long as travel time of water. The <u>trailing</u> edge of the tracer is important too.

Modelers can make good use of the information gained from the different methods of injection (one-point injection/one-point collection <u>versus</u> multi-point injection/collection flow field with one-point tracer injection).

1.2.3 The residence time of the tracer/water is very important. Chemistry must be given time to work. Experiments must adequately check basic assumptions as to linear sorption chemistry. LASL/ANL has the best handle on this with concept of "sorption half-life". Equilibrium may be more important than kinetics. Flow rates of $\sim$1 m/month may be necessary to check this.

Refer to pages 33-34 of LASL plan. Current models do not really involve chemistry other than linear K_d's. Statements on the top of page 34 may not be accurate.

1.2.4 Emphasis on isolating and duplicating groundwater is perhaps unnecessary. It is important, but level of control proposed is perhaps too high.

1.2.5 Plans miss the point of saturated versus unsaturated flow. There is no clear boundary to the flow path; hence a two-phase flow problem near the boundaries. Also, it will be impossible to duplicate the flow field from one test phase to the next. Thus, a nonsorbing tracer should be included with every test. This is especially important to LASL, where plans are to use different locations of some fracture. (What is the point of using different locations?)

1.3 Analysis and Bench Tests

1.3.1 Sensitivity analyses need to be done to get a handle on what to expect as to concentrations, practical level of resolution, solubility levels, arrival times, etc.

1.3.2 Parallel Plate Test. These bench tests may be okay for testing and calibrating injection on collection hardware, but will not be worthwhile for flow field investigations. Parallel plates set up in (for example) ground granite cores would be more accurate. Edge effects will influence flow field with lucite plates. A day's work with a computer model will benefit bench tests greatly as to distance between injection and collection points. It would be a good idea to have a variable distance between the plates.

These bench tests will also be useful for flow intercept tests and to check predictive ability of models.

2.0 COMMENTS ON LASL/SLA/ANL

2.1 "Motherhood"

2.1.1 This program plan sounds like a "shopping list" -- efforts need to be prioritized. Engineering test plan should be more specific. However, present contingencies in plan shows a good grasp of what problems might be faced. GO/NO GO decision points are a good idea for program plans.

2.1.2 Derivation of relationships between lab and bench and field tests is missing (e.g. Figure 10 of LLL plan).

2.2 Tracers

2.2.1 Tracers mentioned for cold experiment (except for U-235) will sorb like crazy and never be observed at collection point. Plutonium will not be useful as a tracer. Plan should be revised to better reflect Friedman's presentation of 8/18; that is, include some more mobile tracers.

2.2.2 There are questions as to the reliability of the cyclotron within schedule constraints. An appraisal should be made as to commercial availability of isotopes. How important is it to have such ultra-low concentrations, especially in elements that exist in nature?

2.3 Techniques

2.3.1 Pulse injection of tracer downstream of water injection could alter flow field and lead to mass balance problems from three water sources -- existing water, established flow, and tracer injection.

2.4 Other

2.4.1 The issue of matrix porosity needs to be resolved. Will diffusion into the matrix be a problem? (This is, however, a low priority issue.)

2.4.2 Reference page 44, Item N: "If the technology...is sufficiently well developed..." What is expected here? Should colloids be investigated at all?

2.4.3 Should discuss natural analogs -- Oklo, uranium deposits, etc.

2.5 Terminology

2.5.1 Need euphonic acronym for LASL/SLA/ANL.

3.0 COMMENTS ON LLL

3.1 Recognize that many of the comments on LASL plan apply here also.

3.2 In contrast to LASL plan, LLL may be oversimplifying things. This implies LLL may not appreciate complexity of problem. Some contingencies would be a good idea.

3.3 If actinides are to be included as tracers (see Peer review comments attached). Should concentrate on neptunium and uranium.

4.0 SUMMARY

These program plans are, on the whole, very satisfactory. The effort in preparing them was well spent.

FINDING OF RNM PEER REVIEW

COMMENTS ON TESTS

GRANITE

1. Why wouldn't block test in laboratory be a valuable addition?.

   a. as an aid in designing in situ test

   b. as supplemental method of achieving objectives.

   Dana: Yes, but can ONWI afford it?
   Committee: Do it regardless of cost.

2. We suggest that in conjunction with block test, a post-test evaluation procedure of coring (pilot hole to inject plasticizer) to investigate sorption sites is needed before application in field. How many cores/m^2 are needed?

3. We suggest that actinide migration should be included in this program rather than subsequent programs.
   (1) Neptunium
   (2) Plutonium
   (3) Uranium

TUFF

1. Applaud idea of appropriate block tests for actinides as a way of better defining the conditions to be studied in situ. Emphasize necessity of pointing towards in situ test.

2. Concerned about effect of diffusion into porous medium.

3. Concerned about how to extrapolate from 1 - 1.5 m flow field in single fracture to next appropriate scale, for more complex geometrics and test configurations. (Test configuration is unrealistic for larger scales).

Assessment of radionuclide migration is critical to DOE's waste management program, and we believe these two programs with their different approaches in different rock types will provide an important contribution to the understanding of the parameters controlling migration. A careful coordination between programs will be very beneficial.

University of California

LOS ALAMOS SCIENTIFIC LABORATORY

Post Office Box 1663 Los Alamos, New Mexico 87545

In reply refer to: TRN-CNC7-12/80-3
Mail stop: 514

December 15, 1980

DATE	ACTION		INITIAL
		NO	
		KUNICH	
		NELANDER	
		BROWLEY	
		Secretary	
		Sandia Lv	

APPROVED

FILE

R. M. Nelson, Project Manager
Nevada Nuclear Waste Storage Investigations
U. S. Department of Energy
Nevada Operations Office
P. O. Box 14100
Las Vegas, NV 89114

Dear Bob:

Enclosed are the LASL/SNL/ANL replies to the peer reviews of the project entitled "Nuclide Migration Field Experiments" to be carried out in tuff in G-tunnel at the Nevada Test Site.

Sincerely yours,

Bruce

Bruce R. Erdal
Technical Project Officer

BRE:kk

enc: a/s

xc: M. P. Kunich, NV
R. C. Lincoln, TOC, SNL
D. C. Hoffman, CNC-DO, MS-760
K. Wolfsberg, CNC-7, MS-514
D. R. Fortney, SNL
A. M. Friedman, ANL
D. Isherwood, LLNL
TRN file
BRE file

WMPO

NUCLIDE MIGRATION FIELD EXPERIMENTS
G-TUNNEL, NEVADA TEST SITE

Replies to Peer Review Comments

A peer review of the following document was held on August 18-19, 1980, at the Nevada Operations Office of the Department of Energy, Las Vegas, Nevada:

B. R. Erdal, K. Wolfsberg, J. K. Johnstone,
K. L. Erickson, A. M. Friedman, S. Fried, and
J. J. Hines, "Nuclide Migration Field Experiments:
Program Plan," Los Alamos Scientific Laboratory
report LA-8487-MS (1980).

The project will be carried out by the Los Alamos Scientific Laboratory, Sandia National Laboratories, and Argonne National Laboratory.

The peer reviewers were:

G. E. Grisak, Environment Canada
J. M. Cleveland, U.S. Geological Survey
L.-C. Hsu, University of Nevada
J. K. Osmond, Florida State University
P. A. Witherspoon, Lawrence Berkeley Laboratory
P. R. Fenske, University of Nevada
F. P. Sargent, Whiteshell Nuclear Research Establishment
D. T. Snow, Georesults, Inc.

In addition, several programmatic reviewers selected by the Office of Nuclear Waste Isolation (ONWI) were also present:

F. H. Dove, AEGIS project, PNL
R. J. Serne, WRIT project, PNL
W. F. Ubbes, ONWI
S. Pahwa, SCEPTER project, Intera
G. R. Kilp, Westinghouse

The following summarizes the replies to the comments of the reviewers. It is the composite of replies received from B. R. Erdal, K. Wolfsberg, and W. R. Daniels at LASL, D. R. Fortney and J. K. Johnstone at SNL, and A. M. Friedman at ANL.

I. G. E. Grisak

A. Will matrix diffusion be addressed?

As indicated by the reviewer, we are well aware of the need for quantifying diffusion of solute into the relatively immobile pore waters in the tuff matrix. However, the program plan has been changed to further emphasize this need. A laboratory program to obtain the requisite data is briefly described. Existing or developing models that include this effect will be used in the design and interpretation of the laboratory and field experiments.

B. Verification of the flow field

As indicated in the program plan, we intend to try to develop flow path tracers (e.g., $^{222}Rn \rightarrow Bi$, Pb) which would be identified during the post-experiment analysis of the fracture and matrix. Of course this will not identify the pressure distribution. We are concerned with the reviewers suggestion to use pilot holes. Since the tuff being considered may not have sufficient mechanical strength near the fracture, the pilot holes will perturb the flow field. Also, the suggestion sounds difficult and costly.

C. Type of tracer injection

We agree that a pulsed tracer input gives the most information from this type of experiment, particularly when the duration of the tracer injection is optimized using available models of the flow system (see Sec. I.A) and laboratory studies. Tracer mass balance will be achieved in the laboratory and field experiments by collection of all the fluid exiting the fracture and by the post experiment analysis. We are concerned that much information will be lost if continuous injection is used and the total amount of tracer involved would be too great. The latter is important when special tracers are used. In any case, the concentrations of the tracers will be below the solubility limit of that element in the groundwater used for the experiments. This may require use of continuous tracer injection in order to have enough tracer to measure. These concerns will only be answered by sensitivity analyses based on the experimental and modeling efforts.

D. Volume of water required

As indicated in the program plan, we plan to keep the residence time of a slug of water in the fracture sufficiently long that the chemical interactions with the rock surface will be maximized. The residence time will be optimized based on the laboratory flow and kinetic studies but should be in the range of 0.5 to several days. Therefore, the volume of water used will not be terribly large. However, since natural groundwaters from wells or seeps equilibrated with rock from G-tunnel will undoubtedly be used, sufficient quantities are available.

E. Applicability of results of larger-scale tests

The principal purposes of these field experiments is to develop the experimental methods to conduct these small-scale field experiments and to address the validity of data obtained in the laboratory to field conditions. Obviously, we would be severely remiss if we did not keep in mind the use of such results and methods for future field investigations.

II. J. M. Cleveland

A. Are budget and manpower sufficient?

We have attempted to be as accurate as possible in our budget request for this project in all its complexity. However, tradeoffs may have to be made to accommodate changes during the course of the project, particularly since we attempted to keep the budget as low as possible. It should be noted that the cost of this project is only a factor of two more than the project to be carried out by the Lawrence Livermore Laboratory.

We are rather concerned that the project plan is described as a "shopping list." We feel that it is the duty of the planners of a complex project to present the options available to meet the experimental objectives. Indeed, we could be criticized for not listing them. Perhaps, this document more properly should be called a technical concept rather than a program plan.

B. Are review processes adequate?

We believe that the scheduled reviews at the critical stages in the project are more than adequate. Furthermore, the NNWSI project has a history of extensive yearly peer reviews of which this effort will be a part. Informal reviews will also be done at the individual laboratories and by the project as a whole. The quarterly progress reports will be widely circulated.

C. Control of groundwater conditions

We agree that control of the groundwater conditions is very important, as mentioned in the program plan. Specific details for control or monitoring of conditions or parameters such as Eh, pH, and temperature will be developed in the laboratory studies. An engineering test plan will be issued for each field experiment and the control and montoring methods will be described. (It should be noted that the in situ conditions in the experimental site are expected to control many parameters, more or less.)

III. L.-C. Hsu

A. Will laboratory tasks be accomplished in time to support the field experiments?

We are making every effort to ensure that the detailed laboratory studies, including the block tests, are completed in sufficient detail prior to the first field experiment so that intelligent planning and realistic predictions are possible. This is reflected in the detailed logic networks given in Figs. 11-13. As described in the program plan, we do plan to limit the radionuclides used in the field experiments with the selection being based on the conclusions of the laboratory studies, nuclide availability, NWTS priorities, etc. The radionuclides listed are only those that may be of interest.

B. Will the mineralogy of the fracture be determined?

As described in the program plan, we plan to perform a detailed post experiment analysis of the flow path in each field and large scale laboratory experiment. Indeed, this is the unique portion of the project. The analysis will include determination of the mineralogy, as well as the location of the transported elements, the flow path of the water through the fracture, diffusion into the matrix, etc. The mineralogy will also be determined along another portion of the same fracture during the detailed site characterization prior to the field experiments.

C. Importance of Eh

We are aware of the importance of the redox potential on groundwater chemistry and behavior of certain multivalent elements. Indeed, knowledge of the effects of Eh and measurement methods for Eh are major areas of uncertainties in our capability to model element transport. However, we cannot run a meaningful field experiment if we significantly alter the natural conditions of that fracture. This includes the mineralogy, groundwater composition, etc.

D. Injection of mixtures of radionuclides

We plan to inject mixtures of radionuclides in the field and laboratory experiments using several "staged" injections of the migrating element and water movement tracer. We plan to use as few injections as possible. We do not expect to have common-ion effects since the concentration of each of the radionuclides will be significantly less than the solubility or less than say 10^{-8} M.

E. Will temperature be controlled?

Temperature cannot be controlled in field experiments of the type described in the program plan since the rock mass will do this. We will monitor the temperature. The laboratory experiments will also be conducted at known temperatures.

IV. J. K. Osmond

A. Laboratory vs. field studies

As described in the program plan the laboratory studies are a major component of the total project. Sufficient information and understanding must be available from the laboratory studies that the field experiments can be designed and operated properly. Furthermore, the interpretation of the field experiments depends critically on the laboratory studies which obviously implies that the laboratory work will not be terminated when the field work is initiated. However, the validity of extensive laboratory studies under field conditions must be proven. This is the principal goal of the project.

B. Matrix diffusion and fissure permeability

The question of matrix diffusion versus fissure permeability in nonwelded tuff is significant and we are well aware of its importance. The program plan has been changed to further emphasize the need for quantifying diffusion of solute into the matrix. A principal purpose of the project is to identify the critical information that must be available in order to describe nuclide migration in a fracture in a porous, relatively non-permeabile matrix.

C. Specific radionuclides for "hot" experiment

The candidate radionuclides to be used in the "hot" experiment are given in Table I of the program plan. Final selection will be based on the laboratory studies, availability of the radionuclides, NWTS priorities, etc. The nuclides selected and the reasons for their selection will be addressed in the experiment and engineering test plans.

V. P. A. Witherspoon

A. Saturated system, trial injections, and water movement tracers

We agree that it is necessary to have a saturated fracture for the experiments to be meaningful. We will try to minimize dehydration of the nonwelded tuff, which is originally near 100% saturated, by performing the experiments some distance in from the drift wall and by flowing the groundwater through the experimental area for some weeks prior to injection of tracer. We will perform fracture and matrix permeability tests as part of the detailed site characterization. We will also perform a limited, near-wall, fracture-flow test to develop operating parameters to optimize fluid return. However, it should be noted that the detailed post-experiment analysis of the fracture should allow a detailed mass balance to be obtained.

We plan to develop non-sorbing tracers that can be used to measure the movement of the water front when diffusion into the matrix is a strong possibility. Other non-sorbing tracers will be used to assess the degree of matrix diffusion. These tracers will be used as part of the characterization of the site before the sorbing tracers are injected.

B. Recovery of the fracture surface

We agree that recovery of the fracture surface with minimal alteration is difficult. However, removal of the fracture and associated bulk rock should not be difficult in nonwelded tuff since its mechanical strength is not great. Indeed, this is one of the reasons that nonwelded tuff was chosen for this project. Without extensive laboratory work, we would hesitate to stabilize the fracture by use of grout or other material since the stabilizing material itself could change the distribution of the elements on the surface. However, this idea will be examined as time permits.

Since the post-experiment analysis of the total fracture surface will be made, we will be able to address whether the same information could have been obtained by drilling. This is particularly true since we are trying to develop a method of "mapping" the flow field using, for example, the detection of the daughter products of ^{222}Rn which would be dissolved in the groundwater. The identification would be done during the post-experiment analysis. Detection of the injected fluid during the course of the experiment is outside the current scope of this experiment.

VI. P. R. Fenske

A. Need for analysis of the water flow path

As indicated earlier, we are very aware of the need to know the water flow path. This is the reason for developing the post-experiment analysis methods, including the flow path tracers (^{222}Rn daughter products, dyes, etc.).

B. Matrix diffusion, fracture flow, and water velocity

We are well aware of the need to quantify diffusion of solute into the matrix and to optimize the groundwater residence time in the fracture. These are addressed in program plan in some detail and earlier in this report.

C. Representative groundwater

We are certainly aware of the problem of collection of representative groundwater for the experiments. As indicated in the program plan, an extensive set of experiments and analysis of "natural" groundwaters is planned. The principal criteria for the selection of a suitable groundwater is that it must not change composition during an experiment. We will monitor this composition before and after flow through the fracture.

VII. F. P. Sargent

A. Relationship to the LLL experiment

As indicated in the program plan, the LASL/SNL/ANL and LLL experiments are both being coordinated through the NNWSI. A formal interchange of ideas, plans, etc. exists via the NNWSI project officers, reviews, formal interfaces, etc.

B. Need cost breakdown

We do not think that a program plan, or more properly a technical concept document, should include detailed costs for each task. The document presents detailed work-flow diagrams that identify the sequence of tasks that must be accomplished to meet the project objectives and the estimated time to accomplish the tasks. Since this is a planning document, only the yearly estimated costs can be given. However, we have attempted to be as accurate as possible in our budget estimates.

C. Need better definition of field work

This document was specifically designed to present the options available to meet the objectives of the experiments. Perhaps it should more properly be called a technical concept document, as indicated earlier. Based on the laboratory and field investigations, the experiment test plans will address the specifics of the field work.

D. Need to investigate the nature of porosity and matrix diffusion in tuff

The concerns with respect to porosity, matrix diffusion, and pressure effects on matrix diffusion are addressed in the program plan and earlier in this report. We believe that sufficient experimental work will be done to adequately quantify this process. However, the program plan has been changed to further emphasize this need.

E. Groundwater equilibrium

As indicated earlier, we are aware of the need to have a groundwater for the experiments that is in equilibrium with the rock. We believe that we have identified a set of measurements and studies in the program plan which will solve this problem.

F. Avoid use of crushed material

In general, we agree that use of crushed material for sorption studies in the laboratory should be minimized. However, there is evidence that, at least for rocks having a high porosity which are not crushed below the grain size, sorption behavior on the crushed and consolidated material are the same. We will emphasize use of consolidated material (whole cores, and blocks) but we will do some parametric studies on crushed material because of their relative simplicity.

VIII. D. T. Snow

A. Comments on hydraulics

We appreciate the comments on the possible hydraulic potential distributions in the tunnel systems at the NTS, particularly with respect to the possible origin of the drainage ("seeps") into the sub-surface workings. We will investigate the possibility of measuring the hydraulic potential in the injection and sampling holes in the experimental array.

B. Non-ideality of fractures

We are very aware that natural fractures are not ideal and thus cannot be described by a parallel-plate model. Indeed, this is mentioned in the program plan and is the principal reason for development of water flow path tracers.

C. Groundwater collection

As discussed in the program plan, we are very aware of the need to have a proper groundwater for the experiments. The principal criteria for selection of the composition of this water is that it must not change composition during the experiments. It is not clear that "seep" waters are at or near equilibrium since the residence time in a given tuff horizon may not have been long enough to reach equlibrium. In addition, the seeps may not be from the same tuff horizon as the one in which the experiments are to be run.

D. Distance from drift wall

We agree that it is necessary to conduct the field experiments some distance in from the tunnel wall. We have always used two to three tunnel diameters as a rule-of-thumb. As indicated in the program plan, the layout for the field experiment includes this distance. Tracer injection and, most likely, some of the water collection would be done by use of angle holes from the tunnel wall. Preliminary drilling in HF-23 seems to indicate that the bedding plane opening continues deep into the formation away from the excavation face. In any case, we are trying to develop the experimental methods to run such field tests which particular emphasis on the chemistry, dispersion, and diffusion that occur and on ascertaining whether data obtained in the laboratory are valid in the field.

E. Reproducibility of results

The goal of this project is to develop methods that allow an "intimate knowledge" of the "transportive-dispersive-retardive" processes that occur in a fracture in a porous, non-permeable rock. That is the principal reason for the detailed post experiment analysis and large-scale laboratory tests. The reproducibility of such results will be addressed by running two field experiments with at least some elements common to both; these will be run in a different part of the same fracture. The large scale laboratory tests will also be performed using samples from the same fracture.

IX. R. J. Serne and F. H. Dove (Programmatic reviewers)

A. Type of tracer injection

We agree that the concentration of any radionuclides or tracers must be significantly below the solubility limit. However, it must be emphasized that this is the solubility in the particular groundwater used for the studies. As such the tracer concentrations used may have to be arrived at empirically.

Our first choice for the type of tracer injection would be a Dirac delta function. Since this may be hard to achieve experimentally, a square input is our next choice. We have not yet optimized the duration of the injection using models that seem to be applicable to our system, i.e., include dispersion, matrix diffusion, etc. Therefore, we cannot say with certainty that the duration of the injection should be equal to that of the water travel time through the fracture.

B. Scaling from laboratory to field

Although this is not specifically discussed in the program plan, methods of obtaining any information (e.g., surface area, cation exchange capacity, porosity, etc.) necessary for scaling from laboratory to field are described. However, we cannot categorically state what information is critical.

C. Dissolved organics

We were aware that Dr. J. L. Means was under contract to ONWI to provide analyses of organics in groundwaters and solids. However, negotiations with Dr. Means had not been completed at the time of the peer review and so were not mentioned. Subsequently, Dr. Means has agreed to do these analyses.

D. Minimum residence time

As indicated in the program plan and as indicated by the reviewers, we are very aware of the need to optimize the residence time of the groundwater in the fracture in order to maximize chromatographic behavior. The formula for the number of mass transfer units given in the reviewers comments is

$$n = \frac{K_d}{\varepsilon} \cdot \frac{kx}{v}$$

where n is the number of mass transfer units, K_d is the distribution coefficient, ε is the porosity, k is the desorption rate constant, x is the column length, and v is the fluid velocity. This formula was developed in the 1940's and is known as the Thomas solution in the theory of chromatography. It was not developed at PNL.

We have always endeavored to keep the flow rates for a given column low enough such that enough mass transfer units are available to provide equilibrium. The behavior of ^{85}Sr on a column of tuff from Yucca Mountain is an example. Batch desorption studies show no significant change in the R_d from 20 to 40 days. The average deviation was less than 10 percent. If the change were in fact 10 percent over a period of 20 days the "half life" for desorption would be about 6 days and therefore $k = 1.3 \times 10^{-6}$. The K_d is 50 and the porosity was 0.3. Therefore, the number of mass transfer units is more than 22. This is a conservative estimate and since the K_d's in all of the other experiments we have run are higher and desorption experiments exhibited no rates of desorption which were much faster, this represents a worst case and a lower limit to the number of mass transfer units. Furthermore, the columns were run at two flow rates and no change in the retardation factor was observed. In spite of this, "anomalies" have frequently been observed.

We believe that it is necessary to do the rather complete set of laboratory studies described in the program plan along with the development or implementation of a transport model including matrix diffusion and the results of the flow tests in the near-wall array before the actual flow parameters are fixed. Obviously, planning and design would be done using simpler models and less detailed information.

E. Tracers

We state in the program plan that water movement tracers, like iodide and bromide among others, will be used. Indeed the list of candidate tracers includes ^{125}I and ^{131}I. As indicated in the program plan (Sec. IV.L) we plan to measure the sorption isotherms, as well as many other parameters, for all tracers involved in the experiments.

F. Sensitivity analysis should be done

The analyses are planned and are presently being defined. Existing and developing fracture flow models that include matrix diffusion are being evaluated. Subsequently we will determine parameter sensitivities, optimize injection and residence times, etc. These calculations will also be done to aid in design of the water and tracer injection systems and the water collection systems. These analyses will be coupled to the laboratory experiments to evaluate and enhance the applicability of the models to the field experiments. Existing thermodynamic geochemical models available at LASL and the USGS will be used to estimate the degree of equilibrium in the groundwater composition.

G. Editorial comments

The editorial comments are appreciated and have been incorporated as appropriate. We prefer the use of immobile phase to rock since we are concerned with the rock and the pore water within the rock.

H. Programmatic comments

1. AEGIS and WRIT support. Any work performed by the AEGIS and WRIT projects in support of this project must be under the direction of the LASL/SNL/ANL project. This is due to the need to meet the milestones and objectives of this project on a timely basis since we are on severe time constraints. In addition, the project personnel are in the best position to define immediate needs. However, we have not specifically requested such support and no funding is available for these efforts. Comments from WRIT and AEGIS are welcome; recommendations will be given due consideration. All published information detailing experiments and results will be distributed in a timely manner to the AEGIS and WRIT projects, as described in the program plan. In particular, any deficiencies in the transport models will be communicated to the NWTS participants.

2. WRIT pre-screening. It would be impossible to meet the costs, milestones, and technical objectives of this program if we had to submit our every action to review prior to performing that action. Peer reviews are the only reasonable alternative. Such reviews, along with published data, will serve as the principal connection to the WRIT project and to the WIPAP modelers. In

principal, however, we are in agreement with the need to use standard procedures when they have been developed and agreed upon. Note that many of the methods used by WRIT investigators were developed at LASL or ANL which implies that we are scientifically capable of designing useful experiments. We believe that we are in the best position to "translate" our data, when needed, to input data for models since we are familiar with the information and whether the models include the effects addressed by the data or experiments.

X. W. F. Ubbes (Programmatic reviewer)

A. Programmatic direction

We believe that the program plan more than adequately describes the interfaces to WRIT and WIPAP and the amount of laboratory work required. It is essential that the field project have control of the work done. We do not envision that any effort would be done by WRIT. Some pre-experiment predictions and other calculations may be done by AEGIS or SCEPTER but only in support of the field project and not funded by the field project.

B. Techniques

The concerns with respect to mode of tracer injection, groundwater residence time, groundwater composition, and sensitivity analyses were addressed earlier in this report.

The reviewer did not understand that we plan to inject a water movement tracer and a suite of sorbing tracers in each phase (i.e., staged injection) of the two field experiments. Please note that tuff is nearly 100% saturated. Lastly, we cannot use the same fracture for the two field experiments if the flow path is exhumed for detailed post-experiment analysis.

The suggestions concerning parallel plate tests are already part of the program plan.

C. "Motherhood" statements

The purpose of this technical concept document is to cut across a variety of potentially important and useful experimental elements. We agree that priorities and specific details must be established early in the program. However, we would be remiss if we did not list the options at this early stage. The priorities and specific details will be reported in experiment and engineering test plans addressing program tasks.

D. Tracers and mass balance

As indicated in the program plan, the tracers listed are only candidates. The final selection will depend upon the laboratory studies, nuclide availability, NWTS priorities, etc. We cannot definitely say that plutonium is not useful since, although the R_d values may be high (unknown), colloid migration must be considered.

The exhumation of the flow path and matrix will allow a detailed mass balance to be made. This will allow the determination of when, how much, and where the tracer moves.

E. Other

Since this is a program plan for field and laboratory experiments, we cannot see the need to discuss natural analogs. Obviously, this could be done since LASL is heavily involved in such projects.

Since colloids have been observed to form in groundwaters in the laboratory and have also been found to migrate in whole-core column studies, behavior of colloids in large-scale laboratory tests and field experiments must be studied.

The issue of matrix diffusion is not a low priority issue and cannot be dismissed easily. This has been addressed in comments earlier in this report.

XI. S. Pahwa and G. R. Kilp (Programmatic reviewers)

No comments were received from these individuals.

LAWRENCE LIVERMORE LABORATORY

INFO December 10, 1980

ACTION WMPO

INFO

FILE from Granite RWM

Robert M. Nelson, Jr.
Waste Isolation Branch
Engr. & Energy Applications Division
Nevada Operations Office
U.S.D.O.E.
P. O. Bo 14100
Las Vegas, NV 89114

Dear Bob:

The following comments are in response to the Peer Review of LLNL's "Field Radionuclide Migration Studies in Climax Granite" held August 19-20, 1980, at NVO in Las Vegas. The Program Plan has been revised and released as an LLNL document (number UCID-18838). It will be sent to you separately this week.

We appreciated that all the reviewers agreed that the research outlined in our proposal is needed and will contribute significantly to the NNWSI Program. In general, there appears to be no concern for the technical quality of either the researchers or the experimental concept. Many of the comments are general statements or advice that provide food for thought, but which do not require a separate response or changes in the Program Plan. For example, Osmond advises us to "pursue laboratory studies vigorously", Grisak says "keep large scale in mind when designing test details", and Cleveland reminds us that it is "important that data from the two tests be comparable".

While many of the comments are general, there are a few which are so specific in terms of experimental or equipment design that they will have no effect on the Program Plan, but will guide us in the development of the Engineering Test Plan. We plan to investigate fully 1) Witherspoon's suggestions regarding the immobilization of sorbed or precipitated species via injection of grout to protect the fracture surfaces prior to the back-coring phase; 2) Grisak's advice on how to measure and verify the flow field between the boreholes using small pressure pilot holes with appropriate instrumentation; and 3) Snow's recommendations on determining the degree of saturation prior to the radionuclide migration experiments.

In some cases where it was suggested that we put more or less emphasis on a particular aspect, we have chosen to remain neutral and made no changes in the Program Plan. We believe that the Program Plan needs to be general and maybe a bit vague at times to allow for changes in direction and priorities as the project evolves.

Two areas of concern raised by the Peer Reviewers warrant a more detailed response: 1) the addition of block tests similiar to those described in the tuff Program Plan, and 2) the use of actinides.

Block Tests. The immediate response of the Peer Reviewers at the time of the oral presentations was that laboratory studies using large granite blocks should be included as a means of perfecting techniques to be used in situ, and also to provide a basis for comparing laboratory and field data. It was felt that we would have a better understanding of the flow field (e.g., sheet vs. ribbon flow) with block tests. We would also be able to define the boundary conditions and devise analytical methods of locating sorbed or precipitated species on the fracture surface. The Peer Reviewers' written comments show less concern with the need for block tests (with the exception of Cleveland). Although Osmond recommends that "more review of the possibilities is warranted" and Sargent encourages us "to perform block tests", the reviewers' suggestions appear to be more a matter of looking into the feasibility of block tests, rather than a necessity for including them in the planning at this time.

Even under the best of experimental conditions, block tests can't provide all the answers. Considering the variability that exists in the physical characteristics of different fractures or even within the same fracture (e.g., aperture), we cannot hope to gain a complete understanding of the flow conditions we will encounter in the migration experiments by using a single large block or even several large blocks. Including block tests as a part of our project is a significant undertaking. It would be costly and could delay the field experiments. The additional cost of block tests cannot be supported by present funding. In the next couple of months, we plan to evaluate the feasibility of excavating a large block without disturbing the fracture orientation and of performing flow and migration tests that simulate the field conditions. If we can demonstrate that block tests will provide needed information, we will submit a proposal outlining our approach and ask for funding.

Actinides. In the general discussion following the oral presentation of the Program Plan, most of the reviewers supported the addition of actinides to the list of radionuclides we plan to study in the migration experiments. In the reviewers' written comments, only one reviewer, Cleveland, remained a strong supporter of the use of actinides. We have examined the possibility of using actinides and have decided to keep the nuclear chemistry as simple as possible by not including actinides in the initial migration experiments. Plutonium, in particular, was not selected since laboratory studies have shown it to be so easily sorbed by rocks and minerals that it is essentially immobile. As it is not expected to move, the extra experimental problems it generates makes its use impractical for an initial test. Until we can prove that the migration experiments are feasible, safe, and give valid results, the use of actinides is not warranted. The increased costs related to actinide production and radiation safety requirements would pull funds away from our primary mission of developing techniques for studying radionuclide migration in the field. At the successful completion of this project, we can propose a second series of experiments desinged to handle actinides. We agree with Osmond that "it would be nice to include actinide studies, but only if it could be done without diluting the effort described in this proposal".

Miscellaneous - Review Comments and our Response.

In various discussions with both peer and programmatic reviewers during and after the oral presentation, a number of minor concerns were raised, some of which are noted in the reviewers' written comments. The following response addresses those concerns we considered important enough to warrant small changes in the revised Program Plan.

1. Iodine-131 has been added to the radionuclide list to represent I^{129}, an important component of nuclear waste.

2. Total organic carbon has been added to the list of groundwater analyses. We plan to provide water samples to Dr. J. Means of Battelle, Columbus for analysis.

3. We have added a brief description of how the groundwater analyses will provide information for modeling the equilibrium state and chemical evolution of the groundwater.

4. We have increased the time planned for doing the initial flow tests from 6 weeks to 16 weeks. This required changes in milestone dates and modifications of the network charts. This extra time will allow us to make additional tests that were recommended by different reviewers.

5. Our description of the hydrologic modeling efforts has been changed to reflect a cooperative effort with SCEPTER contractors. A more detailed explanation of the modeling will be left for the Engineering Test Plan when we have a better idea of the level of effort that will be required.

Budget. The extension of time for the initial flow tests plus a increase in LLNL overhead chanrges required an increase in the budget. The budget does not include increased drilling costs for holes to monitor the flow field between the inlet and outlet holes. Until we can devise a workable monitoring scheme and instrumentation, we are unable to estimate the drilling costs. We have not provided a more detailed budget in the published program plan as requested by some reviewers. Laboratory management views such information in a LLNL technical document series as inappropriate.

We've tried to address the major concerns of our reviewers. We thank them for their suggestions and hope that we have made the most of them.

We accept full responsibility for any omissions and plan to share our progress in the coming months with both peer and programmatic reviewers.

Sincerely,

Dana

Dana Isherwood
Principal Investigator

DI:ky
Enclosure

cc: L. Ballou
D. Coles
A. Miller
E. Raber
L. Ramspott
R. Stone

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S. Pahwa w/enclosure
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Sandia Laboratories

Albuquerque, New Mexico 87115

April, 20, 1981

Mr. Robert M. Nelson, Director
Waste Management Project Office
U. S. Department of Energy
Post Office Box 14100
Las Vegas, NV 89114

Dear Bob:

Attached are the Technical Overview Contractor summary comments relating to the Radionuclide Migration Field Studies Peer Review held during August 1980. These comments are based on the peer input comments, the principal investigation replies, and the resulting program plan modifications documented in UCID-18838 and LA-8487-MS.

Sincerely yours,

R.C. Lincoln

Richard C. Lincoln
NTS Waste Management Overview
Division 4538

RCL:4538:fg

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As stated

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RADIONUCLIDE MIGRATION STUDIES - G-TUNNEL TUFF

Grisak, Osmond, and Sargent were concerned about diffusion of the solute into the rock matrix. The program plan has been changed to place more emphasis on this aspect of the experiment.

Five of the eight Peer Reviewers commented in some way on water chemistry. There appeared to be two primary concerns: first, that the water used in the experiment should accurately reflect ground-water chemistry, and second, that the chemistry should be adequately controlled during the experiment. Erdal, for the most part, agreed with the comments and did not feel that it was necessary to modify the program plan. We suggest that any future modifications of the plan should include an amplified discussion of the water which will be used in the experiments and plans for either controlling or monitoring its chemistry.

Two of the Peer Reviewers asked for moderately detailed budget information, but we do not feel that a technical program plan is the appropriate forum for such information. Two programmatic reviewers felt that additional review would be helpful. However, we feel that the review process currently in use gives adequate assurance that proper experimental techniques and resources will be used appropriately while still allowing the experimenters appropriate flexibility to deal with problems as they arise.

Erdal's response addressed the reviewer's letters in considerable detail. We concur with the response not specifically mentioned above.

RADIONUCLIDE MIGRATION STUDIES - CLIMAX GRANITE

Four Peer Reviewers expressed concern that fracture saturation be assured before tracer injection. Snow in particular supplied lengthy suggestions as to how this could be done. Grisak and Hsu recommended that extra holes be drilled to measure the flow field between boreholes; Fenske also referred to the need for understanding the flow field. Witherspoon suggested the possibility of grouting prior to back coring. We support LLNL's decision to investigate these aspects of the experiment more fully. We also endorse LLNL's positive response to a number of minor changes suggested by one or more Peer Reviewers.

We concur with LLNL's position against incorporating actinides into the experiment at this time. The RNM studies are in part an attempt to develop experimental techniques as well as to measure fracture flow and radionuclide migration; at this stage it is appropriate to keep the chemistry as simple as possible.

Several Peer Reviewers suggested that block tests be added to the RNM program. In response to the reviewers' comments, LLNL submitted in January, 1981, a proposal for block tests. However, increased funding would be necessary for inclusion of block tests in the program.

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